F-15 EAGLE

THE EARLY YEARS

BY

JACK M. ABERCROMBIE

INTRODUCTION

The F-15 Eagle, the world's most capable fighter aircraft (arguably better than the much, much later F-22 except in the area of stealth technology) first flew in July 1972. It is thus far the last totally designed-in-St. Louis piloted airplane to be produced by McDonnell Douglas or the successor, Boeing. By the end of this decade, more than 1500 F-15s will have been produced.

But this essay is not meant to be an exhaustive study of the F-15. There are many good books and magazine articles which have done a far better job than I could do in this regard. Instead, the memoir is about my intimate relationship with the F-15 (mostly as an Aerodynamics/Flight Controls Engineer) over a span of more than 35 years beginning with Concept Design in 1969.

I was part of the McDonnell test team at Edwards Air Force Base from the first flight through the completion of USAF Category II tests and for two short-duration periods of Air Force testing in later times. Several airplane improvements and advanced versions resulted from Advanced Design studies in which I participated. As Technology Integrator, I was part of the team that designed, built, and tested the F-15 Short Takeoff and Landing/ Maneuvering Technology Demonstrator (F-15 S/MTD). More recently, after having been retired from McDonnell Douglas for thirteen years, I served as a Consulting Engineer to Boeing on the Republic of Korea F-15K program. It was the latter experience, along with recent correspondence and informal conversations with friends who in one way or another are part of the F-15 experience, that inspired me to compose this story.

This memoir is intended to be for reading by my children, grand-children, and whoever else may follow. Consequently, I've tried to keep this writing non-technical—a most difficult task for an engineer. There are no foot notes, but the parenthetical comments may be tedious for the reader—for this, I apologize. And there are no References—the text is based on memory and the daily log I maintained for more than 20 years as well as upon material in my personal library. Many of the photographs can be credited to McDonnell Douglas—most have been widely circulated.

To the reader unfamiliar with organizational concepts used by large aerospace industries, I'll note that organization charts usually have two pecking-order paths—one associated with a specific *function* (e.g., Aerodynamics, Propulsion, etc.) and one associated with a specific *product* or *project* (e.g., F-15, F-18, etc.). Thus, I mention my *Functional* as well as my *Project* supervisors at various times.

I've put names to some of the heroes in the story as well as to some who I at times may have regarded as villains. In any worthwhile endeavor involving people, there is an element of "give and take"—hopefully always leading to the best compromise with the proper balance of "*better*" and *"good enough"*. Most of the individuals were/are respected colleagues and friends. But by whatever label, they all made the F-15 what it is and were an important part of my career. Of course, there are many important people in the history of the F-15 who I did not name but whose contributions to the program were equally important as mine.

Other very important contributors to my F-15 experience are my family, Carolyn ("KK") my wife, and our children, David and Karen. They held up well during the stressful 1969 period when I could not be with them so much we would have desired. They, too, were part of the team. This was true also during our years in California with the test program.

Jack Abercrombie
05 Feb 006

CHRONOLOGY
JACK ABERCROMBIE AND THE F-15 EAGLE

December 1965—Air Force FX Concept Formulation studies begin.

December 1968—Requests for Proposals for Contract Definition for FX (F-15) issued by the Air Force.

July 1969—F-15 Proposal reports submitted to the Air Force.

September 1969—Serious F-15 directional stability problems identified based on wind tunnel tests.

December 1969—Air Force announces McDonnell Aircraft winner of the F-15 competition.

July 1972—F-15 first flight.

August 1972—Supersonic directional stability and rudder control problem identified in flight test.

December 1972—F-15 #1 completes 100th flight.

June 1973—Solution to cross-wind landing problems defined.

October 1973—Satisfactory speedbrake configuration defined.

March 1974—Wing tips clipped, solving high subsonic, high load factor buffet problem.

June 1974—First spin, unintentional with F-15 #1.

December 1974—"One More Turn" mode spin encountered.

July 1975—Completion of high angle of attack/spin test program without loss of aircraft.

September 1975—T.A.C. evaluation of Conformal Fuel Tanks (CFT).

April 1979—Test Pilot Gary Kincaid lost when F-15 breaks up in flight near Mach 2.

January 1983—F-15C #1 with CFTs installed spins.

October 1984—MCAIR wins STOL and Maneuvering Technology Demonstrator (S/MTD) program.

December 1986—F-15E "Strike Eagle" makes first flight.

September 1988—F-15 S/MTD with non-vectoring, round nozzles makes first flight.

May 1989—F-15 S/MTD with vectoring and reversing two-dimensional nozzles makes first flight.


U.S. AIR FORCE
U.S. AIR FORCE

PRELUDE TO FLIGHT

1969. That was the year U.S. astronauts walked on the Moon for the first time, the year we built the back-yard swimming pool, and when the alligator bit my cheek and wouldn't let go. In 1969, the country installed a crook as President who pledged "Peace with Honor" in Vietnam. General Motors stopped production of the "Corvair." Our kids, Dave and Karen missed two weeks of school—Dave with pneumonia, Karen with strep-throat—both at the same time. In1969, during the middle of the Cold War, the F-15 was born.

F-15 CONCEPT FORMULATION

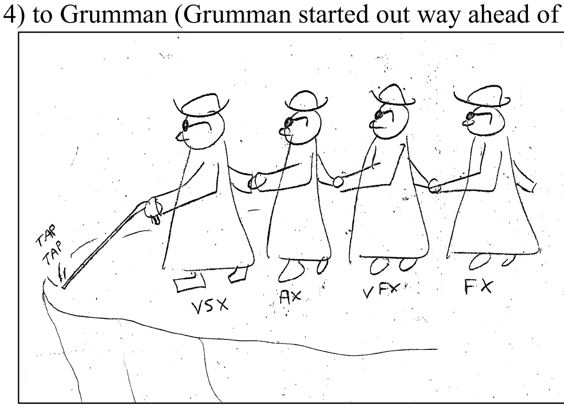
To explain the rationale for the F-15 and how crucial it was to the future of MCAIR (the McDonnell Aircraft Company component of the McDonnell Douglas Corporation) and to my career, it's necessary to delve briefly into some programmatic issues to "set the stage" for the remainder of the story.

The F-15 was conceived during Air Force and Contractor studies for an "FX" (Fighter, Experimental) aircraft. These studies began in 1965 and continued for nearly four years. Four distinct, sequential study contracts were awarded during this time—McDonnell won some of these contracts and lost some. These studies, addressing the Soviet threats by the Mig-21, Flagon A, and the Mig-25 "Foxbat," established the need for a USAF fighter with improved avionics and armament suitable for both long-range (beyond-visual-range missiles) and close-in (the old "dog-fight" scenario) combat, the ability to operate in any weather, and with vastly improved, even un-precedented, flying qualities (flying qualities are airplane characteristics defining the "drive ability" during all kinds of maneuvers), and combat capabilities over a large flight envelope. The flight envelope extended to Mach 1.2 at sea level and Mach 2.5 (about 1700 miles per hour) at altitude. In addition, the airplane needed sufficient "legs" to fly out a certain specified distance, perform combat maneuvers, and return to home base with adequate fuel reserves. (Stealth—low radar, infra-red, and acoustic signatures—was not a requirement. Low radar return techniques were being explored at this time in "Black" programs, e.g., the forerunners of the F-117 fighter).

The four-year Concept Formulation study stage culminated in Requests for Proposals (RFPs) being issued in December 1968 to three airframers—MCAIR, Fairchild-Hiller, and North American Rockwell—for a Contract Definition effort leading to designing, building, and testing the FX, now designated as the F-15. Some 127 aircraft could potentially be delivered in the near-term by the winning contractor.

STRATEGY TO WIN

The competition to win this "last ever fighter airplane" was something fierce. The corporation had recently lost three major combat aircraft competitions—the VSX (S3-A) to Lockheed (a mis-managed proposal by our new partners, Douglas Aircraft Company—had DAC won, the work would have been transferred to MCAIR), the AX Prototype (A-9 and A-10) to Northrop and Fairchild-Hiller (we didn't really try very hard for this one), and the VFX (F-14) to Grumman (Grumman started out way ahead of us on this one. They had been in partnership with General Dynamics with the ill-fated carrier-based version of the F-111 which Grumman helped kill in favor or their own F-14 design). A cartoon circulated around the work stations at MCAIR showing the blind leading the blind in our combat aircraft efforts.



The loss of the F-14 competition hurt badly. After 30 years of being the premier Naval fighter factory, we were dead-in-the-water. And now, the Air Force version of the McDonnell F-4

Phantom was to be replaced by the newer, more capable F-15. In order to survive, a major over-haul of our *modus operandi* was essential.

We were fortunate to have a top-notch Program Manager for the F-15—Don Malvern, previously an aerodynamicist and flight test engineer. Don had successfully led the F-4 program for a number of years before joining the F-15. He and Corporate V.P. for Marketing, Bob Little, brought in a high-priced, well-respected, defense management consultant, Jim Beveridge and Associates, Inc. Beveridge laid it on the line to both MCAIR and Corporate top management. His criticisms of company practices included:

- Use of "shot-gun" approaches to competitive bidding rather than concentrating on the big one with a dedicated effort.
- "Reacting" instead of "acting" on several fronts.
- A lack of total commitment.
- Not understanding Program Management and no single-point of contact for key issues.
- The reception of suggestions for improvement likened to "a large amorphous sponge."
- "Each move you make is slow and deliberate…and compromised…too little and too late and too wishy-washy."

On and on, Beveridge chewed the company out and made several recommendations to help us win:

- Switch the plant from Navy cognizance to that of the Air Force. (Every defense contractor has representatives from one of the services located permanently in the plant). Since we were still turning out Air Force Phantoms, this would be a logical move.
- "Make programmed customer contact the number one effort…" *at all levels.*
- Emphasize the importance of good, well-written Proposal reports.
- Examine each paragraph, each visual in the Proposal for the message it delivers. Let each graphic deliver its own message.

About the same time, our Corporate V.P. for Engineering and Research, Ken Perkins, took the company to task for not having enough aerodynamicists and that the ones we had were seriously overloaded (recognizing this as a problem throughout the industry as a result of the then-current emphasis on space astronautics in the nation's universities).

Obviously, the company was in trouble. It was widely accepted that to achieve a win, drastic changes were needed. And changes were made, including promotional schemes within the company to make everyone a part of the "Think F-15" effort. This was never done before at MCAIR, and it hasn't happened since.

CONCEPTUAL DESIGN and PROPOSAL EFFORT

I was assigned to the F-15 project in January 1969. I had advanced reasonably quickly since joining the company in 1957 (followed almost immediately by a three-year leave of absence to serve my time in the USAF as a flight test engineering officer at Eglin AFB, Florida). At the time of my assignment to the F-15, I was considered to be at the mid-level of engineering—five steps up from the entry level, five steps below the V.P. level.

My previous jobs had pretty well covered the field of Aerodynamics—high lift characteristics for the F-4 Phantom take-off and landing configurations; advanced design studies for the F-4; drag, performance, stability and control for various hypersonic, sub-orbital aircraft advanced design studies; a brief assignment on the ill-fated VSX program (joining after proposal submittal); heading a small Methods group developing computer programs for flying qualities evaluations; and a brief assignment on the proposal effort for the VFX (which we lost).

By the time I joined the program, the F-15 configuration concept had been mostly defined. The process used for concept development throughout the industry is to:

- Define the key mission and combat performance requirements.
- Assume a size/shape/power plant based on engineering experience and judgment.
- Establish preliminary values for the fuel fraction, structural weight, thrust and drag, and wing size.
- Crank out performance capabilities and iterate, iterate, iterate until the lowest weight (cost) airplane satisfying the requirements emerges.

During our effort, wind tunnel tests of various models (800 configuration variants including 74 different wings and 58 different wing variable camber devices) were conducted. The results were fed into the above discussed iteration process. We had developed an innovative Computer Aided Design and Evaluation (C.A.D.E.) program for the iteration effort (virtually the same program is still in use). In the process, thousands of concepts were evaluated to arrive at a near-optimum. I should note that all the aerodynamic work was done without the aid of Computational Fluid Dynamics developed later—even today, CFD has limited application to fighter aircraft.

My technical job on the F-15 was leading a small group evaluating the lateral-directional flying qualities (airplane rolling rotation about the longitudinal axis, yawing or directional rotation about the vertical axis, and lateral <side> motion *along* the left/right axis are intimately related—collectively referred to as "lateral-directional"). This effort included determination of spin characteristics, developing control schemes/laws for the flight control system, supervising wind tunnel tests in support of those areas, and writing my portion of the Proposal which was due in five months.

The calculation tools we used were primitive by today's standards—in the days before PCs, we relied mainly on the use of slide rules and some simple three-degrees-of-freedom flying qualities computer programs previously mentioned. But because of the stringent maneuvering requirements imposed on the F-15 over a larger flight envelope than ever before encountered, for some calculations, we needed to use ***six***-degree-of-freedom (pitch, roll, yaw, up-and-down, fore-and-aft, and sideways) computer programs which had been developed a little more than a decade earlier. It's a formidable task to even think about motion in six degrees, let alone the job of solving the six simultaneous equations. At one time, I had the 6-DOF equations of motion memorized. Of course, the punched-card computer facilities were primitive at the time—it was usually an over-night job for only a few runs of four or five seconds of (real) time history providing one had sufficient priority to pre-empt others desiring use of the computer facility/personnel.

MCDONNELL AIRCRAFT COMPANY

(U) SIX DEGREES OF FREEDOM EQUATIONS OF MOTION

(BODY AXES)

$$\dot{V} = \frac{1}{\cos\beta\cos\alpha}\left[rV\sin\beta - g\sin\theta + \frac{T}{m} - \frac{\bar{q}S}{m}\left(C_{A_{(\alpha)}} + C_{A_{\delta_{H(\alpha)}}}\delta_H\right)\right] + V(\dot{\alpha} - q)\tan\alpha + \dot{\beta}V\tan\beta$$

$$\dot{\alpha} = \frac{1}{\cos\beta\cos\alpha}\left[-p\sin\beta + \dot{\beta}\sin\beta\sin\alpha + \frac{g\cos\theta\cos\phi}{V} - \frac{\bar{q}S}{mV}\left(C_{N_{(\alpha)}} + C_{N_{\delta_{H(\alpha)}}}\delta_H\right)\right] - \frac{\dot{V}\tan\alpha}{V} + q$$

$$\dot{\beta} = \frac{1}{V\cos\beta}\left[-\dot{V}\sin\beta + g\cos\theta\sin\phi + \frac{\bar{q}S}{m}\left(C_{Y_{\beta_{(\alpha)}}}\beta + C_{Y_{\delta_{R(\alpha)}}}\delta_R + C_{Y_{\delta_{a(\alpha)}}}\delta_a\right)\right] + p\sin\alpha - r\cos\alpha$$

$$\dot{p} = \frac{I_Y - I_Z}{I_X}qr + \frac{I_{XZ}}{I_X}(\dot{r} + pq) + \frac{\bar{q}Sb}{I_X}\left[C_{\ell_{\beta_{(\alpha)}}}\beta + C_{\ell_{\delta_{a(\alpha)}}}\delta_a + C_{\ell_{\delta_{R(\alpha)}}}\delta_R\right] + \frac{\bar{q}Sb^2}{2VI_X}\left[C_{\ell_{p_{(\alpha)}}}p + C_{\ell_{r_{(\alpha)}}}r\right]$$

$$\dot{q} = \frac{I_Z - I_X}{I_Y}pr + \frac{I_{XZ}}{I_Y}(r^2 - p^2) + \frac{\bar{q}S\bar{c}}{I_Y}\left[C_{m_{(\alpha)}} + C_{m_{\delta_{H(\alpha)}}}\delta_H\right] + \frac{\bar{q}S\bar{c}^2}{2VI_Y}\left[C_{m_{q_{(\alpha)}}}q + C_{m_{\dot{\alpha}_{(\alpha)}}}\dot{\alpha}\right]$$

$$\dot{r} = \frac{I_X - I_Y}{I_Z}pq + \frac{I_{XZ}}{I_Z}(\dot{p} - qr) + \frac{\bar{q}Sb}{I_Z}\left[C_{n_{\beta_{(\alpha)}}}\beta + C_{n_{\delta_{a(\alpha)}}}\delta_a + C_{n_{\delta_{R(\alpha)}}}\delta_R\right] + \frac{\bar{q}Sb^2}{2VI_Z}\left[C_{n_{r_{(\alpha)}}}r + C_{n_{p_{(\alpha)}}}p\right]$$

MCDONNELL DOUGLAS CORPORATION

4.34

MDC A0503, PART I
30 JUNE 1970

Besides my technical assignment, I and one other engineer (a structural type) were given the task of providing technical liaison between our engineers in St. Louis and our counterparts in the Air Force System Program Office (SPO) at Wright-Patterson AFB in Dayton, Ohio as part of the company's “programmed customer contact” effort. It was required that we coordinate also with non-SPO personnel in the event they would be assigned to the proposal evaluation team. My specific job was to represent the disciplines of Aerodynamics, Guidance and Control, Propulsion, Thermodynamics, and C.A.D.E., but the job frequently expanded into the overlapping disciplines of Air Data, Fuel Systems, Contracts, and Marketing.

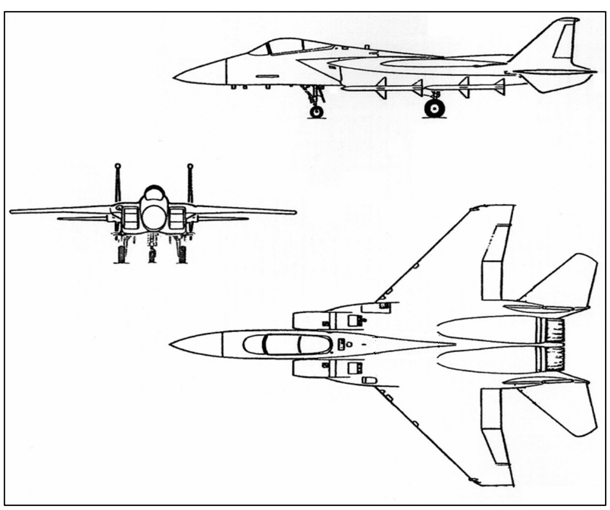
Some aspects of my Dayton job were both enjoyable and educational. I learned more about what the Customer wanted and was able to convey these wants to the St. Louis technical community. I learned a bit about the disciplines outside of Aerodynamics. At the same time, I made some great friends with the USAF technical people, Herbert “Skip” Hickey, a flying qualities expert, in particular.

Of course, besides the task of being the conduit for flow of information between the Contractor and the Customer, it was our job to keep our eyes/ears open for any information about our competition, and probably most important, to act as marketeers. This aspect of the job I detested—to purposely call up a complete stranger, make an appointment to see that person, and follow up with an appropriate interchange of information was very difficult for me—I’ve never been an outgoing individual. But apparently, I did well enough to be asked to take a job in Washington, D.C. marketing to Pentagon personnel and Congressional staffers—that was the only job request I ever turned down. I was supposed to be in Dayton at least three days a week, but I managed to minimize nights away from home by making two (sometimes three) trips a week. During this period, I attended a two-day proposal writing seminar in New York City conducted by the same consultant, Jim Beveridge, previously discussed.

I felt that I was successful overall in my “Customer Contact” job. A few weeks before our Proposal submittal, a member of the SPO commented that our company’s openness and honestly and not hiding things under the table were very much appreciated.

The technical coordination job was, of course, in addition to my St. Louis job of being an Aerodynamics engineer and proposal writer. For six months, we worked usually 12 hour days, often seven days a week analyzing wind tunnel test results, calculating/documenting airplane characteristics, and writing our portion of the Proposal. For the duration of the proposal effort, all vacations of F-15 personnel were cancelled.

Finally, after spending 2.5 million man-hours, the proposal (37,500 pages in 382 separate subject volumes) was wrapped and delivered. The doors of the SPO were generally closed to the competing contractors (except for USAF requested meetings) for the six-month proposal evaluation period.



After a short vacation, I got back to work as an engineer—that’s when the fun began!

LATERAL-DIRECTIONAL PROBLEMS

Our wind tunnel tests had continued during the proposal preparation effort and through the summer. In addition, NASA conducted tests on the proposal configurations of all three contenders in a "fly-off" of sorts. When our configuration was tested complete with its complement of external weapons throughout the flight envelope, a problem was revealed—the airplane was seriously lacking in directional stability at the previously untested higher subsonic angles of attack (above about 20 degrees nose high relative to the oncoming wind) and at high supersonic (above about Mach 1.4) conditions.

Directional stability is necessary to keep the airplane nose pointed forward into the wind; it is achieved with aft-mounted vertical surfaces as in the tail feathers of an arrow. A directionally stable airplane will tend to rotate about its center-of-gravity always pointing towards the relative wind like a weather vane about its support.

The high angle of attack problem arose as a result of adverse airflow characteristics in the vicinity of the vertical tails caused by the flow separation around the engine inlets and the inboard portions of the wings. The aft fuselage wide body prevented the airflow from recovering before reaching the tails.

The supersonic problem resulted from ineffectiveness of the lower surface ventral fins located almost directly behind the fuselage-mounted missiles. At these conditions, the ventrals were just along for the ride in the missile wake creating nothing but drag.

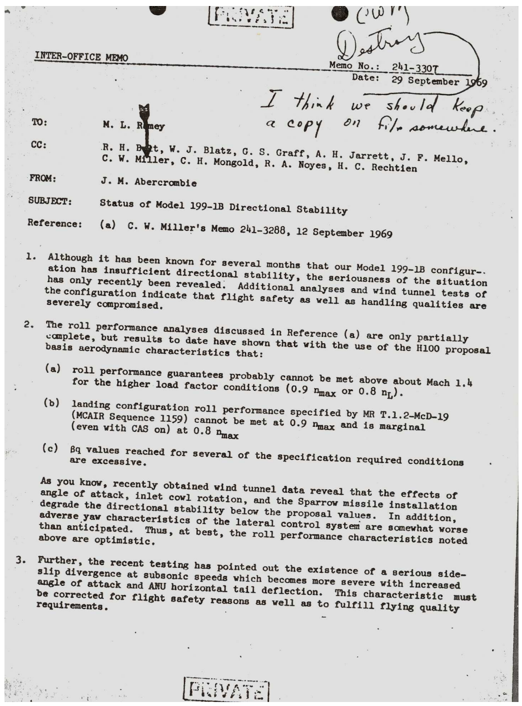
In addition to weak directional stability, we learned that the configuration exhibited a greater amount of "adverse yaw" due to roll control surfaces than had been anticipated. (Adverse yaw is when the airplane will initially yaw about its vertical axis in a direction opposite to the desired direction of turn requested by the pilot application of roll control—e.g., right stick results in initial left yaw). Many airplanes have some level of adverse yaw. To counter it, pilots are taught to coordinate their control inputs with a blend of stick and rudder pedal inputs.

The F-15 basic specification requirements, however, were to be able to roll the airplane (rapidly) with "feet on the floor"—no rudder pedal input. Further, the spec required that this be accomplished without the aid of electronic, computer controlled inputs to the control surfaces. In other words, the airplane control surfaces had to be able to respond to pilot input using only a mechanical control system (push-rods, linkages, hydraulic actuators). An electronic Control Augmentation System (CAS) was provided, but it was to be merely "frosting on the cake." These mechanical control system requirements were imposed because at the time, electronics and computers for such application were primitive and unreliable compared to those of a decade later.

Well, I was quite alarmed at these developments. Not only could the airplane not achieve the flying qualities characteristics we (***I*** in particular) had promised the Customer, in some flight conditions, the characteristics could be life-threatening to the air crew. I concluded that the configuration had to be changed. My voiced opinion was not very popular, needless to say—a configuration change in the midst of a proposal evaluation was something to be avoided. Also, there was a philosophy in some quarters of management that "The best fighter design is one which is marginal in all aspects" (quote of Herman Barkey) which may be true considering all economic aspects but allowing no room for growth.

However, I knew my counterparts at the SPO were examining the same wind tunnel test data as was I, and I didn't feel we had the luxury of ignoring the problem. So, in desperation, I put my job on the line and wrote an interoffice memorandum to the V.P. of Engineering Technology, "Pete" Ramey with copies to a number of high-ranking engineering management individuals, including the V.P. of Engineering, George Graff. In the memo, I outlined the problems, suggested appropriate modifications to the airplane, and recommended a wind tunnel test devoted to the specific issues.

The first reaction my memo got was the instruction from Graff to destroy the memo and all copies! (Through the rest of my career, that memo was known as the "Burn Memo". However, not all copies were destroyed—a few years later when I became Chief of Aerodynamics, I found that the then Chief, Chet Miller, had hung on to his copy, so I resurrected it).



PRIVATE

(own)
Destroy

INTER-OFFICE MEMO

Memo No.: 241-3307
Date: 29 September 1969

I think we should keep a copy on file somewhere.

TO: M. L. R[illegible]mey

CC: R. H. B[illegible]t, W. J. Blatz, G. S. Graff, A. H. Jarrett, J. F. Mello, C. W. Miller, C. H. Mongold, R. A. Noyes, H. C. Rechtien

FROM: J. M. Abercrombie

SUBJECT: Status of Model 199-1B Directional Stability

Reference: (a) C. W. Miller's Memo 241-3288, 12 September 1969

1. Although it has been known for several months that our Model 199-1B configuration has insufficient directional stability, the seriousness of the situation has only recently been revealed. Additional analyses and wind tunnel tests of the configuration indicate that flight safety as well as handling qualities are severely compromised.

2. The roll performance analyses discussed in Reference (a) are only partially complete, but results to date have shown that with the use of the H100 proposal basis aerodynamic characteristics that:

   (a) roll performance guarantees probably cannot be met above about Mach 1.4 for the higher load factor conditions (0.9 n_{max} or 0.8 n_L).

   (b) landing configuration roll performance specified by MR T.1.2-McD-19 (MCAIR Sequence 1159) cannot be met at 0.9 n_{max} and is marginal (even with CAS on) at 0.8 n_{max}

   (c) βq values reached for several of the specification required conditions are excessive.

   As you know, recently obtained wind tunnel data reveal that the effects of angle of attack, inlet cowl rotation, and the Sparrow missile installation degrade the directional stability below the proposal values. In addition, adverse yaw characteristics of the lateral control system are somewhat worse than anticipated. Thus, at best, the roll performance characteristics noted above are optimistic.

3. Further, the recent testing has pointed out the existence of a serious sideslip divergence at subsonic speeds which becomes more severe with increased angle of attack and ANU horizontal tail deflection. This characteristic must be corrected for flight safety reasons as well as to fulfill flying quality requirements.

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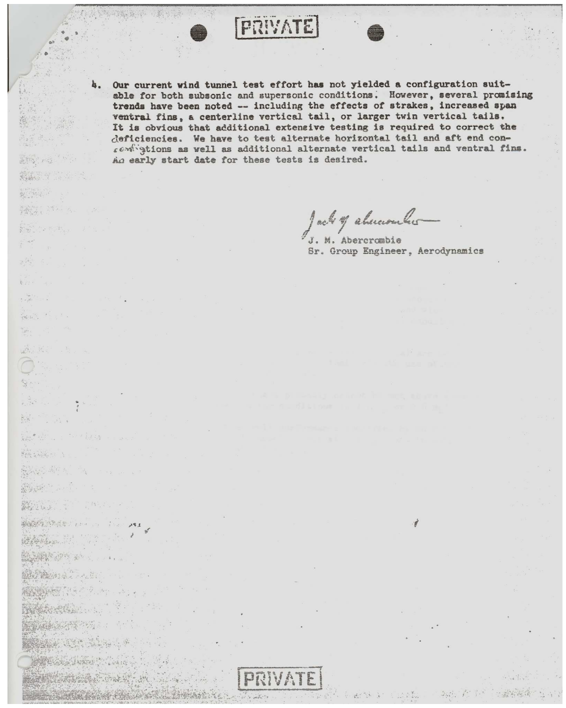

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4. Our current wind tunnel test effort has not yielded a configuration suitable for both subsonic and supersonic conditions. However, several promising trends have been noted -- including the effects of strakes, increased span ventral fins, a centerline vertical tail, or larger twin vertical tails. It is obvious that additional extensive testing is required to correct the deficiencies. We have to test alternate horizontal tail and aft end configurations as well as additional alternate vertical tails and ventral fins. An early start date for these tests is desired.

Jack M. Abercrombie
J. M. Abercrombie
Sr. Group Engineer, Aerodynamics

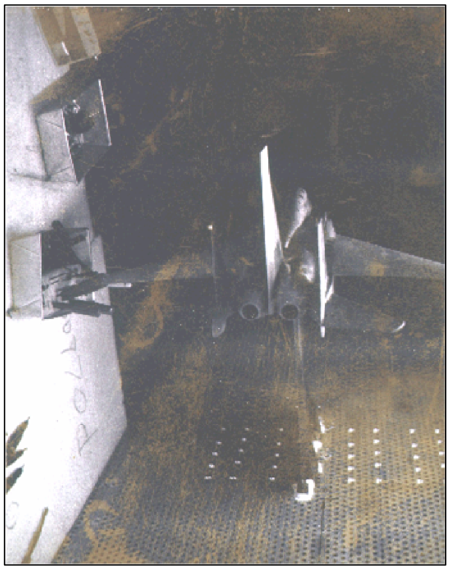
PRIVATE

The second reaction to the memo was the approval of a modest, 80 occupancy-hour wind tunnel test in our Polysonic Wind Tunnel (PSWT) to evaluate my suggestions and, also, to look for some "magic" minor configuration refinement that would seem insignificant on a 3-view drawing (we at the working level called such changes "postage stamps" or "coupons") and, thus, would not blatantly admit to any kind of failure on the part of the company (in spite of the new *modus operandi,* it was hard to get rid of old habits).

Wind tunnel tests began in mid-September '69. The one authorized test of a 4.7% scale model (2 feet wing span) in the PSWT quickly developed into two simultaneous tests including the Low Speed Wind Tunnel (LSWT) utilizing a larger, 13% scale model (5 feet, 6.5 inches wing span). The two tunnels operated in three shifts, around-the-clock, seven days a week for a period of six weeks and 2100 occupancy-hours—26 times the original 80 hour authorization. In addition to the wind tunnel Laboratory personnel, we worked 12 Aerodynamics engineers, one Project Aerodynamicist, two Section Managers, the Chief of aerodynamics, and a few consultants from other projects. Most participants worked their shifts then went home, but not I. In addition to my intimate involvement with test planning and data analysis, I was required to have a report (hand-written since secretarial help wasn't available in the wee hours) summarizing the previous 24 hour happenings on the desks of the V.P.—Engineering Technology Pete Ramey, the F-15 Deputy General Manager-Technology Bill Blatz (also a V.P.), the F-15 Director for Configuration Development Al Jarrett, and my own functional supervision by the time they came to work at 0800. And then, a daily three to four hour meeting was held with the same personnel beginning at 0900.

Oh, we tested a lot of major configuration changes as well as "coupons" consisting of some 56 strakes/fences, 11 aft fuselage contour fairings, 38 variations to the vertical tails, 23 variations to the ventral fins, and nine horizontal tail variations in addition to combinations of these variables. We performed various airflow surveys to document the confused flow at the transonic (M=0.9) high angle of attack (25 degrees) conditions (breaking one model off at its support due to the violent shaking—total destruction).

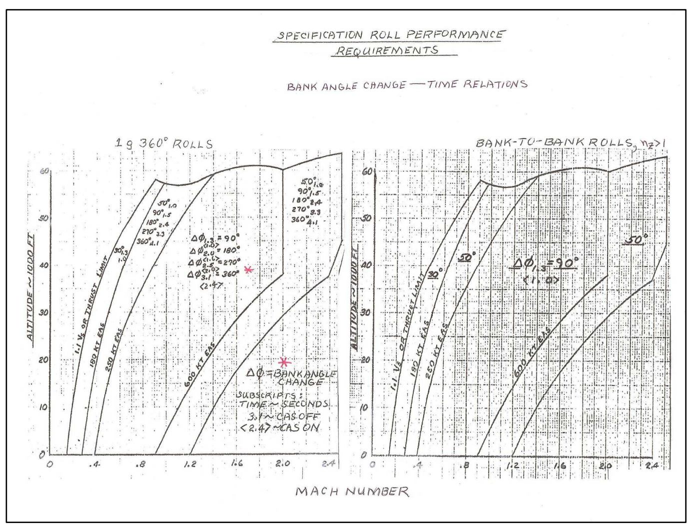
Testing concentrated on three critical conditions (M=0.9 high angle of attack, M=1.6, and M=2.25) with additional explorations at both higher and lower speeds. In my way of thinking, the primary area of concern was the high supersonic conditions—without adequate directional stability and with a roll control input, the airplane could turn sideways and break up, as indeed, our analyses showed (although at the time there were uncertainties about aeroelastic effects on the empennage and on control surface hinge moment characteristics). Management, however, was not so impressed with my concern, believing that in the real world, the airplane would rarely fly at those supersonic conditions.



The other critical condition was not where the airplane would necessarily break up if it got out of control, but it was where the airplane would either not respond adequately to pilot input to roll the airplane, or, at worst, would depart from controlled flight and possibly enter a spin. This condition was the subsonic, high angle of attack flight regime.

The solutions to both these problems, I believed, were taller, larger vertical tails.

The Detail Specification for the F-15 did not adequately address the supersonic condition, but it did impose some tough roll performance requirements for the subsonic, high angle of attack conditions. It was possible to use the Specification as a management tool (a tool to convince management)—and use it we did.



During all this time, the NASA wind tunnel test results were being analyzed by our Customer, the F-15 SPO. In October 1969, we were summoned by the SPO to discuss the results of the NASA tests. I suggested to my management that we admit to directional stability shortfalls as well as to the adverse yawing moments which aggravated the supersonic airplane response and to tell the SPO what we were doing to solve the problems. My suggestions were dismissed, and we met with the SPO on 10 October. My early-morning, hand written report about the SPO meeting of the day before noted that:

"At the risk of being re-branded as a pessimist, I felt we stepped in over our heads and immediately sank…At times I felt like I had "blown my cover"—after spending months trying to convince [the SPO] that our management is flying qualities conscious, it seemed that [they] realized I was toeing the party line by keeping my mouth shut about how serious our problems are and how we considered them as serious."

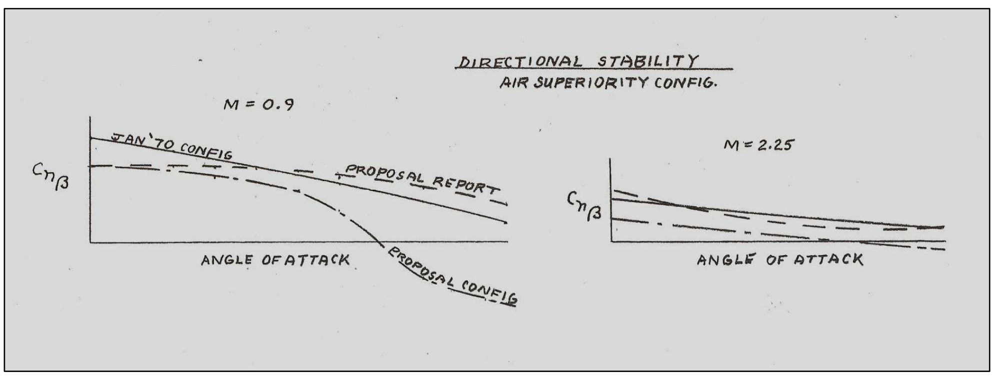
A comment made to the SPO group by our F-15 Deputy General Manager—Technology, Bill Blatz, that we were “just now” getting in the tunnel (about three weeks after the actual start) to work on the adverse yaw and that we hoped to “improve our directional stability a *little bit*” was not well received by the Customer.

After nearly three months of frustrating discussions and presentations, finally, in a flash of brilliance, the F-15 Director for Configuration Development, Al Jarrett, decided on a solution—we would merely “lift the vertical tail tip upwards 26 inches.” This was his idea of making a change without being too obvious! There were some other “coupon” changes (fences on the outside of the nacelle inlet), but, of course, the increasing of the vertical tail size by 33 percent helped the most. But even this change was not enough (more later).

Yearly promotions at that time were determined by November. Although due for a promotion, I was held back. By this time, I feared for my job. My family had been seven years in our house with a 30 year mortgage. Our kids were ages 11 and almost nine. I had no desire to be looking for work.

But a few days before Christmas, our company stock shot up, a Long Island (home of one of our competitors) senator started bitching about the program, and we started getting reports of extreme pessimism from our other competitor in California. These were good indications that we had won! Two days before Christmas ’69, the Air Force announced its decision—the winner of the F-15 competition was McDonnell! We had a great celebration party that evening (I interrupted a vacation to attend), and for the first time in several months, I felt job security—at least for awhile.

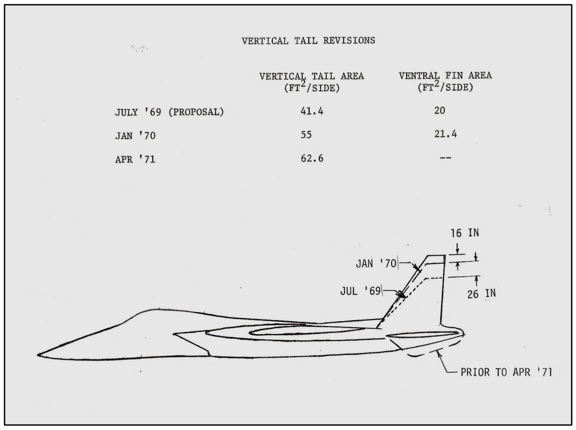
Less than a month after the win announcement, we formally presented the new configuration (vertical tail increase, the nacelle fences, as well as some other changes unrelated to the lateral-directional problems.



DETAIL DESIGN

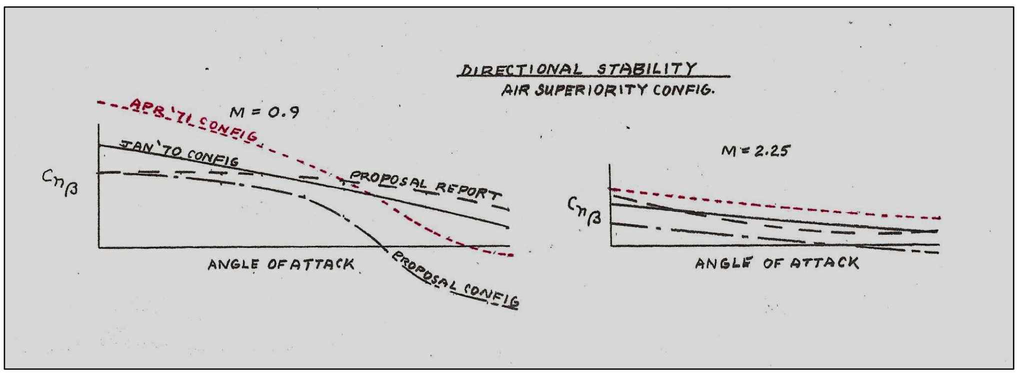
For the next year, we continued wind tunnel testing, analyzing, and documenting the airplane characteristics while Detail Design activities ramped up (and I was promoted in December ’70, a year after my “Burn Memo”). I made several trips to Government wind tunnels at NASA Langley, NASA Ames, and the Air Force AEDC. But the supersonic low level of directional stability, although improved with the January ’70 increase to the vertical tail size, still was not being accepted as real by some of our management.

We knew from our 1969 wind tunnel tests that the ventral fins in the presence of the missiles mounted on the lower corners of the fuselage (ahead of the ventrals) were ineffective at supersonic conditions. (I likened the ventrals to being about as useful as the rear fender fins on a ’56 De Soto). One of my functional supervisors, John Mello, being more politic than I, had kept that information in his pocket for more than a year. I never understood the reason for his inaction until the spring of 1971—at this time, he used the information in bargaining with the Director for Configuration Development and the V.P. of Engineering for a further increase in vertical tail size while *completely eliminating the ventral fins.* As a result, besides helping the directional characteristics, he could save a few counts of drag and a few pounds



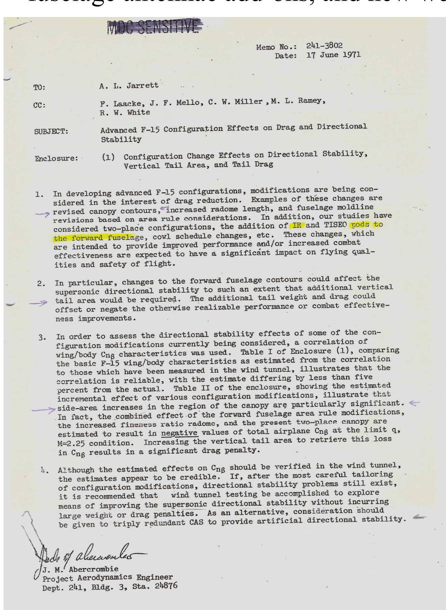
of structure for improved performance. Thus, the vertical tail tip was again lifted upward another 16 inches (now higher by 42 inches than in the Proposal configuration of 1969) for a further increase in tail size (of course, once again, barely noticeable!). With this change, the directional stability was marginally acceptable based on our understanding of the aeroelastic characteristics and the control surfaces hinge moments. (The clever reader will observe that I've mentioned these two factors a couple times. We'll revisit this area later).

My supervisor was credited with improving the airplane performance through drag and weight reductions. I was transferred to a different job. I don't know if my transfer was intended to be a reward or punishment.



ADVANCED DESIGN

The project to which I was transferred was still with the F-15, but in an Advanced F-15 group whose task was to promote the airplane by inventing modifications for improvement, new weapons carriage concepts, performance enhancements, etc. In short, the group was looking for means to extend the hardware sales beyond the initial 127 vehicles. Among the potential changes we considered were increased fuel with low subsonic drag "strap-on" Conformal Fuel Tanks (CFTs) on the sides of the fuselage below the wings (in the "armpits"), the addition of improved avionics capabilities which required forward fuselage antennae add-ons, and new weaponry.



MDC SENSITIVE

Memo No.: 241-3802
Date: 17 June 1971

TO: A. L. Jarrett

CC: F. Laacke, J. F. Mello, C. W. Miller, M. L. Ramey, R. W. White

SUBJECT: Advanced F-15 Configuration Effects on Drag and Directional Stability

Enclosure: (1) Configuration Change Effects on Directional Stability, Vertical Tail Area, and Tail Drag

1. In developing advanced F-15 configurations, modifications are being considered in the interest of drag reduction. Examples of these changes are revised canopy contours, increased radome length, and fuselage moldline revisions based on area rule considerations. In addition, our studies have considered two-place configurations, the addition of IR and TISEO pods to the forward fuselage, cowl schedule changes, etc. These changes, which are intended to provide improved performance and/or increased combat effectiveness are expected to have a significant impact on flying qualities and safety of flight.

2. In particular, changes to the forward fuselage contours could affect the supersonic directional stability to such an extent that additional vertical tail area would be required. The additional tail weight and drag could offset or negate the otherwise realizable performance or combat effectiveness improvements.

3. In order to assess the directional stability effects of some of the configuration modifications currently being considered, a correlation of wing/body $C_{n\beta}$ characteristics was used. Table I of Enclosure (1), comparing the basic F-15 wing/body characteristics as estimated from the correlation to those which have been measured in the wind tunnel, illustrates that the correlation is reliable, with the estimate differing by less than five percent from the actual. Table II of the enclosure, showing the estimated incremental effect of various configuration modifications, illustrate that side-area increases in the region of the canopy are particularly significant. In fact, the combined effect of the forward fuselage area rule modifications, the increased fineness ratio radome, and the present two-place canopy are estimated to result in negative values of total airplane $C_{n\beta}$ at the limit q, M=2.25 condition. Increasing the vertical tail area to retrieve this loss in $C_{n\beta}$ results in a significant drag penalty.

4. Although the estimated effects on $C_{n\beta}$ should be verified in the wind tunnel, the estimates appear to be credible. If, after the most careful tailoring of configuration modifications, directional stability problems still exist, it is recommended that wind tunnel testing be accomplished to explore means of improving the supersonic directional stability without incurring large weight or drag penalties. As an alternative, consideration should be given to triply redundant CAS to provide artificial directional stability.

J. M. Abercrombie
Project Aerodynamics Engineer
Dept. 241, Bldg. 3, Sta. 24876

Many of the items we evaluated were bound to impact the supersonic directional stability negatively, particularly for the two-place, trainer version of the airplane which had a larger canopy with a greater side area than for the single-place airplane. I expressed this concern in June '71 and suggested we should begin to think about working on a triply redundant (for better reliability) Control Augmentation System (CAS) for Advanced F-15 versions which might not be saddled with the unaugmented, mechanical control system Specification requirements for flying qualities.

We also devoted a considerable effort configuring the F-15 to compete with the Navy F-14 by modifying the high lift system and landing gear to make the airplane suitable for aircraft carrier operations. This, of course, would have been a major hardware change, but it was a necessary effort in the event the

F-14 stumbled as it had in December ’70 when the first test airplane crashed and was destroyed on its second flight. At the time of my joining the Advanced F-15 effort, the F-14 test program had just restarted after being on hold for five months awaiting the completion of the second airplane and approval to re-start (F-14 No. 2 made its first flight in May ’71).

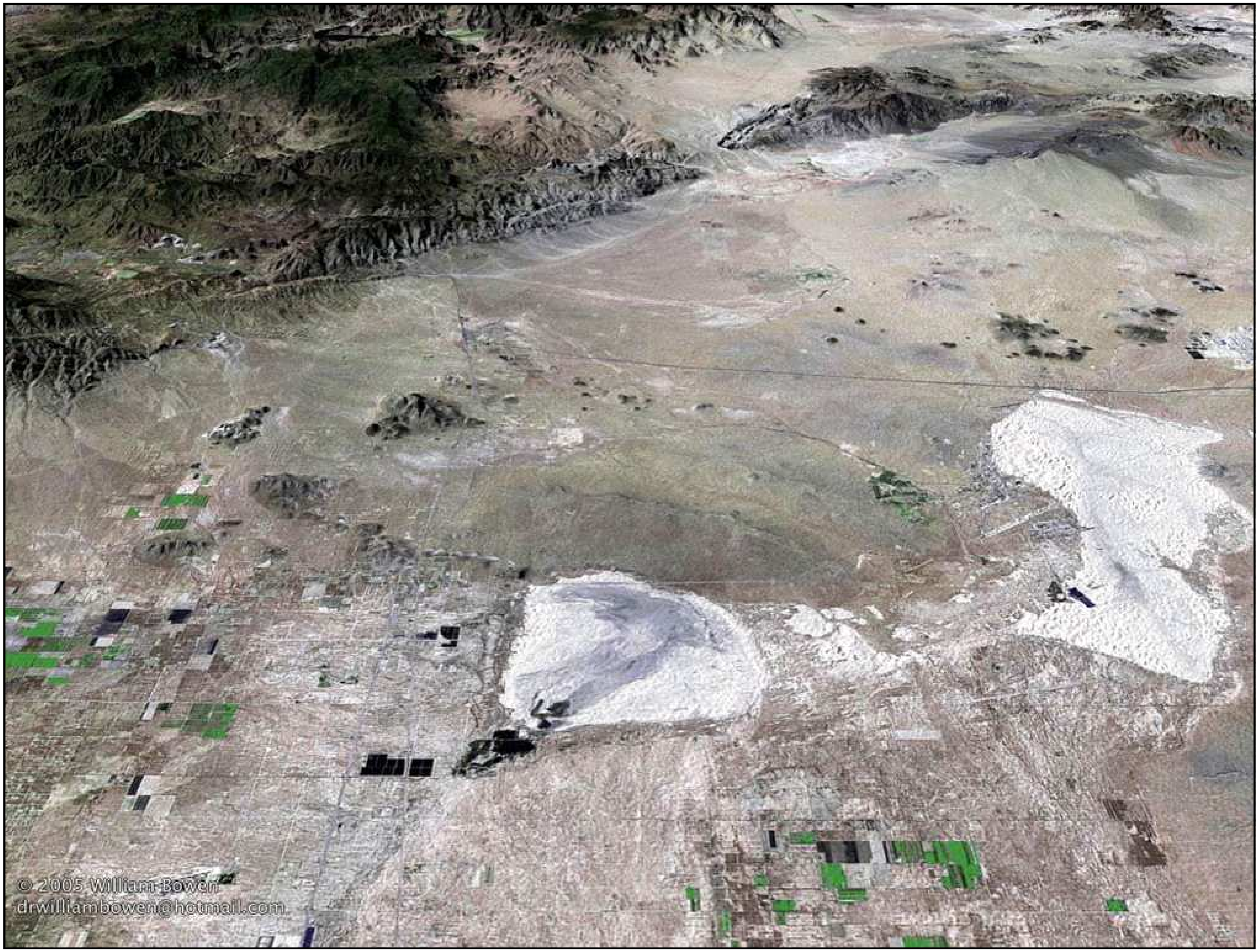
In August ’71, Nixon implemented his first, three-month period wage and price freeze. Salary increases were eliminated that December. A Government-established pay board finally loosened its controls, and in May ’71, I received a very nice raise along with a six-month retro-active adjustment. Of course, Nixon’s effort did nothing to control inflation.

