



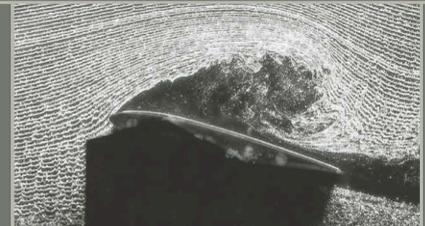
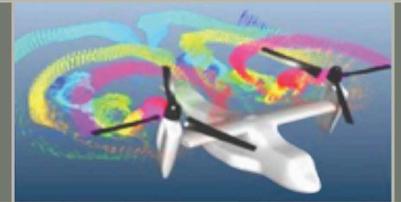
FIGURES OF MERIT

Remembrances of Those Who Built an Army-NASA Collaboration
and a New Age of Rotary-Wing Technology

1965–1985



	2-DIM	3-D (NON-ROTATING)	3-D (ROTATING)	3-D FORWARD FLIGHT
STEADY				
UNSTEADY				



Robert A. Ormiston and Irving C. Statler (Editors)

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*Figure of Merit—A measure of how closely a helicopter's
performance approaches its theoretical ideal.*

Cover Photo

The U.S. Army JUH-60A Rotorcraft Aircrew Systems Concepts Airborne Laboratory (RASCAL) flying over NASA Ames Research Center and the National Full-Scale Aerodynamics Complex (NFAC).

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Foreword

Who said engineers can't write? When Dr. Statler asked me if I would be interested in writing a Foreword to a book that would capture the accomplishments of the Army and NASA Ames Research Center through the memoirs of managers and engineers, I was a bit skeptical at first. I wondered if enough of the activities would be remembered to encompass the truly outstanding accomplishments resulting from the close relationship that developed when the Army and NASA agreed to work together. After reading the book, I see that my concerns were unfounded. This is really a great memorial to the many men and women who accomplished so much in aviation research and development (R&D) under the unique Army-NASA Joint Agreement.

During the early 1970s a blue-ribbon committee, known officially as the Army Materiel Acquisition Review Committee (AMARC), was established to look at improving the total acquisition process within the Army. AMARC recommended that the various commands that comprised the Army Materiel Command (AMC) be divided into R&D commands and readiness commands. This was implemented. The Aviation Systems Command (AVSCOM) was divided to form the Aviation Research and Development Command (AVRADCOM) and the Troop Support and Readiness Command (TROSCOM). The reorganization was underway about the time I arrived at AVSCOM (July 1975). I was assigned as the deputy commander with the primary duty of managing AVSCOM R&D. In that role, I had the opportunity to work on the Table of Organization and Equipment for the new R&D command. We came up with an organization that consisted of some 2,200 military and civilian personnel to staff 5 laboratories, 3 procurement activities at 3 helicopter companies, a flight test activity, program offices, and a headquarters. Three of the laboratories were to be collocated with NASA facilities at Ames Research Center, California; Langley Research Center, Virginia; and Lewis (now Glenn) Research Center, Ohio. The largest of these three Army organizations was at Ames where a Joint Agreement had already been implemented in 1965.

The decision to combine NASA and Army research efforts was a good one. It resulted in accomplishments that I'm sure exceeded expectations. In the Army, we certainly received a "bigger bang for the buck." NASA provided total overhead for our activities. Our engineers worked together with NASA engineers on programs needed by both activities. Sometimes the team leader was Army, other times NASA. It was a really great and productive relationship.

One of many accomplishments of the Army-NASA Joint Agreement was the joint development of the experimental vertical lift (XV-15) tiltrotor aircraft. This research-aircraft program, initiated by Mr. Paul Yaggy, Army, and Dr. Hans Mark, NASA, was highly successful and demonstrated technology applicable to military operational development. The XV-15 concept became the basis for the joint services vertical lift aircraft experimental (JVX) program. JVX was to be a joint program consisting of NASA, Army, Air Force, Navy, and Marines, and a task force of engineers from the services and Bell Helicopter, the contractor for the XV-15, convened at AVRADCOM headquarters. Mr. Charles Crawford, the Director of Engineering at AVRADCOM, was the team leader. The goal was to develop a tiltrotor aircraft that would meet the varied requirements of all the services. I attended a meeting at the office of the Secretary of Defense, Mr. Casper Weinberger. He was most appreciative of the Army-NASA Joint Agreement that led to his first joint service program. He was extremely proud of the program and even hung a certificate on his office wall. He hoped it would be the first of many joint efforts.

The Army, with NASA assistance, was to manage the program since they had the expertise. Unfortunately, the Navy wanted the Army, as manager, to fund half of the development costs. The Army declined and turned the program over to the Navy. The JVX later became the V-22 Osprey.

I would be remiss if I did not compliment Drs. Robert Ormiston and Irving Statler on the results of their excellent and challenging work in conceiving and pulling this book together. I'm not surprised because I know firsthand of the splendid work these two men accomplished and especially of Irv's service as Director of the Army Aeromechanics Laboratory at Ames when I served as Commanding General AVRADCOM. I'll confess that Irv and I had some great and productive trips together visiting foreign helicopter companies with whom we shared development efforts.

Major General Story C. Stevens, U.S. Army (Ret.)
Commanding General, Aviation R&D Command

Foreword

One of the most important factors that led to my decision to join NASA Ames Research Center was the close relations that the Center enjoyed with the Army and the Air Force. The connection with the Army was particularly strong because the U.S. Army Aeronautical Research Laboratory (AARL) was located at Ames. This relationship was initiated in 1965, 4 years before my arrival. I knew that AARL existed and that it was in good shape under the Army-NASA Joint Agreement. I sensed that my new mentors wanted me to pay attention to NASA issues, so it was about a year later before I was able to look into the Army laboratory in more detail. What really got my attention was the fact that the Army Materiel Command was in the process of establishing similar laboratories at NASA Langley Research Center and NASA Lewis (now Glenn) Research Center. The obvious concern was that the Army or NASA Headquarters would want to possibly relocate our laboratory to another Center. It was at this point that I really began to see how important the Army group was for Ames and our country. The years 1971 and 1972 marked the climax of the Vietnam War, and I realized the crucial role that helicopters played in that conflict.

We managed to hold on to the headquarters of the Army's combined organization, the Air Mobility Research and Development Laboratory (AMRDL), largely because of the efforts of Mr. Paul Yaggy, who was the founding Technical Director of AARL and would become leader of AMRDL. There were also two, very influential people in Washington D.C. who helped us to bring about the right outcome: Dr. Charles Poor, the Deputy Assistant Secretary of the Army for Research and Development, and Mr. William Harper, the NASA Deputy Associate Administrator of NASA for Aeronautics.

NASA management in Washington D.C. stipulated that Army personnel at NASA Research Centers would be treated as NASA employees. This had two very important consequences. The first was that Paul Yaggy, then Director of AMRDL encompassing all three collocated branches of the Army laboratory, would be treated as a NASA organizational director and would be a member of the NASA Ames Center management. He would also be responsible for dealing with the Army's Aviation Headquarters in St. Louis. The second consequence was that both NASA and Army employees at the three Centers would participate in the hiring process. Since the NASA Centers had very high admission standards, they tended to select the best people. An example of this is Bill Ballhaus and Jim McCroskey who were both hired by the Ames-Army Lab and both were eventually elected to the National Academy of Engineering.

I may be prejudiced but I believe that the development of the tiltrotor aircraft was the single most important item that was produced during my time at Ames in terms of impact on the world. I vividly remember a meeting about 6 months after I joined Ames. The topic was the tiltrotor, and Woody Cook had just done some work on using turboprop propulsion for tiltrotor aircraft with the turbine engines mounted on the wing tips. In essence, Woody described what would become the Bell XV-15. I really did not understand very much, but I knew that the group around the table—at least three or four individuals—agreed with Woody's suggestions. There were about 15 people in the room and the discussion was slightly heated. Finally, Woody looked at me and loudly questioned, "Are you the director?" I mumbled something because this was totally unexpected. All I could do was desperately look around for help. Then Woody said, "Ok, you are

the director! Then you have to fish or cut bait!”—and you all know that this was not the *real* statement.

I guess Woody’s “technical” arguments were strong enough for me to agree with what he had in mind, but his firmness in language as well as his supporters were probably more persuasive. This incident marked the beginning of the program to create the Bell XV-15 tiltrotor aircraft that proved the concept that a tiltrotor could be very useful. A group, headed by David Few, was established to develop the aircraft. A relationship with Bell Helicopter, the company that would eventually build the XV-15, was established. I vividly remember the day when Project Manager David Few, Aeronautics Director Leonard Roberts, and I met with Mr. James Atkins, the Chief Executive Officer of Bell Helicopter. (In addition, I have a vague memory that the Assistant Secretary of the Army for Science and Engineering, Norman Augustine, was also present.) We agreed, after a good discussion, that we would build two XV-15 tiltrotor aircraft with half the cost to be paid by the Army and half by NASA.

The first flight of the Bell XV-15 occurred on May 3, 1977, while I was still Director. It was a great day! David Few did a masterful job as project manager, and the entire Aeronautics Directorate turned out to help. The climax of the Bell XV-15’s adventures happened in early 1981 when I was still Secretary of the Air Force, helping with the transition for the Reagan Administration. I had a telephone call and I recollect that it was probably from Dr. Irv Statler. He wanted to know if I still had enough pull to send an XV-15 in a C-5 aircraft to the Paris Air Show in June. As it turned out, I was able to persuade the Chief of Staff, General Lew Allen, to go along with this caper.

To make a long story short, the XV-15, because of its unique flight characteristics, was the hit of the whole show. In addition, Dan Dugan, one of the Ames pilots, would have the aircraft hover and then bow its nose to the audience at the end of his routine, which always drew a thunderous cheer. All of this had enormous consequences. Both the new Secretary of the Navy, John Lehman, and Senator Barry Goldwater asked if they could fly the XV-15. John was a naval aviator and Goldwater was a Major General in the Air Force. We could not do this at the Air Show, however, both of them flew the plane at Patuxent River Naval Air Station back in the U.S. It was the Paris Air Show that made it easy for us to advocate the development of the Bell-Boeing MV-22 Osprey. We now have hundreds of these aircraft executing important missions for our national security, and each time I think about it, I remember Woody Cook’s admonition to me long ago!

Hans Mark

Director, NASA Ames Research Center, 1969–1977

Acknowledgments

The editors wish to acknowledge the contributions of many people who made this book possible. First, we wish to sincerely thank our co-authors, our Figures of Merit and fellow travelers on this reflective journey down memory lane. Many of them probably did not expect the demands we would place on them for deadlines, revisions, fact checking, and editorial changes. We are indebted to them for meeting those demands. At the same time, we are grateful for their belief in this book and their encouragement along the way. Without their considerable efforts and support, this book would not exist. At the outset, we extended a call to as many of the early members of the Ames-Army Lab as we could find and encouraged them to participate. Nearly everyone was enthusiastic about the idea. We then decided to expand the net to include authors from NASA who were closely associated with the Army. Our intent was to make a good faith attempt to enlist authors with an interest in the book. If we inadvertently omitted any potential authors, we sincerely apologize.

The Commanding General during much of the time of these memoirs was Major General Story C. Stevens. The NASA Ames Center Director was Dr. Hans Mark. We especially wish to thank them for the Forewords they wrote for this book. We also wish to thank Barry Lakinsmith, recently retired Director of the Army Aviation Development Directorate (Ames) for writing the Epilogue. The accumulated wisdom of these three gentlemen, and their unique and unmatched perspectives, provided the right accent and tone to open and close this book. They also deserve great credit for their immeasurable contributions over many years that were essential to the success of the Ames-Army enterprise.

We would also like to acknowledge Bill Warmbrodt, Chief of the rotorcraft Aeromechanics Branch at NASA Ames, for being a long-time supporter of rotorcraft and the Army-NASA collaboration, and in particular for his support and encouragement of this memoir.

Several of our authors went above and beyond the call of duty to assist us in important ways. We especially wish to thank Kathy (Yaggy) Hemingway for her contributions on behalf of her father, Paul Yaggy; Tom Snyder for his wise advice and guidance throughout the effort; Pat Horn for her outstanding editorial skills to help with our authors' early chapter drafts; and George Tucker for reviewing a large number of his fellow co-authors' chapters. We also wish to acknowledge John Davis for his expertise on the history of Army Aviation R&D and the Army-NASA Joint Agreement.

Finally, we owe an enormous debt to the tireless efforts of our professional editor, Catherine Dow, for her painstaking and meticulous efforts to edit and format all of the written material in this book. Neither of us anticipated the magnitude of this task, and we are thankful that we had her skills and energy to meet the challenge.

As the effort expanded, not surprisingly, beyond our original expectations, our families felt the impact. We sincerely wish to acknowledge the forbearance and sacrifices of our wives, Mary Jo Ormiston and Renée Statler throughout this project.

Table of Contents

Introduction.....	The editors
Background of Army Aviation R&D and the Army-NASA Joint Agreement.....	The editors
Paul F. Yaggy—The Ingenious Architect.....	The editors
Personal Memories of Paul Yaggy, 1965–1974	Kathy (Yaggy) Hemingway
An Aeronautical Odyssey	Edwin W. Aiken
Army Lab Experience	William F. Ballhaus, Jr.
Back to Truth and Science	Chris L. Blanken
The Gift of Flight, 1980–1989	Lorene (Lori) Cross Blanken
Personal Recollections of the Ames-Army Laboratory in the 1970s	Frank Caradonna
Memories of My Career at the Army Lab	Larry Carr
My Years Under the Army-NASA Joint Agreement, 1967–1983.....	Lloyd D. Corliss
An Army Experimental Test Pilot Remembers	Daniel C. Dugan
Interface Between the Engineer and the Project.....	Bob George
Army-NASA Ames Research Center Memoir	Peter M. Goorjian
The Best Lab in the U.S. Army, 1965–1995.....	Terry Gossett
Army-NASA Memoirs.....	Dewey H. Hodges
Remembrances of a Secretary.....	Pat Horn
Recollections of Army and NASA Rotorcraft Research, 1970–1986	Wayne Johnson
A Unique Partnership and a World-Class Research Activity	Andy Kerr
Army Memoir: Makings of a Helicopter Handling-Qualities Specification	David L. Key
Remembrances From AFDD	Donald L. Kunz
Memories of the Army-NASA Joint Agreement, 1978–1985	J. V. Lebacqz
The Evolution of Tiltrotor Technology—An Army/NASA Collaborative Effort.....	Martin Maisel
Army Research Lab in the Early Years	Kenneth W. McAlister
Army-NASA Memoirs.....	William J. McCroskey
Recollections of Working at NASA Ames Research Center.....	Ken Mort
A New Frontier—Rotorcraft Research at Ames.....	Robert A. Ormiston
The Golden Age of the Army Research Labs.....	David A. Peters
My Involvement With the Army Air Mobility R&D Laboratory at Ames.....	Victor L. Peterson
Remembrances—My Early Experiences as a Research Scientist.....	Frederic H. Schmitz

Table of Contents (cont.)

Adventures in Low Disc Loading VTOL Design at Ames.....	Mike Scully
A Dream Career	C. Thomas Snyder
My Life With Army Aviation Research, 1970–1985	Irv Statler
2GCHAS, An Epic Effort, 1979–1991	Wendell Stephens
“If You Follow Your Passion, There Will Always Be Great Opportunities”.....	Mark B. Tischler
On Being in the Right Place, at the Right Time... ..	George E. Tucker
My Experience in Rotorcraft Aerodynamics Research in the Army at Ames.....	Chee Tung
I Was Not an Engineer.....	Rande Vause
Life is Good	William Warmbrodt
A Korean in the U.S. Army	Yung Yu
Epilogue	Barry R. Lakinsmith

Introduction

Robert A. Ormiston and Irving C. Statler (Editors)

The authors of this book are the Figures of Merit—the scientists, engineers, technicians, secretaries, test pilots, managers, visionaries, and leaders who built a unique interagency collaboration under the Army-NASA Joint Agreement at Ames Research Center and ushered in a new age of rotary-wing technology. The U.S. Army Aeronautical Research Laboratory (AARL) was formed in 1965 to strengthen the Army’s capabilities in aviation R&D, and the Army-NASA collaboration at Ames was intended to benefit both agencies by sharing personnel and facilities for research in areas of common interest in low-speed aviation.

The stories in this memoir are about the emergence and evolution of AARL and the collaboration at Ames Research Center, from 1965 to about 1985. AARL, birthed 53 years ago, grew from literally nothing into a leading, internationally recognized institution for rotorcraft research. How did that happen? How did AARL and its successor organizations achieve such success? Was it the people, the leaders, the environment, the times, or all of these together? Ames, steeped in the National Advisory Committee for Aeronautics (NACA) culture, was full of storied, eminent researchers. AARL was fortunate to be situated in the midst of the vitality of Ames Research Center, with excellent experimental facilities, wind tunnels, new computers, and flight research capabilities. Nearby Stanford University offered rich and stimulating intellectual resources, and Silicon Valley was about to awaken. Perhaps the California dream, the San Francisco Bay Area, or even the turbulence of the mid-1960s, played a part.

The AARL story is one of university fresh-outs, experienced NACA researchers, and helicopter industry veterans, learning together under leaders with creative vision and instinctive management skills. Given all this, how could they not succeed? This collective memoir comprises many parallel intersecting stories about the Army-NASA collaboration at Ames—by and about the people who were a part of it. For all the rapid progress and success, there were setbacks and struggles too. In their stories, the authors seek to convey who they were, what they did, and most of all, what the experience was like.

Over the past 50 years, because of unique performance and unmatched versatility, rotorcraft have become an ever more important part of aviation. Helicopters came of age with the military in Korea and their numbers exploded in Vietnam under the Army’s Air Mobile Doctrine. In the civil sector, helicopters are essential for transportation, public safety, agriculture, corporate transport, offshore oil operations, medical evacuation, firefighting, and news operations. Once viewed as an unreliable curiosity, the helicopter, thanks to modern technology, has become today’s ubiquitous miracle machine. The revolutionary tiltrotor is now an indispensable part of the modern military and is destined to make its way in civil aviation. Horizons are opening rapidly for even newer vertical takeoff and landing (VTOL) and autonomous concepts.

Since its inception and despite its small size, AARL (and successor organizations, referred to herein as the “Ames-Army Lab”), was disproportionately influential in these paradigm shifts in aviation. The outsize role of the Ames-Army Lab is evident in a broad spectrum of innovation from fundamental science to revolutionary aircraft...from unlocking the fundamental mechanisms of rotor acoustics to launching the XV-15 and the birth of tiltrotor aviation...from solving the mysteries of rotorcraft dynamics to creation of the Army Aeronautical Design

Standard-33 (ADS-33) flying-qualities specifications now used worldwide...from advancing human factors and man-machine interactions to elevating rotorcraft computational fluid dynamics (CFD) codes like Helios that are revolutionizing design of future rotorcraft. The list goes on.

We undertook this memoir to memorialize and reflect on the Army-Ames experience in the early days—and to document it for ourselves, our colleagues, and anyone interested in the collaboration of two federal agencies in rotorcraft research. We sought to tell the compelling story of the vision of the early leaders and the research environment at Ames that enabled AARL to flourish. We enlisted Army and NASA principals from the early years to tell the story in a series of memoir chapters based on their varied perspectives. We aimed to:

- Provide an image and an understanding of the research environment created by the Army-NASA Joint Agreement and the Army and NASA employees who worked within it.
- Demonstrate and celebrate the success and the value of the Joint Agreement and the Army-NASA collaborative relationship and how it achieved its remarkably influential role in the progress of international rotorcraft R&D.
- Inform the current staff of their heritage and inspire them to uphold the vision, goals, and standards of excellence of the original Army-Ames staff members.
- Distill and convey the lessons learned from our experience—the recipe for success—for the benefit of future R&D professionals.

Although similar agreements were later established at NASA Langley and Glenn Research Centers, this memoir is focused on Ames because that is where it all started and where the Army-NASA collaboration was exemplary. The memoirs focus primarily—although not exclusively—on the period of 1965 to about 1985 because these were the most unique and formative years for establishing relationships and learning how to work together.

In their individual accounts, the authors of this book describe their accomplishments in various degrees of detail. This book evidences our premise that the Ames-Army Lab was outstandingly successful, and the Army-NASA collaboration at Ames was a key enabler of this success. Collaborative research under the Joint Agreement at Ames built upon over four decades of prior rotorcraft research beginning with the British Royal Aircraft Establishment, then NACA at Langley, Wright Field during WWII, and the Army's Transportation Research Command (TRECOCOM) and Aviation Materiel Laboratories (AVLABS) in the late 1950s. Before the 1960s, rotary-wing R&D was somewhat the poor stepchild of aeronautical research, burdened with complex physical phenomena and fewer available resources than fixed-wing aircraft R&D. Progress was limited. With the advent of AARL, this began to change as rotorcraft research at Ames got underway and started to produce notable results.

AARL research focused initially on the complex aerodynamics of rotary-wing aircraft. The Ames-Army Lab made seminal discoveries about boundary layer phenomena, dynamic stall, unsteady transonic flow, and rotor wake phenomena that laid the foundations for improving rotor performance and reducing vibratory airloads. These contributions came from small- and large-scale experiments on airfoils and rotors, as well as analytical and computational studies. Army and NASA collaborators pioneered CFD for unsteady transonic flow phenomena. They also cooperated closely with academia, industry, and international groups under many Memoranda of

Understanding (MOUs). Techniques, methodologies, and research approaches developed at Ames were quickly adopted across the entire international rotorcraft community.

Similarly, rotor acoustics research led to seminal discoveries that explained fundamental phenomena and changed the thinking of the scientific community. Once again, small-scale, large-scale, and pioneering flight test methods were combined with theory and analysis and established the Ames-Army Lab as a leader in the field.

Stimulated initially by the Army's difficulties with the AH-56A Cheyenne, rotorcraft dynamics and aeroelasticity became fertile ground for comprehensive analytical and experimental investigations that greatly expanded the fundamental understanding of hingeless rotor aeroelastic stability and produced the first rigorous nonlinear structural dynamic models for elastic rotor blades. These investigations contributed significantly to accurate prediction codes essential to design advanced rotorcraft with optimum performance and reduce the risks of the unknown during development of new rotorcraft.

The Ames-Army Lab's early emphasis on rotorcraft aeromechanics soon expanded to encompass flight dynamics and control, human factors, handling qualities, automated flight control, cockpit integration, display design, and the development and use of ground-based and in-flight simulators. Numerous investigations significantly impacted advanced flight controls for civil and military users. In particular, ADS-33 for flying- and handling-qualities of helicopters was developed and then adopted worldwide. Advanced system identification (CIFER[®]) and flight control design methods (CONDUIT[®]) were developed and found wide acceptance, not only in the rotorcraft design community but, notably, in the fixed-wing industry as well—demonstrating far reaching recognition of the technical leadership of Army-Ames rotorcraft researchers.

The preliminary design group associated with the Ames-Army Lab became renowned for its expertise and influence on the Army's decision-making process in fielding new systems, including the Black Hawk, Apache, Kiowa Warrior, and the Navy-Marines V-22 Osprey.

Arguably the most important achievement of the Army-NASA collaboration was the development of the tiltrotor. The leverage of the Army/NASA partnership yielded rigorous analytical tools and full-scale semispan proprotor wind tunnel testing that overcame earlier difficulties and paved the way for the resoundingly successful NASA/Army/Bell XV-15 tiltrotor, without which, today's revolutionary V-22 Osprey would not exist.

We hope that the stories in this book will convey to the reader a sense of the value of these accomplishments and insights about the qualities of the Army-Ames enterprise that led to them—the NASA Ames environment, enlightened management, and the R&D approach established in the early years. We feel that the authors' stories are infused with these insights. One of our hopes for this book is that the lessons learned from the Army-NASA collaboration will benefit future R&D leaders, planners, and managers hoping to emulate our success. What were the elements that contributed to Army-Ames success? The recipe boils down to a few essentials, in addition to interagency collaboration and outstanding managers. It starts with exceptional people, providing a supportive environment, and trusting their skills and instincts. For a collaborative relationship, both partners need to be invested at all levels. For an organization like the Ames-Army Lab, the ultimate mission must be kept in mind. This requires a delicate balance between the scientific interests of the researcher and the requirements of the military user. We believe that this book is a direct expression of the success of the Ames-Army

enterprise and that it conveys an important message that collaboration and working together pays off and that *the principles and methods developed by NACA/NASA and followed by the Army at Ames* contributed to significant R&D progress. We hope the reader will agree with us.

Before proceeding to the memoirs in this book, a short chapter is provided to acquaint the reader with the background of Army aviation R&D leading up to the evolution of the Ames-Army Lab and to describe the Joint Agreement and how it worked.

The main body of this book consists of the individual memoir chapters of Army and NASA personnel who worked at the Ames Research Center from 1965 to about 1985. Each chapter is the author's memories of events and moments during the time that she or he worked at Ames. The chapters are arranged in alphabetical order by authors' name, and the authors are identified by their affiliation—Army, NASA, or both. Most chapters include a list of references and an index.

The chapters reflect a diverse range of content with perspectives from a variety of positions at Ames. The treatment is informal, and because this book is a memoir rather than a history, full details of timelines, organizations, or personnel, are not included. Authors provided differing amounts of early biographical information and some, depending on their tenure at Ames, extended their accounts beyond 1985. Chapters written by managers are generally broader in scope and include activities of their respective groups. Notwithstanding the diversity of the authors' accounts, the flavor and the essential message of the Ames-Army Lab emerge from the collective memories.

As befits his special role in the Army-NASA collaboration at Ames, the first chapter, in two parts, is devoted to Paul Yaggy, the first Technical Director of AARL and also the first Director of the U.S. Army Air Mobility Research and Development Laboratory (AMRDL). The final chapter is an epilogue written by Barry Lakinsmith, the recently retired Director of the Army Aviation Development Directorate (Ames) that reflects on memories of the past in the context of the Army and NASA organizations as they exist today.

Finally, we hope that the reader will not only enjoy the stories, but will also gain an impression from the compilation of these memoirs of what life was like in the inimitable setting of the Ames-Army Lab.

Background of Army Aviation R&D and the Army-NASA Joint Agreement

Robert A. Ormiston and Irving C. Statler (Editors)

The genesis of the Army-NASA collaboration at Ames grew out of the Army's need to strengthen its technical capabilities for aviation R&D. After the Ames-Army Lab was formed in 1965, the Army R&D structure evolved continuously over the years. To help orient the reader and to better appreciate the context for the research described in these memoirs, we felt it would be useful to outline the formation of the Ames-Army Lab and the organizational changes that occurred throughout the years addressed by the authors of this memoir—1965 to 1985 and beyond. The concepts and workings of the Army-NASA Joint Agreement will be described as well.

As far back as the mid-1940s, the Army had begun to take steps to establish its capabilities to procure rotorcraft and to develop its aviation R&D expertise. Army aviation R&D began in 1945 under the direction of the Ordnance Corps and was transferred in 1952 to the Transportation Corps at Ft. Eustis, Virginia [1, 2]. The Transportation Corps was responsible for all Army transportation R&D; however, since it was not allowed to establish “independent” aeronautical facilities, an Aviation Field Office was established (around 1952) at Wright Field to handle Army aviation procurement and R&D. But the Army–Air Force arrangement was not entirely satisfactory, and the Army was authorized to conduct research “to determine requirements.” This was the first instance of the Army—principally the Transportation Research Command (TRECOCOM)—undertaking aircraft R&D. In 1962, the materiel development functions of the Army's Technical Services, including aviation, were merged into the U.S. Army Materiel Command (AMC). TRECOCOM was transferred into the Aviation Command (AVCOM) when it was established at St. Louis. In 1965, TRECOCOM was renamed the U.S. Army Aviation Materiel Laboratories (AVLABS) under the Aviation Systems Command (AVSCOM).

In 1965, the Army was given authority to participate directly in the development, engineering and procurement to meet its aviation needs (Defense Procurement Circular 23, February 1965) [3] and in-house capabilities were established to conduct R&D in engineering, flight test, and qualification. Despite significant accomplishments, the work at AVLABS was largely contractual in nature and, with only limited in-house facilities, it could not conduct needed in-house research. Therefore, AMC sought a means to obtain this capability and to strengthen low-speed aeronautical research.

The Army conducted a survey of existing federal research organizations and found that NASA Ames Research Center at Moffett Field, California, had the wind tunnels, flight simulators, computers, research aircraft, and personnel with expertise in aeronautical R&D that the Army needed. At the same time, utilization of the aeronautical R&D facilities, personnel, and funding at Ames were decreasing as NASA's activities shifted to space. The Army and NASA saw a synergistic opportunity in locating an Army aeronautical R&D operation at Ames that would use facilities like the 40- by 80-Foot Wind tunnel, which was particularly well suited to testing helicopter rotors. Consequently, some brilliant minds in the Army and NASA conceived the innovative idea that two agencies could work together with no exchange of funds. Under an Army-NASA Joint Agreement, they would share personnel and facilities to collaborate on R&D

in areas of mutual military and civil interest in rotary-wing aircraft. Ames would accommodate Army personnel and grant the Army priority access to its experimental and support facilities.

In 1965, the Army and NASA entered into an agreement signed by AMC Deputy Commanding General, Major General William B. Bunker, and NASA Associate Administrator, Dr. Robert C. Seamans, Jr., that provided for the two organizations to collaborate. But it was not these high-level signatories who made the concept a reality. We who were the beneficiaries acknowledge and pay tribute in these memoirs to the real “Founding Fathers” of the Army-NASA Joint Agreement. On the Army side, they included Dick Ballard, Department of the Army, and Norm Klein and John Beebe, Army Materiel Command. On the NASA side, the most influential role was that of Bill Harper, NASA Ames, during his tenure at NASA HQ [4].

In February 1965, the Army Aeronautical Activity was established at Ames with Colonel Cyril D. Stapleton as the Commander. Mr. Paul F. Yaggy, a former NASA Ames employee, was appointed Technical Director in September 1965. In 1967 the name was changed to the Army Aeronautical Research Laboratory (AARL). In 1968, when Colonel Stapleton retired, Paul Yaggy became the Director. At essentially no cost for facilities and an initial increase of only about 30 people, the Army had an operational R&D capability within 3 years of signing the Joint Agreement, and NASA had augmented its aeronautical research and support staff at Ames at no direct cost.

At the same time that the Army-NASA experiment in collaboration was getting underway at Ames Research Center, AMC initiated new planning for air mobility R&D. In April 1966, the Army and the Air Force entered into the Johnson–McConnell agreement, between the Army and Air Force Chiefs of Staffs, over the control of fixed-wing and rotary-wing aircraft. Under this agreement the Air Force gained control of all fixed-wing aircraft and the Army was given control of all rotary-wing aircraft. The Air Force agreed to relinquish all claims for helicopters and future rotary-wing aircraft designed and operated for support of Army Forces. Accordingly, the Army gained responsibility for the development of rotary-wing aircraft.

In September 1966, AMC initiated an Ad Hoc Committee of members chosen from industry and academia to assist in planning its air mobility R&D. The committee was chaired by Dr. Phillip R. Carlson of Lockheed California Company. The report, known as the Carlson Report [5], was released February 1, 1967, and recognized the great value of air mobility demonstrated in Vietnam. It also recognized the expanding Army responsibilities and opportunities in the field and recommended establishing an Army Air Mobility R&D Center. It recommended doubling Army air mobility R&D to \$200M/year before 1970 and that the Army’s dispersed efforts, including AARL and AVLABS, should be consolidated into an Air Mobility R&D Center located close to the headquarters of the Aviation Command and its procurement, supply, and maintenance organizations. The committee recommended a technical and support staff of 1,000 personnel (expanding by 50 percent in 10 years) with a 70/30 ratio of in-house to contract R&D. Facilities required for in-house R&D were estimated to cost \$30M in the first 5 years.

While the Army agreed with the concept of the Carlson Report, there were insufficient funds to establish the recommended Air Mobility R&D Center and provide research facilities. The Army did have the nucleus for an R&D center, but these elements were widely dispersed. AVSCOM in

St. Louis provided engineering development of air mobility systems, acquisition, and support while AVLABS at Ft. Eustis, Virginia, supported exploratory (6.2) and advanced development (6.3). AARL at NASA Ames provided aeronautical research (6.1). Ultimately, AVLABS and AARL became the nucleus for the Army's expanded in-house laboratory capability in the rotary-wing field.

When Hans Mark became Director of NASA Ames Research Center in early 1969, he learned that the Army was in the process of expanding the Army-NASA Joint Agreement to encompass laboratories similar to AARL at other NASA centers. He was concerned that Ames would lose AARL, and he urged that the headquarters of the expanded Army organization be located at Ames.

In November 1969, NASA and AMC entered into an expanded collaboration under a Master Joint Agreement that encompassed arrangements at NASA Langley Research Center and at Lewis Research Center (renamed Glenn in 1999), similar to the one used to establish AARL at Ames Research Center.

On July 15, 1970, the U.S. Army Air Mobility Research and Development Laboratory (AMRDL) was established by combining the NASA co-located organizations together with AVLABS at Ft. Eustis, Virginia, as four subordinate directorates. AMRDL HQ was located at Ames and reported to AVSCOM in St. Louis, and the four subordinate directorates were:

- Ames Directorate located at NASA Ames Research Center (the former AARL).
- Eustis Directorate located at Ft. Eustis, Virginia (the former AVLABS).
- Langley Directorate, located at NASA Langley Research Center, Hampton, Virginia.
- Lewis Directorate located at NASA Lewis Research Center, Cleveland, Ohio.

Paul F. Yaggy left AARL to become the first AMRDL Director while Dr. Irving C. Statler became the new Director of the Ames Directorate in 1971. The objective of the Army's aviation R&D program was to demonstrate technology needed to provide simple, rugged, reliable air mobility equipment of superior performance. The Laboratory mission was defined as: "Plan, develop, manage, and execute for AVSCOM the research (6.1), exploratory development (6.2), and advanced development (6.3) programs through demonstration and technology to provide a firm technical base for future development of superior airmobile systems."

In 1977, AMC became the Army Development and Readiness Command (DARCOM) while the R&D functions of AVSCOM were placed under a new R&D command called the Aviation Research and Development Command (AVRADCOM). Correspondingly, AMRDL became the Research and Technology Laboratories (RTL) under AVRADCOM. The former AMRDL directorates became RTL laboratories, i.e., the Ames Directorate became the Aeromechanics Laboratory (AL), the Langley Directorate became the Structures Laboratory (SL), the Lewis Directorate became the Propulsion Laboratory (PL), and the Eustis Directorate became the Applied Technology Laboratory (ATL). Prior to this reorganization, Paul Yaggy had retired in 1974 as Director of AMRDL and was replaced by Dr. Richard M. Carlson. In 1977, Richard Carlson then became the Director of RTL while Irv Statler became Director of AL.

At about the same time, another significant change took place when NASA HQ reviewed the roles and missions of its field centers. Ames was named Lead Center for rotary-wing aircraft. Many of Langley's rotorcraft program responsibilities were transferred to Ames; one of the most significant changes was the transfer of Langley's flight research rotorcraft to Ames, along with a corresponding expansion of the rotorcraft organizations at Ames. Among the reasons cited for the decision was that the Army-NASA relationship at Ames was the most effective of the three co-located Army-NASA organizations.

A few years later, in 1984, the Army reestablished AMC and AVSCOM, which then recovered the aviation R&D function and placed it under a new organization in St. Louis—the Aviation Research, Development, and Engineering Center (AVRDEC). The RTL became the Army Research and Technology Activity (ARTA), remaining headquartered at Ames and reporting to AVRDEC and AVSCOM. As in 1977, the four directorates were again renamed: the Aeroflightdynamics Directorate (AFDD) at Ames, the Aerostructures Directorate (ASTD) at Langley, the Propulsion Directorate (PD) at Lewis, and the Aviation Applied Technology Directorate (AATD) at Ft. Eustis. Dick Carlson continued as the Director of ARTA. Shortly thereafter, when Irv Statler retired as Director of AFDD at Ames in 1985, Andrew W. Kerr, who had been Chief of the Advanced Systems Research Office (ASRO) at RTL HQ, became the new Director of AFDD.

This arrangement continued until about 1990 when ARTA HQ at NASA Ames was abolished and the four directorates, AFDD, ASTD, PD, and AATD, along with the Advanced Systems Research and Analysis Office (ASRAO) at Ames, reported directly to AVRDEC in St. Louis. Dick Carlson served as the Chief of ASRAO until he retired in 1995. Another change occurred in 1992 when the Army Aerostructures and Propulsion Directorates at NASA Langley and NASA Lewis were transferred from AVSCOM to the Army Research Laboratory (ARL) Vehicle Technology Directorate (VTD) at Aberdeen Proving Ground, Maryland, to conduct basic research. The portion of the Langley Aerostructures Directorate engaged in aeromechanics research was retained by AVRDEC and attached to AFDD.

The accumulated changes diminished the breadth and scope of the Army-NASA collaborations at the co-located locations. Of the original four ARTA directorates and ARTA HQ, only AFDD and ASRAO at Ames and AATD at Ft. Eustis remained. Except for the Joint Research and Planning Office (JRPO) at Langley, the Army-NASA collaboration had reverted back to a single NASA organization, where it all began, at Ames Research Center.

At the upper level, significant Army reorganizations were ongoing, but with less direct effect on collaboration under the Joint Agreement. In 1991, AVSCOM was reorganized to become a part of the Army Aviation and Troop Command (ATCOM). A few years later, as part of the 1995 Base Realignment and Closure Commission (BRAC), ATCOM was disestablished to merge with the Missile Command (MICOM) and form the combined Army Aviation and Missile Command (AMCOM) in Huntsville, Alabama. Within AMCOM, AVRDEC (aviation) and MRDEC (missile) became the Aviation and Missile Research, Development, and Engineering Center (AMRDEC), and this arrangement continued for nearly 13 years. Around 2002 AMRDEC was assigned under the new Army Research, Development, and Engineering Command (RDECOM).

Under the Army-NASA Joint Agreement at Ames since 1985, AFDD continued relatively unchanged until 1997 when the Army and NASA rotorcraft groups at Ames were reorganized to

survive the changing climate for government research (see Andy Kerr's chapter). The rationale aimed to reduce costs and streamline management. The two parallel organizations were merged into a single integrated rotorcraft R&D organization with joint Army and NASA management. The new Army/NASA Rotorcraft Division existed in both the Army and NASA hierarchies reporting to the NASA Ames Aeronautics Directorate and AFDD. The Division comprised two Branches, the Aeromechanics Branch and the Flight Control and Cockpit Integration Branch. At both the Division and Branch levels, the Chief and Deputy Chief positions were filled by one manager from the Army and one from NASA.

Ironically, a few years later, the climate for aeronautics research within NASA changed again. The NASA Administrator, Dan Goldin, stated that rotorcraft technology had reached maturity and research was no longer needed; plans were made to end the NASA rotorcraft R&D program at Ames and the joint Army/NASA Rotorcraft Division was abolished in 2005. The Army and NASA researchers reverted back to separate NASA and Army organizations. AFDD now comprised the Aeromechanics Division and the Flight Control and Crew Systems Division with Army managers. Subsequently, NASA reversed its decision to abandon rotorcraft research and the NASA-Ames rotorcraft program was revived.

In 2011, AFDD and AATD were reorganized yet again and integrated organizationally into a new AMRDEC Aviation Development Directorate (ADD) with headquarters at Redstone Arsenal in Huntsville, Alabama, and with Dr. William Lewis as ADD Director. This included the members of JRPO at Langley Research Center. The integrated organizational structure diminished the distinctions between Ames, Langley, and Ft. Eustis activities, and they became, to a certain extent, parts of a virtual organization. Functional technical groups included researchers from different geographical locations, and managers of these groups were also distributed among the different locations.

The background of organizational evolution outlined above illustrates the large number of organizational changes and associated organizational names during the Army-NASA collaboration of the past 53 years. Throughout this memoir, the organization names that pertained to the period of the material being described were used. Nevertheless, keeping track of the evolution of the organizations and their names throughout this memoir may present a bit of a challenge for the reader. Therefore, the table on the next page summarizes the names of the Ames-Army Lab from 1965 to the present including the next two levels in the reporting hierarchy.

The Army-NASA Joint Agreement and How it Worked

The Army-NASA Joint Agreement is not only unique in terms of interagency support and efficient sharing of resources for common objectives, but also because the co-location and direct personal interactions and collaborations between Army and NASA rotorcraft researchers created a stimulating and exceptional environment for both. High-quality science was accomplished because the Joint Agreement provided an environment of close and personal collaborations between representatives of both the "pull" of the Army user, anxious to exploit the latest technologies, and the "push" of the researchers, eager to find application for their latest discoveries.

Ames-Army Lab Organizational Evolution

Year	Ames-Army Lab	Reporting Hierarchy	
1965–1970	Army Aeronautical Research Laboratory (AARL)		Army Materiel Command (AMC)
1970–1977	Ames Directorate (AD)	U.S. Army Air Mobility Research and Development Laboratory (AMRDL)	Army Aviation Systems Command (AVSCOM)
1977–1985	Aeromechanics Laboratory (AL)	U.S. Army Research and Technology Laboratories (RTL)	Army Aviation Research and Development Command (AVRADCOCOM)
1985–1990	Aeroflightdynamics Directorate (AFDD)	U.S. Army Research and Technology Activity (ARTA)	Army Aviation Research, Development and Engineering Center (AVRDEC)/ Army Aviation Systems Command (AVSCOM)
1990–2002		Army Aviation Research, Development, and Engineering Center (AVRDEC)	Army Aviation Systems Command (AVSCOM)/ Army Aviation and Troop Command (ATCOM)/ Army Aviation and Missile Command (AMCOM)
2002–2011		Army Aviation and Missile Research, Development and Engineering Center (AMRDEC)	Army Research Development and Engineering Command (RDECOM)
2011–present		Aviation Development Directorate (ADD)	

At each of the NASA Centers, there were groups of Army and NASA scientists and engineers who worked independently on rotary-wing R&D programs in their respective Army and NASA organizations, but there were also Army employees within the NASA organizations working side-by-side with NASA researchers under NASA technical managers. The Army also provided support personnel to NASA (e.g., wind tunnel mechanics and administrative people) in lieu of funding to offset the added burdens imposed by the Army's presence. The Army research personnel were organized into two groups, the Army Aeronautical Research Group (AARG) and the Joint Aeronautical Research Group (JARG), while the Army support personnel comprised a Technical Support Group (TSG). AARG personnel conducted fundamental and applied research in rotorcraft aeromechanics relatively independently from NASA and were located in office space provided by NASA in Building N-215 along with the adjacent Ames 7- by 10-Foot Wind Tunnel No. 2. Collaborations of members of this group with NASA researchers were mostly limited to rotor tests in other Ames wind tunnels or computer facilities. JARG comprised Army personnel who were integrated in various NASA organizations throughout Ames. These Army scientists and engineers collaborated with NASA counterparts on a daily basis in areas of

research such as the stability, control, and handling qualities of rotary-wing aircraft and the human factors of their operation. The Army support personnel assigned to TSG were integrated in NASA organizations similar to JARG. The AARG, JARG, and TSG organizational structure remained a part of AARL and the AMRDL Ames Directorate until Irv Statler's reorganization of the Aeromechanics Laboratory in 1978.

A further enhancement of the environment for collaboration was the agreement reached in 1977 among the Army, NASA, and the Office of Personnel Management that permitted the personnel-management responsibilities for Army employees assigned to NASA Ames be taken over by NASA's personnel office at Ames. This became the model for personnel management at the other Army Directorates at Lewis and Langley Research Centers.

Army personnel could compete for supervisory positions in the NASA organization and remain Army employees up to the level of NASA Branch Chief supervising both NASA and Army personnel. If an Army employee was selected for a position at a higher level of NASA management, the employee would transfer to NASA. All of this was fully transparent; one could not differentiate an employee as either Army or NASA. This feature strengthened the intent of the Joint Agreement to focus on subjects mutually important to both organizations' needs for R&D in rotary-wing technologies.

At management levels in both the Army and NASA, enthusiasm for the Joint Agreement waxed and waned as funding, organizational structures, and management personnel changed in both organizations over the years. However, at the working levels in both the Army and NASA there was never any doubt as to the benefits of their efforts to produce the highest quality rotary-wing technology. The R&D that was (and continues to be) accomplished contributed substantially to progress in the arena of vertical lift.

The innovative concept of the Joint Agreement proved to be outstandingly successful, and it has been acknowledged as an extraordinary demonstration of the benefits of interagency cooperation for the mutual support and efficiency of both agencies.

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Paul F. Yaggy—The Ingenious Architect

**Bob Ormiston
Irv Statler
Army**

Preface

If one figure stands out in the remarkable saga of the Army-NASA research collaboration at NASA Ames Research Center, it would be Paul F. Yaggy, the first Technical Director of the U. S. Army Aeronautical Research Laboratory (AARL). Paul rose from the ranks of hard-working, dedicated, right-thinking National Advisory Committee for Aeronautics (NACA) engineers to spark a tiny Army laboratory, created outside the mainstream Army research, development, test, and evaluation (RDT&E) establishment, into a leading, internationally recognized institution for rotorcraft research. The rare quality and harmony of the Army-NASA collaboration can be attributed in large measure to Paul's ingenuity and imaginative leadership.



Paul Yaggy (circa 1965).

Indeed, there would have been no incentive to write this book had it not been for Paul. We who have enjoyed the opportunity to work in this remarkable environment at Ames owe our experiences to Paul, and we gratefully acknowledge Paul's role in enabling us to succeed. It is fitting, therefore, that we devote the first chapter to Paul Yaggy himself. This first of two parts briefly outlines Paul's early career and recounts how he built the Army research infrastructure at Ames and across multiple NASA research centers. The second part of this chapter contains the heartfelt and intimate personal reflections of his daughter, Kathy, who has written Paul's chapter "in his place." We hope to provide a sense of Paul's well-deserved legacy and set the stage for the authors of this book as they recall their personal reflections of Paul, his contributions, and his influence on their lives.

NACA/NASA Career

Paul Yaggy began his career in 1944 with the NACA Ames Aeronautical Laboratory and continued at Ames Research Center when NACA became part of NASA in 1958. He served until 1965, except for active duty Navy service in 1951 and 1952. Paul first participated in wind tunnel testing on various WWII aircraft. He was a researcher in the 40- by 80-Foot Wind Tunnel Branch and later became a team leader and then Assistant Branch Chief. His research focused on aerodynamics, structures, and aeroelasticity of rotors, propellers, and ducted fans of V/STOL aircraft. Paul's collaborations with several members of the branch are described in the chapter by Ken Mort, one of Paul's long-time NACA/NASA colleagues.

Some of Paul's earliest work during the 1950s involved predictions and measurements of the effects of the flowfield of airplane wing-body configurations in the vicinity of the airplane propeller. Collaborators included Vernon Rogallo and John McCloud. Later investigations addressed the effects of the nonuniform flowfield on the oscillatory airloads acting on the



Members of the 40- by 80-Foot Wind Tunnel Branch, December 1958. Ken Mort left, kneeling left, John McCloud in wheel chair; Branch Chief Woody Cook, dark suit with large cigar; Assistant Branch Chief Mark Kelly, bow tie and large cup of coffee; Paul Yaggy, third row, second from right.

airplane. Other studies dealt with supersonic propeller stall flutter for various thrust conditions. NASA and the Army became increasingly interested in vertical/short takeoff and landing (V/STOL) aircraft in the 1960s, and Paul was involved in numerous investigations of the aerodynamics and performance of ducted propellers of such aircraft. In particular, this included the U.S. Army-sponsored Doak VZ-4DA tilting ducted fan V/STOL aircraft with two 4-foot-diameter ducted fans mounted on the wingtips. Paul was leading a group of investigators at this time, and Ken Mort describes these tests in his chapter. Subsequent testing involved a full-scale 7-foot-diameter ducted propeller that was used for the Department of Defense (DoD) Tri-Service Bell X-22A V/STOL research aircraft. The group picture taken from Ken's chapter shows Paul with members of the branch in 1958.

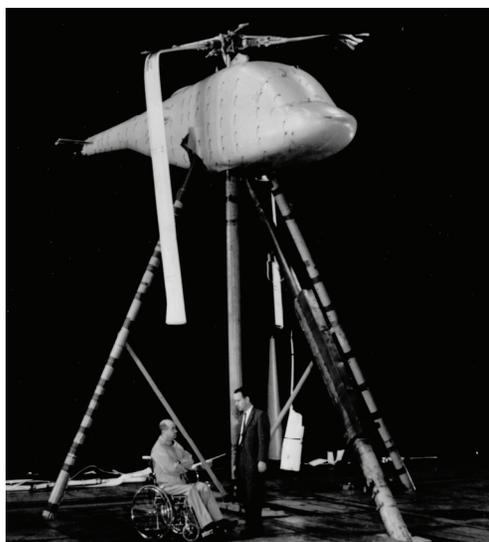
When interest in space exploration and the Apollo moon program began in the late 1950s and the NACA became the nucleus for NASA in 1958, one of the new research priorities for the 40- by 80-Foot Wind Tunnel Branch involved spacecraft recovery. Paul's group became heavily involved in a wide range of related activities. These included testing of recovery parachutes and experiments with inflatable paragliders. Again, Ken Mort's account describes some of the trials and tribulations of this testing. Paul also became involved with the lifting body reentry vehicle research that eventually helped prove that accurate horizontal landings were feasible for unpowered reentry vehicles like the Space Shuttle.

The pictures on the next page show the M2-F1 lifting body reentry vehicle installed in the Ames 40- by 80-Foot Wind Tunnel and being readied for flight testing.



Left: the M2-F1 lifting body in the 40- by 80-Foot Wind Tunnel. Right: Paul Yaggy with NASA research pilot Milton Thompson.

The 40- by 80-Foot Wind Tunnel Branch was also active in helicopter and advanced rotorcraft testing, including compound helicopters and tiltrotors. This was not Paul's primary research focus, but his group was involved in this area and John L. McCloud III was one of the original NACA/NASA rotary-wing specialists at Ames. One of these experiments demonstrated that testing in the 40- by 80-Foot Wind Tunnel, typical of experimental research, was not always routine. A memorable example was an experimental Lockheed hingeless rotor that failed during a 1962 test. The picture shows Paul Yaggy and John McCloud contemplating the aftermath of the accident. Some observers have offered a humorous interpretation of their supposed conversation following the test failure: John, seated in his wheelchair, and who subsequently grew a beard, saying, "Well, I guess I'll have to grow beard so people won't recognize me after this fiasco." And Paul adding, "Well, I'll probably have to leave NASA; maybe I should apply for a job at that new Army laboratory I've heard about."



Paul Yaggy and John McCloud (in wheelchair) contemplating a rotor test failure in the 40- by 80-Foot Wind Tunnel.

Army Aeronautical Research Laboratory

In 1965 the Army Aeronautical Activity, later the Army Aeronautical Research Laboratory (AARL), was organized at Ames under the original Army-NASA Joint Agreement as described in the Introduction. Paul Yaggy was selected from among many candidates at NASA Ames to be its Technical Director. He was sworn in by the Director, Colonel Cyril D. Stapleton, with NASA Ames Director, Smith J. DeFrance, Russell G. Robinson, Ames Assistant Director for Aeronautics and Flight Mechanics, and Gil Morehouse, Executive Officer in attendance.



Colonel Stapleton swearing in Technical Director Paul Yaggy. From left, Colonel Stapleton; Russell G. Robinson; Smith J. DeFrance, Ames Director; Paul Yaggy; and Gilbert Morehouse.

This was the beginning of a first-of-its-kind, unique experiment in interagency collaboration that, despite its limited scope, ultimately grew to become a major force in rotary-wing research and development. Paul Yaggy was a critical element in the rapid, early success of AARL. Despite his NACA/NASA background in the 40- by 80-Foot Wind Tunnel Branch, Paul was not a leading theoretician, analyst, or designer, but he had broad experience in aeronautical technology and a very high respect for leading researchers. He was a quiet but persistent leader, with strong convictions and a vision that he pursued diligently—his energy was contagious, and he created excitement that stimulated innovation and discovery. Paul was a natural people manager,

genuinely interested in young researchers, and he provided sincere encouragement and support. All these qualities made him an excellent choice as Technical Director of a new and unformed organization, and his impact was soon felt. Three years later, when Colonel Stapleton retired, Paul was named Director of AARL.

In addition to his strong NACA-NASA background, Paul's success with AARL can be attributed to several factors.

Paul's research philosophy was a natural outgrowth of the NACA culture and traditions established as it became arguably the most successful aeronautical research organization in the world. Paul was able to create a first-class R&D organization by acquiring and cultivating a team of capable researchers. Paul was a good judge of technical expertise and he had extensive contacts; he scoured universities and industry to find good people and then provided them with a supportive and challenging environment in which they could learn, grow, and thrive. He recognized success and rewarded young new hires.

Paul believed that “the researcher at the bench knows best,” meaning that the researcher is the expert and often a better judge of research opportunities than his boss. Consequently, researchers were given broad responsibilities, and encouraged to develop research plans and explore opportunities. At the same time, Paul knew the mission was important and that researchers needed to be relevant. He wasn't one to tolerate endless dabbling in the “proverbial research sandbox.”

Paul knew how to deal with Army and NASA organizations and managers at all levels. These included the Army Aviation Systems Command (AVSCOM) in St. Louis, Army Materiel Command (AMC) in Washington, and the Army Aviation Materiel Laboratories (AVLABS) in Ft. Eustis, along with NASA's Ames Research Center, Langley Research Center, and Headquarters (HQ) in Washington. Paul's natural instinct for cooperation greatly benefitted the Army-NASA collaboration. He was good at dealing with bureaucratic challenges and adept at balancing the sometimes conflicting interests of the Army and NASA. Paul was very effective at representing either of the organizations when they were dealing with one another. He could also leverage one organization and play it against the other when the situation required. Under his leadership the Army-NASA collaboration proved highly beneficial to both agencies.

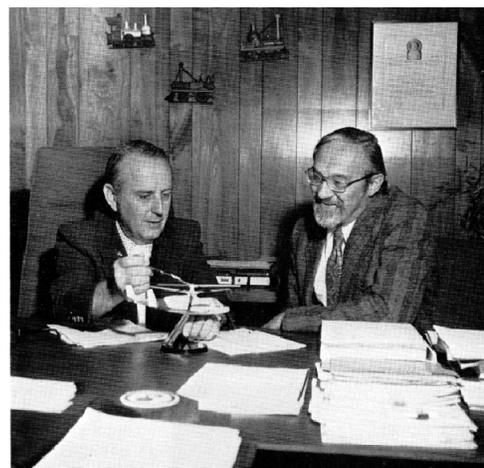


Paul receiving a commendation from Colonel Stapleton.

Paul sought to create an environment of excellence for an organization built with NACA veterans, industry retirees, and inexperienced university graduates, many of whom arrived with limited knowledge of rotorcraft. He brought in rotary-wing technical leaders, world-class researchers, and consultants to extend the reach of AARL by collaborating, consulting, and mentoring its staff; many spent summers at Ames. These “luminaries” augmented the NASA Ames experts as Paul created his team of researchers, influencing them to set the bar high, to strive for excellence, and to become “world class.”

Paul's luminaries from the relatively small field of rotorcraft R&D provided technical advice and sound guidance as he charted a path for AARL's early technical program. Henry (Hank) Velkoff, an Ohio State professor and an early rotary-wing engineer at Wright Field, later Wright-Patterson Air Force Base, was personally acquainted with the leading autogyro and helicopter pioneers and designers throughout the U.S. and European industries. He was a walking encyclopedia of aviation knowledge, an original thinker, and a tireless proponent of vertical flight.

Enoch Durbin, a Princeton University professor and former NACA Langley researcher, helped to educate and then collaborated with several of the Princeton graduates who joined AARL in the early days. He had political connections and served on Army advisory boards, including AMC's influential Carlson Committee. Enoch Durbin is remembered by several chapter authors. Professor Barney McCormick, another academic from Penn State, and a leading authority on V/STOL aerodynamics and helicopters, worked at AARL during several summers. “Steppy” Stepniewski, a Boeing Vertol researcher and strong AARL supporter, was a rotorcraft and vertical takeoff and landing (VTOL) pioneer with a European background who collaborated with AARL researchers.



Paul conferring with rotary-wing pioneer, Hank Velkoff.

Kurt Hohenemser was a famous German pioneer who studied under Prandtl and worked as a principal designer for Anton Flettner. Aside from wide-ranging interests in quantum physics, nuclear disarmament, and wind energy, Hohenemser was one of the greatest scientist-engineers in the field of rotorcraft. After the war, he worked at McDonnell Aircraft and was a key designer of several helicopters including the revolutionary XV-1 compound helicopter of the 1950s. He taught at a Washington University, and many of his graduate students went on to hold influential positions in industry, academia, and government, including Sam Crews, Dev Banerjee, and Dave Peters. AARL supported his rotorcraft research, and he served as a key consultant for the Army during the Lockheed Cheyenne Program.

Another of Paul's luminaries was Gerhard Sissingh, a famous German theoretician and rotorcraft pioneer who emigrated to the U.S. after WWII. Dr. Sissingh worked for Hiller and Lockheed near the end of his career and also consulted for AARL. Famed airfoil designers Richard Eppler and F. X. Wortmann were also associated with AARL through Paul Yaggy.

U.S. Army Air Mobility R&D Laboratory

Paul Yaggy's seminal role in the growth and influence of AARL and the Army-NASA collaboration at Ames under the original Army-NASA Joint Agreement led to even greater contributions to expand the Army and NASA cooperation into the Air Mobility Research and Development Laboratory (AMRDL) in 1970. As described in the Introduction, with the advent of the Air Mobility doctrine of the 1960s, the Army needed an Air Mobility Center to support its aviation research and development interests. The success of the Ames experiment under Paul's leadership convinced the Army that it was not necessary to build a new aviation research center from scratch, as originally planned, and that large savings could be realized by expanding the NASA partnership.

In 1969, Paul actively encouraged the Army to expand the seminal experiment in inter-agency collaboration at Ames to include other NASA aeronautical research centers. Paul helped negotiate an agreement between the Army and NASA to establish similar organizations at NASA Langley and Glenn (then Lewis) Research Centers. As noted earlier, these were combined with the Army's existing AVLABS at Ft. Eustis, Virginia, to make up the four Directorates of a new R&D organization. Thus, the AARL that Paul evolved became the foundation on which AMRDL was built. Paul succeeded in obtaining approval for the organizational structure and intergovernmental agency relationship from the Commanding Generals of the Army Materiel Command and the Army Aviation Systems Command, and from the authorities at NASA. The new organization was established on July 15, 1970. The Army wanted the AMRDL HQ to be in or near the Army Aviation Systems Command in St. Louis, Missouri. Paul refused to move. The Army backed down and agreed to locate the AMRDL HQ at Ames if Paul would agree to be its Director.

Paul guided the development of the organizational concept and the delegation of missions and functions for each of the Directorates. The implementation of AMRDL entailed not only the assignment of responsibilities among the four Directorates, but also the commensurate distribution of the limited personnel and funds, which caused a problem with the organization at Ft. Eustis. Prior to the establishment of AMRDL, all the Army Aviation R&D work and budget went to AVLABS at Ft. Eustis (except for the tiny upstart at Ames). In the AMRDL, most of the basic research (6.1 funding) and the exploratory research (6.2 funding) would be performed by

the three Directorates co-located with NASA. Activities, funding, and personnel positions would move from Ft. Eustis to the other three Directorates. The folks at Ft. Eustis were not happy campers. Key people in management at Ft. Eustis predicted disaster and had developed close ties in the Department of the Army (DA) with those who supported their views. Fortunately, Paul, too, had influential patrons in the DA (e.g., Dick Ballard) and the Army Materiel Command (e.g., Norm Klein and John Beebe). With a good deal of diplomacy and strategic influence, Paul succeeded in overcoming the naysayers and distributing the unhappiness evenly across all four Directorates. Paul was ready to submit for approval the documentation that would establish the AMRDL.

But Paul had used up all his available time. He had committed to delivering the documents before the end of the year and it was Christmas 1970. Paul took a “red-eye” to Washington on Christmas eve to make the deliveries to the DA and to the Army Materiel Command knowing very well no one was going to look at them until after New Year’s Day.

Paul Yaggy deserves great credit for using his superb technical knowledge and leadership skills to persuade the NASA and the Army leadership to adopt the expansion proposal. Paul also served as the AVSCOM Director of Research, Development and Engineering from 1972 to 1974.

Within AMRDL, Paul organized an accomplished team of experienced managers with strong backgrounds in rotorcraft RDT&E from industry, government, and academia. A picture (above,



Paul and his AMRDL management team, from left: Richard M. Carlson, Frederick H. Immen, Paul F. Yaggy, John B. Wheatley, and Colonel Norman L. Robinson.

right) from the early 1970s shows some members of the AMRDL HQ management team at Ames Research Center. Shown along with Paul Yaggy in the picture are Richard M. Carlson, Chief of the Advanced Systems Research Office (ASRO), formerly Chief of Rotary Wing Technology for Lockheed California Company, Frederick H. Immen, ASRO Structures Lead, from Boeing Vertol, and John B. Wheatley, ASRO Propulsion Lead, and pioneer rotary-wing researcher with NACA Langley Research Center. Deputy Director Colonel Norman L. Robinson is also shown.

As with AARL, Paul was unfailingly generous in recognizing the accomplishments and contributions of his researchers and support staff. The picture at the top of the next page shows Paul recognizing members of the AMRDL HQ staff.

With vision and ingenuity, Paul led the Laboratory in its collaboration with NASA to an unmatched record of world-class rotary-wing R&D. At its peak, Paul Yaggy vectored over \$100M/year and nearly 500 Army scientists and engineers in this collaboration with NASA that



Paul recognizing the accomplishments of the AMRDL HQ staff.

brought similar numbers of personnel, research funding, and importantly unique facilities such as the National Full-scale Aerodynamic Complex (NFAC), the Vertical Motion Simulator (VMS), and variable stability research aircraft to bear on rotorcraft R&D.

This close association between the Army and NASA produced multiple benefits for both agencies. It provided the Army with direct access to NASA's world-class research facilities and expertise, and their application to Army requirements, while conserving the resources of both agencies in the performance of research of common interest to both. It increased contributions to national security and led to a better understanding of the problems, and their possible solutions, faced by the military and civilian use of rotary-wing aircraft.

As events developed, Paul's achievements at Ames had a significant impact on the future of NASA Ames Research Center. Locating the AMRDL HQ and Ames Directorate at Ames was, in fact, a new beginning for NASA Ames role in rotary-wing R&D. In 1977, NASA decided to consolidate its rotorcraft research at a single center. At that time, NASA Langley Research Center had the strongest program in rotorcraft research. Nevertheless, NASA chose to relocate all of Langley's rotorcraft flight research assets to Ames because of (according to NASA's advisory panel) the exceptional relationship that existed between NASA and the Army at Ames, the particularly strong research cadre the Army had established at the Ames Directorate, and the Army's decision to locate its AMRDL HQ at Ames. Paul Yaggy's impact thus contributed to NASA Ames becoming an international leader in rotorcraft R&D.

Selected Highlights

Paul had a natural inclination for cooperative activity, including a broad international outlook going back to his NASA days and his earliest activities at AARL. He was instrumental in these early international collaborations with René Dorand and Marcel Kretz of Giravions Dorand



Early meeting at AMRDL of U.S./French Memorandum of Understanding for a cooperative research project on helicopter dynamics. Front row, from left: Marcel Kretz, Roland Dat, Captain Jean-Pierre Mermillod, and Jean-Jacques Philippe. Back row, from left: Dr. Robert Ormiston, Paul Yaggy, Dr. Irving Statler, and Andrew Morse.

personnel to apply their well-known capabilities in VSTOL aircraft to study the handling-qualities requirements of rotary-wing aircraft. These studies produced a NASA Ames-led international activity entailing ground-based simulations, in-flight simulators, wind tunnel tests, and advanced system identification methods development by France, Germany, Canada, and Israel, and at Ames. The result was the first rotorcraft-specific handling-qualities standard, Aeronautical Design Standard-33 (ADS-33), which has since been endorsed internationally.

The Army-NASA Bell XV-15 Tilt Rotor Research Aircraft, described by several authors throughout this book, was one of the best-known achievements of the Army-NASA collaboration at Ames. NASA Ames had been an early supporter of the joint Air Force–Army Convertible Aircraft program that produced the pioneering McDonnell XV-1 compound helicopter and the Bell XV-3 tilt rotor aircraft. Along with Woody Cook and other NASA Ames proponents, Paul was influential in promoting the tiltrotor research that led to the XV-15 program. He succeeded in obtaining shared funding from the Army and NASA for the Convertible Aircraft program to support Bell Helicopter’s follow-up of their initial work with the XV-3. The XV-15 demonstrated to the world that tiltrotor technology had arrived when it



Paul with Army and NASA leaders examining a tiltrotor model. From left: Major General Frank Hinrichs; Hans Mark, Director NASA Ames Research Center; General Henry Miley; and Paul Yaggy.

became the “star” of the 1981 Paris Air Show. The knowledge gained in subsequent years of flight-testing the XV-15 at Ames and Bell was directly applied to the Marines and Air Force V-22 Osprey tiltrotor aircraft—the first operational aircraft of its kind in the world. The XV-15 development has been acknowledged as one of the most successful of all experimental aircraft programs conducted by NASA.

Legacy

It is difficult to capture Paul’s true essence in the brief glimpse contained in this chapter. The many honors and awards testify to the esteem in which the technical community and his peers held him. In her part of this chapter, Kathy (Yaggy) Hemingway describes several of the recognitions that she remembers as having special meaning for Paul. We will note here two that are of special significance. First, the American Helicopter Society International, Inc. (AHS), the professional technical society of vertical flight, presented Paul with its highest honor, the Dr. Alexander Klemin Award in 1973, “for notable achievement in the advancement of rotary-wing aeronautics.” The citation accompanying the honor reads: “In recognition of his outstanding contribution to helicopter technology and his leadership of the U.S. Army Air Mobility Research and Development Laboratory.” For Paul, this must have been the most fitting way to recognize his rise from a junior wind tunnel engineer to an innovator who helped build the premier Army R&D organization that he ultimately ascended to lead. As a measure of the success of that organization, eight more researchers associated with the Ames-Army Lab would become Klemin Award recipients in subsequent years.

In recognition of Paul’s unique role in broadening the reach of NASA Ames Research Center to embrace a groundbreaking cooperative relationship with the U.S. Army and Ames, the authors of this chapter nominated Paul for the Ames Hall of Fame. In 2009, with a flood of endorsements from the entire rotorcraft community, Paul Yaggy was inducted into the NASA Ames Hall of Fame.

Paul Yaggy joined NACA in 1944 and became the AARL Director in 1968. When he retired in 1974 after 31 years of government service, he left a remarkable record of accomplishment, particularly his impact on Army aviation RDT&E during a remarkably brief 10-year period of time. Few people have had such a positive and sustained influence on worldwide rotorcraft research and development. These accomplishments would not have happened without Paul Yaggy’s ingenuity and skillful leadership of the unique and pioneering Army-NASA Joint Agreement that enabled a collaboration of unprecedented effectiveness to benefit the Army, NASA, and the county. Paul was the right man, in the right place, at the right time—and he took the collaboration to heights of accomplishment that could not have been imagined by its originators. He truly was the ingenious architect.

The imprint of Paul’s legacy is manifest in the Army and NASA at Ames to the present day. In particular, his legacy is alive and well among the authors of this memoir who were fortunate enough to work with, and for, Paul. We experienced firsthand the environment of innovation and discovery he created and that gave us so many wonderful memories. In Part 1 of this chapter, we have written about Paul from the perspective of two people who had the pleasure of working with him in his stimulating environment. In Part 2 of this chapter, we are fortunate that Paul’s daughter, Kathy (Yaggy) Hemingway, has contributed her personal perspective, one that only a daughter can share, on what she calls Paul’s *heart and soul*.

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Personal Memories of Paul Yaggy, 1965–1974

Kathy (Yaggy) Hemingway

My name is Kathy Hemingway and I am Paul Yaggy's third daughter. Dr. Irv Statler and Dr. Bob Ormiston asked me if I would be willing to write about my father for this memoir project. My father has passed away, and unfortunately he never wrote his memoirs, so this request was very challenging. Since I had the privilege of working at the Aeromechanics Laboratory of the U.S. Army Research and Technology Laboratories at Ames from late 1979 to early 1982, and was a bit familiar with the environment, I agreed to give it a try. Dr. Statler and Dr. Ormiston have written about my father's technical and leadership accomplishments in Part 1 of this chapter. In Part 2, I will attempt to capture the essence of my father in his role as the trailblazer of the environment of success under the Army-NASA collaboration. I believe his personality and leadership abilities were his true contribution to the endeavor; he was the right man at the right time. It is my hope that together, both parts of this chapter will help the reader understand the importance of my father's contribution to the Army-NASA Joint Agreement and how the interagency collaboration was able to achieve its remarkable influence on the progress of international rotorcraft R&D.



Paul Yaggy, U.S. Navy.

After completing high school, Paul enrolled in Taylor University in Indiana. However, WWII offered him the opportunity to enroll in the Navy's V-12 program, allowing him to further his education at the University of Notre Dame as an electrical and aeronautical engineering student before being fully commissioned and sent to active duty in Mountain View, California, at the NACA Ames Aeronautical Laboratory. His first assignment was working in the 16-Foot Wind Tunnel.

With the outbreak of the Korean War, he was recalled to active duty and, after a stint in Hawaii with the Navy, he was released to inactive duty. Paul returned to Ames and took a job at the 40- by 80-Foot Wind Tunnel where he was later promoted to Assistant Branch Chief. He also attended San Jose State College where he finally completed his B.S.E.E. degree in 1963. He worked for NASA for 19 years, gaining national recognition for research in vertical/short takeoff and landing (V/STOL) aircraft rotors, propellers, and ducted fans, as well as recovery systems for spacecraft and lifting body reentry vehicles.

In 1965, Paul was asked to take the position of Technical Director at the newly formed U.S. Army Aeronautical Activity at Ames (renamed the Aeronautical Research Laboratory (AARL) in 1967 under the Army-NASA Joint Agreement). I remember at dinner one night he was telling us about the new position and that he was going to accept it, especially since Woody Cook at the 40- by 80-Foot Wind Tunnel Branch had told him that "he didn't have a chance for further promotion there." He had his little "impish" smile when he said that, so I was pretty sure that was Woody's way of telling him to "go for it." His leadership abilities were always recognized wherever he went, and he was about to embark on what my mother called "a new adventure in his life."



Paul Yaggy and the 40- by 80-Foot Wind Tunnel Branch staff at his “Moving On” party in 1965.

Paul served as Technical Director of this new venture at Ames from 1965 to 1968. Working closely with Army Commander Colonel Cyril Stapleton, he began to build a world-class organization. In 1968, with the departure of Colonel Stapleton, Paul was promoted to the position of Director. This was an exciting challenge for him. Because of the nature of the Army-NASA Joint Agreement, he utilized all of the facilities and personnel at his disposal and began to hire some of the finest researchers in the area of aeronautical R&D. He truly enjoyed recruiting and hiring personnel who he felt would enhance the lab’s reputation while advancing their own careers as well, and he enjoyed the challenge of creating something from the pieces and parts that surrounded him. He worked hard to help create the environment that would make the mission succeed.

In 1967, Paul was appointed a member of the Fluid Dynamics Panel for NATO’s Advisory Group for Aerospace Research and Development (AGARD). This and other existing activities at the lab opened the doors for international travel and friendships, and an even broader scope of knowledge. These opportunities helped to shape his perspective for his leadership at the lab and for the responsibilities he would eventually assume after his government service.

During this time he also attempted to further his own education. I remember him trying to complete his Master’s degree at Stanford University but finding it impossible along with the responsibilities of his family and his consuming workload. The result was that Paul was in the position of leadership of a world-class research lab without having any advanced degrees behind his name. This was a unique time when knowledge and performance stood out above all else. I don’t believe this could happen in today’s environment. However, this was one of the things that made Paul such a successful leader; he recognized the abilities and sacrifices of those who had achieved academic success, adding to the respect and humility he felt toward them.

In late 1970, along with Paul’s active encouragement, the Army implemented the plan for the U.S. Army Air Mobility Research and Development Laboratory (AMRDL). This was a brand new concept consisting of the AARL, the contract-type group at Ft. Eustis, and the new labs at NASA Lewis and NASA Langley Research Centers, encompassing around 500 personnel. Paul was offered the position of Director of the AMRDL—an opportunity of a lifetime for him.

I remember the night he came home to tell all of us about the new opportunity. He hadn’t accepted it yet. At that time, it meant relocation to St. Louis since that would be where headquarters would be established. He needed to pray about it, and he also wanted all our inputs because it would affect us as well. As you can imagine, four California teenagers had no interest in moving to the Midwest and my mother hated the cold weather, so the consensus in the house was “hope you like living alone in the snow.” In addition to the mutiny in the house, his aging parents lived nearby and he felt an obligation to them too. After his family’s less than

sympathetic input and much prayer, he decided to turn the job down. Shortly thereafter he came home with the news that they were moving headquarters to Ames instead, and he was asked to reconsider and take the position. Was the change in location made to accommodate him? I believe so, but you can come to your own conclusion. The rest is history.

In 1970, Paul started what he described as “the greatest challenge of his career.” Paul needed to increase his staff to meet the demands of the growing organization, so he had the privilege of hiring even more world-class talent. During this time, the lab became recognized as the leader in cutting-edge aeronautical research.

Paul was deeply dedicated to the lab and to his staff, but at the same time he was able to keep his life in balance. As requested by my mother, when he wasn't travelling, he was home for dinner at 6 p.m. every night. And weekends were for family and church activities. The Lord blessed him with a wonderful and supportive wife who walked the journey with him, and the Lord also blessed him with the ability to lead the lab and be there for his family as well.

I remember the time my mother was ill, so I went to the airport to pick him up from a trip. When he walked off the plane, he was yellow. When I got him home, my mother took one look at him and off to the hospital we went. He had a good case of hepatitis, which he believed he had gotten from eating bad oysters in Texas. After a week in the hospital, he was confined to bed rest at home for another 5 weeks. So bed rest it was, but as far as he was concerned, it was not work rest. Work just seemed to have a way of showing up at our house. My mother stepped in, and since I was the designated nurse at the time, she informed me that no more people or work were to enter through the front door; after all, he was sick and still contagious. Unfortunately, Irv Statler was the deliverer that fateful day. I remember the shocked look on his face when I, following strict orders, not only refused to let him in but refused to take the package he held in his hand. Somehow, I was forgiven, but then Paul knew that when it came to health issues, no one out-ranked Nurse Dorothy Yaggy, not even the U.S. Government. He eventually recovered and returned to the office, but he never ate oysters again.

With all his knowledge and abilities, I believe Paul's greatest contribution was his genuine respect and joy working with the people around him including industry, the academic and scientific communities, Army project managers, the Pentagon, and Air Force and Navy personnel. He enjoyed watching those under his leadership, and those working in cooperation with his organization, grow and develop into nationally and internationally known scientists and researchers and leaders. He loved to teach others and learn from others, and in so doing, he developed many friendships that lasted beyond his years at Ames.



Dorothy Yaggy receiving her Recognition of Support Award from Major General Frank Hinrichs at Paul Yaggy's retirement dinner.

In 1969, I had the opportunity to accompany him on one of his many European trips. I met several of his European contacts. I got to see not only what he did in the technical world but also the many friends he had made. I've taken the names of some of them from my travel journal. We were picked up in Frankfurt by a Mr. Wornecke and warmly welcomed. From there we travelled on to Stuttgart where we spent 2 delightful days with Professor Richard Eppler and his family. We were entertained in his home and later toured the Aerodynamik Schule. The two of them talked as if they had known each other all their lives. Professor Eppler presented a book to us about Schwabish Hall, his birthplace, which we promised to visit and did.

We attended an AGARD meeting in Basel, Switzerland (yes, I got to go to the meeting too) and visited the Office National d'Etudes et de Recherches Aérospatiales (ONERA) wind tunnel facilities in Modane, France. We enjoyed a delightful time at the AGARD banquet with other gentlemen by the names of Dr. Hermann Kurzweg, Chief Scientist of the National Aeronautics and Space Administration, Mr. Douglas, and Mr. Schneider. We dined with Dr. Kurzweg on several occasions and managed to laugh through almost every meal.

The Paris Air Show was also on the itinerary and we attended with Mr. Marcel Kretz of Giravions Dorand, being invited to the company's viewing booth and having the privilege of seeing the Boeing 747 and the Concorde for the first time. The excitement shared by this international group was contagious. Paul and Marcel had a wonderful friendship. And as Paul's daughter, Marcel and his wife, Maddie, welcomed me with open arms. One of the highlights of our visit was a very special dinner cruise on the River Seine.

Our last stop was in London where I met Mr. Marshall and his colleagues, and I learned to do two things that my father had already mastered, eat very slowly and enjoy escargot.

In addition to the technical knowledge Paul shared with these people, his friendship with them was warm, genuine, and personal. I watched a marvelous mutual respect develop between them. And I myself was welcomed into their lives and visited them during some of my later travels. They were also welcomed into our home when they visited the United States. Paul kept in contact with them as well during his travels after his retirement from the Army. This was also true of friends he made in the United States.



Kathy Hemingway (Yaggy), Paul Yaggy, and Maddie Kretz waiting for the dinner boat.



On the River Seine dinner boat with the Kretz family.

Paul enjoyed sharing not only his technical interests, but also model railroading, world affairs, family, history, architecture, good food, places of interest, and his deep faith and relationship with Jesus. He had an ease with people, and they found themselves at ease with him. Perhaps the example of his relationship with Dr. Stepniewski (Steppy as Paul knew him) portrays this. According to my dad, and what I was able to experience firsthand, he and Steppy shared not only conversation about technology but several hours exploring philosophy, ethics, morality, religion, and many other things. This wonderful relationship continued on beyond Paul's retirement.

Paul's ability to get to know and relate to his colleagues enhanced his ability to work with them. He simply had a "knack" for getting to know people. Sometimes this "knack" was expressed in covert ways. When he interviewed Lorraine Shaw for the job as his administrative assistant, he offered her a cigarette (back when smoking in the office was permitted). Now, he never smoked, but wanted to make sure that she didn't either. He could be a bit sneaky at times.

Paul loved to laugh with people and always enjoyed a good joke. At the AGARD banquet, I remember our little group gained the attention of a dining hall full of hundreds of people as we got a bit rowdy playing pranks on each other. At a restaurant with Dr. Hermann Kurzweg, we were all belly laughing at my and Dad's mistake. While on our drive to Switzerland we were eating animal crackers and trying to learn their German names, but we got the name of the hippopotamus a bit wrong. We called it a pferdwasser (horse water) rather than a flusspferd (river horse), much to Hermann's delight.



Irv Statler, Paul Yaggy, and Dorothy Yaggy enjoying a laugh together.

However, not all of Paul's ways were endearing to those around him. One in particular was almost the undoing of Irv Statler. Paul had a little red VW Bug, his true and reliable friend. How Paul loved that car and the portable Blaupunkt radio in the front seat (I guess everyone needs some type of a status symbol). But, Irv came to dread that car. Paul had a habit of cramming as much work as possible into as little time as possible, especially before leaving for the airport. And many times Irv rode with him. If you know Irv, you know that he is always early for everything, so you can imagine his frustration. I know Irv wished that little car would leave earlier, run faster, or just go away, but what he never knew was that five of us in the household owned VW Bugs. Our home looked like a used VW car lot. Though I know Irv wished that car would die, there were four more waiting in the wings to be used just in case.

Paul had a propensity to pack in as much as he could, be it work or play, right up until it was time to leave. During my previously mentioned trip to Europe he did just that. We were to leave Savona, Italy, drive through Monaco, and then catch the auto/railroad to take us over the mountains to Modane, France. Well, you guessed it. We missed the train, and then we had to drive over the snow-packed, narrow, windy mountain road in the pouring rain, in the dark, and yes, in our rented VW Bug. We didn't arrive in Modane until 2 a.m., but we did get in safe and sound. And I had to laugh when I read through my mother's journals she kept through the years. How many of them read, "Paul was late in getting home, he missed his flight again!"

I have shared with you a little bit of the heart and soul of Paul Yaggy, but I would be remiss if I didn't describe the abilities that enabled him to succeed in creating and leading AARL and AMRDL. In order to do this, I talked with several people who worked under his leadership in varying capacities, both inside and outside of the technical field. The following is what they had to say:

First and foremost, Paul was a man of prayer. He regularly prayed for his staff and their families, as well as for direction for his decisions regarding his staff and the organization.

Paul was a visionary, one who had the "big picture" and could readily see the future and how to reach it. He had the ability to make his vision clear to those around him. He recognized that change was difficult, so he applied a systematic approach to implement complex structures and concepts.

Paul led by the influence of his wisdom and humility, not by the authority of his position. He instinctively understood interpersonal relations, and he fostered friendship and support under the umbrella of accountability. He often used the metaphor of "three people in the boat," (those in the boat rowing with you, those with their oars in the air, and those rowing against you), and the importance of influencing all three at once to eventually row with you.

Paul had a belief in, and an infectious passion for, what he was doing. His work ethic set the example for those around him. He had an exceptional capacity to see potential in people and to utilize their roles and strengths to better both the organization and the individual. He was able to provide a context in which people could effectively work and carry out their tasks and responsibilities.

Many described Paul as having a tolerance for chaos. Lorraine Shaw, Paul's administrative assistant and a vital part of his smooth-running office, admired his ability to deal with this, especially with such grace and gentleness. However, she did tell of the one time that she saw him get angry. It seems that after a decidedly "lively" discussion in his office, and after everyone had left, she observed him kick the leg of the table. I personally believe that kind of learned restraint came from raising four daughters!

I can remember sitting down with him for a father/daughter conversation when I was beginning my leadership role and responsibilities as an engineer/manager in the Space Division at Lockheed Missiles and Space. He shared with me the three most important leadership lessons he learned while working within the Ames Army-NASA research environment:

- Leadership is a trust and requires proper stewardship of the resources, money, facilities, and equipment, but primarily the people, entrusted to you. A leader is in place to serve, not use. He told me he needed to learn to serve, not to suppress initiative, and to be careful not to create dissension or resentment or demean others. He needed to learn to develop to the maximum the potential represented in those entrusted to his leadership. He needed to learn



Irv Statler presenting Paul Yaggy with the "Flying VW Bug" at his retirement party.

to build a team committed to each other and to the goals that they jointly held. In addition, he learned to build up, and then celebrate with, people who moved on to other or higher positions and responsibilities.

- Making the right choices scientifically, globally, and in the area of human nature requires cooperation. He needed to learn that “in the mouths of many counselors is wisdom.” And of course, he needed to draw close and look to the Lord for direction.
- Don’t believe the saying that “it’s lonely at the top.” It doesn’t have to be, nor should it be. Learn to rely on respect vital to the exercise of authority rather than position. He learned to share relationships and to become vulnerable to them. He learned to exercise mercy and peacemaking, and he strove to maintain a pure heart.

Paul received several awards during his service with the Ames-Army Lab, but there were a few that were very special to him which I will mention below:

- Paul was recognized for his service on the AGARD Fluid Dynamics Panel and for being deeply involved in the international aerospace/aeronautical research and development community. He was excited to be part of the international technical community and valued the opportunities he had to develop friendships around the world. He loved learning about other cultures and sharing his own. He genuinely enjoyed the broadening experience this allotted him. He was also proud to have served his country in a positive and lasting way.



Advisory Group for Aerospace Research and Development (AGARD) recognition certificate presented to Paul Yaggy.



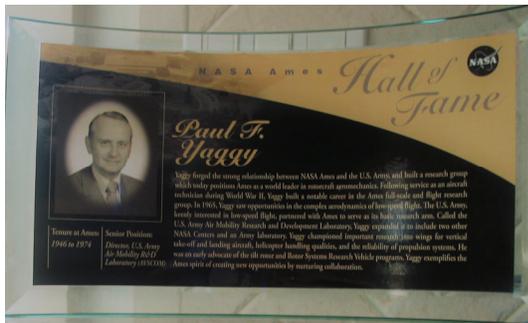
Paul Yaggy with the Palo Alto Charter Chapter of the ABWA Boss of the Year Award.

- He was honored to be asked to appear before the U.S. Senate committee, and he was proud of the congressional letters recognizing his three presentations on work being performed at the Ames-Army Lab. Paul was very patriotic; he loved his country and found it a privilege to serve in a meaningful capacity.
- Paul was surprised to learn that he received the Boss of The Year Award from the Palo Alto Charter Chapter of the American Business Women’s Association (ABWA). He treated all people with respect and encouraged everyone to work to their greatest potential. The women in his organization were well aware of this and nominated him to show their appreciation. This award reminded him of the value of all of his employees.

- One of Paul's favorite awards was the American Helicopter Society's Dr. Alexander Klemin Award for "notable achievement in the advancement of rotary-wing aeronautics" that signified the highest recognition of an individual by his peers in the rotorcraft technical community. He told me he was amazed at what the Lord had accomplished through him in his technical career and how the Lord had blessed him and the lab through it all. This award constantly reminded him of the Lord's faithfulness and the joy he had in his work.
- And though the final award was presented long after his retirement, Paul was inducted into the NASA Ames Hall of Fame in February 2009. This award not only recognized his abilities but also the growth and development of the research group that today positions Ames as a world leader in rotorcraft research and development. Because Paul was advanced in years, he truly cherished this award and appreciated being remembered years after leaving government service.



Paul Yaggy receiving the Dr. Alexander Klemin Award.



Paul Yaggy's NASA Ames Hall of Fame Award.

was all right. She said he had been up all night wrestling with his decision to move on from government service, and that he felt it was the right time and God's leading. It was an intensely difficult decision for him, but he felt that he had properly positioned the organization so that those in place would be able to successfully carry on the vision and the work, and take it to greater heights. In the summer of 1974 he submitted his resignation, retiring officially in September of that year.

Paul answered another calling and moved on to his service with Calvary Church and eventually One Challenge International (OC) where he once again applied the leadership skills he had learned and honed during his wonderful years at Ames. He always held the fondest memories of his relationships, friendships, and experiences at Ames.

Although Paul was offered other jobs in Washington, at the Army Material Command and in the Pentagon, he turned them down. He felt that his true value and interest was in research, so he chose to remain at Ames. However, the day came in 1974 when he felt it was time to move on. I remember one night being unable to sleep and feeling like I needed to be in prayer for my dad almost the entire night. Early the next morning I called my mother and asked her if dad



Major General Frank Hinrichs presenting the Army Civilian Service Award for Extremely Meritorious Service to Paul Yaggy at his retirement dinner.

My father's greatest desire was, at the time of his death, to meet the Lord and have him say to him, "Well done my good and faithful servant." I believe beyond a shadow of a doubt that was what he heard.

Thank you for letting me share a bit of my father, Paul Yaggy, with you. I hope this adds some insight into what made the amazing Army-NASA collaboration work.



Colleagues, but even more, friends, at the NASA Ames Hall of Fame Awards. From left: Jim McCroskey, Frank Caradonna, Irv Statler, Paul Yaggy, Fred Schmitz, and Robert Ormiston.

An Aeronautical Odyssey

Edwin W. Aiken
Army/NASA

The Early Years

My first memories of aviation were supplied by my father, a career Marine Corps officer. As a Marine, he was intent on becoming an aviator, but his eyesight failed him. However, he never lost his interest in aircraft and aviation. I can remember frequent visits to the airport just to watch the aircraft take off and land. He actually arranged a personal tour of a TWA Lockheed Constellation for my brother and me when it was introduced at Washington National Airport.



Ed Aiken

When he was assigned to Pearl Harbor, he would take us to watch the takeoffs of the Martin JRM Mars seaplane, *Hawaii Mars*, a noisy, impressive flying machine bound for San Francisco.

Later, when he was assigned to Camp Lejeune, North Carolina, we observed amphibious landing exercises at Onslow Beach, involving landing craft, fixed- and rotary-wing aircraft, and of course, gyrenes on foot. These memories are still very vivid for me.

In 1958, I started high school in Alexandria, Virginia. I did well academically, especially in math and science, but my first love was English and foreign languages. When it came time to decide on college, my father advised me to consider the Naval Academy, which he attended prior to his service in the Marine Corps. I applied and was accepted at the Naval Academy, but I also surreptitiously applied to several other schools, including Princeton. We visited Princeton during the summer, after spending some time at the Naval Academy, of course. During my admissions interview there, I admitted that my favorite instrument was the bagpipes; I am convinced that my taste in music ensured my admittance to the Princeton Class of 1966. However, the next hurdle was to convince my father. I secretly applied for a Navy Reserve Officers' Training Corps (ROTC) scholarship to Princeton, was successful, and my father relented.

Because of my father's influence, I selected an engineering course of study at Princeton, but I was uncertain about an engineering major. The Department of Aerospace and Mechanical



TWA Lockheed Constellation.



Martin JRM Mars on takeoff.



Amphibious landing at Onslow Beach.

Sciences (AMS) won me over with an introductory flight in their Cessna 310, a “Learjet with propellers,” to demonstrate its dynamic characteristics. It was either AMS or sitting at a lab bench doing circuit board analysis in the Electrical Engineering Department. I was convinced.

The AMS Department, and especially its faculty, were impressive. The Department Chair was Courtland Perkins, an admirable and internationally respected aeronautical engineer and co-author of our “bible,” *Airplane Performance, Stability, and*

Control. Court had a hands-on approach to engineering and a wealth of war stories to share with his students. Another professor, David Hazen, also spiced up his lectures with war stories; a memorable one for me was of the development of the Navy’s Vought F7U Cutlass, known fondly as the “Buttless Cutlass” because of its unusual tail design.

Most of my summers were spent training with the Navy, which included duty on board the USS Intrepid, a WWII aircraft carrier, and flight training at Corpus Christi Naval Air Station in Texas. I spent my one “free” summer after my senior year working at Cornell Aeronautical Laboratory (CAL) at the Greater Buffalo International Airport while living in a rooming house in Niagara Falls, New York, then home of the Bell Aerospace plant. While at CAL, I was assigned to help Jack Beilman, developer of the Low Range Airspeed System (LORAS), with its testing. That summer, one of the two Bell X-22A quad ducted-fan vertical/short takeoff and landing (V/STOL) aircraft, Bell’s entry in the Tri-Service V/STOL competition, was being flight tested near the Bell plant. The aircraft underwent a complete hydraulic system failure and crashed in a nearby field. Thankfully, there were no casualties.



The Vought F7U Cutlass.

Both the undamaged X-22A and the remains of the crashed vehicle would become a major part of my professional life.



The Bell X-22A.



Aftermath of the Bell X-22A flight-test crash.

The Navy allowed me to spend an additional year at Princeton earning a Master's degree under the guidance of Professor Edward Seckel. I did not impress Seckel as being a stellar graduate student, but I soldiered on with a thesis project focused on assessing the feasibility of using stored energy aboard an aircraft to provide a short-term, short-field landing capability. David Ellis, a Princeton test pilot and instructor, helped salvage my degree by assisting and mentoring me in implementing an in-flight simulation, using the Princeton Variable Stability Navion, to study the effect of releasing compressed gas during landing approach to shorten the runway requirements.

In the classroom, I was fortunate to be taught by such luminaries as Seckel, Dunstan Graham, and Howard (Pat) Curtiss. Professor Curtiss was especially impressive with his ability to simplify the complex dynamics of helicopter flight. He continued to have a positive impact on my career after Princeton.

Many of my fellow graduate students became valued friends and colleagues throughout my career: Jack Franklin, Vic Lebacqz, Bob Ormiston, Fred Schmitz, and Jim McCroskey appeared and reappeared throughout my life. I am very thankful for them and their support.

As the end of my graduate school program approached, I had to make a choice about my required 5 years in the Navy. I was naturally attracted to Naval Aviation, but my Marine Corps veteran father advised me to avoid naval aviation as a career. He was troubled by our country's participation in the Vietnam War and concerned about how many young aviators were dying in that war. I thank him for that advice to this day; several of my classmates in the Princeton Navy ROTC program died in Vietnam. Instead, I chose to enter the nuclear submarine service, a decision that was made easier by my childhood love of submarines; the *Silent Service* was one of my favorite TV shows.

After being interviewed and accepted by Admiral Hyman Rickover I went through Nuclear Engineering training in Bainbridge, Maryland, and Submarine School in New London, Connecticut, in 1967, before being allowed to set foot on a submarine. I was then assigned to the Engineering Department of the USS Mariano G. Vallejo (SSBN-658). My family and I "saw the world" with the Navy, moving with the Vallejo from Guam to Pearl Harbor to Charleston, South Carolina, via the Panama Canal, and finally to Holy Loch, Scotland.

As my required time in the Navy wound down, I had to decide whether to stay in the submarine service, venture out on my own, or try something completely different. I had applied to the Massachusetts Institute of Technology Department of Ocean Engineering and was accepted as a graduate student, but now that I had a family, that option was not realistic. My previous



USS Mariano G. Vallejo (SSBN-658).



A submariner on patrol, 1971.

connection with CAL paid off, and I was offered a position in their Flight Research Department, my old stomping grounds.

Back to Buffalo (May 1972–June 1977)

At CAL (now Calspan), I was assigned to a team of engineers conducting flight experiments on the X-22A aircraft, now modified by CAL to be a variable-stability-and-control research aircraft. My initial assignment was to assist in developing a fixed-base ground simulator facility using the cockpit of the aircraft that had crashed during my 1966 summer at CAL.

Once the ground simulator was functional, I conducted piloted research to generate control-system and cockpit-display requirements for decelerating, descending transitions to a hover under instrument conditions [1]. It was then that I was introduced to Bob Harper, co-developer of the Cooper–Harper Handling Qualities Rating Scale

(with George Cooper, chief test pilot at NASA Ames Research Center). This simulator research program expanded into a major flight program on the X-22A [2] and, because of our success, the Navy asked our research team to develop an in-flight simulation of the Harrier aircraft using the X-22A [3].

Unbeknownst to me, many of my friends and colleagues at CAL during those 5 years would reappear in succeeding chapters of my odyssey, including Dave Key, Bob Chen, Vic Lebacqz, Warren Hall, Bob Till, John Wilson, and Irv Statler. On the West Coast, we became known as the *Calspan Mafia*.

The Blizzard of 1977 in Buffalo was a message to my family and me—time to move on to warmer climates!

T for Texas (and Training) With Singer-Link (June 1977–July 1978)

Singer-Link, a developer of training flight simulators headquartered in Binghamton, New York, had recently expanded its software development operation in Houston, Texas, adjacent to the NASA Johnson Space Center. This new organization was responsible for the development of the software for the UH-60 Black Hawk training flight simulator, and I was in charge. One of my first assignments, of course, was to travel to Binghamton in the winter to assist in the development of a program plan, since I had significant previous experience with, and the proper clothing for, winter weather in upstate New York.

My Houston adventure lasted only a year. Thanks to my old friend from Princeton, Jack Franklin, now a NASA Branch Chief, and others, I was offered a position working for Dave Key and the Army at Ames Research Center, Moffett Field, California. My family and I were very happy to see Texas in our rearview mirror.

Go West, Young Man! Ames Research Center (July 1978–May 1985)

As an Army employee at Ames Research Center, I was assigned to the NASA Flight Dynamics and Controls Branch, working under the supervision of Dr. Jack Franklin, NASA Branch Chief, and Dave Key, Chief of the Flight Controls Division of the Army Aeromechanics Laboratory. This unique, seamless working relationship was enabled by the Army-NASA Joint Agreement at Ames. One of my first assignments as a researcher at Ames was to develop a mathematical model of the AH-64 Apache attack helicopter that could be used in future piloted-simulation investigations [4]. I designed and conducted a simulator experiment on the old S.01 6-degree-of-freedom (6-DOF) simulator using my model to investigate the effects of variations in control-system characteristics, cockpit-display format, and symbol dynamics on a pilot's ability to perform target designation and weapon delivery tasks during a nighttime attack helicopter mission [5]. I was assisted by Lieutenant Colonel Bob Merrill, whose piloting capabilities and sense of humor helped make my initial research project at Ames an enjoyable and fruitful one.

Using this same mathematical model, Chris Blanken and I undertook a piloted simulator investigation of means to minimize transient aircraft motions that might occur as a result of engaging the Apache's Back-Up Control System (BUCS). The simulation was accomplished by breaking a shear pin at the pilot's controller [6]. Key elements of the simulation were the representation of a control system jam—the pilot breaking a shear pin in the jammed control—and the resultant BUCS engagement. To minimize the excursions in aircraft motion that could result from the pilot's control inputs after shear-pin breakage, the BUCS control function was blended in gradually. The experimental results indicated that optimum time to full control authority after shear-pin breakage was 3 seconds in all axes for certain critical tasks.

Because of the Army's interest in freeing up cockpit space by implementing small, side-stick controllers rather than the conventional center stick and collective controllers, we conducted piloted simulator investigations using the Ames Vertical Motion Simulator (VMS). These experiments assessed the effects of side-stick controller characteristics, and the level of stability and control augmentation on handling qualities for several low-altitude control tasks. These studies demonstrated the feasibility of using a single, properly designed, limited-displacement, multi-axis controller for certain relatively routine flight tasks in a two-crew rotorcraft with nominal levels of stability and control augmentation. However, for the more demanding terrain flight tasks, separated two- or three-axis controller configurations are required for acceptable handling qualities unless high levels of stability and control augmentation with a high degree of reliability are incorporated. Ours was one of the first rotorcraft simulations to use the four-window computer-generated imagery (CGI) provided by the VMS facility. The results of this experiment formed the basis for



The Vertical Motion Simulator (VMS).



Army/Boeing ADOCS Light Hawk demonstrator aircraft.

the controllers and control laws eventually implemented in the Army/Boeing Advanced Digital Optical Control System (ADOCS) demonstrator aircraft [7-10].

Katie Hilbert, a new Princeton-educated engineer, and Boeing engineers Ken Landis and Phil Dunford assisted in these side-stick controller experiments. At the time, Dunford was a fresh-faced, junior engineer with Boeing, recently arrived from England. He was soon to become Vice President, Boeing Military Aircraft!

Together with Mike Lewis, a newly arrived engineer from Princeton, we developed a mathematical model and conducted a piloted simulation designed to examine the effects of terrain proximity and control system design on helicopter performance during one-on-one air combat maneuvering. The NASA Ames VMS and its CGI systems were modified to allow two aircraft to be independently piloted on a single CGI database. Engagements began with the blue aircraft already in a tail-chase position behind the red, and with the aircraft originating from positions unknown to each other. Maneuvering was very aggressive and safety requirements for minimum altitude, separation, and maximum bank angles typical of flight test were not used. Results indicated that the presence of terrain features added a degree of complexity to the task compared to maneuvering in clear air away from terrain, and that a mix of attitude and rate command-type stability and control augmentation system (SCAS) design might be desirable. The simulation system design, the flight paths flown, and the tactics used were reviewed by evaluation pilots, and compared favorably to actual flight test experiments [11].

As a result of the Army-NASA Joint Agreement at Ames, I was able to become a Group Leader within NASA's Flight Dynamics and Controls Branch, as an Army employee.

NASA/Army Group Leader (May 1985–September 1987)

As Group Leader, I worked with Doug Watson, a young, enthusiastic NASA engineer, to investigate control cross-coupling effects and control-display effects for a precision hover task using simulation and flight [12]. Working with friends and colleagues at the German Aerospace Research Establishment (DLR) in Braunschweig was a very memorable event; we designed and conducted the first flight experiment using the variable stability capability of their Bo-105 research helicopter [13, 14]. I vividly remember being embarrassed (and honored) at a technical meeting that included Peter Hamel, DLR Director, when my friend, Jürgen Pausder, served me a

bottle of excellent German beer. NASA test pilot Ron Gerdes and I traveled to the flight test facility at Manching (near Munich) with Pausder at the wheel, blazing along the Autobahn.

After an enjoyable and fulfilling 10 years at Ames, I was ready for new challenges and an opportunity to participate directly in some exciting industry programs. Because of my recent association with Boeing Helicopters, I was offered a job with Boeing in Philadelphia, Pennsylvania, which I accepted.



Ron Gerdes and Ed Aiken with the DLR Bo-105.

Go East, Young Man! Boeing Helicopters (October 1987–December 1989)

Bruce Blake, the Director of Research and Technology at Boeing Helicopters and a man whom I admired and respected, hired me as the Manager of the Flight Simulation Laboratory (FSL). As the FSL Manager working with Ken Landis, Flying-Qualities Manager, I oversaw a significant upgrade in the capability of that facility including a high-performance motion base and two dome visual systems. I provided technical leadership and guidance in the development of a unique simulation system that included a cab and 6-DOF motion base located inside a fixed dome used for outside visual cueing. The facility was used for major programs, including the Bell/Boeing V-22 Osprey and the Army's ADOCS program.

It was a privilege to work with the team of engineers and managers at Boeing, but my family and I decided that we were not really "East Coast people," and we were anxious to return to Northern California. That opportunity came in the form of a job offer from Greg Condon, the NASA Ames Aviation Systems Division Chief, prompted by Jack Franklin.

Home Again! Ames Research Center (December 1989–January 2005)

In March of 1992, I was fortunate to be selected as the Chief of the old NASA Flight Dynamics and Controls Branch. During my service as Branch Chief, branch members made significant technical contributions, including: 1) the successful completion of the V/STOL Systems Research Aircraft (VSRA) flight program and the transfer of flight-validated control/display technology to the Joint Strike Fighter Program and contractors; 2) the development, in simulation and flight, of effective and safe low-noise procedures for both helicopters and tiltrotor aircraft; and 3) completion of the acquisition and development of the unique rotorcraft flying laboratory, the Rotorcraft-Aircrew Systems Concepts Airborne Laboratory (RASCAL), a UH-60 Black Hawk helicopter modified to include a high-performance, full-authority, programmable flight control system.



When the Ames-Army Lab was in the process of acquiring the Army/Boeing ADOCS demonstrator aircraft that became the RASCAL, they needed a Program Manager (PM). I was very happy to be considered and enthusiastically accepted the offer. During my 2 years as PM, I was ably assisted by such team members as Bob Jacobsen and Michelle Eshow, NASA engineers; Court Bivens,

Army pilot/engineer; and Bill Hindson, NASA pilot/engineer. My two major personal contributions to the program were: 1) the concoction of the RASCAL acronym for the aircraft that typified the personality of our team, and 2) negotiations with Warner Brothers for the use of my favorite cartoon character, the Tasmanian Devil, in our RASCAL logo.

The RASCAL is configured as an airborne laboratory capable of simulating a wide variety of air vehicle characteristics and flight control configurations [15-18]. The evaluation, or subject, pilot occupies the right seat and flies the aircraft through its Research Flight Control System (RFCS), which includes a suite of intelligent safety monitors to ensure that the system does not command dangerous control inputs and to disengage the system immediately in case of a failure. The safety pilot, in the left seat, is provided with standard controls that are back-driven by the RFCS to allow monitoring of its operation and the ability to disengage the system manually should the need arise. The research system operator is seated in the rear of the aircraft, sets up the experimental configuration, and monitors the experiment in progress. The RASCAL in-flight simulator was subsequently used for many investigations and led to many groundbreaking accomplishments, including the modern Aeronautical Design Standard for Rotorcraft (ADS-33).



Army/NASA RASCAL research helicopter at Ames Research Center.

In October of 1995, my NASA bosses strongly suggested that I become NASA's first Flight Simulation Facility Group Director, responsible for the strategic management of all of NASA's piloted flight simulation facilities, located at four NASA field Centers. There was some understandable opposition from the other Centers to an Ames manager taking over their simulation facilities, but, in the end, I was able to convince them that "I come in peace." As Jack Franklin's idol, Yogi Berra, reminded us, "When you come to a fork in the road, take it."

As a natural extension of the Joint Agreement, the Army and NASA at Ames joined forces to create the Army/NASA Rotorcraft Division—a new organization at Ames co-managed by Army and NASA employees. In March 1997, I was selected to head up this new Division, with Wendell Stephens, followed by Mike Rutkowski, as Army Deputy Division Chief. This reorganization process included the development of a “strategic vision” for joint NASA/Army rotorcraft research that resulted in a funded rotorcraft research and technology program.

However, these were bleak days for the NASA rotorcraft program, with minimal support from the Center and from NASA Headquarters. The Army and the U.S. rotorcraft industry came to our rescue, with Andy Kerr, Army Aeroflightdynamics Director and fellow Princetonian, and Rhett Flater, American Helicopter Society (AHS) Executive Director, joining forces in support of the NASA program. The potential loss of a rotorcraft flight research capability at Ames was of particular concern to me.

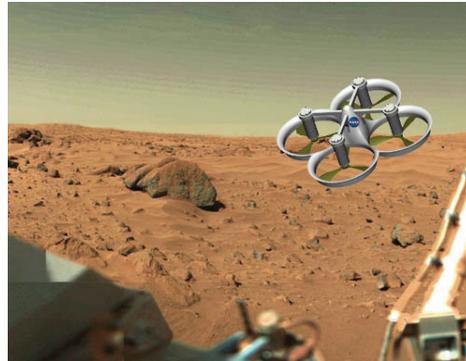
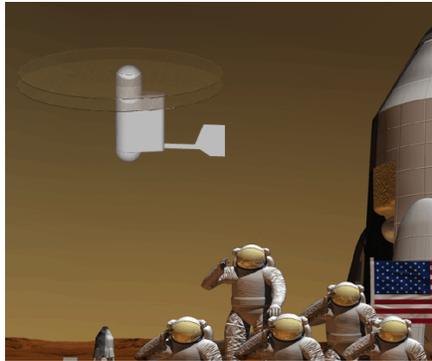
In 1996, NASA determined that all NASA-owned research aircraft stationed at its various field installations should be consolidated at Dryden Flight Research Center at Edwards, California. This directive required that all aircraft located at NASA Ames Research Center in northern California, including associated personnel and selected ground support equipment, were to be moved several hundred miles to southern California. This directive also involved mothballing facilities at Ames and deactivating support for aircraft located at Ames. The Ames-Army Lab and the associated fleet of aircraft were impacted by this decision. Before the NASA decision to consolidate its aircraft assets, the Army aircraft shared support resources provided by NASA. The Army elected to keep its aircraft at Ames and was faced with the loss of facilities and ground support. As the senior manager associated with the Army-NASA relationship at Ames, I assumed the responsibility of finding a way to support the Army rotorcraft. We were able to acquire the resources and coordinate the necessary logistical operations for the Army to keep its rotorcraft at Ames with full ground support and appropriate hangar facilities. The Army Flight Projects Office was an extremely productive unit and received many commendations and awards for its outstanding efforts.

I am particularly proud of the significant contributions made by our Division to U.S. industry. These accomplishments included the development of a unique ground test device for tiltrotor designs, the Tilt Rotor Aeroacoustical Model (TRAM), which received the 1998 AHS Grover E. Bell Award, and an integrated flight control law design tool, Control Designers’ Unified Interface (CONDUIT[®]), which was readily adopted in both the fixed- and rotary-wing industry as a means of improving the efficiency of the flight control system design and testing process.

We also initiated an effort to apply “intelligent autonomy” to small Unpiloted Air Vehicle (UAV) rotorcraft. Matt Whalley, an Army engineer, developed these concepts and created an extremely successful and productive research program, including the use of the RASCAL research helicopter as a test platform.

I am also proud of our planning and advocacy efforts for a true “Future Directions” effort in NASA rotorcraft research and technology. Together with NASA engineer Larry Young, we developed the concept for a Martian Autonomous Rotorcraft to assist in NASA’s planned planetary exploration programs and collaborated with the Ames Information Systems Directorate and Center for Mars Exploration. I am convinced that, one day very soon, we will see an autonomous rotorcraft in operation on Mars. In fact, in a May 2018 press release, NASA

announced: “NASA is sending a helicopter to Mars. The Mars Helicopter, a small, autonomous rotorcraft, will travel with the agency’s Mars 2020 Rover mission, currently scheduled to launch in July 2020, to demonstrate the viability and potential of heavier-than-air vehicles on the Red Planet.”



Martian Autonomous Rotorcraft concepts.

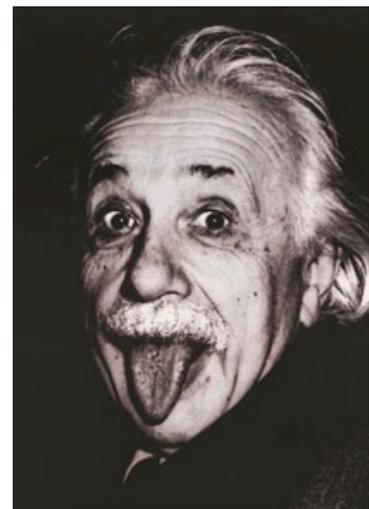
Epilogue

There is one person who has been with me through thick and thin at Ames Research Center. I was first introduced to Linda Vollenweider in 1978 when I arrived at Ames as a new Army engineer in Jack Franklin’s Flight Dynamics and Controls Branch, and she was serving as branch secretary. Linda, with her sense of humor and ability to network throughout the Center, continued to provide me with moral support and some necessary prodding throughout my career as Group Leader, Branch Chief, Simulation Facility Group Director, and Division Chief. She was even there to convince me to return to Ames from Boeing Helicopters. She was, and is, a true treasure, and the secret behind my success and enjoyment at Ames.

At some point early in my career, I was advised by a wise person to try different varieties of organizations conducting aeronautical research: government, industry, and university. Although not part of any grand plan, I did exactly that, and have thoroughly enjoyed my “aeronautical odyssey” and the colleagues and good friends I have made on the journey.

I retired from NASA in January 2005 and am convinced that our current NASA and Army organizations, having benefitted from the cooperation and collaboration that led to the Army/NASA Rotorcraft Division, are in excellent hands. Lieutenant Colonel Hynda, of the Army Aviation Development Directorate (acting), and Dr. William Warmbrodt, the NASA Aeromechanics Branch Chief, are outstanding leaders who continue to forge new directions for our unique and ongoing cooperation.

In closing, these words of advice from another Princetonian: “When you look at yourself from a universal standpoint, something inside always reminds or informs you that there are bigger and better things to worry about.”



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Index

A

Advanced Digital Optical Control System (ADOCS), 6-8
Aeronautical Design Standard for Rotorcraft (ADS-33), 8
Aerospace and Mechanical Sciences (AMS), 1-2
American Helicopter Society (AHS), 9
Ames Aviation Systems Division, 7
Ames, 4-10
Ames-Army Lab, 8-9
Army Aeromechanics Laboratory, 5
Army Flight Projects Office, 9
Army, 1, 4-10
Army/NASA Rotorcraft Division, 9-10
Army-NASA Joint Agreement, 5-6, 9

B

Back-Up Control System (BUCS), 5
Beilman, Jack, 2
Bell Aerospace, 2, 7
Berra, Yogi, 8
Bivens, Court, 8
Blake, Bruce, 7
Blanken, Chris, 5
Bo-105, 6-7
Boeing, 6-8, 10
Buttless Cutlass, 2

C

CAL Flight Research Department, 4
Calspan, 4
Chen, Bob, 4
computer-generated imagery (CGI), 5-6
Condon, Greg, 7
Control Designers' Unified Interface (CONDUIT), 9
Cooper, George, 4
Cooper-Harper Handling Qualities Rating Scale, 4
Cornell Aeronautical Laboratory (CAL), 2, 4
Curtiss, Howard (Pat), 3

D

Dryden Flight Research Center, 9
Dunford, Phil, 6

E

Ellis, David, 3
Eshow, Michelle, 8

F

Flater, Rhett, 9
Flight Dynamics and Controls Branch, 5-7, 10
Flight Simulation Laboratory (FSL), 7
Franklin, Jack, 3-5, 7-8, 10

G

Gerdes, Ron, 7
German Aerospace Research Establishment (DLR), 6-7
Graham, Dunstan, 3
Grover E. Bell Award, 9

H

Hall, Warren, 4
Hamel, Peter, 6
Harper, Bob, 4
Harrier aircraft, 4
Hazen, David, 2
Hilbert, Katie, 6
Hindson, Bill, 8

J

Jacobsen, Bob, 8
Joint Strike Fighter Program, 7

K

Ken Landis, 6-7
Kerr, Andy, 9
Key, Dave, 4-5

L

Landis, Ken, 6-7
Lebacqz, Vic, 3-4
Lewis, Mike, 6
Low Range Airspeed System (LORAS), 2

M

Mars 2020 Rover mission, 10
Mars Helicopter, 10
Martian Autonomous Rotorcraft, 9-10
Massachusetts Institute of Technology, 3
McCroskey, Jim, 3
Merrill, Bob, 5

N

NASA Ames Research Center, 4-5, 7-10
NASA Johnson Space Center, 4
Navy, 1-4

O

Ormiston, Bob, 3
Osprey, 7

P

Pausder, Jürgen, 6-7
Perkins, Courtland, 2
Princeton University, 1, 3-4, 6, 9-10

R

Red Planet, 10
Research Flight Control System (RFCS), 8
Reserve Officers' Training Corps (ROTC), 1, 3
Rotorcraft-Aircrew Systems Concepts Airborne Laboratory (RASCAL), 7-9
Rutkowski, Mike, 9

S

Schmitz, Fred, 3
Seckel, Edward, 3
Simulation Facility Group, 8, 10
Singer-Link, 4
Stability and control augmentation system (SCAS), 6
Statler, Irv, 4
Stephens, Wendell, 9

T

Till, Bob, 4

Tilt Rotor Aeroacoustical Model (TRAM), 9

U

UH-60 Black Hawk, 4, 7

Unpiloted Air Vehicle (UAV), 9

V

Vertical Motion Simulator (VMS), 5-6

vertical/short takeoff and landing (V/STOL), 2, 7

V/STOL Systems Research Aircraft (VSRA), 7

Vollenweider, Linda, 10

W

Warmbrodt, Bill, 10

Watson, Doug, 6

Whalley, Matt, 9

Wilson, John, 4

Y

Young, Larry, 9

Army Lab Experience
William F. Ballhaus, Jr.
Army/NASA

It was a stroke of fate and very good luck that landed me a summer job in the Army Air Mobility R&D Laboratory (AMRDL) at NASA Ames Research Center in June of 1971. I had just finished my Ph.D. work at University of California, Berkeley (UCB), and was getting my dissertation signed off by the faculty members on my thesis committee. In talking with one of them, Murray Protter who covered my Ph.D. minor in mathematics, I mentioned that I had a commitment to serve on active duty as an officer in the Army beginning sometime the following year. He put me in touch with one of his former students, Peter Goorjian, who had a similar commitment and was serving it at the Army Lab at NASA Ames. Following a telephone call with Peter, I was able to get an interview with Major Gordon Berry, who then hired me for the summer.



William F. Ballhaus, Jr.

During the summer of 1971, I had the opportunity to work with Frank Caradonna on using finite-difference methods to solve the flow around high-speed helicopter rotor tips. I occasionally shared an executive office (Paul Yaggy's old office, I believe, after he was promoted out of the Army Aeronautical Research Laboratory) with Morris Isom, a professor from New York University, who derived the governing equations for Frank to attempt to solve.

The finite-difference method we used was adapted from work done on flow past airfoils in the late 1960s by Earll Murman (Boeing) and Julian Cole (University of California, Los Angeles). Professor Cole gave a lecture at Berkeley on their work when I was a grad student there. My thesis advisor, Professor Maurice Holt, asked me to pick up Professor Cole at the Oakland Airport, and I got to know him a bit. It turned out that he and my father were both Ph.D. students at Caltech in the class of 1947. I also met Earll Murman and really got to know him after he left Boeing, spent some time at a company called Flow Research, and then wound up at NASA Ames. He subsequently went to Massachusetts Institute of Technology in 1979 and retired from there a few years ago. Maurice Holt was a major proponent of computational fluid dynamics (CFD). I remember him speaking at a dinner in the late 1960s, predicting that much of the engineering analysis and design work then done in wind tunnels would be done using CFD in the future. Maurice was the organizer of the Second International Conference on Numerical Methods in Fluid Dynamics, held in Berkeley. The first conference was held in Novosibirsk in the Soviet Union 2 years prior. One of my tasks as a grad student was to help Maurice organize the conference and edit the many CFD papers.

Frank Caradonna was wonderful to work with, and CFD was an area of research I really wanted to pursue. As Frank notes in his chapter, before the summer was over, an Army position opened in the NASA Ames Computational Fluid Dynamics Branch. I interviewed with the Branch Chief, Harvard Lomax, and with the Assistant Branch Chief, Bob MacCormack. By that time, because of the excess number of junior officers in the Army, my active duty service was reduced to

3 months in 1972 with a requirement to serve in the active reserves until 1976. I was hired at NASA Ames and continued my work with Frank on high-speed rotor tips, and I also began working with Ron Bailey and Harv Lomax on finite-difference solutions of 3-D flows past swept wings. Frank and I published two papers together [1, 2].

In those days, NASA Ames was the place to be for CFD. The Center Director, Dr. Hans Mark, was a driving force in making Ames a center of excellence in CFD research. I had met Dr. Mark when he was Chair of the Nuclear Engineering Department at UCB. He was 39 years old when he became Director of NASA Ames. The Chief of the Thermo- and Gas-Dynamics Division, Dean Chapman, was also a strong proponent of CFD as an alternative to wind tunnel test facilities. I had heard Dean give a seminar at Berkeley on the Ablation of Tektites when I was a grad student and was so impressed that I called my father that evening to tell him about it. He told me that he, too, had been impressed with Dean, who was also a member of the Caltech Ph.D. class of 1947. Chapman, Mark, and Mel Pirtle co-authored an article [3] that initiated a discussion of the future importance of CFD and led to increased investment in that future. Chapman believed, as did Holt, that computers would play an increasingly important role as Moore's Law continued to advance computer hardware and as algorithm innovations continued. This helped set the stage for the opportunity we had subsequently to convince NASA, the Office of Management and Budget (OMB), and Congress to invest tens of millions of dollars to develop state-of-the-art supercomputer facilities at Ames, when I became Director of Astronautics and later, Ames Center Director. We had the great good fortune to do this while Hans Mark, a strong advocate, had moved from Secretary of the Air Force to Deputy Administrator at NASA. But I am getting ahead of myself.

During the 1970s and 1980s, Center management at all levels were the true intellectual leaders at NASA. As a young researcher, I had five managers above me on the NASA side: MacCormack, Lomax, Chapman, Sy Syvertson, and Hans Mark. All five were elected to the National Academy of Engineering (NAE). I was elected in 1988. (Note that the Army Lab's Richard Carlson was also elected to the NAE in 1990 and Jim McCroskey in 1996.) Such is not the case today at any NASA Center and will likely never happen again. Through a series of NASA Administrators, the intellectual leadership and principal responsibility for program formulation and management has migrated to Headquarters and away from Center management.

In any case, we made great strides in CFD during the 1970s. CFD became a practical tool for the design and analysis of aerospace systems. For example, the swept-wing transonic flow CFD code that Ron Bailey and I developed was used extensively by engineers at Northrop in the design of the B2 Stealth Bomber. I didn't find out about this until I was briefed into the program several years later. In 1978 I was asked to form the Applied Computational Aerodynamics Branch and serve as its chief. I had just recently been promoted to GS-14, having gone through the NASA and Army promotion boards. I owe a special debt of gratitude to Victor Peterson, Chief of the Thermo- and Gas-Dynamics Division, for selecting me for the position and mentoring me as a young researcher who had a lot to learn about performing as a branch chief. Vic was technically competent and a very experienced manager who eventually became Deputy Director of Ames.

In early 1980, Dean Chapman retired as Director of Astronautics, a Senior Executive Service (SES) position. The Deputy Center Director, Tom Young (later, Director of NASA Goddard and subsequently, President of Martin Marietta Corporation), asked me to apply for the position, which, given my limited management experience, was an unimaginable stretch. In any case, in



Bill Ballhaus was the first Army employee to become NASA Branch Chief, 1978. From left: Andy Morse, Irv Statler, Bill Ballhaus, and Vic Peterson.

February 1980 I assumed that position, which then marked the end of my Army civil service career. During this time, I had the good fortune to work on interesting CFD research with a number of collaborators. One in particular was Dr. Peter Goorjian, a member of the newly formed Applied Computational Aerodynamics Branch, who had initially alerted me to the interesting work at the Ames-Army Lab. Together we published several papers, Ballhaus and Goorjian [4-6] and Ballhaus, Goorjian, and Yoshihara [7].

A couple of days prior to Christmas 1983, Jack Boyd asked me to submit, within a few days, a strategic plan for the future of NASA Ames. I submitted the plan, and in February 1984 I was appointed to succeed Sy Syvertson as the Ames Center Director. NASA Administrator Jim Beggs was willing to bet on the potential of a 38-year-old when there were far more experienced, and much lower-risk, candidates he could have selected.

As the new Center Director, one of the initial critical goals was to complete the reconstruction and testing of the National Full-Scale Aerodynamics Complex (NFAC). In his chapter, Ken Mort provides a description of some of the aerodynamic issues with the NFAC and the failure that occurred in Integrated Systems Test. The NFAC development project had operated on a constrained budget, a fact that had contributed to the failure. As the Center continued to uncover issues, the cost estimates to complete the project continued to escalate. After several months of increasing cost estimates, I believed we finally had a good handle on how much additional funding would be required to finish the project. As I recall, it was an additional \$11M. I

addressed this with the NASA Administrator's staff, and the feedback I got was that there was no interest in bringing the overrun forward to him at that time. So, I made an appointment with Jim Beggs to discuss the additional funds needed. He asked me what it was going to cost, and I told him an additional \$11M. He asked if I could guarantee that we could finish the project with that funding. I told him that the funding was adequate to fix the problems we knew about. However, if we encountered any additional unknowns, I would let him know immediately. His response was that he would find the money and his direction to me was, "Just do it right." The project team did complete the project for the requested additional funds, and the result was a major, unique national facility that NASA and the Army could be proud of.

In 1988–1989, I responded to a request from then Administrator James Fletcher to act as the Associate Administrator for Aeronautics and Space Technology at NASA Headquarters. Coincidentally, I had also just been elected President of the 40,000-member American Institute of Aeronautics and Astronautics (AIAA), newly headquartered in Washington, D.C. In the two senior NASA positions, I worked with the Army in a broad array of activities. We were able to rebuild the NFAC, upgrade simulators, develop some of the nation's most powerful computational facilities, and advance automation, flight controls, and human factors research, etc. I will always be grateful to my wife, Jane, for all the challenges she had to deal with during the 14-month assignment I had in Washington, D.C., as well as all the other moves we made together.

My government service lasted 18 years, half of which were as an employee of the Ames-Army Lab. I had the opportunity to work closely with outstanding Army technical people and managers, like Frank Caradonna, Jim McCroskey, Bob Ormiston, Andy Morse, Fred Schmitz, Richard Carlson, Irv Statler, and others. I have spent over 18 additional years, while in industry, as a special government employee, serving on the Air Force Scientific Advisory Board, the Defense Science Board, the National Oceanic and Atmospheric Administration (NOAA) Science Advisory Board, and the NASA Advisory Council. Likely life would have taken a completely different path had I not had the very good fortune to get a summer job at the Army Lab in 1971.

In 1989, I joined Martin Marietta in Denver, initially running the Titan IV Centaur Upper Stage Development, then I organized and served as President of Civil Space and Communications. I subsequently served as President of Aero and Naval Systems, running the old Glenn L. Martin plant outside of Baltimore, and then became the Corporate Vice President, Engineering and Technology, when Lockheed Martin Corporation was formed in 1995. In 2000, I retired from Lockheed Martin, moved back to Palos Verdes, California, after an absence of nearly four decades, and became President and CEO of the Aerospace Corporation, retiring at the end of 2007. I am very proud of the work Aerospace has done in helping the Air Force and the National Reconnaissance Office reestablish the recipe for successful program formulation, execution, and mission success, after the failed acquisition reform practices of the 1990s. These failed acquisition practices resulted in multiple launch vehicle and satellite system failures and extensive cost/schedule overruns due to "birth defects" in critical national security space system development efforts. I now serve on several boards, and Jane and I enjoy watching our 10 grandchildren learn and grow.

It has been fun remembering the time I spent at the Ames-Army Lab—the stimulating technical discussions and challenges, the ping-pong matches at lunch, and Frank's rag-time piano playing. I am grateful to all those members of the Lab who made this initial part of my career so

enjoyable and productive. I was fortunate to become a participant in the unique collaboration between the Army and NASA at Ames that opened the pathway for an Army Reserve Captain to become the Director of NASA Ames Research Center and act as the NASA Associate Administrator for Aeronautics and Space Technology.

Incidentally, the photo of me, taken in 1978 or 1979, shows the afro that Frank Caradonna mentions in his chapter.

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Index

A

Aero and Naval Systems, 4
Aerospace Corporation, 4
American Institute of Aeronautics and Astronautics (AIAA), 4

Applied Computational Aerodynamics Branch, 2-3
Army Air Mobility R&D Laboratory (AMRDL), 1

B

Bailey, Ron, 2
Ballhaus, Jane, 4
Beggs, Jim, 3-4
Berry, Gordon, 1
Boyd, Jack, 3

C

Caradonna, Frank, 1, 4-5
Carlson, Richard, 2, 4
Chapman, Dean, 2
Civil Space and Communications, 4
Cole, Julian, 1
Computational fluid dynamics (CFD), 1-3

Computational Fluid Dynamics Branch, 1

F

Fletcher, James, 4

G

Goorjian, Peter, 1, 3

H

Holt, Maurice 1, 2

I

Isom, Morris, 1

L

Lockheed Martin Corporation, 4
Lomax, Harvard, 1-2

M

MacCormack, Bob, 1-2
Mark, Hans, 2
Martin Marietta, 2, 4
McCroskey, Jim, 2, 4
Morse, Andy, 3, 4
Murman, Earll, 1

Ballhaus, W.

N

National Full-Scale Aerodynamics Facility (NFAC),
3-4

National Oceanic and Atmospheric Administration
(NOAA), 4

O

Office of Management and Budget (OMB), 2

Ormiston, Bob, 4

P

Pirtle, Mel, 2

S

Schmitz, Fred, 4

Senior Executive Service (SES), 2

Statler, Irv, 3-4

Syvertson, Sy, 2-3

T

Thermo- and Gas-Dynamics Division, 2

Titan IV Centaur Upper Stage, 4

U

University of California, Berkeley (UCB) 1-2

Y

Yaggy, Paul, 1

Yoshihara, Hideo, 3

Young, Tom, 2

Back to Truth and Science

Chris L. Blanken

Army

I began working for the Army in 1980, and continue to work for the Army at Ames as of this publication. The research performed, the people I have met and who have mentored me, and the life experiences I've had while working at Ames have been incredible. I have been extremely fortunate.

The following is a brief overview of how I arrived at Ames, highlights of the 5-year period from 1980–1985, and since I still work for this prestigious Army research laboratory, an epilogue that extends this story to the present time.

In beginning . . .

I was born and raised in Santa Maria, a small farming community along the central coast of California. I was an only child and attended many adult functions with my parents. I learned at an early age to explore/entertain/learn about things on my own, and I somehow knew by junior high school that I wanted to be an aeronautical engineer when I grew up. This career path was likely influenced/reinforced by several things: my uncle was a Captain for Pan American Airways (his wife, my aunt, was a stewardess for Pan Am)—so there were lots of flying stories at family gatherings; Santa Maria was near Vandenberg Air Force Base and missile launches were very visible/audible (early evening launches were spectacular as the missile ascended from dusk back into the sunshine); and Dean Borgman—Dean would influence my career several times and would also precede me as one of the early employees at the Ames-Army Lab.

My parents, Chuck and Julie, were friends with the Borgman family, whose children included Dean. When I told my parents that I was interested in aeronautical engineering, they reached out to Dean, who was an aero student at Cal Poly, in San Luis Obispo. He came over to our house to explain some of the various aspects/fields in aeronautics and describe classes taken as an Aero student. This only cemented my interest and desire to be an aeronautical engineer. After graduating from high school, I went to a local junior college (Allan Hancock) for an A.A. degree in math, followed by enrollment in the Aero Department at Cal Poly. I enjoyed nearly all of my classes—it was everything I thought it would be and more—and joined a social fraternity, Delta Sigma Phi. Dean Borgman's second-influence came in between my junior and senior year at Cal Poly; Dean connected me with a friend of his, David Coe, for a summer job working at Northrop-Ventura. Northrop was in competition with Ryan for a high-speed subsonic target drone. I was hired for the summer in 1972 to reduce/plot data and support a test in the 6- by 6-Foot Wind Tunnel at NASA Ames Research Center. In August, I worked the swing shift in the wind tunnel, and watched the Munich Olympics during the day (including the abduction and killing of Israeli athletes). Little did I know that I would someday return to Ames Research Center as an Army employee.



Chris Blanken, 1980.

During my senior year at Cal Poly, my ex-Pan Am stewardess aunt gave me flying lessons as a graduation present. A fellow student in my aircraft design class was an instructor at the local airport and soon I was off learning to fly. It was a great experience for someone who always loved aviation. The lessons and subsequent flying were really fun and mostly uneventful. The only surprise occurred when I was practicing power-off stalls by myself for the first time, and the aircraft rolled first, before pitching down. (All of my prior stalls had occurred with the instructor in the right seat of the two-seat Cessna 150, thus making the aircraft nearly balanced in roll; when performed without the instructor in the right seat, the aircraft immediately rolled to the left during the stall build-up—something I had not expected.)

I graduated with a B.S. degree in aeronautical engineering in 1973, and I had several job opportunities: Boeing-Seattle working in a weights group, United Technologies Research Center working in propulsion, and Rockwell International working in a flight control group. I consulted with the Dean of the Cal Poly Aero Department about these opportunities and he provided some great advice: “Don’t take the highest paying job—take the job in the field you want to work in, thus gaining experience in the desired specialty field for future opportunities.” So I accepted the job with Rockwell International B-1 Division in El Segundo, California. I joined the Flight Control group and began running a Flight Dynamics program assessing the terrain following/terrain avoidance performance of the B-1A at Mach 0.85 and 200 feet above ground level. This effort became the basis for my first technical publication, a U.S. Air Force Flight Dynamics Laboratory report on the development of automatic terrain following/terrain avoidance decoupling techniques (1974). Another highlight was working on a hinge-moment limiting problem associated with the B-1’s all-moving horizontal tail (the horizontal tail not only provided symmetrical movements for aircraft pitch control, but also differential movement for aircraft roll control). Rockwell had miscalculated the hinge moments, and the horizontal tail actuators were half the power needed. This was not normally a problem, but for high dynamic pressures (low-altitude and fast), there were some issues. During a high-speed low-altitude automatic terrain following/avoidance test flight, one side of the horizontal tail was moving trailing-edge down, hinge-moment limited (while the other side continued to move unrestricted trailing-edge up). This caused an “un-commanded” pitch up. Needless to say, this surprised everyone, including the flight test crew. I became involved in a flight dynamics modeling analysis and ground-based simulation study to modify the flight control system to address this hinge-moment limiting pitch-roll interaction. The fifth B-1A test aircraft was nearing final assembly when, on June 30, 1977, newly elected President Jimmy Carter cancelled the B-1A program. Rockwell’s response was to lay off all of the senior folks (“they cost too much”) and all the new hires within the last 5 years (“they don’t know anything”), and keep all the engineers in the middle (“they know something and don’t cost too much”). It was an eye-opening experience. Soon, morale deteriorated and I left Rockwell in 1978, moving to Phoenix, Arizona, to become a residential and commercial realtor.



B-1A Lancer (Rockwell International).

For fellow enthusiast Dr. Robert “Bob” Ormiston, I should mention that around 1976 I learned how to hang glide. My college roommate, Gary Spanos, and a friend of his, had purchased a simple Rogallo-wing hang glider and had taken lessons. At a Cal Poly reunion, Gary mentioned the hang glider and said he and his friend could teach me to fly—going to the same site in Ojai, California, where they had been trained. I agreed. On a Saturday morning we drove from West Los Angeles to Ojai. There was a hill with a gradually increasing slope, which allowed training to occur at different parts of the hillside for variations in hang glider sizes/rider weights and flight height above the ground. By the end of the day, I was confident in my ability to control the hang glider, and I jumped off the top of hill. It was great fun, but I didn’t pursue the sport.

Army Lab Employment . . .

In 1980, I had had enough of the unscrupulous dealings with the real estate “profession,” and decided it was time to get back to “truth and science.” Enter Dean Borgman again. I had received an invitation for an interview with Lockheed Space and Missiles in Sunnyvale, California. Before departing for the interview, I contacted Dean to see if there were any possibilities of Army employment at NASA Ames. Dean was working at the Army’s Directorate for Advanced Systems in St. Louis, Missouri. He reached out to the Ames-Army Lab and arranged an interview while I was in town to visit Lockheed. I met with David Key, Chief of the Flight Control Division of the Aeromechanics Laboratory at Ames. A few weeks later, I received a job offer from the Army (something not possible with today’s government bureaucracy). On April 21, 1980, I joined the Army’s Aeromechanics Laboratory at Moffett Field, California.



David Key, Flight Control Division Chief.

My first few days of Army employment were memorable: I first checked in with the human resources folks (Betty Gaffney and Bob Pike) co-located in a NASA building, then went to check in with David Key who had an office in the Army Aeromechanics Laboratory in Building N-215 above the NASA Ames Health Unit. As his secretary, Pam Baca, was temporarily out of the office when I arrived, Dr. Bob Ormiston’s secretary, Lori Cross, greeted me. Lori conferred with Mr. Key, telling him I had arrived, but I was instructed to go home and come back the next day. Upon arrival the next day, Dave informed me that he had no desk for me, and I would need to sit at the conference table in his office. This seemed a bit weird, but had the advantage of allowing me to pass by (and get to know) the secretaries in the adjoining office—especially, Dr. Ormiston’s secretary, Lori. I was against dating fellow employees, but the chemistry was too strong. We dated secretly for some time before letting the cat-out-of-bag and eventually became engaged and married. Thank you Dave for hiring me and not having a desk for me to sit at. And thank you Dr. Ormiston for hiring Lori.

Dave Key had several employees in his division, but they were all co-located within the NASA Flight Dynamics Branch, led by Dr. Jack Franklin. They included Ed Aiken, Lloyd Corliss, and Dean Carico. I was Dave’s first totally Army employee. My first assignment was an Apache helicopter simulation to investigate the easy-on time constant for the Back-Up Control System (BUCS) engagement following a jam in the primary control system. If there was a mechanical jam in the primary flight control system, the pilot could apply a large force (~ 60 lbs) to break a shear pin, allowing the pilot’s inceptor (cyclic, collective, or pedals) to move freely. At this

point, the inceptor was connected electrically to the helicopter rotor actuators. The key issue was that once the jam was detected, the pilot would apply a large force to break the shear pin, and the inceptor would then immediately travel to the control stop. To avoid or minimize this large control input going to the helicopter actuators, an easy-on time was implemented, i.e., over a specified time period (easy-on time), the sensed inceptor position would ramp up from 0 to 100 percent. Hopefully, during this ramp-up time, the pilot could return the inceptor to the trim position to minimize large control inputs to the helicopter. With mentoring from Ed Aiken and the Army pilot at Ames, Bob Merrill, I was able to plan and conduct a piloted simulation on the NASA Ames S.01 6-degree-of-freedom (6-DOF) motion simulator in the high bay of Building N-210. We added the jam logic, engagement logic, and sequence of the BUCS, and the aerodynamic effects and control of a moveable stabilator to the existing Apache mathematical model. We updated the stability derivatives and the reference aerodynamic trim maps that account for the speed derivatives.

This was my first introduction to the NASA simulation labs and their excellent staff. My simulation engineers were Norm Bengford and Joel Rosado. Norm was the digital computer specialist and Joel was the analog computer guru; his ability to diagnose and fix analog computer wiring boards was amazing. To this day, I do not know how he could go into an analog computer board and select one wire, out of hundreds, to replace—it was eerie.



S.01 6-DOF motion simulator.

The simulation was performed in support of the Apache Program Management Office and provided design data for definition of the easy-on time constant. It also acquainted the Hughes test pilots with the engagement transients and degraded flying qualities prior to flight testing. We submitted an abstract on the simulation for the 1981 37th Annual Forum of the American Helicopter Society (AHS), held in New Orleans, Louisiana. This was my first AHS Annual Forum and first presentation at such a large and distinguished event. Needless to say, I was a bit nervous at the prospect of presenting and practiced my speech diligently for weeks prior. (By the way, the optimum easy-on time was 3 seconds.) I traveled to the Forum with several other Army and NASA engineers from the NASA Flight Dynamics Branch, including Ed Aiken, Vic Lebacqz, Bob Chen, and Dave Key. Once at the Forum, I discovered that these folks had worked at Calspan in Buffalo, New York, prior to coming to Ames, and they were using the Forum to get together with their former colleagues (Bob Radford, Charles “Chic” Chalk, and Bob Harper of the Cooper–Harper Handling Qualities Rating Scale). On the first evening of the Forum, we all went out to dinner together. After dinner, we met at the hotel bar for socializing and story telling. About midnight, I finally got to bed, with my presentation bright and early the next morning. With plenty of practice before hand, I was able to get through the presentation just fine. That evening, with my talk complete, I was looking forward to a repeat of the prior night and hearing a few more good stories, and was disappointed to learn that Mr. Aiken and Dr. Lebacqz had presentations to make the following day and they were intent on going to bed very early. The other memorable event from my first AHS Annual Forum was a post-Forum dinner in downtown

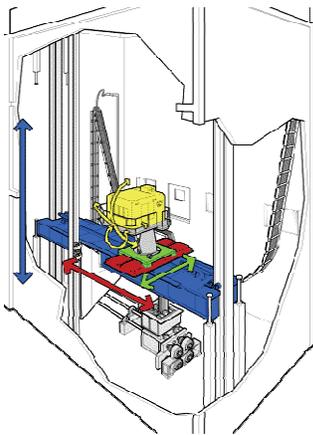
New Orleans. Lloyd Corliss and Vic Lebacqz had somehow (before the days of Google) found a fancy restaurant that had an upstairs dining room with a great view, over the levee, of the Mississippi river. With the Army, NASA, and Calspan contingent, we probably had 15 to 20 people in our party for dinner. After a very good dinner, with plenty of libations, the \$2,000-plus bill arrived. I was shocked. This was my first encounter with “add in the tip and divide the bill by the number of people in the party.” I think individual bills were in the \$120 ballpark—far above the government per diem! However, looking back, it was well worth it to dine with this distinguished group and hear some great aeronautical research stories.

Returning to Ames in May 1981 after the AHS Annual Forum, I was in wedding mode. Lori and I were married on August 15th. Several folks from the Army Lab attended our wedding: Irv and Renée Statler, Dave and Pam Key, Ed Aiken, Linda and Bob Ormiston and their daughter Wendy, Steven Pucci, Bob and Lorraine Gaines, Kathy Yaggy, and Bill Eckert. It was a great day and afterwards we took a couple weeks of vacation in Hawaii.



**Mr. and Mrs. Chris Blanken,
August 15, 1981.**

Nineteen eighty-two was a turning point in my career. Dave Key had initiated an effort to update the military handling-qualities specification, MIL-H-8501. The Army project engineer for this effort was Dean Carico. In 1982, Dean decided to leave the Army and move to Patuxent River, Maryland, to work for the Naval Air Systems Command (NAVAIR). His move was to my benefit—Dave asked me to take over Dean’s duties on the 8501-update effort. This initial update effort was a competition between Calspan and Systems Technology, Inc. (STI) to develop a new format and structure for MIL-H-8501. Chic Chalk and Bob Radford were the lead engineers for Calspan, and Warren Clements and Wayne Ferguson were the lead engineers for STI, with help from Roger Hoh and Dave Mitchell from the STI-Hawthorne office. After Calspan and STI presented their 8501-update proposals at the Army headquarters in St. Louis, Missouri, the STI format was chosen to go forward.



NASA Ames Vertical Motion Simulator (VMS).

As the actual language in the 8501 update was primarily being worked on by Roger Hoh and Dave Mitchell from STI, the Army and NASA engineers at Ames were busy generating a handling-qualities database to support criteria development.

In 1982, I helped Ed Aiken and John Hemingway (a NASA engineer from the helicopter/vertical takeoff and landing (VTOL) human factors office) conduct a simulation to investigate the effects of an isometric side-stick controller on pilot workload while flying a helicopter in low-altitude terrain flight.

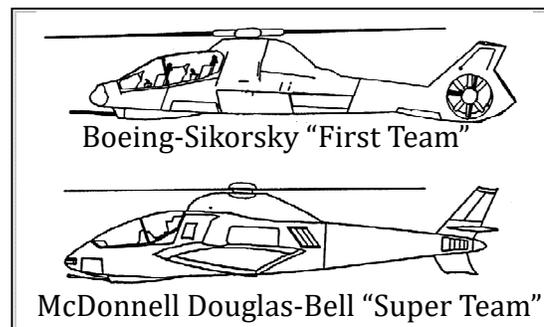
In 1983, I became involved in the first of many helicopter simulations on the newly constructed NASA Ames Vertical Motion Simulator (VMS). One of my first assignments on the

VMS was helping Lloyd Corliss with a helicopter thrust response simulation. Lloyd was not only a great technical mentor, but also a “political” mentor. He helped me established my rapport with the VMS simulation engineers, like bringing in food and goodies during the simulation to keep everyone in good spirits and holding an “after-sim” party to thank all the VMS engineers who contributed. At that time, the Army paid NASA a set amount for all the Army simulations conducted throughout the year. The VMS was on double-shift schedule, with a day shift from 8:00 a.m. to 4:30 p.m. and a swing shift from 4:30 p.m. to midnight. Simulations were usually 4 to 5 weeks long with about half of the simulation conducted during the day shift, and the remaining half during the swing shift (allowing the other simulation to have some time during the day shift). It was always a challenge to switch your work and sleeping schedules to and from swing shift.

I was involved in several other VMS simulations toward generating a handling-qualities database for the 8501-update effort. These included a helicopter roll-control simulation, a simulation to explore longitudinal thrust vectoring for air-to-air combat, a helicopter height-control-criteria simulation, and a pitch-roll cross-coupling simulation. In 1984, I presented a paper on the “Effects of Engine Thrust Response on Handling Qualities” at the Army Science Conference at West Point. It was a very memorable conference, and I enjoyed being able to walk around the campus in my free time.

During this time, Dave Key hired several new engineers; Hossein Mansur and Matt Whalley were both hired in 1984 from Cal Poly, San Luis Obispo. (After their interviews, I remember them calling me at work and asking if they would have to wear Army uniforms if they came to work for the Army at Ames.) Several others were hired along the way including Michelle Eshow, Mike Lewis, Court Bivens, and Bob Lemble. It was also during this time that Dr. Fred Schmitz formed a simulation group, and hired Mark Tischler and Adolph Atencio. Eventually, we were all merged together under Dave Key, in the Aircrew-Aircraft Systems Division. Fred went on to lead the Fluid Mechanics Division and, eventually, the 40- by 80-Foot Wind Tunnel facility.

In 1983–1984, the 8501-update effort became focused on the Army’s Light Helicopter Experimental (LHX) program and handling-qualities requirements for a scout/attack helicopter. The results from this update effort were adopted by the Army Aviation Engineering Directorate as Aeronautical Design Standard 33 (ADS-33), published in May 1987. This was the first of several versions of ADS-33. ADS-33 was used in the LHX source selection process between the two competing designs: one from Boeing-Sikorsky (the “First Team”) and another from McDonnell Douglas-Bell (the “Super Team”). This program became the RAH-66 Comanche reconnaissance/attack helicopter.



LHX competitors.

The Army-NASA relationship afforded many cool things. For example, drawing on their past relationship with Calspan, Ed Aiken and Vic Lebacqz were able to persuade Calspan to send its variable-stability Learjet up to Ames after visiting the Air Force Test Pilot School at Edwards



Chris Blanken next to the Calspan variable-stability Learjet.

Air Force Base. Rides were provided to the Army-NASA flight dynamics/handling-qualities engineers. I remember my flight was with Bill Decker (and the Calspan test pilot). I was able to see variations in the longitudinal dynamics, and Bill had the lateral-directional dynamics. The photo on the left was taken before my flight. During the Learjet flight I discovered the importance of the inceptor force-feel system and its impact on handling qualities. The Calspan pilot changed the aircraft dynamic response to pilot input,

making it difficult to fly. He then returned the dynamic response to a good-flying aircraft, but degraded the inceptor force-feel dynamics. The aircraft also flew equally poorly, and I could not tell if the difficulty was due to degraded inceptor characteristics or the poor dynamic response. The flight was enlightening and was only available through our great Army-NASA collaboration.

NASA also had a flight operations group, with a couple rotorcraft and several fixed-wing aircraft. The Flight Operations Branch and Flight Dynamics and Controls Branch worked to provide an introductory flight for the Army/NASA rotorcraft engineers in either the UH-1 Huey or the AH-1 Cobra helicopters. I was able to go up in the Cobra. It was another enlightening experience, from the vibration, to the noise, to the difficulty to control. There was also a high-altitude research group, which had access to several U-2 aircraft. It was always exciting to watch the U-2 take off, usually at 11:00 a.m. as they had to plan and clear the airspace around Moffett Field for takeoff. As our office was near the airfield, we usually needed to halt telephone conversations for a minute or so until the U-2 reached an altitude where the loud engine exhaust signature was somewhat minimized.



NASA Ames AH-1S Cobra helicopter.

A few other benefits to Army employees co-located with NASA included the magnificent NASA Ames Library; a health unit that provided free annual physicals; the NASA Ames Travel Office; the NASA Ames Graphics and Exhibits Branch, and the Publications Branch; and being able to participate in NASA-sponsored clubs (e.g., the NASA Ames Photo Club) and activities (e.g., the NASA Ames Bowling or Basketball League). I recall that you could call the Ames Travel Office and in a few minutes tell them when and where you needed to travel. In a day or so, they would call you back with all of your flight, hotel reservation, and rental car information. A few days before the trip, they would call you again, saying they had your airline tickets and travel paperwork. You could fill out the voucher on the plane ride home, mail it in the next business day, and be done. (Now, the travel office is gone, and the engineers must make their own travel reservations through a government travel website, sometimes taking hours to complete—and they call this progress.) The Ames Graphics and Publications Branches were quite phenomenal.

In the 1980s we did not have personal computers for writing reports and making figures; we took handwritten text on engineering paper to the publications staff to type and format. We took our pencil drawings to the graphics staff, and they turned them into publication-quality figures. Sometimes these figures were then turned into 35-mm slides for presentations at major conferences or meetings (we always had the best slides at meetings). And I still remember a great sign that hung on a wall behind the counter in the Graphics and Exhibits Branch that read: “Your lack of planning does not constitute an emergency on our part.” In other words, consider the Graphics Branch in your time management allotments. I have many fond memories of participating in the NASA Ames-sponsored basketball league. Through another Army employee, Steven Pucci, I was fortunate to join a team called the Beer Barrels, made up of engineers and researchers at Ames. We won the league trophy a couple of times in the early to mid 1980s.



Members of the 1985 Beer Barrels basketball team. Standing, left to right: Doug Sweet, Ron Langhi, Mike Lewis, and Dan Door. Kneeling, left to right: Chris Blanken, Dan Kojiro, and Jerry Johnson.

Another very cool thing was being co-located on a Navy base. Moffett Field had a golf course, a swimming pool, a gymnasium for basketball, a Baskin Robbins ice cream parlor, and an annual air show. The air show usually had the Blue Angels flight team, along with many single-aircraft performances and static displays. The aircraft typically arrived at Moffett a day or two before the formal weekend air show. On the Friday before the show, the performers, including the Blue Angels, would practice their routines. This was a great day to be an Ames employee; the public was not yet allowed on base so the employees had front-row seats, with little or no crowds, to watch the practice for the air show. For an aviation enthusiast, this was most exciting.

Nineteen eighty-five was a memorable year. Through the Army-NASA Joint Agreement, I was able to participate in the NASA-Stanford co-op program that allowed Ames engineers to take classes at Stanford. I started taking one, or sometimes two, classes per quarter in 1981. By 1985, I had taken a sufficient number of classes to earn an M.S. degree in aeronautical engineering. Also in 1985, the Request for Proposal for the Army’s LHX was released. Source selection meetings were held in old Army barracks in Granite City, Illinois.

Epilogue 1986–2017 . . .

In 1986 I became a participant in the U.S.-German Memorandum of Understanding (MOU) on Helicopter Aeromechanics. The MOU was a bilateral agreement between the U.S. Department of Defense and the German Ministry of Defense that allowed the Army and the German Aerospace Research Establishment (DLR) to perform cooperative rotorcraft research. I was astutely

mentored by Ed Aiken, the U.S. principal investigator for the handling-qualities task within the MOU. Ed, along with NASA experimental test pilot Ron Gerdes, had participated in DLR-led flight tests in their variable-stability S-3 helicopter (based on a Bo-105 helicopter). Under the MOU umbrella, Jürgen Pausder, the German principal investigator for the handling-qualities task, and I became good friends, and we performed some very timely research on the effects of bandwidth and time delay on handling qualities, and the effects of pitch-roll cross coupling on handling qualities. Several joint conference papers and *Journal of the AHS* articles resulted from this collaborative work. Our wives also became good friends, especially during our 6-month stay in Germany in 1996.



Ron Gerdes (left) and Ed Aiken (right) with the DLR S-3 (mid-1980s).

In the late-1980s to early-1990s, I became highly involved in the AHS, serving as the Arrangements Chairperson and the Treasurer for the local San Francisco Bay Area chapter. In 1988, I was also the Chairman of the AHS Handling-Qualities Technical Committee, session chair for the Handling-Qualities technical session at the Forum, and a session chair for the Liverpool Handling-Qualities Specialist meeting in Liverpool, England.



U.S.-German MOU meeting, 1986. Kneeling, from left: Chris Blanken, Dave Key, Richard Boitnott, Dewey Hodges, Bernd Gmelin, and Jürgen Kaletka. Standing, from left: Ken Landis, Larry Dooley, Chris Kindervater, Dieter Petersen, Andy Kerr, and Jürgen Pausder.

the database for criteria development. Dave Key's Flight Control Branch became part of the Simulation and Aircraft Systems Division, managed by Terry Gossett. The Army Aviation Engineering Flight Activity (AEFA) at Edwards Air Force Base conducted assessments of ADS-33 using an Apache and a Chinook helicopter. Dave hired Dan Hart, and we worked together on

Nineteen eighty-nine was a memorable year, as the U.S. contingent of the U.S.-German MOU group was in Germany for our semi-annual meeting. I'll never forget coming downstairs in the hotel to the breakfast area and seeing Berend van der Wall holding a newspaper with a photo showing that the Berlin wall had been opened. This was a historic day that would change the future. I recall that our Director, Dr. Richard Carlson, had "unknowingly" booked his return air travel through Berlin, with the weekend stay in Berlin prior to flying back to the U.S. I think Dr. Carlson was able to see the celebration up close and personal.

In the 1990s, we continued to perform handling-qualities experiments to expand

several projects including the Chinook flight test at Edwards. In the mid-1990s, there was a lot of work and interaction with the DLR-Institute of Flight Systems in Braunschweig, Germany, under the U.S.-German MOU. The Army experimental test pilot co-located in the NASA Ames Flight Operations Branch, Major Rick Simmons, and I participated in several DLR-managed flight tests using their variable-stability Bo-105 S-3 research aircraft. This included a very successful bandwidth and time delay handling-qualities study that encompassed a wide range of initial configurations flown and assessed using the NASA Ames VMS. Then key points were flown and assessed using the S-3 aircraft. A similar approach was used for a subsequent pitch-roll cross-coupling investigation. The data were used to improve and revise the handling-qualities criteria and boundaries in ADS-33. Many collaborative reports covering this work were presented at prestigious international rotorcraft forums and published in technical journals. In 1996, I competed for, and won, an Army research grant that allowed me (and my family) to move to Germany for 6 months (see Lori Blanken's chapter for a family photo while we lived in Germany). I worked at the DLR Institute of Flight Systems in Braunschweig, Germany. This was the same group of engineers I had worked with on the U.S.-German MOU since 1984, so it was a great experience, both technically and culturally, for me and my family.

In 1998, in addition to my normal handling-qualities engineer duties, I assumed the duties of Aircraft Manager for our instrumented Black Hawk helicopter, tail #748. It was an interesting experience to be so "close" to a helicopter, being responsible for all the work orders related to research equipment modifications made on the aircraft.

In 1999, Lieutenant Colonel Chris Sullivan and I led an assessment of ADS-33 using the instrumented UH-60A Black Hawk (#748) from the Army's Flight Project Office at Ames. The assessment was conducted with the aircraft configured empty, an internal ballast, and an external slung load. Assessments were conducted over the taxiways and runways of Moffett Field and included evaluations from a variety of experimental test pilots. We were fortunate to have access to the NASA and Army experimental test pilots at Ames (George Tucker, Munro Dearing, Lieutenant Colonel Chris Sullivan, and Major Dave Arterburn) but we also wanted to get some Navy input for diversity. Lieutenant Colonel Sullivan was able to get the Navy Test Pilot School flight instructors and classroom instructors to participate. With this wealth of experience, it was an enlightening and fruitful assessment, which became a key ingredient in the latest update to ADS-33 (ADS-33E-PRF, March 2000).



UH-60A with external slung load performing ADS-33 lateral reposition maneuver.

In 2000, Dave Key retired. His group of engineers had merged with the NASA flight dynamics group when the Army/NASA Rotorcraft Division, led by Ed Aiken, was formed in 1997. Dr. Michael Rutkowski (Army) was the Deputy Division Chief, Dr. Yung Yu (Army) was the Senior Scientist, Linda Vollenweider was the Administration Specialist, and Tracie Forrette was the division secretary. At the branch level, Barry Lakinsmith (Army) was the Flight Control and Cockpit Integration Chief, Dr. Jeffrey Schroeder (NASA) was the Deputy Chief, and Genoveva

Rosales was the secretary. The “three musketeers”—Linda Vollenweider, Tracie Forrette, and Laura Iseler—kept the place running efficiently and always upbeat and fun. With the merger of all these like-minded engineers working on high-level, top-priority research topics related to single-main-rotor helicopter flight dynamics and control problems, it was a fun time to be an engineer at Ames. Following Dave Key’s retirement, I assumed the duties of U.S. Project Lead for the U.S.-German MOU. In addition, I joined a North Atlantic Treaty Organization (NATO)/Research and Technology Organization (RTO) panel on Control and Handling Qualities, which allowed travel to meetings in several interesting places: National Research Council in Ottawa, Canada; the Netherlands; and Berlin. Also in 2000, I won the Army’s 1999 Research and Development Achievement (RDA) award. It was a great honor.

In 2005, the great Army/NASA Rotorcraft Division was broken apart. The Army folks moved to Modular Building T12-B and the NASA folks stayed in Building N-243. The Army engineers and human factors folks formed the Flight Control and Cockpit Integration Division in T12-B under Barry Lakinsmith, and Rey Pasion was the Administrative Assistant.

In 2007, my Army career changed direction again when Andrew Kerr, Director of the Ames-Army Lab, now the Aeroflightdynamics Directorate (AFDD), and the Deputy Director, Wayne Mosher, retired from federal service. This caused a ripple effect. My Division Chief and Supervisor, Barry Lakinsmith, was asked to move up to take Mr. Kerr’s place while the Army searched for a new permanent replacement. Once a permanent director was in place, Barry would become deputy director. I was asked to take over running the Flight Control and Cockpit Integration Division. The division comprised a great collection of engineers and research psychologists, divided into several groups: the Flight Control group was led by Dr. Mark Tischler; the Human Systems group was led by Jay Shively; the RASCAL helicopter group was led by Jay Fletcher; and the Autonomous Rotorcraft Project group was led by Matt Whalley.

In 2014, Dr. William Lewis took over as the Director of the Army Aviation Development Directorate (ADD) at Redstone Arsenal, Huntsville, Alabama. ADD is the headquarters for the Ames-Army Lab, AFDD, and the Army lab at Ft. Eustis, Virginia, the Aviation Applied Technology Directorate (AATD). Dr. Lewis reorganized the technical structure of the laboratories so that the Flight Control and Cockpit Integration Division was divided into two technical areas: the Vehicle Management and Control (VM&C) technical area (where I remained as the supervisor); and the Human System Integration technical area (led by Ernie Moralez).

In 2016, I had the privilege of being nominated and selected to be a Technical Fellow of the AHS. I owe a great debt to Dr. Mark Tischler for the excellent nomination package and all of his hard work toward obtaining endorsement letters from industry, academia, NASA, the Navy, and international partners. It was a tremendous honor to be selected for this prestigious award. The awards ceremony at the 2016 AHS Annual Forum in West Palm Beach, Florida, was truly memorable.



AHS Technical Fellow Award.



2016 VM&C group photo with the RASCAL helicopter, the VMS, and Hangar 1 in the background. From left: Luigi Cicolani, Ondrej Juhasz, Jeff Lusardi, Hossein Mansur, Mark Tischler, Kevin Kalinowski, Chris Blanken, Oliver Fields, Marc Takahashi, Marcos Berrios, Greg Schulein, Rhys Lehmann, Brian Fujizawa, Christy Ivler, Joey Wagster, Tom Berger, Eric Tobias, Kenny Cheung, Jonathan Soong, Dexter Hermstad, Geoffrey Jeram, and Robert Clark.

In 2017, at the writing of this memoir, I am still the lead for the VM&C technical area. The group continues to produce high-quality research in the areas of rotorcraft handling qualities, modeling and simulation, system identification, flight control design and analysis, in-flight simulation, and autonomy (with emphasis on algorithms for autonomous obstacle field navigation and safe landing area determination).

The VM&C technical area is involved in the Future Vertical Lift program and the Science and Technology (S&T) effort (Joint Multi-Role Technical Demonstrator (JMR-TD)) to reduce risk. The JMR-TD aircraft are a lift-offset coaxial rotor helicopter (called the Defiant, SB>1) by Sikorsky and Boeing, and an advanced tiltrotor by Bell Helicopter (called the V-280 Valor). These vehicles should fly within the next year. It is an exciting time to be an aeronautical engineer working for the Army!



Sikorsky-Boeing Defiant SB-1.



Bell V-280 Valor.

Index

A

Aeroflightdynamics Directorate (AFDD), 11
 Aiken, Ed, 3-6, 9-10
 Allan Hancock Junior College, 1
 American Helicopter Society (AHS), 4-5, 9, 11
 Ames Research Center, 1
 Ames, 1, 3-8, 10-11
 Army Aviation Development Directorate (ADD), 11
 Army Aviation Engineering Flight Activity (AEFA), 9
 Arterburn, Dave, 10
 Atencio, Adolph, 6
 Aviation Applied Technology Directorate (AATD), 11

B

Baca, Pam, 3
 Back-Up Control System (BUCS), 3-4
 Bengford, Norm, 4
 Berger, Tom, 12
 Berrios, Marcos, 12
 Bivens, Court, 6
 Blanken, Chris, 1, 9, 12
 Blanken, Lori, 10
 Boitnott, Richard, 9
 Borgman, Dean, 1, 3

C

Carico, Dean, 3, 5
 Carlson, Richard, 9
 Chalk, Charles “Chic,” 4-5
 Chen, Bob, 4
 Cheung, Kenny, 12
 Cicolani, Luigi, 12
 Clark, Robert, 12
 Clements, Warren, 5
 Coe, David, 1
 Cooper–Harper Handling Qualities Rating Scale, 4
 Corliss, Lloyd, 3, 5-6
 Cross, Lori, 3

D

Dearing, Munro, 10
 Decker, Bill, 7
 Delta Sigma Phi, 1
 Dooley, Larry, 9
 Door, Dan 8

E

Eckert, Bill, 5
 Edwards Air Force Base, 6-7, 9-10
 Eshow, Michelle, 6

F

Ferguson, Wayne, 5
 Fields, Oliver, 12
 Fletcher, Jay, 11
 Forrette, Tracie, 10-11
 Franklin, Jack, 3
 Fujizawa, Brian, 12

G

Gaffney, Betty, 3
 Gaines, Bob, 5
 Gaines, Lorraine, 5
 Gerdes, Ron, 9
 German Aerospace Research Establishment (DLR),
 8-10
 Gmelin, Bernd, 9
 Gossett, Terry, 9

H

Harper, Bob, 4
 Hart, Dan, 9
 Hemingway, John, 5
 Hermstad, Dexter, 12
 Hodges, Dewey, 9
 Hoh, Roger, 5

I

Iseler, Laura, 11
 Ivler, Christy, 12

Blanken, C.

J

Jeram, Geoffrey, 12

Johnson, Jerry, 8

Joint Multi-Role Technical Demonstrator (JMR-TD),
12

Juhasz, Ondrej, 12

K

Kaletka, Jürgen, 9

Kalinowski, Kevin, 12

Kerr, Andy, 9, 11

Key, David, 3-6, 9-11

Key, Pam, 5

Kindervater, Chris, 9

Kojiro, Dan, 8

L

Lakinsmith, Barry, 10-11

Landis, Ken, 9

Langhi, Ron, 8

Lebacqz, Vic, 4-6

Lehmann, Rhys, 12

Lemle, Bob, 6

Lewis, Mike, 6, 8

Lewis, William, 11

Light Helicopter Experimental (LHX) program, 6, 8

Lusardi, Jeff, 12

M

Mansur, Hossein, 6, 12

Memorandum of Understanding (MOU), 8-11

Merrill, Bob, 4

Mitchell, Dave, 5

Moralez, Ernie, 11

Mosher, Wayne, 11

N

NASA Ames, 1, 3-5, 7-8, 10

Naval Air Systems Command (NAVAIR), 5

North Atlantic Treaty Organization (NATO), 11

O

Ormiston, Linda, 5

Ormiston, Robert, 3, 5

P

Pasion, Rey, 11

Pausder, Jürgen, 9

Petersen, Dieter, 9

Pike, Bob, 3

President Jimmy Carter, 2

Pucci, Steven, 5, 8

R

Radford, Bob, 4-5

Research and Development Achievement (RDA), 11

Research and Technology Organization (RTO), 11

Rosado, Joel, 4

Rosales, Genoveva, 10-11

Rutkowski, Michael, 10

S

Schmitz, Fred, 6

Schroeder, Jeff, 10

Schulein, Greg, 12

Science and Technology (S&T), 12

Shively, Jay, 11

Simmons, Rick, 10

Soong, Jonathan, 12

Spanos, Gary, 3

Statler, Irv, 5

Statler, Renée, 5

Sullivan, Chris, 10

Sweet, Doug, 8

Systems Technology, Inc. (STI), 5

T

Takahashi, Marc, 12

Tischler, Mark, 6, 11-12

Tobias, Eric, 12

Tucker, George, 10

V

van der Wall, Berend, 9

Vehicle Management and Control (VM&C), 11-12

Vertical Motion Simulator (VMS), 5-6, 10, 12

Vertical takeoff and landing (VTOL), 5

Vollenweider, Linda, 10-11

W

Wagster, Joey, 12

Whalley, Matt, 6, 11

Y

Yaggy, Kathy, 5

Yu, Yung, 10

The Gift of Flight, 1980–1989

Lorene (Lori) Cross Blanken Army

I have been interested in things that fly since I was a young girl. In December 1979, for my 21st birthday, my parents gave me the wonderful gift of flight. The morning of my first lesson dawned foggy with some drizzle, but I arrived at South County Airport in San Martin, California, with excitement and great anticipation. My instructor told me that birds don't even fly in that kind of weather! He showed me around, told me a few stories, and then the sun came out. We climbed into a Piper Tomahawk, taxied to the end of the runway, and up we went. He gave me the controls, and I found myself in a new and beautiful world. The flying bug had bitten me!



**Lori Cross, 1980,
West Valley College
Graduation.**

It was during this time that I completed my A.A. degree in Business Administration at West Valley Junior College and an internship at IBM. I sent my resume out to many companies and received several good offers but one stood out: an Army aviation group at Moffett Field was interested in me. In February 1980 I accepted a position with the U.S. Army Aeromechanics Laboratory and it felt like the perfect fit.

As secretary to Bob Ormiston's Rotorcraft Dynamics Division, with a dozen or so research engineers, I typed a lot of technical reports. This was a difficult task at first. The heavy stack of engineering paper that was fastened with a thick, black binder clip looked quite daunting sitting in my in-box. Flipping through it I discovered just how tiny the engineers could write, and the equations had symbols I had never seen before! The secretarial staff used IBM Mag Card II typewriters. A magnetic card was inserted into a typewriter that had space for up to 8,000 characters. These would be stored in its electronic memory. There were multiple typeball elements that could be interchanged depending on the required type font (e.g., Times New Roman), type style (e.g., bold italic), type size (e.g., 12 point), and scientific notation. The typeball would be changed each time there was a change in the font, style, size, or notation. The need to change the typeballs frequently, sometimes multiple times in the same sentence, was tedious and really slowed down the work. Over time, I looked forward to the challenge and became proficient at it. I thought it was so sweet of Bill Bousman to bring me flowers from his garden whenever he left one of his papers in my in-box.

I complained a few times to my mother about how long the commute was from my house near IBM on Cottle Road in San Jose to Moffett Field. She was an elementary school principal, and she knew the father of a family that lived near me who also worked at Moffett Field. I began commuting with Marty Maisel in his old Chevy Impala with a three-speed shift on the column, and I learned all about the new aircraft he was working on called the XV-15 Tilt Rotor Research Aircraft.

While I was sitting at my desk one day, minding my own business, rubber bands started to fall all around me. I looked up to find Seth Dawson lying prone in the hallway just outside of my door, shooting rubber bands at me. About that time, I saw a very familiar figure come around the

Blanken, L.

corner and head in our direction. Without missing a beat, Dr. Statler said, “Good afternoon, Mr. Dawson,” as he continued past Seth. I thought Seth was going to die! Dr. Statler greeted me and proceeded into Bob’s office as if nothing had happened!

My office partner was Pam Baca, secretary to Dave Key and the Flight Control Division. She took me under her wing and taught me the ins and outs of the place. I would have been lost without her. In mid-April 1980, Pam mentioned that Dave had a new hire who was over in the personnel office and would be checking in soon. She was going to the other end of the building and asked me to keep an eye out for him. Sure enough, he showed up while Pam was out, and we introduced ourselves. Because Dave would be in meetings all afternoon, he asked that his new hire, Chris Blanken, return the next morning. Little did I know that in exactly 1 year, this new guy would propose marriage to me and I would say yes!



Chris Blanken and Lori Cross, August 1980.

Bob Ormiston decided to play matchmaker with Chris and me. On occasion, Bob would walk over to Baskin Robbins in the afternoon for a frozen treat. He asked if I wanted to walk over with him and I said, “Sure.” Then, as if an afterthought, he asked Chris if he wanted to join us. Chris said, “That sounds great,” and we took off for the ice cream parlor. Chris and I were trying very hard to sound natural and act normal because we had actually been dating for several months and were trying to keep it incognito. Later, when we got engaged, Bob was convinced he was the one who brought us together. Chris and I hated to burst his bubble but I guess the jig is up now!

NASA Ames had a variety of programs and activities that were available to anyone who worked on base. I find it humorous that today big tech companies in Silicon Valley offer all kinds of employee perks and seem to think they are doing something new and innovative. Well, I think NASA Ames and the Navy had the jump on them 40-plus years ago; they have offered some awesome perks that tech companies would be hard pressed to duplicate today.

Air shows were a particular favorite employee perk of mine. The best shows were on Friday afternoon before a weekend air show when all of the aircraft flew in and only employees were allowed on the flight line. The pilots were doing a bit of practice for the upcoming show, but they were also showing off for their fellow pilots. I was not, however, prepared for the patriotism I felt when I drove onto the base one morning and discovered Air Force One sitting on the tarmac. It was an impressive sight that took my breath away.

NASA hosted a series of guest speakers who were always interesting and enlightening. One that stands out as a favorite was Story Musgrave. He is a retired physician and space shuttle astronaut who was very informative and extremely funny. His opening story was *All you wanted to know about space travel but were too embarrassed to ask*. He had everyone rolling in the aisles with laughter. I was disappointed when his speech ended!

NASA Ames also has a very nice exercise gym open to all employees, a health unit that gives all employees a complete physical every year around their birthday, and a childcare center.

The Navy side had quite a few perks too: a basketball gym, a swimming pool, the infamous Baskin Robbins, a U.S. Post Office, a park, a golf course, and a flying club.

In the summer of 1981, Dr. Statler, the Director of the Aeromechanics Laboratory, asked if I would consider filling in for his long-time secretary, Lynda Jones, while she was out on maternity leave. I said yes, of course, but was actually terrified. He and Lynda had the kind of rapport that came from many years of working together, and she knew what he wanted and needed before he ever had to ask. The learning curve was going to be steep, and I would need to pay close attention. Being an executive secretary brought me into contact with a lot of new people and duties. I was now interfacing with NASA management, heads of the helicopter industry, foreign diplomats, scientists and engineers from almost all of the North Atlantic Treaty Organization member nations, the Army's Inspector General, and even a two-star U.S. Army General.

I helped Dr. Statler organize and host a U.S.-France Memorandum of Understanding (MOU) meeting and dinner. It turned out to be a valuable lesson, as years later I would assist my husband in his duties as the U.S.-German MOU Technical Project Officer. In the spring of 1996, by some miracle, Chris and I, and our two very young sons, went to Germany to live for 6 months under the auspices of the U.S.-German MOU. Chris' German counterpart was Jürgen Pausder. We forged a friendship with many of the Germans, but Jürgen and his wife, Wallau, became very close and dear to us. We still communicate with them and visit each other whenever the opportunity presents itself.



Chris, Lori, Matt and Miles, Goslar, Germany, 1996.

Working for Dr. Statler helped me make the jump, in 1983, out of the secretarial field and into a management analyst position in the Army Research and Technology Laboratories (RTL) with Dr. Carlson as Director. There were a lot of secretaries back then, but only a small handful of us were executive secretaries that worked for a Senior Executive Service-level director. I believe Rick Parnell, who headed up RTL's Management Support Office, hired me because of my experience and skills but also because I was a pilot. Rick had flown C-130s and once he saw my pilot's license listed on my resume, we didn't talk about too much else! I became Dr. Carlson's Security Manager, and it was in this capacity that I collaborated with the NASA Ames Security Office to discuss the handling of classified materials, projects, and meetings.

The R&D Coordinator, Lieutenant Colonel Tom Almojuela, was a good friend, and he stopped by my office one day and asked me if I was on the flight list, was free tomorrow, and if I wanted to go flying with him. I said, "Yes, yes, and yes!" We walked down to the flight line and climbed into a UH-1 Huey helicopter. As he got the beast started, I was amazed at the shaking and the absolute cacophony of noise. I tried to talk to him and realized I would need to scream for him to hear me. He started to laugh and showed me where the COM buttons were located. I now understood what all the fuss was about. Some of our engineers were working to make our rotorcraft quieter and have less vibration, and I could now see why it was so important. Tom took us out to the "tomato patch" at the north end of the runway and performed a hover and a

Blanken, L.



Matt, Miles, and Chris Blanken visiting the VMS, 2004.

hovering turn. He asked me to give it a try and told me of a phenomenon that happens during the turn with winds; the helicopter needs a little extra yaw “push” to complete the 360-degree turn. I successfully performed a hover and then a 360-degree turn in both directions. We headed over to the coast and Tom let me do a little flying. It was a fascinating, enlightening, and valuable lesson that gave me a better understanding of what our group of engineers and scientists were trying to accomplish.

I left the Army in August 1989 when Chris and I started our family. Our two sons are currently at NASA Ames. Matthew, our eldest, is a Simulation Engineer working on the Vertical Motion Simulator (VMS), FutureFlight Central, and Crew Vehicle Systems Research Facility. Miles, our youngest, works part time for Indev, a small business providing air traffic management services, and volunteers to help support the Airspace Technology Demonstration 2 (ATD) project. I guess the apple really doesn’t fall far from the tree.

Index

A

Almojuela, Tom, 3,4

B

Baca, Pam, 2

Blanken, Chris, 2-4

Blanken, Matthew, 3-4

Blanken, Miles, 3-4

Bousman, Bill, 1

C

Carlson, Dick, 3

D

Dawson, Seth, 1-2

J

Jones, Lynda, 3

K

Key, Dave, 2

M

Maisel, Marty, 1

Memorandum of Understanding (MOU), 3

Musgrave, Story, 2

O

Ormiston, Bob, 1-2

P

Parnell, Rick, 3

Pausder, Jürgen, 3

Pausder, Wallau, 3

R

Research and Technology Laboratories (RTL), 3

S

Statler, Irv, 2-3

V

Vertical Motion Simulator (VMS), 4

Personal Recollections of the Ames-Army Laboratory in the 1970s

Frank Caradonna
Army

In the Beginning...

Almost my entire working life has revolved around rotorcraft and fluid mechanics, and it's been a great experience; there was so much to discover and so much fun doing it. This came about mainly because of my good fortune in having worked for over 40 years at the Ames-Army Lab—a unique place that, for its intellectual, material, and personal support, should be better known than it is as a model for an effective and productive research environment, and a government research environment at that. The essence of the Lab was already in place when I arrived, so I'm not the best person to discuss how one builds such a place or the essential ingredients, but I do have some ideas. I recall Irv Statler's parting words when retiring from the Lab 30 years ago. He said something like, "It's not enough to just have money and great facilities; if you don't have the right people, you don't have anything." I do think that people were the key ingredient to the Ames-Army Lab and, in the following, I'd like to confine myself to my earliest memories of the Lab and give a personal recollection of some of what we did and especially the people who made it happen.



Frank Caradonna

My first contact with the Ames-Army Lab occurred over 50 years ago at the end of the summer of 1965. I was then a summer employee at NASA Ames in Building N-210 working with the Manned Spacecraft Simulation Branch. As the time approached for my return to classes at Stanford, I heard that there was a new Army research organization at Ames. This was of interest to me, since I had an Army Reserve Officers' Training Corps (ROTC) commitment, as did many of us in those days. So, I went over to Building N-215 and spoke to one of the Army people; I don't recall who it was, probably Paul Yaggy or Andy Morse. I don't think I gave much thought to this little group until about 4 years later, when the war in Vietnam had become very hot and I was about to enter active duty. It seemed wise to see if those vaguely remembered Army folks still existed. So, I phoned them, and, though I can't recall the conversation or who I spoke to, something must have happened, as my name was put on a list for assignment to Ames.

By June of 1970, I had completed 6 months of basic officer and mechanical maintenance training at Aberdeen Proving Ground and was ordered to duty at the Army Aeronautical Research Laboratory (AARL) at Ames Research Center. It was a very hot, humid summer in Maryland, so it was with delight that Monika and I loaded our old Chevy II and headed back west. About a week later, I reported for duty, early in the morning and in uniform, to Building N-215 at Ames Research Center. On reaching the top of the stairs, I was nearly knocked over by a woman on a unicycle! It was Georgene Laub, one of the engineers, practicing her skills by using the hallway walls to aid her balance. This, I thought, was a somewhat different kind of Army than I had previously known.

I had arrived early and Georgene was making the most of the then-empty building. Soon, the unicycle was stowed and the building filled with proper, business-like people. It was then that I met Andy Morse, who would be my boss, and friend, for well over the next 20 years. Andy assigned me an office with three other engineers, and handed me a number of rotorcraft articles to read. I knew next to nothing about rotorcraft at the time. He showed me the various facilities, including the computer room and wind tunnel. I recall the tunnel as a beehive of activity and I was impressed. He introduced me to the staff, which included an active duty Army representative, Major Gordon Berry. He was a fine officer and person, but I suspect this wasn't an easy assignment for him—he was “regular Army” and not accustomed to the herding-of-cats (i.e., basic research people) atmosphere. I especially remember being introduced to Paul Yaggy, the Director who succeeded Colonel Cyril Stapleton. Paul was a no-nonsense person, and I still recall his stern admonition that we were there to do serious business—I took this very seriously.

Finding a Direction

I didn't realize then that the Lab's biggest task at that time was to find its role in the world. Its marching orders were to find and fix the many aeromechanical problems of rotorcraft, but at that time there was only a vague idea of what those problems were, their priority, or what to do about them. That summer, it was Andy who slowly helped me to realize that I was being tasked with finding something—he didn't know what—but something important that would make a difference to the Army and to rotorcraft in general. He didn't speak then in such ambiguous terms, as I wasn't ready for it. But it wasn't all “clouds and air” with Andy; his entire background had been as a hard-nosed industrial engineer and he understood the importance of clear, down-to-earth problem solving. I still marvel at his ability to balance futuristic thinking with the everyday working necessities of a technical organization. I recall that one of the articles that Andy gave me was called *The Future of Rotorcraft* (Raoul Hafner, *The Aeronautical Journal*, Vol. 72, No. 696, Dec. 1968). He was clearly trying to educate me about rotorcraft and to think about the future. We had a few rotorcraft veterans in the Lab beside Andy, and I learned a lot from listening to them. I also started to spend a lot of time in the wonderful NASA Ames Library, to which I had free access under the Army-NASA Joint Agreement.

I recall learning about the twin problems of dynamic stall and transonic flow, both of which limit high-speed rotorcraft performance. I became intrigued with the then-intractable problem of predicting transonic rotor-tip flow, and I started to read the latest literature. At that time, there was a considerable mathematical controversy over the possibility of achieving shock-free flows over transonic airfoils. (If such flow could be achieved, much of the high-speed rotor flow problem could be eliminated.) The epicenter of this controversy was at the Courant Institute at New York University (NYU). There was a fascinating thesis by David Korn (a student of Professor Paul Garabedian) [1] on predicting shockless transonic flow using a complex hodograph method. It was a difficult approach, but I was determined to understand it. In order to do so, I wrote a program to solve the toy problem of predicting incompressible flow using a complex method-of-characteristics. This was my first introduction to the in-house computation facility—an IBM 1800, a big red computing machine, which was mainly used for processing wind tunnel data. Eventually, my program worked, but what I learned from it was that I needed a simpler way to perform transonic computations.

I went back to reading and eventually came across another paper by Earll Murman and Julian Cole on using finite-difference relaxation methods for computation of transonic flow. (I believe

this was a Boeing corporate report that was later published by the American Institute of Aeronautics and Astronautics (AIAA) [2].) This was an old approach that was originally pursued by Howard Emmons during the war without much success; of course, in those days there were essentially no computers—just platoons of women armed with adding machines. What attracted me to this method was that it seemed so simple—I actually felt I understood it on first reading it. To test that, I started writing another program, this time based on a simplified version of Murman’s method for the prediction of two-dimensional (2-D) airfoil flow. As simple as it initially seemed, it involved a lot of trial and error and long hours. Autumn came, and I continued to work with little success on my code. All I knew then was that Andy Morse would be sure to drop by first thing in the morning with his cup of coffee and talk about what was happening—he did this with everybody—and I didn’t have anything to tell him. I was beginning to worry. But late one evening in October, with the building nearly deserted, I was hand plotting a computed pressure profile over a simple biconvex airfoil and was startled as a crisp, beautiful, shock appeared. Andy happened to walk by my office and heard my gasp and asked what was happening. I showed him my shock plot. I don’t recall what he said, but he was interested and wanted to see more. Looking back, I realize it was at that point that I had accomplished my task for that summer; I had found a worthy rotorcraft problem to work on—transonic aerodynamics using Computational Fluid Dynamics (CFD). Throughout the 1970s, this would be one of the study areas that helped put our Lab on the map; some of the other areas were in rotor dynamic instabilities, high-speed rotor acoustics, and dynamic stall (more about that one later). I had no idea then that this was also the beginning of decades of computational work and experimental work by myself and many others.

Some of the People—the Character of the Army Aeronautical Research Laboratory

Besides doing my own work, I was also an interested observer of the work of our other engineers. I shared an office with Dave Sharpe, Georgene Laub, and Wayne Empey. Dave, at the time, was doing experimental work on the performance of tiltrotors using a small test rig that he had designed and built in the NASA Ames Machine Shop. Dave, like Andy, had an industrial background—in fact, he had worked with Andy at Hiller Helicopters. I learned a lot about rotorcraft from both Dave and Andy. I can’t recall what Georgene was working on at the time, but, over the years, I had the good fortune to work with her on many model rotor tests. Not only was Georgene an excellent engineer, but she knew all the ins and outs of the NASA Ames shops and worked well with all the shop people. Wayne Empey was also a mechanical engineer. At the time that I moved into his office, he was working on at least two projects—a joint experimental study, together with Bob Ormiston, of the interaction of a tail rotor with the main rotor wake in a crosswind (this had been the cause of at least one major accident), and he was building a 1/10th scale model of the Army 7- by 10-Foot Wind Tunnel to enable flow improvement studies. Wayne really impressed me with his energy. It seemed as though he could design and build anything in record time. I felt a bit like an oddball at first in this office, with three excellent mechanical engineers doing hardware-oriented testing—and then there was me with my papers, theory books, and computer cards.

Another person who stands out in my memory is Bob George, who was then our chief instrumentation expert. As I recall, Bob and I were usually among the first people to arrive in the morning and we would have interesting conversations over coffee. Our conversations concerned both technical and personal interests, but it is the latter that I remember. Music would sometimes be the topic, because Bob’s wife, Colleen, and I shared a love for the piano. I also recall Bob’s

interest in gardening—he belonged to a garden club, I believe, and he blossomed into a great rhododendron expert. His backyard became one of Cupertino’s horticultural wonders. In later years, when I became involved in testing, it was Bob who helped me plan properly for tests, taught me about pressure transducers, and coached me on how to conduct a test. Reading Bob’s chapter of these memoirs reminded me of this and of his dedication to aiding the engineering staff. Forty-six years later, I still have warm recollections of Bob and Colleen George—they are among the first people on our Christmas card list.

There were a lot of other folks who I met then and whom I still remember warmly today. Paul’s secretary, Alice Meyers, was a warm, efficient person, who kept things running in ways most of us engineers were unaware of. Andy’s secretary, Cathy Byrne, was young, full of spunk, and taught some of us a bit about women’s lib (primarily over the issue of who makes the morning coffee). We had a mathematics group that was in charge of the computer and data support: Don Adams, Gary Vander-Roest, Mike Kodani, and Mary Pollack. There was a small administrative group consisting of Alta Steengrafe and E. C. Carvell, both of whom were veteran Army civilian administration people, probably from Sixth Army Headquarters. (Our main administration was then at Sixth Army Headquarters at the Presidio, I believe.) I began to appreciate how many nontechnical people were required to keep a well-oiled technical organization going. And this group really was a smoothly running team. Making that happen was something that occurred before my arrival, but I suspect that Andy was responsible. I say this because it seemed to me that while Paul Yaggy’s role was primarily to keep the Lab connected to the Army back in Washington (he did much more than that), it was Andy who tended to the inner workings of the Laboratory. Paul and Andy were a wonderful team.

There’s something I need to say about Andy. While everyone gives lip service to vision—that is, to seeing beyond the minutiae and distractions of the day—Andy really had it in him to the core, and he actively encouraged and looked for it in his people. I recall the day that he asked me to write down what I intended to do in my transonic work in the next 10 to 15 years. That was over 45 years ago, and I didn’t know then how unusual that request was. I think Andy was the only person who ever asked me that question—at least in that way. He asked in a casual way, not wanting much time spent on it, but wanting to know something of my real thinking. In later times, other managers would ask similar questions, but there was always something more “official” about it. With Andy, it was casual and spontaneous (that is, coming from him rather than some management theory) because he really cared. And because it was so casual, I thought it must be a very normal thing. But I hadn’t even asked that question of myself at the time. So I thought about it, gave it my best shot, and in about 2 days I showed Andy my thoughts, which were handwritten on two sheets of engineer paper. (We wrote very neatly in those days before word processing.) Andy read it, seemed to approve, and gave it back to me, and we never spoke about it again. So, I filed this write-up in my desk and forgot about it, never to see it again until 10 or 15 years later when I was performing one of my rare cleanups. On reading it, I was astonished; it turned out that I had done just about everything that I had written for Andy. I thought about this and came to realize how much Andy appreciated a long-term vision and how actively he supported it. I recall how much of a resource Andy was whenever I ran into trouble—this usually took the form of active advice on design problems, finding needed funds, on how to plan tests or, that biggest of problems, on deciding what problem I should work on next. I suspect that, after learning the content of my newly articulated vision, he made it his business to support it. I also suspect he was less concerned with the content of that vision than to make sure

there was one. After all, if there were problems with it, he would be there to subtly bend things in the right direction—without my even realizing it. This reminds me of the biblical teaching that one who would lead must be a servant.

This was a period when the Lab was hugely concerned with acquiring its research staff. A number of our stalwarts had already been hired. I recall Don Boxwell, who came to us from Boeing-Seattle the previous year, following the massive downturn in the aerospace industry at that time. He was a typical midwesterner from Iowa State who didn't say much, but he was as conscientious as can be. There was Larry Carr, who came to us from Sikorsky Helicopters with a Ph.D. from NYU; Ken McAlister (Ph.D., University of Arizona) who, like myself, had come as an ROTC Army officer; Bob Ormiston (Ph.D., Princeton University); and Jim McCroskey (Ph.D., Princeton University), also courtesy of ROTC. Shortly after I arrived, Fred Schmitz, another Princeton Ph.D., arrived and moved into my office. Then the next month Bill Bousman, another ROTC alumnus from Cornell and MIT (and from our Army St. Louis offices) arrived across the hall. He was shortly joined in that office by two more Stanford students, Dewey Hodges (also with an ROTC commitment) and Dave Peters. Late in the year, Mike Martin, another Ph.D. and ROTC officer from the University of Notre Dame joined us. Perhaps the two most common themes that best describe this growing staff would be “top schools” and “ROTC.” Of course, there was also “youth” and “energy.” It's interesting too that it was not required that these new people have rotorcraft experience (there weren't many in those days); the important quality was the ability and drive to ask good questions and to learn. These people were indeed notable for their drive and their wonderful personal qualities, and there was a special kind of group identity and cohesiveness. The first thing that knit us all together was our common love of aeronautics—a lot of us had been model airplane builders as kids. We'd often have lunch together with hilarious conversations and occasional games of ping-pong. Later on, there would be frequent family potlucks on the weekends, and we got to see each other's kids growing up. (Ken McAlister's chapter includes a group photo, taken in the Caradonna's backyard, from one of those weekend potlucks. The photo includes two of the group's children, our son, Michael, and the Schmitz's son, Freddie.) We did a lot of things together. On weekend mornings, we'd often encounter each other doing home projects in the 7-by 10-Foot Wind Tunnel shop. Such home projects were so common at Ames that there was a name for them—“G-jobs.” These “G-jobs” were especially common just before the cub scouts held their pinewood-derby races—I can recall as many as five pinewood derby cars in the shop at one time. (Needless to say, Paul was unaware of this—I think.) We enjoyed each other.¹ But I think our cohesion actually had its beginning with Andy Morse and his morning coffee tours.

¹ I should add that the group spirit did not immunize us from conflict. We were all deeply engrossed in our own projects, but lab resources were limited and competition for those resources was inevitable. It was Andy who managed this competition, and it is a mark of his leadership that this competition never became personal. It is even more remarkable that this “competitive collegiality” (I wish I could think of a better word for it) lasted long after Andy's passing. A good illustration of this was once related to me by Darlene Kiefer, a secretary to several of Andy's successors (I am reminded of this now because I have just heard of Darlene's passing). Her desk was just outside Andy's open door, and she would see everyone coming and going. She recalled many times when several engineers would charge into that open door in a state of high agitation, and it was a bad sign when the door closed. She would then sit there, in a state of suspense, waiting for fireworks to start. She went on to say that she was invariably disappointed—typically, the door would eventually open and everyone would walk out laughing and they'd all go to lunch. Thanks for the story, Darlene, we'll miss you.

About that time, I had started to make a lot of improvements to my transonic airfoil code, such as learning how to model lift effects, the understanding of far-field boundaries, and code efficiency improvements. Most of all, I was starting to perform three-dimensional (3-D) wing computations. This was difficult because it required a huge increase in computer power—our IBM 1800 was not up to such computations. It turned out, however, that NASA had a much larger machine, an IBM 360/65. It was primarily dedicated to processing the Ames wind tunnel data, but it was available for other problems in its free time, and the Army was granted access under the terms of the Army-NASA Joint Agreement. So, I started to learn how to use this very special facility with the help of NASA personnel. Initially, this was done by submitting a deck of computer cards to be run overnight on the machine. The next day, I would pick up a large printout, if I was lucky. Most often, there was some arcane “bug” that quickly ended the run and I would spend endless time debugging. I needed a lot of help with this, and I spent a lot of time in the office of NASA’s computation advisors, Nataline James and Sally Rogallo. This was a very slow process—and this was how much of the next year proceeded. Meanwhile, there was time to observe, appreciate, and even become involved in the activities of our other researchers.

