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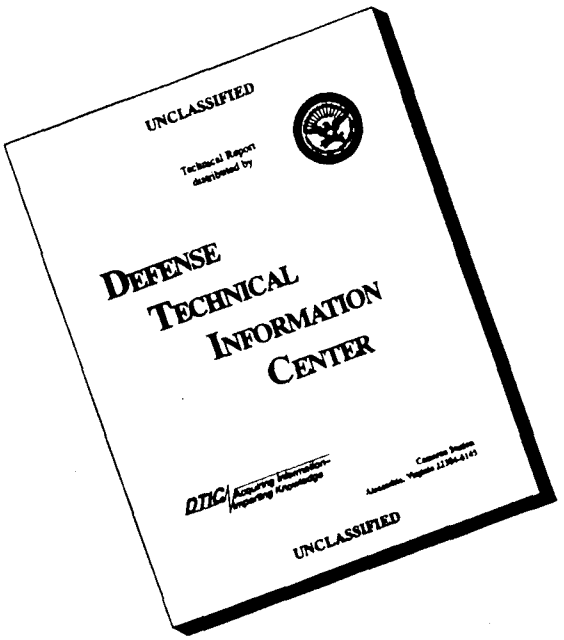

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13. ABSTRACT *(Maximum 200 words)*

The hypersonic revolution has been a particularly American one, borne of the national pursuit of transonic and supersonic flight technology. True, it does have both domestic and international dimensions, in the prophecy of Robert Goddard, Hermann Oberth, and Konstantin Tsiolkovskiy at the beginning of the twentieth century, and in the prescient (if impractical) studies of Eugen Sanger and Irene bredt (later Irene Sanger-Bredt) near mid-century. But if its inspiration was sometimes international in flavor, its execution was American--from the early pre-X-15 studies of the 1950's through the pioneering missions of Columbia in 1981. Primarily, the hypersonic revolution grew out of the traditional federal-industrial partnership that had benefitted American aviation since the First World War. It germinated and flourished amidst the laboratories of the Air Force, Navy, the National Advisory Committee for Aeronautics (NACA), and its successor, the National Aeronautics and Space Administration (NASA) , and the major aerospace manufacturers. Not merely an aerodynamic revolution, the hypersonic revolution--like the supersonic breakthrough and the drive for the "modern" airplane before it--involved the creative integration and exploitation of diverse technologies.

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THE HYPERSONIC REVOLUTION: EIGHT CASE STUDIES IN THE HISTORY OF HYPERSONIC TECHNOLOGY

Edited by

Richard P. Hallion

Volume II:

From Scramjet to the National Aero-Space Plane

(1964-1986)

With Contributions By

John Becker

Richard Hallion

John Vitelli

James Young

A Special Staff Office Study

Aeronautical Systems Division

Wright-Patterson Air Force Base, Ohio

1987

The second volume of this study is dedicated to the men and women of the National Aero-Space Plane Joint Program Office at Aeronautical Systems Division, Wright-Patterson Air Force Base, Ohio, who are working to extend the hypersonic revolution into the twenty-first century, and whose motto is "The sky is no longer the limit."

VOLUME II

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FOREWORD TO THE SECOND VOLUME

The case studies in this second volume range from a bold experiment to develop an actual supersonic combustion ramjet engine for hypersonic flight, to piloted lifting body demonstrators, and on to the Space Shuttle itself. Finally, this volume contains a concluding essay that, in effect, is a case study and introduction to advanced hypersonic vehicle concepts pursued from the time of the Shuttle's inception to the onset of today's National Aero-Space Plane (NASP) study effort.

As with the first volume, it is hoped that the case studies in this one will prove useful to members of the hypersonic community as they pursue contemporary projects, as well as to those seeking an understanding of the past.

I wish to acknowledge with special gratitude the editorial contributions to both Volumes I and II made by Ms. Mitzi Vitucci of the ASD Directorate of History and subsequently the Life Support System Program Office, and the manuscript preparation support given to this project by Vicki Cowburn of Headquarters Squadron Section and Terri Anderson and Patricia Robbins of the ASD Deputy Chief of Staff for Resource Management. In particular I wish to thank Mr. Ronald C. Isemann, the Deputy Chief of Staff for Resource Management, and Mr. Les Mosher of the Reprographics Branch, for their assistance in bringing this study to fruition. Finally, I wish to thank the staff of the ASD Directorate of History, Mr. Albert E. Misenko, Dr. Bruce R. Wolf, Mr. James F. Aldridge, Ms. Diana G. Cornelisse, and Joyce E. Bettencourt, for their unfailing assistance to this project.

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1 October 1987

CASE VI

CONFRONTING SCRAMJET:

THE NASA HYPERSONIC RAMJET EXPERIMENT

by

John V. Becker

EDITOR'S INTRODUCTION

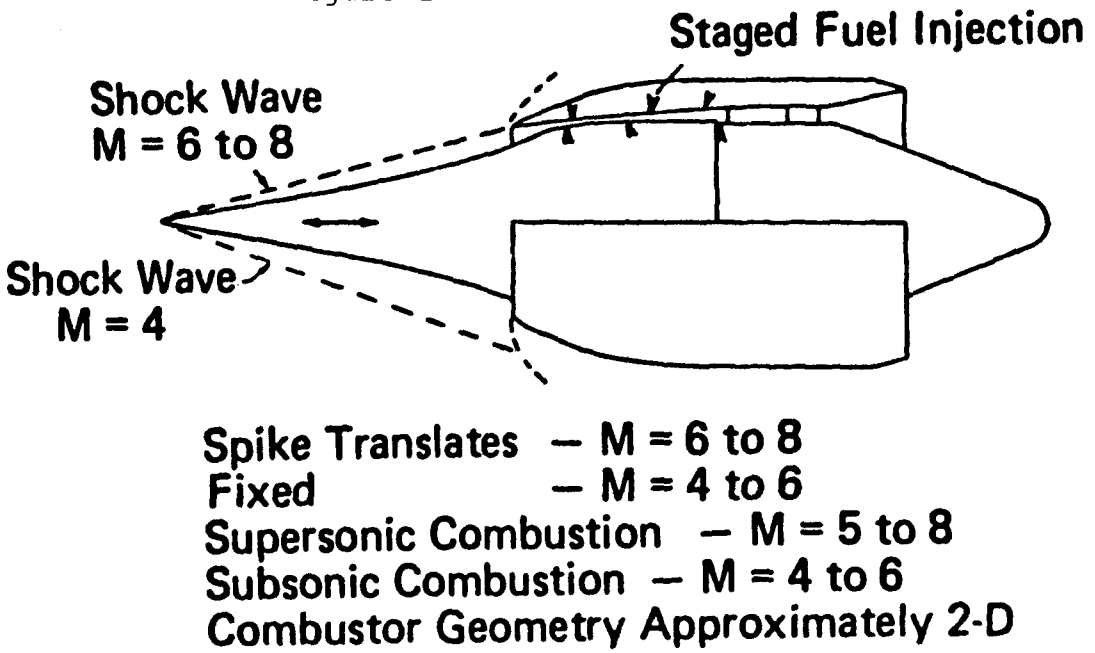
This case study is an anatomy of a failure. During the mid-1960s, the National Aeronautics and Space Administration attempted to develop a flight-worthy supersonic combustion ramjet. This project, the Hypersonic Ramjet Experiment (HRE) (Figure 1), cost approximately $50 million, and generated only one noteworthy accomplishment: "the illumination of critical unknowns," as author John V. Becker writes. It is a graphic example of what happens when an immature technology is pushed too fast too soon, in the absence of comprehensive thought and planning, and with zeal replacing insight. Ultimately, the kind of technical approach taken with the HRE--namely, the axisymmetric circular-cross section podded engine a la earlier subsonic ramjets such as those used on the Bomarc surface-to-air missile--was discredited, and the modular scramjet approach where the engine is fully integrated into the aircraft rose into prominence. The HRE story reminds one of other great propulsion disappointments that afflict aviation history, such as the British A. B. C. Dragonfly of World War I. Like the HRE a half-century later, the A. B. C. Dragonfly attempted to take an immature technology--in this case that of the radial piston engine--and adapt it to the needs of high-performance aircraft. The Dragonfly proved a disaster, and the first successful radial engines did not appear for nearly another decade. In the 1950s, the sad story of the Westinghouse J46 engine echoed some of the experience of the Dragonfly. In the J46 case, Westinghouse underestimated the difficulties of going from the earlier J34 family of axial-flow turbojets to the more complex and larger J46, assuming incorrectly that it involved little more than a matter of scaling. The J46 taught turbojet engine manufacturers bitter lessons in the importance of comprehending engineering problems and capabilities, for the failure of this engine program resulted in major disruptions and cancellations in

the development of new military aircraft. The HRE story should be read as a cautionary lesson by today's scramjet enthusiasts.

The scramjet is a form of ramjet engine that makes use of supersonic combustion; the acronym scramjet is, in fact, an abbreviation of supersonic combustion ramjet. It is inextricably caught up in the evolution of hypersonic aerodynamics--now more than ever, since proponents now recognize the necessity of thorough airframe-engine integration. Since the days of the original Aerospaceplane concept of the 1950s and early 1960s and continuing into the present-day studies of the National Aero-Space Plane program, scramjet potentialities have influenced planners defining and forecasting the final performance capabilities sought of such vehicles. Fig. 2A offers a comparison of the scramjet and ramjet.

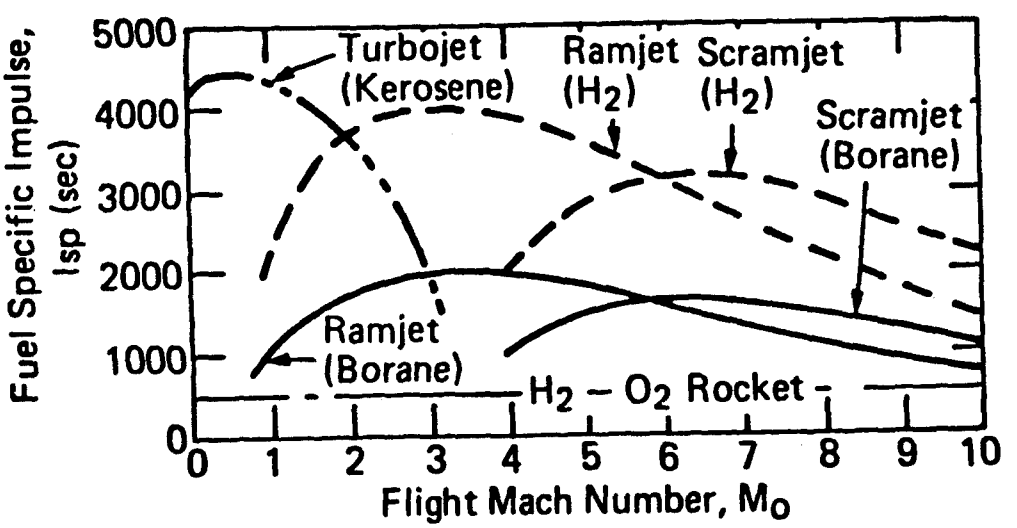
The ramjet itself--from whence scramjet sprang--has an interesting aeronautical history. The "traditional" ramjet is of circular cross-section, and makes use of a spike-like inlet and diffuser that acts in a method analogous to the compressor of a conventional turbojet engine. Air entering through the inlet is compressed, fuel is injected into it, the mixture is ignited, and combustion is stabilized by some form of flameholders. The exhaust jet passes through a convergent-divergent nozzle before being expelled. Since the ramjet lacks a rotating compressor, it requires boosting to some minimal airspeed (usually at least 250 mph) before it can sustain combustion and generate thrust greater than its own drag. A close relative of the ramjet, the pulsejet, powered the V-1 cruise missile of World War II. Because the field of gas turbine and rocketry advanced so rapidly in the 1940s and 1950s, the ramjet rapidly dropped out of favor as a propulsion system for conventional aircraft. As propulsion historian and hydrogen fuel pioneer John L. Sloop has written, "The ramjet, always needing an auxiliary propulsion system for starting, got squeezed between improved turbine engines and rockets during the 1950s and never recovered."[1] Nevertheless, its clear potentialities for hypersonic flight in the region between the turbojet and the rocket understandably (as shown in Figure 2) led (and continue

Figure 1



NASA HYPERSONIC RAMJET EXPERIMENT

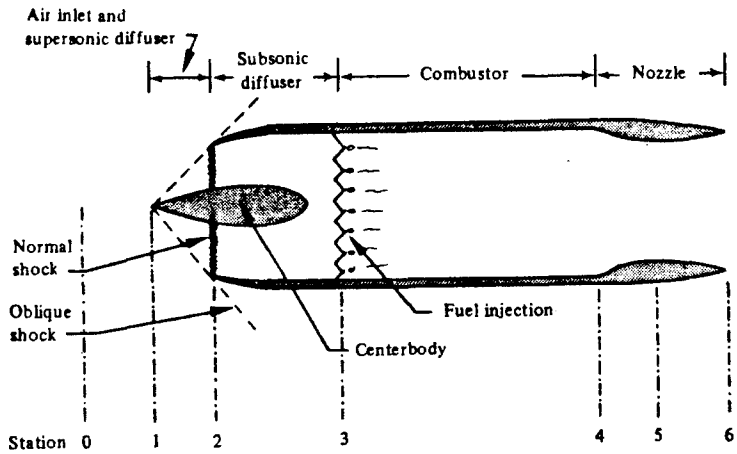
Figure 2



FUEL SPECIFIC IMPULSES FOR VARIOUS PROPULSION SYSTEMS

FIGURE 2A

COMPARISON OF SUBSONIC AND SUPERSONIC COMBUSTION RAMJETS

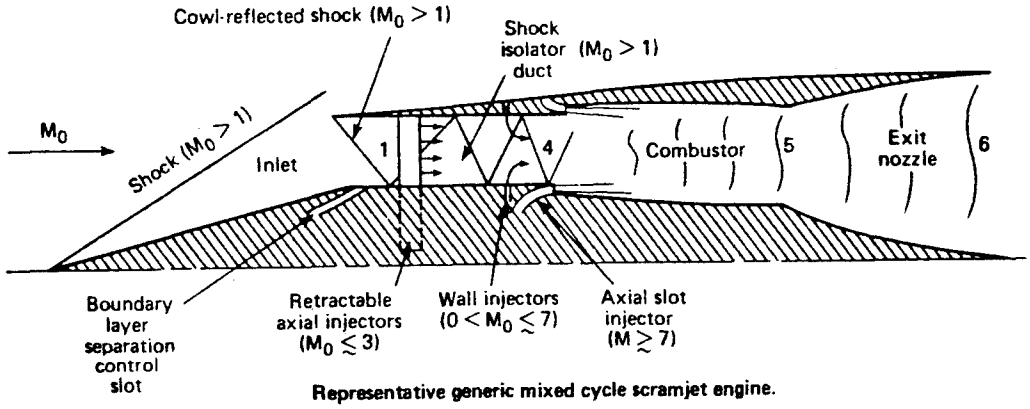


Station Number	Location
0	Vehicle flow field immediately upstream of the air induction system
1	Capture station - beginning of internal flow system
2	Cowl lip
3	Diffuser exit - combustor entrance
4	Combustor exit
5	Nozzle throat
6	Nozzle exit

Liquid-fueled Ramjet Nomenclature

(a) Subsonic Ramjet

Source: Chemical Systems Division, United Technologies, THE POCKET RAMJET READER (n.d.), p. 13



Representative generic mixed cycle scramjet engine.

(b) Mixed-cycle dual-mode Scramjet

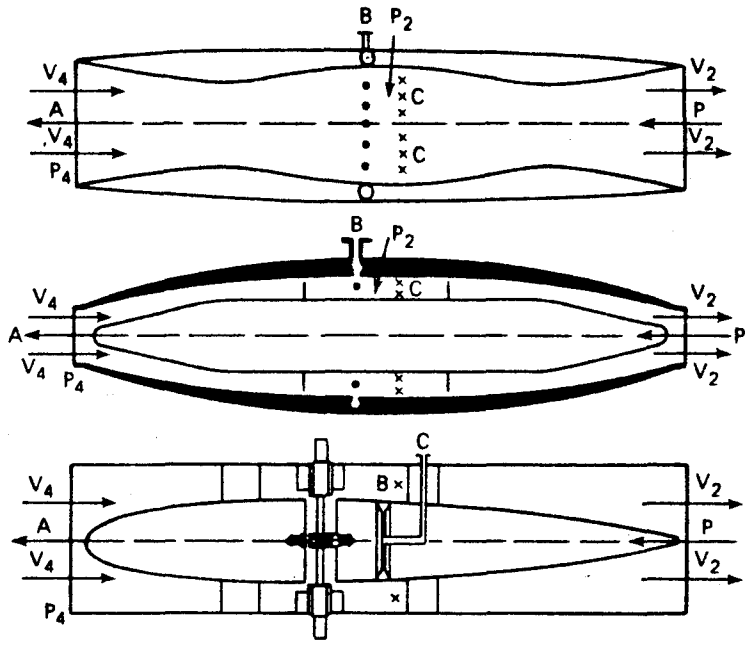
Source: Waltrup, HYPERSONIC AIR-BREATHING PROPULSION (1987), Fig. 28

to lead) hypersonic advocates to examine its potential as a propulsion method for advanced aerospace systems.

The story of the ramjet begins with theoretical studies by René Lorin in France in the years prior to World War I. Fellow Frenchman René Leduc actively pursued ramjet development during the 1930s and designed a pioneering ramjet-powered aircraft, the Leduc 0.10, though the intervention of World War II prevented it from flying until 1949. Interestingly, however, a group of Soviet engineers and scientists pushed ramjet research further than advocates in other nations in the years prior to World War II. In April 1933, a team under the leadership of Yuri A. Pobedonostsev bench-tested a small ramjet engine for five minutes. Recognizing that the ramjet fully realized its advantages only at supersonic speeds, they decided to build test ramjets from hollowed-out 76 mm shells, firing them from artillery field pieces: an idea advanced by British researchers in 1926. The modified shells had their nosecaps removed in favor of aerodynamic inlets, and the bursting charge replaced by solid fuel (white phosphorus or solidified hydrocarbon). A nozzle throat replaced the baseplate of the shell, and a sabot inserted into the nozzle prevented the powder charge of the cannon from destroying the ramjet as it was fired. From 1933 to 1935 the Pobedonostsev group fired a series of cannon-launched ramjets, achieving speeds up to Mach 2 (though it must be remembered that the muzzle velocity of the projectile was already approximately Mach 1.72, indicating that the ramjet boosted projectile speed by only approximately Mach 0.28). Numerous problems manifested themselves, but, overall, the tests encouraged Soviet ramjet enthusiasts to build small rocket-boosted ramjets, again using solid fuel. A research team led by Igor A. Merkulov launched several of these two-stage vehicles (consisting of the rocket booster and the second-stage ramjet) in 1939; because of the small size of the models, the ramjets only had fuel sufficient to reach an altitude of approximately 6000 feet following "staging" from the booster at approximately 2000 feet. Merkulov and Aleksei Shcherbakov (designer of Nikolai Polikarpov's later biplane fighters) next decided to test

FIGURE 2B

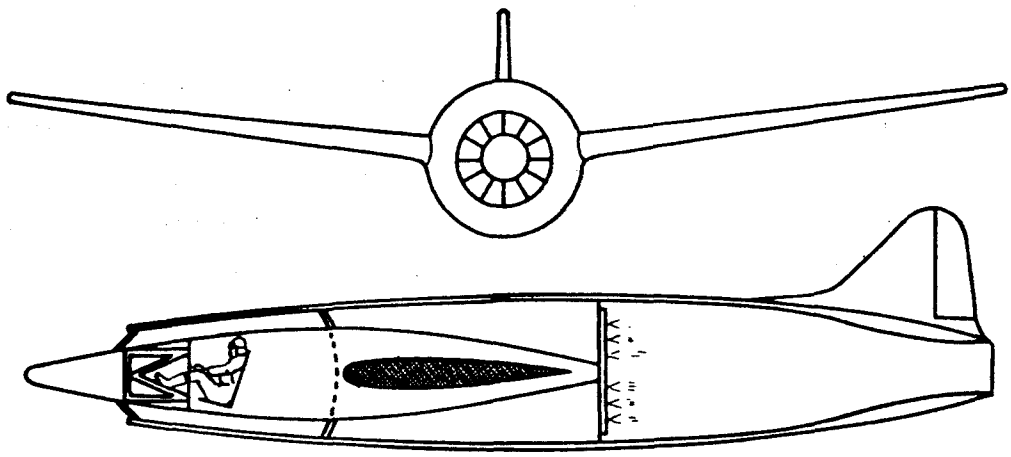
EARLY RAMJET CONCEPTS



Reproduction of figure from Fono's 1928 patent. B – fuel injection nozzles; C – flameholders.

(a) Patent drawing by Albert Fono for ramjet concepts suitable to propel supersonic aircraft, Germany, 1928

Source: Waltrup, HYPERSONIC AIR-BREATHING PROPULSION (1987), Fig. 5



Ramjet-propelled Airplane as Conceived by Leduc

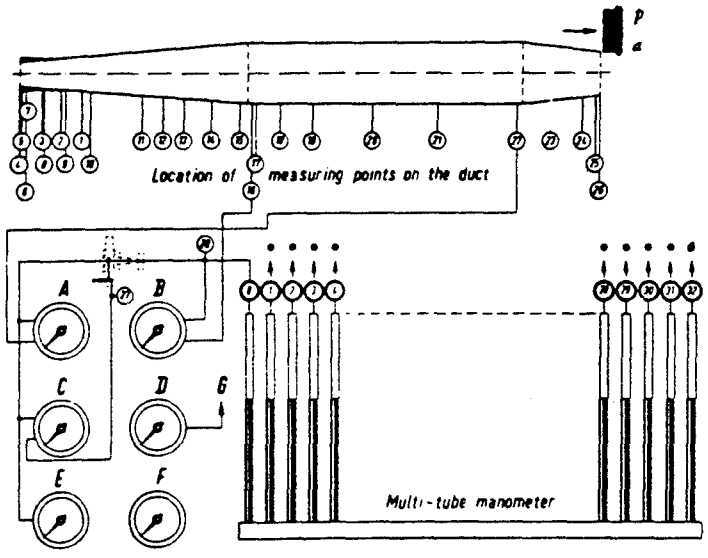
(b) Internal layout of proposed ramjet aircraft designed by René Leduc, France, circa 1938. (This became the postwar Leduc 0.10)

Source: Chemical Systems Division, United Technologies, THE POCKET RAMJET READER (n.d.), p. 1

ramjets on aircraft to evaluate their potential as combat boosters enabling obsolete fighters to engage higher-performance opponents. Eventually, on January 25, 1940, test pilot Petr Loginov flew a modified Polikarpov I-152 biplane fighter powered by two auxiliary liquid-fuel ramjets: the first flight by a ramjet-powered aircraft in aviation history. The Soviets eventually conducted further tests on modified I-153 and Yak-7 fighters, (as well as some miscellaneous types), until the rapid development of Soviet gas turbines (using German and British technology) rendered such auxiliary propulsion schemes superfluous in the late 1940s.[2] Before and during World War II, (beginning with studies by W. Trommsdorff) Nazi Germany likewise tested ramjet artillery shells and modified Dornier Do 17Z and Do 217E-2 bombers carrying experimental ramjets. By the end of the war, German designers Alexander Lippisch and Eugen Sänger (among others) had numerous ramjet-powered aircraft study projects underway. Experimental ramjet artillery shells of 15 cm (5.9 in.) diameter were fired at Mach 2.9 muzzle velocities, accelerating under ramjet propulsion to speeds of Mach 4.2 before burn-out. The Nazi success with ramjet artillery contrasted sharply with Soviet efforts, and stemmed from the greater care taken by the Germans to understand inlet and combustion processes, and the behavior of a variety of solid, liquid, and slurry fuels.[3] (See Figure 2C)

In the United States, engineer Roy Marquardt pioneered ramjet development during World War II, working under a Navy contract with the University of Southern California. The Army Air Forces tested some of his engines on modified testbed aircraft at the end of the war. Two M.I.T.-designed 20 in. diameter ramjets mounted on the wingtips of a modified North American P-51D Mustang boosted the fighter's speed by 40 mph during trials in 1945 at Wright Field. In 1948, Lockheed added two 30 in. diameter Marquardt ramjets to the wingtips of an experimental P-80 Shooting Star, and the aircraft flew on the power of its ramjets alone, with the J33 engine at idle.[4] In 1949, Leduc's 0.10 reached speeds of more than 500 mph following release at 200 mph from the back of a modified SE 161 Languedoc transport. He developed two advanced

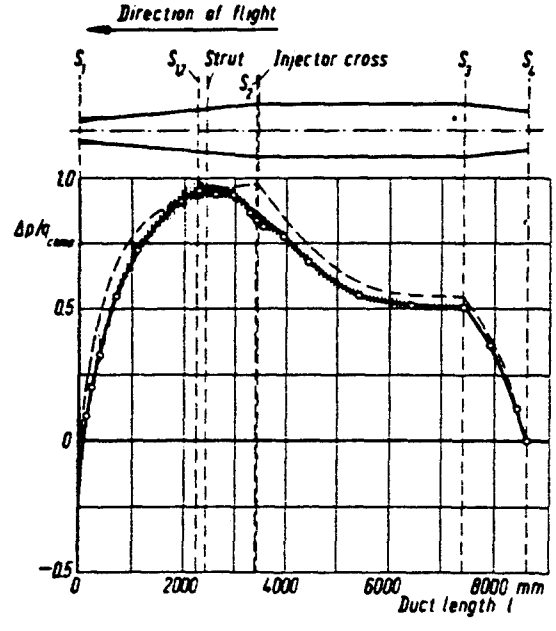
FIGURE 2 C



Measuring system for flight tests with 1000 mm duct

1—4: Pressure measuring points on the exterior wall of the diffuser
5—26: Pressure measuring points along the inside of the duct
For each test, the pressure measuring points 1 to 26 as well as the flight ram pressure point 27 and the basic pressure point 28 are connected to the columns 0 to 32 of the multi-tube manometer as far as needed, by using pressure tubes with capillary damping. The feelers a to p of the impulse rake are, as far as needed, connected to the columns of the multi-tube manometer.

A = Machmeter controlling excess pressure at measuring point 22
B = Machmeter controlling excess pressure at measuring point 16
C = Machmeter controlling ram pressure measuring
D = Injector pressure meter
E = Altimeter
F = Clock
G = To fuel injector pipe



Comparison of measured (full line) and calculated (dotted line) pressure distributions along the inner duct surfaces at full powered test flight in 4 km altitude. (Hatched area: space of medium error.)

SCHEMATIC VIEW OF RAMJET TEST RIG AND PARTIAL TEST RESULTS FROM INSTALLATION OF A RAMJET ON A DORNIER Do 217 E-2 RESEARCH AIRPLANE, GERMANY, 1942-43.

Source: Paul Spremberg, "The Flying Stovepipe," Cockpit, XVIII, 2 (April-June 1983), Figs. 2 & 3.

prototypes, the 0.21 and 0.22 (the latter a proposed Mach 2 interceptor), but the French government withdrew support in 1957, bringing Leduc's ramjet work to a close. In 1959, however, another French experimental aircraft, the canard delta Nord 1500 Griffon reached Mach 2.19 (1448 mph) under the combined power of a turbojet and ramjet, the ramjet contributing approximately 80% of the total thrust of the propulsion system. Interestingly, the United States Air Force supported Nord research via trans-Atlantic research contracts.[5] These projects were the exceptions, however, for overall, as Sloop has written, the success of the turbojet overshadowed that of the temperamental and fuel-hungry ramjet.

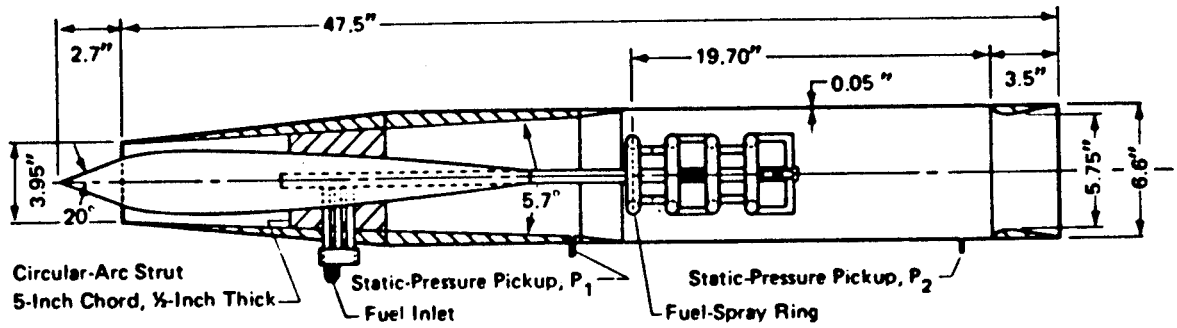
There were some areas where the ramjet excelled, however, involving primarily the field of missile development. The military services expressed an early and intense interest in ramjet propulsion for missiles, and the NACA contributed greatly to a ramjet knowledge base with research programs at the Lewis and Langley laboratories in the 1940s and 1950s, initially undertaken at the behest of the Navy's Bureau of Aeronautics in 1944. NACA researchers at the Langley Memorial Aeronautical Laboratory did not limit their research merely to missile applications, however. A team composed of Eastman Jacobs, Arthur Kantrowitz, Macon Ellis, Clinton Brown, and Coleman du Pont Donaldson investigated possible piloted ramjet vehicle applications. In December 1945, in a NACA Advance Confidential Report, Brown and Ellis proposed a ramjet supersonic research aircraft air-launched or towed to altitude, and then accelerated by small rocket boosters to transonic speeds where the ramjet would function. This 2500 lb. aircraft would have had a maximum speed of Mach 1.4, and though it did not emerge from the conference stage, it is nevertheless interesting as an example of the directions Langley ramjet researchers were taking at war's end.[6] In 1947, a NACA research ramjet reached Mach 1.45 after a small rocket booster accelerated it to ignition velocity. In 1949 and 1950, the NACA tested the more advanced ethylene-fueled F23 ramjet (Figure 3), which reached Mach 3.12 at an altitude of 67,200 feet (coasting to a peak of 159,000 feet after engine burnout) during one flight test at Wallops Island on

June 6, 1950. NACA also undertook air-launched ramjet trials, launching them from modified North American F-82 Twin Mustang and McDonnell F2H-2 Banshee fighters. Figure 4 shows one such test vehicle, with a Thiokol T55 solid-fuel rocket booster contained within the ramjet and then expelled through the nozzle as the ramjet fired up. This ramjet, designed by the Lewis laboratory, burned pentaborane fuel, and on February 23, 1956, demonstrated satisfactory operation using pentaborane up to Mach 3.02 following air-launch from an F2H-2 at 42,000 feet. A slurry-fueled ground-launched F29 ramjet (the slurry consisting of a mix of 50% JP-4 hydrocarbon and 50% magnesium, magnesium oxide, and aluminum) reached Mach 3.84 during testing at Wallops in 1958, a record for NACA ramjet testing.[7]

The ramjet asserted itself most notably in early missile programs such as the Navy's Talos and Air Force's Bomarc surface-to-air missile systems, and the abortive North American Navaho (MX-770). Of these three, the Navaho was the most ambitious, being a Mach 3+ intercontinental cruise missile. Though a subscale turbojet-powered demonstrator, the X-10 series, completed a number of successful flights, tests of the Navaho itself were disappointing, and the Air Force had, in any case, greater faith in the pure ballistic intercontinental missile as typified by the on-going Atlas program. Talos and Bomarc were successful programs, seeing widespread shipboard and land-based service, respectively; the Bomarc program benefitted from the earlier Lockheed X-7 effort, for the X-7 had tested the RJ-43 Marquardt ramjets subsequently incorporated on the Bomarc SAM.[8]

John Becker's essay offers a detailed examination of the scramjet field as it pertained to the NACA/NASA and the evolution of the NASA Hypersonic Ramjet Experiment program. As a more general background, it is interesting to consider the work undertaken by other organizations in this area, as well as the climate of thought regarding the field of ramjet propulsion for very high speed flight. Following closely on the heels of research papers by other scramjet enthusiasts beginning in 1958, Antonio Ferri of

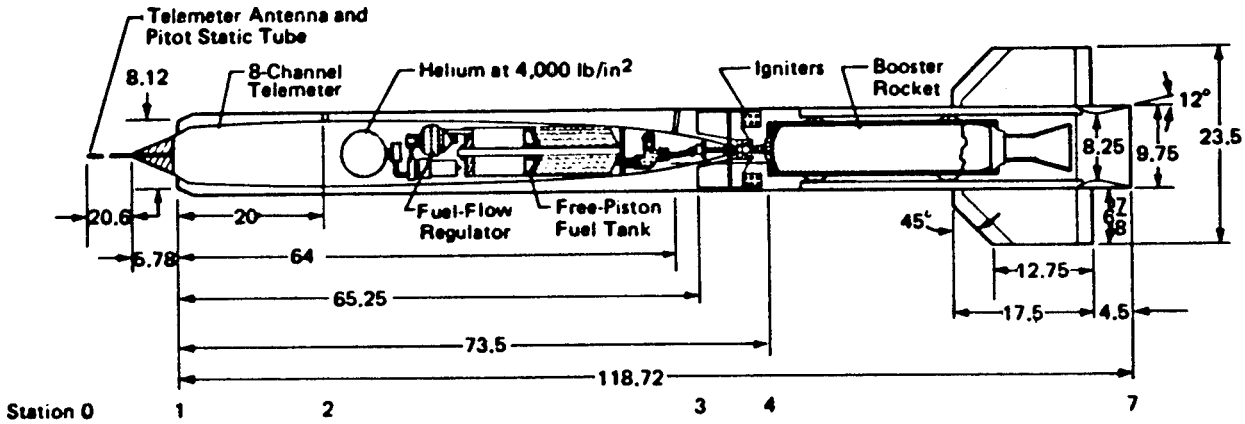
Figure 3



NACA F23 ETHYLENE-FUELED RAMJET

(GROUND-LAUNCHED BY SOLID-FUEL ROCKET BOOSTER)

Figure 4

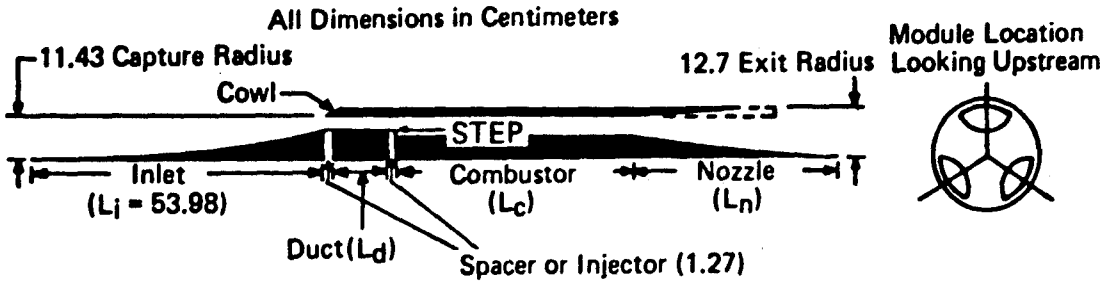


NACA 9.75 in. PENTABORANE-FUELED RAMJET

(AIR-LAUNCHED BY F-82 OR F2H-2 AIRCRAFT)

General Applied Science Laboratories (and the Polytechnic Institute of Brooklyn) had argued forcefully and eloquently for consideration of supersonic combustion ramjets for hypersonic propulsion in a series of meetings and papers delivered in the early 1960s before the aerospace community both within the United States and abroad.[9] The great amount of interest that this noted aerospace scientist stirred-up led researchers within the Air Force, NASA, and the Navy to investigate the scramjet's potential as a propulsion system for hypersonic vehicles. Generally speaking, only the Air Force and NASA envisioned scramjets propelling manned hypersonic vehicles; the Navy's interest lay primarily in the field of scramjets for missile propulsion using storable propellants. Ultimately, after the denouement of the HRE experimental program and the sorry story of the proposed National Hypersonic Flight Research Facility (which is discussed in the next case study, and which would have made use of scramjet propulsion, at least in part), the majority of Air Force interest in scramjets turned, like the Navy, towards missile applications, except for die-hard advocates at the Aero-Propulsion and Flight Dynamics Laboratories at Wright-Patterson AFB, who kept the service's manned-vehicle scramjet applications alive until the onset of the National Aero-Space Plane (NASP) program. Before discussing Air Force work contemporaneous with that of the HRE, it is useful to examine the Navy-sponsored work, undertaken by a team of researchers at the Applied Physics Laboratory of the Johns Hopkins University. Already as early as 1958, APL researchers Fred Billig and G. L. Dugger had achieved net positive thrust during tests of a small scramjet model in a Mach 5 airstream (the model burned an aluminum alkyl fuel). In 1961, Billig and Dugger took out the first scramjet patent, for a proposed naval surface-to-air missile. Beginning in the early 1960s, APL devoted its attention to component development (i.e.: inlets, fuel injectors, and combustors), and in 1968, this work culminated in the initiation of testing of an actual borane-fueled "free-jet" scramjet engine at free-steam Mach numbers of 5.0, 5.8, and 7.0 (Figure 5). The engine featured three separate combustion modules arrayed

Figure 5



Model	L_d	L_c	L_n	Exit Nozzle	A_{ex}/A_{in}*
Taper	0.00	55.88	22.85	15° Conical	1.0000
Step	0.00	55.88	22.85	15° Conical	1.0000
Long-Isolator-Taper	34.29	55.88	22.85	15° Conical	1.0000
Long-Isolator-Step	34.29	55.88	22.85	15° Conical	1.0000
Short-Isolator-Step	8.89	46.36	38.10	Contoured	1.2346

*Model Exit-to-Inlet Area Ratio

Dashed Lines Indicate Conical Nozzle

Taper – Tapered Combustor
Step – Step Combustor

JOHNS HOPKINS APPLIED PHYSICS LABORATORY SCRAMJET TESTBED ENGINE

axisymmetrically at 120 deg. intervals; researchers tested this engine at the Ordnance Aerophysics Laboratory at Daingerfield, Texas at Mach 5 and 5.8 velocities, and at APL's own free-jet facility for the Mach 7 tests. Testing indicated "a number of areas where further development is needed," including structural cooling of the engine, cheaper and less-toxic fuels, and research on fabrication techniques.[10]

In the first quarter of 1965, the Air Force Aero-Propulsion Laboratory at Wright-Patterson Air Force Base established a Scramjet Technology Division following recommendation by an Air Force Systems Command Scramjet Ad Hoc Working Group that the service increase emphasis on ramjet technology hopefully leading to "early exploitation of supersonic combustion ramjets and hypersonic vehicles."[11] To effect greater coordination, NASA and the Air Force formed a joint NASA/Air Force Ad Hoc Working Group co-chaired by Robert Supp of the AFAPL and John Becker of Langley Research Center. In May 1965, this group issued its findings after reviewing progress in scramjet technology and hypersonic vehicle programs, concluding that both the Air Force and NASA recognized the potential of the scramjet and intended to "exploit this concept vigorously," that no undesirable duplication of research effort seemed to exist, that "a major joint goal" should be "a small manned hypersonic scramjet-powered test vehicle" (for test in the 1973-1974 time period), and that future close coordination including joint in-house symposia was highly desirable. The group recognized six key scramjet flight test research programs as forming the core of NASA-USAF research in the 1965-1975 time period including an Air Force incremental scramjet study program (1965-1969); X-15A-2 HRE testing by NASA (1965-1970); Mach 3-Mach 12 scramjet flight testing by the Air Force (1965-1971); an advanced hypersonic cone study by NASA (1965-1968); development and testing of the delta-wing scramjet-powered X-15 by NASA (1965-1970); and, finally, study, design, development, fabrication, and flight testing of a manned hypersonic research vehicle (1966-1975).[12]

Thus, by 1965, the Air Force and NASA's leading experts in the field of scramjet and hypersonic vehicle development had committed themselves to an ambitious program of hypersonic air-breathing research. In fact, precious little of this program ever reached fulfillment, due to the occasional unpredictable meanderings of the R and D process and the budgetary pressures of the Vietnam War. A limited Air Force incremental scramjet flight test program was undertaken (to be discussed subsequently) though no actual "hot" scramjet firings occurred; the X-15A-2 flew only with a dummy HRE installation; the Mach 3-Mach 12 testing did not take place; the delta X-15 fell by the wayside; and, finally, the proposed manned demonstrator lingered on for over a decade until cancellation of the National Hypersonic Flight Research Facility in the late 1970s. (As of 1986, in fact, no scramjet is known to have been tested in actual flight, though Canadian researchers tried twice, with horizontal and near-vertical firings of test engines from modified 16 in. naval cannon in 1968 and 1974; in each case problems unrelated to the scramjet concept prevented a successful flight. That no further test work was undertaken may be seen as a commentary on both the difficulties of this technology as well as, perhaps, a commentary on how the scramjet had declined in glamour vis a vis aerospace research priorities following the heady days of the 1960s).

In February 1966, the Aerospace Vehicles Panel of the Air Force Scientific Advisory Board endorsed comprehensive investigation of the supersonic combustion ramjet concept, stating that:[13]

> It is the feeling of many members of this Panel, that hypersonic aircraft with air-breathing propulsion of some variety will eventually be required for military purposes. The form that this requirement will take is today quite vague but could be useful in the staging of various vehicle systems into earth orbit, or for very fast reacting cruising systems. At the writing of this report no clear application of hypersonic flight has been identified to the point where a strong case can be made for an immediate development of very high cost. The Panel instinctively feels that this will come and therefore the Air Force should continue to carry out fundamental work in the critical areas of structures, flight

dynamics and propulsion for hypersonic vehicles to preserve the option to go this route if a real requirement develops rapidly as it has done so often in the past.

One of the most intriguing new concepts for the propulsion of hypersonic vehicles is the supersonic combustion ramjet (SCRAMJET). This device offers the potential of a new mode of propulsion at high speed and high flight altitudes and appears to be capable of being developed into a propulsion system for hypersonic aircraft with additional future effort.

While early experimental work on the SCRAMJET is encouraging, many problems remain to be solved in such areas as high efficiency nozzles and inlets, aerodynamic heating, structures, materials and efficient combustion over the full range of flight speeds and altitudes. Operation of the SCRAMJET in the regime of M-6.0 to 12.0 appears less difficult than in the regime of M-3.0 to 6.0 or above M-12.0.

The Aerospace Vehicles Panel agrees with the recommendations made by the SAB Ad Hoc Committee on Air-breathing Propulsion "Hypersonic Propulsion Program" (December 1965). Their report recommends a careful analysis be made by the Air Force of the potential usefulness of hypersonic vehicles relative to other Air Force needs and ways of accomplishing its objective. If the conclusion from this analysis is that hypersonic vehicles are likely to be needed, then serious support should be given at a level to insure an acceptable development of the program. In the meantime work should continue vigorously in a continuation of the present background technology of which the SCRAMJET is one element, with structures and flight dynamics of equal importance.

In the years following 1964, the Air Force embarked on a number of hypersonic scramjet engine ground test demonstration and component development programs including a variable geometry Mach 5 18 in. diameter scramjet developed by the United Aircraft Research Laboratory, a General Electric 9 in. diameter Mach 7 component integration model scramjet, a low-speed fixed geometry testbed scramjet developed by General Applied Science Laboratories under the direction of Ferri, a Marquardt 18 in. flightweight hypersonic scramjet, and a Marquardt dual-mode (i.e.: subsonic combustion at lower flight speeds and supersonic combustion at hypersonic speeds) scramjet, the latter achieving dual-mode performance by using fuel injection from different locations within

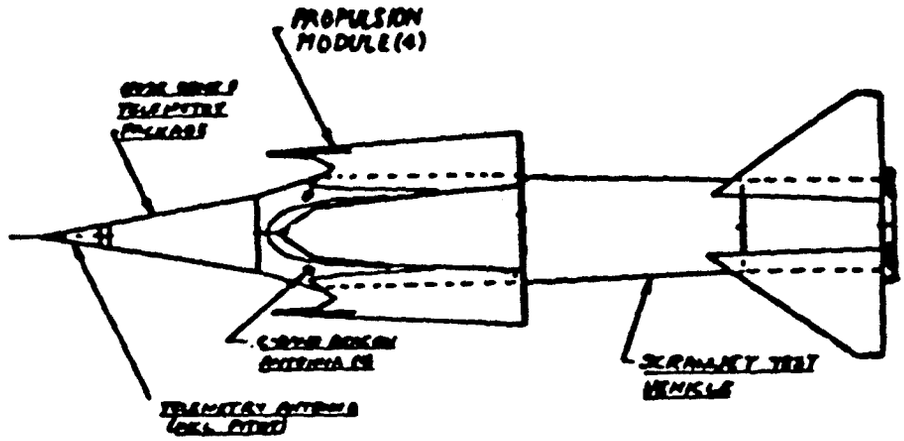
the duct depending on the mode of performance sought. As a summary report on these efforts concluded:[14]

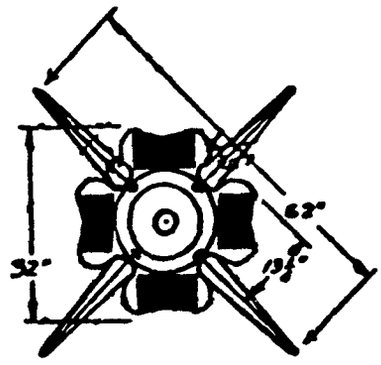
> These engines were hydrogen fueled and the performance levels theoretically predicted were, in general, substantiated. Although many were aerodynamically designed to operate over a wide range of hypersonic speeds, ground testing was restricted to a narrow Mach number range because of facility limitations. Hence, the full potential of these engines was never documented.

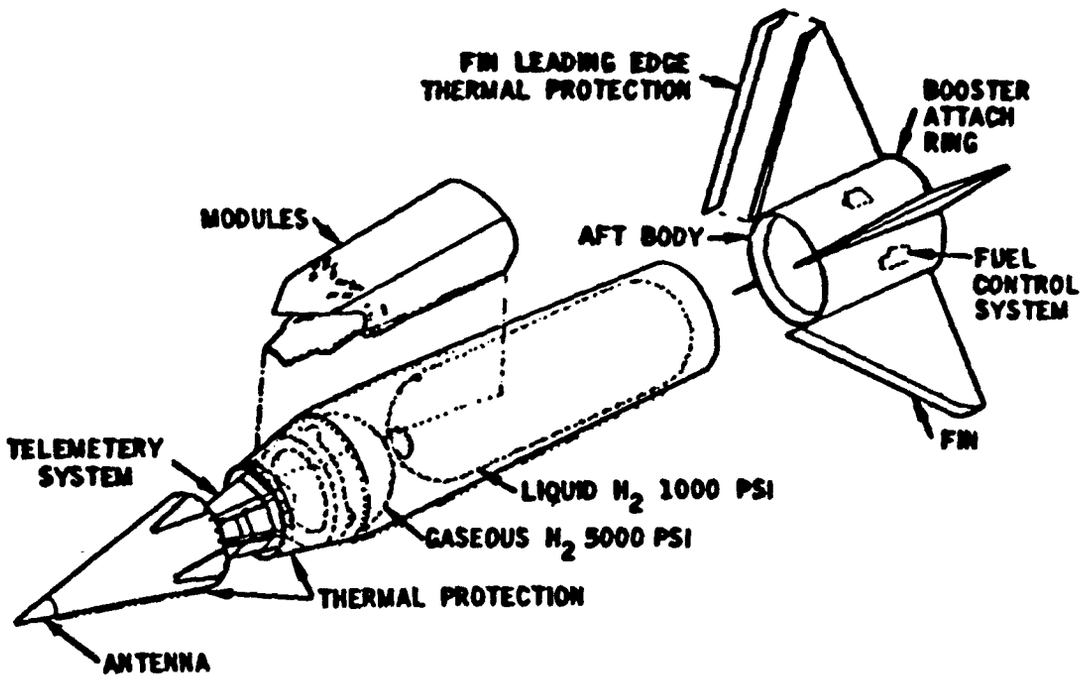
One noteworthy American attempt to generate actual flight test results in a "real world" environment from a scramjet engine was the Air Force's Scramjet Incremental Flight Test Program, an Air Force-Marquardt effort of the mid-1960s under the supervision of the USAF Aero-Propulsion Laboratory. This program effort began in October 1964 and included component testing on the ground followed by a planned comprehensive flight test program involving a dummy non-active scramjet checkout flight followed by three "hot" scramjet launches. The program had the goal of demonstrating positive thrust from a scramjet in flight, and providing useful follow-on data for subsequent scramjet exploitation.

Program managers sought to achieve scramjet operation at flight speeds greater than 6000 ft./sec., an engine burn time in excess of five seconds, and a "delta V" (change in velocity) greater than 600 ft./sec. Marquardt chose to develop a small test vehicle having four axisymmetric hydrogen-fueled scramjet modules (Figure 6). This vehicle, accelerated to separation and scramjet ignition speeds by a Castor solid-propellant rocket booster, would be launched from Vandenberg Air Force Base. Subsequently, Marquardt did a great deal of development work on this program, testing the modular engine units at Mach 5.73 at a simulated altitude of 79,000 feet in the test facilities of the General Applied Science Laboratories. Marquardt determined desirable scramjet ignition conditions to be Mach 5.4 at an altitude of 58,740 feet and a trajectory angle of 45.7 deg., based upon the characteristics of the Castor booster. As might be expected, Marquardt faced serious design challenges in developing the modules themselves,

Figure 6







MARQUARDT SCRAMJET INCREMENTAL FLIGHT TEST VEHICLE

adopting no less than five separate module structural design approaches to overcome anticipated heating problems. The vehicle design itself underwent refinement in AEDC tunnels and in the 4 ft.-by-4 ft. Supersonic Wind Tunnel of the Lockheed-California Company's Rye Canyon Research Laboratory, where engineers evaluated it over a Mach number range of 0.4 to 4.4. Drag reduction became a major concern and occupied the attention of designers virtually continuously from April 1965 through July 1966. Weight grew appreciably, from a 400 lb. target upwards to a revised 600 lb. target, and finally, when the dummy test article flew in 1967, to 680 lbs. On January 11, 1967, a test firing of the dummy vehicle revealed continued drag problems. Higher than estimated drag reduced the desired separation Mach number from 5.4 to 5.18, and lowered the separation altitude by nearly 1000 feet. Postflight data analysis attributed the higher drag to unexpectedly high "spillage" drag from the scramjet module inlets, compared to the results of ground testing. (Drag reduced the test vehicle's estimated apogee from a predicted 228,500 feet to an actual 177,712 feet, with a corresponding reduction in impact distance as well). While Marquardt claimed that ground testing had demonstrated the actual thrust of the scramjet to within 80 percent of its predicted value, clearly much more work remained to be done. Despite the value that continued development testing of the Incremental Test Vehicle might have had, the Air Force canceled any remaining flight tests because of, in the words of the Marquardt summary report,[15]

> a shift in emphasis by the Air Force from hydrogen fuel to the use of storable fuels and concentration on more near-term application objectives, and in part on difficulties encountered in the Scramjet module aerothermodynamic development.

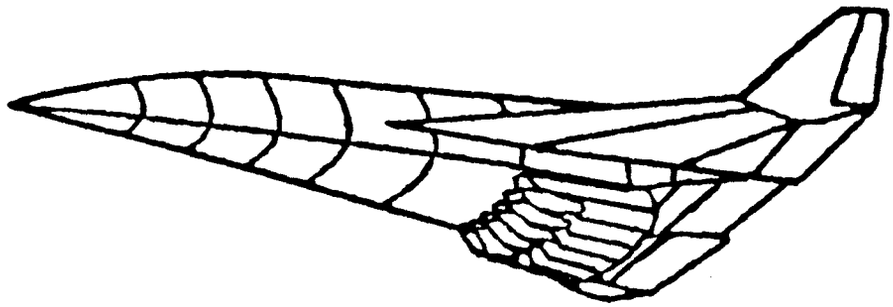
So passed the most ambitious American scramjet program conceived and taken to the hardware stage, with the exception of the NASA Hypersonic Ramjet Experiment itself. Air Force interest shifted to ramjet-powered missiles, adopting the same philosophy as had governed the Navy's work in the field. The scramjet's future for

full-size vehicles belonged to the modular airframe-integrated scramjets of the post-1960s time period. The airframe-integrated scramjet (Figures 7 and 8) makes use of careful integration of the scramjet with the aerodynamic shape of the hypersonic vehicle itself to enhance propulsion system performance. It utilizes the fixed geometry of the aircraft forebody to furnish inlet compression via the undersurface bow shock; an annular inlet running across virtually the entire undersurface furnishes a large enough inlet capture area to generate the required thrust. This inlet area is subdivided into many smaller rectangular units--the actual scramjet modules themselves--utilizing mixed-mode fuel injection for efficient subsonic and supersonic combustion. Exhaust nozzle flow expansion is aided by the fixed geometry of the vehicle's aft underbody surface, which acts as a partial nozzle. This airframe-integrated approach remains attractive to this day, as evidenced by numerous airframe-integrated modular scramjet installations postulated for various hypothetical hypersonic vehicles. Researchers would do well, however, to remember the cautionary words of Robert A. Jones and Paul W. Huber of Langley Research Center's Hypersonic Propulsion Branch, who wrote in 1978 that:[16]

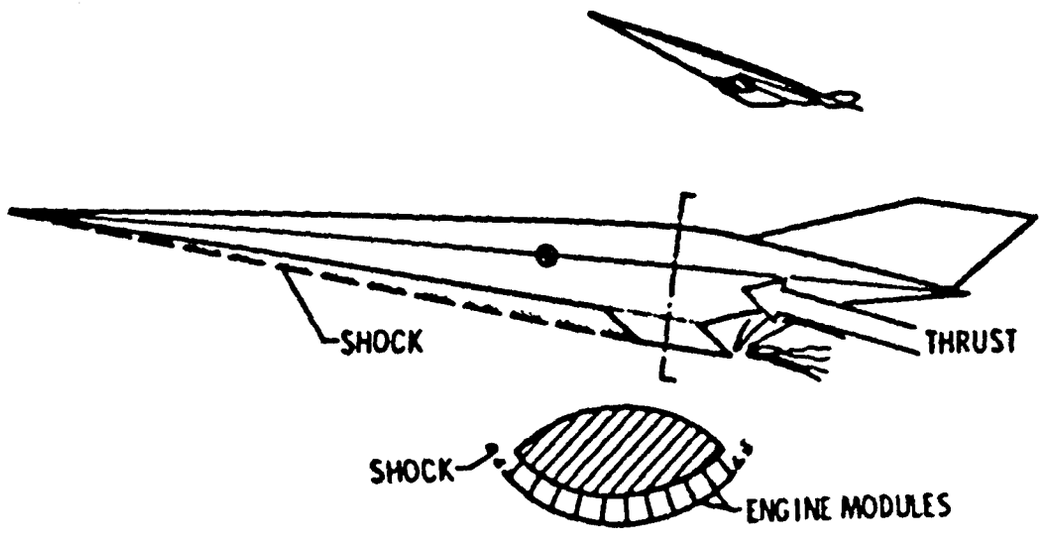
> Unlike the case of turbojets, where performance for the most part could be assessed in ground facilities, the integrated scramjet must undergo hypersonic flight tests to resolve major questions related to its extensive engine/vehicle integration and high-Mach-number operation. When such tests are done, the hypersonic speed range of the scramjet can be exploited.

Until NASP, the NASA Hypersonic Ramjet Experiment constituted the most challenging scramjet development effort undertaken to date, and its story, as stated earlier, draws forth emotions of sympathy and bewilderment in equal portions. It is fortunate that John V. Becker, a key and impartial participant in the whole NASA-Air Force scramjet story, prepared this study, undertaken at the behest of the National Aeronautics and Space Administration's Aeronautical Propulsion Division of the Office of Aeronautics and Space Technology. Though conducted under NASA Contract NAS 1-14250 and completed on July 1, 1976, the study presents the

Figure 7

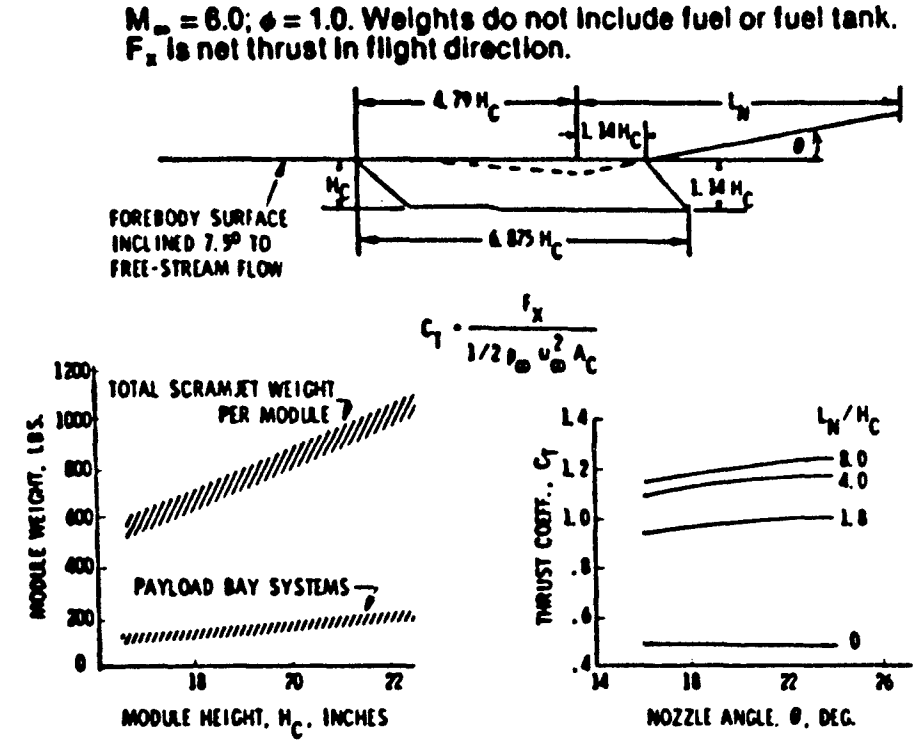


TYPICAL AIRFRAME-INTEGRATED SCRAMJET VEHICLE

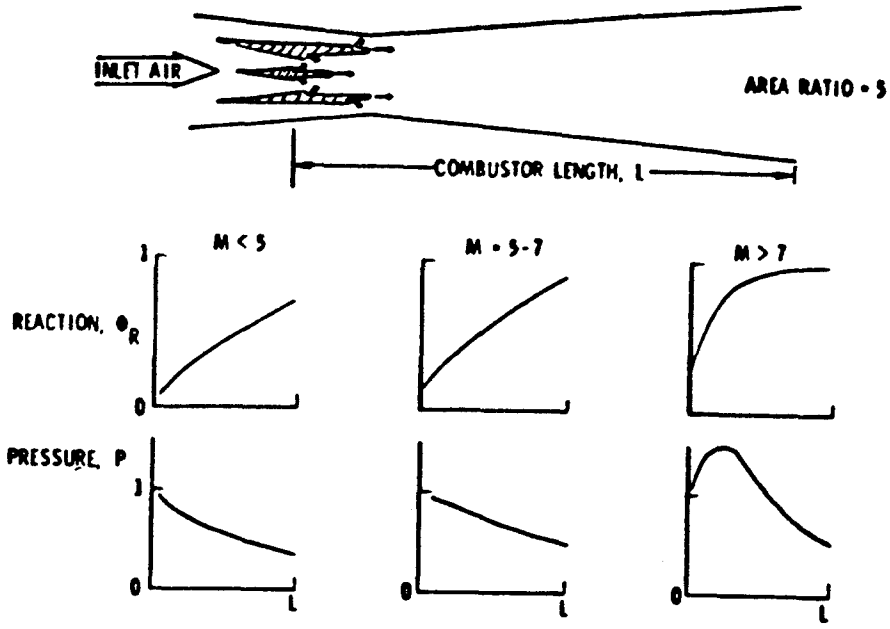


SCRAMJET-VEHICLE INTEGRATION

Figure 8



LANGLEY INTEGRATED SCRAMJET, 1978



COMBUSTOR OPERATION OF THE

LANGLEY INTEGRATED SCRAMJET

NOTES

1. John L. Sloop, Liquid Hydrogen as a Propulsion Fuel, 1945-1959, SP-4404 (Washington, D.C.: NASA, 1978), p. 279. The single best overall survey of ramjet/scramjet development is Paul J. Waltrup's Hypersonic Airbreathing Propulsion: Evolution and Opportunities, Paper No. 12 of Conference Proceedings on the Aerodynamics of Hypersonic Lifting Vehicles (Neully sur Seine, Fr: AGARD, April 1987), pp. 12-1 to 12-29. I wish to thank Dr. Waltrup for making this paper available to me.

2. For Soviet ramjet work, see A. Ya. Shcherbakov, "Flight Testing of Ramjet Engines on Aircraft Designed by N. N. Polikarpov, 1939-1940," Translation FTD-HT-23-1233-68 (Wright-Patterson AFB: AF Foreign Technology Division, 22 August 1969), and related articles within this translation, including Yu. A. Pobedonostsev, "35 Years Since the Creation of Air-Breathing Jet Engine Theory by Academician B. S. Stechkin;" M. M. Bondaryuk, "Applying the Theory of Academician B. S. Stechkin to the Creation of Ramjet Engines;" I. A. Merkulov, "The First Experimental Tests of Ramjet Engines by the GSJP;" V. P. Kaznevskiy, "Launching the World's First Ramjet Engines;" and K. A. Putilov, "Scientific-Experimental Preparation of Ramjet Engine Flight Tests on an Aircraft Designed by A. S. Yakovlev, 1942-1944." See also I. A. Merkulov, "Basic Stages in the Development of the Theory of Ramjet Engines," NASA Conference Publication 2014, Essays on the History of Rocketry and Astronautics: Proceedings of the Third Through Sixth History Symposia of the International Academy of Astronautics (Washington: NASA, 1977).

3. Nazi ramjet work is covered in Paul Spremberg, Entwicklungsgeschichte des Staustrahltriebwerkes (Mainz: Krauskopf Flugwelt Verlag, 1963). See W. Trommsdorff, "High Velocity Free-Flight Ram-jet Units (TR-Missiles)," in AGARD, ed., History of German Guided Missile Development, Proceedings of 1st AGARD Guided Missile Seminar, (Munich: AGARD, April 1955) pp. 352-374. See also W. H. Avery, "Twenty-five Years of Ramjet Development," Jet Propulsion, XXV, n. 11 (November 1955).

4. Grover Heiman, Jet Pioneers (New York: Duell, Sloan, and Pearce, 1983), pp. 195-209.

5. Michael J. H. Taylor, Jane's Pocket Book of Research and Experimental Aircraft (New York: Collier Books, 1976), pp. 125-127, 167.

6. Interview with Macon C. Ellis, 12 November 1971; interview with Clinton E. Brown, 12 May 1971; interview with John P. Stack, 19 May 1971; interview with Floyd L. Thompson, 31 May 1972; Arthur Kantrowitz and Coleman du Pont Donaldson, "Preliminary Investigation of Supersonic Diffusers," NACA ACR L5D20 (May 1945); Macon C. Ellis and Clinton E. Brown, "Analysis of Supersonic Ramjet Performance and Wind-Tunnel Tests of a Possible Supersonic Ram-Jet Airplane Model," NACA ACR L5L12

(December 1945); Macon C. Ellis and Clinton E. Brown, "Proposal of Supersonic Ram-Jet Missiles," NACA Memorandum Report 30 January 1945.

7. Shortal, A New Dimension, pp. 125-126, 166-171; 260-261; 323-326; 403-406.

8. Edmund Beard, Developing the ICBM: A Study in Bureaucratic Politics (New York: Columbia University Press, 1976), pp. 93-105; Frank D. Stull, Robert A. Jones, and William P. Zima, "Propulsion Concepts for High Speed Aircraft," National Aerospace Engineering and Manufacturing Meeting of the Society of Automotive Engineers, Los Angeles, 17-20 November 1975.

9. For example, see the following Ferri papers: "Supersonic Combustion Progress," Astronautics and Aeronautics II, 8 (August 1964); "Review of Problems in Supersonic Combustion," Journal of the Royal Aeronautical Society LXVIII, 645 (September 1964); and "Review of Scramjet Technology," AIAA Journal of Aircraft V, 1 (January 1968). See also Waltrup, passim.

10. Paul J. Waltrup, Griffin Y. Anderson, and Frank D. Stull, "Supersonic Combustion (Scramjet) Engine Development in the United States," 3rd International Symposium on Air-Breathing Engines, Munich, Germany, March 1976; Waltrup, 12-6 and 12-9.

11. AFAPL Scramjet Technology Division Semiannual Historical Report, 1 January-30 June 1965, ASD/HO archives.

12. John V. Becker and Robert E. Supp, "NASA-USAF Ad-Hoc Working Group Hypersonic Scramjet Aircraft Technology," (20-21 May 1965), passim; copy in ASD/HO archives as attachment to AFAPL STD Semiannual Historical Report 1 January-30 June 1965.

13. USAF SAB, Report of the USAF Scientific Advisory Board Aerospace Vehicles Panel (February 1966), pp. 2-3.

14. Stull, Jones, and Zima. See also Waltrup, Anderson, and Stull for an elaboration on the results gained from testing these engines.

15. M. L. Brown and R. L. Maxwell, Scramjet Incremental Flight Test Program, I: Summary, AFAPL-TR-67-112 (The Marquardt Corporation, February 1968), pp. 3-4, and passim.

16. Robert A. Jones and Paul W. Huber, "Toward Scramjet Aircraft: A Technology Report," Astronautics and Aeronautics, XVI, 2 (February 1978) p. 48.

CHAPTER I

ORIGINS AND START-UP

The present practice of many government agencies of closing out large projects with only technical reports, supplemented by self-serving agency press releases and special certificates for the project personnel, relegates to oblivion a complex mix of management and technical experiences and insights which if carefully examined, might be found to hold important guidance for future projects. The present study is, in a sense, an experiment to test whether such hindsight assessments can be truly worthwhile. It also provides a technical history of the project written by a research engineer who participated in many of the key events.

The HRE project was chosen because it offered fertile possibilities for analysis, from both management and technical standpoints. It was a major undertaking of the NASA Aeronautics program involving over 10 years of effort and some $50 million total cost. Although much useful new technology was generated, HRE produced no dramatic achievements. It was a victim of hard times for hypersonic R & D and narrowly missed termination on more than one occasion. Perhaps more can be learned from study of this kind of project than from the happier dramatically successful ones.

Background

The environmental background in which R & D ideas take shape and eventually achieve approved project status is of fundamental importance in a study such as this. In this section, we will review the decade prior to HRE in which the scramjet concept emerged. Section 3 will deal specifically with the formative stages of HRE.

Stimulated by the emergence of the large missile rocket engines after World War II, a few venturesome aeronautical engineers began toying with fascinating new rocket aircraft concepts having speeds and ranges far greater than anything they had previously dreamed of. These were frustrating exercises, however, because the new aircraft would have to fly in the hostile high-temperature hypersonic environment, so far beyond the bounds of 1950 technology that their true feasibility and effectiveness could not be determined. Nevertheless, there was a clear consensus that the apparent potential of these new systems was so great that R & D programs to develop the needed technologies were justified. The X-15 project was the first major national step in this direction.

Although the X-15 was eventually to play a dominant role in HRE, air-breathing propulsion was not considered at all in its conceptual development. At that time, 1954, it was commonly believed that hypersonic air-breathing engines were improbable if not impossible, and that the rocket engine was the only promising candidate. Subsonic-burning ramjets were under extensive development for missiles such as the Boeing IM-99 Bomarc, but there appeared to be fundamental obstacles to their use at hypersonic flight speeds. In Avery's comprehensive survey of ramjets in 1955 (1), he concluded that Mach number 4 was about the highest speed that ramjets could be used. At higher speeds, two problems centering on the very high engine air temperatures appeared insurmountable: the lack of structural materials to cope with combustor design requirements, and the serious energy loss due to dissociation of the propulsive air flow and failure of this plasma to recombine in the nozzle. A supposed third problem, often predicted by piston-engine and jet-engine old-timers throughout the '50s, lay in the very low pressure recoveries inherent in hypersonic inlets. This, however, proved to be an invalid extrapolation of low-speed engine experience. By the latter '50s, it was understood that inlet kinetic energy efficiency, rather

than pressure recovery, was the correct index of hypersonic performance and that high performance was possible in spite of pressure recoveries that would be disastrous at low speeds.

A remarkable change from pessimism to optimism in the outlook for hypersonic air-breathing propulsion occurred in the decade prior to the start of HRE. By 1964, it was widely accepted that the limiting problems feared in the beginning could be circumvented by advanced design. The ramjet using hydrogen fuel and supersonic combustion, in particular, appeared to have the dramatic potential of useful performance throughout the entire speed range up to near-orbital speeds.

This situation did not come about by any single stroke of genius, invention, or prophetic study. It was, rather, the confluence of results from a number of unrelated research efforts. The first important contribution was the series of external burning studies at the NASA-Lewis Research Center based on I. Pinkel's analysis of external burning as a means of lift and maneuverability enhancement for supersonic fighters (2). A basic question arising in these studies was whether combustion in supersonic flow was a practical possibility. Fletcher, Gerstein, Dorsch, and others at Lewis set up a small experiment to find the answer. Fletcher published a report in 1953 (3) revealing a somewhat debatable "confirmation" of supersonic burning at Mach 2.5 and 3.0. Commenting on this early achievement about a decade later, G. S. Bahn of the Marquardt Company stated that a more convincing demonstration was accomplished at Marquardt in 1957 (4, 5). They used parallel injection of gaseous hydrogen and so avoided Fletcher's difficulties which were associated with his use of a vaporizing liquid fuel injected from the wall.

Other exploratory supersonic combustion investigations were conducted in the latter fifties by Experiment Inc., the Applied Physics Laboratory of Johns Hopkins University (6, 7), A. Ferri

and his group at Brooklyn Polytechnic Institute, and others. Documentation of this early work is poor, much of it being in the form of notes and obscure memos. There is, however, general agreement that the feasibility of diffusive combustion in supersonic flow was demonstrated repeatedly during the '55 - '60 time period.

A second unrelated line of development at Lewis that had an important impact was their pioneering use of hydrogen fuel in large ramjets and later in a jet aircraft (8, 9). This successful work did much to encourage the later acceptance of hydrogen as a fuel for hypersonic systems.

The supersonic combustion work at Lewis as applied to external burning inspired R. J. Weber in 1954 to think about the possibility of using supersonic combustion internally, in a ramjet engine (10). Although he doubted that shock-free combustion would be possible in a supersonic combustor, Weber decided to analyze the ideal performance attainable in a ramjet with shock-free heat addition, and with realistic assumptions for inlet, nozzle, and frictional losses. The work had very low priority and proceeded slowly, but the result (11) was the first definitive analytical assessment of scramjet. (The acronym "SCRJ," first used by Weber, had an unpleasant sound and soon gave way to "SCRAMJET." "SCRAM" was first used by the Navy (12) to mean supersonic combustion ramjet missile.) Weber and his collaborator, J. S. Mackay correctly identified all of the exciting features of scramjet -the relaxing of inlet/diffuser requirements, the reduction in combustor heating, the minimizing of the nozzle dissociation problem, the alleviation of variable geometry requirements, and the potential for performance levels higher than any other air-breather at speeds above about Mach 7. The effects of combustor area ratio, thermal compression, and other design parameters were determined for the first time. The next few years saw general confirmation from the studies of McLafferty (13), Dugger (14) and others.

By the time the Weber/Mackay paper was published in 1958, the authors had moved on to other areas of research, believing that there would be little interest in, and few if any applications for scramjets. This was the time of transformation of NACA into NASA. Lewis was in the process of abandoning all work on air-breathing engines in favor of rocket engine development for the burgeoning space program. Weber was thus surprised early in 1959 to receive an invitation from the Air Force to speak at their 2nd Symposium on Advanced Propulsion Concepts as a specialist in supersonic combustion. An entire session under the chairmanship of Weldon Worth had been organized to discuss supersonic combustion ramjets. Worth was then Technical Director for the USAF Aero-Propulsion Laboratory at Wright-Patterson Air Force Base. Early in 1957 he had fostered the beginning of USAF activity in hypersonic propulsion (15, 16, 17). Much of the discussion at the Boston meeting (18) now appears, naturally, to be rather naive; however, it was evident that Worth had already become an advocate for scramjet. Recalling the poor vision of the propulsion industry in this country in the '40s with respect to the turbojet, he advocated an open-minded approach to explore the full potential of scramjet, even though its possible applications were, at that time, obscure (19). Following this philosophy, he was sponsoring a substantial exploratory scramjet program in the Air Force, the first fruits of which were seen at the Boston meeting.

The Space Age with its unprecedented demands for new propulsion provided an enormous stimulus and a justification for scramjet development which had previously been lacking. The most sensational potential application was the Aerospaceplane, the monster air-breathing airplane with which the aircraft industry proposed to take off conventionally, fly into orbit, and return to base in the same manner in which conventional aircraft flight operations were conducted. Shortly after the start of the Mercury program in 1959 the major aircraft companies, expressing distaste

for brute force one-shot rocket systems, became involved in studies of the Aerospaceplane. Republic enlisted the services of A. Ferri and his cohorts at GASL to collaborate on orbital system concepts. Drawing heavily on Ferri's inspirations they became firmly committed to an air-breathing system which used scramjet for primary propulsion from Mach 8 to orbital speed. Competitive concepts using imaginative new schemes such as the "ACE" and "LACE" air collection systems were studied by the other major companies. Within the government a number of exploratory in-house studies were undertaken, one by Worth's team in the Aero Propulsion Laboratory, one by the JHU/APL group, and several smaller studies by various groups at NASA/Langley (20, 21, 22).

The first public airing of hypersonic propulsion and some of its possible applications came at the 4th AGARD Colloquium at Milan in April of 1960. Ferri fired the imaginations of his audience with the prospects of air-breathing engines working all the way to orbit (23). Many of the older men present were politely skeptical. Nonetheless, Ferri established himself at this meeting as the chief prophet of scramjet propulsion.

During the next few years the single-stage-to-orbit systems were examined more closely in a second round of studies with USAF support (24, 25). In all of these studies, it was necessary to assume both highly speculative propulsion and highly speculative lightweight structures. Reflecting the conclusions already reached by most of the contractors and government analysts, the USAF Scientific Advisory Board at its meeting in July 1962 recommended that work on single stage systems be deemphasized and that a coordinated effort on the less speculative 2-stage systems be pursued (26). It is clear now, after the passage of some 14 years, that the recommendation was a sound one. Using the same scramjet performance assumptions as before (now known to be much too optimistic), Republic evolved an all-air-breathing concept in which the second stage was scram-powered from Mach 8 to

orbit (27). They also proposed a scram-powered multi-purpose military machine, hydrogen-fueled and capable of semi-global cruise range (28). To develop and demonstrate hydrogen scramjets, Republic proposed a small research airplane to be developed and ready for flight in 26 months. The other companies studied various 2-stage versions of their earlier concepts, and a 2-stage refuelling concept was evolved by Douglas (29). Several promising scram missiles were also under study during this period (12).

Thus, by the time the HRE concept evolved in late '63, there was strong justification, on the basis of multiple potential applications, to pursue a hypersonic air-breathing engine R & D program. Following typical government practice in such matters, USAF and NASA R & D managers set up an ad hoc team in the summer of '62 charged to define a 2-year program of technology development in the critical areas of hypersonic air-breathing systems so that a national decision could be made in 1964 whether or not to proceed with an air-breathing orbital launch vehicle (30). This exercise is noteworthy as the first occasion on which the working-level of R & D professionals of the two agencies were brought together in large numbers to consider a coordinated national program in hypersonic technology. As they can be relied upon to do in such circumstances, the "troops" came up with a long list of "urgent" tasks to be pursued "vigorously" - provided of course substantial additional funds, amounting to $20 million and additional manpower amounting to 167 positions were made available (30). In the propulsion area no mention was made of research engines; hypersonic inlets appears to have been the only subject considered for flight testing, using one-shot rocket models. The resources for this joint program never materialized and it quickly disappeared from the scene.

During the 1959-1963 period when the application studies were in full swing considerable progress was made in scramjet component technology. Through fiscal year '62 a total of some $10 million

had been invested (31) in the USAF and NAVY programs (including the system studies). The question of feasibility of supersonic combustion had been answered affirmatively and no "concept-killer" obstacles had been encountered. Equally important was the appearance of a rapidly growing cult of ardent scramjet enthusiasts of which Ferri was the chief spokesman and Worth the chief benefactor. Avery, Dugger, and Billig in the Navy-supported program were also strong advocates.

It is obvious now that the readiness technology to support a flight-weight research engine development was greatly overestimated in 1964, at least in part because of the optimism engendered by the scramjet enthusiasts. A careful reading of some of the principal scramjet papers of the period is quite revealing in this regard (31, 32, 33, 34). In his Lanchester lecture of 1964 (33), Ferri concludes that the understanding of mixing-controlled combustion had reached the stage of "complete confidence" for application in engine design. He cited test facilities and structures as remaining important problems. However, in an obscure answer to a question at the close of the lecture, Ferri admitted that "actual" supersonic combustor flows were "too complex to be calculated," and his conclusion of "complete confidence" applied only to highly simplified flow models. Unfortunately, this major qualification was lost in the persuasive optimism of the lecture. Swithenbank's over-optimistic 1964 summary (34) states that "scramjets are passing into the development stage" and lists no less than 19 institutions said to be involved, 5 of whom are said to be "testing complete engine models."

In 1964, the Air Force conducted one of its periodic summer studies of advanced technology opportunities referred to as "Project Forecast." Largely through the efforts of Worth and Ferri, scramjet was recommended as an area meriting special emphasis. Consequently, General Schriever who then headed the USAF Systems Command, established a special Task Force to examine

scramjet technology and potential in detail. The routine work of this exercise devolved upon a "working group" comprised largely of scramjet specialists from Worth's shop at Wright Field. They were aided by presentations from all of the companies involved in scramjet studies or related technology programs. By that time some $22 million had been spent on scramjet and the working groups were able to show a substantial assemblage of data and study results. The following remarkable pronouncements, taken from their report (32) reflect their general optimism:

> "No foreseen (combustor) problems . . . are unsolvable by engineering methods."
>
> "Supersonic combustion can be mixing-controlled to Mach 25."
>
> "Successful future research is forecast to provide a sufficiently large body of information for actual combustor design."
>
> "Solution of the structural problems rests in the excellence of future design ingenuity."

The Task Force envisioned a focused high priority national program to develop a mature scramjet design technology. In his letter thanking them for their services, General Schriever noted especially their great enthusiasm.

Behind the scenes in 1964 were skeptics including both those who sincerely doubted that the technical problems of scramjet could ever be solved, and old timers in the engine business who knew by experience and intuition that the bringing forth of a complex new type of propulsion system would be many times more difficult, more time consuming, and more costly than the eager proponents of scramjet expected. Little was heard from the

skeptics; they were unwilling to buck the USAF hierarcy which had obviously decided to embark on a crusade. A few cases came to light in the interviews in which certain of the skeptics had been specifically advised to remain quiet.

In the decade prior to HRE, then, several supposed barriers to hypersonic air-breathing propulsion were progressively toppled. The enormous demands of the Space Age stimulated the development of air-breathing space launch vehicle concepts employing ramjets or scramjets for primary propulsion at speeds ranging from about Mach 4 to orbital speed.

By the early '60s, a cult of enthusiasts had developed with Ferri as the persuasive principal spokesman. Although actual solid progress in scramjet technology was limited, the field was rich in unproven new ideas and tentative potentialities. The effects of this environment of inflated technical optimism on HRE will be apparent in the succeeding sections.

Origins, Promotions, and Approvals

The origination of large government R & D projects is rarely a logical orderly process and HRE was no exception. The initial motivation appeared in the industrial firm which built the X-15. North American Aviation was interested in the new business that might come its way through extensions of the X-15 program into new areas. And thus it was that E. W. Johnston of North American visited the NASA and USAF Centers in May of 1962 with a proposal to modify an X-15 so that it could serve as a flying test bed for hypersonic engines (35). North American's collective imagination of what might profitably be done in this new field was uninhibited, ranging from a large variety of captive test engines to X-15-launched nonrecoverable test engine modules. There was little evidence of interest in these proposals at the time.

A few months later, however, an event occurred which changed the picture. The number two X-15 was extensively damaged in a

crash landing in November 1962, and this gave North American a unique opportunity to reiterate their proposal to modify the airplane for propulsion testing - the modification to be made in conjunction with the necessary repairs to number two. In order to reach speeds high enough for scramjet tests, they proposed to add dropable external propellant tanks and to coat the exterior of the X-15 with an ablator to permit flights to Mach 8. A fuselage extension to accommodate a liquid hydrogen fuel tank would also be made. The total cost of the revisions and the repairs would be in excess of $6 million.

General support for the plan was found in the Air Force, which was willing to foot the bill. NASA managers were opposed to the modifications (36) preferring only to repair the crash damage. Balch and Loftin, who had just completed the joint USAF/NASA hypersonic program study referred to previously, (30), both regarded Mach 8 X-15 capability of limited value for propulsion research (37). Nevertheless, NASA decided not to press its objections, and work on the modifications was started. Thus the prospect in early 1963 was that a Mach 8 carrier vehicle would shortly be available for propulsion testing - but the propulsion test objects themselves were completely undefined.

To correct this illogical situation, the NASA Flight Research Center at Edwards quickly launched a study aimed at determining what type of engine testing to pursue on the X-15. Recognizing that the expertise for monitoring such a study lay chiefly in the other Centers, they circulated a letter soliciting comments on the RFP for the proposed study (38). No support was forthcoming at any of the other NASA Centers. On the contrary, K. F. Rubert (who later became HRE Project Manager) expressed his opposition to flight programs, stating "this would be an unwise expenditure of government funds" . . . "(engine R & D) is better done on the ground" . . . "there is no genuine need (for flight tests)" (39). Under the circumstances, FRC obviously had no alternative but to

press on in spite of such comments. They did, however, shift the emphasis to a "feasibility and preliminary design study" of the most desirable engine test system, which was to be selected after brief initial screening of the several possibilities. In defiance of their critics, FRC stated firmly that they planned "to take an active role in advanced air-breathing propulsion and the X-15 should prove very useful in this regard" (40). Marquardt was selected to perform the 4-month study, and it was no surprise that they chose three ramjets as being the most desirable test engines, a subsonic-combustion, a supersonic-combustion, and a convertible design. To reduce costs to a minimum, they proposed to equip the engines with simple structures insulated within and without by ablative material adequate for the X-15's Mach 8 heat pulse (41).

None of the NASA reviewers outside of FRC believed it justifiable to proceed with the Marquardt engines. They pointed out that the emphasis on low cost would result in overly simplified designs. In particular, the use of ablative materials would produce contaminants which might strongly affect combustion. In discussing the problem with Rubert just prior to his departure for the final briefing of the study at FRC, J. V. Becker who then headed the Aero Physics Division at Langley, suggested that Rubert offer an alternative test plan which would be worthwhile. Rubert then described in general terms a concept very close to what later became the HRE - a sophisticated dual-mode engine, thoroughly researched on the ground and utilizing a clean internal metallic structure, possibly regeneratively cooled.

At the conclusion of the Marquardt briefing, D. Wall, who was in charge of the X-15 research engine activities, called an informal meeting of the NASA Center reviewers to discuss the next move. The outside Center representatives were unanimously aligned against the Marquardt engines. Rubert stated his views on what would be acceptable to Langley, emphasizing the need for a much enhanced ground program as a necessary precursor to a flight

spectacular. Wall argued convincingly that financial support for an extensive scramjet program in NASA was quite unlikely unless it was tied to an X-15 engine as the end objective. Some of the Center people regarded this end objective as really unnecessary, but all agreed that it was probably the only device by which a meaningful scramjet effort could be started.

A formidable obstacle was recognized in the almost certain opposition to such a plan of Lewis management. Not only was Lewis traditionally unsympathetic to research airplanes, but also they had recently abandoned air-breathing engine research almost in toto. Therefore, it was decided to propose the plan as an FRC/Langley project, with Langley managing the ground development phase and FRC the flight phase (42).

Bikle endorsed this scheme and presented it verbally to NASA Headquarters who agreed to take it up with Langley management. In Bikle's proposal he specifically suggested Rubert for Project Manager. Langley Director Thompson and his assistants, Donlan and Loftin, were understandably surprised. Loftin, of course, had recently recommended against such a project (37). Thus, the first top-level Langley reactions were negative. In the Langley Aero Physics Division in which Rubert's Branch was located, there was a quite different reaction. For years, the propulsion-related fluid mechanics and hypersonic inlet/diffuser work of this Division had suffered from a dearth of real-life applications. Therefore, the prospect of total involvement with a real engine for X-15 testing offered an exciting infusion of much needed vitality. Another argument that was appealing to Langley managers was the prospect that an HRE project would reveal whether the many paper performance claims for scramjet were really valid. Still another important consideration was the complete lack in 1964 of ground engine test facilities for true-temperature simulation with clean air at Mach numbers above 5. The X-15 was thus viewed as a unique test facility which should be exploited. Importuned with these arguments, Langley management agreed to support the HRE project.

Of the various arguments for the project, the need for engine testing in the true environment of hypersonic flight appeared to be most important to N. Rekos, who was responsible for propulsion programs in the Aeronautics Division of OART. He was concerned that the only large hypersonic facility in existence at that time, the Navy Daingerfield facility, was capable of only Mach 5 true temperatures with clean air. Mach 7 to 8 could be simulated only with combustion-heated air containing large quantities of water vapor. Various schemes for correcting this situation had been suggested, but all had their shortcomings. Rekos thus considered the X-15 as a unique engine-test facility, and he became a principal Headquarters advocate for the HRE program primarily for this reason.

Rubert was directed to formulate a specific proposal for agency discussion and approval. He tackled the job with great energy and enthusiasm. If the project actually materialized, it would be his most important assignment since coming to NACA 23 years earlier. In short order, a preliminary proposal document was completed (43), containing the following ambitious statement of objectives for a 3-phase program:

Phase I

1. To define a practical, high-performance, Mach 3-to-8 hypersonic ramjet engine, of laboratory size.

2. To design, develop, and build this engine.

Phase II

3. To measure the performance of and to qualify this engine in the laboratory.

Phase III

4. To measure the performance of this real engine in maneuvering flight, from Mach 3 to 8.

5. To validate the technology of certain ground-based hypersonic air-breathing engine test procedures; for example, the use of vitiated air in combustion experiments.

6. To advance and crystallize the technology of hypersonic air-breathing propulsion systems, and to reevaluate for more effective planning the requirements for future research.

Objective 6 was obviously a general "motherhood" type goal which applied throughout the project.

Significantly, the proposal document assumed that the reasons for interest in scramjet were so well known that they need not be discussed. In regard to the state of the art it was stated flatly that the "gaps" in component technology "had been filled" leaving only uncertainties "which can be discovered and resolved only by design and construction of a truly practical research engine." No one at Langley challenged these claims, which, by hindsight, provide evidence of the inflated technical confidence level that existed then. Rubert was Langley's best authority on scramjet, having spent several years studying hypersonic propulsion as a special assignment, and no one was inclined to question him. Neither were the estimates in the prospectus of the time allotted to the program (4 years, total) or the cost ($30.4 million plus operations costs of the X-15) delved into in any detail. It was left to the Phase I study to provide exact figures later, but the document implied that the estimates were undoubtedly of the correct magnitudes.

In April and May of 1964, the proposal sailed through its approval briefings for OART and top NASA management with no opposition and few questions asked. It had the important advantage of being a joint venture of two Centers. Behind the scenes Director Floyd Thompson of Langley, in his capacity as chairman of a headquarters committee charged with recommending agency policy regarding powered lifting systems, had provided important advocacy for

the project. Dr. Hugh L. Dryden, Deputy Administrator and the senior approving official, was personally inclined favorably towards research engines, having made an early suggestion in 1949 that they deserved consideration (44). Dryden had also previously approved the modifications of the second X-15 for hypersonic engine testing. Dr. Robert Seamans was also favorably disposed; he had been under pressure to enhance aeronautics funding relative to space and HRE provided an opportunity. Seamans asked why Lewis was not involved, and when told he said, "Okay, but what does Abe Silverstein have to say about HRE?" Silverstein, who was then Director of Lewis, was called by telephone. Although he had some doubts about HRE, he said he would not oppose it and that Lewis would provide consultation. Dryden and Seamans thereupon approved the entire project in principle. Release of funds for Phase I was approved on June 13, 1964.

Project proposal presentations at that time were usually carefully rehearsed all-out advocacy propositions containing all of the positive arguments and no unrefuted negative arguments. Vital considerations negative to the proposal were often omitted entirely even though they were a source of much worry to the proposers. NASA managers were of course aware of these tactics and when they neglected to look for the hidden problems, they were simply expressing their general approval and willingness in this case, to accept the plans of subordinate managers. Perhaps they considered the project too small to devote any further time to it. After all, $30 million was only half of 1 percent of the upcoming NASA budget. A more likely explanation lies in the fact that the X-15 program in early 1964 was at the zenith of its success. By association this new proposal of the X-15 team had high credibility and seemed to offer continued exploitation of a proven technique. It should also be noted that HRE was the first major scramjet development and was by far the largest developmental project in the propulsion field ever undertaken by OART/Aeronautics up to that time; the lack of an objective detailed inquiry at the outset may be charged in part to this inexperience.

In the remainder of this section, we will consider some of the important issues either never brought to light or never considered in meaningful detail in the HRE approval process. Most basic perhaps is the fact that no assessment was made of alternative less costly and possibly more cost-effective methods of advancing scramjet technology - improved ground facilities, expanded wind-tunnel testing of subscale engines, rocket models, future flight vehicles, etc. Actually the very high cost of X-15-related projects was not really a serious factor in the eyes of the Center engineers who made the proposal. NASA personnel at that level in the mid-sixties had the attitude that all research is expensive, especially flight research. The only really meaningful consideration of costs was what the system would tolerate. Thirty-million dollars could not be handled in a ground research program but as an add-on to a glamorous three hundred million dollar flight vehicle program it looked feasible.

Equally difficult to justify by hindsight was the absence of any coordination with the established USAF and Navy programs. The HRE proposal disposed of this question by saying coordination would be initiated if the proposal were approved. In fairness to the proposers it should be remembered that up to that period NASA personnel had rarely been required to coordinate projects of this kind with other agencies before their approval. Each agency acted independently and only after large established efforts showed signs of duplication or overlap might an attempt be made to "coordinate."

As a matter of fact, concurrent with the birth of HRE, USAF was bringing forth a competitive rocket-boosted scramjet engine program, the first inkling of which came to NASA on September 18, 1964 in the form of a copy of USAF Technical Development Plan 651E (45). Their plan was to boost a series of "incremental" scramjet engines to progressively increasing speeds starting at about Mach 5 and eventually reaching about

Mach 9 (46). Simplified low-cost versions of the ablation-protected engines Marquardt had proposed in their NASA study were to be the test objects. Rubert adopted the convenient view that these "quick and dirty" tests would supplement rather than duplicate the "more sophisticated" HRE. It is interesting that the Schriever Task Force had nothing whatever to say specifically about either the HRE or the Incremental projects, even though they were briefed on them in late 1964. They did say that flight projects should await the realization from ground programs of a firm technology base - advice which was ignored by both agencies as they forged ahead with both flight programs.

Finally, no one asked how Langley proposed to compensate for its lack of experience in engine hardware development and testing. Were experienced consultants from the engine industry to be used on a regular basis? Were Lewis personnel who had been intimately involved in Bomarc, for example, to have a place on the team? What steps had been taken to confirm the time and cost estimates with engine specialists in this field? What was the success/failure ratio in test flights of past programs using captive test engines? Had FRC ever achieved 25 successful flights of the X-15 in 12 months let alone 25 flights with a complex research engine with many unsuspected problems and low reliability?

In sum, then, the idea of captive hypersonic engine testing on an X-15, originating with North American Aviation, was at first rejected by agency researchers and managers who favored ground-based approaches. They finally became convinced, however, that the only way to achieve a substantially funded scramjet technology program in NASA was to focus on the goal of a glamorous X-15 flight engine. Other persuasive arguments included: consideration of the X-15 as a unique engine test "facility" not subject to the various shortcomings of the "hot" ground facilities of the mid-sixties, the need for "real" engine performance data to validate paper performance claims, and a desire of Center researchers

for the stimulus and educational values of clost association with a flight engine development.

The specific HRE proposal in 1964 - to conceive, design, develop, qualify, and test a sophisticated engine in transient flight conditions on the X-15 up to Mach 8--all in the short time of four years for NASA costs of only about $30 million--is seen by hindsight by a majority of the interviewees to have been naive and premature. It reflected both the general overestimation of technology readiness that prevailed at that time and the lack of actual engine hardware development and test experience of the Langley team.

Each management level of review and approval of the proposal seems to have been willing to assume that the previous level had done its homework, the result being that no comprehensive searching assessment was ever made. Issues of major importance that received only cursory scrutiny include: comparative cost and effectiveness of other approaches, coordination with the long-established USAF and Navy programs, technology readiness, realism of the very short time schedules and low costs relative to previous engine developments, the need for consultation with experienced engine developers, and major problems of extension of X-15 operations and funding.

If these issues had been thoroughly explored and costs relative to other approaches had been weighed seriously, it seems unlikely that the proposal would have been approved, even in the relaxed funding atmosphere of 1964.

Concept and Contractor Selection

In the promotional literature of the HRE (43), the test engine was described in general terms as a "truly practical complete engine," a "real" engine, a "typical" engine, the testing of which would provide "solid," "factual," or "realistic" performance data

said to be needed by decision makers for operational systems. If all of the hopeful claims of the original prospectus could have been realized, this single engine project would have advanced the technology from the early exploratory research stage to full readiness for hypersonic aircraft systems applications. An elaborate subscale prototype engine would obviously be required to fulfill such claims. Phase I was intended to establish in detail the concept and the preliminary design of an engine which could accomplish these ambitious objectives, exact schedules and costs, and at the same time provide the basis for selection of the contractor who would build the engine in Phase II.

The type of engine actually called for in the Phase I Statement of Work differed significantly from the "truly practical" complete engine described in the prospectus. The SOW called instead for the "best possible research engine" (47). That most important of all subsystems in a "real" hypersonic engine, the critically difficult high-temperature regeneratively cooled structure, was not a principal problem area to be considered for this research engine, nor was the equally critical problem of external drag. The objective, said the SOW, was "to deal primarily with the aerothermodynamic (internal flow) problems." Structure and external features were to be "refined only to the extent necessary for compatibility" with the internal flow performance requirements. The only definite specification for the structure at this stage was that a heat-sink, cooled, or other nonablating type should be employed to avoid contamination of the propulsive gases.

This downgrading of the engine concept from the advanced prototype inferred in the prospectus to the aerothermodynamic boilerplate research engine called for in the SOW was never openly acknowledged or discussed in the HRE documentation. Project personnel now say they realized that funds were too limited to support a prototype and had no alternative but to pursue the more

limited approach. Throughout the entire life of HRE, unfortunately, the engine continued to be spoken of as though it really was some sort of prototype. For reasons never made clear, the performance test model AIM, for example, was always inferred in the project writings to be somehow much more "realistic" and significant than the several other boilerplate performance models successfully developed and tested by other investigators.

In spite of the absence of a requirement in the SOW, a flight-type structure was developed to meet X-15 weight limitations. It is ironic that the successful development and test of this structure is now generally regarded as the most important contribution of HRE. In other respects, the plan called for in the SOW followed closely the 18-inch diameter pod-type engine installation suggested in the Marquardt study (41).

The achievement of very high internal performance levels was stated to be the primary goal, and the SOW contained a chart showing both minimum acceptable levels of internal specific impulse and thrust coefficient and "goal" levels (Fig. 1). The lower bound was said to be the limit below which scramjet engine performance would be of "no interest." The upper bound coincided approximately with the levels assumed in some of the early system studies. It was apparently believed possible of attainment for highly refined "point design" engines, although several years later HRE personnel began to call this goal the "liars' curve" (48). A different, more realistic "goal" curve has been used by USAF (Fig. 1) and neither of the arbitrary HRE limiting curves are now considered to have any real significance. The "engine data" points shown on Fig. 1 were calculated from combustor test data existing in 1964 together with assumed performance for inlet and nozzle, except for the "GASL engine" point which was for the only complete (but rudimentary) aerothermodynamic Mach 7 engine available in 1964. The SOW urged contractors to strive towards the "goal," although it acknowledged that some reduction would be unavoidable in a design capable of Mach 4-8 operation.

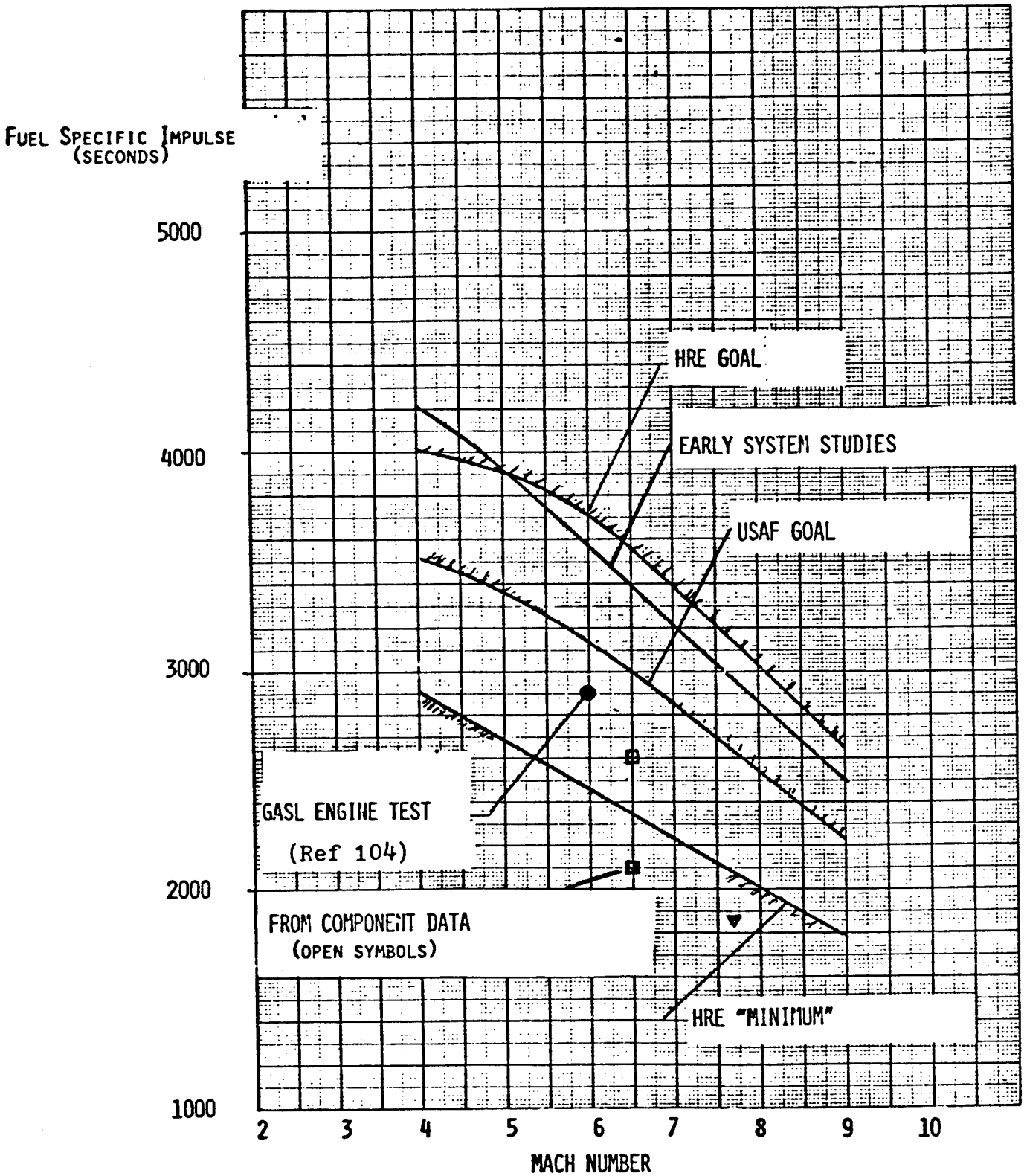


FIG. 1 - SPECIFIED HRE PERFORMANCE BOUNDARIES AND COMPARISON WITH AVAILABLE DATA AND OTHER SCRAMJET PREDICTIONS OF 1964-65.

The decision to seek the highest possible internal performance and to impose no thrust-minus-drag requirement had several unfortunate consequences. In order to comply, the contractor who eventually won the Phase I competition used a higher-than-optimum degree of external compression which caused high external cowl drag. In his final design the cowl was thickened to house equipment, still further aggravating the drag, to the point where the thrust-minus-drag of the engine at Mach 6 for stoichiometric burning was near zero. A secondary effect was to reduce the height on the inlet annulus at Mach 8 to only about 1/2-inch, almost half of which was filled with boundary layer from the spike. The near-zero net thrust characteristics came to light later in the program causing a loss of interest and support for HRE at Lewis and OART. Langley interest also shifted to other concepts which offered very low external drag losses (50). It was difficult to reconcile the final high-drag HRE design, heavily slanted as it was to favor internal performance, with the claims that it was a "truly practical complete engine."

Five tasks were defined in the Phase I SOW (51):

- Develop concept
- Make preliminary design
- Determine performance, life, weight, safety
- Provide substantiation
- Provide development plan, manufacturing plan, costs, and management plan for Phase II

The RFP for this 9-month, 27,500 man-hours study was mailed to 35 companies. Only four responded - General Electric (G. E.), Pratt and Whitney (P & W), Marquardt, and Garrett - three of whom were to be chosen to conduct parallel studies.

The choice was obvious from the start because only three of the contractors had made a serious effort to compete. It was

never understood why the fourth (P & W, Florida Division) had submitted his proposal. Nevertheless, a full-scale source evaluation exercise was conducted involving some 50 professional engineers and researchers from 4 Centers, who were tied up for some 4 to 5 weeks. The makeup of the Source Evaluation Board was wholly NASA personnel. The supporting committees and working panels were similarly constituted, with only one notable exception, Frank D. Stull from the USAF Aero-Propulsion Laboratory. To the large majority, scramjet was a subject in which they would get their first significant exposure in this evaluation. Only a very few had ever participated directly in the actual development of any type of jet engine, except through inlet aerodynamic tests or the like. There was little or no concern among the NASA people about this weakness. They would do the best they could, but neither individually nor as an agency had they much to lose whatever the outcome, provided all of the government rules and substantiating paperwork requirements were observed.

Actually, although the evaluation literature does not say so, the responses to the Phase I RFP were disappointing to many of the NASA team. Langley had no preconceived ideas, but it had hoped that "America's crack design teams," as they are often called in the press, would come up with some exciting, fresh ideas for the "best possible" research engine. Three of the designs were simple, axisymmetric pods. The fourth was a much more complex design of Ferri's incorporating his "thermal compression" scheme. No lack of inventiveness in this one, but unfortunately it was essentially a copy of the concept Ferri was developing in the Air Force program.

Five months after receipt of the proposals on May 28, 1964, fixed price contracts for the three Phase I studies were awarded. Nine months later, the same small army of evaluators reconvened, this time with three large stacks of substantial engineering documents to study. The three concepts remained basically the same as before (Fig. 2). The evaluation took seven weeks and before it

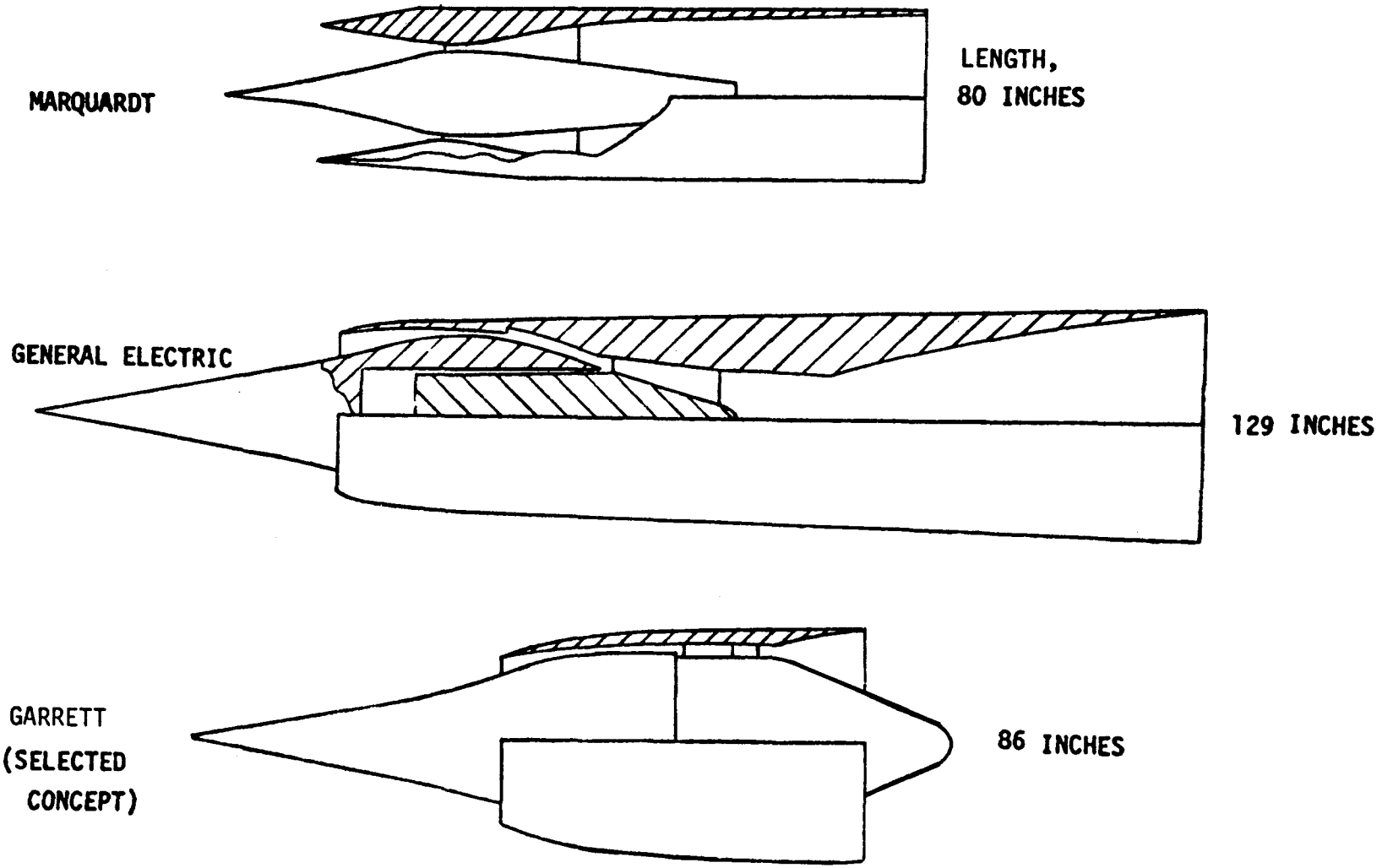


FIG. 2 - HRE CONCEPTS. (MARQUARDT NOT AXISYMMETRIC)

was over there was some grumbling from the out-of-town participants.

The outcome of the evaluation (52) seemed to validate the rough time and cost estimates made prior to Phase I. It so strongly favored Garrett that there could be little doubt that Administrator Webb would have to concur. Phase II with Garrett as contractor was officially approved on July 11, 1966.

With benefit of hindsight it is possible now to see some flaws in the deliberations which led to Garrett's selection. I would emphasize, however, that the judgments made were honest and probably the best ones that could have been made, given the particular background and preconditioning of the NASA troops.

First, let us examine why Garrett was such a heavy favorite. Their engine (Fig. 2) was the smallest, simplest, easiest to cool, and had the best structural approach of the three designs. Added to these indisputable credits was a judgment credit that it had very high research potential because of the quasi-2-dimensional nature of the flow in its shallow annular combustor. It was believed this would simplify analysis of the combustor data and make it applicable to any future engine employing quasi-2-dimensional flow - for example, combustors of rectangular cross section. On this latter consideration, G. E.'s engine was penalized because its combustor annulus was judged so deep that 2-dimensionality could not be assumed. Their engine was also large, heavy, and hard to cool (Fig. 2). G. E. was penalized further because of their proposed long development schedule and estimated high costs - roughly double the government's cost guideline. We now know that the G. E. estimates were much more realistic than either of the others and in retrospect should have been awarded credits.

There was another powerful consideration in Garrett's favor. Under the leadership of A. duPont, Garret had exhibited energy and

zeal unmatched by the others. Drawing on $250,000 company funds they had built in 60 days a full-scale HRE combustor model, and later had operated it successfully in the Daingerfield Facility at simulated Mach 6 conditions (53). This suggested to NASA that Garrett's engine would be easy to design, quick and inexpensive to develop, and that the team was aggressive and competent.

The Marquardt (Ferri) engine was downgraded because of its complex 3-dimensional flows and general lack of substantiation of the claims made for it, including the thermal compression effect used to avoid variable geometry. Its engineering design also appeared inferior to the others.

Three specific flaws in the evaluation can now be cited:

1. The supposedly simple "2-dimensional" flow with wide future applicability for which the Garrett engine received high credit was illusory. The thick wall boundary layers, focused shocks, and resulting separations and complex interactions in the combustor made the actual flow virtually unanalyzable, unique to this particular engine and undesirable from all standpoints (See the section on test results). The generally better performance eventually obtained with the G. E. combustor (54, 55) suggests that it should have rated at least equal with Garrett in research value. It is interesting to note that in the oral presentations made by the contractors during Phase I, Ferri called attention to the problems of the axisymmetric designs, including "focused shocks," "high losses," and "high cowl drag" - the difficulties which actually plagued the selected design. The evaluators unfortunately dismissed these comments as prejudiced.

2. A major flaw in the logic of the Phase I study was the belief that it would reveal true time and costs in a situation where the contractors had been given the government's guideline estimates of what the time and cost should be. Furthermore, the two contractors who agreed with the guidelines were credited with

"responsiveness." The one who correctly but unpopularly estimated large increases was penalized.

3. The direct-connected combustor model developed and tested during Phase I for which Garrett received special credits was actually very unfortunate in two respects. First, it created a false impression that the combustor could easily be made to work. There is now a strong indication that part of the observed pressure rise formerly credited to heat release in this early combustor test may have been due to separation (49). In any event the actual combustor development proved to be the primary problem of HRE. Second, this exercise gave a completely false indication of time and cost requirements. Garrett now blames their misleading experience with this model for their gross under-estimates of the true time and costs for the actual engine.

Regardless of the foregoing questions about specific items of the evaluation, many of the interviewees believe that a single factor, Garrett's previous lack of experience in scramjet aerothermodynamics was sufficient reason for disqualification. They were doubly critical of the combination of an inexperienced contractor and an inexperienced management group. Several believe that the unhappy record of the HRE combustor development (to be discussed later) was directly caused by this dual weakness. Actually the question of Garrett's qualifications to do the job had come up in 1965 before the Phase I study contractors had been selected. Garrett was in fact interrogated on this matter by Langley personnel and judged "to understand" scramjet problems (51). The fact that Garrett was then proceeding at their own expense with a combustor model was cited, apparently with the inference that this actual experience would provide acceptable qualification.

It was also suggested in the interviews that a major exploratory undertaking of the scope and complexity of HRE demanded the

best national talents in each of its problem areas; a single principal contractor choosing one or two subcontractors for business reasons left much of the experienced prime talent of the country unused. They believe the project should have been designed from the outset for broad industry participation throughout the development phase.

As can be seen, then, the concept called for in the Phase I SOW was an aerothermodynamic (internal performance) boilerplate research model, much more limited in scope than the advanced prototype engine which would have been required to fulfill all the objectives and claims of the original prospectus. This discrepancy between what HRE was advertised to be and what it really was continued to cause confusion and misunderstandings throughout the life of the project.

The three particular design concepts developed in Phase I were generally disappointing to many of the NASA evaluators. Two were simple axisymmetric pod-type engines. The third was more sophisticated, but conceptually it was the same thermal-compression modular type already being pursued in the USAF program, and it utilized complex flow fields which would have required lengthy development.

The government evaluation team was inexperienced in both scramjet technology and in jet engine development, however they were well intentioned, well organized, and did the best they could under the circumstances. A substantial consensus of those interviewed was found to exist on the following points of weakness in the evaluation which are now evident in retrospect: failure to make contractor experience in ramjets or scramjets a major item of the evaluation; the assignment of high credits for "research value" to Garrett's shallow annular combustor simply because its geometry favored 2-dimensional flow, without any demerits for such critical difficulties in the actual flow in such a design as

reflecting strong shocks, interactions of shocks and thick boundary layers, major flow separation effects, and effects of fuel injection superimposed on the above phenomena; and penalizing as "unresponsive" the contractor who determined that the actual costs and time to complete Phase II would be much greater than the government projections.

Belated Start and Stretchout of Phase II; Termination of the X-15

When NASA Administrator James Webb approved HRE/Phase II with Garrett as contractor on July 11, 1966, he had every reason to believe that it could be accomplished according to plan. However, almost immediately after the start of negotiations with the contractor, the hard realities, which up to this point apparently no one had ferreted out, began to emerge: modifications and government supplied equipment (GSE) for the X-15 would cost a staggering $8.7 million, millions more than expected. The contractor could already see other large overruns; and the Project Manager himself, only six weeks after the start of Phase II, proposed a whopping $10 million increase, half of which was to be a contingency fund for the overruns which he believed were inevitable in an R & D program (56). These shocking new demands struck NASA headquarters at a very inopportune time of national belt-tightening forced by the mounting costs of the war in Viet Nam. It was now, at last, mandatory that NASA managers start to ask the hard questions and face up to the enormous difficulties and real costs of developing a complex new engine for X-15 flight research. To make matters worse, a new set of top managers generally not favorably disposed to the X-15/HRE plan, had replaced Dryden, Bisplinghoff, and Zimmerman. Mac Adams, a ballistic missile specialist from AVCO, had taken over from Bisplinghoff as head of OART. Immediately below Adams and heading the Aeronautics program was Charles W. Harper, a former Ames division head.

Another serious blow was struck when DOD indicated its intention to discontinue its funding of the X-15 program. Since 1955, USAF had picked up the tab for operations, vehicle alterations, repair, and maintenance. NASA had provided only instrumentation, experiment packages, and a large technical staff which worked with USAF staffs in flight planning, flying the airplanes, and analyzing and reporting the results. But now, in an action endorsed by Secretary McNamara on August 6, 1965, the USAF funding plan to upgrade the X-15 to achieve Mach 8 for HRE testing was disapproved with the suggestion that NASA should take over X-15 funding. The AACB (Aeronautics/Astronautics Coordinating Board) was asked to resolve the matter (57). The Aeronautics Panel of the AACB took up the question of extended X-15 operations and their funding early in 1966. C. W. Harper made a good case (58) for continuation of USAF funding of the X-15 on the grounds that both the HRE and Delta Wing projects were of great value to USAF as well as to NASA and were actually now formally agreed upon parts of the joint national program organized in May of 1965 by the Becker/Supp USAF/NASA team (46). A meeting of the Aeronautics Panel was held in the Pentagon on June 13, 1966, to review this matter. Presentations of the joint program were made by Becker, Supp, and Clodfelter (of ASD). The HRE Project and the Delta Wing proposal were the principal future uses of the X-15 after 1968 although a number of lesser experiments were also advocated. After brief discussion, the Aero Panel endorsed these programs and recommended to AACB that a proper cost sharing formula be arrived at later (59). The AACB, however, rejected this plan and decided in its meeting of July 5, 1966, that NASA should assume total responsibility for all X-15 costs on January 1, 1968 (60).

The withdrawal of USAF support for extending the X-15 program, coming at the outset of Phase II, was a serious setback because it meant that the NASA/OART Aeronautics Budget would have to be increased by at least some $8 million per year, an enormous increase in the funding level for hypersonic technology, and it

was by no means certain that NASA top management would actually accept the burden of promoting such an increase. At the same time there was another more subtle adverse development on the USAF side - the gradual drying up of USAF R & D funding and management support for hypersonic technology in general and scramjet in particular. General Schriever had been unsuccessful in his attempts to obtain the ambitious funding initially planned for the 1965-1968 USAF scramjet program (on the order of $50 million (46)).

Losing both the funds and its principal top management advocate, the USAF scramjet activity quickly started to disintegrate. What turned out to be the final meeting of the USAF/NASA Working Group was held at Langley on September 21-22, 1966, to review progress in the technology programs (60). Upwards of 100 persons attended the meeting. Ostensibly, the program was booming; but privately, Supp confided to Becker that he was quite pessimistic. A major funding problem had developed in the rocket boosted "Incremental" scramjet flight program mentioned previously (45, 46). Originally projected to cost $3.5 million, this program having been modified extensively to obtain more elaborate data, was now estimated at some $12 million. Furthermore, the Marquardt/GASL engine for these flight tests was proving much more difficult and costly to develop that originally anticipated by USAF. This engine was similar to the one rejected by NASA in Phase I.

A review of the joint program was held by Adams and General M. C. Demler of AFSC in January 1967, shortly before the actual start of Phase II work. Neither agency was candid about its problems; Becker and Supp tried to accent the positive, but there was a generally gloomy air about the meeting. Adams asked which flight program, HRE or Incremental, was the more cost effective. He was given the glib non-answer that the programs were designed for different purposes and complemented rather than

duplicated one another. Afterwards, Becker felt obliged to write Adams a full explanation, pointing out that Langley and HRE personnel did not really believe in the Incremental program, that it was far more risky and less cost effective in terms of data points per dollar than HRE, and that it was beset by cost increases and developmental problems (61).

Coming at a time when OART funding for FY69 and '70 was limited because of the economic problems created by Viet Nam, the projected huge increases in cost and loss of USAF support for HRE/X-15 flights raised large questions of how to proceed with Phase II. The question of whether to proceed seems not to have been raised at all. That question was considered settled when the contractor had been selected. In the mid-sixties cost increases and other difficulties seemed to be normal features of this type of R & D program, solved by going back to the public till and stretching out the time as necessary.

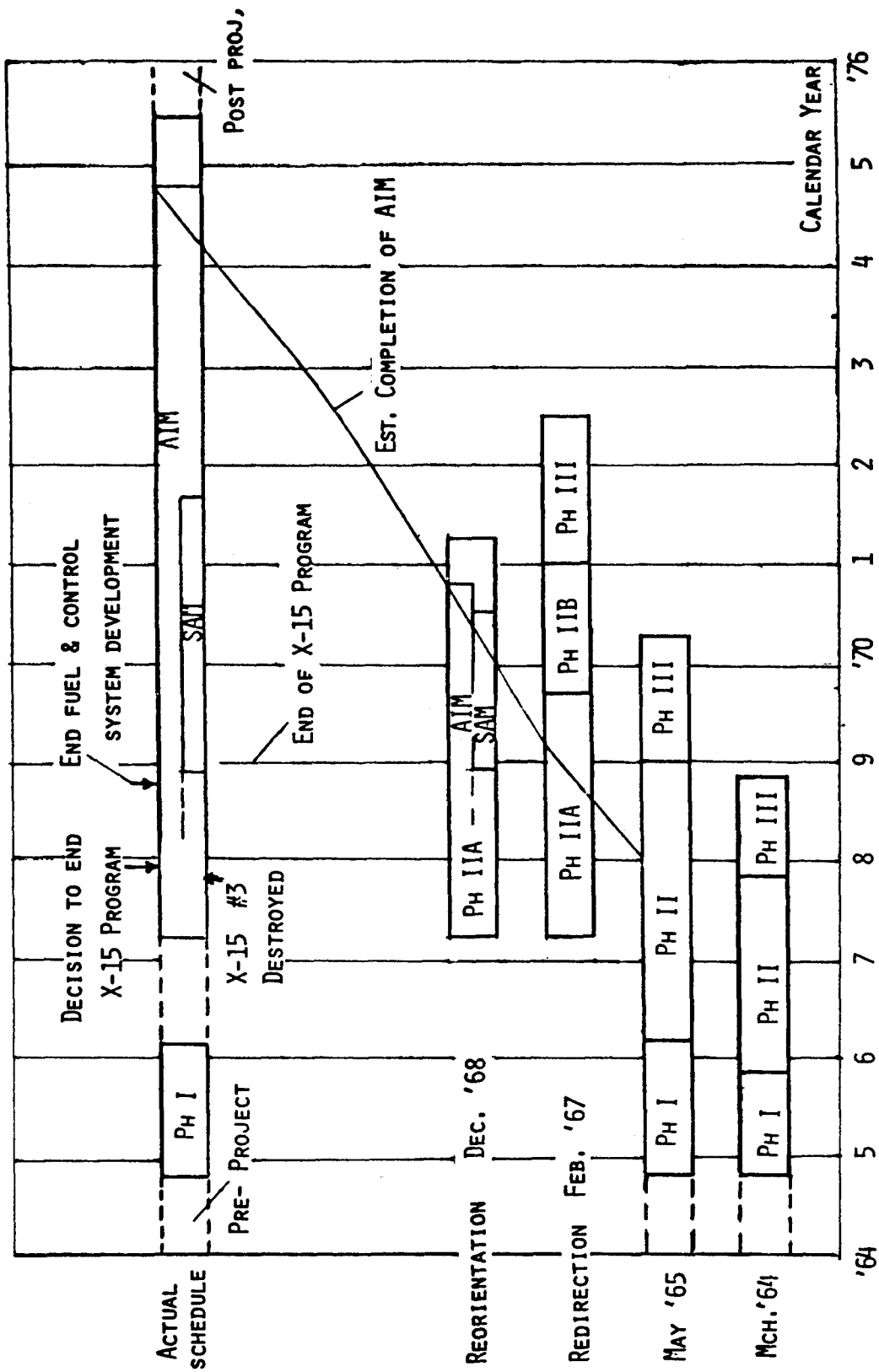
The decision to initiate Phase II was slow in coming in spite of periodic pressure to expedite matters by A. J. Evans and N. Rekos of OART and L. Loftin and K. F. Rubert of Langley. Langley had moved ahead in September with formal establishment of a project office with Rubert designated as Project Manager, B. C. Deis as Assistant for Engineering, and E. A. Mackley as Assistant for Aerothermodynamics (62). By year's end some 12 persons had been assigned to HREPO.

The next part in this classical bureaucratic drama was played by H. Wetzel, President of Garrett, who was politely impatient at the delay in getting started: In a letter to Mr. Webb in December he threatened to invoke "pre-project costs" under a provision of the proposed contract if delays should continue (63). This seemed to have the desired effect because early in January, a letter came down from Adams summarizing OART's stretchout plan (64). Phase II was to be replaced by Phase IIA which would cover development and

one "Pre-prototype" flight weight engine and consume approximately the same time as previously planned for Phase II. Phase IIB would follow and would produce six prototype engines for ground tests, qualification for flight, and later flight tests on the X-15 in Phase III. Only Phase IIA was approved for implementation at a target cost of $15.6 million including fee. Phase IIB was estimated at $13 million, but no estimates for Phase III were made, and nothing was said about the added costs due to NASA's assumption of X-15 operations funding.

Some of the implications of this stretchout can be seen from Figure 3, which shows how the HRE schedule evolved from the four year project plan of the original prospectus (bottom of Fig. 3) to the final actual 11-year history (top of Fig. 3). The original plan was in reality little more than a Parkinsonian compression of the project into the time remaining in the X-15 program before its scheduled termination in 1968. This schedule was actually shorter than Marquardt's preliminary estimate for their much simpler X-15 engine (40). By the start of the Phase I studies the schedule had already slipped 15 months solely because of the addition of realistic times for evaluation, source selection, and negotiation. However, there was little concern about this slippage at the time (May 1965) because it was beginning to be assumed that the X-15 schedule could be extended beyond 1968 as necessary. A proposal to equip one of the X-15's with a delta wing was being promoted in 1965, providing added hope that the program would be extended. The Phase IIA plan proposed in late 1966 contemplated a two and a half year stretchout beyond 1968 which now assumed much graver implications because NASA would have to provide the extended X-15 operations.

FIG. 3 - HRE SCHEDULE EVOLUTION



Faced with the long stretchout, enormously increased costs, loss of USAF support, waning interest in hypersonic technology generally, and the prospect of austere funding for R & D in the years ahead, NASA managers privately saw no real hope in late 1966 for completion of HRE as planned. They considered it unlikely that the necessary costly extensions of the X-15 program under NASA funding could ever be successfully promoted. None of the documentation of the period explicitly states these pessimistic views, but the interviews revealed that this unhappy outlook was shared by all of the NASA officials involved, from Rubert on up the line through Harper and Adams. Most of the USAF and Industry personnel interviewed had similar pessimistic outlooks at that time.

Why was the project not abandoned outright in view of these gloomy prospects? A majority of those interviewed on this point believe that HRE should have been terminated on the basis of the facts at hand in late 1966. The general rationalization of the NASA managers, however, seems to have been that useful scramjet technology would be developed in Phase IIA in any case, regardless of the final fate of the project, and since no one could say with absolute certainty what the future of the X-15 might be, the project was allowed to continue in the stretchout mode.

In situations of this kind where managers are under conflicting pressures to save a costly troubled project from extinction on the one hand and to reduce R & D costs on the other, one often finds considerable obfuscation in their correspondence. Thus we read in the Headquarters letter establishing Phase IIA (64) that the stretchout is consistent with FY69 and '70 agency funding policies. No mention is made, however, of the enormous prospective added costs of the stretchout; instead attention is called to several possible (but in reality unlikely) "cost savings." These included: The use of the proposed Lewis Plumbrook facility for

engine testing rather than the existing Navy OAL facility (no mention of Plumbrook's development and construction costs); and "savings" due to accomplishing X-15 modifications by Garrett rather than by North American (in retrospect extremely questionable). The letter even suggested that overall HRE costs might be reduced by the new plan.

Thus, although both the plan and the outlook for completion were now drastically different than the HRE Phase II package approved by the Administrator in July, the general tone of this letter to the Administrator's top assistant was optimistic, and the really important new facts of the case were not brought into the open. There was therefore little apparent reason for the Administrator to reconsider his decision, and Phase IIA was finally launched on February 3, 1967, with the signing of the contract with Garrett.

In the entire seven month period of delay and revision from July 1966 to February 1967 the most basic questions of all were never asked by any NASA official as far as I have been able to determine: "With prospective program costs now at least double the July 1966 estimate, is an X-15 flight research engine the most cost-effective way to advance scramjet technology?", and "Is continued pursuit of such a program still justifiable in view of the now almost certain prospect of X-15 termination in 1968?" With these questions unanswered and without any discernible logic other than that "technology will be developed," the project shifted into high gear and forged ahead for some ten months, when as expected, NASA decided definitely to abandon the X-15 program in 1968 when USAF support would dry up.

It will prove worthwhile at this point to discuss the termination of the X-15 and its impact in some detail. Much of the information was obtained from the interviews and is not well documented. Paul F. Bikle, Director of Flight Research Center,

had long believed that any extended operation of the X-15 program beyond achievement of its original objectives was unwise and hard to justify in view of the high cost and risk involved. As early as 1961 he had put forward this view and suggested 1964 as a desirable termination date (65). When the X-15-2 airplane was seriously damaged in a landing accident in 1962, he had argued on these grounds for minimal repairs rather than modifications for propulsion testing (36). As time went on he felt that continued extensions were becoming harder to justify, and he personally had strong doubts that either the Delta Wing or the HRE would ever reach flight status on an X-15. In spite of these personal misgivings Bikle went along with the party line, supporting the X-15 extensions in his public statements (66). C. W. Harper had also continued to work on behalf of the X-15/HRE plan. Following a good deal of staff work with USAF and DOD he had succeeded by mid 1967 in obtaining a new Memo of Understanding between NASA and DOD to supplant the previous USAF/NASA/Navy Memo of Agreement which had stood since 1955 as a guide to X-15 operations (67). The new memo reestablished USAF responsibility for X-15 operations costs, and specific other responsibilities of the two agencies were spelled out in detail. All of the agreements, however, were subject to the ominous proviso, "funds permitting." To most NASA managers this meant that NASA would still have to face up to total funding of X-15 operations.

Harper and Adams in the fall of 1967 made what was in effect a last ditch effort to find funds for extending the X-15 program. They solicited the help of the NASA Office of Manned Spaceflight on the grounds that both the HRE and the Delta Wing projects would produce new technology for an advanced Space Shuttle. The attempt failed, however, because OMSF was in trouble promoting the present rocket shuttle concepts and did not want to add to their problems by supporting an attractive-sounding alternative. Ink was hardly dry on the new agreement when a tragic accident occurred which quickly eliminated any remaining question of an extension of the

X-15 program. In November the third X-15 crashed killing its pilot. Although the program could have been extended indefinitely with the two remaining aircraft, this accident underscored Bikle's views on the risks and high costs of program extensions. In the discussions following the accident Bikle also convincingly speculated on the enormous costs of a real HRE flight program involving years of delay in getting started, malfunctions, repairs, modifications, etc. Both NASA and USAF decided in December of 1967 to cancel all plans for extending the program beyond 1968.*

Because it had long been anticipated, the actual demise of the X-15 part of the program seems to have caused only minor distress in the Project Office. They suggested that the X-15 had served a very useful purpose by imposing "real" design requirements for the engine, and they surmised that some "90 percent" of the program objectives would be obtained without Phase III. By this time about half of the Phase IIA Contract funds had been firmly committed and virtually all of this large investment would be lost if the project were terminated. In addition, there would be sizable other costs associated with closing the project. These were uncertain, but Langley's speculative estimates suggested that a major part of the remaining uncommitted contract funds might also be sacrificed. It was this unhappy prospect that convinced Rekos and Evans that HRE should be continued as a ground-based program. OART therefore hastily altered its budget presentations for OMB and Congress in January of 1968 to show HRE testing at Lewis (Plumbrook); loss of the X-15 flight test phase was not discussed.

Rubert journeyed to Garrett early in 1968 to institute work stoppages on the X-15 modifications and other items relating to flight testing. These turned out to be relatively minor, amounting to only about $300 thousand (68). It is important to note, however, that work on the flight engine subsystems including the

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*See Hallion, On The Frontier, p. 125, for further details.

structures, fuel, and control systems was not stopped. The rationale here was that these systems should be completed to achieve a "realistic" engine capable of flight testing even though it was now destined only for the wind tunnels. And thus was HRE adroitly decoupled from the X-15 which gave it birth and left to make its own way, apparently unchanged but actually now stripped of its glamour and its principal reason to exist.

And so it was that early in the negotiations for the Phase II contract the specious nature of the "confirmations" of HRE costs and schedules in Phase I became apparent, and the hard realities, both technical and fiscal, started to emerge. Faced with the dilemma of burgeoning costs in a time of restricted R & D funds, OART decided on a stretched out schedule to meet funding rate limitations, then estimated to add some two and a half years to the procurement time. Although the contractor's estimates and other data showed conclusively that the new schedule would eventually add an enormous increment to project costs, the OART document setting up the changed plan spoke only of dubious areas of "saving." Privately NASA managers now held little hope that the X-15/HRE flight phase would ever occur, but nevertheless, they permitted Phase IIA to start with X-15 flights still the objective. Ten months later the X-15 was definitely marked for termination in 1968 and HRE was then skillfully and quickly decoupled from the flight program with no change in the engine itself. It was argued that the X-15 had served usefully by imposing realistic design requirements on HRE and that "90 percent" of the objectives could now be realized by wind tunnel tests of the engine.

CHAPTER II

DEVELOPMENT AND TESTING

Reorientation

HRE had survived the loss of the X-15 flight phase by only a few months when mounting cost and schedule overruns and other problems in Phase IIA forced abandonment of the original plan to develop and test a complete hydrogen burning engine. Notification of the first overrun ($2 million) had been made in November 1967 (69), and even before this was processed Garrett notified NASA of a second ($3 million) overrun. Both were additional to the cost and time increases that had been negotiated when Phase IIA was started. Confronted with the discouraging prospect of raising at least $5 million more in a time of fiscal austerity for an emasculated ex-flight project, OART was forced at long last in the summer of 1968 to call for a comprehensive review of HRE and discussion of its future course.

Significantly, and following past practice in such situations, OART and Center managers placed the responsibility for critical review and analysis of alternatives in the hands of the HRE Project Office. No attempt was made to obtain the services or opinions of specialists free from emotional ties to the project. Thus it behooved Rubert, Mackley, and Deis to come up with justifications so strong that the project could continue with increased funding, or to define alternative plans of reduced scope and cost. They spent the better part of three months evolving the following possibilities:

Outright Cancellation. Project Offices are not likely to recommend their own liquidation and the HRE group rejected this out of hand--because, they said, virtually the entire contractual

investment up to that time (about $10 million) would be lost with little to show as useful accomplishment.

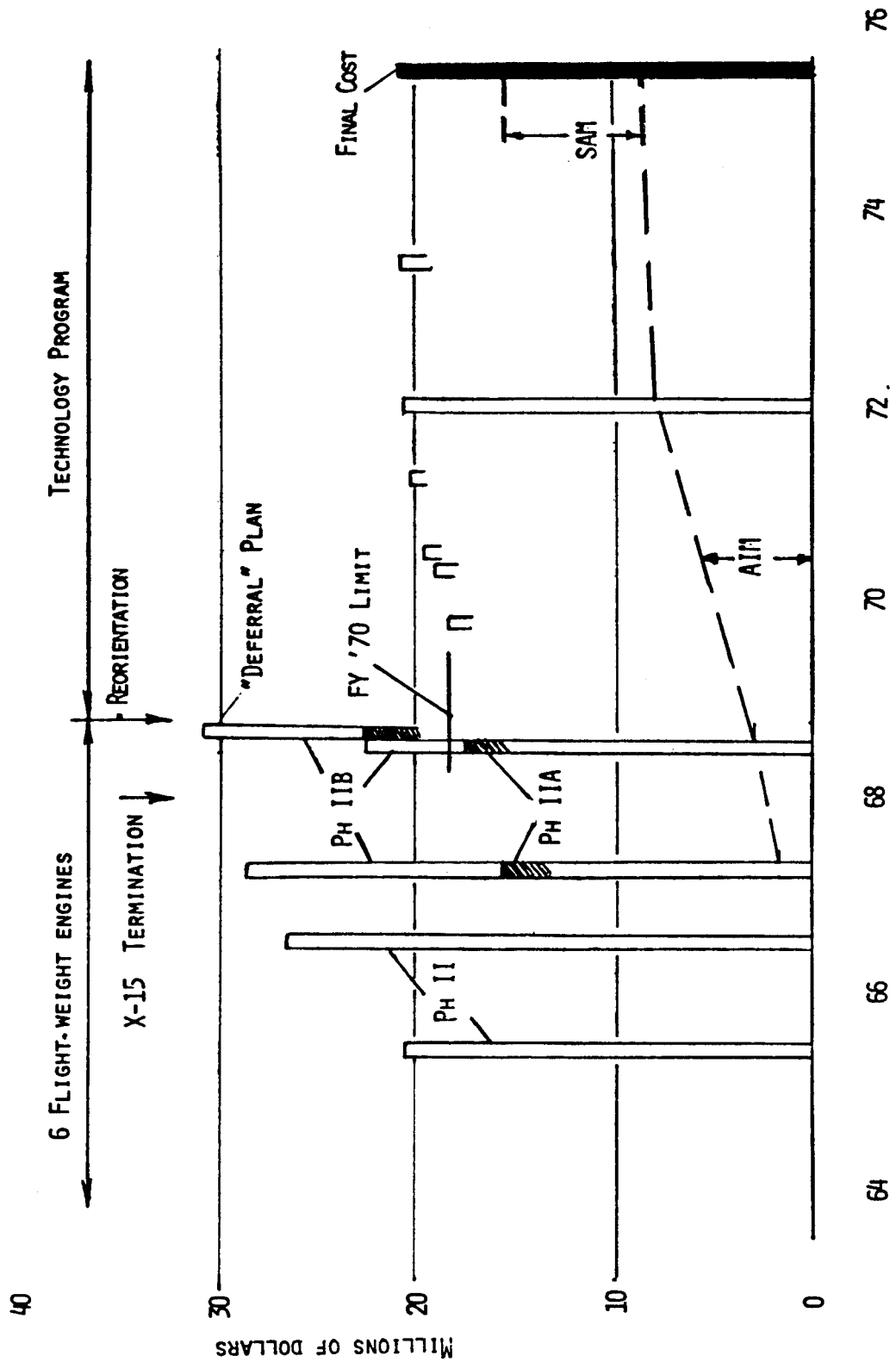
Reduced Scope to Limited Technology Objectives with no Funding Increase. This was also vigorously rejected. It would mean, they said, attaining aerothermodynamic test data only from "the boilerplate engine, in which the true thermodynamic environment cannot be properly simulated. This is the fundamental weakness of all other (previous) hypersonic propulsion investigations."(70). Staying within the approved 18.1 million contract limit would mean, they said, testing only the boilerplate test model plus two selected structural components. (This turned out to be a gross underestimate of what was actually possible, explainable only in part by their assumption that some additional funds would be expended to conclude the subsystem development.)

Expenditure Deferral Plan. This was the plan desired by the Project Office. Although its title diplomatically implied only a stretchout (to 1972) the plan also called for an additional $12.6 million to cover all the then anticipated cost increases. As seen in Figure 4, in this plan the Phase IIA costs were increased as needed, and Phase IIB costs were reduced by reducing the number of engines to three.

A More Limited Deferral Plan. This was similar to the above except that the test engines would be built with the Phase I contours, saving in tooling and development costs about $3.7 million. Loftin favored this plan but Rubert was much opposed to it because it might mean somewhat poorer internal thrust performance than for the Phase IIA design.

Ignoring the loss of the X-15 flights as a thing of the past the HRE advocates spoke easily of "achieving all objectives" with the expenditure deferral plan. Nor was the unhappy contrast mentioned between the actual achievement by mid 1968 (Phase IIA about 30 percent complete) and the original plan in which half of

FIG. 4 - PHASE II CONTRACT COST ESTIMATES



the engine flight test program was to have been finished by this time. The large cost and time overruns were conveniently dismissed as "the characteristic stamp of R&D undertakings."

A. J. Evans and N. Rekos of the Aeronautics Program Office in OART supported the increased funding. C. W. Harper's position was in effect neutral; however, he agreed to carry the matter to the head of OART, James W. Beggs, who had recently replaced Adams. New to the job, Beggs assigned responsibility for the HRE decision to his deputy B. Lundin, formerly of Lewis and soon to become director of Lewis on Silverstein's retirement. Lundin exhibited the traditional Lewis lack of interest in hypersonics and any propulsion R&D pursued by the non-propulsion Centers. When they learned that the thrust-minus-drag characteristic of HRE was near zero, the Lewis hierarchy became quite negative towards the project. This characteristic, which followed from the overriding emphasis placed on internal performance in HRE, arose repeatedly in the ensuing years to plague the project. A research engine with near-zero stoichiometric net thrust is not unlike a research airplane that cannot fly. The protestations of the HRE group on this point to the effect that external drag is a separate problem had a hollow ring.

The withdrawal of USAF support for scramjet was a deterrent to OART approvals of any HRE fund increases because traditionally the easiest way for NASA to obtain support in Congress was to say a need existed in USAF or the Industry. The three systems studies started by USAF in 1965 to seek impressive scramjet applications had been completed with indifferent results (71, 72, 73). All three listed many possibilities but few new ones, and DOD strategists now playing Viet Nam war games were not interested.

The shift of research interest at Langley to an entirely different type of scramjet also contributed to the softening of OART support. Applying their growing understanding of scramjets,

Langley propulsion engineers were now well along on a design in which the vices of HRE (high drag, high internal friction and coolant requirement, major shock and viscous interactions in the combustor) could apparently be eliminated. This work (74) had been started by Becker in 1967 with establishment of a contractual study at Bell Aero systems to determine feasibility and practicality of active airframe cooling using the excess fuel heat sink not required for engine cooling. The favorable results of this study (75) lent encouragement to the development of a low-friction low-coolant engine design, the principles of which were first stated and applied by J. R. Henry and S. Z. Pinckney (74).

Under this threatening cloud the HRE group traveled to Washington to plead their case on August 13, 1968, aware that they faced an uncertain future. Rereading their arguments now reveals a sense of desperation (70). Lundin and Harper listened patiently, but it was obvious that their minds had been made up earlier and that there was no hope for increased funds. Lundin's preference was to disband the project and continue to pursue the technology in the general research program at a yearly funding level of perhaps $3 million. This is the course which many of those interviewed think should have been taken. At the time, however, Lundin put the idea forward simply as a suggestion. It served to emphasize to the Project personnel that their only hope of survival lay in accepting reorientation to the limited technology goals plan (76). Lundin advocated completing the combustor development, and testing the boilerplate engine in the Lewis Plumbrook HTF facility rather than in the Navy Daingerfield facility as previously planned.

Although they were still in business, Rubert and his aides had lost their battle to save the HRE complete-engine development plan. They retained their Project organization, but their goal

now had been reduced to procurement and testing of an aerothermodynamic boilerplate wind tunnel test model and two selected structural test samples--functions usually carried out by straight-forward non-project methods in the general research program. It was a dejected group which picked up its charts and headed back to Langley. Evans and Rekos however, knew how close to termination HRE had come, and they were satisfied to see it still alive.

Immediately following the decision of August 13 to reorient, the contractor was directed to terminate his efforts in some eighteen areas of work (76), including the fuel and control subsystems for the flight-weight engines which had been partially completed. Terminal summary reports in these areas were authorized to salvage as much as possible for the record. It has been customary in most presentations of HRE results to display specimens and photographs of this partially complete subsystem hardware, particularly for the hydrogen cooling and fuel systems designed to be commanded by a digital computer. A total of about $4 million was spent on these systems up to the time they were discontinued in 1968. A turbine-driven pump for the liquid hydrogen fuel and high-temperature hydrogen valves controlling the fuel flow to the injectors were probably the most advanced items partially developed. The opinions on this part of the project of the individuals interviewed who were best qualified to judge was that these components in themselves did not represent especially significant advances in technology. However, the fact that the whole system had reached obstacles to success was thought to be important.

There was no question after the August 13 decision that the two-dimensional combustor development and the aerothermodynamic boilerplate engine program should be continued and planning for installation of this engine in the Lewis Plumbrook Facility were started at once. Previous plans for testing this engine in the

OAL Daingerfield facility were voided with finality by the Navy's decision to close down this facility. Daingerfield had virtually no business left except the planned HRE tests which in themselves did not justify the high cost of keeping the facility alive.

The Lewis Plumbrook hypersonic test facility ("HTF") had been conceived and promoted by E. Lezberg and his research group at Lewis, starting a year or two before HRE. It was basically a plan to make use of a large available nitrogen heater which had been originally constructed at Plumbrook for the discontinued nuclear rocket program. By adding oxygen to the hot nitrogen, synthetic hot air having Mach 7 total temperature could be generated. To extend the simulation to about Mach 9, Lezberg proposed the decomposition of nitrous oxide as a supplement to the heater but this mode of operation was never developed. HRE provided an application for the HTF, without which it probably would not have been built. Its construction schedule was too slow for all of the initial HRE testing requirements, but with the reorientation this problem disappeared.

In the regeneratively-cooled structures technology area the question of how to proceed after the reorientation was less clear. The established HRE plan had been to test some of the full-scale structural components of the engine in the North American high enthalpy facility, some in OAL Daingerfield, and the complete preprototype engine in Lewis/Plumbrook. Increasing difficulties with the North American testing, termination of the Daingerfield facility, and elimination of the preprototype engine obviously called for new planning. During the fall of 1968 the Contractor proposed that a simplified version of the Phase 1 "preprototype" engine, in which the entire regeneratively-cooled structure would be correctly represented, should be assembled for structural demonstration tests. Wherever possible components already existing or under fabrication would be employed. The flight weight fuel and control systems would not be used and no

combustion would be present. Combustor temperatures crudely approaching those with combustion would be simulated by using reduced rates of coolant flow. The tests would be made in Langley's eight-foot High Temperature Structures Tunnel at Mach 7 conditions because the Plumbrook facility, lagging in procurement, was now committed to the aerothermodynamic tests. Langley was enthusiastic about this scheme and baptized it with the acronym "SAM," for Structures Assembly Model. At the same time, following a suggestion of E. A. Mackley, the Aerothermodynamic Integration Model was dubbed "AIM." A new schedule appeared (Figure 4) calling for the "AIM" and "SAM" tests in the fall of 1970 (77). Changes in the project work statement in accord with these reorientation plans did not appear until September 5, 1969 (78).

After the AIM/SAM approach had been adopted and was well under way, a formal unsolicited proposal was received from the contractor (79) for a $7 million follow-on program which was essentially a costly regeneration of the complete preprototype flight-weight engine originally planned as the climax of Phase IIA. It will be recalled that the inlet and duct lines of this engine were to be those of the Phase I design, (not the finalized lines to be used in AIM). Structural components would be the same as those of SAM. Thus the name "HOT SAM" suggested itself for the follow-on proposal. Rubert estimated the cost of the proposal might be reduced to about $4.4 million by using AIM/SAM wind-tunnel-type fuel and control systems rather than the more costly flight-type systems suggested by the contractor. There was little possibility of promoting such a follow-on in the indifferent climate which now prevailed towards hypersonic research in USAF and NASA managements, even the $4 million version.

Actually, the same basic scheme as "HOT SAM" was one of the alternative plans rejected by the Project Office prior to the meeting with Lundin, Rubert being adamantly opposed to performance

testing with the Phase I duct contours. In retrospect it is not at all clear that the Phase I shapes would have produced inferior performance (80). Hot testing with both correct wall temperatures and heating rates would have added realism over the partial simulations of AIM and SAM (see the subsequent sections on SAM and AIM), and it seems probably now that "HOT SAM" would have been cheaper than AIM/SAM (80). One must remember, however, that it was expected in 1968 that the two-dimensional combustor development would achieve improved duct shapes and performance levels, a hope which did not actually materialize. Still it is regrettable that the HOT SAM option was not put forth for full detailed consideration at the reorientation meeting.

All of the interviewees were asked for their opinions of the reorientation. Aside from those closely connected with the project and contractor's offices who generally defended AIM/SAM, nearly all felt that HRE should have been terminated outright either at the ending of the X-15 program or at the Lundin review in August 1968. A few suggested that the SAM program only should have been pursued. The commonly expressed view was that key issues as highlighted by the HRE experience up to the fall of 1968 should have been made the subject of individual follow-on focused technology programs.

Thus, as the realities of the HRE undertaking continued to emerge, cost overruns for Phase IIA mounting to $5 million by the spring of 1968 together with dwindling support for hypersonics forced a major reorientation of the project. All plans to construct and test a complete hydrogen-burning flight-weight scramjet engine were abandoned and a technology program using the techniques of partial simulation was substituted. A boilerplate wind tunnel performance test model (AIM) having the desired finalized duct lines (but lacking in simulation of wall temperatures and heat transfers), plus a non-burning structural demonstration model (SAM) for hot wind tunnel testing at Mach 7

were selected by the Project Office for the technology program. By hindsight, a "HOT SAM" alternative to the selected AIM/non-burning-SAM approach now appears feasible and attractive. Those interviewed tended to agree that after years of disappointing progress and loss of the flight phase the project should have been terminated--either at the demise of the X-15 or at the reorientation. By asking the Project Office to recommend alternatives, it was a foregone conclusion that the original image of the project would be preserved as far as possible as it was in AIM and SAM. It is now widely believed that a general research program focused on the problems highlighted by HRE but not slaved to the HRE concept itself would have been more productive and less costly.

SAM

The SAM developments and Mach 7 demonstrations are almost universally considered to be HRE's finest contributions. In this section we will examine the reasons behind this accomplishment, recalling that the Statement of Work contained no requirement for structural development other than the prohibition of internal ablating materials. The weight limits imposed by the X-15 provided the main incentive for a sophisticated structure. Garrett had had outstanding prior experience with design and fabrication of cryogenic heat exchanges, most of which was directly applicable, and it followed naturally that they should pursue a regeneratively cooled structure for HRE.

The detailed account of the successful solution of the many difficult structural problems of HRE (81) reveals clearly how the contractor brought to bear a strong background of experience. The time and the technology were indeed right to build and demonstrate a complete variable-geometry engine structure.

Some idea of the difficult problems faced and solved by the SAM designers can be had from the following brief summary:

Design, precise fabrication, and testing of actively-cooled panels free of surface discontinuities and coolant passage obstructions, tolerant of non-uniform heating, and capable of stable operation in parallel circuits

Design of actively-cooled cowl leading edge 0.03 inches in diameter, capable of heat fluxes up to 1800 BTU/ft^2sec, and tolerant of foreign object damage

Development of integral coolant manifolds and shell stiffeners which reduce weight and thermal stress

Development of four-step brazing, testing and inspection procedures for shell assemblies.

The original plan had been to test the various structural components separately in hot airstreams and then to test the complex preprototype engine with combustion in Lewis/Plumbrook until a major failure occurred, hopefully not complete destruction of the engine. When it became necessary to reorient the program, and use the Langley eight-foot High Temperature Structures Tunnel for the structural testing, combustion had to be eliminated because the test gas in this facility is largely vitiated and there is no oxygen replenishment system. A single stage of the fuel injections was provided, however, to establish local interference heating near the fuel jets. SAM was assembled from the components to have been used for the preprototype. A rudimentary control system was used in which manually preset valves established the relative flow rates to five circuits in the engine. A single fuel dump valve, modulated by selected temperature sensors, controlled the total flow to the system. (In a flight system, of course, each circuit would be individually modulated by its own temperature sensor). The valves controlling flow to the combustor and nozzle areas were set so as to produce average initial temperatures crudely simulating those expected for Mach 8 design conditions with actual combustion.

The eight-foot Hot Structures Tunnel was a relatively new facility which had never been used for the kind of repetitive

testing necessary for SAM.* The tunnel combustion heater and throat sections were the source of many costly delays. A total of 55 test runs (30 minutes of exposure) were made at total temperatures up to 3400 degrees R and total pressures up to 224 atmospheres (dynamic pressures up to 0.94 atmospheres). Approximately half of the design fatigue life of the engine was consumed without any evidence of failure due to thermal cycling (it was impractical to test the structure to its full design life). The serviceability of the SAM plate-fin cooled structure was convincingly demonstrated even when foreign object damage occurred at the leading edge. The structure and cooling system were tolerant of large local heating non-uniformities.

In describing the HRE accomplishments Project personnel have sometimes used language which implies more significance to SAM than is really deserved, for example, "The SAM tests have proved the overall successful design of a hydrogen-cooled flight-weight engine structure for hypersonic flight to Mach 8."(82). Perhaps so, if the mission can be satisfied with an engine capable of only 100 cycles of operation which is the thermal cycle design limit for HRE. One of the principal results of the HRE structural development, in fact, was bringing into sharp focus the fundamental problem of thermal fatigue for cooled double wall hypersonic structures. The cause of the difficulty is "creep," or plastic deformation of the hot surface which in HRE was as much as 1000 degrees F hotter than the thicker relatively cold inside wall. When the entire shell cools down after operation a tension stress is thus left in the previously "hot" surface causing compression in the inside surface and associated distortions. With each additional cycle these stresses are increased, and the name "thermal ratcheting" has aptly been applied to the process.

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*See the Foreword to Volume I for a brief discussion of this high-temperature facility.

Reducing the temperature difference between the cold and hot surfaces will obviously reduce this thermal fatigue problem. However, if we attempt to achieve fatigue limits of the order of 10,000 cycles as might be needed for, say, a hypersonic transport, we find that such a goal is well beyond present capabilities (83). This problem had been encountered prior to HRE, but the SAM experience emphasized its importance and provided verification of the design and analysis procedures.

Another limitation of SAM played down in the Project literature is the absence of actual combustion effects. The contractor's own assessment of this deficiency given in his rejected proposal for "HOT SAM" lists the following "shortcomings" of SAM (79):

> The lack of the high heat fluxes that would exist in the combustor and nozzle with actual combustion and the corresponding absence of gas-side heat transfer data.
>
> Lack of actual flow field, shock structures, and local hot spots that would exist with combustion.
>
> Lack of effects of thermal transients on structural fatigue of components of widely varying mass such as manifold/support rings, shells, etc.

Clearly, a fuel-burning structural test engine such as "HOT SAM" could have provided much more valuable data and a more convincing demonstration than was achieved with SAM.

Generally judged to be the premier contribution of HRE to scramjet technology, the development and test demonstrations of SAM in a severe Mach 7 environment successfully validated the first complete flight-type regeneratively cooled scramjet structure. Major challenges remain to extend the thermal cycle life of this type of structure and to evaluate the effects of combustion. Underlying the SAM successes was the Contractor's solid background of previous design and structural fabrication experience with cryogenic heat exchangers and turbo-jet engines.

Thus he was uniformly successful in solving the many difficult problems faced in HRE. The technology was ready and a skillful practitioner applied it intelligently.

AIM

When HRE was reoriented in 1968 the boilerplate performance engine test program was greatly elevated in importance, becoming the primary remaining effort. In contrast to the highly successful SAM the AIM program was at best only marginally successful in spite of the enormous resources expended on it. There is a great deal to be learned from the AIM story and we will look first at the development of the inlet, combustor, and nozzle components:

Inlet Development: The "non-requirement" for low drag and high net thrust was a significant factor in AIM design, encouraging the use of higher-than-normal external compression. Specific requirements called for inlet "starting" at Mach 4 to deliver high mass flow and contraction ratios increasing from 6 at Mach 4 to 14 at Mach 8 - needed to achieve the high specific impulses hoped for. At Mach 8 the air in this design is compressed to a thin annulus of about 0.35 inches in depth at the throat. The throat is about half filled with boundary layer on the spike side and on the cowl side the total pressure is depleted by the strong cowl lip shock, a significantly stronger shock than would occur on a low-drag cowl. Thus, although the inlet met its design requirements in terms of pressure and mass flow, it presented the combustor with a formidable complexity of boundary layers, reflecting strong shocks, and their interaction effects.

Preliminary tests of a 1/3 scale model of the inlet started by the contractor during Phase I resulted in failure to achieve starting at Mach 4, and Rubert subsequently requested help from Center specialists in solving this problem. It was found eventually that the boundary-layer thinning effect of heat flux to

the cooled spike wall had a favorable effect on the starting problem (86). This was the first major supersonic inlet development in which wall-cooling had to be properly simulated in order to obtain valid test results.

Previous extensive experience with axisymmetric inlets and Billig's two-dimensional work (87) suggested that for the high contraction ratios and pressure rises found in the AIM inlet boundary layer bleed would be needed to avoid major flow separations and associated losses. Accordingly, the 2/3 scale test model of the finalized inlet design developed by subcontractor Lockheed/California included provisions for bleed. Also included were variable geometry and provisions for internal cooling with cold nitrogen gas. The tests of this elaborate and very costly model verified starting at Mach 4, provided the wall was cooled to design temperature ratios, and established the other flow field and performance data for the final inlet design (88). With wall cooling, the performance was judged acceptable without boundary layer bleed, and it was therefore possible to eliminate bleed from the design of the AIM inlet, a very important simplification of the HRE.

An important series of tests of the 2/3-scale model was made with helium injection to simulate the effects of fuel injection and combustion near the throat. The results indicated that inlet unstart occurred when the pressure at the throat was increased by a factor of about 2 1/2 due to this simulated injection and burning, corresponding to a ϕ burned of only about 0.38 at Mach 6. This was the basis for the use of downstream or "staged" fuel injection at Mach 6--to add the additional fuel needed for stoichiometric combustion without unstarting the inlet. It is important to note however, that only a small increase in throat Mach number would probably allow $\phi = 1$ conditions for the throat injectors. At Mach 8 where the average inlet Mach number was about 2.8 for example, $\phi = 1$ conditions did not unstart the inlet

as they did for Mach 6 where the inlet Mach number was about 2.5. It follows that with some relatively minor redesign, reduced external compression, for example, the Mach 6 case might have avoided staged combustion entirely. In view of the great difficulties discussed later for the Mach 6 staged design, it seems unfortunate that this approach was not evaluated. This possible solution of the Mach 6 problem must be regarded as tentative because inlet Mach number may not be the only dominant parameter; the effects of local Reynolds number, velocity, and ratio of stream energy to heat energy added should also be considered.

Center researchers now regard the AIM inlet as configured to meet such a special set of requirements (including closure during acceleration of the X-15) that it will probably find no actual applications (89).

Combustor Development: Both the Contractor and the Project Office were confident that the combustor development would be simple and quick. On his first attempt with the direct-connected combustor in Phase I (53) Garrett had obtained what appeared to be good combustion. As late as August 1968 the Project Office was still totally optimistic, stating in its review for Lundin (70) that the Phase I results had:

"supported mixing analyses"

"demonstrated the effectiveness of the design"

"proved step-less sub- and supersonic combustion"

"demonstrated mode transition by fuel control"

"highlighted effects of surface roughness"

Considering the pressures of the August 1968 meeting we can forgive these now painfully obvious exaggerations (project

personnel now believe that flow-separation rather than heat release caused part of the pressure rises in the Phase I tests). In any event the HRE team was so confident initially, that only $191 thousand was allocated to the two-dimensional combustor program. The Contractor's selection of a relatively inexperienced sub-contractor, North American Rockwell, with a small relatively undeveloped test facility, to conduct the combustor program is further evidence of how the problem was being underestimated.

Two-dimensional combustor models for direct-connected testing were designed to cover a wide range of variables. Misleadingly called "true scale," these models were not actual representations of the AIM design although their throat depth of 1/2 inch was typical of AIM. Another deficiency in these tests was the absence of the correct inlet conditions to the combustor. In particular the strong shock from the cowl lip was not present. Plate-fin construction was used to provide correct wall-to-stream temperature ratios. Procurement of these elaborate models took much longer than expected and it was not until almost two years after the start of Phase IIA that they were ready. Unfortunately, the tunnel heater would not operate properly at the very low mass flows of these models and additional months of delay occurred while the facility was being modified. Almost as much time was spent on the facility as on the combustor tests in this program but finally a picture of the combustor problems began to emerge, problems for which there were no ready solutions.

It is necessary at this point to review the design of the AIM combustor. One would naturally expect in a research venture of this magnitude and importance that a straightforward unified account of the combustor design assumptions and compromises, second order refined theoretical flow parameter calculations, and test data employed would be a prominent highlight of the project literature -- but not so. There are several general qualitative descriptions showing, for example, which injector rows are to be

used for different flight speeds (82), but there is no quantitative information on predicted heat release schedules, pressure distributions, Mach number distributions, and the like. Early in Phase IIA a first attempt at such calculations using one-dimensional flow assumptions was made for the preliminary duct lines in order to provide approximate pressure data for structural design (90, 91). But nothing even this approximate can be found for the final lines, and the project literature for the AIM combustor results does not show any theoretically expected flows for comparison with the test data. Therefore, in the following description of the combustor design it has been necessary to glean bits and pieces of information from several reports and from the discussions and interviews.

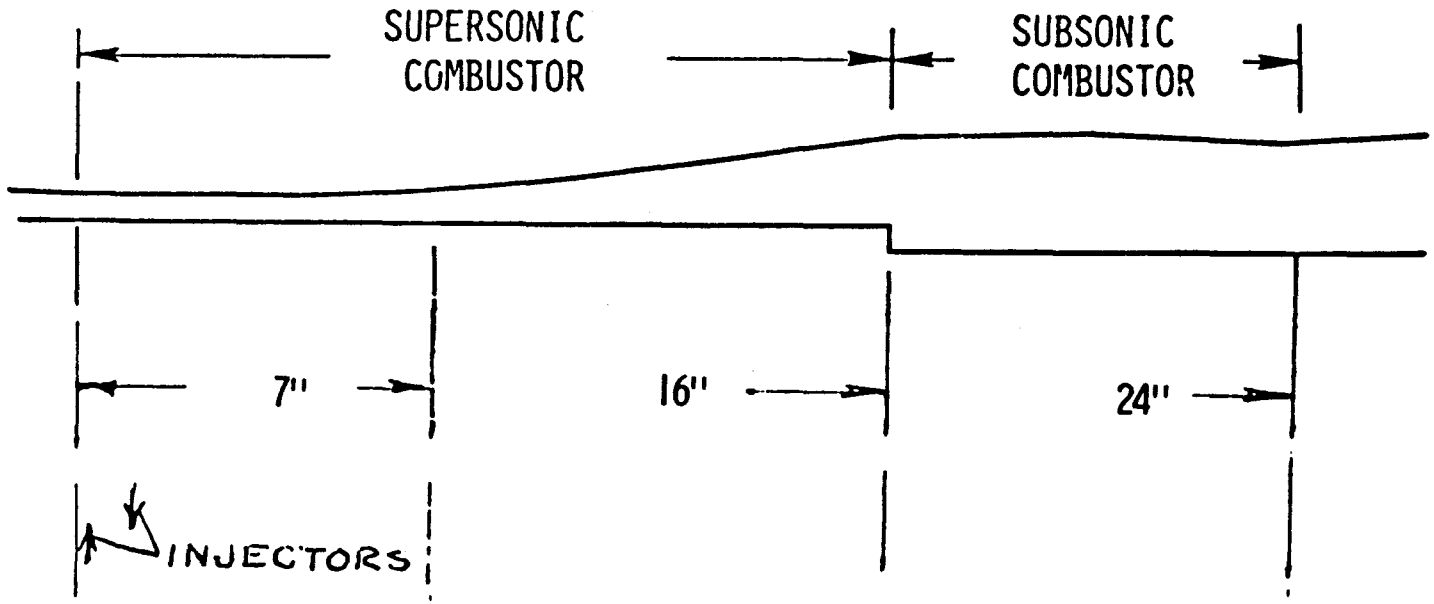
The HRE designers followed generally the guidance originally provided by the Weber and Mackay study (11) which suggested selecting an area development in which the combustor Mach number was depressed to about 1 at the combustor exit for stoichiometric heat release. Obviously Mach 1 is by definition the lower limit for true "supersonic" combustion, and thermal choking would occur if more heat were added in uniform duct flow beyond this point. To provide a small margin, an exit Mach number 1.3 was used in the preliminary Mach 8 design (91). This design situation for Mach 8 is illustrated on the top line of figure 5. General verification of kinetic and mixing calculations showing that stoichiometric burning could be achieved in the seven inch combustor length was obtained in the two-dimensional testing (92). The remaining 17 inches of combustor length for the Mach 8 case penalizes Mach 8 performance and is useless except that it fulfills part of the nozzle function.

AIM was never operated at Mach 8, and we are therefore more interested in Mach 6 where valid testing was accomplished for comparison with the design intentions. As previously indicated only a minor fraction of the stoichiometric fuel flow could be

injected in the first rows; the design value of 0.25 is shown in Figure 5, for which it is presumed a slightly supersonic flow would be maintained at the seven inch station. With the remaining fuel injected at this station the condition $\phi \sim 1.0$ would be reached by the end of the supersonic combustor at a design Mach number of roughly 1.6. This is somewhat higher than the value 1.3 mentioned earlier because the area ratios were arbitrarily increased to allow for possible flow separations (91, 92). The expected pressure distribution for Mach 6 would thus be a sharp rise in the first few inches, and a second large rise downstream of the second rows of injectors. These effects would be superimposed on and would interact with the effects of the strong reflecting shock from the cowl lip. Unfortunately, as stated previously, no prediction of the pressure distributions is available from the project literature--either derived from calculation or from the two-dimensional tests.

The "subsonic" combustor design was seriously compromised to favor the Mach 6 supersonic combustor. Only a five percent contraction was allowed to insure that choking at the exit would not occur for the supersonic combustion case. This meant that the inlet Mach number for the subsonic combustion mode would have to be about 0.8 or much higher than desired for efficient subsonic design. At Mach 6 this geometry required a strong normal shock far downstream of the inlet throat, with attendant high losses. At Mach 4 the matching is better (shock nearer the throat) but burning at Mach 0.8 - 1.0 is still required. Thus for each flight speed and type of burning the HRE combustor incorporated compromises which left much to be desired.

Returning now to the two-dimensional "true-scale" test programs we must note first that it had been so long delayed that the AIM combustor design had been nominally "frozen" some months



	M	M Ø	M Ø	M Ø
MACH 8				
DESIGN	2.8	1.3, 1	2.5, 1	
MACH 6				
DESIGN	2.5	~1.3, .25	1.6, 1	—
TEST	2.5	<1 , .15	1.7, .48	1 , .95

809

FIG. 5 - COMBUSTOR DESIGN CONDITIONS AND COMPARISON WITH MACH 6 TEST RESULTS FOR "SUPERSONIC" COMBUSTION. Ø ≈ 1

prior to start of the "true-scale" tests, on the strength of hopes that these tests might not dictate any major changes. Unfortunately, the tests revealed a grave problem for which no solution had been found in the two-dimensional program: Efficient combustion of the fuel injected in the second-stage rows could not be obtained with any combination of the design variables tested; the results indicated efficiencies ranging from only 10-50 percent.

This came as an unwelcome surprise to the HRE team, but hurried calls to the losing HRE contractors and other specialists revealed that similar problems with combustion in diverging channels had been noted previously by G.E., Marquardt, APL, and by Cookson in England. There was, however, no generally accepted explanation. Kinetic quenching was the theory favored by Garrett, and this effect would have been aggravated by the water vapor present in the North American facility. The other explanation favored by the Project Office suggested that the mixing action needed for good combustion was inhibited in the falling pressure of the diverging duct. They postulated that moving the second stage injectors forward into the region of reduced divergence would eliminate the effect.

A major meeting to debate the matter was held at the Contractor's plant on September 29 and 30, 1969. Breaking the precedent so far set for this project, Rubert approved the Contractor's suggestion to hire a consultant, Dr. J. W. Shetz formerly of GASL and then of VPI/SU. To acquire an up-to-date background Shetz call upon Dr. Fred S. Billig, the leading scramjet combustion specialist in the Navy program. Billig had recently completed an analysis of hydrogen combustor design, a major part of which had been funded by NASA with J. Henry of Langley as monitor. Billig felt that HRE had neglected to make use of this work in their combustor design, rather illogically since much of it had been NASA-sponsored (95). He had not been

called upon by HRE to participate in any way, and now his brains were being picked by Shetz, indirectly for the benefit of HRE. Billig was understandably unhappy, especially because his previous offers of help to HRE had not been accepted by them. In checking on this matter I found that some of Billig's progress reports on the NASA-sponsored work had been sent to HRE and probably were forwarded to the Contractor. However, there is no evidence they were used. Ames Research Center personnel also expressed interest in visiting the Contractor to learn how they could help in this problem but their request was denied by the Project Office, which stated that any flow of information to or from the Contractor would have to come through the Project Office. Ames support of HRE quickly deteriorated after this event.

Actually the September meeting turned up no new ideas. There was general agreement that two additional rows of injectors should be added to the AIM combustor just upstream of the existing second stage rows. Rubert proposed also that the two-dimensional test program be terminated and that it be left to AIM "to resolve the problem" in its tests in Plumbrook (96). The Contractor protested this course as being much too risky and he proposed two optional approaches both of which involved continuation of the two-dimensional testing, delaying AIM as necessary until a satisfactory design was demonstrated (97). His proposals were rejected on the grounds that excessive time and funds had already been used up in the poorly executed two-dimensional program ($2 million or ten times the original budget figure), and furthermore the vitiated flow and other problems of the two-dimensional testing were questioned. Rubert felt that only the complete engine test in Plumbrook would provide truly valid data.

By hindsight this decision to terminate the simple two-dimensional testing and utilize a complex, inflexible, unproven, complete engine to resolve a basic problem appears clearly to be a violation of well established R&D practice and one of the pivotal

mistakes of the project. If this had been a general research operation instead of a Project with an engine model to build and time schedules to meet, unquestionably the two-dimensional work would have been continued before any commitment to a complete engine model was made.

During the ensuing two years some support for the "mixing" theory of the diverging-duct combustion problem was forthcoming from tests made at UARL with USAF cooperation (98), and from the Contractor's analyses of the "true-scale" data including both mixing and kinetic considerations (99). Neither of these, however, in any way obviated the basic need for further two-dimensional testing of the AIM combustor prior to commitment to the complete model.

Nozzle Development: Several of the interviewees pointed out that the AIM nozzle could have been improved by variable geometry, increased expansion and skirt length, and refined contours. These views seem to be at odds with the HRE literature covering the nozzle which refers to it usually as an "optimized" design (70). The discrepancy lies largely in semantics. The Project Office used the word "optimized" to mean "best compromise with fixed geometry" for performance through the HRE speed range. The compromises involved both aerodynamics and structures, for example the use of a conical rather than cusped shape for the nozzle innerbody. Cooled-wall aerodynamic testing of the 1/3 scale model of the nozzle was completed without incident and the desired performance levels were obtained. This was the only major component for which aerodynamic development cost remained within the budget estimates (Section 15).

AIM Structure and Fabrication: In the original project planning it was essential to use a water-cooled boilerplate structure for AIM because fabrication had to start prior to completion of the flight-weight structural developments. Retention of this approach

was more or less automatic after considerations of the preprototype engine were ruled out in the reorientation decisions. The Project Office liked the thick metal walls of the boilerplate for another reason - they might conceivably be re-machined to provide small but important changes in the duct contours if this should be found desirable.

AIM proved far more difficult to design and fabricate than the Phase I boilerplate combustor model. It was a much more complex shape with inlet spike, leading edges, support struts, etc., than the Phase I model, and its problems were aggravated by the 1000 psi total pressure of the Plumbrook facility which was more than double that of Daingerfield. Metal thicknesses up to 1/2 inch were required. Welding of the basic material, nickel 200, to itself, to stainless steel, to Hastelloy X, and to Zirconium-copper presented extreme difficulties. Electron beam welding had to be supplemented by furnace welding and explosive welding which required much experimentation and development.

These problems are blamed in part for the large cost overruns of AIM (see the subsequent section on HRE costs). Lopez now believes the preprototype or "hot SAM" structural approach could have been used for less cost, and he has suggested the possible deletion of AIM-type boilerplate models in future programs. (94)

Test Facility Problems: Past experience with the Daingerfield facility suggested that a test engine as large as 24 inches in diameter could be used successfully with its 36-inch free jet. This was a primary consideration determining the size of the HRE engine. With a test model this large there is no possibility of course, of proper simulation of the external flow aft of the inlet. The objectives of this kind of testing are:

to provide valid flow conditions

to provide stream pressures at the nozzle exit low enough that the supersonic flow in the engine nozzle cannot be affected by disturbances from the facility

to provide axial force measurements which can be used, after careful corrections, to determine the internal thrust performance

to provide complete internal pressure distributions, heat transfer, etc.

The change from Daingerfield to Plumbrook necessitated reexamination of the starting problem. The Plumbrook HTF facility had 42-inch nozzles, which eased the blockage problem, but it had other features whose effects were not favorable, in particular the exhauster capacity controlling the test cell and exit pressures was inferior to Daingerfield. The detail features of the new test installation were derived from an arrangement used successfully by G.E. in their Mach 7 scramjet tests (54). Model tests of the Plumbrook installation made in a Langley facility indicated successful starting could be achieved and suggested detail features which were incorporated in the HTF (100). Unfortunately, the actual installation did not perform as expected, due apparently to small but vital differences in scale, geometry, and other factors.

Aeronautical R&D history is replete with examples of the unpleasant consequences of attempting, simultaneously, to develop a complex new research object and a complex new facility. The AIM/Plumbrook experience probably exceeds any previous example, because here the new facility and new engine were joined in proportions lying on the fuzzy boundary between the possible and the impossible. The language of the AIM test reports does not

dwell on the interminable difficulties and problems of this program--instead, the final test report states (94): "The tests were performed in general as planned." Another statement is somewhat more indicative of what actually happened: "six and one-half months of engine testing was completed with one-hundred and ten minutes of running time." Of the 110 minutes running time, however, we can deduce from other data in the report that less than 60 minutes was useful engine operation. The realities of the program are best indicated by the following self-explanatory statistics:

- Total test runs 63
- Test runs in which little or no useful data were obtained 42

Starting on September 14, 1972, when the first run was made, the test crews struggled with tunnel and engine starting problems through 24 unsuccessful runs. Finally on May 24, 1973, the first "good" run was achieved. Hardly had the good runs been started when major heater malfunctions caused a four month shutdown. Spread throughout the AIM test run record is the cryptic notation, "TAFP" -- Test Aborted due to Facility Problems. Even the one out of three runs which produced useful results were seldom completed without difficulty. Inlet unstarts, fuel control problems, instrumentation problems, coolant leaks, skin cracking, formation of a "step" between leading edge and combustor wall due to heat distortion, and erosion due to graphite heater particles in the airstream (including some the sizes of marbles and golf balls)--these were the more common difficulties that plagued the "good" runs. As can be inferred from the above, there was a continuous deterioration of the heater as the program progressed. When the Mach 7 nozzle was installed in January 1974, the heater could be operated only at Mach 6 temperatures, making the "Mach 7" tests virtually meaningless as far as scramjet performance was

concerned. The test crews were comprised on contractor, Plumbrook, Lewis, and project office personnel who performed with great perseverance and resourcefulness in the face of these difficulties. Most of the "fixes" developed for the facility problems were in the nature of "cut and try" aerodynamic blacksmithery. There is great doubt that they would be applicable to other installations differing in detail from the HRE, and there is thus little profit in reviewing them here. Suffice it to say that if AIM were done again, a smaller ratio of engine/facility diameters would be used. As to the HTF facility, which is now "mothballed," major changes in the nitrogen heater design appear necessary if this is ever to be a routinely operational tunnel.

Test Results: After the first "successful" runs were obtained in May 1973, the HRE team was jubilant. A preliminary assessment of the Mach 6 thrust data indicated that it was in line with AIM expectations. Dr. Rubert was so well satisfied that he promptly announced his retirement as of July 1, 1973, and in a memorandum to his former boss, L. K. Loftin, Jr., he claimed virtually unqualified success for HRE in all areas--notwithstanding the loss of the flight phase and the reorientation--and he proclaimed "mission accomplished" for the entire project (101). Recently, however, Dr. Rubert has expressed considerably less expansive and more critical opinions which are summarized in a supplementary section.

The discussion in this section, as in the AIM reports, is a largely qualitative assessment characterized by "data chasing" and speculation. This approach is all that can be done in the absence of any attempt to analyze rigorously the complex flows existing in AIM. Although the combustor flow is clearly not one dimensional and has the discontinuous non-uniformities characteristic of reflecting strong shocks, the contractor has averaged the inner and outer wall pressures at each station, assumed the duct to be flowing full, and has thus calculated, on a one-dimensional basis,

Mach number distributions for the combustor based on the pressure data. Heat release distributions are obtained with the same assumptions, and obviously any pressure rise due to flow separation is included in this definition of "heat release." It is most unfortunate that there was no way to observe or measure the actual combustor flow except by wall pressures. The reason given for this deficiency is that any duct instrumentation might have adversely affected the thrust.

Fed with the thick boundary layers and strong reflecting shocks from the HRE inlet, the AIM combustor exhibited a huge total pressure loss; with no fuel injection, 75 percent of the inlet total pressure was lost, or twice what could be accounted for by skin friction. With fuel injection from the forward rows, the total pressure loss is still further increased - far more than can be accounted for by the effects of heat release. Further evidence of the degradation of combustor performance with this inlet can be seen by comparing AIM data with the direct-connected Phase I combustor data which showed better performance (102).

While the inlet clearly had a large adverse effect on the combustor, there was no evidence that combustor operation adversely affected the inlet, provided that the fuel injected remained below the critical values.

The combustor flow differed drastically from the hopes and expectations for Mach 6. It should be stated at once that the basic problem of second-stage combustion in the diverging duct was not resolved and the status of this problem remains the same as at the end of the two-dimensional program. Actually, only one of the two added rows of fuel injectors which hopefully were to "resolve" this problem was ever tested and the only beneficial effect it exhibited was increased interaction with the forward rows due to its closer location to them. The indicated combustion efficiency of the second-stage rows was at the same low levels shown in the

two-dimensional program. In fact, a valuable result of AIM was general confirmation of the trends and conclusions reached in the vitiated two-dimensional testing.

Referring to Figure 5 and comparing the "test" and "design" results for Mach 6, we note the following:

- Stoichiometric fuel injection resulted in apparent subsonic duct flow in the vicinity of the second-stage rows, presumably due to flow separation and reduction in effective duct area.

- By the end of the diverging Mach 6 supersonic combustor, only about 48 percent of the injected fuel had been burned in contrast to the complete combustion expected.

- Shock and other disturbances in the portion of the combustor designed for the subsonic burning mode unexpectedly triggered release of the remaining heat. Without this bonanza, AIM performance would have been far below its design values.

Some of the interviewees, notably Dr. Ferri and Dr. Billig, believe that despite the obvious complexities, the AIM combustor data is amenable to more precise analysis. If shock and separation effects could be accounted for, new insights as to what was actually happening would obviously be obtained. Dr. Ferri, with understandable satisfaction, called attention to the fact that he went out of his way to warn NASA about these inherent problems of the Garrett engine design during the evaluation but no one listened to him (103).

In regard to the "subsonic" burning results, the calculated one-dimensional Mach number distribution for $\phi \propto 1$ shows about the

same average level of Mach number and pressure as for the "supersonic" case. That is, comparable apparent "transonic" conditions existed in both cases. The observed overall performance was thus as might be expected, about the same. These facts certainly greatly weaken any conclusions drawn relative to the ease of transition from "subsonic" to "supersonic" burning by control of the fuel injector schedules. Furthermore, the results teach us nothing significant about the relative performance levels of "subsonic" and "supersonic" burning attainable in more effective designs operating in reasonable conformance with the design postulates.

How well did AIM do in achieving the objectives advertised for it in 1971 by the NASA and contractor Project Managers? These goals can be paraphrased as follows (82):

1. Obtain "realistic," "solid," or "factual" performance data for a large engine model of such refinement of detail and test measurements as to provide new insights, and so for the first time to establish the true state of the art.

2. Illuminate critical unknowns.

3. Provide interim solutions to the new problems encountered and stimulate basic research for more effective solutions.

Objective 1. The opinion of a majority of those interviewed is that the AIM performance data are little, if any, more valuable as indicators of the true state of the art than the results obtained previously with the smaller test engines. The size of AIM was an advantage with regard to Reynolds number and instrumentation, but a disadvantage in the test operations. The concerted slanting of the design to favor high internal performance is considered a major detraction. In reality the AIM performance data show the same general level of internal performance that was already established by the small test engines

(Fig. 6), a much lower level than hoped for in the original HRE prospectus.* The fact that the Mach 6 point for AIM falls exactly on the AIM prediction is cited by the contractor as "most gratifying" on page 114 of (117). But on page 115, we learn that the agreement is coincidental, that the assumed nozzle performance used was too high, and that if the actual nozzle performance (3 or 4 percent lower under stoichiometric burning than in cold flow) could be improved, then the actual performance would be significantly higher than predicted. Unfortunately, insufficient information is given in the AIM reports to allow the reader to judge the methods used in the predictions. In general, one can only conclude that Objective 1 was not truly achieved.

Objective 2. Here AIM deserves a higher score. It did shed new light on the real-life problems and true status of supporting technology for scramjet combustors. Much of the information, unfortunately, applies to undesirable flow and combustor situations more to be avoided than to be used in future applications. None of the researchers or company personnel interviewed expect to make any use of the AIM data, and most of them were not even aware that copies existed in their files and libraries.

Objective 3. This objective was not achieved. The outstanding example is the failure to resolve the problem of how to attain efficient combustion with staged-fuel injection in a diverging combustor. Neither interim solutions nor stimulated basic research have yet developed as a result of AIM. Dr. Rubert's view on this point is that the AIM tests were prematurely ended, that they should have been continued until

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*It is interesting to note that the small test engines noted above for which performance comparable to AIM was obtained 5-to-8 years earlier at very low cost were based on the GE and Marquardt (Ferri) losing proposals.

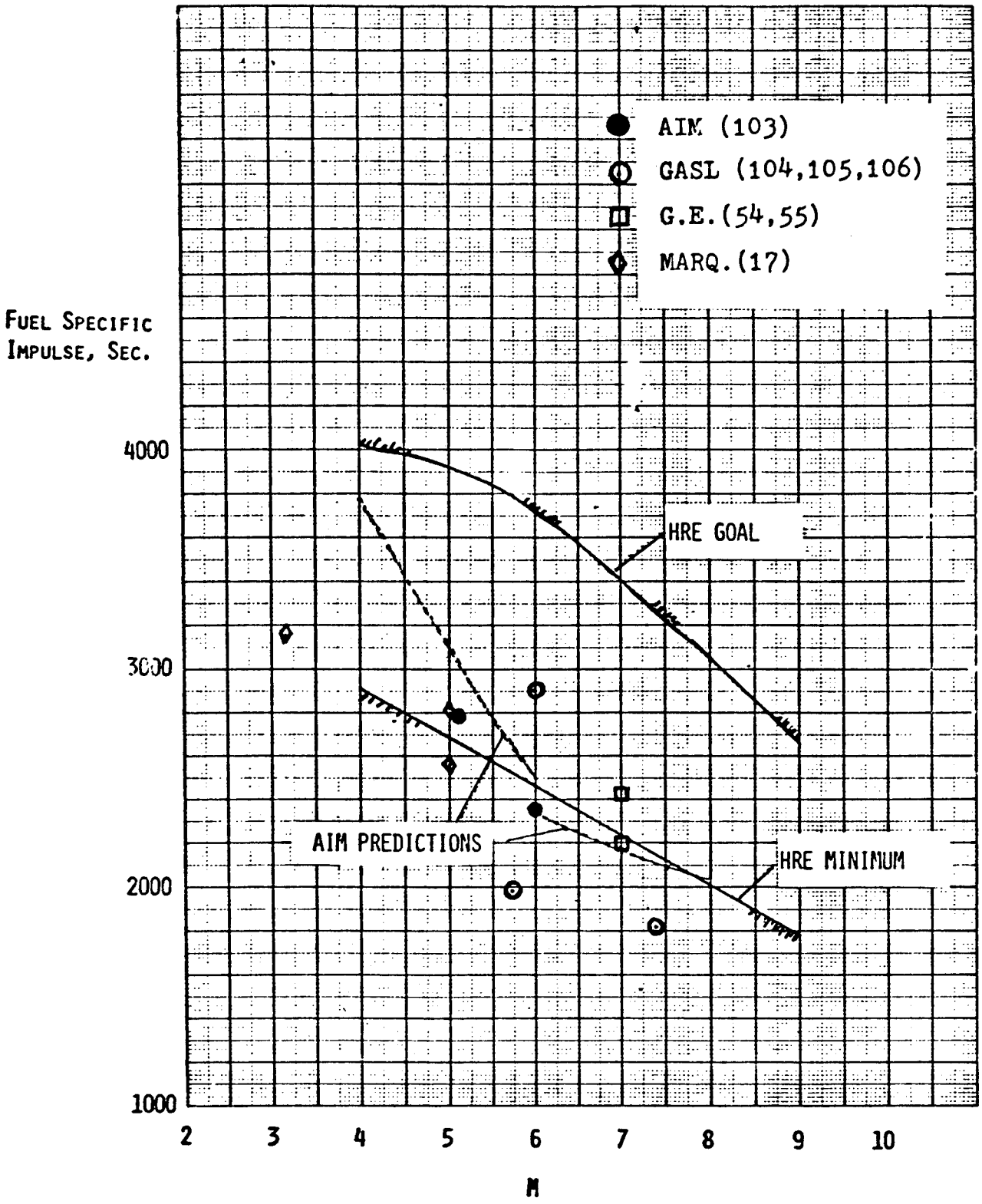

FIG. 6 - AIM RESULTS COMPARED WITH PREDICTIONS, HRE REQUIREMENTS, AND TEST RESULTS FOR OTHER AEROTHERMODYNAMIC MODELS

second-stage injection into a region of constant or rising pressure had been achieved.

As has been demonstrated, then, all aspects of the AIM combustor development were seriously underestimated in the general scramjet euphoria of the mid-1960s. The combination of an inexperienced sub-contractor and an unproven hot facility was ineffective in dealing with the fundamental complex problems of combustion in a diverging channel. After long delays and nearly $2 million in cost overruns, the combustor program was abandoned without ever achieving efficient staged combustion. It was decided to use the complex AIM itself "to resolve" the combustion problem, a decision which appears in retrospect as a grave mistake. Tests of the large AIM model in the new Plumbrook HTF Facility were characterized by the host of agonizing problems that seem always to plague advanced R&D ventures when a very complex research model is operated for the first time in an unproven equally complex new test facility. Of some 63 test runs extending over a period of about 2 years, only 21 produced significantly useful engine data. Efficient staged combustion in the diverging duct was never obtained, leaving that problem still unresolved. Unexpected results included strong beneficial stage interaction effects, transonic flow, and belated combustion of the unburned fuel from the supersonic combustor in a downstream region containing strong disturbances. Although this behavior was quite different from the AIM combustor flow concept, the overall internal performance at Mach 6 fell on the lower boundary specified for HRE, and in the midst of performance data points obtained previously with smaller much less costly test models. Of three ambitious goals laid out for AIM, only one was clearly achieved - "the illumination of critical unknowns." After ten years, the HRE concept would have still required additional years of testing and development before it could possibly have been judged worthy of an X-15 flight phase.

CHAPTER III

HRE IN RETROSPECT

Project Operations

Until his retirement in 1973, K. F. Rubert had spent virtually his entire career in propulsion and aerothermodynamics, first as a teacher, and since 1940, as a Langley researcher and Branch Head. HRE was the most challenging and responsible assignment of his career and he responded with energy and enthusiasm. He was convinced that HRE was truly vital to the future course of aeronautics and national security, and as time went on these convictions seemed to grow stronger. Rubert inspired strong loyalties in his two principal aides, B. C. Deis (Engineering) and E. A. Mackley (Aerothermodynamics). At lower levels, however, he was, in some cases, less successful; a number of engineers departed HRE at least partially because they were unable to adapt to Rubert's strong management, which they considered too autocratic.

The HRE staff was drawn from Langley researchers, design and equipment engineers, and other supporting personnel. None had ever had any significant direct involvement in engine design or development, and only a few had had project experience. The staff of the Project Office peaked at about 14 people in 1968, not including a roughly equal number of support personnel at Langley and the other centers who were not formally transferred to HRE. The total number of effective positions was thus not far from the requirements estimated in the PDP of May 1965 (33 positions). HRE, however, always considered that it was understaffed. As late as September 1972, long after there was any need to augment the

staff, B. C. Deis wrote a long memo purporting to show that understaffing had contributed strongly to the problems encountered with the 2-dimensional combustor development and the fabrication of AIM. He estimated at least $2 million might have been saved if a few additional people had been provided (107).

From its very inception in late 1966, the Project Office operated in an unfortunate environment of waning support for hypersonics generally and HRE particularly. The NASA top managers who had so easily granted their approvals initially were now gone or on the way out, and their places had been taken by skeptical newcomers. Only A. J. Evans and N. Rekos of the Aeronautical Programs Office remained as firm supporters. It was clear from the start of Phase II A in 1967 that the X-15 flight program and the enormous funds needed were not likely to materialize.

In Rubert's opinion, the situation degenerated still further when a reorganization placed HRE under one of the research divisions at Langley. (Previously Rubert had reported directly to Assistant Director for Aeronautics L. K. Loftin, and he had enjoyed a rather free hand.) Fortunately, the division was headed by an outspoken veteran ex-project manager, H. A. Wilson, Jr., and HRE hoped that his crusty advocacy would be a help. Shortly, however, a second reorganization placed HRE in another research division headed by J. V. Becker. Although he had been a proponent of HRE in the beginning, Becker was now actively engaged with J. Henry and others in studies of a new low-coolant modular scramjet concept (74) capable of low-drag integration with hypersonic airframes. Both Becker and Henry, however, continued to support completion of the AIM/SAM test programs on the grounds that the huge investment in HRE would be largely lost if these tests were not completed. Rubert indicated his hope that Division interference in HRE be minimal. He reminded Becker that a project office necessarily had to operate with much more autonomy than research groups.

The progressive degeneration of HRE seemed to inspire in the three principals in the Project Office defensive loyalties which intensified as time went on. Whereas in the early years, HRE was viewed simply as a research device, it eventually began to be regarded as having inherent virtues worthy of application in operational systems. Perhaps this change in attitude started with the proposal for a specific application made by A. duPont, the designer of HRE and Garrett's first HRE project engineer. He advocated a national program to develop a truly reusable airbreathing Shuttle system having operational costs as low as 1/10th those of the current rocket shuttle. The new system would capitalize on HRE technology, which he claimed would make possible its realization many years sooner than if more recent scramjet concepts were adopted. Podded HRE-type engines would be used, modified to improve their Mach range and thrust-minus-drag, and equipped with a cryogenically-pumped injector augmentation system providing take-off thrust (108). As a precursor to the new shuttle system he proposed a research airplane which in effect was a sub-scale prototype (109).* A. duPont aggressively peddled this proposal throughout NASA, to the Congressional Staffs, to the Nixon White House Staff, and elsewhere. In the course of these promotions he felt obliged to denigrate the Langley modular low-drag low-coolant scramjet in order to enhance his HRE-derived engines. He was a skillful and brilliant salesman and manipulator of arguments, and the Langley staff was obliged to prepare a white paper detailing what they believed to be the true facts in rebuttal of the duPont claims (50, 110). A. duPont eventually gave up his promotion, still maintaining, however, that the argument was not settled to his satisfaction.

A second application was a hydrocarbon-fuel version of HRE studied by Garrett under USAF sponsorship as a possible powerplant for an interceptor missile in the Mach 5 category.

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*A concept similar to the X-30A proposed research vehicle of today - ed.

Inspired by these potential applications, the Project Office came to regard HRE as something more than an object for research. In late '71 Becker received a memo from B. C. Deis comparing HRE-type pods and the Langley modular engine from the structural weight standpoint, the result suggesting the pods might be so much lighter as to have an overall performance advantage in spite of their higher drag. (Recent in-depth structural studies by Garrett and others do not support the Deis arguments.)

Most of the interviewees who were in a position to judge agreed that the Project Office had a highly exaggerated view of the importance of HRE. Many examples could be cited; a typical one was the outraged reaction of the Project Office when they learned that Langley, as an economy measure was discontinuing the microfilming of all HRE contractor drawings (111). Rubert fired off a hot memo to the Director for Administration emphatically insisting that the drawings were a major element of the "HRE product" and should be reproducible on demand of the many future users of the technology whom he visualized. His protests were of no avail. A check of Langley files in 1975 revealed that not a single request had been received for any of these drawings. On this same point it was discouraging in my talks with the interviewees to learn that none of them had examined the final AIM reports. In most cases they were not even aware that the reports were available to them in their files or libraries. Many hours of video tape recorded during AIM has seen negligible use.

Another indicator of the inflated importance HRE had developed in the eyes of the Project Office was their continuing insistence on rigid Confidential classification. Rubert argued on many occasions and still insists that knowledge of the AIM duct lines was a vital asset for the United States and should have been protected. Considering the failure of both the 2-dimensional program and the AIM program to achieve efficient staged performance in diverging combustors and the still speculative

nature of proposed "fixes," it is difficult to understand his attitude. Classification was retained nevertheless after Rubert's departure until data from French tests of a combustor remarkably similar to AIM (102) became available. Then, with declassification of much similar material in the USAF program and the shift of interest to more important configurations, there remained no credible need to retain confidentiality. The entire project was declassified with the unanimous concurrence of all NASA officials involved in late 1974.