In spite of Nixon, productive work continued as the F-15 moved closer and closer to reality. At long last, planning for flight testing was in work. On Ground Hog’s Day 1972, I was asked to transfer back into the basic F-15 Project and move to Edwards Air Force Base for the Flight Test Program. Tough decisions had to be made with a lot to think about—two kids in school, a dog and a cat, a house, two cars, and KK’s mother living with us while she recuperated from a broken hip. As the Flight Test organization was being assembled for an anticipated July first flight, there was less than six months remaining to get affairs in order.

Ground Hog’s Day was on a Wednesday—I was given until Monday for my decision. For four days, we talked it over (kids included) and concluded that the flight test assignment would be best for my career in spite of the hardships. So we made the necessary arrangements and tried to approach the move as an adventure.

My transfer back into the F-15 Project occurred in March ’72 after having spent a year away from the real-world details of airplane design. In addition to my qualifying experience encompassing so many engineering disciplines (including my three years in the USAF as a Flight Test Engineer), I learned that one reason I was chosen for the job was the belief that I could “stand up to the parochialism” of the designated Director of Engineering, Hermon Cole (from the Design Engineering Division) and protect the interests of the Engineering Technology Division! Without getting too far ahead of my story, Hermon Cole was probably the best engineering supervisor I ever had—it will be later illustrated that the team at Edwards was not parochial in the least. We were a close-knit team with the success of the F-15 test program foremost in our minds.

Jack Abercrombie
05 Feb 006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drwilliambowen@hotmail.com

DESERT AERIE

ANTELOPE VALLEY

The western-most area of the Mojave Desert of California comprising some 3000 square miles is called, “Antelope Valley.” There have been no antelope in the valley for about a century. And “valley” is a misnomer—it actually is a “basin” with what little rain-fall and snow-melt the area receives, drains from the bordering San Gabriel and Tehachapi Mountains into the low spots through about a dozen small named creeks (usually dry) and more un-named “washes.” The lowest (about 2300 feet elevation) areas in the valley are the playas; Rogers Dry Lake and Rosamond Dry Lake are the largest, both very flat and with considerable saline deposits.

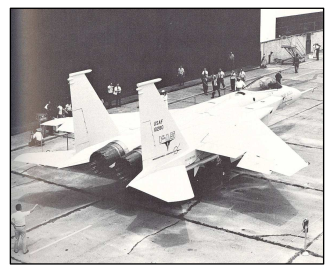
In the 1970s, Lancaster was the largest city in the region with a population of less than 50,000. About 23 crow-flying miles from Lancaster at the western edge of Rogers Dry Lake, is Edwards Air Force Base, home of the Air Force Flight Test Center (AFFTC) and the NASA Dryden Flight Test Research Center (DFRC).

Edwards has a history dating back to 1942 of exciting flight research and is regarded as the foremost flight research center in the world (even more so than the Groom Lake Complex on the Nellis Air Force Base, Nevada reservation where testing for top-secret and “Black” programs are carried out) or the Navy test facility at Patuxent River, Maryland.

In St. Louis, my family was excited about our pending move to the Mojave Desert, and we devoted most of our time from February to July ’72 getting ready for it. Dave and I painted the outside of the house and some of the interior. We built storage areas in the basement, found a renter for the house (a young engineer who worked for me, Max Givan), and moved KK’s mom (who had pretty well recuperated from her broken hip) back to Kansas City.

I traveled to Lancaster, California in early June ’72 on a house-hunting trip. Immediately, I became infatuated with the high desert country. I rented a Spanish-style 1930s house (the “Haunted House on Fern Street”) with a swimming pool and a lawn which never needed mowing.

Meanwhile, at the factory in St. Louis, flight test planning continued. On 26 June '72, F-15 Roll-Out ceremonies were conducted, after which, the first airplane was partially disassembled for shipment to Edwards in a C-5 Galaxy. The first flight of the Eagle was to be conducted at Edwards rather than from the St. Louis plant in the interest of safety—since *both* the airframe and the engine were new and relatively un-tried, risk assessment dictated Edwards as the better location.



We left St. Louis about the first of July, did some sightseeing along the way, and arrived in Lancaster on 10 July '72. At this point, the most satisfying three years in my career began. But before exploring that further, a few words about the subject of flight testing are appropriate.

FLIGHT TEST—AN OVERVIEW

Most people no doubt think of the flight testing of a fighter airplane as being highly glamorous. Their minds may conjure up visions of a Test Pilot "wringing out" the subject airplane, narrowly escaping death, being supported on the ground by hundreds of engineers with eyes glued to their computer monitors under the watchful eye of a harried looking Mission Controller. Then, after the flight, the pilot is met by a beautiful babe running across the tarmac before he jumps behind the wheel of a sports car to, perhaps, film a battery commercial for television.

All that is only partially correct. Most of the Experimental Test Pilot's job consists of preparing for the test flight—rigorously "flying" similar missions in a simulator, planning the specific sequence of events, reviewing anticipated results with appropriate engineers (the pilot is also an engineer), assessing risk, making a short test flight, and then participating in the post-flight debriefing meeting about what went right and what went wrong (and possible reasons why). Only after the post-flight debriefing can he change from his sweaty flight suit, nurse his black eyes from too many "eye-balls-out" g-forces, and meet (or look for) the babe on the tarmac.

For the F-15 flight tests, the Mission Controller (in our case, a Senior Flight Test Engineer) was surrounded by and in constant communication with only a handful of key engineers each of whom was regarded as being an expert in his/her particular discipline. Depending on the specific test being conducted, the supporting Technology Engineers normally consisted of the specialties of Aerodynamics, Flight Controls (the Guidance and Control Department for us at that time), Propulsion, and various Data Engineers. For certain tests, depending on the critical nature of the planned events, other disciplines (Loads, Structural Dynamics, Thermodynamics, etc.) were on hand.

During a test sortie, the supporting engineers continuously watched time histories of selected parameters telemetered from the instrumented test aircraft. In the early days of the F-15, "Brush" recorders were used to trace up to eight measureands onto large rolls of continuous motion graph paper. We used three or four recorders for every flight positioned side-by-side so the engineer could view the output from the recorders to the side as well as the one in front of him. There were no computer screens nor software to make real-time comparisons of predicted and flight test aerodynamic data or aircraft response as in today's test world.

Each aircraft was instrumented with close to a thousand measureands, about half of which were telemetered to the ground station. Each airplane had a unique instrumentation package depending on its assigned task. A dozen Eagles were tested at Edwards during 1972-1975, each of which were devoted to specific test purposes or mission types. Eight more were delivered directly to the Air Force during this time, although the Air Force Test Team participated in the early flying of the first 12 airplanes at Edwards. In general, a new airplane arrived on-site about once every two months for the first couple years.

PRIMARY TEST AIRCRAFT ASSIGNMENTS

F-1 Envelope Expansion, Flying Qualities, External Stores
F-2 Engine Development, Performance
F-3 Avionics, Airspeed System
F-4 Structural Loads
F-5 Armament, Tank Jettison
F-6 Avionics, Fire Control System
F-7 Armament
TF-1 Two-Seater Evaluation, Training, VIP Fam Flights
F-8 High AOA, Stalls, Spins
F-9 Aircraft and Engine Performance
F-10 Tactical Electronic Warfare System, Radar [Airplane Tested at Eglin AFB, FL]
F-17 Time-to-Climb Record Setting
TF-2 Special Programs

At the peak of the Edwards activity, we had around half dozen test pilots assigned to the Contractor (Category I) program (with additional pilots performing some testing out of St. Louis). Further, the Air Force utilized about the same number of test pilots for the often concurrent Customer (Category II) program. In addition to the F-15s, virtually all test flights were accompanied by a "chase" aircraft.

Between test sorties, the supporting Technology engineers analyzed data and made recommendations for appropriate airplane system modifications as well as for future test plans. The Flight Test Engineers mothered their airplanes (with the help of numerous Shop personnel) and made specific plans for the next sortie while the Data Engineers performed the necessary data reduction and transmittal of data, and maintained the data software. A cadre of Design Engineers and sub-contractors supported the effort with airplane and system modifications when required (which was often).

In the beginning, about 100 personnel were transplanted from St. Louis. At the peak of activity nearly 400 were St. Louis transplants while an additional 100 or so were local hires.

By no means was the flight testing all glamour. Most of the work was meticulous, but "exactness" was not an option available with the time constraints which were imposed and with the relatively primitive tools we had to use. Of necessity, we had to improvise and make approximations in our work—that's what experts are for.

With that overview of what our flight testing was all about, it's time to relate some of my personal experiences during the test program—some very stressful, some mundane, and some exhilarating.

Jack Abercrombie
6 April 2006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USAF

FIRST F-15—FIRST 100 FLIGHTS

FIRST FLIGHT

27 July 1972. The pre-dawn desert temperatures were in the mid-seventies, bringing the promise of another 115˚ or more day to the Antelope Valley. I noted on my early drive to work that a full moon made for deep, dark shadows on the rugged slopes of Red Hill at the western edge of the Edwards reservation. In the small hills and dunes between Rosamond and Buckhorn Dry Lakes, the desert floor was splotched with primeval shadows cast by the many Joshua trees.

I arrived at my work station in Building 1881 at 0500 PDT that morning—a full hour before dawn. We engineers directly involved with the flight test activity had not slept well the previous night. Likely, the Chief Experimental Test Pilot, Irv Burrows, had not slept at all. This was to be the day—First Flight!

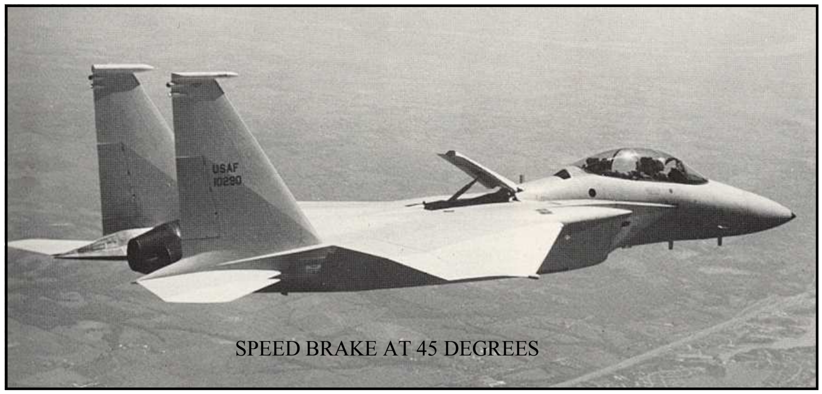
The first flight of a new airplane can never be forgotten by those who have had a hand in the development. All of us were on edge that morning—apprehensive yet eager to get the event behind us. This would be a test of whether the hundreds of engineers responsible for the design were correct in their analyses, tests, simulations, and judgements. But more important, this day would focus on the pilot of the F-15 and the handfull of engineers in the telemetry (T/M) room. In some respects, the test pilot was fortunate in that, if something went wrong, he had the option of punching out (ejecting)—*we* would have to live with whatever errors may occur. If all went right, our biggest thrill would be for the airplane to successfully land after a meaningful flight. Until that time, the entire flight would be to us like what the few seconds before the opening kick-off is to the players in a football game.

I was stationed at T/M Station No. 1 watching the strip charts with telemetered traces of airspeed, angle of attack, pitch rate, roll rate, sideslip angle, and both the LH and RH stabilator surface deflections. Loyal Guenther, our Guidance and Control Engineer, was stationed to my right to observe various control system parameters. Loyal and I watched our own as well as each other's strip charts. Hershel Sams (Propulsion) and Jack Wilcox (Strength Engineer) were each positioned at nearby stations. Frank Roberts and Don Nash (Flight Test Engineers) were behind us observing everything and communicating with the pilot. Data Engineers were also in attendance to make certain the ground equipment worked properly.

Although we had participated in a few "practice" simulations in St. Louis and had been involved in four taxi tests at Edwards (the latter building up in speed to just beyond nose-wheel lift-off just short of takeoff speed), probably none of us felt fully prepared for the moments following "brake release."

Brake release at 0820 (about 25 minutes after engine-start) was followed by an uneventful lift-off from Runway 04 (northeast over the dry lake bed in the event of an emergency landing or worse) after a ground roll lasting about 30 seconds. Lift-off took place in front of the assembled crowd of family members who had been transported by bus to a small hill near the predicted lift-off point. The guests remained there for the duration of the maiden flight and witnessed the landing as well as the takeoff. (It would be after seven more flights before I saw the F-15 in the air).

During this time, we in the T/M room had our eyes glued to the orange graph paper with blue ink strip charts rolling by so fast it was impossible to immediately assimilate all the data. Except for the airplane's left main gear door which refused to close all the way (resulting in conducting an alternate, gear-down test plan), there were no major surprises in the airplane's behavior during the first flight. The only other disappointment was with the speed brake—when deflected its full amount (60˚), it caused more than expected air-frame shaking and buffeting and oscillations of the vertical tails (recall that the speed brake was located behind the canopy and forward of the verticals).



SPEED BRAKE AT 45 DEGREES

After several gentle maneuvers and engine throttle transient tests, the landing was made some 51 minutes after brake release. The post-flight de-briefing was conducted, and we then settled down to do some quick analyses of the data acquired and to un-wind in preparation for the First Flight Celebration planned for that evening at the Edwards Officers' Club.

In spite of an un-welcome interruption that afternoon responding to concerns of a hostile U.S. senator (more discussion below), the evening party was a huge success. Several dignitaries were in attendance including Florence "Pancho" Barnes, the famous aviatrix and proprietress of the "Happy Bottom Riding Club;" the World War II ace, C.E. "Bud" Anderson (who was employed by MCAIR at Edwards); and, I believe, "Chuck" Yeager of sonic barrier fame as well as having been Anderson's wing-man. (Yeager spent so much time at our facility that I don't recall precisely whether he was at the party or not). Test Pilot Burrows got the traditional dunking in the swimming pool. Our spirits were lifted, and spirits flowed freely as we let off months of built-up steam.

The afternoon phone call to address Senator Inouye's concerns continued to nag us, however. Inouye had for some time tried to terminate the F-15 program for some unknown reason. His staffers had apparently fed him information leading him to question the Pentagon about the F-15. There were six areas of concern, three of which were in my purview:

- Susceptibility to spins (autorotation at angles of attack beyond the stall).
- Speed brake-induced flow disturbances on the vertical tails (the magnitude we actually experienced was more than hoped for).
- Cross-wind landing with the relatively narrow landing gear (the effect of wind from the side while trying to stay on the runway).

All his questions were "no-brainers," the same questions anyone in the business would ask; we'd been addressing the same concerns since the beginning.

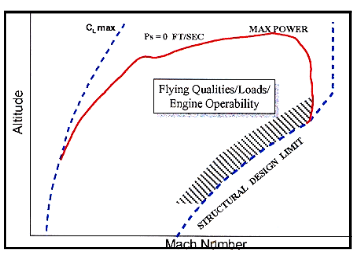
Eight or ten of us, including Don Malvern (MCAIR Program Mgr.), General Ben Bellis (Air Force Proggram Mgr.), Fred Rall (Air Force Engineering Mgr.), Skip Hickey (my SPO counterpart), and I placed a call to the then current Devil's advocate in the Pentagon in charge of answering the Senator's concerns. Fred, Skip, and I did most of the talking. Of course, we had been doing sub-scale model *spin testing and analyses* since 1969 and had a pretty good handle on the situation. The *speedbrake* had been reconfigured a couple times since the Proposal was submitted—from the initial position on the wing surfaces forward of the ailerons, to the tops of the engine nacelles, and finally to the position aft of the canopy and forward of the vertical tails—we didn't have much real data on its aerodynamic characteristics. We had studied the *cross-wind landing* characteristics at length with the tools then available—we had ample aerodynamic power. We assured the Pentagon that we were on top of each of the issues and that our test program fully addressed them. Further, we noted we had plenty of flexibility to change control laws with minimal impact on cost if required.

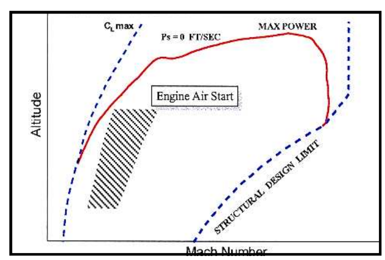
But we had more pressing issues to deal with than answering political concerns of some disgruntled senator. With first flight behind us, it was time to get down to the methodical tests and analyses (and development as required) to open up the flight envelope—nibbling away at the edges in a disciplined manner. There will be additional discussion of spins, speedbrakes, and cross-wind landing in the following narratives.

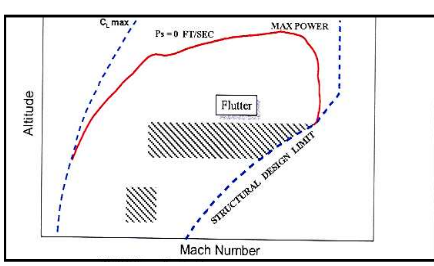
ENVELOPE EXPANSION

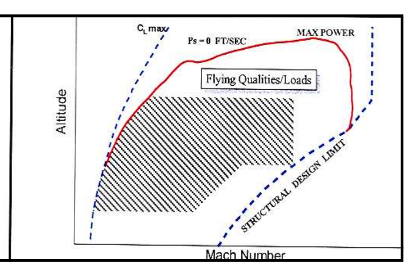
Every new airplane with the complexity of a modern-day fighter begins its flight test program with a script—a test plan of pre-determined test conditions to satisfy the needs of the various disciplines. Throughout the development effort, each discipline (e.g., Aerodynamics, Structural Dynamics, Propulsion, etc.) analyzes and performs ground tests to define how the airplane is expected to behave in a wide variety of conditions. With this process, portions of the flight envelope and certain maneuvers are identified as being more critical or risky (read "scary") than others. As a consequence, test matrices and aircraft instrumentation packages are defined so that all the known concerns are covered. Unfortunately, all this is too much to be accomplished on a singe airplane. Thus, the different airplanes in a test program are assigned to certain dedicated tests (see the Aircraft Assignments figure on Page 13). To further complicate matters, the earlier airplanes in the program are required to clear the path, or open the envelope, to those which are to follow with their specific test objectives. Each discipline determines the tests and test conditions needed to satisfy its requirements.

The Flight Test Engineer has the formidable task of putting all this together in the most efficient manner and defining the sequence of tests in order to proceed safely. Until the envelope is cleared for test operations, restrictions (or placards) are defined to avoid unnecessary risk. As testing progresses successfully, the envelope is cleared, and subsequent test objectives are accomplished. Of course, some parts of the envelope are more critical to some discipline than the others—the Propulsion engineers may be concerned with operability in the left-hand side of the envelope. The Flying Qualities and Loads engineers may be more worried about a different part of the envelope. Yet again, the Structural Dynamics engineers have concerns about different portions of the envelope where "flutter" may be encountered (Flutter is an unstable structural oscillation that can rapidly lead to catastrophic structural failure such as what occurred with the first Tacoma Narrows Bridge near Seattle). The list goes on and on.









In a perfect world, the test program runs smoothly, no placards are "busted" before their time, test points are completed efficiently, and the test plan never has to be modified. Unfortunately, in the flight test business, the real, imperfect world behaves differently. Surprises are encountered, something breaks, airplane modifications need to invented and incorporated, and test plans need to be changed.

My primary areas of interest during envelope expansion testing were with Flying Qualities and Flight Controls along with our on-site Guidance and Controls engineer, Loyal Guenther. The two of us were responsible for helping to assure safety of flight by making on-the-spot judgments about whether or not the airplane response to pilot input was as expected. And if the response was not as anticipated, we had to determine why not through our own on-site analyses or through communicating with the appropriate experts in St. Louis or with control components vendors.

Flight testing proceeded briskly the next few weeks. In the two months to the end of September, we had accomplished 65 flights with the first airplane. On six occasions, we completed three sorties per day. Five more pilots had their first flights in the airplane (Pete Garrison—9 August, USAF Lt. Col. Wendy Shawler—12 August, Pat Henry—8 September, USAF Major Cecil Powell—23 September, USAF Captain Mike Sexton—26 September). We at Edwards were overwhelmed with work. Help from St. Louis personnel was compromised due to data transmission problems in the system as well as cost reduction efforts to greatly curtail the amount of data to be processed. St. Louis did not have reliable data for such fundamental parameters as pitch control surface deflection until nearly the end of September. Most of the critical data analysis had to be performed on-site using the telemetry strip chart data. In short, we were on our own.

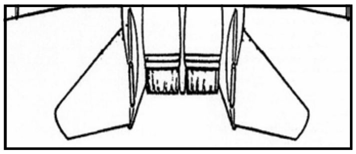
Our days were long. My complaints about needing more help fell on deaf ears until I put the matter in writing in my weekly formal report of 25 September 1972: **"I feel that our effectiveness (and the program) will suffer without additional manpower."** That one sentence produced results—within a month, I had Aerodynamics help in the person of Jack Massa. He remained with the program until having served a year and then returned to St. Louis in October 1973.

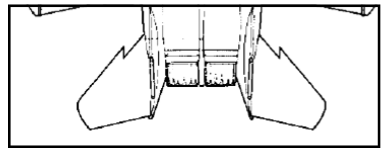
> Manpower
>
> With the sharp curtailment of data reduction requests, additional time is required to perform quickie analyses of the T/M data and to justify our requests. Currently, some 4 1/2 to 5 hours are required in direct support (pre-flight and post flight briefings, T/M monitoring during the flight, selection of data to be reduced, recording brief results of data not reduced, and justifying data requests) of each flight. Little time is left for trouble-shooting, trend analyses to be used for future flight planning, or staying abreast of where we've been and where we're going. When No. 2 A/C starts flying, the situation will be worse than intolerable. I feel that our effectiveness (and the program) will suffer without additional manpower.

Although there were many airplane problems uncovered during the first months of testing, most were minor and were relatively easy to correct. Indeed, some were even anticipated before first flight since actual hardware availability had in some cases not kept pace with the airplane design. Other problems were more serious and required considerable time to fix—crosswind landing characteristics, the directional control system along with low levels of directional stability, fuel system anomalies when transferring fuel from one tank to another, and airframe buffet / shaking at high g loads in the heart of the Air Combat Maneuvering (ACM) envelope. These problems and their solutions will be further discussed later.

One incident with the second flight test airplane, which had arrived on the scene in late September, could have resulted in its loss—a fan for the environmental control system disintegrated during the second flight (27 September) and severed a longitudinal (pitch) control rod.. This, of course, resulted in some unusual control responses. Fortunately, the pilot, Pat Henry, and Loyal Guenther in the T/M room recognized the general cause of the unusual response, and the landing was made using only the electronic control—what is called a Fly-by-Wire (FBW) situation for which the airplane was not designed.

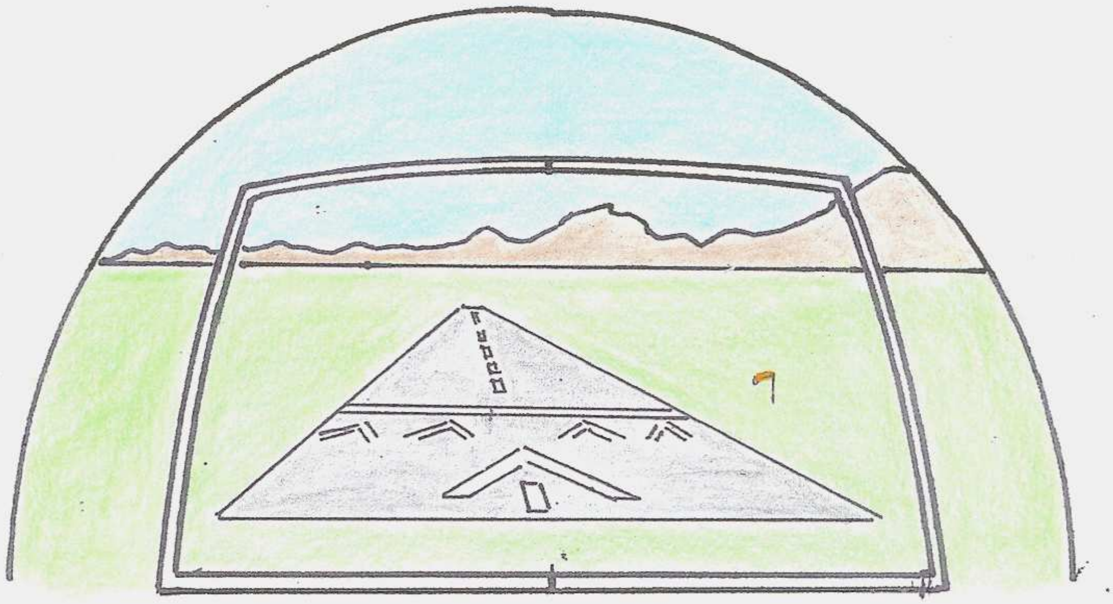
During this time, the Structural Dynamics engineers in St. Louis had discovered a horizontal tail flutter problem during wind tunnel testing of a dynamically scaled model (flexible model with structural dynamics which mimic the real airplane). To solve this problem, the inboard portion of the horizontal tail (stabilator) leading edge was removed. Although the effects on the aerodynamics were minor, the impact on the structural oscillation modes was dramatic. A quick fix to the real airplane stabilator was fabricated, and that modification known as a “snag” was successfully evaluated in flight.

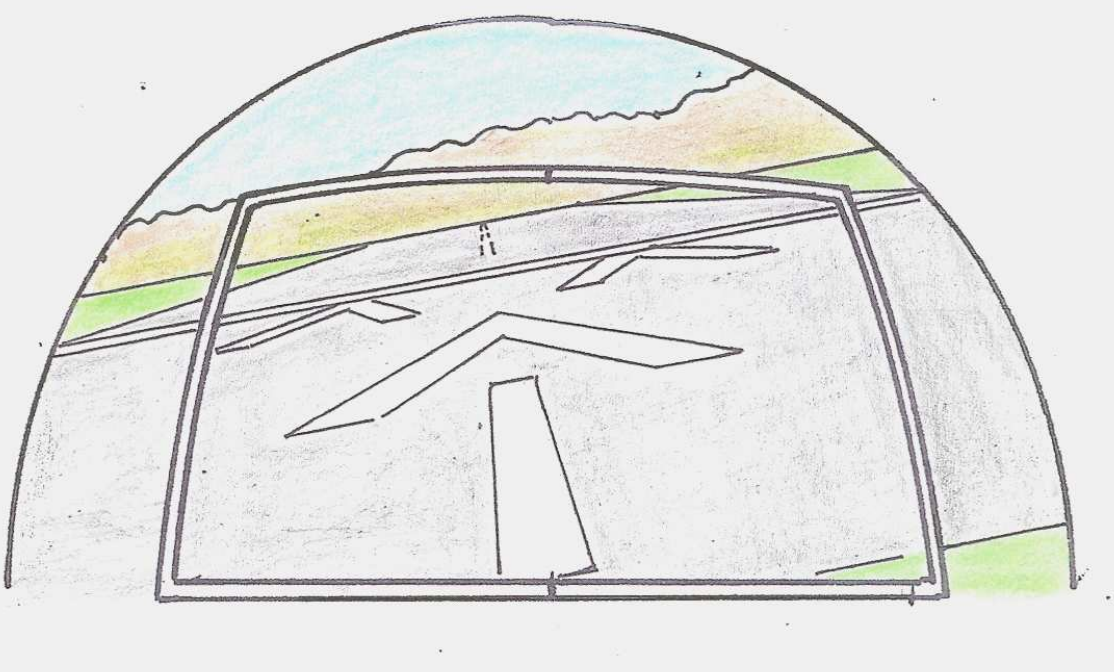




The milestone 100th flight with the first airplane was completed on 14 December 1972 (while the second airplane had completed 39 flights). Much had been accomplished in a short time—the test program had opened the envelope to 45,000 feet altitude at Mach 2.3, it had identified some skin structural problems at high dynamic pressure (high wind loads) corresponding to Mach 2 at 35,000 feet, the airplane had exceeded Mach 1.0 in Military power, most of the subsonic envelope with three external fuel tanks installed had been cleared, 360 degree rolls had been performed at supersonic speeds out to Mach 1.5, and high load factor (5.86 g) bank-to-bank roll maneuvers had been evaluated (experiencing a g load of 5.86 feels to one’s neck like wearing a 60 pound hat on your head). Perhaps most important, a successful formal Air Force Preliminary Evaluation consisting of eleven flights under Air Force control (with our assistance). Although 100 flights in less than five months was a significant milestone, there was little celebration. We still had plenty of work to do.

Jack Abercrombie
15 September 006



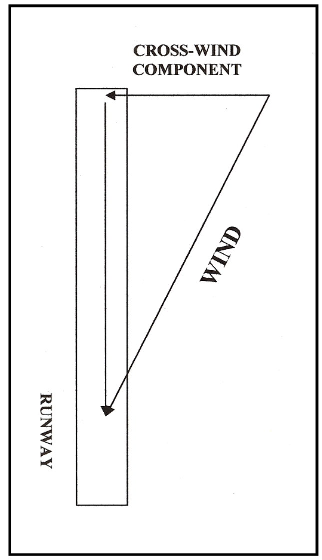


CROSS-WINDS AND SKATE BOARDS

The reader may wonder why cross-winds became an issue with the F-15. To provide the answer, it's necessary to briefly discuss some aerodynamics fundamentals, some flight control innovations incorporated in the airplane, as well as some landing gear structural issues.

CROSS-WIND KINEMATICS

A cross-wind is the *component* of the moving air mass which blows perpendicular to the runway centerline. Thus, for a runway heading of west (270˚) and a 20 knot (23 statute mph) wind out of the northwest (say 300˚), the cross-wind component is 10 knots (20 X sin [300-270]) perpendicular to the centerline.



Airplanes are, of course, designed to be at home in the air, not on the ground. Certain aerodynamic characteristics good for flight may not be good for ground travel, particularly in a cross-wind—directional stability as discussed on Page 5 in tending to keep the nose pointed into the wind is not desirable when trying to stay on the runway centerline if the wind is blowing from the side. Another desirable aerodynamic quality for flight, lateral stability (stability *about* the longitudinal axis extending from nose to tail), leads to a tendency for the upwind wing to rise relative to the downwind one due to the rolling moment or torque imposed. If the landing gear is not stiff enough or if the pilot fails to input enough roll control to counteract the torque, the airplane will tilt downwind, and the resulting inclination of the lift vector may "slide" the airplane off to the side.

GROUND LOOP

The combination of lateral and directional stability in a cross-wind will try to turn the airplane towards the upwind direction while "sliding" it toward the downwind side of the runway. The net result could be a turn-over, or what is known as a "ground loop."

PILOT TECHNIQUE

Airplanes are designed also to have adequate lateral and directional *control* to overcome the inherent *stability* as required. Pilots are taught to use the controls to handle operations in cross-winds. Most pilots are taught in most airplanes to accomplish cross-wind landings by making the *approach* to landing with the nose pointed toward the wind a sufficient amount to preclude any side force (the pilot's silk scarf would be trailing straight back) but with the airplane ground track in line with the runway centerline extension (the airplane would be approaching the runway at a skewed angle—the runway would be in view off to the side of the wind screen.

Then just prior to touchdown, the upwind wing is lowered, and the rudder is used to turn the airplane to align it parallel to the runway. At this point, the airplane is performing a "sideslip" toward the on-coming cross-wind component. The upwind landing gear will touch down first so that for a short time the airplane is banked slightly and rolling on one wheel (but not as in the above-discussed "ground loop").

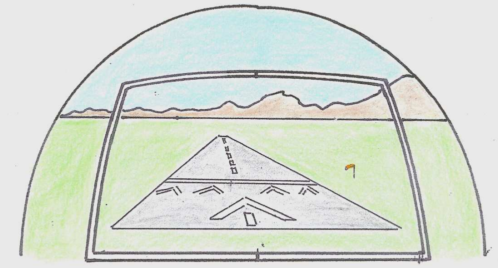
All this manipulation (wing down, top rudder) when well executed minimizes wear and tear on the landing gear by alleviating side loads and prevents the even worse ground loop situation from occurring. Of course, a well executed cross-wind landing is a mark of pride for the pilot. (I take pride in having completed a successful landing in my Cessna "Cardinal" in a 25 knot cross-wind but at about half the landing speed as for the F-15—about double the sideslip angle—it was a lot of work, and I wouldn't want to do it again).

BAD LANDING WITH HIGH SIDE LOADS ON GEAR

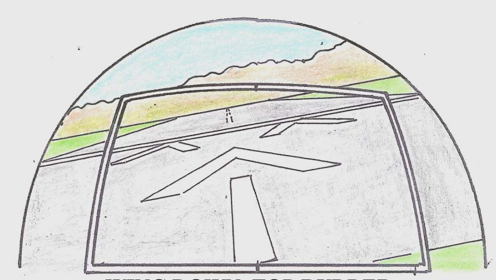
PERFECT "WING DOWN, TOP RUDDER" LANDING

F-15 SPECIFICATION REQUIREMENTS—CROSS-WIND

The F-15 specification required that it could safely accommodate landings in cross-winds up to 30 knots (34.5 mph) although an Air Force study had previously concluded that 25 knots would enable operational effectiveness 99.5% of the time based on environmental conditions for existing military air fields over the world (runways tend to be built so that they are lined up with the prevailing wind direction).



WINGS LEVEL CRABBED APPROACH



WING DOWN, TOP RUDDER

The accepted means of showing compliance with the requirement during the design stages is to be able to analytically generate the required sideslip angle for alignment with the runway using airplane *cross controls* (another way of saying, "wing down, top rudder"); e.g., *right* roll control, *left* rudder control, without exceeding certain maximum values of control stick force or rudder pedal force required of the pilot. Of course, the F-15 had "ample aerodynamic power" as I had stated earlier in response to Senator Inouye's concerns (see Page 16).

CROSS-WIND LANDING PROBLEMS

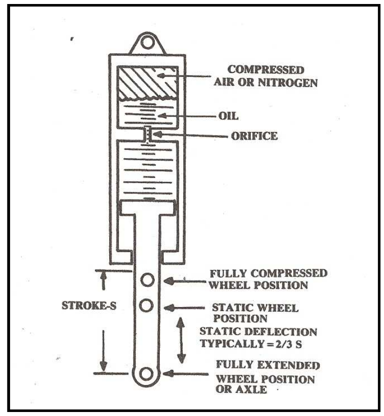
Alas, on the third flight of F-15 No. 1 (01 Aug 72), the pilot complained that the landing characteristics just after touchdown but prior to lowering the nose gear to the runway were very "uncomfortable" with the existing modest 8-10 knot cross-wind. The nature of the problem was poorly understood, and the recorded data from the instrumentation package shed little light. But the problem appeared to be associated with the harmony between the rudder pedals and the lateral (roll) control stick input. The Aileron-Rudder Interconnect (ARI) received much of the blame from some quarters. (Recall that I mentioned back on Page 5 the need to perform rolls with "feet-on-the-floor." This requirement was addressed through the incorporation of ingenious mechanical contrivances, the Roll Ratio Changer and the ARI. These devices provided rudder deflection as a function of roll control stick input while reducing the lateral control surface deflection for the higher angle of attack conditions—there will be more about these systems in a later chapter).

However, after this flight, Jack Wilcox reported that the main gears were stroked un-evenly with the gear struts stuck in positions which caused the airplane to lean to one side like an automobile with a broken spring. Later complaints included undesirable cross-wind characteristics in both the two-point and three-point attitudes, and asymmetric struts became quite common. It was obvious to most of us that more than *aerodynamics* and the flight control system was involved in the cross-wind problem.

By mid-September, a special task force was established in St. Louis to invent potential solutions. We began making control surface schedule modifications somewhat haphazardly at times, and we tested them as winds permitted. Means of rapidly destroying wing lift and firmly "planting" the gear on the ground were pursued. We evaluated trailing-edge-up deflections of the landing flaps, not because the aerodynamicists thought the scheme would have much impact, but because it was relatively easy to accomplish. A suggestion came out of St. Louis to "bolt on" some make-shift lift-spoilers to the wings and make high speed taxi test runs—this suggestion got no further than my ear on the telephone!

MAIN GEAR CHARACTERISTICS

Wilcox and I became more and more concerned that insufficient attention was being paid to the roll of the main gear in cross-wind landings. However, before proceeding further with this discussion, a few explanatory comments about gear characteristics in general are in order. Although landing gear technology was never my forté, I had to learn a bit about it.

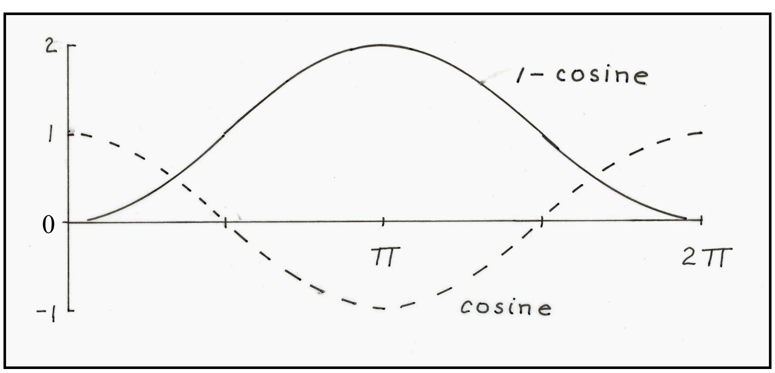


Modern fighter aircraft employ oleo-pneumatic (oil-gas) landing gear struts to soften contact with the ground. They do this by absorbing the energy at contact and smoothing out the bumps during the roll-out. In the simplest form, the landing gear shock absorber combines an air (or nitrogen) chamber which acts as a spring and an oil chamber with a small orifice at the end for the oil to flow through under pressure to damp the stroking motion (slow it down).

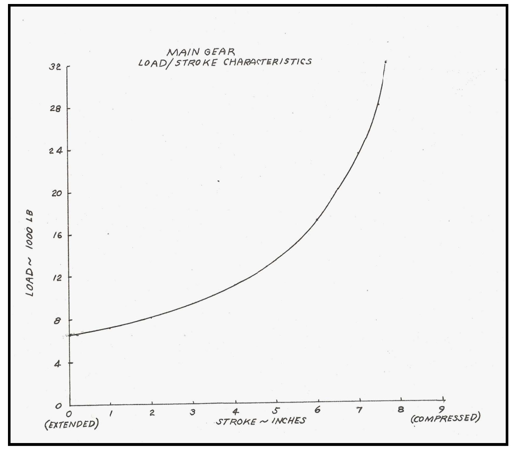
A more complicated form of shock absorbing strut may employ non-linear internal volume / stroke relations so that the spring action is non-linear (the more compression, the stiffer the strut becomes). There may be several chambers involved as well as metered orifices which change size as the gear strokes.

Of course, for high speed airplanes, more complications result from the need to retract the entire gear and button it up within the airplane mold lines. Other complexities may include wheel speed sensors and anti-skid brakes.

The F-15 gear was designed to absorb the energy resulting from a touch down of 15 feet per second vertical velocity (equivalent to dropping the airplane from a height of about 3 ½ feet—about the same energy involved as in dropping an automobile from the top of a three or four-story building. In addition, an unprecedented requirement was to be able to withstand the loads imposed by rolling over pre-defined rough surfaces. The Air Force had to construct a special speed bump facility at Edwards at significant expense to assess the F-15 capabilities when rolling over a "1-cosine" bump of specified dimensions at a certain gross weight and speed (I've forgotten what those conditions were).



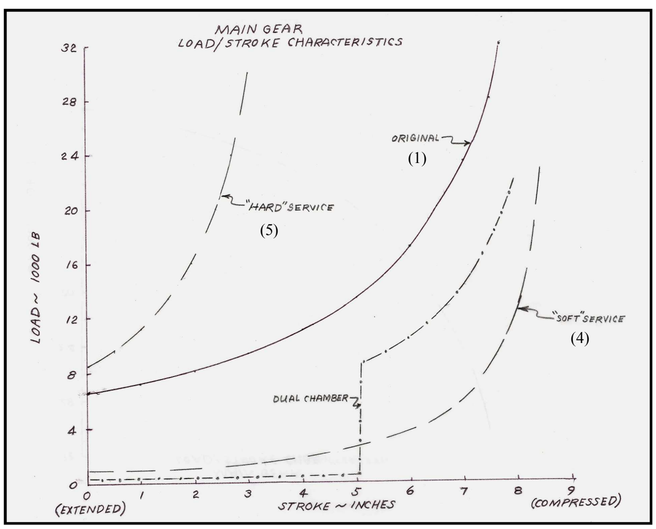
The F-15’s main gear strut design was characterized as a non-linear load-stroke strut with an initial break-out force (the point at which enough airplane weight has settled on the gear during landing to begin to compress from its fully extended position) of about 6500 pounds per gear. However, as already pointed out, the struts tended to stick at times so the point of breakout could never be assured.

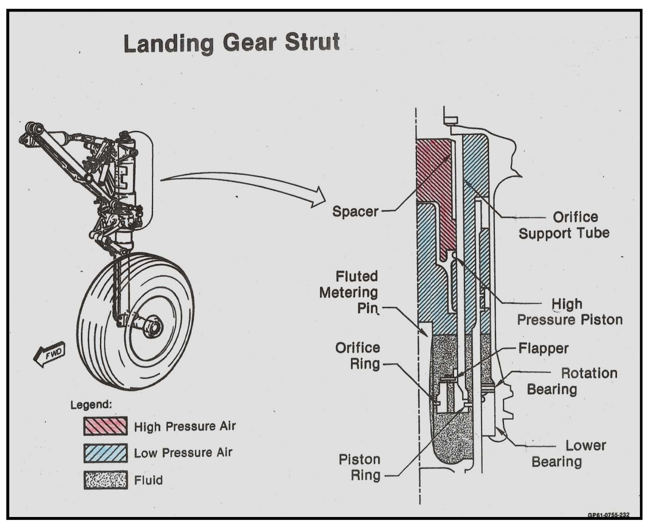


My weekly report of 14 November 1972 suggested that the main gear load-stroke characteristics should be modified to provide greater rolling resistance. By December, it was becoming obvious even to the skeptics that the landing gear was a big contributor to the cross-wind problem. The pilots were beginning to compare the airplane to a “skate board.” A skateboard would be O.K. for maneuvering along a winding road in no-wind conditions, but for traveling a straight line with a side wind tilting the airplane toward the downwind side, such characteristics are not desirable.

Several more formal suggestions for a revised gear design went un-heeded until March 1973. At that time, Wilcox and I put together an ordered program for evaluating different load-stroke characteristics with the existing gear to scope the problem and to provide data for *possible gear re-design.* During the subsequent test program, an on-board movie camera revealed that the main gear tended to toe out, or caster, when under load—*a skateboard, indeed!*

After another three months of preaching to St. Louis management that the gear we had would never satisfy all requirements, a decision was made to design a new, low friction, non-castering “dual-chamber” strut. The decision was made easier by a desire to move the gear aft nearly 3½ inches at the same time to improve the airplane “tip-back” tendency at aft centers of gravity.





CONTROL MODIFICATIONS

Meanwhile, we had been evaluating various control system modification schemes including disabling the ARI and giving the pilot full roll (lateral) control authority for ground operations. Although our preliminary design back in the 1970-71 time period had identified these full authority features as being good for cross-winds, it had been decided to minimize design complications unless testing proved we needed them. In addition, because of concern for potential safety of flight problems during the transition from “up and away” flight to the landing roll-out, there was considerable debate about the proper controls logic for providing full control for *landing* without compromising *flying* characteristics.

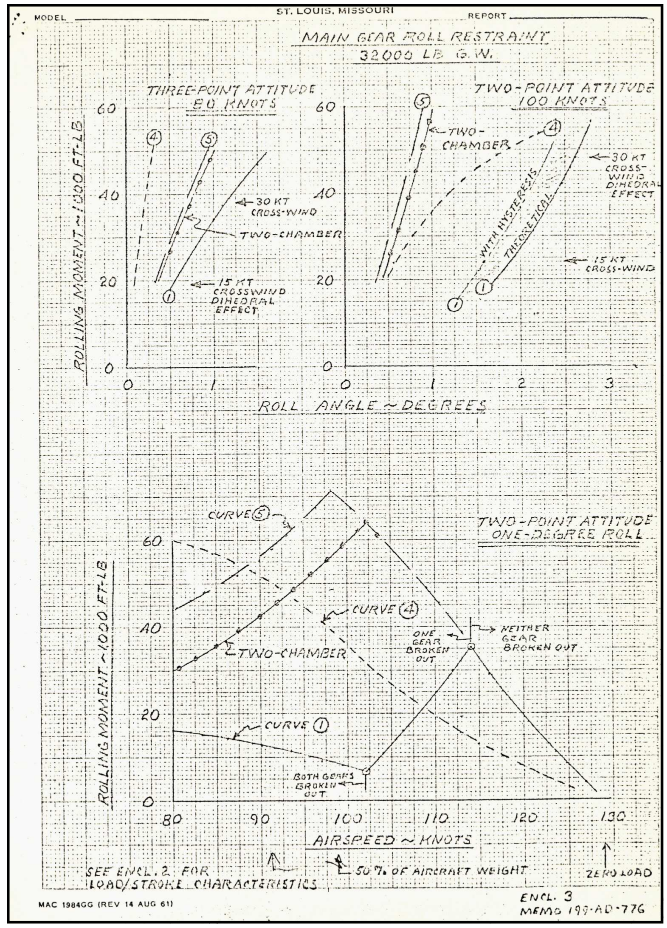
SUMMARY

The cross-wind landing program was conducted over more than a one-year period. It involved 50 flights using several test airplanes and about 20 airplane system variations.

CROSSWIND LANDING MODIFICATIONS EVALUATED

- Three variations of the main landing gear load/stroke characteristics with the original single-chamber struts (see Enclosure (2)).
- One load/stroke curve with dual-chamber struts (see Enclosure (2)).
- Elimination of the mechanical lateral control authority washout with ANU stabilator deflection.
- Elimination of the CAS lateral control authority washout with angle of attack.
- Increased maximum CAS lateral authority ("super" CAS).
- Elimination of both the mechanical and CAS aileron-rudder interconnect.
- Parabrake (drag 'chute).
- Flap "snatch" to trail position as well as to 30° trailing-edge-up (symmetrically and asymmetrically).
- Two variations of nose-gear load-stroke characteristics.
- Three variations of the nose-gear steering gain.
- Two levels of mechanical rudder authority.
- Two longitudinal locations of the main gear.
- Two variations of the longitudinal stick force characteristics.

(From JMA memo 199-AD-776, 14 Nov 75)



The three most significant modifications which were subsequently incorporated into production were:

- Elimination of the mechanical ARI at wheel spin-up.
- Achievement of full mechanical lateral control authority upon activation of the "gear down" handle.
- Incorporation of the low-friction dual chamber main gear struts.

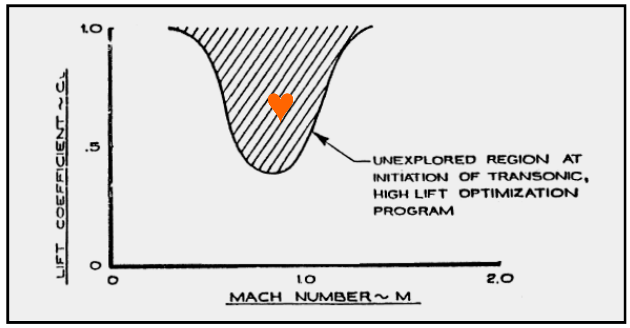
In order to test at high cross-wind conditions, we had to utilize the dry lake bed so we could pick any "runway" heading we desired. Even then, we were unable to realize more than a 25 to 28 knot wind. However, the specification cross-wind requirement of 30 knots was formally satisfied during USAF Qualification testing. Airplane No. 7 on its 310th flight had adequate cross-wind to perform the evaluation—it landed successfully on the paved runway in a gusty 28-37 knot cross-wind. The pilot described the event as a "normal full flap landing."

Although I regarded the cross-wind problem as more of a *frustration* than a *technical challenge,* had the problem gone unsolved, there would have been serious results at some point. The changes to the control system apparently have not compromised safety of flight in the transition phase from flight to touchdown during the nearly 35 years of F-15 flying. The landing gear re-design, although it was a hard sell to management and was quite costly, turned out to be as effective as the control system modifications in solving the cross-wind landing problem.

Jack Abercrombie
28 November 006

THE SAGA OF THE F-15 WING TIPS

During normal envelope expansion testing following first flight, we began exploring the limits of the airplane by working up in small steps of Mach number, angle of attack, maneuvers, engine throttle transients, etc. One of the primary performance requirements for the F-15 was to be able to excel at climbing and accelerating/decelerating in a dog-fight environment. Much of our wing configuration investigations during the preliminary design effort (discussed on Page 3) was aimed toward achieving specified performance in the **heart** of the air combat maneuvering (ACM) arena.



When we first penetrated this region (Flight 26 on 25 August 1972), the test pilot, Pete Garrison, complained that in a very small portion of this envelope (Mach ≈ 0.9, about 4.5 g, 30,000 ft. altitude), the airframe shaking was so bad that he couldn't read the cockpit instruments—they were blurred due to vibration. This limiting condition was very bad news—no matter how good the performance parameters (e.g., rate of climb) might be, the airplane was virtually unusable in this small but so important portion of the flight envelope. Thus began the saga of the F-15 wing tip.