A Tragic Loss

One of our most interesting lines of research was the dynamic stall work that Jim McCroskey was just starting. He made arrangements with Boeing Vertol for the construction and testing (in the Boeing wind tunnel) of a simple rotor model, which had just enough pressure instrumentation to glimpse the retreating-blade stall process. This glimpse had convinced him that, in order to more easily study the stall process, a nonrotating model was needed. So, he performed a trial test of a small oscillating airfoil model in the Army 7- by 10-Foot Wind Tunnel (I believe Wayne Empey and Tom Wynn helped design and fabricate this model). This convinced him of the possibilities of oscillating airfoil testing, and he then proceeded to construct a much larger oscillating stall model. This model had a huge 4-foot chord and was equipped with a generous array of pressure measurement transducers and smoke ports for flow visualization. Again, Wayne Empey helped with the model design, including the oscillating mechanism. This was a big project and a number of engineers became actively involved, especially Mike Martin. The test was run in 1972, I believe. It was a spectacular test to watch, and the forces on the wind tunnel were a little frightening. One could stand outside the tunnel as the airfoil oscillated and observe the tunnel crossbeams vibrate with an amplitude exceeding 6 inches in places. Nowadays, this would be cause to stop the test for a safety review—but that’s not how things were done then. By the end of the year, a lot of data had been acquired (though not analyzed), but there was enough data to safely commit to writing a paper for the following year’s annual forum of the American Helicopter Society (AHS). However, Jim had consented to spend a year on a personnel exchange in France and would not be around to see the processed data or to write the proposed paper. Thus, the impending paper became Mike Martin’s task—Mike was an excellent engineer and anxious to do this paper. But disaster struck around Christmastime when Mike was diagnosed with a terrible cancer. He was operated on at the Letterman Army hospital in San Francisco and was treated there for several months. Clearly Mike was not going to be able to do the data analysis, and we were all in a state of shock over Mike’s condition. Wanting to help Mike, I took it upon myself to gather up the data (which was in the form of pressure-time histories on rolls of Visicorder paper) and find some digitizing machines (actually, they were modified IBM keypunch machines) located at NASA’s Unitary Wind Tunnel complex. The digitizing was a manual process in which I converted the Visicorder rolls to computer punch

cards. I was able to produce plots of chordwise pressure coefficient time variations, as well as lift and pitching-moment histories. The data clearly showed the development and transport of the dynamic stall vortex that Jim had first seen on the Boeing rotor test. It also showed the effect of this vortex on producing excess pitching moments and negative damping. I was so busy doing this that it didn't occur to me to tell anybody what I was doing. While this was happening, I was spending my time at the Unitary complex and was nowhere to be seen—so Andy probably assumed I had been doing my usual transonic computations. One day he asked me if I could somehow help Mike, and he was quite startled to learn that I was already well along in producing the results needed for writing Mike's AHS paper. As soon as I had produced what seemed like a suitable set of results, I took all the plots and brought them to Mike's bedside at Letterman hospital. Mike was very happy with this and gave the go-ahead to start on the paper draft. Mike recovered sufficiently to return to the office—I don't recall if we had a first draft by then, but we certainly did a lot of rewriting after his return. Both Mike and I were quite green and, though we had data such as the world had never seen, we were both upset that we didn't know how to explain it (actually, there is no simple explanation to this day). So, Mike telephoned Jim in France to ask if he thought such a paper was presentable. Jim thought the data was so novel that we had to be crazy to doubt presenting it. So, the paper was completed and, by that time, Mike was strong enough to travel. It was good to see Mike leave for Washington to present his paper [3].

The presentation was very well received, and it probably marked the beginning of the modern era of dynamic stall studies. But this was Mike's last paper. He weakened from then on until he was no longer able to come to the office. I visited Mike and Connie at their apartment in Sunnyvale one day. I brought ice cream and we enjoyed the time. But it was clear that Mike understood his situation. I still recall him saying "Frank, I just don't know how to get out of this." We said our goodbyes, and that was the last time I saw him. Mike died around Thanksgiving—he was 27. After that, I returned to my full-time transonic flow studies. I've never forgotten Mike Martin.

A Small Incident From the Early Days of CFD

The Ames-Army Lab even became involved in the traditional competition between NASA's Ames and Langley Research Centers to which I was a party. As I've mentioned, 1971 was mostly spent learning to perform 3-D transonic computations at the NASA Ames Central Computer facility. These were very large computations for those days, and the machine was usually too busy during the day to run such cases. So, I eventually took to running my cases at night. Throughout the year, there would be long stretches of time when nobody saw me in the office because I was working in the evening and into the early morning. At that time, a new time-share system for job submission came into being. Instead of punching a deck of cards, a program was written on a terminal that was a converted IBM Selectric typewriter. At first, I was simply trying to perform a 3-D computation on a wing because I didn't know how to account for rotation effects. By mid-year, I was producing simple pressure printouts and giving these to Mary Pollack to hand plot—we didn't have pen plotters yet. At the time, though, I may have been the only person in the world doing 3-D transonic computations. I found this out in a rather amusing way. I had become friendly with several of the NASA personnel in Harvard (Harv) Lomax's computation branch and would often attend some of their meetings. One day, Jerry South from Langley was visiting. He gave a seminar and, afterward, he had an informal meeting with Harv's group. The conversation concerned the 2-D computations being conducted at both Langley and Ames and, eventually, it turned to the prospects for performing 3-D computations

and whether these would be practical with our current machines—perhaps they were feeling each other out as to which of these competitive NASA groups would be the first to perform 3-D computations. If that was so, I was able to resolve their speculations. I happened to have one of Mary Pollack’s 3-D hand-plots with me and was only too happy to demonstrate that 3D computations could readily be done—and that it was the Army that had already done it on the NASA Ames computer.

Summer Visitors

In those days, we often had visiting professors spend their summers with us. In 1971, one of these was Professor Morris Isom from the New York University Aeronautics Department. Morris was given a desk in my office (Fred Schmitz had moved to another office), and we immediately struck up a warm friendship. Morris is one of the best analysts I have ever known, and he had a vast knowledge of transonic flow theory. I learned an awful lot from him. Toward the end of summer, he had developed a small-disturbance potential flow equation for 3-D rotors, one that properly accounted for rotation effects. I immediately set about writing a program for this equation. This meant spending my nights again at the NASA computer center. Typically, I would stay around in the morning long enough to see Morris when he came in and let him know how the program was coming along. Eventually, it became clear that the code was working pretty well, and we decided to submit an abstract for the AIAA Aerospace Sciences Meeting to be held January 1972 in San Diego. In November or early December, I visited Morris at his home in Armonk, New York, armed with a load of plots, and together on his dining room table we wrote a manuscript for our AIAA paper [4].

That paper was the world’s introduction to transonic rotor computations, and it was quite well-known for many years. But, the real pleasure of that experience was having a colleague like Morris Isom to work with. His combination of humor, knowledge, and wisdom are truly unique. In years to come, Morris would spend a lot of time with us, and he always made a big impact on our thinking. Several years later, we did a similar paper for unsteady flow—it was another first of its kind. Morris later had a huge influence on our thinking in acoustics; that acoustic work was done with Fred Schmitz.

Summer Intern Makes Good

During the summers, we would also have a number of student interns. The most unforgettable of these was a Berkeley graduate (with an ROTC commitment) by the name of Bill Ballhaus. He was anything but a typical Berkeley student—very nattily dressed (down to his leather briefcase), with a large afro-like hairdo, and a charismatic personality—he definitely stood out. That summer, he worked with me doing 2-D transonic airfoil computations. He had been a student of Professor Maurice Holt at Berkeley, and he knew a little about numerical methods. He spent a lot of time on a “Gelinsky–Telenin method” with the idea of speeding up the computations. The main thing I remember is that we became friends, and he fell in quite nicely with everyone. He shared an office the following summer with Morris Isom. Morris and I would occasionally have some fun at Bill’s expense. We had a big laugh at lunchtime when Morris remarked on the contents of Bill’s briefcase, a *New York Times* and a hairbrush. But we were all impressed by Bill. Bill and I submitted an abstract for a paper on the effect of rotor planform geometry for an upcoming Advisory Group for Aerospace Research and Development (AGARD) conference on rotorcraft aerodynamics to be held in Marseilles, France. Bill, however, was about to enter active

duty and report to Aberdeen Proving Ground for officer training. So, I did the necessary computations. This again meant a lot of nighttime and early morning hours, and weekends too. Weekends were a problem because the computer center was locked up then. However, I found a removable grating in the back of the building that gave access to a ladder going down to the boiler-room entrance, which was never locked. From there I would find my way up to the main computer room. The computer operator was only too happy to have some company. Security was sure a different thing in those days. After plotting all the results, I made a quick trip to Aberdeen where Bill and I wrote the draft for our paper [5].

The Marseilles meeting was a memorable experience. Some of the presentations gave me good ideas (especially on the subject of wakes) that I drew on many years later. And I met several people with whom I would later work—especially Jean-Jacques Philippe, whose experimental work would be important for validation of my codes. I was very nervous about the paper presentation itself, but it went very well. I especially recall the very generous comments made by Jan Drees, one of the great pioneers of rotorcraft aerodynamics.

It was becoming clear by then that CFD should have a bigger role in Army research. So, our management, with Jim McCroskey's urging, I believe, decided that we should try to position an Army scientist in Harv Lomax's Computational Fluid Dynamics Branch. I had a friend in Southern California who would have been a good choice for this position, and I arranged for him to fly up and talk to Harv. The meeting went well, but my friend decided that the Army labs were too new, and that being an Army civilian employee in a NASA organization was too unusual to take such a personal gamble. My friend's concerns are amusing when considering the career of the next candidate for the job. I may have been the one who first mentioned this job opening to Bill Ballhaus—and he jumped right on it. Bill made a good impression on Harv, and he was assigned as an Army employee to NASA's Computational Fluid Dynamics Branch at Ames on the completion of his officer training at Aberdeen Proving Ground. The rest is history. Eventually a new branch was created—the Applied Computational Aerodynamics Branch—and Bill was selected as Branch Chief while still an Army employee. Then he transferred to NASA when he was selected to be Director of the Astronautics Directorate at Ames. And he then went on to become the Director of the entire Ames Research Center. I'm tempted to say that he also had to give up his afro, but I suspect the Army took care of that at Aberdeen Proving Grounds.

Eventually, Bill left Ames for greater things and we lost contact with each other. The last time I saw him was at a 50th Anniversary of the Stanford Aeronautics and Astronautics Department about 8 years ago. One of the most memorable presentations was given by three distinguished speakers, all sharing the name of William Ballhaus—father, son (our Bill) and grandson. I had never met Bill's dad, but I really enjoyed his presentation of work he had done on aircraft that I (as an aviation enthusiast) was familiar with. I also thought Bill's son gave an impressive talk too, but I became distracted by the sudden recollection of a hilarious incident involving him that had happened 30 or more years before. Bill and I were writing a paper (I don't recall what it was for) on the kitchen table in his apartment off Villa Street in downtown Mountain View. In the background, there came gentle splashing sounds from the bathroom, as Bill's wife was bathing their two little boys. Suddenly, the splashing became violent, accompanied by loud shouts; and then a wet, naked 4-year-old (the aforementioned Ballhaus grandson) escaped the bathroom and ran past us—with a look of horror resulting from his younger brother having relieved himself (in the worse possible way) in the bathtub. Bill and I were laughing for quite a while before we could get back to work. (I don't recall Bill's wife being particularly amused.) More than 30 years

later, it was difficult for me to maintain my attention or composure while recalling this incident and trying to listen to a speech given by its principal participant, now an impressive business executive.

Building the Future Research of the Ames-Army Lab

Around the time of the Marseilles meeting I had a talk with Andy in his office about the future direction and requirements of our transonic studies. My computations were going very well, but Andy brought up an obvious problem. It wasn't enough to have a working computational method; it was also necessary to satisfy ourselves that I was simulating reality—and at that time there were no data in existence with which to compare my computational results. Since my high-speed rotor computations were unique in the world (and would remain so for quite a few years), it seemed clear that it was going to be our job (read that as *my* job) somehow to acquire the necessary data for my code validation. I was a bit nervous at this prospect, never having been an experimentalist, but quickly concluded, “What the hell!” I'm sure this was precisely what Andy hoped would result from this conversation, and I left his office thinking the whole thing was my idea. Much later, it dawned on me that Andy had exercised a kind of benevolent seduction. (Later on, I would try to do the same thing myself, but Andy was the master of the technique.) So, from that time forward, for many years, I was actively involved in the design and preparation for a high-speed validation test—in addition to continuing my computational work.

At that time, the Ames-Army Lab did not have a generic rotor-test capability; that is, any requirement for rotor testing would have to be met with a special rotor apparatus, typically with minimum capability, built solely for that purpose and no other. This made it very difficult to build up a credible capability in rotor testing. It was Andy's idea that we should have a general rotor-test apparatus capable of supporting a wide variety of rotor tests. So, Andy formed a study committee charged with the design and construction of such a test apparatus. The committee consisted of Andy, Fred Schmitz, myself, Georgene Laub, Dave Sharpe, Bob George, and a contractor, Jacques DuBois. Bob George was our principal instrumentation expert and was an integral part of almost every major test. He was clever, reliable, and wonderful to be around. Jacques' role as a contractor was to translate our ideas and requirements into fabrication-ready drawings. Jacques was a Swiss gentleman, much older than the rest of us, very sure of himself, headstrong, and a lot of fun. And he was a very experienced mechanical design engineer who prevented our requirements from going “over the top,” while, at the same time, resulting in an apparatus that was reliable and versatile. The resulting design was made of major subcomponents with a standard interface, such that it could be reconfigured to fit in a wide range of test facilities—principally the 7- by 10-foot test section of our wind tunnel and the soon-to-be-built hover chamber. Over the years, this rig would also be used in the DNW German-Dutch Wind Tunnel in the Netherlands and the NASA 80- by 120-Foot Wind Tunnel. Except for the motors and rotor balance, the test components were all fabricated in the NASA Ames Machine Shop. The rotor balance was designed and built by Ling-Temco-Vought (LTV) under the leadership of Jim McClure, a gentleman, a Texan, and a wonderful engineer, with whom we would work several times over the years. Jim had an unusual expertise in rotor-balance development (acquired during the XC-142 project in the early 1960s). The resulting test stand, which became known as the Rotary Wing Test Stand (RWTS), was indeed a very general apparatus and is still operational nearly 45 years after its initial design. This stand was designed to do many different things, but not everything. It did, however, provide a basis from which almost any need could be met with the proper additions. Anyone who needed more capabilities had only to put in the time

and effort to develop them—but with the RWTS as a starting point, this was always doable. Without the RWTS, nothing but the most rudimentary of designs would have been possible.

With my new charter for developing a high-speed rotor apparatus for code validation, I began developing requirements. The RWTS had many capabilities that I could use, but there were several that I would have to add. I had decided that a good place to start my validation studies would be with a high-speed hover test. Two reasons for this decision were: 1) I had not yet developed an unsteady computation capability, and 2) the original RWTS configuration assumed use of a hover facility. Both reasons were temporary, but the decision was made and, as it turned out, it wasn't a bad one. The decision had both theoretical and hardware implications. The former was that it would be necessary to develop a wake-modeling capability. The hardware implications were that I would need to build a model rotor with extensive pressure measurement capability, and that I would need a lot more power capability than was currently available. The latter implication resulted in our contracting the Boeing Vertol Company for the fabrication of a new floor-mounted transmission system designed to accommodate three air motors—this would provide a power capacity exceeding 500 hp. The wind tunnel group at Boeing did this transmission work. This was the beginning of a long and fruitful relationship with Boeing-Philadelphia. I recall that it was on a trip to Boeing (together with Bob George) that I first met Leo Dadone, one of the most respected aerodynamicists in the business, a charming person, and a good friend. With our power system in Boeing's competent hands, the next thing to work on was the pressure-instrumented rotor blade. This was a much more complex piece of equipment, and I needed some time to think of how best to approach the problem. I had no experience with testing rotors, so I designed, and the wonderful NASA Ames Machine Shop built, an uninstrumented rotor on which to do preliminary testing. These first tests were performed with a view to evaluating the new Boeing power system. Aside from proving our power system, I also used this testing to get my first glimpse of the high-speed rotor flow field. I did this by placing an array of microphones outboard of the rotor and using them to trace the geometry of the radiating shock. I never published this, as I was too busy. The main event now was to build the pressure-instrumented rotor. I had somehow become convinced that a steel-bonded blade with embedded pressure tubes would be the easiest way to do this. Together with Bob George, we decided to acquire the pressure data with a special Scanivalve assembly located in the hub. Soon the fabrication began and it was a long process.

The End of the Beginning

At the same time that we were building the RWTS, other things were happening. Morris Isom and I wrote a paper on unsteady 3-D rotor flow [6]. I also did a lot of studies with 2-D unsteady, lifting and nonlifting airfoil computations. We had extended visits from the Office National d'Etudes et de Recherches Aérospatiales (ONERA) under a U.S.-French Memorandum of Understanding (MOU) in exchange for the visits to France that Jim McCroskey, and later, Bob Ormiston, had made. The French scientists were a lot of fun, and I only have time here to name some of them—there was Jean-Jacques (J.J.) and Annie Philippe, Joel Costes, Jean-Jacques and Jeanine Thibert, Tran Cam Thuy, Jean-Jacques Chattot, Andre Desopper, and many others through the years. The work of J.J. Philippe was especially significant for me. Although I was the only one doing rotor transonic computations, he was a good experimentalist and had decided that he would build a rotor test rig with pressure instrumentation. But his rotor was intended for use in a wind tunnel (the S2 at Chalais-Meudon, a small research tunnel). So, unlike the facility I was building, J.J.'s rotor would require unsteady-pressure transducers, and his data would

primarily be non-lifting (at first, anyway). So, France eventually had non-lifting unsteady data and I had a capability for predicting this flow. When we compared the data and computations, I think we were both astounded by the quality of the comparison—I certainly was. We presented these results at the second European Rotorcraft Forum in Bückeburg, Germany [7].

From that point on, I suspect, nobody in the world could doubt the future importance of CFD for rotorcraft development. But, at that point, there was still much to develop, especially the ability to predict the loads on lifting rotors.

Load prediction on lifting rotors is far more difficult than for lifting wings. This is because a rotor is constantly flying by a very small distance over its own wake—that is, the tip vortices from the preceding rotor blade. My first step in including a full wake computation was to employ potential discontinuity sheets to model the presence of passing adjacent tip vortices. This resulted in a very good model of near rotor wake, and it formed a part of my Ph.D. thesis [8]. (I forgot to mention that I hadn't obtained a Ph.D. yet—and, frankly, I wasn't interested in getting it, but Andy persuaded me otherwise—a long story.)

In order to do a full lifting-rotor computation, it was necessary to model the entire wake. One big problem was that we did not know then how to compute the location of the nearby passing vortices. Eventually, I evolved the idea of combining the near rotor region (including a potential discontinuity passing vortex model), with a simple Biot–Savart downwash computation of those vorticity elements outside of the near rotor region (we would refer to this as a partial inflow computation). And to find the location of the passing tip vortex, I decided that I would simply measure it with a traversing hot-wire probe. In preparation for my final test, I performed many simpler preliminary tests, one of which was to develop the ability to track the rotor tip vortex with a hot-wire probe. At last the day came, after years of preparation, when my high-powered rotor stand was assembled, complete with a pressure-instrumented rotor and hot-wire wake traverse, and we started acquiring data. We had been acquiring data over a range of blade angles-of-attack (from 0 to 8 degrees) and tip Mach Numbers (from around 0.5 to 0.84) when the blade bonding started to fail. I've always thought that we could have done a mechanical patch for the blade, requiring about 2 weeks' work in the NASA Ames Machine Shop—if there had been time in the Lab's busy test schedule to allow this (unfortunately, there wasn't). But with critical test points yet required, my choice was to either stop the test and fix the blades before continuing, which would have required about 2 years' work, or simply continue testing and hope for the best. Having other plans for the next 2 years, I chose to continue taking data, and I was able to acquire all the data required. However, while reducing RPM, while still at high speed, the blade disintegrated. Since the test was held behind thick concrete walls, there wasn't a loud noise; rather, it felt somewhat like a small earthquake. Don Boxwell was running the tape recorders, and I recall the voice track with the sound of a small bump and Don calmly saying, "Test complete."

My next step, after the mess was cleaned up, was to process all the data, complete the wake model, and do model/test comparisons. Most of the processing was done on the same DEC11/45 that was used for the data acquisition. I had help organizing the data into a report and developing the final wake computation. Chee Tung had recently joined us, coming from the Brookhaven National Laboratory in New York. He was also an NYU graduate. Chee was and is wonderful to work with and a great friend. This effort was our first work together. Chee was able to accelerate our wake partial inflow computation by modifying an existing analysis to do the Biot–Savart

computation. The total analysis used the measured passing tip vortex location to properly locate the potential discontinuity vortex model. This was then combined with a partial inflow computation, using the computed circulation distribution to give our wake element strengths. The code would iterate on this circulation until convergence. This gave us complete blade-pressure distributions that we could then compare with our measured values, and the comparison was excellent. These results were presented in a series of three papers; the last of these, which included Andre Desopper as an author, was presented at the European Rotorcraft Forum at Aix-en-Provence. We assume that Andre presented this paper, since neither I nor Chee recall attending that Forum. That last paper also included our first attempt at predicting lifting forward-flight loads—and that approach, which incorporated the Drees approximate inflow, was Andre’s idea, as I recall [9-11].

The publication of these results was a milestone—there was no longer any question of the capability of CFD to accurately compute a wide range of rotor flows. Over the years, NASA TM-81232 [10] has been among our most requested papers—not only for the analysis results, but especially because the tabulated pressures were very easy to use, and they constituted a good test case for anyone developing a new code. This work has contributed greatly to the development of rotor analysis codes worldwide.

I had spent 8 or 9 years getting to this point. Originally, I really knew nothing about rotorcraft problems, but now I understood something about high-speed aerodynamics and rotor wakes and was able approach the future with some confidence. We had learned that CFD had the predictive capability that we required, but we still had a lot to learn about CFD. We had learned how to do high-speed rotor testing, and we would use this knowledge many times over in the future. We had learned how to compute transonic rotor flow, but we also realized that predicting the wake was essential and we did not know how to do this—it would be many years before CFD would predict these wakes with reliability. And there were many things that we had yet to appreciate. For instance, the thought of integrating structural and flow computations still lay in the future. However, we could now envision that future with far greater clarity, and better plan future work. So, while this was not the “beginning of the end,” it was definitely “the end of the beginning.”

In Conclusion

This seems to be a good point to conclude my recollections of the Ames-Army Lab’s early days. I could go on. The 1980s were also very full and productive years. But I’ve said enough for now, for the purpose of these memoirs of the early days of the Army-NASA Joint Agreement. I can say that, after a full life, I look back over the 1970s and marvel at all the things we were able to accomplish in those early years. Of course, while all of this was going on at the Lab, back at home we were buying and remodeling houses, and having babies. Small wonder then that I can’t recall everything. And I have neglected a lot of people who really should be remembered. In some cases, their names simply elude me, even though I see them in my mind’s eye. Or even if I can recall names, I often can’t recall the everyday things these people did, but I know that I would have been helpless without them. I started out with no background in numerical or experimental methods, but by the end of that decade, I was recognized in both areas. For this, I can only thank my colleagues, the wonderful mechanical and design engineers, instrumentation technicians, and tunnel staff, plus having access to some of the best minds in the country on analytic and numerical methods and to world-class computational facilities. I also had the opportunity and pleasure of contributing to the work of others over the years. None of this would

have been possible without the unique working environment inspired and enabled by the Army-NASA collaboration under the Joint Agreement at Ames Research Center, which was probably at its peak in the 1970s.

But everything changes over time. I'll never forget the day in the 1990s when a certain NASA administrator addressed an open meeting in the Ames auditorium and severely criticized the wonderful Ames environment as being too "academic." Fortunately, the ensuing damage was not total—and one of the factors limiting this damage was the Ames-Army Lab, which was more or less isolated from the madness of those years. Today, it is good to see that real research still goes on in the Ames-Army Lab. I look back with some pride in having been a small part of that research history. I don't know of a nobler or more human form of endeavor than the research process and, in my mind's eye, I can see a long line of dedicated people who made all this possible. Many remain good friends and many are now gone, but, of all the people I've known, the one I recall the most warmly is Andy Morse, a manager, colleague, and friend like no other. I can't possibly recall all the things he did to ease the way for me and others, with no great recognition other than the satisfaction of doing his job well. We miss you, Andy.

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Index

A

Aberdeen Proving Ground, 1, 9
Advisory Group for Aerospace Research and Development (AGARD), 8
American Helicopter Society (AHS), 6-7

American Institute of Aeronautics and Astronautics (AIAA), 3, 8

Army Aeronautical Research Laboratory (AARL), 1

B

Ballhaus, Bill, 8-9

Biot–Savart, 12
 Boeing Vertol, 6, 11
 Boeing–Seattle, 5
 Bousman, Bill, 5
 Boxwell, Don, 5, 12
 Brookhaven National Laboratory, 12
C
 Carr, Larry, 5
 Carvell, E. C., 4
 Chalais–Meudon, 11
 Chattot, Jean-Jacques, 11
 Cornell University, 5
 Costes, Joel, 11
D
 Dadone, Leo, 11
 Desopper, Andre, 11, 13
 DNW German–Dutch Wind Tunnel, 10
 Drees, Jan, 9, 13
 DuBois, Jacques, 10
E
 Empey, Wayne, 3, 6
G
 Garabedian, Paul, 2
 Gelinsky–Telenin method, 8
 George, Bob, 3-4, 10-11
 George, Colleen, 4
H
 Hiller Helicopters, 3
 Hodges, Dewey, 5
I
 Isom, Maurice, 8, 11
J
 James, Nataline, 6
K
 Kiefer, Darlene, 5
 Kodani, Mike, 4
L
 Langley Research Center, 7
 Laub, Georgene, 1-3, 10
 Ling–Temco–Vought (LTV), 10
 Lomax, Harv, 7, 9
M
 Mach, 12
 Marseilles, 8-10
 Martin, Mike, 5-7
 McAlister, Ken, 5
 McClure, Jim, 10
 McCroskey, Jim, 5-6, 9, 11
 Memorandum of Understanding (MOU), 11
 Meyers, Alice, 4
 MIT, 5
 Morse, Andy, 1-5, 7, 10, 12, 14
 Murman, Earll, 2-3
N
 NASA, 1-3, 6-14
 New York University (NYU), 2, 5, 12
O
 Office National d’Etudes et de Recherches
 Aérospatiales (ONERA), 11
 Ormiston, Bob, 3, 5, 11
P
 Peters, Dave, 5
 Philippe, Annie, 11
 Philippe, Jean-Jacques, 9, 11
 Pollack, Mary, 4, 7-8
R
 Reserve Officers’ Training Corps (ROTC), 1, 5, 8
 Rogallo, Sally, 6
 Rotary Wing Test Stand (RWTS), 10-11
S
 Schmitz, Fred, 5, 8, 10
 Sharpe, Dave, 3, 10
 South, Jerry, 7
 Stanford, 1, 5, 9
 Stapleton, Col. Cyril, 2
 Statler, Irv, 1
 Steengrafe, Alta, 4
T
 Thibert, Jeanine, 11
 Thibert, Jean-Jacques, 11
 Tran, Cam Thuy, 11
 Tung, Chee, 12-13
V
 Vander-Roest, Gary, 4
Y
 Yaggy, Paul, 1-2, 4-5

Memories of My Career at the Army Lab

Larry Carr
Army

Prologue

My interest in helicopters began with a course in helicopter aerodynamics presented by Professor Rene Miller at the Massachusetts Institute of Technology (MIT) as part of my undergraduate program of study. This led to a job at Sikorsky Aircraft in 1960, where I worked in the advanced design department, with focus on the helicopter proposal that became the CH-53 helicopter. I had the opportunity to perform wind tunnel tests on a 1/5-scale model of the CH-53, and I gained a lot of experience about flow behavior and drag reduction.

I attended New York University as a Master's-degree student under the Sikorsky continuing education program and ultimately attended New York University (NYU) full time as a Ph.D. student. At that time, I worked on wind tunnel design, leading to the construction of a hypersonic wind tunnel. As part of my Ph.D. thesis, I developed a computer program to model turbulent boundary layers and applied the code to drag reduction studies.



Larry Carr, 1978.

I enjoyed these studies, but my interest in helicopters led me to consider returning to the rotorcraft world. When I started looking for a job at the end of my Ph.D. studies, I received a range of offers from major aeronautical companies, and one inquiry from a small laboratory in California, the U.S. Army Aeronautical Research Laboratory (AARL) at NASA Ames Research Center. I was invited by the various companies to visit their facilities; by comparison, the Technical Director of AARL visited me and my wife at our apartment in the New York City area! Paul Yaggy came to talk to me about possible employment. We discussed a range of topics about helicopters, my time at Sikorsky, and my projects at NYU.

The possibilities for research in helicopter technology at the Ames-Army Lab interested me more than my other opportunities, so I accepted the offer to join the Lab. We were on our way to California to start work when my wife and I stopped in Seattle to visit a former MIT roommate, then working for Boeing. While there, he hosted a get-together of people from his office, and I learned that AARL already had a very good reputation. When I mentioned that I was starting work at AARL, several engineers made comments about how highly respected the laboratory was (and at that time it was only 3 years old)!

A Nice Beginning

A few days after I started work, in July 1968, the Technical Director, Paul Yaggy, asked me to come to his office. I went to his office, expecting to be given an "orientation to the Lab" lecture. Instead, Paul pulled a box from a drawer in his desk, opened it, and proudly showed me his latest brass model railroad locomotive. I then remembered that when he visited me in New York, we

enjoyed a lengthy discussion about model railroading! What a nice way to start my career at AARL!

When I arrived at NASA Ames Research Center, I was excited by the size of the Center, the wind tunnels, and the atmosphere of AARL. The philosophy of the Lab was definitely “can do”—whenever an idea looked worthwhile, we received the financial, technical, and moral support necessary to tackle the task and develop the idea. This was also the time of the Vietnam War, and we knew that what we were doing could make a real difference to the soldiers in the field; this was an additional factor that helped keep us focused on the work we were setting out to do.



Larry Carr and Andy Morse, 1978.

I remember being invited to Paul Yaggy’s office another time, where he discussed what he was attempting to do about rotor performance with Dr. René Dorand, of Giravions Dorand, who had developed the Dorand Jet-Flap rotor. To be included in a discussion of such an interesting new technology was pretty heady stuff for me as a new Ph.D. I also remember the many discussions I had with Andy Morse, my manager and “sounding board,” as we explored the pros and cons of the many new rotor concepts being proposed.

One of the first tasks I was involved in at AARL was a wind tunnel experiment of a Variable Deflector Thruster technique using Coanda effect technology to control the lift dynamically on a helicopter rotor blade.

Another early effort was a study of the behavior of wind tunnel flow when testing helicopters at very low forward flight speed. This test, with Frank Lazzeroni, documented the way the flow in the tunnel changed as a helicopter model was tested at progressively lower speed; ultimately the flow in the tunnel no longer stayed attached to the floor of the tunnel, thus strongly distorting the flow into the rotor system. We explored a variety of ways to keep the flow on the tunnel floor attached, including the use of distributed blowing. This study renewed my interest in turbulent boundary layer behavior.

Turbulent Boundary Layer Research

As noted earlier, my interest in turbulent boundary layers began at Sikorsky Aircraft when I worked on drag reduction as part of a wind tunnel test in support of the CH-53 helicopter proposal. I worked next on turbulent boundary layers while developing a turbulent boundary layer computer program as part of my Ph.D. thesis. I became interested in unsteady turbulent boundary layer separation after learning about dynamic stall in my early years at AARL.

As part of my study of turbulent boundary layer behavior, I worked with Dr. John Nash of Lockheed-Georgia, learning how to apply his turbulent boundary layer computer code to helicopter applications [1]. Dr. Robert Singleton, who later became the Director of the U.S. Army Research Office, was a co-investigator on this task.

Another of my early research efforts was working with Professor James Miller at the Naval Postgraduate School in Monterey, California, using his unsteady flow wind tunnel to impose adverse pressure gradients on a turbulent boundary layer in unsteady flow to determine what impact the unsteady flow would have on turbulent boundary layer development. This study demonstrated to me the immense difficulty of quantifying the behavior of unsteady turbulent boundary layer separation.

A side note: The Naval Postgraduate School wind tunnel produced unsteady flow by rotating doors that rapidly blocked and unblocked the flow in the tunnel, producing dramatic, rapid changes in flow velocity. However, we learned that it did even more when we got a call from the main office at the Naval Postgraduate School telling us that a lady in a house about a mile away reported her living room window moving in and out at the same rate as our tunnel pulsing!

My next project was to do a survey for the Advisory Group for Aerospace Research and Development (AGARD) compiling the experimental data on unsteady turbulent boundary layers then existing in the U.S. and Europe. As part of this project, I met with scientists throughout the U.S. and Europe, and I published these results in an AGARDograph [2]. This opportunity to learn what the various scientists experienced in their efforts to document the behavior of this challenging flow supported my growing belief that this flow was a very complex and interesting area of research [3, 4]. One of the scientists, Dr. Jean Cousteix of the Office National d'Etudes et de Recherches Aérospatiales (ONERA), visited our home in Los Altos. During his visit, he started talking to our son, David, in French, and this led to an invitation for David to visit Dr. Cousteix and his family in France, followed by a visit by their son to our home—a family version of the U.S.-France Memorandum of Understanding (MOU)!

I also had the chance to work with Professor William Reynolds of Stanford University. We had a series of conversations about the challenges that faced any experimenter who attempted to quantify the effect of unsteadiness on the development of unsteady separation of turbulent boundary layers, and this led to a proposal to the U.S. Army Research Office to develop an unsteady turbulent boundary layer tunnel. I was excited by the potential for this tunnel, and met with Professor Reynolds and his graduate students as they developed the tunnel and obtained some important insights into this complex problem [5].

Dynamic Stall Research

My study of unsteady turbulent boundary layers led me to consider the challenges associated with the physics of dynamic stall and the severe aerodynamic loading that occurs on helicopter rotor blades when pitched rapidly past the static stall angle. I became very interested in the unsteady turbulent boundary layer separation that was a major factor leading to the dynamic stall event, and I started looking at how these unsteady effects might change turbulent boundary layer separation. At this point, an experiment was being proposed to study dynamic stall in the Ames-Army Lab wind tunnel. I asked if I could be part of the team performing the experiment, since the next logical step in my study of unsteady turbulent boundary layer separation was to determine the behavior of the flow on an oscillating airfoil.

I joined Jim McCroskey and Ken McAlister in the first of a series of dynamic stall experiments quantifying the character of dynamic stall on a wide range of airfoils. This series of experiments, starting with a large airfoil oscillating to high angle of attack, followed by a test of a smaller airfoil with modifications of the leading edge of the airfoil, demonstrated the range of stall



Jim McCroskey, Bob George, Larry Carr, and Ken McAlister.

behavior that can lead to the dynamic stall event. This led to a comprehensive study of a set of eight airfoils representing the various airfoils then in use on helicopters, as well as airfoils that explored the behavior of airfoils not usually considered for helicopter use. These experiments documented the details of flow behavior during the dynamic stall event, and comprise a set of experimental data still being used as test cases for modern computational modeling of dynamic stall [6, 7].

The series of dynamic stall experiments was quite comprehensive, and Ames-Army Lab management decided that future emphasis should be placed on other aspects of rotorcraft aerodynamics. I was asked if I would like to work on rotorcraft acoustics, or if I would be interested in working in the NASA Aerodynamics

Research Branch as an Army employee assigned to NASA as part of the Army-NASA Joint Agreement that had created AARL. At a meeting with Sandy Davis, the Branch Chief, I learned that I would be working on the development of an unsteady boundary layer tunnel. I joined the Aerodynamics Research Branch in 1984 to return to my interest in unsteady turbulent boundary layer behavior.

During this period I had been compiling a survey of dynamic stall research in an invited American Institute of Aeronautics and Astronautics (AIAA) paper that was published in reference [8]. I presented this survey at the AIAA summer conference in 1985 and was also asked to present it at a meeting on Supermaneuverability hosted by the Air Force Office of Scientific Research (AFOSR) in Washington, D.C. After presenting this survey at that meeting, I was approached by scientists managing aerodynamic research at AFOSR and asked if I would be interested in a year-long position as a Visiting Scientist at AFOSR.

After discussion with my Army and NASA managers, it was arranged for me to hold a joint Army-NASA-AFOSR assignment for 1 year. I rented an apartment in Washington, D.C., and assisted AFOSR in managing the AFOSR Supermaneuverability Initiative. My year in Washington included periods of time at NASA focused on my ongoing research on boundary layers and dynamic stall. (As an interesting side note, I was in turn loaned by AFOSR to the Department of Energy (DOE) to be a member of the DOE National Review Committee on Wind Turbine Technology.)

Upon completion of my tour of duty at AFOSR, I returned to my position at NASA and spent the rest of my career working on the extremely interesting problem of dynamic stall. I had the chance to work with Professor Israel Wygnanski and Dr. Avi Seifert of the Tel Aviv University, Israel, under the U.S.-Israel MOU. This effort focused on the use of oscillatory blowing as a tool

for delaying turbulent boundary layer separation, and ultimately the delay of dynamic stall on oscillating airfoils.

A joint research program with Professors Satya Bodapati and Muguru Chandrasekhara of the Naval Postgraduate School studying the effect of compressibility on dynamic stall was particularly interesting. This project included investigation of techniques for suppression of dynamic stall at high Mach number, and we were able to demonstrate that a leading-edge slat could significantly delay the stall even at $M = 0.4$. At a free-stream Mach number of 0.4, the flow near the leading edge of an oscillating airfoil can dramatically exceed the speed of sound when the airfoil is at high angle of attack, and the resultant shock waves can cause the flow on the airfoil to separate, leading to dynamic stall.

Studying the character of dynamic stall continued to be both challenging and satisfying. In fact, I was so interested in this topic that I decided to accept a position as a NASA Ames Associate when I retired in January of 1999, continuing as a U.S. Army Volunteer Emeritus Scientist when that program was established. I finally “retired” in early 2016, ending my 48-year career at the Ames-Army Lab.

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Index

- A**
- Advisory Group for Aerospace Research and Development (AGARD), 3
 - Air Force Office of Scientific Research (AFOSR), 4
 - American Institute of Aeronautics and Astronautics (AIAA), 4
 - Ames-Army Lab, 1, 3-5
 - Army Aeronautical Research Laboratory (AARL), 1-2,
- B**
- Army-NASA Joint Agreement, 4
 - Bodapati, Satya, 5
 - Boeing, 1
- C**
- Chandrasekhara, Muguru, 5
 - CH-53 helicopter, 1-2
 - Cousteix, Jean, 3

Carr, L.

D

Davis, Sandy, 4
Department of Energy (DOE), 4
Dorand, René, 2

G

George, Bob, 4
Giravions Dorand, 2

L

Lazzeroni, Frank, 2
Lockheed-Georgia, 2

M

Massachusetts Institute of Technology (MIT), 1
McAlister, Ken, 3-4
McCroskey, Jim, 3-4
Memorandum of Understanding (MOU), 3-4
Miller, James, 3
Miller, Rene, 1
Morse, Andy, 2

N

NASA Ames Research Center, 2
Nash, John, 2
Naval Postgraduate School, 3, 5
New York University (NYU), 1

O

Office National d'Etudes et de Recherches
Aérospatiales (ONERA), 3

R

Reynolds, William, 3

S

Seifert, Avi, 4
Sikorsky Aircraft, 1-2
Singleton, Robert, 2
Stanford University, 3

T

Tel Aviv University, 4

U

U.S.-France MOU, 3
U.S.-Israel MOU, 4

V

Vietnam War, 2

W

Wynanski, Israel, 4

Y

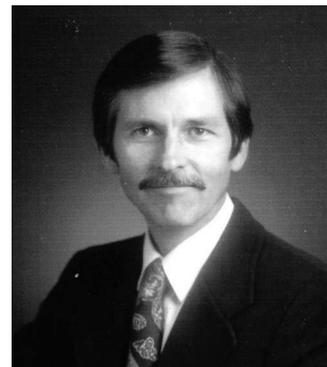
Yaggy, Paul, 1-2

My Years Under the Army-NASA Joint Agreement, 1967–1983

Lloyd D. Corliss
Army/NASA

Prologue

Even for me, it has been a bit of a fascination as to how my career evolved. As a youngster, I had always been interested in gadgetry, partly stemming from my dad's ability to fix anything. However, my technical interests seemed to be driven more by intuition and less by environment; my family was engaged with the cereal industry (i.e., Kellogg's of Battle Creek). It was my mom, however, with her joyful and supportive spirit, who encouraged me to apply at the local junior college just a few weeks before classes were to begin.



Lloyd Corliss, circa 1980.

In thinking back, I do recall an incident in the sixth grade, when I was the only student to pursue an extra credit project of building both a parallel and a series circuit of bulbs, switches, and a 6-volt battery. Subsequently, in high school, I recall taking a standard aptitude test, after which a counselor reviewed the results with me. Comparing the results to a list of occupations, he noted that I qualified to be a *pretzel bender*. (Hmm, someone with my sense of humor?) In any case, he finally narrowed my aptitudes to the field of engineering. Aside from knowing that an engineer “drove” a locomotive, I wasn't sure what that meant, so I made a trip to the library. One reference had a list of engineering disciplines, from agriculture to zoology. Faced with more uncertainty, I then placed my finger on the list, closed my eyes, and after moving my finger up and down a few times, I opened my eyes. My finger was on electrical engineering and my nail pointed to chemical engineering. Well, I *was* a chemistry enthusiast but, after recalling my sixth-grade successes with circuits, I chose electrical engineering

My venture into Modern Control Theory was an equally “well-planned and rigorously thought out” move. When I transferred from Kellogg Community College to Michigan State University (MSU), the Electrical Engineering Department placed all the transfer students (19, I think) into a new program involving State Equations (system modeling using matrix algebra), Laplace transforms (and later, Z transforms), and the early beginnings of optimal control. This approach was suitable for analyzing a variety of systems (e.g., mechanical, electrical, hydraulic, etc.). I believe the word “continuum” was used to describe the bridge between system types. In any case, this approach has grown over the years, and very early on I was exposed to what became known as Modern Control Theory. It is fair to say that my path into the technical world was by no means a well-planned venture, but rather a path of fortunate circumstances.

Introduction

My career with the Army Aeronautical Research Lab (AARL) at NASA Ames Research Center, Moffett Field, California, began in January 1967, after receiving a Master's degree in electrical engineering from MSU. The assignment included work in the fields of piloted simulation, flight

control law development, aircraft systems development, and flight research. I had actually applied directly to NASA Ames and was contacted by Dr. G. Allen Smith of the NASA Guidance and Control Systems Branch. He had two Army positions allotted to his branch under the Army-NASA Joint Agreement, and he had already filled one with Terrence Gossett (who became a treasured friend). At the time, Colonel Cyril Stapleton led the Army lab, with Paul Yaggy as Technical Director. Alice Tice and Glen Ross were administrative assistants. My initial Army paperwork was administered out of the U. S. Army Northwest Procurement Agency, in Oakland, California, by Personnel Officer Alta Steengrafe and then by Brad Wilson of NASA Ames.

When Dr. Smith first contacted me in June 1966, I had just accepted a Teaching Assistant position at MSU, which would last through December of 1966. Dr. Smith agreed to hold the Ames position for me until January. At that time, H. Julian Allen was Director of NASA Ames with Leonard Roberts as Director of Aeronautics and Larry Clousing as Division Chief.

Prior to this time, I had completed 2 years at NASA Lewis (now Glenn) in Cleveland, Ohio, so this was a re-start of a 37-year career with the government (16 of which were with AARL and its successor organizations). After I retired in 2000, I returned to NASA as a consultant and rounded my career at 43 years. Regarding my Lewis years, I so disliked the surrounding Cleveland area, that I credit that experience with literally “driving” me back to graduate school. As I indicated in my prologue, that turned out to be a fateful decision, since at the time, MSU was one of a few universities pioneering in Modern Control Theory.

An interesting side story regarding my time in Cleveland has to do with the 10-unit complex where I lived. As often happens with close neighbors and, in this case several other young NASA engineers, we would get together for potlucks and parties. However, one of the engineers, Dan Goldin, often stayed late at work, which left his wife, Judy, to attend these events. Perhaps, even then, Dan had his sights on loftier goals . . . such as NASA Administrator.



“Research and Development! My goodness!”

In any case, upon graduation from MSU, I was off to California to start a new career (January 1967) and a new family (April 1967). The move resulted in a wonderful career and two wonderful children (Troy and Becky). The Army-NASA Joint Agreement was in its infancy, but it developed into a very productive bond of two organizations with common goals. My experience as an Army employee within a NASA branch was seamless and allowed for an expanded capability of both organizations.

X-14B VTOL Aircraft

Upon arrival at Ames, I was assigned to the X-14 vertical takeoff and landing (VTOL) aircraft project, within the NASA Guidance and Control Systems Branch. My co-workers were Terry Gossett (AARL) and Frank Pauli (NASA). The X-14 was originally built for the Air Force by Bell and first flew in February 1957 (the concept originated in 1955, as shown below). NASA had acquired the vehicle in about 1960 and had conducted a number of vertical/short takeoff and landing (V/STOL) studies with Stewart Rolls as project engineer and Fred Drinkwater as test pilot. On my arrival, in 1967, a new 100-percent-authority variable stability system (VSS) was being proposed for the X-14 and, at the time, the Army still had an interest in a variety of V/STOL concepts. Hence, Terry and I, with the support of Army funding, were committed to the effort.

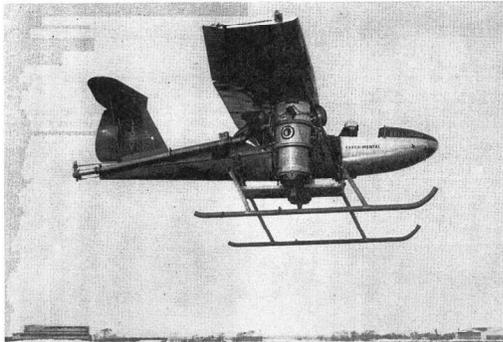
1955
Bell Aircraft's
Twentieth Year

BELL *Aircraft* NEWS

Two Decades
Of Aviation
Progress

Vol. IX, No. 6
Published by Bell Aircraft Corporation
February 7, 1955

Bell Develops Jet - Powered VTOL



FIRST PLANE TO APPLY JET PRINCIPLE for forward flight, Bell VTOL carries two 1,000-pound-thrust jet engines which lift craft from ground, rotate to provide forward thrust.

Revolutionary Aircraft Can Take Off Vertically, Then Fly Like Fixed-Wing Plane

Bell Aircraft has produced another aviation "first." Employing an entirely new principle of aircraft flight, Bell has built and flown a jet-propelled vertical-rising airplane which takes off and lands without need for a runway.

First of its kind to be flown anywhere, the new Bell development is a VTOL (vertical take-off and landing) airplane which uses twin jet engines to provide thrust for vertical operation and horizontal flight.

'Completely Satisfied,' Says Larry of VTOL

In announcing Bell Aircraft's VTOL, President Larry Bell described it as "the most significant single development in aircraft flight since the Wright brothers' first airplane."

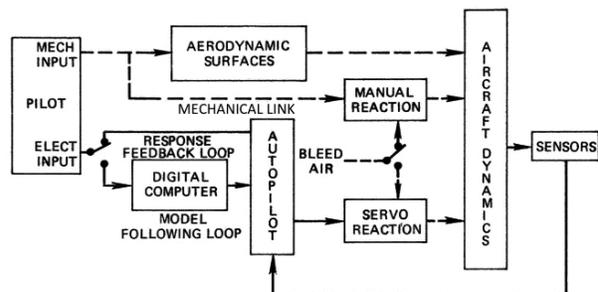
"We set out to prove a principle," he said, "and we are completely satisfied with the result. Our test vehicle is only the starting point. With operational aircraft of this type, military flight operations can be conducted right in the field in the same way our helicopters operated in Korea."

"Just as the helicopter brought

Company to Unveil XV-3 Convertiplane | **VTOL Handles Like Conventional Airplane** | **'Going Up and Down' Is Old Story to Howe**

Bell Aircraft jet-powered VTOL, 1955.

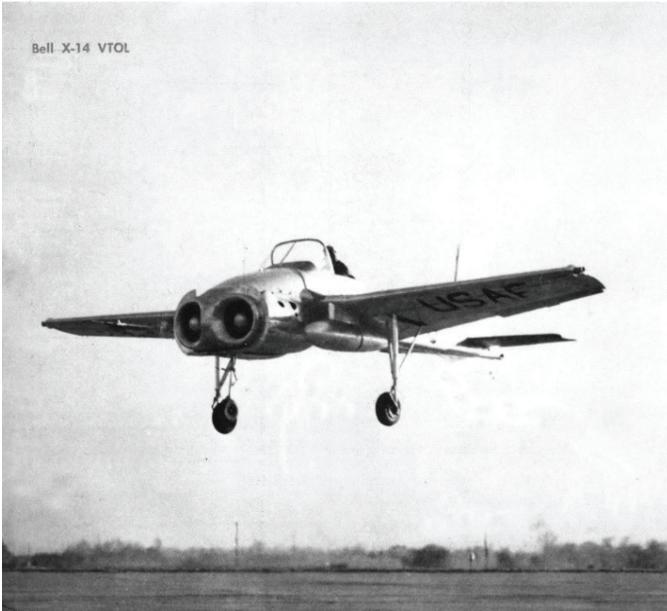
The new fly-by-wire hover control system would consist of an analog response feedback system and a digital model following system. Terry and I became very interested in the new discipline, called Optimal Control Theory, for determining the control system gains. The use of the quadratic performance index was at the core of this approach. Several papers [1-3] and a NASA report [4] resulted from this work. It's always dangerous to say "the first," but the digital system was certainly a pioneer in the sense of a fully digital model following control system, which used optimal control techniques to establish the system gains.



X-14B control system block diagram [5].

July 8, 1957 50 Cents
**AVIATION
WEEK**
A MCGRAW-HILL
PUBLICATION

**Bell X-14 Flies;
Transition Next**
•
**Doppler Systems
Enter Civil Field**



Bell X-14 VTOL first flight, 1957.

The aircraft modifications were significant enough to redesignate the aircraft as the X-14B. They gave the aircraft capability as an in-flight simulator for performing studies of near-hover handling qualities. At the time, there was a need to update the handling-qualities specifications, particularly MIL-H-8501A. In support of this effort, several activities were initiated, including a novel test stand, a flight test trailer, modifications to the engine diverters/seals, more powerful aircraft engines, support facilities for a first-of-a-kind digital flight control system, and first use of an onboard magnetic tape data acquisition system. Up to that point, flight data had been recorded on 6-inch-wide photographic film with several signals of various colors superimposed on each other. A light table, along with a good ruler and calipers were required to read the data. I particularly liked the sound of

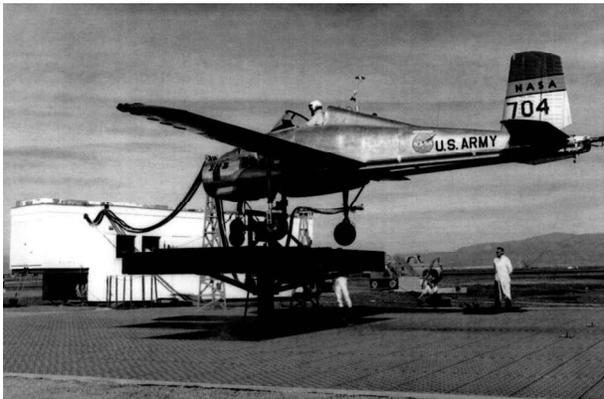
the word magenta and usually placed an important signal, such as roll rate, on that trace.

A Request for Proposal for the X-14B flight control system modifications was released in 1967, and other support systems were developed in house. Subsequently, Northrop in Hawthorne, California, was selected for the control system, with Richard Greif as the Ames contract monitor. The system consisted of new servo-driven bleed-air nozzles in pitch, roll, and yaw; height and engine control remained manual. This VSS system consisted of four primary boxes, a signal/sensor conditioner, an autopilot (for control feedback signals), a flight-worthy 16K/16-bit IBM CP-2 digital computer (for model following control) and a data adaptor to process computer input/output . . . only 16K, my goodness! IBM supported the computer, which was of the type that went to the moon on Apollo 11. Terry and I attended a software programming class in Boston for the unique computer language. The talented team at Northrop consisted of Joe (project manager), a first-generation Irishman; Rudy (the systems engineer), a German; and Ivan (the computer hardware and software engineer), a Russian. The three of them provided entertaining discussions when they argued over different approaches to solving an issue.

Acceptance testing at Ames was a lengthy process. In 1971, Terry Gossett became the Project Engineer, while my focus was on the CP-2 computer, the autopilot, and the signal conditioner box; Frank Pauli remained busy with the test stand activities. Ron Gerdes was the test pilot and Dan Dugan would later be named as backup. Dan flew the X-14B once before the program ended. Cy Sewell was crew chief, with Dave Walton as assistant. Lonnie Phillips was the electrician and Don Reynolds was our instrumentation engineer.

A very useful facility was developed in the NASA Ames Hangar for aircraft hardware-in-the-loop checkout; a TR-48 analog computer was used to represent the simple in-hover dynamics of the X-14. With the use of a ground power unit, all the aircraft systems could be powered and interfaced with the analog computer. The aircraft could literally be “flown IFR” in the hangar. Additionally, all system signals were made available on a large terminal board, and several strip chart recorders were available for data recording. Also, a field operating unit (FOU) was connected to the CP-2. This hardware-in-the-loop simulation capability was one that I would strongly advocate on all subsequent programs.

The X-14 tethered test stand (shown below) was another unique facility. It consisted of a sting with a trailer-hitch-type ball, on a scissor fixture, which was mated to a socket placed at the center of gravity (cg) of the aircraft. The aircraft could then lift off to 13 inches under power to the upper stop of the scissor and rotate about the ball. The capable motions were ± 9.7 degrees in roll, ± 9.3 degrees in pitch, and ± 6 degrees in yaw. To minimize the negative ground effects of the X-14, the test stand incorporated grating over a 5-foot pit and a hydraulic hoist to lift the aircraft and scissor/sting an additional 5 feet. Frank Pauli and Sid Selan were masterminds of this facility. In addition, a test trailer was fastened to the aircraft systems through flexible cables and



X-14B on tethered test stand.

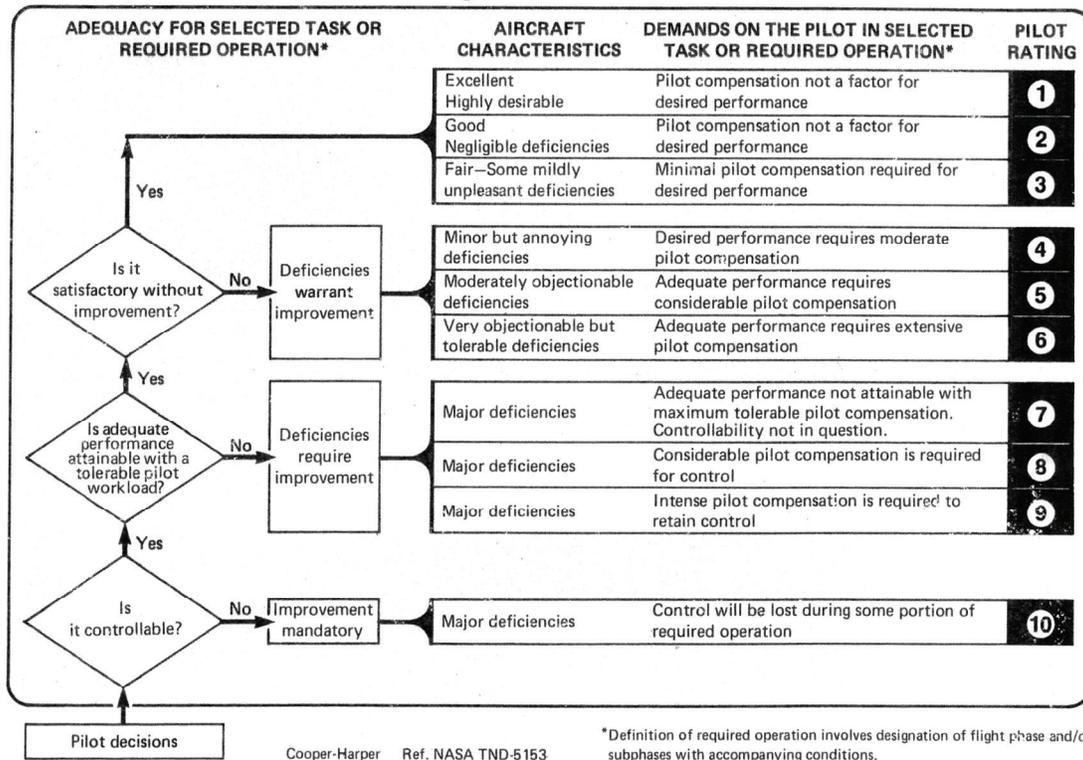
hence, all of the signals available in the hangar were also available in the test trailer for recording.

The initial experiment with the X-14B was on the test stand [5] and centered on handling-qualities studies of hover control concepts conducted on the Ames S.01 6-degree-of-freedom (6-DOF) simulator, by Richard Grief and Terrence Gossett in the mid-1960s. These hover control modes included acceleration command, rate command, and attitude command. The Cooper-Harper Handling Qualities Rating

Scale was the primary evaluation tool. In 1972, I extended those simulation studies to include translational rate command. I felt that was one of my more creative efforts, but because of a lack of editorial support at the time, I was faced with typing the report [6], which contributed to several typos/spelling errors (one of my many shortcomings). However, the findings did make their way into the Army helicopter handling-qualities specification update, Aeronautical Design Standard-33 (ADS-33).

Somewhere during the test stand effort, Terry transferred to the Army Air Mobility R&D Laboratory (AMRDL) Headquarters at Ames and Frank Pauli retired. By that time, the branches in Brad Wick’s division had been reorganized, and I was assigned to the Flight and Systems Research Branch, first under Maurice White and then Dr. James Franklin. Also during that period, I assumed the role of Project Engineer and was faced with orchestrating the first free flight, ground hover, of the X-14B. October 14, 1974, was perhaps the most tense day of my entire 43-year career. It was the day we would have test pilot Ron Gerdes lift the aircraft off in untethered hover, in what we would call ramp hover. It was indeed successful, but we were not yet ready for extensive near-ground hover testing.

To Lloyd. George E. Cooper



Cooper-Harper Handling Qualities Rating Scale.

Our first flight experiment was to verify the tethered test stand studies. To accomplish this, the X-14B was first outfitted with the new magnetic tape data acquisition system. The initial location chosen for this new box was on the rear cockpit wall next to the pilot, and close to the aircraft's cg. However, the additional weight of the data system was not a good prospect for the marginal thrust/weight ratio of the aircraft. In an ongoing quest to reduce weight, we had our electrician crawl into the cramped rear fuselage and remove all old and unnecessary wiring, achieving an important weight reduction of 40 pounds. After removing a portion of the original lead ballast in the nose to maintain the proper cg location, we still had the "dead weight" of the remaining lead ballast. After a bit of head scratching and a lot of convincing, we were allowed to move the data acquisition box to what looked like a wart on the aircraft's nose (see photo on the right) and allow the last of the nose ballast to be removed. So we were able to retain an acceptable thrust/weight ratio and maintain the required alignment of the cg with the thrust vector. The point of this discussion is to illustrate the need for a project engineer to think outside the box ... so to speak.



X-14B ramp hover.

After these preparations and a few others, we were finally ready for free flight data. From a flight safety standpoint, our first phase was a series of hover tests at an altitude of 2,500 feet over the San Francisco Bay. Without a peripheral reference, the pilot could determine that he was in hover by watching a piece of yarn on a stem, mounted just above the cockpit windshield. When the yarn went limp, the aircraft was in hover; this was a simple technique, but not recordable. Our initial flights were to validate the test stand handling-qualities studies, which resulted in two publications [7, 8].

The Ames photographer, Lee Jones, was always there in a chase helicopter (either the Hiller UH-12 or Bell OH-6) to capture every moment of flight, even photos that weren't always "politically correct," like a hint of the X-14B strafing Highway 101. Hover flights at altitude were necessarily short and demanded tight turns. As can be seen in the photos below, the X-14B was an open cockpit aircraft, and perhaps a one-of-a-kind for a jet aircraft.



Typical X-14B pre-flight activities; pilot Ron Gerdes, Lloyd Corliss, and photographer Lee Jones in background.



X-14B in hover at 2,500 feet altitude.



Return to base.

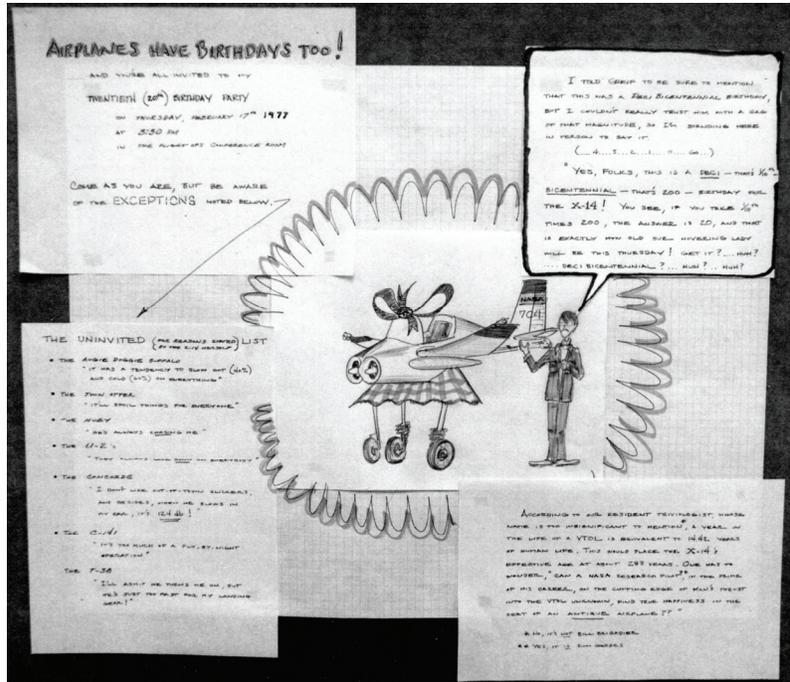
By 1977 it was clear that the Army's interest was centered on rotorcraft, nap-of-the-earth (NOE), which had been heavily influenced by Vietnam, and my efforts in that area will be discussed later in this chapter. As a result of this change in focus, I handed over the X-14 to another engineer, but not before observing the aircraft's XX (20th) anniversary of first flight; it was complete with a birthday cake. The celebration was also staged with an XX-formation photo of all Army/NASA personnel that had been associated with the aircraft over the many years at Ames. A few folks were not present, including Terry Gossett who was always on travel by then.



X-14B team on the 20th Anniversary. Front row: Fred Drinkwater, Jim Meeks, Lonnie Phillips, Jim Kozalski, Vic Bravo. Second row: Bill Carpenter, Sid Selan, Dick Gallant, Terry Stoeffler. Third row: Ron Gerdes, Lloyd Corliss. Fourth row: Cy Sewell, Dick Greif, Ed Vernon, Lee Jones. Fifth row: Dan Dugan, Jim Rogers, Dave Walton, Terry Feistel. Back row: Frank Pauli, Seth Anderson. Not pictured: Terry Gossett, Bob Innis, Stew Rolls, and Lawson Williamson.

Because of the longevity of the X-14, there are a couple of additional stories worthy of mention. I recall that on the aft fuselage, just behind the pilot's seat, there were the words "ARTHUR GODFREY SAT HERE." Also, I recall that during the checkout of the digital flight control system, the Northrop engineer, Rudy, would often step on the computer/data adapter box while entering the cockpit. In response to this, the Program Manager, Joe, stenciled the words "RUDY NO STEP" on the lid.

In 1981, the aircraft suffered significant damage and was retired to the NASA Ames Hangar. At the time, it had been the longest (in-flight status) X series aircraft, nearly 25 years, and I believe that record still stands. Later in the 1980s, an Army team from Ft. Rucker acquired it, along with the XV-5B fan-in-wing VTOL for possible museum display. The X-14 remained at Ft. Rucker, in a side yard, and was eventually destined for scrap when a private citizen and military armament historian, Rick Ropkey, recognized it during a visit to Ft. Rucker around 1990. He was able to acquire the vehicle, and he moved it to the Ropkey Armor Museum, in Indianapolis, Indiana, where it resides to this day. After a recent visit, I understand the vehicle may end up in the U.S. Air Force Museum in Dayton, Ohio.



X-14B 20th Anniversary celebration—artwork by Richard Greif.

Tiltrotor

My first brief involvement with tiltrotor aircraft began in late 1971. I remember being at a Director's review when Wally Deckert stepped to the front and said to Hans Mark, something like, "Are you ready to wake up?" I nearly fell out of my seat hearing such a bold statement addressed to the Director. Anyway, he then presented the notion of a new tiltrotor research aircraft. On March 20, 1972, Hans issued a memo (shown on the next page) that stated an agreement had been made between NASA and the Army to explore a tiltrotor proof-of-concept



Army/NASA Bell XV-15 tiltrotor.

Corliss, L.

NASA-Ames F:200-3
Moffett Field, California
March 20, 1972

MEMORANDUM for Staff

From: Hans Mark, Director

Subject: Tilt-Rotor Research Aircraft Project

The NASA and the United States Army have signed an agreement for the joint development and operation of Tilt-Rotor Proof-of-Concept Research Vehicles. Overall program management responsibility will be shared by OAST, through the Military Aircraft Programs Office under the direction of Mr. A. J. Evans, and by the Office of the Chief of Research and Development, Department of the Army, through Headquarters Army Air Mobility Research and Development under the direction of Mr. Paul Yaggy. The vehicles will be used in a flight research program to provide verification of basic tilt-rotor technology and to establish the necessary technological base to develop acceptable military and civil V/STOL tilt-rotor aircraft.

Project responsibility for the Tilt-Rotor Research Aircraft Project has been assigned to the Ames Research Center. There will be joint participation at the project level between the Ames Directorate, Army Air Mobility Research and Development Laboratory, and the Aeronautics and Flight Systems Directorate, Ames Research Center.

In order to provide proper management and technical direction for this project, the Tilt-Rotor Research Aircraft Project Office is established effective this date as part of the V/STOL Projects Office. Mr. Woodrow Cook, in addition to his duties as Chief, V/STOL Projects Office, is named Acting Project Manager. Further assignments of both Ames and Army personnel to this Project Office will be announced in the near future.

The Center is committed to support this project in the strongest possible way and I urge all of you who may be involved to give the Project Office the best possible support.

Hans Mark
Hans Mark

72/53

NASA/Army Tilt-Rotor Research Aircraft Project.

responses on pilot handling qualities. With knowledge from those studies, I devised a simple compensation algorithm for their fuel control, which I handed off to Gary; but I never checked to see if it was implemented.

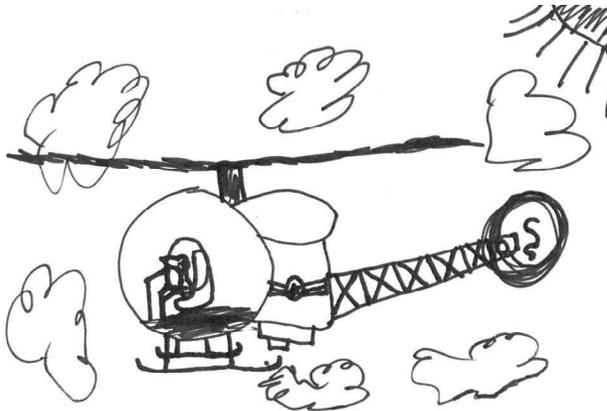
One additional tiltrotor experience came much later, after the Bell-Boeing V-22 Osprey had suffered several accidents and had been grounded. Through some source, the Navy obtained my name, and asked NASA Ames to allow me to be appointed as Chairman of a software review committee to review the V-22 software development process. We were fortunate to have several talented members on that committee from across the nation, and I asked that Mike Morrison (a contractor at Ames) accompany me. Since the V-22 is now performing successfully, perhaps our committee had some positive impact.

UH-1H V/STOLAND System

When Irv Statler became Director of the Ames Directorate in 1971, he began to expand the lab's research program beyond aeromechanics to address Army needs in the area of helicopter flight control and handling qualities. As Dr. Statler said, "It's now time to focus on rotorcraft." After

vehicle. As a result of that memo, a small team was sent out to several military installations to query the mission and need for such a vehicle. The 2000 NASA tiltrotor monograph by Maisel, Giulianetti, and Dugan on the XV-15 history [9] mentions this fact-finding trip on page 38 with "Shorty Schroers (a Project Office member) and two other engineers." Well, those two other Army engineers were myself (for flight controls), and Peter Putman (for avionics); Pete left Ames shortly after that trip and I've not heard about him since. However, following that trip, we wrote a joint 6-page trip report to Hans, and the XV-15 project was initiated a short time later as a joint Army-NASA undertaking with shared personnel and funding.

My next involvement was during flight tests of the resulting XV-15 when Gary Churchill approached me with an issue regarding the XV-15 engine/fuel control response. At the time, I had been conducting several simulations on the effects of rotorcraft fuel control



Turning to rotorcraft (artwork by Troy Corliss).

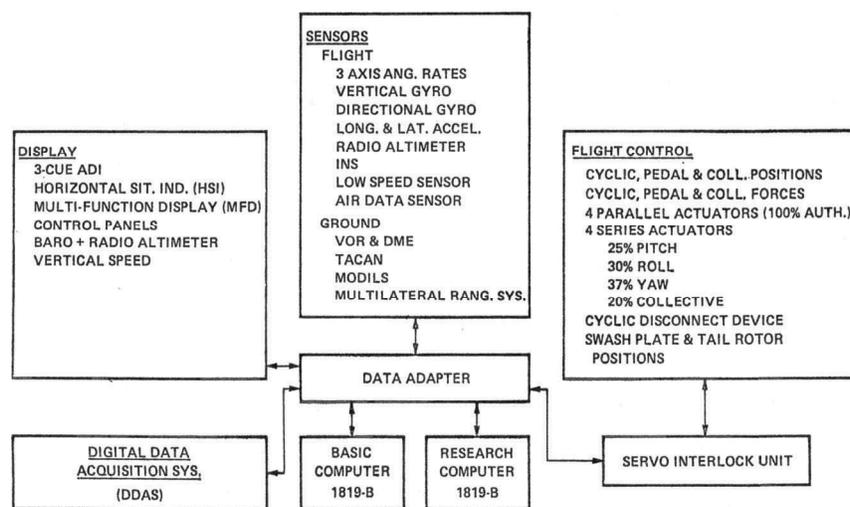
my initial involvement with the X-14B and XV-15 VTOL aircraft, I too turned my attention to rotorcraft.

During my transition to rotorcraft in the mid-1970s, I attended a meeting held by the NASA Guidance and Navigation Branch. George Xenakis was the lead engineer, and a proposal was being formulated to install a guidance and navigation display system on the Ames UH-1H helicopter to study rotorcraft approach and landing schemes. The system was to include series and parallel actuators on the control linkage to achieve autonomous flight. Sperry was in

attendance because of their experience in building the STOLAND digital guidance, control, and display system. In the meeting, I suggested that if we disconnected one of the two sticks, we could also provide a piloted fly-by-wire capability. There was a resounding response that there was no room for a stick disconnect device. However, George (with his never-say-no attitude) entertained an initial look at the idea. My idea for such a proposal had come from a colleague, Jack Ratcliff, who had installed a compact, limited disconnect mechanism in the linkage of the Ames F-100 fighter, giving the aircraft a variable stability capability. He still had a sample of this device, and he had explained how it worked. So, I carried the device into the next meeting and placed it in the center of the conference table . . . the rest is history. Sperry designed a similar disconnect/reconnect device, and we were able to give the research pilot fly-by-wire capability.

NASA Project Engineers Fred Baker and Dean Jaynes provided the contract monitoring and acceptance testing for the automatic digital flight and guidance system known as V/STOLAND [10]. They were key to the success of the build, and held regular and frequent review meetings.

With V/STOLAND, the Army and NASA at Ames entered the rotorcraft fly-by-wire business, and George Xenakis provided the in-house oversight of preparations for conducting flight research. First on the list was the need for a simulator to support hardware checkout, and for the generation of flight experiment profiles. For the simulator cab, we used a salvaged UH-1A fuselage, complete with hydraulic swash plate actuators. For the UH-1H math model,



Elements of the V/STOLAND system.

Peter Talbot was instrumental in writing the equations of motion, and validation was done by comparing simulation time histories with flight data. This math model was dubbed UNCLE [11].

As time progressed at Ames, Bob Chen developed a more comprehensive rotorcraft model, which was called ARMCOP. Toward the 1990s, as I became interested in parallel processing, we teamed with an outside vendor, Ron Duval of Advanced Rotorcraft Technology, Inc. (ART), who developed a blade element rotor math model and installed it using parallel processing [12].



Air-cooled rack of V/STOLAND equipment.

Since the field of view of rotorcraft can be very broad, the buildup of a new simulation out-the-window visual system was necessary for the simulator. We incorporated three large TV monitors, which could be rearranged to include the chin bubble (i.e., downward looking), depending on our particular simulation. An entirely new terrain board map and scanning camera system were developed. For the map, we visited Ft. Ord and Hunter-Liggett Military Reserve and flew over landscapes typical for conducting military operations. A terrain board was needed that would allow us to conduct simulations of NOE, bob-up, target acquisition, and slalom-type maneuvering. Using topographical maps and photos of areas in the Military Reserve, we could splice together a very suitable map. With the support and guidance of Dave Key, and several folks from NASA's Simulation Branch under Dave Jones, we were able to build a large terrain board. In later years, visual scenes would be created by computer generated imagery (CGI). However, during that time, I would make extensive use of this simulation facility, both in support of the checkout of the V/STOLAND system and in the development of flight test experiments.

One particular concern with the V/STOLAND system was the effect on the motion of the vehicle from a failed series or parallel actuator. To study this effect, we used two interlinked simulator cabs (shown in the figure on page 14). This two-cab arrangement was necessary since the research pilot's stick was disconnected from the aircraft linkage and the safety pilot's stick. The safety pilot needed to take control in the event of an actuator failure. This somewhat novel simulation arrangement had the research pilot seated in a UH-1A cab, and the safety pilot (or recovery pilot) seated in the S-01 6-DOF motion simulator. The motion simulator and stick would be driven along by the activities of the research pilot and, when unannounced failures were introduced, it was necessary for the safety pilot to take over control. No serious safety issues were encountered in this study [13], so approval for fly-by-wire operation was granted.

During one of these simulations and flight test studies it was noted that the single most helpful control augmentation was in yaw [14, 15]. Court Bivens had joined Ames during these studies



A typical terrain board.

and was mentored on both the simulations and flight tests. After my retirement he repeated and reconfirmed this yaw control result. Additionally, we conducted height control augmentation simulations for bob-up and target acquisition.

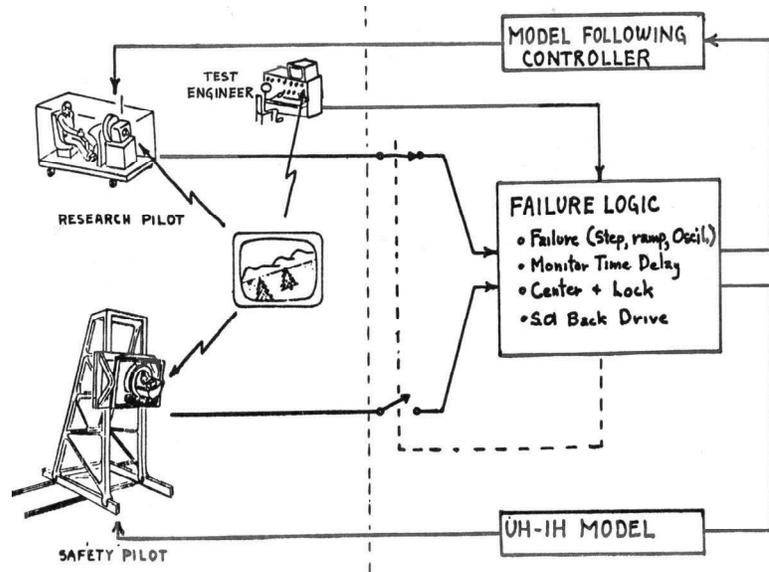
The UH-1H V/STOLAND system was used for a variety of flight experiments, usually flown at Crows Landing Naval Auxiliary Landing Field near Patterson, California. Several of my simulation experiments were repeated there, in flight. These mostly focused on NOE and slalom-type maneuvers, looking at cross coupling and lateral damping effects. Other experiments that I was involved in included a novel rate laser-gyro promoted by A. Carestia and interfaced by contractor Stan Schmitz. This Inertial Navigation System was also used in flight control and was compared with a conventional rate gyro [16]. Vic Lebacqz made use of the aircraft for instrument flight rules (IFR) handling qualities [17]. In addition, Fred Schmitz conducted a flight experiment using the VSTOLAND system to determine whether collective or attitude control was a preferred method for height control of a helicopter (attitude won out). Also, Dean Carico conducted helicopter field-of-view studies [18].



Researcher's station.

Transition From the Army to NASA

Perhaps my most extensive series of simulation studies occurred during the early 1980s and, as mentioned earlier, centered on the study of the effect of rotorcraft engine response, rotor RPM droop, rotor inertia, and height control dynamics on helicopter handling qualities [19-24]. Chris Blanken had just arrived at Ames, and he played a significant role in these simulations. As a result of this effort, I received a Department of the Army Research and Development Achievement Award in 1983. During this time I was wooed away from the Army to join NASA for the “famous” X-Wing program of the U.S. Navy and the Defense Advanced Research Project Agency (DARPA). The Army either wasn’t invited or chose not to participate in this program, perhaps because a portion of the flight envelope was to be fixed wing. Subsequently, the project was cancelled, primarily due to software and cost overruns on the quad redundant flight system. Software development for a flight control program of that scale was in its infancy, and if the X-Wing program made any contribution to the industry and my future, it was in the area of software management. Perhaps it also explains the Navy’s interest in me for the V-22 software review committee, mentioned earlier.



Two-cab actuator failure effects simulation configuration.



X-Wing VTOL concept.

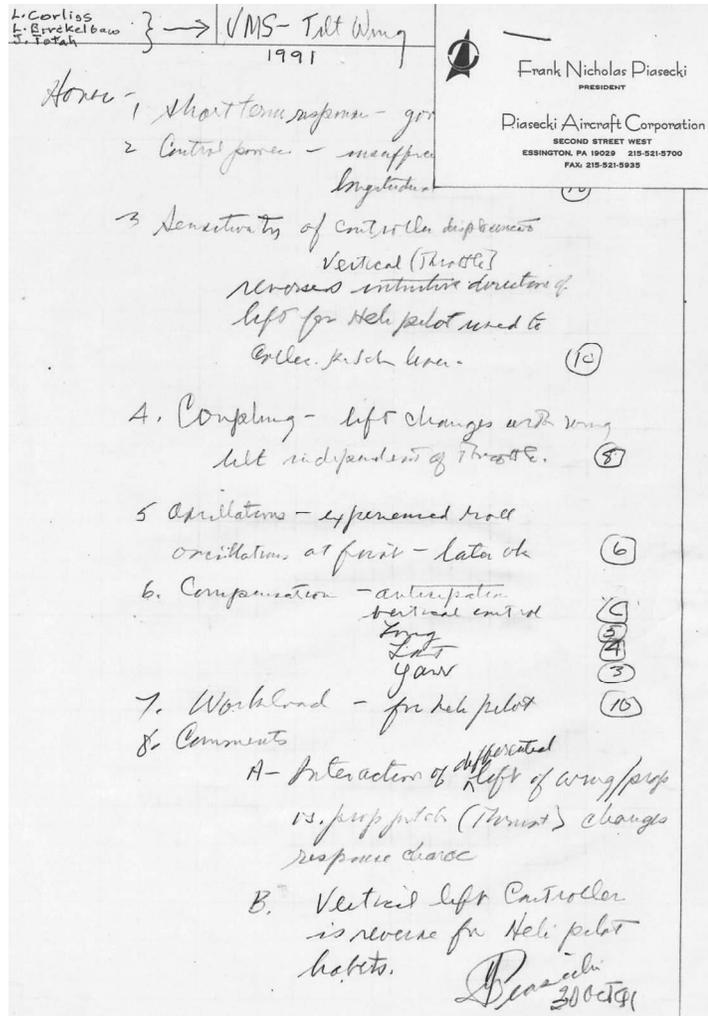
My experience on the X-Wing program also provided me with an abundance of lessons learned [25-27] and perhaps lent some help later to the tremendously successful X-36 tailless aircraft program.

One final side note: You never know who may visit while you're conducting a simulation. Late in my career, I was using the Ames "showpiece," the Vertical Motion Simulator (VMS), when who should walk in? None other than Frank Piasecki, "Mr. Tandem Rotor," and the last of the rotorcraft giants. I believe he was with his son, and as we discussed my simulation, he asked if he could try it out (as I recall, it was a tilting configuration). After he got out of the cab, he wrote up an evaluation. I know several of the co-authors of this collection of memoirs have brushed elbows with many renowned individuals but, for me, this was a unique experience.

Epilogue

As I have already stated, I feel that I, somewhat, stumbled into my exciting career, which began with a position under the Army-NASA Joint Agreement. I subsequently traveled through many remarkable flight research projects. Unfortunately, not all endeavors were completely successful, and that is the norm when new frontiers are explored. Flight research projects are always a challenge, and one must be prepared for a few failures in meeting all goals. However, sometimes programs do succeed, and I am proud to have been a small member of the team that carried the X-36 research aircraft (shown on the next page) to unquestionable success. When success is achieved on a program, it can be the best reward of our "industry." Research and Development . . . My goodness!

Before I end this memoir, Vic Lebacqz deserves a special note of thanks. We would often bring him in during the early program procurement stages. At critical meetings he had the most "pleasant" way of getting the contractor's attention, and his inputs consistently helped to drive a program towards success. We called him our "Hired Gun." Another very important person, who was always there to lend support and team spirit, was our faithful staff assistant Linda



Frank Piasecki visits the VMS and evaluates a VTOL, 1991.

Vollenweider. Because of the two l's in my name, she referred to me as la la Lloyd . . . and then just la la. That name followed me into my home life and beyond. It continued until I had grandchildren, then I finally became Papa Lloyd. Whew!



NASA X-36 tailless fighter agility research aircraft program team, from left: Mark Sumich, Program Manager, Lloyd Corliss, Gary Cosentino, and Dwight Balough, 1977.

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Index

A

Advanced Rotorcraft Technology, Inc. (ART), 12
Aeronautical Design Standard-33 (ADS-33), 5
Allen, Julian, 2
Army Aeronautical Research Laboratory (AARL), 1-3
Army Air Mobility R&D Laboratory (AMRDL), 5
Army, 1-3, 5, 8-11, 14-15
Army-NASA Joint Agreement, 1-2, 15

B

Baker, Fred, 11
Bivens, Court, 12
Blanken, Chris, 14

C

Carico, Dean, 13
Chen, Bob, 12
Churchill, Gary, 10
Clousing, Larry, 2
computer generated imagery (CGI), 12
Crows Landing Naval Auxiliary Landing Field, 13

D

Deckert, Wally, 9
Defense Advanced Research Project Agency (DARPA),
14
Drinkwater, Fred, 3, 8
Dugan, Dan, 4, 8, 10
Duval, Ron, 12

F

Field operating unit (FOU), 5
Ft. Ord, 12
Ft. Rucker, 9

G

Gerdes, Ron, 4-5, 7-8
Goldin, Dan, 2
Gossett, Terrence, 2-5, 8
Grief, Richard, 5

H

Hunter-Liggett Military Reserve, 12

I

IBM, 4
Instrument flight rules (IFR), 5, 13

J

Jaynes, Dean, 11
Jones, Dave, 12

K

Key, Dave, 12

L

Lebacqz, Vic, 13, 15

M

Mark, Hans, 9-10
Maisel, Marty, 10
Michigan State University (MSU), 1-2
MIL-H-8501A, 4
Morrison, Mike, 10

N

Nap-of-the-Earth (NOE), 8, 12-13
NASA Ames 1-2, 10
NASA Ames Hangar, 5, 9
NASA Guidance and Control Systems Branch, 2-3
NASA Guidance and Navigation Branch, 11
NASA Lewis Research Center, 2
Northrop, 4, 8

P

Pauli, Frank, 3-5, 8
Phillips, Lonnie, 4
Piasecki, Frank, 15
Putman, Peter, 10

R

Ratcliff, Jack, 11
Reynolds, Don, 4
Roberts, Leonard, 2
Rolls, Stewart, 3
Ropkey, Rick, 9
Ross, Glen, 2

S

Schmitz, Fred, 13
Schmitz, Stan, 13
Schroers, Shorty, 10
Selan, Sid, 5, 8
Sewell, Cy, 4, 8
Smith, G. Allen, 2
Sperry, 11
Stapleton, Cyril, 2
Steengrafe, Alta 2
STOLAND, 11

T

Talbot, Peter, 12
Tice, Alice, 2
Tilt Rotor XV-15, 9-11

U

UNCLE, 12
U.S. Army Northwest Procurement Agency, 2

V

variable stability system (VSS), 3-4
Vertical Motion Simulator (VMS), 15
vertical/short takeoff and landing (V/STOL), 3
vertical takeoff and landing (VTOL), 3-4, 9, 11, 14-15
Vollenweider, Linda, 15-16
V/STOLAND system, 10-13

W

Walton, Dave, 4, 8
Wilson, Brad, 2

X

Xenakis, George, 11
X-14 VTOL, 3-9, 11

Y

Yaggy, Paul, 2

An Army Experimental Test Pilot Remembers

Daniel C. Dugan
Army/NASA

“They were the best of years”—period! I became an Army experimental test pilot in November of 1965. That was a goal I had set back in 1961 when the Army sent me to Virginia Polytechnic Institute (VPI) to pursue a Master’s degree in Aerospace Engineering. My first assignment, after a 1-year tour in Korea, was to the Army Test Activity (ATA) at Edwards Air Force Base (EAFB) in California. The primary objective of ATA in those days was to get the performance and handling-qualities data into the flight manuals and other publications for Army helicopters and/or systems that, in some cases, were already coming off the production lines. These were the early Vietnam War years and ATA was very busy. After only a few weeks, over the Christmas holiday period in 1964, a quota for the Navy’s Test Pilot School came up. Did I want it? Absolutely! So, it was back across the country with my wife, three kids, dog, and cats to Patuxent River, Maryland, for 9 months of intensive flying (including jet transition), classroom work, and report writing. I was one of three Army pilots in Class 41, and we took both the Fixed Wing and Helicopter syllabi, which meant more classes and more flights and reports—but it was all worth it. Then I was back to EAFB as an Army test pilot.



Dan Dugan

My first project was the UH-1C helicopter, which was equipped with the 540-rotor system that was also installed in the AH-1G attack helicopter. We tested at Bishop, California (4,500 feet Mean Sea Level (MSL)) and in the White Mountains (11,000 feet MSL) to get hover and takeoff performance data, and later at Leadville, Colorado (9,700 feet MSL) to get zero wind conditions in the winter of 1966. Subsequent projects included the CH-47B Army Preliminary Evaluation flown out of Boeing Helicopter’s Philadelphia facility, the XM-30 cannon system, and a brief evaluation of the ACH-47A Armed and Armored Helicopter (Guns-A-Go-Go, one of four built for the Army).



Dan at Patuxent River.

After a 1-year assignment in Vietnam (Cu Chi and Long Binh), I was off to Ft. Leavenworth, Kansas, for the Army’s Command and General Staff College for a year. Upon completion in 1970, I was offered a nice choice of assignments: return to VPI to pursue a Ph.D., return to ATA (now Aviation System Test Activity) as a test pilot, or be assigned to the Army Air Mobility Research and Development Laboratory (AMRDL) at Ames Research Center. What was that all about? I contacted a friend from graduate school assigned there, Major Gordon Berry, and learned that it was a prime assignment that my family and I would enjoy.

In July of 1970, I reported to Paul Yaggy and then to my NASA boss in the Flight Operations Branch, George Cooper. As an Army employee under the terms of the Army-NASA Joint Agreement, I

Dugan, D.

worked in the NASA organization alongside NASA test pilots. It was like being a kid with a key to the candy store.

I was checked out in the T-33 and LearJet, for starters. Then I was given the last seven flights on the Army/NASA XV-5B, and I did a classic Navy handling-qualities evaluation of the XV-5B (that lift fan aircraft was very impressive). I recall hovering the aircraft over the taxiway on a very foggy morning, and the aircraft was so extremely stable with its Attitude Command system that, on one touchdown, I didn't even realize I was on the concrete. On one of the last flights before the XV-5B was to go into the 40- by 80-Foot Wind Tunnel for evaluation, I returned late and the field was supposed to be closed for a Navy Change of Command ceremony. I was making a 10-degree approach to a point and the aircraft behaved as if it were on a string—sliding down to a hover. There was no acceleration, even with the nose pointed down 10 degrees at the hover point. What a control system! At the time, I was looking at control with “collective” stick versus twist grip throttles for the two J-85 turbojets. The lift fan concept had lots of promise but was never adopted.

I was integrated into the NASA pilot staff as an Army pilot and had some very exciting opportunities. The next was the YOY-10A Rotating Cylinder Flap aircraft. A prototype OV-10 was highly modified—cut down CL-84 propellers, cross shafting, T-53 turboshaft engines of 1100 shp vs. the 700 shp Garrett engines, fixed landing gear, and four hydraulically driven, rotating cylinders immersed in the trailing edge of the 34-foot wing (production OV-10s had a 40-foot wing). It also had 90-degree flaps! The concept was pioneered by Professor Alberto Alvarez-Calderon from Peru. The idea was to energize the boundary layer over the flaps to avoid flow separation at high angles of attack. The test program proceeded without incident, but the aircraft had a shortcoming—negative static longitudinal stability. So, there I was at 30 degrees angle of attack with the stick on the forward stop! Not really a problem—just pull the power off and the nose dropped when the wing stalled. But, the only way to proceed was to increase the incidence of the horizontal stabilizer. That modification was considered too expensive for the perceived value of the program. The aircraft was grounded and put into storage. We in the Flight Operations Branch thought that the aircraft would go to some museum as a one-of-a-kind concept aircraft, but that was not to be. A man showed up from Ypsilanti, Michigan, one day to claim the aircraft for his “training” school! As an Army pilot, I was very disappointed and immediately contacted the Army's Aviation Systems Command in St. Louis; it was too late and they were not interested. It was a *fait accompli* by the General Services Administration. Well, I was able to get the two T-53s pulled and, along with the spare we had, sent back into the Army's supply system. The gent from Ypsilanti got the two valuable Garretts, though, and an aircraft that could never fly again.



Dan and the XV-5B.

Another unusual opportunity presented to me was checking out the Convair 990 and flying it to places like Brazil and Newfoundland (from Barbados). At 0.91 Mach, that jet transport aircraft was fast, even by today's standards. Yes, the position was so unique, and the opportunities so unlimited, that I turned down a promotion to Colonel in order to retire from the Army and

continue my career as a research pilot with NASA. It was Irv Statler's endorsements on my military efficiency ratings that put me on that promotion list.

About then, the tiltrotor concept [1] was getting a lot of interest from the Army and NASA. Bell and Boeing competed for the opportunity to build two tiltrotor research aircraft. A Source Evaluation Board (SEB) was formed, and in December 1972 I went to Boeing and Bell to see what they had in mind. The Boeing entry had some interesting features such as leading edge "umbrellas" to reduce the download on the wing in hover, and the engines remained horizontal as the rotor systems and transmissions rotated. Both concepts were "flown" in the Flight Simulator for Advanced Aircraft at Ames. Although the simulation models were somewhat primitive at that time, the piloted simulations did give Army and NASA researchers some insights into the designs.

But, it was Bell that had a lot of prior experience with the XV-3, and they had even produced transmission hardware for a Model 300 they were working on. The contract was awarded to Bell in early 1973, and the SEB moved into high gear. I had the opportunity to make some recommendations that were incorporated into the design of the now Model 301 or XV-15. My first recommendation was to change the ejection seats from the Bell-recommended Martin-Baker seat to the proven North American LW-3B. This seat was used in the Marines and Air Force production OV-10s. It had a number of impressive "saves," such as pilots walking away from an inverted ejection from the ill-fated X-19 at approximately 400 feet. Another impressive "save" was from a CL-84 that lost a propeller in flight and went into uncontrollable rolls. Again, both pilots walked away. In fact, this rocket seat was classified as "zero-zero," which meant that it would recover pilots from the ground while the aircraft was stationary.

A crashworthy fuel system was also on my agenda. The Army developed systems to prevent the many thermal injuries and fatalities from otherwise survivable crashes. They were very successful in that role. Although it meant thicker, tear resistant fuel bladders with a small loss of fuel capacity, it was well worth the safety that it provided. Breakaway fuel-line fittings, which sealed on impact, were also incorporated. I also suggested a "Conversion Guide" design, which was a 2-inch instrument in the shape of an engine nacelle with the pointer as a small spinner, prominently located on the instrument panel. It was graduated from 95 to 0 degrees with straight up indicating helicopter mode and 0 degrees indicating airplane mode. Bell added a nice feature—two nacelles co-located (one on top of the other), so if any asymmetry developed, a red area was displayed to catch your eye. An aside here: the MV-22 had a tiny, about 3/8-inch conversion guide showing in the upper left corner of their Multi-Function Display—a big mistake, in my opinion.

The program plan specified that wind tunnel testing would be conducted before the first flights, but because the program was running behind schedule in 1977, the decision was made to permit a 3-hour block of flight time at Bell's test facility in Arlington, Texas, in May. The strict limitations prescribed flight only in the traffic pattern, not to exceed 40 knots, and with the landing gear down. Two Bell pilots, Ron Erhart and Dorman Cannon, flew most of that time, but I was allotted the last 35 minutes for a brief evaluation. During this period, I spent many weeks at Arlington participating in long hours of ground running on a test stand Bell built, which allowed operation in all modes of flight, including full airplane mode.

In 1978, that XV-15 (702) was scheduled for a complete evaluation in the 40- by 80-Foot Wind Tunnel at Ames. Remote controls were installed to permit a wide range of power settings, RPM, and modes of flight. These tests were required and proved not only the safety of the aircraft, but permitted a wide range of conditions to be investigated. The second XV-15 (703), came on line in 1979 and, after ground-run trials, Bell began an extensive series of flight tests during which the first complete conversions to airplane mode were accomplished. During the ground-run investigations, the cabin door swung open in the airplane mode and the door was damaged, as well as the blade that hit it. The blade was beyond repair, but fortunately Ames had a right-side blade available. Flight tests were eventually moved to Dryden Flight Research Center for further evaluations.

We accepted the XV-15 N703NA from Bell at Dryden in October 1980. Ron Gerdes and I were to expand the flight envelope of the XV-15 in the “safer and more protected” airspace of EAFB. That was not the case—one pilot had to be on constant lookout for other aircraft in the area. It was scary at times. On one occasion, a B-2 flew under us at a few hundred feet, and on another occasion a T-38 did a loop around and over us. I don’t think that he ever saw us. On one notable flight, we did achieve or exceed 300 knots true airspeed. Frank Harris was trying to verify that speed from my flight notes where pressure, altitude, temperature, and indicated airspeeds were recorded. It was a relief to get the aircraft up to Ames in the spring of 1981. There we had Crows Landing in the San Joaquin Valley and a much less congested airspace. During this period, we brought in an Army pilot, Major Ron Carpenter, to participate in the flight tests. He and Ron Gerdes continued flying N703, while I got to go to the 1981 Paris Air Show with Bell.

That was a notable operation between the Army and Bell. Laurel “Shorty” Schroers was a key player for the Army, and Mike Bondi also supported the operation. The XV-15, N702, was shipped over to Farnborough, England, by C-5—courtesy of Hans Mark, our former Ames Director and then Secretary of the Air Force. Dr. Mark was always a staunch advocate of the tiltrotor and did much to advance its development.

After reassembly at Farnborough (wing and proprotor blades), practice flights were made there to adhere to the strict 6-minute timing allotted at the Paris show. It was time to ferry the XV-15 to Paris. A turboprop chase aircraft was chartered by Bell, and the first leg was north to Manston—a Royal Air Force Base used extensively back in World War II. After refueling and watching the weather for a while, the decision was made to launch for Paris even though there were broken clouds and some rain across the English Channel. Rain was bad news for the XV-15 because of sensitive blade instrumentation that could be ruined by the impact of water. As an aside, Shorty Schroers and Mike Bondi were located at Beachey Head, England, so they could monitor critical data channels as far as possible during the Channel crossing. Remember, this was a research aircraft with approximately 300 channels of data being recorded and a number of more critical channels monitored in real time including structural loads, temperatures, and such.



From England to France.

I was flying in the chase turboprop with a French pilot for whom everything was “No problem.” We were leading because the XV-15 did not have the necessary navigational radios, and the pilots did not need to be bothered with the voice communications in French airspace. Not long after the crossing, we started getting some scattered rain in the broken cloud conditions, and I elected to divert to a field we were passing at Beauvais. We flew in there to the amazement of the tower controllers and ground personnel. It was a good opportunity to refuel and have a little lunch while we waited out the weather. The sun finally came out, and it was blue sky from there to the French Air Force Base at Melun. Melun was our scheduled stop outside of Paris before the ferry flight to Le Bourget and the start of the Paris Air Show. An historical aside: Le Bourget was the field where Charles Lindbergh landed at the end of his transatlantic crossing in 1927.

Prior to the official start of the show, we had to demonstrate the flight routine to the show officials. Timing was strict, and all teams were threatened with diversion to Charles de Gaulle airport nearby should it be exceeded. That 6-minute demo was followed by 10 straight days of flying at Le Bourget—by a research aircraft. Mike Bondi, across the field with the data van, monitored critical channels for every flight. Early one day, an Italian turboprop landed gear-up on the runway, and after that the XV-15 was the only aircraft to fly. We didn’t need the runway! On another day, the weather was down and nothing flew—except the XV-15. We managed to fly the demo between rain showers with no damage to instrumentation. My duties included filing a flight plan each day and flying with either Ron Erhart or Dorman Cannon as their copilot. I timed the demo and I also had another important task—keeping the governor on line. There was a manual governor “wheel” immersed in the center console, and when large, rapid power changes were made (always during our demo), the governor would drop offline, which required resetting and caused a distraction. We found that by holding a thumb and forefinger on the wheel as it spun, it could be slightly slowed down and would not kick off. You had to apply just the right amount of friction or it would also drop offline if it was slowed too much—a delicate task. Another duty I enjoyed was always eating lunch in the well-stocked Bell Chalet.

The XV-15 was unchallenged as the star attraction of the 1981 Paris Air Show, and the U.S. Congressman, Department of Defense, and military brass in attendance sat up and took note. This exposure to the international aviation community played a large part in the start of the Joint Vertical Lift Experimental (JVX) military tiltrotor program with the Army initially as the Executive Service.

A personal aside—the United States and the Union of Soviet Socialist Republics were still in a Cold War status in 1981, but I ventured into the Russian pavilion with the objective of trading pins and such. I was led into a back room and there met “Boris” or whatever his name was—the guy in charge. We did a little “horse trading,” but his eyes really lit up when I produced a Tootsie Toy scale model of the Space Shuttle. Trading was good, and I got a lot of loot plus a few slugs of good Vodka. Was I responsible for the design of the Russian Buran shuttle? It sure looked like a direct copy of our shuttle—just very slightly smaller. When I returned to the Paris Air Show in 1989, the Russians had a full-scale mockup of Buran on display. Well, before I get visited by the Federal Bureau of Investigation or the Central Intelligence Agency for this revelation, remember they spent billions of rubles on the Buran and it never flew!

When the show ended, we ferried back across the English Channel to Farnborough. Before preparations for C-5 shipment back to Ames, we put on a demo for the Royal Aeronautical Society and it was well attended. John Magee also gave a lecture to the society one evening. One

issue that I am compelled to mention is that NASA got a free ride on the entire “Parisian adventure.” Bell paid for the expenses of the trip and their large contingent of support personnel. The Army paid the per diem for all the federal employees involved, and NASA paid my salary—period! NASA Ames even neglected the public relations opportunities and perhaps only mentioned that the XV-15 went to Paris in the Ames *Astrogram* employee newsletter—and not anywhere else.

Back at Ames, tests were continuing on the N703 and, at one point, a decision had to be made on releasing the N702 to Bell for essentially their marketing use. The XV-15 Project Office insisted that they have a say in which pilots Bell would bring in to demonstrate the XV-15. Bell said “No,” so I flew an “acceptance” flight in the N702 to officially place it back in the custody of Ames. That action caused Bell to acquiesce to our terms, and the N702 was put on an indefinite loan to Bell. It was also a banner day for me because I flew both XV-15 aircraft that day (I flew the N703 on an earlier data flight). It was also a reminder of the changes that Gary Churchill made to the 703’s lateral control system by increasing roll damping. In the airplane mode, lateral forces were higher and it felt like a larger, heavier aircraft. But, in ground effect, it was very stable, without the annoying lateral oscillation that would develop—especially with a “high gain” pilot in low ground effect.

As the XV-15 program proceeded, I had a larger AH-1G battery installed with an over-temperature warning system (compared to the original Bell 206 battery). I also had “landing” lights installed in the front of the “sponsons” for in-flight detection of the XV-15 by other aircraft during night operations.

Another very successful flight-test investigation on the XV-15 at Ames in 1985 was that of the side-stick controller. A three-axis side-stick was installed to the right of the pilot in the right side of the cockpit with an armrest. Simulation results were used to set the breakout and force gradients; however, from static tests in the hangar, the gains were reduced by 50 percent and never changed after that. The objective of the tests was to ascertain whether the aircraft could be safely controlled in the event of stability and control augmentation (SCAS) failure. Eight pilots flew evaluations and, without exception, all stated that the XV-15 could be safely flown with the loss of SCAS. The preferred side-stick configuration was 2-1-1, which meant that the side-stick was used for pitch and roll control only, while yaw and collective pitch (or throttle) were implemented with the pedals and thrust control lever, respectively. The data from this test were never published, but fortunately the senior engineer on the project from Bell, Roger Marr, took meticulous notes. From those notes, I published a qualitative report on the evaluation in 2000. By then, Gary Churchill had died, but he was included as a co-author of the report. Bell, the V-22 Program Office, and others later requested copies of that report, and it turned out to be very fortuitous that the investigation had been documented [2].

An unsuccessful investigation was that of Boeing’s Advanced Technology Blades (ATB). These blades were designed with changeable tips and cuffs along with a myriad of different fasteners. These fasteners would prove to be a serious problem as the program proceeded. One issue that seemed to slip by without much attention was that these blades were about 50 percent heavier than the original blades! This eventually resulted in the requirement for new, stronger steel swashplates to handle the loads.

Many delays were experienced. An opportunity to demonstrate a Harrier-type throttle was put on indefinite hold. What finally “killed” the ATB program was a blade cuff slippage in forward flight mode at Crow’s Landing that resulted in instantaneous, severe vibration and immediate landing. The aircraft was trucked back to Ames on a flatbed. That was the last flight of the N703 with those blades.

During the 1980s and early 1990s, Bell had been giving demonstrations to the military services all over the country and flying many distinguished visitors including Barry Goldwater, Secretary of the Navy Lehman, and Harrison Ford, to name a few. In June 1992, however, those demonstrations ended abruptly when the N702 was destroyed in an accident at Arlington, Texas. Both pilots walked away, however. The accident was not caused by the tiltrotor design or pilot error—it was just a common maintenance mishap. A cotter key had been left off a castellated nut on the left collective actuator, allowing the nut to back off over an unknown period of time. The bolt finally worked its way out. On the left side, that nut had to be observed by using a small mirror. Ron Erhart and the French pilot (Guy Dabadie) from Aérospatiale had just returned from a flight, and Guy wanted to see just one more hover maneuver on the taxiway. As the XV-15 started to hover, the bolt dropped out of the actuator, full left collective pitch was applied, and the aircraft rolled inverted and crashed. Fortunately, the pilots suffered only minor contusions. There was no post-crash fire, and the pilots were extricated when the side and overhead canopy removal system failed to operate—the actuating handle had not been pulled fully out of the receptacle. I was sent to Arlington, Virginia, to work with the National Transportation Safety Board, but the investigation was very brief. When the left nacelle was opened, there was the actuator bolt lying loose in the nacelle. Case closed!

Meanwhile, the now MV-22 Osprey was designed with a Harrier-type thrust control lever (TCL). The original helicopter-type collective control had been discarded by the Program Manager (PM)—a Marine from the Harrier community. This design resulted, as a secondary cause, in the loss of the no. 4 V-22 on its first flight, and a costly and lengthy redesign. Perhaps this loss could have been prevented if a Harrier-type throttle had been evaluated on the XV-15. The downward arc of the TCL was confusing, as “forward and down” was “up,” and “aft and up” was “down.” It was not hindsight that the helicopter community predicted that that convention would cause problems—especially in an emergency—which it did.

In those days, we had periodic air shows at Moffett Field, California. I extended the 6-minute Paris demo to 10 minutes at Moffett, adding a short takeoff and landing (STOL) takeoff and a 95-degree nacelle angle approach to a hover.

Back at Bell, demos were being conducted for the military services all over the country. Back at Ames, we were doing the “pick and shovel” handling-qualities and performance tests. One criticism was the dearth of reports published for some of the tests. Climb and Takeoff performance reports were published as Letter Reports by Jim Weiberg (I have rare copies), but they did not receive very wide distribution. Mark Tischler, however, did an excellent job of documenting “frequency domain identification of XV-15 dynamics” tests in references [3–6]. Gary Churchill was a brilliant flight controls engineer, and it was like pulling teeth to nail him down to write reports—but we got a lot of it done. One such effort was to document the entire series of XV-15 simulations from 1973–1981 [7]. Gary, by the way, was the only known passenger on any XV-15 flight. Back around 1986, some ground runs were being conducted with N703 on the Ames ramp and Gary was in the back of the fuselage tweaking some parameters.

Dugan, D.

There were no seats, only instrumentation. He called for more power from the pilot, Grady Wilson, and that he got. The aircraft lifted into a low hover and Gary became that rare passenger.

Around the time that the ATB program was terminated, the NASA Administrator, Dan Goldin, was not looking favorably at the rotorcraft program at Ames. In fact, he stated that “Rotorcraft technology was a mature discipline,” or words to that effect. Yes, we would be getting out of the rotorcraft business. In fact, Ames would be getting out of the flight-test business altogether. We conducted all the helicopter, vertical takeoff and landing (VTOL), and STOL flight research for NASA, but that was evidently deemed unimportant. In 1995, Goldin wanted to shut down all flight research activities at all NASA Centers except Dryden. Congress would not go along with that plan, but Goldin was able to get the flight operations at Ames terminated.

But, I digress. Let me discuss the V/STOLAND program briefly. A very sophisticated “autopilot” was installed in one of our UH-1H helicopters about the time that the XV-15 program was getting underway. It was a digital avionics system, developed by Sperry Flight Systems, for navigation, guidance, control, and displays. With the system engaged, the helicopter could automatically track a “racetrack” or holding pattern and maintain set airspeeds using the Microwave Landing System (MLS) installed at Crows Landing Naval Auxiliary Landing Field. Then, upon command, it could initiate a one- or two-helix approach (depending on altitude at initiation) to a fixed point at the approach end of the runway. Automatic deceleration to a hover was also accomplished. Pilots would then closely monitor and allow an automatic touchdown if winds permitted. A tight position hold over the touchdown point to allow landing was not always possible if crosswinds were significant. This restriction also applied to headwinds, because slow rearward movement was sometimes encountered, and the system had to be “punched off” while the pilot accomplished the landings. George Xenakis was the PM, and many talented Army and NASA engineers contributed to the program—Fred Baker, John Foster, Lloyd Corliss, Dean Jaynes, Bob Merrick, Leonard McGee, and Sam Liden. I was the project pilot for this very innovative and advanced system.

In those days, the MLS was a prime candidate for a more modernized approach system. Different approach azimuths and glide-path angles were selectable, which made it very versatile. In the final analysis, the Global Positioning System was selected over the MLS as it was cheaper and did not require expensive ground installations with the accompanying maintenance, calibration, and general upkeep requirements.



Dan and XV-15 N703NA at the Udvar-Hazy Center.

But, all good things evidently must come to an end and, as a result of the anti-rotorcraft leanings of the NASA Administrator, the Army-NASA Joint Agreement was “downsized” so to speak. Army personnel were no longer integrated into NASA organizations and could not be accommodated in NASA buildings, and vice versa. Furthermore, one of the most productive divisions at Ames Research Center, the Army/NASA Rotorcraft Division, was disbanded. In terms of a research organization’s only real product—the

technical papers, technical memoranda, presentations, and other dissemination of aeronautical data—that division was second to none.

The photo on the previous page is from the Steven F. Udvar-Hazy Center, the companion facility of the Smithsonian’s National Air and Space Museum, where the XV-15 N703NA has been on display since it was retired in 2003, 26 years after its first flight.

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Index

A

Advanced Technology Blades (ATB), 6-8
 Alvarez-Calderon, Alberto, 2
 Ames Flight Operations, 8
 Ames Research Center, 1, 8
 Army Air Mobility R&D Laboratory (AMRDL), 1
 Army-NASA Joint Agreement, 1, 8
 Army Preliminary Evaluation, 1
 Army Test Activity (ATA), 1
 Attitude Command system, 2
 Aviation Systems Command, 2

B

Baker, Fred, 8
 Berry, Gordon, 1
 Boeing, 1, 3, 6
 Bondi, Mike, 4-5
 Buran, 5

C

Cannon, Dorman, 3, 5
 Carpenter, Ron, 4

Churchill, Gary, 6-8

Conversion Guide, 3

Cooper, George, 1

Corliss, Lloyd, 8

Crows Landing Naval Auxiliary Field, 4, 8

E

Edwards Air Force Base (EAFB), 1, 4

Erhart, Ron, 3, 5, 7

F

Flight Operations Branch, 1-2

Flight Simulator for Advanced Aircraft, 3

Foster, John, 8

G

Gerdes, Ron, 4

Global Positioning System, 8

Goldin, Dan, 8

H

Harris, Frank, 4

J

Jaynes, Dean, 8

Dugan, D.

Joint Vertical Lift Experimental (JVX), 5

L

Le Bourget, 5

Liden, Sam, 8

M

Magee, John, 5

Mark, Hans, 4

Marr, Roger, 6

Mean Sea Level (MSL), 1

Merrick, Bob, 8

Microwave Landing System (MLS), 8

Moffett Field, 7

MV-22, 3, 7

N

NASA, 1-3, 6, 8

P

Paris Air Show, 4-5

Patuxent River, 1

Program Manager (PM), 7-8

R

Rotating Cylinder Flap, 2

Rotorcraft Division, 8

Royal Aeronautical Society, 5

Royal Air Force, 4

S

Schroers, Shorty, 4

short takeoff and landing (STOL), 7-8

Source Evaluation Board (SEB), 3

Space Shuttle, 5

Sperry Flight Systems, 8

stability and control augmentation (SCAS), 6

T

Thrust Control Lever (TCL), 7

Tischler, Mark, 7

V

vertical takeoff and landing (VTOL), 8

V/STOLAND program, 8

Virginia Polytechnic Institute (VPI), 1

W

Weiberg, Jim, 7

Wilson, Grady, 8

X

Xenakis, George, 8

XV-15, 3-9

Y

Yaggy, Paul, 1

Interface Between the Engineer and the Project

Bob George
Army

Prologue

I see my role, in the big picture, as an interface between the test engineer and the final results of a completed project. The art of conducting a test program—a problem with many sets of conditions—requires the application of many avenues of technical aid for its successful conclusion.

I learned and became (I hope) successful in aiding the engineer in preparing and conducting his/her project from what co-workers, whom I held in the highest regard, taught me. I grew in various stages while learning the art of interfacing from these co-workers and friends. Some lessons were quick; others took a while.



Bob George

My first contact with a wind tunnel was in 1956, when I took a position with the National Advisory Committee for Aeronautics (NACA) at the 14-Foot Transonic Wind Tunnel at Ames Research Center, Moffett Field, California, as an engineering technician, working under the direction of Charles F. Coe. There I learned the importance of clean and responsive data acquisition. It was at this point that I first became aware of the measurement of dynamic pressure data via the use of pressure transducers. Charles Coe designed and had a number of these devices built. These units were then installed on the surface of a model under test. These units were very simple, consisting of one leg of a Wheatstone bridge circuit, about 1/4 inch in diameter and 1/8 inch high, attached to the model with the balance of the network outside the unit. They were installed on the surface of the model in a pattern to sense the change of pressure over their surface caused by wind direction and/or model shape change. This was one of the major steps that shaped my career; it got me on the right path to being a positive aid to both the engineer and the project. I worked on wind tunnel projects from July 1957 through 1967. A couple of tests during that time were reported in references [1] and [2]. These projects led me toward further studies of dynamic pressure transducers.

When NACA was dissolved in 1958, the transfer of assets and personnel to NASA caused some shifting of work areas, and the 14-Foot Transonic Tunnel was one of them. In 1964 my friend and lead engineer, Charles Coe, went to the Unitary Wind Tunnel complex and I ended up in a new area. I worked on some acoustic-related programs, but it never felt like a good fit. I guess I missed the wind tunnel or, more likely, I missed working with Charles Coe.

In 1967 I moved to The Boeing Company in Seattle, Washington, as a wind tunnel specialist, and it was a good decision. My experience at the 14-Foot Transonic Wind Tunnel carried a lot of weight at Boeing. I was responsible for test planning for instrumentation needed during the various stages of testing in programs such as the Boeing 737, 747, and the supersonic transport (SST). I also traveled to other wind tunnel locations as part of my job. As it turned out, this was the second step in my path to becoming an interface.

Unfortunately, the Boeing 747 program was having serious difficulties with the Pratt & Whitney jet engine. This, in turn, caused Boeing to lay off lots of people. Lucky for me, I wasn't one of them, but I was worried about that possibility and, with our children getting older, I wanted a more stable environment. I knew a few folks at Ames from my previous employment. So I pulled all my lucky four-leaf clovers and rabbit feet together and made a stab at returning to NASA Ames. It turned out that there was an opening at the U.S. Army Aeronautical Research Laboratory (AARL) for an electronic technician, and there was also an opening at the NASA 40- by 80-Foot Subsonic Wind Tunnel with the same job title. I made a few inquiries, and the Army position sounded like a good bet as to what I was looking for. This turned out to be the right and final step in my search for the perfect position—the home I'd been looking for—a good interface between the test engineer and the desired results.

My life at AARL

I was introduced to my new position when I climbed the steps of Building N-215 on July 13, 1970, and met my new boss, Andrew Morse, who was waiting for me at the top of the stairs. The Army and Andy had been waiting for my arrival for some time (I had postponed coming to Ames until I had completed my last project at Boeing). Andy walked me around the Lab and introduced me to all of the various people I would be working with. He showed me where he had planned to station me for my duties as an electronic technician. It was in an area under the test section in the building where the 7- by 10-Foot Wind Tunnel No. 2 was located. (Note: very large flags went off. I could see where this was going, and I spoke up immediately). As it turned out, Andy Morse was an understanding person, and he listened to my concerns and my suggestions as to how my services might best be put to use. If my abilities were to be helpful, I needed to be located where my help would be needed. The area in the 7- by 10-Foot Wind Tunnel building was too remote, and I would come into the planning process too late to be of maximum benefit. I thought I'd be more helpful and of better use if I was located in Building N-215 with the test personnel doing the preparations and planning of various test programs. Andy decided that I should share an office with Frank Lazzeroni, which I did, and Frank really helped me become a full member of AARL.

Frank was the local expert on “flying saucers” and had a file on sightings. He was the person to confer with if any strange sighting was detected in or around our area. I never made any reports and often wondered if any were ever secretly made.

Memories of my early years with AARL have faded, as there were so many test projects between the various research and test engineers. After doing so many different tests, they all sort of run together. The latter years were more interesting as I became more involved in planning, as well as conducting, the test projects. But to reflect a bit on the early stages of my Army career, one thing I do recall was how some of the engineers were interested in my technique of using my workbook and my drawings of the test project, showing placement of both instrumentation and hardware. These drawings helped me keep all parts working (thanks to lessons learned at Boeing). I was pleased to find that some of the test engineers started using my methods in their plans as well.

We did a number of short test programs for the Army, aside from the normal in-house research programs. These were always interesting, as we met people with new and intriguing ideas, and they were so pleased when we could help them along on their road to discovery. Here are some

of the projects where I was a member of the team and one of the recipients when we received a commendation or formal expression of thanks.

- Wind tunnel tests of the “AEQUARE” Long-Range Remotely Piloted Vehicle were conducted. Andy Morse, Gil Morehouse, Don Adams, Gary Vander-Roest, Bob Gaines, Gene Wells, Art Cocco, Dave Ray, and I received commendation from USAF Colonel Allen of the Department of Defense, Wright-Patterson Air Force Base.
- Lockheed-Georgia Company was another outside entity that used our facilities. This particular test was for the “Knee Blown Flap Model.” The other members of the test team were Georgene Laub, T. Oesch, Ozzie Swenson, Gene Wells, and Bob Gaines. Our commendation came from J. E. Hackett and R. A. Boles of the Advanced Flight Sciences Departments at Lockheed-Georgia.
- A third such wind tunnel test was for U.S. Army Missile Laboratory, Redstone Arsenal, Alabama, in support of the FOG-M flight test program. I was the only member of the team singled out for commendation by K. W. Plunkett, U.S. Army Missile Laboratory.
- Wind tunnel tests were conducted for the Defense Advanced Research Projects Agency (DARPA) under a contract with Poseidon Research. Frank Lazzeroni, Georgene Laub, Wayne Empy, Art Cocco, Bob Gaines, Dave Ray, Ozzie Swenson, Gene Wells, and I were commended for work well done.
- Again, I had the good fortune to be included in a couple of particularly interesting oscillating airfoil dynamic stall programs that were led by test engineers Larry Carr, Ken McAlister, and Jim McCroskey [3].
- Appreciation was expressed to Georgene Laub, Bill Brandow, and me by our Army colleague, Martin D. Maisel, for our assistance in a two-dimensional airfoil download wind tunnel test that Marty directed.
- Jon Lautenschlager and I were commended for our contributions to the water tunnel test of airfoil circulation control conducted for Dr. Blair G. McLachan of NASA Ames.
- Bob Spencer of Boeing Vertol expressed his special thanks to Frank Caradonna, Dave Fluck, Chee Tung, and me for our support of his Boeing Model 360 helicopter rotor test. Kenneth I. Grina, the Boeing Vertol Vice President of Engineering, also thanked the entire Army Team.

A very large part of the next 10 years (1976–1986) was devoted to projects concerned with helicopter noise and involved both wind tunnel studies and flight test programs. I became a part of this dual program, providing instrumentation support and hands-on testing at various sites. These sites included the Army’s 7- by 10-Foot Wind Tunnel No. 2 at Ames; Edwards Air Force Base (EAFB); the large-scale, French anechoic wind tunnel, CEPRA 19; and the German-Dutch wind tunnel, DNW.

I must have been doing something right because I received many citations including:

- “High Level of Excellence” in May 1976.
- Cash Award for Suggestion: “Aircraft as Test Base for Ground Based Testing” in January 1977.
- Cash Award for Suggestion: “Improved Method of Transducer Calibration” in September 1980.
- The “Army Research and Development Achievement Award” in September 1982.
- The “Director’s Award for Exceptional Service” in November 1983.

George, B.

This seems like a good place to comment on the operation of AARL, which became the Ames Directorate of the U.S. Army Air Mobility R&D Laboratory (AMRDL) in 1970. Looking back now, I can see how the methods used by the Directorate leaders were successful in developing a team of people that enjoyed working together to achieve a set goal. The Directorate maintained a policy of high standards of operation and of openness where all members were encouraged to speak up and/or share ideas with each other. Each problem was solved not by one, but by many. It is unfortunate that it takes time to see the results. From my viewpoint, being at the support level, it was a real honor to have been a member of that team.



Team from top left: Fred Schmitz, Bob George, Don Boxwell, and Georgene Laub (bottom middle).

I recall a research project on the subject of “Blade Slap.” In the mid-1970s, I was a member of an exceptional Army/NASA team that invented a novel method of measuring rotorcraft impulsive noise. A Lockheed YO-3A, originally built as a surveillance aircraft, was flown in formation ahead of the test helicopter at selected airspeeds and rates of sink at which the helicopter was known to radiate large amounts of impulsive noise. Microphones installed at the wing tips and the tail of the YO-3A measured the magnitude, direction, and source location of the noise radiated from the helicopter.

There were good times and some rough times. One of the memorable times was when we were on the flight line at EAFB, in the dark at 4 a.m., preparing for takeoff ahead of the high desert winds. Still half asleep when setting up the test calibrations for the early morning flight, I picked up a yellow paint can, thinking it was a cleaning spray to clean the recorder heads. Lesson learned: remove all paint cans from test site!



Rotor-noise measurements.

Another memory of what can happen during a test run was when we were flying various test patterns across the open desert in Death Valley, heading towards California City (a one-time property scam that failed). The streets and buildings appeared deserted, devoid of people. The YO-3A was making dives at high speed using one of the deserted buildings as a target. I was in the Huey as chase and setting distance for the Huey pilot to keep during the

run. As we neared our target building, we discovered that it was not empty, because an old man appeared outside the doorway. He was just reaching for a coke from a vending machine on the front porch as the YO-3A arrived on target. He appeared to be a bit surprised!

One of the problems that always plagued our rotor acoustics experiments was extraneous noise, whether we were in the Ames 7- by 10-Foot Wind Tunnel No. 2, or in the CEPRA 19 wind tunnel in France, or in the DNW in The Netherlands, both countries with which we collaborated. The signals from the transducers were small, and it didn't take much background noise to wash out the real data. I was always fighting this problem, and often it could be cleared up with some small fixes, *but* sometimes it was not so easy. I had the French and Dutch folks chasing the noise. I think they often thought I was pulling their leg, but that was not the case. We had to dig deep and locate where it was coming from. We did.

Our pioneering work in rotor acoustics was well documented in many publications in the open literature, describing what we learned about this classic problem during the program. Just a few examples are shown in references [4-9]. The collaborations with the Europeans resulted in other publications including references [10-12].

Working away from home was generally lots of fun, and the opportunity to visit places like Paris and Amsterdam was wonderful. It was a great adventure, and I had the good fortune to make a lot of friends along the way.

My wife and I have since had a number of French, Dutch, and German friends come stay with us here in Washington State, bringing their spouses and then traveling beyond.

Well, this is a good place to stop and to reflect on everything we have done. I am so proud to have been a member of such a great team.



The acoustics research team with the YO-3A. Standing from left: Don Boxwell, Fred Schmitz, Bob Williams, Bob George, and Vance Duffy. Kneeling: Lee Jones.

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Index

A

Adams, Don, 3
AEQUARE, 3
Allen, Colonel, 3
Ames Directorate, 4
Ames Research Center, 1-2
Army Aeronautical Research Laboratory (AARL), 2, 4
Army Air Mobility R&D Laboratory (AMRDL), 4
Army Missile Laboratory, 3

B

Boeing Company, 1-2
Boeing Vertol, 3
Boxwell, Don, 4-5
Brandow, Bill, 3

C

Caradonna, Frank, 3
CEPRA, 3, 5

Cocco, Art, 3

Coe, Charles, 1

D

Death Valley, 4
Defense Advanced Research Projects Agency (DARPA), 3

Department of Defense, 3

DNW, 3, 5

Duffy, Vance, 5

E

Edwards Air Force Base (EAFB), 3-4

Empey, Wayne, 3

F

Fluck, Dave, 3

FOG-M, 3

G

Gaines, Bob, 3

German-Dutch wind tunnel (DNW), 3, 5

L

Larry Carr, 3

Laub, Georgene, 3-4

Lautenschlager, Jon, 3

Lazzeroni, Frank, 2-3

Lee Jones, 5

Lockheed-Georgia Company, 3

M

Maisel, Martin D., 3

McAlister, Ken, 3

McCroskey, Jim, 3

McLachan, Blair G., 3

Moffett Field, 1

Morehouse, Gil, 3

Morse, Andrew, 2-3

N

NASA, 1-4

National Advisory Committee for Aeronautics (NACA),

1

O

Oesch, T., 3

P

Poseidon Research, 3

R

Ray, Dave, 3

Redstone Arsenal, 3

S

Schmitz, Fred, 4-5

Spencer, Bob, 3

Supersonic transport (SST), 1

Swenson, Ozzie, 3

T

The Netherlands, 5

Tung, Chee, 3

U

Unitary Wind Tunnel, 1

U.S. Air Force (USAF), 3

V

Vander-Roest, Gary, 3

W

Wells, Gene, 3

Williams, Bob, 5

Wright-Patterson Air Force Base, 3

Army-NASA Ames Research Center Memoir

Peter M. Goorjian
Army/NASA

My college years, September 1957 to March 1969, were spent at the University of California, Berkeley, earning a Master's degree in physics and a Ph.D. in mathematics. When I was finishing my dissertation, I called the Army laboratory at NASA Ames Research Center, the Army Aeronautical Research Laboratory (AARL), to see if I could get stationed there. I went down for an interview and had the good fortune to meet Jim McCroskey. Jim got me a job at AARL for the summer of 1969. Jim also arranged for me to be assigned to AARL to fulfill my Army Reserve Officers' Training Corps (ROTC) commitment. My active-duty service was from October 1969 to March 1972 and after I finished basic training, I started working with Jim at AARL in January 1970. We worked on potential flow and actuator disk theory. Upon completion of my military commitment in 1972, I left AARL, then the Ames Directorate of the Army Air Mobility R&D Laboratory, and worked in academia. Then, in January 1976, I came back to work for NASA at Ames Research Center, where I remain today.



Peter Goorjian

Paul Yaggy, the Director of AARL, was a strong, competent leader. He interacted with the personnel in a personal manner, and he advised me on my work. He had two very competent "lieutenants" under him: Andy Morse, supervisor of the civilian personnel, and Major Gordon Berry, my commanding officer. There was a constellation of researchers there, including Frank Caradonna, Bob Ormiston, Larry Carr, Ken McAlister, Fred Schmitz, Dave Peters, Don Adams, Mike Kodani, Dave Sharpe, and Wayne Empey. In addition, there were several visitors who provided significant research collaboration, including Professor James Wu of the Georgia Institute of Technology and Professor Morris Isom from New York University. Morris gave lectures on transonic flow and handling the nonlinearities; I had no clue as to what he was talking about. I worked with Jim Wu on hovering rotors. Jim introduced us to the Chinese restaurant, Chef Chu's, one of the best in Silicon Valley.

In addition to the encouraging atmosphere for doing research, we had some enjoyable times at lunch. One event I vividly remember was a lunch on the cafeteria patio, just across the street from the offices that NASA had assigned to the Army. Jim McCroskey was there. Bob Ormiston had taken up hang gliding and he was describing his experience to us. R. T. Jones came up to our table and joined in the discussion on hang gliding. He had done much of the early theoretical work on swept wings during WWII while the Germans had obtained extensive swept wing wind tunnel data during that time. The talk turned to the question of human-powered flight and the Kremer prize for the first person to do the required flight and maneuver. As I remember, R. T. said that it was impossible. A few years later, someone did it.

In 1971, I was able to help another U.C. Berkeley graduate find his way to the Ames-Army Lab. My dissertation advisor, Professor Murray Protter of the math department, was on Bill Ballhaus' thesis committee as the outside member. Bill had a commitment to serve on active duty as an

officer in the Army, and Professor Protter suggested that Bill call me about being stationed at the Ames-Army Lab. I told Bill to call Major Berry; Bill did, and he got a summer job. He was hoping to be assigned there on active duty, but his only active duty was for 3 months of training at the Ordnance Officer School at Aberdeen Proving Grounds. Eventually Bill was hired by the Ames Directorate.

After I left the Ames Directorate in 1972, I taught mathematics for 3 years at the University of Delaware and at San Jose State University. My research was in General Relativity. I ran into Frank Caradonna at Ames and he told me that Bill Ballhaus (now with NASA; see Bill's chapter) had a job opening in the Computational Fluid Dynamics (CFD) Branch of NASA. I started work at NASA Ames in January 1976 and have been there since. The first day, Bill took me to look at some films of European research and we met Frank, who said, "Happy New Year." For several years, Bill and I, along with Terry Holst, worked together on transonic flows and on aeroelasticity in transonic flows. Later, Bill moved into Ames management and I continued to work in CFD, including work with Jim McCroskey. Later, my work was also in radar cross section, nonlinear optics, space optical communications, and the Constellation program.

The CFD Branch, under Harv Lomax, was at the epicenter of that field. The members included Barret Baldwin, Yoshiaki Nakamura, Bob MacCormack, Joe Steger, Bob Warming, Dick Beam, Paul Kutler, Lou Schiff, Ron Bailey, Bob Rogallo, Tony Leonard, Steve Deiwert, Bill Ballhaus, Terry Holst, Alan Wrey, and Tom Pulliam, to mention a few. There were experts in all aspects of fluid dynamics; as Bob MacCormack said, "This branch can do everything." It was an exciting time. Visitors came from all over the world and would stay for months or longer. Many young researchers earned their Ph.D.'s under an association between Ames and Stanford University. Many professors contributed to our work at Ames, including Holt Ashley of Stanford, in aeroelasticity.

The key to our success was the development of supercomputers at Ames. In 1976, Bill Ballhaus came back from a tour of labs in Europe and made a report to Hans Mark, Director at Ames. In it he said that the Europeans were just as smart as we were, and they knew just as much and were just as creative. The only advantage we had was our superior computer facilities. So, a very deliberate and thought-out effort was made to form a supercomputing facility at Ames. Ron Bailey, Bill Ballhaus, and Harv Lomax led that effort. On February 8, 1983, NASA headquarters approved the program, and the Numerical Aerodynamics Simulation (NAS) facility was built at Ames. It is now called the NASA Advanced Supercomputing (NAS) Division and is used for many different areas of research.

Bill Ballhaus convinced Jim McCroskey to become involved closely with his newly formed Applied Computational Aerodynamics Branch, so Jim was assigned to us by the Army and eventually moved with us into the new NAS building. Jim and I did some work on helicopter flow. He was having trouble adding coding for gusts and vortices into a code that I used, which had been developed by Bill. He kept asking me to add the additional algorithm, but the code kept blowing up when I tried. Then he came in one day with a working code; he had tried a new approach. Jim made the branch aware of the transonic flow problem on the advancing blade of the helicopter. I now share the corner office that Jim once had to himself. The collaboration between the Ames-Army Lab and NASA went the other way also. Joe Steger (NASA) and Frank Caradonna (Army) worked together on many problems in algorithm and code development for helicopters.

I also did some work on radar cross sections. Here, the knowledge of developing algorithms for fluid equations was applied to Maxwell's equations. Those equations are linear, so there isn't the problem of shock-wave-like behavior. I used my knowledge of algorithm development to solve the nonlinear Maxwell equations, obtaining solutions for solitons and light bullets, nonlinear microscopic objects in optical materials. (The nonlinear behavior of electromagnetic fields in these materials is determined by solving the nonlinear Maxwell's equations. The equations become nonlinear because the electric polarization is now determined by a nonlinear relation to the electric field intensity.) For the first time, the knowledge of algorithm development for nonlinear phenomena in one field allows one to solve nonlinear equations in a completely different field.

One of the most important projects I was involved in was in space optical communications. A laser terminal from the Lunar Laser Communication Demonstration project was "piggy-backed" on the Lunar Atmosphere and Dust Environment Explorer spacecraft. In orbit around the moon, on October 17, 2013, 311 Mbps (and on October 19th, 622 Mbps) of data were sent by laser beam from the space terminal to the Lunar Lasercomm Ground Terminal (LLGT), at White Sands Complex, White Sands, New Mexico. The telescope had a 10-cm aperture, a 15-micro-radian beam, and a 6-km spot at the LLGT, which was 400,000 km away. The pointing accuracy was in the micro-radians. The laser used 1/2 watt of power, and photon counting was utilized at the ground terminal. As a result of this success, NASA is developing a future network of radio frequency and optical satellites in a constellation around the Earth, Moon, and Mars. Exciting times!

I have fond memories of my days at the Ames-Army Lab and especially of knowing Jim McCroskey, Bill Ballhaus, and Frank Caradonna, who were such important, positive influences on my life.

Index

A

Adams, Don, 1
 Ames, 1-3
 Ames Directorate, 1-2
 Applied Computational Aerodynamics Branch, 2
 Army, 1-3
 Army Aeronautical Research Laboratory (AARL), 1
 Army Air Mobility R&D Laboratory, 1
 Ashley, Holt, 2

B

Bailey, Ron, 2
 Baldwin, Barret, 2
 Ballhaus, Bill, 1-3
 Beam, Dick, 2
 Berry, Gordon, 1-2

C

Caradonna, Frank, 1-3
 Carr, Larry, 1
 Computational Fluid Dynamics (CFD) Branch, 2
 Constellation Program, 2

D

Deiwert, Steve, 2

E

Empey, Wayne, 1

G

General Relativity, 2
 Georgia Institute of Technology, 1

H

Holst, Terry, 2

I

Isom, Morris, 1

J

Jones, R.T., 1

K

Kodani, Mike, 1
 Kutler, Paul, 2

L

Leonard, Tony, 2
 Lomax, Harv, 2
 Lunar Atmosphere and Dust Environment Explorer, 3
 Lunar Lasercomm Ground Terminal (LLGT), 3
 Lunar Laser Communication Demonstration, 3

Gootjian, P.

M

MacCormack, Bob, 2

Mark, Hans, 2

Maxwell's equations, 3

McAlister, Ken, 1

McCroskey, Jim, 1-3

Morse, Andy, 1

N

Nakamura, Yoshiaki, 2

NASA Advanced Supercomputing (NAS) Division, 2

NASA Ames Research Center, 1-2

Numerical Aerodynamics Simulation (NAS), 2

O

Ormiston, Bob, 1

P

Peters, Dave, 1

Protter, Murray, 1-2

Pulliam, Tom, 2

R

Rogallo, Bob, 2

S

San Jose State University, 2

Schiff, Lou, 2

Schmitz, Fred, 1

Sharpe, Dave, 1

Stanford University, 2

Steger, Joe, 2

U

University of California, Berkeley, 1

University of Delaware, 2

W

Warming, Bob, 2

Wrey, Alan, 2

Wu, James, 1

Y

Yaggy, Paul, 1

The Best Lab in the U.S. Army, 1965–1995

Terry Gossett
Army

Prologue

What a remarkable journey for a small-town Texas boy. The first step of my journey began when I left Texas on May 31, 1962, in a blue '55 Ford emblazoned with "California or Bust." I had just received a B.S.E.E. from Texas A&M, and I was a brand-new United States Air Force (USAF) Second Lieutenant with no clue about what lay ahead. Prior to leaving Texas, all my jobs had been in the lumber, paper, and oil field industries. Little did I know that I would soon be meeting, and working with, world-class engineers, test pilots, chief executive officers (CEOs), generals, visionaries, and astronauts throughout the aviation industry, and with NASA, the North Atlantic Treaty Organization (NATO), other Allies, and the Department of Defense (DoD).



Terry Gossett

My assignment was the 1137th USAF Special Activities Squadron with duty at Edwards Air Force Base (EAFB) where I was assigned to work on the X-15 program, one of the most successful research-aircraft programs ever conducted by NASA and the Air Force. The NASA branch chief I worked for, Jon Ball, made me project manager for the Hypersonic Flow Direction Sensor, fondly called the *Ball Nose*, designed to provide angle of attack, sideslip, and Mach number. As it turned out, the experience I gained in this assignment was critical to my ability to pique the interest of the yet-to-emerge leaders of a new Army lab at NASA Ames.

While at EAFB, I became familiar with the culture of NASA, the Air Force, and flight testing. I worked with people and pilots with *The Right Stuff*. After-duty hours, I attended classes at the University of Southern California (USC) and finished with an M.S. degree in Electrical Engineering with an emphasis on simulation. At that same time, Neil Armstrong was also taking USC courses for an M.S. in Aerospace Engineering.

NASA Dryden (now called Armstrong) Flight Research Center was led by Paul Bikle, holder of the world altitude record for gliders and a great leader who achieved many world records for the X-15 program. He led a cadre of premier test pilots: Joe Walker, Bob White, Neil Armstrong, Jack McKay, Bob Rushworth, Milt Thompson, Joe Engle, Bill Dana, and Major Michael J. Adams, who lost his life in the 191st flight of an X-15. The X-15 had a total of 199 flights. NASA helped Chuck Yeager prepare for his flight attempting an altitude record in the NF-104, as commemorated in the book, *The Right Stuff*, by Tom Wolfe.

In my opinion, NASA Dryden (Armstrong) is the best Flight Project Center in the United States of America, if not the world. In the 1960s, the X-15, YF-12, F-104, M2-F1/3, HL-10, X-24, Lunar Landing Research Vehicle, and many more were tested. The proximity to the Army Aviation Engineering Flight Activity (AEFA), the Air Force Flight Test Center (AFFTC), and the Rocket Test Center all made EAFB a truly busy place 24 hours a day, 7 days a week, 365 days a year.



X-15 with Neil Armstrong after flight (with his hand on the Ball Nose), 1962.

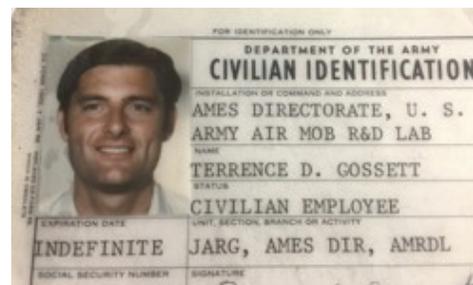
People are the linchpins for projects, and NASA Dryden was packed with talent. While Bikle was the director, he had numerous competent researchers, administrators, and flight support personnel. In David Fisher's chronicle of flight research at NASA Dryden [1], the foreword by Ken Szalai, Dryden Director in 2007, says, "The legacy of a research organization is the written history of its findings, whether the results were expected or unexpected, whether the predictions are valid or not." That report cites 2,900 technical publications from 1946–2006, including a Technical Note I co-authored with Chester Wolowicz [2].

Preparations for X-15 flights required all hands on deck to ready all systems. During flights, all key personnel manned their stations, and then, during pre- and post-flight briefings, we would listen and interact with the test pilot. One especially memorable post-flight debrief occurred when Neil Armstrong calmly described how he encountered a violent 12-Hz vibration while careening through space at Mach 5, which caused him to sequentially disconnect his roll, then pitch and yaw dampers, and take control manually. It should be noted that the primary resonant frequency of the X-15, the flight control system, and the Ball Nose were *all* 12 Hz. Neil had been in a serious pickle, but he was so relaxed that during his debrief I thought he might fall asleep. Recall that Neil later had control problems on his Gemini 8 mission; he was tumbling at 1 rev/sec and had to quickly take over control manually with backup thrusters. And prior to his moon landing, Neil also had to punch out of the Lunar Landing Research Vehicle in Houston.

Early Days at Ames

I am a very lucky guy. My USAF rater while at EAFB was Lieutenant Colonel Gil Morehouse, who knew about a new Army lab just about to start at NASA Ames, and, as I had decided to leave the Air Force, he urged me to apply to the Army for a job at Ames. The Army and NASA had entered into an agreement that included the establishment of the Army Aeronautical Research Laboratory (AARL) at NASA Ames Research Center, for collaboration in research of mutual interest to the two agencies. I took Gil's advice, applied, and accepted the Army's offer, even though it was the lowest paying of six great job offers, because I just felt that the Ames-Army Lab would be my best choice. (Shortly thereafter, Gil also joined AARL.)

When I arrived at the Ames-Army Lab in June 1965, I was assigned to a position in the NASA organization. A unique arrangement under the terms of the Army-NASA Joint Agreement was that my immediate supervisor could be a NASA manager even though I was an Army employee. My new NASA boss was Dr. G. Allan Smith, Chief of the Guidance and Control Systems Branch. G. Allan was a dynamo of a man from Yale University with advanced degrees from Stanford University. He strongly encouraged his employees to pursue additional education when off duty. Hence, after work, I was able to finish an M.B.A. at San Jose State University and do graduate work on Aeronautics and Astronautics at Stanford



(until “students” damaged the Reserve Officers’ Training Corps (ROTC) building in a night of rage, and the Army terminated support).

At the Ames-Army Lab, I was part of the seamless collaboration between the Army and NASA, and I got to work with, and learn from, top-notch Army and NASA engineers and pilots—experts in my field. The first person I met in the Simulation Lab was Tom Snyder (NASA), bent over an EAI 231 analog computer panel, rewiring it for a simulation. Dick Greif (NASA) led the Vertical/Short Takeoff and Landing (V/STOL) flight-control simulation and flight test team, including Emmett Fry (NASA), Lloyd Corliss (Army), pilot Ron Gerdes (NASA), and myself.

Army Lieutenant Colonel Thomas Nahanee Almojuela (the first American Indian graduate of West Point) was chief of ROTC at Stanford and later was an Army Research and Development (R&D) coordinator at the Army Air Mobility Research and Development Laboratory (AMRDL) headquarters (HQ). When Tom left Ames for another assignment, Wayne Mosher and I served as emcees at his dinner at the Moffett “O” Club. One of his gifts was a large aerial infrared image of the bay area. Tom, a bachelor, quipped, “See that hot spot in the lower, right? That’s my apartment!”

The first director of Ames Research Center (initially called Ames Aeronautical Laboratory) in 1940, Smith J. “Smitty” DeFrance, retained that position for 25 years. DeFrance established the standards and style that characterized Ames. Shortly after I arrived, H. Julian Allen (the genius behind the “Blunt-Body Theory” of re-entry aerodynamics) took over as the second director.

Harry Goett was initially the head of the 7- by 10-Foot Wind Tunnel at Ames (in 1947 he suggested to the National Advisory Committee for Aeronautics (NACA) HQ that Ames set up a “pilotless aircraft” test operation). Harry later became the Full-Scale and Flight Research Division Chief. This division included the 40- by 80-Foot Wind Tunnel Branch, and Harry would later be the first director for Goddard Space Flight Center. In 1982, I worked for Harry Goett when he headed an Army Science Board on unmanned aerial vehicles (UAVs). On the flight line George Cooper and Larry Clousing were kings. Life was very good.

I also had the opportunity to work with academics who were under contract to NASA Ames. For example, Dr. Smith assigned me to be the technical representative on a flight-control study being conducted by Princeton Professor Enoch Durbin and his team. On one of my visits to Princeton (during a period of huge unrest on college campuses in America) Professor Durbin invited me to his home for dinner with his family, and we all discussed values and free speech on campus. What a very kind and hugely memorable event with a wonderful family.

Led by Dick Greif, I was a member of a team conducting hover simulations on the S-01 simulator to evaluate flight-control systems; I worked with test pilots of the top V/STOL aircraft in the world whose participation Dick had arranged. They included Major Dave Tittle (later killed flying in the XV-5A), George Bright (who, a year earlier, had to bail out of the EWR VJ-101C in Germany), and Major Phil Neale from AFFTC (later killed while flying a Dassault Balzac V/STOL in France).

Many test pilots died in the 1960s advancing aeronautical goals and were eulogized in *Contrails over the Mojave*, by George Marrett, in his detailed account of their deaths. After eight F-4Cs crashed, Captain Marrett presented his test plan to Major General Hugh Manson, Commander, AFFTC, and was told, “Don’t kill yourself.”

When I started work in the NASA Guidance and Control Systems Branch under Dr. G. Allan Smith, the branch had been allotted two Army positions. The remaining position was filled by Lloyd Corliss, and we quickly established a close relationship. Lloyd and I, together with NASA engineer Frank Pauli, were assigned to build and evaluate a new 100 percent authority Variable Stability System for an X-14 Vertical Takeoff and Landing (VTOL) aircraft with Northrop as the contractor.

Lloyd and I developed a unique pre-filter, digital, model-following flight-control system, which gave the aircraft the capability of an in-flight simulator for performing in-flight studies of near-hover handling qualities. Lloyd describes this very successful project in more detail in his chapter. The results of this work were presented at several venues and documented in references [3-6].

Lloyd presented a paper on our work at West Point, and, in 1970, I was selected to brief the Assistant Secretary of the Army for Research, Development, and Acquisition (ASARDA) on the merits of our pre-filter, model-following control-system concept.

Then, Paul Yaggy, Director of AARL, asked me to present a briefing on our simulation results and the comparison of various control systems for VTOL aircraft to the Army Science Board during their meeting at Ames. Professor Rene Miller of Massachusetts Institute of Technology (MIT), a member of the board at that time, and I had a very “robust” discussion. As Paul and I were walking back to the lab after the meeting, he said he was pleased with my exposure to Rene. I grinned and said, “Yup, I feel exposed all right.” We both laughed heartily.

Our simulation results were further validated on the subsequent X-14 flight test program. During the program, many other NASA people became involved including Stu Rolls, Terry Feistel, and pilots Fred Drinkwater and Ron Gerdes.

An interesting side note is that Northrop, our flight-control-system contractor, selected the IBM 4Pi series digital computer, the same class computer that Neil Armstrong and Buzz Aldrin used on Apollo 11 to land on the moon; Apollo used the AP-1 version, and we used a CP-2 variant with similar programming. Lloyd and I went to MIT for 2 weeks to learn how to program the CP-2 for the X-14 flight test program. No one at NASA wanted a dead-end job but someone had to be the programmer, so it was me, Lloyd, and a Northrop engineer.

As an Army employee, I became involved at times in support assignments that drew on my technical expertise but did not involve research activity. However, I gained an understanding of Army requirements and translating them into specifications, and made connections with many influential people that were useful when I led major R&D projects.

In 1972, I was assigned as a member of the Source Selection Evaluation Board (SSEB) on the Utility Tactical Transport Aircraft System (UTTAS), the



The X-14 in the 40- by 80-Foot Wind Tunnel with Crew Chief Cy Sewell and Ed Varette.

precursor to the full-scale development of the Blackhawk helicopter. The SSEB evaluated several industry proposals to select the winner of the competition to produce the Army's future troop-carrying helicopter. As part of this extensive activity, I was assigned as the flight-control evaluation member of the Army team that included Charlie Crawford, Dick Lewis, Tom House, and John Shipley. We convened in St. Louis for 98 days. Of the over 20 SSEBs to which I have been assigned in my career, the UTTAS Board was the most thorough, and serving on it was the most educational for me.

Transfer to AMRDL HQ

In 1974, I transferred from AARL at Ames to the Advanced Systems Research Office (ASRO) at AMRDL HQ so, instead of being in a branch commingling with NASA, I was now in an all-Army group of specialists. Dick Carlson was the AMRDL Director, his deputy was a colonel on triennial assignment, and the ASRO technical specialists included Fred Immen for Structures, Dick Dunn for Human Factors, Bill Andre for Weapons, Andy Kerr for Aeromechanics, and Mike Scully for Preliminary Design.

John Wheatley was our preeminent propulsion expert. Mike Scully, a sharp MIT Ph.D., headed Aviation Preliminary Design efforts. Andy Kerr was our Aeromechanics Specialist and would later head the Aeroflightdynamics Directorate (AFDD) at Ames. Wayne Mosher was our Foreign Intelligence Officer, and he later became AFDD Deputy Director. Other key people included Dean Borgman and majors and/or lieutenant colonels as military R&D coordinators.

My specialties were avionics and systems. As such my duties were primarily to interact with other AMRDL directorates, mostly the Ames Directorate and the Eustis Directorate at Ft. Eustis, Virginia, but also with Aviation Systems Command (AVSCOM) Program Managers (PMs). In addition, I had considerable interaction with other major Army commands such as Communications-Electronics Command (CECOM), (Avionics Research and Development Activity, Night Vision Laboratory (NVL), and Signals Warfare Lab (SWL)), Intelligence and Security Command (INSCOM), and Army Training and Doctrine Command (TRADOC) (Combined Arms Combat Development Activity (CACDA), Ft. Sill, Ft. Rucker, and Ft. Huachuca). I also interacted with AVSCOM, Department of the Army (DA), and Defense Advanced Research Projects Agency (DARPA) as directed or requested.

In the late 1970s, Dean Borgman left ASRO to take over an office in St. Louis; he later "took over" McDonnell Douglas as President and then Sikorsky as President. When Dean left ASRO, he came by for a very detailed desk-side interview with each specialist to better understand their level of expertise, scope, and types of interactions with internal and outside agencies, as well as with industry. Dean had a keen understanding of the Army and the aviation industry. He impressed me greatly.

RPV, Aquila, and UAV

Remotely piloted vehicles (RPVs) have a long history—I gave a presentation at Stanford on that rich UAV history in 2006—but given the pace of emerging technologies and Army needs in the early 1970s, I soon found myself at the center of fast-moving events. In May 1974, the Army labs received an urgent request from General William DePuy, first commander of TRADOC, for an "over the hill reconnaissance with an unmanned aerial system." Clearly, he had read, understood, and believed the highly classified after action report on the October 1973 Yom

Kippur War, about the emerging merits of UAVs. In 1967, I had suggested an unmanned solution to similar requests for reconnaissance emanating from Vietnam. Then Ben Gadberg (NASA) explored radio-controlled (RC) flight capabilities while I investigated sensor and data-link possibilities.

In response to General DePuy, Professor Hank Velkoff from Ohio State University and I immediately headed to the Eustis Directorate of the AMRDL in Ft. Eustis, Virginia, to help develop a Request for Proposal (RFP) for what became the Aquila System Technology Demonstrator (STD). A NASA aircraft synthesis program, ACSYNT, developed by Tom Gregory, Pres Nelms, and Rod Bailey of NASA, proved very helpful in refining the UAV system description. Later that year, I was on the SSEB for the Aquila RPV at Ft. Eustis, Virginia. Lockheed Missiles & Space Company (LMSC) won, and Teledyne Ryan (TR) was second. TR protested. We had to quickly resolve the protest, so the SSEB and Lieutenant Colonel Davies Powers, Army RPV PM, tasked me to debrief the President of TR, Teck Wilson in San Diego, California, who subsequently withdrew his protest. The TR proposal was based on the Model 262 Manta Ray, a balanced observable RPV with great merit, previously tested in the Army 7- by 10-Foot Wind Tunnel at Ames.

However, the Aquila STD program had had 8 crashes in its first 13 flights; the program was stopped at the lieutenant general-level in Army Materiel Command. Andy Morse, of the Ames Directorate, headed the review team and I was Andy's deputy. We had a dozen national experts to assist us in recommending 77 changes to hardware, software, and procedures. LMSC quickly implemented all changes, and Aquila had no further crashes until flight #48. Major General Story Stevens, Commander of the Aviation R&D Command (AVRADCOM), visited LMSC to review the investigation of flight #48, which revealed that the cause of the crash was a procedural error by Air Vehicle and Payload Operators during a normal Return To Base. The Aquila STD program ended successfully in 1978 with a total of 218 flights.

Actually, there were two Aquila programs: the Aquila 6.4 Full-Scale Development (FSD), which was managed by the RPV PM (I served as his Tech Chief during the SSEB), and the Aquila 6.3 System Technology Demonstrator, which was managed by the Aviation Applied Technology Directorate (AATD) and required very few military specifications and standards. I maintained a Tech Rep office at the contractor, LMSC, for both programs. However, the Aquila FSD had unbelievably stringent specs and standards; furthermore, a series of DA/Office of the Secretary of Defense (OSD)-directed changes greatly increased the cost and complexity of the program. I participated in, and followed, both programs, and witnessed the Aquila FSD program cancellation.

In 1976, it became clear that RPVs needed a stronger technology base so, after briefings to AVSCOM, the Army Materiel Command (AMC), and the DA, I was appointed as Technology Manager for 6.2 Exploratory Development of Army RPVs, with an annual budget of \$1.5M. We quickly identified capabilities in the directorates of the AMRDL, with other Army labs, and across industry in flight controls, engines, structures, sensors, anti-jam data links, jammers, and strike RPVs. Bill Andre and I worked with Developmental Sciences Inc. (DSI), Dr. Gordon Harris, and Abe Karem to develop the successful Strike program. After briefings to AMC on our strike RPV results and a briefing from the Superfly program, Signals Warfare Lab, a special program office within DA, was created. Years later, in 1995, Tom House, AVRADCOM

Technical Director, assigned me to the Joint Precision Strike Demonstrator (JPSD) program at Topo Engineering Center Sensitive Compartmented Information Facility (SCIF) and the Pentagon for 6 months to exploit key strike technologies.

Lieutenant Colonel Davies Powers, RPV PM at AVSCOM, managed the 6.3 Advanced and 6.4 Engineering Development programs. I worked very closely with Powers and, in May 1979, I received the Outstanding Contributor Award for development of the RPV tech base at the meeting of the National Association for Remotely Piloted Vehicles held at the Hotel Del Coronado in San Diego. The banquet speaker was Richard Helms, former Director of the Central Intelligence Agency (CIA) and ambassador to Iran; his talk on Iran and Islam still haunts me today. Rear Admiral Walt Locke, Director of the Joint Cruise Missiles Project Office (JCMPO), was in attendance. Helms' CIA and Locke's JCMPO were two of the foremost sponsors of UAVs at that time.

In 1976, as local president of both the American Helicopter Society (AHS) and the Association of Remotely Piloted Vehicles, I put together a joint meeting with the objective of maximizing attendance. It worked, but only with copious help from Dr. Hans Mark, NASA Ames Center Director, and Cy Sewell, my past crew chief on the X-14. In conjunction with Maynard Hill, holder of 25 RC world records, we contacted all VTOL RC modelers on the West Coast for a Saturday morning "fly-in." Dr. Mark approved Saturday morning base access to the VTOL test area off Zook Road on Moffett Field, and Cy Sewell made crews and the NASA Ames Hangar available for 250 members and family attendees. Add coffee and donuts, 13 VTOL RC flight demonstration attempts, (including a tiltrotor), and movies in the hangar! It was a huge success, with many, many thanks to NASA.

My interactions with DARPA and Maynard Hill were just beginning, spurred by his many advances in electrostatic autopilots, components, and procedures for RC aircraft. After several briefings to our groups on the West Coast, Maynard invited me to present a paper about Army RPVs at one of his conferences to be held at the new Kossiakoff auditorium at the Johns Hopkins Applied Physics Lab. I accepted, but, as I approached the stage to make my presentation, Maynard said, "Terry, do you mind if I slip in another speaker?" I asked, "Who?" He replied, "Paul MacCready." Dr. MacCready headed AeroVironment, Inc. and had designed the Gossamer Condor that won the first Kremer prize. He was the recipient of a multitude of honors, including the American Society of Mechanical Engineers Engineer of the Century award. I was so in awe of Paul that I could barely speak when it was time for my talk, especially following his memorable and humble lecture.

Subsequently, Dr. Jay Sculley, ASARDA, called Dr. MacCready and me to his Pentagon office to see if we could adapt Paul's unique man-powered flight capabilities to the Army's RPV efforts. We all agreed, and Paul and I adjourned to a Pentagon City coffee shop, where Paul told me he had never worked for the Pentagon before. No problem. We crafted a statement of work on two paper napkins for what turned out to be the successful Hiline RPV program, later used in supporting programs for the Intelligence and Electronic Warfare PM and the Strategic Defense Initiative (Star Wars).

As a quick aside, AeroVironment, Inc., a development of the Army's Hiline program, led to efforts by NASA (1992–2003) on the Environmental Research Aircraft and Sensor Technologies (ERAST) program to support science programs on long-duration flight at high altitude. As part

of the ERAST program, AeroVironment built the Helios and Pathfinder solar-powered platforms. Helios set the world altitude record of 96,863 feet for sustained horizontal flight by a winged aircraft in August 2001. An Army engineer at Ames, Dr. Mark Tischler in the Flight Controls Branch, helped NASA and AeroVironment on system identification and adaptive control for the fragile craft—just *one* example of how Army initiatives also aided NASA programs.



Helios, Hawaii, July 14, 2001.

Colonel Dick Belton, Combined Arms Combat Development Activity—Materiel Integration Directorate (CACDA-MID), Ft. Leavenworth, Kansas, and I co-chaired a study for future UAV payload systems for the Senate Appropriations Committee and Undersecretary of the Army. The Soviet invasion of Afghanistan in December 1979 provided the urgency for the study. We gave our final briefing, in May 1980, to Dr. Walt LaBerge, Principal Deputy to the Undersecretary of Defense.

For a brief segue from UAVs to helicopters, in 1982 Wayne Mosher, Foreign Intelligence Officer, and Colonel Walt Urbach, AMRDL Deputy Director, asked me to be a member of a NATO Advisory Group for Aerospace Research and Development (AGARD) study group AAS 17, a 2-year study to counter Warsaw Pact helicopter operations up to the target year of 2005. All NATO (European) helicopter developers and defense ministries were on the team, including Agusta's Bepi Virtuani, Program Director of A129 and the Italian Ministry of Defence, Messerschmitt-Bölkow-Blohm and the German Federal Ministry of Defence, BAE Systems, Westland, and United Kingdom Wing Commander Rod Mundi, and Aerospatiale and the French Ministry of Armed Forces.

In 1984, General Max Thurman, Vice Chief of Staff Army (VCSA) requested a rank ordering of the Free World UAVs; we devised a scoring template extracted from the Aquila FSD, and the plan was briefed to, and approved by, the Army major general responsible for the Command, Control, Communication, Computers and Intelligence Surveillance, and Reconnaissance Office. I chaired the study and was joined by John Donnelly, NVL, and Bill Stephens, Applied Technology Laboratory of the Research and Technology Laboratories (RTL), successor to AMRDL. The Army UAV PM and his staff provided input. We rated the Developmental Sciences Inc. (DSI) R4-D as the best of all RPV candidates in the world, other than Aquila.

Immediately after our DA and OSD briefings on Free World UAVs, INSCOM initiated the Gray Wolf Project at the direction of General Thurman. I became chief engineer to adapt the DSI R4-D for the Outside Continental United States (OCONUS) night missions. Major Dave Crowley, INSCOM, and Major Joe Reames, AVRADCOM, were project leads and hosted an all-hands meeting at AVRADCOM, where parameters and responsibilities were quickly agreed on. Flight test started at EAFB in 1984. All systems were delivered to General Paul Gorman, Commander of United States Southern Command (SOUTHCOM), surpassing all requirements, on schedule and on budget.

Our successful UAV team efforts in support of AVSCOM PMs, other Army Labs, CACDA, Jay Sculley, SOUTHCOM, and General Thurman brought great positive exposure and recognition to our Army RTL HQ, and to the Aeromechanics Lab, successor to the Ames Directorate at Ames.

Crew Station R&D Facility

In 1985, General Thurman requested the development of a simulator to aid the Army in the design of the Light Helicopter Experimental (LHX), to compare the effectiveness of one- and two-crew stations in performing reconnaissance and attack missions, and to be used by the project manager to help him select the winning industry proposal for the LHX that became the Sikorsky-Boeing RAH-66 Comanche.

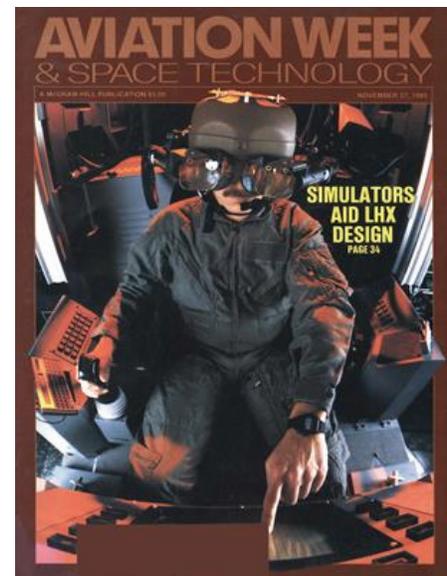
In response to this requirement, on May 15, 1985, Dr. Carlson initiated a program to develop the Crew Station R&D Facility (CSRDF) and assigned me to be its PM. Once again, I made use of NASA expertise. Dave Jones and Tony Cook of Ames, and their people, assisted our Army team that included Dr. Dick Dunn, Major (Dr.) Jim Voorhees, Dr. Nancy Bucher, and Doug Haller.

At the first CSRDF team meeting in mid-May 1985, we spent 3 long days using Venn diagrams to decompose our project into 11 distinct procurement elements, with very distinct technologies, names, and sub-elements. We then constructed a procurement strategy to maximize win-win outcomes with clear interfaces among all contractors, while minimizing probabilities for any nasty “finger-pointing” blame between contractors and/or the government.

Within 83 days, our team had awarded 11 contracts, and I had briefed all levels of management on the project strategy, from Major General Andy Andreson, PM LHX, up to Dick Ballard, DA. The full-mission simulator for the LHX/Comanche helicopter with 200 combat entities was developed in under 20 months. We established a Defense Development Share Program (DDSP) with Canada, so that CAE Inc. could be our prime contractor. The CSRDF was featured on the cover of *Aviation Week* in November 1989 with articles describing how simulators played a key role in LHX contractor selection. The CSRDF was demonstrated to the LHX teams at CAE Inc. in Montreal, Canada, and then installed at Ames at the direction of the LHX PM, Major General Andreson.

Shortly thereafter, General Louis Wagner, Commander of AMC, came for a tour of Ames and CSRDF, and he only had one question: “I know what you did, but how did you do it so fast?” I responded, “Sir, I cannot tell you that, but I can tell you that we complied with all Army Acquisition Regulations; however, we did have to use some agencies outside DoD. Whenever we sensed a procurement delay or impasse, we would switch procurement strategies and/or agencies.” Surprisingly, he accepted my brash response.

Just one more story about developing the CSRDF. When tasked by General Thurman and PM LHX to build the CSRDF, we knew that *the* key component was an Image Generator (IG), made by General Electric, called the Compuscene IV. We also knew that we had to get one immediately, and that the next available IG was being



CSRDF on the cover of *Aviation Week*, November 27, 1989.

delivered to the PM at Training Devices (TRADE), who, like AVSCOM, reported to AMC. I said, “Hmm, let’s send a letter to Lieutenant General Jerry Bunyard, Deputy Commander of AMC, and copy all parties including General Thurman, his boss, requesting delivery of the IG to us.” It worked! We got the IG we needed quickly, but it created some real friction with TRADE because that IG was scheduled for immediate delivery to Europe. We called that gambit “the sandwich.”

Just after completing the CSRDF, George Singley, Deputy Assistant Secretary of the Army for Research and Technology, requested that I assist in preparing a brief for Craig Fields, Director of DARPA, on Simulation Technology (Sim Tech). DARPA was the leading sponsor of developing distributed interactive simulations, and the CSRDF was an important aviation hub in that network. Our team consisted of Army, NASA, RAND Corporation, Bolt, Beranek and Newman Technologies, and USAF experts. We toured a dozen different simulation facilities within the U.S., including SIMNET (via Colonel Jack Thorpe, Ph.D., DARPA) at Ft. Knox, BBN in Boston, and Williams AFB in Mesa, Arizona. I gave the final brief to Craig at DARPA in November 1989 and recommended three priced alternatives for a Sim Tech facility at Ames to allow DoD leaders of all simulation domains (training, R&D, testing, logistics, war gaming, and operations) to view and select the appropriate level of Sim Tech for their needs. Craig approved the brief, but before proceeding with funding he was removed by President Bush.

There was an interesting and challenging consequence of our success with the CSRDF and the reputation we had acquired within the Army for expertise in simulation technology. The Association of the U.S. Army (AUSA) is the premier forum to showcase the legacy of the Chief of Staff, Army (CSA) to all other Army generals and CEOs of corporate America. General Gordon Sullivan, CSA, wanted to showcase Army Distributed Interactive Simulation Initiatives as his legacy in May 1993. In late January, I received a call from George Singley to create live networked demonstrations in May 1993 in Orlando, Florida. Our team and network included the CSRDF and our entire crew at Ames, and people and facilities from the aviation test bed at Ft. Rucker, the LHX/Comanche at Sikorsky in Stratford, Connecticut, and the Tank and Automotive Research and Development Command crew station in Orlando, Florida. The LHX Early Operational Capability team provided the tactically correct scripts, Colonel P. J. Penny, Director of Simulation at Ft. Rucker, narrated the 11 live presentations, and Mike Ferranti of Sikorsky manned the network pit.

Just prior to the live shows, George Singley asked me about the status of our live simulator demonstrations. I told him we were about to do a final, closed dry run, and Singley responded, “Great, I’ll get Max (General Thurman).” When I told our test crew that George and Max were going to sit in for the last dry run, George Sipprell, deputy PM for LHX/Comanche at Sikorsky, went ballistic. He said to me, “Tell them NO!” I told Sipprell, “I’m not going to tell them. You tell them.” He declined.

But all went well—very well in fact. At the end of the AUSA conference, General Sullivan awarded medals to key personnel, including the lead from CAE Inc., Lindsay McCullough, and me. We had clearly demonstrated our capability for a widely distributed, multiple-entity, interactive, tactically correct real-time simulation.

Several articles describing the CSRDF, Distributed Interactive Simulation, and the AUSA show appeared in *Army Aviation Magazine* [7-10].

The Simulation and Aircraft Systems Division

After my diverse earlier activities at AARL and AMRDL HQ, and the success of the CSRDF that entailed Army-NASA teamwork, my career culminated in a challenging assignment in 1987 when I returned to the Ames-Army Lab. At that time, it was called the Aeroflightdynamics Directorate (AFDD) and was headed by Andy Kerr. Andy assigned me to be chief of the new Simulation and Aircraft Systems Division (SASD). SASD relied heavily on the Army's access to NASA expertise, support, and facilities, and benefitted from the team work. I was assisted by four very capable branch and office chiefs.

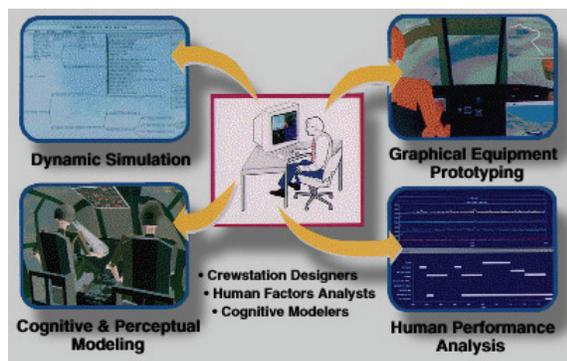


RASCAL on the flight ramp, NASA Ames, 1993.

Major (Dr.) Jim Voorhees and Dr. Nancy Bucher had joint responsibility for the operations of the CSRDF and for its use in networked full-mission simulations.

David Key headed the Flight Control Branch. A key research facility of the branch was the Rotorcraft Aircrew Systems Concepts Airborne Laboratory (RASCAL), developed and supported jointly by the Army and NASA. The RASCAL is a highly modified JUH-60A Blackhawk helicopter used as a variable stability in-flight simulator. While RASCAL was in development, Key's branch, with the participation of NASA Ames, completed a number of simulation and flight campaigns, many arising from international collaborations with the UK, Germany, and Canada, which laid the foundation for the Aeronautical Design Standard ADS-33, *Handling Qualities Requirements for Military Rotorcraft*.

The Computational Human Engineering Research Office was responsible for the development and utilization of the Man-machine Integration Design and Analysis System (MIDAS). MIDAS was a 3-D crew station prototyping environment with embedded human performance models (anthropometry, vision, cognitive, and motor response) that enabled computational design, visualization, and evaluation of complex man-machine system concepts in simulated operational environments.



MIDAS

MIDAS was an ambitious effort to provide human-in-the-loop ground-based simulation of helicopter cockpits—the equivalent of what computational fluid dynamics had become to wind tunnels for aerodynamic and structural design [11]. This long-standing project extended the Army-NASA Joint Agreement into the important and challenging human engineering domain.



FLITE with the Ames Army-NASA test team. Front row: Tom Reynolds, Nick Proett, Sean Hogan, Loran Haworth, and John Browning. Second row: Mary Kaiser, John Spooner, Richard Lee, Munro Dearing, Sue Laurie, Paul Aristo, Alan Lee, Zsolt Halmos, Zoltan Szoboszlay, Dick Denman, and Lee Mountz. Back row: David Foyle, Millard Edgerton, Ron Fong, Trudy Schlaich, Gary Leong, Linda Blyskal, Brian Hookland, Steve Timmons, Fran Kaster, Wendell Stephens, Alex Macalma, and Dana Marcell.

The fourth group, headed by Dr. Wendell Stephens and aided by Deputy Office Chief Major Loran Haworth, was responsible for the NAH-1S Flying Lab for Integrated Test and Evaluation (FLITE). This aircraft was used extensively by Major Haworth and Zoltan Szoboszlay for flight research in sensor, targeting, and crew station auditory and display symbology studies.

SASD collaborated closely with NASA flight, simulation, and human factors activities, especially in rotorcraft and vertical lift programs. The staff and facilities within SASD were very successful and productive, garnering significant external Army funding for payment of civil service salaries and developing a range of flight control, crew-system, and simulation technology for Army users, trainers, and industry.

The capabilities within SASD motivated AVRADCOM to organize a government-industry team to assist PM Longbow and McDonnell Douglas in conducting the Crew System Design Validation 3 for the Longbow Apache in 1991–1992. The team comprised Loran Haworth and representatives from the Army Human Engineering Laboratory, Army Research Institute, Simulation, Training, and Instrumentation Command (STRICOM), and AEFA. I was assigned as team chairman. Our mission was to finalize and validate the new glass cockpit design, as well as

crew interfaces, while allowing the TRADOC Systems Manager to explore tactics, techniques, and procedures for use of the new radar capabilities in full mission simulations. Subsequently, I received an award from the AVRADCOM Commander, with the citation “As Chair of the Longbow Apache Simulation Assessment Team...resulted in the most complete and best documented simulation evaluation of a crew station interface ever conducted on an Army aircraft.”

Another outstanding demonstration of the effectiveness of the Ames-Army enterprise came during Operation Desert Shield (August–October 1990) when eight Army helicopters were lost because their pilots flew into sand dunes in night operations while using ANVIS-6 image intensifier goggles. DA requested help. Two NASA vision experts, Drs. Mary Kaiser and Walt Johnson of the Ames Human Factors Research Division, rapidly designed a simulation study using the CSRDF and devised a solution called the “Double Delta Cueing Light Configuration.” Mike Kelly (NVL) quickly implemented and provided 100 kits using AN/PAQ-4 IR aiming lights for Desert Storm (January–February, 1991). It was a great example of rapid laboratory to fielding initiatives, thanks to Mary and Walt who were both subsequently granted a patent for their invention.

I retired from the Army in 1995; SASD was dissolved and restructured in 1996.

Retirement, Consulting, and Moving to Texas

In 1996, I began consulting on DARPA programs managed by Rod Bailey and Mark Sumich, NASA, for McDonnell Douglas (X-36) and Boeing (X-50), and I served as a member of the National Technical Advisory Board on the Boeing X-45A Unmanned Combat Air Vehicle. From 2000 to 2005, I was PM for Northrop Grumman for a High Tech Simulation Consortium of six companies and Stanford for the U.S. Army and NASA at Ames. Finally, in November 2006, I gave a presentation on the history of UAVs at Stanford.



Terry and Bee, 2016.

At the end of our remarkable journey, Bee and I moved to College Station, Texas—and immersed ourselves into Texas A&M—so that we could renew old friendships and share stories. Now my old Texas Aggie buddies and I delight in working together to help each other, and to launch students and cadets on their own remarkable journeys.

Epilogue

I found my “propitious niche” (an M.B.A. term) at the Ames-Army Lab located in the heart of Silicon Valley, nestled next to great universities and creative companies with world-class experts and facilities, enabling scientific discovery and rapid application to the development of technologies to support visionary national leaders.

The confluence of dissimilar technical achievements in computers, human systems interfaces, networking protocols, manned and unmanned systems, and strike capabilities evolved during the 1980s and were realized in the 1990s. Similarly, real-time, full-mission simulations among distributed interactive locations with hundreds of combatants fully immersed in realistic tactical scenarios also evolved and were realized. DARPA was the lead agency and sponsor in most of those domains. The Ames-Army enterprise had a major role as well, working closely with DARPA.

In my career at Ames, I saw a recurring paradigm of “technology push and user pull,” particularly in the environment established by the Army-NASA Joint Agreement. I viewed myself as an “outside” guy who was simply a *matchmaker* and a team builder, bringing talented experts together for Ames Army-NASA to respond rapidly to users’ urgent needs. For me, the technology visionaries were many: Paul Yaggy, Harry Goett, Hans Mark, Rod Bailey, George Singley, Abe Karem, Paul MacCready, and Maynard Hill. Military visionaries included USAF Colonel (Ph.D.) Jack Thorpe, DARPA; Major General Story Stevens, AVRADCOM; Major General Andy Andreson, PM LHX; General DePuy, Commander TRADOC; General Max Thurman, VCSA; and General Gordon Sullivan, CSA.

After our Ames Army-NASA teams were assigned a project, we would structure our approach, and then work within and across the “stovepipes” of our technologies and respective agency mindsets, as well as within needed industries, academia, and allies as appropriate. Speed was always essential. When our Army-NASA teams were given a job, we did not hesitate to invoke the name of our tasking authority to get their job done well, on time, and on budget.

I know that without the Army-NASA Joint Agreement most of the projects listed in my memoirs would not have been possible. I also know that, and as just one example, the cooperative development of CSRDF is *the* essence and *the* beauty of the truly collaborative agreement between the two agencies.

In sum, the Army-NASA Joint Agreement led the way in building “The Best Lab in the U.S. Army!”

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Index

A

Adams, Michael J., 1
 Advanced Systems Research Office (ASRO), 5
 Advisory Group for Aerospace Research and Development (AGARD), an agency of NATO, 8
 Aeroflightdynamics Directorate (AFDD), 5, 11
 Aeronautical Design Standard (ADS), 11
 Air Force Flight Test Center (AFFTC), 1, 3-4
 Air Force, 1-2
 Aircraft synthesis program, ACSYNT, 6
 Allen, H. Julian, 3
 American Society of Mechanical Engineers, 7
 Ames Directorate, 5-6, 9
 Andre, Bill, 5-6
 Andreson, Andy, 9, 14
 Aquila, 5-6, 8
 Armstrong, Neil, 1-2, 4
 Army Aeronautical Research Laboratory (AARL), 2, 4-5, 11
 Army Air Mobility R&D Laboratory (AMRDL), 3, 5-6, 8, 11
 Army Aviation Engineering Flight Activity (AEFA), 1, 12
 Army Lab, 1-3, 10
 Army Materiel Command (AMC), 6, 9-10
 Army Research Institute, 12
 Army-NASA Joint Agreement, 2, 11, 14
 Assistant Secretary of the Army for Research, Development, and Acquisition (ASARDA), 4, 7
 Association of the U.S. Army (AUSA), 10
 Aviation Systems Command (AVSCOM), 5-7, 9-10
Aviation Week, 9
 Avionics Research and Development Activity, 5

B

Bailey, Rod, 6, 13-14
 Ball Nose, 1-2
 Ball, Jon, 1
 Ballard, Dick, 9
 Belton, Dick, 8
 Bikle, Paul, 1-2

Boeing, 9, 13
 Bolt, Beranek, and Newman, 10
 Bright, George, 3
 Bucher, Nancy, 9, 11
 Bunyard, Jerry, 10
C
 CAE, Inc., 9-10
 Carlson, Dick, 5, 9
 Chief of Staff, Army (CSA), 10, 14
 Clousing, Larry, 3
 Comanche, 9-10
 Combined Arms Combat Development Activity–
 Materiel Integration Directorate (CACDA-MID), 8
 Communications-Electronics Command (CECOM), 5
 Compuscene IV, 9
 Computational fluid dynamics, 11
 Cook, Tony, 9
 Cooper, George, 3
 Corliss, Lloyd, 3-4
 Crawford, Charlie, 5
 Crew Station R&D Facility (CSRDF), 9-11, 13-14
 Crowley, Dave, 8

D

Dana, Bill, 1
 Dassault Balzac, 3
 Defense Advanced Research Projects Agency (DARPA), 5, 7, 10, 13-14
 Defense Development Share Program (DDSP), 9
 DeFrance, Smith J., 3
 Department of the Army (DA), 5-6, 8-9, 13
 DePuy, William, 5-6, 14
 Developmental Sciences Inc. (DSI), 6, 8
 Director of Simulation, 10
 Distributed Interactive Simulations, 10
 Donnelly, John, 8
 Double Delta Cueing Light Configuration, 13
 Drinkwater, Fred, 4
 Dunn, Dick, 5, 9
 Durbin, Enoch, 3

E

Early Operational Capability, 10
 Edwards Air Force Base (EAFB), 1-2, 8

Gossett, T.

Engle, Joe, 1
Environmental Research Aircraft and Sensor
Technologies (ERAST), 7,8

F

Feistel, Terry, 4
Ferranti, Mike, 10
Fields, Craig, 10
Fisher, David, 2
Flight Controls Branch, 8
Flying Lab for Integrated Test and Evaluation (FLITE),
12
Fry, Emmett, 3
Full-Scale Development (FSD), 6

G

Gadberg, Ben, 6
Gerdes, Ron, 3-4
Goett, Harry, 3, 14
Gorman, Paul, 8
Gossett, Bee, 13
Gray Wolf project, 8
Gregory, Tom, 6
Greif, Dick, 3
Guidance and Control Systems Branch, 2, 4

H

Haller, Doug, 9
Harris, Gordon, 6
Haworth, Loran, 12
Hill, Maynard, 7, 14
House, Tom, 5-6
Human Engineering Lab, 12
Human Factors Research Division, 13

I

IBM, 4
Intelligence and Electronic Warfare, 7
Intelligence and Security Command
(INSCOM), 5,8

J

Johnson, Walt, 13
Joint Cruise Missile Project Office (JCMPO), 7
Joint Precision Strike Demonstrator (JPSD) program, 7
Jones, Dave, 9

K

Kaiser, Mary, 12-13
Karem, Abe, 6, 14
Kelly, Mike, 13
Kerr, Andy, 5, 11
Key, David, 11

L

LaBerge, Walt, 8
Lewis, Dick, 5
Light Helicopter Experimental (LHX), 9-10, 14
Locke, Walter, 7
Lockheed Missiles and Space Company (LMSC), 6
Lunar Landing Research Vehicle, 1-2

M

MacCready, Paul, 7, 14
Man-machine Integration Display and Analysis System
(MIDAS), 11
Manson, Hugh, 4
Mark, Hans, 7, 14
Marrett, George, 3-4
Master of Business Administration (M.B.A.), 2, 13
McCullough, Lindsay, 10
McDonnell Douglas, 5, 12-13
McKay, Jack, 1
Miller, Rene, 4
Ministry of Defence, 8
Morehouse, Gil, 2
Mosher, Wayne, 3, 5, 8
Mundi, Rod, 8

N

NASA Dryden Flight Research Center, 1-2
NASA, 1-14
National Association of Remotely Piloted Vehicles, 7
Neale, Phil, 3
Nelms, Pres, 6
Night Vision Laboratory (NVL), 5, 8, 13
North Atlantic Treaty Organization (NATO), 1, 8
Northrop Grumman, 4, 14

O

Operation Desert Shield, 13
Outside Continental United States (OCONUS), 8

P

Penny, P. J., 10
Powers, Davies, 6-7
Program Manager (PM), 5-10, 12-14

R

Radio Control (RC), 6-7
RAND, 10
Reames, Joe, 8
Remotely Piloted Vehicle (RPV), 5-8
Request for Proposals (RFP), 6
Research and Technology Laboratories (RTL), 8-9
Reserve Officers' Training Corps (ROTC), 3
Rolls, Stu, 4
Rotorcraft Aircrew Systems Concepts Airborne
Laboratory (RASCAL), 11
Rushworth, Bob, 1

S

Sculley, Jay, 7, 9
Sensitive Compartmented Information Facility, (SCIF),
7
Sewell, Cy, 4, 7
Shiple, John, 5
Signals Warfare Lab (SWL), 5
Sikorsky, 5, 9-10
SIMNET, 10
Simulation and Aircraft Systems Division (SASD),
11-13

Simulation, Training, and Instrumentation Command
(STRICOM), 12

Singley, George, 10, 14

Sipprell, George, 10

Smith, Allan, 2-4

Snyder, Tom, 3

Source Selection Evaluation Board (SSEB), 5-6

Stanford University, 2-3, 5, 13

Stephens, Bill, 8

Stephens, Wendell, 12

Stevens, Story, 6, 14

Strategic Defense Initiative, 7

Sullivan, Gordon, 10, 14

Sumich, Mark, 13

System Technology Demonstrator (STD), 6

Szalai, Ken, 2

T

Tank and Automotive Research and Development
Command, 10

Thorpe, Jack, 10, 14

Thurman, Max, 8-10, 14

Tischler, Mark, 8

Tittle, Dave, 3

Topo Engineering Center, 7

Training and Doctrine Command (TRADOC), 5, 13-14

Training Devices (TRADE), 10

U

Unmanned aerial vehicle (UAV), 3, 5-9, 13

Urbach, Walt, 8

U.S. Air Force (USAF), 1-2, 10, 14

Utility Tactical Transport Aircraft System (UTTAS), 5

V

Varette, Ed, 4

Variable Stability System, 4

Vertical Takeoff and Landing (VTOL), 4, 7

Vertical/Short Takeoff and Landing (V/STOL), 3

Vice Chief of Staff Army (VCSA), 8, 14

Virtuani, Bepi, 8

Voorhees, Jim, 9, 11

W

Wagner, Louis, 9

Walker, Joe, 1

West Point, 3-4

White, Bob, 1

Wilson, Teck, 6

Wolowicz, Chester, 2

Y

Yaggy, Paul, 4, 14

Yeager, Chuck, 1

Army-NASA Memoirs

Dewey H. Hodges Army

I grew up on a farm in middle Tennessee, near Clarksville. With scholarships, including one from the U.S. Army Reserve Officers' Training Corps (ROTC), loans, summer jobs, and part-time jobs during the school year, I managed to complete a B.S. degree in Aerospace Engineering at the University of Tennessee at Knoxville in 1969, just as I turned 21. The Army ROTC scholarship had come with a 4-year commitment to the Army, but I was told that my active duty could be delayed if I were to receive a fellowship of national or international significance to attend graduate school. I received a telegram from a Stanford professor offering me a NASA Traineeship (a prestigious fellowship of international significance) to attend graduate school at Stanford University. The Army approved the delay, and I was ecstatic. I worked that summer at the Oak Ridge National Laboratory and could hardly wait for fall. I arrived in the Bay Area in September 1969.



Dewey Hodges

I was seeking a Ph.D. in aeronautics and astronautics and obtained an M.S. as a byproduct in June 1970. About that time, I started working at the Army Aeronautical Research Laboratory (AARL) at Ames Research Center, collocated with NASA. My advisor at Stanford, Professor Holt Ashley, had made sure that Bob Ormiston of AARL and I connected. I met Bob and soon thereafter met Jim McCroskey, Ken McAlister, Larry Carr, Andy Morse, Dave Sharpe, Bill Bousman, Gil Morehouse, Frank Caradonna, and many other AARL personnel. A new guy, Dave Peters, joined about the same time as I did. Dave had an M.S. degree from Washington University and became a friend quickly—and eventually a close friend.

In my first meeting with Bob, he outlined his goals for me, which included deriving equations for flap and lag bending of a cantilevered elastic rotor blade. At that time, there was much interest in the new so-called rigid or hingeless rotors that offered certain benefits by eliminating the flap and lead-lag hinges of conventional articulated rotors, but which had opened up concerns about potential aeroelastic instability. Bob had already started a derivation for a simple approximate model based on a rigid blade with spring-restrained flap and lag hinges, and he wanted to know the inaccuracy of modeling the elastic blade with the simpler rigid blade. We expected the approximate rigid-blade result to be reasonably close to the more elastic blade but, when the time came to compare our results, we were astonished to find a very large difference for typical blade pitch angles. After a good bit of anguish and head scratching, we determined that the difference did not arise from approximating the bending of the elastic blade with a hinged rigid blade, but rather from the fact that we had inadvertently represented the variation of the blade's structural properties with pitch angle differently. My flap and lag bending stiffness orientations rotated with the blade when it changed pitch, while Bob's flap and lag hinge springs did not.

In a stroke of brilliance, Bob figured out that solutions to these equations would exhibit behaviors representative of two types of rotors: elastically uncoupled in flap and lag, and



Margaret and Dewey, 1975.

elastically coupled in flap and lag. Moreover, he figured out how to generalize the result in terms of an elastic coupling parameter, R , to represent the distribution of blade bending stiffness inboard and outboard of the blade pitch hinge. Insights from this work led to significantly improved understanding of the potential design issues of hingeless rotors and also helped lead to U.S. Patent 3,999,886. My contributions to this work are shown in references [1-3]. Importantly, this work was expanded in scope over the next decade to address the more complex problems of torsionally elastic cantilever blades for a variety of configurations, as well as bearingless rotors.

When the summer was over, I went back to Stanford while working part-time at AARL. I had to keep the hours short to stay within the rules of the fellowship. The following summer I was back at AARL, now the Ames Directorate of the U.S. Army Air Mobility R&D Laboratory, full-time. I got married to Margaret in August of 1971. Four of our five sons were born while I worked at the Ames-Army Lab. I continued to work part-time during the next academic year, moving my office from Stanford to the Ames Directorate in spring 1972.

My work morphed into focus on elastic cantilever blades that are flexible in torsion as well as bending, leading to the now famous Hodges-Dowell equations. These equations have been used by many researchers around the world and are still found in the Army's Rotorcraft Comprehensive Analysis System (RCAS) computer code (as the governing equations for the NLB nonlinear beam element). I had the privilege as an Army employee (before and during active duty) to collaborate extensively with Professor Earl Dowell of Princeton University, starting while he was at AARL in the summer of 1972 on sabbatical. Later I was able to visit Professor Dowell at Princeton and close the loop on the now well-known Hodges-Dowell NASA Technical Note (TN) [4]. It remains to this day one of the most widely cited works among my refereed papers (now numbering over 200). My part of that TN was a significant portion of my doctoral dissertation, which I submitted in December 1972. Over the next few years, the Hodges-Dowell equations enabled us to explore the aeroelastic stability characteristics of a wide variety of hingeless rotor configurations [5-7], and also inspired many other researchers to begin to explore the field as well. I was ordered to active duty in February 1973, and in May returned to AARL full-time as an active duty officer (First Lieutenant), rising to the rank of Captain in July 1975.

One quite beneficial aspect of the Ames-Army enterprise was having the world's leading authority on rotorcraft aeromechanics, Dr. Wayne Johnson, right down the street. At the time, Wayne was an Army employee assigned to the NASA Full-Scale Aerodynamics Branch. I first met Wayne when the Hodges-Dowell TN was ready to be internally reviewed. Wayne was the chairman of the review team. I had no idea what to expect. Then came the written comments—almost as voluminous as the report itself. I had never seen so much red ink! The report draft was obviously in need of a lot of revision—but then there were the myriad misconceptions. For example, he kept insisting that there were no nonlinear terms in the equations! He wouldn't budge on this. It was almost unbelievable—I have no idea exactly what I said that got us moving—but I knew what a nonlinear term was. As a big brother may intimidate a boy, I felt a

bit uneasy around Wayne for some time. However, over the years I grew to appreciate his enormous capability and intellect. And his getting married really did turn him into a great guy!