Major problems in Project operations developed in 1970 and continued throughout the AIM program which are worth recounting. B. Lundin was now Director of Lewis and it was a time of reduction-in-force for the Center staff. From his direct prior knowledge of HRE Lundin felt that this was an area of R&D which should be terminated in a shrinking organization. Lewis/Plumbrook was furthermore committed to the testing of Viking hardware, an activity which competed with work on the hypersonic engine test facility (HTF) for HRE/AIM. Thus, in the summer of 1970 Lundin directed that work on the HTF should stop pending reassessment of the remaining facility development problems and manpower requirements.

The Project Office was shocked by this turn of events, and was quite vocal in their righteous indignation. They pointed to Abe Silverstein's letter of 1967 (112) which they regarded as a "promise" that HTF would be completed and used to test AIM. On August 12, 1970 Becker sat down with Evarrd and Manganiello of Lewis management in an effort to resolve the problem. They questioned at first why Langley wished to proceed with HRE, citing (a) the zero net thrust capability, (b) the unsolved combustor problem, and (c) serious difficulties with cracks developed in the graphite blocks of the N_2 heater at Plumbrook, requiring costly redesign. After some discussion they accepted the argument that the HTF tests were essential to avoid total loss of return on the

enormous agency investment in AIM. The talk then turned to reactivation of the HTF program with Langley manpower being brought to Plumbrook to supplement the diminishing Lewis complement. Upon returning, Becker took up the matter of Langley manpower support for Plumbrook with LRC management and they agreed that HRE should proceed on this basis.

To help save the program Rubert solicited a testimonial letter from the Air Force Aero-Propulsion Laboratory Commander, Col. Tipton P. Mott-Smith, extolling HRE and urging completion of HTF and the AIM and SAM programs (113). About a year earlier the previous APL commander, Col. Herbert A. Lyon, had also been solicited and supplied with material for a similar letter sent to Loftin at LRC (114). These letters affirmed what was already known, that USAF ramjet R&D was concentrating on hydrocarbon-fueled ramjet engines for small missiles, and they added that NASA was being depended on to carry the ball for hydrogen-fueled scramjets. Obviously solicited letters of this kind normally have little influence on Center managers, but there is always the hope that they may prove useful in Congressional or OMB justifications. In this case they had little benefit.

Following up on the Langley/Lewis tentative agreements, Rekos visited Langley on August 24, 1970, to review the updated objectives for AIM testing and Langley manpower support for the HTF test program. He returned to Washington with the necessary material to justify continuation of the HTF/AIM program. The proposed use of Langley manpower was approved in principal; however, it eventually became clear that the transport and training of Langley personnel in sizable numbers was a very inefficient way to accomplish the job and this part of the plan was never implemented. Work on HTF was resumed and the basic facility was completed at about the time AIM was delivered for testing in August 1971. There was a long delay however in completing the installation of the special test equipment needed

for the AIM program. An appeal was made directly to Lundin to expedite the final phases of preparation, but it was unsuccessful. In a meeting with Rekos, Becker, Rubert, and Hennings of HTF on December 7, 1971 (115), Lundin stated flatly that were he to reexamine HTF/HRE manpower at Plumbrook it would be for the purpose of withdrawing support. And thus AIM sat idle from August 1971 to May 19, 1972 when installation in the HTF was started.

When AIM testing was resumed after the interruption from June to October 1973 to repair the facility heater, another threat appeared in the form of a Lundin decree that no HRE testing would be permitted after December 31, 1973. The entire Plumbrook operation was scheduled to be terminated by July 1974 as part of an agency-wide retrenchment program. The December 31 date, 6 months earlier than the terminal date, according to Lundin's planning would give Plumbrook personnel time to find other jobs or be reassigned within NASA. This gave AIM roughly half of the time estimated to be needed for the remaining testing. It goes without saying that the situation looked more and more hopeless as December 31 approached and only a fraction of the planned testing had been accomplished. Lundin's aim presumably was to keep maximum pressure on the hapless HRE troops. He offered no signs of relenting, even in the face of several memos (116) and letters, and phone calls from OART and from Langley Director Cortright. At a Langley meeting called by Rekos on December 6 to review AIM progress a new threat appeared: the shortage of fuel oil. Oil was needed in large quantities to fire the steam plant which operated the HTF exhausters. Hennings had to pull special strings to get the needed fuel in the months ahead which saw the first national oil crisis reach its peak. Finally in the last days of December, Lundin indicated that AIM might continue for one more month. Eventually this was extended, by default, to over four months; the last test was run on April 22, 1974. For almost seven months AIM testing had been conducted under the constant

apprehension that the Lundin ax might fall at any moment, months prior to the actual closing of Plumbrook. At times it was not clear who was running the project in the day-to-day operations. Lewis, Plumbrook, Project Office, and Contractor personnel got along as best they could, but each group had the same complaint: it was not certain who was in charge. A crystal-clear statement of authority for the AIM/Plumbrook operation by OART would have eliminated these difficulties.

The defensive psychology that developed in the HRE team made it difficult for them to report test results in the fully objective detached manner of researchers searching for the truth and letting the chips fall where they may. The natural tendency of the project team was to try to show that the test object did what it was supposed to do, confirming and validating the vast development efforts they had put into it. Thus we find that the HRE reports tend strongly to accent the positive, and deemphasize, tone down, or in some cases omit, the negative results. This is done instinctively rather than from any conscious attempt to distort or deceive. Some obvious examples seen in AIM accomplishment lists in the project literature of 1974 and 1975 are:

"Maximum thrust close to predictions was obtained"

"Realistic engine performance levels were established for Mach 5 to 7"

"Customer efficiency of 95 percent was achieved"

"Transonic combustion--was the most efficient heat addition process . . ."

A first reading of these claimed accomplishments by the uninitiated leaves him with the belief that HRE did everything it was supposed to do, with maximum performance near to the goals

hoped for in the beginning, and with important detailed findings added such as the result of some supposedly meaningful controlled comparison of "transonic" vs "supersonic" heat additions. If, however, the AIM results are looked at without these built-in biases, we see an entirely different picture--the picture described briefly at the end of Chapter II which shows only a partially successful effort, failure to achieve efficient staged performance in the supersonic combustor, etc. (Similar slanted statements for SAM results were pointed out in Chapter II as well.)

Immediately following the final Oral Review (117) HRE personnel met with Langley scramjet personnel and reviewed the AIM results with more candor and detail than they had displayed publicly (118). In particular, the problems of combustion in the diverging duct were aired and their implications for the combustor of the Langley modular scramjet were discussed. In the final years of the project when the Project Office had dwindled down to the three principals plus one or two others, there was a good deal of time for unhappy philosophizing especially during the long delays before AIM was ready for testing in Plumbrook. Rubert and Deis in particular felt that NASA HQ had failed to provide the support "promised" when Phase II A was approved and the Contractor selected. No level of management was spared in these sessions. Some of the complaints and charges were recorded in a memo by Deis (114), in which the various past events and decisions marking the reorientation and decline of HRE are reviewed in rather bitter terms.

Thus, during its entire 9-year lifetime the HRE Project Office operated in an atmosphere of declining support and successive crises of survival starting with the termination of the X-15 and ending with the premature closing of the Plumbrook HTF. Understandably in these circumstances the principals in the Project Office developed defensive attitudes, tending to

exaggerate the merits and importance of HRE and to belittle the value of other scramjet programs. In the end it was difficult for the HRE team to give a completely objective account of the SAM and AIM research results because of their long preoccupation with the hardware procurement and their battles to make the hardware "work" in new and undeveloped test facilities. On the positive side, the long HRE history of set-backs and problems created a dogged perseverance in the team without which AIM might never have been made to operate at all.

Cost Considerations

Cost considerations have figured prominently in the preceding sections, and we will be concerned here mainly with issues not already covered. Foremost, is the question of how much HRE would have cost in total if the flight program had been accomplished. Referring to Section 15 we note that the AIM/SAM effort cost to the taxpayer was about $50 million. If the escalation factor actually experienced for AIM/SAM is applied to the remaining tasks for delivery of six flight-weight engines, we obtain $75 million as the preflight total cost. (This makes no allowance for the unknown but undoubtedly substantial added cost of continued development of AIM to achieve a successful combustor, engine redesign, etc.) The costs of the 3-year flight program now believed needed to achieve 25 successful X-15 flights for HRE would be at least 30 million. No account is taken here either of the large but uncertain costs of the 5-10 year stretchout of the X-15 operation to await the readiness of the test engines. Thus we conclude that $100 million is the very least total cost level that could be projected and $125 million would be a more likely guess--4 times the NASA costs quoted in the original prospectus.

None of the interviewees believed HRE would have been approved at such a figure--even in the free-wheeling environment of 1964.

It is now apparent that a much closer estimate of the real costs could probably have been made in 1964. Shortly after Garrett had been selected as Phase II contractor with an estimated Phase II cost plus fee of only about $19 million, Lopez was contacted by B. Pinkel of RAND. Pinkel had made studies of jet engine development costs, including ramjets such as Bomarc. His rough estimate for the program was $70 million. But fresh from his easy success with the Phase I test model, Lopez argued that HRE was a research engine different in character and less costly than the pre-production engines studies by Pinkel. Experienced industrial engine developers also seriously questioned the Government's guideline estimate, as evidenced by G. E.'s submission of nearly $40 million for Phase II alone.

It is quite obvious now that the Government's guideline rough estimate quickly assumed an importance and influence far beyond what it was worth. Clearly, a major effort to arrive at a better figure should have been made through detailed consultation with experienced engine development cost specialists.

In discussing the cost problem, Project personnel now point out that their original concept of HRE was really a flying wind tunnel model to demonstrate internal aerothermodynamics, which they still believe might have been accomplished for $15 million. However, the actual HRE was forced in the direction of a costly subscale prototype by X-15 requirements. It is also obvious now that the overly ambitious claims made in "selling" the proposal could only be fulfilled by a subscale prototype, and this was certainly a factor in the escalation.

We have already covered in Chapter I the failure of Phase I to reveal true costs as it was expected to do. In this connection it is worth noting that the Government's cost evaluators were unable to detect any falacies in the Phase I results. In its negotiations with Garrett for the Phase II contract the Government

negotiators spent two days in pursuit of a 3 percent reduction in Garrett's proposed cost; they were unable to see the explosive increases which shortly became so obvious.

By the time of the reorientation in mid-1968 the HRE team had been hurt so badly and so often by cost overruns that they overestimated the cost of the reoriented program; in other words, they underestimated what could be accomplished with the remaining funds. This had an important bearing on the choice of what course should be undertaken. The claim at that time was that only the AIM plus two structural samples could be built and tested. Actually, AIM was accomplished in spite of cost overruns of about $4 million, plus SAM, which was a much more elaborate undertaking than the two structural samples originally proposed. True, funding add-ons of $2.6 million were also provided, but this delta covers only the approximate average increase in wholesale price index during the '68-'72 time period (115). It is apparent in retrospect that with the same monies two "Hot SAM" models with some modifications and improvements could have been procured and that both the structural and aerothermodynamic programs might have been importantly enhanced thereby. As it was, with the inflated cost figures and without benefit of 'pencil-sharpening' in the fuel and control systems costs, the price of a single "Hot SAM" appeared quite prohibitive in the summer of '68.

We have seen the inability of the Government's cost evaluation and contract negotiation teams to detect serious flaws in the contractor's estimates--either when they were grossly too low at the start of Phase II A, or when they were too high at the reorientation. The underlying cause was the unprecedented and uncertain nature of the project; the engine companies themselves were doubtful of their ability to foretell HRE costs. It is most disconcerting, however, that nowhere in the evaluation or negotiation literature did the government teams admit the true facts of this situation. Starting with their evaluations of

engineering and manufacturing methods and continuing through the associated cost studies and negotiations, the impression given is one of comprehensive investigation of what was to be done and meticulous assessment of costs down to the last dollar. This misleading impression was clearly favorable to winning the Administrator's approvals for HRE, but in the longer run it was obviously a disservice.

The ability of the Government to keep track of the contractor's cost accounting during the course of the project also seems open to question. The $1.5 million spent in this area for Defense Contract Administrative Services was not a worthwhile expenditure in the hindsight opinion of Project Office personnel. The following is one example of the difficulties encountered in assessing the cost situation: At the time of delivery of AIM to Plumbrook in August 1971 the Project Office indicated that the remaining HRE funds were extremely tight and that continued addition of substantial Langley non-contract R&D monies to help support the project was essential. Over $1 million of these monies was accordingly supplied in fiscal years '72 and '73. In February of 1973 Becker asked HRE for a detailed accounting of their funding situation in order to judge the merits of HRE's request for $250,000 of Langley support in FY74. The HRE reply (122) contained some large doubtful "ifs," in particular "if Garrett can be negotiated downward" from their recent cost increase estimate "from $366,410 to $175,000" - then the project could be completed as planned. However, Deis stated, "this is a precarious position" and "if the negotiation above fails, or if added schedule slippage occurs, or if unforeseen problems occur" additional funds would be required. Garrett was allowed contract cost increases totalling approximately $366,000 for the slippages at Plumbrook, the last payment being negotiated on May 3, 1973. From then on as added slippages continued and costly new problems developed, it seemed certain that AIM was on the verge of bankruptcy. Rather than drain Langley R&D funds to bail out HRE,

Becker proposed to delete what he considered non-essential and relatively unimportant tasks from the Garrett contract, such as the final design review, final oral, and final HRE reports (123), a proposal that was denounced harshly by the Project Office. As slippage and added costs continued to mount through 1973, it seemed imminent that AIM would have to be terminated prematurely, when suddenly the whole funding problem disappeared. A call from Garrett said in effect, "no problem, we have more than adequate funds to complete AIM, present the reviews, write the reports, etc." AIM testing continued on into mid-'74 and the contract, with several added small tasks, was extended to April 22, 1975 - all at no added cost. In the end, $60 thousand was left over and returned to the U. S. Treasury!

So although the promoters of the HRE aimed in Phase I only towards an internal aerothermodynamics performance test model, the combination of the ambitious general claims made for the project and the X-15 flight requirements forced it in the direction of a much more costly subscale prototype with realistic structural and other subsystems. The total cost of an HRE program including 25 flights of the X-15 is now estimated to be $125 million or about 4 times the original guideline estimate of NASA costs. A more reasonable initial estimate could have been obtained at the close of Phase I with the assistance of experienced industry cost specialists. The Government's cost evaluation and negotiation teams were not able to detect large flaws in the contractor's estimates, either when they were too low in the Phase I studies or when they were too high at the reorientation. The inherent difficulties of maintaining accurate Government understanding and control of the contractor's fiscal operations were also apparent.

Recommendations

Recommendations based on the HRE experiences are summarized in this section rather than scattered throughout the text where supporting discussion is given.

1. Something more effective than the primitive advocacy system used in the mid-sixties for project proposals should be sought. ("Advocacy" is used here in the legal or advertising sense in which all unfavorable factors are minimized or omitted.) In a mature proposal presentation the reviewing office should require discussion of: the real mix of underlying motivations, opinions of leading figures in the field representing outside R&D interests, discussion of weaknesses and plans to add strength, coordination with related projects and plans elsewhere, alternatives if the project is not approved, and a summary of the basic reasons why, on balance, the proposal should be supported.

There is no reason why the presentor of such a candid, mature proposal cannot still exhibit the vigorous enthusiasm so admired in today's successful technical salesman. But unfortunately, few of today's proposers and project reviewers have the sophisticated attitudes necessary for implementation of this recommendation. A gradual evolution away from the advocacy practices will be required.

2. Three vital functions left up to the Project Office/Prime Contractor team in HRE should be handled differently in future R&D projects where extensive new technology must be generated:

- A carefully considered plan for broad-based participation by the most knowledgeable potential sub-contractors should be established prior to selection of the Phase II contractor to insure that the best national talents will be utilized in each critical problem area.

- When it becomes necessary to consider drastic redirection or reorientation, it is a mistake to leave it entirely up to the Project Office to define and recommend alternatives. This responsibility should, rather, be placed with an Intercenter Review Team, with

participation of both the Project Office and selected outside consultants. The main questions should be, "What is the most cost-effective way to achieve the technology goals as we understand them now?", and not, "How can the project be redirected and still adhere to the concepts, objectives, and project image laid down several years ago?"

- If the Project approach is to be used in the future to acquire complex test hardware systems, the test operations, data analysis, and reporting of the findings should be the responsibility of independent researchers rather than the Project team. This is desirable because the strong biases which grow in project teams during the course of hardware development make it difficult for them to conduct the research and report the results in an objective manner.

3. The following more specific (and in some instances rather obvious) recommendations were derived from specific deficiencies in the HRE project apparent by hindsight:

- The Government's initial guideline cost estimate has an overriding influence on the initial approvals of a project and on subsequent development planning. It should be established firmly with the aid of experienced specialists in engine development costs from the Industry, non-profit groups, and selected senior managers in the Government. If a Phase I study is to reveal true costs, rather obviously the contractors must not be influenced by the Government's estimate.

- Major weaknesses or inexperience in an evaluation team or Project Office should be compensated by use of outside specialists to the maximum extent legally permissible.

- The head of each panel of the Phase I evaluation team should be required to submit a recommendation on whether his panel considers it desirable to proceed with Phase II.

- Technology demonstration engines should be balanced designs rather than designs slanted to favor any one parameter such as internal thrust performance.

- Fundamental design problems such as the question of how to obtain efficient staged fuel injection in a diverging combustor should be solved in component development programs before commitment to complex inflexible complete configuration models, regardless of pressures to meet project schedules or costs.

- In a design like HRE where strong reflecting shocks from the cowl lip play a dominant role in the combustor, simulation of the shocks should be included in the combustor component development program. In particular, the location of fuel injectors relative to the points of shock impingement may be critical. Instrumentation to observe and survey the flow within the combustor should be provided.

- The possibility of eliminating the need for staged fuel injection by proper selection of inlet Mach number, injector locations, use of boundary layer removal, etc., should be examined in future designs.

- Aerothermodynamic wind tunnel test engines on the order of 1 foot diameter, and designed for easy modification are preferable to large inflexible models like AIM. Simple metal heat-sink structures should be used instead

of the thick-walled water-cooled nickel type of construction used in AIM.

- Strong OAST management direction should be applied when a project requires cooperative intercenter activities like the AIM Plumbrook/Lewis/Langley program.

4. Recommendations relating to Scramjet demonstrations in flight: Looking ahead we note that scramjet technology for all of its progress in the past 18 years is still not ready to support a direct application to an operational system. Before that could happen one or more preprototype experimental engine systems based on modern integratable concepts will have to be undertaken and demonstrated. Previous engine developments teach that such a step is costly, but indispensable in the development process. While this could be entirely ground-based like some of the previous programs, it is likely to be better focused and more valuable if the demonstrations include actual flight testing of the kind contemplated in recent USAF/NASA studies. Such a program would in effect be a latter-day HRE in which the ambitious original goals are finally realized. HRE experience teaches:

- Establishment of the basic concept and preliminary development of its components should be accomplished in a general pre-project program prior to the start of a flight engine procurement.

- The entire venture should be a national enterprise rather than a parochial promotion of a particular research center.

- The program should be designed throughout to enlist the active participation of selected outside specialists drawn from the Universities and Industry.

- A small working advisory group of perhaps 5 or 6 nationally established propulsion consultants should be considered as an aid to program managers.

Concluding Comments

The results of this study, which have been summarized previously at the end of each section, reveal it to be an unusual record of a major NASA R&D undertaking. The bulk of the study is documented technically-oriented history, supplemented by hindsight critiques and consensus opinions obtained from many individuals relating to the project in widely differing roles. While there is no expectation, of course, for universal agreement on all of the judgments expressed in the study, it should be interesting and useful to historians, students of the R&D process, and to prospective project personnel.

HRE was the victim of unusual times. In the beginning the inflated technical optimism of the early sixties resulted in over-estimation of technology readiness, and failure to examine in depth the difficult problems and high costs of combining an elaborate new flight engine with a complex hypersonic research airplane. Agency approvals were granted too easily with little depth of conviction and no real commitment on the part of high-level managers. Later, in the economic retrenchment necessitated by the war in Viet Nam, the approvals so readily given were just as readily withdrawn; the X-15 program was terminated, and HRE degenerated to a costly wind-tunnel program using partial-simulation test models.

Substantial advances in scramjet technology were accomplished in the HRE wind tunnel programs. These have been summarized in detail in Chapter II and need not be repeated here. The majority of those interviewed in this study believes as I do however, that these values were purchased at unjustifiably high costs. If a

thorough agency review of the HRE situation at the time of X-15 cancellation had been made, the consensus opinion is that HRE would have been terminated as a project and its technology problems taken up more effectively in the general R&D programs. A minority opinion exists to the effect that some of the technology generated by HRE--for example the SAM structural contributions--could not or would not have been realized except through the Project approach.

Finally, attention must be given to a special problem of hindsight study that became obvious as this work progressed. It became very clear that an objective, candid inquiry into a project which progressively degenerated almost from the start, has a quite different character than the study of successful projects. Inevitably in the HRE study we were dealing heavily in unpleasant facts--the inflated initial claims, the gross underestimation of technology problems and costs, termination of the flight vehicle, interminable delays, etc. Negative events also occur, of course, in the successful projects, but their impact is softened by the final success.

This distinction between successful and less-than-successful projects has a very important practical consequence: only the studies of successful projects are likely to receive significant circulation. One of the strongest instincts of any bureaucracy is to accent only its successes and to bury its mistakes and failures. It is obvious, therefore, that hindsight studies should be sponsored by an office which has no direct responsibility for the field of work under consideration.

Appendix A

Summary of HRE Costs

Phase I Contracts:			$1.6M
Facility Support, Test Fees, Instrumentation:			$8.1M
Ord. Aerophysics Lab.		$2.7M	
Lewis Plumbrook HTF		2.8M	
Langley 8 ft. HTST		1.5M	
Ramjet Instrumentation		1.1M	
Phase II A (Garrett) Contract:			$20.7M
AIM		$8.2M	
Inlet (orig. est. $.6M)	$1.7M		
Combustor (orig. est. $.2M)	2.0M		
Nozzle (orig. est. $.3M)	.3M		
Fab., Tests (orig. est. $.9M)	4.2M		
SAM		$7.0M	
Subscale Tests	$1.6M		
Fab. Techniques, Dies, etc.	1.4M		
Hardware Dev., Tests	4.0M		
Subsystems, X-15 Mods, (not completed)		$4.0M	
Project Management		$1.5M	
Defense Contract Adm. Services:			$1.5M
Center Manpower:			$7.2M
Langley	$3.2M		
Lewis	3.3M		
Flight (Dryden)	.5M		
Ames	.2M		
USAF & NASA Facilities Not Requiring HRE Fees:			$2.5M
Misc. Plus R&D Funds Not Included Above:			$1.7M
X-15-A2 Repairs, Conversion for Mach 8 Propulsion Testing, Drop Tanks, Ablator, Dummy HRE, Build-up Testing:			$7.5M
Total HRE Cost			$50.8M

Note:

The cost of HRE is usually cited in agency literature as $20.7 million; the value of the Garrett contract as shown above. The principal intent of this cost summary is to assess the actual total cost, when the many other expenditures, hidden and otherwise, which can logically be charged to HRE are included. Some of these charges were hard to determine accurately (Center manpower for example), but nevertheless it is apparent that the overall cost was about $50 million.

Appendix B

Selected Views and Opinions on the NASA HRE Effort

This appendix was added at the suggestion of a reviewer to provide further details of my involvement with HRE, and to give some selected individual opinions which differed from the consensus.

Hypersonic propulsion aerodynamics at Langley was concentrated in a Branch of the Aero Physics Division. As Branch Head and Division Chief, Dr. Rubert and I first became involved with HRE in the pre-project period when the question of using the X-15 for propulsion research came up. Rubert and I had worked together in this relationship for many years and usually reached harmonious agreements on issues of this kind. In this case we both believed that an X-15 program would be unwise. However, our views underwent an about-face in early 1963 when the Flight Research Center first proposed that Langley should manage a major R&D program to obtain a sophisticated test engine. For about one year or until June 1964 when Phase I was approved, I worked closely with Rubert in the normal relationship of Division Chief and Branch Head on all aspects of project definition and promotion. Thus any criticisms of what was done in this period apply equally to myself as well as to the others.

Neither Rubert nor I had had much experience with large R&D procurements of this kind and Director F. L. Thompson decided initially that the project would be managed by an experienced systems engineer, J. H. Parks, reporting to Assistant Director L. K. Loftin, Jr. A few months later Parks received another assignment and was replaced by Rubert. To aid in overall direction of the project Loftin set up a small advisory group of which I was a member, but the group ceased to function after a few meetings. Thus for all practical purposes I had no direct management functions or control over the project during the 5-year

period from early 1965 to 1970. During this time my division provided considerable developmental testing, consultation, participation on evaluation teams, etc.,--the same type of support functions we routinely supplied to many Langley projects.

A special assignment of mine during the May 1965-early 1967 period which had a strong interface with HRE should be mentioned. I co-chaired an ad hoc NASA/USAF team set up to coordinate the hypersonic technology programs of the two agencies, and HRE was of course a major element of the NASA program. I have described the activities of this group in the text, but I wish to call attention to the fact here that my pro-HRE letter of January 1967 (61) might have influenced OART decisions to favor the project, although I had no part in any of the actual decision-making in the '65-'70 period.

My attitudes towards the HRE changed radically during the '67-'70 period. Our understanding of scramjets had been greatly improved by our connections with HRE. As we looked broadly at the problems of scramjet engine installations we could see more clearly as time went on that the HRE pod with its zero-net-thrust characteristic and shallow annular flow passages was not likely to have much pertinence to future scramjets. At the same time it was difficult to obtain 250-500 thousand dollars per year for our general R&D program - in contrast to the millions going into HRE hardware. Experience with the more advanced engine concepts in recent years has confirmed our beliefs that took shape during the '67-'70 period; HRE performance data has not proved very useful in the succeeding programs.

In 1970 when Langley management asked me to provide a home for HRE in its final years in my division, I agreed, with the definite understanding that there would be no attempts to extend the project beyond the projected SAM and AIM wind tunnel tests. By that time the major funding had been committed. Nevertheless

each year it was the practice of the Project Office to seek substantial supplemental support from the limited general research funds of the propulsion program. I insisted that the need for these added funds should be assessed relative to the many other needs of the general program. Rubert thus found himself in an adversary relationship with the Branch heads of our Division, a situation which he did not accept without protest. Several times he explained his philosophy to the effect that a project, which had been approved by Congress as an identifiable item in the budget, should not have to compete with general research - even in this case in which it sought to use general research funds. Considering the extent to which HRE had degenerated from the original plan and the major faults which were now apparent, it seemed to me that Rubert's aggressiveness at this late stage was quite unwarranted. Nevertheless, I realized that throughout the project he had done his best as a vigorous manager, and I was instrumental in obtaining a substantial Langley award for him before his retirement.

There were a number of important individual opinions which differed in varying degree from the consensus. To complete the picture, I have used my notes and tapes to paraphrase as accurately as I could the pertinent comments of five principal participants in HRE as follows:

A. duPont (HRE designer and first HRE Project Engineer for Garrett)

Behind the scenes in the early sixties he knew of a large number of skeptics who believed efficient supersonic combustion was probably unattainable and that scramjet would not prove to be a viable concept. They were not vocal like the enthusiasts and in some cases they were asked to suppress their views, but nevertheless they were influential. A substantial flight engine program was therefore needed in the mid-sixties to prove

dramatically to top management that scramjet was a real system of great potential, and HRE fulfilled this need to a large extent even though it never flew. He considers whatever it cost a cheap price for all of the technology that HRE developed. He agrees that use of a small working advisory panel of experienced engine developers and consultants such as combustion specialists would be desirable in future projects of this kind provided they are not required to perform management functions.

H. Lopez (Second Garrett HRE Project Engineer)

He considers their structural accomplishments in HRE more significant than the aerothermodynamic. A principal value of AIM was to highlight a "cliff-type" problem, namely the inherent difficulties of staged combustion in a diverging combustor. The major overall value of HRE was in "putting it all together," proving for example that control systems can be designed which are compatible with combustor requirements, and revealing for the first time that the total set of systems comprising a real scramjet can be put together successfully as in integrated engine. Their design and test work went far enough to prove this even though a complete engine was not actually built.

F. K. Rubert (HRE Project Manager for NASA until 1973)

He is proud of the original HRE plan and the work statements which he authored and sees little that he would change. He realized there would be problems in combustor development - the statement in the RFP to the effect that component technology was sufficiently far advanced for applications did not mean to imply there were no remaining tasks for development.

He considers it foolish to belabor minor questions like the above when there were glaring faults in the conduct of the project which include in his opinion:

- NASA top management approved the project, but then did not follow through with the resources necessary to accomplish it.

- NASA HQ and Lewis managements did not keep Plumbrook operational long enough to permit adequate AIM testing. He believes efficient combustion in diverging combustor will prove possible and that it requires injection locations and fuel flow distributions which maintain a constant or rising pressure, rather than injection in regions of falling pressure. Additional AIM testing and possibly additional injector configurations should have been employed to prove this postulate.

- NASA researchers failed to follow up with 2-dimensional or other combustor programs to answer the vital questions highlighted by AIM.

- Declassification of HRE eighteen months after his retirement was a mistake. He had always insisted on strict confidentiality of basic duct lines and test data, information which he considers vital to the United States. He wants it known that he is "very bitter" about this.

In regard to the use of experienced engine developers, combustor specialists, etc., as consultants to supplement inexperience in the Government project teams, he feels that this is desirable and was in fact actually done to the extent needed in HRE - through his personal contacts with a wide circle of acquaintances in the propulsion field.

E. A. Mackley (HRE Project Manager for NASA, 1973-1975)

Projects are able to command higher levels of funding and they operate with higher pressure and faster schedules than the

general research program. Thus they can deliver substantial R&D accomplishments which would be impossible or long in coming in the general program. An obvious example is the SAM venture which in all probability would never have been undertaken by structures research groups. In any major review or redirection the Project Office should be given the opportunity to state its case and make recommendations.

He agrees that a more broad-based national participation in HRE would have been beneficial. However, he would like to see active specific working participation rather than general consulting.

By hindsight, AIM testing problems and costs could have been significantly reduced by using a 2/3 scale model. In regard to additional testing of AIM in Plumbrook, the first requirement would be to rebuild the heater to provide Mach 7 total temperatures. With the higher temperatures the problems in the diverging duct might be alleviated, but whatever the result, additional understanding would be obtained.

He believes Langley is correctly using its limited resources to work the combustor problem of the airframe-integrated scramjet directly, rather than to pursue additional AIM testing or 2-dimensional combustor testing.

Compared with Space projects he believes HRE costs were not out of line, although he agrees that many opportunities for major cost savings are evident by hindsight.

Finally he emphasizes that the technology developed and team experience gained in HRE were invaluable in establishing the present advanced scramjet program.

A. Ferri (Former head of the Aerospace Department at NYU, President of GASL, and chief prophet of the scramjet cult of the sixties.)

The NASA teams were inexperienced in scramjet. They did not understand and therefore did not trust advanced concepts based on the most sophisticated level of technology existing in 1964. Thus NASA was comfortable with a contractor who also had only a rudimentary understanding of scramjet and who offered an apparently simple and easily understood but an unimaginative concept. The Work Statement demanded an engine that was too complex--too much like a prototype with all flight subsystems represented requiring far more money and time than was available. However, if NASA had been willing to build on the technology which already existed for his engine scheme and had concentrated on a simple aerothermodynamic performance demonstration using boilerplate structures, he believed a useful flight program might have been accomplished within the time and cost limits originally suggested.

In regard to the SAM tests he felt this type of structural demonstration was valuable but it could have been made at any time for any of the engine concepts and did not need to be a flight program. He considers SAM the only positive accomplishment of HRE. Everything else is valuable only as an example of what not to do. He had predicted that AIM would suffer from the focused duct shocks and the other problems actually observed. The one-dimensional analysis of AIM data made by the Contractor is too crude and could be greatly improved upon but no one would want to undertake this because the axisymmetric engine with its shallow annular duct, large wetted surfaces, high internal friction and heating, and unavoidable external drag is of little interest.

The present* Langley propulsion program is built on the principles he espoused in 1964, but it is too slow moving and in the end will cost more than a vigorous adequately funded program.

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*1976 - ed.

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I was in an unusually favorable position to undertake this HRE study, having observed at close range nearly all of the principal events as they actually unfolded, while at the same time being free from HRE management responsibility during the critical first 5 years of the project when most of the major decisions were made. My retirement from NASA in 1974 afforded time and added perspective for the work.

An indispensable ingredient in any contemporary history, especially where hindsight opinion is involved, is a canvass of the views of the principal living participants. To this end I interviewed 35 key individuals and acknowledge here both the valuable factual data and the important opinions they contributed to the study. With only a single exception, the interviewees were sympathetic with the aims of the study. I would acknowledge, especially, the very extensive and valuable help of E. A. Mackley, HRE Project Manager from 1973 to 1975.

After completion of this report, NASA suggested that further details of my involvement with HRE and brief resumes of the opinions of certain other individuals involved with the project be included. This information has been added as Appendix B.

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CASE VII

THE PILOTED LIFTING BODY DEMONSTRATORS: SUPERSONIC PREDECESSORS TO HYPERSONIC LIFTING REENTRY

by

Richard P. Hallion

John L. Vitelli

EDITOR'S INTRODUCTION

This case study is a study on the pursuit of body shapes for hypersonic flight. The notion of using lift generated by the body shape of a flight vehicle is generally credited to Vincent J. Burnelli, a noted aviation pioneer, who developed and flew a family of propeller-driven designs in the 1920s through 1940s that could best be described as combined flying wings and lifting bodies. These subsonic aircraft excited a great deal of interest, but were not destined to see general acceptance or large-scale production.[1] The hypersonic lifting body concept, derived from a different set of assumptions involving tailoring nose cone shapes, and dating to the mid-1950s, proved attractive to proponents within the National Aeronautics and Space Administration, the United States Air Force, and the aerospace industry, and this enthusiasm was subsequently translated into actual hardware in the form of three distinct technology programs: the M2 lifting body family, the HL-10, and the X-24A, the latter a "spin-off" of the hypersonic SV-5D effort. Subsequently, the little X-24A evolved into the graceful X-24B, the last American rocket-propelled research aircraft to fly. It, in turn, gave rise to a brief attempt to build a hypersonic lifting body, the so-called X-24C. That project, in continuing evolutionary fashion, became caught up and eventually overwhelmed by the abortive attempt to build a joint NASA-USAF "National Hypersonic Flight Research Facility," the NHFRF, canceled in 1977. While the lifting body per se did not materialize for the ultimate Shuttle, the concept was a serious contender for such a configuration for quite a while, and the aircraft mentioned above played a significant part in influencing Shuttle capabilities and operations from a "pilot in the loop," human factors, and approach and landing standpoint.

Interestingly, none of the above were capable of hypersonic flight themselves, though their shapes had all been tested at hypersonic speeds in various ground facilities, and found to possess satisfactory characteristics. They were, rather, subsonic, transonic, and supersonic technology demonstrators built to assess the performance of hypersonic configurations at speeds from approximately Mach 2 down to landing, and from approach altitudes of up to 90,000 feet. Though none spent a second at Mach 5 or anywhere near it, they are a vital part of the hypersonic story, and their development history--often involving outright attempts to cut through existing bureaucratic, acquisition, and funding heirarchies--are instructive, particularly on how dedicated and resourceful teams can often achieve the unexpected in the face of organizational inertia and numerous unknowns.

The text of Chapters I and II is, in the main, drawn from Chapter 8 of the author's previously cited history On the Frontier, though greatly elaborated and expanded upon. A portion of Chapter II had been adopted (with editorial changes) from John L. Vitelli's previously cited history of the PRIME program, and thus he is listed as a co-author of this study. I wish to acknowledge with appreciation the assistance of the staff of the Air Force Museum Research Library, the Flight Dynamics Laboratory (particularly Alfred C. Draper), the NASP Program Office (particularly Frank Boensch), the Office of Advanced Manned Vehicles, Air Force Flight Test Center (particularly Johnny Armstrong and Bob Hoey), and finally, the staff of the Hugh L. Dryden Flight Research Facility (particularly Ralph Jackson, Jack Kolf, Wen Painter, and Milt Thompson).

NOTES

1. I am grateful to Chalmers "Slick" Goodlin, Chairman and President of the Burnelli Company, Inc., for having supplied me with comprehensive information concerning the work of Burnelli on the lifting body principle. For a definitive survey of Burnelli's work, see a five-part series by Howard Levy and Richard Riding, "Burnelli's Lifting Fuselages," Aeroplane Monthly (March-July 1980), and Howard Levy, "Burnelli Lifting Fuselage Projects," Aeroplane Monthly (October 1980).

CHAPTER I

THE M2 AND HL-10: NASA'S APPROACH TO LIFTING BODY TECHNOLOGY

The conclusion of the SV-5D hypersonic reentry study program roughly coincided with the onset of a comprehensive flight testing program to assess the supersonic, transonic, and subsonic characteristics and performance of three "rival" lifting body shapes: the NASA/Northrop M2, NASA/Northrop HL-10, and the Air Force/Martin X-24A. Of the three, the X-24A owed its existence to the PILOT project within the earlier SV-5/START program effort. Subsequently, it helped spawn the much-different X-24B effort which, in turn, led to serious examination of a proposed X-24B growth vehicle, the so-called "X-24C," for actual hypersonic flight trials. While each of these configurations differed from that adopted by the subsequent Space Shuttle Orbiter, the flight testing experience gained with them benefitted the knowledge base required for developing the Shuttle. Overall, these three lifting body programs, the X-24B which followed, and the general rocket-propelled research aircraft experience ranging from the X-1 to the X-15, convinced Shuttle developers that the Shuttle itself would not require or benefit from so-called "landing engines;" with proper energy management techniques, a lifting reentry logistical spacecraft could return to earth in unpowered fashion, spared the complexity that an auxiliary propulsion system would introduce.

Two camps of reentry thought existed in the early 1960s: those favoring a ballistic or near-ballistic plunge through the atmosphere _a la_ Project Mercury or the Soviet _Vostok_ vehicles, and those favoring some sort of lifting reentry using a purely winged vehicle, some sort of lifting body, or a hybrid such as a lifting body deploying a variable-sweep wing at low altitudes. A minority considered more far-fetched schemes such as combining a ballistic

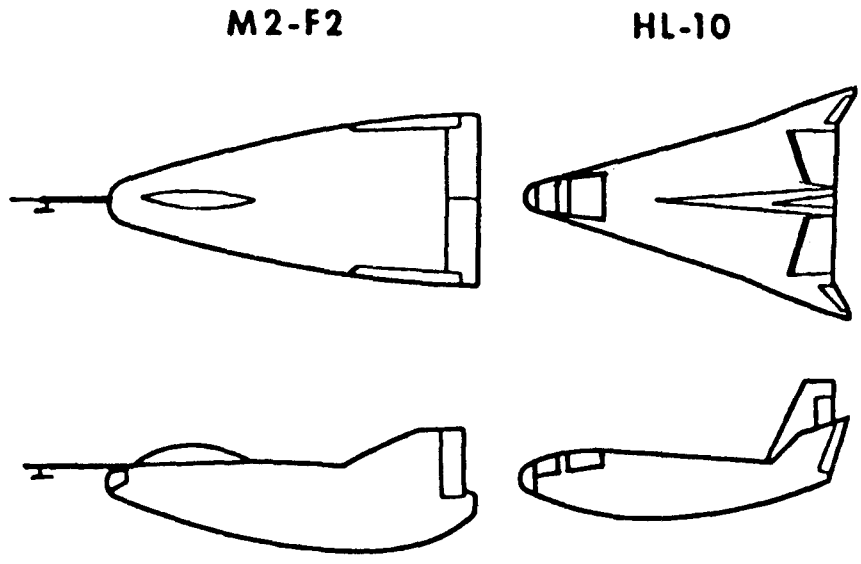
reentry shape with a stowed Rogallo flexible wing (as was unsuccessfully pursued within the Gemini program), or combining a ballistic reentry shape with a stowed rotor that could deploy, converting the craft into an autogiro for landing. But these latter ideas faded fast amid the realities of the 1960s, and the lifting reentry field remained dominated by the lifting body on one hand and the winged vehicle on the other.

The lifting body achieved popular acceptance as largely a product of the NACA-NASA, but as the earlier case study of the SV-5D effort shows, the actual lifting body story is more complex, and involves individuals and organizations not only within NACA and NASA, but within industry, the Air Force and DoD community, and academic world. For convenience, it is less confusing to discuss the programs separately than it is to attempt a coherent discussion of all at once, for the programs overlapped and basically broke down into those sponsored by two "camps:" the NASA-sponsored M2 and HL-10 vehicles, and the Air Force-sponsored X-24A, X-24B, and the ill-fated X-24C and National Hypersonic Flight Research Facility (NHFRF).

M2: The Early Days

The M2 and HL-10 (Figure 1) lifting bodies dated back to the blunt-body studies of H. Julian "Harvey" Allen, an imaginative engineer at NACA's Ames Aeronautical Laboratory. Allen conceived the blunt body theory in 1951. Together with Alfred Eggers, Allen concluded that a ballistic missile warhead having a blunt, rounded nose (as opposed to a pointed shape) would better survive the intense heat generated as it entered the atmosphere from space at near-orbital velocities. The blunt shape produced a strong, detached bow shock wave that, in effect, gave the following warhead excellent thermal protection. Allen's work remained highly classified, but the fruits of it appeared on the Atlas missile's deadly nose.

Figure 1



BODY PLANFORM AREA, FT^2	160	160
BODY SPAN, FT	9.5	14.5
BODY LENGTH, FT	22	22
LANDING WT, LB	6150	6400
LANDING WING LOADING, LB/FT^2	38	40

GENERAL ARRANGEMENT OF THE M2 AND HL-10 LIFTING BODIES

Necessarily the blunt body had a very low lift-to-drag ratio, far less than 1. It flew a ballistic descent path having a minimal "cross-range footprint." Allen and Eggers, together with Clarence Syvertson, George Edwards, and George Kenyon, recognized that designers might be able to combine the blunt body with a manned orbital vehicle in such a way that it had an acceptable lift-to-drag ratio, on the order of 1.5. This could reduce reentry g loadings from the 8 g experienced by a blunt body to 1 g and give a cross-range footprint in excess of 2400 kilometers from the initial point of atmospheric entry. Eggers deduced that one desirable shape for such a vehicle would be a modified half-cone (flat on top) with a rounded nose to reduce heating. Working at Ames, Eggers, Syvertson, Edwards, and Kenyon refined the concept in 1958, deriving the M2 configuration, a 13 degree half-cone with a rounded nose having a lift-to-drag ratio of 1.4 at hypersonic speeds. At subsonic speeds, however, its woefully inadequate stability characteristics made it prone to tumble end over end. Eventually the Ames engineers "boat-tailed" the top and bottom of the shape, giving it an airfoil cross-section and curing most of the stability difficulties. This final M2 version had a protruding canopy and twin vertical fins--the fins earning it the sobriquet "M2 Cadillac." By 1960 the lifting body work at Ames was far from fruition, but engineers had chosen a basic shape.

Ames was not the only NASA center engaged in lifting-body studies. The High-Speed Flight Station* did not have the hypervelocity tunnels, guns, and shock tubes needed for such research, but the staff kept in touch with colleagues at the larger centers and were aware of what was going on. One night over drinks at the Antelope Valley Inn, Walt Williams, Director of the High Speed Flight Station, prophetically suggested to Eggers

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*The High-Speed Flight Station became the NASA Flight Research Center in 1959 and, in 1976, the NASA Hugh L. Dryden Flight Research Center.

that the HSFS could build a piloted M2 shape for low-speed stability and control tests, launching it from a B-52. The HSFS engineers would make their own contributions soon enough, originating the flight-test programs for the lifting bodies. At Langley, engineers favored a more traditional approach over sawing a cone in half. They opted for modified delta configurations. Eventually, as a result of the work of Eugene S. Love, Langley devolved the shape for the HL-10 (HL standing for Horizontal Lander). It first appeared on Langley drawing boards in 1962 as a manned lifting reentry vehicle. In sum, then, the Ames M2 and Langley HL-10 shapes were all outgrowths of the same climate of research that had created the Dyna-Soar program; their roots were in "Round Three" thinking.

Robert D. Reed, Research Division engineer with the NASA Flight Research Center, was fond of building flying models. While recognizing that models are limited in the range of information they can return, he knew they could validate basic stability and control characteristics of a new configuration. Reed had followed with interest the Ames work on the M2, noting that while it had potentially excellent hypersonic characteristics, doubts existed that the M2 could successfully fly to a landing because of difficulties in handling at transonic and subsonic speeds. Other NACA engineers had suggested in the 1958 HSFS research assessment that NACA develop low-speed testbeds of proposed hypersonic shapes to determine their landing behavior. In February 1962, Reed built a 24-inch model of the M2, which he launched from a larger radio-controlled "mothership" having a 60-inch wingspread--a typical FRC approach scaled down in size. Reed's wife filmed some of the flights to show center director Paul Bikle, deputy director De Beeler, and Alfred Eggers. Reed also flew small lifting body models down the corridors at FRC, causing raised eyebrows among skeptics. But Eggers promised the use of wind tunnels at Ames, and Bikle authorized a six-month feasibility study of a cheap, manned, lightweight M2 glider, the "next step" suggested by Reed--who also flew sailplanes as a hobby.[1]

In September 1962 Bikle authorized design and construction of a manned M2 glider. Victor Horton headed the effort, assisted by Dick Eldredge and Dick Klein. FRC engineers built the tubular steel structure, and Gus Briegleb of the Sailplane Corporation of America built the plywood outer shell. At first, Reed, Horton, Eldredge, and Klein wished to test various lifting body shapes, including M1, M2, and a lenticular "flying saucer" concept. The M2 seemed the most practicable, however, and was the only one the FRC proceeded with. Technicians set aside floor space in a hangar, walled it off with canvas, and put up a sign reading "Wright's Bicycle Shop." The project team drew on many other FRC staffers for assistance, especially the large local NASA community of aircraft "homebuilders," mostly members of the Experimental Aircraft Association. Bikle ran the project out of local funds on a nickel-and-dime basis, because he feared he could not secure Headquarters' support rapidly enough to permit a quick development program. Bikle's concern over complicating the project by working through the system was well founded: one major aircraft company informed the FRC M2 team that it would have cost $150,000 for the firm to build such a vehicle. By using in-house funding and exacting cost control, FRC engineers kept expenditures on the design and fabrication of the M2 glider, including support, beneath $30,000. Briegleb's own construction team, consisting of three mechanics and a draftsman working at El Mirage Dry Lake, built the mahogany plywood body shell (.9-inch thick) in 120 days. The FRC team had stipulated that the body shell weigh less than 300 lbs; Briegleb's team managed to complete it at 273 lbs.[2]

The FRC-Briegleb team finished the M2 glider, which the FRC designated the M2-F1, early in 1963. A tubby vehicle, it measured 20 feet long and 10 feet high, with a width of 13 feet. It had two vertical fins, just like the earlier Ames "M2 Cadillac" study, and stubby elevons were mounted on the fins. The body had trailing edge flaps for trimming purposes and landing gear wheels from a Cessna 150 airplane. With its pilot, the M2-F1 weighed

1138 lbs. The pilot sat under a large bubble cockpit; though at first the craft had no provisions for emergency ejection, the FRC later added a lightweight Weber rocket-propelled zero-zero seat. Later the craft also had a 24-lb-thrust solid fuel rocket developed by the Naval Ordnance Test Center at nearby China Lake to assist in the prelanding "flare" maneuver if this became necessary. The craft was trucked to Ames for low-speed tunnel testing in the 40 x 80 ft tunnel. The tunnel tests, completed in March 1963, were very encouraging. NASA project pilot Milt Thompson often sat in the cockpit of the M2-F1 during the studies, "flying" the rigidly mounted craft in the cavernous maw of the full-scale tunnel. Satisfied, Ames gave the shape its blessing, and FRC took it back to Edwards in preparation for its first flights, a series of Paresev-like ground tows.

Strange enough already, the M2-F1 program now took a real turn toward the bizarre. Obviously, the shape had a lot of drag, requiring a tow vehicle with great power and speed. NASA's general-purpose trucks and vans just could not do the job; a specialized, high-performance tow car was needed. The solution did not take long. Out in the desert lived a number of racing aficionados, many of whom worked at FRC. After consulting with them, the FRC M2 team bought a stripped-down Pontiac convertible with the largest engine available, a 4-barrel carb, and a 4-speed stick shift, capable of towing the M2 to 110 miles per hour in 30 seconds. Then the team turned it over to "funny car" expert Mickey Thompson's shop in Long Beach, where technicians fine-tuned the engine, added rollbars, installed radio equipment, turned around the right passenger bucket seat to face aft, and removed the rear seats, installing another bucket seat for a second observer facing sideways.

Fearful lest a critic hastily conclude that this was somebody's private toy paid for with government funds, the team quickly painted "National Aeronautics and Space Administration" on

the sides and sprayed the hood and trunk high-visibility yellow, like any other flight-line vehicle. The NASA engineers added a tow rig and some airspeed measuring equipment, and then took it to the Nevada desert, with its (then) anything-goes speed limit to calibrate the speedometer--just like any other research airplane. Team members fondly recollect the strange head-shaking stares of California and Nevada highway patrolmen as the exotic auto rumbled along, driven by Walter Whiteside, engine exhausts roaring. Its gasoline mileage wasn't good--just six kilometers per gallon. Finally, by the spring of 1963, all was ready. Milt Thompson ventured out on the lake, the M2-F1 rigged behind the Pontiac on a tow line for its first excursion into the air.[3]

The M2-F1 completed its first ground tows on April 5, 1963 and made 45 others by the month's end. From then until the first air tows, the little lifting body made over 100 tows, an accumulated air time of nearly four hours. Generally speaking, the M2-F1 had acceptable flying qualities, warranting its being air-towed to altitude and released; but Thomas Toll, FRC's chief of research and one of the men responsible for the X-15 concept, had serious misgivings. He became especially concerned after Thompson's first flight, when the pilot encountered a dangerous lateral oscillation. Nevertheless, Bikle went ahead and approved air tows. FRC had a Douglas C-47 "Gooney Bird" assigned for general duties. The C-47, the military version of the legendary DC-3, had been an excellent glider tug during World War II in such campaigns as Sicily and Normandy. Vic Horton of the FRC's M2 team scrounged up a C-47 tow mechanism from a junkyard. The team installed it on the plane, and on August 16, 1963 Milt Thompson piloted the little lifting body as the center's C-47 towed it off the lake. On this and other flights, the C-47 generally climbed at about 120 miles per hour to over 10,000 feet, the M2-F1 trailing on a 1,000 foot towline. The towplane would release the glider above its intended landing spot on Rogers Lake, and Thompson would guide the rapidly sinking craft to a touchdown about two minutes after release,

landing at 85 to 90 miles per hour. On September 3 FRC unveiled the craft to aviation news reporters. The lifting body concept at once became a hot journalistic item.[4]

The first flights of the M2-F1 had proved that the lifting body shape could fly. As early as mid-April 1963, Bikle was convinced enough to bring NASA headquarters into his confidence. He told Milton Ames, NASA's director of space vehicles: "The lifting-body concept looks even better to us as we get more into it. We also recognize a rising level of interest in the concept at Ames and at Langley."[5] There was a rising level of interest on Capitol Hill as well, as word got back to Washington. By mid-April 1963, many congressmen were quizzing NASA headquarters officials on the M2 flight program, and causing consternation among some Department of Defense officials who apparently had no idea that the M2 was flying. Some congressmen feared the low-budget M2 might soar overnight to a major multi-billion-dollar post-Apollo development program; others later suspected that the program was a way for NASA to circumvent the decision to cancel Dyna-Soar. Hugh Dryden and OART's Raymond L. Bisplinghoff defended the FRC effort, and the M2 program continued.[6]

At Edwards, seven other pilots checked out in the airplane; NASA test pilots Bruce Peterson, Donald Mallick, and Bill Dana; and the Air Force's Chuck Yeager, Capt. Jerauld Gentry, and Lt. Col. Donald Sorlie. Colonel Yeager clambered out of the craft after his first flight exclaiming "She handles great!" He hoped to use similar vehicles, powered by small jet engines, as lifting body simulation trainers at the Aerospace Research Pilots School, which he commanded.

Eventually the little M2-F1 completed approximately 100 flights and 400 ground tows before retirement to the Smithsonian's National Air and Space Museum (it is now held in storage at Dryden for the museum). FRC did have to make some

modifications to the craft. On one flight, NASA's Bruce Peterson landed with sufficient force to shear off the landing wheels, and the M2-F1 sustained minor damage; during the tow to altitude, the automobile-type shock absorbers had become chilled, and the cold hydraulic fluid simply failed to function properly on touchdown. NASA replaced the Cessna 150 landing gear with more rugged gear from a Cessna 180. On two other flights, Jerauld Gentry became involved in some extremely hazardous rolling maneuvers. On one occasion, Vic Horton glanced out of the C-47 in time to see Gentry and the M2-F1 rolling inverted on the towline; for several seconds, the launch crew in the C-47 did not know if the errant lifting body had ploughed in. When they next saw it, however, it rested safely on the lake: Gentry had cast off, stabilized the M2-F1, flared, and landed--just another close call. Not wishing to take further chances, Bikle shut down the M2-F1 program. It had served its purpose: it proved that the lifting body shape could fly and encouraged further research with supersonic, rocket-powered lifting bodies, to determine if the shapes so desirable for hypersonic flight could safely fly from supersonic speeds down to landing, through the still tricky area of transonic trim changes. When the tubby M2-F1 completed its last air-tows in August 1964, work was already well along on two "heavyweight" aluminum follow-ons--the M2-F2 and the HL-10, both Northrop products.[7]

Establishing a Joint M2 and HL-10 Lifting Body Program

With the encouragement afforded by the M2-F1, FRC pressed forward on its lifting body studies, which eventually led to the Northrop M2-F2 (and later the M2-F3) and the Northrop HL-10. Air Force interest resulted in formation of a joint NASA--Air Force lifting body program. The Air Force Flight Test Center and the NASA Flight Research Center issued a Memorandum of Understanding on the program in April 1965.

Early in 1963, as the M2-F1 took shape at Edwards and El Mirage, Dale Reed's M2 team had preliminary studies under way on an air-launched, "mission-weight," rocket-propelled, Mach 2 lifting body using off-the-shelf systems and equipment. This research vehicle, informally dubbed "Configuration II," could return useful information on the supersonic and transonic behavior of such craft, piloting problems and workloads, and approach and landing characteristics of a "mission weight" lifting body. The earlier lightweight M2-F1 had a "wing loading" only 1/5 of that expected with a fully developed and operational space-rated lifting body. Oddly, NASA recognized from the outset that the lightweight lifting body would be considerably more difficult to land than the heavyweights. Even though both had the same lift-to-drag ratio, the lightweight M2 had an inherently shorter time between the pilot's landing flare and touchdown than the heavyweight would have. This increase in time available before touchdown was desirable from a piloting standpoint, but the heavyweight vehicles also landed much faster. The FRC M2 team had decided to proceed with the lightweight M2 tests, even though the vehicle would be difficult to fly, because its low touchdown speed (around 90 miles per hour) reduced the risk of pilot injury.[8]

Bikle's almost-covert M2 operation at Edwards proved a big success in boosting the lifting body concept. The flight test results encouraged greater participation by other NASA centers and Headquarters through the Office of Advanced Research and Technology under NASA Associate Administrator Raymond Bisplinghoff. On September 15 and 16, 1964, just after the end of the M2-F1 program, Bisplinghoff and some of his staff met with Paul Bikle and the M2 team at Edwards. What came out of this meeting was a directive to the NASA center directors asking that they document "existing research effort on entry vehicles of the lifting-body class," with a view toward possible construction of a hypersonic lifting body. OART now strongly supported the lifting body research program at Edwards; Bisplinghoff wrote, "I believe it is

essential that we have a strong in-house research effort covering all the technical problem areas of importance to lifting-body vehicle design and operation."[9]

By this time the "heavyweight" program was under way. In February 1964 FRC solicited proposals from 26 firms for two heavyweight, low-speed lifting body gliders. NASA would test them in the full-scale Ames wind tunnel and also air-launch them from a B-52 flying at 45,000 feet. The firms had five weeks to submit proposals. OART would supervise the program with Ames, Langley, and FRC participating. One glider would be an M2, and the other would be Langley's own proposed HL-10 modified delta shape. Only five companies submitted proposals; FRC selected the Norair Division of the Northrop Corporation to build the vehicles. On June 2, 1964 the FRC awarded a fixed-price contract to Northrop for the fabrication of the M2 and HL-10 heavyweight gliders for $1.2 million apiece. Northrop would deliver the M2-F2 in the late spring of 1965, with the HL-10 following six months later.[10] The lifting body program had moved into its next phase. FRC and Headquarters still favored going beyond gliders to powered supersonic lifting body trials; in early August 1964 Bikle, Bisplinghoff, and Bisplinghoff's deputy Alfred Eggers agreed on incorporating provision for XLR-11 rocket engines in the two new gliders.[11]

What most influenced Bikle and the FRC project team in their selection of Northrop were the elements of simplicity and costs. Northrop, a company in the midst of a highly successful "private" fighter venture (the F-5 program), assured FRC that it could build the two gliders cheaply. Richard Horner, who had worked with Bikle first at Edwards, then from NASA headquarters, was now executive vice president of Northrop. The two men dispensed with all unnecessary paperwork and red tape. The result of this simplification was that the vehicles, which one industry spokesman had predicted could cost $15 million apiece, wound up costing just

$1.2 million apiece, unheard of for complex research airplanes. Bikle assigned FRC engineer John McTigue as NASA program manager, while Northrop assigned Ralph Hakes as Norair's program manager. The two men devised a Joint Action Management Plan to minimize paperwork, to minimize the number of employees working on the project, to make decisions by individuals and not by committees, to locate the project in one area where all necessary resources could be easily and quickly directed to it, and to fabricate the vehicle using a conservative design approach. As Hakes recalled,

> We never had more than a handful of engineers . . . They were all twenty-year men who had worked to government specifications all their lives and knew which ones to design to and which to skip. McTigue's people and ours would talk things over and decide jointly what was reasonable compliance with the specifications. Decisions were made on the spot. It didn't require proposals and counter-proposals.[12]

Because of his long Air Force association, Paul Bikle always worked closely and effectively with his Air Force Flight Test Center counterparts, much as Walt Williams had before him. He recognized that, like the X-15, the lifting body program required some sort of joint operations agreement because the program was getting too large for NASA to manage and operate alone. He knew that the NASA--Air Force--contractor flight testing relationship was a close one; as with the NACA in the late 1940s at Muroc, there were few if any disagreements among the working-level personnel. Such disagreements as existed were imposed from above. Bikle saw that the Air Force and NASA had similar interests in the lifting body concept; over the early spring of 1965, he met with Maj. Gen. Irving Branch, commander of the Air Force Flight Test Center at Edwards. Out of these meetings came a Memorandum of Understanding on April 19, 1965. The memo drew on previous X-15 progam experience, alluding to the similarities between the programs and the excellent working relationships that had existed between Air Force and NASA personnel assigned to the X-15 program.

The memo created the Joint FRC/AFFTC Lifting Body Flight Test Committee composed of 10 members: director of FRC (chairman), commander of the AFFTC (vice-chairman), NASA and Air Force pilots, NASA and Air Force engineers, NASA and Air Force project officers, NASA instrumentation representative, and medical officer from the Air Force.

The joint flight test committee had overall responsibility for the test program; it also assumed responsibility for all outside relations and contacts. FRC had responsibility for maintenance, instrumentation, and ground support of the craft, while the AFFTC assumed responsibility for the launch aircraft, support aircraft, medical support, the rocket power plant, and the pilot's personal equipment. AFFTC and FRC assumed joint responsibility for planning research flights, analyzing flight data, test piloting, range support, and overall flight operations.[13] Bikle and Branch issued the memo two months before Northrop rolled out the M2-F2. But the M2-F2 and the HL-10 were no longer the only "heavyweights" under construction. A year and a half later, on October 11, 1966, the AFFTC and FRC amended the memo to cover NASA participation in an Air Force-sponsored lifting body program, the Martin SV-5P.[14] (As related subsequently, the Air Force and Martin had further expanded upon the company's PRIME work and had derived PILOT--a proposed Mach 2 "low-speed" research vehicle that the service could test to determine its supersonic, transonic, and subsonic-to-landing behavior. This vehicle the company designated SV-5P.[15] Martin also proposed a low-speed lifting body trainer, the SV-5J, to be powered by a small turbojet, for use at the Air Force test pilot school. Nothing came of this, though the company built the shells of two such vehicles. The Air Force awarded Martin a contract for one SV-5P vehicle in May 1966, and the company began development under the direction of engineers Buz Hello and Lyman Josephs. Martin completed it a little over a year later, rolling it out of the Baltimore plant on July 11, 1967. The Air Force designated the craft X-24A. It soon

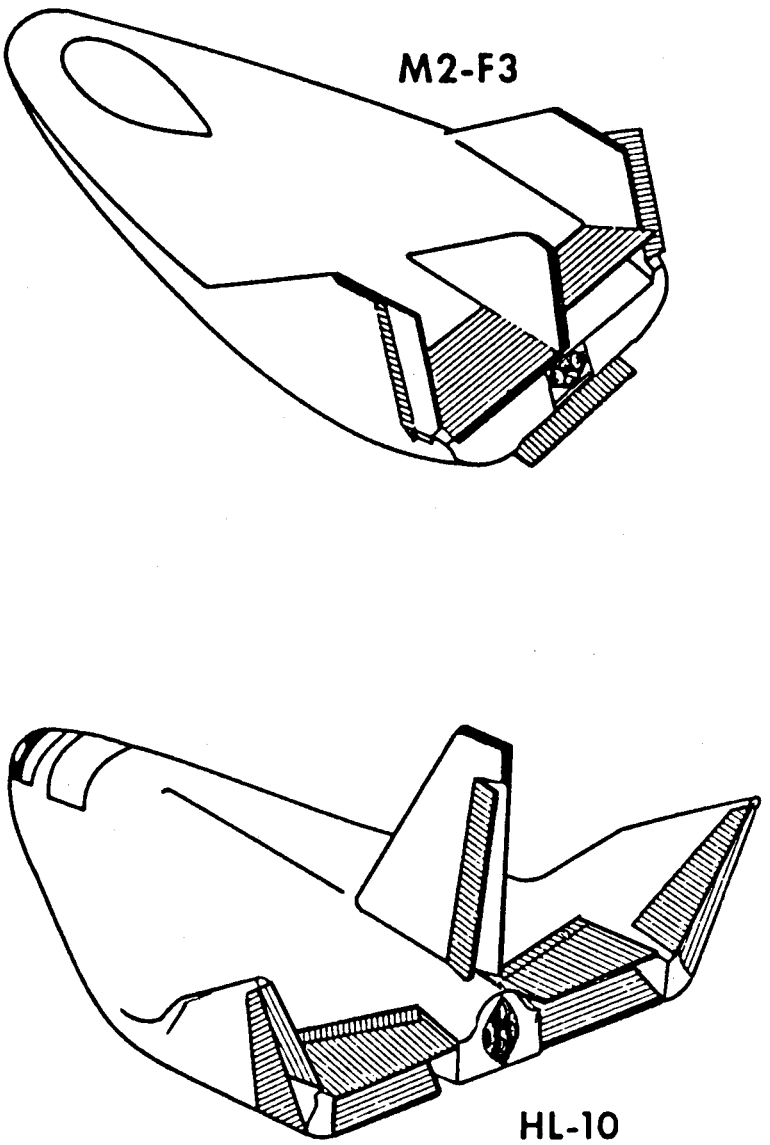
journeyed to Ames for comprehensive wind tunnel testing, and from there to Edwards, where the other lifting bodies, the M2-F2 and HL-10, had already flown.)[16]

The "Heavyweight" M2-F2 and M2-F3

Without a doubt, the lifting bodies were the ugliest of the postwar research aircraft. Only two were passingly handsome: the HL-10 was pleasingly plump and the X-24B, with its laundry-iron shape, had rakish lines that hid the tubby bulge of its X-24A ancestry. Despite their lines, they generally flew satisfactorily. "Lifting bodies," one test pilot remarked at the unveiling of the X-24A, "fly a lot better than they look." Figure 2 shows the control surface layout for the HL-10 and M2 vehicles.

The M2-F2 rolled out of Northrop's Hawthorne, California, plant on June 15, 1965 and was trucked over the hills to Edwards the next day. It resembled the earlier M2-F1. At its unveiling, it still lacked the planned XLR-11 rocket engine; NASA would fly it first as a glider and then modify it for powered flight. Fabricated from aluminum, the M2-F2 weighed 6150 lbs and measured 22 feet in length, with a span of 9.5 feet. Like the earlier M2-F1, it had two vertical fins, but lacked the earlier craft's horizontal control surfaces. Unlike the M2-F1, it had a retractable landing gear, assembled from off-the-shelf components, including the main landing gear of a Northrop T-38 trainer and the nose gear of a North American T-39 Sabreliner. High-pressure nitrogen would blow down the gear just prior to touchdown. It had a complex series of body flaps: a full-span ventral flap controlled pitch, while split dorsal flaps controlled roll (lateral) motion through differential operation and pitch and trim through symmetrical operation. The twin vertical fins provided directional (yaw) control and also acted as speed brakes. The

Figure 2



CONTROL SURFACE ARRANGEMENT OF THE M2 AND HL-10 LIFTING BODIES

M2-F2 had a stability augmentation system to assist the boosted control system in damping out undesirable vehicle motions. The pilot could use four throttleable hydrogen peroxide rockets rated at 400 lb thrust apiece for "instant lift" during the prelanding flare; if the craft proved unmanageable or some other calamity struck, the M2-F2 had a modified zero-zero ejection seat from a Convair F-106 Delta Dart.

At FRC, technicians checked out the aircraft, added research instrumentation, and then trucked it to Ames for two weeks of tests in the full-scale wind tunnel. Ames completed 100 hours of testing in August 1965; apart from a correctable high-frequency oscillation of the upper surface flaps, the M2-F2 received a clean bill of health. It returned to Edwards for its initial flight trials. Northrop furnished a special adapter so that the M2-F2 could launch from the B-52 mothership's existing X-15 launch pylon. On March 23, 1966 the M2-F2 completed its maiden captive flight. Following a series of similar checkouts, NASA readied the craft for free flight.[17]

In preparation for the M2-F2's first flights, FRC launched a cooperative pilot training and aircraft simulation program with the Cornell Aeronautical Laboratory of Buffalo, New York. Earlier, FRC had flown Cornell's highly modified variable-stability Lockheed T-33A jet trainer to simulate the low lift-to-drag reentry characteristics of the X-15. Now, in the spring and summer of 1965, the FRC again flew Cornell's T-33A, this time on lifting body studies, using the M2-F2 as the reference type. The variable-stability T-33A--in its own right, one of America's most successful postwar research aircraft--had "drag petals" installed on its wingtip tanks. These petal-shaped surfaces, extended in flight, varied the lift-to-drag ratio of the aircraft from the T-33A's normal 12-14 to as low as 2, the approximate ratio of an M2 lifting body. Typical lifting body approaches were executed by Cornell test pilot Robert Harper and by FRC pilots Milt Thompson,

Bruce Peterson, Bill Dana, and Fred Haise. The T-33A tests indicated that the M2-F2 aircraft would have undesirable lateral control characteristics under certain conditions--a fact that later assumed critical importance. In addition, NASA's pilots simulated lifting body approaches and landings using the center's F-104s and a Douglas F5D.[18]

The M2-F2 completed its maiden flight on July 12, 1966. NASA pilot Milt Thompson dropped away from the B-52 mothership at 45,000 feet flying at 450 miles per hour. During the brief flight--not quite four minutes--Thompson made a 90 degree turning descent, performed a practice landing flare maneuver at 25,000 feet, made another 90 degree turn onto final approach, increased his gliding speed to 350 miles per hour, initiated the landing flare at 1200 feet reducing his rate of descent from 250 feet/second to 10 feet/second, lowered the landing gear, and touched down exactly at the planned aiming point on Rogers Lake at 200 miles per hour, coasting 1.5 miles across the lakebed. The M2-F2's first flight had been an unqualified success. By mid-November 1966 the craft had completed an additional 13 flights, piloted by Thompson, Bruce Peterson, and the Air Force's Capt. Jerauld Gentry and Lt. Col. Donald Sorlie. Following flight 14 on November 21, NASA grounded the M2-F2 for installation of its XLR-11 rocket engine. On May 2, 1967 the M2-F2 made its first flight carrying, but not using, the rocket engine, another glide flight piloted by Jerauld Gentry. Along with all other pilots who had flown the craft, Gentry did not like the M2-F2's poor lateral-directional stability characteristics. At low angles of attack at high speeds, it often developed a rolling motion that increased in severity. If the pilot increased the angle of attack, this motion damped out. On the very next flight this behavior contributed to a major accident that set back the entire lifting body program and seriously injured NASA's Bruce Peterson.[19]

On May 10, 1967 Peterson launched away from the B-52 at 45,000 feet heading to the north and flying east of Rogers Dry Lake. All went well as the M2-F2 sank like a stone, until the wingless craft reached 7000 feet. Then, flying with a "very low" angle of attack, the M2-F2 began a "dutch roll" motion, rolling from side to side at over 200 degrees per second. Peterson, who earlier had turned a nearly uncontrollable first flight in the HL-10 into a brilliantly successful landing, was an excellent pilot; he quickly and instinctively raised the nose, damping out the lateral motions. The recovery had carried the craft away from its intended flight path. The pilot realized he was too low to reach the planned landing site near lakebed Runway 18 and was rapidly sinking toward a section of lakebed that lacked visual runway reference markings, which were needed to estimate height above the lake with accuracy.

At this point, a rescue helicopter appeared in front of the M2-F2. Peterson, overburdened, disoriented from the rolling motions, now had an additional worry. He called, "Get that chopper out of the way," following this seconds later with "That chopper's going to get me, I'm afraid." FRC chase pilot John Manke, flying an F5D, assured Peterson the helicopter was clear, and it did chug off, out of Peterson's path. Realizing he was very low, Peterson fired the landing rockets, and the M2-F2 flared nicely. He lowered the landing gear, which needed only 1-1/2 seconds to deploy from up-and-locked to down-and-locked. But time had run out. Before the gear locked, the M2-F2 hit the lake, shearing off its telemetry antennas. In the control room, engineers saw the needles on their instrumentation meters flick to their null points. Startled, they looked up to the video monitor--in time to see the M2-F2, as if in a horrible nightmare, rolling over and over across the lakebed at more than 250 miles per hour. It turned over six times before coming to rest on its flat back, minus its canopy, main gear, and right vertical fin. Peterson, who by all expectations should have died in the accident, was

badly injured. Rescue crews pulled him from the wreckage, rushed him to the Edwards hospital for emergency surgery, then to the hospital at March Air Force Base, and several days later to UCLA's University Hospital. He pulled through, though losing the sight of one eye. The plucky airman remained at the FRC as the center's director of safety and continued to fly as a Marine reservist.[20]

Instead of simply trucking the M2-F2's remains to a scrapyard, NASA returned them to Northrop's Hawthorne plant. Technicians placed the battered lifting body in a jig to check alignment, removing the external skin and portions of the secondary structure. The inspection took 60 days. In March 1968 NASA's Office of Advanced Research and Technology authorized Northrop to restore the primary structure and return the vehicle to FRC. There it sat, while lifting body advocates from Ames and FRC determined its future. In light of its poor handling characteristics, the craft obviously needed modification. By this time the rival HL-10 was already demonstrating superior handling qualities. Nevertheless the M2 shape still appeared worth studying; on January 28, 1969 NASA headquarters announced that the agency would repair, modify, and return the M2-F2 to service as the M2-F3.[21]

The rebuilt aircraft, which returned to Edwards and first flew in 1970, looked much like its predecessor, except for a short stubby vertical fin located midway between the two large vertical fins. This center fin acted as a large "flow fence" to improve lateral control. The craft had a new jet-reaction roll-control system, which NASA hoped might be used on future lifting body spacecraft so that the pilot could rely on a single control system all the way from orbit to landing, rather than the multiplicity of systems used on such craft as the X-15. NASA planned to employ the M2-F3 as a testbed for research on the lateral control problems encountered by lifting body vehicles.

On June 2, 1970 Bill Dana completed the craft's first flight, a glide flight to evaluate how the modifications changed the plane's performance from that of the earlier M2-F2. A planned powered flight on November 25 went awry when the engine shut down prematurely. Air Force test pilot Jerauld Gentry, the only pilot at Edwards to fly the M2-F2, HL-10, and M2-F3, flew the plane on February 9, 1971 and said it flew as well as the HL-10; this was praise, for the HL-10 flew much better than the unmodified M2-F2. NASA and the Air Force then embarked on a joint program of incrementally increasing its speed and altitude performance, with the last two flights setting the fastest and highest M2-F2 marks. On August 25, 1971 Bill Dana had made the craft's first supersonic flight, attaining Mach 1.1. Over a year later, on December 13, 1972, Dana attained Mach 1.613, 1065 miles per hour, the fastest M2-F3 flight. On the last flight of the craft, on December 20 FRC test pilot John Manke attained 71,500 feet, an M2 record. On only one occasion did trouble occur; on its tenth flight, September 24, 1971, the M2-F3 experienced an engine ignition malfunction; Dana shut down the XLR-11 engine, but a small amount of propellant flared briefly in the engine bay before extinguishing itself. Dana made "a hard but otherwise uneventful landing" on Rosamond Dry Lake, the alternate emergency landing site to Rogers. Toward the end of the craft's flying career, FRC technicians installed and evaluated a rate command augmentation control system, a kind of fly-by-wire system that used an analog computer and a side-arm control stick in addition to the regular control stick. Altogether, the M2 completed 43 flights. Sixteen as the F2 and twenty-seven as the F3. Retired at the end of December 1972, the plane subsequently joined the collection of the Smithsonian Institution.[22]

The HL-10 Program

NASA complemented the M2-F2 and M2-F3 trials with an extensive evaluation of the Northrop HL-10. In contrast to the accident-marred M2 flight test program, HL-10 testing moved along quite smoothly--once the aircraft had been modified after a very frightening first flight. The HL-10, product of Eugene Love's work at Langley Research Center, was among the most successful of the lifting bodies; indeed, when the Space Shuttle began to take shape, the consensus among NASA engineers at the Flight Research Center was that it should look like the HL-10. Unlike the M2, which had a cone-shaped underside, the HL-10 had a flat bottom and a rounded top; it was, in effect, an inverted airfoil in cross-section, with a delta planform. It had three vertical fins, two of them angling outwards from the body, and a tall center fin. The flush canopy did not protrude above the body lines of the vehicle. Like the M2-F2, it measured 22 feet in length, but it was wider (14.5 feet) and higher (11.5 feet). It used many off-the-shelf components from the T-38, T-39, and F-106, among others. The control system consisted of upper body surface and outer fin flaps for transonic and supersonic trim, blunt trailing edge elevons, and a split rudder on the center vertical fin. It had a three-axis stability augmentation system, landing rockets, and provisions for an XLR-11 engine, though the engine was not installed at roll-out from Northrop's Hawthorne plant January 18, 1966.[23]

Northrop shipped the vehicle to Ames for testing in the 40 x 80 ft full-scale tunnel. The tunnel tests proceeded uneventfully, though some tests hinted at flow separation over the outer vertical fins, a condition engineers did not consider serious. At the Flight Research Center on December 22, 1966 NASA pilot Bruce Peterson completed the craft's first glide flight. It was anything but routine. During the 3-minute descent to landing, Peterson discovered that he had minimal lateral control over the

lifting body; flow separation was much worse than anticipated. Peterson managed to set the HL-10 down safely on Rogers Dry Lake, no small tribute to his piloting skills. NASA immediately grounded it for study, also taking the opportunity to install its rocket engine. The first flight, in the words of Langley engineers, "once again demonstrated the value of flight tests as proof-of-concept."[24] Langley undertook a series of tunnel tests. As a fix, NASA engineers modified the leading edge of the outer vertical fins so as to direct more air over the control surfaces. Technicians added the new leading edges, constructed of fiberglass, late in 1967, smoothing over the installation with epoxy paint. The HL-10 experience reemphasized to engineers that aerodynamically shaping lifting body designs for good subsonic performance could lead to potentially disastrous flow separation problems in the absence of thorough design analysis. "This experience," Langley engineers concluded, " . . . pointed up the significance of seemingly minor shape changes . . ."[25]

When the HL-10 took to the air again on October 23, 1968, it handled very nicely. What was to have been the first HL-10 powered flight had to be aborted after launch when only one of the XLR-11's chambers fired; Jerauld Gentry made an emergency landing on Rosamond Dry Lake. On November 13 everything clicked; NASA pilot John Manke reached Mach 0.84 (523 miles per hour) using two of the engine's four thrust chambers. NASA now began incrementally working toward the craft's maximum performance. The HL-10 went supersonic for the first time on May 9, 1969; this was the first supersonic flight of any manned lifting body and a major milestone in the entire lifting body program. The craft exhibited acceptable transonic and supersonic handling characteristics. On February 18, 1970 Air Force test pilot Maj. Peter C. Hoag reached Mach 1.86 (1290 miles per hour), the fastest lifting body flight ever made; nine days later, on February 27, 1970, Bill Dana reached an altitude of 90,302 feet, another record for the lifting body program. The HL-10 thus became the fastest and highest-flying piloted lifting body ever built.[26]

Toward the end of the HL-10 flight test program, NASA embarked on a series of powered landing trials. By 1970, the Space Shuttle was being discussed. One critical question was whether it should make unpowered landing approaches or, like a conventional transport aircraft, fly a powered approach and landing. Engineers had several schemes for the powered landing, the most popular being "pop-out" retractable turbojet "landing engines" that the Shuttle crew could deploy at subsonic speeds while approaching the earth. Advocates thought the landing engines would give Shuttle a shallower descent angle, reducing pilot workload and enhancing overall mission safety. While popular with many industry and government engineers who had little background in the "Round One," "Round Two," and lifting body programs, this scheme was not at all popular at Edwards. Test pilots and engineers alike recognized the complexity that landing engines would add to any Shuttle design, as well as the danger to a Shuttle crew if one of the engines failed during the final and most critical portions of flight. Because of the popularity of this idea elsewhere, FRC engineers embarked on a powered-landing program using the HL-10.

In February 1970, following the record altitude and speed flights, NASA grounded the HL-10 and replaced its XLR-11 rocket engine with three 500-lb-thrust Bell Aerosystems hydrogen-peroxide rocket engines. NASA planned launching from the B-52 in the vicinity of Palmdale; the pilot of the HL-10 would ignite the rocket engines as the lifting body passed through an altitude of 6500 feet. The rockets would reduce the approach angle of the aircraft from its customary 18 degrees to 6 degrees and give the HL-10 an airspeed in excess of 350 miles per hour. At 200 feet above the lakebed, the pilot would shut down the rockets and extend the landing gear, executing a routine landing. The HL-10 completed two of these flights piloted by Pete Hoag on June 11 and July 17, 1970, the latter flight being the craft's final mission. The flights gave much more encouragement to the Edwards viewpoint than to those in favor of landing engines. The shallow descent

angle had in fact increased pilot workload and degraded mission safety. Hoag found he had more trouble in determining the landing aiming point, and the higher approach speed aggravated control-sensitivity problems.

The HL-10 tests carried the day for advocates of a "deadstick" Shuttle reentry, approach, and landing. As Milton Thompson, a test pilot with experience in numerous low L/D research aircraft, subsequently stated,[27]

> the shuttle, whether it has landing engines or not, must be maneuvered, unpowered, to a point near the destination because the engines cannot be started until the vehicle is subsonic and only limited fuel will be available. To us it seems ridiculous to maneuver to a position where power must be relied upon to reach the runway.

During its brief flying career, the HL-10 completed 37 flights. It was a fine flying vehicle, and its flight test program encouraged Eugene Love of Langley to advocate the HL-10 design concept for any future NASA shuttle. For a variety of reasons, this did not come to pass. Pilots who flew the craft uniformly praised its handling characteristics, reserving criticism only for its bubble Plexiglas nose. The lenticular-shaped nose acted as a giant "demagnifying" lens at low altitude, causing severe visual distortion and misleading pilots into thinking that they were much higher over the lakebed than they really were. Consequently they sometimes waited too long before extending the landing gear. With experience, however, they learned to compensate for this distortion, and the problem disappeared.

Clearly the M2 and HL-10 efforts constituted an important approach to hypersonic flight technology, even though they themselves never approached hypersonic velocities. Today approximately ten million visitors per year have the opportunity to see the M2-F3 at the National Air and Space Museum. Sadly,

after years of exhibition at the Dryden center, the HL-10 was dropped and seriously damaged when being carelessly raised into position for exhibit at the California Museum of Science and Industry. It is hoped that eventually it will be at least cosmetically restored so that future generations will have the opportunity to see it and recognize it for what it was: an important step on the path of lifting reentry.

NOTES

1. Reed interview; NASA X-Press, 10 March 1967; Klein interview; Williams interview.

2. Interview with Gus Briegleb, 18 May 1977; Reed interview; Klein interview; Bikle interview; NASA X-Press, 10 March 1967.

3. Klein interview; NASA X-Press, 18 May 1968.

4. M2-F1 interflight worksheets, in DFRC pilot office files; Reed interview; Klein interview; "M-2 Research Craft Landed Seven Times," Aviation Week (9 September 1963), p. 34; Russell Hawkes, "M-2 Flight Successes Spur Interest in Lift Re-entry," Missiles and Rockets (9 September 1963), pp. 14-15; Thompson, "I Fly Without Wings," pp. 12-13; NASA X-Press, 10 March 1967.

5. Letter, Bikle to Milton Ames, 24 April 1963.

6. U.S. Senate, Committee on Aeronautical and Space Sciences, Hearings: NASA Authorization for Fiscal Year 1964, Pt. 1: Scientific and Technical Programs, 88th Congress, 1st Session, April 1963, pp. 40, 608-634; Pt. 2: Program Detail, 88th Congress, 1st session, June 1963, pp. 919-920.

7. NASA news release 32-63; "Pilot Declares 'Flying Bathtub' Makes Fine Re-entry Vehicle," The Oregonian, 6 December 1963; Klein interview; DFRC monthly flight activities files for 1963 and 1964, DFRC pilot office files; interview with Jerauld Gentry, 11 September 1975.

8. Drake, "Aerodynamic Testing Using Special Aircraft," pp. 8-10; NASA FRC Five Year Plan.

9. Ltr., Bisplinghoff to NASA center directors, 28 October 1964.

10. NASA news releases 64-41, 64-93, and NASA FRC release 14-64.

11. Letter, Eggers to Bikle, 18 August 1964; Bisplinghoff memo for NASA Associate Administrator, 20 August 1964.

12. Northrop, "Lifting Bodies: Coming In on a Vanishing Wing," p. 7; Bikle interview.

13. AFFTC-FRC memo of understanding, 19 April 1965, reprinted as Appendix V in FRC-AFFTC, Lifting Body Joint Operations Plan (Edwards: 1 September 1969), pp. 8-9 to 8-9b.

14. Addendum to memo of understanding, 11 October 1966, reprinted as part of Appendix V in ibid., p. 8-9c.

15. U.S. House, Committee on Science and Astronautics, 1967 NASA Authorization, Pt. 2, 89th Congress, 2nd session, February-March 1966, pp. 1073-1077; ibid., 1968 NASA Authorization, Pt. 2, 90th Congress, 1st session, Apr. 1967, pp. 1011-1012; USAF AFSC news release 31.65; "START and SV-5 Shuttle Gets Go-Ahead," Space Daily, 4 March 1965; "A Decision on AF Manned Space Shuttle Nears," Space Daily, 20 September 1965; William J. Normyle, "Manned Flight Tests to Seek Lifting-Body Technology," Aviation Week & Space Technology (16 May, 1966), pp. 64-75.

16. Evert Clark, "Rocket Plane May Let Astronauts Land at Airfields," The New York Times, 12 July 1967; USAF AFSC news releases 85-66 and 59-67.

17. NASA X-Press, 23 April and 18 June 1965.

18. NASA X-Press, 7 May 1965; NASA FRC news release 12-65; G. Warren Hall, "Research and Development History of USAF Stability T-33," Journal of the American Aviation Historical Society (Winter 1974).

19. NASA X-Press, 15 July 1966; chronology of lifting body flights prepared by Nancy Brun, NASA Historical Office; Gentry interview; NASA release 66-329; NASA X-Press, 5 May 1967.

20. A comprehensive collection of official NASA accident reports and pilot interviews was published as "Pilot Work Load Cited in M2-F2 Crash," Aviation Week & Space Technology (1 October 1967); interview with Bruce Peterson, 26 March 1976; interview with Wen Painter, 8 August 1977; NASA X-Press, 19 May 1967. The accident eventually inspired both a popular novel, Martin Caidin's Cyborg, and the TV series The Six-Million Dollar Man, the latter of which ran footage of the accident at the beginning of each episode. The footage clearly reveals the pronounced "Dutch roll" motions that preceded the crash itself.

21. NASA FRC news release 10-68; NASA news release 69-15; NASA Ames Astrogram, 30 January 1969.

22. Brun chronology; M2-F3 progress and flight reports; OART M2-F3 Flash Reports.

23. NASA X-Press, 14 and 28 January 1966; "HL-10 Delivered Today," Space Daily, 18 January 1966.

24. Robert W. Rainey and Charles L. Ladson, "HL-10 Historical Review" (Hampton: LaRC, July 1969).

25. Ibid.

26. Brun chronology; HL-10 progress and flight reports; OART HL-10 flight reports.

27. Hoag interview, AFFTC, 1983; NASA Flight Research Center, Flight Test Results Pertaining to the Space Shuttlecraft, NASA TM X-2101 (Edwards, CA: FRC, 30 June 1970) p. 149; NASA FRC news release 8-70; NASA news release 70-71; Brun chronology; HL-10 flight reports.

CHAPTER II

THE AIR FORCE AND THE LIFTING BODY CONCEPT

In the mind of the aviation-oriented public at large, the SV-5D program and the subsequent X-24A and X-24B exemplified Air Force interest in lifting bodies. In fact, however, Air Force involvement in the lifting body story was more complex and extensive than simply these three efforts; as has been discussed in the previous chapter, the service operated as a full-fledged partner of NASA during M2 and HL-10 flight testing at Edwards. Even prior to the onset of the PILOT project within the larger SV-5/START program, the Air Force had expressed serious interest in the lifting body concept. For example, as has been related in the X-20 case study, Space Systems Division sponsored studies of a manned lifting body for satellite inspection purposes, the so-called SAINT II. In May 1961, SSD had undertaken a study of the requirements for a manned lunar landing expedition dubbed LUNEX, which would have incorporated a three-man M2-type lifting body for the return to earth. The acknowledged center of Air Force interest in lifting bodies, however, was at Wright-Patterson Air Force Base, Ohio--the Air Force's traditional source of new configuration evaluation and major research and development initiatives--and specifically located within the Directorate of Aero-Mechanics, and its successor, the Flight Dynamics Laboratory.* It is instructive to examine this early Air Force lifting body work so as to put into proper perspective the subsequent X-24A, X-24B, X-24C, and National Hypersonic Flight Research Facility (NHFRF) effort.[1]

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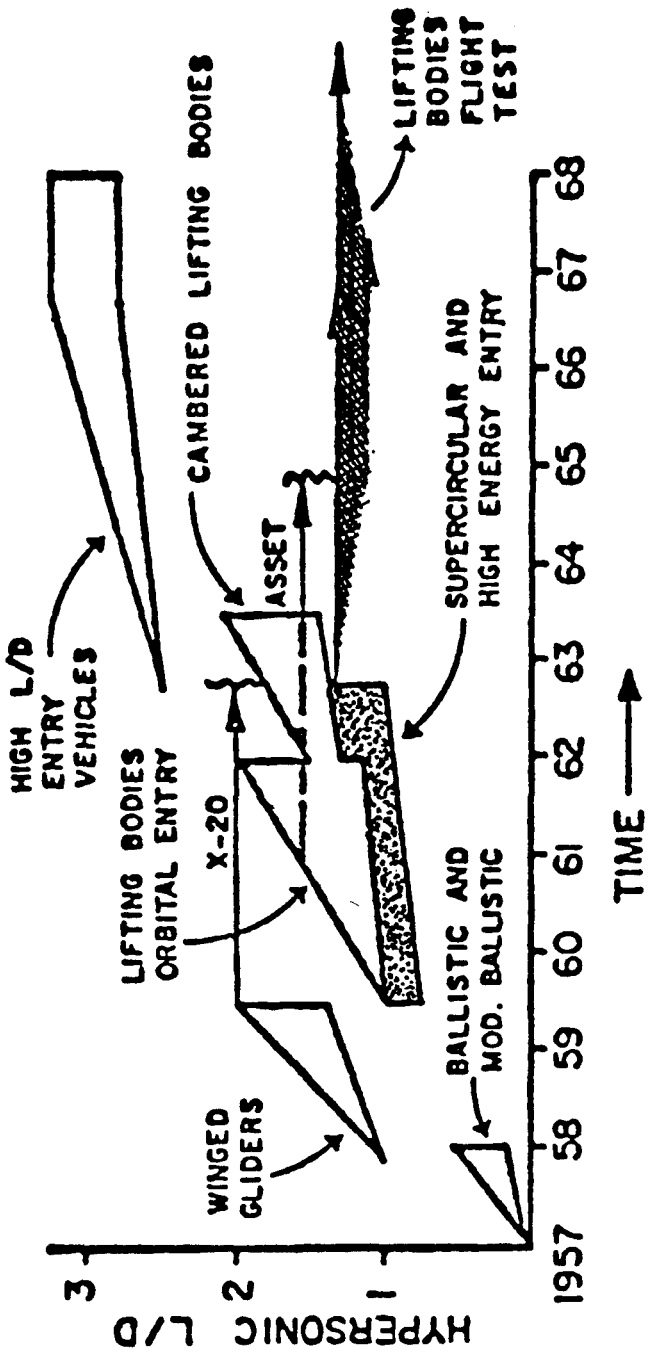
*The Flight Dynamics Laboratory was established at Wright-Patterson AFB on March 8, 1963, from the reorganization of the earlier Directorate of Aero-Mechanics.

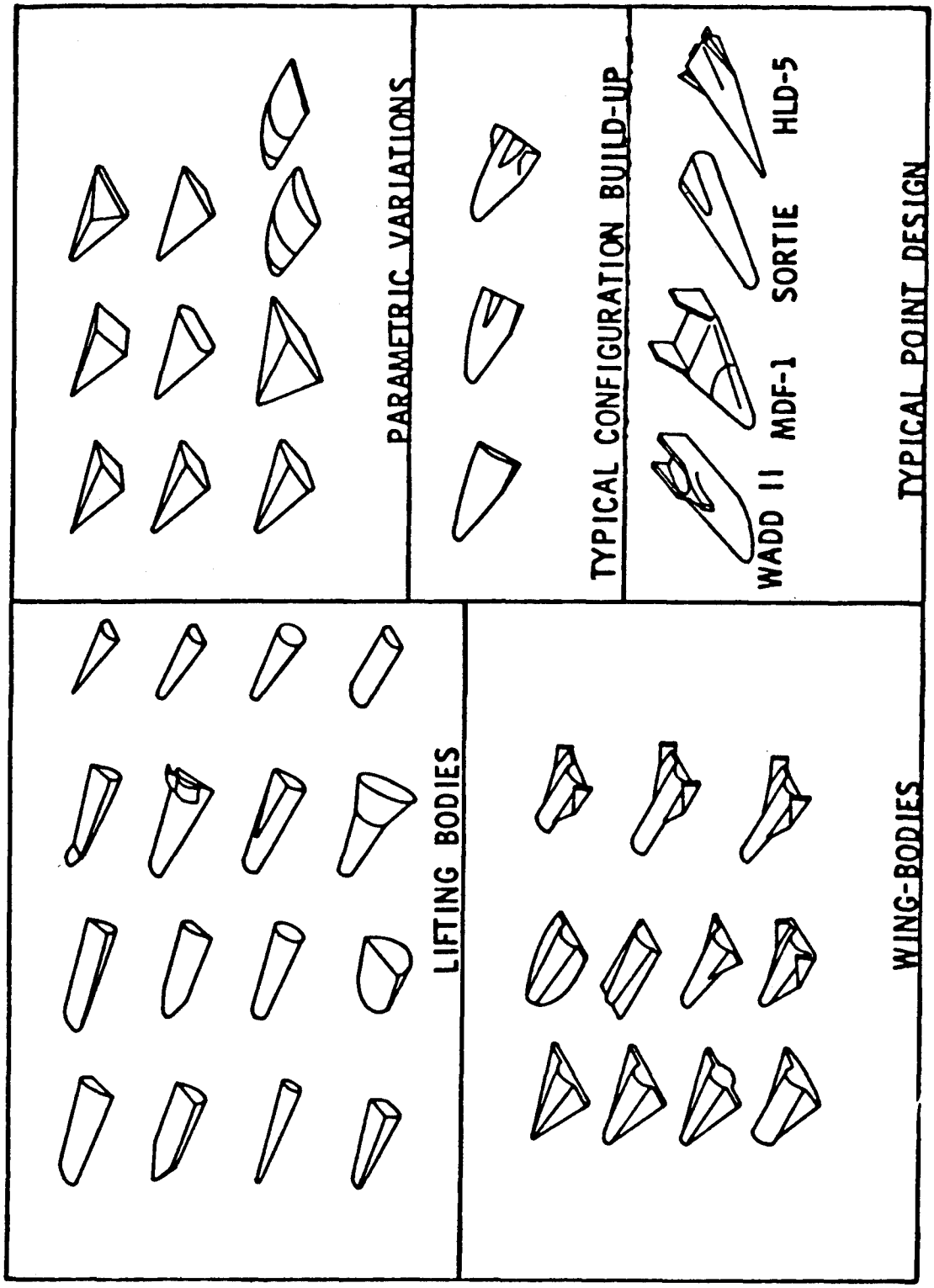
Figure 1 shows a general arrangement of configuration choices for specific lift-to-drag values plotted against time from 1957-1968: essentially an evolutionary graph that reflects as much upon the research interests of the Flight Dynamics Laboratory and its technical staff as it does upon the overall state of interest in hypersonic vehicle design.[2] Figure 2 shows a variety of lifting bodies, wing-bodies, parametric variations (i.e.: variations on a technical theme), and typical point design (i.e.: configurations drawn up to tightly written mission or performance goals) configurations, examined in the approximate 1959-1968 time period. Generally speaking, these configurations ranged from "low" lift-to-drag (L/D) ratios on the order of 1 to "high" L/D's of 3 (though, of course, these values were all extraordinarily low when compared to those attained by a conventional airplane). From the outset, however, most Air Force partisans--particularly Alfred C. Draper--expressed pronounced enthusiasm for the higher L/D concepts, and eventually (as will be mentioned subsequently), this led to a whole family of so-called FDL shapes optimized to generate high lift-to-drag hypersonic values. The early "point" vehicles represented various interesting approaches to the problem of lifting reentry and orbital flight. WADD II (for Wright Air Development Division, the predecessor of the present Aeronautical Systems Division) was a relatively low L/D configuration dubbed the "lead sled" by Draper and his assistants, and appeared as one of the Phase Alpha suggestions during the X-20 development effort. (A WADD III also existed, a delta having a low wing-loading and thus known as the "light kite"). SORTIE was a so-called supercirculation reentry vehicle optimized for very high hypersonic velocities. MDF-1 (MDF being the organizational symbol of Draper's group) was a modified Clark Y airfoil--the Clark Y being one of the major subsonic airfoils of the 1920s, derived by Virginius E. Clark, a former commanding officer of McCook Field, the predecessor of Wright Field--turned into a lifting body shape; it had surprisingly good hypersonic as well as low-speed

Figure 1



AIR FORCE LIFTING REENTRY RESEARCH INTEREST EVOLUTION, 1957-1968

Figure 2

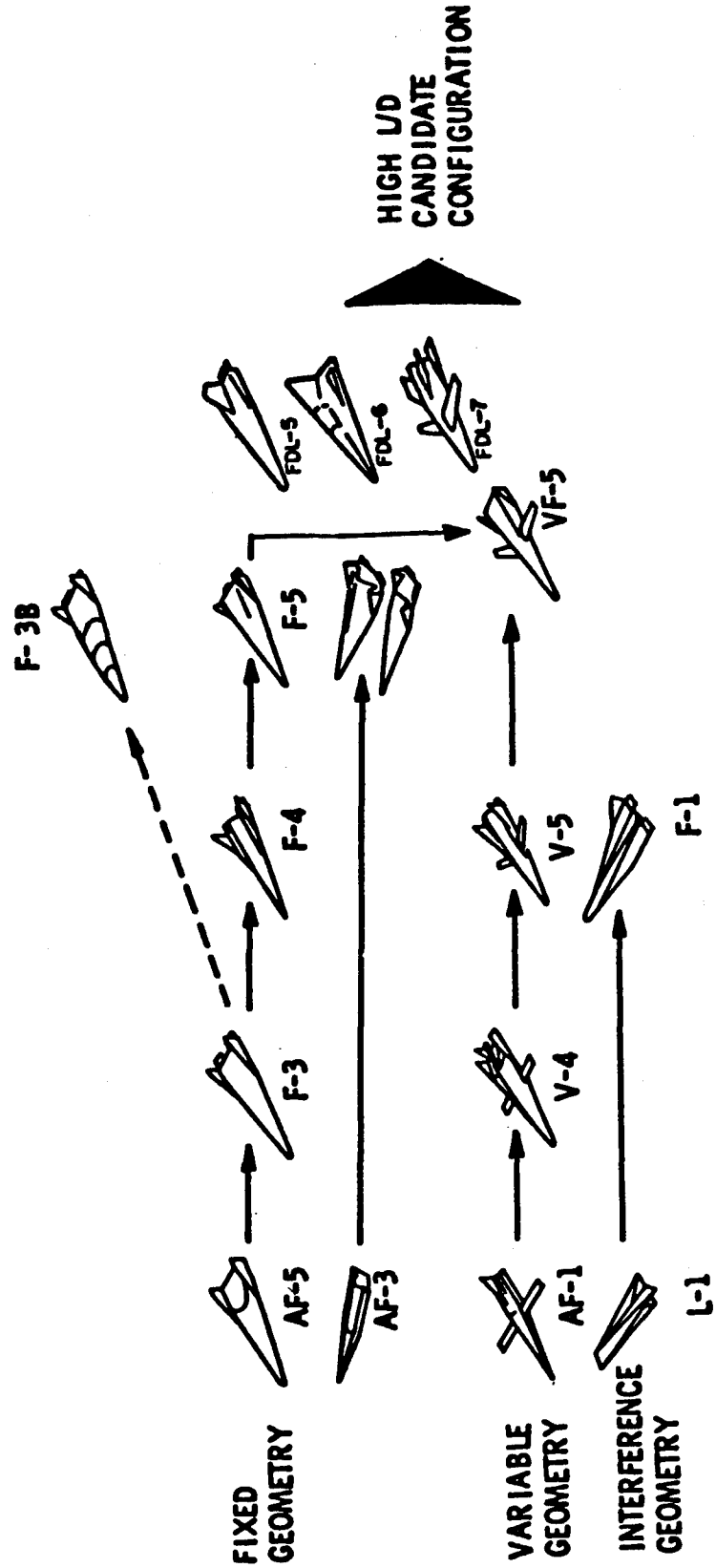


CONFIGURATIONS EXAMINED BY THE FLIGHT DYNAMICS LABORATORY
AND ITS PREDECESSORS, APPROX. 1959-1968

characteristics, and it was this shape that Draper showed to SSD on the West Coast as well as to Hans Multhopp during the critical design configuration phase of the early PRIME program. HLD-5 was an early study for a high L/D shape and might be properly considered the first of the so-called FDL series that culminated in the seductive X-24B.[3]

During its studies of lifting reentry body shapes, Draper's group found it could count on strong support from such figures as Alexander Flax, René Miller, Raymond Bisplinghoff, and Cortland T. Perkins, all of whom held major positions of significance within the governmental, academic, and aerospace communities. As early as 1962, as part of the so-called LAND proposal, Draper's group had proposed that the Air Force look at lifting bodies via actual flight testing of piloted demonstrators from subsonic through supersonic speeds. NASA at the time was willing to support such a program if the Air Force elected to do so, but the service chose not to, much to the puzzlement of the national space agency, which soon embarked on its own heavyweight M2 and HL-10 efforts which aimed at accomplishing the same thing. The Draper group nevertheless went ahead with its laboratory studies, generating a whole series of tailored body shapes, some having variable-sweep (variable geometry) configurations whereby the vehicle would enter the upper atmosphere as a lifting body and at transonic speeds deploy a variable-sweep wing to increase its L/D during its approach to landing. Figure 3 shows the evolutionary relationship of these high L/D configurations examined over approximately a half-decade (63-68) by the Draper group. In addition to fixed geometry and variable sweep configurations, the group also explored so-called "interference" configurations, using complex undersurface designs to position shock flows for favorable increases in lift. (The North American XB-70A Valkyrie utilized a somewhat similar technical approach to improve its supersonic L/D via folding wingtips that also improved its directional stability characteristics. Likewise, proposed Nonweiler or "caret" wave

Figure 3



EVOLUTIONARY PROGRESSION OF FDL HIGH L/D REENTRY SHAPES

rider wing configurations made use of favorable flow interference to increase hypersonic L/D, an approach still being pursued today.) Interestingly, however, the Draper group got most of its ideas for such interference configurations from the aero-propulsion community--specifically, those individuals working on inlet design, where interference is an accepted fact of life. Draper and his team discovered, however, that these interference configurations demanded complicated lower surface geometries that aggravated aerothermodynamic problems by creating intersections and junctures that greatly aggravated the heating situation of such vehicles. Simplistic configurations, with proper aerodynamic tailoring, proved equally good, and without any of the heating difficulties manifested by the interference shapes. "It worked," Draper recollected two decades after this interference work, "but not to a sufficient degree to make it worth your while."[4]

Flight Dynamics Laboratory did not limit its studies to lifting body configurations, and the FDL (and the USAF-DoD community at large) made major inputs into the Shuttle itself and its predecessors. In Project Forecast, an Air Force Systems Command study effort in 1963-1964, FDL and ASD representatives examined a wide range of hypersonic atmospheric and space vehicles, their work anticipating many of the issues subsequently examined in the formative stages of the Space Shuttle. Likewise, through such activities as the joint NASA-Department of Defense Aeronautics and Astronautics Coordinating Board (a joint NASA-DoD Board formed in 1960 to provide the highest level of coordination of R&D between DoD and NASA), FDL made clear its views and opinions on the matter of lifting reentry from space. (This work is discussed subsequently in the Shuttle case study.)[5] Thus, though the X-24A and its successors remained the most visible example of Air Force interest in lifting reentry following X-20, ASSET, and PRIME, it was by no means the only example of such "blue suit" interest.

From SV-5P to X-24A

As discussed within the SV-5D case study, the START program and SV-5 effort spawned a project termed PILOT (for PIloted LOw-speed Tests), a manned transonic and supersonic demonstrator subsequently designated as the SV-5P. PILOT gathered speed slowly. By the end of December 1964, for example, PILOT still had not received official approval and thus the START office had great difficulty persuading the NASA Ames and Langley Research Centers to schedule wind tunnel test time so that engineers could assess the SV-5P's anticipated transonic and supersonic force, pressure, and hinge moment characteristics, as well as the interference and separation characteristics of the B-52/SV-5P combination and their behavior as the SV-5P launched from the bomber. (Similar testing had greatly benefitted the X-15 during its own development in the late 1950s.) At the same time, PILOT faced a number of key management questions involving procurement and management structure, the number of test vehicles to be procured, and not least of these, who the actual fabrication contractor would be. Northrop had an edge, having experience with the on-going M2-F2 and HL-10 "heavyweights," but Martin had the aerodynamicists and aerodynamic experience peculiar to the SV-5 configuration.[6]

On December 18, 1964, the START office briefed Maj Gen Ben I. Funk, Commander of Space Systems Division, on the PILOT effort. By this time the procurement method followed by Paul Bikle with Northrop at the NASA Flight Research Center appeared as highly desirable and quite attractive since it promised less cost than an SSD procurement and reflected an attractive spirit of cooperation between NASA and the Air Force. Briefers emphasized that gaining Aerospace Corporation's support for PILOT was almost impossible and that general systems engineering and technical direction might have to be done by the Air Force's own Research Technology Division (RTD). General Funk assured the START Program Office that the SV-5 configuration would be demonstrated in the low-speed

regime, and asked that a method of contracting for this project and managing it be sought which would not require additional personnel in the program office and would utilize the resources at Aeronautical Systems Division (ASD) and RTD. This, of course, was quite a demanding request.[7]

In response, on December 24, the START office sent a request for support to the Air Force Flight Test Center (AFFTC) at Edwards. The tentative flight schedule for the SV-5 called for 20 glide flights and 40 powered flights over an 18-month period beginning in May 1966. All of these flights would require a B-52 mothership capable of dropping the SV-5 from at least 40,000 feet and at a speed of Mach 0.8. Planners anticipated the test program would be a joint NASA-AFFTC effort and would follow the program set up for the M-2 and the HL-10.[8] In early January 1965, ASD, RTD, and AFFTC representatives held meetings in preparation for a January 15 briefing to General Funk. Rumor at the time indicated that DDR&E did not recognize the need for this lifting body flight test program; however, $2.6 million remained in the fiscal 1966 budget as programed funds. Thus, the matter seemed to be to insert appropriate wording in the development plan to secure approval.[9]

On January 26, SSD received word that Headquarters Air Force desired a briefing on PILOT in early February. Alternatives to be considered and presented at this briefing included: (a) a single rocket-powered vehicle program; (b) a rocket-powered and a jet-powered vehicle program; and (c) a single jet-powered vehicle program. In all cases the vehicles should be easily convertible to either jet or rocket propulsion. Obvious points also to be covered included cost, schedules, resources, B-52 support availability, flight simulator capabilities, and ASD/RTD support functions. If the existing NASA-Northrop procurement method were to be used (i.e., add-on), time was important because planning and engineering effort would have to be completed quickly for

fabrication work to be accomplished on a phase-over basis from the HL-10. A January go-ahead seemed to be economically optimum.[10]

The development plan briefing went to AFSC in late January and proposed a basic program procuring one rocket powered SV-5 at a cost of $1.5 million, also advocating a second vehicle with more sophisticated instrumentation systems at an added cost of $2.6 million. The PILOT briefing was given by Colonel Curtis Scoville to the AFSC staff and to General O. J. Ritland and General E. B. Giller in Air Force headquarters on February 15 and 16. Good support was gained at AFSC, as evidenced by General Ritland's February 24 letter to Air Force headquarters, on the "START Low-Speed Program" which is quoted in part below:

> A number of alternative approaches for an expanded low-speed program have been examined in considerable detail by the Space Systems Division in close cooperation with the Aeronautical Systems Division, the Research and Technology Division and the Air Force Flight Test Center. The alternatives have been reviewed by the Hq AFSC staff and by myself and General W. A. Davis. Our consensus is that the preferred approach is as briefly described below and as presented to you in an oral and documented briefing on 16 February 1965.
>
> We recommend that the present single-vehicle low-speed program be augmented by addition of a second rocket-powered vehicle and one jet-powered vehicle. All three vehicles should be acquired by the Systems Command from the Martin Company on a sole-source basis. A third B-52 launch aircraft should be added to the two presently on hand at AFFTC. Total program costs, including operating costs through FY 1968, are estimated at $8.9 million.
>
> The advantages of a second rocket-powered vehicle are self-evident. It provides a backup in the event of loss or extensive damage to the single vehicle. It provides maximum use of available flight time in that at least one of the two vehicles would ordinarily be available. It provides a more flexible flight program and an increased data-gathering rate.
>
> The principle advantage of the jet-powered vehicle is that it adds significantly to the capability of

testing in the low subsonic, flare, and landing regimes. Its sustained flight time provides for extensive investigation of changes in center of gravity, control system gains and interconnects, and vehicle handling qualities. It is independent of the B-52 and thus permits very flexible flight scheduling and economical flights. The present low-speed program, as well as the NASA lifting body tests, will be severely restricted by limited B-52 availability. Particularly, in the event that a third B-52 cannot be provided, the jet-powered vehicle will permit accomplishment on a timely and economical basis of a considerable portion of the test program.

The total program costs have been estimated as $8.9 million. We believe this cost is reasonable and justified by the program proposed. We recognize, however, the difficulties imposed by the FY 1966 funding proposed in the briefing, and have further examined the funding requirements. Funding can be rearranged to remain within available FY 1966 funds and within the same total cost of $8.9 million although some schedule readjustments may be necessary. The changes made are primarily reductions in FY 1966 funds to the contractor and postponing of B-52 mods to FY 1967. The proposed revised cost schedule is as shown in the first part of the following table. The second part of the table shows proposed sources of funds. The funds required for FY 1966 are available or budgeted although it would be necessary to transfer $2.6 million from P. E. 64415034 to START. Systems Command will initiate the necessary actions towards budgeting FY 1967 and later requirements.

	FY-65	FY-66	FY-67	FY-68	TOTAL
			Estimated Costs		
Martin Co.	1.3	2.4	1.6	-	5.3
GFE	-	0.1	0.2	-	0.3
Spares	-	0.1	0.2	-	0.3
AGE	-	0.2	0.3	-	0.5
B-52 mods.	-	-	0.5	-	0.5
Test Support	-	-	1.0	1.0	2.0
Total	1.3	2.8	3.8	1.0	8.9

	FY-65	FY-66	FY-67	FY-68	TOTAL
		Proposed Sources			
START	1.3	0.2	-	-	1.5
P.E. 63409874					
A/C Ops Spt	-	2.6	-	-	2.6
P.E. 64415034					
New Requirement	-	-	3.8	1.0	4.8
Total	1.3	2.8	3.8	1.0	8.9

> We believe that the proposed program will provide a sound, well-balanced research and test program on a timely basis, at a reasonable cost, and without significant gaps. It will provide for thorough exploration of flight characteristics and man-machine relationships of a lifting body vehicle in the subsonic, transonic and low supersonic range. We recommend that the program be approved.

Clearly, as the Ritland letter shows, considerable uncertainty remained in the minds of program enthusiasts. This uncertainty persisted a little longer. In early March, General Giller briefed Dr. Alexander Flax and General James Ferguson on the PILOT project in preparation for a more complete presentation to be given by the START Program Office the week of March 22. He indicated that he thought chances of getting approval for <u>two</u> rocket vehicles were fairly good but that anything in addition to that was rather poor. General Giller requested that the briefing for the week of March 22 contain the following alternatives, comparisons, and guidelines:[11]

> A program of one rocket-powered SV-5 vehicle capable of manned flight, after B-52 launch, from Mach 2 to landing. Another program with two rocket-powered vehicles with this same capability. Purchase cost comparisons should be based upon vehicles of the same physical dimensions. If more than one size of SV-5 vehicle is considered, individual contractor costs should be given for each size. The presentation of two rocket vehicles should be limited to operational considerations. No requirement exists for a jet vehicle. Only the simplified control system should be considered.

> Schedules should be based on the two B-52 launch aircraft presently available. Another schedule based on the three B-52 aircraft can be presented for later consideration.

General Giller also said that the briefing team should be able to answer the following questions:

> What are the total cost breakdowns, in detail, relative to sole source Martin Company; sole source Norair; and limited competition between Martin and Norair for procurement of one, and again for two, low-speed vehicles? Cost breakdowns should include support items. In regard to the procurement methods above, what are the differences in cost if two rocket vehicles are ordered at the same time or the second is ordered after six months or a year's delay? What is the rationale of relationship of the SV-5 low-speed program to the SV-5 hi-speed program and to the NASA HL-10/M-2 program? What aerodynamic data between hi- and low-speed SV-5 vehicles with emphasis on canopy? Can we justify the low-speed program on other grounds, if there is sufficient difference between the low-speed configuration and the hi-speed configuration to invalidate the original justification (one shape from orbit to landing)? Has consideration been given to changing the low-speed configuration to match possible future changes in the hi-speed SV-5 configuration? What is the rationale of the NASA/FRC (Mr. Bikle's) concern about the larger SV-5 vehicle's engine? (This question has been asked in a separate message to meet a 5 March deadline.)

On March 10, 1965, Dr. Fubini signed out a letter confirming DDR&E's antipathy towards the PILOT effort. The portion on the low-speed program stated:

> In conjunction with discussions you have had with DDR&E in the past few weeks, I wish to reconfirm that DDR&E desires to approve the detailed plan for the 680A (START) lifting reentry test program. We have, in particular, questions on the scope and approach to your low-speed flight test portion of the program, and I request that the Air Force refrain from committing funds to this portion of the program until we have reviewed and approved your plan. I also feel it important that we segregate the low-speed lifting reentry vehicle effort from our data capsule hypersonic work, and request you submit a plan for a new Program Element.

Clearly, therefore, there was some interest at DDR&E in separating PILOT from PRIME. The PILOT project, however, complemented PRIME and the two were very closely interrelated. Clearly, there was strong justification and rationale for continuing these projects under single management as had been planned within the START Program Office. SSD believed any future application of the knowledge gained by PRIME and PILOT would be under the province of the Space Systems Division. Therefore, based on this reasoning, General Funk recommended to General Schriever that it would be in the best interests of the AFSC to continue the management of lifting body development efforts within the Space Systems Division and specifically within the START Program Office, despite ASD's clearly evident lifting body interests. This recommendation was made in mid-April.[12]

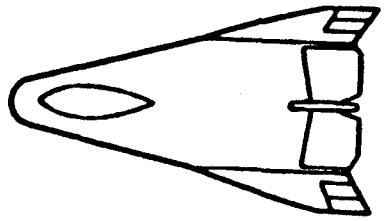
During April and May 1965 PILOT received little attention because PRIME had its major perturbations in the areas of vehicle size, weight, cost and procurement methods. On May 26, General Funk emphasized that every effort should be made to get PILOT approved now that PRIME was back on course. On May 28 a talking paper was prepared for General Funk on the PILOT project. General Funk discussed the PILOT project with General Schriever on May 30 and recommended that Dr. Flax be approached with an urgent request to furnish executive direction for PILOT. General Funk recommended that the Air Force procure the PILOT vehicle through NASA from Northrop immediately so that costs would not continue to grow using that approach. A strong justification accompanied this request.[13]

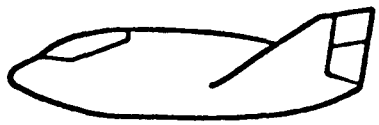
While AFSC headquarters was establishing a position on PILOT, a key meeting took place at ODDR&E on June 16, 1965, called by D. J. Brockway of ODDR&E in order to address some of the performance nuances of the SV-5 in the lower speed regimes. Martin reviewed the data from the transonic stability tests over the past few years, presenting data showing that there were some

instabilities in the configuration first tested in the Langley 8-foot tunnel in April 1963. These had been corrected by adding the nose ramp. The configuration was again changed by adding a large bubble canopy and was retested at Langley. Again, instabilities were apparent. The canopy was immediately suspect since it was the only major change since the last series of tests. Later, a smaller canopy was tested at Cornell and again at Langley. The results were satisfactory and the canopy problem was solved. Brockway indicated that he thought the test results were very satisfactory.[14] Attendees also discussed PILOT and PRIME vehicle differences, noting that differences included the canopy, the middle fin (which acted like a large flow fence), six more moveable control surfaces, and the difference in scale size (see Figure 4 for control surface layout). PILOT vehicle anticipated performance was presented by Martin. The vehicle would have an engine burn time of 137 seconds, giving an estimated maximum velocity of Mach 2.4 at an altitude in excess of 80,000 feet. Total flight time would be about 13 minutes and the landing speed was estimated at 135 knots.[15] The meeting continued with discussions as to the necessity for the PILOT project. Brockway acknowledged the points made by Martin and indicated that as soon as DDR&E received the memorandum stating the Air Force position that they would be prepared to respond. Colonel Scoville also talked with a representative of the ODDR&E who indicated that he felt Dr. Fubini was quite serious about a new Program Element for PILOT. Colonel Scoville also found out during this visit that the START line item for fiscal 1966 was at only $31.7 million rather than the $35 million that had been advertised up to that point in time. This new figure was being carried by DDR&E who also wanted a new development plan for the PILOT project.[16] PILOT's gestation pains thus continued.

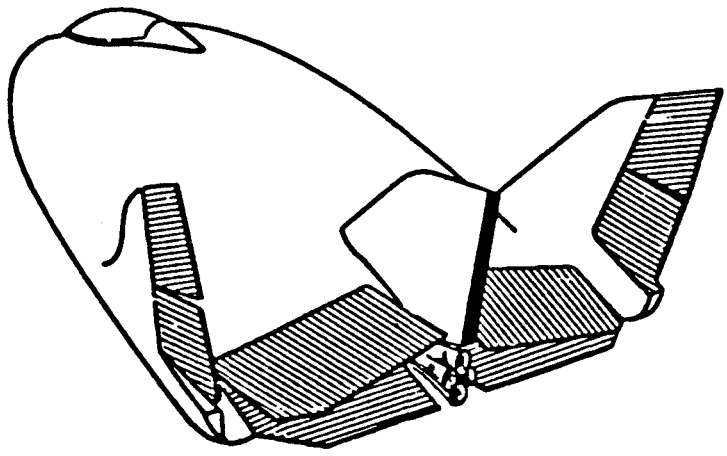
As a consequence of discussions between General W. A. Davis and Air Force Secretary Zuckert in very early July, General Davis, Vice-Commander of AFSC, signed out a letter to Secretary Zuckert

Figure 4





BODY PLANFORM AREA, FT^2	**191**
BODY SPAN, FT	**13.5**
BODY LENGTH, FT	**24.5**
LANDING WT, LB	**6000**
LANDING WING LOADING, LB/FT^2	**31**



SV-5P (X-24A) SPECIFICATIONS AND CONTROL SURFACE LAYOUT

regarding the PILOT project. A letter had also been prepared for Dr. Flax to forward to DDR&E; however, it was assumed that Secretary Zuckert had asked that this letter be held up for a while. The letter to Secretary Zuckert, entitled "START Low-speed Program" and dated July 8, 1965, read as follows:

> This letter is in response to your telephone request of 30 June.
>
> I am somewhat apprehensive about trying to summarize a very complicated situation that has evolved over the past seven or eight months. However, in an attempt to be responsive to your specific questions, I will outline only the most cogent points and suggest that a detailed briefing would be in order if you care to pursue the matter further.
>
> Several months ago, the START Program (Space Technology and Reentry Techniques) had a primary objective, defined as the development and test of a maneuverable data return vehicle capable of recovering 80 pounds of payload from low earth orbit with a CEP of less than 10 nautical miles. It had as a secondary objective the development and flight test of an SV-5 vehicle with characteristics as required to not only satisfy the primary objective, but also provide as much information as possible in support of the other objectives; i.e., economics of recovery and manned maneuverable ferry vehicles. The data recovery vehicle became known as the high-speed portion of the program and the manned maneuverable vehicle became known as the low-speed portion of the program.
>
> At the outset of the START Program we tentatively planned to acquire a single piloted SV-5 vehicle through the NASA Flight Research Center as an add-on to the NASA lifting body contract with Norair. This approach was considered primarily because it appeared to be economical and consistent with the current START technology objectives. However, during February the program was thoroughly reviewed by this headquarters and it was decided that this approach was inadequate. On February 24, we submitted to Hq USAF (AFRST) a revised plan wherein we recommended that the single vehicle low-speed program be augmented by addition of a second rocket-powered vehicle and one jet-powered vehicle. All three vehicles were to be acquired by the Systems Command from the Martin Company on a sole source basis. We also proposed a third B-52 launch aircraft be added to the two

presently on hand at the Flight Test Center. Total program costs, including operating costs through FY68, were estimated at $8.9 million. This consisted of $5.3 million to Martin for the three vehicles; $.3 million, GFE; $.3 million, spares; $.5 million, AGE; $.5 million, B-52 Mods; and $2.0 million, Test Support. $1.3 million of this was required and available in FY65, $2.8 million was required in FY66, $3.8 million in FY67, and $1.0 million in FY68. We proposed a method of funding within the then currently approved FY66 budget.

I felt that we gave sufficient justification for this program and still believe that it is completely defensible. However, the Air Staff disagreed with the requirement for the turbojet-powered vehicle and asked for alternative programs. This information was provided as requested on 22 March, but to date we have received no indication from Hq USAF as to their disposition of our request.

I believe that the necessity for development of manned maneuverable reentry vehicles is self-evident when our Nation's manned space programs are viewed in the context that our ballistic reentry vehicles and water recovery are projected into the future. It seems hardly reasonable to assume that we should continue to deploy a large portion of the U.S., Navy, and Air Force units to recover each manned space vehicle. It seems to me that our proposed program is a bargain basement price to pay for technology that we were pursuing for several years with the Dyna-Soar program.

Much of the delay since February has, I am sure, been required in resolving questions with DDR&E and at the present time, the DDR&E has deferred funds on the low-speed program and has requested that we establish a new program element. I have recently asked for another review of the low-speed program and preparation of a technical development plan which will be ready for submission about the first week in August. I can only suggest that a decision be deferred until the new plan is available.

Presumably, this hard-hitting letter achieved its objectives, for if Secretary Zuckert had any reservations in PILOT, he nevertheless did not mention them; subsequently, on July 8 AFSC headquarters sent a message requesting that SSD prepare a PILOT Development Plan and forward it no later than July 23. The plan

was to include two rocket-powered vehicles with appropriate aerodynamic and performance data. The contracting method and the contractor were not to be specified and a new program element number was given to be used. The plan was forwarded to General Schriever on July 23 and the estimated cost of two rocket vehicles was $3 million. This was a firm-fixed-priced proposal made by Vern Rawlings, Vice-President, Martin-Baltimore in a letter to Colonel Scoville, dated July 12, 1965.[17] On August 11, 1965, General Davis signed out two letters concerning the PILOT project. They both transmitted the PILOT Development Plan and recommended its approval. The first was to Air Force headquarters and read in part:

> The program described in the plan contemplates acquisition and flight test of two rocket-powered, SV-5 configuration, lifting body vehicles. The vehicles would be acquired by the Air Force and tested as a joint effort by the Air Force Flight Test Center and the NASA Flight Research Center as a continuation of the present lifting body test program. Total program cost would be about $5.77 million including $3.96 million for vehicles, ECPs, wind tunnel tests, and technical support, and $1.81 million for flight test support including propellants, XLR-11 engine refurbishment and spares, and B-52 modification. An additional $125 thousand per year would be required for AFFTC support. The total cost to the Air Force for a two-year test program then would be about $6.0 million, a very modest price indeed for the benefits gained, particularly when compared to the approximately $82.0 million cost of the total START program and its predecessor, the M-103 Program. There are sufficient funds in those presently approved for the START program to meet FY66 requirements for this proposed program. However, additional funds will be required in FY67 and FY68.
>
> This program, together with the currently approved START program, will provide a complete flight test and evaluation of the SV-5 configuration across the entire flight regime from orbital to landing speeds. The SV-5 has been very thoroughly tested across the same speed range in ground facilities to the extent possible within present capability. Flight tests are required to provide the basis for data correlation.

These tests are of interest to NASA as well as the Air Force. In fact, the NASA Flight Research Center has agreed to pay about half of the costs of B-52 modification as well as providing some maintenance support for the SV-5. It should be noted that the SV-5, while of the same general class as the NASA vehicles, is a distinctly different configuration.

Although the development plan has been prepared under a new program element number, it is suggested that a new program element is not consistent with current efforts to reduce, rather than increase, the number of individual program elements. Furthermore, in this case a new program element is not desirable since this program is properly part of the START program.

The development plan shows program management by the Space Systems Division and does not indicate participation by the Research and Technology Division, an arrangement which is not entirely satisfactory. We are giving further consideration to the management and technical direction aspects of the program with the intention of insuring maximum utilization of the technical capability of the Research and Technology Division.

This program is considered of vital interest to the Air Force. It, together with the other elements of the START program, will provide a firm, realistic basis on which to make timely future decisions relative to the development of medium L/D class maneuvering reentry vehicles. It is recommended that the program be approved.

The second was to Secretary Zuckert and read:

This letter is a follow-on to my letter of July 8 on the same subject.

As you will recall, I suggested that a decision on the program be deferred until a new development plan was available. The plan has been prepared and a copy is attached for your information. Additional copies are being submitted through normal channels to the Air Staff, a copy of the transmittal letter being attached. Also, the Space Systems Division is preparing a briefing on the program which we can present to you about August 16 or later at your convenience.

Very briefly, this program contemplates acquisition by the Air Force of two rocket-powered lifting body

> vehicles of the SV-5 configuration and flight test of the vehicles by a joint Air Force Flight Test Center/NASA Flight Research Center effort. This would be a continuation of the present flight tests of NASA lifting bodies.
>
> Total cost to the Air Force would be about $6.0 million including acquisition of vehicles and support costs of a two-year flight test program. I believe this to be a very modest price to pay for the benefits gained. The data from this program together with that from the other parts of the START program will provide the basis required for future decisions relative to the development of maneuvering reentry vehicles.
>
> I strongly recommend that the program be approved.

While the development plan was being looked at in Washington, Colonel Scoville continued to set the foundation for the PILOT project. He briefed General Irving L. "Twig" Branch, Commander of the Air Force Flight Test Center (AFFTC) on August 13, for the purpose of gaining his support of the PILOT flight test program. AFFTC had seen the development plan; however, no firm indication of coordination had been received. The briefing was received well, and it was clear that PILOT would receive support from AFFTC. Based on X-15 experience, General Branch was solidly behind the procurement of at least two SV-5 vehicles for the test program. (It was a sad loss for the program and the Air Force as a whole when this fine officer, who had eventually played a key role in advancing hypersonic vehicle testing, was killed in the crash of a Northrop T-38 Talon jet trainer during a cross-country flight.)

At last, on September 7 the long-awaited direction for PILOT came thundering down from Dr. Flax. It was not what had been anticipated, being stiff and a bit surprising to the START Program Office and SSD, particularly in its pronounced endorsement by direct reference of the capabilities and work of the Flight Dynamics Laboratory at Wright-Patterson AFB. Dr. Flax's letter, addressed to General Davis, read as follows:

This is in reply to your letter to Secretary Zuckert on the START low-speed program.

In your letter and the attached development plan you reopen the question of the scope and funding of the Low-Speed Research Program on Lifting Bodies. The substance of the proposed program which you forwarded was considered as one of the alternatives when we previously reviewed the program.

Our reasons for deciding in favor of a single vehicle program were as follows:

(1) The hypersonic START vehicle program is not now oriented toward manned reentry vehicle development. The low-speed program which we are now to undertake should therefore be aimed much more strongly at exploring a range of configurations and developing generally applicable technology than was previously the case.

(2) The development of a manned lifting body reentry vehicle is now necessarily on a considerably later schedule than was originally contemplated when planning the START program. Low-speed investigations can therefore proceed at a slower pace with less commitment to a single vehicle configuration.

(3) The combination of the two NASA vehicles and the Air Force vehicle operated in a single research and technology program offers the best opportunity for the Air Force to explore and acquire flight experience with a wide range of vehicle configurations.

(4) The cost of the single vehicle program will be considerably lower not only because of lower vehicle unit cost and the reduction in number of vehicles, but also because common support of the Air Force and NASA vehicles and certain technical services to be provided in common with NASA will reduce the operating costs. Further, an additional B-52 will not be required at AFFTC and the requirement for 60 additional manpower spaces cited in the development plan should be open to substantial reduction.

In view of the fact that, as explained above, we no longer have an integrated program aimed at the specific development of a manned ferry vehicle, the DDR&E recommendation that START and the low-speed lifting body program should not be in the same program element is sound, and I see no advantage to the Air Force which

> would accrue from keeping them together. Further, we have a very adequate reservoir of people competent in low-speed aerodynamics and aircraft design in the Flight Dynamics Laboratory at Wright Field, as well as at AFFTC. There is little reason to require Aerospace Corporation support for this program, although for purposes of assuring a continuing relationship with respect to future manned reentry development it would be desirable for the Aerospace Corporation and SSD to maintain close liaison with this program, perhaps to the extent of participation of a man or two.
>
> I believe it is essential for technical reasons, as well as for programing and contractual reasons, to proceed with this program without further delay. As I indicated to you previously, the AFSC desire for continued reconsideration of the larger program can only have the effect of further delaying progress in this area. I am not prepared to support the larger program on either technical or financial grounds and even if I were prepared to support it, I have fairly positive indications that DDR&E would not approve it. I would therefore appreciate your cooperation and, hopefully, your enthusiastic support, in getting on with a reasonable and meaningful low-speed lifting body technology program.

SSD briefed General Ferguson on the latest developments with PILOT including the Flax letter on September 10 and a message was signed out to AFSC by General Giller following the briefing. This message requested cost data, type of contract, contractual route, and recommended management approach data. Total funding available for this program was to be $1.5 million. The AFSC position with regard to interpretation of the Flax letter was expected within the week and it seemed probable that this position would not only separate the PILOT project from PRIME but also assign the PILOT project to ASD or RTD. These expectations proved correct, and by mid-September it was apparent that PILOT would be transferred to ASD. Colonel Scoville immediately contacted John McCollum at ASD and arranged a meeting for September 23 to establish plans for an orderly transfer. A quick transfer of responsibility was desired by the START Program Office so that further delays would not be absorbed in getting the PILOT vehicle off the ground.[18] In

response to requests by AFSC, Paul Bikle, Director of NASA's Flight Research Center, informed the Air Force that the option to procure either a 110 or 120-inch vehicle through a NASA fixed-price incentive contract remained open; however, the price had increased to about $1.65 million because of the delay already incurred. Based on best cost estimates at the time, the minimum cost approach would be to procure the vehicle through AFSC channels. Either Northrop or Martin could produce a vehicle which would meet the project requirements. A 120-inch vehicle was recommended to AFSC.[19] AFSC sent a message to Air Force headquarters on September 29 stating their position regarding PILOT. Funding requirements for PILOT were estimated at $1.5 million for fiscal 1966, $.9 million for fiscal 1967 and $.87 million for fiscal 1968. The command anticipated that a firm fixed price for the vehicle would be negotiated after limited competition between Northrop and Martin. Management of the program would be assigned to ASD with technical direction to be provided by the RTD; the START Program Office would maintain continued liaison with ASD. The flight test program would be conducted as a joint AFFTC/NASA effort.[20] The development plan now had to be revised to reflect changes in the program such as number of vehicles, cost, management, manpower and schedule. This, however, now fell upon ASD's shoulders.

On October 4, 1965, General Davis signed out two letters, one to Lieutenant General C. H. Terhune, Commander of ASD, and one to General Funk, formally transferring PILOT to ASD. The letter to General Terhune stated in part that:

> The Advanced Development Program for START, submitted under Program Element 6.34.09.87.4 by SSD in January 1965, included low-speed testing of a lifting body configuration. Subsequent events resulted in the preparation of an ADP for an expanded program for Piloted Low-Speed Tests (PILOT Project) under a new program element. This plan was submitted to Hq USAF on August 11, 1965. Further details of the chronology of events are outlined in Section 1 of the attached Development Plan.

I have discussed this program with Secretary Zuckert and the Development Plan has been reviewed at SAF level. Dr. Flax (SAFRD) has requested prompt initiation of a single vehicle program utilizing the resources of the AFFTC and the Flight Dynamics Laboratory. A copy of Dr. Flax's letter is attached.

Lead division responsibility for this project is herewith assigned to ASD. The flight test program is to be conducted by the NASA (FRC)/AFFTC team in conjunction with other current efforts on low-speed lifting bodies. It is suggested that this program be managed by the X-15, X-21 Project Office with technical support from the Systems Engineering Group and the Flight Dynamics Laboratory. It is important, however, that close liaison be maintained with SSD to insure proper correlation of the hypersonic and low-speed lifting body programs.

It is requested that a revised ADP for the single vehicle program be prepared and submitted to this headquarters within 45 days. A new Program Element number will be assigned at a later date.

The total amount available for this program is $1.5 million of FY66 funds which have been previously identified in the START financial plan. If timely program initiation is to be effected, certain planning information is required by this headquarters as soon as possible. Data to be provided should include total costs, cost breakdown, type of contract, and method of contracting contemplated. In order to expedite assembly of cost data the attached message has been sent to SSD. ASD should immediately establish liaison with SSD (SSTRS) to participate in collection and evaluation of this cost information.

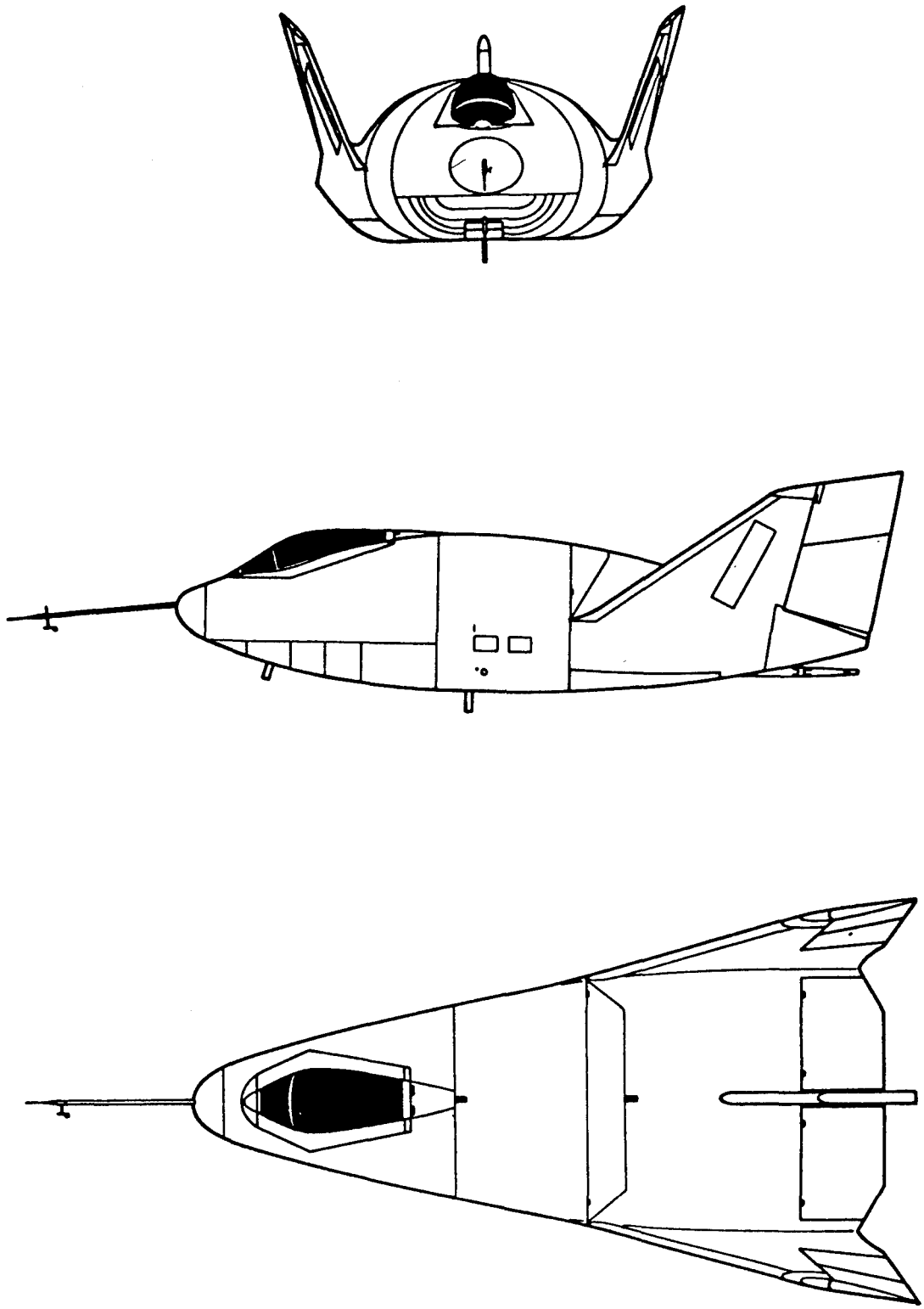
On February 11, 1966, the ASD PILOT Project Office received proposals by Northrop and Martin for the fabrication of one rocket-powered, manned SV-5P vehicle. The proposals were evaluated by a team (included was a member of the START Program Office) on February 13-16, and as a result the Martin Company was selected to design and build the SV-5P. On April 27-29, 1966, the SV-5P project office conducted the initial design review. During this review, technical staff examined vehicle design features and made modifications to vehicle design where appropriate. Nearly all subsystem design received approved pending implementation of

the recommendations of the review panel. In March 1966, the Air Force transferred $500,000 from the START program funds to the ASD for initiation of PILOT. PILOT now gave way to a more appropriate and fitting designation: X-24A.

The Martin X-24A (AF serial 66-13551) rolled out on July 11, 1967 amid ceremonies at Martin's Middle River, Maryland plant (Figure 5). Lieutenant General Terhune, newly appointed Vice-Commander of AFSC, was the keynote speaker. The first flight of the X-24A was scheduled for early 1968 after spending the fall of 1967 at Ames Research Center and at Edwards AFB for pre-flight checkout and instrumentation. In October 1967, NASA and the Air Force concluded a Memorandum of Understanding on use of the X-24A; the memo also confirmed the previous NASA-Air Force relationship in place with the earlier joint lifting body program agreements.[21]

Martin's little X-24A lacked the aesthetic appeal of other research airplanes, and indeed, even among its fellow lifting bodies it could lay claim to being the most unattractive. Its ultimate maturation into the sleek X-24B shape thus has elements of the story of the ugly duckling that turned into a swan. Its body shape differed greatly from the M2 and HL-10 family; whereas the former was a modified half-cone and the latter a fattened delta with negative camber, the plump X-24A had positive camber. After rollout on July 11, 1967, Martin shipped the craft to the Ames Research Center for comprehensive subsonic full-scale tunnel testing. That completed, NASA shipped the craft to Edwards in early 1969 for flight trials. Jerauld Gentry completed the maiden glide flight on April 17, the craft making nine more glide flights before its first powered mission. Gentry flew the X-24A's first powered flight on March 19, 1970, reaching Mach 0.87, well into the transonic region. Following this flight, Gentry, NASA pilot John Manke, and Air Force test pilot Maj. Cecil Powell steadily opened the X-24A's performance envelope. On October 14, 1970, 23 years to the day since Chuck Yeager's first supersonic flight,

Figure 5



MARTIN X-24A LIFTING BODY RESEARCH VEHICLE
(LANDING GEAR NOT SHOWN)

Manke piloted the X-24A on its own initial excursion past Mach 1, reaching Mach 1.19 (784 miles per hour) at 67,913 feet. Not quite two weeks later, Manke flew the X-24A to 71,110 feet, simulating a Space Shuttle approach and landing from that altitude. On March 29, 1971 Manke reached Mach 1.60 (1,036 miles per hour), the X-24A's fastest research flight. On June 4, 1971 the 28th and final research mission was a disappointment because only two of the XLR 11 engine's four chambers ignited, limiting the craft to subsonic speeds.[22]

The little X-24A had no vices, though it once gave researchers a bad moment. The rocket engine shut down prematurely and a small fire erupted in the engine bay, but Gentry made an emergency landing. Damage to the four maneuvering flaps, wiring, and flap instrumentation kept the ugly duckling grounded for nearly two months. The X-24A did have one bothersome quirk: during boost, it exhibited a pronounced nose-up trim change that prohibited low angles of attack during powered flight. FRC engineers concluded that the aerodynamic effects of the rocket exhaust plume impinging on the craft caused the nose-up condition, and warned the designers of the Shuttle to beware similar problems in that ambitious project. Though such trim changes sound innocuous, they could impose unacceptable aerodynamic loads on the Shuttle during its boost to orbit. Aside from this quirk, the X-24A flew very well and the pilots liked it. Flight testing of the X-24A led to one significant accomplishment: the SV-5 shape was the only one evaluated in actual free-flight at hypersonic, supersonic, transonic, and subsonic velocities. Like the M2-F3 and HL-10, the X-24A demonstrated that shuttle-type hypersonic vehicles could make precise landings without power. The X-24A pilots found they could land the vehicle on lakebed Runway 18 with an average 250 feet longitudinal "miss" distance from the intended touchdown spot. Indeed, NASA lifting body team members had no qualms about attempting landings on a confined concrete runway, such as the 15,000 foot runway at Edwards. This had not been attempted with

earlier lifting bodies only because they lacked nosewheel steering. All the lifting body trials gave great confidence to advocates of landing an unpowered Space Shuttle on a conventional runway after its return from space. This was the plan ultimately followed for the Space Shuttle and demonstrated at Edwards with the Orbiter Enterprise in 1977.[23] Had this been all that the X-24A contributed, the program would have been satisfactory. Instead, however, the ugly duckling turned into the sleek and significant X-24B.

From X-24A to X-24B and Beyond

The X-24B was America's last postwar rocket-propelled research aircraft. It owed its existence to the activities of the Flight Dynamics Laboratory in the late 1960s which had led to the evolution of three specific reentry shapes, the FDL-5, -6, and -7, having a hypersonic L/D = 2.5 and large internal volume (see Figure 3). These configurations were all suited to hypersonic aircraft capable of flight from Mach 4 to orbital velocities, but were tailored primarily for aircraft operating in the Mach 8-12 performance regime. The Air Force hoped that these shapes would prove useful for sustained hypersonic-cruise aircraft using air-breathing propulsion, as well as for unpowered boost-glide orbital reentry vehicles capable of landing at virtually any convenient airfield. The Flight Dynamics Laboratory saw an opportunity to explore the capabilities of these new shapes by using the approach previously taken with the M2, HL-10, and SV-5: namely, to build a low-speed piloted demonstrator for supersonic, transonic, and subsonic testing.

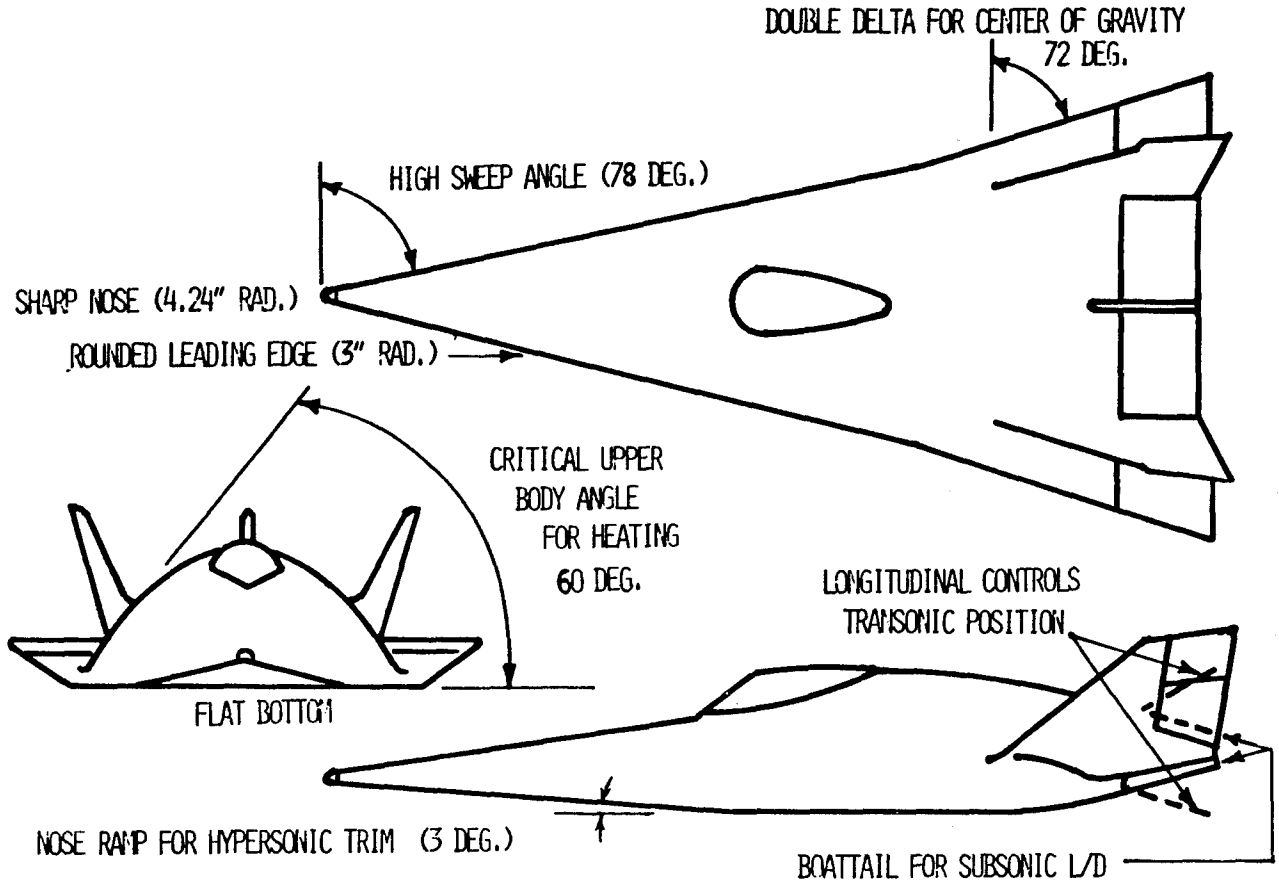
At first, Alfred Draper and his colleagues at the Flight Dynamics Laboratory thought that the shape could be gloved onto one of the two abortive SV-5J jet-powered trainers that Martin had proposed as potential Test Pilot School trainers, but which had been canceled for safety reasons. The gloved FDL-7 shape wrapped

around the SV-5J, modified to incorporate the three vertical fins of the existing SV-5 configuration, received the designation FDL-8. In January 1969, the FDL issued a proposed development plan for the project, the jet-powered craft to be launched from a B-52 mothership. As studies matured, however, the advantages of rocket propulsion for higher performance became more and more obvious. During a research trip to Edwards, Draper decided to wrap the configuration around the X-24A itself; configuration studies using tunnel models tested at Wright-Patterson confirmed that the shape had potential, and FDL planning went ahead. Already NASA lifting body supporters, including Flight Research Center's Paul Bikle, and Fred J. DeMeritte (OART's chief of the NASA lifting body program) had endorsed the plan and offered tentative NASA support. At this juncture, however, a critical stumbling block appeared: Major General Paul T. Cooper, Chief of RTD, rejected the notion of using the X-24A as a basis for the shape test, made clear his opposition to the entire flight test concept, and asked that the proposal be reviewed by a joint Air Force Scientific Advisory Board - National Academy of Sciences panel. Draper and Robert Hoey of the AFFTC briefed the panel on the concept; happily the panel concluded that the Air Force could not afford to do <u>without</u> the project. Thus secure, the plan advanced rapidly. By the end of August 1970, the directors of both the NASA FRC and the AFFTC had concurred that such a program was worthwhile, but Air Force Systems Command delayed its approval pending suitable arrangements for joint NASA-Air Force funding. On March 11, 1971 NASA transferred $550,000 to the Air Force to initiate acquisition of the aircraft. The Air Force pledged a similar amount, and on April 21, 1971 the AFSC's director of laboratories gave the program its go-ahead, five months later than supporters had desired. On June 4, 1971 the X-24A completed its last flight. On January 1, 1972 the Air Force awarded the Martin Marietta Corporation the modification contract. On February 4, 1972 Grant L. Hansen, Assistant Secretary of the Air Force for

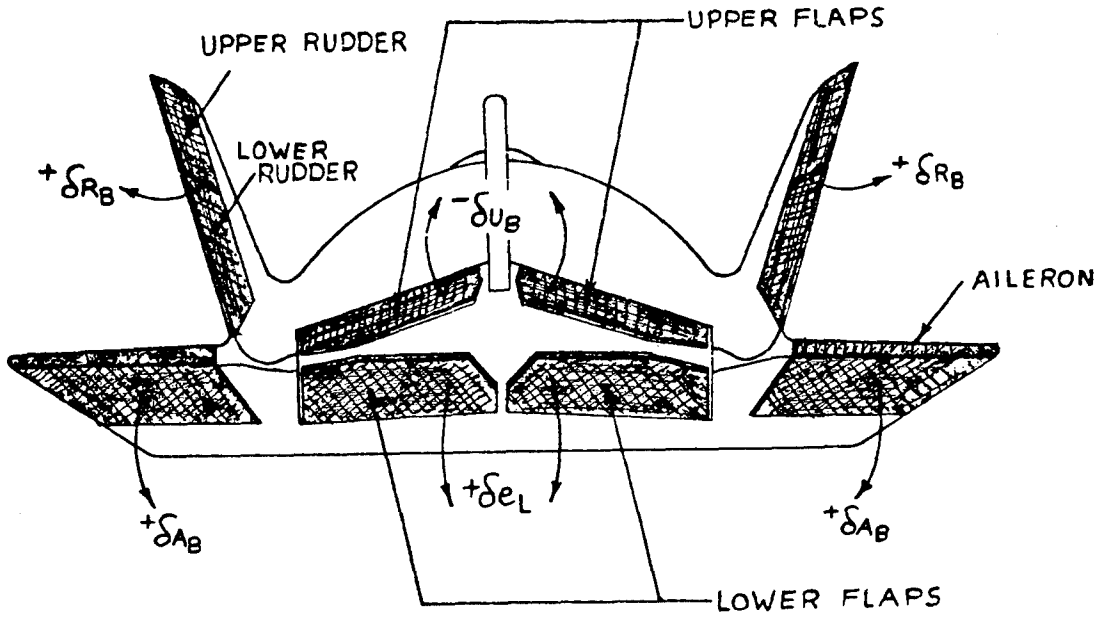
Research and Development, and John S. Foster, Jr., the Director of Defense Research and Engineering signed off a joint Memorandum of Understanding between the Air Force and NASA on the conduct of the upcoming X-24B program. This memo had previously been signed by George M. Carr, NASA Deputy Administrator, and Roy P. Jackson, NASA Associate Administrator for Advanced Research and Technology, and it superseded the previous X-24A MOU; the X-24B program was now officially under way. Modifying the existing craft secured for $1.1 million a research vehicle that could have cost $5 million if built from scratch. Hypersonic tests at the Air Force's Arnold Engineering Development Center indicated that the FDL-8 shape performed well at those speeds. However, as always, the big question was what happened when the vehicle decelerated to much lower velocities. As Fred DeMeritte stated at the beginning of the program, "We are looking for surprises as we go through transonic."[24]

Martin Marietta Corporation's Denver plant delivered the X-24B in the fall of 1972. It had grown 10 feet in span and 14.5 feet in length and weighed 13,800 lbs at launch. It had a 78 degree "double delta" planform for good center-of-gravity control, a boat-tail for favorable subsonic lift-to-drag characteristics, a flat bottom, and a sloping 3 degree nose ramp for hypersonic trim. Figure 6 shows the aerodynamic design features distinguishing this craft from the earlier X-24A as well as its control surface layout. Like the earlier lifting bodies, the X-24B used several off-the-shelf components; portions of its landing gear, control system, and ejection system came from the Northrop T-38, Lockheed F-104, Martin B-57, Grumman F11F, Convair F-106, and the North American X-15. It had an XLR 11 rocket engine and Bell Aerosystem landing rockets. Once the aircraft was back at Edwards, technicians installed a research instrumentation package. Program managers Johnny Armstrong (Air Force) and Jack Kolf (NASA) supervised preparations for the first flight.[25]

Figure 6



AERODYNAMIC DESIGN FEATURES OF THE X-24B



FLIGHT CONTROL ARRANGEMENT OF THE X-24B
[ALL CONTROLS IN NEUTRAL (δ = 0 DEG) POSITION]

John Manke completed the X-24B's first glide flight on August 1, 1973, launching from the B-52 carrier aircraft at 40,000 feet, coasting earthward at 460 miles per hour, and performing a series of handling-qualities maneuvers and a practice landing approach before making a 200 mile per hour landing on the lakebed. The flight initiated the usual sort of programs and investigations that accompany all new research aircraft. On succeeding missions, Manke and the Air Force project pilot, Maj. Michael V. Love, checked the vehicle's behavior in a variety of configurations. Following this series of glide flights the X-24B made its first powered flight piloted by John Manke on November 15, 1973. As always, the pilots practiced for their brief seven minute sojourns in the X-24B with numerous lifting body simulation approaches in T-38 and F-104 aircraft. (By the end of the X-24B program, pilots had flown more than 8000 such approaches in support of the entire lifting body program.) On the X-24B's sixteenth flight, on October 24, 1974, Mike Love reached Mach 1.76 (1164 miles per hour), the craft's fastest flight. Manke followed this on May 22, 1975 by making the craft's highest approach and landing, coming down to the lake from a height of 97,000 feet. Both Love and Manke were pleasantly surprised by the handling qualities at all speed ranges, and with and without engaging the control dampers in the stability augmentation system. Even in turbulence the aircraft flew surprisingly well; its handling qualities, including the landing approach, reminded pilots of the F-104. Its subsonic handling qualities in general earned the X-24B a rating of 2.5 on the Cooper-Harper pilot rating scale, a very high mark. In short, it was a fine airplane.[26]

By mid-1975 the Space Shuttle was well into its design phase; mission planners were still interested in whether such unpowered low L/D reentry shapes could demonstrate successful landings on the relatively confined geographical and heading constraints of a fixed runway. John Manke was convinced that the X-24B could execute such an approach and landing. He recommended that the

lifting body - which, in contrast to its fellows, did have nosewheel steering - make a series of landings on the main 15,000 foot concrete runway at Edwards, Runway 04/22. Manke, Love, and others considered such a demonstration important to developing the confidence to proceed with similar landings of the Space Shuttle itself. In January 1974 the X-24B Research Subcommittee approved the proposal. Manke and Love began a three-week familiarization program flying F-104 and T-38 approaches that simulated the X-24B's characteristics. Manke alone shot over 100 such approaches. The payoff came on August 15, 1975, when Manke launched from the mothership B-52, ignited the XLR 11 engine, climbed to 60,000 feet, and began his descent. Seven minutes after launch, Manke touched down precisely at the planned target mark 5,000 feet along the Edwards runway. Afterward he said: "We now know that concrete runway landings are operationally feasible and that touchdown accuracies of ±500 feet can be expected. We learned that the concrete runway, with its distance markers and unique geographical features, provides additional 'how goes it' information not available on our current lakebed runways." Two weeks after Manke's first runway landing, Mike Love duplicated the feat. The runway landing program, a major accomplishment, brought the X-24B research program to a conclusion. The Air Force and NASA embarked on a series of pilot checkout flights.[27]

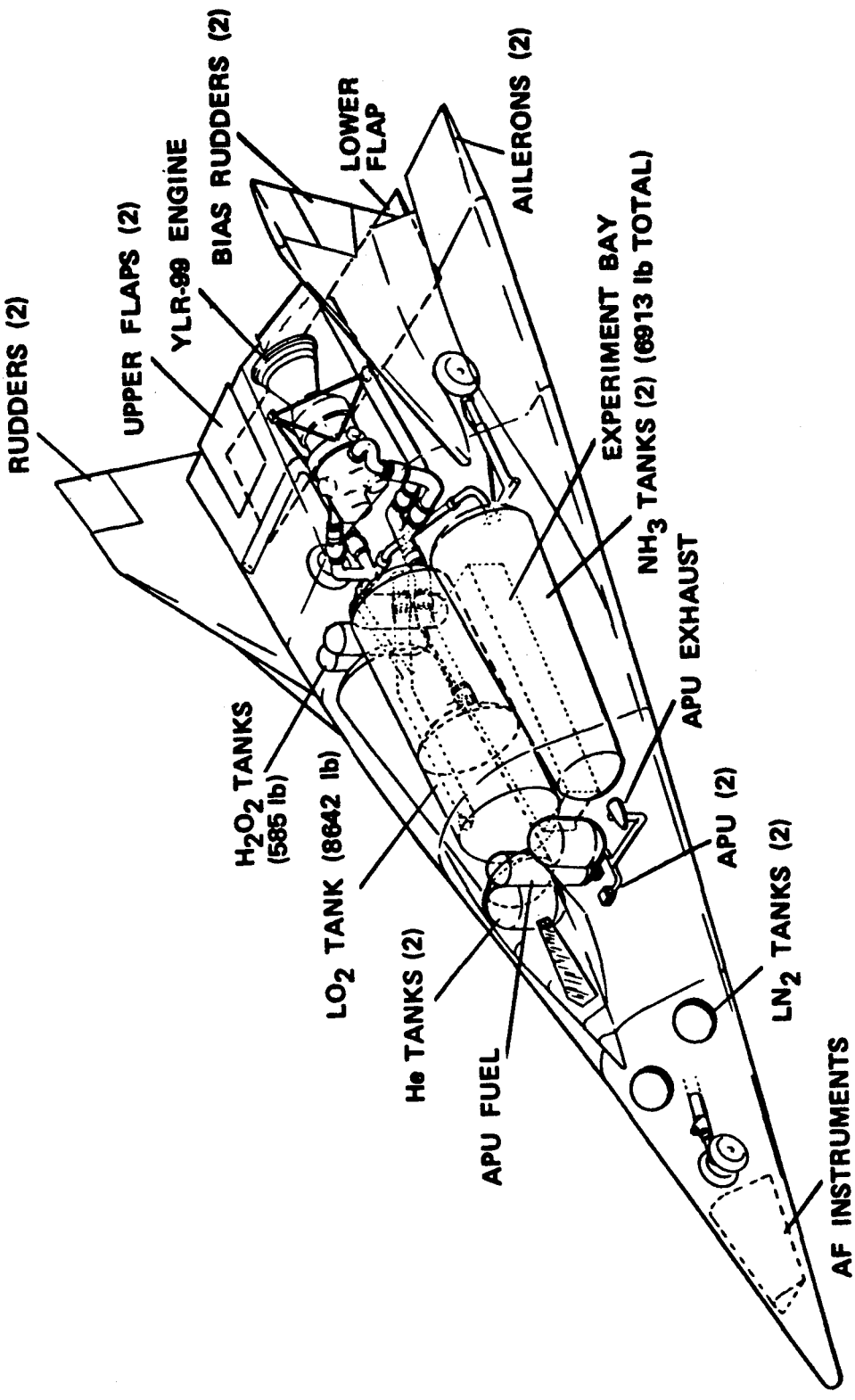
On September 9, 1975 Bill Dana completed the X-24B's last powered flight, a flight that also brought to an end the postwar American rocket research aircraft program. Following Dana's flight, the X-24B completed a series of six pilot familiarization glide flights, by Air Force Capt. Francis R. Scobee and NASA's Einar Enevoldson and Tom McMurtry. On November 26, 1975 the X-24B dropped from the sky for the last time, piloted on its 36th flight by McMurtry. The NASA flight report concluded laconically that "all objectives for this flight were attained." Through the following spring the X-24B remained at Edwards, resplendent in blue and white. Then it departed for the Air Force Museum, where

it is currently exhibited. Next to it sits ASSET and PRIME, and a mocked-up SV-5J configured as the X-24A. It is a fitting display of the Air Force's contributions to hypersonic flight.

Even before the X-24B plunged from the skies over Edwards, Flight Dynamics Laboratory had more ambitious plans: making an actual hypersonic Mach = 5.05 research aircraft powered by the X-15's LR99 engine and having the same generic configuration as the X-24B. This proposal became known as the X-24C, and eventually evolved itself (combining with NASA hypersonic research aircraft interests) into an ambitious but ill-fated program known as the National Hypersonic Flight Research Facility (NHFRF). Reputedly this awkward name was chosen to prevent the program from being known by a breezier acronym, but, if this was the intent, it was to no avail, as program proponents (and critics as well) referred to it as if the abbreviation were pronounced "nerf."

Eventually, X-24C gained the distinction of being the most extensively studied and analyzed "non-flown" hypersonic vehicle. It was conceived and designed "in-house:" that is, within the Flight Dynamics Laboratory. Draper and his team pressed hard for fulfillment of this idea, and he had enthusiastic support from the Commander of Air Force Systems Command, General Sam Phillips, best known previously as the Director of the Apollo lunar landing program. Draper and his team anticipated that the X-24C could be accomplished for $60 to $70 million--a not inconsiderable sum, but still relatively modest by research and development standards of the 1970s. X-24C had its supporters within NASA, notably those of the NASA Flight Research Center, but the onset of proposed work on the X-24C coincided with a series of hypersonic aircraft proposals underway within NASA's Langley Research Center. Eventually, these overtook the X-24C and (to a degree) swamped it in a sea of increasing cost and complexity.[28] Figure 7 shows a 1973 Flight Dynamics Laboratory drawing of the proposed internal configuration for the X-24C. The program soon changed dramatically.

Figure 7

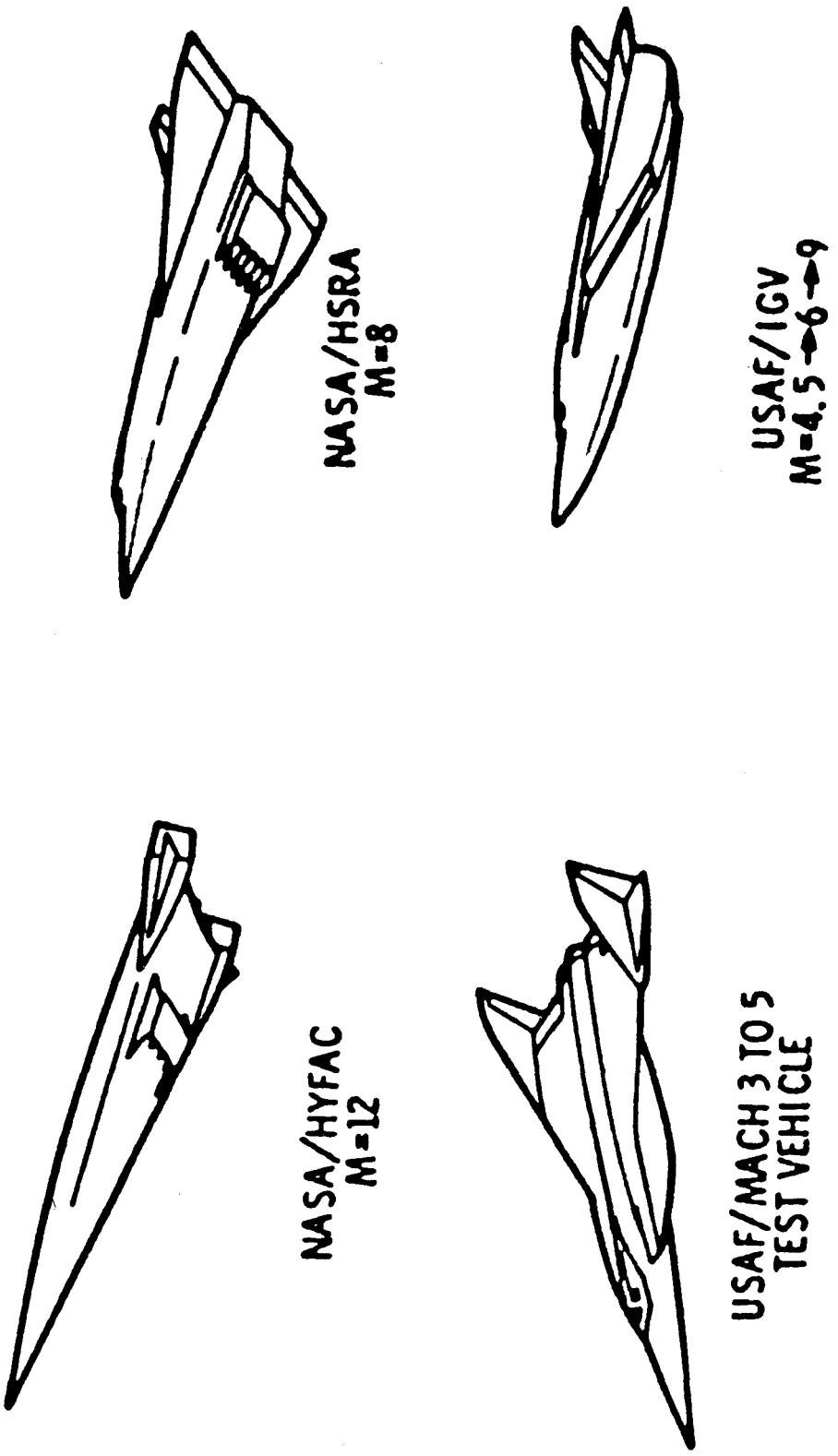


FDL X-24C PROPOSED INTERNAL ARRANGEMENT, 1973

During the late 1960s and into the early 1970s, the Air Force and NASA had played around with a number of proposed hypersonic technology demonstration projects. Northrop, at one point (for example) had offered to build a Titan-launched M2 lifting body for NASA for $200 million, but received no takers. Calls for a winged hypersonic demonstrator became clamorous following the collapse of the proposed delta-wing scramjet-powered X-15 derivative. Figure 8 shows four of these proposals, two each by NASA and the Air Force. In 1972, FDL had proposed a Mach 3-5 technology demonstrator looking somewhat like a delta-wing F-15 fighter; the next year, the service followed this with a so-called Incremental Growth Vehicle (IGV), a test aircraft initially intended for flight at Mach 4.5 but which could be progressively upgraded to Mach 6, and finally, for flight at Mach 9. For its part, NASA envisioned somewhat similar hypersonic vehicles. Langley Research Center generated the ambitious Hypersonic Research Facility (HYFAC) concept, for a Mach 12 vehicle, but followed this with a less ambitious study in 1974 for a High-Speed Research Aircraft (HSRA) intended for Mach 8. All, it will be noted, had provisions for air-breathing propulsion. By and large, Air Force studies tended to emphasize configurations suitable for military missions such as interception, reconnaissance, and strike, while NASA emphasized configurations suitable for long-range hypersonic transports (including space-capable transportation systems). X-24C, in many ways the most practical of all of them, building, as it did, upon a proven bed of aerodynamic and propulsion technology and using (even in its X-24C configuration) a substantial portion of "off the shelf" equipment, was swept along by these, and the confluence of the two streams, Air Force and NASA, joined together to spawn NHFRF--a much more ambitious program with a $200 million price tag that might have soared upwards to $600 million before completion.[29]

Starting in July 1974, after recognizing the probable high costs of the program, the Air Force and NASA teamed to study the

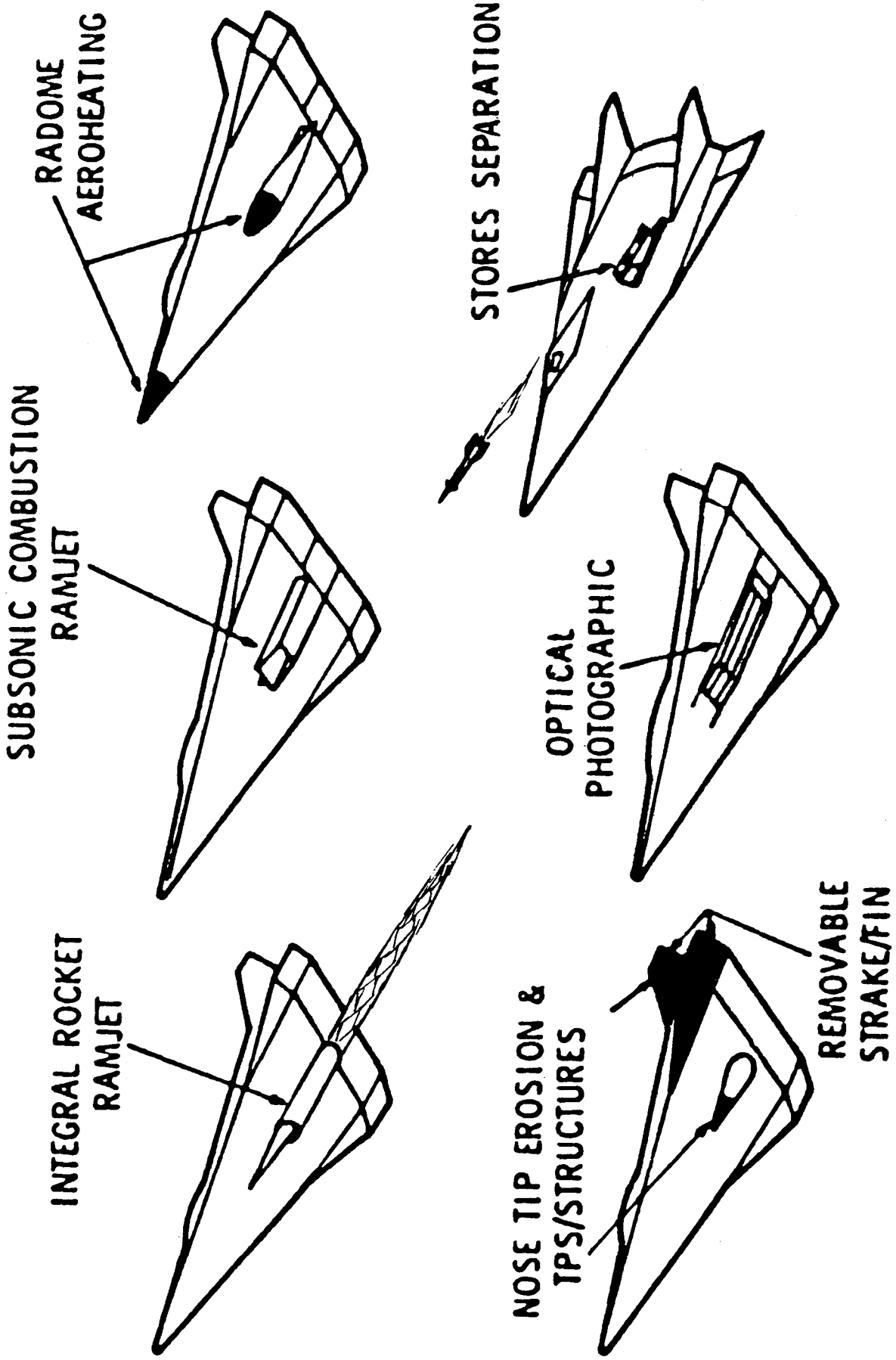
Figure 8



USAF-NASA PROPOSED HYPERSONIC DEMONSTRATORS, 1972-1974

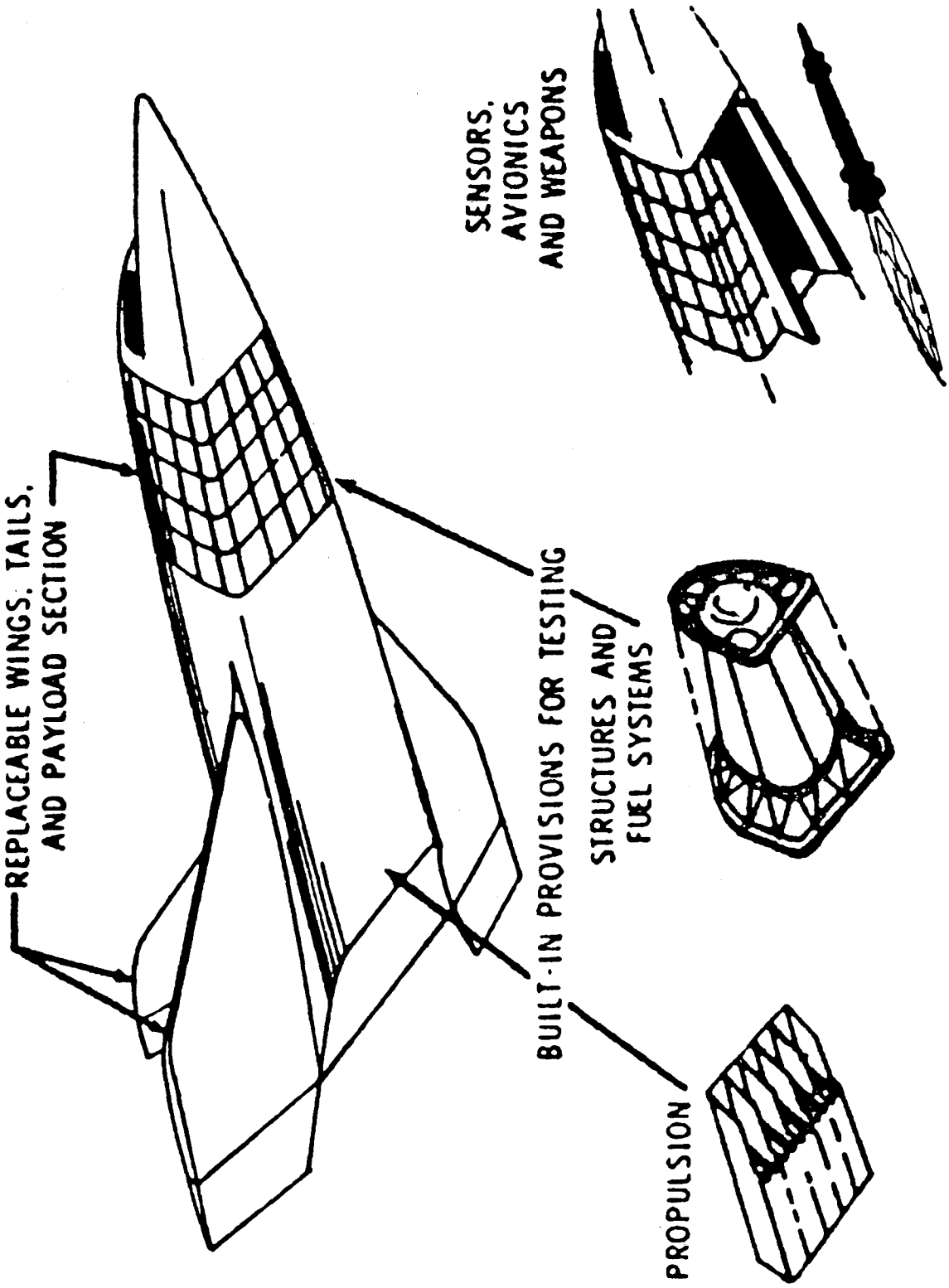
requirements for an advanced air-breathing hypersonic research aircraft. In December 1975, the Air Force and NASA formed a X-24C Joint Steering Committee, composed of the commanders of the Flight Dynamics Laboratory, the Flight Test Center, and the directors of NASA's Langley and Flight Research Centers. In July 1976, out of this joint body, came NHFRF. NHFRF's rationale was expressed well in a chart presented to a meeting of the committee; it argued a need for a single joint USAF-NASA "flight facility" that could serve as a versatile test bed to demonstrate the pacing technologies in hypersonic flight and to provide a focus for American hypersonic technology. For its part, the Air Force envisioned a vehicle that could undertake a series of military flight test experiments, as indicated in Figure 9, which shows a reproduced drawing from an X-24C steering committee report. However, when X-24C gestated into NHFRF, it lost its X-24C look and instead more closely resembled the configurations predominant in Langley thinking, as shown in another reproduced drawing included as Figure 10. By January 1977, Flight Dynamics Laboratory had prepared an advanced development technology program plan that anticipated selection of a single contractor for detail design and fabrication in March 1980, a preliminary design review in October 1980, critical design review in March 1981, first flight in January 1984, and routine flight operations commencing in October 1984. Despite a joint Memorandum of Understanding between the Assistant Secretary of the Air Force for Research and Development and NASA's Associate Administrator for Aeronautics and Space Technology, executed on December 10, 1975, NHFRF was not on solid ground, particularly as its complexity increased and costs rose. The Air Force had much of its money committed to force restructuring, a vital aspect of enhancing national defense capabilities, and NASA had much of its budget committed to the Space Shuttle. By early 1977, program costs had risen to an anticipated $500 million. In September 1977, the axe fell. NASA headquarters canceled the program, with the excuse that "the

Figure 9



PROPOSED MILITARY X-24C FLIGHT TEST EXPERIMENTS

Figure 10

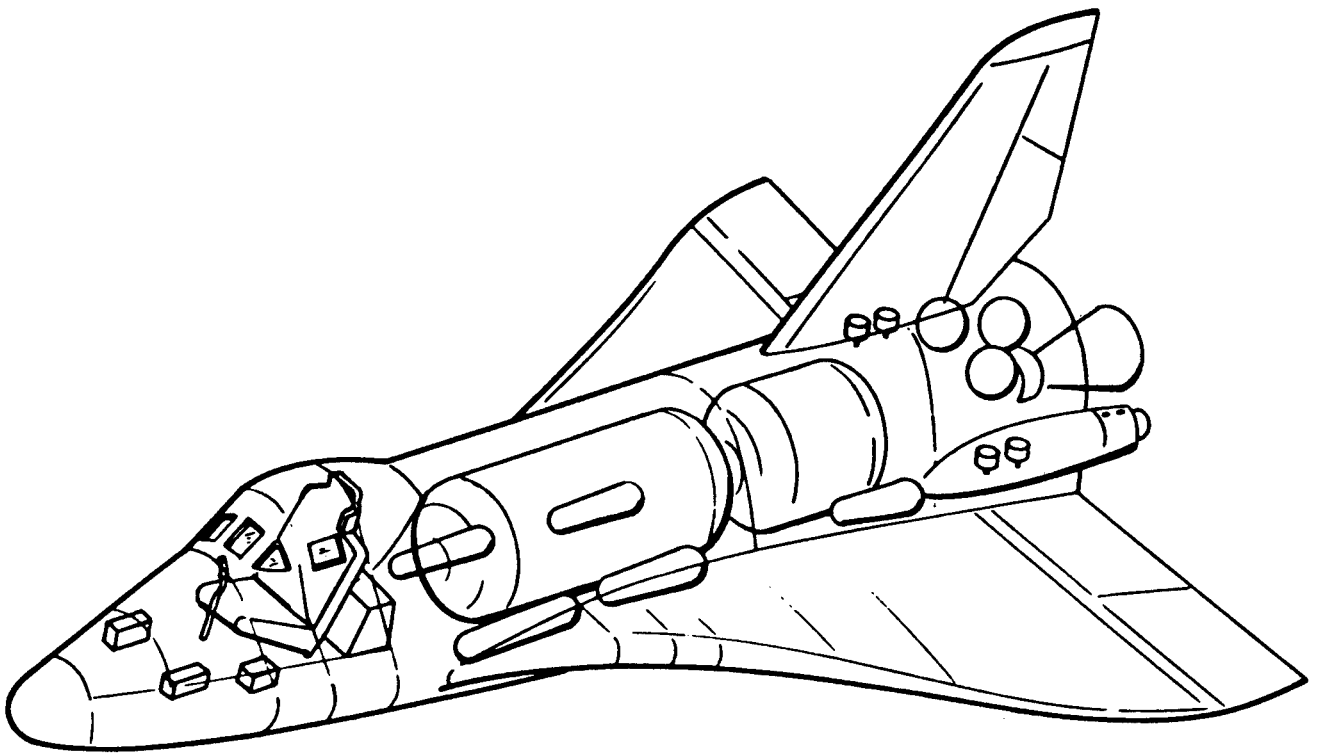


NHFRF DESIGN CONFIGURATION SHOWING MODULAR TEST PACKAGE APPROACH

combination of a tight budget and the inability to identify a pressing near-term need for the flight facility had led to a decision by NASA not to proceed to a flight test vehicle at this time." The Air Force had no option but to follow suit, as it lacked the money to go it alone. NHFRF was dead, and with it, the last chance for the United States to build a hypersonic technology demonstrator prior to the ambitious X-30 effort of the late 1980s.[30]

There was yet another effort to build a hypersonic test vehicle during the 1970s, but this was for a far different purpose than generic hypersonic technology demonstration; it was, instead, to validate the design of the actual Space Shuttle Orbiter itself via a small subscale piloted "prototype." Figures 11 and 12 show the general internal arrangement and dimensions of this small winged test craft, as well as various "growth" versions powered by LR11 and LR99 engines offering performance variation from Mach 1 through Mach 5. Proposed by the Flight Research Center in 1972, this concept died virtually immediately in the face of intense criticism from the NASA wind tunnel community, whose critics argued that the cost might rise to $150 million, well above the $19.7 million program enthusiasts anticipated for developing the Mach 5 version. Proponents hoped that NASA's Office of Aeronautics and Space Technology would authorize an immediate go-ahead so that the vehicle could fly as early as 1975 and validate wind tunnel predictions of Shuttle's stability, controllability, and performance in the hypersonic, supersonic, transonic, and subsonic regime. Despite support from NASA Manned Spacecraft Center director Robert R. Gilruth, as well as support from Martin, Northrop, and the Shuttle manufacturer itself, Rockwell International, the subscale shuttle proposal remained a paper airplane. However valuable it might have proven, it did not play a role in Shuttle development, and the Shuttle proved to have "as predicted" performance when it made its first spectacular reentry from space in 1981. Nevertheless, the success of Shuttle should

Figure 11



Length: 36 ft.

Span: 23 feet

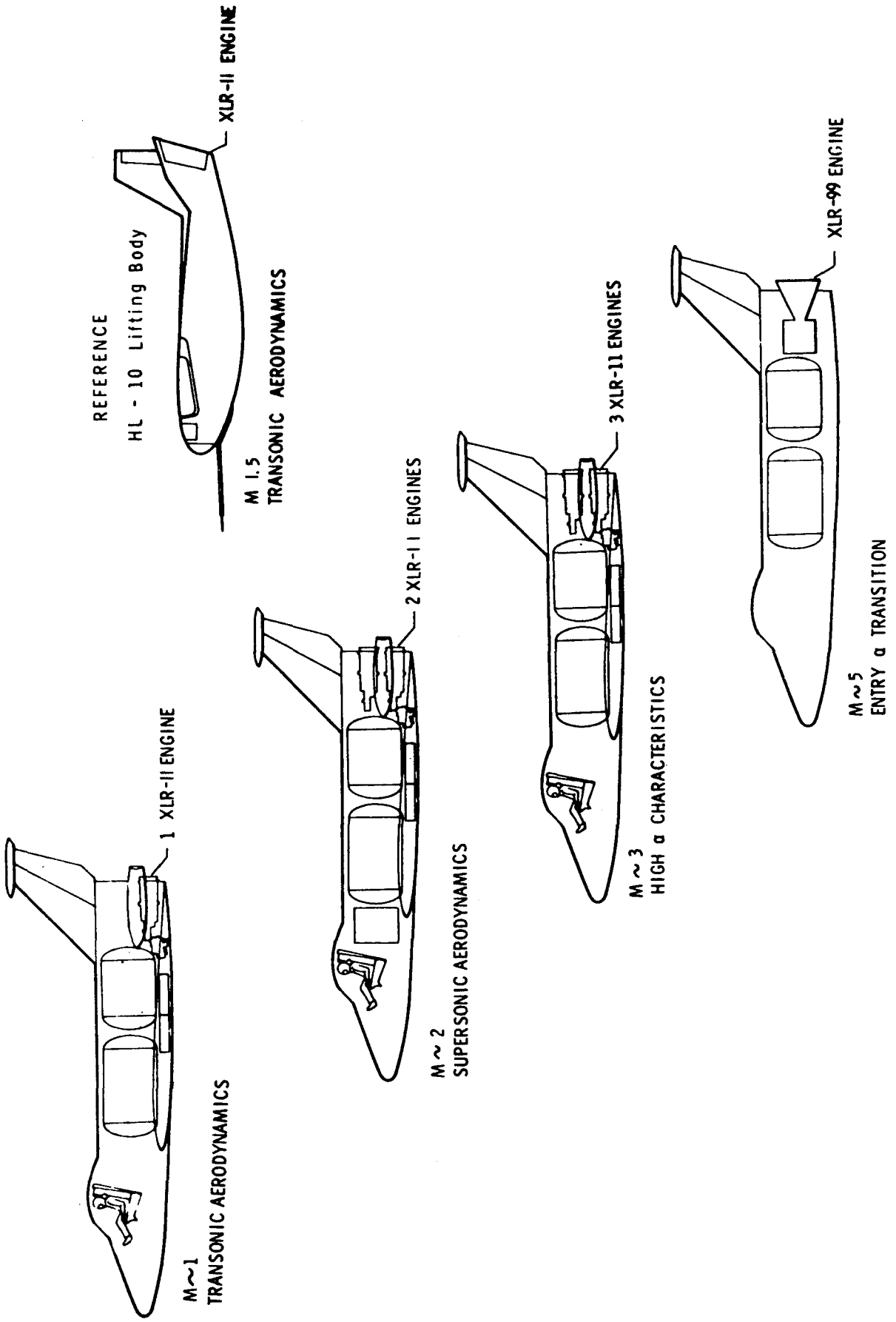
Gross weight: 30,320 lbs.

Off-the-shelf systems from M2-F3, YF-16, X-15, Apollo, YF-12, F-15, and F-4 vehicles

Structure of aluminum with thermal protection system overlay

INTERNAL ARRANGEMENT OF SUBSCALE SHUTTLE PROPOSAL

Figure 12



PROPULSION AND PERFORMANCE OPTIONS FOR SUBSCALE SHUTTLE

not obscure the fact that the Flight Research Center's subscale shuttle idea was a good one--and one that, in serendipitous fashion, may have had far more significance to hypersonic flight than originally intended had it been flying when NHFRF went through its final spasmodic collapse.[31]

All of this, of course, is conjecture; X-20, the Northrop $200 million M2 proposal, the FRC mini-Shuttle, and the X-24C and NHFRF all belong to that class of "what might have been" projects. In the realities of hypersonic history, it is sufficient to say that they played their part, and the actual vehicles developed--from X-15 through ASSET, PRIME, and the M2, HL-10 and X-24A and X-24B joined with them in producing a technology base sufficient, by the end of the 1960s and beginnings of the 1970s, for the United States to embark on the most ambitious hypersonic venture of all: the design and development of an actual winged logistical spacecraft--the Space Shuttle.

NOTES

1. LUNEX is discussed in the Air Force Space Systems Division's Lunar Expedition Plan: LUNEX (Hq. SSD, Support Systems Plans Division, May 1961), copy in the files of the NASA JSC History Office, and in Courtney G. Brooks, James M. Grimwood, and Loyd S. Swenson, Chariots for Apollo: A History of Manned Lunar Spacecraft (Washington, D.C.: NASA, 1979), p. 62.

2. From Alfred C. Draper and Charles J. Cosenza, "Technological Prospects for High-Performance Spacecraft," a paper prepared for the American Institute of Aeronautics and Astronautics (AIAA), May 1968, transmitted to the author by Charles J. Cosenza.

3. Drawing transmitted to the author by Alfred C. Draper. I wish to acknowledge with grateful appreciation the assistance of Mr. Draper to my research on lifting reentry technology and hypersonic flight.

4. Interview with Alfred C. Draper, December 18, 1986. For contemporary interest in hypersonic wave riders, see Kevin G. Bowcutt, Optimization of Hypersonic Wave Riders Derived From Cone Flows Including Viscous Effects (College Park, MD: Ph.D. Dissertation, Department of Aerospace Engineering, University of Maryland, August 1986), and Kevin Bowcutt, John Anderson, and Diego Capriotti, "Viscous Optimized Hypersonic Wave Riders," Paper 0247, American Institute of Aeronautics and Astronautics, January 1987. Models of Bowcutt's concepts are scheduled to be tunnel-tested by the NASA Langley Research Center in the near-future.

5. The Project Forecast studies were issued by AFSC in March 1964, and the detailed contents are still classified SECRET. Readers possessing appropriate clearance and having a need to know can examine these studies, which are in the historical files of ASD's History Office, and in the files of the History Office, Air Force Systems Command, Andrews AFB, MD; Draper interview; for an example of the DoD input into pre-Shuttle designs (to be discussed more fully in the Shuttle case study), see Aeronautics and Astronautics Coordinating Board, Report of the Ad Hoc Subpanel on Reusable Launch Vehicle Technology (supporting Space Research and Technology Panel, Aeronautics and Astronautics Coordinating Board), 14 September 1966, passim. Copy in the Space Shuttle records files of the NASA JSC History Office in folder "June-September 1966."

6. Ltr., Colonel C. L. Scoville, Director, Program START, to Hq. AFSC (STLO), Ames Research Center, 10 December 1964, subj: SV-5 Wind Tunnel Test Program. Ltr., Colonel C. L. Scoville, Director, Program START, to Hq. AFSC (MSFU), 10 December 1964, subj: SV-5 Wind Tunnel Test Program in NASA Facilities.

7. Ltr., Capt. K. Hughey, Project Officer, to Colonel C. L. Scoville, Director, Program START, 19 December 1964, subj: PILOT Portion of 18 December Briefing to General Funk.

8. Ltr., Colonel C. L. Scoville, Director, Program START, to AFFTC (FTO), 24 December 1964, subj: SV-5 Low-Speed Flight Tests.

9. Formal submittal of information for General Funk's weekly staff meeting, submitted by Colonel Scoville, 11 January 1965.

10. Msg., MSFU-12126, AFSC to SSD, 26 January 1965.

11. Msg., AFRST-91926, Hq USAF to Hq. AFSC, 6 March 1965.

12. Ltr., General B. I. Funk, Commander, SSD, to General B. A. Schriever, Commander, AFSC, 14 April 1965, subj: START Program.

13. Msg., SSG-1005, General Funk to General Schriever, 28 May 1965.

14. Memo for Record, Colonel Scoville, Director, Program START, 18 June 1965, subj: PILOT Project Meeting at ODDR&E, 16 June 1965.

15. Ibid.

16. Ibid.

17. Martin Company letter (MT-00884), V. R. Rawlings, President, Martin Company, to Colonel C. L. Scoville, Director, Program START, 12 July 1965, subj: Proposal for SV-5 Low-Speed Manned Test Vehicles.

18. Formal submittal of information for General Funk's weekly staff meeting, submitted by Colonel Scoville, 20 September 1965.

19. Ltr., Colonel L. S. Rochte, Deputy for Technology, SSD to Hq. AFSC (MSF), 27 September 1965, subj: Single Vehicle, Minimum Cost PILOT Project.

20. Ibid.

21. Memo, Bisplinghoff to NASA Associate Administrator, 30 June 1965; NASA Management Instruction 1052.96, "NASA-DoD (USAF) Memorandum of Understanding: Use of X-24A Research Vehicle in Joint Lifting Body Flight Research Program," 7 November 1967.

22. Brun chronology; X-24A flash reports.

23. Kolf interview.

24. Everly Driscoll, "The Shape of Things to Come?" Science News (15 September 1973), pp. 171-172; Michael L. Yaffee, "X-24B Lifting Body Nearing Completion," Aviation Week & Space Technology (4 September 1972), pp. 77-79; John A. Manke and M. V. Love, "X-24B Flight Test Program," 1975 Report to the Aerospace Profession, The Society of Experimental Test Pilots, 26 September 1975; Johnny G. Armstrong, Flight Planning and Conduct of the X-24B Research Aircraft Flight Test Program, AFFTC-TR-76-11 (Edwards AFB: AFFTC, 1977), pp. 12-14 (hereafter cited as Armstrong X-24B Report); Draper interview.

25. Manke and Love, "X-24B Flight Test Program;" NASA Management Instruction 1052.96B, "Provision for the Use of the X-24B Research Vehicle in a Jointly Sponsored NASA-DoD (USAF) Lifting Body Flight Research Program," 4 February 1972; Armstrong X-24B Report, pp. 16-26; NASA news release 71-139; NASA FRC news release 30-71; notes of telecon, C. Karegeannes with Ralph Jackson, 13 December 1972, in NASA History Office X-24B project files.

26. NASA X-24B flight reports; Brun chronology; Manke and Love, "X-24B Flight Test Program;" Armstrong X-24B Report, pp. 89-97.

27. Manke and Love, "X-24B Flight Test Program;" Brun chronology; interview with John Manke, 1 December 1976; Armstrong X-24B Report, pp. 78-82.

28. Draper interview.

29. Ibid.; see also Richard E. Brackeen and William L. Marcy, X-24B Growth Version Feasibility Study, AFFDL-TR-73-116 (Wright-Patterson AFB, OH: AFFDL, October 1973), passim; Donald P. Hearth and Albert E. Preyss, "Hypersonic Technology: Approach to an Expanded Program," Astronautics & Aeronautics, XIV, 12 (December 1976), pp. 20-37.