***

Before proceeding further with this story, a little bit of *Aerodynamics 101* may be in order. But it's perfectly permissible to skip over the next couple pages.

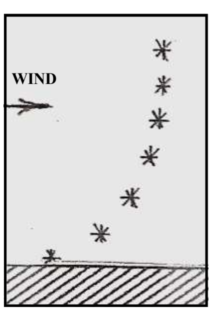
Some of the fundamental truths (or almost truths) put forward in *Aero 101* are:

- *Air is sticky.*
- *Wind is stronger on top of the hill than on the plains below.*
- *A mix of subsonic and supersonic air flow over the surface of an airplane is impossible to completely analyze.*

And someone expressed a "Principle of Equivalency" which notes that:

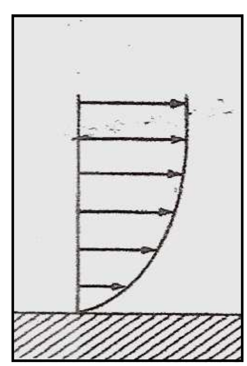
- *The aerodynamics of an airplane moving through a stationary mass of air are identical to those of an airplane fixed in space with the air moving over it (ignoring inertia effects).*

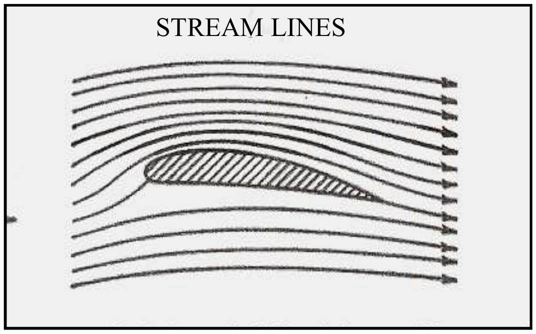
In other words, a 100 mph wind blowing over a stationary object is equivalent to the object traveling 100 mph in no-wind conditions. This simplification makes it a lot easier to explain things.



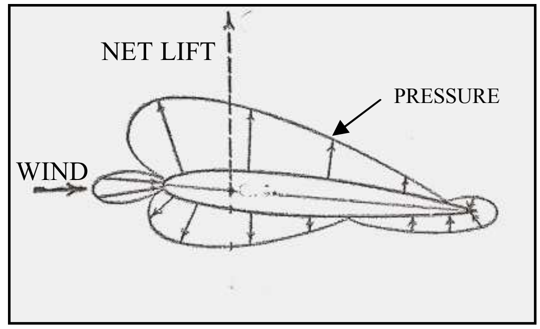
The "stickiness" of air, or "viscosity," can be demonstrated by throwing a handful of dandelion seeds into the air in a mild breeze. The floating motion of the silky floss will be noticed to be slower near the ground than at the higher elevations—the stickiness or friction of the air on the ground slows the breeze to zero speed at the ground surface. Above the ground, the floss moves faster until at an elevation of 50 or 60 feet, the velocity reaches its maximum—the free stream velocity. This is depicted in the sketch to the left.

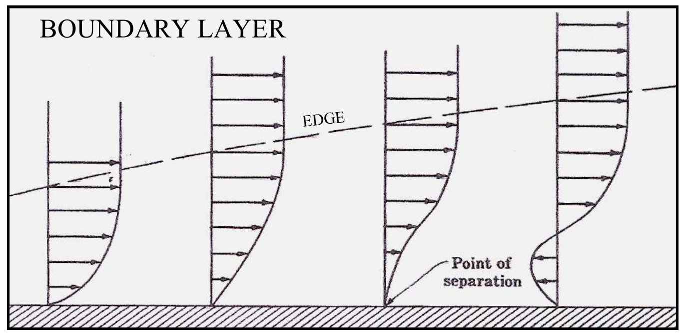
Replace the floss * symbols in the sketch with arrow heads, and draw in some arrow shafts extending back to where the velocity is zero. The resulting sketch, on the right, is a vector representation of a "boundary layer," and the arrows represent "stream lines." The edge of the boundary layer (defined for convenience as where the velocity is 99.5% of the free stream value, represents the boundary layer thickness. The thickness is, of course, much smaller on an airplane wing than on the ground. Wing boundary layers are normally inches, or fractions of inches, thick.





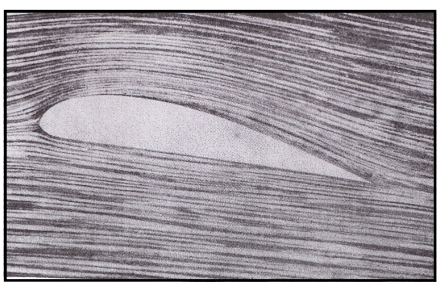
Stronger wind at the top of the hill has nothing to do with friction. The increased wind speed results from the physical constriction of the area for the air to flow and the resulting increased distance for a given "chunk" of air to travel. In order to keep up with its neighbors way up in the free stream, the chunk needs to travel faster. A faster velocity creates a region of reduced surface pressure. Replace the hill with the convex curvature of a wing airfoil section—it can be illustrated how the stream lines become closer together and the pressure on the surface is reduced. As a consequence, a net "lift" is experienced. The same phenomenon, the "venturi" effect makes it possible for a carburetor to work or an old-fashioned perfume atomizer to do its job.



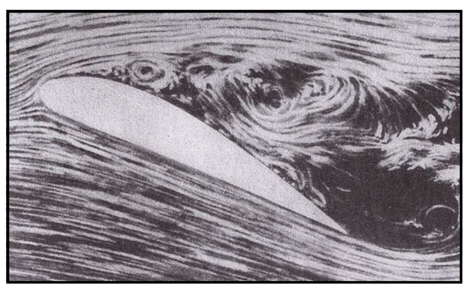


If the back side of the hill or wing surface is too steep, the pressure increases at a high rate, and the boundary layer becomes so thick that the sluggish air within the boundary layer cannot negotiate the turn. It "slides" of, and the air next to the surface actually reverses direction. The point at which this occurs is the air flow "separation" point.

Associated with air flow separation is buffeting, a loss of some lift, and an increase in drag—wing "stall." Sometimes stall can be very abrupt, but normally, with proper design, stalling is gradual so that the airplane can be easily controlled and brought back to un-stalled flight conditions. The buffet onset is actually a good indicator to the pilot that stall is approaching.



UN-STALLED



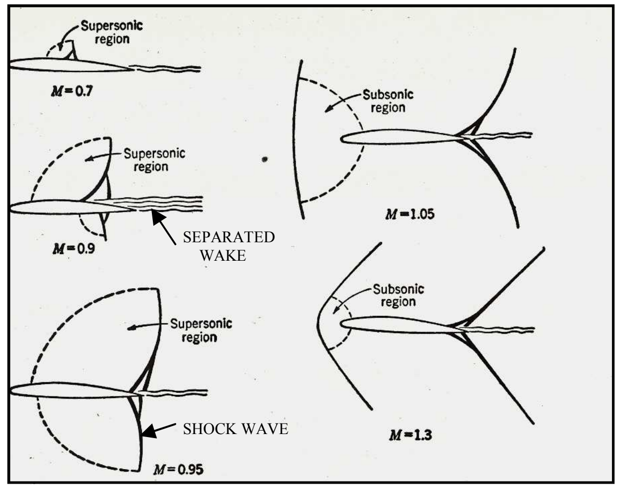
STALLED

All fairly straight-forward so far, but it becomes complicated when part of the air flow over a complex airplane shape is subsonic and part is supersonic. This "transonic" regime becomes more complicated when the real-world effects of viscosity are considered.

Now we need to discuss "sound" and "shock waves." It is generally well known that "sound" results from the action of very small pressure waves traveling through the air by agitated air molecules striking their nearby neighbors which, in turn, strike other molecules, and so on and on. These waves continue traveling even farther from the source of the sound pressure pulse (as with the wave motion from a pebble dropped onto the surface of water). It may not be fully appreciated that even in still air, molecular activity is virtually continuous. Fill a container with air—the molecules bounce around hitting each other and the walls of the container like bouncing ping pong balls (or like *pairs* of ping pong balls stuck together—N_2 and O_2 primarily). Although the velocity of a particular molecule varies during its trajectory, the *average* speed of all molecules is known as the "speed of sound".

If the closed container is heated up, the molecular agitation becomes more intense (the speed of sound increases), the molecules strike each other at higher speeds, and the collisions with the container walls occur at higher energy levels—resulting in an increase in air pressure. At some point, the pressure may become so high as to burst the container. Contrariwise, cool the air a sufficient amount and the average speed of collision (speed of sound) decreases. At increasingly lower temperatures, the molecular motion reduces until at *absolute zero* (about –460 °F), there is *no* molecular motion (as if the molecules were all huddled in a corner to keep warm), and the internal pressure reaches a perfect vacuum. Of course, under this condition, there could be no "sound."

When a solid body (e.g., a wing) moves through the air, it generates infinitesimal pressure pulses much like sound. These pulses, of course, travel at sonic velocity in every direction from the moving body and more or less notify the air mass ahead that the body is approaching. As a result, the air "moves aside" to allow the body's passage as was illustrated in the previous sketches of streamlines around an airfoil. At some point the higher airspeed regions of the flow around some portion of the wing airfoil may exceed the speed of sound; e.g., where the streamlines are closer together "at the top of the airfoil hill." This may happen even thought the airplane, itself is still subsonic. In this circumstance, the flow of air approaches the airplane subsonically and is accelerated to supersonic conditions. Farther along the airfoil, the local flow again becomes subsonic. The boundary between the super and subsonic regions marks an abrupt change in thermodynamic properties—a "shock wave"—through which the air pressure rises almost instantaneously (it is this phenomenon that creates a "sonic boom"). The abrupt rise in pressure adversely affects the boundary layer and may cause airflow separation similar to that previously discussed but now defined as "shock induced" separation.



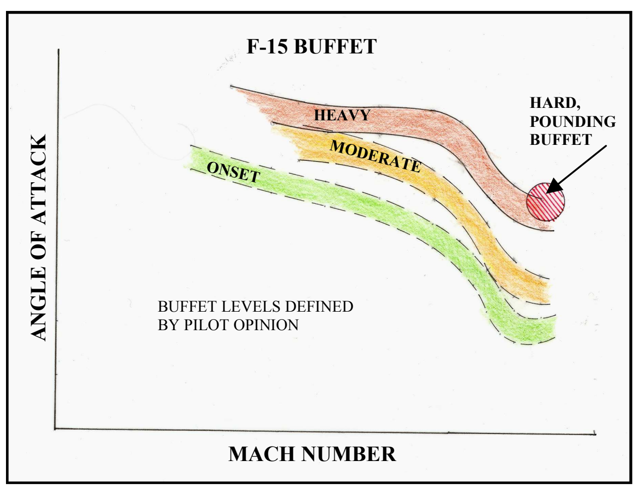
Airflow separation phenomena for complex shapes are extremely difficult to analyze mathematically, particularly in the transonic regime. Empirical studies and wind tunnel tests can help in understanding and ameliorating the effects of separation, but a thorough understanding can be obtained only through flight testing in the real world. That is as true now as it was 40 and 50 years ago.

So much for *Aerodynamics 101*. Back to our story.

***

PROBLEM DEFINITION

The F-15 airframe buffet problem was described as "hard, pounding buffet" which was so bad, the cockpit instruments were so blurred as to be unreadable. It was so bad that we were reluctant to pull up to higher angles of attack to determine if the situation changed for the better (or worse). We were able to determine that the problem existed in a very small portion of the Mach/altitude envelope—in the neighborhood of 0.9 Mach and 30,000 feet. It hit hard at about 4.5 g load factor, right at the threshold of the main part of the ACM envelope for which we had spent so much effort in the design. Although limited investigations were conducted during the next three or four weeks, it was not until the Air Force Preliminary Evaluation (AFPE) debriefing of 4 October 1972 that much excitement was generated within the company management.



By 10 October 1972, strain gauge data had become available—strain gauges can measure even minute changes in the length of structural members. The data indicated that the vertical tails were *not* the cause of the problem as some had suggested (there were still some who wanted to return to the original empennage configuration), but that the hard buffet correlated with major, large amplitude load oscillations on the outboard portions of the wings, and I reported this in my weekly status report. These and additional strain gauge data obtained on six more flights were transmitted to St. Louis for (presumably) in-depth analyses. The cursory analyses we were able to do at Edwards reinforced the conclusions this writer had made. In addition to the airframe buffet, a slight pitch-up tendency which had been noted by the pilots was found to occur simultaneously with the hard buffet—strongly suggesting that separation was occurring on the wing tips. It had all the earmarks of ***shock induced*** *separation.*

For the next several months, we (at Edwards) concentrated on other problems including speedbrake buffet, cross-wind landings, and supersonic directional characteristics. But in February 1973, the buffet program was re-started with even more instrumentation (wing tip acceleration, wing surface pressures outboard of the flap/aileron intersection, empennage dynamic loads, and surface tufts for flow visualization). The data obtained with this additional instrumentation again revealed no other contributor than outer wing panel load oscillations. (The tuft photos revealed nothing of note).

A very extensive wind tunnel test program was established in July 1973 to evaluate about every configuration modification that the St. Louis management people could dream up—wing leading edge snags and notches, vortilons, strakes, fences, etc. in hopes of finding some "magic" to solve the problem. Some of those things sometimes work for subsonic flow problems, but rarely for those in the ***transonic*** regime. St. Louis management (a team of about half a dozen from Branch Chiefs to Vice Presidents) even added wind tunnel test configuration changes including horizontal tail anhedral, decreased horizontal tail span, etc., etc.—still looking for magic.

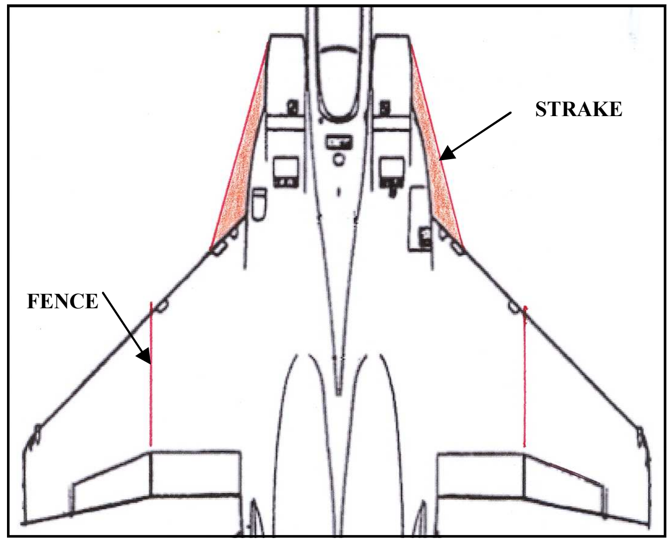
On 10 September 1973, this writer suggested to the Edwards Technology Integration supervision (Hank Rechtein) that ***we fly one of the airplanes with the outer wing panels removed.*** There is a structural break just outboard of the ailerons—the modification could have been accomplished quite readily. Shock induced separation problems can often be eliminated by an increase in wing taper ratio (the ratio of the tip chord length to that of the root chord—the chord being the distance from leading edge to trailing edge). This suggestion was not well received—MCAIR was very proud of the 1969 design effort in selecting the F-15 wing planform; as noted on Page 3, the design effort involved hundreds of wings tested in wind tunnels as well as a Computer Aided Design Evaluation (CADE) process to help select the winner. A suggestion to change the wing planform was tantamount to heresy!

At a Management Review held at Edwards in late September 1973, a decision was made to shift the buffet investigations to F#9 which was still in St. Louis having recently left the assembly line. Curiously enough, also at this same Review, the St. Louis-based Loads Engineer, J. T. Johnston, offered his conclusion that the ***hard buffet was coming from the wing instead of the empennage!*** based on analysis of data from F#4. ***(This writer and meeting attendee commented sarcastically, "How about that!").***

The tests planned for F#9 in St. Louis included such items as wing fences, gun bump fences (the gun bump being the fairings forward of the intersection of the wing and nacelle), wing leading edge droop, stall strips, wing contour changes, gun bump strakes, vortex generators, fuselage strakes, etc. I never became familiar with many of the many items which were dreamed up. However, at this time, I reiterated by telecom to my St. Louis supervision that I felt the only big improvement would realistically be due to such things as a lower aspect ratio, a higher taper ratio, and a higher wing sweep back angle—all typical solutions to shock induced separation phenomena. ***I added the query, "Why not clip the tips off?".***

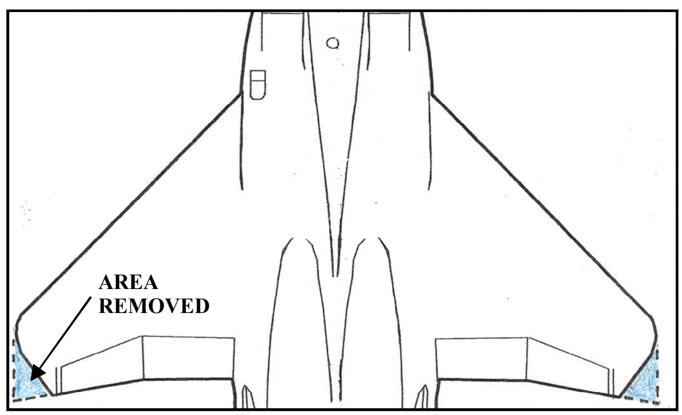
After a few St. Louis flights with F#9, some of the St. Louis engineers (although not the Aerodynamicist in charge) were beginning to conclude that the problem was due to ***shock induced separation.*** Further, it appeared that the separation was exciting the fuselage first bending mode at 8.5 to 10 Hz, or cycles per second. (Every elastic body has several vibration modes, much like a violin string with its first, or fundamental, mode, and perhaps several harmonic modes).

On 29 November 1973, my St. Louis supervisor, John Mello, reported a "break through" on the buffet—a four-inch high wing fence and a "gun bump" strake appeared to have eliminated the hard, pounding buffet based on three flights by test pilot, Charlie Plummer. (*Fences* as used on airplane wings are merely flat, thin blades mounted vertically along a butt line to block the flow from moving laterally. *Strakes* are similar but mounted horizontally usually along a waterline—somewhat akin to a splash rail on a boat hull). This break through was substantiated with flights by Jack Krings and Joe Dobronski. However, further tests to refine the configuration continued—apparently many "hot showers and cold douches." In late February, a "***final***" buffet improvement configuration was selected from the many St. Louis candidates—wing fences at the inboard edges of the ailerons and strakes on the gun bump.



However, other things were happening somewhat behind the scenes. In early March 1974, a decision was made (*very quietly*) by St. Louis VP Pete Ramey and agreed to by Edwards management to ***clip the wing tips*** of F#4 to investigate a perceived loads problem—high bending moments on the outer wing panels at a M=1.02, 20K feet altitude condition. There was fear that the "final" selected buffet improvement modifications would make things worse. I don't think the Air Force was made aware of this decision until some time after the fact.

So, at the direction of a VP who had a *structural* background, the wing tips of F#4 were clipped off in the dark of night by simply sawing off some 4.5 square feet of the aluminum honeycomb from each wing tip using a "Saws-All" and smoothing the ragged edges with duct tape (the shape of the cut was mostly defined by Edwards designer, Bob Grogan, so as to retain the wing tip navigation lights. This circumcision occurred about ***seven months and many flight tests/wind tunnel tests after virtually the same suggestion had been made by the Edwards Aerodynamics Project Engineer*** for the purpose of eliminating the shock induced separation problems—not to solving a loads problem for a different flight condition.



In mid-March 1974, F#4 made flights 153 and 155 with Pete Garrison as the pilot to investigate the impact of the clipped tips on the hard buffet. My report of 18 March 1974 in regard to the clipped tips noted that:

> ***"<u>Buffet</u>. Although it has long been anticipated that clipping the wing tips would improve the limiting buffet situation, it may be that the clipped tips have completely eliminated the problem…"***

On a later flight, Pilot Charlie Plummer who had flown nearly all the various buffet "fixes" found the clipped tip configuration almost as good as the so-called "final" fence and strake. Meanwhile, the Air Force had flown the "final" fence and strake configuration and was happy with the buffet but were concerned about the potential of cost and impact on airplane performance. As it turned out, the cost and effects on airplane drag were considerable. Consequently, in late April 1974, it was decided to incorporate the clipped wing tips into production, and the fence/strake configuration was dropped.

During a trip to St. Louis on 8 May 1974, this writer was admonished for referring to the wing tip configuration as ***"clipped"—***from that day forward, they were referred to as ***"raked"*** per Pete Ramey's direction.

Thus ended the F-15 wing tip saga.


BT
061

SPEEDBRAKE INDUCED FLOW DISTURBANCES

Every U.S. operational jet powered fighter, with but two exceptions, has incorporated a speedbrake (earlier called dive brakes or air brakes) function.

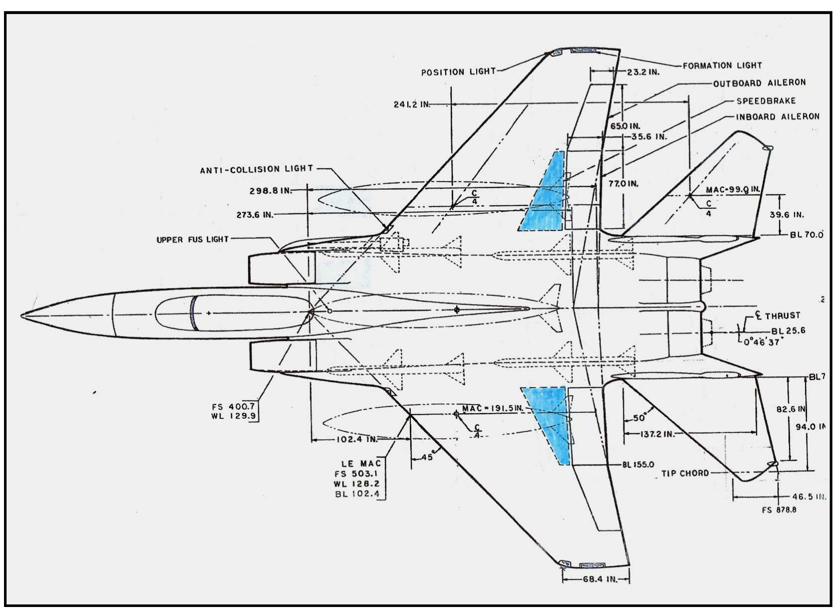
One of the two exceptions was the 1942 P-59 "Airacomet, the first American jet fighter, one of which is displayed on a pylon at Edwards AFB. The P-59 performance was so poor, it didn't need a speedbrake! The **P**-59 (fighters in those days were designated with "P" for Pursuit) never actually saw service as a fighter. Instead, all P-59s were utilized for training (a "T" designation would have been appropriate). The other exception to the list is the F-117 so-called "stealth fighter"—it has been used only for air-to-ground operations (a "B" for Bomber or "A" for Attack would be better designators for the **F**-117). Of the remaining jet "fighters", the F-105 and the F-111 may as well be eliminated from the list since they were really designed for the ground attack roll.

The newest fighter, the Lockheed F-22 which became operational in December 2005 (173 months—nearly 14 1/2 years—after contract go-ahead contrasted to only six years for the F-15), has no speedbrake *per se.* Instead, it utilizes a combination of symmetrically deflected ailerons (both trailing edges up), wing flaperons (trailing edges down), and "mirror-image" twin rudder deflections (trailing edges outboard). Such unconventional use of the primary flight control surfaces is easily accomplished with digital fly-by-wire (no mechanical push-rods, cables, etc.) technology which was not available for the earlier airplanes. (The 1978 McDonnell/Northrop F-18 uses mirror-image rudder deflection to augment pitch control in some portions of the flight envelope).

All of the remaining 30 or so basic fighter configurations (not counting the "almost-look-alike" derivatives) had/have *dedicated* speedbrake surfaces—essentially flat plates—used for no other purpose than to increase aerodynamic drag as required.

I present this background to illustrate that speedbrakes were nothing new when the F-15 came along. However, the speedbrake capability *requirements* imposed on the F-15 were unprecedented. The specification requirements were based on idle engine power deceleration from non-afterburner maximum speed at 25,000 feet (about Mach = 1.0) to 80% of that speed as well as on a descent from altitude to sea level—both in specified minimum times. It had been shown in air-to-air combat simulations that the airplane that could satisfy these requirements had a significant advantage, all other parameters being equal.

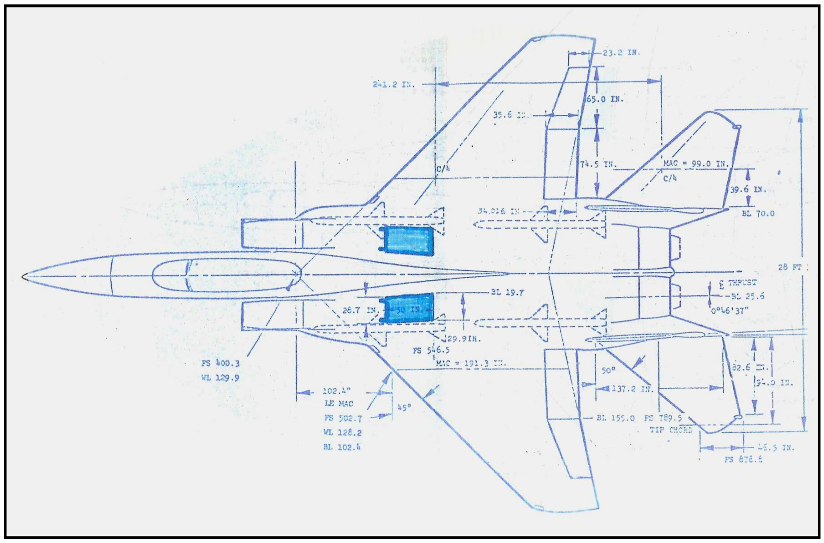
Satisfying the deceleration requirement was equivalent to being able to generate about a 1/2 g "eye-balls-out" g-force with speedbrake deployment (about the best one can do braking a modern automobile is about 3/4 g on a hard, dry pavement).



So, the F-15 speedbrake requirements were most significant. However, speedbrake design was fairly low on our priority list during the Preliminary Design phases. Choosing the location for the speedbrake was a difficult chore—the attitude of most designers seemed to be, "Not in my neighborhood" because the mechanism would eat up space perhaps more suitable for fuel, primary flight controls, engine air induction systems, etc.

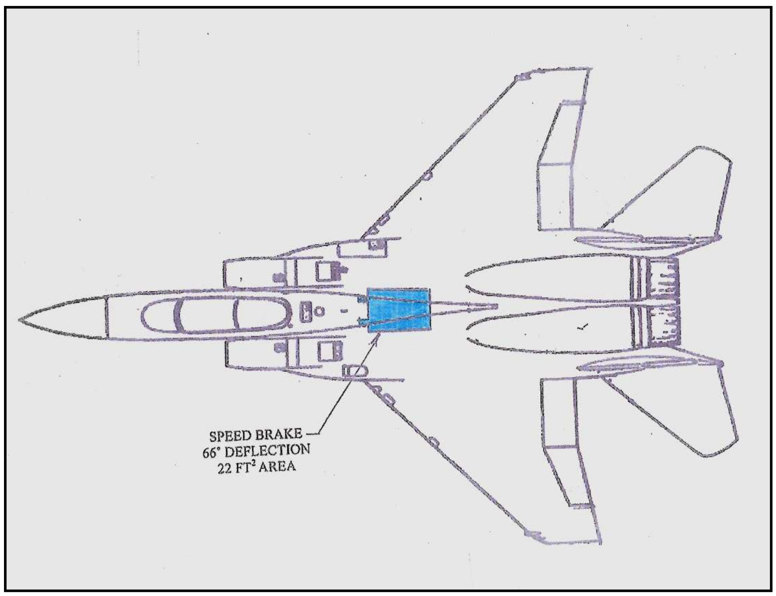
The June 1969 Proposal configuration showed two ***lower*** wing surfaces similar to the design used for the earlier F-4 Phantom airplane. The combined surface area was 27.5 square feet, and the surfaces were located just forward of the wing flaps. But wind tunnel tests in the

Summer of '69 revealed an excessive amount of pitching moment due to brake deployment. Thus, by December '69, the two surfaces had migrated to the tops of the engine nacelles with a total surface of 20 square feet.

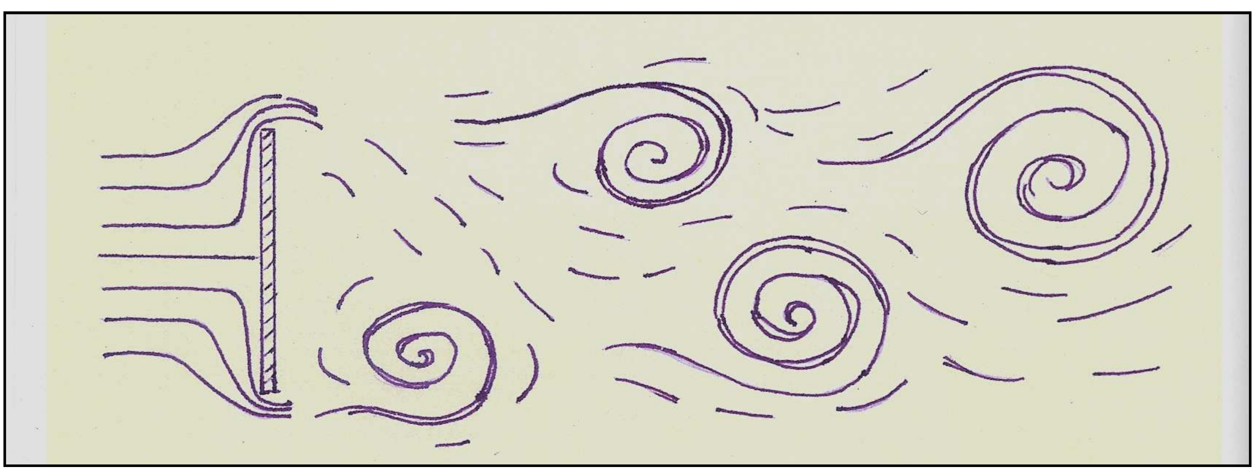


In a matter of weeks, the two surfaces had blended into one surface of 22 square located on the top centerline behind the cockpit and well forward of the empennage—the engine inlet control system had won the priority battle. There it remained through Detail Design and Fabrication.

The first 12 aircraft were built and flown with this centerline configuration which had a deflection capability of 66 degrees from the stowed position (Page 15 notes a deflection of *60* degrees—at one time, the reference was to the amount of deflection relative to a waterline, an arbitrary reference line used for lofting boats and airplanes. Later, we adopted a more conventional nomenclature: 66 degrees from the retracted position).



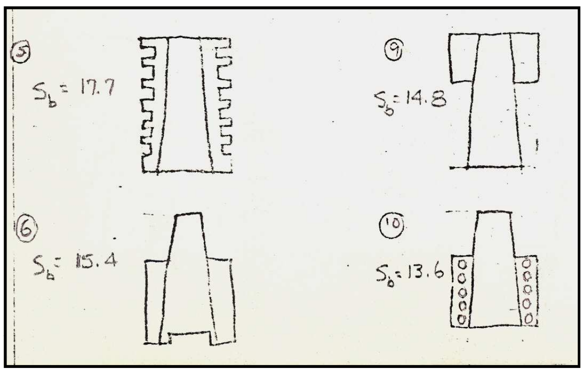
The planform resembles that of a sheet of plywood (and about 70% the size of a 4 x 8 ft sheet). As a matter of fact, the aerodynamic characteristics were similar to those of a sheet of plywood—with a massive separated wake behind it when deflected the full amount. Such flow disturbances can cause a large amounts of buffeting for anything in the wake—somewhat akin to driving behind an 18-wheeler traveling at highway speeds.

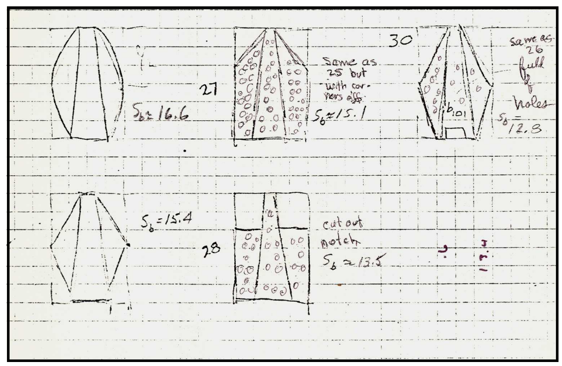


Test Pilot Irv Burrows was not at all happy with the speedbrake on the very first F-15 flight, although with what seemed like the whole world listening, he didn't transmit much information during the flight nor say much at the official flight de-briefing. He did comment that at full speedbrake deflection, there was significant buffet with the vertical tails shaking at an estimated four or five Hz (cycles per second). The chase pilot estimated the shaking at about 15 Hz. A further pilot complaint was the lack of precise control of deflection with the speedbrake switch.

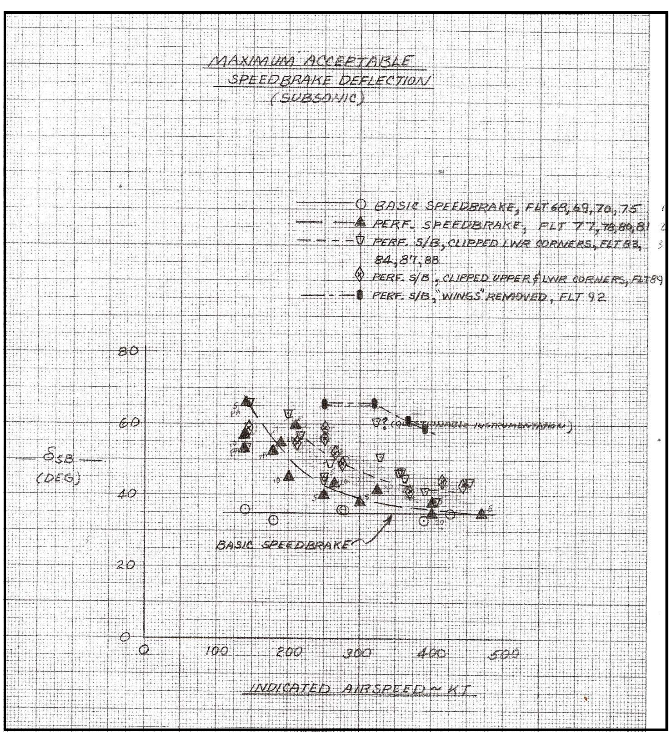
We had little instrumentation on the test aircraft that could specifically nail down the relationship of speedbrake wake and subsequent interference with the vertical tails, but it was obvious in this case that the "tail was wagging the dog." Immediately, the switching was modified to prevent deflections beyond 35 degrees, and plans were generated to solve the problem.

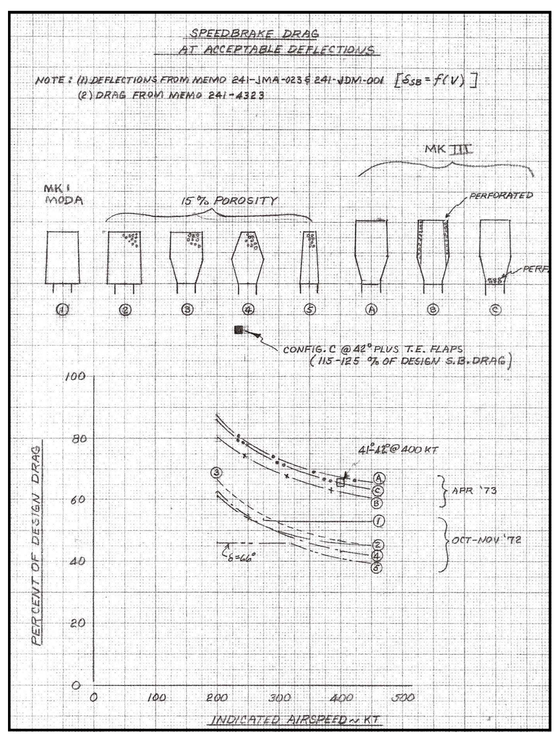
By mid-August '72, St. Louis was performing wind tunnel tests to define speedbrake configurations that would satisfy both drag requirements and the need for improved buffeting characteristics. These tests used fully instrumented, dynamically scaled aeroelastic vertical tails so that the effects could be quantitized. Concurrently, drag tests were being performed in the high speed wind tunnel. An incredible number of configurations was tested including planform shape variations, different degrees of perforations (holes through the speedbrake surface), slots, notches, etc. At least 30 different configurations were evaluated through a range of deflections. Although all the various shapes appeared to reduce buffet intensity, not many made the grade for the required drag levels.



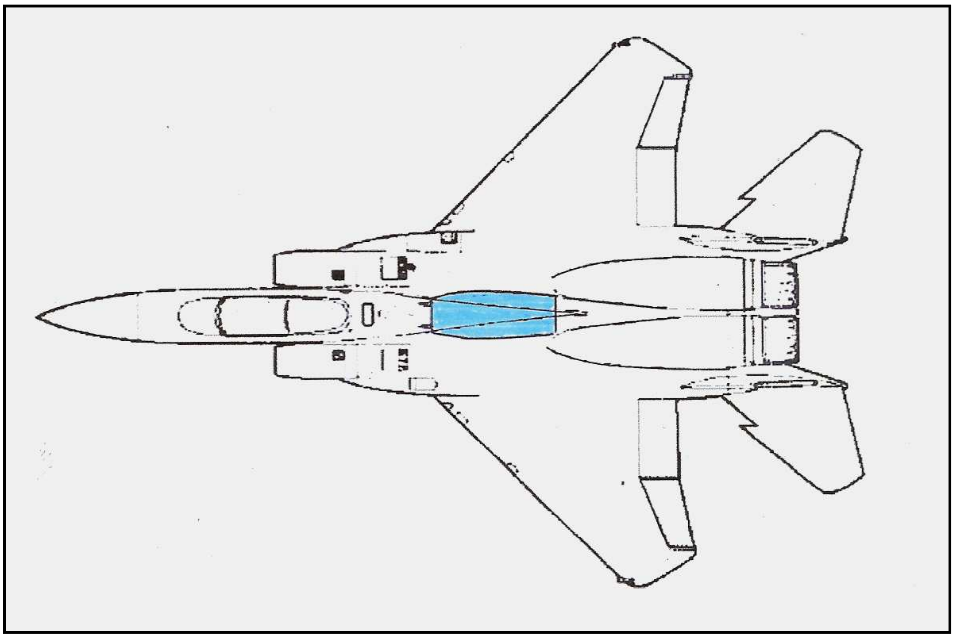


By mid-October '72, alternate speedbrakes were available for flight test. We used several different pilots to attempt to average out pilot variance, and, as test priorities changed, we used different airplanes as well. When my work priority allowed, I tried to make sense of the information we were getting and document the results.





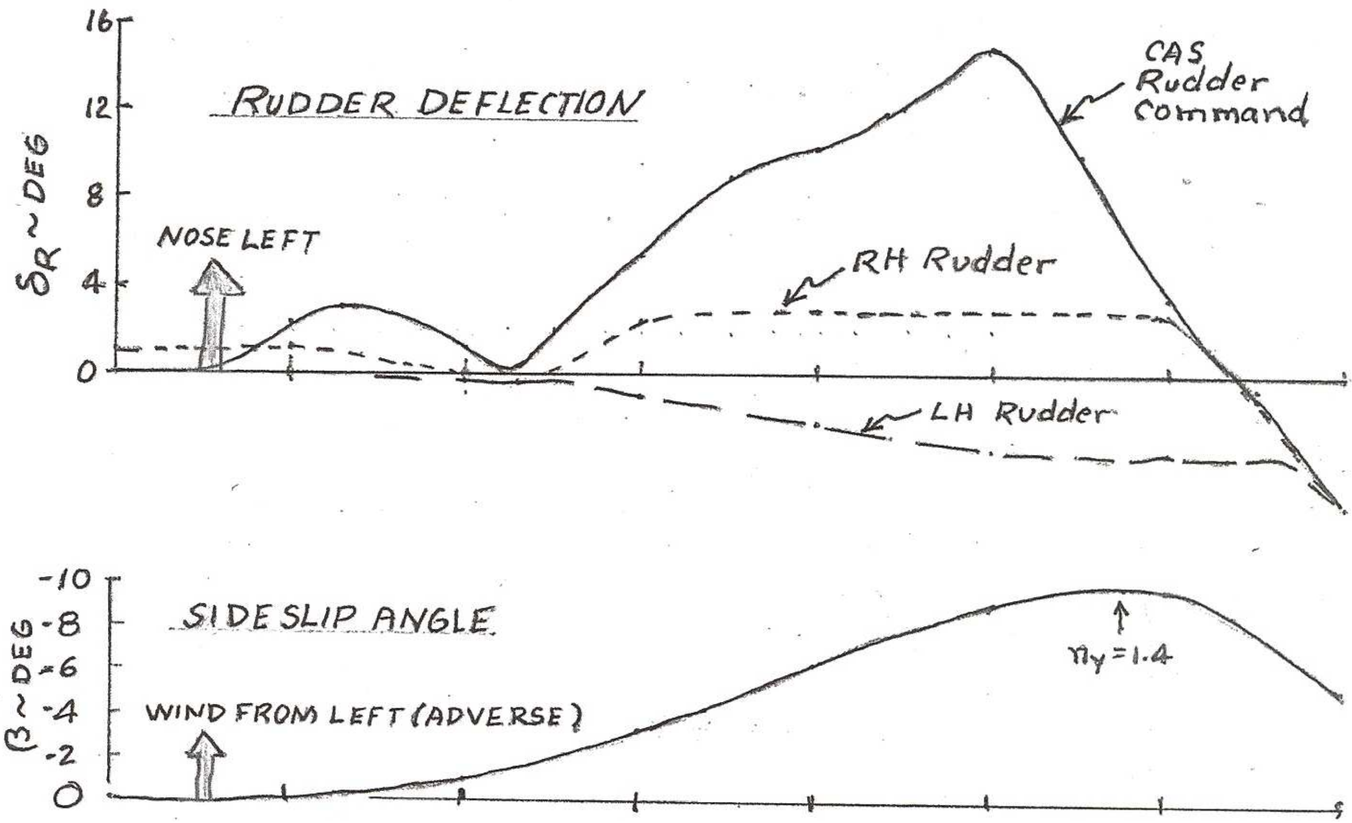
Finally, after testing on three airplanes evaluating some 12 different planforms with different amounts o f porosity, and using a variety of pilots, my report for 8 October 1973 states, "Evaluations of the Mark IV Mod C speedbrake were accomplished this week. It appears that a 45 degree maximum deflection will be acceptable [from both the drag and buffet standpoint]." (This final, solid surface design of 31.5 square feet is 98% the size of a full sheet of plywood).



This chapter in my experiences with the F-15 was a frustrating, sometimes boring episode. The only reason I find it worth writing about is the fact that, of all the airplane modifications necessary as a result of flight testing, this one was undoubtedly the most expensive due to the structural aspects. In addition, it was the one problem that received the most adverse publicity.

I wish the subject would have ended in October '73, but, unfortunately, there is a sequel which will be covered in a later chapter.

Jack Abercrombie
18 March 2007


RUDDER DEFLECTION
16
12
8
4
0
δR ~ DEG
NOSE LEFT
CAS Rudder Command
RH Rudder
LH Rudder
SIDESLIP ANGLE
-10
-8
-6
-4
-2
0
β ~ DEG
WIND FROM LEFT (ADVERSE)
ny = 1.4

TROUBLE IN THE UPPER RIGHT HAND CORNER

This is a difficult chapter to write. Re-telling of the early F-15 directional control problems in the high Mach portions of the flight envelope (the upper right hand corner) recalls frustrations, stress tempered with a few minor triumphs, and some bitterness and disappointments. In addition, to this day, there are some unanswered questions—we may never know those answers. But the words have been bubbling for too long—it's time to put them to paper.

A short Glossary is presented below to assist in understanding some the complexities presented in the next few pages.

GLOSSARY

α	Angle of Attack—Angle between *xy* plane and relative wind (+ for nose high)
β	Angle of Sideslip—Angle between *xz* plane and wind (+ for wind in the right ear)
p	Roll Rate—(+ for right wing down)
q	Pitch Rate—(+ for nose up)
r	Yaw Rate—(+ for nose right)
I	Moment of Inertia—(mass) x (radius of gyration)^2 where mass is in slugs [one slug is equivalent to 32.17 pounds].
x	Longitudinal (fore and aft) Axis
y	Lateral (left and right) Axis
z	Vertical (normal to the *xy* plane) Axis
C_n	Yawing Moment Dimensionless Coefficient
C_h	Hinge Moment Dimensionless Coefficient
δ_R	Rudder Deflection (+ for trailing edge left)
δ_H	Stabilator Deflection (+ for trailing edge down)
F_{LAT}	Lateral Control Stick Force for commanding Control Augmentation System (CAS) Roll Rate.
n_y	Lateral Acceleration (expressed in terms of "g")—aircraft acceleration divided by acceleration due to gravity (32.17 feet per second squared).
n_z	Normal (to the *xy* plane) Acceleration
Δ	Incremental Notation
•	Dot above a symbol denotes a temporal derivative, i.e., degrees per second

Adverse—Sideslip which hinders desired rolling motion

Proverse—Sideslip which aids desired rolling motion

Hinge Moment—Torque applied to a hinged surface by flow field or an actuator; e.g., a force of 10 pounds applied to the edge of a 30 inch wide door to open it against the wind is equivalent to a 300 inch-pound hinge moment.

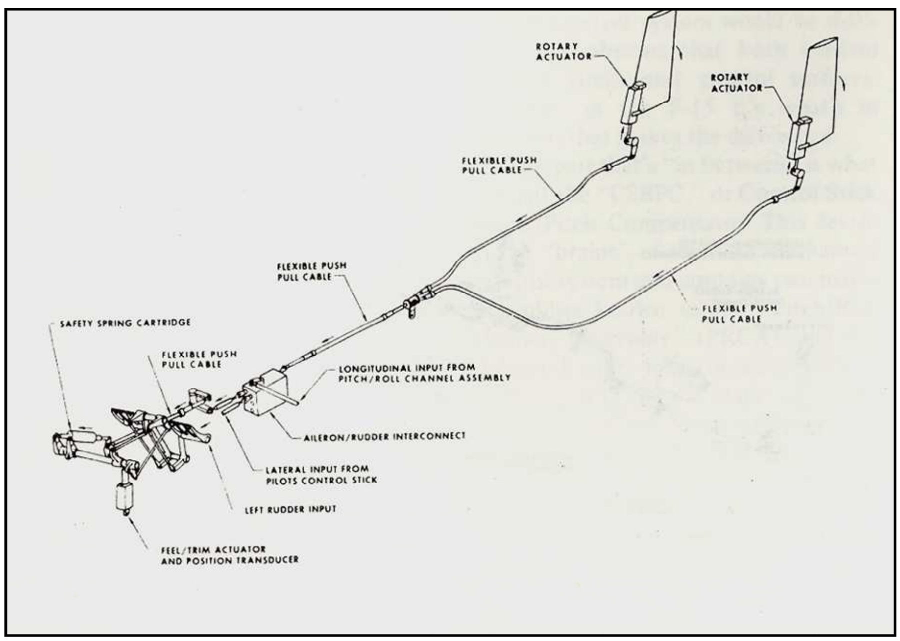
PIO—Pilot Induced Oscillation. More properly called Pilot-Aircraft Coupling—a bad phenomenon in which the pilot input/reaction and aircraft response are out of phase with each other. It can lead to complete loss of control and possible structural failure. Most common frequency for occurrence is about 1/6 to 1.0 cycles per second. (The loss of American Airlines Airbus Flight 587 in November 2001 was due in large part to PIO and subsequent structural failure).

Inertia Coupling—An undesirable coupling and transfer of motion about one axis into motion about a different axis through gyroscopic effects.

RUDDER SYSTEM

There will be considerable discussion of the F-15 rudder system in the following narrative, so a brief description is in order. Limited or special-purpose airplanes without rudders have been designed and successfully flown, but to address the myriad needs of a multi-mission fighter such as the F-15 with the complications imposed by supersonic flight and high angle of attack maneuvering, the rudder and its control system are essential. At times, my most difficult challenges were to convince some elements of management of this fact.

The following text relies extensively on both my contemporary formal weekly reports as well as on entries in my daily log book, so it may read somewhat like a diary with the major events in a chronological order. It may appear to the reader that "St. Louis" is treated rather harshly. My frustrations with the slowness of detailed engineering analyses by St. Louis personnel of the flight test data in the critical safety-of-flight areas will be obvious. But in all fairness to the St. Louis technical people, they did a good, meticulous job with what they had, and it *did* take time to do it right. My real concerns were with the apparent lack of urgency in the coordination of efforts between primarily three technical departments—Aerodynamics, Flight Controls, and Airloads. In my opinion, the Technology Integrator (the same individual who in 1969, in order to minimize the *appearance* of change, decided to "lift the vertical tail tip upwards" 26 inches) could have expedited solutions to the high Mach number directional problems. As it was, we at Edwards were operating in "fast time" compared to St. Louis—our decisions were made "on the spot" with the Test Pilots relying on our judgment. Three plus decades have not altered my opinions.