Bob always wanted ways to validate the results of our derivations. Dave Peters and I discovered the literature of lateral-torsional buckling. We applied the Hodges-Dowell equations to the problem and found that the correction to the critical load was not quite right. I couldn't believe it! Dave found an old paper from the 19th century by Prandtl, and I found another paper, nearly as old, by Hans Reissner, the father of Eric Reissner. Eric was at U.C. San Diego (UCSD) at the time (mid-1970s). To our surprise, these papers gave results we found elsewhere in the literature, but they did not agree with each other. The geometrically exact equations used in the computer code Flexbeam Air Resonance (FLAIR) gave a still different answer. Now what does one make of that? Dave kept at it until he found an error in Prandtl's derivation. I was doing the same with Reissner's and found that, although he had used the geometrically exact equations, he had dropped a term that was of the same order as those retained. Now the corrected Prandtl and Reissner equations each agreed with that obtained from the geometrically exact equations with FLAIR. Dave and I wrote a paper [8] for the *International Journal of Solids and Structures*, edited by George Herrmann, my Ph.D. advisor.¹ Eric Reissner, whom I later met and visited at UCSD, wrote a nice tribute about the paper, saying it represented "a remarkable advance in the field... ." Privately, though, he was irritated that we criticized his father's work.

During this highly eventful period of ground-breaking research on rotating beam structural dynamics and aeroelasticity of hingeless rotors, many other government and university rotorcraft researchers entered the field, some of them no doubt inspired or stimulated by our results. This contributed to progress but sometimes it also led to heated controversy, debates, and letters to the editor over conflicting results. Such is the nature of research during times of rapid progress. It also produced some humorous situations. One technical dispute involved the influence of choosing a coordinate axis system rotation sequence (kinematics) to derive the equations of motion of a rotating beam. During a lively discussion after one such paper in the Dynamics Session of the American Helicopter Society (AHS) Annual Forum, a member of the audience questioned some assertions of the author. The presenter's co-author became so excited that he ran to the front of the room to voice his objection—while wrapping his arm around his head and grasping his nose at the same time to demonstrate the impossible rotation of the coordinate system.

By the mid-1970s, I wanted to move in the direction of modeling bearingless rotors. It was normal for me to pray about my work, and, having done so, I became very determined to pursue this new line of work, but Bob wasn't sold on it. Indeed, it became a source of tension between us—but he finally gave me permission to work on this exciting new direction after Boeing Vertol began development of an experimental full-scale bearingless main rotor (BMR) to be flight tested on a Bo-105 helicopter. The bearingless rotor analysis went from being verboten to "We need it NOW!" So, in a marathon session that lasted about 6 weeks, I derived geometrically exact beam equations for the flexbeam coupled to geometrically exact rigid-blade equations with pitch change hardware of four types. The equations filled pages and pages, so the coefficient matrices of the linearized equations were written on very large pieces of paper. Dr. Don Kunz

¹ Holt Ashley remained my advisor through my M.S. degree, but he was hard to find. I chose to work with George Herrmann for my Ph.D. He was hard to find too. My closest advisors were Bob Ormiston and Earl Dowell.

(a relatively new hire from Georgia Tech) was assigned by Bob to check over the derivation—which he did. He found a sign error and that was it. This was miraculous, and I rejoiced in what appeared to be a dramatic answer to prayer. The computer code written from the equations was called FLAIR and was intended to examine air and ground resonance [9-11]. In addition to its extensive use in supporting the Boeing Vertol Bo-105 BMR development, the work led to several papers, experiments, and more papers [12-14]). In 1979, I received the U.S. Army Research and Development Achievement Award citing my work on FLAIR.

The challenging and stimulating research described above was characteristic of the people and the environment in the early days of AARL. At the same time, the interactions of the staff in everyday events contributed to the unique atmosphere and added levity to the experience as illustrated in a few examples. The printer for our branch in AARL was in the computer room connected to the IBM 1800. It would sit idle while the computer ran a job, and then print the job at its end. It was not uncommon to run cases overnight or over a weekend for especially big jobs. If the printer jammed, the results would be lost, as it was impossible to read what was printed, and the computer lacked sufficient memory to store the results. One too many times it was loaded improperly, and I lost my entire weekend results. When I discovered it, I said, “Oh crap.” Gary Vander-Roest sharply admonished me: “Watch your language!” as I was making a sign to tape to the computer printer. The sign read, “If you cannot properly load the printer, don’t load it at all!” It was not a nasty note, but I was seething.

Once I submitted a whole stack of batch jobs to be run over the weekend, and Monday morning I was very pleased to see that they had all executed successfully. Then came the arduous task of plotting up all the results. As this was the mid-1970s, all plotting was done by hand with the aid of French curves (anyone remember those?) and graph paper. Hours and hours turned into days and days—all the while Bob was anxiously awaiting those results. Finally, I arranged to meet with him and carefully organized the plots on his table. Group by group he methodically sifted through them, nodding with approval for each group of plots—until he came to the effects of elastic coupling and cross-sectional inertias. He seemed at first perplexed, then alarmed, and then upset! “Why do cross-sectional inertias interact this way with elastic coupling? These are inertial terms! They shouldn’t be affected by elastic coupling.” I quickly answered (not a good idea), “That’s the way the numbers came out of the computer... .” Then steam started coming out of his ears (as it were), his face turned red, and he very sternly told me, in no uncertain terms, “If you ever say that again, you’re fired.” I never said that to him again. Later I learned that Bob, intending to be funny, was simply recounting a favorite anecdote he’d encountered during a summer job at Boeing, from a seasoned manager still coming to grips with newbie engineers and the emerging computer revolution.

In December 1970, I became a Christian. Soon thereafter, I met other Christians at Ames, and in 1972 I started a weekly Bible study and prayer group. Years later it was written up in the Ames newsletter and eventually (not kidding) by a national tabloid. Our group grew to number near 50 at its peak, and it continued until I left the Aeroflightdynamics Directorate (AFDD) in 1986. (I still lead a similar group at Georgia Tech.) The national exposure led a group of people to write letters to me. Amusingly, one was addressed to Dr. Dewey H. Hodges, NASA Research Group on Healing, Washington, D.C., and it actually found its way to me! This may have been another example of the seamless integration of Army and NASA personnel under the unique Army-NASA Joint Agreement in which I worked.

One of my office mates, Gil Morehouse, was around me a good bit in the months surrounding my conversion to Christianity. Having noticed changes in my demeanor and speech, he asked me one day, what was new in my life. I said, quite happily, “Jesus Christ.” Gil replied, “Oh, so you’re into blasphemy now?” “Not exactly,” I said, as I started an explanation that would take some time and patience. Eventually Gil joined my Bible study. He and I became close friends. I kept in touch with him and his wife until I got word he had died.

I had several other office mates over the years. Watching one of them, Mike Martin, fade away as cancer overcame him, was one of the most difficult things I had ever faced. He eventually died despite many prayers.

Another office mate was Chee Tung. Chee was very friendly, engaging, and fun to talk to—too much so, because it was hard not to chitchat. Margaret and I enjoyed meeting his children. He got us addicted to his lovely recipe for Chinese roasted peanuts marinated in star anise. Yum.

Then there was Stu Hopkins. By the late 1970s, I had become interested in analysis of the aeromechanical stability of bearingless rotor helicopters using a flexible multi-body dynamics approach. No one had previously done this, and Stu came along just at the right time to complement Don Kunz and me, who were both novices in the multi-body aspects. Stu was the ideal choice for architect of the General Rotorcraft Aeromechanical Stability Program (GRASP), which was initiated in the early 1980s. I served as the group leader; Don and I worked on the unsteady aerodynamics and rotor blade modeling, respectively [15-23]. Stu was extraordinary in his programming skills as well as his knowledge of multi-body dynamics. He was quite productive, but his estimates on how long it was going to take to accomplish certain objectives were always very optimistic. His estimates were far more realistic with this conversion factor: increase the units by one, and double the number (e.g., 2 hours became 4 days).

I had been developing my squash game since 1975. When Stu found out, he expressed an interest. He was a bit better than I was; he sent me an email once that said, “Beware the Black Knight.” I had no idea what that meant until our next squash match, when he pulled out a shiny new squash racquet called by that name. I had reason to beware.

Don Kunz and I never shared an office (at least not that I recall). He was a quiet man, a very competent dynamicist and aeroelastician. Though we worked together, our social lives did not intersect very much. I did know that he and Kathy Yaggy played racquetball regularly. He could not interest me in racquetball, and I could not interest him in squash. I developed a deep respect for his newly acquired abilities in computer science and data structuring—subjects that seemed really foreign to me. The important connection between us is that Kathy Yaggy is a Christian, and she may have influenced him to become one after I left AFDD. Over the last 30 years, Don and I have gone from being merely former coworkers to being good friends. We have meals together at conferences when possible, and we and our wives have dined together. I’ve been to his home in Ohio, where Don is a professor at the Air Force Institute of Technology. And we’ve both bought each other’s dynamics books.

Grady Wilson and I had a preference for diesel cars, and I let him know I was interested in acquiring an Oldsmobile diesel. Grady talked me out of it, though. His comment on maintaining Oldsmobile diesels was simply, “That thing will eat your lunch!” I’m so grateful that I didn’t buy one! I bought a Peugeot diesel instead. Gil Morehouse later bought a Peugeot diesel like mine (only newer). And then Peugeot stopped selling cars in the U.S. Sigh.

Ping-pong at the water tunnel was a regular lunchtime event. Larry Carr, Ken McAlister, Don Adams, and I were among the regulars, and there were several others who dropped in occasionally. When my second son Jon, at age 5, was asked what his daddy did at work, his answer was “play ping pong!”

Don Adams, who was our official computer geek, often amused us with his view of the world. On one occasion, while waxing philosophical on the subject of official business travel, he opined: “I’d rather travel than be at home,” he said. “At home my wife always has things for me to do....”

Bill Bousman one day noticed that the Xerox machine (that’s what photocopiers were called in those days) was on fire. He took decisive action, climbing on his hands and knees over the top and unplugging it. The fire was extinguished somehow (I don’t recall exactly), but the glass on top of the machine was broken in the process. Bill fashioned a sign that read, “In case of fire, break glass.” His dry humor could turn a near disaster into something very humorous.

Bill was an amazing experimentalist. Because of the Army-NASA Joint Agreement, our laboratory had access to the best of facilities. Bill and I collaborated a couple of times, and both of us learned the immense value of theoretical/computational types (like me) working together with experimentalists to design the experiment and make sure the configuration being tested is really going to put the computational model to the test.

The highlight of my 16 years at AARL and AFDD was a 6-month stint at the German Aerospace Center (now DLR), during the last half of 1984. It was my first sabbatical (and probably my last, because Georgia Tech doesn’t have a sabbatical leave program). Oh, it was wonderful—even if my German was fractured. I would say something to a co-worker in German, only to be answered in English. I learned how to ask for directions, but I could hardly ever understand the answers! My wife and I enjoyed our time in Germany, and she and our sons count it as one of their best family memories ever. Technically, through interaction with excellent German colleagues such as Dieter Petersen, I made some real progress in development of methodology to handle geometrically nonlinear composite blades.

In early September 1986 our family packed up and moved to Atlanta. I had finally realized a lifelong dream and secured an academic position; I accepted an offer from Georgia Tech to join them as a full professor. Dave Peters had moved from St. Louis to Georgia Tech a year earlier and had been instrumental in getting me to move there too. We lived in the same neighborhood for 7 years, a mere 0.2 miles apart, before he moved back to St. Louis to become the Head of the Mechanical Engineering Department at Washington University.



Dewey presenting the 2014 Nikolsky Lecture.

It turned out that I would broaden my areas of interest while continuing to work closely with AFDD and NASA people for over 30 years after leaving. I would be remiss not to mention the enormous gratitude I feel for the technical background I obtained at Ames, both from the Army and NASA. There is not a doubt in my mind that several of the

honors I've received over the years, (e.g., the 2014 AHS Alexander A. Nikolsky Honorary Lectureship), were outgrowths of the technical background I gained there and the mentoring I received from Bob Ormiston, especially.

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Index

A

Adams, Don, 6
Aeroflightdynamics Directorate (AFDD), 4-6
Air Mobility R&D Laboratory, 2
Alexander A. Nikolsky Honorary Lectureship, 6-7
American Helicopter Society (AHS), 3, 7
Ames Directorate, 2
Ames-Army Lab, 2
Army Aeronautical Research Laboratory (AARL),
1-2, 4, 6
Army-NASA Joint Agreement, 4, 6
Ashley, Holt, 1

B

bearingless main rotor (BMR), 3-4
Bible, 4-5
Bo-105 helicopter, 3-4
Boeing Vertol, 3-4
Bousman, Bill, 1, 6

C

Caradonna, Frank, 1
Carr, Larry, 1, 6

D

Dewey, Margaret, 2, 5
Dowell, Earl, 2

F

Flexbeam Air Resonance (FLAIR), 3-4

G

General Rotorcraft Aeromechanical Stability Program
(GRASP), 5
German Aerospace Center (DLR), 6

H

Herrmann, George, 3
Hodges-Dowell equations, 2-3
Hopkins, Stu, 5

J

Johnson, Wayne, 2-3

K

Kunz, Don, 3, 5

M

Martin, Mike, 5
McAlister, Ken, 1, 6
McCroskey, Jim, 1
Morehouse, Gil, 1, 5
Morse, Andy, 1
N
NASA, 1-2, 4, 6
Nikolsky, 6-7
Nonlinear beam element (NLB), 2

O

Oak Ridge National Laboratory, 1
Ormiston, Bob, 1, 7

P

Peters, Dave 1, 3, 6
Petersen, Dieter, 6
Prandtl, 3
Princeton University, 2

R

Reissner, Eric, 3
Reissner, Hans, 3
Reserve Officers' Training Corps (ROTC), 1
Rotorcraft Comprehensive Analysis System (RCAS), 2

S

Sharpe, Dave, 1
Stanford University, 1-2

T

Tung, Chee, 5

U

University of California at San Diego (UCSD), 3
University of Tennessee at Knoxville, 1

V

Vander-Roest, Gary, 4

W

Washington University, 1, 6
Wilson, Grady, 5

Y

Yaggy, Kathy, 5

Remembrances of a Secretary

Pat Horn
Army

I was a secretary when there still were such people, before they all became admin assistants.

About the mid-1980s, our children grown and gone, I decided to try getting back into the workforce after 20 years at home. Knowing I needed a brush-up, I took a short series of night-school Office Procedures classes, and then started reading want ads in the San Jose Mercury News. I was looking for possibilities near home and learned that NASA Ames Research Center was hiring secretaries. That possibility seemed made to order, since my husband, Harry, was already working for Ames as a journeyman glassblower or *experimental glass fabricator*, to use his full title. If I did go to work there, I could ride in with him.



Pat Horn

Many agencies shared the field with Moffett Naval Air Station near Mountain View, California. One entire side was dedicated to Ames Research Center. That side also hosted an office of the Judge Advocate General, as well as a branch of the Federal Aviation Administration, among others, with a working relationship web that included many government, industry, and university departments, most having to do with aerospace and flight. It also included the Aeromechanics Laboratory as well as its parent organization, the U.S. Army Research and Technology Laboratories. As I recall, Dr. Statler was Director for the first year or so after I started working there. The Aeromechanics Laboratory had several divisions. The Rotorcraft Dynamics Division, where I eventually went to work, dealt with the behavior of helicopters, specifically the rotors thereof, and the research by the scientists, engineers, and interns who used computer code and hands-on investigation to figure out rotor problems and improvements. My little part started like this:

After applying for the post I'd seen advertised, I passed the typing test at the Ames Personnel Department, and was interviewed by two principals, one in the Ames Gas Thermodynamics Branch, and the other in the Army's Rotorcraft Dynamics Division. Its Chief, Dr. Bob Ormiston, hired me and I went to work in November 1984. As Bob found out, I was pretty green and untried on my arrival. It was his patience that turned me into a capable secretary, along with the helpfulness of the other staff. What I chiefly remember (and miss) is their presence in my working life, as well as their commitment (and mine, once my understanding gleamed) to our jobs.

Year after year, the people I worked with gave to others and did not count the cost. This giving included the official, annual "Combined Federal Campaign," a weeks-long flurry of paperwork geared to amassing donations for a long list of charities. The employees across the entire field were known for their generosity, and it was not limited to this type of top-down effort.

One example of that generosity that I'll never forget happened toward the end of my years at the Rotorcraft Dynamics Division. When my mother became ill, I had to be absent often to care for

her. Many people, in and out of our division, donated hours and hours of their leave time to me, so that I would lose as little pay as possible. But it was not only the *time* they all gave so generously, that meant so much. The inconvenience suffered by others in Rotorcraft Dynamics, because of my absence, was also a never-alluded-to gift of the spirit.

Most people know what a secretary does. Being new, I filled my head and hands with as many procedures as I could, as fast as I could, to spare as much of Bob's time as possible. Looking back, it seems to me that his art as a manager had a lot to do with bringing people along with him, but at their own pace, to get the job done. Sometimes, I was able to look ahead and anticipate what was needed. Working as we did with people from the Army, from NASA, with contractors from the aerospace industry, as well as from various universities that had Memoranda of Understanding (MOUs) with our division, I saw the many small details of my job as timesavers, so that Bob could do the important work of moving *his* tasks ahead. Any mistake I made at work, I would mull over at home, hoping to get over stumbles as fast as I could.

One incident happened early on, and really stuck in my mind despite my efforts to dislodge it. At the time, we secretaries would key (word process) our reports or letters using DOS, the computer's "disk operating system." Limited as it was, if judged by today's standards, it did have the virtue of logic. One day, while cleaning up files, I gave the screen the following command: "Delete *.*" and the screen replied, "Are you sure? Y/N." I immediately hit the "Y" (oh, wages of hubris!)—*all* my files disappeared.

Panic! Doom! Smog! But for the skill of our information technology people—Gary VanderRoest and Diana Winkler, who worked downstairs—I would have resigned in shame. But they saved me, and all I had to reconstruct were the characters in the last row of 80-column text. Fortunately, I was a fairly fast typist and got it done in little moments between requests.

Our division occupied one end of the second floor of Building N-215. One day, I saw a little curl of smoke and sparks from an electrical outlet across from my desk. I immediately pulled the fire alarm and grabbed the fire extinguisher. Everyone exited, and once outside, our Director Andy Kerr (who had taken over after Dr. Statler left in 1985 and we had become the Army Aeroflightdynamics Directorate (AFDD)) asked me who had given me permission to clear the building. I replied, "There were sparks shooting out of the wall." I heard nothing further about it. As I said, generous leaders (and I'm guessing Bob went to bat for me on that one).

I mentioned that my husband was employed elsewhere on the field. Because he had a flextime schedule, and mine was a straight eight, most days I would ride in with him. His day always began at 6:30 a.m., and since mine didn't begin until 7:00 a.m., on the days I came in with him I would get to my desk a full half hour before my scheduled start time. That was just fine with me, as I could take a deep breath before plunging into the day's duties. After my initial 2 weeks or so on the job, when I was mostly scared of mucking things up irretrievably, I really began to look forward to being in the lab.

On 2 days in March, 1991, apparently experiencing some quiet time, I jotted down my tasks as I did them. What follows is an expansion of the notes I took on those days:

On March 11th, coming in, I made coffee for the lab, loaded the copier, and put away some stacks of paper reams. I connected my computer to Aviation Systems Command (AVSCOM) in St. Louis, prior to entering TAPS data. (I believe TAPS stood for The Army Personnel System.)

Then I sent some facsimile requests, one for Chee Tung to Italy, and one for Mike Rutkowski and Gene Ruzicka, to the American Helicopter Society (AHS) in Washington D.C. The phones were not too insistent that morning, but I did route a phone inquiry about lasers from a visiting student, then distributed the mail. A surprise that morning from a co-worker was a chocolate pastry, happily received and promptly disposed of. Then I authorized a visitor request from one of our contractors, who needed to see Mike. Wayne Empey, from the wind tunnel area, asked me to prepare a letter, some specifications, and some lists, which I did. The laser printer was going into the red zone, which meant “get tech help for it” and I told Gary it needed attention. I signed out the car, twice. A new proposal came in; I tracked the AROeval log on that, then I routed a purchase request back to Don regarding software, and went to lunch.

After lunch, I ran travel reimbursement requests over to the Ames offices for Bob, stopping at the credit union on my way back. The Director’s secretary had to be away, and asked me to cover her phones for part of that afternoon. About that time, the Directorate’s facsimile machine (which was in my office) started to scream “feed me!”—so I tested the new kind of fax paper we were going to order. It seemed fine. I planned Easter brunch. (That’s in my notes so I must have done it; it probably had nothing to do with work!) Moving along, I prepared a form for one of our contractors, Amir Izadpanah, and finished the day reading the current all-field secretary’s newsletter. I noted we now had a new Specials service and that there was a new secretary at the Ames Director’s office. Also—if I remember—one of these issues had my little grammar column called “Wordcrunchers!”

March 12th began with a sore throat that was not too awful, and I came into the office tired from a night of coughing. I processed mail, checked and distributed the faxes that had come in, made coffee, and had a conversation with Ken about a messy stockroom user who turned out to be an outside person. I typed a service request for Dave Sharpe. At 7:05 a.m. I checked with Georgia Crenshaw, at our administrative office, about Wendell Stephens regarding a clearance he needed. At that time Wendell, one of our division’s researchers, was on sabbatical to the nation’s capital, serving in one of the Senate offices. At 7:35 a.m. there was a phone request from an Army auditor who needed to meet with Dr. Ormiston; I followed up with Bob to set a time. I then read and recorded the mileage on the car assigned to our office; it was checked out once. I talked with Tom Maier about gardening for a bit, and answered a question for Bill Bousman about some service requests. I re-checked the car log. At 8:40 a.m. I worried about Harry, my husband, for 10 minutes. He had caught my cold. The Director’s secretary, Lynda Jones, told me it would be Friday before we got the fax paper we had ordered. I found a purchase request Bob needed to look at, #1055, then copied vugraphs (overhead transparencies) for the auditor and delivered them to Bob. The auditor came in; I announced him and showed him into Bob’s office. Bob asked me to find and bring in the purchase request log from another division, which I did. Then I sent a fax for Ray Piziali, to France. I followed up with Georgia about Wendell’s clearance, which was still in the works. At 10:00 a.m. the ear and throat were still iffy, but I was feeling peppier and okay otherwise. I copied and routed our Director Andy’s request about business cards, and tracked the back orders in the stock folder. Georgia called back; according to her colleague Cherie Tolson, they were waiting on AVSCOM St. Louis regarding Wendell’s clearance. Gail (another admin person in our office) was to return on Monday. I called Wendell in Washington to let him know. He was momentarily out of the office and I was put on hold. However, there were compensations: Senator Grassley’s office phone played Albéniz while I waited! After that call, I continued answering phones, taking what steps were needed. Then I

dealt with a question about installing a mirror in the angle of the hallway. After lunch, I did some copying for Bob, and Don helped me carry boxes of Xerox paper into storage in the women's room. I continued to answer the Director's phone, and was feeling better; would probably live. The last task of the day was to make a five-column table and transfer it to Bob's computer using TOPS (I can't remember this acronym; obviously file-transfer software).

Wonders, too many to list in detail:

Once, Bob asked me to catalogue his library of about 500 technical publications, literature mostly within his field. He was always very good at finding projects that would stretch my abilities, and this was a good one! I worked it into my days as time allowed, and learned so much by it: the standard way to list contributors, for example, and the uses of the Latin *et al.*! I ordered his library by principal author, cross-matched it by title, and (if memory serves) by publication date; this last one is vague. I do know I made three lists, but I'm sure that by now they are well out of date.

From time to time, our office would host an intern from overseas for a few months, and I remember most vividly a young man from France, who worked out of Bill Bousman and Tom Maier's office. His name escapes me, but because I knew a bit of French, when I received a call for him, I wrote the yellow message slip for him in his language. He was surprised, of course, and came to my desk to see about it. He thanked me, and pointed out very gently (when I asked) that I had made one very small grammatical goof. One of the funniest things he once told me was, "Americans don't drink coffee! They drink coffee juice!"

Another memorable and happy instance involving visiting researchers happened at one of our lab picnics. One young scientist there was from Gabon, and he brought his wife and 6-month-old baby to the picnic. She knew very little English, but spoke French. Sitting with her, somewhat apart from the others, I asked if I could hold her baby. When he got sleepy, I rocked him naturally in my arms, whereupon his mother murmured to me, "Il cherche votre sein!" Of course, the baby sought nourishment! I sat him up and she and I shared a smile of understanding. It was one of those instant, universal, and transcendent moments that only mothers would recognize.

Every year, there was an AHS meeting in Washington, D.C., which several of our Ph.D.s and engineers attended and took part in. Getting ready for that meeting involved many deadlines for the participants, and of course for the support folks like me, who tried to make sure they would have any materials, transport, flights, and lodging that they needed. It was one of the busier times in the lab.

Not all was work in the Rotorcraft Dynamics Division (or in the Directorate). We were able to plan holiday parties for the lab, celebrate birthdays at break times, and generally get along so well that the work we did, most of the time, seemed more like play. The Director and the Division Chiefs always took their secretaries to lunch somewhere really special on Secretary's Day. One Christmas, all the secretaries and admins dressed in red and sang our version of Eartha Kitt's *Santa Baby* for everyone—for more laughter and fun.

Coming back to my theme—the generosity of the people I worked with: About midway through my tenure there, I was allowed to spend a week at the Asilomar Conference Center near Monterey, studying a pretty intensive six-module course (including math, my nemesis!). This class was for office support staff, promoted by the Institute for Certifying Secretaries, with the

laudable aim (I believe) of getting recognition for a career not always respected in the business world. The class ended with receipt of a certification as a Professional Secretary. Pretty heady stuff, and I've got the pin to prove it! That was a good week too, but not as good as being in the office, greeting people from all over the world, and knowing that I was doing work that mattered.

Instead of spending those almost 10 years of my middle age at home with my homely pursuits, in working for Bob I was able to associate daily with intelligent, educated, motivated, and kind people whose various interests and conversations enriched me for the rest of my life. Where else?

Jacob Bronowski once said that because of the nature of their work, scientists come naturally by goodness. They search for truth, so scientists are good in minute particulars. Such are the people I worked with, November 1984 to January 1994.

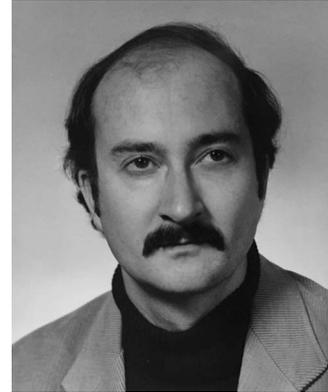
Index

- A**
 Aeroflightdynamics Directorate (AFDD), 2
 Aeromechanics Laboratory, 1
 Albéniz, 3
 American Helicopter Society (AHS), 3-4
 Ames Research Center, 1, 3
 Asilomar Conference Center, 4
 Aviation Systems Command (AVSCOM), 2-3
- B**
 Bousman, Bill, 3-4
 Bronowski, Jacob, 5
- C**
 Crenshaw, Georgia, 3
- E**
 Empey, Wayne, 3
- I**
 Izadpanah, Amir, 3
- J**
 Jones, Lynda, 3
- K**
 Kerr, Andy, 2-3
- M**
 Maier, Tom, 3-4
- Memoranda of Understanding (MOUs), 2
 Moffett Naval Air Station, 1
- O**
 Ormiston, Bob, 1, 3
- P**
 Piziali, Ray, 3
- R**
 Rotorcraft Dynamics Division, 1-2, 4
 Rutkowski, Mike, 3
 Ruzicka, Gene, 3
- S**
 Sharpe, Dave, 3
 Statler, Irv, 1-2
 Stephens, Wendell, 3
- T**
 Tolson, Cherie, 3
 Tung, Chee, 3
- V**
 Vander-Roest, Gary, 2
- W**
 Winkler, Diana, 2

Recollections of Army and NASA Rotorcraft Research, 1970–1986

Wayne Johnson
Army/NASA

In the spring of 1970 I was finishing up my Doctor of Science (Sc.D.) research at Massachusetts Institute of Technology, working with Professors Norman Ham, Sheila Widnall, and Rene Miller. On the advice of Norm Ham, I applied for a job with the U.S. Army Aeronautical Research Laboratory (AARL) at Ames Research Center. I went with Professor Ham to a Project Themis conference at the Illinois Institute of Technology. Paul Yaggy, Director of the Army laboratory, was at the conference, and he interviewed me for the job while we were standing in the hallway outside the meeting room. He said that working with Professor Ham, I must have an appreciation for rotor dynamics, and I agreed. Soon thereafter I was offered the job. I knew little about the Army laboratory, but California seemed more interesting than Virginia, and the Army was trying harder than NASA Langley to get me to work for them.



Wayne Johnson, 1984.

I graduated from MIT in June 1970, and I spent the summer working on a couple of final reports. In September I started my way west, previously having only gotten as far as Texas for a summer with Bell Helicopter in 1967. Getting a Corvair over the Rocky Mountains proved challenging, but it rolled into the motel in Mountain View before it decided it wasn't going to start again. While the car was being fixed, I found an apartment in Palo Alto, a city I liked because it had trees.

In late September 1970 I arrived at Ames Research Center to start work. I went to the Director's office in Building N-215 and found Paul Yaggy on the telephone, which I learned was a common state. His assistant told me I was assigned to the NASA Large-Scale Aerodynamics Branch that operated the 40- by 80-Foot Wind Tunnel, and he sent me over there. I did not see much of the Director or Building N-215 in the next few years. The Army lab was expanding that year and increasing its presence in NASA branches. The arrangement was that the Army would provide personnel for NASA, in exchange for offices, facilities, and infrastructure. I was one of several Army engineers assigned to the 40- by 80-Foot Wind Tunnel. It wasn't required that we work on rotorcraft, but of course that would be best for the Army. I spent the next 16 years in the NASA 40x80 Branch.

At the NASA 40- by 80-Foot Wind Tunnel, rotor research had been conducted by a number of NASA engineers, notably John McCloud, starting in the late 1950s, and James Biggers, starting in the early 1960s. When I arrived, the rotorcraft group consisted of McCloud, Biggers, and Army engineers Robert Stroub and Jack Rabbott. The Branch Chief was Mark Kelly. The Army's connection with the NASA 40x80 Branch was strong; indeed, Paul Yaggy was a former member of the branch. I was given a desk in the corner of an office with Biggers and McCloud, underneath the tunnel expansion section. Mark Kelly told me to think about what I'd like to work on. I don't believe he was quite sure what to do with an Sc.D. from the Army in his wind tunnel. Six weeks later I said, "Maybe I should develop a tiltrotor analysis, since we're going to be

doing all that testing.” The research leading to the development of the XV-15 was well underway. I missed the tests of the Bell proprotor in 1969, but Boeing proprotor tests would be taking place soon, and it was expected that the research aircraft would be tested in the 40- by 80-Foot Wind Tunnel.

I continued work begun at MIT, including blade-vortex interaction analysis [1], dynamic stall (a paper with Norm Ham [2]), rotor airloads calculations (a paper with Michael Scully in 1972 [3]), and perturbation theory for rotor flap stability [4, 5]. Perturbation theory application to aerodynamics was a major research topic at MIT, developed and taught by Ashley and Landahl [6] and Widnall. Applying the techniques to the rotor flap stability problem was interesting, if not particularly practical, since other bad things happen first at high advance ratio.

The tiltrotor analysis project started with development of an elastic blade model, based on Miller’s Newtonian approach for deriving the equations [7]. That work was slowed by my participation in a number of tests in the 40- by 80-Foot Wind Tunnel, so I redirected it to a simple rotor and cantilever wing model, reported at the 1974 American Helicopter Society (AHS)/NASA Ames Specialists’ Meeting on Rotorcraft Dynamics [8] and in a NASA Technical Note [9]. The Army-NASA Joint Agreement allowed Army engineers to author NASA reports, eventually with dual Army and NASA report numbers. The 1974 meeting was very successful, with many papers now seen as important milestones. When we were planning the subsequent 1984 AHS conference, my contributions were to name it the 2nd Decennial Specialists’ Meeting on Rotorcraft Dynamics, and to persuade Bill Bousman to be Technical Chair. The elastic blade analysis development was completed from 1974–1976 and described in NASA reports [10-12].

In 1978, the XV-15 Tilt Rotor Research Aircraft arrived at the 40- by 80-Foot Wind Tunnel. The methodology to predict tiltrotor whirl flutter was developed during this period. In the 1972 pre-design work, Bell Helicopter’s prediction of the stability boundary was 540 knots at sea level (SL) and 660 knots at 12,500 feet, using an analysis that only included the rotor gimbal motion. My paper [8] and Ray Kvaternik’s [13] presented at the 1974 meeting showed the importance of including the blade lag mode, using rigid blade models. Kvaternik’s code [14] was similar to what Bell was using, but it had a better airframe structural dynamic model, so Bell eventually adopted his code. An elastic blade model was needed to correctly identify the significant influence of effective pitch-lag coupling on tiltrotor whirl flutter. Such coupling had been found to have significant influence on hingeless rotor stability [15], and the same physics applied to the gimballed, stiff-inplane rotor design of the XV-15. My analysis [10] predicted stability boundaries of 295/335 knots (SL/12.5k). Since the design requirement was 120 percent of dive speed, or 360/432 knots (SL/12.5k), the XV-15 project office did not like these numbers, but everyone was satisfied by removing any mention of “Bell” or “Boeing” from the report. Bell was using a rigid blade analysis, so they had to estimate the effective pitch-lag coupling from a separate code. With refinement of that process, Bell’s stability predictions were 400/455 knots. Working with Bell, we accounted for the actual geometry of the pitch link (which at high collective reduced the pitch-flap coupling) and updated the control system stiffness. Then the stability boundary prediction by the government was 320/420 knots, and Bell’s was 365/410 knots. Up to that point, Bell and the XV-15 project office were prepared to ignore my calculations, but now their predicted boundary at altitude was below the requirement. That motivated Bell engineers to think again about the physics of whirl flutter, and they recognized that the airfoil lift curve slope starts to decrease at high Mach numbers. Accounting for this lift divergence brought Bell’s predicted stability boundary up to 365/445 knots. My predictions were

still somewhat lower (320/420 knots), but the flight tests never took the aircraft close to any of these predicted boundaries.

Tiltrotor whirl flutter analysis required a representation of the entire aircraft, and the structural dynamics and aerodynamics of blades at large pitch angles. Therefore, it was straightforward to generalize the tiltrotor stability analysis to other rotorcraft configurations, in flight and in a wind tunnel (work documented in 1977 [16], with applications following in a number of reports [17, 18]). In 1977 I also worked on optimal control of tiltrotor gust response [19], and power-off landing of helicopters [20]. Power-off landing was a nice problem for optimal control. I could not get this work published in a journal, but it was the basis for several subsequent investigations [21, 22].

In 1972, Professor Norm Ham of MIT presented a proposal to the Army for an experimental and theoretical investigation of tiltrotor gust response. As was proper for a former student, I stole the proposal from Bob Ormiston, to be funded by NASA. The gust generator concept used the shed wake from a pair of wings with oscillating lift to produce vertical or longitudinal velocity oscillations in the tunnel. There were concerns that tunnel wall interference would reduce the effectiveness, but I developed a nice theory showing the magnitude of the wall effect. We obtained a tiltrotor model with hingeless blades from Boeing (Model 222), and had the model builders at Princeton University add a gimbaled rotor. The gust generator was successful. From 1973 to 1978, the project developed control systems for gust alleviation, tested in the wind tunnel, and compared with predicted performance [23, 24]. I first met Tom Lawrence, who spent his career at Sikorsky, when he was on the floor of the Wright Brothers' wind tunnel, rewiring the model. The theory was developed by Masahiro Yasue, who recently retired as Director General, Technical Research and Development Institute, Ministry of Defense in Japan. From 1979 to 1988, NASA funded Professor Ham's experimental and theoretical work on Individual Blade Control (IBC), which for Ham meant rotating frame sensors, feedback, and actuation on each blade to control the response of each blade individually. The applications of IBC included gust alleviation, stall flutter suppression, lag damping augmentation, and vibration alleviation [25, 26]. Contributing to the work were Robert McKillip (Princeton University and Continuum Dynamics, Inc. (CDI)), Todd Quackenbush (CDI), and Norman Wereley (University of Maryland, now Chair of the Department of Aerospace Engineering). I consider these two programs of Professor Ham, along with the Army-funded work of Professor Kurt Hohenemser, to be the finest rotorcraft research arcs.

Arriving at Ames in late 1970, I also just missed the excitement of the AH-56A Cheyenne test in the 40- by 80-Foot Wind Tunnel. I did get to participate right away in the evaluation of proposals for an advanced rotor test capability. The intent was to be able to quickly build and test any idea for advanced rotor geometry (planform, twist, airfoil). We tried twice to obtain this capability. The first try, with Sikorsky Aircraft, was not successful; it was too hard to build thick composite structures at this time. We did get the Rotor Test Apparatus (RTA) out of the contract. For the second try, with Hughes Helicopters in the late 1970s, it proved too expensive to build composite blades. The contract was for five rotor sets to be tested on the RTA, but because of cost overruns only one rotor set was delivered. We learned about the cost growth during a briefing at Ames, and when we asked why they had not let us know earlier, the response was that they could not find a place on the form for that information. They expected more money would be coming, but we told them to de-scope the work. When finally tested in the 40- by 80-Foot Wind Tunnel,

one blade decided to fly in its own plane. We never did figure out why, even though Fort Felker spent a year measuring the properties of the blades.

In 1972, I helped with the test of the Boeing Model 222 proprotor in the 40- by 80-Foot Wind Tunnel. The Model 222 used a hingeless hub, based on the technology of the Bo-105 rotor, and consequently needed hub moment feedback for operation in cruise flight. For the wind tunnel test, hub moments could not be sensed directly, so instead chord and torsion moments measured on the outboard part of the wing were fed back to cyclic, regrettably with a low-pass filter. Boeing also followed Bell in testing whirl flutter stability augmentation, using feedback of vertical acceleration of the nacelle to cyclic. The Model 222 whirl flutter stability test was a practical lesson in the meaning of gain margin, the hub moment feedback testing setting a record for the most instabilities in the 40- by 80-Foot Wind Tunnel without breaking the rotor. The low-pass filter was second-order, so it only took a small phase shift from the wing-rotor dynamics to produce an unstable system. Boeing had encountered the phenomenon in small-scale tests (using the model that we later sent to MIT). When they mentioned it in pretest planning, we asked whether it was an instability, but we were assured it was merely a high-gain oscillation. The Boeing tiltrotor concept was fatigue-challenged due to the design of the blade attachment to the pitch case, so it was necessary to keep the blade 1/rev loads low. They could not claim success of the hub moment feedback testing, but solved the problem with wing flap deflection scheduled to control rotor shaft angle in airplane mode.

When the RTA was delivered in 1974, several 40- by 80-Foot Wind Tunnel entries were devoted to shake tests and ground resonance investigations that also required my programming the new Dynamics Analysis System, which was built around a DEC PDP 11-45. Sikorsky had recently encountered ground resonance in their tunnel (in testing of a variable diameter rotor, breaking two rotors before deciding there was a problem), which was stabilized by adding lots of struts from the model to the wind tunnel walls. Rabbott listened to Sikorsky's advice, so we added some fore-aft struts with automobile shock absorbers to the RTA, but all that did was introduce new (low-damped) modes. Eventually Sikorsky found an error in their ground resonance calculations, but we all learned a lot about shake testing [27] and ground resonance of rotors in the 40- by 80-Foot Wind Tunnel.

I was involved with tests of the Controllable Twist Rotor, McCloud, 1975 [28]; Multicyclic Twist Rotor, McCloud, 1976 [29]; S-76 Rotor, Stroub, 1977 [30] (I wrote the data report when Bob went to another branch [31]); XV-15 Tilt Rotor Research Aircraft, 1978; Circulation Control Rotor, 1978, analyzed by Chopra [32]; X-Wing Rotor, McCloud, 1979 (Inderjit Chopra handled the dynamics); Research Rotor, Johnson, 1979 (this was the rotor that flew with two tip-path planes); and the Bearingless Main Rotor (BMR), McCloud and Warmbrodt, 1980 [33]. Beginning around 1978, I worked with Fred Silva to develop the Rotor Data Reduction System software for the 40- by 80-Foot Wind Tunnel [34]. While the wind tunnel was shut down for the construction project, we conducted hover tests in the 40x80 test section, including the BMR (Warmbrodt, 1983) and the Bo-105 rotor (Peterson, 1983 [35]). I was on top of the control room, using a strobe to track the BMR, when Bill Warmbrodt broke the rotor, thereby establishing his credentials for moving into higher management positions. The direction of the cyclic control system was wired incorrectly, which could not be checked until the rotor was turning since it relied on the N/rev pulse from the rotor for harmonic analysis of the flap motion. We had borrowed the Boeing rotor from the Army, but no one knew (or at least no one told us) that reducing RPM through the lag mode 1/rev resonance with non-zero cyclic control would cause

motion large enough to damage the flexbeams. This was the only rotor to break in my presence in the 40- by 80-Foot Wind Tunnel, and no hardware actually left the rotor. I even missed the collapse of the wind tunnel vane set in 1982, having picked that day to take the afternoon off.

When I joined the government, I had taken the helicopter courses at MIT and done my doctoral thesis on blade-vortex interaction, but I wanted to learn more about helicopters. So, from 1971 to 1974, I spent many evenings reading papers and making notes on helicopter theory. I had in mind writing a book, but the major publishing houses did not see any market. Eventually I found interest at Princeton University Press. I decided to write the manuscript, so Princeton could make a decision based on a full draft, not just a prospectus. It was soon clear that I needed devote full time to this, so I took leave-without-pay (easy to do back then). From November 1976 to January 1977 I spent my days writing the book at the Stanford Library, and I spent February typing the draft. Princeton University Press was a small house, so editing and typesetting took a long time, but the book *Helicopter Theory* was published in 1980 [36]. The book received the American Institute of Aeronautics and Astronautics (AIAA) Pendray Aerospace Literature Award (1986), and it was a major reason I received the AHS Grover E. Bell Award (1982).

In 1977, I became head of the Rotorcraft Research Section in the 40x80 Branch. Army engineers could take leadership and even supervisory positions in the NASA branches. My direct supervisor was the NASA Branch Chief. I was a member of the Army Rotorcraft Dynamics Division, but I didn't get to Building N-215 much to see the Division Chief, Robert Ormiston. I remember Bob called me into his office (sometime in 1979, I think) intending to invite me to join the 2nd Generation Comprehensive Helicopter Analysis System (2GCHAS) development group. But he didn't get very far, for he started the conversation with, "Since the 40x80 isn't running now..." That was true (the 40- by 80-Foot Wind Tunnel's maximum speed was being increased by replacing the fans, and the 80- by 120-Foot Wind Tunnel was being constructed), but at the time I had nine people in the Rotorcraft Research Section, with a budget of about \$2M/year. After I left the government, I did work as a consultant to the 2GCHAS development project.

After finishing the draft of my book, I was looking for the next big thing to do. I started to think about writing a comprehensive analysis, building on my rotorcraft dynamics model and adding a wake model (which I had worked on for my MIT thesis) and nonlinear blade motion solution. A consideration was that the Army was embarking on a major development of the comprehensive analysis 2GCHAS. But I was Section Head, so I didn't really have to ask permission from anyone, and I thought it would be interesting. From 1978–1979 I developed the rotorcraft comprehensive analysis CAMRAD, published in 1980 as reports with NASA and Aviation Research and Development Command (AVRADCOM) numbers [37-39]. I am not good at inventing computer program names, but I had observed what happened with other codes, so I made sure the report title (*A Comprehensive Analytical Model of Rotorcraft Aerodynamics and Dynamics*) spelled something I could live with. Live with it I have, as my wife even got me a "CAMRAD2" license plate.

The transfer of most Langley rotorcraft research (including joint NASA/Army projects such as the RSRA) to Ames in 1977 resulted in a significant expansion of rotorcraft work at Ames. I have observed, too frequently, that every so often the government throws away a good and productive organization. There are several later examples just in rotorcraft research organizations. By 1980, the 40x80 Branch staff had grown large enough to split into Research and Operations groups, and then into separate branches. In 1981, I transferred to NASA to take

the job of Assistant Branch Chief in the Low Speed Aircraft Research Branch, with responsibility for the rotorcraft research. The rotorcraft research group included as many as 18 engineers and a budget up to \$5M/year. Leaving the Army, I received my first award: the Commander's Award for Civilian Service, Army Aeromechanics Laboratory, in May 1981, followed by the NASA Medal for Exceptional Engineering Achievement in November 1981.

A lasting contribution from my time as Section Head and Assistant Branch Chief was the people I brought to Ames. I am fortunate to still work with William Warmbrodt (1978, now Chief of the rotorcraft Aeromechanics Office at Ames), Tom Norman (1982, leader of the UH-60A Airloads Wind Tunnel test, and many other major rotorcraft tests in the 40- by 80-Foot Wind Tunnel), and Gloria Yamauchi (1982, currently Associate Project Manager for Strategy of the Revolutionary Vertical Lift Technology (RVLT) project). Through post-doctorate programs we developed lasting contacts with Keiji Kawachi (1978, now Emeritus Professor at University of Tokyo), Shigeru Saito (1980, retired leader of rotorcraft research at JAXA in Japan), and Inderjit Chopra (1978, now Distinguished University Professor at University of Maryland). At MIT I had shared an office with Michael Scully for 2 years. In 1975, Mike finished his work at MIT on helicopter design and rotor free wake geometry, and he joined the design group at the Army Air Mobility R&D Laboratory (AMRDL) Headquarters at Ames. When Dick Carlson asked about him, I told him that Mike had an encyclopedic knowledge of all things aeronautical, including airplanes, rotorcraft, and propulsion, and that was my part in recruiting Mike.

I walked away from the NASA management position in 1983. Two years was enough to convince me that the view was not better one level up, and the frustrations were not less, just different. I realized that I really liked research much better. That was a fortunate decision for many people, since Bill Warmbrodt replaced me, and became Branch Chief a few years later, to the great benefit of all who work for him, including hordes of interns. I was almost pulled back into management in 2005, when I was ready to take a lead role in the renewed NASA Rotorcraft program, but we all voted for Susan Gorton to lead the Subsonic Rotary Wing Project, and a fine decision that was.

I continued research, in 1982 using CAMRAD to apply dynamic inflow theory to the ground resonance test data obtained in Army research by Bill Bousman [40], and summarizing self-tuning regulator (higher harmonic control) algorithms [41]. In 1986, I developed Computational Fluid Dynamics/Computational Structural Dynamics (CFD/CSD) loose coupling with Army engineers Chee Tung and Frank Caradonna [42]. I wrote a survey paper on rotorcraft dynamics for an AGARD lecture series in 1985 [43]. Expecting to be asked to lecture on aerodynamics, I had written a survey paper that was eventually published by the AIAA [44].

In May 1986, I left the government to work for myself, establishing Johnson Aeronautics. I wanted to develop the definitive rotorcraft comprehensive analysis, written and supported as commercial software. And I wanted to write a really good book on helicopters. CAMRAD II was first released in 1993, but it wasn't until 2013 that *Rotorcraft Aeromechanics* was published by Cambridge University Press [45]. Since it turns out I can publish a book every 33 years, I do not need to start work on the sequel for a while.

I have been fortunate to receive a number of awards, including Fellow of AIAA and AHS, Ames Fellow, AHS Alexander Klemin Award, and the AHS Alexander A. Nikolsky Honorary Lectureship in 2010 [46]. I very much appreciate these awards because I realize how much work my friends had to do to nominate me. In 1998, I came back to government service, joining the Army/NASA Rotorcraft Division at Ames Research Center—an organization that was the direct result of the long collaboration between Army and NASA, and in my judgment the strongest rotorcraft research group ever.

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Index

A

AH-56A Cheyenne, 3
 Alexander A. Nikolsky Honorary Lectureship, 7
 Alexander Klemin Award, 7
 American Helicopter Society (AHS), 2, 5, 7
 American Institute of Aeronautics and Astronautics (AIAA), 5-7
 Ames Research Center, 1, 3, 5-7
 Army Aeronautical Research Laboratory (AARL), 1
 Army Air Mobility R&D Laboratory (AMRDL), 6
 Ashley, Holt, 2
 Aviation Research and Development Command (AVRADCOM), 5

B

Bearingless Main Rotor (BMR), 4
 Bell Helicopter, 1-2, 4
 Biggers, James, 1
 Bousman, Bill, 2, 6
 Building N-215, 1, 5

C

Caradonna, Frank, 6
 Carlson, Dick, 6
 Chopra, Inderjit, 4, 6
 Comprehensive Analytical Model of Rotorcraft Aerodynamics and Dynamics (CAMRAD), 5-6
 Computational Fluid Dynamics/Computational Structural Dynamics (CFD/CSD), 6
 Continuum Dynamics, Inc. (CDI), 3

G

Gorton, Susan, 6
 Grover E. Bell Award, 5

H

Ham, Norman, 1-3
 Hohenemser, Kurt, 3
 Hughes Helicopters, 3

I

Illinois Institute of Technology, 1
 Individual Blade Control (IBC), 3

K

Kawachi, Keiji, 6
 Kelly, Mark, 1
 Kvaternik, Ray, 2

L

Landahl, Marten, 2
 Lawrence, Tom, 3

M

Massachusetts Institute of Technology (MIT), 1-6

McCloud, John, 1, 4
 McKillip, Robert, 3
 Miller, Rene, 1-2
 Ministry of Defense, 3

N

NASA 40x80 Branch, 1, 5-6
 NASA Langley, 1, 5
 Norman, Tom, 6

O

Ormiston, Robert, 3, 5

P

Pendray Aerospace Literature Award, 5
 Princeton University, 3
 Princeton University Press, 5

Q

Quackenbush, Todd, 3

R

Rabbott, Jack, 1, 4
 Revolutionary Vertical Lift Technology (RVLT), 6
 Rotor Data Reduction System software, 4
 Rotor Test Apparatus (RTA), 3-4

S

Saito, Shigeru, 6
 Scully, Michael, 2, 6
 Second Generation Comprehensive Helicopter Analysis System (2GCHAS), 5
 Sikorsky Aircraft, 3-4
 Silva, Fred, 4
 Stroub, Robert, 1, 4

T

Tung, Chee, 6

U

University of Maryland, 3, 6
 University of Tokyo, 6

W

Warmbrodt, William, 4, 6
 Wereley, Norman, 3
 Widnall, Shiela, 1-2
 Wright brothers, 3

X

XV-15 Tiltrotor, 2, 4

Y

Yaggy, Paul, 1
 Yamauchi, Gloria, 6
 Yasue, Masahiro, 3

A Unique Partnership and a World-Class Research Activity

Andy Kerr
Army

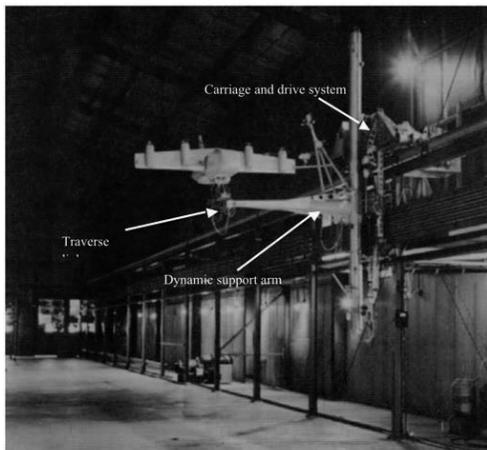
Formative Years—Recognizing the Challenge of Rotary-Wing Technology

When I arrived at Princeton University as an undergraduate in 1958, I had planned to major in Electrical Engineering (EE). However, during a presentation by Professor David Hazen at Freshman Engineering Orientation, I decided that a dual major in Electrical and Aeronautical Engineering would cover my interests much better. After a year of this endeavor, it became evident that concentration in Aeronautical, with a minor in Electrical, made more sense to me. The emergence of computers as useful engineering analysis and design tools drove this decision, since computer science was not yet identified as a field of study but was covered in the EE curriculum. My scholarship was dependent on working to support myself, and Professor Hazen hooked me up with a part-time and summer job at the Forrestal Research Center as a tech aid. I was assigned to the Princeton Forward Flight Facility—the “Long Track” in the Flight Mechanics Lab. The facility was built and operated under contract to the U.S. Army Transportation Command’s Aviation Labs (AVLABS) at Ft. Eustis to investigate the low-speed dynamic stability and control of vertical takeoff and landing (VTOL) aircraft. I was hired by Professor Alexander Nikolsky and assigned to work directly for a graduate student, Howard C. (Pat) Curtiss. Thus, I had my first exposure to the world of rotary-wing aircraft, working on projects for the U.S. Army. Here I encountered the fascinating complexity of rotorcraft, the design and operation of a major aeronautics test facility, powered rotary-wing model design and fabrication, and hybrid computer analysis of dynamic systems. I learned to design model components and fabricate them in the machine and model shops. Along the way, I was assigned to support a graduate student from Texas, H. Kipling “Kip” Edenborough, on his Master’s



Andrew W. Kerr

project. After Kip and I spent considerable time in industry, we found ourselves working together again for the Army and NASA at Ames Research Center.



XC-142 model in the Princeton Forward Flight Facility.

After receiving a B.S. degree in Aeronautical Engineering in 1962, I remained at the Flight Mechanics Lab as a full-time Research Assistant for a year, working under Professor Nikolsky and Pat Curtiss. I joined the American Helicopter Society (AHS), reflecting an intense emerging interest in the complexity and technical challenges associated with VTOL flight. I assisted in the design, fabrication, and testing of a fully functional dynamic model of the XC-142 tiltwing aircraft, also under contract to the Army. This association with the Army was destined to define the course of my entire career.

The Lockheed Years

My wife, Francie, and I were married in June of 1963 after she received her B.A. degree in Mathematics from Wellesley College. I had been in discussions with Sikorsky, United Technology Research Labs, and Boeing Vertol since before graduation. Lockheed was moving into the helicopter business and recruiting people to work in that area. Of the various professional and educational alternatives available, I accepted a Work/Study Fellowship from Lockheed-California Company in Burbank, California, studying Aerospace Engineering at the University of Southern California (USC). They offered us a package deal, hiring Francie as a Scientific Computer Programmer (she had gained some experience in the field working summers at the Gas Dynamics Lab at Princeton). We developed Lockheed's first helicopter rotor blade-element performance codes together using emerging computer hardware capabilities. When we arrived at Lockheed, the helicopter engineering activities were combined with the fixed-wing activities. We had only been there for a week when there was a major layoff, resulting in the release of almost all of the recent hires. I believe that we may have dodged that bullet simply because the layoff list was probably made before we reported for work. Two lessons here: in large organizations, the right hand often isn't aware of what the left hand is doing, and it isn't over 'til it's over! These lessons apply equally in industry and government, and helped me make it through a career in a field that is continually reinventing itself.

When we arrived at Lockheed, the helicopter program was still in its infancy. The development of the Lockheed gyro-controlled rigid rotor concept had proceeded rapidly from an invention devised by Irv Culver. After successfully demonstrating a small-scale radio-controlled concept model, a small team of engineers and mechanics quietly created and tested the CL-475, a three-bladed-rotor helicopter that demonstrated the unique characteristics of the Lockheed rigid rotor concept. Construction of the aircraft started in July 1959, with first flight in early November of that year. The flight characteristics of the new concept were exceptional, showing its potential, but it was plagued by very high vibration. The rotor design was improved, and the cockpit was isolated on springs from the dynamic system. Eventually the aircraft was successfully demonstrated for the Federal Aviation Administration (FAA), NASA, and the military. Lockheed proposed a more refined variant for the Army's Light Observation Helicopter (LOH) competition. It was not selected. However, the Army and Navy contracted with Lockheed to build the CL-595, designated as the XH-51A, as demonstrator for the new rigid rotor concept. This vehicle made its first flight in November 1962, at which time I was looking for my next challenge after I completed my Research Assistant assignment at Princeton.

Prior to our arrival, Lockheed had recruited heavily to obtain personnel with rotorcraft design and development experience. The search for experience led them to hire several senior rotorcraft engineers from established helicopter companies. The skill-mix and interaction between disciplines was different from the fixed-wing engineering activities. This turned out to be a boon to me as I got a chance to see how the art was practiced at the other companies in the field. When I first arrived, there was little active rotorcraft engineering going on in the department. Kip Cheney, who sat at the desk next to mine, had come from Sikorsky. Norm Gorenberg had come from Boeing Vertol after having worked on the XV-1 at McDonnell Aircraft under Dr. Kurt Hohenemser. Our aerodynamics group lead for helicopters was Ray Prouty, who had worked at Sikorsky, Bell, and Hughes. Ray was a natural teacher who could take very complex concepts and make them readily understandable. These were my industry teachers as I discovered how the helicopter engineering business worked. It was interesting that each of these companies

approached the engineering and development process for rotorcraft in somewhat different ways, and I quickly got a feel for what was common practice and what might be done differently. As the development of the XH-51A proceeded and Lockheed positioned itself to compete for the Army's Advanced Aerial Fire Support System (AAFSS) program, the management put together a dedicated technology team for helicopters. Compared to the fixed-wing technology organization, it was extremely lean. As a result, there was ample opportunity to participate in a wide variety of engineering tasks.

Over the first couple of years while I worked on my M.S. degree in Aerospace Engineering at USC, I developed aerodynamic performance computer programs for helicopters and worked on wind tunnel testing and analysis of advanced configurations. I was a member of the team preparing a proposal for the AAFSS design study, and I participated in supporting the flight development and demonstration of the XH-51A. In May of 1965, I completed my last final exam at USC at 9:30 p.m. and went directly to the Lockheed Low-Speed Wind Tunnel to work the graveyard shift on testing configuration options for the AAFSS proposal. I was considering continuing studies for a Ph.D., but I became immediately involved in such a wide variety of engineering projects, working regular 50- to 60-hour weeks, that I never returned to my formal graduate engineering studies.

In 1964, Dr. Richard M. ("Dick") Carlson joined Lockheed after 14 years at Hiller Aircraft where he served as manager of the Aero/Structures Department for 8 years. During that time, he earned his Ph.D. and developed and presented a series of VTOL courses at Stanford University covering dynamics, aeroelasticity, and configuration design. While he was at Lockheed, he commuted from Burbank to Stanford each week to teach these courses. Later, after joining the Army at Ames, he continued his teaching activities at Stanford until he became the Director of the U.S. Army Air Mobility Research and Development Laboratory (AMRDL) and passed the baton to Fred Schmitz (see Schmitz' chapter) who expanded the program keeping rotorcraft technology prominent in the aerospace curriculum at Stanford. This was ultimately of great value to the Army and NASA for developing workforce in the rotorcraft disciplines at Ames.

In late 1965, when the AH-56A Cheyenne development project was formally launched, Lockheed-California's rotary-wing functions were separated from the fixed-wing engineering activities and moved to Van Nuys Airport under Jack Real, the new Vice President and General Manager, Rotary-Wing Aircraft. Jack was an experienced engineer who had risen through the flight test side of the Lockheed organization. Dick Carlson was assigned as Division Engineer responsible for directing research and advanced design activities for all rotary-wing projects except the AH-56A Cheyenne. Included in this mandate was providing advanced analysis capability and support for the AH-56A as well. I was assigned to the Aerodynamics Group of the Flight Sciences Department, which also included propulsion, flight controls, and flight test analysis groups. It worked closely in concert with the Dynamics Group and the



Lockheed AH-56A Cheyenne.

Loads Group in the Structures Department, which were responsible for dynamic stability and airloads.

Our principal customer was the Army, but we also looked in depth at designs to meet the requirements of the other services and civil applications, including research in support of NASA and the FAA. This brought me into contact with, and working for, the Army and NASA. My first interface with Ames Research Center was in 1965 on a NASA-Lockheed Short-Haul Transport Study that was conducted for Wally Deckert and Dave Hickey at Ames. I was responsible for the rotary-wing configuration performance design analysis and developing automated techniques for design trade-offs and optimization. Later, as Lockheed developed the Stopped/Stowed Rotor concept in 1967 to support the Army's Composite Aircraft Program (CAP), I encountered the U.S. Army Aeronautical Research Laboratory (AARL) at Ames. We needed to develop a better understanding of the aerodynamic forces and moments in the rotor-hub involved with the three-bladed rotor as it folded. The Lockheed low-speed wind tunnel was booked solid, so we were looking for an alternative. Dick Carlson had worked closely with Andy Morse at Hiller, and he knew that Andy was working for the Army at Ames. The Army 7- by 10-Foot Wind Tunnel was available, so I packed up my model and equipment, and a wind-tunnel mechanic and I headed to Ames. I was welcomed by Andy Morse, who took me to meet with Paul Yaggy, and we reviewed the requirements for the test. Dale Hutchins and Dave Sharpe joined the team, and we set up to measure forces and moments on the wind tunnel balance. We jury-rigged a smoke generation system to get flow visualization around the hub and stub blades. The experience was very positive, and we were able to complete the testing in a few days to obtain the data we needed to complete our design. Working with AARL, I was highly impressed by the professional capabilities, creative energy, and can-do attitude of the organization.

Other assignments at Lockheed included: wind tunnel configuration design and testing, and rotor airfoil design analysis; transonic airfoil and blade tip design and testing; whirl tower and flight test support and data analysis; aerodynamics support and coordination of other Technology Division support including dynamics, stress, loads, and weights for the Utility Tactical Transport Aircraft System (UTTAS) design study configuration development and proposal preparation; high-speed compound helicopter flight demonstration; a high-altitude, long-endurance observation helicopter design; flight-control and handling-qualities design and demonstration; FAA rotorcraft single pilot certification; an Army helicopter airfoil working group organized by AVLABS; and a wide variety of civil and military helicopter and compound helicopter designs. As Group Engineer Aero/Propulsion I was also overseeing propulsion system integration analysis. For 6 months, I acted as the manager of the Flight Sciences Department. This wide variety of activities exposed me to many of the significant technical challenges of rotary-wing vehicles across a broad range of engineering disciplines.

I also became involved in the development of the Revised and Extended Rotor (REXOR) interdisciplinary rotary-wing engineering analysis. This major undertaking arose from the need to consolidate, validate, and coordinate the various analysis capabilities within the Rotary-Wing Technology Division. Each engineering discipline created its own set of analysis codes developed by individual specialists. Performance, airloads, structural dynamics, stress and fatigue, flight control, and propulsion analysis dealt with representations of the basic physics associated with various aero-mechanical phenomena. Every year more advanced computer equipment and capacity made more in-depth analysis possible and practical. Working in the

rotary-wing division of a major fixed-wing company highlighted the differences in the challenges encountered analyzing rotorcraft. It was increasingly evident that all of this technology was orders-of-magnitude more difficult to apply to a configuration that included elastic lifting rotor blades coupled to a flexible control system, operating on a helicopter in forward flight with a complicated unsteady aerodynamic flow-field. The aerodynamics range from transonic conditions, through dynamic stall, reverse flow, span-wise flow, and vortex wake interactions of other blades. Any of these phenomena can have a potential impact on one design discipline or another. The assumptions, like symmetry used for simplification of fixed-wing design analysis, were not applicable for even steady, level flight conditions.

At Lockheed, each discipline had an analyst developing a design analysis code involving one or two engineers and, in most cases, a full-time computer programmer. They also had to fight for time on the large, mainframe computer system. Each of these models was developed and supported by one or two analysts and a computer programmer. Through the development of the Cheyenne, each of these analyses was expanded and applied. Over time, one analysis emerged as the most generally applied design tool. To provide a math model for evaluation of flight-control and handling-qualities characteristics of helicopters in real-time piloted simulation, John "Jack" Hoffman had designed and developed a hybrid simulation analysis using all of the power of the Lockheed hybrid lab. In addition to his driving interest in helicopters and aviation, Jack was also an extremely talented electrical engineer and innovator. He immediately saw the value of the integration capabilities of high-speed analog op-amps to perform the integration functions necessary to make dynamic rotor blade aerodynamic loads and structural response computations. Using an elastic blade modal representation that included the flapping and lead-lag stiffness characteristics of the rotor blades and hub, it was possible to include blade response in the calculation. To make this work, it was necessary to push the response of the hybrid computer components to the limits of their design specifications. In order to evaluate and calibrate his hybrid real-time analysis, he produced a non-real-time digital version to check and calibrate the hybrid analysis. He dubbed the hybrid version "Rotor Junior" but it was never fully implemented and employed as a real-time piloted simulation driver. However, the digital version called "Rotor Senior" became Lockheed's most detailed integrated aeromechanics design code and was used for all aspects of the Cheyenne program and Lockheed advanced rotorcraft design efforts.

Jack left Lockheed to start his own rotorcraft analysis company, leaving the Rotor Senior program in the hands of several other aeromechanics analysts for further development and application. It was used extensively in the development of the Advanced Mechanical Control System for the Cheyenne. It became evident that the code had been patched and modified by different users to the point that the general integrity of the program was compromised. While in use, coding errors were often discovered, and users who wished to add capability were not aware of assumptions and detailed program structures in the system. Engineers in multiple disciplines continued to grow capabilities of other analyses. Documentation was limited and, in many cases, incorrect. It was decided that it was necessary to produce an updated version of the Rotor Senior analysis system to overcome its perceived shortcomings. Very significant resources had been invested over a period of years. Computer technology continued to improve at an astounding rate, and as computer codes grew in size and complexity, it was becoming increasingly important to ensure the integrity of those computer codes and knowledge of their content, assumptions, and limitations.

Lockheed's rotary-wing technology management, led by Dick Carlson and Paul Kesling, his Deputy, asked me to organize an effort to update the Rotor Senior analysis. The update was to be called the Revised and Extended Rotor Senior (REXOR). We pulled together a team of those most familiar with analysis needs. We first established that an intense, short-term effort was needed and that the necessary resources could be made available. The analysis itself would be the focus of the activity; redesign of the user interface would be addressed later. Based on the group's extensive experience with both Rotor Senior and other analysis codes, we would review assumptions and approaches for adequacy and inclusiveness, and we would bring in additional help as required. Extensive effort would be applied to full interdisciplinary participation in the formulation and the quality and integrity of the derivation and code. The approach used is documented in reference [1]. No one on the team had any basic training in the emerging field of large-scale system software development, so we made it up as we went along. This was in no way the approach that had been generally applied in previous analysis system development. Up front, it appeared to be ridiculously redundant and excessively time consuming. However, REXOR was completed in significantly less calendar time than was the norm in developing previous programs, and over the next several years, only three "bugs" were encountered in the code, and extension was very straightforward. In less than a year, a very robust system had been produced that was well understood by a wide-ranging user/developer community. With this background, I felt that it should be feasible to provide a flexible analysis system for the Army to use to evaluate and support the design and evaluation of advanced rotorcraft systems.

In 1972, the Cheyenne program was cancelled. That led to another extensive effort to develop a configuration for a Lockheed proposal to meet the Advanced Attack Helicopter (AAH) program competition. I was responsible for leading the design trade-offs and preliminary design analysis for the proposed configuration. The Army requirements specification for the vehicle was defined with bands of "required to desired" characteristics. In their interface with the Army, Lockheed's marketing team had determined that the "desired" level of vehicle performance was highly preferred, even if it required a power upgrade to the specified engine. Our design, with a maximum speed of over 170 kts and significantly higher development cost, lost the competition to the Hughes AAH proposal. The experience provided me with an in-depth exposure to the analysis capability of all of the engineering disciplines. In a side note, when I presented the proposed design to the Lockheed corporate engineering board, Kelly Johnson commented that the design looked too smooth for the Army, and he directed me to "ugly it up" some before we submitted it.

In 1972, after the cancellation of the Cheyenne program, Dick Carlson accepted an offer from Paul Yaggy to join AMRDL Headquarters (HQ) to head up the Advanced Systems Research Office (ASRO). During his transition we co-wrote, and I presented, a paper on rotor loads prediction analysis for an Advisory Group for Aerospace Research and Development (AGARD) meeting in Milan [2]. This was my first exposure to the world of international technical cooperation. Following the loss in the AAH competition, Lockheed decided to get out of the helicopter business in 1974. Dick Carlson had contacted Ray Prouty to see if he would be interested in joining AMRDL's ASRO to cover the aeromechanics functions. Dr. Hank Velkoff was returning to Ohio State University. Ray had moved to Hughes to work on the Apache, and decided that he would like to stay in industry and in the Los Angeles area. He suggested that I contact Dick to see if I might be of use. I visited Dick, and he encouraged me to apply. I was

interested in the challenge of rotorcraft, and the opportunity to work with some of the most creative engineers and scientists in the field was very attractive.

U.S. Army Air Mobility R&D Laboratory Headquarters

In April 1975 I arrived at Ames Research Center. Francie and our two girls remained in the San Fernando Valley to complete the school year and pack up for the move. I moved into a small furnished apartment in Sunnyvale and reported to my new workplace in Building N-207. During the period between the time I applied for the staff position of Aeromechanics Specialist in the ASRO and my arrival, Paul Yaggy had retired, and Colonel Norm Robinson, the Military Deputy, was Acting Director. (Shortly thereafter, Dick Carlson was named Director.) I was shown to my new office, which I shared with John Whang, a Systems Specialist, and a desk that Hank Velkoff had recently vacated. Dick Carlson was not around that day, so Lorraine Shaw, the Director's secretary, took over, getting me settled and properly signed in. She sent me over to get badged and to receive a NASA Ames overview. When I returned, I found that the well-used wooden desk in my office had been replaced with a gray metal desk. John told me that Lorraine had arranged for the swap of furniture because I was a GS-14, and you had to be a GS-15 to have a wooden desk. This was my first indication that I was no longer in industry and was now in the government. There was much more to learn.

At the time, AMRDL was comprised of the Ames, Langley, Lewis, and Eustis Directorates. According to my job description, my primary function in ASRO was to coordinate and report on aeromechanics activities among the AMRDL Directorates and interface with research activities in other government labs, universities, and industry. This was in aid of identifying needs and gaps in the program, justifying costs, and avoiding duplication. How to do this effectively was up to the individual specialist, but it was necessary to be able to provide input to the annual AMRDL R&D plan and higher HQ plans, and defend the program at reviews at higher HQs. Paul Yaggy and Dick Carlson were looking for specialists with a research background who also had experience in actual flight hardware design and development. However, this was just the beginning because, as a member of the HQ staff, you were available and on-call for any "other duties as assigned." Shortly before I arrived, Dave Key, who Irv Statler had recruited and was assigned within ASRO as Stability and Control and Flying Qualities Specialist, moved over to the Ames Directorate to implement the plan he had developed in ASRO with several other Army researchers in the NASA Flight Dynamics and Controls Branch. I was assigned the ASRO staff responsibility for those disciplines as well. Human Factors, another major Ames Directorate initiative, was assigned to Dr. Dick Dunn, a research psychologist.

I offered my assistance with automated design optimization methodology to Mike Scully who was establishing the preliminary design function within the ASRO. His reaction was that he had been doing design trade-offs for years and could do fine with his non-automated approach. He performed a design study and, when I looked it over, it was evident that he reached an optimum design point without the need for a big computational engine and an advanced computer optimization algorithm, so I retreated. Don't mess with the Master!

I did not know a lot about the details of the Army's relationship with NASA when I arrived at ASRO. It was expanding and improving with each passing year. I was heavily indoctrinated in the history and structure as documented by Irv Statler. It was a great time to be growing a research program. Budgets were high, hardware demonstrations were in vogue, and interesting

new programs were supported by experienced technical leadership in Washington. Personnel for expanded research activities were relatively easy to come by. The approach to Army/NASA partnering by Paul Yaggy, Irv Statler, and Dick Carlson was very effective at Ames. The structure and relationships they built were the basis for the long-term survival of the lab as the climate for aeronautics research support eroded substantially in the late 1980s and 1990s. Paul was a great partner; having come from inside NASA, Paul knew his partner intimately. A successful partner is one who does not start a negotiation with, "What can you do for me?" but asks, "What do you need that I might be able to help you with?" In a successful partnership, you must keep on top of your partner's situation. Another key tenet is, "Share the credit and the blame." Be prepared to give credit for successful activities in a form most useful to your partner's parent organization.

The aeromechanics activities at the AMRDL labs were performed by the three different Directorates at Ames, Langley, and Ft. Eustis. Each Directorate had a unique culture reflecting a mix of Basic Research 6.1, Exploratory Development 6.2. and Advanced Development 6.3. Like NASA Center programs, the culture reflected the research facilities available at each location and the capabilities and proclivities of the workforce. I will focus my discussion primarily on the activities at the Ames Directorate, and other duties as assigned. At the time I arrived in 1975, the Ames Directorate aeromechanics program was primarily in 6.1 and early 6.2, but it also included a major 6.3 project, the NASA/Army XV-15 Joint Tilt Rotor Demonstration program. This was at the peak of the establishment and expansion of the program led by Paul Yaggy and Irv Statler, and assisted by Andy Morse. All of this was made possible by the unique Army-NASA Joint Agreement described earlier in this memoir.

When Dick Carlson asked me to come aboard, having overseen my approach to developing the REXOR interdisciplinary analysis system at Lockheed, I was given an immediate assignment to find a way to undertake a similar development activity by the Army and to make sure that it followed our approach at Lockheed. At the time, this seemed like a reasonable task, but this was another occasion where industry and government have significantly different cultures.

Other early assignments included leading review teams to evaluate problems associated with the Aquila Unmanned Aerial Vehicle (UAV) launch and recovery activities and the development of an advanced composite rotor blade for the AH-1Q Cobra. I served as a consultant to the AAH Source Selection Evaluation Board that led to the selection of the AH-64 Apache. I was subsequently a member of a government "red team" for the AH-64 project manager to resolve issues that included loads and control problems in low-speed and transition flight. I participated in the preparation and presentation of the Army's aeromechanics portion of the annual Office of Secretary of Defense (OSD) Air Vehicle Science and Technology (S&T) Review. I was eventually asked to take on a more active role in this activity, and for a number of years, along with Tom House from the Eustis Directorate, covered the Army's programs in that annual review process. This brought me into close contact with the technical staff at OSD and provided contacts for coordinating S&T activities with the S&T leadership of the other military services and NASA HQ.

In 1977, AMRDL was renamed the Research and Technology Laboratories (RTL). By that time, together with its NASA partner, RTL was recognized as a world leader in the development of rotorcraft technology. We received a request from Dick Ballard to identify some younger members of academia for consideration for invitation to serve on the Army Science Board as

senior members retired. A quick review of the field turned up an interesting finding. Paul Yaggy and company had managed to recruit and train the best in the field, and, for the most part, they were flourishing in the Army lab environment. Students from Massachusetts Institute of Technology (MIT), Princeton, Stanford, and University of California, Los Angeles (UCLA) that had active exposure to the rotorcraft technical challenge and basic aeromechanical phenomena critical to characterizing them were encouraged by their professors to join the organization. Coming from industry, I was most impressed by the dedication of the Army scientists, who could be found any time of the day or night pursuing their research. They chose to live in a high-cost area to have the opportunity to work on the fascinating challenge of rotorcraft. There was very little turnover in the workforce. This was extremely impressive.

Second Generation Comprehensive Helicopter Analysis System. Upon my arrival at ASRO in 1975, after Dick Carlson assigned me the responsibility for an Army interdisciplinary analysis system, I reviewed all of the ongoing activities and plans in aeromechanics analysis and had discussions with the researchers in all of the AMRDL directorates. The Eustis Directorate was planning to contract for a replacement for C-81 (a comprehensive analysis program developed by Bell Helicopter) as an Army comprehensive design and evaluation tool. Researchers at the Langley Directorate were assessing a variety of existing codes including C-81 and REXOR to support their wind tunnel and advanced rotor research. At the Ames Directorate, the principal focus of activity was basic research to more accurately model the complex aerodynamic, structural dynamics, and flight mechanics characteristics of rotorcraft. In 1974, Bob Ormiston had organized an AHS activity to assess and evaluate existing codes to identify areas that needed significant improvement. At Lockheed this was known as the “Ormiston Blackmail program,” since the Army provided no financial support for the industry efforts. I was not personally involved in the activity but had followed Lockheed’s participation from a supervisory perspective.

Dick Carlson’s vision was to create a high-quality interdisciplinary analysis system that was well understood by government, industry, and academia for use as a flexible, open architecture for advanced rotorcraft system development and technology transfer. The objective was not to replace all existing and proprietary analysis tools, but to ensure that, for new rotorcraft development, the industry would be knowledgeable about the Army’s analysis capability for evaluation as a starting point for technical dialogue. At the time, when a new design was proposed by industry, limited correlation and calibration data were presented at the beginning of the technical proposal, and then volumes of predictions data were presented to substantiate the design.

It was decided that the new program would be undertaken by the Eustis Directorate, led by Jeff MacDonald, and it was called the Second Generation Comprehensive Helicopter Analysis System (2GCHAS) as summarized by Wayne Johnson [3]. Jeff and I had long discussions about the appropriate name for the program, and we settled on 2GCHAS partly because the acronym did not appear to lend itself to a cute nickname—something that we wanted to avoid. After the program was announced, Jim Biggers of NASA Ames took one look at the acronym and yelled “2G-Charlie.” This was the first objective of the program unmet. The Army coordinated joint workshops to address the technology specification and integration requirements, while the government and industry got in-depth exposure to the latest state-of-the-art large software system development methods. One of the basic assumptions was that, in order to have a knowledgeable, intelligent user base for the system, a broad user/developer community base was required. Most

of the analyses in current use had been the work of one primary analyst, and experience had shown that when the originator moved on, the cohesiveness of the analysis and its useful life were compromised. The program began with a predesign phase of three study contracts where three major software companies were each teamed with a different helicopter company. At the completion of the studies, feedback from the industry indicated support for proceeding, but that it would require a dedicated government team with some significant hands-on analysis development and application experience to successfully lead the effort—similar to the contracting approach NASA had used for NASTRAN, a finite element analysis program. It was desired to have this team collocated with researchers who were working on the latest analysis approaches.

This led to the establishment of the 2GCHAS Project Office at the Aeromechanics Lab in 1978. I was assigned as the Program Manager as “another duty” from ASRO in RTL HQ, and I pulled together a team of specialists to staff the activity. These included John Davis, who had been a significant contributor to, and user of, C-81 while at Bell; Dr. Wendell Stephens, who had been involved with the development and application of NASTRAN at NASA Langley; and Art Ragosta, who had been a key member of the oversight team of the earlier 2GCHAS efforts at the Eustis Directorate, all of whom were assigned through Bob Ormiston’s Rotorcraft Dynamics Division. Gil Morehouse, who had been a key player in supporting a number of Army activities over the years, took over administrative activities and assisted in the procurement process. The project office awarded a contract to the Computer Science Corporation (CSC) for the development of the “Executive Complex” for the system (i.e., the framework into which rotorcraft technology and analysis modules could be plugged). Frank Douglas, Doug Haller, and Larry Babb were the key personnel from CSC who supported the project office.

During the early days of this phase of the project, there was support from the community at large, but not much interest from the research scientists in the Aeromechanics Lab. The general feeling was that the basic analytical approaches for aeromechanics modeling of rotorcraft were not yet sufficiently developed to include in the system. The engineering analysts in industry used an approach where they applied the best available math basis to create a design tool that was then tested and calibrated. Shortcomings and inadequacies were identified and addressed where practical, and approaches were suggested for future research. It became evident that the approach that we had used for REXOR development was not going to be as effective in developing 2GCHAS. We had a large interdisciplinary team for REXOR, but we were able to come to consensus on modeling issues by locking the door until everyone agreed on a way forward. This proved impractical in the distributed government/industry environment in which we were operating.

In the meantime, Wayne Johnson was making great progress with his Comprehensive Analytical Model of Rotorcraft Aerodynamics and Dynamics (CAMRAD) analysis demonstrating that a single extraordinary analyst could produce an outstanding product in a much more expeditious manner. CAMRAD was by far the most powerful comprehensive analysis tool ever developed and extensively applied through the 1980s and 1990s. Its only shortcoming was that it had been developed by one analyst without the active involvement of an extensive cadre of developers and users intimately familiar with all aspects of the tool. Wayne was a valuable consultant to the 2GCHAS Project Office, but he could not wait for the cumbersome process to play out if he could execute the development himself. Wayne did an outstanding job of documentation and training for users, and CAMRAD II continues to be a very effective comprehensive rotorcraft

analysis tool. In the ultimate, I quote the esteemed Frank Harris, who stated, “We are fortunate to have the parallel development of CAMRAD and 2GCHAS/Rotorcraft Comprehensive Analysis System (RCAS) available to the community providing a check on both analysis systems.”

In 1982, I succeeded Fred Immen as Chief of ASRO when he moved to the Engineering Division at the Aviation Research and Development Command (AVRADCOM) HQ in St. Louis. In this role I functioned as the Chief of the technical staff for the Director of RTL, coordinating the planning and reporting for the activities at all of the RTL laboratories, and supervising the preliminary design activities at RTL HQ. By this time, Bob Ormiston, although still a bit leery about the practicality of the objectives of the program (see Bob’s chapter), took over the direct responsibility for 2GCHAS in his division. I continued to provide RTL HQ oversight for the activities and ensured funding the revised direction, but was no longer involved in day-to-day activities of 2GCHAS. Wendell Stephens remained with the program, but John Davis and Art Ragosta transferred to Mike Scully’s Preliminary Design Team within ASRO with Davis becoming the lead for methodology development and Ragosta supporting analysis activities and managing information technology functions.

International cooperation. In my role as ASRO Aeromechanics Specialist, I became familiar with the international cooperative research activities being conducted under Irv Statler’s leadership at the Ames Directorate (see Irv’s chapter). I attended a number of meetings and was very impressed with these basic research programs. However, in some cases the efforts lacked a critical mass of researchers. Through his participation in AGARD, Irv had developed relationships with government research institutes in France, Germany, and Italy with common interests in rotorcraft, and this helped to identify young scientists to participate in the cooperative research activities. In most cases, there was little or no comparable work going on anywhere else in the U.S. By working together, researchers could enhance their efforts to better understand the basic physics of complex phenomena. Personnel exchanges also offered a unique opportunity for Ames Directorate researchers to be exposed to a different research environment and learn how similar investigations were approached in laboratories in other countries. The development opportunity for Army scientists was very valuable to our U.S. participants, and the collaborations considerably enhanced our research products. Individual chapters in this memoir give many examples from their authors’ experiences. In 1982, under the German Memorandum of Understanding (MOU), the Ames-Army Lab and the German Aerospace Research Establishment (DLR) performed the first aeroacoustic rotor test on an Army AH-1G rotor in the new, state-of-the-art German/Dutch low-speed anechoic wind tunnel in the Netherlands. This activity, under the direction of Fred Schmitz and his team, established the gold standard for this type of rotor research. This led to the identification of U.S. offset funding to the Netherlands to cover the tunnel operations and support costs for the Aerodynamic and Acoustic Testing of Model Rotors (AATMR) series of tests (see Fred Schmitz’ chapter). This highly successful program included a series of tests involving Boeing, McDonnell Douglas, and Sikorsky over the next several years that generated an exceptional database and close working relationship between the Army, NASA, and U.S. industry.

In 1976, I was approached by Dick Ballard at the Department of the Army (DA) and asked to participate in The Technical Cooperation Program (TTCP). This cooperative military R&D program was administered under a treaty between the U.S., United Kingdom (UK), Canada, Australia, and New Zealand that was managed for the U.S. by the Office of the Under Secretary of Defense for Research and Engineering. Dick was the U.S. Army member of a subgroup

addressing Aeronautics Technology. In addition to the three military services, NASA was an active participant in the subgroup activities. There was a distinct interest between the U.S. and the UK in establishing cooperation in helicopter technology. Dick was tasked with establishing a Technical Panel (TP) to address the area. He arranged for me to meet with Ralph Maltby, the head of the Structures Department at the Royal Aircraft Establishment (RAE) at Farnborough. Ralph had overall responsibility for all aspects of helicopter research at RAE. We exchanged overviews of ongoing research initiatives in the two nations and created an Action Group (AG) to see if we could develop some cooperative activities of mutual benefit in the areas of helicopter aerodynamics, dynamics, and man-machine integration. The U.S. members of the AG included Dick Dunn, Human Factors Specialist in ASRO, Kip Edenborough representing NASA, and Dale Hutchins representing the U.S. Navy. Initial visits were made to research organizations in both nations, leading to the definition of several areas of interest worth pursuing. These activities were approved by the Aeronautics Subgroup, and the AG evolved into a TP. Canada, Australia, and New Zealand were becoming active in a number of helicopter technology areas, and as cooperative opportunities arose, these nations also became active participants in the panel. U.S. participation in these activities was not limited to Ames Directorate personnel, and enhanced active cooperation between U.S. research activities in a number of areas resulted. Significant contributions to Ames Directorate aeromechanics, flight controls, handling qualities, simulation, and man-machine activities were realized.

The process for establishing collaboration under TTCP was a bit different from that of AGARD and the bilateral MOU activities. To establish the potential for collaboration, each member nation presented an integrated picture of their programs related to each technical panel within a subgroup, exposing the panel members to the plans, but also introducing the panel to the researchers performing activities in the different nations. In the Aeronautics Subgroup, U.S. activities were coordinated by the same staff specialists who performed the annual OSD Air Vehicles S&T review activities. It became evident to me that the program information shared by all of military services and NASA in our TTCP TPs and study areas was often more complete and clear than that presented in the OSD and NASA inter-service reviews. The U.S. teams also described their activities in a more integrated manner in order to better convey the details of our national programs. This was in part due to the vestiges of inter-service rivalries and competition between the services for mission and resources. Over the many years that I was associated with TTCP, I found that the dialogue and search for common interest and leveraging of resources helped significantly in coordinating research activities among the U.S. services. It had an additional benefit of expediting assistance between the services in a wide variety of research activities and aircraft development programs. I also built a close working relationship with the OSD staff that was extremely useful in sustaining support for Army programs when NASA and the Army significantly reduced resources for Aviation S&T in the late 1980s and 1990s.

As Dick Ballard prepared to retire, he had me appointed to represent the Army on the TTCP Aeronautics Subgroup. This subgroup later became the Aerospace Systems Group on which I served until my retirement. Bill Bousman and Nancy Bucher, who had been leading rotor loads and man-machine integration research cooperation, took over the U.S. leadership of the helicopter TP. After Don Dix retired from OSD, I was asked by Dr. Dolores Etter, Deputy Undersecretary of Defense for S&T and TTCP U.S. Principal, to serve as the U.S. National Representative for the Group, including 3 years as Group Chair. Our Army bilateral MOUs were invaluable to the development of the Army research scientists and contributed a great deal to our

research programs. TTCP provided a similar vehicle for working with the UK, Canada, Australia, and New Zealand, with a focus more on easy access to specialists, facilities, and equipment, and participation in test activities and workshops in lieu of long-term personnel exchanges. It had the additional benefit of significantly improving the constructive cooperation with the Navy, Air Force, and other Army research activities, with somewhat fewer management meetings and less reporting overhead.

Aeroflightdynamics Directorate

In 1985, RTL became the U.S. Army Research and Technology Activity (ARTA) and the Aeromechanics Laboratory became the Aeroflightdynamics Directorate (AFDD). In 1986, I was selected to replace Irv Statler as Director of AFDD after he departed to become the Director of AGARD. Irv had left a well-organized, fully-functional organization that had been performing, and continued to perform, world-class research. The partnership with NASA at Ames Research Center was running very well and effectively addressing a wide range of research areas critical to Army missions. The three research divisions, Fluid Mechanics, Rotorcraft Dynamics, and Flight Control and Man-Machine Integration, were ably led by Fred Schmitz, Bob Ormiston, and Dave Key, respectively. They supervised all of the technical personnel reporting to them directly and those assigned within the NASA research organizations. Andy Morse had retired, and Bill Eckert headed up the Research Support Division that provided all of the Army supervision for those Army personnel imbedded within the NASA organization as “quid pro quo” under the Army-NASA Joint Agreement (see Irv Statler’s chapter). Two invaluable assets were Alice Meyers, Administrative Officer, and Lynda Jones, Irv’s secretary of many years. They knew the business and all of the details of its operations, which made it easy for me to carry on. In later years, I tried to encourage Lynda to pick up more administrative functions to qualify for promotions, but she insisted that her desire was to continue to provide the functions that she was comfortable with and did so well. She was still doing them flawlessly when I retired in 2007. Since I had been following the directorate’s programs, advocating for resources, and defending the programs at higher HQs for many years across the street at ASRO, I was able to hit the ground running. As noted by a number of the authors of this memoir, and to paraphrase a standard greeting from Lieutenant Colonel Chris Sullivan when he was Chief of the Army Flight Projects Office in AFDD, “It was a great day to be in Army rotorcraft S&T.”

Other chapters in this memoir track the technical and professional accomplishments of the Army activities at Ames Research Center and beyond. I will concentrate on covering the period after Irv Statler’s departure, with particular attention to the sustainment and evolution of the organization that is still operating in partnership with NASA today.

The partnership. The Army leadership at Ames understood the principles of good partnerships and applied them to build a relationship that is still effective 50 years later. All institutions evolve and flexibility makes partners strong. When one partner is having trouble, there is often a great opportunity to realign operations and programs and come out of difficulties stronger than ever. This is especially true when one partner is the U.S. Army. In my early days on the new job, I received some insight, from an unlikely source, into the world where I was now operating. A young Major from the Eustis Directorate told me: “In the Army, 3 is infinity.” He said that the normal Army tour of duty in any military job assignment is 3 years. The first year you learn your new job, the second year you do it, and the third year you are focused on finding your next job. This somewhat cynical observation did contain an element of truth that helped to orient my

understanding of organizational dynamics within the Army and how it was different from my experience in industry. The bottom line is that flexibility and agility are necessary to survive and thrive in a dynamic resource and management environment.

The high-level vision and directions of all large institutions are continually in a state of flux. By the mid 1980s, it was becoming evident that the resources for advanced rotorcraft S&T were in a gradual decline. The Army's 6.1 Basic Research and 6.2 Exploratory Development budgets for aviation remained relatively constant, but the cost of performing the research was impacted significantly by inflation.

Every few years, both NASA and the Army were looking for opportunities to eliminate or consolidate activities. For most of the life of the partnership, the concept of "dual use" was embraced by both agencies. The limited national investment in rotorcraft technology was modest, and the partnership allowed the sharing of resources to address both military and civil needs. The Army was designated as the lead service for the development of military rotary-wing technology for the Department of Defense (DoD), so the close relationship at Ames was ideal for both agencies. In 1977 Ames had become the lead NASA Center for rotorcraft research and technology. Organizational structures and personal working relationships established during the first 20 years of the partnership were critical to accomplishing the mission for both agencies to meet national technology needs.

Crew Station Research and Development Facility. The mid-1980s saw a significant change in emphasis in areas where the Army's researchers in flight control, man-machine integration, and automation were primarily working within NASA's organizational structure. Major programs like the Crew Station Research and Development Facility (CSRDF) (see Terry Gossett's chapter) were designed to study the one- versus two-crew-member(s) and cockpit design requirements for the Army's Light Helicopter Experimental (LHX) attack helicopter. This was a major Army initiative that could only have been developed and operated in the integrated technical and administrative environment at Ames. The Human Factors program that Irv Statler had been building had attracted a cadre of talented research psychologists, and the joint activities in the development and operation of the Vertical Motion Simulator (VMS) provided an environment to attack the issue. In 1985, when the project was initiated, Bill Ballhaus was the Ames Center Director. He provided wholehearted support to Dick Carlson and Terry Gossett's CSRDF team using all of the mechanisms and relationships developed over the previous 20 years by Paul Yaggy, Irv Statler, and Dick. In 1987, Terry Gossett transferred to AFDD from ARTA HQ to become Chief of the Simulation and Aircraft Systems Division, providing outstanding leadership of the Integrated Flight Controls and Man-Machine Integration programs.

JVX program and the V-22 Osprey. The partnership at Ames was critical to the initial development of the V-22 Osprey (see chapters by Maisel and Statler). The multi-service requirements study conducted at Ames in 1982 for the Joint Vertical Experimental (JVX) program took the knowledge gained in the XV-15 program and a series of predesign studies led by Mike Scully at RTL HQ to develop a set of requirements to meet Army, Navy/Marines, and Air Force needs. As the program got underway, the Army functioned as the lead service, supported by NASA, for technical oversight and support in the development program. In the mid-1980s, Dr. James Ambrose, Undersecretary of the Army, withdrew the Army from the program for affordability reasons centered on the higher priority need for the LHX Comanche armed reconnaissance attack helicopter and Future Combat Vehicle armor program. The

technical oversight for the JVX program passed to the Navy, and the Army was directed to cease its support.

The government's tiltrotor expertise at that time was consolidated in the combined Army/NASA Tilt Rotor Aircraft Office. Since the Army had been the DoD lead service for rotary-wing technology R&D, the Navy had relatively little background in advanced rotorcraft technology and the tiltrotor configuration. Culturally, the military services often tend to be reluctant to ask for support from each other, but it was very acceptable to consult with NASA. The expertise in the Tilt Rotor Aircraft Office at Ames was completely integrated technically, and the experts in any area might be on the Army or the NASA payroll. The project office was not in competition with anyone, and availed itself of assistance from anyone in either organization—a product of partnership. As a result, when problems arose in the V-22 development program, the Navy was able to request assistance from NASA, and accept the assistance of key Army personnel, engineers, and pilots, to address those problems. AFDD personnel, traveling on NASA travel orders, were able to provide key expertise to support the program. Army scientists and engineers continued to be assigned within the NASA Tilt Rotor Aircraft Office to jointly pursue the technology. All during this time, the ARTA HQ Advanced Design Team supported NASA civil tiltrotor studies while conducting similar design studies for future Army aircraft.

Rotorcraft Centers of Excellence. In the early 1980s, the DA decided that it could significantly enhance the scope and quality of its 6.1 Basic Research programs by establishing centers of excellence for various technologies at major universities. Rotorcraft was one area identified, and the Army Research Office (ARO) in Durham, North Carolina, which managed the program, solicited academia for establishing three centers for rotorcraft. These Rotorcraft Centers of Excellence (RCOE) would be funded at a level to create a critical mass of work that would address rotorcraft technology challenges and also create a trained workforce to support government and industry needs. Dr. Robert Singleton from ARO solicited AFDD scientists to assist in preparing a Request for Proposals and subsequent evaluation of proposals for this program. Up to that time, ARO and AFDD had been funding individual professors at a number of institutions with relatively small grants, but generally on an individual principal investigator basis. As is evident from reading the individual backgrounds of the contributors to this memoir, several universities had significant research and courses addressing technology relevant to rotary-wing vehicles. MIT, Princeton, and Stanford graduates were significantly represented among NASA and Army researchers at Ames. Bidders were evaluated on their proposed research activities, new courses and facilities, and staff qualifications. Based on the evaluation criteria, Georgia Institute of Technology, Rensselaer Polytechnic Institute, and the University of Maryland were selected in 1993. They started from a smaller baseline than the established programs and were able to show considerably more growth in their proposed programs. ARO recruited two teams to assist them in technical and management oversight of these centers. A number of AFDD scientists participated on the Technical Review Panel along with senior technologists from the Army, NASA, the Navy, and the helicopter industry. I was asked to lead the Management Review Panel made up of senior technology managers from the same organizations. Annual meetings of each panel were held on-site at each center, with the technical panel getting into more details on the individual research activities. This presented an opportunity for the ultimate users of the technology and graduates from these centers to compare notes on how these products would meet their needs. This also assisted in technology transfer from the universities and the government labs to industry. Tom Snyder represented NASA Ames

as a member of the management panel. The RCOE program became more relevant to the Ames program in the 1990s when RCOE graduates and research products began to have a significant impact on industry and government R&D activities.

Advanced Systems Research and Analysis Office. In 1989, the S&T management function of ARTA HQ was moved to the Directorate for Advanced Systems (DAS) at the U.S. Army Aviation Research, Development, and Engineering Center (AVRDEC) in St. Louis, leaving only the ARTA preliminary design and analysis activity from ASRO in place at Ames. This marked the first time in the history of the Army-NASA Joint Agreement that the management function for Army Aviation S&T was not located at Ames. The remaining functions at Ames were assigned to the Advanced Systems Research and Analysis Office (ASRAO) led by Dick Carlson, reporting directly to the Director of DAS. NASA continued to support this activity administratively under the local Army-NASA Joint Agreement, and Mike Scully's Advanced Design Team continued its support of NASA Ames advanced rotorcraft design and analysis activities. Upon Dick Carlson's retirement in the mid-1990s, the ASRAO Advanced Design Team was assigned to AFDD along with all residual administrative and support personnel. Wayne Mosher, who had been the Deputy Chief of ASRAO, assumed the role of Deputy Director, AFDD, and Art Ragosta was appointed Research Support Division Chief, supervising all Army personnel assigned within NASA support functions and all internal Army support functions. He was also named Chief Engineer of the Directorate, responsible for all safety issues and Safety of Flight certification.

National Rotorcraft Technology Center. In 1995, Dr. Wesley Harris from MIT was appointed Associate Administrator for Aeronautics at NASA HQ. I had worked with Wes on occasion while he served on the Army Science Board. Soon after he arrived at NASA, I met him at the Pentagon while I was briefing the Board on the Army's advanced rotor programs. After the briefing, I met with him to discuss the possibility of organizing a roadmap for the future directions of national rotorcraft programs. He was interested, and we decided to invite representatives from across the rotorcraft R&D community to propose a roadmap for technology development. Rhett Flater, the Executive Director of the AHS, agreed to provide a venue for an informal meeting with senior technology managers across the industry and government community to explore the concept at the 1993 AHS Annual Forum in St. Louis. Wes and I sat at the end of the table, and as Wes called the meeting to order and welcomed the attendees, George Singley, the Deputy Assistant Secretary of the Army for R&T, walked into the room and sat down at the table. As I was about to add my welcome, George stepped in and did it instead. From then on, I became an observer, and Wes adapted to follow George's lead. The desirability of a coordinated approach to the planning of rotorcraft research and technology was accepted by all present. NASA and the Army, leveraging the experience of the Army-NASA Joint Agreement, took on the task of organizing a process to do this. Wes asked Rhett Flater to set up a meeting with George for informal discussions of possibilities for such an activity involving DoD, industry, and academia. The National Rotorcraft Technology Center (NRTC) was born.

Over the next year, a great deal of effort was expended by a wide variety of organizations with an investment in rotorcraft research to develop the concept of an entity for focusing the common investment effectively and sharing the results. Tom House, Executive Director of AVRDEC in St. Louis, was assigned by George Singley to flesh out a concept of operations (CONOPS) for the Army. The Center was to be located at NASA Ames because of the history of Army Aviation S&T management oversight by ARTA for many years, similar to the situation that had

contributed to the NASA decision to name Ames Lead Center for rotorcraft back in 1977. George directed Tom not to let me get my hands on the process for fear that I would try to skew the new system too close to the other AFDD technical missions.

In the late 1980s, I had worked with John Johns from the Naval Air Development Center (NADC) on the TTCP helicopter panel that I led. After Bill Eckert left his AFDD position as Research Support Division Chief to join NASA for a development assignment at NASA HQ, I was searching for a suitable replacement. I asked the U.S. TTCP panel members if they knew of anyone who might be good for, and interested in, the job. John had done an outstanding job earning his technical stripes at the NADC, and it turned out that he was planning to resign and pursue a full-time MBA degree before his next career move. He called me the following week and said that he might not have enough experience for the position, but he would be willing to forego graduate school to join the Army to learn the ropes under the leadership he had observed during our TTCP activities. He joined AFDD and was instantaneously an immense help. By 1993, when Tom House needed assistance in developing the NRTC CONOPS, he called on John to work on fleshing out the details. John's flair for organization and management was put to good use in establishing the NRTC approach, and he later moved to AVRDEC in St. Louis as Assistant Director-Systems and subsequently became the Deputy Commander for Systems Support at the Aviation and Missile Command (AMCOM) in Huntsville. He later returned to the Navy at NADC before moving to OSD where he served for many years as Deputy Assistant Secretary of Defense for Maintenance Policy and Programs. John was the only aeronautical engineer that I ever worked with at AFDD who regularly read the Harvard Business Review for entertainment.

The NRTC was established as a geographically distributed organization consisting of elements from the government, industry, and academia. The Government Office was established at Ames, and staffed by personnel from the Army, the Navy, NASA and the FAA. This office was the overseer of a portfolio of cooperative research projects performed and co-funded, 50/50, by the government and industry. The industry program was administered by the Rotorcraft Industry Technology Association (RITA), which included rotorcraft manufacturers, suppliers, and academia. Wes Harris recruited Tom Snyder (see the epilogue of Tom's chapter) to serve as Director, and Tom House assigned John Davis to be Tom's Deputy Director. A Naval aviator and an FAA engineer joined the office, along with John Yuhas, from George Singley's office at DA. The management of the RCOE program was transferred to NRTC from ARO, and Yung Yu joined the staff as Chief Scientist to manage the RCOE program and the government participation on some collaborative efforts within the community. John Ward, who was retired from NASA, was hired as Executive Director of RITA. The AHS Executive Director, Rhett Flater, played an active role in shaping the structure, operating principles, and organization of RITA; he offered a neutral environment for the industry to develop a way to work together to cultivate the trust to share their dwindling Independent Research and Development (IRAD) resources to attack critical precompetitive technology development. He was offered a non-voting seat on RITA's Board of Directors. In 1995, NRTC commenced operations under an agreement modeled after the Army-NASA Joint Agreement, with the Navy and the FAA as co-signatories.

After Tom Snyder retired in 1998, it was deemed appropriate for the Army to appoint an Army executive, with a NASA Deputy, to replace him. The concept was to alternate leadership appointments between the agencies when they became vacant. There are many fewer Senior Executive Service positions available in support of Army Aviation S&T than there are at a major

NASA Center, so Tom House appointed me to be Director of both AFDD and NRTC. Joe Elliott, who had been an Army researcher at Langley for many years and served as rotorcraft program coordinator under Bob Whitehead at NASA HQ, became a NASA employee and was assigned as my Deputy. John Davis returned to AFDD and became as the Chief of the Aviation Advanced Design Office. In spite of George Singley's original wishes, I ended up in the NRTC after all.

Joint Army/NASA Rotorcraft Division. By the early 1990s, everyone was feeling the squeeze of reduced S&T budgets and a continued effort to consolidate and eliminate research activities. One of the favorite techniques to accomplish this in government is to impose stringent hiring and promotion freezes. The situation at Ames was starting to degrade so that individual initiatives by the two agencies were difficult to staff effectively, and the professional development of many research scientists was being impacted. John Burks, then Ames Director of Aeronautics, and I appointed Mark Tischler and Fred Schmitz to head up an Ames Vision Team with members from all relevant organizations at the Center to review current rotorcraft programs and research activities at Ames, identify future directions for these activities, and assess the research staff available and needed to achieve the combined research goals of both agencies. The current structure had been in place for decades, and the team was asked to determine if a somewhat different approach could strengthen the Army and NASA rotorcraft research programs at Ames. An alternative that emerged was to create a new organizational structure that would provide a more cohesive arrangement to flexibly optimize personnel assignments and technical directions in a more unified way. It also would provide an environment to further develop the leadership skills of the workforce.

The structure that emerged from the study was a joint Army/NASA Rotorcraft Division that would exist in both organizations. At the Division level, a Chief would be selected from one agency, and a Deputy would be selected from the other. This would be mirrored at the Branch level. The proposed structure was an organization with two branches: an Aeromechanics Branch, and a Flight Control and Cockpit Integration Branch. The proposal was briefed through the Army and NASA chains of command and endorsed. This was an unusual structure between agencies, but reflected other project office assignments of personnel that had been making the partnership work very well over the years, such as the Tilt Rotor Research Aircraft Project Office. The Chief or Deputy of each Division or Branch would be the Supervisor of Record for their agency's members of the Division. By 1997, Fred Schmitz had become the NASA Ames Director of Aeronautics. The proposed concept was briefed to all concerned, and anyone interested in a leadership position in the Division was encouraged to interview for it. Fred Schmitz, Ed Aiken, Wayne Mosher, and I comprised a panel and conducted interviews covering technical qualifications, the candidate's view of the new vision, supervisory capability, and career goals and plans. On April 2, 1997, the new Division launched, with Ed Aiken as Division Chief and Wendell Stephens as Deputy Chief, and Bob Chen and Yung Yu as Senior Scientists. The Aeromechanics Branch was led by Bill Warmbrodt, Chief, and Chee Tung, Deputy, with Bob Ormiston as Chief Scientist. The Flight Control and Cockpit Integration Branch was led by Bill Hindson, Chief, and Barry Lakinsmith, Deputy.

For most of the next decade, the Army/NASA Rotorcraft Division at Ames performed a flexible, world-class, highly recognized research and technology program, and provided outstanding technical support to the Army, Navy, and industry. Ed Aiken's leadership style and capabilities were central to making this process work.

Army Flight Projects Office. In 1997, the NASA HQ decision to transfer all NASA flight research operations in the agency to Dryden Research Center at Edwards Air Force Base in Southern California was another development that had a transformational impact on the partnership. Throughout the partnership, the Army had provided a number of research aircraft to Ames to support a wide variety of both NASA and Army research objectives. These aircraft were assigned to NASA at Ames, and were managed, maintained, and operated by the NASA Flight Operations organizations at Ames. The Army also provided test pilots and support personnel to these organizations. NASA was responsible for all operations of the aircraft and overall airworthiness release and safety issues for them. This was at a time when the Army was under fire to consolidate and eliminate organizations, so the prospect of AFDD having to establish a new organization at Dryden, and try to relocate impacted Army support personnel to Dryden, was a nonstarter. In that same year, in response to the 1995 Base Relocation And Closure (BRAC) Act, the Army was merging the aviation portion of the Aviation and Troop Command with the Missile Command to form AMCOM and moving the aviation activities in St. Louis to Huntsville. The immediate threat to the rotorcraft flight research community at Ames was dire.

Bob Whitehead, the NASA Associate Administrator for Aeronautics, and Dr. Henry “Harry” McDonald, Ames Center Director, got together and proposed a solution. They suggested that AFDD establish a flight operations function in a hanger that they would make available under the local Army-NASA Joint Agreement. AFDD would assume responsibility for all of the research support functions, including test pilots and airworthiness responsibilities. Since NASA flight operations support positions were moved to Dryden, Bob Whitehead would ensure that NASA would provide approximately \$2M a year for the Army to use to fund a contractor to provide the operations functions to replace those that were critical to rotorcraft flight research. In return, AFDD would supply the flight operations to support NASA and Army rotorcraft research at Ames. After negotiations at the Army Command level, the Army agreed with the proposal, and AFDD established the Army Flight Projects Office in Building N-248. Lieutenant Colonel Chris Sullivan, a newly arrived experimental test pilot, was assigned as Chief. NASA HQ was satisfied that their directive had been met. There was an Army sign on the hanger. Marty Maisel was assigned as Airworthiness Officer and Contracting Officer’s Technical Representative (COTR) for the support contract, and other personnel were assigned from other related Army assignments (see Marty Maisel’s chapter). They hit the ground running with an outstanding flight research support operation. Ames management expedited contracting and facilities setup to support the effort. Another creative, expedited solution to a very complex problem swiftly resolved by the resilient partnership. Without the capabilities of this operations office, the world-class, highly recognized AFDD contributions to flight control, handling qualities, guidance, human systems integration, autonomous rotorcraft operation, and battlefield situation awareness would not have been possible. Bob Whitehead, Harry McDonald, and the entire NASA support arrangement and outstanding personnel made it possible.

Computational fluid dynamics. Progress in computational fluid dynamics (CFD) continued to be a highlight of the partnership at Ames. It is well documented by Jim McCroskey in his chapter. The products of the joint Rotorcraft CFD Group at Ames were highly recognized in the field. Bob Meakin’s modeling of the flow around the V-22 Osprey tiltrotor showed the great potential for the technology. Roger Strawn continued to recruit top talent for the group, adding capability with funding from the DoD High-Performance Computing Modernization Program (HPCMP). To model rotorcraft flow-fields accurately requires modeling of the interactions

between multiple moving bodies. Meakin modified the NASA Overflow CFD code to accommodate moving bodies in addition to other enhancements (Overflow-D). When the Columbia space shuttle accident occurred in 2003, one of the crew members was a NASA colleague of Bob's from Ames. Bob Meakin came to me and asked if he could divert his near-term rotorcraft efforts to analyze the debris' loads and dynamics to support the accident investigation. I encouraged him to do so. His contributions expedited the analysis of the accident, providing a clear example of the partnership's value to NASA.

The ability to significantly reduce the reliance on expensive experimental facilities by replacing them with robust analysis methods for vehicle design and development was still an important goal for our rotorcraft R&D program. As the Army aviation community continued to look for ways to reduce system development cost and risk, a request was made by DA S&T management in 2005 to look at the state of the technology and determine the progress and adequacy of emerging design tools, including CFD, computational structural dynamics (CSD), and comprehensive analysis, and define what would be needed to reach the goal of reliable "computation-to-flight" for the development of Army aircraft. Bob Ormiston assembled a team to define a 10-year program of methodology development and a definitive set of experiments that could be performed to evaluate and certify the adequacy of the computational technology. This would require an investment of just under \$100M for both analysis development and experimental validation. In the meantime, HPCMP was soliciting proposals for establishing 6-year HPC Institutes. The Army proposed the analysis portion of the 10-year program that led to the establishment in 2006 of the HPC Institute for Advanced Rotorcraft Modeling and Simulation (HI-ARMS), which was established at AFDD with Bob Meakin as the project manager. In 2007, Bob Meakin was named project manager for the rotorcraft portion of the DoD Computational Research and Engineering Acquisition Tools and Environments-Air Vehicles (CREATE-AV) program, which addressed the full range of aircraft design. The HI-ARMS rotorcraft activity was subsumed in the CREATE-AV program and continued to be implemented at Ames.

A New Century—Partnership Challenges

Dale Compton replaced Bill Ballhaus as Ames Center Director in 1989; along with Vic Peterson, his Deputy, Dale placed a high priority on active involvement in, and support for, the partnership. In 1994 Dale retired and was replaced by Ken Munechika, who, with his Air Force background, identified with the Army mission at Ames. He contributed enthusiastically to the robust continuation of the partnership. In 1996, Harry McDonald was considered for the position of Ames Center Director. Harry had worked closely with Bob Whitehead in prior years. When NASA was negotiating the possibility of his appointment under an Interdepartmental Personnel Assignment (IPA) as Ames Center Director, Cecil Rosen from NASA HQ called me and said that Harry would be visiting Ames to meet with a few people. He asked me to meet with Harry to ascertain how well he might work with the Army and the Joint Agreement. We had a very constructive discussion, and I enthusiastically endorsed the concept of working with Harry. Returning to Ames after his tenure as AGARD Director, Irv Statler, as NASA Division Chief for Human Factors, enabled the joint development of computational man-machine integration technology that he had initiated when he was the Director of AFDD. Jack Boyd continued to be an invaluable resource for maintenance and enhancement of the Army's partnership with NASA. He had an in-depth background in the partnership, and had an unmatched understanding of how to make things happen in the NASA Ames environment.

As we approached the millennium, NASA continued to suffer from inadequate funding for its aeronautics program and its panoply of expensive facilities. The obvious approach to the problem was to look for ways to economize on facility operations, and to look for technology areas that were mature enough to be cut significantly or eliminated from the NASA aeronautics research portfolio. Daniel Goldin was the NASA Administrator during this period, and the agency instituted a policy of supporting major facilities through full-cost recovery of facility operation expenses from programs and outside customers and other government agencies. This impacted the Army partnership in two major areas. The first involved the closing of the National Full-Scale Aerodynamics Complex (NFAC). It later precipitated a need to restructure the Army-NASA Joint Agreement and to separate the staff of the joint Army/NASA Rotorcraft Division and revert back into separate NASA and Army organizations.

The U.S. rotorcraft technical community is very small compared to the rest of the aerospace technology business. George Singley's vision for the NRTC/RITA partnership kept the community in constant contact through weekly teleconferences between RITA, NRTC, and the industry technology managers. The RITA Board met regularly and comprised VP Engineering-level executives from the rotorcraft prime manufacturers. As the community worked together, they gained trust and identified common interests that needed to be addressed. One of these was concern about the health of NASA's support of rotorcraft technology in a resource-constrained environment and affordable access to critical national research and test facilities.

One additional source of support emerged in 1998, when Hans Mark became Director of Defense Research and Engineering (DDR&E) at OSD. He was intimately familiar with the technology and had played a major role in the development of our Ames partnership. While he was awaiting confirmation for his appointment he asked Jack Boyd to arrange a meeting with me, Mike Scully, and Jim McCroskey so that he could get a candid view of the status of rotorcraft technology development and the Ames partnership. He asked me to keep him informed about developments that might have an impact in the days ahead. I later kept both Hans and Delores Etter apprised of the long-term health of our NASA partnership and the difficulties we foresaw in maintaining access to the NFAC for our advanced rotor initiatives.

National Full-Scale Aerodynamics Complex. As NASA unilaterally increased the access costs to the NFAC under its new accounting practices, other agencies saw their program plans severely impacted in the near term. The Joint Strike Fighter was due for testing in the NFAC, as were a number of Department of Energy wind energy programs. NASA and the Army had a number of entries planned for rotorcraft, and NASA had some parachute testing in support of the space program. NASA informed its "customers" that the new fully burdened costs must be met, but NASA was not stabilized in setting the costs for each facility until they could accurately predict facility utilization. NASA had a transition path for funding their own activities. They identified costs that had traditionally been covered in their facilities operation budget and transferred resources to book-keep these costs to the NASA programs using the facilities. Unfortunately there was no such mechanism for other agencies and customers. The price kept varying, and whoever signed up first for entry under the new accounting system would be charged a lion's share of the annual operations costs. Customers fled. Things were not looking good.

In FY00, \$1.5M was moved from the NASA rotorcraft budget to cover ongoing preparations for rotorcraft testing. In FY01, \$9M of NASA rotorcraft program funds were used for NFAC operation. In FY02, NASA identified rotorcraft a mature technology and zeroed out its rotorcraft

program. Action by the industry resulted in a congressional earmark to continue rotorcraft research, some of which was used to complete the testing of the UH-60A airloads research rotor and an additional set of wide-chord rotor blades. In January 2003, the testing was completed, and NASA closed the NFAC.

The debate over reopening the NFAC and restoration of rotorcraft to NASA's Aeronautics program continued to rage in Washington. The DoD engaged in the process with considerable encouragement from industry. Leaders of the U.S. rotorcraft industry cooperated very effectively to develop a cogent case, in large part because of the working relationship developed during the NRTC/RITA partnership. The industry approached Congress to use the earmark process to sustain the tech base while they worked to restore critical rotorcraft R&D to the NASA Aeronautics program. Rhett Flater played a major role in coordinating the many interfaces. When Hans Mark left DDR&E, he was replaced by Dr. Ronald Sega, a retired Air Force Major General and former astronaut, who became a key player in bringing the NFAC back online.

Meanwhile, back at Ames, the local community took action. Bill Warmbrodt rallied the troops to keep an eye on the NFAC, since abandoned facilities deteriorate and get cannibalized quickly in a research environment. Bill secured permission to move Tom Aiken into an office in the NFAC to keep an eye on things. Bill and Mark Betzina put together a team of knowledgeable folks under Tom Norman to assess the state of the facility equipment, identify critical storage and maintenance required to avoid deterioration, and start a plan for what would be needed to bring the facility back online. The team also looked seriously at the earlier operations model that had been in place to see if there were ways to streamline procedures and testing approaches to reduce the cost of operation. In the meantime, the DoD was assessing the situation.

The details of the DoD process is a story in itself, but a few high points are worth noting. I kept Dr. Tom Killion at DA apprised of the situation, but he warned that the Army would not be in a position to provide significant funds for the operation of such a major facility in the short- to mid-term. The OSD Office of the Director of Operational Test and Evaluation tasked the Institute for Defense Analysis to perform an in-depth study of the NFAC closure [4], including an assessment of alternatives if it was not feasible for NASA to operate the NFAC. The Ames community provided excellent support to the study team, including providing the findings of local planning efforts. The bottom-line findings of the study were that the facility was critical to the DoD and that there was a sufficient user community to sustain continued use under the funding rules for the DoD Major Range and Test Facilities Base. Under this arrangement, customers would be charged only the direct costs of using the test facilities.

The DoD determined that the NFAC should be assigned to the Air Force Arnold Engineering Development Command at Tullahoma, Tennessee. The Ames team worked with the Air Force to develop a plan for bringing the NFAC back online. The facility was to be run by an Air Force manager, with an Army Deputy provided by AFDD. Mark Betzina transferred from NASA to the Army, returning to the agency that first hired him to work at Ames, to fill that post as the NFAC was put back online and resumed testing.

Restoration of the NASA Rotorcraft program. In 2005, Dr. Lisa Porter became the NASA Associate Administrator for Aeronautics Research. In her previous position as a Defense Advanced Research Projects Agency (DARPA) senior scientist, she had been searching for the most technically challenging applications for advanced CFD and had initiated the Helicopter

Quieting Program (HQP). In surveying the aeronautics landscape, she discovered the technical challenges that had initially drawn me to rotorcraft. In the 1990s, NASA had also identified grand challenge problems to attack in its high performance computation initiative, one of which was the complex unsteady flow fields of rotary-wing aircraft. While Lisa was formulating the HQP for DARPA, she arrived unannounced at my office one afternoon. She said that she had spent the last few weeks researching the problem and talking to rotorcraft researchers. Several of them told her that she needed talk to me before she finished her planning. She told me what she had discovered, and her observations and assessments of the state of the art were surprisingly complete and insightful. I advised her that her research goals were optimistic, but agreed to provide her with technical support as needed. She proceeded with HQP, and some time later asked me for a reference as she applied for the NASA post. She was selected, and her exposure to the challenges of rotorcraft technology led her to advocate for, and oversee the restoration of, the NASA rotorcraft program with obvious benefits for continuation of the Army-NASA collaboration at Ames. On the minus side, she did not believe that human engineering research should be a major aeronautics initiative, resulting in a loss to a part of the AFDD program.

Final Thoughts

My career in rotorcraft technology coincided with the emergence of computers as the primary design tool for the industry. I was inspired by the vision that Hans Mark espoused for the development of “computation to flight,” and the goal of significantly reducing the need for major wind tunnel facilities and testing. We have made astounding progress toward that goal, but, especially for rotorcraft, we have not yet arrived.

My original career plans called for retiring when I was 60 years old and pursuing a career in music. In the year 2000, we were in a battle to sustain our quest for a better understanding of rotorcraft physics and engineering. Wayne Mosher was also preparing to retire in the near future, but he committed to remain at his post until I retired. In the beginning of 2007, my retirement criteria were met: a clear view that the Army’s rotorcraft program and partnership at Ames was on a secure path for at least 18 months. The NASA system required the restructuring of the Army interface with NASA, but the reinstatement of the NASA rotorcraft program and the working relationships forged over 40 years made continuation of our joint activities possible. A lot of the credit for arriving at this juncture goes to Wayne, who dealt with the myriad changes in local operational and administrative processes, and the steadfast support of Bill Warmbrodt. Barry Lakinsmith picked up the reigns with the help of Dick Spivey, who filled the AFDD Director’s position as an IPA. Through it all, NASA’s Jack Boyd was there for consultation and guidance.

I did learn another lesson. It turns out that you can keep making music effectively for many years as long as you keep doing it regularly, but after 60, hand-eye coordination and system bandwidth degrade at an alarming rate, and it is next to impossible to reoccupy the high ground to your own satisfaction. This severely impacted my post-retirement music performance plans. However, it was well worth the investment to be able to leave a functioning partnership in place to continue producing world-class results in support of one of the most fascinating challenges in aerospace research.

And, it isn’t over until it’s over.

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Index

A

Advanced Attack Helicopter (AAH), 6, 8
Advanced Systems Research and Analysis Office (ASRAO), 16
Advanced Systems Research Office (ASRO), 6-7, 9-13, 16
Advisory Group for Aerospace Research and Development (AGARD), 6, 11-13, 20
Aerodynamic and Acoustic Testing of Model Rotors (AATMR), 11
Aeroflightdynamics Directorate (AFDD), 13-20, 22-23
AH-56A Cheyenne, 3
Aiken, Ed, 18
Aiken, Tom, 22
Ambrose, James, 14
American Helicopter Society (AHS), 1, 9, 16-17
Ames Directorate, 7-9, 11-12
Army Advanced Aerial Fire Support System (AAFSS), 3
Army Aeronautical Research Laboratory (AARL), 4
Army Air Mobility Research and Development Laboratory (AMRDL), 3, 6-9
Army Aviation Labs (AVLABS), 1, 4
Army Aviation Research, Development, and Engineering Center (AVRDEC), 16-17
Army Composite Aircraft Program (CAP), 4
Army Research and Technology Activity (ARTA), 13-16
Army Research Office (ARO), 15, 17
Arnold Engineering Development Command, 22
Aviation and Missile Command (AMCOM), 17, 19
Aviation Research and Development Command (AVRADCOM), 11

B

Babb, Larry, 10
Ballard, Dick, 8, 11-12
Ballhaus, Bill, 14, 20
Base Relocation And Closure (BRAC) Act, 19
Betzina, Mark, 22
Boeing Vertol, 2, 11
Bousman, Bill, 12

Boyd, Jack, 20-21, 23

Bucher, Nancy, 12

Building N-207, 7

Building N-248, 19

C

Carlson, Richard, 3-4, 6-9, 14, 16

Chen, Bob, 18

Cheney, Kip, 2

Comprehensive Analytical Model of Rotorcraft Aerodynamics and Dynamics (CAMRAD), 10-11

Compton, Dale, 20

computational fluid dynamics (CFD), 19-20, 22

Computational Research and Engineering Acquisition Tools and Environments-Air Vehicles (CREATE-AV), 20

computational structural dynamics (CSD), 20

Computer Science Corporation (CSC), 10

concept of operations (CONOPS), 16-17

Contracting Officer's Technical Representative (COTR), 19

Crew Station Research and Development Facility (CSRDF), 14

Culver, Irv, 2

Curtiss, Howard C. (Pat), 1

D

Davis, John, 10-11, 17-18

Deckert, Wally, 4

Defense Advanced Research Projects Agency (DARPA), 22-23

Department of Defense (DoD), 14-16, 19-20, 22

Department of Energy, 21

Department of the Army (DA), 11, 15, 17, 20, 22

Director of Defense Research and Engineering (DDR&E), 21-22

Directorate for Advanced Systems (DAS), 16

Dix, Don, 12

Douglas, Frank, 10

Dunn, Dick, 7, 12

E

Eckert, Bill, 13, 17

Edenborough, H. Kipling, 1, 12

Elliott, Joe, 18
 Etter, Dolores, 12, 21

F
 Federal Aviation Administration (FAA), 2, 4, 17
 Flater, Rhett, 16-17, 22
 Forrestal Research Center, 1

G
 Georgia Institute of Technology, 15
 German Aerospace Research Establishment (DLR), 11
 Goldin, Daniel, 21
 Gorenberg, Norm, 2
 Gossett, Terry, 14

H
 Haller, Doug, 10
 Harris, Frank, 11
 Harris, Wesley, 16-17
 Hazen, David, 1
 Helicopter Quieting Program (HQP), 23
 Hickey, Dave, 4
 High-Performance Computing Modernization Program (HPCMP), 19-20
 Hiller Aircraft, 3-4
 Hindson, Bill, 18
 Hoffman, John (Jack), 5
 Hohenemser, Kurt, 2
 House, Tom, 8, 16-18
 HPC Institute for Advanced Rotorcraft Modeling and Simulation (HI-ARMS), 20
 Hutchins, Dale, 4, 12

I
 Independent Research and Development (IRAD), 17
 Institute for Defense Analysis, 22
 Interdepartmental Personnel Assignment (IPA), 20, 23

J
 Johns, John, 17
 Johnson, Kelly, 6
 Johnson, Wayne, 9-10
 Joint Strike Fighter, 21
 Joint Vertical Experimental (JVX), 14-15
 Jones, Lynda, 13

K
 Kerr, Francie, 2, 7
 Kesling, Paul, 6
 Key, Dave, 7, 13
 Killion, Tom, 22

L
 Lakinsmith, Barry, 18, 23
 Light Helicopter Experimental (LHX), 14
 Light Observation Helicopter (LOH), 2
 Lockheed, 2-6, 8-9

M
 MacDonald, Jeff, 9
 Maisel, Marty, 14, 19
 Maltby, Ralph, 12
 Mark, Hans, 21-23
 Massachusetts Institute of Technology (MIT), 9, 15-16

McCroskey, Jim, 19, 21
 McDonald, Henry (Harry), 19-20
 McDonnell Aircraft, 2
 Meakin, Bob, 19-20
 Memorandum of Understanding (MOU), 11-12
 Meyers, Alice, 13
 Morehouse, Gil, 10
 Morse, Andy, 4, 8, 13
 Mosher, Wayne, 16, 18, 23
 Munechika, Ken, 20

N
 NASTRAN, 10
 National Full-Scale Aerodynamics Complex (NFAC), 21-22
 National Rotorcraft Technology Center (NRTC), 16-18, 21-22
 Naval Air Development Center (NADC), 17
 Nikolsky, Alexander, 1
 Norman, Tom, 22

O
 Office of Secretary of Defense (OSD), 8, 12, 17, 21-22
 Ormiston, Bob, 9-11, 13, 18, 20

P
 Peterson, Vic, 20
 Porter, Lisa, 22
 Princeton University, 1-2, 9, 15
 Prouty, Ray, 2, 6
 Ragosta, Art, 10-11, 16
 Real, Jack, 3
 Rensselaer Polytechnic Institute, 15
 Research and Technology Laboratories (RTL), 8, 10-11, 13-14
 Revised and Extended Rotor Senior (REXOR), 4, 6, 8-10
 Robinson, Norm, 7
 Rosen, Cecil, 20
 Rotorcraft Centers of Excellence (RCOE), 15-17
 Rotorcraft Comprehensive Analysis System (RCAS), 11
 Rotorcraft Industry Technology Association (RITA), 17, 21-22
 Royal Aircraft Establishment (RAE), 12

S
 Schmitz, Fred, 3, 11, 13, 18
 Scully, Mike, 7, 11, 14, 16, 21
 Sega, Ronald, 22
 Sharpe, Dave, 4
 Shaw, Lorraine, 7
 Sikorsky, 2, 11
 Singleton, Robert, 15
 Singley, George, 16-18, 21
 Snyder, Tom, 15, 17
 Source Selection Evaluation Board, 8
 Spivey, Dick, 23
 Stanford University, 3, 9, 15
 Statler, Irv, 7-8, 11, 13-14, 20

Kerr, A.

Stephens, Wendell, 10-11, 18

Strawn, Roger, 19

Sullivan, Chris, 15, 19

T

The Technical Cooperation Program (TTCP), 11-13, 17

Tischler, Mark, 18

Tung, Chee, 18

U

United Technology Research Labs, 2

University of California, Los Angeles (UCLA), 9

University of Maryland, 15

University of Southern California (USC), 2-3

unmanned aerial vehicle (UAV), 8

Utility Tactical Transport Aircraft System (UTTAS), 4

V

Velkoff, Hank, 6-7

Vertical Motion Simulator (VMS), 14

vertical takeoff and landing (VTOL), 1, 3

W

Ward, John, 17

Warmbrodt, Bill, 18, 22-23

Wellesley College, 2

Whang, John, 7

Whitehead, Bob, 18-20

X

XC-142, 1

Y

Yaggy, Paul, 4, 6-9, 14

Yu, Yung, 17-18

Yuhas, John, 17

**Army Memoir:
Makings of a Helicopter Handling-Qualities Specification**

**David L. Key
Army**

Early Days

I always was interested in airplanes. As a boy, I made models. I could not afford kits, but I got plans and bits of balsa wood to shape and paint. After years as a Cub Scout and Boy Scout, in my mid-teens, I joined the Air Training Corps (ATC). This was a civilian organization, sponsored by the Royal Air Force (RAF), led by ex-Air Force officers. We met at my town's local airport, Southend, about 40 miles east of London. We were taught to march and do drills with WWI .303-caliber Lee Enfield rifles. They actually had us take them home at times, without the bolt of course. Rarely, we got to fire a few rounds at a shooting range. More rarely, they brought a plane to Southend, or we went to a RAF Station and got a 30-minute flight in an Avro Anson navigation trainer. The highlight of my ATC career was a week at RAF Station Hawkinge in the south of England. Along with about 20 local cadets, I got the chance to learn to fly a glider and got as far as three solos flights to qualify for a C certificate from the British Glider Association.



Dave Key, 1975.

I managed to get into the English equivalent of a local community college, Southend-On-Sea Municipal College, and after 2 years graduated, at age 18, with a technical qualification called an Ordinary National Diploma in Electrical Engineering. I might note that this college tuition did not cost me a penny; my parents just had to feed and clothe me.

I did have passing thoughts of joining the RAF and, at that time, there was a compulsory draft. Instead I managed to be accepted by the Hawker Aircraft Company for a 5-year Student Apprenticeship. This earned me a deferment, and the icing on the cake was that I went to classes full time, at the nearby Kingston Technical College, to work towards a Higher National Diploma (HND) in Mechanical Engineering. During vacation breaks I went back to the plant and gradually circulated through all the production and design areas. I remember being bored out of my mind working on the lathes and milling machines, and getting ringing ears from pneumatic rivet guns in the production area making Hunter jet-fighter parts. As the years passed, I got to do



Dave ready for his first solo flight.

more interesting things such as spending a summer at Royal Aircraft Establishment (RAE) Farnborough helping on a wind tunnel test of a P1127 vertical and/or short takeoff and landing (V/STOL) test model and eventually working in the Design Project Office. This P1127 design evolved into the Harrier and has seen over 50 years of service with the RAF and U.S. Marines, among others.

After two years I finished my HND, so the third year of my apprenticeship was in the plant full time. I took

Key, D.

some evening classes on aeronautical topics such as fluid dynamics and structures. My goal at that time was to extend my education by attending the prestigious postgraduate school, the College of Aeronautics, Cranfield. Entry was highly competitive, especially since it provided draft deferment to engineers finishing their schooling and apprenticeships. Hawkers sponsored me, and I was lucky enough to get a Society of British Aircraft Constructors scholarship for the 2-year course.

Cranfield was a marvelous experience, both academic and lifestyle. The college was located on an ex-RAF airfield out in the countryside about 11 miles from the nearest town, Bedford. Students lived in what had been officers' quarters—individual rooms in dormitory buildings. Meals were in the main building. It was quite formal: jacket and tie, and waitress service. This was the best life I had ever experienced. Not only were all my accommodation costs and tuition paid, but they were paying me a stipend that amounted to about twice what I would be earning as a 4th-year apprentice working back in the plant at Hawker Aircraft.

My year's intake (class) was about 105 students representing all the British aircraft companies, most United Kingdom (UK) engineering universities, Commonwealth countries such as Canada, Australia, and even the U.S. military.

I specialized in aerodynamics and propulsion, and I finally started to get some flight time. One optional course was Flight Test Techniques. This course involved tests such as determining aircraft lift-to-drag ratio. We were flown in a De Havilland Dove to take data. The Dove contained about five test consoles, and the flights typically lasted about 45 minutes. In fall and winter, Cranfield was nearly always overcast, often 100 percent. We would climb up through the clouds and take data in the usually sparkling clear blue sky above about 5,000 feet. Then came the bumpy flight back down through the clouds. Many days, lunch was ruined by such a morning exercise.

Flight time spent earning a private-pilot's license was a more enjoyable experience. Students, especially those specializing in aerodynamics, were given the opportunity, and if they showed some aptitude, were provided access to 50 hours of training to qualify for a British private-pilot's license. The trainer aircraft was a two-place tail-dragger called an Auster Aiglet, essentially identical to a Piper Cub. I managed to squeeze in the last of the 50 hours to qualify just before graduation ceremonies and departure.

My first flight in a helicopter was very short, but extremely memorable. During my first year, 1959, Britain was building its first freeway, the M1 motorway from London to the north. The project manager for this first section of about 50 miles used a helicopter to travel up and down the motorway. Since the motorway passed within about 2 miles of Cranfield, the helicopter was based there and the pilot often stayed in the Cranfield visitors' lodge. One of the students got to know the pilot and convinced him to give us a ride. The helicopter was a Bristol Sycamore leased from British European Airways (BEA). The pilot and a Cranfield Crew Chief took the front seats, and I and two other students climbed in the back. I was in the middle. We started to undo the neatly buckled seat belts, but the pilot advised us not to bother, as he planned only a short hop around the hangar and back. He started the engine, cranked up the rotor RPM, raised collective, lifted off, and put the nose down to initiate translation and climb. Unfortunately, the pitch down did not stop, so the nose wheel hit the ground and we wheel-barrowed for a bit. Then the helicopter rolled onto its left side and slid about 50 yards. The rotor blades disintegrated as

they hit the ground. The rotor stubs soon stopped thrashing and the engine stopped. Someone said, “Quick, get out in case of fire.” Being in the middle seat, I had nothing to hold on to, so when the nose wheel hit, I was thrown up and my head broke through the Plexiglas roof window. My head then bounced outside on the grass until we stopped. I was first out through the hole my head had made. Lucky for me we had started on grass, which, being spring in England, was thick and soft. I had minor cuts from the Plexiglas and some abrasions from the grass. There were no other injuries and no fire. In those days, the rotor blades were made of wooden ribs covered with plywood, and they shattered into hundreds of little pieces scattered all around. The steel blade stubs were bent back. No doubt the engine and transmission were damaged, and the tail boom was twisted and bent. The above photo of the wreck was taken the following day.



Dave survived the crash of the Bristol Sycamore.

There was no hearing, so we never got any explanation, but we heard that the helicopter was written off. A student friend worked for BEA and had access to the helicopter’s flight handbook. His hypothesis was that the pilot had forgotten to adjust the ballast to compensate for the forward shift of the center of gravity caused by adding three passengers. It was years before I had anything else to do with helicopters, and for months during flight training I had terrible feelings of panic when we made steep banked turns anywhere close to the ground.

Upon graduation from Cranfield with a Diploma of the College of Aeronautics, Hawkers offered me a job in their Project Office. For years, this had been my ultimate goal, but company retrenchments and industry consolidations, cancellation of major aircraft projects, and politicians predicting “the end of the manned fighter” gave me the impression that the British aircraft industry was not a good bet for a long-term career. So, I accepted an offer to work for Canadair in Montreal, Canada. My new bride, Pamela, and I flew to Montreal in August 1960.

Late-August weather in Montreal was hot. I was placed in an office without air conditioning. A few times, the temperature reached 90 degrees and we were sent home. Winter was something different: cold, snow, and freezing rain. It was consistently below freezing for weeks at a time, and they had to truck snow off the streets and dump it in the St. Lawrence River. England is not exactly known for good weather, but these extremes were a shock; I thought only Eskimos lived in such conditions. I got used to the extreme weather and enjoyed life in Canada, but, after a couple of years, the future of Canada’s aerospace industry seemed no more prosperous than Britain’s, and the U.S. beckoned. So, in November 1962, we moved to New York, and I went to work for Bell Aerosystems in Niagara Falls.

I learned a lot at Bell. Here I had my first introduction to using analog systems to represent math models, coupling them with a cockpit, and working with test pilots for handling-qualities evaluations. First, we did evaluations of the Lunar Landing Research Vehicle being built by Bell for NASA; this was relatively simple. Math model development and implementation for the X-22A was much more complex. The X-22A was a research V/STOL aircraft with four tilting ducted propellers being built for the Navy and required a 6-degree-of-freedom simulation, with cockpit and controls, to investigate handling qualities over the full flight range of the X-22A.

Key, D.

Bell was good to work for, but they had no prospects beyond the X-22A and were unlikely to even be given the job of exploiting it in a flight-research program. Cornell Aeronautical Laboratory (CAL), which was designing and building the variable stability system for the aircraft, was much more likely.

I had been looking around for a more stable job in, hopefully, a better climate, and was on the verge of heading out to Boeing, Seattle. By chance I had a phone conversation with a co-worker who had recently moved from Bell to CAL. A short while later, I got a call from his boss, John Schuler, Section Head in the CAL Flight Research Department. He wondered if I might like to work for them and if I could meet with him that Saturday. Being only a 20-mile drive away, I had nothing to lose and was curious about CAL. I knew little about them except that their reputation was excellent, so I accepted the visit. John described their organization and typical research programs, and showed me around the hangar and inside their variable stability aircraft, including two B-26s and a T-33. Needless to say, I was enchanted by this organization and decided that being part of a small research group was more attractive than getting lost in a big organization like Boeing, no matter how prosperous a company, and despite the attractions of Seattle over Buffalo.

The CAL Flight Research Department was full of handling-qualities experts, both research engineers and test pilots. Notable were researchers such as Charles Chalk, primary author of updates to the fixed-wing flying-qualities specification MIL-F-8785, and engineer-test pilots such as Robert Harper who was co-author with George Cooper, NASA Ames, of the Cooper-Harper Handling Qualities Rating Scale.

My first big job assignment was to develop detailed documentation of the T-33 aircraft's variable stability system. Subsequently, I got involved in research projects such as using the T-33 to investigate M-2 Lifting Body landing characteristics and the B-26 to study approach and landing a very large aircraft, specifically the C5A. Building on my work at Bell, I generated a math model of the X-22A for use by CAL in the variable-stability-system design work. This X-22A background was critical in two subsequent projects: first, a 3-year project for the U.S. Air Force to develop flying-qualities criteria for V/STOL aircraft (this became MIL-F-83300), and second, a study for the Federal Aviation Administration (FAA) to generate certification standards for V/STOL aircraft. The FAA's intent was to form a basis in the event that V/STOL candidates applied for certification. The final report was published as the *Yellow Book*. Both of these reports were a blend of the current helicopter specs, MIL-H-8501, for hover and low-speed flight, and MIL-F-8785 for forward flight. Some V/STOL hover and low-speed data were used to generate new concepts for transition.

It was obvious that the helicopter spec MIL-H-8501—a 1961 revision of a 1952 document—was naive and badly in need of updating. It was being used as the basis for specifying handling qualities in U.S. Army and Navy helicopter procurements and formed the essence of FAA helicopter-certification standards. I think the authorities were aware of the limitations, but, unfortunately, there were no data on which new criteria could be based. No one had done, or was doing, systematic research on handling qualities for helicopters.

While at CAL, they paid my tuition for part-time study at the State University of New York, Buffalo. I got an M.S. degree in Mechanical Engineering Systems in June 1969. At about the same time, I became a U.S. citizen. Over 9 years at CAL, I evolved from an aeronautical

engineer into a Section Head leading a group of about a dozen engineer scientists. Being heavily involved in handling-qualities research, it seemed appropriate to get more flight experience to increase my ability to communicate with test pilots. So, I joined the CAL Flying Club and reactivated my pilot skills by going through the FAA private-pilot training regime. Flying out of Buffalo Airport was a bit different from the relaxed operations at Cranfield where we took off and landed on grass and did not even use radios.

After about 8 years, things started to change at CAL. The owners, Cornell University, wanted money instead of a research facility. CAL was 100 miles away from the university campus and had little interaction with, or benefit to, the university. Sales efforts by the university and resistance by the staff led to much turbulence and bad feelings. The head of the CAL Applied Mechanics Department, Dr. Irving Statler, (along with most of the charter members of CAL) was an outspoken critic of the university's efforts to sell to a profit-making organization. Curtiss-Wright had transferred its research laboratory to Cornell University at the end of WWII, with the agreement that it would be operated as a not-for-profit organization for the public good. Dr. Statler and many others were "encouraged" to leave. Dr. Statler went to work for the U.S. Army Air Mobility Research and Development Laboratory (AMRDL) in California. His new organization needed new talent and, fortunately, he remembered me. He got me a job offer and I accepted.

In the middle of August 1973, together with my wife, 10-year-old son, and 8-year-old daughter, I set off for California in our 1969 Plymouth Barracuda. It was loaded to the gills with clothes and tent camping equipment, and, surprisingly, we made it to Sunnyvale on schedule with no emergencies.

U.S. Army Air Mobility R&D Laboratory

I started work as a Stability and Control and Flying Qualities Specialist in the Advanced Systems Research Office (ASRO) in the AMRDL Headquarters at Ames Research Center, Moffett Field, California, in September 1973. My job description was: *Assess Army stability and control and flying qualities needs, identify R&D shortfall and recommend a program. Prepare long-range plans and estimate resources. Provide specialist advice to other parts of the Army as required. Undertake some participation in Army-related research programs at NASA Ames Research Center.*

There were four Directorates reporting to the AMRDL Headquarters. Three were located at NASA research centers: The Ames Directorate at NASA Ames specializing in aeromechanics; the Langley Directorate at NASA Langley specializing in structures and materials; and the Lewis Directorate at NASA Lewis (now Glenn) specializing in propulsion. The fourth was the Eustis Directorate located in Ft. Eustis, Virginia, whose responsibilities were technology development and implementation.

Paul Yaggy was the Director of AMRDL. His deputy was an active-duty Colonel, Norman Robinson. Dr. Richard Carlson was the ASRO Head, supervising specialists like me. I had hardly gotten to know Paul Yaggy when he retired, and Dick Carlson took over as Director. My recollection is that Fred Immen, the structures technology specialist, took over as the lead of ASRO.

Key, D.

Other specialists in the ASRO were Richard Dunn, Human Factors; Terry Gossett, Electronics and Systems; Wayne Mosher, Foreign Technology; and Mike Scully, System Design. Shortly after I arrived, Andy Kerr was hired to be the specialist for Aeromechanics.

I spent a year getting to know the Army R&D people, particularly in the other AMRDL Directorates. I visited the Eustis and the Langley Directorates. I wrote a report documenting my vision of the needs in my area of technical responsibility [1]. Because of my background, it emphasized handling qualities and flight control. I did not see a capability within the AMRDL to address control-system hardware, and it would take a major infusion of resources to generate people and facilities to catch up with industry. Reviewing the specifications for the new helicopter programs for the Utility Tactical Transport Aircraft System (eventually the UH-60 Black Hawk) and the Advanced Attack Helicopter (eventually the AH-63 Apache), it was clear that they still relied extensively on MIL-H-8501 for handling-qualities specifications. From my work at CAL, I knew that MIL-H-8501 was sadly lacking, so an update was a major ingredient of my proposals.

After a year developing great ideas, I was challenged by Dr. Statler, by then the Director of the Ames Directorate, to transfer to his organization and work with the Army and NASA to actually accomplish some of the objectives I had advocated in my report.

The Ames Directorate included a group of Army engineers, scientists, and technicians located in Building N-215 that NASA had assigned with office space and a wind tunnel. This group worked under Army supervisors. However, the Ames Directorate also had at least as many other Army employees assigned to various NASA organizations. Under the terms of the Army-NASA Joint Agreement, some of these employees were assigned to support elements such as personnel management and contract management to help compensate for the additional administrative workload the Army imposed on NASA Ames. Many were engineers and scientists assigned to participate seamlessly with NASA personnel conducting research of mutual interest to the Army and to NASA.

With this background understanding, in 1974 I transferred from ASRO and moved across the road from Building N-207 into Building N-215 as a member of the Ames Directorate. I forget whose office I shared. All the engineers there were rotor dynamics or aerodynamics specialists—great people but none talked handling qualities—so there was no one to collaborate with. This was not a venue with access to the expertise and facilities needed for the project I proposed; those were in NASA's Flight Dynamics and Controls Branch.

I learned afterward that Dr. Statler was having trouble getting key NASA people, including the Chief of that Branch, Maurice D. White, to agree to undertake a flying- and handling-qualities study of helicopters. They were reluctant to divert resources from their successful work on V/STOL aircraft, and they did not want to stir up a competition with NASA Langley. Dr. Statler prevailed with the help of the Ames Center Director, and my proposed project found a tentative home in NASA's Flight Dynamics and Controls Branch. In 1975, almost 2 years after I arrived at Ames, I finally moved into Building N-210 as a member of that branch to work on the project I had been advocating to the Army, in accordance with my Army job description, but under a NASA supervisor. Fortunately, Maurie White, and later Dr. Jack Franklin, who succeeded Maurie, were supportive of my Army-related endeavors and left me alone.

In the Flight Dynamics and Controls Branch, I worked closely with Lloyd Corliss and Peter Talbot, two experienced Army engineer-scientists who had been participating in NASA's V/STOL research activities. Lloyd had worked on several vertical takeoff and landing (VTOL) programs studying hover and low-speed handling qualities. He had done flight testing with the Bell X-14A and ground-based simulations with the NASA 6-degree-of-freedom simulator. See Lloyd's chapter. Well, Army aviation means helicopters, not VTOL aircraft, so the first order of business was to get some piloted simulation capability for helicopters, initially ground based, but hopefully an in-flight capability too.

NASA had several fixed- and moving-base simulators, and it was not difficult to get a cockpit set up with rudimentary helicopter controls. However, the visual displays were narrow field of view and could only show fixed-wing takeoff and landing tasks, and we did not have any helicopter representative math models. Dr. Statler had sent me off with a small budget, so our first contract was awarded to Systems Technology, Inc. (STI) for Bob Heffley to develop a simple generic helicopter math model and a set of aerodynamic derivatives for a range of helicopters including the UH-1, OH-6, and Bo-105. Also, Peter Talbot started developing and programming a math model.

An accepted definition of "handling qualities" is that they comprise all the aircraft characteristics that govern the ease and precision with which a pilot is able to perform the tasks in support of an aircraft role. To get a fix on just what tasks the Army was expecting to do with its helicopters, Lloyd Corliss and I went to the Center for Army Aviation at Ft. Rucker, Alabama. We got to fly on training missions with an instructor pilot (IP) and two student pilots (SP's), one flying, and the other navigating. For miles around Ft. Rucker, the terrain is almost flat, thickly wooded, with few identifiable features. How these Army pilots could find their way around with only a paper map and a compass, flying just above the treetops, was a miracle to Lloyd and me. For example, because of the dense trees, even streams marked on the map were invisible until we were right overhead. The pilots learned to look for a slight change in the green tint that followed along the stream bed. Flight tasks we observed included translating at speeds from just above hover up to maximum speed (as low as a few feet above the treetops), decelerating descents to hover in constrained clearings, mask-unmasking bob-up tasks among the trees, and sideways translation while masked to bob up at another location.

These tasks were the essence of the current doctrine, called nap-of-the-earth (NOE) flying. NOE evolved from lessons learned in Vietnam. Helicopters epitomized by the UH-1 Huey and AH-1 Cobra generated a "flop-flop" sound that was directional and could be heard long before the helicopter arrived. If they flew at a comfortable height, like 500 feet, they were sitting ducks for ground weapons. So, they learned to fly almost touching the treetops. Then, even though they could still be heard well ahead of arrival, they could not be seen until they were almost overhead, and were very quickly, safely out of sight again.

To get another perspective on NOE flying, I attended a 3-week Tactical Training course for Army Aviators at the Yakima Proving Grounds, Washington. This was very mountainous terrain overlooking the Columbia River. Like Ft. Rucker, the team was in a UH-1, two SPs with an IP. This time the SP's were experienced Warrant Officers. I sat looking over their shoulders from the back. We would climb up a ridge and, before cresting, the IP would say, "hold there," and discuss the details of the maneuver as we went over the top and down the other side. "Watch out for gusty crosswinds at the top. Do not initiate the descent too quickly or you could get mast

bumping. Stay close to the surface, but be careful not to drag the tail as you flare to decelerate on the down side.” During this instruction, we were hovering. With the high-power setting, the engine would occasionally make coughing noises like it wanted to quit. If it had quit, we would have crashed and rolled down the steep cliff side for thousands of feet. Eventually, going over the top was a relief followed by an exhilarating ride down the other side. Even in the lower reaches, there were few places flat and level enough to accommodate a forced landing. Back in the classroom, I heard stories of three precautionary landings during the first 2 weeks. As luck would have it, they all found a safe place to land. The pilots talked about how, if forced down in a confined draw or gully, they would roll right to make sure it was the advancing rotor blade that hit ground first and thus drive the transmission housing back, not forward where it would crush the pilots. Unlike the Army aviators, I could quit any time, and I did so after 2 of the 3 weeks, before my luck ran out.

Based on all of this, we realized that Army handling-qualities-research objectives would require us to develop the capability to simulate helicopters performing NOE tasks such as very low point-to-point flying, flight into confined areas, vertical unmasking from behind a line of trees, and side steps while masked.

Ground-Based Simulation at Ames

At that time, NASA Ames had a Simulation Sciences Division that could perform state-of-the-art handling-qualities simulations of fixed-wing aircraft. The Flight Simulator for Advanced Aircraft (FSAA) was the best. It was an element in a complex system that provided motion, out the window and head-up displays, and sound cues to the pilot. Cockpits could be reconfigured to provide a representation of the essential flight instruments and appropriate force-deflection characteristics of the flight controls. Teams of research scientists provided support for programming the desired aircraft math models for real-time operation on the most up-to-date digital computers. Extensive recording systems for model checkout and data recording were available. The extensive setup and checkout could take 6 months or more from the time a researcher first met with the simulation sciences personnel to discuss their test plans, to their actual time on the facility for checkout and data taking. The data-taking time was typically 4 to 6 weeks, and upwards of 10 to 12 simulation projects could be supported during a year. Two-shift operation was possible and, with compatible cockpits, two projects could share a day.

The FSAA motion system provided angular displacements of ± 20 degree pitch, ± 40 degree roll, and ± 25 degree heading. Translational limits were ± 3.5 feet longitudinal, ± 40 feet lateral, and ± 4.2 feet heave. The impressive lateral displacement maximized cueing fidelity in coordinated banked turns; a dominant characteristic at fixed-wing aircraft flight speeds. At low speeds, such as in a helicopter in and around hover, the other translational degrees of freedom need large cueing motions, and those displacements were modest.

The FSAA out-the-window scene was presented by a 25-inch cathode ray tube television (TV). To get maximum field of view, it had to be quite close to the pilot (30 inches) so it was viewed through a collimating lens that gave the appearance of images at infinity. Field of view was about 36 degrees vertically and 48 degrees horizontally. This is a tiny fraction of what a helicopter pilot has and needs, especially near hover. The FSAA out-the-window scene was generated by “flying” a TV camera over a terrain model. The model had an airfield with runway, buildings, fields, town, etc. It was about 72 feet by 15 feet in size, and the scale was about 600

to 1. This allowed simulation of a reasonable approach, landing, and circuit for a 2-mile runway with a 2-mile approach and 1-mile climb out. The TV camera looked through a periscope or “probe” that could get close to the surface, but it was expensive and fatally damaged if it hit the “ground.” Software stopped it at about 1/4 inch, and a “whisker” gave a physical limit signal at about 1/8 inch. (On flat terrain this worked well, but with some hills and trees on our future-terrain board we frequently broke the expensive, long-delivery-time probes.) At 600:1 scale, 1/4 inch clearance means about 12 feet minimum height. Not a bad eye level at landing for a fixed-wing aircraft, but a bit high for most helicopters. Also, the scaled trees were too tiny to fly behind and between, as into clearings. Clearly, we needed a special piece of terrain modeled for NOE tasks. A larger-scale terrain model was the easiest problem to fix, and Lloyd and I found a model maker somewhere in Mountain View who could make us a suitable model. At a scale something like 100:1, an area 5 feet by 10 feet could contain a heli-pad, some woods with a clearing to do descents, and bob-ups and masked translation. The Army paid for the new terrain board, and NASA covered the costs of its installation and implementation.

Peter Talbot’s generic math model, roughly representative of a UH-1, was programmed for real-time operation, and NASA set us up with a helicopter cockpit. Later we were allocated sessions on the FSAA. We were soon doing some NOE flying tasks and starting to investigate limits on longitudinal dynamics, engine response, height damping, and ground effect.

The need to address helicopters and their idiosyncrasies specifically, not as generic V/STOLs, is discussed in reference [2]. For this memoir I will briefly illustrate some of the factors. First, the tasks are quite different: the V/STOL basically lifts off and moves away in a climbing acceleration, or slows down and descends to land. The Army helicopter does that, but may have to stay in hover while it positions a slung load, or do a bob-up from behind trees to observe or fire at a target, and then descend back into cover, or it may have to reposition by sliding laterally while remaining hidden below the tree tops. Obviously, the different tasks demand large differences in agility and maneuverability. Second, the vehicle parameters of interest can be quite different. Helicopter aerodynamics, coupled with rotor and engine dynamics, are much more complex and involve quite different parameters from typical V/STOL aircraft. Take the simple height management task that is an ingredient of both vehicles’ tasks. Basic parameters are the available thrust margin, T/W , the thrust response rate, and the aerodynamic heave damping, Z_w . Thrust margin is critical because it limits payload, so the minimums must be determined accurately. Faster thrust response rate and more heave damping (we now know) allow lower thrust margins. On a jet V/STOL such as the X-14 or Harrier, thrust response rate depends almost entirely on the engine response, that is, how quickly the engine can increase thrust on demand. This thrust response can be characterized quite well by a first-order response, τ . Jet engines are a bit slower than pilots would like, and to make matters worse, jet-lift configurations tend to have low values of heave damping, and not much help from ground effect (sometimes they even get suck-down). This is a critical area for V/STOLs, so lots of studies had looked at the interaction of T/W , τ , and Z_w . Helicopters are quite different. They have much higher levels of heave damping, considerable ground effect (usually helpful), and their thrust response is initially dominated by the rotor blade inertia. The pilot increases thrust by raising the collective control, which increases blade angle and gives almost instantaneous thrust increase. However the increased thrust puts more rotor torque load on the engine so RPM slows (droops), and the thrust increase bleeds off. The engine governor has to sense this droop, and open the throttle to develop compensating torque and bring the RPM and thrust back up. Bell single-rotor helicopters have

relatively high inertia; others such as the OH-6 have much lower inertia. Engine governor characteristics have a big influence on the resultant thrust, and at that time governors were beginning to evolve from mechanical-analog to digital to improve performance. This topic of height management got a lot of our early attention.

With this work, we had started to scratch the surface looking at helicopter handling qualities, but it was clear we needed better simulation capabilities. Obviously, the Army would never invest enough money, let alone personnel, to develop capabilities comparable with the NASA Ames facility. The only realistic approach would be to supplement what Ames had. So, we developed a concept to get Army money to purchase new, more capable hardware, especially tailored for helicopter research, and get NASA to incorporate it into their facilities, and let them operate it to the benefit of all users. This idea was accepted by my boss and subsequently by Ames management, so I got the job of developing specifications and preparing a briefing to sell it to the higher levels of the Army. I got a lot of help from the NASA team, and especially, through a contract, from John Sinacori, a very gifted engineer in this subject [3].

The general arrangement proposed was to generate motion by six hydraulic ramps (a hexapod). This gave about ± 3 feet of travel in heave, longitudinal, and lateral directions, and about ± 25 degrees in roll, pitch, and yaw angles. These displacements were available when displaced individually but unfortunately were drastically reduced when multiple axes were displaced at the same time. The hexapod was to carry a dome that surrounds the cockpit. The dome was to provide a wide field-of-view screen with imagery projected on the inside. The imagery was to be generated by the new computer-generated-technology methods, not from a camera-model system and, although still providing only a relatively small field of view, the image field would be helmet tracked and moved to follow the direction that the pilot was looking.



Dave explaining simulator concept to Major General Hinrichs, Commander AVSCOM, 1975.

The proposal was approved and not only did we get funds, (about \$20M, I think) but Dr. Statler convinced the Army to assign a full Colonel to his Directorate to be the project manager of the Advanced Research Systems Integration Simulator (ARSIS). The first incumbent project manager was Colonel Billy Odneal. He lasted about a year and was replaced by Colonel Arlin Deel. Over several years, much work was done on the project, but it never became operational as a complete package. Instead, as a viable alternative, some of the components that were developed for ARSIS were eventually incorporated into an upgrade of NASA's Vertical Motion Simulator (VMS). For the VMS, the ± 40 feet of lateral travel on the FSAA was reduced to ± 20 feet, but the vertical travel was increased to ± 30 feet and it provided ± 4 feet of longitudinal displacement. However, it had only modest capability for rotational degrees of freedom. The six-leg hexapod developed for ARSIS was used to improve the angular degrees of freedom, and large rotational angles became usable simultaneously. Several interchangeable cabs were developed so that cockpits could be configured off-line and, when ready, quickly installed on the motion base. The visual display area was increased by feeding imagery to multiple windows, and

image generation was improved by changing over to the rapidly improving, computer-generated imagery. Simulations on the VMS subsequently ranged from helicopters and V/STOLs, blimps, and commercial and military aircraft, to the Space Shuttle.

For many years, we got at least two entries per year on the VMS to address our latest helicopter handling-qualities topic at very minimal cost to the Army [4]. From my viewpoint, this frequent, low-cost, ready access to the VMS was the most valuable benefit of the Army-NASA Joint Agreement. It allowed us, at relatively short notice, to research a handling-qualities topic systematically on a high-fidelity facility, safely and comprehensively. Then, with the knowledge that those results gave us, we could define a focused, but inherently more limited, follow-up in flight test, as I will discuss later.

We continued to build on our in-house capabilities to do helicopter handling-qualities research but needed more skilled people. One source was my alma mater, CAL; by this time they were under new management and called Calspan. The first person I managed to get to join us was Robert Chen. I think he was hired in a NASA slot, but he did work on one of our important topics, math modeling and parameter identification. Later Ed Aiken and Vic Lebacqz joined us (see their chapters in this book).

In the photo below, I have identified Army employees and indicated with italics NASA employees who, to the best of my recollection, were working on helicopter-related projects.



NASA Flight Dynamics and Controls Branch (Oct. 1977). Standing: Elwood Stewart, Vern Merrick, Leo Power, Bob Kuhlman, Tom Carson, John Davis (Army), Bob Behens, Peter Talbot (Army), Luigi Cicolani, Alan Smith, Bob Jacobsen, *Bill Decker*, Barbara Short, Major Bill Brigadier (Army), Dick Greif, Katsuyawa Miyajima, Charlie Hynes, Emmett Fry, Jack Franklin, David Key (Army), Gerd Kanning, Linda Vollenweider. Kneeling: Brian Doolin, Bruce Tinling, Walt McNeill, Bob Sammonds, Jack Stevenson, *Bob Chen*, and Lloyd Corliss (Army).

Reorganization of the Aviation Systems Command

When I joined the Army organization at Ames in September 1973, we were called the Army Air Mobility Research and Development Laboratory (AMRDL) with four Directorates, and we reported to the Army Aviation Systems Command (AVSCOM). On July 1, 1977, AVSCOM was reorganized and its R&D mission was assigned to a new Army Aviation Research and Development Command (AVRADCOC) with Major General Story C. Stevens commanding. AMRDL became the Research and Technology Laboratories reporting to AVRADCOC. Each of the former four Directorates became a "Laboratory." The Ames Directorate became the Aeromechanics Laboratory. Dr. Statler reorganized the Aeromechanics Laboratory and established Divisions for Fluid Mechanics, Rotorcraft Dynamics, Flight Control, and a Support Division. In December 1978, I was formally appointed Chief of the Flight Control Division of the Aeromechanics Laboratory. I moved out of the NASA Branch in Building N-210 back into the Aeromechanics Laboratory in Building N-215. Bob Ormiston and I had offices at the end of the building opposite the offices of the Director (Dr. Statler) and the Fluid Mechanics Division Chief (Andy Morse), and we each had a secretary in the office between us. See the chapter by Lori Blanken, Bob Ormiston's secretary. I might note that when ARSIS project manager, Colonel Billy Odneal, first arrived, he shared my office and secretary. That was something of a comedown for a full bird Colonel but Colonel Deel fixed that. Personnel assigned to the Flight Control Division, and their NASA locations were:

- Division Secretary (not NASA located), Pamela Baca
- Flight Dynamics and Controls Branch, Jack Franklin (Chief), Edwin Aiken, Dean Carico, Lloyd Corliss, and John Davis
- Tiltrotor Office, John Magee (Chief), Gary Churchill, and Laurel (Shorty) Schroers
- Simulation Sciences Division, Various Branches, Donald Billings, George Danek, Richard McFarland, and Larry Webster
- Human Factors Branch, Ed Huff (Chief), Earl (Jim) Hartzell, and Robert Wright
- Flight Test Department, Fred Drinkwater (Chief), Major Robert Merrill, Major John Henderson, and Stephen Mathews
- Contracts Department, Roberta Booth

I still have photos of some of these initial Flight Control Division personnel, unfortunately not all. The photos below were taken by NASA photographers at award or promotion ceremonies during the period from 1978 to 1987.



Pam Baca (1978).



Ed Aiken (1978).



Gary Churchill (1987).



George Danek (1978).



Richard McFarland (1978).



Jim Hartzell (1982) .



Shorty Schroers (1985).



Bob Wright (1978).

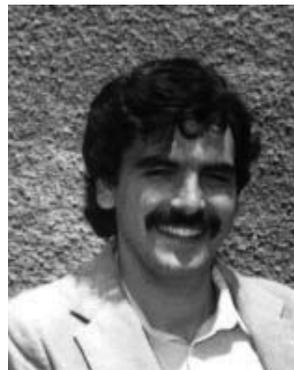
Chris Blanken, Hossein Mansur, Mark Tischler, and Matthew Whalley were not on the 1977 list of division personnel, but joined us by about 1985. All subsequently made important contributions to our efforts that I want to recognize. See the chapters by Chris Blanken and Mark Tischler for details. The photos of these distinguished members of the Flight Control Division are shown below.



Chris Blanken (1985).



Hossein Mansur (1987).



Mark Tischler (1986).



Matthew Whalley (1987).

From the division personnel assignment list, you can see that Peter Talbot had already left the Army for a NASA slot. This happened a lot. It was not long before Lloyd Corliss followed. I might have been a tough taskmaster, but there was no doubt that NASA offered more career opportunities and, in particular, high, G-14 grades were easier to get than with the Army. I must admit I also applied several times for NASA management positions, but was never successful.

While on the subject of personnel and organizations, I should point out that, at that time, all the people supposedly in my division were actually assigned to a NASA branch under the supervision of a NASA branch chief. This apparently worked well in all the service branches, and even the Tiltrotor Office, but was less convenient in the technical area I was directly leading. Branch Chiefs Jack Franklin and later Vic Lebacqz were always supportive, but my lack of direct supervision and control made it difficult for me to keep research projects precisely focused on my vision of the spec database needs, and to get support in responding to Army requests for

technical help or budget planning and briefing exercises. In later years, I moved again, out of the Aeromechanics Lab building and into a row of offices in Building N-211. There I did have some engineers working directly under my supervision, and it made a big difference. The first of these engineers was Chris Blanken who joined my division in 1980. In subsequent years he not only ran many simulation projects, but he also became my invaluable right-hand man for the spec development work. When I retired in 2000, he took the lead in answering questions from industry and government test agencies, and he led developments to further refine an Aeronautical Design Standard (ADS), specifically ADS-33E-PRF. See Chris Blanken's chapter.

In-Flight Simulators

Ground-based simulation is an efficient, safe environment to make systematic variations in vehicle and task parameters. Pilot ratings of such configurations give data that can be used to define boundaries of acceptability. Unfortunately, although trends are usually accurate, the absolute values of limits must be treated with some skepticism. Many aspects of simulation can introduce differences from the real world. The math model may not accurately represent the intended helicopter, and its implementation on digital computers for real-time operation introduces time delays that, in the early days, were significant, and usually required extensive simplifications. The motion system provides cues that are basically washed-out initial accelerations, hopefully at low, imperceptible thresholds. Big motion travel helps minimize these miscues, but extraneous dynamics are introduced. The visual cues, even in later years with advanced computer-generated imagery, had modest resolution, lacked fine detail, and had restricted field of view, so, in effect, we were always flying in a Degraded Visual Environment (DVE).

Our goal was always to get an in-flight simulator, which I will interchangeably refer to as a variable-stability helicopter (VSH). Ideally, a VSH would remove most of the ground-based simulator limitations even though it introduces some limitations of its own. First, it may not be possible to match the desired evaluation configuration because of VSH control-system limitations: control power, system stability (gain limits), and of course, no ability to modify side-force characteristics. Perhaps the biggest limitation of most VSH, though, is the inability to fly very close to the ground because of safety concerns. Unless the system providing the variable stability has limited authority actuators, which inherently limits what can be done, or is a multi-redundant system, which would be very complex, expensive, and difficult to set up, a "hard over" in an actuator while flying close to the ground could be catastrophic. Even at altitude, a "hard over" could cause an unrecoverable upset, or in a teetering-rotor helicopter such as a UH-1, could result in mast bumping. Despite the difficulties, the possible advantages were such that we spent considerable effort trying to get a capable VSH at Ames.

The earliest possible candidate was an on-going program in the NASA Navigation Systems Branch. The program, called V/STOLAND, was to develop a system providing advanced control stabilization with coupled navigation and versatile flight-director displays. The intent was to investigate various V/STOL decelerating, descending approaches to a terminal area. The system was built by Sperry, and installed in a UH-1. We monitored the activity for handling-qualities potential, but the control system did not have the needed flexibility or dynamic response range to address our interest in visual maneuvering tasks close to the ground.

In 1976, when the designation of lead center for rotorcraft technology was transferred from Langley to Ames, five research helicopters were transferred. One of these helicopters was a CH-47B Chinook. I remember that NASA engineer and flight-test pilot Bill Hindson did a fantastic job, leading efforts for what seemed like years, to make a usable VSH out of it. Primary obstacles were getting adequate instrumentation installed and demonstrating to the NASA flight-safety authority that it could be operated safely reasonably close to the ground. Indeed, it did eventually get used in some experiments. I think one topic was to investigate functionality and force cueing in multi-axis side-stick controllers. Again, my memory is vague, but I think it was not operational very long before being called back by the Army and returned to Boeing where it was upgraded to a D model and put back in the active fleet.

Eventually we did get a UH-60 Blackhawk assigned to the Army at Ames, and sufficient funds to pay Boeing for actuators and instrumentation to make a competent VSH. The contract was signed in 1993. The system was installed and was flying at Ames by 1997. See Ed Aiken's chapter. However, the Rotorcraft Aircrew Systems Concepts Airborne Laboratory (RASCAL) had not done much for our handling-qualities database by the time I retired in 2000.

We had access to highly qualified NASA test pilots like George Tucker, Ron Gerdes, and the aforementioned Bill Hindson to perform our in-flight simulation investigations. We also had at least one, often two, Army test pilot graduates of the Navy Test Pilot School (NTPS) assigned for a period to the Army at Ames. At the time my division was formalized, the Army pilots were Majors Robert Merrill and John Henderson. But we did not have a capable VSH when we needed it in the late 1970s and early 1980s. We sought out collaborations to fill our need.

Flight-Test Community: Collaboration and Coordination

The Army Aviation Engineering Flight Activity (AEFA), at that time located at Edwards Air Force Base (EAFB), was a critical organization we had to work with. Their responsibility was basic engineering flight test. They had to assess if new helicopters, or helicopters having significant modifications, had safe flying qualities and were mission capable with their installed systems. Assessments were based on measuring how well the helicopter met the standards defined in the system specification, plus subjective handling-quality assessments by the test pilots. They had frequently documented conflicts between pilots' opinions and the system spec handling-qualities criteria, and were receptive to our ideas. We went to EAFB, briefed the flight-test engineers and pilots, and got them involved in our simulator evaluations. AEFA routinely sent a pilot and a test engineer to Ames to participate in the evaluations.

It was most important to get AEFA involved in setting the performance standards in our flight-test maneuvers. We got funds to sponsor them to run complete evaluations of the Apache and Chinook using our new spec [5, 6]. The test results provided valuable guidance in setting aggressive boundaries for some of the mission tasks. In addition, they uncovered new shortcomings and helped identify the sources of shortcomings that had been noticed by the pilots in previous tests.

Another organization that had to be converted to using our new spec was the NTPS. This school had been teaching test pilots how to do handling-qualities evaluations for many years. They were the experts, but more importantly for us, they would be teaching the future test pilots, and we needed them to be teaching our up-to-date specification. So, as with AEFA, early on we briefed them about our ideas and got them involved in the ground-based and in-flight simulations. We

Key, D.

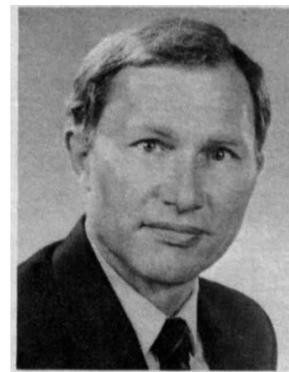
gave them lectures on how to test for the new criteria, and they particularly liked the Flight Test Guide that we eventually formalized [7]. The test-pilot students and most of the lecturers were military, and moved through after a few years. However, there was a core of civilian lecturers who were there for the long haul. They greatly helped develop the necessary assimilation, and, though their names have faded from my memory, I am very grateful for their collaboration. Through their efforts, ADS-33 is now an integral part of their curricula [8].

In 1979, Dr. Statler and his German contact on the Advisory Group for Aerospace Research and Development (AGARD) Flight Mechanics Panel, Dr. Peter Hamel, got approval for a mechanism to collaborate on helicopter research topics: the German *Memorandum of Understanding (MOU) for Cooperative Research on Rotorcraft Flight Control and Aeromechanics*. The rationale was to bring together motivated scientists and engineers to use complementary research facilities, share individual resources, and elucidate corresponding research perspectives. The U.S. participants were the U.S. Department of the Army and the Army Materiel Command through the Aeromechanics Laboratory, and the National Aeronautics and Space Administration (NASA) through Ames Research Center, with Dr. Statler as Project Officer. The German participants included the German Ministry of Defense through the Institute for Flight Mechanics, and Braunschweig of German Aerospace Center, DFVLR (now the DLR) with Dr. Hamel as Project Officer. I took Dr. Statler's place as U.S. Project Officer in 1985. In the 1990s, we had personnel exchanges, with Chris Blanken going to Germany for 6 months and Bernd Gmelin coming to work at Ames.

Complementary ground-based and in-flight simulation facilities at NASA Ames and DFVLR-Braunschweig played an indispensable role in this handling-qualities task. Although, initially, handling qualities was a small part of their work, they soon generated valuable flight-test data to assess a Bo-105 against our handling-qualities criteria [9]. Then Dr. Hamel's Institute modified another Bo-105 with a variable-stability system and performed systematic research investigations to generate data on agreed topics. A few years later, DFVLR replaced their VSH Bo-105 with a brand new EC-135, built with a variable-stability system installed at the factory. That system continued to provide the flight-test data we needed in the development and evaluation of our handling-qualities specifications. This very productive collaboration ended when the MOU expired in 2012, after 33 years of collaboration.

I met Mac Sinclair in 1978 when I replaced Dr. Statler as a member of the AGARD Flight Mechanics Panel. Mac was the Flight Research Department Head at the Canadian National Research Council (NRC), Ottawa. Among NRC flight-test aircraft, they had a Bell 47G set up as a modest VSH, and since the mid-1960s they had been doing research on V/STOL handling qualities. Around 1980, they were just about finished developing an up-to-date VSH replacement based on a Bell 205. Mac expressed interest in our helicopter handling-qualities efforts and offered to take on some topics to investigate. This evolved into our first source of handling-qualities research data from flight tests, and, over the years, NRC became the most robust basis for most of our low-speed-response requirements. I must mention the NRC project pilot, Murray Morgan, who, for many years, led the setup of the helicopter configurations to be evaluated, prepared the necessary ground-cueing devices, and acted as the helicopter safety pilot. In later years, Murray was helped by Stewart Baillie, who eventually succeeded Mac Sinclair as the Flight Research Department Head [10].

Unlike the joint research with Germany, which was initiated and proceeded under the auspices of a formal MOU, for several years we collaborated with the NRC Flight Research Department on an informal technical level. Eventually this collaboration was incorporated into the government-to-government Tripartite Technical Cooperation Program (TTCP), the UK being the third member, with RAE Bedford their primary research facility.



Roger Hoh, 1988.

The Army and NASA at Ames started collaborating with Canada and the UK through the TTCP on rotor aerodynamics and dynamics topics sometime in the late 1970s and added the topic of handling qualities around the mid-1980s. The primary researcher we interacted with was Gareth Padfield at RAE. In the 1990s, we exchanged personnel; Matthew Whalley worked at RAE Bedford for about 6 months, and RAE engineer Jeremy Howitt came to Ames for about the same period. Over the years, we had many valuable exchanges of ideas with RAE personnel.

RAE Bedford had a good flight-test capability, and they provided valuable basic data on helicopters such as the Lynx and Puma. Typical experiments would involve flying the real helicopter for the same tasks that had been investigated on the Ames simulators. On the simulator, we could systematically and safely vary the helicopter handling-qualities characteristics from very good to bad, so as to define the boundaries of the various handling-quality rating levels. Flight testing a real helicopter provides one very good data point to compare with the simulator results.

In retrospect, I realize the benefit to the Army (and to me, personally) of having had NASA as a participant in these collaborations, particularly those with foreign organizations. It was attractive to the heads of the organizations we partnered with to be able to report to their superiors and/or Board of Directors that the collaboration included NASA. Moreover, we also benefitted, because the NASA personnel whose help we needed enjoyed having the opportunity to visit the foreign agencies, engage with their experts, and fly their aircraft.

Development of ADS-33

By about 1981 I, and the other Army and NASA engineers working with me on helicopter handling qualities, had performed several tasks in support of the Army Program Managers (PMs). One was particularly momentous. The PM for the Light Helicopter Experimental (LHX), Colonel Walt Rundgren, was trying to get some handling-qualities standards incorporated into the system development spec. Colonel Rundgren was a graduate of the NTPS and recognized the shortcomings of MIL-H-8501, on which he had to base handling-qualities requirements for the LHX (which ultimately evolved into the RAH-66 Comanche). He recognized and supported our efforts to update the criteria, and liked what we were doing. As a result, he designated \$250,000 of his project funds to us to get some contractor support. This was a major turning point. We developed a statement of work and procurement package, and the NASA Ames Procurement Office handled a competitive source selection for us.

The only realistic bidders for such work were the people I used to work with at CAL, now Calspan, in Buffalo, New York, and Systems Technology, Inc. (STI) in Hawthorne, California. Both organizations had people with many years of experience in handling-qualities research. We

selected the STI bid, primarily because they recommended a new method for characterizing the short-term dynamic response, bandwidth. Calspan stuck with the old helicopter idea of time until the pitch (or roll) response to a step control input was concave down, or for higher speeds, the frequency and damping ratio of the short-term response similar to fixed-wing criteria. Unfortunately these simplifications just do not adequately represent a helicopter. The proposed STI criterion was bandwidth. This is a measure of how closely the aircraft's attitude response (e.g., pitch or bank angle) follows the corresponding control input, not as a time history response to a step control input (we could never get that to work), but in the frequency domain over the range of frequencies that are important to the pilot. It is measured from a plot of amplitude ratio and phase of the aircraft response with respect to the control input as a function of frequency, with the bandwidth being the frequency at which there is at least a 6-dB amplitude margin or 45-degree phase margin from instability, which occurs when the helicopter response lags the control input by 180 degrees (i.e., 180-degree phase shift). This parameter adequately characterized the essence of the multi-parameter helicopter response. Dynamic response to control criteria for pitch, roll, and yaw, in both hover, low-speed, and forward flight were eventually built around this concept.

Initially the STI effort was led by a researcher from their office in nearby Mountain View. However, before long Roger Hoh took over. After a year or two, Roger separated from STI to form his own company, Hoh Aeronautics Inc., and STI dropped out of the picture. Helped by his employee, David Mitchell, and at times by his associate, Robert Heffley, Roger became the primary scientist-engineer for the development of the new helicopter handling-qualities specification, ADS-33.

During many ground-based and even in-flight simulations and trials at Ames and with our collaborators, Roger led experiment design, provided critical set-up support, and often acted as an evaluation pilot. He and Dave Mitchell prepared the actual drafts of the spec and the monster report documenting the criteria rationale. It was called the Background Information and User Guide (BIUG) [11]. I wrote many of the requirements, oversaw all the reviews, and did lots and lots of wordsmithing in our efforts to satisfy the "standards" people. So, along with Chris Blanken, I consider myself a co-author, but Roger Hoh deserves much praise and recognition. In fact, even now, 34 years later, I understand that Chris still uses Roger Hoh as a resource.

Some Closing Thoughts

Looking back, it seems that my whole career was designed to give me the background and vision to lead development of a new handling-qualities spec for helicopters. My role at the Army research organization and the Army-NASA Joint Agreement eventually provided the opportunity. The authority inherent in representing the U.S. Government agency that led the country in rotorcraft research and development was an essential ingredient for leading the other government agencies, industry, and international collaborators. It took lots of initiative, ingenuity, and help to realize that opportunity. We needed new ideas and, more critically, new data to refine and validate the ideas. Fortunately, the Joint Agreement gave us access to facilities, support, and expertise that the Army would never have been able to afford. The Army provided people and funds for contract support, and allowed us to play that leadership role.

The editors of this memoir intended to emphasize the early years of the Army research labs. Specifically, the period from the labs formulation in 1965 until about 1985. By 1985, we had just

about got started. We had a core of in-house researchers, some contract money to get help from outside experts, and access to in-house and outside collaborator's facilities that were beginning to generate a believable database. We had developed some new criteria and a brand-new spec structure that addressed Army missions including DVEs. It followed principles developed over many years by the fixed-wing community in MIL-F-8785, but it had many innovations. It was not comprehensive, but it was superior to anything ever used before.

After reviews with Army PMs, we decided to put together a first version of a specification focused on the light scout attack helicopter, since that was actively being developed in conceptual studies by AVSCOM. This meant we could focus our handling-qualities research on high-maneuverability agility tasks such as air-air combat and ignore topics such as a heavy-lift helicopter doing a sling-load pick-up.

We also talked with the AVSCOM specification authorities. They advised against calling our work a new Mil-Spec (for example MIL-H-8501C), because that involved coordination with the tri-service specification authorities. This was appropriate because our package would be limited, even for Army roles and missions, and it made no mention at all of the high-priority Navy mission, helicopter ship landing. Instead we were able to publish as an AVSCOM controlled document, an Aeronautical Design Standard, ADS-33.

A draft of the first version of ADS-33 and its associated BIUG was circulated to industry and agencies such as AEFA and NTPS in 1985. After review meetings, we incorporated responses to comments and published the first version in 1986. This, and future versions as they evolved, were incorporated by reference into the LHX, subsequently Comanche, system specification.

Refinement and validation, and expansion to cover a comprehensive range of helicopter roles and missions, continued for many years. One big event occurred in 1993–1994. The Secretary of Defense at that time was very sensitive about military specifications getting into too much detail and pronounced that, henceforth, specs could only define what capability, or performance level, was desired from a system. Specs containing descriptions of the hardware needed to achieve that performance were to be changed or eliminated. Fortunately, none of our criteria were stated in terms of the hardware; everything was defined by the level of performance that was to be achieved. However, as our draft went through scrutiny by the standards people, they decided that, if we were to be a spec, we should meet the specifications for a spec; yes there is a spec on specs! That was tedious, but not too difficult to comply with.

The current version of ADS-33 has been signed off as a Performance Specification by the Army Material Command and the Program Executive Office Aviation Standards Executive: Aeronautical Design Standard, Performance Specification, Handling Qualities for Military Rotorcraft, ADS-33E-PRF. (March 2000) [12]. In addition, there is a Flight Test Guide [7].

The comprehensive set of criteria in ADS-33 cover mission roles from scout-attack to heavy-lift cargo helicopters. Use for a specific application requires “tailoring,” that is sorting out which criteria actually apply for that system. The user has to define the operational missions and the expected environment. This includes not only maneuvering flight envelopes, altitude, and temperature, but also the expected visual conditions, the day, night, and visibility parameters. A technique is specified for determining the Usable Cue Environment (UCE) achieved in the expected DVE with the planned vision aids and displays. As the visual environment degrades, pilots need more stabilization to perform missions with good handling qualities. A major

innovation in ADS-33 was developing this concept and defining required response types as a function of the UCE. The required level of performance of those response types is also specified and varies with the mission tasks. Parameters are specified for hover and low speed and for forward flight. The various stability and control parameters allow a prediction of the handling qualities that the design should achieve. Another significant innovation was defining a set of mission tasks that can be performed in the aircraft to determine the Assigned Level of Handling Qualities, the “proof of the pudding.” Ground referenced maneuvers, together with visual cueing aids and appropriate quantitative performance standards, are defined in detail. This means that long before the aircraft is built and flown, the contractor can use a math model and piloted simulator to evaluate the expected handling qualities. When the aircraft actually flies, the test pilots can base their evaluations using a set of prescribed maneuvers including the level of aggression and precision required to perform their evaluations.

This is a package the Aeroflightdynamics Directorate can be very proud of. Working as consultants for the Comanche PM, our liaison work with Boeing and Sikorsky engineers as they applied ADS-33 had a big impact on Comanche handling qualities and system safety. If the few bugs remaining when the program was terminated could have been worked out, the Army would have gained a scout-attack helicopter with excellent handling qualities. Many of the concepts of ADS-33 have been incorporated in upgrades to the existing Blackhawk, Apache, and Chinook fleet, and the ADS-33 is ready and waiting to guide a new system development.

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Index

A

Advanced Research Systems Integration Simulator (ARSIS), 10, 12
 Advanced Systems Research Office (ASRO), 5-6
 Advisory Group on Aerospace R&D (AGARD), 16
 Aeroflightdynamics Directorate, 20
 Aeromechanics Laboratory, 12, 14, 16
 Aeronautical Design Standard (ADS), 14, 16-20
 AGARD Flight Mechanics Panel, 16
 Aiken, Ed, 11-12, 15
 Ames Directorate, 5-6, 12
 Ames Research Center, 5, 16
 Ames, 4-6, 8, 10, 12, 14-18
 AMRDL Directorates, 6
 Army Air Mobility R&D Laboratory (AMRDL), 5-6, 12
 Army Aviation Engineering Flight Activity (AEFA), 15, 19
 Army Aviation R&D Command (AVRADCOM), 12
 Army Aviation Systems Command (AVSCOM), 12, 19
 Army Materiel Command, 16
 Army, 1, 4-13, 15-20
 Army-NASA Joint Agreement, 6, 11, 18

B

Baca, Pamela, 12
 Background Information and User Guide (BIUG), 18-19
 Baillie, Stewart, 16
 Behens, Bob, 11
 Bell Aerosystems, 3-4, 7, 9, 16
 Billings, Donald, 12
 Blanken, Chris, 13-14, 16, 18
 Bo-105, 7, 16
 Boeing, 4, 15, 20
 Booth, Roberta, 12
 Braunschweig, 16
 Brigadier, Bill, 11
 British European Airways (BEA), 2-3
 Buffalo, 4-5, 17

C

CAL Flight Research Department, 4
 Calspan, 11, 17-18
 Canadian National Research Council (NRC), 16-17
 Carico, Dean, 12
 Carlson, Dick, 5
 Carson, Tom, 11
 Chalk, Charles, 4
 Chen, Robert, 11

Churchill, Gary, 12
 Cicolani, Luigi, 11
 College of Aeronautics, Cranfield, 2-3, 5
 Cooper, George, 4
 Cooper-Harper Handling Qualities Rating Scale, 4
 Corliss, Lloyd, 7, 11-13
 Cornell Aeronautical Laboratory (CAL), 4-6, 11, 17
 Curtiss-Wright, 5

D

Danek, George, 12
 Davis, John, 11-12
 Decker, Bill, 11
 Deel, Arlin, 10, 12
 DFVLR (now DLR), 16
 Doolin, Brian, 11
 Drinkwater, Fred, 12
 Dunn, Richard, 6

E

Edwards Air Force Base (EAFB), 1, 15
 Engineering Evaluation, 20
 Eustis Directorate, 5-6

F

FAA, 4-5
 Flight Control Division, 12-13
 Flight Dynamics and Controls Branch, 6-7, 11-12
 Flight Simulator for Advanced Aircraft (FSAA), 8-10
 Flight Test Department (NASA), 12
 Franklin, Jack, 6, 11-13
 Fry, Emmett, 11
 Ft. Eustis, 5
 Ft. Rucker, 7

G

Gerdes, Ron, 15
 Gmelin, Bernd, 16
 Gossett, Terry, 6
 Greif, Dick, 11

H

Hamel, Peter, 16
 Harper, Robert, 4
 Hartzell, Earl (Jim), 12, 13
 Heffley, Bob, 7, 18
 Henderson, John, 12, 15
 Hindson, Bill, 15
 Hinrichs, MG, 10
 Hoh Aeronautics, 18
 Hoh, Roger, 17-18
 Huff, Ed, 12
 Hynes, Charlie, 11

Key, D.

I

Immen, Fred, 5
Institute for Flight Mechanics, DFVLR, Braunschweig,
16

J

Jacobsen, Bob, 11

K

Kanning, Gerd, 11
Kerr, Andy, 6
Kuhlman, Bob, 11

L

Langley Directorate, 5, 6
Lebacqz, Vic, 11, 13, 20
Lewis Directorate, 5

M

Mac Sinclair, 16
Manseur, Hossein, 13
Mathews, Stephen, 12
McFarland, Richard, 12-13
McNeill, Walt, 11
Memorandum of Understanding (MOU), 16-17
Merrick, Vern, 11
Merrill, Robert, 12, 15
Mitchell, Dave, 18
Miyajima, Katsuyawa, 11
Morgan, Murray, 16
Morse, Andy, 12
Mosher, Wayne, 6

N

Nap-of-the earth (NOE), 7-9
NASA, 3-18
NASA Ames Research Center, 5
NASA Langley Research Center, 5-6
National Research Council (NRC), Ottawa, Canada, 16
Navy Test Pilot School (NTPS), 15, 17, 19
NRC Flight Research Department, 17

O

Odneal, Billy, 10, 12
Ormiston, Bob, 12

P

Padfield, Gareth, 17
Power, Leo, 11

R

Research and Technology Laboratories, 12
Robinson, Norman, 5
Rotorcraft Aircrew Systems Concepts Airborne
Laboratory (RASCAL), 15

Royal Aircraft Establishment (RAE), 1, 17
Royal Air Force (RAF), 1-2
Rundgren, Walt, 17

S

Sammonds, Bob, 11
Schroers, Laurel (Shorty), 12-13
Schuler, John, 4
Scully, Mike, 6
Short, Barbara, 11
Sikorsky, 20
Simulation Sciences Division, 8, 12
Sinacori, John, 10
Smith, Alan, 11
Sperry, 14
Station Hawkinge, 1
Statler, Irving, 5-7, 10, 12, 16
Stevens, Story C., 12
Stevenson, Jack, 11
Stewart, Elwood, 11
Systems Technology, Inc. (STI), 7, 17-18

T

Talbot, Peter, 7, 9, 11, 13
Tiltrotor Office, 12-13
Tinling, Bruce, 11
Tischler, Mark, 13
Tripartite Technical Cooperation Program (TTCP), 17
Tucker, George, 15

V

variable-stability helicopter (VSH), 14-16
Vertical Motion Simulator (VMS), 10-11
Vollenweider, Linda, 11
V/STOL, 1, 3-4, 6-7, 9, 11, 14, 16
V/STOLAND, 14
VTOL, 7

W

Webster, Larry, 12
Whalley, Matthew, 13, 17
White, Maurice (Maurie), 6
Wright, Bob, 12-13
WWII, 5

X

X-22A, 3-4

Y

Yaggy, Paul, 5

Remembrances From AFDD

Donald L. Kunz
Army

Prologue

I started working at the Ames Directorate of the U.S. Army Air Mobility R&D Laboratory (AMRDL) in April of 1976, and I left in October of 1989, then the Aeroflightdynamics Directorate (AFDD) of the U.S. Army Aviation Research and Technology Activity. Without a doubt, the 13-plus years that I spent in Building N-215 at NASA Ames Research Center were some of the most fulfilling of my career, which now spans slightly more than 40 years. The biggest reason for the fondness with which I look back on those years is the people with whom I worked. Unlike many of my colleagues who made ground-breaking contributions to helicopter technology, my contributions were much more modest. However, the benefits I received, both personally and professionally, from those years at Ames Research Center, far exceeded my modest contributions. Therefore, my memories relate more to the people I worked with, and to what I learned from them, than to my accomplishments.



Don Kunz, 1977.

An Unexpected Hiring

I expect that everyone has a story about how they ended up working at Ames, and I am no different. In the spring of 1975, I was about a year away from completing my Ph.D. at the Georgia Institute of Technology (Georgia Tech) under Professor G. Alvin Pierce. Earning a Ph.D. had not been in my career plan. My plan was to earn an M.S. degree and work for an aircraft manufacturer. That plan fell apart because of the dearth of jobs for aerospace engineers when I completed my M.S. degree at Georgia Tech in 1972. It was only through the encouragement and support of Dr. Pierce that I was able to continue my education. Had it not been for him, I probably would have ended up living in my parents' basement (a terrible disgrace in those days).