30. Ibid., Ltr., Robert A. Frosch (NASA Administrator) to Dale Milford (Chairman, Subcommittee on Transportation, Aviation and Weather, House Committee on Science and Technology), 9 November 1977 (copy in "Hypersonic a/c" file, NASA History Office); see also F. S. Kirkham, L. Robert Jackson, and John P. Weidner, "Study of a High-Speed Research Airplane," AIAA Journal of Aircraft (November 1975), pp. 857-863; ltr. of agreement between AFFTC, AFFDL, NASA LaRC, and NASA FRC, 15 March 1977; AFFDL, "Technology Program Plan: NHFRF" (January 1977), passim; quote from "NASA to End Manned Hypersonic Effort," Aviation Week & Space Technology (26 September 1977), p. 24. I am indebted to Mr. Frank Boensch of the NASP Program Office, Wright-Patterson AFB, Ohio for providing me with a compilation of X-24C Joint Steering Committee Reports and documents, which proved most useful to this study.

31. NASA FRC, "Subscale Shuttle" (17 August 1972); interview with Milton O. Thompson, 9 August 1977; interview with Joe Weil, 18 August 1977.

CASE VIII

SPACE SHUTTLE: FULFILLMENT OF A DREAM

by

Richard P. Hallion

James O. Young

EDITOR'S INTRODUCTION

The development of the actual Space Shuttle system posed engineering challenges on a scale unparalleled in previous aircraft-type systems, and equaled only partially by the demands of the Apollo program of the 1960s. A review of Shuttle history indicates several major phases representing periods in which certain design goals seemed desirable and/or likely. Figure I offers a greatly simplified perspective on the historical contributions to Shuttle's technology base. In the 1950s, still in the "pre-history" phase of Shuttle, conceptualizers envisioned two-stage fully reusable winged configurations, with piloted fly-back launch vehicles and winged orbiters. Briefly, during the Aerospaceplane program of the late 1950s and early 1960s, planners toyed with the idea of a complex air-breathing and air-extraction single-stage-to-orbit vehicle, ultimately opting for a two-stage flyback launch vehicle and orbiter before this program experienced cancellation. From this point on, Shuttle conceptualizations were characterized by increasing pragmatism. Planners conceptualized phased development, using small lifting body orbiters lofted by expendable boost stages, followed by small lifting body orbiters boosted by flyback stages, followed by larger lifting body orbiters boosted by winged or lifting body flyback launch vehicles. Then, in the mid to late 1960s, two somewhat rival trends appeared: using partially expendable stage-and-a-half approaches to spaceflight, and developing complex fully reusable winged orbiters and launch vehicles. The realities of the political situation, coupled with an increasing appreciation of the problems and complexities of the technology inherent in any Shuttle-type craft eventually dictated adaptation of a partially expendable approach using a delta wing orbiter. Development of this system took over 9 years from the time of Executive Branch go-ahead in January 1972 to its first orbital flight in April 1981. Along the way were numerous headaches and crises involving

Figure I

HISTORICAL CONTRIBUTORS TO SHUTTLE'S TECHNOLOGY BASE

development of its main engines and the flight control and thermal protection systems, coupled with unrealistic mission models and over-optimistic mission schedule expectations. Unfortunately, the system developed was still fatally flawed, as the loss of the Orbiter Challenger in January 1986 tragically demonstrated. In bitter irony, it was not the high risk systems--the thermal protection system, or the main engines and their associated turbopumps, for example--that caused the loss, but, rather, an item chosen for its track record of constant reliability: the solid-fuel rocket boosters. Challenger's accident thus reminds one of the loss of the first De Havilland Comet jetliners in the early 1950s, the first commercial turbojet aircraft. The loss of the Comets delayed Great Britain's commercial turbojet airliner effort to the point where American rivals--notably the Boeing 707 and Douglas DC-8--were able to sweep the commercial jetliner market. While such is unlikely to occur with the Space Shuttle, by the time that the Shuttle returns to orbit (currently planned for June 1988, but that is a date likely to slip), other foreign lifting reentry logistical spacecraft efforts will be far along on the road to initial flight test, notably the Soviet Union's own Shuttle concept (bearing a remarkable resemblance to the American Shuttle), and France's less advanced but nevertheless well-along project for the Hermes spaceplane which likewise draws heavily upon the technology of the American Shuttle. Waiting in the wings as they pass across drawing boards are the British HOTOL, the German Sänger II, and the American National Aero-Space Plane (NASP), all ventures aimed at the late 1990s to the early turn of the next century.

This study is an examination of the evolution of the Space Shuttle vehicle and its initial flight tests. Chapter I is by Richard P. Hallion, with the brief section on the Shuttle's Approach and Landing Tests (ALT) drawn largely from the previously cited On the Frontier. Chapters II, III, and IV were prepared by Dr. James O. Young of the History Office, Air Force Flight Test Center, Edwards AFB, drawing largely upon material from the files of the Office of Advanced Manned Vehicles, AFFTC. I wish to

acknowledge the assistance of a wide range of individuals whose work and recollections proved most valuable to this study, including Alfred C. Draper; Maxime Faget; Johnny Armstrong; Bob Hoey; Col. Joe Henry Engle, USAF (ret); William Lamar; A. Miles Whitnah; Jim Grimwood; Dr. Edward C. Ezell; Dr. Sylvia Fries; Milton O. Thompson; Dr. Roger Bilstein; Dr.-Ing. Walter Rathjen; Dr.-Ing. Injas Widjaja; Col. William J. Knight, USAF (ret); Dr. John Guilmartin; Dr. John Mauer, and Lee Saegesser.

CHAPTER I

THE PATH TO THE SPACE SHUTTLE

The present Space Shuttle, the national Space Transportation System, represents a confluence of several broad technical streams ranging from the rocket research aircraft programs of the postwar years to the experience gathered within the manned and unmanned spacecraft programs undertaken since 1957. It represents an institutional accomplishment achieved by the activities of several agencies and many industrial concerns - activities that found fruition in a system generated for one agency, but useful to many others as well. NASA, the Department of Defense, and the national aerospace industry can all lay claim to a portion of the credit for developing the Space Shuttle. For these reasons, it is challenging to attempt a sorting-out of developmental influences, since virtually all players were intensely involved throughout the development process on almost every technical issue. To give but one example, aerospace companies, working two different contracts, would often utilize the same configuration for a NASA study as they had for a DoD one; when it is remembered that many of these "generic" shapes had been developed in the first place within Federal research centers (such as the Flight Dynamics Laboratory or the NASA Manned Spacecraft Center), the problem of determining "paternity" is readily apparent. This chapter will attempt to sort much of this out by showing the various influences, such as the thrust of DoD work (as represented by the activities of the United States Air Force); the relationship between DoD and NASA, exemplified by the Aeronautics and Astronautics Coordinating Board, foreign activities, and finally, the drive within NASA to generate the Shuttle from 1969 onwards.

A useful point to keep in mind for reference while reading of NASA and DoD interest in Shuttle-like craft is that the majority of these concepts envision large multi-purpose vehicles for ferry, logistical support, and transportation missions, carrying at least two crewmen. Overall, NASA concepts--at least through 1970--emphasize civilian space applications using low L/D designs. The DoD, on the other hand, is naturally more concerned with military missions such as satellite insertion, retrieval, and inspection, and tasks such as reconnaissance and possible strike from orbit. In contrast to NASA, the Air Force consistently champions higher L/D configurations. Opinion is split within both NASA and the Air Force on whether such craft should be winged or lifting body designs. Generally speaking, through much of the 1960s, both agencies express a largely unspoken but technically self-evident preference for winged boosters launching refined lifting body orbital vehicles (this, incidentally, is not limited to the United States, but also characterizes European work as well).

The Air Force and Early Shuttle Concepts

As has been stated earlier in the Dyna-Soar and PRIME case studies, the Air Force had an interest in what now would be considered Shuttle concepts as early as the 1950s. Dyna-Soar itself, it will be remembered, had provisions for a small payload bay, and while it could not be compared in terms of logistical value to some of the larger concepts dating to the mid-1960s that succeeded it, it nevertheless would have constituted a significant step forward. In 1960, the Air Force briefly envisioned using the North American B-70A Valkyrie for Mach 3+ launching of space payloads up to 15,000 lbs., contemplating 15-year savings of $2.63 billion over using more conventional staging systems. The study even contemplated using the B-70 to launch "hypersonic boost-glide and/or orbital strategic systems, like Dyna-Soar, shuttle vehicles, and a wide variety of research satellites." The

B-70 proposal (dubbed B-70 RBSS, for Recoverable Booster Space System), like the X-15/Blue Scout proposal of the same time, subsequently was not pursued, though the Air Force did brief it in Senate testimony. The first major attempt to develop a true large-scale military logistical spacecraft capable of flying into space and returning to earth in a lifting reentry was the so-called "Aerospaceplane" program of the late 1950s and early 1960s. In many ways, it bears the same relationship to the current Space Shuttle that Samuel Langley's Aerodrome bears to the Wright 1903 Kitty Hawk Flyer: an important if then-impractical pioneering effort.

Aerospaceplane was an outgrowth of 1957 Air Force Study Requirement SR-89774 of 1957, which explored the concept of developing recoverable space boosters. By 1959, it had evolved into the so-called ROLS: Recoverable Orbital Launch System. ROLS promised some remarkable attributes, notably being a single-stage design capable of taking off horizontally and boosting into a 300 mile-high orbit using a complex propulsion system using an air-collection system that would have extracted air from the upper atmosphere, compressing, liquefying, and distilling it to produce liquid oxygen which would then have been mixed with liquid hydrogen and fed to the engines. Designers hoped that by adopting this approach, the overall weight of the vehicle could be kept to a minimum, and its mission utility would be enhanced. Over time, this propulsion concept (dubbed LACES for Liquid Air Collection Engine System) and others such as ACES (Air Collection and Enrichment System) and scramjet concepts underwent evaluation. Not unexpectedly, major technical problems quickly developed, and in a bid to avoid them, some advocates actually advocated Mach 6 aerial refueling of the Aerospaceplane before it boosted into orbit, this air-refueling concept receiving the acronym HIRES (for Hypersonic In-flight Refueling System). (X-15s would have flown hypersonic formation flights to validate this idea; fortunately, cooler heads prevailed before undertaking such potentially

catastrophic experiments.) Like X-20 before it, Aerospaceplane did not have many friends in the Kennedy administration, and this at-best lukewarm support from the top, coupled with the growing technical questions, combined to eventually kill it. In 1962, in the face of the massive uncertainties involved in developing a single-stage-to-orbit (SSTO) design, the Air Force shifted focus to two-stage-to-orbit (TSTO) concepts. At an early point in Aerospaceplane studies, space payload specialists had determined that it should have a large payload bay on the order of 10 ft. by 25 ft. x 40 ft., though the Aerospace Vehicles and Propulsion panel of the Air Force Scientific Advisory Board subsequently stated that such a specific requirement was "premature," given the tentative state of the project. There was, naturally, general enthusiasm over the concept, largely because such a reusable launch system seemed to offer great flexibility for a variety of military missions including orbital supply and strike, space rescue, and a more economical method of boosting payloads into space than with expendable throwaway boosters. Most of these goals--not including orbital strike, of course--reappeared subsequently in discussions of the planned Space Shuttle, particularly those of reducing payload-to-orbit costs, orbital supply, and mission flexibility. General Dynamics, Lockheed, Douglas, Republic, Boeing, Goodyear, and North American Aviation (the predecessor of Rockwell Corporation) undertook complete system design studies for the Aerospaceplane, and General Dynamics, Douglas, and North American each received $500,000 contracts from Aeronautical Systems Division on June 21, 1963 for detailed development planning studies. Martin, under contract to the Structures Division of the Flight Dynamics Laboratory, built a full-scale wing-fuselage structure representative of an Aerospaceplane. Under the aegis of Air Force Study Requirement 651, companies undertook a variety of potentially useful research on such subjects as air collection enrichment systems, Mach 8 subsonic combustion ramjets, the LACE concept, scramjets, advanced

turboramjets termed turboaccelerators, structures and materials, and aerodynamics.

But Aerospaceplane's time was running out. Aerospaceplane had numerous critics, including some within the Scientific Advisory Board, which, as early as December 1960, had warned that it was "gravely concerned that too much emphasis may be placed on the more glamorous aspects of the Aerospaceplane resulting in neglect of what appear to be more conventional problems." In October 1963, Aerospaceplane reached the end of its tether; the SAB, having completely lost faith in the program, damned the effort in no uncertain terms:[1]

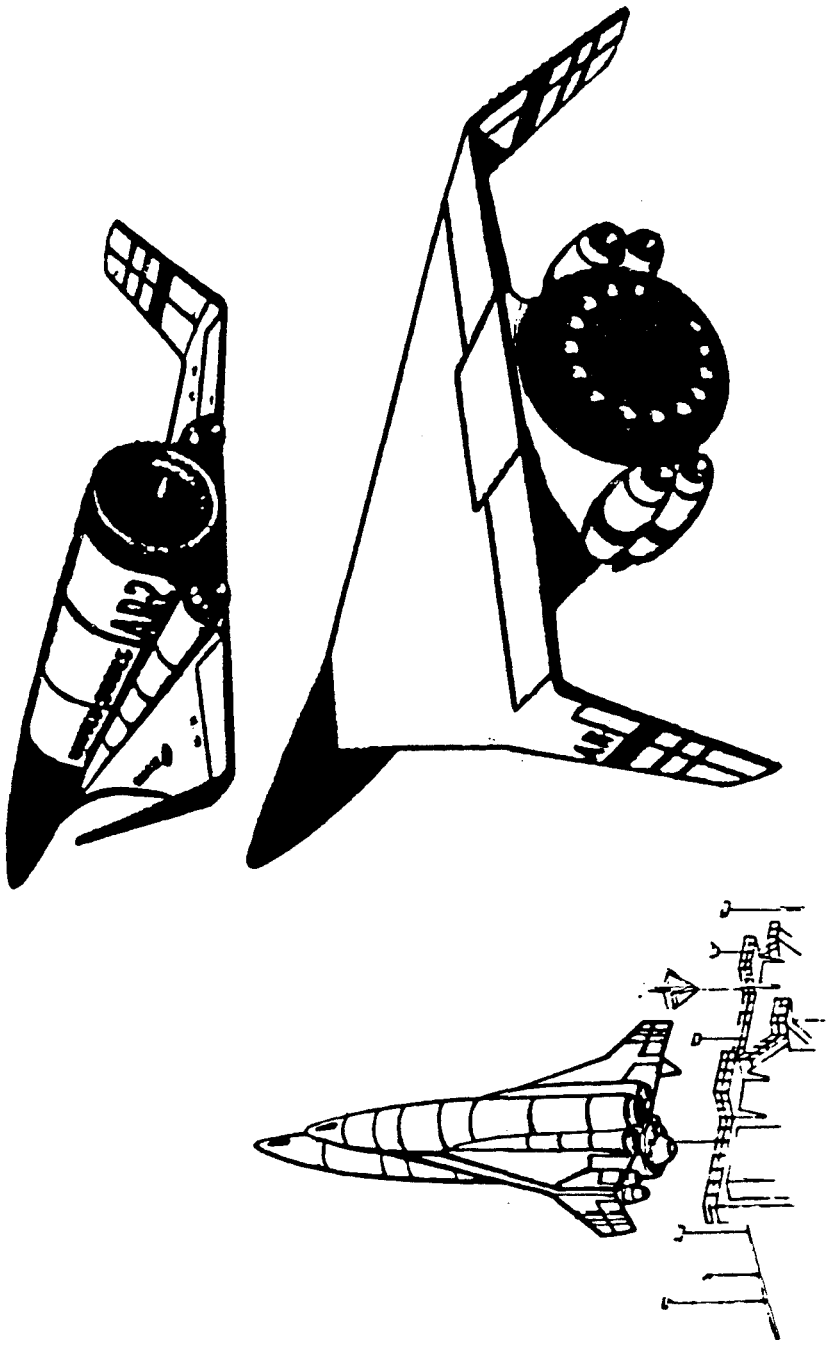
> The difficulties the Air Force has encountered over the past three years in identifying an Aerospaceplane program have sprung from the facts that the requirement for a fully recoverable space launcher is at present only vaguely established, that today's state-of-the-art is inadequate to support any real hardware development, and the cost of any such undertaking will be extremely large. While these factors dominate the picture, the Air Force must focus on advancing the important technical fields involved and prepare themselves for the time when the projected total payloads into orbit per year will increase to the point where such recoverable launching systems are competitive . . . [the SAB Aerospace Vehicles and Propulsion Panel] feels that the so-called Aerospaceplane program has had such an erratic history, has involved so many clearly infeasible factors, and has been subjected to so much ridicule that from now on this name should be dropped. It is also recommended that the Air Force increase the vigilance that no new program achieves such a difficult position.

Aerospaceplane per se finally died when Congress slashed its funding for FY64 and DoD's executive leadership declined to press for its restoration. As a 1984 Air Force Aeronautical Systems Division news release stated, "All Air Force and contractor efforts diminished to zero shortly after cancellation of Dyna-Soar." Instead, DoD directed the Air Force to reorient its 651 study program to address the needs of hypersonic cruising

flight within the atmosphere. Air Force Systems Command generally concurred, but successfully argued that the cruise studies should also address the potential of such systems as launchers of space payloads. Under this modified mandate, the Air Force continued studying both sustained cruise hypersonic atmospheric vehicles and hypersonic vehicles operating in the upper atmosphere as launch platforms for manned and unmanned second stage space payloads. Aerospaceplane had a significance that transcended its actual disappointing history. First, it was an exhaustive attempt to define the technology and mission requirements for a Shuttle-size logistical system. Its SSTO and subsequent TSTO approach to spaceflight resulted in the generation of numerous design studies that influenced subsequent Shuttle-like conceptual efforts. Secondly, the Aerospaceplane concept--a single-stage earth-to-orbit vehicle--excited the imagination. While premature, given the state of technology in the late 1950s and early 1960s, it held the promise of practicable future systems, as witness the contemporary National Aero-Space Plane (NASP) development effort, or the British HOTOL. In summary, though Aerospaceplane died, its concept and technical legacy continued on.

Dyna-Soar and Aerospaceplane had represented significant examples of military interest in space using lifting-type vehicles, but they were certainly not the only expressions of such interest. The early 1960s were a time of intense study by government and industry of the potentialities of lifting reentry vehicles for both military and civilian missions. Boeing, for example, derived a whole family--the F-6 series--of small winged lifting vehicles having more sharply swept planforms than the firm's Dyna-Soar; engineers dubbed these as "Swallow" configurations, since the planform resembled the tail of a swallow in flight. Martin promoted a TSTO concept called the Astrorocket (see Figure A) for a two-stage logistical spacecraft having a gross lift-off weight (GLOW) of approximately 2,500,000 lbs. The three-man orbiter could remain in orbit for up to two weeks; the

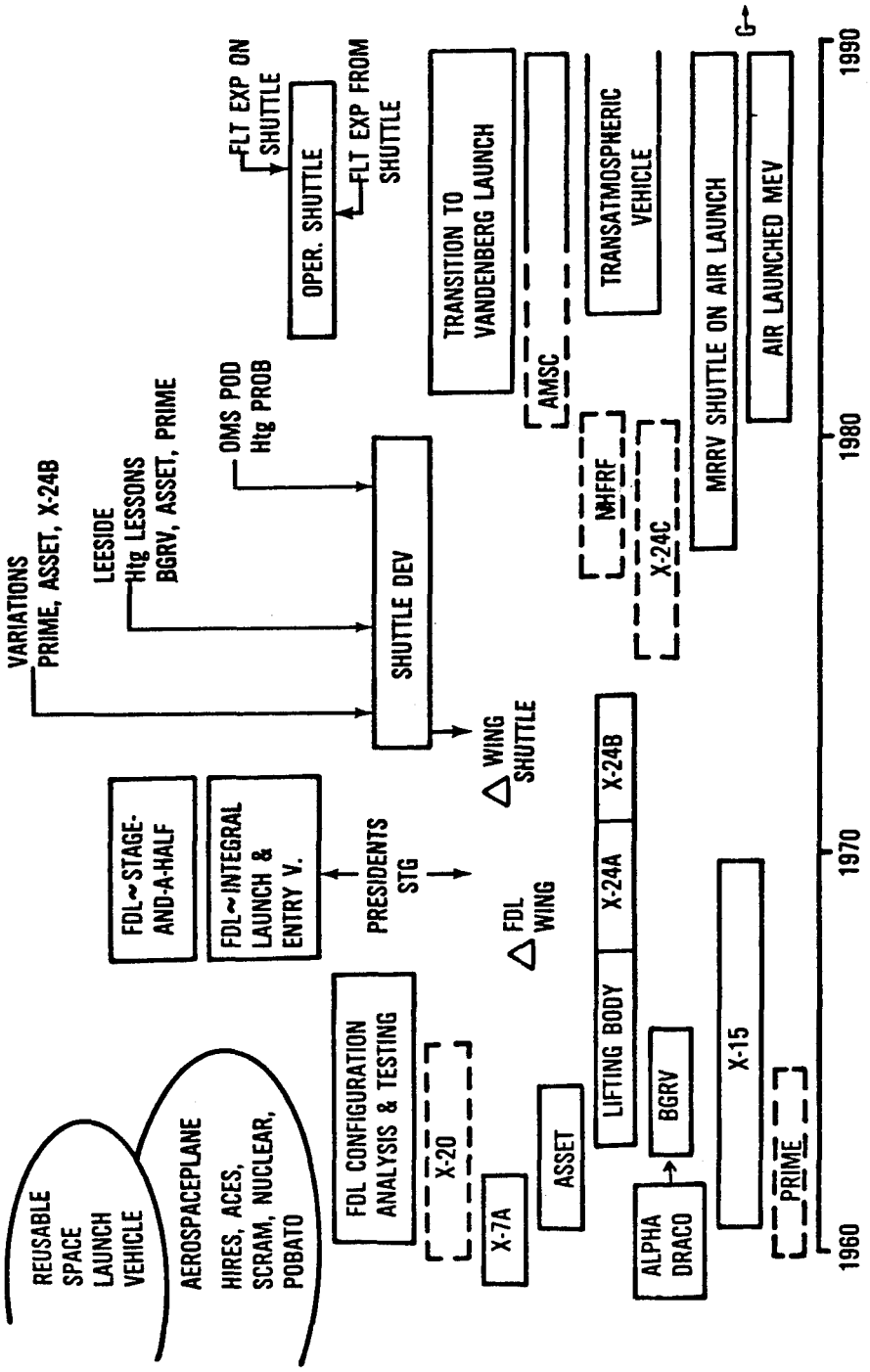
FIGURE A



MARTIN ASTROROCKET TSTO LOGISTICAL SPACECRAFT CONCEPT

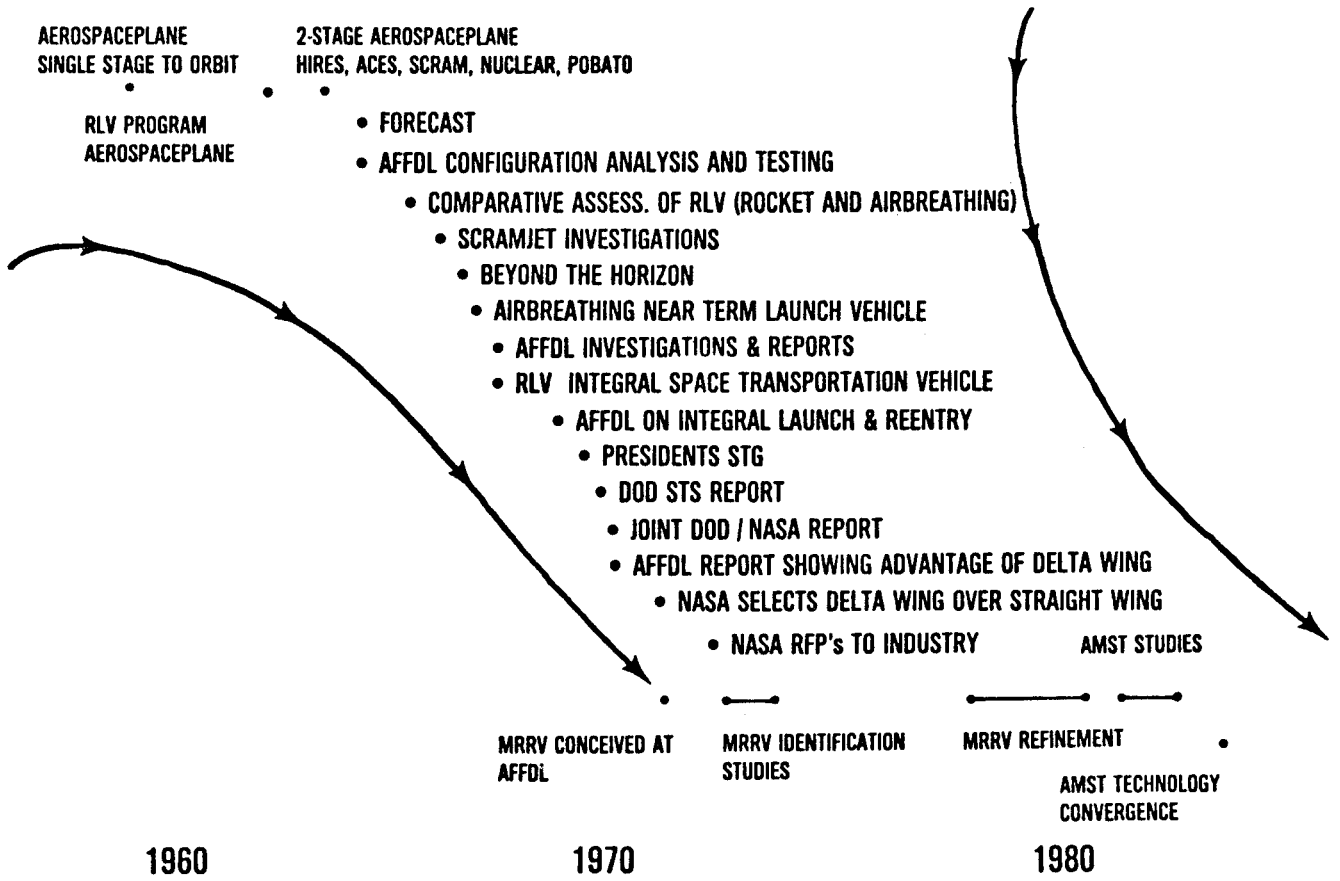
fly-back booster employed turbofan engines to cruise back to its landing site following its own reentry from space. The Astrorocket was one of the earliest of many similar proposals (such as Germany's Junkers RT-8-01) for craft that essentially employed a X-20-like configuration for the orbiter, and a booster resembling some of Ames Research Center's 1950's winged reentry vehicle design concepts. NASA's Langley Research Center derived another family (the D-series) of lifting reentry vehicles and, of course, its well-known HL-10 shape. The Air Force was deeply involved with ASSET, and PRIME was well along. The majority of military work on lifting-type vehicles undertaken by the Air Force took place at Wright-Patterson Air Force Base, the service's traditional research and development center. Figure 1 shows various Air Force-sponsored aerospace vehicle studies together with some of the service's contributions to on-going Shuttle development from roughly 1960 to the present, emphasizing work undertaken by the Flight Dynamics Laboratory. Figure 2 presents essentially the same perspective, in terms of research fields, technology starts, planning efforts (such as Project Forecast), and Air Force participation in key study efforts (such as the President's Space Task Group in 1969). What is most interesting about these studies, whether one is examining Air Force or NASA concepts, is the degree to which industry depended upon the government (either the Air Force or the civilian space agency) to furnish configurations upon which companies could base their own design studies: the government, in effect, furnishing a shape, and industry adding the contents and "filling in the blanks," activities such as Boeing's clearly being an exception to this general rule. These studies followed on the heels of Project Mercury development, occurred largely contemporaneously with Gemini, and are approximate in time with the Apollo lunar landing effort, where the national space agency had given contractors relatively little free hand in dealing with the proposed configurations of those spacecraft. The tight nature of early

Figure 1



AIR FORCE INITIATIVES IN THE SHUTTLE FIELD, 1960-PRESENT

Figure 2



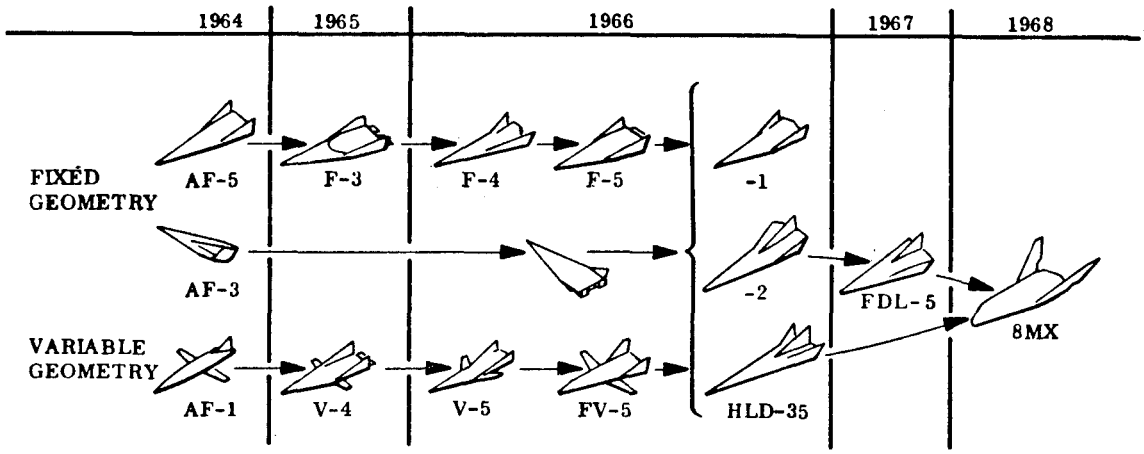
FLIGHT DYNAMICS LABORATORY SPACE-RELATED WORK, 1960-1980

agency-industry relationships on configuration development with these initial manned spaceflight projects has led some commentators and even participants to comment that such had loosened by the time of Shuttle development, giving contractors a freer hand to undertake their own design. Such comments, however, seem unwarranted when examining the overall nature of Shuttle studies from the early 1960s onwards: in each case, whether dealing with Air Force-inspired configurations or NASA-inspired ones, contractors generally danced to an Air Force or NASA tune as regards the overall configuration itself.[2]

A good example of this can be drawn from the evolution of Flight Dynamics Laboratory's lifting body shapes and their application to proposed Lockheed Shuttle-like vehicles. Lockheed worked with the FDL on studies of high L/D reentry vehicles, starting with FDL's fixed-geometry F-3 and variable-geometry V-4 concepts, each of which had a maximum hypersonic L/D = 2.7, and moving on through the FDL-5 shape. Figure 3 is taken from a Lockheed study report of 1969, prepared in response to an Air Force contract, and consisting of a conceptual study for a so-called "stage-and-a-half" space transportation system. Figure 4 shows a schematic view of the elegant delta lifting body orbiter (with small variable sweep wings) nestled between two huge fueltanks, hence the name. Essentially, this proposed orbiter drew on two streams of Flight Dynamics Laboratory work: first, the development of the stage-and-a-half concept (shown in Figure 3b, using a parallel tankage configuration from a Flight Dynamics Laboratory paper prepared for the American Institute of Aeronautics and Astronautics in May 1968 by Alfred Draper and Charles Cosenza) utilizing two jettisonable external tanks, and second, the stream of increasingly high L/D FDL reentry shapes (shown in Figure 3a) that eventually culminated in the candidate shape for the Lockheed vehicle, the so-called 8MX configuration. Figure 5 gives details on proposed missions and capabilities using such a configuration, and Figure 6 shows the profile of a typical

Figure 3

Fig. 3a: FDL SHUTTLE EVOLUTIONARY CONCEPT STUDIES



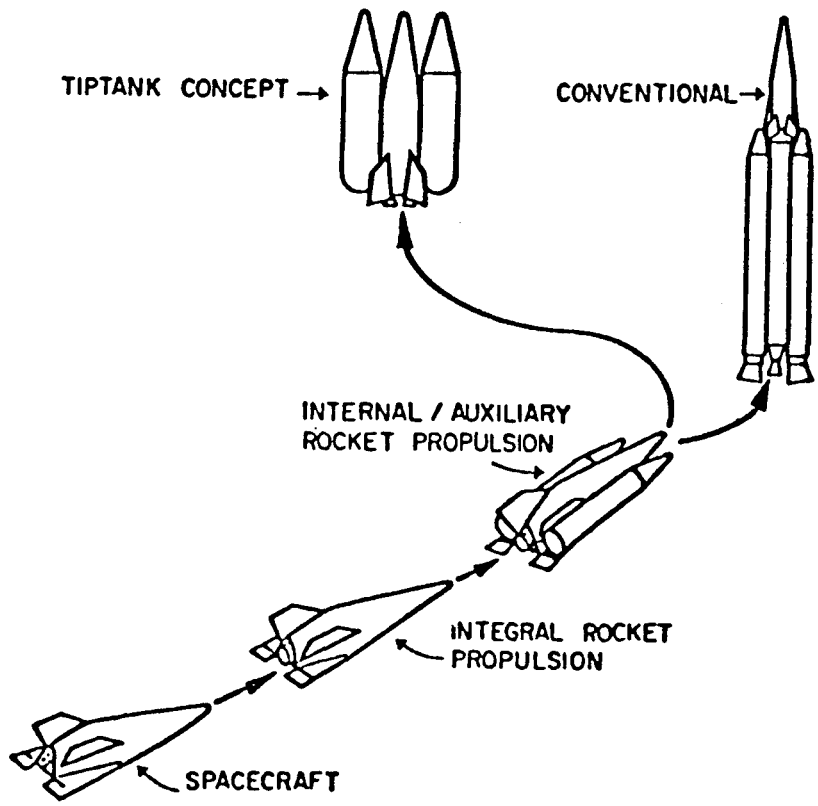
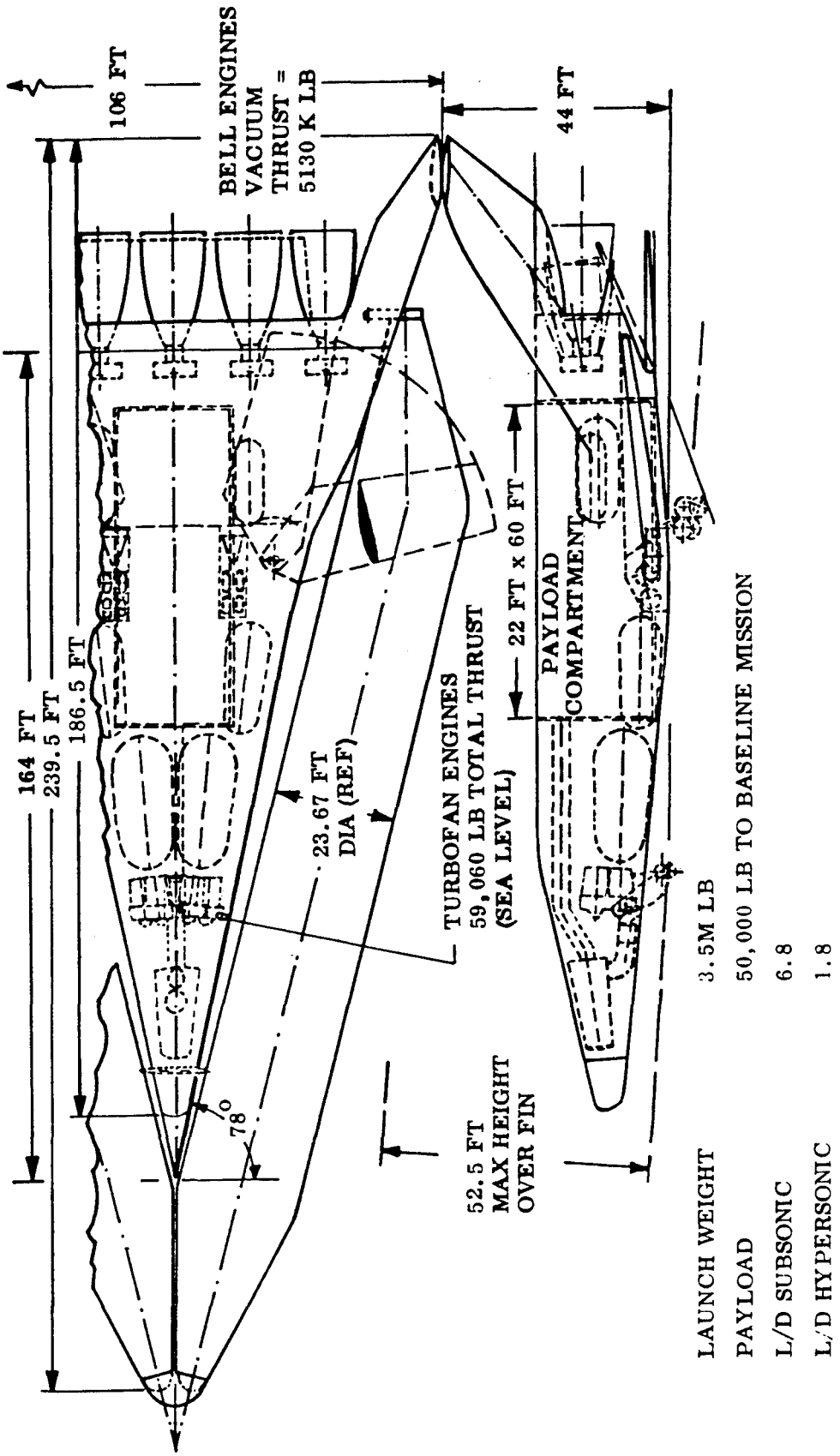


Fig 3b: FDL EVOLUTION OF STAGE AND A HALF CONCEPT

Figure 4

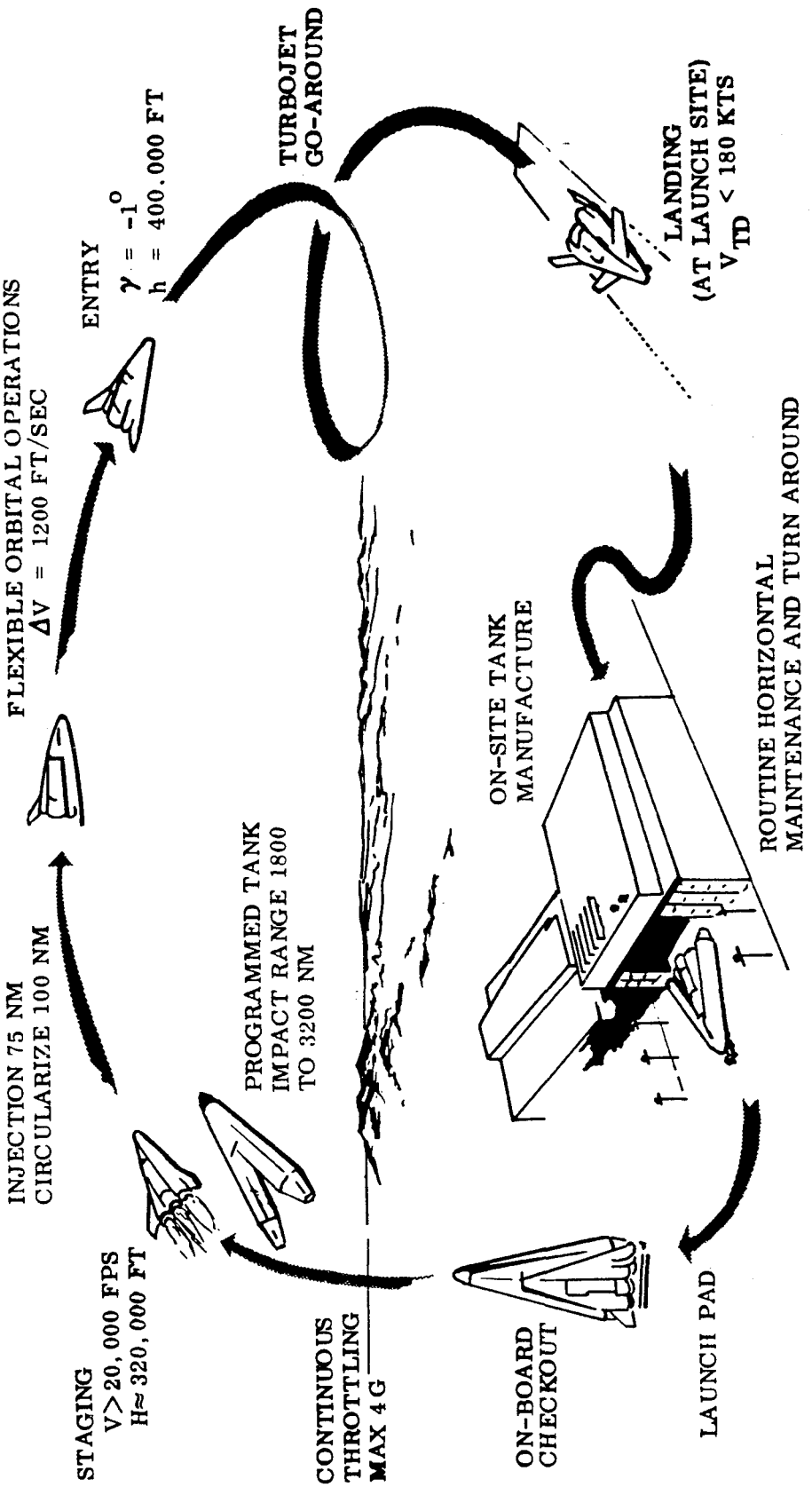


LOCKHEED SPACE TRANSPORTATION SYSTEM STUDY, 1969

Figure 5

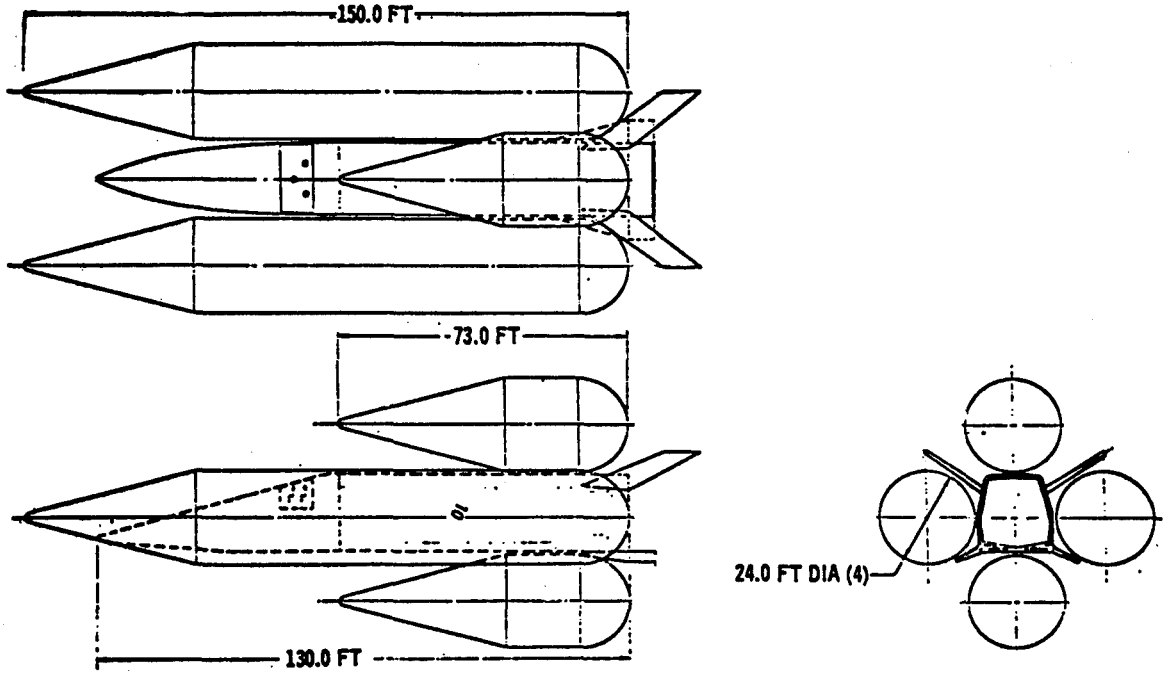
	BASELINE	ALTERNATE BASELINE	LOGISTICS SUPPORT	SATELLITE PLACEMENT AND SERVICE	RECOVERY		HIGH-ENERGY MISSIONS		SHORT-DURATION ORBITAL	GROUND BASED RESCUE
				10	11	12	13	20	21	0
ALTITUDE	100	100	270	100 SUN SYNCH	300 SUN SYNCH	800	100	100	300	100	270
INCLINATION	90	90	55	1,000 WTR	1,000 WTR	90	55	28-1/2	28-1/2	-65	-55
ON ORBIT ΔV	1,200 WTR	1,200 WTR	2,000 ETR	4	4	1,000 ETR	1,000 ETR	800 ETR	500 ETR	500 ETR	2,000 ETR
LAUNCH SITE	4	3	3	32,902	33,843	4	4	4	4	4	4
LAUNCH G						35,818	31,862	31,168	31,773	32,748	33,654
TOTAL ΔV	32,706	32,778	32,626								
PAYLOAD	50,000	48,500	51,000	41,000	18,000	0	74,000	97,000	77,500	48,000	23,000

Figure 6

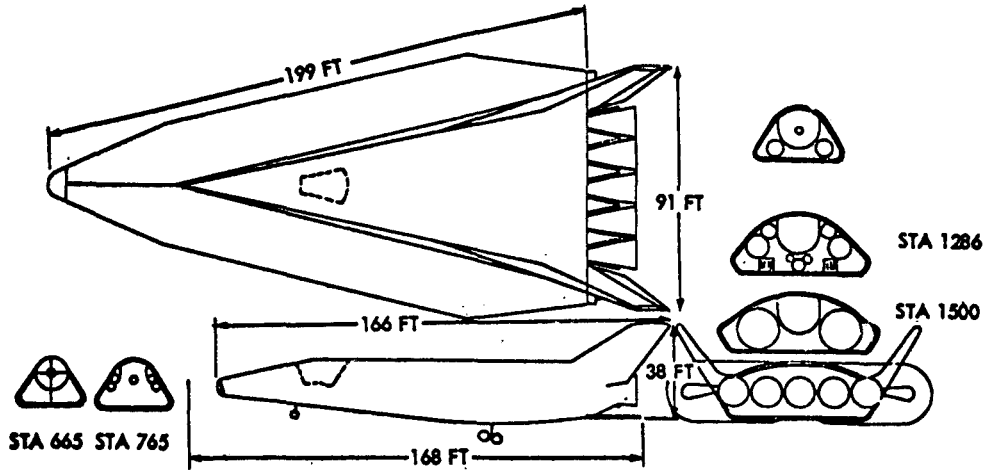


LOCKHEED SPACE TRANSPORTATION SYSTEM MISSION PROFILE

mission. Noteworthy in this concept are the number of similarities between the capabilities sought of this craft and those of the ultimate Shuttle, namely reduced operating and developmental costs via a partial throwaway configuration, the large diameter payload bay, and the high payload weight capabilities of this design. One of the great difficulties of single or two-stage fully reusable configurations such as the abortive Aerospaceplane had been extremely high research, development, test, and evaluation costs inherent in their procurement and acquisition. Because of this, maneuvering orbital reentry vehicles had always tended to be small; thus gargantuan concepts such as Aerospaceplane surprised nearly no one - particularly R&D pragmatists - when they fell by the wayside. In their 1968 paper, Draper and Cosenza argued convincingly that such partially expendable concepts as stage-and-a-half would make larger orbiters attractive, since they could now carry their propellants externally, freeing up internal volume for payload, and thus not requiring development of vehicles large enough to carry their propulsion, fuel, and payload all internally within a fully reusable structure. These arguments would assume importance in the early 1970s as NASA's Shuttle evolved from schemes for grandiose two-stage fully reusable vehicles with a flyback booster system to a more modest, practical and affordable semi-expendable system. After submittal to the Air Force, Lockheed submitted a similar stage-and-a-half concept for NASA evaluation (the Lockheed Star Clipper)--again demonstrating how industry tailored "generic" configurations to meet the oftentimes disparate needs of several agencies. Though its basic configuration was not adopted (in part because of NASA reluctance to acquire a system so totally dependent upon external tank-orbiter aerodynamic integration), capabilities sought by this craft subsequently appeared on the Space Shuttle, and NASA generally endorsed the technical feasibility of the stage-and-a-half concept.[3] Figure 7 shows another stage-and-a-half concept using parallel tankage rather



McDonnell-Douglas Parallel Tankage Stage-and-a-Half Approach



Lockheed Star Clipper Vee Tankage Stage-and-a-Half Approach

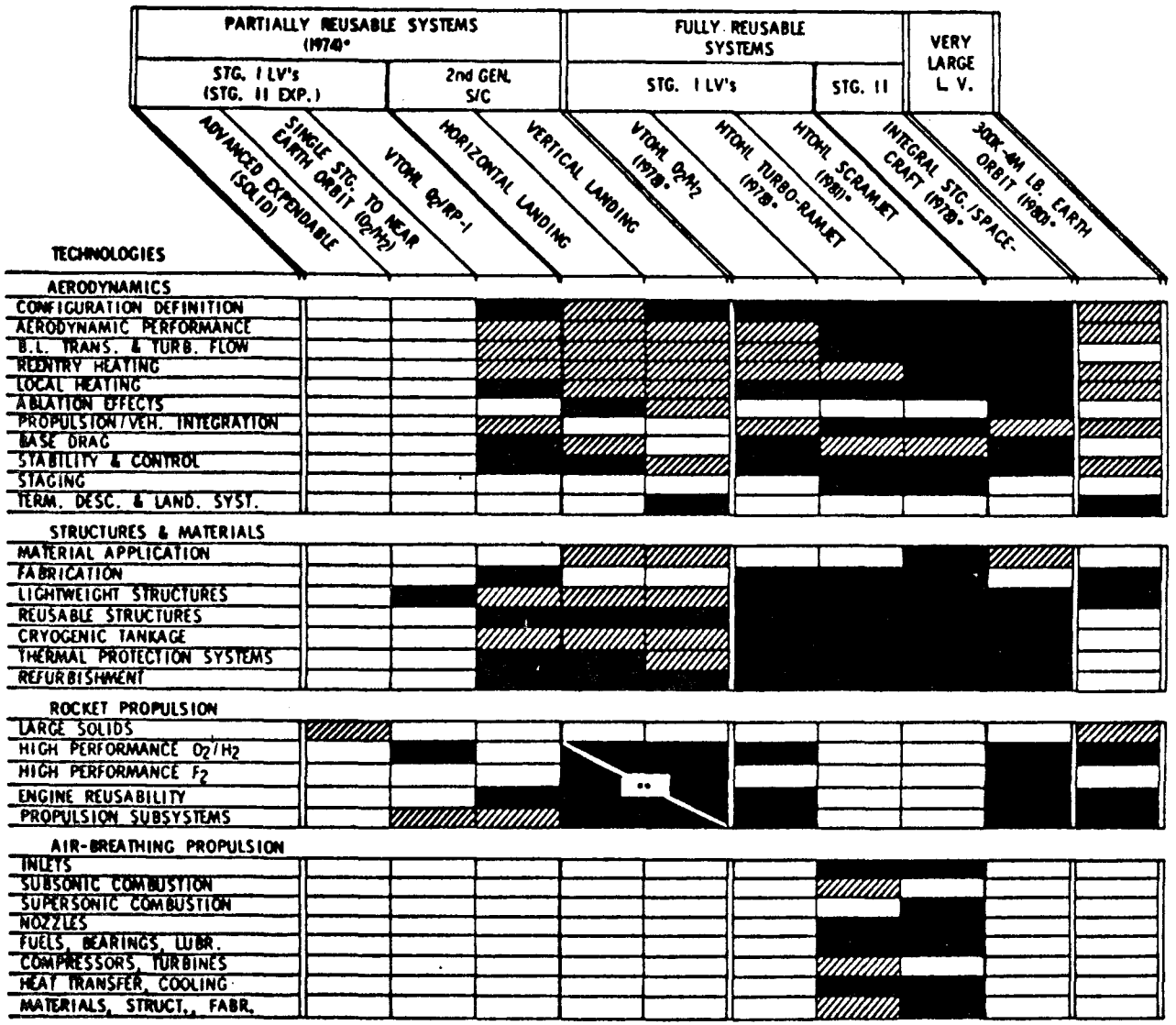
STAGE-AND-A-HALF CONCEPTS STUDIED BY NASA'S SSTG, 1969

than a "Vee" approach, derived by McDonnell-Douglas and studied by NASA's Space Shuttle Task Group. The similarities to the Draper-Cosenza paper are obvious. (The Star Clipper is shown with it for comparison.)

The AACB Report: A DoD-NASA Joint View

As has been mentioned previously, Aerospaceplane's termination and the subsequent redirection of Air Force hypersonic vehicle studies led to examination of hypersonic launch platforms. Within Flight Dynamics Laboratory, this effort spawned the RLV and RSLV (for Reusable Launch Vehicle and Reusable Space Launch Vehicle) studies. Obvious continuing Air Force interest in lifting reentry systems operating in space, as well as the growing interest within the NASA community for such systems as a as yet vague and undefined successor program to Apollo led to the joint Department of Defense--National Aeronautics and Space Administration Aeronautics and Astronautics Coordinating Board (AACB) to establish an Ad Hoc Subpanel on Reusable Launch Vehicle Technology, consisting of NASA and DoD joint chairmen, eight DoD representatives, and ten NASA ones. The committee, established on August 24, 1965, finished its work slightly over a year later, issuing a seminal report in mid-September 1966. During that time panel members intensively examined candidate reusable launch vehicles making use of hypersonic air-breathing and rocket-powered stages. The subpanel concluded that numerous cost uncertainties and technical risks required resolution, but that other factors, notably an expected increase in manned earth orbital activity, encouraged development. At the time, the AACB's panelists could not identify one single concept capable of satisfying both the future needs of NASA and the needs of DoD, and thus the subpanel summarized a variety of proposed systems, including ones utilizing horizontal and vertical takeoff, single vs. multi-stage configurations, air-launching, and craft blending air-breathing and rocket propulsion. Figure 8 is reprinted from the AACB report

STATUS OF LIMITING TECHNOLOGIES FOR CONCEPTS OF POTENTIAL INTEREST



LEGEND:

- TECHNOLOGY ADEQUATE OR NOT APPLICABLE
- TECHNOLOGY MARGINAL
- TECHNOLOGY INADEQUATE

* I.O.C. DATES AS GIVEN IN FIGURE

** FOR INTEGRAL PROPULSION; FOR PROPULSION MODULE IS MARGINAL.

Figure 8

and offers, from the mid-1966 perspective, an interpretation of the status of limiting technologies affecting the development of space transportation vehicles. Interestingly, AACB panelists considered a number of key technical areas marginal or inadequate, including major portions of the aerodynamics, structures, and propulsion fields. Despite these great uncertainties, the AACB panel nevertheless expressed optimism towards the eventual evolution of lifting reentry launch vehicle technology. Figure 9, adopted from the report, shows possible evolutionary progression for three classes of vehicles the AACB panel defined. The panel expected rudimentary lifting spacecraft could perform initial missions as early as 1974, with progressively more advanced vehicles coming on line in 1978 (Class II) and 1981 (Class III), the latter having advanced air-breathing propulsion such as scramjets! In light of subsequent history, this optimistic scenario exudes a kind of charming naivete; the actual Shuttle itself did not fly until 1981. This part of the AACB report is an excellent example of the problems--afflicting even experts within narrowly focused fields--inherent in futures forecasting and technological prediction. Figure 10, also adopted from the report, is a much more general glimpse at the trends in earth orbital transportation systems; it is difficult to fault (in part because of its general non-technologically specific nature) and, in fact, seems as valid for the present as it was in the mid-1960s.[4]

In general, the panel recommended partially reusable launch vehicles, believing they would cost less to develop than fully reusable systems, and that in any case their costs could amortize in a shorter period of time with fewer flights, thus making such craft competitive with throwaway systems in the 1975 time period. While the latter point--competitiveness with throwaway systems--ultimately proved a chimera (at least in pure economic terms) for the Shuttle as finally built, the overall argument made a great deal of sense. Nevertheless, fascination with fully reusable

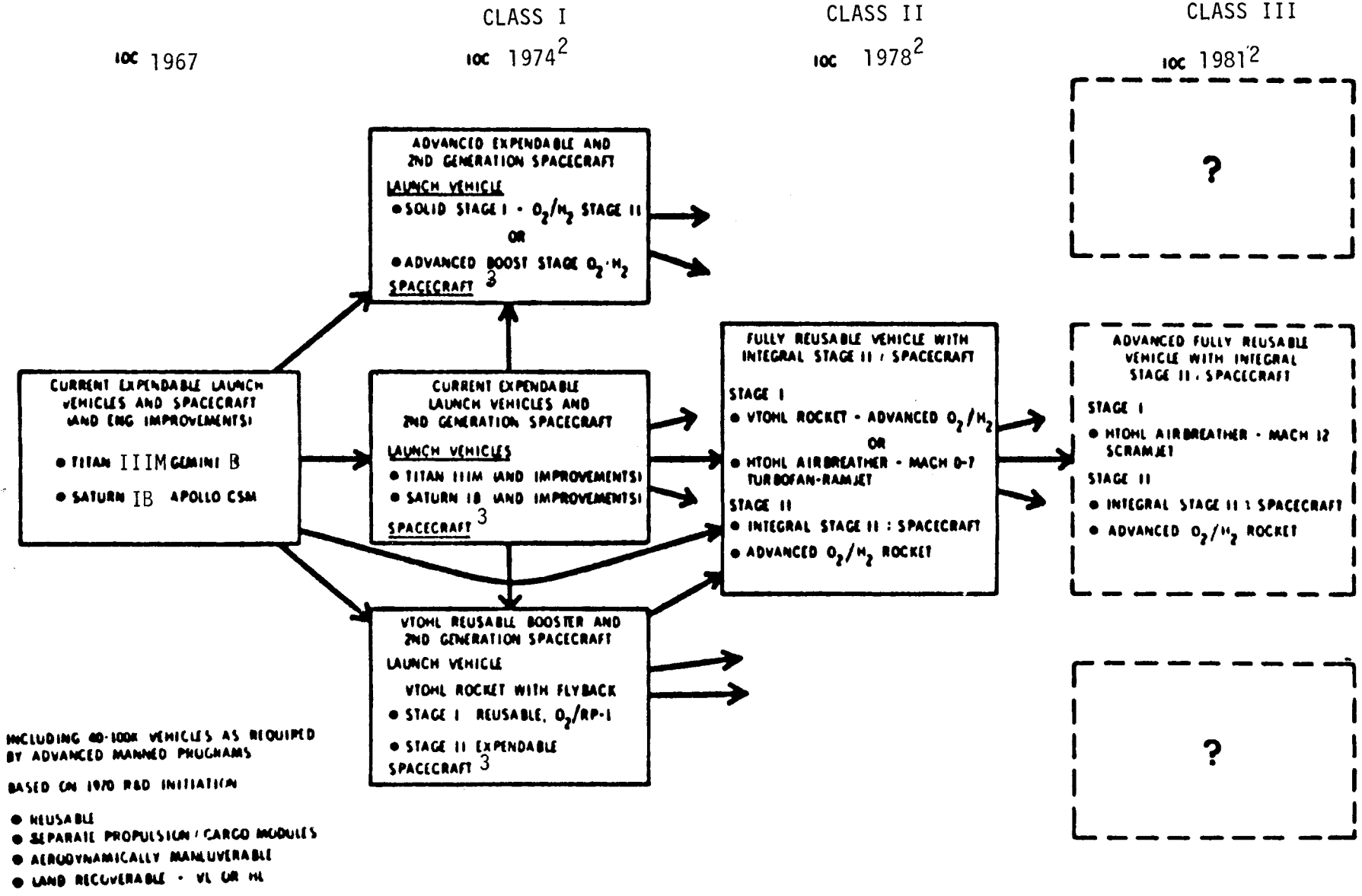
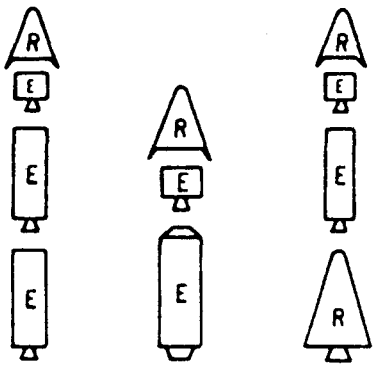


Figure 9

POSSIBLE EVOLUTIONARY PROGRESSION PATTERN (1966 PERSPECTIVE)

TIME PERIOD	CURRENT	MID-TERM	FUTURE
GENERALIZED VEHICLE CONCEPTS			
OPERATIONAL OBJECTIVES	• EXPERIMENTAL SYSTEMS DEVELOPMENTS • EXPLORATORY SPACE PROGRAMS	• INITIAL SPACE SYSTEMS LOGISTIC SUPPORT • IMPROVED OPERATIONAL FLEXIBILITY • GROWTH IN SPACE FUNCTIONAL CAPABILITY	• INCREASED LOGISTICS TRAFFIC • INCREASED SPACE SYSTEMS CAPABILITY • REDUCED OPERATIONAL AND SUPPORT SYSTEMS COST • IMPROVED OPERATIONAL CHARACTERISTICS - NOMINAL AND CONTINGENCY
DESIRED VEHICLE CHARACTERISTICS	• EXPENDABLE LAUNCH AND SPACECRAFT VEHICLES • WATER RECOVERY OF SPACECRAFT	• FLEXIBLE MAN-RATED LAUNCH VEHICLE CAPABILITIES (ADVANCED EXPENDABLE OR REUSABLE VTO STAGE I) • REUSABLE ENTRY SPACECRAFT (SECOND GENERATION) • ORBITAL MANEUVERING PROPULSION • NOMINAL LAND-RECOVERY OF SPACECRAFT	• FULLY-REUSABLE, TWO-STAGE VEHICLE SYSTEMS

E - DENOTES EXPENDABLE UNIT
R - DENOTES REUSABLE UNIT

TRENDS IN TRANSPORTATION FROM EARTH TO ORBIT AND RETURN

—MANNED PROGRAM SUPPORT—

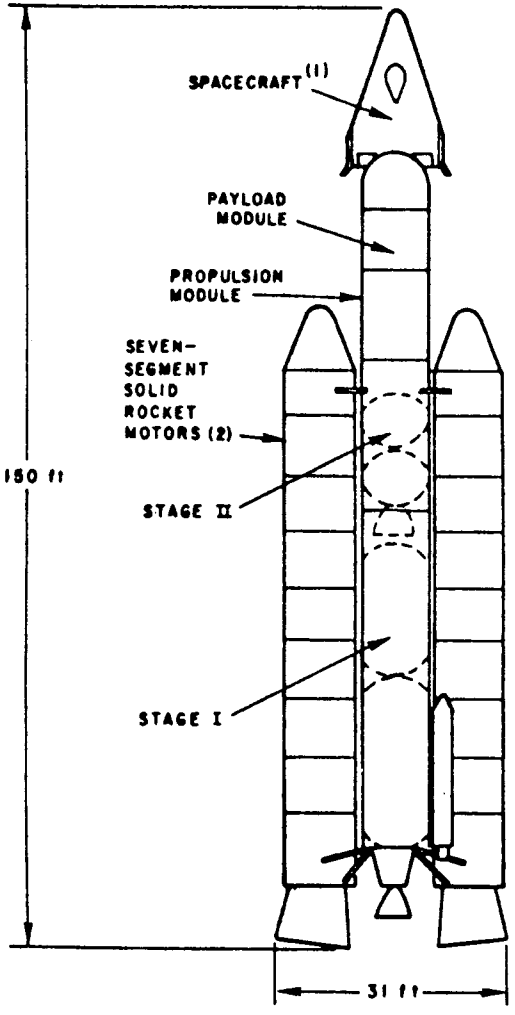
(1966 PERSPECTIVE)

Figure 10

systems would dominate Shuttle thought for another half-decade until the post-Apollo 11 malaise forced a hasty reordering of priorities within NASA in the budget-conscious days of the early 1970s. Panel members envisioned three potential vehicle classes: Class I (ready for service in 1974-1975), Class II (ready in the 1978 time period), and Class III (ready in the post-1980 time period). Naturally, the classes differed in technological sophistication, with the anticipated maturation required determining the expected operational dates. Interestingly, (allowing for the "20-20 hindsight" factor) is that it took until 1981 to achieve the development of a logistical spacecraft--the Rockwell Space Shuttle--having characteristics running somewhat in between Classes I and II. The capabilities of a Class III vehicle (anticipated in 1966 as being ready in the 1981 time period) are not yet available at present, and are the stuff of conceptual efforts such as the British HOTOL and the American NASP; again, so much for technological prediction. Figures 11-19, adopted from the AACB report, show anticipated Class I-III configurations. Generally speaking, these followed the progression highlighted in Figure 10: i.e., adapting existing launch vehicles such as Titan and Saturn for reusable upper stage lifting reentry vehicles, progressing to higher performance throwaway lower stages with these new upper stage concepts, developing vertical or horizontal takeoff TSTO configurations using rocket propulsion, developing horizontal takeoff and landing (HTOL) TSTO configurations using air-breathing propulsion based on turboramjets, and finally, HTOL TSTO configurations having a mix of turbofan/ramjet/scramjet propulsion. In each of these cases, the actual upper stage flying into space and returning to earth was a lifting body configuration, and not itself a winged vehicle.

Shuttle Concepts: The View From Europe

It is interesting for comparative purposes to digress briefly and consider what the European nations thought of the Shuttle



OPERATING MODES
- VERTICAL TAKEOFF
- EXPENDABLE LAUNCH VEHICLE STAGES AND ORBITAL MODULES
- REUSABLE SPACECRAFT WITH
  - 4-6 MAN CAPACITY
  - HORIZONTAL LANDING
  - LAND RECOVERY

PROPULSION
- STAGE 0
  - SEVEN-SEGMENT 120-in SOLID ROCKET MOTORS
  - LIFT-OFF THRUST 3,000,000 lb
  - I_s (SL) = 245 sec
- STAGE I
  - LR87-AJ-9 ENGINES N_2O_4/MIXED HYDRAZINES
  - VACUUM THRUST 520,000 lb
  - I_s (VAC) = 280 sec
- STAGE II
  - LR91-AJ-9 ENGINES N_2O_4/MIXED HYDRAZINES
  - VACUUM THRUST 102,000 lb
  - I_s (VAC) = 293 sec
- PROPULSION MODULE
  - (2) RL-10-(A-3) ROCKET ENGINES - OXYGEN/HYDROGEN
  - THRUST (VAC) = 30,000 lb
  - I_s (VAC) = 445 sec
  - MANEUVERING VELOCITY = 2500 ft/sec

WEIGHTS

• LIFTOFF	1,820,000 lb
• STAGE 0	
GROSS	1,400,000 lb
INERT	197,000 lb
• STAGE I	
GROSS	307,000 lb
INERT	16,000 lb
• STAGE II	
GROSS	77,000 lb
INERT	7,600 lb
• PROPULSION & PAYLOAD MODULES	
GROSS	20,500 lb
INERT	1,450 lb
PAYLOAD	13,000 lb
• SPACECRAFT	
GROSS	15,500 lb
PAYLOAD (DISCRETIONARY)	2,000 lb

COSTS[2] (INCL SPACECRAFT & PROP MOD)

DEVELOPMENT	$ 0.7 x 10^9
RECURRING	$ 1.9 x 10^6

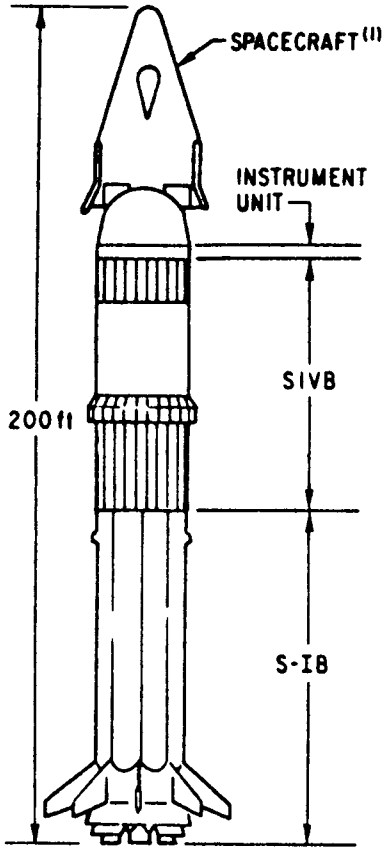
(1) HORIZONTAL-LANDING LIFTING BODY IS ILLUSTRATED; NEAR-BALLISTIC SPACECRAFT WITH LAND-LANDING CAPABILITY IS ALTERNATE CONCEPT

(2) ORDER OF MAGNITUDE ESTIMATES; SEE TEXT FOR CLARIFICATION

— CONCEPTS OF POTENTIAL INTEREST —

CLASS I: TITAN III M WITH SECOND-GENERATION SPACECRAFT

Figure 11



OPERATING MODES

- VTO, 2 STAGES
- STAGE I - EXPENDABLE S-IB
- STAGE II - EXPENDABLE S-IVB
- SPACECRAFT - REUSABLE

PROPULSION

- STAGE I - (8) H-1 ROCKET ENGINES $O_2/RP\text{-}1$
  - THRUST (SL) = 1,640,000 lb
  - I_s (SL) = 263 sec
- STAGE II - (1) J-2 ROCKET ENGINE O_2/H_2
  - I_s (VAC) - 427 sec
- SPACECRAFT - STORABLE PROPELLANTS

WEIGHTS

• LIFT-OFF	1,296,000 lb
• STAGE I	
GROSS	1,000,000 lb
INERT	86,000 lb
• STAGE II	
GROSS	256,000 lb
INERT	26,400 lb
• SPACECRAFT (INCL. MANEUVERING PROPELLANTS AND DISCRETIONARY PAYLOAD)	40,000 lb

COSTS(2) (INCL. SPACECRAFT)

• DEVELOPMENT	$ 0.7 x 10^9
• RECURRING	$ 36 x 10^6

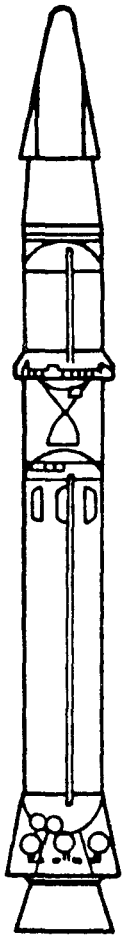
(1) HORIZONTAL-LANDING LIFTING BODY IS ILLUSTRATED; NEAR-BALLISTIC SPACECRAFT WITH LAND-LANDING CAPABILITY IS ALTERNATE CONCEPT

(2) ORDER OF MAGNITUDE ESTIMATES; SEE TEXT FOR CLARIFICATION

— CONCEPT OF POTENTIAL INTEREST —

CLASS I: SATURN IB WITH SECOND-GENERATION SPACECRAFT

Figure 12



(1) HORIZONTAL-LANDING LIFTING BODY IS ILLUSTRATED; NEAR-BALLISTIC SPACECRAFT WITH LAND-LANDING CAPABILITY IS ALTERNATE CONCEPT

(2) ORDER OF MAGNITUDE ESTIMATES; SEE TEXT FOR CLARIFICATION

OPERATING MODES

- VTO, TWO STAGES
- STAGE I – EXPENDABLE
- STAGE II – EXPENDABLE
- SPACECRAFT – REUSABLE, LAND RECOVERY

PROPULSION

- STAGE I – 260 in. DIA SOLID PROPELLANT MOTOR
  - THRUST (SL) = 5,500,000 lb
- STAGE II – MODIFIED S IVB
  - (1) J-2 ROCKET ENGINE – O_2/H_2
  - THRUST (VAC) = 200,000 lb
  - I_s (VAC) = 427 sec
- SPACECRAFT – STORABLE PROPELLANTS
  - I_s (VAC) = 315 sec

WEIGHTS

• LIFT-OFF	4,141,000 lb
• STAGE I	
GROSS	3,780,000 lb
INERT	311,000 lb
• STAGE II (INCL. INSTRUMENT UNIT)	
GROSS	267,000 lb
INERT	28,000 lb
• SPACECRAFT (INCL. MANEUVERING PROPELLANTS AND DISCRETIONARY PAYLOAD)	95,000 lb

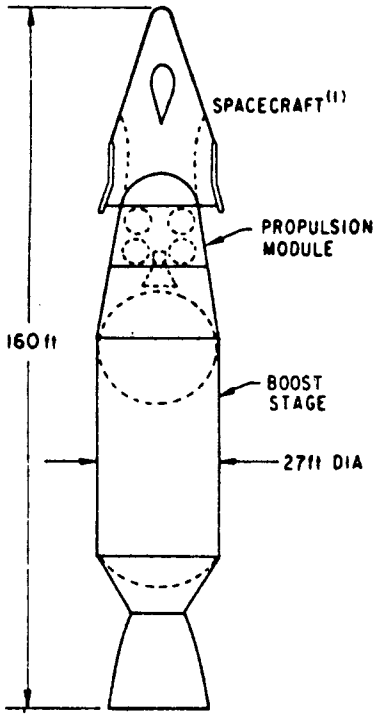
COSTS (2) (INCL. SPACECRAFT SYSTEM)

• DEVELOPMENT	$ 1.1 x 10^9
• RECURRING	$ 36 x 10^6

Figure 13

— CONCEPT OF POTENTIAL INTEREST —

CLASS I: ADVANCED EXPENDABLE LAUNCH VEHICLE WITH SECOND-GENERATION SPACECRAFT



VEHICLE	
• LIFT OFF WEIGHT	858,000 lb
• SEA LEVEL THRUST	1,250,000 lb
BOOST STAGE	
• GROSS WEIGHT	773,000 lb
• PROPELLANTS	O_2 / H_2
• ENGINE TYPE	HIGH P_c MULTIPLE CHAMBERS 2:1 THROTTLING
• NOZZLE TYPE	ALTITUDE COMPENSATING
• SPECIFIC IMPULSE	
SEA LEVEL	357 sec
VACUUM	448 sec
• STAGING VELOCITY	21,000 fps
PROPULSION MODULE	
• GROSS WEIGHT	50,000 lb
• PROPELLANTS	STORABLE
• ΔV ON-ORBIT	2,500 fps
• ΔV LAUNCH	5,000 fps
SPACECRAFT	
• GROSS WEIGHT	35,000 lb
• DISCRETIONARY PAYLOAD	15,000 lb
COSTS (2) (INCL SPACECRAFT & PROP MOD)	
• DEVELOPMENT	$ 2 x 10^9
• RECURRING	$ 14 x 10^6

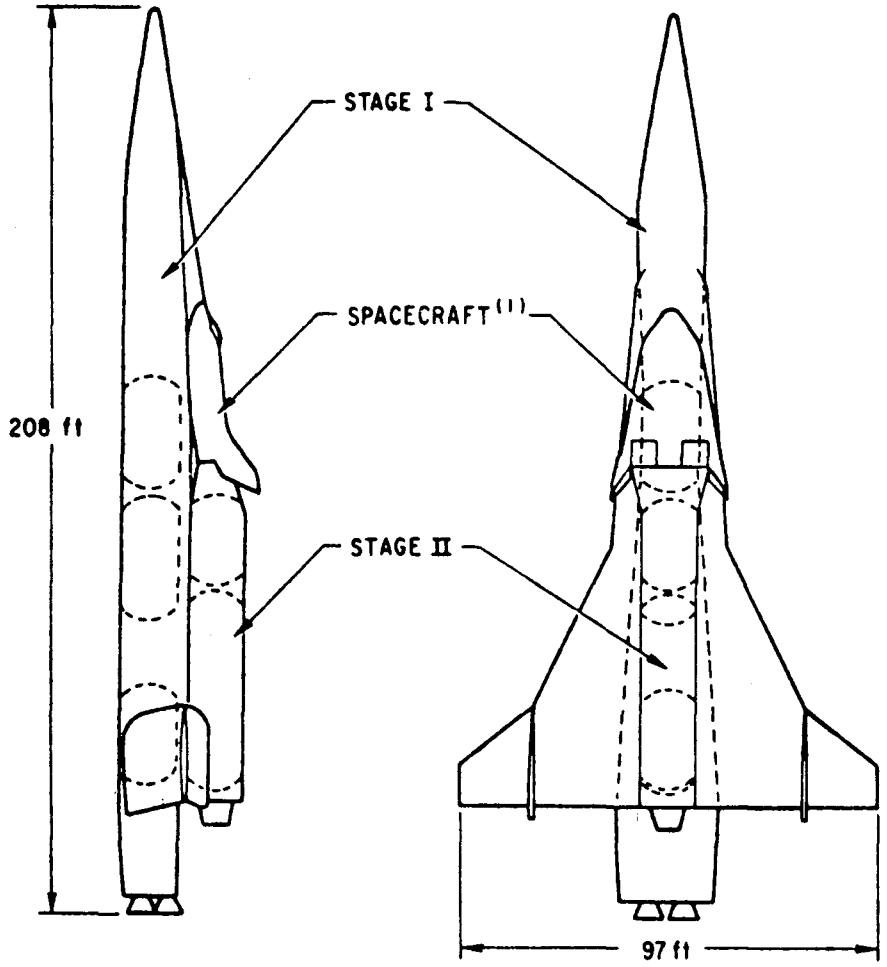
(1) HORIZONTAL-LANDING LIFTING BODY IS ILLUSTRATED; NEAR-BALLISTIC SPACECRAFT WITH LAND-LANDING CAPABILITY IS ALTERNATE CONCEPT

(2) ORDER OF MAGNITUDE ESTIMATES; SEE TEXT FOR CLARIFICATION

— CONCEPT OF POTENTIAL INTEREST —

CLASS I: ADVANCED EXPENDABLE LAUNCH VEHICLE WITH SECOND-GENERATION SPACECRAFT

Figure 14



OPERATING MODES

- STAGE I – VTOHL REUSABLE
  - POWERED FLYBACK
  - STAGING VELOCITY – 5500 fps
- STAGE II – EXPENDABLE
- SPACECRAFT – REUSABLE, LAND RECOVERY

PROPULSION

- STAGE I – F-1/H-1 ROCKET ENGINES – $O_2/RP\text{-}1$
  - THRUST (SL) = 1,700,000 lb
  - I_s (SL) = 268 sec
- STAGE II – (1) J-2 ROCKET ENGINE – O_2/H_2
  - I_s (VAC) = 430 sec
- SPACECRAFT – STORABLE PROPELLANTS
  - I_s (VAC) = 315 sec

WEIGHTS

• LIFT-OFF	1,200,000 lb
• STAGE I	
GROSS	800,000 lb
INERT	195,000 lb
• STAGE II	
GROSS	370,000 lb
INERT	41,000 lb
• SPACECRAFT	
GROSS (INCL. PAYLOAD)	25,000 lb

COSTS(2) (INCL. SPACECRAFT SYSTEM)

• DEVELOPMENT	$ 2.5 x 10^9
• RECURRING	$ 15 x 10^6

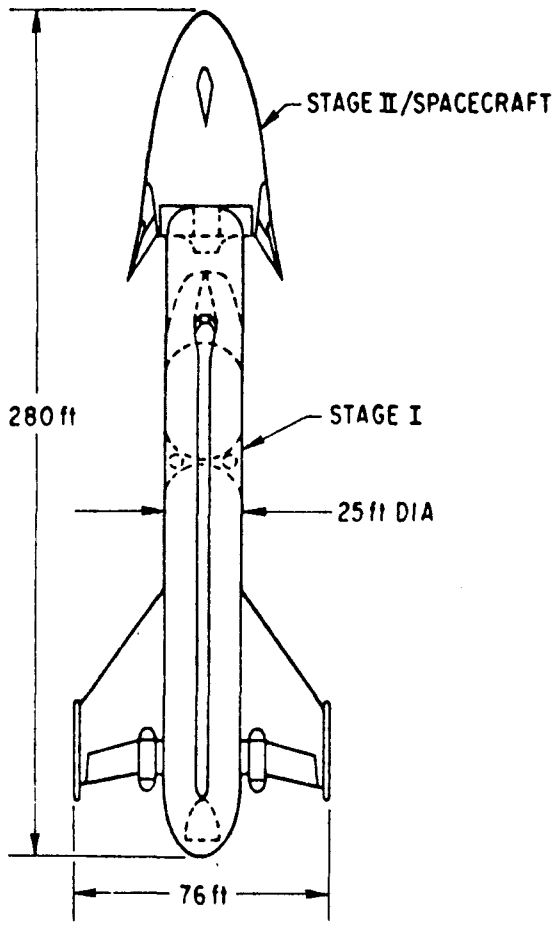
(1) HORIZONTAL-LANDING LIFTING BODY IS ILLUSTRATED; NEAR-BALLISTIC SPACECRAFT WITH LAND-LANDING CAPABILITY IS ALTERNATE CONCEPT

(2) ORDER OF MAGNITUDE ESTIMATES; SEE TEXT FOR CLARIFICATION

— CONCEPT OF POTENTIAL INTEREST —

CLASS I: VTOHL REUSABLE STAGE I WITH SECOND-GENERATION SPACECRAFT (PARALLEL STAGING)

Figure 15



OPERATING MODES
- VTOHL, TWO STAGES
- STAGE I REUSABLE
- STAGE II REUSABLE
- STAGING VELOCITY: 11,000 fps

PROPULSION
- STAGE I
  - ROCKET - LOX/LH_2 ; I_s (SL) = 374 sec
  - THRUST (SL) - 2,400,000 lb
  - TURBOFANS (4) RETURN CRUISE AND LANDING
- STAGE II
  - ROCKET - LOX/LH_2 ; I_s (VAC) = 450 sec
  - THRUST (VAC) - 275,000 lb

WEIGHTS

• LIFT-OFF	1,643,500 lb
• ON ORBIT (100 n mi)	65,000 lb
• PAYLOAD (DISCRETIONARY)	15,000 lb
• STAGE I	
GROSS	1,390,000 lb
INERT	250,000 lb
• STAGE II/SPACECRAFT	
GROSS	253,500 lb
INERT	50,000 lb

COSTS*

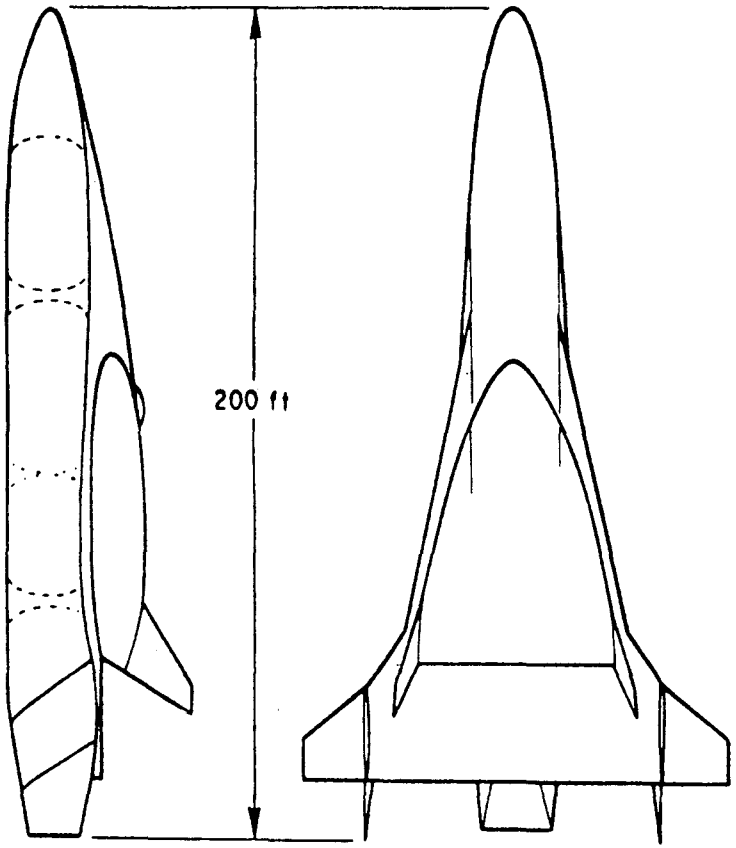
• DEVELOPMENT	$ 3×10^9
• RECURRING	$ 4×10^6

* ORDER OF MAGNITUDE ESTIMATES; SEE TEXT FOR CLARIFICATION

— CONCEPT OF POTENTIAL INTEREST —

CLASS II : VTOHL FULLY-REUSABLE ROCKET WITH INTEGRAL STAGE II/SPACECRAFT

Figure 16



OPERATING MODES

- HTOHL, TWO STAGES
- STAGE I - REUSABLE; POWERED FLYBACK
- STAGE II - REUSABLE INTEGRAL PROPULSIVE STAGE AND SPACECRAFT - POWER-OFF FLYBACK
- STAGING VELOCITY - 8000 fps

PROPULSION

- STAGE I - ADVANCED CRYOGENIC ROCKETS - H_2/O_2 I_s (VAC) = 455 sec
- STAGE II ADVANCED CRYOGENIC ROCKETS - H_2/O_2 I_s (VAC) = 455 sec
- LIFT-OFF THRUST - 2,000,000 lb

WEIGHTS

- LIFT-OFF - 1,320,000 lb
- STAGE I

GROSS	1,025,000 lb
INERT	310,000 lb

- STAGE II (LESS PAYLOAD)

GROSS	275,000 lb
INERT	61,000 lb

- PAYLOAD (DISCRETIONARY AND MANEUVERING PROPELLANTS) 20,000 lb

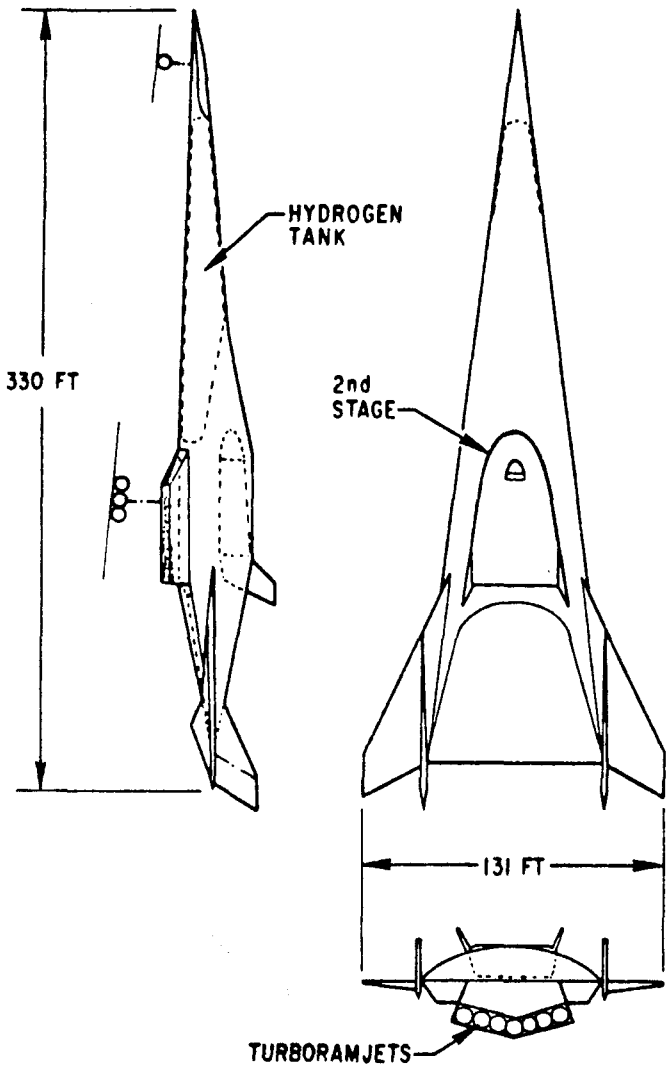
*COSTS**

DEVELOPMENT	$\$ 4 \times 10^9$
RECURRING	$\$ 4 \times 10^6$

*ORDER OF MAGNITUDE ESTIMATES; SEE TEXT FOR CLARIFICATION

CLASS II: VTOHL FULLY-REUSABLE ROCKET WITH INTEGRAL STAGE II/SPACECRAFT (PARALLEL STAGING)

Figure 17



OPERATING MODES

- HTOHL, TWO STAGES
- STAGE I - REUSABLE; POWERED FLYBACK
  - STAGING VELOCITY: MACH 7
- STAGE II - REUSABLE INTEGRAL PROPULSIVE STAGE AND SPACECRAFT - POWER-OFF FLYBACK

PROPULSION

- STAGE I - HYDROGEN-FUELED TURBOFAN-RAMJET (SUBSONIC COMBUSTION)
  - I_s = 5000 TO 1500 sec
- STAGE II - ADVANCED CRYOGENIC ROCKETS - H_2/O_2; I_s (VAC) = 455 sec
- TAKE-OFF THRUST - 530,000 lb

WEIGHTS

• LIFT-OFF	1,000,000 lb
• STAGE I	
GROSS	675,000 lb
INERT	400,000 lb
• STAGE II (WITHOUT PAYLOAD)	
GROSS	290,000 lb
INERT	63,000 lb
• PAYLOAD (DISCRETIONARY AND MANEUVERING PROPELLANTS)	35,000 lb

COSTS*

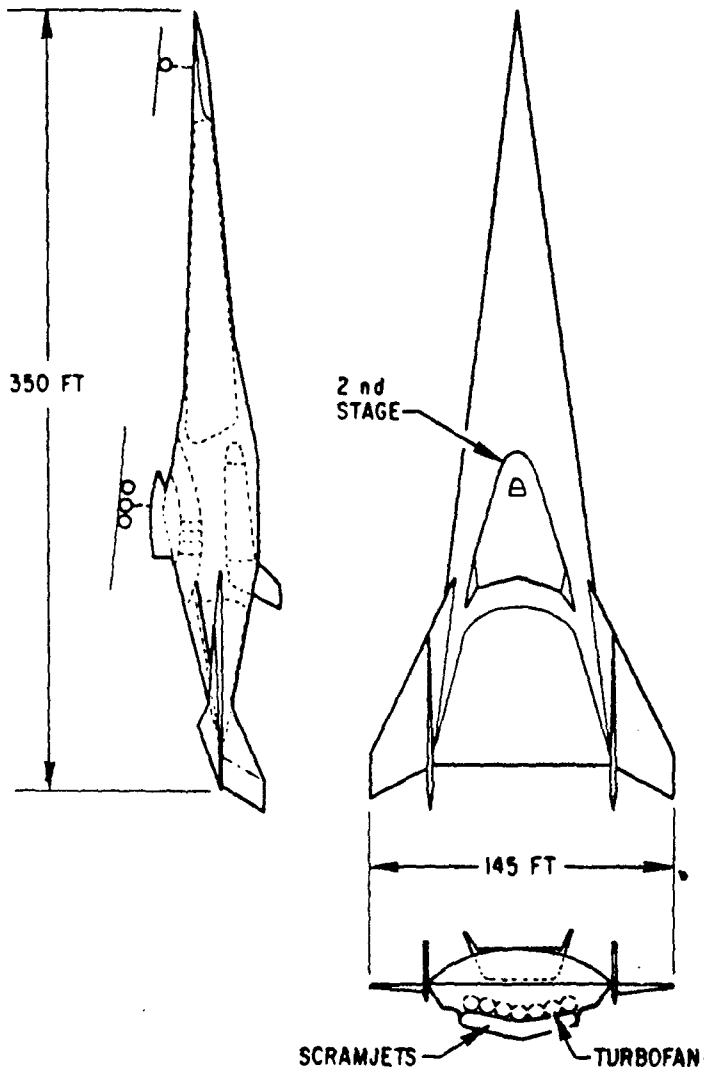
• DEVELOPMENT	$ 7 x 10^9
• RECURRING	$ 4 x 10^6

*ORDER OF MAGNITUDE ESTIMATES; SEE TEXT FOR CLARIFICATION

—CONCEPT OF POTENTIAL INTEREST—

CLASS II: FULLY REUSABLE HTOHL AIRBREATHER (SUBSONIC COMBUSTION)

Figure 18



OPERATING MODES

- HTOHL, TWO STAGES
- STAGE I - REUSABLE, POWERED FLYBACK
  - STAGING VELOCITY - 11,000 fps
- STAGE II - REUSABLE. INTEGRAL STAGE AND SPACECRAFT - POWER-OFF FLYBACK

PROPULSION

- STAGE I - HYDROGEN-FUELED
  - MACH 0-6 - TURBOFAN-RAMJETS
  - MACH 6-12 - SCRAMJET
  - TAKE-OFF THRUST - 395,000 lb
  - I_s = 5000 TO 1500 sec
- STAGE II - ADVANCED CRYOGENIC ROCKETS - H_2/O_2 ; I_s (VAC) = 455 sec

WEIGHTS

• LIFT-OFF	750,000 lb
• PAYLOAD (DISCRETIONARY AND MANEUVERING PROPELLANTS)	40,000 lb
• STAGE I	
GROSS	520,000 lb
INERT	350,000 lb
• STAGE II (WITHOUT PAYLOAD)	
GROSS	190,000 lb
INERT	42,000 lb

— CONCEPT OF POTENTIAL INTEREST —

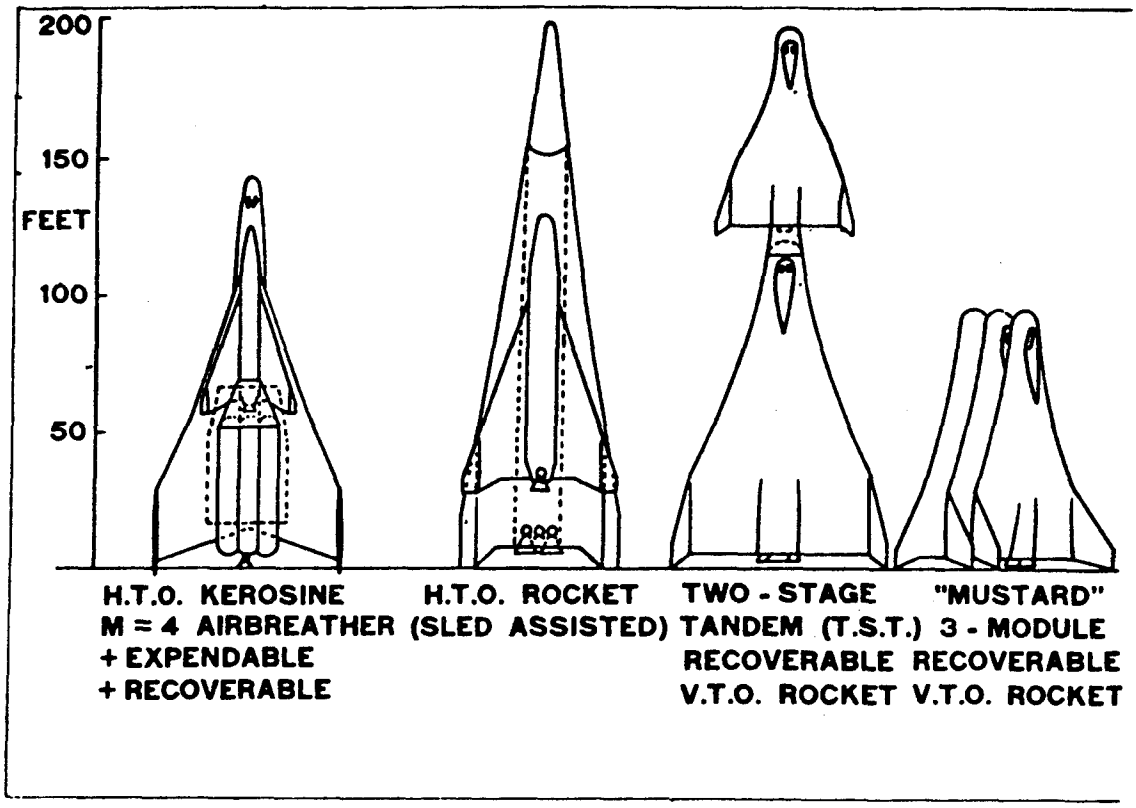
CLASS III: ADVANCED FULLY REUSABLE HTOHL AIRBREATHER (SUPERSONIC COMBUSTION)

Figure 19

concept, and to examine the configurations that they espoused. On the whole, Shuttle-related studies in the United States were mirrored by studies made abroad by European enthusiasts. Further, to a remarkable degree, these configurations resembled those of their American counterparts. There appears to have been little formal technical interchange from America to Europe and thus it must be concluded that the similarities in design stem from a traditional characteristic of technological development: communities of engineers familiar with a subject field tend to pursue similar solutions when confronting similar challenges. Interestingly, these foreign advocates pursued such concepts even though the economic situation of their nations generally mitigated against any possibility of their ideas being translated into hardware. Eventually Great Britain, France, and Germany all developed orbiter configurations. As early as 1958, British advocates had studied a rounded delta orbiter similar to the contemporary Dyna-Soar effort in the United States. In the 1960s, their conceptions turned bolder and more futuristic. Britain's Royal Aircraft Establishment (RAE) studied two-stage horizontal takeoff transporters. At the same time, the British Aircraft Corporation (now British Aerospace) investigated a variety of horizontal and vertical takeoff conceptualizations (see Figure 20 for examples) culminating in the three-module MUSTARD scheme (Figure 21). MUSTARD (for Multi-Unit Space Transport And Recovery Device) consisted of three lifting bodies somewhat resembling the NASA HL-10 shape launched either as a symmetrical cluster or as a stack, the latter mirroring an American General Dynamics concept called Triamese (to be covered subsequently). Hawker-Siddeley Aviation Limited suggested a TSTO transporter consisting of a Nonweiler (caret) winged first-stage wave rider surmounted by a lifting body orbiter (Figure 22).[5]

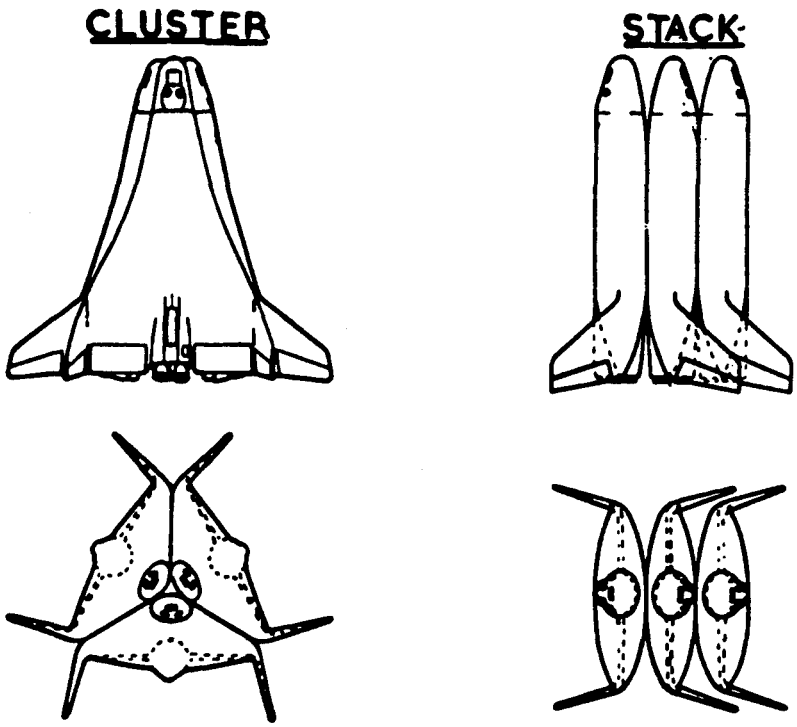
In France, the Dassault, Nord, Sud, SNECMA, ERNO, and ONERA firms and agencies worked singly and as teams on various transporter and "space taxi" proposals. Evaluated by the

Figure 20



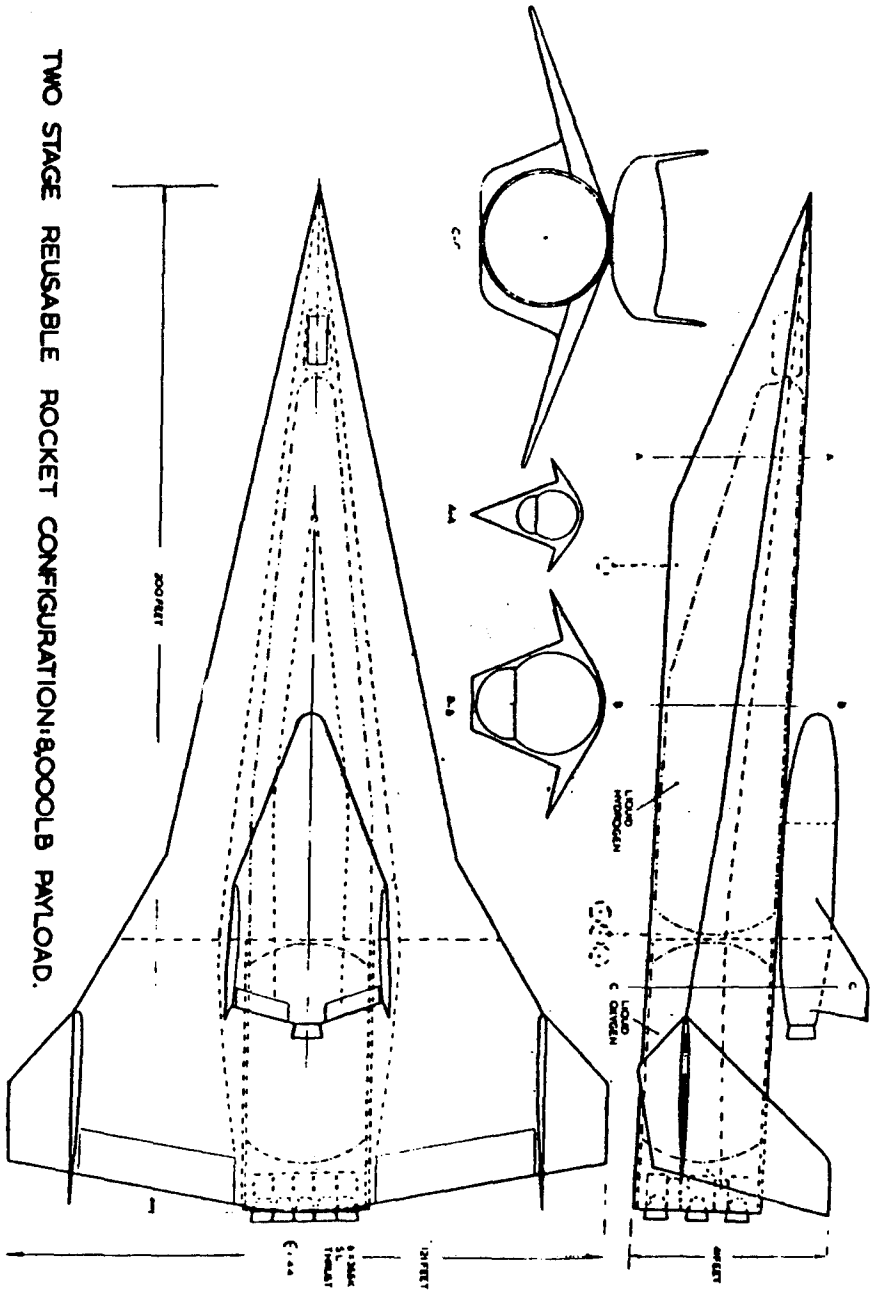
ALTERNATIVE HORIZONTAL TAKEOFF (HTO) AND VERTICAL TAKEOFF (VTO) CONCEPTS FOR A SHUTTLE EXAMINED BY THE BRITISH AIRCRAFT COMPANY IN THE MID TO LATE 1960's. MUSTARD, FAR RIGHT, EVENTUALLY EMERGED AS THE FAVORED CONCEPT BY 1971.

Figure 21



MUSTARD ALTERNATIVE LAUNCH SCHEMES

Figure 22



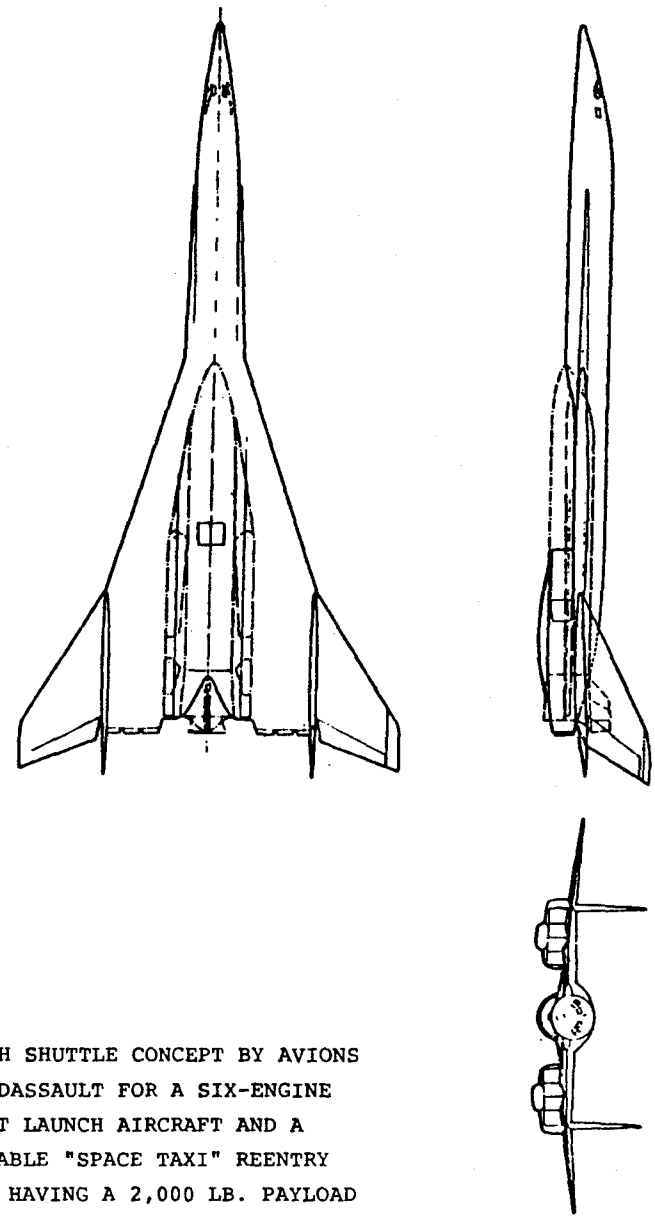
TWO-STAGE FULLY REUSABLE SHUTTLE POSTULATED BY HAWKER-SIDDELEY AVIATION LIMITED OF GREAT BRITAIN, 1971.

Note the unusual wave-rider (caret wing) configuration

Centre National d'Etudes Spatiales (CNES), French studies fell into two broad categories: large-payload hypersonic launch vehicles serving as platforms for manned and unmanned upper stages, and small research or space taxi concepts. One of these, by Dassault, is shown in Figure 23. Dassault's firm began such studies in 1963, eventually developing a series of HTO TSTO configurations making use of a large delta-wing Mach 4 launcher powered by turboramjets. This elegant design (somewhat like Concorde, but with twin fins and more angular lines) would then launch a small space taxi designed by CNES (see Figure 24), which would boost into space, conduct some sort of task, and then return through the atmosphere. In the lower atmosphere it would deploy variable-sweep wings and tail surfaces, relying on turbofan propulsion to cruise to its landing site. Nord Aviation proposed the so-called Mistral project (Figure 25) for a jet-propelled launch vehicle carrying a large lifting body; this eventually became a major cooperative effort between France and Germany involving Nord, SNECMA, and Entwicklungsring Nord (ERNO). Sud Aviation pursued similar ideas, but generally favored much more sharply swept delta planforms for the launch aircraft (see Figures 26 and 27). ERNO supported all of these efforts by running model drop tests of proposed orbiters from Luftwaffe Transall C-160 transport aircraft over the Baltic and Mediterranean. Additionally, Nord proposed development of a Mach 10 hypersonic technology demonstrator called VERAS (Figure 28) for research into aerothermodynamics and structures, though, in fact, it was never built.[6]

In Germany, Messerschmitt-Bölkow-Blohm (MBB) and Junkers examined various space transporter concepts. German postwar work had begun in 1962 as a result of the active lobbying efforts of Eugen Sänger; he died soon thereafter, in 1964. Between 1962 and 1969 the Federal Republic of Germany expended approximately DM 16.5 million on Shuttle-related studies to (in the words of the West German Minister for Education and Science) "prepare German

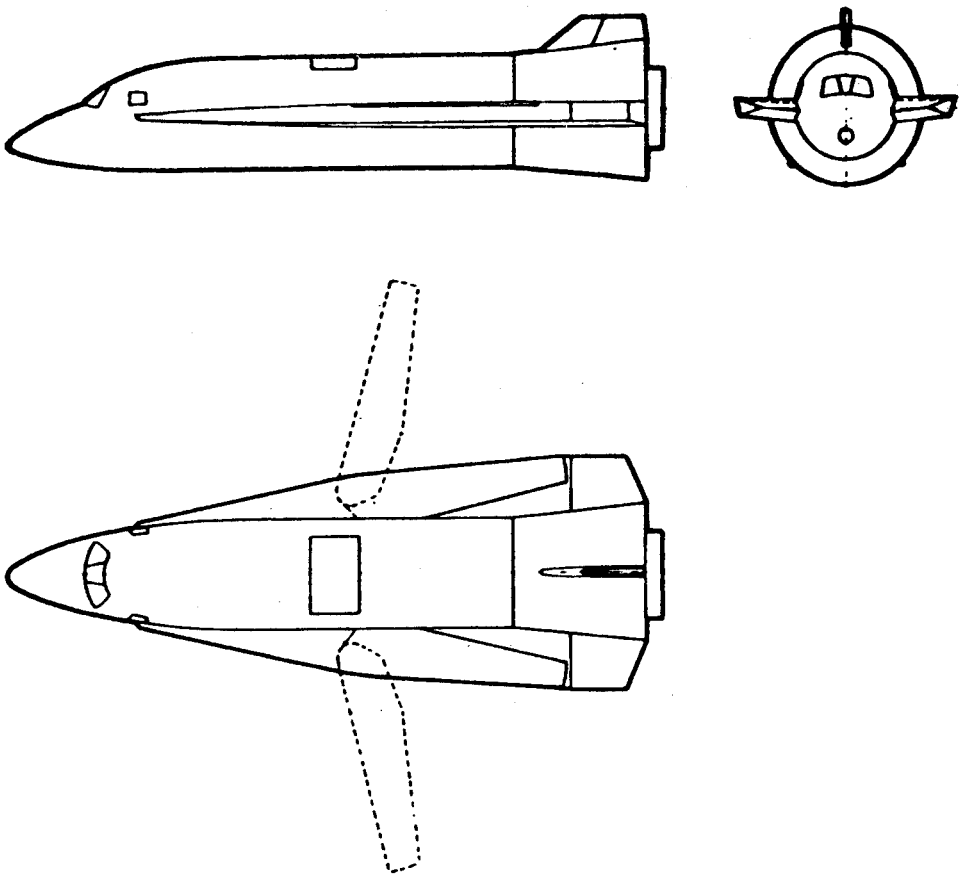
Figure 23



500,000 LBS. TOW - FULLY RECOVERABLE

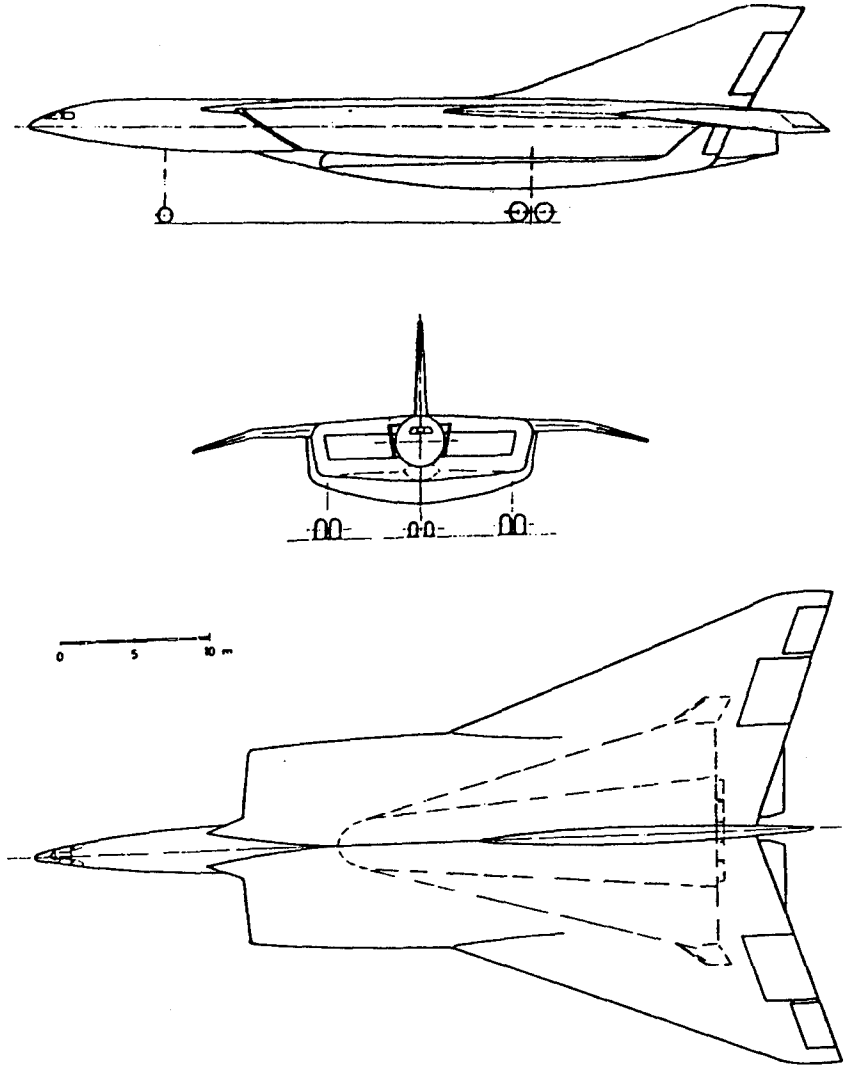
A FRENCH SHUTTLE CONCEPT BY AVIONS MARCEL DASSAULT FOR A SIX-ENGINE TURBOJET LAUNCH AIRCRAFT AND A RECOVERABLE "SPACE TAXI" REENTRY VEHICLE HAVING A 2,000 LB. PAYLOAD CAPABILITY.

Figure 24



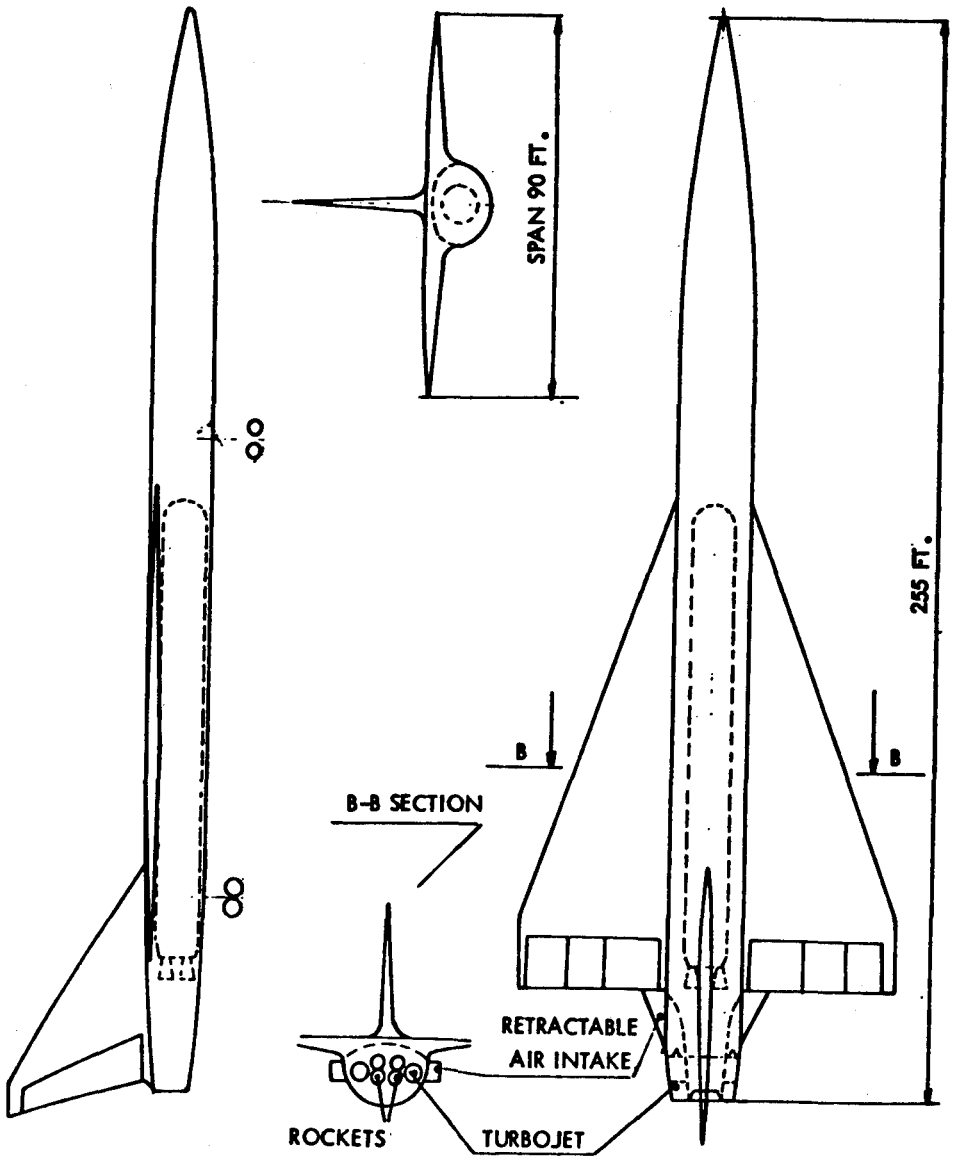
FRENCH "SPACE TAXI" PROPOSAL BY THE CENTRE NATIONAL D'ETUDES SPATIALES. THIS CRAFT, CAPABLE OF CARRYING UP TO THREE OR FOUR TONS INTO EARTH ORBIT, WOULD BE LAUNCHED FROM A HORIZONTAL TAKEOFF-TYPE WINGED AIRCRAFT POWERED BY TURBO-RAMJET PROPULSION. AFTER COMPLETING A LIFTING REENTRY, IT WOULD DEPLOY A VARIABLE-GEOMETRY WING FOR ITS FINAL GLIDE TO EARTH.

Figure 25



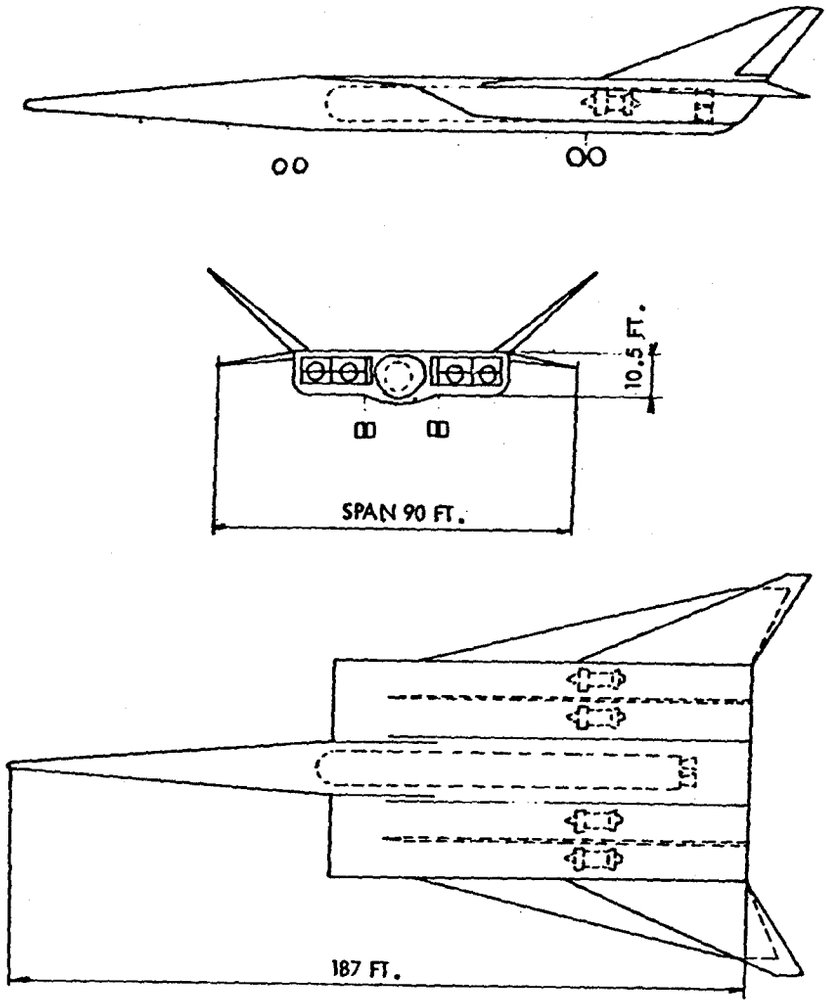
JOINT FRANCO-GERMAN COOPERATIVE SHUTTLE DESIGN STUDY BY NORD, SNECMA, AND ENTWICKLUNGSRING NORD (ERNO) FOR AN "AEROSPACE TRANSPORTER" KNOWN AS THE MISTRAL, CONSISTING OF A WINGED JET-PROPELLED LAUNCH AIRCRAFT AND A LIFTING BODY ORBITER NESTLED BENEATH IT. LATER VERSIONS OF THE MISTRAL WERE LESS ELEGANT IN APPEARANCE, AND CONFIGURED TO LAUNCH CONVENTIONAL UNMANNED UPPER STAGES.

Figure 26



ROCKET AEROSPACE LAUNCHER
PLT/SUD-AVIATION CONCEPT

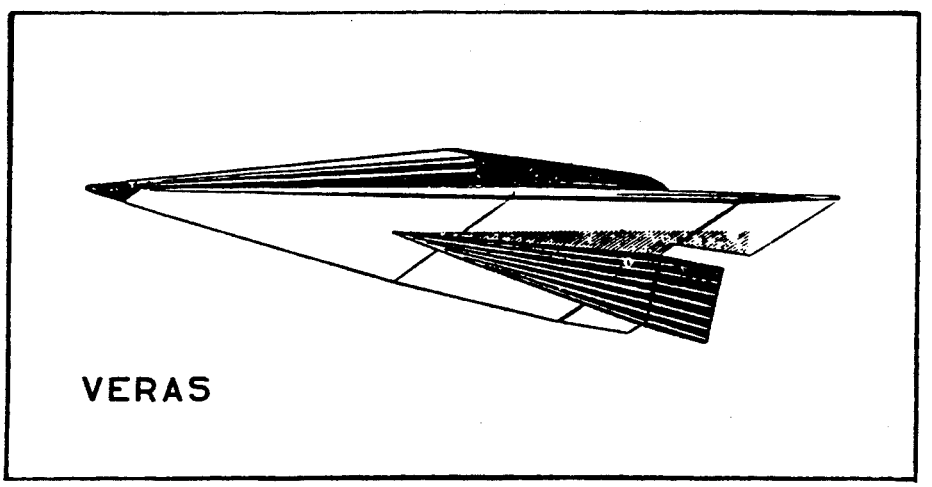
Figure 27



TURBOFAN-RAMJET AEROSPACE LAUNCHER
PLT/SUD-AVIATION CONCEPT

JOINT STUDY BETWEEN THE FRENCH MINISTRY OF DEFENSE AND THE SUD-AVIATION FIRM FOR A HYPERSONIC LAUNCH AIRCRAFT CAPABLE OF ACTING AS A LAUNCH PLATFORM FOR INTERNALLY HOUSED EXPENDABLE BOOSTERS. NOTE THAT THIS IS NOT A TRUE SHUTTLE CONCEPT.

Figure 28

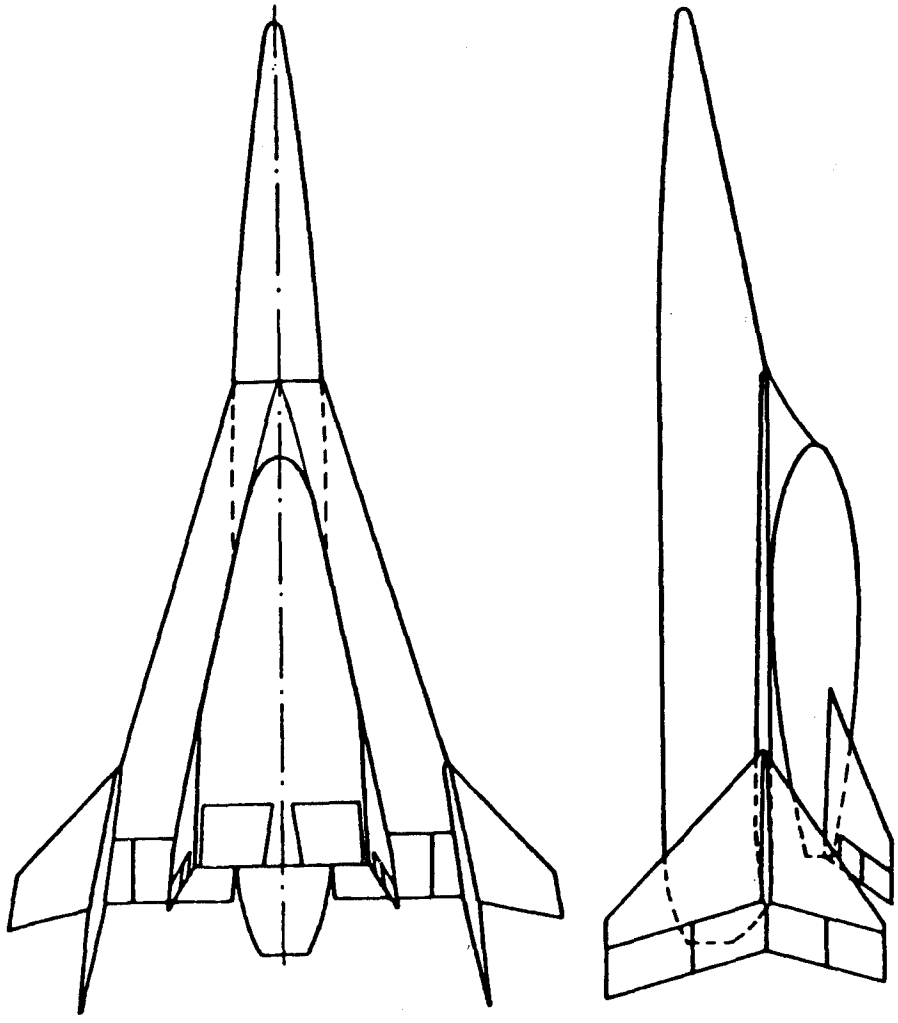


FRENCH HYPERSONIC RESEARCH AIRCRAFT PROPOSAL. THIS PROJECT, KNOWN AS VERAS (FOR VEHICLE FOR RESEARCH IN AEROTHERMODYNAMICS AND STRUCTURES) CONSISTED OF A HIGHLY SWEPT ARROW WING HAVING A COMBINED CYLINDRO-CONICAL FUSELAGE AND, UNCHARACTERISTIC OF SUCH DESIGNS, A VENTRAL FIN. IT WOULD HAVE A MAXIMUM WEIGHT OF 3,000 LBS., AND BE CAPABLE OF A 500 SECOND DURATION MACH 10 CRUISE AT 150,000 FEET. THE VERAS CONCEPT PROVED A USEFUL TECHNOLOGICAL EXERCISE, MUCH AS THE AMERICAN X-20A PROGRAM HAD.

industry for future possible bilateral or multilateral international cooperative projects and to build up active teams."[7] Like French advocates, German enthusiasts believed that there was a need to carry a mix of large and small payloads (ranging from 6,000 to 50,000 lbs.) into orbit. Any such craft, they felt, should be partially expendable to reduce costs and development uncertainties (thus echoing unconsciously the American AACB study), though they conceded that a partially expendable spacecraft could be regarded only as an interim solution before the "final space transport." MBB investigated many possible configurations, but finally selected a TSTO approach with a winged booster and an HL-10-like orbiter mounted piggyback (Figure 29). Junkers, working closely with Sänger, postulated a winged booster and winged orbiter for their RT-8-01 Raumtransporter (space transport) study completed in 1964; the RT-8-01's orbital craft closely resembled the canceled X-20A Dyna-Soar.[8]

What work went on in the Soviet Union is not yet known, at least in open literature. That the Soviets had interest in such concepts seems beyond doubt, going back to the story of the attempted kidnapping of Sänger-Bredt in the immediate postwar years (related by G. A. Tokaty, a defector who was present at a Kremlin meeting where no less an individual than Josef Stalin himself gave the order). Reports stated that as early as 1962 Artem Mikoyan, best known as the father of the MiG series of jet fighters, was directing studies of such craft. Speculative art by the well-known Soviet space artist Andrei Sokolov showed one configuration that may have represented a serious Soviet thought: a two-stage design consisting of a lifting body orbiter looking like a cross between the HL-10 and an FDL-5/8MX design fired from the back of a winged booster. Technical press reports in 1978 announced that a suitable configuration had been drop-tested from a modified Tupolev Tu-95 Bear mothership. In 1982 the Soviets launched the first of a series of subscale lifting reentry vehicles. The following year the Department of Defense announced

Figure 29



FEDERAL REPUBLIC OF GERMANY STUDY FOR A TWO-STAGE-TO-ORBIT FULLY REUSABLE SHUTTLE BY MESSERSCHMITT-BÖLKOW-BLOHM GmbH, LATE 1960's.

that the Soviets had embarked on a program to build a heavy lift Shuttle capable of placing a 60,000 kg. payload into a 180 km. orbit. If imitation is the sincerest form of flattery, the United States might well have been pleased, for it apparently used a generally similar aerodynamic configuration to that of the American Shuttle, as well as a similar parallel-burn boost technique.[9]

It is neither proper nor accurate to suggest that these European conceptions played a role in influencing the subsequent design of the American Space Shuttle, for they did not. But it is interesting from a technological standpoint to examine how nations not possessing a great deal of experience with "real-world" hypersonic configurations and vehicles approached the task of developing hypersonic space transporters. The broad range of European studies reflected the vigorous interest displayed by European enthusiasts of lifting reentry and Shuttle-related technology. In tacit recognition of this, by the end of 1970, working partnerships had developed between American and European firms engaged on Shuttle-related research. North American-Rockwell, McDonnell-Douglas, and Grumman all negotiated with diverse European aerospace concerns, forming organizational ties to such groups as the British Aircraft Corporation, Messerschmitt-Bölkow-Blohm, ERNO, Aerospatiale, Dornier, Dassault, and Hawker-Siddeley, emphasizing the multinational interest in Shuttle development. Much of this European work was discussed at a major Shuttle development symposium held at the Museum of Natural History of the Smithsonian Institution on October 16-19, 1969: American researchers and their European counterparts discussed the merits of many different configurations and many different technological approaches; it followed by three years an earlier similar meeting between American, British, and German Shuttle advocates on the West Coast. NASA followed some of this development work, particularly the British MUSTARD concept which resulted in the agency taking a close look at a more streamlined

(and unrelated) American concept by General Dynamics called Triamese. Overall, however, desires by the Europeans to involve themselves in the American Shuttle program "fell by the wayside" in the words of LeRoy Day, former NASA Deputy Director of the Space Shuttle program. Britain wanted in on construction of the orbiter and its avionics, and Germany desired to make contributions to the Shuttle's propulsion system, but after consideration, NASA decided that the management was "too complicated and it would really make it too tough for us to do." Europe's portion of the Shuttle program came instead with the Spacelab orbital workshop. That Europe was sincerely interested in developing its own or other people's Shuttles is glaringly evident. Even before the Space Shuttle flew in 1981, national interest in France had crystallized in the Aerospatiale Hermes mini-Shuttle designed for launch by an Ariane booster, and (to minimize unknowns) making use of the Shuttle's basic aerodynamic configuration. Studies since that time have resulted in more elaborate British and German projects, notably the NASP-like (but unpiloted) British Aerospace-Rolls-Royce HOTOL, and the MBB-ERNO Sänger II proposal.[10]

NASA and the Space Shuttle: From Phase A to Redirection

NASA employed many individuals who had studied the Space Shuttle-like craft as early as the 1950s, in the days of the NACA. Engineers at the Manned Spacecraft Center (now the Lyndon B. Johnson Space Center) had followed closely work done in industry and by other governmental organizations such as the Air Force, and during the early 1960s had themselves, in conjunction with the Marshall Space Flight Center at Huntsville, Alabama, sponsored a series of Shuttle-like studies from private industry, from Lockheed, Boeing, McDonnell Douglas, Martin, Convair, and North American-Rockwell. In June 1964, NASA's Ad Hoc Committee on Hypersonic Lifting Vehicles had endorsed development of two-stage-to-orbit shuttlecraft; the agency had participated in the AACB

studies previously discussed; thus, overall, NASA was predisposed towards the Shuttle concept.[11]

NASA had spent little time thinking about space beyond the Apollo program, in part because the frantic pace and single-mindedness of purpose required to meet President Kennedy's challenge to place a team of astronauts on the moon before the end of the decade of the 1960s allowed NASA scant time to think of anything else. What attention NASA did focus on the future tended to coalesce in vague plans for an orbiting space station, and (possibly) a manned exploratory voyage to Mars. Obviously, any orbiting space station would require some sort of logistical support craft; it was in this context that NASA increasingly thought of the Shuttle as the 1960s advanced. Thus, within NASA, Shuttle was born of two impulses: on one hand, the natural "higher-faster-farther" impulse springing from the hypersonic airplane to the genuine spacecraft, and the desire to travel between an orbiting space station and the earth on the other. In between fit Shuttle.

Unlike Apollo, there was no clear moment, no great national address by the President, that triggered the Shuttle program. When NASA first detailed engineers to study Shuttle with a view to developing an actual system (as opposed to an exercise in mere technological forecasting), it did so decisively but surprisingly as well. LeRoy E. Day, former NASA Deputy Director of the Shuttle program, recalls his first exposure to the concept came when NASA's Associate Administrator for Manned Space Flight, Dr. George Mueller, called him into a meeting with other engineers in early 1969, prior to the historic flight of Apollo 11. Day arrived somewhat late, and listened as Mueller reiterated all the tasks that NASA had to accomplish to develop a Shuttle. To Day, the meeting seemed to have nothing to do with his Apollo work, and he concluded he had arrived before Mueller was ready to discuss Apollo matters with him. Instead, Mueller started tasking Day to

support the Shuttle effort. Day averred that he could set to work "'in a couple of weeks, right after this [Apollo XI] flight readiness review.' 'No,' he said, 'you don't understand. You don't understand. I want you over here now to begin work on the Shuttle.' I said, 'What does now mean?' He said, 'Tomorrow morning.'" NASA established a Space Shuttle Task Group under the direction of Day to evaluate both the agency's needs and various system concepts. Day took charge of preparing a draft proposal for submission to the recently established President's Space Task Group.[12]

On October 30, 1968, the Manned Spacecraft Center (MSC) and the Marshall Space Flight Center (MSFC) issued a joint Request for Proposal (RFP) for an eight-month study of an "Integral Launch and Reentry Vehicle System" (ILRV). Interestingly, this was an apparent adaptation of a term originally coined by researchers at the Air Force's Flight Dynamics Laboratory, who had used the same expression for studies of stage-and-a-half concepts the previous year. Both MSC and MSFC emphasized that the RFP would not necessarily result in an actual development program, and based the ILRV studies on an anticipated requirement to place 5,000 to 50,000 lb. payloads in orbits between 100 and 200 n.m., with 260 n.m. being the baseline altitude. The RFP emphasized that studies should emphasize "economy and safety rather than optimized payload performance." The issuance of this RFP marked the beginning of so-called "Phase A" NASA Shuttle studies. Starting in 1965, NASA had established a four-phase project planning scheme, though it did not become a codified ritual until the issuance of detailed guidelines in August 1968; the ILRV effort constituted one of NASA's first projects formally undertaken under this "PPP" system (for "Phased Project Planning"), though, in fact, NASA had been following a phased planning scheme for most of its projects, to a greater or lesser extent, since creation of the agency in 1958. The four phases initially consisted of Phase A: advanced studies, Phase B: project definition, Phase C: design,

and Phase D: development/operations. Over time, this schema varied somewhat, with Phase A being termed preliminary analysis rather than advanced studies. The overall rationale behind the PPP idea was to foster competition throughout the development process, reducing the number of competitors as the phases advanced so that, at the Phase C (design) stage, competition would be restricted by NASA to those firms that could be expected to continue into the Phase D follow-on stage. It was under this system that NASA would undertake the future acquisition of the Space Shuttle.[13]

In February 1969, NASA awarded four study contracts to Lockheed, General Dynamics, McDonnell-Douglas, and North American-Rockwell for Phase A studies. Meanwhile, LeRoy Day's Space Shuttle Task Group had been evaluating potential Shuttle needs and missions, and five months after the award of the Phase A study contracts, in July 1969, the SSTG issued a summary report of its efforts, concluding that ILRV-class systems should be capable of performing six major space missions:

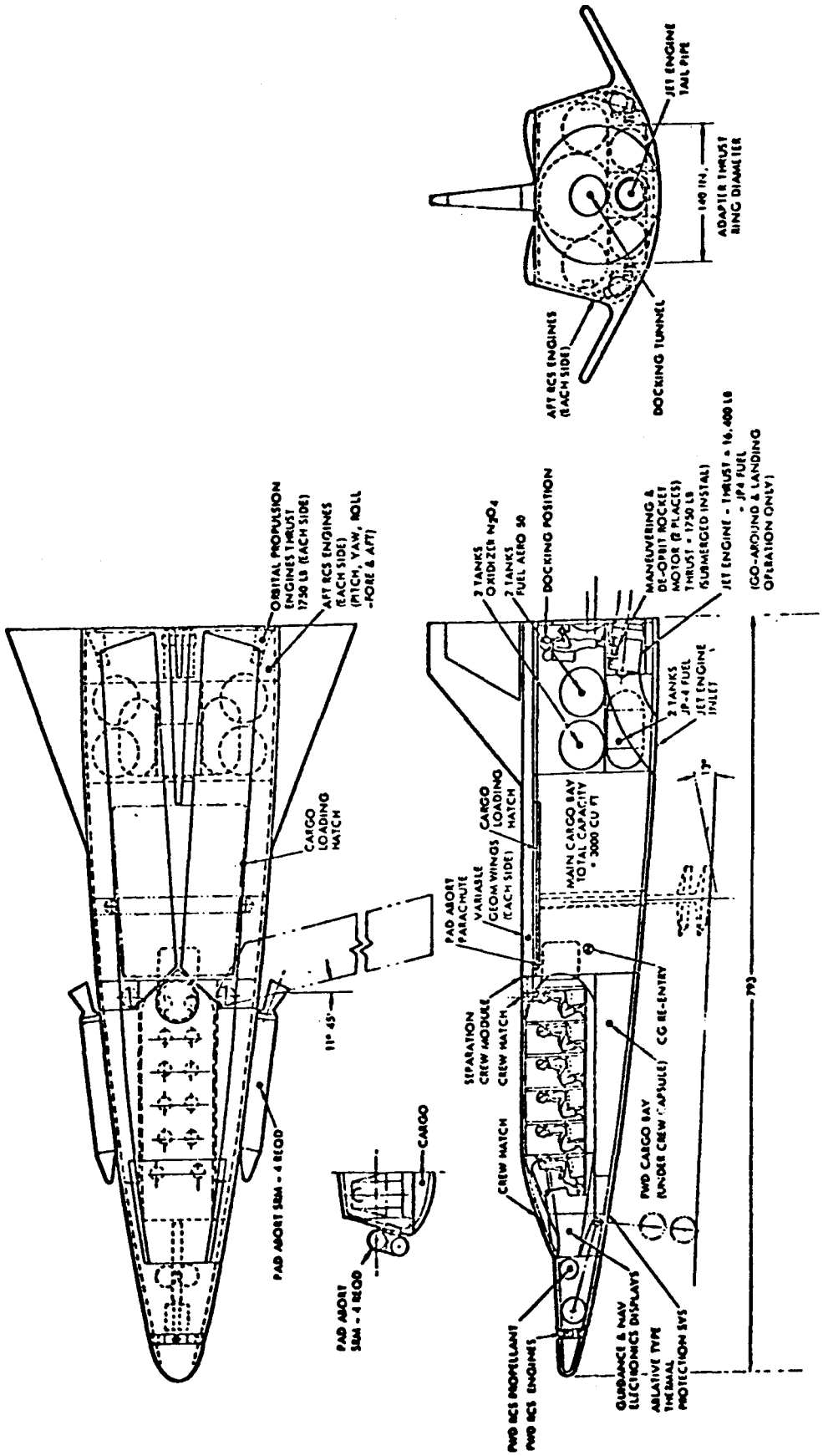
- Logistical support of a space station
- Orbital placement and retrieval of satellites
- Delivery of propulsive stages and payloads to space
- Propellant delivery to orbit
- Satellite servicing and maintenance
- Short-duration manned orbital missions

As a group, the SSTG expressed a marked preference for fully or near-fully reusable systems, endorsing the Shuttle as "the keystone to the success and growth of future space flight developments for the exploration and beneficial uses of near and far space."[14] The challenge lay in deriving a design to satisfy these demanding missions. Such a task, of course, was no easy one. Any design capable of fulfilling all of these missions would require numerous design tradeoffs, some of which included:

- partially or fully reusable system
- "flyback" piloted booster vs. expendable unmanned booster
- winged vs. lifting body configuration
- if winged, whether a delta, sweptwing, straight-wing, or variable-sweep wing
- off-the-shelf engines vs. a new propulsion system
- vertical vs. horizontal launch
- low (200 n.m.) vs. high (1100+ n.m.) cross-range
- small vs. large payload bay and capacity
- sequential staging vs. parallel-burn staging

At this very tentative stage in Shuttle development, the Space Shuttle Task Group divided the concept studies it reviewed into three classes: Class I consisted of recoverable orbiters using expendable boosters. Class II were stage-and-a-half concepts. Class III involved fully reusable TSTO vehicles. The payload size and capacities of these craft (up to 15 x 60 feet and 50,000 lbs.) were determined by NASA's anticipated needs to carry space station components and sections into orbit. It should be noted that the SSTG classes bore no relationship to those of the earlier AACB study effort. Six specific configurations received detailed review. The first, a Class I concept, was the so-called MURP (Manned Upper Reusable Payload), a McDonnell lifting body (Figure 30) based upon a NASA configuration, using variable-geometry wings and a landing engine, and ultimately rejected for further consideration due to poor landing visibility, anticipated heating problems, and a complicated wing-pivot mechanism. The second and third configurations were the previously discussed Lockheed Star Clipper and a McDonnell parallel-tankage stage-and-a-half concept illustrated in Figures 4-7, both falling within Class II. The remaining three configurations examined belonged in Class III. The fourth was a TSTO design based on the NASA HL-10

Figure 30



MCDONNELL-NASA MURP LOGISTICAL SPACECRAFT

lifting body shape; the Aero-Physics Division at Langley Research Center proposed using one HL-10 shape as a booster, launching a second HL-10 orbiter into space. Both orbiter and booster would have had air-breathing engines to cruise back to the landing site following atmospheric entry (see Figure 31). The fifth, by General Dynamics' Convair Astronautics Division, grew from the company's earlier "Triamese" studies. Convair had studied variable-geometry lifting reentry vehicles starting in 1965. "Triamese," (Figure 32a) consisted of three equally sized and aerodynamically similar stages joined together to form a two-stage booster with an orbital stage. This MUSTARD-like concept had been initially undertaken by Convair for the Air Force's Space and Missile Systems Organization (SAMSO), and with NASA's rapidly jelling interest in space transporters, Convair evolved this concept into a new orbital vehicle for the civilian space agency. The Convair approach for NASA differed from the firm's previous work for the Air Force in that Convair's earlier efforts had been marked by a conscious attempt to reduce developmental costs by utilizing three basically similar vehicles. The new concept retained the notion of aerodynamic similarity between the stages, but partially rejected outright commonality since maximum performance, minimum weight, and minimum recurring costs were considered more important than minimal development costs, and identical sizing of the stages was incompatible with these avowed design goals. The Convair concept envisioned two derivative vehicles, the FR-3A and the FR-4, the former a "two-element" TSTO, and the latter a "three-element" TSTO. Figures 32b-32d show these configurations plus an anticipated mission profile. The FR-3A consisted of a large booster stage and a smaller orbital stage; the FR-4 consisted of two booster stages mated to an aerodynamically identical and equally large orbital stage--thus FR-4 came closest to the earlier Triamese idea, though it lacked some propellant cross-feed arrangements that had characterized the earlier vehicle. The Convair vehicles had a slender "flatiron"

Figure 31

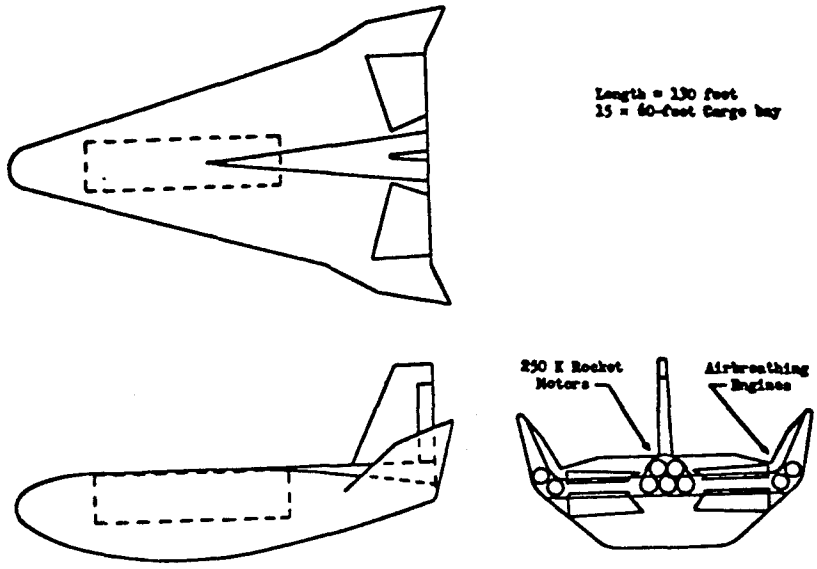


Fig. 31a: HL-10 orbiter

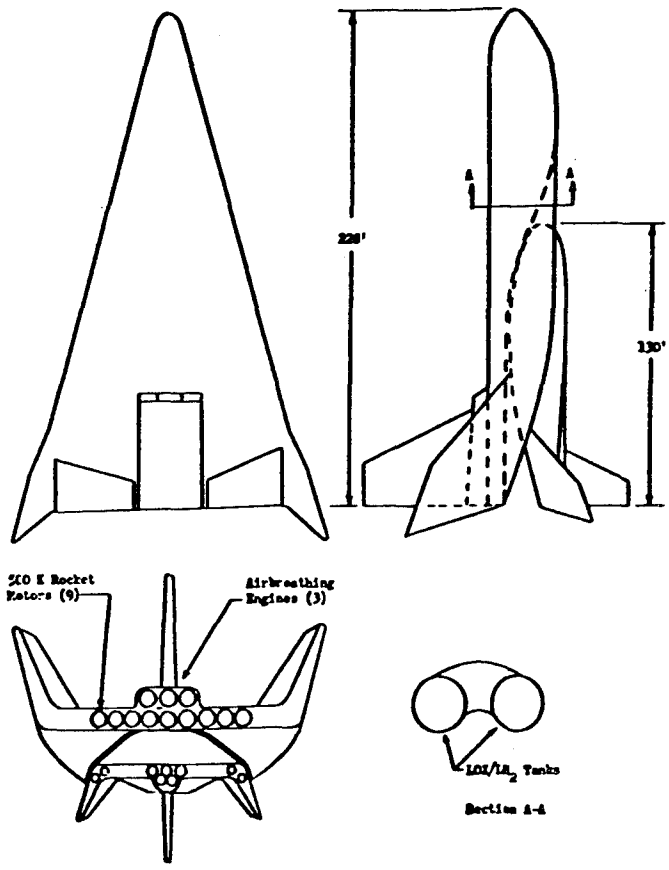
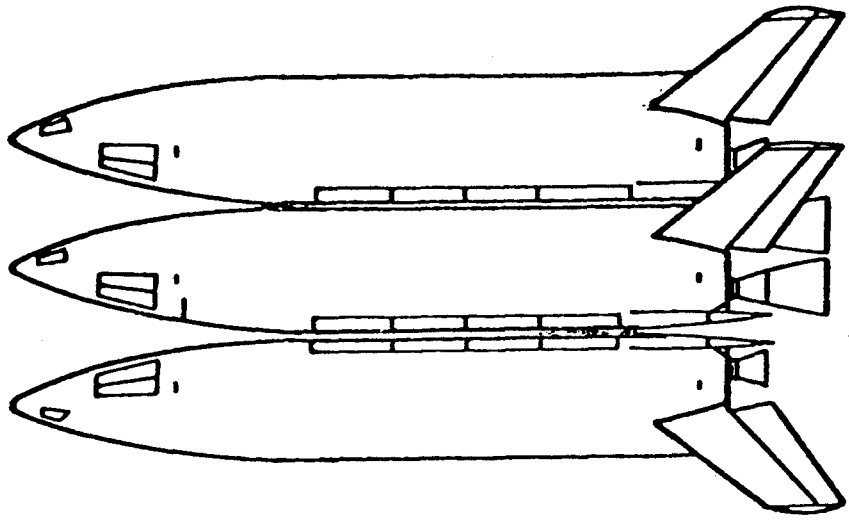
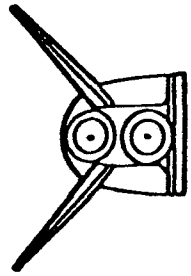


Fig. 31b: HL-10 booster mated to orbiter

Figure 32a



CONVAIR TRIAMESE STUDIED BY NASA'S SSTG, 1969



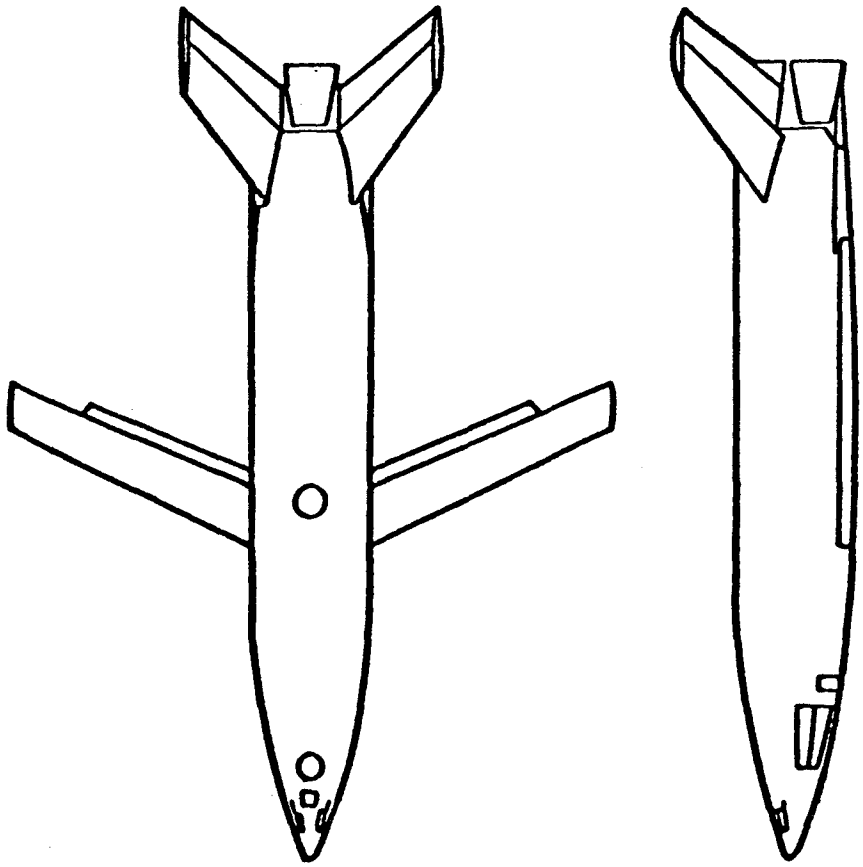
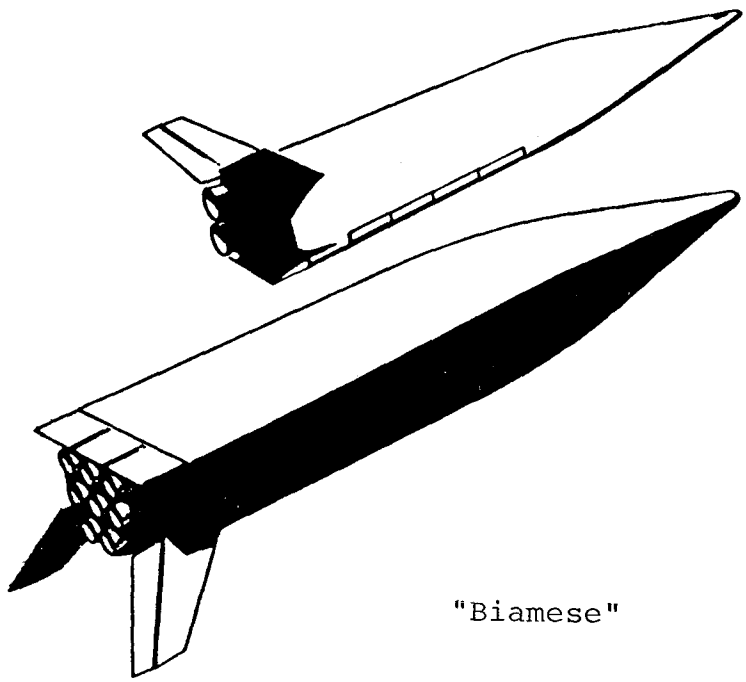


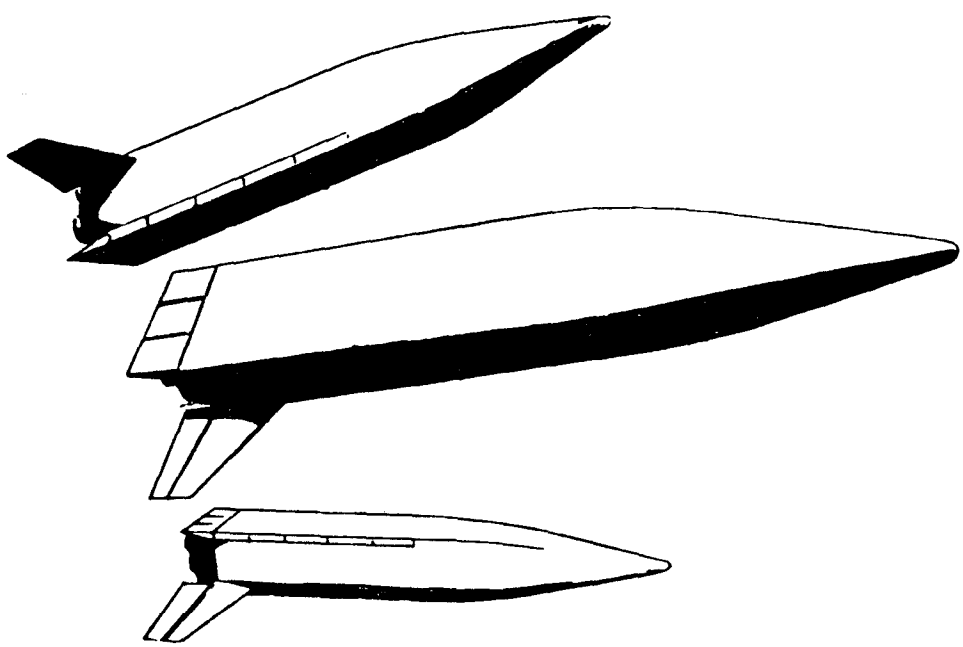
Figure 32b

TWO-STAGE, TWO-ELEMENT CONCEPT - FR-3A



"Biamese"

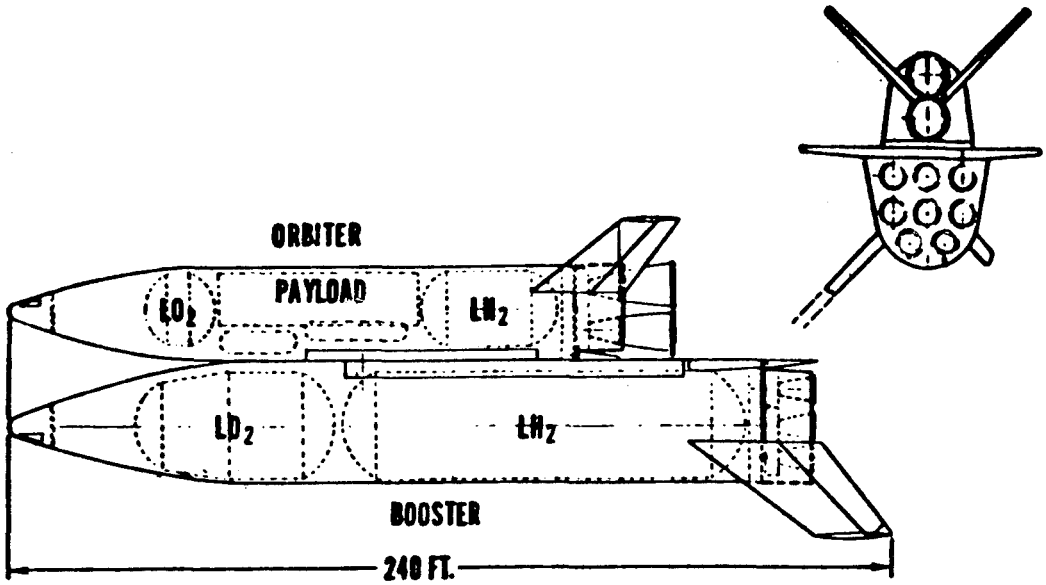
TWO-STAGE, THREE-ELEMENT CONCEPT - FR-4



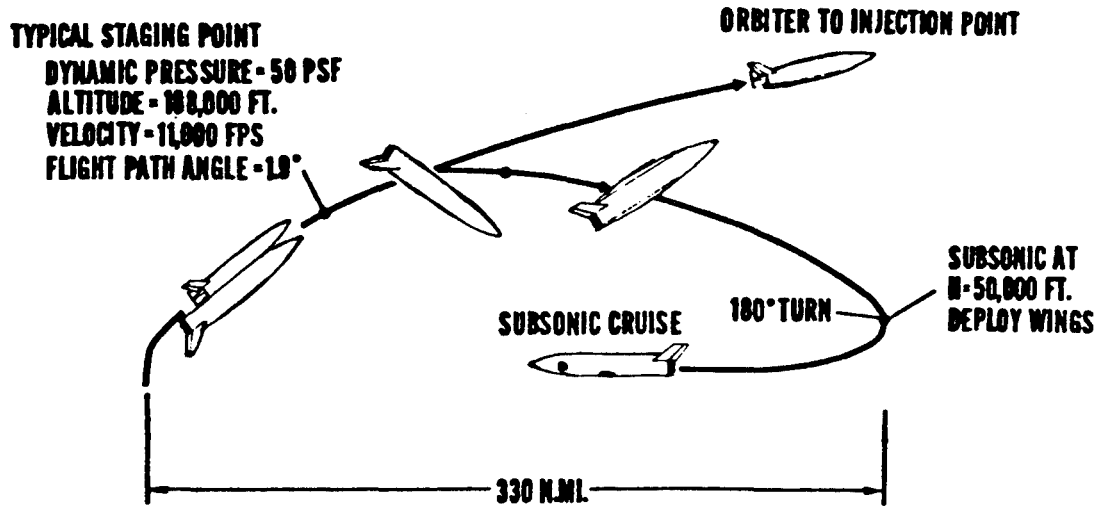
"Triamese"

CONVAIR FULLY REUSABLE SHUTTLE CONCEPTS, 1969

FR-3A GENERAL ARRANGEMENT



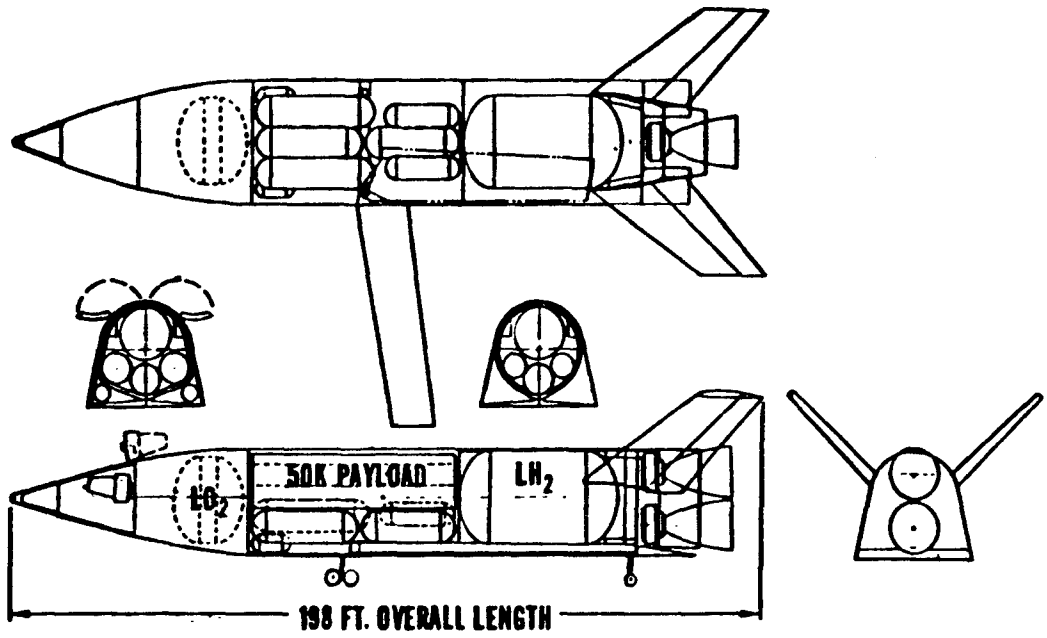
TWO-STAGE FR-3A FLIGHT PROFILE



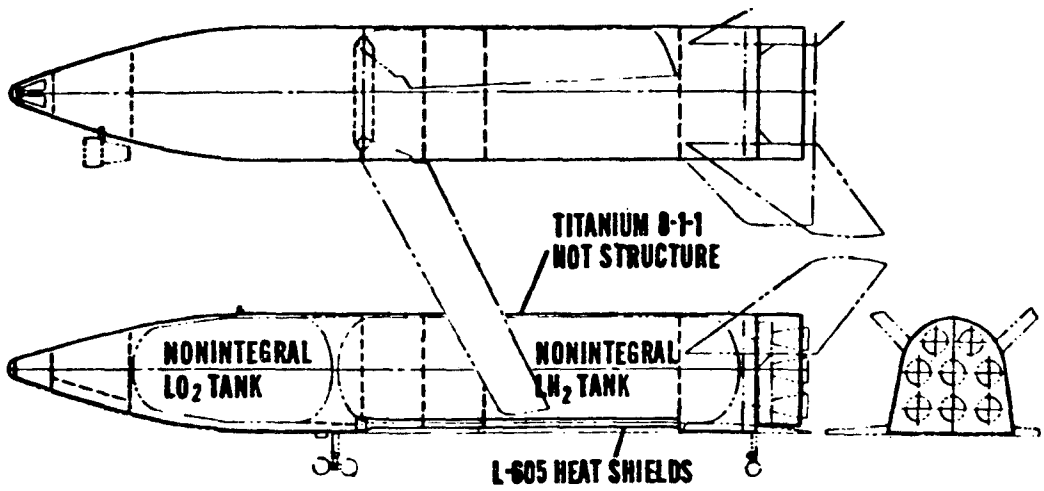
FR-3A LAUNCH CONFIGURATION AND MISSION PROFILE

Figure 32d

ORBITER GENERAL ARRANGEMENT



FR-3A BOOSTER STRUCTURE CONCEPT

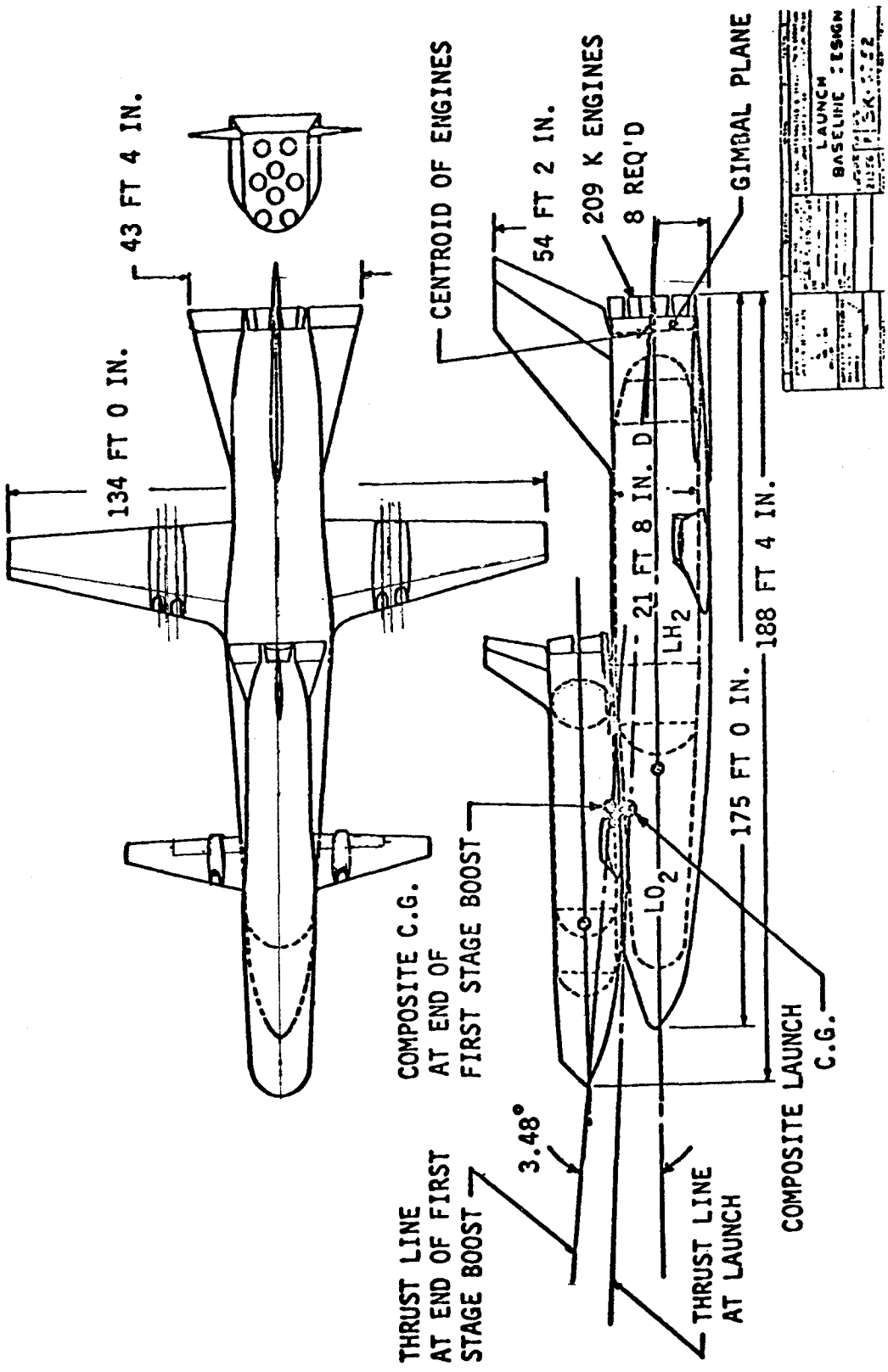


CONVAIR ORBITER AND BOOSTER CONFIGURATIONS

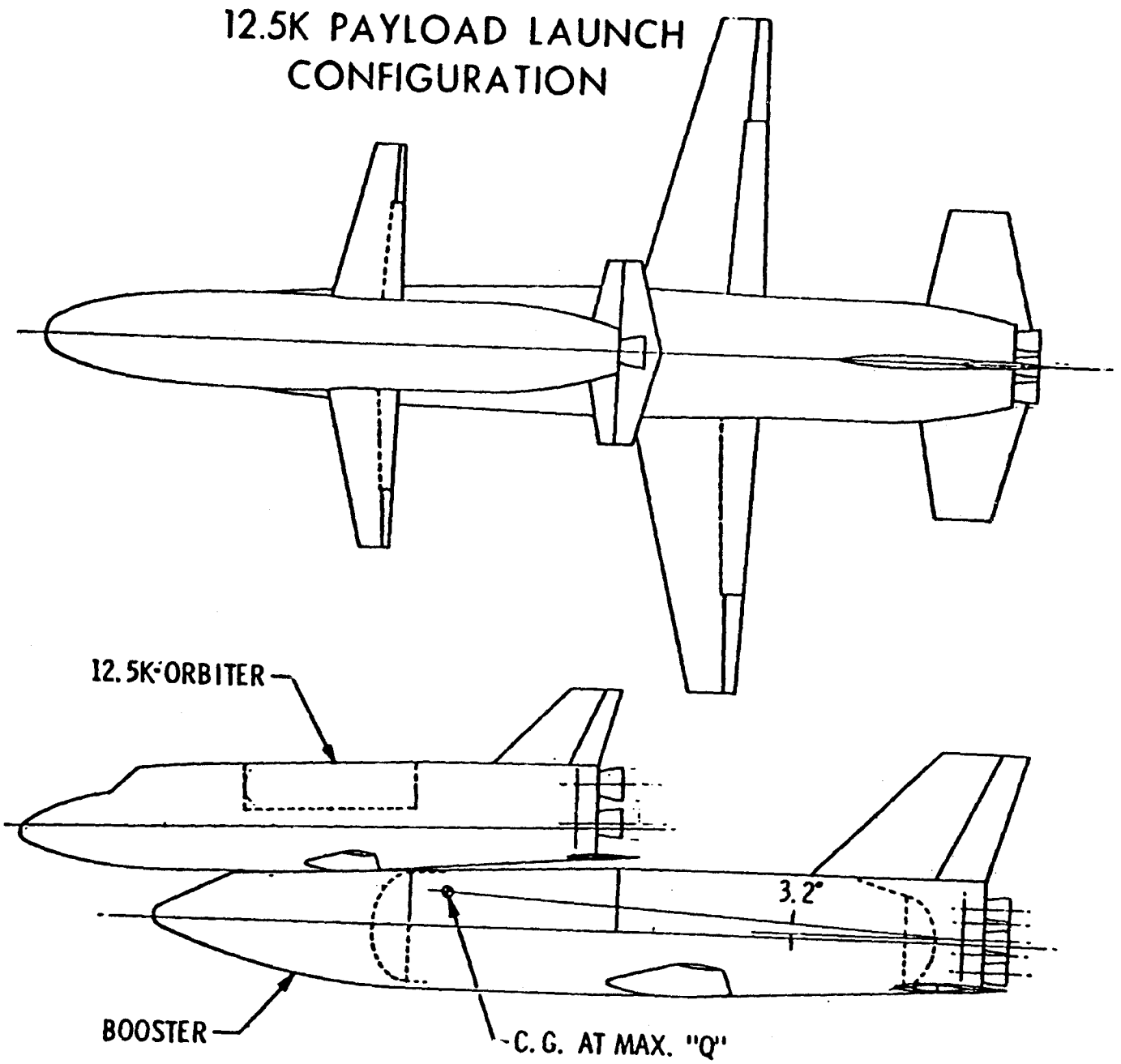
appearance, and made use of stowed landing engines and variable-sweep wings that gave them "decoupled" hypersonic and subsonic characteristics: i.e.: they had good hypersonic performance and equally good subsonic performance, using configuration variation to achieve both. After transiting the atmosphere, the vehicles would deploy their engines and wings to cruise to an appropriate landing site. The Convair transporters received serious consideration, but were not pursued further than Phase A. The sixth configuration evaluated during this stage of Shuttle development, however, underwent widespread analysis, and served as the basis for numerous design spinoffs: a controversial straight-wing orbiter designed by Maxime Faget of the Manned Spacecraft Center.[15]

Faget's orbiter concept consisted of a low-wing design of moderate aspect ratio having a blunt-nosed and relatively slab-sided fuselage. Most noteworthy about the design, however, was the straight wing, which gave the craft the look of a 1940s airliner. Faget, a believer in small-payload shuttles having minimal cross-range (on the order of 200 n.m.), intended that his orbiter design would enter the upper atmosphere in a steep angle of attack, with the orbiter inclined 60 degrees to the flight path. It would remain in this attitude until it completed reentry; in the lower atmosphere, around 40,000 feet, it would pitch down to a lower angle of attack, ignite landing engines (either in the wings or nose), and cruise to its landing site. This design formed the core of subsequent NASA Shuttle studies until the agency's engineers were almost forcibly made to recognize that the vehicle possessed several fatal flaws--such as a suspected inability to withstand the thermal environment of reentry, and a tendency to hypersonic spins. The basic layout is shown in Figure 33; selected variations on this theme are shown in Figures 34-58. A review of configurations demonstrates that NASA engineers varied such values as aspect ratio, sweep, tail location, and the like, using parametric manipulation to derive

Figure 33



NASA MSC ILRV TSTO CONCEPT, 1969



FAGET STRAIGHT-WING SHUTTLE CONCEPT: GENERAL CONFIGURATION

Figure 35

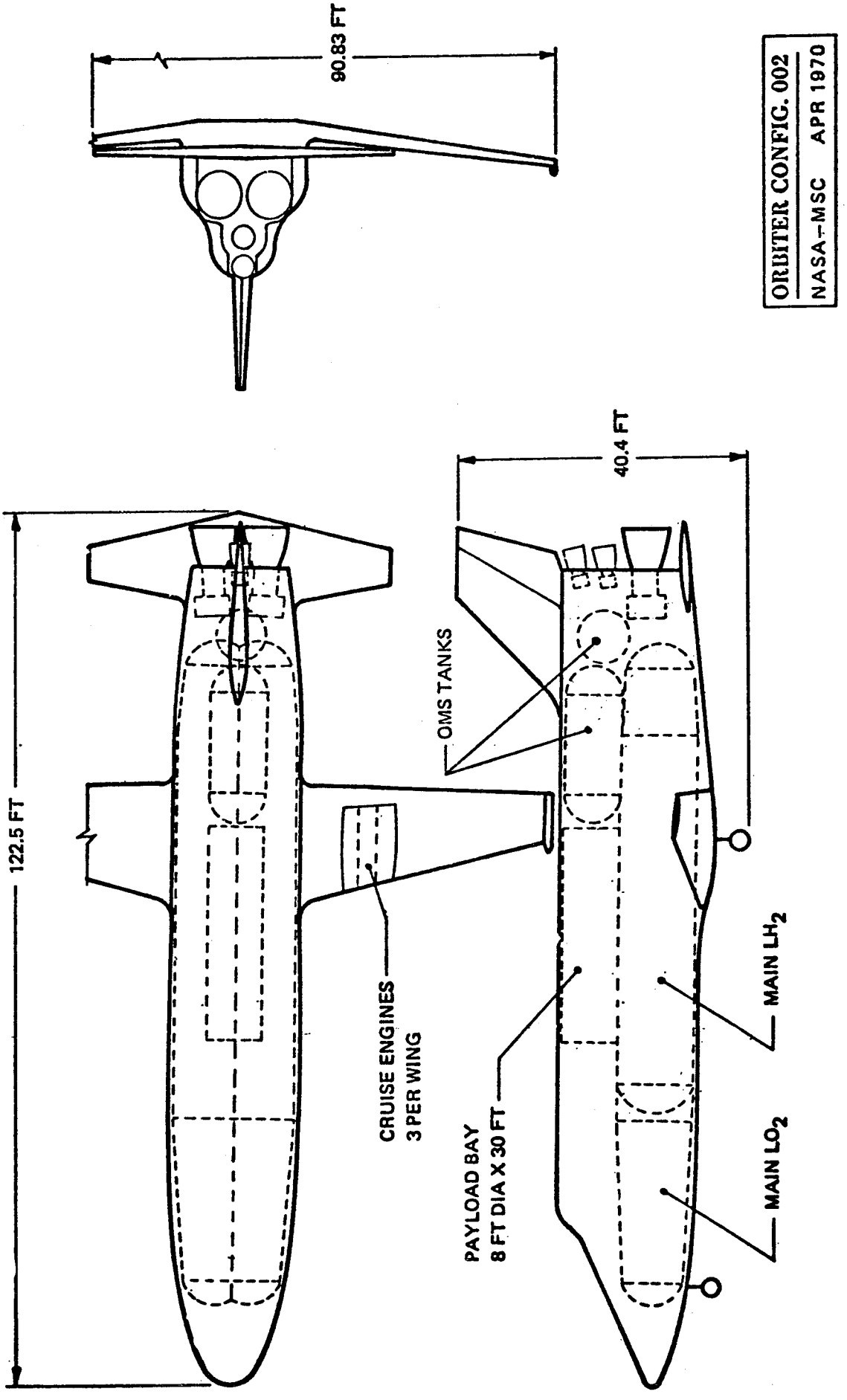

90.83 FT
ORBITER CONFIG. 002
NASA-MSC APR 1970
40.4 FT
122.5 FT
OMS TANKS
CRUISE ENGINES
3 PER WING
PAYLOAD BAY
8 FT DIA X 30 FT
MAIN LH_2
MAIN LO_2

Figure 36

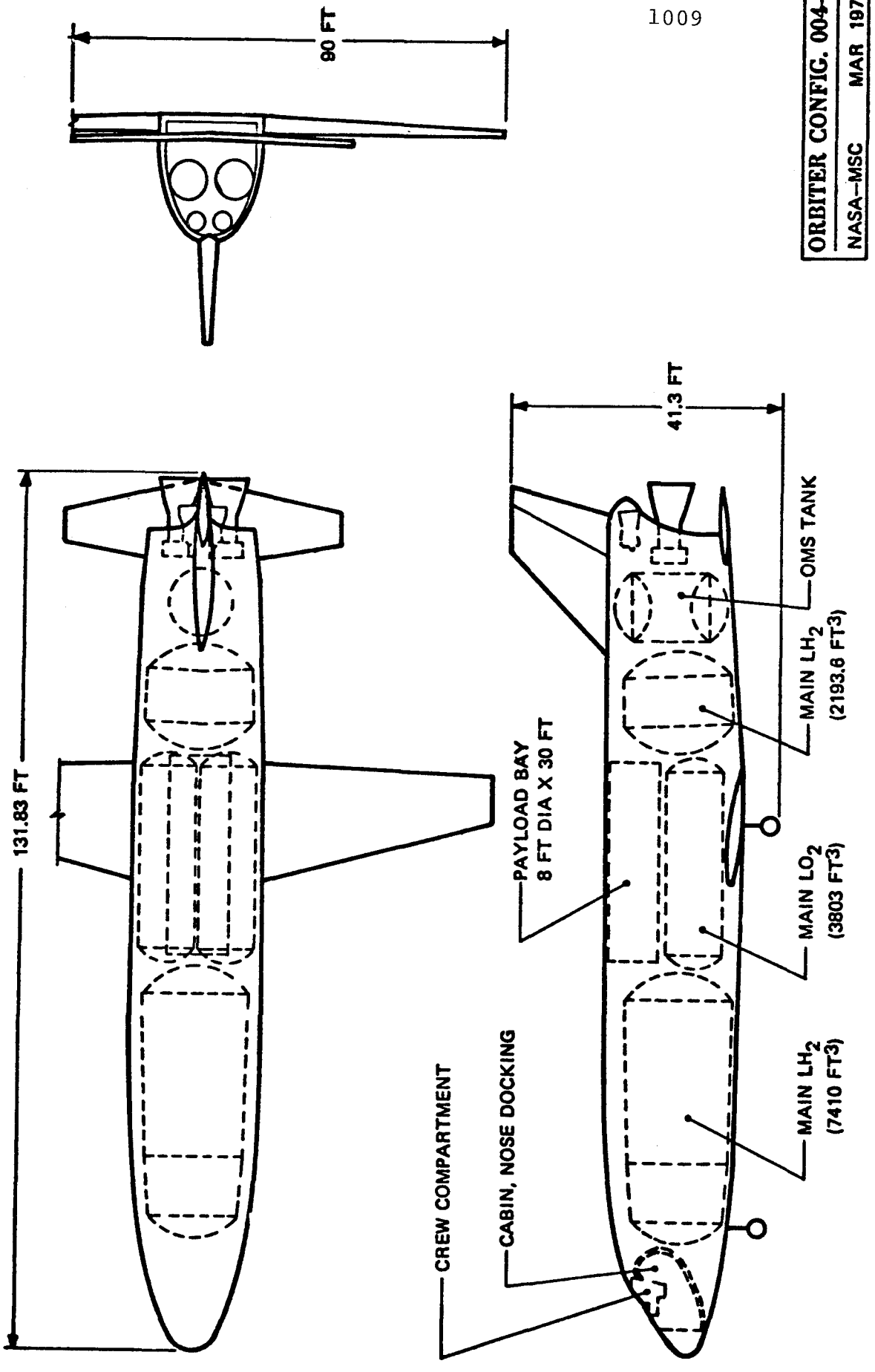
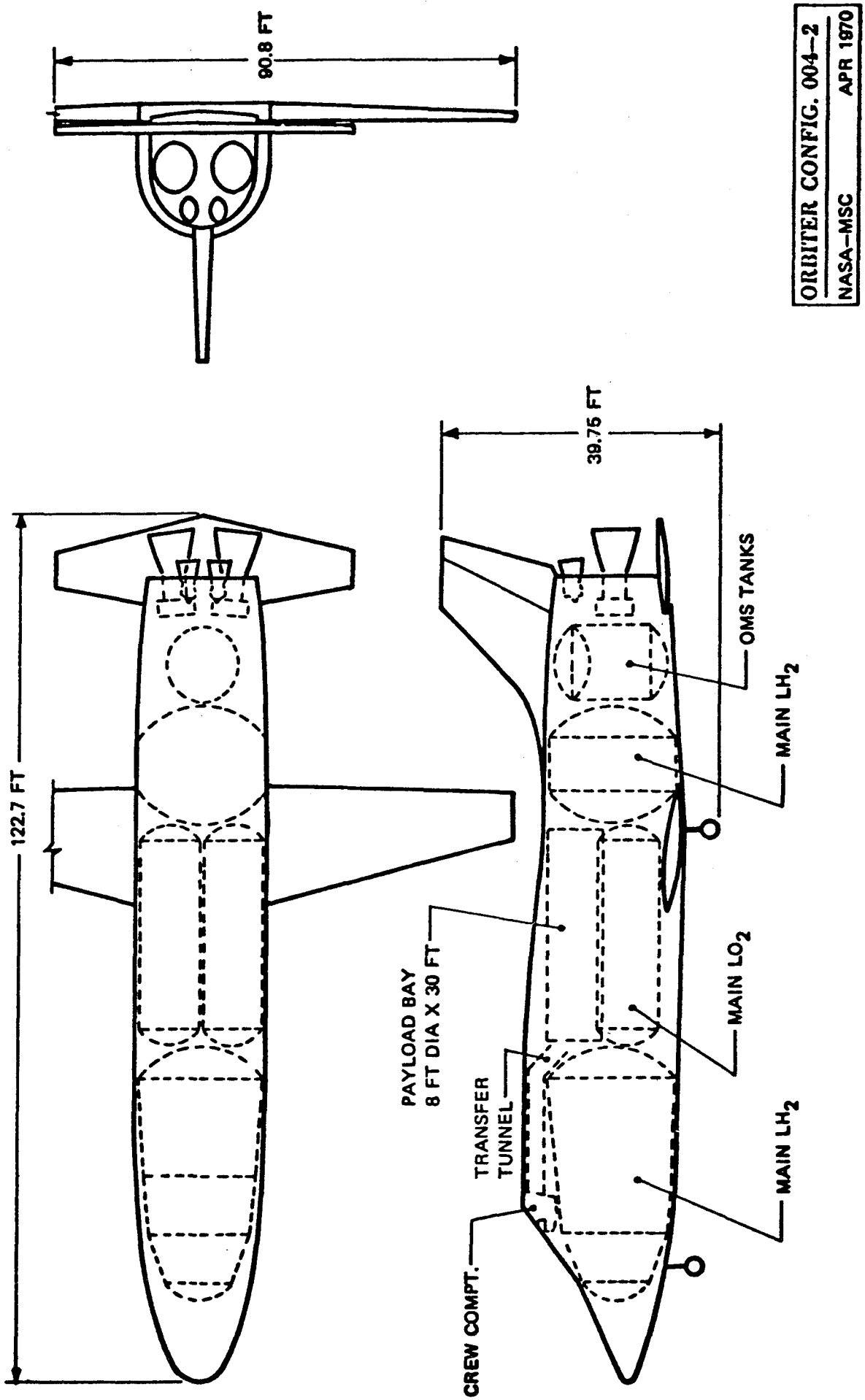
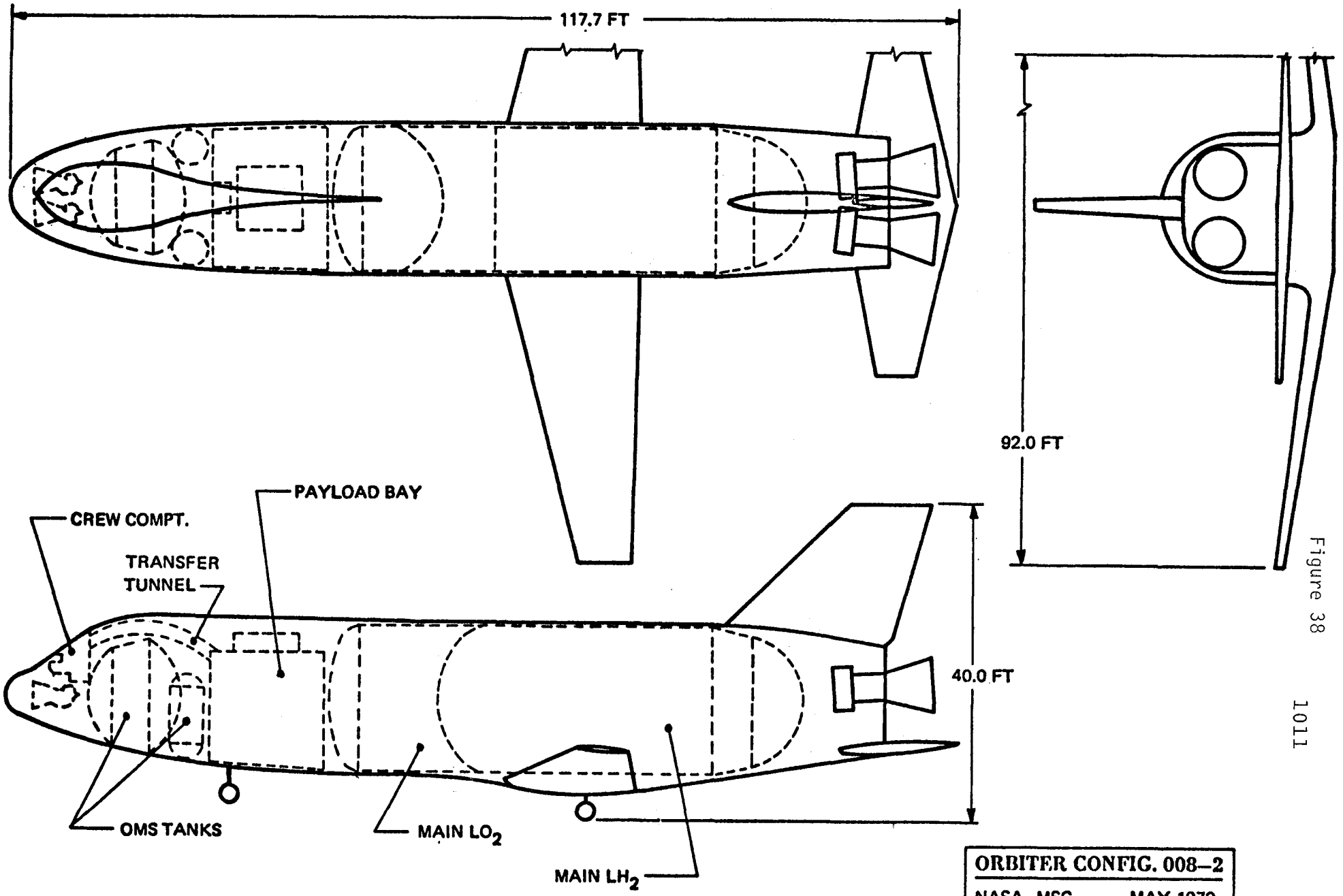

90 FT
41.3 FT
131.83 FT
OMS TANK
MAIN LH2
(2193.6 FT3)
PAYLOAD BAY
8 FT DIA X 30 FT
MAIN LO2
(3803 FT3)
MAIN LH2
(7410 FT3)
CREW COMPARTMENT
CABIN, NOSE DOCKING
ORBITER CONFIG. 004-1
NASA-MSC MAR 1970

Figure 37


90.8 FT
ORBITER CONFIG. 004-2
NASA-MSC
APR 1970
39.75 FT
122.7 FT
OMS TANKS
MAIN LH_2
PAYLOAD BAY
8 FT DIA X 30 FT
MAIN LO_2
TRANSFER
TUNNEL
MAIN LH_2
CREW COMPT.