With that introduction, it's time for a simplistic description of the F-15 rudder control system. The mechanical system consists of twin rudder surfaces powered by rotary actuators located on the rudder hinge lines. The pilot rudder pedal inputs and those of the mechanical Aileron-Rudder Interconnect (ARI) are transmitted to the actuators by a flexible push-pull cable about 40 feet long. The electronic Control Augmentation System (CAS) applies its rudder deflection input directly to the actuators. The CAS (in the early F-15s) is a two-channel analog system—if the dual input/output signals differ by a certain amount, the electronic system shuts down, but the mechanical system continues to operate normally. The mechanical system was designed such that, with the CAS inoperative, the mission can still be completed but with some increase in pilot workload and attention.

FIRST SIGNS OF TROUBLE

Recall the trying days of 1969-71 with regard to directional stability problems (Pages 5-9). As a consequence of those experiences, I was "wired" to be on the alert for any indication of directional problems as the envelope was expanded into the supersonic regime. We first went supersonic (to about M=1.5) on Flight No. 5 on 3 August 1972 without performing any but very gentle maneuvers. The only anomaly observed was a "noticeable yaw" when the afterburners were turned off at the end of the run.

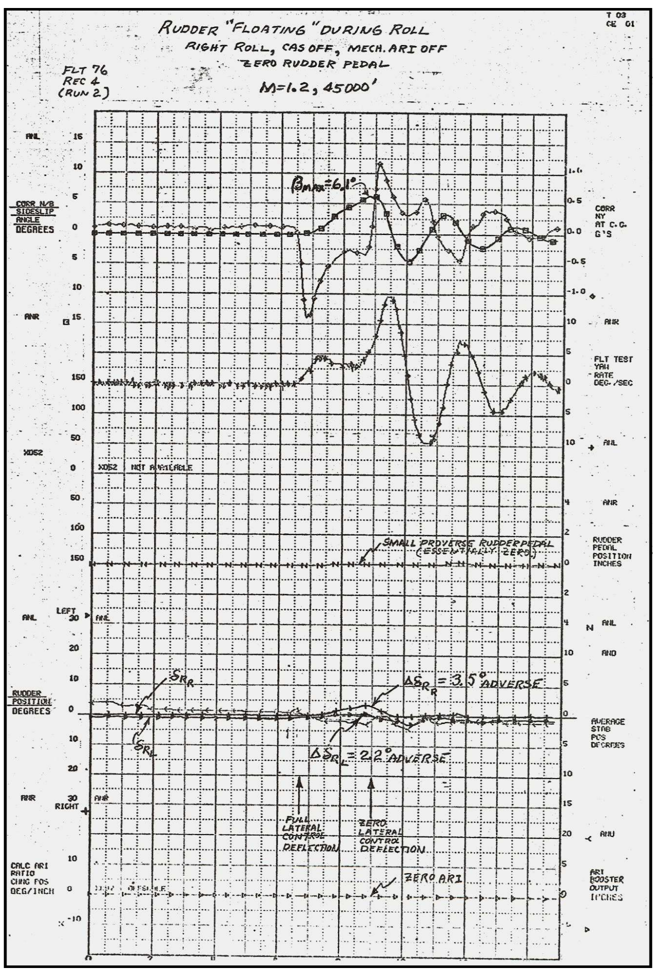
However, on the next supersonic flight involving somewhat more aggressive maneuvering, some rudder deflection hysteresis was noted —they were not returning to their initial neutral positions after being deflected. Directional "looseness" was noted at M=1.7 on Flight No. 9; however, engineering analysis was hindered by data

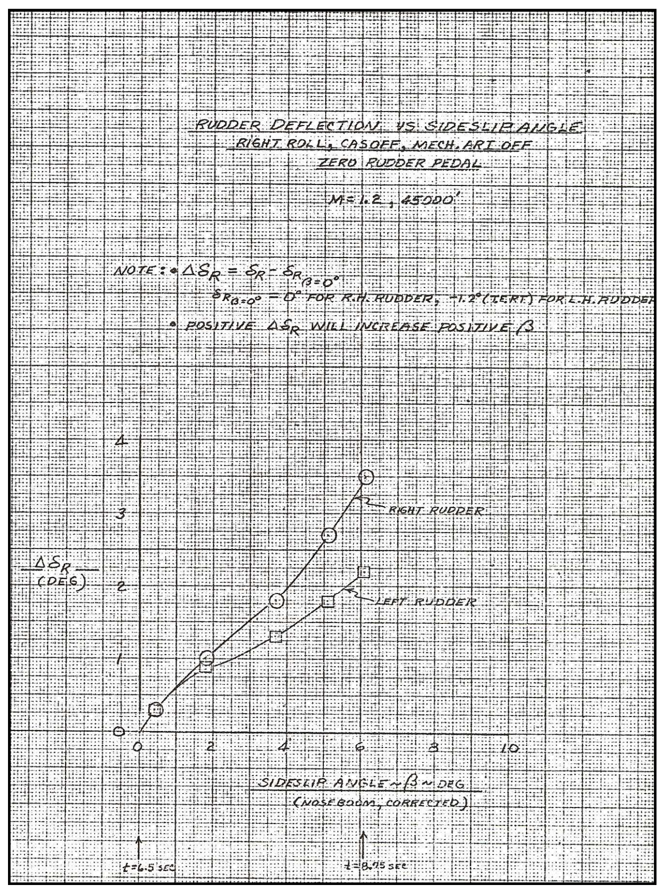
Instrumentation problems as the system was still being wrung out—essential data were missing. But on 10 August 72, M=1.7 at 50,000 feet, Test Pilot Burrows complained openly (normally, he was very reserved and voiced major complaints more or less in private outside of the de-briefing room) about the airplane exhibiting "uncomfortable" directional characteristics in mostly straight and level flight while shutting off the afterburners. Now when an Experimental Test Pilot complains about being "uncomfortable", the Flying Qualities Engineer on the ground takes notice. Likewise, the F-15 Program Manager in St. Louis, Don Malvern, was concerned—a 21 August 72 management meeting in St. Louis put the supersonic lateral-directional characteristics as *top priority.*

INDICATIONS OF EXCESSIVE RUDDER HINGE MOMENT

Without the benefit of thorough analysis, my concern was that the airplane had a short-fall of directional stability at the higher supersonic conditions. This was the area in which I had expended so much of my energy during the preliminary design phase. Finally, though, after observing more real-time data, it became apparent that the major cause of the problems was anomalous rudder behavior—more than simply the hysteresis we had noted earlier. The rudders were not following their commanded deflections at all times because of what appeared to be excessive aerodynamic surface loads—the hydraulic actuators were "stalling out" at some conditions when they should not have been.

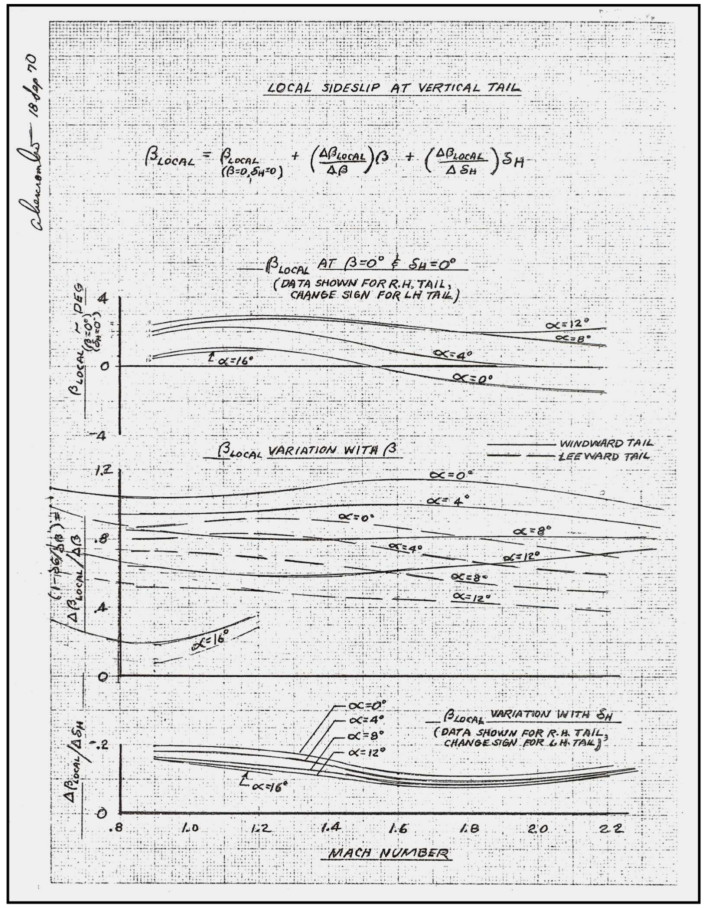
By 17 October 72, there was certainty—the evidence was convincing that the rudders were being over powered by airloads during supersonic maneuvers. My weekly report for that date presented time history data of the rudders more-or-less floating with the breeze during an M=1.2 rolling maneuver—not doing their job of reducing sideslip but, instead, they were deflecting to *reduce* the effective directional stability, thereby *increasing* sideslip adversely.





My report concluded, **"The causes of these characteristics (excessive hinge moments, weak rudder actuator, etc.) are not defined. However, the problem needs to be wrung out since it could cause serious difficulties at higher Mach numbers."** It seemed to me that we had a life-threatening problem on our hands.

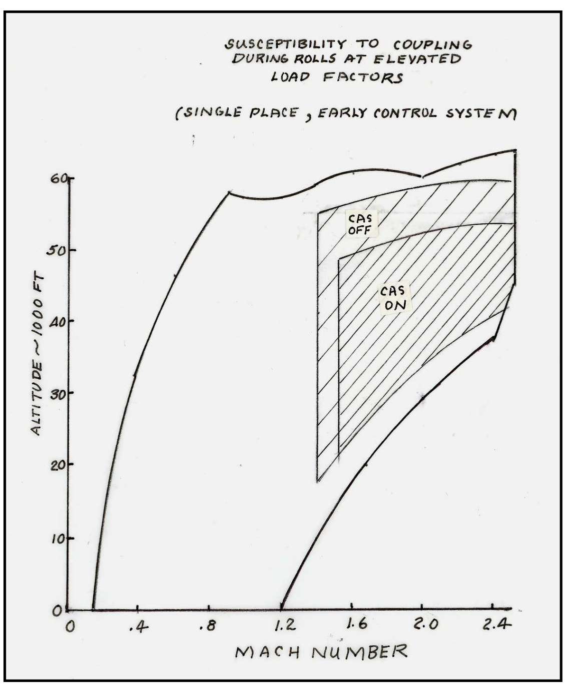
Some of the mechanical control system experts at that time laid the blame on system flexibility in the actuator mechanism, the push-pull control cables, and the other mechanical interfaces. I wasn't so sure.



My weekly memo of 4 December 72 reported on more bad results at about Mach 2.0 and noted, **"...we need to resolve the uncommanded ("floating") rudder deflection problem…."** By this time, I knew that I was somewhat of a nuisance with continual complaints, but they kept on. On 18 December 72, I reported on "mirror image" rudder deflections (both trailing edges inboard) during straight and level flight at supersonic conditions—we aerodynamicists had had some understanding of the complex airflow environment of the vertical tails for more than two years based on our wind tunnel tests and analyses, but the department then responsible for estimating control surface hinge moments obviously did not understand the phenomena (at that time, the Airloads department had the responsibility).

The Airloads representative at Edwards, John Kunzelman, shared my concerns and made similar reports on the problem. He even became fearful of losing his job as a result of his written concerns. He credited my intervention with his St. Louis supervision during a long evening over a bottle of Jack Daniels for saving his job.

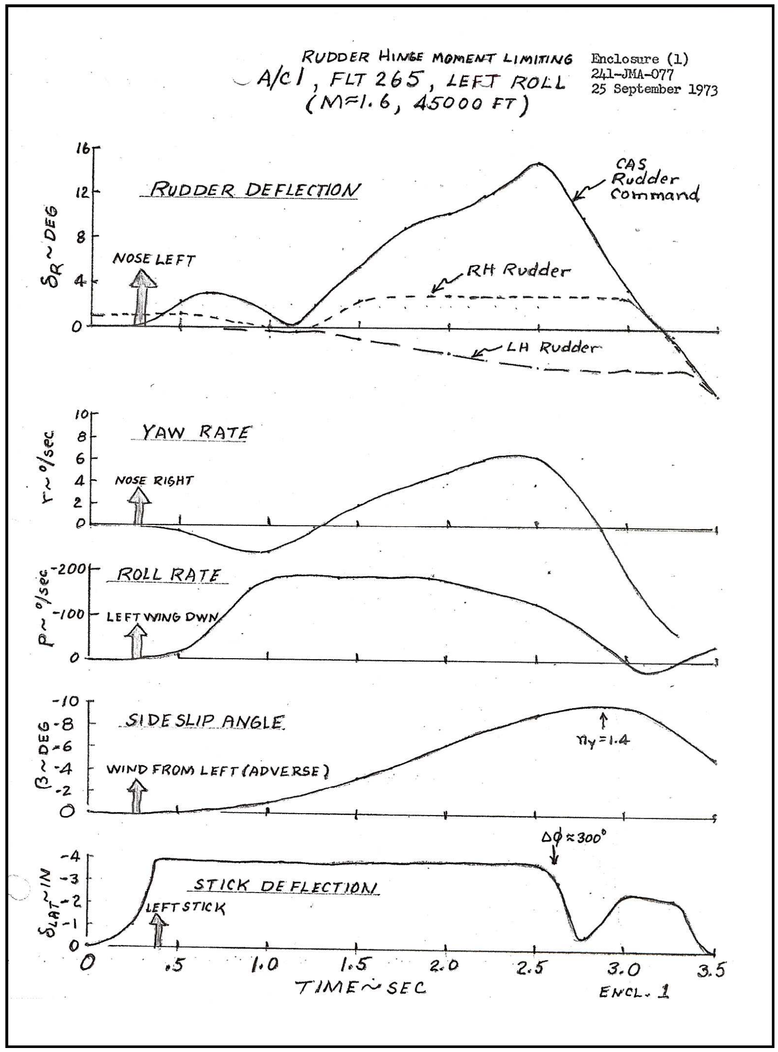
Gradually, with the gathering of more data during our envelope expansion, others became convinced. St. Louis got deeper and deeper into detailed analyses and simulations and verified what the Edwards team had been saying for weeks. The studies showed that we were in trouble above about Mach 1.4 whether or not the CAS was operating. A top-level management meeting in early January 1973 once again identified the supersonic directional characteristics as the number one problem!



By early March 73, with more incidents occurring, I requested more involvement by the appropriate disciplines. I felt that we were just not making any progress.

During the following weeks, some minor changes were made to reduce the flexibility, friction, and compliance of the mechanical rudder system, some "tuning" of the electronic CAS was defined, and a decision was made to modify the rudder actuators to provide a modest 17 percent increase in hinge moment capability (from 12,000 to 14,000 inch-pounds). I had previously gone on record that based on my observations, such a small hinge moment change would not be sufficient.

ROLL-YAW COUPLING



The situation became more serious in late September 1973—during a rolling maneuver at M=1.6, 45,000 feet, we observed a dramatic example of rudder hinge moment limiting. During the maneuver, even though the CAS was commanding 15 degrees (its maximum authority) proverse rudder to coordinate the roll, the windward rudder actually deflected about three degrees the wrong direction (floating with the breeze) while the leeward rudder was able to deflect about the same amount (but in the correct, proverse direction). The result was a net *zero* rudder deflection which led to the airplane reaching a large sideslip angle of about 10 degrees with a corresponding lateral (sideways) acceleration, n_y, of 1.4**g** (anything more than about 0.3 **g** is very uncomfortable and tends to bash one's head against the side of the canopy)**.** A less skilled pilot very likely would have lost the airplane.

To better explain the phenomenon we were up against, the reader is referred to the six-degree of freedom equations of motion described on Page 3 and to the Glossary on Page 33. Without getting too involved in the math and physics, the more important players in this particular problem are in the third equation where,

$$\dot{\beta} = \ldots p \sin \alpha - r \cos \alpha \ldots$$

The rate of change of sideslip, β, is partly a geometric conversion of roll rate to sideslip rate as the airplane rolls about its longitudinal axis. The yaw rate during the maneuver is primarily a result of the adverse yaw rate acceleration through the inertia coupling terms,

$$\dot{r} = pq(I_x - I_y)/I_z \ldots \approx -(0.7)\,pq \ldots$$

Had the maneuver been allowed to continue, there was a possibility of the airplane behaving like a "tumbling" gyroscope with no way to control it (This phenomenon of inertia coupling caused the loss of several of our early "century" series fighters—F-100, F-101, etc.). Of course at this flight condition, such a departure from controlled flight would have broken the airplane into many pieces.

The following week, another top-management review was held at Edwards where I presented the time history data along with word charts (see the following page) describing the configuration and the maneuver, and listed other examples of hinge moment limits causing problems. Note that the airplane had been modified with the increased hinge moment capability rudder actuators, identified as "increased stiffness" on my chart. One chart even suggested some configuration changes which could ameliorate the problem.

Management made a promise that, **"Action will be taken regarding the rudder hinge moment situation."**

38

AIRCRAFT F-1, FLT 265 INCIDENT

CONFIGURATION:

- All CAS on
- M=1.6, 45000 ft, V_i = 488 kt, $n_z \le 1.6$
- Roll CAS command limit at 175 °/sec (std config. provides 212 °/sec at full stick)
- All currently available control system fixes (CAS ARI, Lateral sensitivity, increased stiffness rudder actuators, etc.)

INTENDED MANUEVER:

- Full left stick 360° roll

RESULTS:

- Full left stick applied following build-up manuever with 80 % stick
- Sideslip immediately started adverse (wind from the left) reaching 10 degrees at roll termination ($\Delta\phi \approx 300°$).
- Load factor: n_z went from 1.6 to 0.4 to 2.1 g's during the roll. n_y reached 1.4 g's at roll termination
- Roll rate achieved -180 °/sec (LWD)
- Yaw rate achieved +6.5 °/sec (ANR)
- Pitch rate achieved +25 °/sec (ANU)
- All control system parameters operated normally except rudder deflection:
  - CAS commanded rudder deflection ~ reached the maximum value of 15° ANL (to reduce sideslip and yaw rate); however, the left rudder (windward side) was overpowered by aerodynamic hinge moments and actually traveled to 3.5° ANR. The right rudder was hinge moment limited to traveling only 3° ANL.

24 September 73

RUDDER HINGE MOMENT

PROBLEM:

- Insuffient rudder actuator hinge moment capabilities to provide required rudder deflection during supersonic rolls (totally inadequate for aero. moments)

EXAMPLES:

- Aircraft F-1, Flt. 142 ~ M=1.4, 35000 ft, CAS on, $n_z \approx 1$
- Aircraft F-2, Flt. 81 ~ M≈1.55, 30000 ft, CAS on, $n_z \approx 1$
- Aircraft F-1, Flt. 261 ~ M=1.6, 35000 ft, CAS on, $n_z \approx 1$
- Aircraft F-1, Flt. 265 ~ M=1.6, 45000 ft, CAS on, $n_z \approx 1.6$

POSSIBLE EFFECTS:

- Uncontrollable departure

IMMEDIATE SOLUTION:

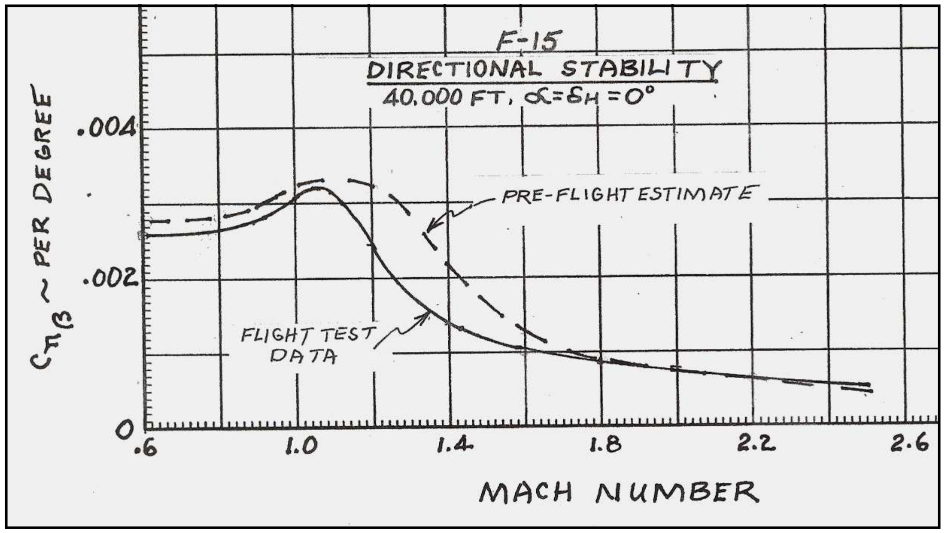
- Manuevering placard:

  "No more than one-half lateral stick deflection will be utilized above M=1.4"

POSSIBLE LONG RANGE SOLUTIONS:

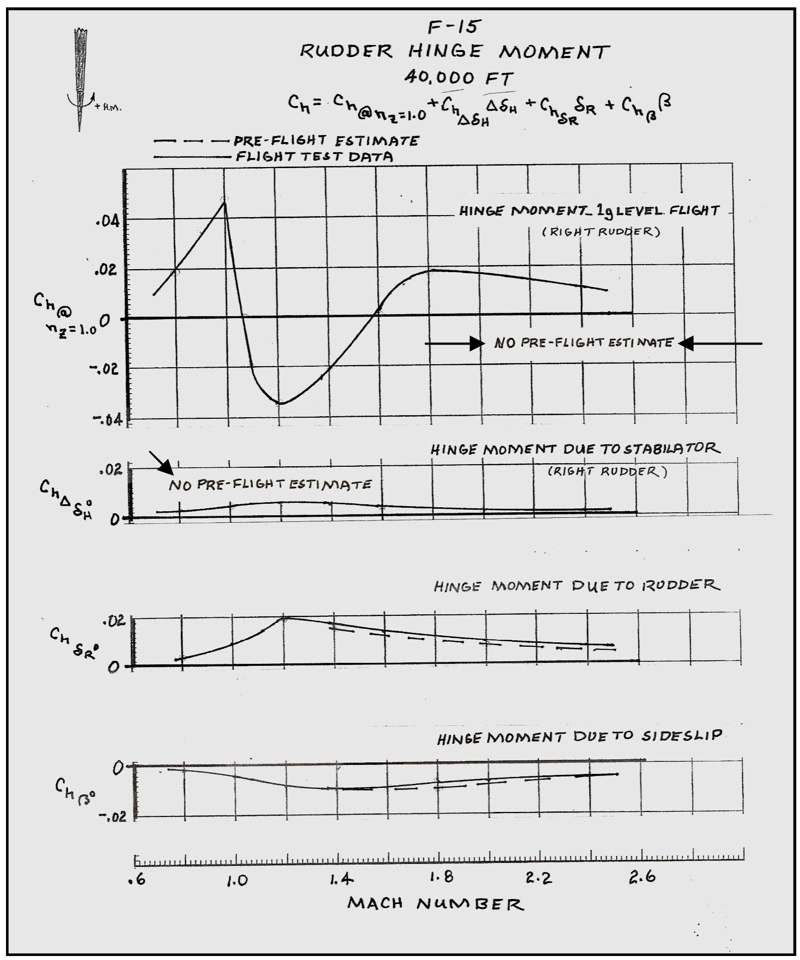
- Permanent manuevering placard
- Reduced roll authority
- Reduced wing dihedral
- Horizontal tail anhedral
- Increased actuator capabilities

Two weeks later, still uneasy, I drew a line with my 8 October 73 report with, **“St. Louis has been alerted that we will await their authorization for every maneuver to be accomplished above 1.4 Mach number which requires placard relaxation. This is necessary because of the inability to presage coupling/divergence based on cursory, on-site analysis.”** But we had a test program to continue; I soon found the line I drew to be completely imaginary!



However, soon after this event, the mathematical models used in the St. Louis simulations were being refined, primarily with improved hinge moment modeling. Test pilots “flew” the simulator evaluating various control law modifications (mostly reduced roll rate capability through changes to both the CAS and the mechanical control system—to reduce the **p sin α** coupling). During this development effort, even the experienced test pilots “lost” the airplane in the simulations.

The “parameter identification” processes used to refine the mathematical models for the simulations soon verified that our pre-flight estimates of directional stability, Cn_{β}, were reasonably good, particularly in the portion of the flight envelope that really counted.



However, the pre-flight estimates for rudder hinge moment, C_h, were way off the mark—no account had been made for the “mirror image” flow field effects on the two rudders, $C_h@n_z=1.0$, or the additional complication of stabilator deflection on the hinge moments, $C_{h\,\Delta\delta H}$.

It would seem that a simple solution would have been to further increase the capabilities of the rudder actuator beyond the 14,000 inch-pound level. Unfortunately, there was, and still is, an influential school of thought throughout the industry (both domestic and world-wide) that the best way to protect the airplane structure from becoming over-loaded and prone to failure was to limit the amount of deflection that can be commanded by either the pilot or the automatic control system. For the F-15, this was accomplished by incorporation of a rudder pedal command limiter and limits to the mechanical as well as the CAS authority. Further, the rudder actuators were actually designed to *relieve* at a specified load. So occasionally, airplanes can be lost after an upset because there is simply no means of applying control!

ALTERNATE SOLUTION AND A MAJOR SCREW-UP

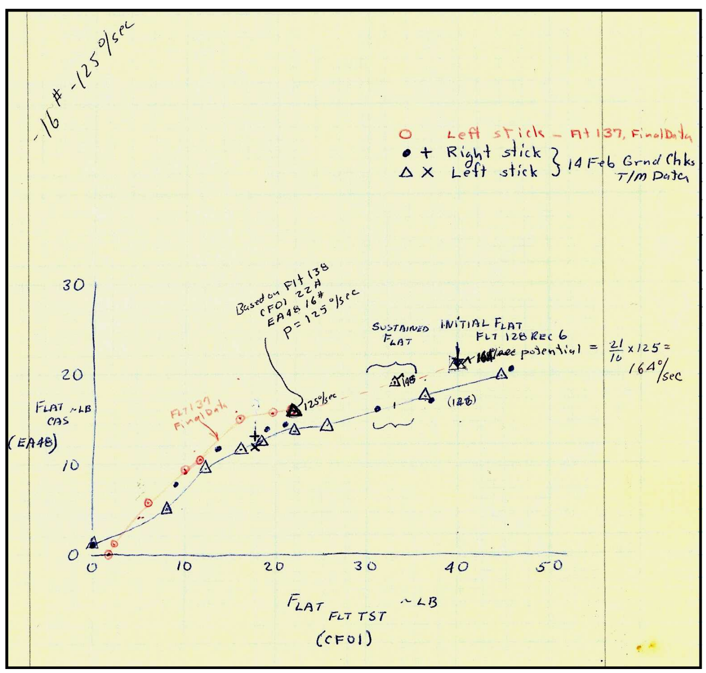
Thus, with limits to the use of the rudders, the only solution remaining for the directional departure tendency was to reduce the amount of roll rate which could be commanded . With less roll rate, there would be reduced coupling with angle of attack and less change in sideslip during a rolling maneuver. After a near disaster with airplane No. 4 on 22 January 74, it was decided to reduce both the mechanical and the CAS roll authority as well as the amount of roll *rate* which could be commanded by the CAS.

> 1.0 Rudder Hinge Moment Characteristics.
>
> Although we've penetrated the upper right hand corner of the envelope many times, apparently the region remains nearly as much a virgin as eleven months ago when rudder hinge moment limiting during rolls was proven with F-1. During a 4.6 g RPO at 1.8 M, 37.5K with F-4, a sideslip of about 7° (ny = 1.6) was reached. During the maneuver, the rudders were hinge moment limited to the extent that even though the CAS called for 9° proverse rudder, the leeward rudder did not move, and the windward rudder actually backed off about 5° (adverse). It should be noted that St. Louis similation results indicated considerably more docile characteristics for this maneuver. As a result, further evaluation of roll maneuvers which violate present placards above 20,000 feet have been suspended until:
>
> ° installation of proto-type CAS and mechanical control mods defined by DDM 1027
>
> ° verification that the above control mods operate as designed
>
> ° verification of an adequate match between simulator and flight test results.

On Valentine's Day, 1974, the modified CAS unit with the reduced roll rate command design limit of 120 degrees/second was evaluated during one-**g** rolls at M=1.8 with satisfactory results in near agreement with simulator-based predictions. The acid test would be rolling maneuvers performed at elevated load factors at higher angles of attack and greater pitch rate going into the roll, subjecting the airplane to a higher degree of roll/yaw coupling. Such a test was scheduled for the next day.

However, I learned from John Mello in St. Louis that the simulation of the CAS roll rate command limiting was not identical to that of the flight control software, and that the roll rate command "limit" might be more than the specified 120 degrees/second. So, at my request, flight preparation was delayed to allow us time to assess the actual roll CAS characteristics. Most of my day on the 15th was spent analyzing the meager data we had obtained on overnight system ground tests and the data from the two one-**g** rolls. During this time, both my Edwards supervision and our flight controls experts assured me that, at worst, the CAS would command no more than 12**8** degrees/second.

Late that afternoon, as the (delayed) scheduled flight time came and passed, I continued my crude analyses while Bob Luckey, the Flight Test Engineer, looked over my shoulder. When I shook my head negatively, Bob went over to the flight schedule blackboard and scratched out the scheduled flight. Now it's serious business to cancel a flight considering all the coordination required to arrange for a chase plane, telemetry needs, range time, etc. My supervisor, Hank Rechtien, stormed out of his office when he learned of the cancellation, exploding in a very loud voice at my irresponsibility—his yelling was heard throughout the entire floor.



I stayed at work late that evening polishing my previous crude workup. The results suggested a very strong probability that instead of the CAS command limiting to **120** degrees/second, it would actually have allowed for about **165** degrees/second—this would have almost certainly spelled disaster during the planned maneuver.

Rechtien never apologized to me about his outburst. However, his own weekly report of 22 February 74 contained the paragraph,

> 3.1 In Reference (2) Report, Paragraph 2 points out that strip-out data indicated that we had roll CAS command capability well in excess of 120° per second. It is lucky that Abercrombie caught this condition. The DDM prescribed the fix and specifically stated that the CAS should limit at 120° per second. Some place between the DDM and the hardware somebody assumed what was needed rather than making sure. As a result of this we had to expedite diode limiters on the CAS command signal. Whoever is responsible, please don't make another mistake like this. We could lose an airplane!

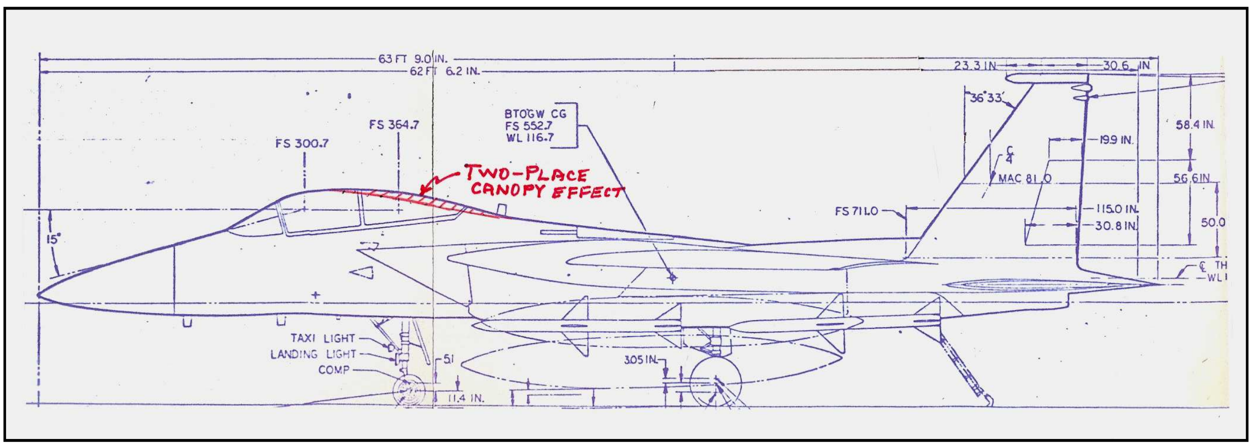
(The referenced report in the paragraph was my weekly memo). As a consequence, the Roll CAS underwent a final modification with a "hard stop" at 120 degrees/second.

Finally, St. Louis and Edwards agreed to some ground rules for making go-no-go decisions for the remainder of the rolling maneuver test program. We proceeded cautiously and completed the planned high supersonic roll maneuver test program on 24 May 1974 for the single-seat F-15—several system modifications and about 22 months after the first indications of trouble.

TWO-PLACE AIRPLANE

For the previous 11 months, TF-15 No. 1, the two-place trainer version had been performing a variety of tasks evaluating buffet improvement modifications, control system dynamics peculiar to the two-place configuration, some cross-wind landing evaluations, weapons separation, radar and fire control system checkout, and VIP demonstrations for such notables as Senator Barry Goldwater and the Shah of Iran.

In March 1974, high supersonic lateral-directional testing began with TF#1 using the previously developed control system modifications resulting from the single-place testing. Unfortunately, as noted on Page 9, the

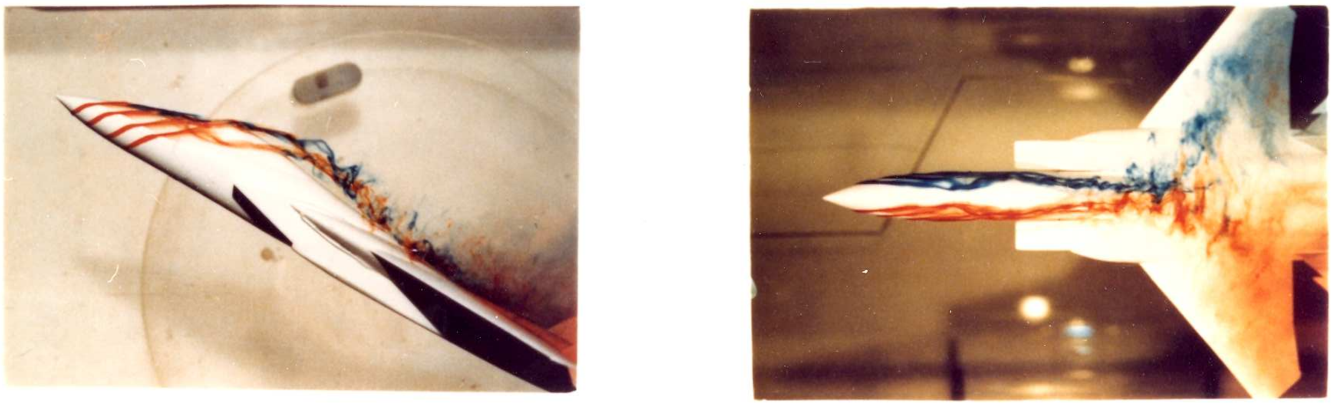


additional side area of the two-place canopy degraded the supersonic directional stability.

As a result, unsatisfactory characteristics were observed the first time a 360 degree roll was performed at M=1.6, 30,000 feet—another case of the rudder hinge moment relief feature; the system simply could not accommodate the degraded directional stability. But before we could thoroughly evaluate the full impact of the two-place canopy, TF#1 was turned over to the Air Force for their testing, but not before very stringent placards were imposed for the supersonic conditions.

AIR FORCE TESTING—F AND TF

The next significant rudder adventures (although at *subsonic* conditions) occurred in March 1975. The Air Force had been conducting Air Combat Maneuvering (ACM) at the Yuma ACM range for a brief time using F#11 and TF#1 against various "threat" aircraft. These "close-in" dog-fight maneuvers occurred for the most part at high subsonic, high angle of attack conditions where the vertical tails took a lot of beating from the turbulent airflow associated with both high angle of attack as well as from the frequent use of the speedbrake.



$\alpha = 30^{\circ}$ **WATER TUNNEL TESTS 30 DEG AOA** $\alpha = 30^{\circ}$

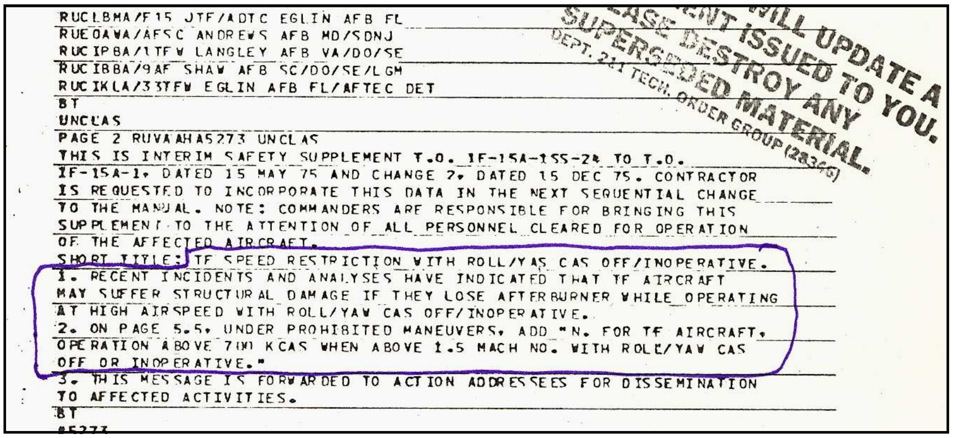
On 19 March, F#11 with Captain George Muellner returned to Edwards with the top half of the left hand rudder completely gone! That was bad news for our structural engineers in St. Louis. Of course, they had a need to examine the remaining parts to try to determine the failure mode. I happened to be the ranking engineer on hand that day (I had taken over Rechtien's job) but had no authority to approve a travel request—but I signed off on one anyhow and sent Ray Morse to St. Louis on the "Red Eye" hand carrying the broken rudder. The very next day, I had to interrupt a meeting going on in St. Louis on the subject with a telephone call—after some more ACM, TF#1 was returning to Edwards with the *entire* left rudder gone!

The next few days conferences were held involving the USAF pilots, structural engineers from St. Louis, and engineers at Edwards. Although some structural changes may have been made during the following months, I don't recall, and my records don't show it. (Whatever may have been done to improve the strength, it was inadequate—a few years later [11 January 83], while I was acting as a "guest St. Louis expert" for high angle of attack testing at Edwards, I observed an F-15 after returning from a test flight with one rudder "permanently" deflected about 15 degrees. The failure of the rudder actuator had apparently occurred during a Mach 0.6 high angle of attack rolling maneuver. Apparently one of the gears had broken a key and then rotated on its shaft.).

MORE TWO-PLACE AIRPLANE PROBLEMS

My next log book entries for the two-place configuration directional stability and control problems were a few months after I had returned to St. Louis to an assignment not directly involved with flight test. TF-15#2 on 21 January 1976 while investigating longitudinal control sensitivity with the Pitch CAS turned off got into serious trouble at M=1.75, 22,000 feet. Apparently, one of the engine afterburners blew out creating a sudden yawing moment leading to a 1.7**g** lateral (sideways) acceleration. In spite of encountering a longitudinal PIO with normal load factor excursions from –1.5 to +7.2 **g**s, the skilled Experimental Test Pilot, Denny Behm, managed to re-establish control after three complete oscillation cycles.

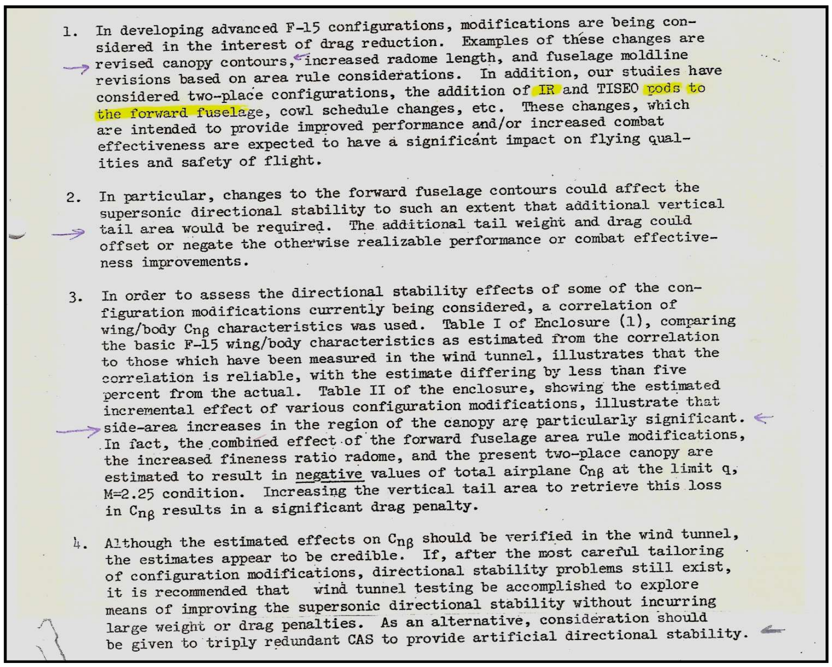
In early April, after several meetings and negotiations with the F-15 SPO and the USAF Joint Test Force, two-place airplanes were placarded to no more than 700 knots calibrated airspeed (100 knots less than for the single place airplane) above M=1.5 if the Roll/Yaw CAS is inoperative.



RUCLBMA/F15 JTF/ADTC EGLIN AFB FL
RUEOAWA/AFSC ANDREWS AFB MD/SDNJ
RUCIPBA/1TFW LANGLEY AFB VA/DO/SE
RUCIBBA/9AF SHAW AFB SC/DO/SE/LGM
RUCIKLA/33TFW EGLIN AFB FL/AFTEC DET
BT
UNCLAS
PAGE 2 RUVAAHA5273 UNCLAS
THIS IS INTERIM SAFETY SUPPLEMENT T.O. 1F-15A-1SS-24 TO T.O. 1F-15A-1, DATED 15 MAY 75 AND CHANGE 2, DATED 15 DEC 75. CONTRACTOR IS REQUESTED TO INCORPORATE THIS DATA IN THE NEXT SEQUENTIAL CHANGE TO THE MANUAL. NOTE: COMMANDERS ARE RESPONSIBLE FOR BRINGING THIS SUPPLEMENT TO THE ATTENTION OF ALL PERSONNEL CLEARED FOR OPERATION OF THE AFFECTED AIRCRAFT.
SHORT TITLE: TF SPEED RESTRICTION WITH ROLL/YAW CAS OFF/INOPERATIVE.
1. RECENT INCIDENTS AND ANALYSES HAVE INDICATED THAT TF AIRCRAFT MAY SUFFER STRUCTURAL DAMAGE IF THEY LOSE AFTERBURNER WHILE OPERATING AT HIGH AIRSPEED WITH ROLL/YAW CAS OFF/INOPERATIVE.
2. ON PAGE 5-5, UNDER PROHIBITED MANEUVERS, ADD "N. FOR TF AIRCRAFT, OPERATION ABOVE 700 KCAS WHEN ABOVE 1.5 MACH NO. WITH ROLL/YAW CAS OFF OR INOPERATIVE."
3. THIS MESSAGE IS FORWARDED TO ACTION ADDRESSEES FOR DISSEMINATION TO AFFECTED ACTIVITIES.
BT
#5273

DEPT. 241 TECH. ORDER GROUP (28346)
SUPERSEDED MATERIAL.
RELEASE SENT ISSUED TO YOU.
WILL UPDATE AND DESTROY ANY

Finally, nearly five years after my June 1971 memo expressing concern about the negative impact of the two-place canopy on supersonic directional stability, everyone was on board. The memo, a portion of which is reproduced below in larger format than on Page 9, suggested that a triply redundant CAS should be considered, particularly if additional forward fuselage modifications were to be incorporated.



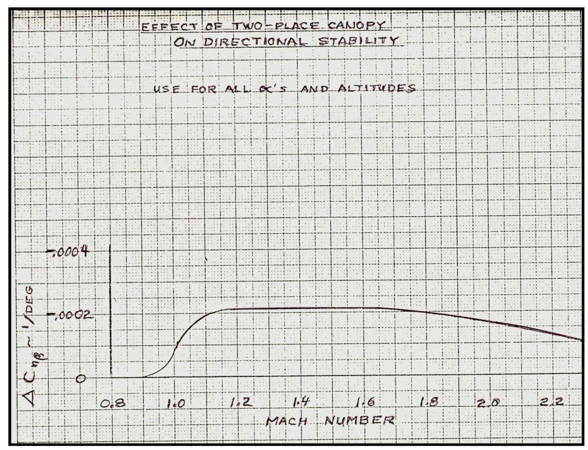
1. In developing advanced F-15 configurations, modifications are being considered in the interest of drag reduction. Examples of these changes are revised canopy contours, increased radome length, and fuselage moldline revisions based on area rule considerations. In addition, our studies have considered two-place configurations, the addition of IR and TISEO pods to the forward fuselage, cowl schedule changes, etc. These changes, which are intended to provide improved performance and/or increased combat effectiveness are expected to have a significant impact on flying qualities and safety of flight.

2. In particular, changes to the forward fuselage contours could affect the supersonic directional stability to such an extent that additional vertical tail area would be required. The additional tail weight and drag could offset or negate the otherwise realizable performance or combat effectiveness improvements.

3. In order to assess the directional stability effects of some of the configuration modifications currently being considered, a correlation of wing/body $C_{n\beta}$ characteristics was used. Table I of Enclosure (1), comparing the basic F-15 wing/body characteristics as estimated from the correlation to those which have been measured in the wind tunnel, illustrates that the correlation is reliable, with the estimate differing by less than five percent from the actual. Table II of the enclosure, showing the estimated incremental effect of various configuration modifications, illustrate that side-area increases in the region of the canopy are particularly significant. In fact, the combined effect of the forward fuselage area rule modifications, the increased fineness ratio radome, and the present two-place canopy are estimated to result in negative values of total airplane $C_{n\beta}$ at the limit q, M=2.25 condition. Increasing the vertical tail area to retrieve this loss in $C_{n\beta}$ results in a significant drag penalty.

4. Although the estimated effects on $C_{n\beta}$ should be verified in the wind tunnel, the estimates appear to be credible. If, after the most careful tailoring of configuration modifications, directional stability problems still exist, it is recommended that wind tunnel testing be accomplished to explore means of improving the supersonic directional stability without incurring large weight or drag penalties. As an alternative, consideration should be given to triply redundant CAS to provide artificial directional stability.

Flight testing of the two-place airplane with various forward fuselage modifications continued through the Spring of '76. The results were no surprise.



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Fast-forward to 25 April 1979. By this time, I had moved on to a different project. I was in charge of the F-18 "Hornet" Aerodynamics group (Performance, Flying Qualities, Store Separation). My secretary came into my office that Wednesday afternoon with teary eyes and told me that a friend, Gary Kincaid, a MCAIR Production Test Pilot had been killed in an in-flight disintegration of an F-15B (a.k.a. TF-15) S/N 77-0167. Gary was a young ex-Navy fighter pilot who had flown many combat missions over Vietnam. He had been with the company about two years and had just recently taken a job as a Production Test Pilot responsible for checking out newly manufactured airplanes just prior to turning them over to the Customer pilots.

As I understand the flight profile, it involved high angle attack (up to full aft stick) maneuvers immediately followed by a supersonic acceleration to Mach 2.0 at 40,000 feet. Eyewitnesses on the ground near Fredericksburg, MO over which the supersonic runs were normally made, reported his contrails corkscrewing briefly before airplane parts came raining down over a wide area.

Over the next several months, both MCAIR and the USAF conducted accident investigations evaluating about a dozen possible scenarios, from the obvious possibilities of afterburner blow-out, PIO, controls malfunction, engine disintegration, etc. to more remote possible chains-of-events. Neither investigation team was able to reach a conclusive finding. I was not on the investigating team; of course, by that time, everyone knew of my concerns. The investigations were very extensive; however, they had little to act on—broken parts, certainly, but there was no way to determine what failed first or why. The airplane, not being a test aircraft, had no telemetry package or flight data recorder; the electronic control system components being analog rather that digital, left no clues when power was interrupted. The cause of the breakup will remain a mystery.

*****

By coincidence, the day after Kincaid's fatal accident, I was named to lead a team to perform "major surgery" on the F-18 "Hornet" flight control system (the original had been designed by software engineers using "Microsoft Mentality"— "Fly it an patch it."). We made very major changes to the F-18 control system, and although there may have been some shortfalls, at least we didn't make the same mistakes as on the F-15. The F-15 "Lessons Learned" were fully put to use for the F-18, but that's another story.

It may be appropriate to point out that the latest versions of the F-15, beginning with the "Strike Eagle" which first flew in 1986 incorporate a ***triply redundant, digital CAS*** and thrust sensing algorithms which in the even of sudden loss of thrust in one engine at critical flight conditions, will ***automatically reduce the thrust in the other engine to minimize asymmetric directional effects.***

Jack Abercrombie
15 May 2007

"...OUTSTANDING HIGH ANGLE OF ATTACK CHARACTERISTICS..."