While looking through *Aviation Week & Space Technology* magazine one day that spring, I saw that the Ames Directorate of the U.S. Army AMRDL was looking to fill a vacant position. This was my dream job, since my doctoral research applied to unsteady aerodynamics on helicopter blades. I did not know at the time that the position had already been filled by Dave Peters. I sent in my résumé and waited, and waited ... and waited. In the fall of that year, I finally received and accepted a job offer from Clarence Perisho at McDonnell Douglas in St. Louis. I had no other offers and, honestly, I had forgotten that I had even applied for the position at Ames.

In December, my brother, Dick, was getting married to Stephanie Latta, and I was to be the best man at their wedding. Along with several friends, I traveled to Cincinnati, Ohio, where the ceremony was to take place. One evening, a couple of days before the wedding, we were all at the Latta's home, and one of Stephanie's sisters excitedly told me that I had a call from California. Without a clue as to who it could possibly be, I took the call. It was Bob Ormiston,

wanting to conduct a telephone interview right then and there. Apart from admitting that I had already accepted a job with McDonnell Douglas, I remember none of the conversation with Bob. Since I was totally unprepared for the interview, I don't know how I possibly could have made a positive impression, but apparently I did. Bob told me that the reason why no one had responded to my application was that he had been in France most of that year. With Dr. Pierce's help, I was able to gracefully rescind my acceptance of the McDonnell Douglas position and accept the position that Bob offered me.

By the way, I later found out that Bob went through quite a bit of effort to contact me in Cincinnati. When he couldn't reach me at my apartment in Atlanta, he called Dr. Pierce to ask if he knew how I could be reached. Dr. Pierce knew I was in Cincinnati, but he didn't have a telephone number, so he gave Bob my parents' phone number in Geneva, New York. Fortunately, my parents had not yet left for the wedding, and they were able to provide Bob with the Latta's telephone number.

Introduction to the Ames Directorate

My first day at Ames Research Center started out typically for a new employee. The in-processing was easy compared to what it is today, and by the middle of the morning I reported to Bob Ormiston's office in Building N-215. We talked for a while, then we walked the halls and he introduced me to the men and women who staffed the Ames Directorate. I was assigned to an office on the back side of the building. The windows had a great view of the corrugated metal sheeting on the 7- by 10-Foot Wind Tunnel, but there was a lot of sky visible as well. Best of all, I would be sharing the office with Georgene Laub. Georgene was an outstanding engineer who had a hand in solving many of the issues that always came up when experiments were being performed. But most of all, Georgene was a wonderful person who helped with my transition from student to working research engineer. We kept in contact for many years after she retired and I went on to other pursuits.

After I had settled in, Bob came by and asked if I would like to join him and a few of the others for lunch. Little did I know that that lunch would leave a lasting impression on me. I followed Bob out to the patio area next to the Ames cafeteria, where we sat at one of the circular tables. We were soon joined by Dewey Hodges, Bill Bousman, Jim McCroskey, Frank Caradonna, and Fred Schmitz. As I sat at the table, I realized that in my dissertation I had cited the works of most, if not all, of the gentlemen sitting at the table with me. At that moment, I realized that I could benefit immensely if I kept my mouth shut and my ears open—and I did. That story is one that I pass on to all my students as a life lesson in the proper way to approach any new situation.

After the first day, I joined that group for lunch as often as I could. The discussions encompassed a wide range of topics, as one would expect from such an intellectually gifted group. Everyone's opinion was considered and respected, regardless of the subject. Conjecture was welcomed and actively encouraged. I recall one day when the discussion was particularly lively. Someone (I don't remember who) sat down and offered that he had some fact that could shed some light on the discussion. He was politely asked to keep his facts to himself, since they just might ruin the discussion. The lunchtime group lasted for 5 or 6 years after my arrival, as I recall; but as the members' responsibilities expanded, it became more difficult to take time away from their desks, and the group gradually dissolved.

Colleagues and Friends

The people I worked with most closely were Bob Ormiston, Dewey Hodges, Stu Hopkins, and Mike Rutkowski. Bob and Dewey were already at the Ames Directorate when I arrived, and Mike and Stu arrived later. I never worked directly with Bob on a research project, but I depended on him greatly when I inevitably ran into a problem that I could not solve. One particular area where I found him to be most outstanding was in extracting an understanding of the underlying physics from analytical results. The insights that he provided, and the way that he approached problems, benefit me to this day.

When I arrived, Bob assigned me to the theoretical side of the dynamics group, which was somewhat surprising because my doctoral dissertation was experimental. However, that was my good fortune, because I started to work with Dewey Hodges. Dewey was an outstanding mentor, before I even knew what a mentor was. I worked either for or with him on several different research topics, starting almost immediately after my arrival.

Mike Rutkowski (who passed away in 2014) and I were office mates for about 12 years, from the time he arrived in 1977 until I left in 1989. When Mike arrived, Georgene moved to another office, more convenient to the experimental facilities where she worked. Mike and I became good friends, but we never worked on a research project together. That did not mean, however, that we didn't collaborate, although our collaborations were not usually directly connected to our official duties. For example, for the first 2 or 3 years after I arrived, Ken McAlister and Larry Carr had the only computer terminal in the building in their office. One day, Mike and I decided that it would be much easier to submit jobs to the NASA Ames computer center if we had a computer terminal, too. One of us found out that the computer center had a Tektronix 4014 terminal that had been paid for with Army funds. We grabbed a cart, walked over to the computer center, and returned to our office with the terminal.

Another collaboration between Mike and me was the creation of the Perry White, Editor (PWE). Sometime after the great Tektronix hijacking, the laboratory purchased a computer that lived in Building N-215, and everyone in the building could access it via terminals on their desks. The computer was a Digital Equipment Corporation (DEC) product, and it came with the standard DEC editor, EDT, plus an extensible editor with its own language that could be used to customize it. There were a few things about EDT that Mike and I found annoying, so I started learning the programming language and made some changes to the basic EDT editor. Over several months, Mike and I would discuss features that we both would like to see implemented; I would handle the actual implementation, and we would both beta test my programming. Eventually, quite a few people in the building were using PWE (and I have been told that it was being used by a few people in the headquarters building, too). In case anyone wonders how PWE got its name, the DEC computer in the building was named Krypton, so Mike and I decided to carry on the Superman motif.

The experimental side of the Rotorcraft Dynamics Division was in the capable hands of Bill Bousman and Dave Sharpe. Much later in my tenure, a couple of younger guys (Seth Dawson and Mike McNulty) were also hired to work on the experimental side of the house. I rarely worked with Bill and Dave directly, but I learned a lot, especially from Bill. To this day, I believe that he is the best experimental engineer I have encountered. One of Bill's nuggets, which I regularly pass on to others, is *Bousman's Law of Presentations* (my title). *Bousman's*

Law of Presentations states that the length of a presentation can be calculated by multiplying the number of slides by an average of 1-1/2 minutes per slide. It is remarkable how accurate that rule of thumb has proved to be. I have since added a corollary that says, “If you can’t spend at least 30 seconds talking about a slide, it should be removed from the presentation.”

Another individual with whom I formed a close friendship was Wendell Stephens. Wendell was assigned to the Second Generation Comprehensive Helicopter Analysis System (2GCHAS) project office when he arrived. Although I never officially worked on 2GCHAS, Wendell and I had many discussions regarding features and implementation. More notably, we became running buddies, and almost daily at lunchtime we ventured out to run around the Moffett Field runway.

I can’t leave this topic without mentioning Paul Yaggy and his daughter Kathy. Paul had retired from government service before I arrived at Ames, but he still had a significant impact on me. I met him through Kathy, who worked part-time at the Ames-Army Lab, now the Aeromechanics Laboratory of the U.S. Army Research and Technology Laboratories, while getting an engineering degree at San Jose State University. Kathy and I became close friends. I started attending Calvary Baptist Church in Los Gatos, where her father was on staff. Calvary Church was also where I met my wife, Christine, and Reverend Yaggy officiated at our wedding.

Dr. Statler and I had little direct interaction apart from those times when Army personnel were gathered together for awards and promotion ceremonies and other Army Lab events. However, a statement he made at one of those events stuck with me. He said (and I am paraphrasing) that he believed that one of his major responsibilities was to shield us from much of the unnecessary Army and NASA administrivia, so that we could excel at our research. I didn’t appreciate how well he performed that responsibility until many years later. Now, I frequently use that as a metric to evaluate the effectiveness of my supervisors.

Research

The first research project I was assigned was working for Dewey Hodges. My job was to re-derive Dewey’s rotor-blade equations of motion [1] in order to check his derivation. As I recall, the derivation wasn’t all that difficult, but it was very tedious. Dewey kindly acknowledged my efforts in the resulting report.

My first solo research was an investigation of using unsteady aerodynamics, instead of quasi-steady aerodynamics, on a rotor-stability problem [2]. The motivation was to attempt to resolve some discrepancies between the damping measured in some experimental data and the damping predicted by analysis. One suggestion was that unsteady aerodynamics was causing the discrepancy. It wasn’t—at least that is what my analysis showed.

In the late 1970s, and continuing into the early 1980s, the Army started taking an interest in reducing the vibration levels in helicopters, and that is where I began to concentrate my efforts. At first, I investigated linearized models of simple, coupled rotor-body configurations. However, my analyses didn’t really yield any breakthroughs. So, I moved on to developing, or trying to develop, more complex, nonlinear models. Again, I didn’t discover anything that substantially increased our understanding of the sources and remedies for helicopter vibrations. However, I did come up with an outstanding acronym for my analysis: Qualitative Investigation of Vibration of Rotorcraft (QUIVR).

My vibration work did yield one funny, or, more accurately, embarrassing story. I was presenting my research [3] at the 1980 American Helicopter Society Forum, and Dave Peters' presentation on the same topic was scheduled right before mine. As I was listening to Dave's presentation, I realized that his approach to the problem was much simpler and significantly more elegant than mine. So, I began my presentation with a disclaimer that if anyone in the audience was considering a similar investigation, they should use Dave's method and not mine. My presentation clearly demonstrated why the disclaimer was true, and not just a bit of false humility.

While I was still engaged in the vibration effort, Dewey had started developing the General Rotorcraft Aeromechanical Stability Program (GRASP), and had hired Stu Hopkins as a contractor to help out with the program development. I wasn't involved with GRASP at first, but I became involved in order to help Dewey push back on an attempt by external influences to terminate its development. We prevailed, and Dewey asked me to join the project. In terms of the division of labor, Dewey provided the theoretical and mathematical expertise needed for the development of GRASP, while Stu provided his expertise in computer science and multibody dynamics. I didn't have near the expertise that either of them had in any of those areas, but I knew a little something about all of them. Therefore, one of my major contributions was to serve as a translator when either Dewey or Stu was deep into explaining something in his area, and the other didn't understand the issue. I also did a lot of software testing. There was one time when we were trying to figure out why the program was running so slowly. I was able to profile the code and determine that the cause of the slowness was one particular (Fortran) subroutine that was not only inefficient, but also was called a multitude of times.

My experience working with GRASP evolved into some informal consulting with the 2GCHAS program. Both programs involved extensive code development and the meshing of diverse technology elements. Therefore, I could transfer some of the lessons learned during the development of GRASP to the 2GCHAS development.

It should be apparent from my description of the research in which I was engaged that most of it was analytical and/or computational. As a result, access to computers was vital to the research, and I was grateful that, under the provisions of the Army-NASA Joint Agreement, we had unfettered access to the NASA Ames state-of-the-art computer center. In later years, access to those mainframe computers became less important, since computers were eventually installed in Building N-215.

Summary

Between April 11, 1976, and October 19, 1989, (13 years, 6 months, and 8 days) it was my privilege to have been employed beginning with at the Ames Directorate of the U.S. Army AMRDL and concluding with the Aeroflightdynamics Directorate of the U.S. Army Aviation Research and Technology Activity. I can't imagine a better way to start a career, and I am sure that under different circumstances I would still be there, alongside the friends and colleagues who remain.

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Index

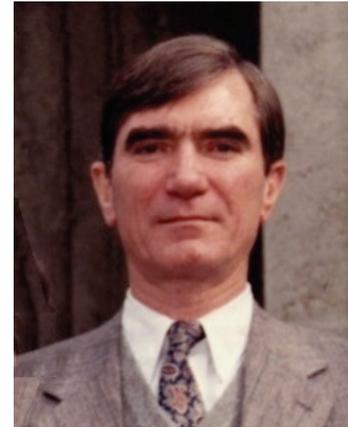
- A**
Aeroflightdynamics Directorate (AFDD), 1
Aeromechanics Laboratory, 4
Air Mobility R&D Laboratory (AMRDL), 1, 5
American Helicopter Society, 5
Ames Directorate, 1-3, 5
Army, 1, 3-5
Army-NASA Joint Agreement, 5
Aviation Research and Technology Activity, 1, 5
- B**
Bousman, Bill, 2-3
- C**
Calvary Baptist Church, 4
Caradonna, Frank, 2
Carr, Larry, 3
- D**
Dawson, Seth, 3
Digital Equipment Corporation (DEC), 3
- G**
General Rotorcraft Aeromechanical Stability Program (GRASP), 5
Georgia Tech, 1
- H**
Hodges, Dewey, 2-4
Hopkins, Stu, 3-5
- L**
Laub, Georgene, 2-3
- M**
McAlister, Ken, 3
McCroskey, Jim, 2
McDonnell Douglas, 1-2
Moffett Field, 4
- N**
NASA Ames Research Center, 1
- O**
Ormiston, Bob, 1-3
- P**
Perisho, Clarence, 1
Perry White, Editor (PWE), 3
Peters, Dave, 1, 5
Pierce, Alvin, 1-2
- Q**
Qualitative Investigation of Vibration of Rotorcraft, (QUIVR), 4
- R**
Rotorcraft Dynamics Division, 3
Rutkowski, Mike, 3
- S**
San Jose State University, 4
Schmitz, Fred, 2
Second Generation Comprehensive Helicopter Analysis System, (2GCHAS), 4-5
Sharpe, Dave, 3
Stephens, Wendell, 4
- T**
Tektronix, 3
- Y**
Yaggy, Kathy, 4
Yaggy, Paul, 4

Memories of the Army-NASA Joint Agreement, 1978–1985

J. V. Lebacqz
NASA

Background and People

On Monday, August 14, 1978, I reported for my new job at NASA Ames, as the newest NASA research engineer in the Flight Dynamics and Controls Branch, working for its Chief, Jack Franklin. I was fortunate that NASA Headquarters (HQ) had recently named Ames as the lead for rotorcraft research at NASA, and a few NASA personnel slots opened up. I am certain that this decision was based on the Joint Agreement already in place at Ames between NASA and the Army, so I really owe my NASA career to the Army and NASA people who put that agreement in place. That Monday marked the beginning of my 28-plus-year career with NASA. I performed several research investigations on rotorcraft handling qualities, and then, starting in 1985, I held a variety of NASA management positions (first, in fact, at Ames, as Chief of that branch, and ending as Associate Administrator for Aeronautics Research at NASA HQ). The purpose of this memoir is to remember and discuss those first years of rotorcraft research as part of the Army-NASA Joint Agreement at Ames, from 1978 to 1985, years for me that were filled with wonder, excitement, fulfillment, and friendship.



J. V. Lebacqz, 1987.

I believe the strength of a career, and of a life, too, is determined by the people in it. Ecclesiastes (4:12) says that “a three-fold cord is not easily broken.” Those three strands in my life were Princeton University, where I met friends and colleagues and went to undergraduate and graduate school; Cornell Aeronautical Laboratory, where I had my first job after school; and NASA, Ames in particular, where I was fortunate to join in this unique partnership.

The first strand was Princeton. A significant number of very smart people whom I met there actually came to Ames, either as Army or NASA employees. Jack Franklin, my first boss at Ames, was my friend at Princeton and best man at my wedding. Fred Schmitz, another Princeton friend with whom I taught rotorcraft aerodynamics and dynamics in a series of graduate courses that he initiated at Stanford, later became my boss after he had switched from the Ames-Army Lab to NASA and eventually became NASA Ames Director of Aeronautics. Bob Ormiston lived with my parents for a while when he came from Princeton to work for Paul Yaggy in the Ames-Army Lab, prior to becoming a Division Chief and then Aeromechanics Chief Scientist. Jim McCroskey was another colleague from Princeton who ended up at the Ames-Army Lab. And finally Ed Aiken—who later joined me at Calspan and whose wedding reception I and my grad school housemates hosted—came to Ames. All these guys had brilliant careers at Ames, and some have chapters in this book. As a side note on Princeton, Wes Harris, who did not come to Ames like the fellows above, was also an acquaintance of mine there; Wes became NASA Associate Administrator for Aeronautics 10 years before I did!

The second strand was Cornell Aeronautical Laboratory (later Calspan), where I worked before I came to Ames. At Calspan, I met Irv Statler while on a project to develop system identification

procedures for the X-22A aircraft; Irv left and became head of the Ames Directorate of the U.S. Army Air Mobility Research and Development Laboratory (AMRDL) (see his chapter herein). My boss at Calspan was Dave Key, who also left to head the Flight Control Division of the Ames Directorate. My mentors at Calspan were Bob Harper, who co-authored the Cooper–Harper Handling Qualities Rating Scale with George Cooper at Ames, and Rogers Smith, a pilot-engineer who left to become Chief Pilot at Canada’s National Research Council and later became Chief Pilot at NASA Dryden Flight Research Center. I was privileged to work with two outstanding research engineers at Calspan who also went to Ames. First, I worked for Bob Chen on the system identification project where I’d met Irv; Bob went to work for Jack Franklin as a NASA employee doing rotorcraft research a year or so before I went to Ames. Then I worked with Ed Aiken on an X-22A control-display tradeoff handling-qualities study for vertical takeoff and landing (VTOL) aircraft [1]; Ed left Calspan around the same time as Bob, first going to Singer-Link in Houston and then coming to Ames to work in Dave Key’s AMRDL Flight Control Division shortly before I arrived. Another Calspan pilot-engineer, Warren Hall, also came to Ames as a “platform” pilot flying the fixed-wing aircraft hosting science experiments. Interestingly, one of the X-22A electronic technicians, John Wilson, had also left Calspan and was working in the hangar where my new NASA office was located!

Ames was the third strand, after Princeton and Calspan. When I got to Ames and began to braid the three strands together, I had already worked with a significant number of people who were there, so I had a running start on meeting and working with new NASA and Army employees in the rotorcraft community. In addition to Bob Chen and me, Bill Decker and Peter Talbot were also NASA rotorcraft employees in Jack Franklin’s Branch, along with Dick Grief and Vern Merrick, whose focus was on VTOL aircraft. In the next few years after I arrived, Jeanine Weber, Katie Hilbert (who later became Assistant Branch Chief when I became Chief), and Barbara Sweet (Townsend at the time) also joined Jack’s Branch as NASA employees focusing on rotorcraft. John Foster, another NASA rotorcraft person, was in Dallas Denery’s Guidance and Navigation Branch but worked with us, and later became an Assistant Branch Chief when I was Chief of that Branch. At the time, he was responsible for the UH-1H VSTOLAND helicopter, which I’ll discuss later. In addition, Ed Aiken and Lloyd Corliss were Army employees in Jack’s Branch, focusing both on VTOL and rotorcraft handling qualities. Subsequently, Lloyd and I worked on the Rotor Systems Research Aircraft (RSRA) X-Wing project; Lloyd also entrained several of us (Ed, me, Jack, Fred, Bob, and John) into a wine-tasting group of friends that continues to this day. Other “youngsters” who arrived during this time frame were Doug Watson, Jay Fletcher, and Mike Lewis. Michelle Eshow joined Dave Key’s Flight Control Division later, and, along with Katie Hilbert, worked with Bill Hindson to make the CH-47 a productive research helicopter.

Flying-qualities research to determine vehicle handling qualities that increase the safety and effectiveness of rotorcraft operations necessarily involves extensive ground simulations and flight tests. Therefore, we worked closely with NASA and Army test pilots from the Flight Operations Branch: Ron Gerdes, George Tucker, and Gordon Hardy (NASA), and Pat Morris, Dan Dugan, Bob Merrill, and Grady Wilson (Army), along with Bill Hindson, who was with Stanford initially but joined NASA around 1985. I was also privileged to work with several Federal Aviation Authority (FAA) pilots and with Peter J. G. Harper of the British Civil Aviation Authority (CAA). It is really impossible to overstate how my life was enriched by the friendships developed with all these guys (at that time, all the test pilots were male).

Experimental Facilities

For a young engineer interested in flying qualities, the suite of ground simulators and aircraft available for handling-qualities experiments was remarkable at Ames. In 1978, the “primo” simulator was the Flight Simulator for Advanced Aircraft (FSAA). This six-degree-of-freedom (6-DOF) simulator was built at Ames to more exactly replicate the lateral motion cues that were thought to be important for the landing characteristics of the U.S. supersonic transport design, so the cab ran laterally along a 100-foot track. The visual scene was a terrain board with a moving camera that replicated (in scale) the motion of the vehicle being simulated. The first three simulations discussed below were done on the FSAA. Soon thereafter, however, a new Vertical Motion Simulator (VMS), more directly geared to the needs of rotorcraft, was completed. It was also a 6-DOF simulator, but the major motion was now 60 feet of travel along the vertical axis, coupled with 40 feet of lateral motion along a horizontal track. It later became the first simulator at Ames to use computer-generated imagery (originally pretty limited but rapidly gained sophistication), and it had the capability to include a “chin window,” which was important for rotary-wing simulations. Later helicopter simulations and all of the tiltrotor instrument approach simulations we conducted were done on the VMS; these were later led by Bill Decker.

The helicopter simulator experiments used a mathematical model developed by Bob Chen [2] that included the 3-DOF rotor tip-path plane dynamics—as determined by design parameters such as hinge offset, hinge restraint, blade Lock number, and pitch-flap coupling—along with the rigid-body 6-DOF. One of the capabilities built into the math model was full-state feedback, which enabled it to be used similarly to a response-feedback variable stability system, which I’d become familiar with at Calspan. While tip-path plane models are not accurate for detailed rotor dynamics modeling, at low speeds (e.g., less than 60 knots) their influence on vehicle dynamics is captured with adequate fidelity. Tiltrotor simulations used a 6-DOF aircraft model with quasi-steady rotor dynamics and generalized XV-15 or V-22 characteristics.

At that time, two flight vehicles were also used for handling-qualities research: the UH-1H VSTOLAND helicopter, which John Foster managed, and then the NASA Langley CH-47D helicopter. Lloyd Corliss’ chapter summarizes the UH-1H, and I’ll talk later about using it to validate some of the instrument approach simulator results. The CH-47D was a tandem rotor machine that had been fitted at Langley with redundant full-authority servos in all four control channels, as part of their VTOL Approach and Landing Technology (VALT) research program, to provide an in-flight simulator; the left-side controllers were mechanically disconnected from the rotor blades and instead drove computer models (initially provided by a TR-48 analog computer!) that then drove the right-side controls to fly the aircraft. One of my first tasks after arriving at Ames was to go to Langley and arrange for the transfer of the CH-47D to Ames. Bill Hindson was a driving force at Ames in getting this machine operating, as I discuss later.

Projects and Activities 1979–1985

Helicopter instrument flight rules (IFR) airworthiness standards. Between 1979 and 1982, I, along with NASA, FAA, Army, and United Kingdom (UK) CAA colleagues, conducted five experiments examining civil helicopter instrument approach airworthiness requirements. Two experiments were done on the FSAA, two were on the VMS, and one was in the VSTOLAND UH-1H [3-8]. These investigations were carried out as part of a joint NASA and FAA program at Ames to investigate helicopter IFR certification criteria. The genesis of this program was that the

FAA's airworthiness requirements at the time were based primarily on fixed-wing aircraft characteristics, and did not consider aspects such as stability and control augmentation systems, advanced display characteristics, or rotor type influences (e.g., cross-coupling in hingeless rotors). For example, the longitudinal stick gradient with speed for fixed-wing aircraft corresponds directly to the static angle-of-attack stability derivative, and is therefore required to be "stable" in criteria. However, for helicopters at low speed the gradient does not apply to this derivative, so requiring it to be "stable" doesn't necessarily apply in the same way. In our experiments, we considered a wide range of all of these variables to instrument approach handling qualities.

NASA and the FAA at Ames had previously collaborated on airworthiness criteria for supersonic transports and for short takeoff and landing (STOL) aircraft, and that partnership continued with a new focus on rotorcraft airworthiness issues. Jack Cayot, a bomber pilot in WWII who spent much of the war in a prisoner-of-war camp in Italy, ran the FAA office, which included Ray Forrest and Barry Scott. Ray conducted the first of five helicopter IFR simulations at Ames with Bob Chen and Ron Gerdes [9], and he subsequently became my FAA counterpart in the rest of the experiments. He was an excellent engineer and a real gentleman; I truly enjoyed working with him and became good friends with him and his wife, Jo. Tom Alderete, a contractor at the time, was the lead simulation engineer on the first experiment, and NASA's Ron Gerdes was the pilot [3]. Ron and Bob Merrill were the Army pilots on the second IFR simulation [4]. Ron flew again in the third simulation, a UH-1H flight experiment, along with two other NASA pilots and two Army pilots, one of whom was Bob Merrill. Jeanine Weber and Lloyd Corliss of the Army conducted the experiment with me [5]. The fourth simulation experiment examined changes made to the airworthiness criteria, partially as a result of the earlier experiments, and was the first in this sequence to expand by including UK CAA pilot Peter J. G. Harper and FAA pilot Dennis Tuck [6]. The fifth and last simulation experiment in this series examined decelerating approaches and included another FAA pilot, Jim Ericsson [7]. These experiments are summarized in reference [8], and were presented at the 1983 European Rotorcraft Forum [9]. Additionally, Peter and I wrote a general paper on the role of simulation in airworthiness certification studies [10].

These experiments were instrumental in changing the long-standing FAA regulations on longitudinal stability, a first example being a Sikorsky aircraft certification, and in providing the basis for UK CAA special conditions for certification of longitudinal stability, augmented controls, and flight directors. I'm proud of these contributions, but mostly grateful to the fine engineers and super pilots with whom I worked at that time.

OH-6A crash. In 1981, a NASA OH-6A helicopter used for proficiency flying crashed onto the median of Interstate 680 near the Sheridan Road exit, in Sunol, California. Two NASA platform pilots, Dave Barth and "Tex" Ritter, were flying from Moffett Field to Travis Air Force Base, where Tex had National Guard duty. Dave was killed instantly, and Tex was critically injured. Ames immediately put together an Accident Review Board, and I was assigned as the performance/control subject matter expert. Dr. Ed Huff from the Ames Human Factors organization was also assigned, and I think (I don't have our report, as it was confidential) Shorty Schroers of the Army, who was an XV-15 test director, was the Board Chair. Initially, there was no evidence to support a cause of the crash. I therefore basically helped focus attention on contributing factors by eliminating, as far as possible, performance and control elements as causes. We were finally able to obtain a radar flight track from Moffett airfield that helped sort

out the primary cause. I still remember the sorrow at the memorial gathering for Dave at Ridge Winery where members of the San Francisco Symphony Orchestra performed at the request of Dave's girlfriend. This tragedy had a profound influence on me and was later partly responsible for my change of focus at NASA toward the human factors influence on aviation safety.

Teaching at Stanford. Fred Schmitz began teaching a sequence of graduate courses at Stanford on helicopter performance and stability and control, which he devised and “sold” to Stanford, in 1980 or possibly earlier—I can't quite remember. Of all the smart people I knew at Princeton and NASA, Fred was perhaps the most innovative, and he continues to be so to this day. He asked me to teach the second course in the sequence, on stability and control, for the winter quarter in 1982, which I did every other year for the next several years. Fred later expanded the sequence of helicopter courses to include aeroelasticity, taught by Dewey Hodges of the Army, and VTOL handling qualities, taught by Jack Franklin. We taught the classes at Stanford, with a remote feed to Ames so that both Army and NASA people could attend. I especially remember teaching a class on January 28, 1986, and learning that the Space Shuttle Challenger disaster had occurred while I was lecturing and no one at NASA was on the feed. Katie Hilbert, Barbara Sweet, and Dave Schleicher, the young folks we hired during the 1980s, took my course, along with pilots Gordon Hardy and Grady Wilson, as well as other NASA and Army staff. Since my mother and father had both received Ph.D. degrees from Stanford and now lived on campus, they were particularly happy that Fred gave me this opportunity. It was a very rewarding experience.

CH-47 variable stability helicopter. At the same time that I began teaching the graduate course, I was also working with Bill Hindson of Stanford to prepare the CH-47 for flying-qualities experiments, including flying as flight engineer on the computers, so I would frequently arrive at Stanford for the late afternoon class still in my flight suit! Grady Wilson and George Tucker both served as safety pilots during these preparatory flights, and I shared flight-engineer duties with Katie Hilbert and, later, Michelle Eshow and Ernie Moralez. Bill had been a pilot at the Canadian National Research Council and was a very smart pilot/engineer; he deserves all the credit in the world for bringing the CH-47 online and turning it into a very useful research tool. Katie Hilbert conducted a flight experiment of a model-following control system [11], Bill and George Tucker looked at height response characteristics [12], and Bob Chen demonstrated in flight a rotor-fuselage dynamic instability (in roll) that he had predicted using the simulator math model I discussed earlier [13]. I had a lot of fun flying the flight engineer station on many of these experiments. I particularly remember Grady, who'd been an Army CH-47 pilot prior to joining the Army at Ames, doing modified nap-of-the-earth flying over the East Bay hills during our early morning flights out to the NASA site at Crows Landing, with his Mississippi drawl, saying, “I wonder what everyone else is doing this morning.”

Tiltrotor VTOL instrument transition requirements. Starting in 1983, NASA, the FAA, and now the CAA transitioned the rotorcraft IFR airworthiness research from helicopters to commercial tiltrotor aircraft. This program was driven by a request from Bell Helicopter, the developer of the XV-15 tiltrotor for the Army and NASA at Ames, to the FAA for a draft set of airworthiness requirements for this class of aircraft. Initially, we used a generalized math model of the XV-15 and later transitioned to one that was representative of the Marines V-22 Osprey. When Ray Forrest retired, Barry Scott became the lead person at the Ames FAA Office, carrying on from his role in previous STOL certification studies. Bill Decker became the NASA focal point starting around 1985, particularly on the V-22 studies, and Bill Chung joined Tom Alderete to help conduct the simulations.

Barry and I conducted two simulation experiments using the XV-15 model to examine some of the main features to be considered in airworthiness requirements for instrument, decelerating approaches, and transitions of tiltrotors [14]. With the CAA now a full participant, Peter Harper continued to be a primary pilot, along with Ames pilot Ron Gerdes and two new FAA pilots, P. Balfe and J. Arnold. The experiments were conducted on the VMS, and approaches were made to a computer-generated visual representation of an ocean-based oil rig. Michelle Eshow was the simulator engineer; shortly thereafter, she transferred first to Dave Key's Flight Control Division and later to the NASA Ames Air Traffic Management (ATM) organization (Dallas Denery's Aircraft Guidance and Navigation Branch), where she was responsible for the development of a large air traffic database called Sherlock. Major variables considered were 1) how and where in the approach the conversion from "airplane" to "helicopter" mode was made; 2) the width of the conversion corridor (maximum/minimum speed versus thrust angle); 3) pitch/heave coupling during the conversion; 4) stability and control augmentation; and 5) conversion control implementation (thrust angle rate "beeper" speed, replacement of the beeper by selected intermediate thrust angle similar to a flap selector, and fully automated thrust angle conversion depending on commanded speed). These initial investigations were amplified in subsequent experiments using the V-22 model, and the results were compared in reference [15]. Finally, these tiltrotor results were compared with other VTOL results for decelerating approaches in a paper I co-authored with Jack Franklin and Vern Merrick [16].

RSRA/X-Wing control system software. I have mentioned Lloyd Corliss a couple of times already, first in his role starting a wine-tasting group, and then as a partner in the our UH-1H VSTOLAND flight experiment. Lloyd really became a friend during the 1983 European Rotorcraft Forum in Stresa, Italy, particularly when he met me and my wife, Ginny, in Paris with a picnic prepared for our train ride to Stresa, and then at one of the Forum's fancy dinners when he and my wife got to laughing almost hysterically over some food that landed on the floor, bringing silence to the rest of the room. Around 1983, Lloyd left the Army to work for NASA on the RSRA/X-Wing project, a joint NASA-Defense Advanced Research Projects Agency (DARPA) activity where Jim Lane was the NASA Program Manager (PM) and Dr. Ken Rosen was the Sikorsky director. The RSRA was developed at Langley as a flying wind tunnel to test advanced rotor concepts and then transferred to Ames when Ames became NASA's lead center for rotorcraft. The X-Wing, which was intended to operate as a main rotor for hover and then stop for forward flight, certainly qualified as an advanced rotor! The dynamic modeling of such a configuration was very difficult, and the quad-redundant digital software control system was exceedingly complex. Lloyd was the software lead on the project, and he asked me to help as his "hired gun." He and I travelled (initially on Trans World Airlines (TWA)) back and forth to New York and then to Stratford, Connecticut, *many* times over the next couple of years, staying frequently at an old motel on Long Island Sound called Marnick's (I think), and trying desperately to resolve all the problems. We flew on TWA so often that we were constantly upgraded to first class in the Lockheed 1011s they were flying then, and I remember on one occasion the flight attendants gave us two bottles of champagne when we disembarked! As Lloyd notes in his chapter, the X-Wing project ran over budget and over schedule, and was finally cancelled, but I found the experience fascinating, and the friendship that the Ames-Army enterprise enabled continues.

Epilogue 1985–2005

At the end of 1984, Jack Franklin asked to be replaced as Chief of the Flight Dynamics and Controls Branch so that he could concentrate on his own VTOL research. I competed for the job, and I was selected by Greg Condon, the Flight Systems Division Chief, to replace Jack as Chief, starting early in 1985. Greg was an outstanding boss, mentor, and person, and I learned an immense amount from him then and in the years to come. Over the next 6 years, we continued rotorcraft and VTOL research and aircraft development, including the development of a new NASA controls research program called Super-augmented Control for Agile Maneuvering Performance (SCAMP), which we tried to sell to HQ. As I mentioned earlier, I selected both Katie Hilbert and then John Foster to serve as Assistant Branch Chief during those years. By 1991, however, I was increasingly interested in addressing aviation safety issues beyond rotorcraft flying qualities. The Director of Aeronautics, Tom Snyder (also a good man and boss), suggested that I take on a new challenge—leading the Flight Human Factors Branch, which was in the Human Factors Division and had pioneered research in areas such as Crew Resource Management and Fatigue Countermeasures. I took on this role from 1991–1994, during which time I was also 1 of 46 (out of something like 1300 applicants) selected for the first NASA Senior Executive Service (SES) Candidate Development Program (CDP). Interestingly, as he recounts in his chapter, Irv Statler served as the Human Factors Division Chief during part of this time, so he became my boss for a while!

In 1994, the Aeronautics Directorate at Ames underwent a reorganization, one aspect of which was to combine Greg's Flight Systems Division with the Human Factors Division, and Greg asked me to rejoin him as the deputy of the new division. I believe that most interesting and important problems need to be solved at the intersection of disciplines by combining the talents of disparate organizations, and this assignment was a perfect fit in that regard. In fact, in 1995, as part of my SES CDP requirements, I had a development appointment at HQ working for the Associate Administrator, Dr. Bob Whitehead, who had succeeded Wes Harris (see Princeton paragraph on the first page of this chapter) only days before I got there. Together with Dr. Whitehead and Bob Pearce, who later worked for me, we developed an interagency Integrated Project Team (IPT) with the FAA to utilize the ATM research that was being done in Greg's division at Ames. I was the Deputy Division Chief from 1994 until 1996, during which time I concurrently served as the first PM of NASA's Rotorcraft Base R&T program after HQ reorganized all the aeronautics activities. I now had responsibility for the entire NASA Rotorcraft program across the three aeronautics centers; Ruth Martin was the lead at Langley and Jaiwon Shin was the lead at Glenn (7 years later I selected Jai to be my Deputy Associate Administrator when I was at HQ).

Greg retired from NASA at the end of 1996, and I was appointed Division Chief. A year later, however, the new Center Director, Dr. Harry McDonald, reorganized the division again to more strongly emphasize the Information Technology activities, and the Human Factors people were removed. As part of that reorganization, I was promoted to Director of the Airspace Capacity program and the Aviation Operations Base R&T programs, which I led until 2000. I then became Deputy Director of Aerospace at Ames, followed by Associate Center Director for Programs at Ames. In 2002, I was selected as Deputy Associate Administrator for Aeronautics and Space Technology at NASA HQ, and became NASA's Associate Administrator of Aeronautics there in 2003 (following Princeton classmate Wes Harris and previous NASA Ames Center Director Bill Ballhaus—pretty heady company)! During my time there, I worked with FAA Administrator

Marion Blakey and others to develop the interagency Joint Planning and Development Office for ATM, a final manifestation of the interagency IPT from years before, which Norm Mineta, Secretary of Transportation, formally announced in 2004. Jaiwon and I also developed some of the strategy for the return to X-planes, which he has done an excellent job of promoting since he became the Associate Administrator in 2008.

In Memory

To the memory of three pilots who enriched my professional and personal life immeasurably: George Cooper, Ron Gerdes, and Peter Harper. Thank you so much.

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Index

A

Aiken, Ed, 1-2
 Air Traffic Management (ATM), 6-8
 Alderete, Tom, 4-5
 Army Air Mobility Research and Development Command (AMRDL), 2

Army, 1-6
 Arnold, J., 6

B

Balfé, P., 6
 Ballhaus, Bill, 7
 Barth, Dave, 4-5
 Bell Helicopter, 5
 Blakey, Marion, 8
 British Civil Aviation Authority (CAA), 2-6

C

Calspan, 1-3
 Canada's National Research Council, 2, 5
 Cayot, Jack, 4
 Chen, Bob, 2-5
 Chung, Bill, 5
 Condon, Greg, 7
 Cooper, George, 2, 8
 Cooper-Harper Handling Qualities Rating Scale, 2
 Corliss, Lloyd, 2-4, 6
 Cornell Aeronautical Laboratory, 1
 Crows Landing, 5

D

Decker, Bill, 2-3, 5
 Defense Advanced Research Projects Agency (DARPA), 6
 Denery, Dallas, 2, 6
 Dugan, Dan, 2

E

Ericsson, Jim, 4
 Eshow, Michelle, 2, 5-6
 European Rotorcraft Forum, 4, 6

F

Federal Aviation Authority (FAA), 2-7
 Fletcher, Jay, 2
 Flight Simulator for Advanced Aircraft (FSAA), 3
 Forrest, Ray, 4-5
 Foster, John, 2-3, 7
 Franklin, Jack, 1-2, 5-7

G

Gerdes, Ron, 2, 4, 6, 8

Grief, Dick, 2

H

Hall, Warren, 2
 Hardy, Gordon, 2, 5
 Harper, Bob, 2
 Harper, Peter, J. G., 2, 4, 6, 8
 Harris, Wes, 1, 7
 Hilbert, Katie, 2, 5, 7
 Hindson, Bill, 2-3, 5
 Hodges, Dewey, 5
 Huff, Ed, 4

I

Instrument flight rules (IFR), 3-5
 Integrated Project Team (IPT), 7-8

J

Joint Planning and Development Office, 8

K

Key, Dave, 2, 6

L

Lane, Jim, 6
 Lewis, Mike, 2

M

Martin, Ruth, 7
 McCroskey, Jim, 1
 McDonald, Harry, 7
 Merrick, Vern, 2, 6
 Merrill, Bob, 2, 4
 Mineta, Norm, 8
 Moffett Field, 4
 Moralez, Ernie, 5
 Morris, Pat, 2

N

NASA, 1-7
 NASA Ames Aeronautics Directorate, 7
 NASA Dryden Flight Research Center, 2
 NASA Langley Research Center, 3, 6-7

O

Ormiston, Bob, 1

P

Pearce, Bob, 7
 Princeton University, 1-2, 5, 7

R

Ritter, "Tex," 4
 Rosen, Ken, 6
 Rotor Systems Research Aircraft (RSRA), 2, 6

Lebacqz, J.

S

Schleicher, Dave, 5
Schmitz, Fred, 1-2, 5
Schroers, Shorty, 4
Scott, Barry, 4-6
Senior Executive Service (SES), 7
Shin, Jaiwon, 7
Short takeoff and landing (STOL), 4-5
Sikorsky, 4, 6
Singer-Link, 2
Smith, Rogers, 2
Snyder, Tom, 7
stability and control augmentation systems, 4
Stanford University, 1-2, 5
Statler, Irv, 1-2, 7
Super-augmented Control for Agile Maneuvering
Performance (SCAMP), 7
Supersonic transport, 3-4
Sweet, Barbara, 2, 5

T

Talbot, Peter, 2

tiltrotor, 3, 5-6

Travis Air Force Base, 4

Tuck, Dennis, 4

Tucker, George, 2, 5

U

United Kingdom (UK), 3-4

V

Vertical Motion Simulator (VMS), 3, 6

vertical takeoff and landing (VTOL), 2-3, 5-7

VSTOLAND helicopter, 2-3, 6

VTOL Approach and Landing Technology (VALT), 3

W

Watson, Doug, 2

Weber, Jeanine, 2, 4

Whitehead, Bob, 7

Wilson, Grady, 2, 5

Wilson, John, 2

X

XV-15 tiltrotor, 3-6

Y

Yaggy, Paul, 1

The Evolution of Tiltrotor Technology—An Army/NASA Collaborative Effort

Martin Maisel
Army

Like many of my colleagues in the fields of aeronautical engineering and research, I have had a life-long fascination with airplanes. My introduction to “aerodynamics” was obtained by building powered airplane models and participating in model airplane contests in my early teenage years. As a cadet in the Civil Air Patrol, I had my first flight in an airplane at the age of 16.

In 1960, when I graduated from the Polytechnic Institute of Brooklyn with a degree in aeronautical engineering, the United States and the Soviet Union were engaged in a threatening “cold war.” The Soviet Union had recently launched Sputnik 1, an Earth satellite, causing great concern that our enemy had the capability to produce intercontinental ballistic missiles. In the U.S. there was a pronounced effort to catch up to the Soviet’s large missile technology and to develop faster, more capable jet fighters and bombers.



Marty Maisel, 1986.

Hamilton Standard Propellers

Not surprisingly, most of my Polytechnic classmates were anxious to enter the rocket and high-speed jet industries. But not me! I was interested in low-speed aerodynamics, so I applied for a job at the Hamilton Standard Division of United Technology Corporation in Connecticut. Hamilton Standard was the nation’s largest producer of airplane propellers. What could be more “low-speed aerodynamics” than propellers? I was assigned to the Propeller Aerodynamics group and was initially involved in the design and performance analysis of propellers for conventional aircraft such as Lockheed’s C-130 and P-3 where the critical flight conditions were usually cruise, climb, and perhaps, loiter.

In the early 1960s there was a growing interest in vertical takeoff and landing (VTOL) aircraft, primarily for military applications, and at Hamilton Standard we began to assess our ability to predict performance in the “static” (i.e., hover) condition. We modified the decades-old Goldstein-Lock analysis and proceeded to design propellers for the Ling-Temco-Vought XC-142A tiltwing and the Bell Aerospace X-22A ducted-propeller VTOL aircraft. We soon discovered that the accurate prediction of propeller hover performance was more challenging than expected, and we consulted with Theodore Theodorsen, former National Advisory Committee for Aeronautics (NACA) aerodynamicist, to improve our methodology.

Boeing Helicopters

In 1967 I was invited to join the Research Aerodynamics unit at the Helicopter Division of the Boeing Company (previously called Boeing Vertol). Boeing had recently established the unit to explore a variety of advanced VTOL configurations. The small research group included Gary Churchill, who focused on flight dynamics and control law for the advance aircraft, and John



Hover performance test of a 13-foot-diameter rotor mounted on the AFAPL propeller test stand.

Magee, who worked on refining the propeller/rotor performance code recently developed at Boeing [1]. More about Gary and John later.

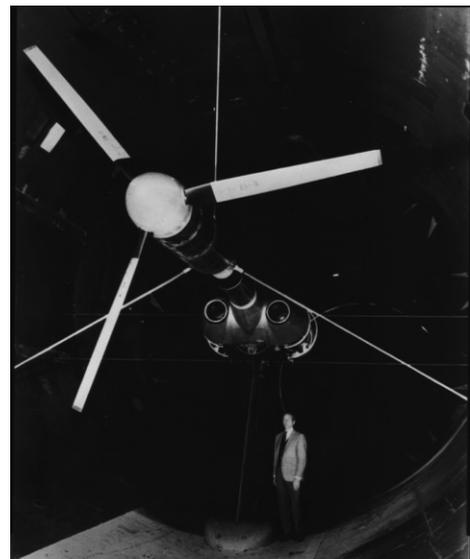
The studies at Boeing focused on variations of tiltwing and tiltrotor aircraft. As the designs progressed it became necessary to validate the analytical predictions of performance, loads, flight dynamics, and aeroelastic stability by conducting a variety of wind tunnel tests and piloted simulations. The design of the rotor for Boeing's proposed Model 160 tiltrotor aircraft was particularly challenging because of the significant differences in inflow and blade loading between the critical, highly loaded hover condition and the lightly loaded cruise-mode design point. I was given the task of providing empirical verification

of predicted performance in the hover and airplane (cruise) modes of flight.

In reviewing prior propeller/rotor hover test data, it became clear that many prior tests were unsuitable to validate full-scale performance because of their small size. While tests of small-scale propellers are less costly, the lift and drag characteristics along the blades are adversely affected by the low sectional Reynolds Numbers. Since cost was a limiting factor, it was necessary to select the smallest model diameter that would yield hover performance results near full-scale values.

A 13-foot-diameter model was selected to provide adequately high Reynolds Numbers to obtain near full-scale rotor hover performance, and static tests were conducted at the Wright Air Development Center (WADC) Air Force Aerodynamic Propulsion Laboratory (AFAPL) propeller test stand in early 1968. A shaft extension one diameter in length was used to minimize the "ground effect" of the massive test rig on the rotor inflow. Three blade twists were tested to assess the ability of the performance analysis to properly predict the effect of blade loading variations.

Employing the same model rotor for cruise-mode testing required the use of a wind tunnel that was large enough to minimize the influence of the tunnel walls on the airflow and was capable of producing high airspeeds. The Office National d'Etudes et de Recherches Aérospatiales (ONERA) S-1 wind tunnel near Modane-Avrieux, France, met those requirements, and high-speed tests were performed in May of 1968 [2].



Cruise performance test of a 13-foot-diameter rotor, ONERA S-1 wind tunnel, Modane-Avrieux, France.

An agreement between NASA and the U.S. Army Materiel Command established in 1965 provided for the joint (Army/NASA) participation in the development of advanced aircraft technology. Under this Army-NASA Joint Agreement, an effort to address an array of tiltrotor aircraft aeromechanical issues and deficiencies that had surfaced during flight and wind tunnel tests of the (first-generation tiltrotor aircraft) XV-3 was promoted by Paul Yaggy, Director of the Army Air Mobility Research and Development Laboratory (AMRDL) and Woodrow “Woody” Cook, NASA Ames Advanced Aircraft Programs Office.



Bell XV-3 tiltrotor aircraft.

The most challenging aeromechanical problem, and perhaps the principal technical barrier to the success of the tiltrotor concept revealed by the XV-3 program, was the whirl stability of the rotor, pylon (nacelle), and wing when flying in the airplane mode. The elastic properties of that structure, coupled with nonaxial forces produced by the large rotors, caused the pylon and wing to bend and twist. That condition could lead to a divergent instability with the potential to destroy the aircraft.

Other deficiencies included extremely poor hover and cruise performance. The XV-3 could not match the capabilities of production helicopters of the day. The tiltrotor would have to provide significant performance advantages for it to be viable.

Ames issued a Request for Proposal (RFP) to conduct additional tiltrotor model performance tests and analytical predictions, and we quickly produced a proposal at Boeing. In order to deliver the proposal to the government before the deadline, I arranged to meet Paul Yaggy at Dulles Airport as he was about to return to California from a trip to Washington, D.C. During a brief conversation, Paul mentioned the development of the Ames-Army Lab and indicated that there might be an opportunity for me there. At that time I was having a blast at Boeing and was not considering leaving.



A 5-foot-diameter rotor test in the Army 7- by 10-Foot Wind Tunnel.

Boeing was awarded the contract that called for additional experiments in the ONERA S-1 wind tunnel. Under that contract the 13-foot-diameter rotor was also tested in the NASA Ames 40- by 80-Foot Wind Tunnel. To evaluate model scale effects, 5-foot-diameter models (of the same configurations as the 13-foot-diameter rotors) were tested in the Army 7- by 10-Foot Wind Tunnel at Ames on Dave Sharpe’s propeller test rig.

The Contracting Officer's Technical Representative (COTR) for that contract was Wallace "Wally" Deckert at NASA Ames. Wally accompanied the Boeing test team for the tests in France, and one of the nontechnical highlights of that trip for me was when Wally and I were invited to the Paris home of Philippe Poisson-Quinton for dinner. The delightful "PQ" was a highly regarded aerodynamicist and served as the director of ONERA's international affairs with regard to aerodynamics.

The Air Mobility Research and Development Laboratory

By mid-1970 an economic downturn had affected all of Boeing's divisions, and all the members of my propeller/rotor aerodynamics research group were laid off. With an uncertain future at Boeing, I called Paul Yaggy and asked if he was still interested in having me join the Army unit at Ames. He answered affirmatively, and in early September 1970 I packed my pregnant wife and our three young boys into our Ford station wagon and drove across the country.

The road trip went smoothly, but our furniture did not fare as well. The moving truck driver showed up with a young lady who mentioned that she was going to be taught how to drive the truck. It seemed a bit odd, but I felt it was "not my problem."

We showed up in California, closed on a new house in San Jose, and contacted the moving company. They said there would be a slight delay so we waited a few days. After further excuses from the moving company, I asked Alice Meyers, Paul Yaggy's secretary, for help. She found out that the driver never showed up, and the moving company was reporting the truck, with my furniture, as stolen. Since I had a house with no furniture, Dave Sharpe kindly provided a folding cot and a small TV to help us get by temporarily.

Within a few days we got word that the truck was located in Las Vegas, where it was impounded, and the truck driver was arrested. We got our furniture released, transferred to another truck, and delivered with some items missing and some things we had never seen before! Oh—there was one more problem: the hijacked truck did not have room for one last mattress so it was strapped to the back of another truck from the same company. Unfortunately that truck encountered a major rainstorm en route, and the mattress arrived soaking wet and severely mildewed.

V/STOL Projects Office

At Ames I was assigned to a NASA branch (later called the Vertical/Short Takeoff and Landing (V/STOL) Projects Office) headed by Woody Cook. Assistant Branch Chief Wallace Deckert had been the project engineer during tests of Bell's XV-3 tiltrotor aircraft at Edwards Air Force Base in 1959 [3] and, in spite of the XV-3's deficiencies, he was also an advocate of the tiltrotor aircraft. Jim Weiberg, an experienced NASA wind tunnel test engineer, was the other start-up member of this team charged with establishing that all known tiltrotor aeromechanic and performance issues had been addressed and resolved—short of proof-of-concept flight tests.

In 1971 Gary Churchill, my co-worker from the Boeing Helicopter Research Aerodynamics unit, joined AMRDL and became the third member of the team. Gary had an extensive background in VTOL flight control system development and would take on that role in the NASA/Army tiltrotor program.

To resolve the technically challenging problem of the whirl-mode aeroelastic instability revealed by the XV-3, Earl Hall [4], Kip Edenborough [5], and Troy Gaffey [6,7] at Bell performed pioneering investigations in that field. However, the work of Army engineer Dr. Wayne Johnson [8] firmly established that we had sufficient confidence in our analytical tools to ensure successful exploitation of the tiltrotor VTOL approach. Wayne's methodology was borne out by the subsequent success of the Bell and Boeing semi-span wing tests in the 40- by 80-Foot Wind Tunnel prior to initiation of the full XV-15 program.

With confidence that the technical issues were adequately understood and resolved, contracts were awarded to Bell and Boeing to conduct preliminary tiltrotor aircraft design studies. Bell's approach employed a gimbaled rotor system that was tested for aeroelastic stability in the Ames 40- by 80-Foot Wind Tunnel in 1969, followed by similar testing of Boeing's hingeless rotor design in 1972.

Tilt Rotor Research Aircraft Project Office and the XV-15

A new organization, the Tilt Rotor Research Aircraft (TRRA) Project Office, was established in May 1972. NASA experimental flight test engineer David Few led the office, and Dean Borgman, AMRDL, was deputy project manager.

By late 1972, with engineers in industry and government reporting that all critical aeromechanics issues were in hand, top-level aeronautics managers at NASA Headquarters promoted the development of a tiltrotor proof-of-concept aircraft to NASA Administrator, Dr. James Fletcher, and Deputy Administrator, Dr. George Low. It was decided that if Army support was obtained, approval for the project would be granted.

Obtaining Army funding appeared to be a formidable task; the Army's Assistant Chief of Staff for Force Development, Lieutenant General Bob Williams, who set policy for aviation research, had previously stated that he was opposed to further tiltrotor development after the XV-3 apparently failed to prove its value. In an effort to convince Lieutenant General Williams of the recent technological developments and of the potential merits of the tiltrotor, Paul Yaggy presented a briefing prepared by Dave Sharpe and Dean Borgman. Shortly after that meeting, Williams issued a letter stating that he was reversing his prior opposition to tiltrotor research and directing his staff to provide full support to the tiltrotor effort. Army funding was soon made available, and additional funds were programmed into succeeding year Army budgets. The TRRA project was a "go."

Around that time the TRRA Project Office gained additional Army personnel. Laurel "Shorty" Schroers had prior rotorcraft flight test experience, and Lieutenant Colonel Dan Dugan, designated as the project pilot, would provide guidance in areas related to flight safety and crew station design.

Prior to the development of the research aircraft, the TRRA Project Office staff was required to prepare several documents for top management approval. One of the key documents was the NASA/Army Project Plan that described the technical objectives of the project, defined major program elements, provided a management plan, and specified government funding, facilities, and manpower requirements for the duration of the project. After several iterations to reduce cost by reducing scope, the TRRA Project Plan was approved by Bruce Holloway, NASA's Acting

Associate Administrator for the Office of Aeronautics and Space Technology, and Norman Augustine, Assistant Secretary of the Army, Research, Development, and Engineering.

The initial phase of the project involved aircraft design and analytical studies of performance, noise, stability and control, and structural loads and dynamics. Of the four responses to the RFP, Boeing Vertol and Bell were selected to receive fixed-price contracts for the design studies. As a result of those contracts, the companies produced aircraft designs in January 1973; each 12-volume design study weighed about 30 pounds. These designs would serve as each company's proposal to fabricate and flight test two tiltrotor research aircraft.

Members of the TRRA Project Office and other Army and NASA engineers participated in the Source Selection Evaluation Board proceedings, and in April 1973 Bell Helicopter Company was selected for the "build and fly" Tilt Rotor Research Aircraft Phase II activity.

Additional changes in the TRRA Project Office staff occurred in 1974. Dean Borgman departed and was replaced by Army aviator Lieutenant Colonel James "Jim" Brown, thereby maintaining the joint NASA/Army lead management positions. When Dave Few was promoted to Deputy Division Chief of NASA's V/STOL Technology Division, Brown took over as the TRRA project manager and Mike Carness (NASA) served as deputy manager.

Around that time, John Magee, a former co-worker of mine in the Boeing Research Aerodynamics unit, joined the Government Project Office. While at Boeing, John had been a Principal Investigator in numerous tiltrotor studies and had managed the 1972 test of Boeing's 26-foot-diameter rotor in the Ames 40- by 80-Foot Wind Tunnel [9].

As Bell began to design their Model 301 proof-of-concept tiltrotor aircraft, NASA engineer Jim Lane was appointed as Government Resident Project Manager and relocated to the Bell engineering plant in Hurst, Texas.

The Bell Model 301 required a government designation. Initially we were told it would be called the XV-14 (I think the Air Force controlled X-aircraft designations and I guess 14 was the next number in the XV series). The project office responded that XV-14 was not a good choice because the Bell X-14 was currently flying at Ames, and the similarity of designations might lead to a safety problem. Okay, let's call it the XV-15. At that time NASA's North American X-15 hypersonic research aircraft was no longer in flight status so the tiltrotor proof-of-concept aircraft type became known as the XV-15.

By the mid-1970s the staff of the project office had grown to about 12 Army and NASA engineers. The joint team benefited from the personnel resources of both organizations in obtaining the experience and expertise required to make the project a success. The project office performed as a highly functional team, fully dedicated to a common goal—to prove the viability of the tiltrotor aircraft. Furthermore, it is unlikely the TRRA project would have been initiated without the joint funding provided under the Army/NASA Joint Agreement.

Each engineering member of the project office was assigned responsibility for a number of disciplines or design elements and would maintain close contact with their counterpart at Bell. My tasks included aerodynamic performance (rotor and airframe), acoustics, wind tunnel test, systems tests, and the hydraulic, fuel, electrical, and environmental control systems.

Throughout the mid- and late-1970s the TRRA Project Office staff members were frequent visitors to the Bell facility in Texas. During the engineering phase there were numerous progress and design reviews. When system functional tests were conducted, government witnessing and sign-off was required.

Ground Tests

I remember Shorty Schroers telling about the events preceding the “zero-zero” (i.e., ground-level, zero-forward-speed) ejection seat test at the Rockwell International, Tulsa, Oklahoma facility (where the fuselage was fabricated). The design allowed both side-by-side pilot and copilot seats to eject simultaneously through the overhead cockpit window. The test was to verify that the seats (with 95 percent anthropomorphic test dummies—dressed in flight suits and helmets—strapped in) would safely clear the window frame and surrounding structure. To determine if the seats or dummies struck the airframe, the interior structure and windows were marked with various color shades of a substance that would be transferred to the dummy upon contact; the “substance” chosen was lipstick. In what might have been one of the most unusual moments of the TRRA project, Shorty and his team of engineers and technicians went to a local cosmetic store to purchase a large quantity of various shades of lipstick. It is hard to imagine what the salesperson must have been thinking.

Prior to flight operations, each of the XV-15 aircraft was subjected to an extensive series of ground runs to check out the transmissions, engines, conversion mechanisms, and rotor systems. To perform these tests, Bell had constructed an elevated test stand that allowed the rotors to be operated in the airplane mode. On September 2, 1977, during the start of a ground run, the pilots noticed a caution light indicating that the cabin door bolt was not in the fully latched position.



XV-15 N702NA on the Bell elevated tiedown test stand.



Simultaneous static test firing of XV-15 ejection seats.

That caution light had previously been the source of numerous nuisance indications that were suspected to be due to the improper setting of the latch bolt microswitch. The position of the door handle was checked, found to be in the correct position, and the engine start procedure was continued. After a few minutes, with the rotors in the airplane mode and while increasing power, the cabin door opened and was struck by the blades of the right rotor, scattering pieces of aluminum over the aircraft. I was in the control room when we saw the right rotor loads go off scale and heard the pilot say, “We just had a blade strike.” The Bell test director in the control room responded, “What’s a blade strike?”

A new door was fabricated with an improved latching system, and two of the rotor blades were restored to a flightworthy condition. One blade was damaged beyond repair.

I noted earlier that two XV-15 aircraft were built. As the aircraft neared completion, it became apparent that the government did not have the funding resources and personnel to operate both aircraft. Following negotiations between the government and Bell, the contract was modified to allow Bell to operate one XV-15 (tail number N702NA) for military evaluations and demonstrations at no cost to the government. The agreement was later changed to a bailment that made Bell responsible for the day-to-day airworthiness accountability of N702NA. The other aircraft, N703NA, would be operated by the Ames TRRA Project Office to acquire tiltrotor performance, loads, stability and control, and acoustics and aeroelastic data.

XV-15 Wind Tunnel Test

Prior to flight envelope expansion of the XV-15, the Government TRRA Project Plan called for a wind tunnel test “to be certain all critical mode analyses are valid and that the analytical methods properly assess the dynamic characteristics, capabilities, limitations and operating behavior of the tiltrotor flight research aircraft.” I had the feeling (although it was never substantiated) that establishing a requirement for a wind tunnel test of the XV-15 was motivated by supporters of the Ames 40- by 80-Foot Wind Tunnel who were concerned that the high cost of operating that large facility might make it a target for closure in the event of future budget reductions. Bell had not advocated a wind tunnel test of the XV-15 aircraft prior to flight.

Provisions for the wind tunnel entry built into the XV-15 included “hard points” under the wings and at the tail to mount the aircraft on the wind tunnel struts, and remote control devices for the engines and flight controls. Connections for external fuel, electrical, and hydraulic power sources were also installed. Jim Weiberg was the lead test engineer for the May through June 1978 wind tunnel test entry [10]. I was Jim’s assistant.

The tests revealed a number of issues, the most significant being the high empennage loads that occurred in helicopter forward flight and in portions of the conversion flight mode. It was determined that the empennage loads were caused by the aerodynamic excitation of the vertical “H” tail surfaces arising from the close proximity of the inboard rotor tip vortices in those flight conditions. After the wind tunnel test, changes were made to the empennage retention structure to accommodate the higher-than-expected loads. In the end, the wind tunnel tests served an important purpose.



XV-15 in the Ames 40- by 80-Foot Wind Tunnel, May–June 1978.

Further changes to the Government Project Office leadership occurred in 1979 and 1980 when John Magee became the project manager and Army aviator Lieutenant Colonel Clifford “Cliff” McKiethan served as deputy project manager.

As the XV-15 began to demonstrate the potential benefits of tiltrotor aircraft during its flight test program, Lieutenant Colonel McKiethan and I visited several military sites to brief senior staff about the new technology. During a visit to Ft. Bragg, North Carolina, the home of the Army’s

Airborne and Special Operations Forces, we entered an elevator to get to the conference room on the third floor. I was holding a small XV-15 model that we usually brought along to help illustrate the tilting rotors. When the elevator stopped at the second floor, a Major holding a model tank got in. I had to restrain myself from strafing the Major's tank with my XV-15 model.

We also briefed the General in command of the El Toro Marine Corps Air Station in Southern California. I think the General must have been a fixed-wing pilot because, when we showed him that the tiltrotor's flight envelope included high-speed airplane-mode flight, he said, "That's great, we need to get rid of those damn complex helicopters." I didn't have the courage to tell him that the tiltrotor would be just as complex as the Marines' helicopters.

The Paris Air Show

By early 1981, the flight envelope of the XV-15 had been sufficiently explored in all flight modes to provide at least a first-order verification of all design-critical parameters. The NASA/Army Project Office and Bell decided that the emerging tiltrotor technology should be demonstrated to a broad aviation community. The venue for this public debut would be the renowned Paris Air Show. Participation of the XV-15 at the Paris Air Show was a cooperative effort between NASA, the Army, the Air Force, and Bell. Dr. Irving Statler, Director of the Army Aeroflightdynamics Directorate (AFDD) enthusiastically supported this activity. Further support was provided by then Secretary of the Air Force, Dr. Hans Mark, who previously served as Director of NASA Ames Research Center when the TRRA project was initiated. Dr. Mark arranged for transport of the XV-15 and support equipment to Farnborough, England, onboard a U.S. Air Force Military Airlift Command C-5A and C-141 transport aircraft.



XV-15 flying from England to the Paris Air Show, Le Bourget, France, May 1981.

The XV-15 was reassembled at Farnborough, tested, and ferried to Le Bourget on June 1, 1981. At the air show it was the only aircraft to perform its flight routine daily, even when a wheels-up landing of a transport aircraft closed the airfield's only runway—the XV-15 simply took off vertically. The XV-15 was reported to be the "darling" of the 1981 Paris Air Show and received significant attention from dignitaries, journalists, and international aviation leaders.

At the Paris Air Show TRRA Project Pilot Dan Dugan shared flight demonstration duties with Bell's Dorman Cannon and Ron Erhart. Dan shares some of his experiences during that event in his chapter.

Senator Barry Goldwater (Republican, Arizona), then Chairman of the U.S. Senate Armed Services Committee, was among the enthusiastic observers. Senator Goldwater, a former military pilot, requested and was granted a flight demonstration in the XV-15 at Bell and became one of the first non-test-pilots to fly in the experimental aircraft. A few months after Senator Goldwater's flight, Secretary of the Navy John Lehman, who also witnessed the XV-15 at the Paris Air Show, flew in the Bell-operated N703NA at Quantico, Virginia. Both Senator

Goldwater and Secretary Lehman were instrumental in obtaining Congressional support for the future acquisition of a military tiltrotor aircraft.

Just prior to Senator Goldwater's flight, the NASA/Army TRRA team at Ames conducted its first guest pilot demonstration for General Story Stevens, Commander, U.S. Army Aviation and Troop Command; nearly 420 guest pilot demonstrations would follow.

Additional remarks about the XV-15 at the Paris Air Show are given in Dr. Hans Mark's Foreword and in Dr. Irv Statler's chapter.

Navy Carrier Suitability Tests

When the TRRA program faced funding shortfalls in the early 1980s, the Army and NASA sought a new partner to continue flight activities. Under a NASA/Army/Navy agreement, arrangements were made to conduct a carrier suitability test in exchange for the infusion of Navy funds into the XV-15 project. When word got out about the planned test, there were reports claiming that the tiltrotor aircraft was unsuitable for carrier operations because of the "deck edge effect." The reports claimed that when the tiltrotor aircraft moved laterally to land on the carrier, one rotor would be over the deck and the other would be over the edge of the ship. The resulting difference in the "ground effect" on the thrust of the rotors would cause an uncontrollable roll.



XV-15 Tiltrotor carrier suitability tests aboard the USS Tripoli, August, 1982.

Well, it turned out that I had recently conducted a series of hover performance tests to investigate the variation of rotor thrust with height over the ground. The test conditions varied from a 2-foot wheel height (where the rotor was considered in-ground-effect) to a 50-foot wheel height (an out-of-ground-effect condition). The data from that test showed that only a slight lateral movement of the cyclic control stick would correct for the differences in ground effect encountered as the tiltrotor aircraft moved laterally over the carrier deck. Furthermore, the required cyclic control adjustment was essentially within the normal

"dither" of the flight control during hover operations. The "deck edge effect" did not exist, and tests onboard the Navy amphibious assault ship USS Tripoli in August 1982 revealed no adverse flight conditions. Navy Lieutenant Commander John Ball, who flew the carrier suitability test with Bell pilot Dorman Cannon, reported that he "was struck by how easy and just plain fun it was to control" [11].

Rotor Hover Performance and Download

In 1984 a series of full-scale rotor hover performance tests was conducted on the NASA Ames Outdoor Aerodynamic Research Facility [12]. The XV-15 rotor had a higher Figure of Merit (a measure of static thrust/power efficiency) than previously thought. This, coupled with data from earlier XV-15 hover [13] and tiedown tests, led to the determination that the magnitude of thrust loss due to the presence of the wing below the hovering rotor disc ranged from 13 to 16 percent, as a function of flap position. At the time of the design of the XV-15, the magnitude of the download was believed to be about 5 to 7 percent of the rotor thrust [14]. The apparent inconsistency spawned new tests to better comprehend the download issue.

I initially conducted flow visualization tests using tufts on the upper wing surface. Those tests showed a significant spanwise flow over the inboard half of the wing in hover and low-speed flight conditions. Additional flow visualization tests were conducted using smoke, with the aircraft mounted on an elevated tiedown test stand at Ames. Photos of the smoke revealed that the opposing airflows from both rotors converged over the longitudinal center of the fuselage, creating a previously unknown vertical “fountain.” The large fountain flow contributed significantly to the download. The higher rotor thrust combined with a higher download loss, due to the previously unknown fountain, now made sense.



Early morning preparations for the XV-15 rotor hover performance test on the NASA Ames OARF, 1985.

The smoke tests also showed that, near the rotor hub, the flow appeared to be nearly chordwise, with air spilling over the leading and trailing edges of the wing. This condition was later explored by testing various two-dimensional wing configurations at inflow angles near -90 degrees [15]. The test was conducted in the Army 7- by 10-Foot Wind Tunnel. Georgene Laub of the Army lab and I shared principal investigator duties. Dr. Jim McCroskey provided additional guidance in this investigation and also reported on the topic [16].

Discussing the smoke test brings to mind an incident I encountered during preparations for that investigation. To generate large volumes of smoke, I ordered a number of smoke grenades. The test apparatus consisted of a heat-insulated box into which the smoke grenade would be dropped, a blower at the outlet of the box, and ducting leading from the blower to the top of the wing. Since this was to be a low budget test, Jim Weiberg generously provided his electric leaf blower to pump the smoke. During the first test of the smoke apparatus, everything worked fine—for a short while. After seeing large volumes of smoke initially emerge from the duct, the sound of the leaf blower’s motor changed from a roar to a high-pitch squeal, and smoke started flowing from the box instead of the end of the duct. Following a fast shutdown, we discovered that the leaf blower was equipped with a plastic fan that had melted into a glob from the heat of the smoke grenade. Although the meltdown was humorous (I couldn’t stop laughing when I saw the melted leaf blower), I felt bad and bought Jim a new one. I then obtained a commercial blower with metal blades, and the smoke system worked flawlessly for the remainder of the test activity.



Flow visualization smoke test illustrating the fountain flow over the fuselage center.



Flow visualization smoke test illustrating the chordwise flow near the wing tip.

Final XV-15 Flight Operations at Ames

By the late 1980s both Gary Churchill and Shorty Schroers had passed away, and John Magee accepted a position at Bell. The flight research operation of the XV-15 at Ames was then conducted under the Rotorcraft Flight Investigations Branch led by William “Bill” Snyder. In 1991 I served as acting chief of that branch and as the COTR for the continuing XV-15 contract with Bell.

At that time all technical proof-of-concept objectives identified in the 1972 NASA/Army Project Plan had been successfully achieved. Bell had also conducted effective demonstrations of the tiltrotor aircraft in numerous military and civilian scenarios, thereby accomplishing many of the project plan’s advanced objectives.

The XV-15 at Ames now required a major transmission overhaul and extensive inspection but, because of a lack of funds for the overhaul, it was placed into temporary storage at Ames with no specific technical goals scheduled.

On August 4, 1992, I received a call from Bell informing me that XV-15 N702NA had crashed during a demonstration flight for Eurocopter’s chief experimental test pilot. While in a low hover, the aircraft entered an uncommanded rapid roll resulting in the right rotor blades and engine nacelle contacting the ground. The impact fractured the wing at the root, and the airframe came to rest inverted. Except for a crushed nose section, collapsed vertical fins, and a gash from a rotor blade, the fuselage and cockpit area received little damage. The inverted position of the fuselage prevented the use of the ejection seats, but ground personnel were able to rescue the crew by removing sections of the side window Plexiglas. Thankfully, both pilots received only minor injuries.

An accident investigation conducted by the National Transportation Safety Board determined that the loss of control was due to a nut backing off from the rotor collective angle actuator linkage [17]. Furthermore, it found that the critical nut was not secured by a cotter pin. The accident was caused by human error and not because of any inherent characteristics of the tiltrotor aircraft.

With the government XV-15 flight test program at a standstill, and with further military evaluations and pilot demonstrations still being planned, N703NA was disassembled and transported to Texas so Bell could continue their work. The forward section of N702NA was also turned over to Bell where it was subsequently employed as a fixed-base tiltrotor simulator cockpit.

Flight in the XV-15

Prior to Bill Snyder’s retirement, I contacted Bell to see if we could arrange for Bill to get a demonstration flight in the XV-15. They agreed and invited me to come along for a ride as well. On March 27, 2001, Bill and I were thrilled to become XV-15 guest pilots, numbers 308 and 309. Over 100 more guest pilots would follow.

National Air and Space Museum

In early 2002 I received a call from Roger Conner, Curator of the National Air and Space Museum (NASM) Vertical Flight Collection. Roger had requested that the remaining XV-15

be transferred to the museum when it completed its flight program. I began to work on the transfer procedures prior to my retirement later that year.

Bell continued to fly the XV-15 until 2003. At that time they were immersed in the V-22 production program and were developing the Model 609 commercial tiltrotor aircraft. The XV-15 had served well, and it was time for it to be retired.

In July 2003, 10 months after I retired from the Army, I received a contract to facilitate the transfer of XV-15 N703NA from NASA (it was on the NASA property book) to the NASM in time for the grand opening of the museum's new Udvar-Hazy facility located at Washington Dulles airport. Michael Rutkowski was the NASA Ames COTR for that contract. To accomplish that task, I worked with personnel at Ames, NASA Headquarters, and Bell. On July 28, 2003, Ed Aiken, Chief of the Army/NASA Rotorcraft Division at Ames, issued a memo declaring that the XV-15 had exhausted its programmatic utility and, recognizing its historical significance, recommended its transfer to the NASM.

Plans were made with Bell to deliver the aircraft to Dulles and to terminate the long-running Bell/Army/NASA Memorandum of Agreement associated with the aircraft. The XV-15 was to be one of the few aircraft on display at Udvar-Hazy that actually flew in to the facility. After the aircraft arrived at Dulles, arrangements were made with Bell and a local Navy installation to remove all hazardous material (such as the ejection seat cartridges, the window primer cord, fuel and hydraulic oil) prior to putting the aircraft on display.



XV-15 Tilt Rotor Research Aircraft N703NA at the NASM.

On December 15, 2003, I was Roger Conner's guest at the grand opening of the Udvar-Hazy annex of the NASM. The XV-15 remains there on permanent display in the shadow of the Concorde Supersonic Transport.

XV-15 History Monograph

Prior to my retirement, I collaborated with pilot Dan Dugan and NASA engineer Demo Giulianetti in writing *The History of the XV-15 Tilt Rotor Research Aircraft, From Concept to Flight* [18]. The report was published as a NASA Historic Monograph. While I had Paul Yaggy review the document for its Army content, I'm embarrassed to say that I failed to submit it to my Army management prior to publication. I quickly learned that was a mistake.

JVX Study and the V-22 Osprey

In 1982 the U.S. Army, the Air Force, and the Marines, seeking a replacement for the venerable, but aging, CH-46 helicopter, conducted a 4-month study to identify the VTOL vehicle that would be best suited for current and future vertical lift missions. Teams consisting of scientists, engineers, and military personnel from each military service and from NASA convened at Ames Research Center to evaluate conventional helicopters, compound helicopters (with auxiliary propulsion), fan-in-wing aircraft, and tiltrotor aircraft for performance, operational effectiveness,



The Bell-Boeing V-22 Osprey in helicopter-mode flight over Dubai.

cost effectiveness, life-cycle costs, combat survivability, and maintainability. Each parameter was assessed for a number of diverse military missions.

John Magee led the tiltrotor study effort. As a member of the team, I was responsible for evaluating the aerodynamic performance of the preliminary design vehicle we selected for the competitive evaluation.

The study concluded that the tiltrotor aircraft best met the requirements, and Congress subsequently authorized funding to initiate the design and development of the Joint Vertical Experimental (JVX) tiltrotor aircraft. The JVX was later designated the V-22, and the aircraft was designed and built by a team of former competitors, Bell and Boeing.

Army Rotorcraft Operations at Ames

In 1997 NASA management decided to consolidate its flight operations at the Dryden Flight Research Center, and the NASA Ames aviation assets were ordered to be relocated. The NASA decision had an unexpected impact on the Army's flight research program, which had previously employed Ames resources for modification and maintenance of their Army research rotorcraft under the existing Joint Agreement. To continue the rotorcraft flight programs, the Army AFDD established the Flight Projects Office (FPO), and the helicopters were moved to the NASA Ames Hangar, Building N-248.

I was assigned to the FPO and tasked with the duties of Airworthiness Officer and COTR for the rotorcraft maintenance contractor (a division of the Raytheon Company). I also managed the funds allocated for the maintenance and operation of the Army helicopters.

Army aviator Lieutenant Colonel Chris Sullivan, new to Ames, was appointed as FPO Chief and took on the challenge of establishing all relevant procedures. To assist Chris, Master Army Aviator Warrant Officer Richard Huber joined the FPO team. Looking back it seems that the FPO enabled the continuation of the ongoing helicopter flight research programs without a significant interruption.

I got into a bit of hot water during my tenure at the FPO. The regional parts depot for Army helicopters was in Ft. Lewis, Washington. We had authorized Raytheon to order parts as required, and I would make payment through NASA's financial system. This worked fine for a while until Ft. Lewis decided that, since they were an Army unit, they wouldn't accept funding from NASA. My attempt to negotiate a solution failed. I had two choices: stop ordering parts and put a halt to the rotorcraft flight research, or continue to acquire parts and work out a payment method later. I chose to have Raytheon order parts.

After several months, the head NASA Ames financial officer became aware of the issue. By that time we had amassed a bill of nearly \$2.5 million (a rotor blade cost over \$100K and a transmission over \$250K). Needless to say, he was very unhappy with me and explained that

obligating government funds required an authorized “warrant,” which I didn’t have. I was in violation of some Federal code. I explained that I had the funds but did not have a way to get payment to Ft. Lewis. Eventually a method of payment was arranged and I did not go to jail.

I have had the privilege of working for many highly capable superiors, but I hold a special feeling for the positive “can do” attitude, competence, leadership skills, and friendliness of Lieutenant Colonel Sullivan.

Closing Remarks

Jimmy Doolittle, aviator, scholar, hero, and American patriot, titled his autobiography, *I Could Never Be So Lucky Again* [19]. Doolittle captured my sentiments precisely. What more can I say?

At the Ames-Army Lab and at NASA Ames Research Center I found myself surrounded by world-class scientists and engineers. I was apprehensive at first, but soon discovered that these people who I admired for their contributions to rotorcraft technology were also a friendly and helpful bunch. I would be remiss if I did not mention my sincere appreciation for the gentle guidance of my Army supervisors throughout my career at Ames. Paul Yaggy, Andy Morse, Bob Ormiston, and Irv Statler allowed me to find my way and provided support when needed, without undue interference.

My fascination with aviation remains as strong as ever. I got my private pilot license in 1966, I recently completed a plans-built aircraft, and I still enjoy building model airplanes. I also serve on the Board of Directors of a local organization that advocates for the interests of our local airport and provides education scholarships for students interested in aviation.

I Could Never Be So Lucky Again.

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Index

A

Aeroflightdynamics Directorate (AFDD), 9, 14
Air Force Aerodynamic Propulsion Laboratory (AFAPL), 2
Air Mobility Research and Development Laboratory (AMRDL), 3-5
Augustine, Norman, 6

B

Ball, John, 10
Borgman, Dean, 5, 6
Brown, Jim, 6

C

Cannon, Dorman, 9, 10
Carness, Mike, 6
Churchill, Gary, 1, 4, 12
Conner, Roger, 12-13
Contracting Officer’s Technical Representative (COTR), 4, 12-14
Cook, Woodrow, 3-4

D

Deckert, Wallace, 4
Doolittle, James, 15
Dugan, Dan, 5, 9, 13

E

Edenborough, Kip, 5
Erhart, Ron, 9

F

Fletcher, James, 5
Flight Projects Office (FPO), 14
Ft. Bragg, 8
Ft. Lewis, 14-15

G

Gaffey, Troy, 5
Giulianetti, Demo, 13
Goldwater, Barry, 9-10

H

Hall, Earl, 5
Holloway, Bruce, 5
Huber, Richard, 14

J

Johnson, Wayne, 5
Joint Vertical Experimental (JVX), 13-14

L

Lane, Jim, 6
Lehman, John, 9-10
Low, George, 5

M

Magee, John, 2, 6, 8, 12, 14

Mark, Hans, 9-10

McKiethan, Clifford, 8

Meyers, Alice, 4

Morse, Andy, 15

N

National Advisory Committee for Aeronautics
(NACA), 1

National Air and Space Museum (NASM), 12-13

O

Office National d'Etudes et de Recherches
Aérospatiales (ONERA), 2-4

Ormiston, Bob, 15

P

Paris Air Show, 9-10

Poisson-Quinton, Philippe, 4

R

Request for Proposal (RFP), 3, 6

Rutkowski, Michael, 13

S

Schroers, Laurel "Shorty," 5, 7, 12

Sharpe, Dave, 3-5

Snyder, Bill, 12

Statler, Irv, 9-10, 15

Stevens, Story, 10

Sullivan, Chris, 14-15

T

Theodorsen, Theodore, 1

Tilt Rotor Research Aircraft (TRRA), 5-10

V

vertical/short takeoff and landing (V/STOL), 4, 6

vertical takeoff and landing (VTOL), 1, 4-5, 13

W

Weiberg, Jim, 4, 8, 11

Williams, Bob, 5

Wright Air Development Center (WADC), 2

X

XV-3, 3-5

XV-15, 5-10, 12-13

Y

Yaggy, Paul, 3-5, 13, 15

Army Research Lab in the Early Years

Kenneth W. McAlister
Army

A Call to Remember

I'm sure that I didn't have any serious goals in life when I was 7 years old, but I did have an emotional experience that definitely gave me direction. I took a city bus to downtown San Antonio and watched a movie called *We've Never Been Licked*. Parts of the movie were filmed at a military college, and I was determined that one day I would go there. Years later that youthful dream was realized, but it wasn't until my 4th year in college that I matured enough to appreciate the beauty of engineering. Then I found a book in the library that challenged what I knew about fluid mechanics, a subject of particular interest to me. While I expected it to be a casual and entertaining read, my level of understanding on this subject proved to be both shallow and humbling, so much so that I requested a deferment from active military service and enrolled in graduate school to remedy this deficiency. This decision "to know more" gave meaning and direction to my first professional goal, which was to qualify myself for research by pursuing an advanced degree in engineering science.



Kenneth McAlister

While trying to recall my early years at the Army Aeronautical Research Lab (AARL), I was first struck by two thoughts: how quickly people and events fade from conscious memory and how providential it is that seemingly disparate incidents have landed us where we are. In my case it started with a casual conversation in 1967 with "Skip" Fletcher, a fellow doctoral student at Arizona State University (ASU), who later became Director of Aeronautics at NASA Ames in 1999. I shared stories with Skip from the days when I had graded papers for his father (who taught in the Mechanical Engineering department) while I was a graduate student at Texas A&M University. In return, Skip gave me a life-changing suggestion.

Knowing that my student deferments would soon be converted to a likely active duty station in Vietnam (I had a Reserve Officers' Training Corps commission in the Medical Service Corps), he recommended that I contact Helen Davies at NASA Ames Research Center for help in getting a different assignment. He also suggested that I write a letter to the Surgeon General of the Army, thinking he would find an engineer to be a better fit with NASA. Amid this uncertainty, I began active duty as a Captain at Ft. Sam Houston, Texas, where I was taught the basics of medical field operations. Then, at last, I received my assignment.



Skip Fletcher at ASU.

Transfer to the West Coast

In 1968 I was assigned to the 6th Army at the Presidio of San Francisco (now a part of the Golden Gate National Recreation Area), but my actual duty station was at NASA Ames Research Center. This would lead to another pivotal event in my

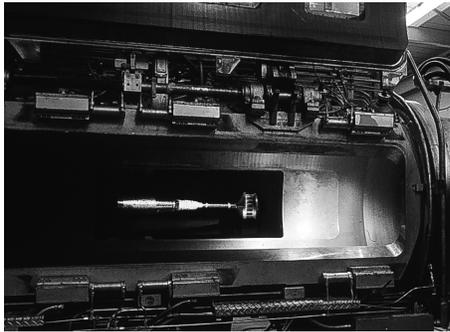
professional journey. I was introduced to Vic Peterson, who at that time was Assistant Chief of the Hypersonic Aerodynamics Branch.

Acting as my mentor, Vic asked me to take over the testing of an atmospheric reentry configuration (at Mach 10) in the 3.5-Foot Hypersonic Wind Tunnel (photo below, with access door open to show the model and sting-mount support).

To facilitate technical briefings frequently given on this project, Vic would hold up a small plastic replica of the reentry vehicle we were testing to illustrate its tumbling descent into the atmosphere, and discuss how the aerodynamic fence would arrest the tumbling motion and bring about a single stable orientation. Vic was a great speaker, but his use of this simple prop taught me the importance of complementing a technical narrative with the strength of an easily understood visual aid.



Vic Peterson at NASA Ames.



Model installed in 3.5-Foot Hypersonic Wind Tunnel.

When Vic retired in 1994 as Deputy Director of Ames Research Center, he called me and said that he had something to give me. It was the plastic model of the reentry vehicle that he had held up in front of countless audiences. This memento was of great emotional value to me, and it now resides on my “trophy” shelf at home. But I have digressed.

My early experience under Vic’s mentorship was a great introduction to the culture and operations at Ames. No matter where I went, whether to the NASA Ames Library, the Machine or Model Shops, or to Graphics or Publications, it was like dealing with a diverse but amicable family. Even when one of my tests involved firing a model down an enclosed ballistics range, an “old timer” came over to help me read the banks of binary timer lights. Even though Vic believed that the current configuration (which he had selected) would have the best stability characteristics, he allowed me to design and test an alternate shape as well. This led to my first publication at NASA [1]. Vic’s leadership was the kind of oversight, mixed with freedom of thought, I would soon find to be representative of another leader from the National Advisory Committee for Aeronautics (NACA) era—his name was Paul Yaggy.

Just Down the Street

With my Army tour of duty drawing to a close in 1970, and wishing to continue with the kind of research available at Ames, I contacted Mark Kelly at the 40- by 80-Foot Wind Tunnel and asked about any openings. He had one, but had already decided to offer it to Larry Olson (a great choice, by the way). However, Mark told me about the new Army Aeronautical Research Laboratory (AARL) under Paul Yaggy, just down the street (formed under an Army-NASA Joint Agreement in 1965).



Vic’s reentry vehicle model.

This recommendation led to another important event. I received a phone call from Jim McCroskey (part of Paul's new team). Jim invited me to his home and offered me a glass of fruit wine. It was a very pleasant visit. We were both from Texas, however Jim attended the University of Texas as an undergraduate and I attended Texas A&M (fierce rivals for many years). Fortunately, we both liked fluid mechanics and Jim knew a lot about boundary layer theory. I later realized that many of us were hired based on face-to-face impressions, rather than solely on impersonal resumes. Alta Steengrafe, who was in charge of administrative matters, delivered a more formal welcome with a kind voice and told me that my purpose was to



W. J. (Jim) McCroskey

to provide technical assistance in the area of Army aviation rather than to publish papers. Introductions didn't take long, since Paul, the Director, Andy Morse, the Chief, Army Aeronautical Research Group (AARG), and the administration, engineers, and secretaries were all located in just 14 rooms on the second floor of Building N-215. (Under the Joint-Agreement, other Army engineers of the Joint Aeronautical Research Group (JARG) were located in other buildings of the NASA organizations to which they were assigned.) The west wing of the first floor was occupied by the Ames Health Unit and the east wing was primarily devoted to electrical units that powered the 7- by 10-Foot Subsonic Wind Tunnel operated by NASA. An identical wind tunnel was located just to the north and was operated by the Army. Both facilities were built around 1940 and were the first wind tunnels constructed at Ames.

First Impressions

Admission to Moffett Field was controlled by the military (Marines, I think) stationed at the main gate, beyond which there was unrestricted access to both the Navy and NASA sides. Permission to pass through the gate was simple; the guard would observe the sticker on the front bumper of your car and wave you through. Parking was never a problem, although you were expected to avoid two places in front of Building N-215 marked for the Army director and the physician attending the Ames Health Unit. Trees were rare. Throughout the day it was common to hear the roar of the wind tunnels and the sound of Navy P-3 Orions practicing their touch-and-go landings.

Men's attire was typical for the 1970s, consisting of slacks and an ironed shirt (either short or long sleeve) with a tie. Shirt pockets carried pencils and pens stuffed inside a plastic "pocket protector," note cards, and an employee badge clipped to the edge (Larry Carr always had the largest assortment). Anyone wearing a jacket was assumed to be a manager. Our offices were clean, but spartan, featuring venetian blinds, metal desks and file cabinets, uncarpeted floors, tilting chairs with four legs on casters (now banned), a black chalk board, party-line phones, and



Paul Yaggy, AARL Director.



Building N-215 with 7- by 10-Foot Subsonic Wind Tunnels (circa 1940).

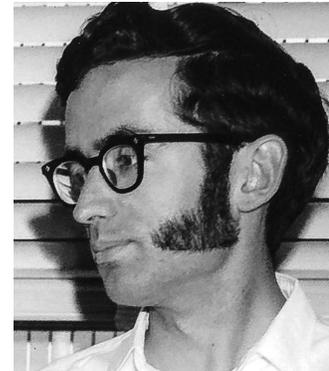


Alice (Tice) Meyers

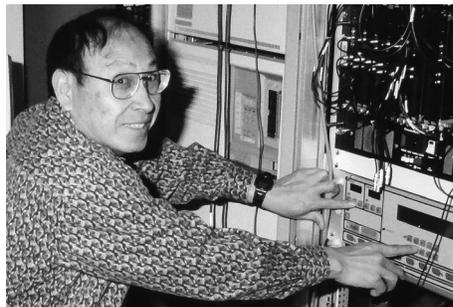
a squawk box located near the door. Alice Meyers provided secretarial support for Paul Yaggy on the east end of the building, and Barbara Goff was stationed on the west end next to Andy Morse's office. Barbara was a very energetic, delightful person who typed our reports (using carbon paper) and delivered our mail right to our desks. When near her desk, she would answer incoming phone calls and then announce over the "squawk box" who should pick up the receiver and talk.

Next to my original office (which I shared with Tom Wynn and Georgene Laub) was a room containing our prized IBM 1800 computer, a control console, a card reader, and a printer. Adjacent to the computer room was a room with three doors. One door was for access from the hall, the second door opened into the computer room, and the third door led to Don Adam's office. Opposing walls contained large windows that allowed Don to keep a vigilant eye on the status of the 1800 from his desk, and the doors allowed him to rush over and reboot the system any time an unexpected crash occurred.

While data acquisition during tests in our 7- by 10-Foot Wind Tunnel received top priority, engineers were allowed to submit programs as well. A program consisted of a deck of punched cards, each card containing a line of "write-once" Fortran code. Cards with a different color would often be used to distinguish subroutines, thereby making it easier to rifle through the deck to locate a card with a "bad" code. At first, we only had a single keypunch machine. Since the computer read the programs in the order they were stacked in the card hopper, there was always a frantic scramble to correct mistakes and quickly reenter the queue. We were pretty good sports when enforcing queue etiquette, and sometimes excuses like "I only have a simple error" would earn you permission to "lift the stack" and go to the head of the line. Three other people sat in the same office with Don Adams: Gary Vander-Roest provided programming support (his handwriting was readable at five paces) and he was familiar with the protocols necessary to run programs on various mainframe computers; Mike Kodani specialized in system architecture and communications and enjoyed talking about the historical development of computers and storage devices; and Mary Pollack helped engineers by plotting their data by hand. I do not recall ever working with Mary while she was at the lab, but I do remember her leading a small uprising over what gender should be in charge of making the coffee. You can probably guess who won!



Don Adams



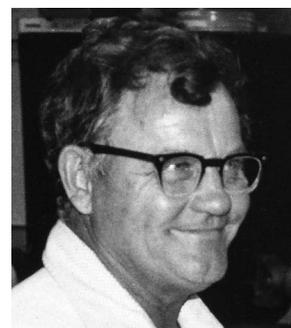
Mike Kodani assisting with test instrumentation.

A device for making copies shared the room with the keypunch machine. Regrettably, the resulting prints had a strong odor, darkened with time, and tended to curl up (an immediate press under heavy books helped). Nevertheless, it was very convenient to have such an "in-house" capability for making copies. While we were still working with the IBM 1800, something transformative was about to happen. NASA was going to make its much larger IBM 360 available for general engineering use. At

first, programs would continue to be produced on punch cards. Don Adams placed a table next to the window facing the street. This would serve as the pick-up point for card decks to be submitted and where decks and printouts would be delivered. A small cutout was made in the window screen so that a colored card could be seen by the driver to signal that a deck was waiting upstairs to be picked up. For those in a hurry, small decks, wrapped with a rubber band, were often ushered by bicycle over to Building N-233 where the 360 was located. When I retired in 2008, I glanced back at the second-floor window above the entrance to Building N-215 and saw that the card-sized cutout in the screen was still there. Would anybody wonder about it?

My original office, along with the adjacent “cold room” dedicated to our IBM 1800, would soon be combined and converted into a very modern conference room (with curtains and dimmable lights). Another early change was the appearance of a “dumb” terminal (it did not have a central processing unit) in the same room with our keypunch machines (we now had several). Rather than submitting cards, it was now possible to remotely type your code directly into the memory of the 360. Frank Caradonna was the first to recognize its significance. The rest of us were simply glad to have one less person competing for time on our old 1800. However, it didn’t take long for the keypunch machines to completely disappear and for every engineer to have his or her very own terminal with a monochrome display on the desk. Computer cards were still in fashion since they made great note cards.

Our warehouse was located near the Army 7- by 10-Foot Wind Tunnel; it was primarily used to store models, test equipment, a water tunnel, and a tenth-scale replica of our wind tunnel. The open space in the center of the warehouse was just right for lunchtime table-tennis matches. I was a frequent player, along with Jim McCroskey, Don Boxwell (always reserved), Cathy Byrne (later married to Don Boxwell), Frank Lazzeroni (with amazing spins), Don Adams, Frank Caradonna (most powerful slams), and Dewey Hodges. Once a year our mechanics, spearheaded by Gene Wells and Bob Gaines, hosted a “thank you” party in the warehouse, complete with music, Christmas lights, and tables of food.



Gene Wells

Certain people were invited, but no one was turned away. This event was mainly to show appreciation to various technicians in the machine, model, metal, and welding shops who helped out our mechanics with quick repairs throughout the year (without a formal service request). Engineers also enjoyed a great deal of cooperation from the NASA shops. I could always tell when Georgene Laub had been to one of the shops—there would be a plate of cookies on the workbench. Because most jobs were fabricated “in house,” it was easy for an engineer to discuss his or her project with an experienced craftsman even before submitting it to the shop. Once the job was underway, details of the design were still open to modification, since they were not restricted by a contract. Much of this collaborative spirit vanished when NASA decided to eliminate many of our shops and replace government technicians with contractors.



Bob Gaines

Getting Down to Work

Being invited to join a newly formed organization was exhilarating. The leadership was solid and approachable (Paul Yaggy and Andy Morse). A number of seasoned engineers were present (Dave Sharpe, Georgene Laub, Tom Wynn, and Wayne Empey). Many had extensive academic backgrounds (Jim McCroskey, Bob Ormiston, Fred Schmitz, and Larry Carr). Some contributed to the lab and then went on to attain prominent positions in the helicopter industry (Dean Borgman and Rande Vause).



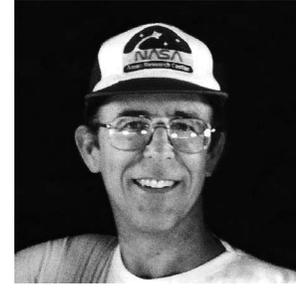
Dave Sharpe



Georgene Laub



Tom Wynn



Wayne Empey

At first, we were all members of the AARG working under Andy Morse. However, to better focus our efforts, two primary research groups were established. Jim McCroskey became the group leader for fluid dynamics issues and Bob Ormiston was the group leader for rotorcraft dynamics. Some members of the lab had basic engineering, math, or computer interests, and were assigned to various projects as they were needed. Others had an immediate preference for either fluid or structural dynamics and were encouraged to assume a more responsible role in developing and carrying out research projects in those areas. The overarching impetus for any project was to successfully address concerns expressed by the Army relating to helicopter flight. In particular, the “issues of the day” for the fluid dynamics group related to acoustics and dynamic stall. I was not a part of the acoustics assignment, but I do remember that Fred Schmitz, Rande Vause, Don Boxwell, and Yung Yu formed a team and were heavily engaged in this effort.

We were a happy group, and Alice Meyers never missed an opportunity to make sure it stayed that way. The picture (circa 1971, after AARL became the Ames Directorate of the Air Mobility R&D Laboratory (AMRDL)) shown on the the following page was taken at a summer picnic and included engineers, staff, spouse, and children. Also present (front row, near left end) was visiting Georgia Tech Professor Jim Wu and his wife. Later, our summer gatherings moved to Chase Park, located on the Navy side of Moffett Field. Smoke from the barbecue pit filled the air as clusters of people relaxed around picnic tables and traded stories while their children raced about. It was a great time to hear about the “less serious” sides of the people that we worked with, to meet their spouses, and to assess how many children they had had since the previous summer (Dewey Hodges won that count). Another event (open to everyone at Ames) that our children looked forward to occurred during the Christmas season—a bell rang as the NASA Ames Hangar doors rolled open to reveal Santa Claus arriving in a helicopter, and it was our very own Bob Gaines wearing the red suit and white beard! Who could deny the Army presence at Ames?



Summer picnic (circa 1971).

It was already decided that I would work with Jim McCroskey. One day Larry Carr and I entered into a friendly “get-acquainted” discussion, and I learned that he had studied at Massachusetts Institute of Technology and New York University and was especially passionate about turbulence. Everything seemed in place for us to begin working as a team, but first I was to join Andy Morse on a special project. Our lab often received requests to review research proposals submitted to the Army Research Office located in Raleigh-Durham, North Carolina. One such proposal was to consider funding a university study on the drag-reducing effects of passive compliant surfaces. It was said that images had been taken of overweight Russian subjects being towed underwater, and that the undulating movement of their skin might be responsible for a reported reduction in drag. With Andy’s considerable help, we designed a means of testing various mediums in the wind tunnel in such a way that only skin-friction drag would be recorded. Tom Wynn, who always carried a notebook in his hand and had a smile on his face, took charge of the fabrication and assisted in the test. We did not observe a reduction in drag for the conditions we studied [2].

By this time, Larry and I were sharing a two-man office, and Jim and Frank were sharing a similar size office next door. Both offices enjoyed a few feet of open space between our large windows and the side of the 7- by 10-Foot Subsonic Wind Tunnel No. 1 that NASA operated (the No. 1 tunnel would eventually be turned over to the Army and the No. 2 facility dismantled). The tunnel was fairly loud when running at high speed, but usually not loud enough to drown out normal conversations in our offices. Andy Morse would stop by the



Andy Morse, Chief,
AARG.

office every day and enter into a casual conversation while sipping from his cup of coffee. As the topic of discussion became technical and drifted into greater depth, Andy would light up a cigarette and begin to expand on the issue. Although few people in the building smoked, it wasn't an issue. In fact, I would often have a cigar or pipe lit myself. Years later I no longer smoked, and smoking was banned from inside all buildings. As I think back on those early days, I remember Larry putting up with all that smoke in the office and never saying a word!

The Era of Dynamic Stall

While I was writing a report on our compliant surface experiment, Mike Martin (along with Wayne Empey and Jim McCroskey) was conducting our first oscillating airfoil experiment in the Army 7- by 10-Foot Wind Tunnel around 1972. The NACA 0012 model featured a smoke port at the leading edge, and an array of dynamic pressure sensors and hot wires intended to capture details of the stall vortex as it passed over the upper surface of the airfoil. Unfortunately, Mike became ill and Frank Caradonna, who had recently joined us, quickly volunteered to help complete the analysis and publication of the results.



Frank Caradonna

During this time, it was customary for a number of us to break for lunch and enjoy hamburgers and peanuts at the Saint James Infirmary located a couple of blocks away from the Ames campus. Mike had good days and bad ones, depending on when he had his last chemotherapy treatment. One day he didn't order any food—he just wanted to “hang out.” The last time I saw Mike was at a group meeting where the results from the oscillating airfoil test were being applauded. Rather than standing and taking a bow, he simply gazed at our faces as if to communicate a silent goodbye. I recall when Mike's wife, Connie, drove up to the front of our building to collect Mike's personal things from his desk and donate some of his books to the lab. As Connie began to close the trunk of her car, I can still see the box containing some of Mike's text books as well as the pajamas he last wore in the hospital. I think Mike's passing really left an impression on many of us because we were all so young and hadn't yet learned to appreciate how brief life can be.

Another sad event that affected our entire community occurred on April 12, 1973. A Navy Orion P-3 turboprop (typical of the ones we saw flying everyday) and NASA's Galileo Convair 990 jet were accidentally directed onto the same approach path into Moffett Field. The two aircraft were less than 1,000 yards from the runway when they collided and crashed onto a public golf course (just short of Highway 101). A fire broke out and all but one perished. Pressed against the window of our office building, Larry and I could see the black smoke rising into the air. I later found out that one of the crew members on the Galileo was a photographer that I had worked with at NASA. Despite the sad events of that year, all of us found that getting back to work was the best therapy.

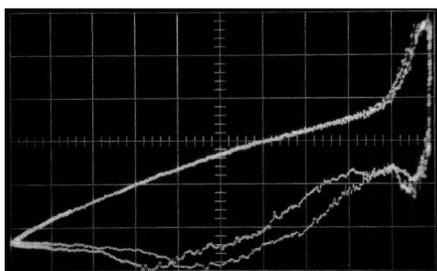
The best therapy for me was to delve into the study of dynamic stall. Jim's initial team had demonstrated the utility of studying rotor dynamic stall on an oscillating two-dimensional airfoil during the 1972 “phase 1” test, and there was great interest in learning more. Specifically, what precipitates the shedding of the stall vortex, what role does airfoil geometry play, and what is the

corresponding pressure signature and velocity profile over the surface. This was the beginning of many years of collaboration between Jim McCroskey, Larry Carr, and myself. Thus began the 1974 “phase 2” test, using the same 4-foot-chord airfoil except for modifications to the leading-edge region. The test included changes in nose radius and camber (with and without a boundary layer trip) and the addition of small vortex generators (a serrated strip) just beneath the leading edge. The Visacorder introduced during the “phase 1” test was still useful for capturing and analyzing dynamic signals, but it could be overwhelming to acquire high-frequency data with sufficient resolution. Besides, digitizing analog signals recorded on paper was quite laborious.

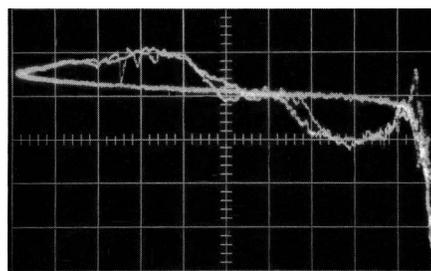


Jim McCroskey preparing to roll up a burst of Visacorder output.

Strip-chart recordings were clearly impractical for the large number of cases we wanted to study. Instead, analog recordings of the unsteady pressure and hot-wire signals were now captured on tape recorders. Based on timing marks included on each magnetic tape, the signals were digitized (courtesy of Floyd Moffett at NASA), stored on digital tapes (each test case resulted in about half a million words of data), and submitted for analysis on NASA’s IBM 360. This arrangement offered a major improvement in data management compared to the “phase 1” experiment, but the turnaround time (about a week) was still too long to support the real-time decisions that Jim had to make regarding the matrix of parameters to be explored. Fortunately, I had some experience with electrical analog systems and was able to design two summing circuits. Knowing the locations and sensitivities of the pressure transducers on the airfoil, their local contributions to the normal force and pitching moment on the airfoil could be individually summed and the two resulting voltages fed to the vertical inputs of an oscilloscope. By using the voltage from the angle-of-attack transducer as the horizontal input to the oscilloscope, real-time hysteresis loops could be observed (example screen shots [3] shown below).



Normal force vs. angle-of-attack.



Pitching moment vs. angle-of-attack.

Bob George, shown on the next page performing a delicate “operation” on a bundle of cables imbedded in the center section of the airfoil, was an important member of the test crew. Bob’s instrumentation skills cannot be overstated. All connections had to be precise and efficiently routed to various recording and monitoring devices. Bob was always pleasant, which greatly helped to reduce everyone’s stress during a test.



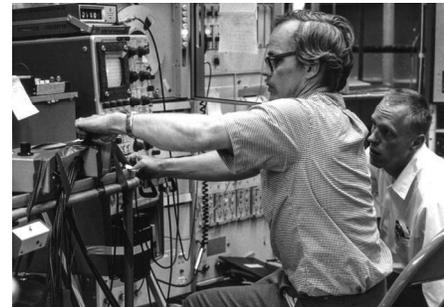
Bob George (instrumentation).



Ozzie Swenson (circuit designer).

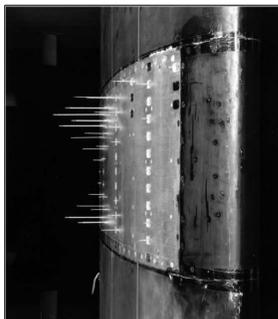
Every test typically required numerous special purpose electronic devices to be designed and built—often on an urgent basis. This was Ozzie Swenson’s specialty (shown above testing a circuit). After explaining to Ozzie what you needed, he would quickly lay out a circuit on a scrap of paper and then begin looking for components. If he didn’t have what he needed on his workbench, he would search in his car (only the driver’s seat was free of “stuff”). In just a day or so an aluminum box would appear that contained a circuit that featured “easy to change” connections (meaning lots of alligator clips and tape). These “Ozzie boxes” were rarely marked (except for “in” and “out”) since they were only intended to satisfy a unique request. Nevertheless, it was our habit to save everything from a test, so after many years we accumulated several cabinets full of Ozzie boxes, with few clues as to their original purpose.

With Larry Carr looking on, Jim McCroskey is seen in the photo on the right selecting which pressure to be displayed on the oscilloscope. Several Ozzie boxes are also present. In the spirit of getting on with the test, c-clamps and military duct tape were frequently used to hold things in place.



Jim McCroskey (left) and Larry Carr examining a test signal.

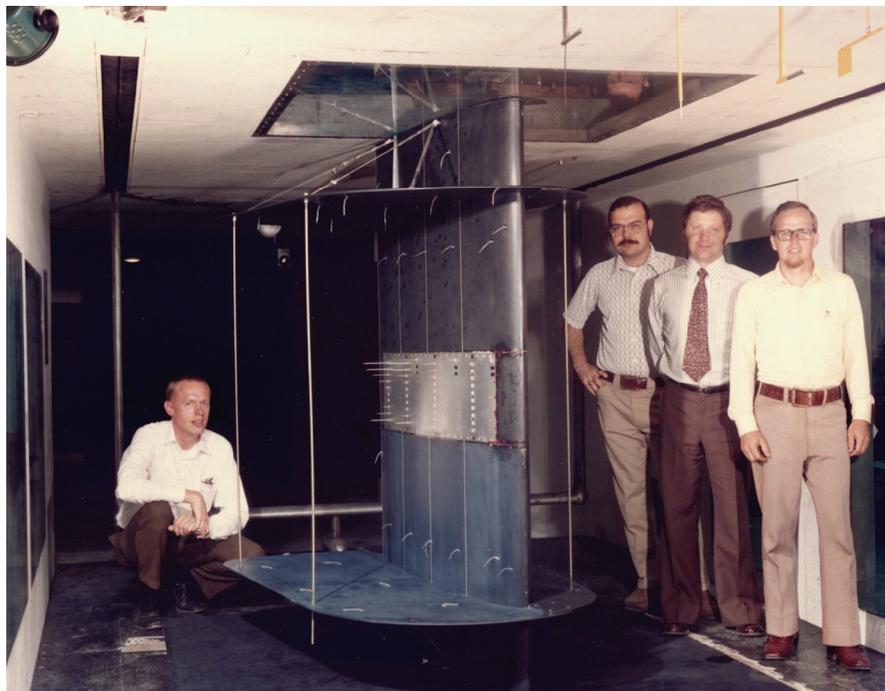
The responsibility for calibrating and monitoring the hot wires was primarily up to Larry Carr.



Hot wire “porcupine” array on airfoil surface.

Dust particles were always present in the wind tunnel and posed a major hazard to the delicate wires. When a collision occurred, Larry would use specially designed tools to remove a damaged element and replace it with another. Anyone walking around the model (especially wearing a loose lab coat) would be severely cautioned. The array (shown left, mounted on the airfoil) was affectionately known as the “porcupine.” Hot wires had to be calibrated often due to drift, and calibration had to be performed in still air. The same applied to the hot wire located ahead of the model to measure the free-stream velocity. No problem; we simply placed a Styrofoam cup over the hot wire to temporarily shield it from any small air currents that might be present.

On one occasion after taking a reading, Larry and I calmly joined the mechanics in the control room and watched through the test-section window as the tunnel was brought up to speed. Much to our horror, a white object was observed flying past our model, and with frantic voices we shouted for the tunnel to be shut down. Fortunately, the cup (that we forgot to remove after calibrating the free-stream hot wire) missed the entire array of wires on the model. We did not make that mistake again. Although

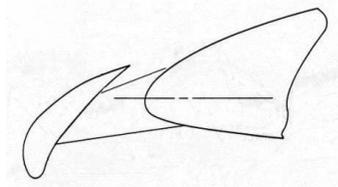


Larry Carr, Ken McAlister, Jean-Jacques Philippe (ONERA), and Jim McCroskey with the 4-foot-chord airfoil model.

different types of stall were observed, this test confirmed that the shedding of the stall vortex from near the leading edge remained the dominant feature. In most cases the stall process was precipitated by a breakdown of the turbulent boundary layer rather than by the bursting of a laminar-separation bubble (as was previously believed [4]).

Sanctioned under a Memorandum of Understanding (MOU) with France, we developed a strong relationship with Jean-Jacques Philippe (pictured above, circa 1974) who represented the Office National d'Etudes et de Recherches Aéropatiales (ONERA) (equivalent to our NASA) during our joint studies of dynamic stall. Many of us participated in the French MOU. Formal meetings were held in both France and the U.S. and were usually scheduled to coincide with annual conferences of the American Helicopter Society and the European Helicopter Society. On the last day of our meetings, we would gather for informal conversations and a meal. The picture on the bottom on the next page (representing the U.S. in the areas of dynamic stall, transonics, and acoustics) was taken in Frank's backyard.

After the dynamic stall "phase 2" test was completed, Jim allowed the model to be modified so that Larry and I could explore more drastic measures either to alter or to prevent the formation of the stall vortex. This effort was encouraged by Andy Morse, who challenged us to find any method that worked, without regard for how difficult it might be to apply to a rotor. Several methods came to mind that might extend the static-stall angle, for a good start. A number of methods were passive and one involved an active flow control. One passive method involved the placement of vortex generators along the 1/4 chord in order to entrain high-energy free-stream flow into the low-energy boundary layer. A second passive method involved a leading-edge slat that would cause the overall configuration to achieve a higher value of circulation, and therefore higher values of velocity over the main element. The active flow control method was achieved



Multi-element airfoil.

by forming a cavity along the leading edge of the airfoil, injecting high-pressure air from the ends, and allowing the air to eject over the upper surface from narrow slots along the span. Mineral oil mixed with titanium dioxide was used to indicate the behavior of the flow over the airfoil surface during fixed angle-of-attack runs. Two methods appeared to be encouraging, especially the leading-edge slat case [5] (sketch, left). Although only the main element was instrumented, results indicated that even during high-angle oscillations (well exceeding stall angles on helicopter blades), the flow over the upper surface remained attached.

Encouraged by the evidence that the stall vortex could be thwarted, we were anxious to find a way to explore concepts more easily and with better flow-visualization capabilities (see next section). Our motivation reached a peak on the day that Larry and I were pushing for more air flow during the active flow control test. We were both in the control room when one of the high-pressure hoses ruptured, sounding much like the boom from a battleship gun.

Over the next several years a major campaign was undertaken under Jim's leadership to document the significance of key parameters (like airfoil shape, Mach number, Reynolds number, reduced frequency, mean angle of oscillation, and amplitude) on dynamic stall. By about 1980 the campaign to test eight distinctly different airfoils in the 7- by 10-Foot Wind Tunnel was ready to begin. All airfoil models were the same size (2-foot chord), and each consisted of an upper and lower shell so that a common spar and instrumentation package could



Informal gathering of (from left) Ellen and Ken McAlister, Monica and Frank Caradonna, Larry and Jan Carr, Fred and Sheila Schmitz, and Betty and Jim McCroskey (circa 1974).

be used. Instrumentation consisted of single-surface dynamic pressure transducers, skin-friction gages, and hot wires. The drive system for oscillating the airfoils and the procedures for obtaining the data had already been perfected during the “phase 2” experiment (except we now had a 32-channel analog recorder). The test went smoothly, but it took more than a year for Jim McCroskey, Larry Carr, Steve Pucci, and myself to acquire this amount of data. Analysis also took a long time, but finally in 1982 three NASA publications (consisting of some 790 pages [6-8]) were produced, along with several journal papers. Of particular value was the fact that all of the airfoil shapes were studied under the same wind tunnel conditions (similar free-stream turbulence and test-section wall effects). This ambitious study allowed us to conclude, at least for Mach numbers below 0.3 (the limit for this facility), that airfoil shape was less important than the unsteady parameters governing the pitching motion of the airfoil. Enough requests for the digital data files were received from academics, as well as from the helicopter industry, that NASA agreed to archive the results and make them formally available to the public.

Concurrent Studies in the Water Tunnel

There were many questions about dynamic stall still to be answered. Our warehouse contained a closed-circuit water tunnel, built by Boeing, with an oscillating airfoil mechanism and means for visualizing the flow using hydrogen bubbles. Larry and I started with the well-known NACA 0012 profile and designed a model with embedded electrodes along the chord. An upstream electrode (stainless-steel wire) that stretched across the tunnel produced bubbles that delineated the free-stream flow. The electrodes on the model (activated one at a time) revealed the flow in the boundary layer. As shown in the photographs below, flow reversal was clearly witnessed for the first time advancing upstream toward the leading edge (prior to dynamic stall) while the boundary layer was showing no signs of separation. In 1978, the details of this study were published by NASA and the American Society of Mechanical Engineers *Journal of Fluids Engineering* [9].



Hydrogen bubbles from a free-stream electrode.

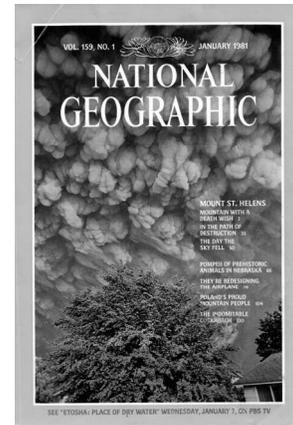


Hydrogen bubbles from surface electrodes.

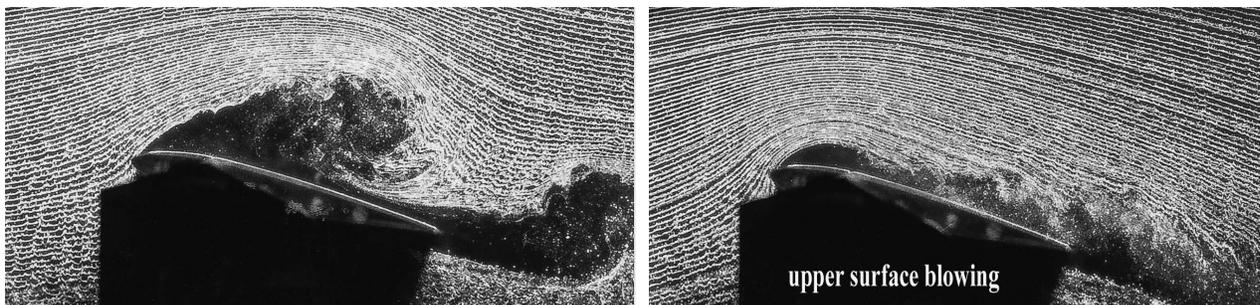
In the year that Mount St. Helens exploded and received much media attention, National Geographic was also taking notice of the pioneering work in aviation occurring across the nation, and a photographer was assigned to visit Ames Research Center. After being directed to the water tunnel, he began setting up his equipment for various pictures while I discussed our dynamic stall program. He insisted on using his own cameras and strobes. Unfortunately, he placed one of his strobes too close to the intense airfield landing lights that I was using at the time, and we soon noticed the unpleasant smell of melting plastic. He was not too pleased since he was scheduled for another assignment while on the West Coast. Nevertheless, among the many pictures that he was able to take, one earned a two-page spread in the January 1981 issue of *National Geographic* magazine.



Ken McAlister observing dynamic stall process.



While we were waiting for the wind tunnel models to be built, Larry and I continued our dynamic stall modification studies in the water tunnel. We first reduced the risk of cavitation (especially likely to occur around the suction peak on the airfoil) by degassing the water with negative pressure and then applying a positive pressure. The freestream electrode was also reshaped so that the hydrogen bubbles would shed at evenly spaced locations, thereby better delineating the geometry of the streamlines. A NACA 0012 airfoil was constructed with a spanwise plenum that accepted water pumped from the tunnel and then channeled through slots over the upper surface of the airfoil. With a sufficiently high level of blowing, the stall vortex that normally formed (left photo below) was instead arrested (right photo below) and allowed to simply collapse (without shedding) as the angle of attack decreased [10]. Although we were not able to measure force and moment loads at that time, it appeared that the lift overshoot that typically accompanied the stall vortex during its formation might be retained as a benefit, without suffering from an undesirable pitching moment caused by the movement (and eventually shedding) of the stall vortex.



Vortex development on an oscillating airfoil with and without upper surface blowing.

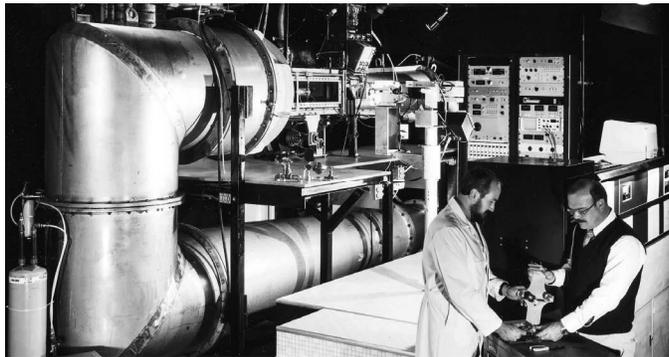
At that time, we only had visual evidence of our successes in the water tunnel. It was clear that flow visualizations using hydrogen bubbles and fluorescent tracers (principally Fluorescein dye) would be even more useful if they could be augmented with force and moment measurements. Since dynamic stall on airfoils undergoing pitch oscillations was still the main focus of our studies, an apparatus for measuring only static loads would not be acceptable. The design had to account for the frictional moments from the water seals while simultaneously measuring the total lift, drag, and pitching moment on the airfoil. The design was completed around 1982 and tested with analog signal lines routed underground to Building N-216A where our current data acquisition system was located. Sensitive transistor gages mounted on the water seals caused

some problems initially. Fortunately, Jon Lautenschlager, our resident instrumentation specialist, helped me solve some ground-loop issues. Jon was remarkably methodical and seemingly a wizard at debugging problems. On one occasion I was sure that one of my channels was being contaminated by what sounded like a Paul Harvey radio broadcast. We had a good laugh, but it did cause us to consider that my free-stream electrode was acting like a radio antenna.

To accommodate the increased interest in testing in the water tunnel, Richard Losee was hired as a mechanic to maintain the facility.



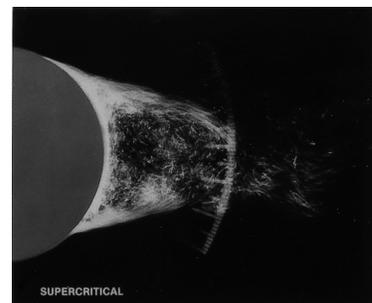
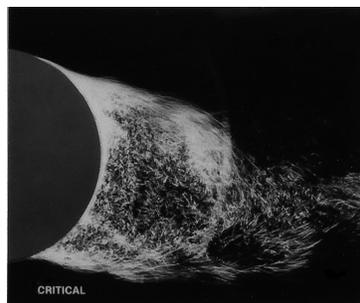
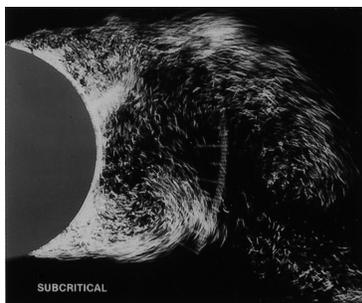
Jon Lautenschlager
(instrumentation specialist).



Richard Losee (left) and Ken McAlister preparing a calibration device.

protracting the current flow through the strobe [11]. Rather than “freezing” the hydrogen bubbles, the duration of the light pulse was sufficiently extended to show particle path lines, making it easier to study the flow.

Because of control concerns arising from asymmetric shedding of the wake from missiles, Dan Almosino (a NASA research associate from Israel) asked me to partner with him to study the Reynolds number dependence of flow separation from a circular cylinder. In addition to measuring the unsteady lift and drag on the cylinder, a water cooled, linear xenon strobe was used to visualize the flow. By this time I had borrowed an idea from WWII radar technology and designed a variable pulse-forming circuit for



Hydrogen bubble path lines showing the transition from subcritical to supercritical flow.

For the first time, flow visualization and force measurements were simultaneously obtained to document the existence of this critical Reynolds number regime [12]. Whenever the asymmetry occurred, it was normally steady. However, there were times when it suddenly switched sides, very much reminding us of the radical behavior of the “knuckleball” pitched in baseball games. This may also explain why the smoothbore muskets used early in the Civil War were considered to be accurate to only 150 yards, beyond which bullets were observed to travel in mysteriously “wild” directions (deviating either left, right, high, or low).

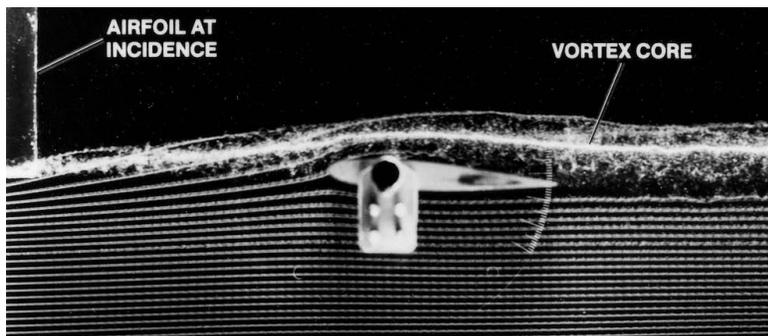
A New Direction

A number of changes occurred in the early 1980s that affected the direction of my research. Larry Carr had discovered that compressibility effects near the leading edge had an important influence on dynamic stall at higher Mach numbers. Since our 7- by 10-Foot Wind Tunnel was not capable of speeds higher than $M = 0.3$, Larry moved his office to be more conveniently located next to NASA's indraft wind tunnel facilities managed by the Fluid Mechanics Laboratory. Jim McCroskey became interested in focusing on advanced computational methods and moved his office to the Numerical Aerodynamic Simulation Facility. There was also a suggestion that we should limit our research on two-dimensional dynamic stall and give more attention to rotor aerodynamics in general, and rotor wakes in particular. I recall that, in 1979, Irv Statler, our Director at that time, had asked several of us to meet with a potential new hire from Brookhaven National Laboratory. His name was Chee Tung.



Chee Tung

Not only was Chee clearly qualified and quite personable, I was impressed that he mentioned rotor wakes (suggesting that he was already thinking about the future). It didn't take long for us to become office mates and good friends. In our first joint study, we examined the interaction between a streamwise vortex and a lifting airfoil [13].



Streamwise vortex interacting with a lifting airfoil.

These were the early years, and I have only mentioned a fraction of the number of people that I worked with. Army scientists at our lab that I didn't work with, like Bob Ormiston, Bill Bousman, Mark Fulton, Mike Rutkowski, Dave Peters, Dewey Hodges, Fred Schmitz, and Yung Yu were, nevertheless, powerful daily reminders that I was a part of a unique fellowship of very

talented people. Beginning in 1985, another 20 years would pass before I retired. Just as Frank and Chee had predicted, rotor wakes became a major focus for many of us. During this new era, I found myself turning away from the dynamic stall vortex and beginning to focus on the rotor wake trailing vortices. Fortunately, necessary diagnostic techniques became available, such as laser-Doppler velocimetry and particle-image velocimetry, allowing Chee and I to co-author another 20 papers dealing with the rotor wake.

It is indeed fascinating to reflect on the changes that I have experienced since I first came to the Ames-Army Lab, armed with a state-of-the-art slide rule, graph paper, and an assortment of French curves. In reality, however, my personal story at Ames (1968–2008) is not unique. I can recall stories passed down to me from people who worked at Ames Research Center during the early days of the NACA. Future readers will no doubt regard my recollections with the same amusement that I did when I was told about “Smitty” DeFrance (the first Ames Director, serving

from 1940 to 1965) who did not allow a cup of coffee to be on your desk (coffee was only permitted in the hallway during formal breaks) and how he might even summon you to his office to account for your work. It's true that the years have brought about many changes, and while many trees now line the streets, and the tools available to engineers and scientists have greatly improved, the spirit of basic research remains a hallmark for the people working for NASA and the Army at Ames Research Center. I'm thankful that I was able to play a small part.

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Index

A

Adams, Don, 4-5
 Almosnino, Dan, 15
 Ames Health Unit, 3
 Arizona State University (ASU), 1
 Army Aeronautical Research Group (AARG), 3, 6-7
 Army Aeronautical Research Lab (AARL), 1-3, 6
 Army Air Mobility R&D Laboratory (AMRDL), 6
 Army Research Office, 7

B

Boeing, 13
 Borgman, Dean, 6
 Bousman, Bill, 16
 Boxwell, Don, 5-6
 Brookhaven National Laboratory, 16
 Byrne, Cathy, 5

C

Caradonna, Frank, 5, 8, 12

McAlister, K.

Caradonna, Monica, 12

Carr, Jan, 12

Carr, Larry, 3, 6-14, 16

Chase Park, 6

D

Davies, Helen, 1

DeFrance, Smitty, 16

E

Empey, Wayne, 6, 8

F

Fletcher, Skip, 1

Fluid Mechanics Laboratory, 16

Ft. Sam Houston, 1

Fulton, Mark, 16

G

Gaines, Bob, 5-6

George, Bob, 9-10

Goff, Barbara, 4

H

Harvey, Paul, 15

Hodges, Dewey, 5-6, 16

Hypersonic Wind Tunnel, 2

J

Joint Aeronautical Research Group (JARG), 3

K

Kelly, Mark, 2

Kodani, Mike, 4

L

Laub, Georgene, 4-6

Lautenschlager, Jon, 15

Lazzeroni, Frank, 5

Losee, Richard, 15

M

Martin, Connie, 8

Martin, Mike, 8

McAlister, Ellen, 12

McCroskey, Betty, 12

McCroskey, Jim, 3, 5-13, 16

Memorandum of Understanding (MOU), 11

Meyers, Alice, 4, 6

Moffett Field, 3, 6, 8

Moffett, Floyd, 9

Morse, Andy, 3-4, 6-7, 11

N

National Geographic, 13-14

Navy P-3 Orion, 3, 8

Numerical Aerodynamic Simulation Facility, 16

O

Office National d'Etudes et de Recherches
Aérospatiales (ONERA), 11

Ormiston, Bob, 6, 16

Olson, Larry, 2

P

Peters, Dave, 16

Philippe, Jean-Jacques, 11

Pollack, Mary, 4

Presidio of San Francisco, 1

Pucci, Steve, 13

R

Rutkowski, Mike, 16

S

Saint James Infirmary, 8

Schmitz, Fred, 6, 12, 16

Schmitz, Sheila, 12

Sharpe, Dave, 6

Statler, Irv, 16

Steengrafe, Alta, 3

Subsonic Wind Tunnel, 3, 7

Swenson, Ozzie, 10

T

Texas A&M University, 1, 3

Tung, Chee, 16

U

University of Texas, 3

V

Vander-Roest, Gary, 4

Vause, Rande, 6

Vic Peterson, 2

W

Wells, Gene, 5

Wu, Jim, 6

Wynn, Tom, 4, 6-7

Y

Yaggy, Paul, 2-4, 6

Yung Yu, 6, 16

Army-NASA Memoirs
William J. McCroskey
Army

From Hypersonics to Microsonics 1966–1968

In January 1966, I finished my Ph.D. program at Princeton University in the Department of Aerospace and Mechanical Engineering. My thesis was based on measurements in the rarefied hypersonic flow of one of the hot nitrogen hypersonic wind tunnels of the Gas Dynamics Laboratory, supervised by Professor S. M. Bogdonoff. Boggy kept me busy for another 5 months as a post-doctoral research assistant, until the time came for me to report for active military duty on June 26, 1966, at Aberdeen Proving Ground, Maryland.



Jim McCroskey, 1970.

During my undergraduate days in Texas (1955 to 1960) I had joined the Army Reserve Officers' Training Corps (ROTC), with a commitment to serve on active duty upon graduation.

However, most cadets with technical backgrounds, myself included, were allowed to defer the start of their military service while in graduate school. Furthermore, some of those who were deferred could find temporary positions in government research labs or test centers. During my graduate studies, Boggy had introduced me to the Supersonic Wind Tunnel Group at the Ballistics Research Laboratory (BRL) at Aberdeen, and that seemed to be my best hope for a meaningful technical assignment.

So, in March, I drove down from Princeton to BRL at Aberdeen, Maryland, met with some of the engineers there, and then I was introduced to the Colonel who was the Director of BRL. He patiently listened to my spiel about what wonders I could do for them, and then he smiled and said that he was absolutely saturated with newly-minted First and Second Lieutenants who didn't want to go to Korea, Vietnam, or Germany (the options available at that time) for their first assignments. As a result of this surplus of junior officers, he would not write a formal request for me to be assigned to BRL. But then he added that he had recently heard about a new Army Aviation Activity on the West Coast that was hiring civilians. He thought maybe they could use some military officers with backgrounds in aerospace engineering.

I continued to Washington in search of more information about this new Army laboratory. After knocking on many doors in several buildings, I finally found someone who knew about the Army Aeronautical Research Laboratory (AARL), and someone else who said that if the AARL Commanding Officer (Colonel Stapleton) would send him the required name request, they would consider assigning me there after I finished my Basic Officers' Training program at Aberdeen.

Boggy arranged invitations for me to give seminar lectures on my thesis at University of California, Berkeley and the University of Washington in the Spring of 1966. This gave me an opportunity to visit AARL in person, where I met Paul Yaggy and Colonel Cyril Stapleton. At first, I was a little skeptical about working on helicopter problems, and they were a little skeptical about my interest in hypersonics. But they decided to give it a shot, and Paul agreed to

let me explore, part time, the activities of the NASA Ames Gas Dynamics Branch and their renowned 3.5-Foot Hypersonic Wind Tunnel.

After I finished a 9-week Officers' Basic Training program at Aberdeen, my wife, my two preschool daughters, and I set off for Moffett Field, California, to begin our next big adventure.

Upon our arrival, Paul Yaggy, Andy Morse, and Colonel Stapleton were very cordial and helpful in getting us settled. I was the first of a number of ROTC-commissioned junior officers to wind up at AARL over the next 5 or 6 years. Andy Morse introduced me to the AARL staff, which was very small at that time. He showed me around Building N-215 and the 7- by 10-Foot Wind Tunnel #2, and described the surrounding NASA organizations. I was overwhelmed by the big wind tunnels, especially the *huge* 40- by 80-Foot Wind Tunnel. Andy encouraged me to explore on my own. Soon I found the stable of fascinating experimental aircraft in the NASA Ames Hangar and the impressive NASA Ames Library (which was made even more outstanding a few years later by Hans Mark when he became the Ames Research Center Director). And so began my 50-year career at (love affair with) Ames.

I don't remember the details, but Paul, Andy, and Colonel Stapleton must have described the brief history of AARL (less than 2 years at that point). In short, the Army had recognized its need for a research laboratory dedicated to its specific aviation requirements, whereas NASA had shifted its emphasis from aeronautics to space and was left with some unique facilities that were underutilized. Therefore, it made sense for the two agencies to collaborate; and they did, within the formal framework of the first Army-NASA Joint Agreement signed in February 1965. The participation of both parties would grow significantly and the Ames-Army Lab would expand and be reorganized several times in the subsequent years.

Because I knew next to nothing about helicopters, Paul and Andy decided to send me on a whirlwind technical tour of the research groups at Sikorsky Aircraft, Boeing Vertol, and Bell Helicopter. I was instructed to 1) find out what aerodynamic subjects they were most interested in, 2) get to know some of the key players on each team, and 3) solicit suggestions for how AARL could contribute to the national rotorcraft technology base. I found each group to be understaffed, and there was no real enthusiasm or management commitment to long-term basic research. Therefore, if Paul Yaggy wanted to pick up the slack with his new organization, so much the better.

I came back to Ames feeling that helicopter rotor blades were much more than just pieces of metal and plastic on which some very interesting fluid-dynamic flows develop. I was enthusiastic about the possibilities of carving out individual niches in the wide array of fundamental aerodynamic problems associated with helicopter rotors, as depicted in figure 1. Most of the individual blocks in the figure would either

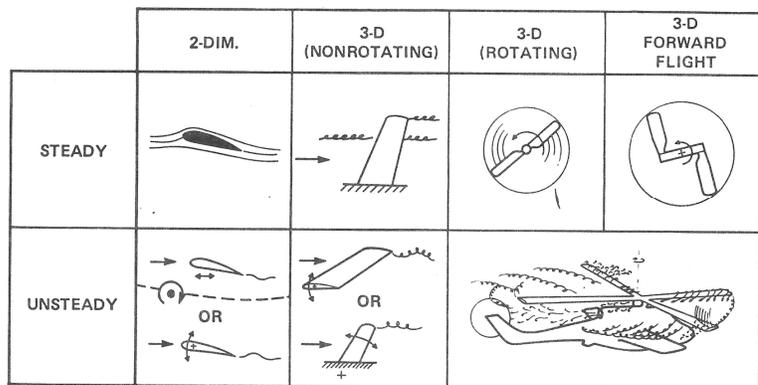


Figure 1. Development of rotor blade aerodynamics from simpler cases.

draw on existing knowledge in the larger aerodynamics community, or be more easily studied in isolation. Although the cartoon in the lower right-hand corner of figure 1 represented the obvious long-range goal, I certainly did not anticipate how much rotorcraft engineering technology would advance in that direction before my tenure would expire.

With approximately 20 months remaining in my 2-year military commitment, I chose two research topics that appealed to me personally and that seemed applicable to most of the blocks in figure 1; namely, 1) viscous boundary layers on rotating blades and 2) aerodynamic instrumentation on rotating blades. Both topics took on longer time scales when Paul offered me a civil service position to continue this basic research.

New Research Projects 1967–1972

Boundary layers on rotating blades. A literature search by the invaluable NASA Ames Library led me to the Ph.D. dissertation of L. E. Fogarty at Cornell University, who studied the 3-D, steady laminar boundary layer on rotating blades in hover. I naively thought it would be straightforward to extend this approach to the *unsteady* case of forward flight. In fact, it wasn't all that straightforward; but when combined with certain numerical results developed by my friend, Professor Harry Dwyer at University of California, Davis, it provided some physical insights into the differences between boundary layers on 2-D steady airfoils and those on helicopter rotor blades. And it helped me make the transition from hypersonics to microsonics. It also resulted in my first publication [1] as an Army employee.

Aerodynamic instrumentation on rotating blades. There was an obvious need for experimental data to complement the 3-D unsteady theory and computations. Hot-wire anemometry seemed promising on small model rotors, but I felt that the probes would probably be too fragile for tests of full-scale rotor blades. On the other hand, surface-mounted, heated thin films seemed more promising for measurements of skin friction, surface flow direction, boundary-layer transition, and flow separation.

A commercial strain-gage manufacturer assured me that their photoetching techniques could mass produce heated film skin-friction filaments with highly reproducible geometrical, electrical, and thermal properties. It was noteworthy that the gages could be bonded easily to a variety of surfaces, including ones with curvature, using standard strain-gage cement and wiring techniques. Also, they would be *much* more rugged than hot-wire probes. A number of customized thin films were purchased, and I set about calibrating them in a small channel-flow wind tunnel built by the NASA Ames Model Shop. The thin-film gages were controlled by conventional constant-temperature hot-wire circuitry.

Enoch Durbin from Princeton arrived for a sabbatical during this period. I had barely known him during my graduate school days, but we hit it off well from the beginning. Soon we were playing squash regularly in the Navy Moffett Field gymnasium at lunch time. When I told him about the flow-direction properties of my thin-film skin-friction gages and their accuracy requirements in measuring the small differences between two relatively large signals, he was immediately interested. It turned out that he had just developed and patented, for an entirely different application, a differential power concept that he thought would lead to what I wanted [2]. Over the next couple of years, we tinkered with several variations of his ideas, but we never completely eliminated all the bugs in the electronics. However, over the next decade, the thin-film skin-friction gages were used on rotor blades and oscillating airfoils in the 40- by 80-Foot

Wind Tunnel at Ames, the Boeing 20- by 20-Foot Vertical/Short Takeoff and Landing (V/STOL) Wind Tunnel, the Ames 7- by 10-Foot Wind Tunnel #2, and in some of the Office National d'Etudes et de Recherches Aérospatiales (ONERA) wind tunnels in France.

Retreating blade dynamic stall. In 1968, Frank Harris, a Boeing Vertol engineer who became a close friend and adviser, postulated in reference [3] that both 2-D unsteady and 3-D steady effects (see figure 1) contribute significantly to the airloads on a helicopter rotor at high thrust in forward flight. These were precisely the effects that I wanted to study on a model rotor with pressure-instrumented blades, and I talked with Frank about building such a rotor and testing it in the Boeing 20- by 20-Foot V/STOL Wind Tunnel. He responded with a formal proposal to construct an 8-foot-diameter “dynamically representative” model of the CH-47C three-bladed rotor, with 20 Kulite miniature semiconductor pressure transducers on one blade at the 75-percent blade radial station, and 10 thin-film skin-friction gages mounted on a second blade. The third blade would be painted white for possible tuft studies. The rotor would duplicate the geometrical and dynamic properties of the rotor used in reference [3].

The instrumented model rotor was constructed in 1969 and installed in the Boeing V/STOL tunnel in March 1970. The bright young Boeing test engineer was Richard Fisher, Jr. I flew to Philadelphia for the test, bringing with me several of Enoch Durbin's double-bridge controllers and a lab coat. The pressure transducer installation was awful; the surface in the vicinity of the instrumentation was very irregular, and it had to be reworked to get a reasonable airfoil shape. I almost provoked a union grievance over my active participation in this endeavor, as engineers were not allowed to touch the hardware—that was reserved for the unionized shop workers.

Because of the extensive instrumentation, the test program was run in two parts; first the data from the pressure-instrumented blade were recorded, followed by separate runs to record the signals from the skin-friction gages. After a series of false starts, shakedown tests, and hover runs, we started recording forward flight pressure data in the early evening of March 26. We got permission from Frank Harris to run the tunnel overnight, which I didn't know meant only the graveyard shift, until 7:00 a.m. We continued into the next morning until about 11:00 a.m. When Frank checked in with us, he became apoplectic, asking us if we had any idea how much more the electric power cost in the daytime, after 7:00 a.m.

A large amount of time-varying data was recorded on multichannel tape recorders, harmonically analyzed, and tabulated on microfiche off-line by the Boeing Flight Test Center. After the data were processed, we had the very good fortune to be assisted by Harvard Lomax's Computational Fluid Dynamics (CFD) Branch at Ames. A 16-mm movie camera was placed in front of a cathode ray tube (CRT), on which a computer-generated 2-D x-y plot was displayed. The camera was advanced frame-by-frame while a series of plots at successive time steps was displayed on the screen of the CRT. The resultant “movie” was especially well suited to interpreting the distortions in the chordwise pressure distribution, C_p vs. x/c , as the blade passed through the third and fourth quadrants of the rotor disk. (Note that this “state-of-the-art technology” for data display was developed before the advent of time-dependent graphical displays on modern digital workstations.)

Even though the blade instrumentation was limited to one radial station, the combined pressure, skin friction, surface flow direction, and blade torsion information was sufficient to show that retreating blade stall is comprised of a sequence of several distinct physical events that occur

over a significant segment of the rotor azimuth. The time-varying pressure distributions suggested that a strong vortex-like disturbance is shed from the leading-edge region and grossly distorts the flow field as it passes over the upper surface of the rotor blade. Furthermore, our *3-D unsteady* data on a realistic rotor configuration was remarkably similar to the *2-D unsteady* airfoil results of Ham and Garelick [4] for comparable flow conditions. This conclusion led to a major shift in the direction of my research to the leading-edge region of 2-D airfoils oscillating in pitch at high amplitudes.

Dick Fisher and I wrote a paper [5] for the next American Helicopter Society (AHS) Annual Forum in May 1971. The presentation, especially the aforementioned movie, was very well received. Varying segments of the movie were included in many of my technical presentations over the next two decades.

First-generation oscillating airfoil experiments. Even before the Boeing model rotor test pointed the way, the rotorcraft community was stepping into the realm of 2-D unsteady airfoil aerodynamics, with emphasis on the leading-edge region, where the onset of dynamic stall was thought to originate.

Tom Wynn and Wayne Empey helped me design, build, and test a 6-inch-chord by 12-inch-span NACA 0012 model that was oscillated in pitch between end-plate inserts in the Army 7- by 10-Foot Wind Tunnel. The model was lightly instrumented, with one Kulite pressure transducer, one heated skin-friction gage, and three hot wires protruding slightly above the surface of the model. This experimental configuration served dual purposes: 1) it gave us valuable experience in testing oscillating airfoils, and 2) it provided data to evaluate some simple theories about the onset of dynamic stall [6].

To study the leading-edge region in more detail, where the boundary layer is very thin, we sought to use *larger-than-real-life*, highly instrumented models. Ideas that were floated and discarded included an 8-foot-chord by 40-foot-span oscillating wing spanning the vertical dimension of the NASA Ames 40- by 80-Foot Wind Tunnel, and an abbreviated airfoil consisting of a short, instrumented leading-edge section mated to a high-lift trailing-edge flap. In the end, we settled on a 4-foot-chord model spanning the vertical dimension of the Army 7- by 10-Foot Wind Tunnel, which was simpler in operation and better suited to exploratory research.

Wayne Empey helped design the oscillating mechanism and the model, which was constructed by the NASA Ames Model Shop and installed in the 7- by 10-Foot Wind Tunnel in June 1972. The model was instrumented with 16 surface pressure transducers, 6 hot-wire probes, and a smoke port in the leading edge for flow visualization. The time-histories of the pressure transducers were recorded on rolls of Visicorder paper, which were read and digitized after the test was concluded. Fortunately, Frank Caradonna located the necessary digitizing equipment in the Ames Unitary Plan Wind Tunnel facility (another side benefit of the Army-NASA Joint Agreement).

A newly commissioned ROTC officer, Mike Martin, joined the project shortly after his arrival from Notre Dame in December 1970. Mike took over the experimental task when I left in August 1972, for a 10-month personnel exchange under the auspices of the U.S.-France Memorandum of Understanding (MOU) for a Cooperative Research Project in Helicopter Dynamics. It had been clear from the preliminary raw data that new and exciting details of the dynamic stall flow

phenomenon were contained in the measurements. Mike was encouraged to submit an abstract for presentation at the 1973 AHS Annual Forum in Washington, D.C., which he did.

Unfortunately, as Frank describes in his chapter, Mike Martin was diagnosed with a serious malignant cancer in December 1972. While Mike was recovering from surgery in the hospital, Frank got enough of the data tapes digitized and plots made that he and Mike started preparing the AHS paper. Mike called me in France for assurance that they had enough new information for a suitable paper.

I gave him my enthusiastic approval. Somehow, they met the deadlines, and Mike became strong enough to travel to Washington in May to present the paper at the Forum. His paper and presentation were well received, and the paper was published in the *Journal of the AHS* [7]. Figure 2 shows some of the results. Sadly, Mike died on October 30, 1973.

French MOU: 1972–1973

The U.S.-France MOU for a Cooperative Research Project in Helicopter Dynamics was signed in late 1971, permitting research scientists from the Ames-Army Lab and ONERA to collaborate on fundamental research in helicopter aeromechanics. A few months earlier, Paul Yaggy had taken Irv Statler and me with him to a conference on “Advanced Rotorcraft” at NASA Langley, sponsored by the North Atlantic Treaty Organization (NATO) Advisory Group for Aerospace Research and Development (AGARD) Flight Mechanics Panel. Paul wanted to introduce me to internationally renowned Philippe Poisson-Quinton of ONERA (commonly known as “PQ” outside of France), who would become my “patron” during my stay in France. During the course of dinner one night and a stroll afterward, I suggested that the success of the MOU would be enhanced by face-to-face collaboration of the research participants, i.e., technical personnel exchanges. I volunteered to make the sacrifice of going to France, and they agreed to think about it.

Although aerodynamics would rapidly enter the picture, the French management had indicated that their initial participation would likely be within the Structures Department of ONERA. Therefore, Paul told me to propose a paper on retreating blade stall for the next meeting of the AGARD Structures and Materials Panel in Denmark in April 1972, where Roland Dat of the Structures Department would be in attendance and would like to meet me. I did just that, and had a very successful session with Monsieur Dat. I also met Jean-Joel Costes, who also gave a paper there, and who would later spend 5 months with us at Ames. Monsieur Dat indicated that my first job would be to help him define the tasks and prepare work statements for the MOU.

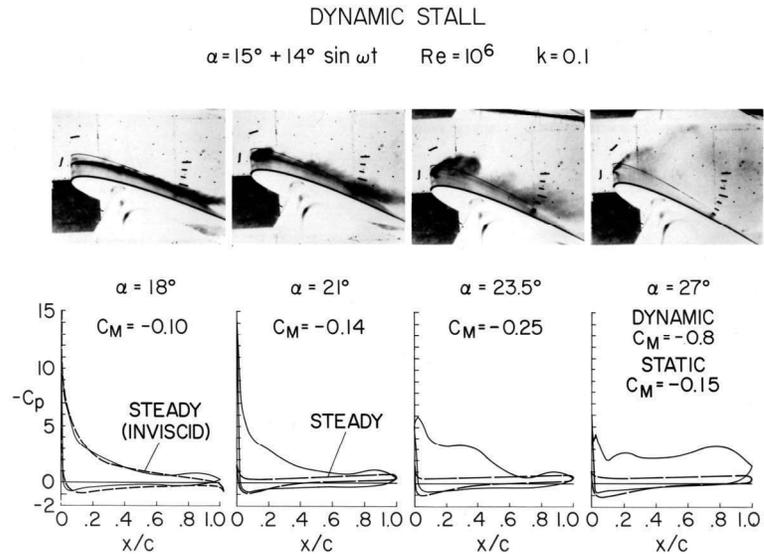


Figure 2. Flow visualization and pressure measurements of the vortex shedding phase of dynamic stall on the large oscillating airfoil [7]. (A tribute to J. M. Martin 12/18/45–10/30/73.)

After the AGARD conference in Denmark, I made a whirlwind trip to the National Aerospace Laboratory in Amsterdam, to ONERA in Paris (where I met Jean-Jacques Philippe), to the Royal Aircraft Establishment, Farnborough and Bedford, and to the University of Southampton. Then home to prepare for the adventure of living in Paris.

My wife Betty, who spoke French, was enthusiastic from the beginning about living in Paris, and we decided the timing should coincide with the academic year for our daughters, Nancy (age 11) and Susan (age 8). But when they saw how I was struggling with a crash course in French 4 days a week at the Berlitz Language School in Palo Alto, they became apprehensive about enrolling in a French public school, where they wouldn't know anyone nor their language. Fortunately, PQ and his charming wife, Anne Marie, had a daughter who had taught at a bilingual private school in Paris that had a special adaptation curriculum for English-speaking foreigners. PQ assured us that Nancy and Susan could gain admission. That diffused our anxiety (somewhat), and in the end, it turned out to exceed almost all our expectations. On the other hand, my Berlitz experience was far less successful. I mainly learned to not fear (not too much anyway) the wrath of the French when I butchered their beautiful language. Perhaps this attitude derived from the uncommon culture of the Army group at Ames, where in the early days we were encouraged to innovate, to take risks, and to not be afraid of making mistakes.

After a very hectic spring and summer, we arrived in Copenhagen on August 23, picked up a car at the Volvo factory in Sweden, and drove to Paris with overnight stops in Amsterdam and Brugge, Belgium. Anne Marie Poisson-Quinton had arranged lodging for us in the 6th Arrondissement until we could find an apartment. This was almost impossible, but fortunately we found one from a list at the American Embassy that was in the same part of Paris as the girls' bilingual school.

We barely had time to drop off our luggage in our apartment before leaving for the AGARD Fluid Dynamics Panel (FDP) Conference on "Aerodynamics of Rotary Wings" in Marseilles, for which Paul Yaggy was Co-Chairman. Bob Ormiston, Frank Caradonna, and I gave papers at the conference (Frank's paper was co-authored with Bill Ballhaus). All three of us were pleased and inspired by the quality of European rotorcraft research that was presented, and Paul seemed to take pride and satisfaction in the success of the conference, with 3 of the 25 papers coming from AARL.

Back in Paris after the Marseilles conference, I was assigned to share the office of Jean-Jacques Philippe, and I was introduced to the staff of the Aerodynamics and Structures Departments. Each time that I attempted to say a few words in French, the Frenchmen would shift to English, some good and some not so good. Jean-Jacques wanted to improve his English, so we agreed that he would speak in English and that I would try to speak in French. That way the vocabulary and sentence structure would remain simpler and more easily understood than the other way around. Our conversations were slow, but this strategy worked reasonably well; Jean-Jacques improved faster than I did.

In the days that followed, Jean-Jacques showed me the ONERA subsonic wind tunnels in nearby Chalais-Meudon, where later he would produce the data to validate Frank Caradonna's transonic rotor calculations [8].

Our collaborative research efforts were mostly on the subject of dynamic stall on oscillating airfoils. We both had run preliminary experiments, and we both had failed to predict the observed vortex-shedding phenomena with the available methodology. With some new French data from Toulouse, Jean-Jacques and I showed improvements in some of the predicted features of simpler unsteady boundary layers [6], but not the vortex-shedding phenomenon that distinguishes dynamic stall from static stall. Interestingly, 40 years later, dynamic stall is still not completely understood or consistently predicted.

My family and I left Paris on June 30, 1973, and returned to California on July 22, by way of The Netherlands, Copenhagen, the Volvo factory in Sweden, and Texas, to visit family. By this time, the Army-NASA Joint Agreement had been expanded to include “Directorates” at NASA Langley, NASA Lewis, and Ft. Eustis, under the command of the new Army Air Mobility Research and Development Laboratory (AMRDL) at Ames led by Paul Yaggy.

We were barely settled when Jean-Joel Costes of the Structures Department at ONERA arrived on September 3. Joel was the first person from France to come to the U.S. under the auspices of the personnel exchange task of the MOU. He was assigned to Bob Ormiston’s Rotorcraft Dynamics Group. I don’t remember what project he worked on, but Paul Yaggy, head of AMRDL, and Irv Statler, Director of the Ames Directorate, liked the suggestion that Joel and I should make a trip to the major American helicopter companies, similar to the trip Paul and Andy Morse had sent me on in November 1966, when I first arrived at Ames Research Center.

The NASA Ames Travel Office cheerfully took on the task of scheduling a whirlwind trip in mid-November for an Army employee and a French national, flying to Hartford, Connecticut (Sikorsky Aircraft and United Technologies Research Center), Philadelphia, Pennsylvania (Boeing Vertol), Hampton, Virginia (NASA Langley), Dallas/Fort Worth, Texas (Bell and my parents, about 50 miles away), and back to San Francisco International Airport in 8 days. Of course, that schedule probably paled in comparison with what they often did for Paul Yaggy. Anyway, Joel came back very pleased, but exhausted; partly from jet lag and partly from having to carry every conversation in English.

The Dynamic Stall Decade: 1973–1983

Second-generation oscillating airfoil experiments. I was eager to return to the large oscillating airfoil experiment that Mike Martin and Frank Caradonna had reported on at the 1973 AHS Forum [7]. A new series of tests was planned to explore the effects of leading-edge geometry, since that appeared to be the region that emitted the prominent dynamic stall vortex. About that time, it was commonly believed that the onset of dynamic stall was associated with the abrupt “bursting” of a laminar separation bubble that forms downstream of the suction peak in the chordwise pressure distribution. Therefore, we wanted to examine the relationship between this bubble and the shed vorticity that feeds the dynamic stall vortex.

Figure 3 shows the 4-foot-chord NACA 0012 model in the 7- by 10-Foot Wind Tunnel. The NASA Ames Model Shop constructed a series of leading-edge extensions to this basic NACA 0012 geometry, including reducing the leading-edge radius by factors of 1.5 and 3, and drooping the nose to replicate the French “0012 à Extension Cambré” profile that Jean-Jacques Philippe and I had worked on at ONERA. Additional pressure transducers, hot wires, thin-film skin-

friction gages, smoke ports, and tufts were installed on the model. The data-acquisition system was upgraded from paper strip-chart recorders to analog magnetic tape recorders, whose signals were converted to digital format off-line by NASA.

Ken McAlister assumed responsibility for processing the pressure data and for filming the smoke-flow visualization, and Larry Carr calibrated and monitored the hot-wire anemometry. Bob George played an invaluable role in all aspects of the pressure instrumentation and in recording the signals of all the sensors onto the analog tape recorder, and Ozzie Swenson designed and built a number of special analog circuit boxes. Jean-Jacques Philippe joined the team during his 6-month stage at the AMRDL, February to August 1974.

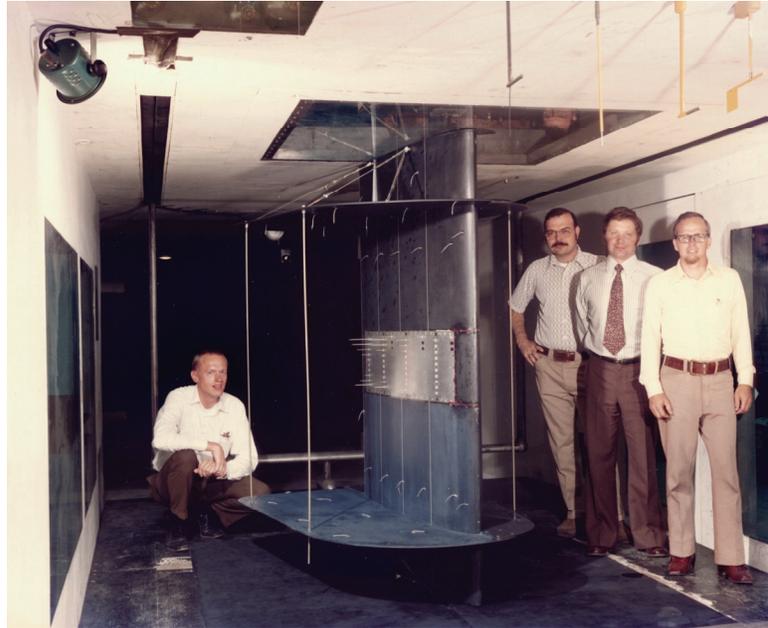


Figure 3. The large oscillating airfoil installed in the 7- by 10-Foot Wind Tunnel. From left, Larry Carr, Ken McAlister, Jean-Jacques Philippe, and Jim McCroskey.

Three different types of boundary-layer separation and stall were observed in this experimental campaign. First, the aforementioned “bubble bursting” was observed, but this phenomenon only occurred on the model with the smallest nose radius and on the ONERA Cambré at large negative incidence. Second, and more often, an attached turbulent boundary layer emerged from the bubble and separated abruptly some distance downstream. Third, trailing-edge stall on the ONERA Cambré developed from a relatively gradual progression of separation from the trailing edge toward the leading edge. Despite these differences, the shedding of the strong dynamic stall vortex from the leading-edge region remained the dominant feature.

The highlights of this test were presented at the American Institute of Aeronautics and Astronautics (AIAA) Aerospace Sciences Meeting in January 1975 and published in reference [9]. Ken McAlister’s and Larry Carr’s meticulous analysis of the data was documented in references [10, 11].

Third-generation oscillating airfoil experiments. Having documented the effects of changes in the leading-edge region of the classical NACA 0012 airfoil, we turned our attention next to advanced airfoils that were being proposed, or in some cases already being flown, on contemporary helicopters. Seven distinctly different helicopter sections and a fixed-wing supercritical airfoil shown in figure 4 were tested in the Army 7- by 10-Foot Wind Tunnel at Mach numbers up to 0.30 and Reynolds numbers up to 4×10^6 . The NACA 0012 airfoil served primarily as a standard reference section, and the fixed-wing supercritical NLR 7603 extended the range of leading-edge geometries.

The 2-foot-chord models of the eight airfoils shown in the figure consisted of interchangeable upper and lower shells that were constructed of wood and fiberglass. These shells surrounded a stainless-steel spar that spanned the 7-foot vertical dimension of the wind tunnel test section and that contained the instrumentation and wiring. The shells contained special fittings for the pressure transducers, hot wires, and hot-film skin-friction gages that facilitated model changes without disconnecting the instrumentation. After considerable dialogue with the NASA Ames Machine Shop, which constructed the steel spar, it was decided to precision-machine each set of shells while they were mounted on the spar. (I have forgotten the logistics of how this was actually done, but it was a major accomplishment, made possible by the friendly cooperation engendered by the Army-NASA Joint Agreement.)

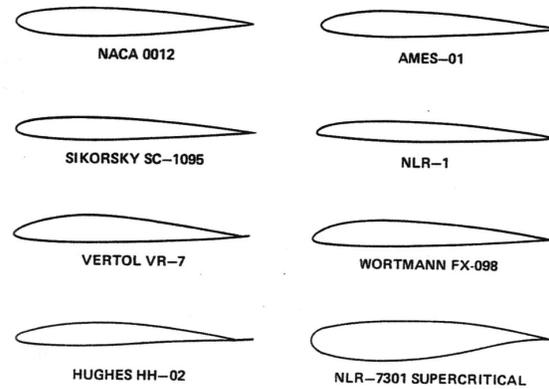


Figure 4. Advanced airfoils tested in the Army 7- by 10-Foot Wind Tunnel.

This test campaign was unique in that the eight same-sized models were tested in the same facility with the same instrumentation, and with the capability to match the Mach and Reynolds numbers and key parameters of the unsteady airfoil motion, i.e., frequency of oscillation, mean angle, and amplitude. As in the previous test, Ken McAlister and Bob George managed the pressure data, and Larry Carr monitored the hot-wire and skin-friction signals. Steve Pucci and Lieutenant Bob Indergand, U.S. Air Force, participated in the wind tunnel operation and in the data acquisition and reduction. Ozzie Swenson again built a number of special analog circuit boxes in support of the online monitoring and data acquisition. Pressure data from a total of 1,225 test points were archived, and later were copied by Ken McAlister onto a single digital compact disc, labeled “AIRFOILS,” circa 1998.

The results of this experiment showed important differences between airfoils, which would otherwise tend to be masked by differences in wind tunnels. All the airfoils tested provided significant advantages over the conventional NACA 0012 airfoil. However, the parameters of the unsteady motion were found to be more important than airfoil shape in determining the dynamic forces and moments. In contrast with the results of the previous tests at low Mach number, locally transonic flow was observed near the leading edge of the seven helicopter airfoils for free-stream Mach numbers above about 0.2.

The highlights of this investigation were presented at the AHS Annual Forum in May 1980, and published in reference [12]. The details of the experiment and plots of the data are given in references [13-15].

Subsequent dynamic stall experiments. Following the tests of the eight advanced airfoils, a number of other important dynamic stall investigations were conducted at Ames Research Center:

- Experiments in the Army Water Tunnel by Ken McAlister, Larry Carr, Dave Weaver, Chee Tung, Soogab Lee, and P. Plantin de Hugues.
- Flow visualizations of unsteady flow separation and dynamic stall [16, 17].

- Flow control by upper-surface blowing, and extendable slat and profile changes.
- Stall suppression by a leading-edge slat.
- Dynamic stall on an oscillating wing by Ray Piziali [18].
- Compressibility studies in the Ames Fluid Mechanics Laboratory by Larry Carr (Army) and M. S. Chandrasekhara (NASA).

A Glimpse of the World of CFD

One of the important success factors in the Army-NASA Joint Agreement was the close coordination between Army and NASA people working on common, or closely related, tasks. However, with a few notable exceptions, this was not particularly my experience in my first decade at Ames Research Center. Rather, it was mostly the overall informality, creative atmosphere, supporting services, and especially the activities of my Army and NASA colleagues that I found so rewarding and stimulating. Now I was ready to work on something different. NASA Ames was at the forefront of the rapidly evolving field of Computational Fluid Dynamics (CFD). Harv Lomax's CFD Branch was populated with world-renowned seasoned veterans, and Bill Ballhaus, an Army employee, had been installed in 1978 as Chief of the newly formed Applied Computational Aerodynamics (ACA) Branch. Therefore, this seemed like a good place to start a new effort.

With my weak background in CFD and computer science, I knew I couldn't compete in developing new algorithms or in writing new code, but I had published several comprehensive technical review papers and reports [19-22], and I had some experience with experimental research. So, I talked with Vic Peterson (Chief, Thermo- and Gas-Dynamics Division) and Irv Statler about possibly bridging the growing gap between the numerical analysts and experimentalists as each group became more specialized. Both managers agreed that such a role could be useful, and we decided that I should have a desk in the ACA Branch. This arrangement would further demonstrate the close interaction engendered by the Joint Agreement. And so, a few months later, I took up residency among the ACA Branch on the first floor of Building N-202A. The CFD Branch occupied the second floor, and the NASA Ames Library was conveniently adjacent in N-202. By this time, Bill Ballhaus had been chosen to replace Dean Chapman as Director of the NASA Ames Astronautics Directorate, and Paul Kutler had been selected Chief of the ACA Branch.

Peter Goorjian had left the Ames Directorate when he finished his Army tour of duty in 1972, and he returned to the ACA Branch of NASA Ames in 1976. He and Kristin Hessenius had inherited ownership of the 2-D unsteady versions of the transonic small-disturbance codes that Bill Ballhaus had initiated after his collaboration with Frank Caradonna. Peter patiently guided me through the listings of the code LTRAN-2, and I decided to try to add concentrated vortices to the flow field to simulate rotorcraft blade-vortex interactions, commonly known as BVI. Once we got it working, we had a new tool to study the generation of noise due to unsteady BVI. Soon thereafter, a former colleague of Paul Kutler, G. R. Srinivasan, who had experience with Navier-Stokes CFD codes, joined us as a contractor. Srinivasan's task was to replicate our transonic small-disturbance capabilities with Euler and Reynolds-Averaged Navier-Stokes (RANS) technology. This was a big step forward for us.

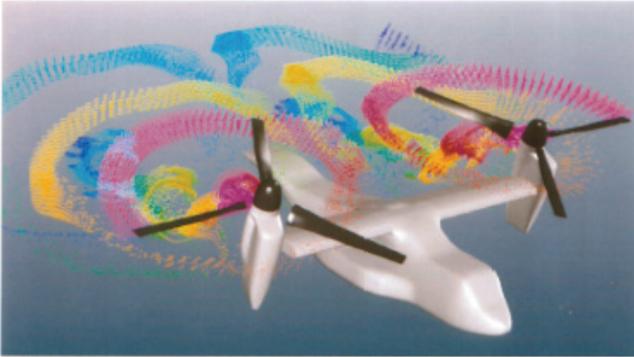


Figure 5. CFD simulation of the V-22 Osprey tiltrotor aircraft at 75 knots [22]. Unsteady particle path traces of the tip vortices.

Concurrently, Dean Chapman highly recommended one of his graduate students at Stanford, Jim Baeder, to Harv Lomax at Ames. NASA had no openings at the time, but Lomax passed the recommendation on to me. I convinced the Army management to hire Jim, to assign him to work with me in the ACA Branch, and to partially fund Srimi, thus forming the critical mass of a small Rotorcraft CFD (RCFD) group. As I recall, John Bridgeman joined the RCFD group circa 1983–1984. We felt stimulated by the creative environment that surrounded us, and the whole ACA Branch was growing.

A major step in the development of supercomputer technology that had an impact on the RCFD group occurred in 1987. NASA unveiled the Numerical Aerodynamic Simulation (NAS) facility, later renamed the NASA Advanced Supercomputing facility. The ACA Branch, including the RCFD group, was chosen to be one of the primary application guinea pigs for these new high-powered supercomputers and their supporting hardware and software. The NAS Building, N-258, opened on my 50th birthday, March 9, 1987, to much fanfare.

Slowly, new people were added to the RCFD team: Roger Strawn, Earl Duque, Sharon Stanaway, C. L. Chen, Venkat Raghavan, and eventually Bob Meakin, Sungho Ko, Arsenio Dimanling, and Jasim Ahmad. The group was given generous allocations of computer time on the NAS supercomputers, and we responded by producing a series of CFD solutions for rotor blade tips, multiblade rotors, blade/vortex interactions, acoustic wave generation and propagation, and helicopter fuselages. Bob Meakin produced Navier–Stokes solutions for the flow around the V-22 Osprey tiltrotor aircraft in hover and transitional flight, an example of which is shown in figure 5, from reference [23].

At this point, life was good, and I could see definite progress toward applying advanced CFD technology to rotorcraft. But in early 1997, I felt it was time to pass the leadership of the RCFD group to Roger Strawn. This coincided with the formation of the fully integrated Army/NASA Rotorcraft Division. Roger quickly picked up the ball and ran with it, securing generous amounts of time on the rapidly advancing Department of Defense (DoD) system of supercomputers, as well as the NAS facility. Roger also obtained funding from the DoD High-Performance Computing Modernization Program (HPCMP) to hire additional personnel for a series of rotorcraft CFD software development activities.

Starting in 1996, with funding provided by the DoD HPCMP through its Common High Performance Computing Software Support Initiative (CHSSI), Bob Meakin, Andrew Wissink, and Mark Potsdam created a distributed-memory parallel-computing version of NASA’s well-known OVERFLOW CFD code. This new version of OVERFLOW provided enhanced capabilities for rotorcraft CFD applications, with demonstrated engineering applications for both the V-22 Osprey and RAH-66 Comanche rotorcraft development programs. Additional CHSSI

funding supported the coupling of Computational Structural Dynamics (CSD) computations of the rotor blade motion with the rotor CFD solution for the airflow around the blades. Mark Potsdam, Hyeonsoo Yeo, and NASA's Wayne Johnson demonstrated groundbreaking advances for CFD/CSD combined rotor airloads predictions compared with UH-60 Blackhawk flight test data [24].

The RCFD group received another boost in 2006 when the HPCMP established the HPC Institute for Advanced Rotorcraft Modeling and Simulation (HI-ARMS). The HI-ARMS team developed an entirely new CFD architecture, consisting of a relatively simple infrastructure and interchangeable plug-in modules for a variety of CFD flow solvers. The resulting software product, called "Helios" [25, 26], was specifically designed to treat complete rotorcraft, including relative motions between the airframe and multiple rotors, combined aerodynamic and structural dynamics of the rotor system, and high-fidelity capturing of rotor wake systems. Today Helios continues to undergo upgrades and extensions, such as automation for problem setup and grid generation, and solution accuracy and computational efficiency on next-generation parallel computers, funded jointly by DoD and the Army.

International Activities

AGARD. The NATO Military Committee's Advisory Group for Aerospace Research and Development (AGARD) is described in some detail in Irv Statler's Chapter. AGARD was the creative brainchild of Theodore von Kármán for international scientific cooperation among the NATO member nations. There were up to 11 technical panels populated by experts from government laboratories, universities, and aerospace industries. Paul Yaggy represented the U.S. Army on the AGARD FDP from 1968 to 1974, when he retired and nominated me as his replacement.

My first meeting as a member of the FDP was in Göttingen, Germany, in May 1975, and featured a symposium on Flow Separation and a special round table discussion (RTD) on the subject of Unsteady Aerodynamics. At the RTD, 10 invited speakers from 6 countries described recent accomplishments and work in progress that were felt to be representative of the current efforts within the fluid and structural dynamics communities. Paul Yaggy asked me to summarize the activities being pursued in the U.S., and later I was asked to present the highlights of all 10 presentations to the AGARD Propulsion and Energetics Panel at their next meeting in Monterey, California, in September 1975. These two exercises brought me into close working contact with leaders in European research institutions and with the talented and helpful staff of the NASA Ames Library (before the days of Google Scholar searches). In addition, I began to learn how to organize and think in terms of technical *reviews*, as opposed to presenting one or a few new scientific results. (It has been said that copying from a single source is called "plagiarizing," while copying from many sources is called "a review.")

Rotorcraft technologists were never in the majority on the FDP, although many of the topics that were discussed had broad application beyond fixed-wing aircraft and missiles. Paul Yaggy managed a major symposium in 1972 in Marseilles on "Aerodynamics of Rotary Wings," and I organized and co-chaired a symposium in 1994 in Berlin on "Aerodynamics and Aeroacoustics of Rotorcraft." In between those two major conferences, I served as Chairman of the FDP, and I was active in the Publications and Southern Flank Support committees. I also served on numerous program committees for Symposia, Specialists' Meetings, Lecture Series, Special

Courses, and inter-panel coordinating activities. Through the auspices of the FDP support program, we provided technical consultations, assistance, seminars, and equipment to Turkish universities in Ankara and Istanbul, including establishing a cooperative research agreement between NASA Ames Research Center and the Middle East Technical University. In 1995, I established a similar cooperative agreement with the Technical University of Lisbon. I was awarded the 1995 von Kármán Medal for my contributions to AGARD. Like the Statlers, Betty and I made many good friends through our AGARD contacts and activities.

U.S.-France MOU. In addition to the personnel exchanges mentioned in connection with my 10-month stage at ONERA, participants in the tasks of the MOU meet semi-annually to exchange technical information, review their progress, plan joint publications of the results, and coordinate plans for future efforts. Dr. Statler was the U.S. Project Officer until he left to become the Director of AGARD in 1985. I was pleased to remain actively engaged in the technical and administrative aspects of the MOU until my retirement in 2000.

The initial tasks, starting in 1972, were in the areas of dynamic stall and rotor blade dynamic stability. New tasks involving transonic flow on blade tips, unsteady turbulent boundary layers, blade/vortex interactions, wakes, acoustics, airloads data analysis, rotor/fuselage aerodynamic interactions, flow control of separation on fuselages, human factors, unmanned aerial vehicles, and autorotation analysis were added over the years, as tasks were completed and phased out and new ones started. It should be mentioned that in more than four decades, over 30 research scientists and technicians have participated in personnel exchanges of 2- to 10-months duration, but no funding or computer codes were exchanged. This exchange of personnel and information has been highly beneficial to the U.S. Army during the past 40-plus years.

Finally, this activity recently received the prestigious AHS Agusta/Westland International Helicopter Fellowship Award. Over the years, the name “Memorandum of Understanding” has changed to “Memorandum of Agreement,” and then to “Project Agreement,” but the collaboration is as strong and productive today as ever.

Concluding Remarks

My professional career was blessed with a series of extraordinarily lucky and timely opportunities. As I prepared to leave Princeton in June 1966 for an uncertain future, my faculty adviser, Professor Seymour Bogdonoff, told me that one of my most important career factors would be my first post-doctoral mentor. Little did I know at that moment that I would soon fall into the combined good hands of Paul Yaggy and Andy Morse, and the creative culture of NASA Ames Research Center.

I was assigned to a new, vital research organization, getting in almost on the ground floor. It still seems remarkable that two big government agencies were willing to combine and share personnel, programs, and facilities. These two agencies provided supportive management, and the NASA service organizations displayed an attitude of “What can we do to help you?”

The local management trusted and encouraged me and the other incoming young engineers to innovate, to take risks, and to not be afraid of making mistakes. They instilled a feeling of cooperation and friendly competition among the research staff. Positive outcomes were recognized and rewarded.

Our small groups operated inside a large, creative research center that encompassed a wide range of “aeronautics and space” programs and projects. There were lots of interesting things besides rotorcraft research going on at Ames Research Center, and there still are.

Individuals were encouraged to participate in activities outside of the Ames-Army Lab, such as professional societies (e.g., AHS, AIAA), seminars and courses at Stanford, technical conferences, and international activities. Also, there were many opportunities for hosting technical visitors.

Finally, I am grateful for the friendships that I made over the years that made my professional career and personal experiences so memorable and rewarding.

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Index

A

Advisory Group for Aerospace Research and Development (AGARD), 6-7, 13-14
Agusta/Westland International Helicopter Fellowship Award, 14
Ahmad, Jasim, 12
AHS Annual Forum, 5-6, 8, 10
American Helicopter Society (AHS), 5-6, 8-10, 14-15
American Institute for Aeronautics and Astronautics (AIAA), 9, 15
Ames Directorate, 8, 11
Applied Computational Aerodynamics (ACA) Branch, 11-12
Army, 1-3, 5-15
Army Aeronautical Research Laboratory (AARL), 1-2, 7
Army Air Mobility R&D Laboratory (AMRDL), 8-9
Army-NASA Joint Agreement, 2, 5, 8, 10-11

B

Baeder, Jim, 12
Ballhaus, Bill, 7, 11
Ballistics Research Laboratory (BRL), 1
Bell Helicopters, 2, 8
Blade-vortex interaction (BVI), 11
Boeing Flight Test Center, 4
Boeing Vertol, 2, 4-5, 8
Bogdonoff, Seymour M. (“Boggy”), 1, 14
Bridgeman, John, 12

C

Caradonna, Frank, 5-8, 11
Carr, Larry, 9-11
Cathode ray tube (CRT), 4

Chalais-Meudon, 7
Chandrasekhara, M. S., 11
Chapman, Dean, 11-12
Chen, C. L., 12
Common High Performance Computing Software Support Initiative (CHSSI), 12
Computational Fluid Dynamics (CFD), 4, 11-13
Computational Structural Dynamics (CSD), 13
Costes, Jean-Joel, 6, 8

D

Dat, Roland, 6
Dimanling, Arsenio, 12
Duque, Earl, 12
Durbin, Enoch, 3-4
Dwyer, Harry, 3

E

Empey, Wayne, 5

F

Fisher, Richard, 4-5
Flight Mechanics Panel (AGARD), 6
Fluid Dynamics Panel (AGARD FDP), 7, 13-14
Fogarty, L. E., 3
Ft. Eustis, 8

G

Garelick, 5
George, Bob, 9-10
Goorjian, Peter, 11

H

Ham, N. D., 5
Harris, Frank, 4
Hessenius, Kristin, 11

High Performance Computing Institute for Advanced Rotorcraft Modeling and Simulation (HI-ARMS), 13

High-Performance Computing Modernization Program (HPCMP), 12-13

I

Indergand, Bob, 10

K

Ko, Sunggho, 12

Kutler, Paul, 11

L

Lee, Soogab, 10

Lomax, Harvard, 4, 11-12

M

Mark, Hans, 2

Martin, Mike, 5-6, 8

McAlister, Ken, 9-10

McCroskey, Betty, 7, 14

McCroskey, Nancy, 7

McCroskey, Susan, 7

Meakin, Bob, 12

Morse, Andy, 2, 8, 14

N

NASA Advanced Supercomputing (NAS) Facility, 12

NASA Ames, 2-3, 5, 8, 10-11, 13-14

NASA Langley, 6, 8

NASA Lewis, 8

NATO Advisory Group for Aerospace R&D (AGARD), 6, 13

Navier–Stokes, 11-12

NLR 7603, 9

North Atlantic Treaty Organization (NATO), 6, 13

O

Office National d'Etudes et de Recherches Aérospatiales (ONERA), 4, 6-9, 14

ONERA Aerodynamics Department, 7

ONERA Structures Department, 6, 8

Ormiston, Bob, 7-8

Osprey, 12

P

Peterson, Vic, 11

Philippe, Jean-Jacques, 7-9

Piziali, Ray, 11

Plantin de Hugues, P., 10

Poisson-Quinton, Anne Marie, 7

Poisson-Quinton, Philippe, 6-7

Potsdam, Mark, 12-13

Princeton University, 1, 3, 14

Propulsion and Energetics Panel, (AGARD), 13

Pucci, Steve, 10

R

Raghavan, Venkat, 12

Reserve Officers' Training Corps (ROTC), 1-2, 5

Reynolds-Averaged Navier–Stokes (RANS), 11

Rotorcraft CFD Group (RCFD), 12-13

Round table discussion (RTD), 13

Royal Aircraft Establishment, 7

S

Sikorsky Aircraft, 2, 8

Srinivasan, G. R., 11-12

Stanaway, Sharon, 12

Stanford University, 12, 15

Stapleton, Cyril, 1-2

Statler, Irv, 6, 8, 11, 13-14

Strawn, Roger, 12

Structures and Materials Panel (AGARD), 6

Swenson, Ozzie, 9-10

T

Tung, Chee, 10

U

United Technologies Research Center, 8

U.S. Air Force, 10

U.S.-France Memorandum of Understanding (MOU), 5-6, 8, 14

V

Vertical/Short Takeoff and Landing (V/STOL), 4

von Kármán, Theodore, 13-14

W

Weaver, Dave, 10

Wissink, Andrew, 12

Wynn, Tom, 5

Y

Yaggy, Paul, 1-2, 6-8, 13-14

Recollections of Working at NASA Ames Research Center

Ken Mort
NASA

Hired

I was hired in July 1957 with a Master's degree in mechanical engineering from Stanford University. I had been going to school with a student deferment, and I had already received my "greetings from the President," so I didn't have a lot of time to think about it. I decided to go to work at Ames, where I had worked the previous summer, instead of being drafted for the Korean War by the Army or enlisting in one of the Military services. Ames was considered an essential industry, and if you went to work in essential industry, you were draft exempt. At the time, I wasn't sure if I wanted to work at Ames permanently but, in retrospect, I am very glad I did. Ames was a terrific place to work.



Ken Mort

I was assigned to the 40- by 80-Foot Wind Tunnel Branch where I had worked the previous summer. Bill Harper was the Branch Chief and Woody Cook was the Assistant Branch Chief. The Division Chief was Harry Goett. They were three outstanding world-class managers. Generally, at the 40- by 80-Foot Wind Tunnel Branch, the researchers were organized into groups of three or four. There was always a group leader who was the senior engineer, and the group often included a recent graduate or two. The groups were rather informal, and, if another group needed help for a wind tunnel test, other group members would help. As a result, in the early days, I got experience working with different engineers on a wide variety of models and aircraft. It was fun and a great learning experience.

Once a month there were project reviews for Harry Goett. There were 20 to 30 projects or so ongoing in the branch, and project representatives would describe their status. It was great for the new hires to see and hear about all the projects. After Harry left the division the reviews were discontinued, which I thought was too bad, but they did take a lot of time and involved the entire branch.

There were lectures for new engineers given by senior researchers such as R. T. Jones, John Spreiter, Sy Syvertson, and Morris Rubesin. It was great experience learning from world-class scientists. The wind tunnel mechanics were very helpful describing how the wind tunnel worked and should be operated. Bud Farris, the head mechanic when I was hired, was very informative and helpful.

New engineers were issued a 2-foot-long slide rule for improved accuracy. This was before pocket calculators or personal computers.



Most of the 40- by 80-Foot Wind Tunnel Branch in December 1958. Author kneeling to the left of John McCloud in the wheelchair; John pioneered helicopter testing in the tunnel. Branch Chief Woody Cook with the big cigar in the dark suit. The Assistant Branch Chief, Mark Kelly, in the back right with the bow tie and the large cup of coffee. Paul Yaggy in the third row, second from right. Jim Weiberg kneeling in the front wearing the patterned sweater. And the head computer, Ruth Gibson, the woman with glasses in the center of the third row.

Research

I initially worked with Bill Evans, with whom I had worked the previous summer. He was attempting to develop a procedure to predict the maximum lift coefficient of arbitrary two-dimensional airfoils. We used Theodorsen's airfoil theory to estimate pressure distributions on airfoils at increasing angles of attack. We then attempted to correlate the adverse pressure gradient with stall onset. We would calculate the maximum lift coefficient at the angle of attack at stall onset. We weren't successful in precisely predicting the maximum lift coefficient, but I learned about National Advisory Committee for Aeronautics (NACA) airfoils and their characteristics. We published a report describing the work [1].

Next I joined Paul Yaggy's group. Paul was a pleasure to work with, and he was incredibly patient. It was fun work, and I learned about a variety of test objects. At the time, I thought our group got all of the unusual projects, including research on propellers, ducted fans, parachutes, paragliders, and lifting bodies. The propellers were for vertical/short takeoff and landing (V/STOL) aircraft and were built by Vertol Aircraft Corporation and Curtiss-Wright Corporation. The Vertol propeller was from the Vertol 76 (VZ-2), a tiltwing V/STOL experimental aircraft

being test flown at Langley Research Center. Langley was performing flight research with the aircraft for the U.S. Army under the direction of the Office of Naval Research. The propeller had flapping hinges. In support of the flight tests at Langley, we did experiments on the aerodynamic characteristics with and without the flapping hinges, as well as experiments identifying the propeller vortex-ring state during descent conditions [2]. It was important for safety of flight to define the safe descent conditions. Eliminating the propeller flapping hinges would significantly increase the control power but would also increase the blade-bending stresses. Propeller operational boundaries were established using the wind tunnel test results, and Langley accomplished a successful flight-test program using our data to help avoid problem areas.

The Curtiss-Wright propeller was designed to have high normal force and was to be used for a tilt-propeller V/STOL aircraft. High normal force would allow reduction of the propeller angle of attack and possibly a reduction in the size of the wing required for cruise. Descent tests were performed to define the vortex-ring state boundaries for the propeller. After the propeller characteristics were defined in the tunnel, Curtiss-Wright (under the Tri-Service (Army, Navy, and Air Force) program) built experimental aircraft, the X-100 and then the X-19, using propellers that produced high normal force. The X-100 was subsequently tested in the 40- by 80-Foot Wind Tunnel, and Jim Weiberg was the project leader.

Blade strain gages were attached to the propeller blades. In those days, it was very time consuming to get the required instrumentation installed and hooked up. Support from the Instrumentation Branch was minimal and only as required. We used oscillographs to record the blade strains and, during tests, we would look into the small window on the oscillograph to monitor the blade strains. It was often a challenge to ensure that allowable strains were not exceeded. After the test, the oscillograph records had to be read manually, which also took considerable time.

After the propeller tests we would collect all of the cabling, which was on the order of 125 feet long, and put it into a couple of large boxes that Paul insisted on saving; it entailed a lot of time and work to make up new cabling, and getting help from the electronic technicians who were not part of the branch was often problematic. Saving the instrumentation cabling enabled more rapid preparation for the next tests.

The propellers were mounted to surplus airplane gearboxes, which were driven by 1,500-hp electric motors on the propeller test rig. These gearboxes had limited life but were usually acceptable for wind tunnel tests. On the graveyard shift, there would typically be two wind tunnel mechanics and one engineer. One of the mechanics ran the tunnel, and the other was the observer who watched for problems. There was an emergency stop button next to the observation window. During one graveyard shift as we were running a propeller test the observer said to me over the intercom, "Come up and see the contrails the model is producing." I ran up the stairs from the control room to observe the "contrails," which were from gearbox oil being vaporized by heat from a self-destructing gearbox and visible because of the very cold night! Needless to say, we shut down the tunnel immediately and had a cup of coffee while the gearbox cooled off. I was afraid that if we opened the gearbox too soon, the oil would catch on fire. The gearbox had died, and the smell from the vaporized oil persisted in the tunnel for several weeks. I got picked on for a few weeks for stinking up the tunnel.

NASA and the Army had a lot of interest in ducted fans, and Paul Yaggy's group began testing a semispan model of the Doak VZ-4DA V/STOL airplane sponsored by the U.S. Army. A spare aircraft 4-foot-diameter ducted fan was used. The Doak aircraft used a ducted fan, which could be rotated on each wing tip. We did many experiments and modified the fan so we could readily vary the fan blade angle. The ducted fans used inlet guide vanes for roll control of the airplane during hover and low speeds because the airplane fans were fixed pitch. We did experiments on the effects of varying the angles of the inlet guide vanes and on the effect of doubling the number of vanes. At high duct angles of attack, the flow on the upstream duct lip (lower lip for the airplane) would separate, causing a reduction in performance and a dramatic increase in noise. We defined the stall boundaries during the wind tunnel experiments. The ducted fans also had an exit vane, which could be used for pitch control or to decrease the duct angle of attack and increase the lip-separation boundary during transition or aircraft descent. We also did experiments on increasing the number of exit vanes to allow further trade-offs of the duct angle of attack and exit-vane angle. The airplane was able to successfully perform level, 1-g transitions from vertical flight to cruise flight. One of the impressive features of the Doak ducted fans was the exceptional quality of their construction; unlike many other early experimental aircraft and a tribute to a small company.

As part of the Doak experiments, the flow was surveyed at candidate tail locations. The 40- by 80-Foot Wind Tunnel had a survey rig that was supported by rails on the ceiling of the test section and primary diffuser. An operator rode in the control cab that was suspended from the rails. In addition to controls for movement of the cab, there were controls for movement of an arm that had six pitot-static direction probes at the end. Nearly the entire test section could be surveyed if required. Inside the cab was a water manometer board and a 70-mm camera. The operator would control the location of the probes and, when in position, take a picture of the manometer board. The film was developed by the Ames Photo Lab for subsequent reading of the recorded pressures by the test engineers. It was a primitive system, but was extensively used in the early days of the 40- by 80-Foot Wind Tunnel. The cab was accessed by a ladder from a hatch in the top of the test section. When the survey rig wasn't being used, it was parked at the end of the primary diffuser to minimize its blockage effects. Since I was the junior engineer for the Doak experiments, I rode in the control cab and controlled the survey rig. It wasn't a lot of fun because visibility was very poor, and it was scary climbing down the ladder and getting into the cab. It was way before the Occupational Safety and Health Administration, and I'm sure it wouldn't have passed. Several reports were written describing the results of the 4-foot-diameter ducted fan experiments; for example, see reference [3].

Experiments were also done on the 7-foot-diameter Bell Aerospace Company X-22A ducted fan. The Bell Aerospace Company X-22A airplane had four ducted fans; two in front, mounted on the sides of the fuselage, and two in back on short wings. This aircraft was part of the Tri-Service program. We did many experiments on this fan. It was a semispan model, and both the forward and aft ducted-fan positions were simulated. Performance characteristics were defined, and the duct-lip flow separation boundaries were determined [4]. The effect of vortex generators on the upstream duct lip to improve the separation boundaries was determined; this was important for descent conditions. The X-22A was a very successful airplane that could readily perform level, 1-g transitions from hover to forward flight.

During this period Berl Gamse joined our group. Berl was a lot of fun to work with. We conducted numerous experiments on parachutes, ducted fans, paragliders, and lifting bodies. During one experiment, Paul and Berl were running parachute tests and I was in the office planning the next test. The parachutes were attached to a control rig mounted to the tip of one of the main struts and were being flown horizontally. (We would sometimes fly the parachutes horizontally and sometimes vertically.) They were extending the shroud line length capability by adding about a 10-foot-long, 10-inch-wide flange I-beam forward on the top of the wind tunnel main strut so the parachute control head could be moved further forward about 10 feet, and the parachute line lengths could be increased accordingly. Paul called me and asked me to check the deflection of the I-beam. So I checked the I-beam and told Paul that it would probably hold up the world. A while later Paul called me again and said to come out to the tunnel test section to have a look. When I arrived at the test section, Paul was standing on the I-beam moving the control head side to side many inches. I had forgotten to check the I-beam in torsion (I-beams are poor in torsion). Somewhere in the archives there is a picture of Paul standing on the I-beam, which, of course, was about 20 feet above the floor. Needless to say, that was a lesson I never forgot, and I appreciated Paul's patience.

We did experiments on the North American Aviation paraglider, which had inflatable booms and an inflatable spreader between the booms; sail cloth was attached to the booms to form the triangular shape of the paraglider. It was going to be used for land landing the Gemini Capsule and, if successful, for land landing the Apollo Capsule. The paraglider achieved an impressive lift/drag ratio (L/D) of over 4 [5], but the deployment tests were a disaster. We tried it twice and destroyed two paragliders. The intent had been to use the partially deployed paraglider as a drogue chute, and after the capsule had slowed down, deployment of the paraglider would be completed. The problem was that the partially deployed paraglider was very unstable and would self-destruct. This effectively ended the North American paraglider program.