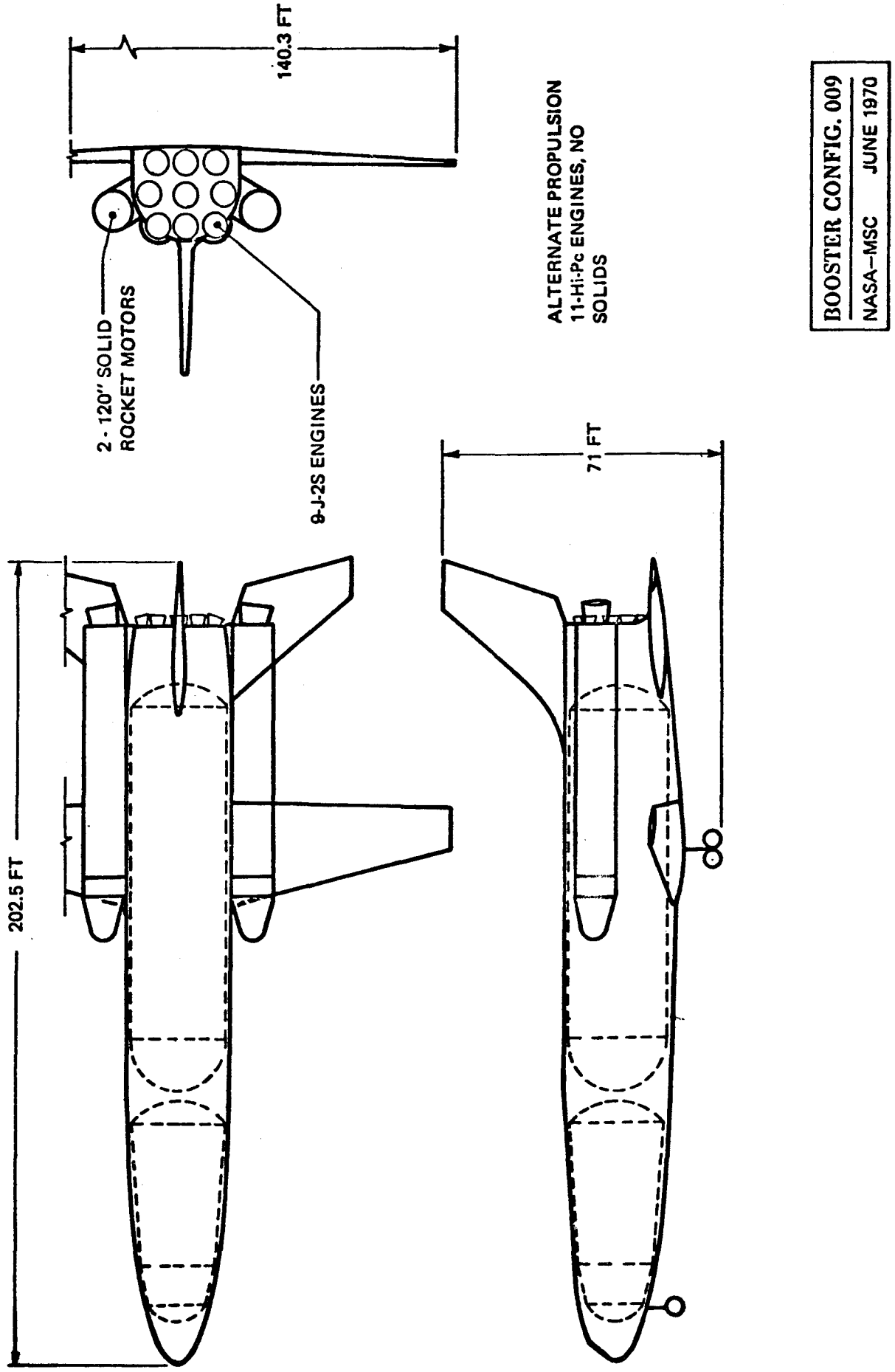


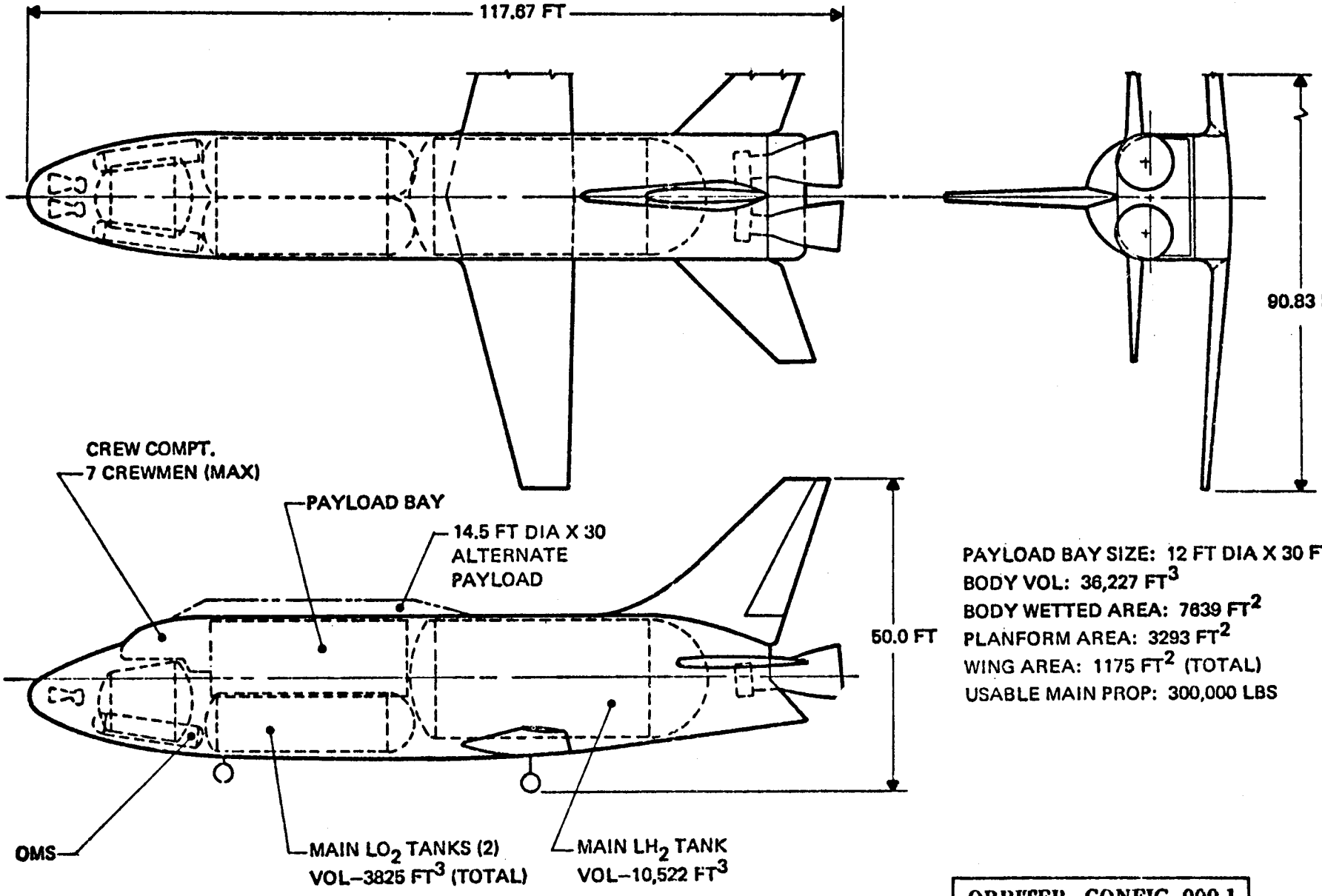
ORBITER CONFIG. 008–2

NASA–MSC MAY 1970

Figure 38

Figure 39


140.3 FT
2 - 120" SOLID ROCKET MOTORS
9-J-2S ENGINES
ALTERNATE PROPULSION
11-Hi-Pc ENGINES, NO SOLIDS
BOOSTER CONFIG. 009
NASA—MSC JUNE 1970
71 FT
202.5 FT

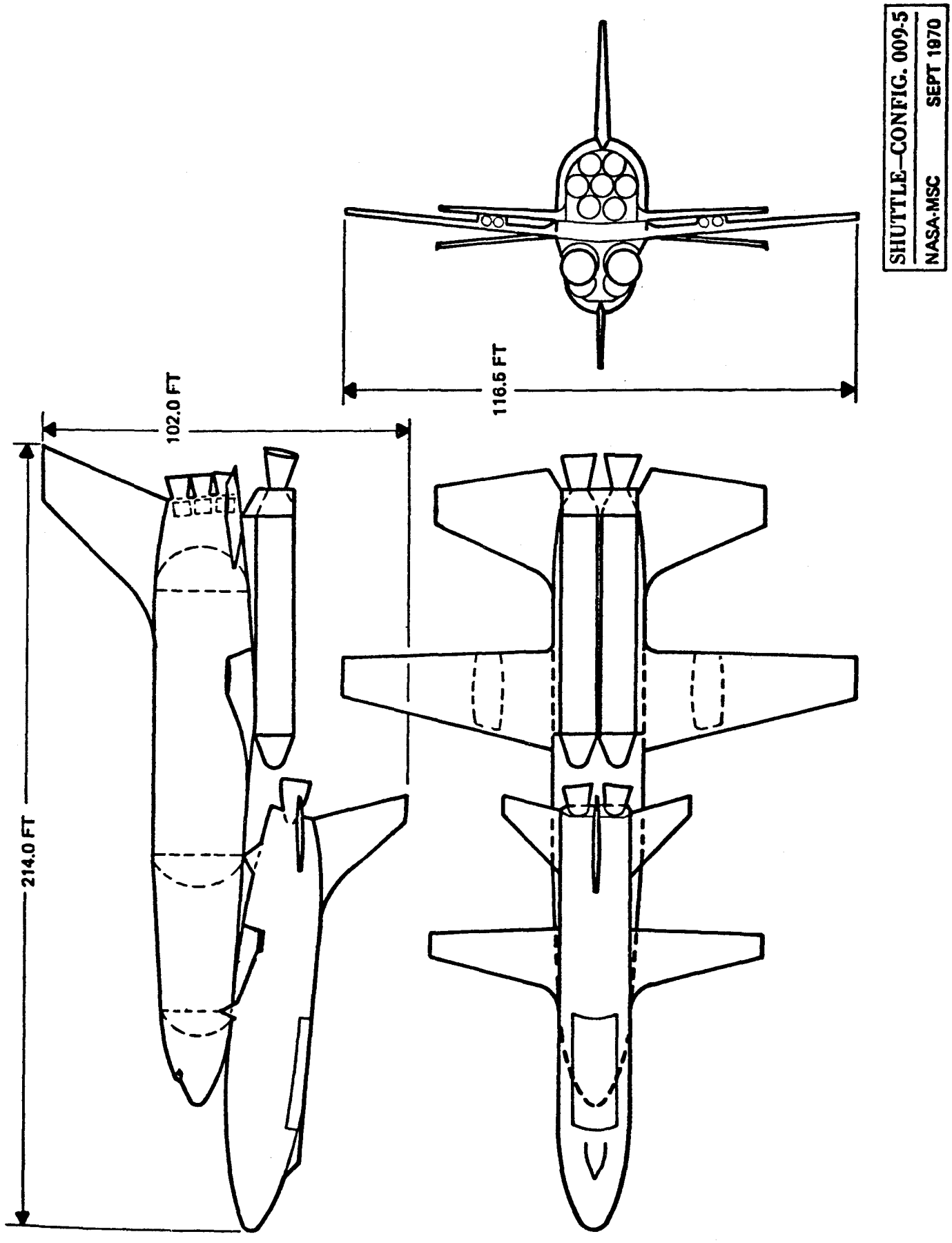


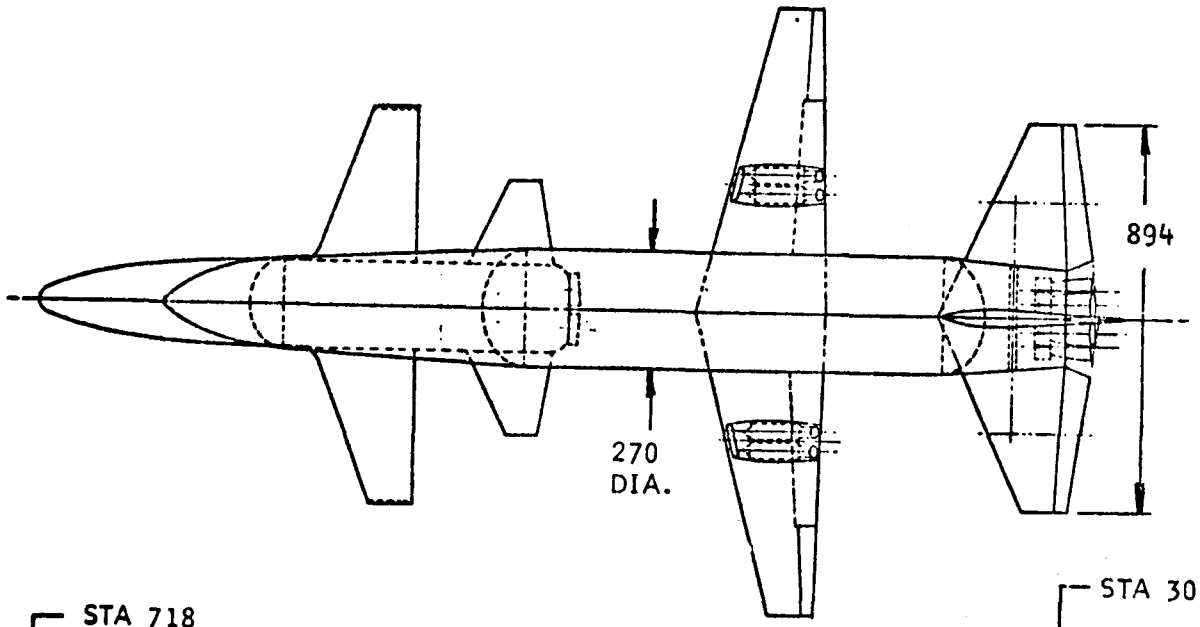
PAYLOAD BAY SIZE: 12 FT DIA X 30 FT LONG
BODY VOL: 36,227 FT^3
BODY WETTED AREA: 7639 FT^2
PLANFORM AREA: 3293 FT^2
WING AREA: 1175 FT^2 (TOTAL)
USABLE MAIN PROP: 300,000 LBS

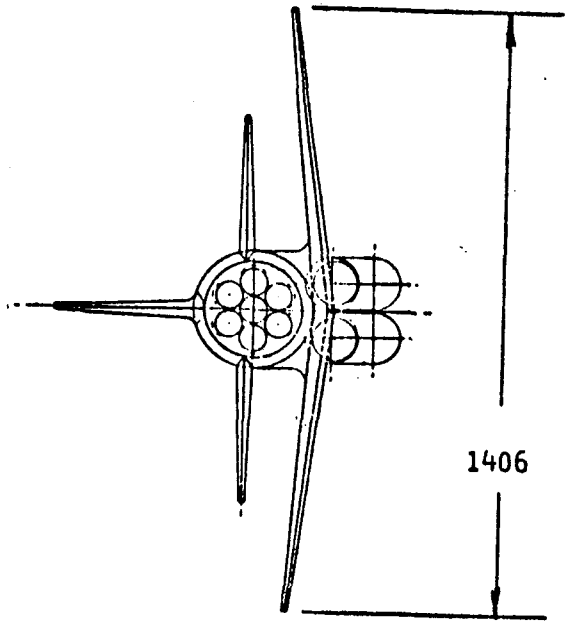
ORBITER–CONFIG. 009-1	
NASA-MSC	AUG 1970

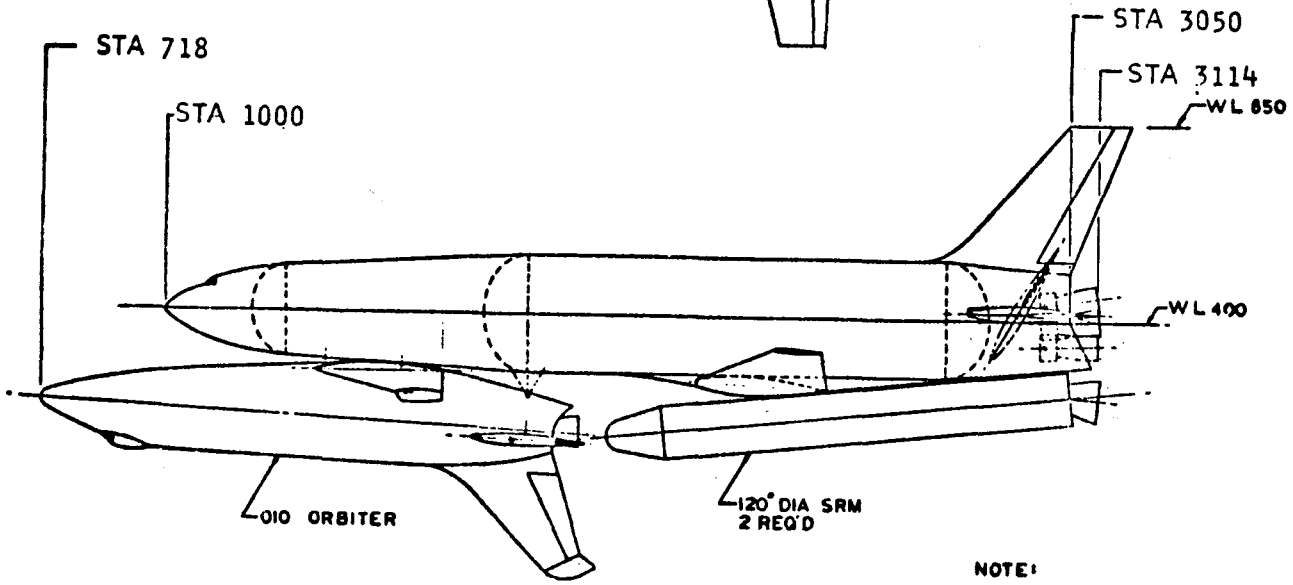
Figure 40

Figure 41


SHUTTLE—CONFIG. 009-5
NASA-MSC SEPT 1970
116.6 FT
102.0 FT
214.0 FT







NOTE:
INITIAL LAYOUT, DESIGN INTEGRATION INCOMPLETE

BOOSTER:

•ALUMINUM HEAT SINK TPS	
•LIQUID CORE USABLE PROP	1020,000Lbs
•SRM USABLE PROP	1002,100Lbs

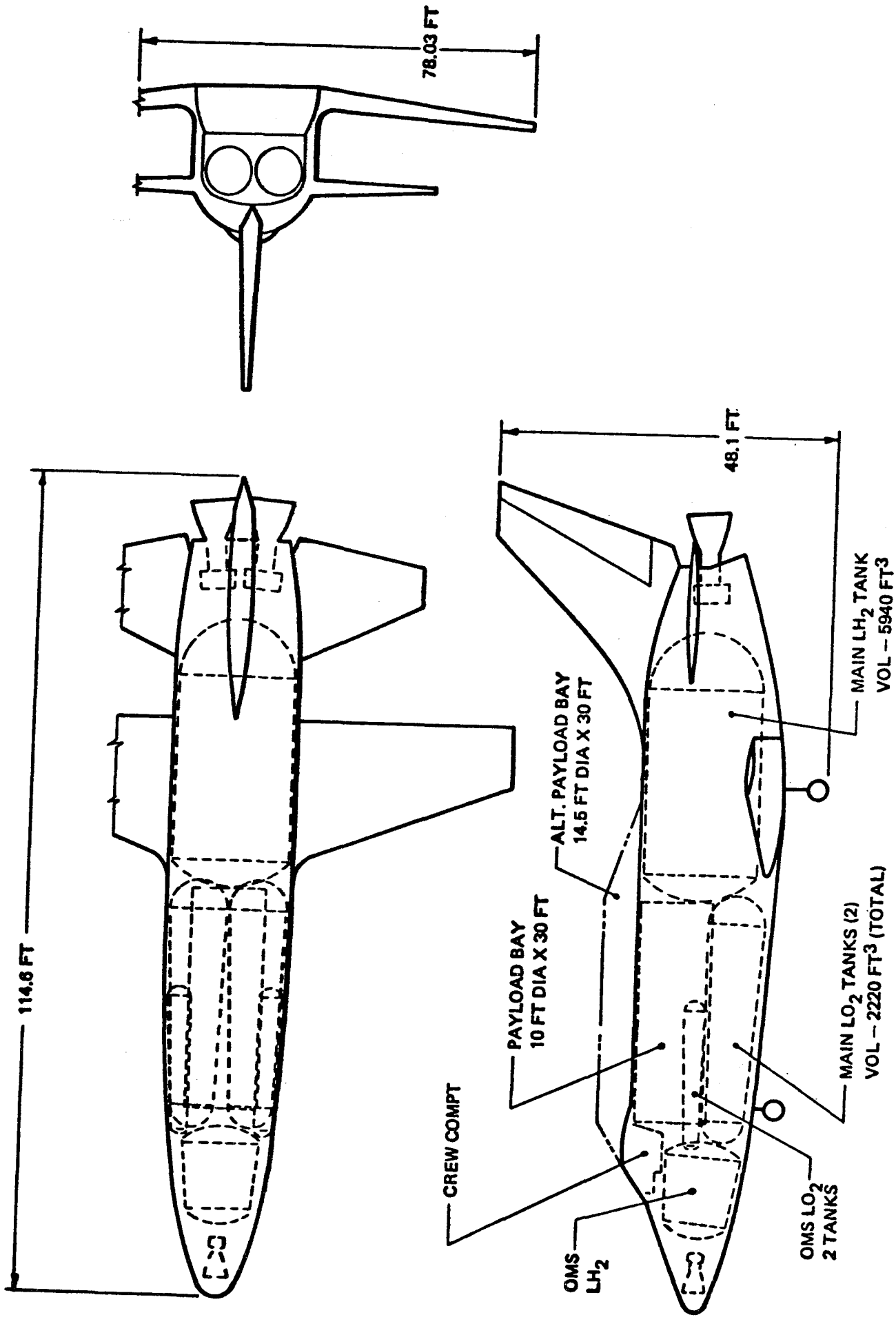
ORBITER:

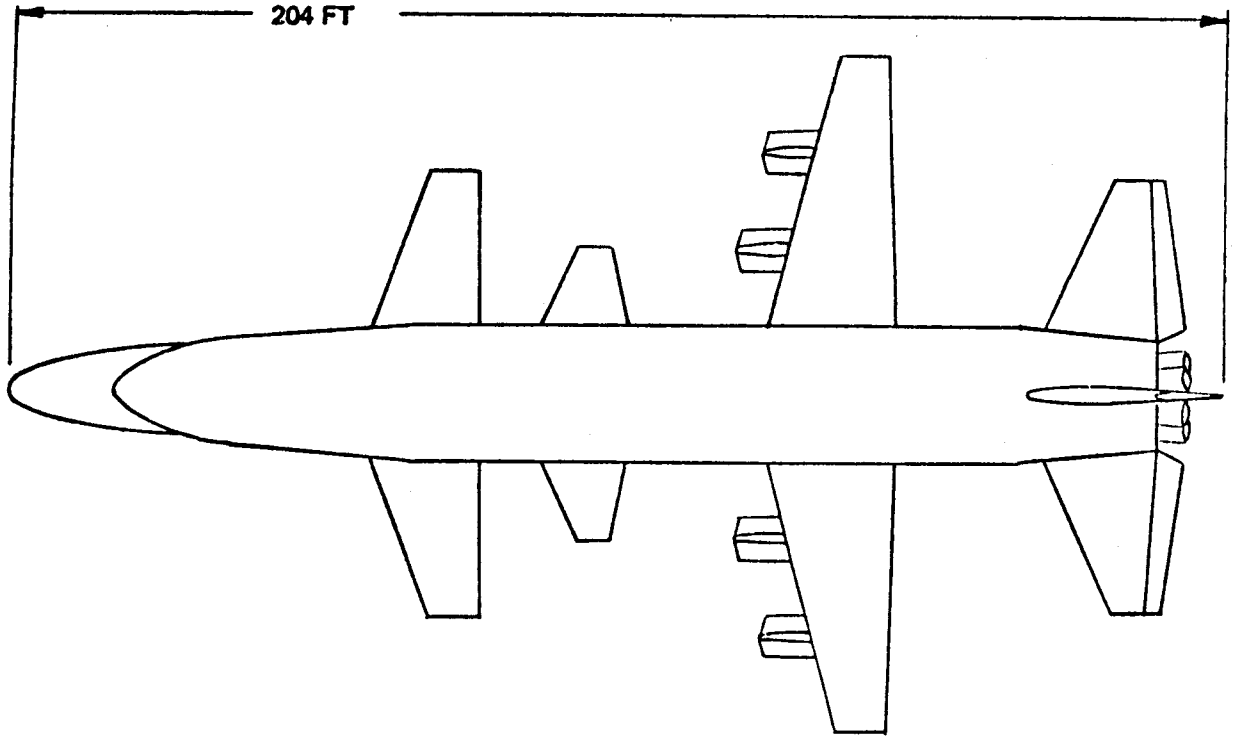
•USABLE PROP	183,250Lbs
•PAYLOAD	25,000Lbs
-LAUNCH T/W	1.402
-MAX Q	500PSF
-GLOW	2,722,950Lbs

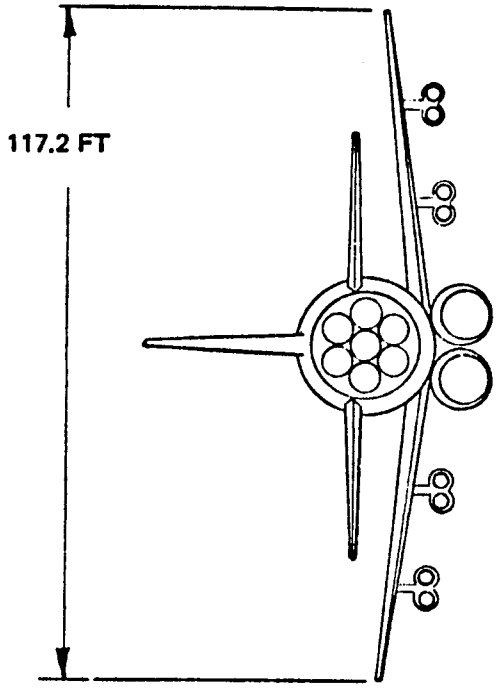
LAUNCH CONFIGURATION 010		
21356	E	SAY-44101260

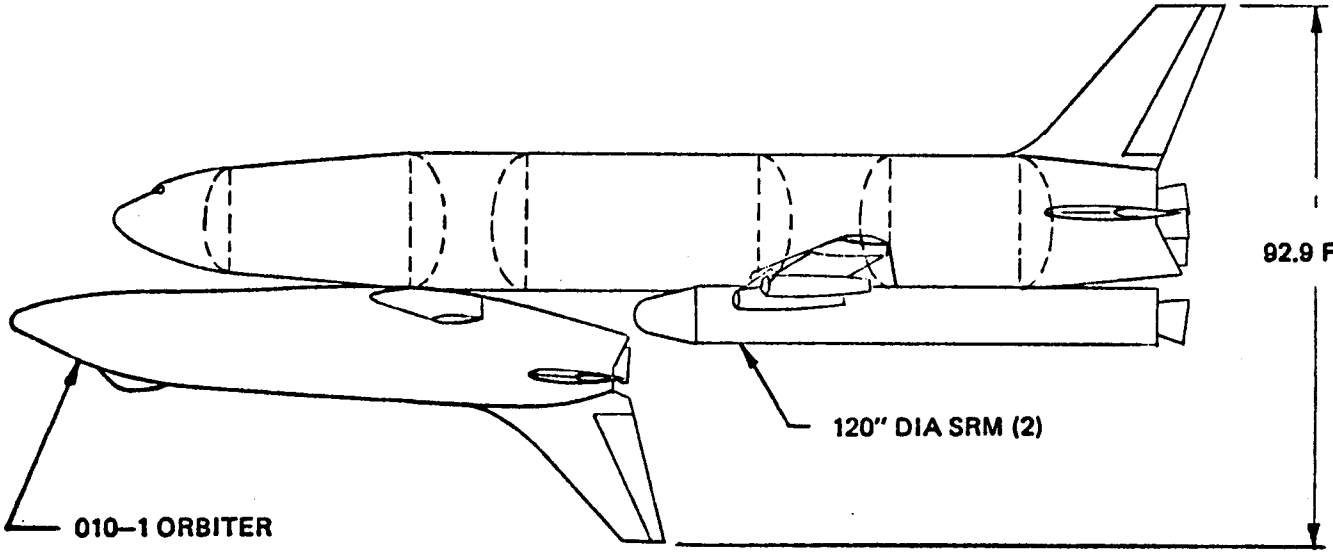
Figure 42

Figure 43


78.03 FT
114.6 FT
48.1 FT
MAIN LH2 TANK
VOL – 5940 FT3
ALT. PAYLOAD BAY
14.5 FT DIA X 30 FT
PAYLOAD BAY
10 FT DIA X 30 FT
MAIN LO2 TANKS (2)
VOL – 2220 FT3 (TOTAL)
CREW COMPT
OMS
LH2
OMS LO2
2 TANKS
ORBITER—CONFIG. 010-1
NASA–MSC
OCT 1970







BOOSTER:

ALUMINUM HEAT SINK - TPS
LIQUID USABLE PROP. = 1,020,000 LB
SRM USABLE PROP. = 1,002,100 LB

ORBITER:

USABLE PROP. = 183,250 LB
GLOW = 2,722,950 LB

SHUTTLE CONFIG. 010–1	
NASA–MSC	NOV 1970

Figure 44

1017

Figure 45

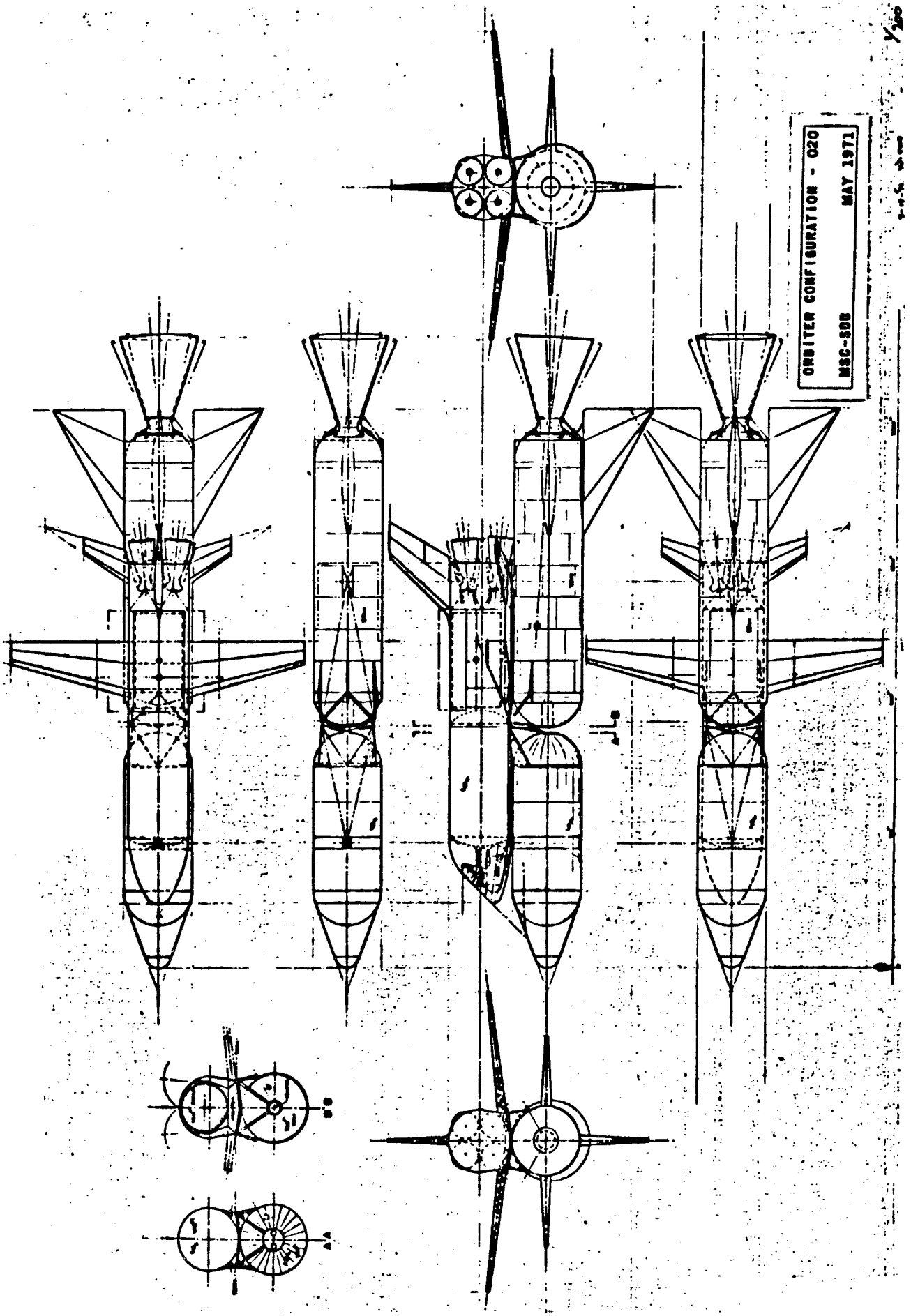


Figure 46

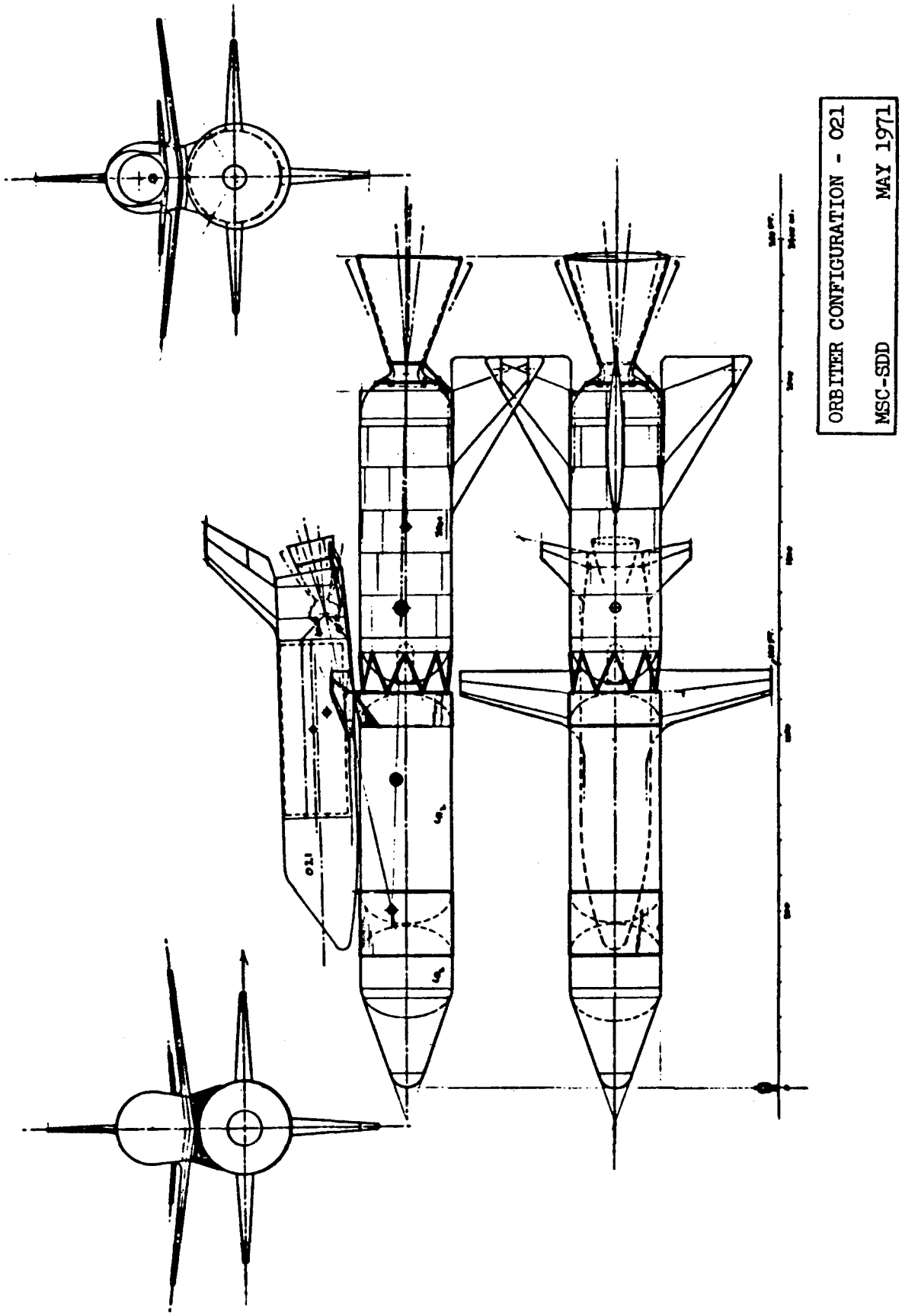


Figure 47

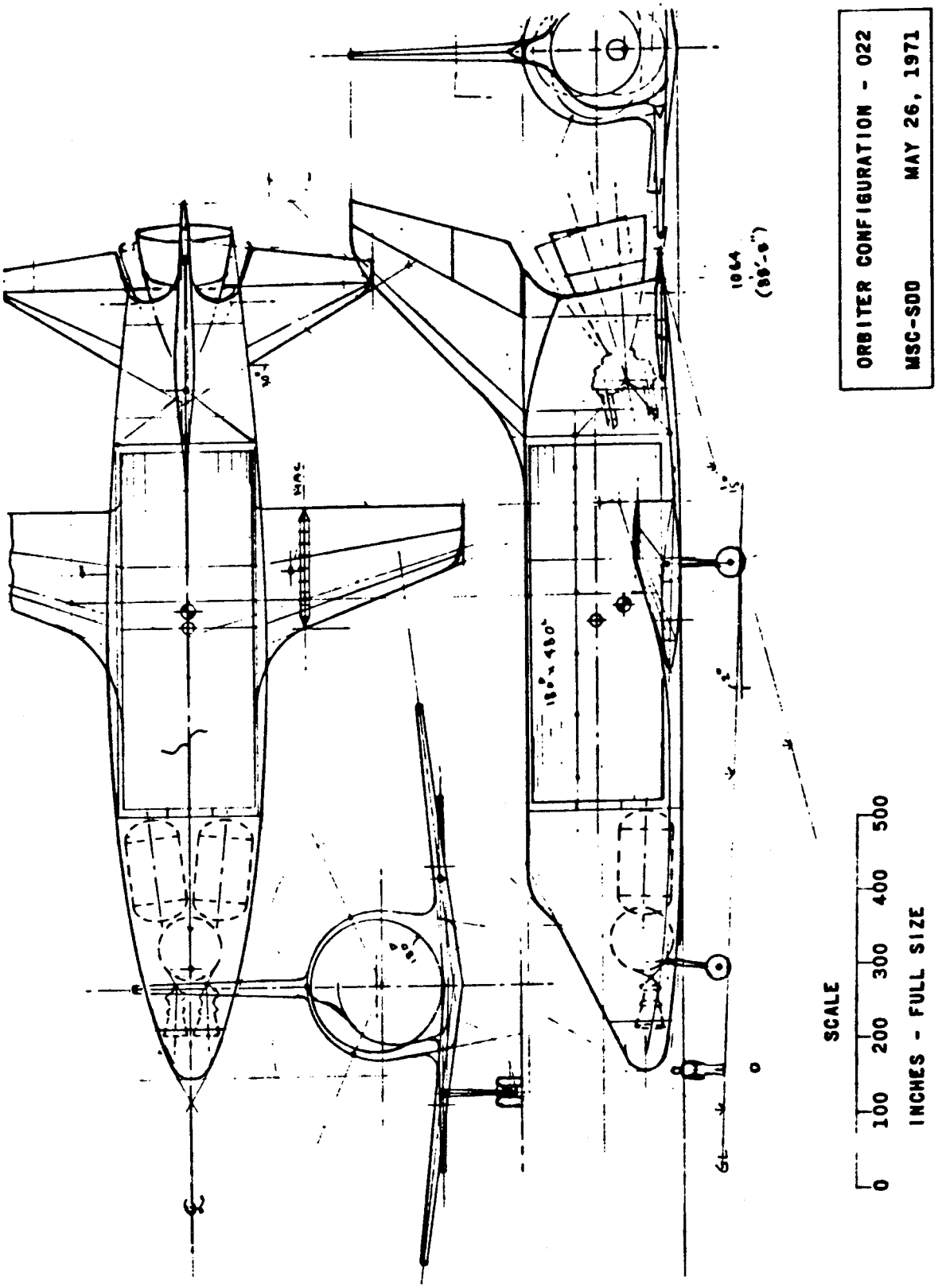

ORBITER CONFIGURATION - 022
MSC-SDO MAY 26, 1971
SCALE
0 100 200 300 400 500
INCHES - FULL SIZE

Figure 48

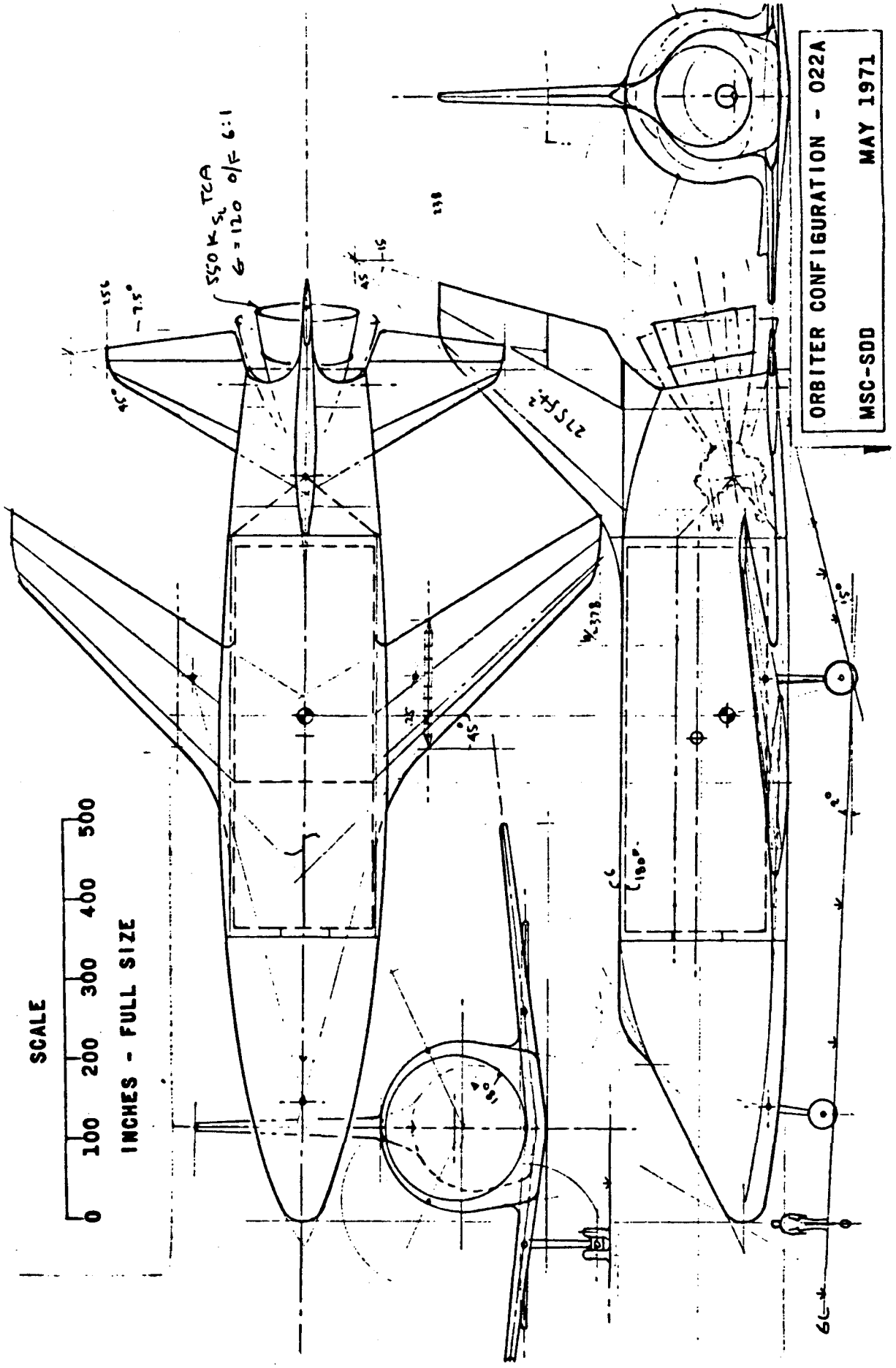

ORBITER CONFIGURATION - 022A
MSC-SDD
MAY 1971
SCALE
0 100 200 300 400 500
INCHES - FULL SIZE

Figure 49 - 50

ORBITERS 022 and 024

(024 is derived by adding shaded sections
to smaller planform of the 022 configuration)

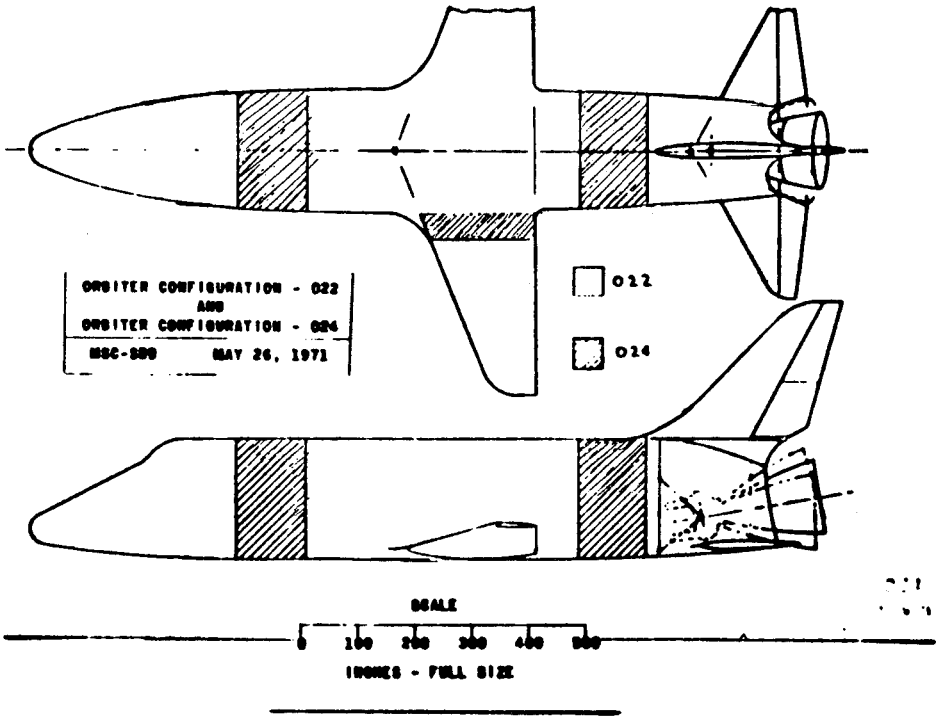


Figure 51

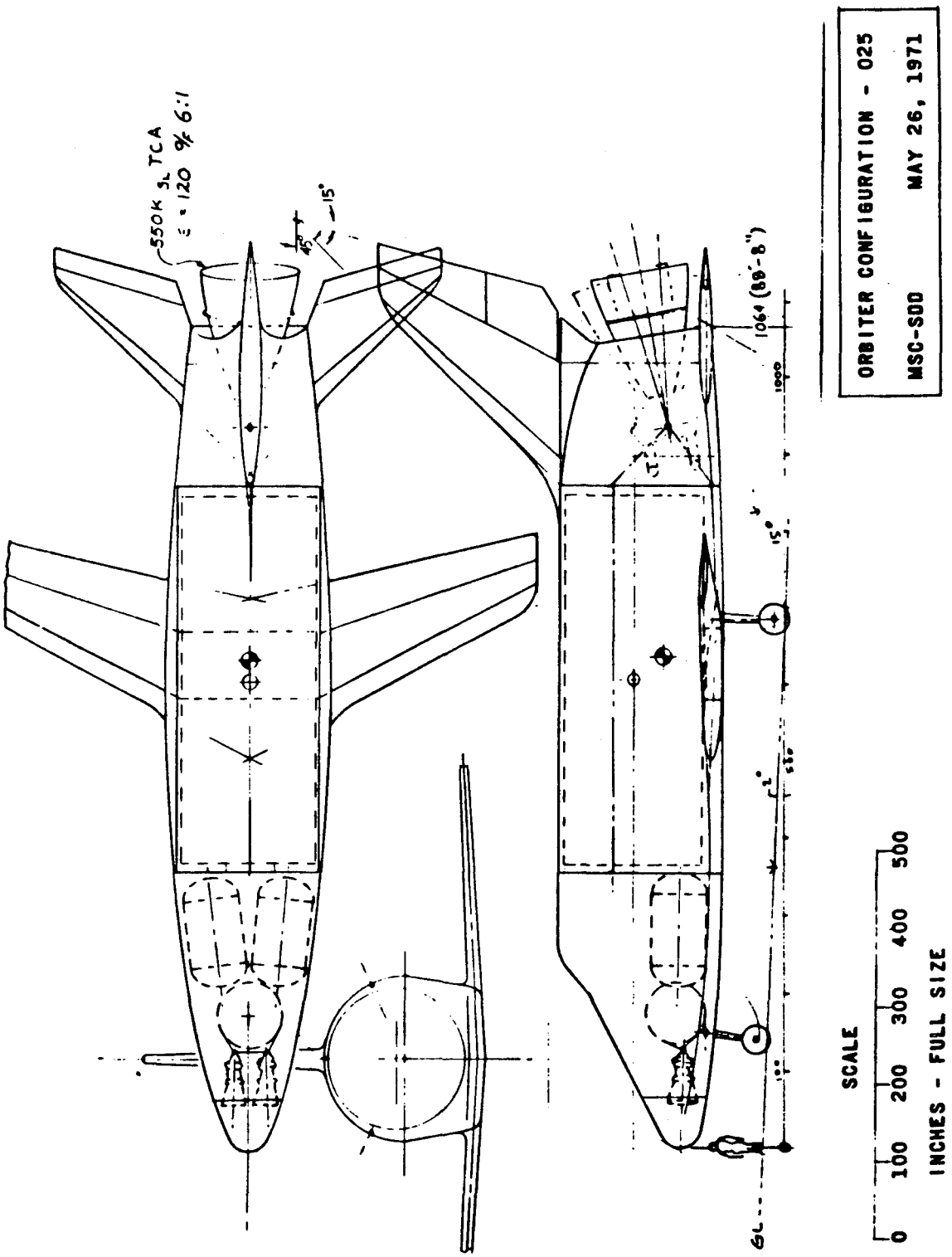

550K SL TCA
ε = 120 % 6:1
15°
15°
1064 (88'-8")
1000
ORBITER CONFIGURATION - 025
MSC-SDD MAY 26, 1971
SCALE
0 100 200 300 400 500
INCHES - FULL SIZE

Figure 52

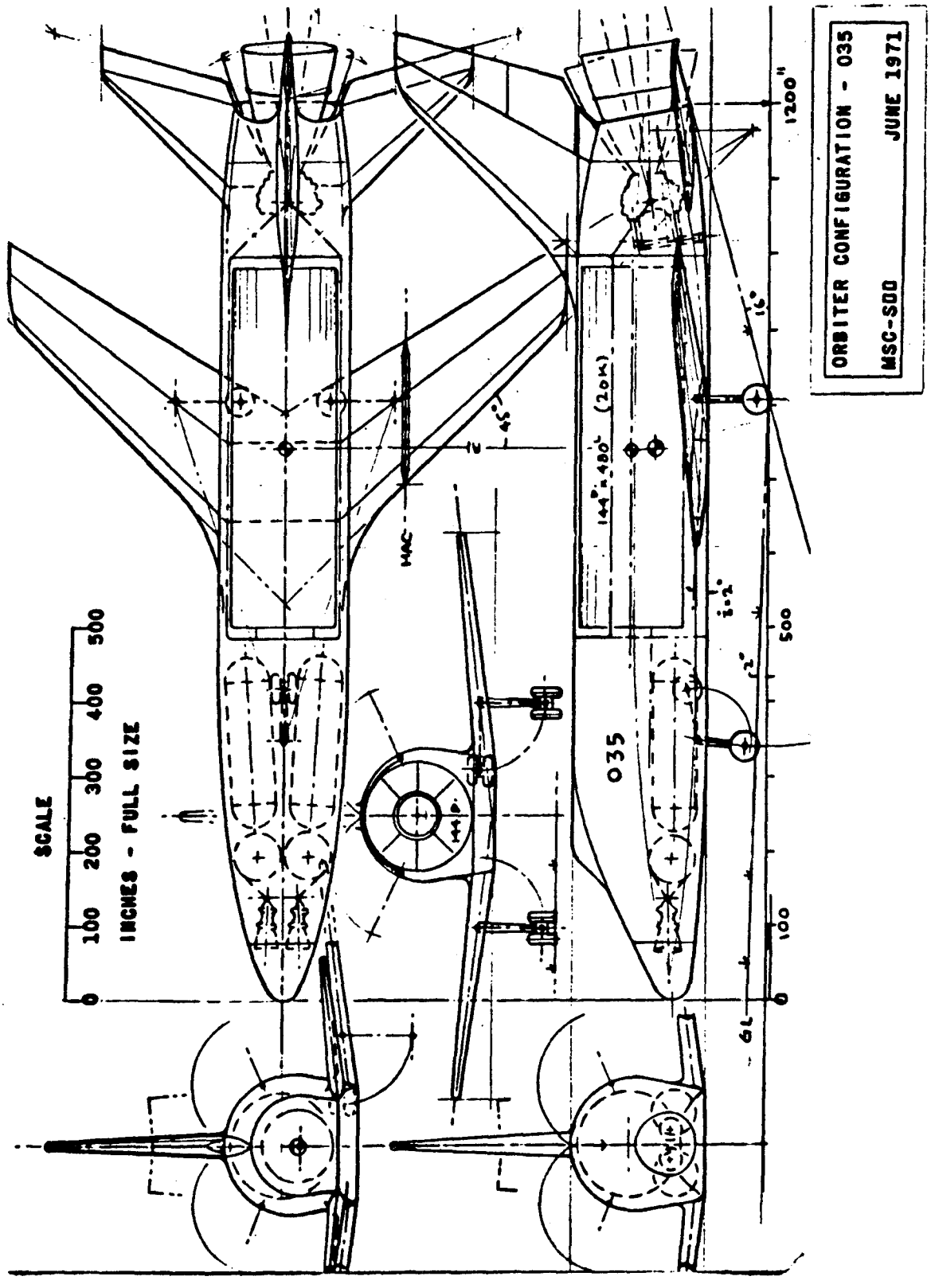

ORBITER CONFIGURATION - 035
MSC-500
JUNE 1971
SCALE
INCHES - FULL SIZE
0
100
200
300
400
500
035
MAC
45°
144"=480" (20M)
1200"
16°
500
100
0
61
2°
1.2°

Figure 53

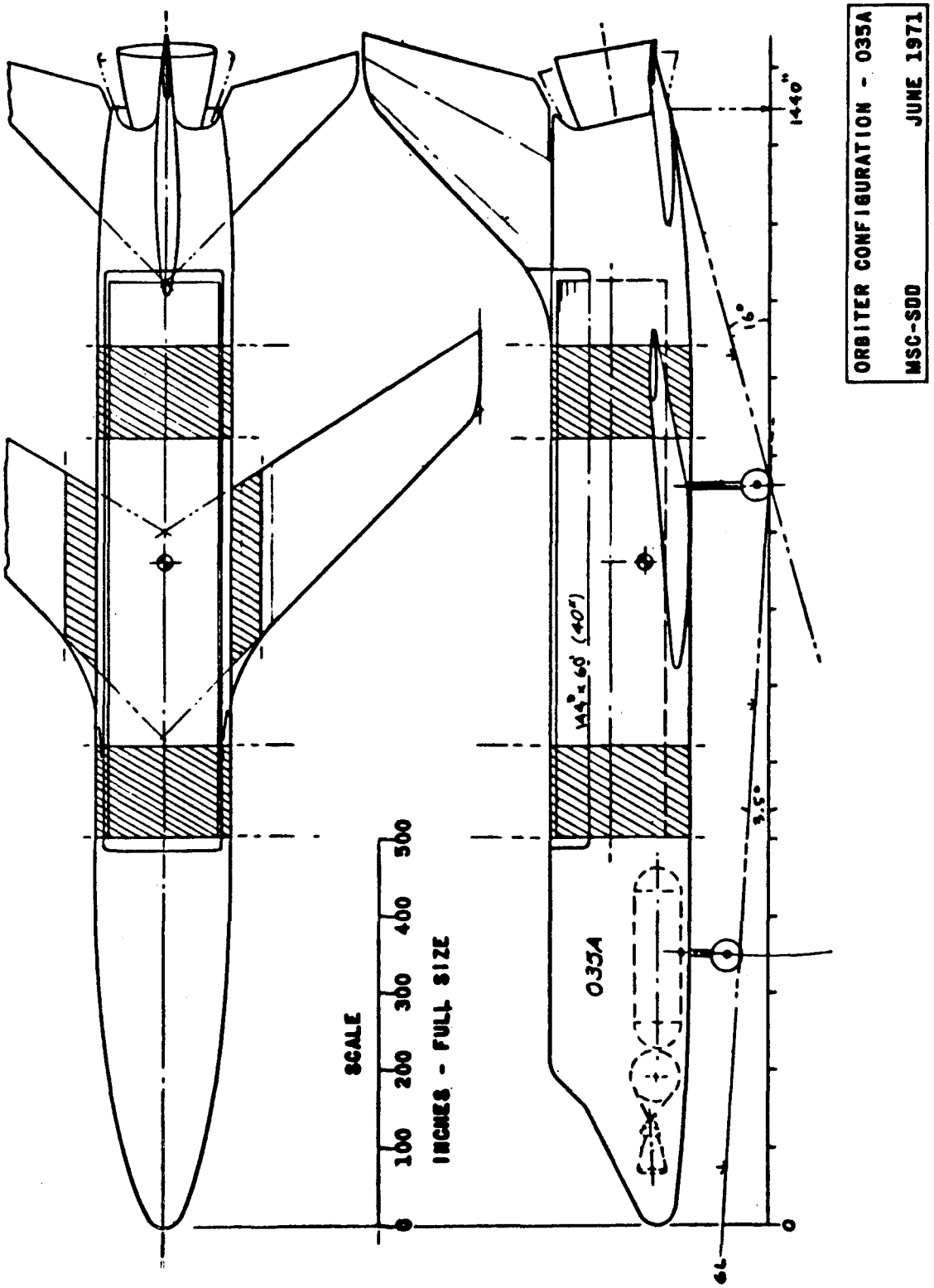

ORBITER CONFIGURATION - 035A
MSC-SDD JUNE 1971
1440"
16°
145° x 60' (40')
3.5°
0
0354
SCALE
INCHES - FULL SIZE
0
100
200
300
400
500

Figure 54

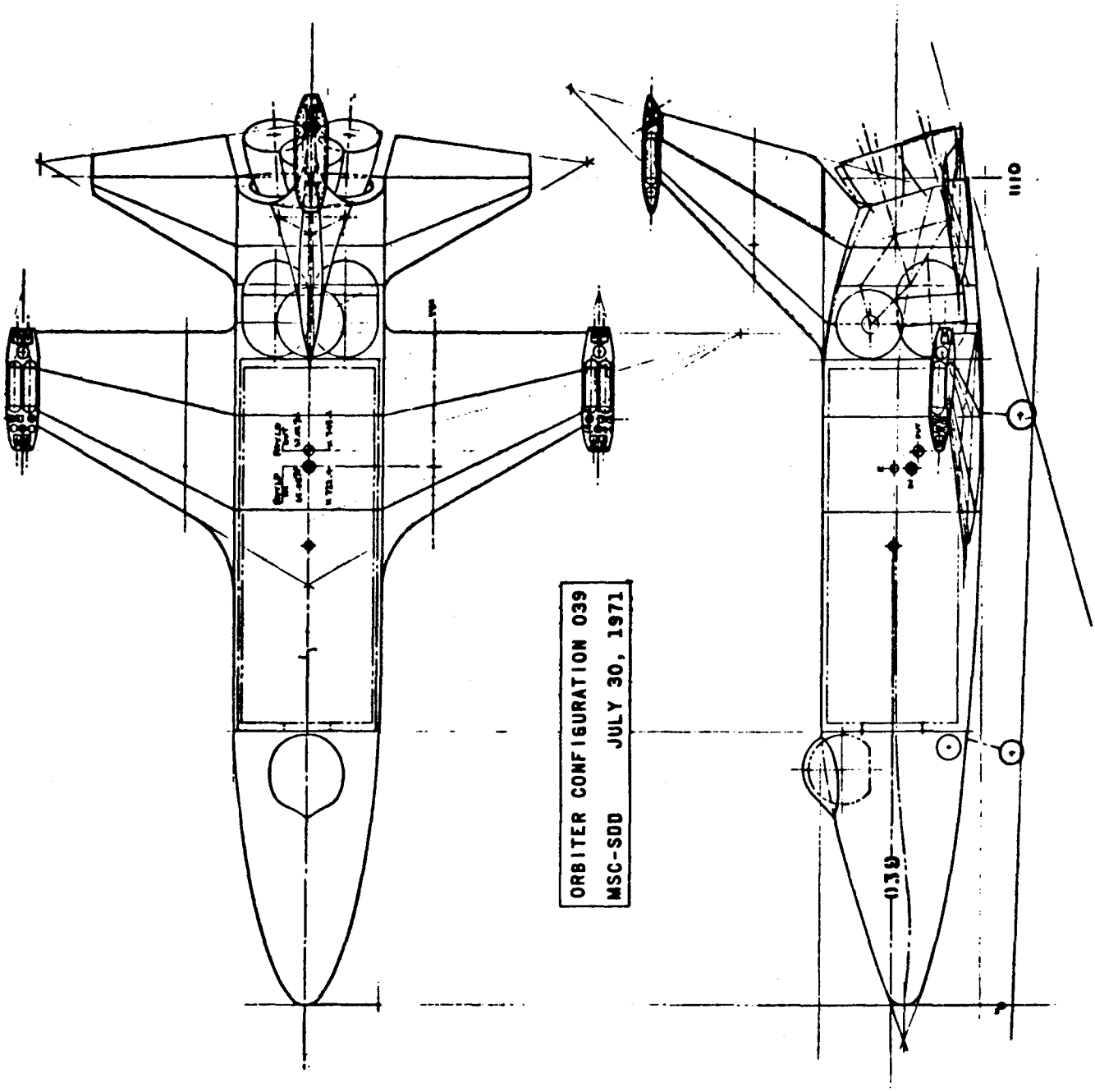
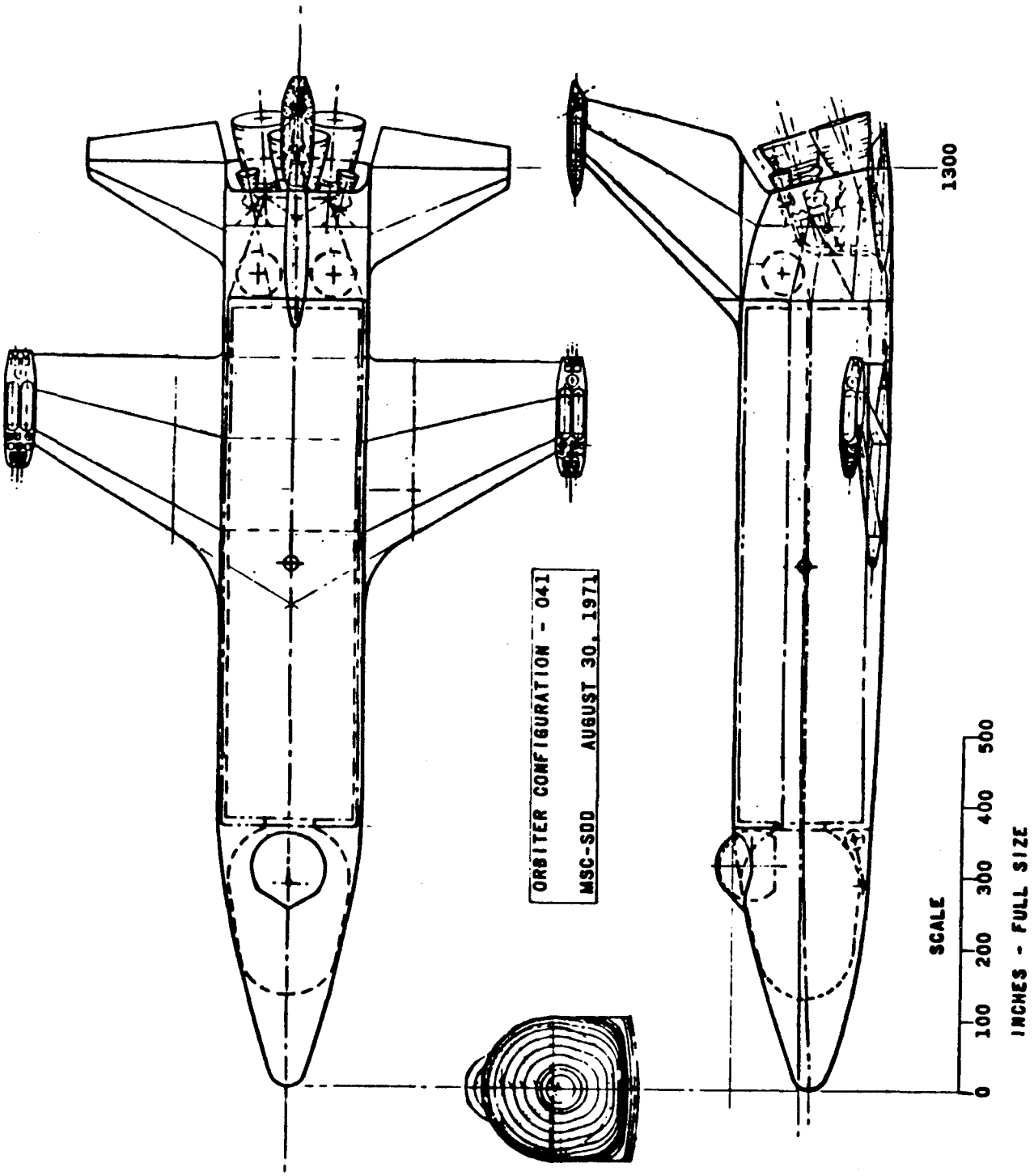


Figure 55


1300
ORBITER CONFIGURATION - 041
MSC-SDD AUGUST 30, 1971
SCALE
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INCHES - FULL SIZE

Figure 56

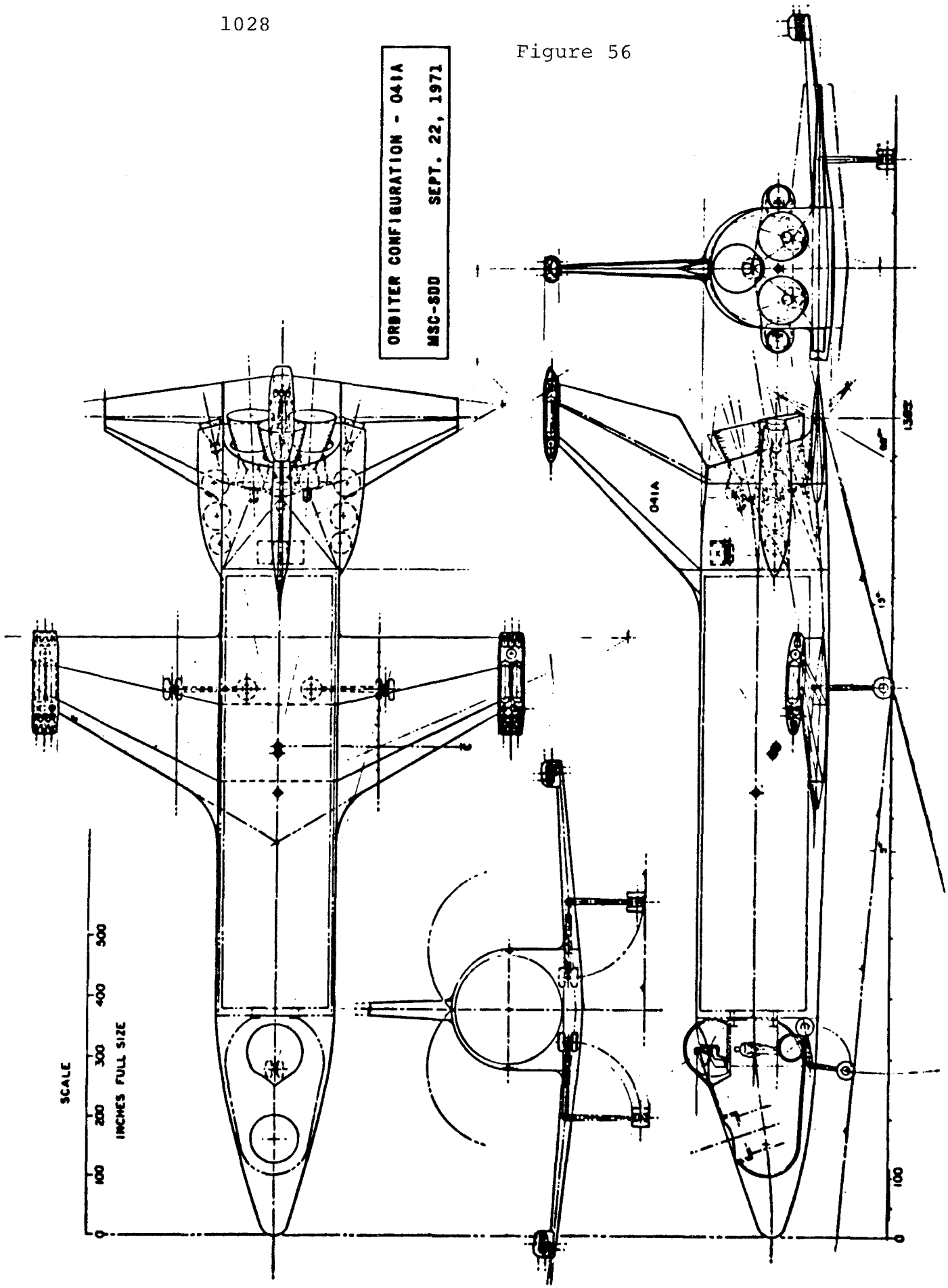
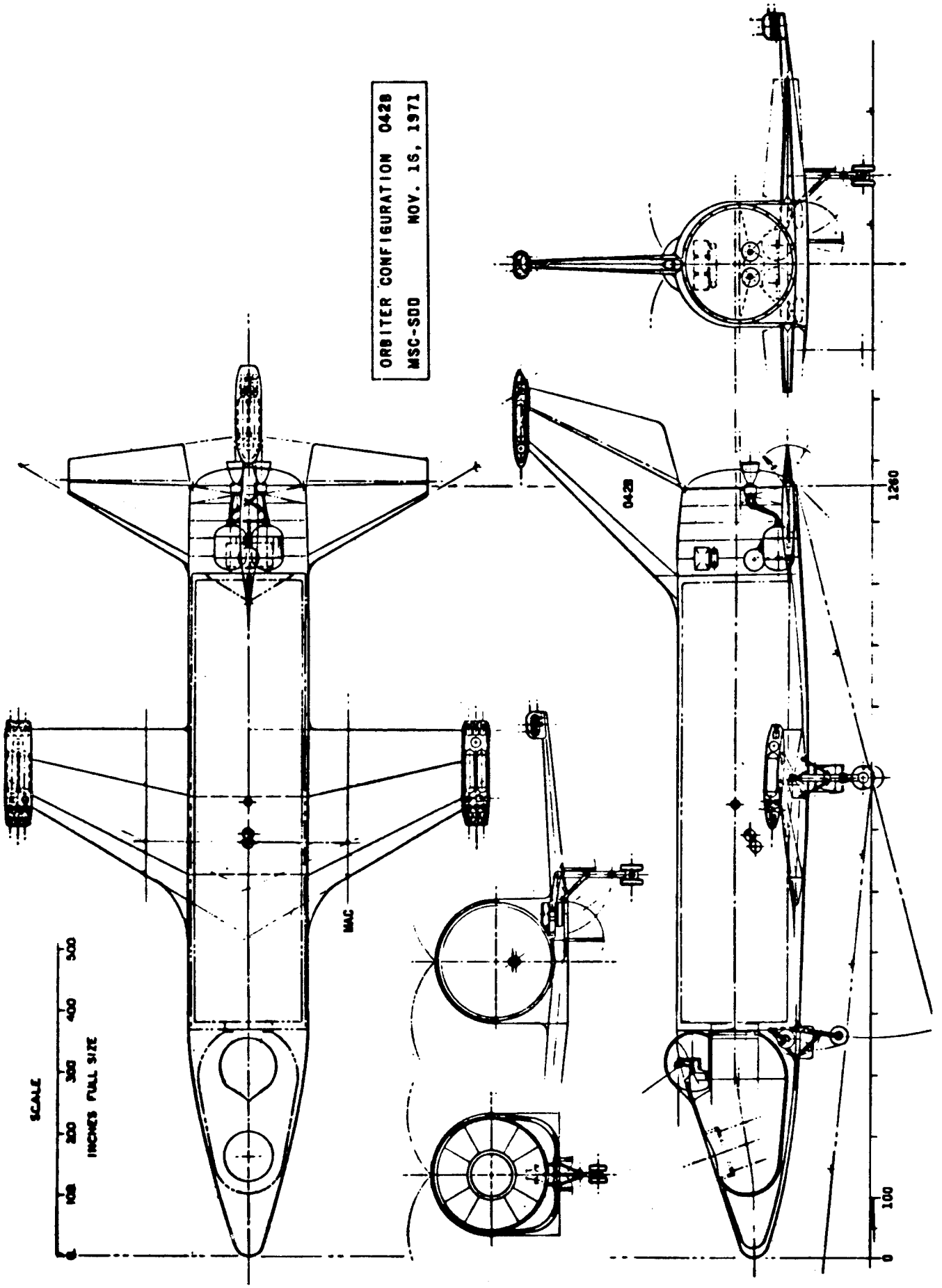

ORBITER CONFIGURATION - 041A
MSC-SDD SEPT. 22, 1971
041A
SCALE
INCHES FULL SIZE
0
100
200
300
400
500

Figure 57


ORBITER CONFIGURATION 042B
MSC-SDD NOV. 15, 1971
042B
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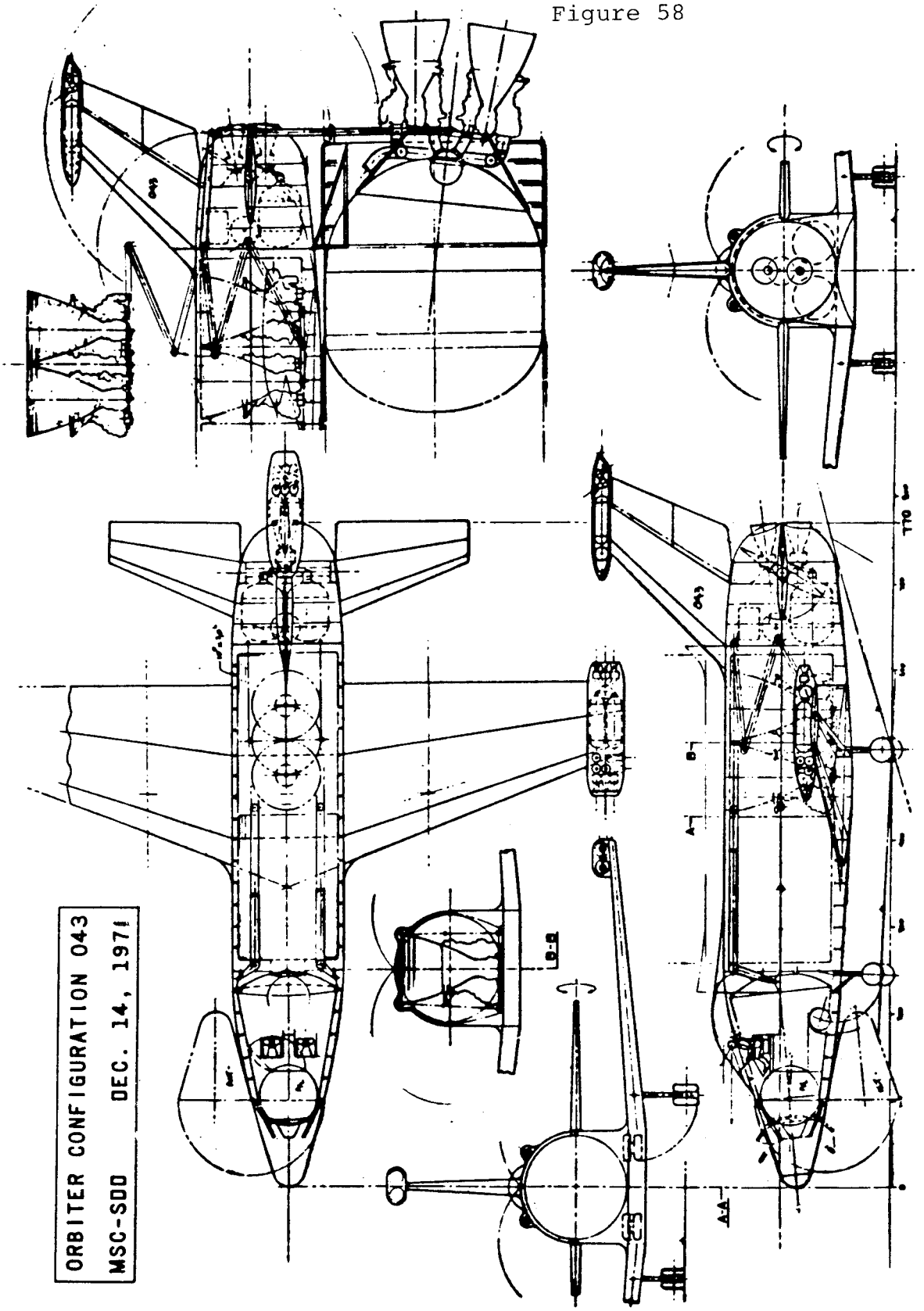
SCALE
INCHES FULL SIZE
500
400
300
200
100

Figure 58



whole families of orbiters and launch vehicles. Even after January 1971, when NASA ostensibly shifted its studies to deltas and increasingly concentrated on partially reusable systems consisting of an orbiter with external propulsion and propellants, the basic Faget vehicle remained a "contender." What is even more remarkable is that it remained a viable concept in some NASA eyes after the evolution of the so-called 040 baseline orbiter--a delta that eventually directly evolved into the Space Shuttle. Indeed, the last appearance of the Faget concept came at the very end of 1971, with the conceptualization of the 043 configuration (Figure 58).[16]

During the Phase A studies, North American-Rockwell and McDonnell-Douglas examined the Faget concept, NAR with a TSTO design using the Faget vehicle concept for both booster and orbiter, and McDonnell-Douglas with a HL-10 orbiter riding a Faget booster. Lockheed preferred its own "Star Clipper" stage-and-a-half design described earlier, and General Dynamics emphasized its own Triamese and "Biamese" concepts. (At the same time, Martin submitted an unsolicited study of its own for a complex vehicle called the Spacemaster consisting of an elegant lifting body orbiter nestled between two parallel boosters joined by stubby wing and tail surfaces, the joined boosters functioning as a single launch vehicle--somewhat akin to the layout of the old F-82 Twin Mustang--having the ability to return to earth with its own crew). Langley Research Center monitored the McDonnell studies, while Marshall Space Flight Center monitored the Lockheed and General Dynamics work. The Manned Spacecraft Center monitored North American-Rockwell's efforts. In addition, the Air Force's Space Division and the Aerospace Corporation also functioned in oversight roles examining NASA's activities and coordinating these studies with their own in-house study efforts.[17]

The Air Force and NASA had already teamed up on joint studies of the technology base for reusable aerospace vehicles; the

formation of the President's Space Task Group, formed by President Richard M. Nixon on February 13, 1969 and chaired by Vice President Spiro Agnew, led to even closer bonds between the service and NASA, as it involved senior-level participation by both NASA and Air Force representatives. In early 1969, the Flight Dynamics Laboratory became aware of the Faget design, and summarily dismissed it, issuing two in-house analytical reports in June and November of 1969 that pointed to its difficulties in reentering without experiencing structural failure of the wing by aerothermodynamic effects. The Flight Dynamics Laboratory followed this up with two AIAA papers--one presented in Houston--in October 1970 and January 1971 (by which time Shuttle was well into Phase B) that completely rejected the straight-wing approach and showed, in no uncertain terms, that the delta wing configuration would furnish an orbiter with better crossrange and safety characteristics. Within NASA, advocates of the Faget approach hung on, convinced that the Air Force did not appreciate NASA's expertise in low L/D approaches using an exceptionally high angle of attack--a critical point in safely operating the Faget design. Even after a visit by NASA engineers to Wright-Patterson AFB (home of the FDL), this feeling persisted within NASA's Spacecraft Design Division at the Houston Center. But when the Faget concept finally died, it died as much from internal NASA dissension as from external criticism by the Air Force. Specialists at NASA's Flight Research Center were uniformly skeptical, favoring a lifting body approach instead. Then, when Dale Myers, a former North American executive, joined NASA as Associate Administrator for Manned Space Flight, succeeding George Mueller, the axe fell. Already Michael Yarymovych, Assistant Secretary of the Air Force, had approached Mueller to state the Air Force's need for high-crossrange and large payload size and capacity, bluntly threatening not to support the Shuttle unless it met these needs. This alone (in Yarymovych's recollection) was sufficient to get NASA to abandon the Faget concept. However,

there is a different perspective that seems more plausible. In 1970, then-Air Force Secretary Robert Seamans and NASA Administrator Thomas O. Paine had established a joint USAF-NASA Shuttle coordination board, the so-called "STS Committee," co-chaired by Grant Hansen, the Assistant Secretary of the Air Force for R&D, and NASA's Dale Myers, who had succeeded Mueller in 1969. The committee anticipated that the Shuttle, designated the Space Transportation System (STS), would furnish the United States with an "economical capability for delivering payloads of men, equipment, supplies, and other spacecraft to and from space by reducing operating costs an order of magnitude below those of present systems." For its part, the STS Committee reviewed the program and recommended decisions ensuring that the STS met both NASA and DoD needs; the recommendations of the committee included such matters as "development and operational aspects, technology status and needs, resource considerations, and interagency relationships."[18] Myers had a strong background in aircraft design and development stemming from his years at North American. Hansen certainly was aware of FDL's position on the Faget design. Presumably, FDL's concerns were transmitted to Myers who, by inclination and his own familiarity with criticism of the design from within NASA, was predisposed to favor a delta wing approach. As LeRoy Day recalled in a 1983 interview with a NASA historian,[19]

> . . . for a long time we were still continuing to argue about this low crossrange, high crossrange, or the straight-wing versus the delta-wing configuration. And Dale was the one, I think, that really held the line and said, "No, we're not going to go for this straight-wing business. We're going to go for a delta-wing vehicle." . . . And by that time there was lots and lots of evidence from experimentation done within and outside of NASA that the straight-wing configuration had so many limitations that we really ought not to embark on that. We ought to build a hypersonic vehicle that was, in fact, a hypersonic vehicle, and that meant that it had to be a delta wing. [Emphasis added.]

That the Faget concept still had its advocates can be readily seen in the continuance of studies (see Figures 45-58) after

January 1971 when, ostensibly, NASA opted for a delta configuration as its prime Shuttle candidate.* As 1971 progressed, however, the chances for a Faget vehicle being built decreased dramatically, with the 043 configuration of December 1971 being its last gasp. A bold concept, it had been laid low by intrinsic flaws illuminated by Air Force and internal NASA studies.

While the Faget concept had taken roughly three years to blossom and fade--from 1968 to the end of 1971--other configurations had been explored that had shorter lives. Two of these are shown in Figure 59, which shows what might be called the "DC-9" approach to Shuttle design. This craft would have had two engines in its tailcone, a "de-orbit" engine located in the nose behind a swinging nose-cap, and either a relatively conventional tail location (Figure 59A) or a "T-tail" (Figure 59B, hence the "DC-9" analogy). This concept, dubbed the "Blue Goose" by NASA, appeared in the fall of 1970, and died almost as quickly. Besides having complicated heating problems, the configuration posed structural ones as well; NASA engineers at first planned variable-geometry canards at the nose, but subsequently abandoned them in favor of having a sliding mechanism built into the craft so that the entire wing could be translated twelve feet fore and aft; the design team envisioned only moving the wing slowly, and in orbit "so no great horsepower is required." Claims that such a sliding wing-root arrangement would involve "little structural penalty," however, seem to deny historical reality, given the experience of

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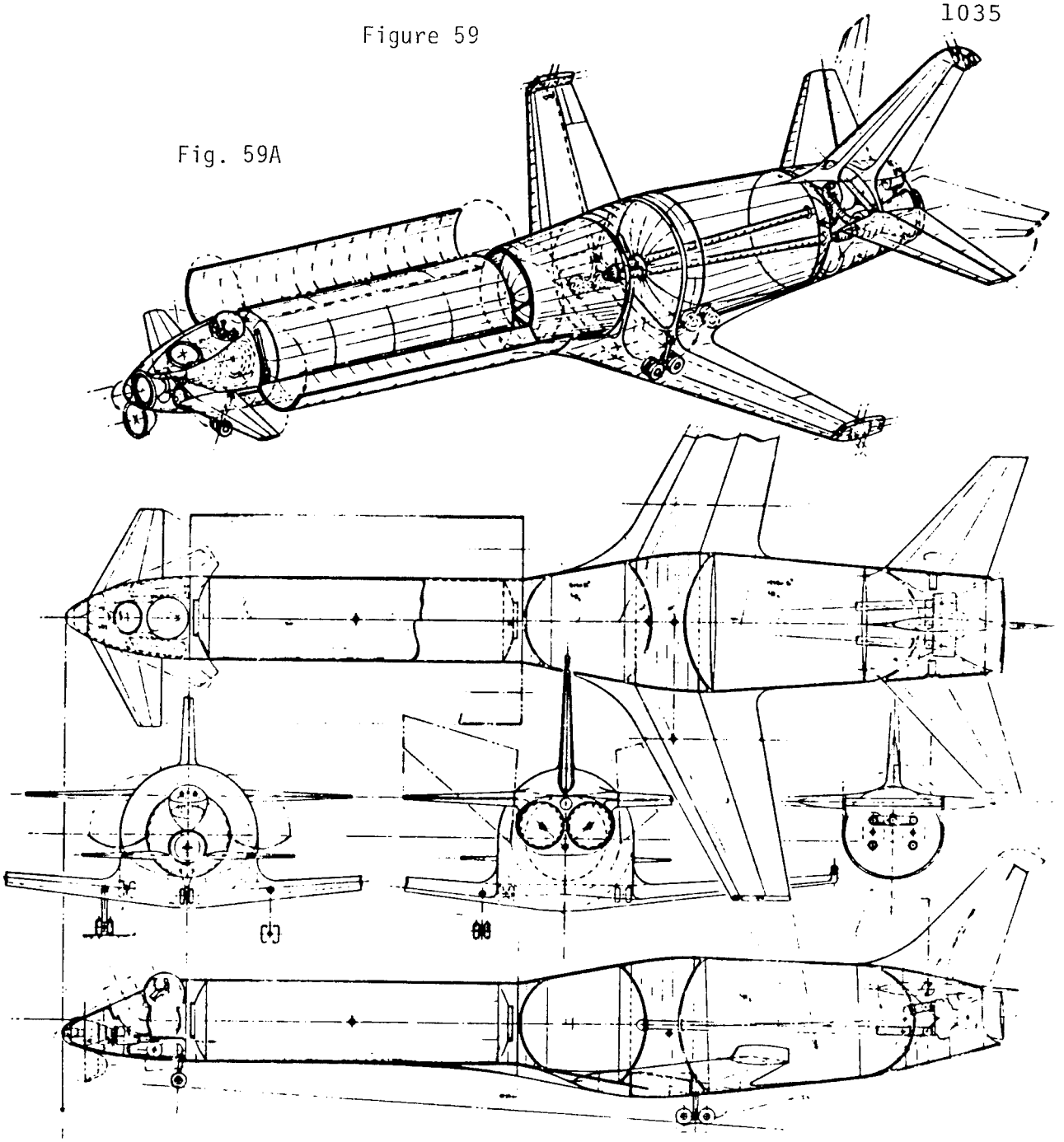
*Faget himself, however, eventually accepted the delta, recognizing its good c.g. properties compared to a straight-wing layout; his next contribution to Shuttle configuration evolution came with his insistence that Shuttle sacrifice elegance of line in favor of a volume-and-payload benefitting "boxy" appearance. His views greatly influenced the development of later orbiter designs (such as the 040) that had a relatively slab-sided appearance.

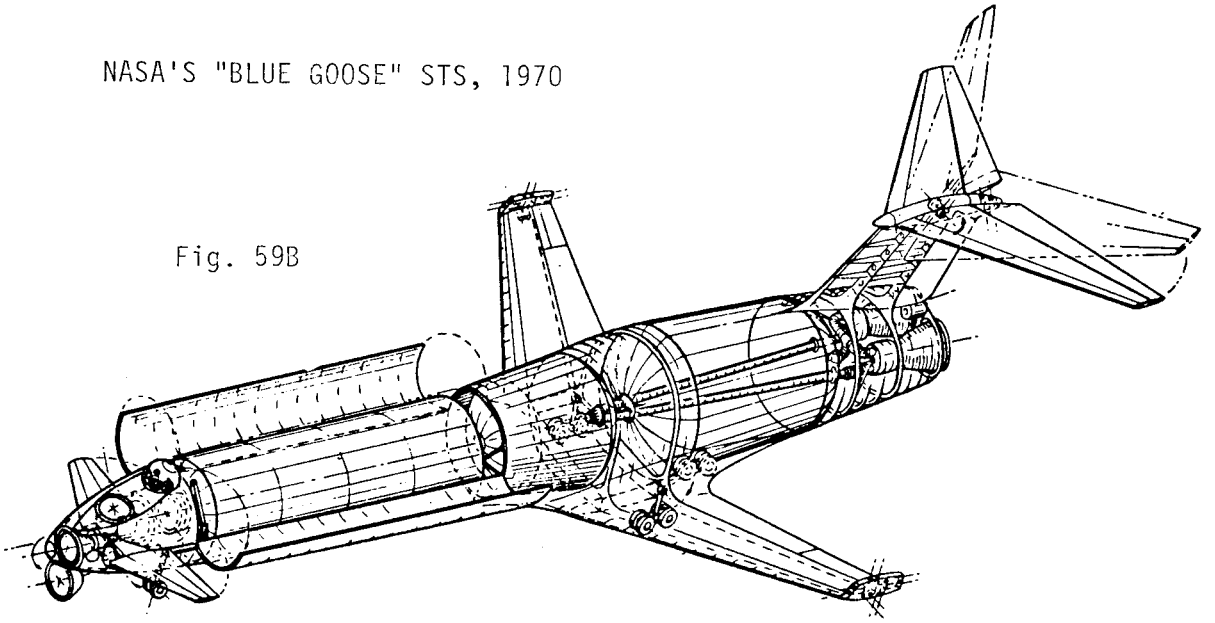
Figure 59

Fig. 59A



NASA'S "BLUE GOOSE" STS, 1970

Fig. 59B



two such earlier efforts involving aircraft: the Bell X-5 (the world's first variable-sweep airplane), and the Grumman XF10F-1 Jaguar (the first attempt to apply variable wing sweeping to an operational military airplane). After some minor flings with bizarre configurations such as the ill-fated Blue Goose, the Spacecraft Design Division turned its attention to the only really logical approach--a delta configuration.[20]

Before exploring the range of delta orbiters derived by the Spacecraft Design Division, it is prudent to examine the state of Shuttle development in the 1969-1971 time period. Phase A studies, begun in February 1969, had resulted in Phase B proposals in June 1970; the following month NASA awarded Phase B follow-on contracts to McDonnell-Douglas and North American-Rockwell. At the time of the Phase B study award, the agency and contractors were generally unanimous in considering the design of large, two stage fully reusable craft, with fly-back piloted boosters and orbiters that carried both their payload and fuel internally. As a hedge, at the same time that NASA awarded the Phase B contracts, it also awarded two additional Phase A studies--to a Grumman-Boeing team, and to Lockheed for examination of partially expendable systems as alternatives to the more elaborate Phase B studies then undergoing evaluation. From 1970 into 1971, however, some major changes were taking place that would greatly affect the ultimate design of the Shuttle.

NASA and the Space Shuttle: Redirection to Flight Test

Figure 60 shows the state of Shuttle developmental activity from the issuance of the original RFP leading to Phase A through the selection of North American-Rockwell in 1972 for Phase C/D Shuttle development. "Big G," the proposed derivative of a Gemini vehicle to carry up to 12 astronauts to an orbiting space station, had faded in mid-1969, replaced by the notion of the ILRV. Big G died in great measure because it was a personnel carrier, and not

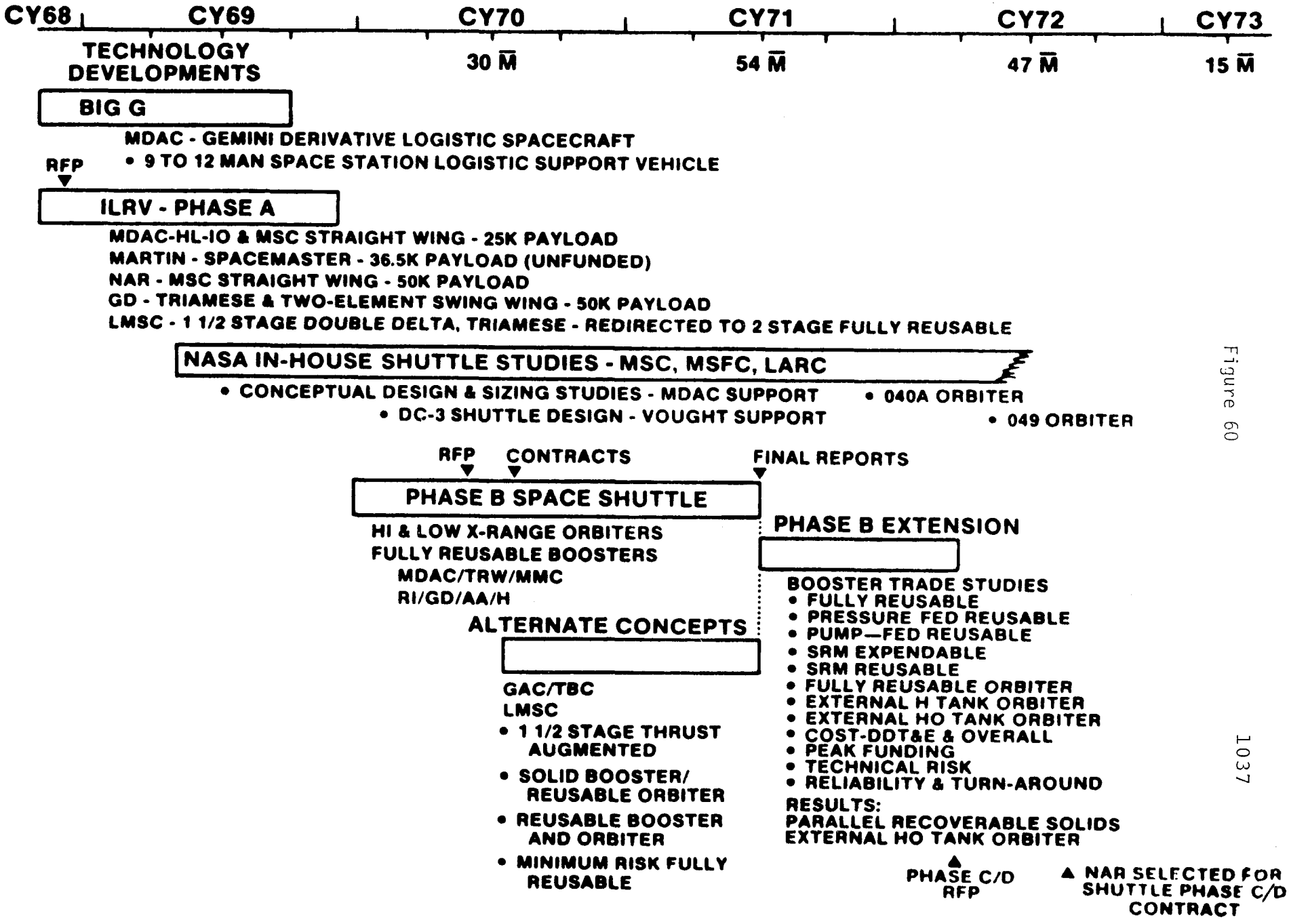


Figure 60

SHUTTLE DEVELOPMENTAL ACTIVITY, 1969-1972

suitable as a high volume-high payload logistical transport. Phase B followed Phase A, and eventually Phase B itself underwent an extension called B' (B Prime) and B" (B Double Prime) before the awarding of the Phase C/D contract to North American-Rockwell in 1972. All this while, NASA continued in-house Shuttle studies using engineering design support from the Manned Spacecraft Center, Marshall Space Flight Center, and Langley Research Centers. NASA had established the so-called "lead center" concept to administer the growing Shuttle effort; essentially this placed the Manned Spacecraft Center (later renamed the Lyndon B. Johnson Space Center) as the overall integrator of other NASA Shuttle-related activities. The choice was a natural one, since MSC assumed responsibility for the Shuttle orbiter. Other centers took on tasks appropriate to their expertise; for example, the Flight Research Center (later the Dryden Flight Research Center and, subsequently, the Dryden Flight Research Facility) had responsibility for planning the initial approach and landing tests, while the Marshall Space Flight Center controlled booster development. The notion of running Phase A and Phase B contractor studies at the same time that the agency pursued its own in-house configuration design efforts might appear an unnecessary duplication; NASA's rationale for continuing in-house studies, however, was that such an effort helped improve the technical competency and expertise of its own staff, placing them in a better position to review and monitor the activities of the contractors. As a rule, NASA did not apply results directly into the contractor's efforts, primarily because the agencies considered them more of an internal learning curve. As Milton Silveira later recalled to historians Joe Guilmartin and John Mauer in 1984,[21]

> We really trained our people on what the Shuttle was all about by doing these in-house baseline designs. And, as a result, we came to appreciate some of the problems a whole lot better, so that when we worked with a contractor, we had a better feel for what had to be done and what [the contractor] should be doing for us.

Manned Spaceflight Center's engineers dubbed these on-going in-house Shuttle conceptualizations "DC-3 studies," a reference to the Douglas airliner of 1935 which revolutionized air transportation and made commercial aviation both practicable and profitable. George Low, a NACA veteran and former director of the Apollo spacecraft design effort, had become Deputy Administrator of the NASA at the end of 1969, and as NASA's in-house and Phase A and B studies progressed he increasingly injected a note of caution into such efforts, emphasizing that while fully reusable systems might be economical to operate, their associated technical risks, the large number of parallel efforts requiring pursuit, and their associated large development costs all mitigated in favor of smaller partially reusable systems. The strong external pressures mitigated against development of fully reusable systems as well, pressures that had much more to do with economic realities than with technical uncertainty and risk. The national climate following the first lunar landing, and the associated Congressional response to that climate after Apollo 11's triumphant journey, dictated a turning away from the ambitious but ill-defined plans that NASA had for a post-Apollo space station and a possible Mars mission. In 1970, the agency recognized that it could not have both the station and the Shuttle, and accordingly, shelved plans for the former. The Shuttle, a more modest, attainable, attractive, and--most importantly--more defendable concept, survived, though its major justification shifted from space station logistical support to use as a more economical substitute for "throwaway" launch systems such as the Delta booster (the latter justification soon proving a chimera). The next year, 1971, the Office of Management and Budget (OMB) expressed its unwillingness to support NASA at budget levels above the space agency's 1971 annual figure of $3.2 billion. This placed Shuttle in serious jeopardy; program costs for a fully reusable TSTO Shuttle were already rising to an estimated $12 billion. Though NASA planners still favored the fully

reusable approach, economic considerations, supported by outside analysis, clearly dictated otherwise. NASA now had to adopt a partially expendable booster system. That the Shuttle survived both the restructuring of the national space program and the secondary economic barrage from OMB was due, in no small measure, to support from the Department of Defense, in particularly the United States Air Force. This support played a profound role in shaping the configuration and capabilities of the Space Shuttle.[22]

Science and technology policy analyst Scott Pace has identified five key issues affecting the ultimate design of the Shuttle, including:

- determining the capacity and dimensions of the payload bay
- determining an optimum crossrange
- choosing a TAOS (Thrust-Augmented Orbiter Shuttle) design
- deleting plans to incorporate air-breathing "landing" engines
- selecting aluminum as a primary structural material

The first two were ones for which Air Force input was critical, and the technical fall-out from these influenced, together with other external factors, the other three as well. Although it is popular to describe the United States in the 1960s and early 1970s as having two sharply delineated space programs, one civilian and one military, there were, of course, numerous areas of mutual interest, as the activities of the AACB and the formation of the STS Committee attest. Further, from the outset, Day's Space Shuttle Task Group had included Air Force/DoD representation, resulting in a series of classified documents on future DoD/NASA relations on the Shuttle. The Air Force had virtually no money to put into Shuttle development, since in the early 1970s the service

found itself in the process of force-restructuring: replacing the initial generation of supersonic jet fighters and strike aircraft with newer-technology aircraft such as the A-10, F-15, F-16, and B-1. Nevertheless, the service was committed to operating the Shuttle on military missions including polar launches from Vandenberg AFB, California, and developing a Space Tug (subsequently abandoned) to place DoD payloads in higher orbits than could be achieved by the STS itself. In brief, the Air Force was willing to support the Shuttle in Congressional hearings provided it had utility for the defense community. For its part, NASA considered such support vital if the STS were to withstand the attacks of Congressional critics from both parties questioning the space program's need and rationale (one such critic, New York Congressman Ed Koch--subsequently the mayor of New York City--swiped at space exploration, arguing "I just for the life of me can't see voting for monies to find out whether or not there is some microbe on Mars, when in fact I know there are rats in the Harlem apartments.") NASA, interestingly, was not concerned with the Air Force's lack of money to support Shuttle development; having just NASA responsible for funding simplified the budgetary approval process in Congress, and eliminated the danger of having a program dependent on <u>two</u> sources of funding with <u>double</u> the danger of budget cuts. (As the 1970s progressed, however, Congressional opposition to space in general and Shuttle in particular gradually withered, freeing NASA to proceed with Shuttle development unimpeded by significant Congressional opposition). Air Force support for the STS hinged on two key areas: payload capacity and bay size, and the crossrange of the orbiter. Already the Air Force--largely through studies by the Aerospace Corporation--had established particular criteria for these including:

- a payload bay 60 ft. long and 15 ft. in diameter
- a payload capacity of 65,000 lbs. into

a due east 100 n.m. orbit and 40,000 lbs. into polar orbit

- a crossrange of 1500 n.m. (later reduced to 1100 n.m.)

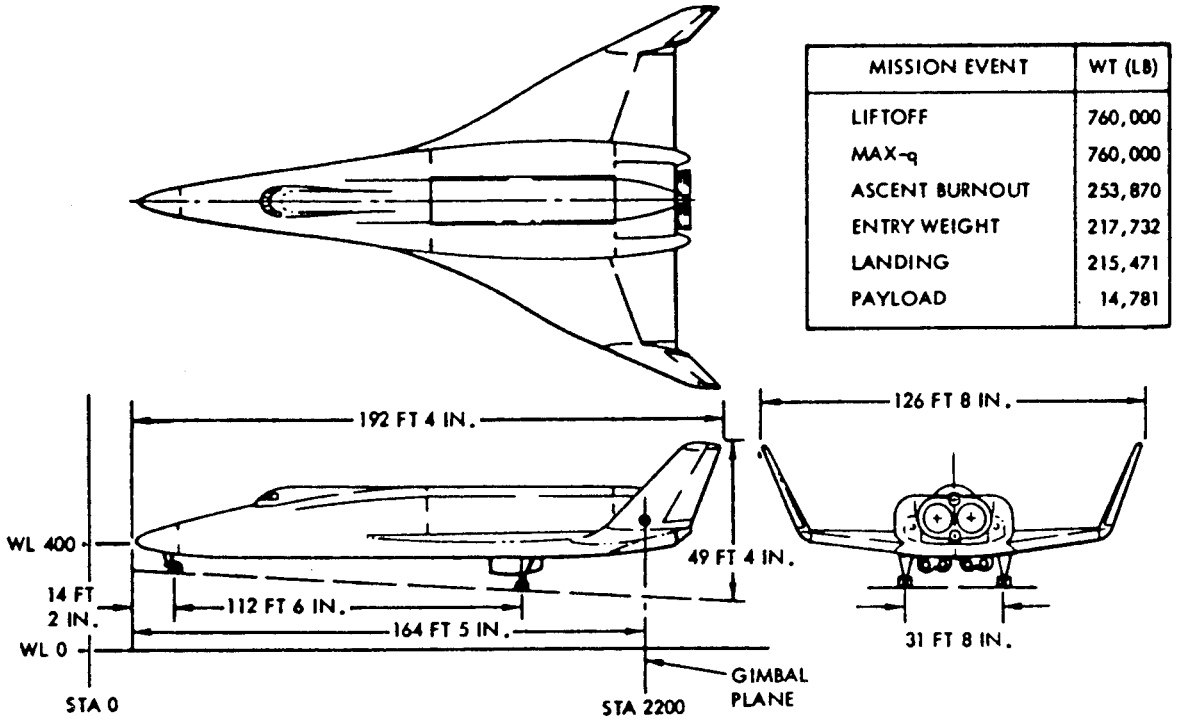
The first two were required by the size and weight of planned DoD operational systems. The latter was a necessary safety concern for a single-orbit flight returning to Vandenberg after placing a satellite into polar orbit, since the earth would have rotated sufficiently to require that the orbiter have a high crossrange so that it could adjust its course to reach the California base. In a major STS meeting held between NASA, the Air Force, and industry representatives on January 19-20, 1971, all of these Air Force requirements--the 15 x 60 ft. payload bay, the 65,000 lbs. easterly and 40,000 lb. polar payloads, and the 1,100 n.m. crossrange--were all adopted as baseline design decisions, a major point. (Subsequently, the Air Force contributed another significant input to NASA on the Shuttle--that of payload bay structural criteria. NASA had originally wanted a 9_g crash load capability for safety purposes. Using studies by the Aeronautical Systems Division, however, NASA revised its crash load criteria to a lower and more realistic level, based on Shuttle's anticipated 3.3_g ascent load factor, a switch that eased greatly the task of payload developers.) The acceptance of these dictated another major point--adaptation of the delta wing planform, for which the Air Force had argued strenuously. In particular, the demand for a Vandenberg return following a polar orbit payload insertion had killed the last remaining chance that the conventional-wing orbiter might have had, even if its doubtful ability to survive the anticipated thermal environment could have been resolved. As LeRoy Day subsequently recalled, "If you were making a polar-type launch out of Vandenberg, and you had [Max Faget's] straight-wing vehicle, there was no place you could go. You'd be in the water when you came back." As has been mentioned, the NASA Spacecraft Design Division continued to doodle straight-wing orbiters through

December of 1971. But with acceptance of the above mission requirements and recognition of the deficiencies of the straight-wing approach, the straight-wing was on the skids, and the delta was firmly in the minds of program planners.[23]

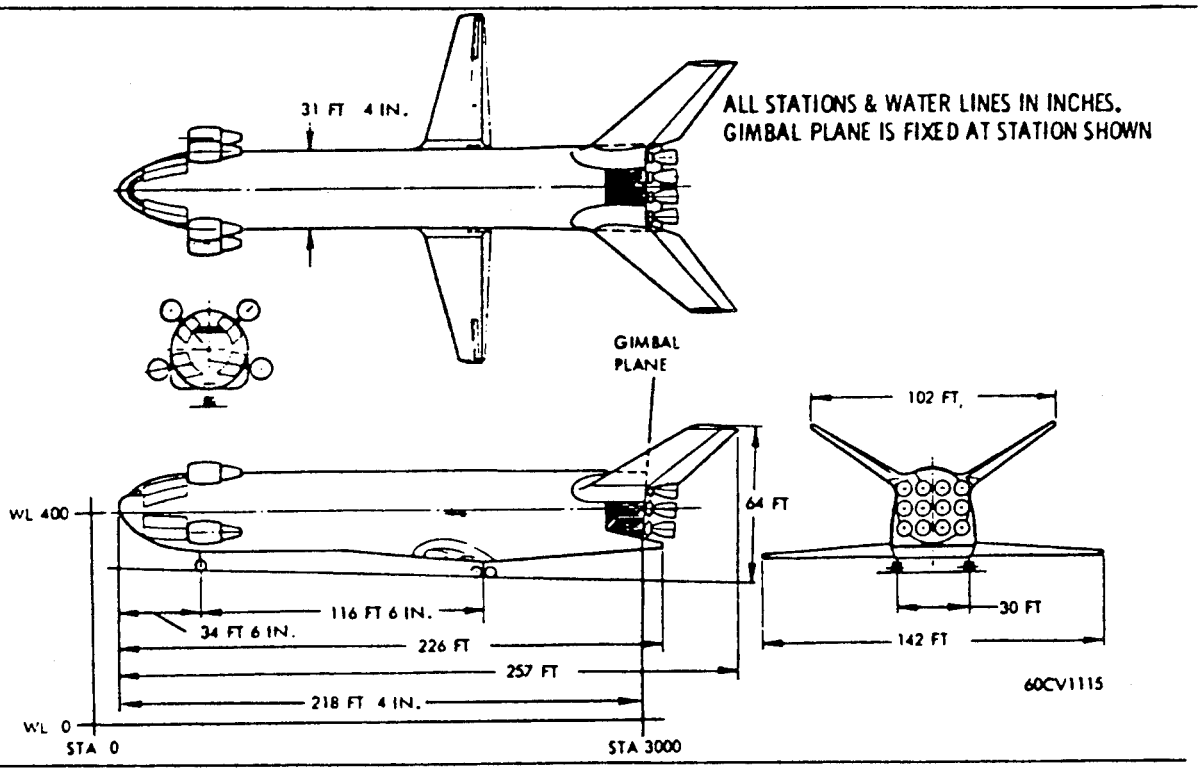
The Phase B studies had resulted in some truly enormous two-stage vehicles designed to meet NASA's initial preference for full reusability. Figures 61-64 show some of these studies. Figure 61 depicts a North American-Rockwell high-cross-range TSTO concept, with an elegant orbiter--a characteristic of North American designs at this time--exhibiting a high degree of wing-body blending, joined to a modified Faget-like booster with a butterfly tail. The payload was far too small to warrant developing such a complex system. (At the same briefing, North American-Rockwell also discussed a 200 n.m. crossrange straight-wing orbiter of Faget configuration; somewhat offsetting its poor crossrange was its nearly 40,000 lb. payload capacity). This North American-Rockwell concept, presented at a 1970 MSC Shuttle "Baseline" briefing, typifies the large size and "flying fuel tank" nature of the fully reusable designs. Figure 62 shows a Phase B McDonnell-Douglas concept briefed at the MSC on February 1, 1971; a delta orbiter is mated to an awkward-looking canard booster. Figure 63 shows a refined Rockwell concept briefed at the same meeting, consisting of a curvaceous-looking orbiter joined to a somewhat angular canard booster having a delta-wing planform reminiscent of the Convair B-58 or F-102. Figure 64 shows yet another view of the latter concept, with some slight dimensional differences. All of these concepts would have been expensive, fraught with major development risks (even more so than the Shuttle actually developed), and would have taken many years to develop. Time, money, and a willingness to accept the kind of risks inherent in the development of these craft were matters NASA had little patience for in the early 1970s, given the realities of Federal funding and the perceived need to acquire a

Figure 61

ORBITER CONFIGURATION
(1500 N MI CROSS RANGE)



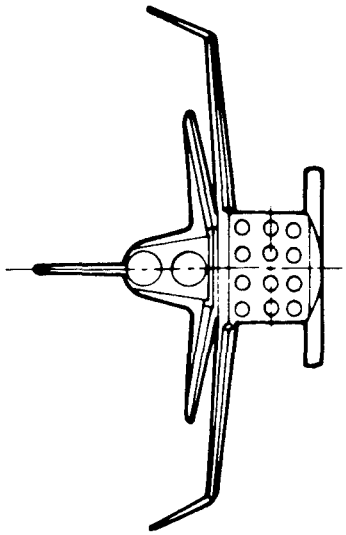
BOOSTER CONFIGURATION

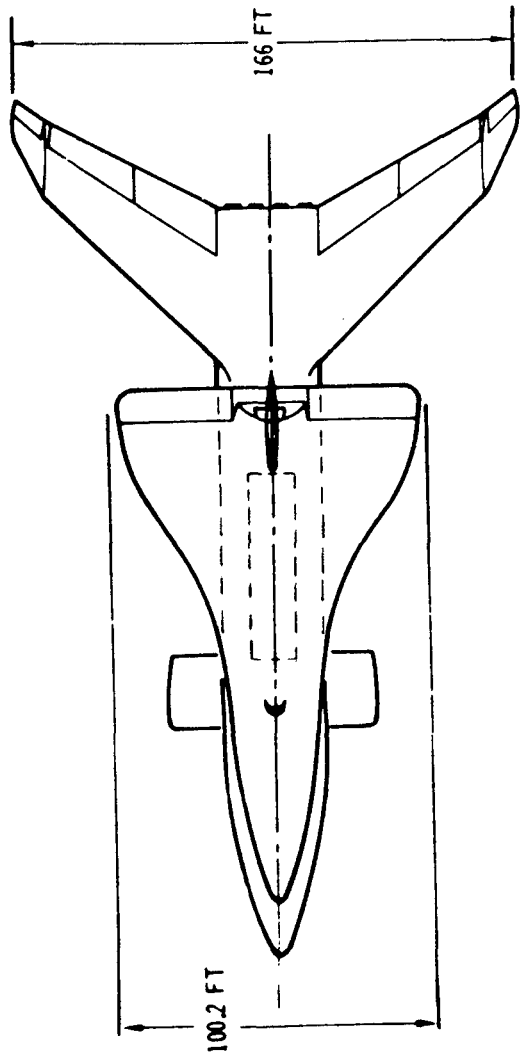


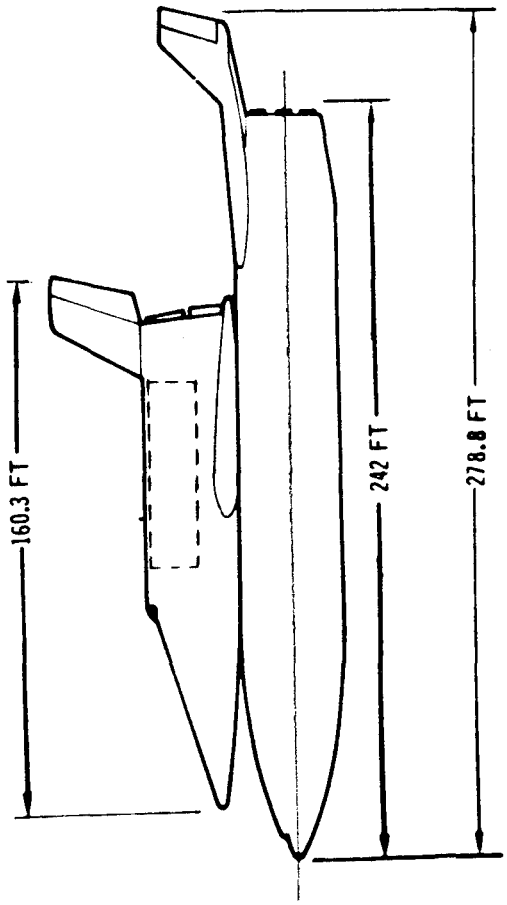
ROCKWELL TSTO CONCEPT, 1970 MSC SHUTTLE BASELINE BRIEFING

- 25 K LB PAYLOAD TO REF. ORBIT
- 40 K LB TO SOUTH POLAR, WITHOUT ABES
- 4.60M LB GLOW
- JP AIR BREATHING ENGINES
- 520 K LB MAIN ENGINES

	ORBITER	BOOSTER
DRY WEIGHT (LB)	226,400	511,000
LANDING WEIGHT (LB)	264,700	533,000
GROSS WEIGHT (LB)	836,000	3,764,000
USABLE ASC. PROP. (LB)	524,000	3,064,000
NO. MAIN ENGINES	2	12
CRUISE/LDG. PROP (LB)	4,000	148,000
NO. A.B. ENGINES	4	10

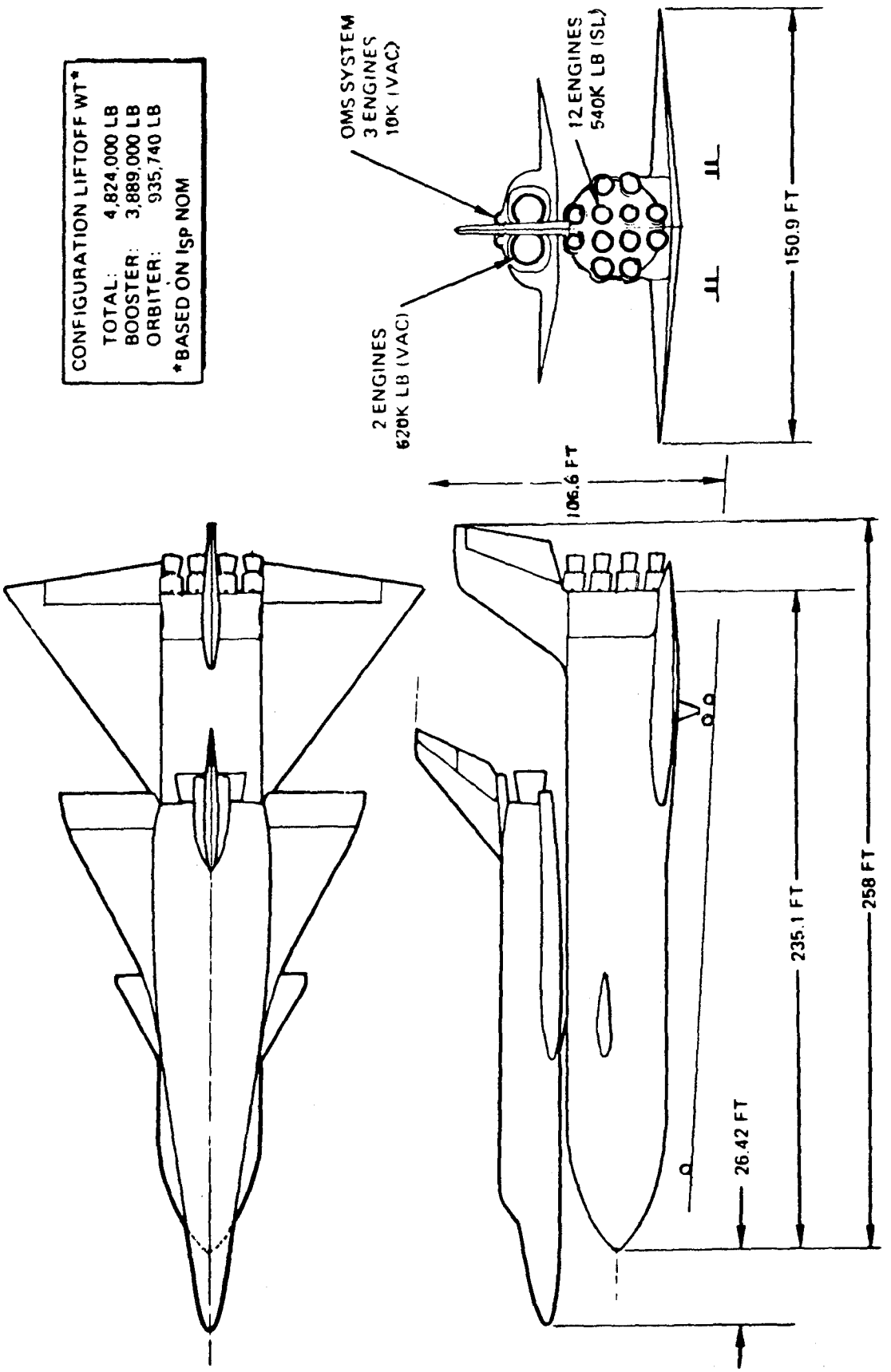






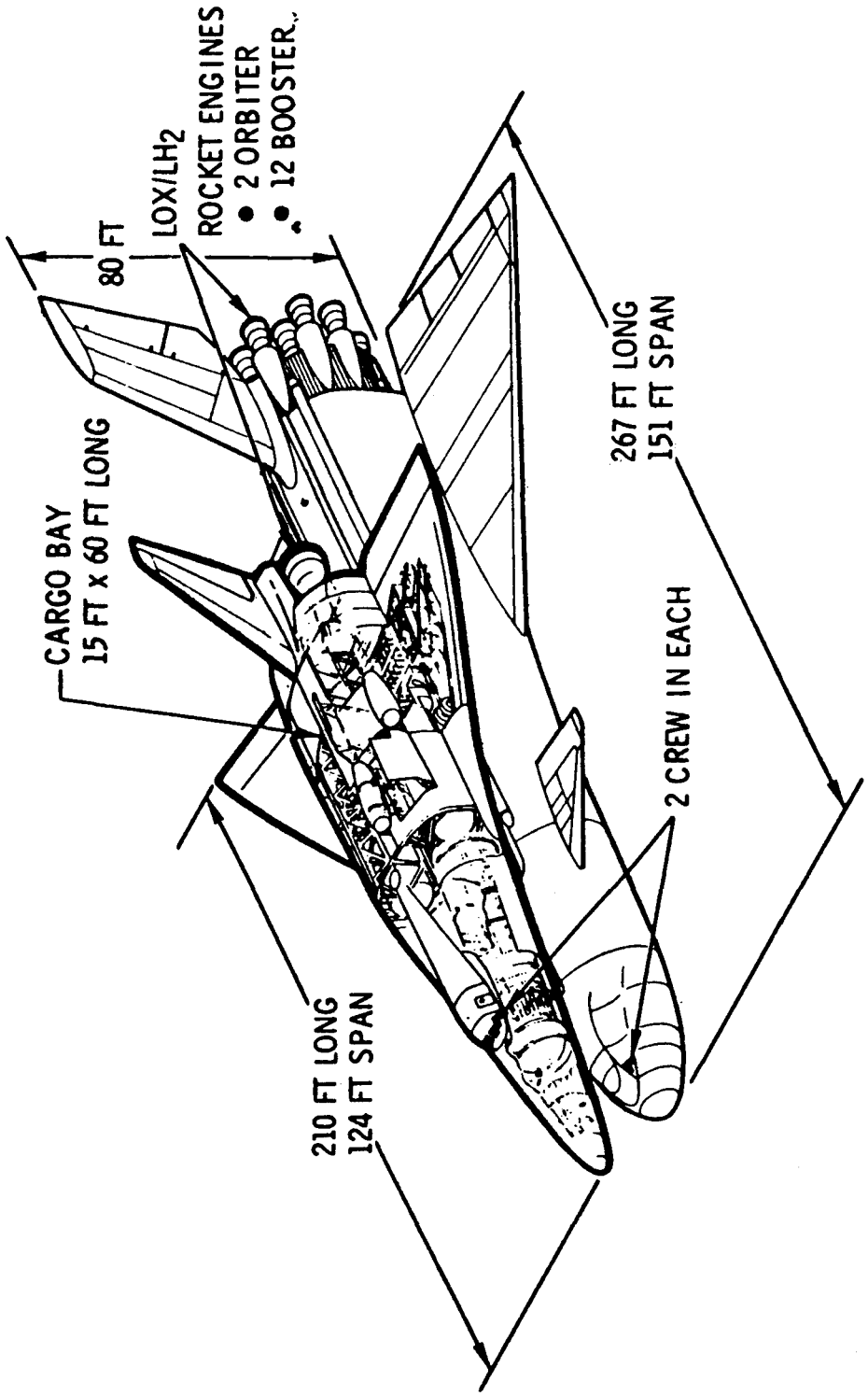
McDONNELL-DOUGLAS TSTO BRIEFED AT MSC, FEBRUARY 1, 1971

Figure 63



ROCKWELL TSTO BRIEFED AT MSC, FEB. 1, 1971

Figure 64



SCHEMATIC VIEW OF THE ROCKWELL TSTO CONCEPT, 1971

system as soon as practicable. Accordingly, NASA Shuttle managers increasingly paid greater attention to criticism of the fully reusable concept from within and outside the agency, and watched closely the Phase A study then underway for a joint Grumman-Boeing configuration, the H-33, that made use of an orbiter carrying some of its propellants externally, (and, eventually, using a modified Saturn booster) as well as a series of NASA MSC in-house studies that examined various orbiter concepts joined to external tankage. Once OMB stepped on NASA's funding hose, the trickle of money coming through made debate over fully vs. partially reusable a moot point: NASA would have a partially reusable Shuttle or none at all. Seen in this light, the main value of the Phase B studies lay less in their refining an ultimate configuration, and more in their giving two contractors an opportunity to do intensive "real-world" oriented studies. So, as 1971 progressed, the ultimate configuration of the Shuttle and its throwaway components still remained open to question.

The major question, of course, involved the generalized delta configuration of the orbiter vehicle, and the degree to which the orbiter would carry its own propellants. Configurations could have ranged from shapes reminiscent of the abandoned X-20 to the refined blending exhibited by North American-Rockwell's TSTO of 1970-71. The range of delta configurations explored via in-house studies by the Spacecraft Design Division at MSC was a broad one, and bound up in the equally significant question of what the external booster(s)/propellant tankage would look like. Table I is a summary of all MSC design studies for orbiters (including straight, swept, and delta designs) having external tankage and propulsion, beginning with Orbiter 020 in May 1971. Orbiter 020 (Figure 45), derived in May 1971, was the last incorporating internal liquid oxygen stowage, and the first to make use of an external liquid hydrogen tank. Orbiter 021 (also derived in May 1971, and shown in Figure 46) took the next natural step, incorporating all the propellants in external tankage, and freeing

TABLE I

MSC SHUTTLE CONFIGURATIONS

VEHICLE	LANDING WEIGHT	WING	WING AREA	PAYLOAD SIZE	PAYLOAD WEIGHT	BODY LENGTH	FEATURES
020	130K	St (AR7)	1275 ft^2	15x30	20K	1272"	Ext H_2, Int O_2, 4 Eng. Orb. SRM Booster
021	85K	St (AR7)	785 ft^2	15x40	20K	1080"	Ext H_2 & O_2, SRM Booster
022	95K	St (AR5)	792 ft^2	15x40	20K	1064"	Ext H_2 & O_2, SRM Booster
022A	--	45° LE SW	1120 ft^2	15x40	20K	1064"	Ext H_2 & O_2
022B	--	Delta	2100 ft^2	15x40	20K	1064"	Ext H_2 & O_2
023	135K	Delta	2700 ft^2	15x60	40K	1325"	Ext H_2 & O_2, Reusable Booster
024	125K	St (AR5)	1000 ft^2	15x60	40K	1315"	(Stretched 022) Ext H_2 & O_2
025	--	45° LE SW	1414 ft^2	15x60	40K	1315"	(Stretched 022A) Ext H_2 & O_2
026	125K	Delta	2500 ft^2	12x40	40K	1200"	Ext H_2 & O_2, Reusable Booster
027	95K	Delta	1900 ft^2	12x40	40K	1120"	Ext H_2 & O_2 & Main Engine, OMS Tanks in rear, SRM Booster
028	128K	Delta	2560 ft^2	15x40	40K	1080"	(Shortened 023) Ext H_2 & O_2, Reusable Booster
029	--	Delta	1900 ft^2	12x40	40K	1080"	OMS Tanks Amidships, Ext Main Engine
030	103K	St (AR5)	860 ft^2	15x40	20K	1140"	3 J-2S Engines
031	133K	St (AR5)	1110 ft^2	15x60	40K	--	3 J-2S Engines
032	130K	Delta	2600 ft^2	15x40	40K	1140"	3 J-2S Engines, SRM Booster
033	100K	Delta	2000 ft^2	12x40	20K	1200"	(Modified 026) SRM Booster
034	95K	Delta	1900 ft^2	15x30	20K	960"	(Shortened 023 & 028), SRM Booster
035	95K	45° SW	1200 ft^2	12x40	20K	1200"	(Modified 033)
035A	135K	45° SW	1700 ft^2	12x60	40K	1440"	(Stretched 035)
036	110K	Delta	2200 ft^2	15x40	20K	1110"	3 J2S/SRM Booster
036A	110K	Delta	2200 ft^2	15x40	20K	1180"	3 J2S/SRM Booster
036B	110K	Delta	2200 ft^2	15x40	20K	1110"	3 J2S/SRM Booster
036C	114K	Delta	2500 ft^2	15x40	20K	1060"	3 J2S/Pressure Fed Booster
037	145K	Delta	2900 ft^2	15x60	40K	1400"	3 Uprate J2S/Recoverable Booster
037A	145K	Delta	2900 ft^2	15x60	40K	1400"	3 Super Uprate J2S/(036) SRM Booster
038	100K	Delta	2000 ft^2	15x40	20K	1070"	550K H_1P_c/Solid Booster
039	113K	30° SW	1290 ft^2	15x40	20K	1110"	3 J2S/Pressure Fed Booster
040	140K	Delta	3100 ft^2	15x60	25K	1315"	4 J2S/Pressure Fed Booster
040A	140K	Delta	3180 ft^2	15x60	25K	1315"	4 J2S/Pressure Fed Booster
040B	140K	Delta	3180 ft^2	15x60	25K	1315"	4 J2S Retractable/Press Fed Booster
040C	190K	60° Delta	2900 ft^2	15x60	40K	1315"	3 HiPc, SRM Boosters
040C-1	190K	50° Delta	3200 ft^2	15x60	40K	1315"	3 HiPc, 150 ft^2 Canard, Twin Tail
040C-2	190K	35°/-19° Delta	3000 ft^2	15x60	40K	1315"	3 HiPc, 300 ft^2 Wing Glove, Twin Tail SRM Boosters

MSC SHUTTLE DESIGNS UTILIZING EXTERNAL TANKAGE AND/OR PROPULSION

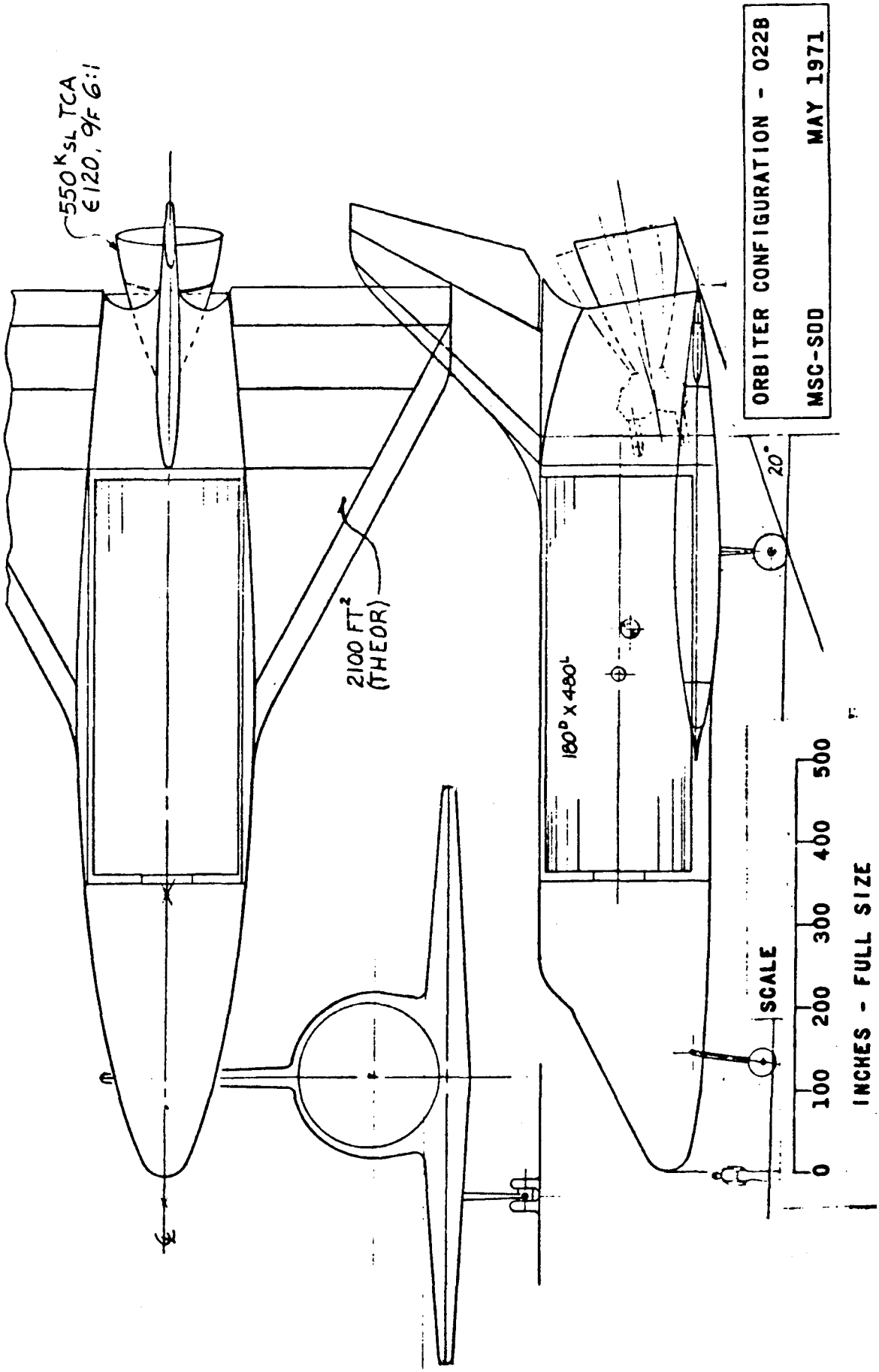
TABLE I (concluded)

MSC SHUTTLE CONFIGURATIONS

VEHICLE	LANDING WEIGHT	WING	WING AREA	PAYLOAD SIZE	PAYLOAD WEIGHT	BODY LENGTH	FEATURES
040C-3	190K	50° Delta	4150 ft^2	15x60	40K	1315"	3 HiPc
040C-4	190K	60° Delta	4440 ft^2	15x60	40K	1315"	3 HiPc
040C-5	190K	50° Delta	3200 ft^2	15x60	40K	1315"	3 HiPc, 100 ft^2 Canard, Twin Tail
040C-6	190K	35°/-19° Delta	2800 ft^2	15x60	40K	1315"	3 HiPc, 150 ft^2 Canard, Twin Tail
041	114K	30°SSW	1290 ft^2	15x60	15K	1300"	3 J2S/Pressure Fed Booster
041A	114K	30° SW	1290 ft^2	15x60	15K	1365"	3 J2S/Pressure Fed Booster
042A	110K	Delta	2500 ft^2	15x60	25K	1260"	Glider, TIII L6 Booster
042B	105K	30° SW	1235 ft^2	15x60	25K	1260"	Glider, TIII L6 Booster
043	83K	30° SW	900 ft^2	10x30	27K	770"	Glider, 2 HiPc on Ext Tank, PF Booster
044	100K	60° Delta	2000 ft^2	10x30	25K	880"	2 HiPc, PF Booster
045	--	--	--	--	--	--	--
046	165K	--	--	14x45	25K	--	3 HiPc, PF Booster
047	185K	49°/-5° Delta	3450 ft^2	15x60	40K	1315"	2 HiPc, Twin Tail, SRM Boosters
048	205K	35°/-19° Delta	3240 ft^2	15x60	40K	1315"	4 HiPc, 324 ft^2 Wing Glove, Twin Tail, SRM Boosters
048A	195K	35°/-19° Delta	3080 ft^2	15x60	40K	1315"	4 - 400K, 308 ft^2 Wing Glove, Twin Tail, SRM Booster
049	205K	75°/35° DBL Delta	1190/3420 ft^2	15x60	40K	1315"	3 Hi Pc, 390 ft^2 Wing Glove, Twin Tail 156" SRM, 62" ASRM
049A	215.3K	75°/35° DBL Delta	1250/3600 ft^2	15x60	40K	1315"	3 Hi Pc, 423 ft^2 Wing Glove, Twin Tail 178" SRM, 62" ASRM
050	--	--	--	--	--	--	--
051	165K	35° Delta	2000 ft^2	15x60 Up	25K	1050"	3 Hi Pc Swing Engines, 156" SRM, 180 ft^2 Canard
052	175K	35° Delta	2120 ft^2	15x60 Up	25K	1230"	3 Hi Pc Swing Engines, 149" SRM 75' Bay with OMS in rear, 180 ft^2 Canard
053	185K	35° Delta	2240 ft^2	15x75 Up	25K	1230"	4 GG Swing Engines, 120" SRM 75' Bay, 190 ft^2 Canard
054	185K	35° Delta	2240 ft^2	15x75 Up	25K	1230"	4 Hi Pc Swing Engines, 140" SRM 75' Bay, 190 ft^2 Canard

up the orbiter itself to be basically an orbiting delivery van. Figures 65-98 show the various delta configurations the Spacecraft Design Division studied starting with Orbiter 022B in May 1971, and concluding with Orbiter 054 slightly over a year later. The generation of these designs is extraordinary; 29 in all, slightly more than one every 2 weeks. While it is true that many of these (such as the 036 or 040 family) were parametric variations on a theme, one must nevertheless marvel at the pace of analytical work taking place at the MSC in this one year--after all, the SDD's designers were likewise evaluating the last schemes for the Faget orbiter during this same period. Table I offers details on these various concepts. Generally speaking, NASA evaluated deltas having simple triangular wing planforms, more complex "ogee" leading edges and trailing edge taper, double-delta configurations a la the SAAB J-35 Draken fighter, and combinations of deltas and canards. The two most influential "families" were the 036 series and the 040 series. The 036 series had the same three-engine concept as utilized on the ultimate Shuttle orbiter, but all of these vehicles (Figures 74-77) had 15 x 40 ft. payload bays, and made use of adaptations of "off-the-shelf" propulsion, in this case three J-2S engines from the Saturn program. After an interim look at the 037 family, with uprated J-2S propulsion and a 15 x 60 ft. payload bay, MSC engineers drew up plans for a larger family of orbiters having the same generalized configuration as the earlier 036 family, but having the 15 x 60 ft. payload bay and four of the J-2S engines. The result of this was the 040 design (Figure 80) which first appeared in August 1971. Desires for increased payload led to design of the 040C (Figure 83, January 1972), using three new high-pressure engines; the 040C formed the basis for the ultimate Space Shuttle. With the evolution of the 040C configuration, NASA had reached the point where it had a shape suitable for the anticipated roles and missions of the actual Shuttle. 040C thus formed the baseline for Rockwell to work to when it embarked upon Phase C/D. After 040C, all other

Figure 65


ORBITER CONFIGURATION - 022B
MSC-SDD
MAY 1971
550 K SL TCA
ε 120, 9F 6:1
2100 FT² (THEOR)
180° X 480'
20°
SCALE
0 100 200 300 400 500
INCHES - FULL SIZE

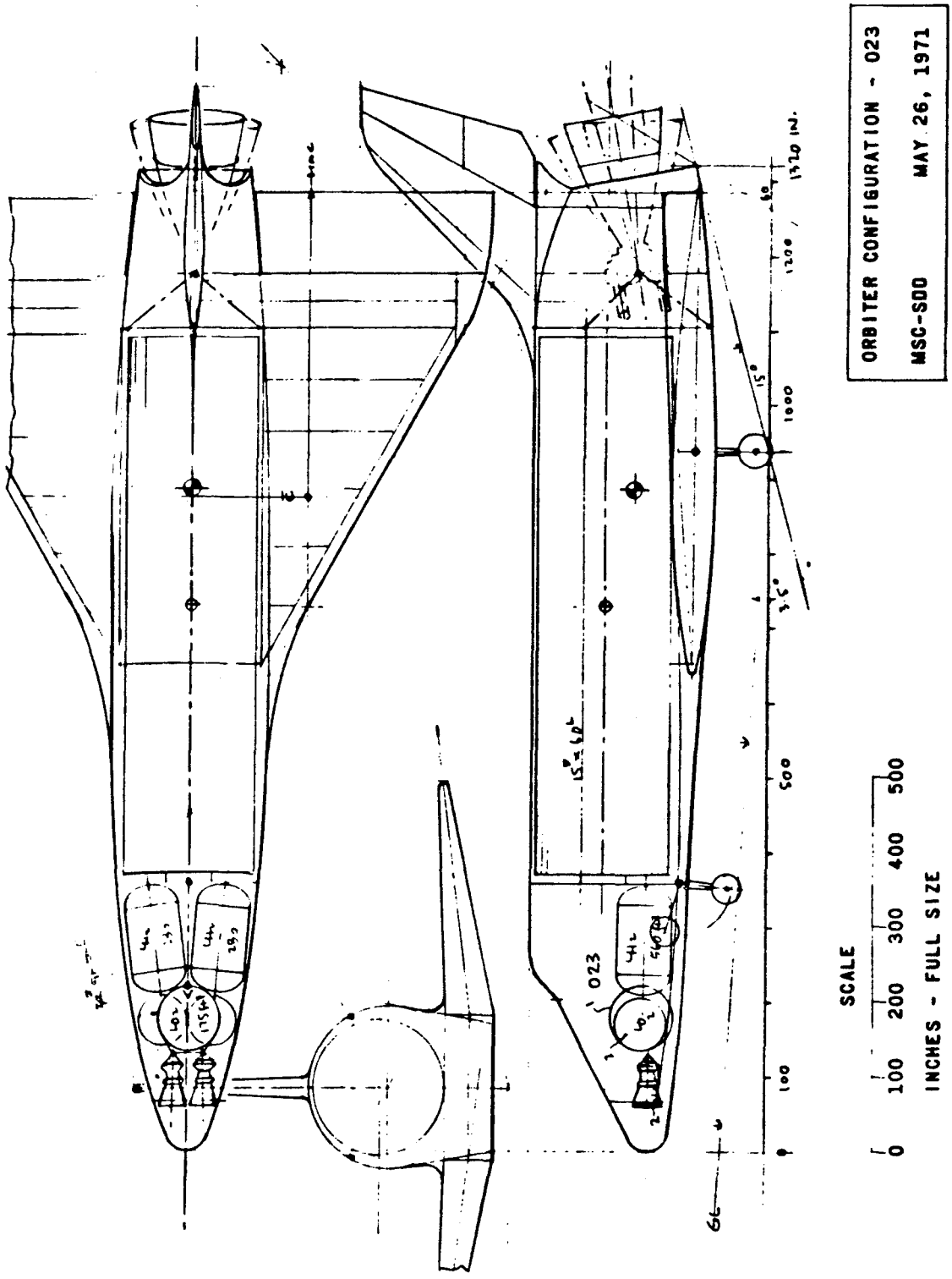

ORBITER CONFIGURATION - 023
MSC-SDD MAY 26, 1971
SCALE
0 100 200 300 400 500
INCHES - FULL SIZE

Figure 67

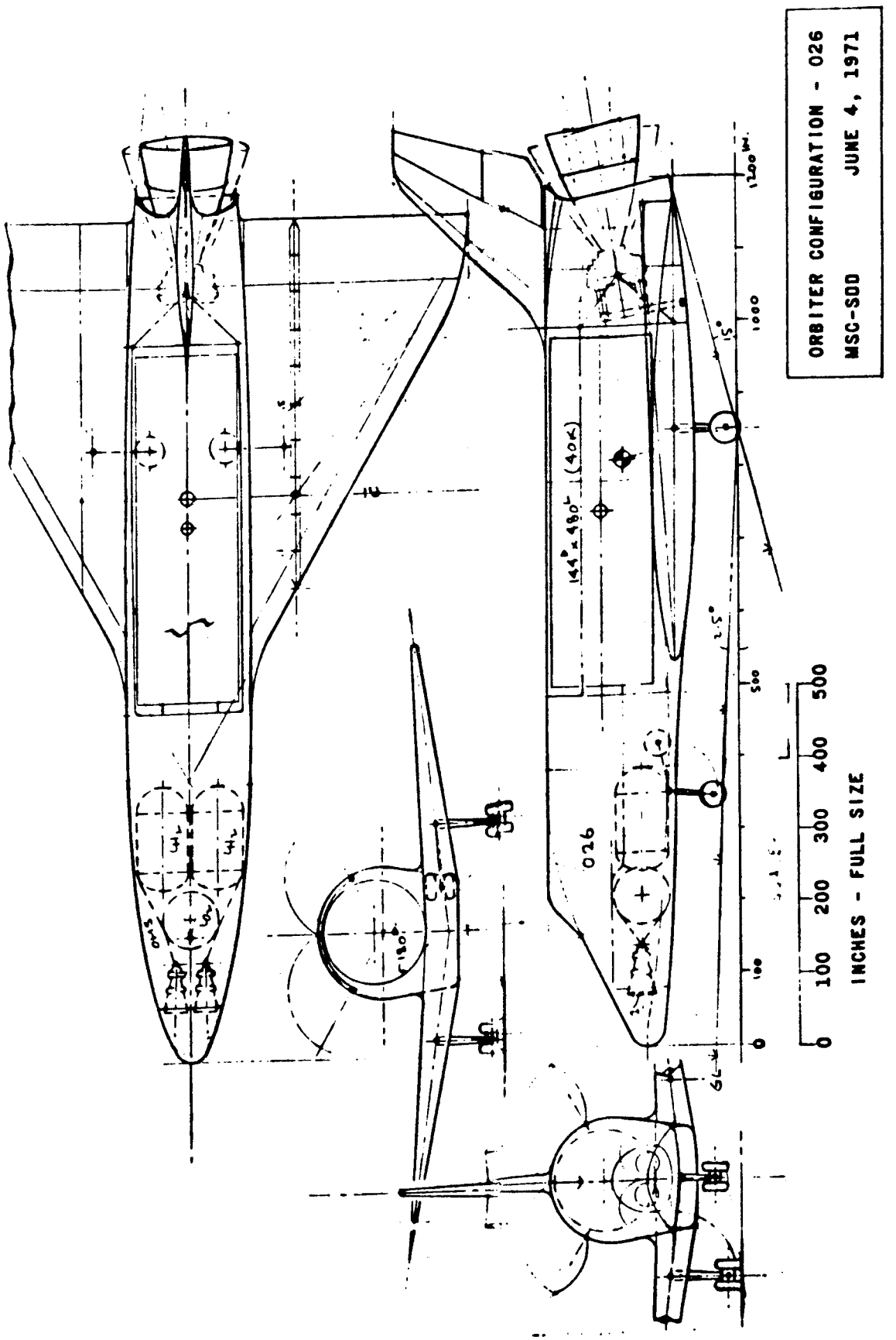

ORBITER CONFIGURATION - 026
MSC-SOD JUNE 4, 1971
INCHES - FULL SIZE
0
100
200
300
400
500
026

Figure 68

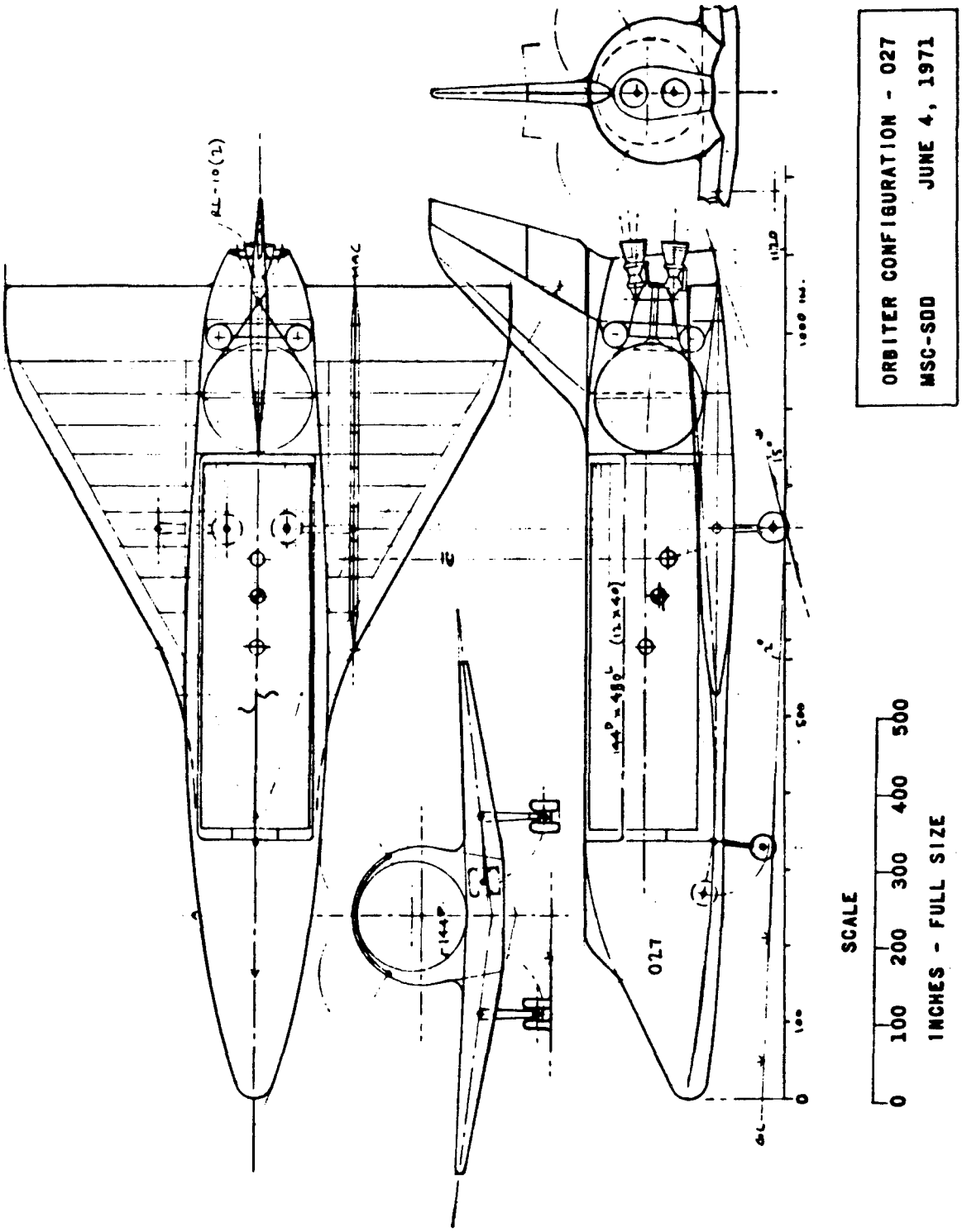
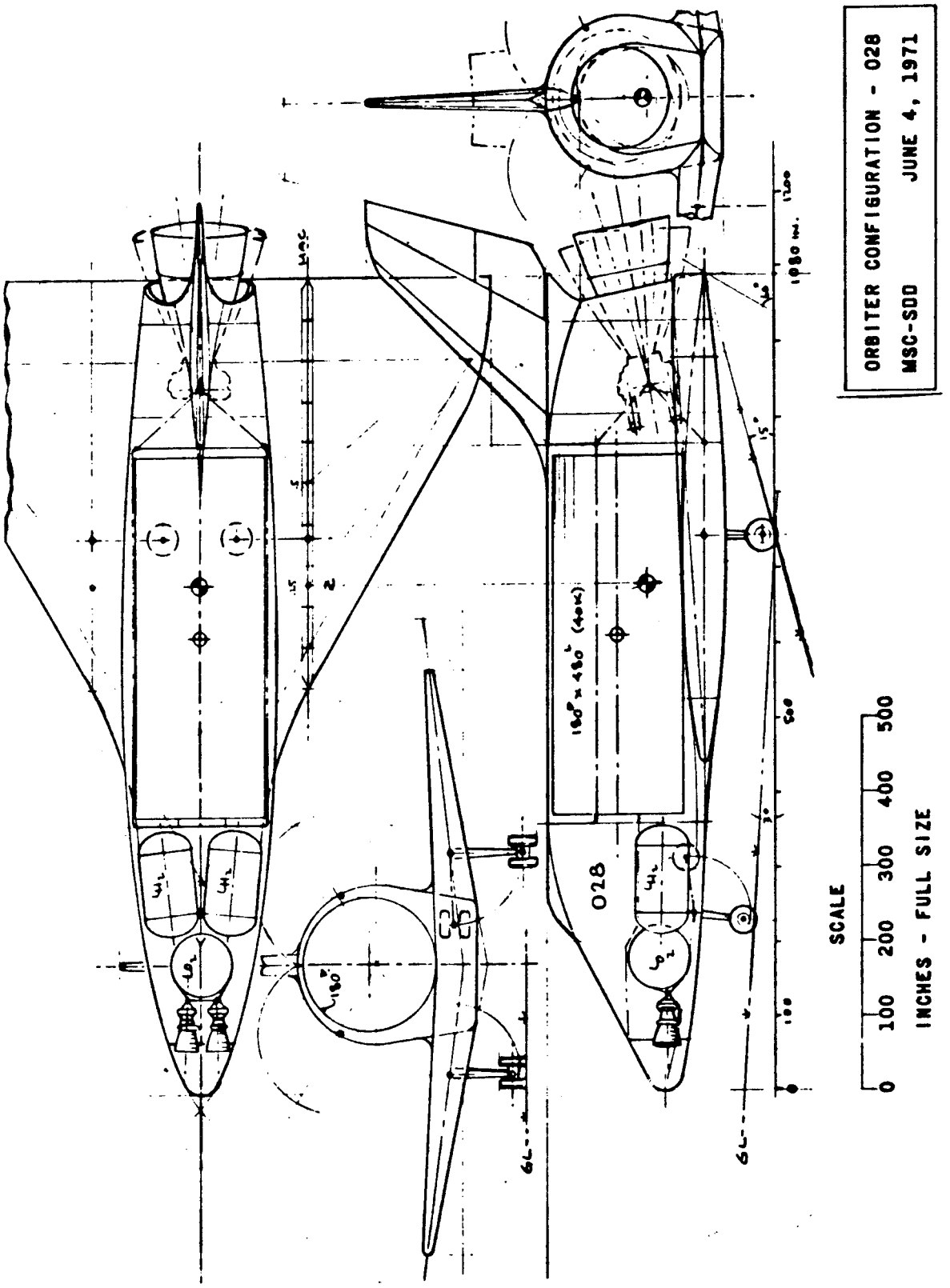

ORBITER CONFIGURATION - 027
MSC-SOD JUNE 4, 1971
SCALE
0 100 200 300 400 500
INCHES - FULL SIZE
027

Figure 69


ORBITER CONFIGURATION - 028
MSC-SDD JUNE 4, 1971
028
SCALE
0 100 200 300 400 500
INCHES - FULL SIZE

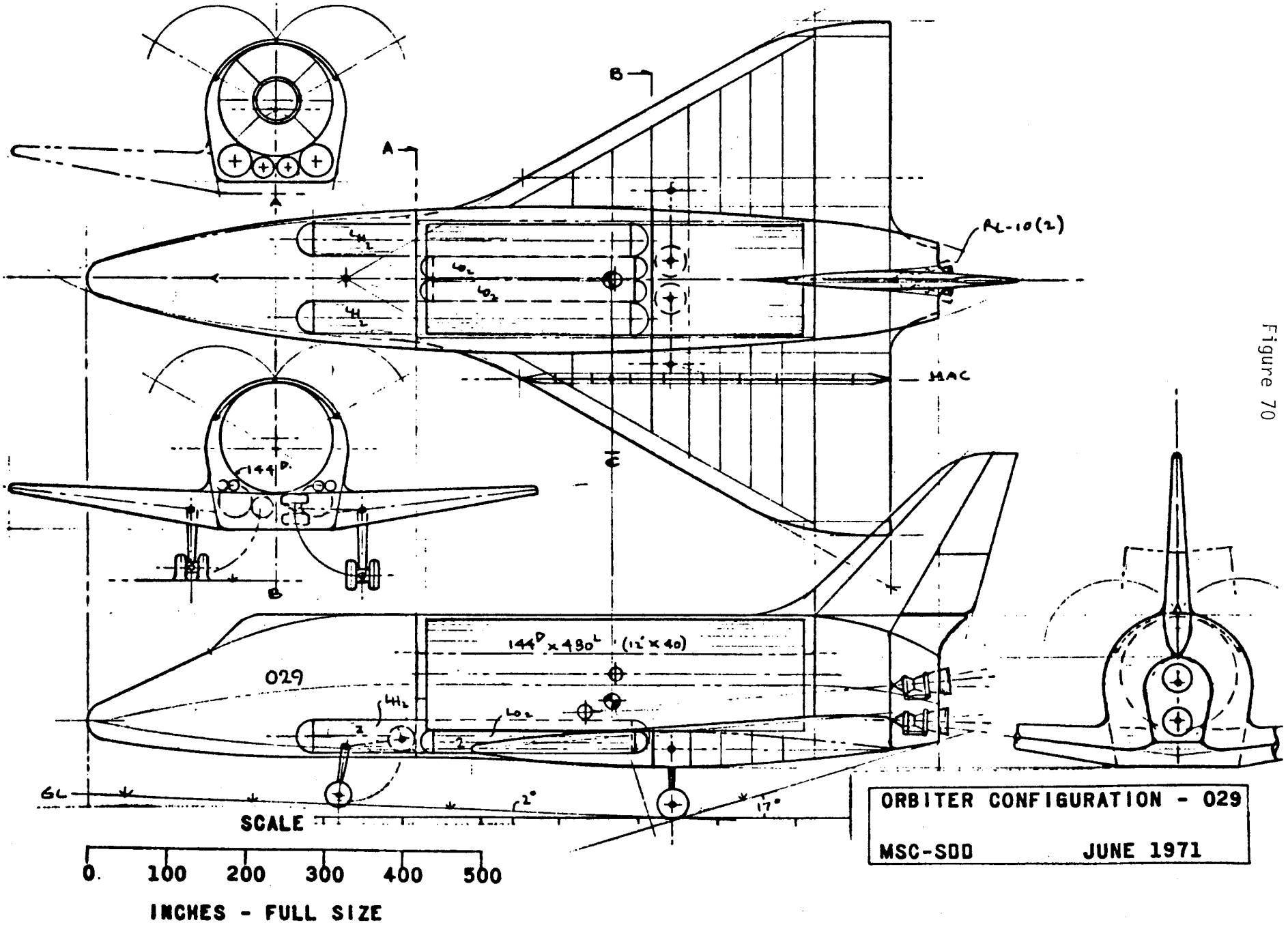

B
A
RL-10(2)
MAC
144D
144D x 480L (12' x 40)
029
GL
2°
17°
SCALE
0 100 200 300 400 500
INCHES - FULL SIZE
ORBITER CONFIGURATION - 029
MSC-SDD JUNE 1971

Figure 70

Figure 71

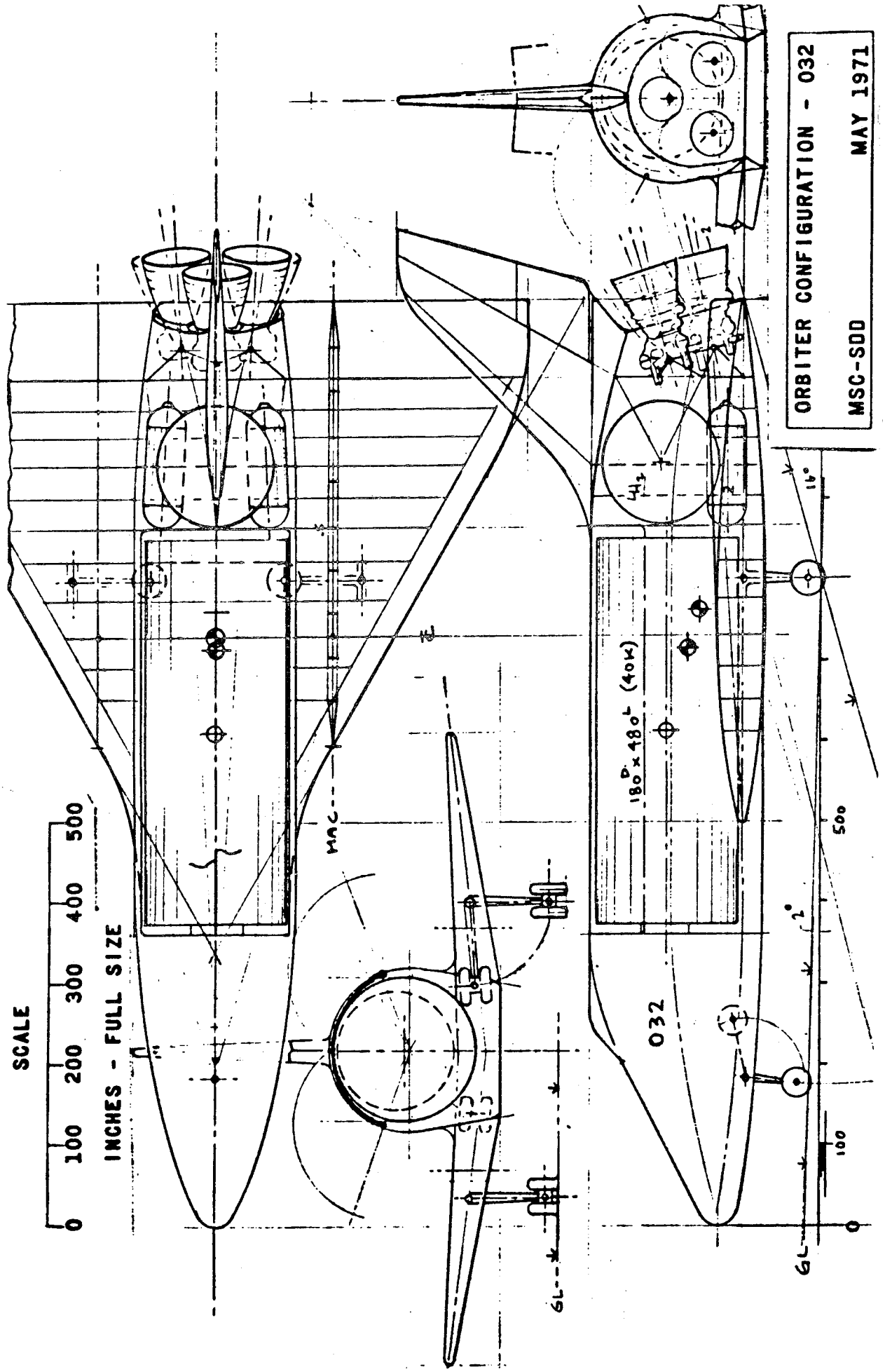

ORBITER CONFIGURATION - 032
MSC-SDD
MAY 1971
032
180D x 480L (40K)
MAC
SCALE
INCHES - FULL SIZE
0
100
200
300
400
500

Figure 72

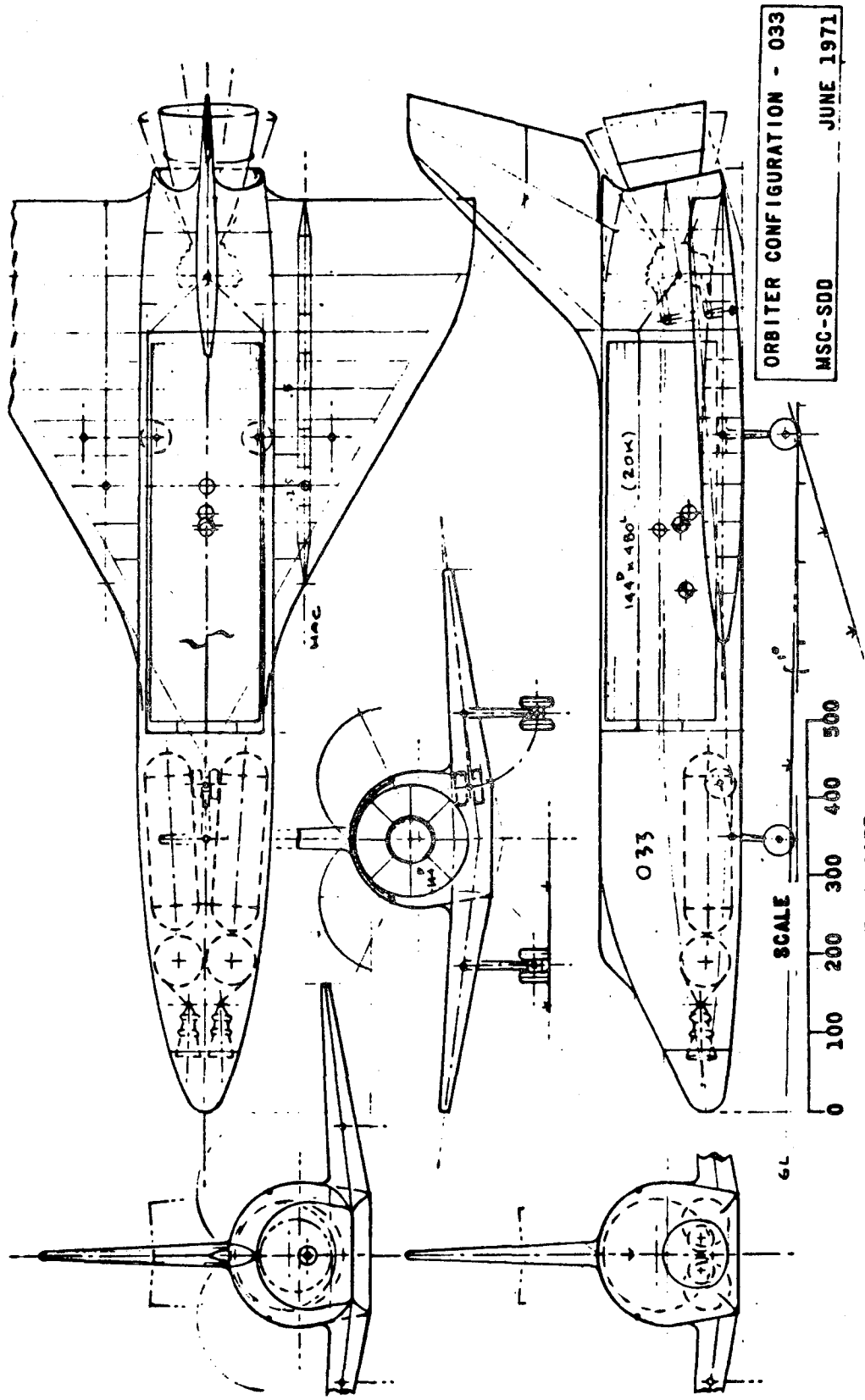
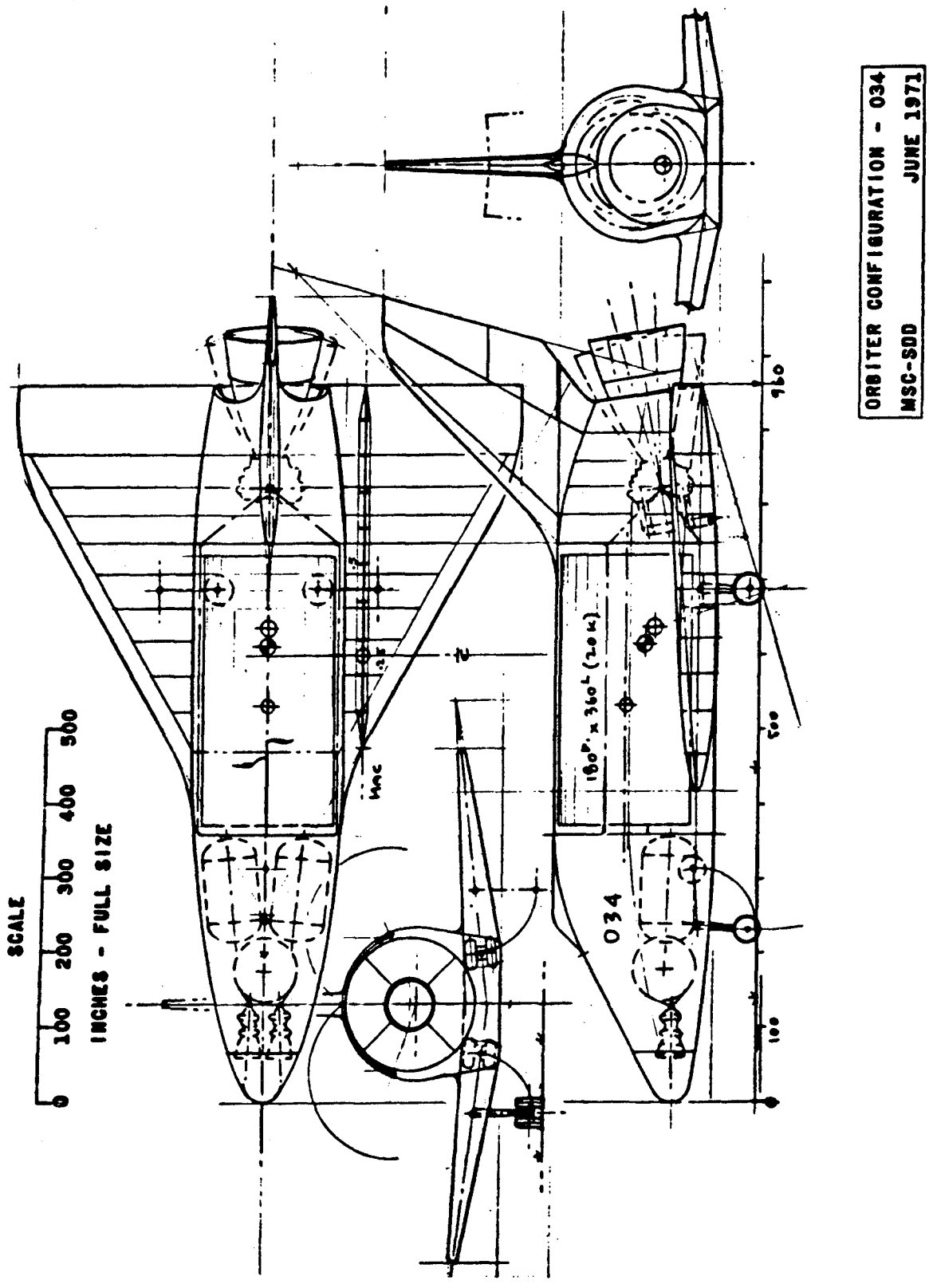

ORBITER CONFIGURATION - 033
MSC-SDD
JUNE 1971
144"D x 480" (20K)
033
SCALE
0 100 200 300 400 500
INCHES - FULL SIZE

Figure 73


ORBITER CONFIGURATION - 034
MSC-SDD
JUNE 1971
SCALE
0
100
200
300
400
500
INCHES - FULL SIZE
034

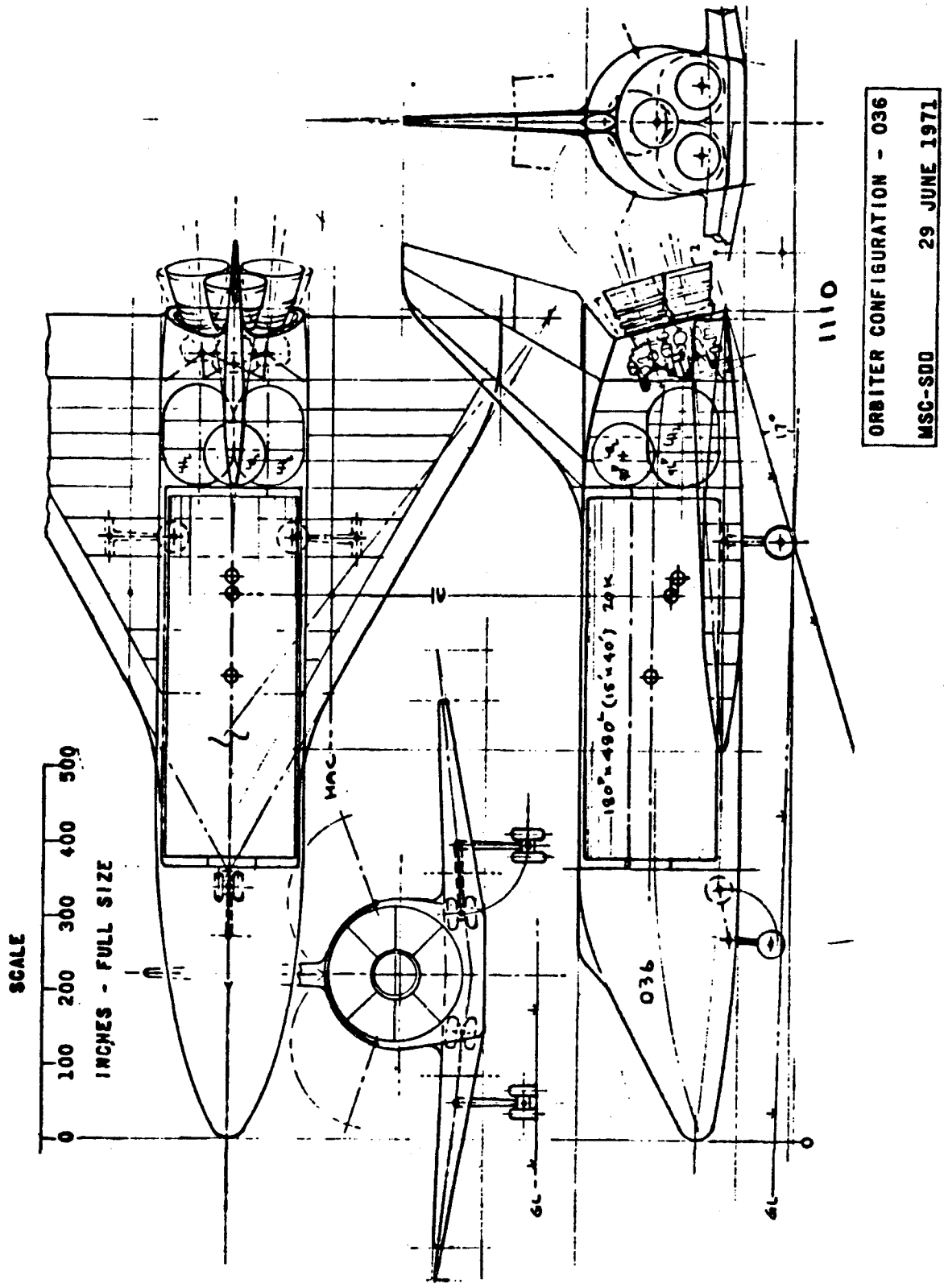

ORBITER CONFIGURATION - 036
MSC-SDD 29 JUNE 1971
SCALE
INCHES - FULL SIZE
0 100 200 300 400 500
1110
036

Figure 75

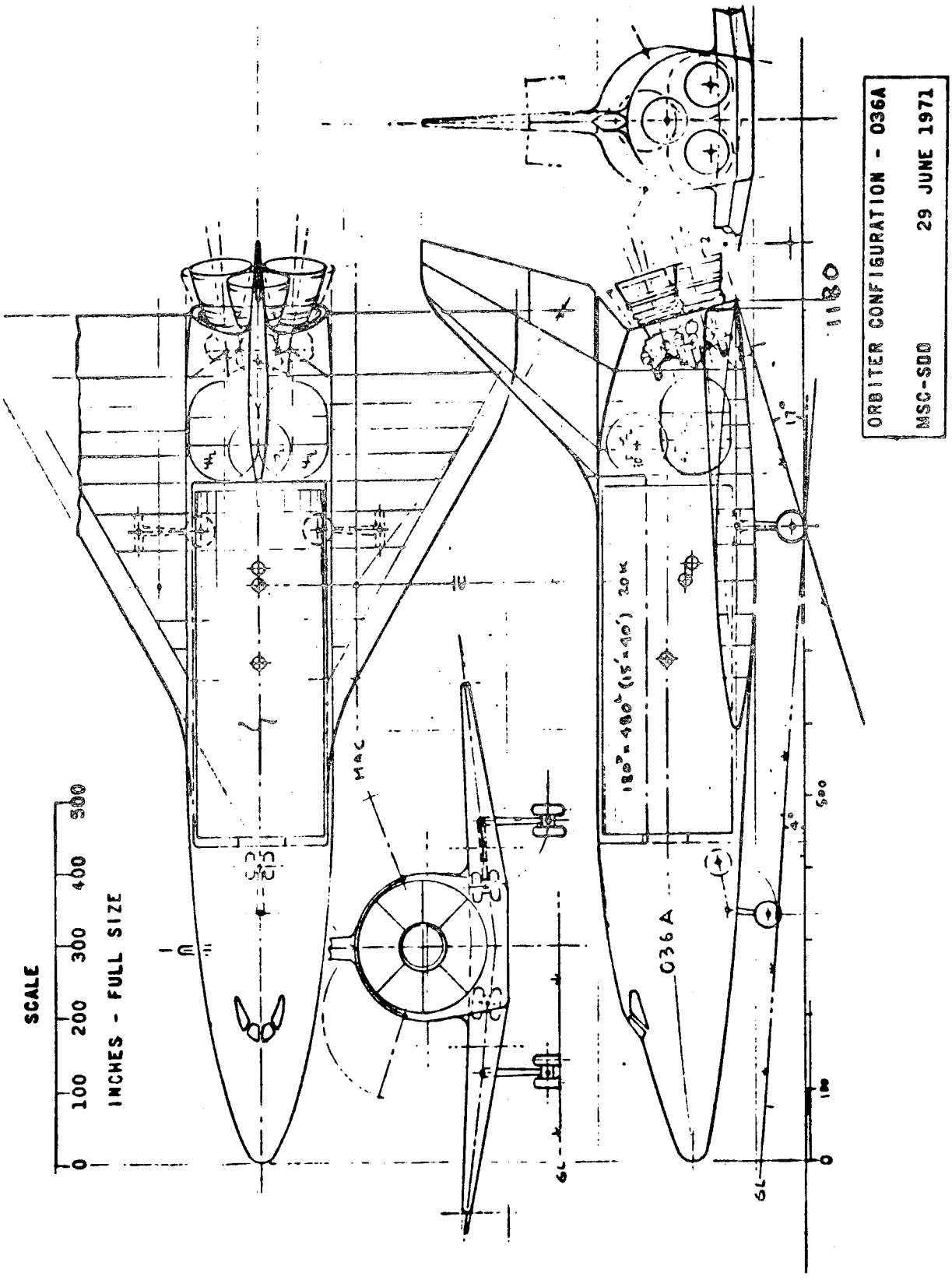

ORBITER CONFIGURATION - 036A
MSC-SDO 29 JUNE 1971
SCALE
INCHES - FULL SIZE
0 100 200 300 400 500
MAC
036A
1180
500
180
0

Figure 76

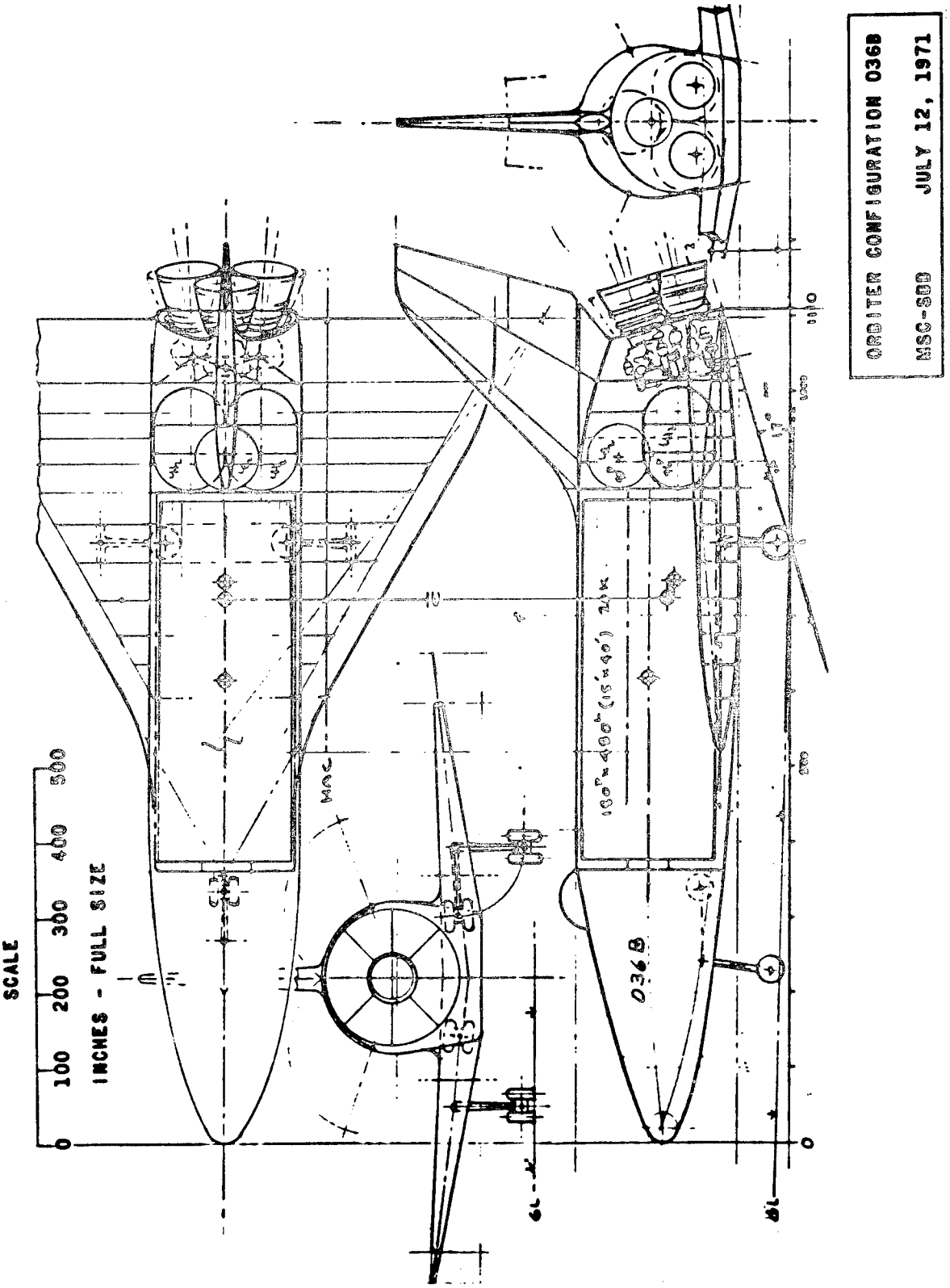

ORBITER CONFIGURATION 036B
MSC-800 JULY 12, 1971
SCALE
0 100 200 300 400 500
INCHES - FULL SIZE
MAC
036B

Figure 77

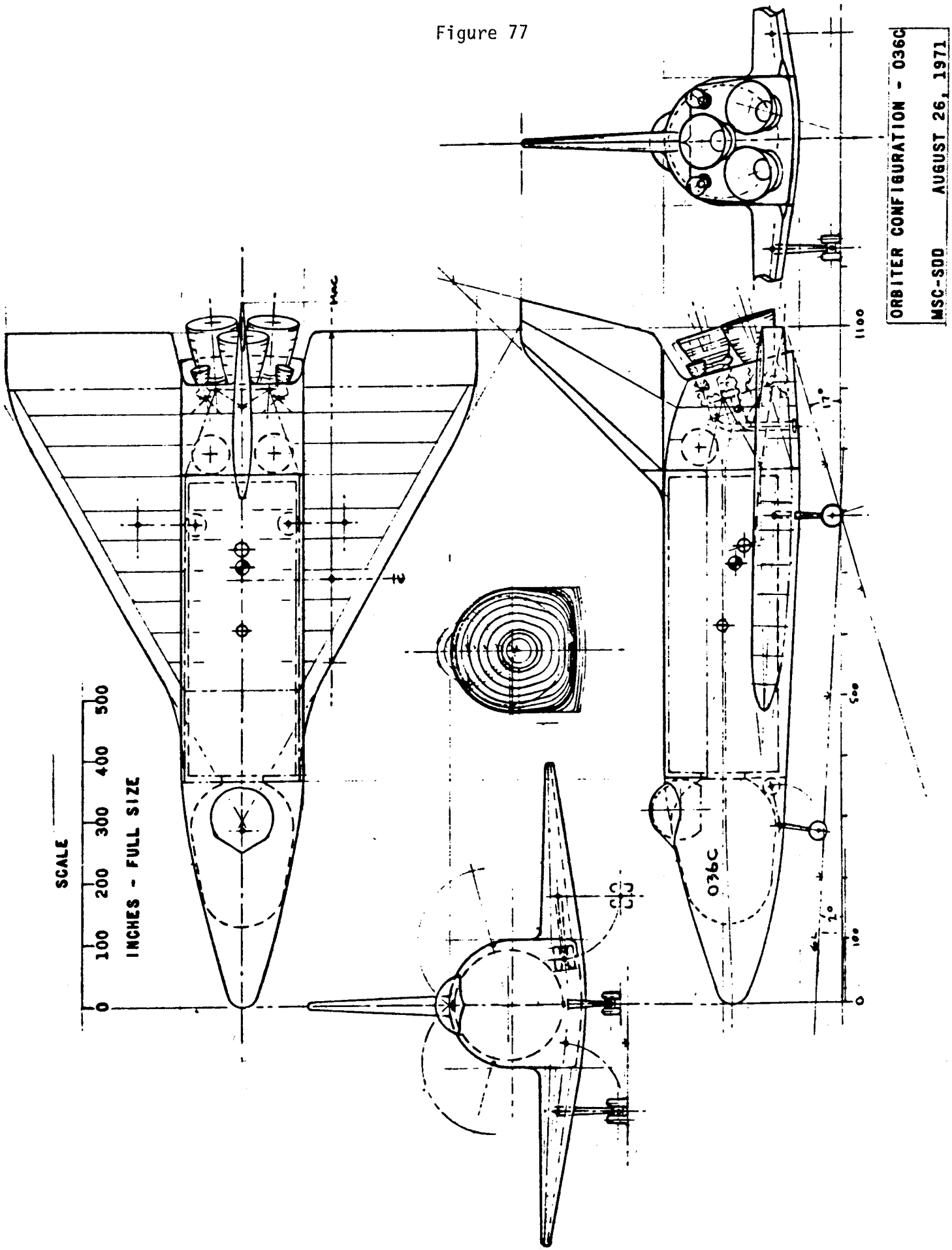

ORBITER CONFIGURATION - 036C
MSC-SDD AUGUST 26, 1971
SCALE
INCHES - FULL SIZE
0
100
200
300
400
500
036C

Figure 78

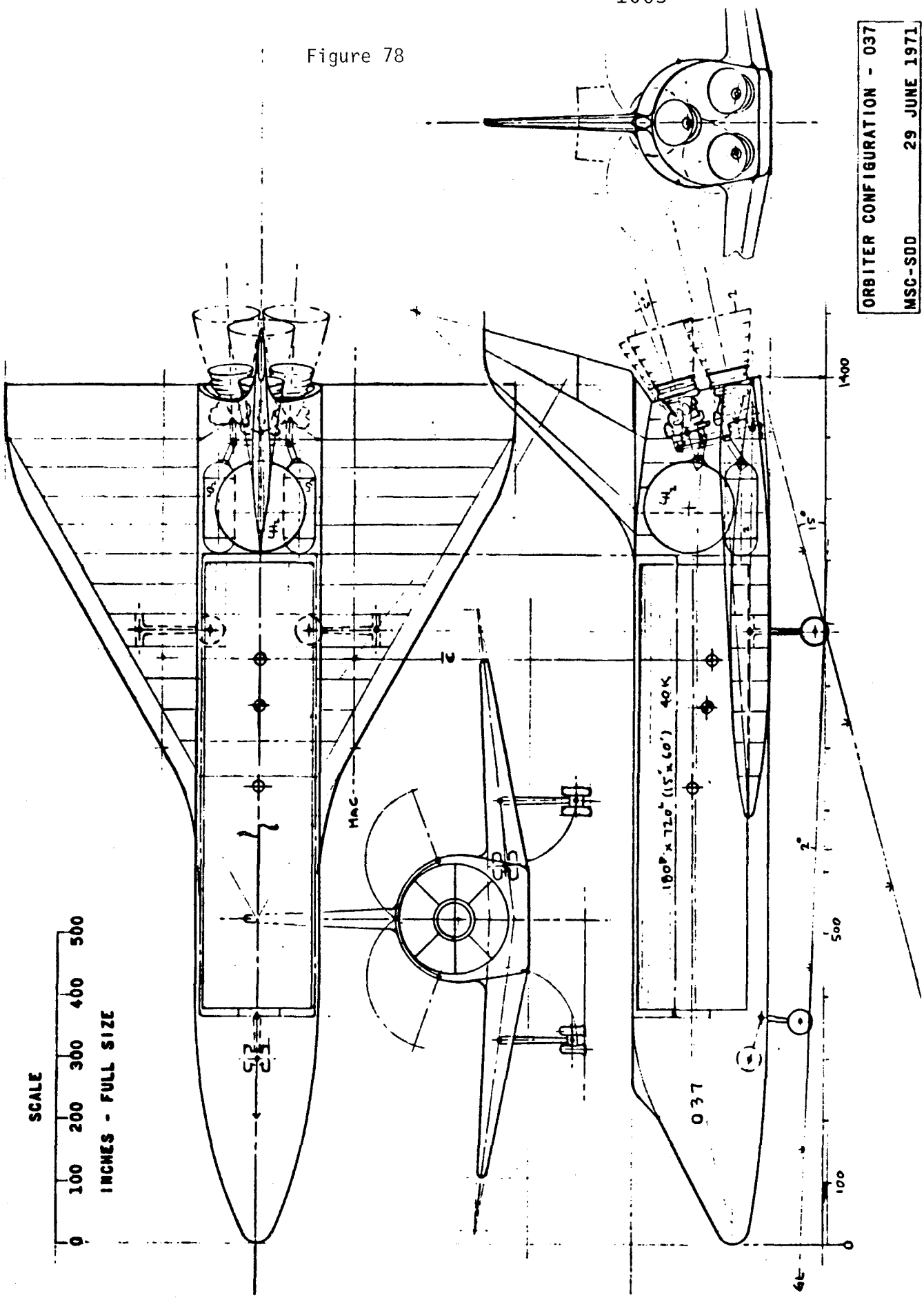

ORBITER CONFIGURATION - 037
MSC-SOD 29 JUNE 1971
SCALE
0 100 200 300 400 500
INCHES - FULL SIZE
MAC
037

Figure 79

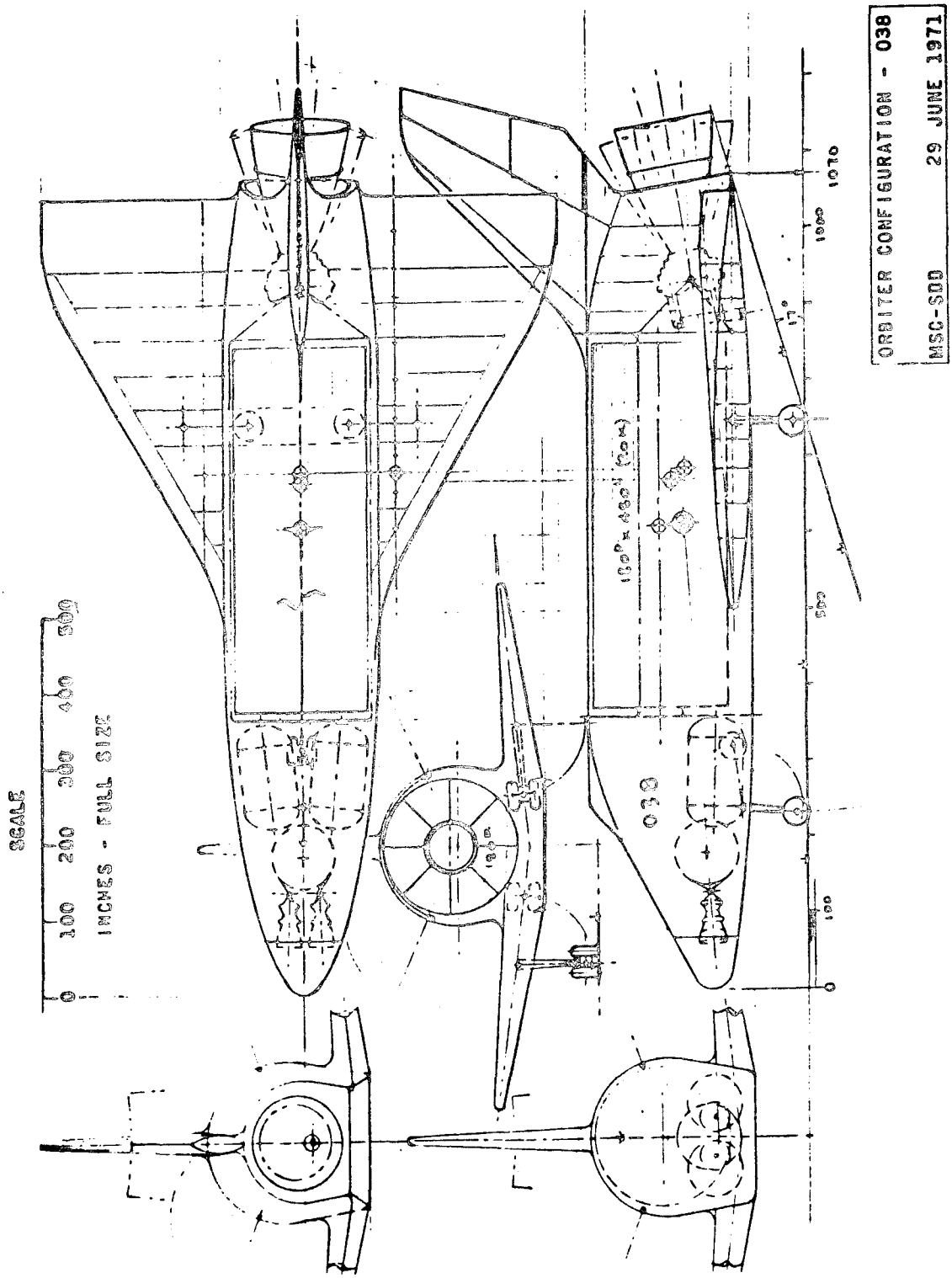

ORBITER CONFIGURATION - 038
MSC-SDD 29 JUNE 1971
SCALE
0 100 200 300 400 500
INCHES - FULL SIZE
038

Figure 80

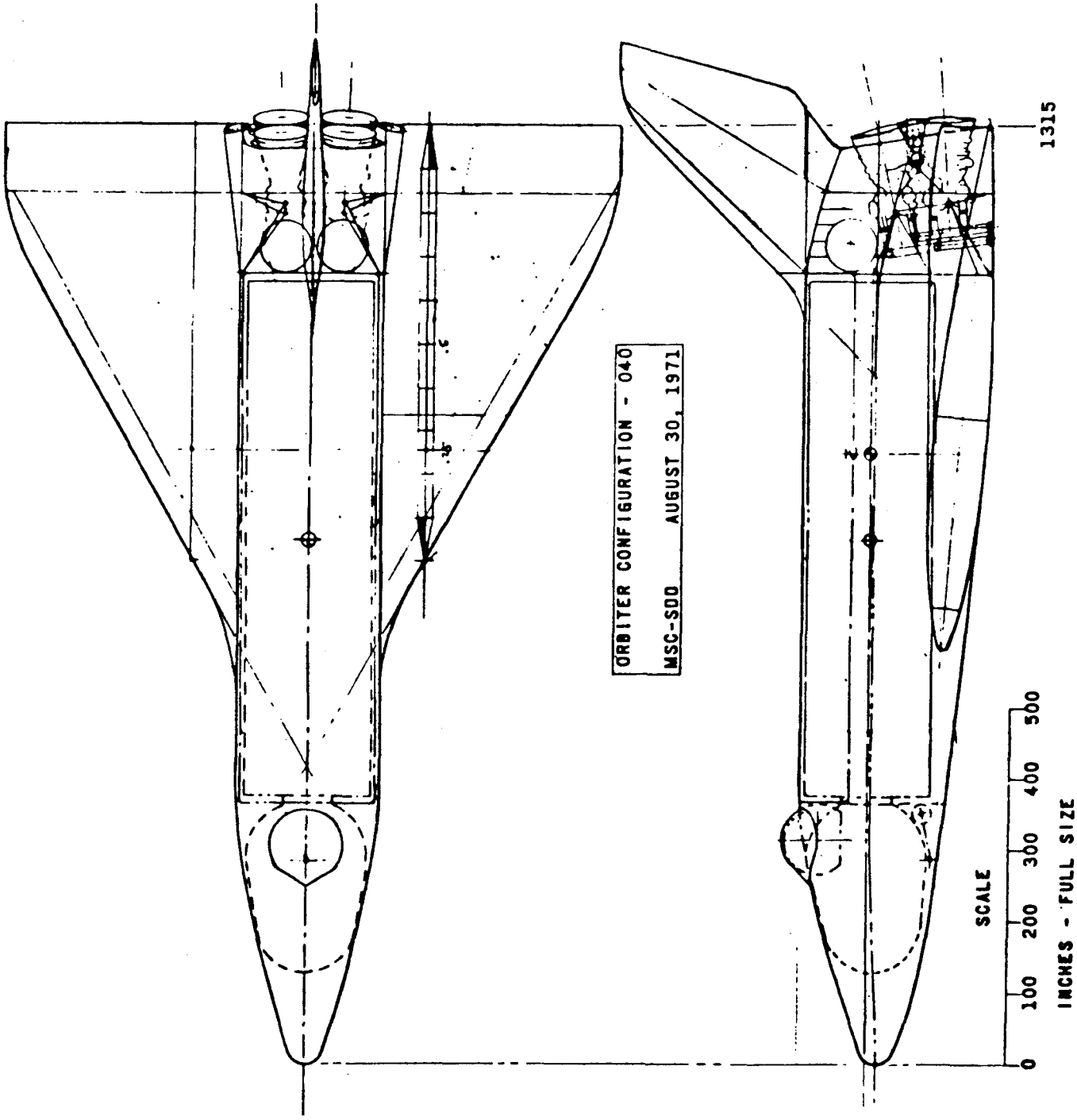
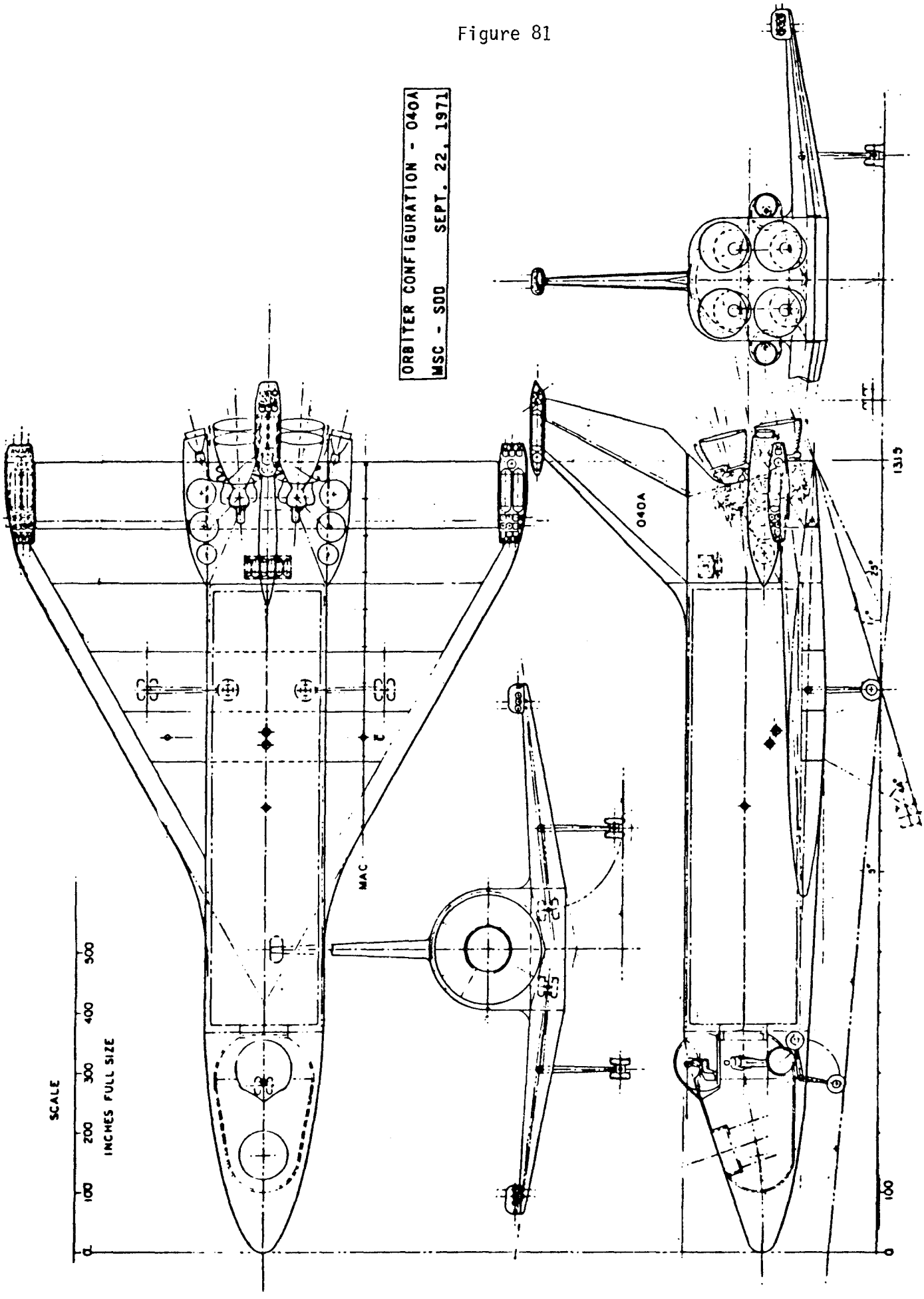