"...the F-15A exhibited outstanding high angle of attack characteristics when compared to any other tactical jet fighters currently in the Air Force inventory. It proved capable of effective operation at angles of attack in excess of those attainable by previous Air Force fighters. These excellent characteristics should greatly enhance the capability of the F-15A to fulfill its primary mission of air superiority…."

Air Force Flight Test Center Report AFFTC-TR-75-32, January 1976

So now, after a decade of effort on the parts of many dedicated individuals, the United States had a fighter aircraft with high angle of attack capabilities superior to any in history. For the first time, the combat pilot would be able to control the skies without undo risk of losing the battle due to departures from controlled flight in the high angle of attack, "dog fight" environment.

Unlike the last chapter, there are no "villains" in this one. To the contrary, everyone associated with the F-15 high angle of attack, stall, and spin program did an exemplary job. But there are a few who should be singled out for their efforts.

First, the McDonnell Test Pilots:

Denny Behm—With the cards stacked against him, he unintentionally flew the first F-15 spin, but with a cool head, he brought the airplane home safely.

Jack Krings—Jack was the designated primary McDonnell spin test pilot enduring some 63 spin maneuvers out of the total of 115 performed during the formal Category I (Contractor Development) and Category II (Customer Development) tests. He holds the record for having made some 251 complete turns around the compass during upright spins as well as having experienced 12 ½ turns in a single spin!

The USAF Test Pilots:

Pete Winters, Lt. Col.—Pete flew several early Air Force participation spin flights as well as most of the Category II (USAF controlled testing) high angle of attack and spin test flights. Pete totaled some 40 spins.

Dave Peterson, Maj.—Dave was one of the F-15 "Streak Eagle" time-to-climb record-breaking pilots. He joined the high angle of attack program late but managed to fly several of the Category II spin flights.

John Hoffman, Lt. Col.—John was not involved in the original high angle of attack program, but he became very much involved some eight years later when we had a surprise event (which will be discussed in a later chapter).

The Flight Test Engineers, *Gary Trippensee* and (later) *Tom McDuffee* for McDonnell, *Don Wilson* for the Air Force; and subscale model test expert, *Jim Bowman,* of NASA Langley Research Center share a large part of the credit for a successful program. And we could not have gotten along without the McDonnell analytical experts, *Dick Thomas* and (later) *Pat Wider.*

But one individual deserves special mention—*Skip Hickey* of the F-15 SPO (see Pages 4 and 16). Skip had joined the SPO in 1968 with the task of establishing specification requirements for the F-15 flying qualities, including stall, spin susceptibility, and spin recovery. He, with the support of his supervisor, *Fred Rall,* had to battle his way "up stream" in the Air Force's Aeronautical Systems Division to have the requirements he defined accepted by the Air Force community. One of those requirements has already been discussed— "feet-on-the-floor" roll maneuvering utilizing only a mechanical control system (see Page 5). The other requirements pertinent to this chapter were for the airplane to have a low susceptibility to departure from controlled flight, low susceptibility to spinning, and, if a spin were to occur, the airplane could be easily recoverable.

My part in the high angle of attack, stall, and spin program was to digest the technical information generated by the experts; communicate analysis and test results to pilots, other engineers, and management; provide guidance to the flight test team through on-the-spot, cursory analysis of data; and help define appropriate control system modifications for the airplane. As it developed, I also became somewhat of a historian for the spin test program.

My historian credentials were based on the fact that I was the one engineer who was consistently most involved throughout the entire program. Of the 115 spins accomplished during the development testing, I monitored 85% of them in real-time in front of the telemetry data charts. In this capacity, I evaluated the data for accuracy, selected which data should go through the complete data reduction routine for transmittal to St. Louis, I participated en every pre-flight briefing and post-flight debriefing, I analyzed *every* spin, and I documented the events with both daily, informal reports as well as with weekly, formal ones.

In addition, I advised and kept track of the NASA activities on our behalf.

F8 FLT 145 9 Dec 74 — High α
Krings

Same as Flt 144

- (1) Cont. Cycle
- (2) TO
- (3) Phase
- (4) 35K, 180 kt, SB out, 30 unit, 360° Roll, Full δ_{LAT}, 2100/2700#, CAS off
- (5) " " " " BB Roll " 2000/2150 CAS on
- (6) " " " " " Full Pedal 1800/2200 "
- (7) " 30° α_i, CAS off, SB in then out, 1750/2100
- (8) 45K, .9M WUT to 30° α_i, SB in then out, CAS off – 55°/s r, 28° β w/SB
- (9) " X-cont/defeat ARI attempt, SB out, 1500/1700 – α decreased
- (10) " " " " " " 1100/1400 – "
- (11) " " " " " SB in, 600/900 – spin r_{max} = 85, α_{max} = 75. Neut Rec. oscillatory, violent
- (12) " " " " " SB out – α decreased

(CAS ON — items 9–12)

- (13) 35K, PA, SB out, CAS off, 1g stall attempt to control roll-off with stick. A/c started right yaw – left lat stick increased yaw rate. Cycled stick back & forth – wound up with stick slight fwd – Brought stick back and yaw rate increased – went to smooth, stdy left spin – α_i ≈ 62°, r ≈ -55°/s. After ~2 turns, full lat didn't help – Gear up/flaps up – full lat control – 2 turns – no help, Brought SB in for 2-3 turns – no apparent effect. Pulled chute @ 22K, made 1 more turns. ~8 turns total.

9 Dec 74

F8 – Flt. 145 – Record 13

Spin Entry Conditions:

1G stall, PA configuration with speedbrake out, CAS off, 38K — Mk IV Mod C

While attempting to control roll-off with lateral stick, it was noted that yaw motion was aggravated by stick input. Right stick was applied at a nominal 30°/sec right yaw rate while AOA oscillated between 22° and 60° indicated. Yaw rate decreased to zero at which point, the lateral stick was neutralized, 1 sec later Yaw rate continued towards the left, building up to a nominal 60-70°/sec at a nominal AOA of 70°. Full anti-spin lateral control was applied about 12 seconds with no apparent effect. Gear and flaps were retracted while full lateral anti-spin stick was maintained. Lateral ratio changer was at near minimum authority after the gear was retracted for about one turn until the proper stick position for full authority was found. Full anti-spin control was maintained for another turn while the speedbrake was retracted – no apparent response. The spin chute was deployed at about 22000 feet (at which point, AOA may have been decreasing. Final data will be required to determine whether this was actually the case). About one turn was made after the chute blossomed with recovery occuring at 21000 feet.

Spin parameters were AOA 62°, yaw rate -55°/sec, very steady. A total of eight turns were made.

But probably most important, I maintained records. Although I considered all this as part of my job, I did it for the pure enjoyment as well. Although there were some nervous moments during the spin program, I regarded this part of my assignment as an exhilarating learning experience—not at all like the tense times described in the previous chapter.

MEMORANDUM
241-JMA-174
0035.41
16 December 1974

SUBJECT: F-15 Flight Test Activity – Aerodynamics (9 through 13 December 1974)

TO: H. H. Cole

CC: D. R. Barklage, V. W. Bender, C. E. Chisolm, L. E. Cooper, J. R. Hanson, P. Henry, J. E. Krings, S. Kuhlman, R. Luckey, W. C. Moody, D. E. Nash, K. G. Richter, M. K. Schwartz, E. I. Staley, G. A. Trippensee, D. Walker

J. W. Agnew, I. L. Burrows, H. F. Creel, C. P. Garrison, L. Guenther, D. B. Hamer, H. W. Hamm, B. P. Hoffman, A. L. Jarrett, J. T. Johnston, F. Laacke, J. F. Mello, C. W. Miller, C. H. Mongold, W. P. Murden, W. G. Nelson, R. A. Noyes, M. L. Ramey, R. D. Samuelson, D. L. Schock, E. R. Shields, D. J. Thompson, W. C. Trent, J. Warakomski, F-15 ECRF (1)

FROM: J. M. Abercrombie

1.0 F-8 HIGH ANGLE OF ATTACK CHARACTERISTICS

1.1 Spin Chute Recovery

A spin chute recovery was made at 22,000 feet in accordance with established procedures on flight 145. The following summarizes the significant events of the maneuver leading up to the spin, the spin characteristics, and the subsequent recovery:

- Departure in the PA configuration (production speedbrake with the 45° actuator) occurred at 26° angle of attack.
- Attempts to control the directional motion with the lateral stick merely aggravated the yaw excursions.
- Even with neutral lateral-directional control, the yaw rate and angle of attack continued to increase reaching a spin condition at 31,000 feet with the angle of attack peaking at 74° and the yaw rate peaking at -71° per second.
- Anti-spin lateral control reduced the angle of attack and yaw rate to a mildly oscillatory condition with a nominal angle of attack at 55° and -48° per second yaw rate. After a little more than 1 1/2 turns with anti-spin control maintained, recovery was not apparent. The pilot elected to clean up the aircraft, retracting the gear and flaps.
- Upon gear retraction, anti-spin control was effectively lost for about two turns (the lateral control authority reverted to the washed out, near minimum schedule) with attendant increases in angle of attack and yaw rate.
- During the next two turns, while still maintaining anti-spin lateral stick deflection, angle of attack decreased slightly to about 53°; and yaw rate stabilized at -52° per second (yaw rate remained at a constant level for about 5 1/2 seconds).
- At 22,000 feet, the controls were neutralized, and the spin chute was deployed (a ground rule established many months ago); and recovery occurred in one more turn.

241-JMA-174
0035.41
16 December 1974
Page 2

A total of eight turns were made with an altitude loss of about 10,000 feet (the altitude loss was very nearly in perfect agreement with estimates). Anti-spin lateral stick deflection was maintained for a total of 29 1/2 seconds or about 4 3/4 turns.

Whether the two turns during which yaw rate stabilized prior to spin chute deployment could be called a new spin mode is debatable. Although the angle of attack was decreasing slightly, there was no indication that the yaw rate was decreasing. The characteristics during this time were in close agreement with those observed very briefly on the NASA 13% as well as the 3/8 scale spin models for neutral lateral control. But even on the models, this characteristic was not identified as a separate mode, as it was never allowed to proceed beyond about two turns. Piloted simulation studies last spring revealed a similar characteristic; but at that time, the accuracy of the simulation was in question. Needless to say, analysis is proceeding.

1.2 Speedbrake Effects

Testing with the 45° speedbrake is completed. The wind tunnel predictions of degradation to stability about all axes at moderately high angle of attack was verified by the flight results. There is no question that an automatic speedbrake retraction device will be required in view of the PA configuration characteristics noted above as well as the characteristics observed with the clean aircraft (speedbrake extension in the neighborhood of 25° angle of attack causes immediate departure). The aircraft will be in layup all next week for installation of the auto-retraction device.

1.3 Negative Angle of Attack

The initial high negative angle of attack investigations were begun on flight 148. Stabilized negative 22-23° angle of attack were achieved as predicted from wind tunnel tests as well as with the 3/8 scale model.

2.0 TEWS POD/AFT LIMIT INVESTIGATION

F-1, flight 479, made the first flight for evaluation of the aft C.G. limit with two TEWS pods, four AIM-9E's, and four AIM-7F's. Data were obtained at C.G.'s ranging from 26.9 to 27.6 percent MAC. Thus far, characteristics have been satisfactory.

3.0 FOUR-G 360° RPO'S

F-4, flight 296, completed theCAS off, 4G 360° RPO TIS requirements. For the conditions investigated, although extremely uncomfortable (requiring considerable stick dumping to prevent excessive G spikes), safety of flight did not appear to be compromised.

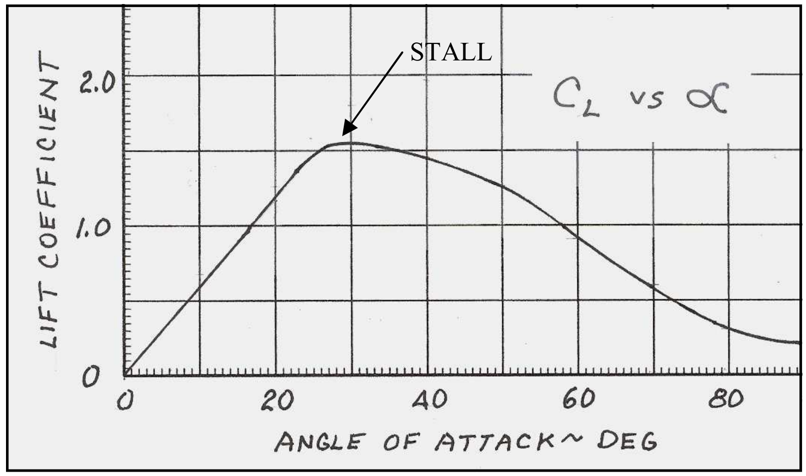
4.0 STREAK EAGLE

F-17 completed the Edwards portion of the Time-To-Climb activity with satisfactory results, achieving greater than 96,000 feet on flight 42.

Jack M. Abercrombie
J. M. Abercrombie – Project Aerodynamics Engineer – Dept. 241-EAFB-Sta. 72054

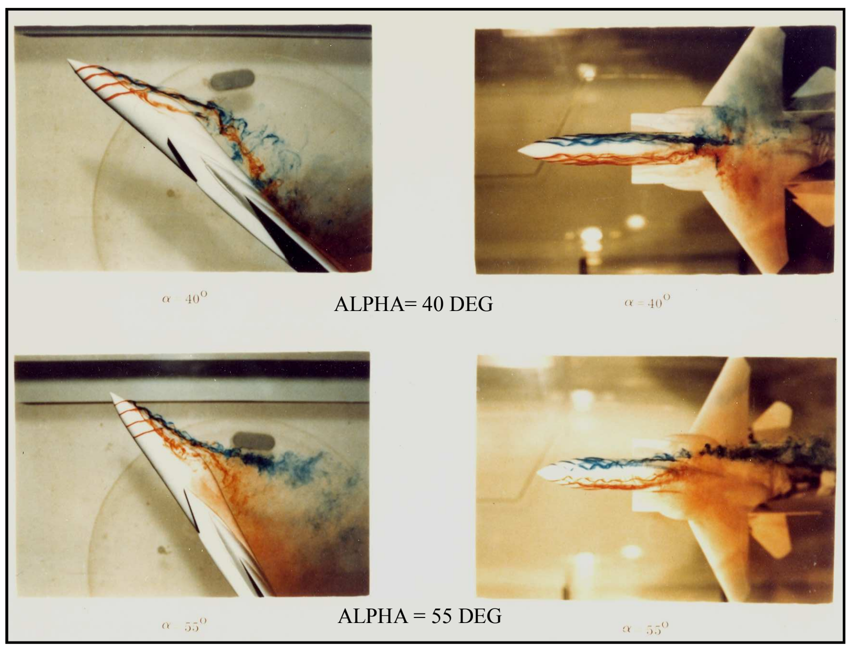
But I'm getting ahead of the story. A bit of background is necessary.

STALL AND SPIN



Wing *stall* was briefly discussed in the earlier chapter about wing tip shape, air-flow separation, and buffet. At the stall, any further increase to angle of attack will not produce any additional lift for the same airspeed, so the wing is said to be *stalled.* Any increase in angle of attack beyond that for stall, will result in a loss of lift. In other words, stall represents the most lift that can be generated.

Flight at the stall condition yields the lowest airspeed at which the airplane can be flown. The ideal landing is one made at the minimum speed—stalling with the landing gear just a few inches above the ground results in the shortest landing distance. Birds routinely make full-stall landings employing all their high lift devices (raptors have been observed to occasionally stall on take-off if the loading is excessive—in this case, stall is not good). The point is that stall, per se, is not a bad thing providing it occurs at the proper location in the sky and providing that there is some warning to the pilot (e.g., buffet) of approaching the stall, that the lift loss beyond the stall is not too abrupt, that the stall is fairly symmetrical on both wings, and providing that the airplane does not immediately depart into uncontrolled flight.

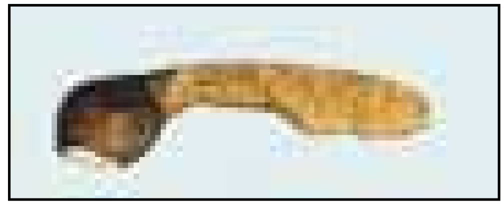


Any flight testing of a new airplane configuration must involve evaluation of stall characteristics including the impact of both slow and rapid approaches to the stall, the effect of mis-applied controls, post-stall aircraft motion, altitude lost during stall recovery, and so on—whatever is needed to ensure that the airplane is safe in the hands of a reasonably trained pilot.

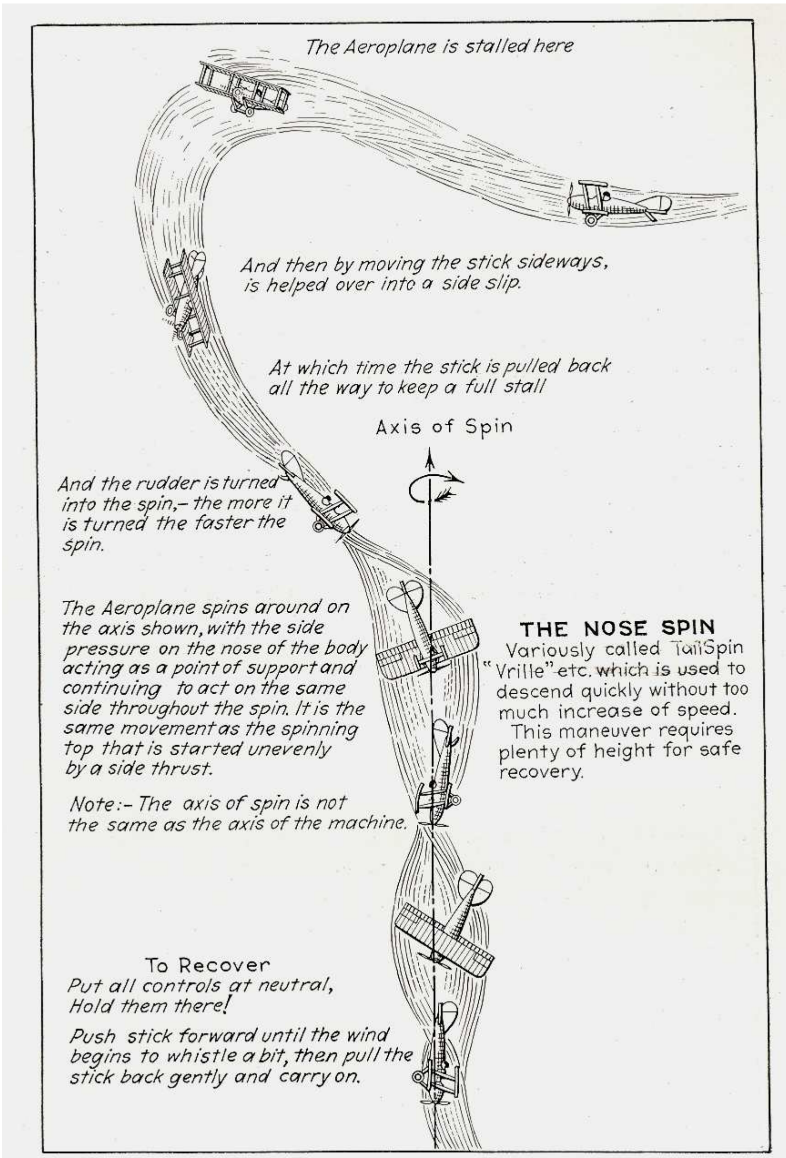
Post-stall motion of an airplane can be very unpredictable, and occasionally, the gyrations can be violent. In some cases, a *spin* may be encountered. A spin is a stable auto-rotation about a vertical axis at angles of attack above the stall. In a spin, the aerodynamic forces and moments tend to take a back seat to those resulting from inertia (gyroscopic) effects because of the low forward airspeed involved and the massive air flow separation regions in which the aerodynamic surfaces are immersed.

An airplane may have multiple spin *modes* varying from smooth motion to extremely oscillatory; the airplane nose may be nearly level with the horizon with rapid rotation about the spin axis; or the nose may be pointing well below the horizon. A spin may be upright (erect) with the sky in the pilot's view, or it may be inverted with nothing but the earth in view. The spin axis may be through some point in the airplane or displaced some distance away.

The modal qualities are greatly influenced by the aerodynamic characteristics even though the aerodynamics are relatively weak at spin conditions. What may be surprising, the steady state spin rate is a strong function of the aerodynamic *pitching* moment; the aerodynamic *roll and yaw damping* impact the bank angle; and the angle of attack (or whether the nose is high or low) is a function of the aerodynamic *yawing moment stability derivatives.* As a consequence, in a spin, the airplane doesn't always respond "normally" to pilot control input.



The most obvious spin occurrences in nature are the spins used by the seeds of maple trees and other samaras for dispersal and subsequent species propagation. When they launch from the parent tree, the seed pair clusters split into two asymmetric winged configurations. The asymmetric configuration may be likened to an airplane with one wing so completely "stalled" that it produces near-zero lift—as for the missing wing of the seed pod. The remaining seed pod wing may be stalled across some of the inboard portion of its span. The falling seed quickly stabilizes at a very "nose high, flat spin" with a high rotation rate (about 1000 rpm) but at a slow descent rate thereby allowing the wind to carry it for some distance before ground contact.



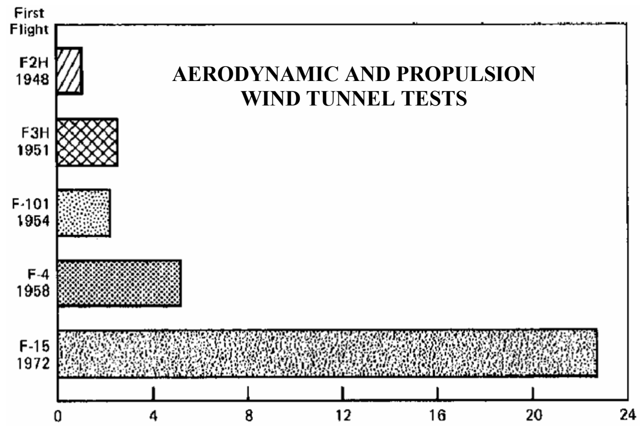
There was a time during World War I when some pilots advocated spinning the airplane as a tactical maneuver to escape a pursuer. Training manuals included instructions for entering a spin and recovering from one after the resulting desired altitude change. Note in the figure on the left that recovery was to be recognized when *"the wind begins to whistle a bit.."* However, the spin later came to be regarded as, at best, a nuisance, and at worst, a potential killer in some modern combat airplanes.

Now that the reader knows as much about stalls and spins as the writer, it's time to return to the F-15 story. The following may get a bit technical in spots; the reader will be warned so that the technical material can be skipped or pursued to further depths.

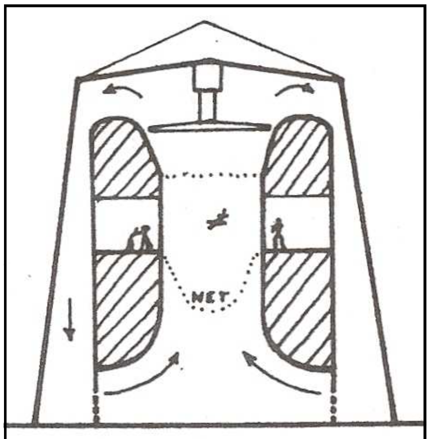
SUB-SCALE F-15 MODEL DEVELOPMENT TESTS

The F-15 development utilized the most extensive wind tunnel test program ever conducted for an aircraft. Aerodynamic and Propulsion testing alone accounted for nearly 23,000 wind tunnel occupancy-hours prior to first flight (that was about five times as many hours as had been utilized on the F-4 Phantom). This figure does not include the time for Loads, Store Separation, or Structural Dynamics testing. Wind tunnel tests continued during the flight test program as the configuration evolved or when additional information was required.

In addition to the Aerodynamics / Propulsion *force* and *moment* measuring wind tunnel tests before first flight, the program made extensive use of NASA Langley Research Center (LaRC) high angle of attack expertise in the testing of dynamically scaled free (or tethered) flight models in Langley facilities—a 1/30 Scale Spin Tunnel model (beginning in 1969), a 10% Scale Tethered Wind Tunnel model (early 1971), and a 13% Scale Helicopter-Drop, Remotely Piloted model (1972).



WIND TUNNEL HOURS TO 1ST FLIGHT (1000 HOURS)



NASA 20 FT SPIN TUNNEL

The dynamically scaled model tests provided the best "real world" experience available without using the actual airplane.

1/30 SCALE SPIN MODEL**.** The 1/30 scale model had a wing span of about 17 inches and a weight of about 2 pounds, 14 ounces. Spin tests began in 1969 during the time that the three competitors' F-15 proposals were being prepared. The model test results were actually factored into the source selection process. The tests utilized the NASA Langley vertical wind tunnel (the wind blows upwards to simulate the relative motion of an airplane descending vertically) into which the small model is launched by hand with an initial spin as if throwing a "Frisbee." Often, the model will stabilize in a certain spin mode "hovering" in space supported by the vertical wind. High speed photography is used to determine the spin characteristics (attitude and spin rate). But more important, the models are outfitted with remotely activated controls and/or a recovery parachute to define spin recovery characteristics. Once recovery from the spin occurs (or not), the model is captured in a net and used again. The 1/30 Scale F-15 Spin model tests were most satisfying. It was concluded that:

"(1) The model has both a steady and an oscillatory spin mode requiring pro-spin controls to maintain each. There was a natural tendency to recover with neutral controls.
(2) Recoveries were rapid and consistent, requiring about two turns from steady spins, and one turn from oscillatory spins with anti-spin controls…."

13% SCALE HELICOPTER-DROP MODEL**.** The 13 % scale drop model was remotely piloted and was used to evaluate departure (from controlled flight) characteristics or spin susceptibility to aggravated control maneuvers as well as to verify recovery characteristics as determined by the "Frisbee" tests. This model, tested in early 1972, had a wing span of 5.55 feet and weighed about 145 pounds. The model was un-powered, so data had to be obtained during descent prior to initiating deployment of the recovery parachute.

Again, model test results were quite satisfactory. The model was very departure resistant. In order to enter a spin, it was necessary to apply stick and rudder controls in a certain sequence which later became to be known as the "**ARI Defeat**" scheme (more will be said later about this). The spin mode and recovery characteristics determined with the 1/30 scale spin model were verified.

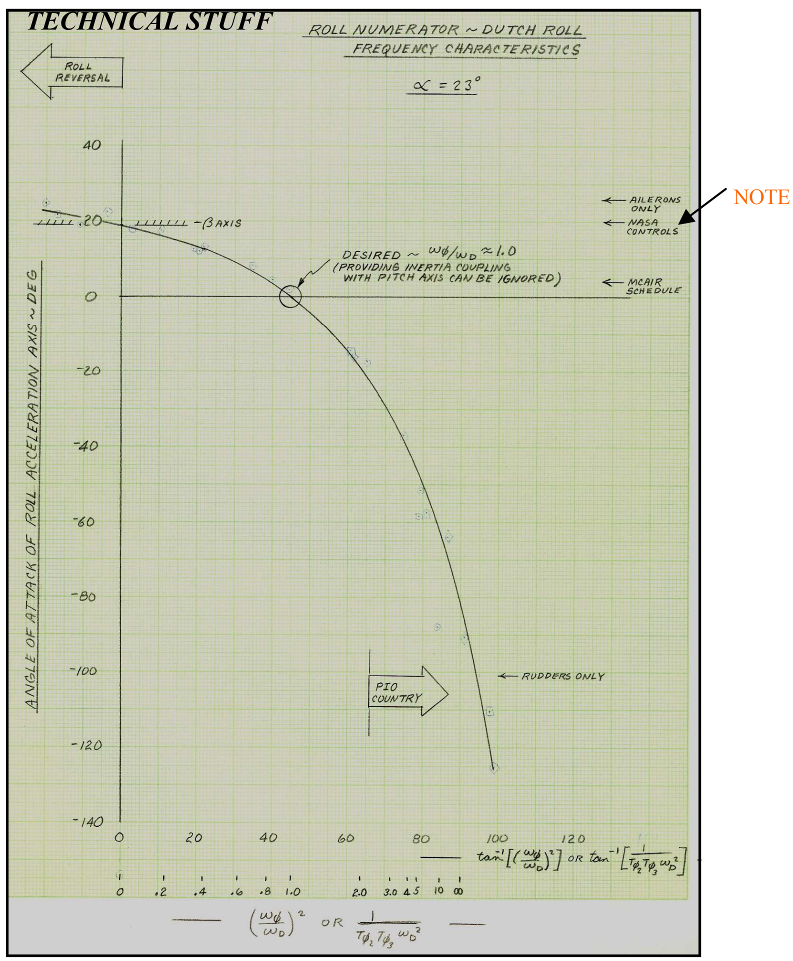
10% SCALE, TETHERED WIND TUNNEL MODEL. In between the spin model tests, one office at NASA Langley (*not* the spin experts headed up by Jim Bowman) performed some wind tunnel testing of a remotely controlled, tethered, "free flight" model F-15 in early 1971. This office created quite a stir in that the NASA office thought they had discovered a serious problem beginning at 23 degrees angle of attack. The facility wrote a damning letter to the SPO (Skip Hickey) and to MCAIR (my colleague John Havey) noting that the "present F-15" exhibits "directional divergence at high angles of attack" and that "considerable documentation of aerodynamic characteristics must be undertaken." In other words, they wanted their (large) piece of the pie. Both Hickey and Havey were on temporary assignment to Langley during these tests to protect F-15 interests. At this time, I was filling a job on the Advanced F-15 (see Page 9), but was loaned back to the Project to help put out the fire. The NASA office had attributed the "problem" to a rarely occurring phenomenon called a "coupled roll-spiral" oscillation.

TECHNICAL STUFF

Every airplane exhibits three different oscillation modes—one in which a slow oscillation in altitude and speed occurs trading back and forth between potential and kinetic energy; one quick oscillation mode in which the airplane pitch attitude and angle of attack "vibrate" much like a spring-loaded, swinging saloon door; and one involving the roll and yaw axes in which a hunting occurs in bank angle, heading angle, and sideslip. Although these oscillations generally cause no problems, tracking tasks compromised by the latter two can be improved with use of automatic stability augmentation systems. A fourth mode is extremely rare—a very slow oscillation involving the roll and yaw axes, a "coupled roll-spiral" which is at a frequency very nearly impossible for a pilot to control.

In 1967, when NASA was heavily involved in "lifting body" technology (which ultimately led to the Space Shuttle), Test Pilot Bruce Peterson was nearly killed in the crash of the M2-F2 lifting body. It is widely believed that a "coupled roll-spiral" oscillation caused the spectacular crash. Peterson, although battered badly and losing one eye, had the distinction of having his crash shown in the 1973 movie, "The Six Million Dollar Man" as well as at the beginning of every program over and over again in the weekly television series of the same name.

My job back on the Project was to discount the NASA conclusions and find out the real reason for their loss of control with the "free flight" model. With the expertise of Engineer, Dick Thomas, it did not take long to conclude that the real problem was one caused by NASA themselves—they were using their own control system schedules for the blending of the aileron and rudder programmed as a function of the amount of lateral control stick applied. As a result, the model experienced "roll reversal"—control stick input in one direction resulted in rolling in the opposite direction! On the contrary, the aileron-rudder interconnect (ARI) schedule we, MCAIR, had developed nearly two years earlier worked quite well. The problem was not the dreaded roll-spiral coupling.



Needless to say, we got the NASA office off our backs.

The time spent stomping out this unnecessary fire was resented somewhat at the time, but, overall, the exercise didn't hurt us. It enabled us to more fully inform our management and the customer of the effort we had been devoting to high angle of attack maneuvering during the previous two years.

TECHNICAL STUFF

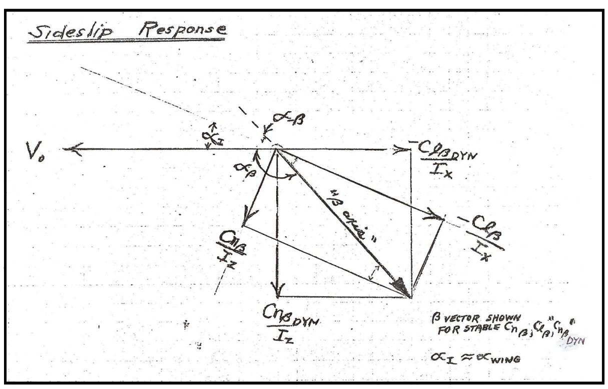
An esoteric but interesting and troublesome fact about sub-scale model testing—it is impossible to completely replicate the full size airplane characteristics with a sub-scale model. There happen to be eight relations to duplicate, but there are only seven variables. So, one has to be very choosy. Where air compressibility effects (shock waves and things) are most important, it's important to match Mach number (the ratio of airspeed to the speed of sound or, actually, the ratio of Inertia to Elastic forces). Where viscous skin friction predominates, a good match of Reynold's Number is important (the ratio of Inertia to Viscous forces). For the case of dynamic spin testing where Aerodynamic forces take a back seat , it's important to match Froude number (the ratio of Inertia to Gravitational forces)—named for father and son William and Robert Froude who developed the concept for testing ocean-going vessels during the 1870s.

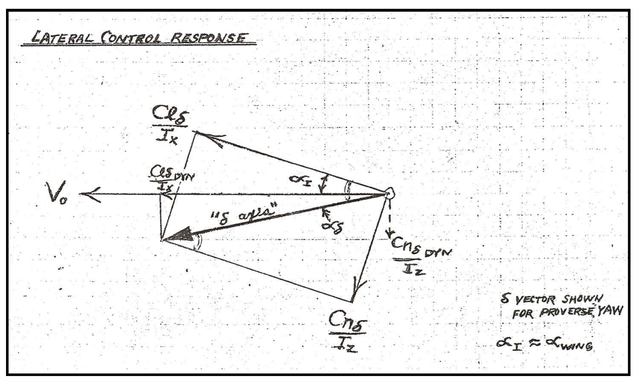
Consequently, there are certain rules for scaling a small spin model to match the characteristics of the real airplane: ***[Model Weight] = [Airplane Weight] x [Scale Factor]***^{3} *if tested at the same altitude. So, the weight for a 10% (1/10) scale model should weigh 1/1000 that of the real airplane. A further adjustment would be required if tests were not performed at the same altitude.*

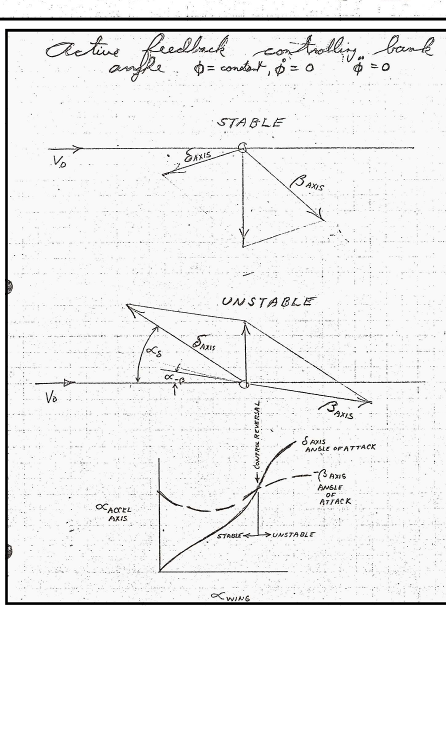
Further application of the rules would then result in the model spinning much faster than the real airplane. If Ω**= spin rate=** $(p^2 + q^2 + r^2)^{1/2}$ *with* ***p,q,r*** *representing roll rate, pitch rate, and yaw rate, and with* λ *representing the scale factor, the real airplane spin rate would be represented with the equation,* $\Omega_{full\ scale} = \Omega_{model}\ (\lambda)^{1/2}$ *. Thus, for the 1/10 scale example, a model spin rate of 300 degrees/second would represent the real airplane with a spin rate of 95 degrees/second*

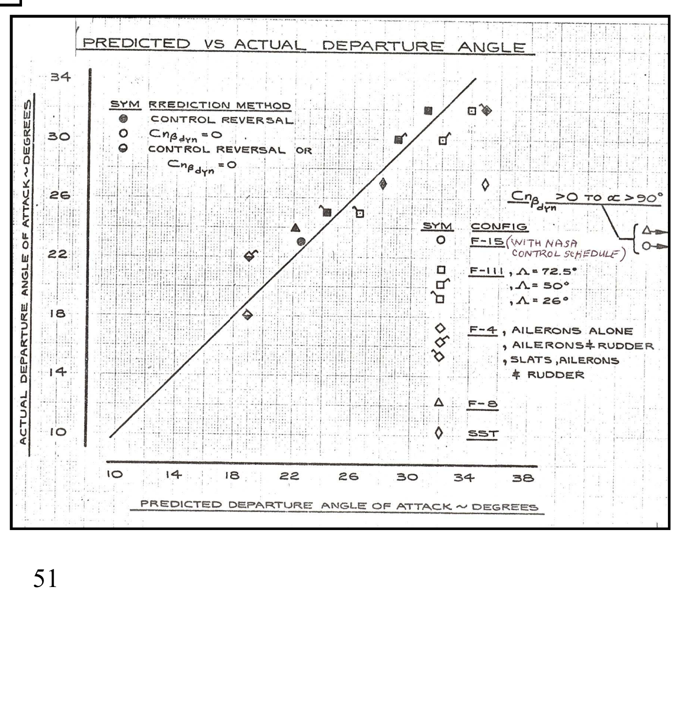
ANALYTICAL PROGRESS

The pace of high angle of attack analyses we had set during the proposal period never let up. We developed airplane departure prediction criteria which were new to the industry.

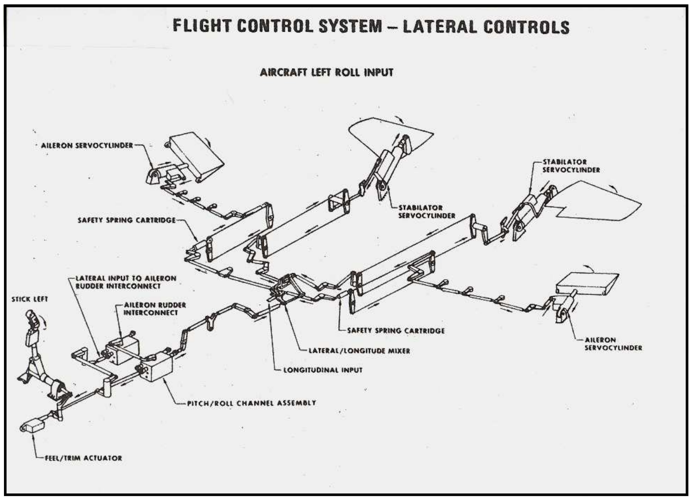


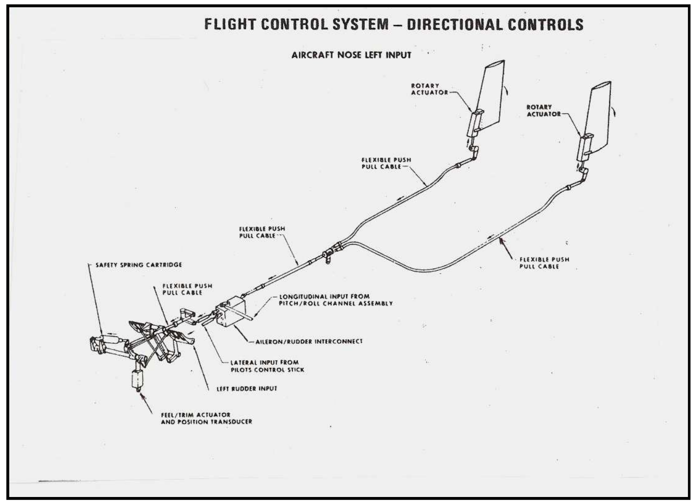


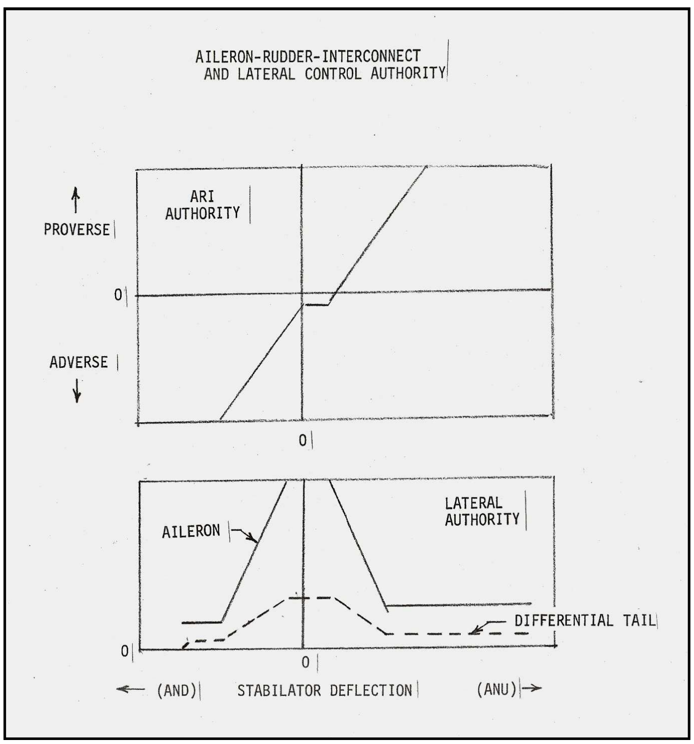


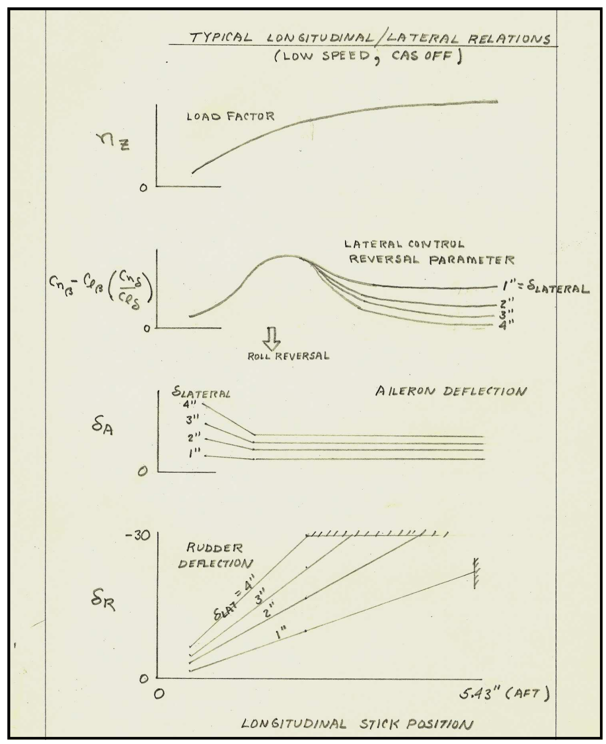


Using the results of NASA's spin model tests, we fine-tuned our aerodynamic data base. Our piloted simulations increased our confidence in the mechanical control scheme in allowing true "feet-on-the-floor" roll maneuvering at high angle of attack. The blending of lateral control authority with the commanded rudder deflection via the Aileron-Rudder Interconnect (ARI) was validated as a tremendous improvement over any other scheme ever flown.







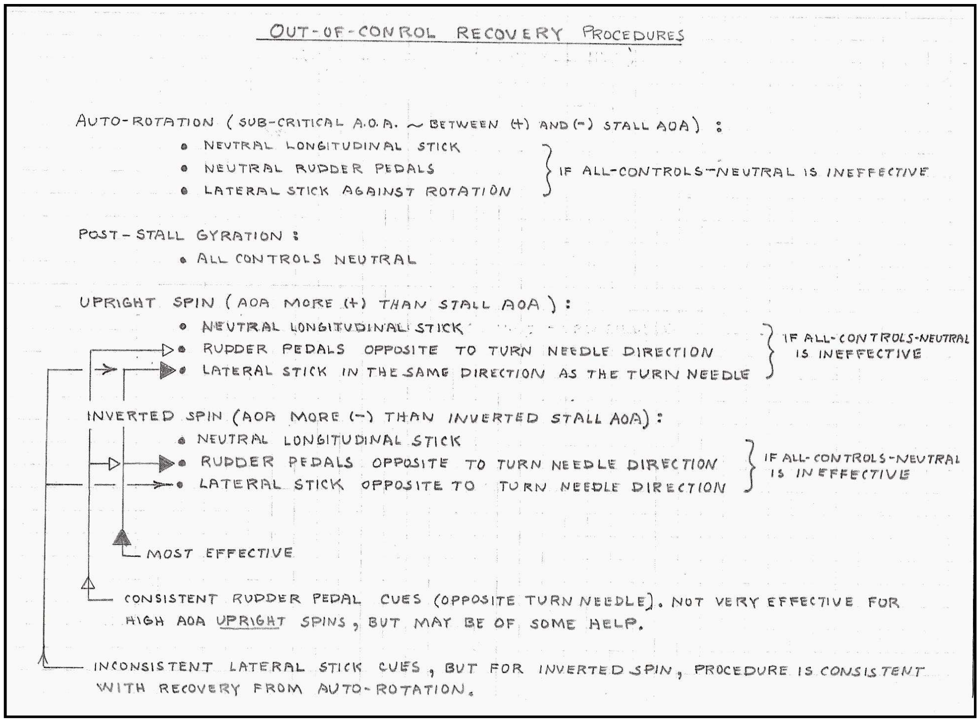


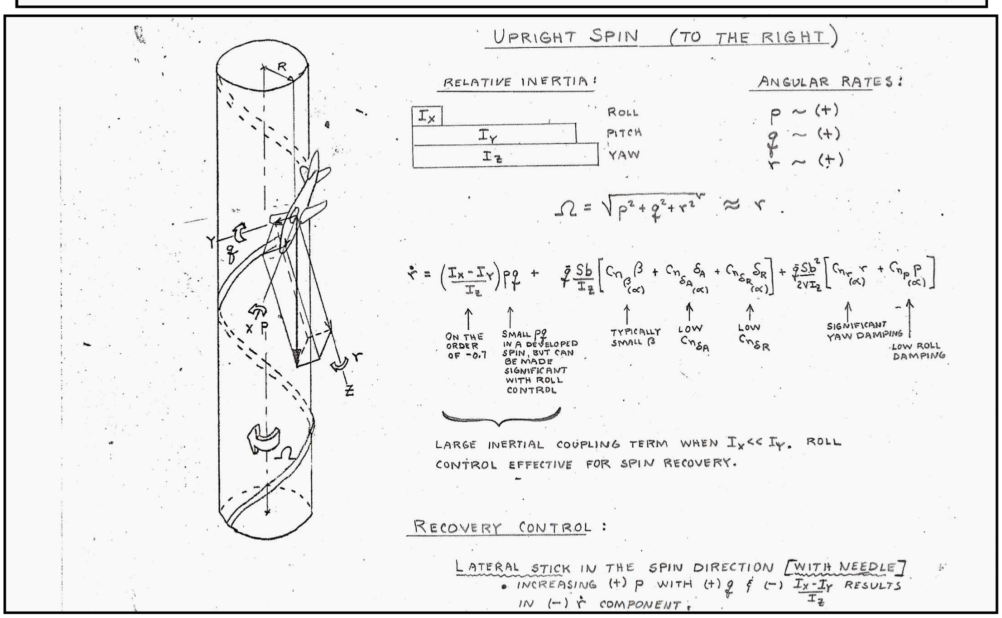
By the time of first flight in July 1972, we were confident that we could conduct a safe high angle of attack / stall / spin flight test program. Two upright, or erect, spin modes for the airplane had been identified. Although we did not have a data base or model experience to identify an inverted mode (upside down), we anticipated that there would be an inverted mode.