NASA became interested in lifting bodies for space-return vehicles. Dale Reed of Dryden Research Center (now Armstrong Research Center) proposed a light-weight flying model of the M2 lifting body. The M2 configuration was essentially a cone shape cut in half to achieve higher L/Ds than a symmetric capsule could achieve. The flying model was designated the M2-F1. Experiments were performed in the 40- by 80-Foot Wind Tunnel before flight-testing. The wind tunnel test was successful, and the maximum L/D exceeded 3, which was thought to be the requirement for unpowered landings [6].

The M2-F1 Flight-Test program was successful, without major incident, and led the agency to approve construction of the so-called heavy-weight lifting bodies. The M2-F2 and HL-10 were constructed by the Northrop Corporation. The HL-10 was Langley Research Center's version of a lifting body. The Air Force wanted to get into the act, so they sponsored the Martin Company's S-V5 lifting body. These aircraft were designed to be launched from the B-52 like the X-15 had been. All of the lifting-body flight vehicles were tested in the 40- by 80-Foot Wind Tunnel prior to flight. It was an interesting program and defined the minimum characteristics required for unpowered landings. The L/D was low (on the order of 3) and required that the landing gear be deployed at the last minute; the landing gear reduced the L/D to levels not much higher than 2, which was too low for a safe landing.

The last of the lifting-body configurations that our group tested was the M1L. It was not a flight vehicle; it was a wind tunnel model that had an inflatable afterbody. The model was built by

Goodyear Aerospace. The forebody had a wider angle so it had a greater volumetric efficiency than the other lifting bodies. However, the maximum L/D was low; it was only a little more than 2. The inflatable afterbody and control surfaces were successfully deployed and demonstrated the feasibility of the concept of an inflatable afterbody [7], however that was the extent of the M1L program because of the low L/D.

One day Paul Yaggy came by my office and asked if I wanted to go for a ride in the Goodyear blimp, Columbia. Of course, I said, “Absolutely,” so we drove Paul’s red VW over to the Oakland airport for a blimp ride. I think two of Paul’s daughters went with us. It was a lot of fun and an amazing experience.

In 1965, Paul Yaggy left our group to become Technical Director of the U.S. Army Aeronautical Research Laboratory (AARL) under the Army-NASA Joint Agreement at Ames. Berl left to go to Israel for a few years to help with their wind tunnel design and construction, as well as subsequent wind-tunnel experiments. I saw Berl when he returned; he had gotten married and was going back to school, but, unfortunately, I later lost touch with him. Mike Falarski was hired by the Army, assigned to the 40- by 80-Foot Wind Tunnel, and joined our group. Mike and I did many ducted-fan experiments together. The Army group hired many engineers and some were assigned to the 40- by 80-Foot Wind Tunnel; I’m sure I knew who most were at the time, but I’ve since forgotten. It didn’t matter who was who—we all worked very well together.

I didn’t work on rotorcraft even though many were tested in the tunnel for NASA and the Army. However, I did become the 40- by 80-Foot Wind Tunnel expert on armor plate as a result of the Cheyenne helicopter accident during a test in the tunnel. The Cheyenne rotor-tip weight (about 20 pounds) had penetrated the test-section tunnel wall in line with the observation room and knocked down a light fixture, which injured one of the test engineers. This was the first time the tunnel wall had been penetrated by a test object. I was assigned the task of determining test-section armor plate requirements. It was an interesting assignment. We met with the guy who designed the armor plate for the President’s limo, as well as an armor-plate expert who worked for Food Machinery Corporation in San Jose. We determined that the steel armor plate should be about 5/8 inch thick and strategically placed to protect personnel from flying objects in the tip-path planes of helicopter rotors and propellers. Armor plate was purchased and installed on the inside of the test section.

Occasionally Harlan D. Fowler would visit. He had invented the Fowler flap, which was used on many commercial aircraft. I think the airplane companies were still paying him residuals. He was a real character and was always coming up with new ideas—mostly bad. When Mark Kelly was Branch Chief, Mark would call me up and say Fowler was coming for a visit. The meetings would always start off with a really bad joke by Fowler. On one of the visits, he had an idea for a ducted-fan-powered, deflected-slipstream model. Mike and I were tasked with building the model and doing some experiments. The results were typical for deflected-slipstream configurations. The flap system wasn’t able to turn the flow a full 90 degrees, and the cruise drag was high [8].

I started working with Bill Eckert (another Army employee) when Mark Kelly tasked us to figure out why the maximum airspeed in the 40- by 80-Foot Wind Tunnel had slowly decreased over the years. Bill was energetic, hard working, and a pleasure to work with. We did several experiments together. Joe Piazza also joined our group. (Joe was an excellent designer and later

started the branch design group.) Joe designed a pitot-static probe that looked like a missile. It could be raised and lowered along cables that were stretched from floor to ceiling in the primary diffuser. We would get in the attic above the primary diffuser and then move the probe up and down the guide cables, using another cable that had pressure tubes attached to a manometer board in the attic, and record the pressure data. It was crude, but worked well in such a large tunnel. We also had rakes in the corners at the end of the primary diffuser, rakes in front of the fan drive, and static pressure tubes along the floor of the primary diffuser and north cross leg. After many experiments and analyses, we concluded that the loss in performance was probably due to many small effects, including holes in the tunnel walls and soot on the turning vanes and fan blades.

We left the diffuser and fan rakes in place for several years. I had one of the tunnel's 80-tube, water-manometer boards in the office hall hooked up to the diffuser rakes, and every time the tunnel was run I would see how the flow was doing in the primary diffuser. It turned out that the primary diffuser had "transitory stall." The flow would separate in one of the corners, then it would reattach and separation would occur in one of the opposite corners. The flow would switch back and forth with a period on the order of a half a minute. It was the source of low-frequency pulsing in the test section, which had been present from day one.

For a while I shared an office with Bob Page who was an excellent designer and aerodynamicist. Our office was close to where I had the manometer board set up. Bob and I discussed the unsteady flow in the tunnel. We thought, why not put in vortex generators at the end of the test section to energize the diffuser flow? We proposed the idea to Mark Kelly, and he said, "Go for it." So Bob and I studied vortex generators. Bob designed some simple ones that we put at the end of the test section within reach of the test-section crane. The vortex generators were a great success and exceeded our expectations. The diffuser flow was much steadier, and the test-section pulsing was greatly diminished. Not only was the diffuser flow improved, but the inflow to the drive fans was significantly improved. Needless to say, we became believers in vortex generators.

Study of New Large Wind Tunnels

The agency decided that there were limitations in the capability of the 40- by 80-Foot Wind Tunnel. It wasn't big enough to test modern fighters. Helicopters and V/STOL aircraft needed a bigger test section for low-speed tests, and the 200-kt maximum air speed was too low to test high-speed rotorcraft. At low airspeeds, helicopters and V/STOL aircraft produce wakes that become more vertical, and corrections for the effects of the walls are impossible to estimate. Recirculation caused by the tunnel walls causes the wind tunnel airflow to be unrepresentative of unconstrained flow. This condition was encountered during a significant number of tests. Experimental aircraft, and especially compound rotorcraft, were beginning to exceed the 200-kt maximum airspeed of the 40- by 80-Foot Wind Tunnel. It became highly desirable to be able to investigate these aircraft at airspeeds up to at least 300 kts [9].

As a result, the three Office of Aeronautics and Space Technology centers (Ames, Lewis, and Langley) began studies of new, large subsonic wind tunnels; it was a friendly competition. We called it the super-tunnel studies. I studied diffusers and non-return wind tunnels to try to figure out a way to make the tunnel more compact because of the very large test sections. Dave Hickey, the Assistant Branch Chief at that time, had started experiments on wide-angle diffusers that had slots to let in air. These slots would create an air jet at the start of the diffuser to energize the

boundary-layer flow so wide-angle diffusers could be used. I took over the experiments to study diffusers. I couldn't get Dave's diffuser to work consistently and gave up on the idea. We also tried vaned diffusers, which had good pressure recovery but unacceptably high losses. We essentially verified the diffuser that I had been studying.

We also did many experiments on non-return wind tunnels, which turned out to be a good approach to reducing construction cost. We designed and built a model in a very short time. At night I would make sketches and then the next day give them to Structural Fab, which was building the model. Conrad McCloskey, an excellent electrical engineer, tracked down an off-the-shelf angle-grinder motor that was advertised to produce 4-1/2 hp. The fan was another problem. I was talking this over with Jim Biggers at lunch one day, and he suggested that we go to the parking lot and check out radiator fans. We did, and we found a British Motor Corporation (BMC) Sprite sports car that had a relatively small plastic radiator fan. I visited BMC on my way home from work and talked to them about their fans. They said the original Sprite fans self-destructed at high RPM and sometimes took out the radiator as well; they even gave me a sample of a fan that had self-destructed. BMC said the replacement fans seemed to be doing okay and that I should call BMC racing in San Francisco. I called, and they said the replacement fans were good for about 10,000 RPM, so I bought one and we tested it. The price was right—it was \$4.50. Jerry Barrack and I built a single-fan test rig using an angle-grinder motor and a modified Sprite fan. We instrumented the test rig and actually measured over 3 hp delivered to the airstream, which was impressive, so I was happy with the result.

We then had eight motor and fan drives assembled for our super-tunnel model. We had decided that the eight fans in the drive system for the model would be arranged two fans high by four fans wide. It was a cost-effective drive system; the angle-grinder motors only cost \$95 each. Studies had indicated that a drive system with a three-high by six-wide fan arrangement would be required for the full-scale tunnel because of the availability and practicality of acquiring large motors and fans. During some of the experiments we simulated the 18-fan drive arrangement by using transition ducts downstream of the model drives; the concern was the effect of winds on the exhaust. The total power required for the full-scale wind tunnel was estimated to be on the order of 400,000 hp.

We did many experiments with the model in the 40- by 80-foot test section on a wide variety of inlet and exit treatments. Bill Eckert and Joe Piazza helped run the experiments and analyze the data; for example, see reference [10]. We concluded that a non-return wind tunnel was feasible and could have the same performance and flow quality as a closed-circuit wind tunnel, and would be significantly cheaper as long there was a screened area at the inlet and a vertical exhaust. Of course it was also very important to have, and maintain, a clear area in front of the intake.

Modification of the 40- by 80-Foot Wind Tunnel

Studies on modifying existing wind tunnels were also performed. As a result, NASA decided (with much encouragement from Ames) that modifying the 40- by 80-Foot Wind Tunnel would be cost effective and would improve large-scale subsonic wind-tunnel testing capability substantially. The approach was to increase the airspeed in the 40- by 80-foot test section and add a non-return 80- by 120-foot test section, which would share the drive and be part of a non-return circuit. The goal was an airspeed of 300 kts in the 40- by 80-foot test section and at least

100 kts in the 80- by 120-foot test section. The total drive power required was estimated to be about 135,000 hp. It was expected that the modified 40- by 80-Foot Wind Tunnel would meet test requirements for large-scale investigations into the foreseeable future and cost an order of magnitude less than a completely new wind tunnel [11, 12].

I led the group performing aerodynamic and acoustic studies. Extensive model experiments were performed, and three model test facilities were built: a 6-foot-diameter fan model that included the fan diffuser, a 1/50-scale model of the entire facility, and a 4-foot-diameter fan-driven vane and louver tester. Bill Eckert, Paul Soderman, and Bob Page helped perform the studies. New hire Ed Schairer helped with the fan-blade load studies. A shift's worth of test engineers (about six engineers) from the support service contractor that was running the Unitary Plan Wind Tunnels also provided help. Extensive contracted studies were performed by General Electric, Westinghouse, and Stanford University.

The 1/50-scale model was built in the NASA Ames Model Shop, where Gene Thomas was the lead craftsman. To build the 40- by 80-Foot Wind Tunnel part of the circuit, Gene used the original 40- by 80-Foot Wind Tunnel construction drawings. Six, 10-hp model motors were borrowed from Langley for the drive. I did the aerodynamic design of the fans using Clark Y airfoils, and the Ames Model Shop made the fans using spruce laminations similar to the original full-scale fan construction.

The 1/50-scale model was tested in the 40- by 80-Foot Wind Tunnel to develop the inlet treatment for the 80- by 120-foot test section. Based on our previous studies, it was to be oriented northwest to more or less head into the prevailing wind. Wind measurements taken out in the field where the inlet was to be located convinced us that minimum inlet treatment would be acceptable since it was rare that the wind shifted directions. In addition we had 25 years' worth of wind data that the Navy had collected at Moffett Field. It was, of course, mandatory that a clear area be maintained in front of the intake to ensure that there would be no disturbances to the airflow.

Acoustic baffles were proposed for the inlet, so Paul Soderman and his acoustics group performed full-scale baffle experiments in the 7- by 10-Foot Wind Tunnel.

All of these experiments were done in a relatively short period of time. Bill Eckert and Jean Jope wrote a code that gave wind tunnel performance based on procedures in various references. The software also gave wind tunnel wall pressures for the wind tunnel structural designers to use. The code was published later and is still being widely used [13]; it also contains some design guidelines. Bill also wrote an aerodynamic requirements document for the architect/engineers (A/E) design team.

The A/E team that designed the modified wind tunnel comprised URS/Blume, Ralph M. Parsons, and Fluidyne Engineering. URS/Blume was an expert in seismic analysis of large structures and URS did environmental impact statements. Parsons was an expert in designing large rotating machines, and Fluidyne was experienced in designing wind tunnels. During the design process, Dr. Hans Mark, the Ames Center Director, hosted environmental hearings for the local communities in the evenings. It was important for us to emphasize that the repowered drive would be significantly quieter despite absorbing nearly four times the power. We expected the maximum noise to be on the order of 10 dba lower than the existing tunnel. We pointed out that low background noise was also very important for the research that was being performed in the

facility. No community members showed up for the first meeting, so Dr. Mark took us all out for a beer at St. James Infirmary, a local restaurant. Subsequent hearings were well attended by community members and yielded positive results.

Construction of the modified wind tunnel began in November 1978. The tunnel was built by about 50 fixed-price, low-bid contractors; some were excellent and some were marginally acceptable. Periodically we would perform “value engineering” studies to save construction money.

Often, if a bid came in too high, we would see what could be done to revise the design. The in-house staff in Systems Engineering performed the redesign. Often this required aerodynamic analyses, which were done by Bill Eckert and me. Vane sets 3 and 4 were redesigned using commercial decking and reinforced streamlined struts, which were in the airstream. Vane set 5 was redesigned using flat-plate panels fabricated from plastic-cladded plywood that was used for truck bodies. The fans were originally planned to have aluminum spinners, but the fixed-price bids were much too high. Next, an in-house redesigned spinner using composites went out for bids; that also came in way too high. As a result, I proposed a fixed nose cone made out of sheet metal. It turned out to be cost effective. It required support struts, so we specified the shapes and locations to minimize their drag. The fixed nose cones worked out well. In addition, posts inside the primary diffuser were proposed to minimize reinforcement of the primary-diffuser cross-frames, which would have been expensive. The posts were streamlined per our aerodynamic requirements. The loss in performance was minimal and acceptable. The fan contractions length was cut in half by using compound curves for the steel walls. Because of the complexity of the shapes, many shop drawings were done in-house by Wil Vallotton. Not only did this save construction money, but the curved shapes were also aerodynamically superior to the straight cone sections of the original design.

After construction was completed, I was selected as project manager of the modification, and the Integrated System Test (IST) began in December 1981. The performance and acoustic goals were met, and the drive was exceptionally smooth. I am very proud of the aerodynamic and acoustic performance of the National Full-Scale Aerodynamic Complex (NFAC) drive. The fan efficiency was on the order of 95 percent,



The NFAC drive: six 40-foot-diameter fans with 15 rotor blades and 23 stators per fan.

despite poor fan inflow because of the boundary layer buildup (the boundary layer was about 20 feet thick at the fans), and at full power (135,000 hp) the drive was about 10 dba quieter than the original 36,000-hp drive.

However, the flow in the 40- by 80-foot test section was rougher and the fan inflows worse. The air-exchange system was problematic. To keep costs down, off-the-shelf louvers were used for the air-exchange intakes. This was the same system that had been used in the tunnel originally. To increase the air-exchange rate to accommodate the higher power and minimize cost, the number of louvers was increased. In addition, the flow quality in the 80- by

120-foot test section was not as good as desired. Turbulence was also higher than desired, and flow separation existed on the contraction walls near the inlet acoustic baffles, which also served as flow straighteners. The flow on the tails of the baffles was partially separated. During the IST, we developed a list of improvements and told NASA headquarters to expect a request for more construction funds when the IST was completed.

On December 9, 1982, when the IST was nearly completed, a catastrophic accident occurred during a heat run for the drive-motor acceptance. Vane set 5, just upstream of the drive, failed, and the drive was destroyed. There was also significant damage to the wind tunnel. Unfortunately, at the time, I was at the hospital with my wife, who had torn her anterior cruciate ligament on a ski trip and was about to undergo surgery. I rushed to Ames to survey the damage; it was very traumatic for me as well as for a lot of others. Up to the time of the accident, the performance and acoustic levels had met or exceeded our expectations. Needless to say December 9, 1982, was a very difficult day that I will never forget.

An accident board was formed, and Bob Swain of Langley was appointed chairman. An accident report was issued a couple of months later. Improvements, as well as repairs, were recommended and were agreed to by NASA headquarters. Areas of the project that were worrisome could now be fixed. The initial project had a construction budget of \$85M, which was difficult to maintain in view of increased steel prices and other unforeseen elements that frequently occur with the construction of large, unique facilities. A new project office was established, headed by Lee Stollar. John Peterman led the design group, and Vic Corsiglia, and then later Larry Olson, led the aerodynamics group. The final total cost of the project was \$122M.

A couple of months after the accident, Lloyd Walsh, the head of procurement, Dave Englebert, Systems Engineering Division Chief, and I went to Permali Gloucester Ltd., United Kingdom (UK), to order a new set of fan blades. John Peterman had marked up the original Blade Specification Statement of Work, and I worked on it on our flight to the UK. We met with Stan Richardson who had been the Permali project manager for the original set of fan blades. Stan was a real gentlemen and a pleasure to work with. The 6-fan drive required a total of 90 blades, 15 blades per fan. Headquarters allowed us to buy 15 spare blades, so we could replace a set of blades for one of the fans in the event of an accident. A short time later, I convinced Stan and John that we should claim that 3 of the damaged blades were repairable so we could have 18 spare blades. My rationale was that if we had 18 spares, we could put 3 blades on each motor if we had another catastrophic accident and run the tunnels at about 1/3 maximum airspeed. This would allow on the order of 100 kts in the 40- by 80-foot test section, which would still be a significant improvement over the Langley 30- by 60-Foot Wind Tunnel.

Management

After the accident, I was selected to be the Assistant Chief of the Systems Engineering Division to work with Dave Englebert who was the Division Chief. Dave was a pleasure to work with. Both of us liked technical work, so we shared the technical work as well as the management functions. I was also part of the review board reestablished for the modified 40- by 80-Foot Wind Tunnel. I tried to point out potential problem areas during the course of the redesign and construction and repairs. I wanted to conduct a better review than had been done previously, but, as a result, I think I annoyed Ames upper management as well as the project manager.

While I was Assistant Division Chief, I served as Bob Eddy's assistant for 3 months. Bob was the head of the Support Engineering Directorate in Building N-200. He was recruiting for an assistant and was rotating various people in the position for 3-month periods. Bob was good to work with, and I learned a lot about how Ames was managed. However, I decided that I did not want to become part of Ames upper management if I had the opportunity (which I didn't). While there, I was asked by the American Institute of Aeronautics and Astronautics (AIAA) to write a paper on the optimum subsonic wind tunnel (several members of the AIAA were asked to write similar papers). Paul Soderman and Larry Meyn helped me write the paper; it was, in many ways, a tongue-in-cheek report that essentially recommended that the original super tunnel would be the optimum subsonic wind tunnel. The paper had 70 references, many of which would be useful when designing subsonic wind tunnels regardless of size [14].

The IST for the repaired and improved tunnel started in September 1986 and finished in March 1987. The flow quality in the 80- by 120-foot test section was improved. The new air-exchange system worked well; air-exchange rates of up to 10 percent could be readily achieved. The wind tunnel improvements were successful for the most part, but the fan blade loads were higher. The magnitude of the loads had been somewhat higher than specified the first time around, but the second time the four-per-revolution (4/rev) stress levels were as high as the 1/rev stress levels, which was a problem because it shortened the life of the blades.

New High-Productivity NASA Wind Tunnel Studies

Later Dave suggested that I join the group that was studying new subsonic and transonic wind tunnels that had high productivity. Boeing had started studying new wind tunnels and recommended that NASA should do it instead. NASA agreed, and a temporary project office was set up at Langley Research Center and headed by Sammie Joplin. Sammie was a pleasure to work with and an excellent manager. The project team produced an outstanding report, *The Risk Reduction Study*. Later, a more permanent project office was established in which many of the airplane companies, including Boeing, were involved. The project office was at Lewis Research Center and was headed by the ex-head of Lewis, Larry Ross. I worked on the project for a couple of years, travelling to Lewis every week, but it began to look to me like it was never going to go anywhere. I decided to retire in January 1995.

Retirement

After I retired, I was an Ames Associate for several years, working on wind tunnel design guidelines and procedures. My intent was to write a report or a book describing how to design a wind tunnel in more detail than what currently exists. I never completed it—I think because I kept adding more information.

I greatly enjoyed my career at Ames. It was a lot of fun, and I learned a lot. I worked with some terrific people; many were world-class scientists. I do have to say that I enjoyed performing research and doing technical work with small groups a lot more than being in management.

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Index

A

Air Force, 3, 5
 American Institute of Aeronautics and Astronautics (AIAA), 12
 Ames Associate, 12
 Ames Model Shop, 9
 Ames Photo Lab, 4
 Ames Research Center, 1, 11-12
 Army Aeronautical Research Laboratory (AARL), 6
 Army, 1, 3-4, 6

B

Barrack, Jerry, 8
 Bell Aerospace, 4
 Biggers, Jim, 8
 Boeing, 12
 British Motor Corporation (BMC), 8

C

Cheyenne, 6

Cook, Woody, 1-2

Corsiglia, Vic, 11

Curtiss-Wright Corporation, 2-3

D

Doak, 4

Dryden (Armstrong) Research Center, 5

E

Eckert, Bill, 6, 8-10

Eddy, Bob, 12

Englebert, Dave, 11

Evans, Bill, 2

F

Falarski, Mike, 6

Farris, Bud, 1

Fluidyne Engineering, 9

Food Machinery Corporation, 6

Fowler, Harlan D., 6

Mort, K.

G

Gamse, Berl, 5
General Electric, 9
Gibson, Ruth, 2
Goett, Harry, 1
Goodyear Aerospace, 6

H

Harper, Bill, 1
Hickey, Dave, 7

I

Integrated System Test (IST), 10-12
Israel, 6

J

Jones, R.T., 1
Jope, Jean, 9
Joplin, Sammie, 12

K

Kelly, Mark, 6-7

L

Langley Research Center, 3, 5, 7, 9, 11-12
Lewis Research Center, 7, 12

M

Mark, Hans, 9
Martin Company, 5
McCloskey, Conrad, 8
McCloud, John, 2
Meyn, Larry, 12
Moffett Field, 9

N

NASA Office of Aeronautics and Space Technology,
7
NASA, 1, 4-6, 8, 11-12
National Advisory Committee for Aeronautics
(NACA), 2
National Full-scale Aerodynamic Complex (NFAC),
10
Navy, 3, 9
North American Aviation, 5
Northrop Corporation, 5

O

Office of Naval Research, 3
Olson, Larry, 11

P

Page, Bob, 7, 9
Permal Gloucester Ltd., 11
Peterman, John, 11
Piazza, Joe, 6, 8

R

Ralph M. Parsons, 9
Reed, Dale, 5
Richardson, Stan, 11
Ross, Larry, 12
Rubesin, Morris, 1

S

Schairer, Ed, 9
Soderman, Paul, 9, 12
Spreiter, John, 1
Stanford University, 1, 9
Stollar, Lee, 11
Swain, Bob, 11
Systems Engineering, 10-11
Syvertson, Sy, 1

T

Thomas, Gene, 9

U

Unitary Plan Wind Tunnels, 9
URS/Blume 9

V

Vallotton, Wil, 10
vertical/short takeoff and landing (V/STOL), 2-4, 7
Vertol Aircraft Corporation, 2

W

Walsh, Lloyd, 11
Weiberg, Jim, 2-3
Westinghouse, 9

Y

Yaggy, Paul, 2, 4, 6

A New Frontier—Rotorcraft Research at Ames

Robert A. Ormiston
Army

Preface

In retrospect, it seems highly improbable. What were the chances that I'd arrive in the right place, at the right time, and find all of the elements for an ideal career? The California environment, the NACA/NASA culture and Ames legacy, rotorcraft coming of age with intoxicating challenges of emerging technology, and the chance to get in on the ground floor of a new organization. The Army brought helicopters into the mainstream and they needed to know what made them tick. So the Army entered into an unprecedented new collaboration with NASA. Opportunities for research were everywhere and new tools were now available. That was the setting for a new frontier of rotorcraft research, and in 1968 I found myself in the middle of it. This is my story. After outlining my background, I will describe my introduction to the Army Aeronautical Research Laboratory (AARL) in 1967 and the period up to 1985. I will extend parts of the story beyond that time to cover the outcome of a couple of key projects. I will try to hit the highlights with enough technical detail for the reader to get a sense, I hope, of my experience and its significance.



Bob Ormiston, 1972.

My Aviation Interest Came Early (1952–1963)

Like most of my colleagues, I was not a product of the internet age, nor the computer age, and I grew up before the space race. I was, rather, a product of the Golden Age of Aviation that started after the Wright brothers, when America became aviation-minded and then went aviation-crazy after Lindberg [1]. As a young boy caught up in this atmosphere, I was entranced with airplanes and built small-scale flying models of my favorites, especially the WWII P-51 Mustang and F4U Corsair. At the same time, I was fascinated with the physics of flight although I found the technical details in the popular model magazines incomplete and unsatisfying. Noting my success at math and science, my dad pointed me toward aeronautical engineering almost before I understood the difference between an engineer and a train driver. When I was in the 5th grade, he was a salesman for McGraw Hill, the publisher of *Aviation Week*, and I became an avid reader. My knowledge and interests sharpened with time, and I attended Rensselaer Polytechnic Institute (RPI) majoring in aero engineering. RPI was suggested by a neighbor who worked at Cornell Aeronautical Laboratory (CAL), and years later people from CAL would figure significantly at both the Army and NASA at Ames.

During my freshman year, the space race had just begun. The American Institute of Aeronautics and Astronautics (AIAA) was formed from the merger of the Institute of Aeronautical Sciences and the American Rocket Society. My hobby had already introduced me to the National Advisory Committee for Aeronautics (NACA) and its key role in advancing U.S. aeronautical technology. At RPI, I learned much more about NACA research, consulting numerous NACA TRs and TNs and Wartime Reports in the Aero Engineering library. Exposure to NACA research, as well as the leading researchers, created a lasting impression on me. The NACA was

Ormiston, R.

soon swept into NASA and the focus on space intensified. Nevertheless, my interests remained focused on aviation.

Graduate School at Princeton (1963–1968)

Despite my long-standing interest in airplane design, my experience at RPI whetted my appetite to understand the deeper intricacies of aeronautical science and I was drawn to graduate work. Another possible incentive was that I wasn't ready for the real-world reality of a 40-hour workweek. When I sought advice from my RPI Professor Bob Duffy, he inquired about my interests. When I said, "flight mechanics," he replied, "then Princeton's the place." Thinking my chances of acceptance at the Ivy League were slim, I was surprised to receive a letter of acceptance and an offer of a full fellowship from Princeton's Professor Ed Seckel. I knew of Professor Seckel from my studies at RPI and he later served as one of my advisers; Professor Duffy's recommendation must have been a good one.

It was my great good fortune to arrive during the heyday of Princeton's Department of Aerospace and Mechanical Sciences. Along with Ed Seckel, I was fortunate to learn from Court Perkins, Dunstan Graham, Earl Dowell, and Enoch Durbin. Given the mentoring of my professors, advisors, and the technical staff, as well as relationships with my fellow students, Princeton turned out to be a pivotal experience. Many became lifelong friends including Fred Schmitz, Jim McCroskey, Jack Franklin, Vic Lebacqz, and Ed Aiken, and later became Army and NASA colleagues at Ames.

Phil Condit and I were office mates at Princeton, working in Tom Sweeney's low-speed aerodynamics lab along with George Sibert, a West Point Army Captain. We were studying a novel, foldable flex-wing concept called the Princeton Sailwing, and George piloted the test aircraft for performance and control evaluations. Phil designed a novel lateral control system and since he came from the University of California, Berkeley, he painted a California Condor and



The Princeton Mafia, NASA Ames, 1998. From left: Andy Kerr, Kip Edenborough, Jim McCroskey, Sheila Schmitz, Fred Schmitz, Ed Aiken, Jack Franklin, and Bob Ormiston. Not shown, Vic Lebacqz, Bill Decker, and Hiro Miura.

the name “Condit’s Condor” on the side of the airplane. I had spent the summer of 1965 working on vertical/short takeoff and landing (V/STOL) control systems at Boeing and when Phil was ready to leave Princeton with his Master’s degree, he asked me what I thought about the company. I gave it a good recommendation, but whether or not that made any difference, Phil joined Boeing and his career took him to the top; he rose to Chief Executive Officer (CEO) in 1997.

A Funny Thing Happened on the Way to Seattle

In the spring of 1967, after more than 4 years at Princeton and having put off reality as long as I could, I began to think about the future. Despite my plans to return to Seattle and work on the Boeing Supersonic Transport (SST), I decided to explore another option. Knowing of NASA Ames reputation in aerodynamics and flight research, I sent in a job application. NASA never responded, but one day I received a call from a Mr. Paul Yaggy. “Paul who?” I wondered. After explaining that he was the Technical Director of a new Army laboratory at Ames, he said that he’d be visiting Princeton in the near future and “would I be interested in talking with him?” When we met he explained that his small organization was just getting started and it was focused on helicopters. Despite having taken several rotorcraft and vertical takeoff and landing (VTOL) courses, I was intrigued by Boeing’s exciting new SST; helicopters seemed a bit old fashioned and distinctly outside the mainstream. I countered Paul’s sales pitch by suggesting that it would help if I could visit his laboratory at Ames. In retrospect I’m a bit chagrined by my chutzpah, but Paul was undeterred and responded with, “Sure, no problem, I can make you a consultant and bring you out for a couple of days.”

Soon I was on my way to experience the Golden State for the first time. To say the least, the next few days turned out to be unforgettable. Paul met me personally at San Francisco International Airport (SFO) and drove me to Mountain View in his infamous VW Bug. I met the staff of AARL, including Jim McCroskey, recently from Princeton, but already well established. Andy Morse showed me the Army’s small 7- by 10-Foot Wind Tunnel and then gave me a tour of Ames including NASA’s immense 40- by 80-Foot Wind Tunnel. He could tell I was impressed but I’ll never forget what he said: “Bob, what they do here in the full-scale tunnel is more like civil engineering than aeronautical engineering—in our small-scale tunnel we do *aeronautical research*.” Despite knowing the importance of full-scale research, I immediately grasped the wisdom of his remark and what it would mean in terms of opportunities for hands-on research. Seeing the excitement and dynamism of the new staff, I could feel the urge to be a part of this unique young organization.

After my Ames visit, I spent the weekend touring the Bay Area and this only heightened my growing interest. It was June, 1967, the Summer of Love in San Francisco. In perfect weather I visited Golden Gate Park and the Haight-Ashbury, rode the trolley cars, and visited Sausalito and Berkeley. It was truly, and—walking among the flower children in Golden Gate Park—literally, mind-bending. As I headed back to the East Coast, I was ready to let go of the SST and give helicopters a chance—at least for a while. I joined AARL in February 1968 with nary a second thought. The SST program was subsequently cancelled and Boeing went into a slump: “Will the last one out of Seattle please turn out the lights.” If I’d joined Boeing I might have worked on swept-wing jet transports for my entire career. Instead, with the Army at Ames, I quickly learned that rotorcraft were both serious business and technically fascinating—even after decades of effort, researchers had barely scratched the surface and, unlike fixed-wing technology, there

were still many basic technical issues waiting to be addressed. It literally was a new frontier—and I never looked back!

Moving to Ames, Life in Building N-215

In February, I left behind the winter in Buffalo, New York, and drove cross-country to California. Vic Lebacqz had generously offered his parents' hospitality in Redwood City while I looked for temporary housing in Mountain View. The next Monday morning, February 12, 1968, I arrived at Ames and headed for AARL in Building N-215. I can still see Jim McCroskey, bounding up the stairs on a temporary break from a 7- by 10-Foot Wind Tunnel test, calling out a quick hello as he headed to his office. Jim symbolized perfectly the dynamism of the young new organization. I was welcomed by Paul Yaggy, Colonel Cyril D. Stapleton, Andy Morse, and my new co-workers—and struck by a palpable sense of action and excitement. This was a happening place—and I had arrived. I must confess, however, fresh out of grad school, that a fleeting thought crossed my mind, “Would I fit in, could I keep up?”

Nevertheless, I was excited to be on the ground floor of a vital new organization with the opportunity to have an impact. In 1968 AARL was structured with a group of Army researchers in Building N-215 called the Army Aeronautical Research Group (AARG). Under the terms of the Joint Agreement with NASA, other Army researchers, comprising the Joint Aeronautical Research Group (JARG), were distributed throughout Ames organizations working under NASA supervisors as described in the Introduction.

There was an undeniably special feel in the atmosphere of Building N-215 in the early days; an eclectic family, presided over by Colonel Stapleton and Paul Yaggy. There were experienced old-timers like Frank Lazzeroni from NACA/NASA, and Gil Morehouse, retired from the Air Force, and Georgene Laub. A contingent from Hiller Aircraft included Andy Morse and Dave Sharpe with real-world helicopter development experience. And there were new graduates in addition to Jim McCroskey—Dean Borgman, Rande Vause, Wayne Empey, Tom Wynn, and others. Dean was from Cal Poly and, as my first office mate, helped acquaint me with AARL and the Bay Area. Dean was involved in wind tunnel testing, initiated research in acoustics, and became a key resource for Paul Yaggy. He quickly advanced to increasingly important management activities for AARL, successive Army organizations, and the helicopter industry, ultimately becoming CEO of Sikorsky Aircraft. As with Phil Condit at Princeton, I seem to have had a knack for boosting my office mates' careers more successfully than my own. In truth, I was far more attracted to the challenges of research than becoming a titan of industry so it all turned out right.

It was a special bonus that I became associated with Jim McCroskey at AARL. I didn't really know Jim at Princeton since he was a couple of years ahead of me and in a different research group. But in a remarkable coincidence, he had been a teenage hero of mine because of his reputation as three-time National Champion model airplane contestant. He was famous as the designer of a popular model of the legendary post-war F-51H Mustang. Both Fred Schmitz and I had built one of Jim's Mustang models long before we ever went to Princeton or came to Ames [2]! As AARL's first Ph.D. researcher, Jim was a pivotal early member of the Army staff and his energy, enthusiasm, and research accomplishments helped immeasurably as AARL began to establish its technical reputation. Moreover, by example he set a high standard for all of us later arrivals.

The administrative staff included Alta Steengrafe, Glenn Ross, E. C. Carvell, and the quietly competent Alice Tice. Glenn could be counted on to provide regular drama with his overwrought tirades about the administrative bureaucracy. And of course, the secretaries, including the effervescent Brenda Welk and energetic Barbara Goff, were essential. Barbara kept the engineers in line with irrepressible humor and energy. More than once in the days before the word processor I would hear, “And Bob, that’s it, I’m not going to type that manuscript one more time!” On the other hand, Don Adams who was in charge of our own brand-new IBM 1800 computer, with all of 50 kB of memory, and an IBM



The Mustang trio, Ames-Army Lab, Building N-215, at the time of Jim’s retirement, 2000. From left: Bob Ormiston, Fred Schmitz, and Jim McCroskey.

card keypunch machine, fretted that we engineers weren’t using it enough in the early days: “Bob, how are we going to spend all this money? If you folks don’t run more jobs, ‘they’ are probably going to take the computer away from us.” A group of us would gather for outdoor lunches and animated technical discussions on the cafeteria patio as the SFO Airways S-61 commuter helicopter clattered overhead on its way from San Jose to SFO. After-hours socializing included weekly bowling with Brenda Welk, Georgene Laub, Don Adams, Frank Lazzeroni, and Mike Kodani. Times change—patio lunches, SFO Airways, and the bowling group are now but long forgotten memories.

Early Assignments—Getting My Feet on the Ground

After my arrival at AARL I had a lot to learn. Several short-term assignments helped me get to know the people and the organization, and find my place within the Army and rotorcraft R&D. The environment could not have been more supportive. Initially I was involved in several experiments in the Ames 7- by 10-Foot Wind Tunnel No. 2 that NASA had assigned to the Army as part of the Joint Agreement. I learned the capabilities of the tunnel, how testing was conducted, and I got to know the tunnel mechanics, and instrumentation technicians, and the seasoned NACA old hands that Paul had recruited. As a young know-it-all there was nothing like being told what it was really like by Frank Lazzeroni who’d actually done it. Everyone was happy to help a young engineer learn about the real world.

Among the mechanics—Art Cocco, Dave Ray, Lou Puccinelli, and later Bob Gaines—the irrepressible Gene Wells stood out. Between tunnel runs, he’d offer me tips, whether I wanted them or not, on the best singles’ bars in Mountain View, and I kidded him by wondering how he, a married man, knew about these places. A few years later, I found him in the shop acting a little down and he confessed to me things were not going well at home. “Bob, I think I may be headed for a divorce, I’ve got an appointment to talk to a lawyer.” The next day I ran into him again and he was all smiles this time. Responding to my query, he said, “Well Bob, after talking to the lawyer, I discovered I couldn’t afford to get a divorce, so I decided to fall in love with my wife

all over again.” That was typical Gene, and he would bend over backward to do whatever it took to get a part or tool, or other material to get your experiment back on track.

Beyond the general orientation in the wind tunnel, Paul and Andy suggested several potential research activities. The Army’s AH-1G Cobra had been experiencing problems of tail rotor yaw control effectiveness during operations in Vietnam, and I initiated a research program to investigate the interactions of the main and tail rotor in ground effect. I visited Bell Helicopter in Texas along with Bob Huston from NASA Langley, and Bob Lynn (VP Engineering) and his staff briefed us on their investigations and experiences. Wayne Empey later joined with me to run model tests in the 7- by 10-Foot Wind Tunnel settling chamber. Eventually we all discovered the source of the problem. Reversing the tail rotor rotation direction to oppose the rotation of the main rotor wake ground vortex effectively solved the problem. This research was a good example of AARL’s ability to address the Army’s real world needs with technical expertise and research capability within the Army itself.

During this time I was also completing my Ph.D. When I arrived at AARL my thesis was incomplete. I was determined not to let my new environment distract me from finishing the thesis. Paul Yaggy felt the same and his encouragement and support, along with various Ames support services, were very helpful. Paul also helped with my local draft board after my student deferment expired. In applying for a work-related deferment, Paul feared my comments to the local draft board regarding the Vietnam conflict might be counterproductive, and he persuaded me accept a few of his suggestions and emphasize my AH-1G Cobra research.

As I was finding my way as a researcher, Paul and AARL were searching for an identity as an organization; how best to establish a presence, support Army needs, and contribute to Army Aviation R&D. These explorations led to some interesting, and at times dubious, projects in our 7- by 10-Foot Wind Tunnel (see Rande Vause’s chapter), including the flying casket, the Army’s Golden Knights human parachutists, helo-launched sensor packages for an electronic “Maginot Line” in Vietnam, and the all-singing-and-dancing white elephant sting mount for the wind tunnel—AARL’s best example of technical overreach. I was assigned as test engineer for one interesting concept, a free-wing tiltrotor proposed by Air Vehicle Corporation, a small R&D company in Southern California. The company president was a quiet, unforgettable Norwegian who flew Spitfires for the Royal Air Force in WWII and wore a patch over one eye. His concept was never successful, but the testing led to my first NASA/Army technical report [3].

Other early activities included jet-flap rotors, circulation-control airfoils and rotors, and monitoring a Ling Temco Vought (LTV) contract for tip-drive rotors and stopped-rotor VTOL research. Paul asked me to study advanced airfoil design for helicopter applications, and I became familiar with the work of Paul’s friends F. X. Wortmann and R. Eppler. Because of my long-standing interest in Prandtl–Glauert lifting line theory, I also began to investigate rotor wake modeling for both hover and forward flight. The complexity of rotor wakes presented an interesting challenge, and researchers had struggled with the problem for many years.

The free-flowing atmosphere of the early days at AARL can be illustrated by my early attempts to treat the vortex wake of a hovering rotor. Peter Goorjian, a recent Army Reserve Officers’ Training Corps (ROTC) graduate with a degree in physics and a Ph.D. in math, was assigned to AARL and became my office mate for a time. I tried to interest him in the complex helical-vortex rotor wake contraction problem but he became enamored of simplified momentum theory

instead. This approximate method dated back to the dawn of helicopter research and was inadequate for my purposes. He soon discovered a math error in the classical early work and, being a rigorous mathematician, was determined to find a solution. He eventually succeeded and published a note in the *AIAA Journal*. Although I wasn't much of a mathematician, I recognized Peter's skill. Peter later went on to collaborate with Bill Ballhaus as they pioneered the emerging field of computational fluid dynamics (CFD) aeroelasticity at NASA Ames as described in Bill's and Peter's chapters.

The Ames Culture and Learning Rotorcraft R&D From the Pros

Much of my early learning about the real world of rotorcraft and research came from the culture and environment of both NASA and the Army at Ames. That environment and the management style of AARL were significant contributors to its success. Colonel Stapleton, AARL's leader, exuded a genial sense of professionalism and unquestioned support for the entire staff. Nearly all of my technical guidance came from Paul Yaggy and Andy Morse. They created a working environment with very high esprit de corps and enthusiasm for research. AARL was a meritocracy with an atmosphere that fostered success that fed upon itself and multiplied. Jim McCroskey, Fred Schmitz, and I were very much beneficiaries of Paul's management approach in the early days of AARL.

Andy Morse, engineer par excellence, came from Hiller Aircraft with a solid background in helicopters and VTOL aircraft. He was a true rotorcraft enthusiast with vast practical experience and he loved to share that experience. Andy was Paul's right-hand man, later became Chief of the AARG, and he contributed much to the AARL environment. His management style was enthusiastic but low key; he made suggestions, pointed out opportunities, and brought out the ideas of the new staff. He reminded young researchers to keep the real world in mind when dreaming up nebulous research. He seemed more like a collaborator and helper than a manager.

I remember many conversations with Andy about his work at Hiller on ducted fan flying platforms as well as his views on compound helicopters, then the object of considerable attention by the Army. I still remember earnest conversations as he explained to me the inherent limitations of the compound helicopter and that it would never succeed, doomed by the weight and complexity of auxiliary wings and propulsion. He was absolutely convinced, and it wasn't until many years later that I began to seriously question, and then reject, that view. Alas, the jury has not yet declared me the winner.

In building the new organization, Paul brought in many young people, including Ph.D.s. As a young graduate student, I thought that a Ph.D. was a sure sign of competence and Paul seemed to think so too. But when a few of Paul's new hires didn't work out so well, I began to realize that that a Ph.D. alone was no guarantee. Sometimes, Paul would solicit my opinion of a job applicant; one in particular comes to mind. I was in Paul's office one day when he handed me a Form SF52 and asked, "What do you think of this one?" I briefly scanned the application noting that the candidate was graduating from Massachusetts Institute of Technology (MIT) in an accelerated Ph.D. program, and that he had "aced" all of his courses. Then I recognized him as the author of a recent article on rotor airloads in the *Journal of the American Helicopter Society* (AHS) that had recently caught my attention. I quickly suggested to Paul that it might be a good idea to make a job offer. He agreed, and it would be an understatement to say that the decision

worked out quite well for the Army and NASA: Wayne Johnson was certainly one Ph.D. who was destined for success.

I was struck from the beginning by the world-class rotorcraft R&D experts that Paul brought in to strengthen the technical environment of AARL. Many of us arrived with limited knowledge of the specialized technology of rotorcraft, and it was very stimulating to be able to learn first-hand from these eminent figures—about both their technical knowledge and their own unique experiences in learning research.

One of these learning experiences came from Penn State Professor Barney McCormick, an authority on helicopters and V/STOL aerodynamics, who worked at AARL during the summer. I remember a seminar he gave on mapping the trailing wake vorticity behind his instrumented L-19 aircraft. I had read his paper in the *Journal of Aircraft* and his description of his research approach seemed deceptively straightforward. His seminar, on the other hand, described the convoluted process of dealing with the real-world difficulties of flight research and how he overcame them. It was an eye-opening lesson that research is not always as simple as the final publication makes it seem.

I was also privileged to get to know one of the true helicopter giants, German pioneer Professor Kurt Hohenemser of Washington University, originally Prandtl's deputy at Gottingen and then Anton Flettner's designer [4, 5]. Hohenemser served as a key consultant on the Lockheed Cheyenne program. Paul Yaggy supported his research and eventually I was assigned as contract monitor. Through our many years of association, he taught me an enormous amount about rotorcraft aeroelasticity, rotor dynamics, and flight dynamics.

One memorable experience occurred when we separately visited Sikorsky Aircraft and decided to share the drive back to Kennedy Airport. During the several-hour trip, Hohenemser described his experience as a young engineer at Anton Flettner's pioneering helicopter company. Flettner had posed the question: "So what configuration should I pursue for my first helicopter?" Hohenemser first suggested the side-by-side rotor arrangement of the recently unveiled Focke-Achgelis Fa-223. Flettner objected saying that he was not about to copy his German rival, so Hohenemser proposed Sikorsky's single main and tail rotor configuration. Again, Flettner resisted—he wanted something unique for his helicopter. At that point Hohenemser suggested the intermeshing twin rotor, or synchropter. Thus was born the Flettner FL-282, the world's first production helicopter, later to be adapted by Kaman Aerospace. I realized later that I'd experienced a connection with the birth of the helicopter from one of the true pioneers. Though not as well known today, Hohenemser was, in my opinion, perhaps the greatest rotorcraft scientist-engineer.

Some of my earliest on-the-job training in helicopter research came from my associations with John McCloud and Jim Biggers, NASA engineers working in the 40- by 80-Foot Wind Tunnel under Branch Chief Mark Kelly. When I arrived at AARL, John and Jim were the only NASA rotorcraft researchers at Ames. In those early days, Paul Yaggy engaged in several collaborative industry and international activities—and was supporting a number of joint projects with NASA. Paul encouraged me to interact with Jim and John and, needless to say, they taught me much about rotorcraft R&D, wind tunnel techniques, and Ames. Jim and I later became involved together with the Cheyenne wind tunnel test. Jim and John were the nucleus of what would

become a much larger rotorcraft presence at Ames in subsequent years, in part as a result of the influence of the Army presence.

I also remember the genuine interest of NASA Ames top management in both the Army/NASA relationship and rotorcraft research. After Hans Mark became Director of Ames in 1969, he occasionally visited AARL in Building N-215 and I remember one informal session in our conference room. Jim McCroskey, Paul Yaggy, and maybe Frank Caradonna or Andy Morse were present. Hans sat on the table and asked insightful questions about the arcane details of helicopter phenomena like rotor blade stall, boundary layers, and rotor wake flow fields. I had a strong interest in the latter at the time, and Jim and I held forth on our ideas in response to Hans' questions. I remember him commenting expansively as we wrapped up the discussion, "Okay Jim, you're gonna solve the aerodynamics problems, and Bob's gonna solve the wake problem." A vast overstatement, but it sounded great at the time. Listening to a renowned nuclear physicist express genuine interest in the intricacies of helicopter aerodynamics certainly made an impression on this brand-new researcher.

The Army Lockheed Cheyenne—From Catastrophe to Career

Little did I know it at the time, but in September 1969, a catastrophic helicopter accident in the NASA Ames 40- by 80-Foot Full-Scale Wind Tunnel was to have an outsize impact on the course of my Army-Ames career in rotorcraft research.

The hingeless rotor is a fascinating development in rotorcraft technology. Although it traced back to the 1930s, interest in the concept increased significantly in the 1960s. Benefiting from advances in materials and technology, the cantilever blades improved control power, maneuverability, and flying qualities. They also eliminated the rotor blade flap and lead-lag hinges of traditional articulated rotors and therefore helped to reduce maintenance and improve reliability. Lockheed was a newcomer to the rotary-wing business in the late 1950s when it originated an innovative gyro control system for the hingeless rotor. They called it the "gyro-controlled rigid rotor." The company had great success developing small research aircraft prototypes and generated great interest with its impressive XH-51 helicopter. Then came the AH-56A Cheyenne.



(a) Before



(b) After

Lockheed AH-56A Cheyenne installed for testing in the Ames 40- by 80-Foot Wind Tunnel, September 1967.

In the mid-1960s the Army determined a need for an armed helicopter to escort the troop-carrying helicopters of the new airmobile warfare doctrine, and this became the Army's Advanced Aerial Fire Support System (AAFSS). Lockheed, leveraging its innovative new rigid rotor technology, took the established rotary-wing community by surprise and won the AAFSS development contract. The AH-56A Cheyenne was an advanced compound helicopter encompassing sophisticated new rotor and weapon systems technology.

Since AARL's mission was to expand the Army's expertise in such technology, Paul Yaggy wanted someone to address this new field and turned to Larry Carr. I had already been assigned to look into airfoil research opportunities for helicopters. When Larry mentioned to me that Paul had asked him to take on rotorcraft dynamics, we realized that we were more interested in each other's topic than our own and we hastened to inform Paul. Without hesitation, Paul endorsed our desire to switch topics. From that moment on I began to explore research in rotorcraft dynamics and aeroelasticity, focusing initially on the peculiarities of the novel hingeless rotor.

I started getting up to speed by supporting and monitoring the Cheyenne development program. Unfortunately, things were not going well for Lockheed, or the Army. The program ran into technical, schedule, and cost difficulties culminating in the tragic loss of an aircraft and test pilot in early 1969. After some deliberation, the Army and NASA decided to test one of the prototypes in the 40- by 80-Foot Wind Tunnel to investigate the technical issues. After preliminary testing with the pusher propeller and tail rotor, the main rotor was installed for checkout on September 17. For some reason I chose that day to join the test team in the tunnel control room to observe the initial testing. At one point while the rotor controls were being pulsed to check dynamic response at increasing levels of rotor thrust, I left the control room to see the aircraft close up through the tunnel observation windows. The big 51-foot rotor, just a scant few feet away, was an impressive sight as the blade tips shook ominously in response to the control pulses. The noise and vibration of the rotor, the 2500-hp T-64 turbine engine, and the roar of the tunnel at 100 kts created an unforgettable impression.

As I returned to the control room moments later, the team was preparing for the next collective pitch increase. I sat down on my stool next to Bill Anderson, a highly skilled, soft-spoken Lockheed dynamics engineer (years later Chief of Dynamics on the F-22 program in Marietta, Georgia), to await the next control pulse and monitor the rotor blade strain gauge data spewing out of the strip-chart recorder. Just as the Lockheed test pilot "flying" the rotor controls announced that he was increasing collective pitch to 10 degrees, Bill and I saw the blade strain gauge strip-chart traces suddenly begin to grow alarmingly. As the recording-pen traces diverged off scale, Bill turned to the pilot, and was barely able to utter the words: "Uh, wait a sec . . .," when all hell broke loose! There was a huge BANG; the control room shook like an earthquake, the lights went out, and the aircraft tumbled its way down the tunnel emitting deafening sounds before being stopped by the turning vanes. There was pandemonium—cries of distress came from the rear of the control room; someone yelled "fuel supply!" and nearly everyone, myself included, raced pell-mell for the exit. Left behind was poor Jeff MacDonald, an Army observer from the Army Aviation Materiel Laboratories (AVLABS) in Ft. Eustis, who had been injured by flying debris.

As we clambered down the stairs to ground level, Ray Prouty, one of the Lockheed engineers, was coming up the stairs. He looked at me with alarm, "Bob, you're bleeding!" I then realized I'd been nicked in the forehead by a small piece of glass from a fluorescent light bulb. It turned

out that, when the rotor went out of control, one of the blades cut off the tail boom and ripped the aircraft off the tunnel mounts. The 20-lb blade tip weight punched through the 3/8-inch steel tunnel wall, entered the low-ceilinged control room, and ripped out the fluorescent light fixture just above my head. The light fixture struck Jeff, while the tip weight, its kinetic energy spent, fell harmlessly into a box in the corner of the control room. Naturally, as Ken Mort describes in his chapter, NASA designed armor plating to ensure containment for future testing.

A couple of brave souls, the tunnel mechanics, stayed behind to secure the fuel supply, tend to Jeff, and assist John McCloud out of the control room in his wheel chair. Jeff ended up in the Stanford Hospital that night and called his wife back in Virginia to give her the news on their wedding anniversary. Milt Alberry, the Army mechanic, later received an award for bravery. After my encounter with Ray Prouty, I rushed over to the infirmary to be patched up with a tiny Band-Aid. I checked in with Paul Yaggy and then returned to my office to draft a memo describing the event. Later, Mark Kelly recounted his experience while sitting at his desk in his first-floor office located directly under the turning vanes downstream of the wind tunnel test section. When he heard the thunderous sound of the wreckage inexorably smashing its way down the tunnel toward the turning vanes, he feared the vehicle would break through the tunnel floor above his office and dove under his desk for safety. Fortunately, the ceiling held.

Over the next few days, the traumatic experience shook my psyche and my appetite for helicopters evaporated. A few days later, however, all I could think about was digging into the data to try to understand what went wrong. Little did I know at the time, but it would turn out to be a seminal event in my career and the beginning of a research odyssey during which I would discover and explore new areas of rotorcraft dynamics. Lockheed ultimately redesigned their feathering moment feedback gyro-control system to become the Advanced Mechanical Control System (AMCS) based on direct flapping moment feedback and a small internal gyro. The AH-56A eventually succeeded in meeting the original specs but by then the Army had cancelled the production contract, in part because of a roles and missions dispute with the Air Force, and the Cheyenne was dead. At the Ames-Army Lab, however, we were already underway on a multi-faceted research program that helped lay the foundation for much of the rotor structural mechanics and aeromechanics technical knowledge that exists today. The Cheyenne accident served as a strong stimulus for this research, and Army and NASA researchers played a significant role in these accomplishments.

In early October 1969, after an exhilarating year and a half at AARL, punctuated by the dramatic Cheyenne accident, I returned to Princeton for my thesis defense and final public oral examination. Under normal circumstances, facing my professors to defend my thesis as a graduate student would have been intimidating to say the least, but with the experience and confidence I had gained at AARL, the occasion was more liberating than intimidating.

As a postscript, my friend and classmate, Fred Schmitz, had also left Princeton with his thesis unfinished and was working at Boeing Vertol. After following me by 1 year at both RPI and Princeton, he ended up, 10 years later, defending his thesis at Princeton 1 day ahead of me. Shortly thereafter he joined us at AARL (see Schmitz' chapter).

Upon my return to Ames, with academia now behind me and my passion for rotorcraft growing, I was ready to immerse myself fully in research. The immediate fallout from the Cheyenne accident was to galvanize my interest in hingeless rotors and aeroelastic stability. The accident

was dramatic evidence of how much the technical community didn't know about advanced rotor technology. I could not foresee it at the time, but this experience was to open up multiple pathways to new fundamental knowledge that I pursued for decades, continuing even to the present day. Before delving into these areas, I should first elaborate on another event that took place 6 months after the Cheyenne accident.

The HLH Brief—Nothing but Stars

Here's another example of Paul Yaggy's style. I was initially hired at AARL as a GS-II with the promise of promotion to a GS-12 when I completed my Ph.D. When I was married in March 1970 at Stanford University, Paul attended the wedding. During the reception after the ceremony, in a bit of dramatic timing, he pulled me aside and slipped an envelope into my hand. I was to receive my degree in June, and the envelope contained a Form SF-52 with my official promotion. Before we returned to the merrymaking, Paul whispered to me, "And when you return from your honeymoon, I'll have a new assignment for you . . ."

Later, back in the office, Paul explained that he wanted me to prepare a briefing for the Army Material Command (AMC) Headquarters (HQ) on the technology readiness of the Army's upcoming new Heavy Lift Helicopter (HLH) program. The principal manufacturers were generating considerable hype about the virtues of their proposed contenders, and I was to compare their respective technologies and design concepts. These included Sikorsky's conventional single rotor design, Boeing Vertol's tandem rotor configuration, and upstart Hughes Helicopters novel but unconventional warm cycle tip-drive rotor system. For 1 short month, I dug into the designs and put together a picture of their relative merits. For a 120,000-lb helicopter, the tandem benefited from decreased transmission weight of two rotors having much less torque than a single rotor, but the Hughes tip-drive concept eliminated the heavy transmission entirely. On the other hand, gas flow losses within the rotor blades significantly reduced the overall efficiency.

Paul and I arrived at AMC on a Monday morning after taking the red-eye to Dulles Airport. It didn't fully sink in at the time that I would be briefing the Commanding General, four-star Joseph Chesarek. Paul, John Beebe, and Norm Klein wanted me to give them a dry run and they were, apparently, sufficiently satisfied. I remember being somewhat surprised that the audience in the small auditorium comprised about two-dozen officers; none had less than two stars. My punch line for the Hughes design drew attention to the inherent hazards of the flight crew's close proximity to the 500-degree-Fahrenheit engine exhaust gas ducting that filled the tiny fuselage of the external-cargo crane configuration. That point seemed to register with the General and overall, the briefing went well. I subsequently participated in the Source Selection Evaluation Board, and many months later Boeing's giant tandem XCH-62A won the competition. Alas, it never saw production.

The underlying rationale for the HLH briefing (which was not shared with me earlier) soon became clear when Paul dropped by my office a few days later. He casually informed me that our trip had succeeded because AMC had decided to approve an initiative that Paul was supporting, to expand the Army-NASA collaboration at Ames into the multi-center NASA-Army organization of the Air Mobility R&D Laboratory (AMRDL). The reason for the briefing was to showcase AARL's technical expertise and demonstrate the Army's in-house capability as a "smart buyer" for the development of new air-mobility systems. In the process, I was beginning

to more clearly understand and appreciate my role and AARL's role in expanding Army R&D capabilities. It also illustrated Paul's trust and confidence in the abilities of his new young researchers to handle challenging briefings. At the same time, he demonstrated his managerial skills supporting and motivating his staff.

Getting Started in Aeroelastic Stability Analysis

After the dust settled from the Cheyenne accident, and being duly stimulated and challenged, I began to delve into aeroelastic stability of hingeless rotors. At the time a somewhat heated controversy was underway in the *Journal of the AHS* about the nonlinear stability of coupled flap and lead-lag motions of hingeless rotor blades. Hohenemser and one of his students entered the fray and undertook an in-depth study based on a rigid blade with spring-restrained flap and lead-lag hinges. Their results appeared to settle the dispute.

This work provided me a starting point, and I soon stumbled upon a small but important error in Hohenemser's analysis that I was able to correct. This confirmed the potential for flap-lag instability of hingeless rotors. Most importantly, I had the good fortune to enlist the help of Dewey Hodges, newly arrived at AARL in June 1970, to help assist with a more accurate elastic blade model [6]. Unexpectedly, many fundamental insights emerged from this work [7] and helped to clarify the complexities of hingeless rotor aeroelastic stability. In fact, during a visit to Lockheed to review Cheyenne program developments, I remember discussing some of my new results with Dick Carlson, Lockheed's Director of Rotary-Wing Design Technology, who was quite excited about the new insights. From these beginnings, the work expanded in various directions as other Ames-Army Lab researchers became involved including Dave Peters who arrived from Washington University a few months after Dewey Hodges (refer to the chapters by Hodges and Peters).



Dewey Hodges

Nonlinear Beam Theory—Hodges-Dowell Equations (1972)

One essential characteristic of the hingeless rotor is that cantilever blades experience significant elastic bending in virtually all flight conditions—unlike traditional articulated rotor blades where hinges accommodate much of the blade motion and relieve most of the elastic bending. The presence of hinges enabled the analyst to avoid the complications of elastic blade bending for many problems, especially the nonlinear coupling between elastic bending and torsion. We knew that this was not appropriate for hingeless rotors like the Cheyenne. At the time, the rotor blades equations of motion developed by Houbolt and Brooks at NASA Langley were widely accepted. However, the Houbolt and Brooks equations were based on linear beam theory and did not include nonlinear bending-torsion coupling, and we needed to remedy that omission to properly model hingeless rotor blades. As an interesting note, George Brooks preceded me at Princeton and John Houbolt had been an outspoken proponent for the controversial Lunar Orbit Rendezvous technique ultimately adopted for the Apollo program.

I first discussed the problem with Dave Peters but he wasn't particularly interested at the time. I turned to Dewey Hodges and he was willing to dig in. I preferred the Newtonian force method but he wanted to use Hamilton's principle so I hit upon the idea of using both methods by

inviting Earl Dowell, one of my thesis advisors at Princeton, to join us at AARL for the summer to help. Earl was already a leading authority in structural dynamics and aeroelasticity and was quite interested in the challenges of rotor dynamics. Working with the two of them was a memorable experience. The result turned out to be one of the seminal developments in rotorcraft structural dynamics, also described by Dewey Hodges in his chapter, and became known simply as the Hodges-Dowell equations [8]. Their equations superseded the classic Houbolt and Brooks work and provided a rigorous basis for aeroelastic analysis of hingeless rotor blades—while at the same time providing a more accurate basis for all types of rotor blades. Ironically, along the way, Dewey and Earl generated so much excitement wrestling with technical issues that Dave Peters could not resist being drawn into the effort, and in the end he contributed an important appendix to their report.



Dave Peters

The pioneering Hodges-Dowell work filled a long-standing major gap in the technology base that had contributed to the troubles with the Cheyenne. For many years thereafter, this work inspired many researchers and graduate student to address the problem and yielded a flood of technical papers and Ph.D. theses on the subject. I should note that Peretz Friedmann, while working on his Ph.D. at MIT under Professor Rene Miller’s AARL contract (for which I was the Technical Monitor), developed similar nonlinear beam models for hingeless rotor aeroelastic stability. Lively debates subsequently arose about some aspects of the work and provided a certain degree of entertainment for the rotorcraft dynamics technical community. Peretz went on to become one of the leading figures in the rotorcraft and fixed-wing dynamics community. Dewey and I applied the Hodges-Dowell equations to extend our initial investigations of flap-lag stability to more realistic torsionally flexible rotor blades including the effects of many design variables [9, 10]. Many other papers and reports are summarized in the 1987 survey paper by Ormiston, Hodges, Peters, and Warmbrodt [11].

Hingeless Rotor Response and Dynamic Inflow

At the same time that we were studying aeroelastic stability, we also began to investigate other topics. I will now digress to discuss our research in hingeless rotor control response and the unsteady rotor wake inflow, commonly called “dynamic inflow.” Research on dynamic inflow represents one of the significant contributions of AARL and would expand significantly beyond the Ames-Army Lab in subsequent years. In 1970, however, we more or less stumbled into this area.

It started with a Lockheed R&D contract to experimentally investigate the control response characteristics of hingeless rotors at high advance ratios in our 7- by 10-Foot Wind Tunnel. Lockheed was interested in technologies for future high-speed rotorcraft beyond the Cheyenne compound helicopter including stopped- and folded-rotor convertible aircraft. The technical monitor for the contract, Dave Sharpe, who had joined AARL from Hiller Aircraft, was also the test engineer. The Lockheed team included Bill Kuczynski, the young principal investigator, and Gerhard Sissingh, the noted German rotorcraft pioneer with whom Dave Sharpe had worked at Hiller. The experiments involved a 7.5-foot, four-blade, hingeless rotor especially designed and constructed by Lockheed for testing at advance ratios exceeding 2.0 that also included the



Dave Sharpe and the 7.5-foot Lockheed hingeless rotor model in the Army 7- by 10-Foot Wind Tunnel.

capability for high-frequency excitation of the washplate controls and the rotor angle of attack. In view of the fundamental nature of the work and because the hingeless rotor technology base was quite limited, I followed Dave's wind tunnel testing with considerable interest.

The first results from these tests did not compare well with Lockheed's analysis, even for the hover condition. The rotor hub moment response to cyclic pitch was significantly less than expected and varied with the rotor thrust, something not observed for traditional articulated rotors. I suspected that this might be caused by the rotor wake inflow and this had been anticipated, in fact, by Professor Pat Curtiss (another of my Princeton advisors) and one

of his students at Princeton. At this time, Dave Peters was developing elastic blade analytical models to study hingeless rotor control response cross coupling, and I suggested that he expand that work by incorporating various models for the quasi-steady rotor wake inflow that we developed from simple momentum and vortex theory. We were able to achieve some significant improvements in comparisons with the 7.5-foot rotor data [12].

Very quickly, Dave was able to extend the quasi-steady inflow model to include the unsteady effects by developing a suitable apparent mass model based on potential flow theory. We adopted the name "dynamic inflow" for this model, and it has been used ever since for a variety of increasingly rigorous finite-state rotor wake models. Even the early momentum theory-based model performed remarkably well when compared with the Lockheed 7.5-foot rotor frequency response data [13]. Kurt Hohenemser and his students also tested a small-scale dynamic model and provided additional validation of the early dynamic inflow models [14]. This work is also summarized in reference [11].

Now is a good time for me to give a short sketch of Dave Peters who, along with Dewey Hodges, was among the truly stellar researchers who emerged from AARL's early days. Among Kurt Hohenemser's many contributions to the Army, probably none was more important than Dave Peters, his most brilliant student at Washington University. Dave's Master's thesis on Floquet Theory solved a long-standing problem in rotor dynamics. Until that time, there was no practical solution for rotor blade stability in forward flight since even the linear equations of motion contained periodic aerodynamic coefficients. This was an important issue for rotors operating at high advance ratio like the Lockheed Cheyenne. Dave's work was an impressive breakthrough in rotorcraft aeromechanics theory. Naturally, Paul Yaggy and I both encouraged Dave to join AARL and our success yielded large dividends for Army rotorcraft research and for me personally. Dave was central to many important research contributions of the Ames-Army Lab.

Unfortunately, Dave's tenure was far too brief but his true place in the technical world would be teaching and research in academia. When Hohenemser retired from Washington University in 1975, the opportunity to take his place would prove irresistible. Dave remained closely associated with the Ames-Army Lab ever since. He expanded his research on dynamic inflow by applying rigorous potential theory to create an entirely new field of rotor wake aerodynamics that

is widely used by rotorcraft technologists for rotor performance, airloads, aeroelastic stability and flight control. Dave described this work in his very popular 2008 AHS Nikolsky Lecture. Considering his entire body of work, I have often thought of Dave as a modern-day embodiment of Prandtl, Glauert, Theodorsen, and Loewy all rolled into one. In 2011 the AIAA awarded him the Reed Aeronautics Award, the highest honor an individual can receive for achievements in the field of aeronautical science and engineering.

Rotor Dynamics Experiments—Adventure and Discovery

One of the most effective approaches for research is to leverage the combination of theoretical and experimental methods. This is particularly true when exploring new technical areas. Experiments often uncover new physical phenomena even before a theoretical model exists. When an analytical model becomes available, assumptions and simplifying approximations need to be checked and the overall accuracy of the analysis needs to be determined. At the outset of our work in hingeless rotor dynamics the technical landscape was relatively unexplored. Following Jim McCroskey's philosophy, we used a reductionist approach and broke a big problem into smaller problems: 1) the isolated rotor blade, 2) the coupled rotor-fuselage problems in hover, and then 3) the forward flight problems. This section will discuss early investigations of isolated rotor blades in hover.

While Dave Sharpe's experiments with the Lockheed 7.5-foot hingeless rotor were underway, my early investigations of flap-lag stability with Dewey suggested another opportunity. Given the controversy surrounding flap-lag stability described earlier, I was quite interested in confirming the results of our own analytical work. Bill Bousman had recently joined AARL in October 1970 from the Aviation Systems Command (AVSCOM) in St. Louis, and we decided to embark on an experimental program to investigate flap-lag instability. We started with a primitive setup using a test stand and rotor blades borrowed from another experiment. We designed a simple two-blade hub with flap and lead-lag flexures to match our hinged, rigid-blade analysis. With few ideas about how to excite blade response or evaluate stability, we cobbled together a high-pressure air nozzle to aim at the spinning blades. Aside from being ineffective, this turned into a fiasco when the nozzle mount broke loose. Eventually, by trial and error and a little luck, we were able to confirm our analytical predictions and demonstrate the existence of flap-lag instability.



Early flap-lag stability model in the Army Hover Test Chamber.

Evaluating flap-lag stability at high thrust was another matter since our analysis did not treat airfoil stall. Our preliminary experiments confirmed the predicted destabilizing effects of flap-lag coupling below stall, but a few data points showed the opposite trend at higher thrust. We assumed that the increased drag at stall would inhibit flap-lag instability by increasing lag damping, and our initial analytical results, using a rudimentary model for airfoil stall, asserted just that. We presented a paper at the Army Science Conference at West Point [15] based on these results and our paper received a second-place award—all the more satisfying since the conference encompassed a wide range of basic research disciplines well beyond engineering.



Commendation award for 1972 Army Science Conference paper. From left: Colonel Norman Robinson, Bill Bousman, Bob Ormiston, Paul Yaggy, and Andy Morse.

Unfortunately, we'd been a little too quick to publish our results. Bill and I continued testing and more complete results at higher thrust levels produced a much stronger instability than before. In view of the increased airfoil drag at high lift, this seemed completely counterintuitive. However, this episode would turn out to yield one of those dramatic "aha" experiences that can be so compelling for a researcher.

I remember driving home that Friday evening after our perplexing discovery, struggling to understand what could cause such a result. Then it came to me—when the blade approached high thrust, the reduced airfoil lift curve slope from stall effectively nullified the blade flap damping. This was equivalent to a large reduction in Lock number. I could easily check this hypothesis by using a crude approximation and setting the Lock number to zero. I couldn't wait to head out to the lab on Saturday morning, change one card at the IBM keypunch machine, and load the card deck into our IBM 1800 computer. Within a few minutes, the line-printer churned out the results. I leaned over anxiously to scan the emerging printout and suddenly I could see the eigenvalues—the numbers were positive—unmistakably revealing a strong instability. Wow, how exciting can digits on a piece of paper be! We ended up testing several rotor hub configurations to represent various hingeless rotor types, and by using a refined blade stall model, the analysis showed excellent agreement with the experiment. We called this a "flap-lag stall instability" [16], and in subsequent years this could be identified in analytical results of numerous investigators.

Our initial experimental investigations were relatively primitive. But as we gained experience over the years, we developed progressively more sophisticated experimental techniques, striving to improve the accuracy of the data. If some element of the experimental apparatus was difficult to model analytically, or compromised the quality of the data, we would modify or redesign the offending hardware and re-run the experiment. For example, one rotor model used blade pitch links with conventional rod-end bearings and the bearing stiction distorted the data for the blade inplane oscillation time histories. We redesigned the pitch links with flexures to eliminate the nonlinear stiction and obtained very clean data. Attention to detail, while sometimes time-consuming, was often the difference between a successful experiment and dubious results. For new investigations, the experiments invariably took two attempts—"one



Refined flap-lag stability model.

time to get it wrong, a second time to get it right.” We wanted our research efforts to provide value.

By the early 1970s we had developed a very effective experimental program to study the fundamental characteristics of hingeless rotor aeroelastic stability. In subsequent years the Ames-Army Lab was known for its exceptional group of experimentalists—and Bill Bousman was one of the best. Our success in dynamics and aeroelasticity owed much to Bill’s outstanding and varied skills. In addition to understanding and contributing to the analytical work, he was a meticulous and methodical researcher, keeping track of the myriad details of complex experiments necessary for success. At times I could get impatient and want to push ahead, but this was not Bill’s style. He devised many novel experimental techniques and methods for data reduction and analysis. As a team leader, Bill raised the level of our experimental team members. Without his careful oversight and attention to detail, our experimental program would never have completed so many successful projects and produced so much data of such high accuracy. His influence ultimately yielded an impressive body of outstanding results.

I would be remiss not to mention the outstanding support of our technicians, including Bob George, Ozzie Swenson, and Jon Lautenschlager. We also had outstanding support from the NASA Ames fabrication shops and instrumentation and calibration laboratories.

Following Jim McCroskey’s reductionist approach, we gradually expanded the scope of our experiments with simple rigid-blade, isolated rotor models to address more complete and complex experiments. We studied blade stall effects, various aeroelastic and structural elastic couplings, and configurations with improved lead-lag damping. The experimental story will continue with these developments but first I will digress to describe a few parallel topics.

Aeromechanics Research Accelerates

As described earlier, after my HLH briefing at AMC in April 1970, the Army-NASA collaboration at Ames was expanded to encompass Army laboratories at NASA’s Langley and Lewis Research Centers. The U.S. Army Air Mobility R&D Laboratory (AMRDL) was established in July 1970 and comprised four Directorates and with the HQ located at Ames. AARL became the Ames Directorate and was joined by the new Langley and Lewis Directorates. AVLABS at Ft. Eustis, Virginia, was included and renamed the Eustis Directorate. Paul Yaggy became the first AMRDL Director and Irv Statler, who had joined AMRDL HQ from CAL, became Paul’s replacement, and thus my new boss, at the Ames Directorate in 1971.

In the early days, there was little structure to AARL’s research program. It encompassed a multitude of eclectic projects from basic to applied research in diverse technical disciplines that included phenomenological research, analytical and experimental research, assessment of novel concepts and inventions, and engineering support for Army customers. Paul provided general direction, but he and Andy Morse typically asked researchers to come up with ideas and details for research opportunities we ought to pursue. Eventually a structure began to emerge with individuals coalescing into informal groups focused on the interests of a few individuals. The early reputation of AARL grew out of the accomplishments of these groups as these successes began to have an impact on the technical community. In our areas of interest, we were beginning to “write the book” in terms of rotorcraft fundamental knowledge and technology.

In the beginning, Jim initiated studies of rotor blade boundary layers and retreating blade stall and was soon joined by Larry Carr and Ken McAlister. Frank Caradonna expanded rotor aerodynamics into transonic analyses. When Fred Schmitz joined the Ames Directorate, he took over early acoustics studies by Dean Borgman and drew in Rande Vause, Don Boxwell, and Georgene Laub to form a strong acoustics team. I focused on rotor dynamics and aeroelasticity working with Dewey Hodges, Dave Peters, Dave Sharpe, and Bill Bousman. Soon these informal groups were recognized as teams within Andy Morse's "in-house" AARG. In essence, Paul's hiring of recent Princeton Ph.D.s created a troika of team leaders for research in fluid mechanics, acoustics, and dynamics. Between teams, there was mutual respect and support, although at times there was considerable competition for our experimental facilities and resources—the Hover Test Chamber, the 7- by 10-Foot Wind Tunnel, the wind tunnel mechanics, and the instrumentation support staff. Andy Morse's real-world helicopter experience and cheerleading helped keep the operation running smoothly and efficiently.

The Ames-Army Lab was in its R&D heyday during this time. The atmosphere was one of action and excitement. We kept the NASA Ames shops humming. Many new results and exciting discoveries poured forth on an almost daily basis—perhaps unmatched at any time in our history, before or since. This was the result of accessible facilities, low-cost small-scale experiments, and a close link between analysis and experiment. We had in-house control, considerable freedom, management support, and little bureaucracy. In addition, there was open cross-fertilization with NASA, industry, academia, and emerging international collaborations. This atmosphere contributed to lots of teamwork, new ideas, and a very high esprit de corps.

All of this work benefited from the NASA Ames environment enabled by the Army-NASA Joint Agreement. The facilities, the model shops, the technical services, and the technical staff at Ames were all essential to making our research progress. Although most of our research was not done in direct collaboration with NASA researchers, contacts with experienced researchers in the Large-Scale Aerodynamics Branch and other Ames organizations were often very helpful.

AHS/NASA Ames Specialists' Meeting on Rotorcraft Dynamics

In 1972, I was a member of the American Helicopter Society (AHS) Dynamics Technical Committee and was looking for ways that the committee could engage the technical community. Kurt Hohenemser was also a member and felt the same way. Bob Wood, the Committee Chairman, was interested in organizing a specialists' meeting so I proposed we hold one at Ames and the committee endorsed my suggestion. In those days, the Army was not in a position to be a co-sponsor but that was not a problem for NASA. The Army and NASA would benefit each other in this manner many times during the Army-NASA collaboration.

I was appointed Technical Chairman and Jim Biggers was named Administrative Chairman. We needed a suitable General Chairman so I approached a few industry technical leaders explaining that, since Jim and I would be doing most of the work "on the ground," their job would be easy. Eventually Ted Carter, Chief of Aeromechanics at Sikorsky, whom I already knew, agreed to serve as our General Chairman. As an aside, Ted was a staunch supporter of the AHS and was shocked to discover that I was not even a member of the Society; for some strange reason I was a member of the AIAA but not the AHS. Somewhat sheepishly, I hastily joined the AHS and regained Ted's respect. In subsequent years I too became a staunch supporter and held many technical positions within the AHS. In those days, the Army policy fully supported conference

travel and participation in technical societies in view of the great benefits for enhancing researchers' professional development and furthering R&D productivity. Unfortunately, this diminished in subsequent years by a shortsighted policy of reducing travel costs.

Jim and I worked well together except when he got the idea to capitalize on the occasion and proposed that we also form a local chapter of the AHS. I felt that this would divert us from the demands of the meeting itself, and I tried to dissuade him from his mission. Not to be deterred, Jim went ahead anyway and the San Francisco Bay Area Chapter of the AHS was born.

The meeting was held at Ames in the Main Auditorium of Building N-201 in February 1974. With considerable support from many Army and NASA folks at Ames, the attendance was excellent, the technical paper quality was very high, and the meeting was a great success. Paul Yaggy served as the keynote speaker at the dinner banquet. Several of the papers were chosen for a special issue of the *Journal of the AHS* later in the year.

A highlight of the meeting was a panel session devoted to a survey of rotor loads prediction methods. Participants were asked to predict airloads and blade loads for a representative, but hypothetical, rotor. I collected and compared the results in a presentation that was then discussed by the panel members [17]. The exercise generated considerable notoriety since industry technical managers were reluctant to have their results from their methodology exposed in a public comparison but, at the same time, they did not want to appear to be uncooperative. Despite the public grousing, the engineers who performed the calculations were generally enthusiastic supporters. The results showed, not surprisingly, fairly wide differences and served as a stimulus for improving prediction methods. It also stoked an emerging controversy about a government-funded initiative to develop a comprehensive analysis tool.

After the meeting, Hans Mark requested that Jim and I report on the conference at one of his staff meetings in the Committee Room of Building N-200. I still remember Hans, at the conclusion of his meeting, turning to Jim and me and announcing to his staff, "Okay, now let's hear something about helicopters!" This was another instance when NASA Ames management recognized the importance of rotorcraft R&D and the relationship afforded by the Joint Agreement.

The 1974 AHS/NASA-Ames Dynamics Specialists' Meeting [18] was an excellent example of the benefits of the Joint Agreement and the growing presence and influence of the Army-NASA relationship on rotorcraft research at Ames and beyond. An important consequence was that it helped put Ames on the map as a focal point for rotorcraft research. Indeed, Army and NASA researchers from Ames authored many of the papers. Subsequently, specialists' meetings were held every 10 years: the second decennial rotorcraft dynamics meeting at Ames in 1984 was co-sponsored with AHS, and the third through fifth decennial aeromechanics specialists' meetings sponsored by the AHS San Francisco Bay Area Chapter were held in San Francisco in 1994, 2004, and 2014.

Life in Paris—Rotorcraft Research With a French Accent

As if my research in the dynamic Ames environment wasn't exciting enough, the prospect of embracing a French connection in Paris significantly upped the ante! In 1971, the U.S. and France agreed to undertake cooperative rotorcraft research between AMRD and the Office National d'Etudes et de Recherches Aéropatiales (ONERA). The U.S.-France Memorandum of

Understanding (MOU) in Rotorcraft Dynamics embodied the spirit of international cooperation created by Paul Yaggy and furthered by Irv Statler.

The MOU included a provision for personnel exchange and Jim McCroskey was the first subject (see Jim's chapter). Jim and his family spent a year in Paris and thoroughly enjoyed the experience while forging important connections with the French to advance his aerodynamics research. I was invited to be the next U.S. participant. Initially I was skeptical of dropping my research activities and relocating nearly 5,500 miles from Ames. I was also less than sanguine about the research benefits because the interests of ONERA were not well aligned with our dynamics research at the Ames Directorate. I soon came to my senses and realized that this was an opportunity I'd regret declining and agreed to spend 6 months in Paris from June to December 1975. I've treasured the experience and the memories ever since.



Visiting Bretagne on the north coast of France in the summer of 1975; Bob and Wendy Ormiston, Jean-Jacques and Sebastian Philippe.



U.S.-France MOU meeting at ONERA. Back row, second from left: Bob Ormiston, U.S. Army. Front row, from left: Bill Twomey; Roland Dat, ONERA; Irv Statler, U.S. Army; Philippe Poisson-Quinton, ONERA; Jean Renaud, Aérospatiale; and Jean-Jacques Philippe, ONERA.

Space does not permit doing justice to the kaleidoscope of technical and cultural activities I became exposed to. In no particular order these included an apartment in the center of Paris, 14 rue de Rivoli, a few blocks away from the Place de la Bastille, that we rented from the wife of the famous French playwright, Eugène Ionesco; the incomparable Paris Metro, though having my pocket picked—twice; visiting the amazingly effective Paris lost and found to retrieve my wallet—twice; frequent short visits throughout Europe every month; a vacation at a villa on the French Riviera; a visit to Jean-Jacques and Annie Philippe's family home in Bretagne; presenting a seminar, in French, at Aérospatiale in Marignane and having Jean-Joel Costes rate my French as "pas mal"; being escorted by a Paris motorcycle policeman during rush-hour traffic to report a burglary; an unforgettable weekend hang gliding in the French Alps with an exuberant group of non-English-speaking pilots; and much more.

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Despite having left my technical activities at the Ames Directorate, I used the opportunity at ONERA to collaborate with my French co-workers Jean-Joel Costes, Tran Cam Thuy, and expatriate American Bill Twomey, in Roland Dat's Structures Department at ONERA. I dabbled in one of their prime interests, whirl flutter of the convertible (the French name for tiltrotor), but more importantly I began to expand my own aeroelasticity work to include vehicle degrees of freedom needed to treat coupled rotor-body stability. During the fall semi-annual MOU meeting that was held at ONERA, I presented some preliminary results and Irv Statler got to see what I was up to. Back in California, he was too busy expanding Ames Directorate programs and NASA relationships to focus much on our little team of dynamicists. As I remember, he was pleased with what he saw. At the same time, Bill Bousman kept the group together and updated me on progress. I still remember one of his lengthy, precisely lettered handwritten letters informing me that our predictions for the Accoflex rotor had been successfully verified by the experiments that we'd planned before I left for France.

After 6 months, my sojourn in France had expanded my horizons and, rather than impeding my research, actually accelerated it far more than I could have expected.

Advancing Aeroelastic Analysis—Hingeless and Bearingless Rotors

Back in California after my sojourn in France, I focused on my coupled rotor-body aeroelastic analyses and began to address the application of our knowledge to advanced rotor concepts as well.

Despite the unfortunate experience with the Lockheed Cheyenne, interest in hingeless rotors continued to grow, especially for soft-inplane configurations. One of the virtues of the Lockheed "stiff-inplane" hingeless rotor was immunity from ground resonance because the stiff cantilever blades' lead-lag frequency was greater than the rotor speed. However, this made the blades heavier. The "soft-inplane" rotor offered less weight, but the lead-lag frequency was less than the rotor speed and introduced the potential for ground and air resonance instabilities involving rotor and fuselage interactions, i.e., rotor-body coupling. Indeed, in the 1960s, the soft-inplane German MBB Bo-105 and British Westland WG-13 Lynx experienced these challenges. Conventional articulated rotors dealt with ground resonance by incorporating auxiliary dampers at the blade lead-lag hinges, but this added complexity and more weight. For hingeless rotors, ground resonance could also occur in flight (air resonance), and knowledge of this phenomenon was limited. All of this added impetus to the need for investigating coupled rotor-body stability.

Our earliest investigations of flap-lag aeroelastic stability had greatly improved our fundamental understanding of hingeless rotor behavior and had already suggested the possible benefits of tailoring rotor blade structural properties to improve aeroelastic stability. We found that a combination of blade pitch-lag kinematic coupling and flap-lag elastic coupling would significantly increase the blade lead-lag damping [19]. Small-scale model rotor experiments had confirmed our analytical predictions—for the isolated rotor blades [20]. Anticipating that this would provide a viable means to prevent ground and air resonance of soft-inplane hingeless rotors without resorting to auxiliary lead-lag dampers, we applied for and received a patent for our concept. Interestingly, our Patent No. 3,999,886 missed being the 4 millionth patent granted by the U.S. Patent Office by only 114 patents!

The new research objectives were to better understand hingeless rotor air resonance and to search for effective aeroelastic couplings to solve the problems of ground and air resonance without

resorting to auxiliary lead-lag dampers. However, the goal would turn out to be more elusive than we had hoped.

The coupled rotor-body analysis provided new understanding of hingeless rotor ground and air resonance [21, 22], but the initial results also revealed that aeroelastic couplings that benefited the stability of isolated rotor blades were generally ineffective for the coupled rotor-body system. We continued our investigations [23] along with universities and industry but did not find a fully satisfactory solution. Perhaps future investigators will succeed in finding aeroelastic tailoring for simple, lightweight “damperless rotors” for future rotorcraft.

An interesting and curious footnote in my experience with hingeless rotor development involved the YUH-61A, Boeing Vertol’s losing entry in the Army’s Utility Tactical Transport Aircraft System (UTTAS) competition. Based on refinements of the Bo-105 rotor, Boeing engineers were able to design a soft-inplane hingeless rotor without a lag damper. At the time, we in the Ames Directorate were strong proponents of hingeless and bearingless rotors and apparently some in the technical community—including some at Boeing Vertol—thought this implied a broader Army endorsement. I remember visiting Dick Gabel, Chief of Dynamics, after they lost the UTTAS competition to the Sikorsky UH-60. Dick mentioned that Ken Grina, Boeing’s Chief Engineer, wanted to talk with me; I said fine and, along with Bill Bousman, accompanied Dick to Ken’s office. Ken was clearly unhappy over the UTTAS loss and seemed to think that I, as a proponent of the hingeless rotor, could have ensured Boeing’s success. In effect, he was saying, “We gave you what you wanted, why did we lose?” I was at a loss for words, surprised to think that he felt I had such influence.

During this time, interest began to emerge for an improved hingeless rotor. For this “bearingless” rotor, the pitch change bearing was replaced with an elastic flexbeam that could twist for rotor blade pitch control and bend to accommodate blade flap and lead-lag bending. This improved maintenance and reliability by simplifying the rotor and reducing parts count. In response to this interest, the AMRDL Eustis Directorate initiated the Boeing Vertol Bo-105 Bearingless Main Rotor (BMR) program. Unfortunately, the analytical tools needed to design bearingless rotors were woefully inadequate. It was not surprising when Dewey Hodges conceived an innovative approach to model the bearingless rotor (outlined in his chapter) and the Flexbeam Air Resonance (FLAIR) analysis was born. This turned out to be very versatile and was used extensively to support the Boeing Vertol Bo-105 BMR [24].

The FLAIR analysis was based on a judicious blend of a low-fidelity rigid-blade model with an innovative elastic flexbeam and pitch-control mechanism. It remained to develop a more general modeling capability for bearingless rotors based on nonlinear finite elements. Dewey teamed up with Stu Hopkins, a spacecraft multi-body dynamics specialist, to accomplish this feat—a first for rotating dynamic systems. The development was difficult and protracted, but they ultimately succeeded in 1985. Don Kunz, who came to us from Georgia Institute of Technology, helped in this work. The General Rotorcraft Aeromechanical Stability Program (GRASP) was a landmark development that significantly advanced the state of the art for rotorcraft dynamic analysis [25].

Soon after the GRASP analysis was completed, in 1986, with a growing family and rising costs in Silicon Valley, Dewey chose to leave the Ames-Army Lab, now the Aeroflightdynamics Directorate (AFDD), and join Georgia Tech. After 16 years of incredible accomplishment in rotorcraft aeroelasticity and fundamental structural dynamics, he went on, like Dave Peters, to

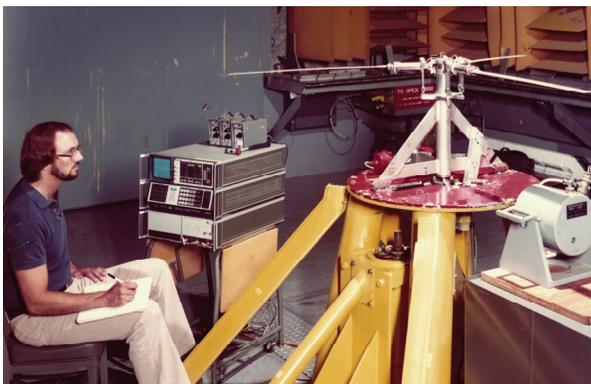
further heights in academia, pioneering new methods in composite beam theory, publishing several books, and becoming one of the leading figures in aerospace research [6]. Without doubt, the opportunity to work with the likes of Dewey Hodges and Dave Peters at the Ames-Army Lab has been one of the most memorable parts of my career.

The investigations of coupled rotor-body aeromechanical stability and the development of the analytical methods for treating bearingless rotors were notable achievements for the Ames-Army Lab. However, in terms of advanced rotor technology, after considerable fundamental research by industry and government, and notwithstanding a degree of moderate success, the bearingless rotor has not (yet) emerged as the configuration of choice for rotor design. Experience has shown that a variety of articulated, hingeless, and bearingless rotor types can all succeed. Current typical bearingless rotors combine a torque-tube for blade pitch control with an elastomeric snubber-damper as originated by the Bell Model 680. While the snubber-damper increases rotor weight and parts count, it sidesteps the challenge of finding aeroelastic couplings to suppress ground and air resonance. Similarly, the torque tube provides effective blade pitch control, but it increases rotor drag. Hence the goal of the “ideal” lightweight, low-drag, damperless, bearingless rotor has proved elusive. Perhaps future researchers may succeed in this endeavor.

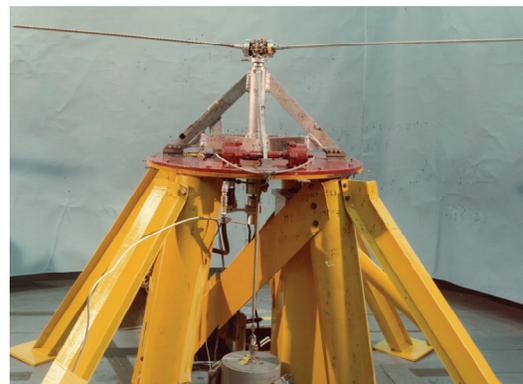
Expanding Experimental Aeroelastic Stability Research

In keeping with our basic philosophy of research, our experimental activities expanded to keep up with our progress in analytical methods. We conducted many experiments with torsionally flexible elastic blades, bearingless rotor blades, rotor-body coupling, and forward flight investigations in the 7- by 10-Foot Wind Tunnel. Our experimental team expanded accordingly as Seth Dawson, Mike McNulty, and Tom Maier joined the Ames-Army Lab. Bill Bousman’s talents were a mainstay for our experimental program, and he became the experimental team leader in rotorcraft dynamics and aeroelasticity.

To validate our aeroelastic stability analyses based on the Hodges-Dowell equations, we needed to develop a hingeless rotor with elastic blades having significant flexibility in bending and torsion. The Torsionally Soft Rotor (TSR) was designed by Seth Dawson and Dave Sharpe and was used to conduct several very successful experiments [26]. Seth also designed a generic BMR



Seth Dawson and the bearingless main rotor (BMR) model.



Torsionally soft rotor (TSR) model with torsionally flexible elastic blades set up for testing in the Hover Test Chamber.

to validate Dewey's FLAIR analysis and subsequently conducted hover tests with several different blade pitch control configurations [27].

Bill Bousman began to explore coupled rotor-body stability with the Rotor Dynamics Model (RDM), and we investigated a number of rotor blades, aeroelastic couplings, and excitation methods for inducing transient response of the rotor blade and body degrees of freedom. These experiments were very successful and generally compared well with Dewey's FLAIR analysis for air and ground resonance stability characteristics including the effects of aeroelastic couplings [28, 29]. As noted earlier, these couplings showed diminished effectiveness in stabilizing air resonance. We were also interested

in the effects of dynamic inflow. As we had anticipated from our earlier analytical studies, the experiments revealed that the unsteady wake significantly influenced the body pitch and roll damping. Unfortunately, we were not able to include the wake model in our coupled rotor-body analyses in time to avoid being scooped, much to my chagrin, by Wayne Johnson who used the experimental data to investigate these effects with his own analysis [30].

At this time we began to extend our aeroelastic stability experiments beyond the hover condition and address forward flight conditions. Mike McNulty conducted some of our first experiments in the 7- by 10-Foot Wind Tunnel using the rigid-blade RDM model to evaluate flap-lag stability in forward flight [31].



RDM for investigating coupled rotor-body aeromechanical stability in hover.



RDM set up in the 7- by 10-Foot Wind Tunnel for testing isolated rotor flap-lag stability in forward flight. From left: Bernadette Peleau, ONERA; Dave Sharpe; Bob Ormiston; Jack Olilla; Joel Gunter; Mike McNulty; Jon Lautenschlager; and Bill Bousman.

In 1989, our experiments culminated with Tom Maier's sophisticated Advanced Dynamics Model (ADM), a Mach-scaled, four-bladed, swashplate-controlled, hingeless rotor with torsionally flexible elastic blades. Straight and swept-tip blades were tested both in hover and in the 7- by 10-Foot Wind Tunnel at high advance ratios. Given the sophistication of the model and scope of testing, the ADM experiments [32] were not completed until 1993, but they provided invaluable new forward flight data for validating analytical methods. The data were also used for cooperative studies under the U.S.-France MOA [33] and were later instrumental in helping to validate the first CFD/computational structural dynamics (CSD) analyses of rotor aeroelastic stability [34].



ADM with swept-tip blades installed in the Hover Test Chamber. Back row, from left: Brian Chan, Dave Pfluger, Jerry Wang, Mike Kodani. Middle row, from left: Tom Maier, Jim Hogan, Terry Fish. Front row, from left: Anita Abrego, Elizabeth Hendley, Ozzie Swenson, Steve Chan, Dave Sharpe, and Bryan Yount.

Needless to say, these experiments constitute a unique, and perhaps unparalleled, body of research and experimental data on hingeless and bearingless rotor aeroelastic stability. Summaries of the results of these experiments are available in a number of papers, and many are summarized in references [11] and [35]. Much of these data were accumulated into a database [36, 37] that was used during the Integrated Technology Rotor/Flight Research Rotor (ITR/FRR) program (discussed below). The ADM was the last in the long series of experimental aeroelastic stability investigations at the Ames-Army Lab. This reflected a shift in research priorities although the need for such experimental research has not diminished.

ITR/FRR (1979–1984)

In 1977, AMRDL reorganized as the Research and Technology Laboratories (RTL) under the U.S. Army Aviation R&D Command (AVRADCOM). The former AMRDL Directorates became Laboratories; the Ames and Eustis Directorates became the Aeromechanics Laboratory (AL) and Applied Technology Laboratory (ATL), respectively. During this period, in the mid-to-late 1970s, Director Dick Carlson, Director of AMRDL and then RTL, initiated a series of 6.3 advanced development programs to advance propulsion, structures, flight control, and rotor technology with flight demonstrators.



ADM set up in the 7- by 10-Foot Wind Tunnel for testing isolated rotor flap-lag-torsion stability in forward flight.

AL and ATL were jointly assigned the new advanced rotor technology initiative as a natural outgrowth of the AL's 6.1 and 6.2 hingeless and bearingless rotor research and ATL's 6.2 Boeing Vertol Bo-105 BMR program. In addition to bearingless rotor technology, the program also encompassed advanced aerodynamics, acoustics, and structures technologies. Accordingly, the project was named the Integrated Technology Rotor (ITR) at the suggestion of Andy Kerr who coordinated the program as the Aeromechanics Lead of the Advanced Systems Research Office (ASRO) at RTL HQ. Bob Powell of ATL and I were named Co-Project Managers. Bob, a former Navy pilot with many years of R&D experience, was a true gentleman, and we became good friends with none of the East Coast/West Coast rivalry that sometimes existed between AL and ATL.



ITR/FRR group with RSRA in background, 1984. From left: Bill Bousman; Len Haslim, NASA; Bob Powell, Army ATL; Jeff Cross, NASA; Ed Seto, NASA; Jim Biggers, NASA; Dave Sharpe; Bill Snyder, NASA; Greg Condon, NASA. Not shown, Paul Mirick, Army ATL; and Bob Ormiston, Army.

During the same time, NASA reorganized its rotorcraft research program and designated Ames as the Lead Center for rotorcraft research (refer to the Introduction), based in part on the success of the strong Army-NASA relationship at Ames. This decision included the transfer of personnel, aircraft, and resources from Langley and had a significant impact on Ames. The change that most affected my activities was the establishment of the new Helicopter Technology Division under Sam White. There were two branches—the Helicopter Flight

Investigations Branch headed by Greg Condon, and the Rotor Systems Technology Branch headed by Jim Biggers. Flight research was aimed at exploiting the Rotor Systems Research Aircraft (RSRA) that has been transferred from Langley. The RSRA was intended to serve as a flying wind tunnel to test new rotors in a full flight environment without the constraints of a wind tunnel and without the necessity of building an entirely new aircraft. It was a controversial idea, and I always felt the negatives outweighed the positives. With its new mission responsibilities, and in keeping with the Army-NASA relationship, the new NASA Division became interested in the ITR and soon joined the Army in a three-way partnership with AL and ATL. Jim Biggers became the NASA Co-Project Manager. Since NASA's interest included flight research as well as advanced rotor technology development, the program was expanded to develop an instrumented version of the ITR called the Flight Research Rotor (FRR) for testing on the RSRA. Thus, the name ITR/FRR. Although a bit unwieldy, the three partners worked together quite well.

The plan consisted of four phases: Phase 1 Predesign included Methodology Assessment, Concept Definition, and FRR Predesign; Phase 2 Preliminary Design included model rotor testing; Phase 3 was Detail Design and Fabrication; and Phase 4 Flight Test included testing the ITR on a dedicated flight test aircraft and FRR flight research on the RSRA.

Because of the aeroelastic challenges of bearingless rotor technology, the Methodology Assessment studies were intended to evaluate the ability of the industry design tools to accurately predict ground and air resonance stability of candidate design concepts. Here, industry designers checked the accuracy of their codes against the data acquired from our Ames Directorate small-scale aeroelastic model testing along with some of the Boeing Vertol BMR model test data. Initially, the industry predictions were not very successful; the assessment exercise enabled several of the companies to significantly improve the accuracy of their analyses and thus the ITR/FRR program helped to advance industry design methodology [36, 37].

Five companies were awarded Concept Definition contracts to study candidate designs for the ITR [38], including Bell Helicopter Textron, Boeing Vertol, Hughes Helicopters, Kaman Aerospace, and Sikorsky Aircraft. The Preliminary Design phase down-selected to three contractors, Bell, Boeing, and Sikorsky, who then conducted preliminary designs and tested small-scale models. NASA awarded two FRR Predesign contracts. Although the Concept Definition and Preliminary Design contracts were completed successfully, the Army funding ran out in 1983 before Phases 3 and 4 could be initiated to design, build, and flight test the ITR and FRR rotors.

Despite the unfortunate and premature ending, the ITR/FRR program was not without its accomplishments. First, the rotorcraft industry gained considerable experience in the design of hingeless and bearingless rotors, and their aeroelastic analysis methods were also significantly improved. Second, although Hughes Helicopters, one of the Concept Definition contractors, was not selected for a Preliminary Design contract, they decided to embark on the company-funded Hughes Advanced Rotor Program (HARP) to develop their ITR concept rotor—undoubtedly encouraged by the ongoing ITR/FRR program. The HARP bearingless rotor subsequently entered production on the MD-900 helicopter. Third, the choice of a bearingless rotor for the Army's Sikorsky-Boeing RAH-66 Comanche scout attack helicopter was likely influenced by the ITR/FRR program. However, the Comanche rotor proved technically challenging, with particularly marginal air resonance damping in high-speed maneuvering flight. Nevertheless, a solution using active flight control system stabilization was successfully flight tested. In the end, the Comanche program was cancelled largely because of changing Army requirements. And finally fourth, cancelling the ITR/FRR program led NASA to pursue the highly successful NASA/Army UH-60A Airloads Flight Test research program that will be mentioned below.

Unfortunately, the Army did not undertake another 6.3 advanced rotor development program after the ITR/FRR program. Several successive planning efforts were conducted by AFDD and the Aviation Applied Technology Directorate (the ATL successor organization), such as the High Maneuverability Advanced Rotor and Control System (HIMARCS), but these were never funded. As a result, it is likely that the Army missed significant opportunities to advance rotor technology. It was not until 2013 that the Army launched a full-scale technology demonstrator flight test program for new advanced rotorcraft concepts—the Joint Multi-Role Tech Demonstrator (JMR) program now underway for two flight demonstrator aircraft.

The Rotorcraft Dynamics Division—Becoming a Manager

In 1978 Irv Statler reorganized the Aeromechanics Laboratory. One of Irv's early initiatives after he became Director in 1971 was to expand the scope of our research beyond the aeromechanics disciplines and leverage existing NASA Ames research to build a new Army group for rotorcraft flight control and handling qualities. His 1978 reorganization created four new divisions: fluid mechanics, rotorcraft dynamics, flight control, and a support division. The new organization reflected the informal research teams that had coalesced around the team leaders described earlier. Andy Morse became Chief of the Fluid Mechanics Division that encompassed Jim McCroskey's team of aerodynamics researchers and Fred Schmitz' aeroacoustics group. Dave Key, who'd been working in the new area of flight control, was named Flight Controls Division Chief, and Frank Lazzeroni was chosen to be Support Division Chief. The reorganization combined engineers of the AARG, in Building N-215, and of the JARG, located throughout

NASA organizations, into the four Divisions and the new Chiefs supervised former members from both groups.

For me, Irv's reorganization was a significant change. It made sense that my rotorcraft dynamics and aeroelasticity team would become the Rotorcraft Dynamics Division (RDD)—and that I would be chosen as the new Division Chief. That was not certain, however, until Irv went through a formal selection process. The suspense was short lived, and I found myself in management with supervisory responsibilities and a group of former JARG employees scattered throughout NASA organizations with whom I had not worked closely, including Bill Eckert, Bob Stroub, Wayne Johnson, John Magee, Marty Maisel, Kip Edenborough, Ray Piziali, Jack Rabbott, and a few others, if memory serves me correctly. I also inherited a large office and a Division secretary. Initially, I was probably as unsure of how to handle these folks as they were of what to think of me. I remember Bill Eckert, a wonderful Army test engineer in the NASA Large-Scale Aerodynamics Branch, coming into my office, anxious about his future in the Division of "dynamics specialists." However, NASA supervisors remained their first-line managers and I think we all adjusted pretty quickly. I continued to work closely with my former team members who were now part of RDD.

I owe a great deal to Irv Statler as a mentor and for his strong support. Irv was experienced in aeromechanics research having built the pioneering rotorcraft research program within the Applied Mechanics Department at CAL. Like Paul Yaggy, Irv was a visionary and focused on launching his new program in flight controls research. He did not engage heavily in our aeromechanics research since he was well versed in those disciplines based on his experience at CAL and because the research programs had matured and were functioning well under Andy Morse—but he nevertheless provided appropriate oversight. He was well organized and valued regular planning and progress assessment. He provided a supportive environment and protected the staff from the never-ending bureaucratic onslaught. He knew how to motivate and reward his folks and when to let well enough alone. He was able to increase our budget every year and gain additional personnel slots. He also understood the philosophy of "hire good people, trust them, and get out of the way." Most importantly, Irv understood the unique value of the Army-NASA relationship and worked with NASA Ames leadership to leverage that relationship for the benefit of both partners. The Army owes a considerable debt to Irv Statler for his contributions to the Ames-Army Lab and to rotorcraft research and technology,

My new position brought demands beyond the technical focus of a team leader, and I endeavored to deal with administrative and resource management responsibilities as necessary. I was now responsible for filling the RDD staff positions, including our secretary. This was particularly important since management entails many tasks that are not so interesting to the technically inclined. These can be a significant burden without the right helper. I must say that finding the right secretary was sometimes challenging and I'll skip my initial, fortunately short-lived, choices, but I must mention two stellar individuals who left me with many fond memories. Lori Cross (Blanken), starting in 1980, and Pat Horn, in 1984, were as different as could be. Lori, just out of school, was an effervescent bundle of energy who quickly caught the eye of engineers and managers alike. All too soon, she moved up the career ladder to become Irv's secretary and then an administrative assistant. Pat Horn, who had decided to return to work after raising a family, was quietly efficient and possessed many dimensions and hidden talents. Both Lori and Pat were favorites of the RDD staff, and I am pleased that they both contributed chapters to this memoir.

I also had more opportunity to influence the Aeromechanics Lab program and manage resources though that was often a mixed blessing when funding and hiring authority diminished over the years.

As Division Chief, I also I remained actively involved in the technical activities of the Division and continued to support the R&D philosophy we evolved in the early years that I believe was important to our success. Along with keeping a healthy analytical/experimental balance, I also felt it was important to emphasize a broad approach to research. That is, in addition to conducting research that fully addressed the basic-to-applied *research spectrum* defined by the Department of Defense (DoD) R&D categories of 6.1 basic research, 6.2 exploratory development, and 6.3 advanced development, it seemed important to encompass diverse *research dimensions* such as discovering new knowledge, developing prediction methods (theory, analysis, and codes), accumulating engineering databases, creating new designs, and inventing new devices. All of these research dimensions ultimately needed to contribute to future vehicles (rotorcraft) with greater mission capabilities for the user. It seemed to me that narrowly focused, one-dimensional research, say, theory without experiment, or prediction methods without invention, would inhibit the success to be gained from the R&D enterprise.



Members of the RDD, 1985. Back row, from left: Dewey Hodges, Stu Hopkins, Mike Rutkowski, Don Kunz, Mike McNulty, Pat Horn, Dave Sharpe, and Howard Hinnant. Front row, from left: Bob Ormiston, Matt Dawson, Bill Bousman, Wendell Stephens, and Jacques Esculier (ONERA).

I should probably say a few words about a couple of illustrious members of the Ames rotorcraft community. I have already described my small role in facilitating Wayne Johnson's arrival at the Ames-Army Lab. Since he was assigned to NASA in Mark Kelly's 40- by 80-Foot Wind Tunnel Branch, I was not particularly aware of his early activities, although that changed quickly because he was a prolific researcher and published frequently. In one of our earliest encounters, long before I became a manager, I was assigned to the editorial committee for one of his first technical reports—on proprotor aeroelastic stability. This was back in the days when the rigorous NASA editorial review process was considered an intimidating obstacle course. Bruce Tinling, Assistant Chief of the Flight and Systems Research Branch, was the Committee Chairman. I quickly observed that Wayne had not included flap-lag elastic coupling terms in his equations and when I mentioned this during a review committee meeting, Wayne responded that the terms were not that important. Of course, I had recently discovered otherwise in my work with Dewey Hodges and I was only too happy to point that out. I don't remember how the issue was resolved, but when I saw Wayne's next report on the subject I noted that he had included the terms in question. In subsequent years I did not find many other instances like that.

After Irv's reorganization, Wayne was assigned to RDD but remained within the NASA Large-Scale Aerodynamics Branch, eventually rising to Assistant Branch Chief. Typically of serious demeanor, he did exhibit a smile on the occasion of a 1979 promotion while he was a member of my Division, and the incontrovertible evidence is shown in the accompanying photo. Of course, early on, Wayne became a leading figure in rotorcraft aeromechanics R&D, an influential supporter of the tiltrotor, developer of the CAMRAD comprehensive analysis codes (discussed below), and author of seminal books on helicopter theory and rotorcraft aeromechanics. He has been highly recognized by the American Helicopter Society and NASA Ames Research Center. I would submit that he is an excellent example of the success of the Army-NASA relationship at Ames. Wayne transferred to NASA in 1981.



Smiling Wayne Johnson receiving a certificate of promotion, 1979.

Another story involves “one that got away.” In 1978, I was a good friend of Peretz Friedmann, then a professor at the University of California, Los Angeles, and he alerted me that Bill Warmbrodt, one of his star students, was about to graduate with his Ph.D. Perhaps Peretz told Bill about me as well. I met with Bill when he visited Ames and it was clear that he was destined for success. I found him, even then, to be unusually smooth, confident, and discerning for a young graduate. Naturally I tried to convince him to join the RDD, but I was unable to reel him in. Instead, he chose to join NASA and went on to a stellar career in rotorcraft R&D. He is known today as a strong leader for his people, NASA, the rotorcraft community, and the Army-NASA relationship.

Before concluding my reflections on management, it is interesting to note that no fewer than five members of the Division were ultimately awarded the AHS Alexander Nikolsky Memorial Lectureship: Dave Peters (2008), Wayne Johnson (2010), Bill Bousman (2011), Dewey Hodges (2014), and, bringing up the rear, myself in 2015.

2GCHAS Saga—Army Hard

My story would not be complete without including the development of the Second Generation Comprehensive Helicopter Analysis System (2GCHAS), pronounced “two-gee-Charlie,” as it came to be known. It was undoubtedly the most difficult, at times the most frustrating, but ultimately one of the most rewarding technical activities of my Army career. A broader account is beyond the scope of this memoir, but I will try to convey the gist of the story, and to do it justice I will need to carry it beyond 1985. Though the Army-NASA relationship was not central to 2GCHAS, the two intertwined the culture, policies, and researchers at Ames in various ways. It also serves as a cautionary tale of the risks and pitfalls of government-sponsored R&D. For other perspectives, see the chapters by Wayne Johnson, Andy Kerr, and Wendell Stephens, and for a good history of rotorcraft comprehensive analysis see Johnson, reference [39].

In the field of rotorcraft aeromechanics, “comprehensive analysis” means an analysis that integrates the key technical disciplines together within a single computational system—by definition, an interdisciplinary analysis. Treating the interactions of rotorcraft aerodynamics,

dynamics, and flight control is essential to accurately predict the performance, structural loads, aeroelastic stability, etc., to enable reliable and cost-effective rotorcraft design. The lack of such capability undoubtedly contributed to the Cheyenne difficulties. I first heard Paul Carpenter of Ft. Eustis AVLABS suggest, during a presentation at Ames, that the government should sponsor the development of what he called a “global analysis.” The idea was controversial; some felt that the fundamental mathematical basis for such a system was not sufficiently well developed. Others felt we needed to get started anyway. Along with Kurt Hohenemser, and a few others, I leaned toward the former position. No less an authority than Dick MacNeal, President of MacNeal-Schwendler Corporation, one of the principal developers of the finite-element analysis program NASTRAN, will be remembered for his comment at the 1974 AHS/NASA-Ames Specialists’ Meeting. In response to a question put to members of the Rotor Loads Panel Session: “Do you think the government should undertake the development of a comprehensive analysis?” Dick replied dramatically, “I have a one-word answer for that question . . . No!”

Although I remained ambivalent, many in the technical community embraced the concept of a well-funded, government-sponsored enterprise to overcome the limitations of the existing “first generation codes.” Naturally there were many supporters in industry. In 1976, Dick Carlson, Director, AMRDL, no doubt remembering the AH-56A Cheyenne and his experience as Director of Rotary-Wing Technology at Lockheed, decided that the Army should proceed and asked for volunteers. Our sister lab, the Eustis Directorate, stepped up to the task. Jeff MacDonald, survivor of the Cheyenne 40- by 80-Foot Wind Tunnel accident, was the Project Manager; principal team members included Ed Austin, Paul Mirick, and Art Ragosta. A Government Industry Working Group (GIWG) was set up to advise and help guide planning. Several of us at Ames served as members. The traditional Ft. Eustis R&D approach was to contract out the work rather than undertake any of it in-house. The plan included a predesign phase with contracts to three of the helicopter industry primes, each partnered with a leading software company. In 1978, after completion of the predesign studies, planning for the main system development contracts was initiated. Not surprisingly, given the daunting challenge, progress was slow. A 2GCHAS Unified Equations Workshop held at Ames in February 1979 to clarify the technical issues seemed unable to produce a community consensus for a viable mathematical approach. Sentiment grew for an Army team with more hands-on experience in development of analysis methods and more closely allied with Army researchers working on relevant technical issues. Accordingly, the project was transferred to the RTL HQ (formerly AMRDL) at Ames in 1979.

A Project Office was set up and Andy Kerr, Aeromechanics Lead in the RTL Advanced Systems Research Office (ASRO), took over as Project Manager. Andy’s project team included Art Ragosta from the original Ft. Eustis team, John Davis from the Ames Directorate with real-world experience at Bell Helicopter, and Wendell Stephens from NASA with experience in structural dynamics. The original contracting approach of distributing the 2GCHAS development among industry contractors was retained. The 2GCHAS plan envisioned software comprising a Technology Complex (TC) of software modules that would be developed by the rotorcraft industry and an Executive Complex (EC), the “software executive” that would knit together the TC modules. In those days, the project focused heavily on software development methodology, database management, systems analysis, detailed specifications, and system design. There was less emphasis on the core technical issues of rotorcraft analysis. While planning for the TC continued, a Request for Proposal was released for the EC, and Computer Sciences Corporation (CSC), a major software company, won the contract.

In September 1982 Andy Kerr became Chief of ASRO, and 2CGHAS was transferred from RTL HQ to my division in the Aeromechanics Lab. Now I was the Project Manager. Irv Statler and I had already been concerned about progress, and we had wondered whether it would help if we brought 2GCHAS into the Aeromechanics Lab.

Given my long-standing ambivalence about the overall feasibility of a comprehensive analysis, I certainly had some reservations about becoming directly involved. One day Ray Piziali, a good friend and a wise sage of rotorcraft, widely respected for pioneering CAL work on rotor wake vortex codes, came into my office and said, “Bob, why are you getting tangled up in this 2GCHAS business? This could easily have a negative impact on your reputation.” I remember replying something to the effect that the Army technical leadership had committed to do it, our group was probably the best qualified to help pull it off (if it could be done at all), and therefore it was the right thing for us to do. Then I confessed that I shared his concern and that there was a good chance he would turn out to be right.



Wendell Stephens, 1987.

When I became Project Manager, I inherited Andy’s team of Ragosta, Davis, and Stephens plus several supporting contractors. Concerned about the EC contractor’s commitment to the project, Irv and I visited CSC HQ in Virginia and this led to a shake-up of CSC’s 2GCHAS team management. I still had concerns over the technical challenges and the technical approach that had been cast in concrete years earlier—there was little chance to turn the whole thing upside down and start over. For several months I tried to make the best of a difficult situation. But with differing philosophies and frustrations on both sides, the government team inevitably unraveled, in somewhat melodramatic fashion. In fact, it was a mutiny of sorts. I explained the problem to Irv Statler and he responded by telling me to sit back and he would look into all sides of the situation. I nervously waited out the week as he carefully interviewed all the players. In the end, I was relieved that he didn’t fire me, and instead, instructed me to proceed. Art Ragosta and John Davis soon left the team and found success in other positions within AL. But for me, the difficulties were far from over. For one thing, I was concerned that industry engineers, focused mainly on helicopter development, were not sufficiently conversant with the technical challenges of rotorcraft theory and analysis to meet the rigorous goals of 2GCHAS.

One of the long-standing problems was a lack of sufficient technical expertise within the Project



Mike Rutkowski, 1991.

Office. Trying not to compromise the ongoing research within the Division, I moved several people into 2GCHAS, principally Mike Rutkowski, Stu Hopkins, and several new hires including Carina Tan, Gene Ruzicka, and Joon Lim, and attempted to carry on. In 1988 I asked Wendell Stephens to take over as Project Manager. Wendell is a wonderful guy, cheerful, irrepressible, and was liked by everyone. An eternal optimist, no problem was too difficult—he never met a challenge he could not overcome. In many ways he was perfect for the job. Wendell worked mightily and not without some success. However, during that time, the project continued to progress slowly, 2GCHAS was losing its luster, and enthusiasm in the technical community declined. Our bosses, including Charlie

Crawford, Director of the Aviation RD&E Center (AVRDEC) at AVSCOM in St. Louis, were not pleased. In his chapter, Wendell recounts a notable example, where even his own good nature was tested by Charlie's ill-advised public comments.

The project continued, but the challenges and difficulties did not let up. CSC soldiered on with the EC contract. Six TC contracts were awarded to the rotorcraft industry and R&D companies. These included Kaman Aerospace Corporation, McDonnell Douglas Helicopter Company, Boeing Helicopter Company, Sikorsky Aircraft, Advanced Rotorcraft Technology, Inc. (ART), and Sterling Federal Systems, Inc. When the EC and TC modules were ready to be integrated, we awarded System Integration contracts to CSC and ART. At one point, Johnson Aeronautics was enlisted to help and Wayne became a member of the team. Then, after over 10 years of striving for progress, Wendell Stephens wanted a break to broaden his résumé and departed for Washington in January 1991 on a leave of absence to work for Iowa Senator Chuck Grassley (R). Being a liberal progressive, that was a good example of the lengths to which some folks would go to escape the 2GCHAS pressure cooker.

After Wendell left, Mike Rutkowski took over as Project Manager in 1991. I can't say enough about Mike. Beyond being a valued and loyal friend, he was a diligent worker and could lead the team and manage the numerous contractors. In 1994, 2GCHAS achieved basic operational capability and the First Level Release was distributed to the user community although the system was still flawed with functionality and run-time efficiency issues. Over the years, many in industry had become skeptical and were slow to come on board. Nevertheless, in the mid-1990s, when the Army cut off the ongoing 6.2 AH-76J funding line for 2GCHAS, believers in the rotorcraft industry rallied to the cause. High-level technical managers sent letters appealing the decision to George Singley III, the Deputy Assistant Secretary of the Army for Research & Technology, all to no avail. In fact, I was told by my upper management, Tom House, the



The 2GCHAS Development Team, March, 1990. From left, front row: Hossein Saberi, ART; Peter Johnson, CSC; Chris Dueker, CSC; Lisa Trinh, CSC; Dung Chau, CSC; Carina Tan, Army; Hamid Rafati, ART; Larry Babb, CSC; Skip McKenna, Kaman. Back row: Bob Ormiston, Army; Rupert Seals, Boeing; Mary Sredanovic, CSC; Wendell Stephens, Army; Carla Vernacchia, CSC; Karan Sangha, McDonnell Douglas Helicopter Co.; Gene Ruzicka, Army; Jerry White, Sterling Federal Systems; Dharmendra Kumar, Sterling Federal Systems; George Rogers, Sterling Federal Systems; Mike Rutkowski, Army; Bruce Gustavson, Kaman; Sebby Cassarino, Sikorsky; and Omar Amrani, ART.



Hossein Saberi, 2006.

Executive Director of the AVRDEC, now at the U.S. Army Aviation and Troop Command (ATCOM) in St. Louis, to call off “the campaign.”

Since much of the contracted effort had already been completed, the funding cut did not end development activity. The Project Office staff continued to deal with the technical issues, and we also initiated several Small Business Innovation Research (SBIR) contracts. In December 1997, ART was awarded a Phase II SBIR contract to address underlying math basis and run-time efficiency issues of 2GCHAS. Technical innovations, largely formulated by Dr. Hossein Saberi of ART, ultimately led to resolution of the long-standing difficulties that had plagued 2GCHAS and inhibited acceptance by the user community.

Another significant development occurred at Ames in 1997, when the Army and NASA reorganized and integrated their separate rotorcraft organizations into a single combined Army-NASA Rotorcraft Division to streamline management and reduce costs. The existing Army divisions and NASA branches were replaced with two new branches with integrated staffs of Army and NASA researchers managed by NASA and Army Branch Chiefs. RDD was abolished and I became the Chief Scientist of the Aeromechanics Branch with Bill Warmbrodt and Chee Tung as Chief and Deputy, respectively. Mike Rutkowski became Deputy Chief of the Army-NASA Rotorcraft Division.

Under the new management structure, the status of 2GCHAS became somewhat tenuous. Except for the SBIR projects, contract funding had ended and Project Office members no longer reported to me. Nevertheless I remained closely involved with the project. As ART’s run-time efficiency SBIR continued, we needed to decide whether to embrace ART’s technical approach for resolving the math basis and run-time efficiency issues. Although ART’s approach appeared to hold considerable promise, this decision became a source of controversy within the Project Office. I felt that the choice was essentially self-evident, but when I polled the team for their concurrence, Stu, Joon, and Gene felt that this would unwisely compromise the integrity of the government software. I consider this the second 2GCHAS “mutiny,” and it effectively ended the team from my point of view. Since ART remained under the SBIR contract, they continued working to complete the planned technical refinements. In April 1998, Brent Wellman was appointed Project Manager. In December 1998, the 2GCHAS Project Office was officially disbanded by the Army/NASA Rotorcraft Division, and Joon was named Principal Investigator to wrap up a final 2GCHAS release as Version 3.0. By 2000, 2GCHAS development was essentially complete and, for a period of time, Version 3.0 continued with a small user base and limited government support. Clearly, the management no longer viewed 2GCHAS as a priority.

However, this was not the end of 2GCHAS. When the ART SBIR was finally completed in May 2000, the technical refinements and enhancements proved to be very successful. The fundamental limitations that had plagued 2GCHAS since its inception had been resolved, and 2GCHAS was now able to function as originally intended. After reviewing this remarkable accomplishment with Hossein Saberi, I was convinced that 2GCHAS could now fulfill the expectations of its original Army proponents. Considerable work remained to clean up details,

perform testing and validation, and upgrade the documentation. Restoring the confidence of the long-lost user community would take some time.

Eventually I took on the role of government point-of-contact for the system. We changed the name from 2GCHAS to Rotorcraft Comprehensive Analysis System (RCAS) and in August 2000, the Army entered into a Cooperative Research and Development Agreement with ART to protect the rights of both organizations and ensure that the original Army objective was preserved—to make a government-sponsored code freely available to government-approved users within the U.S. rotorcraft industry and R&D community. An extended period of testing and validation was initiated; the diligent efforts of Hossein Saberi and Mahendra Bhagwat (now with the Army Lab) at ART, Stu Hopkins at AFDD [40-42], and key industry users, notably Brahmananda Panda at Boeing Helicopters, led to the success and wide use that RCAS enjoys today [43, 44].

From my point of view, the 2GCHAS-RCAS project has been one of the most important rotorcraft technology contributions from the Ames-Army Lab. I believe it has fulfilled the original vision of Dick Carlson and the technical community. It is a technical achievement that could not have been fully appreciated a quarter-century ago, and it has brought together so many organizations and so many people that it has become a part of the technical consciousness of the rotorcraft community. As long as it retains the endorsement of the government, there is no reason that it should not remain vital indefinitely. Higher fidelity methods will continue to emerge, but the flexibility and versatility of RCAS provides a unique and essential capability for researchers and designers alike. The modular software architecture will admit continual integration of newly developed rotorcraft modeling. Although some R&D managers can only conceive of projects with defined beginning and end points, an analysis system such as RCAS is a “living” project and, like R&D itself, will only be complete when the Army no longer needs to efficiently develop rotorcraft with improved mission effectiveness.

2GCHAS—RCAS Reflections

The 2GCHAS Project was certainly not a stellar example of how to conduct an R&D program. Given the unanticipated development time and cost, it was not a model of efficiency. In fact, it was a “helluva way to run a railroad.” At one time or another, 2GCHAS violated many common principles of good management. Ideally, the best way is to focus on priorities, properly assess the technical risks, constrain the effort to realistic requirements, lay out a sensible plan of approach, and above all, enlist the most capable experts to do the job. Hiring a large team and spending lots of money is not a good way to meet a difficult technical challenge.

Looking back, the initial objectives were too lofty, the technology base was immature, the risks were underestimated, and the technical expertise in the industry was not up to the task. In 1975, nonlinear beam finite elements, multi-body dynamics, coupled rotor-fuselage structural dynamics, and computationally efficient solution methods were not yet available.

The few rotorcraft people most qualified to do the job were government employees, engaged in related research that would eventually support comprehensive analysis. Early in 1977, as 2GCHAS was just getting underway at Ft. Eustis, I approached Dewey Hodges and Wayne

Johnson to inquire if they would be interested in teaming up to undertake an alternative to 2GCHAS, since they were perhaps the most knowledgeable and capable individuals in rotorcraft dynamics and aerodynamics analysis and would have as good a chance of success as anyone. Although Dewey might have been willing, I knew that Wayne would be a long shot, and my idea went nowhere.

To this point, I have not mentioned the comprehensive codes developed by Wayne Johnson, more-or-less in parallel with 2GCHAS. The first of these, CAMRAD, was completed in 1980 based on Wayne's research on helicopter and tiltrotor analysis methods as an Army employee during the 1970s; CAMRAD JA was a Johnson Aeronautics commercial version developed after Wayne left the government in 1986. A completely new code called CAMRAD II was released in 1993. The success of these codes was confirmed by their rapid acceptance and wide use by the U.S. and international rotorcraft industry. Moreover, these codes showed that, with a proper technical approach by a highly qualified individual, daunting technical challenges can be overcome with modest investment in reasonable time. It might be argued that CAMRAD II was, to some extent, a beneficiary of the 2GCHAS struggles that helped to focus the principal requirements and delineate the technical pitfalls and challenges.

With some trepidation, I will offer one last 2GCHAS story. Bill Warmbrodt, a true friend, is a unique and multi-faceted figure whose energy, forcefulness, and sheer will have enabled him to accomplish great things for the Ames rotorcraft community; this story provides a glimpse of Bill in action. During the early-to-mid-1990s, when 2GCHAS was just getting on its feet, "Uncle Bill" approached me, in his inimitable fashion, with a proposal that offered the Army a way out of "its dilemma." He was carrying a detailed two-page plan for the Army to join NASA in support of CAMRAD and gradually phase out 2GCHAS. I don't remember the specific details and Bill later burned his proposal, but it was pitched as a generous offer for the Army to gracefully extricate itself from a difficult situation, conserve resources, save face, and declare success! The Army and NASA would then share a common vision and unite behind a single code. I had to admire Bill's chutzpah and brazenness to imagine that the Army would abandon, after years of struggle, the contributions of all those who had toiled so tirelessly. When I failed to take the bait, Bill, not one to waste time in pursuit of a lost cause, never mentioned the idea again.

Despite the trials and tribulations of 2GCHAS, it is safe to say that the overall successes of both rotorcraft comprehensive analyses, CAMRAD and RCAS, strongly reflect the impact of the Army-NASA relationship at Ames. All parties in the 2GCHAS-RCAS effort had a hand in its ultimate success, including the fundamental research in rotorcraft dynamics at the Ames-Army Lab, the parallel development of CAMRAD, Dick Carlson's original vision, the early 2GCHAS team efforts at Ft. Eustis, and the contributions of all the members of the several Project Office incarnations at Ames. Many contractors in the rotorcraft industry contributed software, and the CSC EC software remains at the heart of RCAS. Advanced Rotorcraft Technology, Inc., and Hossein Saberi in particular, deserve great credit for long-standing support and technical contributions to both 2GCHAS and RCAS. In later years, the Army-NASA relationship, as well as Army users of CAMRAD, have enabled CAMRAD to be leveraged to aid RCAS testing and validation.

The Rotorcraft Computational Fluid Dynamics (CFD) Revolution

Having brought the saga of 2GCHAS beyond 1985 to the present day, I am compelled to briefly outline another accomplishment of the Army-NASA collaboration and one that ultimately extended to encompass the entire rotorcraft technical community. During the later years of development, RCAS became a part of the breakthroughs that enabled rotorcraft to fully benefit from the CFD revolution. The key breakthrough was achieving the ability to couple rotorcraft computational structural dynamics (CSD) analysis to CFD aerodynamics analysis to enable the central aeroelastic challenges of rotorcraft to be addressed (see chapters by Caradonna, McCroskey, Tung, and Johnson).

Following the cancellation of the ITR/FRR program including the FRR intended for testing on the RSRA, the NASA Rotor Systems Flight Investigations Branch ultimately undertook a scaled-back, but nevertheless very ambitious, flight test program to measure detailed rotor blade loads and airloads of an Army UH-60A helicopter. The blades were extensively instrumented with strain gauges and pressure transducers, together with a sophisticated data acquisition system, and successfully tested in 1993 over a wide range of steady loading and maneuvering flight conditions. The NASA/Army UH-60A Airloads Flight Test proved to be one of the most productive rotorcraft research programs ever conducted and provided an enormous amount of new information about rotor blade aerodynamics and airloads [45]. The Army-NASA relationship at Ames contributed significantly to the success of this program, particularly the technical expertise and project leadership of AFDD's Bill Bousman that had much to do with advancing the fundamental state of the art of rotorcraft aeromechanics [46, 47]. The data from this program proved to be pivotal in helping to achieve the long-sought breakthrough in successfully coupling CFD and comprehensive analysis codes.

In 1999–2000, I proposed a cooperative project under the National Rotorcraft Technology Center (NRTC)/Rotorcraft Industry Technology Association (RITA) to bring together aeromechanics experts from industry, academia, and government laboratories to jointly address unresolved technical issues that prevented accurate prediction of rotor airloads and blade structural loads. This prompted Yung Yu of AFDD, then assigned to NRTC, to initiate an informal meeting in 2001 to explore such a cooperative effort, and this turned into the UH-60A Airloads Workshop that endured for 17 years [48]. The Workshop collaborators focused their attention on 1993 NASA/Army UH-60A Airloads Flight Test data, and this quickly led to the seminal CFD/CSD loose-coupling breakthrough by Potsdam et al. [49] in 2003. This breakthrough marked the achievement of the long-sought goal to fully couple a rotorcraft CFD aerodynamic analysis with a structural dynamics model from a rotorcraft comprehensive analysis. The NASA OVERFLOW CFD code was successfully coupled to both CAMRAD II and RCAS. It was now possible to include high fidelity CFD aerodynamic modeling for interdisciplinary (aeroelastic) analyses and enable the power of CFD methods to bear on the central problems of rotorcraft aeromechanics. A full solution of this problem had eluded the technical community for nearly 20 years, since the early CFD/CSD coupling was developed in 1985 as discussed by Chee Tung and Wayne Johnson in their chapters. The CFD/CSD loose coupling breakthrough was followed in 2007 by applying CFD/CSD coupling to the NASA/Army UH-60A Airloads Flight Test data to solve another long-standing aeromechanics stumbling block, the challenging maneuver loads problem [50]. A new era in rotorcraft aeromechanics analysis and design had opened. The last meeting of the UH-60A Airloads Workshop was held at Ames in February 2018 and marked the conclusion of 17 years

of remarkable advancement in rotorcraft aeromechanics technology. It is safe to say that without the Army-NASA collaboration this community-wide program would not have occurred.

As a direct result of the CFD/CSD breakthroughs of the Airloads Workshop, the DoD High Performance Computing Modernization Program (HPCMP) funded a 6-year HPC Institute for Advanced Rotorcraft Modeling and Simulation (HI-ARMS) program at AFDD to undertake development of an entirely new CFD architecture for rotorcraft computational analysis. This work was expanded and transitioned to the DoD HPCMP Computational Research and Engineering Acquisition Tools and Environments–Air Vehicles (CREATE-AV) program and became the AFDD Helios Code. This is now a leading focal point for the rotorcraft CFD revolution [51].

Concluding Thoughts

My story began when I applied for work at NASA at Ames Research Center. By a quirk of fate, I joined the Army instead and was swept up in a grand experiment of two organizations leveraging their common interests in low-speed aviation for the good of the country. The Joint Agreement benefited the Army, NASA, and me. The Army adopted the NACA/NASA culture and created a premier research organization. I experienced opportunities and developments I could not have imagined, and I was privileged to witness the early days of AARL and its amazing evolution.

I have tried to convey my experience of the Army's early days at Ames, highlighting the people, the environment, and the management approach that contributed to its success. Clearly, I was in the right place at the right time. Many leading lights in rotorcraft research got their start during the early days at AARL, and it was my great good fortune to be able to work closely with several of them on fundamentally new scientific issues.

I believe that the Army-NASA relationship forged at Ames in 1965 has produced more advancement in fundamental rotorcraft science and technology than had occurred in all prior years. The new frontier of rotorcraft technology has changed the world of aerospace. I believe that this is a direct result of the environment, the NACA culture, and the visionary leadership of the pioneers at Ames Aeronautical Laboratory.

The research approach evolved at the Army-Ames Lab contains elements that can serve other R&D organizations, and I have tried to describe these throughout my narrative. The approach boils down to a few time-honored, basic ingredients. It starts with exceptional people with a supportive environment. Enlightened management with vision and trust in the skills and instincts of the researchers is essential. Mission requirements must be kept in mind, and this requires a delicate balance between near-term applied research and unconstrained longer-term basic research. For a joint relationship, both partners need to be fully invested at all management levels. And finally, success requires the good fortune to bring all the ingredients together because the formula for success is fragile, and one sour note can spoil the magic.

In January 2014, I retired from AFDD, but I continued my association with the Army under the Volunteer Emeritus program. For 46 years at the Ames-Army Lab, my work never seemed like a job and the research opportunities always seemed endless. I hope to continue to pursue my interests in rotorcraft technology and support the R&D program from within. It is a privilege to work with world-class researchers, and I hope to energize, inspire, and lead by example. As for

management, I plan to exhort, harangue, and harass them to do the right things and hew to the principles laid down by Paul Yaggy and reinforced by Irv Statler.

Postscript for a Future Opportunity

After a career of research aimed at advancing rotorcraft technology, I have become increasingly convinced of an opportunity with important merit for civil and military rotorcraft. In keeping with the spirit of my story in this chapter, I would like to call attention to this opportunity.

At the outset of my chapter, I mentioned that Andy Morse, AARL's great mentor, held firm convictions about compound helicopters. Andy insisted that the weight and complexity of auxiliary wings and propulsion would diminish the overall mission effectiveness compared to a pure helicopter—a losing proposition. Caught up in the challenges of dynamics research at the time, I didn't really question Andy's views. Today, I have come to believe otherwise and suggest instead that, with today's advances in technology, the compound now represents a promising opportunity for future rotorcraft.

When Dick Spivey became AFDD Director in 2009, after an illustrious career at Bell Helicopter Textron [52], he posed the question, "What is AFDD doing that it should not be doing and what is it not doing that it should be doing?" I suggested that we exploit the potential of the compound helicopter. Shortly thereafter, Dr. James Snider, the new AMRDEC Director of Aviation Development, was scheduled to visit AFDD to familiarize himself with our R&D program, and I suggested to Dick that we propose a new Army R&D initiative for the compound. Although Dick had always been a dedicated proponent of the tiltrotor, he also recognized the potential of the compound helicopter. Mike Scully, an unabashed tiltrotor supporter, got wind of my suggestion and asked to speak on behalf of the tiltrotor. Dick agreed, but cautioned that, "I don't want the two of you to get into a public debate!" The opportunity could not have been timelier because Dr. Snider was already thinking about a technology demonstrator program—but for a *conventional* helicopter. He readily accepted our arguments that the Army should leap beyond the helicopter, and from that point forward the program planning included options for the helicopter, the compound, and tiltrotor vehicles. Eventually this initiative became the JMR with a nominal speed target of 230 kts that opened the door for further development of compounds and tiltrotors. The JMR is currently well underway to develop and flight test the Sikorsky-Boeing SB>1 Defiant lift-offset coaxial compound helicopter and the Bell V-280 Valor tiltrotor.

For many years, along with my aeromechanics colleagues, I regarded the aerodynamic complexities of the helicopter rotor—retreating blade stall and advancing blade compressibility—as objects of noble research that, if sufficiently well researched, would eventually be overcome. Or that boundary layer control, morphing airfoils, and active controls, would ultimately elevate the meager forward-flight performance of the conventional helicopter. After years of exhaustive research, this began to seem less and less likely. Stall and compressibility are fundamental limits that are not going away any time soon. It seems better to recognize fundamental reality and turn to an auxiliary wing and propeller to provide the lift and propulsion needed for a new class of efficient high-speed compound helicopters.

One of the ironies of rotorcraft history is the remarkably rapid demise of the compound helicopter following the Army's cancellation of the Cheyenne program, along with the near-simultaneous resolution of tiltrotor technical issues and the dramatic success of the Army-NASA XV-15 tiltrotor. This stunning reversal of fortunes of the compound and tiltrotor seemed to

confirm the perception that the tiltrotor was the right solution for high-speed rotorcraft. It is ironic that the XV-15, perhaps the greatest achievement of the Army-NASA collaboration at Ames, likely ended serious R&D for the modern compound. Nevertheless, after remaining dormant for decades, interest in the compound began to reappear as evidenced by Piasecki's X-47, Sikorsky's X2 and S-97 Raider, and Airbus Helicopters' X³. Mission design studies by NASA and the Army at Ames, beginning around 2005, showed potential for civil and military applications, particularly for the winged compound variant.

My rationale for the potential of the compound, very briefly, is that it offers a viable place in the mission spectrum between the conventional helicopter and the tiltrotor; it should offer a favorable tradeoff of performance and weight, cost, and reliability to enable future Army missions to benefit from increased speed and range without the cost and complexity of the tiltrotor [53, 54]. Coming full-circle from Andy Morse, I now believe that we should aggressively pursue R&D to build the technology base needed to exploit the potential of a modern compound helicopter, and I appeal to the next generation of researchers to take up this challenge.

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Index

A

Abrego, Anita, 26
Advanced Dynamics Model (ADM), 25-26
Advanced Mechanical Control System (AMCS), 11
Advanced Rotorcraft Technology, Inc. (ART), 34-36
Advanced Systems Research Office (ASRO), 26, 32-33
Aeroflightdynamics Directorate (AFDD), 23, 28, 36, 38
Aeromechanics Laboratory (AL), 26-27, 33
Aérospatiale, 21
AH-1G Cobra, 6
AH-56A Cheyenne, 9-15, 22, 32, 40
AIAA Journal, 7
Aiken, Ed, 2
Air Mobility R&D Laboratory (AMRDL), 12, 18, 20, 23, 26, 32
Alberry, Milt, 11
Alexander Nikolsky Memorial Lectureship, 31
American Helicopter Society (AHS), 7, 13, 16, 19-20, 31-32
American Institute of Aeronautics and Astronautics (AIAA), 1, 7, 16, 19
Ames Directorate, 18-19, 21-23, 27, 32
Ames-Army Lab, 11, 13-15, 18-19, 23-24, 26, 29-30, 36-37, 39
Amrani, Omar, 34
Anderson, Bill, 10
Apollo program, 13
Applied Technology Laboratory (ATL), 26-28
Army Aeronautical Research Group (AARG), 4, 7, 19, 28
Army Aeronautical Research Laboratory (AARL), 1, 3-16, 18, 39-40
Army Aviation Materiel Laboratories (AVLABS), 10, 18, 32
Army Aviation R&D Command (AVRADCOM), 26
Army Material Command (AMC), 12, 18
Army's Advanced Aerial Fire Support System (AAFSS), 10
Army-NASA Joint Agreement, 4-5, 19-20, 39
Austin, Ed, 32
Aviation Systems Command (AVSCOM), 16, 34
Aviation Week, 1

B

Babb, Larry, 34
Ballhaus, Bill, 7
Bearingless Main Rotor (BMR), 23-24, 26-27
Beebe, John, 12
Bell Helicopter, 6, 28, 32, 40
Bell Model 680, 24
Bhagwat, Mahendra, 36
Biggers, Jim, 8, 19, 27
Boeing Helicopter Co., 34, 36
Boeing Supersonic Transport (SST), 3

Boeing Vertol YUH-61A, 23
Borgman, Dean, 4, 19
Bretagne, 21
Brooks, George, 13-14
Building N-200, 20
Building N-201, 20
Building N-215, 4, 9, 28

C

CAMRAD, 31, 37
CAMRAD II, 37-38
CAMRAD JA, 37
Caradonna, Frank, 9, 19, 38
Carlson, Dick, 13, 26, 32, 36-37
Carpenter, Paul, 32
Carr, Larry, 10, 19
Carter, Ted, 19
Carvell, E. C., 5
Cassarino, Sebby, 34
Chan, Brian, 26
Chan, Steve, 26
Chau, Dung, 34
Chesarek, Joseph, 12
Cocco, Art, 5
Computational fluid dynamics (CFD), 7, 25, 38-39
Computational Research and Engineering Acquisition Tools and Environments program (CREATE-AV), 39
Computational structural dynamics (CSD), 25, 38-39
Computer Sciences Corporation (CSC), 32-34, 37
Condit, Phil, 2-4
Condon, Greg, 27
Cooperative Research and Development Agreement, 36
Cornell Aeronautical Laboratory (CAL), 1, 18, 29, 33
Costes, Jean-Joel, 21-22
Crawford, Charlie, 33-34
Cross (Blanken), Lori, 29
Cross, Jeff, 27
Curtiss, Pat, 15

D

Dat, Roland, 21-22
Davis, John, 32-33
Dawson, Matt, 30
Dawson, Seth, 24
Decker, Bill, 2
Department of Defense (DoD), 30, 39
Dowell, Earl, 2, 14
Dueker, Chris, 34
Duffy, Bob, 2
Durbin, Enoch, 2

E

Eckert, Bill, 29
Edenborough, Kip, 2, 29
Empey, Wayne, 4, 6

- Eppler, R., 6
 Esculier, Jacques, 30
 Eustis Directorate, 18, 23, 26, 32
 Executive Complex (EC), 32-34, 37
- F**
 F4U Corsair, 1
 Fish, Terry, 26
 Flettner, Anton, 8
 Flexbeam Air Resonance (FLAIR), 23, 25
 Flight Research Rotor (FRR), 26-28, 38
 Floquet Theory, 15
 Focke-Achgelis Fa-223, 8
 Franklin, Jack, 2
 Friedmann, Peretz, 14, 31
 Ft. Eustis, 10, 18, 32, 36-37
- G**
 Gabel, Dick, 23
 Gaines, Bob, 5
 General Rotorcraft Aeromechanical Stability Program (GRASP), 23
 George, Bob, 18
 Georgia Institute of Technology, 33
 Goff, Barbara, 5
 Goorjian, Peter, 6-7
 Government Industry Working Group (GIWG), 32
 Graham, Dunstan, 2
 Grassley, Chuck, 34
 Grina, Ken, 23
 Gunter, Joel, 25
 Gustavson, Bruce, 34
- H**
 Haslim, Len, 27
 Heavy Lift Helicopter (HLH), 12, 18
 Helicopter Flight Investigations Branch, 27
 Hendley, Elizabeth, 26
 High Maneuverability Advanced Rotor and Control System (HIMARCS), 28
 High Performance Computing Modernization Program (HPCMP), 39
 Hiller Aircraft, 4, 7, 14
 Hinnant, Howard, 30
 Hodges, Dewey, 13-15, 19, 23-24, 30-31, 36
 Hodges-Dowell equations, 13-14, 24
 Hogan, Jim, 26
 Hohenemser, Kurt, 8, 13, 15, 19, 32
 Hopkins, Stu, 23, 30, 33, 36
 Horn, Pat, 29-30
 Houbolt, John, 13-14
 House, Tom, 34
 HPC Institute for Advanced Rotorcraft Modeling and Simulation (HI-ARMS), 39
 Hughes Advanced Rotor Program (HARP), 28
 Hughes Helicopters, 12, 28
- I**
 IBM 1800 computer, 5, 17
 IBM key-punch machine, 5, 17
- Integrated Technology Rotor (ITR), 26-28, 38
 Ionesco, Eugène, 21
- J**
 Johnson Aeronautics, 34, 37
 Johnson, Peter, 34
 Johnson, Wayne, 8, 25, 29-31, 37-38
 Joint Aeronautical Research Group (JARG), 4, 28-29
 Joint Multi-Role Tech Demonstrator (JMR), 28, 40
 Journal of the AHS, 7, 13, 20
- K**
 Kaman Aerospace, 8, 28, 34
 Kelly, Mark, 8, 11, 30
 Kerr, Andy, 2, 26, 31-33
 Klein, Norm, 12
 Kodani, Mike, 5, 26
 Kuczynski, Bill, 14
 Kumar, Dharmendra, 34
 Kunz, Don, 23, 30
- L**
 Langley Directorate, 18
 Langley Research Center, 6, 13, 18, 27
 Laub, Georgene, 4-5, 19
 Lautenschlager, Jon, 18, 25
 Lazzeroni, Frank, 4-5, 28
 Lebacqz, Vic, 2, 4
 Lewis Directorate, 18
 Lewis Research Center, 18
 Lim, Joon, 33
 Ling Temco Vought (LTV), 6
 Lockheed, 8-11, 13-16, 22, 32
 Lynn, Bob, 6
- M**
 MacDonald, Jeff, 10, 32
 MacNeal, Dick, 32
 MacNeal-Schwendler Corporation, 32
 Magee, John, 29
 Maier, Tom, 24-26
 Maisel, Marty, 29
 Mark, Hans, 9, 20
 Massachusetts Institute of Technology (MIT), 7, 14
 MBB Bo-105, 22
 McCloud, John, 8, 11
 McCormick, Barney, 8
 McCroskey, Jim, 2-5, 7, 9, 16, 18, 21, 28, 38
 McDonnell Douglas Helicopter Co., 34
 McKenna, Skip, 34
 McNulty, Mike, 24-25, 30
 Memorandum of Understanding (MOU), 21-22
 Miller, Rene, 14
 Mirick, Paul, 27, 32
 Miura, Hiro, 2
 Morse, Andy, 3-4, 7, 9, 17-19, 28-29, 40-41
 Mort, Ken, 11
- N**
 NASTRAN, 32

Ormiston, R.

National Advisory Committee for Aeronautics
(NACA), 1, 4-5, 39

National Rotorcraft Technology Center (NRTC), 38

Nikolsky Lecture, 16, 31

O

Office National d'Etudes et de Recherches
Aérospatiales (ONERA), 20-22, 25, 30

Olilla, Jack, 25

Ormiston, Bob, 1-2, 5, 14, 17, 21, 25, 27, 30, 34

Ormiston, Wendy, 21

P

P-51 Mustang, 1

Panda, Brahmananda, 36

Peleau, Bernadette, 25

Perkins, Court, 2

Peters, Dave, 13-15, 19, 23-24, 31

Pflugger, Dave, 26

Philippe, Annie, 21

Philippe, Jean-Jacques, 21

Philippe, Sebastian, 21

Piasecki, Frank, 41

Piziali, Ray, 29, 33

Poisson-Quinton, Philippe, 21

Potsdam, M., 38

Powell, Bob, 26-27

Prandtl-Glauert, 6, 8, 16

Prouty, Ray, 10-11

Puccinelli, Lou, 5

R

Rabbott, Jack, 29

Rafati, Hamid, 34

Ragosta, Art, 32-33

Ray, Dave, 5

Reed Aeronautics Award, 16

Renaud, Jean, 21

Rensselaer Polytechnic Institute (RPI), 1-2, 11

Research and Technology Laboratories (RTL), 26,
32-33

Reserve Officers' Training Corps (ROTC), 6

Rogers, George, 34

Ross, Glenn, 5

Rotor Dynamics Model (RDM), 25

Rotor Systems Flight Investigations Branch, 38

Rotor Systems Research Aircraft (RSRA), 27, 38

Rotor Systems Technology Branch, 27

Rotorcraft Comprehensive Analysis System (RCAS),
36-38

Rotorcraft Dynamics Division (RDD), 29-31, 35

Rotorcraft Industry Technology Association (RITA), 38

Royal Air Force, 6

Rutkowski, Mike, 30, 33-35

Ruzicka, Gene, 33-34

S

Saberi, Hossein, 34-37

Sangha, Karan, 34

Schmitz, Fred, 2, 4-5, 7, 11, 19, 28

Schmitz, Sheila, 2

Scully, Mike, 40

Seals, Rupert, 34

Seckel, Ed, 2

Second Generation Comprehensive Helicopter Analysis
System (2GCHAS), 31-38

Seto, Ed, 27

Sharpe, Dave, 4, 14-16, 19, 24-27, 30

Sibert, George, 2

Sikorsky Aircraft, 4, 8, 12, 19, 23, 28, 34, 40-41

Singley, George, 34

Sissingh, Gerhard, 14

Small Business Innovation Research (SBIR), 35

Snider, James, 40

Snyder, Bill, 27

Source Selection Evaluation Board, 12

Spivey, Dick, 40

Sredanovic, Mary, 34

Stanford University, 12

Stapleton, Cyril D., 4, 7

Statler, Irv, 18, 21-22, 28-29, 33, 40

Steengrafe, Alta, 5

Stephens, Wendell, 30-34

Sterling Federal Systems, Inc., 34

Stroub, Bob, 29

Sweeney, Tom, 2

Swenson, Ozzie, 18, 26

T

Tan, Carina, 33-34

Technology Complex (TC), 32, 34

Thuy, Tran Cam, 22

Tice, Alice, 5

Tinling, Bruce, 30

Torsionally soft rotor (TSR), 24

Trinh, Lisa, 34

Tung, Chee, 35, 38

Twomey, Bill, 21-22

U

UH-60A Airloads Workshop, 38

University of California, Berkeley, 2

University of California, Los Angeles, 31

Utility Tactical Transport Aircraft System (UTTAS), 23

V

Vause, Rande, 4, 6, 19

Vernacchia, Carla, 34

vertical/short takeoff and landing (V/STOL), 3, 8

vertical takeoff and landing (VTOL), 3, 6-7

W

Wang, Jerry, 26

Warmbrodt, William, 14, 31, 35, 37

Washington University, 8, 13, 15

Welk, Brenda, 5

Wells, Gene, 5

West Point, 2, 16

Westland WG-13 Lynx, 22
White, Jerry, 34
White, Sam, 27
Wood, Bob, 19
Wortmann, F. X., 6
Wright brothers, 1
Wynn, Tom, 4

X

XH-51 helicopter, 9
XV-15 tiltrotor, 40-41

Y

Yaggy, Paul, 3-4, 6-12, 15, 17-18, 20-21, 29, 40
Yount, Bryan, 26
Yu, Yung, 38

The Golden Age of the Army Research Labs

David A. Peters
Army

Getting There

In 1970, I was a young engineer at McDonnell Douglas Spacecraft Corporation, working on the structural dynamics of Skylab and escaping Vietnam on a defense deferment. I was also attending Washington University (WU) part-time, working on an M.S. degree in Applied Mechanics with Professor Kurt Hohenemser as my thesis advisor [1-3]. The research was funded by the newly formed Army Aeronautical Research Laboratory (AARL) at Moffett Field, California. One day the phone rang in my office, and it was Paul Yaggy, founding Director of the Ames-Army Lab. He told me that they were looking for engineers and that my advisor, Dr. Hohenemser, had recommended me. If I came, he said, they would also pay to send me to Stanford part-time to work on my doctorate.



David A. Peters

I said, “Mr. Yaggy, I would love to come; but a new ruling has just been issued by the government—only one defense deferment per person per lifetime. If I leave McDonnell Douglas, I will lose my deferment and be drafted into the Army.” There was a long pause on the other end of the line, and Paul Yaggy cleared his throat. “David,” he said, “We *are* the United States Army. We can get you a deferment.” I did not ask the salary; I did not ask the title; I did not ask my wife. I just said, “When do you want me there?” We packed our bags and arrived on September 1, 1970.

After we arrived, I walked into AARL, located in Building N-215, and announced I was there. Our wonderful administrative assistant, Ms. Steengrafe looked shocked. “What are you doing here? You can’t move here until you are issued orders!” she admonished. Well, I was there without any orders, but she fixed it up. My mover was actually cheaper than the government movers, so all was forgiven. My wife (who was already a civil service employee in St. Louis) was even given a job in the NASA Ames Hangar. We were nurtured and supported from the start.

Being There

Right away, I knew that it was going to be fun. My boss was this great guy named Bob Ormiston, and his boss was the seasoned Andy Morse. At that point, I knew Floquet theory but knew little about helicopters; I had no idea what a swashplate was. Bob taught me everything. I really did not know whether I worked for the Army or NASA until my first paycheck arrived; the organizations were that intermeshed. The wind tunnel models had NASA stenciled on one side and U.S. Army on the other. One of the first things Bob had me work on was the control response characteristics of hingeless rotors in forward flight. The idea was to study the influence of solidity, Lock number, blade flapping stiffness, and pitch-flap coupling, and to accurately account for reverse flow, elastic bending modes, and higher harmonic responses in comparison with simpler approximations then in common use. I developed a matrix formulation for generalized harmonic balance and wrote BAFOL and MABOL codes for rigid and elastic blade

models, respectively, which ran on our IBM 1800 computer. Paul Yaggy kept his word and I went to Stanford; the work on BAFOL and MABOL eventually became the subject of my Ph.D. thesis [4, 5]. When I received my Ph.D. in 1974, Paul Yaggy called me personally to congratulate me on being the first to complete the program with Stanford—it was the kind of personal touch that made the Army labs a great place.

Dynamic Inflow

At that time, Dave Sharpe had a research contract with Lockheed and had been running a small-scale 7.5-foot hingeless rotor in the 7- by 10-Foot Wind Tunnel. The data did not agree with Lockheed's analysis, and Bob remembered that Norm Shupe, one of Pat Curtiss' students at Princeton, had investigated the effects of nonuniform inflow on hingeless rotor response characteristics. Bob suggested that I incorporate nonuniform inflow in BAFOL and MABOL. I had ruled out all the usual suspects for the failed correlation: reverse flow, higher harmonics, tip losses, elastic modes, etc. I took on that challenge, and Bob and I developed a number of inflow models based on momentum and vortex theory that compared very well with Dave Sharpe's data in both hover and forward flight [6, 7].

Dave's Lockheed contract included subsequent wind tunnel entries to measure rotor frequency response characteristics by exciting the swashplate cyclic control, as well as fuselage pitch-roll motions to measure the unsteady rotor pitch and roll moment responses. We wanted to include the nonuniform inflow for the frequency response predictions and find some way to account for the unsteady wake as well. In my spare time while I served on the Utility Tactical Transport Aircraft System (UTTAS) Source Selection Evaluation Board, I came up with the idea of modeling the unsteady rotor wake inflow with the apparent mass of a circular disk based on potential theory. I incorporated this concept in my BAFOL and MABOL codes, and I was able to correlate all of Dave's Lockheed data perfectly. The theory of dynamic inflow was formed and the rest is history. I presented that work at the 1974 American Helicopter Society (AHS)/NASA Ames Specialists' Meeting on Rotorcraft Dynamics [8].

Being at the Ames-Army Lab meant a lot of fun with the gang. We had lunch together regularly at the NASA Ames Cafeteria where we exchanged technical ideas and found out whose wives were pregnant. (Frank Caradonna learned he was going to be a dad when he bit into a note that his wife had placed in his sandwich.) We also went out some, which is where I was introduced to Chinese food at Chef Chu's and Mexican food at Estrellitas. It was at one of these lunches that I coined the phrase, "Apple Pie-a-la-Mode Shape." Frank Caradonna, my office mate Dean Borgman, Bill Bousman, Fred Schmitz, and I went on skiing trips together. There was a strong Princeton connection, and this young kid from the Midwest was made to feel right at home.

Close Call

There was one point, however, where I thought I would not get my Ph.D. Stanford had kicked the Reserve Officers' Training Corps (ROTC) off campus in protest against the Vietnam War, and the Army issued an edict that they would no longer send their employees to universities that banned ROTC. This was where Irv Statler came to my rescue. He had taken over from Paul Yaggy and came to tell me the bad news. Then he added, "But here is what we will do. I will put you on the NASA payroll for now to go to Stanford, and they won't notice for a while. Meanwhile, I am going to talk to Georgene Laub."

Georgene was one of the few female engineers at Ames in 1970. She was a wonderful person. She bought a unicycle and would practice in our narrow hallways before work started. She almost ran our director down once. So, Irv went to her and said, “Georgene, you want to go to Stanford.” “No, I don’t,” she replied. “Oh, yes, you do,” Irv said. “But you cannot go because of the Army directive whereas the NASA man working right next to you as your peer is allowed to go. You are being discriminated against, and you are going to file an official complaint.” “You are right,” Georgene said. “I *do* want to go to Stanford!”

After Georgene’s formal complaint, the Army rescinded their order, and I was put back on the Army payroll and was able to finish my degree. This is how the people in the Ames-Army Lab cared for each other and helped each other. It was what allowed us to do our research unbridled by needless bureaucracy. We were always a family. Even when I accidentally spilled an entire cup of coffee into Bob Ormiston’s file drawer, he never raised his voice or reprimanded me. The Ames-Army Lab was simply a wonderful place to work.

Big Advancements

With this type of support and camaraderie, we were able to make great advances in many areas besides just dynamic inflow. I extended my Floquet theory to include the blade lead-lag degree of freedom and documented for the first time the need to trim a rotor before doing a linearized flap-lag stability analysis in forward flight [9]. Earlier, Bob and Dewey Hodges had developed the first correct analysis of hingeless rotor blade flap-lag stability for the hover condition, and I was able to contribute to applications of the Routh–Hurwitz stability criterion. One day in Professor Kane’s Dynamics class at Stanford, he explained the Kelvin–Tait–Chetaev theorem. I stopped listening to the lecture after hearing that. I immediately knew the application to Bob and Dewey’s flap-lag analysis and started working it out. From that, we found closed-form approximations to flap-lag damping that explained the earlier results in terms of basic physics [10].

Bob asked Dewey and Princeton Professor Earl Dowell to extend the fundamental equations for the nonlinear bending and torsion of rotating elastic cantilever beams needed for more rigorous analyses of hingeless rotor systems. They soon had to deal with transformation of strain-displacements relationships in the elastic deformed and undeformed coordinate systems, and I was able to help work out the transformation for the deformed blade coordinate system. I wrote the appendix for the derivation of this transformation for their seminal 1974 NASA TN D-7818 [11]. These nonlinear deformation effects also influenced the blade angle of attack and aerodynamic terms in aeroelastic analyses [12]. Another important step was an approximate formulation of frequency and modes in flap, lag, and torsion to put into the Galerkin analysis [13].

To test the new Hodges–Dowell nonlinear beam theory, Princeton performed basic laboratory experiments with uniform cantilever beams with simple tip-mass loading. The experiments generally confirmed the theory very well for the intended moderate deformation conditions. However, we discovered that the buckling loads of the experiments did not exactly agree with our theories or with the classic buckling formulas in the literature. We pulled out every buckling text we could find to see what they said, and discovered that there were some secondary coupling effects that had been studied by Prandtl and Reissner. While we were waiting for papers from the NASA Ames Library, we set out to derive our own theory of second-order effects.

When the papers came in, we discovered that the theories of Prandtl (1899) and Reissner (1904) led to much more complicated equations than our own. We further discovered that the two theories were not even equivalent to each other. In working out the details, we found separate errors in Prandtl and Reissner. When these errors were corrected, their complicated equations reduced to our simple one, which correlated the experimental buckling load exactly. At first, Dewey Hodges and I did this on our own, not knowing if the Army would support us investigating errors in ancient civil engineering papers. However, when we finally told Bob Ormiston what we were up to, he gave us his blessing. The idea that our Army bosses would let us work on such fundamental issues (such as getting the equations right) is what allowed advances to be made in rotorcraft analysis. Our work ended up being published in the *Journal of Solids and Structures* [14].

The work on flap-lag stability led to a U.S. patent on a rotor with improved stability, as well as several NASA Technology Utilization awards. I received the American Society of Mechanical Engineers (ASME) Pi Tau Sigma Gold Medal for my part in the buckling saga.

Another wonderful part of being at the Army labs was the practical experience with real-world problems. I was on the review team to solve mast bumping of OH-58 helicopters. I worked on stability of the icing spray rig down at Edwards Air Force Base, and we flew down in an Army light plane, landing on the dry lake! I was on the Source Selection Evaluation Boards for both UTTAS and the Advanced Attack Helicopter (AAH), spending long hours at the Army Depot in Granite City, Illinois, and at the Aviation Systems Command (AVSCOM), which was then in St. Louis. It was typical that our organization was willing to loan us out on other projects for our good and the good of the Army.

Saying Goodbye

I finally left AARL, then called the Ames Directorate of the U.S. Army Air Mobility R&D Laboratory (AMRDL), in 1975 to come back to the Midwest and to WU. However, the Ames-Army Lab continued to mold me, and I endeavored to continue making contributions. In 1978, I was invited back to spend a summer there and work on the equivalent Lock number and equivalent drag coefficient for flap-lag stability, both of which derived from dynamic inflow [15].

The dynamic-inflow ideas proposed by Bob Ormiston were developed to full maturity at WU into first the Pitt model [16] and then at the Georgia Institute of Technology into the He model [17]. These models are now in wide use throughout the rotorcraft technical community for applications including performance, loads, aeroelastic stability, and flight control. They have also been incorporated in the Army's Rotorcraft Comprehensive Analysis System (RCAS) and the Advanced Rotorcraft Technology, Inc. real-time flight simulation code FLIGHTLAB. I have had several students work at the Army labs. Unfortunately, one of those students, Ivo Zvolanek, only lasted a day. An earthquake hit over the weekend and Ivo decided to return to St. Louis to work at McDonnell Douglas. Despite that setback, however, many of my students have ended up working for the Army and for the Ames-Army Lab.

In summary, I can say that those days at the newly formed AARL were a golden age and a golden time in my life. They formed the person I would become as a researcher, and they set the stage for the next 40 years of rotorcraft research in my life and in the industry. What made the experience great was the people who were there, how they cared for each other, and how they

were driven to find out the true physics behind the complicated behavior of rotary-wing aircraft. I will never forget those days.

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Index

A

Advanced Attack Helicopter (AAH), 4
American Helicopter Society (AHS), 2
American Society of Mechanical Engineers (ASME), 4
Army Aeronautical Research Laboratory (AARL), 1, 4
Army Air Mobility R&D Laboratory (AMRDL), 4
Aviation Systems Command (AVSCOM), 4

B

BAFOL, 1-2
Borgman, Dean, 2
Bousman, Bill, 2

C

Caradonna, Frank, 2
Curtiss, Pat, 2

D

Dowell, Earl, 3

E

Edwards Air Force Base, 4

F

FLIGHTLAB, 4
Floquet theory, 1, 3

G

Georgia Institute of Technology, 4

H

He model, 4
Hodges, Dewey, 3-4
Hohenemser, Kurt, 1

I

IBM, 2

K

Kelvin–Tait–Chetaev theorem, 3

L

Lock number, 1, 4
Laub, Georgene, 2-3
Lockheed, 2

M

MABOL, 1-2
McDonnell Douglas Spacecraft Corporation, 1, 4
Moffett Field, 1
Morse, Andy, 1

O

Ormiston, Bob, 1-4

P

Pitt model, 4
Prandtl, 3-4
Princeton University, 2-3

R

Reissner, 3-4
Reserve Officers' Training Corps (ROTC), 2
Rotorcraft Comprehensive Analysis System (RCAS), 4

S

Schmitz, Fred, 2
Sharpe, Dave, 2
Shupe, Norm, 2
Skylab, 1
Stanford University, 1-3
Statler, Irv, 2
Steengrafe, Alta, 1

U

Utility Tactical Transport Aircraft System (UTTAS), 2, 4

W

Washington University (WU), 1, 4

Y

Yaggy, Paul, 1-2

Z

Zvolanek, Ivo, 4

My Involvement With the Army Air Mobility R&D Laboratory at Ames

Victor L. Peterson
NASA

I joined the staff at Ames in 1956 when it was the Ames Aeronautical Laboratory, one of three laboratories, Ames, Langley, and Lewis, together with the Muroc Flight Test Station, that comprised the National Advisory Committee for Aeronautics (NACA). These four NACA organizations provided the base upon which NASA was formed in 1958. The NACA brought with it a long history of working closely with the military. After serving as an individual researcher, and holding several branch-level research management positions, I was appointed in 1974 to the position of Chief of the Thermo- and Gas-Dynamics Division to succeed Dean Chapman, who became Director of Astronautics. Our division was responsible for research in computational fluid dynamics (CFD), experimental fluid dynamics, hypersonic aerodynamics, physics, and thermal protection systems, as well as management of hypersonic wind tunnels, shock tubes, and arc-jet facilities.



Victor Peterson

A major thrust in the division was pioneering the discipline of CFD. Ames was at the forefront of CFD at that time, in part because Ames researchers were leading the development of algorithms for numerically solving approximations to the governing Navier–Stokes equations and, in part, because Ames had access to the most powerful digital computers available. Ames management was constantly on the lookout for ways to advance the discipline of CFD with superior researchers and leading-edge digital computers.

In 1965, NASA and the Army established, under a new Army-NASA Joint Agreement, the Army Aeronautical Research Laboratory at Ames, for collaborative research of mutual interest to both parties. A young captain, William F. Ballhaus, a recent Ph.D. graduate from University of California, Berkeley, was among those assigned by the Army in 1971 to work at Ames-Army Lab, by then the Ames Directorate of the Army Air Mobility R&D Laboratory (AMRDL). Bill's expertise was in finite-difference methods for solving flows around high-speed helicopter rotor tips. This research was closely related to the larger NASA effort in CFD involving 3-D flows past swept wings, so he was placed in the CFD Branch at Ames. This was allowed under terms of the Joint Agreement, wherein Army personnel could be assigned to work in NASA organizations under NASA supervisors. Bill remained an Army employee but was assigned to the NASA branch. His performance appraisals were conducted by his NASA supervisor, but reviewed by Dr. Irv Statler, Director of the Ames Directorate.

Bill Ballhaus was immediately accepted into the elite group of NASA CFD researchers—"they spoke the same language." Before long Bill teamed with Ron Bailey, and they developed the "Bailey–Ballhaus code" for treating transonic flows past swept wings. This code became a workhorse for solving practical transonic aircraft-design problems. It gained considerable notoriety in the NASA-sponsored Highly Maneuverable Aircraft Technology (HiMat) program.

The contractor built an aircraft that initially failed to meet design goals. In a desperate effort to correct the situation, the contractor came to Ames, presented the problem, and worked with Bailey and Ballhaus to adjust the design. The new design allowed the HiMat program to meet its goals.

The work with the HiMat program, in addition to similar examples, showed that CFD was ready for “prime-time use” as a practical design tool. This prompted me to approach my supervisor, Dean Chapman, then head of the Astronautics Directorate at Ames, with a proposal to create a new Applied Computational Fluid Dynamics Branch. The charter for the branch was to focus on the development of tools to apply CFD to practical aircraft design problems. Dean agreed that we should proceed with the plan. Efforts were set into motion to develop a branch charter and to issue an advertisement for a branch chief. Several researchers applied for the position, including Bill Ballhaus. Bill’s established reputation in the field of applied CFD, and his already-demonstrated management skills, made him the obvious choice for the position.

Because Bill was an Army employee, I had to obtain permission from Dr. Irv Statler to make the selection. There was no precedent for an Army employee to serve as a NASA Branch Chief. It was up to me to break the news to Irv about our selection. I wondered what would happen if Irv told me that an Army employee couldn’t lead a NASA branch, so I was prepared to make the strongest case possible. When I gave the news to Irv, he broke out into a big smile. He told me that the news couldn’t be better, because it was an example of how tightly connected the programs were between Ames and the Army. He had been telling his superiors all along that this was the case, and this action provided clear evidence. In Bill’s chapter there is a photo of four smiling faces as Irv officially appoints Bill as the NASA Branch Chief.

In 1980, 2 years after Bill assumed the branch chief position, Dean Chapman decided to retire from Ames and move to a position at Stanford University. A number of people, including Bill Ballhaus and I, applied to fill the position being vacated by Dean. Most everyone assumed that I would be selected, since I was Dean’s senior subordinate and we had been working closely together for a long time to further Ames thrust in CFD and supercomputing. To the surprise of many, Bill Ballhaus was selected. This marked the end of Bill’s tenure as an Army employee. He became a member of the NASA Senior Executive staff.

Following the announcement of Bill’s selection, I offered my congratulations and knew that we could continue to work closely in pursuit of our common goals. I sensed that Bill was concerned about our future relationship, because he scheduled a meeting with me immediately after the announcement. We had deep respect for each other. That night I decided to put Bill’s concerns to rest by preparing a letter to him in which I framed the situation to be like the pride a college professor (me) has when one of his star students (Bill) graduates and becomes a superstar in the aerospace world. Many years later, Bill told me that that was one of the best letters he had ever received.

Four years after Bill Ballhaus became Director of Astronautics, he was chosen to be the Ames Center Director, when then-Director Sy Syvertson retired. Bill then selected me to replace him as Director of Astronautics. I served in that position for 4 years, reporting to Bill as my boss until he left NASA. Dale Compton became the new Center Director, and Dale selected me as Deputy Center Director. I served 4 years in that position, until my retirement from NASA in 1994.

Throughout his career in the Army and NASA, Bill Ballhaus played an important role in advancing the discipline of CFD and its importance in the design of aerospace vehicles. Both his technical contributions and his management skills helped Ames gain the lead NASA role in advanced supercomputing, a position it still holds to this day. More importantly, his work was used extensively to justify the value of supercomputers in maintaining this country's leadership role in aircraft design and development. Clearly, the NASA-Army Joint Agreement proved to be a major asset to Ames, to NASA, and to the country.

Index

A

Ames Directorate, 1
 Ames Research Center, 1-3
 Applied Computational Fluid Dynamics Branch, 2
 Army Air Mobility R&D Laboratory (AMRDL), 1
 Army, 1-3
 Astronautics Directorate, 2

B

Bailey, Ron 1-2
 Ballhaus, William F., 1-3

C

Chapman, Dean, 1-2
 Compton, Dale, 2
 Computational Fluid Dynamics (CFD), 1-3

H

Highly Maneuverable Aircraft Technology (HiMat),
 1-2

L

Langley, 1
 Lewis, 1

N

NASA, 1-3
 NASA-Army Joint Agreement, 3
 National Advisory Committee for Aeronautics
 (NACA), 1
 Navier–Stokes, 1

S

Stanford University, 2
 Statler, Irv, 1-2
 Syvertson, Sy, 2

T

Thermo- and Gas-Dynamics Division, 1

U

University of California, Berkeley, 1

Remembrances—My Early Experiences as a Research Scientist

Frederic H. Schmitz
Army/NASA

Joining the Ames-Army Lab

Undergraduate and graduate school. I began my professional career at Rensselaer Polytechnic Institute (RPI) in upstate New York. My initial goals were to become an aeronautical engineer and a pilot to fly fighter jets in the Air Force. To this end, I joined the Air Force Reserve Officers' Training Corps (ROTC) my freshman year at RPI. However, after my sophomore year, my eyesight became 20/30 in one eye, which was enough to prohibit me from becoming an Air Force pilot at that time. There were about 20 others in the ROTC program who had similar physical problems. To keep us in the program, if we qualified we could go directly to the graduate school of our choice, under an Air Force Institute of Technology (AFIT) program for up to 2 years, to obtain a Masters of Engineering as a second lieutenant. Because I had good grades as an undergraduate and performed well at the 4-week basic training class in the summer of my junior year, I was promoted to Wing Commander, the leader of over 1,000 Air Force ROTC cadets at RPI.



Fred Schmitz

With the promise of funding through the AFIT program, I investigated possible graduate schools that qualified for AFIT support. I chose Massachusetts Institute of Technology (MIT), working with Dr. Wrigley, an MIT professor who was working on guidance and control problems for the NASA space program. He assured me a place under his guidance for the fall of 1964. However, I also visited the Aerospace and Mechanical Sciences Department at Princeton University and was impressed with their guidance and control group as well. My future advisor at Princeton, Professor Dunstan Graham, knew that I was probably going to accept MIT's offer but warned that I should "never trust the military." He suggested that he would help me apply for a NASA fellowship at Princeton, working in the guidance and control area, just in case. In the spring of 1964, the year of my graduation, I was informed that the AFIT program was cancelled and that I now had to accept my commission in the Air Force without graduate training. I was devastated. However, a week or so later, I also received a letter from Princeton University stating that I had been selected for a 3-year (all expenses paid) NASA fellowship. I attribute this lucky break to Professor Dunstan Graham.

I became a graduate student under Dunstan's guidance for the next 4-plus years. I was also fortunate to work closely and share an office with Mr. Theodore Dukes, another notable guidance and control expert who had immigrated to the United States during the Hungarian Revolution. Although he was confined to a wheelchair his entire adult life, he made major contributions to aircraft visual displays by incorporating rate feedback information on display symbology. He taught me how to become a good researcher by always remaining positive and confident in my abilities.



Ted Dukes and my first son at Princeton University.

My tasks during this period were associated with developing theoretical stability and control models of new vertical takeoff and landing (VTOL) aircraft—especially the XC-142 tri-service tiltwing. I was in charge of calculating lateral stability derivatives for this vehicle and helping set up verification experiments on the Princeton Long Track (see photo below).

After passing the Princeton University qualifying exam, I was asked what topic I was going to choose for my Ph.D. thesis. This was the hardest thing that I had had to do in my entire educational experience to date, because I wanted to choose something new, technology wise, but something that would also have a practical application. The experience of having to find a good Ph.D. research topic, with almost no faculty assistance, would guide my life for my entire career. It taught me to evaluate the available technical options critically and carefully, and to choose topics that were relevant and that interested me deeply. I

decided to focus my thesis on optimal control methods for low-speed aircraft and helicopters.

Life was good at Princeton during these early years. The professors were excellent teachers, and they had acquired enough experience to impart a worldly view of the aerospace industry. I met several fellow graduate students who became lifetime friends and with whom I stayed in touch throughout my entire career—including Bob Ormiston, Victor Lebacqz, Jack Franklin, Jim McCroskey, Hiro Miura, and Edwin Aiken. After my first year at Princeton, I married Sheila, the love of my life, whom I had dated for 3 years at RPI. I obtained my private pilot's license at a nearby airport in Pennsylvania, and played tennis with some of the faculty and graduate students. We also went skiing at the nearby ski areas and once, even “borrowed” the Princeton Navion airplane to scout out the snow conditions on the day before we went skiing. It was truly a special time.

One afternoon at Princeton, I was casually introduced to Mr. W. Z. “Steppy” Stepniewski, a noted pioneer in the aeronautical and VTOL research community working at the Boeing Vertol Company, who was visiting Princeton. He had just given a seminar at Princeton and was discussing the preparations for a future Princeton teaching assignment, where he would teach Professor Pat Curtiss' lectures on VTOL aircraft, while Pat took a sabbatical leave. While at Boeing Vertol, in Philadelphia, Pennsylvania, Steppy designed and developed the first tiltwing VTOL aircraft. I remember our meeting and the subsequent discussion quite well, because his research interests and my own were almost identical. He had done his thesis in Poland some years earlier on the optimization of low-speed aircraft flight. Because of WWII, his thesis was never published and he never received his Ph.D.



The XC-142 tiltwing VTOL aircraft model mounted on the Princeton University Long Track.

At the end of my time at Princeton, my wife and I were fortunate to bring our first son into this world. This complicated things for me as my funding support had just run out and I needed to find a job. Fortunately, the aircraft industry was hiring and I had several employment opportunities. I enthusiastically went to work for Steppy in the Advanced Research Department at the Boeing Vertol Company. I also took a part-time job at the Forrestal Research Center at Princeton, one day a week, to consult on an existing Army contract and to finish my thesis. Fortunately, while I was working for Boeing Vertol, I completed all my requirements and graduated from Princeton in the Spring of 1970. My Ph.D. was titled *Takeoff Optimization for STOL Aircraft and Heavily Loaded Helicopters* [1].

W. Z. Stepniewski and Boeing Vertol. I cannot say enough good things about Steppy. He was an excellent boss, mentor, and good friend. He had a worldly view of technology and loved to discuss the technical details of any project. He also had a select group of Polish friends who had come over from the old country and who often inspired spirited lunchtime discussions. They frequently peppered me with fundamental questions to see if I was really on board with a research agenda. Most often they knew and appreciated what the key problems were in the fundamental aeromechanics and design of VTOL aircraft.

Steppy had a caring side that I had not found elsewhere in the rotorcraft industry. He would come to the rescue of people whose self-esteem had dropped to low levels because of some unfortunate event. In one instance, he created a job for a fellow worker whose elder son had accidentally shot his younger brother. He gave this support while the dad and his family tried to work through their terrible loss.



From left: Steppy, his wife Luiza, and me on the roof of a Paris hotel in 1971.

When I arrived at Boeing Vertol, I continued to work on vertical/short takeoff and landing (V/STOL) aircraft applying my thesis to optimal performance problems of Boeing designs. I remember working on a takeoff profile for a Light Intratheater Transport (LIT) V/STOL being proposed to the Air Force that would dramatically reduce takeoff distance in the short takeoff and landing (STOL) configuration. Unfortunately, the project was cancelled as the Vietnam War wound down.

Steppy was well known in the VTOL field because he was the chief designer of the VZ-2, the first tiltwing VTOL aircraft. He was also good friends with Paul Yaggy, who was the Director of the Army Aeronautical Research Laboratory (AARL) at NASA Ames Research Center at Moffett Field, California, and was trying to staff an in-house research team with good low-speed and V/STOL researchers.

My main job at Boeing was project manager for an Army/NASA contract for a theoretical study of the noise/performance tradeoffs of a tiltrotor aircraft. Requests for proposals had been sent to Bell and Boeing for a tiltrotor design and identification of critical development technologies. Because noise was becoming a bigger issue, a separate contract was awarded to Boeing to look

at the external noise radiated from these vehicles. I was given full responsibility for this effort, under the direction of Steppy, because of my theoretical modeling and digital programming experience. The program was monitored by AARL (Dean Borgman) and NASA Ames (Woody Cook and Wally Deckert). After the initial kickoff meeting at Ames, the task of monitoring our progress was given to Dean Borgman—who made several trips to Boeing, Philadelphia. Dean and I became good friends during this period.

Unfortunately, the end of the Vietnam War affected the industry; production contracts for helicopters and other aircraft were truncated, and research and development programs were scaled back as the country became anti-military and anti-technology sentiments prevailed. Layoffs during this period were severe, and people who had trained to be aerospace engineers had to find other jobs to survive. Boeing Vertol evaluated its staff and tried to keep the best people. As far as I can remember, the process was fair, but the depth and long duration of the layoffs resulted in the loss of many excellent workers. Although I survived the cut, life did not look promising in the industry because there was very little money for fundamental research efforts. In this regard, I remember approaching Chuck Ellis, the Vice President and Head of Engineering at Boeing Vertol, for some basic research money. He simply turned me down, explaining the dire situation at Boeing, and told me that I was fortunate to have the Army/NASA research project.

My technical work proceeded nicely. I was able to incorporate measured rotor-acoustic and blade-pressure data from the Boeing Vertol wind tunnel in the tiltrotor modeling that tied the performance state of the vehicle to external radiated noise [2]. During lunch, Dean Borgman asked how things were going at Boeing. As I tried to explain the current research atmosphere there, he asked whether I would consider taking a job at the new Ames-Army Lab. I said that I would consider it, and I talked about the possible job offer with Steppy, who, I believe, called Paul Yaggy for further discussions. It was agreed that the matter would be discussed further at a forthcoming Ames program review meeting at NASA Ames.

At my first contract meeting with Paul, Woody, and Dean, I thought that things were going quite well, until Woody Cook took out a cigar and proceeded to light it. He then interrupted my briefing and asked me if I knew what lighting up his cigar meant. He said that it meant that he was less than satisfied with some aspect of my presentation. That statement produced a blank stare from me—a novice presenter—facing what I thought was a friendly audience. At the break (which came right after Woody made his comment), Dean pulled me aside to help explain Woody's comment. It turned out to be a minor point, but it was now clear to me that the Army and Ames management was definitely “hands on” and in charge. In the following years, Woody and I became friends and even bought land in the same vacation community at Lake Almanor, California.

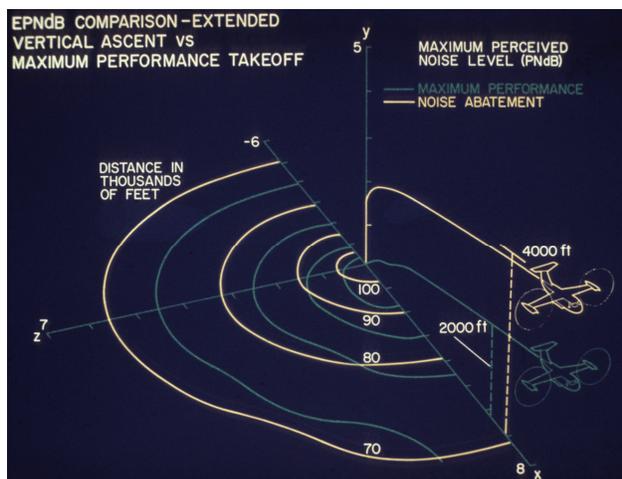
I did receive an offer to come to the Ames-Army Lab to head up acoustic research efforts for the in-house Army Aeronautical Research Group (AARG). I was surprised to learn that Steppy also received an offer to join the laboratory. I believe that he really would have liked to accept the offer, but declined because it would mean relocating away from his many friends and associates in the Philadelphia area. However, he told me that mine was a good job offer that I should accept if I wanted to have a career in fundamental research. After discussing the situation with my wife and realizing that a government laboratory might be a good place to be at that time (“a good place to ride out the anti-technology backlash of the times”), I accepted the offer.

After Chuck Ellis heard that I was leaving to take a job in California, he reconsidered his position and offered me some basic research money. However, it was too late—I had already given my word to Paul and Dean.

We decided that I should travel to California first and Sheila would follow later with our two sons—one just 2 months old. In November of 1970, I set out by car, with a boat (“sunfish”) on the roof, and towing a second car. After having the carburetor rebuilt in Salt Lake City, I finally made it to San Jose, California. To minimize all the uncertainty of the move, I had put down a deposit to purchase a new home on a previous visit to the Bay Area. Since it was a new house, my wife got to pick the final options from far away in Pennsylvania—without actually seeing the finished home before we moved in.

My First Efforts at the Laboratory

VTOL noise/performance tradeoffs. I remember quite well those early days at the Ames-Army Lab, then the Ames Directorate of the U.S. Army Air Mobility R&D Laboratory (AMRDL). I shared an office with Frank Caradonna and reported to Andy Morse, Chief of the AARG, and my immediate boss. Andy reported directly to Paul Yaggy, and along with Major Gordon Berry, ran the Ames-Army Lab. The people were nice and friendly, and we were basically left alone to do research of direct interest to the Army. However, I was also called upon to represent the Army on environmental pollution issues, which were just beginning to become important to the military. I even testified on these subjects before a committee in Congress, and as a result I received my first Army Award at the laboratory.



Boeing tiltrotor ground noise predictions circa 1970.

I continued working on with my theoretical modeling of tiltrotor performance and acoustics while also continuing to collaborate with the advanced research department at Boeing Vertol. In essence, I had two bosses—Andy Morse at the laboratory and Steppy at Boeing. The research results were good enough to be presented at the International Congress of Aeronautical Sciences in Europe the following year and published as a NASA report and in Great Britain’s *Aeronautical Journal* [3]. Using the delivery of the paper as the excuse, my wife, Sheila, and I joined Steppy and his wife, Luiza, on a 3-week journey through Europe. We visited and

presented our work at the International Congress in Amsterdam, at the University of Southampton, and at the Royal Aircraft Establishment in Great Britain. We also attended the Air Show at Farnborough and gave a lecture at the Office National d’Etudes et de Recherches Aérospatiales (ONERA) in France.

It was at that time that I began to have second thoughts about the fundamentals of helicopter-noise prediction. The then available mathematical modeling did not capture the observable physics of helicopter-rotor noise. There were just too many inconsistencies. I began to look for

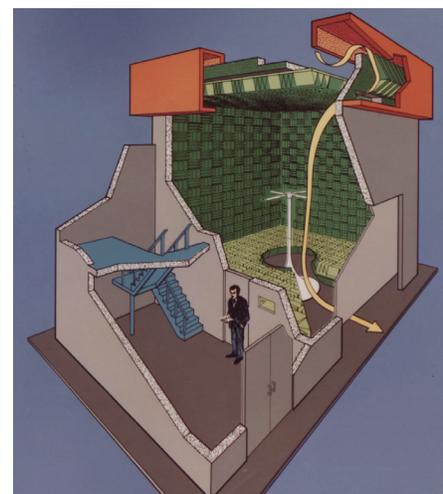
new mathematical modeling and, most importantly, new ways of measuring helicopter noise that could isolate the most important physical sources of helicopter external noise radiation.

Returning to Ames from my European trip, I discovered that a new check-out/testing facility was being developed behind Building N-215 where our Army offices were located. The project was being run by Mr. Frank Lazzeroni, a long-time NASA engineer who now worked at the Ames-Army Lab. I approached Andy Morse and Frank Lazzeroni about the possibility of turning this check-out facility into a static testing facility where hovering rotor performance and noise measurements could be made. Their initial reaction was that it was a little late in the NASA Construction of Facility (CoF) approval process, but if I could show that it could work, they were amenable to making the necessary design changes to the CoF proposal. They also agreed to support and work with me to make it happen.

The main goal was to demonstrate that flow recirculation within the chamber could be minimized and that acoustic reflections from the walls, ceiling, and floor could be mostly eliminated. I immediately began a literature search to see how big a room was needed to test rotors up to 8 feet in diameter. The general rule of thumb for rotor performance testing was that you needed to have a room size of about 10 times the diameter of the rotor (approximately 80 feet) in all dimensions. This size was needed to avoid flow recirculation problems that could adversely influence the static thrust measurements and would contaminate the acoustics of the hovering rotor. Unfortunately, the location and cost precluded a building of that size. So, a new way of testing a hovering rotor was needed—one that did not require such a large testing space.

This required a new concept for the testing area, one that could be adapted for the problem at hand and fit in the designated space that NASA had allowed for this new facility. Don Boxwell, Rande Vause, and I worked nonstop to come up with a concept and preliminary design that would meet these needs. Our unique design incorporated a moveable wedge platform and an integrated work platform. Both were hung from the ceiling and lifted by an electric motor in the work platform. I can still remember sitting on the steps of my new San Jose house as I sanded a prototype model of our unconventional design concept. That scale model demonstrated the important features of this unique design to NASA management. In addition, we proposed, and management accepted, a risk reduction effort to build a 1/10th-scale model of the aerodynamic design to ensure that flow recirculation could be controlled.

Mr. Guy Wong from NASA's Facility Engineering Branch, and a good friend of Frank Lazzeroni, was assigned the task of modifying the original design to meet the new testing requirements. He was very helpful in telling the design team what was and what was not possible—plus he was a good team player and a nice guy. The new design required a 10-foot height extension to the concrete structure with 8 large vents on the sides of the walls near the top of the building to let air enter the building (see figure on right). The inside of the building housed a diffuser/work platform that could be raised and lowered. In the lower positions, the diffuser channeled the flow out two large doors located on either end of the

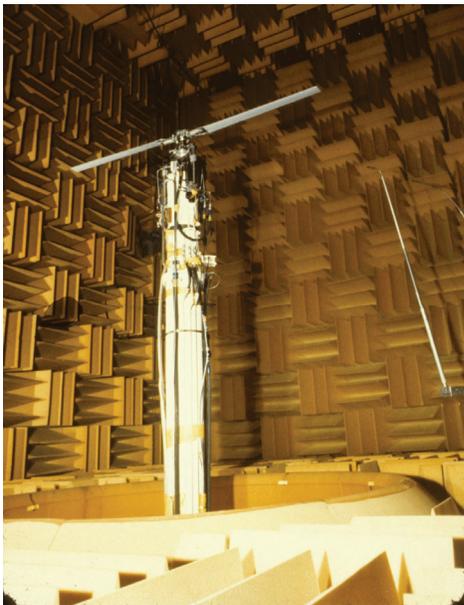


The Anechoic Hover Chamber design.

building. In the upper positions, the diffuser/platform served as a work platform for rotor build-up and servicing.

Georgene Laub took over the task of experimentally determining how to minimize flow recirculation in this new facility. Using the 1/10th-scale model, she diligently used the tools of smoke-flow visualization and thrust measurements and, with the help of an Army Science Fair winner for one summer, determined that it was possible to exhaust much of the rotor flow from this new facility.