1315
ORBITER CONFIGURATION - 040
MSC-SDO AUGUST 30, 1971
SCALE
0 100 200 300 400 500
INCHES - FULL SIZE

Figure 81


ORBITER CONFIGURATION - 040A
MSC - SOD SEPT. 22, 1971
040A
MAC
SCALE
INCHES FULL SIZE
0
100
200
300
400
500

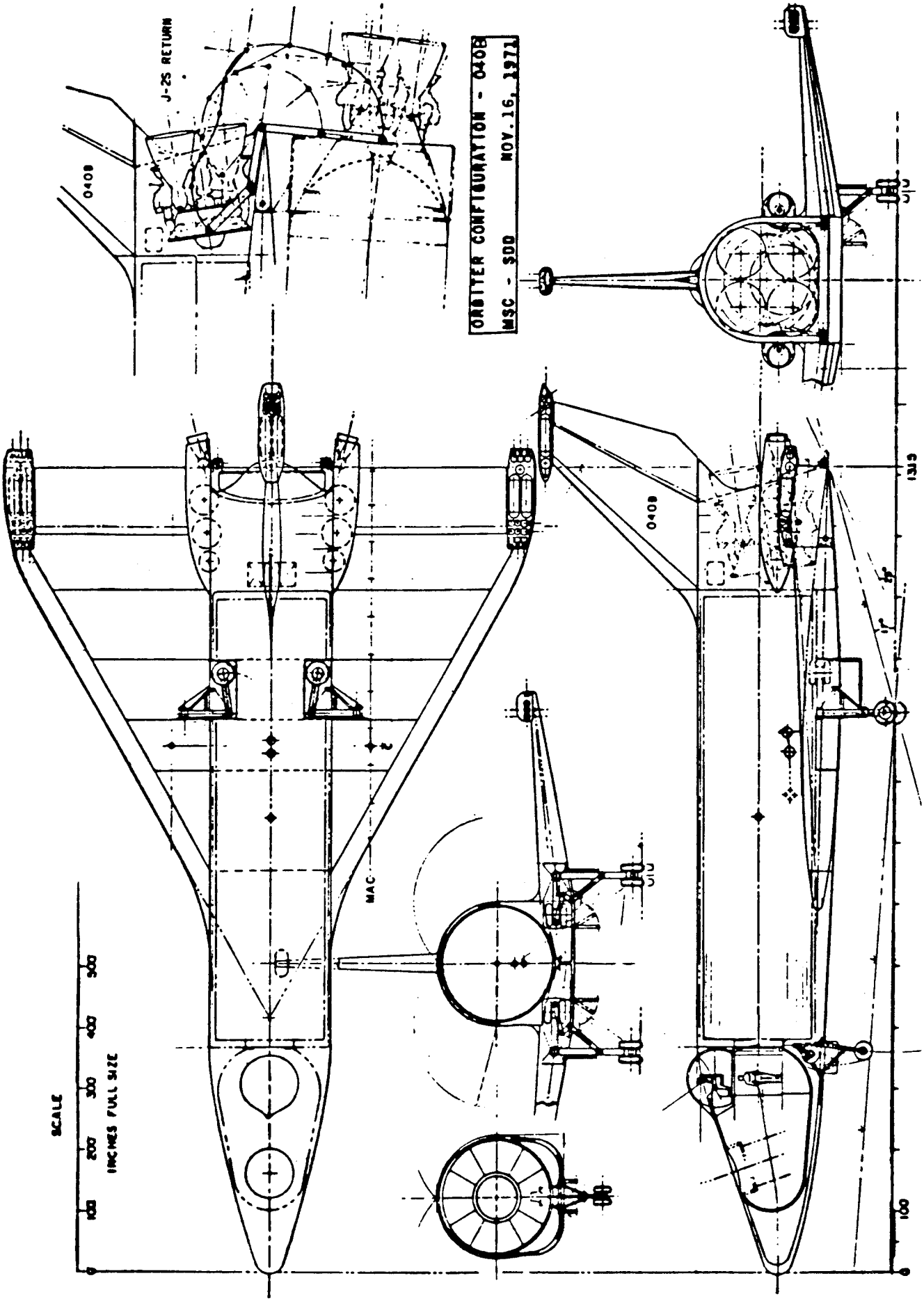

J-2S RETURN
040B
ORBITER CONFIGURATION - 040B
MSC - 500 NOV. 16, 1971
MAC
SCALE
INCHES FULL SIZE

Figure 83

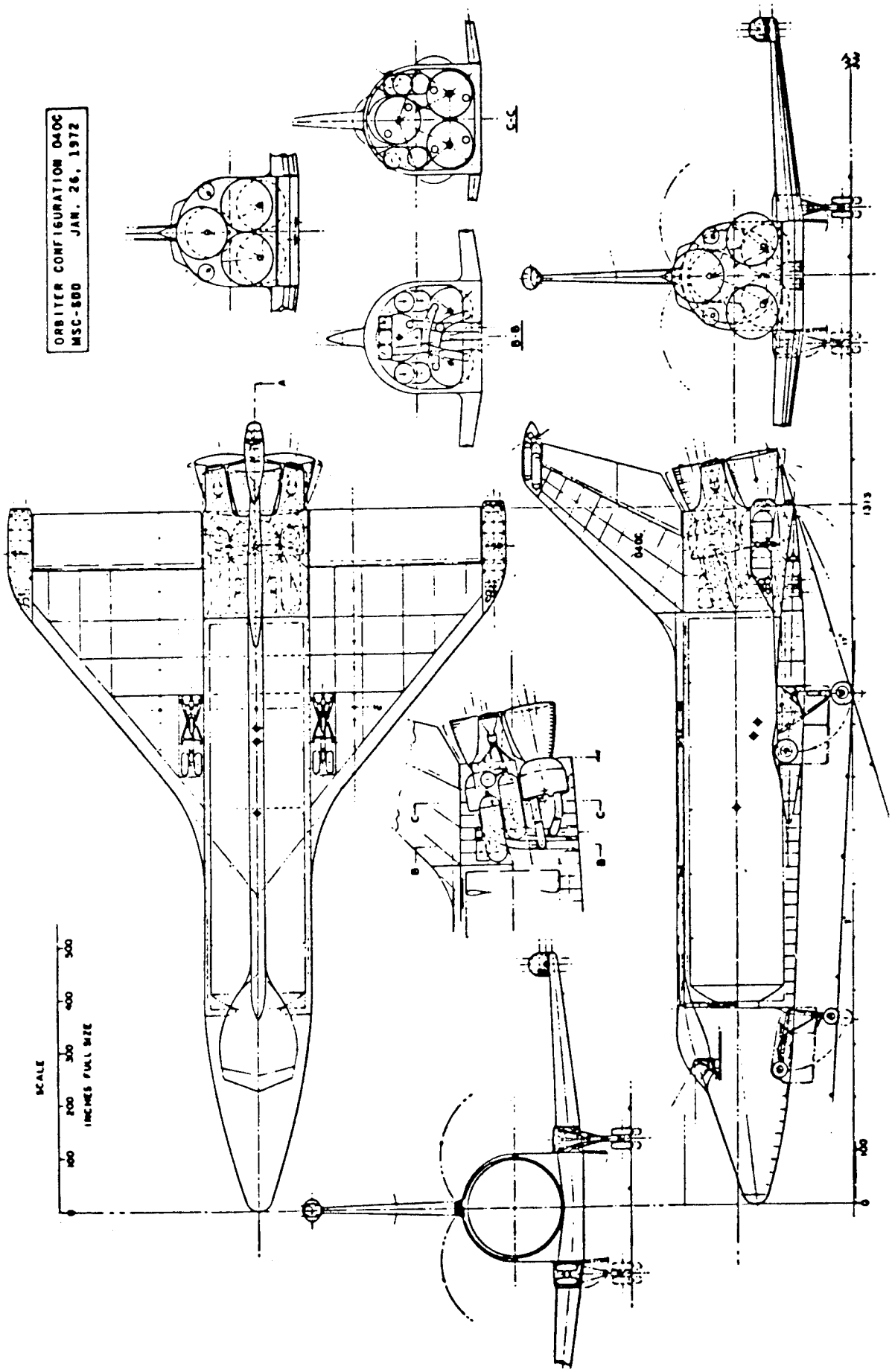

ORBITER CONFIGURATION 040C
MSC-800 JAN. 26, 1972
SCALE
INCHES FULL SIZE

Figure 84

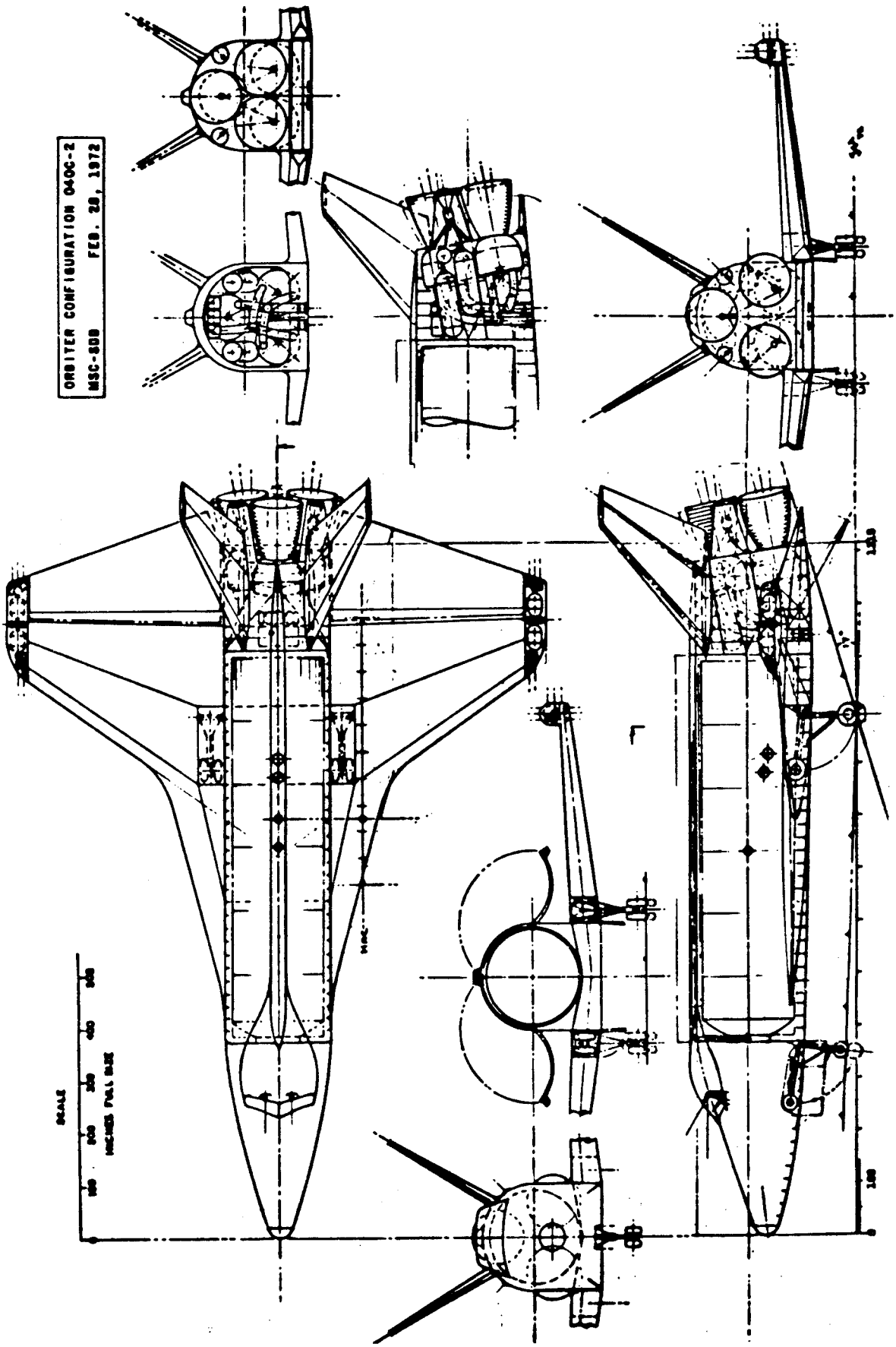

ORBITER CONFIGURATION 040C-2
MSC-808 FEB. 28, 1972
SCALE
INCHES FULL SIZE

Figure 85

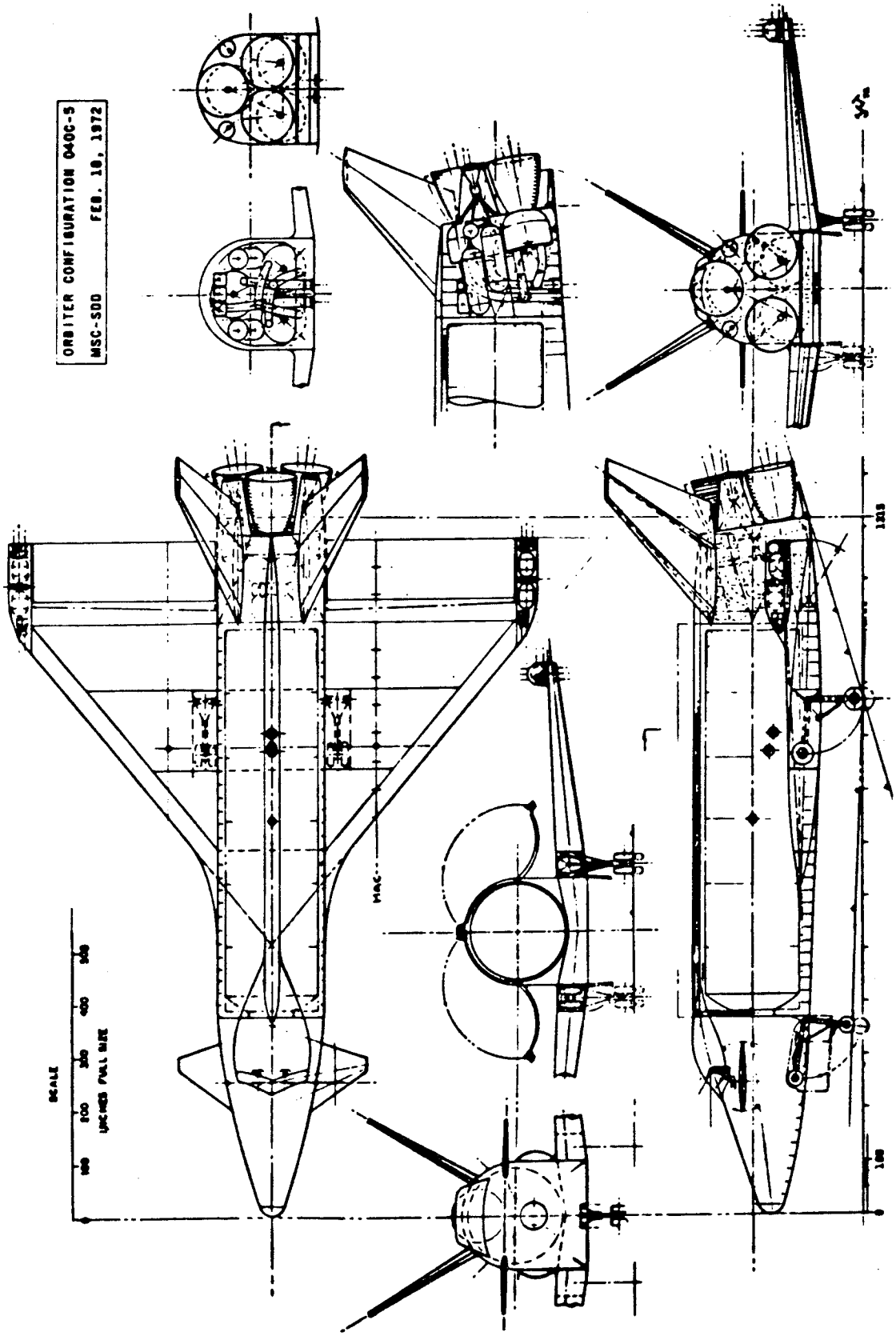

ORBITER CONFIGURATION 040C-S
MSC-SDD FEB. 18, 1972
SCALE
INCHES FULL SIZE

Figure 86

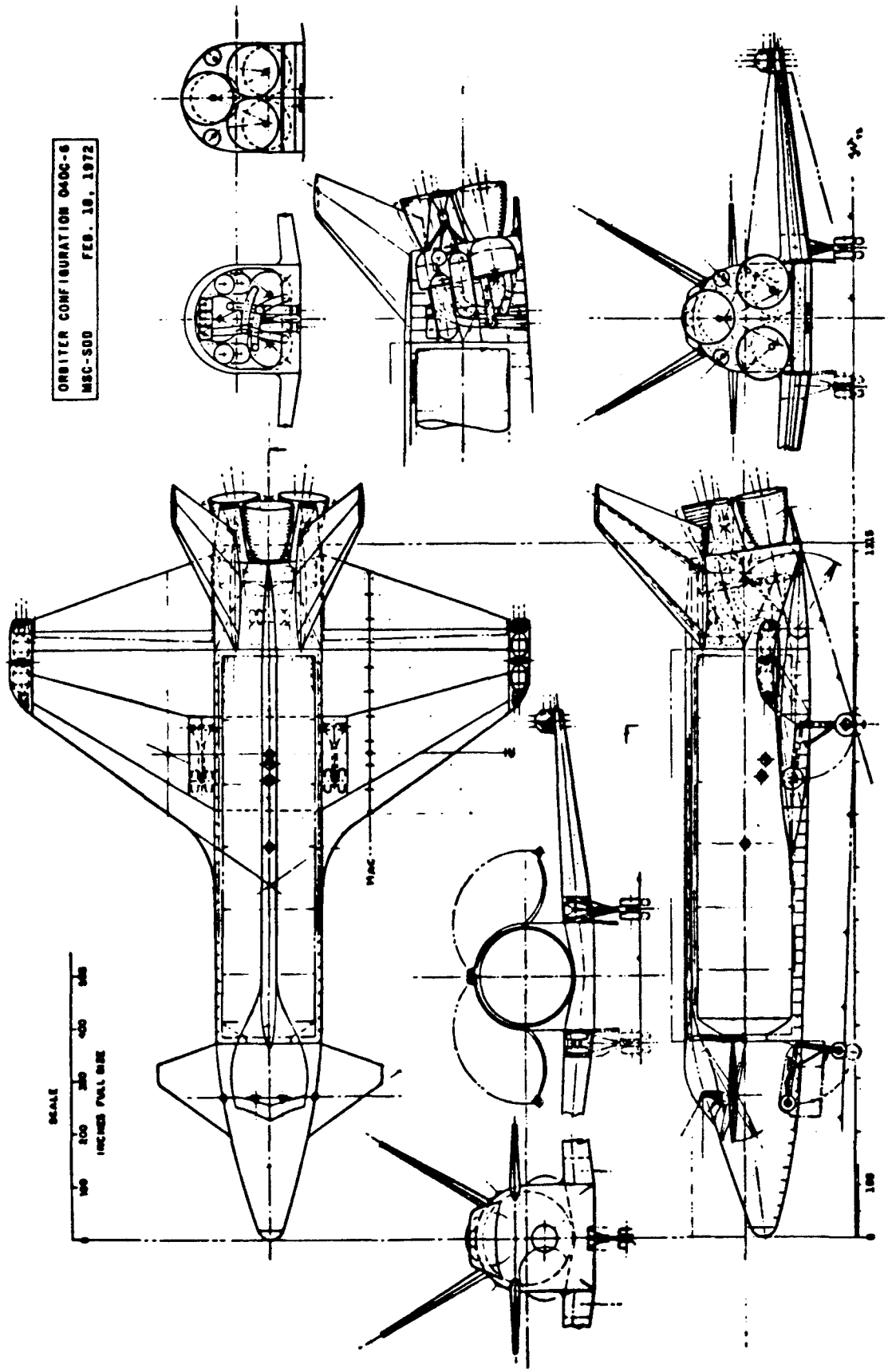

ORBITER CONFIGURATION 040C-6
MSC-SOD FEB. 10, 1972
SCALE
INCHES FULL SIZE

Figure 87

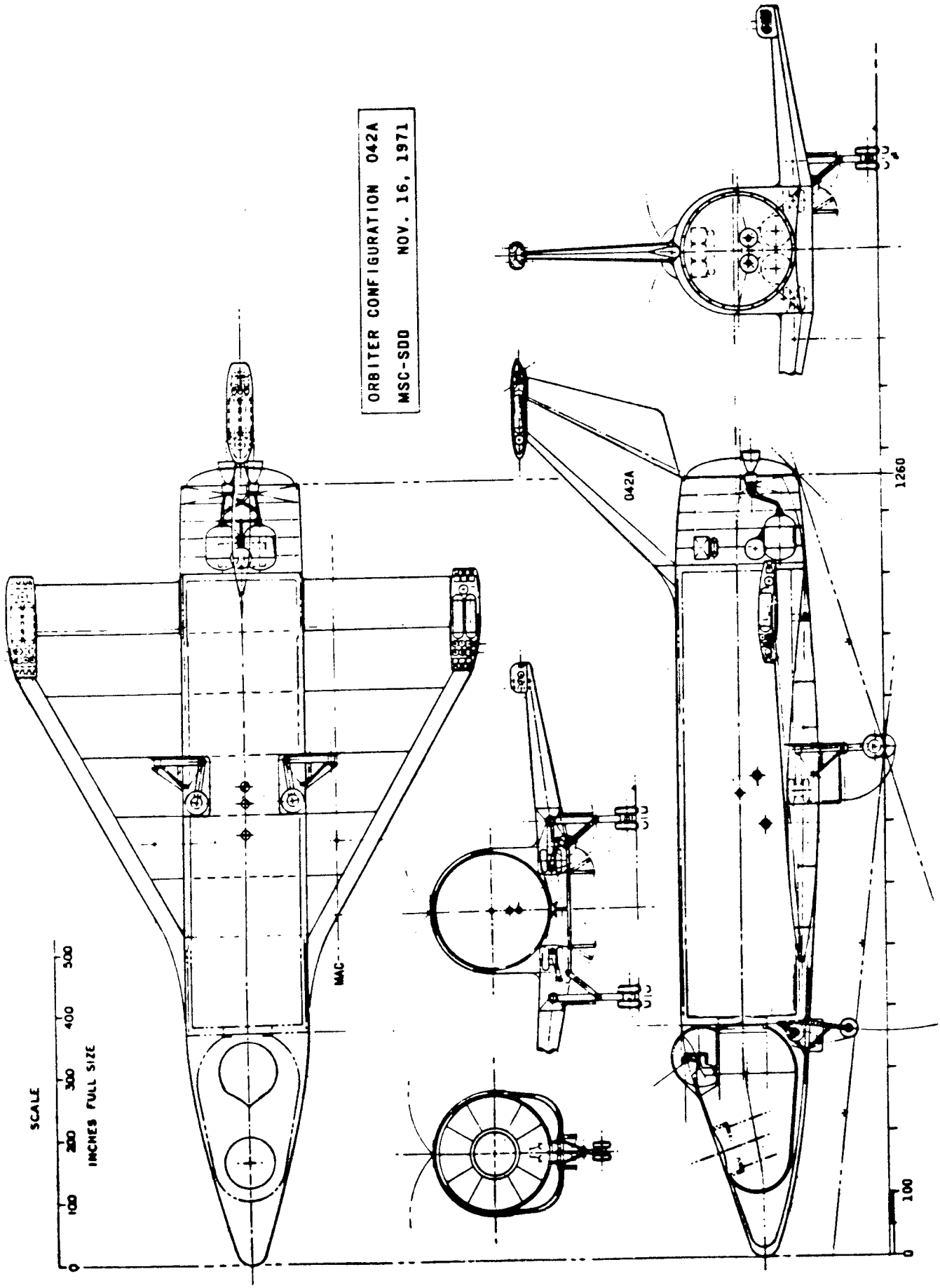

ORBITER CONFIGURATION 042A
MSC-SDD NOV. 16, 1971
042A
MAC
1260
100
0
SCALE
INCHES FULL SIZE
100
200
300
400
500

Figure 88

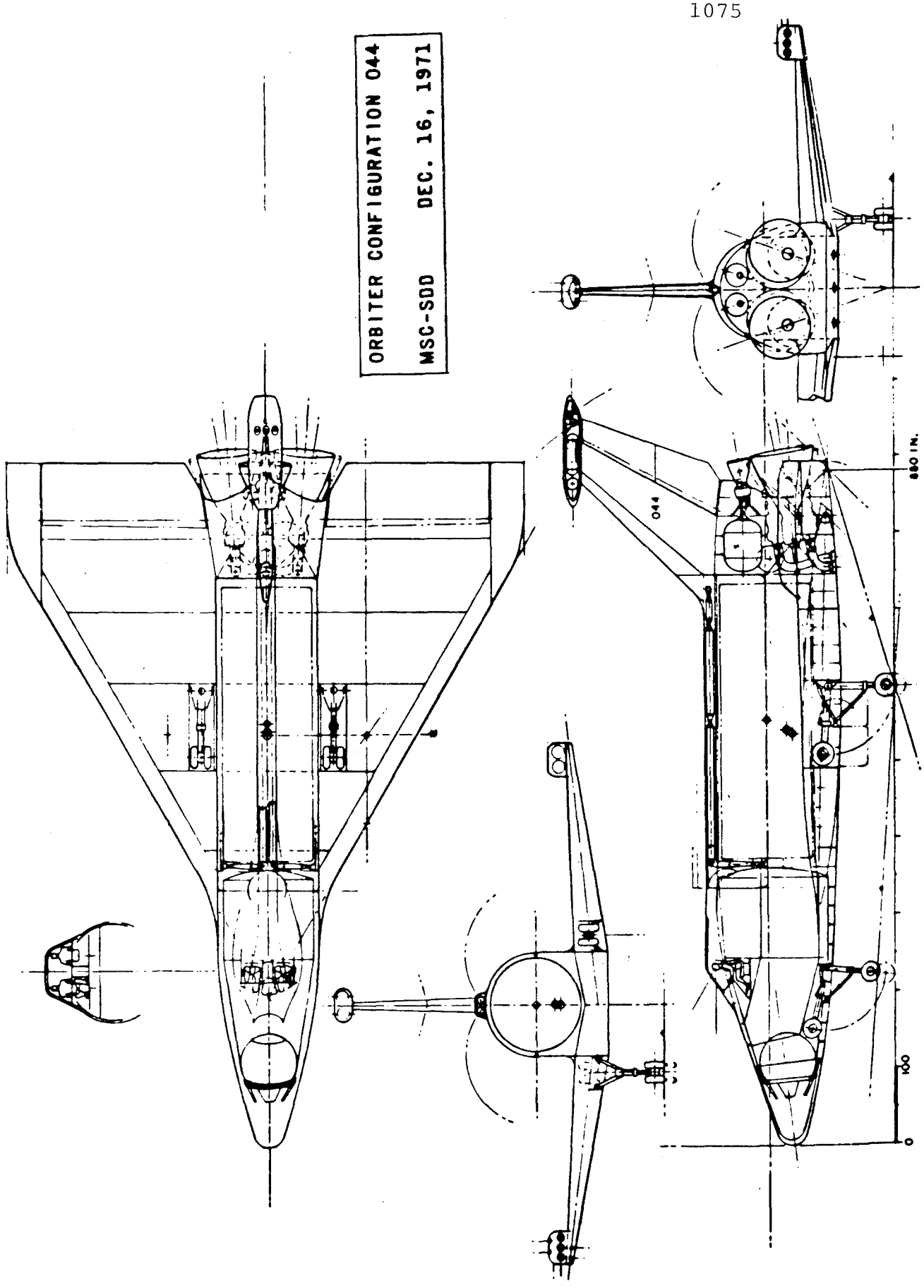

ORBITER CONFIGURATION 044
MSC-SDD DEC. 16, 1971
044
880 IN.
400
0

Figure 89

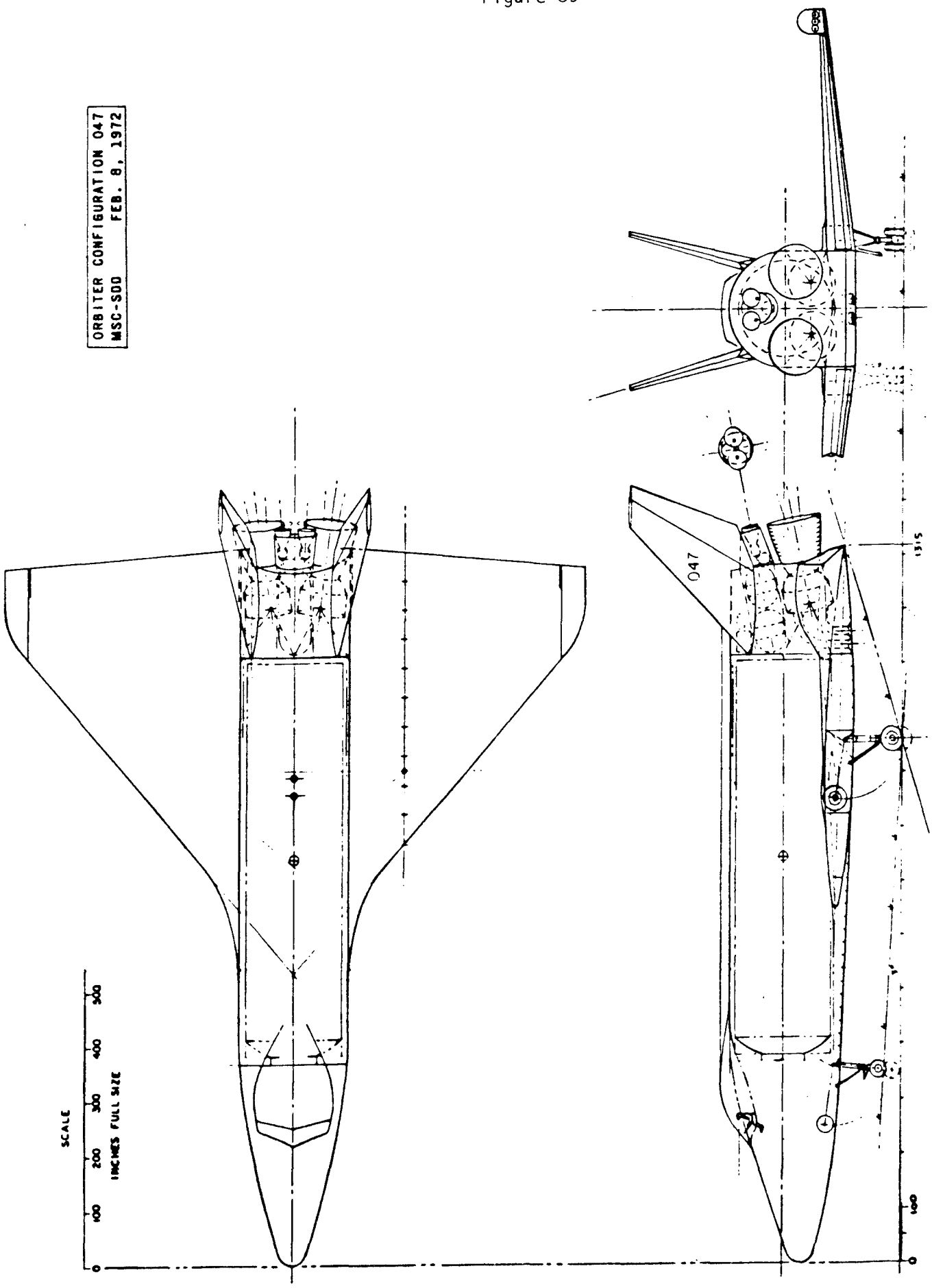

ORBITER CONFIGURATION 047
MSC-SDD FEB. 8, 1972
047
SCALE
INCHES FULL SIZE
0
100
200
300
400
500

Figure 90
Orbiter 048

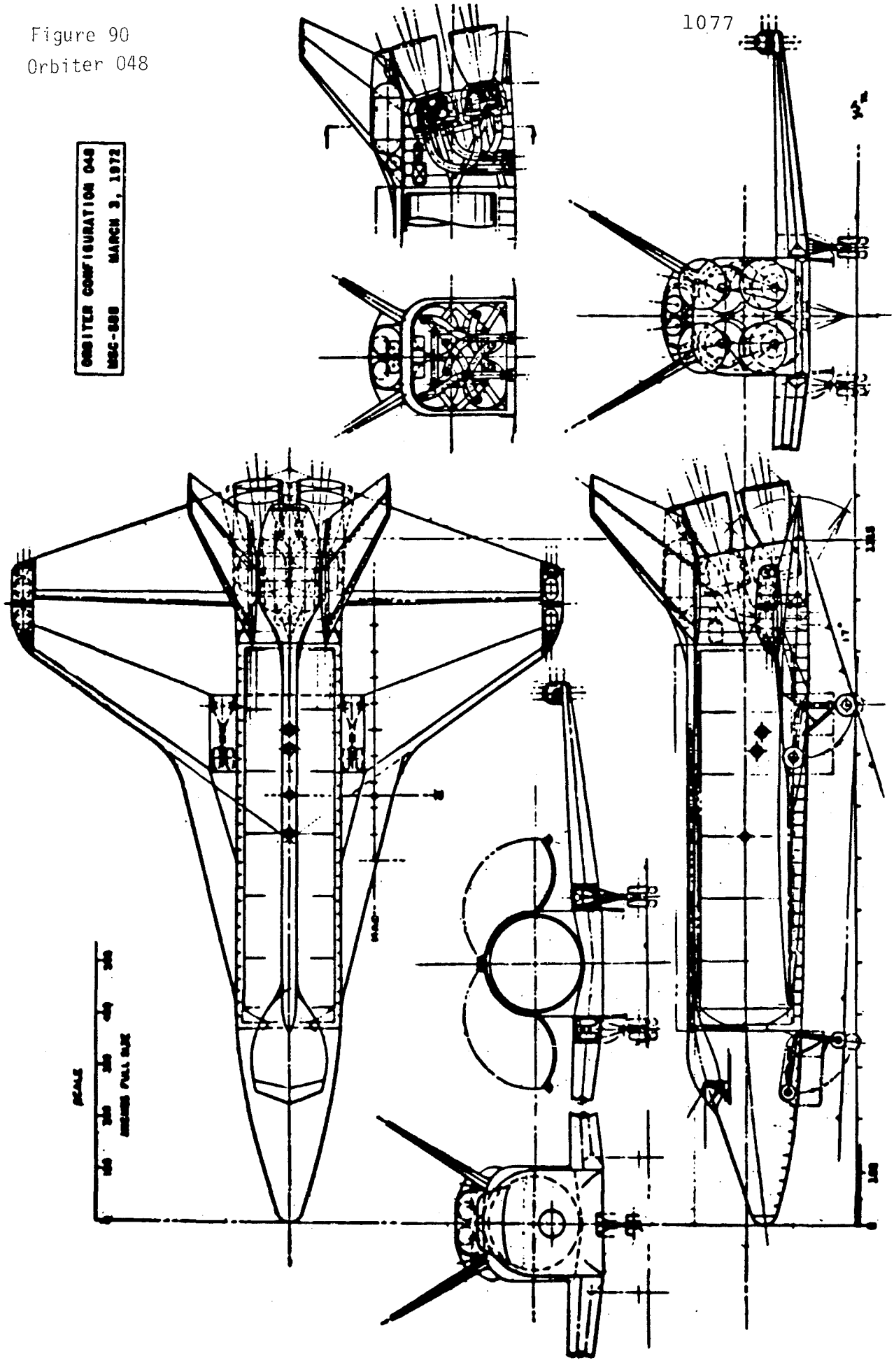


Figure 91

Orbiter 048A

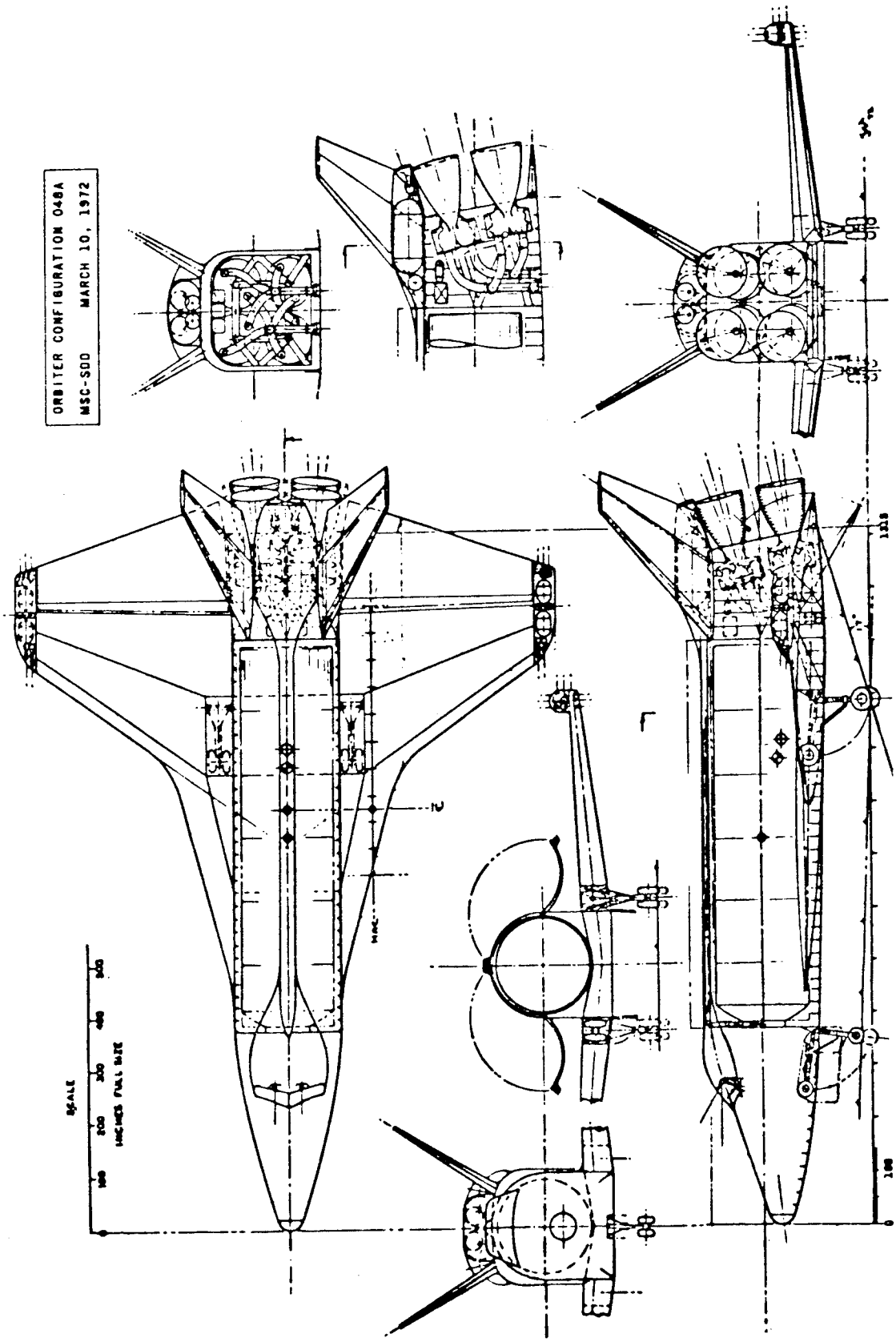


Figure 92
Orbiter 049

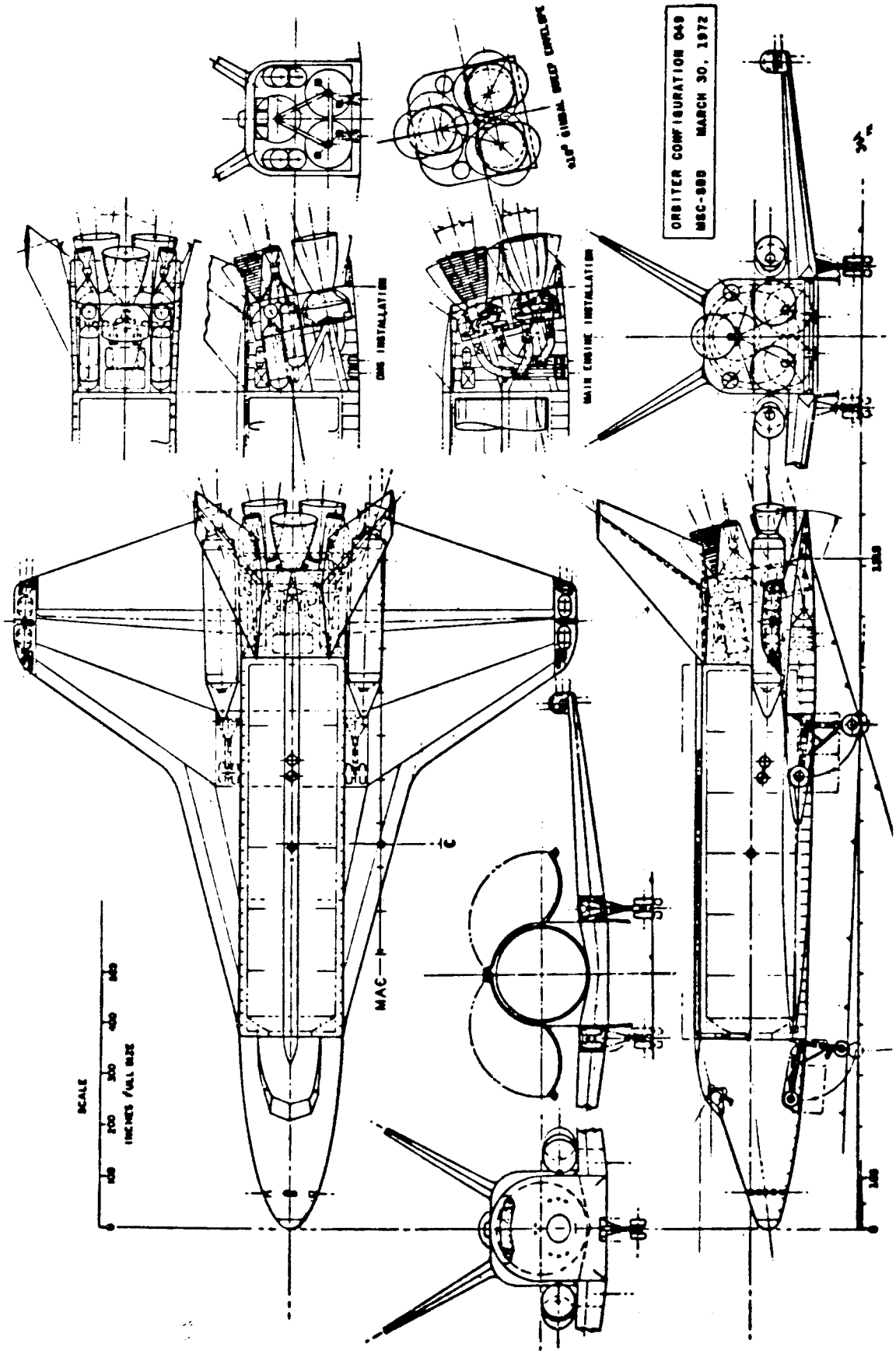
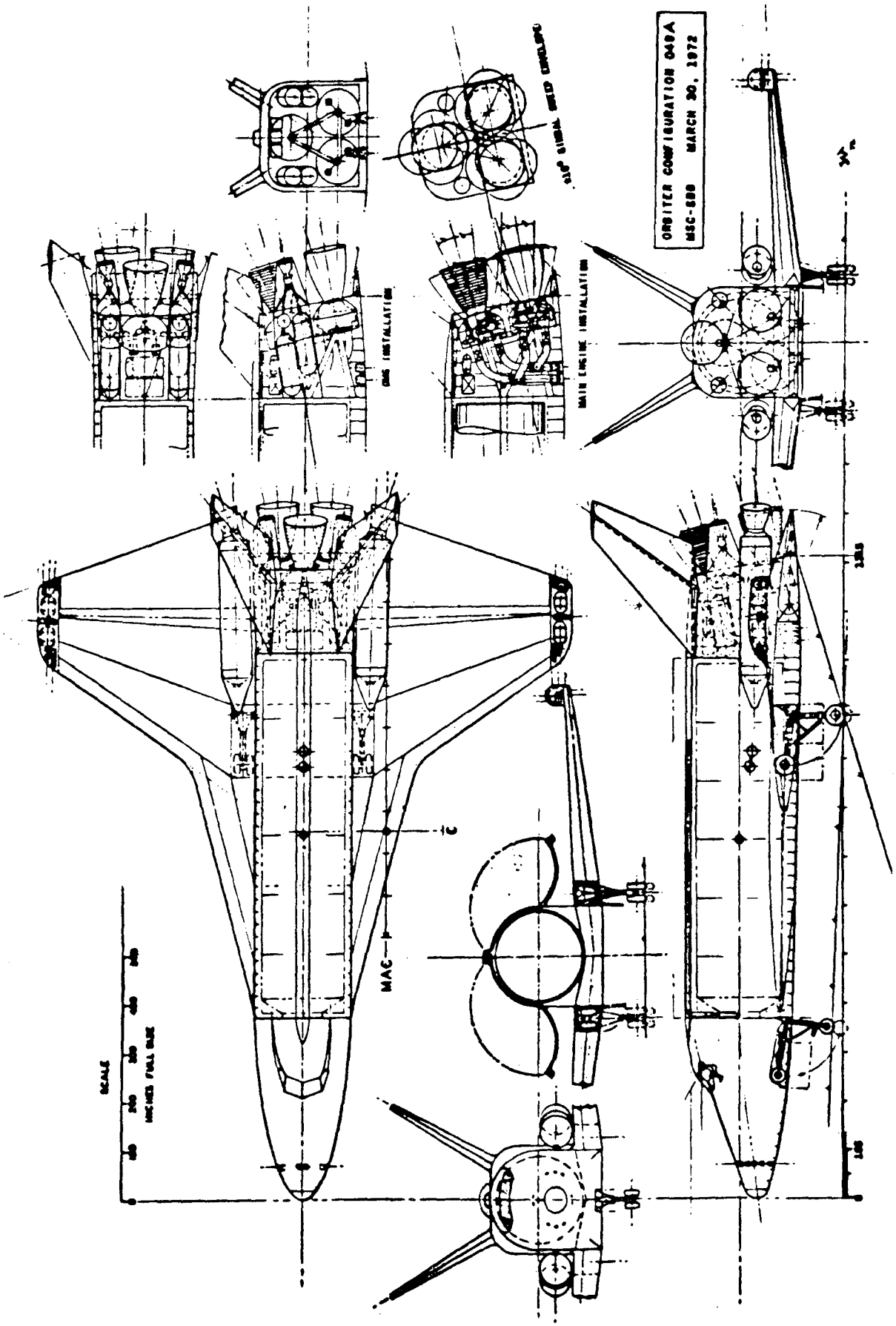


Figure 93

Orbiter 049A



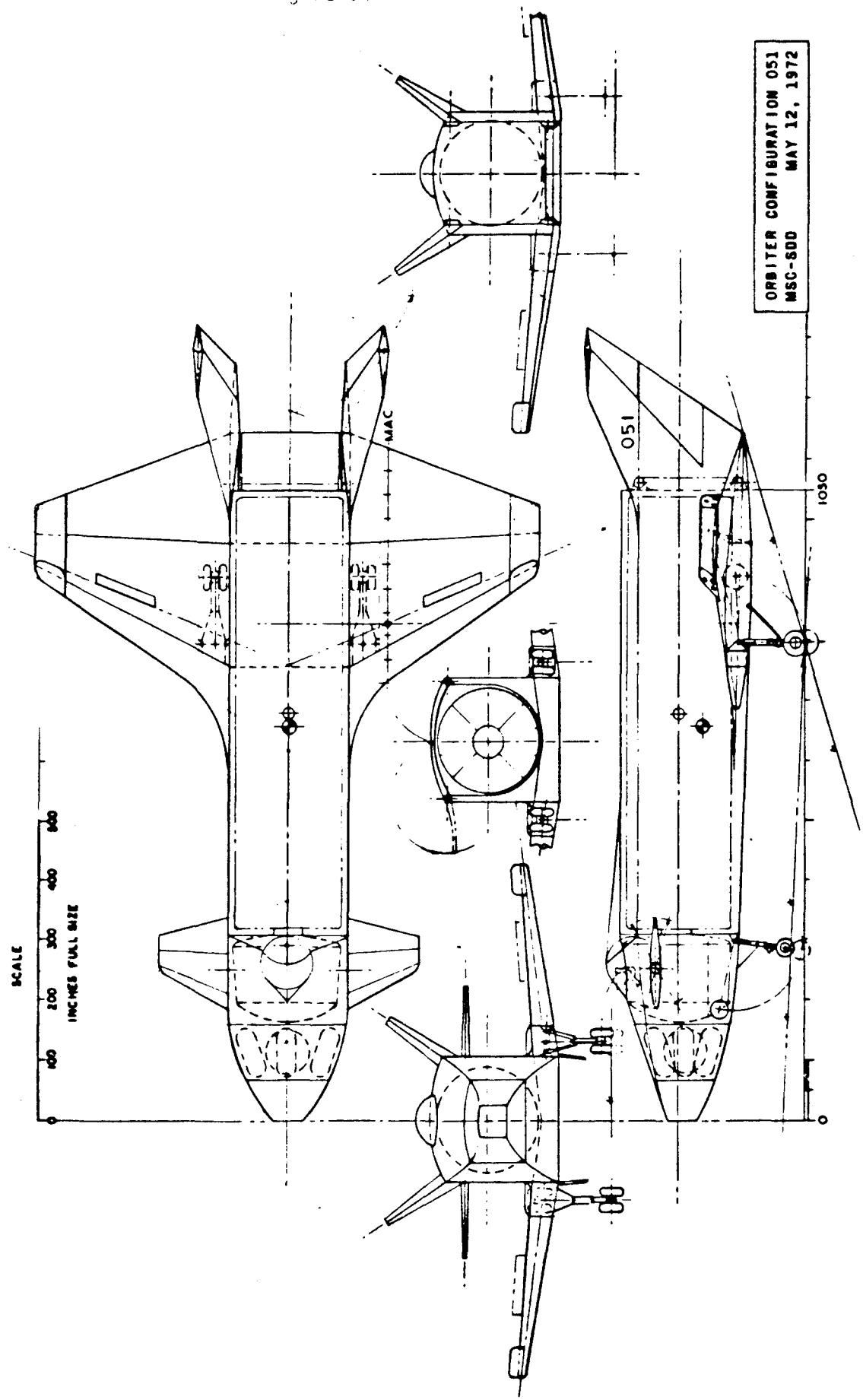

ORBITER CONFIGURATION 051
MSC-SDD MAY 12, 1972
051
MAC
1030
0
SCALE
INCHES FULL SIZE
0
100
200
300
400
500

Figure 95

Orbiter 052

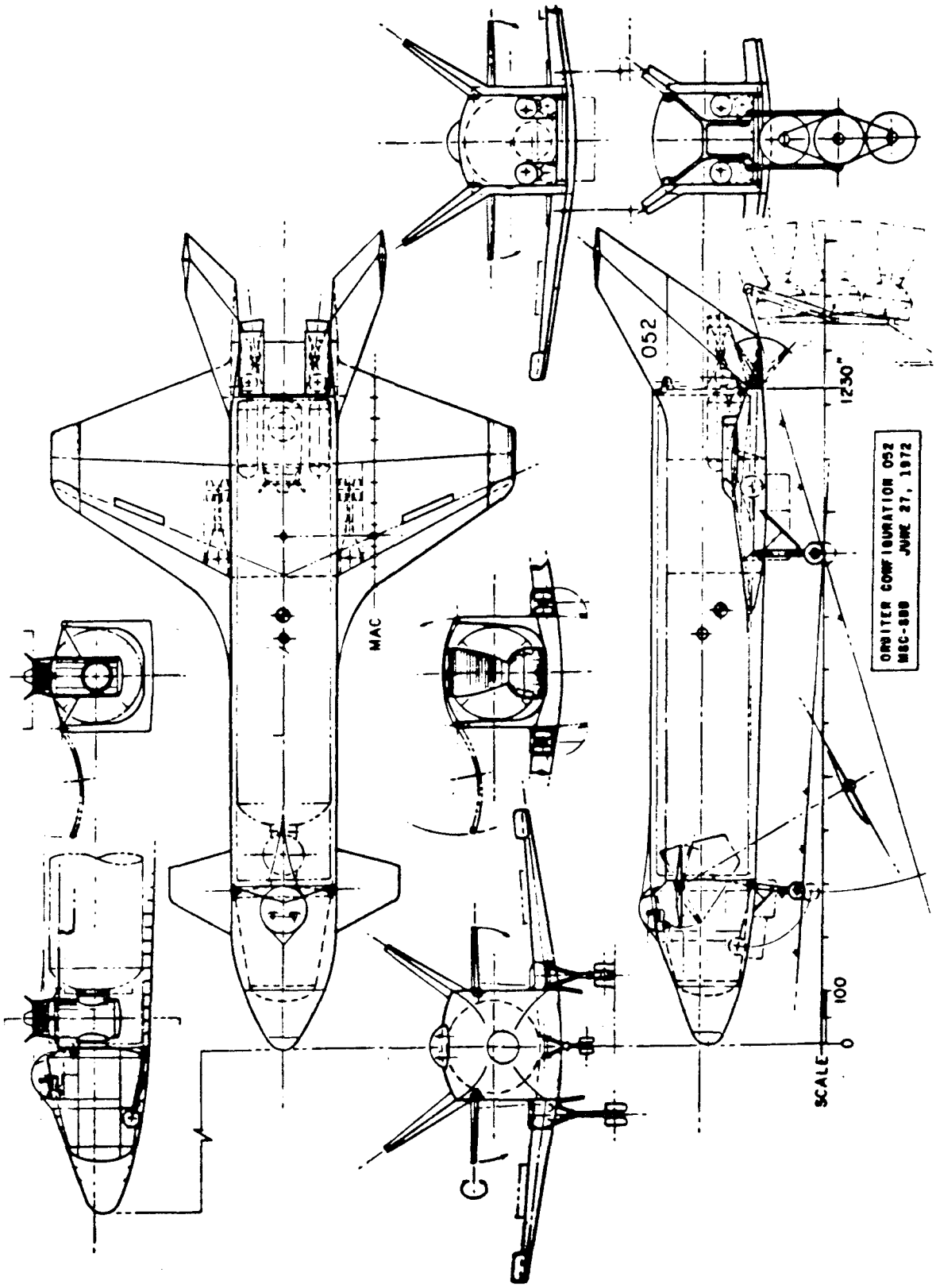


Figure 96

DETAILS OF SWING ENGINE INSTALLATION OF ORBITER 052

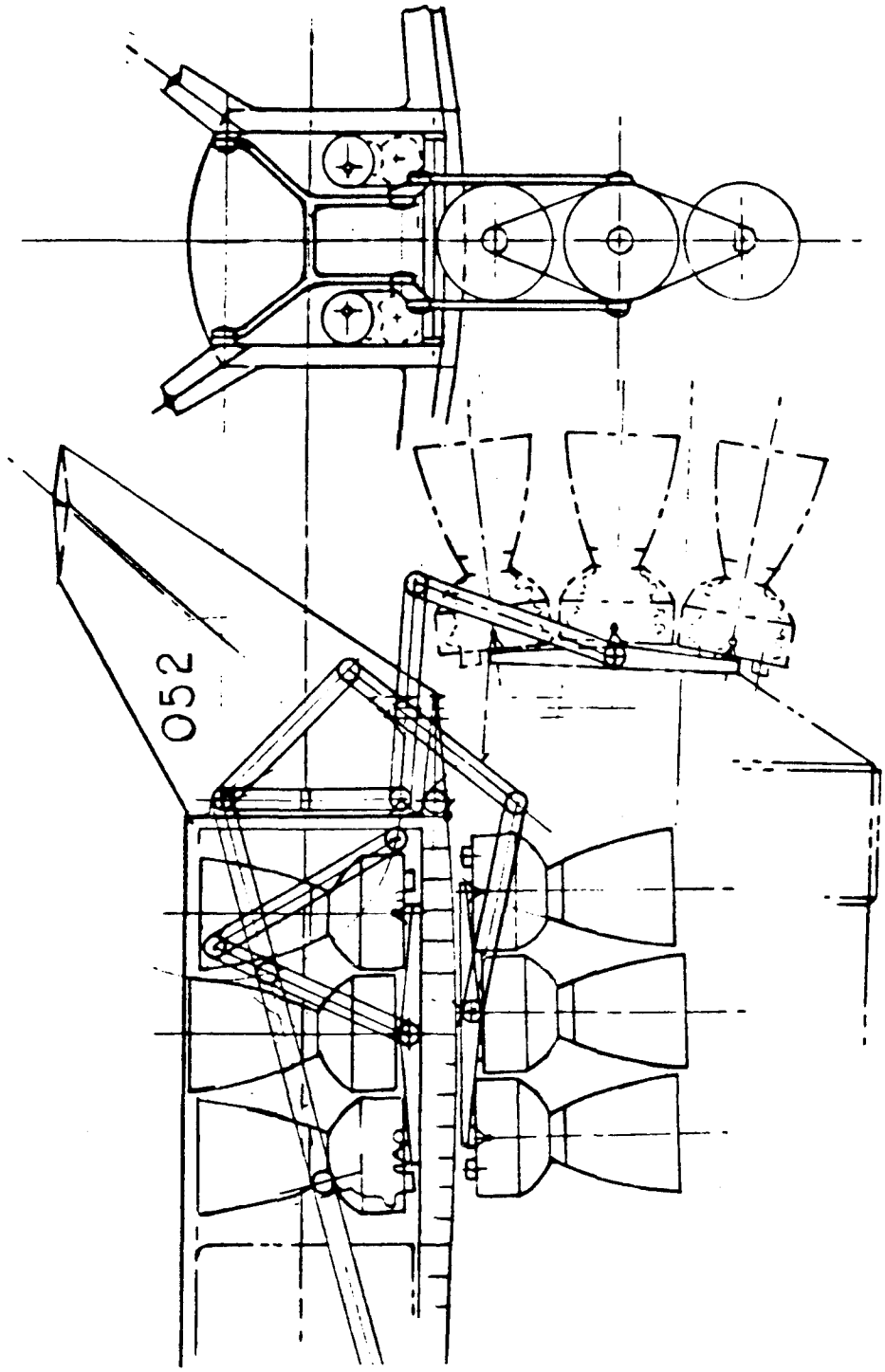


Figure 97

Orbiter 053

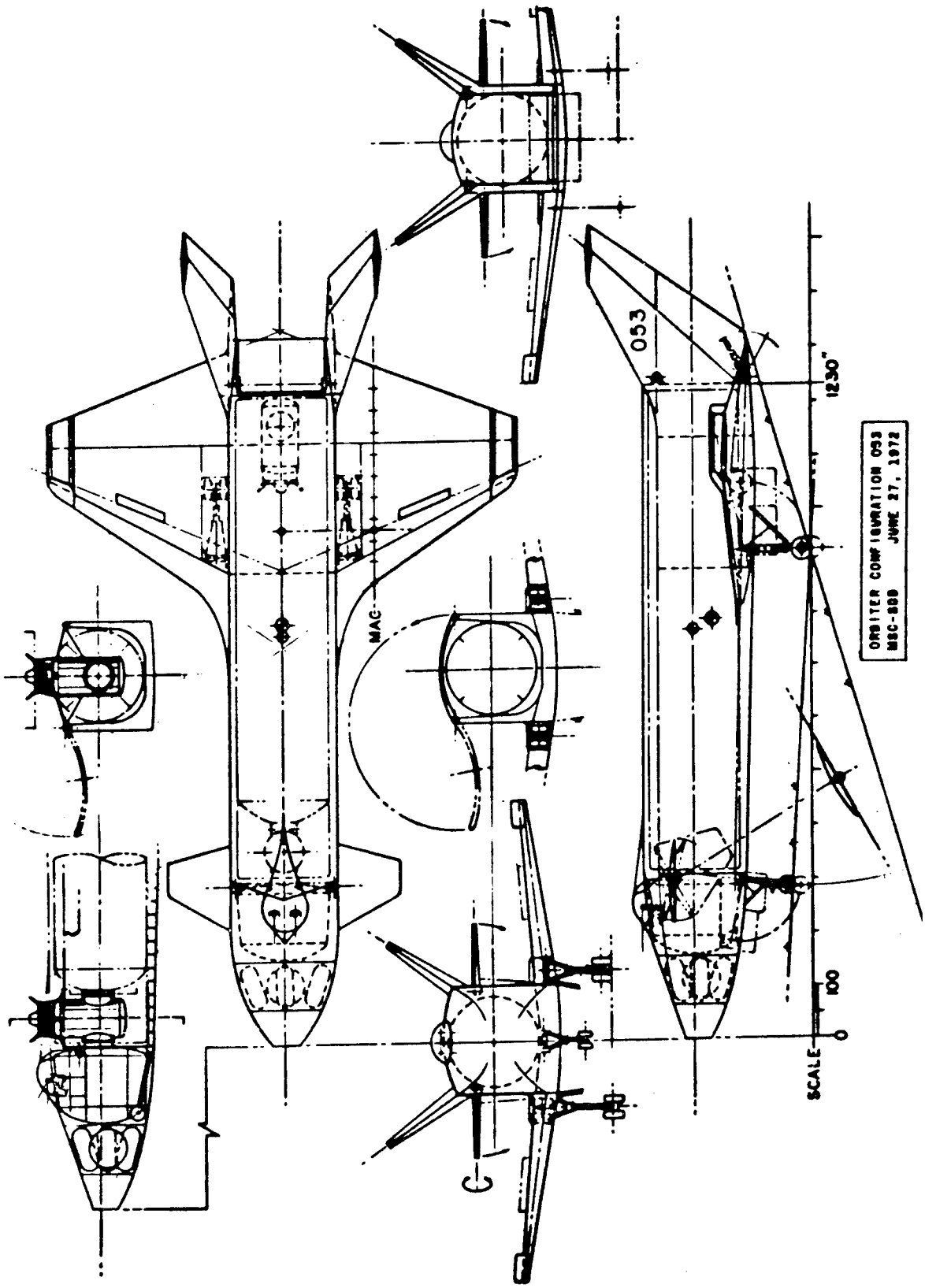
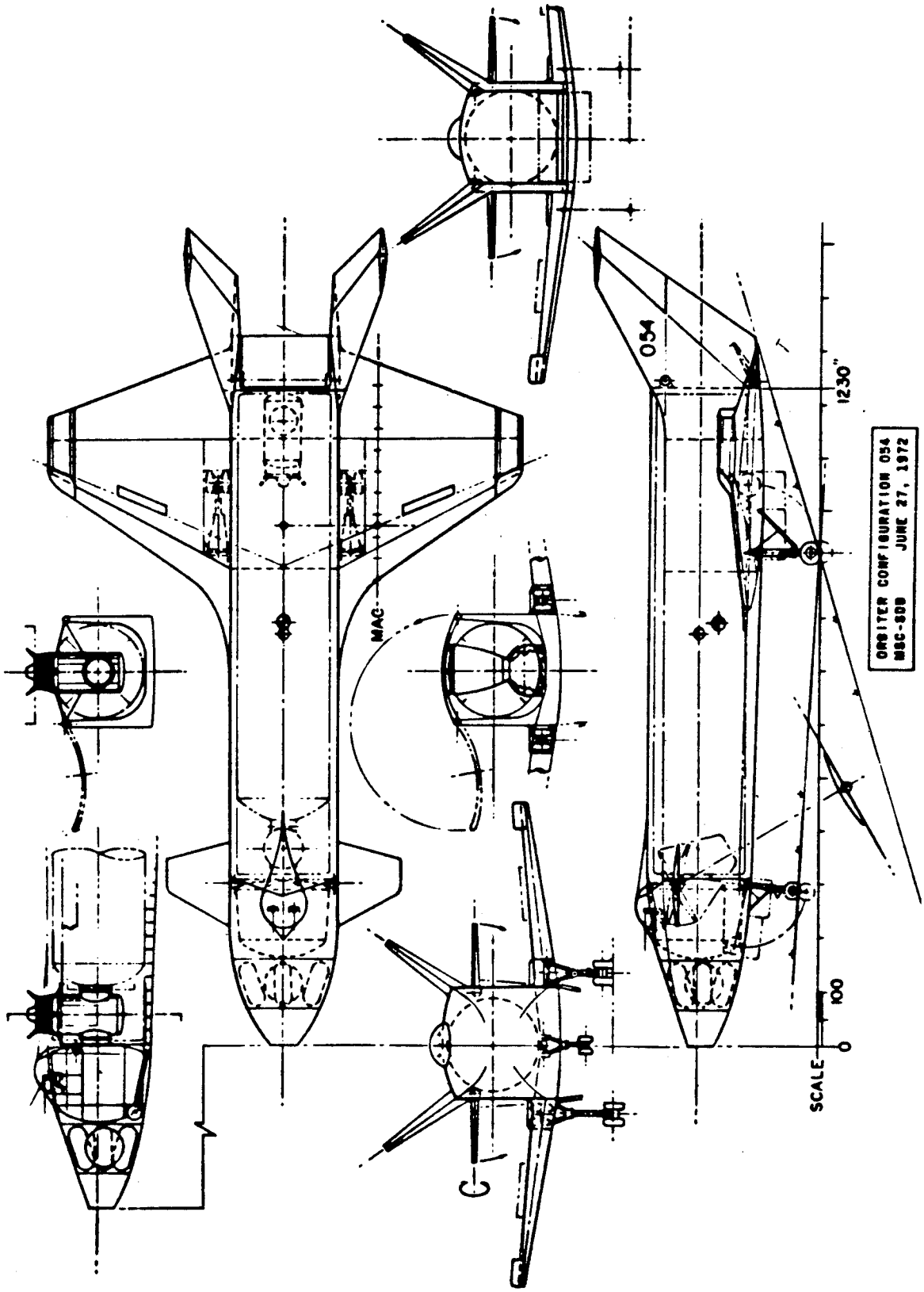


Figure 98

Orbiter 054



MSC Shuttle configurations were anticlimatic: A perusal of Figures 84-98 show that creativity still abounded at MSC, with generation of twin-tail variations on the 040C, canard concepts, and even Shuttles having so-called "swing" or retractable engines (Figures 95-96). But like a pendulum swinging from one extreme to another, NASA's design work ranged from the deficiencies of the Faget orbiter to the complexities and uncertainties of complex canard-deltas. In between was the logical purity of the 040C.[24]

Design of the orbiter, of course, was but one critical task. The other was the external configuration--the entire vehicle, including the orbiter and any external propulsion or tankage. After one last lingering look at a fly-back booster (Orbiter 026, shown in Figure 99, and dating to May 1971), SSD's staff concentrated on concepts involving liquid and solid-fuel propulsion elements involving so-called "parallel-burn" approaches whereby the Shuttle and its booster system would take off with both booster and Shuttle firing. The external configuration that most often appeared were sequential stages, with the Shuttle riding "piggyback," or stage-and-a-half concepts with the Shuttle riding a fuel tank flanked by some sort of solid-rocket boosters. Orbiter configuration 040C, (Figure 109) dating to February 18, 1972, had the now-typical external fuel tank flanked by two 156 in. diameter solid-rocket motors, with the Shuttle riding the stack. The external tank and SRB layout differed appreciably from the present Shuttle, the SRB's nestled close between the tank and the wing of the orbiter, and the tank sporting two large stabilization fins. Nevertheless this layout is clearly the progenitor of the present Shuttle configuration, and, as a review of the vehicle concepts from 040C onwards indicates, it became the standard design configuration for the remaining Shuttle duties that NASA executed over the remaining few months. Figures 99-123 show the range of booster/fuel tank combinations studied for the various delta configurations examined by the SSD.

Figure 99

Orbiter 023

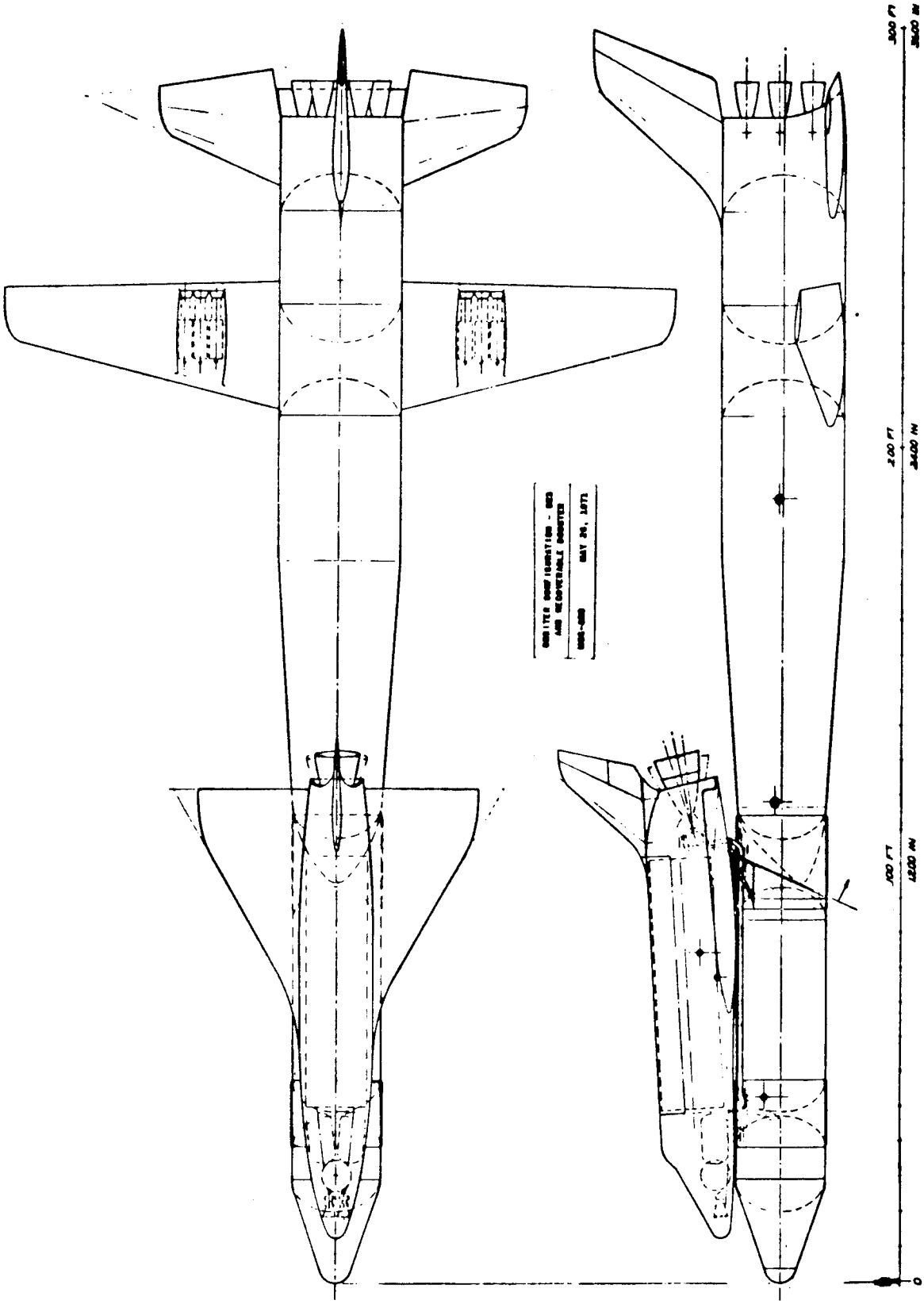


Figure 100

Orbiter 027

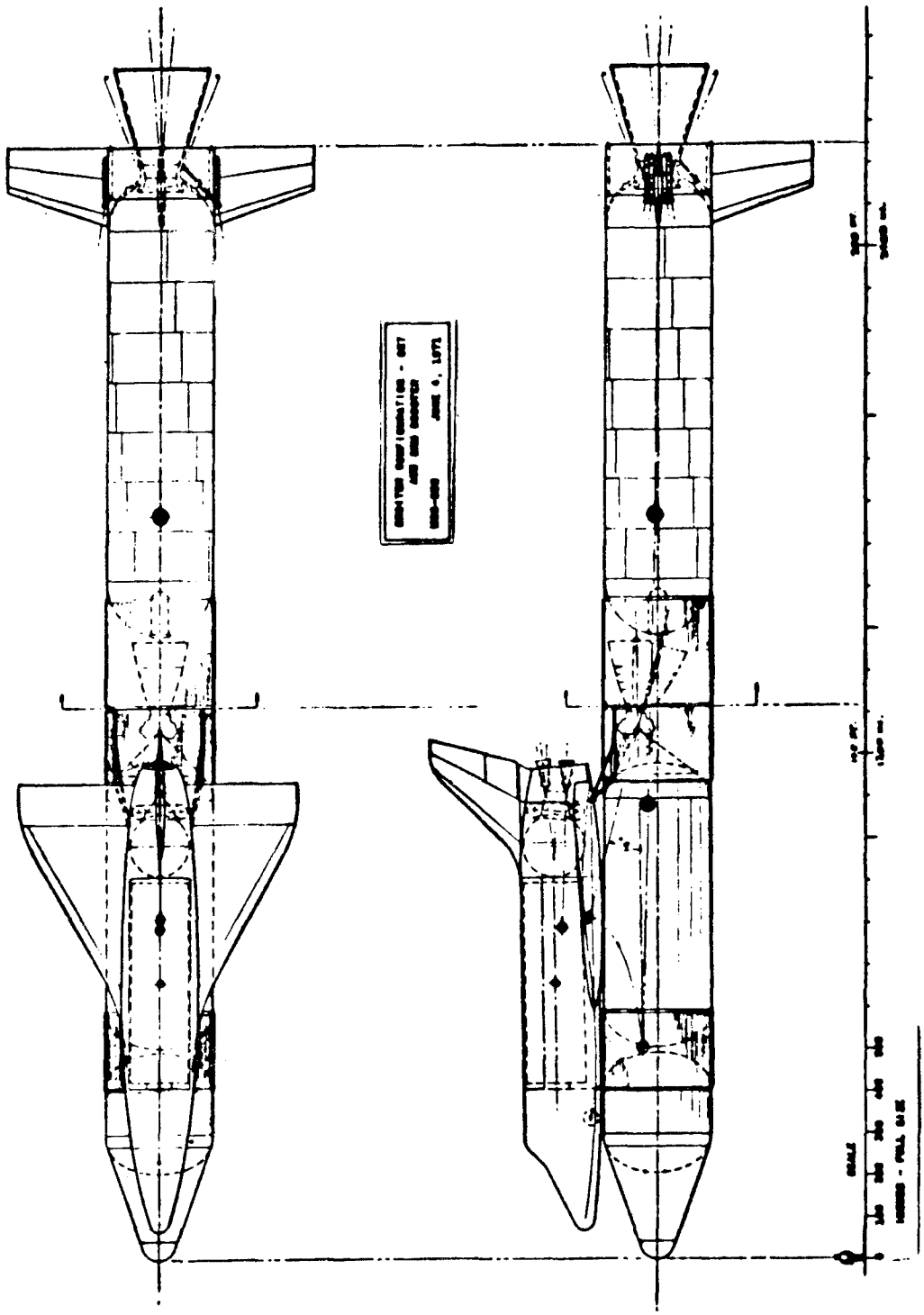


Figure 101

Orbiter 032

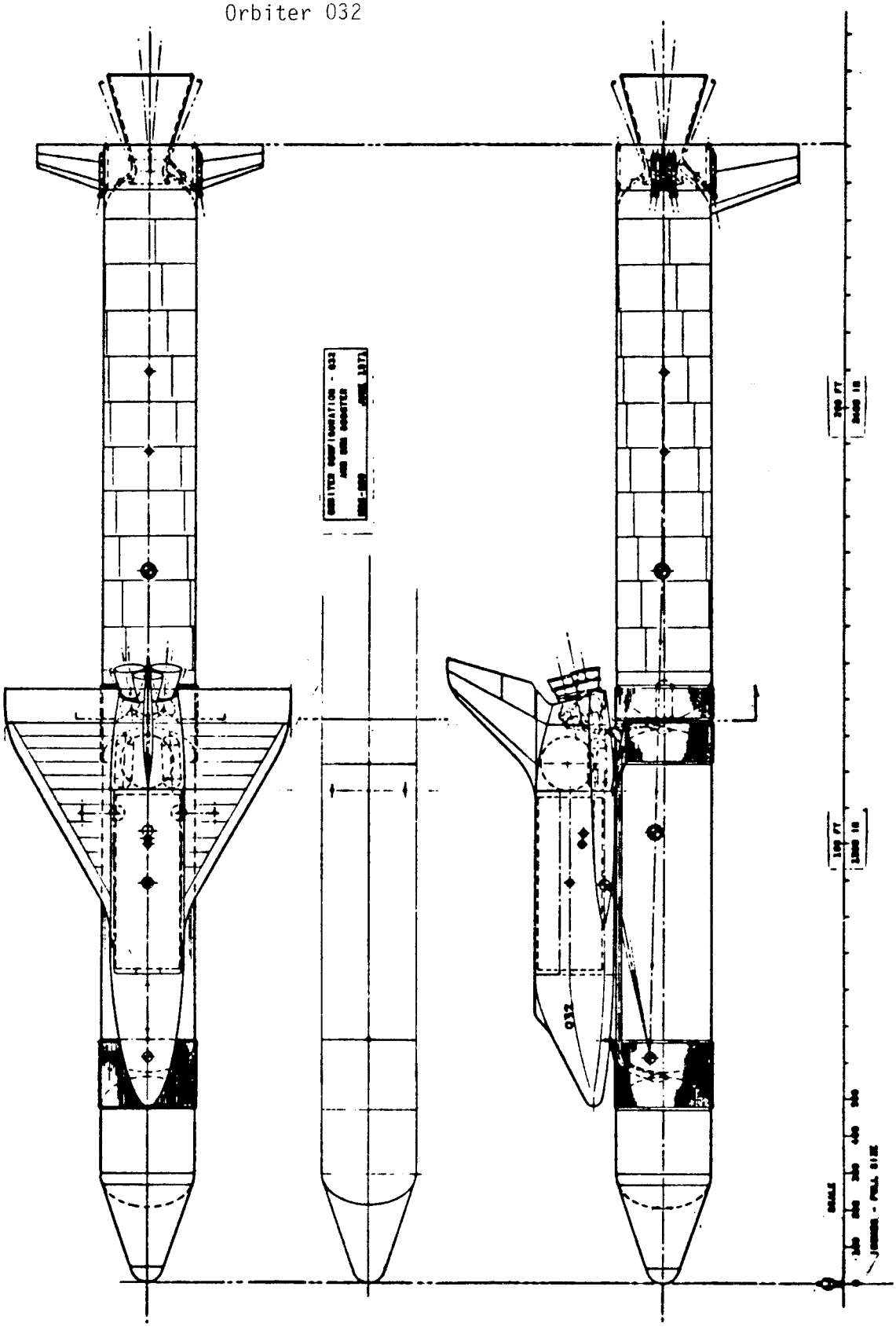


Figure 102

Orbiter 033

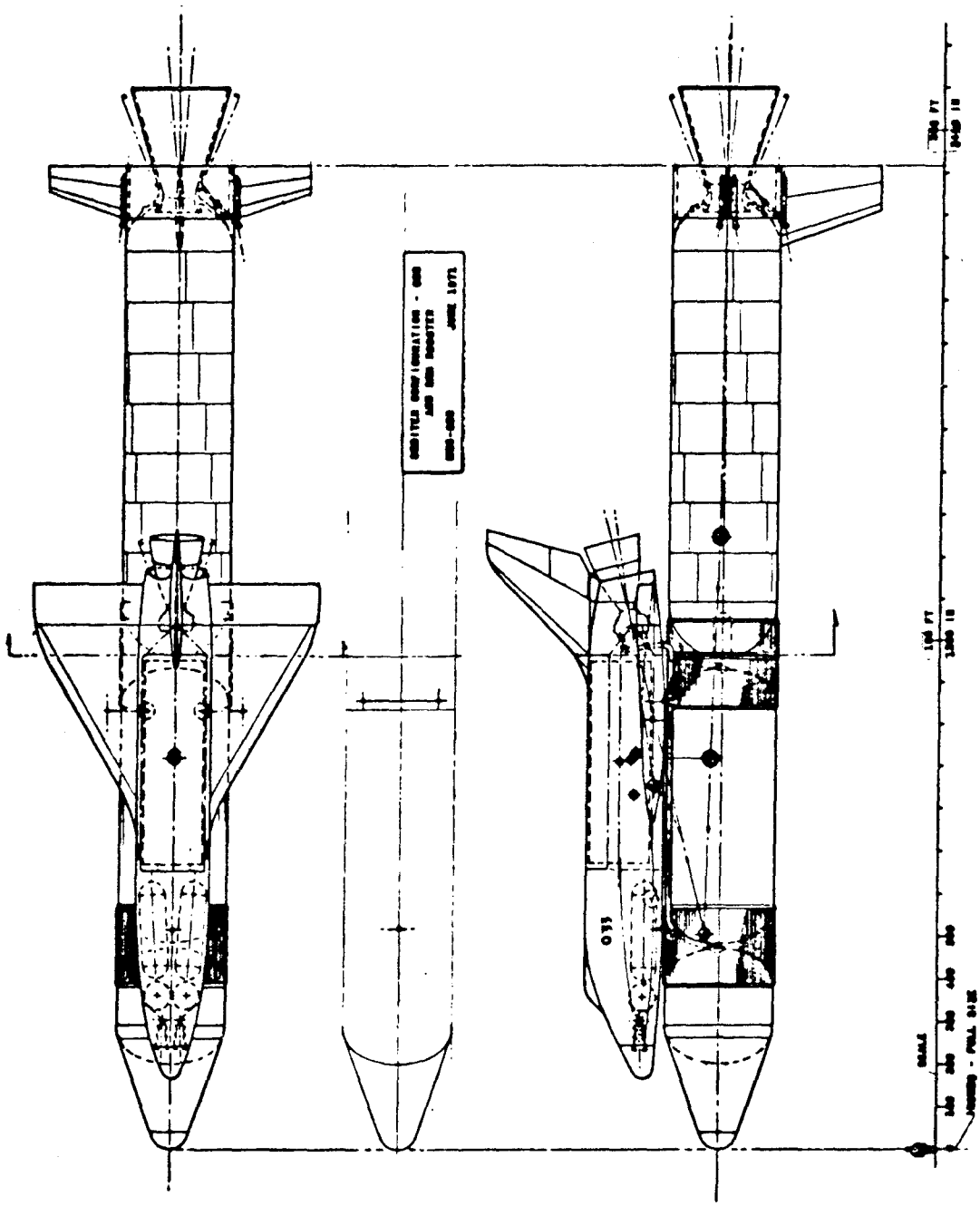


Figure 103

Orbiter 034

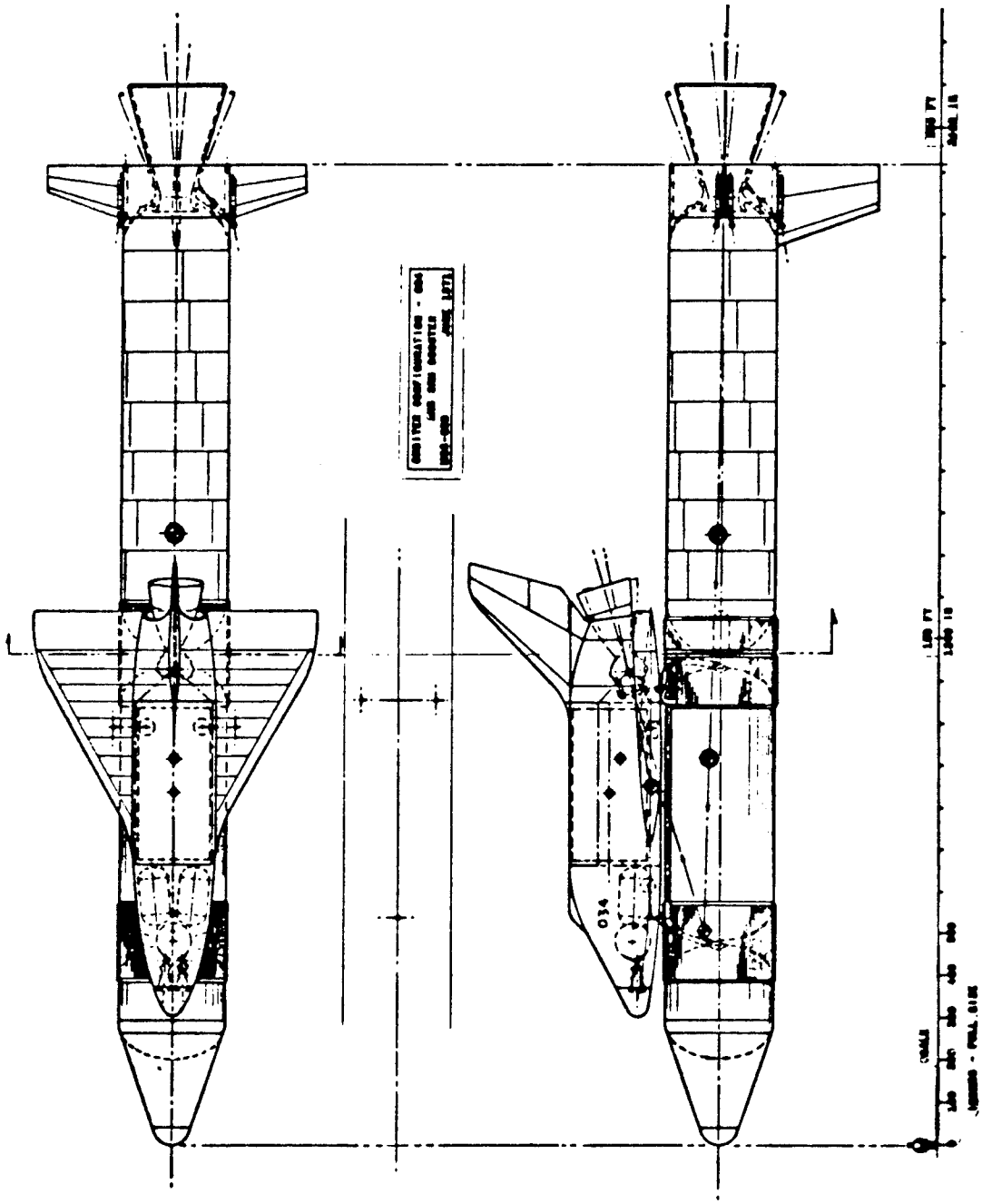


Figure 104

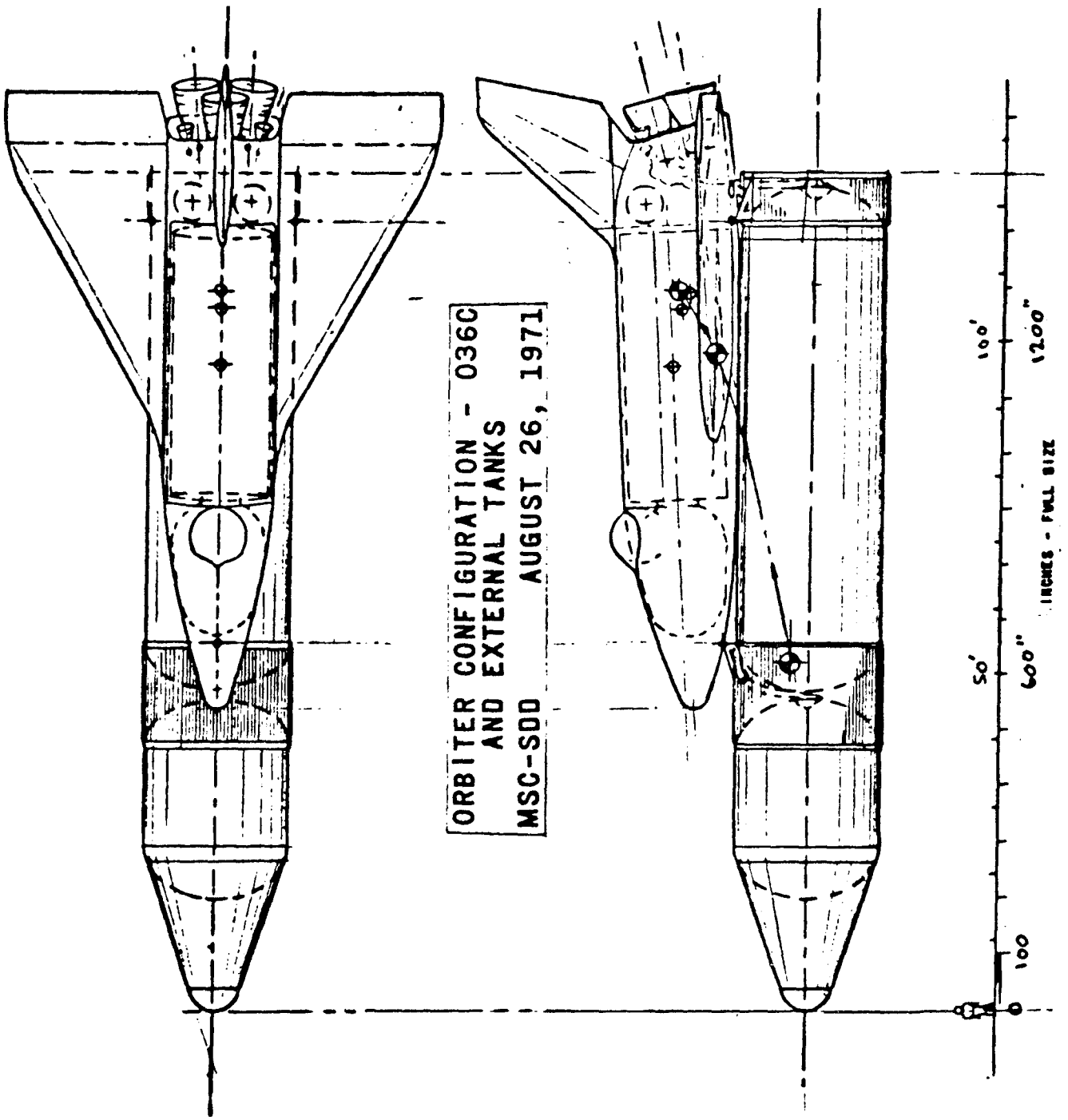

ORBITER CONFIGURATION - 036C
AND EXTERNAL TANKS
MSC-SDD AUGUST 26, 1971
100'
1200"
50'
600"
100
INCHES - FULL SIZE

Figure 105

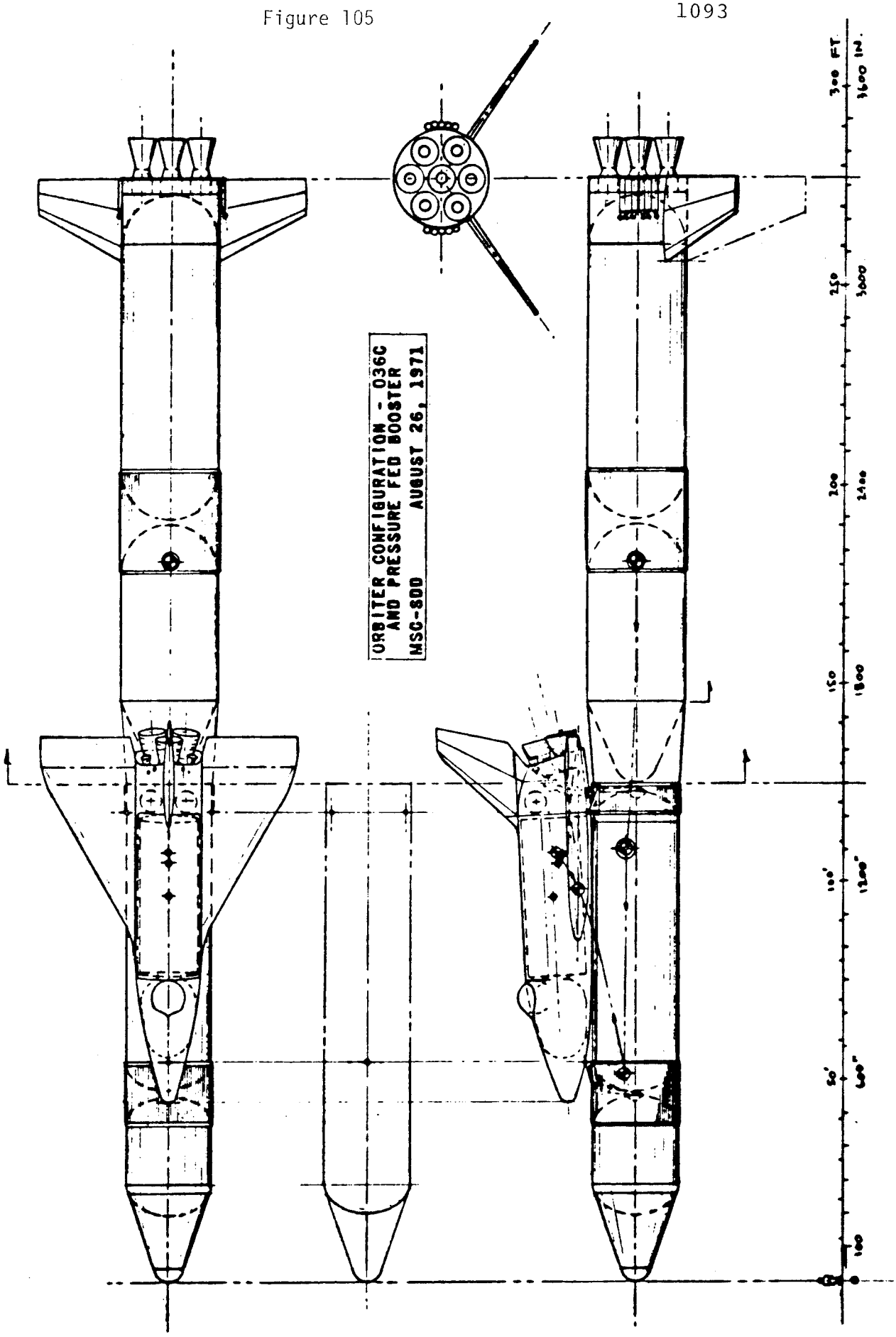

ORBITER CONFIGURATION - 036C
AND PRESSURE FED BOOSTER
MSC-800 AUGUST 26, 1971

Figure 106

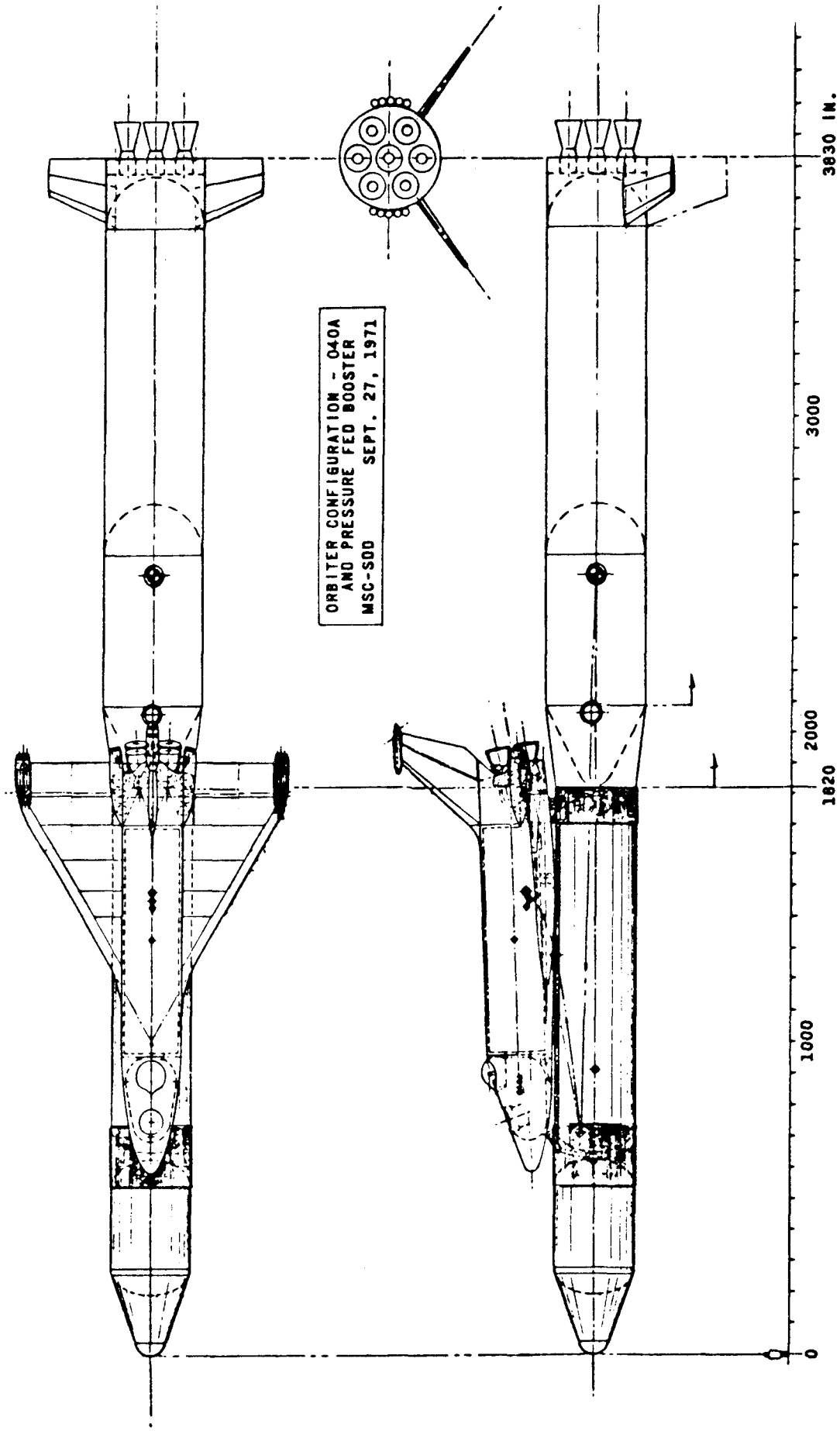

ORBITER CONFIGURATION - 040A
AND PRESSURE FED BOOSTER
MSC-SOD SEPT. 27, 1971
3830 IN.
3000
2000
1820
1000
0

Figure 107

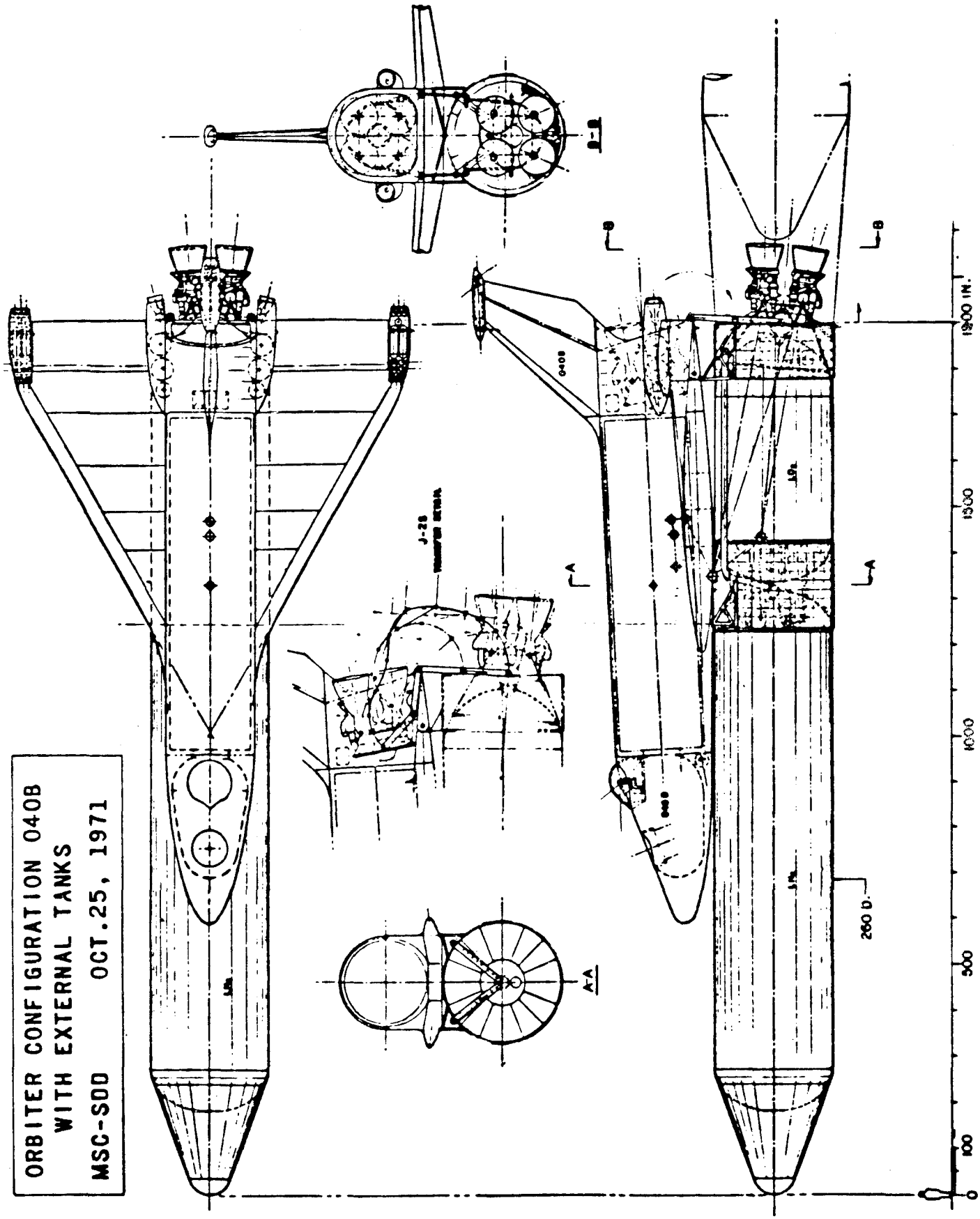

ORBITER CONFIGURATION 040B
WITH EXTERNAL TANKS
MSC-SDD OCT.25, 1971
A-A
B-B
260 D.
1900 IN.
1500
1000
500
100
0

Figure 108

"Swing" engine concept for Orbiter 040B

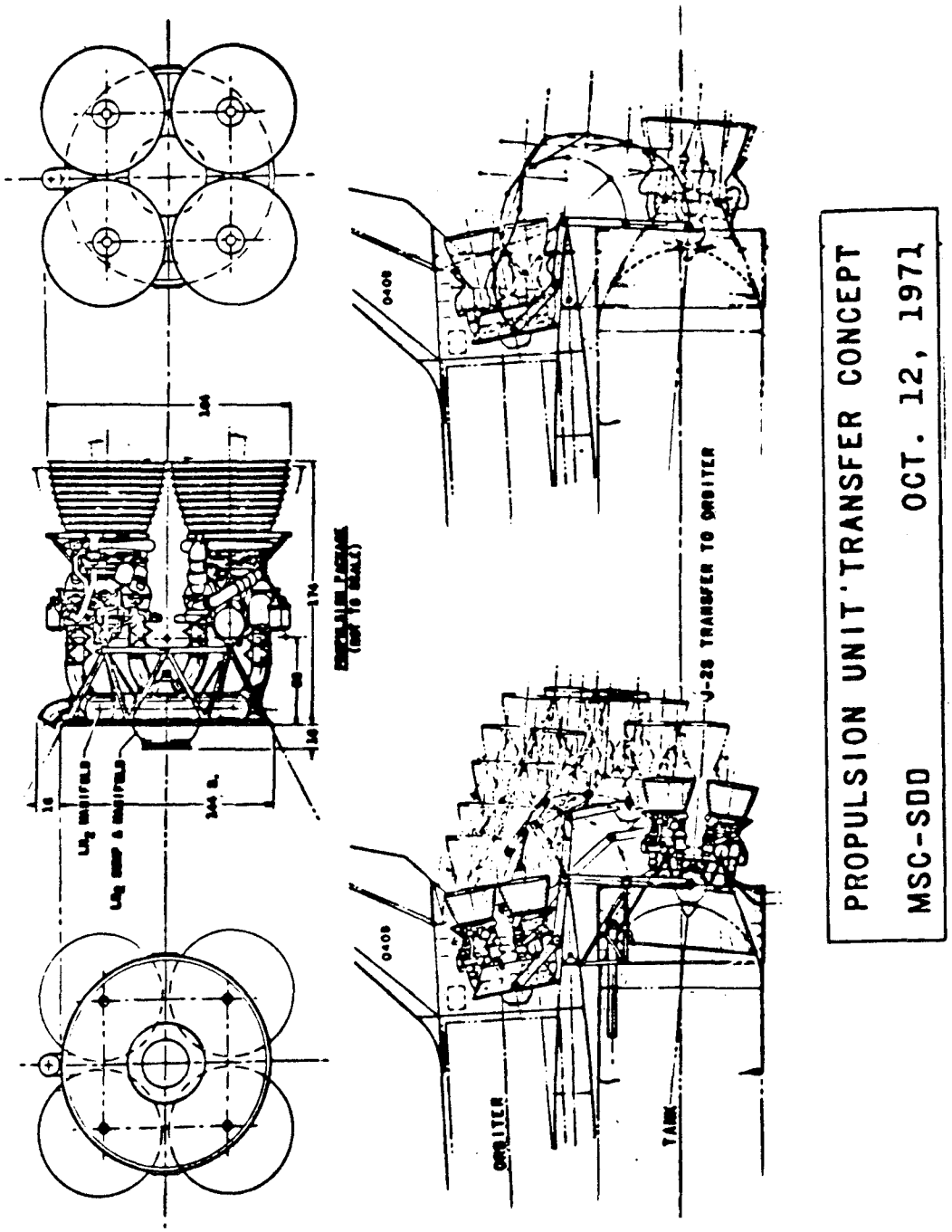


Figure 109

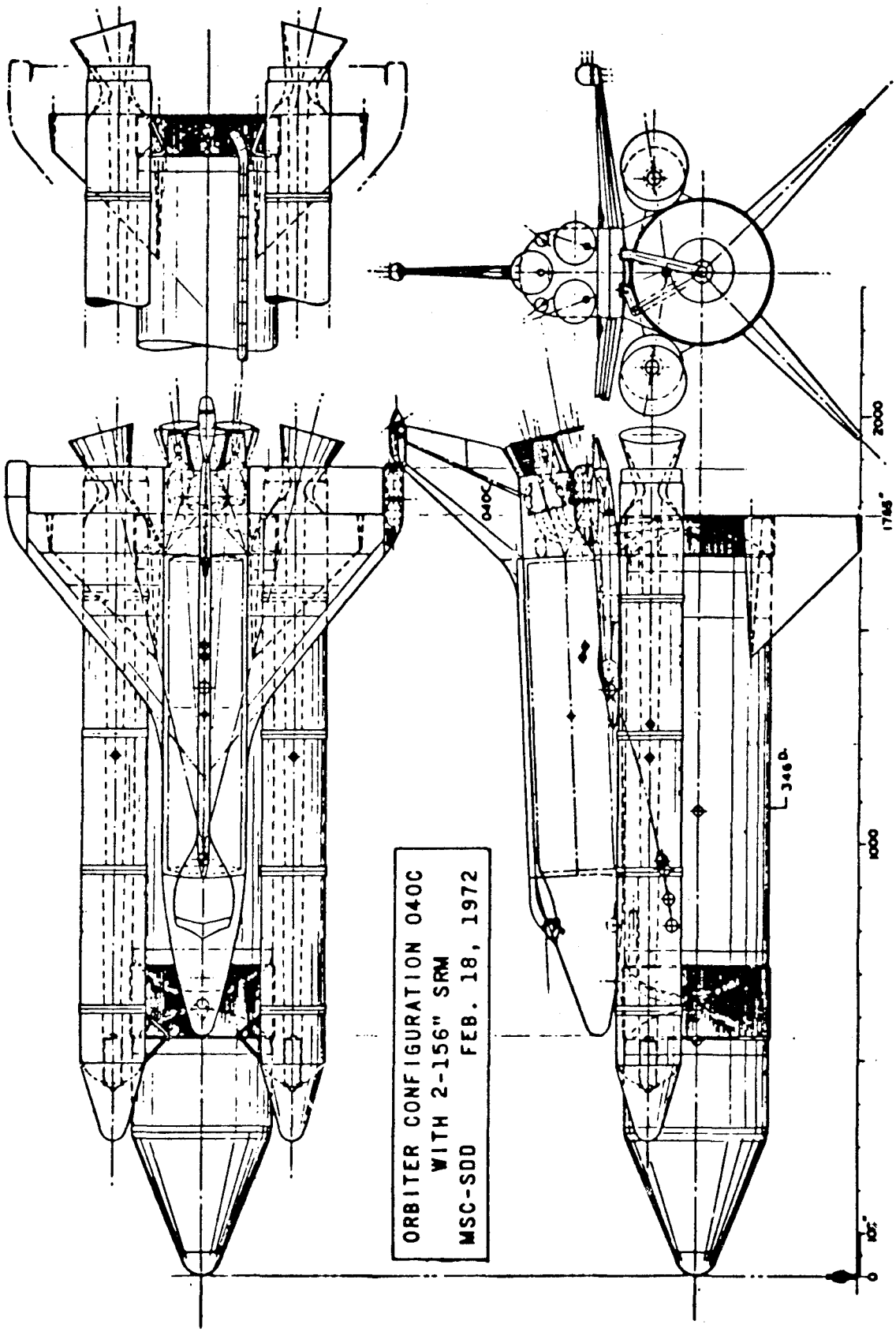

ORBITER CONFIGURATION 040C
WITH 2-156" SRM
MSC-SDD FEB. 18, 1972
040C
2000
1000
0

Figure 110

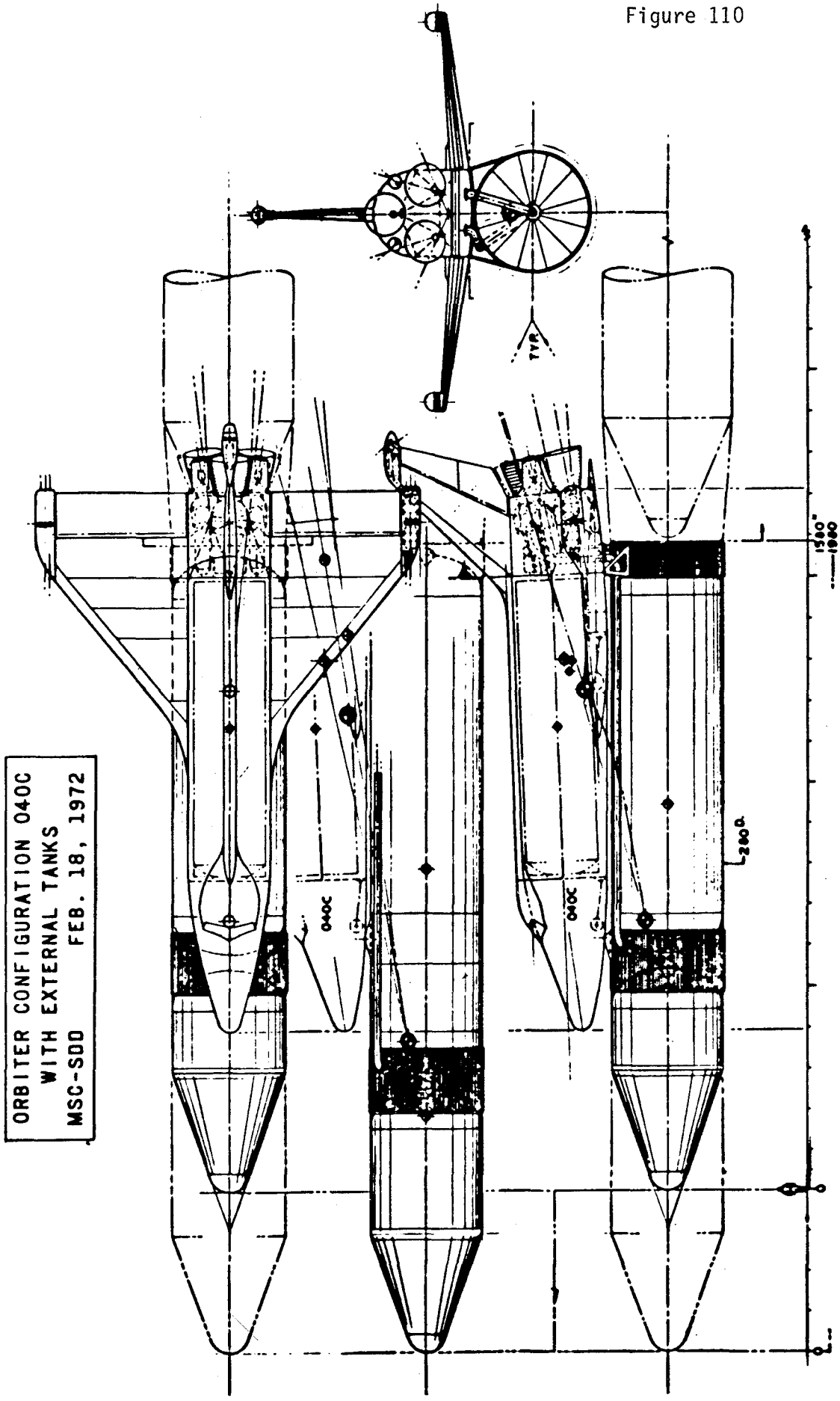

ORBITER CONFIGURATION 040C
WITH EXTERNAL TANKS
MSC-SDD FEB. 18, 1972
040C
040C
1500
1000
200

Figure 111

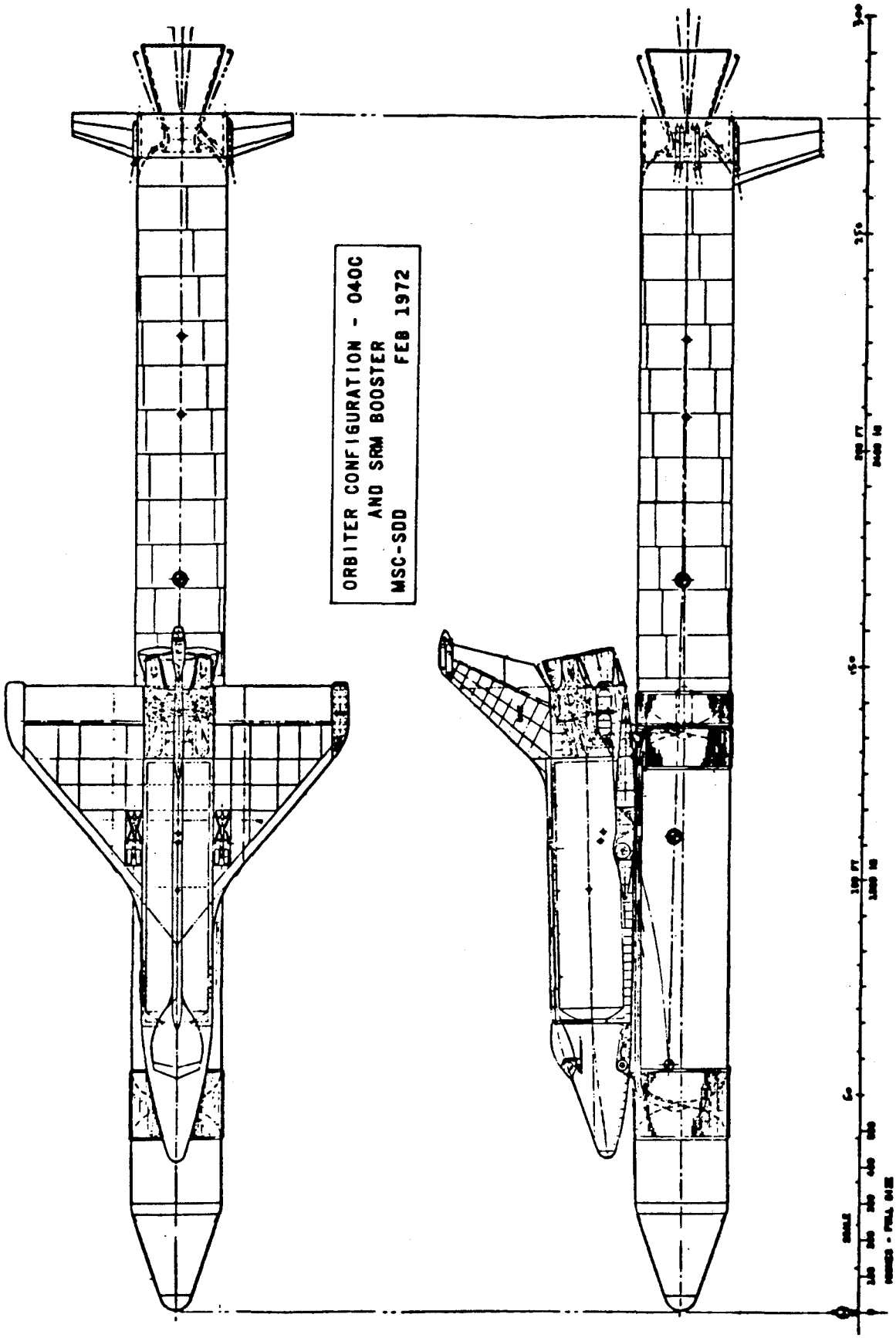

ORBITER CONFIGURATION - 040C
AND SRM BOOSTER
MSC-SDD
FEB 1972

Figure 112

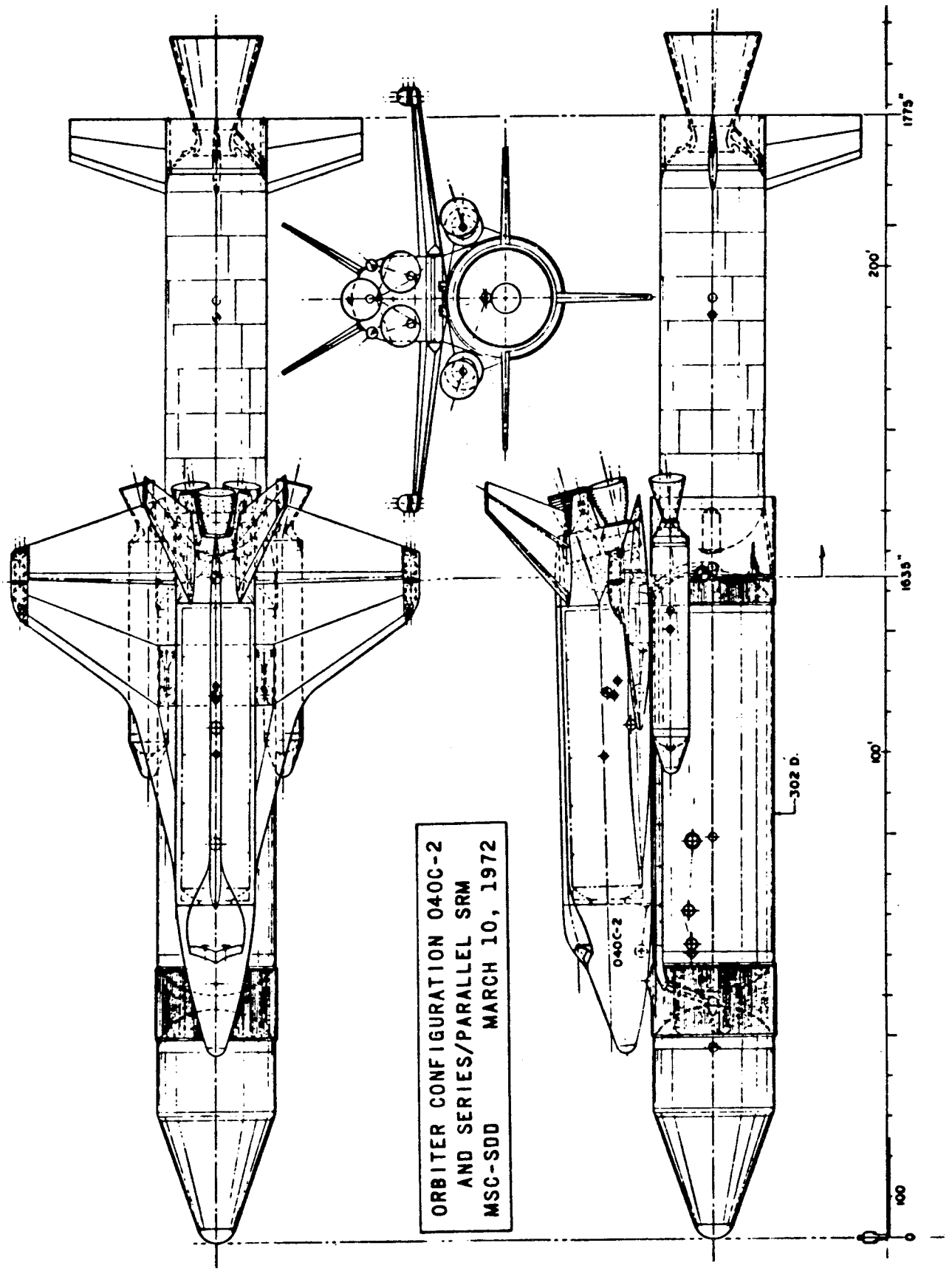

ORBITER CONFIGURATION 040C-2
AND SERIES/PARALLEL SRM
MSC-SDD MARCH 10, 1972
040C-2
302 D.
1775"
200'
1635"
100'
100
0

Figure 113

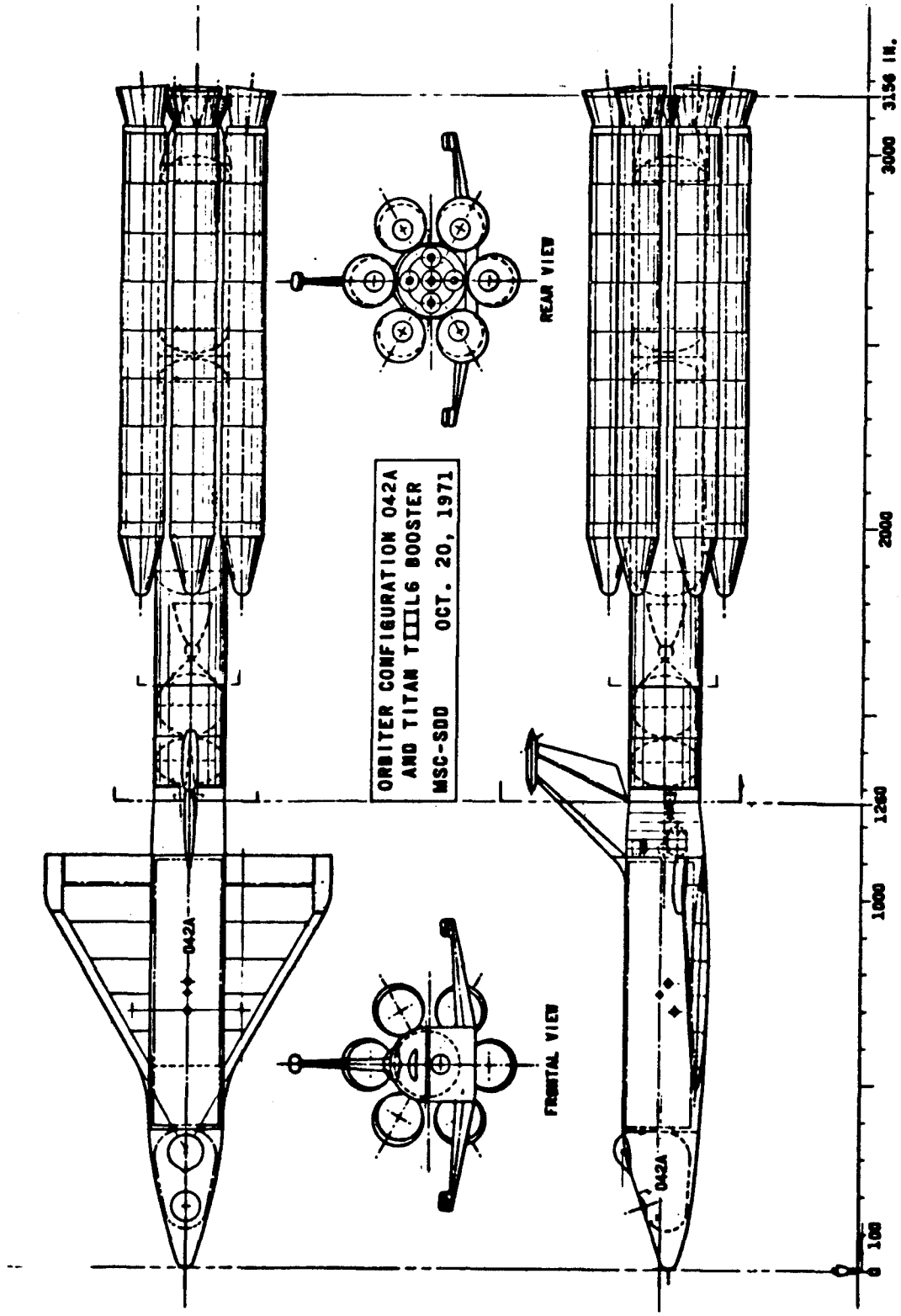

ORBITER CONFIGURATION 042A
AND TITAN T□□L6 BOOSTER
MSC-SDD OCT. 20, 1971
REAR VIEW
FRONTAL VIEW
042A
042A
3156 IN.
3000
2000
1250
1000
100
0

Figure 114

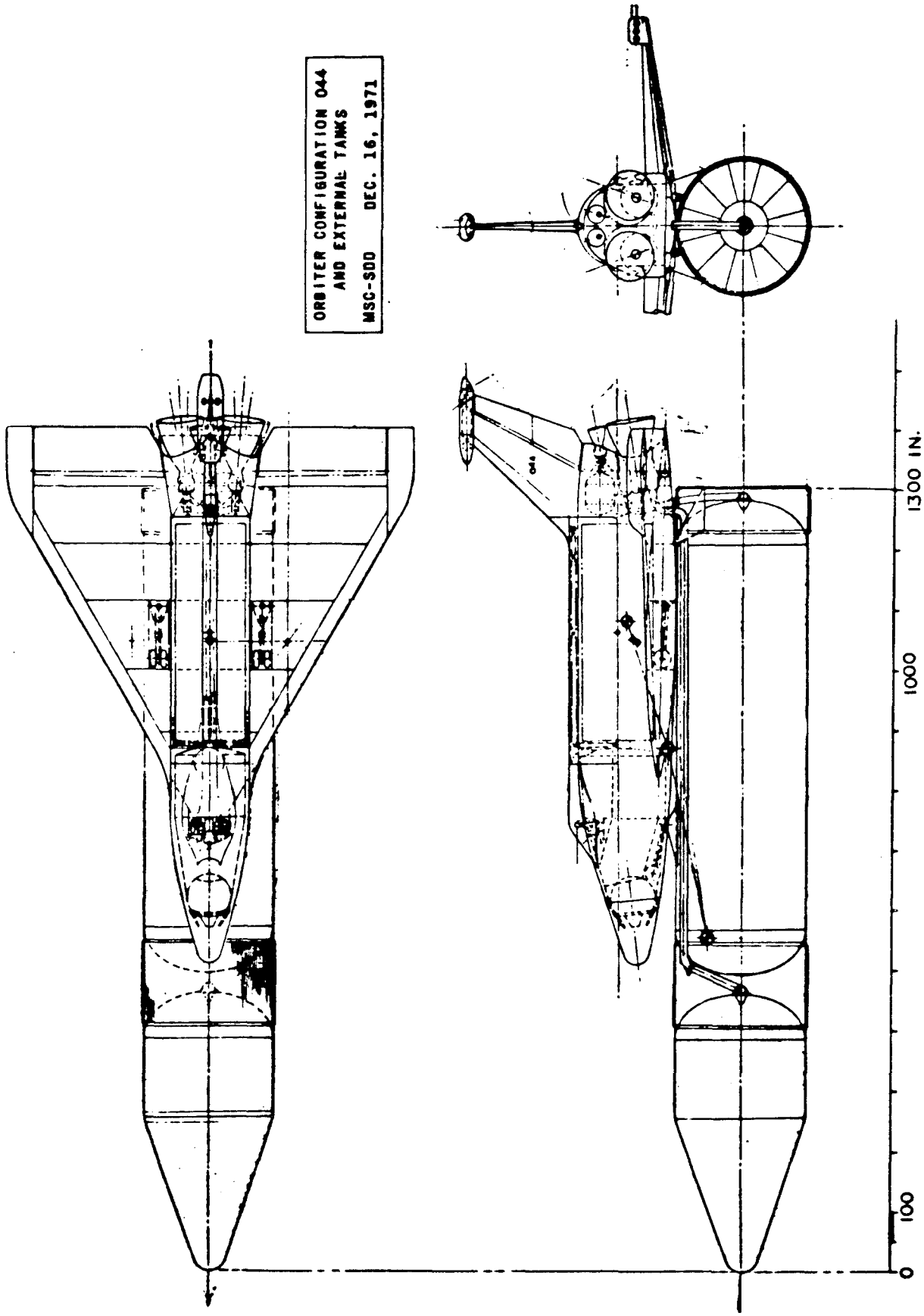

ORBITER CONFIGURATION 044
AND EXTERNAL TANKS
MSC-SDD DEC. 16, 1971
1300 IN.
1000
100
0

Figure 115
Orbiter 048

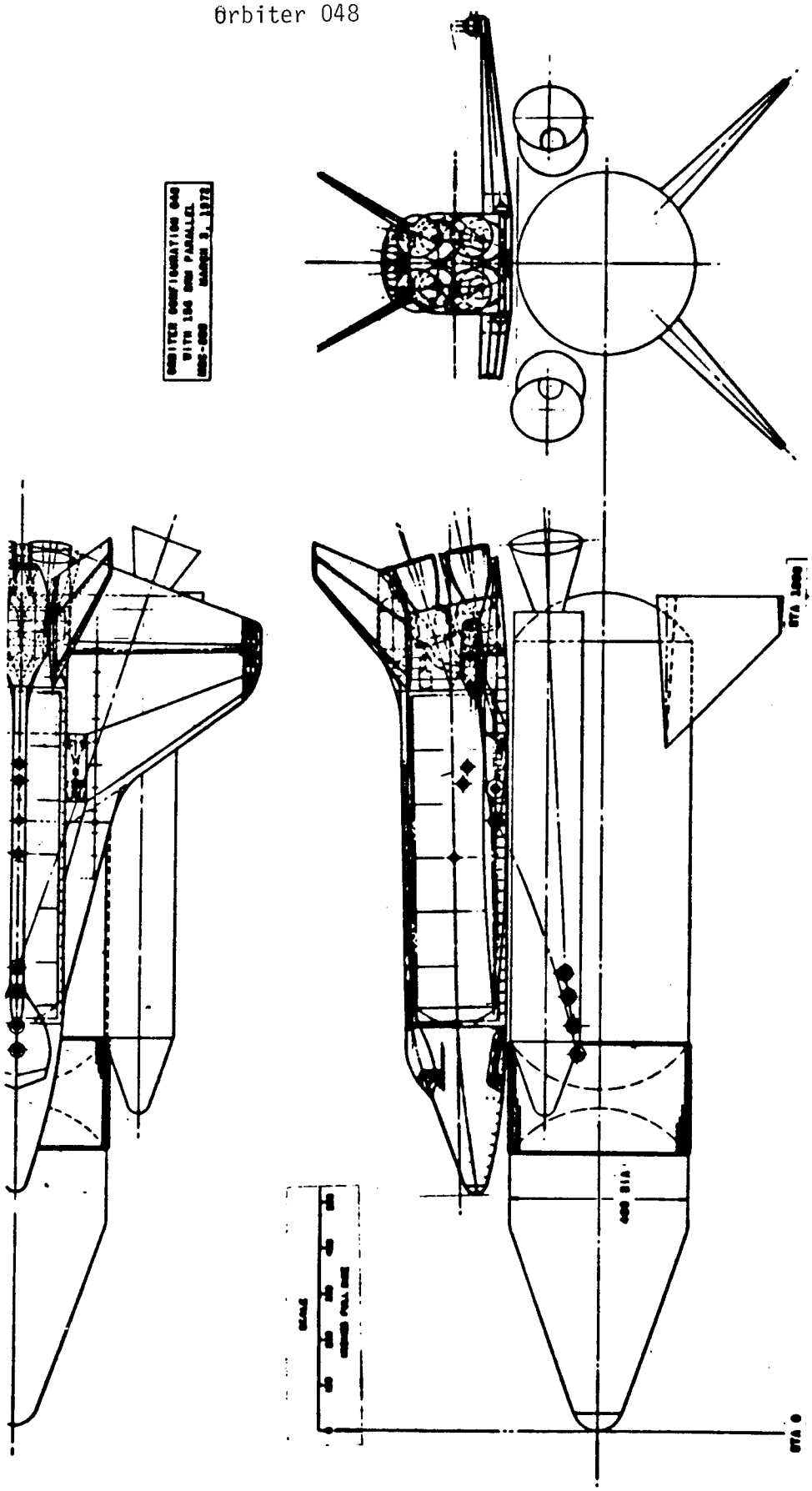


Figure 116

Orbiter 048

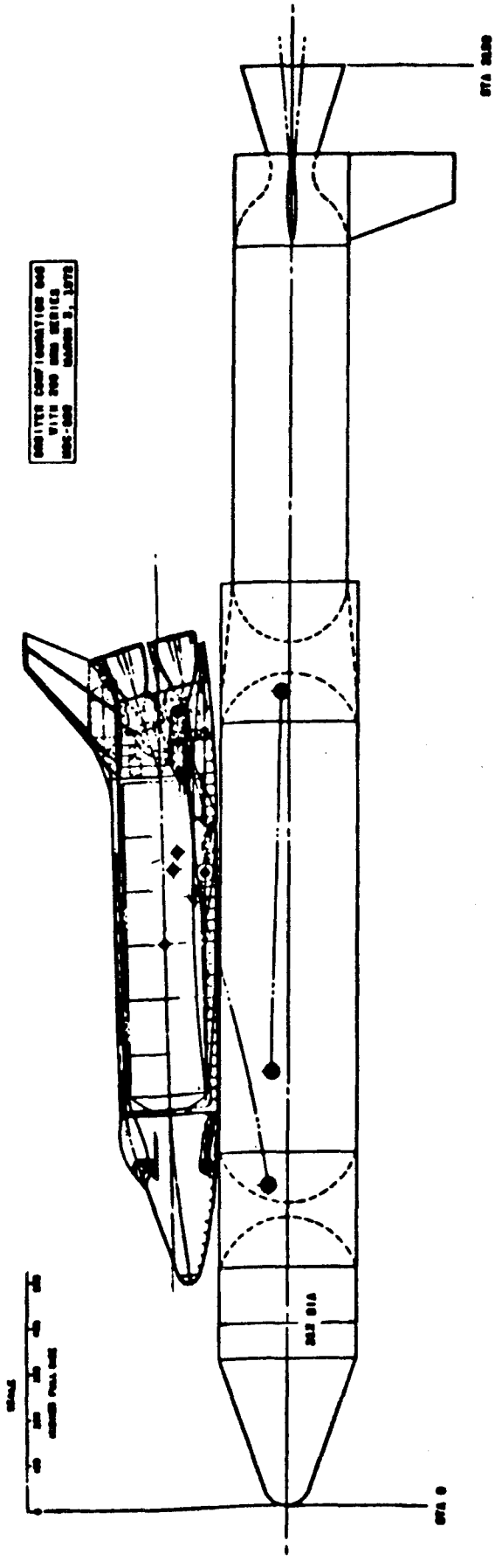


Figure 117

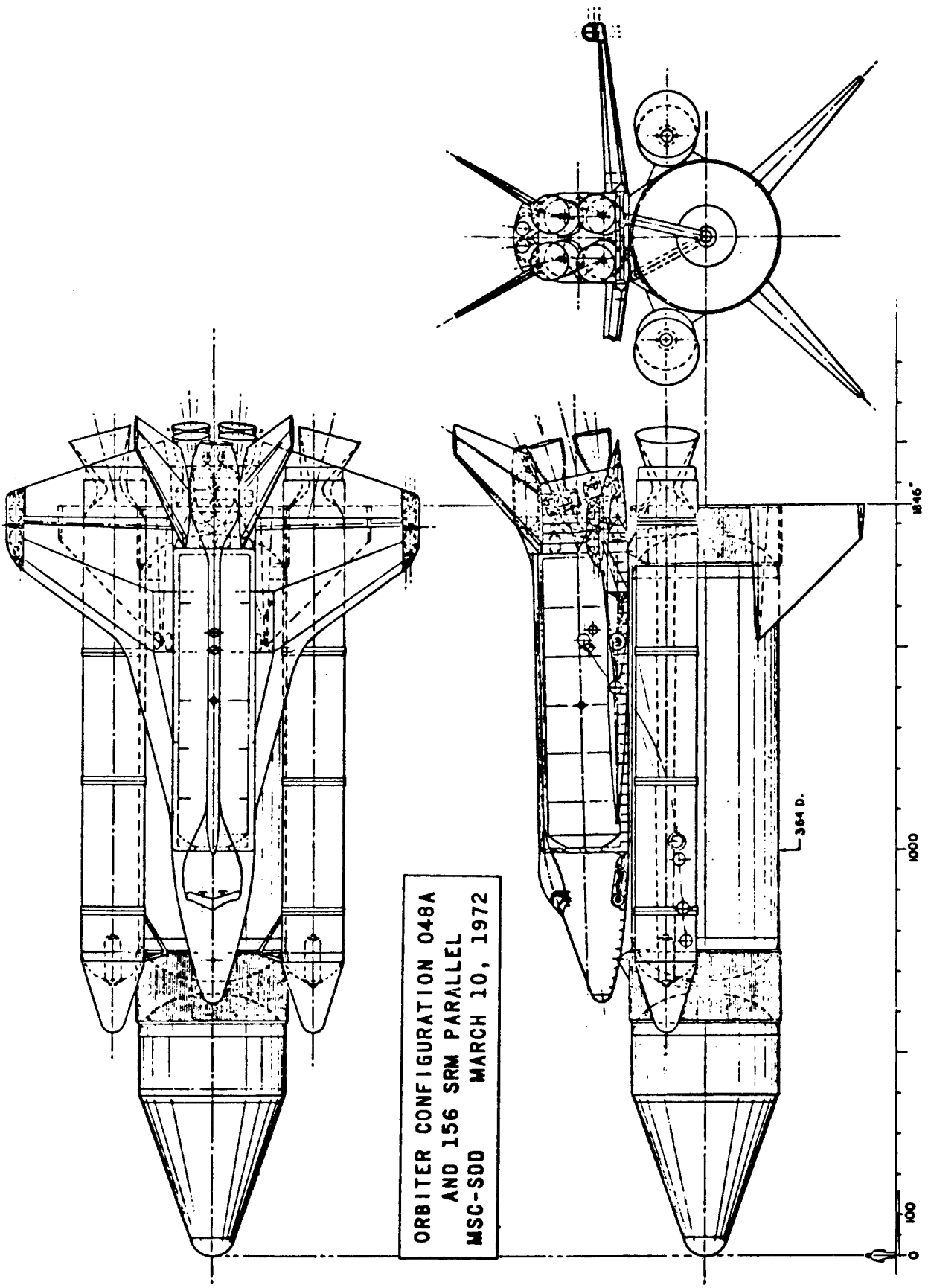

ORBITER CONFIGURATION 048A
AND 156 SRM PARALLEL
MSC-SDD MARCH 10, 1972
364 D.
1346"
1000
100
0

Figure 118

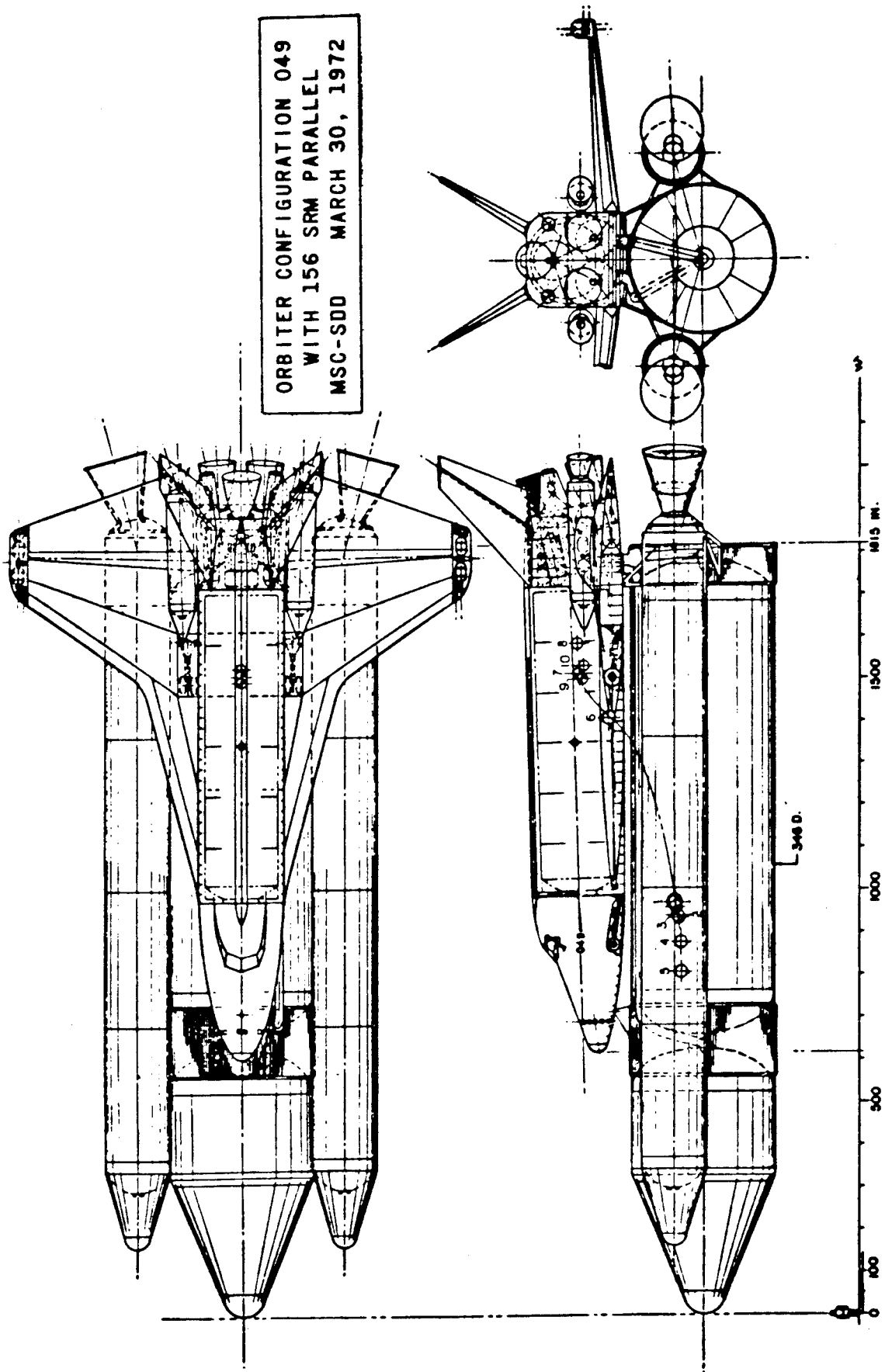

ORBITER CONFIGURATION 049
WITH 156 SRM PARALLEL
MSC-SDD MARCH 30, 1972
1500
1000
500
100
0

Figure 119
Orbiter 049A

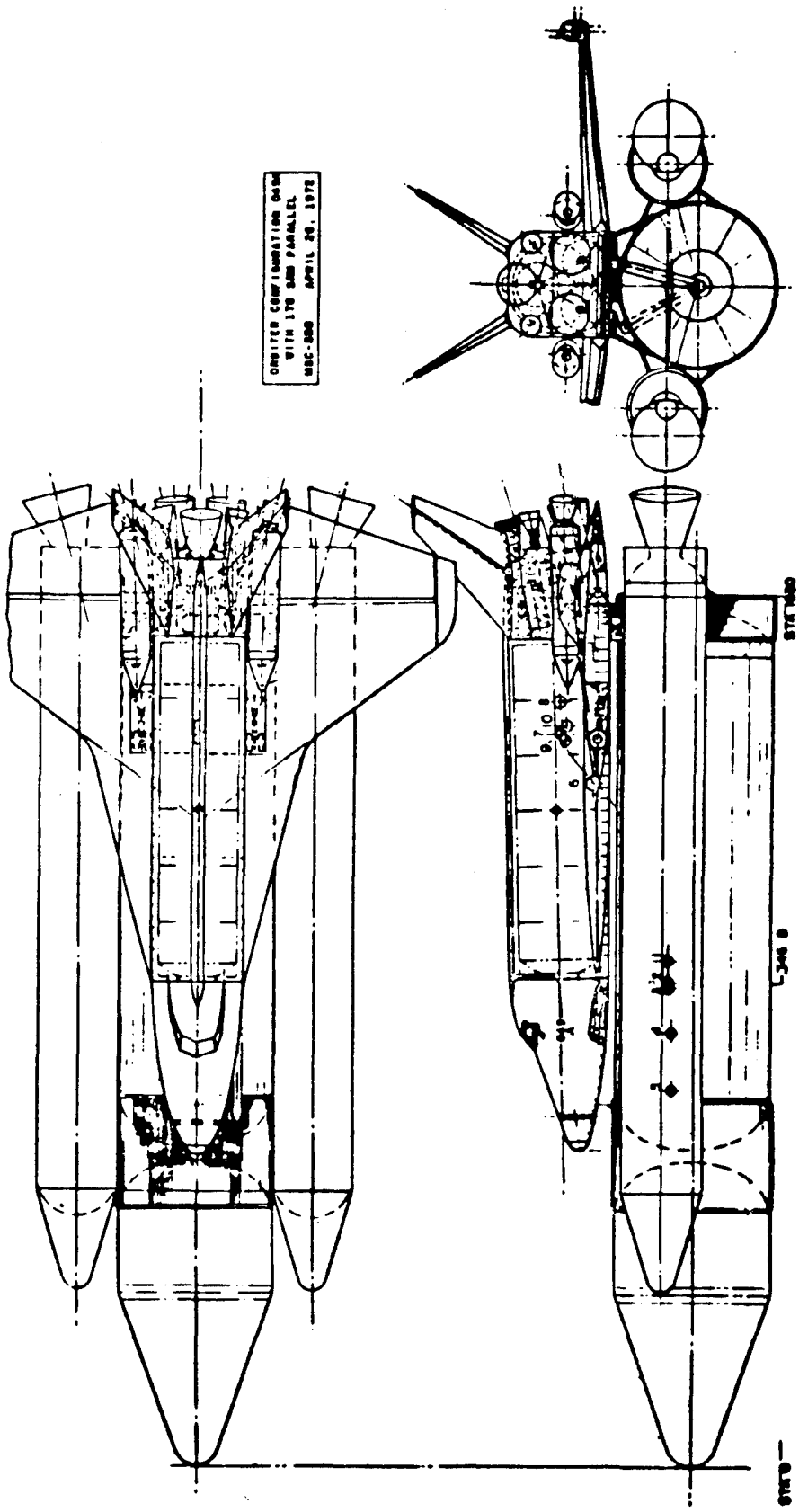
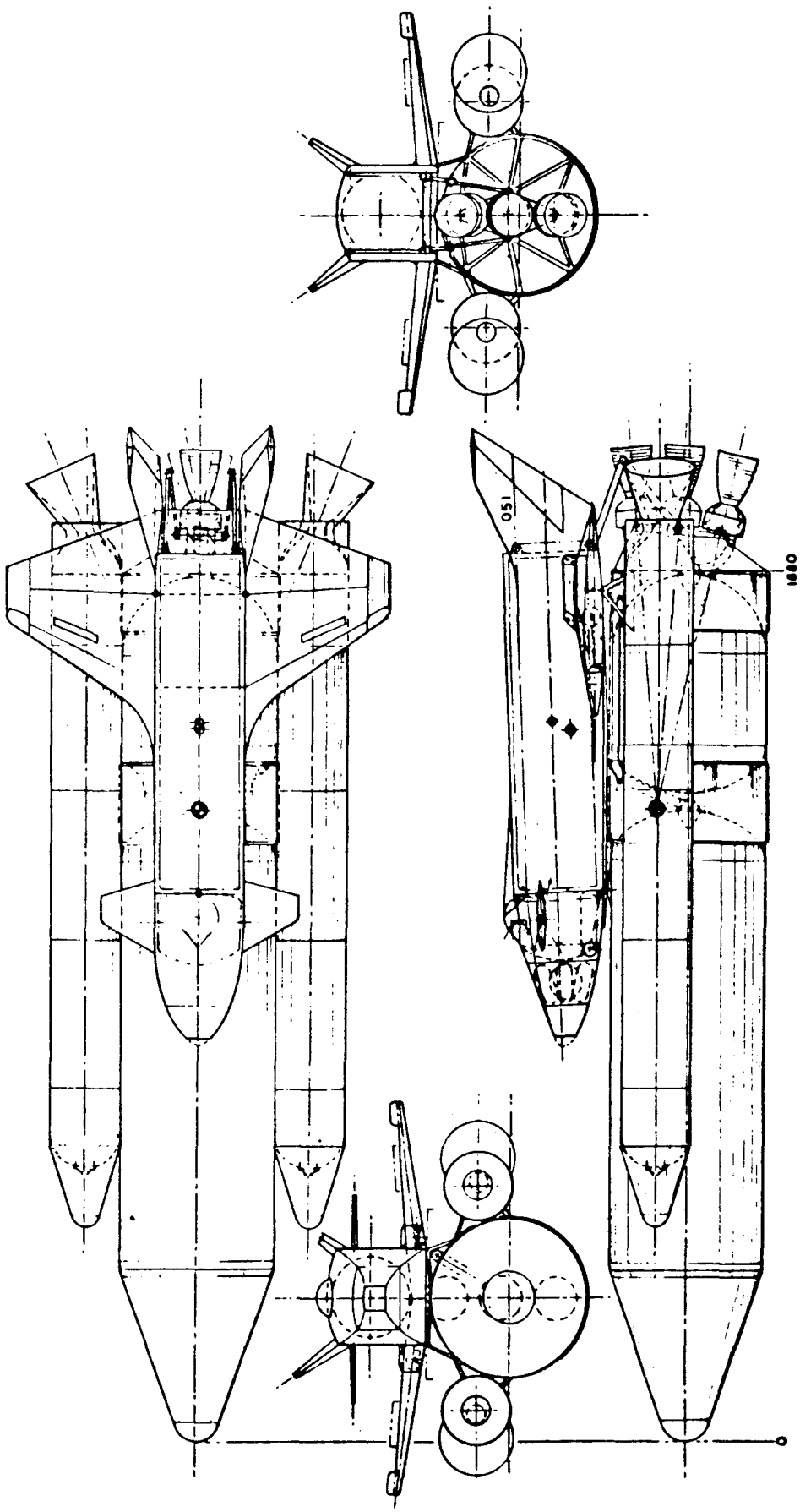
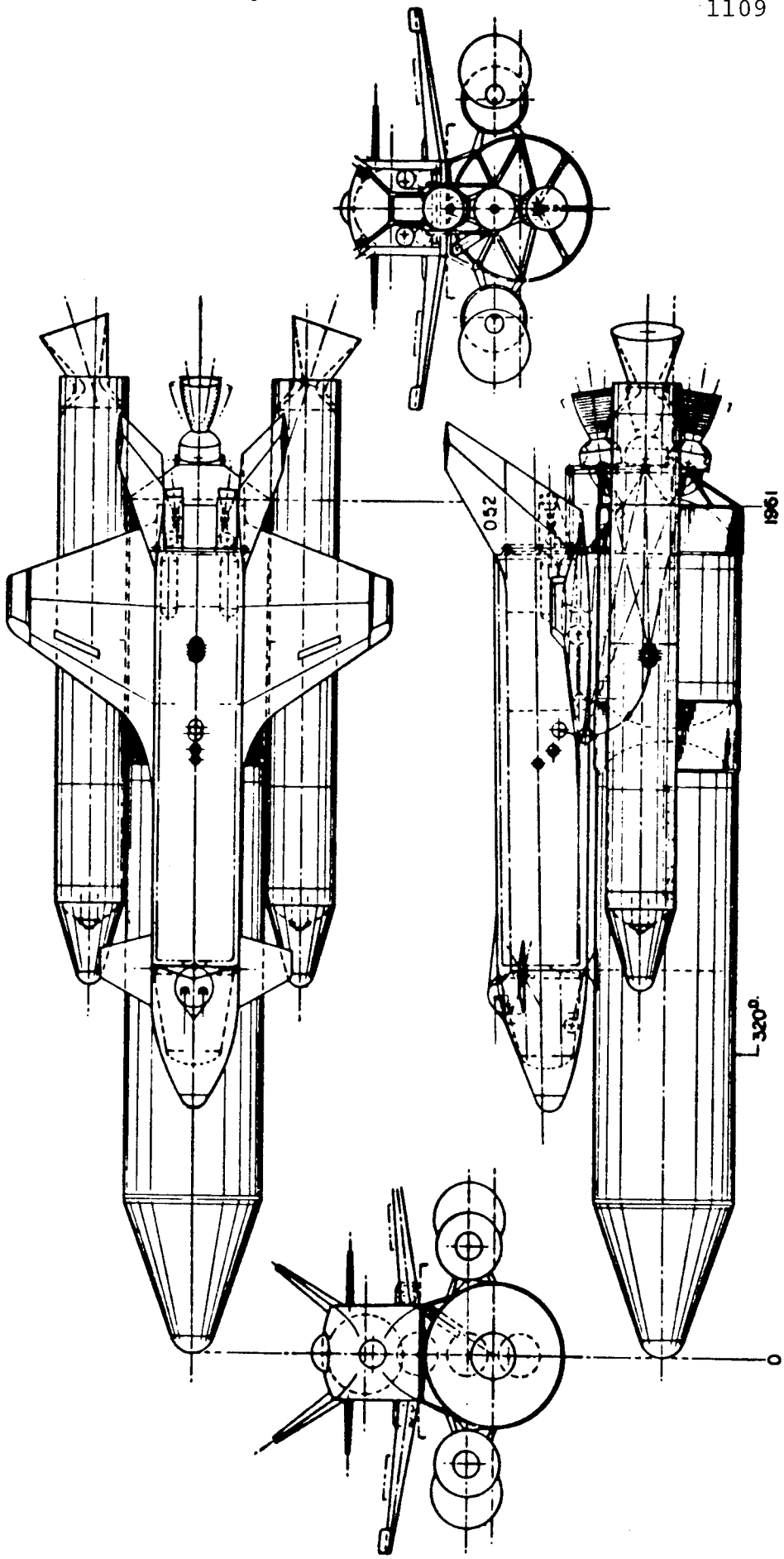


Figure 120



ORBITER CONFIGURATION 051
AND 156 SRM PARALLEL
MSC-SDD MAY 12, 1972

Figure 121



ORBITER CONFIGURATION 052
AND SRM BOOSTER
MSC-SDD JUNE 27, 1972

Figure 122

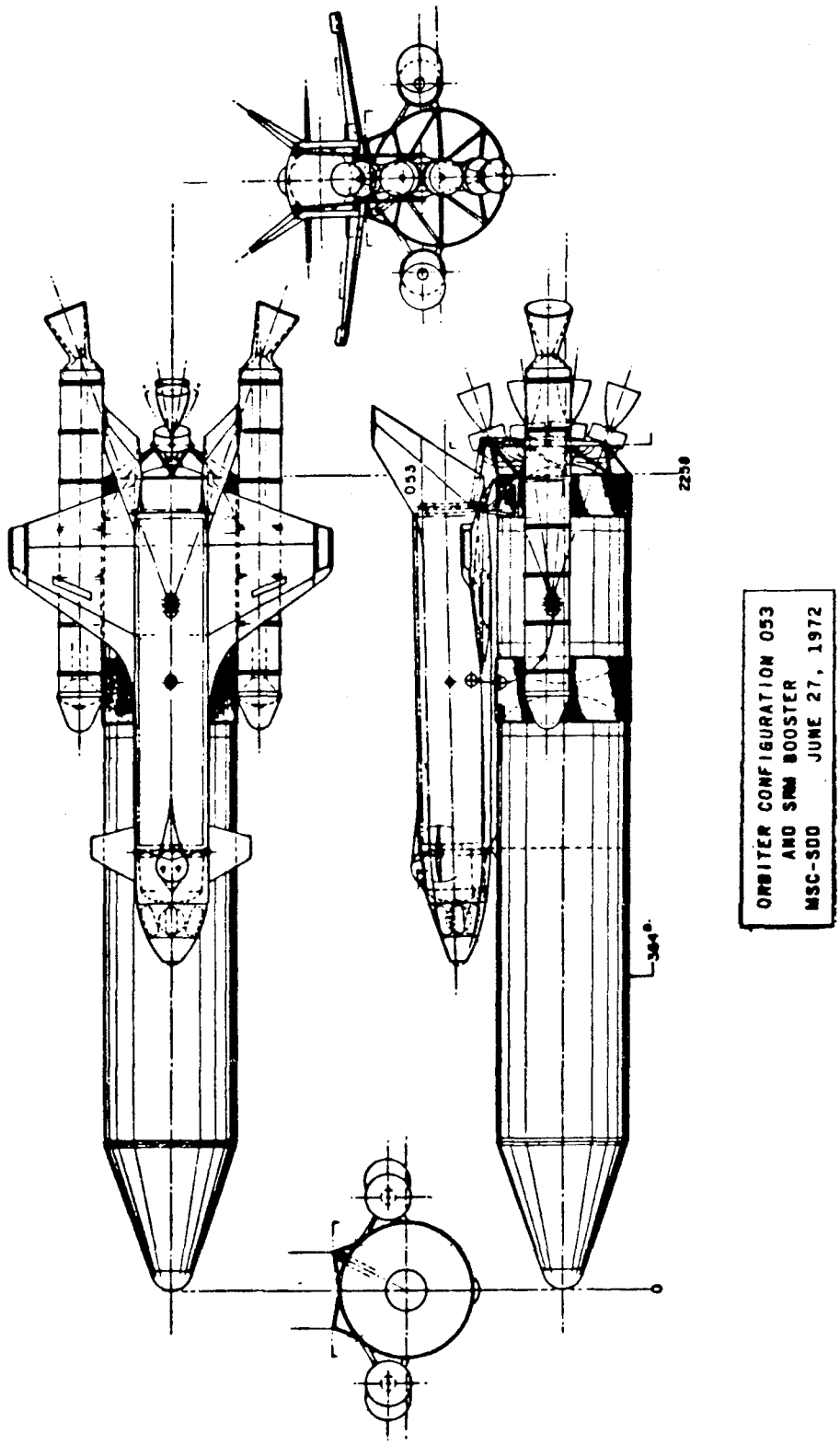
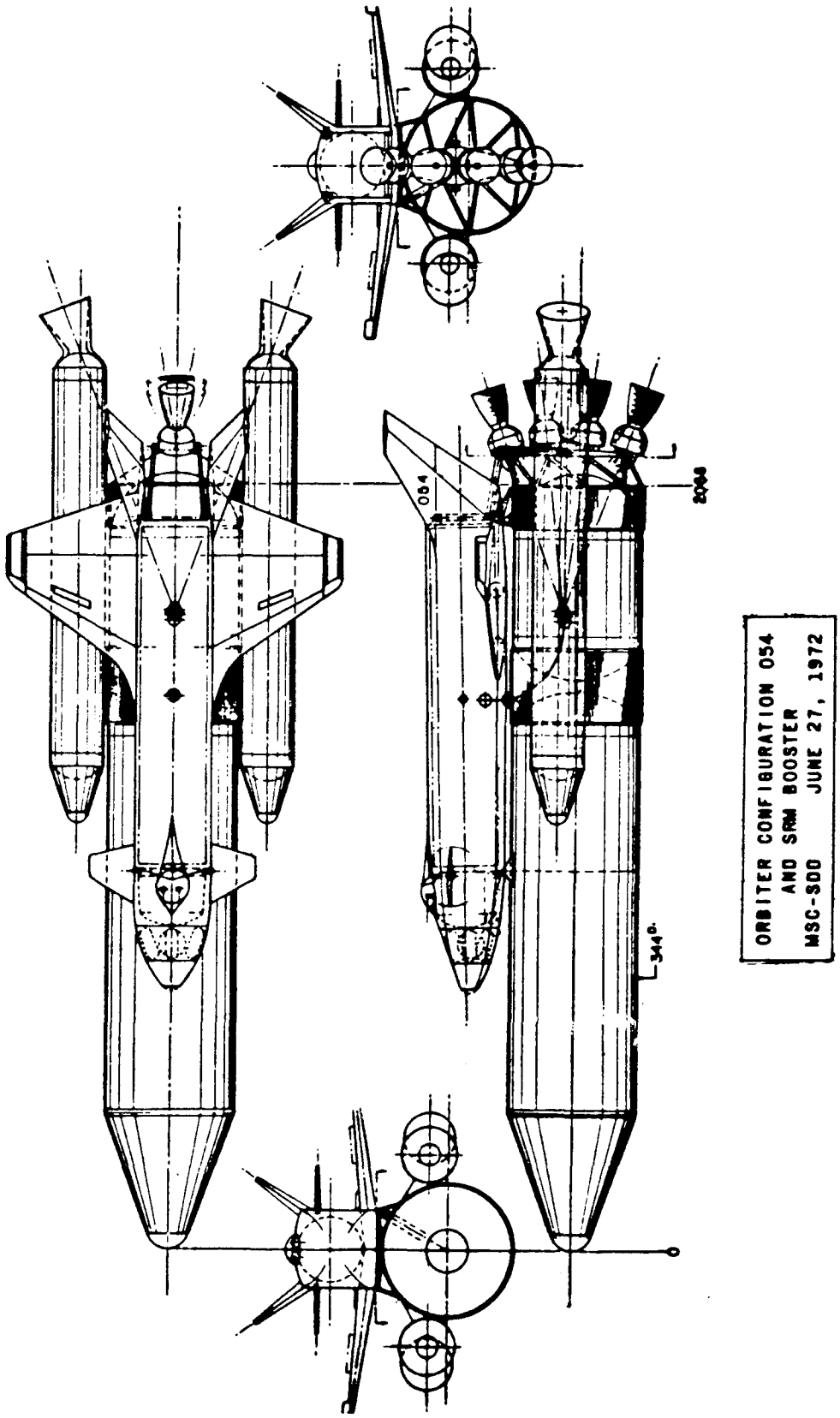

ORBITER CONFIGURATION 053
AND SRM BOOSTER
MSC-SDD JUNE 27, 1972

Figure 123



ORBITER CONFIGURATION 054
AND SRM BOOSTER
MSC-SDO JUNE 27, 1972

During the extension of Phase B, NASA evaluated its options regarding propulsion. Phase B' (Phase B Prime) emphasized placing the liquid hydrogen in tanks external to the orbiter; analysis indicated that while development and acquisition costs were lowered, the cost savings still did not meet overall program cost constraints. Phase B" (Double Prime) thus placed all boost propellants in external tankage. The question now became one of determining the mix (if any) between solid and liquid fuel boosters, and whether Shuttle would utilize a sequential "series" burn or a parallel-burn booster approach. The funded Grumman-Boeing Phase A study, Lockheed's Star Clipper stage-and-a-half design, the earlier advocacy of partially reusable systems by the Air Force at the instigation of the Flight Dynamics Laboratory, and of course, the funding realities of the 1970s all played a role in convincing the space agency to place Shuttle's tankage externally. By doing so, engineers would reduce the size and mass of the orbiter, simplify its design, and generate an external tank that was a simple, efficient column. A two-stage fully reusable Shuttle typically had a payload as little as 2-4% of total vehicle inert weight; the so-called parallel-burn thrust-augmented orbiter had a payload fraction of 9 percent--over double that of much larger and more complex (and certainly more costly) systems. Choice of a Space Shuttle Main Engine (SSME) required great thought. Two engines immediately offered themselves for consideration, both having been used in the Apollo program: the F-1 (burning a combination of kerosene and liquid oxygen) and the J-2S (burning liquid hydrogen and liquid oxygen). However, a higher-impulse high-thrust-level engine could obviate the need for at least one engine, so that Shuttle could use three rather than four, reduce the amount of propellant required, offer greater reusability, and utilize throttling. A NASA study indicated that modifying the existing F-1 and J-2S designs--both of which were out of production--to meet the safety and mission requirements of the Shuttle would be "at least equal in development cost and risk

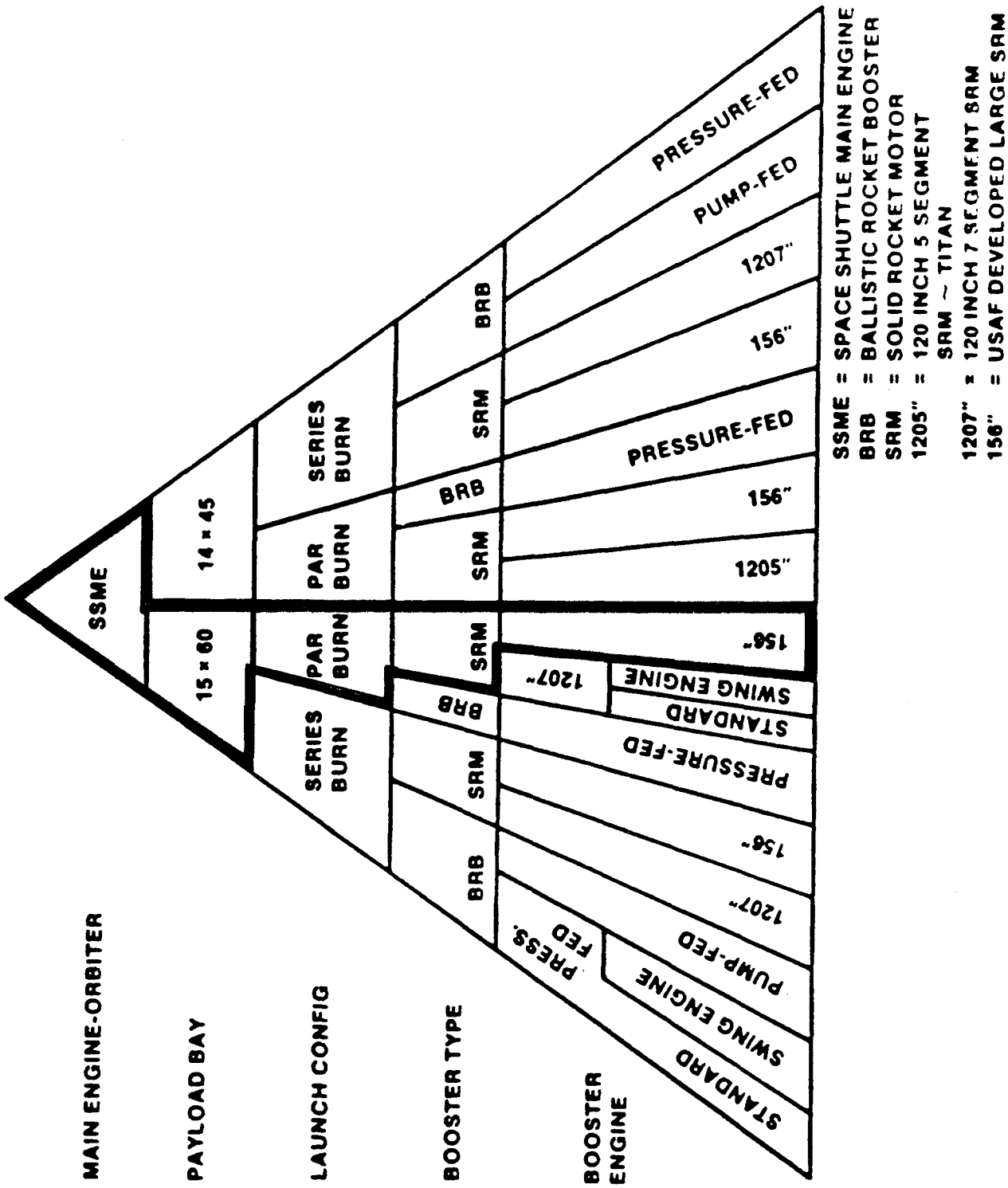
to a new design and could be 'worse' because of unintended inheritance."[25] NASA opted for a high-specific-impulse engine generating 470,000 lbs. thrust, and opened analysis and acquisition for the SSME. Three technical choices offered themselves: utilizing a radical new Rocketdyne concept called the Aerospike (a so-called "plug-nozzle" engine), using the traditional "expansion-bell" rocket engine configuration, or using an expansion-bell approach with staged combustion (the latter involving initial partial burning of propellants followed by complete burning of the propellant mix). NASA rejected the Aerospike because, like a scramjet, it required careful integration into the airframe. Subsequent evaluation of the two expansion bell approaches let to the awarding of an interim development contract to Rocketdyne in July 1971 for a staged combustion engine design. SSME design could have killed the orbiter at an early stage had the wrong engine choice been made; as Milton Silveria stated in a NASA history program interview," . . . the one thing that made Shuttle possible is the performance we got out of the SSME."[26] Even so, the development history of the SSME proved long and torturous. Early in the studies of Shuttle, NASA had opted for a so-called "TAOS" approach--a thrust-augmented orbiter shuttle. TAOS could have been utilized with either a "series burn" booster configuration (such as the thrust-augmented Delta launch vehicle uses) or with a "parallel burn" approach whereby the Shuttle's main engines would be firing. When NASA gradually settled on a generalized configuration of the orbiter feeding off an external fuel tank with some form of thrust augmentation--i.e.: auxiliary jettisonable engines--the choice of such engines (namely whether they would be pressure or pump-fed liquid-fuel engines, or solid-fuel engines) became an important design matter. Refurbishment of liquid-fuel engines after use appeared difficult. Solids, on the other hand, promised easier refurbishment, lighter weight, simplicity, and greater reliability. The Air Force had sponsored development of a large

156-inch diameter solid-fuel engine, and this particular motor appeared more attractive to NASA engineers than clusters of smaller 120-inch units, since only two would be necessary (the SRB produced 2.9 million lbs. thrust each) rather than five or seven of the smaller motors. Figure 124 shows the propulsion options open to NASA on the Shuttle, with the highlighted area including those options that NASA selected.[27]

The last challenge to a large payload bay Shuttle came from the Office of Management and Budget over the fall of 1971, when OMB asked NASA to evaluate smaller Shuttles, including ones having payload weights as low of 30,000 lbs. for easterly orbits, and payload bays measuring only 30 feet x 10 feet. Internal NASA analysis reaffirmed that the 15 x 60 foot bay was more attractive, for the agency deemed payload weight more significant than payload volume, and the larger bay could carry greater weight. Further, shorter payload bays made balancing the orbiter a more difficult task. NASA demonstrated that a smaller payload bay length would have only a miniscule effect on either inert or gross launch weight, and showed that the larger payload bay would accommodate multiple payloads more efficiently. Thus, NASA successfully defended the large payload bay orbiter as the "best buy" in a letter from NASA Administrator James Fletcher to then-OMB director Casper Weinberger on December 29, 1971, bringing the payload bay issue to a close, at last.[28]

By January 1972, NASA had already spent a total of $91,749,000 on Shuttle development. Table II gives a cost breakdown of Shuttle development as of January 1972, showing major contract awards. With early expenditures of this magnitude, Shuttle was clearly emerging as a major governmental development program. On January 3, 1972, NASA received White House authorization to proceed with development of the Shuttle; two days later, President Richard M. Nixon publicly endorsed it, stating that[29]

Figure 124



SHUTTLE PROPULSION OPTIONS

TABLE II

MAJOR SPACE SHUTTLE DEVELOPMENT CONTRACTS*

Contractor	Study	Cost (in millions)
McDonnell-Douglas/ Martin Marietta	Vehicle design & development	$19.3
North American Rockwell/ General Dynamics	" " " "	19.3
Grumman/Boeing	" " " "	13.6
Lockheed	" " " "	4.5
Chrysler	Recoverable launch booster	.750
Chrysler	Pressure-fed engine booster	.865
Mathematica	Economic analysis of program	.645
Aerojet General	High-pressure engine (orbiter)	6.7
Pratt & Whitney	" " " "	6.7
Rocketdyne	" " " "	6.7
Aerojet General	Pressure-fed engine (booster)	.450
TRW Systems Group	" " " "	.450
United Technology	Solid Rocket Motor (booster)	.150
Aerojet	" " " "	.150
Lockheed	" " " "	.150
Thiokol	" " " "	.150
Aerospace Corp.	Operations and Payload Analysis	3.6
Lockheed	Payload Analysis	.599
Parsons	Facilities plan	.715
Loewy/Snaith	Habitability design	.100
Cornell Aero Lab.	Orbiter approach/landing simulators	.175
Rocketdyne	High-pressure orbiter engine (hardware development; interim contract)	6.0
	Total =	$91.749 million

*As of 1 Jan 1972

> . . . the United States should proceed at once with the development of an entirely new type of space transportation system designed to help transform the space frontier of the 1970s into familiar territory, easily accessible for human endeavor in the 1980s and 90s.

NASA anticipated beginning atmospheric drop testing of the Shuttle in 1976, with manned orbital test flights in 1978 and operational missions commencing before 1980. The combined NASA-DoD mission model called for 500 missions over 12 years (1979-1990) of service (although NASA considered this a "conservative" model, events were to prove otherwise, a commentary of the difficulties of estimating the utility and operability of new and complex technological systems). Many decisions, of course, remained to be worked out, not least of which was selection of a prime contractor to develop the spacecraft itself. Smaller ones abounded; NASA, for example, did not resolve the recoverable solid-fuel or liquid-fuel booster question until March 1972. By this time, NASA estimated that the program would cost approximately $6.2 billion, in contrast to the $10 to $12 billion forecast earlier for the Phase B TSTO fully reusable concepts. North American-Rockwell, Grumman, McDonnell-Douglas, and Lockheed had all submitted competitive designs (shown in Figures 125a-125d) for the Phase C/D RFP--all based on the 040c design. On July 25, 1972 a joint NASA-Air Force Source Evaluation Board selected North American-Rockwell to develop the craft, following lengthy analysis. Since the Space Shuttle was the only space game in town following the demise of Apollo and associated Apollo spin-offs (such as the Skylab orbital workshop, a "poor man's space station"), NASA Administrator James Fletcher wished no doubts or controversy surrounding the contract award to North American-Rockwell, and together with NASA Deputy Administrator George Low and Associate Administrator for Organization and Management Richard C. McCurdy, summarized the findings of the evaluation board and the role of NASA management in reviewing their work in an at times bluntly worded memorandum

Figure 125a

LOCKHEED PROPOSAL

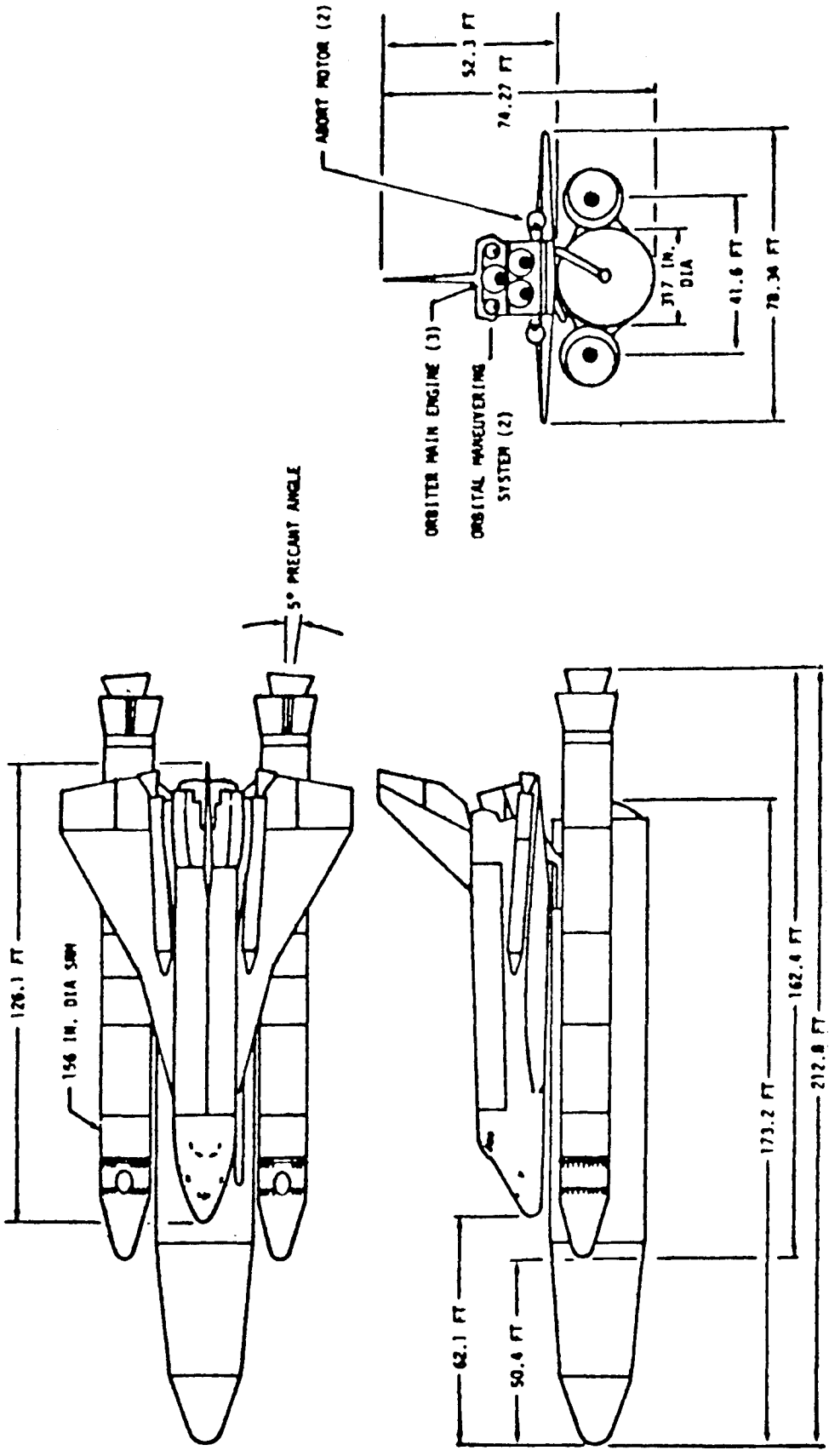
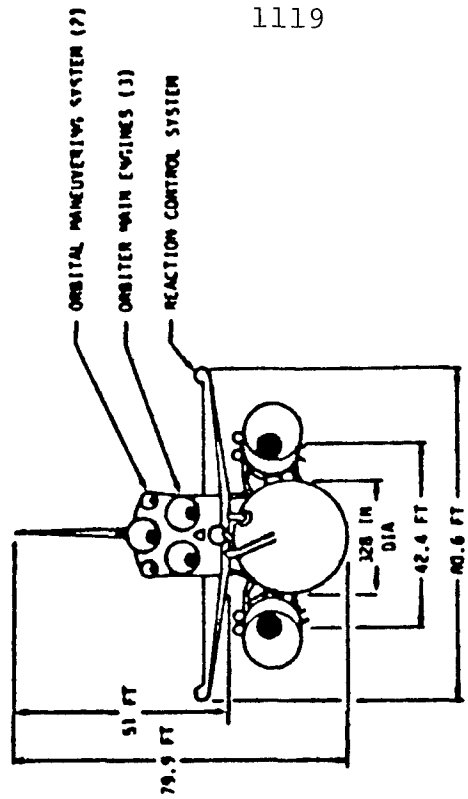
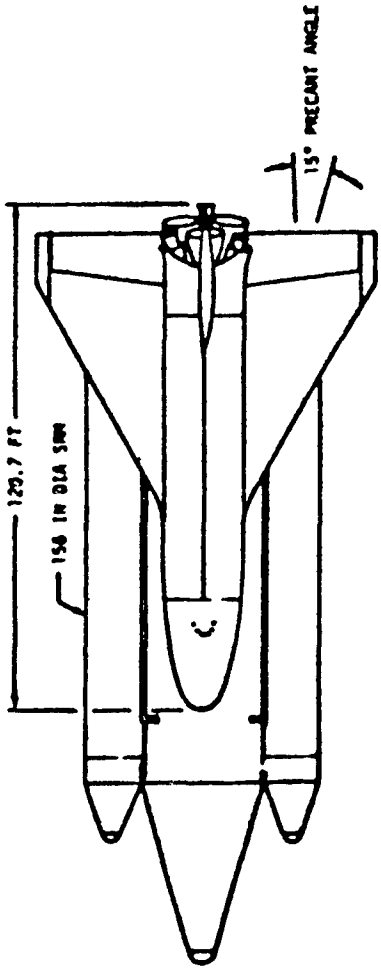


Figure 125b

McDONNELL-DOUGLAS PROPOSAL





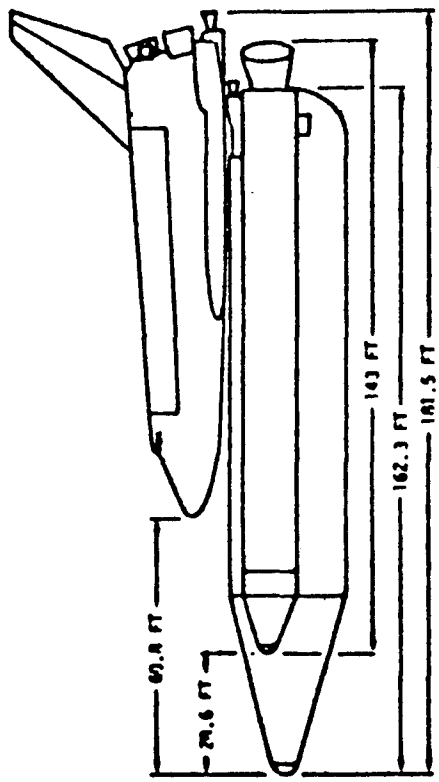


Figure 125c

GRUMMAN PROPOSAL

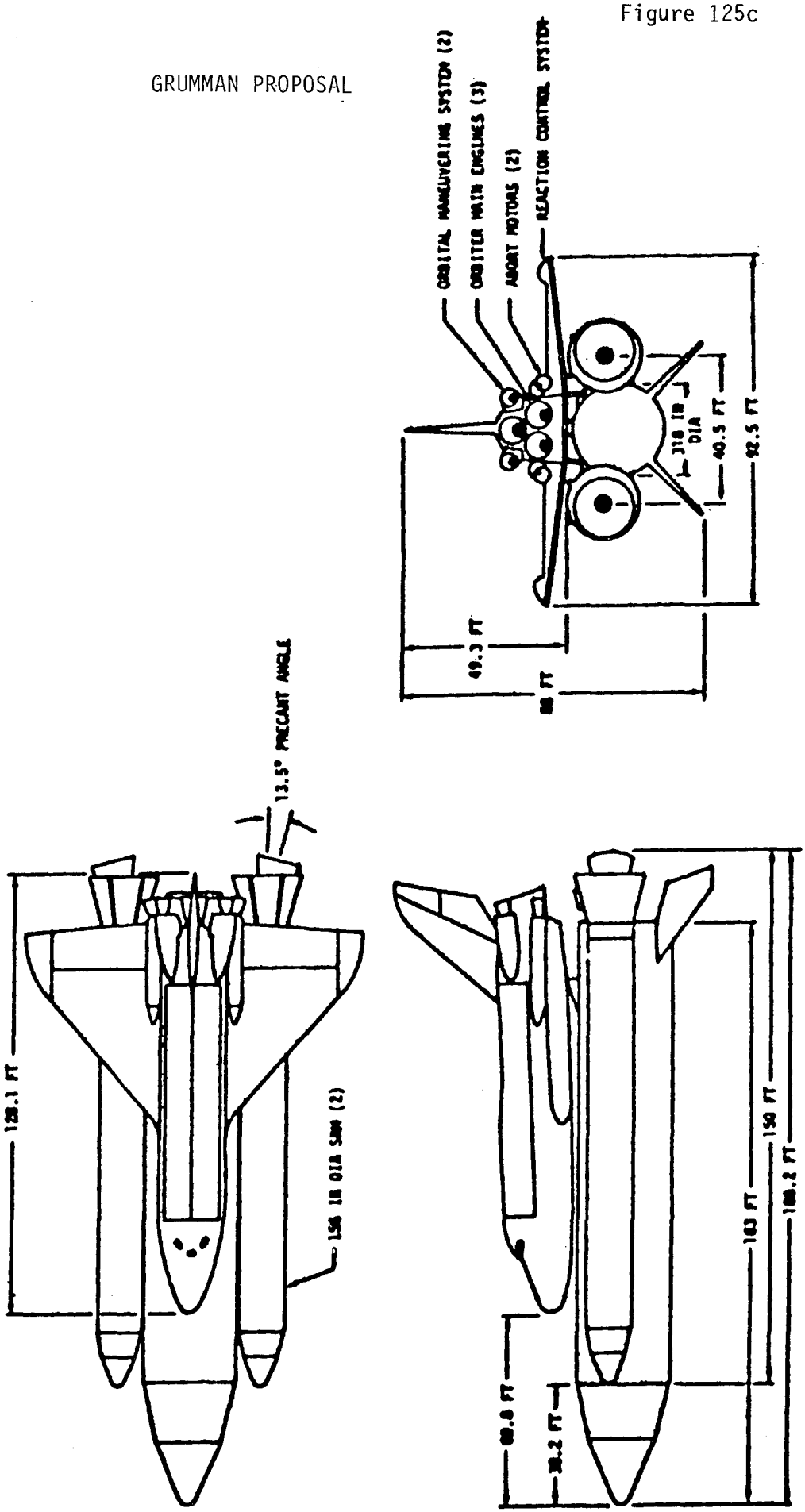
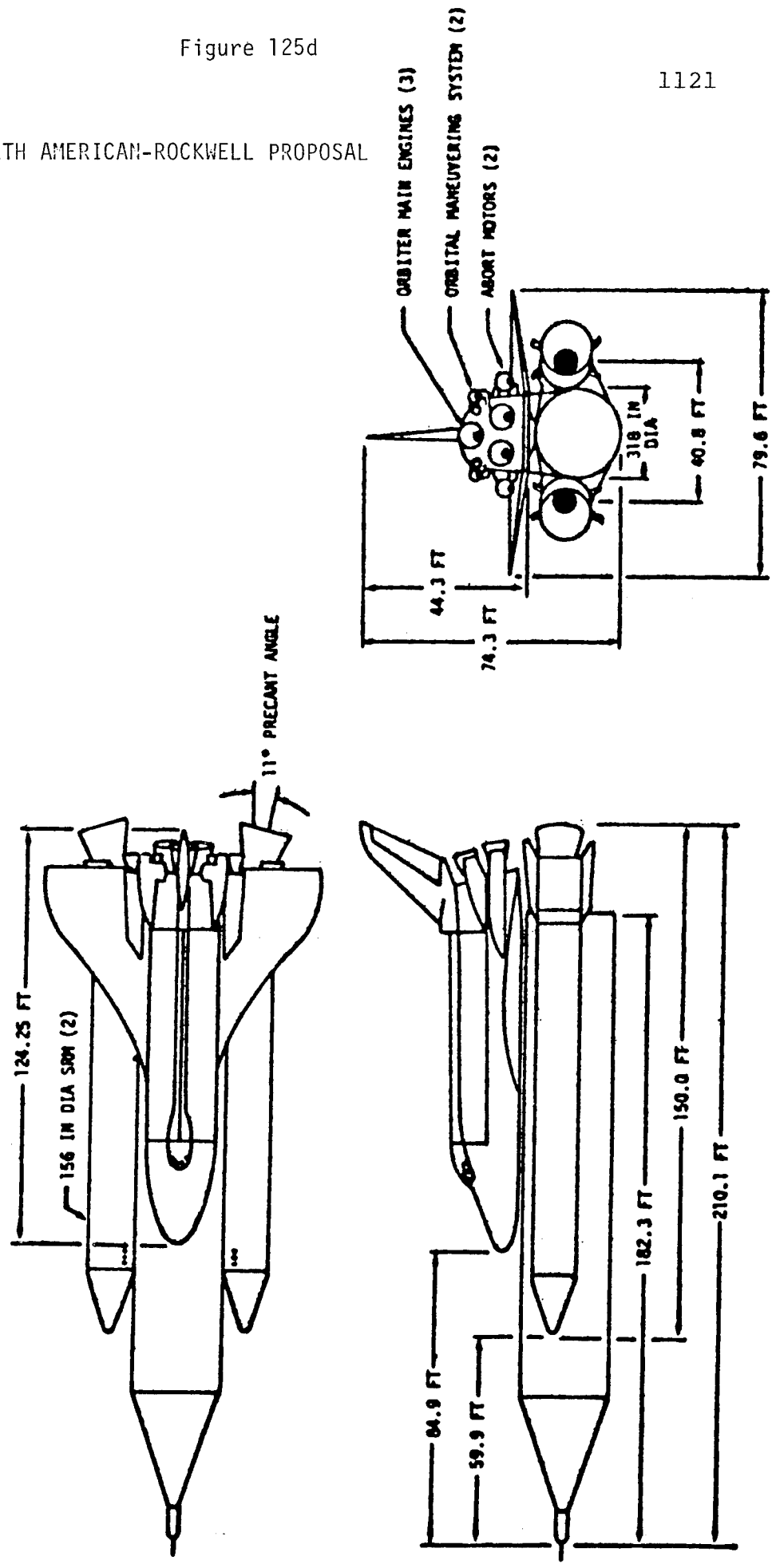


Figure 125d

NORTH AMERICAN-ROCKWELL PROPOSAL



for the record signed by McCurdy, Low, and Fletcher (respectively) on September 14, 15, and 18, 1972.[30]

The source evaluation board had relied upon expert advice using teams and panels, and the total number of Air Force and NASA individuals involved in the proposal review process numbered 416. The board evaluated the proposals on the basis of:

- manufacturing, test, and flight test support
- system engineering and integration
- subsystem engineering
- maintainability and ground operations
- key personnel and organizational experience
- management approaches and techniques
- procurement approaches and techniques

Using this basis, North American-Rockwell finished first, Grumman second, McDonnell-Douglas third, and Lockheed fourth during the initial SEB review. A more detailed analysis followed, and the final ranking mirrored the first: Rockwell, Grumman, McDonnell-Douglas, and Lockheed. As McCurdy, Low, and Fletcher agreed later:[31]

> North American received the highest score in mission suitability and an overall rating in the good to very good range. The North American design provided the lightest dry weight of any of the designs submitted. For guidance, navigation, and control, North American used a triple-redundant single-string approach which the Board considered to be very good as a simple design with minimum interfaces. Its good understanding of all electrical power subsystems reflected the very thorough studies that North American made following the Apollo 13 accident, which had its origin in an electrical subsystem. However, the Board considered North American's choice of a male-female concept for docking to be less advantageous than the androgynous method proposed by the other companies.
>
> North American presented an excellent analysis of maintainability from the standpoint of design criteria and goals to achieve optimum turnaround conditions and

> timing between flights. It designed its orbiter vehicle with very good overall accessibility for maintenance. North American's requirement for a trolley to support the orbiter on the ground was regarded as a weakness, since the trolley complicates the ground systems and causes operational constraints.
>
> North American's greatest advantages over the other offerers, within the mission suitability area, were in management. Its proposal showed efficient centralized control of the program, with a readily identifiable chief engineer and deputy. While all the offerers had well qualified key personnel, the Board reported that North American's top project management team was the best overall, the individuals having very good experience and demonstrated competence relevant to their assigned positions. As a company, North American has strong experience in manned space flight, including especially the Apollo command and service modules and the Saturn II second stage vehicle. A minor weakness is the company's lack of recent experience with large operational airframes.