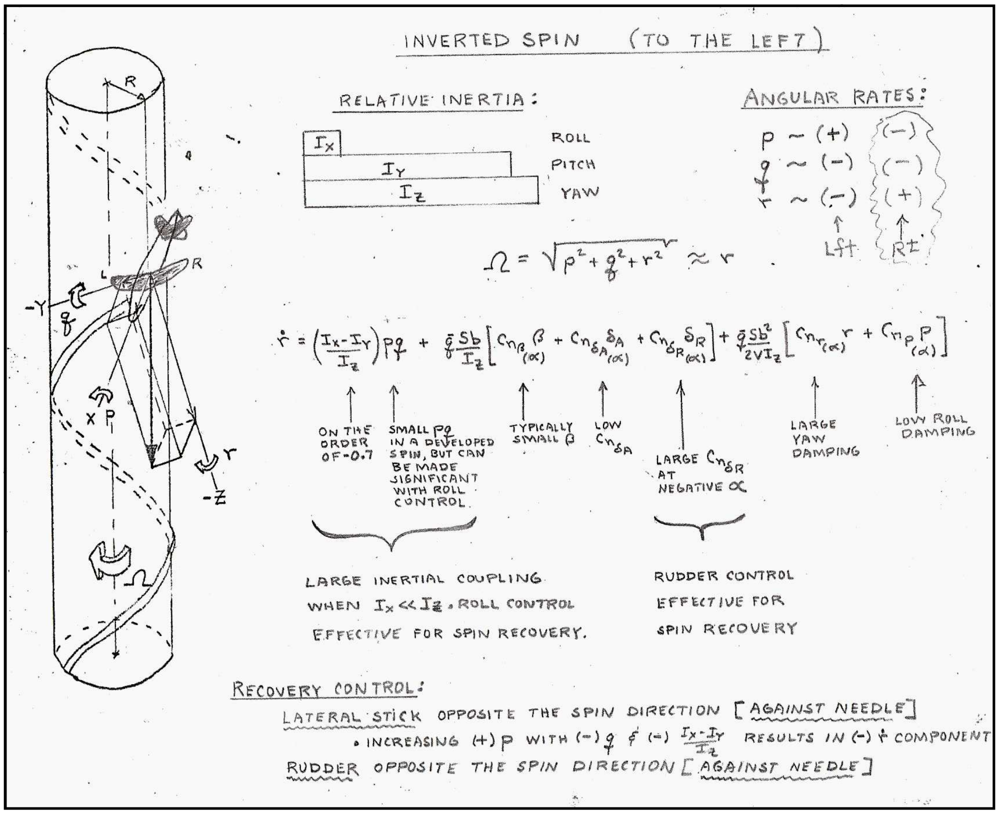
The primary upright spin mode was a steady "flat" spin anticipated to occur at angles of attack varying from about 65 to 82 degrees with a fast spin rate of about 100 to 170 degrees per second. The spin would be faster and flatter with increased amounts of pro-spin control deflection. At these angles of attack, the airplane nose would be pointing from eight to 35 degrees below the horizon. Our spin attempt simulations occasionally resulted in a second mode, an *oscillatory* spin whose angle of attack varied from about 50 to 80 degrees and with a nominal spin rate of about 80 degrees per second.

Of course fitting into a specific mode does not mean that the motion of every spin in that mode is identical—it merely means that there are significant similarities in the characteristic motion.

By this time, we were confident that we had defined the proper control system and could define the necessary pilot input to recover from a spin. The key requirement was for the pilot to recognize whether the aircraft was in an upright or an inverted spin and to scan the cockpit instruments to ascertain the spin direction (right or left) particularly for an inverted spin which can be quite disorienting.







HAZARDOUS FLIGHT TESTS

All flight testing has an element of risk associated with it. Trouble can arise from many sources—some as mundane as the use of the wrong fastener during assembly, a factory worker's tools left inside the airplane during assembly, or the use of the wrong polarity in an electrical component (wired backwards). All these and others have happened on more than one occasion—some with the subsequent loss of the aircraft. More sophisticated risks may involve undetected material flaws leading to premature failure of some critical component (tires, wind shields, engine turbine blades, etc.). The known risks which get the most attention are those associated with explorations of previously untried concepts or of areas that experience with similar test articles have shown to be of a high risk.

To address the last case, a test or series of tests are labeled in advance as "Hazardous Tests." This category receives a lot of deserved attention within both the engineering and program management disciplines. For the obvious concerns, seemingly countless scenarios are considered, safety reviews are held involving the appropriate Contractor and Customer personnel, contingency plans are formulated, and back-up equipment is incorporated in the event of mission-critical equipment failures.

High angle of attack / stall / spin tests fall into the "Hazardous Test" category for good reason. Since the beginning of the jet age, particularly for fighter aircraft, the airplane designer has been met with new challenges—configurations employing highly swept, thin wings and densely packed, "fuselage loaded" layouts. The previously unknown aerodynamic in the transonic speed regime was not fully understood (and is not completely comprehended to this day). To cope with the higher control surface air loads, the use of hydraulics to aid the pilot was necessary—this led to a loss of natural airplane "feel" to the pilot handling the controls; artificial feel systems had to be developed.

The growing pains of aircraft design during this time were many. The problems that early Air Force fighters had (and the Navy aircraft experienced similar characteristics) have been best summarized by Skip Hickey of the F-15 System Program Office. He wrote,

"In order to better understand the F-15 'Spin Story' we should look back a few years to the mid 60s when most of our first-line combat fighter aircraft suffered from unacceptable flying qualities at high Angles of Attack (AOA). The tactical effectiveness of these aircraft was severely limited. Furthermore, the pilot's main concern in a high maneuver environment was often aircraft departure and loss of control. 'Pitch up' characteristics of the F-101 and F-104 caused by loss of pitch stability resulted in the loss of many of these aircraft. The loss of directional stability at high AOA caused a yawing motion or 'nose slice' on F-4Cs and Ds. Adverse yawing motions or actual 'roll reversals' often occurred with the F-4 and F-100 during high AOA rapid rolling maneuvers. And finally, after a 'departure' from normal flight control, many of our fighter type aircraft (F-4, F-5, T-38, F-100, F-101, and F-104) developed certain spin modes which were unrecoverable."

Even flight testing under controlled conditions and with what were considered to be adequate safeguards resulted in some unpleasant surprises. Dedicated high angle of attack test aircraft flown by highly skilled test pilots and supported by staffs of engineer experts were lost due to unrecoverable spin modes. McDonnell Aircraft test airplane losses included two versions of the F-4 "Phantom II" (both a Navy and an Air Force version), two versions of the F-101 "Voodoo" (a single-place F-101A piloted by future MCAIR President, Bill Ross), and a two-place F-101B), and an F3H "Demon.." *(Actually, the F3H wasn't a total loss, but it certainly was an embarrassment. Spin tests were being conducted on the F3H. When the airplane refused to recover from an inverted spin, the emergency spin recovery parachute was deployed. After several more turns with no hint of recovery, the test pilot bailed out. Soon thereafter, for whatever reason, the airplane righted itself, flew for 150 miles until running out of fuel, and then landed perfectly <but wheels up> in a farmer's field. There may have been more damage to the pilot's pride than to the airplane.).* Pete Winters, our Air Force spin pilot had lost a General Dynamics F-111 during a spin test when it wouldn't recover.

Understandably, there was concern on all fronts about the risk associated with the forthcoming stall and spin tests. Not only was the adequacy of the aerodynamic data base and the robustness of the flight control system questioned, but also the operability of the engine / inlet system had to be thoroughly addressed. (The engines were being developed concurrently with the airplane). Contingency plans in case of an otherwise unrecoverable spin called for the incorporation of a 32 1/2 foot diameter spin 'chute fired by a mortar from a canister between the vertical tails of the high AOA test aircraft. In addition, an auxiliary power source or Emergency Power Unit (EPU) to operate hydraulic pumps and a battery pack for electrical power were incorporated in the event the engines quit and slowed down to a very low rotation speed.

Dryden Flight Research Center ECN-3804 Photographed 1973
F-15 Remotely Piloted Research Vehicle mounted
under the wing of NASA's B-52. NASA photo

ANOTHER SUBSCALE MODEL TEST

NASA Dryden Flight Research Center (DFRC) at Edwards AFB came on the scene much later than their counterparts at Langley with a subscale model of the F-15. The model was of 3/8 scale with a wing span of 16 feet and a weight of 1800 pounds). In addition to my other chores (such as dealing with the last chapter's supersonic directional control problems at the time), I was designated to be the MCAIR contact with NASA Dryden.

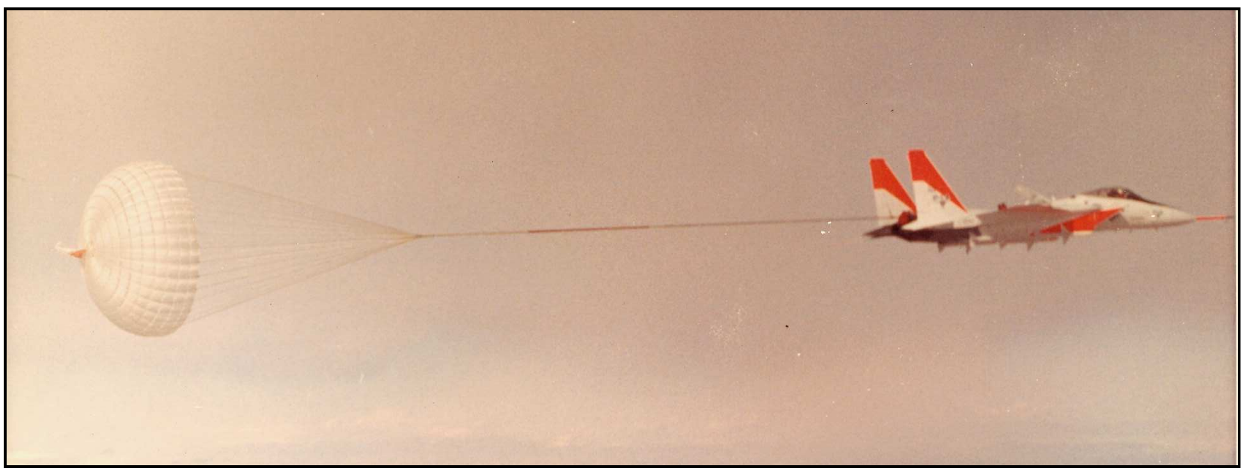
One month prior to the beginning of the full-scale high AOA program, NASA made the first flight of the 3/8 scale Remotely Piloted Vehicle (RPV)—12 Oct 1973. The model was launched from a B-52 (the same airplane that had launched the X-15 research airplane and other NASA research vehicles). Launch conditions (typical for the remainder of the program) were 45,000 feet altitude at 175 knots calibrated airspeed. The first drop was primarily a check-flight up to about 26 degrees AOA, but some engineering data were obtained with the use of their "derivative extraction" algorithms, a technology in its infancy at the time.   The RPV descended to about 15,000 feet in about 10 minutes at which point, a parachute was deployed, and a helicopter from Pt. Magu snatched it at about 10,000 feet and delivered it to the NASA ramp.

The RPV pilot, Einar Enevoldson, sat at a ground-based simulator in front of an instrument panel with telemetered flight data and a TV screen with a view out the front of the model's "cockpit."   I attended the debriefing for the first flight and was amazed at the size of the crowd. The event appeared to be more momentous to NASA than was the first flight of the real airplane to us at MCAIR!

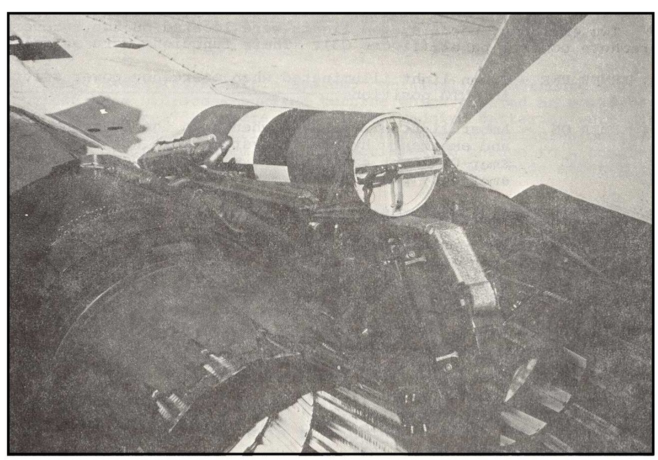
The second 3/8 scale model flight occurred on 8 Nov 1973. I had previously bet a martini with one of the NASA engineers that the model would *not* depart from controlled flight during the planned test. The NASA experts were adamant that the model would depart at 29 degrees AOA. I was late for the post-flight debriefing—as I entered the theater, I overheard Bruce Peterson (the pilot discussed on Page 49, now wearing an eye-patch) remark, "I'm keeping one eye out for Abercrombie!" Needless to say, I had won my bet; the model was very well behaved during the test; and the NASA experts were very favorably impressed with our control system and its control of the model.

RISK REDUCTION VERIFICATION

Continuing this narrative somewhat chronologically, while NASA continued flying the model, we got further along with the real airplane with its planned high AOA program, but it wasn't until January 1974 that we began to check out the operation of one of the safety devices—the emergency spin recovery parachute. The first deployment at a rather benign flight condition was successful.   And then, we had a series of flights to make certain the presence of the parachute canister / boom installation between the two vertical tails would not adversely affect the airplane flying qualities.



The Emergency Power Unit (EPU) burned hydrazine rocket fuel to provide power for driving the airplane hydraulic pumps in the event of a dual-engine flame-out with subsequent loss of engine RPM. (A small battery pack was provided for necessary systems electrical power). The EPU became available in mid-April 1974. Ground and flight tests of the unit continued for a seven week period. However, on 7 Jun 74, the EPU tests came to a screeching halt. The system developed a leak while in flight. The pilot, Pete Winters made an emergency lake-bed landing and immediately evacuated from the cockpit whose rear section was floating in hydrazine!



The hydrazine leak was a serious matter—hydrazine is highly flammable, very corrosive and toxic, and can do terrible things to the lungs. Although hydrazine fuel had been used in the successful space capsule programs (and is now standard equipment in the later F-16 ***single engine*** airplane), the risk of continuing to employ it on the already high risk, high AOA F-15 program was considered too great. On 13 Jun 74, the powers-that-be decided to stop work on the high AOA program until the test airplane could be out-fitted with a battery pack large enough to drive the hydraulic pumps. Consequently, the hydrazine EPU was dropped from the picture. (These decisions were well beyond my pay grade and level of expertise).

The EPU set-back added fuel to the fire in ongoing discussions about ***not*** conducting a full-blown spin program. Momentum was building both at MCAIR and within some portions of the Air Force to do all that was necessary to evaluate stall and departure characteristics using "reasonable" amounts of aggravated control inputs (well beyond what would normally be encountered in operational use) but not to risk deliberate spins. The philosophy of emphasizing "spin avoidance" in the test program rather than demonstrating "spin recovery" was becoming very popular.

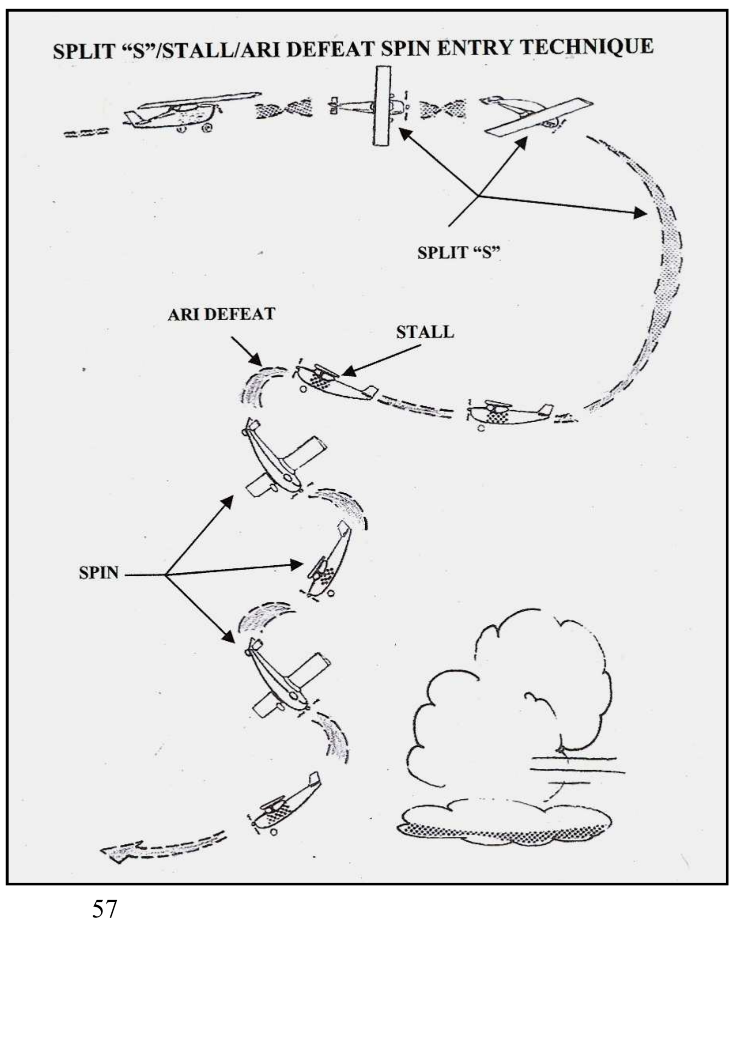
ANOTHER MARTINI BET

Back in February 1974, MCAIR Test Pilot, Denny Behm, flying the NASA Dryden simulator found a good way to make the mathematical model of the airplane spin using a procedure similar to that used with the NASA 13% scale Drop Model—manipulating the controls to defeat the blending of the rudder and lateral controls functions of the control system. Recall the Aileron-Rudder Interconnect (ARI) discussion earlier. At certain flight conditions, by pulling the stick all the way aft to achieve a stall angle of attack, putting in roll control in one direction while cross-controlling the rudders in the opposite direction, then dumping the stick forward so that maximum cross-control deflections existed while the airplane was still at high AOA, occasionally a spin would develop. But the maneuver had to be entered at a high kinetic energy condition, and the controls manipulation timing had to be "just right." This technique was referred to as the "ARI Defeat" maneuver.

NASA DFRC plans for the fifth drop of the 3/8 scale RPV on 6 Mar 74 included the "ARI Defeat" maneuver to be performed at essentially a level flight, low energy condition. I won another martini bet from my Edwards supervisor, Hank Rechtien—the model did ***not*** spin.

Finally, on the seventh RPV flight on 21 Jun 74, a spin was induced by using the "ARI Defeat" technique but from a ***high energy, split "S"*** maneuver rather than from a one-g condition. (There were no martini bets on the outcome of this attempt, as all agreed a spin was most likely). The spin mode was the smooth, flat one which had a spin rate of about –195 deg/sec (corresponding to –120 deg/sec when reduced to the scale of the real airplane). Recovery control input broke the spin in about 3 1/2 turns.

The 3/8 scale model spin was met with a brief flurry of interest by the technical and pilot communities, but what really stirred up some excitement was an event only four days later with the real airplane—F-15#1.



"...THERE'S BETA TAKING OFF—AND—AROUND WE GO…"

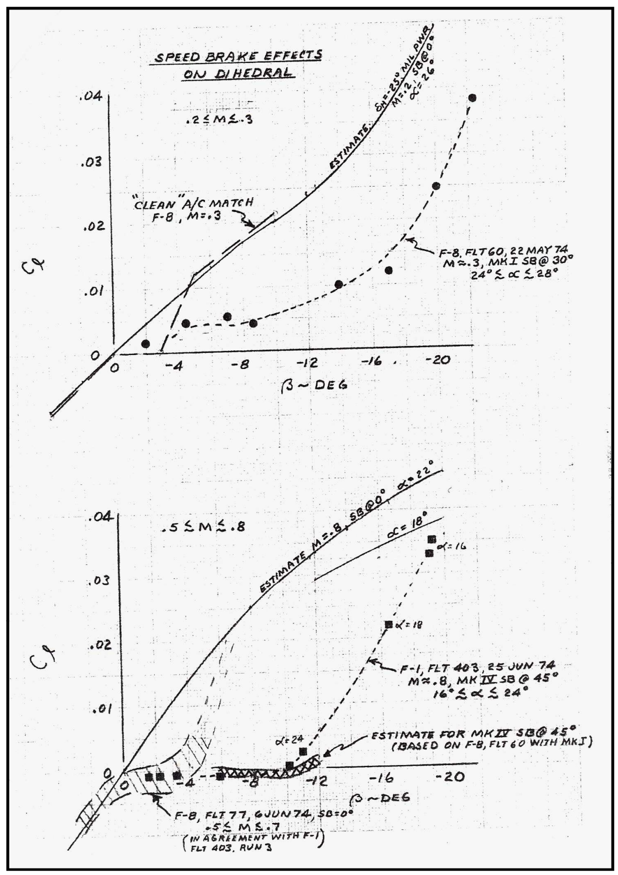
The real airplane test program took a turn, so to speak, in the afternoon of 25 Jun 74 on a test flight whose purpose was to demonstrate the adequacy of the speedbrake to do its job of decelerating the airplane. At the time, F-15#1 was the only airplane configured with the larger Mark IV Mod C speedbrake which had been defined for the production airplane (see Page 32), so it was the one chosen to fulfill the test requirement. The test plan called for a decelerating wind-up-turn (a maneuver which combines pulling g's while in a banked turn) at idle engine power beginning at M=1.2 at 40,000 feet with simultaneous extension of the speedbrake.

All was routine until the airplane suddenly departed to the right (the nose sliced in a right yaw without any control input) at about 20 degrees AOA, M=0.81 and began to roll. In the radio transmission words of the test pilot, Denny Behm,

> **"There's Beta taking off—and—around we go, and we're in a —un-commanded roll situation. Son of a bitch!...[**46 seconds later] **Breathing again! O.K., I think I've got a heartbeat—I just felt it...Holy shit!..."**

In my words (somewhat technical), written late that evening,

> **"...the right yaw departure began. Normal corrective action of stick forward and left (briefly) was applied with no significant effect on angle of attack and none on roll rate...About two** [compass] **turns were experienced before the spin actually developed...Following about two more oscillatory turns with a probable angle of attack oscillating about 70 degrees** [beyond F#1 instrumentation limit], **full anti-spin lateral control (right stick) was applied. Sub-critical** [below stall] **angles of attack ...were reached within two turns…"**



There were other factors contributing to the excitement: the LH engine stagnated during the departure followed soon after by the RH engine. Shut-down of both engines because of high turbine temperature resulted in loss of telemetry for about 15 seconds during the recovery. Re-start was slow but uneventful. The airplane was fully recovered with both engines operating at 15K feet.

Although we had observed briefly on earlier flights that the speedbrake (either the small, pre-production or the larger Mark IV Mod C) could contribute significantly to departure susceptibility, there had not been sufficient time to completely evaluate the repercussions.

But I had seen enough—I immediately recommended that we automatically preclude speedbrake deflection at the higher angles of attack. The recommendation was subsequently adopted some time later.

Another anomaly was noted—the mechanical ARI was not engaged at the time when it could have been helpful. The ARI was designed to be inoperative supersonically and to be operative only at subsonic speeds. During the deceleration from M>1 to M<1, the mechanism hadn't had time to re-engage. My recommendation written that evening to design the system with a quicker ARI turn-on feature when decelerating through Mach 1.0 was also incorporated into production.

But most important, when the test data system were re-interrogated using a different data format, it was discovered that there was considerable Left-Right wing fuel asymmetry due to a faulty fuel system design. The airplane was about 1250 pounds left wing heavy, which was equivalent to a 10,000 ft-lb moment. Apparently, as we later learned, this was nothing new—the airplane had a history of fuel asymmetry. Several of us both at Edwards and in St. Louis share the blame for not observing the situation and raising a red flag. Communication between the fuel system designers, the flight test and flying qualities engineers, and the pilots could have been better had any of us realized the importance of the situation.

In any event, the combination of the speedbrake-induced lateral instability and the wing fuel asymmetry caused the spin. The inoperative ARI, if it had it been operating per the design charts, could have ameliorated the departure problem. But the cards were stacked—there was no way a spin could have been avoided under the circumstances.

In addition to those lessons learned, we learned something else—**We would now have a spin program!** Unfortunately, because of the EPU decision made less than two weeks before, we didn't have a high AOA test airplane!

To recap the 13 days in June 1974 which changed the program so significantly:

- 13 June—Work stopped on the hydrazine EPU for the high AOA test airplane F#8 to await a battery EPU.
- 21 June—NASA DFRC spun the 3/8 scale F-15 model.
- 25 June—F-15#1 made an inadvertent spin.

THE STALL / DEPARTURE / SPIN PROGRAM RESUMES

For the next three months while awaiting the new battery pack EPU, NASA DFRC made six more flights of the 3/8 scale model. Although their effort contributed nothing to the F-15 design, the model test results gave us more confidence in the correctness of our design.

We used this time to "lobby" for control system modifications to retract the speedbrake above a certain angle of attack and to enable a quick turn-on of the ARI, and the fuel system designers worked to gain a better understanding of the cause of the fuel imbalance problem.

Finally, in late September '74, the new EPU was ready, and the high AOA program resumed with another check of the emergency spin 'chute. Alas, the 'chute streamered, and we were down for another three weeks while the installation was re-designed. At last, on 29 Oct 74, Jack Krings in F-15#8, Flight 122, after *nine* attempts using the "ARI Defeat" technique, managed to spin the airplane and recover successfully. The next 22 flights documented the effects of the speedbrake and lateral fuel asymmetry. In addition, aggravated control inputs during gross maneuvers were evaluated. But only with deliberate spin attempts and/or large fuel asymmetries were spins accomplished. In these 22 flights, 10 spins were made. Both the predicted oscillatory mode as well as the smooth, high rate mode were experienced. In all cases, recovery was rapid upon application of recovery controls.

THE "ONE MORE TURN" MODE

We had some excitement on 9 Dec 74 (F-15#8, Flight 145) when a spin was encountered which didn't appear to go by the rules. My report of 16 December summarizes the event.

> A spin chute recovery was made at 22,000 feet in accordance with established procedures on flight 145. The following summarizes the significant events of the maneuver leading up to the spin, the spin characteristics, and the subsequent recovery:
>
> - Departure in the PA configuration (production speedbrake with the 45° actuator) occurred at 26° angle of attack.
> - Attempts to control the directional motion with the lateral stick merely aggravated the yaw excursions.

- Even with neutral lateral-directional control, the yaw rate and angle of attack continued to increase reaching a spin condition at 31,000 feet with the angle of attack peaking at 74° and the yaw rate peaking at –71° per second.
- Anti-spin lateral control reduced the angle of attack and yaw rate to a mildly oscillatory condition with a nominal angle of attack at 55° and –48° per second yaw rate. After a little more than 1 1/2 turns with anti-spin control maintained, recovery was not apparent. The pilot elected to clean up the aircraft, retracting the gear and flaps.
- Upon gear retraction, anti-spin control authority was effectively lost for about two turns (the lateral control authority reverted to the washed out, near minimum schedule) with attendant increases in angle of attack and yaw rate.
- During the next two turns, while still maintaining anti-spin lateral stick deflection, angle of attack decreased slightly to about 53°; and yaw rate stabilized at –52° per second. (Yaw rate remained at a constant level for about 5 1/2 seconds).
- At 22,000 feet, the controls were neutralized, and the spin chute was deployed (a ground rule established many months ago); and recovery occurred in one more turn.

A total of eight turns were made with an altitude loss of about 10,000 feet (the altitude loss was very nearly in perfect agreement with estimates). Anti-spin lateral stick deflection was maintained for a total of 29 1/2 seconds or about 4 3/4 turns.

Whether the two turns during which yaw rate stabilized prior to spin chute deployment could be called a new spin mode is debatable. Although the angle of attack was decreasing slightly, there was no indication that the yaw rate was decreasing. The characteristics during this time were in close agreement with those observed very briefly on the NASA 13% as well as the 3/8 scale spin models for <u>neutral</u> lateral control. But even on the models, this characteristic was not identified as a separate mode, as it was never allowed to proceed beyond about two turns. Piloted simulation studies last spring revealed a similar characteristic, but at that time, the accuracy of the simulation was in question. Needless to say, analysis is proceeding.

Although this spin lasted for almost 30 seconds through eight turns total and had never before occurred with the full-scale airplane, it could not be ***positively*** identified by the experts as a ***new*** mode. There were some of us, however, who had suspicions.

Subsequent St. Louis simulations during the next few weeks with updated aerodynamics suggested that had recovery controls been maintained for just ***one more turn*** (rather than popping the 'chute), the airplane would have recovered. We'll never know.

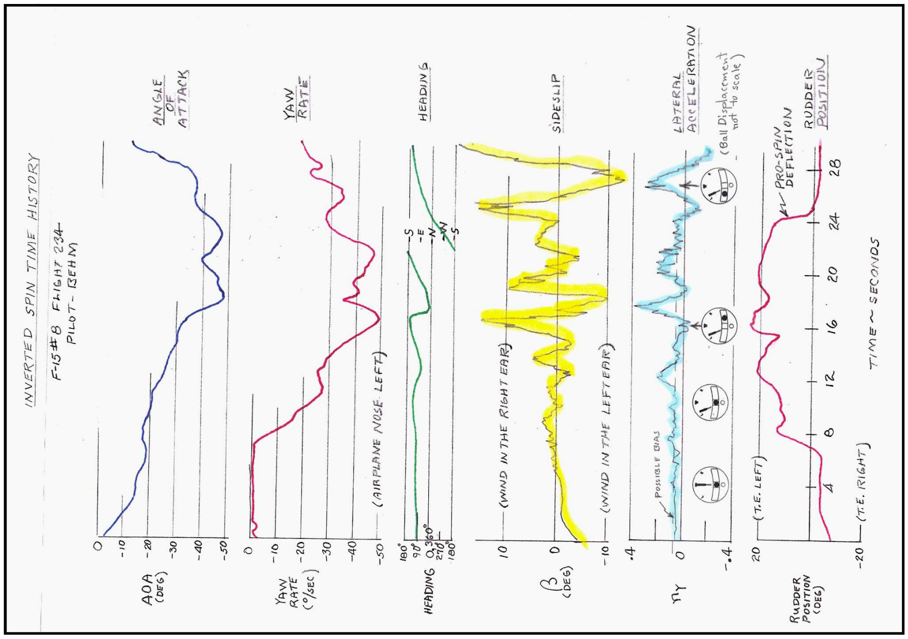
This spin became known (perhaps derisively) as the ***"One More Turn Mode."***

THE TRIVIAL INVERTED SPIN

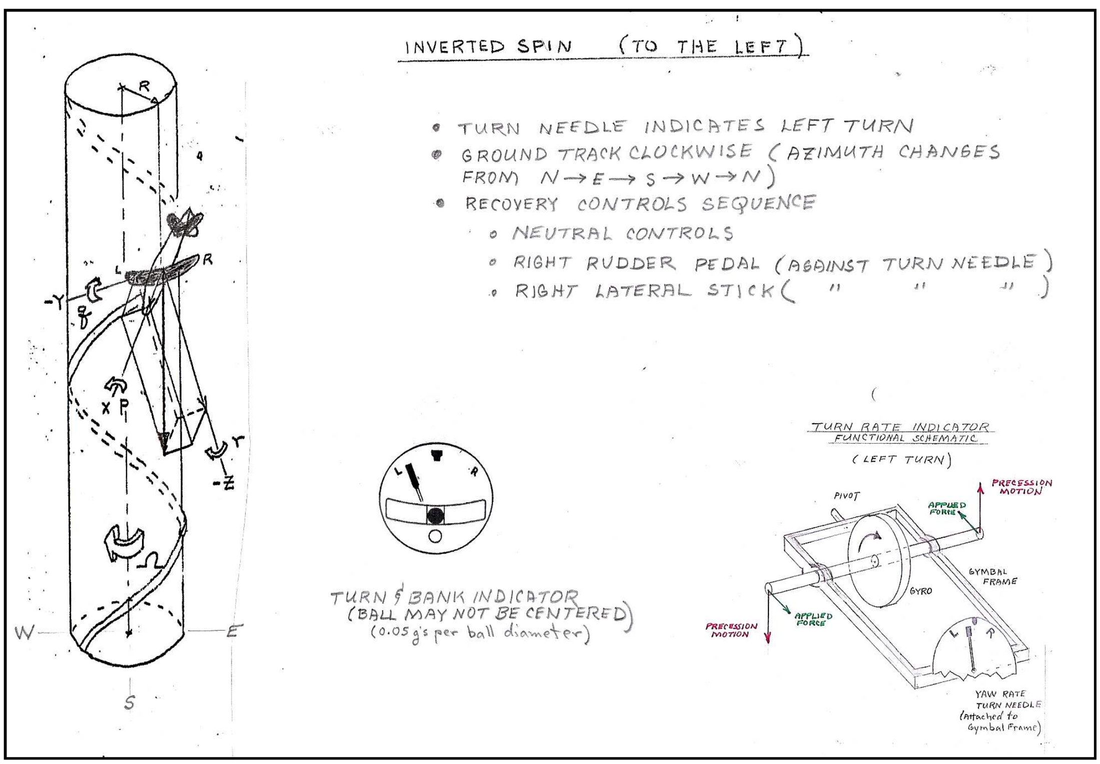
We had no data base for large negative angles of attack. Nor had NASA exercised the small models in inverted spins. Thus, we had no real handle on how the airplane would behave in this regime. Fortunately, in October 1974, NASA's 3/8 scale model did some inverted spin work with satisfactory results. Consequently, we (or at least I) had no qualms about inverted spins for the real airplane which commenced in March 1975.

I recently took some good-natured FLAK for referring to the F-15 *inverted* spin mode as "trivial." In fact, from my perspective as an engineer sitting behind a desk with a slide rule, a time-history of an inverted spin, and a copy of the six-degree-of-freedom equations of motion, the F-15 inverted spin *was* trivial. It was easy to get into—roll the airplane upside down, push the stick into either the right or left forward corner, and hold it there a few seconds. The spin motion was mild (as spins go) with yaw rates of about 36 to 50 deg/sec with angles of attack of about –40 to –50 degrees. And recovery was rapid—let go of the stick (or move it back to the normal centered position), and the airplane self-recovered. However, there *were* occasional engine stalls during the spin as well as unpleasant airplane gyrations during recovery.

Test Pilot Jack Krings had this to say about the inverted spin, **"...the inverted spins...are terrible. You have to do a lot of inverted spins before you get anywhere knowing which way you are going. You are not too sure which way the airplane is turning in a inverted spin because the world is not really working the way it should."**



Indeed, a few months ago, I revisited the issue by trying to visualize the view from the cockpit of the airplane performing an inverted spin. I found that the task can twist one's mind out of shape, particularly when on the outside looking in. In the process, I made some enhancements to the sketch of Page 54 with some gyroscopic information. It may not help the reader much in trying to visualize the inverted spin, but it helped me some 30+ years after first becoming involved with the issue.

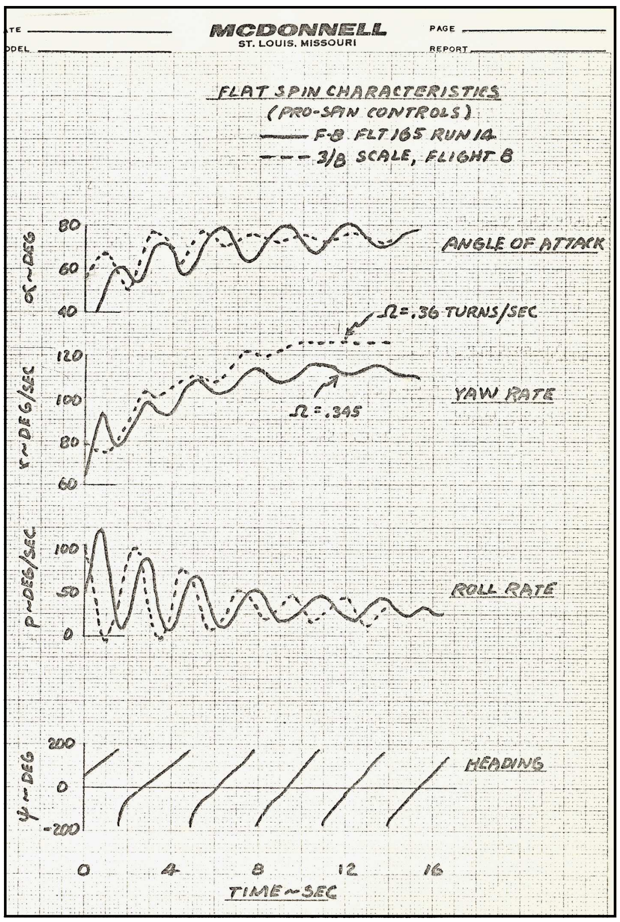


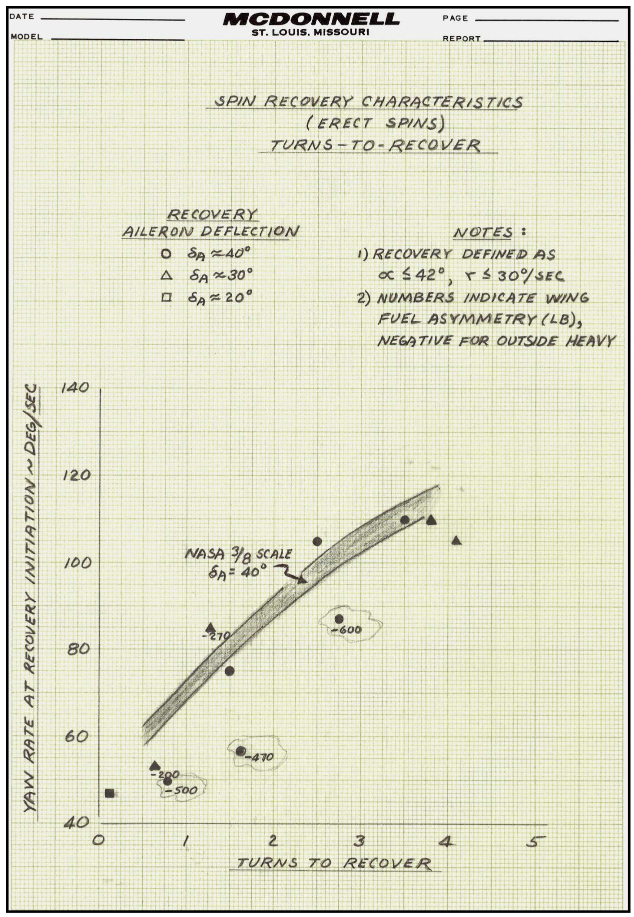
COMPLETION OF THE SPIN TEST PROGRAM

Prior to the first inverted spin of 25 Feb 75, we had accomplished 19 spins total (including the inadvertent spin with F-15#1). During the remaining six months of the program, another 97 spins were made (for a total of 116) from a total of about 200 deliberate spin attempts. With enough practice, the spin pilots were able to reach spin conditions using techniques other that the Split-S entry, but always had to use a version of the "ARI Defeat" technique for the laterally symmetric airplane.

We evaluated the effects of asymmetrical external store loadings (up to 10,000 ft-lb) on departure susceptibility, spin, and spin recovery—a first for fighter aircraft.

During this last six month period, NASA DFRC continued tests with the 3/8 scale model. The comparisons of model characteristics with those of the real airplane were remarkable. The last flight of the model was





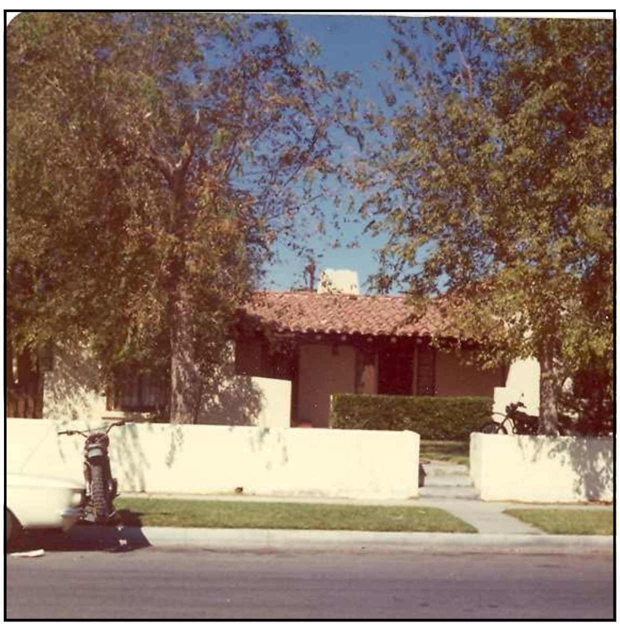
Flight No. 16 in mid-January 1975. The model (actually the second physical model after the first was destroyed in a landing mishap) was severely damaged on ground impact during an attempted desert landing without the parachute and didn't fly again until after the real airplane testing was completed.

The flight test results enabled us to define some minor, but significant, control system modifications and warning cues which were eventually incorporated into production. These included speedbrake retraction at 15.5 degrees AOA, full authority lateral control for spin recovery above a 60 deg/sec yaw rate, a 30 deg/sec yaw rate warning tone. In addition, we recommended incorporation of a fuel asymmetry warning cue for the pilot. The missile launch sequence was modified as a consequence of the asymmetric loading effects we encountered.

The F-15 Stall / Departure / Spin program was arguably the most successful in history. We didn't lose an airplane. For the first time for any spin program, we were able to systematically determine the effects of large amounts of lateral asymmetry, and we demonstrated the value of sub-scale model tests in full-scale airplane development.

Category II testing ended with the last high AOA flight of F-15#8 on 30 Jul 75. The successful conclusion of the test marked the first time in the history of jet fighter aircraft that all development flight tests were accomplished without losing a test airplane. That record still stands today.

With the end of Category I and II development tests, my work at the Flight Test Center was complete. It was time to return to St. Louis operations.   After a final farewell party, we packed up most of our belongings (everything except son, Dave, and his possessions which included his motorcycle and the ‘64 Corvair once owned by my dad. Dave would stay to finish his senior year in the Lancaster high school. On Dave’s 17th birthday, 18 Aug 75, KK, Karen, and I (trailing Karen’s and my motorcycles behind us) departed Lancaster and began the journey east to begin a different job—once again to be assigned to the Advanced F-15 project.



There will be more F-15 adventures in a later chapter. But first, it’s time for a break.

Jack Abercrombie
9 December 2007


Mojave
California City
U.S. BORAX MINE
Soledad
BACKUS RD
WILLOW SPRINGS RACETRACK
BURTON'S TROPICO GOLD MINE
Edwards
Rosamond Hills
ROGERS DRY LAKE
EDWARDS AIR
ROSAMOND BLVD
Rosamond
ROSAMOND DRY LAKE
BUCKHORN DRY LAKE
KERN
LOS ANGELES
WILDFLOWER AREA
Little Buttes
ANTELOPE
Antelope Butte
GEN. WILLIAM J. FOX AIRFIELD
Lancaster
ANTELOPE VALLEY FAIR GROUNDS
LANCASTER WILDFLOWER CENTER
OLD WEST HOTEL HIST. LANDMARK
WEST SIDE GOLF AND RECREATION PARK
GIANT JOSHUA TREE
Quartz Hill
QUARTZ HILL AIRPORT
LAKE ELIZABETH GOLF AND COUNTRY CLUB
PORTAL RIDGE
EARTHQUAKE FAULT
ANTELOPE VALLEY COUNTRY CLUB
AIR FORCE PLANT 42
DESERT AIRE GOLF CLUB
Palmdale
Bouquet Res.
Palmdale Lake

WHEN SPEAKING TO A GROUP, FIRST GET THEIR ATTENTION

My reassignment to St. Louis in August 1975 put me back into the Advanced F-15 Project working on some of the same concepts I had been involved with three and a half years earlier.

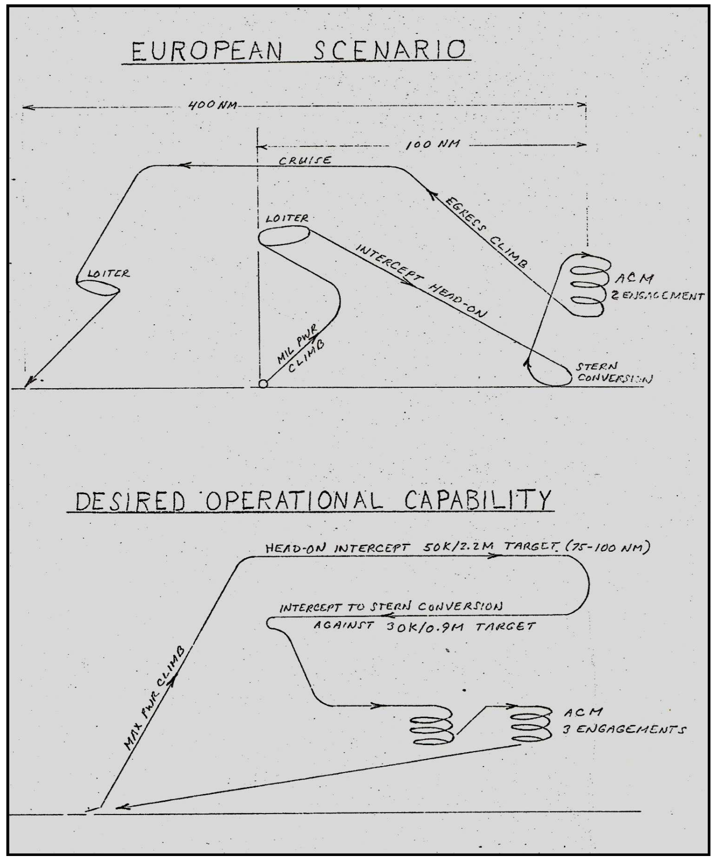
I previously mentioned (Page 9) the "strap-on" low drag, armpit Conformal Fuel Tanks (CFTs) or "pallet" tanks as a means of extending the aircraft range without incurring the high drag penalty of external wing and centerline drop tanks. During my absence from Advance Design, a pair of prototype pallet tanks had been built and flight tested (to a limited extent) in St. Louis. This effort had been completely company-funded in hopes that sales would result. To promote the pallet concept, an acronym, FASTPACK for Fuel And Sensor Tactical Package, was invented by someone in Marketing. (Ykk!). That acronym never caught on.

The CFTs increased the fuel carriage considerably. The basic airplane had an internal capacity of 11,635 pounds (1790 gallons) at that time. An additional 9,800 pounds was provided by the CFTs. (Of course, the external drop tanks could still be utilized along with the CFTs.). When CFTs were installed, the four fuselage "corner" stations for the Sparrow missiles were disabled, but they were replaced with stations on the CFTs themselves.

Of course, as a result of all the extra tankage and fuel, the airplane was considerably heavier than the basic airplane.

The prototype CFTs were mounted on the second two-place airplane to be built, TF-15#2 (by this time designated F-15B#2). The airplane had been flown to various Air Force bases in the U.S. as well as to various foreign markets, but, at this point, our own Air Force appeared to be just lukewarm to the concept.

But coincident with the recent successful completion of the F-15 Category II flight tests at Edwards, the Tactical Air Command (TAC) agreed to evaluate the palleted airplane. They outlined a flight test program to be flown by USAF pilots George Muellner (a TAC pilot I had known at Edwards) and Dave Peterson (one of the Air Force Spin Test pilots who I knew quite well).



The program outlined for the TAC evaluation included obtaining performance data (fuel usage during climb, acceleration, combat, loiter, etc.) for use in two potential operational missions: a “European Scenario” and a “Desired Operation Capability.” These two missions had been postulated by some agency’s conducting of various types of war games. (Remember, this was occurring at the peak of the Cold War.).

In addition, and this is the exciting part, plans called for conducting Air Combat Maneuvering (ACM) exercises over a classified Nevada test site against highly trained combat pilots flying the best aircraft the potential adversary had in its arsenal. The adversary aircraft were known only by their unclassified designations of “Type I” and “Type II”—in actuality, they were Soviet airplanes which had “strayed away from home”.

This was serious stuff! The ACM characteristics could make or break the fuel pallet concept. We had to make certain there were no glitches at the high angles of attack to be experienced in combat maneuvers.

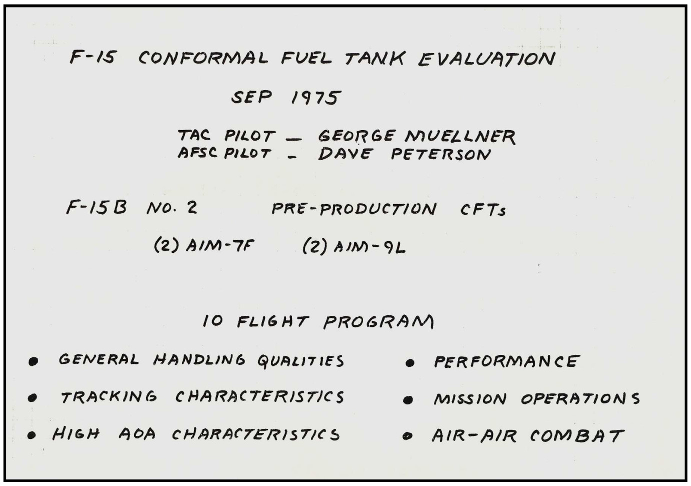
The test program would be partially supported by the *Advanced F-15* group rather than the *F-15 Project.* I don’t know the reasoning for this, but I knew that I was back in the action because of my recent familiarity with flight testing at Edwards (the test was to be conducted out of Edwards but over the Nevada test range for obvious reasons) and because I knew all the people involved. (And just maybe, I was the best qualified.).

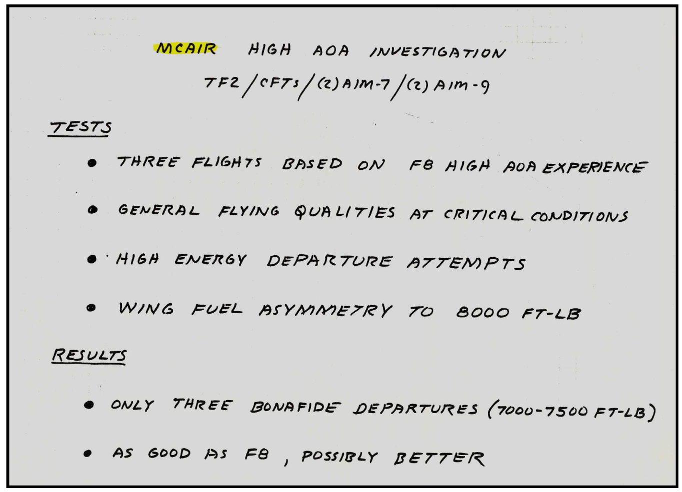
A quick review of the wind tunnel test data that had been obtained convinced me that there were no major aerodynamic concerns particularly at the high angles of attack likely to be seen during ACM. The only concern was the effect of the pallets on causing a nose-up pitching moment at all angles of attack. In an ideal world, we could have adjusted the control system to compensate for that effect, but we didn’t have the time. Instead, we ballasted the airplane to a more-forward center-of-gravity position—that would make it harder to over-rotate to attitudes not thoroughly investigated.