Simultaneously with these efforts, the detailed acoustic design and fabrication contracts were released to private industry. Through Guy Wong, we were fortunate to find a crane manufacturer in the Bay Area who could build the movable platform structure. His experience building some of the amusement rides at Santa Cruz, California, was invaluable. General Acoustics, an



UH-1H rotor in the Anechoic Hover Chamber.

engineering firm in Los Angeles, California, who had previously built sound enclosures for the military, was chosen to do the acoustic work. They ran into challenges with designing a duct system that let outside air into the facility while reducing outside noises from entering the facility. This problem seemed to be intractable at first, and they were ready to default on the work. However, our in-house research team proposed a new approach of adding chevron devices inside the intake ducting. Working with the contractor, the partially constructed acoustic ducting was used as an in-situ experimental facility. When the new chevrons were added to a section of the ducting, it was experimentally shown that the hover facility would meet the desired ambient noise levels specified in the contract. Fortunately, the contractor covered most of the construction and material cost overruns for this recovery—a rare event, especially today.

The Army's Anechoic Hover Chamber at Ames became a world-class facility that was pivotal in discovering the fundamental processes of high-speed impulsive noise. Over the next few years, several seminal experiments were run that demonstrated the importance of transonic aerodynamics, and this played a key role in the development of new rotor acoustic theories.

Experimental validation of heavily loaded helicopter takeoffs. My laboratory work on optimal control methods to control noise took another turn when I met Colonel Jim Burke, an ex-Vietnam pilot stationed at the laboratory. He had learned, through a recent technical paper, that I had done theoretical work as a part of my Ph.D. thesis at Princeton investigating optimal control of heavily loaded helicopters [4]. He invited me to fly along with him at Ames in the UH-1H helicopter while he simulated a heavily loaded condition by limiting collective inputs to experimentally validate my thesis work. The optimal control strategies appeared to halve the distance required to clear a 50-foot obstacle for the simulated heavily loaded helicopter—a very satisfying result.

Because we had an Army UH-1H helicopter stationed at Ames, Jim Burke, Rande Vause, and I convinced Andy Morse that we should develop a program to fully validate the optimal control procedures for overloaded helicopters. Rande led the experiment to build a cargo plate on the floor of the UH-1H to contain lead bricks whose weight ensured that the helicopter was heavy enough to be overloaded. Jim Burke then flew the helicopter following the existing UH-1H maneuvering guide instructions (a simultaneous climb and accelerate procedure) and compared the result with the optimal control maneuver. The comparison is shown graphically below and fully validated the theoretical approach.



Fred and Rande on the steps of the UH-1H helicopter.

Having documented the approach [5], Jim Burke invited Rande and me to go on an Army tour to expose Army instructor pilots to the performance gains by flying the simulated optimal procedure in their helicopters and then showing the validated theory. The first time that we gave this presentation to instructor pilots at Crissy Field in San Francisco, California, I began to be convinced that our results had a real practical application. Rande and I won many awards for this work, and we were asked to go to Washington D.C. to present the work to General Merriman at the Army Material Command. At the end of our presentation, he commented, “You mean I have been training our pilots to fly incorrectly all those years?”

The long-term impact is also significant because the maneuver guide for the UH-1H was changed to incorporate the optimal takeoff procedures for heavily loaded helicopters. More recently, the optimal technique was incorporated in the Navy’s helicopter training documents.



a. Standard technique.



b. Optimal technique.

Optimal takeoff trajectory of a heavily loaded helicopter.

Settling in and Building an Army Program in Fundamental Acoustics Research

Acoustics research group formed. Trying to figure out the various sources of rotorcraft noise became quite important to the Army in these early years, mostly to gain knowledge that could be used to avoid aural acoustic detection. The Vietcong were detecting helicopter external noise miles before the helicopter overflew an observer—giving away the element of surprise. Paul Yaggy had hired me to form an acoustic group to perform fundamental helicopter acoustic research. The group initially consisted of Don Boxwell and Randy Vause. I remember some early discussions with my boss, Andy Morse. He had asked me what part of helicopter acoustics the new group should focus on. My answer was that I was not sure—but it was important to me that the research made a significant advancement in the helicopter acoustic field and that the research benefit the Army in a practical way. He gave me the green light to do whatever was necessary and committed laboratory support for our future adventures. I was truly fortunate to be working for Andy. He was not only a super competent engineer, but he was also very supportive of my big plans for fundamental rotorcraft research. He was like a second father to me during this part of my career.



Fred and Andy Morse.

We began our research by implementing the existing research computer codes and designing experiments to try to validate those codes. Because there was so much controversy in the prediction of helicopter noise at the time using existing codes, we decided that we would start with a clean slate—both experimentally and theoretically—to link cause and effect for the most dominant sources of helicopter noise.

I made a European trip with Steppy around this time that reinforced this view. Steppy and I had just produced a systems analysis paper reporting the findings of a study that I had begun, showing the possible performance versus noise tradeoffs of VTOL aircraft. Besides learning that reducing noise was difficult, I also recognized that noise prediction was woefully inadequate throughout the industry. The best results at that time were based on empirical methods developed from ground-based acoustic measurements [3]. While noise levels were generally well-estimated, it was not possible to reproduce the details of noise sources. This led to falsely identifying noise sources and misunderstanding how noise could be reduced. It was clear that new fundamental acoustic research was needed. It was also clear that available acoustic codes were not satisfactory for noise exposure forecasts.

Germany trip to instruct helicopter pilots “how to fly quietly.” Our Army acoustic research group started to attack the problem at a fundamental level in experimental and theoretical programs aimed at understanding the most important sources of helicopter external noise. The problem was twofold: 1) how to design and fly the helicopter to avoid aural and electronic acoustic detection; and 2) how to design and fly the helicopter to reduce noise annoyance to the surrounding communities.

The importance of the noise-annoyance piece of the problem for the Army was reinforced by a European seminar trip that I was asked to make by Army upper management. The Army was under pressure to be good neighbors to their surrounding communities to lessen the anti-war sentiment of the time. The villages around airfields in Germany were demanding that noise due to helicopter operations be reduced or eliminated. A small group of technical specialists was formed to give a summary of research findings that might be of use to operational pilots stationed at Army air bases in Germany. I was chosen to present the empirical evidence that helicopter pilots might use to “Fly Quietly.” My presentation was built around the experimental research of Charlie Cox, the chief acoustics expert at Bell Helicopter. His team had measured cabin noise on helicopters that indicated that rate of descent could affect the slapping noise heard on the ground. My presentation consisted of his charts plus some of the basic physics of sound generation and transmission. Jim Burke and I had also partially validated these cabin noise trends by flying in both the Army UH-1H and the AH-1G helicopters at NASA Ames.

Icing was also a hazard throughout Germany at certain times of the year. Richard (Dick) Lewis, the Deputy Director of the Army Experimental Flight Activity (AEFA) at Edwards Air Force Base (EAFB), was chosen to brief the latest icing experiments. The experiments consisted of flying instrumented helicopters in formation behind a helicopter carrying a spray rig to produce simulated icing conditions.

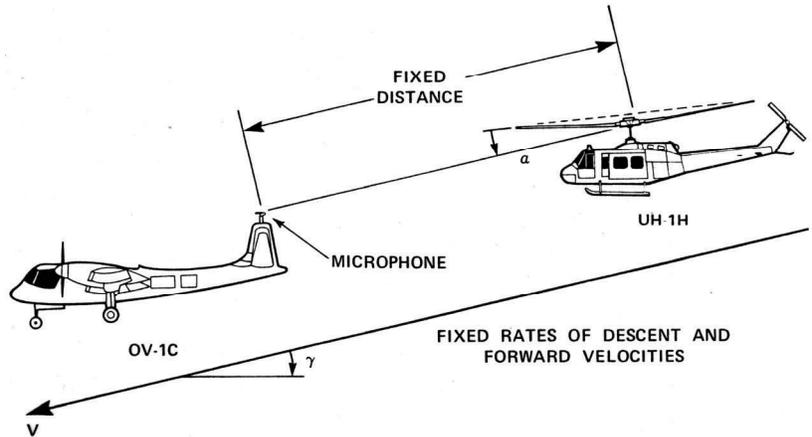
At the end of one of the briefings, I suggested to Dick that we might use formation flying experiments to try to learn more about the directivity and character of helicopter noise radiation. After several beers, we conceptually designed an in-flight noise measurement program using a relatively quiet fixed-wing measurement aircraft, the OV-1C Mohawk that Dick had access to at AEFA. It was agreed that the testing would be done using Army aircraft at EAFB, and that the Army at Ames would provide the acoustic instrumentation and hardware to acquire and reduce the performance and acoustic data.

There is nothing like an extended trip to enhance friendships. Dick and I rented a car on our days off to venture out into German towns to explore the local culture and generally have a good time. He was a history buff who always imagined being the king of a castle—while I was always amazed at how many people it took to satisfy the king. When we returned home, we began to decide how to implement the in-flight acoustic measurement program.

The in-flight acoustic measurement program. When I returned to Ames after being gone almost a month, I explained the idea of gathering external noise on a helicopter using a quiet fixed-wing aircraft to Andy Morse. He immediately supported the idea and the planned cooperation with Dick Lewis and AEFA. When the idea was presented to Irv Statler, then the new Director of Ames-Army Lab, he was quite discouraging. He refused to give me any additional resources and suggested that the laboratory was already spending too much effort on helicopter noise. He felt that little could be done to reduce noise, and that I should use my flight control and optimal control background to do research on helicopter stability and control simulation.

I was quite upset at this turn of events and felt that Irv was shortchanging my idea before giving me a chance to prove its worth. Fortunately, Andy did not concur, and he let me use the laboratory’s resources to join with AEFA to perform this exploratory program. Don Boxwell and I made several trips to AEFA to set up the program and work out the details. Bob George, a

highly qualified electronics technician at the lab, also designed some of the special instrumentation that was required. We decided to monitor the raw acoustic signal, before and after it was recorded on an oscilloscope aboard the fixed-wing aircraft, to make sure that the recorded data were of good fidelity. A special holder for the in-flight microphone was designed and placed on the center tail of the OV-1C. Station keeping was performed visually by inscribing lines on the helicopter cabin (see drawing above).



UH-1H flying in formation with OV-1C to gather first in-flight measurements of helicopter external noise radiation.

Station keeping was performed visually by inscribing lines on the helicopter cabin (see drawing above).

We submitted our work order for the attachment to hold the microphone on the top of the center vertical tail of the OV-1C, but the NASA Ames Model Shop was very busy and it looked as if we would not get the part in time for the flight test. I went over to the Model Shop close to closing time to plead my case to finish the part that very day. This was the first time that I met "Fritz," who was locking the main door on his way out. I explained that I needed the part fabricated immediately. He listened very politely to my story and then proceeded to make the part—staying until nearly 8 p.m. that evening. Of course, I thanked him profusely and added, "Please don't tell the NASA shop steward that I had asked you to work well beyond quitting time." Little did I know that he *was* the shop steward! This was the beginning of a great relationship—the beginnings of which we laughed about for many years.

The results of this flight test were truly significant. For the first time, it was possible to separate and discern the directivity of the noise from two overlapping sources: high-speed impulsive noise and blade-vortex interaction (BVI) noise. Don Boxwell prepared and presented a paper for the First European Rotorcraft Forum that quantified impulsive helicopter noise levels for the UH-1H helicopter. Because of its breakthrough nature, I was asked to present the same paper at the American Helicopter Society (AHS) Annual Forum the following year [6].

Because the data were so uncontaminated by other background noise, most of the acoustic data were presented in the time domain, not in the frequency domain; this was quite uncommon for noise research at the time. This time-domain approach allowed more accurate comparison with theory and helped isolate the sources of helicopter impulsive noise. It also led to the realization, not long after, that the measured data were being viewed incorrectly—by our group and the rest of the helicopter industry. The pre-amplifier in the acoustic instrumentation changed the sign of the viewed pulses, so the time histories were upside down.

Early wind tunnel acoustic testing. In parallel with this effort, a geometrically scaled UH-1H two-bladed model rotor was being prepared for testing in the Army 7- by 10-Foot Wind Tunnel at Ames. I designed two sets of blades that were built by the NASA Ames Model Shop (Fritz)—one that could provide smoke at the tip of the rotor for flow visualization of BVI and one that

contained some limited pressure instrumentation on the blade. The wind tunnel test section was lined with 4 inches of a compressed acoustic foam (see photo below). Both rotor sets were tested at full-scale tip speeds and advance ratios and showed that the data generally replicated levels and trends of the full-scale flight acoustic data.

Georgene Laub, Don Boxwell, Bob George, and I participated in the wind tunnel test. At one point during testing of the pressure instrumented blade, a piece of a foam panel came loose from the wall and the temperature compensation modules came loose from the rotor hub. Attachment wires and compensation modules were flung all over the wind tunnel. Fortunately, nobody was hurt and nothing was broken—nevertheless the incident was reported as an accident. Immediately, a photographer was sent to the tunnel to document the accident. This ritual was explained to me later as the gathering of photos for my future retirement party. Within 2 days, we replaced and reinforced the hold-down for the foam panel, reassembled the same temperature compensation modules, and attached them more securely to the hub. The testing proceeded without further incident and I never saw those photos again.

There were many entries of this testing rig and rotors over a 2-year period in the Army wind tunnel at Ames. Many good lessons about rotor acoustic scaling were learned, including that the microphones should be further from the rotor than the constraints of the 7- by 10-foot test section would allow, and that more acoustic treatment of the tunnel walls would be necessary to make accurate quantitative acoustic measurements.

We were also learning about mathematical modeling. The then current theory attributed the cause of the large increases in impulsive noise to increases in drag from the high tip Mach number on the advancing side of the rotor disc in high-speed forward flight. This increased drag on the advancing rotor blade was postulated to be caused by drag divergence of a two-dimensional airfoil in transonic flow. The increase in drag was acoustically modeled as unsteady drag dipoles that created the high harmonics of radiated impulsive noise. Similar increases in the higher harmonics of noise for high tip Mach number rotors were also measured in the NASA Ames 40- by 80-Foot Wind Tunnel—seemingly confirming theory and experiment. However, the measured increases in noise harmonics did not match theory at very high advancing tip Mach numbers (approaching Mach one). In addition, because of wind tunnel wall reflections, the shape of the measured noise pulse time history was not considered to be representative of helicopter noise radiation—so the shape of the pulse was not compared with drag-rise acoustic theory. It would turn out later that the shape of the pulse would be a big factor in discrediting the drag rise theory.

At about the same time, Tijdeman, a Dutch researcher, made some interesting two-dimensional airfoil measurements in a transonic wind tunnel. He was able to show that, under the right conditions, unsteady shock waves (type 3 waves) moved forward and off of the leading edge of



Don Boxwell, Bob George, and Fred preparing to test a UH-1H rotor in the acoustically treated 7- by 10-Foot Wind Tunnel.

the airfoil. This observation led several researchers to postulate that this was the mechanism for high-speed impulsive noise shock wave radiation. However, we would show later, from rotor testing in the 7- by 10-Foot Wind Tunnel, that the unsteady transonic waves of high-speed impulsive noise did not move past the leading edge of the rotor—but instead, slipped off the tip of the rotor in forward flight.

Early acoustic computational efforts. To further explore theory and experiment, Rande Vause and I wrote a computer program based on the paper “The Sound Field for Singularities in Motion,” the work of a notable acoustic researcher Martin Lowson. We assumed, as did Lowson, that the acoustic sources (thickness and force) could be modeled by a distribution of simple sources over the blade and that these rotating sources would produce waves that would add linearly at an observer location in the acoustic far-field.

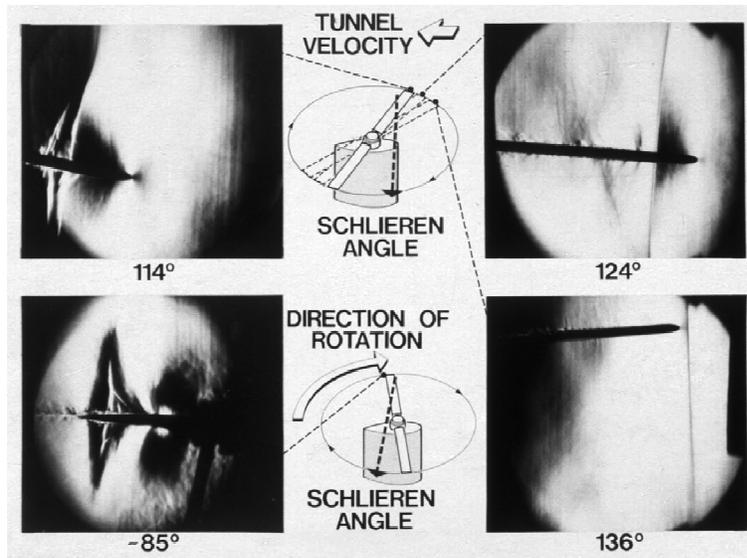
The first results, using our code to predict high-speed impulsive noise, were quite confusing. We were predicting a pulse shape that was very different from what had been measured in flight using the OV-1C aircraft. We were not even sure that the sign of the measured pulse was correct. I remember calling my friends in the helicopter industry who replied that they did not know either, because they almost never measured this noise in the time domain. Upon checking, it turned out that the Army’s microphone recording equipment inverted the time signal. Consequently, the data we had been recording using the station-keeping procedure were being viewed upside down. This was one reason that we were having difficulty comparing the theory with experiment.

However, even after correcting for the signal inversion, the shape of the high-speed impulsive noise signal was still a mystery. At the time, the increasing drag levels were still thought to be the source of the high-speed impulsive noise. Unfortunately, the pulse shapes that were predicted were not like the measured data. Frank Caradonna, who ran the transonic rotor group at the laboratory, also became interested in the high-speed rotor acoustics problem and set about developing his own computer program to predict the shape of the pulse. Working through one of his contractors, he attempted to include transonic aerodynamic drag effects in the predictions. There was a race to explain the general pulse shape of high-speed impulsive noise.

Scientists are funny when it comes to competing—they really, really hate to lose. However, they also realize that some competition really does get the juices moving. Andy Morse, our boss, also became curious and wanted to see logical, physical arguments that explained the general pulse shape.

It turns out that the explanation was quite simple (once you know the answer). We discovered that with just one mathematical source and one mathematical sink near the tip of the blade to represent the blade thickness, linear theory could produce the correct shape of the radiated acoustic pulse. The modeling of the drag rise due to transonic flow contributed little to the shape of the acoustic pulse. In addition, the peak levels of measured noise were low by a factor of about 2.

We were losing confidence in the previous theories, but we were not sure what to do next. Experimentally, we pursued the connections between drag rise and acoustic theory. A logical manifestation of the drag-rise theory was the appearance of shock waves on the airfoil surface that should propagate forward over the leading edge of the rotor on the advancing side of the rotor disk. Consequently, to validate this theoretical hypothesis, pressure transducers were placed



Schlieren photograph showing acoustic waves gathering off the tip of a UH-1H rotor in high-speed forward flight in the 7- by 10-Foot Wind Tunnel.

along the leading edge of the UH-1H model rotor. At the same time, Rande Vause set up a stroboscopic Schlieren system to capture the anticipated shock wave passing by the leading edge of the airfoil. It turns out that Rande was a professional photographer outside of work, taking many wedding photos. I picked him to take these photographs because he was the best person to do the job, and I knew that he would have fun doing it. Many weeks of testing did not reveal any evidence of such a wave. Existing linear theories did not match measurements.

After being discouraged with the lack of verification, I went back to my office for lunch and began to doodle on my large paper blotter—sketching the wave accumulation process of a point source at the tip of the rotor blade moving in an epicycloid pattern. Almost immediately, it became apparent that the high-speed nature of the waves, by themselves, might be the cause of the large measured noise levels. The pattern that emerged showed wavelets accumulating off the tip of the rotor and propagating down the wind tunnel to the acoustic far-field.

That afternoon, the Schlieren system was repositioned to capture such waves and to verify this hypothesis. We obtained the first photographs of the locally formed shock waves moving off of the rotor tip and propagating to the acoustic far-field. The results, published in a classic paper [7], were very exciting, and they significantly changed the direction and focus of the technical community. This series of experiments also showed that high-speed impulsive noise was not dependent to first order on thrust or drag. The Mach number of the advancing blade tip was the dominant parameter, with advance ratio setting the level but not the physics of wave formation process. Therefore, it was possible to study the physics of the high-speed noise process in hover at the same tip Mach numbers—the direction of research that we immediately began in our newly built Anechoic Hover Chamber.

Fortunately, under the direction of Andy Morse and with the help of Georgene Laub, it turned out that the laboratory was also designing a new rotor stand, balance, and swashplate for spinning high-tip-speed rotors in the 7- by 10-Foot Wind Tunnel. The design was modular so that it could be used for hover testing as well as in other wind tunnels throughout the world. In addition, I initiated the development of several other two-bladed model rotor-blade sets that could be tested in hover at high tip Mach numbers—emphasizing the UH-1 and AH-1G helicopters. Some of these rotors were fitted with pressure transducers, but most were not. The goal was to experimentally explore the effect of thickness, twist, aspect ratio, and sweep. They were, however, designed to be run at very high hover tip Mach numbers so we could explore the wave process that was responsible for high-speed impulsive noise.

Army/FBI YO-3A acoustic flight testing. In-flight acoustic testing also took a leap forward thanks to my wife, Sheila. She was reading the *Parade* magazine insert for the Sunday edition of the San Jose Mercury News and spotted an article on the Federal Bureau of Investigation (FBI) using a quiet airplane to capture criminals in the Los Angeles area. There were only about a dozen of these aircraft built for surveillance in Vietnam, and they were now being offered to various government agencies for other activities. I realized that these aircraft could be extremely useful because they were designed to be quiet—thus dramatically reducing the background noise of the in-flight station-keeping method of measuring noise.

My first action was to contact the person at the FBI who was identified in the *Parade* magazine article, FBI agent Vance Duffy. It only took two phone calls for me to speak with him and arrange a viewing of his YO-3A aircraft. When Bob George and I arrived at the Los Angeles airport at our arranged meeting time, Vance was nowhere to be seen. However, we heard the high-pitch sirens of police cars and screeching tires in the distance. Vance finally arrived about an hour late and explained that he was involved in chasing a mob figure of interest, but lost him in traffic. Thus began our great relationship with Vance Duffy and the FBI.

Vance drove us to the Oxnard Air Field outside of the Los Angeles area. After looking over the aircraft, Vance took me for a ride to demonstrate its quiet nature. I remember flying over some people picking crops in the field next to the airport at about 200 feet above ground level and noticing that they did not look up until our shadow passed over them. I was convinced that this aircraft would be a great platform for in-flight acoustic measurements. Bob George was also offered a demonstration flight, but he declined. He explained later that the aircraft looked unsafe. I tried to assure him that it just looked dirty because of its faded camouflage paint, but he would not take the chance.

After asking if we could use the FBI's YO-3A aircraft to measure noise and getting a generally affirmative answer, we decided to go to our respective bosses to get approval and to talk to AEFA (Dick Lewis) for approval to test and support the UH-1H and AH-1G helicopters. Vance also agreed to train an Army pilot in the YO-3A so that Vance could continue to carry out his duties as an FBI agent. It turned out that the aircraft had some squirrely tendencies near stall, so the Army pilot never got confident flying the aircraft in a station-keeping mode. Consequently, I asked Vance's boss at the FBI if he would assign Vance to the two testing programs so that he could fly the YO-3A, and he agreed.

Vance only wanted one favor in return for all these requests. He wanted to get the YO-3A a new paint job so that, upon finishing his work for us, he could hide better at night in the Los Angeles skies. So once Vance flew the aircraft to NASA Ames, I arranged to have the contractor at the NASA Ames Paint Shop paint the aircraft white. My acoustic team, Don Boxwell, Bob George, and I, also installed the acoustic measurement system and three microphones on the FBI aircraft—one on top of the tail, and one at each wingtip (see photo on next page). Vance and I then flew the YO-3A aircraft to AEFA.



FBI YO-3A aircraft before repainting.

This research program was unique in many ways. Because of the Army-NASA Joint Agreement, we were able to use the NASA shops to make brackets and design and build necessary microphone holders. Safety of flight was the FBI's responsibility, and Vance was the committee of one who had final approval of just about everything associated with the YO-3A. All the changes were very fast, and in less than 2 weeks we headed to EAFB for flight testing at AEFA.



The FBI YO-3A aircraft after repainting and acoustic modifications.

It was also a blur as to who actually gave me, and our team, approval for all of this. I was in close contact with Andy Morse, my immediate boss, who wholeheartedly supported our efforts. However, I was never sure that his boss, Irv Statler, was even aware of my efforts. Because Irv originally opposed the in-flight method, I kept away from him for fear he would stop the program. I was hoping that Andy would keep Irv uninformed until the program was over. In hindsight, I believe that Irv really did generally know about program through Andy; apparently, he chose not to interfere.



YO-3A aircraft gathering acoustic data while flying in formation with the AH-1G helicopter.

Two major flight test campaigns were performed at AEFA using the FBI YO-3A with the UH-1H and AH-1G helicopters, each lasting about 3 weeks. The UH-1H acoustic data explored BVI from 60 to 100 knots airspeed and many rates of sink. The sensitivity of rate of sink to radiated BVI noise directivity and level was conclusively documented, helping to understand UH-1H BVI noise [8].

The AH-1G helicopter test was performed using three full-scale-rotor sets: the standard blades, the Improved Main Rotor Blades (IMRB), and a new blade set designed by John Ward of NASA that incorporated an “Ogive”

tip shape [8]. The results proved that tip shapes could reduce BVI noise levels by about 6 dBA, and “halve the acoustic energy.” The acoustic time histories indicated that spreading out the vortex by reducing the chord (reducing the gradient of the circulation in the tip region) reduced BVI noise levels.

In another rare opportunity, the FBI YO-3A was used to support the acoustic evaluations of the Army's Source Selection and Evaluation Boards (SSEBs) for the Utility Tactical Transport Aircraft System (UTTAS) and Advanced Attack Helicopters (AAHs). Four more helicopters were acoustically tested in a very short period of time and reported to the SSEBs as input for the final selection process. This is discussed in more detail later in this chapter.

At this point, it was clear that in-flight acoustic testing of helicopters was an extremely useful scientific technique. It was also clear that relying on the FBI YO-3A aircraft for future test programs was far from ideal. I remember identifying another FBI aircraft that was already in use by NASA for propeller noise testing at Ohio State University on a program that was coming to an end. I suggested to Len Roberts, Director of Aeronautics, that NASA transfer this aircraft to Ames for future joint Army/NASA helicopter noise testing. NASA transferred their aircraft to Ames and completely refurbished their YO-3A under the direction of the Research Aircraft Projects Office. The aircraft was used by NASA to measure noise on several more rotorcraft for the next 15-plus years.

The seminal in-flight acoustic data taken on the AH-1G rotor turned out to be extremely useful for future programs. Several other research programs were underway by the Army and NASA to explore the vehicle performance parameters associated with blade design—including a full-scale fully pressure instrumented AH-1G blade set to be flown at NASA Ames. Anticipating future wind tunnel testing of scaled rotor blades, I had Fritz build sets of 1/7th-scale AH-1G model rotor blades, including a set of model-scale pressure-instrumented AH-1G blades to be tested in wind tunnels.

A great real world opportunity. In the spring of 1975, an unusual opportunity was offered to the engineering staff at the Ames-Army Lab. The Army wanted to expose the helicopter design and operational problems to the laboratory engineers by having the engineers visit the helicopter units in the field. This usually meant having the chosen personnel stay at a nearby motel and drive out to the exercise each day to observe. The planned exercise was a large one, involving about 10,000 troops. It was a simulation exercise to show how to fight in the desert. The troops were split into friendly and enemy sides. I originally declined the offer but then, after some further thought, I decided to talk with the Colonel in charge of the unit that I was to visit. It turned out that Colonel Buzz Snyder of Company A of the 9th Aviation Battalion and I had met before at Ames. He also had doubts about the usefulness of exposing laboratory engineers to the realities of operations and tactics through daily visits to select operations. We both realized that to be useful to the engineering community and not disrupt the planned operations, I really had to be an integral part of the operation. This meant staying with the troops in the field for the entire operation.

I agreed to this new and different experience and was issued an Army uniform without a rank. I flew to Ft. Lewis near Seattle, Washington, to begin my in-situ experience with an Army helicopter assault unit. When I was leaving for this adventure, which no other engineer at the laboratory wanted, I explained to my wife that I was making this trip because I could learn, firsthand, what real Army helicopter problems needed to be solved. She replied that I was really “going to a boys’ camp and probably would have a lot of fun.” She was probably 60 percent correct.

At Ft. Lewis I met with Colonel Buzz Snyder, who assigned me to 9th Aviation Battalion, 9th Aviation Division. I was to work with Major Robert (Bob) Frix, who led Company A. At the beginning of this exercise, Bob and I were both apprehensive about its worth, but we were both willing to give it a chance.

I stayed at Ft. Lewis for about 5 days and then boarded a C-5 transport to fly (with the helicopters) to the New Mexico desert for operation “Gallant Shield.” The simulated battle area was quite large with a high desert surrounded by mountain terrain.

I have too many fond memories from this time in the field to relate here, but the experience was one of the best of my career. I became a close friend with Bob, moving into his command tent in the field. I also became close to many of the pilots and flew on many sorties—either in the front seat of the AH-1G Cobra, nicknamed the “Snake,” or in the UH-1H transport helicopter. I frequently sat alongside the pilot in the UH-1H and flew the missions once the aircraft was underway. In several instances, Bob made his leadership decisions based partially on my observations and input. At the end of my tour of duty, I wrote a detailed memo of my experiences that received accolades from Army management. However, the compliment that I cherish the most was being made an honorary Brevet W1 Warrant Officer of Aviation with full membership in Company A. I was also invited to return to Yakima, Washington, to fire the guns on the Cobra—another weeklong experience that taught me much about Army aviation.

This 8-week break in my research agenda was very helpful on many fronts. It made me realize that, like the takeoff problem, it was important to identify the real problem or problems that you are trying to solve before you dive into the details. My experience flying in the desert and listening to the sounds of helicopters on real sorties helped me focus on the major sources of helicopter noise. It also made me realize that linear acoustic theory might not accurately predict high-speed impulsive noise. This was reinforced by a paper in the *Journal of Sound and Vibration* written by Martin Lowson of Westland Helicopters, which I took with me to the desert, that predicted rotor noise for rotors operating with advancing tip Mach numbers above Mach one. The experimental findings that we were measuring did not match either the trends or the level of his predictions using linear theory. When I returned to the laboratory, I began to focus on including the effects of transonic flow in acoustic prediction methods. The acoustic theory was enhanced to include quadrupole terms in the classical prediction methods.

Another rare opportunity. The tremendous freedom that we were offered in the early days at the Ames-Army Lab came at a price. It was expected that, when needed, I would be available to serve as an acoustics expert on Army programs. In 1976, I was notified that I was to serve as an acoustic expert on the SSEBs for the UTTAS and AAHs. After some discussion, it was also decided that my role was to be an Army consultant, located at Ames, traveling to St. Louis, Missouri, on an as-needed basis.

During my first 2-week visit, I remember meeting with Charlie Crawford and Dick Lewis who gave me instructions to evaluate the UTTAS acoustic properties based on Sikorsky and Boeing document submittals. I told them that it was impossible to do an evaluation because quantitative measurements did not exist and theory was not verifiable. When they asked for suggestions on how to proceed, I proposed that we use the very successful in-flight station-keeping method of measuring the noise using the YO-3A aircraft as the basis for this evaluation. The idea was accepted by the General running the program, provided that the measurements could be made and documented before the evaluation period was completed in about 3 months. The General gave the testing his full backing as did Vance’s FBI boss.

Andy Morse and I now had to gather up the resources to make this testing happen in a very short time period. It turned out the FBI YO-3A needed a new Lycoming engine—a backlogged item.

The program also needed someone to take out the old engine and install the new one. I can remember the government bureaucrats thinking the situation was almost impossible. When we got negative answers about possible schedules, we kept asking who that person's boss was, and were then fast-tracked to meet the "impossible schedule." The Lycoming engine was delivered about 1 week after it was requested. Vance installed the engine that weekend.

The SSEB for the AAH was to convene immediately after the UTTAS board. They were also in the same situation—having little measured acoustic data on the prototype configurations. It was decided to use the in-flight station-keeping method on those prototypes as well. A classified report containing the in-flight measurements plus ground acoustic measurement was produced for each SSEB and used to evaluate the noise for each SSEB.

Besides providing measured acoustics data for both competitions, our research group was able to see the noise characteristics and design implications for these future helicopters. This information was invaluable for planning the fundamental acoustic research program at the Army laboratories for the next decade.

Rotorcraft training and Stanford University. The Ames-Army Lab always tried to build good relationships with universities, especially local ones. This was difficult during the Vietnam War because of on-campus demonstrations and what the protestors claimed were too-close relations between academia and the military. Nevertheless, Paul Yaggy thought it was important to keep those ties alive and forged basic research ties with Stanford during this period. In particular, I worked with Professor Krish Karamcheti on basic acoustics and Professor Art Bryson, the head of the Aerospace Engineering Department at Stanford, on optimal guidance and control, mentoring several students at the university. Dick Carlson, who worked at Lockheed in Southern California and later joined the Army organization, also taught a beginning helicopter course at Stanford. He commuted once a week for this quarter-long course.

One day in 1975, I got a phone call from Professor Bryson, asking if I would consider replacing Dick as the instructor for his course. I immediately went to Dick to see if he had any objection to this arrangement. At this time, Dick Carlson had become the Director of AMRDL, replacing Paul Yaggy after his retirement in 1974. Dick indicated that he was just too busy in his new job to continue teaching at Stanford and gave me his wholehearted support. I was very excited about the idea of teaching graduate school as it had always been one of my lifetime career goals. The laboratory was also pleased because it fostered closer ties between Stanford, the Army Lab, and NASA Ames.

After some conversations with Art Bryson, we decided to change the emphasis of the class to the Basic Aerodynamics of Windmills and Helicopters, a somewhat political decision for the times, and to teach the course twice per week at Stanford with the lectures televised to local industry and NASA Ames. About 25 people registered for this new course; in subsequent years, class attendance grew to almost 40 students. It also became obvious that too much was packed into this one class. A second quarter-long class was introduced on the stability and control of rotorcraft—making a two-quarter sequence of classes on rotary-wing vehicles. In this second class I included a 2-day laboratory to help the students build their own rubber-band-powered helicopters—bringing the practical and theoretical aspects together. The idea of using the rubber-band helicopter in the rotorcraft class came after talking with Bob Ormiston who had built his own rubber-band-powered helicopter from plans developed by John Burkham, an avid modeler



Stanford rubber-band-powered helicopter fly-off; Stanford and NASA students and judges.

working at Boeing Vertol. I employed my two older sons, Fred and Eric, to make and assemble key parts of the “John Burkham” rubber-band helicopter design into a kit, which they sold to the Stanford students. A fly-off contest was held toward the end of the class, with prizes for the best flights and designs. The second class, like the first, was an instant success.

After a couple of years, I realized that I was spending too much time on the Stanford classes to be effective with my research work at the Ames-Army Lab. I went to Art Bryson again and I proposed that we involve other researchers at the Army and NASA to help teach these classes. He agreed with the stipulation that I monitor the class to ensure quality.

Within this framework, the sequence expanded to five different VTOL courses: Dynamics (taught by Dr. Dewey Hodges), Advanced Rotorcraft Aerodynamics and Noise (taught by Dr. Yung Yu), VTOL Handling Qualities (taught by Dr. Jack Franklin), Stability and Control (taught by Dr. Victor Lebacqz), and the first performance class, which I continued to teach.

This arrangement continued until 1981 and gave a rotorcraft education to about 250 students. Some of the students already worked at the Ames-Army Lab and at NASA Ames, and others were hired after receiving their degree. I was fortunate to have great management support for these efforts—especially from my bosses Andy Morse and Irv Statler, as well as Bill Warmbrodt and Bill Snyder, of NASA. I was also fortunate to have the support of Art Bryson at Stanford who gave me the freedom to make it happen.

Yung Yu becomes a member of the research group. Rande Vause always claimed “not to be an engineer” (see his chapter). I personally thought that he was an excellent engineer with many talents. However, his real interests were not in solving fundamental research problems. He was a “big picture guy” who wanted to advance into management in the long term. Realizing this, we worked together on tasks that used his modeling and computer skills while he took classes at Stanford to obtain a Master’s degree in Engineering Management.

Around 1977, Rande took a job with the Advanced Systems Research Office (ASRO) at the headquarters of the recently formed Army Aviation Research and Technology Laboratories (RTL). Anticipating this loss, I hired Dr. Yung Yu, a Stanford graduate who had been working in the 40- by 80-Foot Wind Tunnel as a National Research Council Post-Doc. He received a doctorate in theoretical acoustics working under Professor Krish Karamcheti. I was quite familiar with his work at Stanford, and I was sure that he could pick up and advance the theoretical work that Rande had begun.

Yung's appreciation for experimental work was lacking, mostly because he had never done that kind of research before. I felt that it was important that he acquire some experience with experimental research to help guide his theoretical research efforts. Because I was shorthanded during our SSEB testing at AEFA, Yung and I decided that he would take on the task of helping NASA photographer Lee Jones set up and take ground photos of the SSEB testing. Yung was helpful, but the equipment was quite heavy. As a result, his efforts with the group were characterized as the time Yung went to "Muscle School."

Helicopter flight training. I always believed that becoming thoroughly familiar with the problems at hand was important in finding the potential solution to those problems. Because I had received several awards for my optimal control and acoustic research, my local management asked me what next steps the Army could take to further my career. My answer was consistent: "Send me to the Army helicopter flight training at Ft. Rucker to become trained as a helicopter pilot." I had already received a private pilot's license while I was attending Princeton and had been flying locally through a Navy flying club at Ames. After renting a Cessna 182 and flying my family cross-country to the outskirts of New York City and back, I had accumulated over 100 hours of flight time.

The first few times that I stated this career objective, I was told that it was not possible. Finally, the local management gave me permission to apply to the Army flight-training program at Ft. Rucker—but my application was denied by the Army Aviation R&D Command in St. Louis. After some management changes, I was encouraged to apply again. This time my application made it all the way up to Washington to the Army person responsible for filling the billets for helicopter training. Admittedly stating that I was qualified for this flight training, he still turned down my application because he did not want to set a precedent. Upon hearing these results, the Army management team suggested that I become a helicopter pilot locally—through a commercial helicopter training company. I pounced on the idea.

I immediately visited three of the helicopter training centers in the Bay Area. It was then that I met Steve and Nancy Sullivan who owned and operated ARIS Helicopters out of San Jose Municipal Airport. In my first meeting with Steve, he opened his desk drawer and pulled out my paper, *Optimal Helicopter Takeoffs of Heavily Loaded Helicopters*. As soon as we started talking about my paper, I realized that he was the best person to teach me how to fly helicopters. He was not only an excellent pilot and the only person in the Bay Area authorized to give check rides to California State Police helicopter pilots performing auto-rotations to the ground, but he was up to date with the technical literature. Because I already had enough fixed-wing flight time, I obtained a commercial helicopter license in about a year's time. Steve and I also became good friends for many years to come.

Irv really had different long-term plans for me at the Ames-Army Lab. He thought that I could best serve the laboratory's future research efforts if I returned to my flight control and simulation expertise. I was not sure that I was completely sold on this new research direction. However, because he supported my flight training efforts, I agreed to begin to redirect my research efforts once we completed the efforts in acoustics that we were committed to under the German and French Memoranda of Understanding (MOUs). In the meantime, I remained focused on acoustics research.

Fundamental Acoustics Research

Quadrupoles and radiated noise. As I described earlier, I hired Dr. Yung Yu to further explore the theoretical approach we had been using and to add nonlinear effects by including quadrupoles in an expanded formulation. A paper on this work had been promised for the Third European Rotorcraft and Powered Lift Forum in September of 1977. The first part of the paper built the case for using linear theory at low advancing tip Mach numbers and physically showed the sensitivity of the linear modeling to parametric variations. However, in our rush to meet a publication deadline, we mistakenly interpreted a perturbation velocity to be the hover velocity of the rotor, causing a major error in the estimation of the nonlinear quadrupole terms. The error was quickly fixed and an erratum was sent to everyone who attended the meeting. Unfortunately, the revised paper [9] showed that including the quadrupole contributions only marginally improved the prediction of high-speed impulsive noise. We were still not correctly modeling the radiated noise. We found out later that our predictions did not include the three-dimensional (3-D) transonic aerodynamics in the tip region and the quadrupole contributions in the regions off the tip of the blade.

The Army was also fortunate to be working with a top fluid mechanics mathematician, Professor Morris Isom, under a long-term contract with New York University (NYU). Frank Caradonna and Morris had been the first researchers to apply small potential transonic theory to helicopter rotor problems (see Frank Caradonna's chapter). After Morris, Frank, and I had several discussions concerning high-speed impulsive noise, Morris agreed to expand his theoretical work to look at the high-tip-Mach-number noise radiation. After much encouragement, Morris agreed to join the Army research group at Ames and give up his tenure at NYU. However, Morris was really in an indecisive mode after he had made his decision. After flying out to begin work, and landing at San Francisco International Airport, he had convinced himself that his decision was a bad idea. So, he changed his mind, quit his new job at the Army, and got back on an airplane to return to New York. As Irv Statler said after Morris left for home, "That was the shortest hire that I have ever made."

Even though he did not join our group, Morris began to look deeply at the hovering rotor problem. He spent several summers in California and gave many interesting lectures; we had many lively discussions. The neat characteristic that Morris had was that he never took these often contentious discussions personally. After literally yelling at each other about some technical point in dispute, we could all walk away as good friends. In several instances, we went to dinner to discuss technical points, chose to have too much wine, and aggressively make points that we had not thought out too well. The next day, we would go to work with a good appreciation of each other's point and not hold a grudge. It was good fun and technically stimulating.

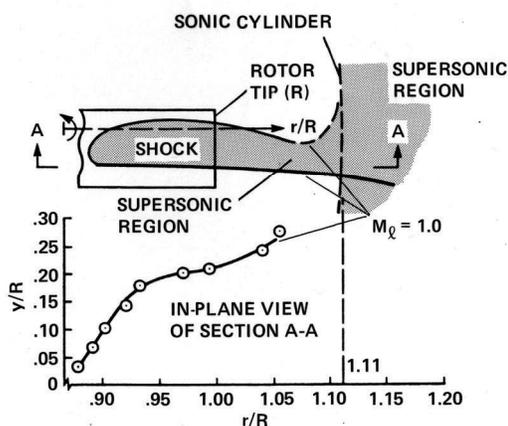
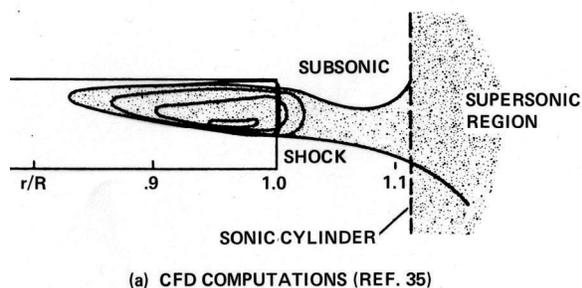
First national meeting on helicopter noise. In 1978, the U.S. Government was tasked with setting commercial helicopter noise certification levels and had several national meetings on the subject, involving industry, NASA, the U.S. Army, and universities. I remember one of those meetings in particular, a technical conference at NASA Langley. Every participant was invited to present a paper on their knowledge of helicopter noise. Andy Morse, Don Boxwell, and I attended and presented our latest findings on high-speed impulsive noise of a hovering rotor operating at high tip Mach numbers. We also presented theoretical calculations showing that, at high tip Mach numbers, linear theory could not match the experimental results for either the shape or the level of the pulse [10]. Dr. Ferri Farassat, of NASA Langley, furiously attacked our theoretical results, claiming that we did not accurately model the linear terms in the theory and that, if we had correctly done so, our theory and experiment would match the shape and level of the measured data. His challenge caused quite a stir at the time and made it difficult to cooperate with him for many years. He also promised to redo our calculations using more accurate linear modeling, and compare his results with our experiments in a future paper.

To his credit, Ferri kept his word and recomputed the linear calculations using what he thought to be more accurate modeling. When he compared the linear theory with our experimental hover data, his theoretical results were the same as those we had shown a year earlier. However, he did not present our theoretical results alongside his, but chose instead to present only his theoretical results against our experimental results from our previous paper. His action did not foster promising future collaborations.

More extensive theory/experimental research. I returned to Ames more convinced than ever that linear theory was inadequate. Up until this time, we had been using Frank Caradonna's computational fluid dynamics (CFD) code to predict events near the tip of the rotor. While we had surface blade pressures in this area, we did not have a detailed experimental map of the aerodynamics off the blade, especially near the blade tip. This field decisively controlled the nonlinear quadrupole terms of the theory. I decided that we could measure this field quite easily if we built a traverse that was hung from the ceiling of the Army's Anechoic Hover Chamber. A hot-wire velocity probe was attached to the probe tip to measure the stationary perturbation velocity field near the tip of the rotor. I designed this off-the-blade surface-measurement system, put it out under contract, and had the system delivered to test in the Anechoic Chamber 1 year later.

We began this new test program by repeating the previous noise measurement test matrix that we had presented at the Langley meeting. We invited Frank Caradonna to join the testing to measure the perturbation velocity field surrounding the blade tip using the new spatial survey rig. A hot wire measured this perturbation velocity field while far-field microphones measured the radiated noise. I designed new aerodynamically scaled UH-1H main rotor blades that were built for high-tip-speed operation in hover. Once again, they were fabricated by the NASA Ames Model Shop, and run at tip speeds exceeding Mach one.

Our results confirmed what we had seen in the wind tunnel using Schlieren methods. As the rotor tip Mach number was increased, there was a critical Mach number where the shape of the radiating noise changed drastically. The hot wire showed that this transformation was due to local shocks on the blade that, at a critical subsonic tip Mach number, formed a sonic-boom type of wave that slipped off the rotor tip and traveled to the acoustic far-field. Upon seeing this for



Delocalized flow field of high-speed noise.

the first time, Frank Caradonna named this process “delocalization,” a label that succinctly described the transformation process [11].

Using these measured data of the transonic velocity field, Yung Yu improved the correlation of the quadrupole theory with the experimental and theoretical results, and we reported our study at the European Rotorcraft Forum that year [11]. However, the theory required an evaluation of the quadrupole field beyond the tip of the rotor—something we were not able to do at that time. Off the tip of the rotor, the calculation required that we evaluate the product of a diminishing perturbation velocity field and a singularity.

In our next paper [12], we found an engineering solution to this “almost zero times infinity problem” and showed that we could markedly improve quadrupole acoustic theory, but only slightly beyond the “delocalization” Mach number. At values higher than the delocalization Mach number, the predicted

pulse shapes and levels still could not match measured data very well.

Our team worked on improving the quadrupole theory for about 2 more years and realized that there were several more challenges for this approach that we might not be able to overcome. I discussed this situation with Dave Hawkings of Westland Helicopters in Great Britain, who was trying to predict high-speed impulsive noise using a Kirchoff approach. He also had calculation difficulties but because of different challenges. His approach required the prediction or measurement of perturbation quantities on the boundaries of computation surfaces that were significantly detached from the rotor. At that time, predicting this field accurately was a CFD challenge and measuring this field was overwhelming. However, we also realized that off-surface methods might eventually become feasible, given the great potential progress of CFD. All we had to do was be patient while more accurate CFD methods were developed.

Word was getting out that our laboratory was developing new methods of predicting and validating harmonic noise radiation from helicopters. Irv Statler was becoming more supportive of our work and suggested that it might be a good idea to share this ability with industry through a short course hosted at the laboratory. Yung Yu and I took up the challenge, and an invitation was issued for a 1-week course at Ames. All major manufacturers sent their acoustics experts, including additional representatives from NASA. A photo of those attending the first Army External Helicopter Noise Class is shown on the following page. Yung and I gave lectures, and we also set up computers for the attendees to do homework so that the knowledge would be reinforced. In particular, I insisted that each attendee write their own Fortran code to predict thickness noise. The course was very successful. In fact, as he was retiring from Sikorsky,



Members of the first Army External Helicopter Noise Class led by Fred and Yung.

Phil LeMasier told me that he still relied on the code he wrote for the class to predict thickness noise on Sikorsky helicopters.

Three-dimensional visualization of high-speed rotor noise in hover. Efforts to improve our visualization of high-speed rotor noise were led by Yung Yu. The new idea was to use laser interferometry to capture the integrated effect of the shock-dominated flow field at several observation angles. Because the rotor was operating in hover, several of these interferograms could be taken at different azimuth angles. Using computer tomographic methods that were commonly used in medicine, a 3-D picture of the waves emanating from the tip of the rotor could be developed. Although it was difficult to make these measurements, Yung, working with a Stanford professor, did produce 3-D images of the compressible flow field off the tip of the high-speed rotor in hover. An interim step in this procedure was the development of a hologram containing the integrated information at one observation angle, where the local shock structure could be viewed by pointing a laser at the measured hologram. It took a careful eye and some tuning, but the integrated shock structure was clearly seen.

Every so often, important visitors were treated to laboratory demonstrations to highlight the scientific prowess of the Ames-Army Lab. On one such occasion, Major General Story Stevens, commander of the Army Aviation R&D Command (AVRADCOM), St. Louis, and Dr. Dick Carlson, Director of RTL, were invited to observe this experimental laser reproduction technique. A hologram was set up to view the flow field of a high-tip-speed rotor and activated by the laser. Yung and I worked to carefully set up the hologram and thought we had everything tuned just right. To see the reproduced wave field, one observer at a time had to look at the flow field at just the right angle.

We first asked Dick Carlson to view the compressible flow field and asked if he could see the complex shock structure. He enthusiastically replied that he could, and we then asked General Stevens to view the same hologram. General Stevens enthusiastically replied that he could also see the shock structure. Yung and I were quite proud of this complex demonstration. However, upon leaving the demonstration area and out of the normal hearing range of our group, I overheard General Stevens ask Dick Carlson if he had really seen the shock structure. His reply was, "No, did you?" General Stevens answered negatively as well. After they left the building, we checked the setup ourselves and realized that no real images were visible.

I realized at this point what a supportive management group we worked for, all the way up the line. Their enthusiasm and appreciation for our work was terrific, even to the point of convincing themselves that we had shown a technical result that was not there.

Many additional acoustic tests were run in the Army's Anechoic Hover Chamber, helping uncover the physics of the delocalization phenomenon. However, it was becoming clear that our experimental acoustics program should return to measurement of rotor sound fields in forward flight. It was also clear that we should refocus our efforts to the Army's acoustic problem of helicopter detection.

Army low-frequency detection of helicopters. The importance of acoustic detection to the Army was never questioned. However, because the helicopter needs to support its weight by forcing the air in an unsteady manner, it radiates a considerable amount of noise to an observer on the ground. Much of this is low-frequency periodic noise that can radiate far from the helicopter. This eliminates the element of surprise and can be used by an adversary for Identification Friend or Foe (IFF). For obvious reasons, much of this research was classified at the Secret level or higher.

With the help of Wayne Mosher, I organized a classified acoustics conference at Ames and invited all of the Army agencies who were involved or who sponsored work in this area. At this conference, I discovered that the U.S. Army Tank Command in Detroit, Michigan, was also interested in the acoustic signature of helicopters. However, their objective was to find and destroy enemy helicopters before they could destroy the tank. Realizing that we were involved with the same physics, Wayne and I went to Detroit to meet Don Reese, the head of the Tank Command's efforts in this area. At that meeting we decided that working together made technical sense. Don and I agreed to share our technical contacts and resources toward a better understanding of helicopter noise. He also suggested that I meet with Dr. Sanford "Sandy" Fidell, a Bolt, Beranek and Newman, Inc. contractor, who was an expert in the subjective aspects of helicopter noise.

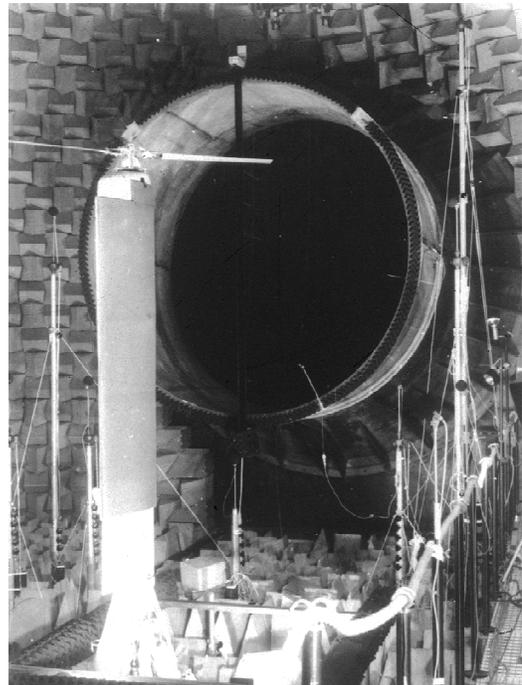
Working with Don Reese was a pleasure. To support our measurement efforts, Don arranged to have two surplus army trailers made available to RTL providing a classified mobile laboratory to gather, process, and report on acoustic programs for the Army. Using the money from the Aerodynamic and Acoustic Test Model Rotor (AATMR) program (to be described later), Don Boxwell purchased acoustic measurement and analysis equipment for this movable classified laboratory. I had written many papers and reports with Don Boxwell in the acoustics area. He was dependable, and one of the most meticulous and thorough persons that I ever met. In effect, he would become the laboratory's classified expert for acoustics for RTL.

At the same time that I was developing this program, I also had the idea for several secret patents that would make it harder to locate and identify helicopters. Fortunately, I had the help of Darrell Brekke, the patent attorney at Ames, who was well connected in NASA. He enthusiastically pursued my patent efforts, and we were successful in all of our submissions. One patent effort stands out because I remember briefing it at higher levels in the Army. To clearly explain the basic physics, I had built a specially designed helicopter out of *Lego* pieces and brought it with me to the briefings. My youngest son, Kent, then about 10 years old, had helped construct the model that explained the basic principle of the invention.

Because we had such good cooperation across the Army, I suggested that it would be beneficial to know if the Air Force and Navy had similar programs underway in the field of helicopter acoustic detection. I was fortunate to get an endorsement from a General who oversaw this area for the Army, who then wrote a “letter of introduction” to help us initiate a conversation about some of these common classified programs. With the help of Sandy Fidell, I contacted key Navy and Air Force people working on classified helicopter detection programs. Sandy and I then went on a tour to several of these classified efforts, but met with considerable reluctance to share technical information—even with our “certified letter of introduction” from the Army. However, we learned that the work that was underway was quite similar to the Army’s efforts in rotorcraft acoustic detection.

Acoustic measurement under the French MOU. The U.S.-France MOU for a Cooperative Research Project in Helicopter Dynamics was signed in late 1971, permitting research scientists from the Ames-Army Lab and ONERA to collaborate on fundamental research in helicopter aeromechanics. As a part of this MOU, a personnel exchange program was set up where researchers from each organization were sent to the other’s institution for a specified period of time. Dr. Jim McCroskey worked in France at ONERA as the first researcher on this exchange, followed by key researchers from each organization.

Originally, I was not too keen on participating in this exchange because I did not see how the Army would benefit technically from it. Our acoustics research group was actively involved with an exciting acoustic program in the U.S., with participation by industry, NASA, and some universities. I felt that we did not need to extend the basic program internationally. However, Irv Statler thought otherwise; he believed that having me spend time abroad on an acoustic task would help strengthen the MOU and acoustics research. For personal reasons, my wife was lobbying for an extended stay in Paris as well.



Helicopter noise measurements in the French CEPRA-19 wind tunnel.

After presenting a paper outlining our experimental results at a European Rotorcraft Forum, I had a good discussion with Dr. Claude Dahan, a rotorcraft acoustics specialist at ONERA, about our work. We agreed that we could extend our experimental scale-model research to forward flight by finding a suitable anechoic wind tunnel. It turned out that the French had a new large propulsion acoustic facility, the CEPRA-19, that might be used to make helicopter acoustic measurements. However, no rotor far-field acoustic measurements had ever been attempted in this facility. This new cooperative program, if approved as a new task under the U.S.-France MOU, would be the first.

We brought the idea of taking a scaled-model rotor to test in the CEPRA-19 wind tunnel to our respective managements. After several technical discussions, we agreed to test several rotors

using the Army's new rotor test stand to explore BVI impulsive noise. Claude would represent the ONERA (French) side and would provide the operation of the CEPRA-19 tunnel, the microphone stands, and some local instrumentation assistance. The Ames-Army Lab would provide AH-1G rotors (both instrumented and un-instrumented), the new Army rotor test stand, and the microphones and their recording devices. Aérospatiale Helicopter would provide a four-bladed rotor set and a hub for additional testing on the Army rotor stand in the CEPRA-19 facility. The test, including set up and tear down, would last 6 months.

Because the tunnel was located on the outskirts of Paris, my wife was looking forward to this experience. I was "allowed" to temporarily move our family to the Paris region. (At that point our three boys were 5, 10, and 12 years old). We kept our boys in U.S. public schools until February when they flew to join us at our apartment in Meudon-la-Forêt, a town just outside of Paris. Olivier Lambert, the French government's liaison and good friend, did an outstanding job helping us to relocate our family during this period (1981). Our boys were enrolled in French public schools and also did U.S. homework assigned by the Palo Alto schools to keep up with their classes back home. While they mostly had a good time, they were normal boys who sometimes got into regrettable situations—keeping my wife busy. One such experience happened while we were out of our apartment taking care of some business. Unbeknown to us, the boys had bought firecrackers (a black-market purchase) at the local *marché* the previous weekend, and they were dropping firecrackers from our ninth-story apartment window, letting them explode on the way down. Somehow, they were never charged with the "crime," but we did get very suspicious looks from our neighbors beneath us.

Unfortunately, I was extremely busy directing the joint test program and did not participate in many of the visitor experiences in the surrounding region. In the evenings, I was often on the phone with Andy working out many of the technical issues. Andy and Irv's support during this period was invaluable. However, on some weekends my wife arranged other activities, which I enjoyed very much.

The testing program was a technical challenge for both contributors—encountering wind tunnel, rotor stand and control, and model-rotor-power problems. Both sides worked diligently to get excellent blade pressure and acoustics data within the capabilities of the CEPRA-19 wind tunnel. At the end of the testing program, I presented a summary of the results to the French technical community—in French (which they insisted on). To this day, I'm not sure which was more difficult—making the presentation in French or performing the program. The excellent data were presented at the 1982 AHS Annual Forum in English, thank goodness! The paper was subsequently published in reference [13].

By this time Irv Statler was developing a real interest in acoustics research. At the beginning of the French MOU acoustic program, Irv asked whether it would be a good idea to expand the acoustic testing of the Army model rotor in the DNW wind tunnel in the Netherlands. I responded that it would be an excellent idea, but that the tunnel cost of \$250,000/week was probably way beyond our budget. He told me to ignore the cost concerns, which he would handle, and to go to the DNW with Claude Dahan and give a presentation to the DNW personnel suggesting a new test program in the DNW wind tunnel.

Claude and I flew to Amsterdam, rented a car, and drove north to the DNW. It was a long day of travel and I arrived tired, but my presentation that afternoon went well, and we left directly for

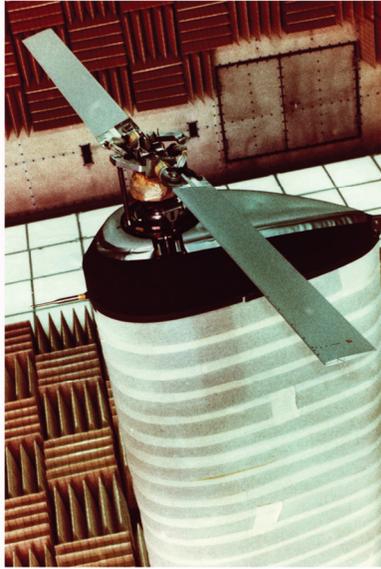
dinner with some of the DNW management staff. After some drinks and wine with dinner, word came that the Chairman of the Board of the National Aerospace Laboratory (NLR) (Professor Dr. Otto Gerlach), who had been unable to attend the presentation that afternoon, was coming down to talk with us about the potential program. He arrived at the restaurant about 10:00 p.m. that evening and asked if I could repeat the presentation that I had made that afternoon. By now, I was really exhausted and feeling the effects of too much alcohol. However, Dr. Gerlach insisted. Sometime after midnight, I finished the presentation and fielded technical questions. Claude later informed me that I had just been “worked over” by professionals to make sure that my technical objectives and approach were sound.

The next day, I found out that the Dutch were satisfied and that we had just been promised up to 3 weeks of testing in the DNW wind tunnel at no cost for tunnel occupancy. The Dutch/German objective for this testing was to show that exceptional rotorcraft acoustic data could be gathered in this new facility. The test program would be performed under a German MOU with the Ames-Army Lab. German, Dutch, and U.S. Army researchers would support the testing under a testing program that I would lead. I was really impressed by Irv’s ability to sell this new acoustic program at high levels within the Army and by his support for this new DNW testing campaign.

Additional French MOU memories. The French MOU was quite active under Irv Statler’s guidance. We had several opportunities to entertain French researchers in the U.S. and be entertained by the French in France. Consequently, many friendships were created that endured for several years with many excellent researchers. My wife and I contributed to these relationships by hosting a meeting at our home in Palo Alto, and by supporting gatherings at other locations in the Bay Area. I also tried to give the French researchers some extra experiences that they might not have in their own country.

During one MOU visit at Ames, I arranged for Jean Renaud, of the Aérospatiale Helicopter Division, to fly in an American helicopter and travel to a nearby airport for lunch. My friend Steve Sullivan, who owned ARIS Helicopters, agreed to provide this service in exchange for the opportunity to talk to Jean about his latest purchase of an Aérospatiale A-Star helicopter—one of the first deliveries of this helicopter in the U.S. During the MOU meeting lunch break, we drove to San Jose Airport, where ARIS was based, to board an OH-6 helicopter to fly to Livermore Airport for lunch. When we arrived at the airport, the OH-6 was being reconfigured from a cargo hauler configuration to the passenger configuration. Rather than wait 30 minutes for the configuration change to be completed, Steve suggested that we take one of his newly purchased A-Star helicopters. After much discussion, Jean insisted that he wanted to wait for the reconfiguration change and stick to the original plan for the OH-6, even though it would make him late to the afternoon MOU meetings.

All went well; we had a nice lunch and returned to San Jose Airport about an hour later than originally planned. On the drive back to Ames, Jean confided to me that his resistance to fly in the A-Star was based on some private company information that he possessed concerning the safety of the A-Star tail rotor. Because there was an on-going company investigation, he did not want to put us at risk and insisted that we fly in the OH-6. I was more than shocked at his reluctance to inform Steve Sullivan of this situation, and I called Steve right away to make him aware. Steve immediately grounded his two A-Star helicopters and phoned Aérospatiale for an explanation. It turned out later that there was a technical problem with the tail rotor that made the A-Star unsafe.



DNW test stand wrapped with acoustic material mounted in the DNW wind tunnel.

The first DNW rotor acoustic test. Planning for the first DNW rotor acoustic testing began while our group was still in France. A completely new structure, designed to support the Army's two-bladed rotor test stand in the wind tunnel, was constructed by the DNW. They also had to attach a large support structure to hold the microphones at scaled far-field positions that attempted to match the positions we had taken during in-flight measurements of the full-scale helicopter. The test program also included testing support from the German Aerospace Center (DFVLR), located outside of Braunschweig, Germany. The Germans brought testing equipment and supported data acquisition for microphones positioned within the anechoic space surrounding the open-jet free stream of the DNW tunnel configuration. It was decided to repeat the CEPRA-19 test matrix and to also explore new higher-speed test conditions that were now possible at the DNW.

The actual testing commenced in the spring of 1982 and lasted 3 weeks. Once again there were challenges that had to be overcome in this new facility that all parties worked through with ingenuity and perseverance. I remember one particularly critical juncture in the testing. We had just discovered that there were severe reflections off the untreated microphone support struts and rotor stand that would distort the measured data. We found this out by mounting and remotely firing impulses (squibs) from known rotor positions and recording the impulses and their reflections in the time domain at selected microphones. (A similar technique had been used in the CEPRA-19 wind tunnel.) It was evident that we needed to reduce these reflections if we were to obtain the high-fidelity acoustic data that we were expecting in the DNW. However, we were also using up valuable tunnel test time trying to fix this new problem. I took the position that it was more important to obtain fewer test points of high quality rather than more test points of lower quality to satisfy the daily test scheduling. The situation was serious enough to have a meeting with the Director of the DNW (Professor Barche) who took the latter position on the matter. Professor Barche asked if I had the authority to make this decision. I replied that I did and suggested that we move on to fix the reflection problem. Later that evening, I telephoned Andy Morse to see what the perceptions were back at the office. Andy told me that he and Irv backed my decision fully with no exceptions. It was great to know that our test team had the complete confidence of our management.

Solving the reflection problem was difficult. It was decided to install extensive absorptive material on all the microphone struts in the tunnel. While we had the acoustically absorbent material, the only good method to attach the material to the struts required a porous tape that was not readily available. Waiting for an order for the porous tape to arrive would further delay the test. One of the DNW mechanics had the solution—we would secure the acoustic material to the struts using hospital gauze. He immediately called his wife, who worked at the local hospital, to purchase the material. Working together through the weekend, we all installed the absorbent material on the support struts. It turned out that we used all of the available gauze at the hospital. The next day, we repeated the squib testing and found that the reflections had been reduced to levels that were consistent with the program objectives. I was very proud of our international

working team, my management for supporting us to make the correct decisions, and the acoustic results gathered in an almost reflection-free acoustic environment. The U.S. Army team of Bob George, Georgene Laub, Don Boxwell, and me, along with our supportive Army management, Andy Morse and Irv Statler, are shown in the photo on the right.



The Army DNW test team from left: Bob George, Georgene Laub, Andy Morse, Don Boxwell, Fred Schmitz, and Irving Statler.

At the end of this test program, we knew we had done something special. We had obtained high-fidelity model-scale acoustic and blade pressure data that could be compared one-to-one with full-scale flight measurements. Reducing this data set and reporting the results would be a pleasure.

Under the auspices of the U.S.-Germany MOU, Wolf Spletstoeser of DFVLR spent the next 6 months at the Ames-Army Lab working with us on the DNW data. He was a very good researcher who could work with almost anyone. We presented several important documents and archived them in journals in the U.S. and Europe [14, 15]. While in the U.S., Wolf and I spent many weekends together enjoying common pastimes—fishing and skiing. Wolf was an avid fisherman who went fishing many times out of the Port of San Francisco. By the time he left the laboratory to return to Germany, my basement freezer was packed with fresh salmon that Wolf had caught. After he returned to Germany, my family’s reply to our frequent dinner choice was, “Not salmon again!”

The Aerodynamic and Acoustic Test Model Rotor (AATMR) program. The DNW test had accomplished the goals of all the cooperating agencies. The DNW had proof that high-quality testing could be accomplished in their tunnel, and they lined up several European customers. They hoped that U.S. industry would invest in testing in their facility as well.

One afternoon back at Ames, I received a phone call from Jacq van Nunen, the Dutch engineer who was the officer in charge of our previous DNW test. He alerted me to a recently announced “off-set” commitment from the U.S. Government to the Dutch government that had just been signed. In effect, because the Dutch government was purchasing the Patriot Missile Defense system from the U.S., the U.S. Government agreed to spend a certain amount of money in the Netherlands as a quid pro quo. It was expected that this money would be spent on infrastructure projects with the Netherlands. He alerted me that his government would be open to using part of this offset money to fund further testing in the DNW program with U.S. industry.

Upon learning of this, Irv Statler went into immediate action. He assigned me to come up with the technical details of such a program, and he notified our contacts at the Pentagon to see how we could make this happen using a portion of the offset money. My friend Dick Lewis had risen to become the Director of Army Research and Technology, a very fortunate development. He

inquired about the offset money and informed us that a meeting to decide how to spend the offset money was taking place at the Pentagon in the near future. He also volunteered a U.S. Army Lieutenant Colonel, Ken Kunstel, to help prepare an effort (briefing) to obtain a portion of the Patriot Missile offset money. I also traveled to the Pentagon to work with Lieutenant Colonel Kunstel to develop the technical plan for this testing and received valuable advice from Dick Ballard, in the Department of the Army, an influential supporter of the Ames-Army Lab.

The essence of our efforts consisted of having Army research facilities (test stand and acoustic measurement systems) and people support the U.S. industry testing in the DNW wind tunnel. Industry would be responsible for providing their rotor systems without support from the U.S. Government. All major helicopter manufacturers were contacted, and they agreed to the general terms of this proposed agreement. It was not an easy sell to U.S. industry because of their nervousness about testing in a foreign country. It was also agreed that the Army would have primary access to the measured DNW acoustic data.

The testing objective was advertised as being focused on BVI impulsive noise. However, getting wind tunnel acoustic measurements for low-frequency noise important for acoustic detection was the main objective for the Army—so the testing program was designed accordingly.

The meeting to resolve the distribution of the Patriot Missile offset money went very well. After I gave my briefing, 3 million dollars were approved to support this industry research testing. I later learned that part of the reason we were successful was that the other branches of the Army were caught off guard—they had no experience fending off this excellent use of offset money. If there were a “next time,” we might experience a different result. However, I learned that it was very important to make good decisions quickly.

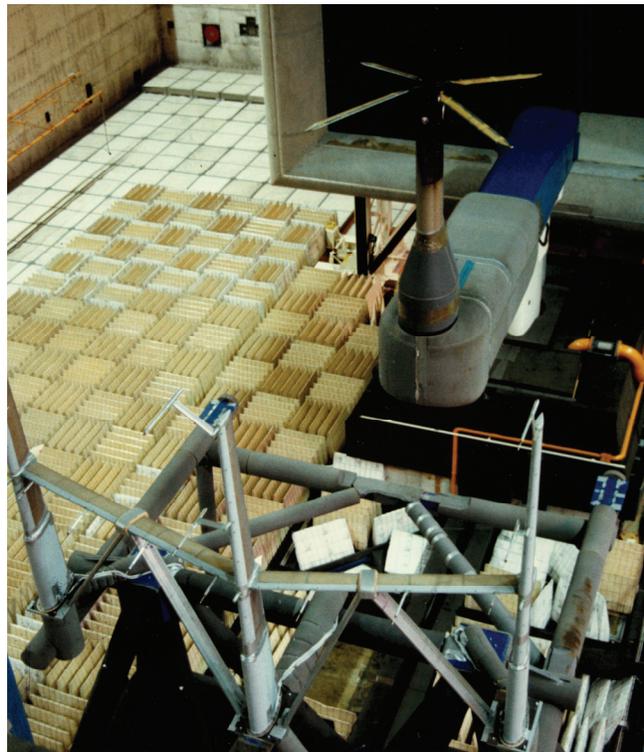
Upon returning to the laboratory, a formal event was planned to commit the offset money as soon as possible (before anyone in authority within the Army changed their mind) and to celebrate this new joint research program, called the Aerodynamic and Acoustic Test Model Rotor (AATMR). The event would be held at the DNW wind tunnel, and Dr. Jay Sculley, the Assistant Secretary of the Army for Research, Development, and Acquisition was invited to represent the U.S. at a signing ceremony. Irv Statler, Jacq van Nunen, and I were also invited to the ceremony because of our extensive involvement in helping create the program. Everything went according to plan, and a cooperative agreement between the U.S. Government and the Netherlands was signed in December of 1984.

It was important that the Ames-Army Lab put its best foot forward at this international event. Irv and I flew commercially to Amsterdam and planned to drive to a town near the DNW to spend the night. The next morning, we would drive to the DNW for the ceremony. We wondered what type of car we should rent for this event. Because the cost for a more prestigious car was not that much more than a less prestigious one, the decision came down to what image we wanted to present. Irv and I decided that it would be appropriate to rent the more prestigious car (a Mercedes Benz) for this important event. When we got to our hotel that evening, we found out that Dr. Sculley, who flew in on a military aircraft, would arrive in an older U.S.-made sedan. In an effort to not upstage Dr. Sculley, we immediately re-parked our car behind the cars of the arriving dignitaries. It was quite an amusing moment.

When we arrived back in the U.S., it was my job (with Irv's help) to convince the U.S. industry to participate in this unique research opportunity. We received positive responses from the Boeing Company and Sikorsky Aircraft. Boeing was planning to test a model-scale Boeing Model 360 advanced rotor in their nonacoustic tunnel, and reasoned that measuring far-field rotor acoustics on this same rotor would result in data that they could not get any other way. Sikorsky was a harder sell. They wanted the government to further support the development of their model-scale UH-60 pressure instrumented rotor that was already being funded by the government. However, after talking with Sikorsky's Engineering Vice President, Jim Satterwhite, who was an Army helicopter pilot and former member of the Ames-Army Lab, Sikorsky and their partner organization, United Technologies Research Laboratory, agreed to become part of the AATMR program.

The Army laboratories also stepped up to do their part. Headquarters designated some funds to purchase a new state-of-the-art data acquisition and recording system. I designed, and the NASA Ames shops built, a new microphone support system that minimized local acoustic reflections. A new swash plate that removed the binding problems we were fighting on previous entries was also designed and built in the NASA shops. We assembled all the Army support equipment at Ames in large truck trailers (given to the laboratory by my friend Don Reese from the Army Tank Command) and had the entire package flown to the DNW in the Netherlands by an Air Force C-141 aircraft.

It was decided that it would also be prudent to conduct a check-out test of this rotor drive system, mounted in the tunnel on the DNW test sting using a similar Boeing uninstrumented rotor. The check-out test was performed 6 months before the planned entry. The test went quite well until one of the special drive motors failed. The Boeing crew changed their flight reservations to leave the Netherlands immediately, thinking that we could not complete the planned test matrix on only one drive motor. Bob George, Georgene Laub, and I looked over the situation and decided that we could still get the test points that we wanted using only one drive motor. It was our experience that these motors were normally very robust and could be run past their design limits if their temperature did not rise above their "do-not-exceed" limits. We then pulled out the broken motor and told the Boeing crew to change their plane reservations again because we would be ready to run the next morning. We finished all of the objectives of the check-out test. In my opinion, this "can do" approach set the mark for the success of the entire Boeing test program.



Noise testing in the DNW wind tunnel using the Boeing rotor.

One not-so-nice fallout of the program also occurred. I was blamed for this accident before we had time to determine the cause of the motor burnout. It was assumed that the motor failure was due to operating one of the two motors in an overload situation. However, I had operated that motor in similar situations at Ames many times before and had not experienced a similar problem. Therefore, I was sure that this motor had some fabrication problems. When I returned home, I learned that others had had similar experiences, but only on motors that were produced after a certain date. It turned out that a key person at the manufacturer had retired some months before our motor failed. Motors delivered before his retirement date were still operating just fine. I was vindicated; I had not destroyed the motor, it was a fabrication problem.

Moving to a Formal Supervisory Role

Army Simulation Manager. I enjoyed working for my immediate bosses at the Army, Andy Morse and Irv Statler. I am convinced that they always really believed that their actions were for the greater good of the Army. Although I would disagree with their decisions sometimes, I would “soldier up” and try to do whatever they asked. In turn, they would support me despite the difficulties I might run into. However, there were some situations where we did not always see “eye-to-eye.”

Upon returning from the first test in the DNW, Irv wanted to “collect” on my promise to alter my career direction from acoustics research to focus on simulation and control. I had made this promise in return for the Army’s support to obtain my commercial helicopter license. After some discussion, Irv created a new “Simulation Office” that I would run at Ames. The purpose of the office was to run Army simulation efforts using the Ames facilities. At my insistence, I would also be allowed to do Army in-house research, with Irv helping to find additional research money. I was immediately moved out of the Fluid Mechanics Division in Building N-215 and into a portable trailer located in the adjacent parking lot.

The actual simulation projects that I received were quite interesting. For example, working with Hughes Helicopters, we set up and ran the first dynamic simulation of the “no tail rotor anti-torque system (NOTAR).” We also worked with Bell Helicopter to develop the first flight simulation of the V-22 (by scaling up the XV-15 tiltrotor simulation for the Ames Vertical Motion Simulator). We used Defense Mapping Data to build a database for training pilots to fly risky missions into hostile areas. However, because these efforts were application oriented, it was difficult to staff Simulation Office positions. Most good researchers at Ames thought that working in this office was a technical dead-end because of a lack of a research focus and, consequently, they would not have a good future. Unfortunately, I agreed with them and tried to get some research tasks for the office. Unfortunately, or fortunately, a few pursued other jobs that would better establish their careers.

My Simulation Office problems were compounded by Irv’s lack of definition of my working relationship with Dave Key, Chief of the Aeromechanics Laboratory’s Flight Control Division. (The Ames Directorate of AMRDL had become the Aeromechanics Laboratory of the U.S. Army Research and Technology Laboratories (RTL) in 1977.) We were both told that we could do research on simulation and control, but only Dave had a line item in the budget to support research work. As I continued to run the Simulation Office, I had many discussions with Irv about my unhappiness in this new job.

Naval Postgraduate School opportunity. About the same time, an advertisement was posted for the position of the Head of the Department of Aeronautics at the Naval Postgraduate School in Monterey, California. I applied for the position and received word that they would be making me an offer. Sheila knew that I was unhappy and was willing to move to support my career. I received the job offer, and I was getting my family ready for the move to Monterey.

At the last minute, Irv called Andy and me into his office for a discussion. Irv told me that he was now aware of the situation and asked if there was anything he could do to “keep me at Ames.” I responded that I had tried to find a research position that I could be happy with, but I could not do so under the current Army organization. After many discussions, Irv and Andy came up with a proposed new plan. Andy would take a new position as Chief Engineer for the Aeromechanics Laboratory while I took over Andy’s former job as Chief of the Fluid Mechanics Division.

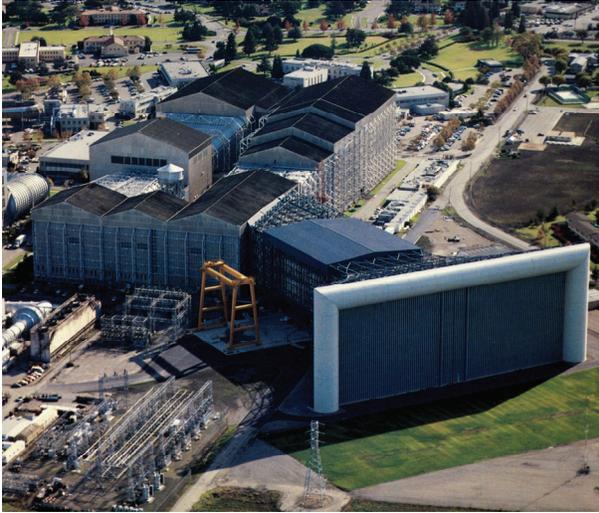
However, too much time had elapsed in this decision process. I had already visited the the Navy Laboratories in Washington, D.C., and had gotten the Commander to agree to commit additional dollars for research funding for Monterey. The management of the Naval Postgraduate School was more determined than ever to encourage me to take over the Aeronautics Department.

My wife and I had many discussions about which career path would be better for me, her, and our family, as well as my potential contributions at each organization. Moving to the Monterey peninsula and uprooting my sons from Palo Alto schools also factored strongly into the decision process. In the end, I decided to accept Irv’s offer and take the position as Fluid Mechanics Division Chief at the Aeromechanics Laboratory. The ability to continue to work with, and be supported by, my Army and NASA friends on fundamental research projects played a big part in this decision.

Life and work were very satisfying for the next 2 years. I settled into my new position easily, keeping quite busy guiding many projects, including preparations to support the first cooperative test program with the Boeing Company under the AATMR program. I also tried to support the AATMR program and focus our research toward goals that would support Army problems through fundamental research. One example of the redirection was to reduce our efforts to support the 3-D holographic interferometry efforts in the Anechoic Hover Chamber to visualize the delocalization process. Although further understanding of hover noise would have been nice to have, the reality was that new experimental efforts were needed to further understand “delocalization” in forward flight. The joint Army, DNW, and helicopter industry program would address these issues over the next 5 years.

Management challenges. To Irving Statler’s delight, in 1985, he was offered and accepted the prestigious position of Director of the Advisory Group for Aerospace Research and Development (AGARD) in Paris, France (see Irv’s chapter). Andy Morse became the acting Director following Irv’s departure and he kept things running smoothly during the search for a new Director.

At about this same time, Tom Snyder, the Director of Aerospace Systems at NASA Ames, approached me to become a candidate to form and run the NASA Full-Scale Aerodynamics Research Division in his directorate. Because of my involvement and excellent rapport with many different branches within his organization, Tom thought that I was well qualified for this Senior Executive Service (SES) position. I thanked Tom for the offer but explained that I was applying for Irv’s old position as Director of the Army Aeroflightdynamics Directorate (AFDD).



The 40- by 80-Foot Wind Tunnel—part of the National Full-Scale Aerodynamics Complex (NFAC).

He replied that he felt that the NASA opportunity was a good fit for me and that he would try to keep the position open until the Army decision was made.

The Army set in motion actions to fill the AFDD Director position. Although I applied for this job, I had mixed emotions at that time. I was pretty content to stay where I was as the Fluid Mechanics Division Chief of AFDD. The fluid dynamics research group that I led was internationally respected and fun to work with. I had been preparing for the first acoustic test with Boeing in the DNW wind tunnel under the AATMR program. However, even to do this, I had to travel extensively, causing my wife and family to adjust to my absence. I was reminded that not

being present during my children's formative teenage years was not even close to ideal. I was not sure that Irv's old job would improve my travel situation.

Andy Kerr, the Chief of the Advanced Systems Research Office in RTL Headquarters, was also competing for this job. I had interfaced with him several times on trips to Europe and we got along fairly well personally, but my impression at the time was that we had different views on the importance of fundamental research. Unlike Irv Statler and Andy Morse, Andy Kerr favored research that was more applied. After I learned that Andy Kerr was selected for the position of Aeroflightdynamics Director, I had the choice of working for Andy Kerr in my present position or taking Tom Snyder's offer to move to NASA.

When Tom Snyder called and asked me to reconsider applying for the new SES job of Chief of the Full-Scale Aerodynamics Research Division, I accepted. Besides my doubts about the possible research direction of AFDD, other important factors entered into my decision. By now, I had experience running complex tests in many wind tunnels throughout the world, and I thought I could bring that experience to bear in the formation of the new NASA Division. I also felt that it might be possible to make the National Full-Scale Research Complex (NFAC) more viable as an acoustic testing wind tunnel that would compete with the DNW in many instances. In addition, I was sure that I would travel less, which would help me spend more time with my family. After much deliberation and many discussions with my family, I accepted Tom Snyder's offer to run Full-Scale Aerodynamics Research Division. I would successfully run the NFAC for the next 8 years—but that is another memoir.

Epilogue

Although I left the Army in 1986 to join NASA, I still had the pleasure of working with my Army colleagues under the Army-NASA Joint Agreement for several years. In my opinion, many of the opportunities that I was fortunate to experience were a direct result of this special relationship that the Army had with NASA. The access to special people and world-class

facilities enabled both NASA and the Army to make many important advances in rotorcraft and VTOL technology.

I faced many exciting challenges and opportunities in my new NASA job as the Director of the NFAC consisting of the 40- by 80-Foot Wind Tunnel, the 80- by 120-Foot Wind Tunnel, and the Outdoor Aerodynamics Research Facility (OARF). Prior to my joining the NFAC, a major accident closed the facility and necessitated major modifications and repairs. The repair team was in the final phase of certification when I came on board to form the new Full-Scale Aerodynamics Division, which consisted of the Rotorcraft Aeromechanics Branch, the Fixed Wing Aerodynamics Branch, the Research Operations Branch, and the Data Acquisition Systems Branch. I stayed in this position for 8 years testing all kinds of aircraft and rotorcraft in the NFAC and winning many national awards. I also obtained funding for and lead the design of a major improvement to the 40- by 80-Foot Wind Tunnel, making it the largest anechoic tunnel in the world. The visibility was a double-edged sword for the NFAC; our team won many awards during this period and also had some notable incidents. In either case, we received the attention of the national press.

When Tom Snyder retired, a major NASA Ames reorganization split wind tunnel research from wind tunnel operations; I chose to remain with the research effort and headed the Full-Scale Aerodynamics Research Division. Later, I moved to become the Assistant Director of the Information Technology Directorate under Dr. Henry Lum. At his retirement, I accepted the position of Director of Aeronautics reporting to the new Ames Center Director, Dr. Harry McDonald. Although running this organization theoretically gave me the authority to choose new research directions, this power was often usurped due to political considerations.

After about 12 years in NASA management, I realized that, at 56 years old, I could take early retirement and go back to research. This was a viable option because I had continued to perform and publish theoretical research in my management roles at NASA. Since I had loved working with a small research group at the Ames-Army Lab as well as teaching and working with students at Stanford, I pursued several university opportunities and accepted an offer from the University of Maryland in College Park, Maryland, to be the “Martin Professor of Rotorcraft Acoustics.” I started in this position the day after my official retirement from NASA.

Over the next 20 years, I built a group of about 6 to 8 researchers to do basic research in rotorcraft acoustics. I mentored talented Ph.D. students, many of whom now hold industry and government positions in the rotorcraft acoustics field. Equally important, I was again able to pursue my love for research and teaching and cap my life-long career in aeronautics and acoustics.

I feel fortunate to have had the opportunity to pursue my dreams and have tried to make a difference. I believe that my early days at the Army Aeromechanics Laboratory under the Army-NASA collaboration at Ames taught me much about how to create an environment where good research can flourish. Working with excellent managers and a highly trained and educated staff, having the use of world-class facilities, and enjoying the freedom to create have benefitted me throughout my career.

Now, during retirement, I continue to value the relationships with scientists that I have established through the years, and I often rely on these contacts as I undertake various consulting opportunities that keep me involved in the field.

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Index

A
Advanced Attack Helicopter (AAH), 16, 18-19
Advanced Systems Research Office (ASRO), 21
Advisory Group for Aerospace Research and Development (AGARD), 35
Aerodynamic and Acoustic Test Model Rotor (AATMR), 26, 31-33, 35-36
Aérospatiale Helicopter, 28-29
Air Force Institute of Technology (AFIT), 1
American Helicopter Society (AHS), 11, 28
Ames-Army Lab, 1, 4-6, 10, 17-20, 22, 25, 27-29, 31-33, 37
Anechoic Hover Chamber, 7, 14, 23, 26, 35
ARIS Helicopters, 21, 29
Army Aeroflightdynamics Directorate (AFDD), 35-36
Army Aeronautical Research Group (AARG), 4-5

Army Aeronautical Research Laboratory (AARL), 3-4
Army Air Mobility R&D Laboratory (AMRDL), 5, 19, 34
Army Aviation R&D Command (AVRADCOM), 25
Army Experimental Flight Activity (AEFA), 10, 15-16, 21
B
Ballard, Dick, 32
Bell Helicopter, 3, 10, 34
Berry, Gordon, 5
blade-vortex interaction (BVI), 11, 16, 28, 32
Boeing Vertol Company, 2-5, 18, 20, 33, 35-36
Bolt, Beranek and Newman, 26
Borgman, Dean, 4
Boxwell, Don, 6, 9-12, 15, 23, 26, 31
Brekke, Darrell, 26

- Bryson, Art, 19-20
 Building N-215, 6, 34
 Burke, Jim, 7-8, 10
 Burkham, John, 19-20
- C**
 Caradonna, Frank, 5, 13, 22-24
 Carlson, Dick, 19, 25
 CEPRA-19, 27-28, 30
 computational fluid dynamics (CFD), 23-24
 Construction of Facility (CoF), 6
 Cox, Charlie, 10
 Crawford, Charlie, 18
 Crissy Field, 8
- D**
 Dahan, Claude, 27-28
 DNW wind tunnel, 28-30, 32-34, 36
 Duffy, Vance, 15-16, 18-19
 Dukes, Theodore, 1-2
- E**
 Edwards Air Force Base (EAFB), 10, 16
- F**
 Farassat, Ferri, 23
 Fidell, Sanford, 26-27
 Franklin, Jack, 2, 20
 Frix, Robert (Bob), 17-18
 Ft. Lewis, 17-18
 Ft. Rucker, 21
- G**
 General Acoustics, 7
 George, Bob, 10, 12, 15, 31, 33
 Gerlach, Otto, 29
 German Aerospace Center (DFVLR), 30-31
 Graham, Dunstan, 1
- H**
 Hawkings, Dave, 24
 Hodges, Dewey, 20
- I**
 Identification Friend or Foe (IFF), 26
 Improved Main Rotor Blades (IMRB), 16
 Isom, Morris, 22
- J**
 Jones, Lee, 21
- K**
 Karamcheti, Krish, 19, 21
 Kerr, Andy, 36
 Kunstel, Ken, 32
- L**
 Lambert, Olivier, 28
 Laub, Georgene, 7, 12, 14, 31, 33
 Lazzeroni, Frank, 6
 LeMasier, Phil, 25
 Lewis, Richard (Dick), 10, 15, 18, 31
 Light Intratheater Transport (LIT), 3
 Lowson, Martin, 13, 18
- M**
 Massachusetts Institute of Technology (MIT), 1
- McCroskey, Jim, 2, 27
 Memorandum of Understanding (MOU), 27-29, 31
 Morse, Andy, 5-6, 8-10, 13-14, 16, 18, 20, 23, 30-31, 34-36
 Mosher, Wayne, 26
- N**
 NASA Ames Model Shop, 11, 23
 NASA Langley, 23
 National Aerospace Laboratory (NLR), 29
 National Full-Scale Research Complex (NFAC), 36-37
 no tail rotor anti-torque system (NOTAR), 34
- O**
 Office National d'Etudes et de Recherches
 Aéronautiques (ONERA), 5, 27-28
 OV-1C aircraft, 10-11, 13
 Oxnard Air Field, 15
- P**
Parade magazine, 15
 Patriot Missile, 31-32
 Princeton University, 1-3, 7, 21
- R**
 Reese, Don, 26, 33
 Renaud, Jean, 29
 Rensselaer Polytechnic Institute (RPI), 1-2
 Research and Technology Laboratories (RTL), 21, 25-26, 34, 36
 Reserve Officers' Training Corps (ROTC), 1
 Roberts, Len, 17
- S**
 San Jose Mercury News, 15
 Satterwhite, Jim, 33
 Schlieren methods, 14, 23
 Sculley, Jay, 32
 Senior Executive Service (SES), 35-36
 short takeoff and landing (STOL), 3
 Sikorsky Aircraft, 18, 24-25, 33
 Snyder, Bill, 20
 Snyder, Buzz, 17
 Snyder, Tom, 35-37
 Source Selection and Evaluation Boards (SSEB), 16, 18-19, 21
 Splettstoesser, Wolf, 31
 Stanford University, 19-21, 25, 37
 Statler, Irv, 10, 16, 20, 22, 24, 27-36
 Stepniewski, W. Z., 2-5, 9
 Stevens, Story, 25
- U**
 UH-1H helicopter, 7-8, 10-11, 14-16, 18, 23
 United Technologies Research Laboratory, 23
 Utility Tactical Transport Aircraft System (UTTAS), 16, 18-19
- V**
 van Nunen, Jacq, 31-32
 Vause, Rande, 6, 8-9, 13-14, 20-21
 vertical takeoff and landing (VTOL), 2-3, 5, 9, 20, 37
 vertical/short takeoff and landing (V/STOL), 3

Schmitz, F.

Vietnam, 3-4, 7, 15, 19

VZ-2 tiltwing aircraft, 3

W

Westland Helicopters, 18, 24

Y

Yaggy, Paul, 3-5, 9, 19

Yu, Yung, 20-22, 24-25

Adventures in Low Disc Loading VTOL Design at Ames*

Mike Scully
Army

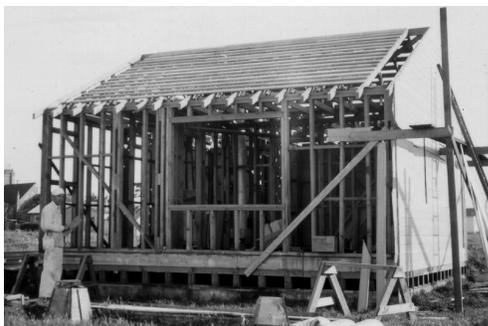
Getting Started

Late in 1945, when I was 3, Mom and Dad bought a 3-acre pear orchard in Contra Costa County, some 15 miles east of Berkeley, California. The orchard sloped up from a row of walnut trees along the county road to three eucalyptus trees at the back. Dad had just returned from the war in Europe. He still had shrapnel in his leg from a Luftwaffe bomb on D+2 off Normandy. Before the war Dad had built a tiny redwood house for Mom in San Pablo, California. It was sold when Dad was drafted for WWII; Mom lived with her parents in Berkeley while Dad served in the Army. Now Mom, Dad, my brother Tim, and I moved to the pear orchard and camped while Dad built our family home under the eucalyptus trees. Good lumber was unaffordable after the war, so Dad built a temporary house using war surplus lumber—mostly unfinished 3- x 12-inch boards. Tim and I grew up in the temporary house with room to play and chores to do. Mom and Dad raised chickens and rabbits, sold pears and walnuts, grew fruits and vegetables for food, and worked their day jobs.



Mike Scully

The area developed and our pear orchard became part of Pleasant Hill. Dad expanded the temporary house and made it more comfortable. The house was becoming less temporary and the new additions had concrete foundations. From 1953–1955 we lived in a redwood lumber mill town (Scotia, California), but Pleasant Hill remained our real home. In 1959 a development went up behind our house, and we bought one of the new houses. Mom and Dad sold the last of the original 3 acres in 1966 when Dad was dying of cancer. Dad watched with some pride as the new owners struggled to demolish the “temporary house” with a bulldozer. In 1974 Mom retired from her medical technician career, sold the “new house” in Pleasant Hill and moved to Albion, California, on the north coast, 8 miles south of the town of Mendocino. Albion became home in my mind.



Tiny house, San Pablo.



Temporary house, under smaller eucalyptus trees.

* This chapter is a shortened version of reference [1] that contains more complete technical details and an extensive list of references.



Dad in Belgium, 1945.



Grandmother, Mom, Mike, Berkeley.



Tim, Mike, big eucalyptus.

We were a family of readers. Mom and Dad encouraged us to explore the world and discover our passions through the magic of books. Mom took us to the basement of the county library where they had tables of children's books for each grade level. We started at grade level but soon explored more challenging books. In addition to our own reading, Mom, Dad and Great-Aunt Ethel read special books to us including *Little Men*, *Swiss Family Robinson*, and *Born Free*. When we lived in Scotia, it was 27 miles to the county library in Eureka. The librarian allowed us to check out more books for a longer time because of the long drive. When we returned to Pleasant Hill there was a new branch library. It had a good collection of science fiction (Asimov, Clarke, Heinlein, etc.). The dream of humanity spreading beyond Earth to the planets and the stars became an exciting possibility. I became interested in both aeronautics and astronautics.

Education was important in our family. Mom's family supported her college education. She graduated from the University of California, Berkeley, and through internships became a licensed medical technician. Licensing was new and she was proud of her 3-digit California license number. Dad studied chemical engineering for 2 years, but then Grandfather Scully decided it was time for his oldest son to quit school and help support the family. Dad had a variety of jobs including some years at a Union Oil refinery. In 1950 he became a firefighter for the Army, eventually becoming an assistant chief. Tim and I were encouraged to study hard and go to college. We were also encouraged make our own decisions about what to study. Medical technicians take turns being on call for medical emergencies. Mom would take Tim and me to the lab with her when there was an emergency. This exposure certainly increased our interest in science. My first real job was as an assistant in a medical lab. Tim built a cyclotron for the high school science fair. After 3 years of high school, Tim became a U.C. Berkeley student with a part-time job at the Radiation Lab on the hill above campus.

I became fascinated with airplanes and flight at an early age. Uncle Carl was a Naval Aviator who had many aeronautical adventures. I also remember listening to accounts of Korean War battles over MiG Alley on the radio. A bookstore on Union Square in San Francisco had a wonderful airplane book, *Aircraft of the World*, and I saved up the money to buy it. I studied this book night and day until I knew every airplane. I still have my original copy, worn but readable. Pacific Lumber Company owned the town of Scotia, and the redwood lumber mill. They had a

de Havilland of Canada DHC-2 Beaver as their corporate aircraft. During 7th grade Mom got me a ride in the Beaver from Rohnerville airport near Scotia to Buchanan Field in Concord, California. That 210-mile trip was my first airplane ride, and I was convinced that aviation was what I wanted to do in the future. I took a drafting class in high school and designed a VTOL flying saucer with a circumferential fan driven by the exhaust of two Bristol Olympos turbojet engines.

On to MIT

Uncle Carl said that with my grades and College Board scores he could get me an appointment to the Naval Academy at Annapolis. This sounded great at first, but then I discovered that my eyesight was not good enough to be a Naval Aviator. Becoming an airplane designer sounded better to me than a non-flying career in the Navy. We applied to three engineering schools: Harvey Mudd College (first choice), the University of California, Berkeley, Engineering (fallback school), and the Massachusetts Institute of Technology (MIT) (the best). My transcript got lost in the mail, so I did not get admitted to Harvey Mudd. I did not really expect to get into MIT and failed to apply for regular financial aid. The choices were U.C. Berkeley Engineering with a California State Scholarship or MIT with only emergency financial aid. The family dug deep, including \$1,000 from Great-Aunt Ethel, and came up with enough money for my freshman year at MIT. Mom and Dad were concerned that working during school would hurt my studies, so I did not work freshman year. After my freshman year, I applied for financial aid. MIT prepared a budget of estimated income and expenses for each applicant. This budget included an expected family contribution and typical student earnings. MIT then offered financial aid (scholarships and loans) to balance the budget. I was able to earn more and spend less than the MIT budget, so very little family contribution was needed.

I was very shy and Mom was worried about a big school like MIT. It was not a problem. The dorm grouped freshmen together so there was a group of instant friends, and the MIT Libraries were a dream come true. They had complete sets of *Jane's All the World's Aircraft* and *Jane's Fighting Ships*. I worked part-time in the MIT Aero Library during my sophomore and junior years for minimum wage (\$1.25 per hour). One of my duties was "reading the shelves" (putting books in order). This was great fun and I became very familiar with the whole collection. During holidays I worked extra hours in the other MIT libraries. This was an opportunity to discover additional technical materials and to explore history, economics, political science, etc. The Aeronautics and Astronautics department had a cooperative education (co-op) program with industry, and this became part of the financial plan. I worked as a co-op student at Boeing Plant 2 in Seattle on the X-20 Dyna-Soar spaceplane. The co-op experience convinced me that I wanted to be an engineer doing creative work, but I would need an advanced degree. It motivated me to work harder, get better grades, and get into graduate school.

Professor Rene H. Miller was in charge of the co-op program, and in my senior year he offered me a job working on helicopter harmonic airloads at \$1.75 per hour. Professor Norman D. Ham had an office next to Professor Miller and a desk was added to his office for me. Professor Miller had many interests. He taught design courses in vertical takeoff and landing (VTOL) aircraft, researched helicopter harmonic airloads, and spent 1 day a week as a consultant to Boeing Vertol. He also founded the MIT Flight Transportation Lab (FTL). I applied to MIT for the Engineer in Aeronautics and Astronautics (EAA) program. The EAA program included twice the course work of a Master of Science (SM) degree and was intended for designers. In addition to

traditional aeronautical engineering courses, I studied Flight Transportation and Operations Research. Field trips were part of these courses, and a trip to the New York Airways helicopter airline operation included a flight to Wall Street heliport in one of their Vertol 107 helicopters. This exposure to aircraft operations and operators greatly improved my abilities as an aircraft designer. The EAA degree was not very well known and, before I finished, Dad suggested that I consider also going for a Ph.D. I took the doctoral qualifying exam and, to my surprise, passed.

I was a full-time Research Assistant in graduate school and Professor Miller was my advisor and my boss. He convinced Civil and Mechanical Engineering to give a small part of the Department of Commerce Northeast Corridor Transportation Study on high-speed rail to FTL. I worked for FTL writing helicopter and tilt wing design codes, and designing VTOL transports to compete with high-speed rail. I worked on both VTOL design and helicopter airloads for several years. Lockheed California worked on a stopped, folded, and stowed rotor concept from 1965–1970. Professor Miller sent me to Lockheed California to learn more about the stowed rotor concept. Dr. Richard M. Carlson was Division Engineer for Rotary-Wing Research and Advanced Design. The funding for VTOL design studies eventually dried up, but the Office of Naval Research (ONR) funding for airloads work continued. My work followed the money and went back to airloads research and this became my Ph.D. thesis. The various jobs I had as an MIT student were both financially necessary and essential to my education as an engineer. I earned SB (6/64), SM (6/67), EAA (6/67), and Ph.D. (2/75) degrees in Aeronautics and Astronautics.

I moved from a desk in Norm Ham's office to various offices shared with other grad students including John Shaw and Wayne Johnson. Another student (Tom Imrich) was looking for partners to buy N72284, a 1946 Model Cessna 140, from his father. Tom had learned to fly in N72284 and wanted to keep it. He was a Certified Flight Instructor and offered to teach me how to fly as part of the deal. Wayne Johnson, who already had a Private Pilot's License, became the third partner. We put up \$1,000 each (in my case, borrowed from Mom) and became aircraft owners. When Wayne graduated and went to work at Ames he bought N72284 from Tom and me. Wayne drove his Corvair to California so I used my new Private Pilot's License to ferry N72284 for him. It was November, so the plan was to avoid the High Sierra mountains by flying to Los Angeles and then up the coast to the Bay Area. On arrival in Los Angeles I tied N72284 down at Van Nuys airport. The next day it too windy to fly a light plane. I called Wayne and asked him to get N72284 when the weather got better. I flew home on PSA before returning to MIT.

Finding My Place

Boeing knew me through my co-op work in Seattle and Professor Miller's consulting with Boeing Vertol. They offered me a job in Philadelphia where John Shaw was working. To my surprise I found that I wanted to return home to Northern California and the only VTOL aircraft career option in the area was Ames Research Center. Norm Ham knew Professor Hank Velkoff who consulted for the Army Air Mobility Research and Development Laboratory (AMRDL) Headquarters (HQ) at NASA Ames Research Center. AMRDL was established in 1970 under the U.S. Army Aviation Systems Command (AVSCOM) in St. Louis, Missouri, and comprised four subordinate directorates. The Ames, Langley, and Lewis Directorates were named after, and co-located with, their respective NASA research centers. The Eustis Directorate was the former U.S. Army Aviation Materiel Laboratories (AVLABS) located at Ft. Eustis, Virginia. The Ames Directorate was the former U.S. Army Aeronautical Research Laboratory (AARL) that pioneered the unique Army-NASA collaborative relationship at Ames Research Center in February 1965.

The Army was looking for an experienced designer to work in preliminary design. Norm and Hank convinced Dr. Richard M. Carlson, now Chief of the Advanced Systems Research Office (ASRO), to offer me a job in preliminary design on the AMRDL HQ technical staff in ASRO. Wayne Johnson had joined the Ames Directorate in 1970 as an Army employee working in the NASA Ames 40- by 80-Foot Wind Tunnel, and he told me that the Ames-Army Lab “has a good technical environment.” I had dreamed of designing aircraft in industry because I knew that government aircraft designs would never go into production. Thinking about aircraft history, I realized that government aircraft specifications needed improvement because they frequently resulted in unsuccessful designs from industry. I decided that government designs supporting better government aircraft specifications could improve the chances of developing successful military aircraft. While Boeing offered me 10 percent more to work in Philadelphia, California represented mountains, ocean, and family, so it won out and I accepted the job at Ames.

Joining the Army Labs

I joined AMRDL on March 3, 1975. The Army-NASA technical environment included world-class facilities and researchers plus senior engineers from industry who had developed actual aircraft and engines. I was blessed to work with John Wheatley and learn from his adventures with the National Advisory Committee for Aeronautics (NACA), and the aircraft and engine industries. Dr. Carlson and Andy Kerr brought a wealth of industry experience including the development of the Lockheed AH-56A Cheyenne compound attack helicopter.

Dr. Carlson had hired a young engineer (Ron Shinn) to conduct Preliminary Systems Design Engineering (PSDE) studies under the mentorship of John Wheatley, a senior engineer and the ASRO Propulsion Specialist. They completed the first two PSDE studies in 1974. I joined Ron Shinn at AMRDL HQ in ASRO Preliminary Design. Ron was starting on a tilt rotor PSDE study when I arrived, mostly adapting and improving design tools. John Wheatley asked me to do an analysis of the Napier Nomad, a turbo-compounded, two-stroke Diesel aircraft engine developed in the United Kingdom after WWII. This subject was of great interest to me and an opportunity to use the Hewlett Packard HP35 pocket calculator the Lab had just provided. My analysis was based on *The Internal-Combustion Engine* by Taylor.

There were two other AMRDL preliminary design activities: 1) the Systems Research Integration Office (SRIO) located at AVSCOM in St. Louis, and 2) a group at the Eustis Directorate. SRIO focused on the early, more conceptual phase of preliminary design, and developed design and performance codes SSP-1 and SSP-2 based on momentum theory. The Eustis Directorate group focused on the later phase of preliminary design when rotor definition is detailed enough to allow the use of blade element theory. Ron Shinn had combined SSP-1 and SSP-2 into a single helicopter sizing and performance code called PSDE. Unlike the MIT civil VTOL design codes, PSDE was developed to size military helicopters, including mission analysis, and it became the basis for further code development.

I came to Ames with a large amount of professional books and papers. They included discarded library materials (NACA/NASA reports, aircraft engine specifications), material from Professor Miller (AVLABS reports, early McDonnell Aircraft engineering manuals), and some 16,000 pages copied from MIT libraries. Building on these materials, the design team has purchased each annual *Jane's All the World's Aircraft* since 1975 and appropriate other books. Boxes of AVLABS reports were stored in the AMRDL HQ public affairs office. I suggested that AVLABS

reports and Army/NASA reports were the net output of AMRDL and they needed to be accessible on shelves. An AMRDL HQ Technical Information Center was eventually created.

HELCOM—My First Joint Service Preliminary Design Study

In June 1975 the Interservice Helicopter Commonality Study (HELCOM) started. It addressed the question, “Can greater use of common helicopter designs cover existing Department of Defense (DoD) missions more affordably?” A Joint Study Group was assembled by Colonel Joseph Rutkowski (Ft. Rucker) with operational and technical membership from the Army, Air Force, Navy, and Marines. I was the Army technical member joining Dave Norman (Air Force) and Dudley Cate (Navy). We identified 22 primary service helicopter missions (7 Army, 5 Air Force, 5 Navy, 4 Marine Corps, and self-deployment) and their mission requirements. These 22 missions were performed by the 9 helicopter types and 6 variants in DoD inventory. HELCOM showed that a commonality program of five baseline helicopter types and three variants could perform the same missions.

The Naval Air Systems Command (NAVAIR) used the helicopter sizing and performance computer program HESCOMP (originally developed by Boeing Vertol). The Army and NAVAIR performed a detailed (source-code level) technical comparison between PSDE and HESCOMP. Ron Shinn and I revised PSDE to better compare with HESCOMP. We concluded that our design tools needed a lot of work. Our HELCOM experience eventually proved useful for the Joint Services Advanced Vertical Lift Aircraft (JVX), which developed into the V-22.

When John Wheatley retired at the end of 1975, I was asked to be acting ASRO Propulsion Specialist. Dr. Carlson, now the Director of AMRDL, defined the staff job as unloading burdens from the directorates so they could focus on research. We were expected to be familiar with the research and able to explain it to the Department of the Army (DA) in the Pentagon so that they could respond to Congress. This was good experience. I became familiar with the propulsion community and the DA staff. I also learned where funding comes from and how it is defended.

Advanced Attack Helicopter Source Selection

Helping the Army be a “smart buyer” of new aviation systems was an important part of AMRDL’s mission. Charlie Crawford’s Flight Standards and Qualification Directorate (FS&QD) and AMRDL were the major sources of the Source Selection Evaluation Board (SSEB) technical leadership and expertise.

During Phase I of the Advanced Attack Helicopter (AAH) program, Bell Helicopter and Hughes Aircraft had designed, built, and tested prototype aircraft, the YAH-63 and YAH-64, respectively. These prototypes were flight tested by the U.S. Army Aviation Engineering Flight Activity (AEFA) during a Government Competitive Test (GCT). Following these tests, the contractors submitted Phase II proposals that included design modifications to overcome performance shortfalls identified during the GCT. In 1976 the Army held the Phase II Utility Tactical Transport Aircraft System (UTTAS) and AAH SSEBs in Granite City, Illinois. Finding experienced government technical staff for two simultaneous SSEBs was a challenge. A GS-12 at the time, I was assigned as AAH SSEB Performance Factor Chief and made an acting GS-13 for the SSEB. Dr. Carlson offered complete support, and through him I could call on the technical resources of the Army-NASA team as needed.

There were two Factor Chief training sessions in June and July 1976. I learned a lot. The job of an SSEB is evaluation. Selection is the job of the Source Selection Advisory Council and the Source Selection Authority. A Contracting Officer (KO) has a warrant to sign contracts that obligate the government. KO staff are the experts on all legal and contractual requirements of an SSEB. Failure to follow the rules could result in a formal protest by the losing offeror. In extreme cases this could require a new competition and years of delay. Security was critical as any leakage of Competition Sensitive Information could compromise the Source Selection.

I reported for SSEB duty in Granite City at the beginning of August 1976. Teams of evaluators were assigned to the factors being evaluated. The Performance Factor consisted of four evaluators: Roger Smith (SRIO), Jim O'Malley (Eustis), Major Mike Summers (RD&E) and myself. Our job was to evaluate performance specification compliance including both vertical flight and forward flight requirements. Cruise speed, level flight airspeed at Maximum Continuous Power (MCP), became the dominant performance issue for the SSEB. We used data from other SSEB Factors (Weights Engineers, Propulsion Factor, and Secondary Systems Factor) in our calculations. This was before the advent of electronic spreadsheets and there was no time to write new computer codes. We used pocket calculators like the HP35 in conjunction with a rotor performance code (ROTOR) for performance evaluation. Roger Smith did the majority of the hand calculations, but we all contributed to this labor-intensive bottleneck in our analysis process. Our calculations were documented in writing and carefully checked; we checked each other's work because fresh eyes were important. The result of this care and teamwork was that we made fewer mistakes than the much bigger engineering teams of the offerors.

The Phase II AAH proposals submitted by Bell and Hughes included design modifications to overcome performance shortfalls identified in the GCT. The specification requirement for cruise speed was 145 to 175 KTAS. We used preliminary AEFA flight test data to calculate GCT cruise speed. It was 141 KTAS for the Hughes YAH-64, and the Phase II proposal included modifications to overcome small performance shortfalls so 145 KTAS seemed possible. We calculated 122 KTAS cruise speed for the Bell YAH-63. This large GCT performance shortfall was a surprise to the SSEB. Jim O'Malley did the original calculations for YAH-63 and triple checked them. I checked the calculations, Ron Gormont (Tech Area Chief) checked the calculations, and Dick Lewis (Deputy Chairman) checked the calculations.

While we continued to work on the YAH-64 evaluation and on other YAH-63 performance issues, YAH-63 cruise speed now consumed most of our effort. A modified Bell YAH-63 Phase II proposal soon arrived, and it included rotor airfoil changes intended to improve high Mach airfoil performance at the expense of stall margin. We needed help to assess the impact of the new airfoils. Dr. Carlson put me in touch with Gene Bingham at Langley Directorate who could test the airfoils in the pressurized, slotted 6- x 28-Inch Langley Transonic Wind Tunnel, eliminating the need for both Reynolds number and transonic corrections. Starting in September, the Army-NASA team was able to test both the GCT and the Phase II airfoils, and provide test results and complementary analytical work to the SSEB on 29 October 1976.

Airframe aerodynamics (both lift and drag) was the area with the largest potential YAH-63 cruise power reduction. A series of 1/6th-scale low-speed wind tunnel tests was the basis for this important change. Bob Stroub, an Army engineer at the NASA Ames 40- by 80-Foot Wind Tunnel, had done airframe aerodynamics wind tunnel testing on a wide variety of interesting

aircraft programs; his experience made him the ideal engineer to observe the Bell YAH-63 low-speed wind tunnel testing. Dr. Carlson was able to make this happen for the SSEB. The result was a good understanding of the wind tunnel data. The SSEB assessed about 10 percent less drag reduction between GCT and Phase II aircraft than Bell.

The Army decided to replace tube-launched, optically tracked, wire-guided (TOW) anti-tank missiles with Hellfire anti-tank missiles, which involved a significant change in drag. Dr. Carlson facilitated wind tunnel testing of TOW and Hellfire missiles and launchers in the Langley V/STOL Wind Tunnel to measure the drag increase. Bob Stroub again coordinated the effort. The wind tunnel test data was delivered to the SSEB in November 1976 and was used for the final evaluation. The testing evaluated missile launcher fairings to reduce Hellfire system drag and showed potential for significant drag reduction. The missile developers were not interested in modifying their just awarded contract to reduce armed AAH system drag and nothing changed.

The C81 Rotorcraft Flight Simulation was a large and complex computer program (developed by Bell under an AVLABS contract) that contained a variety of different math models for the same phenomena. The SSEB guidelines for C81 runs used to substantiate a technical proposal were formalized in a KO letter. Bell submitted a number of C81 runs to the SSEB during the evaluation. The SSEB found so many errors in the C81 input data for these runs that the results were useless. Bell ultimately admitted that they were unable to assemble and substantiate a complete, consistent, and accurate set of C81 input data in the time available.

When Dr. Carlson came to St. Louis for a meeting of the Source Selection Advisory Council, he reviewed the technical details of the performance evaluation. It was a rigorous review with lots of tough questions and some good suggestions. My confidence in our work was significantly bolstered. Charlie Crawford eventually asked the SSEB for a similar review. Dick Lewis was worried about this and asked if I wanted support. I said it was not necessary. Dr. Carlson had given me confidence that we were on solid technical ground. I had heard a lot of stories about Charlie, but had never met him. The technical review was excellent. Charlie was the professional engineer doing his job. It was a relief and a reminder not to worry about myth and rumor.

Charlie brought up the issue of the YAH-63 inlet air temperature rise. Engine power available is a function of the temperature of the air entering the engine, which was measured during GCT. Bell had decided that hot air from around the transmission was leaking into the engine inlets and significantly reducing engine power available. They wanted to very carefully seal the engine inlets from the transmission compartment and measure the inlet air temperature rise again. I assumed all transmission power losses heated the inlet air and used simple thermodynamics to calculate the inlet air temperature rise. The result was a modest increase in temperature. I asked Charlie if additional government flight testing was needed. He said that Bell was becoming concerned that the government was out to get them, and that this was a way to reassure them. This was a new insight for me into the complexities of management and perception.

Bell expressed their concern about the government to the SSEB Chairman, Brigadier General Joseph Jagers, Jr. This resulted in a very large meeting between Bell and the SSEB. The Bell team was headed by their President, and the SSEB team was headed by BG Jagers and Dick Lewis. I was allowed to attend but told to keep quiet. The most technical part of the meeting was Bell insisting that the inlet air temperature rise measured in the planned additional government

flight test be accepted without question. I could not resist stating that if the measured inlet air temperature rise was less than the ideal (adiabatic) temperature rise, I would charge them an accessory loss for a refrigerator. The engineers in the room understood my point, and I did not get in trouble. When the additional government flight test data became available, the hoped-for reduction in inlet air temperature rise was not there, and the whole issue quietly went away.

Accessory losses were a much more serious issue. We had noticed a big difference in the accessory losses between the two offerors and asked Secondary Systems Factor for an explanation. They investigated and said that it seemed to be in the electrical load. The state of the art for rotor blade deicing was electrical resistance heating. This was a very large load, and it was expected to size the capacity of the electrical system. However, running the deicing system on the 35°C (95°F) hot day specified for performance did not make sense. We repeatedly requested a detailed electrical load breakdown from both offerors. Bell had decided that we were looking for data to reduce their performance and ignored our requests. We were concerned that, if we suggested their electrical load was too high, they would agree and create data designed to support improved performance. We were unable to come up with another explanation for the very high electrical load. Finally I called Bell to ask if they were running deicing on a hot day. This resulted in a detailed electrical load breakdown from Bell. Vertical rate of climb (VROC) is very sensitive to power changes, and Bell now met the VROC requirement. Dick Lewis was not pleased, but I believed that it was my call and the right thing to do.

In December 1976, Hughes was announced the winner of the AAH competition. The AH-64 Apache went on to an illustrious career. AMRDL (SRIO, Eustis Directorate, and ASRO) made notable contributions and greatly benefited from the Army-NASA collaboration.

After losing the AAH competition Bell decided to strengthen its engineering capability by hiring Franklin D. Harris from Boeing Vertol. Frank was well known and respected in the rotorcraft technical community. Frank talked to many Bell people about what happened during AAH and wanted to get input from the government. It was agreed that I could talk freely to Frank about Bell. We talked at length one evening, getting deep into the various technical challenges, the limitations of current analysis tools, different types of correlation, the importance of technical substantiation, and the apparent lack of quality control on technical data submitted to the government. Over the next several years Bell invested in improving and correlating their performance methodology [2].

I returned to work at Ames in mid-January 1977 and resumed my duties as the acting ASRO Propulsion Specialist and with ASRO Preliminary Design. I shared my concerns about C81 and comprehensive analysis with Andy Kerr and Dr. Carlson who had initiated an Army effort at the Eustis Directorate to develop an improved comprehensive analysis code called the Second Generation Comprehensive Helicopter Analysis System (2GCHAS). Eventually the 2GCHAS project moved to the Ames Directorate with John Davis and Art Ragosta as the technical core. I visited John and Art to follow 2GCHAS development progress until they left the project. In July 1977 I got the opportunity to observe exercise Brave Shield XVI at the National Training Center in the Mojave Desert. This included 2 days flying as an observer in Army helicopters—8 hours in a CH-54B and 6 hours in a CH-47C. It was a great opportunity to observe real-world Army helicopter operations.

Preliminary Design Team Consolidation and Design Tool Development

The research and development (R&D) functions of AVSCOM were established as a separate command, the Aviation Research and Development Command (AVRADCOM), in 1977. At the same time, AMRDL became the U.S. Army Research and Technology Laboratories (RTL). The four AMRDL directorates became laboratories; the Ames Directorate became the Aeromechanics Laboratory (AL) and the Eustis Directorate became the Applied Technology Laboratory (ATL). AVRADCOM directed that Army Aviation preliminary design activity be consolidated in two locations in July 1977. The Preliminary Design Team (PDT) in ASRO at Ames was responsible for preliminary design methods development, correlation, and conceptual design of emerging aircraft systems including layout, weights analysis, and systems integration. The Design Analysis Team in St. Louis was responsible for maintenance of a performance datum on each current Army Aviation system, for assessing the impact of product improvements and Engineering Change Proposals, and providing data for cost effectiveness and other comparative analysis. Systems and Cost Analysis in St. Louis would provide cost data in support of both groups.

The preliminary design consolidation resulted in three SRIO engineers (Milton Schwartzberg, Dr. Roger Smith, and David Chappell) moving from St. Louis, Missouri, to Ames to join the ASRO PDT. Meanwhile Charlie Crawford got the St. Louis design responsibility transferred to his Development and Qualification (D&Q) Directorate, and he hired Jim O'Malley from ATL. Jim's great performance on the AAH SSEB got Charlie's attention. Charlie offered Jim a well-deserved double promotion to move. The GS-12 happened right away, but the GS-13 took a little longer. Jim brought the Preliminary Design Program code to D&Q, and a tradition of collaboration between Jim and the PDT on appropriate projects developed over time. The remaining preliminary design engineers at Ft. Eustis were reassigned within ATL.

The addition of three experienced engineers and new responsibilities significantly changed the PDT. Dave Chappell assumed responsibility for layout design and structural analysis. He also provided substantial structural analysis and evaluation support to the NASA/Army XV-15 Tilt Rotor Research Aircraft. Milt Schwartzberg started development of a new tilt rotor design code. He also supported and encouraged the preparation of PDT Memoranda for Record documenting design tool methodology. Roger Smith and Milt Schwartzberg worked on aerodynamic performance methodology and provided design support to the Advanced System Technology Integration Office (ASTIO) in St. Louis. In 1977, the new AVRADCOM Commanding General (Major General Story C. Stevens) decided SRIO should be renamed ASTIO and work directly for him. Dean C. Borgman, who had been the Army Deputy Program Manager (PM) for the XV-15 Tilt Rotor Research Aircraft project at Ames, had moved to St. Louis in 1975 to lead SRIO. Two years later he was working directly for the AVRADCOM Commanding General.

After the HELCOM experience, Ron Shinn and I concluded that both PSDE and HESCOMP needed substantial development, including better math models and better technical substantiation, and that PSDE was a better starting point for future code development. The top priority was better math models and better technical substantiation. More flexible mission analysis was needed to address Joint Service mission profiles, and improved productivity for batch runs of design and off-design cases was essential.

The new design code included improvements in flight performance modeling, output depth, mission analysis, parametric weight estimation, design sensitivity studies, off-design cases, and

coding style. Flight performance modeling improvements included better math models and generalizing the equations so that alternate math models could be chosen by input. Mission analysis was generalized to model Joint Service missions that included substantial altitude changes. This required more complex climb, descent, loiter, dash, and reserve mission segment models. It also required a better atmosphere model and a more flexible engine model. Ron Shinn had coded a multiple linear regression algorithm for the development of weight equations. We used this code to develop and substantiate new parametric weight equations, including the development and validation of a database of actual component weights and design parameters.

At this stage it was simpler to split the design code development into parallel efforts to develop two specialized codes rather than a single generalized code. The two codes were Helo (originally called PDPAC) and TR. Helo involved a major cleanup that was only applicable to single-main-rotor helicopters; the ability to design compound helicopters with a wing and/or auxiliary propulsion was eventually added. The second code, TR, was only applicable to tilt rotor configurations. Much aerodynamic performance methodology development was needed to provide detailed level-flight power required versus airspeed at all airspeeds including conversion.

Development of the improved design code was far from complete in August 1976 when I was assigned to the AAH SSEB for 5 months. The SSEB experience demonstrated the need for a better Air Vehicle Technical Description (Aeronautical Design Standard-10 (ADS-10)) to provide accurate design data for analysis, better analysis configuration control, and much better analysis codes. This indicated the need for the PDT to collaborate with aeromechanics researchers who would develop what became known as comprehensive analysis codes.

Advanced Scout Helicopter—My First Concept Formulation

The Advanced Scout Helicopter (ASH) was intended to replace the Army's existing Light Observation Helicopter (LOH) designs, the Bell OH-58 Kiowa and the Hughes OH-6 Cayuse. The Army had studied an ASH off and on for some years. In 1977 the ASH requirement became serious, and Charlie Crawford was now leading the technical effort. Dr. Carlson assigned me to work with Charlie on ASH; I was skeptical at first but it turned out to be a very good partnership.

The Army Aviation Rationalization, Standardization, and Interoperability (RSI) review early in 1978 focused on the ASH requirement. The RSI review included a congressionally mandated contractual study effort with the North Atlantic Treaty Organization (NATO) helicopter industry. This resulted in design studies by the French, German, and Italian helicopter companies. Jim O'Malley and I were co-authors of the Statement of Work and co-technical monitors for these NATO ASH design studies. The design studies were modifications of existing baseline designs so they included detailed descriptions of those baseline designs. There was a midpoint review of the NATO ASH design studies in May 1979 at the Abrams Complex in Frankfurt, West Germany, and Charlie Crawford brought Colonel Walter Rundgren, Jim, and me to the review.

The ASH Special Study Group (SSG) was established in August 1978 as part of the ASH Concept Formulation. The user-defined ASH Required Operational Capability (ROC) was the foundation for the ASH SSG effort. Requirements included a Mast Mounted Sight (MMS), twin engines, signature suppression, a night vision system, and performance, maneuverability, crashworthiness, ice protection, and ballistic protection similar to the AAH. The developer (Army Materiel Command) prepared the Trade-Off Determinations (TODs) that defined materiel

solutions (new development and existing aircraft) to address the ROC. The user (Army Training and Doctrine Command, (TRADOC)) prepared the Trade-Off Analysis (TOA), the Best Technical Approach (BTA), and the Cost and Operational Effectiveness Analysis (COEA). The TOA was a user analysis of the TOD defined solutions. COEA cost analysis and effectiveness modeling of the TOD defined solutions that informed and supported the BTA. This process was essential to get support from Army and DoD senior leadership and funding from Congress.

Charlie was developer lead for the ASH SSG effort and pulled together work from the entire technical community. Our ASRO PDT (Mike Scully, Ron Shinn, Roger Smith, and Dave Chappell) was responsible for the design of new aircraft development alternatives, and the D&Q design analysis team (Jim O'Malley, Harold Sell, and Gene Heacock) was responsible for the design of modifications to existing aircraft (e.g., OH-6 and OH-58), the "mod" designs. We helped each other by reviewing designs and suggesting improvements. Air vehicle designers need lots of input data including mission equipment package and air vehicle equipment, armor density, advanced composites factors, technology factors, and engine characteristics. Much of this data came to us through D&Q, but air vehicle and engine data came from the RTL Labs.

Weights are fundamental to evaluating the impact of design and requirements trades on air vehicle size, weight, and cost. Major ASH design trades included single versus twin engine, side-by-side versus tandem crew seating, and armor protection levels. Weight statements are the data needed for the development of statistical weight equations. The PDT had a good collection of weight statements, and Charlie allowed me to copy D&Q weight statements as part of our ASH partnership. Ron Shinn and I developed improved statistical weight equations and the Advanced Technology Factor technique that modified statistical component weights to account for crashworthiness and survivability requirements, advanced composites, and other rotorcraft technologies. Ron published a detailed technical paper in 1981 [3].

The ASH TOD included technology meetings at St. Louis, Missouri, and Ft. Eustis, Virginia. The PDT was working hard on the Helo design code development, especially better weight estimation. From December 1978 through April 1979 the ASH TOD air vehicle design effort included several meetings with D&Q in St. Louis to develop a briefing, followed by a meeting at Ft. Rucker to present emerging results of the TOD effort. These briefings included lots of design trades quantifying the impact of various requirements on new development ASH designs. Charlie was the briefer, and he energized detailed discussions with the user community that guided our design work. The design meetings in St. Louis used PDT design runs for new development alternatives. They also included 3-view drawings by Roger Smith.

The ASH COEA included CARMONETTE, a stochastic battle simulation developed by the TRADOC Systems Analysis Activity at White Sands, New Mexico, and AVWAR, a computer-assisted manual war game developed at Ft. Rucker, Alabama. I visited both TRADOC sites to better understand the inputs to these analyses. Vulnerable areas were analyzed by the Ballistic Research Lab (BRL) at Aberdeen, Maryland. We worked with BRL to define new development ASH design constraints for survivability.

Ron Shinn and I wrote a 1980 American Helicopter Society (AHS) paper [4] describing rotor trade-offs for the new development ASH baseline designs and some 365 variations. The new development ASH designs with side-by-side (SBS) and tandem seating arrangements were bounded by BTA1 (SBS, 1xATE) and BTA2 (Tandem, 2xATE). BTA1 was a lightweight, "no

frills design,” less survivable single-engine vehicle, while BTA2 was the other end of the spectrum, a fully survivable, twin-engine vehicle.

Work on the mod designs continued in parallel with the new development effort. In March 1979 Charlie led technical meetings at Bell to review OH-58, UH-1, and AH-1 mods. These were large meetings with a team of Army technical experts from D&Q and AMRDL. Airframe structural modifications needed to operate the OH-58 at higher weights were covered in depth. An ATL-sponsored effort to fly an MMS on the OH-58C was reviewed. In June 1979 we evaluated the results of the NATO ASH studies at St. Louis and Ft. Rucker. The Agusta A129 required the least modification and became the best NATO alternative. Hughes 500 MD and Sikorsky S-76 mods were also evaluated. All of this became part of the TOD.

The Army Systems Acquisition Review Council (ASARC) was the top-level Army review body for major acquisition programs. The ASARC met on 30 November 1979 to review ASH. The Technical Alternatives briefing included Near Term Alternatives (OH-58D, 500 MD, OUH-1, AAH/TADS, and OAH-1) and Long Term Alternatives (New Developments, Mod Approaches, and AAH with MMS). New Developments were BTA1 and BTA2 while Mod Approaches were Mod NATO (a modified A129) and Mod U.S. Commercial (a modified S-76). ASARC decided that only the Near Term Alternatives were affordable and could meet a Congressional Initial Operational Capability date (31 December 1984). The Near Term Scout Helicopter (NTSH) was approved and soon became the Army Helicopter Improvement Program (AHIP).

Charlie organized an AHIP Request for Proposal (RFP) preparation effort led by Dan Schrage. I made two trips to St. Louis to help Jim O’Malley support the AHIP RFP work. The AHIP competitors were Bell with a 4-blade modification of the OH-58 and Hughes with a modified OH-6. The AHIP SSEB started in April 1981, and Jim O’Malley was Performance Factor Chief. I spent 2 weeks in Granite City, Illinois, as a consultant, but Jim had the situation well in hand and I was not needed again. The Bell proposal led by Frank Harris was a professional effort and won the AHIP competition to become the Army OH-58D Kiowa Warrior [5]. The Hughes proposal effort struggled. It is human nature for the winners of a competition, in this case Hughes and the AAH, to become over-confident for the next competition and not work as hard. Meanwhile the losers of a competition (Bell) learn from their mistakes and work all-out on the next competition.

The ASH experience developed a partnership of the PDT and the D&Q design analysis team for Concept Formulation. Led by Charlie Crawford, we carried new development and mod aircraft alternatives to an ASARC decision. The mod alternative was carried through RFP preparation and source selection. This experience laid the foundation for JVX Concept Formulation.

Team Building and Tool Building

In October 1978 we hired Henry Lee from Harry Diamond Lab. We needed better engine models, and Henry became the PDT turboshaft engine model engineer. I put together a Turboshaft Engine Math Model that used referred parameters to model power, jet thrust, mass flow, and fuel flow as functions of operating condition. This math model eventually became the Referred Parameter Turboshaft Engine Model in the present-day NASA Design and Analysis of Rotorcraft (NDARC) conceptual design tool [6]. Henry obtained engine models from engine manufacturers and ran them to develop data for referred parameter curve fits for a wide variety of turboshaft engines.

In the summer of 1978 we recruited Tex Jones from the Langley Directorate to work aerodynamics and performance for the PDT. The need for an improved ADS-10 defining Air Vehicle Technical Description data for Army aircraft was an important lesson learned from the AAH SSEB. D&Q was officially responsible for ADS-10. I believed that we could develop a better product and assigned the job to Tex. This was much more challenging than I expected, but provided a solid foundation for the JVX ADS-10. Tex also supported 2GCHAS in the beginning and did some PDT testing of the original NASA Comprehensive Analytical Model of Rotorcraft Aerodynamics and Dynamics (CAMRAD) software [7]. Tex was a member of the NTSH SSEB in 1981 with Jim O'Malley. California real estate prices caused Tex and his wife, Lori, to return to Langley in the summer of 1987, where he continued to be a valued member of our Army-NASA rotorcraft technical community for many years.

I became a GS-14 Team Leader in June 1979. The new responsibility was a learning experience. We had access to various internship programs through NASA. Tex Jones set up a co-op program with Cal Poly, San Luis Obispo, and Mac Dinning joined the PDT as a summer co-op in 1979 and 1980. He became a full-time member of the PDT after he graduated in 1981. Mac left the PDT in August 1984 for the Directorate for Advanced Systems (DAS) in St. Louis. He had an adventuresome career including industry (McDonnell Douglas Helicopter Company (MDHC), Mesa, Arizona), D&Q in St. Louis, Aviation Applied Technology Directorate (AATD) (formerly the Applied Technology Laboratory), and DA in the Pentagon. Mac became the survivability expert for Army Aviation Science and Technology. After retiring from federal service, he has returned to Ames to work for the Concept Design and Assessment (CD&A) Focus Area as a contractor.

Charlie Ingalls joined the PDT in August 1982. He initially worked with Dave Chappell on wing fold design for the JVX tilt rotor. After Ron Shinn left for St. Louis, Charlie took responsibility for PDT weights data and statistical weight equations. He eventually moved from the PDT to technical staff work on survivability, crashworthiness, and logistics. Charlie had a love of flying and joined the 129th Rescue Wing of the California Air National Guard as a second job. He became a C-130 navigator and had many adventures on deployments around the world before retiring as an O-6 after 30 years of commissioned service. Today he is the Administrative Officer and Security and Emergency Manager for the Aeroflightdynamics Directorate (AFDD).

ASTIO in St. Louis eventually became the Directorate for Advanced Systems (DAS). After the ASH ASARC selected the NTSH alternative for the AHIP, the next new development alternative became the Long Term ASH. Roger Smith moved back to St. Louis in December 1979 to design Long Term ASH alternatives for DAS. This effort evolved into the beginnings of what would become the Light Helicopter Family (LHX). Ron Shinn joined Roger at DAS in 1981, and they both came out to Ames to support the JVX Joint Technology Assessment (JTA) in early 1982. After JVX contract award in early 1983, both DAS and the PDT supported LHX. Roger and Ron were responsible for helicopter and compound helicopter designs while the PDT developed tilt rotor designs (with NASA Ames support). In September 1985 Roger and Ron left St. Louis to work for Dean Borgman at MDHC in Arizona.

Dave Chappell took over TR code development after Milt Schwartzberg became ill. Dave developed improved methods to size tilt rotor wing structures and weight. This methodology was eventually documented for JVX. Dave was also responsible for PDT layout design and structural analysis plus substantial technical support to the NASA/Army XV-15 tilt rotor.

Because of our increasing number of programs and the impending emergence of the JVX, we needed reinforcements. Dr. Carlson turned to NASA Ames and hired Rick Peyran who was doing mechanical design for the space program. Dave Chappell trained him in aircraft layout design and structural analysis. Rick quickly became invaluable and replaced Dave when he retired some years later. Jeff Bowles came from NASA systems analysis and took over TR code development from Dave, continuing to develop and run it through JVX. Jeff eventually returned to NASA to work on the National Aero-Space Plane (NASP).

Vertical Takeoff and Landing Aircraft

The helicopter is the original VTOL aircraft. It is wonderful for hover and low-speed flight, however its cruise speed and aerodynamic efficiency are much less than airplanes. This has resulted in the development of a great many VTOL concepts that attempt to combine airplane cruise speed and aerodynamic efficiency with VTOL capability. By the early 1980s, the only VTOL aircraft other than the helicopter to enter production and service was the Hawker Siddeley Harrier, a single-engine jet lift design with rotating nozzles that was in service with the Royal Air Force, Royal Navy, and U.S. Marine Corps (AV-8A).

AMRDL's contribution to Army interest in the tilt rotor VTOL really began when an agreement between NASA and the Army was signed on 1 November 1971. The agreement called for Joint Development and Operation of a Tilt Rotor Proof-of-Concept Research Vehicle at Ames. This NASA/Army agreement provided resources (engineers and funding) to lay the technology foundation for the XV-15 Tilt Rotor Research Aircraft. The XV-15 laid the foundation for development of the Marines/Air Force V-22 Osprey tilt rotor as the third type of VTOL aircraft to enter production and service.

The technology foundation for the XV-15 program included design and testing of full-scale rotors mounted on cantilever semispan wings in the Ames 40- by 80-Foot Wind Tunnel. Two competitors (Bell and Boeing Vertol) were selected for development of this groundbreaking VTOL aircraft. The Bell Model 300 had 25-foot-diameter gimbaled, stiff-inplane rotors and engines that tilt with the rotors. The Boeing Vertol Model 222 had 26-foot-diameter hingeless, soft-inplane rotors and engines that did not tilt. Wayne Johnson developed and correlated with wind tunnel test data an analytical model for the dynamics of both proprotor designs [8]. The NASA/Army program conducted a competition between these two technically viable tilt rotor solutions and selected the refined Bell Model 301 to become the XV-15.

The NASA/Army XV-15 was very successful technically and as a concept demonstrator [9]. In June 1981, the XV-15 got the attention of the aeronautical world at the Paris Air Show with a carefully choreographed daily flight demonstration. In his excellent book, *The Dream Machine*, Richard Whittle quotes the *New York Times*: "...the Bell XV-15...the hit of the show" [10]. Secretary of the Navy John Lehman and Senator Barry Goldwater were both very impressed by the flight demonstration and tilt rotor technology.

The XV-15 also impressed the pilots who flew it. The first Navy evaluation flown in May–June 1980 and reported in December 1980 was very favorable and recommended additional evaluations as the envelope was expanded. In October 1981 Senator Barry Goldwater became the first non–test pilot to fly the XV-15 as part of the Bell guest pilot program. He said, "The tilt rotor is the biggest advance in aviation in a quarter of a century." A January 1982 Aviation Week

Pilot Report [11], and a March 1982 flight by Secretary of the Navy John Lehman were also significant. As described by Hans Mark in his Foreword, and the chapters by Marty Maisel, Irv Statler, and others in this memoir, the XV-15 tilt rotor was perhaps the most significant development program to come out of the Army-NASA collaboration at Ames.

Joint Services Advanced Vertical Lift Aircraft (JVX)

The Army-NASA collaboration at Ames that produced the XV-15 demonstrated how effective combining funding and engineers can be when technology needs to be advanced in one giant step. The XV-15 provided enough confidence for the Department of Defense to launch the Joint Services Advanced Vertical Lift Aircraft (JVX) as the first step towards the Marines/Air Force V-22 Osprey. Less well known is the important and influential role that the Army-NASA team played in the early stages of the JVX program. I count myself as very lucky to have been a part of this major development program at the beginning.

Dick Ballard was Chief of Technology for Aviation Systems in the Weapons Directorate of the Army Deputy Chief of Staff for Research, Development, and Acquisition. In October 1981 Dick Ballard called Dr. Carlson to discuss a memo from the Under Secretary of Defense for Research and Engineering, Dr. Richard DeLauer, to the Navy and the Air Force about consolidating V/STOL and helicopter development programs into a single joint rotary-wing development program. Army leadership was considering entering the discussion. Dr. Carlson and I suggested that the Army should be responsible for subsonic VTOL/V/STOL aircraft with 50 psf or less disc loading. The 50-psf disc loading was chosen to include tilt wing aircraft such as the XC-142.

Whittle [10] describes a 24 September 1980 meeting of Marine General P. X. Kelley and Vice Admiral Richard Seymour with the Secretary of the Navy, John Lehman. The subject was a CH-46 replacement for the Marines. Lehman said, "I am not going to spend 2 billion dollars of non-recurring cost to evaluate a new helicopter." Lehman continued, "I want to bring the Marine Corps into the twenty-first century on the leading edge of technology and that leading edge is tilt rotor." This background was unknown to us at Ames.

During the last week of 1981, Dick Ballard called Dr. Carlson to report that the Deputy Secretary of Defense was about to send a memo to the service secretaries announcing the formation of a new joint rotary-wing development program. There was a strong indication that the new aircraft should be an advanced VTOL aircraft such as a tilt rotor and that the Army would be named as the executive service. This program was to address the Marine Corps replacement assault transport helicopter (HXM) requirement and several others. The HXM was intended to replace the Marine CH-46 performing the Medium Assault mission. The relevant Capability Group 3 of the 1975 HELCOM study included Navy and Marine CH-46s, and Navy and Air Force H-3s, but no Army helicopters (the Army UH-60s, AH-64s, and CH-47s were in Groups 2 and 4). This implied to us that the Army was named lead service because of its technical background and experience rather than a need for aircraft of this CH-46 size.

The HXM was expected to be based on, and operate from, U.S. Navy Landing Helicopter Assault (LHA) ships. Only the Navy had qualified aircraft to operate from Navy ships. Any effort to shipboard-qualify a new joint rotary-wing aircraft would need partnership with Navy qualification authorities. This central challenge meant that Charlie Crawford, as the Army's D&Q authority, was the best Army technical leader for the program. Dr. Carlson and I called

Charlie at home to discuss the challenge and the opportunity. Charlie agreed to lead the effort starting with a JTA at Ames followed by an RFP Task Force at AVRADCOM. He agreed to spend at least 2 days a week on the effort, and he rightly said that the JTA would need to convince him of the best VTOL aircraft configuration for the program.

The next step was to get support from the AVRADCOM Commander. Major General Story Stevens and the AVRADCOM Technical Director (Dick Lewis) were scheduled to visit Ames the first week of January 1982. Charlie flew out to Ames for the visit. Dr. Carlson and Charlie met with Major General Stevens to discuss the opportunity and the proposed approach. He agreed with the proposal and JVX was launched.

Charlie led the developer side of JVX. The user side of JVX, the Joint Services Operational Requirements (JSOR) working group, was headed by Navy Captain Jim Magee. Charlie was effectively the PM for several months before the official PM was appointed. His responsibilities included planning JVX program schedules, engine alternatives, and risk reduction activities (wind tunnel and simulation programs) in addition to the JTA and RFP preparation. Charlie also started working with NAVAIR to develop a plan for JVX shipboard qualification.

JVX Joint Technology Assessment

The JVX Joint Technology Assessment (JTA) was intended to assess the technical feasibility of developing a common family of VTOL aircraft capable of performing all of the DoD JVX missions, propose trade-offs and trade-ups to the JSOR designed to maximize commonality between the various members of the family, and prepare the technical foundation for an RFP for Engineering Development of a JVX aircraft system. The JTA was specifically not intended to set requirements, select a design configuration, or define a program management plan. Part of the JTA team continued working on JVX after the completion of the formal JTA study to provide technical support to AVRADCOM for JVX RFP preparation.

The JTA was an approximately 3-month technical effort held at NASA Ames from February to May 1982. The JTA involved many studies encompassing requirements, cost, mission equipment, and all of the key aeronautical technology disciplines and vehicle configurations. Specialists were assigned from all of the services and NASA. Charlie was the Director, I was Deputy Director Technical, and Marine Lieutenant Colonel Greg McAdams was Deputy Director Requirements. Requirements and Cost (Lieutenant Colonel Greg McAdams) included User Requirements (Major King), Mission Equipment (Lieutenant Colonel Bob Yeend), and Cost (Ralph Tate). Tech Support (Andy Morse) included Tech Factors and Weights (Ray Foye), Propulsion (Dennis Enders), Layout Design (Dave Chappell), Rotor/Aero (Wayne Johnson), and Fly by Wire (Bill Stevens). Configurations (Dennis Earley) included Helo (Jim O'Malley), Compound and Advancing Blade Concept (ABC) (Roger Smith), Tilt Rotor (John Magee), and Lift/Cruise Fan (Sam Wilson). A total of 65 people (some part time) were assigned to the JTA (35 Army, 12 Navy, 3 Air Force, 4 Marines, and 11 NASA). Dennis Earley from D&Q was Charlie's "official spy," which was great because he took care of reporting to Charlie.

JVX was intended to address Army, Navy, Marine Corps, and Air Force mission needs. The Army Special Electronic Mission Aircraft (SEMA) included a high altitude (30,000 feet) Corps Signals Intelligence (SIGINT) mission and a lower altitude (7,000 feet) Division SEMA mission. The Marine Corps HXM included Amphibious Assault and Land Assault missions for both Troop

Lift and External Cargo. There were both Navy and Air Force Combat Search and Rescue (CSAR) missions. The 700-nm-radius Air Force Long Range Special Operations Forces (LR SOF) mission was very challenging. In April 1980 a very complex mission using RH-53D helicopters and C-130 transports was mounted to resolve the Iran hostage crisis by rescuing 52 embassy staff. The mission failed and became known as Desert One. The potential for an advanced VTOL to perform a mission like Desert One without staging was obvious to the technical community. We assumed that the LR SOF mission was intended to address that need. J VX was required to self-deploy worldwide without refueling. The longest over-water leg was west coast to Hawaii (2,100 nm with 85th percentile, headwinds vs. altitude).

The J VX JTA effort was divided into three partially overlapping phases comprising technical foundations (technology review and synthesis model refinement), a first-cut assessment of the missions (point design studies), and development of a common design for all missions (commonality design studies).

The VTOL configurations studied during JTA and addressed for the point designs included single-main-rotor helicopters (with and without auxiliary wings), auxiliary propulsion compounds (winged and ABC versions), tilt rotors, and lift/cruise fan aircraft. Tilt rotor designs were optimized at the upper boundary of allowable disc loading (constrained by shipboard compatibility to about 17 psf). The higher disc loading (typically 50 psf) tilt wing was not studied. The very high disc loading (500 psf) lift/cruise fan concept was included by management direction rather than on technical grounds.

J VX JTA technical foundations. The technical foundations of the J VX JTA included a review of the impact of technology and special requirements on design, plus refinement of design synthesis modeling tools. The technology considered included rotor aerodynamics, advanced composite materials, fly-by-wire flight controls, and advanced drive train components. The special requirements considered included crashworthiness; pressurization; ballistic tolerance; nuclear, biological, and chemical protection; bird strike; signature reduction; and flotation and folding. The technology review identified and documented the level of technology representative of the then current technical state of the art and ensured that it was consistent for all of the configurations under consideration. The consistent technology rule was relaxed during the study to allow higher risk technology for configurations that proved to be least suitable for the J VX missions. This ensured that these configurations were not limited by possibly conservative technology inputs. The tools used for assessing the impact of Joint Service mission requirements on J VX designs were aircraft design synthesis computer models and layout design studies.

The J VX JTA design studies used three different engines: T64-GE-416, T64-GE-418, and the Modern Technology Engine (MTE). The T64-GE-416 was a production turboshaft engine in the 4,000-shp class. The T64-GE-418 was a potential growth version in the 5,000-shp class. The MTE was a new development engine in the 5000-shp class, with turboprop and turboshaft versions. Henry Lee obtained official engine math models for these engines and ran them to develop data for referred parameter curve fits. These inputs were used by the Turboshaft Engine Math Model in our design synthesis tools. Turboshaft engine installation losses were carefully modeled including inlet duct, inlet air particle separator, infrared (IR) suppressor, and exhaust.

The J VX rotor aeromechanics technology (led by Wayne Johnson) used both test data (small- and large-scale wind tunnel tests plus flight tests) and CAMRAD calculations to develop

advanced technology rotor performance data. This data was used to calibrate our design synthesis computer models. CAMRAD was the state-of-the-art rotorcraft analysis code that was not available for the AAH SSEB back in 1976.

The high-speed helicopter rotor technology assessment used production helicopter (Sikorsky S-76) full-scale wind tunnel test data for CAMRAD calibration. Small-scale wind tunnel data was used to extend CAMRAD correlation beyond available full-scale test data. CAMRAD was used to change the full-scale data from production airfoils to advanced airfoils, and to predict the effects of changes in solidity, twist, and propulsive force on rotor performance in both forward and vertical flight. Compound helicopter trim was based on Lockheed AH-56A Cheyenne data.

The ABC rotor technology assessment used data from the XH-59 ABC Technology Demonstrator program. The XH-59 demonstrated developing lift primarily on the advancing blades of a rotor system to improve lift at high speed and maintain airspeed at altitude. Limited XH-59 flight and wind tunnel performance data was available. Bill Pleasants (ATL) modified CAMRAD to model the ABC rotor. He correlated CAMRAD with ABC flight and wind tunnel test data, and modeled the effects of different operating conditions and planform, twist, taper, and airfoils to predict the performance of advanced designs.

A Sikorsky HMX rotor design was selected as the basic configuration for an advanced ABC design. JVX advanced ABC performance estimates were based on both demonstrated capability achieved in flight test, and assumed solutions to problems encountered by the XH-59. The advanced ABC design showed improved performance relative to the XH-59. The advanced ABC rotor did not have the ability to hold performance up to 30,000 feet as desired for some JVX missions. Significantly increased rotor solidity or a wing would be required.

Rotor aeromechanics technology for tilt rotor design used XV-15 full-scale wind tunnel test and flight test data for CAMRAD calibration. CAMRAD was used to change to advanced helicopter rotor airfoils, and to predict the effects of changes in solidity on rotor figure of merit and propeller efficiency. Aeroelastic stability for the tilt rotor was analyzed using CAMRAD to define the wing stiffness required for aeroelastic stability. The JVX rotor system was assumed to be a gimbaled, stiff-in-plane design like the XV-15. Aeroelastic stability boundaries were calculated for the XV-15 and for 10 possible JVX tilt rotor designs. A dimensional analysis developed by Dave Chappell was used for JVX tilt rotor wing weight estimation.

Airframe aerodynamics technology addressed fuselage, wing, and nacelle lift and drag. Airframe drag was an engineering culture issue for JVX. We were moving from helicopters cruising at fixed-landing-gear biplane speeds to advanced VTOL configurations cruising at retractable-landing-gear monoplane speeds. Parasite drag varies as airspeed squared, and parasite power varies as airspeed cubed. The trade-offs of drag vs. weight, complexity, and cost change dramatically. Jim O'Malley led a comprehensive JTA parasite drag effort. Fuselage drag for rotor-borne concepts (helicopter, compound, and ABC) was modeled by a parasite drag area vs. fuselage angle-of-attack trend for similar size fuselages. Wing-induced drag for wing-borne concepts was modeled by fixed-wing Oswald span efficiency. Wingspan extensions could be added to tilt rotor designs outboard of the engine nacelle, for missions requiring long range or high altitude performance. IR suppressor momentum drag was modeled correctly (not as parasite drag). Hover download was estimated conventionally for helicopter, compound helicopter, and

ABC. John Magee (NASA) led a careful study of tilt rotor download. A retracting leading-edge slat was added to the wing of the JVX tilt rotor to reduce download.

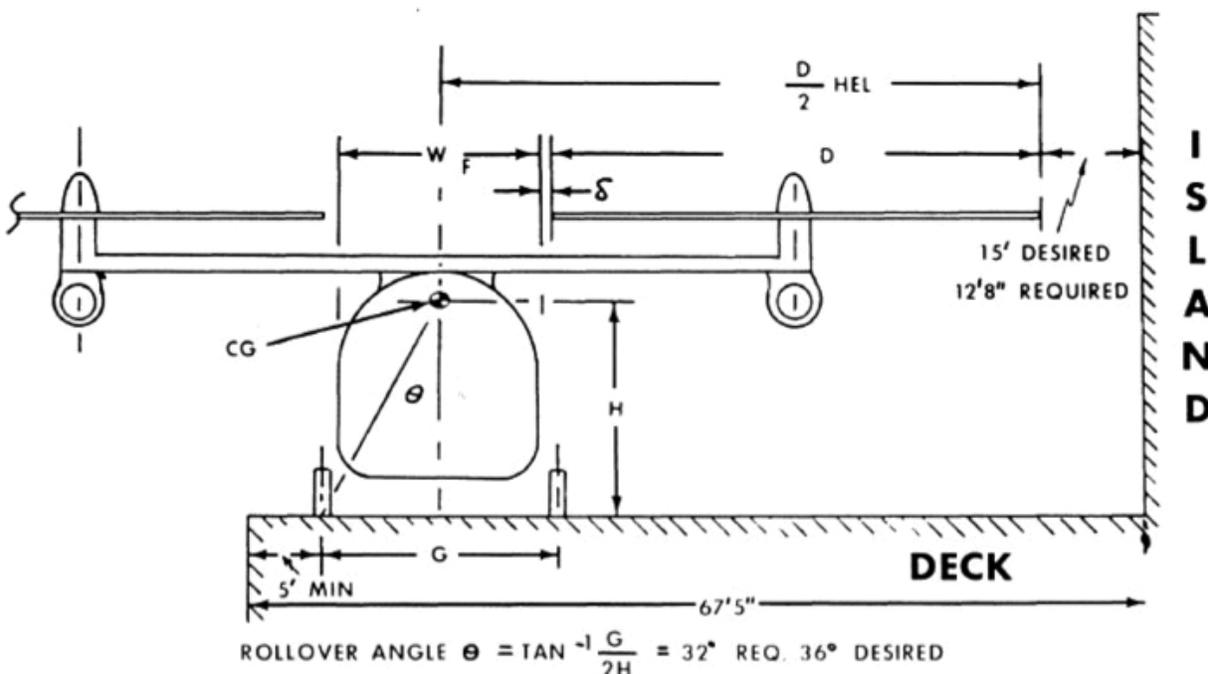
The rotorcraft and V/STOL design codes used for JVX JTA were the result of a continuing in-house preliminary design methodology development effort by the Army and NASA. The JTA required substantial modifications to these codes to accommodate the special requirements of the various services, to develop service-specific variants from the baseline design, and to accurately represent advanced technology projected for the JVX. The Helo design code was used for helicopter and compound helicopter designs, a modified version of the Helo design code was used for ABC designs, and the TR design code was used for tilt rotor designs. Lift/cruise fan designs were based on VASCOMP (a code developed by Boeing Vertol for NASA).

The flight performance models in the rotorcraft design codes were based on simplified physical models of the aircraft, rather than curve fits or tables. Calibration factors were used to match these math models to test data or more detailed analysis tools. This had the great advantage of providing a physically realistic way of interpolating and extrapolating from limited available data, and was essential for JVX where a wide variety of design configurations were considered. The exception was curve fits used for ABC because of limited time.

Preliminary design requires both design synthesis computer codes and layout design. Design synthesis codes determine the size, component weights, and flight performance of the resulting aircraft design, but these codes are not able to evaluate operational considerations and requirements such as operating clearances, crew visibility, cargo handling, troop loading/unloading, fields of view/fire, and shipboard compatibility. Layout design is used to address these characteristics and to provide configuration and geometry inputs for the design codes. Since layout design drawings are based, in part, on dimensions determined by the design codes, preliminary design becomes an iterative process between design codes and layout design.

Shipboard compatibility with LHA and Landing Helicopter Dock (LHD) class ships was a key driver for JVX layout design, particularly the limitations on rotor diameter and wingspan. The design parameters for shipboard compatibility are shown in the figure on the next page. The JVX had to be able to operate from spots abeam the LHA/LHD island superstructure with rotor-tip clearance no less than 12 feet 8 inches with the wheels 5 feet from the deck edge, and the rollover angle had to be at least 32 degrees. JVX folded/stowed size on LHA could not exceed a spot factor of 1.2 times the CH-46E helicopter. JVX spot factor was defined by the maximum number of folded/stowed CH-46Es divided by the maximum number of folded/stowed JVXs.

The JVX designs needed the largest possible rotor diameter and wingspan for aerodynamic efficiency. Landing gear track (G) for a 32-degree rollover angle was minimized by choosing a quad gear design and lowering the CG height (H) as much as possible. The Marines operate the CH-53E helicopter (79-foot rotor) from spots abeam the LHA island. Thus, single main rotor or co-axial rotor JVX designs no larger than 79 feet with quad landing gear should be acceptable. The JVX was required to accommodate 24 troops for the Marine Assault mission. We minimized JVX fuselage width by stretching the length of the 72-inch-wide CH-46E cabin to accommodate 24 troops. Rotor/fuselage clearance (δ) was chosen to be 18 inches based on aerodynamics and acoustics. This sized the rotor diameter and wingspan for JVX tilt rotor designs.



JVX rotorcraft design implications of LHA flight deck width limits.

NAVAIR had developed a formula to predict the spot factor for helicopters from folded dimensions. A Navy facility at Lakehurst Naval Air Station, New Jersey, used large-scale models of ship decks and folded aircraft planforms to evaluate the official spot factor. The Marines wanted an early evaluation of the spot factor for the JVX tilt rotor, so we sent an initial design to Lakehurst based on the helicopter formula. The official spot factor from Lakehurst was too high, so the helicopter formula did not work for tilt rotor designs. The layout design team led by Dave Chappell went into emergency mode. A 12-foot-long drawing of the LHA flight deck and a drawing of the hanger deck at the same scale were created. Scale drawings of the JVX tilt rotor designs were duplicated in a copy machine and used to replicate the official spot factor evaluation procedure. After a very intense weekend, an improved JVX tilt rotor design was sent to Lakehurst. To our great relief, the official spot factor came back as acceptable.

I got a phone call from Ken Wernicke, project engineer for the Bell JVX effort. He reported that they were having great difficulty with the official spot factor evaluation for their tilt rotor design. I said that we had just passed on our second try and shared our design strategy. When I reported this conversation to our design team, moral went over the top. The government team had come through while industry was struggling. In the end Bell stayed with tricycle landing gear and decided that the tail folding added too much complexity to an already complex configuration. The Bell-Boeing team eventually convinced the Marines to accept a larger spot factor.

Layout design work continued and developed design solutions to a wide variety of problems for the four rotorcraft configurations (helicopter, auxiliary propulsion compound helicopter, auxiliary propulsion ABC, and tilt rotor). A large number of specialized drawings and working sketches were produced during this process. The final JTA layout products were the 3-view drawings of the basic rotorcraft configurations.

JVX JTA point designs. A vehicle designed to meet one specific set of mission requirements is referred to as a point design. A point design analysis was carried out for each of the nine service missions for each of the VTOL configurations selected as JVX candidate concepts. The nine service missions plus all services self-deployment were:

- 1D—Navy combat search and rescue (CSAR).
- 2A—Army Corps SIGNIT (SEMA).
- 3A—Army Division electronic warfare (EW) (SEMA).
- 4B—Air Force combat search and rescue (CSAR).
- 5B—Air Force long range special operations.
- 6C—Marine Corps amphibious assault (troop lift).
- 7C—Marine Corps amphibious assault (external cargo lift).
- 8C—Marine Corps land assault (troop lift).
- 9C—Marine Corps land assault (external cargo lift).
- 10—All services self-deployment.

Some missions required a 250-knots cruise speed that was beyond the capability of some configurations (e.g., helicopter). These slower configurations cruised at Maximum Continuous Power (MCP) to establish fuel requirements. The fallout capability of each point design to perform self-deployment was evaluated. Several candidate engines were studied during the initial point design study phase. A final point design airframe and engine combination was then selected, based on minimum mission gross weight, for each mission.

The helicopter point designs were carried out by Jim O'Malley and Ron Shinn, the aux propulsion compound helicopters were done by Roger Smith and Ron Shinn, the aux propulsion ABC designs by Roger Smith, Ron Shinn and Steve Zalesch, and the tilt rotor designs by John Magee, Jeff Bowles, and Marty Maisel. The lift/cruise fan (LCF) designs were carried out by Sam Wilson, Paul Gelhausen, and Moise Devalier.

The JVX helicopter, aux propulsion compound, and aux propulsion ABC designs had engines sized by takeoff or mid-point hover requirements. Fall-out cruise speed was accepted, although this did not meet some or all mission requirements. Disc loading was chosen for minimum mission gross weight. There was no solution for Mission 2A, because rotor-borne designs could not conduct sustained operations at 25,000 to 30,000 feet at any airspeed, without prohibitive increases in rotor blade area. The helicopter blade area was sized for 1.75g at 150 knots, and a small wing was added to satisfy the high-speed maneuver requirement; helicopter maximum speed was limited by vibration rather than power. The rotor RPM of the compound and ABC designs was reduced in high-speed cruise to keep the advancing blade tip Mach number below 0.85. The compound and ABC designs had higher gross weights than the faster tilt rotor designs.

The JVX tilt rotor designs were sized by the same general mission and performance constraints as the various rotor-borne configurations. Two candidate engines were studied, T64-GE-418 and MTE. The designs were first sized by takeoff or mid-point hover requirements using "rubber engines." Fallout cruise speed exceeded mission requirements in all cases, although it did not meet the desired 300 knots in some cases. Point design performance was recomputed using the actual standard engine size, when this was sufficient to perform the mission. Rotor diameter for Navy and Marine designs was limited to 39.3 feet by shipboard compatibility. Larger rotors

reduced mission gross weight for the Army and Air Force designs. Blade loading was determined by low-speed maneuver requirements. Wing extensions were added outboard of the engine nacelle for missions requiring long range or high altitude performance. The self-deployment mission used a rolling takeoff with gross weight limited by a One Engine Inoperative requirement for 100-fpm rate of climb (30-minute power). Wing loading was selected to minimize gross weight for each point design mission. Hover mode rotor tip speed was selected based on figure of merit, weight, and acoustic signature. Airplane mode rotor tip speed was based on both proprotor cruise efficiency and wing torsional stiffness requirements.

The JVX LCF designs studied two high-bypass turbofan engine cycles, CFM56 and QSCEE. LCF was not suitable for external load missions (7C and 9C) because of very high fuel consumption, high velocity downwash, and limited control power. The poor hover efficiency inherent in the very high disk loading (DL) of the LCF resulted in high mission design gross weights compared to more lightly loaded rotorcraft concepts. However, LCF did provide superior high-speed/high-altitude performance that benefited the Army SIGNIT mission. The LCF team decided that a crashworthy fuel system represented an unfair penalty for the LCF because of very high fuel consumption. I pointed out that over half of the lives saved by crashworthiness were due to the fuel system, and the Marines strongly supported the requirement. The LCF team countered that the Marine AV-8 Harrier did not have a crashworthy fuel system. I said that the Harrier had an ejection seat for its pilot, and if providing ejection seats for everybody onboard (4 crew plus 24 troops) was a good trade for a crashworthy fuel system, then show me the numbers. That was the end of the debate.

The JVX JTA point designs for each mission are summarized in the table on the next page. Data includes rotor diameter, engine (+ indicates a rubber growth version), engine size (power or thrust), mission fuel, and mission gross weight. The Marine missions (6C–9C) are represented by the most difficult Marine mission (8C). The compound helicopter and ABC were somewhat optimistic compared to the helicopter and tilt rotor, because they used rubber engines instead of actual (fixed-size) engines. Even with this advantage, the compound helicopter and ABC were heavier and required larger engines than the helicopter and tilt rotor. The compound helicopter and ABC were better than the helicopter in maximum speed and self-deployment (ferry) range, but still failed to meet the JSOR mission requirements. The ABC and tilt rotor both had higher DL than the helicopter or compound helicopter, a disadvantage in rough field and external load operations. The ABC disc loading was higher than the tilt rotor because the rotor system and inter-rotor shaft weights scale as radius times gross weight. The ABC would be more attractive in a smaller size. The tilt rotor and the LCF easily exceeded the required ferry range. The tilt rotor exceeded maximum speed requirements but not the desired maximum speed (300 knots). The LCF easily exceeded the desired maximum speed. The LCF was heavier than either the helicopter or the tilt rotor for all missions, and it was the heaviest of all the configurations for most missions, primarily because of high fuel consumption. The LCF downwash associated with the disc loading was prohibitive for rough field and external load operations. The helicopter, compound helicopter, and ABC were unable to perform the high altitude Army SIGNIT mission.

Summary of JVX JTA point designs.

Mission	1D	2A	3A	4B	5B	6-9C	Design Notes
Helicopter							
Rotor Dia, ft	73	No Solution	66	78	74	67.8	DL = 10-11 psf
Engine	T64-418		T64-416	T64-418	MTE	T64-416	
Engine Size, shp	5,115		4,113	5,115	5,750	4,113	Vmax = 200 kt
Mission Fuel, lb	16,019		9,166	14,653	22,821	6,612	
Mission GW, lb	48,074		36,674	46,382	58,236	38,390	Ferry = 1,100-1,840 nm
Compound							
Rotor Dia, ft	75	No Solution	65	68	75	66	DL = 12-13 psf
Engine (+ rubber)	T64-418+		MTE+	MTE+	MTE+	T64-418+	
Engine Size, shp	6,413		5,859	5,914	7,596	5,653	Vmax = 220-230 kt
Mission Fuel, lb	18,949		10,648	13,621	27,751	7,834	
Mission GW, lb	57,241		42,828	46,598	70,617	44,957	Ferry = 1,400-1,950 nm
ABC							
Rotor Dia, ft	70	No Solution	64	58	66	58	DL = 15-19 psf
Engine (+ rubber)	T64-418+		T64-418+	MTE+	MTE+	T64-418+	
Engine Size, shp	7,596		6,159	6,521	8,007	5,362	Vmax = 225-240 kt
Mission Fuel, lb	22,704		12,857	16,775	30,042	7,848	
Mission GW, lb	64,944		47,476	49,752	72,172	42,603	Ferry = 1,400-1,900 nm
Tilt Rotor							
Rotor Dia, ft	39.3	44.4	46.6	47.5	49.2	39.3	DL = 12-17 psf
Engine	T64-418	MTE	MTE	T64-418	MTE	T64-418	
Engine Size, shp	5,115	5,750	5,750	5,115	5,750	5,115	Vmax = 270-290 kt
Mission Fuel, lb	12,409	8,122	9,472	11,325	17,240	5,930	
Mission GW, lb	41,423	41,525	42,298	42,512	53,150	39,770	Ferry > 2,100 nm
Lift/Cruise Fan							
Engine (+ rubber)	CFM56+	QCSEE+	CFM56+	CFM56+	CFM56+	QCSEE+	DL = 500-700 psf
Engine Size, lbst	39,608	29,178	43,047	38,939	38,848	25,218	
Mission Fuel, lb	27,435	14,623	25,274	23,354	30,677	9,181	
Mission GW, lb	71,726	49,484	73,096	66,121	79,557	42,713	Ferry > 2,100 nm

JVX JTA commonality designs. For each VTOL configuration, the JTA studies attempted point designs for each JVX mission. As shown on the preceding page, some VTOL configurations were not able to meet all of the JSOR service mission requirements. The commonality design studies were intended to produce a single design that would satisfy the requirements of all of the JSOR service missions for each VTOL configuration.

Since the LCF configuration was not suited to the low altitude, low speed, and hover requirements of the JSOR service missions, the LCF was not considered for the commonality designs. The compound helicopter and the ABC, while better than the helicopter in some respects, could not meet all service mission requirements, and they were heavier and required larger engines than the tilt rotor, which could meet all service mission requirements. Thus the compound helicopter and ABC configurations were not considered for the commonality designs. Although the helicopter could not meet all service mission requirements, it was the lightest solution with the smallest engine for some missions, and it provided a benchmark of a conventional configuration to quantify the price of the extra capability provided by the tilt rotor. The tilt rotor was the only configuration that satisfied all service mission requirements, and it was the lightest solution for three of the missions. Therefore the configurations considered for commonality designs were the helicopter and the tilt rotor.

The helicopter and tilt rotor commonality designs started with the baseline point design for the Marine 8C mission, the most demanding of the shipboard compatible missions. The helicopter and tilt rotor designs were then modified to meet as many of the other mission requirements as possible. Fallout performance for the commonality designs was then computed for all the JVX missions using actual (not rubber) engine sizes. Three candidate engines were considered: T64-GE-416, T64-GE-418, and the MTE. Changes were made to the baseline design airframe equipment weight, mission equipment weight, fixed useful load, and payload, plus other changes as required for each mission. This included tilt rotor wing extension kits outboard of each engine nacelle for missions requiring long range or high altitude performance.

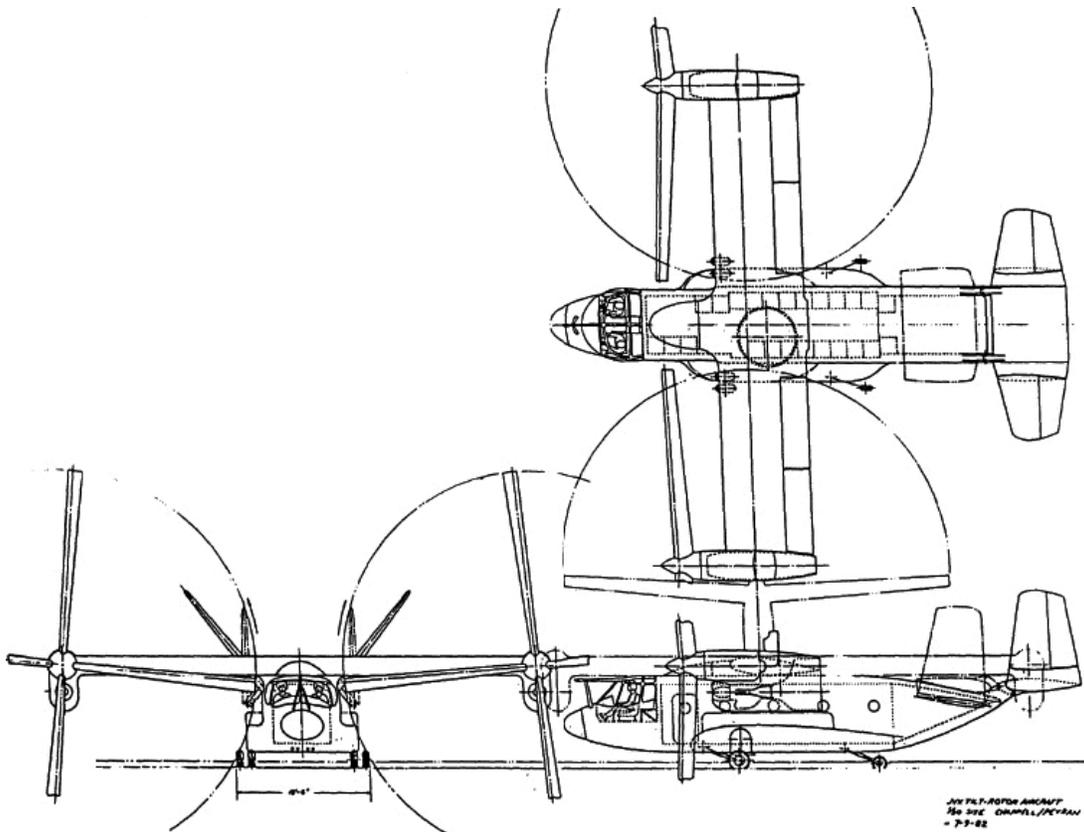
The helicopter could satisfy the Marine (6C–9C) mission requirements, except for dash speed and worldwide self-deployment, using any of the engines considered. The T64-GE-416 engine helicopter could not satisfy any of the other services' mission requirements. The T64-GE-418 could satisfy Army low altitude SEMA mission (3A) and Air Force CSAR mission (4B) requirements, but only partially satisfy Navy CSAR mission (1D) requirements because it was too slow. MTE substantially improved helicopter performance on all missions, but it did not reduce the number of missions that the helicopter could not perform: 1D (speed), 2A (altitude + speed), 5B (payload/radius), 6C (speed), 7C (speed), and 10 (payload/range). The tilt rotor could perform all Navy and Marine missions, including worldwide self-deployment, using the T64-GE-418 engine. The tilt rotor using the MTE could perform all of the JSOR missions, with substantial performance margins in most cases. The most difficult mission, Air Force Long Range Special Operations (5B), had a 50-nm margin over the 700-nm-range requirement.

The commonality studies showed that there was at least one design configuration, the tilt rotor, that could satisfy all of the JSOR mission requirements with a high degree of interservice commonality. The JVX System Specification design demonstrated a feasible design solution.

JVX System Specification Design

The development of the detailed System Specification for the JVX RFP and changes to the JSOR requirements necessitated an update of the JVX JTA tilt rotor commonality design. For example, the structural design load factor was reduced from 6.0 to 4.0, and the maximum gross weight was changed to reflect internal cargo load requirements, self-deployment mission changes, and internal fuel storage requirements. The update was an opportunity to refine the tilt rotor commonality design. The rotor diameter increased from 39.3 to 39.4 feet, dual wheel main landing gear increased weight, and a folding tail reduced drag but increased weight. The folded width increased but the folded length remained the same.

The JVX System Specification tilt rotor was a baseline Marine Mission 8C commonality design with an MTE engine. Fallout performance was computed for the remaining missions. For the Army, Navy, and Air Force missions, wing fuel capacity was increased to fill the available wing volume. Any additional mission fuel needed was carried in external tanks, with added weight and drag. For the Navy CSAR mission, radius with design payload was over 60 percent greater than required. For Army missions, time on station with design payloads was over 15 percent longer than required. For the Air Force CSAR mission, radius with design payload was approximately 20 percent greater than required. The Air Force LR SOF 700-nm-radius requirement was met with a 12-troop payload. For a 24-troop payload, a 450-nm radius was available. The 2,100-nm self-deployment range was exceeded even with heavy Army Corps SIGINT equipment.



JVX System Specification design 3-view.

JVX System Specification tilt rotor design.

Rotors		Propulsion	
No. Blades	4	Engines	2 x MTE
Diameter, ft	39.4	Rated Power, shp	2 x 5,750
Solidity	0.1095	Drive Rating (hover), shp	9,554
Blade Aspect Ratio	11.6	Fuel Tank Capacity, lb	8,303
Tip Speed (hover/cruise), fps	775/550		
		Aerodynamics	
Wings		Rotor Figure of Merit	0.774
Span, ft	50.23	Download, % GW	5.7
Area, sq ft	450	Aircraft Figure of Merit	0.670
Aspect Ratio	5.6	Drag Area (cruise), sq ft	16.8
Thickness/Chord	23%	Propeller Efficiency	0.826
Flap Chord	30%	Equivalent L/D (260 kt)	6.56
Torque Box Chord	45%		
		Baseline Mission (Land Assault)	
Fuselage		Disc Loading, psf	16.6
Length, ft	48.0	Wing Loading, psf	90
Width, ft	7.67	Empty Weight, lb	26,793
Wetted Area, sq ft	1,140	Mission Fuel, lb	5,255
		Payload (24 troops), lb	5,760
Folded Aircraft		Mission Gross Weight, lb	40,543
Length, ft	51.8	Struc Design GW, lb	43,590
Width, ft	13.5	Max Alternate GW, lb	52,885

The 5.7 percent download value for the JVX systems specification tilt rotor deserves some comment. After the JVX JTA, the actual XV-15 rotor was tested on the Outdoor Aerodynamic Research Facility (OARF) at Ames. The data showed much higher rotor figure of merit than expected. OARF data plus XV-15 hover flight test data implied much higher download than estimates of vertical drag from rotor downwash on the wing. This led to a model where downwash from the two rotors meets over the fuselage to form an upward fountain. A JVX tilt rotor download estimate including both vertical drag and the fountain would be about twice as large. Aircraft figure of merit would not change because the increased rotor figure of merit and download would cancel.

JVX Program Briefings

The JVX JTA Flag Officer In-Process Review on 3 April 1982 at Ames was the first major external briefing. NAVAIR commander Vice Admiral Richard Seymour was the senior officer. Major Robert Magnus was the Marine action officer. Major Magnus was an “Iron Major,” since he worked under the Marine 3-star in charge of Aviation. His blunt advocacy of Marine interests to a 3-star Admiral was a sight to behold. The briefing focused on the Draft JSOR requirements and Preliminary Design Analysis (JVX JTA Technical Foundations). We prepared most of Charlie Crawford’s viewgraphs for the briefing but he organized them. This became interesting when Charlie turned the Preliminary Design Analysis part of the briefing over to me during the presentation without any warning. I was very familiar with the material but had no idea which viewgraph would be next. Charlie was great at reading the audience and reacting in real time during briefings. I learned to review the actual sequence of viewgraphs and to be mentally ready for anything. This sink or swim training proved to be invaluable in the rest of my career.

Briefings in Washington, D.C. on 19 April included emerging results from Point Design Analysis and Common Design Options. May was a mixture of giving briefings, refining viewgraphs to include final JVX JTA results, and adjusting our message as we learned. We briefed NASA HQ, AHS, AVRADCOM (Major General Story C. Stevens), NAVAIR, the Pentagon (Army, Navy, Marine, and Air Force 3-stars) and industry (Sikorsky, Bell, and Boeing). The most memorable 3-star was Deputy Chief of Naval Operations for Air Warfare, Vice Admiral Wesley McDonald. Not only was he very helpful with good questions and advice, he also had a “Golden Arches” flag in his office. During the RFP Task Force we gave a major JVX JTA briefing on 28 June to representatives of 25 U.S. and foreign aerospace companies. Marine Colonel James Creech (JVX Program Director) and Charlie gave the main briefing while I did the Preliminary Design Analysis portion. Charlie also published an article “JVX, What an Opportunity!” in *Vertiflite* [12].

JVX Request for Proposals Task Force

The JVX RFP Task Force worked in the Mart Building in downtown St. Louis. AVRADCOM had moved from the Mart Building to Goodfellow Boulevard on the outskirts of St. Louis, so it was familiar territory. The government Draft RFP was mostly developed in June of 1982. During July and August we held industry RFP review meetings. Ken Wernicke was lead engineer for Bell-Boeing and Gary de Simone for Sikorsky. We eventually received 369 industry comments on the System Specification, Preliminary Design Statements of Work, and Airworthiness Qualification Plan in the Draft RFP; 190 of them were ultimately incorporated.

The government JVX System Specification design showed that there was a feasible design solution. The job of industry was to develop better JVX design solutions. It was essential that the System Specification not constrain industry to any specific design. The Joint Services Advanced Vertical Lift Aircraft System Specification made no mention of specific aircraft configurations. It included a dash speed of 275 knots and a 2,100-nm self-deployment mission without refueling with flight time not to exceed 10 hours. The Army Corps SIGINT mission profile included segments at 30,000 feet altitude. These required JVX capabilities were well beyond conventional helicopter technology—hence the name Advanced Vertical Lift Aircraft.

Al Winn (AVRADCOM D&Q) led the JVX RFP Task Force and Marine Lieutenant Colonel Bob Yeend was his Deputy. I worked for Charlie mostly on the System Specification and ADS-10. I reviewed the entire System Specification, and I worked with the engineers to clarify the language, replace required designs with required capabilities, and evaluate the impact on the government design. Jeff Bowles at Ames ran the TR code and worked with the layout team to quantify design impacts. Charlie ruled on significant issues based on evidence we provided.

The Air Vehicle Technical Description (ADS-10) had been an important issue since the AAH SSEB. JVX provided the need and the opportunity to create an ADS-10 that supported state-of-the-art comprehensive analyses and addressed advanced vertical lift aircraft, not just helicopters. A strong team was assembled including Wayne Johnson, Ray Kvaternik, Bill White, and Tex Jones. The ADS-10 they produced was a major improvement. It included different levels of detail to address different stages of aircraft development.

The JVX acquisition strategy included a competitive 23-month Preliminary Design Phase with a series of required trade-off analyses by industry. The Draft JVX System Specification was to be continuously updated to reflect the requirements definition, trade-off analysis, and other lessons learned. A final System Specification was to be part of the Full-Scale Development (FSD) RFP.

The JVX RFP developed under Army leadership was eventually cleared to be released by the Army in December 1982 after the Office of the Secretary of Defense (Richard D. DeLauer, Undersecretary of Defense for Research and Engineering) approved the acquisition strategy. This included approval to go directly from the Preliminary Design Phase to FSD without a Full-Scale Prototype. The Preliminary Design Phase included an extensive program of wind tunnel testing and simulation to support scaling up from the XV-15 concept demonstrator (25-foot rotors) to a full-scale System Specification JVX design (39.4-foot rotors).

JVX Acquisition Strategy and Competition

The Pentagon acquisition strategy for JVX envisioned a teaming arrangement for a single development program followed by splitting of the team for production competition. To reduce risk and increase competition, the acquisition strategy was adjusted to include a competitive 23-month Preliminary Design (PD) Phase followed by a single Full-Scale Development (FSD) contract. Bell Helicopter Textron and Boeing Vertol announced a JVX teaming agreement on 7 June 1982. Teaming of the two prime contractors with the strongest technical background in tilt rotor technology made ensuring a viable competition for the JVX development program much more challenging. Bitter experience had taught us that competition is necessary to ensure the very best efforts from industry. We therefore expanded our efforts to encourage industry interest in JVX by undertaking a specific effort to encourage Sikorsky to compete for JVX by teaming

with another prime contractor. Charlie led this effort and I joined him to provide technical support for key briefings. In October 1982 we briefed Buzz Hello, Vice President of Rockwell's Aircraft Division (formerly North American Aviation) to encourage a partnership on JVX.

On 23 November 1982 we briefed Sikorsky leadership including Bob Daniell (CEO), Bill Paul (Executive VP), and Bob Zincone (VP Research and Engineering). Our case for JVX was that 1) Rockwell was willing to partner on JVX, 2) the Preliminary Design Phase would give a Sikorsky/Rockwell team 23 months and about \$50 million to develop a competitive JVX design, and 3) Sikorsky should not underestimate the ability of the competition to make serious mistakes. We reminded Sikorsky that a few years earlier they thought Bell had UTTAS locked-up because of their Huey experience and almost decided not to submit a proposal. Instead Bell lost the Phase I UTTAS competition while the UH-60 Blackhawk was now in production at Sikorsky. We had a strong case, but in the end Sikorsky declined to bid on JVX and signed up to demonstrate X-Wing on the S-72 Rotor Systems Research Aircraft (RSRA).

JVX Program Funding and Executive Service

During the JVX JTA the Pentagon was busy negotiating both funding and executive service for JVX. Emerging information revealed that the Navy was insisting that the Army (as executive service) had to put up more funding than the Navy. The JVX Memorandum of Understanding signed by the three service secretaries on 4 June included service funding shares of 46 percent Army, 42 percent Navy, and 12 percent Air Force. We naively thought that the service with the strongest technical background in tilt rotor technology would be chosen to lead the program. We were wrong. The Army JVX missions were for SEMA. Traditionally the Army's SEMA mission needs had been met with modified, off-the-shelf, fixed-wing aircraft. Ultimately the Army was not willing to fund a new advanced VTOL for SEMA. In January 1983 *Aviation Week* reported that DoD had restructured JVX with the Navy taking over as the executive service and the lead for the airframe development while the Army would be lead for MTE engine development. Airframe funding would be 50 percent Navy, 34 percent Army, and 16 percent Air Force. MTE funding would be split equally between the services. Two weeks later the Navy released a JVX RFP to industry. It was basically the JVX RFP developed under Army leadership because the Marines were not willing to delay the program. The RFP included a Draft System Specification that, as far as I can tell, was unchanged from the version developed under Army leadership.

JVX Source Selection and Development Risk

When JVX became a Navy program, the Army technical community was expected to drop JVX and focus on LHX. We did not want to abandon JVX until it was safely under contract. Charlie was able to negotiate with Dick Lewis to allow three engineers (Charlie, Bill White, and myself) to participate in the JVX Source Selection. The Source Selection started in early February and continued to mid-April 1983. Our NAVAIR friends were gracious hosts and made room for us in their Crystal City, Virginia, offices. I was seated next to Steve Zalesch in the Evaluation Division. Dudley Cate from the HELCOM study was Chief of Weights for the Evaluation Division. He arranged second-shift copy machine access and allowed me to copy Navy weight statements.

NAVAIR had a DEC VAX-11/780 super mini-computer. It ran one shift a day and was so overloaded that interactive response was excruciatingly slow. Fortunately Andy Kerr knew the NAVAIR computer czar through 2GCHAS work and arranged an introduction. To my surprise

the VAX was left on and turned over to me as the only user on second shift. That was wonderful and so I worked two shifts, with people on the first shift and with the VAX on the second shift. I used the VAX mostly to develop hands-on familiarity with the TR code, modify it to address Source Selection technical issues, and model the proposed industry JVX design.

We were very disappointed when we only received one JVX proposal from industry. Bell-Boeing proposed the Model 901 tilt rotor, which eventually was developed into the V-22 Osprey. The job for the Source Selection was now to determine if the proposal was technically acceptable and to negotiate a Preliminary Design (PD) Phase statement of work appropriate for funding originally intended to support two contractor teams. Bill White worked with Charlie and Bonnie Jones, the wonderful NAVAIR KO, to invest the extra funding in risk reduction efforts (e.g., structural testing, wind tunnel testing, and simulation). I worked with Charlie on the technical issues.

The fundamental question was the risk of scaling up from the XV-15 concept demonstrator (25-foot rotors) to a full-scale Bell-Boeing Model 901 (38-foot rotors) without a full-scale prototype. This concern existed both in the Source Selection and in Congress. Navy Captain Jim Magee, Office of the Chief of Naval Operations (OPNAV), asked NASA (John Magee, no relation) for help with Congress. John Magee produced a NASA White Paper on JVX Program Risk in March to support OPNAV in their work with Congressional Staff. The Source Selection debate included risk reduction during PD Phase (mostly Charlie and Bill White) and modeling the proposed Bell-Boeing Model 901 in the TR code. Technology inputs to the TR code that were needed to match the Model 901 design were compared with the government technology inputs developed and documented during the JVX JTA. Specific technical issues included increased contingency weight (eventually implemented by NAVAIR) and modeling of 3D effects on wing drag due to lift (implemented in TR code based on XV-15 data from John Magee).

The Source Selection meeting with the NAVAIR 2-stars included briefings by both NAVAIR and Charlie. Charlie's much greater experience with briefing flag officers was apparent. The 2-stars also knew that Secretary of the Navy John Lehman wanted JVX to succeed. The end result was the award of a JVX PD Phase contract to Bell-Boeing in April 1983 and the beginning of what became V-22 development.

The V-22 Osprey has gone on to achieve great success with the Marines (MV-22) and the Air Force (CV-22). It is planned to be used for the Navy shipboard re-supply mission (CMV-22) and is also planned for use by several foreign governments. Although the Army never acquired V-22 for its own use, the tilt rotor concept is being vigorously pursued by the Army as part of the Joint Multi-Role Technology Demonstrator program, and the Bell V-280 Valor is currently undergoing flight testing for potential future Army missions.

Observations

My engineering adventures were wide-ranging and extended far beyond Ames, but they were always based on a solid foundation of hands-on government technical capability. The Army-NASA partnership in rotorcraft aeromechanics, structures, and propulsion at Ames, Langley, and Lewis was based on world-class facilities and researchers. The origin and the focal point was the Army-NASA collaboration at Ames, AMRDL HQ, and in subsequent years, AFDD. These technical resources, and the hands-on experience of our team, enabled us to support major rotorcraft acquisition programs with quick turnaround government testing and analysis, and

realistically evaluate industry testing and analysis. Government research was augmented with contract research performed by industry and academia. Contract research ranged from small theoretical efforts, through hardware development and test, to VTOL concept demonstrator aircraft (e.g., the XV-15 and XH-59). The Eustis Directorate (in subsequent years, ATL, and then AATD) supported a large contract research program with in-house testing and analysis while the Army-NASA partnership emphasized in-house work.

Army Aviation had a substantial Development and Qualification technical community. This included technical ground and flight testing to produce engineering-quality data and engineering qualification to ensure that Army aircraft were both safe and effective (full System Specification capabilities). The development of qualification criteria and plans plus the engineering analysis and evaluation of contractor qualification data required substantial in-house technical capability. This grounding in the engineering realities of rotorcraft development and modification provided an important balance to the not yet proven technical possibilities emerging from research.

Major technical challenges such as development of new or substantially modified aviation systems, unexpected development problems, or technical problems with the fielded fleet got an all-hands on-deck response. A government (Army, NASA, and Joint Service) technical team was assembled with access to industry and academia as needed. The depth and breadth of the government rotorcraft technical community and the technical leadership to identify, assemble, and lead the A-team were essential to success.

Computer capability was rapidly improving and Army-NASA development of design and analysis codes gave us world-class tools designed to fit our needs. The government technical community must be able to work with many different VTOL concepts on a fair basis and not be limited by the “tribal knowledge” of individual contractors. This need resulted in the in-house development of flexible tools based on fundamentals, and formed by exposure to a wide variety of ideas and approaches from government, industry, and academia.

Friends, Colleagues, and Mentors

Aircraft designers have substantial freedom to develop creative solutions within the space bounded by available technology and affordability (“laws of physics” and “laws of economics”). The goal of design is to maximize operational effectiveness. Operational effectiveness comes from many things including doctrine, organization, training, materiel, and leadership. Thus the technical R&D that enables better aircraft (materiel) must be supported by military art R&D to understand how to best use new technical possibilities. A team that enables new aircraft development must include both technical people (government and industry) and operational people (soldiers, sailors, marines, and airmen). I have been blessed to work with many dedicated, mission-oriented professionals, both technical and operational.

This memoir of the first 8 years of my Army career in the Army-NASA partnership is dedicated to the friends, colleagues, and mentors who made it all possible. Many are mentioned in this account, but I am sure some have been overlooked. I have learned from you and grown in many ways during our work together. Our work has supported development of aircraft that fly around the world in the service of our nation and has significantly advanced rotorcraft technology. Thank you.

Three great leaders and mentors stand out from the rest. They taught me much about life, leadership, and engineering. I am truly blessed to have worked with and for them.



Professor Rene H. Miller



Dr. Richard M. Carlson



Charles C. Crawford, Jr.