The latter point referred to Rockwell's being out of the large airplane business, building only smaller craft such as the Sabreliner business jet and the Navy's T-2 and Air Force's OV-10, the former being a trainer and the latter a counter-insurgency aircraft. Rockwell was reentering the large aircraft business (which it had left after the cancellation of the B-70 program) with the Advanced Manned Strategic Aircraft (AMSA), subsequently designated as the B-1.

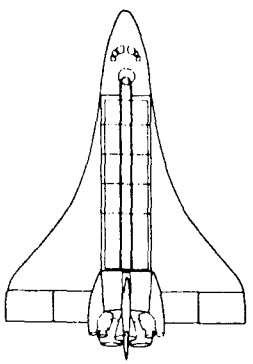
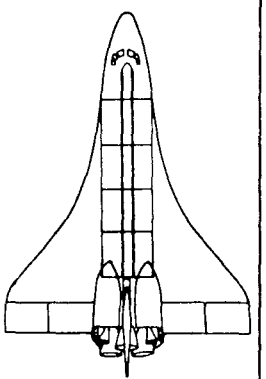
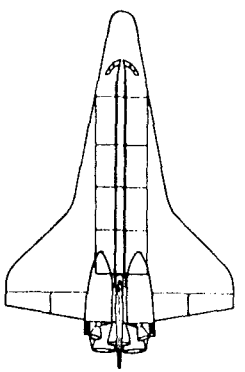
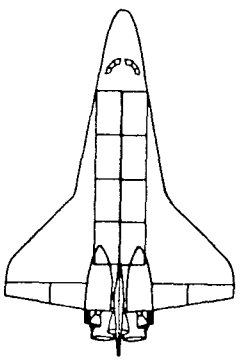
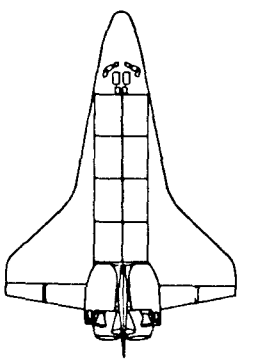
Grumman, a firm building aircraft almost exclusively for the Navy, was a surprisingly strong second. The firm had done a superb job on the Lunar Module (LM) program, which had thankfully functioned flawlessly during the Apollo program. Grumman was particularly strong in detail design of the proposed craft, and it had a team of good people, but the firm fell short on experience with large cryogenic propulsion systems, and displayed a proclivity for unnecessary complexity in the guidance, navigation, and control system, as well as in data processing. NASA feared that Grumman's management concept would result in "premature

hardening" of the specification, leading to lack of flexibility as the program advanced down the development path. McDonnell-Douglas ranked third; while the "McDAC" proposal showed some good technical features (such as well-thought-out air-breathing engine system for landing, and radiator payload bay doors that were better conceived than its rivals), NASA faulted the design of the propellant system, lack of desirable maintainability features (a surprise in view of the Douglas company's long experience with airliners), and management. Lockheed was last, surprisingly rated only fair. (Perhaps this reflected the much smaller amount of funding Lockheed had to draw upon for its configuration study.) Its entry was the heaviest submitted, with questionable complexity, and possessing anticipated flight characteristics that raised concerns about mission safety. As McCurdy, Low, and Fletcher wrote, "In general, the reason for the relatively low Lockheed evaluation was its lack of consistent technical depth."[32]

On July 26, 1972 Fletcher, Low, and McCurdy met privately to consider the report from the Source Evaluation Board. The conversation centered on North American-Rockwell and Grumman, for the three men recognized that McDonnell-Douglas and Lockheed were really out of it. After weighing all factors, they concurred with the board's conclusions and selected North American-Rockwell to design and fabricate the Space Shuttle. The following month, NASA awarded contracts to North American-Rockwell's Space Transportation Systems Division for design and development of the orbiter. Morton Thiokol subsequently received the SRB contract, Rocketdyne (a division of North American-Rockwell) received a definitive contract for the SSME, and Martin Marietta's Denver Aerospace division received a go-ahead to produce the Shuttle's expendable external liquid hydrogen-liquid oxygen tank. Within the space agency, Marshall had responsibility for the SRB, SSME, and external tank. Houston would supervise development of the orbiter. Kennedy Space Center would assemble the Shuttle's components, check them before launch, and conduct launch

operations. Over approximately the next two years, from March 1972 to mid-1974, NASA and Rockwell engineers had much to do. Figures 126a and 126b show the refinement of design thought that occurred over the period, starting with the agency's concept at the time of "Authority to Proceed" (ATP, March 1972), the configuration examined at the time of the Program Readiness Review (PRR), Vehicle 2A, its successor Vehicle 3, 4, and finally, Vehicle 5, 6 of 1974 which became the final design layout for the actual Space Shuttle itself. Figures 127a-127e show configuration evolution of the orbiter from the ATP orbiter to Vehicle 5, 6. Figures 128a-128e show the configuration evolution of the so-called "integrated vehicle:" the orbiter, the external tank, and the SRBs, from the time of the ATP to the Vehicle 5, 6 configuration. Basically, NASA and Rockwell worked together on these configurations, with the company being given, however, a great deal of leeway in determining the final aerodynamic configuration for the entire Shuttle vehicle. The following text, taken from a NASA Shuttle wind tunnel testing summary report, explains the reasons why the configuration evolved as it did from the time of the ATP vehicle through Vehicle 5, 6:[33]

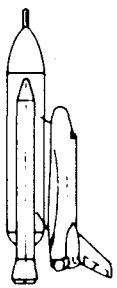
> The ATP Orbiter aerodynamic shape had a 50 degrees sweep delta wing planform sized to provide 150 knots (77.2 m/sec) design touchdown speed with a 40,000-pound (18,100 kilogram) return payload. Elevons were sized to provide trim at hypersonic speeds over an angle of attack range from 20 degrees to 50 degrees with an operational center of gravity (CG) range of 3 percent body length (LB). The cargo bay was 15 feet in diameter (4.57 meters) by 60 feet long (18.2 meters) to accept a wide variety of payloads. The remote manipulator arms were stowed in a dorsal fairing along the top of the payload bay doors. For ferry and entry assist, an air-breathing propulsion system (ABPS) was situated in the aft portion of the pay-load bay. Three main propulsion system rocket engines were located at the base of the aft fuselage and orbital maneuvering systems (OMS) engines were installed in two removable pod modules on the side of the aft fuselage. Reaction control system (RCS) rocket engines were also located in the aft pods and on

CONFIGURATION DESIGNATION	ATP	PRR	2A	3,4	5,6
CONFIGURATION CONTROL DRAWING NUMBER	VL70 - 000001	VL70 - 000040A	VL70 - 000089B	VL70 - 000140, 140A/B	VL70 - 000140C, VC70 - 000002
WING DESIGN	50° BLENDED DELTA	50° BLENDED DATA	45°/79° DOUBLE DELTA	45°/81° DOUBLE DELTA	45°/81° DOUBLE DELTA
WING AREA, FT^2 (m^2)	3220 (299.14)	3220 (299.14)	2690 (249.90)	2690 (249.90)	2690 (249.90)
WING SPAN, FT (m)	84.0 (25.60)	84.0 (25.60)	78.1 (23.80)	78.1 (23.80)	78.1 (23.80)
OVERALL LENGTH, FT (m)	125.8 (38.34)	125.8 (38.34)	125.2 (38.16)	122.8 (37.43)	122.2 (37.25)
PLAN VIEW					
DRY WEIGHT, LB (kg)	170,000 (77,110)	170,000 (77,110)	150,000 (68,039)	150,000 (68,039)	150,000 (68,039)
LANDING PAYLOAD, LB (kg)	40,000 (18,144)	40,000 (18,144)	25,000 (11,340)	32,000 (14,515)	32,000 (14,515)
CG RANGE (% REFERENCE LENGTH)	65.0 - 68.0	65.0 - 68.0	66.0 - 68.0	65.0 - 67.5	65.0 - 67.5

(a) Orbiter Vehicle.

Major configuration evolution definitions.

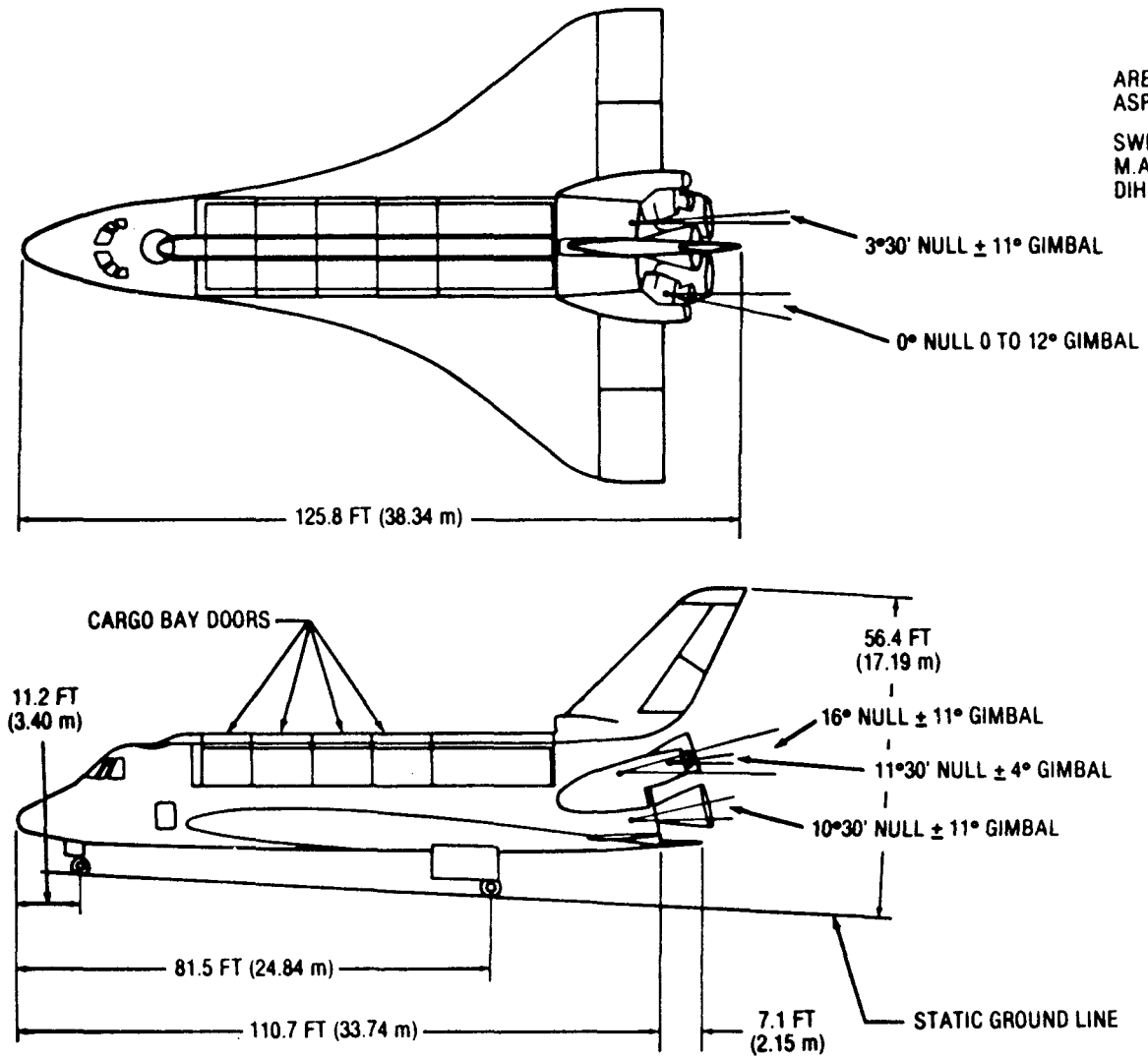
Figure 126a

CONFIGURATION DESIGNATION	ATP	PRR	VEH. 2A	VEH. 3,4	VEH. 5,6
CONFIGURATION CONTROL DRAWING NUMBER	VL72 - 000001	VL72 - 000030	VL72 - 000061A	VL72 - 000088A, 88B	VL72 - 000143D, VC72 - 000002C
OVERALL LENGTH, FT (m)	205.7 (62.70)	214.3 (65.32)	192.3 (58.61)	181.3 (55.26)	183.6 (55.96)
ET LENGTH, FT (m)	182.0 (55.47)	189.8 (57.85)	165.8 (50.54)	155.4 (47.37)	153.7 (46.85)
ET DIAMETER, IN (cm)	318.0 (807.7)	304 (772.2)	324 (823.0)	324 (823.0)	331.0 (840.7)
ET NOSE SHAPE	30° BLUNTED CONE	OGIVE (568" R)	OGIVE (605" R)	OGIVE (600" R)	OGIVE (612" R)
ET NOSE TIP LENGTH, FT (m)	10.33 (3.15)	10.33 (3.15)	11.58 (3.53)	NONE	1.57 (0.48) SPIKE
SRB LENGTH, FT (m)	184.8 (56.33)	175.1 (53.37)	145.1 (44.23)	145.1 (44.23)	149.1 (45.45)
SRB DIAMETER, IN (cm)	156 (396.2)	162 (411.5)	142.3 (361.4)	142.3 (361.4)	146.0 (370.8)
SRB DISTANCE AFT OF ET TIP, FT (m)	17.5 (5.33)	39.3 (11.98)	47.3 (14.42)	36.2 (11.03)	34.5 (10.52)
ORBITER DISTANCE AFT OF ET TIP, FT (m)	80.3 (24.48)	88.6 (27.01)	80.8 (24.63)	56.7 (17.28)	53.9 (16.43)
PROFILE VIEW					
SRB GIMBAL SETTING, DEGREES	11° YAW FIXED	3.5° YAW ± 5°	0° ± 5°	0° ± 5°	0° ± 6°
ET PROPELLANT, THOUSAND POUNDS (THOUSAND kg)	1697 (769.7)	1650 (748.4)	1550 (703.1)	1549 (702.6)	1552 (704.0)
BOOSTER LIFT OFF WEIGHT, THOUSAND POUNDS (THOUSAND kg)	3252 (1475.1)	3276 (1486.0)	2259 (1024.7)	2327 (1055.5)	2327 (1055.5)
GROSS LIFT OFF WEIGHT, THOUSAND POUNDS (THOUSAND kg)	5411 (2454.4)	5261 (2386.3)	4116 (1867.0)	4188 (1899.6)	4197 (1903.7)

(b) Integrated Vehicle.

Concluded.

Figure 126b



	WING	VERTICAL STAB.
AREA	3220 FT^2 (299.14 m^2)	435 FT^2 (40.41 m^2)
ASPECT RATIO	2.19	1.675
SWEEP (L.E.)	50°	45°
M.A.C.	525.5 IN.(1344.77 cm)	205 IN.(520.70 m)
DIHEDRAL (T.E.)	3°30'	NA

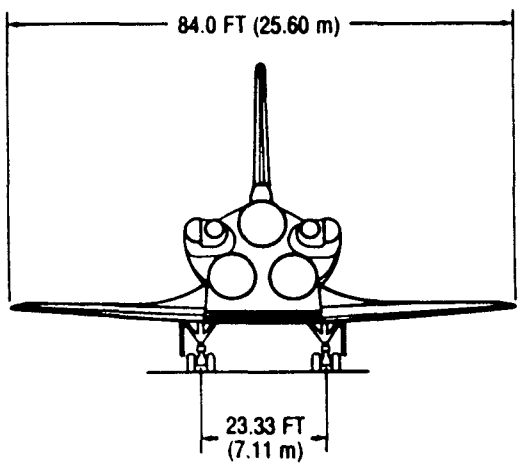
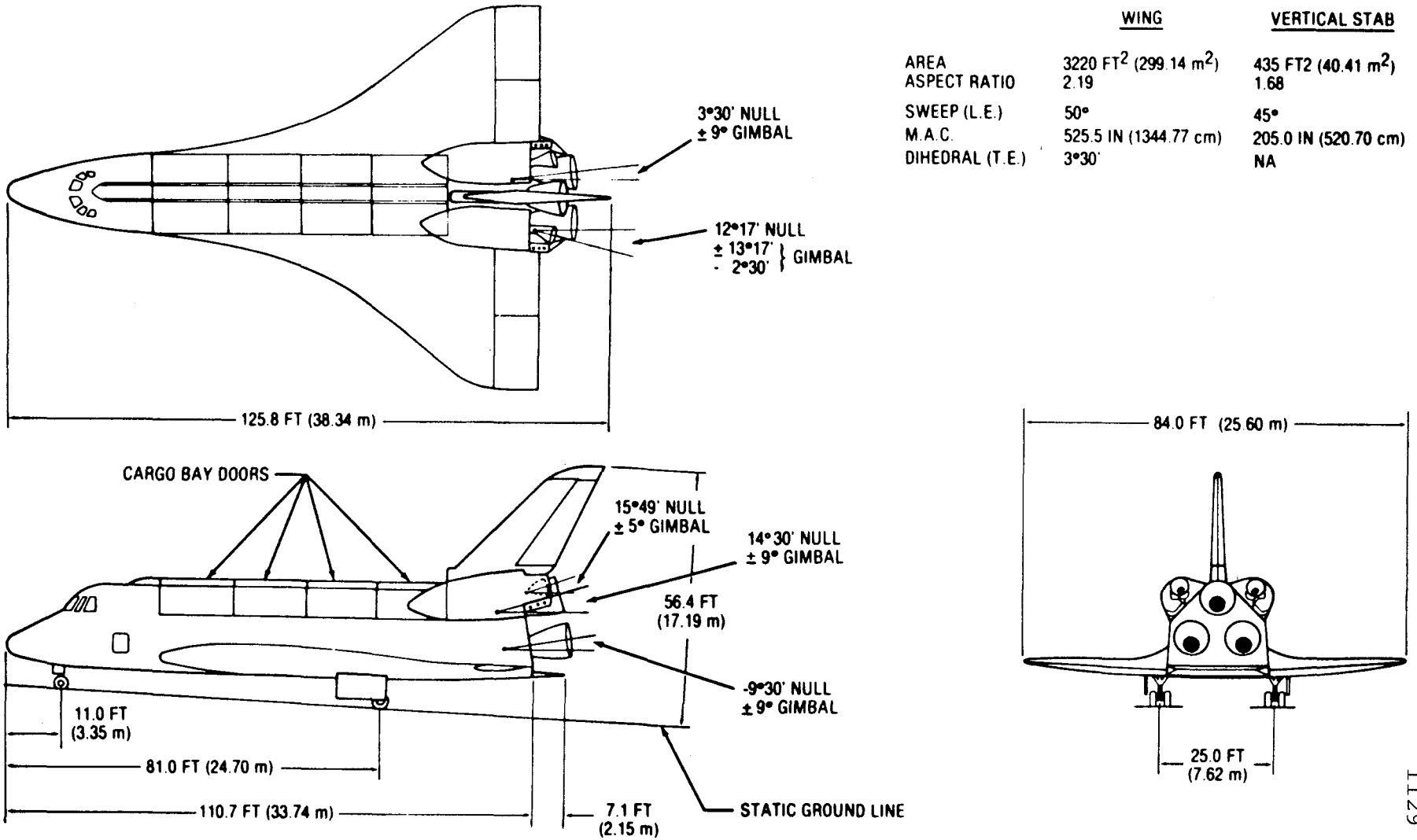


Figure 127a

(a) ATP configuration.

Orbiter Vehicle dimensions; configuration evolution.



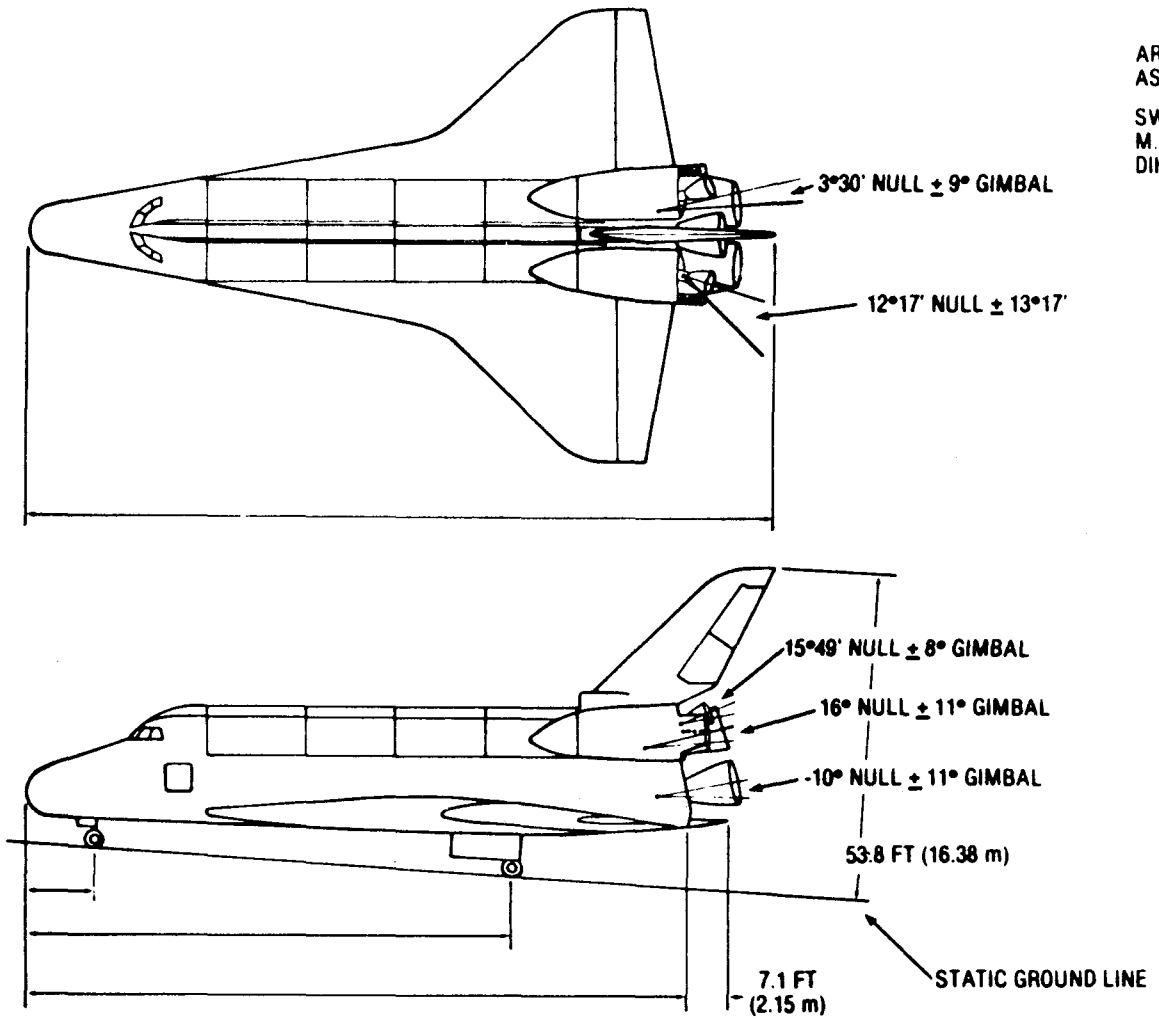
	WING	VERTICAL STAB
AREA	3220 FT^2 (299.14 m^2)	435 FT2 (40.41 m^2)
ASPECT RATIO	2.19	1.68
SWEEP (L.E.)	50°	45°
M.A.C.	525.5 IN (1344.77 cm)	205.0 IN (520.70 cm)
DIHEDRAL (T.E.)	3°30'	NA

Figure 127b

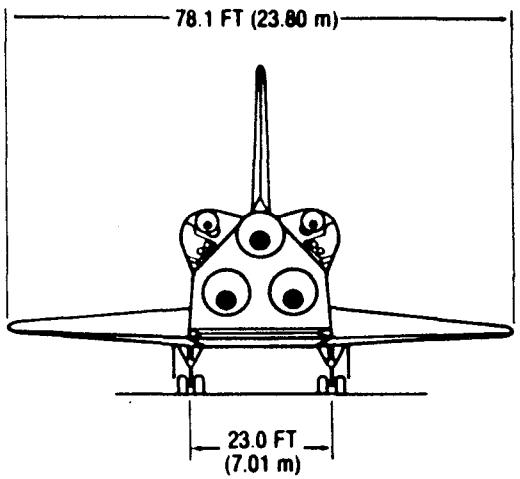
1129

(b) PRR configuration.

Continued.



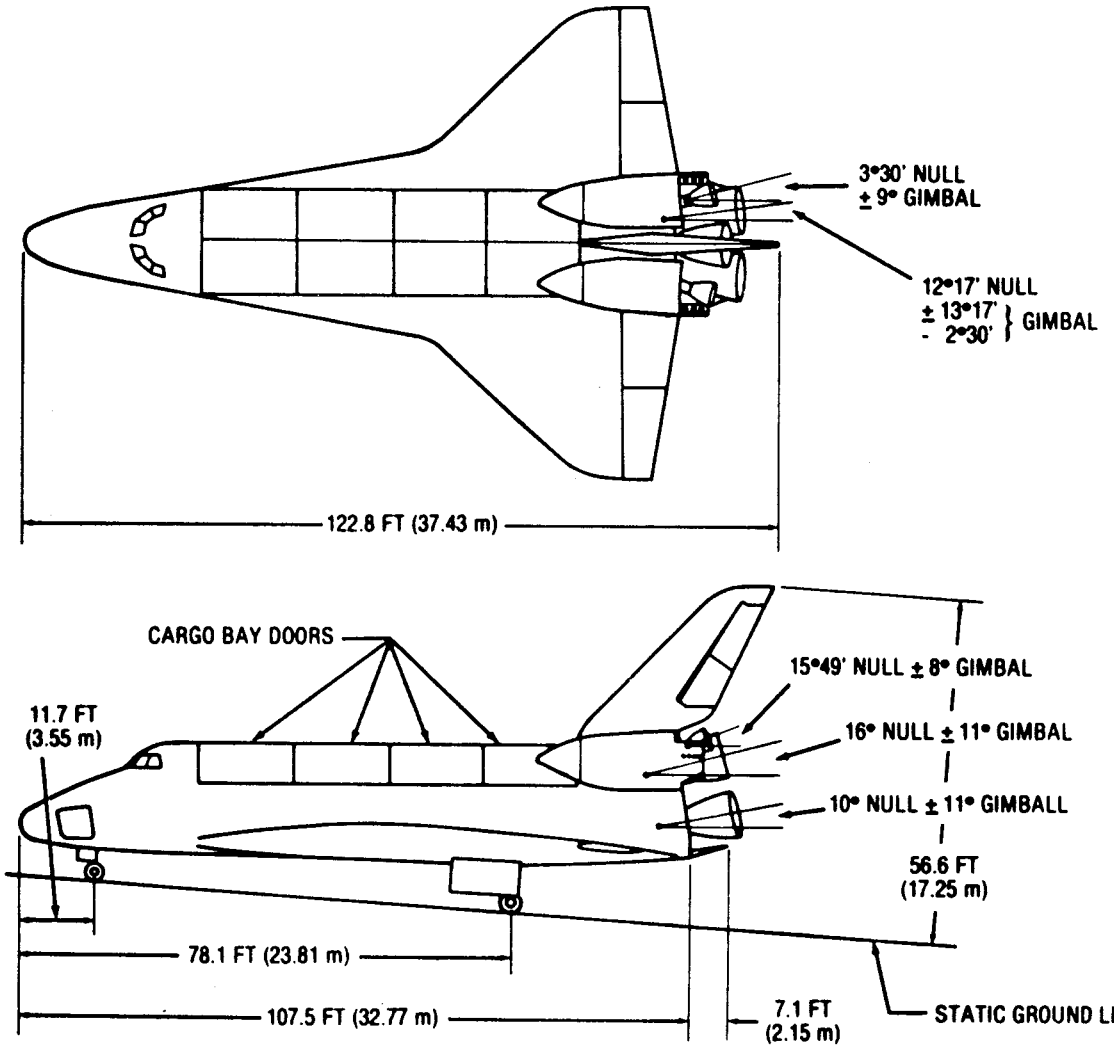
	WING	VERTICAL STAB.
AREA	2690 FT^2 (249.90 m^2)	413.25 FT^2 (38.39 m^2)
ASPECT RATIO	2.265	1.675
SWEEP (L.E.)	45°	45°
M.A.C.	474.8 IN.(1206.0 cm)	199.81 IN.(507.52 cm)
DIHEDRAL (T.E.)	3°30'	—



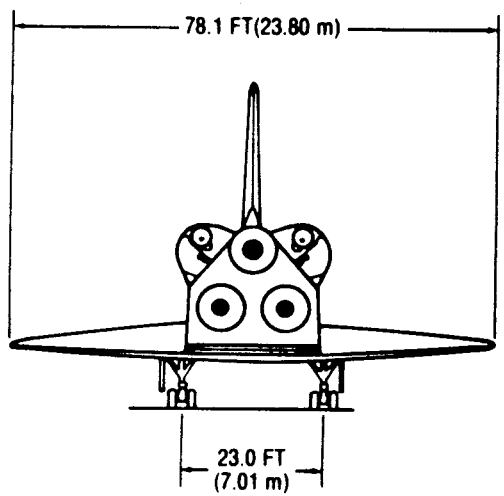
(c) Vehicle 2A.

Continued.

Figure 127c



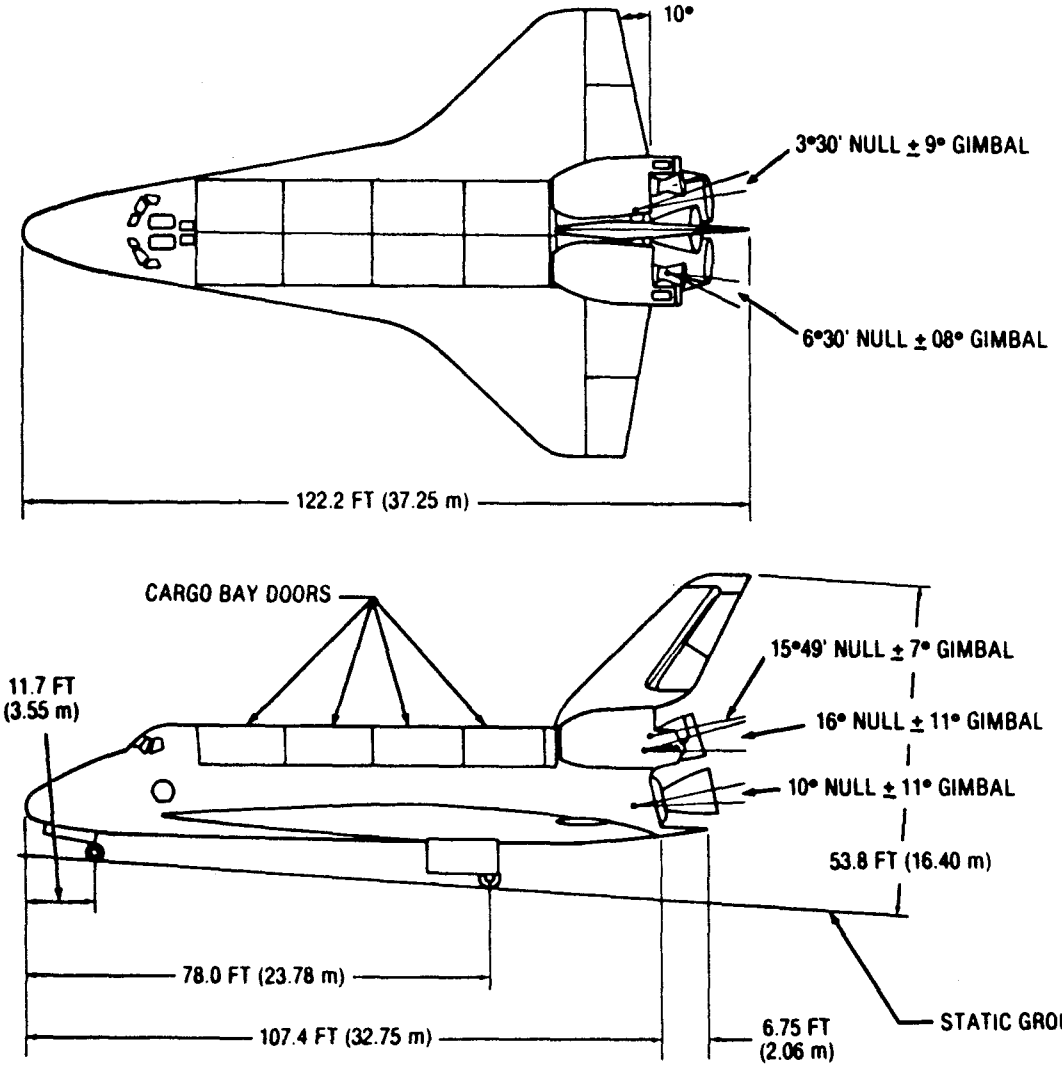
	WING	VERTICAL STAB
AREA	2690 FT^2 (249.90 m^2)	413.25 FT2 (38.39 m2)
ASPECT RATIO	2.265	1.675
SWEEP (L.E.)	45°	45°
M.A.C.	474.8 IN.(1206.0 cm)	199.81 IN.(507.52 cm)
DIHEDRAL (T.E.)	3°30'	—



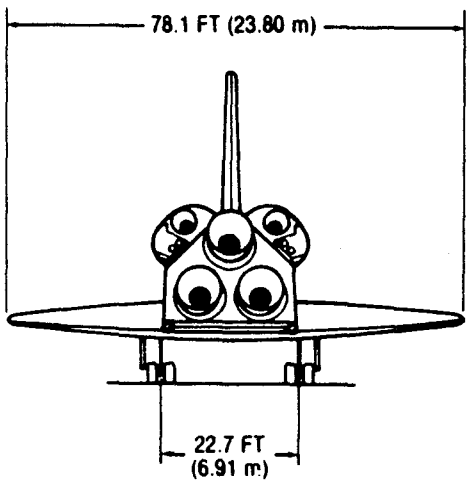
(d) Vehicles 3, 4.

Continued.

Figure 127d



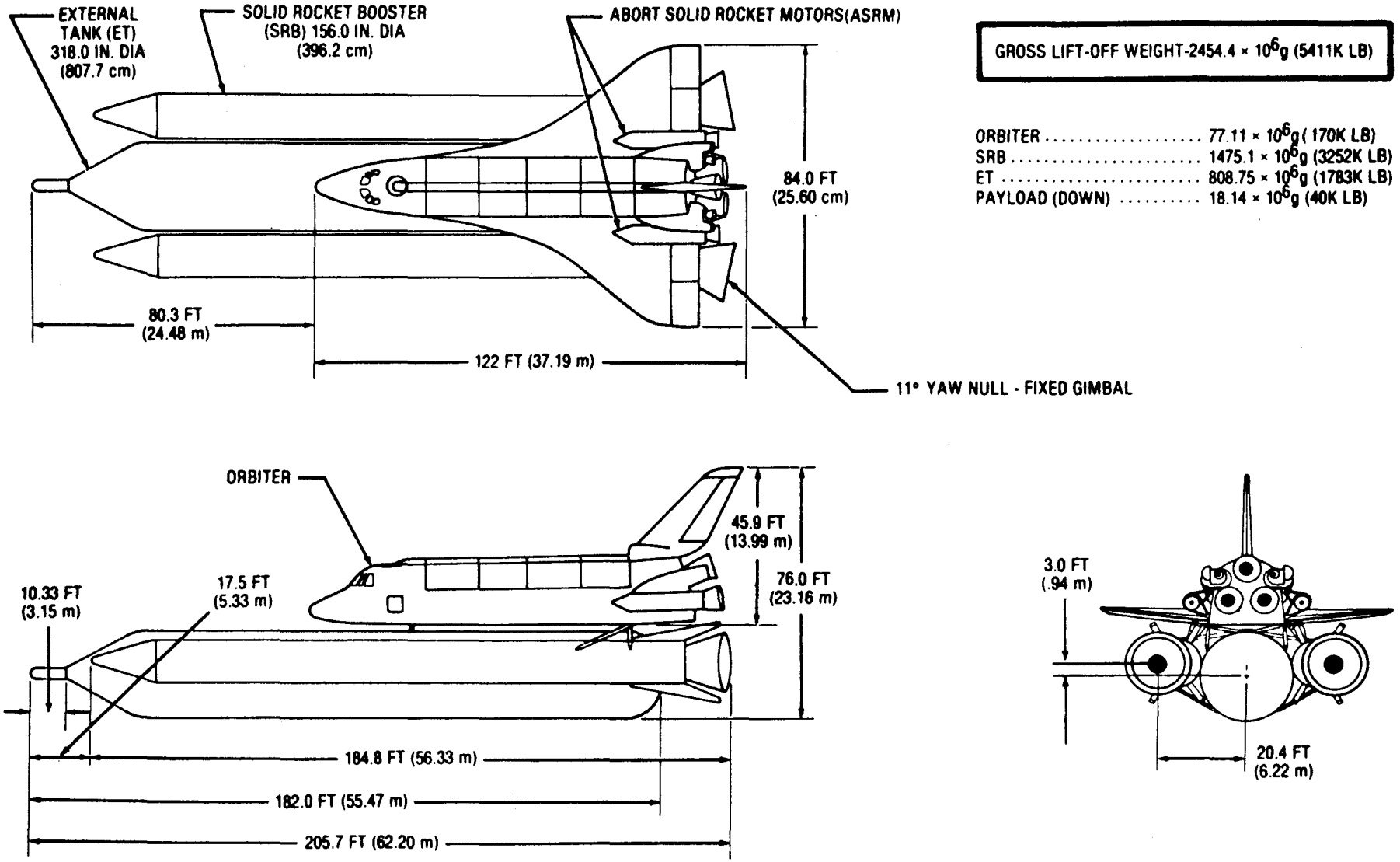
	WING	VERTICAL STAB.
AREA	2690 FT^2 (249.90 m^2)	413.25 FT^2 (38.39 m^2)
ASPECT RATIO	2.265	1.675
SWEEP (L.E.)	45°	45°
M.A.C.	474.81 IN.(1206.0 cm)	199.81 IN.(507.52 cm)
DIHEDRAL (T.E.)	3°30'	—



(e) Vehicles 5, 6.

Concluded.

Figure 127e

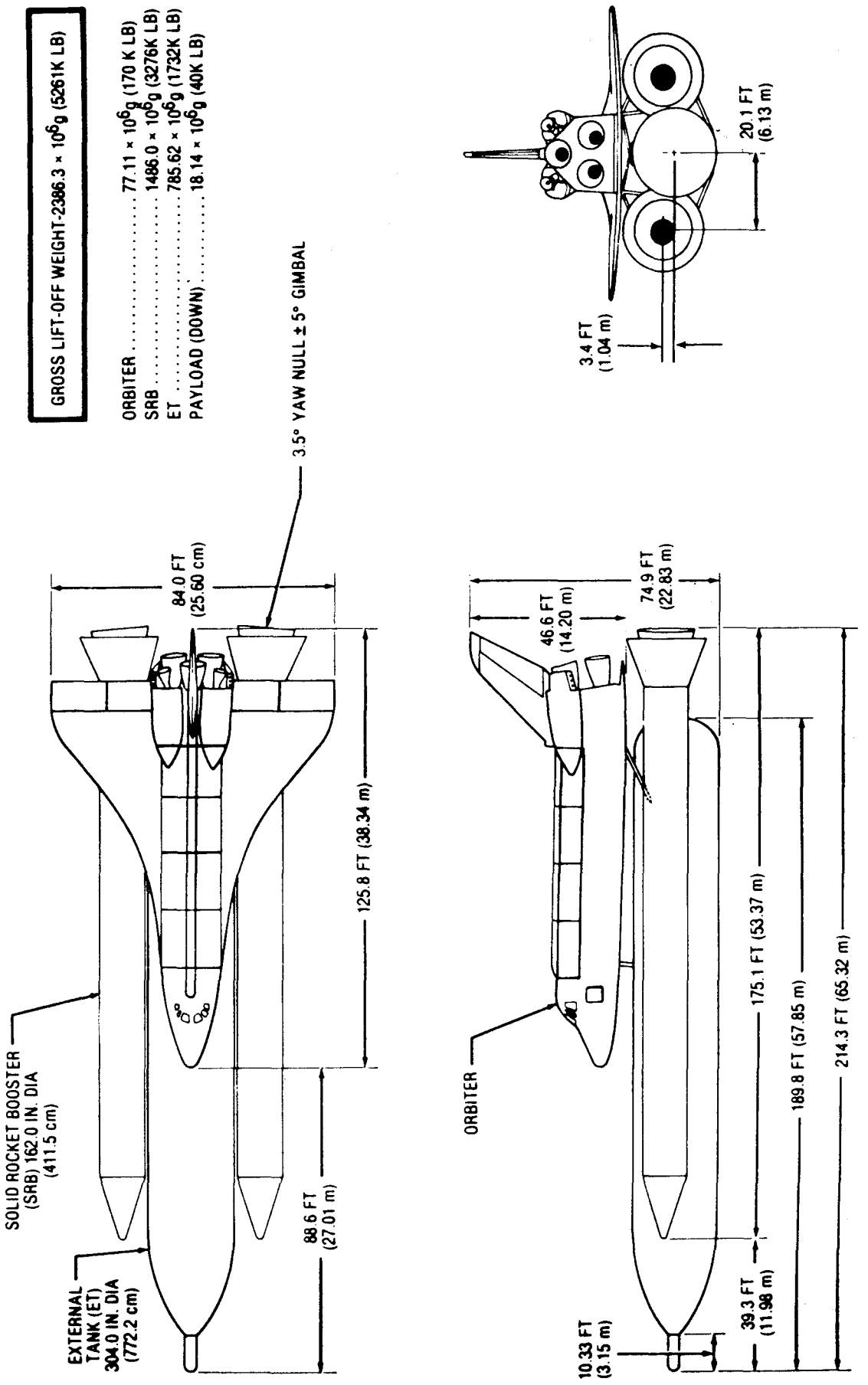


(a) ATP configuration.

Integrated Vehicle dimensions; configuration evolution.

Figure 128a

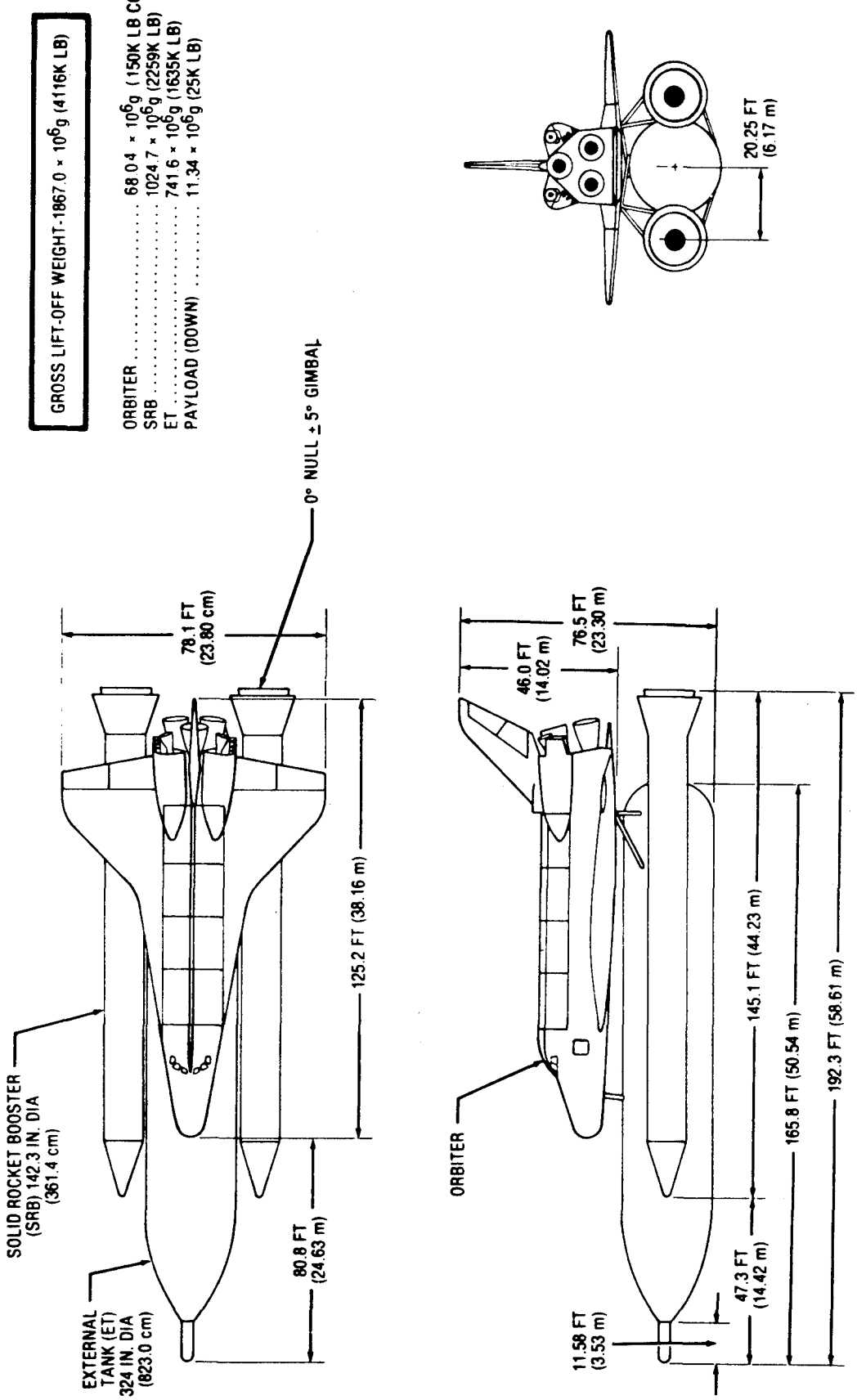
Figure 128b



(b) PRR configuration.

Continued.

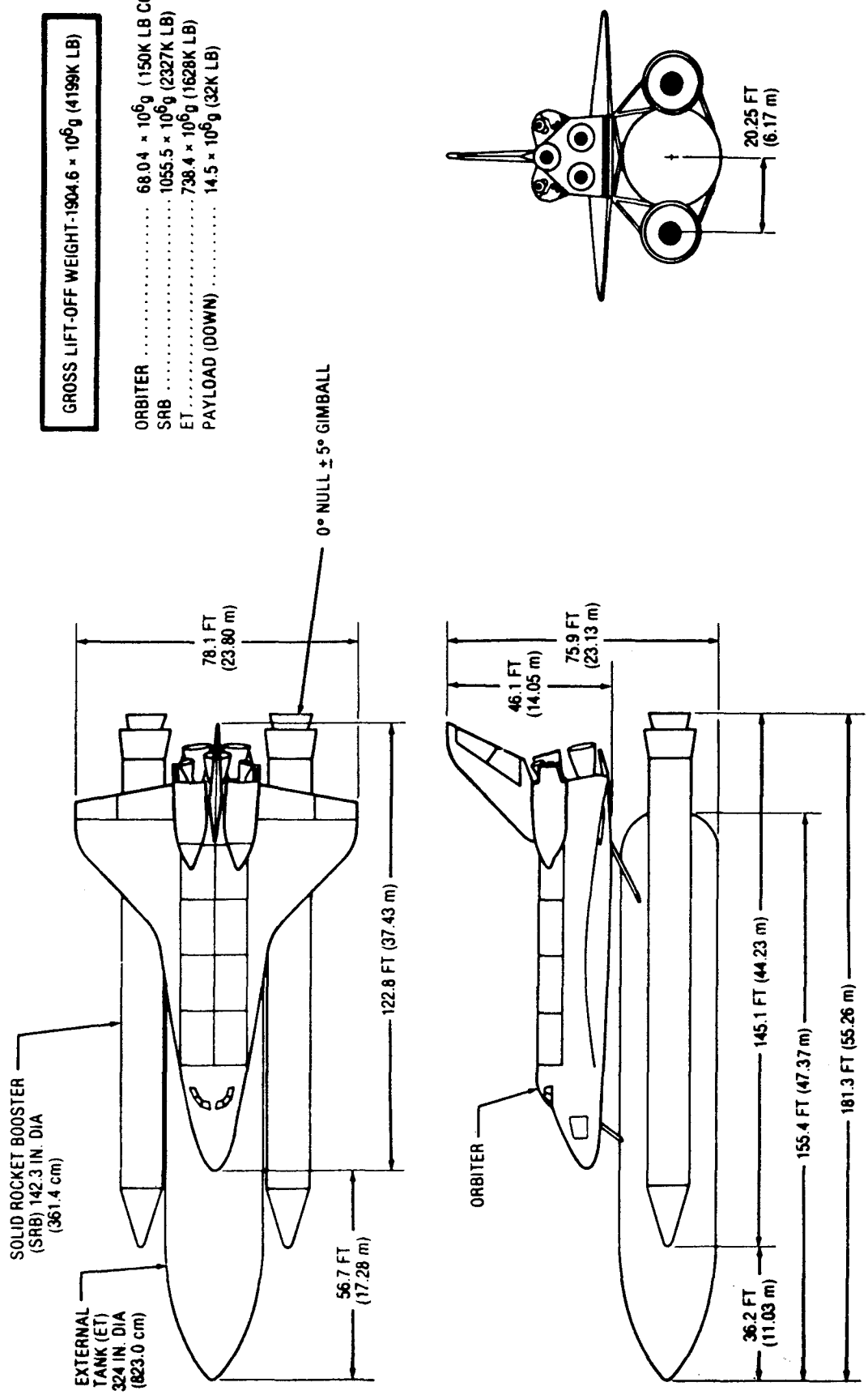
Figure 128c



(c) Vehicle 2A.

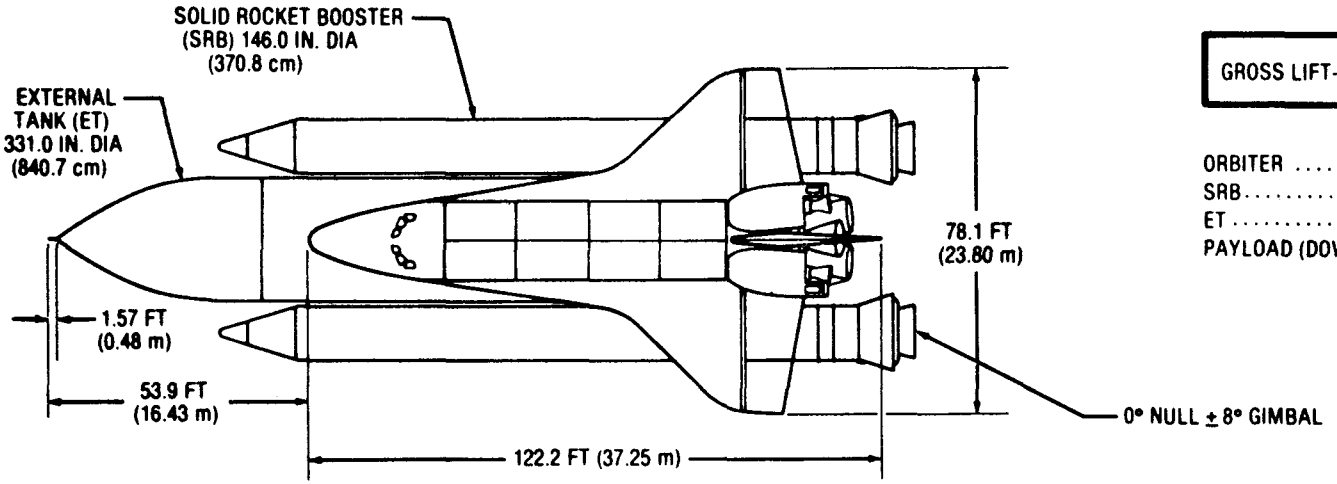
Continued.

Figure 128d



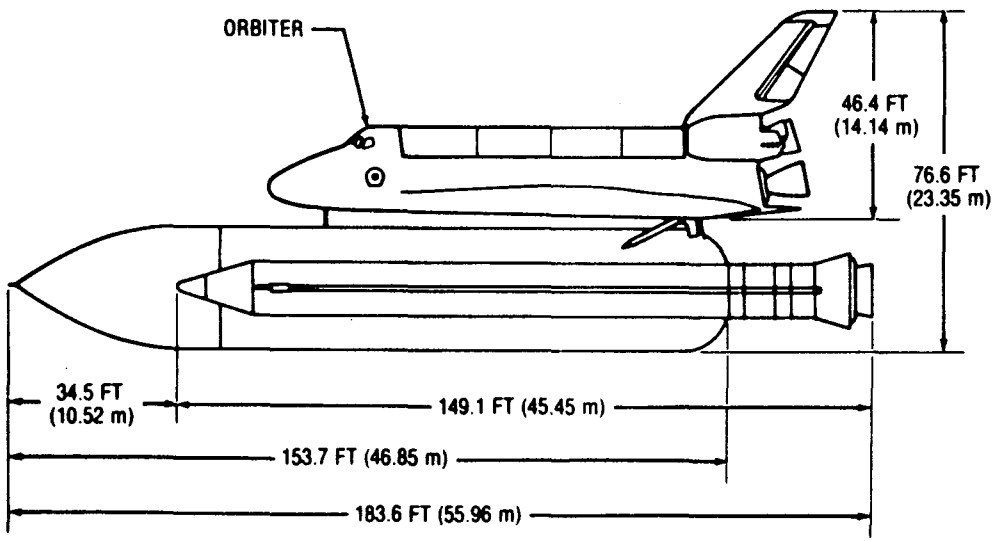
(d) Vehicles 3, 4.

Continued.



GROSS LIFT-OFF WEIGHT-1903.7 × 10^6g (4197 K LB)

ORBITER	68.04 × 10^6g (150K LB CONTROL)
SRB	1055.5 × 10^6g (2327K LB)
ET	739.81 × 10^6g (1631K LB)
PAYLOAD (DOWN)	14.5 × 10^6g (32K LB)



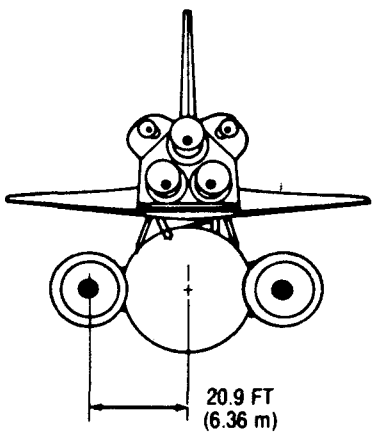


Figure 128e

1137

(e) Vehicles 5, 6.

Concluded.

the forward fuselage. The pilot's eye (cockpit location) was 208 inches (528.3 cm) aft of the nose and had an angular view of 20 degrees up and 24.5 degrees down. The nose radius was 25 inches (63.5 cm) and blended smoothly into the low fineness ratio body. The ATP Integrated Vehicle had the Orbiter attached to the ET "piggy-back" style with the Orbiter nose 80.3 feet (24.48 m) aft of the ET nose. The Orbiter fuselage reference line (FRL) is canted down so that the Orbiter is oriented at a -1.2 degrees incidence with respect to the ET centerline. The SRBs are attached to the ET such that the noses of each SRB are 17.5 feet (5.33 m) aft of the ET nose and 3.1 feet (0.94 m) above the ET centerline. The centerlines of the SRBs and the ET are parallel. Two ASRMs (abort SRMs) are mounted at the aft end of the Orbiter body. The ET is essentially a cone-cylinder arrangement. It is fitted with a retro SRM package at the tank nose to facilitate ET deorbit. The external shape of the retro SRM is a small hemisphere-cylinder with a nose radius of 20.5 inches (52.07 cm). It has a length of 124 inches or 10.33 feet (3.15 m). The conical nose portion of the tank has a semivertex angle of 30 degrees which blends smoothly into the cylindrical section of the ET. The shoulder blending radius at the cone-cylinder juncture is the same as the cylinder radius, 159 inches (403.9 cm). The ET overall length is 182.0 feet (55.47 m). The nose radius of the SRBs is 13 inches (33.0 cm) and the cone semivertex and angle is 18 degrees. The cylinder diameter is 156 inches (3967.2 cm), and the overall SRB length is 184.8 feet (56.33 m). The fixed nozzles are canted outward 11 degrees in the yaw plane so that the boosters will be thrusting through the approximate center of gravity of the vehicle during the boost phase.

The PRR configuration evolved from the ATP configuration based on MCR 0026 (Master Change Record) in October 1972. The most obvious changes follow. The OMS pods were rotated from the aft fuselage side to the aft body shoulder and lengthened slightly. The canopy was moved aft approximately 52 inches (132.1 cm) with an angular view of 7 degrees up and 18 degrees down. The forebody was redesigned to accommodate internal packaging revisions and to improve the transition to the midbody. Wing refinements included an increased thickness ratio, a slight leading-edge droop and minor wing body fillet modifications. The Orbiter incidence was increased to +0.5 degrees to improve the ET separation performance and the OV trim angle. The abort SRMs were deleted. The air breathing propulsion for landing assist following orbital flights was deleted. The ET nose was changed to an ogive

shape to reduce the drag. The SRBs were shortened and moved aft resulting in a slightly longer integrated vehicle. Most of the positioning of the elements (Orbiter and SRBs) relative to the ET, were to improve the element-to-element interference drag. It also alleviated the SRB plume effects on the Orbiter base. The SRBs also had an expanded shroud, had thrust vector control (TVC) added with a reduced precant, and had the aft strakes removed.

Vehicle 2A, also referred to as the "150K Orbiter," (where K indicates 1000 pounds) encompassed the largest changes of all (MCR 0074). Basically the vehicle dry weight and payload down weight were reduced significantly, requiring a complete resizing of the Orbiter. A 45 degrees/79 degrees double-delta wing planform was incorporated with reduced glove leading-edge radius and forward sweep to the trailing edge. It included wing twist, camber, and incidence revisions for improved subsonic performance. Improved low-speed performance and a reduced static margin requirement permitted a reduction in wing size to 2690 square feet (250 square meters) and resulted in rebalancing of the OV to meet stability and control requirements. Nose camber and radius, body cross section, and upward sloping forebody slab sides were selected to improve hypersonic pitch trim and directional stability. By combining them with wing-body blending, entry heating was reduced on the body sides. These changes also simplified the nose structure. The CG travel requirement was reduced from 3 to 2 percent body length. The ferry air breathing engines were moved to a position under the wing (not shown). The SRBs were shortened and moved aft further resulting in a shorter overall vehicle. The SRB yaw gimbal setting was reduced to 0 degrees and the nozzle flare angle was reduced. The ET was also shortened and the Orbiter was repositioned on the ET near its original location.

Vehicles 3 and 4 were essentially the same from an aerodynamic configuration viewpoint. Vehicle 3 was initiated with MCR 0200, with seven revisions carrying through the start of Vehicle 4. Early changes shortened the body by 38 inches or 3.17 feet (0.97 m) and smoothed the body nose area while incorporating a smaller nose radius at the same time. The wing glove leading edge was increased to 81 degrees and the incidence was decreased from 3 degrees to +1/2 degrees. Also some minor airfoil changes were made, the wing was lowered 4 inches (10.2 cm), and the lower body was refaired. In addition the bodyflap span was reduced. The primary purpose of these changes was to improve the overall aerodynamic and

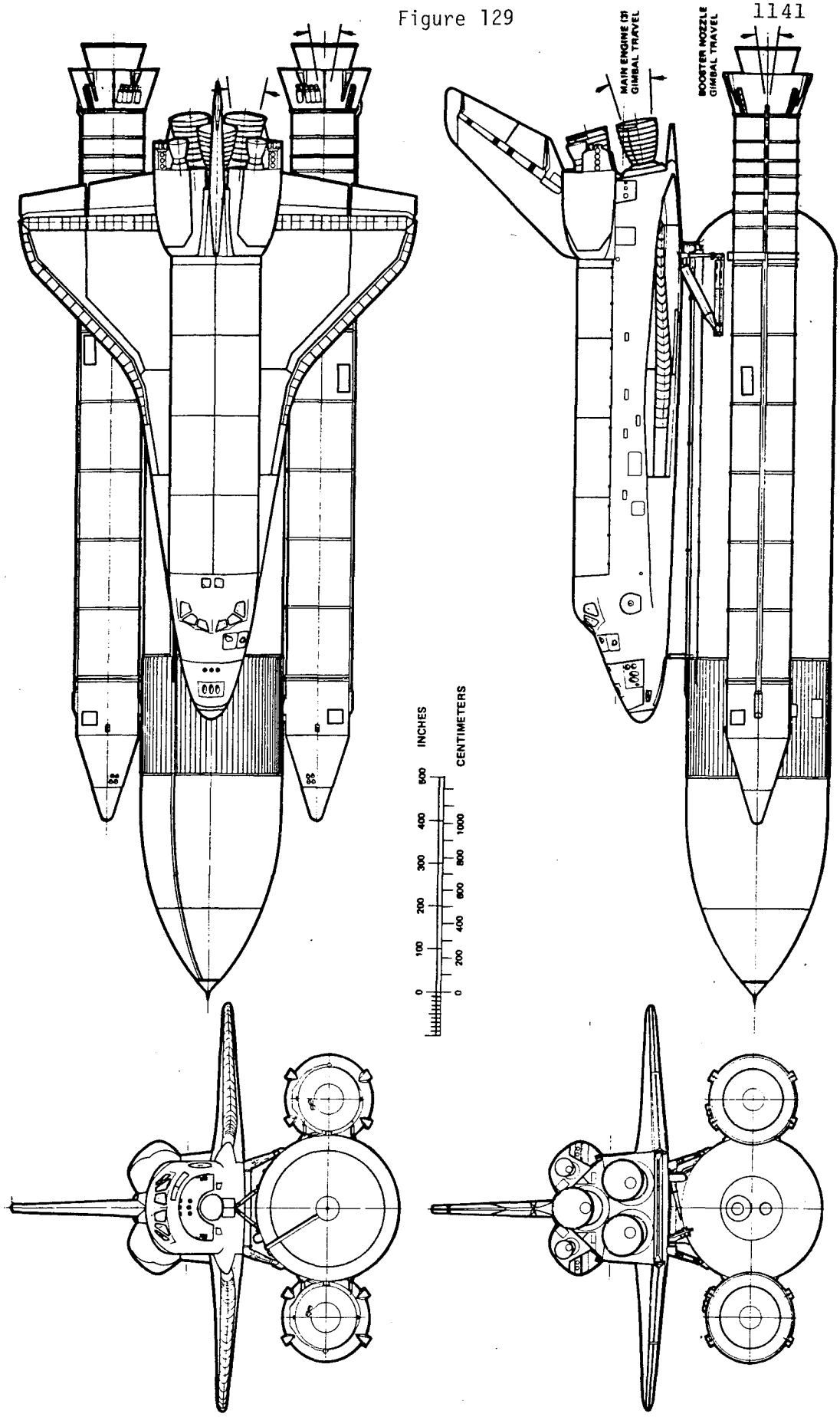
aerothermodynamic performance. The CG range requirement increased to 2.5 percent body length to allow 1/2 degrees for aerodynamic trim uncertainties with a new payload down of 32,000 lbs (14,515 kg). In addition, the manipulator arm dorsal fairing along the top of the payload bay doors was deleted. The manipulator was stowed inside the payload bay. Later Orbiter changes (mid-1973) included thickening the airfoil 6 inches (15.24 cm) at the elevon hingeline. In early 1974 the ferry air-breathing engines were deleted. The SRBs were moved forward relative to the ET as was the Orbiter. The ET was shortened and the retro package (spike) removed. The result was a slightly shorter overall vehicle length.

The Vehicle 5 Orbiter had the OMS pods shortened and refaired to clear the payload bay doors in early 1974 (MCR 0500). Other lesser external shape changes modified the wing tips, increased the elevon gaps, and deleted the vertical tail drogue chutes. Later (mid-1975) changes added recessed thermal glass in the windshield, observation windows, and hatch windows. Also the covers were removed from the forward RCS ports and from the umbilical doors in the aft body. The ET and SRB length changes as well as the distance of the SRB and Orbiter aft of the ET, were in approximately 4 feet or less. An ascent air data system (AADS) in the shape of a cone was added to the tip of the ET.

Vehicle 6 had no significant external shape differences from Vehicle 5.

Figure 129 shows the final Shuttle configuration as flown into space in 1981. With these final configuration changes in hand, Rockwell and NASA were ready to begin fabrication of the first Space Shuttle. Rockwell initiated fabrication on June 4, 1974, completing this first craft, Orbiter Vehicle 101 (OV-101) in September 1976. OV-101, subsequently named the Enterprise, lacked many of the features of a true orbiter, and thus was, in many ways, a full-scale flying mockup of the first orbital Shuttle, OV-102 Columbia which, in any case, did not itself fly until 1981, three years after its planned initial orbital missions, due to unexpected development problems with the SSME and the craft's thermal protection system. With the completion of Enterprise, the Shuttle program moved from the drawing board to the flight stage,

Figure 129



NASA SPACE SHUTTLE TRANSPORTATION SYSTEM

for Enterprise was destined to be the first Shuttle to fly, albeit in the atmosphere, serving to validate the craft's anticipated approach and landing characteristics.

Shuttle's approach and landing tests got underway in 1977, but preflight planning had gone on long before. In the fall of 1974, the Air Force and NASA had executed a joint agreement to established Space Shuttle support and test facilities at Edwards, including the construction of a huge "mate-demate" gantry and hoist facility enabling a modified Boeing 747 transport to carry the Shuttle for air launch and ferry purposes. Edwards would serve as the approach and landing test (ALT) site, as well as the prime landing site for Shuttle's first orbital voyage. NASA's Johnson Space Center (as the Manned Spacecraft Center had been redesignated) would have overall mission control authority, with NASA's Dryden Flight Research Center at Edwards in a supporting role. The Air Force Flight Test Center at Edwards would also provide technical support and analysis on an as-needed basis. NASA would arrange for shipping the orbiter from its construction facility at Air Force Plant 42, in Palmdale, California, approximately 40 miles overland to Edwards, necessitating construction of a special "Shuttle road" (basically an extension of 10th Street East, north of Avenue E up to Rosamond Boulevard, the latter a paved surface road from the town of Rosamond to Edwards).[34]

In the early 1970s, NASA had bought a surplus Boeing 747-100 airliner formerly owned by American Airlines for $16 million for use by Johnson Space Center as a Shuttle transporter and launch vehicle. The selection had been a natural one, as the only alternative aircraft, the Lockheed C-5A Galaxy strategic airlifter, was not only in short supply, but had a so-called "T tail" design layout that did not recommend itself to the Shuttle drop vehicle role. If, for example, the Shuttle rammed the C-5's vertical fin during separation, the C-5 would almost certainly be

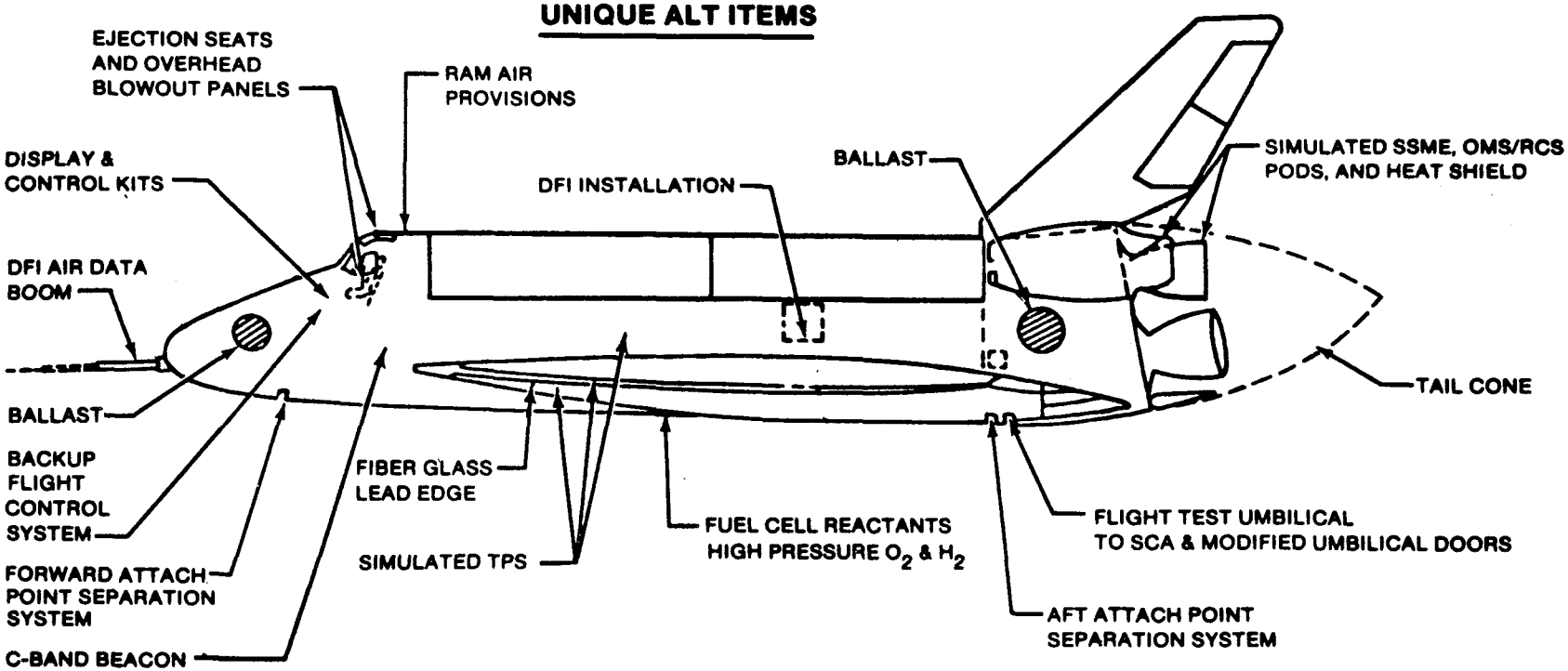
lost since the horizontal tail was perched at the top of the fin The 747, with a more conventional lower horizontal tail location, was much more damage tolerate, and likely could survive even if the top of its vertical fin were damaged or destroyed. The idea of using a 747 to air-launch a spacecraft the size of the Shuttle (itself a vehicle the size of a DC-9 jetliner) struck some individuals as ludicrous. Senator Barry Goldwater of Arizona, for example, arguably Congress's most space-conscious and enthusiastic aviation supporter, had repeated exchanges with NASA officials over the whole issue, which he considered very dangerous; as LeRoy Day subsequently recalled, watching Senator Goldwater when the Shuttle successfully separated from the 747 and landed on Rogers Dry Lake, "I could barely resist the temptation to go up and tell him it was just like we had said it was going to be, and it really wasn't all that dangerous."[35] Figure 130a and 130b show the configuration of OV-101 Enterprise for the ALT, and a schematic view of the mated 747-Orbiter.

By mid-1976, Shuttle test plans were nearing completion. OV-101 rolled out of the Palmdale plant amid ceremonies on September 17, 1976. In January 1977, this craft, named Enterprise, rolled overland on a special transporter from Palmdale to Edwards, using the newly graded "Shuttle road." Briefly, the glare of national publicity descended upon Edwards as news crews more familiar with the lush Florida coast ventured to the High Desert to cover the first flights of the Shuttle program. The flight test program had three phases: captive, captive-active, and free flights. The unmanned captive flights would demonstrate whether the combination 747-Shuttle (which wags dubbed the world's largest biplane) could fly together safely. In the captive-active trials, an astronaut crew would ride in the Shuttle. Finally it would be launched from the back of the 747 and flown down to a landing. During the captive flights and the first of the free flights, the Shuttle's blunt base would be faired over with a tailcone to reduce buffeting on the 747's vertical fin; as another

ORBITAL SYSTEMS NOT INSTALLED

- OMS
- RCS
- SSME'S
- EPS CRYO. TANKS
- PAYLOAD ACCOM
  - WATER, WASTE, & FOOD MGMT. SYSTEM
- RADIATORS
- TPS
- CREW STATION
  - PAYLOAD SPECIALIST
  - ON ORBIT
- STAR TRACKERS
- UNIFIED S-BAND
- RENDEZVOUS RADAR AND KU-BAND

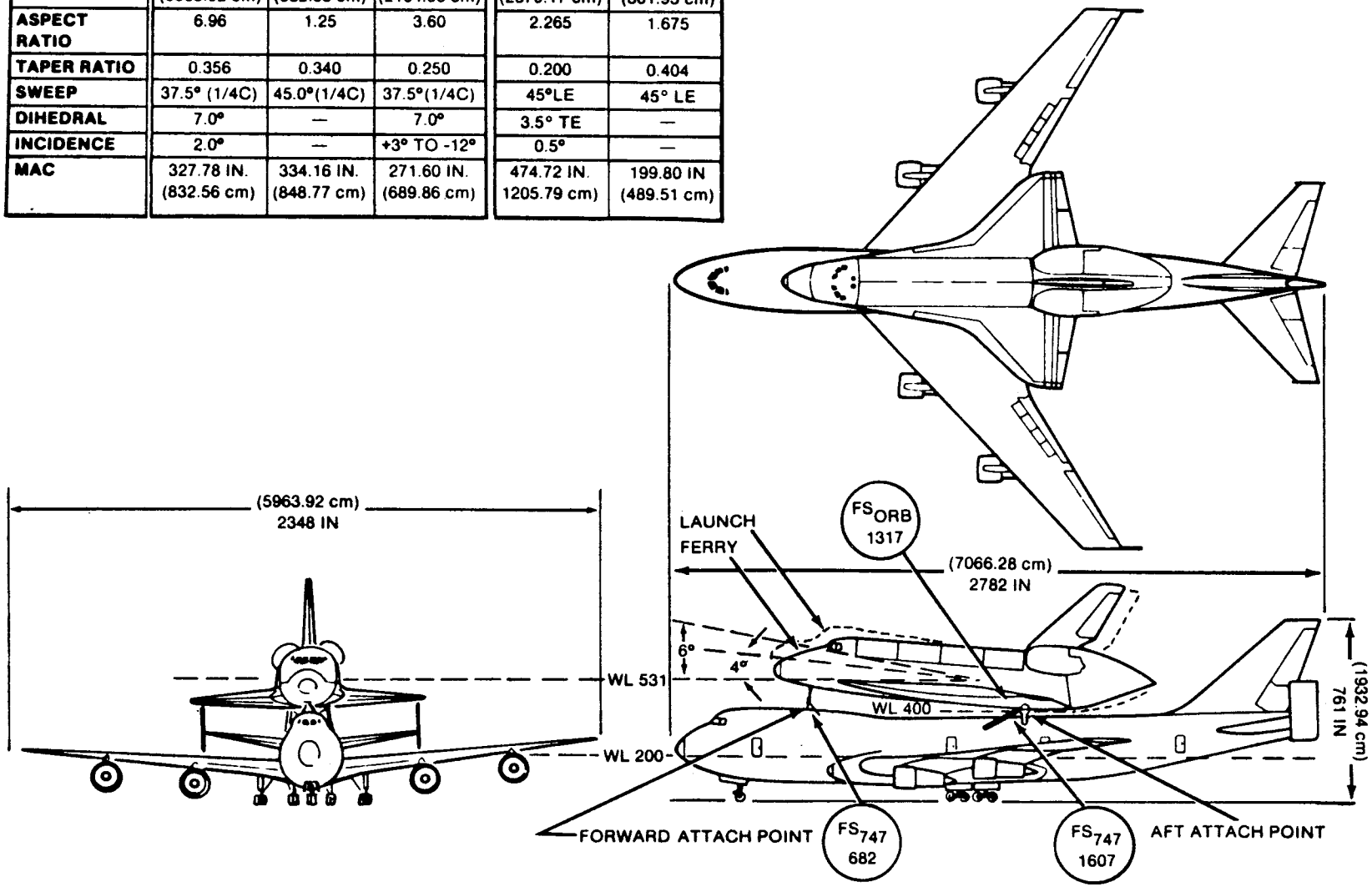
UNIQUE ALT ITEMS



ENTERPRISE AS CONFIGURED FOR THE APPROACH AND LANDING TESTS

Figure 130a

	747			ORBITER	
	WING	VERT	HORIZ	WING	VERT
AREA	5500 FT^2 (1532.85 cm^2)	830 FT^2 (231.32 cm^2)	1470 FT^2 (409.69 cm^2)	2690 FT^2 (749.70 cm^2)	413.25 FT^2 (115.17 cm^2)
SPAN	2348 IN (5963.92 cm)	387 IN (982.98 cm)	864 IN (2194.56 cm)	936.68 IN (2379.17 cm)	315.72 IN (801.93 cm)
ASPECT RATIO	6.96	1.25	3.60	2.265	1.675
TAPER RATIO	0.356	0.340	0.250	0.200	0.404
SWEEP	37.5° (1/4C)	45.0°(1/4C)	37.5°(1/4C)	45°LE	45° LE
DIHEDRAL	7.0°	—	7.0°	3.5° TE	—
INCIDENCE	2.0°	—	+3° TO -12°	0.5°	—
MAC	327.78 IN. (832.56 cm)	334.16 IN. (848.77 cm)	271.60 IN. (689.86 cm)	474.72 IN. 1205.79 cm)	199.80 IN (489.51 cm)



GENERAL ARRANGEMENT OF THE 747 AND SPACE SHUTTLE ORBITER

Figure 130b

precaution, Boeing had added two more vertical fins to the 747's horizontal stabilizer. Toward the end of the flight trials, NASA hoped to launch the Shuttle without the tailcone, which would reduce the Shuttle's lift-to-drag ratio, resulting in a descent path similar to what it would have upon returning from orbit. A series of high-speed taxi tests by the mated 747 and Enterprise in mid-February 1977 went without a hitch.[36]

On the 18th the first Shuttle flight proved to be a media event unparalleled in the brief history of Dryden. For the previous week, Johnson and Dryden public affairs officials had been on hand to meet the demands of the hundreds of media representatives who left plusher locales for the sunny but blustery desert. Those who spend the night in Lancaster and Palmdale had to get an early start. At 5 a.m., the sky was still black and clear, the stars as brilliant as always, the temperature in the low 20s. Autos moved along the Sierra Highway, down Avenue E, then north on 120th Street East. Despite the urban-sounding names, the surrounding country was bare, scrub desert broken only by an occasional homestead. As the sky began to lighten, Joshua trees and the low hills near Hi Vista were outlined. The revolving beacon at Edwards pulsed brightly on the northern horizon, and the 6 a.m. news on KNX reported that the Shuttle would fly today. Dryden itself was controlled pandemonium, the public affairs trailer a madhouse. By 6:45 the sun was spreading a warm glow through the thin fog covering the lakebed. Those present prepared to convoy out to the runway; meantime they drank coffee and watched the TV monitors in the public affairs trailer.[37]

By 7 a.m., the Shuttle launch crew, Fitz Fulton, Tom McMurtry, Vic Horton, and Skip Guidry, were in the 747, the inert Shuttle riding on top. No sooner had the reporters journeyed from Dryden to the press site along runway 04-22 than the Air Force staged an impromptu airshow: the YC-14 took off, followed by the B-1, some

T-38s, an F-4, and the F-16. Finally those at the site watched the 747-Enterprise combination taxi slowly past the Air Force's two large hangars, down to the west end of 04-22. There it held while the test crew completed final checks. Aloft, two NASA T-38s flew over, as if impatient to get on with the flight. Camera crews set up their tripods, shivering in the brisk desert morning. It was a beautiful day. Right down the center of 04-22 flew a gaggle of geese--a large V honking along, heading east, unperturbed by the consternation they were causing. Geese and jet engines do not mix, so NASA delayed the departure of the 747 a little longer. Finally, the 747 started to roll down the runway with that peculiar whine so typical of large fan-jet airplanes.

The world's most improbable aerial combination, after a run of 5900 feet, became airborne, climbing ponderously toward the east, above the lake. For 125 minutes, this strange hybrid flew along, anxiously attended by T-38 chase planes, before Fulton and McMurtry returned it gently to earth. First flights are always cautious, and on this one, the test crew held the combination to a maximum altitude of 16,000 feet and a maximum speed of 287 miles per hour (mph). Everything went well; the 747-Enterprise flew closer in performance to a standard 747 than simulations had predicted. Nothing serious had happened, a tribute to the test planning. That afternoon and evening, the dark interior of Lancaster's Longhorn Saloon echoed to the jubilation of Dryden, Rockwell, and Boeing personnel. The Shuttle had taken to the air.

Back at Dryden, over the weekend engineers worked up the data from the flight. At a technical and crew briefing on Monday, the word went forth: "Testing can go on to expand the envelope as planned." The critical concerns of buffeting, flutter, and tail loads proved to be no problems during four more flights. After five complete successes, NASA abandoned a planned sixth flight, deeming it unnecessary.

NASA had already selected four astronauts for the Shuttle landing tests, placing Fred W. Haise and Charles G. Fullerton on one crew and Joe H. Engle (the former X-15 pilot) and Richard H. Truly on the other. Haise, a former center pilot, had flown on the ill-fated Apollo 13 mission. They prepared for the Shuttle program by practicing in a ground simulator and flying a much-modified Grumman Gulfstream II. Other pilots flew the center's Jetstar to test the Shuttle's microwave scanning-beam landing system (the MSBLS, pronounced "Miss Bliss"). In addition, Dryden managers worked closely with their Johnson counterparts to prepare for a most important part of the Shuttle test program: arranging for the Houston center to control the mission while it was in progress at the desert. Dryden had controlled the captive inert flights, but Mission Control Center at Johnson would have primary responsibility for running subsequent missions, starting when the 747 and Enterprise backed away from the Shuttle mate-demate facility and began taxiing to the runway. By mid-June all was ready, and the Shuttle flight test program moved into its next phase.[38]

On June 18, 1977, the 747 and Enterprise combo went aloft on the first "captive active" test. Inside the Shuttle, Fred Haise and Gordon Fullerton had a magnificent view; not being able to see any portion of the carrier aircraft added to the illusion that they were alone in the sky. The flight lasted nearly an hour and all objectives were achieved. The test data indicated that the Space Shuttle was buffet- and flutter-free up to the maximum speed attained on the flight, 208 mph. The next captive-active mission, flown by Engle and Truly on June 28, involved high-speed flutter tests up to 310 mph. It too was successful. NASA concluded that the four flights originally scheduled for the captive-active phase could be safely cut to three. On July 26 Haise and Fullerton completed the last of the captive-active flights. During this last mission, 747 pilots Fitz Fulton and Tom McMurtry flew a launch separation profile, pushing the 747 over into a shallow

dive at 27,800 feet and lowering the 747's landing gear to simulate the free-flight launch conditions. During approach of the 747-Enterprise combination to landing, Haise and Fullerton lowered the Shuttle's landing gear to check its operation. It went smoothly.

Indeed, the captive-active phase of the Shuttle testing had gone pleasingly well. Some equipment problems had been experienced; auxiliary power units leaked or overheated, computers were "voted" off-line by other computers, and sometimes a computer tried to take the square root of a negative number. These were small concerns that could be remedied by minor fixes or software changes. The important fact was that the Shuttle and the 747 were a safe flying combination. Now NASA could move to the next phase of the approach and landing tests: the actual free-flight testing of the Enterprise. The flock of news personnel, who had left the desert in droves after the first captive flights, now swarmed back.[39]

During the week of August 8, project officials concluded a two-day Shuttle readiness review and a mission readiness review; all conditions were "go." Most visible - and audible - of the preparations were the Shuttle simulation flights that Fred Haise and Gordon Fullerton made in the Gulfstream II training aircraft; for the few days prior to the flight, the center echoed to the occasional rumble of the Gulfstream and its T-38 chase planes climbing out over the lake following another approach to landing, or the center's Jetstar checking the microwave landing system.

The flight plan called for the 747-Enterprise to take off at 8 a.m., from runway 22 and climb to the west. The two mated craft would enter a racetrack pattern, flying south toward Los Angeles, turning north over the mountains, and coming up the east side of Rogers Dry Lake. The craft would nose into a shallow dive from an altitude of over 27,000 feet. At 24,000 feet and an airspeed of

approximately 320 mph, Fred Haise in the Shuttle would press a square white button on the Shuttle's instrument panel, triggering explosive bolts that would separate the Enterprise from the 747. If all went well, Fulton would roll the 747 into a descending left turn, and Haise would pitch up the 75-ton Enterprise to the right. At 20,000 feet Haise would initiate a practice landing flare to evaluate the handling qualities of the Enterprise. Then, sinking like a rock, the astronaut crew would begin a gradual 180 degrees turn to position the Enterprise for a 200 mph touchdown on lakebed runway 17, which, at over 7 miles, was the longest of the Rogers runways.

The flight attracted massive public attention. Over 1000 reporters flocked to Dryden, many from abroad. Parking had to be prepared for the public viewing sites west of Dryden, and the VIP and press sites along runway 04-22 and the west shore of the lakebed, parallel to runway 17. Motels as far away as eastern Los Angeles were booked solid. A wide range of aviation personalities, including NASA Administrator Robert Frosch and such pioneers as Jimmy Doolittle, were present.

As with most test flights, the preparations took days of hard work at all levels. The event came alive in the early morning of August 12. At 3 a.m., the first reporters left Lancaster and Palmdale for the lakebed. Once again, the night was perfectly clear; after driving up 120th Street through the base gate, the cars made their way to the FAA radar facility by Hospital Road on old South Base, turned right, and drove through an Air Police checkpoint. Those with authorizations continued on toward the runway site or the lake. The lake was better, at least for those with a handle on the past and an awareness of the present. Eerily quiet and still, the lake seemed unconnected with civilization. Further away could be seen the night lights of the mines at Boron and the bustle of activity at Dryden. The Air Force side of the field was still and dark, except for watchlights and the tower and runway lights.

For those interested in omens, the Shuttle's day began with a meteor shower. Looking up toward the Milky Way, clearly visible as a faint patchy white in the crisp desert sky, observers saw a rain of fire, with meteors coming down by the minute. There were fireballs breaking apart in greenish-white trails, streaks of russet, streaks of yellow. Then came the desert dawn, the familiar yellow glow lighting up the eastern sky, shining through high clouds, and bathing the rocky outcroppings of Leuhman Ridge in orange, and finally reaching down to illuminate the broad baked expanse of Rogers. The lights on buildings dimmed, and soon an Air Force helicopter clattered noisily over the lakebed, joined by another from Dryden. Far from the lake, between 60,000 and 70,000 visitors streamed along the Sierra Highway and Rosamond Boulevard into Edwards; at one point, the traffic jam stretched 16 kilometers. As journalists whiled away the time setting up equipment and sipping coffee from a Rockwell courtesy van, the technicians, engineers, and flight crews at Dryden readied themselves for the flight.

Finally all was ready, and the 747-Enterprise backed out of the mate-demate gantry at Dryden, ran up its engines, and began the long taxi. The Air Force Huey still clattered above. The first of the T-38 chase planes whistled aloft. The 747-Shuttle reached the east end of runway 22, turned, and held for the last checks. At 8 a.m., right on schedule, Fulton called up full power; the combination, with surprisingly little noise, began to roll and nosed aloft, followed by two T-38s. The aircraft climbed into the prescribed racetrack pattern, joined by the other three chase T-38s. On the ground, the reporters waited for the big moment.

The air launch had been scheduled for 8:45. In fact, higher-than-normal temperatures at altitude caused the climb to take longer than planned. The 747-Shuttle moved majestically around the racetrack, plainly visible most of the time from the lakebed.

The low sun obscured the view of its approach to launch, but video coverage from one of the T-38s outfitted with a portable camera was stunning. The formation continued over Saddleback Butte to the Edwards bombing range. Roughly 48 minutes into the flight, the 747-Shuttle was due east of Rogers lake, at an altitude of 28,390 feet. Fitz Fulton nosed into a shallow dive. Fred Haise radioed Fulton, "The Enterprise is set; thanks for the lift." Then he punched the separation button. Seven explosive bolts detonated and the Shuttle was flying on its own at 24,100 feet. The 747 pitched down slightly and rolled into a diving left turn, and Haise briefly pitched up to the right. He initiated a practice landing flare at about 285 mph and made moderate lateral control inputs to evaluate the Shuttle's response. The big delta handled well. Because of the Shuttle's low lift-to-drag ratio, it would remain aloft only for about five minutes. Later, after removing the drag-reducing tailcone, the Shuttle would sink to earth in about two minutes, a descent rate similar to the X-15's.

On the ground, the separation had been seen by some with binoculars and sun shields. Soon, it became visible to all. The 747 flew alone, trailed by a single T-38, while to the northeast a white speck could be seen growing in size at what seemed a remarkable rate, attended by four T-38s. The cameramen started clicking furiously, and exclamations sounded on all sides. The Shuttle descended over Leuhman Ridge, passed across Highway 58 at Boron, turned west toward Peerless Valley, swung around over North Edwards, and lined up on runway 17. Houston's Mission Control radioed Haise that the Enterprise had a lower lift-to-drag ratio than predicted by tunnel tests. In fact, however, the ratio was just as predicted; Houston had miscalculated. The error caused Haise to fly the final approach at a higher speed, conserving energy to prolong the glide. As a result, the Shuttle was "high and hot" on its final approach. Realizing that the Enterprise would land long, Haise deployed the craft's speed brakes from 30 up to 50 percent. At 900 feet altitude, Haise began the landing

flare. As the Enterprise leveled out, he deployed the landing gear. The Shuttle landed long by about 3000 feet at 211 mph, nearly 5½ minutes after launch. The Shuttle coasted for 2 miles before stopping on the south lakebed; as it slowed, its T-38 chase planes streaked by. Soon the 747 and its lone chase plane swept majestically over the landing site. The first Shuttle free flight had been a success. Now all that was left for most at the lake was the long trip around the base to Dryden, a quick lunch, and the afternoon press briefing. For the engineers, however, the task of data reduction had just begun.[40]

After the press conference, many called it a day and went to one of the many parties being hosted by mission personnel in and around Lancaster. Most wound up at the main blow-out, held at Lancaster's Delta Lady saloon. Others settled for the more tranquil but no less joyous environment of the Desert Inn or Mr. B's Twin Lakes Inn outside Palmdale. Shuttle obviously flew well - better than the Gulfstream simulator. The major remaining question was how the Shuttle would behave without its tailcone. This actually involved two considerations. One was whether the buffet from disturbed air caused by removal of the cone would cause structural problems for the 747's vertical fin during the climb. The other was whether the Shuttle's low lift-to-drag ratio--made even lower by removal of the tailcone--would present serious piloting problems. After all, the descent rate of the craft would just about double, reducing flight time from over five to just over two minutes. Pending a decision to fly "tailcone off," Shuttle testing continued with the Enterprise's blunt end still sporting the pointed tailcone.

Rain on the lakebed and other delays deferred the next free flight to September 13, when former X-15 pilot Joe Engle and copilot Dick Truly dropped down to the lake, all the while taking data on the craft's longitudinal, lateral, and directional response and lift-to-drag and flutter characteristics during

approach and landing. Nothing unusual aloft had occurred, but on the ground a power surge at Dryden had briefly caused a loss of all radar data. Fortunately, after a few minutes everything had come back on line and the flight had continued. Ten days later, Fred Haise and Gordon Fullerton completed the third Shuttle free flight, and events progressed so smoothly that NASA determined to commence tailcone-off testing with the very next flight.[41]

At first, NASA and Rockwell had thought that a series of captive flights with the Shuttle minus its tailcone might be necessary to evaluate whether the buffeting loads on the 747's vertical fin were acceptable. Mission planners soon realized that there was little point in such flights. The 747 could take off with the Shuttle; if the buffeting seemed excessive, the craft could simply abort the mission and land on the lakebed. In preparation for the flight, Rockwell and NASA technicians removed the tailcone from the Enterprise and replaced it with a configuration identical to what the Shuttle would have during reentry from space, including the three main Shuttle engine nozzles and the much smaller nozzles of the orbital maneuvering subsystem. By this time, the massive press attention that had focused on the earlier Shuttle flights had abated and day-to-day activities at Dryden were more tranquil.

Mission planners decided that, during the takeoff roll and liftoff, Fitz Fulton would report any severe buffeting in the cabin. Bill Andrews would monitor the loads on the 747's tail, and if he deemed them excessive, he would call "data abort," terminating the flight. If the 747 was still on the runway, this meant chopping power and stopping. If just airborne, the 747 could land straight ahead on the lakebed. If airborne, Fulton and Tom McMurtry would gingerly return the craft to Edwards.

The actual flight on October 12, 1977 came off without difficulty. Again there was the early morning procession to the

south lakebed and the long wait until takeoff, while some Air Force Phantoms shot landing approaches. When the 747-Shuttle rolled down the runway this time, observers watched for any indication of an abort. Then it was airborne and climbing out to the east, with no visible problems. At Dryden and Johnson, engineers checked monitors. The tail loads were within acceptable boundaries. After about 40 minutes, the 747-Enterprise became visible to the north, approaching the drop. Cameramen peered through telephoto lenses to catch the moment of separation. Fulton pushed into a shallow dive at 25,200 feet above the desert. Thirty-eight seconds later, Joe Engle triggered the explosive bolts. The separation occurred over Peerless Valley; Enterprise nosed down sharply, descending over North Edwards on final approach to runway 17. It quickly became apparent that Enterprise would land right in the aiming area. The steep diving descent, with the Shuttle plunging to earth followed by its T-38 chase, brought exclamations of surprise even from those who had witnessed the earlier tailcone-on flights. Removing the tailcone certainly made a difference. In what seemed an incredibly brief time, Engle had pulled out of the dive into the landing flare and deployed the gear. There was no excess energy to worry about this time, and Enterprise plunked down, streaming a roostertail of playa dust, 2 minutes and 34 seconds after launch.[42]

During the very brief flight the Shuttle had flown well, confirming earlier predictions and simulations. It was, in effect, simply a big X-15. The next question was whether the Shuttle could be landed with confidence on a confined runway. It was a critical issue since NASA planned landing the Shuttle on 15,000 feet runways at Vandenberg and Kennedy. For the next tailcone-off flight, NASA planned to land the Enterprise on the 15,000 foot runway at Edwards. So far, aside from the high and hot first landing, the Shuttle had had little difficulty in landing at a chosen spot on the lakebed runways, even with the tailcone off. Encouraged, NASA scheduled the fifth Shuttle free flight for October 26.[43]

In that flight Enterprise encountered control problems just at touchdown. The Shuttle had been launched at an altitude of 19,000 feet over the desert for a straight-in approach. Mission commander Fred Haise flew a 332 mph approach profile down to the flare maneuver; the Enterprise lost speed very slowly - much slower than the Shuttle's Gulfstream II simulator. Passing across the runway threshold, the Enterprise was about 25 mph faster than planned. Haise used the split-rudder speed brake to slow the craft and nosed down to force it onto the runway at the planned impact point. Instead, the Enterprise entered a left roll, which Haise corrected, touched down on its main landing gear, and bounced back into the air. Haise had brought on a "PIO": pilot-induced oscillation. Copilot Gordon Fullerton told Haise to relax his grip on the controls, and the Enterprise damped out its rolling motions. It touched down again, bounced more shallowly, then touched down for the final time before coasting to a stop. The flight had an important VIP observer: Charles, the Prince of Wales. Prince Charles, a Royal Air Force pilot, was in the United States as part of a goodwill tour. While in Houston, he had "flown" the Shuttle simulator with Haise and Fullerton. Interestingly enough, during one "touchdown" the craft had bounced and Prince Charles had encountered the same sort of lateral PIO during the ensuing skip. The rugged arrival prompted NASA briefly to reconsider adding an additional tailcone-off flight, but mission planners decided that it was unnecessary. The ALT crews themselves had no reservations about the Shuttle's ability to land on concrete runways at Kennedy and Vandenberg, and their feelings did much to influence the decision not to add an extra flight. Dryden did undertake a landing study of the Shuttle, the better to understand its low-speed handling and control characteristics. With their usual penchant for thoroughness, center personnel wanted no unresolved questions or doubts when the Shuttle smoked in to land from a Mach 25 reentry sometime in 1981.[44]

The fifth Shuttle free flight concluded Enterprise's flight testing. Dryden now prepared for the task of ferrying the

Enterprise aboard the 747 to NASA's Marshall Space Flight Center for a series of ground vibration tests. Technicians reinstalled the tailcone aerodynamic fairing. Fulton and the 747 crew completed a series of test flights with the Shuttle in ferry condition (with its front attachment strut lowered slightly to improve the cruise performance of the two mated vehicles) in mid-November. All indications were that the Shuttle could easily be ferried atop the 747. On March 10, 1978 the Enterprise left the runway at Dryden for the last time. Fulton and his crew ferried the Shuttle to Ellington Air Force Base at Houston where, during a weekend stay, it was seen by 240,000 viewers, creating, in the words of Houston police, "the largest traffic jam in Houston's history." On March 13 the 747-Enterprise departed from Ellington on a short flight to Huntsville. Seven thousand NASA and Redstone employees witnessed the arrival of the strange pair. The next day, cranes removed the Shuttle from the 747 preparatory to installing it in a special test rig at Marshall for a series of ground vibration tests simulating the loads a Shuttle would experience in flight.[45]

It would be pleasant to report that the subsequent history of the Shuttle down to its first flight went without incident. Such, of course, was not the case. Three major difficulties confronted the Shuttle between the time of its Approach and Landing Tests in 1977 and its first orbital flight test, which slipped off schedule until April 1981: resolving the flight control system problems that Shuttle had encountered on its last tailcone-off ALT; developing the Space Shuttle Main Engine, and developing the Shuttle's thermal protection system. The first involved primarily software changes to the digital flight control system. The NASA Dryden Flight Research Center flew an extensive program with its modified LTV F-8 Digital Fly-by-Wire (DFBW) testbed aircraft, duplicating the kind of PIO that Haise had encountered. In retrospect, this problem proved a lot less severe than it seemed at the time. The second, however, was a matter of very serious

concern. The SSME operated at much higher chamber pressures than previous liquid-fuel rocket engines for manned spacecraft (in fact, it operated at the same level of pressure that Eugen Sänger had predicted would be necessary for his own "Silver Bird," an interesting and fitting historical curiosity), and incorporated pre-burning of propellants to help generate a higher specific impulse. Its development proved more difficult than the F-1 engine utilized on the Saturn V, since these earlier engines were "throwaways," and not intended for repeated reuse thus not requiring the extensive "man-rating" test procedures necessary with the SSME. In 1978, NASA began so-called Main Propulsion Tests (MPT) at the National Space Technology Laboratories (NSTL) at Bay St. Louis, Mississippi. Testing failures quickly focused NASA concern on the SSME. Critical failures involving a nozzle steer-horn design were traced to an incorrect welding wire mixed in with correct-strength welding wire, forcing examination of all SSMEs and inspecting and repairing their welds, if necessary. By mid-1979, things looked brighter; SSMEs had accumulated 91,000 seconds of test stand time, including abort mission simulation engine runs of 665 and 823 seconds, compared to the 520 seconds required for orbital insertion, during tests at the NSTL and the Air Force Rocket Propulsion Laboratory at Edwards Air Force Base. But in 1980, the old difficulties returned. In July, three SSMEs failed during two weeks of testing at the NSTL, and in November 1980, a weak brazing section on the nozzle of an SSME gave way during a 581-second engine run, punching a hole several inches in diameter through the nozzle. Thanks to the engine's automated self-assessment system, these failures usually resulted in destroyed or damaged components, but not the entire engine, as the engine would shut down quickly enough to prevent the kind of catastrophic failures that had characterized the early space program, when even whole test stands were destroyed. Intensive work followed all of these failures, and testing of the SSME resumed on December 4, 1980, with the successful firing of a SSME

at 102% of its rated thrust for 591 seconds. By early 1981, NASA believed that it had resolved its problems with the SSMEs, and subsequent history proved that to be accurate. Ironically, the engines that everyone took for granted--the solid-rocket boosters--were the one possessing a tragic flaw, one that would slip through design review and quality control and which would, when combined with bad management and decision-making, lead to the deaths of seven astronauts and the loss of the Space Shuttle Challenger.[46]

The third problem NASA confronted getting the Shuttle into space was its thermal protection system. Early in the lifting reentry study programs of the 1960s, proponents of Shuttle-like craft had debated the merits of active cooling systems--using, for example, circulation of fluids to help improve heat transfer--vs. passive cooling techniques. Quickly, attention centered on passive systems since such systems would offer lower system weight and much less complexity. Refurbishable ablators (such as that experimented with on the X-15A-2) had offered some promise, but the associated problems of refurbishment seemed to rule them out from serious contention. The choice quickly narrowed to having a "hot structure" made from some exotic materials, or using an insulated structure, with some sort of ceramic coating or tile system attached to a conventional aluminum airframe. NASA opted for the latter approach early in Phase B studies, selecting ceramic tiles over metallic shingles or a similar radiative system, and confirmed it in a February 1973 decision to use a nonmetallic heat shield on the orbiter. (This decision subsequently triggered a competitive evaluation of silica and mullite (aluminum silicate) for NASA by the Battelle Memorial Institute, which silica won handily, based on its superior thermal performance.) NASA engineers liked the idea of working with an aluminum structure since it was more of a known quantity than, say, one fabricated from titanium. Lockheed had argued for just such an approach using aluminum during their Star Clipper

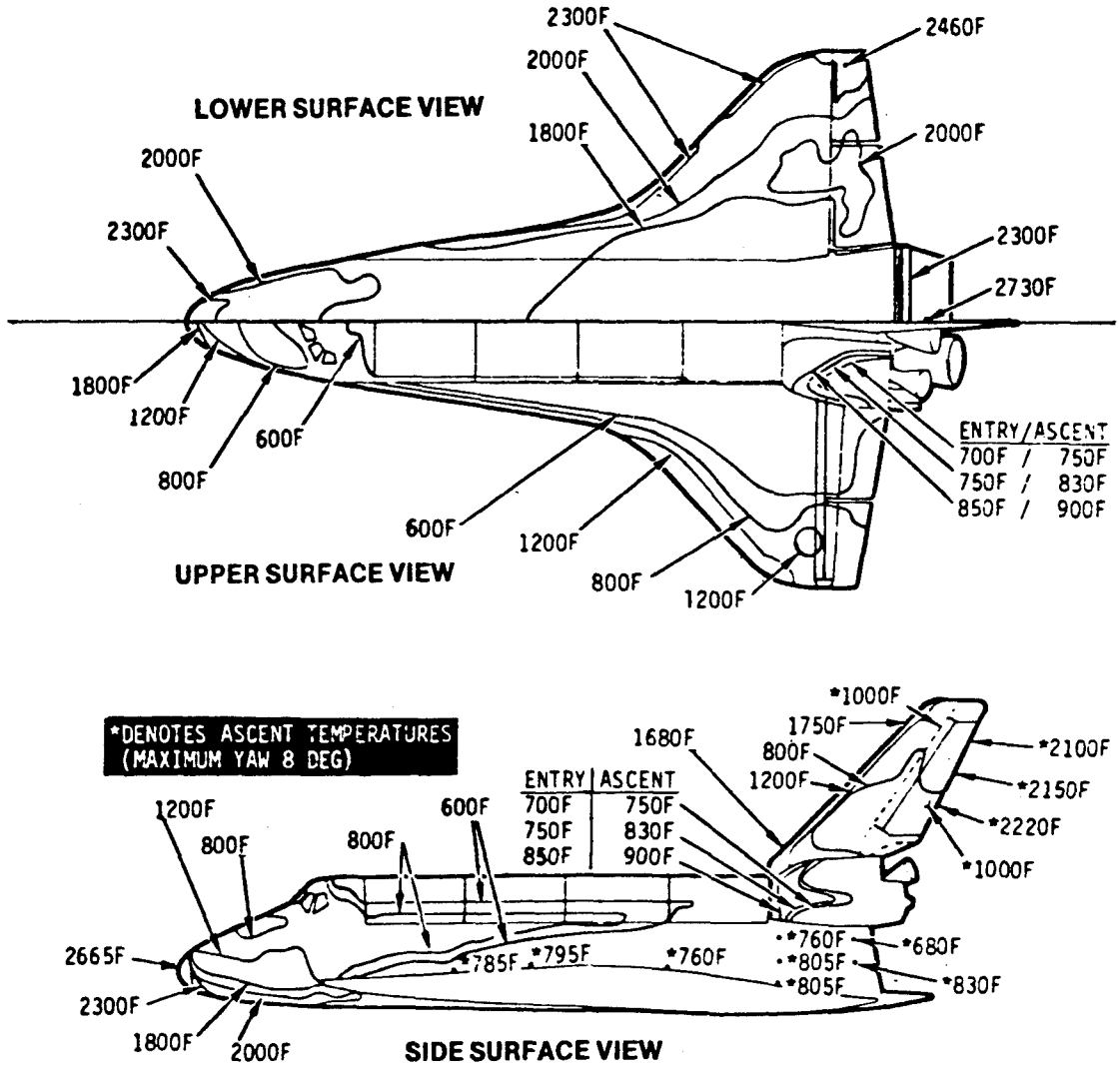
studies--an interesting point since, with their experience on the virtually all-titanium Mach 3+ SR-71 Blackbird program, Lockheed's predeliction for aluminum meant that the aerospace company with the most experience in working with titanium on high-performance aircraft design was, nevertheless, opting for aluminum for its Shuttle vehicle. Lockheed developed ceramic reusable surface insulation (RSI) coatings designated LI-900 and LI-2200; the difference occurred in density, with the LI-900 having a density of 9 lbs. per cubic foot, and the LI-2200 having a density of 22 lbs. per cubic foot. These became so-called Class II (LI-900) and Class III (LI-2200) insulators for the Shuttle, as will be explained subsequently. The ceramic consisted of silica fibers bound together and sintered with other silica fibers, and then glaze-coated by a reaction-cured glass consisting of silica, boron oxide, and silicon tetraboride. Since this mixture was not waterproof, a silicon polymer was coated over the undersurface (i.e., the non-glazed) side of the surface. This coating was highly brittle, with a low coefficient of linear thermal expansion, and thus Lockheed could not coat an entire vehicle with it. Rather, the coating could be in the form of smaller tiles. The tiles would have small gaps between them (averaging less than 1/1000th of an inch) to permit relative motion and allow for deformation of the metal structure due to thermal effects. A second problem concerned the local expansion or lateral movement of the aluminum skin directly under an individual tile; since a tile would still crack under this loading, engineers decided to isolate the aluminum skin from the tile proper by bonding the tile to a felt pad, and in turn, bonding the felt pad to the skin, using a silicone adhesive that would cure at room temperatures. Such a system seemed ideal for the Shuttle; it created a tile structure that radiated heat, and at the same time insulated the orbiter from the hot effects of reentry.[47]

The Space Shuttle design made use of four different kinds of reusable surface insulations, two of which were tiles. Very high

temperature areas of the Shuttle--such as the nose cap and the wing leading edges--were protected from formed reusable carbon-carbon (RCC). Temperatures in these areas hovered in the 2300-2700 degrees F. mark, well above the 350 degrees F. maximum temperature that could be tolerated by the Orbiter's metal skin (see Figure 131 for a plot of anticipated Shuttle flight temperatures as of the end of 1980, before Shuttle actually flew). Areas where temperatures ranged from 1200-2300 degrees F. received so-called high-temperature reusable surface insulation (HRSI), meaning that most of the orbiter received these tiles (either LI-900 or a smaller number of LI-2200 tiles), giving it its characteristic black undersurface. Areas exposed to temperatures between 700-1200 degrees F. received white-faced LI-900 tiles termed low-temperature reusable surface insulation (LRSI). Felt reusable surface insulation (FRSI) blankets covered areas where temperatures would not exceed 750 degrees F. during ascent, or 700 degrees F. during reentry. Figure 132 shows a schematic view of the orbiter with locations of the tiles, FRSI, and RCC.[48]

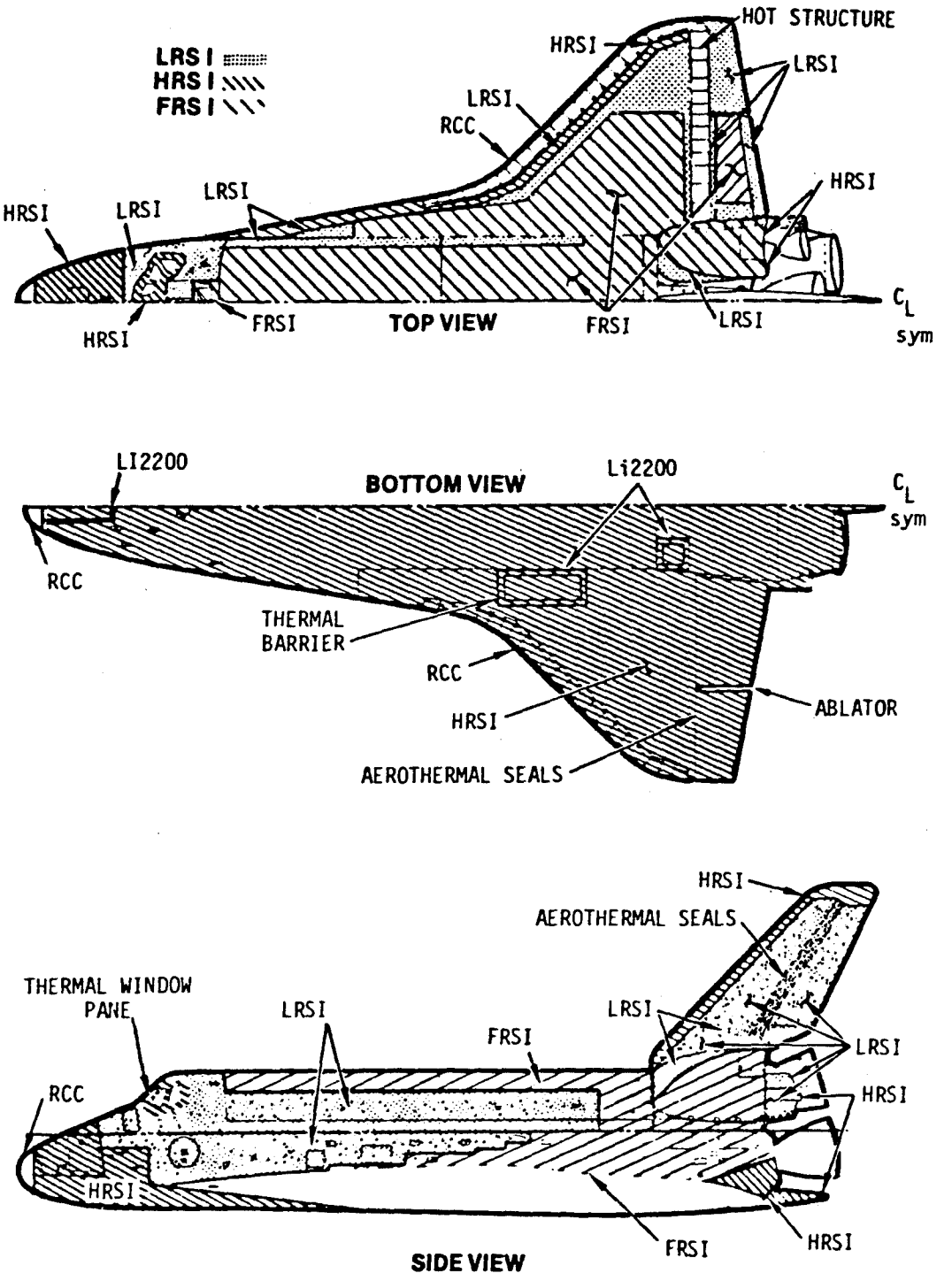
NASA had already experienced concern regarding Shuttle's anticipated heating. The agency had at first thought it could avoid having TPS protection on the lee-side of the Shuttle, but a briefing to a NASA panel in March 1975 by Flight Dynamics Laboratory engineers quickly convinced the agency otherwise, after the FDL team presented classified lee-side heating results from the ASSET, PRIME, and BGRV programs indicating that lee-side heating was a very serious consideration. In April 1978, an AFFTC/FDL team examined potential Orbiter heating problems again, and in January 1979, their studies led to isolation of the Shuttle's Orbital Maneuvering System pod (OMS pod) area as a thermal problem area--a problem that did, in fact, reveal itself during orbital flight tests. To support FDL Shuttle heating studies, the Air Force ran tests using the tunnels at AEDC in May through November 1979, and followed this with additional testing at the Naval Surface Weapons Center's Tunnel 9 in May 1980. By

Figure 131



ANTICIPATED SHUTTLE TEMPERATURES, DECEMBER 1980

Figure 132



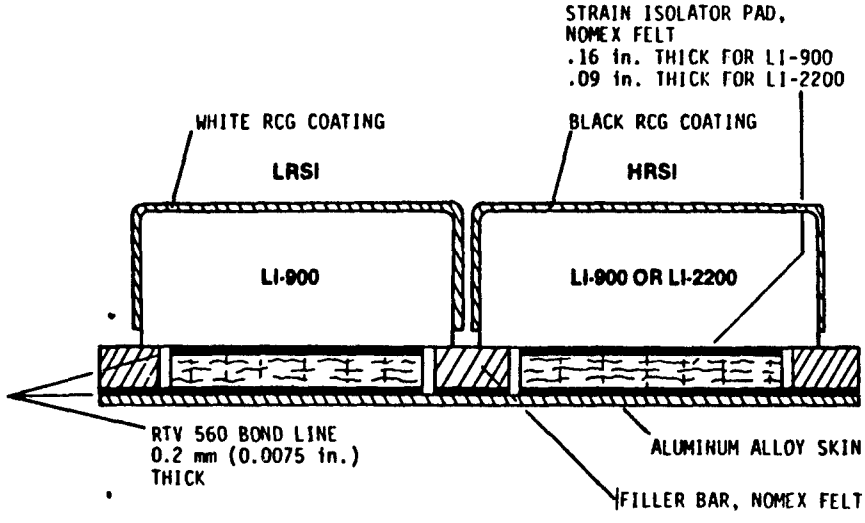
SHUTTLE INSULATION LOCATION

this time, however, a major problem had surfaced with the single most critical element in the Shuttle TPS system: the tiles themselves. As Shuttle moved closer to flight testing in space, NASA and Rockwell developed clearer ideas as to the magnitude of flight loads that the vehicle would have to withstand. In mid-1979 it quickly became obvious that the Shuttle TPS "in certain areas did not have sufficient strength to survive the tensile loads of a single mission."[49] NASA feared that tiles might come loose due to the dynamic loadings experienced in flight, and be shed from the Shuttle during reentry, possibly leading to a fatal "zipper effect" whereby the Shuttle would shed more tiles and cause its primary aluminum structure to fail catastropically. There ensued a massive agency-wide search for a solution, incorporating outside panels and experts from government, industry, and the academic communities. As LeRoy E. Day recalled,[50]

> . . . there is a case [the tile crisis] where not enough engineering work, probably, was done early enough in the program to understand the detail--the mechanical properties--of this strange material that we were using that was neither "fish nor fowl."

NASA's aggressive approach in addressing the tile problem worked, but even so Shuttle slipped off-schedule. The solution to the tiles involved strengthening the bonding between the tiles and the felt strain isolation pads. (Figure 133 shows the tile installation.) Analysis indicated that while each individual component--a tile, the SIP under the tile, the adhesive between the SIP and the tile, and the adhesive between the SIP and the aluminum skin--each had satisfactory tensile strength individually, when combined together as a system, the combination had a reduction of about 50 percent in overall system tensile strength. In October 1979, NASA opted for a "densification" process, filling voids between the fibers at the location of the bonding surface with a special slurry mixture consisting of Ludox

Figure 133



LRSI = Low Temperature Reusable Surface Insulation

HRSI = High Temperature Reusable Surface Insulation

RCG = Reaction Coated Glass

RTV = Room Temperature Vulcanizing Adhesive

INSTALLATION OF TILES ON SHUTTLE

(a DuPont colloidal silica) and a mix of silica and water. After air and oven-drying followed by waterproofing (an exposure to the fumes of methyltrimethoxysilane, a Dow-Corning product known as Z-6070) and acetic acid, the "densified" tiles were ready for installation.[51]

Installation generated its own time-consuming headaches. In March 1979, the Shuttle Columbia flew across country on the back of its 747 mothership, arriving at the Cape and going into the so-called Orbiter Processing Facility. There it spent the next 20 months as technicians worked 3 shifts a day, 6 days a week, installing its 30,759 tiles. They averaged 1.3 tiles per man per week by mid-1979; a strike the previous year had badly interrupted production, and of course, the densification problem added its own woes. With the tile strength issue at last under control in early 1980 and a March 1981 target first-launch date (three years later than originally planned), NASA Administrator Robert Frosch (who had succeeded James Fletcher as NASA Administrator following the election of President James "Jimmy" Carter in 1976) determined not to slip the launch date further. In a decision remarkable for its boldness but also perhaps too bold, Frosch decided to "densify" only half of the tiles recommended for the process, taking care of less-critical ones after the first flight. While this approach did work, in retrospect it seems a bit chancy to have proceeded in this fashion. As a result of Frosch's decision, however, Columbia moved on schedule to the Vehicle Assembly Building (VAB) at the Cape on November 24, 1980, prior to being erected and mated to its external tank and the SRBs. By early 1981, "launch fever" had again struck the Cape, as the work of over a decade and a dream of nearly a century neared the moment of truth: flight into space followed by a lifting return through the atmosphere.[52]

NOTES

1. SAB, "Memo-Report of the USAF Scientific Advisory Board Aerospace Vehicles/Propulsion Panels on Aerospaceplane, VTOL and Strategic Manned Aircraft," (24 October 1963), pp. 1-3; for further details on the Aerospaceplane saga, see the following SAB memos and reports: "Memorandum of the SAB Ad Hoc Committee on Aerospace Plane," (December 1960); "Memorandum of the SAB Ad Hoc Committee on Aerospace Plane," (7 August 1961); and "Report of the SAB Ad Hoc Committee on Aerospaceplane" (23-25 July 1962), all within the files of the SAB, HQ USAF, Washington, D.C. anon., "Aerospaceplane May Be Two-Stage Vehicle," Aviation Week & Space Technology (22 July 1963), pp. 245-249, constitutes a surprisingly useful survey article. The B-70 RBSS study is discussed in Kenneth W. Gatland, "Towards a Recoverable Space Booster," The Aeroplane, (25 November 1960) p. 719. I am indebted to Lt. Col. William Flanagan, USAF, for bringing this article to my attention. For a brief review of air extraction and liquification for Aerospaceplane, see S. W. Greenwood, "Spaceplane Propulsion," The Aeroplane and Astronautics (25 May 1961), pp. 597-599. The X-15 formation flying proposal was transmitted to the author by Col. William J. Knight, USAF (ret), one of the project pilots involved, in a conversation on 19 February 1987. See also ASD news release PAM 84-088. For a typical Aerospaceplane analysis, see F. E. Jarlett, Aerospaceplane: The Payload Capabilities of Various Recoverable Systems All Using Hydrogen Fuel, Report AE 62-0892 (San Diego, CA: General Dynamics Convair Division, 25 October 1962).

2. Materials from Alfred C. Draper; the comment on the dependency of contractors upon the Air Force and NASA for their configurations is my own, based on an examination of numerous government-supported and private venture studies. See also Philip Bono and Kenneth Gatland, Frontiers of Space (New York: Macmillan Publishing Co., Inc., 1976), p. 146.

3. See J. T. Lloyd, Preliminary Design of Two Hypersonic High L/D Test Vehicles, Report No. LR18159 (Burbank: Lockheed, California Company, 10 September 1964); Lockheed Missiles & Space Company, Space Transportation System Study Contract F 04701-69-C-0382 (Sunnyvale, CA: LM & SC, 23 December 1969), passim. I wish to acknowledge the assistance of Robert Hoey of the AFFTC in making this report available for my examination. See also Draper and Cosenza, "Technological Prospects for High Performance Spacecraft," pp. 160-161; For NASA rejection of the stage-and-a-half idea, see transcript of interview of Milton Silveira by Joe Guilmartin and John Mauer, NASA headquarters, 14 November 1984, pp. 13-14, copy in the NASA JSC History archives. For one NASA version of this Lockheed design, see LM & SC, Space Shuttle Flight Simulation Study Final Report, LMSC-A990506, SS-972, MSC-03754 (Contract NAS9-11459), 7 May 1971, passim.

4. AACB, Report of the Ad Hoc Subpanel on Reusable Launch Vehicle Technology (14 September 1966), passim.

5. For early British work see W. F. Hilton, Manned Satellites: Their Achievements and Potentialities (London: Hutchinson, 1965), pp. 88-89; for later proposals see R. F. Creasey and T. Smith, "Space Shuttle Remarks," and P. J. McKenzie, "Space Shuttle Techniques," in Selection of Papers Presented at the Space Shuttle Symposium, Smithsonian Museum of Natural History, Washington, D.C., October 16-19, 1969 (Washington, D.C.: NASA, 1969) pp. 259-289. This report, hereafter referred to as SI Papers, is an excellent seminal volume; a copy is in the JSC archives. For American caret waverider (Nonweiler) interest, see Maurice L. Rasmussen, "Waverider Configurations Derived From Inclined Circular and Elliptic Cones," Journal of Spacecraft, XVII, 6 (November-December 1980), pp. 537-545. Rasmussen is the most notable of America's caret waverider partisans.

6. See C. Bigot, "Space Shuttle Hypersonic Aircraft/Space-Taxi Studies;" Marcel Stoll, "A French Concept of the Space Shuttle;" Mme. Vinas-Espin, "French Contribution to the Design of a Satellite Launcher Aircraft;" R. Marguet, "Participation to the Study of Lifting Reentry;" G. Leroy, "Veras Operation: all in SI Papers, pp. 223-246, 613-638, and 671-684. I wish to acknowledge the assistance of Dr.-Ing. Injas Widjaja of ERNO-Raumfahrttechnik GmBH and Professor Roger E. Bilstein of the University of Houston at Clear Lake City for furnishing information on the VERAS and Mistral projects.

7. Dr. C. Reinhold, "On Aerospace Shuttle," in SI Papers, p. 248.

8. Ibid., pp. 247-257; also F. Mysliwetz, "Aero-Thermodynamic Problems of Space Shuttles," pp. 443-469; H. Tolle, "Thrust Staging on Space Shuttle Ascent Trajectories," both in the same report. For Sänger's last work, see Sänger-Bredt, "Silver Bird," pp. 224-225. There is an excellent 1:10 scale model of the Ju RT-8-01 (Inv.-Nr. 79184) on exhibit at the Deutsches Museum, Munich. I wish to acknowledge the assistance of Dr.-Ing. Walter Rathjen of the Deutsches Museum in facilitating some of my research on German shuttle concepts.

9. Aviation Week & Space Technology, November 6, 1978, p. 19; June 21, 1982, p. 16; March 14, 1983, p. 255; see also Carl A. Forbrich, Jr., "The Soviet Space Shuttle Program," Air University Review, (May-June 1980), pp. 55-61; DoD, Soviet Military Power, 2nd ed. (Washington, D.C.: DoD, 1983), pp. 66, 68.

10. For quotes, see transcript of interview of LeRoy E. Day by John Mauer, October 17, 1983, pp. 5-6, copy in the JSC archives. See also memo, Dale D. Myers to Robert R. Gilruth, December 9, 1970 in the JSC archives.

11. Memo, Floyd L. Thompson to James Webb, 18 June 1964, p. 2, and attached report by the NASA Special Ad Hoc Panel on Hypersonic

Lifting Vehicles with Propulsion, June 1964. Copy in NASA History Office files.

12. Day transcript, pp. 1-4.

13. NASA MSC and MSFC, "Study of Integral Launch and Reentry System," (RFP MSC BG721-28-9-96P and RFP FSFC 1-7-21-00020), 30 October 1968, passim, copy in JSC historical archives; Arnold S. Levine, Managing NASA in the Apollo Era, SP-4102 (Washington, D.C.: NASA, 1982), pp. 158-161.

14. SSTG, NASA Space Shuttle Summary Report (Washington, D.C.: NASA, rev. ed. 31 July 1969), passim; NASA, Office of Manned Space Flight, NASA's Manned Space Flight Program (Washington, D.C.: NASA, 29 April 1969), pp. 233-34.

15. See F. J. Dore, "Space Shuttle Vehicle Sensitivity to System Requirements," SI Papers, pp. 9-33.

16. I wish to acknowledge the assistance of Dr. Edward C. Ezell, former Historian of the NASA Johnson Space Center, for making available to me the various Shuttle configuration studies. Further, I wish to acknowledge the assistance of Dr. Maxime Faget, who added greatly to my understanding of the Shuttle's evolution during an interview at JSC in 1979.

17. J. P. Loftus, Jr.; S. M. Andrich; M. G. Goodhart; and R. C. Kennedy, "The Evolution of the Space Shuttle Design," (Houston, JSC, 1986), pp. 5-11. This is a document written in support of the Rogers Commission investigating the loss of the Shuttle Challenger; a copy is in the historical files of JSC. A good detailed account of NASA's overall technical challenge can be found in Eugene S. Love, "Advanced Technology and the Space Shuttle," 10th von Kármán lecture, 9th annual meeting and technical display, American Institute of Aeronautics and Astronautics (AIAA), Washington, D.C., 8-10 January 1973 (printed as AIAA Paper 73-31).

18. I wish to acknowledge the assistance of Alfred Draper, who furnished copies of FDL documents and technical reports concerning Air Force review of the Faget work. See also transcript of an interview of Milton Silveira by Joe Guilmartin and John Mauer, NASA headquarters, 14 November 1984, copy in JSC archives, pp. 18-19; interview with Milton O. Thompson, Dryden Flight Research Center; Yarymovych anecdote from Jerry Grey, Enterprise (New York: William Morrow and Company, Inc., 1979), pp. 67-68; Scott Pace, "Engineering Design and Political Choice: The Space Shuttle, 1969-1972," M.S. thesis, Massachusetts Institute of Technology, May 1982, pp. 2-3, 103-104; Science and Technology Division, U.S. Library of Congress, Aeronautics and Astronautics, 1970: Chronology on Science, Technology, and Policy (Washington, D.C.: NASA, 1971), p. 53.

19. Day transcript, p. 40.

20. Memo, C. C. Johnson to Robert Gilruth, 10 November 1970, Subject: Shuttle Orbiter Configuration, with attached drawings. Copy in JSC History archives.

21. Silveira transcript, p. 10; Day transcript, pp. 59-65; Loftus, et al., "Evolution of the Space Shuttle Design," pp. 11-12.

22. John M. Logsdon, "The Space Shuttle Decision: Technology and Political Choice," Journal of Contemporary Business, VII, No. 3 (1978), pp. 13-30; J. C. D. Blaine, The End of an Era in Space Exploration (San Diego: American Astronautical Society, 1976), pp. 165-169.

23. Day transcript, p. 41, see also 20-22; Pace thesis, pp. 2-3, 103-104, 116, 135-149. There is an excellent discussion of NASA-DoD relationships on the Shuttle in a report prepared by Barbara N. Luxenberg of the Congressional Research Service, Library of Congress, appearing as Chapter 7, "Space Transportation System," in U.S. House, Subcommittee on Space Science and Applications of the Committee on Science and Technology, United States Civilian Space Programs, 1958-1978, I, 97th Congress, 1st session, January 1981, pp. 445-637; I wish to acknowledge the contributions of Ms. Luxenberg to my research. The information on Shuttle's payload loads criteria is from an interview with Joe Weingarten, 9 February 1987; a letter, M. S. Malkin to Roberta Shapiro, 24 August 1976; and ASD TR-76-30, Cargo Aircraft and Spacecraft Forward Restraint Criteria (Wright-Patterson AFB: ASD, December 1977), passim. The Koch quote is from Ken Hechler, Toward the Endless Frontier: History of the Committee on Science and Technology, 1959-1979 (Washington, D.C.: U.S. House of Representatives, 1980), p. 274. The Hechler book is the best account to date of Congressional wrangling over the Shuttle, and demonstrates the bipartisan nature of both Shuttle support and opposition.

24. I wish to acknowledge with appreciation the assistance of Dr. Edward C. Ezell, former JSC Historian, for supplying me with the following document, from which were taken Table I and the various Shuttle configuration conceptualizations: NASA MSC, Spacecraft Design Division, Summary of MSC Shuttle Configurations (External H O Tanks) (Houston, TX: MSC SDD, rev. ed. 30 June 1972), passim.

25. Loftus, et al., Evolution of the Space Shuttle Design, p. 20; see pp. 15-24 for additional propulsion information.

26. Silviera transcript, p. 6.

27. Loftus, et al., Evolution of the Space Shuttle Design, pp. 17, 23.

28. Ibid., p. 21; Pace thesis, pp. 127; documents useful in tracing the Air Force-NASA relationship on roles and configuration of the Shuttle include NASA MSFC release 70-102, 26 May 1970; letter, Myers to Gilruth, 30 June 1970; Space Shuttle Phase B Study Control Document NAS 9-10960, June 1970; NASA Statement of Work for Space Transportation System, 2 September 1969; these plus other internal and external commentaries and correspondence are in the files of the NASA JSC HO. See also Hans Mark, "The Impact of Our Enterprise in Space," Technology in Society, I, No. 1 (1979), pp. 43-53. I have benefitted from conversations with Maxime Faget, Alfred C. Draper, William Lamar, Joseph Guilmartin, and former NASA Administrator James C. Fletcher.

29. Quoted in NASA news release "Space Shuttle," (February 8, 1972), p. 1.

30. Memorandum for record, James C. Fletcher, George M. Low, and Richard C. McCurdy, "Selection of Contractor for Space Shuttle Program," September 1972, in files of the JSC history archives.

31. Ibid.

32. Ibid.

33. A. Miles Whitnah and Ernest R. Hillje, Space Shuttle Wind Tunnel Testing Summary, NASA Reference Publication 1125 (Washington, D.C.: NASA, 1984), pp. 5-7, and related drawings. I wish to thank Mr. Whitnah for making this report available to me, for it is the definitive accounting of the Shuttle's wind tunnel evolution. See also Fletcher et al., memo for record; and the Presidential Commission on the Space Shuttle Challenger Accident, Report to the President (Washington, D.C.: Government Printing Office, 6 June 1986), p. 4.

34. Letter and attachment, Robert F. Thompson to Myron S. Malkin, 16 October 1974; letter, Christopher Kraft to Lee Scherer, 23 October 1974; JSC history archives.

35. Day transcript, p. 74.

36. Personal recollection by author.

37. Ibid.; Captive inert flight test postflight reports, reports of DFRC Shuttle news conference, 18 February 1977; personal recollection.

38. NASA release 77-16, passim.; Donald E. Fink, "Orbiter Flight Plan Expanded," Aviation Week and Space Technology, 27 June 1977, pp. 12-14.

39. Ibid.; Jeffrey M. Lenorovitz, "Shuttle Orbiter Test Phase Trimmed," Aviation Week and Space Technology, 4 July 1977, pp. 18-19; captive active flight test postflight reports.

40. Free flight postflight reports; Donald E. Fink, "Orbiter Responsive in Free Flight," Aviation Week and Space Technology, 22 August 1977, pp. 12-19; personal recollection.

41. Free flight postflight reports.

42. Ibid.; personal recollection.

43. Free flight postflight reports.

44. Ibid.; Donald E. Fink, "Orbiter Experiences Control Problems," Aviation Week and Space Technology, 31 October 1977, pp. 16-17.

45. NASA DFRC X-Press, 24 March 1978, "Shuttle Orbiter Ferried to Huntsville," Aviation Week and Space Technology, 20 March 1978, p. 15.

46. Day transcript, p. 85; Statement of John F. Yardley (NASA Associate Administrator for Space Transportation Systems) before the Subcommittee on Space Science and Applications, Committee on Science and Technology, U.S. Congress, House of Representatives, 5 February 1980, pp. 2, 6-7; copy in NASA History Office archives; Dave Dooling, "Catch Launch Fever," Astronautics & Aeronautics, XIX, n. 1 (January 1981), pp. 4-5.

47. Loftus, et al., Evolution of the Space Shuttle Design, p. 12; Silviera transcript, pp. 14-16; Paul A. Cooper and Paul F. Holloway, "The Shuttle Tile Story," Astronautics & Aeronautics, XIX, n. 1 (January 1981), pp. 24-34. Eagle Engineering, Inc., Report 86-125C, Technology Influence on the Space Shuttle Development (Houston: NASA LBJSC, 8 June 1986), pp. 6-4 and 6-5.

48. Cooper and Holloway, "Shuttle Tile Story, pp. 24-27.

49. Ibid., p. 25; FDL material from Alfred C. Draper.

50. Day transcript, p. 65.

51. Cooper and Holloway, "Shuttle Tile Story," pp. 29-34.

52. Dooling, "Catch Launch Fever," pp. 4-5; Thomas O'Toole, "Space Shuttle Flight Preparations Going Slowly," The Washington Post, 4 July 1979. The advent of the Shuttle has been marked by a number of popular books on the possible impact of the Shuttle on the exploration and utilization of space. The best single summary on the present-day Shuttle and its capabilities and potential is still NASA SP-407, Space Shuttle (Washington, D.C.: NASA, 1976); incisive technical analyses of the Shuttle, potential Shuttle uses, and possible Shuttle "spin-offs" are examined in Robert Salkeld, Donald W. Patterson, and Jerry Grey, Space Transportation Systems, v. I of the AIAA Aerospace Assessment Series (New York:

American Institute of Aeronautics and Astronautics, 1978); the American Astronautical Society, Space Shuttles and Interplanetary Missions, v. XXVIII of Advances in the Astronautical Sciences (San Diego: AAS, 1970); the American Astronautical Society, Space Shuttle Missions of the '80s, v. XXXII of Advances in the Astronautical Sciences (San Diego: AAS, 1976), and in Gerard K. O'Neill's The High Frontier: Human Colonies in Space (New York: William Morrow and Company, Inc., 1977).

CHAPTER II

HAIL, COLUMBIA: THE INITIAL ORBITAL FLIGHTS OF THE STS

On the morning of April 14, 1981, the world's attention turned to Edwards Air Force Base, California. At 10:20:57 a.m., as an estimated 400,000 spectators cheered and millions around the world watched on television, the Space Shuttle Columbia's landing gear touched down on Rogers Dry Lake, deep in California's Mojave Desert. Astronauts John Young and Robert Crippen had successfully landed the first manned space vehicle to leave the earth under rocket power and return on the wings of an aircraft. Once again, Edwards, birthplace of supersonic flight, had been the scene of high drama; a new era in space exploration--the era of reusable space vehicles--had dawned.

The dramatic moments which culminated on the lakebed that morning had been preceded by years of planning and preparations at the Air Force Flight Test Center. And, long after Columbia had been towed off the lakebed to the NASA Dryden facility where it would be mated to its 747 carrier aircraft, the effort would continue as mountains of data were evaluated and preparations were made for the second, third and fourth landings. The second landing (STS-2), on November 14, 1981, would demonstrate a central concept of the Space Shuttle flight test program--that manned space vehicles could be launched, safely landed, and launched again. Each of the four proposed Space Transportation System vehicles--Challenger, Columbia, Discovery, and Atlantis--was scheduled to make at least 100 such launches and landings during the years of their operational service.[1]

The Air Force and its Space Division had been designated as the Department of Defense organizations responsible for monitoring shuttle development and identifying military requirements which

would have to be incorporated in its design. At the Flight Test Center, the Office of Advanced Manned Vehicles (OAMV) had been designated as the lead test organization for the descent phase of the Space Shuttle's evaluation. Specifically, it was responsible for evaluating the Shuttle's performance, handling qualities, stability derivatives, aerothermodynamics and thermal protection system, guidance and navigation, and other subsystems during the Shuttle's nearly hour-long descent from space.[2]

This small organization, under the leadership of Robert Hoey, had only 12 engineers and technicians, but was uniquely qualified for the difficult responsibility placed upon it. A number of its members had been intimately involved in major flight test programs at Edwards which had contributed directly to the development of the Space Shuttle. During the 1960s, they had worked on the evaluation of the X-15 rocket aircraft. In the late '60s and early '70s, they had been involved in the evaluation of a series of "lifting body" research vehicles--the M2, HL-10, X-24A, and X-24B. These vehicles, though not designed with the Space Shuttle specifically in mind, had demonstrated the feasibility of low lift-to-drag deadstick landings, and the data gathered during their evaluation had provided vital information for understanding terminal area energy management and a number of other subjects.[3]

All of this experience was invaluable; thus the Office of Advanced Manned Vehicles played a significant role in the determination of maneuvers which would be performed in the flight test program to obtain performance, stability and control, and aerodynamic heating data. For example, based upon their substantial flight test experience and data gained from the Shuttle's first reentry, members of the office designed longitudinal maneuvers to extract performance and heating data from the second and subsequent Shuttle landings. Much of this evaluation and planning was conducted on the Flight Test Center's own Shuttle simulator.[4]

Personnel from the office were involved at every stage in planning the test missions. They would monitor the flights in "real time" from the Flight Test Center's Ridley Mission Control Center, the landing support control room at NASA's Dryden Flight Research Center, and the mission control center at NASA's Johnson Space Center in Houston, Texas. The office was also the focal point for the Flight Test Center's support of each of the landings.[5]

Planning and coordinating support constituted an immense task. Well over 500 Flight Test Center personnel provided support for each landing. This support included:[6]

Astronaut rescue and recovery - In the event of an emergency, the rescue of crews and recovery of the vehicle was planned according to zones of responsibility. If the Shuttle landed within the base area, then base personnel would take charge of the operation and recover the crew and the vehicle. The deputy base commander would take command at the scene and direct fire trucks and security police from a mobile command post. Helicopters from Detachment 5 of the 40th Aerospace Rescue and Recovery Squadron would be immediately sent to recover the astronauts. If the Shuttle came down outside of the base's boundaries but within a 25-nautical mile radius, the Flight Test Center would be responsible for crew recovery while NASA would take responsibility for recovery of the vehicle. Outside of this radius, astronaut recovery would be the responsibility of Military Airlift Command Aerospace Rescue and Recovery units. Lockheed HC-130 Hercules aircraft, flown by AF Reserve aircrews out of March AFB, would assist. On board would be Air Force para-jumpers who would jump into areas that helicopters could not reach. In this area, NASA would again be responsible for vehicle recovery.

Medical support - A team of five Air Force flight surgeons, specially trained in space medicine, would serve as an emergency medical team. Flying on-board the rescue helicopters, these personnel would be prepared to provide on-site medical care in the event of an accident. Injured personnel would then be flown to the Edwards AFB Hospital where teams of medical personnel would be on duty prepared to deal with any kind of emergency.

Lakebed fire and rescue operations - Military and civilian firemen and ground personnel (36 for the first landing and 27 for the second), specially trained at the Kennedy Space Center, would suppress any fires and get crew out of the vehicle.

Range and tracking support - Radars, cinetheodolite cameras, and space positioning equipment would feed the various mission control complexes in real time and provide long-range photographic fixes of the Shuttle's exact descent track during the entire landing sequence.

Ground security - A group of 270 Air Force Security Police (including augmentees from the base and the Army) would handle security and crowd control tasks during the entire landing phase. In addition to protecting the Columbia, this meant protecting Air Force property and the safety of hundreds of thousands of visitors. Included in the force would be a pair of four-man "tactical neutralization" teams flying in two helicopters provided by the 6512th Test Squadron. Similar to "SWAT" teams employed by large metropolitan police forces, these teams were highly trained riot control experts with the responsibility of preventing access to any restricted area.

Weather information - Detachment 21, 2nd Weather Squadron, would provide all Air Force and NASA organizations with up-to-the-minute weather reports and forecasts for the Edwards area during each flight.

Photography - More than 40 still and motion picture cameras operated by the Flight Test Center's military and civilian photographers would record all phases of the Columbia's landing sequence for the Department of Defense.

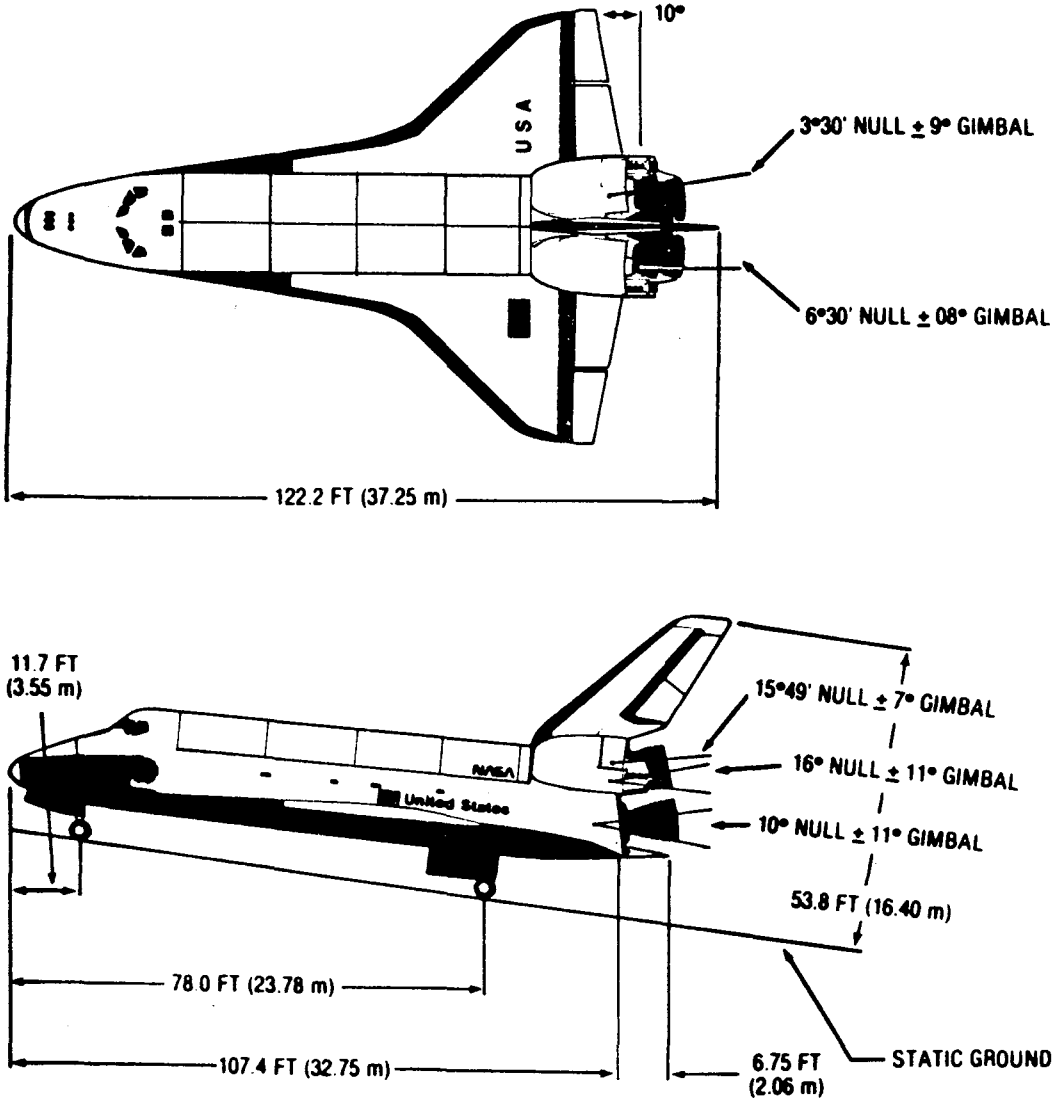
Public Affairs - Flight Test Center public affairs personnel would work closely with their NASA counterparts in supplying information for hundreds of representatives of the news media.

For months prior to the first landing, these support personnel underwent extensive training and exercises in which Shuttle landings were simulated. This phase concluded with a five-day simulated exercise in which all personnel involved had to be prepared as though an actual mission was in progress. Flight Test Center personnel would have to be prepared throughout because of the possibility of an early landing. All of this preparation was accomplished without disrupting the typically busy activities of the base. The personnel involved (indeed, everyone at the Flight Test Center) were enthusiastic. There was a feeling of pride that they were about to participate in an event of international significance.[7]

The Columbia's first landing would actually mark its second trip to Edwards. On March 8, 1979, the 75-ton orbiter had first arrived, traveling 5-10 miles per hour, on the bed of a trailer after a 38-mile cross-desert journey from the Rockwell International manufacturing plant in Palmdale, on its way to the Cape. When it next arrived in the skies over Edwards, it had orbited the earth 36 times and was returning at supersonic speed.[8]

By early 1981 the Shuttle program was at last "go" for launch, and the actual participants exuded confidence. Figure 1 shows the configuration of STS-1. The selected crew, astronauts John Young and Robert Crippen, were ready; Young remarked "we're 130% trained and ready to go. We have more time in the mission simulators than 20 astronauts in the entire Gemini program and we haven't even launched yet." Young, NASA's chief astronaut at age 50, was known for his ready humor and personable manner. He had flown four previous times on Gemini and Apollo, was a graduate of the Naval Test Pilot School, and had set a series of climb records in the Navy's McDonnell F4H-1 Phantom II back in 1962. "Crip" Crippen, another naval aviator, had graduated from the Air Force's test pilot school at Edwards, served there as an instructor, and then had been selected for the abortive Manned Orbiting Laboratory program in 1966. When MOL collapsed, he transferred to NASA's astronaut corps. Columbia's first flight would culminate a 15-year wait for a journey into the black void of space by the 43-year-old astronaut.

Still, there were always the unknown concerns. Veteran astronaut Donald K. "Deke" Slayton, manager of the Shuttle orbital flight test program, cautioned: "In my opinion, about 90 percent of your risk in a total program comes with a first flight. There is no nice in-between milestone. You have to bite it all in one chunk." And John Yardley, NASA's Associate Administrator for Space Transportation Systems, stated, "I'm not worried over any of the problems we have worried about. They're in good shape. The things that you have to be careful about are the unknowns, things that have never happened before, things that people have never thought of simply because we have a configuration that's considerably different from the 30 years of the history of launch vehicles. This is entirely different. A new engineering gremlin could crawl out of the woodwork, one nobody could have predicted." (Tragically, of course, his words would be fulfilled with the flight of Challenger in 1986--and it was a problem that could have



	WING	VERTICAL STAB.
AREA	2690 FT^2 (249.90 m^2)	413.25 FT^2 (38.39 m^2)
ASPECT RATIO	2.265	1.675
SWEEP (L.E.)	45°	45°
M.A.C.	474.81 IN.(1206.0 cm)	199.81 IN.(507.52 cm)
DIHEDRAL (T.E.)	3°30'	—

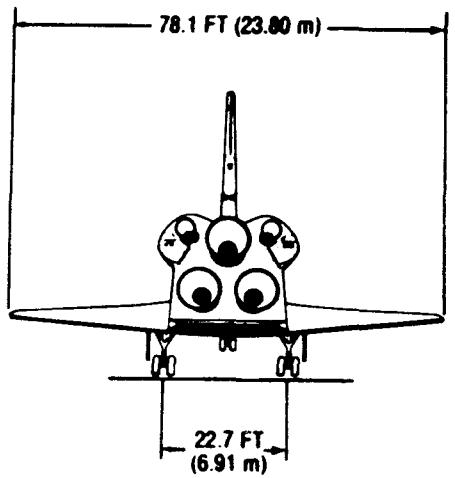


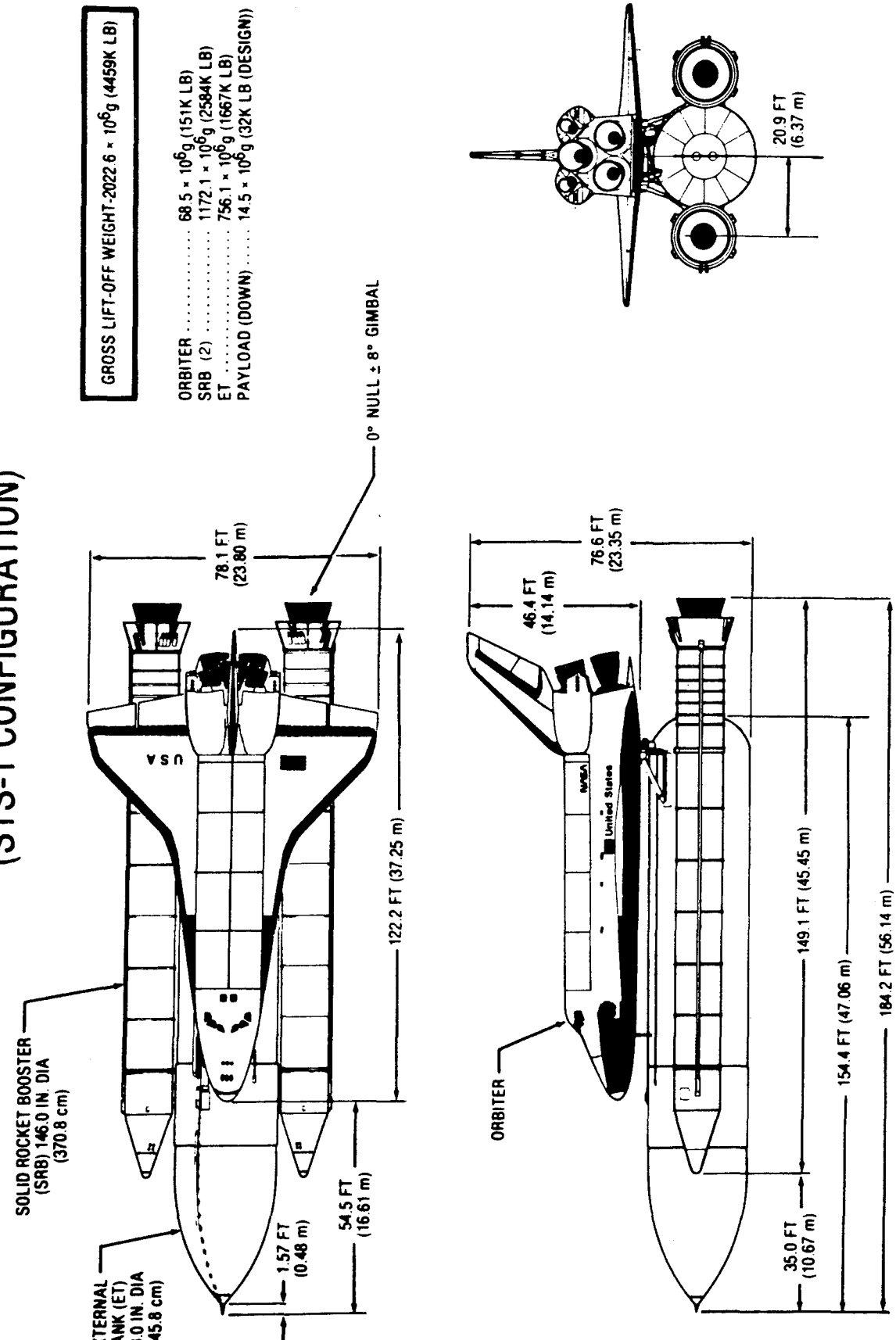
Figure 1

1181

(a) Orbiter Vehicle OV-102.

STS-1 mission configurations.

Figure 1 (concluded)



(b) Integrated Vehicle.

Concluded.

been prevented.) NASA, in any case, was not taking any chances. Alternate emergency landing sites had been prepared around the world. Finally, the crew sat in two SR-71 type ejection seats capable of hurling them away from the Shuttle should anything go wrong with the Shuttle during launch or reentry. For such ejection to be made in safety, the speed of the Shuttle would have to be Mach 3 or less, at 120,000 feet or less. At speeds and altitudes above these figures, the crew would have to trust their lives to the Shuttle until, with luck, the opportunity to eject from the stricken craft would present itself.

The space agency had set a tentative launch date of April 10, at 6:50 a.m. eastern time. That morning found nearly a million people lined along the Banana River and swarming in the Cape area, all attention riveted on the Shuttle. But, that Friday, Shuttle would not fly. A critical computer failed to interact properly with its electronic colleagues and, frustrated, mission controllers scrubbed the launch after a prolonged delay. Over the next day, computer technicians worked to resolve the problem, and finally all was set for launch the next morning, Palm Sunday, April 12.

April 12, 1981 was the 20th anniversary of Yuri Gagarin's first orbital flight, but this day belonged to the United States. In the early hours before launch, preparations and the countdown moved inexorably toward ignition. A Soviet trawler snooping in the expected impact area where the two solid-fuel boosters would land had to be shooed away by a Coast Guard cutter. Security remained tight, and NASA had even taken special precautions to prevent the Shuttle's air-to-ground communications links from being interfered with by Soviet or terrorist organizations. Security forces maintained a close watch at the Cape. President Ronald Reagan, recuperating from a would-be assassin's bullet, watched from the White House. Weather conditions at the Cape and at the emergency landing sites were perfect: warm, almost like summer.

The moment of truth came at 7:00 a.m. Cape time. The countdown reached T-3.8 seconds, and the Columbia's onboard computer system sent a command to the Shuttle's three main liquid-fuel engines. Hydrogen met oxygen in an explosive mix, was ignited in a controlled explosion, and by T+0.24 seconds, all engines were firing at 90 percent thrust, and a timer, set for 2.64 seconds, began to run down to trigger the two solid-fuel boosters on either side of the Shuttle's huge external fuel tank. Once the solids fired, Shuttle would be committed to launch. The first rumble of ignition had not even reached the ears of observers before the solids erupted at about T+3 seconds. An igniter shot a 20-foot sheet of flame down the core of each solid rocket, instantly triggering ignition. Under a full 7.5 million pounds of thrust, the Shuttle lifted quickly away from Pad 39A, rising above a cloud of steam from the pad's water exhaust quenching system, away from the palmetto scrub and toward space in a spectacle that staggered the imagination. It was a fantastic vision: the three liquid-fuel engine exhausts glaring with that peculiarly clear flame coming from hydrogen-oxygen combustion, bright shock-diamonds streaming in each exhaust trail; the two solids, burning with a characteristic dazzling white-yellow flame, generating a broad banner of grayish exhaust smoke; as one observer noted, Columbia looked for all the world like some gigantic Fourth-of-July firework thundering into the sky. Above all was the rumbling, crackling roar - greater, even, than the Saturn's - and the ground itself trembled.

As planned, when the Shuttle reached an altitude of 400 feet, 8 seconds after launch, it began an automatic roll and pitch maneuver that placed it in a climbing arc on its back as it rocketed over the Atlantic Ocean. Inside the spacecraft, the unflappable Young's heart rate idled along at 85 beats per minute while Crip Crippen's enthusiastically touched 135. Shortly after 2 minutes into the flight, the spent solid boosters, their job done, separated from the Columbia at an altitude of 27 nautical

miles, descending to earth under bright orange and white parachutes. Four minutes into the flight, as Columbia raced along faster than the X-15, Crip Crippen proclaimed, "Man, what a view, what a view!" At T+6:30, the Columbia was at Mach 15, pressing on to orbit. At T+8:32, the Shuttle entered a low earth orbit at an altitude of 63 nautical miles; the crew jettisoned the spent external fuel tank 20 seconds later. The tank broke up and tumbled, as planned, into the Indian Ocean. Racing around the globe at Mach 25, in excess of 17,400 miles per hour, Young and Crippen utilized the Shuttle's two orbital maneuvering system rocket engines to loft the craft into a circular 130 nautical mile orbit, then set down to the routine of Columbia's planned 36 orbit, 54.5 hour mission.

During the launch, as expected, the blast from the Shuttle's engines had caused some damage to the launch pad, hurling handrails around and melting, scorching and searing some other parts of the structure. Unexpectedly, however, the launch had also damaged some of the Shuttle's tile thermal protection system. After Young and Crippen opened the Shuttle's cargo bay doors, exposing radiators to carry away the accumulated heat of the craft's electronic equipment and testing the door operation, a TV camera panning the open payload bay and the back of the Shuttle revealed that a small number of tiles had separated during launch. Experts knew that these tiles, situated on the fairings for the Shuttle's orbital maneuvering and reaction control systems, were not critical for the reentry. Nevertheless, the nagging question remained: Had any of the tiles from the critical underside of the craft been lost? NASA arranged for special long-range photography of the Shuttle using Air Force space surveillance cameras. As a result, the agency remained confident, however, that no tiles had, in fact, been shed from the Columbia's underside. Intensive preflight "pull" tests at the Cape and tests with the tiles mounted on experimental aircraft at Edwards had convinced mission planners that the critical tiles would remain on the craft, no

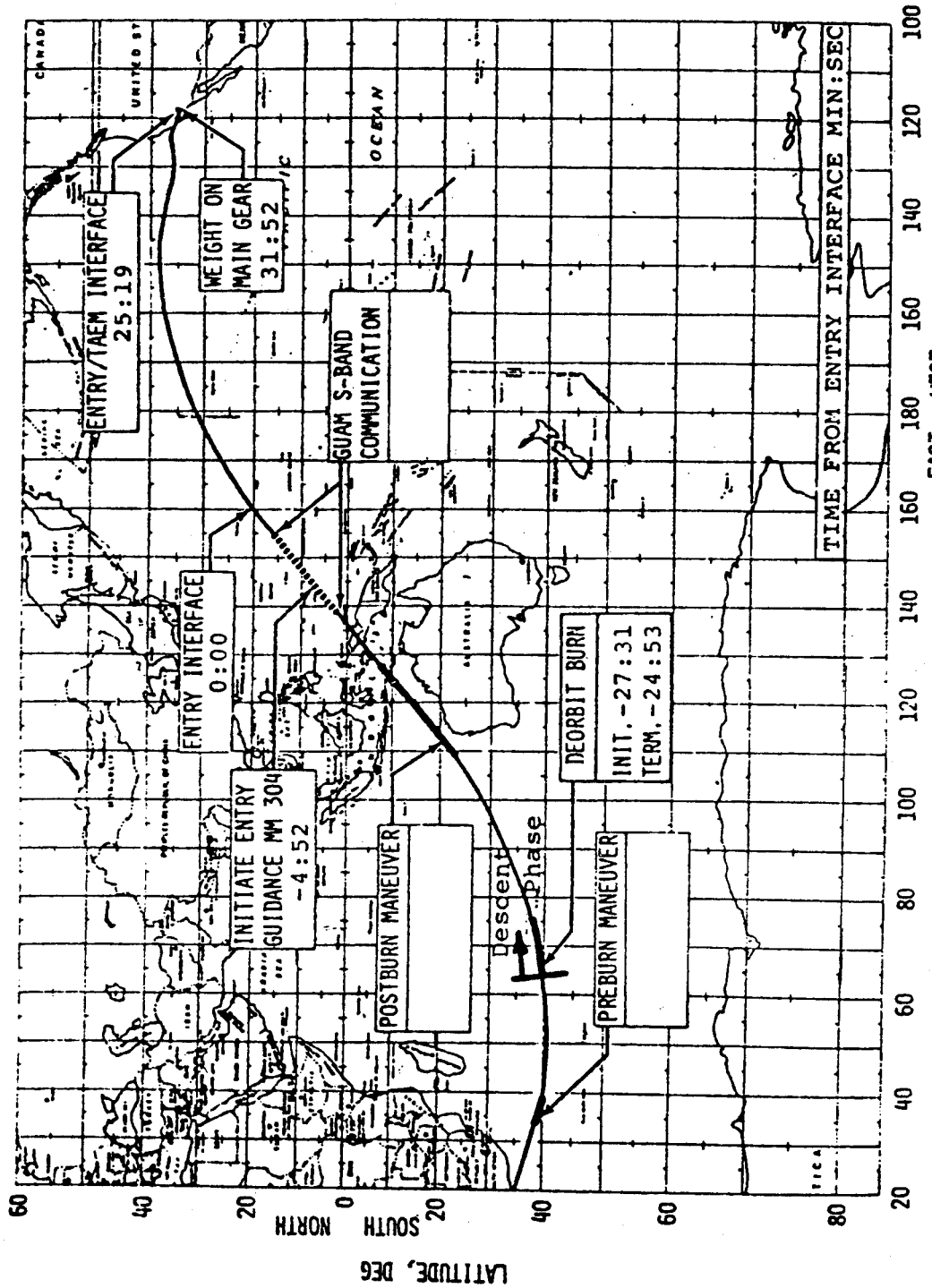
matter what. For two days, Young and Crippen checked out the orbiter and its systems, and aside from very minor problems, nothing remotely serious showed up. Then, on the morning of Tuesday, April 14, the two astronauts prepared for the return to earth, Columbia's second moment of truth.

NASA had planned the Shuttle to land at Edwards, one-hour after beginning the reentry engine "burn" to drop out of orbit. Two and a half hours before the reentry burn, Young and Crippen closed the Shuttle's payload bay doors, which shut snugly. The Shuttle was now orbiting tail forward, on its back, with the astronauts in a heads-down position. The descent phase began with the firing of both orbital maneuvering system (OMS) engines at 17:21:34 Greenwich Mean Time (GMT) over the Indian Ocean (see Figure 2) during the Columbia's 36th orbit (for a detailed listing of descent phase events, see Figure 3). The 155-second engine burn resulted in a 297 ft/sec deceleration rate. After this so-called "deorbit burn," astronaut Young rotated the Shuttle in an "inverted pitch" nose-down position with the belly facing the earth and the craft's nose pointing about 18 degrees below the horizon. As Shuttle raced onwards, it gradually changed its orientation, until, at the moment it entered the upper atmosphere, it had assumed a nose-high attitude, flying with the nose 40 degrees above the horizon. Just before Columbia entered radio blackout, Mission Control in Houston radioed, "Easy does it, John, we're all riding with you." Young briskly answered "Bye bye." And then all was silence, for minutes. Shuttle had entered the atmosphere at 17:49:05 GMT, at 400,000 feet.

As it reentered, the astronauts reported:[9]

> . . . we could see yellow-orange flashes from the reaction control system thruster firings reflecting from the rear of the spaceship. To set up the atmospheric entry, we precisely placed the spaceship at zero roll, zero yaw, and 39 degrees on our attitude flight director indicator (40 degrees angle-of-attack) before selecting

Figure 2



STS-1 PLANNED GROUND TRACK

Figure 3

STS-1 DESCENT PHASE EVENTS

	GMT (Day 104) HR:MIN:SEC	TIME FROM EI MIN:SEC	GEODETIC ALTITUDE FEET	TRUE AIRSPEED FPS	MACH NO.
APU NO. 2 ON	17:18:33	-30:32	------	-----	-----
APU NO. 3 ON	17:18:46	-30:19	------	-----	-----
DEORBIT IGNITION	17:21:34	-27:31	------	-----	-----
DEORBIT CUTOFF	17:24:12	-24:53	------	-----	-----
APU NO 1 ON	17:43:10	- 5:55	------	-----	-----
INITIATE ENTRY GUIDANCE (304)	17:44:13.3	-4:51.7	551460	24370	28.38
ENTRY INTERFACE	17:49:05	0	402880	24560	28.54
FIRST ELEVON TRIM	17:51:48	2:43	321950	24600	27.04
FIRST BODYFLAP MOVEMENT	17:51:55	2:50	318600	24600	27.12
FIRST ELEVON RATE CMD	17:52:48.5	3:43.5	294050	24610	27.33
INITIATE TEMP CONTROL PHASE	17:54:14.3	5:09.3	261770	24590	26.82
INITIATE ROLL CMD	17:54:26.3	5:21.3	258940	24530	26.64
INITIATE MODULATION	17:56:29.3	7:24.3	247730	23760	25.21
INITIATE DRAG-ALT UPDATING TO NAV	18:00:46	11:41	234230	21490	22.43
INITIATE EQUILIBRIUM GLIDE PHASE	18:02:44.3	13:39.3	222400	19900	20.32
FIRST ROLL REVERSAL	18:04:11	15:06	209250	18310	18.01
ALT = 200000 FT	18:05:23	16:18	200000	16580	16.13
INITIATE CONSTANT DRAG PHASE	18:06:28.3	17:23.3	186950	14670	14.17
INITIATE TRANSITION PHASE	18:08:40.3	19:35.3	164720	10370	9.87
SPEEDBRAKES START OUT	18:08:55.5	19:50.5	161080	9880	9.40
SECOND ROLL REVERSAL	18:09:5.5	20:00.5	159130	9530	9.11
SPEEDBRAKES FULL OUT	18:09:28	20:23	156520	8910	8.47
TACAN NAV UPDATE	18:10:38	21:33	140420	7050	6.79
AMMONIA BOILER ACTIVATED	18:11:53	22:48	119760	5290	5.18
THIRD ROLL REVERSAL	18:12:17.5	23:12.5	113670	4790	4.72
ROLL/YAW TO CSS	18:12:20.3	23:15.3	112950	4720	4.66
START COMMANDER INPUT TO ROLL	18:12:20.6	23:15.6	112710	4700	4.64
END COMMANDER INPUT IN ROLL	18:12:49.5	23:44.5	108900	4120	4.08
SPEEDBRAKES START IN FROM 100 PERCENT	18:13:01	23:56	106880	3880	3.85
START COMMANDER INPUT IN ROLL	18:13:11.7	24:06.7	104120	3680	3.65
END COMMANDER INPUT IN ROLL	18:13:18	24:13	102350	3570	3.56
ROLL/YAW TO AUTO	18:13:18.3	24:3.3	102350	3570	3.56
LEFT AIR DATA PROBE DEPLOYED	18:13:20.3	24:15.3	101760	3540	3.52
RIGHT AIR DATA PROBE DEPLOYED	18:13:22.3	24:17.5	101180	3500	3.49
FIRST RUDDER MOVEMENT	18:13:26.6	24:21.6	99740	3410	3.41
FOURTH ROLL REVERSAL	18:14:14.5	25:9.5	86340	2640	2.69
ROLL/YAW TO CSS	18:14:21.3	25:16.3	84340	2530	2.58
START COMMANDER INPUT IN ROLL	18:14:21.5	25:16.5	84070	2510	2.56
AIR DATA USED IN NAV	18:14:22.3	25:17.3	84070	2510	2.56
ENTRY/TAEM INTERFACE	18:14:24.3	25:19.3	83570	2480	2.53
PITCH TO CSS	18:14:28.3	25:23.3	82670	2410	2.46
SPEEDBRAKES STOP AT 65 PERCENT	18:14:30.5	25:25.5	82260	2370	2.42
PAYLOAD BAY VENTS OPEN	18:14:31	25:26	82060	2360	2.41
END COMMANDER INPUT IN ROLL	18:14:39	25:34	80510	2230	2.28
PITCH/ROLL/YAW TO AUTO	18:14:39.3	25:34.3	80510	2230	2.28
70,000 FEET	18:15:20	26:15	70000	1670	1.74
WINGS LEVEL ATTITUDE	18:15:55	26:50	59920	1240	1.29
LAST RCS FIRING	18:16:6.8	27:1.8	56730	1110	1.17
PITCH/ROLL/YAW TO CSS	18:16:41	27:36	48370	860	.90
SPEEDBRAKES START IN	18:16:45.2	27:40.2	47500	840	.87
HAC START TURN TO FINAL	18:17:12.7	28:7.7	41490	790	.82
COMMANDER EXTENDS SPEEDBRAKES	18:18:21	29:16	26200	650	.64
MSBLS NAV UPDATE	18:19:05	30:00	18500	610	.58
ROLL OUT ON FINAL	18:19:33.5	30:28.5	12430	580	.54
COMMANDER RETRACTS SPEEDBRAKES	18:20:16	31:11	------	---	---
PREFLARE INITIATION	18:20:19.4	31:14.4	------	---	---
GEAR DOWN	18:20:43	31:38	------	---	---
TOUCHDOWN - MAIN GEAR	18:20:57.1	31:52.1	------	---	---
NOSE GEAR TOUCHDOWN	18:21:07.4	32:24	------	---	---
WHEEL STOP	18:21:57	32:52	------	---	---
NO. 3 APU OFF	18:22:34	33:29	------	---	---
NO. 1 AND NO. 2 APU OFF	18:22:37	33:32	------	---	---

> autopilot. At approximately 330,000 feet, still in the dark, we saw a light pink airglow form outside the cabin windows. The front window color was indicative of temperatures approaching 3000 degrees F.

As Columbia descended, its body flap deflected to about 14 degrees "down" where it remained until about Mach 15. The elevons, set to trim at 1 degree "up" as controlled by the body flap's deflection, remained at that trim limit. The body flap position was down in the airflow more than anticipated, indicating either a different pitch moment or a different level of body flap effectiveness than had been predicted. As a result, the body flap encountered higher heating than desired; thus, on the second Shuttle flight, the elevon trim schedule was modified to position the elevons 1 degree "down" which would act to raise the body flap.[10]

In order to maintain the desired descent trajectory and ground track, and to dissipate the Shuttle's enormous energy as it entered the atmosphere, the guidance system automatically commanded a series of 80 degrees hypersonic banking maneuvers. The first roll command from wings-level attitude occurred at 258,940 feet and approximately Mach 24.5. As a result of this roll command, the Columbia entered a low-frequency lateral-directional oscillation which required approximately 50 seconds to dampen.[11] The pilots further reported that:[12]

> When the roll rate was achieved, the inertial sideslip attitude error was pegged indicating at least a 2½-degree sideslip error. When the bank angle was achieved, the orbiter took three cycles to damp. Much more than normal thruster rocket activity was noted during the roll damping event. Postflight data showed the peak sideslip angles were ±3.5 degrees. Postflight analysis indicates that the reaction control yaw thrusters were more effective in roll than anticipated in this region.

(On the next flight this, and several other maneuvers in this regime, were flown using control stick steering inputs in order to

gather data to modify the flight control system for subsequent automatic operation.)

During the remainder of the flight, four roll reversals were performed (at Mach 18.5, 9.8, 4.8, and 2.6). The first two were performed by the automatic flight control system. The split-rudder speedbrakes were automatically programed to the fully extended position during the second reversal (at 9880 feet per second). As planned, Young briefly disengaged the automatic system so he could manually perform the two remaining roll reversals (at 4790 and 2640 feet per second). The speedbrakes began a slow automatic retraction at 3880 feet per second and were at the planned 65-percent deflection at 2370 feet per second. At Mach 2.5, as the Shuttle was nearing the Edwards area, Young and Crippen deferred to the automatic control mode again. They subsequently reported that these transitions between control stick steering and automatic control mode were "exceptionally smooth."[13]

Having crossed over the California coastline near Big Sur at 139,000 feet and near Mach 7 (just 10 minutes from touchdown), Columbia began incorporating TACAN* data into its navigation system at Mach 6.79. The Shuttle deployed air data probes at Mach 3.5 and the air data system began functioning at Mach 2.56 (approximately 6 minutes to touchdown). At 59,920 feet (Mach 1.29) the automatic guidance system returned the Shuttle to wings-level attitude for the final descent to the Heading Alignment Circle, an imaginary spatial reference over Edwards. Beginning at about Mach 2.0, the crew felt the first slight evidence of transonic buffet. Its intensity increased until about Mach 0.9 and disappeared completely below Mach 0.8. Although the crew did not notice them because of the buffet, postflight analysis revealed that low frequency ($0.25H_z$), low amplitude lateral-directional oscillations were encountered for

*Tactical Air Navigation.

approximately 5 seconds as the Shuttle decelerated from Mach 2 to Mach 1. The last yaw reaction control firing occurred at 56,730 feet and Mach 1.17 directly over the center, whiplashed by Columbia's triumphant sonic boom. As the orbiter decelerated to subsonic speed, postflight analysis revealed that an abrupt movement of the center of pressure resulted in a momentary increase in the Columbia's angle-of-attack. At 48,370 feet and Mach 0.9, Young switched from automatic control to control stick steering in order to manually perform the turn around the Heading Alignment Circle and the landing.[14] As the pilots later reported:[15]

> In the calm air, control was both precise and positive. Nose and wing positioning of Columbia demonstrated subtle, almost attitude-hold-like solid flying qualities which, to date, we have not properly simulated.

The orbiter rolled out on final at 12,430 feet above ground level and an approximate 20-degree glideslope was flown. Radar altimeter lock-up was achieved at 5000 feet and, at approximately 3000 feet, the speedbrakes were retracted in order to stabilize Columbia at 285 knots by 2500 feet. After speedbrake retraction, however, Columbia accelerated to 305 knots, for the speedbrakes apparently produced more drag than predicted; therefore, when Young retracted them, he dumped more drag than expected and thus Columbia, in "clean" configuration, accelerated more than predicted. The pilot initiated the pre-flare pull up at 1750 feet, the main landing gear was deployed and, as the crew later reported:[16]

> The Columbia was slowly rotated to landing attitude. On the very shallow flight path, the vehicle airspeed slowly decreased. Touchdown was estimated at 185-knots equivalent airspeed. Vertical sink rate felt comfortable. In the calm air, Level I handling qualities were assigned to the landing task. [A reference to the Cooper-Harper flight test handling qualities rating scale.] Postflight data analysis showed that the speedbrakes were more effective as drag devices than

> previously estimated. It also indicated that the drag of Columbia due to tile roughness was less than anticipated. And, in ground effect, more lift and less drag was detected below 50 feet altitude.

These aerodynamic changes, plus the previously mentioned "hot" approach, caused the Columbia to land 3094 feet beyond its target. Despite the problems encountered, which were relatively few, the descent and landing had been remarkably successful. On the operational landing strips - the Cape or Vandenberg - the overshoot could have caused serious problems, but the vast expanse of Runway 23 on Rogers Dry Lake had provided an immeasurable margin of safety.[17] The Columbia was safe on the ground. In slightly less than an hour, it had decelerated from a speed of 17,500 miles per hour, descended from space through the atmosphere as its protective tiles glowed at temperatures up to 2,500 degrees F, and landed gently on the lakebed. As Young and Crippen later concluded:[18]

> For those of us who love flight, using wings for our trips to space represents an achievement almost beyond belief--a dream come true . . . . We believe this new space transportation system will revolutionize operations in space. We know that the first mission of Columbia demonstrated that capability beyond any reasonable doubt.

The flight had been a success; its major objectives had been achieved. Certainly, the highly successful reentry and landing had provided increased confidence that the basic design concept of the Shuttle had the potential to accomplish its mission requirements. The reentry on the first mission, however, had been intentionally benign and would need to be followed by more stressing profiles, including more demanding landing approaches. Landing in crosswinds, for example, would have to be demonstrated.[19]

As they studied the data, members of Edwards' Office of Advanced Manned Vehicles found answers to some of the anomalies

encountered during the first flight. Stability and control derivatives were extracted from the five guidance-commanded bank maneuvers and the previously mentioned oscillations. Data obtained above Mach 2 was of sufficient quality to actually permit engineers to duplicate the Shuttle's flying qualities and motions. Though too poor to consistently duplicate the oscillations which occurred in that region, data obtained between Mach 2 and 1 was good enough to permit informed diagnosis.[20]

Directional stability derivatives obtained from flight data were approximately 30 percent more stable than wind tunnel predictions for most of the maneuvers analyzed. However, below Mach 3, aileron control effectiveness appeared considerably lower than predicted, and below Mach 2, roll due to rudder inputs was considerably higher than predicted. Engineers concluded that the differences between aileron and rudder effectiveness were probably the principal cause of the small lateral-directional oscillations experienced during the deceleration from Mach 2 to 1. The greater authority of the rudder movement was supported by hinge moment data which indicated that the rudder hinge moments were approximately 100 percent higher than predicted between Mach 2 and 1.2. The stability and control derivatives in this region were little understood and Office of Advanced Manned Vehicle engineers recommended that lateral-directional pulses should be performed on the next flight so that a more accurate and complete set of derivatives in the low-dynamic pressure region could be obtained.[21] (It was precisely this kind of information that a subscale Shuttle demonstrator, as advocated by Milton Thompson and other researchers at the NASA Flight Research Center, could have returned prior to Shuttle's first return from orbit.)

After analyzing the large amplitude lateral-directional oscillation which occurred during the first bank maneuver, they discovered that the rolling-moment-due-to-yaw-jet-interaction derivative was approximately 50 percent less than predicted. The

"best" set of data obtained from the analysis of this oscillation, when fed into the simulator, reduced the Shuttle's lateral center-of-gravity capability from 4.8 to 3.7 inches. This indicated a high degree of lateral trim sensitivity in the hypersonic region, a critical concern. In future tests, additional lateral-directional pulse-type maneuvers would have to be performed in the region above Mach 10 so that the lateral center-of-gravity envelope for the Shuttle could be accurately defined.[22]

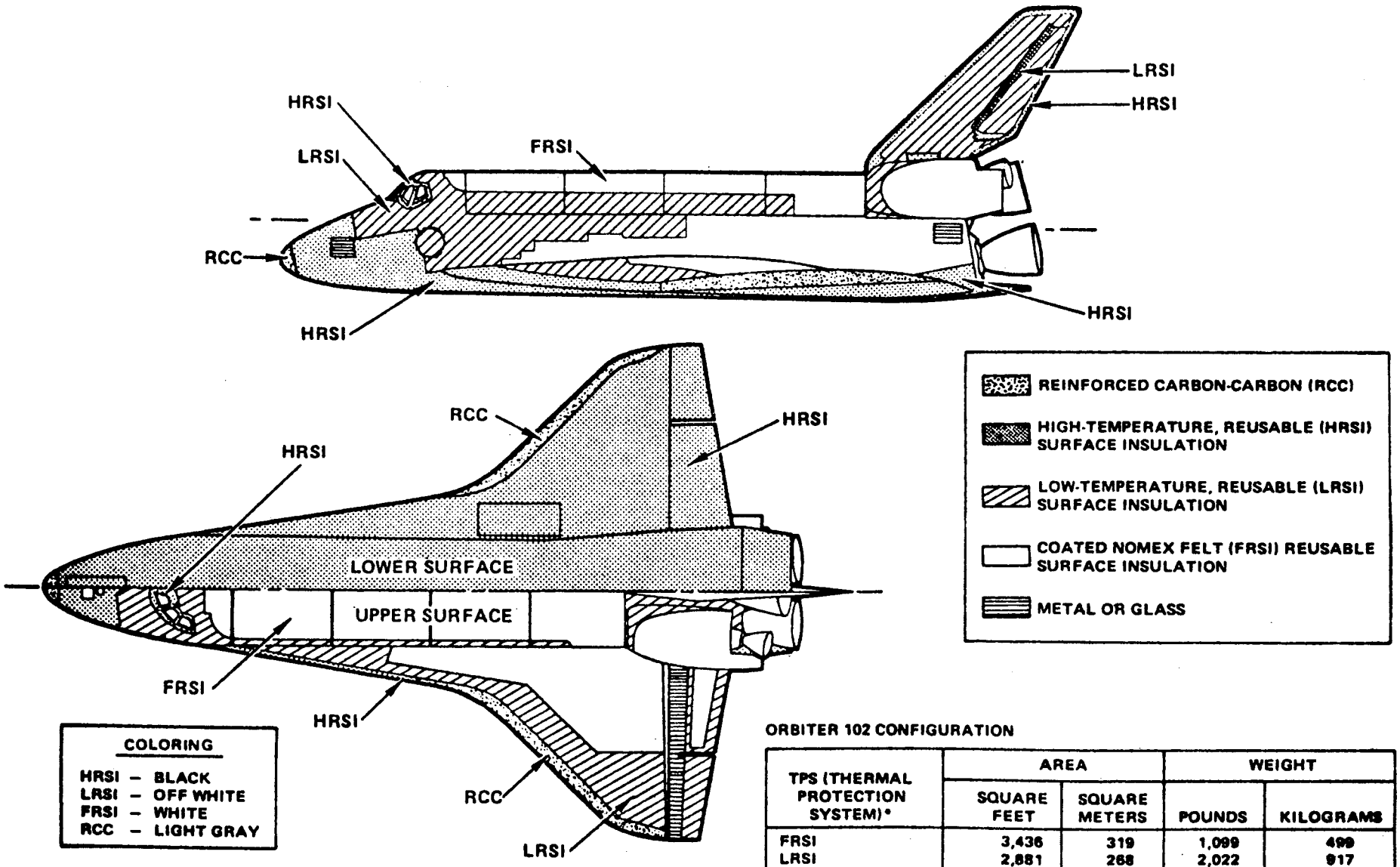
The Office of Advanced Manned Vehicles also evaluated the Shuttle's thermal protection system (see Figure 4). The Shuttle's high-temperature reusable tiles could dissipate heat so rapidly that they could be literally glowing on one side and harmlessly touched by a bare hand on the other. Unfortunately, the team's efforts were substantially hampered in the evaluation of the system because of the failure of an onboard data recorder which resulted in the loss of all temperature measurements for the region above Mach 14.[23]

Their investigation, however, did reveal some significant problems. These included:[24]

> The major cause of damage to the thermal protection system was debris damage inflicted during the Shuttle's takeoff and ascent. Action would have to be taken to eliminate the amount and size of the debris prior to fourth test flight. If such damage had been incurred at more critical locations, the consequences could have been disastrous.

> Entry heating on the tiles covering the Orbital Maneuvering System pods was higher than predicted--hot streaks on the pods were discovered further aft than wind tunnel data had predicted. Higher temperature thermal protection material would have to be installed on the

THERMAL PROTECTION SYSTEM



ORBITER 102 CONFIGURATION

TPS (THERMAL PROTECTION SYSTEM)*	AREA		WEIGHT	
	SQUARE FEET	SQUARE METERS	POUNDS	KILOGRAMS
FRSI	3,436	319	1,099	499
LRSI	2,881	268	2,022	917
HRSI	5,134	477	8,434	3,826
RCC	409	38	3,023	1,371
MISCELLANEOUS			1,394	632
TOTAL	11,860	1,102	15,972	7,245

*INCLUDES BULK INSULATION, THERMAL BARRIERS, AND CLOSEOUTS

Figure 4

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pods prior to the fourth test flight and subsequent high-crossrange missions which would require lower angles-of-attack profiles as the Shuttle passed through the hypersonic region.

Subsurface airflow resulted in scorched filler bars at several locations on the Shuttle's lower surface. These were found in locations with a gap perpendicular to a forward-facing tile step. New gap fillers would have to be installed at all locations with forward-facing steps prior to the next flight.

The team also found, however, that entry heating and bondline temperatures on the Shuttle's lower surface were lower than predicted. Thus, for example, the temperature of the nose gear door reached 1578 degrees F as opposed to the predicted 1800 degrees F at Mach 14 (when data was first acquired) and the surface temperature on the main gear doors was 1294 degrees F instead of the predicted 1550 degrees F. The test engineers attributed these lower temperatures to several "probable" factors, including non-catalytic heating on the tile surfaces at high Mach, late boundary layer transition, and internal convective and radiative cooling.[25] In general, then, Shuttle's lower-surface thermal protection was more than adequate and provided some margin of safety for operational missions which might encounter higher temperatures. The team did, however, find some localized heating problems around the thermal barriers of the lower-surface doors and improvements would have to be made in these areas to prevent potentially serious problems.[26]

Columbia remained at Edwards for 13 days undergoing post-landing tests, detailed inspections of the thermal protection system, and preparation for its ferry flight back to the Kennedy Space Center. Then, mated on the back of its Boeing 747 carrier aircraft, it lifted off the 15,000 foot main runway at 10:30 a.m.

on April 27. Much in contrast to the scene 13 days earlier, only a few Edwards personnel and about 100 spectators witnessed the Columbia's departure. But the relative calm was only temporary; the Shuttle was, at that time, scheduled to fly again in October. In fact, the whole planning and preparation process was already underway.[27]

The second mission, STS-2, included far more dynamic activity than the first, as the astronauts, Air Force Col. Joe Engle and Navy Capt. Richard Truly, maneuvered the Columbia extensively in order to obtain new data for comparison with the data from STS-1. They would apply manual control stick inputs to change the Shuttle's attitude starting at about Mach 24, four times faster than the highest previous speed at which manual control had been attempted in winged aerodynamic flight.[28]

This increased level of maneuvering would continue throughout the descent. Thus, for example, a series of body flap pulses were planned which would require the Shuttle's body flap to be trimmed at different positions so that the elevons would, in turn, be forced to deflect to maintain the vehicle's attitude during entry. This would provide important data on how effective the elevons and body flap were at different positions and flight regimes. Engle further explained the purpose of this maneuver, stating that:[29]

> To fool the vehicle into thinking it has a forward center of gravity you put the body flap down and drive the elevons up to see how much control authority you have . . . . The reason we want to do that is because if we are forced to come in with a forward center of gravity payload, even more forward than we counted on, we need to know whether the vehicle will have adequate control capability for that.