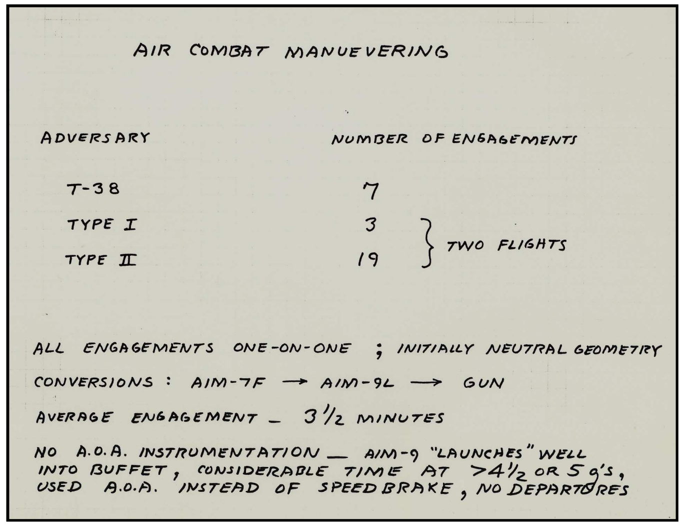
The control system configuration for TF-2 had not kept up with the changes defined during the development tests at Edwards (St. Louis had always seemed to be somewhat in a state of denial). The airplane had been too busy performing marketing flights around the world. One missing modification in particular bothered me—the absence of the Quick-Turn-On ARI (see Pages 58-59). The Assistant Program Manager forbade us from making the ARI change, presumably because of cost considerations. Well, the Flight Test Engineer, Jim Hakanson, trusted my judgment—the modification was incorporated in spite of the admonition to not do so. I then asked Irv Burrows to support our action—the change was subsequently formally approved.

On 18 September 1975, I found myself back in the desert—one month after having left son, Dave, there on his 17th birthday. I took my motorcycle riding gear for use on a borrowed bike so that I could ride at least a little with him and with McDuffee (who had not yet been reassigned to St. Louis). Since I was rusty from not having ridden for five weeks and was a little reluctant to use a borrowed bike, I hoped that when I fell that it would be in a soft spot.

During the next 10 days of the test program, we made 10 long-duration flights (using in-flight-refueling for some of the longer flights) evaluating tracking characteristics, high angle of attack, the two operation scenarios, general performance and flying qualities, and two pilot-exhausting flights north of Nellis AFB, Nevada flying ACM against the adversary aircraft.







On the afternoon of 30 September, my St. Louis supervisor, Frank Laacke, called to inform me that I was needed the next morning back in St. Louis to brief management on how the TAC evaluation went. I quickly departed Edwards (without having a chance to ride a bike) and caught the "Red Eye" flight from Los Angeles to home.

During the flight to St. Louis, I composed my briefing charts for the next morning's meeting with tablet paper and pencil. Now this was in the days before lap-top computers and Power Point software, so my hand-drawn charts were to be copied onto transparencies for use in an "overhead projector" view-graph machine (providing I could find clerical help early the next morning to use the copier machine—all that modern technology was closely guarded; one had to have the proper credentials to operate such equipment).

I may have gotten an hour of sleep at home before arising early to get to the 0800 briefing. When I entered the briefing room, I was overwhelmed at the size of the crowd—most of whom I didn't know (having been away from the St. Louis scene the previous three years). At that time, the "pecking order" at MAC was designated by the color of one's security badge—gold for the president and the two top layers of vice-presidents, silver for the next few layers of management, and so on down the line. I wore a silver badge, but the silvers were vastly outnumbered by the golds—a more important meeting than I had counted on.

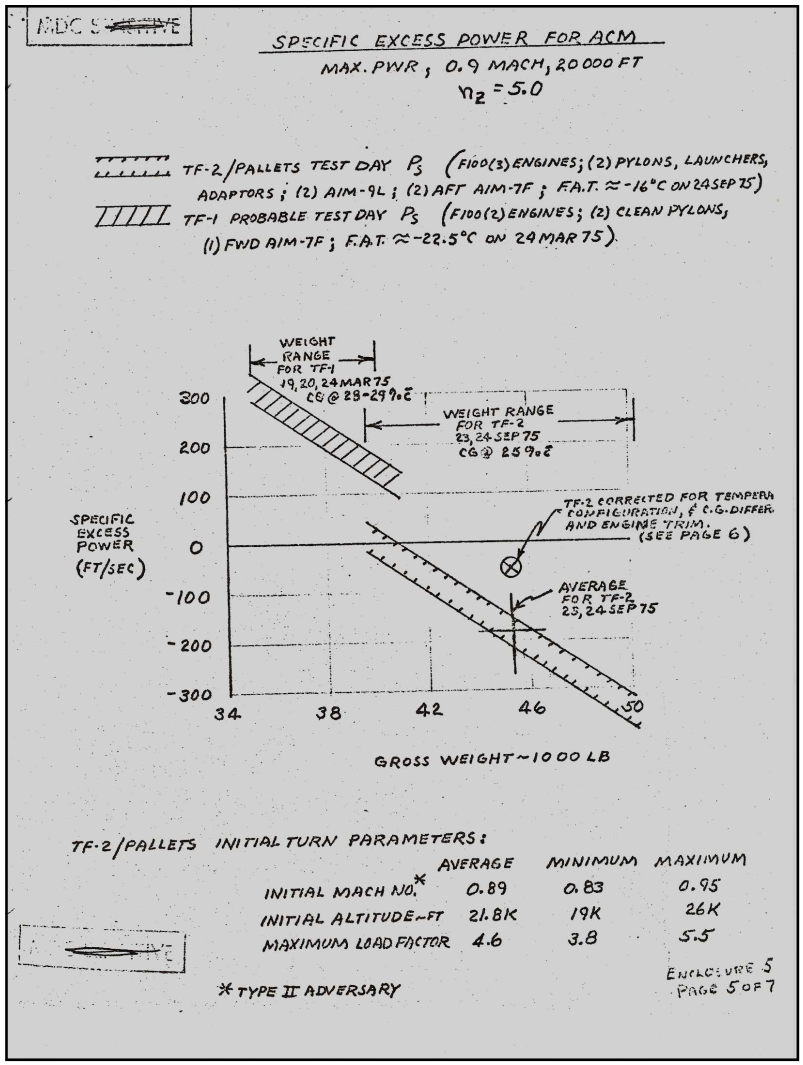
The VP of primary interest for this briefing was Chet Braun, the F-15 Program Manager who had taken Don Malvern's place when Don became company President a little more than two years before. Braun had been a Test Pilot in earlier days (much earlier). Whatever credentials Braun may have had, a degree from Charm School was not one of them. He was certainly no *leader*, and he had a hair-trigger temper. I had been an embarrassed witness to his needless chewing out of one of our Flight Test Engineers at Edwards, my friend, John Roberts.

But my only previous contact with Braun had been in a few meetings at Edwards during the previous 18 months or so, and I really didn't know what to expect of him. I stood before the crowd, the audience silenced, the room lighting dimmed, and I led off with my first view-graph chart (the one appearing at the top LH corner of this page or at least one very similar) projected on the screen behind me. A fairly sterile chart, but the words I verbalized were a little more catchy. The first sentence I spoke was something like, "Overall, the evaluation was successful, but there were a few areas where we could have done better." With that, Braun's fist came down to the table top like a crash of thunder, breaking the light bulb in the projector and casting the room in complete darkness!

While one of the administrators searched other briefing rooms for a replacement light bulb, Braun delivered about a ten-minute tirade lambasting those who had apparently been making adverse noises about his program. He wasn't necessarily talking to only me. He admonished the crowd to *never* say "bad things" about the CFT program.

After he grew silent and the projector bulb was replaced, I said something a little stupid like, "That was precisely the point I was trying to make" and continued on with my presentation. Needless to say, I had everyone's complete attention for the next 30 minutes or so. All eyes and ears were focused on my every word.

Lessons learned: (1) There are several ways to get the attention of the audience. (2) Always check to see if the projector has a spare bulb.



In truth, the CFT configured airplane did quite well with the exception of Air Combat Maneuvering against the adversary aircraft. The tactics employed by our pilots were based on previous experience with F-15s of much lighter weight (no CFTs nor CFT fuel), cooler ambient temperature conditions (more engine thrust), and engines which had not been "down trimmed" (for the purpose of reducing the risk of engine stall at high angles of attack).

The point was driven home that the CFT configured airplane needed more thrust.

After that briefing, I kept rather a low profile until a few weeks later when George Muellner (the TAC pilot) came to St. Louis to give us the official Air Force briefing. George said everything I had said and then some. At least it was a good feeling to feel exonerated. (As a matter of interest, Muellner retired much later from the Air Force as a Lieutenant General and subsequently became a vice-president for McDonnell/Boeing.)

***

During the next two and a half years, I continued my Advance F-15 assignment becoming Program Manager for out support of NASA Dryden Flight Research Center efforts with F-15 experimentations—NASA received F-15#2 and #8 from the Air Force. And our marketing efforts continued to try selling the F-15 to foreign countries including Japan (they bought it), Saudi Arabia (they bought it), Israel (they bought it—during this effort, I met future General Moshe Melnik, the first pilot to score an F-15 victory in actual air-to-air combat), and France (nice champagne party), the UK, Australia, Canada, Germany, and Spain ("no sales" for the last six entries). We performed studies in the use of rectangular thrust vectoring nozzles, extreme high altitude flight modifications, and in October of 1977, I presented a paper at a NATO AGARD meeting in Paris (more in a later chapter).

But the story of the F-15 with CFTs isn't over yet, so the next chapter will jump ahead a few years and cover that rather unpleasant aspect of my F-15 chronicles.

A HORRIBLE SCREECHING SOUND

About mid-year 1978, the Advanced F-15 activity was, frankly, becoming boring to me. At the time, I had Aerodynamics over-sight of not only the Advanced F-15 design effort, but also the Advanced F-18 (the airplane was still several months from first flight), the F-4 Phantom project, and an Aerodynamics Research group. But there were not enough challenges. I knew the F-18 was in serious trouble because of its hap-hazardly contrived control system (see Pages 43-44) and would probably be a disaster when it finally got off the ground (scheduled for later that year). Consequently, I informally requested a change of assignment to the F-18 Project.

Beginning in July 1978, I worked full-time on the F-18. I headed the Aerodynamics effort (performance, store separation, and flying qualities) as well as leading the flight controls development and integration.

In mid-1982, I became Acting Chief of Aerodynamics (the Chief, John Mello <see Page 8> was on extended assignment to a project to demonstrate the use of canards and rectangular thrust vectoring nozzles on an F-15). In addition, I continued with my assignment as F-18 Flight Controls Integrator.

Meanwhile, the F-15 CFT program had continued with Air Force flight testing at Edwards (more extensive than the brief TAC evaluation of 1975). The Edwards testing utilized F-15C#1 (essentially an F-15A but with additional *internal* fuel added to anyplace it could be squeezed into; e.g., in the wing leading edge structure—about 1865 pounds worth).

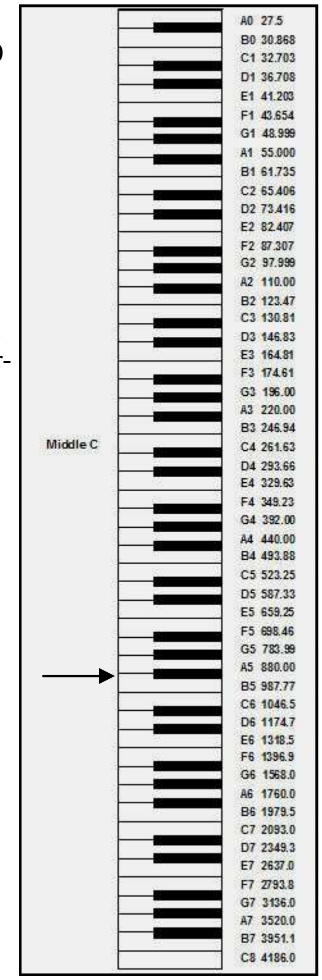
Although I had been up to my neck with the F-18 for the previous four years (and still was), in October 1982, my attention was once again turned to the CFT configured F-15. On the 20th of that month, F-15C#1 with CFTs experienced a departure for no apparent reason at about 40 degrees angle of attack. A second and a third departure followed over the next few flights. The USAF began to agitate for a full-blown spin test program (spin 'chute, emergency power, etc.) for the CFT airplane in November 1982 (what a difference from the prevailing attitude in early 1974!).

I was called on to present the results of the 1975 CFT evaluation to the F-15 SPO in December 1982. Clearly, the Customer was becoming nervous about the CFTs and what were presumed (by some in the Customer community) to be the effects of the pallets on the high angle of attack characteristics. So I dusted off my charts from September 1975 (three of which were shown on Page 87) and pitched to the SPO later that month.

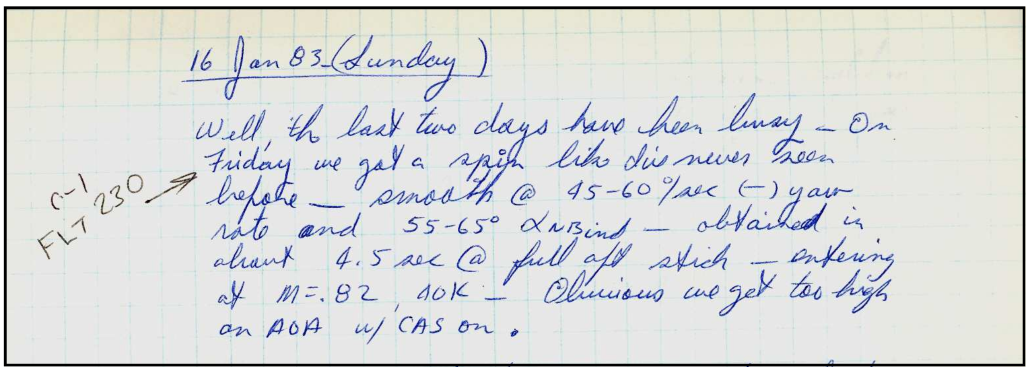
In early January 1983, I was once more in the desert, or as my daily log recorded, "Back in God's Country with FASTPACK (CFT) of all things!" (I never was impressed with the CFTs for a fighter aircraft because of the impact on gross weight.).

Lt. Col. John Hoffman was in charge of the CFT evaluation program. Tom McDuffee (again on "permanent" assignment to Edwards) represented MCAIR as the Flight Test Engineer, and Pat Wider was brought out from St. Louis as the then current F-15 high angle of attack expert. We worked on test plans and had the inevitable Safety Reviews with the Flight Test Center and Base management. Col. Hoffman requested that I stay for the "first couple flights" even though I was by now behind in my St. Louis chores. The planned program included no deliberate spin attempts since the airplane wasn't equipped with the usual spin test emergency gear. Instead, there would be wind-up-turns (wind-up-turns are defined on Page 58) to full aft stick with lateral stick inputs. No tests would be performed if left/right wing fuel asymmetry was more than 200 pounds (the fuel asymmetry warning system which had been recommended seven years earlier had yet to be accepted). It seemed that the "first couple flights" would be kind of "ho-hum" from my standpoint.

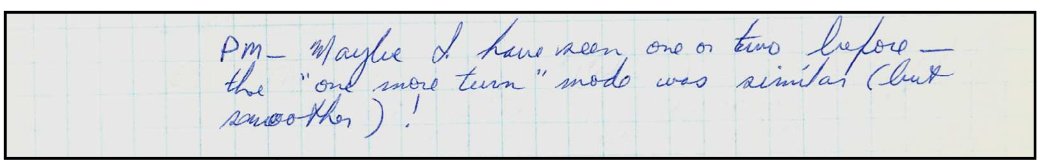
On Friday, 14 January 1983—another *turn* for the worse! While I sat calmly in the telemetry room, Hoffman entered a wind-up-turn at Mach 0.82, 40,000 feet. After about 4 1/2 seconds at full aft stick without any lateral stick input, the yaw rate warning tone (triggered at 30 deg/sec yaw rate) began to warble at an ever increasing rate. What a horrible screeching sound! It was a high pitched scream easy to hear (900 cycles per second—the frequency at which the average human middle ear tends to resonate—nearly two octaves above Middle C) which soon became a steady tone when the yaw rate reached 60 deg/sec. Of course, I had heard the same tone countless times during the Category I and II spin program, but I certainly didn't want to hear it at this time—we were in "Spin City!"



Recovery from the spin was quite satisfactory, but I was devastated! So much for my assurances that a spin was unlikely. All that night, the next day, and well into Sunday, I puzzled over the data trying to make sense of it all. The spin seemed to exhibit very unfamiliar characteristics, and there appeared to be no reason for the airplane to have departed and enter a spin! My Sunday morning log book entry noted that, "...we got a spin like I've never seen before."


16 Jan 83 (Sunday)
Well, the last two days have been busy — On Friday we got a spin like I've never seen before — smooth @ 45-60°/sec (−) yaw rate and 55-65° α NBind — obtained in about 4.5 sec @ full aft stick — entering at M=.82, 40K — Obvious we get too high on AOA w/ CAS on.
C-1 FLT 230

Suddenly, it hit me—my God, this was the notorious "One-More-Turn" mode of 1974 with F#8—the one where the spin 'chute was used for recovery (Pages 59-60)!

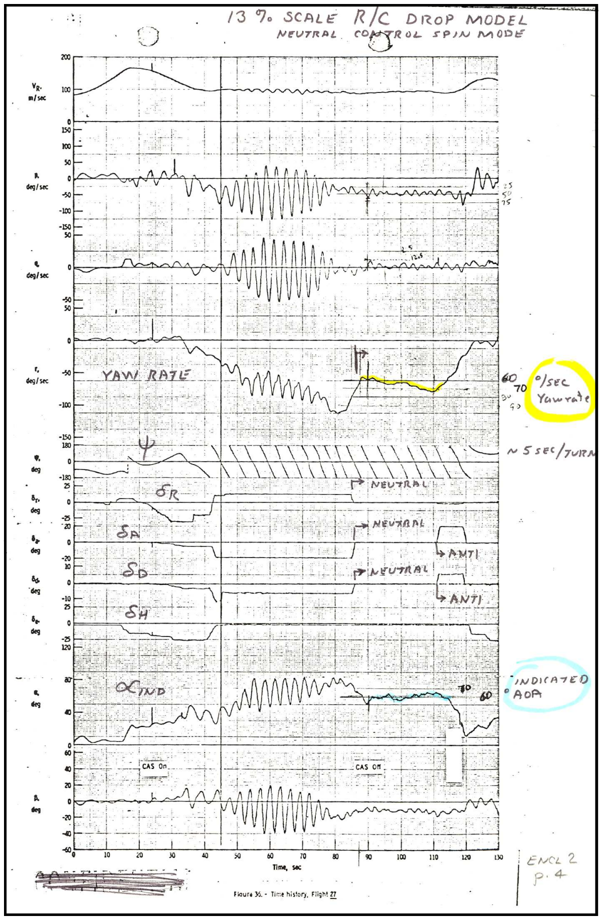

PM — Maybe I have seen one or two before — the "one more turn" mode was similar (but smoother)!

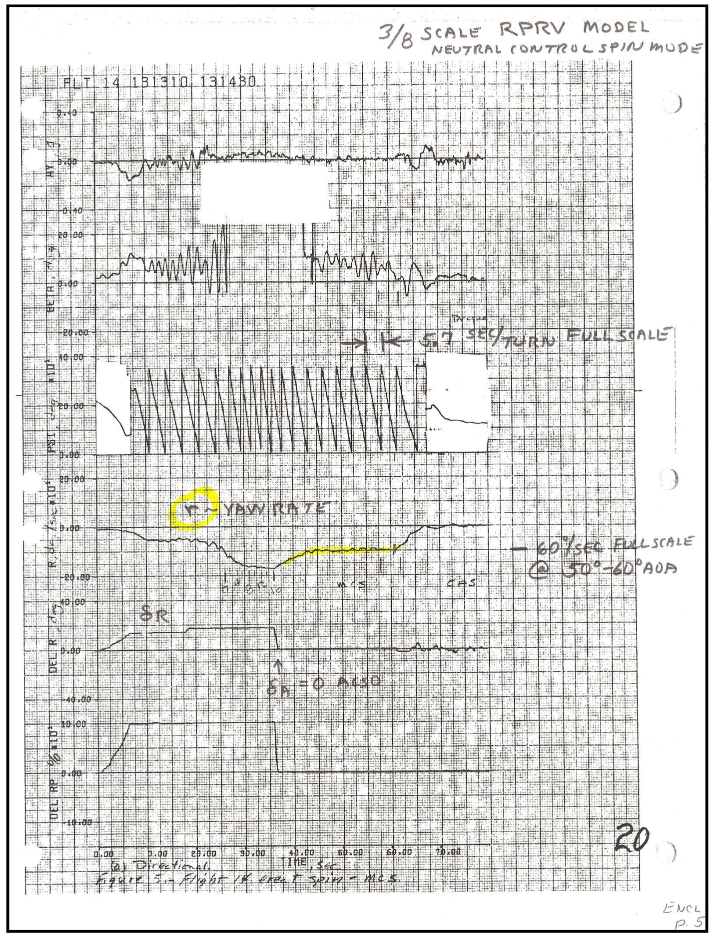
A quick telephone call to my secretary in St. Louis early Monday morning to dig out some of my ancient history notes for comparison to the One-More-Turn mode convinced me—it was, indeed, the same mode.

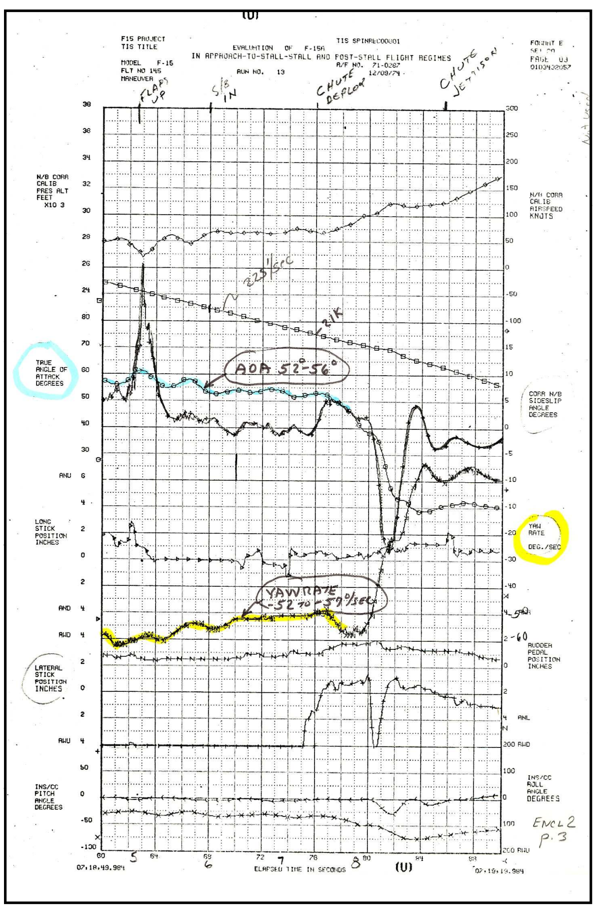
I immediately headed back to St. Louis to regroup. Dick Thomas (the original F-15 spin expert) Pat Wider, and I spent several days reviewing old data trying to determine why the airplane had departed and spun so easily with no aggravated control inputs and no significant fuel asymmetry. We also reviewed the limited data available from the subscale model tests and really tore into the One-More-Turn mode with F#8. We (and that includes the NASA LaRC experts who had performed spin tunnel tests of the CFT configuration in early 1982) were all convinced that the CFT's effect on lateral-directional aerodynamics did not contribute to the problem. But we needed to prove it and determine the reason for the strange behavior and correct it—or the CFT concept was dead!

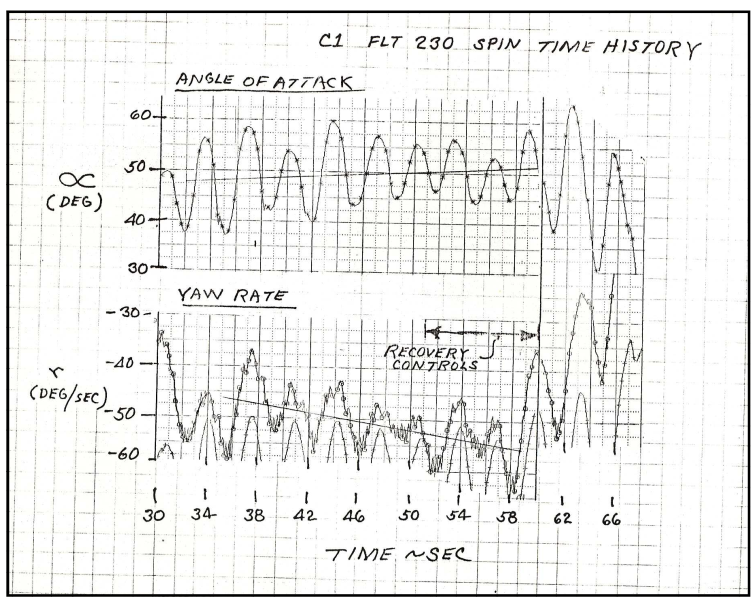
In our review of the subscale model data, we found brief periods in some of the spins where the modal characteristics were virtually identical to those of this low yaw rate, steady spin mode—this was an important discovery!

This discovery would help to convince people that the spin we were dealing with was nothing new, just merely extremely rare, and not so mysterious after all.





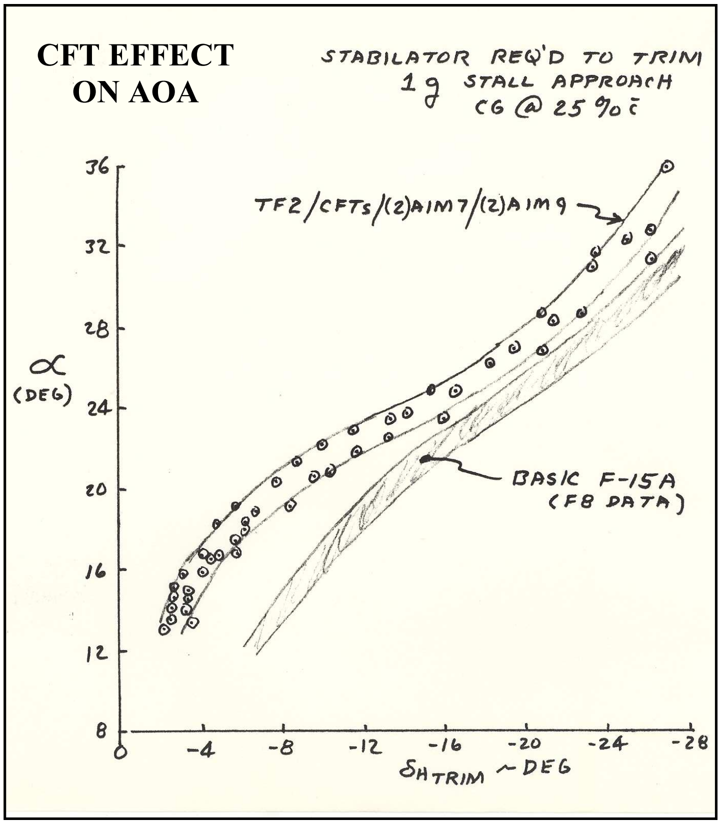




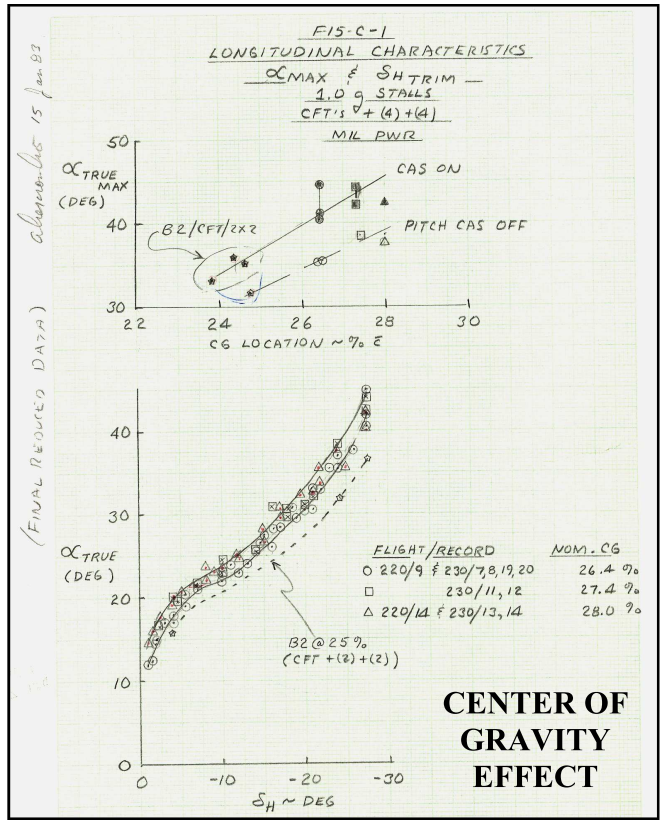
MODEL AND AIRPLANE COMPARISONS

Satisfied that the spin, itself, was not unique, we then turned our attention to the departure. A review of all the data available convinced us that the departure problem for F-15C#1 with CFTs was caused by two separate but related phenomena:

- A higher steady-state angle of attack available with the CFT configured airplane because of *longitudinal* aerodynamic impact not compensated for by adding weight-forward ballast or a simple control change (see Page 86).
- Asymmetric vortex shedding from the airplane nose inducing a very large yawing moment at zero sideslip beginning at about 40 degrees AOA. We suspected this was aggravated by the long flight test nose boom.



CFT EFFECT ON AOA



CENTER OF GRAVITY EFFECT

C1 FLT 230 SPIN

NEUTRAL CONTROLS $r \approx -45$ TO -60 °/SEC AOA ≈ 45° TO 55°

SIMILAR SPIN MODE EXPERIENCED WITH BASIC CONFIGURATION ~ 1/12 SCALE ROTARY BALANCE MODEL, 13 % SCALE R/C DROP MODEL, 3/8 SCALE RPRV, A8 FLT 145, & POSSIBLY A8 FLTS 124, 191, 201. STABLE MODE WITH CONTROLS NEUTRAL, 1 TO 2 TURN RECOVERY WITH ANTI-SPIN CONTROLS.

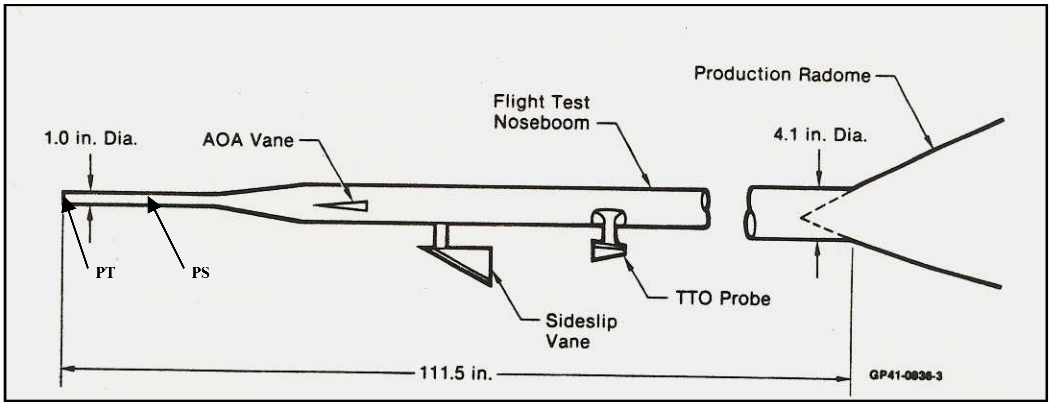
— MEDIUM RATE SPIN MODE —

CONFIG	MODEL	CONTROLS	A.O.A. (DEG)	YAW RATE (DEG/SEC)	SEC/TURN
BASIC	13% SCALE DROP MODEL	NEUTRAL	56-60	62-65	5
	3/8 SCALE DROP MODEL	NEUTRAL	54-60	60-65	5.7
	1/12 SCALE ROTARY BALANCE	PARTIAL ANTI-SPIN	58	60 "WEAK" RECOVERY	5.4
	1/12 SCALE ROTARY BALANCE	ANTI-SPIN	56	50 "WEAK" RECOVERY	6.1
	FULL SCALE (A8/FLT 145)	ANTI-SPIN FOR ABOUT 2 TURNS	51-54	53-55 "WEAK" RECOVERY	5.9
CFT	1/12 SCALE ROTARY BALANCE	PARTIAL ANTI-SPIN	58	53 "WEAK" RECOVERY	5.9
	1/12 SCALE ROTARY BALANCE	ANTI-SPIN	56	45 "WEAK" RECOVERY	6.5
	FULL SCALE (C1/FLT 230)	NEUTRAL	48-56	45-60	5.8

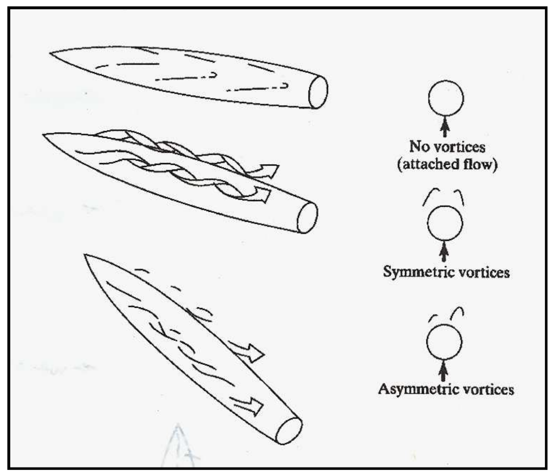
Unfortunately, to address the second bullet at the top of this page about vortex shedding, it's going to be necessary to get into some technical matters and cover some fundamental aerodynamic phenomena. The uninterested reader or one who will merely accept the statement that the bullet makes, may skip the next page.

TECHNICAL STUFF

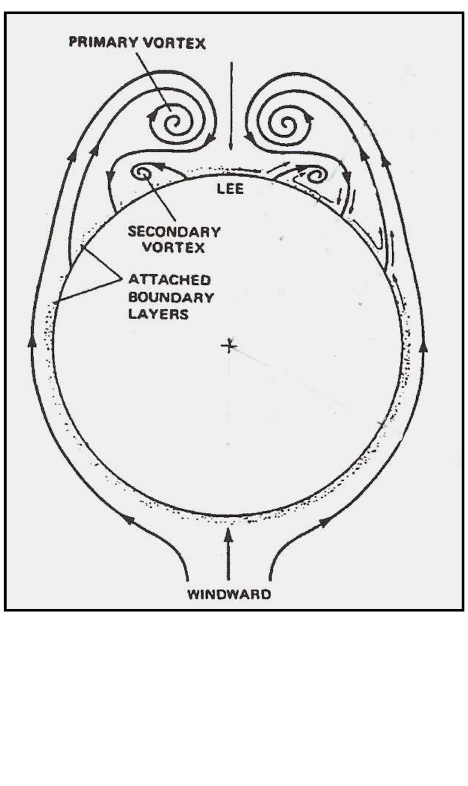
Most of the F-15 photos up to this point have shown a long, pointy thing sticking out in front. This is a

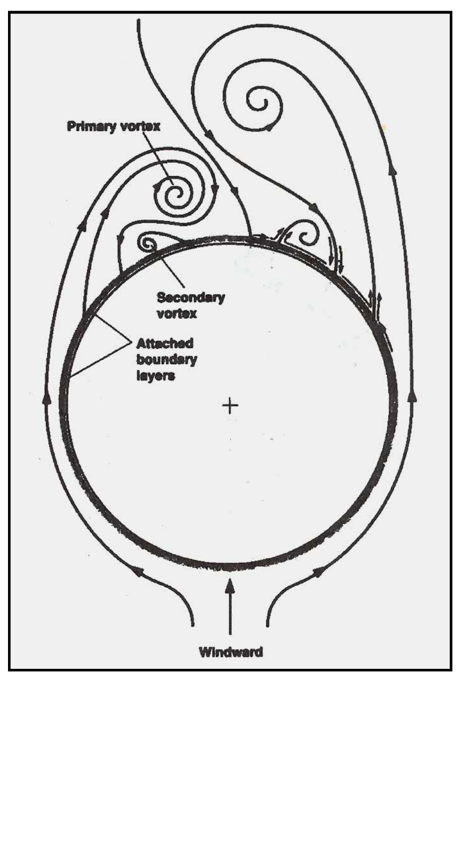


noseboom containing different types of instrumentation devices for measuring airspeed, altitude, free air temperature, and both angles of attack and sideslip. These devices are typically mounted on a long "flight test noseboom" to get them as far away from the aerodynamic influences of the airframe itself. Of course, there is always some influence so that the installations need to be calibrated using sophisticated techniques beyond the scope of this document.



At some angle of attack, usually about 40 to 45 degrees, a thin body of revolution (cone or cylinder-like object) will experience airflow separation similar to that discussed on Pages 25 and in the lower sketch of Page 30). Vortices form at the separation points on each side of the body. Initially, the vortices tend to hug the surface creating low pressure regions. But at some angle of attack, one will break away ("shed"). Then there will be a differential pressure tending to pull the body to one side.



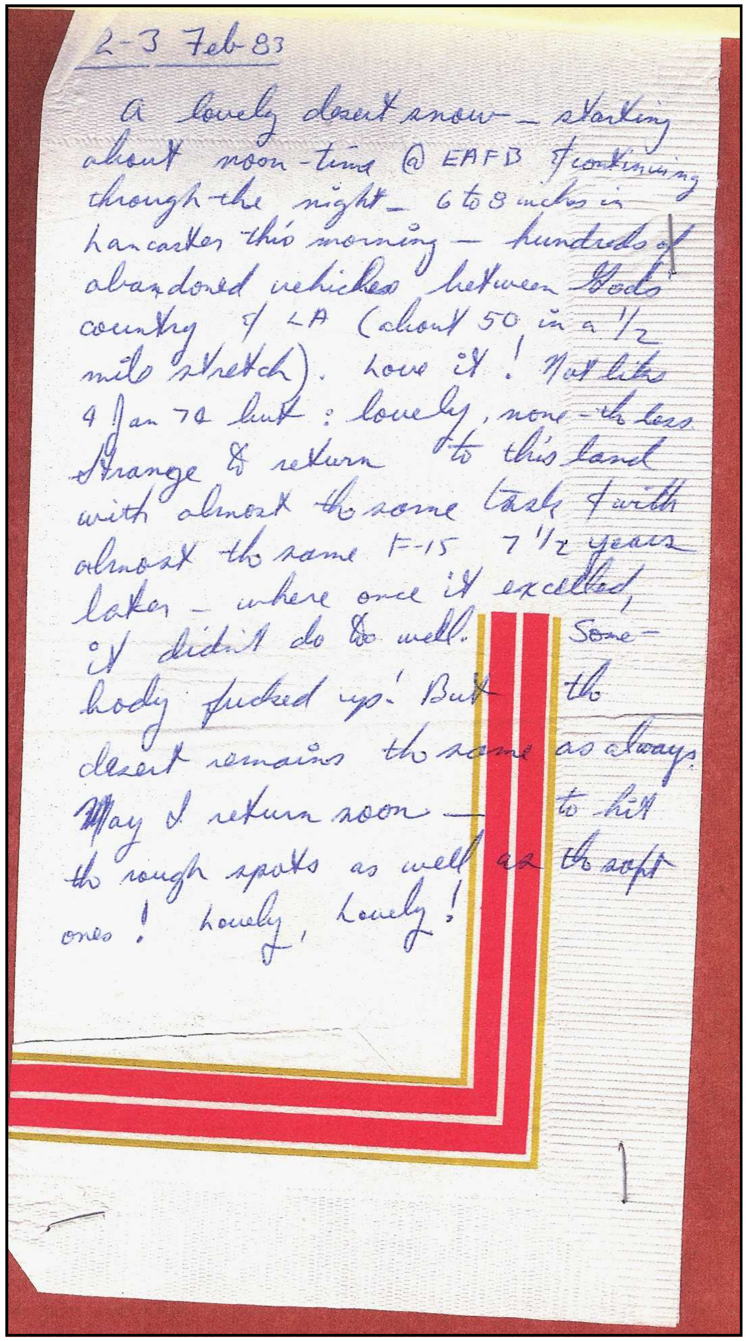


As suggested earlier, we suspected asymmetric vortex shedding as the culprit, but we had no convincing evidence.

Although my personal situation was becoming a bit complicated at this time (I was named "F-18 Flight Controls Integration Manager" to supplement my jobs as "Acting Chief of Aerodynamics" and "Branch Chief—F-18 Aerodynamics"), I managed to make another trip to Edwards for several meetings with the USAF during the last week of January 1983. At these meetings, we presented the results of our findings as have been shown on the previous pages. I also presented some proposed simple modifications to the control system which would help the excessive steady state angle of attack situation.

There was considerable skepticism about our conclusions, and there continued to be considerable agitation to conduct a full blown spin program for the CFT airplane. John Hoffman appeared to be the only Air Force member in attendance who accepted our conclusions.

I left Edwards on 3 February 1983 with a heavy heart. Obviously, not everyone in the Air Force was convinced. My own credibility had suffered a terrific blow. The only cheerfulness was a beautiful desert snow which had fallen throughout the previous night. My musings on a TWA cocktail napkin summarized my feelings.


2-3 Feb 83
A lovely desert snow — starting about noon-time @ EAFB & continuing through the night — 6 to 8 inches in Lancaster this morning — hundreds of abandoned vehicles between Gods country & LA (about 50 in a 1/2 mile stretch). Love it! Not like 4 Jan 74 but: lovely, none-the-less. Strange & return to this land with almost the same task & with almost the same F-15 7 1/2 years later — where once it excelled, it didn't do so well. Somebody fucked up! But the desert remains the same as always. May I return soon — to hit the rough spots as well as the soft ones! Lovely, Lovely!

Back in St. Louis, I occupied myself with my other three "full-time" jobs for the next four months. Finally, on 1 June 1983, the CFT high angle of attack flight test program resumed with the noseboom *removed* (using Inertia Navigation System algorithms and test techniques that we had developed in 1974-75 to substitute for the missing instrumentation measureands). Without the noseboom, the CFT airplane behaved beautifully (and my credibility level climbed a bit)!

The CFT test program continued for the next several months attempting to answer all the questions needed for acceptance by the Air Force. (One of the young MCAIR Test Engineers on the program, Mark Bass, is now <Feb 2008> the Boeing F-15 Vice-President—Program Manager.).

Soon after completion of these flight tests, the Air Force signed up to purchase the Conformal Fuel Tanks for the F-15C and D models.

(In August 1983, I was promoted, finally, to the position of "***Chief*** of Aerodynamics").

***

My next close encounters with the CFT configured F-15 occurred two and one half years later—next chapter.

Jack Abercrombie
25 February 2008

WINDING UP THE SPIN STORY

In December 1985, F-15D#50, with Conformal Fuel Tanks (CFTs) installed, was performing a pilot check-out flight for a Brigadier General (Designate) over the Gulf of Mexico (off Eglin Air Force Base in Florida). During what was described as an abrupt wind-up-turn at 32,000 feet altitude, the Instructor Pilot applied right rudder which apparently aggravated the effects of a left-wing-heavy fuel asymmetry reported to be 300 to 500 pounds (remember the 1975 recommendation for a fuel asymmetry warning discussed on Page 62). As a consequence, there was an immediate yaw departure to the right which was followed by a spin. Based on the pilot observations, the spin appeared to this writer to have all the characteristics of the "One-More-Turn" mode. After a loss of about 24,000 feet altitude during a pilot-reported 15-23 turns while he applied anti-spin control stick (stick in the direction of the spin as called for on Page 53) to no apparent effect, the crew punched out safely and were rescued from the waters of the Gulf.

At this time, I was the Technology Integrator on a Technology Demonstrator program involving a modified F-15 (the subject of yet another chapter). But because of my history with the F-15 spin saga, I was apprised of the MCAIR investigation and response.

Although reports were sketchy and, in some cases, suspect, I learned that during the previous seven years while I was occupied primarily with the F-18 "Hornet" and Aerodynamics Department jobs, there had been possibly three F-15 spin losses due to lateral fuel asymmetry and three other losses of undetermined causes but listed as possible spins. In addition, there had been three documented fuel-imbalance spins from which the pilots had successfully recovered. And, it should be noted, all these incidents had occurred on airplanes *without* Conformal Fuel Tanks.

The cause of the F-15D#50 spin was never determined, but the loss of the airplane was a serious matter. There remained some skepticism about the aerodynamic impact of the CFTs within the Tactical Air Command. To complicate matters, just a year before, McDonnell had won the competition for a "Dual Role Fighter" or "DRF" against the General Dynamics F-16 entry. Besides improvements to the armament capability (with added Air-to-Ground weaponry, primarily), the avionics, cockpits displays, etc., etc., the CFTs with their increased fuel capacity were considered part and parcel to the success of the DRF. The company was heavily engaged in the development work on the F-15 DRF, or as it would be designated, the F-15E "Strike Eagle."

PRIORITY

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Phoned Barb Parker 945AM – 1st Okay to route Copy to — John Suchan

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TO RUCIPBA/HQ TAC LANGLEY AFB VA//DOO/DOV//
RUEDAWA/HQ AFSC ANDREWS AFB MD//TEO//
RHFQAAA/HQ USAFE RAMSTEIN AB GE//DOO/DOV//
RUHVAAA/HQ PACAF HICKAM AFB HI//DOO/DOV//
RUVAAHA/ASD WRIGHT PATTERSON AFB OH//TAF//
RHCQBMA/AFSC AD EGLIN AFB FL//DO//
RUWMBKA/HQ AAC ELMENDORF AFB AK//DOO/DOV//
RUEAUSA/NGB WASHINGTON DC//XOO//
RUWJBDA/AFFTC EDWARDS AFB CA//DOT//
RUWMPFA/HQ AFISC NORTON AFB CA//SER//
INFO RUEAHQA/HQ USAF WASHINGTON DC//XOOT//
RUCIBBA/9AF SHAW AFB SC//DOO//
RUWTFGA/12AF BERGSTROM AFB TX//DOO//
RUFTRJA/17AF SEMBACH AB GE//DO/
RUADJNA/5AF YOKOTA AB JA//DO//
RUCIFAA/NAVPRO ST LOUIS MO//QA//
UCIPBA/1AF LANGLEY AFB VA//DOO//
UWTMFA/6585TESTG HOLLOMAN AFB NM//CC//
PAGE 2 RUVPAAA3410 UNCLAS
RUVAFLC/AFLC LOC WRIGHT PATTERSON AFB OH//TL//
BT
UNCLAS
SUBJECT: F-15 FLIGHT TEST RESULTS REVIEW
1. THE F-15 SYSTEM PROGRAM MANAGER IS CHAIRING A CONFERENCE TO DISCUSS FLIGHT TEST RESULTS FOR F-15 AIRCRAFT MODELS A/B/C/D IN ALL CONFIGURATIONS IN THE AREA OF STALLS, SPINS AND OUT OF CONTROL RECOVERY RECOMMENDATIONS. REVIEW OF FLIGHT TEST RESULTS AND DISPOSITION OF RECOMMENDATIONS WILL BE ACCOMPLISHED. REQUEST YOUR ORGANIZATION PROVIDE SUPPORT WITH INDIVIDUALS MOST KNOWLEDGEABLE IN THE SUBJECT AREA.
2. CONFERENCE WILL BE HELD AT MCDONNELL AIRCRAFT COMPANY, ST LOUIS MO ON 14-15 JAN 1986 BEGINNING AT 0900 HOURS. REQUEST FOR PLANT CLEARANCE FOR INDIVIDUALS PLANNING TO ATTEND MUST BE SUBMITTED TO THE NAVPRO AT MCDONNELL. POINT OF CONTACT FOR ADDITIONAL INFORMATION IS MR WALKER FRICKS, MMFRB, AV468-5465.
ET
#3410
NNNN

PRIORITY

The concern within the Air Force was so great that a large conference was called. The conference would be held at McDonnell in January 1986 to review *all* F-15 stall, spin, and out-of-control flight test results as well as the status of recommendations which had been made. It was requested that the support of individuals most knowledgeable in these areas be provided.