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Index

A

Advanced Attack Helicopter (AAH), 6-11, 13-14, 19, 28
 Advanced Scout Helicopter (ASH), 11-14
 Advanced System Technology Integration Office (ASTIO), 10, 14
 Advanced Systems Research Office (ASRO), 5-6, 9-10, 12
 Advancing Blade Concept (ABC), 17-24
 Aeroflightdynamics Directorate (AFDD), 14, 30
 Aeromechanics Laboratory (AL), 10
 Aeronautical Design Standard-10 (ADS-10), 11, 14, 28
 Agusta A129, 13
 American Helicopter Society (AHS), 12, 27
 Ames 40- by 80-Foot Wind Tunnel, 5, 7, 15
 Ames Directorate, 4-5, 9-10
 Ames Research Center, 4-5, 9-10, 14-17, 27-28, 30
 Applied Technology Laboratory (ATL), 10, 13, 19, 31
 Army Aeronautical Research Laboratory (AARL), 4-6, 9-10, 12-13, 15, 30
 Army Air Mobility R&D Laboratory (AMRDL), 4-6, 9-10, 13, 15, 30
 Army Aviation Engineering Flight Activity (AEFA), 6-7
 Army Aviation Materiel Laboratories (AVLABS), 4-5, 8
 Army Aviation Rationalization, Standardization, and Interoperability (RSI) Review, 11
 Army Aviation Systems Command (AVSCOM), 4-5, 10
 Army Helicopter Improvement Program (AHIP), 13-14
 Army Materiel Command, 11
 Army Research and Technology Laboratories (RTL), 10, 12
 Army Systems Acquisition Review Council (ASARC), 13-14
 Army Training and Doctrine Command (TRADOC), 12
 Aviation Applied Technology Directorate (AATD), 14, 31
 Aviation Research and Development Command (AVRADCOM), 10, 17, 27-28
 AVWAR game, 12

B

Ballard, Dick, 16
 Ballistic Research Lab (BRL), 12
 Bell Helicopter, 6-9, 13, 15, 21, 28-29
 Bell OH-58 Kiowa, 11-13
 Bell V-280 Valor, 30
 Bell YAH-63, 6-8
 Best Technical Approach (BTA), 12-13
 Bingham, Gene, 7
 Boeing Vertol, 3-4, 6, 9, 15, 20, 28
 Boeing, Seattle, 3
 Borgman, Dean C., 10, 14
 Bowles, Jeff, 15, 22, 28

C

California Air National Guard, 14
 Carlson, Richard M., 4-9, 11, 15-17, 32
 CARMONETTE battle simulation, 12
 Cate, Dudley, 6, 29
 CH-46 helicopter, 16, 20
 CH-47 helicopter, 9, 16
 CH-54B helicopter, 9
 Chappell, David, 10, 12, 14-15, 17, 19, 21
 Combat Search and Rescue (CSAR), 18, 22, 25
 Comprehensive Analytical Model of Rotorcraft Aerodynamics and Dynamics (CAMRAD), 14, 18-19
 Contracting Officer (KO), 7-8, 30
 Cost and Operational Effectiveness Analysis (COEA), 12
 Crawford, Charles C. Jr., 6, 8, 10-11, 13, 16-17, 32
 Creech, James, 27

D

Daniell, Bob, 29
 Davis, John, 9
 de Havilland of Canada DHC-2 Beaver, 3
 de Simone, Gary, 27
 DeLauer, Richard D., 16, 28
 Department of Defense (DoD), 6, 12, 16-17, 29
 Department of the Army (DA), 6, 14
 Desert One, 18
 Devalier, Moise, 22
 Development and Qualification (D&Q), 10, 12-14, 16-17, 28, 31
 Dinning, Mac, 14
 Directorate for Advanced Systems (DAS), 14

E

Earley, Dennis, 17
 Enders, Dennis, 17
 Eustis Directorate, 4-5, 7, 9-10, 12, 31

F

Flight Standards and Qualification Directorate (FS&QD), 6
 Flight Transportation Lab (FTL), 3-4
 Foye, Ray, 17
 Ft. Rucker, 6, 12-13
 Full-Scale Development (FSD), 28

G

Gelhausen, Paul, 22
 Goldwater, Barry, 15
 Gormont, Ron, 7
 Government Competitive Test (GCT), 6-8

H

Ham, Norman D., 3-4
 Harris, Franklin D., 9, 13
 Harry Diamond Lab, 13
 Harvey Mudd College, 3

Scully, M.

Hawker Siddeley Harrier, 15
Heacock, Gene, 12
Hello, Buzz, 29
Helo design code, 11-12, 17, 20
HESCOMP code, 6-10
Hughes 500 MD, 13
Hughes AH-64 Apache, 9, 16
Hughes Aircraft Co., 6-7, 9, 13
Hughes OH-6 Cayuse, 11-13
Hughes YAH-64, 6-7

I

Imrich, Tom, 4
Ingalls, Charlie, 14
Interservice Helicopter Commonality Study
(HELCOM), 6, 10, 16, 29

J

Jaggers, Joseph Jr., 8
Jane's All the World's Aircraft, 3, 5
Jane's Fighting Ships, 3
Johnson, Wayne, 4-5, 15, 17-18, 28
Joint Services Advanced Vertical Lift Aircraft (JVX), 6,
13-30
Joint Services Operational Requirements (JSOR), 17,
23-25, 27
Joint Technology Assessment (JTA), 14, 17-25, 27,
29-30
Jones, Bonnie, 30
Jones, Tex, 14, 28

K

Kelley, P. X., 16
Kerr, Andy, 5, 9, 29
King, Major, 17
Kvaternik, Ray, 28

L

Landing Helicopter Assault (LHA), 16, 20-21
Landing Helicopter Dock (LHD), 20
Langley 6- x 28-Inch Transonic Wind Tunnel, 7
Langley Directorate, 7, 14
Langley V/STOL Wind Tunnel, 8
Lee, Henry, 13, 18
Lehman, John, 15-16, 30
Lewis Directorate, 4
Lewis, Dick, 7-9, 17, 29
Lift/Cruise Fan (LCF), 22-24
Light Helicopter Family (LHX), 14, 29
Light Observation Helicopter (LOH), 11
Lockheed AH-56 Cheyenne, 5, 19
Lockheed California Co., 4
Long Range Special Operations Forces (LR SOF), 18,
25

M

Magee, Jim, 17, 30
Magee, John, 17, 20, 22, 30
Magnus, Robert, 27
Maisel, Marty, 16, 22

Marine Corps Assault Transport Helicopter (HXM),
16-17
Mark, Hans, 16
Massachusetts Institute of Technology (MIT), 3-5
Mast Mounted Sight (MMS), 11, 13
Maximum Continuous Power (MCP), 7, 22
McAdams, Greg, 17
McDonald, Wesley, 27
McDonnell Aircraft, 5
McDonnell Douglas Helicopter Co. (MDHC), 14
Miller, Rene H., 3-5, 32
Modern Technology Engine (MTE), 18, 22, 24-26
Morse, Andy, 17

N

NASA Design and Analysis of Rotorcraft (NDARC), 13
National Advisory Committee for Aeronautics (NACA),
5
National Aero-Space Plane (NASP), 15
Naval Air Systems Command (NAVAIR), 6, 17, 21, 27,
29-30
Near Term Scout Helicopter (NTSH), 13-14
Norman, Dave, 6
North Atlantic Treaty Organization (NATO), 11, 13

O

Office of Naval Research (ONR), 4
Office of the Chief of Naval Operations (OPNAV), 30
O'Malley, Jim, 7, 10-14, 17, 19, 22

P

Paris Air Show, 15
Paul, Bill, 29
PDPAC code, 11
Peyran, Rick, 15
Pleasants, Bill, 19
Preliminary Design Analysis, 27
Preliminary Design Team (PDT), 10-14
Preliminary Systems Design Engineering (PSDE) code,
5-6, 10

R

Ragosta, Art, 9
Request for Proposal (RFP), 13, 17, 25, 27-29
Required Operational Capability (ROC), 11-12
Research and Development (R&D), 10, 31
ROTOR code, 7
Rotor Systems Research Aircraft (RSRA), 29
Rundgren, Walter, 11
Rutkowski, Joseph, 6

S

Schrage, Dan, 13
Schwartzberg, Milton, 10, 14
Second Generation Comprehensive Helicopter Analysis
System (2GCHAS), 9, 14, 29
Sell, Harold, 12
Seymour, Richard, 16, 27
Shaw, John, 4
Shinn, Ron, 5-6, 10-12, 14, 22

Signals Intelligence (SIGINT), 17, 25, 28

Sikorsky, 13, 19, 27-29

Sikorsky S-76, 13, 19

Smith, Roger, 7, 10, 12, 14, 17, 22

Source Selection Advisory Council, 7-8

Source Selection Authority, 7

Source Selection Evaluation Board (SSEB), 6-8, 10-11, 13-14, 19, 28

Special Electronic Mission Aircraft (SEMA), 17, 22, 25, 29

Special Study Group (SSG), 11-12

Statler, Irv, 16

Stevens, Story C., 10, 17, 27

Stroub, Bob, 7-8

Summers, Mike, 7

Systems Research Integration Office (SRIO), 5, 7 9-10

T

Tate, Ralph, 17

TR design code, 14-15, 20, 28, 30

Trade-Off Analysis (TOA), 12

Trade-Off Determination (TOD), 11-13

Tube-launched, Optically tracked, Wire-guided (TOW), 8

U

UH-60 Blackhawk, 16, 29

University of California, Berkeley, 2-3

Utility Tactical Transport Aircraft System (UTTAS), 6, 29

V

V-22 Osprey, 6, 15-16, 30

VASCOMP design code, 20

Velkoff, Hank, 4-5

Vertical Rate of Climb (VROC), 9

Vertical Take-Off and Landing (VTOL), 3-5, 15-19, 22, 24, 29, 31

Vertical/Short Take-Off and Landing (V/STOL), 8, 16, 20

W

Wernicke, Ken, 21, 27

Wheatley, John, 5-6

White, Bill, 28-30

Whittle, Richard, 15-16

Wilson, Sam, 17, 22

Winn, Al, 28

X

XH-59, 19, 31

XV-15 Tilt Rotor Research Aircraft, 10, 14-16, 19, 27-28, 30-31

Y

Yeend, Bob, 17, 28

Z

Zalesch, Steve, 22, 29

Zincone, Bob, 29

A Dream Career

C. Thomas Snyder
NASA

Introduction

As I sit down to write this memoir, I have the following objectives in mind: to reflect the atmosphere and excitement of working at NASA Ames Research Center from the mid-1960s to the mid-1980s, and to illustrate some of the advances in aeronautical research capabilities and technologies made during that time, with an emphasis on rotary-wing technologies—many of which were attributable to the Army-NASA collaboration at Ames. In so doing, I hope that this will portray the nature and value of the highly successful Army-NASA Joint Agreement. As I attempt to recall key events and people some 30 to 50 years later, my greatest concern is failing to recognize the contributions of deserving individuals.



Tom Snyder, 1980.

In my retirement dinner remarks in 1998, I said that I considered myself a very lucky man because I had been blessed with a wonderful family and a dream career. Having been an airplane buff while growing up, the opportunity to work for NASA after graduating from college in 1962 was a dream come true. In my youth, I built many model airplanes, both display and control-line flying models. Whenever I heard the nearby sound of model airplane engines, I ran to the school grounds two blocks away to watch friends flying their models. A favorite Sunday afternoon family outing was to drive to the Wichita airport and watch the airplanes taking off and landing. During my college years, I followed the news of the supersonic and hypersonic flight tests at Edwards Air Force Base (AFB) and kept an all-black model of an X-15 on my desk.

1962–1974

In June 1962, I began work at NASA Ames as an aerospace engineer in the Flight Systems and Simulation Research Branch. This branch conducted performance, advanced controls and displays, aircraft safety and operating problems, and handling-qualities research on various aircraft types, including conventional and supersonic transports (SSTs), short takeoff and landing (STOL) aircraft, and vertical/short takeoff and landing (V/STOL) aircraft, using flight testing and simulation. Two very fine gentlemen headed up the branch. Seth Anderson was Branch Chief and had a strong flight test background, and Maurie White was Assistant Branch Chief. He provided much of the guidance for our simulation activities and for a growing simulation capability. We were housed on two floors of offices in the NASA Ames Hangar. Typically, we were in two-man offices, each furnished with desks, bookshelves, file cabinets, and a blackboard on the wall. We had large windows on one wall that faced Building N-210 across the street where the simulation labs were located. The test pilots' offices were just down the hall. As I recall, there were approximately 30 to 35 people in the branch, mostly engineers, but we also had a secretary and a mathematician group (primarily women) who entered flight test data into electromechanical calculators. And, of course, slide rules were essential tools carried by us all.

I quickly realized that my new co-workers were an exceptional group of people, highly competent and very friendly and helpful. Walt McNeill, for example, was a great help in teaching me how to derive the equations of motion and convert them into an aircraft simulation. Walt was a private pilot; he introduced me to the *Happy Pilots Flying Club* at Palo Alto Airport. I joined the club, took flying lessons, and got signed off for solo cross-country flights, but I never completed the work for my private license. Purchasing a home and starting a family took priority. However, the experience I gained was invaluable in my interactions with our test pilots.

After learning some of the nuts and bolts of this business, my first assignment was to work with Chuck Jackson to develop a simulation of a DC-8 four-engine jet transport, to validate the use of simulation in takeoff and landing evaluations and certification criteria development. Chuck was an engineer with an amazingly fertile, hardware-oriented mind, and he was also a good private pilot. This work was stimulated in part by Joe Tymczyszyn, Chief of the Federal Aviation Administration (FAA) West Coast SST Field Office. Joe had a vision of the greater use of flight simulation to evaluate the flight characteristics and develop certification criteria for SSTs, on the drawing boards in the U.S., United Kingdom (UK), France, and Russia at that time. Joe would work with us for a few days to weeks at a time, and was a constant source of encouragement and technical support.

Our flight-simulation capabilities were fairly rudimentary at that time. First, the math models used in simulation were for up-and-away flight, and we had to develop the ground reactions to enable takeoff roll, liftoff, landing touchdown, and rollout, and properly represent the effects of crosswinds and turbulence. And, of course, we had to model the physical and aerodynamic characteristics of the DC-8 properly. In those days, we did our own programming on analog computers using patch cords and patch panels. Our simulator for this initial work was a fixed-base Stroukoff YC-134 transport aircraft cab, with instruments and controls representative of a DC-8. The “real-world” visual display was a closed-circuit TV projection onto a screen 11 feet forward of the pilot. The visual scene was generated by a camera servo-driven in 5 degrees of freedom (5-DOF) over a runway model on a moving belt. This application was pushing the system to its servo-response limits; we spent many hours working with the simulation technicians to optimize its response. We were successful in demonstrating the potential of simulation in addressing takeoff and landing issues, and we identified the need for lateral motion of the cockpit for tests involving asymmetric thrust, where the recognition of engine failure and the subsequent control responses were important. We documented the results in Jackson and Snyder [1].

We then began development of a generalized double-delta SST simulation for use in investigating performance and handling qualities, to identify problem areas and assist the airworthiness authorities in the evaluation of certification requirements for this new class of aircraft. SST characteristics that were significantly different from the subsonic transports included: higher gross weights; higher thrust-weight ratios; low static longitudinal stability; long, slender fuselages with high pitch and yaw inertias; low roll inertia; speed-drag instability during initial climb and landing approach; absence of stall well beyond conventional stall angles of attack; high attitude angles during liftoff and touchdown; and large ground effects upon lift, drag and pitching moment. NASA, FAA, and airframe manufacturer pilots participated in this phase of the program. Results of this work were presented at the NASA Conference on Aircraft Operating Problems at Langley Research Center in 1965 and documented in Jackson and Snyder [2] and Snyder and Jackson [3].

We also began to work with Lockheed and Boeing on simulations of their SST designs. These simulation results played a part in the U.S. competitive design evaluations. I recall one time when Lockheed was up against a deadline and a pilot/engineer team headed by Bill Magruder came in early on Friday morning, and we worked nonstop until about noon on Saturday. They then got on an airplane and laid out their final report on their way home to Burbank.

In developing these analog simulations, the programs became quite complex with the layers of patch cords becoming several inches deep. Each day we would spend hours running static and dynamic checks and fixing loose connections. We increased our productivity significantly by getting permission to run an extra couple of hours on some days and to leave the patch panels on the computer rather than remove them for storage overnight. One morning we came in to find that some joker had placed a very large woman's brassiere over our sagging mass of patch cords!

Over those first 10 to 12 years, I had the opportunity to work on a variety of very interesting programs in addition to our continued work on SST, and I became Group Leader for the SST and the Military Aircraft groups. I loved working with good co-workers like Dick Bray, Walt McNeill, Chuck Jackson, Emmett Fry, Hervey Quigley, Dave Jones (Army) and Ray Forrest (FAA); with our test pilots (George Cooper, Fred Drinkwater, Bob Innis, and others); and with Dave Koenig, Vic Corsiglia and others from the 40- by 80-Foot Wind Tunnel. It was a real thrill to be able to fly a wide variety of aircraft and emerging concepts in the simulators, and I enjoyed doing the library searches, analyzing the data, and seeing my name on formal NASA publications. I would come home at night and exclaim to my wife, Sibyl, that it was amazing to have so much fun and get paid for it!

Another program was a simulator and flight investigation with Dick Bray and Fred Drinkwater on the use of wing spoilers for direct lift control. The impetus behind this program was the coming generation of very large "jumbo jet" aircraft and a perceived need to find ways to quicken the flight path response to control inputs. We reported this work in Bray et al. [4]. One of my more exciting days at work was the time we experienced the failures of both hydraulic systems on our Convair 990 test aircraft during one of our test flights on this project. These failures resulted in reduced control, the loss of hydraulic braking, and an inability to lower the landing flaps, thereby requiring a higher landing speed. (There was an emergency air bottle for limited braking assistance.) The main gear would come down okay, but the nose gear had to be cranked down, and there was no locked indication. Several maneuvers were tried to get the nose gear to lock in the down position, but with no success. The crew discussed the foaming of a runway at Travis AFB and putting down there, but the decision was made to return to Moffett Field. Our crew dumped the excess fuel, and another aircraft flew around us trying to shed some



Convair 990 aircraft.

light on the problem. We decided to make a touch and go at Moffett with the hope that the touchdown would jar the nose gear into the locked position. This attempt was unsuccessful, so a second landing was made with a very gentle let down of the nose gear. The emergency vehicles were racing alongside us as the aircraft came to a stop. One of the ground crew rushed out and put a pin in the nose-gear mechanism to prevent it from collapsing. During these landings, my fellow

engineers and I sat in the far rear of the aircraft in crash position. As we climbed out of the aircraft, we felt very thankful for good pilots and good fortune!

One day, Maurie White came into my office and said that a Boeing 727 airliner had crashed during approach to Cincinnati in a storm, with a loss of all aboard, and that wind shear was suspected as the cause. He asked if I would be interested in conducting an analytical investigation on wind shear effects and the use of various control strategies. I jumped at this opportunity, and the findings of this quick turnaround study are documented in Snyder [5] and White et al. [6].

Reducing aircraft noise in airline operations had become a hot topic. Working with Hervey Quigley as project lead, and with Emmett Fry, Bob Innis, and others, we carried out a significant simulator and flight investigation of methods for implementing noise-abatement landing approaches in transport category aircraft, without increasing pilot workload. Flight validation tests were conducted under contract with Boeing in their 367-80 test aircraft, and the results were documented in Quigley et al. [7].

Our earlier SST work had highlighted the need to better understand ground effects on the lift, drag, and pitching moment during SST landing. Working with Fred Drinkwater, Dave Jones, Stewart Rolls, Dave Koenig, Vic Corsiglia, and others, we conducted a simulator and flight investigation of SST ground effects on landing. Flight tests of a Douglas F5D-1 airplane with a modified ogee wing provided ground effect measurements and flight validation of the simulation. Flights in our Convair 990 subsonic jet transport provided simulator validation and comparison. We performed an extensive literature search for ground effect data on delta planform aircraft and conducted piloted simulator evaluations of SST landing flare characteristics using variations in the ground effect model. These results were reported in Rolls et al. [8] and Snyder et al. [9].



Douglas F5D-1 test aircraft.

On a brief side note, I recall vividly the day that I first exposed Fred Drinkwater to our F5D-1 simulation. Using the manufacturer's airplane data and wind tunnel test data, I was confident that we had a good match. After a short exposure, Fred said that it did not fly like the airplane—that it was too sensitive in pitch. He said, “The airplane is solid as a rock.” I was very surprised and after thinking about it overnight, I asked the folks in flight operations to power up the airplane and measure the stick force characteristics for me. We found that the longitudinal stick force gradient (lbs/in.) in the simulator was a good match, but that the breakout force in the airplane was about twice that of the simulator. We made that one change to the simulator and, voilà, it flew like the airplane, solid as a rock!

In 1969, the Flight Simulator for Advanced Aircraft (FSAA) became operational. This new simulator was a three-man transport cab mounted on a 6-DOF motion base. A unique feature was its large lateral motion capability (plus or minus 50 feet), especially valuable in simulating the lateral motion of the pilots who, typically, sat well forward of the center of gravity (c.g.) on modern long aircraft like the SST. Dick Bray was a master at devising the optimal motion drive



Flight Simulator for Advanced Aircraft.

external visual scene was generated by a closed-circuit TV camera “flying” over a large model terrain board that included a 10,700-foot runway. The pilot viewed a color television monitor through a collimating lens that substituted for the windshield. Hydraulic control loaders provided realistic control feel. Computations were performed by a hybrid digital-analog system, with digital operations on the aircraft simulation, and analog operations dealing with motion washout and the computer-cockpit interface. This computer arrangement enabled changeover from one airplane to another in a short time.

The first research program conducted on the FSAA was a joint NASA/FAA investigation of takeoff certification considerations for the new SST and large wide-body subsonic jet transport designs. I worked with Fred Drinkwater, Dick Bray, Emmett Fry, and Ray Forrest (FAA) on this program. Joe Zuccaro was the FSAA facility manager and a pleasant can-do guy. Two FAA pilots, Bob Lesuer and Lieutenant Colonel Doug Benefield, participated in this study. We simulated three different aircraft: a 370,000-pound SST, a 700,000-pound Jumbo Jet configuration, and a 300,000-pound Reference Subsonic Jet Transport, over a range of c.g. locations. All-engines-operating and surprise one-engine-inoperative takeoffs were made, as well as existing and proposed airworthiness takeoff abuse tests. Takeoff safety speeds and speed margins were investigated and established. Necessary takeoff abuse tests were identified and unnecessary ones eliminated. The findings [10] were significant and began to be recognized in joint U.S./Anglo/French airworthiness discussions.

In 1971, I presented a paper [11] summarizing much of this SST takeoff certification criteria work to date at the

software to give the pilots good motion cues while preventing the moving system from running into the travel limits. All participating pilots were enthusiastic in their acceptance of the motion system and considered the forces imposed on them to be very realistic, blending well with the visual cues. This new capability was just what was needed to address certification criteria issues influenced heavily by the pilot’s reaction to engine failures, especially during takeoff.

Several other advances in the simulation facilities were in place by this time. The



FSAA control room, 1969. From left: J. Zuccaro, T. Snyder, E. Fry, and R. Bray.

NASA Conference on Aircraft Safety and Operating Problems, held at the Langley Research Center.

Another SST design issue was the need to define the minimum acceptable level of longitudinal stability in the event of stability augmentation system failure. Because the center of pressure moves aft as an airplane goes supersonic, the SST c.g. is often located near or behind the subsonic neutral point, causing the aircraft to exhibit neutral to unstable pitch characteristics with the augmentation system in failure. We addressed this issue in a joint NASA/FAA/Cornell Aeronautical Lab (CAL) investigation using the FSAA. This program was in preparation for a flight investigation using the CAL Total In-Flight Simulator, sponsored by FAA. We reported this study in Snyder et al. [12].

In the late 1960s, our work shifted to focus on the joint British-French Concorde development that was proceeding full-bore in Europe. The FAA and their British and French counterparts (Air Registration Board (ARB) and Centre Essais en Vol (CEV)) had agreed to work together in combining their varying proposed certification requirements into a common, agreed-to set of requirements. Our work using the FSAA became central to this activity. A series of Concorde simulation investigations took place at Ames with a continually updated data package from the Concorde developers, British Aircraft Corporation (BAC) and SUD Aviation (later to become



Concorde Supersonic Transport.

Aerospatiale). In addition to NASA and FAA pilots, pilots from BAC, SUD, ARB, and CEV participated. Some of the names I recall included Andre Turcat of SUD, Gordon Corps of ARB, and Gilbert Defer of CEV. The pilots in this program considered the FSAA simulation “remarkably like Concorde.” An exciting side benefit for me was a trip to Toulouse, France, where I got to fly their simulation, and to the flight test facility at Fairford, England, where I observed the flight test of the prototype Concorde from the control room. Dick Bray, Ray Forrest, and I participated in a series of joint

French/Anglo/U.S. SST (FAUSST) airworthiness meetings, where Dick reported on the landing studies, and I reported on the takeoff studies. We also participated in the Flight Session discussions. These joint meetings were highly successful and very satisfying to us, with the FSAA findings playing an important role in the definition of internationally agreed upon SST certification criteria and the Special Conditions for Concorde. They certainly put NASA Ames on the map for its simulation capabilities.

In the early stages of the Space Shuttle program, Fred Drinkwater and I traveled to North American Aviation in Southern California to attend a Space Shuttle meeting. The Shuttle was in the conceptual design phase at that time and, for me, this was a fascinating meeting. I felt lucky to be able to witness this history in the making. Another memorable aspect of this trip was that we flew there and back in a Cessna T-37 jet trainer. It was my first flying experience wearing a parachute and an oxygen mask. The view from this tandem cockpit and bubble canopy while flying over the countryside, mixed with a few aerobatics, made this an especially notable experience.

Another item of major importance in my early career memories was the opportunity I had to pursue a graduate degree at Stanford University. NASA Ames and Stanford had a very fine honors cooperative program that enabled Ames employees to pursue advanced degrees through part-time study, typically taking one or two courses per quarter. (This was, in fact, one of the reasons I chose to accept a position at Ames instead of an offer from Flight Research Center.) From 1963 to 1969, I took advantage of this program and obtained my M.S. degree in Aeronautics and Astronautics in 1969. In those days before educational TV classes, we were given time off to commute 20 minutes to Stanford and attend classes. We would often carpool with other NASA and Army employees. During this time, I had the privilege of studying under such well-known academics as Professors Bob Cannon, Holt Ashley, Walter Vincente, John Breakwell, Dan DeBra, and Krishnamurty Karamcheti. In 1968, I had occasion to call Professor Ashley from Stanford Hospital and tell him why I had missed the final exam—my wife had just delivered twin sons! He said he hadn't heard that one before, and graciously allowed me to come in on the weekend and take my final.

One day in 1971, John Leveen of our Training Office came to my office. He told me that Ames was considering nominating me for the Dryden Memorial Fellowship from the National Space Club and asked if I was interested. After he had told me more about it, I said that I would be honored to apply. I told him that, rather than pursuing a Ph.D. in a specialized area, I was more interested in pursuing the graduate degree of Engineer, which would allow me more flexibility to include some management-oriented classes. I then learned that I only had a few days to put together a plan of study for submission with the nomination. After meeting with my very supportive branch management, I talked with Wally Deckert, who had previously received this fellowship. Then I went to Stanford, met with my adviser, the head of the Department of Aeronautics and Astronautics, and the Dean of the Graduate School of Business. I became more excited about the combination of classes that would enable me to acquire the Engineer degree after 1 year of study beyond the Master's degree and the completion and university acceptance of a thesis. I was chosen to receive this fellowship for 1972; it was presented to me at a formal banquet in Washington, D.C., and Sibyl was allowed to join me at this thrilling event. My year of full-time study at Stanford was a tremendously stimulating experience. Stanford provided me with an office in the Aero Building, and Professor Dick Shevell was my advisor. He and I formed a strong friendship and subsequently attended some Stanford football games together with our wives. I completed the year of classes and got a strong start on my thesis. When I returned to Ames I had a heavy workload, but in 1976 I managed to complete my thesis on the growing roles of probabilistic analyses and simulation in airworthiness flying-qualities requirements [13], and I received my degree of Engineer in Aeronautics and Astronautics.

1974–1980

In 1974, my career path underwent a major and exciting change in direction, from one of conducting research to one of managing research. I was appointed Chief of the Flight Systems Research Division, a large 200-man division consisting of 4 research branches and operations responsibility for the 40- by 80-Foot Wind Tunnel and related facilities. I was fortunate to have a strong and technically excellent set of branch chiefs: Mark Kelly at the Large-Scale Aerodynamics Branch, Maurie White at the Flight Dynamics and Controls Branch, Brent Creer at the Aircraft Guidance and Navigation Branch, and Hank Lessing at the Avionics Systems Branch. I was also blessed with a very good office staff. Norm Johnson was my Deputy Division Chief, and Katrine McCormick was my Secretary. I reported to Leonard Roberts, Director of

Aeronautics and Flight Systems, and he was a terrific boss. Jack Boyd was his Deputy, and I had viewed Jack as a mentor since my arrival at Ames. I was greatly indebted to all these folks for their support, cooperation, and patience in helping me learn the ropes of my new responsibilities.

My new position in NASA management marked the beginning of a long and valuable relationship with the U.S. Army under the terms of the Army-NASA Joint Agreement. In 1965, the Army and NASA entered into a unique agreement to share resources in pursuit of research and development of mutual interest in rotary-wing aircraft. In 1974 the Army Lab at Ames was known as the Ames Directorate, one of four directorates under the Army Air Mobility R&D Laboratory (AMRDL) headquartered at Ames. Two others, the Langley and Lewis Directorates were co-located at NASA's Langley and Lewis Research Centers respectively. The fourth, the Eustis Directorate, was located at Ft. Eustis in Virginia. At the time of my promotion, there were approximately 25 collocated Army personnel in my division, including both research and operations people. In day-to-day activities, they fit in seamlessly, with no apparent differences from their NASA counterparts. Most of my management interactions with the Army at that time were with Andy Morse, Chief, Army Aeronautical Research Group who reported to Dr. Irv Statler, the Director, Ames Directorate.

Hans Mark, a brilliant strategist, was Center Director; shortly after his arrival at Ames, he formed a Strategy and Tactics Committee and I became a member. He was a strong proponent of the concept of the Joint Agreement. Hans kept his finger on the activities and the pulse of the Center in many ways, including by simply walking around. One day shortly after my appointment, we were walking down the sidewalk together. He commented on my new job and asked me what my management style was. I remember that I was not very satisfied with my response, but I said something to the effect that it was a learning process for me, and that I believed in good communication both up and down the line. He responded that I would learn soon enough where and how to obtain support for my programs. He was right. For example, it didn't take me long to learn the importance of maintaining a frequent presence in NASA Headquarters and in developing good working relationships with the program managers there.

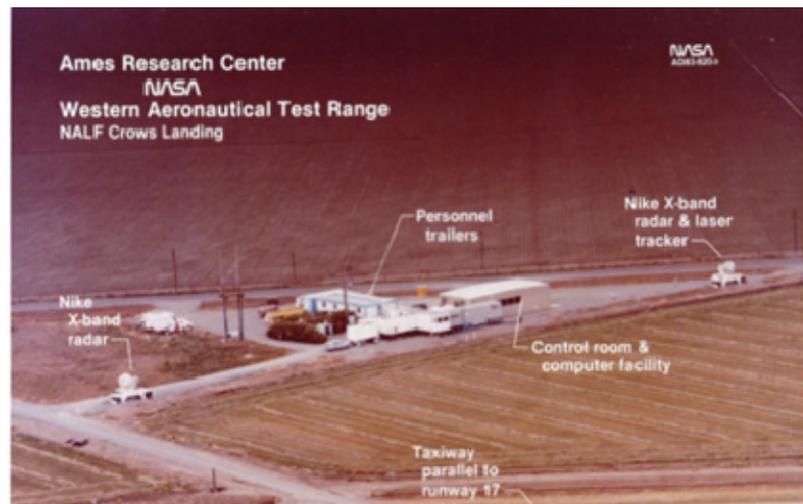
As I think back, I realize that two of the management approach philosophies central to my performance over the years were based on lessons learned from Hans Mark. The first was the importance of developing world-class facilities if you want to do world-class research. These facilities provide a foundation that attracts world-class research personnel and world-class programs and partnering. This leads to the second lesson learned, which was the value of collaborative research or partnering; partnering with another party that has capabilities and resources to bring to the table allows issues to be addressed that would not be possible otherwise. The Army-NASA Joint Agreement is a good example of such partnering, and the very successful proof-of-concept Tilt Rotor Research Aircraft program is an example of a product probably not possible otherwise. Partnering is especially valuable when the partner is part of the user community. User community involvement helps to ensure the relevance of the research activities. For example, in the joint NASA/Army rotorcraft research activities, we enjoyed the benefits of having the "pull" of identified needs intimately involved with developing the aim of the "push" for research and for the application of discovery and invention.

Division activities during the mid to late 1970s included a variety of leading-edge wind tunnel, simulation, and flight programs on STOL, V/STOL, rotorcraft, SST, and military aircraft projects, most with participation by Department of Defense (DoD), FAA, foreign government,

and industry. The Army-NASA Joint Agreement was stimulating a number of new activities in rotary-wing technologies. A series of investigations was conducted on the FSAA to investigate handling qualities of candidate tiltrotor aircraft and support development of the selected XV-15 configuration. The 40- by 80-Foot Wind Tunnel was operating at approximately one and a half shifts per day, testing a variety of powered-lift concepts, advanced rotor systems, and tiltrotors, and supporting military aircraft projects. A new Rotor Test Apparatus completed functional checkout and was introduced into service.

I remember the excitement and anticipation of working with Mark Kelly, Leonard Roberts, and Hans Mark in preparing advocacy material for repowering the 40- by 80-Foot Wind Tunnel and adding a new 80- by 120-foot test section. These modifications would enable testing at speeds up to 345 miles/hour in the 40- by 80-foot test section, and testing many full-scale aircraft up to 115 miles/hour in the new 80- by 120-foot leg. With its reduced wind tunnel wall effects, the 80- by 120-foot test section would provide an important new test capability for powered-lift aircraft and rotorcraft. The Army helped to identify and support the need for such a facility. While there were many individuals who contributed to this project, I want to especially recognize the contributions made by Ken Mort (described in his chapter), who headed up the aerodynamic design activity throughout the life of the project. Ken had a powered scale model constructed of the 40- by 80-Foot/80- by 120-Foot Wind Tunnel, and tested it to evaluate the design and to determine the effects of winds at various angles relative to the inlet. The project was approved, and construction began in November 1978. A major upgrade to the Static Test Facility was also underway at this time, converting this systems-checkout facility into the Outdoor Aerodynamic Research Facility (OARF), to enable acquiring loads data at varying heights for tiltrotor and powered-lift vehicles.

STOLAND and VSTOLAND were advanced flight control and display systems being developed for STOL and V/STOL applications. These research systems enabled evaluation of emerging flight control and advanced display concepts in simulation and flight investigations, including fully automatic landing capabilities for STOL and V/STOL aircraft. Both programs were progressing well. STOLAND flight experiments, led by Don Smith and Del Watson, were being flown on a Twin Otter aircraft and would be transitioned to powered-lift STOL aircraft. VSTOLAND flight experiments, led by Fred Baker, were being flown on the UH-1H helicopter. Many of these flight experiments were conducted at Crows Landing Naval Auxiliary Landing Field, about 50 miles east of Ames. A very effective flight-test capability was developed at this facility by the Avionics Systems Branch, with precision radar and laser tracking systems, a good digital data acquisition and processing system, and a Microwave Landing System (MLS) for approach guidance.



Crows Landing flight test facility.

The FAA provided the MLS in exchange for our testing of the system, and our support of their flight-test evaluations. This system provided Ames with an approach guidance capability far better and more flexible than a conventional Instrument Landing System (ILS). Ames support of the FAA's MLS program played an important part in the FAA's replacement of the nation's ILS systems. Crows Landing was also a very fine facility for aircraft and rotorcraft noise reduction experiments.

In the simulation labs, Heinz Erzberger and his group were making major progress in the areas of fuel-efficient flight procedures, of interest to the airlines, and air traffic management (ATM) research. This ATM work was extremely promising, and we were consciously working to expand this activity. Field evaluations with the FAA were quite successful, and air-traffic-control concepts such as Center TRACON Automation System (CTAS) appeared likely to become part of the FAA's modernization of their air traffic control systems.

The satellite-based global positioning system (GPS) was relatively new at this time. John Bull and his group were having considerable success in applying Differential GPS (DGPS) to the problem of helicopter navigation to off-shore oil rigs, such as in the Gulf of Mexico and the North Sea. Helicopters were increasingly being used to shuttle workers and equipment to and from the rigs, but frequent adverse weather with poor visibility, coupled with the proximity of other oil rigs near the one being serviced, made this a difficult and hazardous task, and acceptable flight conditions were limited. Simply stated, the concept of DGPS employed placing a GPS receiver in a precise location and using its received signal to determine an error correction for civilian receivers in the area, thereby increasing their accuracy. Flight demonstrations in the Gulf of Mexico showed that the application of Differential GPS, coupled with improved flight guidance tools, provided significantly improved operational capability.

An active flight experiments program called Augmentor Wing Jet STOL Research Aircraft (AWJSRA) was underway with participation from the Canadian Department of Industry, Trade, and Commerce. Dave Few and Hervey Quigley, with Bob Innis as project pilot, headed up the original project. Jack Franklin and Bill Hindson headed up a follow-on research program in which STOLAND was used to develop and evaluate advanced control, guidance, and navigation concepts for STOL operations. STOL certification criteria experiments were conducted, with FAA and Canadian airworthiness authority participation.

In 1978, the first of two XV-15 Tilt Rotor Research Aircraft was delivered to Ames. These aircraft were built by Bell Helicopter to NASA/Army specifications, to investigate proof of concept and evaluate the concept for military and civilian applications. NASA and the Army co-funded and co-managed the project. In preparation for its envelope-expansion flight testing, the XV-15 was tested in the 40- by 80-Foot Wind Tunnel to evaluate its aerodynamic and aeroelastic characteristics just before the tunnel was shut down for modification. With growing excitement, we followed the XV-15 flight tests conducted by the project office (not a part of our division), and we had planning underway for follow-on research with the aircraft.

Because of the growing interest in Ames activities important to the FAA, such as aircraft certification criteria, ATM, aircraft noise reduction, aviation human factors, etc., the FAA decided to form a field office at Ames. Jack Cayot was selected to head up this office, and it was my pleasure to assist him in setting this up. Jack managed a staff of about five or six engineers. His office suite was established in Building N-210, down the hall from my division office

complex. This arrangement worked well in coordinating activities, involving FAA engineers and pilots in Ames programs, and getting more FAA visibility into Ames work.

Construction of the new Vertical Motion Simulator (VMS) began in 1975, and initial operations in 1979 demonstrated its high value for approach and landing investigations of various aircraft types, ranging from rotorcraft to the Space Shuttle. The unique feature of this 6-DOF simulator was its 60 feet of vertical travel and 40 feet of horizontal travel, which resulted in very realistic motion cues for approach and landing. In order to define critical vertical motion design specifications, Dick Bray and George Cooper conducted piloted tests using a helicopter cab mounted on a vertical track, on the side of the 40- by 80-Foot Wind Tunnel—a valuable contribution. The VMS represents another good example of the value of the Ames-Army enterprise. The Army, under the leadership of Irv Statler, Director of the (now) Aeromechanics Laboratory, played an important role in defining the need for the VMS, and the VMS structure was constructed as a NASA Construction of Facilities (CoF) project. The Army provided design support and funding for various aspects of the VMS development and operations.



Vertical Motion Simulator.

As part of a NASA roles-and-missions realignment of center responsibilities in 1976, Ames was named as the lead helicopter research center. Contributing to this decision were the findings of the Lundin Study Group headed by Dr. Bruce Lundin, Director of the Lewis Research Center. This group was tasked with determining whether Ames or Langley Research Center, both of which had significant helicopter research programs, should be designated Lead Center. (I was on travel when I was notified that the Lundin Group would be at Ames early the following week, and that I was on the agenda to brief them in two areas. That was a busy weekend for my staff and me!) As a result of this Lead Center decision, Ames was to provide overall helicopter program direction, conduct research on small- and large-scale hardware using aeronautical facilities such as the 40- by 80-Foot Wind Tunnel and flight simulation capabilities, and conduct flight tests with research rotorcraft. Langley would emphasize helicopter structures and acoustics, and continue some disciplinary research in acoustics, airfoils, aeroelasticity, and avionics components. Lewis would emphasize helicopter propulsion.

This Lead Center decision resulted in a 3-year helicopter transfer plan, wherein 72 positions were added to Ames rotorcraft research complement, and some program activities and 5 research helicopters were transferred from Langley to Ames. I remember feeling somewhat uncomfortable as part of a small team from Ames that traveled to Langley to interview some of the research staff about their interest in transferring to Ames. They were quite gracious, however, and several Langley personnel eventually made the move—and contributed significantly to our growing rotorcraft research program.

This Lead Center decision significantly increased the emphasis on our rotorcraft research activities, and on my attention to them. The exceptional collaboration between NASA and the Army at Ames was identified as a significant factor in the recommendations from the Lundin Study Group. I began working more closely with the Army, the rotorcraft industry, and the American Helicopter Society (AHS).

1981–1985

In November 1980, I was promoted to Director of Aeronautics and Flight Systems. I was now responsible for directing the research and facility operations of six divisions: the Aerodynamics Division, the Flight Systems Research Division, the Helicopter Technology Division, the V/STOL Aircraft Technology Division, the Aircraft Operations Division, and the Simulation Sciences Division. In addition to the large-scale aerodynamics facilities, I now had responsibility for the transonic and supersonic wind tunnels, the simulation facilities, and the research aircraft. My boss, Sy Syvertson, was a good Center Director. Lee Stollar was my Deputy, and Betty Copeland was my Secretary. What an opportunity for pursuing the “from computation to wind tunnel to simulation to flight” approach, capitalizing on this unique combination of research capabilities—and we pursued it with fervor. I became even more involved with the Army-NASA Joint Agreement and rotary-wing developments.

The XV-15 Tilt Rotor Research Aircraft project was arguably the hottest thing on the block at that time. Led initially by Dave Few and Lieutenant Commander James Brown, the initial proof-of-concept flight tests and envelope expansion evaluations had been completed. Now under the leadership of John Magee, military mission suitability evaluations such as nap-of-Earth flight, air-to-air combat, shipboard operations, and aerial refueling were underway with the Army, Marines, Navy, and Air Force, and the tiltrotor aircraft was showing its real potential for military applications. In 1981, with the help of Hans Mark, then Secretary of the Air Force, the aircraft was taken to Paris and became the darling of the Paris Air Show (described in Marty Maisel’s chapter). After demonstrating its impressive flight capabilities, it would hover facing the viewing crowd, dip its nose in a bow, and then proceed to land. The crowd loved it!



XV-15 tiltrotor aircraft on demonstration flight.



Boeing/Bell V-22 Osprey.

Around 1983, the Marines came out with the Joint Vertical Lift (JVX) specifications for a Marine assault aircraft that later became the V-22 Osprey. NASA and the Army provided very significant support to JVX in preliminary design studies, computational fluid dynamics (CFD) analyses, aerodynamic and structural wind tunnel testing, simulation, and further XV-15 tests and guest pilot

evaluations. An Advanced Technology Blade set was designed and built for the XV-15 and flown in support of JVX and potential civil tiltrotor applications.

Construction of the 40- by 80-Foot/80- by 120-Foot Wind Tunnel modification was progressing well and was a matter of great excitement at Ames. Highly anticipated operational planning for this new set of capabilities was underway. Construction was completed in June 1982, and Integrated Systems Tests (IST) were underway. During IST on December 9, 1982, this new facility suffered a serious accident. I was in a meeting in the committee room of Administration Building N-200, and I could hear the background noise of the new 80- by 120-foot leg undergoing shakedown testing—a welcome sound, at least to me. Suddenly there was a large “HUMPF” noise and then silence. Sy Syvertson, Center Director, said, “What was that?” We rushed down to the tunnel complex and, with a sick feeling in my stomach, I asked if anyone was injured, or worse (there were observers stationed at various strategic points during this testing). Fortunately, there were no injuries or fatalities—just some very shaken observers.



The 40- by 80-Foot/80- by 120-Foot Wind Tunnel.

Vane set 5, 100 feet upstream of the six drive fans, had broken up and been sucked into the massive drive system. Vane set 5 was a 90- by 130-foot, 77-ton latticework of vanes with nose sections hinged to guide the airflow around a 45-degree corner to accommodate operations in the 80- by 120-foot test section. A slip joint holding the hinge mechanism had failed and allowed the vanes to slam closed while near maximum airspeed. One of the most severe losses was that nearly all 90 of the 11-foot-long, 800-pound fan blades were destroyed. These blades were long-lead-time items from England, made of wood laminated under pressure and high temperature—birch near the hub, then spruce, and then balsa at the tips.

Sy Syvertson appointed my Deputy, Lee Stollar, to head up the recovery project team. This effort became an excellent example of turning despair into hope and an even better outcome. A blue-ribbon panel of experts was convened by NASA and led by Bob Swain of Langley Research Center. Based on inputs from Ames managers and recommendations from the Swain team, approval was given to incorporate additional improvements into the tunnel in its recovery, including aerodynamically improved and stronger turning vanes, a reinforced north leg structure, and better instrumentation. Also during this time, the test sections were lined with 6- to 10-inch-deep acoustic linings that provided enhanced research capabilities for civil and military aircraft noise-reduction and reduced community noise. I served on the Center Review Board during this recovery period and chaired the board for 5 months when the Chairman (the Center Deputy Director) was away at Harvard. This important facility, newly named The National Full-Scale Aerodynamics Complex (NFAC), completed its shakedown tests and became operational in 1987.

Following Lee Stollar’s assignment to lead the 40- by 80-Foot/80- by 120-Foot Wind Tunnel Recovery Team, I selected Dr. Jim Albers as my Deputy. Jim came from Dryden and proved to be an excellent Deputy. He shouldered much of the oversight responsibilities for the operations side of the Directorate and functioned as full Deputy during my frequent travels.

During these initial few years of operation, the VMS had shown its value for SST certification studies and Army rotorcraft nap-of-Earth handling-qualities work, and had become an important element in the Space Shuttle program. Collaborations with the Army and the Space Shuttle program office proved very valuable, as both programs provided funding support for various VMS upgrades over the years. For example, to improve VMS productivity, a system of interchangeable cabs was implemented. This system involved having a suite of cabs that could be refitted, and hardware and software checked out, before cab installation on the VMS platform, thereby minimizing changeover downtime.

One day, Greg Condon (Flight Systems Research Division Chief) and Tony Cook (Assistant Chief of Operations) came to see me and said, “We have a problem.” In designing to cut a large-domed-cab access opening in the side of the VMS tower, the architects had discovered that the six-story-plus VMS tower was vulnerable to a level 7 earthquake. We contacted the Geological Survey experts in Menlo Park, California, and discovered that the probability of a level 7 or greater earthquake over the next 30 years was one we could not ignore. We immediately changed our operations guidelines to only conduct high-priority investigations and to minimize personnel in the area. We had several projects in the CoF queue at that time and proposed reprogramming CoF funds to expedite structurally beefing up the VMS tower. This proposal was approved, and the VMS tower was strengthened. When we experienced the 6.9 Loma Prieta earthquake in 1989, we felt we had dodged a bullet.

As a result of NASA designating Ames as the Lead Center for rotorcraft research and the Army increasing its research staff, the rotorcraft program became even more of a mainstream activity at Ames. I became much more involved with AHS activities and recognized the value of this engineering-oriented organization. I was elected to serve on the AHS Board of Directors and later was honored to serve as President and as Chairman of the Board. John Ward was the Rotorcraft Program Manager (PM) at NASA Headquarters. At Ames, Bill Warmbrodt was Chief of the Rotary-Wing Aeromechanics Branch and Bill Snyder was Chief of the Rotorcraft Flight Investigations Branch—all real assets to the program. There were many exciting collaborative NASA/Army research activities underway. A modern instrumented blades database acquisition program was initiated using the AH-1G blades, with plans to follow on with a UH-60 highly-instrumented blade set. This program included unprecedented aerodynamic pressure and structural loads measurements, correlating wind tunnel and flight data, to provide the design community with the data needed for improved design codes. The Army provided a UH-60 aircraft for this purpose. Various rotorcraft noise-reduction flight investigations were underway. Helicopter MLS evaluations and helicopter and tiltrotor certification criteria investigations were conducted with the FAA. Helicopter Instrument Flight Rules flying-qualities requirements were investigated with the Army and the FAA. Support was provided to the Army’s Light Helicopter Experimental (LHX) program.

One of the helicopters transferred to Ames from Langley was a CH-47B Chinook modified as a variable stability helicopter. This aircraft was further modified at Ames and used in a series of NASA/Army flight experiments in conjunction with VMS simulation studies aimed at new Army helicopter flying-qualities specifications for near-Earth operations. Bill Hindson and George Tucker were the project pilots. These activities led to collaboration with the German center for aerospace research (DFVLR) in which the Ames Army-NASA team participated in flight evaluations in the German Bo-105 research helicopter. This team also flew and evaluated the

Boeing Advanced Digital Optical Control System (ADOCS) installed in an Army UH-60 Blackhawk.

Some time later, Andy Kerr (Director, Army Aeroflightdynamics Directorate) and I journeyed to Washington, D.C., where I briefed a General in the Army Materiel Command, proposing that the Army assign a UH-60 aircraft to Ames for use in flight-control-related research. We planned to install a programmable digital flight-control system in the aircraft, in effect making it a variable-stability in-flight simulator to replace the CH-47 Chinook. This briefing was well received, and a second UH-60 was assigned to Ames by the Army. This UH-60 aircraft was modified as planned and became known as the Rotorcraft Aircrew Systems Concepts Airborne Laboratory (RASCAL). RASCAL has served for a wide variety of investigations to improve helicopter agility, provide carefree maneuvering, and develop advanced sensors and displays for night and adverse-weather nap-of-the-earth (NOE) operations—and continues as a valuable research facility today.



UH-60 RASCAL aircraft.

One of the more challenging and exciting projects was the Defense Advanced Research Projects Agency (DARPA)/NASA Rotor Systems Research Aircraft (RSRA)/X-Wing project. DARPA approached NASA and wanted to use our RSRA as a testbed for an X-Wing rotor. The RSRA was designed for testing advanced rotor systems and was equipped with load cells that allowed accurate measurement of rotor system forces and moments, and wing lift and drag forces. The experimental rotors were equipped with pyrotechnics enabling separation of the rotor if necessary, and the crew could land using wing-borne flight or the ejection seats. A joint agreement was worked out capping NASA's investment, Jim Lane with his excellent credentials was appointed project manager, and the project proceeded. The X-Wing rotor was a four-bladed rotor that enabled takeoff and landing like a helicopter, and that could be stopped in forward flight in an X-Wing orientation to function as a forward-swept-wing aft-swept-wing pair enabling high subsonic flight speeds. This was an extremely complex and technically



Rotor Systems Research Aircraft/X-Wing.

challenging project. Manufacturing the all-composite rotor was a difficult undertaking. The relatively rigid composite blades had circulation control leading- and trailing-edge blowing that varied with the blade rotation and flight condition, and higher harmonic control was an important vibration-reduction feature. Flight control was provided by a quadruplex digital flight control system. As a member of the DARPA/NASA Coordinating Committee, I found myself traveling to Sikorsky and Hamilton-Standard frequently for program review meetings. I found these meetings extremely interesting; progress was steadily



Quiet Short-Haul Research Aircraft.

moving forward but at a rate that could not be sustained financially. Just as the flight phase was getting underway, the program was cancelled because of funding considerations.

The early 1980s was also a very exciting period in the powered-lift research program arena. As the Augmentor Wing Research Aircraft flight investigations were nearing completion, the Quiet Short-Haul Research Aircraft (QSRA) flight program was beginning, under the leadership of John Cochrane, project manager, and Jim Martin, project pilot. The QSRA was a C-8 Buffalo

modified under contract by Boeing with new wings and high-bypass-ratio turbofan engines that incorporated an upper-surface-blowing high-lift system, enabling very impressive STOL performance. The 40- by 80-Foot Wind Tunnel and the FSAA had been used in the design and development of the QSRA. The four engines were mounted to vector the exhaust flow over the upper surface of the wings, which would entrain more flow over and through the sophisticated flap system and produce increased high-lift performance. Also, this allowed the wings to serve as a noise barrier to the community below, and provided relatively quiet operations.

There was considerable interest in the concept for short-haul civil transport operations using short runways at feeder airports or “stub runways” at major airports. The Navy was quite interested in the concept for carrier onboard delivery operations. After flight envelope expansion and proof-of-concept flight tests, the QSRA performed impressive at-sea demonstrations from the USS Kitty Hawk without the use of catapult or arresting gear.

After the success with the XV-15 at the 1981 Paris Air Show, we decided to send the QSRA to the 1983 Paris Air Show. With support from the Air Force, our project team ferried the aircraft over the polar route to Paris, and the aircraft demonstrations were again very successful. Dennis Riddle and Jack Franklin then led significant flight research activities that included integrated flight path and airspeed control, and instrument approach to touchdown capabilities. Gordon Hardy was the project pilot during these research activities. QSRA test results were incorporated in the Air Force C-17 Transport design, and the C-17 Joint Test Team flew the QSRA to evaluate the flight path control augmentation and the head-up display.

Several programs were conducted on a variety of V/STOL lift concepts, such as lift fans and deflected thrust systems. Considerable work had been done earlier in the 40- by 80-Foot



Ames aircraft ramp, 1981.

Wind Tunnel to improve the performance of the AV-8B Harrier. Now the interest was in designs that could incorporate supersonic flight capability and reduced radar cross section, and flight control concepts that integrated the propulsion and flight controls in an intuitive way. There was the joint U.S./UK Advanced Short Takeoff/Vertical Landing (ASTOVL) program and the NASA/Navy Supersonic Short Takeoff/Vertical Landing (STOVL) program. Hover tests were conducted at the OARF, transonic and supersonic aerodynamic tests in the Unitary Wind Tunnels, and flight control and handling-qualities tests in the VMS and an AV-8B Harrier provided by the Marines. Much of this activity contributed enormously to the foundation of design concepts and specifications for the F-35 Joint Strike Fighter, developed later by the Air Force, Navy, and Marine Corps.

As is evident, this was a very exciting time in aeronautical flight research at NASA Ames. Borchers et al. [14] provides a more complete review of this activity.

In 1984, Sy Syvertson retired and Bill Ballhaus was appointed as Center Director. Bill's story is a good one from the NASA/Army perspective (also told in Bill's chapter). He was a very bright researcher who came up through the ranks as an Army employee and quickly made a name for himself in the growing CFD field. Bill became Chief of the Applied Computational Aerodynamics Branch; then he left the Army to work for NASA as Director of Astronautics, then as Ames Center Director, and then on to NASA Headquarters where he served as Associate Administrator of Aeronautics and Space Technology. During his time as Center Director and my boss, I found him to be a very impressive representative of NASA Ames Research Center and the Army-NASA Joint Agreement.

Closing Remarks

My "dream career" continued for another 13 years beyond the time frame described in this chapter, during which I continued as Director of Aeronautics and Flight Systems, then Director of Aerospace Systems, and finally as Director of the National Rotorcraft Technology Center. Also, as a matter of fact, I continued for 4 more years after my official retirement in 1998, as a volunteer worker 1 to 2 days a week. These memories are special for many reasons but, first and foremost, because of the *truly fine people* I was privileged to work with and for. The opportunities to work on exciting and important programs using world-class research facilities were also key.

The Army-NASA relationship was an important component in this regard. This collaboration enabled the development of significant new research capabilities that required a critical mass resource investment and long-term commitment to accomplish and, very likely, wouldn't have been achieved by either agency alone. The best example is perhaps the XV-15 Tilt Rotor Research Aircraft, but other examples include the development of various research rotorcraft and systems, and facility upgrades in the NFAC, the 7- by 10-Foot Wind Tunnel, and the Simulation Laboratories. These capabilities, in turn, spawned a wide range of new research data and findings (design criteria, certification criteria, design tools, design concepts, technology validation, etc.). And these contributed greatly to new U.S. aircraft operating capabilities (such as the V-22 Osprey, F-35 Joint Strike Fighter, low-visibility and low-level rotorcraft operations, noise abatement, etc.), and provide technology for future systems. By merging NASA and Army personnel, we were able to increase our focus on vertical lift research, meet the surge requirements to staff new activities, hire high-value applicants during hiring freezes in one or the

other agency, and establish joint project- and branch-management teams. When briefing visiting NASA advisory committees, DoD Science Board, or other blue ribbon reviewers, it was advantageous to show a united front to address critical technology needs using world-class capabilities. Another advantage was joining with the Army in strengthening some of our research collaborations with foreign aeronautical research laboratories, such as the British Royal Aircraft Establishment, the French Office National d'Etudes et de Recherches (ONERA), and the German DFVLR.

The rewards I received during my NASA career were in many forms and included the achievement of important research milestones, seeing new facility capabilities come online, and watching my people receive the recognition they deserved. Personally, I was honored to receive the Dr. Hugh L. Dryden Memorial Fellowship from the National Space Club, the NASA Exceptional Service Medal, Presidential Rank of Meritorious Executive in the Senior Executive Service, and the Army Aviation Systems Command Director's Award for Interagency Cooperation. I was named a Fellow of the American Institute of Aeronautics and Astronautics, and a Fellow of the American Helicopter Society. It was truly a dream career!

Epilogue

Although it occurred after the time frame of these memoirs, I am including this brief discussion as it represents a crowning example of a unique and highly successful NASA/Army collaboration, as well as showing the effect that the Army-NASA Joint Agreement had on my personal career. One day in 1993, I was in Washington, D.C., and was notified that Dr. Wesley Harris, NASA's Associate Administrator for Aeronautics, wished to see me. In a one-on-one meeting with Dr. Harris, he told me that he and George Singley, Deputy Assistant Secretary of the Army for Research and Development, had been discussing a plan to form an innovative government, industry, and academic research partnership called The National Rotorcraft Technology Center (NRTC). This partnership would emphasize cooperation, cost- and data-sharing, streamlined processes, and minimal infrastructure, in the conduct of research toward the strategic goals of continued superiority of U.S. military rotorcraft, with a view toward expanding the world rotorcraft market and U.S. industry's market share. Two key features of this partnership were 50/50 cost sharing between government (initially Army and NASA, later including Navy and FAA) and U.S. industry (through the Rotorcraft Industry Technology Association), and sharing of data rights by participating members and with the government. NRTC would also integrate the Army's Rotorcraft Centers of Excellence program with three universities into its program. Dr. Harris emphasized that this would require "new ways of doing business" management. He said that because of my relationship with the key players on both the government and industry sides, and my history in managing collaborative research relationships, they would like me to serve as the Director in the establishment of NRTC. I had always enjoyed working with the rotorcraft industry, and this struck me as an intriguing and significant challenge—and I immediately said that I would be pleased to take it on.

It took a great deal of work to get the agreement paperwork developed among the various parties, the government office established and staffed, program definition and review procedures defined, and Congressional approvals in place. Rhett Flater, AHS Executive Director, was an enormous help on the Washington scene. NRTC became an official reality in 1995 [15]. NRTC's program grew from \$15M in 1996, to \$23M in 1997, to about \$30M in 1998. At that point, it involved 4 major airframe manufacturers, 22 subcontractors, 10 government labs, and 19

universities. Eighteen of the 25 projects in 1997 were multi-company, and the emphasis on technology transfer among members gained momentum markedly. Two examples of highly successful multi-company projects honored by prestigious industry awards are Helicopter Health and Usage Monitoring Systems, and Differential-GPS-Guided Noise Abatement Approach Profiles and Procedures. To make a long story short, in December 2000, the White House and DoD awarded Vice President Gore's Hammer Award to NRTC for "reinventing government."



NRTC receiving Vice President Gore's Hammer Award, The Pentagon, December 2000. Left to right: Rhett Flater, Andrew Kerr, Joe Elliot (front), George Singley, III, John F. Ward (rear), Dev Banerjee, Edward Smith, Rande Vause (rear), Yung Yu (front), John Shaw, Kenneth Rosen, Charles Morris, Jr. (rear), LCDR Glen Knaust, John Kamensky, A. Michael Andrews (rear), John McKeown, C. Thomas Snyder, Inderjit Chopra, John M. Davis, Larry Jenkins (rear), Robert Kennedy, Paul Hoepfer (front), Albert Wynn (back), William Wallace (rear), Kathleen Hartle-Giffin (front), Daniel Schrage, John Yuhass, Raj Shenoy, Wesley Harris, and Troy Gaffey.

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Index

A

Advanced Digital Optical Control System (ADOCS), 15
Advanced Short Takeoff/Vertical Landing (ASTOVL), 17
Advanced Technology Blade, 13
Aerodynamics Division, 12
Aeromechanics Laboratory, 11
Air Force Base (AFB), 1, 3
Air Force, 1, 12, 16-17
Air traffic management (ATM), 10
Aircraft Guidance and Navigation Branch, 7
Aircraft Operations Division, 12
American Helicopter Society (AHS), 12, 14, 18

American Institute of Aeronautics and Astronautics (AIAA), 18
Ames Directorate, 8
Anderson, Seth, 1
Andrews, A. Michael, 19
Army Aeroflightdynamics Directorate, 15
Army Air Mobility R&D Laboratory (AMRDL), 8
Army Aviation Systems Command, 18
Army Materiel Command, 15
Army-NASA Joint Agreement, 1, 8-9, 12, 17-18
Ashley, Holt, 7
Associate Administrator of Aeronautics and Space Technology, 17
Augmentor Wing Jet STOL Research Aircraft (AWJSRA), 10

B

Baker, Fred, 9
 Ballhaus, Bill, 17
 Banerjee, Dev, 19
 Benefield, Doug, 5
 Boyd, Jack, 8
 Bray, Dick, 3-6, 11
 Breakwell, John, 7
 British Aircraft Corporation (BAC), 6
 Brown, James, 12

C

Canadian Department of Industry, Trade, and
 Commerce, 10
 Cannon, Bob, 7
 Cayot, Jack, 10
 Center TRACON Automation System (CTAS), 10
 Centre Essais en Vol, 6
 Chopra, Inderjit, 19
 Cochrane, John, 16
 Concorde Supersonic Transport, 6
 Condon, Greg, 14
 Cook, Tony, 14
 Cooper, George, 3, 11
 Copeland, Betty, 12
 Cornell Aeronautical Lab (CAL), 6
 Corps, Gordon, 6
 Corsiglia, Vic, 3-4
 Creer, Brent, 7
 Crows Landing Naval Auxiliary Landing Field, 9-10

D

Davis, John M., 19
 Deckert, Wally, 7
 Defense Advanced Research Projects Agency
 (DARPA), 15
 Defer, Gilbert, 6
 Department of Defense (DoD), 8, 18
 DFVLR, 14, 18
 Differential GPS (DGPS), 10
 DoD Science Board, 18
 Drinkwater, Fred, 3-6
 Dryden Memorial Fellowship, 7, 18

E

Elliot, Joe, 19
 Erzberger, Heinz, 10
 Eustis Directorate, 8

F

Federal Aviation Administration (FAA), 2-3, 5-6, 8,
 10-11, 14, 18
 Few, Dave, 10, 12
 Flater, Rhett, 19
 Flight Dynamics and Controls Branch, 7
 Flight Simulator for Advanced Aircraft (FSAA) 4-6, 9,
 16
 Flight Systems and Simulation Research Branch, 1
 Flight Systems Research Division, 7, 12, 14
 Forrest, Ray, 3, 5-6

Franklin, Jack, 10, 16
 French/Anglo/U.S. SST (FAUSST), 6
 Fry, Emmett, 3-5

G

Gaffey, Troy, 19

H

Hammer Award, 19
 Hardy, Gordon, 16
 Harris, Wesley, 19
 Hartle-Giffin, Kathleen, 19
 Helicopter Health and Usage Monitoring Systems, 18
 Hindson, Bill, 10, 14
 Hoeper, Paul, 19

I

Innis, Bob, 3-4, 10
 Instrument Landing System (ILS), 10
 Integrated Systems Tests (IST), 13

J

Jackson, Chuck, 2-3
 Jenkins, Larry, 19
 Johnson, Norm, 7
 Joint Strike Fighter, 17
 Joint Vertical Lift (JVX), 12-13
 Jones, Dave, 3-4

K

Kamensky, John, 19
 Karamcheti, Krishnamurty, 7
 Kelly, Mark, 7, 9
 Kennedy, Robert, 19
 Kerr, Andy, 15, 19
 Knaust, Glen, 19
 Koenig, Dave, 3-4

L

Lane, Jim, 15
 Langley Directorate, 8
 Langley Research Center, 2, 6, 8, 11, 13
 Lesuer, Bob, 5
 Leveen, John, 7
 Lewis Directorate, 8
 Light Helicopter Experimental (LHX), 14
 Lundin Study Group, 11-12
 Lundin, Bruce, 11

M

Magee, John, 12
 Magruder, Bill, 3
 Mark, Hans, 8-9, 12
 Martin, Jim, 16
 McKeown, John, 19
 Meritorious Executive, 18
 Microwave Landing System (MLS), 9-10, 14
 Morris, Charles, 19
 Mort, Ken, 9

N

Nap of the earth (NOE), 15
 NASA Ames Research Center, 1, 17

Snyder, T.

National Full-Scale Aerodynamics Complex (NFAC),
13, 17

National Rotorcraft Technology Center (NRTC), 18-19

National Space Club, 7, 18

Naval Auxiliary Landing Field, Crows Landing, 9

North American Aviation, 6

O

Office National d'Etudes et de Recherches (ONERA),
18

Outdoor Aerodynamic Research Facility (OARF), 9, 17

P

Paris Air Show, 12, 16

Q

Quiet Short-Haul Research Aircraft (QSRA), 16

Quigley, Hervey, 3-4, 10

R

Riddle, Dennis, 16

Roberts, Leonard, 7, 9

Rolls, Stewart, 4

Rosen, Kenneth, 19

Rotor Systems Research Aircraft (RSRA), 15

Rotorcraft Aircrew Systems Concepts Airborne
Laboratory (RASCAL), 15

Rotorcraft Industry Technology Association, 18

Royal Aircraft Establishment, 18

S

Schrage, Daniel, 19

Senior Executive Service, 18

Shaw, John, 19

Shenoy, Raj, 19

Shevell, Dick, 7

Short Takeoff and Landing (STOL), 1, 8-10, 16

Short Takeoff/Vertical Landing (STOVL), 17

Simulation Sciences Division, 12

Singley, George, 18-19

Smith, Don, 9

Smith, Edward, 19

Snyder, Bill, 14

Snyder, C. Thomas, 19

Space Shuttle, 6, 11, 14

Statler, Irv, 8, 11

STOL Navigation and Landing System (STOLAND),
9-10

Stollar, Lee, 12-13

Supersonic transport (SST), 1-6, 8, 14

Swain, Bob, 13

Syvertson, Sy, 12-13, 17

T

Tilt Rotor Research Aircraft (XV-15), 9-10, 12-13,
16-17

Tucker, George, 14

Turcat, Andre, 6

Tymczyszyn, Joe, 2

U

Unitary Wind Tunnels, 11, 16

V

Vause, Rande, 19

Vertical Motion Simulator (VMS), 11, 14, 17

Vertical/short takeoff and landing (V/STOL), 1, 8-9,
12, 16

Vincente, Walter, 7

V/STOL Navigation and Landing System
(VSTOLAND), 9

W

Wallace, William, 19

Ward, John, 14, 19

Warmbrodt, Bill, 14

Watson, Del, 9

White, Maurie, 1, 4, 7

Wynn, Albert, 19

Y

Yu, Yung, 19

Yuhas, John, 19

Z

Zuccaro, Joe, 5

My Life With Army Aviation Research, 1970–1985

Irv Statler
Army

My Early Years

I can't remember when I became infatuated with flying, but the genesis must have been in the early 1930s. Aviation was a new adventure. At the age of 7, I was building rubber-band-powered model airplanes. I was reading books about adventures in the air and in space—stories about Montgolfier's balloons, the aviation exploits of Lilienthal, Langley, Blériot, and the Wright Brothers, and the trips to the moon by Jules Verne. We were still celebrating Lindbergh's flight, and he was my hero (until he returned from his tour of Germany convinced that the Luftwaffe was far more powerful than it was). Like other kids knew cars, I knew all the airplanes that operated in WWI—British, French, and German, as well as American. I often bicycled out to Gardenville Airport, a private airfield southeast of Buffalo, where I spent many hours admiring the airplanes.



Irv Statler

I loved airplanes, but I do not recall how I decided on aeronautics as my profession. I had no interest in becoming a lawyer or a doctor (which is what my parents would have preferred). My scholastic strengths seemed to be in science and engineering according to various tests. I didn't know what a career in aviation meant and I had no one with whom I could discuss it. Nevertheless, apparently, I decided I would be an aeronautical engineer—whatever it entailed.

So, where would I go to learn to be an aeronautical engineer? A Google search was not available.



My first love, 1936.

Ads about aviation schools were, mostly, those that trained pilots or airplane mechanics. A search of university curricula found that very few offered a full 4-year course in aeronautics. Several offered a 5th-year option in aeronautics after 4 years of mechanical or structural engineering, but they focused on aircraft design and that was not interesting to me. I discovered that the University of Michigan was one of the very few universities in those days that offered a full 4-year curriculum that led to a Bachelor of Science degree in Aeronautical Engineering. Furthermore, the curriculum included many courses on the theories of aerodynamics and fluid flow, as well as the usual structural and mechanical design courses. My decision was made.

I was a junior in high school when I told my parents I wanted to be an aeronautical engineer. My mother said, “You want to be an engineer? What's an engineer? You mean like on a railroad train?” They had no idea what engineering meant, much less aeronautical engineering. To be honest, neither did I, and the acronym “R&D” had not yet been invented. As you are about to learn, I was “assigned” into a career in aviation R&D.

In September 1941, I entered the University of Michigan. Three months later, the U.S. entered WWII. In November, when I turned 18, I registered in the Selective Service System. In the early days of the war, only men between the ages of 21 and 36 were being considered in the lottery. When I appeared before my draft board in Buffalo as required, I was encouraged to continue with my college education and they classified me as *2-A: Registrant deferred in support of the national interest*. They urged me, however, to complete my education “as soon as I could.” I tried volunteering, but my poor eyesight kept me out of any service I preferred.

On February 24, 1945, I graduated with a B.S. degree in Aeronautical Engineering and a B.S. degree in Mathematics. On March 20, 1945, I was inducted into the army at Ft. Dix, New Jersey, and sent to Camp Gordon, Georgia, for infantry basic training. I was pressed to apply for Officer’s Candidate School, but I did not want to commit to 3 years of military service. So, they tried very hard to make me an infantryman.

After completing infantry basic training, I was assigned to go to Europe when I received orders to report to Headquarters prepared to ship out. The Master Sergeant handed me my personal records and orders that I was to deliver to my next post, and train and bus tickets to Ft. Sill, Oklahoma—the field-artillery training center.

Training at Ft. Sill was a memorable instance of *déjà vu*. A lot of my field-artillery basic training was reliving the miseries of infantry training at Camp Gordon. I doubt there were many who had the distinction of having been through full basic training twice—once in infantry and once in field artillery.

Others in my graduating class from field-artillery basic were being shipped out to field-artillery units in the Pacific. I was assigned to the Army Air Corps of the Army Air Forces at Ft. Sill.



L-4 Grasshopper, 1945.

The Army aircraft at Ft. Sill, nicknamed “Grasshoppers,” were used in WWII to spot targets, adjust artillery fire, gather intelligence, direct bombing missions, and shuttle people and documents. Pilots, mechanics, and tower operators of Grasshoppers were trained at the Ft. Sill Field Artillery School. I was assigned to be trained as a Grasshopper mechanic and tower operator.

I wasn’t considered a pilot (they were all Warrant Officers or Lieutenants), but I did a lot of flying because whenever a mechanic did a major repair or engine overhaul, he was the first to fly that airplane. Anyway, after my stint with the Grasshoppers, I had had enough of flying and close calls and I no longer had the urge to become a licensed pilot.

I had been at Ft. Sill for several months, when I was awakened early one morning by the Sergeant, who told me to pack up, return my equipment, rifle, and bedding to the quartermaster, pick up a take-along breakfast, and report to Transportation. This was like my departure from Camp Gordon, except this time there was no mystery about my destination. I was told I was being assigned to Wright Field, the U.S. Army Air Forces center for research, development, and procurement of its aviation systems. They had finally found the right place for me.

In December 1945, I was assigned to Area “B” (Wright Field) of the U.S. Army Air Forces Technical Base. I was at the center of all the technical activity I could possibly have hoped for when I got my degree and that I never expected to experience during my military service. It was a fabulous opportunity to advance my career while completing my military service. I did not realize then the extent to which this assignment would influence key events in my life.



Arriving at Wright Field, I was told to report to the Chief of the Compressibility Unit of the Aerodynamics Branch of the Aircraft Laboratory where I was designated “Technical Aide, Mathematics” and promoted to Private First Class (the highest rank I ever attained in military service).

In 1945, the Compressibility Unit of the U.S. Army Air Forces was composed of four people—Colonel Harold W. Sibert, Chief; Staff Sergeant Jock Brown, Assistant to the Chief; Selma Zickefoos, secretary to the Chief; and a Private First Class (me), general gofer. The arrangement worked greatly to my benefit. Colonel Sibert, Ph.D., was focused on his forthcoming retirement from the military to become a Professor of Aeronautical Engineering at the University of Colorado. He was totally wrapped up in writing a textbook called *High Speed Aerodynamics*. The Staff Sergeant was expert at doing the absolute minimum while he counted his days to discharge. (I never did learn whether he knew anything about aerodynamics or compressibility.) Consequently, I was frequently the representative of the Compressibility Unit at meetings of the Aerodynamics Branch, elsewhere in the Aircraft Laboratory, at gatherings of higher levels, and in response to special requests like participation in Operation Paperclip. I became acquainted with most of the key personnel in the Aerodynamics Branch and the Aircraft Laboratory, and that was to prove useful to me in the future. Among the many friends that I made while I was at Wright Field, one who was exceptional and influential in my future was Courtland Perkins. Court had already departed from the Aircraft Laboratory to be a professor at Princeton University by the time I arrived, but he had been the leader of the Stability and Control Unit of the Aerodynamics Branch and he returned frequently. I had many discussions with Court and our friendship grew. Court became the architect of the Aero Department at Princeton and our paths crossed frequently. Court influenced many of the Princeton grads who came to NASA and the Army at Ames and who are contributors to this memoir.

Although there was interest in the effects of compressibility on flying characteristics of propeller-driven aircraft during WWII, that interest swelled enormously when the Germans surprised us with a jet-propelled fighter aircraft, the Me 262. Suddenly, there was great interest in high-speed, compressible aerodynamics. Our little Compressibility Unit was frequently invited into meetings and discussions about plans for future military aircraft. This opened fantastic learning opportunities for me. It was at one of these meetings that I first heard of the Lockheed P-80¹ that was to become the first jet fighter used operationally by the U.S. It was very successful.

¹ The P-80 would be the data source for my theoretical Ph.D. thesis based on measurements made by CAL Flight Research.

Colonel Sibert started me on a study of slender-body compressible aerodynamic theory. The Germans had developed a theory of the effects of air compressibility when the assumption of a slender body could be used. This was probably work done in association with development of the infamous German V-2 missile. The German literature on those studies was being translated as fast as it was discovered, and it was available at Wright Field. This was a great chance for me to get in at the leading edge of that research in the U.S. I studied every publication I could find on the subject and accumulated a large file of notes on 3- by 5-inch cards of every article I read. Then I wrote a survey of the state of knowledge of compressibility effects in aircraft and slender-body aerodynamics. My studies in slender-body compressible aerodynamics would be important in several soon-to-occur events in my life.

One day, Colonel Sibert was visited by an old acquaintance of his, Dr. Karl Arnstein, Director of the Goodyear-Zeppelin Corporation. He asked Colonel Sibert if he knew of anyone who could come to work for him as an aerodynamicist. Colonel Sibert told him about my studies of slender-body (dirigible) theory and suggested he talk with me. Dr. Arnstein offered me a job. I told him I would be interested in exploring the possibility further if I could be discharged from military service. Colonel Sibert immediately declared me “surplus.” (As things turned out, it was fortunate that the declaration did not entail a commitment to go to work for Dr. Arnstein.) On August 26, 1946, I was honorably discharged at least 6 months ahead of time, and I had a job offer. However, the best laid plans . . .

Dr. Arnstein arranged a visit to Goodyear in Akron, Ohio, after I had spent a little time with my family. Dr. Arnstein and his colleagues were gracious hosts, and before I left, I was offered a position with the Goodyear-Zeppelin Corporation. However, while I left with a very favorable impression of Goodyear, the job, and the people with whom I would be working, I had a very unfavorable impression of Akron. It stunk! From the moment I arrived, I was almost nauseated by the overwhelming odor of burning rubber, and that smell remained with me for days, even after I had returned to Buffalo.

I was not going to live in Akron, Ohio, so I needed a job. I learned about the Cornell Aeronautical Laboratory at the Buffalo Airport in Cheektowaga. In 1943, the Curtiss-Wright Airplane Division at the Buffalo Airport had established a research laboratory. However, shortly after V-J Day, the company was deluged with telegrams canceling contracts for aircraft. Curtiss-Wright could no longer afford its research facilities and so, on January 1, 1946, they were donated to Cornell University to be an independent, self-supporting, not-for-profit R&D organization known as Cornell Aeronautical Laboratory (CAL). CAL was to be an instrument of service to the aircraft industry, to education, and to the public at large. This statement in the transfer agreement would become an important issue in the future.

I decided to explore the possibility of employment at CAL. The CAL employment office directed me to the Flight Research Department, where I was interviewed by Ira Ross, head of the department; William F. Milliken, its technical lead; and Dr. Edmund V. Laitone, the chief aerodynamicist. Dr. Laitone expressed interest in my research on slender-body theory and compressible flow and was eager to see my file of references. I was offered a position. I have always thought that gaining access to my reference file was as much a factor in their decision as were my personal qualifications.

The Cornell Aeronautical Laboratory

The Flight Research Department. I reported for work at CAL on September 23, 1946. Except for a 3-year sabbatical leave, the CAL Flight Research Department was the center of my professional life for the next 11 years.

From 1946 to about 1960, the CAL Flight Research Department conducted pioneering research that led to revolutionary changes in the conception, understanding, and treatment of the dynamic stability characterization of an aircraft and its relationship to controllability. I became a member of a fabulous team whose research left a legacy of knowledge of aircraft stability and control. This legacy would underlie the design of all future aircraft.



Most of the failures of the earliest attempts at powered, fixed-wing flight were associated with inadequate understanding of dynamic stability and control. The Wright brothers recognized the problem and were the first to achieve controlled flight, but they never understood dynamic stability and handling qualities.



Ira Ross

Ira Ross and Bill Milliken spawned the “dream” of finding a way to measure dynamic stability and control characteristics in flight. Ira was an electrical engineer and he proposed the idea of adapting the technique of spectral analysis, well known in electrical engineering, to quantify dynamic stability characteristics. Ira and Bill believed that the key was in understanding an aircraft’s classical equations of motion.² Ed Laitone and I were asked to find a mathematical basis in the equations of motion to support a technique of in-flight measurements for frequency spectral analysis.



Bill Milliken

We dealt with the longitudinal motions of an aircraft. For small motions and negligible longitudinal accelerations, the equations for the 2 degrees of freedom (2-DOF) of heave and pitch motions could be combined into a single second-order differential equation of a simple spring-mass-damper (or the electrical equivalent of capacitor-inductor-resistor). The coefficients corresponding to the damping and spring constants of a spring-mass-damper system could be identified with the stability derivatives that defined the longitudinal fixed-control, short-period mode of the airplane. We could use Fourier analysis for measuring the coefficients of a differential equation.

So, having established an approach on a sound mathematical foundation, the Flight Research team developed a method using precise sinusoidal elevator motions over ranges of frequencies and amplitudes, and recording the responses in pitch attitude, normal acceleration, and control forces on an oscillograph.

² Bill Milliken’s impressive 700-page autobiography, published in 2006 when he was 95, is appropriately titled *Equations of Motion*.

Bill Milliken sold the idea and the Army Air Forces loaned CAL a B-25 bomber for the first experiment to measure dynamic stability characteristics in flight. The funding for the flight tests came from the Aerodynamics Branch of the Aircraft Laboratory at Wright Field. The principals there were Court Perkins, Mel Shores, and Charles Westbrook, all of whom were acquaintances of mine while in military service in the Compressibility Unit of the Aerodynamics Branch. The initial flights with the B-25 were flown by Air Forces pilot Captain Glen Edwards (for whom Edwards Air Force Base was later named) and by Bill Milliken.



The North American B-25.

Measurements of responses and control inputs were recorded on rolls of oscillograph paper and analyzed using a Corradi harmonic analyzer to generate a frequency spectrum, five harmonics at a time, carefully following the recorded trace with the crosshairs of the analyzer. This was tedious, eye-straining work.



The Corradi harmonic analyzer, 1947.

Ed and I were ecstatic when the analyses of the data collected during the first B-25 flight tests confirmed our theoretical methodologies. The B-25 tests demonstrated that, over a large range of frequencies, the 2-DOF representation reliably agreed with the physical facts, that the equivalent spring and damping constants existed, and that reduction of the measured responses to the stability

and control derivatives was plausible. The success of the experiment with the B-25 was the genesis of the pioneering developments at CAL that evolved into the concepts of enhanced stability augmentation, design specifications for handling qualities, the variable-stability aircraft, and in-flight simulators.

I flew in the B-25 on several flights to measure frequency responses. I sat in the bombardier's seat in the nose of the aircraft. John Seal, in the pilot's seat just over my left shoulder, had the habit of chewing on an old cigar butt. The combination of the sinusoidal motions, sitting in the nose surrounded by glass, and the smell of Johnny's cigar was a stomach-wrenching experience. Each flight, I marveled that I had not used the barf bag. I always managed to stumble out of the aircraft feeling unwell for several hours, but retaining my breakfast.

The CAL Flight Research Department established and demonstrated a reliable technique for measuring an aircraft's flying qualities, but the question it raised was, "What flying qualities does the pilot prefer?"

We needed to be able to change the flying characteristics of the airplane in flight to obtain the pilots' opinions on what constituted good or bad flying qualities. Ed and I showed that, if automated control motions were added proportional to aircraft displacements, velocities, or accelerations, it would be equivalent to changing the aircraft's stability derivatives and its flying

qualities. This was the beginning of handling-qualities research and the concept of the “variable-stability” aircraft at CAL.³

About this time, NASA Ames arrived at the same concept except that they came to it having started with the objective of solving an instability problem in an existing aircraft rather than the objective of measuring an airplane’s flying characteristics as we did at CAL. George Cooper, a famous test pilot at NASA Ames, proposed the Cooper Rating Scale for pilot evaluations of handling qualities in 1957, but it was unsatisfactory for several reasons. He and Bob Harper, a test pilot/engineer at CAL Flight Research, elaborated on Cooper’s fundamental scale based on experiments conducted at CAL and at Ames. Their studies led to the Cooper–Harper ratings and a rational determination of desirable flying qualities that became a standard [1].⁴

The methodologies for automatic manipulation of the flight controls in response to selected airplane motions laid the groundwork for the use of automatic control to stabilize inherently unstable aircraft designs, to control stall and ride comfort, and to achieve prescribed operational capabilities. Artificial stability freed designers forevermore from the constraints of fixed stabilizing fins and manually movable surfaces for control as the sole means of achieving satisfactory flying qualities.

Everything that we did at CAL Flight Research was a team effort. Bill and Ira deserve full credit for having thought of the approach, but the demonstration of the viability of their idea relied on the capabilities of the entire team, including the analytical expertise of Walt Breuhaus, Dave Whitcomb, and Ed Laitone; the engineering talents of Bob Kidder, Bob Harper, Chuck Chalk, Jack Bielman, Walt Hirtreiter, Ed Kidd, Oscar Lappi, and Graham Campbell; superb aircraft mechanics like Henry Sonnen, Bill Frey, Lee Maefs, and Shorty Miller; and the piloting skills of John Seal, Nello Infanti, Giff Bull, and Leif Larson.

During those years, there were many others who became “part-time” members of the Flight Research family. I participated in collaborations with world-class experts in aircraft dynamic stability and control such as Court Perkins (whom I have already mentioned), Pat Curtiss, Dunstan Graham, and Ed Seckel⁵ at Princeton; Bob Seamans, Bill Sears, and Cliff Muzzy from MIT; and Irv Ashkenas and Duane McRuer of Systems Technology, Inc. There were many others whose names I have forgotten.

Bill Milliken encouraged a relaxed environment. He opened the hangar on weekends for members of the department to bring in their cars, boats, motorcycles, or whatever, and use the

³ NASA was conducting studies on ways to correct deficiencies in the stability and control characteristics of existing aircraft, while CAL Flight Research was developing a way to measure an aircraft’s stability and control characteristics in flight. Both led to handling-qualities research and the concept of the variable-stability aircraft.

⁴ There is a story about how the pilot-rating system evolved to the current 10 levels of the Cooper–Harper scale. It started with asking the pilot whether he thought a configuration was bad, OK, or very good, but, the test pilots at CAL’s Flight Research Department would invariably say it was between OK and very good, or between bad and OK. So, the flight-test engineers decided that, as the pilots could be that precise, they should give them a scale of five ratings. Invariably, the pilots would return a rating of between two values. Then the engineers tried 7 ratings and finally quit at the 10-level scale of the Cooper–Harper system. Pilots still tend to rate between the 10 levels.

⁵ Ed Seckel was experimenting with a de Lackner one-man flying platform that CAL had transferred to Princeton when he lost control and fell “through” the rotor blades. Miraculously, he suffered only minor injuries.

shop facilities to work on them. Bill, himself, was probably the one who took most advantage of this. Bill was into road racing. He organized the road race at Watkins Glen, New York. October 2, 1948, was a landmark date in the history of American road racing: the first race at Watkins Glen. Everyone from Bill's Flight Research "family" joined in the event. We were in the pits, or we were timers, or scorekeepers, or we maintained logs. We all loved Bill, and we helped him enthusiastically with every one of his dreams—whether in flight research or motorcar racing.



Bill said, "When the front wheels come off the ground, you can be certain you are really racing!"

These were exciting and tremendously creative years, the lasting impact of which we could not imagine at the time. I was extremely lucky to have been a member of the Flight Research team that made important new developments in aviation when I was just at the beginning of my professional career.

From the fall of 1953 to early in 1956, I took sabbatical leave from CAL to pursue a Ph.D. from the California Institute of Technology. When I left for Pasadena, I took Bill Milliken's secretary, Renée Grace Barbara Roll, with me as my wife.

The objective of my thesis was to develop the three-dimensional, compressible, subsonic, unsteady wing theory and to examine the importance of unsteady aerodynamic effects to prediction of dynamic stability characteristics of aircraft at high subsonic speeds. I had all the experimental data from the F-80 flight tests at CAL for verification of my theoretical development. This gave me a great advantage over most doctoral candidates who had to develop such data as part of their research program, because, very often, these experiments would encounter problems that delayed acquisition of the essential data. I completed all the course and thesis requirements by the end of 1955.

I reported back to the CAL Flight Research Department to resume work in 1956 as the Principal Engineer responsible for data acquisition, analyses, and interpretation. In September 1957 (precisely 11 years after I started at CAL), I left Flight Research. I accepted an offer to become the Assistant Head of the Applied Mechanics Department under Walter Targoff. In May 1963, I was named its Head.

The Applied Mechanics Department. Personnel of the Applied Mechanics Department were engaged in a variety of research projects when I became the Assistant Head. The department had many good researchers who were already known for their pioneering work in aerodynamics and dynamics of rotary-wing and vertical/short takeoff and landing (V/STOL) aircraft. The group included Frank DuWaldt, Ham Daughaday, Andy Trenka, Ray Piziali, Orren Tufts, Peter Crimi, George Kurylowich, John Erickson, Sandy Sowyrda, John Grace, and Chee Tung, many of

whom I would try to recruit when I became the Director of the Ames Directorate. Initially, my time was absorbed by a variety of studies Flight Research asked me to undertake, entailing adaptations of the predictive methodologies of aircraft stability and control that Ed Laitone and I had developed.

When I became Head of the Applied Mechanics Department, Richard P. White became the Assistant Head and, together, we focused the department on aerodynamics and dynamics of helicopters and V/STOL aircraft, supported mostly by the U.S. Army Aviation Materiel Laboratories (AVLABS) at Ft. Eustis, Virginia. Initially, Dick was responsible for most of the helicopter-related work as I completed my commitments. I spent a lot of time “selling” to bring money into the department. I gradually became more involved in the projects on helicopters and V/STOL aircraft and performed most of the theoretical work for two studies reported in references [2] and [3].

One of the developments in the Applied Mechanics Department was a new way to test helicopter rotors. It was difficult to obtain accurate aerodynamic measurements on rotors in a typical wind tunnel, because the walls of the tunnel interfered with the rotor flow. In 1967 we designed a flat-deck trailer, with a mast on which a rotor-driven aircraft was mounted, that could be towed by a streamlined tractor down the airport runway at speeds up to about 50 mph. This device worked well as we gradually increased test speeds until, one day, at about 40 mph, the tow bar snapped. The trailer went careening down the runway and eventually went off the side, where the end of the tow bar bit into the soil and the entire trailer went up and over it. It would have been very expensive to repair the extensive damage, so the project was terminated.



The CAL rotor-test trailer, 1967.

Over the next few years, our Applied Mechanics Department gained stature in the community of helicopter and V/STOL research. We added to this recognition when we proposed to AVLABS that they co-sponsor a series of symposia on dynamics and aerodynamics of rotary wing and V/STOL aircraft. The first of these was held in 1963, the second in 1966, and the third (and last) in 1969. The objectives of these symposia were to review the status of research and development, identify problems, and recommend areas for future research in rotor/propeller aerodynamics and rotor noise, wind tunnel testing, and rotor control.

These conferences were well attended with speakers and participants from worldwide organizations representing academia, research agencies, and industry. For the most part, I left the planning and organization of these conferences to Dick, and he did a good job of selecting papers and getting key people to attend, at least for the first two symposia in 1963 and 1966. By the time of the third symposium in June 1969, Dick had left CAL and Al Ritter had replaced him as the Assistant Head of the Department. Al did a remarkable job of organizing that final symposium. The keynote speaker was Dr. Alfred Gessow who was the NASA Assistant Director of Research, Office of Advanced Research and Technology. The banquet speaker was Mr. A. Scott Crossfield, former X-15 test pilot and then Division Vice President, Flight Research and Development, Eastern Airlines. The five session chairmen were Barnes W. McCormick, Paul Yaggy, Robert G. Loewy, Mark Kelly, and Dean C. Lauver.

All three of these symposia were excellent meetings, and they brought CAL increased fame in the helicopter and V/STOL research community. The ties to NASA and the Army brought by these symposia would be important to me when I became the Director of the Ames Directorate.

An incident during one of the first two symposia (I don't remember which) was to be unpredictably significant in my life a few years later. Dick White made the arrangements for the symposium to be held in a hotel in downtown Buffalo in December when the weather was invariably bad, and downtown Buffalo often got the worst of it because of its proximity to Lake Erie. One of the attendees arrived from California without any outer garment, not even a lightweight raincoat. I cannot imagine what possessed him to do such a foolish thing. Anyway, I loaned him a coat for his stay in beautiful downtown Buffalo in mid-winter.

In the 1960s, I became heavily involved in theoretical research of the flight dynamics of rotary-wing and V/STOL aircraft. In 1969, I led a study sponsored by the Air Force Flight Dynamics Laboratory of predictive methods for V/STOL stability and control during transition from hovering to forward flight. We formulated a mathematical model for the dynamic response characteristics of V/STOL aircraft, to study their stability and control properties. We demonstrated the capability of our model by adapting it to the quad, thrust-tilting, ducted-fan vehicle configuration of the Bell X-22A aircraft [4].



The Bell X-22A.

In January 1970, I was promoted from Head of the Applied Mechanics Department to Senior Staff Scientist assisting the Director of the Aerosciences Division, Walt Breuhaus, in the conception and execution of a broad-ranging research program across the Aerodynamic Research and the Applied Mechanics Departments. With this significant promotion and the success of the Applied Mechanics Department under my lead, I had every reason to believe that I would be an employee of the Cornell Aeronautical Laboratory for many more years. But unexpected events were brewing.

In 1946, Curtiss-Wright transferred its research laboratory at the Buffalo Airport to Cornell University with the agreement that it would be operated as a not-for-profit organization for the public good. In 1968, anti-military student groups objected to the university's participation in classified research at CAL and became troublesome to the university officials. The Board of Trustees recommended that the university separate from the laboratory and, at the same time, they decided to make money out of the divestiture.

Many at CAL believed it was unethical of the university to turn CAL into a profit-making entity. We made our opinions known loudly and often to CAL management and Cornell University. I joined the protests and wrote stuff that I sent to whoever would publish it. None of us tried to hide our identities. We never expected retributions from the university for expressing our opinions.

In 1969, the Director of CAL, Ira Ross, was fired because he refused to support the university's position. Several key members of CAL management, including close friends Walt Breuhaus, Harold Cheilek, and Al Flax, resigned in protest. In 1970, the newly appointed Director announced his support of the university's actions and reorganized CAL. In August 1970, he told

me I had 1 day to clear out of my office. I was in the elite company of many of the best brains at CAL who were fired at the same time in the same way.

Being fired from CAL was a huge shock. I was totally unprepared for it. I wrote a résumé and sent it everywhere I could think of. Times were tough; there were few job openings for researchers, and I received no offers until late in September when I received an invitation to visit the Army Aeronautical Research Laboratory (AARL) at Ames Research Center in California.

I went off to Mountain View, California, as fast as I could. I was directed to a building at Ames Research Center where I was to see Mr. Paul Yaggy. The name was familiar and I thought I recognized Mr. Yaggy, but I could not remember where I had met him. But Paul Yaggy remembered me well. I was the one who had loaned him a coat one very cold winter in Buffalo about 5 years before.



Paul Yaggy, 1965.

Paul described the experiment that had started at Ames in 1965. The Army and NASA were collaborating in research and development of rotary-wing aircraft by sharing personnel and facilities, but with no transfer of funds. When the AARL was established under the Army-NASA Joint Agreement in 1965, Colonel Cyril Stapleton was its Commander and Paul came from NASA to be the Technical Director. Colonel Stapleton retired in 1968 and Paul was named the Director, AARL.

Paul said the Army-NASA collaboration had been working so well at Ames that the Department of the Army (DA), the Army Materiel Command (AMC), and NASA Headquarters (HQ) had agreed to expand the concept to the NASA Centers in Langley, Virginia, and Cleveland, Ohio. When he received my résumé, Paul was just starting to put an organizational structure in place all by himself. Paul offered me a job as the first technical staff member in the Army Air Mobility R&D Laboratory (AMRDL) HQ at Ames Research Center. I told him I was interested.

On September 21, 1970, I received an offer of a position as Aerospace Engineer, GS-15, at an annual salary of \$25,176. So, on a rainy, gloomy (typically Buffalo) Friday, November 13, 1970, I set out with my wife, Renée, and our two young sons, Bill and Tom, from Clarence, New York, to drive to Mountain View, California.

The Army Air Mobility R&D Laboratory

On Monday, November 23, 1970 (my 47th birthday), I reported to AMRDL HQ in Building N-207. The first assignment that Paul Yaggy gave me was to design an organization for the AMRDL composed of the three new Directorates co-located with NASA Research Centers at Moffett Field, California, Langley, Virginia, and Cleveland, Ohio, and the existing Army AVLABS organization at Ft. Eustis, Virginia.

During this organizational phase, we were reporting directly to AMC in Arlington, Virginia, and to DA in the Pentagon. We worked with Dick Ballard in the DA, and Norm Klein and John Beebe at AMC, all of whom were truly supportive and helpful.



Paul and I developed the basic organizational concepts of assigning a primary area of responsibility to each of the four Directorates of the Laboratory, knowing there would be a good deal of overlap among them. The three Directorates co-located with NASA would have primary responsibility for fundamental and applied research (i.e., 6.1 and 6.2 categories of funding), while the organization at Ft. Eustis, Virginia, would have primary responsibility for development and implementation (6.3 and 6.4). The directorate at Moffett Field, California, would be called the Ames Directorate; the one in Cleveland, Ohio, would be the Lewis Directorate; and the one at Langley, Virginia, would be the Langley Directorate. The organization at Ft. Eustis, Virginia, would become the Eustis Directorate. I drafted statements of missions and functions for the Army R&D Laboratory and for each of its four Directorates, I prepared the realignment plan in accordance with the assignments of missions and functions, and I wrote the first U.S. Army Aviation R&D Plan. The Ames Directorate would have prime responsibility for R&D in the technical discipline of Aeromechanics, the Lewis Directorate would conduct R&D in Propulsion, the Langley Directorate's responsibility would be Structures and Materials, and the Eustis Directorate would manage work in Applied Technology.

That was the easy part. The tough part was negotiating the distribution of responsibilities, personnel, and funding to everyone's satisfaction. A large portion of the 6.1 and 6.2 R&D work that would be conducted by in-house Army and NASA personnel at the Directorates co-located with NASA was, at the time, being performed through contracts managed by the Ft. Eustis organization. Activities, funding, and personnel positions would move from Ft. Eustis to the other three Directorates. Paul brought Dean Borgman in from AARL to help me and left it to us to decide on and negotiate these moves with the four Directors. This was a very difficult job. Eliminating 6.1 and 6.2 activities at Ft. Eustis meant discharging people who were not willing to move or were not qualified to conduct the research having been just the managers of work conducted by contractors.

Dean and I tried our best to minimize the impact at Ft. Eustis and succeeded in distributing the unhappiness across all the Directorates. Despite objections from all the Directors, Paul accepted our recommendations, and the AMC and the DA approved our proposed redistribution of responsibilities and funding. AMRDL was now officially organized and operational—at great cost to my personal relationships with each of the Directorates in Ohio and Virginia.

Paul and I traveled together often during those early years, and traveling with Paul was an experience. Paul believed waiting at the airport to board an airplane was a waste of time, so he always kept working to the very last minute before heading for the airport. I, on the other hand, am equally notorious about being early for everything. On one memorable trip, I waited impatiently for Paul as he continued to make phone calls and sign papers. I was certain we would miss our flight. Finally, to my great relief, we took off in his ancient VW Bug, only to have Paul announce that he had to make a stop at his bank. We arrived at the airport minutes before departure time. Paul dropped me and our bags off while he went to park his car. They were closing the doors as we got on board.

Paul selected NASA personnel to be the Directors at Lewis and Langley, but he assigned Major Gordon Berry as Acting Head of the Ames Directorate while he searched for his own

replacement as Director at Ames. He was having difficulty finding someone who suited him. Finally, when he ran out of options, he nominated me. I started working as the Director of the Ames Directorate in September 1971. On April 28, 1972, the Commanding General of the Army Aviation Systems Command (AVSCOM) formally approved my appointment.

The Ames Directorate. As I took over leadership of the Ames Directorate, it was very important to me to maintain and enhance the environment for our researchers that Paul Yaggy had established—an environment that was conducive to the enthusiastic pursuit of invention and discovery. I am very pleased that many of the authors of chapters in this memoir speak of the relaxed and collegial atmosphere of the Ames Directorate, one that emulated the working environment I enjoyed at CAL Flight Research.

When I became Director, the work (carried over from AARL) in the Ames Directorate was mostly in aerodynamics and wind tunnel testing of helicopter rotors using the experimental and computational facilities that NASA either made available to or had assigned to AARL, such as one of the two 7- by 10-Foot Wind Tunnels and an IBM 1800 computer. Several Army personnel were assigned to NASA organizational units, working with NASA personnel under NASA supervisors on NASA-directed projects. They constituted the Joint Aeronautical Research Group (JARG). However, most of the Army personnel had their offices in Building N-215 and worked in unstructured teams as members of the Army Aeronautical Research Group (AARG). Both the AARG and the JARG made use of NASA facilities and people, but none were engaged in true Army-NASA collaborative R&D.

Under Paul Yaggy, Andy Morse supervised the theoretical and experimental research activities as Chief of AARG. Andy was an experienced helicopter engineer and researcher who came to AARL from Hiller Helicopters. Paul and Andy were wise enough to realize that they had hired personnel well-trained in the basic disciplines who just needed gentle guidance, support, and encouragement to make them competent researchers in helicopter technologies. He and Paul did not impose any formal structure.

I was fortunate to have inherited such an exceptionally competent, reliable, and likeable person as Andy, whom I could trust implicitly to manage our “in-house” research program. Andy and I had a very open relationship, and I relied on his judgement. He let me know what I needed to know to do my job, and he gave me time to do it by relieving me of the detailed supervision of the AARG. Andy was a gentle man, who acted more as a father than a boss to many of the young engineers under his control. It worked.

I take little credit for the many remarkable achievements of the AARG because, at least initially, my time was consumed in establishing the role of the Ames Directorate in the new and competitive environment of AMRDL. However, I succeeded in supporting the needs of the AARG in the continuous competition for resources among the four Directorates of AMRDL.

Teams emerged loosely organized into groups that focused on theoretical and experimental aerodynamics of rotors informally led by Jim McCroskey, on rotorcraft aeroelasticity and structural dynamics led by Bob Ormiston, and on external rotorcraft noise led by Fred Schmitz. There was continuous interchange among the groups in the flexible environment that Paul and Andy established. Each achieved remarkable accomplishments in their technical disciplines.

Colonel Stapleton and Paul succeeded in getting Jim McCroskey assigned to AARL in mid-1966 to fulfill his military-service commitments. Jim was the first of the Reserve Officers' Training Corps (ROTC)-commissioned officers to be assigned to AARL; over the next few years they included Bill Ballhaus, Frank Caradonna, Ken McAlister, Peter Goorjian, and Dewey Hodges.

Jim was a fundamental experimental aerodynamicist, but his specialty was hypersonic flow. He knew nothing about aerodynamics of rotary-wing aircraft when he started at AARL. Jim had a quick and intense indoctrination into the aerodynamic problems of helicopters. He decided to undertake an experimental program that, over the next 7 years, investigated viscous boundary layers on rotating blades and retreating-blade dynamic stall, developed unique instrumentation for measuring the aerodynamics on rotating blades, and performed pioneering wind tunnel experiments with oscillating airfoils. The team of Frank Caradonna, Larry Carr, Ken McAlister, Jim McCroskey, Mike Martin, and Chee Tung, with the support of Wayne Empey, Bob George, and Tom Wynn, produced notable advancements in all of these areas.

NASA Ames had moved into the lead role in Computational Fluid Dynamics (CFD) and had established the Applied Computational Aerodynamics Branch in 1978, with the Army's Bill Ballhaus as its Chief. Jim had proven his expertise in experimental aerodynamics, and he wanted to move into CFD. I obtained permission for him to move into that branch under the Army-NASA Joint Agreement. Jim played a key role in bridging numerical analysts and experimentalists, and he formed a small Rotorcraft CFD group. As this group grew under Jim's leadership it, too, made internationally recognized achievements in the applications of CFD to rotary-wing aircraft.

In 1968, Paul lured Bob Ormiston away from Boeing's Supersonic Transport to work on helicopters. Within a year, Bob found himself enmeshed in the problems of the Lockheed AH-56A Cheyenne. Rotorcraft dynamics and aeroelastic stability of hingeless rotors became the focus of Bob's interests, research, and achievements over, at least, the next 25 years. This area of interest attracted some exceptionally talented people like Dewey Hodges, Dave Peters, Dave Sharpe, Bill Bousman, Don Kunz, Mike Rutkowski, and others to work in Bob's informal group. Together with consultants such as Earl Dowell of Princeton, they pursued a multi-faceted analytical and experimental research program stimulated initially by problems with the Cheyenne; this research led to seminal knowledge in structural dynamics and aeroelasticity of rotors, control-response characteristics of hingeless and bearingless rotors, unsteady aerodynamics of rotor-wake inflow, blade-stall aerodynamics, and ground and air resonance that have contributed to the design of many current rotary-wing aircraft.

In 1978, I formed four divisions in the Aeromechanics Laboratory, and I appointed Bob Ormiston to lead the Rotorcraft Dynamics Division. Under Bob's leadership, that division gained worldwide recognition for achievements in the fundamental understanding of rotorcraft dynamics and aeroelasticity that have influenced the design of modern rotary-wing aircraft.

In his chapter, Bob describes the saga of the Second-Generation Comprehensive Helicopter Analysis System, or 2GCHAS as it came to be known. Bob describes how he became involved in this infamous program and the myriad twists, turns, trials, and tribulations that it survived to produce its successful and widely used manifestation called Rotorcraft Comprehensive Analysis System (RCAS).

By the time I arrived at the Ames Directorate, Fred Schmitz had already been leading a program to investigate sources of external noise of helicopters and tiltrotor aircraft for more than a year. Fred invented ways to study high-tip-Mach-number rotor-impulsive noise and blade-vortex-interaction (BVI) impulsive noise. Fred, Rande Vause, and Yung Yu implemented Fred's idea of measuring helicopter noise in flight using microphones mounted on a quiet fixed-wing aircraft flying in formation. This led to an international collaboration under the Memoranda of Understanding (MOUs) with France and Germany.

The AH-1G model rotor (used in tests in the Ames anechoic hover chamber and the U.S. Army's 7- by 10-Foot Wind Tunnel) was transported to France, where it was tested in the Office National d'Etudes et de Recherches Aéropatiales (ONERA) CEPRA-19 acoustic wind tunnel under the auspices of the U.S.-France MOU for collaboration in helicopter research.

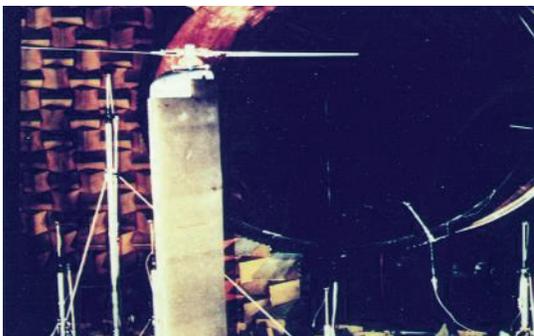
A wind tunnel test with the same AH-1G model rotor was also conducted in the Deutsche-Niederlander Windkanal (DNW) in the Netherlands. The DNW had recently been built in a collaboration between Deutsche Forschungs- und Versuchsanstalt für Luft- und Raumfahrt (DFVLR), Germany, and National Aerospace Laboratory (NLR), The Netherlands. It had characteristics that made it ideal for acoustic testing of helicopter rotors. The DNW staff were eager to demonstrate that they could make high-quality measurements of external rotor noise. The costs were borne by the DNW and were conducted under the auspices of the U.S.-Germany MOU for collaboration in helicopter research.



U.S. Army 7- by 10-Foot Wind Tunnel test.



DNW test.



ONERA CEPRA-19 test.



Full-scale in-flight acoustic measurements.

AH-1 helicopter rotor acoustics testing.

Comparisons were made with full-scale flight-test data of the AH-1G using a YO-3A aircraft as a platform for microphones to measure the noise from the rotor blades and triangulate on its source. The program was a huge success, with several joint international papers showing the quality of the rotor noise data. The results showed that, with some caveats, measurements of BVI impulsive noise quantitatively matched the impulsive noise data gathered in the full-scale flight testing program for the AH-1G rotor.

Fred and his group that included Don Boxwell, Georgene Laub, Rande Vause, and Yung Yu, achieved fundamental breakthroughs in theoretical and experimental techniques for understanding external helicopter noise. That group's exceptional accomplishments under Fred's leadership exemplified the spirit of the dedicated researcher devoted to discovery and invention. Furthermore, their efforts to reduce external helicopter noise were a particularly successful example of the "push" of research interacting with the "pull" of military and civilian needs.

New directions for the Ames Directorate. For the most part, I have been describing the achievements of the Army's "in-house" personnel in the AARL's AARG that were initiated prior to its becoming part of the Ames Directorate and my becoming its Director. AARG personnel had the benefits of access to NASA facilities and support but, with a few notable exceptions, they did not perform this work in close coordination with NASA personnel.

When I became Director of the newly established Ames Directorate, I wanted to embrace, enhance, and exploit the Army-NASA collaboration and expand our activities into other areas appropriate under the new mission statement for the Ames Directorate (that I had composed). The mission statement said the Director was expected to "...initiate, plan, develop, and execute a program in research, exploratory development, and advanced development oriented toward improvement of low-speed, fixed-wing, rotary-wing, and other V/STOL aircraft in coordination with U.S. Army Aviation Systems Command and with NASA Ames Research Center."

An area of R&D that I chose to develop was to support the Army's need for better understanding of flying qualities of helicopters in order to be able to write mission-oriented specifications for acquisitions of new aircraft. It was an area in which I had considerable experience and in which there was known expertise at NASA Ames. I had the benefit of a legacy of ties between CAL and NASA Ames. Many of the people with whom I worked during my 24 years at CAL are authors of chapters in this memoir or are mentioned in them. Associations established at the CAL/AVLABS symposia that we ran from our CAL Applied Mechanics Department were strengthened when I recruited former colleagues from that department to work at the Ames Directorate. My personal relationships with NASA Ames personnel and importation of others with comparable ties to NASA may help to explain the successful expansion of the Army-NASA Joint Agreement at Ames while I was Director of the Ames Directorate.

The early cooperation between CAL Flight Research and NASA Ames Research Center, as they worked together on understanding aircraft stability and control, strongly influenced my choice of activity that would serve both my objectives of expanding the role of the Ames Directorate and of enhancing the Army-NASA collaboration at Ames. Collaboration between NASA and CAL in the pioneering research on aircraft dynamic stability and control, and development of variable-stability aircraft, are reflected in the chapters by Ed Aiken, Dave Key, and Vic Lebacqz. That relationship is exemplified by the development of the pilots' rating scale of handling qualities by

Bob Harper of CAL Flight Research and George Cooper of NASA Ames. The area of R&D that I chose would address an important Army need.

Flight control and handling qualities. By the 1970s, the roles of the military helicopter had expanded to include demanding missions that entailed low-level flying in severely degraded visual conditions. However, the Army did not know how to write handling-qualities specifications to perform these new missions, and the industry did not know how to design helicopters with the flying qualities needed to achieve the handling-qualities specs even if they existed.

I sought to develop a research program to fill these gaps and sold it to the Army. Paul Yaggy was supportive, and I was allotted the funds and personnel slots to undertake the study. Once I had the go-ahead, I sought a leader for this new activity. I identified David Key as the person with the credentials needed to run this project and, in 1973, I succeeded in recruiting Dave to leave CAL Flight Research to take the lead. About this time, several other people from CAL were enticed to join either NASA or the Army at Ames, most of whom Dave and I knew were experts in helicopters, flying and handling qualities, and flight-testing techniques. They included Ed Aiken, Bob Chen, Vic Lebacqz, Ray Piziali, and Chee Tung. Dave and this team had the qualifications to undertake the study. However, some NASA management personnel were reluctant to commit to collaborative activity. Our program was delayed because I had difficulty persuading key people in the NASA Flight Dynamics and Control Branch to work on helicopters in addition to V/STOL aircraft.

Paul was not willing to assign Dave to the Ames Directorate until I had NASA's cooperation. Poor Dave was held in limbo in HQ's assignments for a long time. I doubt that he knew what was going on as I pleaded my case with Brad Wick, Chief of the Full-Scale Systems Research Division, and with Len Roberts, Ames Aeronautics and Flight Systems Director.

NASA Ames personnel had been active in the stability and control characteristics of V/STOL aircraft, but they were not interested in working on helicopters. Traditionally, Langley did the work with helicopters and the NASA folks did not want to enter a competition with them. They were quite happy staying with their successful V/STOL research.

I finally obtained agreement. Talking NASA into working with us on understanding the flying characteristics and the handling qualities of rotary-wing aircraft was one of my notable achievements as Director of the Ames Directorate. Now we could finally start. In 1974, Dave moved from AMRDL HQ to the Ames Directorate. While he was at HQ, he had been reviewing the capabilities in AMRDL to undertake a study of the deficiencies in understanding flying- and handling-qualities of helicopters. He had reported the needs and had prepared a plan to meet them. Dave was well prepared and eager to take on the challenge. In 1975, after further negotiations, Dave became a member of NASA's Flight Dynamics and Controls Branch to work on the project he and I had been advocating to the Army.

In his chapter of this memoir, Dave describes his conception, design, advocacy, and leadership of the theoretical and experimental research in flying- and handling-qualities of helicopters that culminated in the production of the Aeronautical Design Standard (ADS) known as ADS-33. This replaced the long-out-of-date MIL-H-8501A with a comprehensive specification containing criteria that covered modern helicopter-design characteristics suitable for performing current missions at night, in all weather, and with degraded visibility.

Dave Key and his team made innovative use of NASA's world-class ground-based simulation facilities at Ames, but, for many years, there was no capability at Ames to conduct the essential in-flight simulation studies. We had to find help elsewhere to obtain the flight-test data we needed to complement our ground-based simulation data. As Dave recounts, he and I negotiated with DFVLR to use their variable-stability helicopter under the terms of the MOU we already had with Germany. Dave also arranged to obtain flight data from the National Research Council of Canada and from the Royal Aircraft Establishment (RAE) Bedford in the UK.

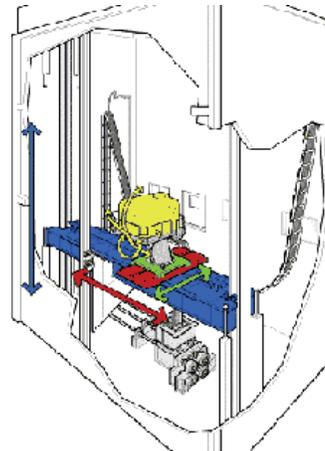
ADS-33 was endorsed by the U.S. Army as the handling-qualities-specifications standard and it was adopted by the international helicopter industry. ADS-33 was an outstanding achievement and a product of the Army-NASA Joint Agreement at Ames.

The Vertical Motion Simulator. The ground-based simulators at Ames had good ranges of translational motion, but the rotational degrees of freedom needed to be enhanced to study helicopter handling qualities. Dave had designed an Advanced Research Systems Integration Simulator (ARSIS) for this purpose, but the Army could not afford to build it. However, I got Army funding and NASA's permission to incorporate the hexapod motion of the ARSIS on their Vertical Motion Simulator (VMS). The modified VMS' translational motion, rotational degrees of freedom of the hexapod, advanced visual displays, and interchangeable cabs made it unsurpassed for simulating helicopters in maneuvering flight near the ground.

Dave led a successful program of ground-based simulations at Ames and in-flight evaluations at international organizations in support of ADS-33.



The Vertical Motion Simulator cab.



The Vertical Motion Simulator tower.

The XV-15 Tilt Rotor Research Program. Another notable success of the Army-NASA Joint Agreement at Ames during my reign as Director was the XV-15 Tilt Rotor Research Aircraft Program. NASA, the Air Force, and the Army separately and jointly had been exploring the technologies of vertical takeoff and landing for civilian and military applications since the 1950s. Between 1958 and 1962, the Bell XV-3 demonstrated the ability of the tiltrotor aircraft to perform conversion from helicopter to airplane and back to helicopter in a controllable manner. The XV-15 Tilt Rotor Research Aircraft project was the culmination of these earlier investigations.

Paul and Hans Mark agreed that the positive results of the tiltrotor technology studies justified the initiation of the development of a tiltrotor proof-of-concept aircraft. During 1971, I joined Paul in many meetings with Army and NASA management to convince them to undertake this next step in tiltrotor development. We succeeded. An agreement for the joint development and operation of tiltrotor proof-of-concept research vehicles at Ames Research Center was signed on November 1, 1971, by Robert L. Johnson, Assistant Secretary of the Army, R&D, and Roy P. Jackson, NASA Associate Administrator for Advanced Research and Development. In 1971, NASA Ames established a Tilt Rotor Research Aircraft Project Office with Dave Few, NASA, as its head, and Dean Borgman, AMRDL, as his deputy. In August 1971, NASA awarded contracts to Boeing Vertol and Bell to conduct preliminary tiltrotor aircraft design studies. On September 30, 1973, a \$50-million contract was awarded to Bell Helicopters for the design, fabrication, and test of two tiltrotor research aircraft jointly funded by the Army and NASA. For a thorough review of the history of tiltrotor technology and the XV-15 program, see the report by Maisel, Giulianetti, and Dugan [5].

However, it took continuous effort to maintain the fragile support from the Army and NASA. In the photo below left, I am lecturing representatives of the Army on the merits of a tiltrotor for Army missions. In the photo below right, we had ganged up on the NASA Administrator to dissuade him from cutting NASA's funding.

The first XV-15 had its maiden flight on May 3, 1977. By the end of August 1982, the two research aircraft had logged 289 hours of successful flight testing, but the Army had lost interest and we had to find financial support. As part of our promotional effort, we arranged for the Chairman of the Joint Chiefs of Staff, at least one U.S. senator, one service secretary, and numerous military brass to fly the XV-15.



Selling the tiltrotor to the Army. From left: Dr. Irv Statler, Dick Ballard, Department of the Army; John Beebe, Army Materiel Command; unknown.



Selling the tiltrotor to NASA. From left: Colonel Norm Robinson, AMRDL Deputy Director; Dr. Irv Statler; Dr. Jim Fletcher, NASA Administrator; Dr. Hans Mark, Ames Center Director; Dr. Dick Carlson, AMRDL Director; Dr. Len Roberts, Ames Aeronautics and Flight Systems Director.



Senator Barry Goldwater.



Chairman, Joint Chiefs of Staff, General Vessey.

In 1981, at Bell's urging, we agreed to take an XV-15 to the Paris Air Show. It was a challenge to get it from the Bell plant in Texas to Le Bourget Airfield in Paris. Hans Mark (who had become the Secretary of the Air Force by then) arranged for a C-5 to transport it from Texas to Farnborough, England, and, with considerable help from European friends, we finally got permission to deliver and assemble the XV-15 in England and fly it to a military airfield near Paris, where it was made ready for the air show.

At the air show, a beautiful routine showed off the XV-15's quietness and speed in straight flight, as well as its agility in vertical and horizontal flight, ending with a graceful nose-dip bow to the VIP grandstand. The XV-15 performed on schedule for 11 consecutive days, including days when other aircraft could not fly because of inclement weather or when an accident shut down the runway. The XV-15 was the star of the 1981 Paris Air Show.

John Lehman, Secretary of the Navy, was impressed by the XV-15 in Paris. Hans Mark and I convinced him to support an evaluation, which was conducted in August 1982 onboard the amphibious assault ship U.S.S. Tripoli, off San Diego. In 1983, the Navy awarded Bell and Boeing Vertol a joint contract to design a tiltrotor aircraft to meet the requirement for the Joint Services' Advanced Vertical Lift Aircraft. The Navy placed orders on behalf of the Marine Corps for production versions of the V-22 Osprey.

By June 1989, the two XV-15 aircraft had accumulated over 825 flight hours and had been flown by 185 pilots with varied experience and capabilities. This was an impressive performance by research aircraft that had been contracted to provide 50 hours of flight testing. The Army-NASA collaboration deserves credit for one of the most successful research aircraft programs ever conducted by NASA. Regrettably, the Army lost interest.

The Man-machine Integration Design and Analysis System. In 1981, my bosses in the DA came to me with a new challenge. Having heard the promises of "artificial intelligence," the question they put to me was: "If we replaced the second pilot with automation, could a single pilot fly an attack helicopter nap-of-the-earth (i.e., under tree-top level), at night, in poor weather conditions, while the bad guys were firing at him, and still locate and demolish a target?"

The traditional approach in response to this sort of question was to put well-trained pilots into a high-fidelity, ground-based simulator of a helicopter, add in the simulated environment, have them conduct the mission, and measure their performance. Some sort of average across multiple pilots would presumably provide a probability of an average pilot's performing the mission. Frequently, these ground-based simulations would be followed with flights in a highly instrumented helicopter with which some of the environmental mission conditions (within limits of safety considerations) could be simulated. This approach with a human at the controls of the simulated and the real helicopter (called man-in-the-loop simulation) has been used successfully with all sorts of aircraft for decades. But using this approach to try to get the answer to the question the Army asked would cost a fortune and take years to complete, and, although the pilots enjoyed participating in such experiments, it was not the best use of their training.

My superiors in the Army were not willing to provide adequate funds to support such a human-in-the-loop study or to wait years for the answer. They were so anxious to get an answer that they agreed to consider my proposal for a radical approach.

I proposed to develop computational anthropometric and cognitive models of the pilot to use in the simulation studies, rather than using a real pilot. If we succeeded, we would be able to explore a large range of variability in pilot characteristics and mission scenarios, at lower cost and in much less time than we could using the human-in-the-loop approach. We could identify the critical areas of human performance so that, later, we could afford to explore these areas more accurately using human-in-the-loop simulation.

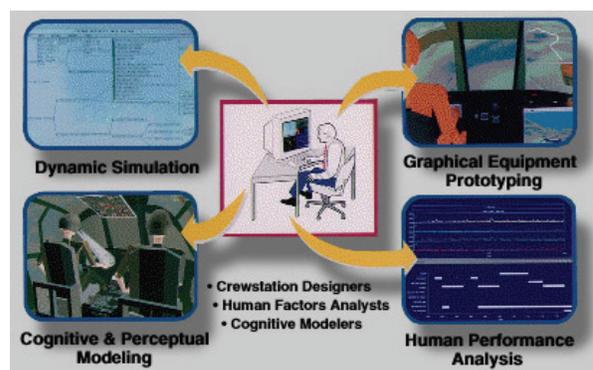


Dr. Kevin Corker.

I needed help from NASA to develop models of human perception, cognitive behavior, heuristic knowledge, decision making, and responses. Once again, it took a good deal of discussion to get Dr. Alan Chambers, Chief of NASA's Human Factors Research Division, to assign a member of his division, Jim Hartzell, to help me. Jim and I developed a proposal, sold it to the Army, and received funding to get started.

Now all we had to do was build computational models of human behavior and cognition. We awarded several small contracts to help us evolve an approach. One of these was with Bolt, Beranek, and Newman (BB&N) who assigned Dr. Kevin Corker. Kevin turned out to be the key member of the team that built the human behavioral models, after we hired him away from BB&N. One of Kevin's unique qualities was the way he could blend with engineers despite his being an experimental psychologist. Kevin became a cherished colleague and a very dear friend whom I have missed greatly since his sudden death due to cancer on January 17, 2008.

When I left the Army in 1985, Jim Hartzell took over as the project leader and it evolved into the Man-machine Integration Design and Analysis System (MIDAS). MIDAS is a set of



MIDAS

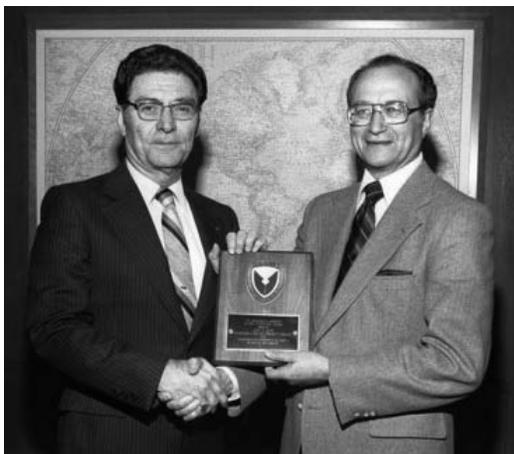
computational modules that simulate humans interacting with crew-station displays and controls, vehicle dynamics, and a dynamically generated environment. Jim and Barry Smith (Lakinsmith) did an outstanding job, and MIDAS has been used in a large variety of applications in aviation, space vehicles, and mission operations. (See, for example, reference [6].)

Administrative innovations. One of the accomplishments of which I am particularly proud entailed a radical change in management of Army personnel at Ames. When I became Director, Army personnel at Ames were being managed out of an office in Oakland. Not only was it inconvenient to drive to Oakland to sign forms or obtain services, but, more importantly, that office had no experience in managing research personnel. Army and NASA personnel at Ames were being managed by different standards even though they worked side by side. There was gross inequality of performance standards and promotions between Army and NASA personnel.

I proposed that the personnel-management responsibilities for Army employees at Ames be performed by NASA's personnel office at Ames. The idea of having one federal agency manage the personnel employed by another agency was revolutionary. I had to obtain approval from personnel management at NASA Ames Research Center and NASA HQ, the Aviation Systems Command, AMC, and DA, before I sought approval from the Office of Personnel Management. Invariably, the initial reaction was that it could not be done. It took patience and nearly 2 years to get dozens of people to sign off on the proposal in 1976.

Now Army personnel would be evaluated against standards in common with their NASA colleagues. NASA agreed to consider Army personnel for promotional positions up to the level of Branch Chief. Army personnel would be considered for management positions above that level with the condition that they would transfer to NASA if selected. During my tour as Director, several Army employees became NASA Branch Chiefs, supervising both Army and NASA personnel. Bill Ballhaus was selected to be Chief of the Ames Applied Computational Aerodynamics Branch. Bill Ballhaus subsequently joined NASA when he became Director of

Astronautics and then Ames Center Director. Fred Schmitz joined NASA to become Chief of the Ames Full-Scale Aerodynamics Research Division and then Ames Director of Aeronautics. The concept was a success and became the model for personnel management at the other Army Directorates at Lewis and Langley Research Centers.



Presentation to Jack Boyd, NASA Ames, of first award for service to the Army.

I instituted an annual recognition of a NASA employee who had provided exceptional service to the Army. In the photo on the left, I am presenting the first of these awards to Jack Boyd, the Deputy Center Director. Subsequent awards went to various people in NASA such as the Procurement Officer and the Head Nurse at the Ames Health Unit.

Hans Mark. One of the people whom I came to admire greatly was Dr. Hans Mark, Center Director at NASA Ames when I became Director of the Ames Directorate. Hans Mark, a renowned nuclear physicist from Lawrence Livermore Laboratory, was the first director to come from outside rather than inside Ames ranks, and he brought a fresh perspective to the Ames mission and to managing its work. He was a man who liked to shake things up. It was fortuitous that he was the Center Director at the time when I wanted to change relationships and collaborations between the Army Directorate and NASA Ames. None of the accomplishments I spoke of earlier would have been possible without the support of the NASA Center Director. I cannot imagine trying to do any of those things with any of the Center Directors who succeeded Hans Mark. Hans had a unique combination of talents that included exceptional technical expertise, an extraordinary comprehension of how to manage R&D, and a rare understanding of the political nature of his position as a NASA Center Director. Most importantly, Hans had the personal stature, presence, and sensitivity to people that made him extremely effective in any negotiations on behalf of NASA Ames.



Dr. Hans Mark.

Hans and I resonated well. We established a good working relationship and a lasting friendship. He was highly supportive of the concept of the Joint Agreement, and it was a joy for me to work with Hans as I sought to improve upon and expand the relationship.

In 1974, Paul Yaggy retired to devote full time to his church. Dr. Richard M. Carlson, who had been with Lockheed California Company for many years and had also been a lecturer at Stanford University, was appointed to succeed Paul as head of the U.S. Army AMRDL. Then, in 1977, the Army Aviation Systems Command (AVSCOM) reorganized.

The Army Aviation Research and Development Command

The Army reorganized our command a couple of times during the years that I served with them. When I started in 1971, we were called the Army Air Mobility R&D Laboratory (AMRDL) with four Directorates and we reported to AVSCOM. On July 1, 1977, the AVSCOM was reorganized, and its R&D mission was assigned to a new Army Aviation Research and Development Command (AVRADCOM), with Major General Story C. Stevens commanding. This resulted in several beneficial changes for us in AMRDL and to me personally. First, and perhaps most importantly, we now were reporting to a command whose sole mission was aviation R&D, with a two-star general committed to obtaining funding for aviation R&D. Second, AMRDL became the Army Research and Technology Laboratories (RTL), reporting to AVRADCOM. Each of the former four Directorates became a “Laboratory,” which increased our stature. As a Laboratory Director, I was negotiating directly with the NASA Ames Center Director rather than with the heads of the Directorates as I had been when we were the Ames Directorate.

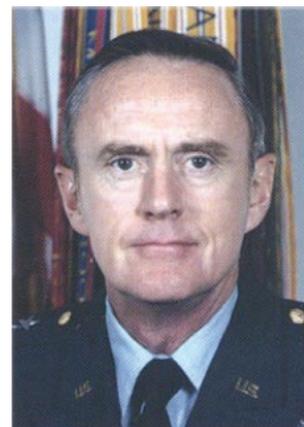


The leaders of AVRADCOM, 1979. Back row, from left: Richard Lewis, Technical Director, AVRADCOM; Irving Statler, Director, Aeromechanics Lab; and Richard Carlson, Director, RTL. Front row, from left: Charles Crawford, Director, Development and Qualification; Thomas Coleman, Director, Structures and Materials Lab; Major General Story Stevens, Commander AVRADCOM; John Acurio, Director, Propulsion Lab; and Richard Long, Deputy Director, Systems Engineering.

The Aeromechanics Laboratory. From its very first days as AARL, the Army's activities at Ames were only loosely structured under the AARG of in-house personnel and the JARG of personnel assigned to NASA organizations. By the time we became the Aeromechanics Laboratory of the RTL under AVRADCOM in 1977, we had grown in size and diversity of activities to warrant a formal structure. I replaced the AARG and the JARG with four divisions that were reflections of the teams and their activities that had emerged over the previous decade. The assignments of the Army personnel, whether previously in the AARG or the JARG, to one of the four divisions served both technical and administrative requirements. Technically, the organization identified the R&D disciplines assigned to the Aeromechanics Laboratory and expert leadership for each. Administratively, each employee's annual performance evaluation had to be signed by an Army supervisor. Previously, NASA supervisors provided inputs to the performance evaluations of Army personnel assigned to the JARG, but I signed the evaluations of all personnel assigned to the Aeromechanics Laboratory. Now, while their NASA supervisors continued to provide inputs for the Army employees assigned to them, there was a designated Army supervisor knowledgeable in each employee's technical discipline to review, discuss, and sign the performance evaluation. I do not remember for certain the initial appointees as Division leads, but I believe Andy Morse was the first Chief of the Fluid Mechanics Division, Bob Ormiston headed Rotor Dynamics, Dave Key led the Flight Controls Division, and Frank Lazzeroni was the Support Division Chief.

I had another purpose for establishing this organization of the Aeromechanics Laboratory, which was to formally put the HQ, RTL on notice of the disciplines that had been assigned to us. I had seen signs of an inclination of the Advanced Systems Research Office (ASRO) to get involved in R&D activities and to exploit the facilities and expertise at Ames that rightfully belonged in our domain of responsibilities. They never considered doing that at Lewis or Langley. I saw this takeover almost happen in flight controls and handling qualities when HQ delayed Dave Key's assignment to our Laboratory and had him prepare an R&D plan.

Major General Story C. Stevens. A pleasant relationship evolved between me and Major General Story Stevens. We made several trips to Europe together that I arranged so that I could introduce him to the contacts I had made there and get his support for the collaborative agreements I had already initiated or had planned. I prepared briefings for General Stevens to present at international meetings we attended during these trips. I arranged for him to fly newly developed helicopters in France and Italy. I always went along on these flights and, of course, Story had to show off his flying skills, presumably to test these aircraft. Much of the time I was in the back, hanging on to anything I could grab, and praying my stomach would stay put.



Major General Story C. Stevens.

Traveling with a two-star General had its ups and downs. Wherever we went, we were extended every military courtesy and utmost security and, on occasion, these went to extremes. One instance left a lasting memory. During a conference in Italy, the attendees were offered a visit to Venice. Story and I wanted to join that excursion, but we had a commitment to go to Agusta Helicopters in Milan early the following morning, and the tour bus would not return in time for us to catch the train to Milan. When we told Agusta personnel attending the meeting about our problem, they said we should go to Venice and they would take care of everything. We did not know what to expect. We had a wonderful visit to Venice and, when Story and I boarded the bus that was returning to the meeting site, we were told that a car would meet the bus somewhere along the way and would drive us to Milan. Much later, the bus pulled off to the side of the road, and Story and I were deposited in the middle of nowhere in pitch-black darkness. Three large black limousines were parked at the side of the highway, with two very large persons dressed in black standing at attention next to each one. We were asked to enter the back seat of the middle car and then all three vehicles took off in convoy at VERY high speed. Speed limits and traffic lights meant nothing. At one red light, the three cars pulled into the left lane, sped around the waiting line of cars, and made a right turn through the red light, crossing in front of the waiting cars. Neither of us had any idea who these people were or where we were going. We arrived in Milan. Apparently, the drivers had simply been ordered to provide security, pick us up on the road, and get us to Milan as fast as possible. They did their job memorably well.

Major General Stevens was an outstanding leader for an R&D Command and an exceptionally nice person. He, I, and our wives became traveling companions. I enjoyed Story's friendship on a personal basis, and it very likely helped a bit in the competition for resources among the directors. I also gained Story's support for my foreign entanglements that I speak about later.

I was very disappointed when Story retired in July 1983. We lost an exceptional leader for military R&D, and I lost a good friend. Major General Orlando E. Gonzales took over command of AVRADCOM. General Gonzales never had the interest in, or the feel for, R&D that Story had. This may have been one of the reasons that in March 1984, less than a year after Story departed, AVRADCOM was disestablished. In another reorganization, AMRDL once again reported to the Aviation Systems Command and, once again, consisted of four Directorates. The wheel had gone full circle. It was the beginning of more dire events.

Major General Stevens was inducted into the Army Aviation Hall of Fame in 1986. Among his credits, Story was said to have been responsible for AVRADCOM becoming a model command for creativity, innovation, and teamwork. At the ceremony, General John R. Guthrie, AMC Commander, described him as “Unquestionably the Army’s most experienced and expert commander and manager in the full spectrum of aviation acquisition.”

Foreign Affairs

AGARD. Paul Yaggy opened new vistas for me in foreign relationships. One that would have a profound effect on my life was exposure to AGARD, the Advisory Group for Aerospace R&D to the NATO Military Committee.

In 1971, Paul Yaggy and I co-authored (that is a euphemism for: I wrote and he presented) a paper titled *Progress in Rotor-Blade Aerodynamics* for presentation at a symposium on Advanced Rotorcraft at the NASA Langley Research Center [7]. The Flight Mechanics Panel (FMP) of AGARD sponsored this conference. I had not had any association with AGARD prior to this meeting.

AGARD was formed in 1952 at the personal initiative of Dr. Theodore von Kármán with the support of General “Hap” Arnold and the U.S. Air Force as an advisory board for NATO, like the Scientific Advisory Board of the U.S. Air Force. I was intrigued by this concept of international cooperation, and when Paul suggested that I become a member of the FMP, I jumped at the chance. AGARD opened a huge new world of associations for me. I became a very active member of the AGARD FMP. In May 1972, I attended the first symposium and business meeting of the FMP as a member. In March 1974, I was elected Deputy Chairman of the FMP and became Chairman in November 1974.

In addition to my administrative positions, I contributed to all of the Panel’s technical activities. For example, as soon as I joined the FMP, I took over representation of the U.S. Army on a working group studying rescue of military helicopter crews. Although I knew nothing about the subject, I was elected to be its Chair, and we produced a report for the Military Committee titled, *Escape Measures for Combat Helicopter Crews* [8]. Later, I headed another working group (which included Colonel Arlin Deel of our Lab and John Sinacori of NASA Ames) on a subject with which I had more familiarity and interest. This one resulted in a report titled *Characteristics of Flight Simulator Visual Systems* [9] and a presentation at an AGARD conference [10].

When I finished my term as Chairman in 1977, the U.S. National Delegate asked me to stay on as the U.S. National Coordinator for the FMP. I continued in that capacity until 1983. I served AGARD for 12 years and I left with great regret. I really enjoyed my time on the AGARD FMP. I had made many friends among the members of the FMP, including Bill Aiken, NASA HQ; Hal Andrews, Navy; Neil Armstrong, NASA Johnson; Ted Carter, Sikorsky; Bill



Drinkin' buddies, Neil and me, 1977.

Hamilton, Boeing; Gerry Kayten, NASA HQ; John Klineberg, NASA Lewis; Bob Lynn, Bell; Rene Miller, MIT; Dick Shevell, Stanford University; Mac Sinclair, Canada; Pierre Lecomte, Jean-Philippe Marec, Philippe Poisson-Quinton, Jean Renaudie, and Jean-Claude Wanner, France; Karl Doetsche and Peter Hamel, Germany; Otto Gerlach, Jan van Doorn, and Theodore van Oosterom, The Netherlands; and Robin Balmer and John Scott-Wilson, UK. Among the members of the AGARD Board of Directors, friends included A. Auriol, R. Fleury, and P-H Chevalier of France; Gero Madelung of Germany; Jan van der Blik and Ben Spee of The Netherlands; Al Flax, Institute for Defense Analysis (IDA); Walt Laberge, DoD; and Al Lovelace, NASA. Many of them became long-lasting friends.

I credit my participation in AGARD and the relationships it enabled with facilitating most of the international collaborations that I helped to arrange for Ames Army-NASA.

Collaborative Agreements. In 1970, Paul Yaggy and Philippe Poisson-Quinton (widely known as PQ, whom Paul had met through AGARD) entered discussions about the possibility of AMRDL and ONERA engaging in cooperative research. I believe that the initial letter that came from France proposed a cooperative program in helicopter dynamics. The concept was supported by DA, and Dick Ballard and Paul wanted to use this as an opportunity for Jim McCroskey to work with PQ in CFD, so an MOU was executed between the U.S., represented by the Ames Directorate, and France, represented by ONERA, for cooperative research in helicopter aerodynamics. The MOU had a provision for personnel exchange, and Jim was the first to go to ONERA. In 1971, I was named the Project Officer for this MOU and continued in that role until I left the Army in 1985. It was a very successful collaboration and continued, with Jim McCroskey as the Project Officer, for many years after I left.



U.S.-France MOU Project Officers meeting at Ames, 1972. From left: M. Fernand D'Ambra, Aérospatiale; M. Roland Dat, ONERA; Captain Gerard Bretecher, French Ministry of Defense and MOU Project Officer for France; M. Marcel Kretz, Giravions Dorand; M. Jean-Jacques Philippe, ONERA; and Irv Statler, MOU Project Officer for the U.S.



U.S.-France MOU Project Officers meeting at ONERA, 1973. Back row, second from left: Bob Ormiston, U.S. Army. Front row, from left: unknown; Roland Dat, ONERA; Irv Statler, U.S. Army; Philippe Poisson-Quinton, ONERA; Jean Renaud, Aérospatiale; and Jean-Jacques Philippe, ONERA.

The photo above left was taken at the first meeting of the Project Officers of the U.S.-France MOU at Ames in 1972. The photo above right was taken on the steps of ONERA Headquarters in Châtillon during the second meeting of U.S.-France Project Officers in September 1973.

Subsequently, with encouragement from the Army, I was instrumental in initiating and serving as Project Officer for MOUs that provided for collaborative research between the U.S., represented by the Ames Directorate (later, the Aeromechanics Laboratory) and NASA Ames, and the following foreign nations represented by their comparable R&D organizations:

- DFVLR (representing Germany) for cooperative research in rotary-wing flight control and handling qualities.
- Agusta (representing Italy) for cooperative research in helicopter dynamics and simulation.
- The Ministry of Defense (representing Israel) for cooperative research in helicopter flight controls and display technologies.

My realization of international collaborations achieved four objectives. First, of course, they added expertise and different perspectives to pursuing R&D on subjects of mutual interest. Second, they contributed to establishing the stimulating environment that I sought for our researchers. Third, they strengthened our international recognition. Fourth, they developed strong, lasting, beneficial friendships among the researchers, especially those who participated in the personnel-exchange programs that were provisions in each of the agreements.

The Army-NASA Joint Agreement was a significant factor in establishing most of these international collaborations. To the benefit of the Army, foreign research organizations liked to announce their associations with NASA.

Farewell to Army Aviation Research

In 1984, I was selected by the AGARD Board of Directors to be the Director of AGARD for 3 years starting July 1, 1985. The U.S. Air Force was the Executive Service for AGARD, so I had to leave the Army and transfer to the Air Force as a Senior Executive. My wife and I were required to take the French language course at the Army's Defense Language Institute Foreign Language Center (DLIFLC) in Monterey starting in April. On March 26, trucks arrived at our home in Los Altos to cart off some of our belongings to long-term storage in some military warehouse, as arranged for us by the Navy at Moffett Field. Three days later, another flatbed truck arrived for the stuff that was going to Paris. We sold our house and, on Sunday, March 31, Renée and I left the Bay area for Monterey.



Farewell to the Army Aeromechanics Laboratory, 1985, and off to Paris.

Our French language instruction at the DLIFLC was a memorable experience. We were the “old folks” among very young military men and women. Regrettably, we had to leave the DLI in June, before the end of the course, to go to Paris. On our last day, the faculty and our fellow students held a special “graduation” ceremony for us and presented us with diplomas, even though we had not completed the full course.

On June 13th, we returned to Mountain View to check out of the Army and NASA. I was grandly “roasted” by the staff of the Aeromechanics Laboratory and NASA Ames at a send-off lunch. Paul attended the lunch. We had lots of laughs.

As I reflect on my 15 years with Army Aviation R&D three decades later, from the perspective of an aging brain and after two intervening totally different careers, I have only positive memories of my years as a rotor-head. They were challenging and rewarding. The primary challenges of my job were 1) to embrace, enhance, and exploit the Army-NASA collaboration; 2) to expand the activities of our Directorate into new areas of R&D that would advance Army aviation; and 3) to establish international relationships and reputation. I believe we met each of these challenges with remarkable achievements.

I thoroughly enjoyed my years with the U.S. Army Aviation Systems Command and the U.S. Army Aviation R&D Command. I liked the work and the opportunities it gave me to innovate. Most especially, I appreciated the people in the Army, in NASA, and in many other agencies and organizations with whom I had the pleasure to work and with whom I established long-term friendships.

I feel that my most important accomplishment was my participation in proving that two independent Federal agencies could sing and dance gracefully together as they shared personnel and facilities to conduct research and development in areas of common interest without any transfer of funds. What a crazy notion!

Epilogue

From July 1, 1985, until June 30, 1988, I was the Director of AGARD, the Advisory Group for Aerospace R&D to the NATO Military Committee, and Renée was considered the “Directress.” Socially and professionally, it was an experience beyond our wildest expectations. AGARD HQ was in Paris, and its Director was considered a Diplomatic Post by the American Embassy. The French Air Force provided the Director with a car and driver. The Paris police provided the Director with security escort. The AGARD Director was expected to visit the NATO nations, and Renée and I were treated royally when we did. I mingled with the elite of aerospace R&D across NATO at levels of the Ministries of Defense, and with the heads of research institutes and aerospace industry, while Renée played hostess for their wives. We made many friends with whom we still stay in touch. We would have happily stayed on if it had been possible.

When it came time to return from Paris, I was offered the position of Chief Scientist for AMC. I turned it down. I had been a manager of R&D for nearly 30 years, and I wanted to see if I could still do bench research.

While I was the Director of the Ames Directorate and later, the Army Aeromechanics Laboratory, I became enamored with the subject of human factors of aviation safety. As Director of AGARD, I briefed the Military Committee on the problems pilots were encountering with the increasing automation being installed in their military aircraft. So, I sought a position at NASA Ames Research Center to “apprentice” in human-factors research. I was offered and accepted a position in the Human Factors Research Division but with a downgrade from Senior Executive Service to GS-15.

I embarked on a whole new career. I became head of the Office of Space Human Factors and I served as Chief of the Human Factors Research Division for 2-1/2 years. I developed and led a large innovative project called Aviation Performance Measuring System (APMS) to monitor commercial aircraft continuously in nearly real time. APMS evolved into an even larger project called Aviation System Monitoring and Modeling (ASMM) under NASA’s Aviation Safety Program. Using the technology we developed under the ASMM project for extracting and merging information from very large dispersed data sources, we demonstrated the concept of a Distributed National FOQA⁶ Archive (DNFA) with data from 10 airlines. We handed the DNFA off to the Federal Aviation Administration (FAA). I retired in 2008 after 20 years with NASA, but volunteered as an Ames Associate for another 6 years. I finally vacated my office at NASA in 2014 when I was 90 years old.

⁶ Flight Operation Quality Assurance (FOQA) was a name given by the FAA to a performance-monitoring concept that was developed many years earlier by several European airlines.

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Index

A

Acurio, John, 24
 Advanced Research Systems Integration Simulator (ARSIS), 18
 Advanced Systems Research Office (ASRO), 25
 Advisory Group for Aerospace R&D (AGARD), 26-27, 29-30
 Aeromechanics Laboratory, 14, 24-25, 28-30
 Aeronautical Design Standard (ADS), 17-18
 Agusta, 25, 28
 Aiken, Bill, 27
 Aiken, Ed, 16-17
 Ames Research Center, 3, 7, 11-12, 14-19, 22-26, 28-30
 Andrews, Hal, 27
 Armstrong, Neil, 27
 Army Aeronautical Research Group (AARG), 13, 16, 24
 Army Aeronautical Research Laboratory (AARL), 11-14, 16, 24
 Army Air Mobility R&D Laboratory (AMRDL), 11-13, 17, 19, 23, 26-27
 Army Ames Directorate, 9-10, 12-13, 15-17, 23, 27-28, 30
 Army Aviation Materiel Laboratories (AVLABS), 9, 11, 16

Army Aviation Research and Development Command (AVRADCOM), 23-24, 26
 Army Aviation Systems Command (AVSCOM), 13, 16, 22-23, 26, 29
 Army Langley Directorate, 11-12, 22, 25
 Army Lewis Directorate, 12, 22, 25
 Army Materiel Command (AMC), 11-12, 22, 26, 30
 Army Research and Technology Laboratories (RTL), 23-25
 Army-NASA Joint Agreement, 11, 14, 16, 18, 23, 28
 Ashkenas, Irving, 7
 Aviation Performance Measuring System (APMS), 30
 Aviation System Monitoring and Modeling (ASMM), 30

B

Ballard, Dick, 11, 19, 27
 Ballhaus, Bill, 14, 22
 Balmer, Robin, 27
 Beebe, John, 11, 19
 Bell Helicopters, 10, 18-20, 27
 Berry, Gordon, 12
 Bielman, Jack, 7
 Blade-Vortex Interaction (BVI), 15-16
 Boeing Vertol, 19-20
 Bolt, Beranek, and Newman (BB&N), 21

Borgman, Dean, 12, 19
Bousman, Bill, 14
Boxwell, Don, 16
Boyd, Jack, 22
Bretecher, Gerard, 28
Breuhaus, Walt, 7, 10
Bull, Giff, 7

C

CAL/AVLABS symposia, 16
Campbell, Graham, 7
Caradonna, Frank, 14
Carlson, Richard, 19, 23-24
Carr, Larry, 14
Carter, Ted, 27
CEPRA-19, 15
Chambers, Alan, 21
Cheilek, Harold, 10
Chen, Bob, 17
Coleman, Tom, 24
Computational Fluid Dynamics (CFD), 14, 27
Cooper, George, 7, 17
Corker, Kevin, 21
Cornell Aeronautical Laboratory (CAL), 4-11, 13, 16-17
Crawford, Charles, 24
Crimi, Peter, 8
Crossfield, Scott, 9
Curtiss, Pat, 7
Curtiss-Wright, 4, 10

D

d'Ambra, Fernand, 28
Dat, Roland, 28
Daughaday, Ham, 8
Deel, Arlin, 26
Defense Language Institute's Foreign Language Center (DLIFLC), 29
Department of the Army (DA), 11-12, 20, 22, 27
Deutsche Forschungs- und Versuchsanstalt für Luft- und Raumfahrt (DFVLR), 15, 18, 28
Deutsche-Niederlander Windkanal (DNW), 15
Distributed National FOQA Archive (DNFA), 30
Doetsche, Karl, 27
Dorand, Giravions, 28
Dowell, Earl, 14
Dugan, Dan, 19
DuWaldt, Frank, 8

E

Edwards Air Force Base, 6
Edwards, Glen, 6
Empey, Wayne, 14
Erickson, John, 8

F

Federal Aviation Administration (FAA), 30
Flax, Al, 10, 27
Fletcher, Jim, 19
Flight Mechanics Panel (FMP), 26-27

Flight Operation Quality Assurance (FOQA), 30
Frey, Bill, 7
Ft. Eustis, 9, 11-12

G

George, Bob, 14
Gerlach, Otto, 27
Gessow, Alfred, 9
Giulianetti, Demo J., 19
Goldwater, Barry, 20
Gonzales, Orlando E., 26
Goorjian, Peter, 14
Grace, John, 8
Graham, Dunstan, 7

H

Hamel, Peter, 27
Hamilton, Bill, 27
Harper, Bob, 7, 17
Hartzell, Jim, 21
Hiller Helicopters, 13
Hirtreiter, Walt, 7
Hodges, Dewey, 14

I

Infanti, Nello, 7
Institute for Defense Analysis (IDA), 27

J

Jackson, Roy P., 19
Johnson, Robert L., 19
Joint Aeronautical Research Group (JARG), 13, 24

K

Kayten, Gerry, 27
Kelly, Mark, 9
Key, Dave, 16-18, 24-25
Kidd, Ed, 7
Kidder, Bob, 7
Klein, Norm, 11
Klineberg, John, 27
Kretz, Marcel, 28
Kurylowich, George, 8

L

Laberge, Walt, 27
Laitone, Edmund V., 4-5, 7, 9
Lakinsmith, Barry, 22
Lappi, Oscar, 7
Larson, Leif, 7
Laub, Georgene, 16
Lauver, Dean C., 9
Lazzeroni, Frank, 24
Lebacqz, Vic, 16-17
Lecomte, Pierre, 27
Lehman, John, 20
Lewis, Richard, 24
Lockheed, 3, 14, 23
Loewy, Robert G., 9
Long, Richard, 24
Lovelace, Alan, 27
Lynn, Bob, 27

M

Madelung, Gero, 27
 Maefs, Lee, 7
 Maisel, Marty, 19
 Man-machine Integration Design and Analysis System (MIDAS), 21-22
 Marec, Jean-Philippe, 27
 Mark, Hans, 19-20, 23
 Martin, Mike, 14
 McAlister, Ken, 14
 McCormick, Barnes, 9
 McCroskey, Jim, 13-14, 27
 McRuer, Duane, 7
 Memorandum of Understanding (MOU), 15, 18, 27-28
 Miller, Rene, 27
 Miller, Shorty, 7
 Milliken, William F., 4-8
 MIT, 7, 27
 Moffett Field, 11-12, 29
 Morse, Andy, 13, 24
 Muzzy, Cliff, 7

N

NASA Ames Aeronautics and Flight Systems Directorate, 17, 19
 NASA Ames Research Center, 7, 11, 14, 16-17, 19, 22-23, 26, 28-30
 NASA Langley Research Center, 11, 12, 17, 22, 25-26
 NASA Lewis Research Center, 12, 22, 25, 27
 NATO, 26, 30
 Netherlands Aerospace Laboratory (NLR), 15

O

Office National d'Etudes et de Recherches Aérospatiales (ONERA), 15, 27-28
 Ormiston, Bob, 13-14, 24, 28
 Osprey, 20

P

Perkins, Courtland, 3, 6-7
 Peters, Dave, 14
 Philippe, Jean-Jacques, 28
 Piziali, Ray, 8, 17
 Poisson-Quinton, Philippe, 27-28
 Princeton University, 3, 7, 14

R

Renaudie, Jean, 27
 Reserve Officers' Training Corps (ROTC), 14
 Ritter, Al, 9
 Roberts, Len, 17, 19
 Robinson, Norm, 19
 Ross, Ira, 4-5, 10
 Rotorcraft Comprehensive Analysis System (RCAS),

14

Rutkowski, Mike, 14

S

Schmitz, Fred, 13, 15-16, 22
 Scott-Wilson, John, 27
 Seal, John, 6-7
 Seamans, Bob, 7
 Sears, Bill, 7
 Seckel, Ed, 7
 Second-Generation Comprehensive Helicopter Analysis System (2GCHAS), 14
 Sharpe, Dave, 14
 Shevell, Dick, 27
 Shores, Mel, 6
 Sibert, Harold W., 3-4
 Sikorsky, 27
 Sinacori, John, 26
 Sinclair, Mac, 27
 Sonnen, Henry, 7
 Sowyrda, Sandy, 8
 Spee, Ben, 27
 Stanford University, 23, 27
 Stapleton, Cyril, 11, 14
 Stevens, Story, 23-26

T

Targoff, Walt, 8
 Trenka, Andy, 8
 Tufts, Orren, 8
 Tung, Chee, 8, 14, 17

V

Van der Blik, Jan, 27
 van Doorn, Jan, 27
 van Oosterom, Theodore, 27
 Vause, Rande, 15-16
 Vertical Motion Simulator (VMS), 18
 vertical/short takeoff and landing (V/STOL), 8-10, 16-17

Vessey, John William Jr., 20

W

Wanner, Jean-Claude, 27
 Westbrook, Charles, 6
 Whitcomb, Dave, 7
 White, Richard P., 9-10
 Wick, Brad, 17
 Wynn, Tom, 14

X

XV-15 Tilt Rotor Research Aircraft, 18-20

Y

Yaggy, Paul, 9, 11-14, 17, 19, 23, 26-27, 29
 Yu, Yung, 15-16

2GCHAS, An Epic Effort, 1979–1991

Wendell Stephens
NASA/Army

In early 1977, I moved from Langley Research Center to Ames Research Center when Ames became Lead Center for rotorcraft research as part of a NASA consolidation effort in rotorcraft and structures work. I had worked in structures and structural dynamics research at NASA Langley and was not a part of the Langley Rotorcraft Group. But, since not many people wanted to leave Langley for Ames, I went over to those in charge of handling transfers and volunteered to move. NASA set up an Ames Helicopter Systems Office headed by Jay Christensen with John Zuk, Bill Snyder, and myself in the group. Zuk was to work with the propulsion group at what is now Glenn Research Center, Bill was to work on aerodynamic issues, and I was the head of the structures effort. I worked with that group for about 2 years, and then I transferred to the Aeromechanics Laboratory of the U.S. Army Research and Technology Laboratories (RTL) specifically to work on the Second Generation Comprehensive Helicopter Analysis System (2GCHAS) project or “2G Charlie” as it was nicknamed.



Wendell Stephens

Sometime in the 1970s, the Army decided it needed a computer program that could analyze any type of rotorcraft configuration—tandem, main rotor-tail rotor, side-by-side, coaxial, etc.—for structural, dynamic, and aerodynamic loads. At that time, they were using programs generated by the rotorcraft manufacturers or were relying on the company’s expertise for analysis. These “hard-wired” programs could only analyze the configurations that the company produced; this approach left a lot to be desired.

The responsibility for developing the program had originally resided with the Ft. Eustis Directorate, later named Applied Technology Lab (ATL) of RTL at Ft. Eustis, Virginia. The initial approach there was to submit the task to companies to determine which analysis concept should be used to analyze any rotorcraft configuration. These were referred to as Predesign Studies. ATL awarded three contracts and, after a period of time, they had three predesign reports for review. The approach was to move the 2GCHAS Project to Ames Research Center and build an internal team that could do the design job. The contractors would still be expected to execute the design and build the software system, but that was easier said than done. Andy Kerr recruited me in June 1979 to assist with the task and gave me the grand title of Lead Technology Designer. The 2GCHAS software was going to consist of an executive complex and a technology complex. The executive complex would assist the different technology modules in communicating together—sort of a quasi-operating system. John Davis was recruited to lead the executive complex development. Art Ragosta was recruited from ATL to make the hardware and software mesh. I do not think any of us really knew what we were getting into.

At first, Art tied us into a computer at our St. Louis Army headquarters, and we made an initial effort to try to use that computer. But the speed of communication in those days was more like

snail mail than high-speed communication. John found some graphics and analysis packages that would work in hopes of tying them into the executive complex with useful functions. I had a concept that I had brought with me from Langley. At that time NASA Langley, via a contractor (McNeal Schwendler), had developed a very successful structural analysis program called NASA Structural Analysis (NASTRAN). It had quickly become the industry standard, using a finite element approach to modeling. And, even in those early days of computer development, they had solved static problems of 20,000 degrees of freedom.

So, that was my approach. Finite elements had never been used in rotorcraft modeling and analysis, and not everyone—well, maybe no one—believed it would work. But the Ft. Eustis study concepts had not fared any better. At that time, I thought I was also a programmer, as I had developed a few research codes in FORTRAN. I was in for a rude awakening as to what programming really was.

2GCHAS did not make any noticeable progress for at least 5 years. We honed paper products—software plans, technical plans—and spent money on related issues, but never quite got off the ground. No one was pleased. Nothing we tried ever meshed. I also blamed the lack of progress on a set of requirements that was so broad that anything conceived of could become a requirement. One of the more memorable requirements was, “could we handle nuclear blast?” Well, no, but I guarantee you the pilot won’t know that. Could we model the helicopter by every known math approach and couple all types to aerodynamic models? I think the Ft. Eustis study and the reviewers thereof must have written down every conceivable requirement anybody could think of, and that list never really got filtered. It got reviewed but not filtered.

My first breakthrough came when we hired a contractor, Ru-Mei Kung, who began to show me how software is planned and how programs are structured. Ru-Mei, a computer scientist from Massachusetts Institute of Technology, helped us lay out the technology processes needed and showed us how to transform those processes into a program design. For the first time, I could see the natural divisions of the technical side of the program and how a finished product could be incorporated and integrated. I could see how we could assign parts of the code to multiple contractors and still have a complete program. In the meantime, John Davis had contracted with Computer Sciences Corporation (CSC) in October 1980 to define the executive complex. They had several good people in that group, one of whom was Larry Babb. He was a bulldog. He was tenacious. You either loved him or could not work with him, and I loved him.

In September 1982, Bob Ormiston was assigned as Project Manager of the 2GCHAS Development Project along with his job as Chief of the Rotorcraft Dynamics Division in the Aeromechanics Laboratory. He had great technical credibility with the contractors and the rotorcraft community. What I did not realize at the time was the level of dedication that Bob would bring to the project. He felt that as long as 2GCHAS was the Army goal, then it had to be supported by our best efforts. That level of dedication never wavered from 1982 to the present day. Andy Kerr, my first 2GCHAS Project Manager, had been asked by Dick Carlson to take on more duties as the new Chief of the Advanced Systems Research Office (ASRO) within RTL Headquarters (HQ) with the goal of eventually becoming Director of RTL when Dick retired. Our 2GCHAS development plan under Andy was documented in reference [1]. That basic idea continued to be refined over the years.

After about a year with Bob in charge, we all became convinced we were ready to finally approach the industry with various requests for proposals based on the technical packages my technical design had identified. We also had some restructuring within the Project Office. By this time Andy had recruited John Davis to become a lead engineer in the ASRO Preliminary Design Team, and we were now relying on CSC to continue to define and develop the executive complex. Also, Art had been recruited by Andy to head up the acquisition and maintenance of all the RTL's computers and software, so he was no longer in the office either. We did pick up Mike Rutkowski, Gene Ruzicka, Carina Tan, and Joon Lim, and they turned out to be great additions. At various times, Bob was also able to tap other people in his department for consultation and advice. These people included Dewey Hodges, Don Kunz, and Stu Hopkins. I also remember a CSC contractor, Dick Gemoets, and an on-site contractor, Lee Helme, as being valuable in the early stages of development.

About this time, we asked Bob for a dedicated computer just for 2GCHAS. I remember that Bob wanted a study to make sure we got the best computer for our needs, and I was in charge. At the first meeting with contractors, Lee Helme and Dick Gemoets, they told me they already knew the answer to the study. It was a VAX machine. I said, "Great, now let's produce the slides that show that." So, that study was done quickly and we bought the VAX. Computing power was advancing very rapidly at that time, and that speedy development probably saved the finite element approach.

Anyway, it was mid-1980s now and we were sending out our requests for quotations. There were to be six new contracts for the technology complex; the executive complex contract was already in hand. The six technology complex contracts were: 1) EL: Finite elements to model the structure, 2) LS: Linear systems, 3) AL/IV: Aerodynamics/induced velocity modeling, 4) AS: Assemble equations/solve, 5) TIP: Technology Input Processor, and 6) TOP: Technology Output Processor.

The first deliverables were the contractor technical designs that were to be reviewed at a meeting at Ames. We had had the contractors in the same room before, but this was the first time we were reviewing their products instead of presenting our plans. After the review, we sent all the contractors home to refine their designs, which were deemed to have a lack of detail.

About this time two things happened. Bob put me in charge of the project (May 1988) and Charlie Crawford, Technical Director of the Army's Aviation RD&E Center in St. Louis, Missouri, came to Ames to be a speaker at a local American Helicopter Society (AHS) meeting. I loved the first part but hated the second part.

I had never had any prior interaction with Charlie, but during his AHS presentation he made several derogatory public remarks about 2GCHAS and made us the butt of his jokes. By the time he finished, I was seething. I knew 2GCHAS was, and had been, a disaster for a long time; that was not the issue. For the first time, we were making real progress, and these remarks were a stab in the back. During my (real) Army tour one thing was stressed: you praise in public and you reprimand in private. It seemed like common sense to me. So, in the receiving line after his talk and while everybody was thanking Charlie, I got my chance to speak with him. By this time, I was really infuriated. I essentially cursed him out. In so many words I told him, "This is your damn project, you're the boss, and if you don't like it, you should damn well end it. But don't make your project and your team the butt of the joke in public!" along with a few more choice

words along that line. Fortunately, Bob was standing beside me and pulled me back a little. Charlie was too surprised to say anything then, and I did not stick around. I did not calm down for a couple of days, and I sent a long memo to Charlie through channels so that Bob, Andy, and Dick saw it too. Eventually, I got a “word-of-mouth” apology from Charlie through channels. Anyway, 2GCHAS was my baby now.

One thing I had learned over the years was that 2GCHAS had all of the incoming questions, complaints, and concerns of our contractors, and we had no outgoing instructions for them. That was going to change. I wanted incoming products and outgoing demands. No more excuses. We had the second design review coming up at Ames. I had already decided that the contractors would pass this one no matter what the products looked like. Mike Rutkowski and I came up with a 2-year plan in two phases: a 54-week phase for build 1 and a 50-week plan for build 2. Contractors would spend 1 week at Ames about every 6 weeks. Each meeting would include deliverables: verified software, Theory Manual inserts, User Manual inserts, and test cases they had run to verify the software. CSC would receive the software and integrate it and run the tests. Our Project Office (Mike) integrated the Theory Manual and CSC integrated the User Manual. We developed “Standard Operating Procedures” aimed at unifying the efforts. CSC had a little error-monitoring code that identified problems to be solved by the next build meeting. It was also during this time that I realized how vital Mike was. He had good common sense and would naturally fill in where needed and keep things going, and he was good at working with multiple contractors.

As you can imagine, 2GCHAS, with its horrible history, warranted a review as to whether to continue the program. Fortunately, the review came at the time that we had our grand 2-year phase 1 and phase 2 plans in place. Dick Carlson assembled his key people to help guide him in this decision. The room was full of people I admired at Ames and maybe a few others from elsewhere. I presented the plan and said we would have working software in 54 weeks and a program in another 50 weeks. I believed that at the time, but I found out later that my hopes were a little utopian. I would need a couple more heroes to bail me out. The review went well, and we continued to develop the program.

One of my new heroes was Wayne Johnson. We signed him to a consulting contract to help make sure the technology complex was correct as proposed. He was a great help in reviewing the theoretical approach and in recommending test cases. Nobody had more credibility with the rotorcraft community and in technology issues than Wayne. He did a great job reviewing our materials and working with the contractors. Later, Wayne continued to help us with the initial technical validation of the code. The other hero was a Stanford University Ph.D. from Iran named Hossein Saberi. Hossein worked on several 2GCHAS technology complex contracts at Advanced Rotorcraft Technology, Inc. (ART), a local Mountain View company founded by an ex-NASA engineer, Ron Duval. He essentially hired bright engineers with predominately Stanford backgrounds. They were mostly foreigners and, therefore, at that time, had difficulty getting jobs in defense department–related companies.

I remember when the contractors delivered their first infant software after 7 weeks, and I thought the integration to working software would go well. Well, it didn't. We had converted the Building N-215 conference room into a working lab for each delivery week throughout the build. Don Adams and Gary Vander-Roest, from our computer support group, set up seven or eight terminals in the conference room and connected them to our VAX for our contractors and the

Project Office to use. We could run and debug the software and try to work simple problems with all contractors on hand. The integration did not go well, but Hossein would stay with the assembled software into the wee hours of the night—night after night—debugging and working until he made progress. He did that with every interim build—every 6 or so weeks. Developing software now had some guidelines from CSC. You had to provide white space to make the code readable. You had to provide comments about what the lines of code were doing. You had to have test cases that showed your software worked, so that Hossein had some idea of what the programmer was trying to do and he could find the bugs. I had only one additional problem. Hossein and Larry Babb were both tenacious. Both were dedicated. And both had different ideas about how to resolve problems and develop software. There were clashes, but they had enough respect for each other and the project that we got the job done.

Sure enough, after 54 weeks of build 1 and 50 weeks of build 2 we had working software. We had 10 test problems identified that we wanted the system to solve. It took a while, but one by one they eventually yielded. I then realized that the testing phase was much more difficult, time consuming, and expensive than I had ever thought. Even after the second phase of build 2, it would take a long time to debug the software. Bob managed to find some funding to assist this post-development phase. Without ART and Hossein, the program might have gone away at this point. ART wanted to take advantage of the investment already made and market its analysis capability for the future, so they stayed on as a contractor with some funding from Bob. Eventually, the code found users in the technical community and at centers of excellence. Our approach was documented at various points of development in references [2-7].

I had requested permission to attend a mid-management training program called LEGIS in Washington, D.C., and went there in January 1991 for the year-long program.

When I got back from my training, Mike and Bob were running the project. It was now a process of making continual improvements, continual testing, and debugging; building a user community in industry, and continually promoting its use in research and analysis. Funding for the project at this point was hard to come by. ART was very interested in it, and Hossein was there to see it through to a successful conclusion. The program is now called Rotorcraft Comprehensive Analysis System (RCAS) and is a useful and capable industry tool. And, amazingly and intentionally, it has a structure that will allow continued technical advancements to be incorporated. I realize now that Bob Ormiston and Mike Rutkowski's long-term dedication was critical to the successful development of the program. I left in 1991. It would take another 10 to 15 years of development and use on real rotorcraft problems before the program would gain a foothold as one of the premier rotorcraft analysis systems. Bob continued to work with in-house and ART engineers to analyze and publish results of studies of interest to the rotorcraft community.

Strictly speaking, 2GCHAS was an Army project. From inception, however, it was conceived as an analysis system that would continue to grow as it developed. As such, it was a goal of 2GCHAS to add concepts developed in the NASA/Army research environment, as well as in universities and industry, to the program. Thus, current and future releases of RCAS will contain dynamic and aerodynamic math model concepts resulting from the Joint Agreement research efforts.

Was it worth it? I think so. Was too much money put into it? Yes, for sure. Did it take too long? Yes, of course. Was it a stretch in technical concept? Yes, it certainly was. How else could it have been done? I still do not see a better or shorter way. Even at our talented lab, there were not many other people—maybe not any—who would have taken it on. Bob was the exception. I very much appreciate the Army management's dedication to this task.

It is strange that the things that saved it were rapidly developing computer power and a few diligent heroes along the way. One thing I was proud of was how well the six or so contractor groups worked together during the 2-year build period. Although from competing companies, they developed a sense of cohesiveness that greatly helped the project. They looked out for one another and crossed company lines. Meeting every 6 weeks at Ames for a week at a time gave them a sense of team unity and an appreciation for each other. The photo below is of the people who came together at Ames to get the job done. At its peak we probably had up to 50 people working on 2GCHAS, and they gave it their best shot. But, in my view, Bob Ormiston and Hossein Saberi are the heroes of the story.



The 2GCHAS Development Team, March, 1990. From left to right, front row: Hossein Saberi, ART; Peter Johnson, CSC; Chris Dueker, CSC; Lisa Trinh, CSC; Dung Chau, CSC; Carina Tan, Army; Hamid Rafati, ART; Larry Babb, CSC; Skip McKenna, Kaman. Back row: Bob Ormiston, Army; Rupert Seals, Boeing; Mary Sredanovic, CSC; Wendell Stephens, Army; Carla Vernacchia, CSC; Karan Sangha, McDonnell Douglas Helicopter Co. (MDHC); Gene Ruzicka, Army; Jerry White, Sterling Federal Systems (Sterling); Dharmendra Kumar, Sterling; George Rogers, Sterling; Mike Rutkowski, Army; Bruce Gustavson, Kaman; Sebby Cassarino, Sikorsky; and Omar Amrani, ART.

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Index

A

Adams, Don, 4
 Advanced Rotorcraft Technology, Inc. (ART), 4-6
 Advanced Systems Research Office (ASRO), 2-3
 Aeromechanics Laboratory, 1-2
 American Helicopter Society (AHS), 3
 Amrani, Omar, 6
 Applied Technology Laboratory (ATL), Ft. Eustis, 1
 Army Aviation RD&E Center, 3
 Army, 1-3, 5-6

B

Babb, Larry, 2, 5-6
 Boeing, 6

C

Carlson, Dick, 2, 4
 Cassarino, Seby, 6
 Chau, Dung, 6
 Christensen, Jay, 1
 Computer Sciences Corporation (CSC), 2-6
 Crawford, Charlie, 3

D

Davis, John, 1-3
 Dueker, Chris, 6
 Duval, Ron, 4

F

Ft. Eustis, 1-2

G

Gemoets, Dick, 3
 Gustavson, Bruce, 6

H

Helmle, Lee, 3
 Hodges, Dewey, 3
 Hopkins, Stu, 3

J

Johnson, Peter, 6
 Johnson, Wayne, 4

K

Kaman, 6
 Kerr, Andy, 1-3
 Kumar, Dharmendra, 6
 Kung, Ru-Mei, 2
 Kunz, Don, 3

L

Lim, Joon, 3

M

Massachusetts Institute of Technology, 2
 McDonnell Douglas Helicopter Co. (MDHC), 6
 McKenna, Skip, 6

N

NASA Ames, Research Center, 1, 3-4, 6
 NASA Glenn Research Center, 1

Stephens, W.

NASA Langley Research Center, 1-2
NASA Structural Analysis (NASTRAN), 2

O

Ormiston, Bob, 2-6

R

Rafati, Hamid, 6

Ragosta, Art, 1, 3

Research and Technology Laboratories (RTL), 1-3

Rogers, George, 6

Rotorcraft Comprehensive Analysis System (RCAS), 5

Rutkowski, Mike, 3-6

Ruzicka, Gene, 3, 6

S

Saberi, Hossein, 4, 6

Sangha, Karan, 6

Schwendler, McNeal, 2

Seals, Rupert, 6

Second Generation Comprehensive Helicopter Analysis
System (2GCHAS), 1-6

Sikorsky, 6

Snyder, Bill, 1

Sredanovic, Mary, 6

Stanford University, 4

Stephens, Wendell, 1, 6

Sterling Federal Systems (Sterling), 6

T

Tan, Carina, 3, 6

Trinh, Lisa, 6

U

User Manual, 4

V

Vander-Roest, Gary, 4

Vernacchia, Carla, 6

W

White, Jerry, 6

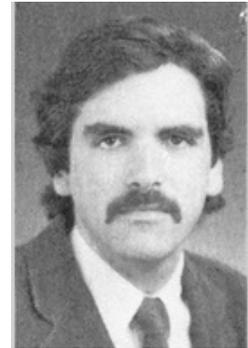
Z

Zuk, John, 1

“If You Follow Your Passion, There Will Always Be Great Opportunities”

Dr. Mark B. Tischler
Army

In the 1970s, with the end of the moon shots and cancellation of the U.S. Supersonic Transport program, there was a major downturn in the aerospace industry. I was in a “magnet” high school for engineering and had been interested in aircraft and rockets since elementary school. So, while I was sure I wanted to be an aeronautical engineer, I was worried that there might not be jobs or a career path for me in this field. In fact, my best friend at the time was also a model rocket enthusiast and was very interested in aeronautical engineering, but his father talked him out of it. My dad had always followed his dreams and visions, so when I asked him what to do when applying for college, he advised, “If you follow your passion, there will always be great opportunities.” So, I did. I never looked back and Dad was right.



Mark Tischler

The prologue of this chapter will address the influences in my life that brought me to the Aeromechanics Laboratory in January 1983, as the Ames-Army Lab was named at that time. I will then review key highlights of various initiatives that I started at the Aeromechanics Lab during the period from 1983 to 1985. Finally, in the epilogue, I will describe how these initiatives turned out in my succeeding years from 1985 up to the present, when the Ames-Army Lab was mainly known as the Aeroflightdynamics Directorate (AFDD).

I. Prologue

A Young Rocket Builder

Dad’s dad (my grandfather) was a Jewish tailor who had immigrated from Europe before WWI. My dad met my mom in Baltimore before WWII and stayed in touch with her throughout the war (Fig.1). Dad was a radar operator in the Pacific theatre during WWII, and he married Mom right after he returned at the end of the war. He could fix anything!

I had three brothers and a little sister, and we grew up in Baltimore, Maryland, near Mom’s family. Dad had a great basement full of model trains, and many tools for woodworking and electronics development and repair. He encouraged all five kids towards hobbies that involved working with our hands—building things from scratch. He taught me how to build a crystal radio with a hand-wound coil and circuit board, a model train and slot car racing table, and a model sailboat with a radio-controlled tiller and backup propeller—all from scratch, nothing from a kit. Dad was an electronics inventor and tinkerer. His shoes were huge; he had invented the first transistorized pacemaker (Fig. 2) and gave one to President Eisenhower’s doctor at the White House due to the President’s heart condition. Dad’s pacemaker was on exhibit at the Smithsonian Museum, and I still remember going to see it there and being so impressed as a young boy. He had several other patents in electronics and biomedical technology. So, with an aptitude for math



Fig. 1. Dad on Army leave to visit Mom during WWII (1943).

me the model rocket set was the best decision Dad had ever made! He was so proud and was always my greatest admirer throughout my career.

I started out making the standard Estes rockets from various kits, but of course I had to build my own fancy multiple rocket launcher from scratch (Dad wouldn't have it any other way). In high school, I became fascinated with the concept of a model rocket-glider. It would ascend as a rocket and then glide down as an airplane. This was a big challenge at that time in model rocketry. I studied all I could on model rocket and model airplane design and especially static stability, since there were no active stabilization systems on these early model aircraft. I tried many unsuccessful designs; one rotated horizontally after coming off the launch rail and flew down the street at about a 20-foot altitude, with me and Dad running in hot pursuit. It landed on the front yard of a neighbor near their infant son—his mom grabbed my rocket-glider design and threw it back at me in pieces. Back to the drawing board...

and an interest in building things, it was always clear to me that I would also be an engineer like Dad.

We stayed home from school to watch the NASA launches in the 1960s. Crowded around our tiny black and white TV, I was amazed and inspired. I was also awestruck watching the jumbo jets takeoff, like some kind of modern miracle. I started making rockets with cigar casings and matches in sixth grade. Then, in junior high school, I asked my parents for an Estes model rocket set for Hanukah. They thought about it for a few days, and then agreed—thus starting my career in aeronautical engineering. Many years later (2007), in my acceptance speech as a Distinguished Alumnus in Aerospace Engineering from the University of Maryland (UMD), with my dad beaming in the audience, I said that agreeing to buy

WESTERN EDITION
The New York Times.

NEW YORK, SATURDAY, NOVEMBER 9, 1963.

**Medical Engineer Patents
A Device to Restart Heart**

**Electric Stimulator Held
Effective Within Three
Minutes of Attack**

By STACY V. JONES
Special to The New York Times.
WASHINGTON, Nov. 8 — A medical device about the size of a transistor radio, and about as simple to operate, restarts human hearts.

Its inventor, Morris Tischler of Baltimore, calls his Cardiac Pacer a life-saving tool.

The heart stimulator, which was patented this week, is manufactured by Electronic Aids, Inc., of which Mr. Tischler is president, and is distributed to hospitals and physicians by the Westinghouse Electric Corporation.

Four thousand cases of sudden heart stoppage occur in this country every year, according to Mr. Tischler. The patient can survive, he says, if rhythmic electric pulses are applied to the cardiac nerves within three minutes.

He cites the case of a Florida surveyor who carried one of the stimulators in the field and taught his assistant to use it. By applying the plate electrodes to the surveyor's chest and switching on the battery current, the assistant enabled

Continued on Page 11, Column 1



Cardiac Pacer patent was awarded to Morris Tischler.



The device approximates a transistor radio in size.

Continued from Page 9

the surveyor to survive 50 heart attacks in two years.

In 1958 Mr. Tischler delivered one to Dr. Howard M. C. Snyder, physician to President Dwight D. Eisenhower, but he does not believe it was used.

The Cardiac Pacer is 3 by 5 by 2 1/2 inches, and weighs 2 1/4 pounds. It fits in a small handbag, along with wires, a strap and the plates. A doctor can carry it in his kit for emergency use. It can also be used during surgery.

Pulse Settings Vary

One knob controls the pulse-rate settings, from 25 to 250 beats a minute, and the other regulates the strength of the current. The pulse rate can be left at the desired setting, and stimulation can be started with the second knob.

His is the first transistorized stimulator to reach the market, Mr. Tischler says. It is protected by patent 3,109,430. He has also invented a monitor for it.

The monitor, which can be used while a patient sleeps, watches his pulse and turns the stimulator on and off. It also signals the nurse or doctor.

A 41-year-old electrical engineer, Mr. Tischler teaches electronics at the University of Maryland medical school, and has helped set up similar courses for doctors at half a dozen other colleges. His company makes medical instruments and educational aids.

Fig. 2. Morris Tischler (Dad) invents the world's first transistorized pacemaker (1963).



(a)



(b)

Fig. 3a. Model rocket glider ready for launch; b. successful wing deployment and long glide back (circa 1973).

Finally, in the 11th grade, I perfected my design. The wings were folded back and secured to the horizontal tail in the ascent (Fig. 3a). This gave the boost configuration both aerodynamic efficiency and, most importantly, strengthened the wings against a flutter failure. When the solid rocket engine propellant was expended, it shot a burst of hot gas from the other end and self-ejected. This released the strings that held the wings folded in place. The wings would snap forward by a shock cord (a heavy rubber band) and the rocket became a glider and quickly self-stabilized (Fig. 3b). The glider stayed airborne for a very long time, with a trim tab to cause it to circle for a long duration over the local community and land softly in a neighboring backyard. I was elated that it all worked flawlessly! I had also greatly improved on the published duration record of the time.

Aeronautical Engineering Studies at the University of Maryland

With my fascination with airplanes and the space program, and all of my efforts with model rocket gliders since I was about 11 years old, it was clear to me from an early age that I wanted to be an aeronautical engineer (they hadn't yet adopted the title aerospace engineering). I have always felt fortunate that I never struggled with the "How do I know what to study?" question. Dad had a degree from UMD, and they had a good in-state Aeronautical Engineering program, so it was all decided that I would go there. As it turns out, my Uncle Jerome "Yanks" Shafer, my mom's brother, was also an aeronautical engineer. He had worked at NACA Langley, and in 1952 moved to Israel and helped found and become a Dean of the Faculty of Aeronautical Engineering at the Technion-Israel Institute of Technology in Haifa, where he taught as a professor. On a visit to Baltimore, my uncle took me around the Aero Department at UMD and introduced me to the faculty with whom he had acquaintances. Through his introductions, once enrolled at UMD, I found myself working part-time in the machine shop under the guidance and mentorship of a wonderful Scottish machinist, Jim Deveney, throughout my undergraduate years. I also worked a stint in the Glenn L. Martin Wind Tunnel as a tunnel operator and technician. My mom passed away in 1976 when I was a junior at UMD, and this had a profound influence on

me. I considered dropping out of school, but Professor John Anderson (the celebrated author of many great textbooks and a curator of the National Air and Space Museum), the then Aero Department Chairman, encouraged me to stick with my studies. I turned inward, becoming a much more serious student, both as a coping mechanism for the terrible loss and to make my mom proud in light of her tireless efforts in raising us. I flourished in the department.

I was especially interested in flight dynamics, and I picked up my rocket-glider project from high school as an aeronautical engineering senior project at UMD. I eventually added a second stage to achieve more altitude in the boost phase, and added a modern radio transmitter for trim tab control during the ascent and maneuvering control during the glide phase. I researched and used low Reynolds number glider airfoils to maximize glide phase endurance. I also made detailed computerized stability and control calculations that allowed me to move the rocket motors to the rear for better aerodynamic streamlining and simplify the wing release mechanism. I tested the entire design in the Glenn L. Martin Wind Tunnel to validate my stability and control calculations. The final design was named MJ-II (M-Mark; J-Jerry Wang, a close friend and my pilot). It flew great at the local Comsat campus!

We tweaked the design and launch trim slightly and flew it many times from winter 1977 to spring 1978 (Fig. 4). Professor Anderson graciously provided department funding for professional filming of several of my MJ-II rocket-glider flights in support of my paper for the American Institute of Aeronautics and Astronautics (AIAA) student competition. The paper [1] and presentation won First Place at the AIAA Mid-Eastern Regional Student Competition held at Pennsylvania State University. I was really elated the entire drive home and couldn't wait to tell my dad.



(a)



(b)

Fig. 4. MJ-II dual stage tests at Comsat, Greenbelt, Maryland: a. ready for launch (Mark, left, and Jerry, right); b. perfect launch (winter 1977–spring 1978).

My advisor for the student project was Professor Jewel Barlow, the long-time Director (40 years as of 2017) of the Glenn L. Martin Wind Tunnel. Professors Barlow and Anderson encouraged me to apply for a Minta Martin research scholarship, which I received, and so I decided to stay at UMD for a Master's degree program, with Professor Barlow as my thesis advisor. We worked together on the prediction of general aviation aircraft spin characteristics, which was of great interest and concern at the time because of the high number of spin accidents—especially in the Grumman Yankee, F-4, and F-14. We came across an excellent research article from the Italian horizontal spin tunnel that demonstrated a graphical approach for determining steady-spin equilibrium trim points. Dr. Barlow immediately recommended that we hire a translator—and the resulting paper was a breakthrough for us. Using experimental data obtained in the NASA Langley vertical wind tunnel, we demonstrated good prediction of both aircraft spin and recovery characteristics based on determining the presence (and disruption) of equilibrium spin conditions [2]. We also developed a nonlinear dynamic simulation that validated the equilibrium spin characteristics and well-predicted dynamic spinning motion and linearized modal characteristics [3]. The successful prediction of spin and recovery had eluded NASA researchers for some time, so this was a breakthrough in the field. Professor Barlow was a wonderful mentor, and he taught me how to do basic research—with a good literature search and annotated bibliography, and how to spot key leads. He gave me room to try out ideas and has remained a wonderful colleague. I received my Master's degree in Aerospace Engineering in December 1979.

My First Job at Systems Technology, Inc. (1980–1982)

Professor Barlow mentioned that the premier company for flight dynamics and control was Systems Technology, Inc. (STI) in Los Angeles and suggested that I contact them for an interview. I will always be grateful for this wonderful suggestion. STI was a small company (about 50 employees at that time), so I wouldn't get lost and would quickly be given a lot of responsibilities beyond my years, as compared to being one of many at a large company. Duane “Mac” McRuer and Irv Ashkenas founded STI. They were storied giants in pilot modeling, aircraft flight dynamics and automatic control, and handling qualities. They had been Chief of Flight Control and Aerodynamics, respectively, and were the key contributors to the Northrop flying wing (YB-35) among many other WWII-era aircraft. Together with Dunstan Graham, they wrote *Aircraft Dynamics and Automatic Control* [4], the standard reference book still famous today.

Irv hired me and I worked for him for most of my time at STI. Living in the Redondo and Manhattan Beach towns was a great change for an East Coast kid, and I loved the proximity to the ocean. Mac and Irv were both very generous with their time, knowledge, perspective, and guidance. Irv could look at my complex derivation for the stability derivative of a new configuration and say, “It can't be right—it must approximately be equal to 1/4 the lift curve slope (or something similar)... .” I thought, it's a new configuration, nothing like his many WWII aircraft designs. But, after several hours of checking, I found a decimal point error and Irv's approximation was right to within 20 percent! I was also very fortunate to be office mates with Tom Myers (who later took over Irv's position as STI Technical Director). Tom was a great mentor, role model, and still-close friend. He taught me how to work from first principles and use STI's famous approximate factors and coupling numerators, and he modeled his incredibly hard work ethic. The environment at STI was magical, with so many “greats” in flight dynamics and control there at the time: Mac, Irv, Tom, together with Henry Jex, Bob Ringland, Bob Stapleford, and Roger Hoh, just to name a few. These veteran “STI-ers” greatly valued first

principles methods such as lower-order dynamics approximations, classical frequency-domain analysis using Bode and Bode-root locus plots, coupling numerators, time vectors, first principles aerodynamics models, etc., that all provide insight and transparency to otherwise complex problems. I read as many old STI reports as I could get that explained and illustrated these methods on a multitude of aircraft projects. I also learned to write detailed reports and papers with better clarity and attention to details.

STI engineers had developed the “frequency-sweep” testing method and an associated Fast Fourier Transform (FFT)-based system identification software analysis package. These tools allowed the extraction of dynamics equations of motion from test data with primary application to fixed-wing aircraft (and ground vehicles). They also developed an early software package for integrated control system design and analysis. I eagerly learned all of these STI methods, and absorbed all I could from the flight control “masters” that I worked with. They all took me under their wings and gave me confidence and knowledge of wonderful frequency-domain tools—a great start to my career, for which I have always been grateful. Most of what I have done for the rest of my career had its start in some way in the 3 magical years at STI, and thanks to the encouragement of Mac, Irv, Tom, and the others there. The STI methods and ways of thinking about problems have stayed with me and shaped my career. I have passed them on to my group members and my Master’s and Ph.D. students over the years.

My main project at STI was a NASA concept called a hybrid heavy-lift airship. It was to be a large (Goodyear-sized) blimp surrounded by four large helicopters. The concept was supposed to be well suited to heavy-lift missions, and STI had a contract with NASA Ames to develop a simulation for flight dynamics and control of this concept. I was responsible for the engineering research and software development. It was a huge project, and at first I wasn’t that excited about working on an airship; I had wanted to work on conventional transport and fighter aircraft (which I did eventually at STI). But developing the airship simulation turned out to be a great project, with many complexities, unique aeronautical aspects, and opportunities to be creative in aerodynamic and simulation modeling. I researched the airship and helicopter aerodynamics, digging into old reports from the early 20th century on airships by Munk [5], and learned the basics of helicopter simulation from Bramwell [6], and Gessow and Meyers [7]. I implemented the simulation model in a large multi-body system in Fortran and exercised the model in research studies of the NASA heavy-lift hybrid airship conceptual design [8]. A key conclusion gained from our research work was that while buoyancy was great for lift and long endurance, it also made the airship very sensitive to turbulence, and not able to hover (or moor) precisely in gust environments, without imparting huge loads to the structure [9]. This downside of buoyant vehicles has often been overlooked.

II. My Early Years at the Aeromechanics Laboratory (1983–1985)

Based on my work on the NASA airship contract, I received a wonderful recommendation from Dr. Hiro Miura (NASA) to Dr. Fred Schmitz, who was starting a Simulation Office at the Aeromechanics Laboratory (see Fred Schmitz’ chapter). Fred pitched me a job position in his new office as a chance to work on simulation for the Army, as well as an opportunity to enroll in the NASA honors co-op program and pursue a Ph.D. at Stanford University. The Army’s ability to access the NASA honors co-op system was a huge benefit of the Army-NASA collaboration. This was a major draw for many of my Ames-Army Lab colleagues who wanted to pursue

advanced degrees. I struggled over the decision to take the Army offer, since I really enjoyed working at STI and the Los Angeles lifestyle (windsurfing, Karate training, and living near the beach). Irv Statler, the Aeromechanics Laboratory Director, encouraged me to join the Ames-Army Lab. When I was trying to understand the relative benefits of an Army versus NASA position, Irv emphasized that research work in the Ames-Army Lab would have a great potential for impact in the Army's large fleet of rotorcraft—and he was right! This all sounded exciting, especially combined with the Stanford Ph.D. opportunity. In the end, I realized that the chance to go to Stanford was a once in a lifetime opportunity. Irv Ashkenas and my girlfriend at that time from STI also felt it was an exceptional opportunity, though they were sorry to see me leave. My dad wanted his son to be a “doctor” and encouraged me too, so off I went to Northern California in January 1983.