In addition to extensive maneuvers throughout the descent phase, the astronauts would be given the option of landing on either Runway 23 or 15 so that, if conditions permitted, they could fly into a 10-15 knot crosswind.[30] The landing would be a homecoming

of sorts for Engle and Truly. Both had been students at the Test Pilot School (which, during the 1960s, was designated the USAF Aerospace Research Pilot School). Engle had graduated with the 1961-C class, and Truly with the 64-A class. They joined Navy Capt. Robert Crippen (Class 65-A), from STS-1, on the long list of Test Pilot School graduates who had flown in space. In fact, six of the eight astronauts on the first four Shuttle flights had graduated from the school.

While preparations continued at Edwards, a series of problems punctuated efforts to launch the Columbia at the Kennedy Space Center in Florida. A hypergolic fuel spill forced a month-long delay in September so that damaged thermal protection tiles could be replaced; Shuttle's flight slipped from October to November. Then, in the first week of November, a final launch countdown progressed perfectly until a scheduled hold at T-9 minutes, when a succession of out-of-tolerance readings caused the automatic launch sequencer to halt. After furious efforts, NASA resumed the count - only to be stalled again 31 seconds before liftoff, when the mission had to be scrubbed. Columbia finally lifted off from the launch pad on November 12. Its problems, however, were not over.

The Shuttle's second mission was planned to last 5 days 4 hours and 8 minutes (124 hours). A failure of one of the spaceship's three fuel cells, however, forced a decision to bring it down early, and made living and working conditions onboard very uncomfortable. Engle and Truly carried minimum mission planning documents on this flight in anticipation of just such a contingency. These had been written so that primary test objectives could be concentrated in the first 2½ days of the planned mission. The Columbia would be returning to Edwards on November 14, three days ahead of schedule, with a tired and dehydrated crew. This did not cause undue alarm at the Flight Test Center; preparations had long-since been made for such a

circumstance. And the relatively short notice did not deter hundreds of thousands of spectators from, once again, assembling on the southern edge of Rogers Dry Lake to witness the event.[31]

About 70 minutes before the scheduled landing, astronaut John Young made an approach to Runway 15 in NASA's Gulfstream 2 Shuttle Training Aircraft. He encountered 18-knot crosswinds - a potentially risky situation - and, several minutes later, NASA decided to bring Columbia down on Runway 23. The Shuttle could have handled a 20-knot crosswind, but at this point in the testing, officials did not want to risk a landing in crosswinds above 15 knots. Thus one of the second landing's more important objectives was canceled even before Engle and Truly fired the engines for deorbit burn.[32]

The deorbit burn occurred during Columbia's 36th orbit, at 20:23:15 GMT (for a detailed listing of the sequence of events during the descent and landing, see Figure 5; for a listing of test maneuvers, see Figure 6). The engines fired for 170 seconds and produced a deceleration of 258 feet per second. After the deorbit burn, Engle and Truly executed a pitch-around maneuver to establish entry attitude. Twenty-seven minutes and twenty-one seconds after the engines had been fired, at 20:50:36 GMT, Columbia entered the atmosphere at 399,516 feet, flying a standard 40-degree angle-of-attack entry profile. Throughout the descent, Engle repeatedly switched to the manual control stick steering mode in order to perform maneuvers as planned. Though onboard guidance signaled for the first roll maneuver at Mach 24.4, Engle performed it manually using roll and yaw control stick inputs to minimize the oscillations which Columbia had experienced on STS-1 while in the automatic mode. Columbia flew the first three bank reversals, at 19,930 feet per second (219,170 feet altitude), 9,362 feet per second (153,972 feet altitude), and 4,795 feet per second (111,503 feet altitude), in automatic mode; Engle controlled the fourth bank reversal, at 2,533 feet per second (83,836 feet altitude),

Figure 5

EVENT		GMT	TIME FROM EI	ALT ABOVE RUNWAY	REL VEL
		HH:MM:SS	MM:SS	FEET	FPS
APU NO. 3 ON		20:18:35	-32.01		
DEORBIT IGNITION		20:23:15	-27:31		
DEORBIT CUTOFF		20:26:06	-24:30		
APU NO.2 ON		20:37:36	-13:00		
APU NO. 1 ON		20:37:43	-12:53		
MAJOR MODE 304		20:46:22	- 4:14		
ENTRY INTERFACE----------		20:50:36	00:00	399516	24515
PITCH,ROLL/YAW TO CSS		20:52:56	02:20	331050	24598
PITCH ASI NO. 1	*	20:53:03	02:27	327838	24600
ACTIVATE ELEVON TRIM		20:53:06	02:30	326464	24603
PITCH,ROLL/YAW TO AUTO		20:53:23	02:47	318273	24610
BODYFLAP TO AUTO		20:53:50	03:14	305740	24617
AEROSURFACE CONTROL ACTIVATED		20:54:21	03:45	291480	24616
PITCH,ROLL/YAW TO CSS		20:54:40	04:04	283654	24608
PITCH ASI NO. 2	*	20:54:42	04:06	282850	24605
PITCH,ROLL/YAW TO AUTO		20:54:54	04:18	277743	24597
ROLL/YAW TO CSS		20:55:02	04:26	274707	24589
ROLL ASI NO. 1	*	20:55:06	04:30	273224	24583
ROLL ASI NO. 2	*	20:55:37	05:01	262142	24524
ROLL RCS DEACTIVATED (OBAR=10)		20:55:47	05:11	259290	24497
PITCH TO CSS		20:55:49	05:13	258754	24491
PITCH ASI NO. 3	*	20:55:56	05:20	256731	24467
TEMP CONTROL PHASE INITIATED		20:56:03	05:27	255127	24444
PITCH TO AUTO		20:56:05	05:29	254699	24437
FIRST NON-ZERO ROLL COMMAND		20:56:08	05:31	254083	24426
FIRST ROLL MANEUVER INITIATED------		20:56:11	05:34	253500	24414
PITCH TO CSS		20:56:47	06:11	248685	24229
PITCH ASI NO. 4	*	20:56:49	06:13	248519	24218
PITCH TO AUTO		20:57:53	07:17	245660	23821
ROLL ASI NO. 3	*	20:58:06	07:30	245024	23734
ROLL/YAW TO AUTO		20:58:20	07:44	244227	23643
PITCH RCS DEACTIVATED (OBAR=20)		20:58:59	08:23	241805	23390
ANGLE OF ATTACK MODULATION INITI		20:59:31	08:55	239134	23152
ROLL/YAW TO CSS		20:59:35	08:59	239265	23123
PTI NO. 1	*	20:59:37	09:01	239134	23108
ROLL/YAW TO AUTO		20:59:50	09:14	238265	23008
PITCH,ROLL/YAW TO CSS		21:02:13	11:37	229895	21704
BODYFLAP TO MANUAL		21:02:15	11:39	229762	21683
START BODYFLAP SWEEP	*	21:02:23	11:47	229162	21590
DRAG-ALT UPDATING IN NAV INIT		21:02:25	11:49	231036	21567
PITCH TO AUTO		21:02:35	11:59	230278	21443
PTI NO. 1	*	21:02:49	12:13	228831	21311
END BODYFLAP SWEEP	*	21:03:31	12:55	225433	20793
PITCH TO CSS		21:03:36	13:00	230055	20736
PITCH ASI NO. 5	*	21:03:41	13:05	224549	20678
START POPU	*	21:03:54	13:18	223418	20508
GUID COMMANDS FIRST BANK REV-------		21:04:12	13:36	221390	20298
END POPU	*	21:04:31	13:55	219395	19946
EQUIL GLIDE PHASE INITIATED		21:04:32	13:56	219170	19930
FIRST BANK REVERSAL----------		21:04:32	13:56	219170	19930
PITCH,ROLL/YAW TO AUTO		21:04:32	13:56	219170	19930
BODYFLAP TO AUTO		21:04:33	13:57	219075	19913
FIRST ROLL REVERSAL COMPLETE		21:04:56	14:20	217361	19545

* TEST MANEUVER

Figure 5 (cont.)

EVENT		GMT	TIME FROM EI	ALT ABOVE RUNWAY	REL VEL
		HH:MM:SS	MM:SS	FEET	FPS
S-BAND FIRST REC'D BY ARIA		21:05:36	15:00	213567	18859
ROLL/YAW TO CSS		21:06:08	15:32	208064	18236
ARIA S-BAND DECOM LOCK		21:06:10	15:34	207723	18196
BODYFLAP TO MANUAL		21:06:10	15:34	207723	18196
PTI NO. 1	*	21:06:21	15:45	205672	17950
START BODYFLAP SWEEP	*	21:06:33	15:57	203698	17693
END BODYFLAP SWEEP	*	21:06:54	16:18	201051	17201
BODYFLAP TO AUTO		21:07:04	16:28	200262	16971
ROLL/YAW TO AUTO		21:07:09	16:33	199791	16830
ALTITUDE =200,000 FEET		21:07:23	16:31	199960	16877
S-BAND ANTENNA SWITCH		21:08:08	17:32	188476	15214
END ARIA DFI S-BAND DECOM LOCK		21:08:08	17:32	188476	15214
CONSTANT DRAG PHASE INITIATED		21:08:42	18:06	181795	14136
ROLL/YAW TO CSS		21:08:44	18:08	181238	14074
BODYFLAP TO MANUAL		21:08:45	18:09	181051	14042
START BODYFLAP SWEEP	*	21:08:50	18:14	179580	13800
PT PILLAR OI S-BAND AOS		21:09:04	18:28	177373	13452
PTI NO. 1	*	21:09:14	18:38	175218	13105
END ARIA S-BAND OI DECOM LOCK		21:09:18	18:42	174470	12978
END BODYFLAP SWEEP	*	21:09:38	19:02	171652	12299
BODYFLAP TO AUTO		21:09:41	19:05	171372	12207
ROLL/YAW TO AUTO		21:09:52	19:16	170525	11873
PT. PILLAR S-BAND DFI AOS		21:10:12	19:36	167643	11222
EAFB S-BAND AOS		21:10:14	19:38	167219	11159
TRANSITION PHASE INITIATED		21:10:35	19:59	162030	10470
SPEEDBRAKES START OUT		21:10:52	20:16	157723	9911
SECOND BANK REVERSAL----------		21:11:10	20:34	153972	9362
TACAN NAV UPDATE		21:11:30	20:54	150969	8781
ROLL/YAW TO CSS		21:11:53	21:17	147591	8168
PTI NO. 1	*	21:12:03	21:27	147591	7883
ROLL/YAW TO AUTO		21:12:13	21:37	142169	7633
ROLL/YAW TO CSS		21:12:16	21:40	141334	7561
PTI NO. 2	*	21:13:32	22:56	122259	5759
ROLL/YAW TO AUTO		21:13:45	23:09	119374	5494
THIRD BANK REVERSAL----------		21:14:18	23:42	111503	4795
ROLL/YAW TO CSS		21:14:43	24:07	107410	4292
PTI NO. 3	*	21:14:50	24:14	106417	4165
SPEEDBRAKE RETRACTION INITIATED		21:15:00	24:24	104916	3980
ROLL/YAW TO AUTO		21:15:07	24:31	103732	3834
ALTITUDE = 100,000 FEET		21:15:25	24:49	99972	3522
AILERON TRIM ACTIVATED		21:15:26	24:50	99738	3505
RUDDER ACTIVATED		21:15:26	24:50	99738	3505
RUDDER TRIM INITIATED		21:15:26	24:50	99738	3505
AIR DATA PROBES ACTUATED		21:15:31	24:55	98194	3402
ROLL/YAW TO CSS		21:15:42	25:06	95028	3219
RIGHT PROBE DEPLOYED		21:15:44	25:08	94418	3186
PTI NO. 4	*	21:15:47	25:11	93611	3137
LEFT PROBE DEPLOYED		21:15:48	25:12	93263	3121
BARO ALTITUDE DATA VALID		21:15:49	25:13	92994	3104
ROLL/YAW TO AUTO		21:15:56	25:20	90943	2973
PITCH,ROLL/YAW TO CSS		21:16:12	25:36	86762	2718

Figure 5 (concluded)

EVENT		GMT	TIME FROM EI	ALT ABOVE RUNWAY	REL VEL
		HH:MM:SS	MM:SS	FEET	FPS
FOURTH BANK REVERSAL----------		21:16:23	25:47	83836	2533
ENTRY/TAEM INTERFACE		21:16:27	25:51	82673	2470
PAYLOAD BAY VENTS OPEN		21:16:32	25:56	81446	2391
BARO ALTITUDE NAV UPDATE		21:16:41	26:05	79411	2259
PITCH TO AUTO		21:16:41	26:05	79411	2259
PITCH TO CSS		21:16:44	26:08	78537	2204
AIR DATA SYSTEM DATA UPDATE		21:16:45	26:09	78323	2175
PTI NO. 5	*	21:16:52	26:16	76650	2080
WINGS LEVEL ATTITUDE----------		21:17:35	26:59	66747	1543
PITCH TO AUTO		21:17:37	27:01	66272	1538
PTI NO. 6	*	21:17:42	27:06	65031	1487
PTI NO. 7	*	21:18:30	27:54	50844	984
LAST YAW JET FIRING		21:18:32	27:56	50287	949
ALTITUDE = 50,000 & MACH 1.0		21:18:33	27:57	50000	935
SPEEDBRAKE TO MANUAL		21:18:37	28:01	48649	911
PTI NO. 8	*	21:18:48	28:12	44924	871
ROLL/YAW TO AUTO		21:19:02	28:26	41290	843
SPEEDBRAKES TO AUTO		21:19:03	28:27	41026	846
PTI NO. 0 (STRUCTURAL)	*	21:19:10	28:34	38667	836
HEADING ALIGNMENT PHASE INITI------		21:19:13	28:37	37781	836
SPEEDBRAKE FULL OPEN (AUTO)		21:19:13	28:37	37781	836
ROLL/YAW TO CSS		21:19:14	28:38	37477	835
PITCH TO CSS		21:19:21	28:44	35147	830
ROLL/YAW TO AUTO		21:20:14	29:38	17876	649
SPEEDBRAKES CLOSED BY AUTO		21:20:31	29:55	14947	566
PITCH TO AUTO		21:20:33	29:57	14692	560
SPEEDBRAKE TO MANUAL		21:20:34	29:58	14843	563
START SPEEDBRAKE SWEEP	*	21:20:34	29:58	14843	563
PITCH TO CSS		21:20:37	30:01	14158	567
ROLL/YAW TO CSS		21:20:48	30:12	12685	530
MSBLS NAV UPDATE		21:20:51	30:15	12511	524
END SPEEDBRAKE SWEEP	*	21:20:58	30:22	11675	533
PITCH,ROLL/YAW TO AUTO (OSCIL)		21:21:11	30:35	9971	523
BODYFLAP & SPEEDBRAKE TO AUTO		21:21:32	30:56	8803	465
BODYFLAP TO MANUAL		21:21:43	31:07	8060	420
TAEM/A&L INTERFACE----------		21:22:16	31:40	5077	411
BEGIN STEEP GLIDE SLOPE		21:22:24	31:47	4240	415
SPEEDBRAKES TO MANUAL		21:22:37	32:01	2464	464
PREFLARE INITIATED		21:22:40	32:04	2022	475
ROLL/YAW TO CSS		21:22:45	32:09	1324	488
LANDING GEAR ACTUATED		21:22:54	32:18	383	460
PITCH TO CSS (BY RHC DEFLECTION)		21:22:56	32:20	273	458
NOSE GEAR DOWN AND LOCKED		21:22:59	32:23	145	436
MAIN GEAR DOWN AND LOCKED		21:23:00	32:24	111	431
TOUCHDOWN		21:23:13	32:37	7	332
NOSE GEAR CONTACT		21:23:26	32:50	-13	238
WHEEL STOP		21:24:03	33:27		
AMMONIA BOILER ON		21:33:00	42:24		
APU NO. 1 OFF		21:38:15	47:39		
APU NO. 3 OFF		21:38:16	47:40		
APU NO. 3 OFF		21:38:18	47:42		

VELOCITY AND ALTITUDE FROM CCT CAERO1

TIMES ROUNDED TO NEAREST SECOND

Figure 6

STS-2 ENTRY TEST MANEUVERS

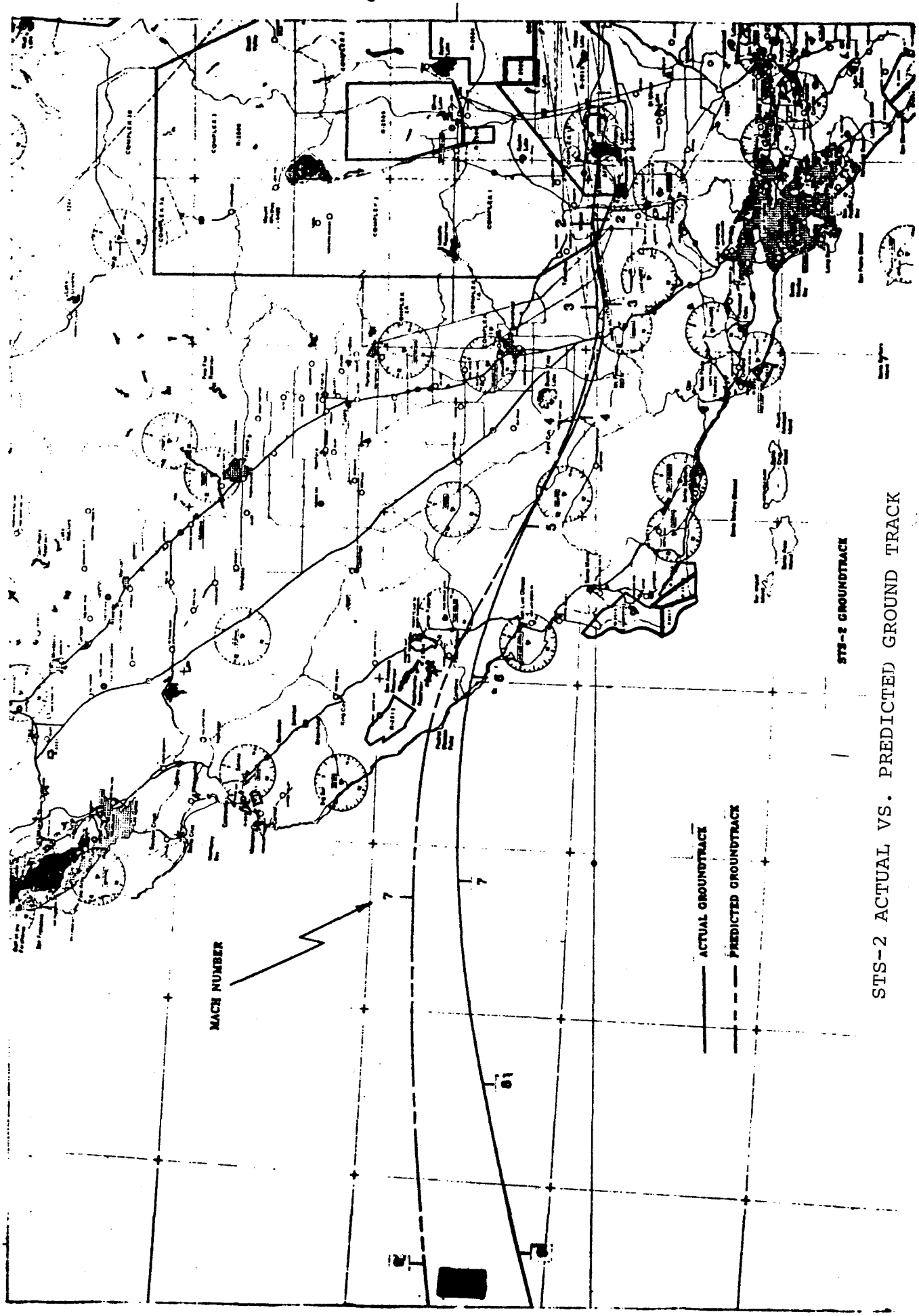
MANEUVER	GMT (HH:MM:SS)	TIME FROM ENTRY INTERFACE (MM:SS)	ALTITUDE ABOVE RUNWAY (FEET)	RELATIVE VELOCITY (FPS)
PITCH ASI NO. 1	20:53:03	02:27	327838	24600
PITCH ASI NO. 2	20:54:42	04:06	282850	24605
ROLL ASI NO. 1	20:55:06	04:30	273224	24583
ROLL ASI NO. 2	20:55:37	05:01	262142	24524
PITCH ASI NO. 3	20:55:56	05:20	256731	24467
PITCH ASI NO. 4	20:56:49	06:13	248519	24218
ROLL ASI NO. 3	20:58:06	07:30	245024	23734
PTI NO. 1	20:59:37	09:01	239134	23108
START BODYFLAP SWEEP	21:02:23	11:47	229162	21590
PTI NO. 1	21:02:49	12:13	228831	21311
END BODYFLAP SWEEP	21:03:31	12:55	225433	20793
PITCH ASI NO. 5	21:03:41	13:05	224549	20678
START POPU	21:03:54	13:18	223418	20508
END POPU	21:04:31	13:55	219395	19946
PTI NO. 1	21:06:21	15:45	205672	17950
START BODYFLAP SWEEP	21:06:33	15:57	203698	17693
END BODYFLAP SWEEP	21:06:54	16:18	201051	17201
START BODYFLAP SWEEP	21:08:50	18:14	179580	13800
PTI NO. 1	21:09:14	18:38	175218	13105
END BODYFLAP SWEEP	21:09:38	19:02	171652	12299
PTI NO. 1	21:12:03	21:27	147591	7883
PTI NO. 2	21:13:32	22:56	122259	5759
PTI NO. 3	21:14:50	24:14	106417	4165
PTI NO. 4	21:15:47	25:11	93611	3137
PTI NO. 5	21:16:52	26:16	76650	2080
PTI NO. 6	21:17:42	27:06	65031	1487
PTI NO. 7	21:18:30	27:54	50844	984
PTI NO. 8	21:18:48	28:12	44924	871
PTI NO. 0 (STRUCTURAL)	21:19:10	28:34	38667	836
START SPEEDBRAKE SWEEP	21:20:34	29:58	14843	563
END SPEEDBRAKE SWEEP	21:20:58	30:22	11675	533

manually. By the time Engle made this maneuver, the Shuttle was over California, having passed the coastline south of Cambria at an altitude of approximately 125,000 feet and a speed of Mach 6.5 (see Figure 7).[33]

A little over 5 minutes later, hundreds of thousands of spectators at Edwards detected the distinctive silhouette of the Shuttle hurtling across the sky, leaving a trail of intermittent contrail segments as its yaw reaction control jets fired in short bursts. As the spectators looked overhead, they heard two sonic booms in rapid succession; Columbia passed directly over the lakebed, flying Mach 1+ at 50,000 feet. Rather than using the automatic mode as planned, Engle opted for manual control to enter the Heading Alignment Circle, because of high upper altitude winds. During this turn, begun at 37,781 feet, the Shuttle descended almost 33,000 feet in just over 3 minutes. Unfortunately, after a brief test maneuver early in the turn, the speedbrakes were inadvertently left in the automatic mode, and as the Shuttle turned into a headwind, the terminal area management guidance system commanded full speedbrakes. The terminal area energy management guidance laws had not adequately anticipated landing pattern winds. This resulted in an excessive loss of energy during the turning pattern and the Shuttle came into the glide slope 40 knots slower and approximately 3000 feet lower than planned, as if STS-2 were determined to avoid the excess energy experienced on STS-1. The autoland system then commanded a pitch-up to get the vehicle on the proper flightpath again. When the automatic flight control system was engaged on final approach, at 9971 feet above the lakebed, the Shuttle was still below the Autoland glideslope capture zone.[34]

As the terminal area energy management guidance system aggressively maneuvered the vehicle toward the capture zone, it caused large pitch and roll oscillations (0.5 to 1.5g pitch and ±20 degrees roll) to occur. The resulting "forced" Autoland

Figure 7



STS-2 ACTUAL VS. PREDICTED GROUND TRACK

capture occurred at 5077 feet above ground level, still 325 feet below the planned glideslope and 63 knots below the desired approach airspeed. At guideslope interception, Autoland pitched the orbiter over to regain lost airspeed. At preflare, the airspeed had increased to 265 knots, still 20 knots less than the planned preflare condition.[35]

Engle was scheduled to perform the flare and landing manually, and at preflare, he selected roll and yaw control stick steering, inadvertently missing the pitch control stick steering switch. As a result, Autoland flew the preflare down to 300 feet above ground level. The maneuver was smooth and "looked right" to the crew who were not aware that the pitch axis was still being flown by the Autoland system. During the preflare, Engle's stick commands in pitch did not exceed the "deadband" (six degrees) required for manual takeover until the Shuttle had reached 300 feet above ground level. At that point reversion to pitch control stick steering occurred and the remainder of the landing occurred under manual control.[36]

Engle had planned to land as close as possible to a prescribed airspeed of 195 knots rather than at a particular point on the runway, and successfully accomplished this objective, touching down only 2 knots below his predicted landing speed: a measure of his abilities, since predictions had been based on a no-wind condition, an April 1981 atmosphere, and an automatically flown trajectory through landing. Touchdown occurred 780 feet beyond the marked threshold of Runway 23 at 21:23:12.8 GMT, 3324 feet short of the point predicted for a 193-knot, no-wind landing. The short touchdown was caused by a strong headwind and the Shuttle's low-energy condition on the final approach.[37]

After the landing, Engle felt that his performance while manually flying the Shuttle on the subsonic Heading Alignment Circle had been subpar, but such was easily dismissed as the

perfectionist nature of a superlative airman. There were a number of factors that conspired to degrade his efficiency. The abbreviated mission had imposed an excessively heavy workload on the crew, leaving virtually no time for sleep. The workload during the descent and landing--the large number of maneuvers and tests--was, under the circumstances, excessively demanding. Finally, both crewmembers were suffering from dehydration. The mission had been performed with fuel cell No. 1 shut down because of the problems encountered early in the flight. In this circumstance, the electrical loads on fuel cells No. 2 and 3 were reduced because of fear that a second cell might fail. In these conditions, more than 3 minutes were required to draw off a cup of water. Because of this, and the presence of gas bubbles in the water, the crew did not ingest adequate amounts. NASA learned a very important lesson on STS-2, and on future missions, special attention would be given to developing realistic work schedules, tasks, and activities so that the crew would be in the best possible condition prior to the very critical entry and landing phases.[38]

Despite the many problems encountered on this flight, Columbia's crew accomplished all of the primary objectives of the mission. Each of the planned entry test maneuvers were performed and, in general, the results again indicated that the basic Shuttle design had the potential of accomplishing both NASA and Department of Defense mission objectives. Most notably, this was the first flight from which surface heating data were obtained above Mach 14 on a manned lifting reentry vehicle.[39]

In general, the Shuttle's ventral surfaces experienced lower heat loads than had been predicted due, once again, to non-catalytic effects, delayed boundary layer transition, and the effects of convective and radiative cooling. Four problem areas, however, were encountered:[40]

Excessive surface temperature on the orbital maneuvering system pods. During a Mach 20 pushover-pullup maneuver, thermocouples on the pods responded with an unexpectedly high increase in temperature below 37 degrees angle-of-attack. The actual heat rate data on the pods, between 35 and 25 degrees angle-of-attack (in the Mach 20 to 8 range), could only be obtained by performing additional pushover-pullup maneuvers in this region on future flights. This data would be necessary in order to optimize the thermal protection on the pods and to permit future crossrange entries to be performed.

Hot boundary layer gases had entered the wing leading edge cavity through leading edge seals at several locations. While this leakage was not considered a safety hazard for the Shuttle's upcoming third flight, it did indicate poor design of the seals and, thus, the test team recommended their modification.

Local hot spots were identified on the aft segments of both payload doors. A thermocouple in one of these areas exceeded a temperature of 900 degrees F for 50 seconds when the Shuttle was at Mach 16. These areas would be isolated for special attention on future flights.

The surface coating on the aft high temperature reusable surface insulation (HRSI) tiles reacted with the aluminum oxide from the solid rocket booster. The resultant reactant appeared as a whitish discoloration on the tiles. Although it was not considered an immediate hazard, the long term effect of the continued contamination of the tiles' coating could impair their emissive properties at elevated temperatures.[41]

While, based on the evidence gathered thus far, it appeared that the Shuttle's aft center-of-gravity limit requirements would be achievable, much more flight test data would be required in order to project the vehicle's ability to meet forward center of gravity requirements. There were still many unknowns in this area, and some stemmed from traditional problems with deltas, particularly the need of a delta to utilize its elevons both for pitch and roll control. Thus, for example, an uncommanded lateral-directional oscillation occurred in the Mach 1.6 region on both flights. A simulator study indicated that the primary cause of the oscillation was lower-than-predicted aileron effectiveness. More forward movement of the Shuttle's center-of-gravity would require even greater elevon deflection in the Mach 1.6 region and reduce aileron effectiveness even further. This study also indicated that the oscillation was slowly divergent with the yaw jets inoperative. The test team recommended that the Shuttle's center-of-gravity should not be permitted farther forward than 66.7 percent and that deactivation of the yaw jets above Mach 1 should not be performed (except under controlled test conditions) until the nature of this oscillatory characteristic in the Mach 2 to 1 region was better understood.[42]

Much was yet to be accomplished on the remaining two test flights. The Space Shuttle had now been twice-launched and brought safely back to earth. In the eyes of the world, this was quite impressive. The problems encountered on the second flight, however, had raised questions in the minds of some observers. Some wondered whether or not four test flights would be enough to shake down the Shuttle before it went into operational service. The two remaining test flights would hold the answers to a lot of questions.[43]

NOTES

1. AFFTC-TR-81-21, Evaluation of the Space Shuttle Orbiter First Orbital Flight; Final Report, Jul 81; AFFTC-TR-82-1, Evaluation of the Space Shuttle Orbiter Second Orbital Flight: Final Report, Feb 82.

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3. Armstrong interview; Memo., Mr. Johnny Armstrong, AFFTC Space Shuttle Evaluation Project Manager, OAMV, to AFFTC History Office, subj: OAMV Involvement in Shuttle Evaluation, 20 Sep 82.

4. Armstrong memo to History Office.

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9. John W. Young, NASA, and Capt. Robert L. Crippen, USN, "Spaceship Columbia's First Flight," Society of Experimental Test Pilots Twenty-fifth Symposium Proceedings (23-6 Sep 81), Technical Review, Vol. 16, No. 2. For quotes on Shuttle, see Dave Dooling, "Launch the Shuttle," Astronautics and Aeronautics, XIX, n. 3 (Mar 81), passim.

10. Ibid.; AFFTC-TR-81-21.

11. Ibid.

12. Young and Crippen, "Spaceship Columbia's First Flight."

13. Ibid.; AFFTC-TR-81-21.

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15. Young and Crippen, "Spaceship Columbia's First Flight."

16. Ibid.

17. Ibid.; AFFTC-TR-81-21.

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19. AFFTC-TR-81-21.

20. Ibid.

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22. Ibid.

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25. Ibid.

26. Ibid.

27. AFFTC News Release No. 4-7-81, 29 Apr 81; AFFTC News Release (draft) ca. Oct 81.

28. Ibid.

29. As quoted in "Second Reentry of Shuttle to Expand Control Data," Aviation Week & Space Technology, 20 Jul 81.

30. Armstrong interview.

31. Ibid.

32. Ibid.

33. Ibid.

34. Ibid.

35. Ibid.

36. Ibid.

37. Ibid.

38. Ibid.

39. Ibid.

40. Ibid.

41. Ibid.

42. Ibid.

43. See, for example, "Editorial: Columbia's Return to Space," Aviation Week & Space Technology, 23 Nov 81.

CHAPTER III

FROM FLIGHT TEST TO OPERATIONAL SYSTEM

The year 1982 continued the firsts for the Space Shuttle: its first landing away from Edwards; its first landing on a conventional concrete runway, and, with a crew increased to four members, its first operational mission. It was a year in which the Shuttle exacted even more widespread public interest--highlighted by the July 4th landing of STS-4 which drew an estimated crowd of 750,000 enthusiastic spectators, among them President and Mrs. Ronald Reagan.

The year also saw the successful conclusion of the Shuttle's formal flight test program: a once fantastic concept had matured into a practical (if fatally flawed) system.

This, however, did not mean that the Shuttle orbiters would no longer undergo flight tests. A large volume of data had been collected and evaluated during the brief 4-flight test program. Even so, four flights were scarcely enough to evaluate a system as sophisticated and complex as the Shuttle. There were a number of areas still requiring analysis in order to understand and exploit the full range of the vehicle's capabilities. There were also some areas where unanticipated problems had surfaced during the formal test program. These problem areas were not necessarily regarded as matters of faulty performance; more often, they were simply surprises. Like any new system under development, the Space Shuttle design had been subjected to intensive simulation studies so that reasonable predictions could be made about its actual performance. In fact, because of the complexity of the system, its very high national priority, and the fact that its

flight test program would be limited to only four missions, the simulation studies for the Shuttle were extraordinarily detailed. As useful as they are, however, simulations are not yet infallible substitutes for reality, and that is why flight test remains such a critical part of the development process. Thus, not surprisingly, there were some areas where the Shuttle's actual performance did not coincide with predictions. These areas needed to be studied and understood. Finally, as each of the three new orbiters joined the Columbia in the Shuttle fleet (or each time any of them underwent significant modification), they would be subject to at least one mission given over largely to flight test to insure the proper functioning of all systems.

Engineers, from the AFFTC's Office of Advanced Manned Vehicles (OAMV), expected to be directly involved with Shuttle testing through 1985, the year when the first launches and landings at Vandenberg AFB were scheduled to begin. This small organization was specifically tasked with the responsibility for assessing the orbiter's ability to survive the reentry environment, maneuver to the selected landing site, and land under all weather conditions with a 32,000-pound cargo. This assessment was broken down into seven sub-objectives: flight control performance, guidance and navigation performance, landing constraints, entry heating and thermal protection performance, maximum crossrange capability, subsystem performance, and payload bay environment. The data collected in these areas would be used not only to improve the Shuttle's mission qualities but also to provide the aerospace industry with aid in developing future manned and unmanned spacecraft.[1]

There was a concerted effort at the Flight Test Center to enhance the OAMV's space test capabilities and to permanently establish the office as the Air Force organization responsible for the developmental test and evaluation of future manned military hypersonic and space systems. Despite the fact that HQ USAF had

"validated" the requirement for such a capability at the Center, no action had, as yet, been taken to allocate permanent positions for the proposed undertaking. The office, made up largely of individuals who had worked on such critical projects as the X-15 and lifting body programs, continued to be manned on a temporary, year-to-year basis. This situation became a high priority concern among officials at the Center.[2]

Major General Philip J. Conley, the AFFTC commander, pressed manpower officials at Systems Command headquarters for a resolution of the matter. Observing that "our independent assessment of the Space Shuttle Orbiter reentry characteristics with respect to DoD mission requirements represents the final step in transitioning Air Force aerodynamic experience with the newly emerging field of military space technology," he argued that the Air Force must have its own self-sufficient test capability. "The only alternative," he noted, "would result in a total Air Force dependence on contractors for this expertise." This should not and need not happen. Though the OAMV, a "small but elite group of Air Force civilians and blue suiters (12 people)," in addition to a few individuals from the Flight Dynamics Laboratory at Wright-Patterson AFB, represented the "total Air Force capability" in the field of manned reentry space systems, this small cadre of experienced personnel would be sufficient to provide a core around which to build an effective independent test capability. Repeated attempts to secure authorizations for permanent positions for these people had come to naught and the Center was scheduled to lose seven of its twelve positions by October of 1983. Should this occur, the Air Force would stand to lose an almost irreplaceable pool of expertise. In closing, General Conley requested assistance in getting the manpower request back on track and, in the meantime, advice on how to "retain this important Air Force capability until permanent spaces become available."[3]

Officials at Systems Command were sympathetic to these arguments, and while no permanent positions had yet been allocated, proposed the following:[4]

> As an interim measure, to solve the immediate problem, we will extend through June 83 (FQ 3/83) the four civilian authorizations presently programed to terminate after Sep 82 (FQ 4/82). For the longer term, we will press for an Air Staff allocation to satisfy the requirement on a permanent basis. Meanwhile, we will work with your servicing MET to try to find a way to extend the short term spaces at least through FY84.

On to New Mexico: The Flight of STS-3

The Columbia's third mission was planned to be a seven-day orbital flight with a landing on Rogers Dry Lake at Edwards. However, 11 days prior to the planned landing, the primary landing site was changed to Northrup Strip at White Sands, New Mexico, because heavy winter rains had inundated Rogers "dry" lakebed. The orbiter was launched from Pad A of launch complex 39 at the Kennedy Space Center on March 22. The flight was extended one day when a "wave off" was called during the last orbital pass prior to the scheduled deorbit burn. Unacceptably high surface winds, and the resultant poor visibility at Northrup Strip, forced the one-day delay. Thus, on March 30 as the Columbia's wheels touched down in Runway 17, the longest Shuttle mission to date came to a successful conclusion.[5]

The crew had initiated deorbit burn during the Columbia's 129th orbit with the vehicle at 122 nautical miles altitude and at an orbital inclination of 38 degrees. The two orbital maneuvering system engines fired for 2 minutes and 29 seconds, producing a 270 feet per second reduction in velocity. After the deorbit burn, Columbia executed a pitch-around maneuver to establish entry attitude; thereafter, it flew a now standard 40-degree angle-of-attack entry profile. Since the deorbit occurred on a different orbit than originally planned, the Shuttle's entry track passed

approximately 40 nautical miles south of San Diego rather than the normal end-of-mission entry for Northrup Strip which would have been 80 nautical miles south of San Francisco. This new entry track resulted in bank reversal velocities other than those planned. The actual ground track, however, was close to that predicted for the revised entry conditions.[6]

The Shuttle's first bank maneuver was performed in the manual control stick steering mode. The first three bank reversals--at 17,784, 9,369, and 4,712 feet per second--were performed in the automatic mode while the fourth reversal--at 2,595 feet per second--was performed manually by the mission commander Col. Jack R. Lousma. Six aerodynamic test maneuvers (five programed test inputs, or PTIs, and one aerodynamic stick input) were performed during the entry. A maneuver designed for the Mach 4 region was inadvertently performed in place of the maneuver scheduled to occur at Mach 8.4. Nor was this maneuver repeated at Mach 4 because it conflicted with a bank reversal. No data were obtained from one of the programed test inputs, a structural test point, because of the failure of a flight instrumentation wide band recorder.[7]

In general, Shuttle's performance on this mission further increased the confidence level in its ability to successfully fulfill operational missions. There were, however, a number of areas of concern: previously encountered anomalies which were still little understood; problems which were encountered for the first time on this mission; and potential problem areas which, for one reason or another, had not yet been subject to evaluation.

Thus, for example, low-amplitude (0.25 Hertz) lateral-directional oscillations were again encountered in the Mach 1.7-1.4 range. They were, in fact, more "noticeable" than on previous flights because more yaw jet firings--five on STS-3 versus one on STS-1--were required to contain the phenomenon.

This, in combination with the fact that the bodyflap was again fully deflected against its upper stop, indicated a continuing requirement for cautious expansion of the forward center-of-gravity envelope. Thus, while OAMV engineers were confident that the Shuttle's aft center-of-gravity limit would be achievable, they remained skeptical concerning flight stability at more forward centers of gravity. Some DoD payloads could, conceivably, impose forward centers of gravity and thus the need for continued careful examination of this area. The possibility loomed that a restrictive forward center-of-gravity placard might have to be imposed on operational flights.[8]

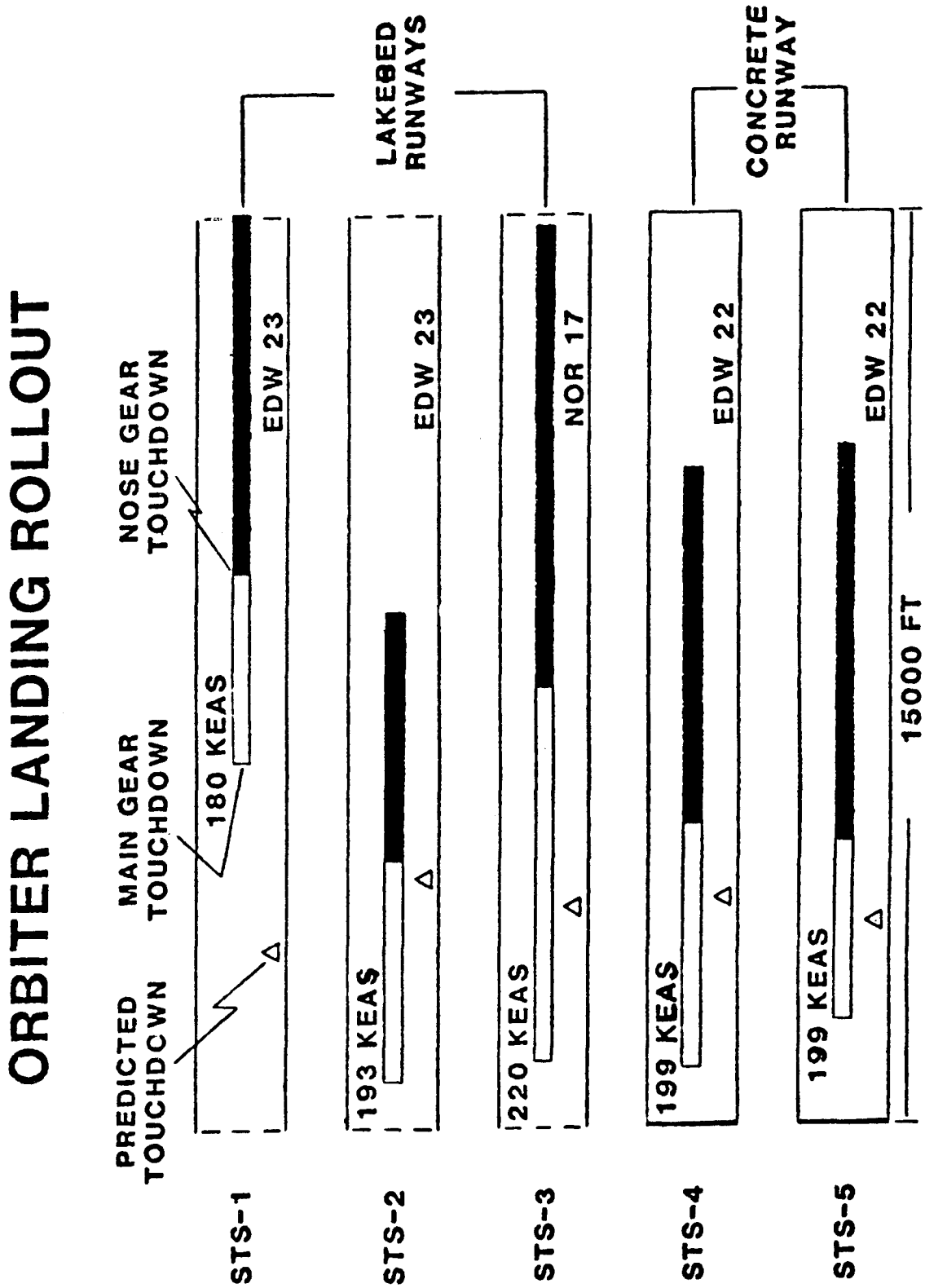
Some concern was also raised over the orbiter's sensitivity to lateral center-of-gravity offsets when an unexpected lateral control trim was observed during the entry. After the elevons were activated at a dynamic pressure of 2 pounds per square foot, the aileron trim integrated slowly to a value of 0.5 degree (3.5 degrees left aileron and 2.5 degrees right) and stayed there until approximately Mach 12. After Mach 12, the aileron trim increased to a maximum value of 0.7 degree near Mach 10 and then slowly decreased again. While there was some indication that flow asymmetry in this region may have been a contributing factor, OAMV engineers recommended that much more precise preflight center-of-gravity predictions should be implemented prior to operational missions. Also, aileron trim requirements would have to be monitored carefully and better understood because only 3.0 degrees would be available to counteract lateral-directional offsets and asymmetries.[9]

Evaluation of the Shuttle's thermal protection system produced findings similar to those of the previous two flights. Generally speaking, temperatures were lower than predicted and, hence, the orbiter's protection system was more than able to withstand the intense heat generated by reentry. There were some areas of concern, however. Post-flight inspections again revealed that

most of the low-temperature reusable surface insulation (LRSI) tiles over the entire vehicle had hairline cracks in their surface coatings. This cracking could conceivably cause the surface tiles to lose their surface coating, thereby requiring replacement and increasing turnaround time when the Shuttle entered its operational phase. The heat environment around the orbital maneuvering system pods was also, once again, higher than predicted. The right hand pod had two areas where the graphite epoxy structure debonded. These areas were exposed by impact damage on the low-temperature reusable surface insulation (LRSI) tiles and damage to the area where the LRSI and the felt reusable surface insulation (FRSI) met. OAMV engineers concluded that the thermal protection system on the pods would require redesign before the Shuttle entered operational service: a dramatic confirmation of pre-STS-1 studies that had predicted such problems. This would be especially critical for the polar orbital missions flying out of Vandenberg AFB where crossrange reentries were expected to subject the Shuttle to significantly higher temperatures than had, heretofore, been encountered. One other area requiring modification would be the payload bay doors. The FRSI on the doors had a design limit of 700 degrees F. A thermocouple located near the area, however, indicated temperatures as high as 1130 degrees F. The thermal protection material in this area would have to be replaced with a material with higher temperature capability.[10]

Columbia's landing on Northrup Strip was less than perfect. A number of factors conspired to generate a relatively rough, high-speed landing followed by the longest rollout of the Shuttle test program (Figure 1). One of the critical concerns of the test program was to ascertain the orbiter's ability to land at a planned location under a variety of weather conditions. The performance of the autoland guidance system was pivotal to successful all weather landings. On STS-3, the vehicle

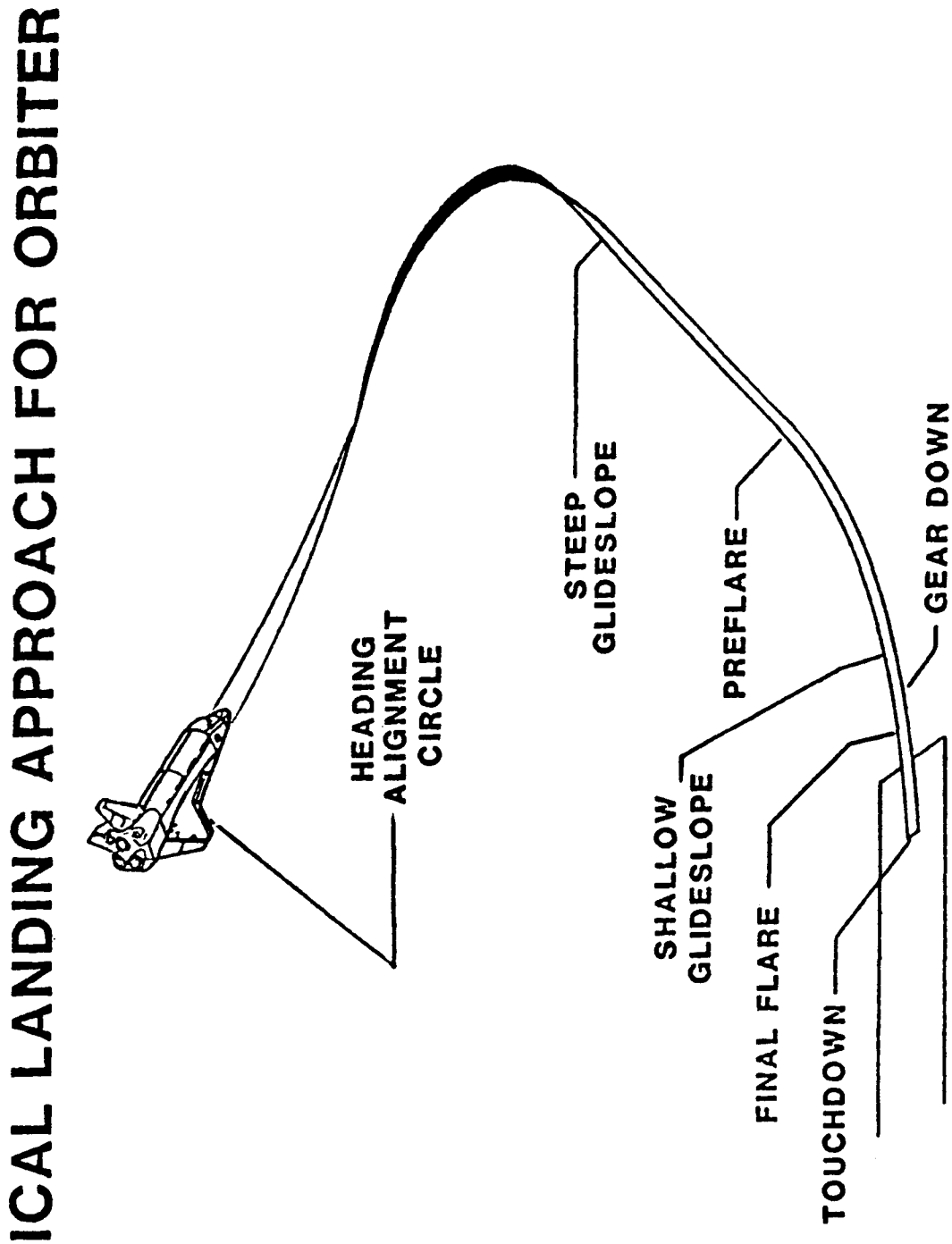
Figure 1


ORBITER LANDING ROLLOUT
LAKEBED RUNWAYS
CONCRETE RUNWAY
NOSE GEAR TOUCHDOWN
MAIN GEAR TOUCHDOWN
PREDICTED TOUCHDOWN
180 KEAS
193 KEAS
220 KEAS
199 KEAS
199 KEAS
EDW 23
EDW 23
NOR 17
EDW 22
EDW 22
15000 FT
STS-1
STS-2
STS-3
STS-4
STS-5

encountered wind shear during final approach and the autoland system was unable to maintain a constant airspeed.[11]

Figure 2 shows a generalized view of a typical Shuttle approach. The sequence of events for STS-3 commenced as the autoland guidance system engaged at 9869 feet above ground level (73 feet below the planned glideslope) and satisfactorily flew the orbiter down the glideslope. The orbiter's track, however, was displaced slightly to the right of centerline of the final approach because of an azimuth bias in the microwave scanning beam landing system. During the final approach the speedbrakes were modulated by the system in an unsuccessful attempt to maintain a planned approach airspeed of 285 knots equivalent airspeed (KEAS) through a moderate wind shear. Then, as the airspeed dropped to 275 KEAS, the auto guidance issued a speedbrake closed command at 4000 feet above ground level (as opposed to the 2500 feet AGL used on previous flights). After passing through the wind shear, the airspeed increased to 303 KEAS at preflare--some 18 KEAS above the desired airspeed. The preflare maneuver was completed in autoland and then, just 143 feet above ground level, Lousma took over in the control stick steering mode for the landing. (He felt that the autoland system had made an excessively low, flat approach and left the orbiter at a higher than desired airspeed at the time he took over control.) He made an aft stick input in an effort to dump some of Columbia's excess energy and land at the planned airspeed of 195 KEAS. Based on the normal acceleration forces he sensed after this input, he felt that the orbiter was about to "balloon." So he applied a forward input, followed by an aft input, and then centered the stick approximately 2 seconds prior to touchdown; momentarily, it looked as if the PIO problem of the ALT flights had returned. Touchdown of the main gear occurred sooner than expected or desired. Lousma stated that the time between manual takeover and touchdown--only 13 seconds--was much too brief to adequately "feel out" the vehicle during the critical, high-stress landing phase. Because longitudinal control

Figure 2


TYPICAL LANDING APPROACH FOR ORBITER
HEADING ALIGNMENT CIRCLE
STEEP GLIDESLOPE
PREFLARE
SHALLOW GLIDESLOPE
FINAL FLARE
TOUCHDOWN
GEAR DOWN

of the vehicle was difficult in high-stress flight situations, the OAMV concurred with Lousma's assessment in this matter. If the landing was to be made manually, the commander's takeover should occur early enough in the approach to allow "adaptation"--preferably at preflare or sooner.[12]

OAMV engineers also recommended that a minimum altitude for manual takeover should be established for landings planned to be made in autoland. The landing experience from STS-3 probably reflected the lowest altitude limit for which a crew could be expected to back up the autoland system in real weather conditions. In fact, all landings in weather conditions with ceilings less than 200 feet and visibility less than about a mile should be precommitted to the all-automatic mode.[13]

Because of the high-energy state at preflare and the inadequate amount of time for Lousma to adapt and properly flare the vehicle, the Shuttle touched down fast (220 KEAS as opposed to the predicted 195 KEAS) and short of the planned touchdown point (1092 feet past the threshold as opposed to the predicted 3613 feet). At touchdown, the sink rate was a very rapid 5.6 feet per second and the orbiter was 29 feet to the right of centerline. Lousma then attempted to hold the nose gear off until the vehicle reached a lower speed. He initially applied a small aft stick input without success. When the nose fell through to an angle of attack of zero degrees at 190 KEAS, he applied one-half full-aft stick and the nose abruptly rose to +6 degrees, certainly getting the attention of spectators. He then applied full-forward stick deflection which limited the angle-of-attack to +6 degrees and, of course, caused a subsequent nose down pitch rate. Nose gear touchdown occurred at a velocity of 175 KEAS and a pitch rate of -7.4 degrees per second. Reporting on this sequence of events, Lousma subsequently stated that there was a significant time delay between stick input and nose attitude change, and that attitude control was clearly non-linear in response to stick command.[14]

A planned braking test was not performed, but light to moderate braking (maximum of 900 psi as opposed to 1500 psia for full braking) was applied from 150 KEAS for about 35 seconds. This resulted in an average deceleration rate of about 0.25g. The combination of high speed and relatively light braking resulted in a rollout of 13,732 feet. The Shuttle came to a stop just 176 feet from the end of the 15,000-foot marked runway.[15]

Similar high-energy landings had occurred on STS-1 and the fifth Approach and Landing Test (ALT). Quite naturally, OAMV engineers remained unconvinced that the Shuttle's landing capabilities had matured to the point where routine safe landings could be made within the confines of a 15,000-foot concrete runway. They recommended that the autoland speedbrake logic and crew training procedures should be modified and tested to ensure that a constant preflare energy state could be maintained through typical wind conditions prior to committing to autoland or manual landings on a concrete runway.[16]

There were some important areas for which flight test data remained incomplete after STS-3. The Shuttle's crossrange capability, for example, had yet to be validated. Crossrange requirements were dictated by the need for an "Abort Once Around" capability, a reduction in on-orbit "wait time" for return, and a relaxation of launch window constraints. An abort once around from a Vandenberg AFB polar launch, for example, would require the Shuttle to make an 1100-mile crossrange entry. While cumulative results from each of the first three flights offered favorable indications that such entries might be possible with future modified orbiters, very little data had been collected on the heat environment and lift-to-drag ratio at angles-of-attack lower than 40 degrees. Crossrange entries would require flight at these lower angles-of-attack. Plans, at that time, called for pushover-pullup maneuvers on future flights which would serve to verify the crossrange capability. Inadequate thermal instrumentation had, to that point in time, prevented implementation of such maneuvers.[17]

Flight testers had hoped to perform a crosswind landing on STS-3. In particular, they wanted to demonstrate the Shuttle's ability to land safely in a 10-15 knot crosswind before they committed it to a concrete runway landing. This would be especially relevant for any landings at the Kennedy Space Center where the runway was not aligned with prevailing winds. STS-3 did not present the opportunity for such a landing and thus flight test officials hoped to be able to attempt it on the Shuttle's next mission.[18]

The Independence Day Mission: STS-4

Shuttle's fourth orbital flight launched from Pad A of launch complex 39 at the Kennedy Space Center on June 27. Planned for 111 orbits, the 7-day mission extended 1 orbit so that the landing could occur at a time which would be compatible with a Presidential visit to Edwards. Piloted by Capt. Thomas K. Mattingly, USN, and Col. Henry W. Hartsfield, USAF (ret.), Columbia touched down on runway 22--the main concrete runway at Edwards--at 9:09 a.m. on July 4, bringing the Shuttle test program to a close. In its 4 test flights, Columbia had made 314 orbits of the earth and logged over 9 million miles.[19]

The sequence of events for the descent phase of the mission commenced with the deorbit burn during the 112th orbit--as the Shuttle was flying over the Indian Ocean--while the vehicle was at an orbital altitude of 175 nautical miles and an inclination of 28.5 degrees. The -308.8 feet per second burn was targeted to place the orbiter at the 411,298 foot entry interface point when it was only 3,815 nautical miles from Edwards, significantly less than the 4,000-4,200 nautical miles of the previous flights. These conditions required the Columbia to make a 580-nautical mile crossrange reentry to the north of its 28.5 degree orbital inclination. Results of this, along with other data from this flight, was expected to increase the Shuttle's crossrange limit to

approximately 900 nautical miles, 200 nautical miles short of its design limit.[20]

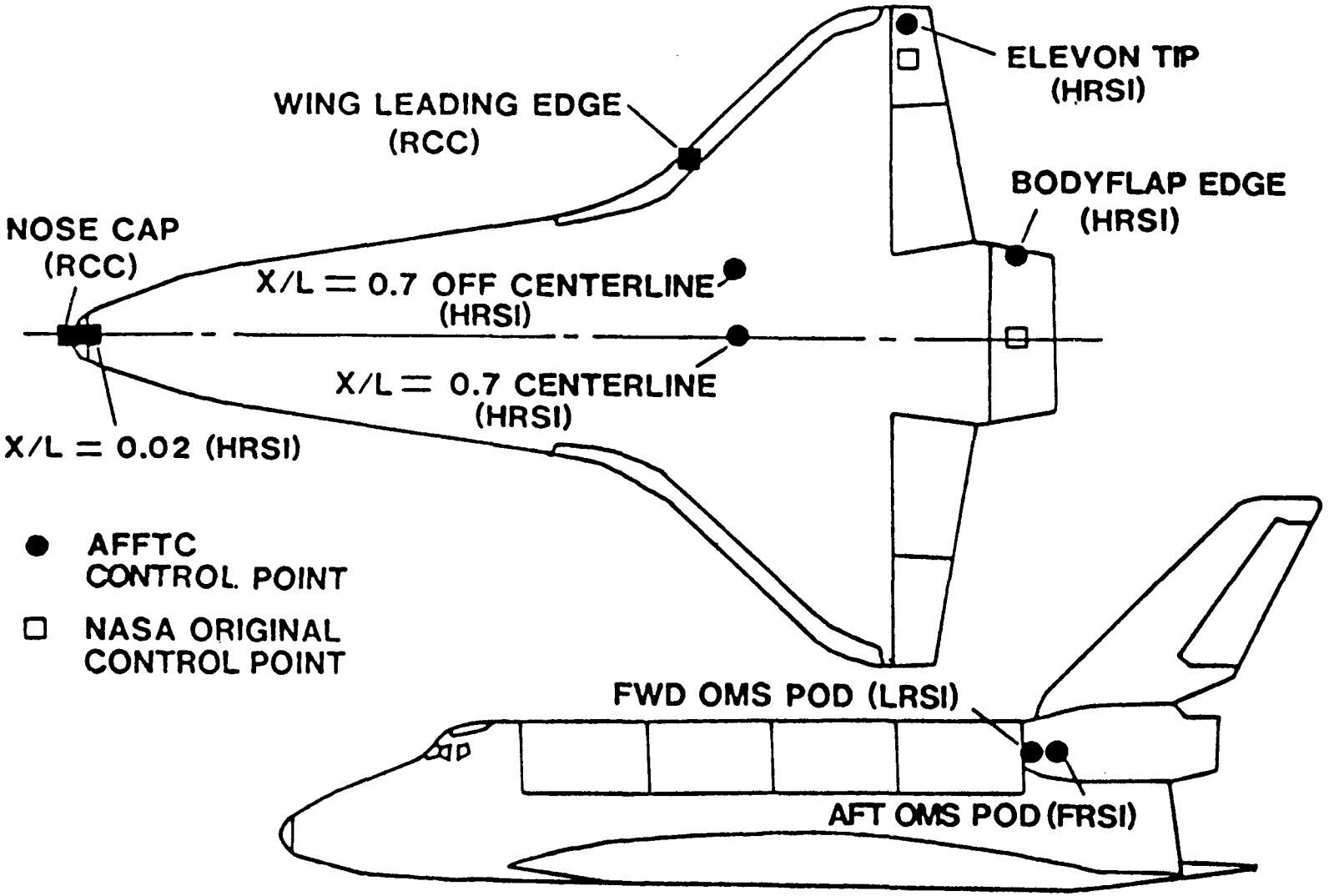
The Shuttle was subjected to nine entry data maneuvers during this flight. They included the following:[21]

Mach No.	Maneuver
23.7	Roll PTI
21.4	Roll PTI
21.1	Pitch ASI
17.9	Roll PTI
16.3	Roll PTI
13.7-12.0	Pullup-Pushover
8.3-7.7	Pullup-Pushover
2.8	Roll PTI
2.2-1.0	Structural PTI

PTI = Programed Test Input
ASI = Aerodynamic Stick Input

The crew performed the two pullup-pushover maneuvers in the manual mode in order to increase temperatures on the vehicle, particularly in the areas around the orbital maneuvering system pods, to generate data permitting future reentries at lower angles of attack where heating would be more severe but crossrange increased. Figure 3 shows heating instrumentation locations on STS-4. The maneuvers yielded excellent performance data covering the angle of attack ranges of 44.5 to 31.4 degrees and 34.4 to 24.3 degrees. While the data from these maneuvers (along with the data from a pushover-pullup maneuver performed at Mach 20 on STS-2) provided "favorable trends that the predicted L/D at lower angles of attack required for high crossrange entries will be realized," they also further substantiated the need for a redesign of the thermal protection system on the orbital maneuvering system

TEMPERATURE CONTROL POINTS



HRSI = High-Temperature Reusable Surface Insulation
LRSI = Low-Temperature Reusable Surface Insulation
FRSI = Felt Reusable Surface Insulation
RCC = Reinforced Carbon-Carbon

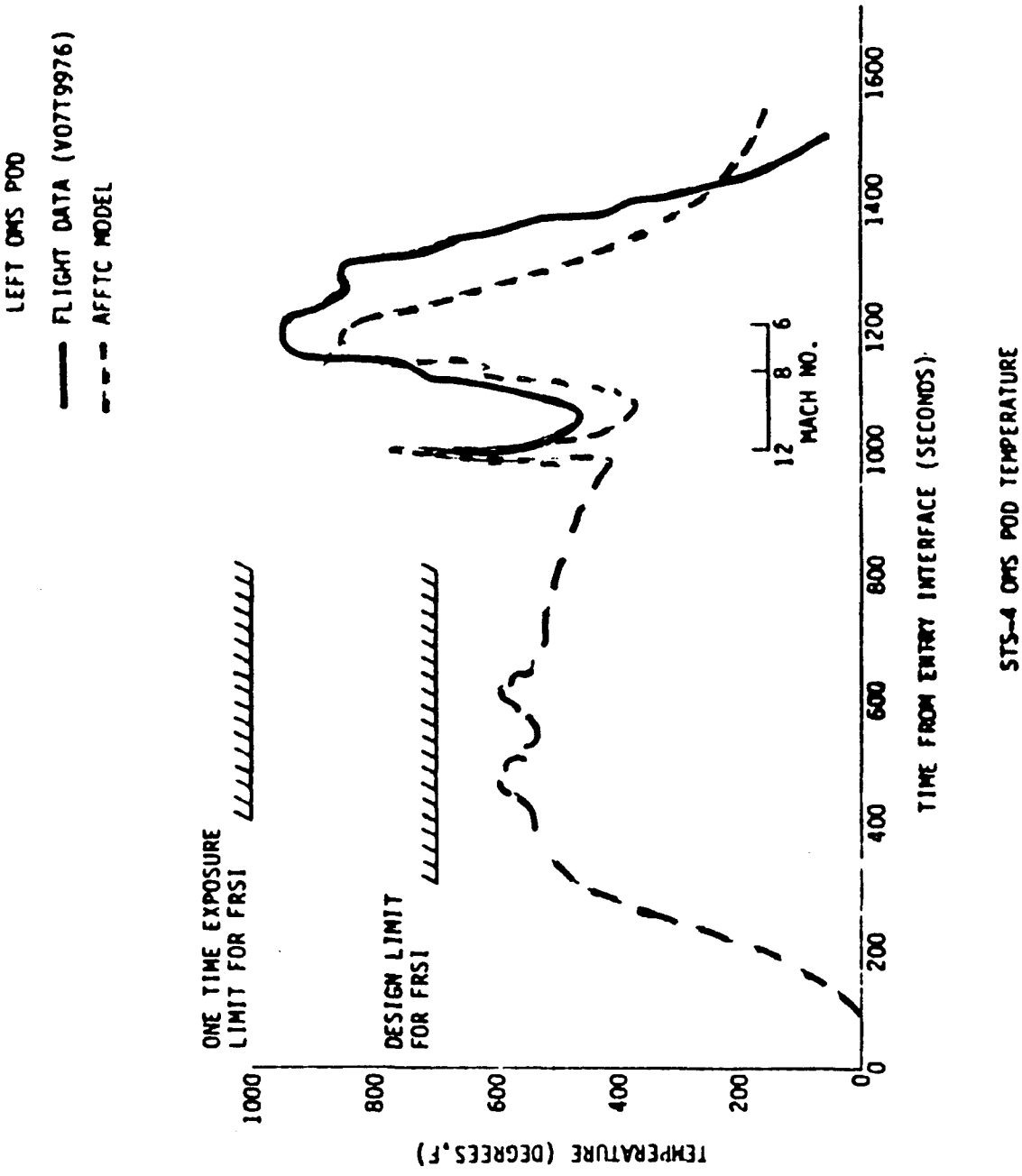
Figure 3

pods. During the second brief pullup-pushover maneuver, the temperature on one of the pods reached a maximum of 947 degrees F (the felt reusable surface insulation on the pods had a design limit of 700 degrees F and a one-time exposure limit of 1000 degrees F--see Fig. 4). The increased heating was due to vortex impingement which occurred at a much higher angle of attack than predicted (36 degrees rather than the 30 degrees predicted by wind tunnel tests). OAMV engineers were also concerned about some potential heating problems on the lower surface of the orbiter. Data from the Mach 13 pullup-pushover maneuver indicated a momentary flow transition at 44 degrees angle of attack. This, along with the slumping of tiles immediately aft of the nose cap and wing leading edge, suggested that the heating environment on the lower surface might be more severe than predicted for angles of attack above 40 degrees.[22]

It was also during this Mach 13 maneuver that an unexpected event occurred. For this maneuver, Mattingly was supposed to gradually pull the orbiter's nose up about 5 degrees to approximately 45 degrees angle of attack, then to push the nose over until it reached 30 degrees. Then, without pausing, he was supposed to pull the nose back smoothly to 40 degrees. The maneuver was designed so that the vehicle's energy and trajectory situation would not be severely affected. Shortly after the maneuver, however, radar tracking data indicated that the Columbia had a 500-600 feet per second sink rate which was substantially in excess of its anticipated rate of 300 feet per second. This high sink rate created an energy situation in which the orbiter was about 20 nautical miles beyond where it would have been if the proper energy state had been maintained. The Columbia's navigation system recognized the error and began correcting for it by flying a higher drag/acceleration profile. This was done by increasing the bank angle via a 101 degree guidance roll command on the roll reversal after the pullup-pushover maneuver was completed. The angle of attack modulation scheme in the guidance

Figure 4

STS-4 OMS POD TEMPERATURES



system was also commanding the maximum possible angle of attack (3 degrees above nominal) until Mach 10.2 to further control the energy state. The error was well within the orbiter's ability to correct and the energy level was back to "nominal" by Mach 3.[23]

It took some time to discover the cause of this "unexpected" event. Pilot error was ultimately determined to be the source of the deviation. Both crewmembers reported that their workload "seemed rushed from Mach 14 to touchdown" during the actual entry (although it did not seem so when they listened to cockpit voice tapes after the flight). During the Mach 13 maneuver, the commander had inadvertently reduced the orbiter's bank angle to 35.5 degrees from the desired value of 60 degrees. This error produced the excess energy condition. OAMV engineers noted that the pilots of previous lifting body and research vehicle programs had experienced similar "rushed" sensations during the brief, but concentrated, high-stress workload phases of their missions. They concluded:[24]

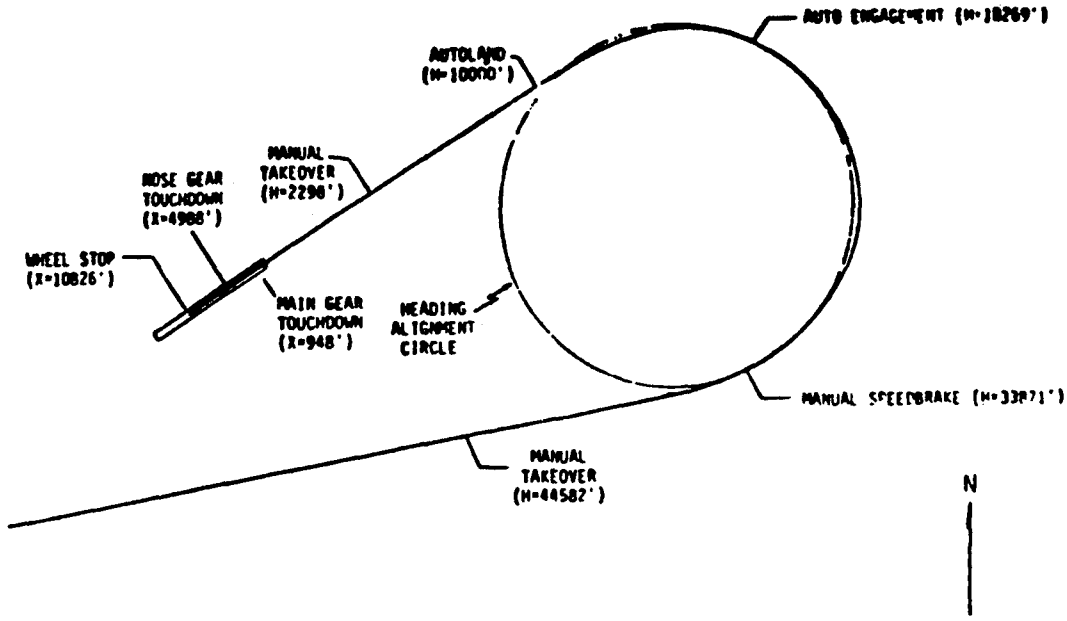
> It was found that by increasing the simulation computation rate by a factor of approximately 1.5, the apparent difference in perceived time experienced during actual flight and in simulator training could be duplicated. Future Orbiter crews should consider training using increased simulation computation rate during entry.

The remainder of the descent was relatively trouble free. Once again, the Shuttle experienced the same 0.25 Hz lateral-directional oscillation between Mach 1.8 and 1.3 which had been encountered on each of the previous flights. Test maneuvers were planned on future flights, beginning with STS-5, which would identify the cause of the anomaly. This, along with full-up deflection of the body flap in the transonic regime, would continue to be an area of particular concern for flights at forward centers of gravity.[25]

The whole landing phase on STS-4 was much smoother than the previous flight (see Figures 5a and 5b). Autoland guidance was engaged at approximately 10,000 feet. The autoland system satisfactorily guided the craft down the glideslope, displacing it slightly to the right of course because of an azimuth signal error from the microwave landing system. Mattingly manually operated the speedbrake on final so that the desired airspeed of 285 KEAS could be maintained. This procedure, plus the fact that winds were far less of a problem on this landing, resulted in airspeed control which was much improved over previous landings.[26]

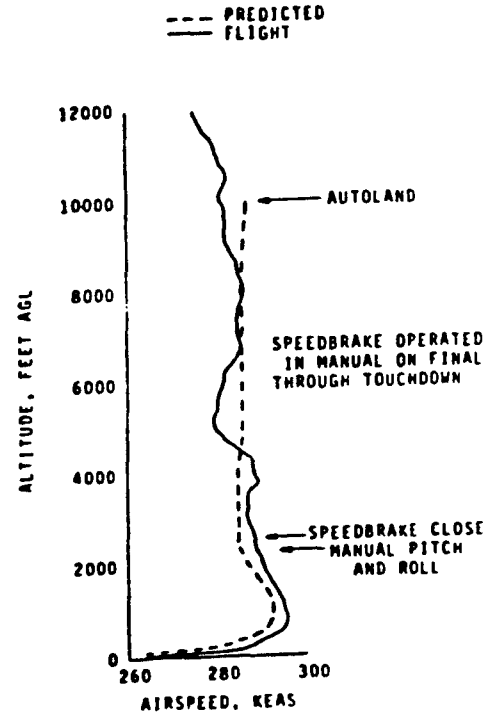
Mattingly closed the speedbrake at 2600 feet (autoland would have commanded this to occur at 4000 feet which would have resulted in a much higher energy approach); he took over manual pitch and roll control at 2298 feet and executed the flare. The main landing gear extended early at 293 KEAS and 421 feet above ground level ("nominal" was 270 KEAS and 200 feet above ground level) so that Mattingly could focus total concentration on the landing task as the orbiter approached the runway. Throughout this sequence and while performing the landing, he relied heavily on calls (airspeed, altitude, etc.) from pilot Henry Hartsfield. He later strongly emphasized the value of this rapid transfer of information during the landing sequence and suggested that an effective Head Up Display (HUD) which would present this information directly to the commander would be "highly desirable" for landings on concrete runways - something Day's SSTG had suggested in 1969. The main gear touched down at 199 KEAS, 2807 feet short of the predicted touchdown point on Runway 22. The landing parameters were as follows:[27]

Figure 5



STS-4 APPROACH AND LANDING

Fig. 5a



STS-4 LANDING APPROACH AIRSPEED

Fig. 5b

	Main Gear Touchdown Actual	Predicted*	Nose Gear Touchdown Actual	Predicted*
Distance from Threshold (ft)	948	3755	4988	7319
Airspeed (KEAS)	199	191	165	157
Sink Rate (fps)	-1.7	--	--	--

*STS-4 Cycle 5.

After the touchdown, Mattingly extended the speedbrake to help control derotation and shorten the rollout distance. During the planned braking test, he was supposed to maintain a deceleration rate of 9 fps^2 beginning at 140 KEAS. Braking commenced at 135 KEAS and the deceleration rate averaged close to 8 fps^2 and peaked briefly at 9 fps^2. The Columbia's total rollout distance was 9,878 feet and it came to a stop 10,826 feet past the threshold (see Figure 1).[28]

Though the landing phase had been relatively successful, there was still some concern about problems which could adversely affect the commander's landing performance. On STS-4 he had been given more leeway on where to fly manual versus auto and the initial portion of the heading alignment circle had been flown in the manual mode so that he could get more of a feel for the Shuttle's handling qualities before the critical piloting tasks associated with landing on a concrete runway. This had contributed to the improved landing performance over the previous mission. The workload and stress level on the commander, however, were still very high. Very substantial effort was required to maintain the proper energy level during the landing phase and to attempt to touch down near the planned location. He still had to concentrate too much on avoiding large inputs which might cause pilot induced oscillations, ballooning, and other over control tendencies. In addition to these handling qualities problems, Mattingly reported that his perception of height above ground was poor--he thought he was higher than he actually was. This, at least in part, explained the short touchdown. Although he was aiming for a point

2500 feet beyond the threshold, the Columbia had touched down just 948 feet beyond the threshold. All of these factors led him to conclude that "he could not consistently repeat the demonstrated landing performance." And, indeed, OAMV engineers shared this concern, concluding that "the Orbiter landings to date have not demonstrated the ability to achieve consistent conditions during final approach and landing."[29]

Although a successful landing on a concrete runway had now been accomplished, test personnel were not about to concede that such landings could be made on a "routine" basis. In addition to the approach and landing problems, the Shuttle's brake system had not yet demonstrated fully adequate performance. Perhaps most disappointing of all, however, was the fact that the vehicle's crosswind landing capabilities had still not been verified. For the operational landings at Kennedy Space Center, such a capability would be an absolute necessity.[30]

Another important capability which had yet to be demonstrated was an autoland approach extending all the way through touchdown. OAMV engineers believed that such a landing was now feasible on a lakebed runway. The Shuttle's subsystems had proven capable of providing accurate data to the autoland guidance system--with the relatively "minor" exception of the azimuth errors generated by the microwave landing system. These errors would be acceptable for a lakebed landing where precise centerline touchdowns would not be required. The only significant modification which would be required would be a change in the autoland speedbrake logic permitting positive control all the way down to the preflare point.[31] But STS-4 was not all technology; it served to focus public feeling for space, for among the watchers at Edwards was none other than the President of the United States.

President Reagan's Address and Challenger's Departure

"You have just given the American people a Fourth of July to remember," were the words with which President Ronald Reagan

greeted astronauts Mattingly and Hartsfield. Standing before the Enterprise used for approach and landing tests, the President formally greeted the astronauts (he had actually greeted them an hour earlier on the runway as they descended from the Columbia) and used the occasion to deliver a long-awaited address on national space policy. NASA officials, in particular, were hoping for a commitment on a fifth space Shuttle and a space station, neither of which, however, they got.[32]

In thanking the astronauts, the President drew upon an assortment of historical allusions. He compared the flight of the Columbia to the voyages of the "Yankee Clippers" when he stated:[33]

> Today, I think you have helped recreate the anticipation of excitement felt in those home ports as those gallant ships were spotted on the horizon, heading in after a long voyage.

He also likened the end of the Shuttle test program to the driving of the gold spike at Promontory Point, Utah, in 1869, an event which signified the linking of the transcontinental railroad. The Shuttle symbolized the resurrection of the nation's confidence in itself. "The space program, in general, and the Shuttle program, in particular," he noted, "have gone a long way to help our country recapture its spirit of vitality and confidence." The Shuttle, and the entire space program, were clear evidence that "Americans still have the true grit to combat a savage wilderness."[34] With these words, the space age had been integrated into the time-honored tradition of Fourth-of-July oratory.

Fourth-of-July oratory was, indeed, appropriate for the festive occasion. Many interested spectators, however, were anxiously awaiting the President's substantive remarks on the direction U.S. space efforts would take over the next decade. This long-awaited policy statement was the result of a 10-month

long interagency review requested by President Reagan in August of 1981. Those who were hoping for specific commitments, however, had to be satisfied with a rather broad declaration that the nation would be looking "aggressively to the future by . . . establishing a more permanent presence in space." Neither the long-sought fifth orbiter nor the space station were mentioned by the President.[35]

As he was speaking, the President noted that Challenger, the second true orbiter, was ready for takeoff atop its 747 carrier aircraft. As it thundered low overhead, 747 pilot Fitzhugh L. Fulton dipped the combo's right wing in a presidential salute. President Reagan acknowledged the gesture by leading the crowd in a chorus of "God Bless America." Then, as the 747 and its ill-fated cargo soared off on the first leg of a two-day journey to the Kennedy Space Center, he quipped: "This has got to beat firecrackers."[36]

Only four days earlier, as the Columbia was zipping around the earth at 17,321 miles per hour, the Challenger had crept along at 5 miles per hour on its journey from Palmdale to Edwards. First of the orbiters to be configured for operational missions in its original design, the Challenger was 2000 pounds lighter than its sister ship. Much of the instrumentation and other equipment necessary for research and development flights was absent. Some parts of the Challenger's structure had also been fabricated out of lighter weight materials. The two orbiters also differed in their thermal protection systems. Between missions, the Columbia was in the process of having its heat-shielding tiles replaced with tough, adhering "densified" tiles. The Challenger came configured with densified tiles. Its orbital maneuvering system pods were also already configured with an advanced thermal protection blanket called advanced flexible reusable surface insulation (AFRSI). The blanket consisted of a silica material sandwiched between an upper and lower quilt which was bonded

directly to the orbiter's skin. AFRSI would replace the low-temperature white tiles on all subsequent orbiters.[37]

The Challenger arrived at the Kennedy Space Center on July 6 after an uneventful trip. Ferrying the Shuttle had now become routine. Fitzhugh L. "Fitz" Fulton, the carrier aircraft pilot who had already made more than 30 such flights, commented that it was "just a matter of flying low and slow." The mated craft was flown at an altitude of about 15,000 feet to prevent freezing of the chemicals loaded in the Shuttle. A commercial 747 would normally fly at an altitude of from 39,000 to 45,000 feet. The carrier aircraft's speed, about 380 miles per hour, was about two-thirds that of the commercial aircraft. Although amazed spectators wondered just how the ungainly looking piggyback combination could even get off the ground, let alone fly across the country, to Fitz Fulton it was "no big deal." Veteran of more than 30 years of test piloting at Edwards (first with the Air Force and then with NASA), Fulton modestly concluded that, although "you always know the orbiter is up there, it only represents a slight bit of buffet."[38]

Into Operation: STS-5

Columbia's first operational mission, and the fifth orbital flight, lifted off Pad A of launch complex 39 at Kennedy November 11, 1982. In its debut as a commercial cargo carrier, the Shuttle deployed two communications satellites, a Satellite Business Systems SBS-3 and an Anik C-3, during the first 30 hours of its scheduled five-day mission. The Columbia added to its already impressive list of accomplishments when it became the first space craft to ferry four men into orbit. No more than three people had ever previously blasted away from earth in the same vehicle. On STS-5, the commander, Vance D. Brand, and the pilot, Marine Col. Robert F. Overmyer, were joined by mission specialists Dr. William B. Lenoir and Dr. Joseph Allen, the first

"passengers" Shuttle carried aloft. Among their other duties on this mission, Lenoir and Allen were scheduled to make the first space walk by American astronauts since 1974. Unfortunately the 3½-hour extravehicular activity had to be canceled when the spacesuits malfunctioned. The mission concluded successfully when the orbiter landed on runway 22 at Edwards at 6:33 a.m. on November 16. In contrast to each of the previous landings at the base, this one was witnessed by a relatively small crowd estimated to be in the 50-55,000 range.[39]

The sequence of events for the descent phase of the mission got underway with the deorbit burn during the 81st orbit--while the Shuttle was flying approximately 1200 nautical miles southeast of Mauritius in the central Indian Ocean--as the vehicle was at an orbital altitude of between 148 and 154 nautical miles and an inclination of 28.5 degrees. The 2-minute-and-24-second deorbit burn produced 268.7 feet per second of retrograde velocity. The burn was planned to place the Shuttle at the 400,000 foot entry interface point 4040 miles from Edwards. The entry ground track was quite similar to STS-4 with a crossrange of 579 nautical miles required to reach the runway at Edwards.[40]

The entire descent phase was flown in the automatic mode down to the heading alignment cone. With but one exception, each of the programed test inputs during this phase were also, for the first time, fully automatic. The single exception was a pushover-pullup maneuver manually flown by the commander at Mach 18. Though eight automatic programed test inputs were scheduled, two were inhibited by the crew (at 16 and 22 pounds per square foot dynamic pressure) when the fuel quantity in the right reaction control system tank dropped below the "redline" value. The entry data maneuvers for STS-5 were as follows:[41]

Mach No.	Maneuver
24.5	Aileron, roll jet, yaw jet
21.4	Yaw jet, aileron, elevator

18.0-17.2	Pushover-pullup
5.6	Yaw jet, aileron
4.4	Rudder, aileron
3.3	Rudder, aileron, yaw jet
1.6	Rudder, aileron, yaw jet, elevator

The highest priority maneuver on this flight was the last one at Mach 1.6. This was the region where the Shuttle had encountered low-amplitude (0.25 Hz) oscillations on each of its previous flights. As Lt. Col. Roy D. Bridges, the mission control spacecraft communicator for this mission, explained:[42]

> We are concerned down in the Mach 2 to Mach 1 region because the body flap tends to saturate there and the elevons therefore drive up off their schedule . . . As a result of this, aileron effectiveness is a little less than one would desire . . . Some of the later flights have forward center of gravity setups planned that would tend to force the elevons up even further. This is why we want to understand the stability and control issues as soon as possible; they could have an effect on later flights.

The programed test input, which provided independent and sharp activation of each control effector, permitted OAMV engineers to obtain "high confidence" stability derivatives in the low-Mach region. This data at last provided them with support for a theory concerning the long-standing anomaly:[43]

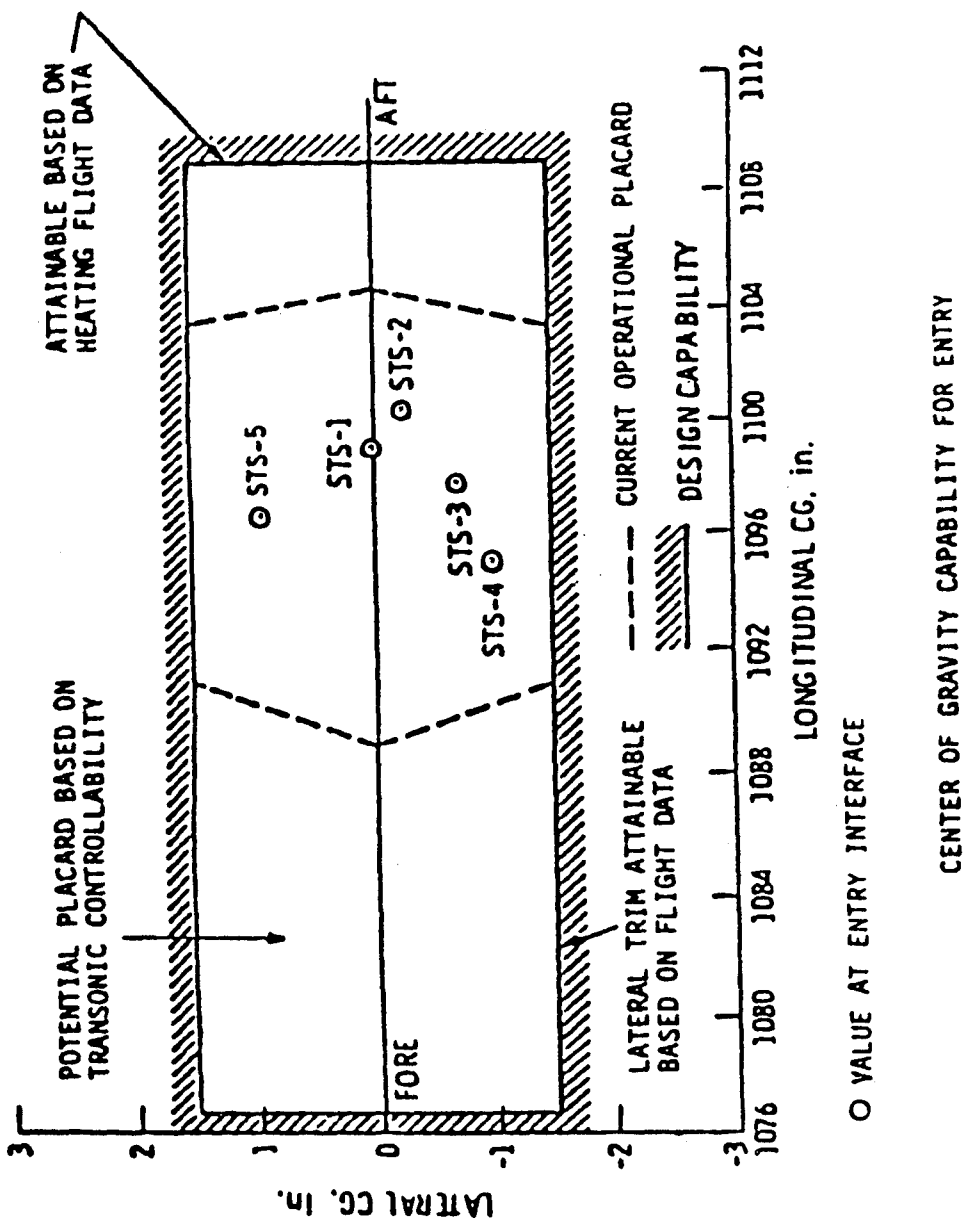
> Preliminary derivatives obtained from the Mach 1.6 PTI were very close to wind tunnel predicted data. Previous AFFTC simulator studies have shown that the 0.25 Hz oscillation cannot be duplicated using wind tunnel predicted data. The AFFTC therefore feels that there is much merit in a recent Rockwell theory which postulates that the speedbrake setting used in flight in this region (55 degrees) causes cyclic shock detachment and reattachment on the vertical fin, which provides the forcing function for the 0.25 Hz oscillation. Strong consideration should be given to investigating the changes in flying qualities by using a different speedbrake deflection angle in the transonic region on

> future flights. The AFFTC also supports the increased roll axis flight control system gain currently planned for STS-8. The unsteady aerodynamics which would be produced by the shock detachment are not modeled by the Modified Maximum Likelihood Estimation (MMLE) derivative extraction program. Derivative results indicative of decreased roll damping . . . were obtained from the Mach 1.6 PTI on this flight. Therefore, increasing the roll axis gain (which produces increased artificial roll damping should reduce or eliminate the 0.25 Hz oscillation. The 0.25 Hz oscillation is not considered a flight safety hazard with the present flight control system since the RCS [reaction control system] jets contain the oscillation. However the RCS jets will probably be deactivated on future flights in the region of the 0.25 Hz oscillation to eliminate the "ZOTS" problem below 70,000 feet. Additional aerodynamic data which provide a full understanding of the 0.25 Hz oscillation is necessary before the RCS jets are deactivated in this region.

The entry on STS-5 was performed with an estimated lateral center of gravity offset of +0.95 inch (based on prelaunch measurements). The Shuttle's lateral center of gravity design capability was 1.5 inches (see Figure 6). An aileron trim value of roughly -0.3 degrees was necessary during most of the entry in order to counteract the center of gravity offset. Since up to three degrees of aileron trim were available to combat lateral trim asymmetries, OAMV engineers believed that the Shuttle would probably be capable of meeting its lateral center of gravity design specification.[44]

The entry heating data on this flight was generally consistent with that from previous flights. Heating on most of the lower surface, for example, continued to be substantially lower than predictions. There were, however, still two significant areas of concern regarding the thermal protection system: the area just beneath the nose cap and the orbital maneuvering system pods. Postflight examination once again revealed serious slumping of the high temperature reusable surface insulation (HRSI) tiles on the lower surface just aft of the nosecap. The slump along the edge

Figure 6


ATTAINABLE BASED ON HEATING FLIGHT DATA
POTENTIAL PLACARD BASED ON TRANSONIC CONTROLLABILITY
LATERAL TRIM ATTAINABLE BASED ON FLIGHT DATA
CURRENT OPERATIONAL PLACARD
DESIGN CAPABILITY
AFT
FORE
STS-1
STS-2
STS-3
STS-4
STS-5
LATERAL CG, in.
3
2
1
0
-1
-2
-3
1076
1080
1084
1088
1092
1096
1100
1104
1108
1112
LONGITUDINAL CG, in.
O VALUE AT ENTRY INTERFACE
CENTER OF GRAVITY CAPABILITY FOR ENTRY

of one tile was three inches long and the gap between it and the adjacent tile was a quarter inch wide. All of the gap filler material was missing down to the inner mold line. Substructure damage included melting of an aluminum panel, removal and charring of approximately ten inches of flow stopper material between the panel and the nose cap's reinforced carbon-carbon expansion ring, and erosion of the underlying insulation bag to a depth of about one inch in the region of flow impact. Tile slumping had occurred in this area on each of the previous two flights but this was the first time damage to the substructure had been detected. OAMV engineers recommended "aggressive pursuit of a fix for this problem prior to the higher heat load missions from Vandenberg AFB." They were also concerned because, for the first time, the temperature on the reinforced carbon-carbon nose cap had come close to the design limit. The Shuttle would encounter higher heat loads during entries from polar orbits and thus this area would also have to be closely monitored and the insulation in this area might well have to undergo modification.[45]

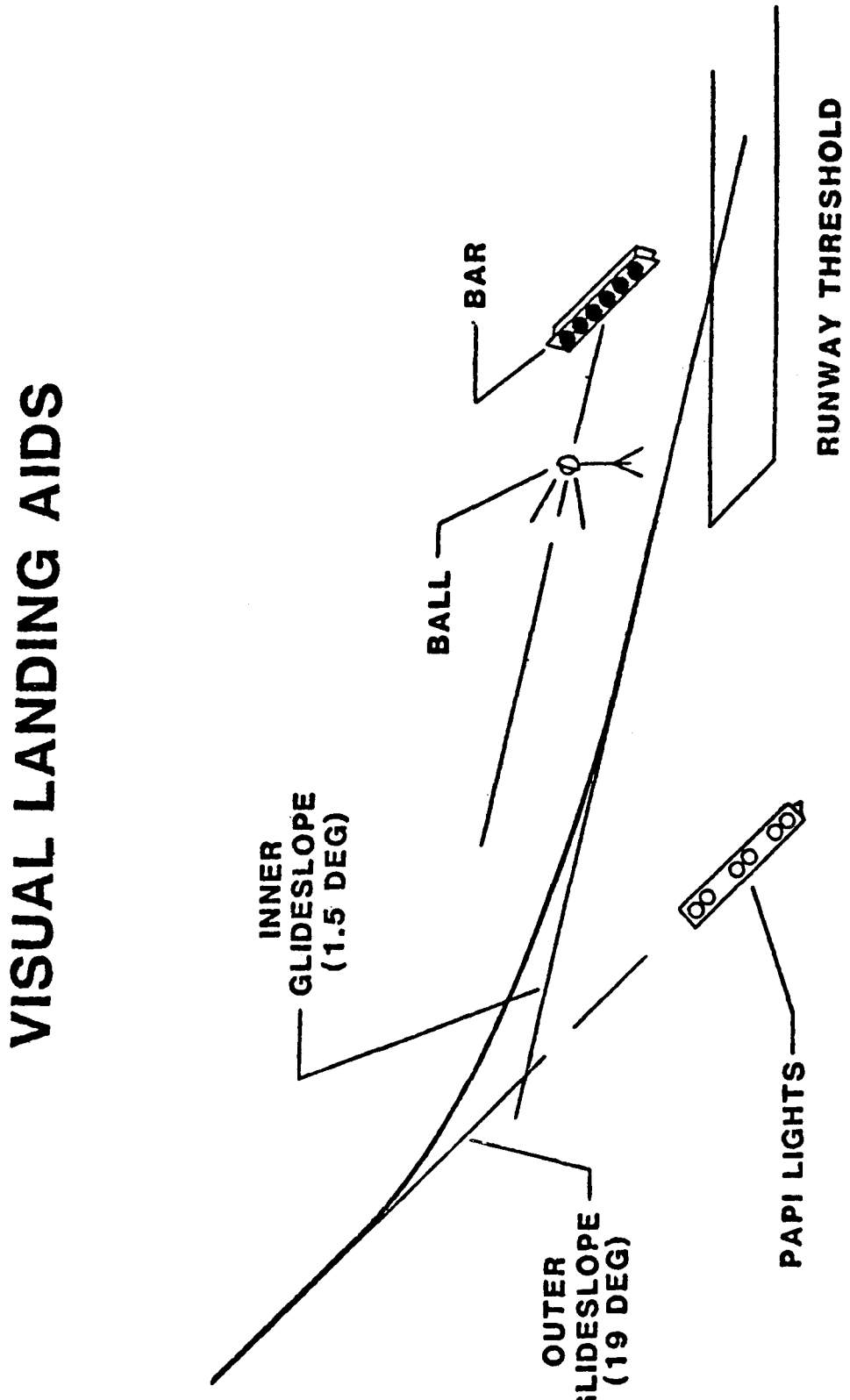
Much concern also continued to exist over the adequacy of the thermal insulation on the orbital maneuvering system (OMS) pods. While bondline temperatures remained well within limits at a peak of 258-degrees F, peak surface temperatures on the flexible reusable surface insulation (FRSI) were 900-degrees F--some 200-degrees F over the design limit. Data from the pushover-pullup maneuver on this flight once again indicated that increased heating on the pods from vortex impingement occurred at a higher angle-of-attack than predicted (36 degrees rather than the 30 degrees predicted by wind tunnel tests). Substantial work needed to be done on the OMS pod area before the Shuttle could be cleared for the lower entry angles-of-attack which would be required for some DoD payloads.[46]

The planned autoland test through touchdown had to be canceled because heavy rains had flooded the lakebed, necessitating a

landing on Edwards' concrete runway. An autoland attempt was out of the question because azimuth errors from the microwave scanning beam landing system had consistently driven the orbiter approximately 50 feet off course on final approach during previous landings (as, indeed, it did on this one). In fact, as it turned out, the autoland was not used at all through the approach and landing sequence. Brand, the commander, took over control of the pitch and roll/yaw axes at Mach 0.88 and 42,500 feet above ground level prior to rolling onto the heading alignment cone and flew the rest of the approach and landing manually. Taking control so early in the profile permitted him adequate time (approximately 3 1/2 minutes) to evaluate and familiarize himself with the orbiter's handling qualities prior to the critical landing phase. He found this very helpful and recommended that future commanders be given a similar option. A cloud cover over the Antelope Valley prevented visual contact with the runway until the orbiter dropped below 14,200 feet above ground level and thus Brand was forced to follow his instruments around the heading alignment cone and onto the final approach. At that point he visually acquired the precision approach path indicator (PAPI) lights (see Figure 7) and transitioned to a visual approach, relying heavily on airspeed and altitude calls from Overmyer. With only minor excursions from the desired airspeed of 285 KEAS, airspeed control was considered quite good throughout the final approach.[47]

Columbia's airspeed peaked at 299 KEAS during the preflare and the commander pulled 1.5 g's while visually aligning the vehicle on the final shallow glideslope with a new ball and bar system. This system was a visual landing aid, originally designed for night landings, which was expected to counter some of the height perception problems experienced on previous flights. Located 50 feet from the left edge of the runway near the approach end, it consisted of a single white source of light (the ball) 13 feet above the ground, and 500 feet beyond, a bar of 6 red lights resting 3 feet above the ground. When the crew visually aligned

Figure 7


VISUAL LANDING AIDS
BAR
BALL
RUNWAY THRESHOLD
INNER GLIDESLOPE (1.5 DEG)
PAPI LIGHTS
OUTER GLIDESLOPE (19 DEG)

the ball with the bar, they were flying on the proper 1.5 degree shallow glideslope (see Figure 7). Brand felt that the ball-bar system was "very valuable in performing the landing smoothly" and recommended its use for future flights.[48]

Despite his favorable assessment of the ball-bar system, Brand touched the Shuttle down 1731 feet past the threshold, still somewhat short of the predicted touchdown point (though closer than any of the previous landings). He commented that he thought the orbiter was still about 5 feet above the ground as it touched down. The nosegear touched down 4679 feet beyond the threshold at a speed of 166 KEAS and the vehicle came to a stop after a total rollout distance of 9430 feet. A successful maximum braking test was finally accomplished on this landing. Maximum braking was maintained for 10 seconds, as the Shuttle decelerated from 124 to 80 KEAS. Shortly after this test, however, a broken brake stator caused the left inboard wheel to lock up, causing the tire to skid the last 50 feet. Physical evidence indicated that the brakes had experienced energy levels in excess of their one-stop limit. This, along with other brake and tire damage from both STS-4 and -5, convinced the OAMV that the Shuttle would not be ready for "routine" landings on concrete runways until the brake system had been more thoroughly evaluated.[49]

The landing had taken place at 6:33 a.m. By 9:20 a.m., the base was already back to normal operation. This was, by far, the shortest turnaround time to date and a clear indication of just how "routine" Shuttle landings had become by the first operational flight.[50]

OAMV Assessment of the Shuttle Program

By any standard of measurement, the Space Shuttle program had been tremendously successful. The magnitude of the accomplishment can be placed in perspective when one recalls that it had only been a little over 30 years since the X-1 had first penetrated the

fabled sonic "wall." Men were now flying an operational winged vehicle at speeds in excess of Mach 24. In the short interval between X-1 and the Space Shuttle, American technology had made a virtual quantum leap. The mysteries of hypersonic flight, aerothermodynamics, and lifting reentry had been fathomed and reasonably mastered. After more than a decade of design and development, the Shuttle had successfully flown its first operational mission after only four test flights. Testing on the vehicle was, of course, by no means complete. But major milestones had been passed.

As they reviewed the Shuttle program after STS-5, OAMV engineers had cause to be pleased: traditional flight testing techniques had been successfully applied to an orbiting space vehicle and new aerothermodynamic flight testing methods had been devised and ably demonstrated in an environment which had been hitherto largely uncharted. By this means, a number of design prediction tools--such as for lifting reentry vehicles and reusable thermal protection systems--had been successfully verified. The detection and evaluation of those areas where the design tools proved to be in error was an even more important accomplishment. This was where flight testing became such a critical element in the development process. For it was only in actual flight that all of all unknowns could be encountered, evaluated, and at last, controlled. OAMV engineers had been confronted with a substantial number of these unknowns during the course of the Shuttle's test program.[51]

Thus, for example, although the Shuttle design had been subjected to the most extensive wind tunnel evaluation in aerospace history, a significant number of mispredictions had been encountered. On STS-1 a large, slow, lateral-directional oscillation had occurred during the orbiter's first bank maneuver. A momentary sideslip angle of over 4 degrees was reached during the oscillation (compared to the 1-degree prediction). The source

of the oscillation was determined to be a misprediction of the effects of yaw jet interaction during the initial phase of entry. The jet interaction effects in that low dynamic pressure environment were much smaller than predicted. As Robert Hoey, chief of the OAMV, explained: "This prediction discrepancy was again a result of inadequate ground test facilities to accurately duplicate the simultaneous high Mach number, low density, rocket-firing environment."[52]

Although the ratio of lift to drag was well predicted over most of the Mach range, the magnitude of the normal force coefficient (the primary contributor to both lift and drag at high angles-of-attack) was overpredicted. The causes of the discrepancy were still not understood but abrupt changes in measured acceleration (up to 19 percent over a period of 1 second), changes which did not correspond to flow changes over the vehicle, indicated that the Shuttle could well be encountering changes in atmospheric density. If these changes were the cause of the anomaly, and if they proved to be random in nature, they would become a very important design consideration for future vehicles. Unfortunately, efforts to more fully understand the phenomenon were hampered by the fact that current onboard measurements of dynamic pressure at high-Mach numbers were not accurate enough to provide any conclusive answers.[53]

Significant discrepancies in pitch trim predictions had also been observed on each of the flights. Thus, for example, on STS-1 the trim bodyflap was at 16 degrees rather than the predicted 7 degrees at Mach numbers greater than 17. Analysis of the bodyflap sweeps and the pitch pulse performed during the next Shuttle reentry established that the major contributor to the trim error was a mistake in the basic pitch curve rather than an error in elevator or bodyflap effectiveness. OAMV engineers concluded that the mispredicted longitudinal trim at hypersonic Mach numbers was due to an error in basic pitching moment. This error for Mach

numbers above 16 varied between 0.026 and 0.029 (averaging 0.0275). This flight-to-flight difference corresponded to an uncertainty of +2.0 inches in the Shuttle's longitudinal center of gravity. The primary cause of the deviation from the predicted basic pitching moment was thought to be "real gas effects." Real gas effects are the aerodynamic consequences of deviations between "real air" thermodynamic properties and an ideal gas with constant specific heat--the difference between what the Shuttle encountered in actual flight and what could be simulated in the wind tunnel. Real gas effects were most significant between 270,000 and 150,000 feet (Mach 8 and above) and recent analytical studies had indicated that they could produce a pitch-up increment of .024 above Mach 18. This was very close to the difference between the predicted results and actual flight. This was yet another example of the fact that all of the variables of the flight environment could not yet be predicted by relying solely on wind tunnels and simulators. If actual flight test data had been available during the design phase, the Shuttle's nose ramp angle could have been reduced slightly, bringing the bodyflap and elevon back to a more desirable faired position. This would have resulted in lower temperatures on both the nose ramp area and the control surfaces.[54]

The flight tests also produced valuable data on aerothermodynamics which would inform simulation studies for future vehicle designs. Test results, for example, consistently revealed a heating environment on the Shuttle's lower surface that was less severe than predicted. Three factors were isolated which combined to produce this situation. First, the laminar heating during the early portion of the entry was less than predicted, especially on the forward portion of the Shuttle. Next, the transition from laminar to turbulent flow occurred later in the entry than expected, producing lower temperatures and a lower total heat load. Finally, after the Shuttle passed through Mach 2.5 (80,000 feet), vent doors opened on the side of the

fuselage in order to equalize the pressure in the payload bay and other internal compartments. The flow of cold air into the vehicle and over its surface had not been included in the conservative heating models used in the design process. This atmospheric cooling was quite significant and caused internal structural temperatures to peak earlier and at lower levels than predicted. OAMV engineers concluded that this last item could prove to be very useful in the design of future reentry vehicles, as Hoey explained:[55]

> The atmospheric cooling effect has been, and should be, highly repeatable. The next generation of entry vehicles might well be equipped, not only with vent doors, but with air scoops and internal baffling to effectively utilize the 3 to 5 minutes of free cooling provided by the atmosphere while descending below 80,000 feet.

These represented only a few of the valuable discoveries produced by the Shuttle's flight test program. Members of the OAMV staff had just cause to be proud. For, in a sense, they were every bit as much pioneers as were their predecessors during the proud days of the X-1. Just like their predecessors, they were testing the current limits of knowledge and then taking that next step beyond--a step which provided tantalizing glimpses of what was yet to be.

NOTES

1. Material for this chapter is drawn from a variety of sources, particularly memos and reports of the Office of Advanced Manned Vehicles, AFFTC. The authors wish to thank Robert Hoey and Johnny Armstrong of the office for their assistance and inputs.

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*For a more detailed discussion of this effort to expand the Center's mission responsibilities, see Chapter I of this volume.

3. Ltr., Maj. Gen. Philip J. Conley, Jr., Commander, AFFTC, to HQ AFSC/MPM, Andrews AFB, subj: Flight Testing Military Space Vehicles, 11 Jun 82.

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5. AFFTC Technical Letter Report, Quick Look Evaluation of the Space Shuttle Oribiter Third Orbital Flight: Descent Phase, OAMV, May 82.

6. Ibid.

7. Ibid.

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19. AFFTC Technical Letter Report, Quick Look Evaluation of the Space Shuttle Orbiter Fourth Orbital Flight: Descent Phase, Sep 82; "A Fourth of July to Remember," Edwards AFB Desert Wings, 9 Jul 82.

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26. Ibid.

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33. "A Fourth of July to Remember."

34. "No Mention Made of a 5th Shuttle;" "500,000 Cheer Return of Shuttle."

35. "500,000 Cheer Return of Shuttle."

36. "A Fourth of July to Remember."

37. NASA Facts, Differences Between Challenger and Columbia, Hugh L. Dryden Flight Research Center, Edwards AFB, CA, Jul 82.

38. As quoted in "Piggyback Shuttle Just Routine Now," Los Angeles Times, 5 Jul 82; also based on several discussions between Fitz Fulton and Dr. James Young, AFFTC History Office, 1982-3.

39. AFFTC Technical Letter Report, Quick Look Evaluation of the Space Shuttle Orbiter Fifth Orbital Flight: Descent Phase, OAMV,

Dec 82; "Columbia Returns Here in 'Routine' Fashion," Edwards AFB Desert Wings, 19 Nov 82.

40. Quick Look Evaluation of the Space Shuttle Orbiter Fifth Orbital Flight, Dec 82.

41. Ibid.

42. As quoted in "Columbia Reentry Tests Low-Mach Characteristics," Aviation Week & Space Technology, 22 Nov 82.

43. Quick Look Evaluation of the Space Shuttle Orbiter Fifth Orbital Flight, Dec 82.

44. Ibid.

45. Ibid.

46. Ibid.

47. Ibid.

48. Ibid.

49. Ibid.; Briefing, Approach and Landing Characteristics of the Space Shuttle Orbiter, Capt. Cynthia A. Bourne, OAMB, 3 Mar 83.

50. Briefing, Lt. Col. Ray Houle, Deputy Commander, 6510 ABG, to 6510 ABG Staff, 17 Nov 82.

51. Office Memo, Robert G. Hoey, Chief, OAMV, AFFTC Overview of Orbiter Reentry Flight Test Results (first five flights), Mar 83. The OAMV put together a series of briefings after the fifth flight which covered each of the test disciplines. These assessments summarized the status of the program at that point. In addition to the overview provided by Hoey, these included: Flight Test Derived Heating Math Models for Critical Locations on the Orbiter During Reentry, by Elam K. Hertzler and Paul W. Phillips; Approach and Landing Characteristics of the Space Shuttle Orbiter, by Capt. Cynthia A. Bourne and Paul W. Kirsten; and Predicted and Flight Test Results of the Performance, Stability and Control of the Space Shuttle from Reentry to Landing, by Paul W. Kirsten, David F. Richardson, and Lt. Charles M. Wilson.

52. Hoey office memo.

53. Ibid.

54. Ibid.; Office Memo, Predicted and Flight Test Results of the Performance, Stability and Control of the Space Shuttle from Reentry to Landing, OAMV, Mar 83.

55. Hoey office memo.

CHAPTER IV

SHUTTLE: THE FIRST YEAR OF OPERATIONS

The conclusion of the Shuttle's formal test program with the July 4, 1982 landing marked the ostensible beginning of operational missions. In fact, as with any new developing system, many of these operational missions incorporated research and development objectives, and the Office of Advanced Manned Vehicles at Edwards continued to be an active participant in postflight analysis during this time. Orbiter vehicles landed at Edwards on six of the seven flights flown during the first year of operations, and OAMV's engineers continued to collect test data in preparation for a final technical report covering the descent and landing phases of the first twelve missions. Though most flight test instrumentation had been removed from Columbia following STS-5, sufficient instrumentation remained to provide some of the information researchers subsequently desired from Shuttle operational experience.[1]

As Shuttle went operational, the AFFTC prepared to provide two different modes of landing support. Edwards would remain the primary landing site for all first flights of new or modified orbiters and for those missions where the orbiters would be returning significant payloads back to earth. For these missions, the Center would provide the same kind of "full-up" support that it had provided during the first five flights. For most missions, however, it would serve as a back-up landing site for the Kennedy Space Center or (hopefully) Vandenberg AFB. For these flights it would provide "stand-by" support in the event of bad weather at either of the primary landing sites. In this mode, the Center

would have to be capable of providing a full-up response on just three hours' notice. Starting with STS-6, the bulk of the missions scheduled for this reporting period were supposed to land at the Kennedy Space Center in Florida. Most of these missions, however, had to be diverted to Edwards because of poor weather conditions at the Cape. On STS-13, in fact, the Center's ability to provide a full-up response on extremely short notice was put to the test. Adverse weather, which quickly developed at the Cape in the pre-dawn hours of April 13, 1984, resulted in a decision to divert the Challenger to Edwards only eight minutes before the crew would have lost communications preceding a reentry burn that would have committed the Shuttle to a Kennedy Space Center landing. Approximately 350 Edwards personnel, many of them living up to 50 miles from the base, had to be contacted through the recall network and they were onsite, ready and waiting, when the orbiter touched down just 121 minutes after NASA had made the decision to divert.[2]

Contingency landings made it very difficult for the Center to project the budgetary requirements to support Shuttle operations. The projected total and reimbursable costs for fiscal years 1983-85 were:[3]

Fiscal Year	Total Costs	Reimb* Costs
1983	$1,258,100	$1,258,100
1984	$1,439,200	$ 775,400
1985	$1,834,300	$1,016,200

Short notice landings and shifting budgetary requirements were not the only problems with which the Center had to contend. After STS-6, NASA announced that "the requirement for airborne aided egress is deleted from STS-7 and subsequent missions at all CONUS

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*These were the projections as of July 5, 1983. They did not include the costs for contingency landings.

landing sites."[4] NASA program managers had achieved "a high degree of confidence in landing the orbiter vehicle on a designated runway." As a consequence, nothing more than normal search and rescue (SAR) support would be required at Edwards and White Sands Space Harbor (formally Northrup Strip). This meant that only those areas which were readily accessible to ground units could be covered.[5]

Officials at the Flight Test Center had serious reservations about this decision. Col. Michael D. Hall, Commander of the 6510th Test Wing, informed Department of Defense Shuttle support officials that:[6]

> Although we agree that the Mode VII concept grew cumbersome and costly as the size of the orbiter crew grew, we do not feel that six (nor sixty) successful landings is sufficient justification to eliminate the entire capability to attempt a rescue should the orbiter go down away from the runway.
>
> AFFTC feels a legal and moral obligation to be prepared to attempt to rescue a Shuttle crew from any accident that might occur within the confines of EAFB. Normal SAR support is not sufficient due to the toxic materials on board which create serious hazards for the rescue team. The immense publicity which would be created by such an event is also a consideration and we must prepare a response plan regardless of the probabilities involved.

Center officials implemented a new response mode, "similar to the current Mode VI, but adding a capability to airlift shuttle-trained firemen and appropriate rescue equipment to any crash site on EAFB which is not immediately accessible by the fire trucks." Colonel Hall concluded that:[7]

> We will be working toward the preparation of a long term plan to provide this capability at EAFB using local resources if at all possible for all future Shuttle flights. The goal is to provide trained personnel and special equipment to attempt astronaut aided egress off the runway within the confines of EAFB but at a lower level of cost and complexity than before.

In August of 1983, Center officials were faced with a new and unique set of circumstances as STS-8, the first night landing, approached. As Col. Peter C. Hoag, the Center's Vice Commander, explained to base personnel:[8]

> There exists an immense security problem created by the night landing here on our very open facility, and hence the possibility of unauthorized persons approaching the Shuttle or the Shuttle runway undetected in the darkness. Therefore, very special security measures must be taken to protect government property and personnel.

These special measures included:[9]

At 1800 the evening before the landing (approximately 61/2 hours prior to touchdown), all roads around the east and south sides of Rogers Dry Lake (i.e., Rich Road, Mercury Blvd., Ave. B, and 140th St.) were closed, evacuated, and controlled until the Orbiter was in the NASA compound (approximately 40 minutes after touchdown).

At 2300, the entire flight line was closed and evacuated except for Shuttle mission-essential personnel and vehicles, security personnel, and emergency vehicles.

Approximately 125 members of the 6510th Security Police Squadron, supplemented by 40 Army and National Guard personnel, established a secure perimeter around the lakebed.

The night landing of Challenger on September 5 came off without a hitch. Security was extremely tight, and in contrast to previous landings, only 3000 witnesses were on hand to view the orbiter as it rolled past the powerful xenon-arc lights at 12:40 a.m.[10]

The night landing was only one of several firsts recorded by the Space Shuttles during this reporting period, as Table I shows:[11]

TABLE I

Mission	Dates	Events and Comments
STS-6	4-9 Apr 83	First flight of Challenger (OV-099); first use of a modified 40-degree angle-of-attack schedule; first use of a head-up dis play; first mission diverted to Edwards AFB; failure of Advanced Flexible Reusable Surface Insula tion (AFRSI) on orbital maneuvering system (OMS) pods; brake failure.
STS-7	18-24 Jun 83	Dr. Sally K. Ride became the first American woman in space; OV-099 diverted to Edwards AFB; brake failure.
STS-8	30 Aug-5 Sep 83	First night launch and landing; brake damage on OV-099; brake failure.
STS-9	28 Nov-8 Dec 83	First European Spacelab mission; Columbia (OV-102) orbited for ten days, completing the program's longest mission to date; general purpose computer failure delayed landing for eight hours; in-flight fire approximately three minutes before landing caused by hydrazine leaks in two of the vehicle's auxiliary power units; brake failure.
STS-11	3-11 Feb 84	OV-099 became the first space shuttle to land at the Kennedy Space Center in Florida; brake failure.
STS-13	6-13 Apr 84	First short-notice diversion to Edwards AFB; brake failure.
STS-14	30 Aug-5 Sep 84	First flight of Discovery (OV-103); brake failure; last mission with significant involvement of the Flight Test Center's Office of Advanced Manned Vehicles.

The landing of STS-14 marked the tenth time, in twelve flights, that an orbiter vehicle had touched down at Edwards. Though each of the Shuttle's landings had been successful, they had not been without problems.[12]

The most persistent problem had been brake failures. The landing gear brakes had suffered from a variety of internal component failures, ranging from minor pad chipping to broken rotors and stators, on every landing except for STS-2 (see Figures 1a, 1b, and 1c). Flight test engineers encountered a number of difficulties in their attempts to evaluate the brake system. For one thing, nine different pilots flew the orbiters during these missions and thus it was difficult just to define an acceptable consensus on the operation of the brake system. They were also impeded by a lack of adequate test instrumentation, long lead times to incorporate modifications, and the lack of high fidelity dynamometer test facility. These problems were compounded by the limited number of landings (only twelve in three years) and even more limited opportunities to collect relevant data. Seven of the first twelve landings occurred on dry lakebed surfaces where significant braking was not required. Only a small number of specific braking tests, in fact, had even been attempted. After the first three flights had proven the basic landing concepts, light-to-moderate braking had been employed in order to achieve an approximate 8-foot/second/second deceleration rate during rollout and to demonstrate the vehicle's differential braking capability. STS-5 was the only landing where maximum braking--for a duration of 10 seconds as the vehicle decelerated from 124 knots to 80 knots--had been employed. Under these limiting circumstances, engineers found that they had to rely on postflight brake assembly teardown analyses as the primary basis for their evaluation.[13]

Four basic failure modes were eventually identified. Most of them occurred in combination, but in some cases, one failure was more severe than others. The modes identified were: (1) stator,

STS Brake Performance Data Summary Chart

FLIGHT NO. VEHICLE NO.	GROSS WT AT MLG TOUCHDOWN lbs	AIRSPEED AT MLG TOUCHDOWN KEAS	GROUNDSPEED AT BRAKES ON KTS	TOUCHDOWN SINK RATE AT MLG TOUCHDOWN fps	RUNWAY	WIND CONDITION kts	RUNWAY ROLLOUT DISTANCE ft	BRAKE ENERGY*			
								LOB	LIB	RIB	ROB
								MILLIONS OF FOOT POUNDS			
STS-1 OV-102	195,472	182	105	1.4	Lakebed 23	2 tail	8993	12.38	14.23	20.85	4.02
STS-2 OV-102	204,262	193	109	2.1	Lakebed 23	16 head 7 cross	7711	12.82	18.27	13.62	9.71
STS-3 OV-102	207,072	220	149	5.6	Lakedbed 17 (Northrup)	12 head 3 cross	13737	12.3	19.9	27.8	21.1
STS-4 OV-102	208,946	199	133	0.8	Concrete 22	12 head 3 cross	9878	22.8	28.4	29.1	23.7
STS-5 OV-102	202,480	199	167	1.0	Concrete 22	calm	9553	37.4	42.6	32.4	29.8
STS-6 OV-099	189,730	190	136	1.7	Concrete 22	15 head 8 cross	7180	34.9	32.3	16.2	13.8
STS-7 OV-099	204,043	206	125	1.0	Lakebed 15	9 head 5 cross	10450	2.88	1.96	13.64	8.26
STS-8 OV-099	203,945	194	154	1.2	Concrete 22	5 head 2 cross	9371	36.6	34.0	29.6	25.7
STS-9 OV-102	220,027	184	125	1.0	Lakebed 17	3 tail	8456	13.75	18.24	14.08	11.87
STS-11 OV-099	201,238	193	129	2.0	Concrete 15 (KSC)	calm	10807	27.6	22.4	27.2	19.1
STS-13 OV-099	196,976	213	104	0.9	Lakebed 17	calm	8716	14.40	12.20	15.30	11.80
STS-14 OV-103	202,270	207	91	<1.0	Lakebed 17	2 head 2 cross	10285	11.0	12.6	7.7	5.6

*As calculated by NASA

Figure 1a

Figure 1b

FLIGHT NO. VEHICLE NO.	CONFIGURATION CHANGES	OPERATIONAL COMMENTS*	BRAKE ASSEMBLY DAMAGE				ENGINEERING/TEARDOWN ANALYSIS
			LOS	LTS	RTS	ROS	
STS-1 OV-102	New wheel and axle configuration.	Light braking.	none	none	One rotor drive slot bent.	none	none
STS-2 OV-102	-	Light braking.	none	none	none	none	none
STS-3 OV-102	-	Moderate braking. Highest sink rate at touchdown to date, 5.6 fps.	none	Wheel scraping on stator O.D.	Rotor drive slot wear. Scraping on rotor I.D. Some BeO formation.	Crushed rotor drive slots. Scraping of rotor I.D.	Axle flexing determined to be the cause of rotor/ stator scraping.
STS-4 OV-102	-	Moderate braking. Pilot complains of poor "feel" in brake pedal.	none	Wheel scraping on stator O.D. Some BeC formation.	Scraped rotor I.D. BeC formation.	none	None additional.
STS-5 OV-102	-	Maximum braking from 124 kts to 80 kts then light braking to stop.	Cracks in 3 rotors. BeC on all 4 rotors. Rotor scraping on I.D. Over-heated actuators	Wheel locked up last 50 feet of roll. Broken stator. Actuator exceeded 325°F limit. BeC formation. Cracks in all four rotors. Rotor scraping on I.D.	BeC on all rotors, most stators. Light rotor I.D. scrape. No cracks. Over-heated actuators.	BeC on all rotors and stators. Rotor I.D. Scrapes. Over-heated actuators.	Failures attributed to high energy combined with long duration total brake application time resulting in high sock temperatures.
STS-6 OV-099	Added 118° "saddle" between axle and brake torque tube to lessen axle flexing.	Light-to-moderated braking. 8 kts crosswind necessitating differential braking, more on left side than right.	none	none	Two cracked stators.	none	Cracked stators caused by interference fit of spacers in beryllium stators during manufacture

STS Brake Component Failure Summary Chart

STS Brake Component Failure Summary Chart

FLIGHT NO. VEHICLE NO.	CONFIGURATION CHANGES	OPERATIONAL COMMENTS*	BRAKE ASSEMBLY DAMAGE				ENGINEERING/TEARDOWN ANALYSIS
			LOB	LIB	RIB	ROB	
STS-7 OV-099	Added Rotor and stator drive clips to eliminate drive lug cracking.	Light braking.	none	none	Two rotors broken. Broken carbon pads, pad retaining washers.	Carbon pads crushed. Loose segments, washer missing. Drive slot and drive clip damage.	Failure of pad retaining washer was primary suspect.
STS-8 OV-099		Light-to-moderate braking.	Some BeC	Some BeC	Missing washer, damaged carbon pads drive lugs and clips.	Same as RIB.	Same as STS-7. In addition, Hydraulic system instability became suspect.
STS-9 OV-102		Moderate braking.	none	none	none	Missing washers, all pads damaged or missing. Drive lug damage.	Same as STS-7 and -8 NASA convened Ad Hoc NASA/Industry Committee to identify problem. Numerous recommendations were made but no single cause identified.
STS-11 OV-099	Removed brakes before towing.	Light-to-moderate braking.	none	none	none	Missing washers, damaged pads and drive clips.	None specific.
STS-13 OV-099	Removed brakes before towing.	Light braking.	Rotor damage, carbon pad and washer damage. Bent rotor and stator drive clips.	Same as LOB	Same as LOB	Broken rotors, pad and washer damage. Bent stator and rotor drive clips.	Series of "fixes" given to Buchwell to implement. See text.
STS-14 OV-103	Removed brakes before towing. Added 360° saddle in OB brakes.	Light braking	none	Several broken washers, several bent clips.	none	One broken washers.	360° saddle inconclusive because some damage sustained with or without saddle. Next flight with all 4 brakes modified with 360° saddle on OV-099.

*Light Braking 200-700PSI
Moderate braking 700-1000PSI
Heavy braking 1000-1500PSI

** BeC - the formation of Beryllium Carbide on the rotor and/or stator due to heat concentration. BeC results in loss of strength of the parent material.

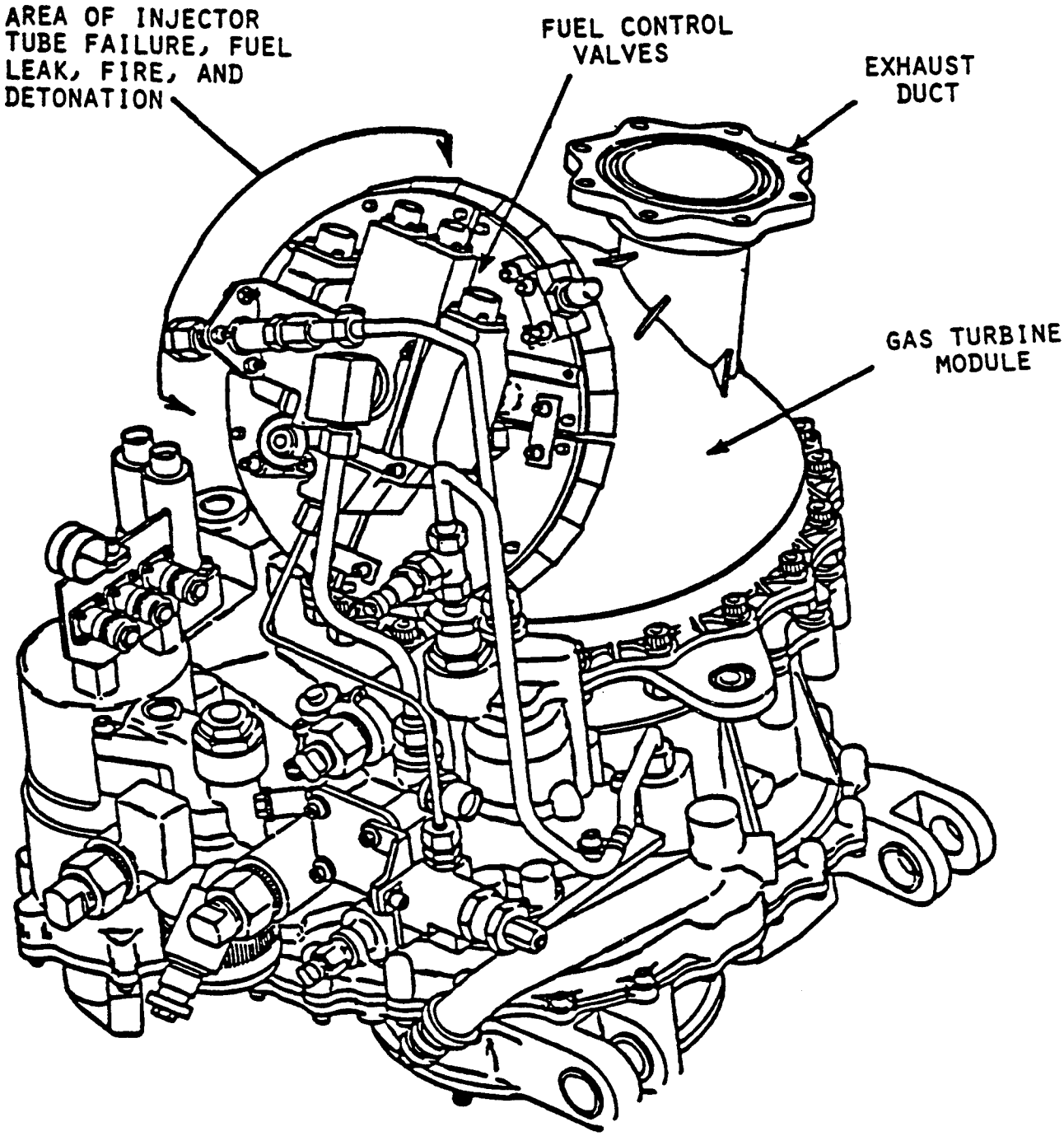
Figure 1c

rotor, and wheel rotating contact, (2) stator and/or rotor failure, (3) stator and rotor drive clip and slot damage, and (4) carbon pad failure. For the reasons stated above, however, the causes of these failure modes were not well understood and thus the brake system was (and would continue to be) a key developmental problem.[14]

One other potentially serious problem occurred at the end of STS-9. Immediately after Columbia's wheels came to a stop, observers noted pulsating flames from the No. 1 and No. 2 auxiliary power unit exhaust duct outlets at the base of the vertical stabilizer. Seven minutes later, the No. 1 unit shut down automatically due to a turbine underspeed condition. About four minutes later, the No. 2 unit experienced very similar occurrences. The No. 3 unit continued to operate normally throughout postflight operations. Postflight examination revealed evidence of fire caused by hydrazine leaking from the first two units. The fuel valve modules and fuel pump of both units exhibited physical external damage consistent with a detonation (i.e., ruptured case and broken fluid lines that connected to the valves and fuel pumps). The area of damage on both units is depicted in Figure 2.[15]

NASA formed an investigative team to determine the cause of the failures. Analysis of auxiliary power unit instrumentation indicated that flames erupted about 41/2 minutes prior to touchdown. This would have put the Shuttle at an altitude of 51,170 feet above ground level. Although leakage had been occurring prior to that time, the minimum amount of oxygen necessary to sustain combustion was probably not available until the Shuttle had reached this altitude (the sequence of events is depicted in Figure 3). The fire damaged electrical wiring and heated up the area of the auxiliary power unit fuel valve modules. The damaged wiring caused each of the two unit controllers to signal an

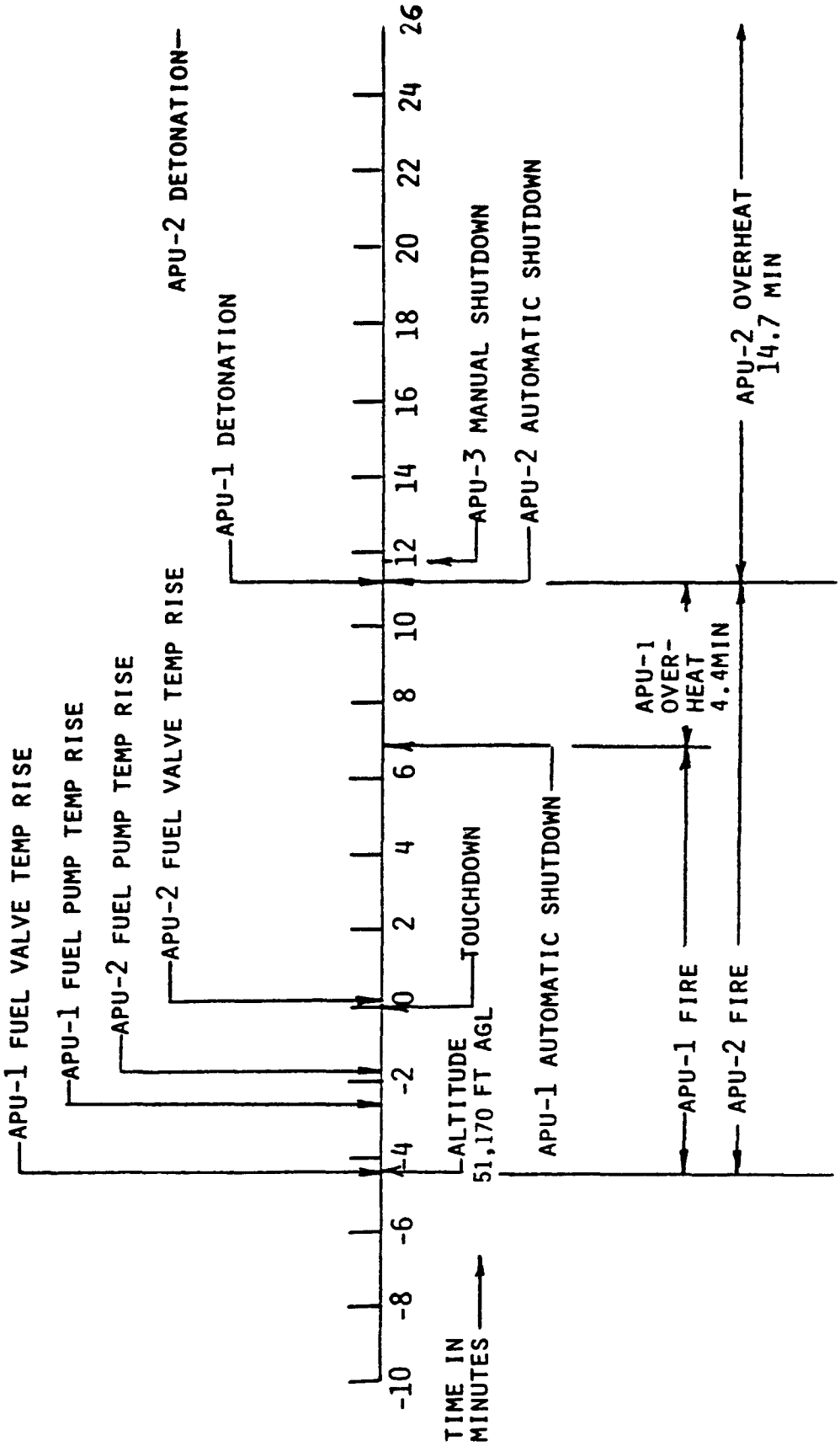
Figure 2



STS Auxiliary Power Unit Assembly

Figure 3

STS-9 Auxiliary Power Unit Failure Time History



automatic underspeed shutdown. When the hydrazine which was trapped inside the fuel valve modules reached 450 degrees F, it detonated, causing extensive damage to each of the modules and their associated hardware. The fires were ultimately extinguished when all the fuel was consumed. The pulsating flame observed in the exhaust ducts at the time of the landing was caused by the ignition of oil that had leaked from the damaged fuel valve modules into the hot exhaust.[16]

The investigation revealed that the leaks were caused by the failure of hydrazine injector tubes in each of the auxiliary power units. These tubes were Hastelloy-B machined structures which connected the valve assembly outlet to the gas generator inlet. Cracks in both tubes were discovered which had been caused by stress corrosion. Three simultaneous conditions, the investigative team reported, had to be present to cause the cracks.[17]

1. High surface tensile stresses on the tube walls: This was caused by a manufacturing misalignment and installation forces between the tube and the mating O-ring bore.

2. Degraded microstructure of the Hastelloy-8 material: This resulted from carbon contamination occurring during electro-discharge machining of the tube's inside diameter and by the lack of adequate quenching after the brazing cycle.

3. Corrosive environment: This was caused by the "weeping" of hydrazine across a valve seat during the long storage period between flights and the subsequent decomposition of the hydrazine into ammonia.

In order to permit continued Shuttle operations, "high-time" auxiliary power units were removed for reworking, a nitrogen purge between system runs was incorporated, and the tube fitting was reworked in order to relieve most of the assembly stresses. Longer term activities including defining the useful life of the existing configuration, altering the injector's manufacturing

process, and a continuing effort to improve the materials, design, and the system's environment between flights.[18]

Although, as reported in the previous chapter, reentry temperatures on the Shuttle's lower surface had actually proven to be lower than predicted, peak temperatures on some areas of the upper surface had exceeded predictions. Higher-than-predicted heating on the orbital maneuvering system (OMS) pods and the side of the fuselage had resulted in at least minor thermal protection system damage on all of the early flights. This caused substantial concern because the Shuttle would have to be able to fly at significantly lower angles-of-attack if it was going to achieve its design crossrange capability of 1100 nautical miles. At lower angles-of-attack, during the hypersonic reentry phases, the vehicle's upper surface would have to sustain even higher temperatures.[19] Figure 4 shows a summary of orbiter entry conditions for STS-1 to STS-14.

The first flight of Challenger, STS-6, provided the opportunity to evaluate a new "advanced flexible reusable surface insulation" (AFRSI) which had been bonded to the OMS pods. These pods were subject to vortex impingement from the juncture of the wing-glove and the fuselage which had been detected at higher angles-of-attack (approximately 37 degrees) than wind tunnel predictions had indicated (approximately 30 degrees). The new surface insulation failed during the descent phase of STS-6. Preliminary analysis suggested that the failure was the result of both aerothermodynamic heating and aerodynamic loading. In late May of 1983, wind tunnel tests were conducted at the NASA Ames Research Facility in an effort to pin down the exact causes of the failure. As Robert G. Hoey, Chief of the Office of Advanced Manned Vehicles, reported:[20]

> The AFRSI test articles were mounted on a 26 x 29-inch 2-dimensional curved surface to simulate the curvature of the OMS pod. The test conditions consisted

Summary of Orbiter Entry Conditions

Figure 4

FLIGHT	INCLINATION	DEORBIT ALTITUDE	DEORBIT ORBIT NUMBER	ENTRY INTERFACE CONDITIONS				VELOCITY AT FIRST BANK REVERSAL
				RELATIVE FLIGHTPATH ANGLE	RELATIVE VELOCITY	DOWNRANGE	CROSSRANGE	
	(deg)	(nm)		(deg)	(fps)	(nm)	(nm)	(fps)
STS-1	40.3	150	36	-1.248	24563	4386	179 DR	18389
STS-2	38.0	137	36	-1.227	24515	4481	54 DR	20289
STS-3	38.0	149	129	-1.173	24455	4130	276 AR	17773
STS-4	28.5	161	112	-1.448	24439	3803	581 DL	11032
STS-5	28.5	160	81	-1.270	24397	4042	579 DL	11354
STS-6	28.5	155	80	-1.313	24395	4064	381 AL	15826
STS-7	28.5	160	97	-1.319	24406	4035	738 DL	7123
STS-8	28.5	160	97	-1.189	24286	4044	512 DL	12893
STS-9	57.0	135	166	-1.105	24857	4345	69 DR	21642
STS-11	28.5	165	127	-1.266	24387	4137	524 DL	12888
STS-13	28.5	272	107	-1.601	24641	4082	381 DL	16190
STS-14	28.5	160	96	-1.339	24416	4119	474 DL	14005

NOTES:

DR = descending orbit (toward descending node), right turn

DL = descending orbit (toward descending node), left turn

AR = ascending orbit, (away from ascending node), right turn

AL = ascending orbit, (away from ascending node), left turn

> of both ascent and entry dynamic pressure conditions. An AFRSI material failure was experienced during the first run simulating ascent conditions. The material began to vibrate at approximately 410 psf dynamic pressure, with material failure beginning at 450 psf. The outer material layer failed at the stitching which led to loss of the outer layer between the 1-inch square stitching. The inner batting material then slowly separated. A 9 x 13-inch section of material damage/ loss occurred that was similar in appearance to the damage observed after STS-6. The wind tunnel test duration was significantly longer than the period of high dynamic pressure during the actual ascent, potentially explaining why the damage did not occur during ascent on STS-6 (although non-observable weakening of the material could have occurred). Two runs were performed to attempt to simulate entry conditions. A vortex generator was installed for one run to produce vortex flow on the test article. No material failure occurred during either of the entry tests. Since the material failed during simulated ascent conditions (did not fail on STS-6 ascent) and did not fail on simulated descent conditions (did fail on STS-6 descent), the wind tunnel simulation of the OMS pod environment is judged to be inadequate. A test was also performed on an AFRSI test article coated with Ludox under ascent loading conditions. The material began to show signs of failure at approximately 450 psf dynamic pressure. Although these tests are important to understand the AFRSI failure modes and to evaluate the predicted material performance at other critical locations on OV-103 and OV-104, it is felt that the leading edges of the OMS pods should be protected with tiles of high temperature capability. This solution would not only solve the AFRSI failure problem, but would provide additional capability for lower angle-of-attack, high-crossrange entries.

Even before the wind tunnel evaluation got underway, NASA had begun to back away from verifying the Shuttle's high-crossrange design capabilities. In early May of 1983, Hoey reported:[21]

> The planned review of the USAF change request (PCIN) on Orbiter crossrange verification at the April 22 NASA PRCB meeting was deleted from the agenda. NASA is beginning to formulate a strong position on deleting the crossrange verification in favor of using additional emergency landing sites to cover launch aborts. The NASA STS program manager has advised NASA headquarters (letter to Gen. Abrahamson dated April 20) that JSC [Johnson

> Space Center] is actively pursuing this course of action and he could expect recommendations for specific landing sites within a month or two. NASA's emerging course of action was, in part, listed as: (1) baseline 40-deg. angle-of-attack as the entry profile and cease further effort on crossrange verification and (2) proceed with negotiations for landing sites such as Tahiti, Easter Island, and Moses Lake as abort sites. NASA views this approach as an opportunity to reduce requirements, reduce risk, save money, and technically simplify operational planning. Although this approach is probably technically feasible, its attractiveness may diminish when all factors are adequately staffed and compared to achieving the inherent capability and flexibility associated with the basic Orbiter design crossrange.

The original design of the orbiters was based, in part, on a Department of Defense requirement that it be able to perform a once-around polar mission from Vandenberg AFB. This requirement was later relaxed to specify only an intact abort to the continental United States for polar missions which permitted some of the Shuttle's re-use design constraints to be relaxed. Both of these requirements, however, would require an entry crossrange capability of 1122 nautical miles in order to permit landings at Vandenberg for all possible launch azimuths. The Shuttle's high-crossrange capability was originally to have been demonstrated on the fourth--and last--flight of the orbital flight test program (STS-4). However, additional wind tunnel testing and the results of the first two Shuttle flights raised strong concerns about upper-surface temperatures and a programmatic decision was made after STS-2 to fly only 40-degree angle-of-attack trajectories during the test program. In September of 1981, the Air Force had requested that additional high temperature thermal protection be installed on Columbia's orbital maneuvering system pods so that crossrange envelope expansion could proceed. This request was rejected by NASA. NASA did agree, however, to retain some of the thermal protection system instrumentation on Columbia beyond STS-4 by installing a modular auxiliary data system on the vehicle.[22]

As the initial flight test data were evaluated, it became quite apparent that the thermal protection system on the orbital maneuvering system pods, and possibly other upper-surface areas, was not adequate. As a result, advanced flexible reusable surface insulation was scheduled to replace the existing low temperature reusable surface insulation tiles on most of the upper surface of Discovery and Atlantis prior to their first flights. By the fall of 1982, a situation had developed whereby Columbia was scheduled to have the proper thermal instrumentation but the wrong thermal protection system, while Discovery would have the improved thermal protection capability but no instrumentation.[23]

NASA had already decided to use Columbia for the first Spacelab mission (STS-9), thus delaying its modification for possible crossrange verification. In fact, it appeared that the vehicle would not be ready for such activity until STS-29 which was scheduled for June of 1985. Thus it became obvious that the verification could not be completed prior to Atlantis' scheduled first flight from Vandenberg AFB in October of 1985.* Under the circumstances, the Air Force requested, in October of 1982, that the crossrange capability be verified, either by installing limited instrumentation on Discovery or by reconnecting the thermal protection sensors on Columbia and upgrading its upper surface insulation. NASA rejected the idea of instrumenting Discovery as too costly and time consuming. NASA agreed to proceed with the original verification plan using Columbia, but it still would not commit to the required thermal protection system modifications to the vehicle.[24]

Finally, as Hoey reported, NASA began to consider the use of emergency landing sites in the South Pacific to cover Vandenberg AFB launch aborts. This proposal drew a strong reaction from the Office of Advanced Manned Vehicles. Hoey itemized some of the

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*Which, of course, never took place.

negative aspects of locating such sites outside of the continental United States:[25]

> Possible launch constraints due to unacceptable weather at the emergency landing sites.
>
> The costs of runway construction or improvements outside of the continental Unites States.
>
> The possible political and environmental issues that might attend runway construction outside of the United States.
>
> The additional cost of in-place support equipment -- such as TACAN, PAPI lights, etc. -- and its maintenance.
>
> The costs and logistical problems of transporting personnel and equipment to and from the sites for both standby and actual recovery operations.
>
> Schedule delays caused by the extensive travel time required to return the shuttles to the United States.
>
> Additional deployment of search and rescue forces.
>
> The many potential problems surrounding Department of Defense payload security, removal and return to the United States.
>
> The heavy reliance on the one-of-a-kind shuttle carrier aircraft or the procurement and modification of a second Boeing 747.
>
> Aerial refueling capability for the 747 shuttle carrier aircraft would have to be implemented.

The Office of Advanced Manned Vehicles argued that the high-crossrange capability which was designed into the Shuttle would avoid these issues and also offer an additional measure of operational flexibility. Despite these arguments, NASA proceeded with its downrange island concept for ascent aborts.[26]

After the failure of the advanced flexible reusable surface insulation on STS-6 in April of 1983, the Office of Advanced Manned Vehicles recommended some changes. A conservative

solution, it suggested, would be to replace the advanced flexible reusable surface insulation (which had a 1,500-degree capability) with the high-temperature reusable surface insulation tiles (which had a 2,400-degree capability) that had already proven their ability to handle any temperatures that might be encountered on the orbital maneuvering system pods. This suggestion, however, was not followed. Instead, Challenger's OMS pods were returned to the original Columbia configuration (low-temperature reusable surface insulation tiles for the forward 17 inches and felt reusable surface insulation from 17 inches aft). Discovery's OMS pod configuration was changed from all advanced flexible reusable surface insulation to the low-temperature tiles for the forward 39.7 inches with the advanced flexible material covering the remaining area. The Office of Advanced Manned Vehicles recommended, in June of 1983, that the area covered by the low-temperature tiles should be extended back to 80.4 inches aft. This change, it argued, would have removed all concerns for thermal protection on the pods during high-crossrange missions. NASA did not implement this recommendation.[27]

Ultimately, a "C-9" coating was applied to all of advanced flexible reusable surface insulation blanks on Discovery prior to its first flight (STS-14). With this coating, the blankets were capable of withstanding temperatures up to 1,800 degrees. Postflight inspection of these coated blankets revealed minimal degradation. NASA decided to modify the OMS pods on each of the orbiters to a configuration identical to that on the Challenger. As the Office of Advanced Manned Vehicles reported.[28]

> The TPS [thermal protection system] configuration was the basic OV-103 configuration which included 2-inch thick LRSI [low-temperature reusable surface insulation] tiles on the front, extending approximately 23 inches further aft than the OV-102/099 [Columbia/Challenger] configuration, and thicker AFRSI [advanced flexible reusable surface insulation] blankets (C-9 coated) replacing the FRSI [felt reusable surface insulation]

> behind the LRSI [low-temperature reusable surface insulation] tiles. The final configuration was to include FRCI [fibrous refractory composite insulation] tiles (21 per side) in the debris impact area on the front of the OMS pods (LRSI TPS area) and 3-inch thick LRSI tiles on the aft lower portion of the OMS pods (replacing thinner black tiles).

These changes were judged "adequate to accomplish high-crossrange entries as originally designed." However, the Shuttle's high-crossrange capability remained unverified up to the loss of Challenger in 1986.[29]

There was one other significant area concerning the descent and landing phase which likewise remained largely unevaluated by the time of Challenger's demise: sufficient data had yet to be collected on the Shuttle's crosswind landing capability. The failure to collect sufficient data in this area stemmed from a pair of factors. NASA was reluctant to employ nosewheel steering on a regular basis because of uncertainties in the non-redundant system, the potentially disastrous effects of a "hardover" nosewheel, and the fact that limited testing had indicated that nosewheel steering might be too sensitive above 80-knot groundspeeds. For this reason, differential braking had been the primary means of steering control during ground rollouts. During the rollout on STS-6, however, Challenger encountered an 8-knot crosswind (the highest of any of the first 12 flights) and each of the left-hand brakes (which were upwind) approached their reuse limit of 36 million foot-pounds of energy while the right-hand brakes experienced less than half of that amount. Predicted energy levels for the same condition had been 18 million foot-pounds per brake on the upwind side. These high energy values raised serious doubts concerning the brakes' ability to provide steering and braking in a 20-knot crosswind. In addition to the crosswinds, a number of other variables were discovered which contributed to side forces on the vehicle. Until the true extent of these forces was better understood and higher crosswind values

had been demonstrated on a lakebed runway, NASA decided to impose the following crosswind limits: 8 knots at the Kennedy Space Center, 12 knots on Edwards' main concrete runway, and 15 knots on Rogers Dry Lakebed runways. Since continued evaluation of this capability depended on infrequent landings and the vicissitudes of weather, the Office of Advanced Manned Vehicles predicted that it would be some time before the Shuttle's crosswind landing capability could be completely defined.[30] Figure 5 shows a summary of orbiter approach and landing conditions for STS-1 to STS-14.

In conjunction with its proposal to use south Pacific islands for once-around aborts, NASA launched a space shuttle carrier aircraft inflight refueling proximity test program in February of 1984. The original objectives of the program were to: determine the performance and handling qualities of the mated 747 carrier aircraft and orbiter in close proximity to a tanker aircraft (KC-135 and KC-10); determine tanker and refueling boom handling qualities in close proximity to the mated carrier aircraft and orbiter; determine tanker handling qualities in close proximity to the mated carrier aircraft and orbiter while simulating reverse aerial refueling (tanker behind the carrier aircraft); determine the impact on the carrier aircraft's fatigue life as a result of mated carrier aircraft and orbiter aerial refueling behind a tanker aircraft, and determine the effects on orbiter health of the mated carrier aircraft and orbiter refueling behind a tanker aircraft.[31]

Only two flights were completed in this program -- one with a KC-135 and another with a KC-10. Neither of them involved the mated carrier aircraft and orbiter combination. Though NASA suspended the progrm after these two flights, they did demonstrate that proximity tests with the mated combination could be conducted in a safe manner.[32]

STS	SELECTED STEEP GLIDESLOPE ANGLE, DEG	SPEEDBRAKE RETRACT ALTITUDE, FT AGL	PREFLARE INITIATION ALTITUDE, FT AGL	PREFLARE INITIATION AIRSPEED, KEAS	AVERAGE ACCELERATION IN PREFLARE, g	AIRSPEED AT GEAR EXTENSION, KEAS	AIRSPEED AT GEAR DOWN, KEAS	MLG HEIGHT AT GEAR DOWN AND LOCKED, FT AGL	THRESHOLD CROSSING AIRSPEED, KEAS
1	19	2920	1526	300	1.36	271	243	63	251
2	19	ON HAC	1870	265	1.20	264	245	162	201
3	19	4000	1998	298	1.34	263	238	32	238
4	19	2600	1728	297	1.29	292	261	76	213
5	19	2500	1417	297	1.32	286	258	95	223
6	19	2500	2111	289	1.26	278	256	129	205
7	19	2500	1969	288	1.30	285	258	92	233
8	19	2500	2021	301	1.33	293	276	127	227
9	17	2500	2187	286	1.23	263	239	79	200
11	19	2500	1732	285	1.22	269	247	83	217
13	19	2825	1887	297	1.32	294	270	103	235
14	19	4000	1975	293	1.24	290	269	97	232

Summary of Approach and Landing Parameters

Figure 5 (page 1 of 2)

STS	LANDING WEIGHT, POUNDS	MLG TOUCHDOWN AIRSPEED, KEAS	MLG TOUCHDOWN SINKRATE, FPS	MLG TOUCHDOWN ANGLE OF ATTACK, DEG	MLG TOUCHDOWN AIRSPEED, KEAS	MLG TOUCHDOWN PITCH RATE, DEG/SEC	MLG TOUCHDOWN DISTANCE FROM THRESHOLD, FT	MLG TOUCHDOWN DISTANCE FROM THRESHOLD, FT	TOTAL ROLLOUT FT
1	195,470	182	1.4	9.5	149	5.0	6059	9158	8993
2	204,260	193	<1.0	7.9	143	4.3	780	3429	7711
3	207,070	220	5.6	6.7	178	7.4	1092	7276	13732
4	208,950	199	0.8	7.9	165	4.7	948	4988	9878
5	202,480	199	<1.0	7.8	165	3.5	1731	4679	9430
6	189,730	194	1.7	8.2	157	3.2	2026	4972	7180
7	204,040	206	<1.0	7.5	155	4.8	2726	6843	10450
8	203,940	194	1.2	7.6	165	4.2	2793	5515	9371
9	220,030	184	1.0	9.9	128	8.5	1649	5897	8456
11	201,240	193	2.0	7.5	150	2.4	1930	5789	10807
13	196,980	214	0.9	5.9	142	3.9	1912	7167	8716
14	202,270	207	<1.0	8.6	154	4.8	2505	6713	10285

Summary of Approach and Landing Parameters

Figure 5 (page 2 of 2)

With the successful landing of Discovery after STS-14, the Center's office of Advanced Manned Vehicles concluded its full-time involvement in the Space Shuttle program. It would continue to support all landings made at Edwards and it would participate, in a much more limited fashion, in a variety of ongoing Shuttle test efforts. The office also recommended that Columbia should be employed as a testbed for further reentry and hypersonic technology research.[33]

With the anticipated decline in Shuttle's own developmental research after STS-14, OAMV turned its attention to other subjects, notably "crossover" technologies and the upcoming Flight Dynamics Laboratory's Boost Glide Vehicle (BGV) program. At the time, no one could have foreseen the tragic loss of the Challenger in January 1986 that would shake the foundations of the Shuttle program, bring renewed attention on how Shuttle had been conceived and developed, and unfortunately, bring an equal measure of uninformed and irresponsible commentary (much of it from the national media and the science and technology reporting community) questioning the need and rationale for the program, and the result of Shuttle's developmental effort.[34] In one of the ironies so plentiful in history, the loss of Challenger coincided with the strong reemergence of an American presence in space and interest in spaceflight, as well as a growing resurgence of interest in hypersonic studies, as measured by the NASA Space Station program, the Strategic Defense Initiative (SDI), the National Aero-Space Plane program, FDL's Boost-Glide Vehicle program, and a general reawakening of interest in hypersonic studies at American colleges and institutions. But such interest was not limited to the United States alone; abroad, the European nations, the Soviet Union, and Japan all had hopes and expectations for new classes of manned hypersonic launch vehicles and orbital craft of their own. Shuttle would not fly again until 1988--but when it did, it would, in all likelihood, not be the only hypersonic space transport system in existence.

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34. With all the hyperbole that surrounded the Shuttle program following the Challenger tragedy, it is well to note an excellent and thoughtful little book written by a noted New Yorker science writer, Henry S. F. Cooper. Entitled Before Lift-off: The Making of a Space Shuttle Crew (Baltimore: The Johns Hopkins University Press, 1987), it takes the reader on an incisive tour of NASA as the crew of Mission 41-G prepare for an eight-day mission flown in October 1984. It offers a fascinating and always interesting perspective on the pre-Challenger tragedy Shuttle program in much the same way that Cooper's earlier 13: The Flight That Failed illuminated the Apollo program.

EPILOGUE

YESTERDAY, TODAY, AND TOMORROW:

FROM SHUTTLE TO THE NATIONAL AERO-SPACE PLANE

by

Richard P. Hallion

EPILOGUE

YESTERDAY, TODAY, AND TOMORROW:

FROM SHUTTLE TO THE NATIONAL AERO-SPACE PLANE

The loss of the Space Shuttle Challenger and its crew in January 1986 unleashed a torrent of criticism of the National Aeronautics and Space Administration in general and the Shuttle program in particular. Critics ranged from thoughtful individuals horrified at what soon became apparent obvious management and technology failures within the Shuttle development and operations process, to neo-Luddites merely dusting off an anti-technological bias dating to the pre-Apollo era, and others merely using Challenger's loss as an opportunity to attack what they perceived as the increasing militarization of the space program, or the notion of manned vs. unmanned space exploration. A commission chaired by former Apollo 11 mission commander Neil A. Armstrong and appointed by President Ronald Reagan undertook a thorough and comprehensive examination of the accident, singling out the now-infamous defective field joint in the solid rocket booster (SRB) as the culprit but also faulting NASA and industry management. Critical warnings, issued as early as January 1979, had been minimized and even ignored, triggering an inexorable chain of events culminating in disaster.[1] While thoughtful criticism characterized much of the Shuttle discussion in the days and weeks following the loss of Challenger, many other critics were far less temperate. A traditional science vs. technology conflict was rekindled by the accident, with many scientists arguing that intrinsic fascination with manned spaceflight had crippled research in space science and exploration, such as space-based solar physics and astronomy, which could be undertaken by unmanned

systems. One, physicist James A. Van Allen (discoverer of the Van Allen radiation belts), proposed to "suspend manned flight indefinitely pending critical assessment of its justification," as well as postponing development of NASA's planned orbiting space station.[2] Political scientist John Logsdon, a well-known analyst of the national space program, argued in the same issue of Science that Van Allen's diatribe appeared, that:[3]

> The Shuttle is likely to be used only for those missions for which it is particularly qualified, and any notion that it can ever be operated routinely or cheaply has been abandoned. Thus, while the Space Shuttle is an impressive technological achievement and gives to the United States capabilities for manned operations in space that no other country possesses, the Shuttle program must be assessed as a policy failure, at least in terms of meeting the objectives that have been its articulated rationale since 1972. (Emphasis added).

Logsdon's analysis of Shuttle as a policy decision based on unrealistic assumptions about its cost effectiveness challenged the economic rationale for the program put forth by NASA during the formative period of Shuttle development. Many other critics of Shuttle made the same charge, but unlike Logsdon, failed to point out that Shuttle offered the United States abilities in space unavailable to other nations--an important point in Shuttle's favor. Logsdon himself was careful to emphasize this interpretation during a subsequent interview with Space World, stating that:[4]

> I think the Shuttle provides a superb technological capability, and that it can and will be made to fly safely. But it also clearly is neither routine nor inexpensive, nor can it serve effectively as a launch vehicle for every conceivable type of payload.

Most of Shuttle's media and science critics, however, were far less insightful and temperate.

Lessons (Painfully) Learned

Undoubtedly, with hindsight, Shuttle offers a cautionary tale for developers of future lifting reentry space logistical vehicles. Logsdon's essential point--the flawed economic models for Shuttle--is an important one to keep in mind, as is the general criticism of Shuttle's overall annual mission model. (In economic terms, Shuttle flight costs were underestimated by 475%.) In the early days of Shuttle development, NASA expected as many as fifty flights per year; the agency subsequently cut this to twenty-four, and present wisdom suggests that after required changes are made to the Shuttle in light of the Challenger tragedy that a figure of less than ten annually is likely. The weakness of putting all one's payload eggs into one booster basket--using the Shuttle as a replacement for expendable throwaway systems--was brutally highlighted by the accident, and has triggered a long-overdue reemphasis upon companion development of advanced "complementary" expendable launch systems including the so-called Titan IV system. (It should be added as a historical note that the progression of the American space program had always previously involved going from one technology level to the next, from less complex to more complex, never looking back, and always replacing earlier systems with subsequent ones. Retrospect indicates that there are times when it might be quite appropriate to continue to stay with a proven level of technology for a variety of reasons, in addition to pressing towards the frontier. Thus, for example, it could have greatly benefitted the United States to have retained the Gemini-Titan combination for a variety of reasons, in addition to going forth with Apollo-Saturn. Likewise, development of Shuttle should not have involved an "either Shuttle or expendable booster" rationale; rather, the space program needed--and needs today--both. Another fact brought forceably into view with the loss of Challenger was any notion that Shuttle was merely some sort of an advanced airplane. Early in the Shuttle program, some proponents had argued that Shuttle

could be developed to require little more than Concorde-type special handling.) (Similar arguments have been advanced for proposed successors, as well.) The astronauts clearly recognized the fundamental weaknesses of this approach, particularly as it applied to crew selection. Shuttle operations, in fact, bear more resemblance to the operations of the X-15 in terms of maintenance and down-time between flights than they do to any other aircraft system--a point to ponder for future hypersonic space-capable launch systems.[5] The reevaluation of Shuttle's anticipated launch schedule after it returns to space was one factor cited in the subsequent decision by the Air Force to cancel the planned Shuttle Operations Planning Complex (SOPC) at Colorado Springs in early 1987.[6]

Shuttle today is grounded, awaiting modifications before again venturing into space. At a briefing before the Society of Experimental Test Pilots in September 1986, astronaut Vance Brand offered a detailed summation of the state of Shuttle redesign. The first and most obvious change, of course, was redesign and recertification of the flawed SRB field joint that had led to the loss of Challenger. But there were other changes being examined as well, including bands around the rocket case to prevent case expansion during firing, use of heaters to keep the O-rings warm while on the pad, improvements to insulation at the field joints, and efforts to reduce throat erosion in the rocket nozzle. The main engines, always a serious safety concern,* are likewise being upgraded, including modifications to the high-pressure oxidizer pump and high-pressure fuel pump (including more rigorous

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*I think it is fair to state that when most individuals familiar with the Shuttle first heard of Challenger's accident, they immediately suspected the SSME's - specifically the turbopumps - of triggering the loss, particularly since it came after clearance to "throttle up" for the boost into space. Few (if any) suspected the "reliable" and "safe" SRBs of triggering the disaster.

selection, insulating, hardening, and contouring of pump turbine blades), and redesign of valves in the propellant feed lines. Shuttle's landing headaches have led to redesign and strengthening of wheel rims, redesign of brakes, and software changes to the nosewheel steering system to cope with more demanding crosswind and tire-failure situations. The flight control system is being modified to permit emergency reentries using only aerodynamic control via the elevons and rudder without yaw jet inputs: a major change, since, at present, Shuttle is dependent upon using yaw jet thrusters down to Mach 1. New launch abort procedures are being examined, including a so-called "Split S" abort from an altitude of 190,000 feet should an emergency arise during launch. Crew escape systems utilizing tractor-rocket extraction systems during stable descending flight are under study, as are a wide range of Shuttle systems of lesser significance. As Brand stated before the Society, echoing comments from other knowledgeable program observers,[7]

> Everyone now realizes that the Space Shuttle will never be operational in the same sense that an airliner or a fighter aircraft is operational. The Space Shuttle will always require operational attention and concern appropriate to its level of complexity. In other words, our vehicle always will require special care and feeding.

In August 1985, the Advanced Programs Office of the NASA Lyndon B. Johnson Space Center undertook a review of the Shuttle program with a view to furnishing "lessons learned" from the Shuttle experience. The review included the preparation of detailed "white papers" on various aspects of the Shuttle system, and since the review was completed before the launch of Challenger, none of the papers reflected the bitter lessons learned from that particular event. Nevertheless, they offer a unique and detailed survey of Shuttle's experience up to Challenger's loss. The summary and conclusions of this study are extracted and presented below:[8]

SIGNIFICANT LESSONS

Preliminary Engineering

Shuttle experience has shown that small amounts of money invested early in technology development substantially reduce overall development costs. Testing and simulation with early designs, as done with the life support and communications systems, greatly reduce technology uncertainties and thereby limit development cost. Future programs should plan for a substantative definition phase, early testing of key technologies, and the use of the Shuttle or Space Station as a test bed. Every effort must be made to complete preliminary design work and supporting engineering analysis before proceeding with the detailed design and building of hardware.

Operations Cost

For future vehicles, an operations scenario and cost model should be developed early in the program. Life-cycle costs should be weighed more heavily in all design trade studies. The temptation to minimize development cost at the expense of operating costs must be avoided.

Servicing of a reusable spacecraft is an operation pioneered with the Space Shuttle. Although payload integration is a complex operation, the primary schedule driver in Space Shuttle turnaround is Orbiter servicing, maintenance, and modification. Experienced ground operations engineers should have an early input in the design of future vehicles so that efficient ground processing features are built into the system. Vehicles which require little ground support or servicing and are not sensitive to weather conditions would be ideal from the ground operations point of view.

Sparing requirements should be developed during program definition. The spare parts inventory for the Space Shuttle was kept to a minimum to reduce costs. The penalty for this is the need to share parts among the Orbiter fleet which increases ground operations costs and could result in flight delays and remanufacturing expense when parts are not available.

Growth Potential

Providing adequate design margins is a challenge in any engineering project and the Space Shuttle is no exception. Areas for potential growth should be identified and sufficient margins and upgrade capability

should be built into critical systems. Shuttle experience in the following three areas should be of particular interest to designers of future vehicles.

The electrical power requirements for the Orbiter grew to 150 percent of the original estimate and the power generation system had to be redesigned to meet this load.

The Orbiter weight grew during development and the landing gear and brake system was not sized to handle such an increase. The result is a brake system which is marginal for some missions and requires more refurbishment than expected.

The exceedence of computer capacity is a universal problem. In the case of the Shuttle there was a tremendous increase in the volume of flight software especially in the backup flight system. The computers were full from the beginning so that later when additional computer applications were identified there was no room for expansion.

Simplicity in Design

There is a high cost for analysis and development of an unsymmetric, multibody vehicle such as the Space Shuttle. This is due to the complex aerothermal flow field around and between the bodies and significant aerodynamic loads on the wings during ascent. Also, non-axial thrust alignment complicates structural loading. In the definition of future launch vehicles, unsymmetric configurations should be avoided.

The degree to which software development and verification adds to operational cost and complexity was unanticipated in the Shuttle program. Software maintenance and verification implications should be considered in selecting redundancy management schemes and software should be modularized as much as possible to minimize the amount of integrated verification that is required. Vehicles should be designed to fly throughout a broad environmental envelope so that standard trajectories and flight software can be used. When possible, ground support and flight computers should use common software.

Mission operations for future systems could be greatly simplified by keeping software and data processing for the transport vehicle and payload completely separate. In fact, all payload interfaces should be standardized and the interaction between payloads and transport vehicles should be minimized. Payloads should be

designed to function in the normal launch vehicle environment without special provisions or procedures.

The Shuttle program has had great success in recovering, refurbishing, and reusing the solid rocket boosters. However, SRB processing is a large contributor to Shuttle operations expense. The booster design is complex, with many components to be disassembled and reassembled during refurbishment. In the design of future reusable systems, simplicity should be a primary goal.

The experience gained in development and operation of the Space Shuttle could be summed up in three basis lessons, which are:

- Do a good job in preliminary engineering
- Keep it simple
- Incorporate proven technology

Although these lessons may appear self-evident, it will require careful planning and discipline to apply them in the development of future space transportation systems.

SYNOPSES OF WHITE PAPERS

The content of each white paper is briefly summarized below. The number in parentheses refers to the white papers which are included in their entirety in the appendix. [Readers are encouraged to consult the full text of this document in order to become familiar with the specific papers].

Program Management

Program and Costs (1)

Relatively small amounts of money spent on technology early in the program saved substantial amounts in the development phase.

Actual schedule achievements generally reach approximately 70% of planned objectives. Historical accomplishment ratios (work accomplished/work planned) for the Shuttle program were as follows:

External Tank	61%
Solid Rocket Booster	78%
Main Engine	65%
Orbiter	68%

Program design and development costs can be estimated within a reasonable accuracy (15-20%), but operations

costs are much more difficult to estimate. Space Shuttle costs per flight were underestimated by 475% due primarily to lack of definition and understanding of the operations program.

Management and Budget (2)

Programs with long definition phases have proven to have less cost and schedule overruns. All technical uncertainties should be resolved before building hardware and programs should be completed quickly to minimize cost.

Accurate budgets have a stabilizing effect on a program. There are strong incentives for low cost estimates at the beginning of a program and so NASA should rely more on internal cost analysis and less on contractor estimates.

Integrated Vehicle

Aerodynamics (3)

Future vehicles should be designed with adequate performance margins to permit the use of standard or automatically adaptive flight software and the program must follow through in this approach if low operating cost is a goal. Also, in the design of reusable launch vehicles, ascent and entry load factors must be incorporated in the initial design. Aerodynamic and structural analysis cycles should be coordinated to ensure that the best possible data are used in the final design. The technique of defining aerodynamic uncertainties and using this data in the design process was effective in the Shuttle program and should be used in the future.

Some design features which should be considered for future Orbiter-type vehicles are: wing tip fins instead of a vertical tail, a canard to improve handling qualities, streamlining or eliminating the OMS pod, increasing the rollout angle of attack to reduce landing loads, and an air data system which is usable for the whole flight regime.

Based on the Shuttle experience, more attention is needed in the analysis of rocket engine plumes, vortex effects, and the flow fields between complex bodies like the Orbiter and external tank. Aerodynamic predictions generally agreed very closely with flight measurements but there were inaccuracies in predictions of hypersonic real gas and viscous pitching moment increments, local

venting pressures, transonic wing rock, RCS jet interference, and ascent wing pressure distributions. More and better calibrated sensors should be installed for future flight programs.

Computational fluid dynamics techniques should be applied wherever possible. More attention to scaling analysis is needed to avoid inaccurate predictions from scale model tests as occurred with the SRB ignition overpressure analysis.

Aerothermodynamics - Ascent and Entry (4)

The Shuttle Program provided a challenge to the aerothermodynamicist due to the complexity of the flowfield around the vehicle during ascent and the varied geometry of the Orbiter at different angles of attack during entry. Computational fluid dynamics techniques can reduce costly wind tunnel programs in future design efforts.

Predictions of Shuttle heating rates were quite accurate with the use of wind tunnel data except in regions of vortex scrubbing. These regions will require flowfield development or extensive flight data analysis.

The value of recovering boosters should be reexamined in light of the impacts associated with meeting the thermal and structural requirements for booster recovery.

Aerodynamic and Aerothermal Testing (5)

Methods must be improved for predicting flight characteristics at hypersonic speeds and high angles of attack and for predicting plume effects at high altitude and high Mach numbers. The effects of SRB and main engine plumes on the Orbiter wing were not predicted accurately and adequate methods to model these effects still do not exist.

The Orbiter structure was baselined before final definition of the aerodynamic loads resulting in wing flight placards. An active load alleviation system should be developed for future winged launched vehicles.

Inconsistent pricing policies for the use of NASA and DOD wind tunnel facilities resulted in the inefficient use of national resources.

Avionics Integration and Verification Test (6)

Vehicle complexity factors should be identified and formally controlled throughout a program to prevent the growth of verification and operations costs, this is especially true in the design of payload accommodations and interfaces. The way that the Shuttle is designed and operated requires a large scale, on-going verification effort which is very costly.

Lifetime operating costs should be considered in comparisons between highly reliable single components and multiple redundant components. Also, management should define the acceptable risk for non-critical systems, otherwise, engineers will strive for maximum reliability for all systems.

It is more cost-effective to provide adequate test and verification facilities from the beginning rather than adding on later. Any requirements for secure operations should be defined when facilities are first planned.

Flight Software (7)

Rigorous configuration control, error detection, and cross-checking is needed early in the program when cost and quality leverage is highest. Independent verification can greatly increase product quality.

Orbiter

Environmental Control and Life Support Systems (8)

Control of noise in the crew module and control of debris in the cabin atmosphere should receive greater attention in the initial design of future vehicles.

Cold plate cooling of all avionics as opposed to air cooling would have provided a simpler system which would create less cabin noise and avoid the problem of debris in cooling fans. In a vehicle like the Orbiter, provisions should be included for cooling payload equipment that is located in the crew module.

Use of a life support system test bed allowed for early resolution of many design problems and this approach should be used in the future. When appropriate, the Shuttle should be used as a test bed for future systems. Shuttle experience indicates that short duration zero-g simulation on the KC-135 aircraft was not adequate for testing the waste management system. Problems with the

flash evaporator were not discovered until testing was done in a very high performance vacuum chamber.

Communications and Tracking Systems (9)

Operational considerations should be an important part of program development. A limited spare parts inventory increases costs and causes schedule delays. Sometimes systems turn out to be more critical in vehicle operations than originally thought as was the case with the Shuttle closed circuit television. Every effort should be made to anticipate future applications of support systems and plan for this in the design.

Vehicle and payload communications equipment should be segregated. Orbiter communications and tracking system requirements were driven by customer accommodation requirements.

Communications and tracking equipment should be standardized for all launch and orbital vehicles and the next-generation TDRSS. Designing the Orbiter systems to be compatible with TDRSS at a time when TDRSS was not firmly defined was a major challenge.

Complex multi-function systems like the Ku-band system should be avoided as they are difficult to develop and limit operational flexibility. This is an example of a design which minimizes weight but results in other problems.

Detailed analysis and systems-level simulations were very important in design of the communications and tracking systems.

Severely limiting development expenditures can result in high operations costs and inferior performance.

Modification of off-the-shelf components can be more costly than design of new hardware. Also, in-house directed development for some components can be cheaper and more effective than independent development under contractor management.

Good documentation promotes efficiency and reduces personnel training time.

The following items of new technology should be considered for incorporation in future vehicles: "windows" in the TPS for easy antenna servicing, monolithic microwave integrated circuits, voice-control

devices for non-critical crew functions, optical storage devices, and solid state sensors in television cameras.

Avionics Redundancy Management (10)

The simplicity gained with an avionics redundancy scheme using five independent strings would have been worth the weight penalty.

Multi-purpose displays and controls with local processing to off-load the central computer should be considered for future vehicles.

Instrumentation System (11)

Future vehicles should make use of new types of solid-state transducers which have built-in data processing and memory devices. This will reduce the weight and cost of instrumentation.

Propulsion and Power (12)

Development and operations costs should be considered in specifications reviews and specifications should be reviewed periodically to maintain agreement with requirements.

A design philosophy based solely on minimizing weight can result in costly or inadequate hardware.

Operational considerations should play a major role in all design decisions. Filters should be used above and below all fluid system components rather than assume that systems will be kept clean. Fluid lines should be designed for higher stress rather than rely on pressure relief valves.

As many operational modes as possible should be identified and accounted for in the initial design to avoid costly redesign. An example of this problem is an experience with the fuel cell storage tanks. The tanks were designed to be always full at launch. When it was later decided for performance reasons to launch on some missions with the tanks less than full, a costly recertification program was required.

Designing means of access for maintenance is an important factor in minimizing operations cost but maintenance procedures cannot always be predicted. The OMS pod was designed to be removed for remote servicing and so it has no external access panels. In practice the pod is not

removed for routine servicing but now must be removed for even very minor repairs because there is no other way to access the inside of the pod.

Power requirements always tend to grow and so redesign can be avoided by sizing the power generation system for 150% of the projected power requirements.

Testing of propulsion systems needs to be as realistic as possible. Main engine components need to be tested with the correct cryogenic propellant to ensure correct thermal conditions. Material compatibility testing with propellants should be conducted with temperature and moisture conditions which match the flight environment.

Structural Loads, Integrated Vehicle and Orbiter (13)

Because of funding constraints, some important engineering analysis work had to be delayed. As a result, the Shuttle program went from the preliminary design to detailed design phase too soon. Hardware was built before the design was mature. For example, the structural design of the Orbiter was essentially complete in 1975, but much of the supporting aerodynamic, thermal, and flight control system analysis was not finished until several years later.

Multi-body vehicles like the Space Shuttle require more complex structural analysis. Complex aerodynamic interference effects with the Shuttle configuration resulted in differences between the predicted and actual pressure distribution on the wings. Launch load analysis can be simplified by using a design which minimizes base moments and horizontal shears in the launch mount.

The landing gear loads could be decreased by increasing the nose gear strut length which would increase the rollout angle of attack.

Flight instrumentation for vibration measurements should have a broader frequency range. The range should be at least 0-50 hertz and 0-300 hertz if possible. (Shuttle range was 0-20 hertz.)

Mechanical Systems (14)

The major design considerations for the landing gear were the requirements for high landing speed and minimum weight. In the future, landing gear should be designed with enough margin to allow for growth in vehicle weight. Pyrotechnic devices should only be used as a backup means

of gear deployment. More commonality is needed in electrical and mechanical devices to reduce procurement and maintenance costs. The Orbiter has 76 electric motors and none of them are interchangeable.

The off-center location of the elevon hinge line complicated the design of the elevon seal system which has 3300 parts per Orbiter.

Orbiter Thermal Protection System (15)

The thermal performance of the thermal protection system has been better than expected. Although thermal tiles are easily damaged by ice and debris, the number of tiles that must be replaced after each flight is less than expected. Waterproofing and surface roughness criteria remain open issues. Damage to OMS pod tiles was determined to be caused by a combination of thermal and dynamic forces which were not combined in certification testing.

Leading Edge Structural Subsystem (16)

The lifetime design margins for the carbon-carbon leading edge material limits potential growth of Shuttle performance without design modification. Oxidation is the most important factor in limiting the mission life of leading edge material, but special coatings have been effective in controlling this problem.

The leading edge material has proven to be very resistant to damage from rain, ice, and debris in the flight environment but it is susceptible to chipping during ground handling.

Integrated Passive Thermal Control Subsystem (17)

Rigid control of manufacturing, shipping, and installation of thermal control materials is necessary to ensure proper flight performance of thermal control systems. Temperature control must be an integral part of the design process for all space vehicle components.

Space Shuttle Main Engine (18)

Manufacturing capability should be a major consideration in engine design. Due to the high performance demands and harsh operating environment of rocket engines, it is important to perform realistic trades between weight reduction goals and materials limits.

In the main engine test program, experience indicates that more test hardware and facilities were needed. In the future, more limits testing should be done on the component level and there should be separate facilities for turbopump acceptance testing.

External Tank (19)

Life cycle costs should be given more emphasis in the program definition phase.

External tank requirements related to ice and debris prevention and thermal loads were received too late to be included in initial design.

Computer-aided design should be utilized from the beginning of a design effort. All contractors should use compatible CAD systems.

Solid Rocket Booster (20)

It should be a goal to provide all reusable launch vehicle elements with permanent thermal protection systems.

The use of fixed nozzles for the solid rocket boosters would simplify the booster design and significantly reduce refurbishment cost.

Spacelab (21)

In future space system programs, the operational baseline should be developed as part of the program definition and sparing requirements should be defined in the initial design.

A modular design should be used for avionics so that obsolete components can be replaced periodically.

Flight Operations

Mission Operations (22)

Orbiter and payload software should be kept separate to reduce the need for integrated testing. The Orbiter downlist should be standardized and the payload downlist should be programmable to allow flexibility in response to manifest changes.

Payloads should be designed to function in the normal Orbiter environment so that special attitude and thermal

constraints are not required. Grapple fixtures should be located on all movable objects in the payload bay. External elements on future vehicles should be designed for compatibility with extra-vehicular activities. Shuttle experience has shown that EVA capabilities were underestimated. EVA backup procedures should be considered in the design of all vehicles and payloads. EVA overhead can be minimized by designing common tools to be used inside and outside the crew module and by considering pre-breathing requirements in selection of the normal cabin pressure level. A training facility which integrates EVA and remote manipulator functions is needed.

Future manned vehicles should incorporate common hardware and software for onboard and ground command and monitor functions. This will facilitate greater standardization, enhance onboard capabilities, and simplify training.

Future training facilities should be more generic in design with flexible, instructor-oriented displays and controls.

Software should be compartmentalized for easy updating and modification. Simulators should have standard interfaces for networking and there should be greater use of part-task trainers. Separate facilities for independent training of ground support personnel should be considered. Astronauts should be grouped in specialized cadres and fly more often to minimize training time. Flying technical specialists, as done with the CFES experiment, is an effective approach.

Flight Crew Operations (23)

For future manned vehicles an effort should be made to expand the intact abort envelope by increasing structural margins and providing automatic flight control software for a wider range of situations. Use of an escape module that will function during all phases of atmospheric flight should be considered and crash exits which meet aircraft standards should be provided. Vehicle systems should be designed to sustain limited impact damage and continue functioning. Design improvements to enhance the operational safety of solid rocket boosters should be studied.

The aerodynamic design of a future Orbiter-type vehicle should incorporate active canards for control and elevons which function as flaps for trim. The handling qualities for approach and landing should be improved.

The flight control system should use four independent strings. A modular design should be used for avionics to simplify component upgrading and fiber optics should be considered for avionics, data processing, and communications systems. The navigation system should utilize the global positioning satellite system.

Main engine control computers should be located inside the vehicle and not on the engines.

Attitude control thrusters should be aligned with the principal vehicle axes to eliminate cross-coupling and unwanted translations. Thruster locations should also be selected to avoid surface impingements. Additional vernier thrusters are needed to provide low thrust translations and for redundancy in rotational control. There should be an interconnect capability between the forward and aft reaction control systems. The "no yaw jet" capability should be extended through Mach 1.

Rendezvous capability should be upgraded by developing onboard autonomy, improving radar displays, and possibly installing transponders on targets. The rendezvous radar system should be independent of the Ku-band communications system and make use of a phased array antenna.

The onboard computer system should be designed for growth and be less reliant on mass memory units. All functions performed onboard with off-line computers and calculators should be incorporated into the main computers. An advanced caution and warning system should be developed to automatically display relevant data and suggested crew responses.

The current Orbiter has too many switches and systems operations are too complex. State-of-the-art display and control technology should be utilized in future vehicles including electronic displays to replace attitude indicators, tape meters, and gages. Heads-up display units should be used in all flight phases including rendezvous.

Electromechanical actuators should be considered as a replacement for hydraulic actuators and in any case actuators should be provided with four redundant channels. Nose wheel steering is a critical function and also requires a high level of redundancy. The brakes must be adequate for landings on runways less than 15,000 feet long at all possible landing weights. The nose wheel strut should be lengthened to increase the rollout

angle of attack. A powerful landing light is needed to support night landings at contingency landing sites.

The external tank umbilical doors must be fail safe, so that the vehicle would survive reentry even if the doors fail to close.

The remote manipulator system should have more electrical redundancy.

All crew module hatches that are exposed to vacuum should open inward. Cabin air filtration should be improved by using electrostatic filters that are easy to maintain.

The overhead and payload bay windows need to be large enough to view objects like the Space Station and provide a complete view of the payload bay. There should be a steerable spotlight in the payload bay. If the pilots' seats were positioned closer to the forward windows, these windows could be smaller and still provide better visibility than in the current Orbiter.

There should be a separate computer for payload interface. This computer should be a standard commercial unit allowing customers to develop their own software.

Mission Support (24)

Experience with Space Shuttle operations points to the need to increase standardization of payload interfaces, procedures, and flight profiles in order to reduce flight-to-flight reconfiguration activity.

Impact of Design Approaches on Operations (25)

Achievement of low operations cost requires the use of a robust vehicle which does not require careful assessment on the day of launch for acceptable environmental conditions. One way to achieve this is to emphasize design envelopes rather than point designs.

Payload Considerations (26)

Autonomy for payloads should be a goal. This goal should be approached by encouraging the design of standard payload attachment fittings and interfaces, containerization of payloads, and self-contained power and cooling capability for payloads. Payload integration analysis should be based on design envelopes rather than unique configurations.

Payload Integration (27)

The complexity of payload operations can be minimized through early contact with customers and the avoidance of irreversible deployment sequences. As much as possible, payloads should be designed to be insensitive to Orbiter operations and flight attitudes. Payload and launch vehicle software should not be integrated.

Ground Operations

Ground Processing (28)

Orbiter processing, and specifically, main engine maintenance, is the most significant pacing item in the Shuttle ground flow. Although payload processing is also complex, it has been accomplished within the schedule required for Orbiter turnaround.

The majority of payloads are processed and integrated into the Shuttle vertically. This permits integration on the pad which is generally desirable for most payloads and necessary for hazardous payloads which include those with upper stages. The major complicating factors for payload integration with the Shuttle as opposed to previous vehicles is the integral payload bay and the need to process multiple payloads for a single launch.

KSC Ground Operations - Portable Purge Unit (29)

In general, operations personnel need to have an earlier input in the design process of ground operations equipment. An example of this is the portable purge unit. Severe design compromises were made to make a portable purge unit, but portability is not required in all locations where the unit is used.

KSC Ground Operations - Hydraulic System (30)

Test points should be provided for all heater circuits and all test point connectors should be located in an easily accessed area. Electrical connectors should be designed to permit easy verification of proper connection.

All hydraulic servicing connections should be located at the vehicle skin line to avoid the need for internal access. Hydraulic isolation valves should be provided for each aerosurface so that work can be done on one surface while hydraulic power is on. With the current Orbiter, some hydraulic servicing can only be done when

the vehicle is horizontal which is a troublesome limitation.

Ground Operations - Communications and Tracking (31)

Incompatibility problems should receive serious attention when considering the trade-off between off-the-shelf hardware and new designs. Sometimes a new design is more cost-effective.

The following are some new technology items that should be considered for future communications systems: solid state devices to replace tubes in the microwave landing system, phased array radar to replace conical scan radar, and new microprocessors for self-test devices.

Ground Operations - Orbiter Main Propulsion System (32)

An adequate supply of spare parts is essential to maintaining the Shuttle engines and meeting the turnaround schedule.

Weather Protection (33)

The Space Shuttle was intended to be an all-weather vehicle and so the launch pad was not designed to provide extensive protection from the elements. However, the Orbiter thermal protection tiles and OMS pods are prone to absorb water which could result in damage. Significant pad modifications and ground procedure changes were necessary to provide protection. The design of future vehicles should include the requirement that the vehicle not be sensitive to moisture, salt air, or rain impingement. If it is determined that this requirement is prohibitively expensive then suitable enclosures should be included in the initial design of the launch facility.

SRB Ignition Overpressure (34)

During the first Shuttle launch, the solid rocket booster ignition overpressure was higher than predicted. Reducing the overpressure to an acceptable level required extensive modifications to the launch pad and the addition of water bags which must be replaced for each flight. In the design of future vehicles, the configuration of the launch pad and resulting induced environment must be an important consideration. Vehicles should be designed to withstand their induced launch environment.

SRB Holddown Post Strain Gages (35)

During design of the mobile launcher platform, a requirement was identified for the installation of strain gages in the solid rocket booster holddown posts. These strain gages must be repaired and retested after each launch resulting in significant recurring costs. In future programs, the vehicle structural support system should be designed with sufficient margins to eliminate the need to measure vehicle loads. If load verification is necessary, it should be limited to static test articles and not be required for flight hardware or ground systems.

CONCLUSION

The Space Shuttle Program broke new ground in aerodynamic, structural, and thermal analysis, operation, recovery, and refurbishment of space hardware, and integration of complex systems. Great strides were made in all areas of space operations and engineering. The Space Shuttle represents the first reusable spacecraft, the first winged reentry vehicle, and the first spacecraft to land on a runway. The development program was accomplished on a very tight budget, with a minimum of test hardware, and with no unmanned flight testing. As a result of this effort, the United States has an extremely successful and highly flexible launch system for personnel and payloads that has proven itself in over twenty flights. Future space vehicle development programs are likely to have goals as ambitious as the Shuttle and are likely to encounter similar technical challenges and budgetary constraints. It is hoped that this summary of Space Shuttle experience will provide some guidance to future programs in the difficult decisions and engineering trade studies that are part of the development of a successful space vehicle.

At the time of the Challenger accident, NASA had begun a research effort termed the OEX Program (for Orbiter Experiment), a Johnson Space Center effort to use the Shuttle as a flight research vehicle for acquiring data to support the future space vehicle technology base. OV-101 Columbia was modified to carry four major experiments, and, by the time of the Challenger accident, had completed one mission (STS-61C, January 12-16, 1986) in the program. The four experiments consist of a Shuttle

Infrared Leeside Temperature Sensing (SILTS) experiment carried in a special experiment pod mounted on the tip of Columbia's vertical fin, the Shuttle Entry Air Data System (SEADS) experiment, the Shuttle Upper Atmosphere Mass Spectrometer (SUMS) experiment, and the Advanced Autopilot Experiment (AAPE). SILTS, a Langley Research Center experiment, uses an IR scanner viewing the upper fuselage and left wing through two silicon windows, in an attempt to understand the little known phenomena of leeside heating so that future reentry vehicles can be designed without more extensive than necessary leeside thermal protection. On STS-61C, the SILTS system acquired 900 sec. of wing and 30 sec. of fuselage imagery. SUMS is intended to measure freestream atmospheric density; when its measurements are compared to another on-board experiment, the Shuttle High-Resolution Accelerometer Package (HiRAP), engineers can calculate Shuttle's aerodynamic coefficients at flight conditions previously unexamined by experimental techniques. SUMS also permits detailed examination of transition aerothermodynamics by measuring equilibrium gas composition at its inlet port. SEADS is intended to provide precise information on Shuttle's attitude and vehicle state by measuring such quantities as angle of attack, angle of sideslip, free stream dynamic pressure, Mach number, total pressure, and the like, using a sensor probe mounted in the Orbiter's reenforced carbon-carbon (RCC) nosecap. Analysis indicated that Shuttle would be unable to acquire air data information using conventional techniques at speeds above Mach 3.5; the SEADS experiment is designed to fill this gap and enable acquisition of data over the entire spectrum of Shuttle's performance, from transonic to hypersonic velocities, and over an altitude range from 0 to 280,000 feet during ascent and from 280,000 feet to sea-level during descent. SEADS development involved construction of a columbium instrument and a web of tubing and pressure transducers installed in a modified RCC Shuttle nosecap. During STS-61C, SEADS flew without incident, and postflight inspection indicated

that the system not only survived reentry without significant erosion or damage to the nosecap, but was in fact "tighter" than before flight (as had been predicted in pre-flight analysis). Via such experiments as SILTS, SUMS, and SEADS, the base of knowledge for Shuttle-like craft will be expanded to prevent some of the unknowns and unexpected interactions noted in the white paper studies mentioned previously.[9]

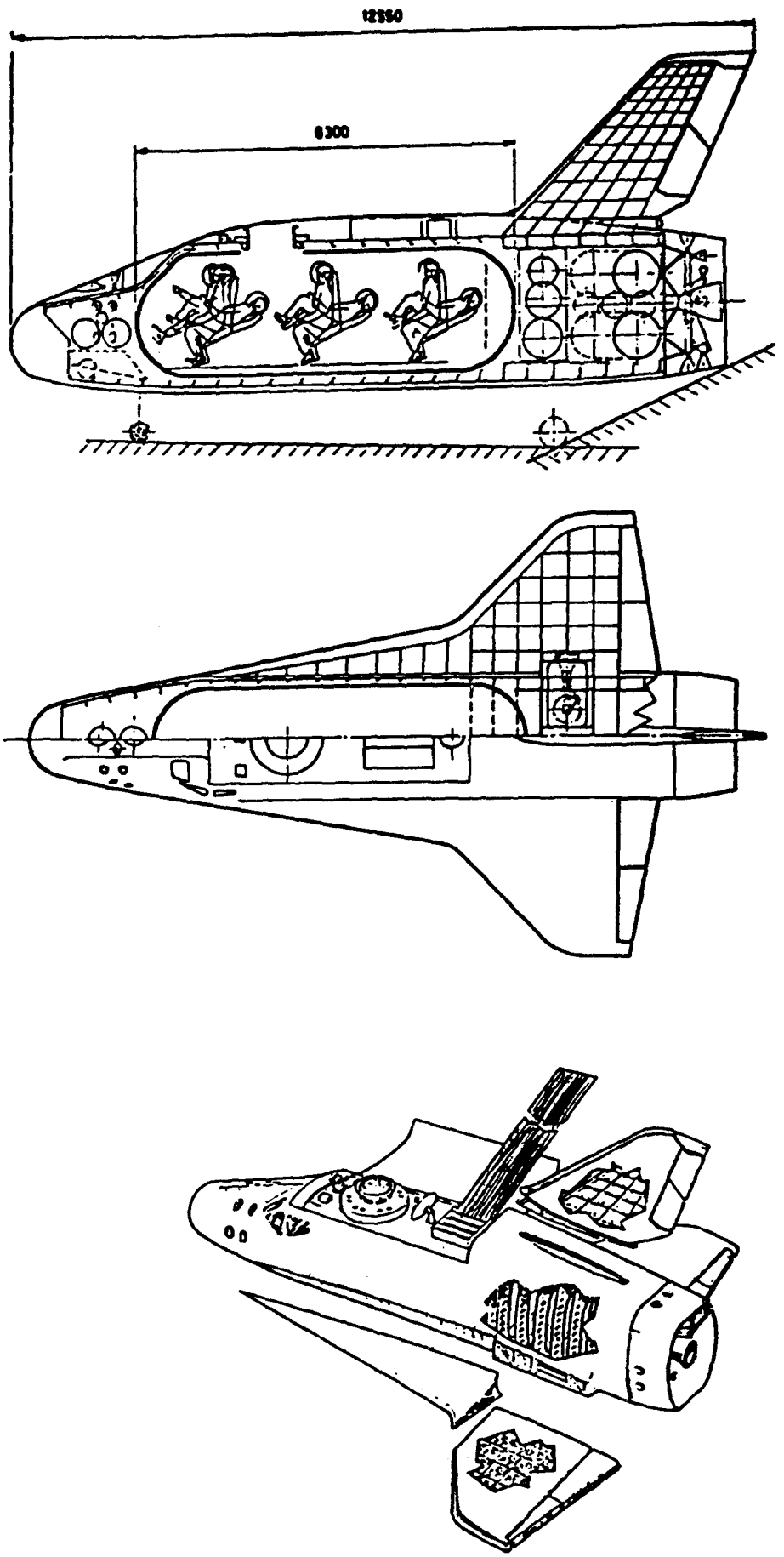
The Resurgence of Foreign Concepts

If imitation is indeed the sincerest form of flattery, then the United States has every right to be pleased, if somewhat uncomfortable as well. Three nations are contemplating lifting reentry logistical spacecraft drawing upon Shuttle-derived technology. Further downstream, two other nations are contemplating advanced winged logistical spacecraft, as is the United States itself. The first three nations are France, Japan, and the Soviet Union, and while the former two are envisioning relatively small craft, the Soviet Union is actively developing a Shuttle-like vehicle (as well as a smaller spaceplane) that is virtually indistinguishable to the untrained eye from America's own STS. A brief discussion of these international efforts is worthwhile, if for no other reason than to place development of America's own Shuttle and its possible successor in a larger and more relevant context.