UNCLAS
SUBJECT: F-15 FLIGHT TEST RESULTS REVIEW
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So once more, I was in the F-15 spin business—mostly as a historian. The conference was convened on the morning of 14 January with many old friends and F-15 experts in attendance including Col. John Hoffman, the test pilot of the previous chapter who had encountered the unplanned (and embarrassing to me) spin exactly three years before to the day; Skip Hickey of the F-15 SPO; and (I believe) Jim Bowman of NASA LaRC—indeed, all F-15 spin experts.

I spoke to the group for more than an hour presenting much of the same material the reader has endured through the previous chapters (although this time, we had enough clerical help to make the charts appear a bit more professional).

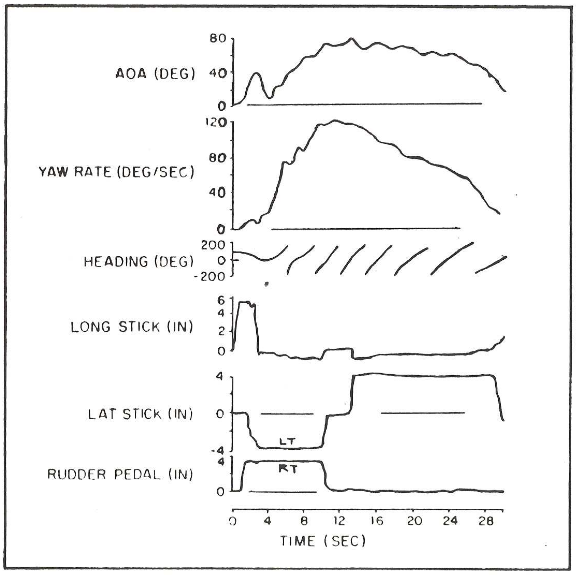
Abercrombie

AGENDA
F-15 DEPARTURE/SPIN CHARACTERISTICS REVIEW
14-15 JAN. 1986

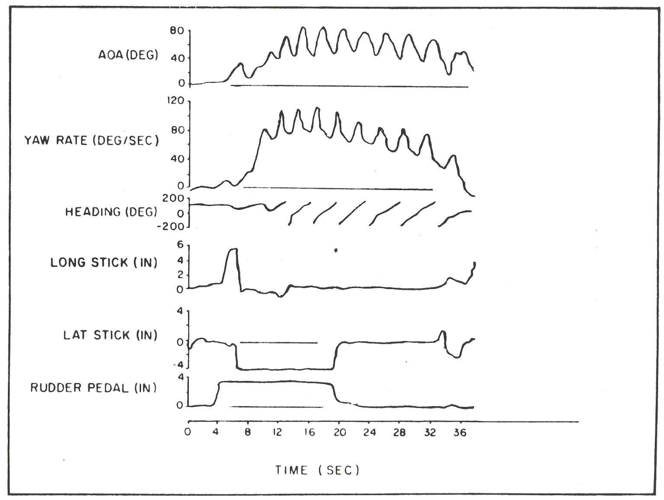
U.S.A.F. OPENING REMARKS	COL. TOM GOLDMAN
MCAIR WELCOME	DAVE THOMPSON
DEPARTURE/SPIN INVESTIGATION	JACK ABERCROMBIE
CONTROL SYSTEM/DISPLAYS	BILL CRAWFORD
SPIN INCIDENCES	MARK RINEHART
FUEL SYSTEM	WALT BECK JIM POUSSON
-1 FLIGHT MANUAL	GARY JENNINGS
RECOMMENDATIONS	BILL WEBER
SPIN RECOVERY AID F-18	ELDON ANDERSON
DISCUSSION	

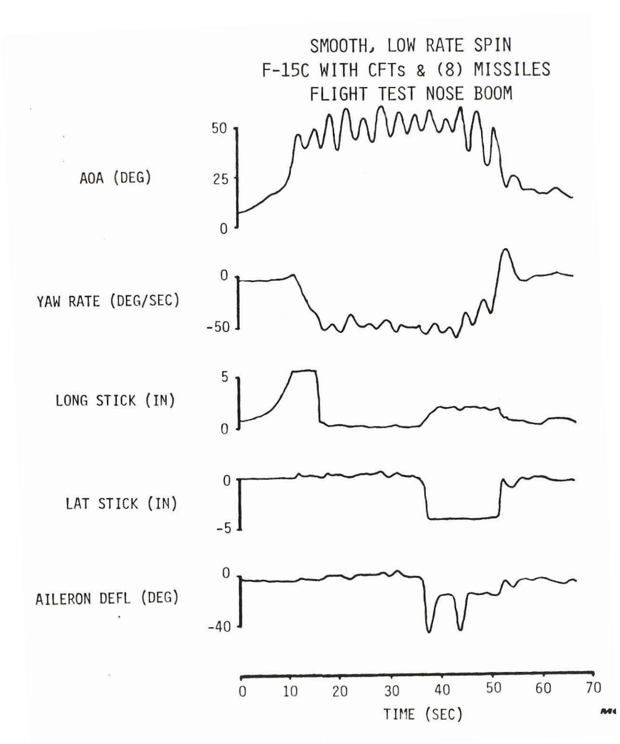
MCDONNELL AIRCRAFT COMPANY

SMOOTH SPIN



HIGHLY OSCILLATORY SPIN





In addition, I took the liberty of renaming the "One-More-Turn" mode to "Smooth, Low Rate Spin."

The concluding chart emphasized that the Conformal Fuel Tanks did not cause high angle of attack problems.

F-15C CCP 212/278
HIGH AOA FLIGHT TEST CONCLUSIONS

- o HIGH AOA CHARACTERISTICS WITH CONFORMAL FUEL TANKS ARE COMPARABLE TO OR BETTER THAN THOSE OF BASIC F-15
- o A FLIGHT TEST NOSEBOOM MAY ADVERSELY EFFECT DEPARTURE SUSCEPTIBILITY DUE TO ASYMMETRIC VORTEX SHEDDING & RESULTANT YAWING MOMENTS

The conference continued throughout that day and the next with McDonnell presentations about fuel system improvements being made for reducing lateral imbalance (finally). Further plans for incorporating a lateral fuel asymmetry warning system were discussed at some length (The heat got turned up on this issue—at long last!). In addition, we obtained consensus that minor refinements to the lateral control system to further improve spin recovery characteristics could be incorporated.

And last, we described the F-18 "Hornet" control schemes and cockpit displays appropriate for spin recovery which we had developed during the previous five or six years. We suggested that these would be appropriate for the F-15E.

***

Eleven months later, on 11 December 1986, the F-15E "Strike Eagle" made its first flight. Delivery of the production F-15E to the USAF began on 12 April 1988.

My part in the F-15 spin story was ended.

Jack Abercrombie
3 May 2008

PERFORMANCE IN PARIS

Chronologically, this chapter belongs between Pages 88 and 89, but the subject matter just doesn't fit there. But the material represents an important episode in my F-15 story, so a few pages here are appropriate.

In early 1977, Harold Altis, the VP in charge of Engineering Technology, requested that I prepare and deliver a presentation to a NATO AGARD (North Atlantic Treaty Organization Advisory Group for Aerospace Research and Development) Specialists Meeting to be held in Paris, France in October of that year. Although the subject matter, dealing with *performance,* ("Flight Test Verification of Pre-Flight-Predicted Performance") of the F-15 was certainly not my favorite, I, of course, accepted the challenge.

Airplane "performance," in the context used throughout the world-wide industry, deals mostly with aerodynamic drag along with thrust, weight, and fuel flow and their impact on such parameters as maximum speed, maximum altitude, climb rate and acceleration, and range, etc. (These are the types of things that Program Managers and members of Congress think they understand). Within Aerodynamics organizations, "performance" is normally regarded as somewhat of a sub-discipline staffed by the appropriate specialists. Another sub-discipline, "stability and control," dealing with control system characteristics, maneuverability, flying qualities, etc. was the subject of most of the preceding pages.

The key to successful airplane design is to achieve the proper blend of *Performance* with *Stability and Control.* Unfortunately, the two sub-disciplines have a tradition of conflict with each other. An airplane with excellence in one arena may exhibit mediocrity in the other. For fighter airplanes, it has always been a conflict between "speed and range" versus "dog fighting...agility" —even well before World War II.

"....inadequate maneuverability in a fighter can be more than compensated for by the superior skill of its pilot. .... the maximum speed of an airplane is strictly limited by its power and the design of the airplane; even the best pilot cannot through his own skill make an airplane fly faster than its designed maximum performance. Conversely, in dogfights, outstanding pilot skill can endow our men with a clear superiority, easily compensating for lack of maneuverability. ....I insist that speed and range be given priority over dogfighting performance."

Lt. Cdr. Takeo Shibata
Japanese Imperial Navy
January 1938

"In a fighter, ...the single most important characteristic is the ability of the airplane to engage successfllly in close-in fighting-dogfighting, as you will. Maneuverability is unmistakably the fighter's most valuable performance requirement, and we should accept a corresponding loss in speed and range in order to secure the necessary agility."

Lt. Cdr. Minouru Genda
Japanese Imperial Navy
January 1938

Although I was well-versed in *performance* (having compiled a handbook for estimating aerodynamic drag in the early 1960s which is still in use nearly 50 years later), I had chosen the *stability and control* path as being more satisfying. Accordingly, virtually all my F-15 detailed experience had been lacking in *performance*—it was a real challenge to create the requested paper.

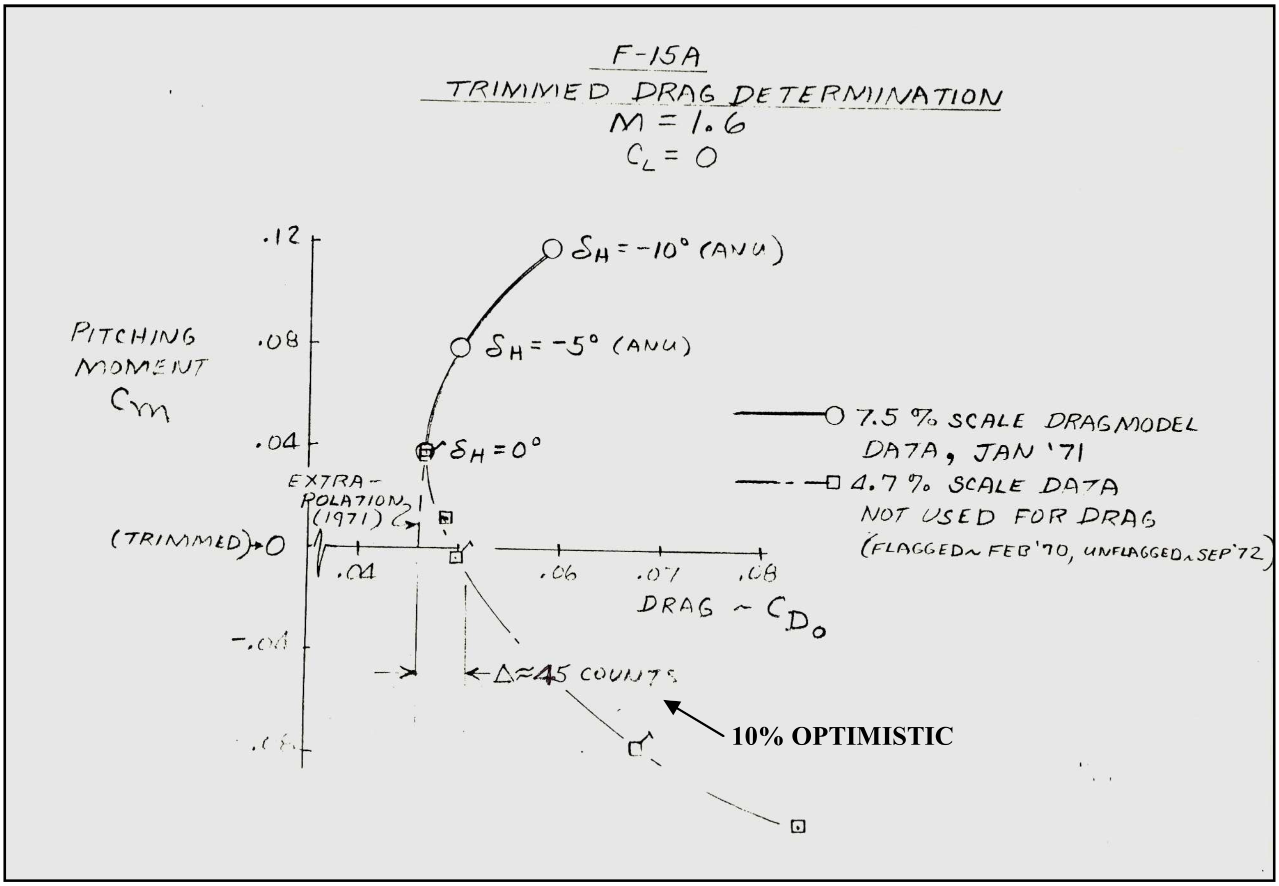
Drag Estimation by the Component Method

For use in Preliminary Analyses

By: Jack M. Abercrombie

To compound the problem, F-15 Performance capabilities were still classified SECRET. In addition, we considered the basic drag data as *Competition Sensitive*—we didn't want to show so much that the drag basis could be reverse engineered by a competitor.

But the most difficult challenge was to avoid disclosing to the world that our Drag and Performance management had neglected to use some key wind tunnel drag test data that would have changed some of the pre-flight performance predictions. During my short time while assigned to the *Advanced* F-15 program in 1971 (see Page 9), I had learned that our published pre-flight drag basis in the supersonic, low angle of attack regions was *grossly* optimistic to the point, in my



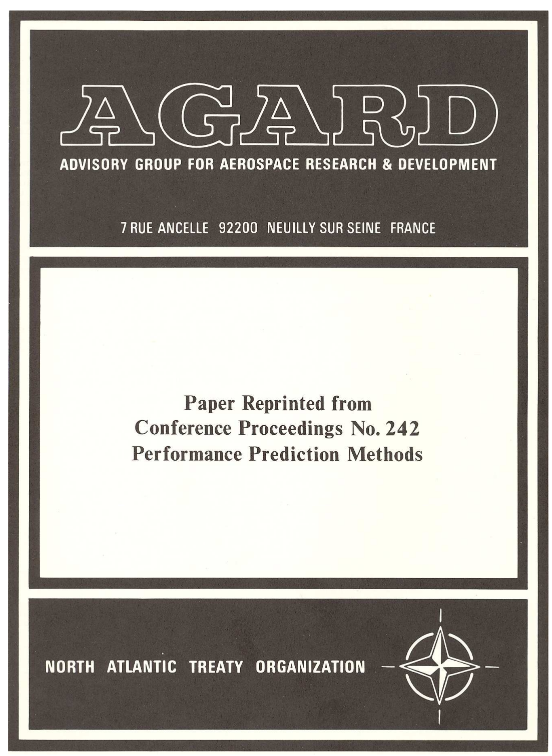
opinion, of being *irresponsible.* Fortunately, there were no safety-of-flight issues involved, so no big fuss was made at the time.

The previous paragraph is not intended to belittle the Drag and Performance technical team—where it really mattered (performance guarantees for high-g maneuvering acceleration and climb), the team did a remarkable job. In the high-g regions, there were no analytical techniques available to accurately assess the configuration complexities in regions where there were mixed subsonic/supersonic flow fields with local shock waves and regions of flow separation. And, in using a wind tunnel test data basis, they were breaking new ground.

I composed the paper with a clear conscience. The complexities of the problem were defined, and the Classification and Competition Sensitive issues were addressed by leaving scales off of the graphs and disguising the ordinates. For the pre-flight predicted drag curves, I used the wind tunnel test data that *should* have been included in the estimates but was not.

FLIGHT TEST VERIFICATION OF F-15 PERFORMANCE PREDICTIONS

J. M. Abercrombie
Section Chief Technology-Aerodynamics
Aerodynamics Department
McDonnell Aircraft Company,
A Division of McDonnell Douglas Corporation


AGARD
ADVISORY GROUP FOR AEROSPACE RESEARCH & DEVELOPMENT
7 RUE ANCELLE 92200 NEUILLY SUR SEINE FRANCE

Paper Reprinted from
Conference Proceedings No. 242
Performance Prediction Methods

NORTH ATLANTIC TREATY ORGANIZATION

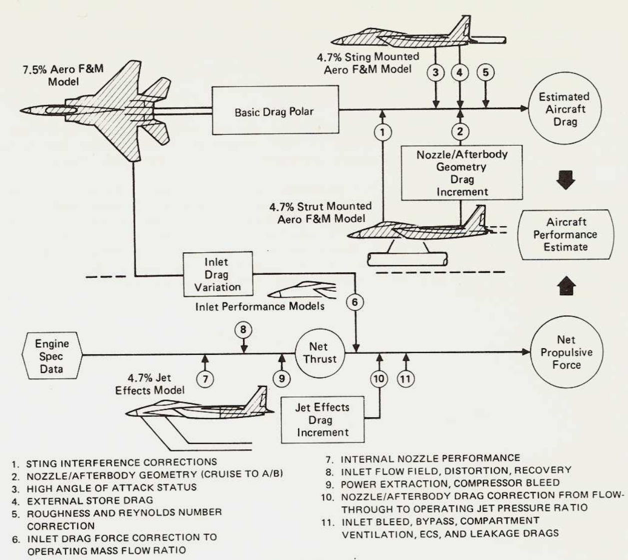


FIGURE 8
F-15 THRUST DRAG ACCOUNTING SYSTEM

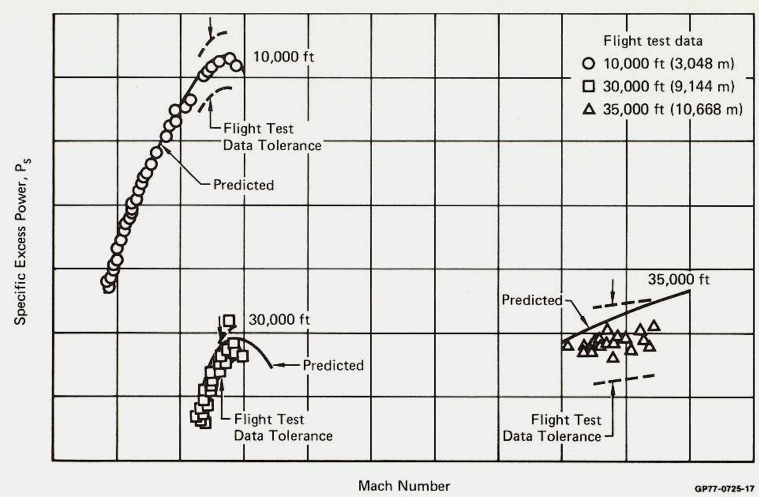


FIGURE 15
SPECIFIC EXCESS POWER COMPARISON
5g Normal Load Factor

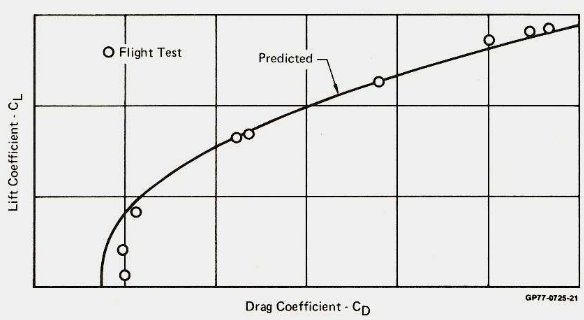


FIGURE 20
DRAG POLAR COMPARISON
SUPERSONIC
h = 30,000 Ft. (9,144 m)

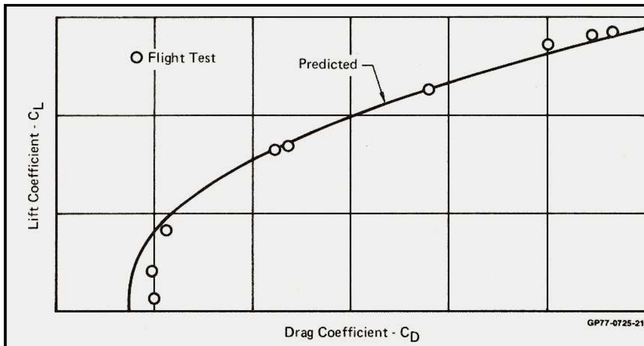


FIGURE 20
DRAG POLAR COMPARISON
SUPERSONIC
h = 30,000 Ft. (9,144 m)

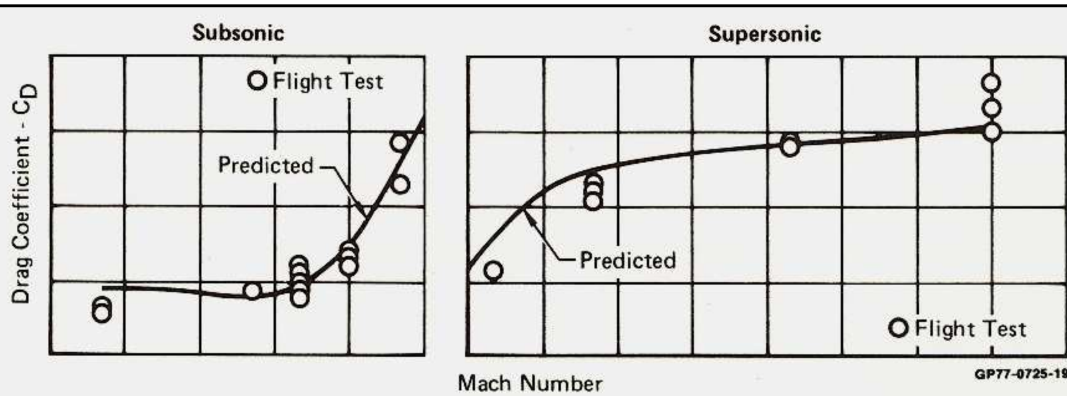
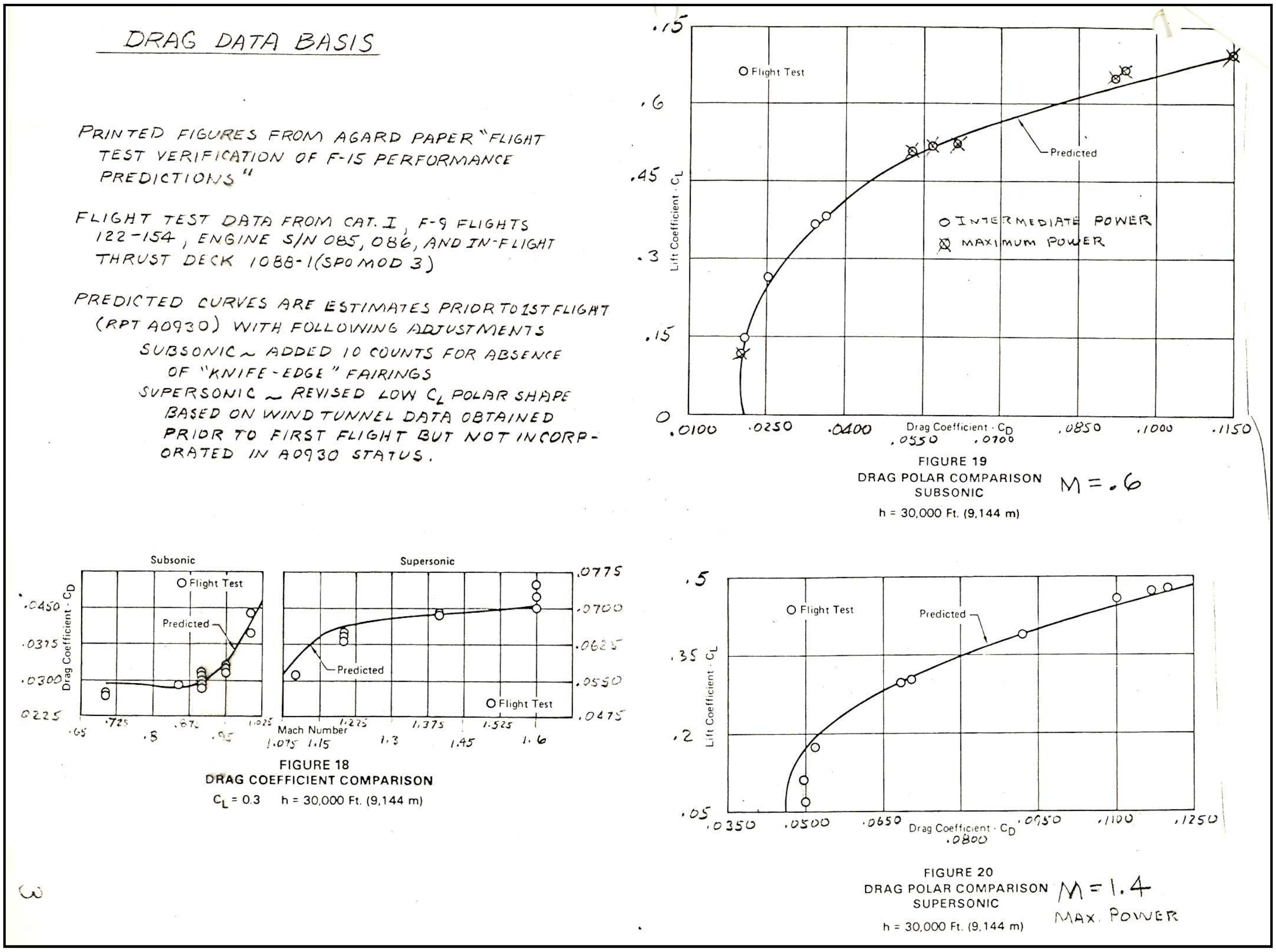


FIGURE 18
DRAG COEFFICIENT COMPARISON
$C_L = 0.3$ h = 30,000 Ft. (9,144 m)

Some months later, I wrote an internal "CYA" memo defining the whole procedure.



As they used to say in the trade, "The paper was well received." However, there was one individual who expressed some skepticism—Gene Rooney, a Naval Air Systems Command civil servant and the leading government expert on airplane drag and performance. Gene had been involved in the USN flight evaluation of the F-15 some five years before; he knew that things were not so rosy as might have been depicted based on his assessment of the real airplane compared to our advertisements of the time. But he was kind and didn't pursue the matter in depth at that time. (A few years later, he wasn't so forgiving when he took McDonnell to task for being too **optimistic** in performance predictions for the AV-8B Harrier. By that time, I was the head of the Aerodynamics Department and had to endure his scathing comments in front of a large crowd. The AV-8B project deserved his derision at the time; I was merely in the line of fire. But between the two of us, we brought about some much needed major changes to McDonnell Program Management *modus operandi*—another story, another time).

EXCESSIVE DISPLAY OF OPTIMISM

- o PROGRAMATIC INFLUENCES - MAINTENANCE OF COMPETITIVE POSTURE IN UNFRIENDLY ENVIRONMENT
- o WITH MANPOWER/FUNDS LIMITATIONS - SOMETIMES ROOM FOR REASONABLE DOUBT, THEREFORE PICK THE OPTIMISTIC
- o PATERNAL PRIDE/DEFENSIVENESS
- o PRESSURES FROM PROGRAM/ENGINEERING MANAGEMENT

In spite of the difficulty maintaining my technical integrity preparing the AGARD paper, I enjoyed the Paris experience and consider it a significant career achievement.

Paris was also a great place to spend a few vacation days.

Jack Abercrombie
17 Aug 2009

NEW DIRECTIONS IN AIRCRAFT DESIGN

Two fighter aircraft developments of the 1980s, although significant at the time, from the perspective of thirty years later, seemed to have consumed an inordinate amount of resources. The developments were ***Stealth*** and ***Supermaneuverability.***

STEALTH—Reduced detectability, or *Stealth,* has been a much sought-after feature for airplanes since the beginning of their use in warfare. Technology has tried to address most of the senses in the quest for reduced signature—visual, acoustic, infra-red heat, and radar reflection. In the mid-1970s, with increased use of digital computers, the capability to analytically determine desired airplane shapes for tailoring radar signature was in the development stages. A proof-of-concept airplane with the code name, "Have Blue," flight tested in late 1977, eventually led to the so-called F-117 Stealth "fighter."

Although the F-117 (both the proof-of-concept and the production vehicles) was developed under highly secret conditions ("Black" Special Access Program), when the Grade "B" movie actor, Reagan, was put into the White House (1981), secrecy of most Department of Defense activity multiplied exponentially. The yet-to-be-indicted for crimes during the Iran-Contra scandal (and pardoned by Bush 1) Defense Secretary, Weinberger, created a multitude of Top Secret programs in various shades of black, many of which were devoted to stealth technology. Silly code names (such as the previously mentioned "Have Blue") were invented for each program—even the code names were classified "Top Secret, Special Access Required." The individuals who were "read in" to a TS/SAR stealth program could not even utter the word "stealth" in public for fear of creating a full-fledged investigation of their loyalty! Each Special Access program employed personnel with different levels of security clearance— Confidential, Secret, and Top Secret with Top Secret further subdivided into different levels. Such was our Cold War mentality.

In spite of all this nonsense, there were some very significant technological advances in stealthy concepts during the early '80s: radar absorptive structural material and coatings, unusual airframe shapes with smaller stabilizer surfaces employing digital computer-generated artificial stability, exotic shape and material treatments to engine air inlets and exhaust systems as well as to avionics apertures, and configurations that would use thrust vectoring to reduce the size of aerodynamic controls.

SUPERMANEUVERABILITY—The term, *Supermaneuverability,* was apparently coined in 1980 by one of the German engineers who worked for McDonnell in 1969 developing our Computer Aided Design and Evaluation program (see Page 3). Wolfgang Herbst published a paper in the *Journal of Aircraft* extolling perceived air-to-air combat (one vs. one) benefits of being able to execute controlled maneuvers at angles of attack well beyond aerodynamic stall. His conclusions were based on "extensive wind-tunnel testing and manned as well as model air combat simulations" according to his paper. Of course, thrust vectoring would be required to supplant the ineffective aerodynamic control at the post-stall conditions.

A frenzy soon developed throughout the industry over Supermaneuverability—it seemed that every aviation organization was in the business of trying to justify the concept and was studying thrust vectoring as a means to accomplish the capability.

***

So, now, there were two developments demanding thrust vectoring to perform the job normally held by aerodynamic surfaces. Since no one could really talk publicly about *stealth* without getting into trouble, dreamers were busy postulating aircraft configurations which relied on thrust vectoring for primary stability and control—justified by a supposed *reduction in total airplane drag with attendant improvements to performance.* (Of course, anyone halfway through an Aerodynamics 101 course would know that thrust vectoring rather than aerodynamic surface control would have an overall negative effect on airplane performance).

During the same period that Stealth and Supermaneuverability were being promoted, the Air Force was formulating plans for the next generation tactical fighter (Advanced Tactical Fighter, ATF) to follow the F-15 and F-16. And, to fill the void, they would decide between competing modifications of the F-15 and F-16 as "dual roll fighters" with enhanced capabilities in both air-to-air and air-to-ground rolls. The F-15 won this competition with a derivative that would become known as the F-15E "Strike Eagle."

So there was a lot going on in the fighter world. The real-world airplane designers and users were skeptical about thrust vectoring for use in a fighter aircraft. Consequently, a new initiative was created within the Air Force to develop and demonstrate vectoring on a fighter airplane configuration. Three airframers entered into discussions with a newly created Advanced Development Program Office (ADPO)—General Dynamics began working on a thrust vectoring version of the F-16, Northrop (our "partners" on the Navy F-18) worked on a thrust vectoring version of the F-18, and McDonnell started design work on an F-15 variant with vectoring.

Investigate, Develop, and Validate Four Technology Areas Related to Providing STOL Capability for Fighters:

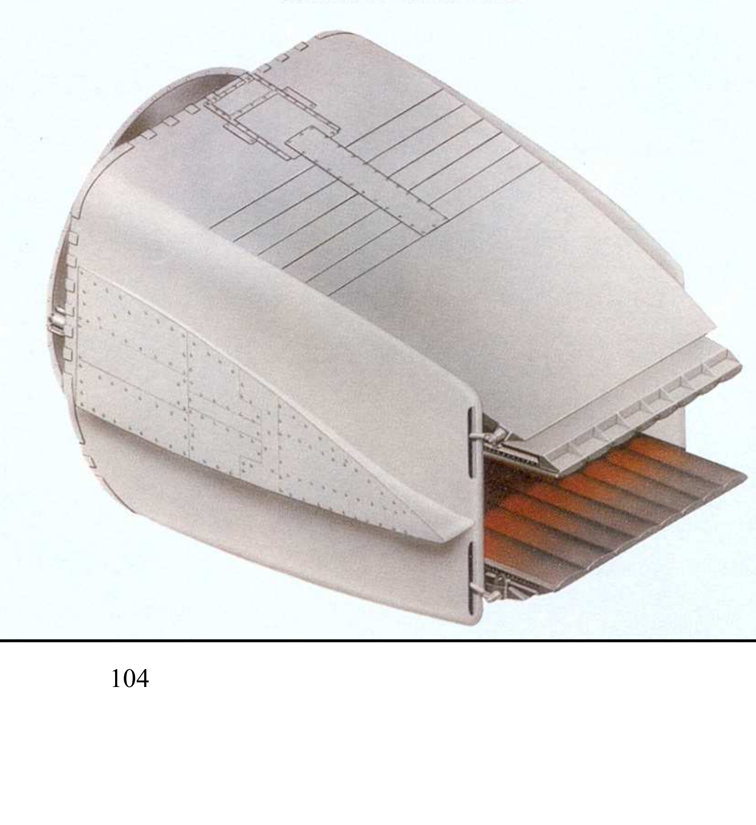
- 2D Vectoring/Reversing Nozzle
- Integrated Flight/Propulsion Control
- Rough/Soft Field STOL Landing Gear
- Advanced Pilot/Vehicle Interface

Provide Design Options for Derivatives and Future Fighter Aircraft (ATF)

After several false starts, the ADPO issued a final Request for Proposal (RFP) in September 1983. By now the flavor had changed from merely a thrust vectoring engine nozzle program to one of "enhanced maneuverability" with emphasis on Short Takeoff and Landing (STOL) features for operations on bomb damaged runways, supermaneuverability (although that term was no longer in use), and better high angle of attack performance than with existing fighters. Besides thrust vectoring, a requirement for in-flight thrust reversing was imposed. It was believed that the nozzle functions could be better obtained through use of rectangular nozzles (so-called Two Dimensional or 2D) rather than the conventional round (axisymmetric) configuration. And, at the same time, the requirement for improved capabilities on *soft* and/or *rough* runways was imposed. With the addition of forward control surfaces (canards) to achieve better high angle of attack performance, things were rather complicated for the pilot. As a result, an improved *Pilot-Vehicle Interface* (PVI) was added to the requirements. (In plain English, the cockpit displays and pilot activated controls needed to be made more effective and easier to use).

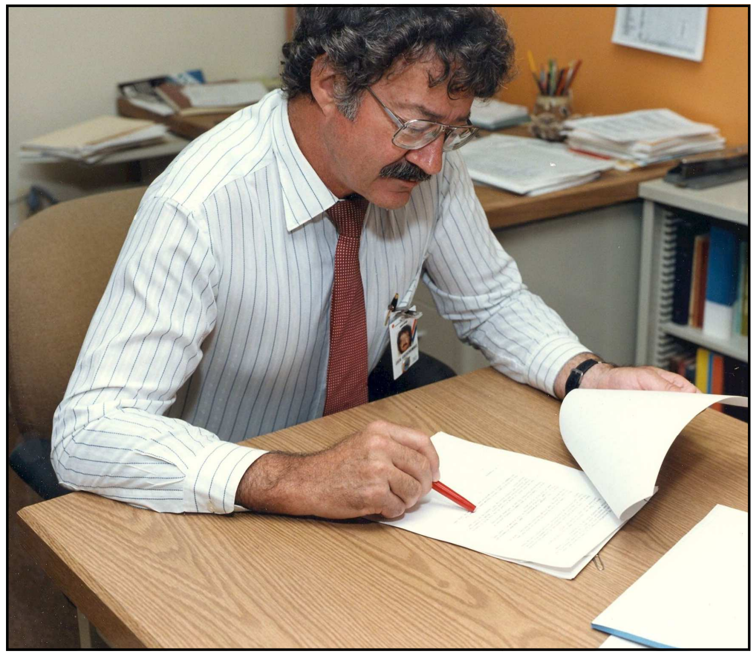
MCAIR was announced the winner of the STOL and Maneuvering Technology Demonstrator (S/MTD) competition in October 1984 (at about the same time that a Draft RFP was issued for the Advanced Tactical Fighter, ATF, program). A short time later, Pratt & Whitney (P&W) was selected to build the thrust vectoring/reversing nozzle. First flight of the F-15 S/MTD was planned for October 1987, three years after Contract Go-ahead. The contract was structured to be one of "cost sharing" where the Air Force would pay for about one-third of the program cost. The remainder would be shared by MCAIR and the various suppliers, with a significant portion from P&W. All this in the interest of expanding our expertise and passing the technology on to future programs in which we hoped to participate.

S/MTD Nozzle



Almost immediately after Contract Award, the engine nozzle schedule began to slip. The promised nozzle weight grew, and engine company costs began to sky-rocket. Of course, P&W began passing on the cost increases to MCAIR.

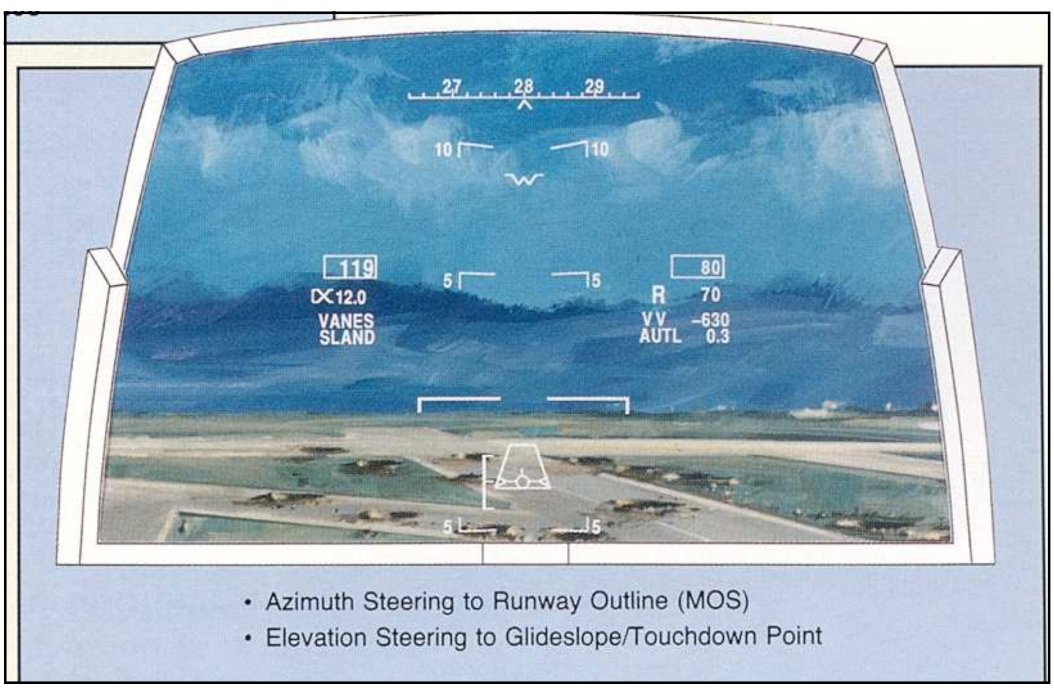
After having served as the Aerodynamics Department head for more than three years, I was asked to transfer to the S/MTD program to serve as Technology Integrator in October 1985. In that assignment, I was responsible for not only Aerodynamics, but also Guidance and Control, Propulsion, Weight, Reliability, and the various Structural technologies. Later in the program, I served as Chief Engineer with responsibility for all the engineering disciplines (including avionics, flight test, wind tunnel test, and the various test laboratories).



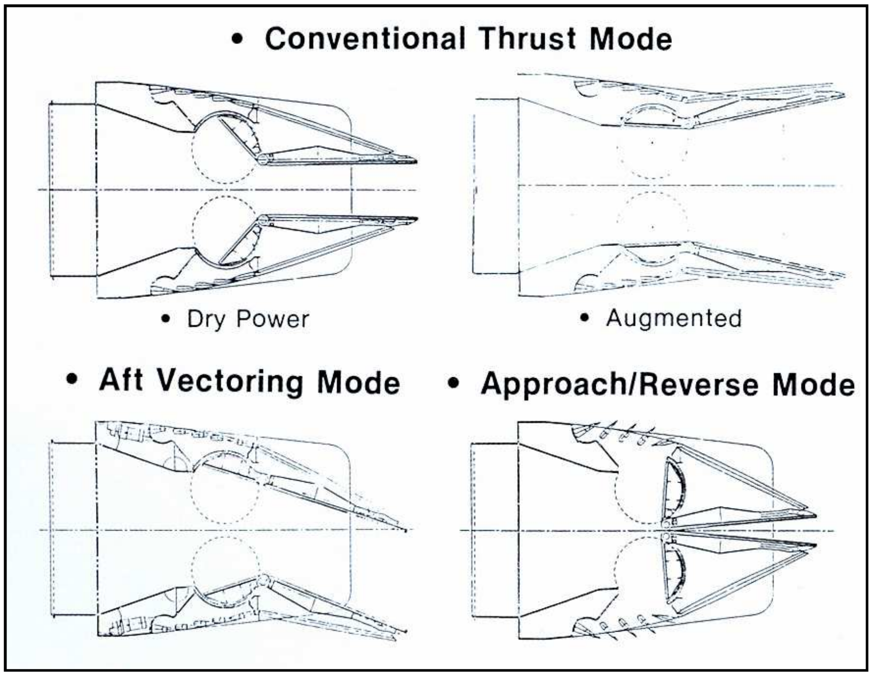
In spite of the ever deteriorating relations with our main supplier, Pratt & Whitney, the S/MTD program was most enjoyable from the standpoint of developing solutions to all the technical challenges we faced.

The airplane we modified for the program was TF-15#2 (by now called an F-15B) which had first flown in 1973. In addition to the engine nozzle changes, we added forward canard control surfaces which were originally F-18 horizontal tails. We made some minor internal changes to both the main and nose landing gears to better accommodate

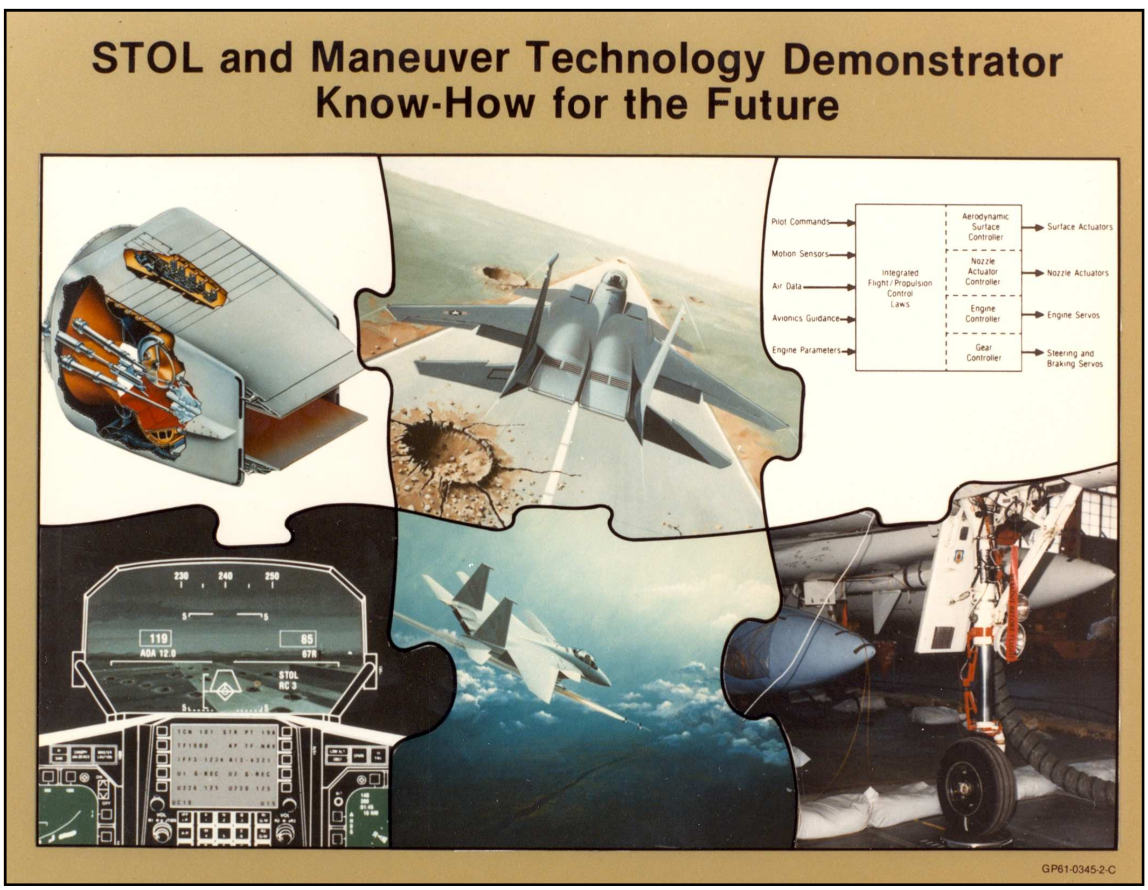
issue, and we made considerable improvement to the pilot's instrument panel including the capability of projecting a virtual runway onto the Heads Up Display.



Perhaps the most significant change made to the airplane was the incorporation of a completely *digital* electronic control system for accommodating both the canard controls as well as the engine/nozzle controls. For this effort, the airplane's mechanical and analog CAS controls (See Page 52) were removed.



There were no technical challenges that presented major problems. Everyone who worked on the program was well experienced—some on the F-15 and F-18 programs—and enthused about the S/MTD. We were convinced there would be payoff for the future of our company.



In July 1986, amidst rumors that the Air Force, for some reason, wanted to slip the program for a year or more, we passed the "Critical Design Review" milestone with flying colors. At this point, the detail design was essentially complete, and the Air Force was satisfied that the design satisfied all requirements. (Coincidentally, four companies/teams submitted proposals for the Advance Tactical Fighter, ATF, at this time).

By October 1986, P&W costs for the S/MTD nozzles had nearly doubled—from $43M at Contract Award to $74M. (About the same time, two teams of Lockheed/General Dynamics/Boeing and Northrop/MCAIR were turned on to a "fly-off" competition of their respective ATF proposal configurations). When a few months later, the P&W costs were projected to be at least $113M (and it became evident that the Northrop/MCAIR ATF team led by Northrop had no interest in thrust vectoring), our company became very disinterested in any more cost sharing on the S/MTD program.

Shortly thereafter, I was re-assigned to a "Top Secret—Special Access" ("Black") program. The F-15 S/MTD program continued. First flight occurred on 7 September 1988 (with non-vectoring, round nozzles since we were still awaiting the 2D nozzles) with Test Pilot Larry Walker at the controls.

Finally, on 10 May 1989, the F-15 S/MTD configured with the 2D thrust vectoring/reversing engine nozzles flew for the first time—nineteen months later than originally planned. By then, P&W costs had swollen to more than $140M—325% of the original estimate! The test program continued quite successfully. Upon completion of the MCAIR/Air Force testing, the airplane was baled to NASA DFRC at Edwards AFB where it continued to demonstrate its remarkable performance, ending with a *round* vectoring nozzle test program.

NASA Dryden Flight Research Center Photo Collection
http://www.dfrc.nasa.gov/Gallery/Photo/index.html
NASA Photo: EC05-0148-31 Date: July 22, 2005 Photo By: Tony Landis

NASA's two modified F-15B research aircraft joined up for a fly-over of NASA's Dryden Flight Research Center on Edwards AFB, Calif., after a research mission.

Meanwhile, back in the ATF world, the Northrop/MCAIR contender, the YF-23, (with *non*-vectoring engine nozzles) made its first flight on 27 August 1990. A month later, 29 September 1990, the Lockheed led team's YF-22 flew for the first time. In the following days, the Lockheed team flew both P&W and GE engines/nozzles and demonstrated the tactical uses of thrust vectoring. In April 1991, the Lockheed team with the P&W *vectoring* engine nozzles won the ATF competition.

The S/MTD program was a strange one—mixed signals from the Customer relative to schedules: "Full speed ahead" to "Slow down." Intra-office battles within the Air Force in front of the Contractor. The most political maneuvering than I'd ever seen before in the Customer community. I had not connected all this information with what was happening in the ATF world until recently. I speculate that P&W got in bed with Lockheed very early in the S/MTD and ATF programs and drug their feet on our program since our ATF management and Northrop had no apparent interest in thrust vectoring for the ATF.

In spite of the political hassles, the S/MTD program was very satisfying to me. The technical problems were all soluble, and, compared to the stressful situations of earlier F-15 experiences, even enjoyable. And, during my time on the program when I wasn't working, I was able to complete training for my Private Pilot License and, with my partner, Tom McDuffee (mentioned several times earlier) acquire an airplane.

I continued working in various "Black" worlds from August 1987 until my retirement from the company in the spring of 1991.

In 2004, I became involved again briefly with the F-15—I taught a course in fighter airplane design to students from South Korea as part of the purchase agreement for the F-15K, but that's another story.

Jack Abercrombie
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