My first Army assignment in Fred's Simulation Office was to work on assessing the fidelity of the XV-15 tiltrotor simulation model using flight-test data, in preparation for the larger JVX program that the Army led at the time that later became the V-22. The successful development and flight test of the XV-15 Tilt Rotor Research Aircraft was perhaps the most widely acclaimed outcome of the Army-NASA collaboration in partnership with Bell Helicopter. Fred was a very supportive boss, and he helped me understand the Army politics. Fred continued to be a great mentor, supporter, and sounding board throughout my career. But after a short period of time, the Simulation Office was disbanded, and I transferred to the Ames-Army Lab Flight Controls Division, headed by Dave Key, the Division Chief. Under the unique Army-NASA Joint Agreement I was located within the famed NASA Flight Dynamics and Controls Branch, led at the time by Dr. Jack Franklin (my first NASA boss), and later by Dr. Vic Lebacqz, and the Army group leader, Ed Aiken. They all really encouraged me to strike out in my own areas of interest.

Soon after starting my Army career, I entered the Ph.D. program under the NASA honors co-op program. When I was preparing to leave STI for Ames, Mac advised me to contact Professor Arthur Bryson (a good friend of his) about being my advisor. They didn't always agree about the relative value of classical-versus-modern control methods, but they were great friends and life-long hiking buddies who truly admired each other. Arthur Bryson was a terrific professor, emphasizing the importance of first principles analysis of complex engineering problems and the value of seeking physical insight—both of which resonated greatly with me. Professor Bryson was also a wonderful advisor, mentor, and role model, and was instrumental in helping me through the various hurdles of the Ph.D. marathon.

My technical career at the Ames-Army Lab from 1983 to present has spanned the technical areas of system identification, flight simulation, flight control design and optimization, handling-qualities criteria development, and flight-test methods. Applications of these methods in my flight control group have included over 50 aircraft and rotorcraft programs, manned and unmanned aerial vehicle (UAV) configurations, and wind tunnel tests. I have also had extensive roles in Army strategic planning and bilateral and multinational collaboration. In this part of my memoir, I will focus on the three main strands of my early career in the Army Aeromechanics Lab that heavily influenced my career to date: 1) system identification, 2) international collaboration, and 3) flight control design and analysis. All three of these strands had their start during the 1983–1985 period and persisted throughout my career. I will highlight how they all turned out in the epilogue.

System Identification

In support of my first job assignment and also my Ph.D. dissertation, I conducted frequency sweep flight tests and associated frequency-domain system identification of the XV-15 tiltrotor aircraft in hover and forward flight (Fig. 5).

The objective of the work was to validate the XV-15 simulation model and analyze the flight dynamics characteristics. I had learned the frequency-sweep test and analysis techniques at STI for use in conventional fixed-wing aircraft, and I didn't think twice about suggesting it for the unique application of the XV-15 tiltrotor aircraft. As a young and eager engineer, I did not consider at the time that this was a new test technique for hovering "rotorcraft." Later, Irv Statler pointed out his earlier work on system identification on the North American B-25J aircraft using sine-wave excitations and a mechanical harmonic analyzer (see Irv Statler's chapter), which I referenced in my Ph.D. dissertation [10]. My advancements in system identification during these early years and shortly thereafter included:

- A more accurate and flexible Chirp-Z (or "zoom") transform as compared to a standard FFT.
- Multi-input/multi-output (MIMO) frequency-response identification methods.
- Composite window averaging that avoided the compromise of a single sliding window.
- Stability and control derivative identification from a MIMO matrix of frequency responses.
- Integration of theoretical accuracy metrics in an integrated model structure determination procedure.
- Robust search algorithm for extracting state-space equations of motion from frequency-response flight data.
- Demonstrating the applicability and limits of these methods to the determination of unstable dynamics of hovering helicopters from closed-loop test data.
- Time-domain verification including sensor bias and reference shift corrections as needed for applications to unstable dynamics systems.



Fig. 5. NASA/Army Bell XV-15 aircraft; hover (left) and cruise (right) configurations.

Many of these advancements were adaptations of previous system identification work at Bell Labs, STI, the Air Force Flight Test Center, NASA Langley, and NASA Dryden. Some of the new advancements, like the composite windowing, took many weeks of experimentation with different ideas, and then a flash inspiration while still at my office desk late into the night. The composite windowing optimization algorithm combined five spectral windows of varying length into a single “composite” frequency response with exceptional broadband accuracy. Composite windowing removed the “black box” dependency of frequency-response calculations on the choice of spectral window length. Also, I adapted the Secant search algorithm from the trimmer in my Master’s degree aircraft spin simulation for the identification of state-space (equations of motion) models from the frequency-response data; this algorithm has worked great on increasingly larger and more complex identification problems. Pioneering work in system identification at NASA by Iliff and Maine [11], and by Milne [12], an earlier Ph.D. student of Professor Bryson, was incorporated for reliable model structure determination and reduction, and reliable metrics of identified parameter accuracy. The state-space model identification has proven to be very robust to uncertainties in the test data and initial estimates of the state-space parameter values, for a wide range of system identification applications, including high-order and dynamically coupled rotor/body equations of motion. The key aspects of the resulting “frequency-response identification method” were explained and illustrated with application to the Bo-105 helicopter in reference [13].

The frequency-domain system identification methods provided accurate linearized transfer function and state-space models of the XV-15 that matched the flight-test data very well and validated the Bell Generic Tilt Rotor (GTR) simulation (Fig. 6) in hover and forward flight [14, 15]. The left image in Fig. 6 shows frequency response and state-space model identification in hover flight data and GTR simulation model agreement. The right image in Fig. 6 shows excellent time-response agreement of the identified model with flight data. The GTR simulation model later became the basis for the V-22 tiltrotor simulation used in the development of this highly successful Navy/Marine rotorcraft. I soon applied these methods to the Bell 214ST (Fig. 7), a first for a conventional helicopter [16], and then to the CH-47 variable stability helicopter in use in our Flight Dynamics and Controls Branch [17]. Demonstrating frequency sweeps and system identification as a safe and practical means to extract accurate frequency responses and linear models for helicopters turned the tide in the ongoing argument of time-domain versus frequency-domain characterization for Aeronautical Design Standard-33 (ADS-33) handling-qualities requirements. The frequency-domain characterization of helicopter dynamics was adopted in ADS-33 and over the following decades, frequency-domain system identification methods became a rotorcraft industry standard. This required perseverance in the face of early criticism about the safety of the frequency-sweep testing procedure [18]; today, however, frequency-sweep testing is often conducted in the early flights of a new helicopter in order to validate the flight control system stability margins and validate/update the math models and associated flight control gains.

Together with an excellent programmer, Mrs. Mavis Cauffman, we evolved my system identification code, first developed for the XV-15, Bell 214ST, and Bo-105, into a sophisticated graphical user interface (GUI)-based tool CIPHER[®] (Comprehensive Identification from Frequency Responses), first released in 1993 [19] and now a worldwide standard. The frequency-domain methods and CIPHER have proven effective for a wide range of applications for manned and UAV aircraft and rotorcraft dynamics. CIPHER was initially applied to the

characterization of structural dynamics response for the XV-15 aircraft [20]. It took nearly 20 years to see our early rotorcraft frequency-sweep test and identification methods widely adopted, but the concerted efforts paid off after many flight-test demonstrations, conference papers, books, short courses, and software development and dissemination—and now the U.S. Army is considered the world leader in this field.

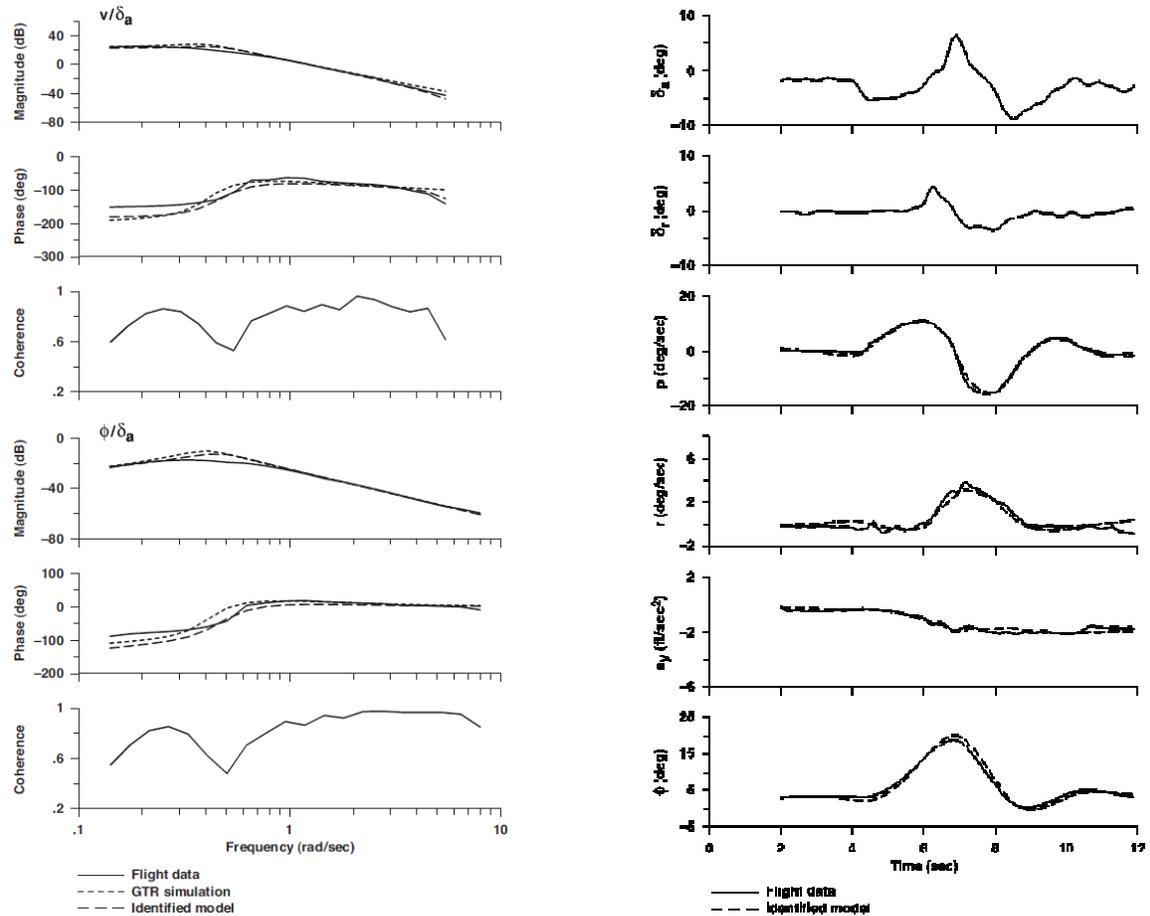


Fig. 6. Example system identification results for XV-15 in hover.



Fig. 7. Bell 214ST used to demonstrate frequency-sweep testing and system identification techniques for a conventional helicopter.

Another one of my first projects at Ames was the continuation of my work on airships. In 1983, NASA had arranged to conduct frequency-sweep flight tests and system identification analyses on a modern ducted fan blimp, the British Airship Industries Ltd. Skyship 500, at the Patuxent River Naval Air Station. These tests were to validate the STI simulation model and document the modern airship dynamics and control characteristics. NASA asked test pilot George Tucker and me to lead and participate in the tests; we monitored the tests in the backseats as the British pilot executed the frequency sweeps over the Chesapeake Bay, the first such tests for system identification of an airship (Fig. 8). This was the first of many frequency-sweep and handling-qualities flight tests and road trips that George and I worked on together as an Army-NASA collaborative effort. From my earliest days at Ames, George was a wonderful mentor, role model, colleague, and the older brother that I always wanted—the one I imagined while watching Wally in the *Leave it to Beaver* TV show reruns when I was young. The identification results showed how well the frequency-sweep method worked for this unique application, and validated the accuracy of the airship simulation modeling methods, and especially the implementation of the apparent mass terms [21]. It was a great epilogue to the airship work, though it seems that interest in lighter than air (LTA) concepts is “reborn” about once every 10 years. Each time, the proposer of the new LTA configuration emphasizes the benefits of the lifting ability, but often doesn’t fully understand that the buoyancy and associated apparent mass effects also dominate the dynamic response to turbulence.

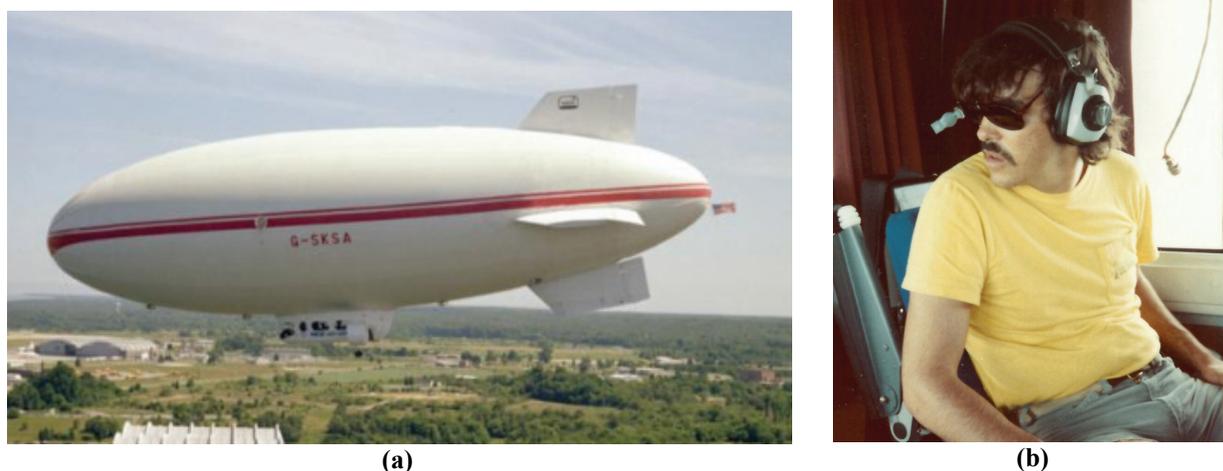


Fig. 8. Frequency-sweep flight tests conducted to validate airship modeling concepts: a. the Skyship 500; b. Mark Tischler monitoring the testing in the back of the cabin (1983).

International Collaboration

From 1985 to 1989, I was a Principal Investigator (PI) in the U.S.-Germany Memorandum of Understanding (MOU). I had the wonderful opportunity to share ideas and collaborate with my counterparts at the German Aerospace Research Establishment (DLR) in Braunschweig. The U.S. team had PIs from both AFDD and NASA Ames, and the joint meetings and trips to Germany together provided a great opportunity to deepen our working relationship. The German PIs used a time-domain system identification approach and I used my frequency-domain approach on common helicopters. I learned an immense amount from the German PIs, Jürgen Kaletka and Wolfgang von Grünhagen. After each meeting with my German colleagues, I came away with many new ideas and areas that needed improvement in the capabilities of CIFER. We

published several joint conference and journal papers comparing the results and relative benefits of our two methods for flight dynamics identification of test data from the NASA/Army XV-15 tiltrotor [22] and DLR Bo-105 helicopter [23]. We learned an enormous amount from each other and, in the end, we each integrated the best aspects of the other's approach.

This collaboration evolved into the Advisory Group for Aerospace Research and Development (AGARD) Flight Mechanics Panel Working Group 18 on Rotorcraft System Identification, led by Dr. Peter Hamel, Director of the DLR Flight Mechanics Institute, and already a supporter and colleague from our interactions during the U.S.-Germany MOU meetings. Jay Fletcher and I participated in the working group as representatives for the U.S. Army (1987–1990). The focus of these efforts was to explain and compare the various methods of the member nations for rotorcraft system identification as applied to three common helicopter flight-test databases (Bo-105, SA 330 Puma, and Apache). A working group that also included collecting special flight-test data and comparing our results was an ambitious undertaking and quite technical for an AGARD working group. The great achievement of this working group was AGARD-AR-280 *Rotorcraft System Identification* [24]. This comprehensive AGARD report presented the requirements for instrumentation, flight tests, data analysis, best practices, comparison of methods and results, and examples of how the results can be used. The working group was unique in that flight-test data was specifically gathered for members to compare analysis with their own methods on a common database. Also, while many system identification studies use simulated data, this report used only actual flight-test data, thus giving the work and its conclusions excellent credibility. AGARD-AR-280 remains one of the best references on system identification methods and results for rotorcraft. It is a testament to the leadership skill of Peter Hamel that the research and report were completed in 4 years by a team of 16 independent practitioners. The AGARD Lecture Series 178 [25] that condensed the report material into a 2-day short course was presented at several locations in Europe and the U.S. in 1991. One of the locations was at UMD, hosted by my Advisor Professor Jewel Barlow (Fig. 9). To come full circle in this way was a fitting closure for my technical education.



Fig. 9. AGARD Lecture Series at the University of Maryland (fall 1991). From left: Dr. Peter Hamel, Professor Jewel Barlow, and Dr. Mark Tischler.

As wonderful as the technical collaborations were in the U.S.-Germany MOU and AGARD working group, I learned an immense amount from observing the outstanding leadership qualities of Dr. Peter Hamel, who headed up both of these activities. Peter's Flight Mechanics Institute was involved in a very wide range of fixed- and rotary-wing research. Yet he gave us his full attention at each meeting, offering excellent technical insights, suggestions, and feedback on our work. I learned from Peter how to lead and foster international collaboration, and how important it was to remain technically curious and interested in the work of the young engineers. Peter was a terrific mentor,

role model, and supporter of my work during those early years and continuing to the present day. I stayed on with the AGARD Flight Mechanics Panel by serving as the U.S. Army representative (1992–1998), and organized two successful international meetings on system identification and flight control [26, 27]. These meetings, organized together with Peter Hamel, were a highlight of my time with AGARD. These years were really magical and inspiring. I was the youngest in the group and benefited by absorbing the rich knowledge of the other members and taking in the culture of each country where we met. The opportunity for a young engineer to serve on such a prestigious international panel was a unique benefit of being a civil servant in the Ames-Army Lab, and it would not have been possible in an industry environment. Also, my international connections in aircraft and rotorcraft flight control, developed in the AGARD panel, gave me the opportunity to be the organizing editor and co-author of my first book, *Advances in Aircraft Flight Control* [28].

In 1985, after seeing the value of international collaborations, I wanted to establish an international MOU with Israel. I was interested in digital flight control and there was a renowned expert in Israel, Dr. Paul Katz, as well as the connections via my Uncle “Yanks” at the Technion–Israel Institute, and the desire to visit some relatives that lived there who I hadn’t seen since soon after my mom passed in 1976. So, my uncle arranged a formal invitation letter from the Technion Aerospace Engineering Department and Irv Statler approved my visit to see if there might be some research of common interest to the U.S. and Israel.

In 1986, the Ames-Army Lab became the Aeroflightdynamics Directorate (AFDD) under the U.S. Army Research and Technology Activity (ARTA). With the encouragement of our new AFDD Director, Andy Kerr, Dr. Richard “Dick” Carlson (Director of ARTA) and I returned to Israel to formally establish the U.S.-Israel MOU on “Rotorcraft Aeromechanics and Man-Machine Integration Technology.” I was named U.S. lead, Technical Project Officer (TPO), for the new collaboration. On our 1986 trip, I took Dick on a wonderful rental car tour—just the two of us navigating much of Israel in a single day. Dick and I returned to Israel several more times, and I greatly cherished our one-on-one time together and his wonderful stories. Once, during a standard “intensive” exit interview while leaving the Israel Ben Gurion airport, I was asked many questions about the nature of our official visit. By then, my Hebrew was good, thanks in part to an intensive Hebrew language program Andy Kerr agreed to send me to in Israel. I amused myself and perplexed the security personnel by conducting the lengthy interview entirely in Hebrew. Then with Dick just behind me, he was asked briefly by Israeli security, “And what is your mission here in Israel?” Without missing a beat, Dick (four management levels above me) pointed at me and answered, “To watch over him!” The Israel security official said, “Then go do your job!” Dick and I laughed each time we recalled that exchange.

I loved Dick Carlson, as a son loves and admires his dad. Dick had an incredible depth of experience in rotorcraft, a wealth of knowledge, boundless technical curiosity, and unpretentious style. He gave me great latitude in setting up the agreement, while also providing outstanding wisdom and guidance drawn from his long and successful career. He was a great supporter of my research work and efforts in international collaboration in AGARD and the MOUs. He was unable to go to Israel one year because of some health issues. I saw him on my return and, since Dick was a great wine connoisseur, brought him a prized bottle of Israeli wine from the famous Yarden winery in the north of Israel. As it turned out, Dick was under doctor’s orders that he couldn’t drink! Dick retired soon thereafter, and continued to attend the U.S.-Israel MOU meetings in the U.S., as a much-honored participant. He provided a wonderful, warm letter of

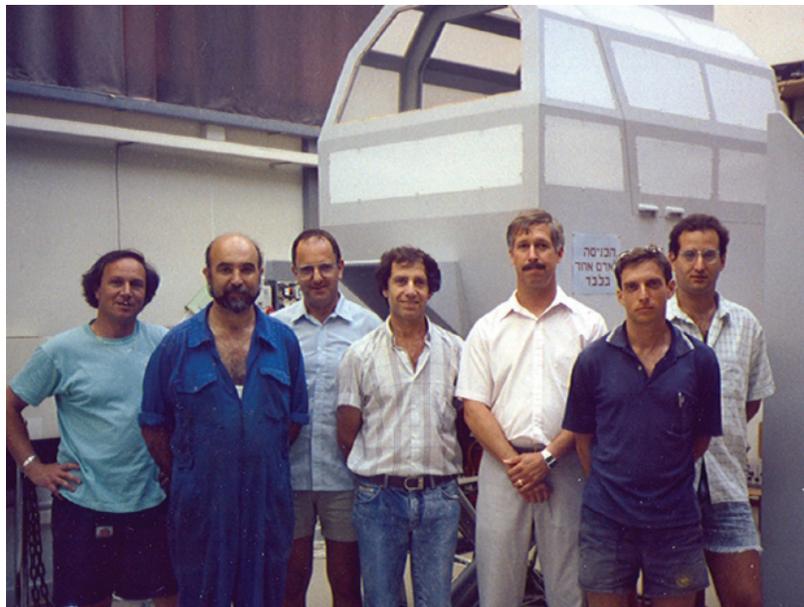


Fig. 10. First U.S.-Israel MOU research task: Image Stabilization of Helmet Mounted Displays (U.S. PI George Tucker is 3rd from right, 1986).

with Israel. I launched the new U.S.-Israel MOU with a single task on Image Stabilization of Helmet Mounted Displays—an important issue since the Comanche helicopter program anticipated using such devices. The sole U.S. PI in those early days was NASA experimental test pilot George Tucker, my friend and colleague from the airship tests. He participated in simulator experiments at Technion (Fig. 10) [29], flew one of the early helmet concepts in a U.S. helicopter, and learned to run CIPHER to analyze the vibration data. He remained involved in many future tasks, especially involving sling-load flight testing and dynamics modeling. George and I made several memorable trips to Israel (Fig. 11) and I greatly cherished his friendship, good counsel, perspective, and support over our long partnership afforded by the Army-NASA Joint Agreement.

The U.S.-Israel MOU flourished greatly, increasing in scope to six simultaneous tasks, two each in the areas of flight dynamics, human systems, and aeromechanics, with the strong support of our AFDD Director, Andy Kerr. The photo in Fig. 12 of me with the senior Army leadership was taken in about 1990 next to the famous “Menorah,” the national symbol of Israel,

friendship for me to read in 1996 at the 10th anniversary celebration, held in Israel, of the U.S.-Israel collaboration. Dick passed too young, and I was deeply saddened at the loss of this great helicopter pioneer, R&D leader, and personal mentor.

The first official meeting of the U.S.-Israel MOU in 1986 had only one task. Peter Hamel taught me the value of starting new collaborations small and establishing a successful track record, while also keeping the collaboration well structured. I applied these lessons in the start-up of the new agreement

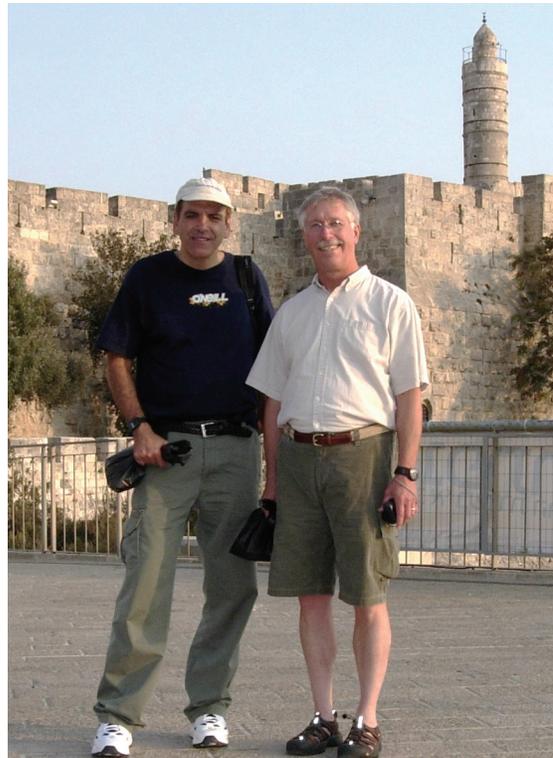


Fig. 11. Mark Tischler and George Tucker in Jerusalem for the U.S.-Israel MOU.

which stands near the Israeli Parliament building in Jerusalem. In these early years, I also had wonderful counterparts on the Israeli side, leading their research teams and ensuring the needed support from their Ministry of Defense (MoD). Avi Kuritsky was the first Israeli TPO from the Israeli MoD Science and Technology (S&T) directorate (our counterpart). He was highly experienced in aeronautics, with many years in Israeli industry and government. Together, we guided the research work in the early years of the agreement, and continually brought new ideas for collaborative tasks, which were key to keeping the agreement vibrant. Avi was a great mentor, role model, and friend (and now continues to be as an MoD consultant). With his terrific sense of humor, deep knowledge of Israeli history, and great storytelling ability, he personally guided the U.S. team of PIs on many wonderful excursions throughout Israel. The U.S. and Israel PIs came from government, academia, and industry with a broad spectrum of research experience. The U.S. PIs have included both Army and NASA researchers—another benefit of the Army-NASA Joint Agreement.

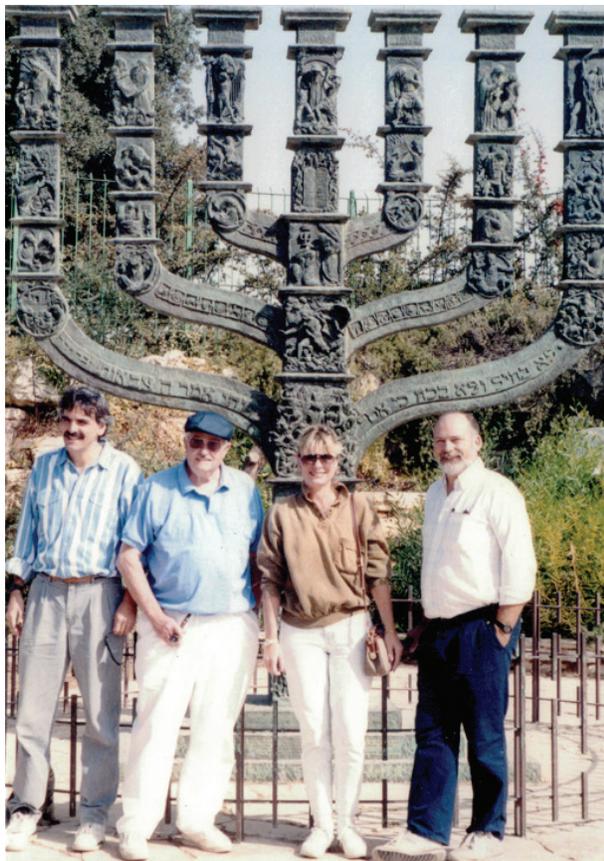


Fig. 12. U.S. leadership delegation of the U.S.-Israel MOU. From left: Mark Tischler, Dick Carlson, Rita Hassinger (U.S. Army International Relations), and Andy Kerr (circa 1990).

Flight Control Design and Analysis

System identification and flight control always remained my two great technical passions. My approaches in both areas were based heavily in frequency-domain classical control methods, from years of learning from my mentors at STI. As such, the system identification and flight control methods were highly complementary for the aircraft development process, and over time the tools were tightly integrated.

At about the same time that the XV-15 flight testing was getting started, the Simulation Office was disbanded, and I moved to Dave Key's handling-qualities division, but sat in the NASA Flight Dynamics and Controls Branch, led at the time by Dr. Jack Franklin and later by Dr. Vic Lebacqz. The small Army flight control group, led by Ed Aiken, was integrated within the NASA Branch. These leaders were strong supporters of the classical frequency-domain methods and first principles analysis techniques. My flight control interests were applied, early on, to the just-emerging digital (sampled-data) control system for helicopters. I had learned about the digital aspects of flight control in my course work at Stanford, as well as some pioneering work at STI. But, digital control research had not yet addressed the specific challenges for helicopter applications.

Experimental fly-by-wire (FBW) for fixed-wing aircraft started with the first flight of the NASA F-8 digital FBW program in 1972, and the first production digital FBW F/A-18 had its first flight in 1972 and was fielded in 1978. Though experimental tests with FBW helicopters began with the Boeing TAGS in the late 1960s, the Boeing Helicopter Advanced Digital Optical Control System (ADOCS), developed under a U.S. Army S&T 6.3 program on the UH-60A Blackhawk helicopter, was arguably the first operationally representative, multiply redundant, full-authority FBW helicopter with a modern side-stick controller. The first U.S. fielded FBW helicopter, the Bell/Boeing V-22 Osprey, and the European NH-90, were both fielded in 2007, some 29 years behind their fixed-wing counterparts.

I saw a key research opportunity in understanding the implications of modern full-authority sampled-data systems for helicopters and began my research in 1984. I conducted a comprehensive literature review and “paper study” on digital control of the UH-60 Blackhawk helicopter using an explicit model-following (EMF) flight control system over the course of a few years. I was motivated by the ongoing ADOCS experiments in the Boeing simulator, NASA Vertical Motion Simulator (VMS), and later, the flight-test demonstrator helicopter.

Analysis showed that the EMF control system could provide good short-period dynamics damping and natural frequency and good command response tracking. However, another key conclusion of the report was that helicopters had much larger inherent time delays due to the rotor system (about 70 msec), in addition to the other components of FBW fixed aircraft. These could yield large overall stick-to-response time delays that could cause pilot-induced oscillations (PIOs) for high-gain tasks, based on a comparison with the fixed-wing literature. Another important finding was the considerable interaction of the sampled-data system and the continuous time dynamics of the actuators and helicopter dynamics that could cause high-frequency excitation or “ripple” in the actuator command. This ripple effect due to the relatively low digital system sample rates of the time (30 Hz) could rate saturate the actuator and excite unmodeled high-frequency dynamics. My early digital flight control research culminated in the publication of a comprehensive technical report, *Digital Control of Highly Augmented Combat Rotorcraft* [30]. I worked very hard on this major flight control report in parallel with my dissertation research on system identification flight test of the XV-15, both published in 1987.

I graduated with a Ph.D. from Stanford in 1987, with my dad proudly watching as his youngest son received the first Ph.D. in our family. It was such a great moment in my career and life, made possible by the Army-NASA Joint Agreement. That same year I also became the Ames-Army Lab’s flight control group leader, taking over for Ed Aiken. In my “free” time, I was teaching dancing every week and windsurfing during the summer. It was a great and crazy year!

The flight tests of the ADOCS demonstrator started in late 1985, and I saw an excellent opportunity to apply my experience in system identification and the knowledge and concepts in my digital control report in supporting the Army flight-test evaluation of the ADOCS demonstrator (Fig. 13). I requested and received approval by Boeing for their pilots to conduct frequency sweep tests of the ADOCS demonstrator at the Boeing Philadelphia Facility. These were the first formal system identification flight tests of a full-authority operationally representative fly-by-wire/fly-by-light (FBW/FBL) helicopter. The flight tests yielded excellent frequency-response characterization of the flight control performance, and I extracted accurate lower-order transfer-function models for handling-qualities analysis (Fig. 14). The analysis of

the results showed desirable (Level 1) short-period damping and frequency (Fig. 15), based on the STI hovering criteria developed by Hoh and Ashkenas [31] in 1980. But the ADOCS system also had a large equivalent time delay ($\tau_e = 240$ msec), which raised concerns about the degradation in handling qualities for high-gain tasks, as based on the fixed-wing literature (Fig. 16) and as highlighted in my report.



Fig. 13. ADOCS demonstrator [32].

Teaming up again with my experimental test pilot colleagues George Tucker (NASA) and Pat Morris (Army), the government test team conducted qualitative Cooper–Harper rating evaluations for various flying tasks. The pilots confirmed that the ADOCS exhibited very good handling qualities for low and medium pilot-gain tasks (precision hover, 360-degree turn, bob-ups, sideward flight, and transition to forward flight/dash maneuver). However, in the very high pilot-gain tasks of slope landing and run-on landings, PIOs, also referred to as aircraft pilot coupling (APC), occurred regularly, arising from the high time delay in the ADOCS response, as predicted in my report. An example time history of the APC for a run-on landing is shown in Fig. 17. Also, the time response of the actuator to pilot inputs exhibited the “inter sample ripple,” confirming the detailed hybrid digital/analog analysis in my report. The Army/NASA government team documented the ADOCS work in a comprehensive paper co-authored with Jay

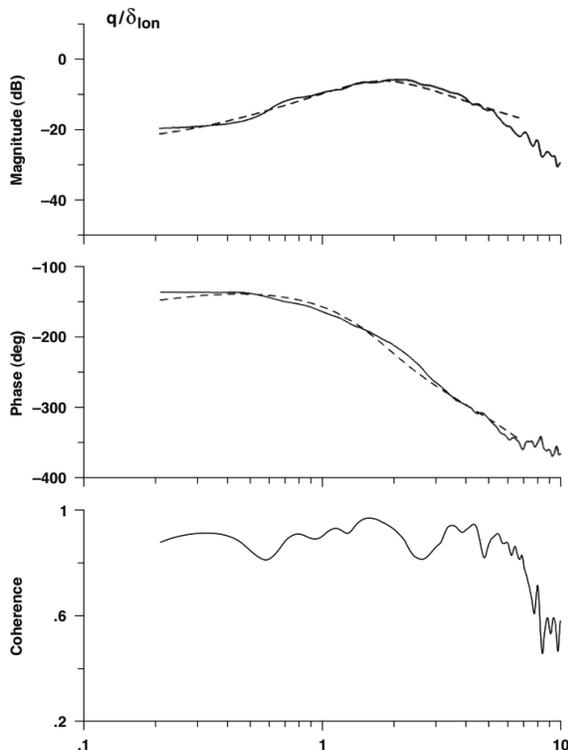


Fig. 14. System identification and flight control analysis of the ADOCS FBW response [32].

Fletcher, Pat Morris, and George Tucker in 1988 [32]. The flight-test results of this early FBW demonstrator tracked the findings in my digital control report. Fortunately, future flight control systems had the benefit of faster cycle rates and actuators, and less filtering, resulting in much lower time delays, and this reduced the possibility of APC tendencies. Also, the increased sample rate eliminated the artifacts associated with the sampled-data systems—the inter sample ripple and excitation of high-frequency dynamics.

The confirmation of the importance of time delay, known in fixed-wing literature for some time, was at odds with the early versions of ADS-33 that allowed much larger delays. This caused some consternation with my Army boss, Dave Key, but later helicopter flight-test work in the United Kingdom and by Chris Blanken on the Bo-105 (under the U.S.-Germany

MOU; see Chris Blanken's chapter) confirmed this finding, and a delay cap of 120 msec for high-gain tasks was incorporated in later versions of ADS-33.

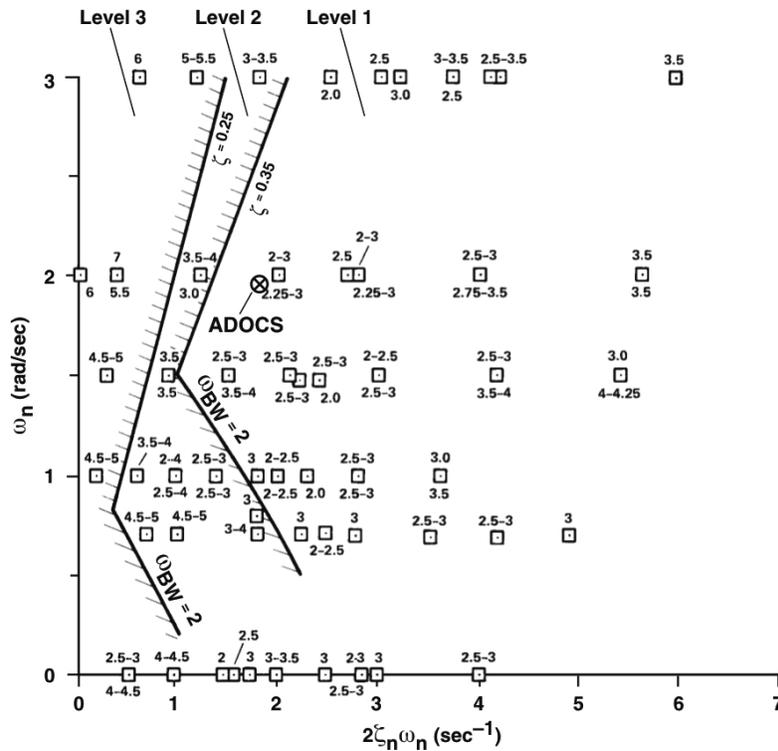


Fig. 15. Handling-qualities analysis of the ADOCS demonstrator shows good short-period response damping and frequency [32].

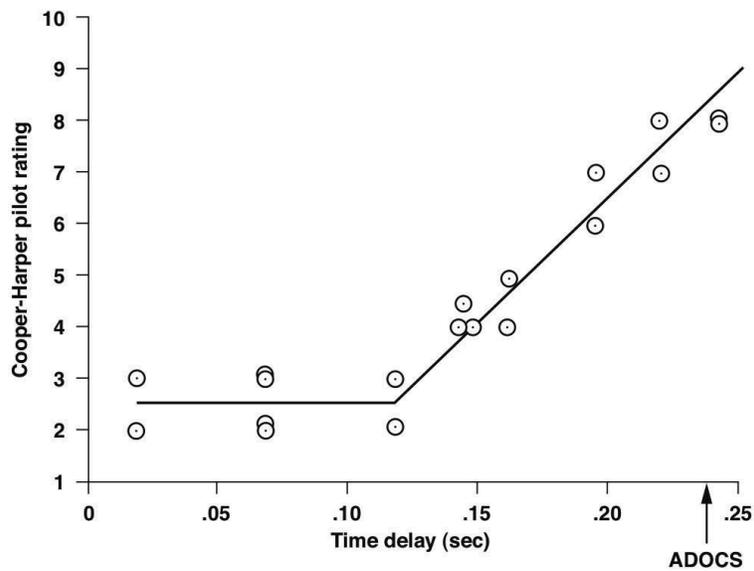


Fig. 16. Time delay of the ADOCS demonstrator showing correlation with fixed-wing literature and the potential for poor handling qualities in high-gain tasks [32].

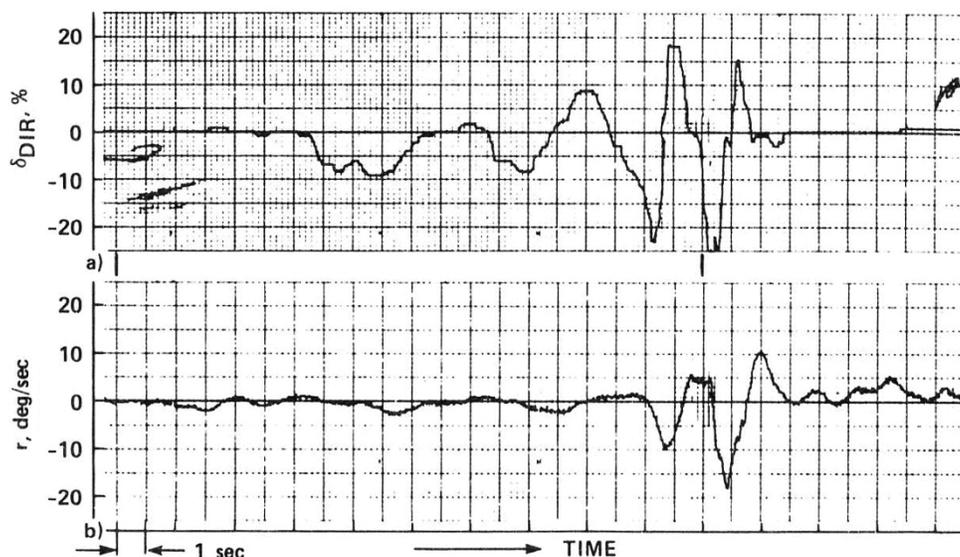


Fig. 17. Aircraft pilot coupling (APC) in run-on landings of the ADOCS demonstrator [32].

At about this time, and building upon my flight control research, I initiated a research grant at UMD. This was led by my Master's degree advisor, Professor Barlow, with two primary goals. The first goal was to determine a higher-order UH-60 linear model from a nonlinear simulation using numerical perturbation methods. This research effort by Professor Roberto Celi, of the Aerospace Engineering Department, provided the high-fidelity flight dynamics model (later named "FORECAST") that was central to the control system design. Professor Celi and I continued our successful collaboration on numerous research projects over the years, many as part of the U.S.-Israel Rotorcraft MOU.

The second goal of the UMD research effort was to apply parametric optimization methods for control system design to numerically optimize a MIMO H_∞ flight controller to meet ADS-33 handling-qualities requirements. The control system optimization research was led by Professor Bill Levine and Professor André Tits of the Electrical Engineering Department. This control system design research and optimization software "CONSOL-OPTCAD" tied the development of ADS-33 rotorcraft specifications with *how* and *if* the specifications could be simultaneously achieved for the installed aircraft control power and the user-selected control system architecture. In a later study, we applied this optimization approach to the ADOCS explicit model following control system and a more intuitive user interface "GIFCORCODE" [33]. In many ways this was the very "linkage" of our work to the ADS-33 development that Dave Key wanted (see Dave Key's chapter).

In discussing our research with industry leaders, I realized that the community needed a sophisticated and flexible "one-stop-shop" flight control design environment for rapid design integration, data resources management, and flight control evaluation/optimization for many design criteria (specifications). The research work evolved into a concerted multi-year in-house research effort at AFDD to develop the Control Designer's Unified Interface (CONDUIT[®]) for aircraft and rotorcraft flight control design and evaluation [34]. This included many Master's thesis projects by Cal Poly San Luis Obispo graduate students conducted on-site at AFDD with

Professor Dan Biezad and myself jointly serving as advisors. The overall goals of our flight control research and software were to develop:

- Comprehensive and flexible GUI that allowed easy setup by non-specialists, rapid hand tuning, automated optimization, and evaluation of the design specifications with supporting analyses.
- Capability for optimization of an arbitrary user-specified control system architecture, and selecting the design parameters within the system to be tuned, such as gains, or weighting matrices, etc.
- Comprehensive graphical library of all pertinent control system requirements with automated scripts for determining performance from the simulation model: ADS-33 (rotorcraft), MIL-F-1797 (fixed-wing), and MIL-DTL-9490 (generic control system requirements).
- Generic “distance algorithm” to convert the control system metrics, as plotted on the handling-qualities and control system specifications, into a set of numerical scores. Given the many and geometrically complex shapes of the Level 1 boundaries, achieving a reliable distance algorithm was essential to the design optimization process and proved to be a challenge.
- Robust and sophisticated feasible sequential quadratic programming (FSQP) parametric optimization engine that could tune the user-defined design parameters so that the control system would meet all specifications with minimum over design—that is, not requiring any extra control usage beyond that needed to meet the requirements. This is the most “economical solution” and is referred to as the “Pareto optimum,” well known to be the best optimization solution.
- Design Margin Optimization (DMO) that allows an automated exploration of the Pareto trade-off front. This evaluated the trade-offs between the control system performance (predicted handling qualities) and increased actuator usage.
- Tight integration with CIPHER to import accurate identified models and associated uncertainty bounds for robustness to uncertainty analysis.
- Capability to accommodate industrial-sized design problems: large block diagram, many specifications, and full-flight envelope gain schedule.
- Comprehensive fixed-wing and rotorcraft flight-test studies to validate the predicted control system performance, optimization results, and trade-offs.

CONDUIT is now a widely used standard for flight control analysis and design. Large centers for fixed-wing and rotary-wing aircraft development, as well as specialists in control system design, and academia use CONDUIT as an integral part of their control system development processes.

III. Epilogue

I have had a long and rewarding career at the Ames-Army Lab, now named the U.S. Army Aviation Development Directorate (ADD). For most of the 30-plus years since the “formal end” of this Memoir (1985), the Ames-Army Lab (then the Aeroflightdynamics Directorate) was led by Andy Kerr (1986–2008). Andy was a great “street fighter” in an environment that was increasingly skeptical of the value of basic and applied (6.1, 6.2) aeronautics research. But Andy

supported the technical interests and intellectual curiosity of the researchers that resulted in “products and payoffs” of our work being 5–10 years out (rather than a short-term application), and he shielded us from the turmoil of the money politics and upper Army management. He was a great personal mentor and supporter of my work.

Andy Kerr made many key decisions that helped my career, and I am greatly indebted to him. Early on, AFDD was asked to support the independent analysis of MH-53J frequency sweep flight-test results. Andy agreed to let me take on the project, as well as agreeing that we should charge the customer for the effort—this was a radical idea that the Army should charge a company for customized project support (outside of our funded S&T mission), but it was an extremely wise move. When customers paid a fee for our work, I found that they valued the effort and results a lot more than if we did it for free (as we had always done before). This external work also allowed us to fund promising new graduates as contractors before civil service slots could be obtained. I always looked for such external projects that involved flight testing to further validate our methods and tools. I also wanted these projects to be fun, different, and applied near-term (usually short in duration, 6–12 months). Besides giving our young folks unique opportunities, these projects provided insight into industry dynamics and control methods, and lessons learned to better focus our internal research. Further, these projects provided an important opportunity for technology transfer of our methods to the companies we supported. This approach has continued for many years and has allowed us to team on many (50-plus) innovative industry flight control development programs. Some of the manned aircraft and rotorcraft external projects included support of the CH-47F Digital Automatic Flight Control System (DAFCS), CH-53X/K, Sikorsky S-92, Armed Reconnaissance Helicopter (ARH), Boeing FBW technology demonstrator that led to the Boeing 787, Cessna FBW business jet concepts, and most recently, the Air Force Test Pilot School (AFTPS) Learjet and Variable Stability In-flight Simulator Test Aircraft (VISTA) F-16. Support of UAV external programs started with the AeroVironment Solar Pathfinder, and continued on to the Shadow, MAV, Northrop-Grumman Fire Scout, Fire-X, and unmanned Kaman K-MAX. Many of these projects can be found in the reference lists of references [35-37] herein. Together with my flight control technology research group, I have authored and co-authored over 160 conference and journal articles, with many publications evolving from the collaborative external projects with industry.

With support by Andy Kerr, Wayne Mosher, and Barry Lakinsmith, we commercialized CIFER[®] (Fig. 18a) and CONDUIT (Fig. 18b), now in use as standard flight control tools in over 75 sites worldwide to date. Extensive user’s guides were developed and maintained for each product. I also realized that the average engineer was not going to read all of our group’s research papers and come up with “best practices” on their own, or teach himself or herself to use the software from the user’s manual. So, starting in 1993, I began teaching short training courses on system identification using CIFER, and in 2001 our flight control team expanded this to flight control design short courses using CONDUIT. I arranged to have both courses professionally videotaped as a training resource for industry users to learn the engineering and software application.

I also realized the need for single integrated reference books for our system identification and flight control design methods, best practices, and example results. In 2001, I started work on *Aircraft and Rotorcraft System Identification: Engineering Methods With Flight Test Examples*, a textbook published by AIAA in 2006 [35]. I greatly expanded this textbook and added new material on flexible dynamics identification and a “model stitching” approach to develop full-flight envelope models from a few point identification models and trim data. This second edition

was published by AIAA in 2012 (Fig. 19a) [36]. In 2007, our group launched a 10-year research effort for software algorithm improvement, flight-test validation, and writing. This culminated in a comprehensive book published by AIAA in 2017, titled *Practical Methods for Aircraft and Rotorcraft Flight Control Design: An Optimization-Based Approach* (Fig. 19b) [37]. Each book comes with student versions of the associated software, student exercises, and solution manuals to enable easy integration of the material into the classroom. I felt very early on in my career that just writing a research paper and moving on was not adequate to ensure transition of our work to industry. Instead, we needed to “go the extra step” by developing industry-grade software and user’s guides, giving short courses, and writing books. Looking back to where we started 35 years ago, this approach has worked well, and we have greatly influenced the methods and tools used in the flight dynamics and control community.



Fig. 18. User’s guides: a. system identification; b. flight control design.

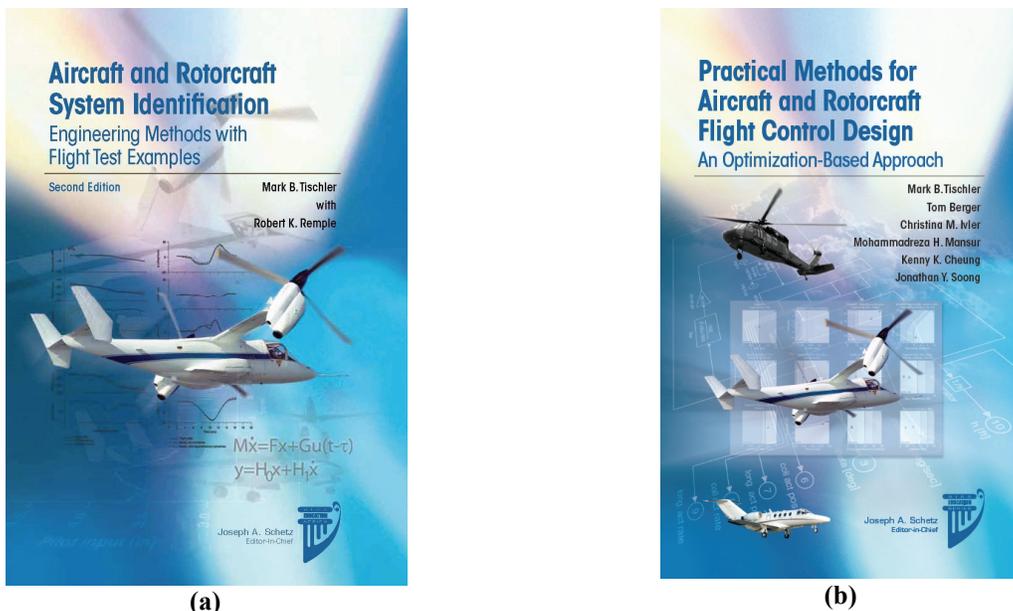


Fig. 19. Textbooks: a. system identification (second edition); b. flight control design.

In the mid-1990s, the Army and NASA considered realigning their rotorcraft researchers. Andy Kerr had the great foresight to set up three strategic planning teams to consider the future of rotorcraft technology and the associated proposed realignment of the workforce. He astutely selected an even balance of nationwide Army and NASA researchers, and asked me to lead these teams. I am greatly indebted to Andy for these terrific opportunities to lead teams in strategic planning. The first team was local to the Ames Research Center, and the second team was nationwide. The work produced a detailed strategic vision for rotorcraft technology anticipated in 2010. Andy asked me to lead a third team in 1996 to consider options for the organizational structure of an integrated Army/NASA Rotorcraft Division that would best meet this strategic vision. Our team considered and debated the advantages and drawbacks of three alternative organizational concepts. We reached a consensus on the best option, and made a recommendation that was accepted by Army and NASA management. When the combined division was formed in 1997, I wanted to stay in my technical role, and so I became the flight control technology group leader for the joint organization.

The Army/NASA Rotorcraft Division (1997–2005) was, in my view, the zenith of Army and NASA collaboration. Together as an integrated division, we made great strides in conducting groundbreaking research and completing the certification of the Rotorcraft Aircrew Systems Concepts Airborne Laboratory (RASCAL) UH-60 in-flight simulator (see chapters by Ed Aiken and George Tucker). The Autonomous Research Project (ARP) was also established during this period of the joint division. ARP was the vision of Ed Aiken, who understood, way ahead of his time, the potential role of autonomous guidance and control of helicopters. Under Ed's leadership of the joint division, NASA purchased two Yamaha RMAX helicopters for ARP autonomy research flight testing. Some of the key research strides accomplished during the time of the joint Army/NASA Rotorcraft Division include:

- System identification methods for determining higher-order models of full-scale and UAV rotorcraft.
- Initial completion of a modern GUI and software for the CONDUIT design tool.
- Extensive flight-test research on external (slung) load systems.
- Important advances in helicopter modeling to achieve correct off-axis response prediction.
- System identification support for Pathfinder to achieve its record for high altitude solar-powered flight.
- Integrated modeling and control of helicopter vibration and flight dynamics.
- Partial authority modernized control laws (MCLAWS) for attitude command/attitude hold (ACAH) to allow for safe helicopter flight in a degraded visual environment—applications to the UH-60 and AH-64.
- System identification and control of ducted fan UAVs.
- Algorithms and flight tests of an autonomous landing system for the RMAX helicopter using safe landing area determination.
- System identification and flight control development of the unmanned full-scale Fire Scout and K-MAX helicopter systems.
- Flight-test extraction and validation of the accurate control equivalent turbulence input (CETI) model for helicopter in hover and low speed.

Pivotal advancements continue in flight control technology to this day. Some highlights of our work in the past 10 years include:

- System identification and flight control support for the ARH scout-attack demonstrator, UH-60MU control laws, and MH-47G DAFCS control system.
- Flight-test development and demonstration of cable angle and rotor-state feedback for precise control.
- Development and flight validation of advanced high-speed control laws for business jets.
- Quasi-multibody simulation modeling of flexible rotorcraft.
- Development and comparison of control allocation methods for aircraft with redundant control surfaces.
- Advanced modeling of the high-speed Sikorsky X-2 coax lift-offset compound demonstrator.
- Validation of full-scale flight control development methods for use on UAV rotorcraft and fixed-wing configurations.
- Extraction of lower-order inflow models from higher-order wake representations (free wake and vortex particle method).
- Development and flight-test validation of the stitched model for full-flight envelope simulation from point identification models and trim data for the UH-60, Learjet, and F-16 VISTA.
- Flight control development and flight-test demonstration of dual-lift on the two RMAX UAV helicopters.

As the Technical Project Officer, I continued to lead the U.S.-Israel MOU until 2016; it eventually became the Rotorcraft Project Agreement (RPA). Some 29 collaborative research tasks were completed in the 30 years of my tenure from 1986–2016. The magic of the cooperation was the sense of family and friendship that gave the PIs the motivation to put in the extra effort needed make international research meetings productive, after long airplane trips and 10 hours of jetlag. The teams of researchers published many outstanding, groundbreaking technical papers, and I was always beaming like a proud parent when I watched a team of U.S. and Israel researchers co-presenting their work at an international forum. The agreement was recognized as the best U.S.-Israeli research collaboration by both countries and won the first U.S. Army International Collaboration award in 2010. My leadership of this agreement culminated with the wonderful 30th anniversary celebration in October 2016, held at the Israeli Ministry of Defense. The U.S. Ambassador to Israel, and senior U.S. and Israeli S&T leadership attended the celebratory toast and picture taking (Fig. 20). Leading and nurturing the U.S.-Israel collaboration from my first visit to Israel in 1985 to its 30th anniversary in 2016 was a personal highlight in my leadership roles. It was a great honor to be recognized with the Department of the Army Superior Civilian Service Award by Major General Cedric Wins (Commanding General, Army Research, Development and Engineering Command (RDECOM) in 2017 for founding and leading the U.S.-Israel cooperation for the U.S.

There have been two other key highlights in my career. The first highlight, thanks to Andy Kerr's considerable efforts, was being named Senior Technologist (ST) of the Aviation and Missile Command (AMRDEC) in 2002. In this role, I have served on many technical awards,



Fig. 20. Thirtieth anniversary celebration of the U.S.-Israel Rotorcraft Project Agreement (2016); Honorable Dan Shapiro (U.S. Ambassador to Israel) seated in center with red tie.

and selection panels; helped craft and express the importance of scientific discovery; and worked to maintain a valued technical career track in addition to a management track. The second highlight, due to the support and confidence in me by Dr. Bill McCorkle (former Director, AMRDEC), was being awarded the Presidential Distinguished Rank Award (Senior Professional) in 2009, the first for the U.S. Army. The award was presented in ceremonies in 2010 at the U.S. State Department (Fig. 21) and was hosted by the Honorable Hillary Clinton. My dad and his wife Marjorie attended the State Department ceremony. My dad was very proud of me, and he also found an opportunity to tell the other guests at our table about his development of the transistorized pacemaker. I was so grateful that my dad lived to see me receive this prestigious award, and I reminded him that it was all due to his encouragement over the years starting with buying me that model rocket set. The presentation of the official award by the Secretary of the Army was held at Arlington National Cemetery (Fig. 22). I felt so proud at that moment of recognition.

The long and successful history of the AFDD flight control technology group, starting in 1985 with Ed Aiken's leadership, and my leadership since 1994, would not have been possible without the strong support of our Ames-Army Lab Flight Control Division Chiefs of many years—Mr. Dave Key (pioneering lead author of ADS-33), Mr. Barry Lakinsmith (who went on to become our current AFDD, now recently retired ADD Lab Director) and Mr. Chris Blanken (renowned handling-qualities researcher and co-author of



Fig. 21. Presidential Distinguished Rank Award, Department of State ceremony with my dad and his wife Marjorie (2010).

ADS-33). These lifelong colleagues and friends understood and supported the need for the long-term investment in our S&T projects, flexibility for our talented team members to reach out to new areas beyond those of our core mission (which paid off in unexpected ways), ensured adequate resources and personnel, and shielded us from the day-to-day disruptions to allow us to focus on our mission. While there has been considerable change in the senior Army aviation research management and our Ames-Army Lab Directors, Dave, Barry, and Chris have provided the long-term stability and corporate memory needed for a coherent and productive research program.

Most important is to recognize the amazing technological advancements of our flight control researchers over the years. Our group has always comprised about 15 full-time researchers and software developers in total, rather small in comparison to similar research groups in government, industry, and academia. Each member has worked incredibly hard to achieve the research breakthroughs, flight-test validation efforts, and new algorithms and software tools for which our group is known worldwide. It has been the highest honor to be a research investigator, group leader, and mentor working with you for these many years. Thanks so much to all of you!

The Ames-Army Lab continues to accomplish amazing research. Our research standards and peer/management review processes were always very demanding. Our publication review process and presentation dry runs were always the toughest, and as a result our papers were widely respected and won many awards for “best of session” and “best of conference.” During my early days at the Lab, while part of the NASA Flight Dynamics and Controls Branch, and later during the combined Army/NASA Rotorcraft Division, we had the benefit of a deep



Fig. 22. Receiving the Presidential Distinguished Rank Award from the Secretary of the Army, Mr. John M. McHugh, and General Ann E. Dunwoody (2010).

“bench” of world renowned Army and NASA mentors. I learned immensely, both technically and politically, from the Army and NASA senior folks who I watched and tried to emulate. This was a key benefit of the Army-NASA Joint Agreement. Another aspect of the Joint Agreement that yielded great achievements was pulling Army and NASA research staff to achieve a critical mass on complex problems—like rotorcraft simulation, flight control, and system identification. We also pulled funds and expertise to develop unique ground and in-flight facilities, including the XV-15 tiltrotor aircraft, the VMS, and the RASCAL in-flight simulator. Research using these joint facilities led to many groundbreaking accomplishments, including the modern Aeronautical Design Standard for Rotorcraft (ADS-33). Finally, the Army and NASA researchers had different managers and customers. This allowed different perspectives to develop, and greatly enriched our research environment. I always felt that the Ames-Army Lab was the ideal balance between academia and industry environments. The Lab provided me with a wonderful place to follow my career passions and the patience to take the long view in developing methods and tools that have shaped modern rotorcraft flight control.

I have been so fortunate and blessed in my career in aerospace engineering to be given numerous opportunities to pursue my interest and work in advancing the state-of-the-art in flight dynamics and control technology. As a kid, my dad often quoted Mark Twain, “If you love what you do, you’ll never *work* a day in your life!” Dad was right about that too.

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Index

A

ADS-33, 9, 17-20, 25-27
 Advanced Digital Optical Control System (ADOCS), 16-19
 Advisory Group for Aerospace Research and Development (AGARD), 12-13
 Aeroflightdynamics Directorate (AFDD), 1, 11, 13-14, 19, 21, 25
 Aeronautical Design Standard-33 (ADS-33), 9, 17-20, 25-27
 AH-64, 23
 Aiken, Ed, 7, 15-16, 23, 25
 Air Force Flight Test Center, 9
 Air Force Test Pilot School (AFTPS), 21
 aircraft pilot coupling (APC), 17, 19
 Airship Industries 500, 11
 American Institute of Aeronautics and Astronautics (AIAA), 4, 21-22
 Anderson, John, 4-5
 Armed Reconnaissance Helicopter (ARH), 21, 24
 Army Aeroflightdynamics Directorate (AFDD), 1, 11, 13-14, 19-21, 25
 Army Aeromechanics Lab, 1, 6-7
 Army Aviation Development Directorate (ADD), 20, 25
 Army Research and Technology Activity (ARTA), 13
 Army Research, Development and Engineering Command (RDECOM), 24
 Army-NASA Joint Agreement, 7, 14-16, 27
 Ashkenas, Irv, 5-7, 17
 attitude command/attitude hold (ACAH), 23
 Autonomous Research Project (ARP), 23
 Aviation and Missile Command (AMRDEC), 24
 Aviation and Missile Command (AMRDEC), 24-25

B

Barlow, Jewel, 5, 12, 19
 Bell 214ST, 9-10
 Bell Generic Tilt Rotor (GTR), 9
 Bell Labs, 9
 Biezd, Dan, 20
 Blanken, Chris, 17-18, 25
 Bo-105, 9, 12, 17
 Boeing 787, 21
 Boeing ADOCS, 16-19
 Boeing Philadelphia Facility, 16
 Boeing TAGS, 16
 Bryson, Arthur, 7, 9

C

Carlson, Dick, 13-15
 Cauffman, Mavis, 9
 Celi, Roberto, 19
 Cessna FBW, 21
 CH-47F DAFCS, 21
 CH-53X/K, 21
 Comanche helicopter, 14
 Comprehensive Identification from Frequency Responses (CIFER), 9, 11, 14, 20-21
 Control Designer's Unified Interface (CONDUIT), 19-21, 23
 Control equivalent turbulence input (CETI), 23

D

Design Margin Optimization (DMO), 20
 Deveney, Jim, 3
 Digital Automatic Flight Control System (DAFCS), 21, 24
 Dunwoody, Ann E., 26

E

explicit model-following (EMF), 16

F

F-14, 5
 F-16, 21, 24
 F-4, 5
 F-8, 16
 feasible sequential quadratic programming (FSQP), 20
 Fire Scout, 21, 23
 Fire-X, 21
 Fletcher, Jay, 12, 17
 fly-by-wire (FBW), 16-17, 21
 fly-by-wire/light (FBW/FBL), 16
 Franklin, Jack, 7, 15

G

German Aerospace Research Establishment (DLR), 11-12
 Glenn L. Martin Wind Tunnel, 3-5
 graphical user interface (GUI), 9, 20, 23
 Grumman Yankee, 5

H

Hamel, Peter, 12-14
 Hoh, Roger, 5, 17

I

Israeli Ministry of Defense, 15, 24

J

Jex, Henry, 5
 JVX, 7

Tischler, M.

K

Kaletka, Jürgen, 11
Katz, Paul, 13
Kerr, Andy, 13-15, 20-21, 23-24
Key, Dave, 7, 15, 17, 19, 25

L

Lakinsmith, Barry, 21, 25
Learjet, 21, 24
Lebacqz, Vic, 7, 15
Levine, Bill, 19
lighter than air (LTA), 11

M

MAV, 21
McCorkle, Bill, 25
McHugh, John M., 26
McRuer, Duane, 5-7
Memorandum of Understanding (MOU), 11-14, 18-19, 24
MH-47G, 24
MH-53J, 21
Ministry of Defense (MoD), 15
Miura, Hiro, 6
modernized control laws (MCLAWS), 23
Morris, Pat, 17
Mosher, Wayne, 21
multi-input/multi-output (MIMO), 8, 19
Myers, Tom, 5

N

NACA Langley, 3
NASA Ames, 6-7, 9-10
NASA Flight Dynamics and Controls Branch, 7, 9, 15, 26
NASA Langley, 5, 9
NH-90, 16

P

Pathfinder, 21, 23
pilot-induced oscillations (PIOs), 16-17
Principal Investigator (PI), 11, 14

R

Ringland, Bob, 5
RMAX helicopter, 23-24
Rotorcraft Aircrew Systems Concepts Airborne Laboratory (RASCAL), 23, 27

Rotorcraft Project Agreement (RPA), 24

S

SA 330 Puma, 12
Schmitz, Fred, 6
Senior Technologist (ST), 24
Shadow, 21
Shafer, Jerome, 3, 13
Sikorsky S-92, 21
Skyship 500, 11
Stanford University, 6-7, 15-16
Stapleford, Bob, 5
Statler, Irv, 7-8, 13
Systems Technology, Inc. (STI), 5-9, 11, 15, 17

T

Technical Project Officer (TPO), 13, 15
Technion-Israel Institute, 3, 13-14
Tischler, Morris, 2, 25
Tits, André, 19
Tucker, George, 11, 14, 17, 23

U

UH-60, 16, 19, 23-24
University of Maryland (UMD), 2-5, 12, 19
unmanned aerial vehicle (UAV), 7, 9, 21, 23-24
U.S.-Germany MOU, 11-12, 18
U.S.-Israel MOU, 13-15, 19, 24

V

V-22, 7, 9, 16
Variable Stability In-flight Simulator Test Aircraft (VISTA), 21, 24
Vertical Motion Simulator (VMS), 16, 27
von Grünhagen, Wolfgang, 11
vortex particle method, 24

W

Wang, Jerry, 4

X

X-2, 24
XV-15, 7-10, 12, 15-16, 27

Y

YB-35, 5

On Being in the Right Place, at the Right Time...

George E. Tucker
NASA

Prologue

One of the hardest things to do when writing any kind of research-pilot memoir (besides, at the age of 70, accurately recalling details of research efforts pursued 30-plus years ago) is give appropriate credit and thanks to all the engineers, mechanics, technicians, crew chiefs, and managers who made the Ames flight-test environment one where you could dependably go up, fly productively, and return safely to fly another day. I have a mind's-eye picture of standing on the hangar floor next to one of our Learjets chatting with Mike Lakowski, the assigned crew chief. I had been a pilot in the flight office for several years and was struck by Mike's reference to the Ames pilots as "the drivers." While I might have casually referred to other, mostly military pilots as "A-4 drivers" or "Herc drivers," I had never thought of the pilots at the Flight Operations Branch as such. The observation, as offered, seemed to affirm both a certain level of respect for the piloting task, and a recognition that we were, as a group, just another cog in a very complex flight operation. As such, this driver offers a most sincere thank you to everyone who made the flight operation go so well for me day by day, year after year, most of whom (regrettably) will receive no individual mention in this chapter.



George Tucker, 1980.

A Fortuitous Visit

I was a fairly senior Marine Corps captain assigned to the Naval Aviation Safety School within the Naval Post Graduate School in Monterey, California, when I first got the bright idea of visiting NASA Ames Research Center. My intent was to find fresh material for the helicopter aero course I was teaching. I had been reassigned from the Rotary Wing Aircraft Test Directorate at the Naval Air Test Center (NATC), Patuxent River, Maryland, in mid-1977, to the Naval Aviation Safety School, and had commenced my assignment using the course outlines of my predecessors in that position. I recognized how relatively unknowledgeable I had been (as a First Lieutenant CH-46 pilot in Vietnam) of some pretty basic helicopter performance and handling-qualities issues. This recognition spurred me to identify and dispel as many of the "old wives' tales" from the world of fleet helicopter operations as I could. It seemed to me that a good starting point would be to study the performance and emergency procedures sections of the various flight manuals used by Navy and Marine Corps aviators every day. Having been primarily a CH-46 pilot prior to attending the U.S. Naval Test Pilot School (TPS) at NATC, and the CH-53E project pilot post-TPS, I was largely unfamiliar with many of the other helicopters in the Navy and Marine Corps fleets. I persuaded the Executive Officer (XO) of the Aviation Safety School to let me make a quick tour of the manufacturers of these aircraft, with a list of questions I had formulated from reading the various flight manuals and from working with my aviator students. I learned a lot from that quick tour and incorporated as much of the material as possible

into my course. With the close proximity of Ames Research Center, Moffett Field, to Monterey, and its recent designation as the Lead Center for rotorcraft research within NASA, it seemed like a no-brainer to try to find some folks at Ames who could help me further freshen the material in my course.

I had recently read an aviation periodical article, by Fred Schmitz and Rande Vause of the Ames-Army Lab, at that time the Aeromechanics Laboratory of the U.S. Army Research and Technology Laboratories, that had some new thoughts about the optimum trajectory to use in clearing an obstacle on takeoff from a hover, based on flight test data. While their work focused on the performance characteristics of their chosen test aircraft, it, nonetheless, demonstrated the value of rethinking a basic takeoff maneuver with the intent of improving it. Before I made any mention of this work in my class, I wanted to better understand the testing that had been done, and I called Fred to arrange a meeting.

Having come from an operationally focused military background, I found the campus at NASA Ames, with its wind tunnels, simulators, and hangars full of aircraft, very exciting. I have a vague recollection of starting my visit to Ames with an appointment to talk to Fred about his article, but a much stronger recollection of ending up in the office of Andy Morse, Chief of the Aeromechanics Laboratory's Fluid Mechanics Division, and talking with Andy and Dave Key. Somewhere along the line, Fred must have inquired as to whether I might be interested in working for the Army at Ames. The conversation was very exploratory. I believe Dave was Flight Controls Division Chief at the time and had been called into the meeting to assess me as a possible hire. Although I had a B.S. with an emphasis on systems engineering from the Naval Academy and Naval TPS in my background, I did not think of myself in any way as an engineer. Dave framed the conversation for us when he described how disenchanted he had been as an engineer at Calspan Corporation, when pilot-engineers in the office would get up from their desks and go to the window each time one of their test aircraft taxied by. Dave made it clear that if I were to work for him there would be no flying associated with the job. Needless to say, any thoughts that I might have had about a career with the Ames-Army Lab largely evaporated. I went back to my classroom at Monterey with no sense of loss as I had not, at that point, considered leaving the Marine Corps.

On a subsequent trip to Ames I met with Dr. Jack Franklin, Chief of the Flight Dynamics and Controls Branch in Building N-211. Jack immediately sized me up as a candidate for the Flight Operations Branch located upstairs in N-211. Jack arranged a meeting for the two of us with Bob Innis, Chief of the Flight Operations Branch. Bob, I believe, was considering how he was going to staff the evolving research pilot requirements brought about by the ongoing transfer of helicopter assets from NASA Langley to Ames. Given that one of the potential assets was a CH-53, my couple of years as the project pilot for the CH-53E Project at NATC may have looked appealing to him. Immediately prior to arriving at NATC to attend TPS, I had completed the Jet Transition course for Army-trained Marine Corps aviators at Naval Air Station Meridian, Mississippi. Long and short of it, Bob had the Ames Personnel folks send me a job offer. While I had not seriously considered leaving the Marine Corps up to that point, the opportunity to become a research pilot at Ames was too appealing to pass up. Delivering that news to the XO of the Safety School, having been in the instructor position only a little over 1 year of an expected 3-year tour, was not easy.

The fellow who had been hired into the Flight Operations Branch immediately before me would not accept the GS-12 salary offered and held out for a GS-13. Bob had struck a deal with Personnel that this fellow could be hired as a GS-13 if the next hire would be at the GS-11 level. Given that I was still single, the salary was not a significant impediment, and I came onboard as a GS-11. I found out later that John Burks, an Academy classmate of mine, and member of the TPS class before mine, had been offered the job, but couldn't accept the salary. I got the flying job, and John got a desk at Ames, albeit a large one as the Director of Aeronautics, ultimately. It never once occurred to me that I had come out of that deal anything less than on top! And the CH-53 was never transferred to Ames.

At that time, the Flight Operations Branch was part of the Aeronautics Directorate headed up by Dr. Leonard Roberts, with Jack Boyd as his assistant. One of the stops in the onboarding process was an interview with Dr. Roberts. I recall meeting with Jack Boyd first and finding him a very pleasant and genteel individual. Jack then introduced me to Dr. Roberts, who was not particularly smiling, and even a bit stern. In my interview, he indicated that, although I had been hired as a research pilot, I would be expected to do much more than "just fly airplanes." (This was sounding, as you might guess, significantly better than the message I had received from Dave Key.) I would be expected to be an integral part of the research effort and to write flight reports and papers. This was neither a surprise nor a disappointment to me, but I did find the delivery thought-provoking—very much as intended, I suppose.

The Flight Operations Branch

I had not thought too much about the relationship between NASA and the Army at Ames, but I knew there were more helicopters coming to Ames, and that some of them were Army assets. With all of them, regardless of ownership, coming to the NASA Ames Hangar, there was little doubt that I would be flying some of them. I had seen the two NASA/Army XV-15s under construction during an earlier visit to Bell Helicopter. I had flown UH-1s as a Marine pilot in Army flight school, flown the AH-1 Cobra at NATC, and had a good bit of combat experience in the CH-46, the "little brother" of the heavier, larger CH-47 Chinook. Two NASA/Army Rotor System Research Aircraft (RSRA) were being built by Sikorsky Aircraft with their ultimate destination being Ames, not Langley as originally planned. In addition to these aircraft that the Army either owned outright or jointly with NASA, there were close to a dozen aircraft in the NASA Ames Hangar that the pilots and flight engineers in the Flight Operations Branch supported. How could this job get any better?

The Flight Operations Branch pilot staff was a pretty senior bunch. Most had been military pilots prior to coming to Ames, and some had been aero engineers at Ames while flying with the reserves. George Cooper, whose name I knew very well from having used his Cooper–Harper Handling Qualities Rating Scale at the Naval Air Test Center, had retired shortly before I arrived. By and large these fellows were excellent, and very adaptable, research pilots. Only one of these pilots, as I recall, had found a way to turn the portentous phrase, "Hey, watch this..." into the rhetorical phrase "Are we going to talk about it, or just do it?"

In addition to the NASA pilots, there was always at least one Army helicopter pilot in the NASA flight office. Dan Dugan was a NASA hire who had previously staffed this position as an Army aviator. Bob Merrill (transferred from Langley with the aircraft), Grady Wilson, Pat Morris, and Rick Simmons each occupied this position during my years at Ames. In addition to the one Army

pilot assigned to the Flight Operations Branch, there were always additional Army pilots assigned to the Ames-Army Lab as engineers, researchers, and managers, who flew the various aircraft of the increasingly large NASA Ames flight operation. Tom Almojuela, Cliff McKeithan, Vic Ross, John Henderson, Chip Adam, Loran Haworth, and Ron Seery served in these roles. In addition to flying the Army helicopters, the Army pilots were generally welcome to fly the remaining NASA aircraft in the hangar, depending on project needs and their personal backgrounds and availability.

Project Work

Having been in naval aviation up to this point in my career and a part of the Naval Air Systems Command, I found working solely with NASA and the Army a bit of a shift in emphasis and style. The working relationship between NASA and the Ames-Army Lab appeared to be seamless and very effective—so much so that I, on occasion, found myself wishing that the Naval Air Systems Command had a similar agreement with NASA.

The research pilot position at Ames was, from day one, nothing but one of a kind. Not only was there an extensive stable of aircraft in the hangar, but NASA had also recently brought online two new flight research simulators with extensive motion envelopes: the Flight Simulator for Advanced Aircraft (FSAA) and the Vertical Motion Simulator (VMS), both housed in a very large, dedicated simulator building. In addition to the FSAA and VMS, there was at least one other full-motion simulator and several small fixed-base-simulator “cabs” in other buildings. On any given day, if you, as a research pilot, were not in one of the aircraft, you were in one of the simulators or participating in project planning of some sort. There seemed to be no end of flight and simulator projects needing some degree of pilot participation.

While I had been hired primarily because of my helicopter background, all the Flight Operations Branch pilots also flew what were generally referred to as “support” versus “research” aircraft that did not involve modification of their flight control systems. These support aircraft included a C-141 and a Learjet 24 used to conduct infrared astronomy studies, an unmodified Learjet 23 and a Convair 990 supporting Earth resources observations, and a Cessna 402 supporting avionics systems development missions. For most of the period considered in this chapter, the Aircraft Operations Division also had a Beechcraft Super King Air to ferry experimenters between Moffett Field and Edwards Air Force Base (EAFB) in support of the ongoing XV-15 safety-of-flight qualification, and between Moffett Field and Naval Auxiliary Landing Field (NALF) Crows Landing in the San Joaquin Valley, where a considerable amount of day-to-day project flying was conducted.

Whereas the NATC had decreed that a pilot would not be simultaneously qualified in more than three aircraft, Ames had no similar restriction. Instead, a pilot could be qualified in as many aircraft as he could stay current and proficient in. Being current was a matter of numbers; being proficient was more subjective, the assessment being shared by the Chief of the Flight Operations Branch and the pilot.

From 1978 to 1985, I spent quite a bit of time in the C-141 and our very versatile Learjets, sometimes to the detriment of flight research and simulation work. The C-141 airborne observatory and Learjet astronomy missions were flown mostly at night, impacting one’s schedule on the day of the flight as well as on the following day. These aircraft commonly

deployed several times a year for periods up to several weeks, resulting in some interesting flying, sometimes outside of the continental U.S. They were also a break from the flight research work. During my first 7 months at Ames, I was deployed in the Learjet to Denver, Colorado, Fairbanks, Alaska, and Athens, Greece, for periods of 1 to 3 weeks each time. While the deployments were disruptive to research project work, the flying was always interesting and, more importantly, contributed greatly to one's general competence in the air.

One of the keys to being fully comfortable in several aircraft simultaneously is to fly often. The more time in a specific aircraft the better, but flying just about any aircraft aids in maintaining one's comfort across several. During these early years at Ames, the Aircraft Operations Division was uniquely supportive of pilot proficiency. The Aircraft Services Branch normally cleared the T-38 and the OH-6 or one of the UH-1s for flight daily and placed them on the flight line. Any of the pilots who were qualified in those aircraft, and had the time to take them up, were encouraged to do so to maintain or improve pilot proficiency. In addition to maintaining currency in the Ames aircraft, several of us were also flying with the Air National Guard or reserves frequently. For my part, flying with the Marine Corps Reserve for a year, and then extensively with the California Air National Guard, proved an excellent way to stay grounded in real-world, day and night military missions. My Air National Guard unit was conveniently located on the other side of the Moffett Field runways from Ames, making it relatively easy to pick up an additional flight training period or two each week, usually at night. The qualification and currency requirements of the Air National Guard were essentially the same as for the U.S. Air Force, and for the mission of "aerospace rescue and recovery," a for-real mission over land in California and off the coast. With the ability to air-refuel our HH-3C helicopters from the unit's HC-130s, we were able to accomplish overwater rescue missions far beyond the range of the Coast Guard. Although the Air National Guard flying did not overlay the day-to-day mission of the Army very well, the unit took operational training very seriously. Summer camps and limited deployments to challenging flight environments created their share of "learning opportunities."

New Rotorcraft Assets

As I saw it, the many new aircraft that came to Ames between 1978 and 1980 proved a challenge in many respects. The flight operation needed more personnel to maintain the aircraft. Some of them came with the aircraft from the manufacturer or from Langley. In some cases, very specialized support equipment and training were required to effectively, efficiently, and safely operate the new aircraft. New research airframes that were joint NASA/Army ventures also required a melding of NASA and Army management philosophies.

The two NASA/Army XV-15 tiltrotor aircraft were first delivered to Dryden Flight Research Center for safety-of-flight qualification, then moved to Ames for the flight research portion of their stay. Dan Dugan (Army/NASA) and Ron Gerdes (NASA) did most of the project flying for the XV-15 in the early years. Likewise, the two NASA/Army RSRAs were delivered from Sikorsky Aircraft to Langley Research Center, Wallops Island, Virginia, for safety-of-flight testing before being trucked to Ames as part of the transfer of rotorcraft assets from Langley to Ames. Warren Hall (NASA) and Bob Merrill (Army) were the project pilots for virtually all the RSRAs flights during the early period. My pilot contributions to these two projects were very limited.

Whereas the SH-3 was ferried from Langley to Ames by Warren Hall and Bob Merrill, the CH-47 was ferried out by a Langley crew and left with little fanfare on the Ames ramp. During this period, the Flight Operations Branch was operating an OH-6A, a couple of UH-1s, an AH-1G Cobra, a de Havilland DHC-6 Twin Otter, a highly modified de Havilland C-8A Buffalo (augmenter wing), another highly modified de Havilland C-8A Buffalo (Quiet Short-Haul Research Aircraft), and a Bell X-14B vertical takeoff and landing test bed. Additionally, a YAV-8B Harrier arrived in April 1984 and an AV-8C in January 1986.

Given my background with the CH-46, and the involvement of most of the other helicopter pilots on the staff with the XV-15 and RSRA project offices, I was assigned as the CH-47 project pilot upon its arrival at Ames. I called a contact Dan Dugan had given me at the Army Aviation Test Board at Ft. Rucker, Alabama, and found that they would both support the flight hours needed for my transition and help me get into a CH-47 ground school class that could be attended simultaneously. Mr. Dean Resch, a senior warrant officer with the Test Board, was my primary instructor. Dean was a superb pilot and instructor, and was clearly very well thought of at the Test Board. Years later, after Dean had retired from the Army and hired on with the Federal Aviation Administration, I was able to wangle an invitation for him to be an evaluation pilot on one of our VMS tiltrotor simulations, as well as receive a demonstration flight in our variable-stability UH-60. Really good people have a way of popping up again and again in the flight test business.

The CH-47B airframe had been operated as a flight-control research facility since the late 1960s, and used by Langley since the mid-1970s in their VTOL Approach and Landing Technology program. When the aircraft arrived at Ames in 1979, the flight control system had been modified with full-authority, parallel electro-hydraulic servos that could be engaged and driven with inputs from the cyclic and collective controllers at the evaluation pilot station on the right-hand side of the cockpit. A Sperry 1819A digital computer and an Electronic Associates, Inc. TR-48 analog computer with its large patch panel provided all the onboard computational power.

Bill Hindson, who had come to Ames from the National Research Council flight test operation in Ottawa, Canada, via the Ames-Stanford Joint Institute of Aeronautics and Acoustics, took the lead in designing projects for the CH-47B and upgrading its facilities. Bill, who resided in the Flight Dynamics and Controls Branch (initially headed by Dr. Jack Franklin and later Dr. Vic Lebacqz), was not only an exceptionally capable engineer, but also a very experienced research pilot. The project approach with the aircraft was to start slow and learn the systems and the useful envelope of the aircraft. Although the electronic control system had been thoroughly qualified by Langley, extensive requalification testing was performed at Ames to provide familiarization with hard-over, slow-over, and force-override characteristics. The variable stability system was qualified for use up to 120 knots, including low-speed and hover operations to touchdown. The CH-47B was subsequently used at Ames as a flight research facility for advanced rotorcraft control-augmentation systems and as an inflight simulator to validate selected results from the VMS. The aircraft was also used to obtain data for the proposed revision of the military specification for helicopter handling qualities, MIL-H-8501A. Over time, the Sperry digital computer was replaced with a ruggedized Digital Equipment Corporation PDP-11/73. The standard flight inceptors at the evaluation pilot station were replaced with a programmable force-feel system developed by Calspan and a four-axis, side-arm controller. A color electronic display system with a programmable symbol generator was installed to permit

the investigation of display formats for a variety of vertical takeoff and landing (VTOL) and helicopter missions.

Bill Hindson was an excellent mentor and very successful in pulling young engineers from the Flight Dynamics and Controls Branch into flight test project work. These young engineers included Bill Decker (NASA), Michelle Eshow (Army/NASA), Jeff Schroeder (NASA), Doug Watson (NASA), and Katie Hilbert (NASA). Michelle, Jeff, and Katie all flew onboard the aircraft as system operators at one time or another, a bit of a rarity for the research aircraft at Ames. Bob Chen (Army) and Vic Lebacqz (NASA) were also very involved in various aspects of the flight test work. Also rare was the fact that the crew chief flew on the aircraft as well. In the early days the crew chiefs were Mike Boyer and Jim Phillips.

Most of the safety pilot flying on the CH-47 was handled by Bob Merrill, Vic Ross, Grady Wilson, and myself. Bill Hindson was by far the most frequent evaluation pilot, although we often had guest evaluation pilots from outside Ames. Flights were generally conducted on the west parallel taxiway at Moffett Field or on the off-duty runway at NALF Crows Landing. Most of the work at Crows Landing started with an early morning launch from Moffett Field to be at Crows Landing while the winds were still low. Second and third flights might shift to tasks that were not so dependent on low winds. One of the few flight incidents that I can recall during my



The CH-47B Team. From left: Ed Farr, Dean Carrico, Bill Decker, Mike Moniuszko, Kit Boyce, Mike Boyer, Vic Ross, and George Tucker.

time at Ames unfolded on one of those early morning departures for Crows Landing. As we started our climb to clear the East Bay hills, a light on the master caution panel indicated that the oil temp on the right-side engine nose gearbox was rising rapidly. We pulled that engine back to idle and executed a turn back toward Moffett Field. I had read an article in a recent copy of the Army's *FlightFax* safety publication where a Chinook in an identical situation was forced by weather to prolong its landing, and had come apart mid-air when the drive shaft from the engine severed at the nose gear box, but continued to flail the control rods and oil lines as it was still being driven from the opposite end by the combining gearbox. With this piece of recent history in mind, neither a transit back across the Bay nor a much longer circumnavigation around the south end of the Bay looked appealing. Instead we made a low-power descent to the decomposing runway of the recently closed Fremont General Aviation airport. The approach to the runway for a single-engine run-on landing became a bit more complicated when we saw that a wire barricade had been erected across the runway to discourage its use. The geometry of the situation was such that it was a much better bet to land long rather than short, to avoid the barricade. We landed comfortably and got the aircraft stopped before the end of the relatively short runway. The systems operator in the back of the aircraft got a virtual front-seat view of the approach via the display for a camera system, one that had been installed under the cockpit for an experiment that we were planning for Crows Landing that day. The barricade clearance, I believe, was much more unnerving to the systems operator than to the pilots, as the eye height of the camera was several feet lower than that of the pilots—and the rear landing gear even lower. The CH-47 became a white-with-blue-trim static display for a couple of weeks, readily visible from the Nimitz Freeway a short distance to the east. My decision not to fly the aircraft back to Moffett Field single-engine did not please everyone, but I felt vindicated once the then-Crew Chief, Perry Silva, had removed the nose gearbox and placed it on the ground next to the aircraft. When the input flange on the gearbox was rotated, the loose ball bearings in the failed bearing(s) made a sound somewhat akin to a runaway pachinko machine! You make the best decisions you can with the knowledge you have at hand; sometimes it works out, sometimes it doesn't.

After a long and very productive life with NASA, the CH-47 was finally returned to the Army in 1989 to be inducted into the CH-47D modernization program at Boeing Helicopters. By the time the aircraft left Ames, I had logged more than 400 hours in it, almost all having been flight project related. The Chinook seemed like quite the old friend, but one whose time had come. The effort to replace it with a variable-stability UH-60 was well under way by that time.

Some Thoughts on the Research Pilot Position

Much of the job of a research pilot at Ames was to be available to the Army and NASA engineers whenever they desired input on the formulation of their projects and/or the flying of their designs. Acceptance of this obligation kept a pilot busy much of the time. As an evaluation pilot, especially of systems that are headed for possible deployment or in the development of specifications for these systems, one of the challenges is determining where your skill level and performance on evaluation tasks fall, in comparison to that of the "average" aviator who will eventually fly the system in the field. It is important to be discerning, not only of how well you are able to accomplish an evaluation task, but also of how you approach it and what variables most impact your performance. It is essential that an evaluator "rate it as they see (and fly) it," especially when his or her demonstrated performance is less than he or she thinks it should be, for one reason or another. If you can't obtain acceptable performance, there's a good possibility that there are other pilots out there who will have similar difficulty. The most important question

to try to answer is “Why is it difficult to fly?” If you have given your best effort in flying each task, and have thoughtfully identified all of the nuances that affect pilot performance, you have earned your paycheck.

There are as many ways of approaching flying as there are pilots. Early in my time at Ames I came across a quote in a book on Zen practice by Suzuki [1] that would stick with me: “In the beginner’s mind there are many possibilities, but in the expert’s, there are few.” Applied to the task of evaluation, this observation encourages the evaluator to be very open-minded when answering the “Why?” question.

Endless Project Possibilities

Being involved in a broad cross-section of projects was one of the perks of the research pilot job. Each interesting project always seemed to lead to even more interesting projects. Some of the endeavors that I was fortunate to participate in are discussed below.

Airships. Soon after arriving at Ames I was asked by Peter Talbot (NASA) and Hiro Muira (NASA) of the Ames Aeronautical Systems Branch to be an evaluation pilot for their fixed-base simulation of a notional “buoyant quad-rotor” airship; a hybrid comprising a gas-filled envelope with a helicopter-like rotor apparatus at each of the four corners of a rectangular frame attached to the envelope. My participation in the simulation led to an opportunity to fly one of the Goodyear blimps, *America*, at its base in Houston, Texas, to become familiar with fundamental airship inflight dynamics. Goodyear’s chief pilot for the *America* operation, John Moran, generously provided me with a fascinating 2-hour familiarization flight in *America* and an unplanned demonstration of just how hard it can be to get an airship back on the ground with gusty, 25-knot surface winds! Mid-1983, Paul Gelhausen (NASA), also of the Aeronautical Systems Branch, requested assistance in an evaluation of an Airship Industries Ltd. Skyship 500 sponsored by the Naval Air Development Center, to be flown at NATC. Mark Tischler (Army) and I were to accomplish the frequency sweep part of the test plan, providing flight data for validation of a NASA simulation of the airship. Mark had recently joined the Aeromechanics Laboratory from Systems Technology, Inc. (STI) and was very familiar with the Fast-Fourier-Transform program being used by STI, the contractor with overall responsibility for the flight test. The Skyship 500 was a much more modern airship than *America*, with a ducted-fan propulsion system and a control column for pitch and heading similar to any general aviation aircraft.

Later, in the early 1990s, a VMS simulation of a proposed airship on the order of 400 feet in length resulted in an opportunity for me to fly an even more modern 220-foot-long airship, the Sentinel 1000, built by Westinghouse Airships, with ducted fans and a fly-by-wire control system with all flight control inputs introduced via a side-arm controller at each pilot station. The eventual “come back” of the commercial airship is always just around the corner.

Helmet-mounted display systems. Early work on helmet-mounted display systems at Ames resulted in an invitation to fly and evaluate a helmet-mounted display system designed and built by Hughes Aircraft for an unspecified customer. The helmet display system presented the imagery on the pilot’s helmet visor from a head-tracked sensor mounted under the nose of the host Hughes 500 helicopter. While the display was easy to view, I do not recall there being any way to determine where the pilot’s head was pointed with respect to the nose of the helicopter, making it somewhat difficult to orient a night scene to the aircraft. The daytime demonstration

flight was made in the Los Angeles basin with the telephoto optics being successfully demonstrated against the occasional backyard swimming pool.

The next opportunity to fly a helmet-mounted display system came in the form of a familiarization program for the Pilot Night Vision System (PNVS) designed for the AH-64 Apache. The system had been installed on two AH-1 helicopters, referred to as “Surrogate Trainers,” operating out of an airfield at Yuma Proving Ground, Arizona.

My instructor pilot for the PNVS familiarization was an Army warrant officer who, as luck would have it, had been a student of mine in the Aviation Safety School at Monterey. The surrogate trainer aircraft were flown nap-of-the-earth (NOE) in the desert landscape in full darkness with the trainee in the front seat on the PNVS and the safety pilot in the back on night vision goggles.

The man-machine interface issues with this system were, in my mind, considerable. I took the details of the experience back to Ames and soon found NASA was obtaining the helmet and head-tracker portion of the PNVS for use in the VMS for handling-qualities studies in the night, degraded visual environment. The fundamental challenge in flying with the PNVS was that the image generated from the infrared sensor on the nose of the aircraft was presented only to the right eye, via a monocle. The left eye, being slaved to the right eye, viewed whatever scene was directly in the line of sight. To concentrate on the sensor-provided scene, the pilot had to will the right eye to be dominant and disregard the image coming into the left eye. However, when the pilot needed to scan the flight instruments or other items inside the cockpit, or view events outside the aircraft, he would necessarily have to will the left eye to be dominant to focus on the instrument panel, or outside scene, respectively. The ability to readily switch dominant eyes does not come easily to everyone and is extremely fatiguing to all, especially as the flying tasks and mission handling requirements become more intense. The challenge in sorting out the superimposed images left me skeptical that the system would be easily and comfortably adopted in the field. I, personally, considered flying with this helmet-mounted display system the ultimate “unnatural act.”

My unconcealed lack of enthusiasm for this system seemed to resonate with Dr. Irv Statler, who invited me to attend a joint-services, night operations working group meeting with him. When it came my turn to offer some observations to the group, I found myself voicing my concerns to a largely disinterested group; the system-unique pilot workload issues were difficult to appreciate if one had not actually flown the system. Also, at that point in time, it was likely a foregone conclusion that the PNVS, as experienced on the surrogate trainer, would be fielded on the AH-64.

So much for having one’s flight experience impact the future implementation of systems. While I have no recollection of having made a positive impact on any of the working group attendees, I did take home a real “keeper” of an observation offered up by an A-7 pilot describing night, low-level missions while wearing night vision goggles: “You can’t beat the record for low altitude flying, only tie it.” Truly, words to live by if you are a frequent denizen of the night, low-level world.

Not only was the PNVS system used extensively in simulation at Ames for several years thereafter, one of the Surrogate Trainers from Yuma found its way to the NASA Ames Hangar as soon as there were sufficient AH-64s in the fleet to provide adequate aircrew training resources.

The PNVIS system has been in the field for many years now, on the AH-64. While obviously the PNVIS has been successful in the field, it remains to this day, for me, the ultimate demonstration of “high pilot workload.”

Frequency sweep testing. The frequency sweep data acquired on the Skyship 500 was one of the first of many sets obtained in simulation and in flight with Dr. Mark Tischler. Not long after the experience with the Skyship 500, Mark asked me to participate in a helmet-mounted display task under the U.S.-Israel Memorandum of Understanding (MOU) on Helicopter Flight Control and Display Technology. The project design consisted of moving a sight reticle displayed on the pilot’s visor in response to aircraft vertical vibration to cancel the effects of the unavoidable movement of the eye through the vestibular-ocular reflex. Part of our contribution to the simulation effort was to instrument the helmet of an AH-64 pilot at the Army flight activity at EAFB with accelerometers in the pitch axis to gather flight data in high-vibration maneuvers. The correlation of aircraft vibration and pilot head motion was analyzed using Mark’s Comprehensive Identification from Frequency Response (CIFER[®]) software to better understand the magnitude of the effect in a currently fielded helicopter. This time around, Mark left most of the data analysis to me, a person for whom root locus and Bode plots had been a bit of a mystery while an undergraduate.

Mark was an excellent mentor, both in the use of the CIFER software and interpretation of the results. We had worked together quite a bit by the time that we, along with Jay Fletcher (Army), were given the opportunity to obtain frequency response data from a UH-60 equipped with the Advanced Digital Optical Control System (ADOCS) being flown by Boeing Helicopters at their Wilmington, Delaware, flight test facility. Pat Morris, then one of the Army pilots in the Ames Flight Operations Branch, had flown a previous evaluation of the ADOCS demonstrator aircraft. During his evaluation flights the flight control system had shown itself susceptible to pilot induced oscillation (PIO) when high-gain tasks were attempted. The control inceptor configuration for ADOCS was somewhat advanced, in that it comprised a three-axis limited displacement, force-actuated controller for the pitch, roll, and yaw axes in the pilot’s right hand, and a displacement-actuated collective controller in the left. When Pat dutifully pointed out the PIO tendency to the government folks sponsoring the evaluation, their first, somewhat defensive response was, “Well, you just can’t ham-fist the controls.” When it came my time to accomplish a comprehensive evaluation of the aircraft handling qualities in advance of the planned sweeps, I too found the PIO tendency. When it came time to do our frequency sweeps, we found that the Boeing test pilot had decided to do them himself, rather than allow me. To be fair to Boeing, the combination of a force controller for pitch, roll, and yaw inputs, combined with the very tight monitors on the full authority ADOCS control actuators, made it likely that I would have to experiment a bit with control technique and control input size and rate to avoid tripping the monitors during the sweeps, possibly taking up an undesirable amount of additional flight time. Nonetheless we could not help thinking that, at the root of Boeing’s concerns, was a fear that I might “ham-fist the controls” during the sweeps. “Ham-fisting” subsequently took its place in our lexicon of professional descriptors associated with pilot evaluation of high-gain tasks on high-bandwidth advanced controls systems.

Helicopter external load testing. At some point along the way, it occurred to Mark that these frequency sweep test techniques could be used to great advantage in analyzing the interrelated dynamics of helicopters and the sling loads that they occasionally carried. Mark was quick to enlist Luigi Cicolani (Army) in transitioning Luigi’s theoretical work on sling loads to the flight

test environment. A Conex container, like those that Luigi had used frequently in his analyses, and a set of slings were obtained, and a test program initiated. The resulting sling-load experiments, conducted under the evolving U.S.-Israel MOU, proved very successful and informative, and are ongoing at Ames today, with far more sophisticated data gathering and active and passive load stabilization systems.

The creation of a modern handling-qualities specification for helicopters. The Army effort to develop a comprehensive handling-qualities specification to replace MIL-H-8501A, *Military Specification: Helicopter Flying and Ground Handling Qualities, General Requirements for* (07 SEP 1961), was initiated early in my time at Ames. This effort was led by Dave Key and Chris Blanken, and, over the years, seemed to take on a life of its own. (“8501” comprised only a few pages of requirements, but had been the sole helicopter handling-qualities specification for all the work I had done at TPS and in the follow-on years at NATC). This effort produced a voluminous and definitive *Aeronautical Design Standard, Performance Specification, Handling Qualities Requirements for Military Rotorcraft, ADS-33E-PRF, 2000 (ADS-33)* and an even larger *Background User’s Guide (BUG)* that described in detail how to use the specification in flight testing. (The fact that the BUG was several times thicker than the specification was a bit of a giveaway to the complexity of the new world of specification compliance determination.)

The path to completion of these updated specs was a long and tedious one, comprising many years of simulation and flight test, both at Ames and various other facilities in the U.S. and abroad. The simple days of the pilot flying a generally defined task and then rating it using the 10-point Cooper–Harper Handling Qualities Rating Scale were gone. While the rating scale would continue to be used, little would be left to the judgment of the pilot beyond assignment of the numerical rating, followed by his/her comments. The new specification would delineate a growing battery of very closely defined, specific tasks based on aircraft mission, with prescribed physical, visual targets for the pilot to use in flying and assessing his own performance, under both good and degraded visual conditions, all the while being timed. It was also no longer possible for the pilot to rate his or her performance overly generously (not that a pilot would ever consider doing this, of course) as the actual performance was easily and precisely measured, and observed by the entire test team. The “that looks about right” method of evaluation gave way to tight definitions of desired and adequate performance. For some of the “old hands” this rigidity was hard to abide. However, on the other side of the coin, the test pilot’s most important job is to faithfully convey to the test engineers, and ultimately the aircraft acquisition folks, a valid assessment of the aircraft—does it, or does it not, meet the specification requirements?

The years from 1978 through 1985 were very busy and very exciting ones for me. It was a great time to be a journeyman research pilot. The work was extremely varied, and most of the folks I worked with were truly excellent at, and dedicated to, whatever they did. In some cases, the experimenters were learning along with the pilots; in other cases, they were the sage voice recommending that “you might want to consider...” and then referring to some relevant historical context.

In Closing

The focus of this book is from roughly 1965 to 1985, while my tenure at Ames, starting in 1978, extended through early 2008 (another 23-plus years). In attempting to reconstruct my activities during those first 7-plus years, I recall so many areas in which we “were just getting started” on

projects that would definitely have lives of their own. The level of flight and simulation work in support of the Army's drive to replace MIL-H-8501A with a modern handling-qualities specification, ADS-33, was accelerating with each passing year—and was eagerly awaited by test organizations in many other countries. The Army's Light Helicopter Experimental (LHX) program was demanding greatly improved rotorcraft flying qualities and helmet-mounted display systems. The effort to identify a replacement variable-stability rotorcraft for the CH-47 was under way and eventually resulted in the Rotorcraft Aircrew Systems Concepts Airborne Laboratory (RASCAL) built on the ADOCS UH-60 airframe. The MOUs that the Ames-Army Lab had with German, Israeli, Dutch, and French research organizations (in which NASA was a full participant under the terms of the Army-NASA Joint Agreement) were bearing considerable fruit and allowing for experimentation in areas that neither the Army or NASA might have initiated on their own. The external load research efforts started in the late 1980s under the U.S.-Israel MOU are still going strong today. I felt very fortunate to have been involved in so many of those efforts just as they were starting to gain momentum.

While I had nothing to do with the administrative side of the Army-NASA Joint Agreement, it was clear that there were stressors during the years covered here, with many of them becoming more serious as budgets grew leaner and as NASA demanded more direct monetary and personnel contributions from the Army. Dan Goldin's tenure as the NASA Administrator, starting in 1991, resulted in the termination of the NASA flight operations at Ames and the movement of some of the aircraft to NASA Dryden and others to the bone yard. It was with a certain pleasure that I watched the Army inform the NASA Administrator that a significant subset of the Ames helicopter fleet belonged to the Army, and that the Army had no intention of moving them from Ames and separating them from the Ames-Army Lab. With the all-but-sure shutdown of the NASA flight operation looming in the mid-1990s, I transferred to NASA's Advanced Air Traffic Technologies project for a couple of years. Meanwhile many of the NASA and Army rotorcraft researchers were moved to a combined NASA/Army Rotorcraft Division in 1997. With the Army helicopters secure in their own assigned hangar at Ames, I was invited to transfer to the NASA/Army Rotorcraft Division as a research pilot. Eventually, even the combined NASA/Army Rotorcraft Division fell victim to NASA's desire to get out of the rotorcraft business.

I always viewed the Joint Agreement at Ames as a wonderful way to gain synergy in shared research goals. As a staff research pilot, I had the opportunity to be directly involved in the planning and execution of many research flight and simulator evaluation projects supporting the shared objectives of NASA and the Army. I was a proud participant, and extremely disappointed to see NASA back out of rotorcraft flight research, choosing to leave as much of it as possible to industry.

Fortunately, NASA and Army researchers at Ames continue to collaborate on non-flight rotorcraft research, and the Army flight operation at Ames is still going strong. I very much admire and respect the NASA and Army researchers and Army pilots who continue to do the good work that improves the state of rotorcraft in this country. Onward and upward!

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Index

A

Adam, Chip, 4
Advanced Air Traffic Technologies, 13
Advanced Digital Optical Control System (ADOCS),
11, 13
Aeromechanics Laboratory, 2, 9
Aeronautical Design Standard (ADS), 12-13
Aeronautics Directorate, 3
Air National Guard, 5
Aircraft Operations Division, 4-5
Airship Industries, Ltd., 9
Almojuela, Tom, 4
Ames Aeronautical Systems Branch, 9
Ames-Stanford Joint Institute of Aeronautics and
Acoustics, 6
Army-NASA Joint Agreement, 13
Augmenter wing, 6

B

Background User's Guide (BUG), 12
Beechcraft Super King Air, 4
Bell Helicopter, 3
Blanken, Chris, 12
Boeing Helicopters, 8, 11
Boyce, Kit, 7
Boyd, Jack, 3
Boyer, Mike, 7
Burks, John, 3

C

Calspan Corporation, 2, 6
Carrico, Dean, 7
Chen, Bob, 7
Cicolani, Luigi, 11-12
Comprehensive Identification from Frequency
Response (CIFER), 11
Cooper, George, 3
Cooper-Harper Handling Qualities Rating Scale, 3,
12
Crows Landing, 4, 7-8

D

Decker, Bill, 7
Dryden Flight Research Center, 5, 13
Dugan, Dan, 3, 5-6

E

Edwards Air Force Base (EAFB), 4, 11
Electronic Associates, Inc., 6
Eshow, Michelle, 7

F

Farr, Ed, 7
Federal Aviation Administration, 6
Fletcher, Jay, 11
Flight Controls Division, 2
Flight Dynamics and Controls Branch, 2, 6-7
Flight Operations Branch, 1-4, 6, 11
Flight Simulator for Advanced Aircraft (FSAA), 4

Fluid Mechanics Division, 2
Franklin, Jack, 2, 6

G

Gelhausen, Paul, 9
Gerdes, Ron, 5
Goldin, Dan, 13

H

Hall, Warren, 5-6
Haworth, Loran, 4
Henderson, John, 4
Hilbert, Katie, 7
Hindson, Bill, 6-7

I

Innis, Bob, 2

K

Key, Dave, 2-3, 12

L

Lakowski, Mike, 1
Lebacqz, Vic, 6-7
Light Helicopter Experimental (LHX), 13

M

Memorandum of Understanding (MOU), 11-13
Merrill, Bob, 3, 5-7
Moniuszko, Mike, 7
Moran, John, 9
Morris, Pat, 3, 11
Morse, Andy, 2
Muir, Hiro, 9

N

Nap-of-the-earth (NOE), 10
NASA, 1-5, 7-10, 13
Naval Academy, 2
Naval Air Development Center, 9
Naval Air Systems Command, 4
Naval Air Test Center (NATC), 1-4, 9, 12
Naval Auxiliary Landing Field (NALF), 4, 7
Naval Aviation Safety School, 1
Naval Test Pilot School (TPS), 1-3, 12

P

Patuxent River, 1
Phillips, Jim, 7
Pilot induced oscillation (PIO), 11
Pilot Night Vision System (PNVS), 10-11

R

Resch, Dean, 6
Research and Technology Laboratories, 2
Roberts, Leonard, 3
Rotor Systems Research Aircraft (RSRA), 3, 5-6
Rotorcraft Aircrew Systems Concepts Airborne
Laboratory (RASCAL), 13

S

Schmitz, Fred, 2
Schroeder, Jeff, 7
Seery, Ron, 4

Sikorsky Aircraft, 3, 5

Silva, Perry, 8

Simmons, Rick, 3

Statler, Irv, 10

Systems Technology, Inc. (STI), 9

T

Talbot, Peter, 9

Tischler, Mark, 9, 11

V

Vause, Rande, 2

Vertical Motion Simulator (VMS), 4, 6, 9-10

Vertical takeoff and landing (VTOL), 6-7

W

Wallops Island, 5

Watson, Doug, 7

Westinghouse Airships, 9

Wilson, Grady, 3, 7

X

XV-15 Tilt Rotor Research Aircraft, 3-6

Y

Yuma Proving Ground, 10

My Experience in Rotorcraft Aerodynamics Research in the Army at Ames

**Chee Tung
Army**

Prologue

Beginning in 1966, my journey to the Army Lab at Ames Research Center took 13 years.

After graduating from New York University in 1966, I joined the Applied Mechanics Department of Cornell Aeronautical Laboratory (CAL) in Buffalo, New York. Dr. Irving Statler was the department head. My first job was working with Mr. Gordon Brady on the minimum power of a helicopter. This was my first time conducting research in the field of rotorcraft. Later I worked with Frank DuWaldt to analyze the H-34 helicopter blade pressure. At that time, I did not know the Applied Mechanics Department at CAL was one of the leading rotorcraft research organizations in the country. Other members in the department working in the rotorcraft area included Raymond Piziali, Andy Trenka, Dick White, Ham Daughaday, and T. T. Chang.



Chee Tung

This was during the Vietnam War era at the end of the 1960s when protests against U.S. involvement began on college campuses. The students and faculty members at Cornell University wanted to break off relationships with CAL because CAL had many classified contracts from the Department of Defense. Consequently, CAL privatized and changed its name to Calspan Corporation.

About the same time, there were changes in the Applied Mechanics Department too. Several key members of the staff left to form two small consulting firms, RASA and Vizex. Three new members, Dr. Al Ritter, Dr. John Erickson, Jr., and Dr. George Kurylowich, joined the department. Sometime in 1970, Dr. Irv Statler announced that he was leaving the laboratory and heading west to join the U.S. Army Air Mobility R&D Laboratory (AMRDL) at NASA Ames Research Center. The once-leading rotorcraft research department had fallen apart. I transferred to a new department, working on the radar signature of reentry vehicles.

In May 1972, I was surprised to get a phone call from Irv. He asked me to join him at AMRDL. Since I had received my U.S. citizenship on May 1 of that year, there was no issue about becoming a government employee. But when I went home and talked to my wife about a new adventure on the West Coast, the idea was met with resistance, as we had just built a new house in Buffalo. Two weeks later, Irv called again. He told me the opening was frozen by President Nixon. This solved the argument with my wife about moving to California. Little did I know that it would take me 7 more years to become an Army employee at Ames. Meanwhile, the winters and snow in Buffalo finally got to me. I took a new job in 1976 at the Containment System Group, Department of Nuclear Energy, Brookhaven National Laboratory (BNL) in New York. During the next 3 years, I often came to San Jose to visit the GE Nuclear Energy Division. I used

the opportunity to contact Irv and visit Ames. At that time, Irv built a rotorcraft flight research group at Ames. He showed me the newly developed Vertical Motion Simulator. I also met two old colleagues from CAL, Robert T. N. Chen and Ray Piziali.

In the spring of 1979, I got a phone call from Jim McCroskey. He asked me whether I was interested in an opening at the Ames-Army Lab, then the Aeromechanics Laboratory of the U.S. Army Research and Technology Laboratories. At that time, my job at BNL was reviewing safety issues of nuclear reactors and I was not doing any research. I could not pass up this opportunity. So, I came to Ames for an interview. I met Mr. Andy Morse, the Chief of the Fluid Mechanics Division, my future boss. I gave a talk titled “Acceleration Forces Generated by an Oscillating Air Bubble” [1]. After the talk, Yung Yu came up to me and asked me about some more detailed calculations. I was impressed by his deep understanding of the mathematical issues. I was also surprised to see Larry Carr in the audience. We had been classmates back at New York University. Soon after I returned home to Long Island, I got an offer from the Army. My wife knew that I was unhappy at BNL because I was not doing research. So, despite the protests of my children, we packed and moved to the San Francisco Bay Area.

At Ames

I reported to work on June 1, 1979. After discussion with Andy Morse, my first assignment was to determine what kind of research needed to be done with rotor wakes. The trailing vortices generated by the helicopter rotor blades influence rotor loads and noise generation in many ways. I thought of first addressing the wake of the hovering rotor and presented my idea to Andy. He asked me to talk to Frank Caradonna, as he was conducting a hovering rotor test, measuring the rotor blade pressure as well as the wake geometry using a single component hotwire. I used the same rotor and collected data using the hotwire, with the help of a young engineer, Steve Pucci. We analyzed the data and published a paper with Army colleagues [2].

Later, Frank told me he wanted to predict rotor-blade surface pressure distributions. This seemed like a simple request but at that time, at the dawn of rotorcraft Computational Fluid Dynamics (CFD), no one had the ability to predict blade surface pressure including the effect of the rotor wake, even in hover. So, we worked closely and successfully, using Frank’s newly developed transonic small-disturbance Finite Difference Rotor (FDR) code, to predict the hovering rotor blade pressure. The idea was simple. Since the computational domain for the CFD code covered a small area around the rotor blade, the influence of the rotor wake outside the computational domain could be treated as a part of the induced angle of attack at the rotor blade. The three-component velocities at the boundary of the computational domain were calculated by the Biot–Savart law. The rotor wake inside the computational domain could be treated as a potential jump. This was the so-called “partial angle concept.” Later it became the hybrid wake concept. The predicted surface pressure results compared well with the test data, particularly in the shock position, but failed at leading edge [3].

The next step was prediction of the rotor surface pressure of a lifting rotor in forward flight. At that time, Andre Desopper of the Office National d’Etudes et de Recherches Aéropatiales (ONERA) joined us under terms of a U.S.-France Memorandum of Understanding for a Cooperative Research Project in Helicopter Dynamics. He suggested using the partial angle concept based on M. J. Drees’ forward flight model for the rotor wake. The results compared to test data were mediocre at best [4]. The difficulty predicting blade surface pressure arises from

the difficulty of accounting for the blade motion and the rotor trim controls for the forward flight condition using a CFD code alone. We needed to adapt a comprehensive code, in which lifting-line theory for lift and drag airloads was replaced by the CFD code. Fortunately, Wayne Johnson of NASA had developed a comprehensive rotor code called Comprehensive Analytical Method for Rotorcraft Aerodynamics and Dynamics (CAMRAD) as an Army employee, before he transferred to NASA. It seemed to be a good idea to combine FDR with CAMRAD to predict forward-flight, blade-surface pressure. I presented the idea to Wayne. As usual, he had to think through the problem before committing himself. After several discussions, he finally agreed to let me modify his CAMRAD source code. The coupling procedure of FDR/CAMRAD code failed to converge, so Wayne introduced the “delta method” to enable the solution to converge in several iterations. The predicted results compared well at high-speed, forward-flight conditions. Thus, the concept of loose coupling of CFD/Computational Structural Dynamics (CSD) was born [5].

A side note is that we completed this work under the pressure of our commitment to present our work at the American Helicopter Society (AHS) Annual Forum. It was the fall of 1983. Frank and I were talking about coupling FDR with CAMRAD in his office when he got a phone call from Dr. Charles Smith of NASA Ames, who was the Aerodynamics Session Chair for the 1984 AHS Forum. He was soliciting abstracts for the next Forum. Without any hesitation, Frank committed to submit an abstract. Looking back, this work would not have developed as quickly as it did without such a commitment. In subsequent years this accomplishment was recognized as a milestone that opened the door to a revolution in rotorcraft computational methods for aeromechanics research and the design of advanced rotorcraft. It stands as an excellent example of the benefits of the close collaboration between the Army and NASA under the Army-NASA Joint Agreement at Ames.

Unsung Heroes

Any experimental test requires teamwork. We had such great support people: wind tunnel mechanics Robert Gaines, William Brandow, Art Cocco, Jack Ollila, and Gene Wells, to name a few; instrumentational technicians like Robert George and Ozzie Swenson; as well as the technical support people from the NASA model shop such as Jim Hansen and Bill Daugherty; and from the NASA machine shop too, like Jim Freel, Pearl Bailey and Fred Lemos. The researchers got all the credit from presenting or publishing test results, but few knew of the hard work by the supporting staff behind them. They are the unsung heroes.

I turned into an experimentalist to take advantage of their support. I got special help from our engineers, Wayne Empey and Georgene Laub. Without them, I do not think I could have gotten any experimental work done.

Looking Back

There were three components to the success of the Army research lab in the early days:

Management vision. They selected young, energetic researchers just out of school and gave them the freedom to attack all the difficult problems related to rotorcraft aeromechanics. They also built the right infrastructure to support those researchers.

Team spirit. Most papers presented/published had multiple authors. They shared the credit. They discussed and exchanged ideas freely without reservation. The best ideas usually came during lunch breaks when people got together sitting under the trees at the cafeteria patio.

The Army-NASA Joint Agreement. Without the collegial support of NASA staff and access to NASA facilities enabled by the Joint Agreement, the Army research team would not have been able to realize the considerable accomplishments that they did.

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Index

A

American Helicopter Society (AHS), 3
Ames Research Center, 1-3
Army Air Mobility R&D Laboratory (AMRDL), 1
Army, 1-4
Army-NASA Joint Agreement, 3-4

B

Bailey, Pearl, 3
BNL Department of Nuclear Energy, 1
Brady, Gordon, 1
Brandow, William, 3
Brookhaven National Laboratory (BNL), 1-2

C

Calspan Corporation, 1
CAL Applied Mechanics Department, 1
Caradonna, Frank, 2-3
Carr, Larry, 2
Chang, T. T., 1
Chen, Robert T. N., 2
Cocco, Art, 3
Comprehensive Analytical Method for Rotorcraft Aerodynamics and Dynamics (CAMRAD), 3
Computational Fluid Dynamics (CFD), 2-3
Computational Structural Dynamics (CSD), 3
Cornell Aeronautical Laboratory (CAL), 1-2
Cornell University, 1

D

Daughaday, Ham, 1
Daugherty, Bill, 3
Desopper, Andre, 2
Drees, M. J., 2
DuWaldt, Frank, 1

E

Empey, Wayne, 3
Erickson, John, 1

F

Finite Difference Rotor (FDR) code, 2-3
Freel, Jim, 3

G

Gaines, Robert, 3
George, Robert, 3

H

Hansen, Jim, 3

J

Johnson, Wayne, 3

K

Kurylowich, George, 1

L

Laub, Georgene, 3
Lemos, Fred, 3

M

McCroskey, Jim, 2
Morse, Andy, 2

N

NASA, 1, 3-4

O

Office National d'Etudes et de Recherches
Aérospatiales (ONERA), 2
Ollila, Jack, 3

P

Piziali, Raymond, 1, 2
Pucci, Steve, 2

R

RASA, 1
Ritter, Al, 1

S

Smith, Charles, 3
Statler, Irving, 1
Swenson, Ossi, 3

T

Trenka, Andy, 1

U

U.S.-France Memorandum of Understanding, 2

V

Vertical Motion Simulator, 2

Vizex, 1

W

Wells, Gene, 3
White, Dick, 1

Y

Yu, Yung, 2

I Was Not an Engineer

Rande Vause
Army

I was flattered when it was suggested that, perhaps, I might make a contribution to this volume. But after thinking about it and reading some of the early inputs, I decided that I really didn't have anything to write about. I even wrote (but didn't send) an e-mail suggesting I was going to turn down the offer. You see, what I learned at the Ames-Army Lab was that I was NOT an engineer. I was never going to be like those others you will read about in this memoir. I was only there for about 10 years; it wasn't long enough to become world renowned, but it was long enough to recognize what a unique place it was and the contribution it made to my life and to the lives of many others. While what I write may not be earthshaking in and of itself, it may be typical of what some of the others were experiencing, and, from my perspective, may be a key to (at a minimum, it might give some insight into) what made the Ames-Army Lab so successful. So, here is my story.



Rande Vause

Home for Christmas vacation in 1965, I heard about a lab that was opening up just a few miles from my house. "Why don't you go put in an application...who knows, you might get a summer job out of it!" So I put in an application and, to make a long story short, I was offered a job. What was surprising about that job offer was that I had no real technical background—I was a math major! Colonel Cyril Stapleton conducted my final interview and extended the offer during my semester break a couple of months later. I don't remember how it came up, but, during the interview, he told me how badly he had always wanted to be an engineer—I think he even graduated with an engineering degree from the Georgia Institute of Technology. While shaving one morning, he said he looked in the mirror and was overwhelmed by the realization that, no matter how badly he wanted to be one, he was not an engineer. As a result, he changed his career path and went on to other things. I'll come back to this in a little while.

I began work at the U.S. Army Aeronautical Activity, later the Army Research Laboratory (AARL), under the Army-NASA Joint Agreement at Ames Research Center, the summer of 1966—the summer before my senior year at Brigham Young University. The Ames-Army Lab was just bringing online the 7- by 10-Foot Wind Tunnel No. 2 that NASA had assigned to the Army. For the life of me, I can't remember how they got the data, but variations in flow velocities across the tunnel were being measured at locations up and down the test section. What I do remember vividly are the hundreds of photo negatives they had accumulated of the monometer board used to record the pressure data. My first assignment was to sit in a darkened room where the negatives were projected and write down the numbers. There may have been 50 per negative. I did that for the first half of the summer—all day, every day. As painful as that was, sitting in a green room (all the NASA offices were painted the same color green) in the library building, keypunching the data, was worse. I was the only one there; the room wasn't much bigger than the keypunch machine and my chair, and the temperature must have been 90 to 95 degrees. At the end of the summer, I thanked everyone, and told them I'd had a wonderful

time, but I would not be coming back. Thanks, anyway. Paul Yaggy, on the other hand, suggested that I be put on leave without pay so that I could come back and work during vacations. Besides, he added, I lived so close by and I could earn some extra money (college guys always needed that). I finally accepted after explaining, once more, that I really didn't want to do this for the rest of my life. Paul said he understood. As it happened, I did go back the following Christmas and semester break, still reminding Paul that it was not something I was going to do for the rest of my life.

After the semester break, a life-changing event occurred: I became engaged to be married. I must say that Colonel Stapleton, Paul, and Andy Morse (whom I worked for) were always sincerely interested in me and in what I was doing. That continued for years to come. It came as no surprise that I received a congratulatory letter from Paul. At the conclusion of the letter, Paul pointed out that, after my graduation, I would, with near certainty, be drafted and sent to Vietnam, as I was quite high on the list. Then he mentioned the brand-new agreement the lab had with Stanford University. The university was accepting lab employees into their Master's program and the lab was paying all expenses and giving time off for classes. If I was interested, he felt sure I could qualify, and he could get me an occupational deferment so that I would not have to go to Vietnam after all. I accepted the invitation with the caveat that I was not going to do this for the rest of my life (I wanted to be a math professor, but I figured the Master's degree would fit in very well).

When I returned to the lab the following summer, things had improved. During the semester break, I had been one of only about half a dozen staff. We now had several more. I did not have to read any more negatives, and I was not again banished to the keypunch machine (at least not to the one in that little room in the library, as we now had our own)! The first assignment Andy gave me was to translate a couple of German reports on single-rotor helicopters (when I had first applied for work at the lab, I had just returned from 2-1/2 years in Germany, and my German was fairly good). I must admit it never occurred to me that perhaps my reading and understanding the technical content of those reports was much more important to their long-range plans than they were to mine. I don't remember anyone else ever reading them. It was the start, however, of a pattern. Andy was our coach, mentor, and/or cheerleader. He was patient and always looked for ways to say "yes" to our thoughts, suggestions, and ideas. And if he didn't think an idea was great, he always found ways to make suggestions to get us back on the right track. Everyone respected his thoughts, judgment, and intentions.

Forces and moments on models in a tunnel are measured by connecting a turntable in the floor of the tunnel, on which the test model is installed, to scales underneath. At this time, the scales were being updated, and I was asked to derive the force equations to be used in the calibration of the scales. It took me a little while to do, but it was something useful and necessary, and something I could do even with my lack of an engineering background. When I finished, Andy looked it over, I believe, and accepted it. It wasn't until much later that I realized I may have been one of the first to have that assignment, but definitely not the last. Several us worked the same problem. It seems, again, I was being gently coached.

One of the great things about the lab was the visitors it attracted. Some came for the summer, some came for sabbatical, and some came just for short visits. Professor Enoch Durbin, from Princeton University and a member of the Army Science Board, was one who came by periodically. He had a test he wanted to run and I was asked to assist him. He and I, eventually,

developed a very warm relationship that lasted until his death just a few years ago. Initially, he would come for a visit with a long list of things for *me* to do. He drove me absolutely crazy. I was not sleeping well one night, anticipating his upcoming visit, when I sat straight up in bed in the middle of the night shouting “I’ve got it, I’ve got it!” I needed to go on the offensive! When Enoch came in the next day, I had a list of things for *him* to do, and I made sure I got the first words in. It worked beautifully. From that day on, we got along famously. At some point, I realized I wasn’t just learning about rotorcraft; I was actually learning life lessons about how to manage programs and people. In later years, Andy just laughed when I told him about this experience.

The first technical paper I ever wrote was with Enoch [1]. We were invited to present it at a national American Helicopter Society meeting. I don’t have any idea how many rotorcraft-related engineers were there, but there were a lot. That completely changed my perspective on what I was involved in—this helicopter industry. I was a young guy at this time, probably 25, and the only thing I knew about the helicopter industry was that there were only about a dozen rotorcraft engineers at Ames Research Center. I had no idea I was part of something so big. Wow, was that an eye opener. After that I began to have second thoughts that maybe I wanted to hang around for a while. We were, actually, a part of something important.

As I mentioned previously, we had quite a few professors associated with the lab. People like Enoch Durbin, Hank Velkoff, Barney McCormick, and a number of others were attracted by, and chose to be a part of, AARL because of the excellent work that was being produced by this small group. Some of the most knowledgeable and influential people in the helicopter world were among these associates. Whatever their personal agendas might have been, they came and left an indelible impression on almost everyone of us. They were interested in us—in helping us become not only better, but the best! How that all happened I don’t know, but Paul and Andy did, I am sure of that.

As one might anticipate, the Vietnam war had a great impact on the lab’s research program. Some of us were a little more involved than others.

- One project I remember, but was not involved in, was run by Dave Sharpe and had to do with “flying coffins.” Extracting injured soldiers was a high priority. Getting them to medical help as quickly as possible was the goal. Someone had an idea that an injured soldier could be put in a box that looked a lot like a coffin. A hook would be floated in the air so that a low-flying aircraft could snag it and reel in the casualty. The only problem was that the coffin would generally start to spin as it was lifted and hauled in (it must have been one heck of a ride)! The lab was asked to figure out how to stabilize it, and our fellows did.
- There were other similar types of projects. We did one for the Army Natick Soldier Systems Center (NSSC) folks. I was asked to mount a C-5A model in the tunnel and survey the wake behind it. The folks from Natick brought in scaled parachutes to see which configurations would best pull payloads/deliverables (food, clothing, ammunition, etc.) out of the back of the airplane.
- One of the more unusual programs involved mounting a “saddle” on a pedestal in the wind tunnel. Doctors came from the Army Aeromedical Research Laboratory, were strapped across the saddle, and “flew” themselves (as in skydiving) to understand the forces on proposed parachutists and to help them determine what body positions and parachutes would work best for the missions under consideration.

- I will never forget the valveless pulse jet we put in the wind tunnel. At the time, it was thought that they could be used as a propulsion source for tip-driven rotors. While the results were positive, the only military application I am aware of was their being dropped into Viet Cong tunnels to drive them out. They were loud (140 dB in our wind tunnel). The vibration outside the wind tunnel in the test area was so intense that we had to stop periodically to give our insides a chance to calm down, so we didn't get sick to our stomachs.

There was no lack of opportunity for creative thinking about real-world problems. And much of what was worked on found timely, real-world application.

As I mentioned previously, Enoch Durbin was a member of the Army Science Board. As a result, he spent considerable time in Vietnam, looking to see where technology might help the war effort. While he was visiting troops one afternoon, the pilot of the helicopter he was being transported in announced to the co-pilot that he (the pilot) was not able to get the helicopter out of the clearing they were in; there just was not enough space. The problem was related to the weight of the helicopter and the outside air temperature. For a heavily loaded helicopter, the problem became more severe as the temperature got higher (e.g., in the afternoon). Apparently, this was not an uncommon problem. It was interesting that some pilots could intuitively do it and others could not, and no one knew why. The co-pilot said "Okay" and got them out. When Enoch returned to Princeton, he asked one of his graduate students to figure out why. This task became the subject of Fred Schmitz' Ph.D. thesis, and a rather complex mathematical analysis was the result. While the analysis was of value, understanding the physics of what was happening was not totally clear to the casual observer. When Fred got to Ames, he decided to do more work—or I should say, he decided that *I* should do some additional work. After many months of analysis, we finally came up with a reasonably decent physical model/explanation of what was happening. Basically, the idea, thought counterintuitive, was to accelerate from hover in ground effect before gaining altitude rather than simultaneously climbing and accelerating at low speed.

What we really wanted to do, however, was to develop some rules that a pilot could use to improve his chances of taking off successfully under heavily loaded conditions, where the physical surroundings did not allow him needed maneuver space. From the pictures on the next page you can see that the "optimized" takeoff produced a much steeper rate of climb than the traditional takeoff did, allowing the helicopter to rise more quickly above obstacles in its flightpath. It took quite a while before we had these pictures, so it was not uncommon for us to get comments like, "So that's what you've been working on?"

Frankly, the pictures helped a lot of folks understand what we were doing. With one very notable exception, eyes would typically start to glaze over after viewing one or two slides filled with the equations we were using. The notable exception was Dr. Hans Mark. Fred and I were asked to brief him not long after he arrived at NASA Ames as the Center Director. I don't believe we got through more than the first slide or two before he stopped and congratulated us, then went on to explain to us what the results of our analysis were. We were absolutely amazed to find out that similar equations were used in nuclear physics (his specialty). According to Dr. Mark, he simply had to put the equations in our frame of reference and he could see the answer. I've heard of people being able to do things like that, but this was my first (and probably only) opportunity to witness someone actually able to do it.



Traditional takeoff.



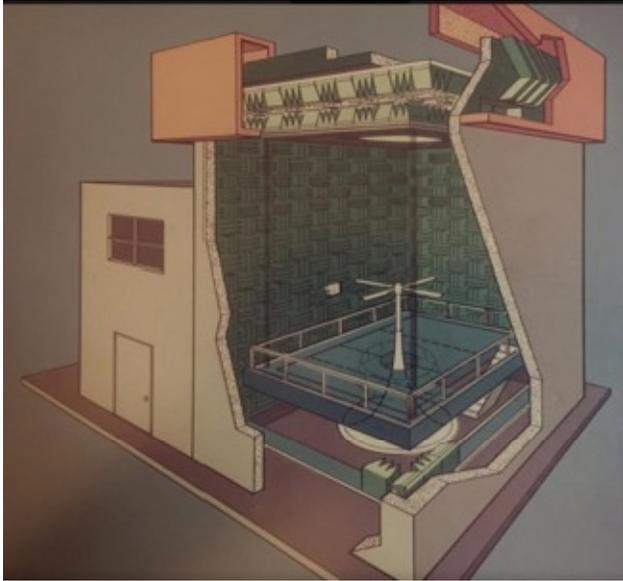
Optimized takeoff.

In 1974, Fred and I were presented with the Army R&D Achievement Award for our takeoff work that, eventually, was incorporated into new pilot training procedures.

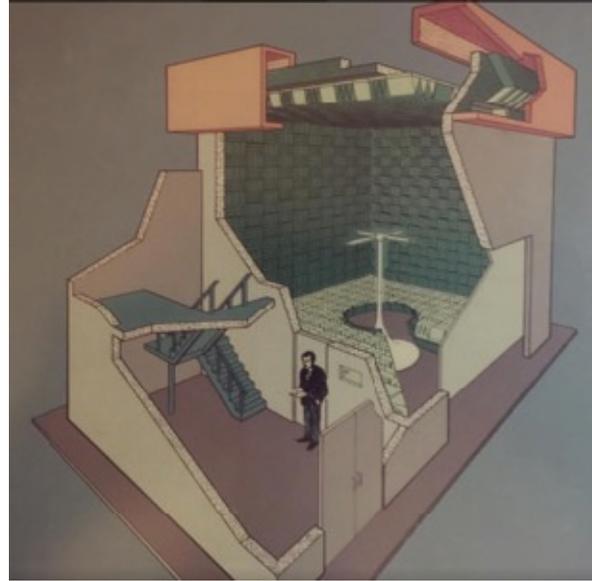
Impulsive noise (the whop-whop-whop sound that helicopters make) became a concern during the 1970s, for a couple of reasons. First, it made helicopters much more detectable in a combat environment and, second, in a noncombat environment helicopters were simply noisy and annoying to local civilian communities. Early acoustic work focused on blade-vortex interactions as a principal source of the noise. Blades in hover were the focus, because they were easier to model and easier to test. Experimental work at Ames (and around the country, as well as internationally) was conducted to locate the source of the noise by using triangulation techniques—using multiple microphones spread out near/around the rotor perimeter to “triangulate” on the location of the noise source. It was believed that if the source was known, then the environment could be better investigated. Hover test stands outdoors and in wind tunnels had significant issues because of unsteady winds (outdoors) and floor and wall interference with downwash from the rotors (indoors).

For the next couple of years our group (Fred, Don Boxwell, and I) worked at trying to understand the impulsive noise generated by a helicopter rotor. Fred was the creative thinker, Don the data analyst, and I got to do some more mathematical modeling. And occasionally we all got to do some fun things.

Fred theorized that, even though a helicopter rotor blade never reached speeds approaching Mach one, a shock wave was being developed on, and transmitted from, the blades under certain circumstances. In order to get better data, we decided that we needed to build an anechoic hover



Floor raised for rotor installation and maintenance.



Floor in the test position.

test facility. To make a long story short, we spent a summer designing the facility as depicted in the renderings above. NASA was very concerned about where hovering rotors might be tested, as they had recently experienced some problems, so we started out with a “safety compliant” concrete building with steel doors, lined with foam wedges to keep the noise from bouncing off the walls. The two biggest challenges were how to get the air into the room, and then out of the room, as cleanly as possible without introducing unwanted flow effects. The idea was that the rotor would suck the air in through the ceiling and it would be blown out through a hole in the test-cell floor, where it would exit the room through two steel doors underneath the floor. But we underestimated the complexity of getting the air in and out of the room. As a result, Don and I spent most of that summer trying to model the passage under the floor so that the air could flow out smoothly. We borrowed an available model tail rotor (that sized everything), built a plywood box (I think it was about 4 feet square) and then carved, sanded, and shaped a piece of fairly rigid green foam to represent the interior of the building. (It wouldn’t have been such a bad job if the temperature in the shop where we were working hadn’t been in the 90s, and if the “dust” from the foam hadn’t gotten into our clothes and made us absolutely miserable, but such was life for two, young, enthusiastic “engineers.”)

Bringing this facility into existence (not just the design of the floor, but the design of the entire facility), along with the entire fabrication and contracting process, was a great, practical engineering accomplishment for Don, Fred, and me. In the process, we learned a lot that summer about fluid flow, experimental techniques, and program/contract management. I don’t know that there was another lab in the world where we would have been permitted, let alone encouraged, to take on such a project. But that is the way it was with the AARL at Ames, with support from NASA staff and shops.

Fortunately, our efforts seem to have paid off. It was, and still is, a unique facility. Much good work has been done there over the years.

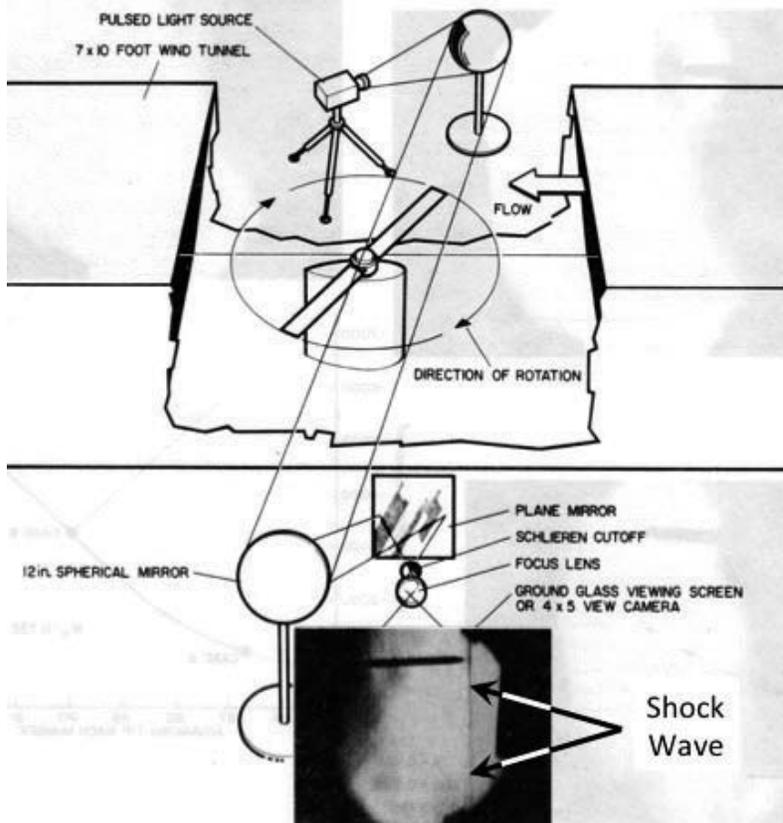
I suppose it's obvious we had to do wind tunnel work to try to correlate with what we were seeing in the hover facility.

As an aside, what Andy Morse generally encouraged, and Fred always insisted on, was that before we started a new project, we had to go to the NASA Ames Library and do a literature search. I never got over the fact that nearly everything we went there to look for had already been done by an engineer some 20 or 30 years before. There was hardly anything we came up with that was new or unique. What we brought to the effort were the computational capabilities that didn't exist when the original work was contemplated. The excellent NASA library was an invaluable source of related experience and work. Under

the tutelage of some outstanding technical managers, many young engineers truly learned a great deal when they did their homework in that library.

Now back to my story. Fred sent me to the library to do my research and asked me to develop a mathematical model of what was happening on the rotor. It was fun and I had a great time doing it. Back in those days, we were just learning about "animated" videos. Fred and I were able to make one showing a shock developing on the blade. I had to print innumerable plots showing incremental rotation of the rotor and the development of the shock wave. NASA then processed those prints for us to make it look like a movie (like flipping cards to make it look like an animated picture). We were confident we were on the right track and set up a wind tunnel test. Bell Helicopter had been using a Schlieren setup (mirrors and lenses) to try and visualize the flow over the rotor. We thought that might work for us, too. We set up cameras and mirrors in the tunnel and ran a test matrix that covered what we thought would be a range of appropriate variables. The only trouble was that by Friday afternoon, at the end of our test time in the tunnel, we still had not seen our shock wave. Fred decided we had time for just one more run, so we huddled and took our best guess at where the shock should be. And darned if it wasn't there. We could hardly believe it! It was as beautiful and bright and clear as it could possibly be. We were elated and went back to the office to share our story.

Before we went home that day, Fred and I talked about what we would do next. We were both excited about how well my little model had predicted what was happening, so I could not believe my ears when Fred suggested that on Monday he wanted me to start over and re-derive the



Schlieren setup in the 7- by 10-Foot Wind Tunnel.

equations in spherical coordinates, rather than the rectangular coordinates I'd used. I couldn't imagine why he wanted to do that or what he expected to learn. That was going to be months of work, solving a problem we had just proved we had solved! I had to go home and think about that—the thought really unsettled me.

Over the weekend I had what is often called an “epiphany.” As I thought about what Fred had suggested and what my reaction had been, I was reminded of the story Colonel Stapleton had told me nearly 10 years before, on my first day of work, about how, while shaving one morning, he realized that no matter how much he wanted to be an engineer, he just wasn't one.

What Fred wanted to do was the right thing for *him* to do...to pick up the puzzle and turn it around so that he could see it from a different perspective and, more than likely, gain some new insight in the process. That's the way Fred was. That's the way others in the lab were. I wanted to solve the problem and move on. *They* wanted to understand, learn, and explain. They had a different kind of patience than I did. They were unique in their abilities to dig deeper and to understand more completely. In the process of doing that, they laid the foundation to be built upon by the entire helicopter research community (including themselves!) for scores of years to come.

In the beginning, in those early days at Ames, I don't know if it would have been possible for others to have seen the vision that Cyril Stapleton, Paul Yaggy, and Andy Morse saw. Perhaps that might become known in the pages of this book. I wonder if such a collection of brilliant, creative minds, as those that found their way to Ames at that one moment in time, could ever come together again?

I would be remiss if I did not include something about the wonderful, committed, talented support people who were always there, always ready to take on something new, or always willing to stay an extra hour if needed. If I were to start listing names, I'm sure I would miss many of them, and I don't want to do that. Not all were Army employees; many of them were NASA folks. Enough can never be said about these ladies and gentlemen, and the contribution they made to the success of the lab. The participation of this combined pool came about because of the unique interagency Army-NASA Joint Agreement. The Joint Agreement made it possible for us to call on one another's skills, making our joint assets and capabilities much greater than either one of us could afford on our own. A more professional, talented, and dedicated staff would have been extremely difficult to find. The inventors of that concept, the unique inter-agency collaboration afforded by the Army-NASA Joint Agreement, were true visionaries.

Not all of us were meant to be engineers—especially not of the class mentioned here—but there are so many of us other folks who have been touched, encouraged, and inspired by those who were there. May this be a reminder of a very special group of people and of all they have done.

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Index

A

American Helicopter Society, 3
Ames Research Center, 1, 3, 4-6, 8
Army Aeronautical Activity, 1
Army Aeronautical Research Laboratory (AARL), 1, 3, 6
Army R&D Achievement Award, 5
Army Science Board, 2, 4
Army, 1-5, 8
Army-NASA Joint Agreement, 1, 8

B

Bell Helicopter, 7
Boxwell, Don, 5-6
Brigham Young University, 1

D

Durbin, Enoch, 2-4

G

Georgia Institute of Technology, 1

M

Mach, 5

Mark, Hans, 4

McCormick, Barney, 3

Morse, Andy, 2-3, 7-8

N

NASA, 1, 4, 6-8

Natick Soldier Systems Center (NSSC), 3

P

Princeton University, 2, 4

S

Schlieren, 7

Schmitz, Fred, 4-8

Stanford University, 2

Stapleton, Cyril, 1-2, 8

V

Velkoff, Hank, 3

Vietnam, 2-4

Y

Yaggy, Paul, 2-3, 8

Life is Good
William Warmbrodt
NASA

I was very fortunate.

I joined NASA Ames Research Center in the fall of 1978 as a research engineer in the Large-Scale Aerodynamics Branch under Mark Kelly. I was assigned to the rotorcraft section under the direct supervision of Wayne Johnson, a U.S. Army employee at the time. My office was in room 107, Building N-247. It was a two-person office. When Wayne Johnson looked up from his desk, he looked directly at me (remember, we didn't have computer terminals on our desks in those days; Wayne's desk was always clean and it still is).



William Warmbrodt,
1985.

I was very fortunate.

I considered a number of job offers prior to starting my career at NASA in the Bay Area. I attended the University of California at Los Angeles (UCLA) and in mid-1977 began seeking my first job. I flew to San Jose Airport, and coming off the stairwell from the plane, Mr. Kelly greeted me with a smile. I met lots of folks that day in 1977. I spent most of the time talking with members of the 40x80 rotorcraft section. Several would make lasting impacts on my career—Wayne Johnson, John McCloud, and Inderjit Chopra to name three. As a poor grad student, I got treated to lunch by Mr. Kelly at the Ames cafeteria. Hey, when you are a grad student, a free lunch is something that you remember. Let's see. Mr. Thomas Snyder and Dr. Leonard Roberts joined us for lunch. Okay. A Branch Chief, a Division Chief, and a Director together having lunch in the Ames cafeteria with one poor grad student. Must happen all the time here at Ames was my thinking.

I returned to UCLA and I really didn't make my decision for about 6 months. My thesis advisor, Professor Peretz Friedmann, didn't force a decision or direct me in any manner. With my UCLA office mates including Friedrich Straub (American Helicopter Society (AHS) Technical Fellow, 2015) and Richard Powers (first flight test of higher harmonic control in an OH-6 helicopter, 1983), I discussed the likelihood that in joining the rotorcraft section in the 40- by 80-Foot Wind Tunnel Branch, I would be arriving after Wayne Johnson departed. During my job interview visit to Ames in 1977, Wayne had made it very clear to me that my decision about NASA should in no way be dependent on his plans. Eventually, after Wayne saw how much I liked NASA, years later he joined NASA, just to be like me. And to cover future eventualities, I even asked Mr. Kelly in writing prior to accepting the NASA position if the opportunity to go to business school was a possibility (he directed me, in writing, to ask the Chief of Human Resources).

While still at UCLA and at the request of Dr. Friedmann, Dr. Dewey Hodges presented to the Los Angeles Chapter of the AHS. His presentation, which I thoroughly enjoyed, was on the dynamics of rotating beams. I recall Dick Powers telling everyone afterwards that the world needed more brilliant people like Dr. Hodges who draw knowledge from many related technical fields and are outstanding educators. We all agreed.

When I joined NASA Ames, I was surrounded by many brilliant people who drew knowledge from many related fields and were outstanding educators and mentors. In the large rotorcraft section, I was mentored by John McCloud on full-scale wind tunnel rotor testing. I was mentored by Alfred Lizak, formerly of Sikorsky Aircraft, on wind tunnel test stand development and operation. I was mentored by Inderjit Chopra on conducting shake tests, analyzing bearingless rotors, and how to trim trees in one's backyard (his yard, not mine). I was mentored by Marianne Mosher on rotor acoustics. There were indeed many others that made lifetime impacts on my career. Did I mention that I shared an office with Wayne Johnson?

I was very fortunate.

In the Large-Scale Aerodynamics Branch, the powered-lift and fixed-wing section was likewise large. The 40- by 80-Foot Wind Tunnel test operations staff was also large with many mechanics, technicians, electricians, programmers, and operators. Many became lifelong friends. Within 3 years, this group became their own Branch under Mr. Jerry Kirk. At this time (1981) the rotorcraft section remained with the powered-lift and fixed-wing section and became the Low Speed Aircraft Research Branch under Mr. David Hickey. Wayne Johnson became the Assistant Branch Chief. And all the organizations continued to grow.

In my case, a growing organization created opportunities. When Wayne Johnson became the Assistant Branch Chief, he decided to create three groups and he needed three group leaders. Woo-hoo! I became a group leader in 1981 for dynamics and acoustics. Marianne Mosher's mentoring became even more important, and we soon hired Cahit Kitaplioglu from Sikorsky Aircraft. With the groups, I began (and continue to this day) to learn how to be a people manager. Being Gloria Yamauchi, Kathy Alkire, and Marianne Mosher's Group Leader was an amazing experience for a young man during the days of the proposed Equal Rights Amendment. Add Stephen Jacklin and Randall Peterson and we were off on an exciting period for full-scale rotor research, including in-flight rotorcraft acoustic flight testing. My peers in the rotorcraft section now grew to include Mark Betzina, Robert Stroub, Alfred Lizak, Ray Piziali, Charles Smith, John Ballard, Fort Felker, Tom Norman, and Larry Young.

It cannot be overstated how important the San Francisco Bay Area Chapter (SFBAC) of the AHS was (and remains to this day) in creating and sustaining the Ames brother- and sisterhood of rotorcraft researchers and engineers. From its founding in 1973, the Chapter has provided frequent opportunities for us to gather, network, share knowledge, and sustain friendships and create new ones. Sometimes the stories were just plain juicy. I heard more than once from John McCloud how he reported to the AHS Headquarters that Bob Ormiston was serving on the AHS Dynamics Technical Committee without having become a member of the AHS. My first formal position with the Chapter was Membership Chairman. I recall the phone ringing in Wayne Johnson's office (have I mentioned that I shared an office with Wayne Johnson?) and Dr. Irv Statler called to ask me to run for the position of SFBAC Membership Chairman. Really? I didn't even know that Dr. Statler knew I existed. Lesson learned—being acknowledged by a senior leader, no matter what the occasion, is truly valued. I have seen many U.S. Army coins presented by senior Army general officers to fellow rotorcraft researchers at Ames, and I often recall that phone call.

Dr. Statler was Director, U.S. Army Aeromechanics Laboratory. For my personal professional growth and experience, the Lab in Building N-215 was the Ivory Tower of Rotorcraft

Aeromechanics Technology—both in theory development and in experimental testing. There's a term that may be a reach too far, but in this context, I'd say for rotor dynamics, aerodynamics, and acoustics with Robert Ormiston, Dewey Hodges, David Peters, Bill Bousman, Dave Sharpe, Jim McCroskey, Larry Carr, Frank Caradonna, Chee Tung, Yung Yu, and Fred Schmitz, this was The Greatest Generation. And Wayne Johnson and Ray Piziali were simply remote Army members being on the other side of De France Avenue. I believe their individual achievements, award recognitions, and legacies sustain this assertion.

I tried to learn with every visit to Building N-215. Walking the stairs to the office suites was an honor. Once upstairs, to the left, the Dynamics Division. To the right, the Fluid Mechanics Division. At the far end of the hallway to the right, the Director's Office (one day it would be the office for the Aeromechanics Branch Chief of the combined Army/NASA Rotorcraft Division). And symbolically, in the center of the building on the second floor—the infamous Building N-215 conference room where dynamicists and aerodynamicists would meet face-to-face and the battle would be engaged. Who was to blame for poor performance predictions? Why were blade loads always under predicted? Would computational fluid dynamics (CFD) ever, and I mean ever, become useful, and more important, practical? It was a wonderful time. It was a dangerous time. It was a glorious time.

And for me, there were the mid-1970s and early-1980s newcomers to the Army Lab. Again, my peers. My cohorts. My friends. Mike Rutkowski, Donald Kunz, Seth Dawson, Mike McNulty, Roger Strawn, Earl Duque, Jim Bader, and Tom Maier. The Greatest Generation begat a very honorable progeny indeed.

But I worked in the big wind tunnel. And how was one to learn about what the U.S. Army was up to in aeromechanics? The Army didn't have an annual NASA Headquarters RJ Review (RJ being the NASA Code for the two or three folks that oversaw NASA's entire rotorcraft investment portfolio at all three Centers) where every Ames rotorcraft area was reviewed. Ah, there was only one real answer to that question. I quickly learned that the best way to see what the Army Lab was up to was to attend the annual U.S.-France Memorandum of Understanding (MOU) meeting held at Ames (around the time of the AHS Annual Forum). Hearing all the aeromechanics activities, including emerging CFD methods and acoustics modeling and model rotor testing, was truly exciting. The breadth of work was amazing and here it was, all in one place. Not surprisingly, U.S. industry came to realize the same thing—this meeting was worthy of their attention. And, by the way, was the Army giving away nationally important technology? I recall an early-1980s meeting in the upstairs Building N-247 conference room, with our French collaborators/visitors present, and Mr. Andy Morse stood up to begin the meeting. It was glorious. Andy greeted the French visitors and the U.S. industry delegation. He mentioned the concern about technology transfer under the agreement first thing. And then he lit into the U.S. industry representatives as the primary, if not the only, perpetrators of U.S. technology transfer overseas. Andy knew what he believed, and he aggressively defended the organization before anyone even mentioned the subject. Lesson learned—the best defense is a good offense.

Now, not from this experience in the Building N-247 conference room but from many, many others during my career, I'd like to acknowledge that the most loved and respected first-level supervisor in rotorcraft at Ames, in my judgment, was and remains Andy Morse. My mentors in the Army and in NASA were always very reverent in their remembrance of Andy's impact on their research, their careers, and the legacy he left to the organization. It was an honor to know

Andy and work with his organization. I saw Andy's health declining during his retirement. At a time when Andy was bedridden, I called Tom Snyder and asked his permission to have the AHS SFBAC create a Lifetime Achievement Award. We needed it immediately. We established the new award that week so we could present the award to Andy at his home. Tom agreed, and together with Andy Kerr, we went to Andy's house. In his bedroom, we presented the very first AHS SFBAC Lifetime Achievement Award to Mr. H. Andrew Morse. There have been many proud and worthy recipients of this award over the past 25 years, but none more deserving. I was able to present and hand the award to Andy myself.

I am very fortunate and continue to be so.

Index

A

AHS Lifetime Achievement Award, 4
Alkire, Kathy, 2
American Helicopter Society (AHS), 1-4
Army Aeromechanics Laboratory, 2

B

Bader, Jim, 3
Ballard, John, 2
Betzina, Mark, 2
Bousman, Bill, 3
Building N-215, 2-3
Building N-247, 1, 3

C

Caradonna, Frank, 3
Carr, Larry, 3
Chopra, Inderjit, 1-2
computational fluid dynamics (CFD), 3

D

Dawson, Seth, 3
Duque, Earl, 3

F

Felker, Fort, 2
Friedmann, Peretz, 1

H

Hickey, David, 2
Hodges, Dewey, 1, 3

J

Jacklin, Stephen, 2
Johnson, Wayne, 1-3

K

Kelly, Mark, 1
Kerr, Andy, 4
Kirk, Jerry, 2
Kitaplioglu, Cahit, 2
Kunz, Donald, 3

L

Large-Scale Aerodynamics Branch, 1-2
Lizak, Alfred, 2
Low Speed Aircraft Research Branch, 2

M

Maier, Tom, 3
McCloud, John, 1-2
McCroskey, Jim, 3
McNulty, Mike, 3
Morse, Andy, 3-4
Mosher, Marianne, 2

N

NASA Ames 40- by 80-Foot Wind Tunnel, 2
Norman, Tom, 2

O

OH-6 helicopter, 1
Ormiston, Robert, 2-3

P

Peters, David, 3
Peterson, Randall, 2
Piziali, Ray, 2-3
Powers, Richard, 1

R

Roberts, Leonard, 1
Rutkowski, Mike, 3

S

Schmitz, Fred, 3
Sharpe, Dave, 3
Sikorsky Aircraft, 2
Smith, Charles, 2
Snyder, Thomas, 1, 4
Straub, Friedrich, 1
Strawn, Roger, 3
Stroub, Robert, 2

T

Tung, Chee, 3

U

U.S.-France Memorandum of Understanding (MOU),
3
University of California at Los Angeles (UCLA), 1

Y

Yamauchi, Gloria, 2
Young, Larry, 2
Yu, Yung, 3

A Korean in the U.S. Army

Yung Yu
Army

My first contact with the Army lab at Ames was in the summer of 1972 when I was a Ph.D. student at Stanford University researching the design of a helicopter blade for low noise. One day, I was told by my advisor, Professor Krishnamurty Karamcheti, that our research contract monitors were planning to visit the campus to figure out what I was doing. At that time, I did not know that the research project was funded by the Ames-Army Lab or what contract monitors were. I was just enjoying my research without worrying about project funding or the practical applications of my research. My professor told me that these contract monitors were very important because they held our “purse strings.” Furthermore, my professor told me that I had to impress them. I did not know how to impress them, but I decided to try with fancy mathematics. Stanford had (and still has) many famous professors in upper-level mathematics, and I was heavily involved with those courses.



Yung Yu

The day came when three contract monitors showed up on our Stanford campus. Naturally, I was very nervous in front of them because of my limited ability with the English language (I am from Korea), my lack of experience in public speaking, and my poor knowledge of practical helicopter theory in general. After the meeting, I was sure I had not made a good impression on the visitors. I had tried to explain how wonderful mathematics work and how with mathematics I could solve any helicopter problem, including noise problems. I was just a naïve and innocent graduate student from Korea.

After the meeting, my professor told me that I had totally confused the contract monitors and that he had to translate my presentation in more practical and physical terms for them. Now I was worried about the possibility of the end of our research funds. But, fortunately, our contract was extended for another year, and I got my Ph.D. in 1974. The contract monitors were Fred Schmitz, Rande Vause, and Don Boxwell of the Ames Directorate of the U.S. Army Air Mobility R&D Laboratory (AMRDL).

This is how my relationship with the Ames Directorate started in 1972. I did not know at that time that this relationship would continue for 30-some years until I retired from the government in 2005.

After I got my Ph.D. in 1974, my wife declared that she would not move; she told me to just get a job in the Bay Area. I had a permanent resident visa at that time. I ended up working at NASA Ames Research Center in the 40- by 80-Foot Wind Tunnel Branch as a National Research Council post-doctorate for 2 years, and I also became a naturalized citizen. One day, as I was working in the office, Fred Schmitz stopped and asked me whether I was interested in a job at his office. Since one of his group, Rande Vause, was moving to AMRDL headquarters, there would be a vacancy in the Ames Directorate.

Finally, in 1976, I became a member of the acoustic group, where Fred Schmitz was a group leader and Don Boxwell was responsible for experiments. Andy Morse was our Chief of the Army Aeronautical Research Group, and Dr. Irv Statler was our Director of the Ames Directorate. My impression was that Dr. Statler was a kind of fatherly figure, strongly protecting us from outside interference; Andy Morse was a kind of motherly figure who was caring, friendly, a good listener, and always willing to help; and Fred Schmitz was like an older brother who knew what to do to acquire proper funding and obtain strong support from headquarters. With this environment, I felt extremely lucky to be a member of the organization, and I planned to just carry out research work as directed by Fred Schmitz. This is how I started my life-long career at the Army lab in October 1976. This wonderful arrangement lasted for about 10 years, until Dr. Statler left for Paris, Andy Morse retired, and Fred Schmitz moved to NASA.

My job was to develop a theoretical model to predict helicopter noise, based on an existing model developed earlier by the group. I had to add on the effect of nonlinear terms, called quadrupoles, in the Ffowcs Williams–Hawkings equation. Meanwhile, I was told to continue a wind tunnel test with the Schlieren technique, which Rande Vause started before he left. I did not know anything about wind tunnel testing or about the Schlieren technique. During my graduate work, I had never been involved with any experiments. After all, I considered myself to be a theoretician playing with higher mathematics. With many ups and downs, I learned a great deal from experiments; I learned to appreciate their importance, and I finally understood why we needed them. It was a long journey for an innocent beginner and theoretician to become a mature and appreciative helicopter man. I strongly believe that this change was possible because of the solid foundation of basic research that was laid down—the environment, the passions of management and researchers, and the proper manpower. All I had to do was get along with everyone, follow the trends, and do the research I enjoyed without causing any trouble. I felt extremely fortunate and appreciated the opportunity, particularly as a foreign-born citizen with a strange accent and sometimes unseasoned vocabulary (which Bob Ormiston constantly tried to fix), to work in a stimulating environment with supportive management in the special organization established under the Army-NASA Joint Agreement.

There were a few interesting events during my career. I worked in the 7- by 10-Foot Wind Tunnel to take Schlieren pictures of the flow around a rotor-blade tip at transonic speeds. When I was just outside of the wind tunnel window, I did not realize how fast the blade tip was rotating and what might happen if something went wrong. I could not see anything inside of the tunnel from the window outside, so I felt relatively safe until Fred Schmitz cautioned me about the transonic speed and explained what could happen if the blade decided to take off while it was rotating at that speed. From then on, I was quite scared and I hurried up to finish the Schlieren test.

But all these wonderful Schlieren pictures had one limitation: they were two-dimensional images of three-dimensional (3-D) events. So, we decided to develop a new technique for taking 3-D pictures of rotor transonic flows. I had taken a holography class at Stanford University taught by Dr. Charles Vest, a professor visiting from the University of Michigan for a year, in which holography and holographic interferometry were described as a potential technique for 3-D measurements. So, I contacted Dr. Vest at the University of Michigan and he was more than happy to talk about the applications of holography and holographic interferometry to the problem of complex rotor transonic flow. With Dr. Vest's wonderful help and excellent support from our

management, we developed an innovative experimental technique for visualizing and quantifying 3-D rotor flows in transonic speed. Our team was extremely excited about it.

Subsequently, the Pentagon big-boss and the Commanding General in our organization scheduled a visit to our lab. Naturally our Director, Dr. Irv Statler, wanted to make sure we made a good impression on them with the 3-D flow visualization, which had not been accomplished before. We made a very big point of this first 3-D flow picture and were eager to show off our new capability. However, showing visitors the 3-D flow in a test setup was extremely tricky. The test room had to be completely dark, and only one person at a time could look through a tiny pinhole. You had to be extremely careful about where you were looking through that very small opening while in a completely dark room. You could very easily miss what there was to see if you were either not in the right position or not looking in the right direction. Since the room was completely dark, we had no idea whether the visitors were doing it right or not. Anyway, we told them what to look for and how to see it. Each visitor understood the procedures, got in position, and looked through a small pinhole one by one. We asked each whether he saw the 3-D transonic flow field. The answer from both visitors was yes, and they agreed it was very good. So, we were relieved and thought we had been successful. About an hour later, we heard a strange story. Someone nearby overheard a conversation between the visitors outside the dark room. They asked each other whether they saw it and one said, "I did not see anything, except dark," and the other visitor replied, "Me either." I realized that the visitors, our big bosses, were extremely generous and did not want to disappoint us. I was overwhelmed to have such wonderful bosses.

As a follow-up, Professor Charles Vest, the very nice, excellent researcher who played a major role in setting up the holographic interferometry system in our anechoic hover chamber, was a young full professor in the Department of Mechanical Engineering at the University of Michigan. About 3 or 4 years later, he became an Associate Dean of Engineering, and a few years after that, he became a provost, the second highest position at the university. Sometime later, I read in a newspaper article that Professor Charles Vest had gone on to become the President of Massachusetts Institute of Technology.

During my years at the lab, we had wonderful working relationships with France, Germany, and The Netherlands under Memorandums of Understanding (MOUs) with these countries. Fred Schmitz was a leader in this international cooperative effort in rotor acoustics research and was extremely successful in my opinion. Since each organization had unique capabilities, working together with the same goals, and a willingness to share and help each other, was a wonderful way to advance the basic technology. The cooperation, relationships formed, and willingness to sacrifice, if necessary, under the MOUs, was excellent!

Our collaborative relationship with NASA was also extremely important. Our lab was a rather small organization, so we relied on help from support personnel and infrastructures at Ames Research Center, such as the machine shop, procurement offices, publication office, travel office, etc. We were very lucky to have such wonderful people in those support groups to help us in any way they could. For example, when I planned to have a wind tunnel test with a rotor, I had no idea how to design a rotor blade. But when I went to the machine shop with a rough hand-drawn sketch, the foreman asked me a few questions and figured out what I was planning to do. Actually, he knew better than I did what I was trying to do. He was very understanding and extremely helpful. It was simply amazing! I was so impressed with his professional manner and workmanship. The same thing happened at other offices. I will always appreciate their smiles

and willingness to help us with any difficult problem, and even with some almost impossible tasks. Because of their professionalism at NASA, we engineers could concentrate our own research much better. What a wonderful Army-NASA collaboration that was!

Another example of teamwork involved our developing a nonlinear acoustic theory with Professor Morris Isom from New York Polytechnic University. Professor Isom was a unique individual with exceptional knowledge in mathematics, transonic aerodynamics, and nonlinear acoustics. He was one of the very few people in the world who understood the field of nonlinear aerodynamics, nonlinear acoustic theory, and fancy mathematics. Professor Isom was a pure theoretician, and we always kidded him by asking about the physical meaning of that damn mathematical term, which he hated immensely. With Professor Isom as a consultant, Don Boxwell was the experimental guy who had patience and extensive knowledge, I was a small theoretician compared to Professor Isom, and Fred Schmitz was the group leader. One day we, as a group, found a very exciting phenomenon, later called delocalization in rotor acoustics. This term came up during lunch at Ames one afternoon. As usual, about a half dozen engineers would get together to eat lunch in the Ames cafeteria, including Bob Ormiston, Jim McCroskey, Frank Caradonna, Fred Schmitz, Chee Tung, myself, and others. We talked about what we had just found, but without a clear understanding of it, or any name for it, either. We didn't know what to call this strange, but exciting phenomenon. Then, suddenly, Frank Caradonna suggested calling it *delocalization*, which I assumed meant *to open up outwardly from a local, confined area*. We all agreed that was an excellent name, and that is how we invented the "delocalization" of helicopter acoustics.

Then we tried to explain this exciting phenomenon in terms of its physical meaning. Professor Isom had developed the nonlinear aerodynamic theory in a rotating frame, our team had developed a nonlinear acoustic model with this nonlinear theory, and Don Boxwell had clearly demonstrated experimental data from our anechoic chamber. Theory and experiment were all well prepared to support the "delocalization" phenomenon.

One day our Director, Dr. Irv Statler, stopped by our office, and we spent some time explaining how a locally confined transonic field around the blade tip expanded to the far field and how nonlinear acoustic waves could propagate to a far field as a blade tip Mach number slightly increased. Dr. Statler shook his head and told us, "I don't see any propagation to a far field from the blade tip when this delocalization occurs." We all were disappointed and tried to figure out how to explain this strange phenomenon in a better, clearer way. We thought, if our Director could not understand our point, we must be doing something wrong. So, back to square one, and we all developed new explanations with new fancy color plots. Shortly thereafter, in 1985, Dr. Statler left to become Director of the Advisory Group for Aerospace R&D (AGARD) in Paris. So, unfortunately, we never got another chance to illustrate the delocalization phenomenon to our boss using our new explanations and fancy color plots.

My career at the Ames-Army Lab continued for many more years and encompassed several more adventures in rotorcraft research. In 1987, I succeeded Dr. Fred Schmitz as Chief of the Fluid Mechanics Division of the Aeroflightdynamics Directorate until formation of the integrated Army-NASA Rotorcraft Division in 1997. In 1998 I joined the National Rotorcraft Technology Center at Ames as Chief Scientist and retired from the Army after 30 years in government service in 2005. I then joined Konkuk University, Seoul Korea, as a chaired professor and eventually retired in 2012 to smell the trees and flowers (for the first time in my adult life) in my

backyard. I will always remember deep in my heart my intellect-stimulating experiences and wonderful friendships/cooperation in the unique environment at Ames Research Center.

Index

A

Advisory Group for Aerospace R&D (AGARD), 4
Ames, 1, 3-4
Ames Directorate, 1
Army Air Mobility R&D Laboratory (AMRDL), 1
Army-NASA Joint Agreement, 2

B

Boxwell, Don, 1, 4

C

Caradonna, Frank, 4

F

Ffowcs Williams–Hawkings equation, 2

I

Isom, Morris, 3

K

Karamcheti, Krishnamurty, 1
Konkuk University, 4

M

McCroskey, Jim, 4

Memorandum of Understanding (MOU), 3

Morse, Andy, 1-2

N

National Research Council, 1

New York Polytechnic University, 3

O

Ormiston, Bob, 2, 4

S

Schlieren, 2

Schmitz, Fred, 1-4

Stanford University, 1-2

Statler, Irv, 1-2, 4

T

Tung, Chee, 4

V

Vause, Rande, 1-2

Vest, Charles, 2-3

Epilogue

Barry R. Lakinsmith
Army

My introduction to the Ames Army-NASA environment occurred in 1988, just after the period of focus and incredible accomplishment chronicled in these chapters. In doing so I had the good fortune of working for and with many of the leaders and technical luminaries that made Ames rotorcraft almost magical—Statler, Kerr, Ormiston, Schmitz, Tischler, Blanken, Gossett, Warmbrodt, Johnson, Franklin, Aiken, and Hindson are salient in that long list.

I directly benefited from the culture, facilities, and critical mass of expertise these pioneers assembled. And like several you have read about, I too had a reluctant path to positions of management. But my desire was strong to help preserve something that I knew was very special, even if it was often hard to put my finger on exactly what made it unique. When our senior Army leadership retired in 2006, I knew it was time for me to step up. Together with John Davis, Dick Spivey, Tom Maier, and Jay Fletcher, we have done our best to maintain the essential elements of an R&D organization that Wayne Johnson describes in his chapter as “...a result of the long collaboration between Army and NASA, and in my judgment the strongest rotorcraft research group ever.”

The foregoing chapters concentrated on Ames rotorcraft people, events, and experiences from 1965 to 1985. During this period there were some major Department of Defense (DoD) rotorcraft development activities, including the XV-15, UH-60, and AH-64 that the Ames Army-NASA community directly supported. However, I assert that the most important and longstanding accomplishments from Army-NASA researchers during this era involved improving our nation’s fundamental understanding of helicopter physics. These complex physics involved stall and compressibility, rotor wakes (and their associated impact on acoustics, aerodynamics, and flight control), and structural dynamics. Importantly, this improved understanding also involved rotorcraft human factors—concepts such as pilot workload, cueing, and human-automation interaction—which had few governing physical equations. This improved knowledge base directly led to a myriad of associated rotorcraft design and analysis tools from the Ames community. These tools enabled the *application* of that improved physical understanding well beyond the confines of Ames, and took the form of comprehensive analyses, acoustic prediction codes, computational fluid dynamics (CFD), real-time simulations, experimental measurement techniques, and system identification methods. Critically, these tools also included validation data and specifications (e.g., Aeronautical Design Standard-33 (ADS-33)) to wisely guide the next generation of rotorcraft and modifications to those already in use.

Not surprisingly, most of the post-1988 activities I was associated with had their roots in the technical curiosity and acumen of the nearly 40 “Figures of Merit” who wrote these chapters and established the roots of this collaboration. We continued fundamental inquiries in dynamic stall, aeroelasticity, and active rotors, frequently under the auspices of foreign agreements birthed decades before the current investigators were born. We continued to add to the knowledge base of rotorcraft handling qualities in ADS-33, with a version F in development today. We carried on the tradition of in-flight simulation started by Jack Franklin, Vic Lebacqz, and Ed Aiken in the UH-1 and CH-47 to the JUH-60A Rotorcraft Aircrew Systems Concepts Airborne Laboratory

(RASCAL), which, thanks to Bill Hindson, Jay Fletcher, Dave Arterburn, Ernie Morales, and Hossein Mansur, has seen widespread application since its initial qualification in 2001. Frequency domain system identification via the graphical user interface-based tool CIFER[®] and the aforementioned handling-qualities criteria directly led to Tischler's development of CONDUIT[®]—a powerful tool for flight control design and analysis.

Fledgling efforts in unmanned aerial vehicles (UAVs), flight guidance, and human factors started with Terry Gossett, Ed Aiken, Jim Hartzell, and Sandra Hart, and led to immense investment and payoff in software for autonomy by Matt Whalley and his team, brown-out cueing and symbology by Zoltan Szobozsly, and emerging manned-unmanned teaming methods and immersive interfaces. The longstanding Army-NASA collaboration in vertical takeoff and landing (VTOL) conceptual design gave rise to Wayne Johnson's development of the NASA Design and Analysis of Rotorcraft (NDARC) code. This software was effectively debugged, extended, and validated through extensive application by the Army's Concept Design and Assessment team, encouraged by the indefatigable Mike Scully.

Many of the authors herein pioneered major advances in aeromechanics computational methods for both comprehensive analyses such as 2GCHAS/RCAS (Second Generation Comprehensive Helicopter Analysis System/Rotorcraft Comprehensive Analysis System) and CAMRAD (Comprehensive Analytical Method for Rotorcraft Aerodynamics and Dynamics) based on conventional aerodynamics, and later with CFD. The heady CFD activity in the 1970s was followed by incredible computational hardware advances and parallelization, allowing the linkage of CFD aerodynamic predictions directly to simulations of rotor structural deformation and dynamics. The Army-NASA collaboration of Tung, Caradonna, and Johnson produced the first coupled CFD-computational structural dynamics (CSD) analysis in 1986. This coupled CFD-CSD arena was pursued during the late 1980s and 1990s until it emerged as one of Ames most visible and significant activities in the early 2000s. The first step, again initiated during the incredible period of rotorcraft R&D you have read about, was acquiring an immense body of accurate and detailed full-scale rotor experimental data during the UH-60A NASA-Army Airloads Flight Test program at Ames in the early 1990s. This large multi-agency team, led by Kufeld (NASA) and Bousman (Army), amassed a database that became the "gold standard" for validation of CFD-CSD predictions and certainly has to be considered one of the most valuable contributions of the Army-NASA collaboration. The next important step came from the UH-60A Airloads Workshop originated in 2001 by Ames collaborators under the National Rotorcraft Technology Consortium (NRTC). This pivotal series of workshops continued for 17 years and focused the efforts of technical community experts on the UH-60 flight test data. In 2003, Potsdam, Yeo, and Johnson achieved the first fully successful CFD-CSD solutions and thus launched today's rotorcraft CFD-CSD revolution. It led directly to the HPC Institute for Advanced Rotorcraft Modeling and Simulation (HI-ARMS) and Computational Research and Engineering Acquisition Tools and Environments—Air Vehicles (CREATE-AV) programs ably led by Potsdam, Meakin, and Strawn, all protégés of those chronicled in this memoir (see chapters by Kerr, McCroskey, and Ormiston). Their associated design and analysis software, Helios, is being adopted throughout the rotorcraft industry and has proven pivotal in the development of the Army's Joint Multi-Role Technology Demonstrator (JMR-TD) platforms, now entering flight test.

In late 2015, together with NASA's Susan Gorton and Bill Warmbrodt, I helped organize and celebrate the 50th anniversary of the Army-NASA rotorcraft collaboration in Washington D.C.

We of course gathered senior officials from the Army and NASA, but we also had representatives from the rotorcraft industry and academe (particularly leaders from the current Vertical Lift Research Centers of Excellence at Penn State, Georgia Tech, and the University of Maryland), as well as various benefiting partners from the Navy, Air Force, Federal Aviation Authority, and Office of the Secretary of Defense (OSD). It was a rare opportunity to look back at the highlights from five decades of collaboration and hear about the impact of that work in industry and in our fielded systems. Since almost all of those lauded accomplishments and awards were made, or at least started, by the authors of these chapters, I want to review some of them here, so that the reader can appreciate the magnitude and scope of output from this most special collaboration:

- Produced 14 AIAA Fellows and 75-plus Associate Fellows.
- Produced 22 American Helicopter Society (AHS) Technical Fellows and 16 Honorary Fellows.
- Executed more than \$3B of U.S. Government aeronautical research, development, test and evaluation.
- Incorporated the research efforts of collaborators from 12 foreign nations.
- Created, and supported for over 25 years, university Rotorcraft/Vertical Lift Centers of Excellence and industry partnerships (NRTC, Vertical Lift Consortium).
- Enabled 204 joint author (Army and NASA) technical publications, 2005–2015 (likely a similar rate for preceding decades).
- Won 45 major AHS International awards.
- Won seven Lichten awards (best first AHS paper) and six Gessow awards (best of AHS Annual Forum).
- Produced 11 AHS Nikolsky Lectureships honoring a highly distinguished career in vertical flight aircraft research and development.
- Contributed to two Collier Trophies—1983 for AH-64 Apache development and 1990 for V-22 Osprey development.

If your yardstick for this collaboration is operational impact, perhaps one only needs to refer to the foreword by Hans Mark and the chapters by Maisel, Dugan, and Statler describing the XV-15. The NASA/Army/Bell development of the tiltrotor is something that most of the Ames rotorcraft community described in this memoir can, and should, feel deeply proud about. And the tiltrotor's reliance on Ames did not end with the last XV-15 flight at Ames in 1991 or even when the last remaining aircraft went to the Smithsonian National Air and Space Museum at Udvar-Hazy Center at Dulles Airport. The Ames tiltrotor connection continues, even today, with the V-22, a fielded weapon system for which Ames researchers performed a wide range of tiltrotor investigations and analyses. This Ames Army-NASA support for the V-22 included engineers and pilots for accident investigations; the 2001 Tilt Rotor Aeromechanics Phenomena Independent Assessment Panel, chaired by then NASA Ames Center Director Dr. Henry McDonald; and multiple wind tunnel investigations of vortex ring state, acoustics, whirl flutter, formation flight, and carrier deck operations. Our CFD codes have been used to assess tiltrotor brownout, the hover fountain effect, and fuel dump provisions on the Air Force CV-22. Our flight control experts, working with Naval Air Systems Command (NAVAIR), have supported the tailoring of ADS-33 handling-qualities requirements to V-22 performance and maneuvers, as

well as the application of envelope limiting and advanced flight control concepts enabled by its fly-by-wire control system. Finally, armed with a wealth of flight-validated tiltrotor design and performance data starting with the XV-15, the Ames concept design and analysis team, lead by Scully, Johnson, Sinsay, and Silva, has expertly guided the development of the Army's advanced tiltrotor configurations as part of the JMR, Future Vertical Lift, and Joint Heavy Lift programs.

Very few of the brilliant people and accomplishments you have read about in these chapters would have arisen without the associated research facilities that attracted them and that enabled their scientific inquiry. Most of Ames unique rotorcraft research facilities, such as the National Full-scale Aerodynamic Complex (NFAC) and the Vertical Motion Simulator (VMS), together with a myriad of new and novel measurement techniques and data acquisition systems, were built during the productive era chronicled herein. To remain viable, they had to be consistently used, maintained, and improved. This was a task that Army and NASA researchers eagerly championed. Sadly, these facilities also had to regularly be "defended" in various non-advocate review teams and financial analyses, both typically charged with finding opportunities for cost savings during the zero-sum budget environment prevalent throughout the 1990s and new millennium.

In the early 2000s, the productive Ames VMS was threatened with closure. Despite its unique motion cueing capability, Army use of the VMS was significantly reduced given their increasing use of flight research. NASA's shuttle program was winding down, and the regular VMS use for shuttle landing emergency procedure training would soon end. Ames growing interest in airspace management vice dynamics and control of individual aircraft placed a premium on other types of simulation. In an era of full-cost accounting, it grew increasingly difficult to support the VMS capability, despite researcher interest. The Army went from performing two or three simulations a year in the facility (and paying only for test-unique development), to paying "full-cost" for one entry every 18 months. From FY03 to FY06 the VMS operated solely on direct customer funding, which created a significant "spike" in what researchers had to bear for using the facility. To make such costs more reasonable, NASA Headquarters created the Shared Capability Asset Program (SCAP) in 2007, enabling the capitalization of unique aeronautical research facilities. If the VMS had not come under that umbrella (and later the Aeronautics Test program) it would almost assuredly be closed today.

Interestingly, the Army rotorcraft community just had one of their most visible and important VMS activities in March 2018—a Ft. Rucker operational concept team assessment of the flight control and handling qualities afforded by the two competing JMR industry configurations funded for flight test. One is an advanced tiltrotor and the other an advanced lift-offset compound. This month-long VMS entry drew not only aviators and program officials, but multiple Army General Officers, including Brigadier General Thomas Todd, Program Executive Officer, Aviation, and Brigadier General Walter Rugen, head of the Army's new Cross Functional Team for Future Vertical Lift. As noted in the preceding chapters, maintaining awareness and interest in the capability of Ames Army and NASA scientists and facilities among DoD Flag Officers is an important and never-ending task.

NASA's NFAC, facing similar financial scrutiny during an era of full-cost accounting, did not fare as well as the VMS. In January 2003 NASA abruptly closed the facility. Were it not for the people you have read about in this memoir—particularly Bill Warmbrodt and Andy Kerr, with the assistance of Rhett Flater and the AHS—this amazing facility, the world's largest wind

tunnel complex, would almost certainly be gone. Bill and Andy, armed with passion and technical rationale from the entire Ames rotorcraft community, began a campaign with industry, academe, the Army, and OSD to restore the capability. That 2-year effort resulted in OSD directing the Air Force Arnold Engineering Development Center (AEDC) to reopen this full-scale aerodynamic testing capability, largely for the benefit of the Army and their planned rotorcraft development. AEDC executed its mission exceedingly well and helped keep Ames, rotorcraft, and the NFAC once again a potent combination. Through a very competent series of United States Air Force (USAF) Directors, starting with Colonel Vince Albert and most recently Mr. Scott Waltermire, the NFAC restarted operations in 2008 and has since seen a decade of productive use involving the final UH-60 Airloads experiment, advanced vertical/short takeoff and landing (V/STOL) configuration assessment, active rotors, wind turbines, parachutes, atmospheric decelerators, and flow control entries. Until a tunnel incident in June 2017, both test sections of the NFAC were booked 2 years in advance. The aerospace industry, every DoD service, and the Defense Advanced Research Projects Agency (DARPA) have all been direct beneficiaries of the USAF's restoration and commitment to the NFAC. Interestingly, perhaps no agency has benefited more than NASA from the NFAC restoration. As of 2018, they have been associated with the greatest number of entries since reactivation.

Flight research facilities at Ames have also had their share of tumult. As part of the “better, faster, cheaper” Dan Goldin era, NASA moved to consolidate their flight testing at Dryden Research Center (now Armstrong) in southern California. Despite various reports and testimony decrying the lack of cost savings and lost capability associated with the plan, Ames forged ahead and sent their aircraft south in late 1996. Langley and Glenn, also slated to consolidate their flight research aircraft at the same time, managed to engage their congressional principals as part of the budgetary process and avoid a similar fate.

NASA's decision to terminate flight operations at Ames forced the Army and local rotorcraft community to consider how to accomplish their research objectives, heretofore performed on Army-owned aircraft bailed to NASA that were unaffected by the consolidation edict. In 1997, the Army at Ames elected to stand up their own flight operation, using their associated helicopters (two UH-60s, the NAH-1S Cobra, and OH-58C, and subsequently two Yamaha RMAX UAVs), military test pilots, airworthiness/engineering assets, and Army Regulation 95-1 flight provisions. The first Chief of the Army Flight Projects Office at Ames was then Major Chris Sullivan. The initial years of Army leadership of Ames rotorcraft operations deeply benefited from the continued participation of NASA test pilots (George Tucker, Bill Hindson, and Munro Dearing), airworthiness oversight (Geary Tiffany and Warren Hall), and NASA Rotorcraft program funding for associated “dual use” flight testing.

The Army flight activity at Ames had an amazing two decades of incident-free operations, averaging 300 flight hours per year. This activity spanned a broad spectrum, from fundamental research in helicopter acoustics, autonomy, slung load dynamics, and pilotage symbology, to major customer-funded efforts in fly-by-wire flight control and risk reduction (UH-60M Upgrade, CH-53K), and Degraded Visual Environment Mitigation (DARPA Sandblaster, USAF 3DLZ, and Army DVE-Mitigation).

Sadly, as part of the creation of the nascent Aviation Development Directorate, based at Redstone Arsenal, Alabama, the Army Aeroflightdynamics Directorate (AFDD) Flight Projects Branch at Ames was directed to consolidate operations at the Ft. Eustis, Virginia, Aviation

Applied Technology Directorate (AATD) during 2017. The move was again motivated by perceived “cost efficiencies” associated with a single location for flight testing. This decision failed to fully consider the difficulty and overhead associated with sponsoring flight tests 2,600 miles away, as well as the impact to productivity and morale of the Ames-Army Lab by losing their local active duty Army officers, experimental test pilots, and flight test engineers, along with their unique and highly modified aircraft. Five Army civilian positions associated with the aircraft will be transferred to Virginia, and the two AFDD-assigned active duty U.S. Army officers/experimental test pilots are expected to follow. These Army Major and Lieutenant Colonel slots have been part of the Ames rotorcraft community for almost 50 years. In addition to being exemplary pilots for our aircraft and simulators, they served an equally important role as representatives of the Army customer, liaisons to the operational Army, and leaders within the local organization. Several of these Army-Ames officer alumni went on to subsequent positions as full Colonels, often Program Managers (PMs) for development or fielded weapon systems, Test Center Commanders, or Directors. Armed with the familiarity of Ames facilities and subject matter experts amassed during their assignment, these now senior officers regularly sought Ames rotorcraft personnel and tools in their follow-on positions in the Army. That era may be ending as well. It will be exceedingly hard for the rotorcraft community at Ames to maintain their degree of research relevance and effectively transition technology when representations of their pilot-using community and their platforms are no longer in proximity.

From 2007–2017 I was the Army Director or Deputy Director of the vaunted research group you have read about in these chapters. My fundamental aim was to preserve or enhance the amazing environment I inherited. Sadly, my inability to successfully argue against and forestall this flight research consolidation was a failure to make good on my original objective in assuming leadership responsibility. In part, it directly led to my decision to retire from federal service in early 2018.

Notwithstanding this appreciable organizational setback, I have tried to be unbiased in using today’s vantage point to look rearward and introspect about what has remained and what has changed about Ames rotorcraft. I am happy to report that many of the ingredients that gave rise to the “golden era” described in this unique memoir still exist. Long technical arguments in the hallways, debates on alternative research approaches, and rigorous scrutiny and rehearsal for technical papers and presentations very much remain a part of Ames rotorcraft into 2018. We still enjoy productive collaborations with other DoD organizations, the North Atlantic Treaty Organization (NATO), and the French, German, and Israeli research establishments that sage managers set up 30-plus years ago. We still exercise many of the same unique and powerful facilities—the NFAC, the VMS, the 7- x 10-Foot Wind Tunnel, and the now Virginia-based RASCAL aircraft—that the authors herein created, modified, maintained, and used in their groundbreaking research. Happily, I can also report that we have added a few more jewels to that crown of accomplishment—notably an award winning 60-work-year investment in Autonomous Rotorcraft led by Matt Whalley, novel UAV interface work by Ernesto Moralez’ human factors team, incredible insight into aerodynamic flows by experimental contributions from Manny Ramasamy, Mahendra Bhagwat, Preston Martin, and Austin Overmeyer, and the widely acclaimed Helios coupled CFD-CSD software, developed by an incredible “world class” team under Roger Strawn (our latest AHS technical fellow).

We still have some of the very best in our specialized field. Through their reputation and the remaining Army-NASA-USAF Ames facilities, we often can land that promising intern or post-

doc from Maryland or Stanford or Georgia Tech or Penn State. And I can attest that I still witness genuine kindness of people when on-boarding new employees or hosting summer interns, the "...personal touches that made the Ames-Army Lab a great place," as Dave Peters remarked.

But the environment that "allowed us to do our research unbridled by needless bureaucracy" as Professor Peters and Schmitz noted, is more rare today. The ubiquitous digital network age and its enabling ability for remote parties to scrutinize expenditures, question capital asset utilization, monitor mandatory training compliance, and far too often to ask "What have you done for me lately?" takes a constant toll on the workforce. The Ames-Army Lab still has the ability for bottom-up, investigator-driven inquiry into fundamental physics, but today's Independent Laboratory In-house Research (ILIR) budget is less than half of what it was a decade ago, and purportedly on its way to zero. In the place of basic research is fairly robust near-term applied research funding, tied to demonstrations with integrated "technology effort objectives" emanating from Headquarters, Department of the Army (HQDA) or "warfighter technical challenges" from the Army user community. These marching orders are metrics for research (and funding) produced by well-meaning Science and Technology (S&T) PMs who have to answer to the incessant scrutiny of their superiors. But rarely have these modern era S&T managers grown up in the trenches with a personal appreciation for the technical challenges associated with complex air vehicle physics. Particularly concerning is that many senior NASA and DoD S&T leaders hold a perception that investment in rotorcraft is chasing after diminishing returns compared to that possible in the Bio-Nano-Info area.

Despite the potent facilities at Ames and the stellar reputation of the Army-NASA rotorcraft researchers, I have found it very hard to attract and maintain a core of scientists and engineers that give me the confidence that 20 years hence there will be a Volume Two of this memoir. Our Army lab of about 80 civil servants presently has over a dozen vacancies, only partly attributable to the recent government hiring freeze. Our demographics reflect that bi-modal age distribution all too often described as the "death spiral" of a core competency if left unattended. The lab has lost several mid-career employees to the lure of personal air vehicles, flexible working environments, and unprecedented compensation to be found just outside the Ames campus in the Silicon Valley infotech juggernaut. And just before my retirement, I had to say "farewell" to one of our best and brightest Ph.D.s in flight control, lured away by cheaper housing, a more academic environment, and those omnipresent "family considerations," which somehow are attenuated when employees have a deeply held sense that they are actually living in the type of era described in this memoir.

But scientists and engineers are foremost adaptive, and there still remain at Ames some of the entrepreneurial mavericks cultivated by Paul Yaggy, Irv Statler, and Andy Morse. In reading these chapters, I was amazed to learn of how some very important decisions on major programs and facilities were made "back in the day." While there was often rigorous technical analysis available, it seems more often than not personal initiative (forged by a deep command of their discipline), a timely brief to a senior official, and tenacious, persistent follow-up, carried the day. One should never underestimate the importance of refusing to take "no" for an answer, in making possible the incredibly potent combination of outstanding personnel, technical excellence, and unique facilities described in these pages, all aimed at rotorcraft challenges.

Epilogue

With a bit more reflection then, I think Chee Tung had it right when long ago I heard him say the secret to good management is “Hire the best people you can, and then get out of their way.” In my waning months as Director I redoubled my efforts therein, in the hopes that this fine and productive Army-NASA rotorcraft collective celebrates a 75th anniversary in 2040. I hope I am alive and able to attend. I expect to see more fruit from today’s crop of Ames “rotorheads,” cultivated by the scientists and engineers, the “Figures of Merit,” you now know through this important and enlightening memoir.

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