In 1979, at an International Astronautical Federation meeting held in Munich, Germany, M. Villain of the Division Systemes Balistiques et Spatiaux of Aerospatiale presented the results of a study first requested in April 1977 by France's Centre National d'Etudes Spatiales (CNES) to "investigate the feasibility and advisability of designing a manned space vehicle which could be launched by an improved ARIANE [expendable booster]."[10] This craft, called Hermes, was to weigh no more than 6,500 kg (14,330 lbs). Further study refined this concept to include a

greater launch weight of 10,000 kg (22,046 lbs), up to a six man crew (or a two man crew with a 1,500 lb payload), and a seven-day mission duration. Launched from France's space center in Guyana, Hermes could undertake observation missions from a 60 deg. inclined circular orbit at an altitude of 200 kms (124.3 miles), a rendezvous with a space station located in a 30 deg. 400 km (248.6 mile) orbit, and rendezvous with an orbiting module located in a 30 deg. 200 km orbit, followed by transfer of the module to a space station in a 30 deg. 400 km orbit. Figure 1 shows the initial configuration for Hermes; its Shuttle origins are pronounced, and, indeed, Villain even remarked in his paper that "In order to hold some strong assumptions on aerodynamics data without doing wind tunnel test on various shapes of the vehicle, the length (12 m) [39.37 ft], the span (7.4 m) [24.28 ft] and the overall shape of the spacecraft have been derived by similarity from the U.S. space shuttle."[11] By the mid-1980s, Hermes, now envisioned as the upper stage of an Ariane 5 launcher, had grown into an estimated $1.6 billion program (which critics charge is optimistic, arguing instead for a likely cost of $2-3 billion) involving participants from thirteen European nations and Canada. Size had increased as well, up to 16,783 kg (37,000 lbs), a wingspan of 9.99 m (32.8 ft), and a length of between 15 m and 18 m (49 to 59 ft). Several alternate configurations appeared, two of which are shown in Figure 2a and 2b, the former reminiscent of Dyna-Soar and European studies of the 1960s, and the latter reminiscent of the later MSC 040C-2, 047, 048, 048A, and 049A Shuttle Orbiter proposals of February-March 1972. Then came the Challenger accident, and in the wake of that event, France undertook a reappraisal of Hermes, adding an ejectable cabin for its crew (now reduced from 6 to 3), reducing payload from 4,550 kg (10,000 lbs.) to 3,000 kg (6,600 lbs), and modifying the design of the payload compartment, solid-fuel boosters, and central liquid-fuel stage. Overall, Hermes grew to 21 metric tons (46,300 lbs) by mid-1987. Hermes is envisioned as a "hot structure" reentry

Figure 1



HERMES (1979 IAF CONFIGURATION)

Figure 2

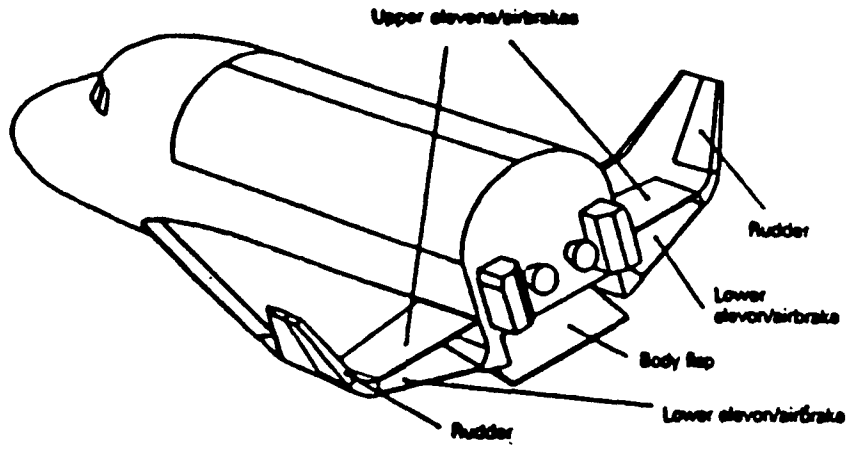
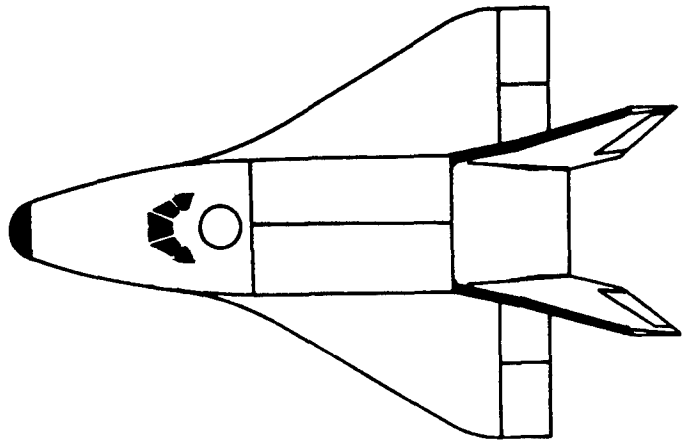


Figure 2a: The "Dyna-Soar" Approach



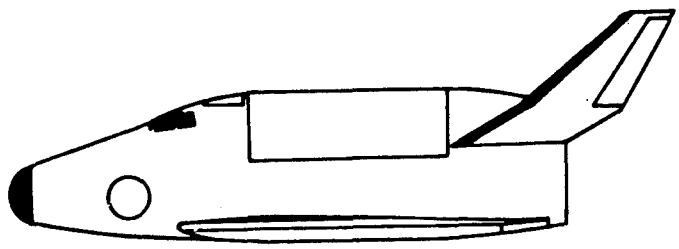


Figure 2b: The MSC 040C-2 et. al. Approach

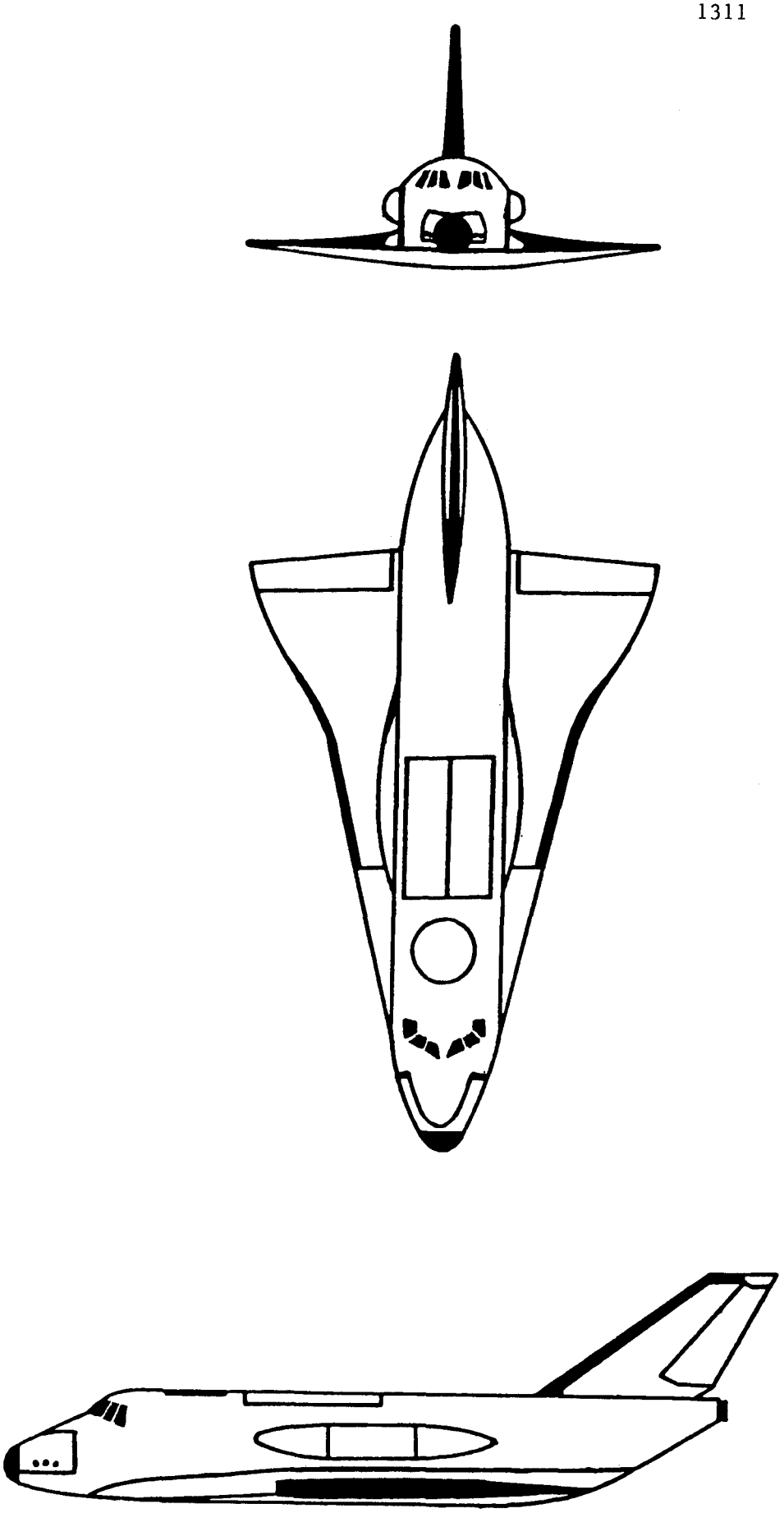
ALTERNATE HERMES CONFIGURATIONS (1986-1987)

vehicle unlike the American Shuttle, and it is hoped to be flying by 1995. To do that will require extensive ground testing taxing Europe's available hypersonic facilities; it is estimated that Hermes will require 20,000 hours of tunnel time involving up to eighty different hypersonic models.[12]

In comparison with Hermes, already well-along in development, Japan's own studies for a hypersonic mini-Shuttle are proceeding more slowly. Currently envisioned to weigh and have dimensions similar to the initial Hermes proposal of the 1979 time period, the Japanese Shuttle will also possibly have small landing engines flanking its payload bay. A very provisional three-view drawing of this craft, under consideration by the Japan Space Agency, is shown in Figure 3. Again, the generalized aerodynamic shape of the Shuttle has been adopted as a means to expedite development--a prudent course of action for a nation possessing limited assets and resources, yet determined to become and remain competitive in space.

It is the Soviet Shuttle, however, that has been the subject of the greatest speculation, and it is the Soviet Shuttle that bears the closest resemblance to its American counterpart, not merely geometrically, but in size and anticipated capabilities as well. On June 3, 1982, the Soviet Union launched a small reentry vehicle, Cosmos 1374, reminiscent of the earlier American ASSET and PRIME spacecraft. Figure 4a shows a view of this craft, recovered successfully in the Indian Ocean. Three more flights (Cosmos 1445, 1517, and 1614) followed. Subsequently, Department of Defense sources indicated that a small spaceplane, anticipated to be similar in configuration to this earlier series of test models, was under development, likely intended for launch using a new SL-X-16 medium lift launch vehicle having a lift-off weight of 400,000 kg (881,840 lbs). This small spaceplane is anticipated to be able to undertake "real-time reconnaissance missions, satellite repair and maintenance, crew transport, space station defense, and

Figure 3



JAPAN SPACE AGENCY MINI-SHUTTLE CONCEPT, 1987

enemy satellite inspection or destruction."[13] Figure 4b shows a likely configuration for this craft, based upon drawings by analyst Theodore A. Talay. The Soviet Shuttle is anticipated to be able to place a 30,000 kg (66,138 lbs) payload in a 180 km (112 mile) orbit. The delta-wing Shuttle, possessing the same size and general configuration of the Space Shuttle Orbiter, would be stacked in similar fashion to the American Shuttle on a new SL-W heavy lift launch vehicle, weighing 2,000,000 kg (4,409,200 lbs.) at launch, and generating a lift-off thrust of 6,138,000 lbs. The SL-W heavy lift launch vehicle, consisting of multiple boosters flanking an external tank, as well as propulsion units at the base of the tank, is also envisioned as a heavy lift launch system for conventional payloads attached to it in similar fashion to the Soviet Shuttle. It is this feature of the Soviet Shuttle--namely the interchangebility of its external components so as to constitute an expendable launch system as well (something suggested often as a logical spin-off of the American Shuttle effort)--which, together with the lack of main engines in the Soviet orbiter itself, most clearly distinguishes the Soviet Shuttle from its American counterpart. The Soviet Shuttle, together with the emerging Soviet spaceplane, and medium and heavy-lift launch vehicles, is anticipated to form the core of an aggressive Soviet space program directed towards the exploitation and utilization of space for military as well as civilian purposes. Figure 5 shows a comparison view of the Soviet spaceplane, Shuttle, and the medium and heavy lift launch vehicles. The wing configuration of the Soviet Shuttle bears a close resemblance to that of the NASA 040C-2, 048 and 048A, and 049 and 049A configurations developed in 1972 by the MSC. While a launch date for the Soviet Shuttle has not been announced, DoD presently considers it in the "final stages of development." Press accounts state it could fly before the end of 1987.[14]

While the French, Japanese, and Soviet programs overtly draw upon the technological base established by the Space Shuttle,

Figure 4

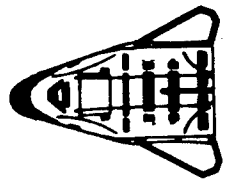


Figure 4a: Cosmos Spaceplane Flight Test Article (1982-1984)

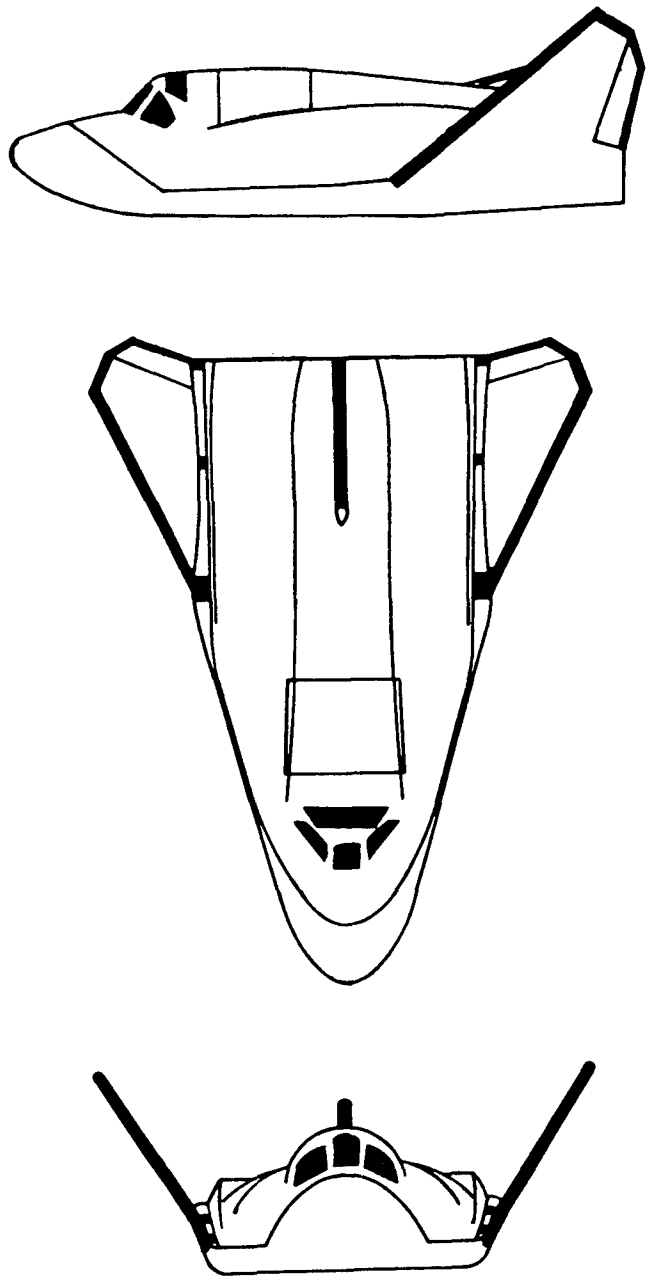
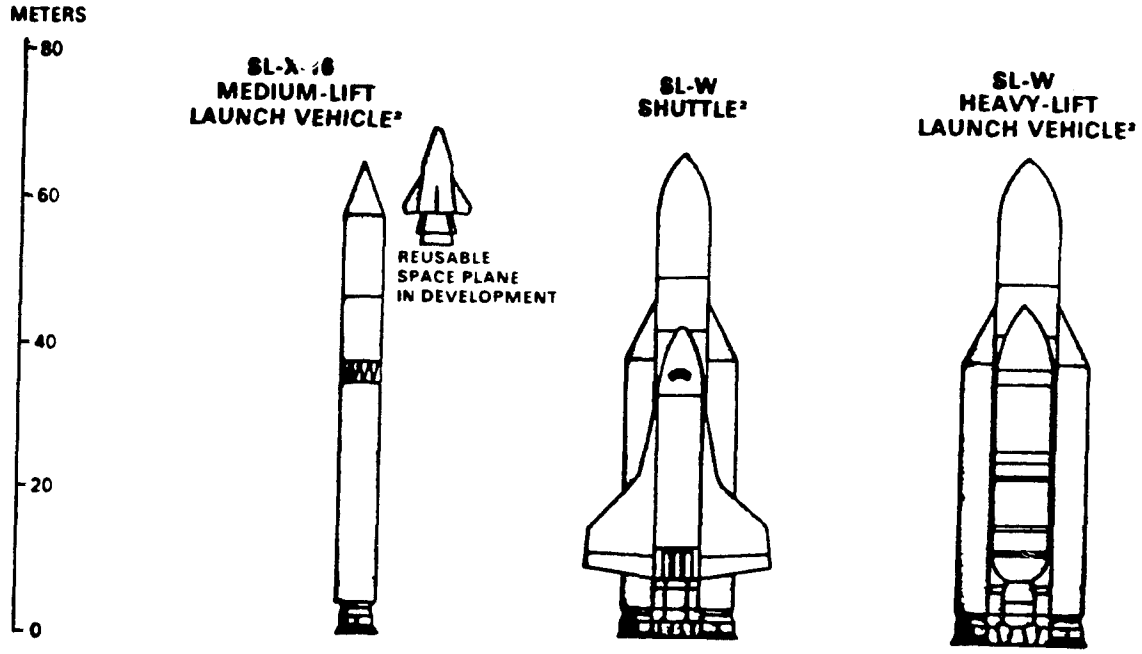


Figure 4b: Possible Configuration of Soviet Spaceplane (1987)

Figure 5

COMPARISON OF THE SOVIET SHUTTLE AND SPACEPLANE

WITH OTHER LAUNCH SYSTEMS



¹ Approximate.
² In final stages of development.

there are projects contemplated for advanced winged orbital transporters that bear little resemblance to the Shuttle. Some of these are as-yet undefined notions for advanced cargo spacecraft, while others are the result of initial planning efforts between agencies such as NASA and the Department of Defense, and will be discussed subsequently. The three most publicized ventures are three concepts for fully reusable single or two-stage-to-orbit transporters making use of combined air-breathing and rocket propulsion: the British HOTOL, German Sänger II, and American NASP.

As shown earlier, Great Britain possessed a fascination with the concept of winged space transporters, and did particularly fruitful work in the 1960s on a variety of schemes such as MUSTARD and (for the time a peculiarly British notion) Nonweiler (caret) wave riders. (This latter class of vehicles subsequently has been studied more recently in the United States as well by researchers at the University of Oklahoma, University of Maryland, AFWAL, and NASA.) The development of the American Space Shuttle encouraged European enthusiasts to proceed with their own plans for equivalent or near-equivalent systems, and out of this interest sprang HOTOL (for HOrizontal Take-Off and Landing), an ambitious British Aerospace effort to produce a remotely piloted (using artificial intelligence) Shuttle-like craft propelled by a mixed air-breathing and rocket propulsion system dubbed "Swallow." HOTOL's goal is the reduction of launch costs to a level about one-fifth that of present-day systems such as the Shuttle. Though HOTOL is intended for pilotless operation, studies are underway for potential piloted applications in subsequent derivatives. In any case, HOTOL will be equipped with provisions to carry passengers in its cargo bay (which is capable of carrying up to 15,435 lbs of payload) though the psychological reactions of passengers at the "mercy" of a pilotless aircraft may well be imagined! HOTOL is a product of British Aerospace, a major conglomerate comprised of what once were formerly independent

companies such as de Havilland, Hawker, and Bristol. The two principal British Aerospace divisions responsible for its design are the Dynamics, Space, and Communications Division at Stevenage, and the Aircraft Group at Filton. Rolls-Royce is responsible for its radical (and largely classified) propulsion system. Swallow, the product of theoretical studies by engineer Alan Bond, is reportedly in some general respects similar to earlier Aerospaceplane LACE concepts from the 1960s: air is taken in during the climb through the atmosphere, chilled by liquid hydrogen, and the condensed liquid oxygen derived by this process is then mixed with the LH_2 propellant and burned in the vehicle's main engines. HOTOL thus is an air-breathing rocket vehicle. Interestingly, it is launched from a takeoff dolly that is laser-guided for precision steering down the runway, much as Sänger-Bredt anticipated launching their Silbervogel from a sled. The configuration of HOTOL resembles a serpentine blending of Shuttle with some fantastic creature such as the mythological Loch Ness monster. In fact, however, its wing configuration owes much to the Concorde SST. As with Hermes, HOTOL has undergone a considerable design metamorphosis since first making its appearance at the 1984 Farnborough Air Show. Figure 6a shows a version displayed at the Paris Air Show of 1985; since that time, as shown in Figure 6b (together with a typical construction detail), HOTOL has lost its vertical fins, the canards are arranged symmetrically around the nose with the vertical canard considerably increased in size, and the aft fuselage has been slimmed to reduce drag. Figure 7a shows a structural breakdown of the craft, with an anticipated military mission model shown in 7b. HOTOL is anticipated to fly in the late 1990s, contemporaneously with Hermes--although this seems optimistic. For a while, considerable British sentiment suggested that France should abandon its own Hermes and embrace HOTOL-an unlikely scenario, given France's heavy commitment to Hermes, and the traditional Guallic nationalist "go it alone" approach to high technology.

Figure 6a

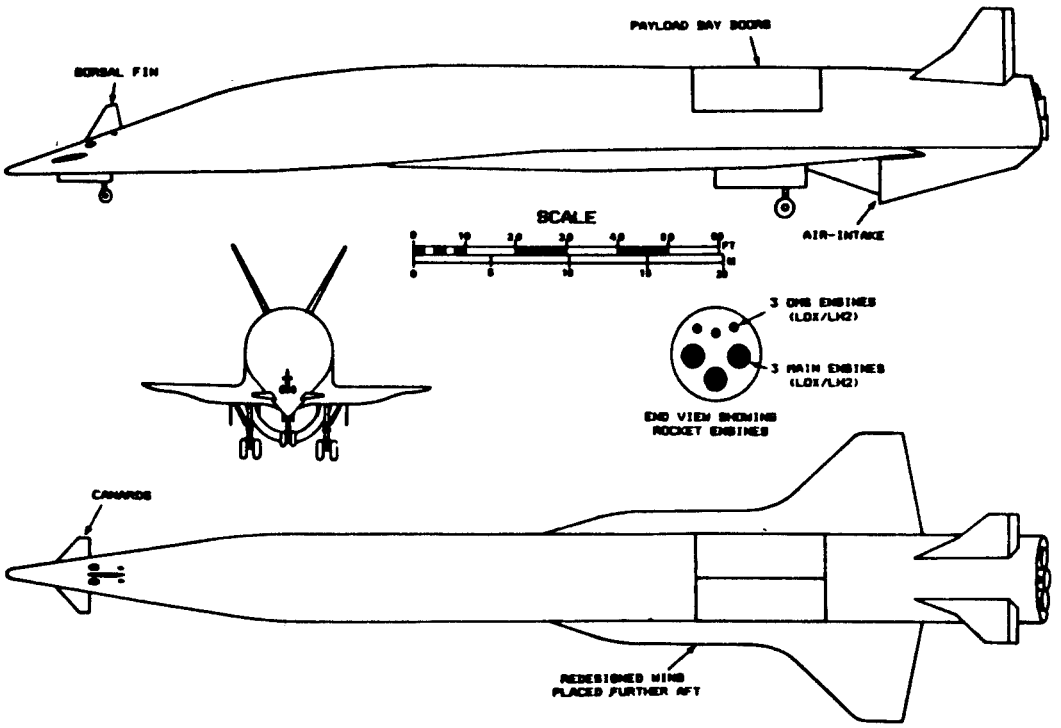
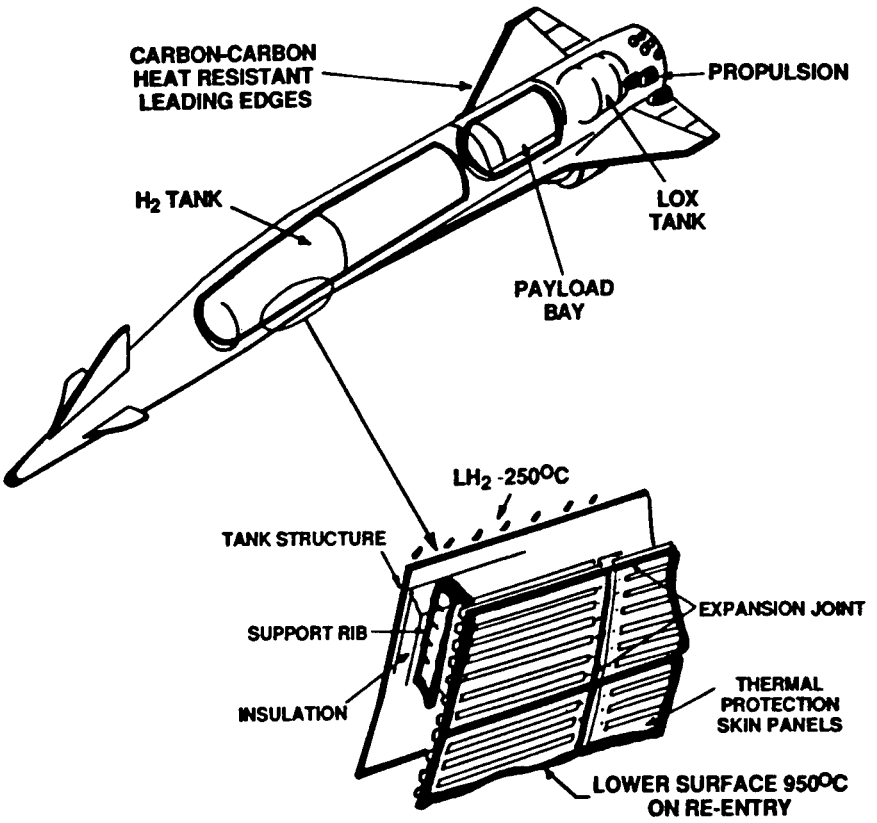
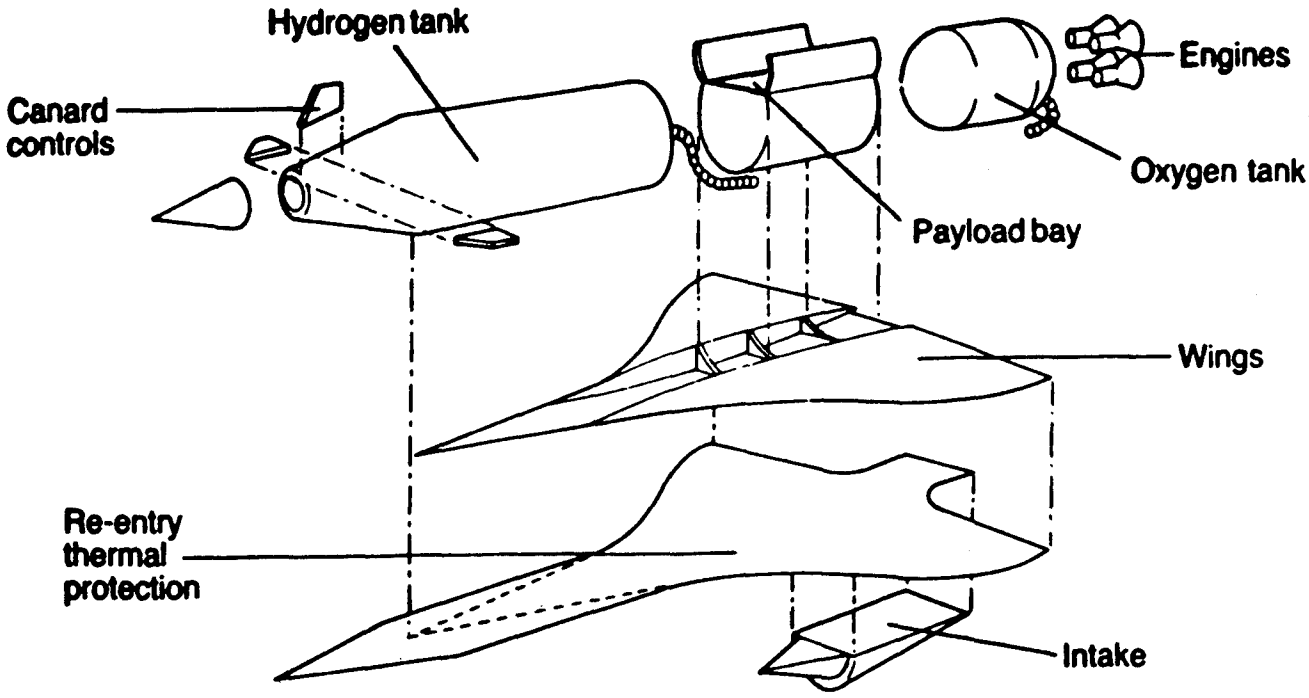


Figure 6b



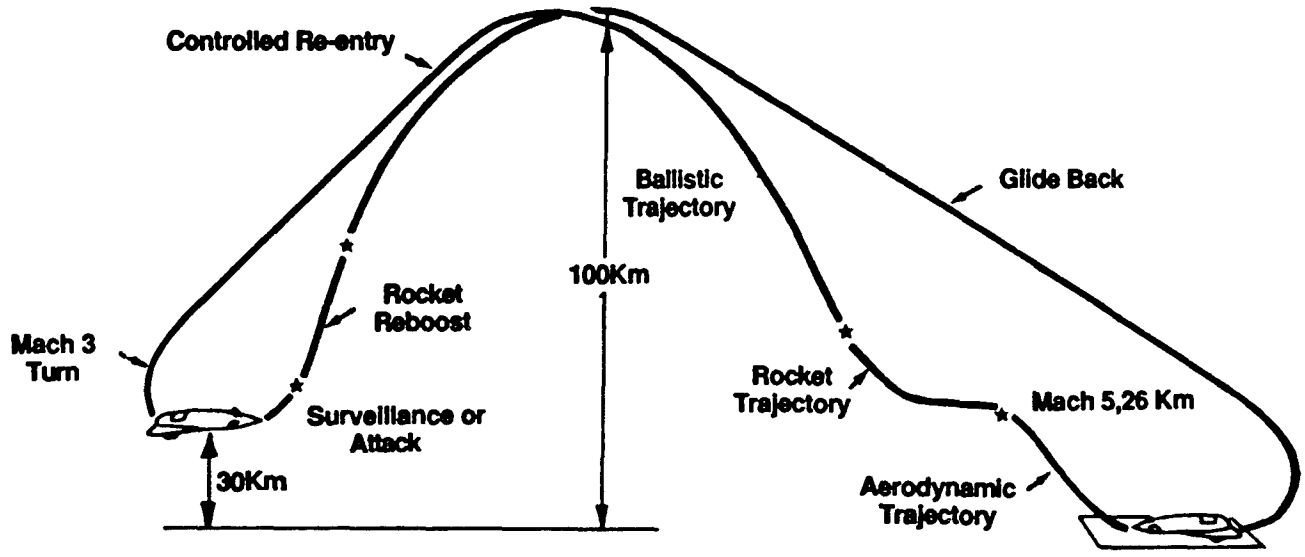
HOTOL: AS CONCEIVED IN 1985 AND REVISED IN 1986

Figure 7a



HOTOL STRUCTURAL COMPONENT BREAKDOWN

Figure 7b



ANTICIPATED HOTOL MILITARY MISSION

Then, in early 1987, Britain's National Space Centre announced that the U.K. would join in the Hermes, in part to encourage "a cross-fertilization" of Hermes and HOTOL technologies, and to improve the overall chances of winning approval of HOTOL by the European Space Agency (ESA). (Formal ESA approval of Britain's participation in Hermes is expected to come in the summer of 1987). In fact, if Europe could afford both, both Hermes and HOTOL would constitute useful complementary systems. HOTOL is anticipated to be a £4 billion ($6.46 billion) program, though this could rise given Britain's experience on high-technology programs.* So far, Britain is showing a strong resolve to keep the program British by minimizing any foreign involvement that might result in HOTOL technology being exploited by other countries. In light of the decision to participate in Hermes, it will be interesting to see if this attitude can be maintained. Intended for both military and civilian missions, including "Transatmospheric Skyliner" service between cities such as London and Sydney, Australia, HOTOL utilizes a hot structure comprised of titanium, Rene 41, Inconel, and carbon-carbon for high temperature applications. Each HOTOL vehicle is anticipated to have a useful service life of 120 flights, with the propulsion system being capable of half of this number of missions before replacement is necessary. All in all, HOTOL is a complex and technology-challenging effort. Given Britain's record for abandoning aerospace projects since the late 1940s (particularly when the Labour Party is in power), it is difficult to predict whether it has a good chance of development or not, but conventional wisdom would argue against its actually being built. Though the technology seems in hand or "around the corner," the ultimate decision to develop HOTOL (as with such decisions in the United

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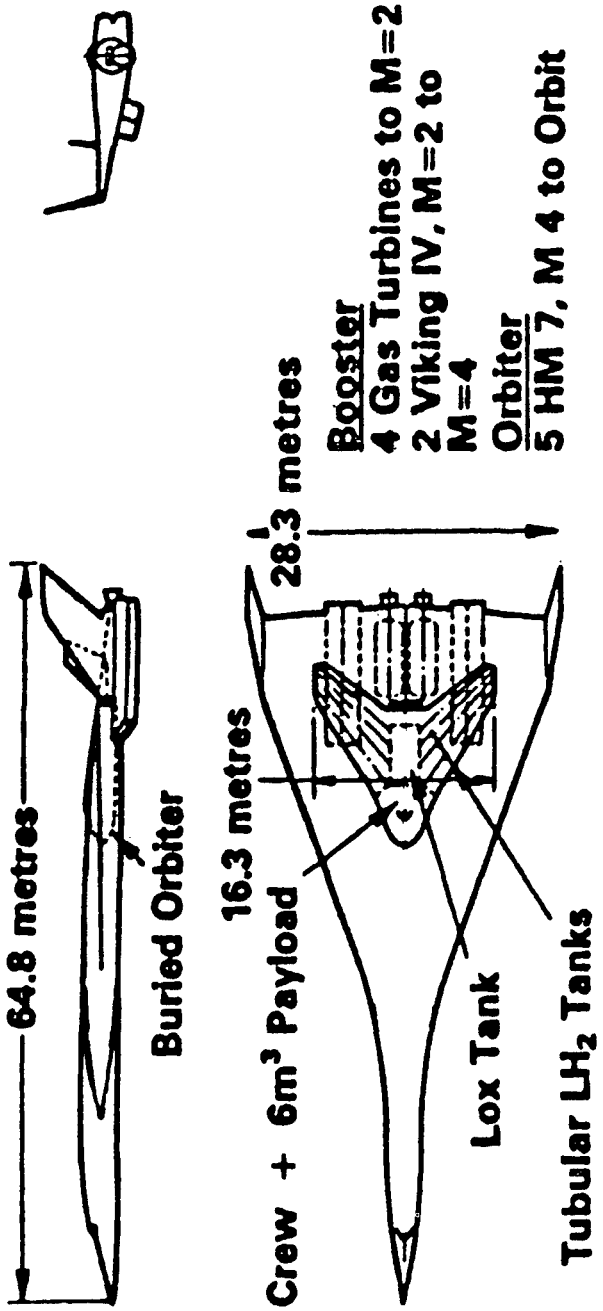
*When Britain and France embarked on the Concorde SST, planners estimated its R&D costs at £150-170 million. Its actual R&D costs exceeded £2 billion. See Peter Hall, Great Planning Disasters (London, 1980) for an interesting discussion of this and other forecasting disappointments.

States) will depend on a host of other social, political, and economic factors, and not merely technological ones, and it is these factors that have traditionally limited new British aerospace initiatives. Overall, HOTOL is the nearest technical equivalent to the proposed American National Aero-Space Plane.[15]

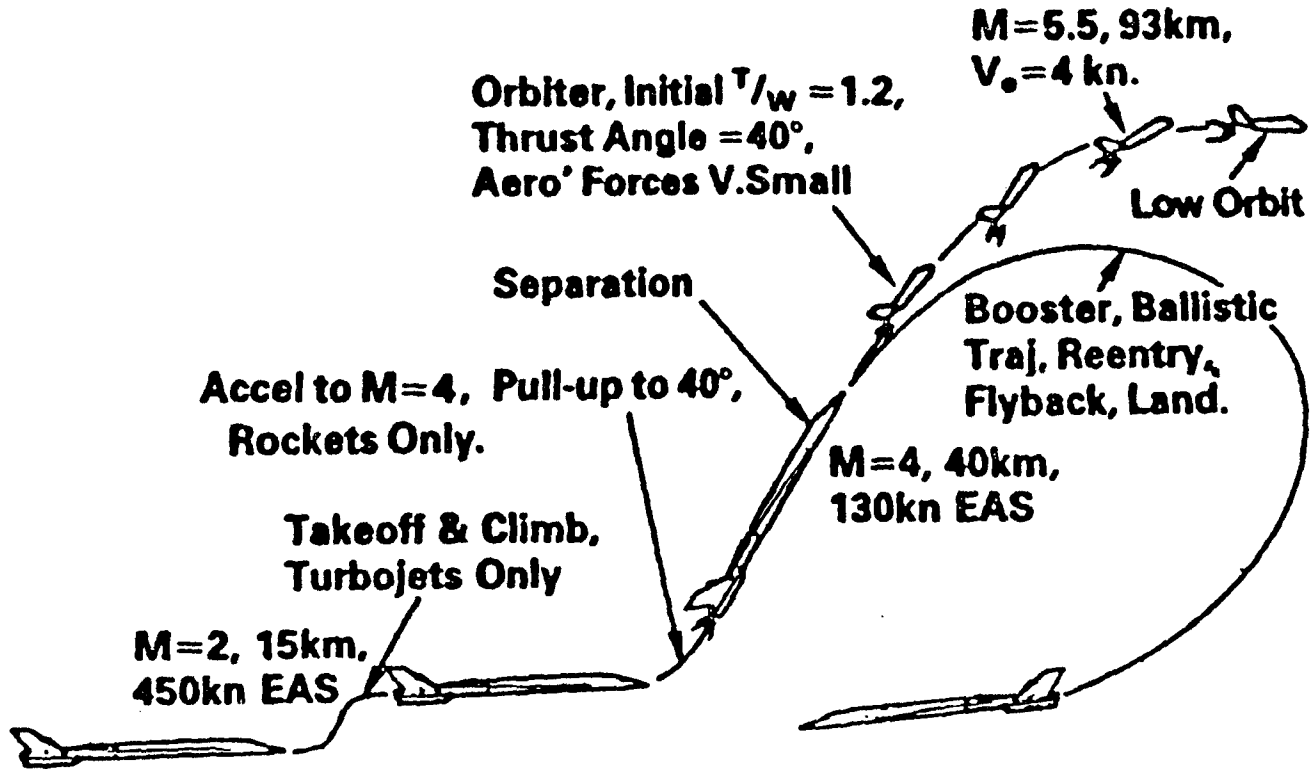
The recognition of the high costs and lengthy development times associated with ambitious projects such as HOTOL has led to a proposal by D.M. Ashford of British Aerospace's Dynamics Group at Bristol to develop Spacecab II: a cheap fully reusable launcher using off-the-shelf technology where appropriate, and seeking costs per flight 1/2000th that of the Shuttle and 1/250th that of the French Hermes project. Spacecab II, shown in Figure 8, is a TSTO concept using a Mach 4 supersonic piloted flyback boost vehicle launching a smaller orbiter. To achieve Mach 4, the booster is flown out to Mach 2 using four Olympus afterburning turbojets (the same engine installation on the Concorde SST). Then, two Viking IV rocket engines, burning unsymmetrical dimethylhydrazine (UDMH), accelerate the booster to Mach 4. (The Viking IV is the Ariane launch vehicle's second stage engine). The orbiter burns LH_2 and LO_2 (with the liquid hydrogen carried in spanwise tubular fuel tanks), has a two-man crew, and has a payload bay with a capacity of 6 cubic m. (approximately 212 cu. ft.). Five HM 7 engines (used as the third stage engines in Ariane) accelerate the orbiter from Mach 4 to orbital insertion. Booster operational costs are estimated at $75,000 per flight (equivalent to Concorde), with orbiter flight costs running about the same, giving a total cost per mission of $150,000, compared to an estimated $300 million per flight for present Shuttle mission costs. The Mach 4 booster avoids the intense heating problems that would be associated with a higher-velocity hypersonic launch platform; Figure 9 shows a typical mission launch profile. BAe estimates that development of Spacecab II could take six years from initiation, and cost £510 million (approximately $825 million, of which $225 million



BRITISH AEROSPACE SPACECAB II CONCEPT (1986)

Figure 9

SPACECAB II TYPICAL LAUNCH PROFILE



would be for orbiter development) making it an attractive alternative concept--and one to watch in the future.

As discussed earlier regarding the technical evolution of the Space Shuttle, Germany had an interest in winged logistical spacecraft that was almost as broad as that of Great Britain, typified during the 1960s by Junkers' RT-8-01 study for a so-called Raumtransporter (space transport). Dubbed after the fact the Sänger I in honor of Eugen Sänger (who had assisted Junkers in their design effort shortly before his death in 1964), this study envisioned a two-stage-to-orbit combination of a delta-wing launch vehicle (powered by three large rocket engines) firing a smaller orbiter reminiscent of the Dyna-Soar configuration. The Sänger I configuration is shown in Figures 9a-9c. While it could be argued that this concept should have been called Sänger II, since Sänger-Bredt's own conceptualization of the Silbervogel clearly qualified for the Sänger I designation, the designation Sänger II has subsequently come to be applied to a proposed new German-inspired European space transporter proposed by the Advanced Space Systems Department of Messerschmitt-Bölkow-Blohm (MBB) and ERNO, the European space research organization.

Two streams of research have blended to generate the current Sänger II concept illustrated in Figure 10a and 10b; one is continuing European interest in hypersonic orbital lifting reentry vehicles, and the other is the interest in Europe in hypersonic air-breathing transports, much like the HOTOL Transatmospheric Skyliner concept or the much-discussed American "Orient Express." MBB conceptualizes the Sänger II as using "a derivative or forerunner of a future hypersonic global transport plane" as a first stage, boosting a second stage orbiter into space. The second stage is based on a concept under study since 1984 dubbed HORUS (shown in Figure 11 in a computer-generated graphic). HORUS, a delta-canard combination, was intended as a second-stage (a la Hermes) for the Ariane 5 launch vehicle. Recently, however,

Figure 9

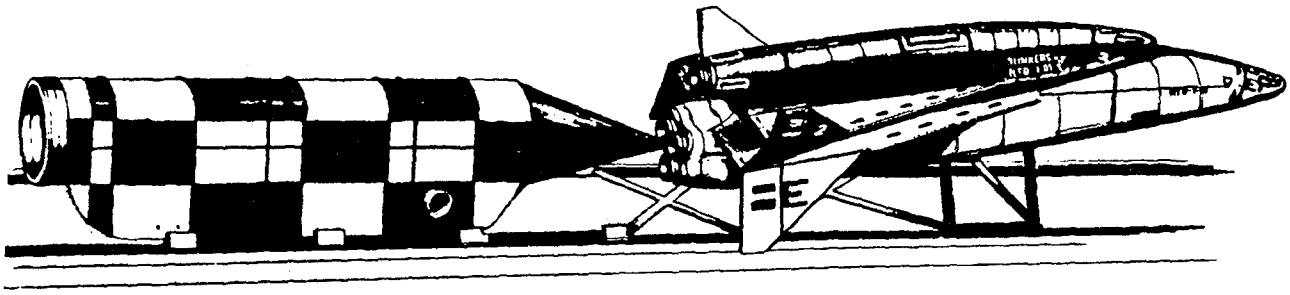


Fig. 9a: Junkers RT-8-01 (Sänger I) TSTO, Utilizing Rocket Sled Launch

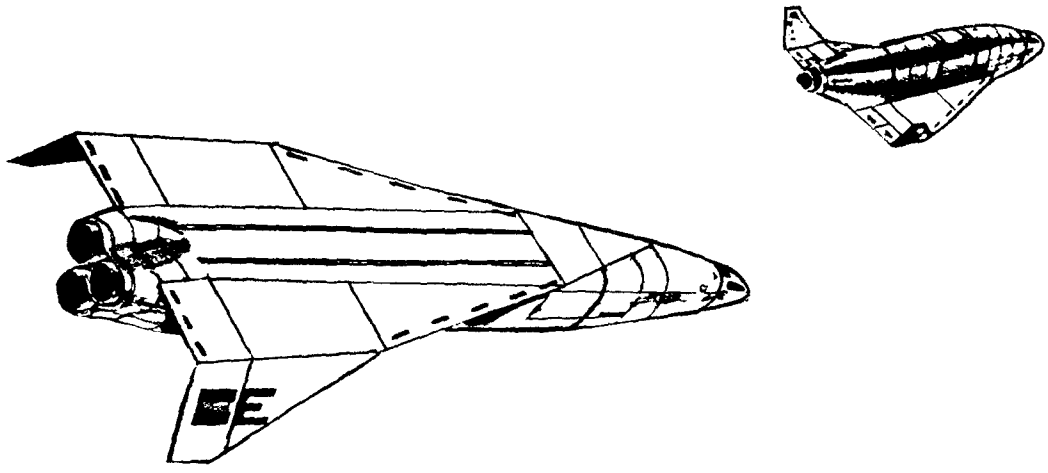


Fig. 9b: Separation of Orbiter (2-3 Ton Payload) From Booster

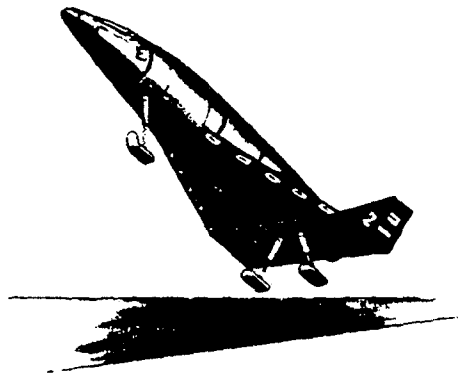


Fig. 9c: Landing of Orbiter on Skid Landing Gear a la X-20

JUNKERS FLUGZEUG UND MOTORENWERKE RT-8-01 RAUMTRANSPORTER (SÄNGER I)

Figure 10

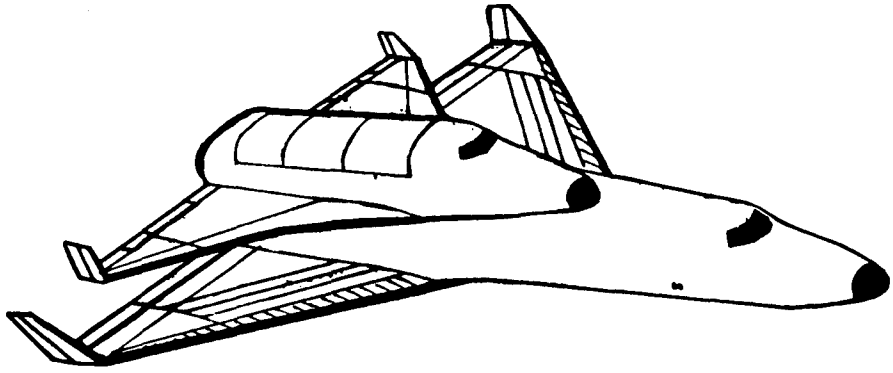


Figure 10a: Sänger II dorsal configuration

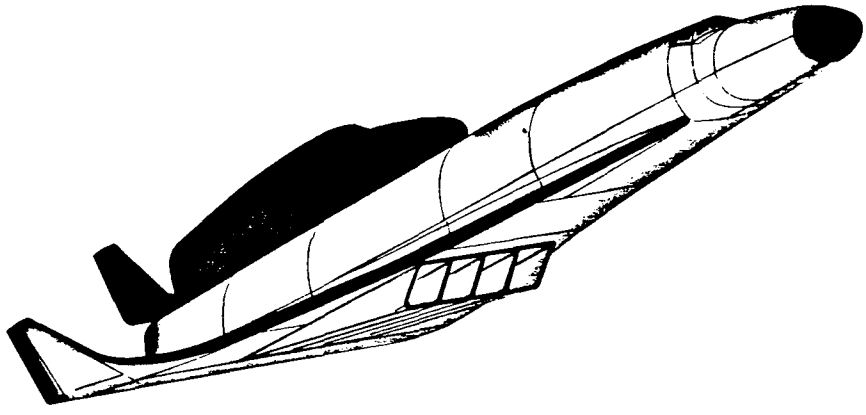
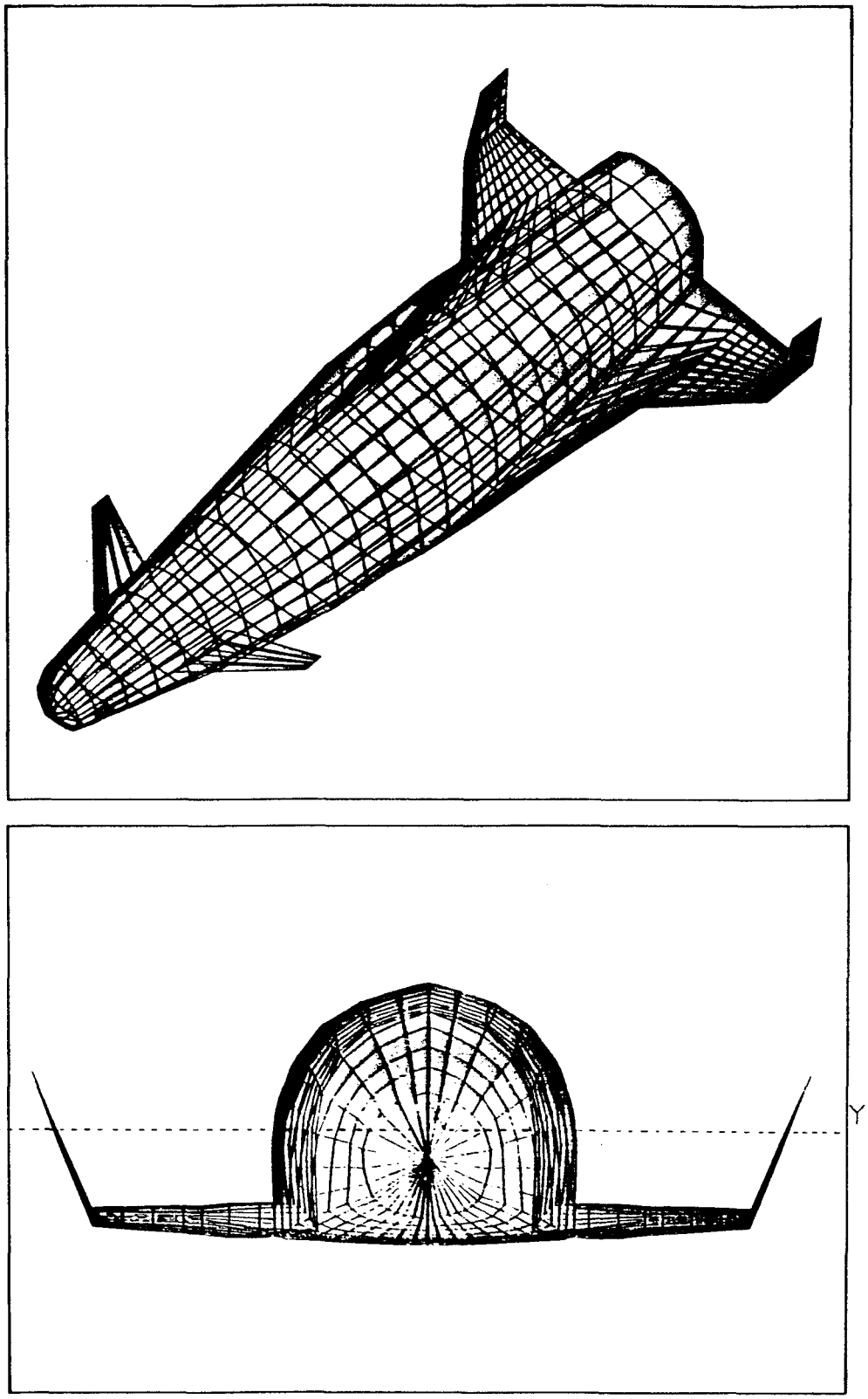


Figure 10b: Sänger II ventral configuration showing inlets for turboramjet engines

Figure 11



MBB HORUS CONCEPT (1986)

MBB shifted the focus of HORUS to include air-launching it from the back of a hypersonic air-breathing mothership capable of Mach 6 flight speeds.[16]

The rationale behind Sänger II is what MBB perceives as Europe's need for a cost-effective space transportation system, cost effective being defined as a system "which reduces the space transportation cost to some 10% of the present high values" incurred by operating partially or fully expendable systems. MBB's view of the desirable European space future is seen in Figure 12, which shows how the Concorde SST is perceived as the forerunner of a genuine global hypersonic transport (the EHTV), and how Hermes, in turn, will foreshadow an orbital rocket plane (the HORUS). EHTV plus HORUS equal the Sänger II concept. Further, as the Ariane 5 will lead into the Ariane 6, the Ariane family experience will spawn an "Economic Cargo Launch Vehicle" which itself is compatible as a payload stage for the EHTV. Sänger II is conceived as an ideal system for five specific missions and payloads:

1. Delivery of a space station crew and supplies, including up to eight astronauts, four tons of cargo into orbit, or two tons of cargo from orbit.

2. Delivery of up to 12 passengers to low earth orbit.

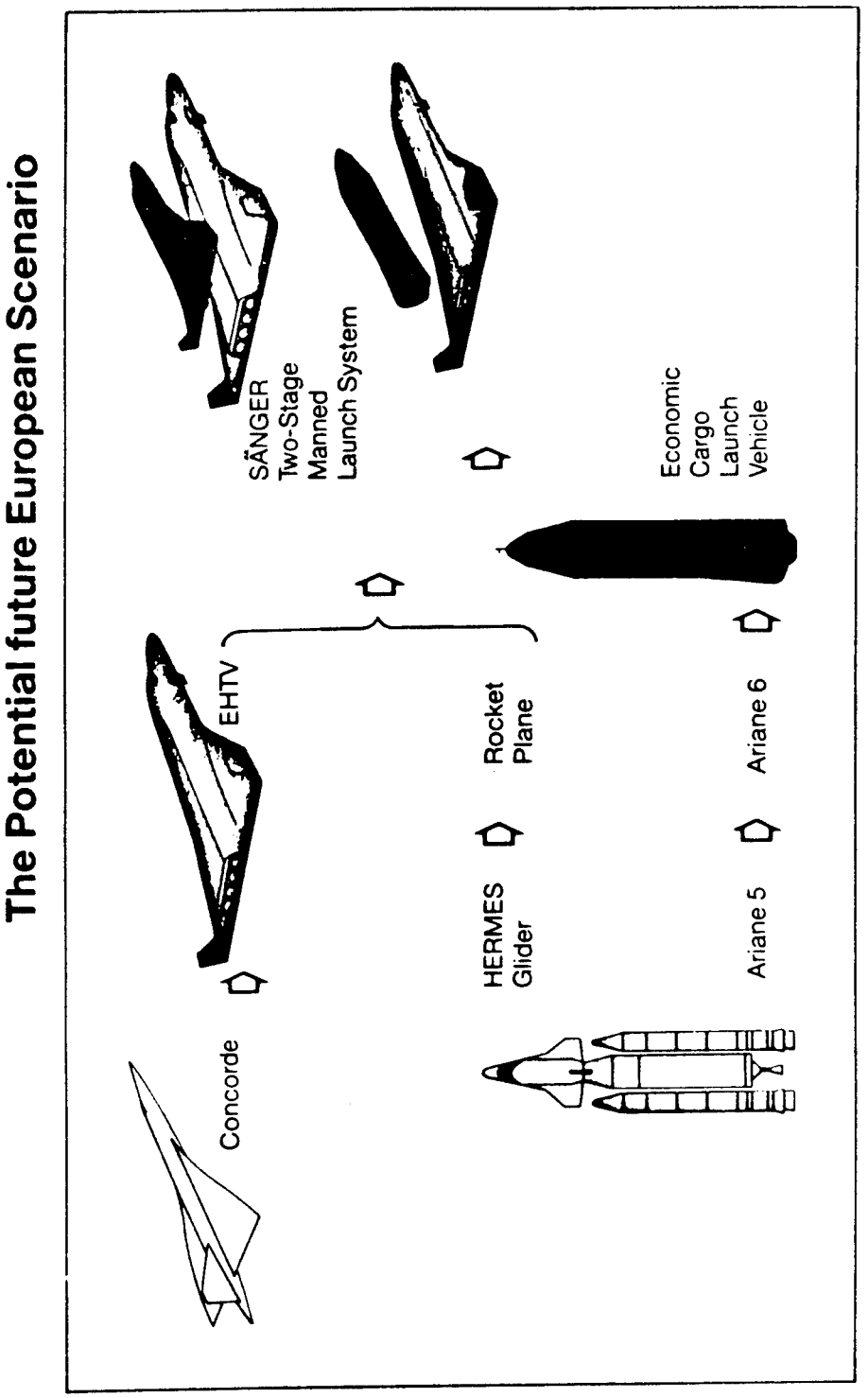
3. Orbital servicing mission for payload exchange, repair, or maintenance, carrying up to four astronauts and three tons into earth orbit (one ton into polar orbit).

4. Small cargo transport for carrying four tons of payload into a 200 km (124 mile) orbit.

5. Heavy cargo transport of up to ten tons of payload using the Economic Cargo Launch Vehicle payload stage.

Figure 12

THE FUTURE OF EUROPEAN AEROSPACE: A MBB VIEW



The combined weight of the EHTV plus HORUS is less than 500 tons, placing Sänger II roughly in the category of a Boeing 747-200 transport (833,000 lbs typical gross weight).

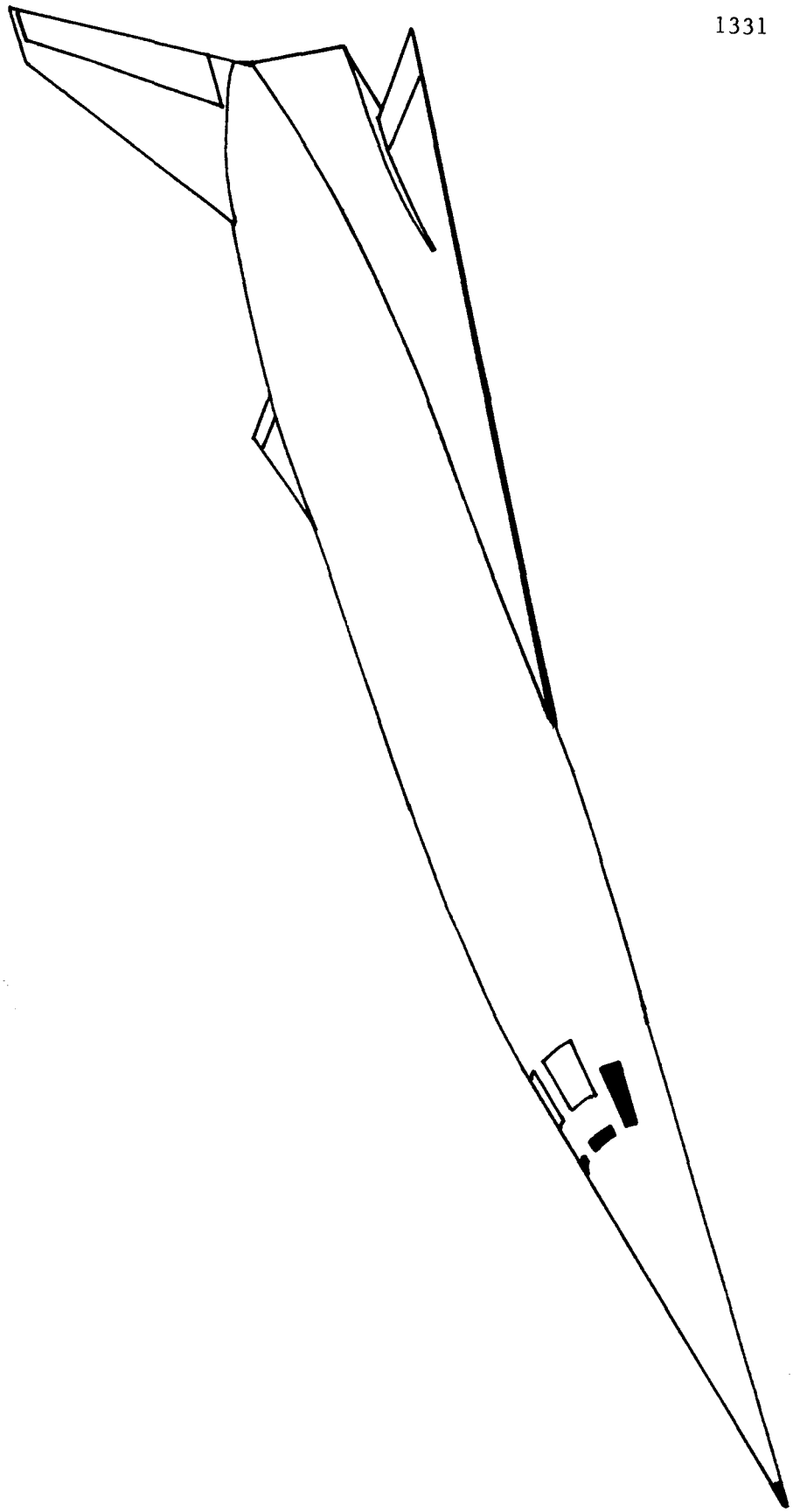
A typical Sänger II mission would involve takeoff and acceleration to Mach 6, cruise from Germany to the Equator, an easterly turn, and launching of the second stage at Mach 6 and an altitude of 35 km (22 miles). The launch vehicle would then return back to Germany, while the second stage payload vehicle would accelerate in 280 seconds to 8,530 meters per second (19,081 mph) at an altitude of 80 km (50 miles). The vehicle would follow a Hohmann transfer trajectory to a final orbital altitude of 400 km (248 miles). Thus, as can be seen, the first stage of Sänger II, though an advanced air-breather, is not comparable in terms of propulsion to the air-breathing single-stage-to-orbit schemes of either HOTOL or the American NASP. It is, in tradition TSTO fashion, "merely" a hypersonic launch platform for an orbital vehicle--either the winged HORUS or an expendable heavy lift upper stage. HORUS is intended to use advanced rocket propulsion, possess a payload bay compatible with requirements to carry up to twelve passengers or up to four tons of cargo, and will have a total weight of approximately 55 tons, including 40 tons of LH_2/LO_2 propellants. It will utilize a metallic multiwall advanced lightweight radiative thermal protection system. Like HOTOL and NASP, MBB hopes that Sänger II will be capable of operating from conventional airfields without the need for the complex launch facilities currently required of the American and Soviet Shuttles. MBB advertises the concept as "the only system to allow launches from European airports into all orbit planes," concluding that the project is "A European initiative proposed by Germany to secure future European technical competitiveness."[17] As with other ventures such as NASP and HOTOL, propulsion is Sänger II's greatest challenge, though not to the same degree as with the American and British ventures. Advanced studies applicable to Sänger II have been underway since the late 1970s on

rocket and air-breathing propulsion schemes by the Institut für Raumfahrtantriebe (Institute for Space Travel Propulsion Systems) of the University of Stuttgart, including some interesting combined systems applicable to vertical takeoff and vertical landing (VTOVL) schemes. Like HOTOL and the NASP, a decision to proceed with Sänger II or a system like it will be dependent upon a host of factors, only some of which are purely technological and scientific.[18]

The Path to NASP

America's National Aero-Space Plane (NASP) announced in January 1986, represents the most ambitious undertaking envisioned to date in the field of hypersonic flight and lifting reentry from space. Figure 13 shows a generalized NASP configuration. To understand the context in which NASP has been conceived, it is worth examining other related endeavors that have preceded it. As discussed throughout this study, the United States, via NASA, the Air Force, and private organizations, has had a continuing interest in developing hypersonic research vehicles. This interest generated such programs as the X-15, ASSET, and PRIME, and abortive efforts such as the X-20, the MRRV, and the X-24C/NHFRF of the 1970s. In 1971, the Flight Dynamics Laboratory (AFWAL FDL) had conceptualized the idea of a special small lifting reentry vehicle to be carried within the Shuttle's payload bay and then launched earthwards from orbit. This manned vehicle proposal received the name Maneuvering Reentry Research Vehicle (MRRV), and in 1972 and 1973, FDL supported their internal studies with contracts to Lockheed and McDonnell-Douglas examining the concept. In 1977, Alfred C. Draper, Paul Lane, and W. P. Zima of the FDL recommended development of such a craft as a bridge between the Shuttle and general hypersonic aircraft technology. The next year, Rockwell received AFWAL FDL contracts to study such a craft and, subsequently, NASA's Langley Research Center tested MRRV wind tunnel models.[19] (Even today, in the NASP concept-era, the idea

Figure 13

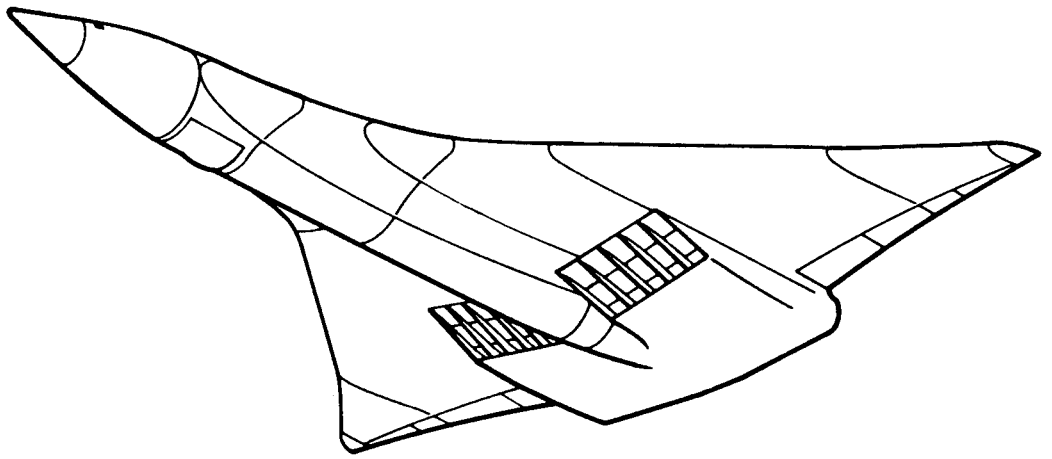


NATIONAL AERO-SPACE PLANE CONCEPT, 1986

of a lifting reentry research vehicle flown from the Shuttle has great appeal). As can be seen, then, FDL's interest in hypersonics certainly did not come to an abrupt end with the cancellation of the X-24C/NHFRF, though it necessarily continued on a lower level. Eventually, in the early 1980s, this continuing interest culminated in the on-going Boost Glide Vehicle (BGV) program: an idea for an approximately twelve-foot-long 1,200 lb. craft having an elegant delta shape somewhat reminiscent of a refined X-24B. Intended for flight testing a la ASSET and PRIME, the BGV offers several orders of magnitude levels of sophistication beyond these earlier efforts. Though some of BGV's program results may prove beneficial to development of NASP, BGV is a separate program run by the Flight Dynamics Laboratory with participation by the Defense Advanced Research Projects Agency (DARPA), and its overall goal is to demonstrate an integrated vehicle system that may lead to operational derivatives for military use. Currently, BGV concepts by Martin and General Dynamics are being studied prior to issuance of a go-ahead for development and eventual flight testing.[20]

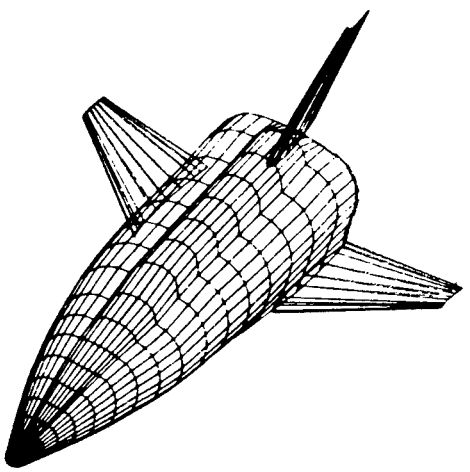
The most seductive vision of all, however, involved developing some sort of advanced single-stage-to-orbit winged spacecraft. By the late 1970s, the state of technology has advanced to the point where many of the obvious problems that had confronted the original Aerospaceplane program of the 1960s were largely relegated to the past. Significant challenges remained, primarily in the structural, propulsion, and computation fields. Even before Shuttle flew in 1981, agencies such as NASA, various individuals (such as Robert Salkeld), and aerospace companies, (particularly Boeing and Rockwell), were examining possible schemes for fully reusable SSTO spacecraft for the 1990s. A Rockwell concept is shown in Figure 13A, and a NASA conceptualization for a rocket-powered SSTO is shown in Figure 13B. Both were designed to support orbital construction of a proposed 10 million kilowatt Satellite Power System (SPS).

Figure 13A



ROCKWELL CONCEPT FOR SCRAMJET-POWERED SSTO LOGISTICAL TRANSPORT (1976)

Figure 13B



Langley Research Center
1990 technology

25 % reduction - structures
15 % reduction - subsystems

Dual expansion ratio SSME's (50/150)

Gas Generator O_2 / kerosene engine

Payload	**5 tons polar** **15 tons east**
Total weight	**1105 tons**
Dry weight	**88 tons**

NASA LRC CONCEPT FOR A ROCKET-PROPELLED SSTO LOGISTICAL TRANSPORT (1979)

Boeing, one of the major players in Aerospaceplane during the early 1960s, advanced the twin notions of a large Reusable Aerodynamic Space Vehicle (RASV) and a smaller 747-launched "space sortie vehicle" dubbed the Air-Launched Sortie Vehicle (ALSV), a forerunner of the subsequent Transatmospheric Vehicle (TAV) concept discussed subsequently. RASV proved to have immediate and significant influence upon Air Force research. In a 1979 memorandum to Air Force Chief of Staff General Lew Allen, Acting Secretary of the Air Force Hans Mark stated that:[21]

> I have been interested in the Reusable Aerodynamic Space Vehicle (RASV) concept for a number of years. Such a single-stage-to-orbit system has the potential of providing us with a manned platform that can be over any point on earth in less than a hour. The RASV could perform a wide variety of very important missions, including reconnaissance, rapid satellite replacement and space defense to name just a few.
>
> I would like to express my continuing support for the RASV efforts. As you know, both SAC [Strategic Air Command] and ADCOM [Aerospace Defense Command] have already addressed RASV requirements. In addition, I believe we have invested about $225K prior to FY 79, and plan to spend about $1M this year and $2M in FY 80 for RASV. Although the technology has a ways to go, I believe the potential benefits of such a system warrant this level of effort. I hope you will give this program your support as well.

Secretary Mark's memo triggered an immediate and emphatic response from the Air Staff; Maj Gen William R. Yost, DCS for Research, Development, and Acquisition and Director of Space Systems and Command, Control, Communications (C^3) wrote to Brig Gen Brien D. Ward, Director of Science and Technology at HQ AFSC that:[22]

> I think it is appropriate to develop a proper thrust and direction for the RASV program. While we clearly are not ready to embark on an acquisition program, a military spaceflight capability offers potential advantages that should not be ignored.
>
> My staff has suggested initiating a modest program to develop the technology base that would give the

> decision makers of the mid-1980's the option of pursuing an acquisition program if the threat and defense priorities so warranted . . . .
>
> There are many unanswered questions concerning this concept, but, when we consider the potential payoff, I feel a high leverage technology program may be a prudent investment.

Air Staff's obvious interest in efforts such as the Boeing-derived RASV fanned the smoldering embers left from episodes such as MRRV and the X-24C/NHFRF, and encouraged hypersonic proponents--particularly, as might be expected, those within AFWAL's Flight Dynamics Laboratory at Wright-Patterson AFB--to intensify their efforts to identify requisite technologies for both large and small hypersonic vehicles, and to define roles and missions in which they could make meaningful defense contributions. AFSC's Space Division initiated a technology research effort, termed Advanced Military Space Capability (AMSC), and in 1981, AFWAL's Flight Dynamic's Laboratory, working in partnership with SD, awarded one-year study contracts to General Dynamics and Rockwell for technology studies of small manned spacecraft based on two generic launch concepts: subsonic air launch and "staged" ground launch. Such studies were reminiscent of FDL's 1960's and 1970's study efforts and, indeed, were characterized by marked similarities to the earlier work in terms of vehicle concepts and anticipated mission models, though (as would be expected), with greater technical depth. FDL and SD were not alone in pursuing these studies; smaller-scale efforts were undertaken by other DoD organizations as well. For example, the Air Force's Rocket Propulsion Laboratory (AFRPL)* at Edwards Air Force Base contracted with Pratt & Whitney to examine whether a cluster of nine RL-10 LH_2/LO_2 engines (which P & W had developed for the

---

*On March 13, 1987, AFSC redesignated the AFRPL as the Air Force Astronautics Laboratory, effective March 22, 1987.

Centaur cryogenic upper stage program) could propel a small unmanned space sortie vehicle air-launched from a Boeing 747, and, if so, what the ideal configuration of the engine cluster should be.[23]

Lt Gen Lawrence Skantze, commander of Air Force Systems Command's Aeronautical System Division at Wright-Patterson from 1979 until August 1982, encouraged study efforts such as FDL's, and in January 1982, he tasked the ASD planning staff (ASD/XR) to investigate what ASD should be doing with regard to studying successors to the Space Shuttle: an important tasking that had profound implications upon future development of the TAV and NASP. Shortly after assuming command of ASD, his successor, Lt Gen Thomas McMullen, went even further. McMullen launched a major planning effort under the overall direction of Stanley Tremaine, then ASD's Deputy for Development Planning, for such craft. Tremaine subsequently dubbed these "Transatmospheric Vehicles" (TAV), since they hopefully could operate with <u>equal</u> efficiency both within the atmosphere and within space.

In September 1983, Tremaine wrote that:[24]

> We believe the Transatmospheric Vehicle Program is a unique potential application of atmospheric and exoatmospheric (space) capabilities. The thrust of our ongoing TAV investigation is to predict, project, and evaluate military missions for this type of system. Once established, these missions will be translated into conceptual TAV systems that use the latest in technology advancements and projections. An evaluation will be conducted into the merit of these conceptual TAV designs against alternative system applications.
>
> The operational concept for a TAV system will be one of the key elements of our investigation. Starting from some features of the present NASA Space Shuttle, we're planning for a military scenario that places more stringent technical requirements than the current Shuttle design can meet . . . .
>
> We see the specific advantage in the military application of a TAV system as its global response

> capability compared to current systems. A TAV could react quickly from the CONUS to any global incident with the versatility to provide reconnaissance, force projection, strategic defense, or interdiction support as desired.

Figure 14 shows possible launch configurations and vehicle concepts, together with a schematic view of the global nature of TAV operations as envisioned by ASD's planners. TAV could make use of a modified existing aircraft such as the Boeing 747, utilize an entirely new launch vehicle developed specifically for it, or be large enough and sophisticated enough to undertake a single-stage-to-orbit mission. (Eventually, all of these were examined in depth, including launching it from ground assisted takeoff devices such as surface-effect machines).

Phase I studies of the TAV concept had begun in May 1983 with the award of a $600,000 ASD contract to Battelle Columbus Laboratories of Columbus, Ohio. Battelle worked with Boeing, General Dynamics, Lockheed, and Rockwell to derive suitable concepts, and McDonnell-Douglas, on their own without participating in the Battelle effort, submitted their own TAV concept. Overall, Phase I resulted in no less than fourteen separate TAV vehicle concepts, as well as a general feeling that the concept did not (in the words of one participant) "violate any laws of physics." Three of these--by General Dynamics, Rockwell, and Lockheed--are shown in Figure 15. The legacy of Shuttle is clearly evident in the Rockwell and Lockheed concepts, while General Dynamics clearly shows the influence of FDL work dating to the 1960s and 1970s. Phase I ended in December 1983 after determining the basic feasibility of building such vehicles. Phase II, initiated in August 1984, emphasized determining the military effectiveness of the TAV. By this time, the AMSC effort had given way to AMST: Advanced Military Space Technology, not to be confused with the previous AMST program for STOL transports undertaken in the 1970's. FDL continued awarding study contracts

Figure 14

TAV OPERATIONAL CONCEPTS, 1983

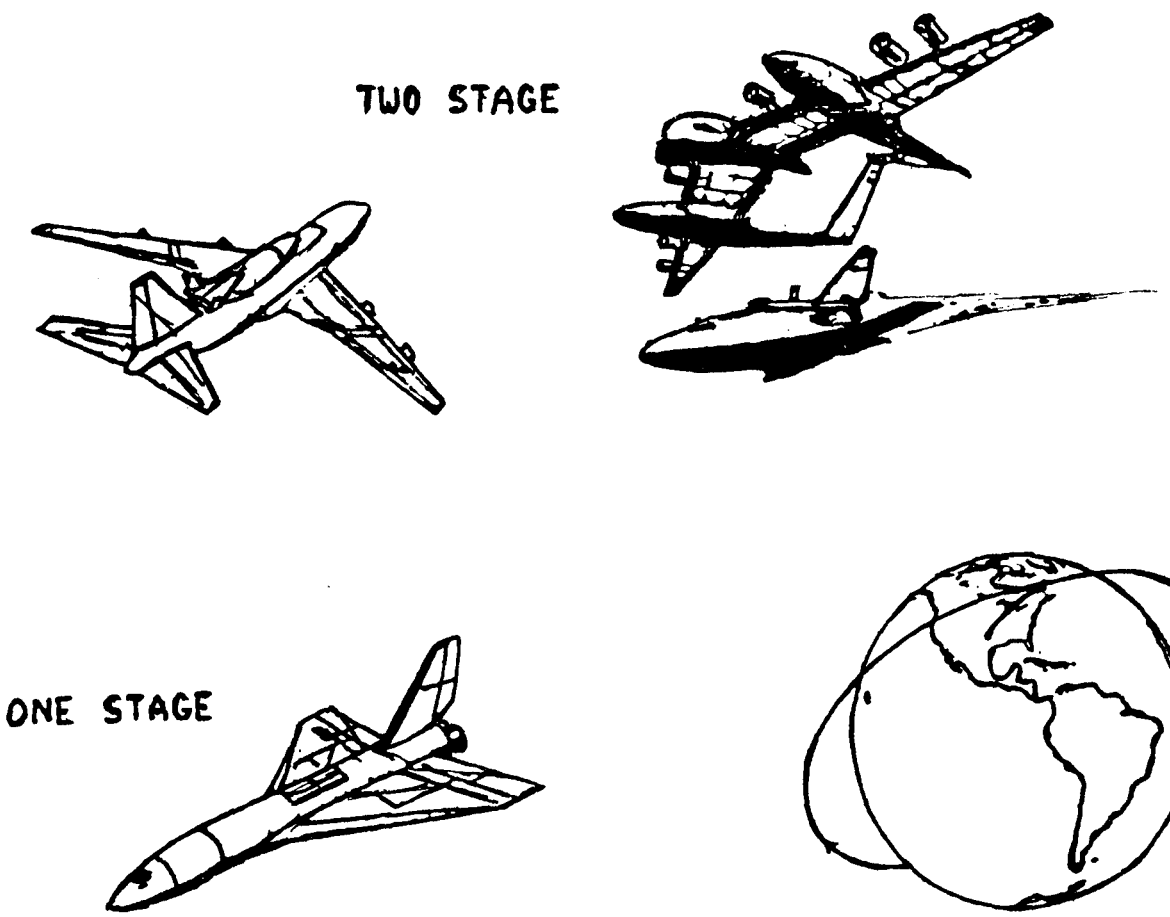


Figure 15

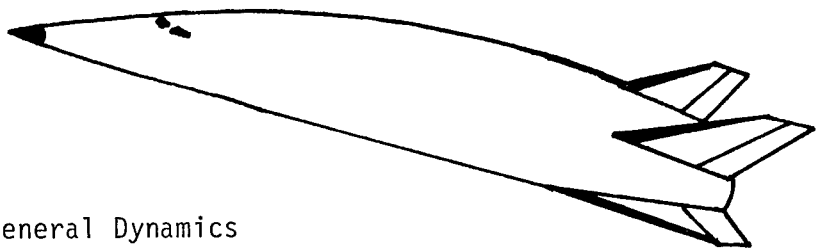


Fig. 15a: General Dynamics

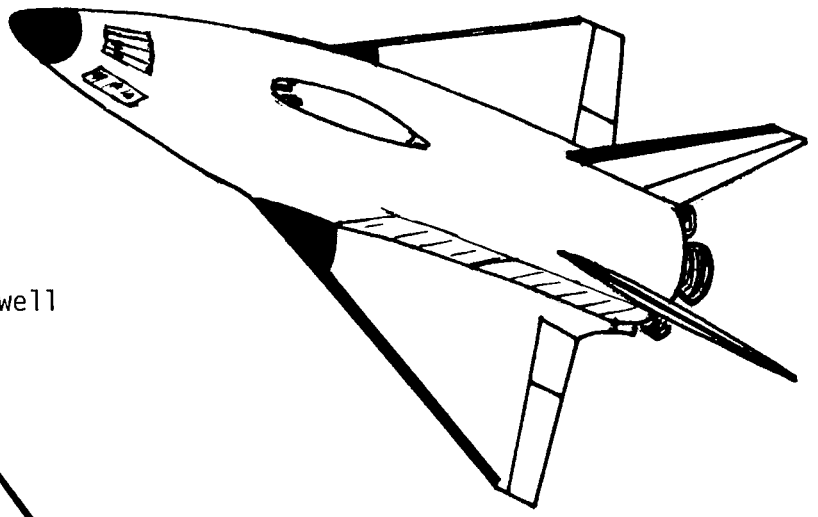


Fig. 15b: Rockwell

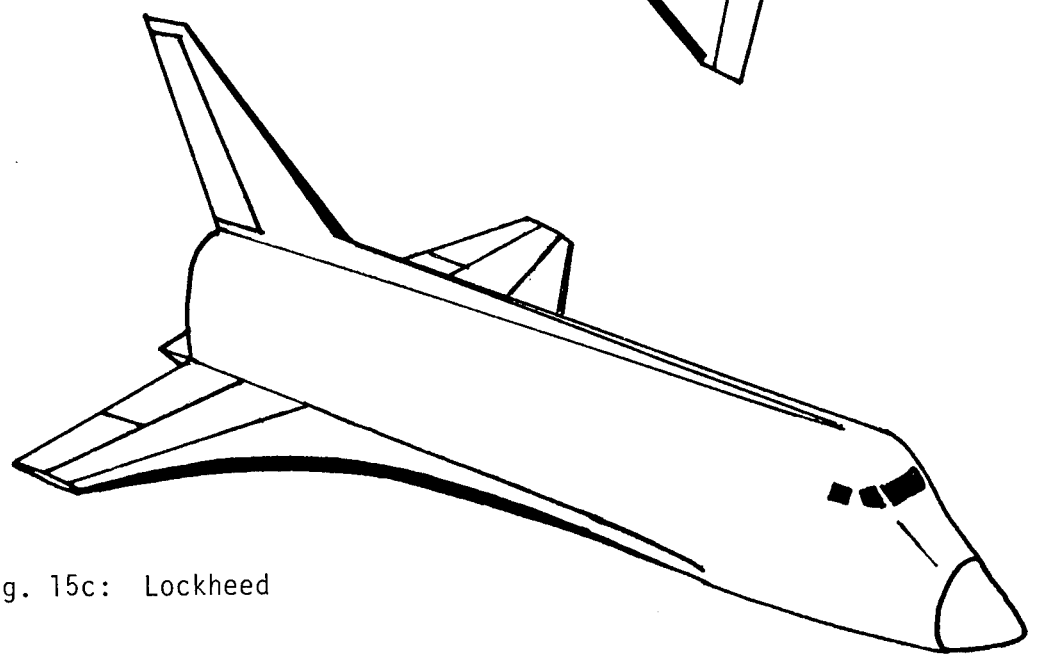


Fig. 15c: Lockheed

under AMST, beginning with one to Boeing's Space Systems Division in January 1984 to determine key aerodynamic and performance parameters associated with air-launching a so-called AMST/TAV orbiter from a carrier aircraft. TAV's Phase II program involved evaluating selected industry concepts against alternative solutions such as advanced aircraft, and further examining necessary technologies within the structures, materials, and propulsion fields that might be applicable to such craft. By the time of Phase II, TAV had grown into a major Air Force study effort, already characterized by overtones of growing into a major DoD-wide and even interagency study effort as well. ASD drew upon SAC, TAC, and Air Force Space Command in defining mission requirements and operational concepts, and representatives from HQ AFSC and organizations such as AFSC's Space Division, Armament Division, Air Force Space Command, SAC, the Navy, and NASA had formed a general-officer-level steering group to evaluate the military potential of the TAV idea.[25]

A series of coincidental events came together to boost the interest in TAV and other advanced orbital systems. President Ronald Reagan, on March 23, 1983, had projected an image of a nonnuclear defense against ballistic missile attack developed from the latest in existing and conceptualized high technology and based in space. Praised as the "High Frontier" by proponents and denigrated as "Star Wars" by its critics, this concept formed the basis for a broad research and study effort called the Strategic Defense Initiative (SDI). NASA pressed for and eventually got a Presidential commitment to build an orbiting space station. Planners generally conceded both concepts would require the services of advanced logistical spacecraft, which would possibly be needed as well for other roles, including defense of such space-based systems. Then, too, the obvious European, Asian, and Soviet interest in advanced orbital systems including winged reentry vehicles further added structure to arguments in favor of TAV and RASV-type systems.

ASD envisioned the TAV as being an aircraft system capable of operating with equal efficiency in space or in the upper atmosphere. This was an important point, and applicable also to later studies such as the NASP and the European HOTOL: the notion of not merely surviving in the upper atmosphere as with a Shuttle's "high alpha" (high angle of attack) reentry, but, rather thriving in the upper atmosphere--maneuvering and making course and orbital changes on a scale not possible with the Shuttle, and doing so via aerodynamic and propulsion control inputs. While TAV might operate in the environment of the Shuttle, it would not be necessarily constrained to using the same technological approach; in short, TAV would not simply be a "small" Shuttle. In fact, TAV would not be all that small in any case, with an estimated "GLOW" (gross lift-off weight) on the order of one million pounds or even more. Opinion seemed split on the degree to which TAV could use "off the shelf" technology or would be dependent upon exploitation of new technological thrusts. While its propulsion system (at least at first) could be relatively "conventional" (for example, a derivative of the Shuttle's own main engines), its structure would not be so traditional. Industry experts forecast incorporating hot structure technology using heat pipes to transfer and dissipate hypersonic heating effects. Some even recommended that its propulsion system should be some sort of complex combined cycle air-breathing and rocket system.[26]

TAV Phase II's emphasis upon determining military effectiveness resulted in a contract from ASD's Deputy for Development Planning in August 1984 to Science Applications, Inc. of Dayton for a twelve-month effectiveness investigation. For its part, Air Force Space Command envisioned the TAV fulfilling four key military space missions: Force enhancement (including global reconnaissance, surveillance, and C^3), Space support (including satellite insertion, rendezvous, inspection, servicing, repair, retrieval, recovery, support of space stations including resupply and space rescue, and acting as a small space platform

for scientific and military research), Space control (including protection of U.S. space assets, satellite attack warning, satellite defense, and anti-satellite operations), and, finally, Force application (combat operations including strategic offense, strategic defense of North America including ballistic missile defense, interdiction in theater conflicts, and "surgical application of force" to protect U.S. interests and avoid crisis escalation). "Force application," one Space Command TAV paper stated, "does not infer the 'weaponization of space'; simply the use of the high altitude flight regime for the operation of military aerospace vehicles," though such a distinction smacked of strained scholasticism.[27]

Achieving a TAV and hopefully having it in service in the early 21st century would be no easy matter. Space Command believed a single-stage-to-orbit configuration the "most desirable," but concluded that "the technology projected to be available in the 1990s will likely dictate a two-stage system."[28] (Though this conclusion, as will be seen, quickly changed). Planners envisioned the TAV to be the size of a small airliner, with a 1,000-2,000 cu. ft. payload bay having a capacity of between 10,000-20,000 lbs, and having a gross liftoff weight of approximately 1 to 1.5 million lbs. It would possess a propulsion system capable of ten to twenty flights between overhaul though, in the interests of time and costs, the first generation TAV was anticipated as using up-rated Space Shuttle Main Engines (SSME), with the SSME's being replaced as later technology engines, possibly burning a mix of liquid oxygen with propane or RP-1, became available. Its structure would employ advanced materials and a simplified thermal protection system, and its avionics would be hardened against nuclear blast-generated electromagnetic pulse (EMP) propagation, fault-tolerant, and "gracefully" degradable. A system incorporating technologies such as these would, Space Command believed, possess great utility. The key was developing a system survivable in a wartime situation, and possessing the

ability to undertake "on-demand" all-weather, near-autonomous operations with a high level of mission reliability, and simplified logistics support. For example, in contrast to the estimated 200,000 man hours per launch required of the Shuttle, the TAV would be able to be mated with a payload within one day. Launch preparations would be simple enough so that the TAV could be launched under warning of a ballistic missile attack. Once aloft, TAV could undertake on-orbit maneuvering using its propulsion system, and use combinations of propulsion with aerodynamic turning maneuvers in the atmosphere to transition from space into the atmosphere and then reinsert itself back into space. TAV would not be restricted to operations from a few launch points such as Kennedy Space Center or Vandenberg. It would possess a cross-range in excess of 1000 miles, and, following recovery, would require a maintenance level no greater than "today's more complex airplanes" (presumably a reference to vehicles such as the Lockheed SR-71 Blackbird). Indeed, Space Command anticipated that in a wartime situation, TAV could fly one mission every other day, with a limited "surge" capability of one mission per day, even given requirements to operate from austere environments with only "blue-suit" maintenance. "The TAV," Space Command concluded (perhaps with greater optimism than warranted) "will be treated just like any other military airplane, with few exceptions. . . . The question is not whether there will be a TAV--only when."[29]

TAV resulted in a focused study effort at Aeronautical Systems Division that, in a way, corresponded in value to NASA's internal design study efforts at the Manned Spacecraft Center on the earlier Shuttle. TAV posed challenges in materials and structures, propulsion, supportability, and called into question (yet again) the role of humans in space operations. Technology "drivers" were problems stemming from the operational environment (including designing a structure to withstand the aerothermodynamic effects, coupled with having a structure

complicated by internal storage of super-cold cryogenic tankage), and the need for aircraft-type operations (characterized by rapid turnaround and minimum maintenance). The materials and structures challenge involved developing a lightweight, simple structure allowing non-destructive inspection techniques, durable enough to minimize maintenance, and with an external surface capable of withstanding the thermal environment of reentry. Planners anticipated using advanced metallic and composite materials, and envisioned fabrication of the structure to be "a key technology [challenge] area." Accordingly, AFWAL's Materials Laboratory studied new materials technologies, and the Flight Dynamics Laboratory examined development of structural concepts themselves. Propulsion challenges involved engine technology not only for the TAV itself but for its launch vehicle (if any). The Aeropropulsion Laboratory initiated studies on air-breathing propulsion systems for launch aircraft, including possible hypersonic engines. TAV's own propulsion work concentrated on using (at least at first) planned uprated models of the Shuttle's SSME, and in these study efforts, the Aeropropulsion Laboratory was able to draw upon associated work at NASA and the Air Force Rocket Propulsion Laboratory. Researchers investigated using more energetic fuels and oxidizers in place of the virtually standard LH_2/LO_2, including so-called "metastable" helium, which promised six times the efficiency of the hydrogen-oxygen mix, and thus would permit a significant reduction in the size and weight of the TAV while still retaining its desired payload capacity. Supportability issues involved three major questions: fueling operations involving cryogenics and oxidizers; ground handling and turnaround; and maintenance. Planners worked closely with representatives of the Air Force Acquisition Logistics Center. On the first issue, planners Tremaine and Dr. Jerry B. Arnett concluded that "Provisions for manufacture, transport, storage, and handling of large quantities of cryogenics need to be explored in depth," since TAV would be expected to operate from a number of

airfields, and not just from a few well-developed sites. They also emphasized aircraft-type ground handling (such as provisions to use standard towing techniques to move an unfueled TAV) offering minimal impact upon normal airfield operations. Finally, the desire for simplified maintenance acted as a driver for other TAV technical requirements such as materials and propulsion. The human factor involved the issue of whether or not TAV should be a piloted system. Planners concluded that if TAV had been intended for extended operations in space, or being launched only once, then maybe an unmanned system relying heavily upon advances in so-called Artificial Intelligence (AI) technology would suffice. But TAV was instead expected to operate nearly autonomously on many missions of brief duration. While TAV would, of necessity, incorporate some degree of AI, Tremaine and Arnett stated that "Our perception to date is that man's capability for real-time fusion of information, his ability to respond quickly to the unexpected and his decision-making ability are essential to a military mission."30

In December 1984, the Deputy for Development Planning at Aeronautical Systems Division established a TAV Project Office under the direction of Lt Col Vince Rausch, with Dr. Jerry Arnett serving as TAV program manager. The new project office, intended to "streamline activities related to long-term, preconceptual design studies" made use of full-time co-located staff members to ensure close ties with ASD's Deputy for Engineering, Deputy for Acquisition Logistics, with the Air Force Acquisition Logistics Center, and with the Air Force Aerospace Medical Research Laboratory, the latter organization working on crew station design. The office could draw upon AFWAL and AFRPL for additional technical support, as well as upon the Deputy for Development Planning's own Mission Analysis and Design Analysis Directorates. TAV thus had moved well along the track from being merely a discussion item to being a full-fledged ASD project. At this point, however, several external developments intervened to

influence the subsequent course of TAV work and, eventually, lead to its termination and absorption of the TAV project office staff in toto within the National Aerospace Plane Joint Program Office (NASP JPO) when the JPO was established at Aeronautical Systems Division in January 1986.[31]

One of these interventions--the so-called Copper Canyon effort--had a direct impact upon ASD's work on TAV and will be discussed subsequently. Another, affecting more the general climate of Air Force-NASA thought concerning advanced logistical spacecraft, was the joint DoD/NASA National Space Transportation and Support Study, initiated in February 1985 and completed with submission of a task team report in May 1986. In May 1985, President Ronald Reagan signed a National Space Security Directive (NSSD) entitled "National Security Launch Strategy," which had been drafted the previous February. The NSSD mandated a joint DoD/NASA study on the development of second-generation space transportation systems, including considering technical options and potential user needs. The directive specified a one-year effort having four goals: to satisfy the future needs of users, reduce the costs of space operations, develop "flexible and robust" space transportation systems, and "maintain world leadership in space transportation." The team operated under a joint steering group chaired by Edwin C. Aldridge, Jr., Undersecretary of the Air Force, and NASA's Rear Admiral Richard H. Truly, USN, Commander of the U.S. Navy's Space Command as well as a veteran Shuttle astronaut. (Subsequently, Lt Gen Bernard Randolph, USAF, replaced Aldridge, and, when NASA appointed Truly as Associate Administrator for Space Flight and thus chief of the Shuttle program in the wake of Challenger's loss, NASA replaced Truly with Jesse W. Moore). Other group members were DoD's Lt Gen Randolph, Lt Gen James Abrahamson (of the SDIO effort), and Dr. Larry Woodruff, and NASA's Dr. William Lucas, Norman Terrell, and Dr. Raymond Colladay.[32]

The joint DoD-NASA study focused on needs from 1995 to beyond 2010. DoD emphasized emerging Strategic Defense Initiative (SDI) requirements, and NASA, for its part, consulted with the National Commission on Space to support its vision of the civilian space future. (The National Commission on Space, appointed by the President at the behest of Congress, consisted of fifteen distinguished engineering, scientific, and political pioneers, administrators, astronauts, researchers, and military leaders--including such individuals as Gen Bernard A. Schriever, USAF ret., Neil A. Armstrong, Dr. Luis W. Alvarez, and Dr. Gerard K. O'Neill--examining America's long-term space future; they issued their summary report at the same time as the joint DoD-NASA panel released its own in May 1986, four months after the loss of Challenger. The National Commission's conclusion applicable to the National Aero-Space Plane will be mentioned subsequently).[33] The joint DoD-NASA study examined combinations of mission needs ranging from "constrained" to "aggressive," ranging from projected use models delivering 1 million lbs. into orbit per year (assuming 80% of this would be civil payloads) to 3.8 million lbs. into orbit per year (assuming 75% of this would be military.) Partly reusable, fully reusable, and advanced expendable launch vehicle concepts were explored, and within this general classification, sub-categories of vehicles examined included flyback boosters with cargo canisters or flyback orbiters, SSTO rocket-propelled winged logistical vehicles, and SSTO advanced air-breathers, such as the contemporaneously evolving National Aero-Space Plane. (A range of other concepts including space-based fully reusable "aerobraked" systems were also examined, though they fall outside the intent of this discussion). Figures 16a, b, and c, drawn from the report, relate key technologies required for an improved Space Shuttle and so-called CELV (Complementary Expendable Launch Vehicles), partially reusable cargo vehicles, and manned fully reusable rocket-powered and air-breathing vehicles. Figure 17 shows how generic technology efforts, focused technology efforts, and

Figure 16

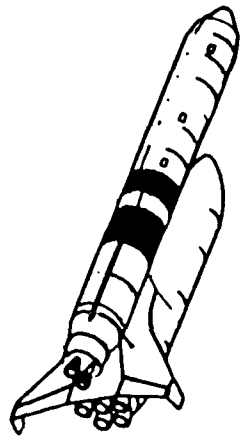
KEY TECHNOLOGIES: IMPROVED SHUTTLE/CELV

- EXPERT SYSTEMS/ARTIFICIAL INTELLIGENCE
- FAULT TOLERANT AVIONICS
- ADVANCED PROPULSION SYSTEMS
- AUTONOMOUS GUIDANCE. NAVIGATION AND CONTROL
- PRECISION RECOVERY SYSTEM

Fig. 16a

KEY TECHNOLOGIES: PARTIALLY REUSABLE CARGO VEHICLE

- EXPERT SYSTEMS/ ARTIFICIAL INTELLIGENCE
- AUTOMATED SOFTWARE GENERATION
- FAULT TOLERANT AVIONICS
- COMPUTER INTEGRATED MANUFACTURING



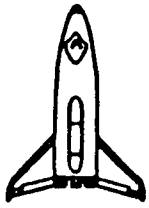
- ADVANCED PROPULSION SYSTEMS
- ROBUST THERMAL PROTECTION SYSTEMS
- AUTONOMOUS GUIDANCE NAVIGATION AND CONTROL
- PRECISION RECOVERY SYSTEMS

Fig. 16b

TECHNOLOGY REQUIREMENTS FOR ADVANCED SPACE LOGISTICAL VEHICLES

KEY TECHNOLOGIES: MANNED FULLY REUSABLE LAUNCH VEHICLES

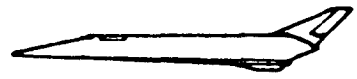
ROCKET



1 STAGE

2 STAGE

AIRBREATHER

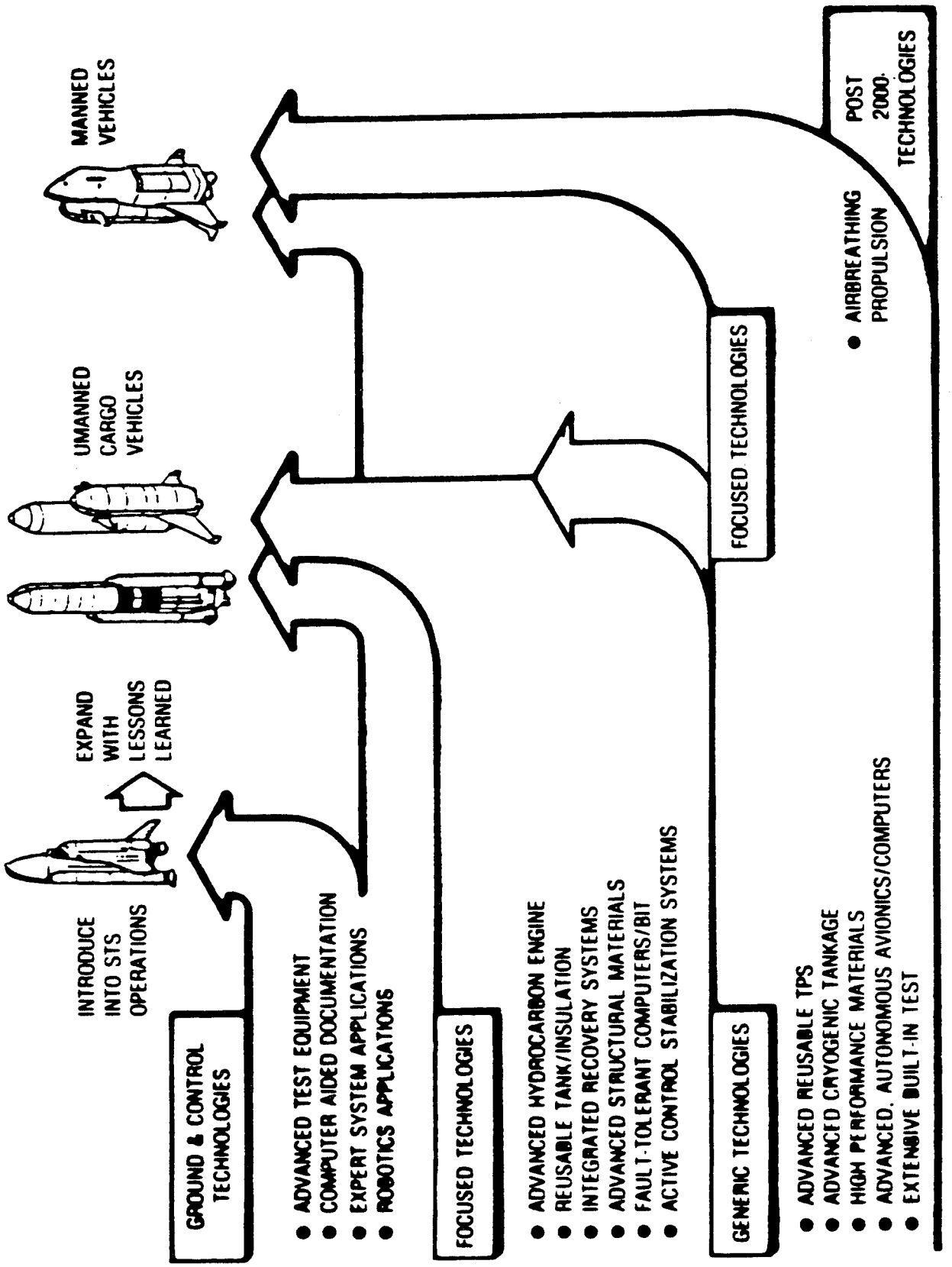


1 STAGE

- EXPERT SYSTEMS/ARTIFICIAL INTELLIGENCE
- AUTOMATED SOFTWARE GENERATION
- FAULT TOLERANT AVIONICS
- ADVANCED PROPULSION SYSTEMS
- LIGHTWEIGHT HIGH TEMPERATURE STRUCTURES
- ROBUST THERMAL PROTECTION SYSTEMS
- AUTONOMOUS GUIDANCE, NAVIGATION AND CONTROL
- INTERDISCIPLINARY ANALYSES

Fig. 16c

Figure 17



TECHNOLOGY INTEGRATION/APPEARANCE ON ADVANCED SPACE LOGISTICAL VEHICLES

advanced ground and control technologies could be expected to be integrated into new operational systems, beginning first with improvements to the STS, then with development of new unmanned launch vehicles (including CELV and fly-back systems) and, finally, with advanced manned TSTO and SSTO systems in the post-2000 timeframe. While this joint DoD-NASA study could not be as exhaustive as, for example, the AACB report of the mid-1960s, due to the time constraints placed upon it, it did serve to identify general technical and "architectural" requirements deemed necessary for further work, as well as to examine potential mission models and generate a basic timeline for anticipated future developments. By its unspoken but obvious endorsement of new generations of winged logistical spacecraft, including SSTO rocket or air-breathing propelled systems, this study played an important role in endorsing the concept of a National Aero-Space Plane, which President Reagan endorsed in his State-of-the-Union address on February 4, 1986. Subsequently, when the National Commission on Space issued its own report in May 1986, the same month that the joint DoD-NASA study effort ended, their report "strongly" recommended that "The technology advances required for aerospace plane development and flight test receive the highest national priority."[34]

By early 1986, TAV as a program was gone, replaced by the newly emergent National Aero-Space Plane (NASP); the entire TAV staff transferred into the NASP JPO. This did not mean that the interest in TAV missions or applications had gone as well, only that the TAV programmatic effort had been overtaken by a new major research and development initiative that required the concentrated application of all available resources, and not the kind of disjointed effort that might result if TAV remained on-going parallel to the more ambitious and demanding NASP. The transition of TAV into NASP is an interesting one, and a full appreciation requires some comprehension of the existing hypersonic technology base in the mid-1980s.

As a result of industry, Air Force, and NASA studies, a broad (if somewhat immature) technical base relating to TAV/NASP-type vehicles existed by 1985, including specific tools permitting analysis of aircraft using computational techniques in addition to the usual laborious tunnel testing. The three major fields in which significant work was occurring-and needed-were structures, computational aerodynamic simulation, and hypersonic propulsion.

The materials and structures story was, in some ways, mixed. Hot structure concepts had appeared as early as the 1950s, but technical practicality had always been a problem. Then, with the Shuttle itself, designers opted for a conventional aluminum airplane approach using refurbishable ceramic-based tiles for its thermal protection system (TPS). From 1970 onwards, then, little work had been undertaken on advanced structures and materials for hypersonic flight--an (at first) puzzling situation, given the sustained (if then-low key) interest in such flight, and also the fact that the 1970s were a time of profound revolution in flight structures: the onset of the composite era, the first great structural advance since the advent of the all-metal airplane in the late 'teens and 1920s. Nevertheless, as a joint NASA-AFWAL survey paper concluded, structures and materials technology had stagnated "Due to accelerating costs and formidable technical challenges."[35] Despite this, some significant progress had been made in key technical areas, including composite and high temperature structures, TPS approaches, concepts for hypersonic air-breathing propulsion structures, and loads and heat transfer analysis (the latter advances being supported by improved high temperature ground test techniques). Most of these advanced concepts involved incorporating polymer matrix composites, advanced aluminum and titanium alloys, aluminum and titanium metal-matrix composites, superalloys, and carbon-carbon composites, in airframe structural design. Metallic research had been slowed by the 1971 decision not to proceed with a Mach 2.7 American SST, and the decision not to use a metallic-base TPS on

Shuttle; carbon-carbon composites had matured in the meantime to the point where they offered significant promise due to their retention of high strength at high temperatures. When combined with advanced fabrication techniques, the advances in materials now offered designers an opportunity to fabricate structures cheaply and with higher geometric efficiencies than possible previously. (Samples are shown in Figure 18a). For example, one NASA Langley Research Center study effort examined a carbon-carbon Shuttle body flap, comparing it to the baseline body flap fabricated from aluminum and covered with heavy reusable surface insulation tiles. The carbon-carbon concept (shown in comparison with the aluminum-tile flap in Figure 18b) weighed only 610 lbs. compared to the 1460 lbs, for the insulated aluminum flap.[36]

Metallic and carbon-carbon TPS concepts, and structural concepts for incorporating supercold cryogenic propellant tankage within the flight vehicle itself rather than in a Shuttle-like external tank offered more practical promise in the 1980s than at previous times. Some specific proposals included a titanium multiwall approach (layers of dimpled titanium foil bonded at the dimples to flat Ti foil sheet), and a superalloy honeycomb concept (consisting of an Inconel 617 honeycomb surface panel separated by a layer of fibrous insulation from an inner titanium honeycomb panel). In both the Ti multiwall and superalloy concepts, the structures were formed into 12 in. by 12 in. square panels, with sealed edges. A carbon-carbon TPS concept termed "multipost" consisted of a rib-stiffened carbon-carbon sheet "attached to the vehicle primary structure by posts with fibrous insulation packaged in a ceramic cloth between the [carbon-carbon] panel and the vehicle structure."[37] Carbon-carbon offered considerable promise for TPS applications, but researchers recognized that it posed challenges in forming very thin sections (such as control surfaces), and had potentially "very serious" oxidation problems in high-temperature flow environments as well.[38] As the joint NASA-AFWAL materials and structures survey stated, "Since reusable

Figure 18

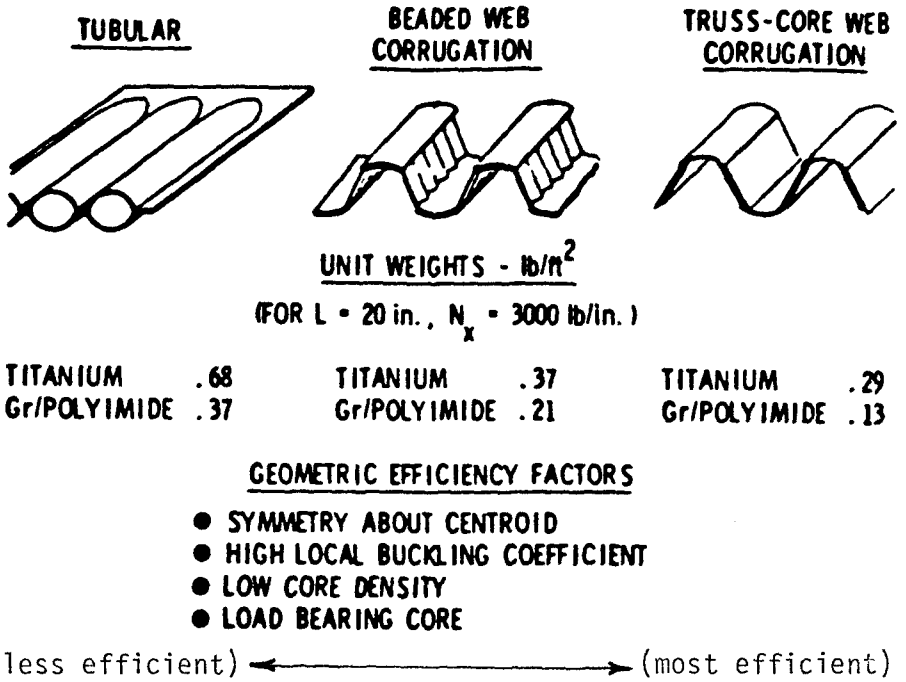


Figure 18a: Advanced Structural Concepts Showing Relative Efficiency

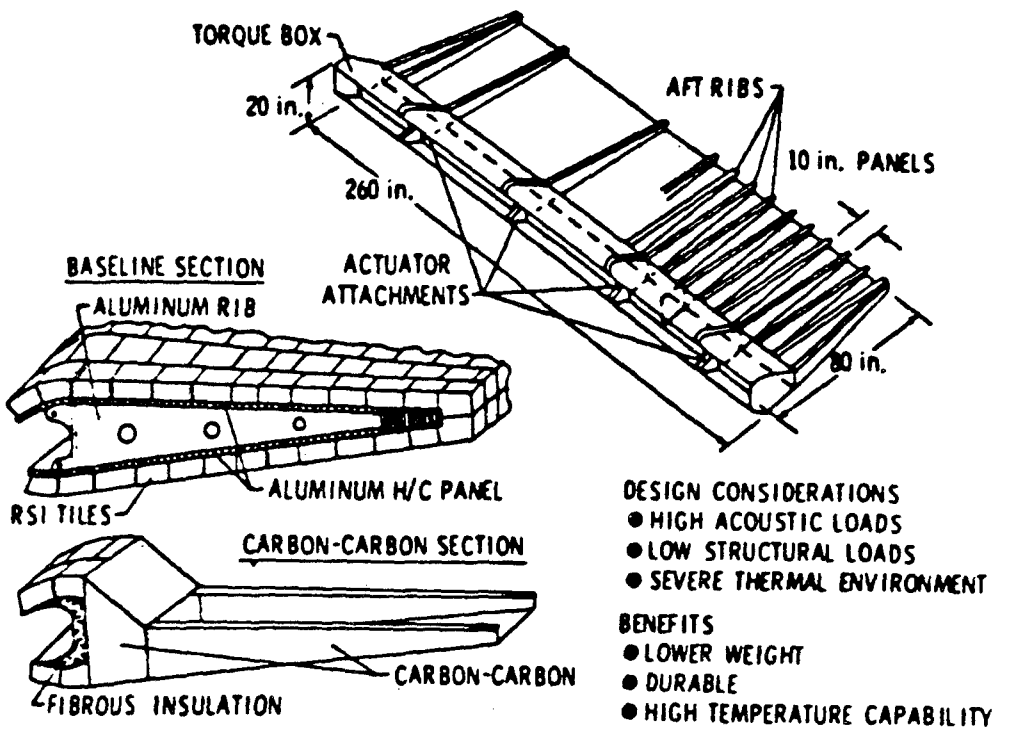


Figure 18b: Carbon-carbon vs. Insulated Aluminum Body Flap

launch systems will become virtually flying cryogenic tanks, the design of such structure will dominate the airframe structures effort for such vehicles."[39] The problem of simultaneously storing LH_2 at a temperature of -423 deg. F. in a structure being exposed to temperatures up to as much as 2,000 deg. F. can well be imagined. Considerable thought had been given (despite the low general level of structures research for hypersonic vehicles in the 1970's) to both insulated and hot-structure design approaches. Three of these concepts--an integral tank/fuselage hot structure, and integral and non-integral tank/structure/TPS concepts--are shown in Figures 19a and 19b, and make interesting comparison with the HOTOL scheme illustrated in Figure 6b. The integral tank/fuselage hot structure approach involved a René 41 skin on the hotter windward side of the vehicle, slotted to help relieve thermal stresses (the slots would be nearly closed when the surface temperature reached 1,400 deg. F.), with a titanium skin on the leeward surface. Tension struts would help relieve pressure loads over the non-circular portion of the structure. The integral and non-integral tank/structure/TPS concepts both made use of fibrous insulation to reduce heating to the tank; Figure 19b shows the specific technical differences between these two approaches.[40]

Air-breathing propulsion systems posed special structural challenges, because of the intense thermal environment encountered. Earlier, during the NASA Hypersonic Ramjet Experiment program, researchers had used liquid hydrogen circulation cooling of the HRE engine to reduce structural temperatures. To their dismay, they found that they needed <u>three times</u> as much hydrogen for <u>cooling</u> as for <u>fuel</u>; additionally, the HRE had a low thermal fatigue life of only 135 operations. By the mid-1980s, researchers envisioned using regenerative cooling--as with the traditional liquid-fueled rocket engine--to cool all engine surfaces wetted by the hypersonic airstream. Cooling scramjet fuel-injection struts constituted a major headache; as the joint NASA-AFWAL structural survey concluded:[41]

Figure 19

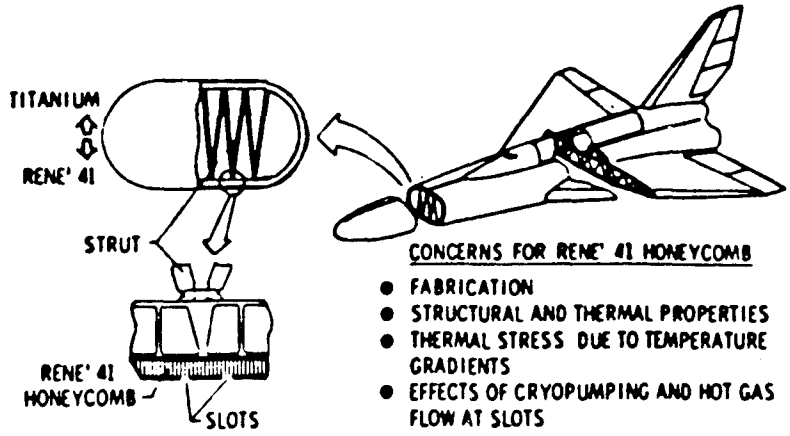


Figure 19a: Integral tank/fuselage hot structure

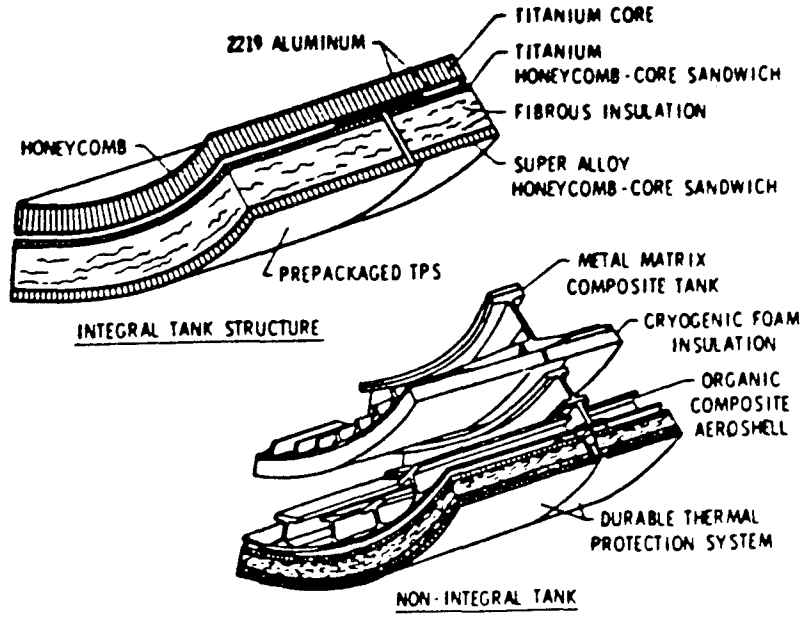


Figure 19b: Integral and Non-Integral tank/structure/TPS structural concept

> The fuel-injection struts presented the most formidable cooling and structural problems. The struts must simultaneously support a large side load, contain high-pressure hydrogen at two temperature extremes, and withstand the high thermal stresses resulting from complex aerodynamic heating as well as convective heating from the hot hydrogen in the internal manifolds. To compound these problems the cross sectional area and contour cannot be altered without significantly changing the engine propulsion performance.

Benefiting the development of new structural materials and concepts for advanced logistical spacecraft were advances within the computational and analysis fields made during the 1960s and 1970s, particularly finite element techniques for structural analysis and finite difference techniques for heat transfer analysis, and the expansion of this work to include integrated flow, thermal, and structural analysis. The computational methods assisting structural analysis were matched by an equivalent computational revolution affecting aerodynamic analysis, a product of the increasing sophistication of new technology computers. The 1970s had witnessed the rapid development of the field of computational fluid dynamics, and it was this area that, in the late 1980s, was judged the "principle" means of assessing the NASP's anticipated performance and aerodynamics. Computational fluid dynamics had quickly advanced to the point where it was a full-fledged partner to the traditional wind tunnel, and its adherents went even further, deeming "Computational aerodynamic simulation" as "a valid inexpensive alternative to wind tunnel testing of new aircraft and aerospace designs."[42] In 1985, a team led by Dr. Joseph J. S. Shang of the Computational Aerodynamics Group of the Flight Dynamics Laboratory's Aeromechanics Division undertook a complete aerodynamic simulation of the X-24C-10D reentry shape including construction of the shockwave pattern around the craft, location of hot spots, and determination of air particle stream paths around the vehicle.[43] This ability to generate and understand the flow field existing around highly

maneuverable reentry vehicles held particular significance for TAV/NASP-like concepts, as well as for more conventional advanced atmospheric aircraft such as fighters and bombers. When emergent computational simulation techniques were matched to advanced computational equipment such as the CRAY II supercomputer, the resulting analysis power gave researchers tools and abilities unknown less than a decade before, when Shuttle was going through its own evolutionary development. (NASA, in fact, had acknowledged this by establishing a Numerical Aerodynamic Simulation Facility at the agency's Ames Research Center at Mountain View, California).

Hypersonic air-breathing propulsion, like structures, had been an area that had not undergone a great deal of development during the 1970s, since it could not compete with the large liquid-fuel rocket engines developed for vehicles such as the Apollo-Saturn in the 1960s or the Shuttle during the 1970s. As the 1970s advanced (particularly following the enunciation of the airframe-integrated scramjet using hydrogen fuel, having fixed geometry featuring pronounced sweepback combined with a rectangular module shape), the prospects for practical hypersonic air-breathing engine systems seemed much improved, even allowing for the associated problems of keeping the engine from burning itself up due to the anticipated severe thermal environment. Traditionally, propulsion has been the most important "pacing" factor in flight development, since before the Wright brothers themselves, and this would not change with the hypersonic frontier. Successful demonstration and development of an airbreathing hypersonic propulsion system, however, could have applications for a TAV/National Aero-Space Plane, a hypersonic transport, or applications for vehicles such as hypersonic interceptors and missiles. Any successful hypersonic propulsion system would require a number of special features not previously encountered in flight vehicle design. A 1986 paper by Dr. William Heiser, Director of Aerojet General's Propulsion Research Institute, listed nine significant requirements for a hypersonic air-breathing propulsion system:[44]

-large air capture area inlets with high pressure recovery
-stable, started inlet flow with combustion isolation
-high heat capacity, cryogenic fuels
-high temperature, fuel-cooled structures
-high response, airframe integrated controls
-variable geometry
-large expansion nozzles
-multi-mode operation
-supersonic combustion

To successfully develop such a system would involve confronting a number of technology issues involving system integration, and inlet, combustor, and nozzle design. One of the most significant system integration issues involved thermal management: the need to maintain temperature control to prevent material failures and degradation of system cycle life, the ability to recoup heat into the fuel to generate maximum performance, and the requirement to actively cool both the airframe and the propulsion system. Doing this, of course, would demand thorough knowledge of the thermal environment, coupled with insightful design, and advanced materials such as ceramic composites, metal-matrix composites, carbon-carbon structures with coatings, and materials possessing integral cooling systems. On top of this were significant control system technology requirements, including integration of engine controls with vehicle controls, preventing inlet "unstart" (a significant problem on earlier high-speed programs such as the XB-70A and A-12/YF-12A/SR-71 Blackbird), controls for engine modal transitions (such as going from an airturboramjet mode to a scramjet mode and thence to a rocket propelled mode), and engine monitoring and diagnosis systems. Overall, Dr Heiser concluded that hypersonic propulsion would involve meeting nine specific challenges:[45]

- stable operation over a wide operating range
- high performance over a wide operating range

- thermal management including active cooling
- advanced composite materials
- high response control system
- low density, cryogenic hydrogen fuel
- propulsion/vehicle integration
- system operation with combined air-breathers plus rockets
- verification of design analysis techniques (involving appropriate derivation of computational fluid dynamics techniques for propulsion system analysis)

By the mid-1980s then, researchers interested in advanced hypersonic vehicles could draw upon an expanding technology base suitable to their needs, as well as a refined appreciation of the various technical challenges confronting them. Virtually all recognized, however, that despite the strong analytical tools and technology base already existing, development of actual hypersonic flight vehicles would require--as it had in the past--the initial step of some form of actual technology demonstrator. These feelings echoed sentiments and judgments expressed earlier in 1979 by Professor Ernest E. Sechler of the California Institute of Technology, and Professor René Miller of the Massachusetts Institute of Technology, when they had participated in a NASA study examining airframe structural technology needs and the flight test requirements of future hypersonic vehicles. Summarizing the importance of appropriate hypersonic flight testing, Sechler had stated:[46]

> Flight test and demonstration offers the only reasonably sure way to discover design "unknowns" before production vehicles become operational....flight testing of hypersonic thermal/structural designs will be an absolute necessity.

René Miller voiced these same sentiments when he stated that: [47]

> It is believed that many [structural design problems] cannot be solved except through flight tests. For example, structural distortion in the presence of flight loads, uneven thermal expansion and high dynamic pressures occur in flight in a way which would be impossible to duplicate on the ground. Flight tests are important to provide this proof of concept information.

It was here--with this clear need for an actual flight test vehicle--that the confluence of all previous design analysis work and the expanding technology and techniques base came together to form the genesis of a new research program: the NASP.

As discussed earlier, in late 1984, ASD had established a TAV Project Office under the direction of Lt Col Vince Rausch. By 1985, TAV had been underway since mid-1982. During that time, ASD had developed increasingly close bonds with other organizations interested in the concept, notably AFSC's Space Division, Air Force Space Command, and DARPA: the Defense Advanced Research Projects Agency. SD and DARPA had been represented at the first major TAV conference, a three-day affair held in August 1982 at ASD. In early 1983, Rausch had journeyed to Washington to meet with Robert Williams of DARPA to discuss the TAV concept, and the state of hypersonic air-breathing propulsion research. During a November 1983 TAV review held at ASD, Williams had remarked that research in the air-breathing propulsion field held enough interest to warrant a study effort of its own. In early 1984, this interest spawned a DARPA program called Copper Canyon, with Lt Col Rausch appointed as project director for DARPA on this effort as well as ASD's TAV project. But, whereas TAV was funded by ASD and SD, with participation from Space Command, Copper Canyon was strictly a DARPA-funded effort, and as Rausch recollected "semi-'black'" (in contrast to the more open TAV effort). Copper Canyon grew to embrace TAV-like ideas--becoming, in effect, a TAV with air-breathing propulsion, as distinct from the rocket-propelled vehicles more typically associated with the TAV effort. In early 1984, a panel of experts had reviewed the

state of air-breathing propulsion and, as with TAV the previous year, concluded as Rausch subsequently recalled "We're not violating the laws of physics."[48] Boeing (originator of the RASV that had revitalized the interest in SSTO concepts back in 1979), Lockheed, and McDonnell-Douglas subsequently all received $100,000 study contracts to pursue their own airframe investigations, and Marquardt and Aerojet General received more substantial propulsion study contracts. In March 1985, Dr George A. Keyworth II, President Ronald Reagan's science advisor, advocated making transatmospheric research a high priority national goal; his remarks came on the heels of a report by the President's Aeronautical Policy Review Committee (chaired by former Boeing executive and jet transport pioneer John E. Steiner) that had endorsed such work. In August 1985, General Applied Science Laboratory (GASL: the late Antonio Ferri's firm and thus an institution with a strong interest in hypersonic air-breathing propulsion) undertook a proof-of-concept demonstration of work completed as part of Copper Canyon that convinced those attending--including AFSC Commander General Lawrence A. Skantze--that a SSTO air-breathing vehicle was at last a very real possibility. In retrospect, Copper Canyon constituted "Phase I" of the NASP Program. In October 1985, following effectiveness evaluation briefings held at HQ AFSC, General Skantze launched an Advanced Aerospace Vehicle (AAV) program embracing ASD and SD efforts. From the inception of Copper Canyon, participants had harkened back to the 1960s and the term "Aerospaceplane" when envisioning the kind of vehicle they were seeking. Increasingly, however, they recognized that such an expensive and complex program would have to be national and multi-agency in character, as opposed to a system developed within a single service, and gradually they prefixed "national" in front of aerospaceplane. By late 1985, DARPA, NASA, the SDIO, and Navy had all become involved in studying the AAV concept, and, fittingly, following senior-level go-ahead from the Secretary of Defense and the NASA Administrator,

the title National Aero-Space Plane (NASP) replaced the earlier TAV/AAV designations as of December 1, 1985.[49]

That NASP had achieved significant stature in future Air Force planning was evident when the service released its Project FORECAST II study in early 1986. Like von Kármán's Toward New Horizons of 1944 and Schriever's FORECAST I of 1964, FORECAST II constituted a major effort to identify new technologies having "exceptional promise" for improving the service's combat capabilities, and was consciously modeled (down, even, to its name) upon the earlier FORECAST I. While NASP per se did not receive detailed attention in the report nor constitute one of the key identified goals for future exploration, it did appear in the FORECAST II Executive Summary under a discussion of propulsion and power; the summary stated:[50]

> We believe it is possible to create an air-breathing propulsive system that will permit relatively inexpensive, efficient hypersonic flight operations that include horizontal take-off, orbital insertion, and horizontal landing. The system will allow the development of the aerospace plane--a vehicle that will permit routine access to space from conventional runways. An aerospace plane could provide rapid worldwide interceptor, transport, space rescue, and other sorely needed military capabilities.

This endorsement mirrored the important administrative changes in the NASP program that were elevating it from the status of a theoretical study to a major new programmatic initiative.

In November 1985, in the waning days of Copper Canyon, a classified Request for Proposal (RFP) had been sent out to leading aerospace companies for proposals to build a hypersonic engine module, design and build appropriate test facilities, and design appropriate airframes. At this time, NASA and DoD envisioned awarding contracts in several months, followed by a 3 to 4 year period in which the airframe would be defined and engine modules

would be built and tested. To do this would require creation of a test facility within approximately 1 1/2 years. Finally, if all went well, in the mid-1990s, an actual flight test demonstrator would be ready for initial flight testing at Edwards AFB, the Air Force Flight Test Center. The airframe RFP went to Boeing, McDonnell-Douglas, Lockheed, General Dynamics, Grumman, Northrop, Fairchild, and Vought; Rockwell requested and was issued the RFP as well. Engine module RFP's went to General Electric, Pratt & Whitney, and Rocketdyne, while the requests for test facility proposals went to Aerojet, Martin-Marietta, and Marquardt. NASA and DoD envisioned that the contracts would be awarded on the basis of an integrated assessment of technical, cost, schedule, and risk considerations, and not merely on the basis of who was the lowest bidder; when contracts were awarded, it would be simultaneously, with the actual work statements classified.[51]

While the aerospace community began addressing the classified RFP, work went ahead on structuring the administrative nature of the program itself. On January 1, 1986, Air Force Systems Command announced establishment of a joint NASP program office at Aeronautical Systems Division, Wright-Patterson AFB, Ohio, and named Colonel Kenneth E. Staten as program manager for Systems Command. Colonel Staten, a Brigadier General selectee (he "pinned on" in May 1986), had formerly served as commander of the 6510th Test Wing at Edwards, where he had also served as commandant of the Air Force Test Pilot School. A strong manager with a distinguished flight testing background, Staten assumed control over ASD's NASP effort--a coalescing of the previous TAV and Copper Canyon efforts--from Lt Col Rausch, thus elevating NASP from the ASD level to Systems Command level; indeed, he reported directly to General Skantze. Staten arrived at Wright-Patterson in early February 1986 and immediately set to work creating a joint program office with Air Force, Navy and NASA personnel. The entire previous TAV staff were transferred into the NASP JPO, which received the organizational symbol AFSC/NA, reflecting its Systems Command, rather than ASD, orientation.[52]

Since Copper Canyon had, de facto, constituted NASP's Phase I technical feasibility study, the creation of the NASP Joint Program Office coincided with the beginning of Phase II: accelerated technology development dealing with the maturation of key technologies, development of propulsion modules, and airframe design--the latter oriented strictly towards a possible technology demonstrator designated the X-30A. Under Phase II, ASD would provide the physical facilities for the JPO, with some personnel coming from ASD, AFWAL, and from other AFSC organizations such as Space Division. On February 1, 1986 Colonel Staten arrived at ASD to assume his duties, and by February 14, the NASP Joint Program Office consisted of hastily refurbished quarters in Building 39 on Wright-Patterson's Area B, housing five directorates: airframe, program integration, technology, advanced planning, and propulsion. Two other directorates, vehicle integration and systems application, were subsequently added in August 1986. NASA involvement in NASP was the responsibility of Dr Raymond Colladay, NASA's Associate Administrator for Aeronautics and Space Technology. DARPA's NASP effort was under the overall direction of program manager Robert Williams. Overall, the NASP program cost for 1986-1993 was estimated at an anticipated $3 billion.[53]

It is well to remember what NASP really involves, as the publicity surrounding this program has typically hailed it as a Shuttle replacement, a critical ingredient of SDI, and as a futuristic hypersonic "Orient Express". In fact, NASP is, at the present time, none of these. Rather, it is a study effort that may eventually lead to a decision to build a SSTO technology demonstrator designated the X-30A: a research vehicle, in Colladay's words, constituting "a subscale of what would eventually be developed for an operational vehicle....the X-airplane has a payload capability of 2500 pounds to orbit with a crew of two."[54] The purpose of the X-30A would be to demonstrate the technologies necessary for hypersonic cruise and for acceleration from take-off into low earth orbit. In testimony on

April 9, 1986 before the Subcommittee on Transportation, Aviation, and Materials of the House Committee on Science and Technology, Colladay unveiled a "technology roadmap" for the NASP program; this roadmap is reprinted as Figure 20b. Figure 20a shows the generalized anticipated three phase schedule for NASP development culminating in flight research in the mid-1990s (tentatively set for 1993) using the X-30A. As this clearly demonstrates, the NASP program is strictly intended to generate the kind of technical knowledge and competency to permit rational decision making possibly involving operational follow-on systems drawing upon the technology contributions of the X-30A, if it is built. To this degree, then, the X-30A relationship to possible future logistical vehicles, military systems, and commercial follow-ons is even more tenuous than the relationship between the original Step I Dyna-Soar proposals and their anticipated "operational" follow-ons of the late 1950s and early 1960s.[55]

This is an important point, for in today's technological decision making environment, it is often as significant to comprehend what decision makers in the external world (Congress, the science community, industry, etc.) believe to be the nature of a high-technology complex program as it is for the internal community (in this case DARPA, NASA, and the Air Force) to know precisely what a program is about. Following the massive public attention focusing on NASP in the wake of the Challenger accident and the President's State-of-the-Union message on February 4, 1986 (when, for the first time, the Executive Branch "went public" in advocating the program, an even akin to the Nixon Administration's endorsement of the Shuttle on January 3, 1972), NASP was widely touted as a plan for a fully developed space transportation system; a point lost to the media in general was that NASP involves first, last, and foremost the development of a technology demonstrator: a vehicle in the grand tradition of the X-1, X-15, abandoned X-20, or the contemporary X-29. Greater expectations are not merely premature, but, as RAND Corporation research associate Scott Pace noted, potentially damaging: [56]

Figure 20

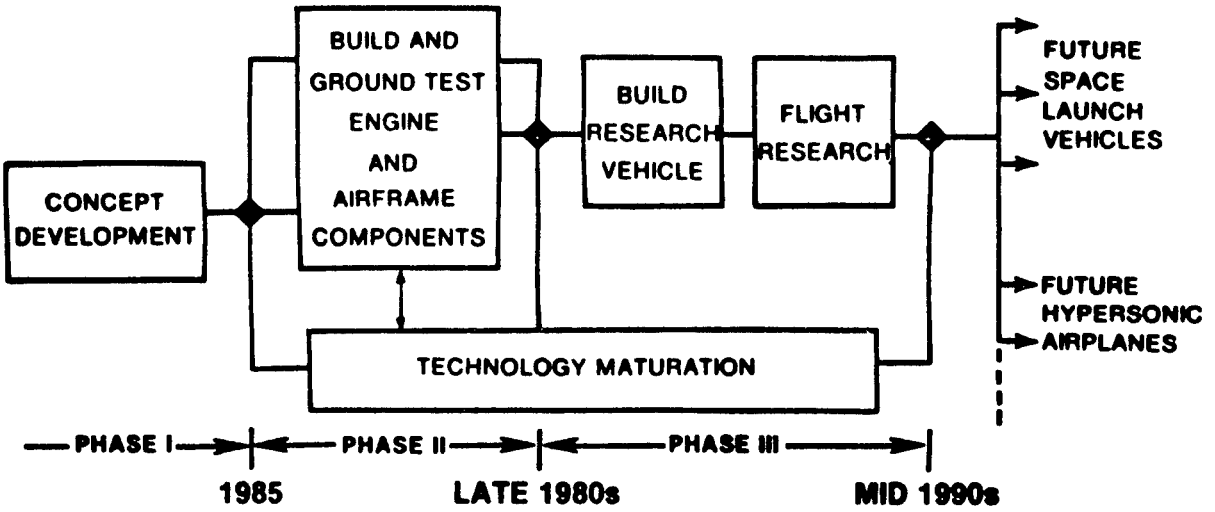


Figure 20a: NASP Phased Research and Development Approach

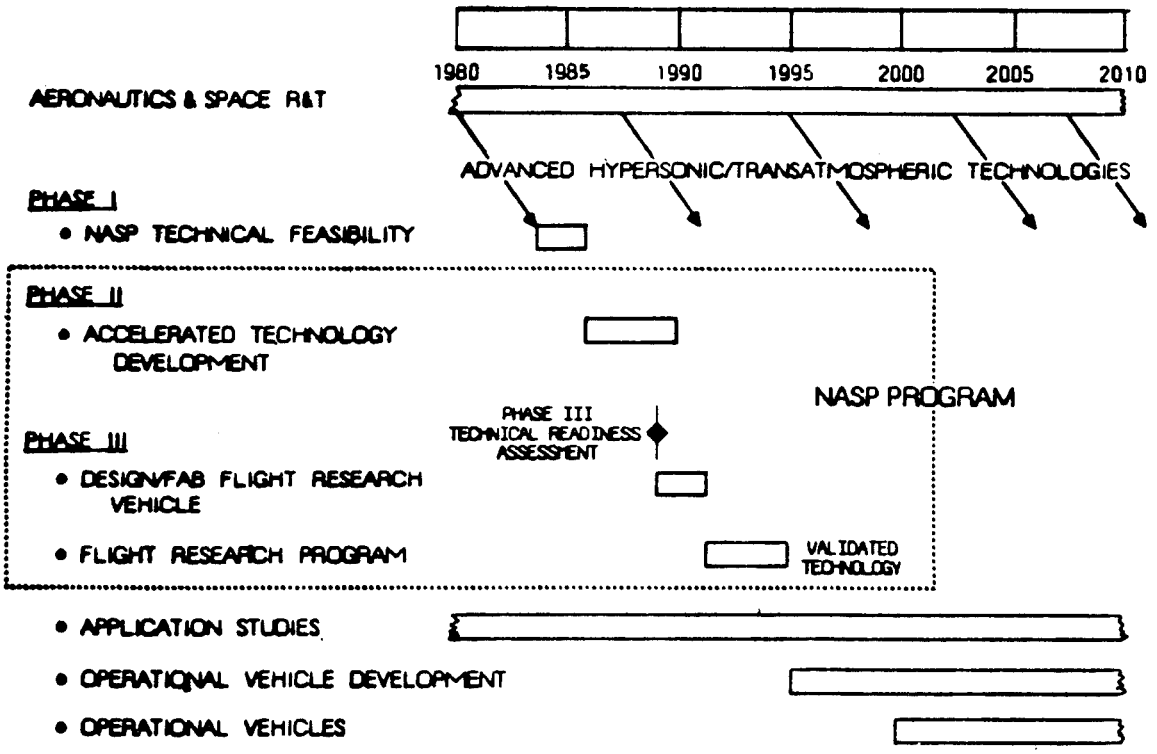


Figure 20b: Anticipated NASP Technology Roadmap, April 1986

> Some justifications for the NASP program may be leading it into serious trouble. Expectations in Congress and the press that a true "Orient Express" may be on the horizon generates pressures to show tangible results in the next few years. Such pressure arises when a basic research and technology program is asked to cite potential benefits of the work before it has been performed. Are extravagant claims necessary to receive large multi-year research support? Representatives of DARPA have contended that it is necessary to sell "dreams" along with the research program. This may be politically true, but experimental programs encourage taking technical risks that include potential failures. The later operational programs, using developed technology, should be the ones asked to meet real-world needs. Their time has not yet arrived.

Pace's comment is a profound one, for even with the aerospace community there is some misapprehension over what NASP is. At the 1987 Aerospace Engineering Conference and Show of the American Institute of Aeronautics and Astronautics, held in Los Angeles, an entire plenary session was devoted to NASP. Out of that session came a good comprehension of the demanding analysis, materials, and propulsion problems confronting the program, but also a general impression among many attendees that NASP was, indeed, aimed towards a long-range hypersonic transport. One speaker emphasized that such a craft could undertake four flights per day--by a single aircraft, no less--to the Orient: an optimistic assertion flying in the face of previous experience in the history of air transportation, where equally extravagant promises had been made about earlier propeller and jet-driven airliners, particularly the abortive experience of the SST, and then found to be embarassingly naive. (Worse, it echoes the kind of hyperbole that surrounded predictions of the Space Shuttle's possible schedule in the mid-to-late 1970s). At the same time, though this same speaker acknowledged that fully 80% of NASP funding involved the Department of Defense, little mention was made of DoD's interest in the craft, leading some attendees to wonder why DoD was so financially involved in what appeared to be a civilian

logistical spacecraft/commercial hypersonic transport development effort.[57] Dr Donald A. Hicks, former Undersecretary of Defense for Research and Engineering, is quoted as having warned against "pretending we have something we don't have," and the "overselling" of NASP. (In fact, in April 1987, a major aerospace trade journal reported that just such confusion over the purposes of the NASP effort had caused DARPA to recall NASP-like configuration models for repainting, so they would look less like a transport and by implication more like a research vehicle)![58]

General NASP anticipated funding, from 1986 through 1993, is shown in Figures 21a through 21d, drawn from research by Scott Pace of the RAND Corporation. Figure 21a shows the "bell curve" of overall NASP funding (approximately $3 billion total), broken down year-by-year according to the funding organizations: DARPA, the USAF, the Strategic Defense Initiative Organization (SDIO), the Navy, and NASA. Figure 21b presents this same overall funding as a pie chart indicating percentages by organization; as can be seen, NASP involves slightly over 80% DoD funding, with slightly over one-third of all NASP funding coming from the Air Force. Figures 21c and 21d present pie charts showing the anticipated funding breakdown for NASP during Phase II, (which it is now in) and Phase III (which would involve fabrication and flight testing of the X-30A technology demonstrator). During Phase II, DARPA and NASA almost equally generate just slightly less than fifty percent of NASP funding, with the Air Force contributing nearly 21% of the total NASP Phase II monies. In Phase III, DARPA funding shrinks to only 3.3%, while Air Force funding predominates, at 37 percent.[59]

Following detailed analysis of the proposals contractors had submitted in response to the classified RFP's that had gone out in November 1985, DARPA Director Dr Robert C. Duncan had presided over contractor selection, subsequently leading to contract implementation by Air Force Systems Command. On April 7, 1986,

Figure 21

1370

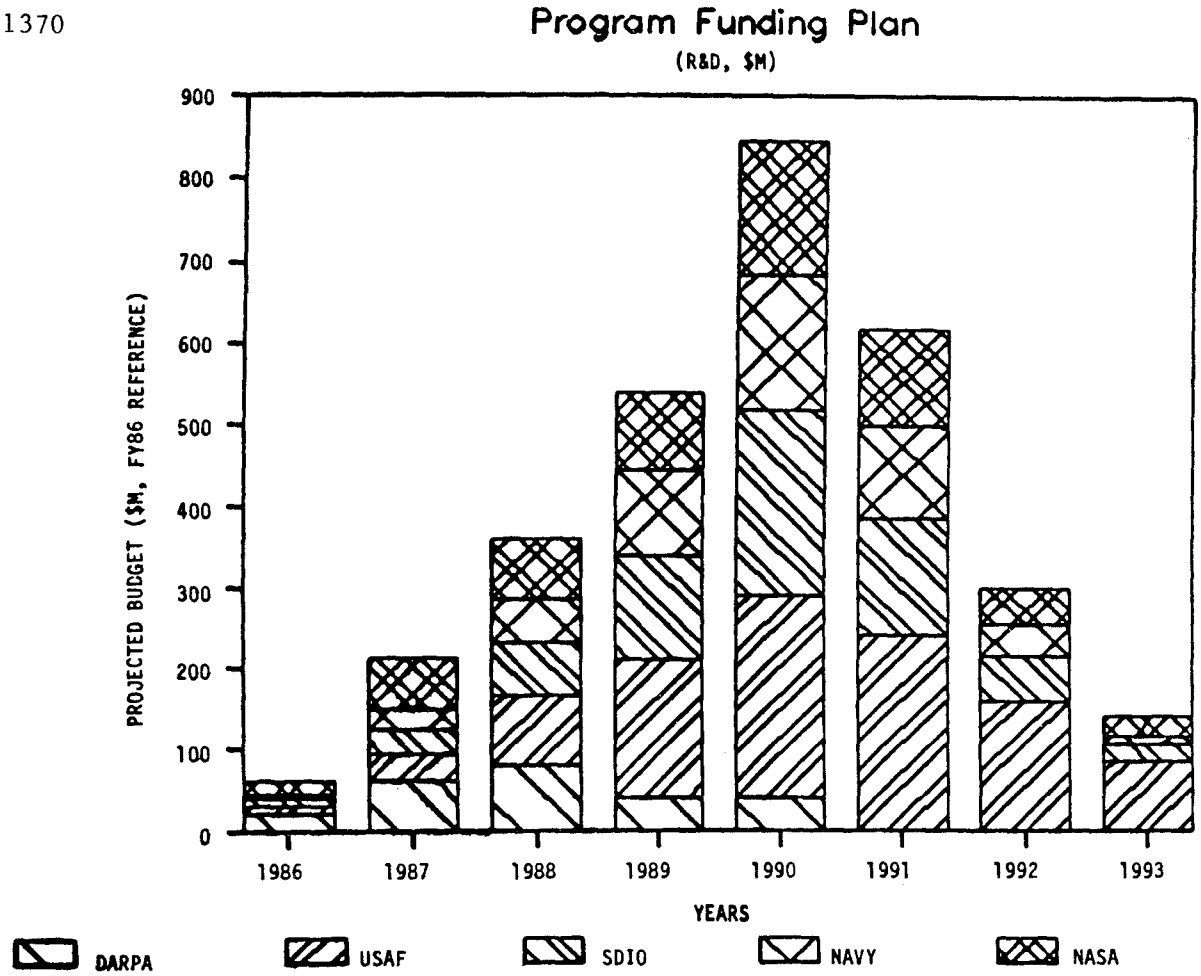


Figure 21a: Overall Program Funding by Year, 1986-1993

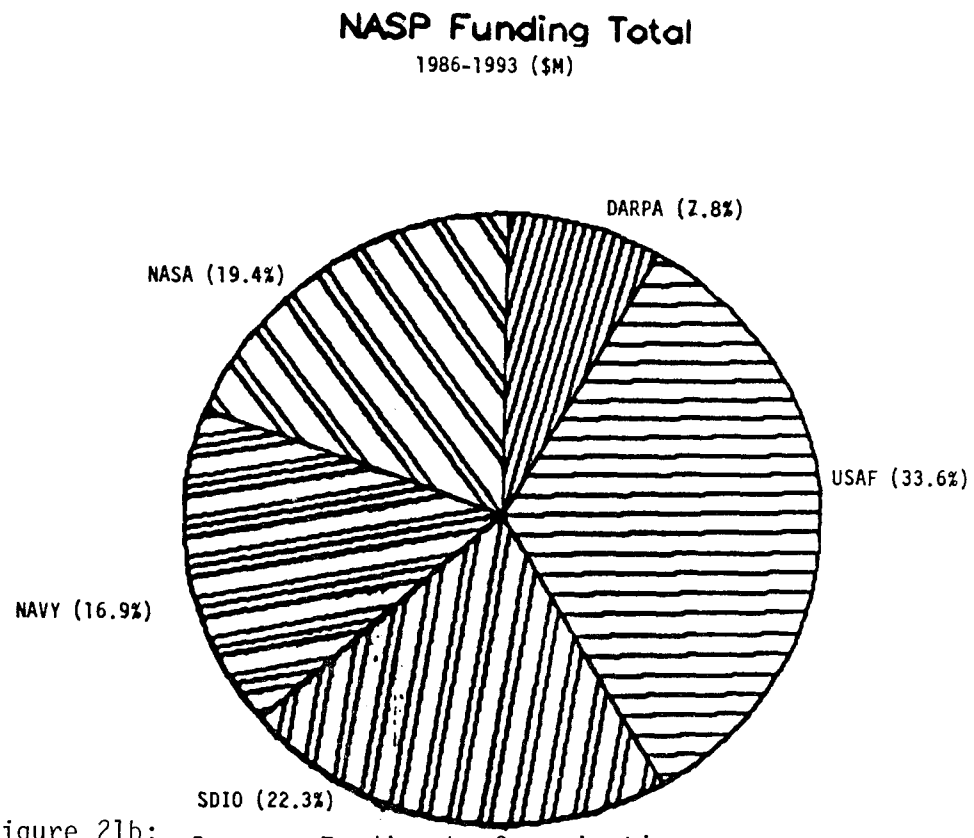


Figure 21b: Program Funding by Organization

Figure 21 (continued)

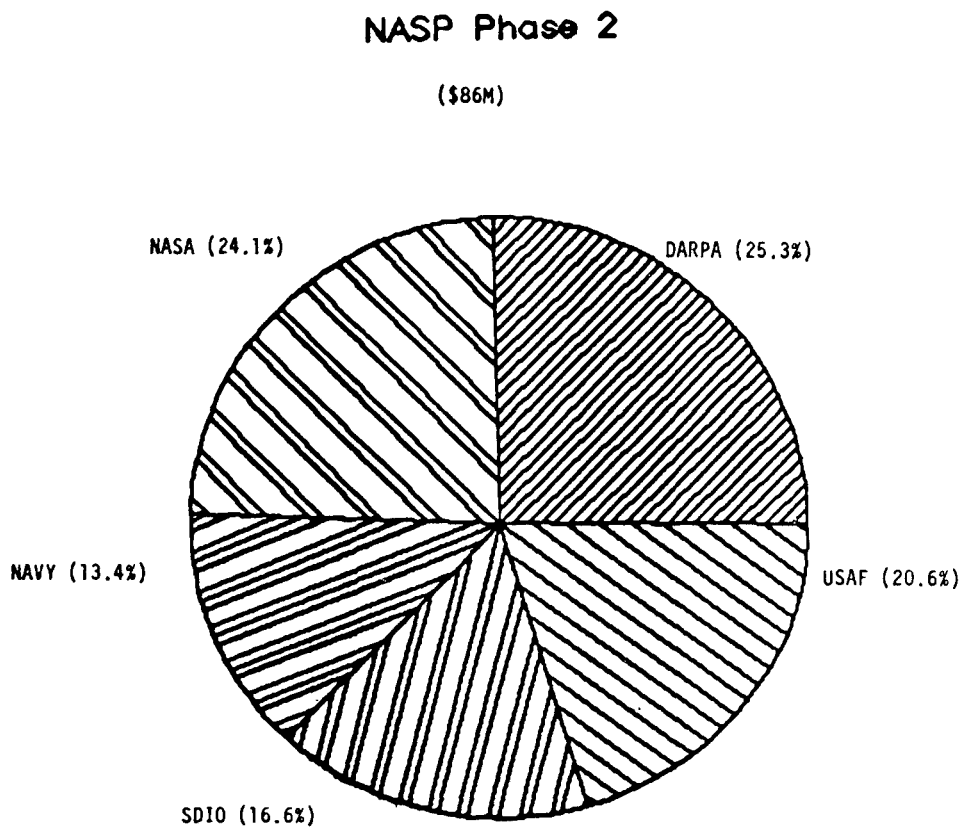


Figure 21c: Fund Breakdown by Organization During Phase II

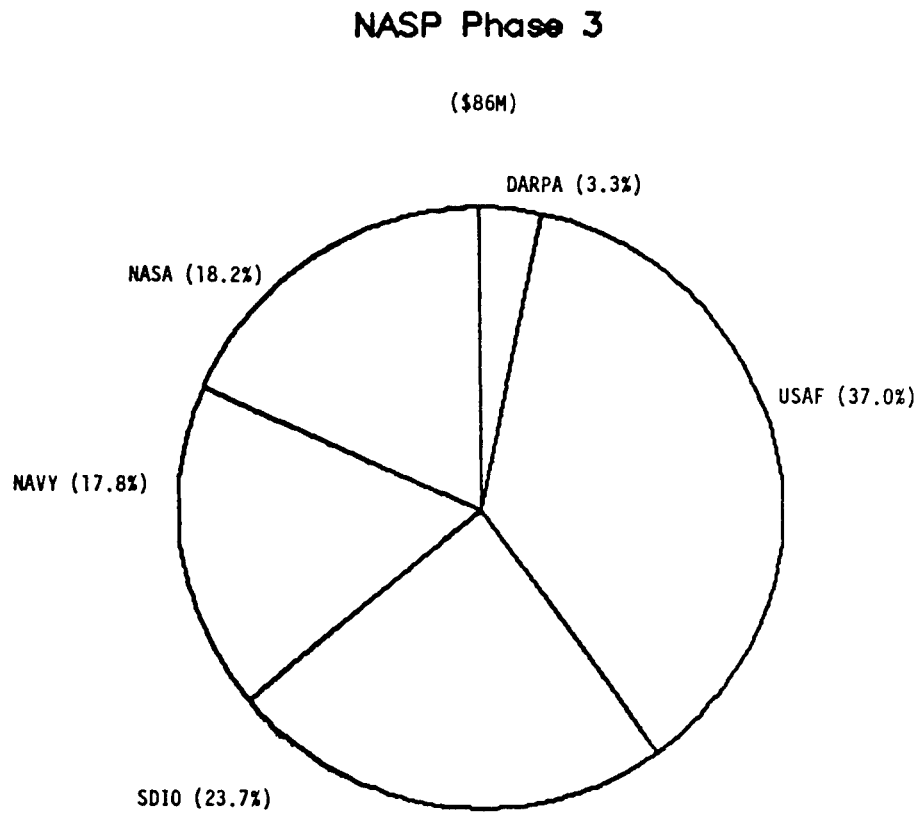


Figure 21d: Fund Breakdown by Organization During Phase III

DoD and NASA issued joint announcement of the award of seven NASP-related contracts valued at a total of approximately $450 million to cover work over a forty-two month period. Five of these were for airframe studies, at approximately $32 million apiece, to Boeing, General Dynamics, Lockheed, McDonnell-Douglas, and Rockwell. Following a one-year configuration development effort by each contractor, NASA and DoD would evaluate the results, and then "down select" to two or three airframe contractors who would proceed onwards. To encourage technical interchange and competency, all five corporations organized interdisciplinary technical teams to examine and address the Significant technical challenges confronting the program. Two propulsion contracts were also awarded, to Pratt & Whitney and to General Electric; both companies would development flight-type scramjet engine modules and then test them in special ground facilities up to Mach 8 (the limit for useful derivation of airbreathing propulsion information from existing tunnel facilities). Eventually, Rocketdyne--by using their own corporate funding--entered the NASP propulsion sweepstakes, proposing a propulsion concept for the program. Figure 22 offers typical examples of configurations under consideration for the X-30, compared to the government baseline vehicle shown here (and also earlier in Figure 13 of this epilogue). Obviously, it is still too early to point to a "definitive" X-30 configuration. Despite this, however, the NASP JPO has already spent considerable effort on defining some of the problems and challenges that will have to be confronted during flight testing of the X-30--if it is built--particularly "envelope expansion" from subsonic through transonic, supersonic, and hypersonic operations. Figure 23 shows a typical envelope expansion flight from Edwards AFB. The X-30 would cruise from Edwards to the Florida coast, turn, and then accelerate to hypersonic velocity. On Early flights, for safety reasons, propulsion would be then shut down to minimize potential heating damage, and the craft would decelerate in a terminal glide back to

Government Baseline Vehicle
Wing-Body; Bottom-mounted Propulsion

Blended Wing-Body;
Bottom-mounted Propulsion

Cone Body with Nonlifting Fuselage;
Annular Propulsion

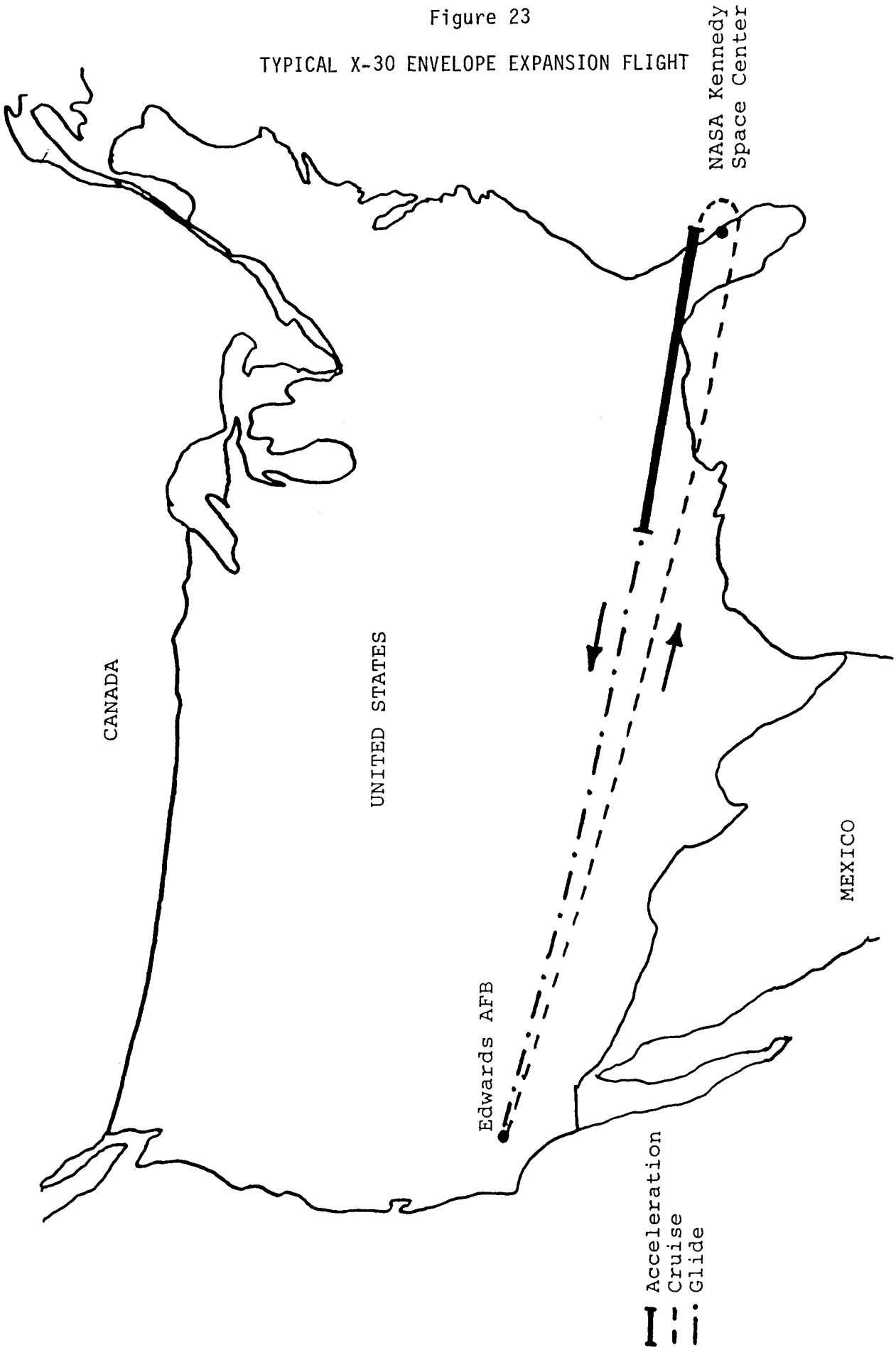
Combination Body; Confined Flow Field
Bottom-mounted Propulsion

POSSIBLE X-30 CONFIGURATIONS

FIGURE 22

Figure 23

TYPICAL X-30 ENVELOPE EXPANSION FLIGHT



Edwards much like the X-15 or the Shuttle itself. (This flight track echoes earlier studies in the 1950's and 1960's for such vehicles as the proposed Ames Mach 10 technology demonstrator and the X-20A Dyna-Soar discussed in Case II of Volume I of this study). Figure 24 demonstrates the immense airspace requirements (fortunately in the upper atmosphere well above conventional traffic) of a hypersonic test vehicle such as the X-30 to complete a powered mission from takeoff through landing, including the feasibility of executing a "go-around" maneuver, something not feasible with today's Shuttle, but highly desirable in a subsequent advanced vehicle such as the NASP. Figure 25 shows the anticipated flight envelope for the X-30; as can be seen, it operates within the airbreathing flight corridor, and differs greatly from the Space Shuttle, whose ascent and entry trajectory is superimposed on the figure for comparison purposes. Obviously, the X-30's anticipated environment is much more demanding in that it involves the attainment of sustained higher Mach numbers at much lower altitudes than the Shuttle.[60]

With the 1986 contract awards for initial airframe and engine studies, NASP Phase II was well and truly off and running. From that point to the present, the JPO has functioned much like other JPO and System Project Office (SPO) operations: assessing contractor performance, and working with senior-level advisory teams and groups to ensure that the NASP program remains on track and schedule. Figure 26 shows the current NASP management structure, and the relationships between NASA, the Air Force, DARPA, the Navy, and the SDIO. By the end of December 1986, two NASP technology symposiums had been held at the Applied Physics Laboratory of the Johns Hopkins University, with government and industry attendees pronouncing these symposiums "highly productive." Then, on December 17, 1986 (the 83rd anniversary of the Wrights' flights at Kitty Hawk in 1903), DARPA approved Version A of the NASP Technical Maturation Plan, which contained numerous separate research tasks to be undertaken by NASA, DoD,

Figure 24

TYPICAL HYPERSONIC GROUND TRACKS ANTICIPATED WITH THE X-30

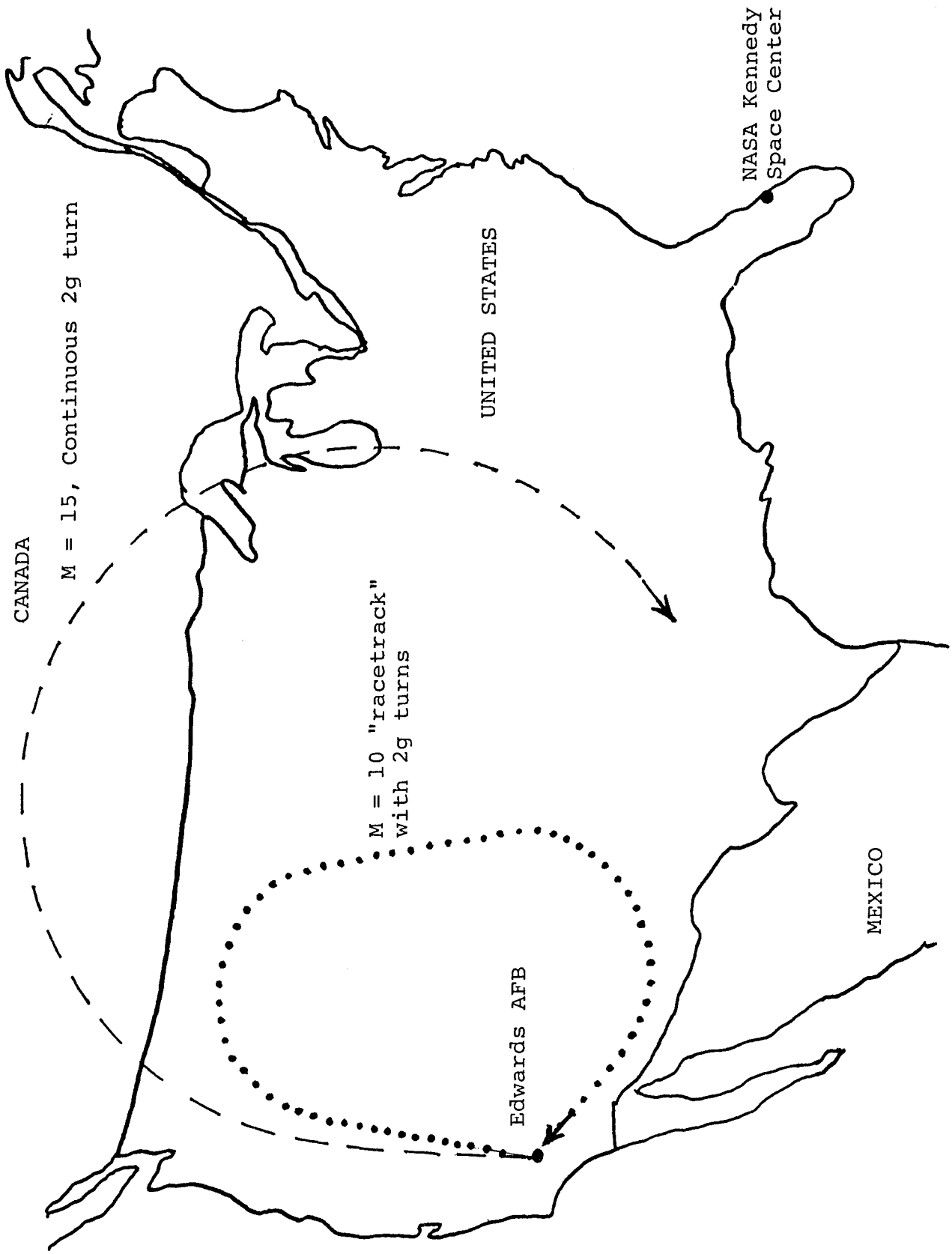


Figure 25

GENERALIZED X-30 FLIGHT ENVELOPE

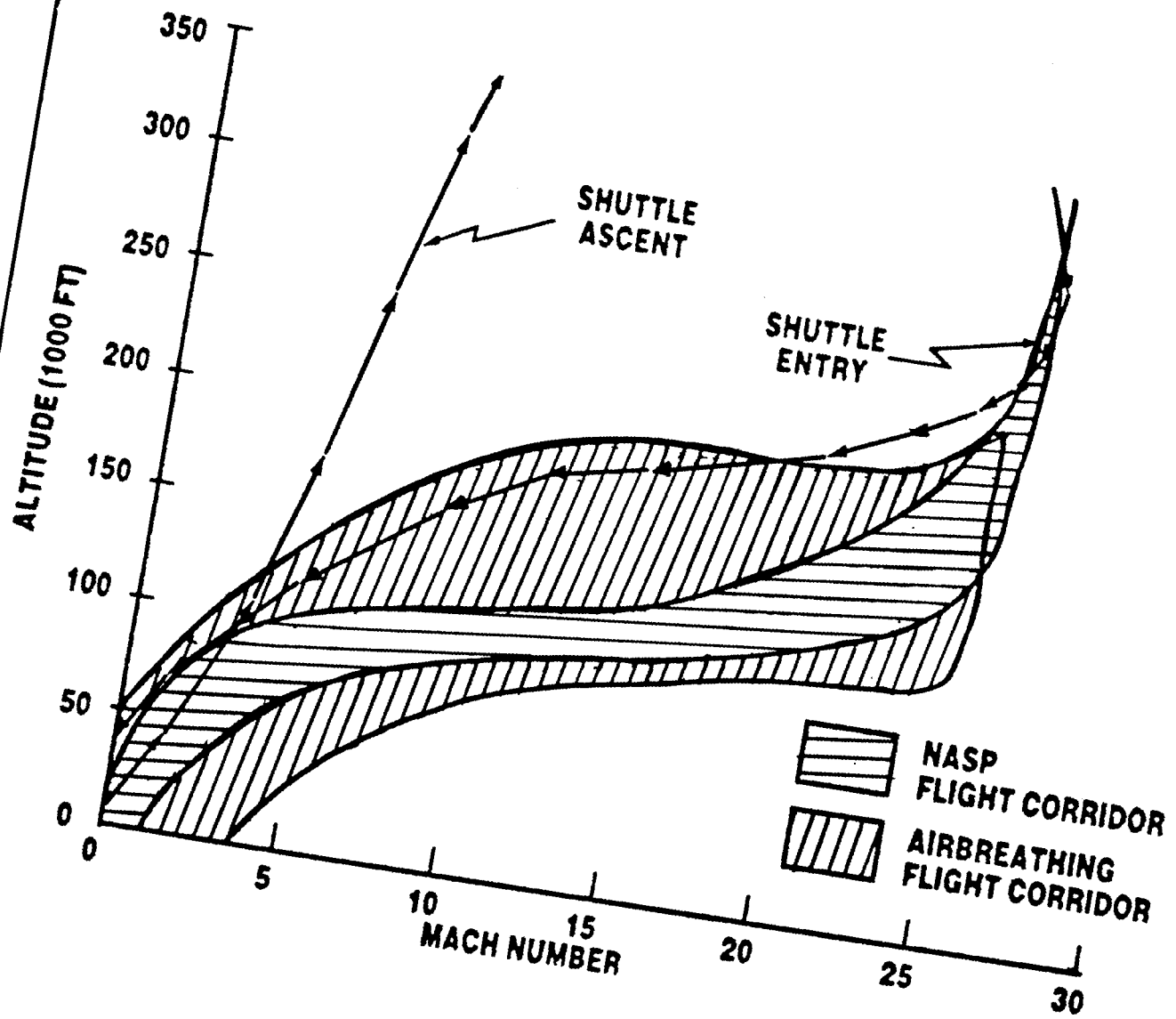
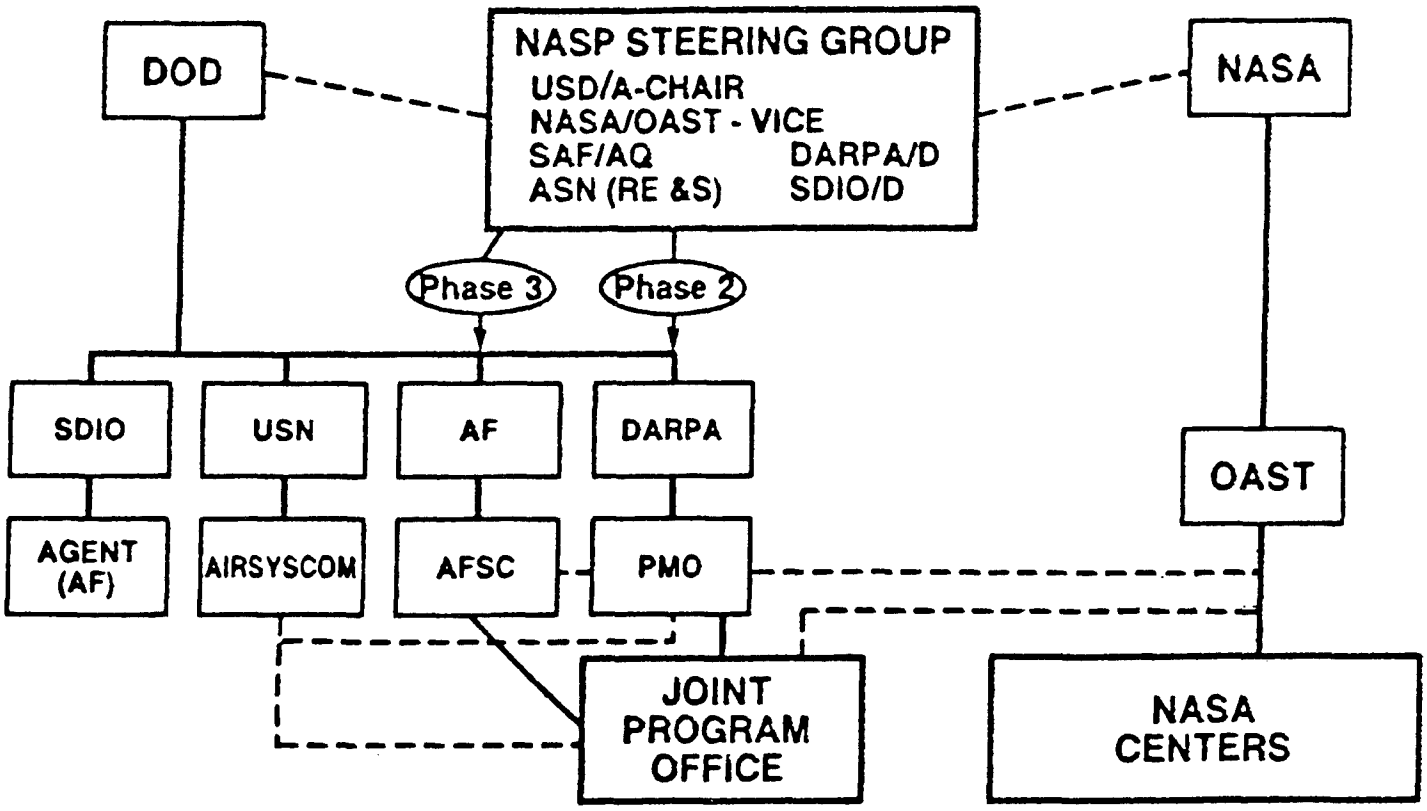


Figure 26

NASP MANAGEMENT STRUCTURE, 1986-1987



and industry. During the following month, January 1987, the first steps were undertaken to implement the plan. While Phase II was still really only in its infancy, as a result of these significant milestones, the infant was alive--and, more importantly--quite healthy. Whether the X-30 would be built was a question requiring a future answer, dependent upon both the advance of technology in Phase II and the climate of the external environment in Washington, particularly over the next several years.[61]

The NASP program passed two important milestones and witnessed some change as well during 1987. In September, DARPA announced selection of Pratt & Whitney and Rocketdyne for advanced propulsion studies in support of NASP. This, of course, represented the first of the "down-selection" decisions. Rocketdyne, which, it will be recalled, was not one of the original awardees, but which has gambled on entering the NASP propulsion sweepstakes with a privately funded proposal, had seen its gamble succeed handsomely. The following month, on October 7, 1987, the airframe down-selection occurred; McDonnell-Douglas, General Dynamics, and Rockwell received authorization to go-ahead with advanced airframe studies. These events coincided somewhat with administrative changes to the program. In August, Colonel David Milam had joined the NASP JPO as Deputy to General Staten. Milam, like General Staten a distinguished test pilot and flight researcher, brought to the program strong management credentials and broad experience with advanced flight control technology and aircraft handling qualities. Then, in mid-September, General Staten received notification of his upcoming transfer to Electronic Systems Division from the new commander of Air Force Systems Command, General Bernard Randolph, effective November 1, 1987. Staten had done a superlative job in welding together a strong team at Wright-Patterson and in shepherding the program through its critical early days of Phase II. In his place stepped Dr. Robert R. Barthelemy, former technical director of the Air Force Wright Aeronautical Laboratory complex. A highly regarded

research and technology administrator with an extensive background in propulsion and materials--two of NASP's key technology fields, of course--Barthelemy took over in time to steer NASP through the latter stage of Phase II and, hopefully, into Phase III as well.[62]

Undeniably NASP is a demanding, complex project pressing for significant advances in the state of hypersonic technology. Even among its partisans, specific disagreements have sprung up concerning the pace of the program and the need for thorough examination of the requisite technology before proceeding. In late September 1987, for example, William R. Graham, the Science Advisor to President Ronald Reagan, received a briefing on the conclusions of a study by the Defense Science Board's NASP Task Force, including a recommendation that NASP stretch Phase II by another two years. Graham's response to the report, in the form of a memorandum to the Deputy Secretary of Defense, the Secretary of the Air Force, the Under Secretary of Defense for Acquisition, and NASA's Associate Administrator for Aeronautics and Space Technology, left no doubt as to his views, and is worth quoting from a length:[63]

> As currently planned, the NASP program is highly concurrent and also quite aggressive, as I think it ought to be. Technology exploration, development and maturation, which are the essentials of the NASP program at this time, of necessity involve technical risks; I remarked to Dr. [Joseph F.] Shea [of the Defense Science Board Task Force] that most of the significant technology breakthroughs which have occurred in this century have been the result of a highly aggressive research approach, including significant risk-taking.
>
> I do, however, agree with the DSB Task Force in that such an approach dictates frequent and thorough evaluation of progress in all areas. When these reveal the need for changes in the scope, direction or pace of the program, then such changes should be effected. The need for program redirection must be based on specific, factual data and not on general indications or impressions. Using such criteria, I cannot support the

> Task Force's recommendation for a two-year slip in Phase 2. While Dr. Shea clearly expressed concern in several areas, he could not point to any specific data which supported the need for a two-year slip. It was clear, however, from both the DSB Task Force presentation, and the indications supplied by the NASP project, that recent events, such as the Engine Concept Review, Congressional budget action, and progress on materials development will probably require program adjustments. I support the Program Management's current efforts to assess the scope of those adjustments.
>
> The NASP is probably one of the key science and technology programs to emerge in the last quarter of this century. It is essential to this Nation's efforts to maintain its competitive advantage in aerospace in the coming decades. I believe that the President remains convinced of the correctness of his vision concerning the X-30 and I plan on supporting your efforts as you continue to shepherd the program through the remainder of the current phase of the Congressional budget process.

Graham's comments are important ones, and have broader and more significant implications going beyond NASP to the issue of Federal policy towards advanced technology in general. They come at an interesting time in the on-going national debate over the future of the American space program and aerospace in particular. Many in the scientific community--as distinct from the technology community--have outspokenly criticized such new defense initiatives as advanced fighters and bombers, SDI, the NASA Space Station and Space Shuttle, and the NASP. While, as discussed earlier in this epilogue, it is unwise to champion capabilities in advance of their being attained, it is, on the other hand, most foolish for critics (particularly from the scientific community) to hinge their arguments on what they perceive to be technologically "impossible," for the realm of the technologically possible is one that constantly expands. The record of scientists predicting what technologists cannot do is a dismal one, particularly with regard to projects in the aerospace field. As a rule, scientists (typified by physicists) seem far more cautious--indeed timid--in pursuing new starts and bucking tradition than do technologists (typified by engineers). And yet, for all its

occasional brash optimism, the aerospace record already indicates that, in a majority of cases, the technology community has been far more prescient and accurate in judging its abilities and future than has the scientific community been in judging the aerospace community's problems and limits to growth. Perhaps the pessimism of the scientist and the optimism of the technologist stem from inherent differences in the psychological mind-set of the two communities: the scientists drawing their inspiration from a long philosophical tradition dating to Plato that emphasizes theory and pure knowledge, and the technologists drawing their inspiration from an even longer craft tradition and from Francis Bacon, a tradition that emphasizes the application of knowledge to actual hardware. Thus, when confronted with a new field, too often the scientist draws upon tradition, pessimistically seeing problems, complexities, and "impossibilities." The technologist, drawing upon a very different tradition, optimistically sees challenges, opportunities, and capabilities. The rational administrator, aware of the strengths and weaknesses of both communities, steers a middle course, weaving Odysseus-like between the Scylla of scientific pessimism and the Charybdis of technological optimism.

And so today, at ASD and at other laboratories, facilities, test sites, and contractor plants across the country, dedicated men and women, both civilian and military, continue to "press on" in the tradition of their predecessors, who had opened up the hypersonic frontier, giving a glimpse of the promise lying beyond. This, then, has been the story of hypersonic flight--a story that goes back in time to the dreams of the turn of the century, and which even now is far from over. It is hard to imagine that nearly two decades have passed since men first walked on the Moon, or that over a decade has passed since Enterprise first experienced lift under its wings. Nearly seven years have passed since Columbia first thundered into orbit, twenty years since the X-15 reached Mach 6.72 over the desert Southwest. In all of the

time that has passed since these events occurred, the collective grasp of hypersonic technology has become stronger, the footing more secure. Significant challenges--such as the propulsion, materials, and computational tasks confronting NASP--remain, but they only serve to remind us that the frontiers of flight are only pushed back and never really vanquished.

NOTES

1. Presidential Commission on the Space Shuttle Challenger Accident, Report to the President (Washington, D.C.: GPO, 6 June 1986), pp. 72, 104, 117-118, 120-148, 161, 176, 198-201, 236. For a typical "civilian vs. military" argument, see Steve Blakely, "The U.S. Space Program: Air Force Blue or NASA White?" Journal of Defense and Diplomacy, V, 2 (1987) pp. 22-27. See also Arie W. Kruglanski, "Freeze-think and the Challenger," Psychology Today (August 1986), pp. 48-49.

2. James A. Van Allen, "Myths and Realities of Space Flight," Science, v. 232, (30 May 1986), p. 1076.

3. John M. Logsdon, "The Space Shuttle Program: A Policy Failure?," Science, v. 232, (30 May 1986), pp. 1099-1105.

4. John Rhea and Tony Reichhardt, "Correcting the Mistakes of the Past: A Conversation With John Logsdon," Space World (Aug 1986), pp. 12-18.

5. To a great degree, the Soviets did take this kind of approach with their Soyuz-Salyut vehicles. Astronauts corps reservations were conveyed to me by Col. Joe H. Engle, USAF in a conversation at the Johnston Space Center in 1981; Engle subsequently commanded STS-2.

6. Anon., "Defense Dept. Cancels Shuttle Planning Complex," Aviation Week & Space Technology (9 Feb. 1987), p. 29.

7. Vance D. Brand, "Space Shuttle Redesign Status," in Society of Experimental Test Pilots, 1986 Report to the Aerospace Profession (Beverly Hills, CA: SETP, 24-27 Sep 1986), pp. 224-235.

8. Extracted from NASA Lyndon B. Johnson Space Center Advanced Programs Office, Space Shuttle Directions, JSC-20939 (Houston, TX: NASA JSC, June 1986), pp. 3-17. Readers are encouraged to review the papers in this document, as well as two other earlier reference publications: NASA Langley Research Center, Shuttle Performance: Lessons Learned, NASA Conference Publication 2283 (Hampton, VA: NASA LRC, March 1983); and NASA Johnson Space Center, Space Shuttle Technical Conference, NASA Conference Publication 2342 (Houston, TX: NASA JSC, June 1983).

9. NASA Lyndon B. Johnson Space Center, Office of the Director of Engineering, Research and Technology: Annual Report [for] 1986, NASA Technical Memorandum 58277 (Houston, TX: NASA JSC, Dec 1986), pp. 38-39.

10. M. Villain, "Hermes, A Manned Space Vehicle," Preprint of IAF Paper 79-F-33, XXXth Congress, International Astronautical Federation, Munich, Germany, 17-22 Sep 1979, p. 1.

11. Ibid., p. 2.

12. I have benefited greatly from materials transmitted to me by Lt Col William Flanagan, USAF. See also Anon., "France Considers New Baseline for Hermes Manned Spaceplane, "Aviation Week & Space Technology (30 March 1987), p. 26.

13. Department of Defense, Soviet Military Power: 1986 (Washington, D.C.: DoD, Mar 1986), p. 52.

14. Ibid., p. 49. For a good discussion of using the Space Shuttle's SRB, SSME, and external tank combination to loft heavy payloads (up to 170,000 lbs.) into earth orbit, as well as other proposals for modified Shuttles having revised passenger capacity and propulsion systems, see A.L. Jones, "Extension of Space Shuttle Capability," in William F. Rector, III, and Paul A. Penzo, eds., Space Shuttle: Dawn of an Era, v. 41, Pt. 1, of Advances in the Astronautical Sciences (San Diego, CA: American Astronautical Society, 1980), pp. 391-411.

15. Clive Simpson, "Flying Into the Future," Spaceflight (Apr 1986), passim; Tony Devereaux, "Space Defence: Europe Must Help Itself," Defense Attache, 4 (1986), pp. 28-35; "Possible Military Role for BAe HOTOL," Flight International, CXXVI, 3926 (22 Sep 1984), p. 728; "British Urge ESA Members to Join HOTOL Launcher Development," Aviation Week & Space Technology, CXXII, 22 (3 Jun 1985), pp. 356-357. I have benefited from discussions with John Scott of British Aerospace concerning HOTOL's early history. See also "U. K. Sees Hermes Participation as Boost to its Industry," Aerospace Daily (30 March 1987), pp. 470-471.

16. The information on Sanger II is drawn from an MBB promotional document, "Sanger II: The Two-Stage Space Transport System," distributed to attendees at the Farnborough Air Show of 1986. I am grateful to Brian Nicklas for his assistance in furnishing me with this document.

17. Ibid.

18. For a detailed examination of IfR propulsion work applicable to a variety of space transportation schemes, see Peter A. Kramer and Rolf D. Buhler, "Airbreathing Booster Stages for Space Transporters Compared with Pure Rocket Systems," IAF Paper 79-09, XXXth Congress, International Astronautical Federation, Munich, Germany, 17-22 Sep 1979, passim.

19. Alfred C. Draper et. al., "A Flight Research Vehicle to Bridge Shuttle and Hypersonic Aircraft Technology," Paper 77-1165, AIAA Atmospheric Flight Mechanics Conference, Hollywood, FL, 8-10 Aug. 1977, passim,; Delma C. Freeman, et. al., "Definition of an Entry Research Vehicle," Paper 85-0969, AIAA Twentieth

Thermophysics Conference, 19-25 June, 1985, passim. See also ASD news release PAM 84-088.

20. I wish to acknowledge with appreciation information gained from conversations with Col D.A. Dreesbach of the Aeromechanics Division, Flight Dynamics Laboratory, and Mr. Johnny Armstrong and Mr. Bob Hoey of the Office of Research Projects, Air Force Flight Test Center, Edwards AFB, California.

21. Memo with attachments, Hans Mark to General Lew Allen, 15 Jul 1979. Transmitted to the author by Johnny Armstrong. For SPS SSTO vehicle concepts, see Final Report, Satellite Power System (SPS) Feasibility Study, Contract NAS 8-32161 (Downey, CA: Rockwell International, Dec. 1976), Executive Summary, passim. See also Hubert P. Davis, "Choices for the Next Generation of Reusable Launch Systems," in Rector and Penzo, Space Shuttle: Dawn of an Era, pp. 413-421. I have also benefitted from a conversation with Robert Salkeld at the 1979 IAF conference in Munich, West Germany. See his "Single-Stage Shuttles for Ground Launch and Air Launch," Astronautics & Aeronautics (Mar. 1974), passim.

22. Memo, Yost to Ward, 4 Sep 1979. Transmitted to the author by Johnny Armstrong.

23. Bruce A. Smith, "Study Shows Space Sortie Concept Viable by 1990," Aviation Week & Space Technology (1 Nov 1982), p. 69. See also Anon., "USAF Studies Small Manned Space Vehicle," Aviation Week & Space Technology (2 Nov 1981), p. 17.

24. S.A. Tremaine, "Air Force Aeronautical Systems in the 1990's: A Major Shift," (26 Sep 1983). Copy in TAV files of ASD Public Affairs Office. For the Skantze-McMullen relationship to TAV, I have drawn upon conversations with Dr Jerry Arnett and Lt Col Vince Rausch of the NASP program office; Lt Col Rausch was director of the ASD TAV Project Office and Dr Arnett was project manager for ASD TAV efforts.

25. ASD/PAM news releases 84-055, and 84-124. The quote is from Lt Col Rausch.

26. James W. Canan, "Bold New Missions in Space," Air Force Magazine (June 1984), pp. 88-93; Lt Col Donald L. Marx, "Transatmospheric Vehicles: A Forecast, " (undated but believed June 1984). Copy in the TAV files of the ASD Public Affairs Office.

27. Marx, "Transatmospheric Vehicles."

28. Ibid.

29. Ibid.

30. S. A. Tremaine and Jerry B. Arnett, "Transatmospheric Vehicles: A Challenge for the Next Century," AIAA paper 84-2414, AIAA/AHS/ASEE Aircraft Design Systems and Operations Meeting, 31 Oct-2 Nov 1984, San Diego, CA, passim. Copy in the TAV files of the ASD Public Affairs Office.

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36. Ibid., pp. 1-9.

37. Ibid., p. 11, see also pp. 9-10.

38. Ibid., p. 11; the quote is from a presentation by Robert M. Williams (DARPA) on NASP at the plenary session, Aerospace Engineering Conference and Show, American Institute of Aeronautics and Astronautics, Los Angeles, CA 17 Feb 1987.

39. Dixon et. al., p. 11.

40. Ibid., pp. 9-14.

41. Ibid., p. 15.

42. ASD PAM news release 85-174; see also Dixon et. al., pp. 18-19.

43. Ibid.

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Technical Session of the Defense Science Study Group, Science and Technology Division, Institute for Defense Analysis. I wish to thank Dr Heiser for transmitting a copy of this paper to me; Heiser served formerly as Chief Scientist of the USAF Arnold Engineering Development Center, and Chief Scientist of the USAF Aero-Propulsion Laboratory, before assuming his present position as Vice President and Director of Aerojet General's Propulsion Research Institute.

45. Ibid.

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48. My information on Copper Canyon, the DARPA-ASD relationship, and the evolution of NASP is from a conversation with Lt Col Vince Rausch of the NASP Joint Program Office. I wish to acknowledge my appreciation to Lt Col Rausch for his assistance. See also James W. Canan, "Mastering the Transatmosphere," Air Force Magazine (June 1986) P. 54.

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50. HQ AFSC, Project FORECAST II Executive Summary (Andrews AFB, MD: AFSC, 1986), p. "Propulsion and Power".

51. Memo and attached summary material, Lt Col R. Douglas McLarty, USAF, to Lt Gen Thomas H. McMullen, 6 Mar 1986; copy in ASD Public Affairs Office NASP files.

52. Lt Col Herbert G. Baker, USAF, AFSC Response to Query for Aerospace Daily, 8 Jan 1986, copy in ASD Public Affairs Office NASP files.

53. Ibid.; information from the ASD History Office files transmitted by Albert Misenko, ASD Chief Historian; McLarty memo to McMullen, 6 Mar 1986.

54. Anon., "Aerospace Plane Development Announced," X-Press, NASA Ames-Dryden Flight Research Facility, XXVIII, n. 3 (28 Feb 1986), p.4.

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56. Scott Pace, "Goals and Realities: The National Aerospace Plane," draft mss., 2 Sep 1986, p. 11. I am grateful to Scott Pace of the RAND Corporation for making this mss. available to me; it was written for Issues, a publication on policy issues in science and technology prepared by the National Academy of Sciences.

57. Personal recollections, and results of discussions I had with attendees at the conference.

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60. DoD news release 159-86 and NASA news release 86-41; the information on Rocketdyne is drawn from a NASP JPO weekly activity report for 25 Feb 1987, and from a conversation with Lt Col Ted Wierzbanowski of the NASP JPO. See also Stuart O. Schmitt, T. J. Wierzbanowski, and Johnny Johnson, "The Challenge of X-30 Flight Test," paper presented at the XXXI Symposium of the Society of Experimental Test Pilots, Beverly Hills, CA, 26 Sep 1987. I am indebted to Lt. Col. Wierzbanowski for making an advanced copy of the paper available for my research.

61. Material drawn from various NASP JPO weekly activity reports, 1986-1987. Figures 24-26 are adopted from Schmitt et.al., "The Challenge of X-30 Flight Test."

62. Conversations with Col David Milam and Lt Col Ted Wierzbanowski, NASP JPO, 18 Sep 1987; conversation with Lt Col Wierzbanowski, 9 Oct 1987; conversation with Jo Anne Rumple, ASD/CPA, 3 Nov 1987.

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