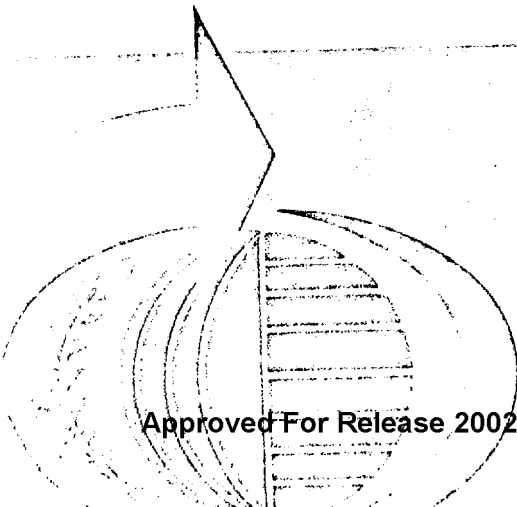


American Astronautical Society

AN ECONOMICAL APPROACH TO SPACE TRANSPORTATION

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AAS 67-133



COMMERCIAL UTILIZATION
OF SPACE

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It is generally agreed today that the exploitation of space is limited by the very high cost of placing objects into orbit. At the present time the U.S. launches about one-half million pounds of stuff per year at a cost of approximately \$7 billion which works out to \$14,000 per pound of payload in orbit. Existing plans for the next decade in both government agencies concerned do not indicate a profound reduction. Much lower costs will be essential before humanity can realize some of the great potentials of space.

There are two major barriers to the development of low-cost space transportation - philosophic and technical. Heated controversy has raged, with justice, on the technical aspects; but the philosophical has achieved broad acceptance and suffers from complacency and lack of challenge. It therefore constitutes the more serious problem. I feel, moreover, that attempts to conjure up technical solutions in the absence of careful treatment of philosophic considerations are and will be doomed to failure. Although we have created a host of new attractive words relating to philosophy of the development process and have applied these assiduously, little progress has been made. It may well be that these words and techniques have taken too little cognizance of the profound changes wrought in our society since World War II. During this period, for the first time in history, it has become possible to satisfy the basic needs of our people - food, shelter, clothing - by the efforts of a small number of men. Education, guidance, and employment of the rest of our people to fulfill higher needs has become paramount. Our destiny will hinge on how we meet this challenge. Are we to become a nation of rebellious, disenchanting beatniks eschewing technical risk and enshrining outmoded administrative dogma, or will we accept the immense and unprecedented challenge confronting us to provide meaningful dignified tasks while deliberately altering our environment on a scale never before possible?

In the light of these changed circumstances, established criteria of merit must be reviewed and carefully modified. Among the words and techniques that fall into this category, I would place planning, the systems approach, cost effectiveness, the learning process, and competition.

Renoir stated that perfect planning was death. I think he was right. The implication of his statement, however, is not to avoid planning - but to assure that it is imperfect and that those involved recognize this. Without planning, modern major research and development efforts would of course be impossible. This function enables us to identify major tasks early, **Approved For Release 2002/09/03 : CIA-RDP71B00822R000200150012-8** encourages comparison of alternative approaches. With respect to this

last, it must be recognized that engineering is an art and that problems falling in this realm rarely have unique solutions. Hence R&D project planning must involve discretion and risk. In employing planning, it is essential to recognize that if the objectives are not to be obsolete when achieved, the planning must be sufficiently imperfect to allow for flexibility and change during the course of a project. A thorough comprehensive plan cannot incorporate knowledge of techniques which arise as the program progresses, and thus inevitably it must serve as a point of departure or enforce stupidity. Perhaps the most deadly aspect of planning stems from the fact that accuracy improves as risks are eliminated. The serious student of the art of planning is therefore generally the advocate of developing obsolete equipment. It is easy to misconstrue planning as "scheduled history," and our present passion for elaborate computer-operated bookkeeping heavily underscores this illusion.

Systems engineering is another popular approach which has been applied with indifferent success to space transportation. There is nothing new about the systems approach; it was surely applied to the erection of the pyramids. On the positive side, it provides aid for establishment of germane interactions of system elements. More importantly, it can act as the tool for quantitative appraisal of diverse approaches, and this enables us to develop a broad assessment of effectiveness. The dangers in mishandling this technique are scarcely less formidable, however, than the advantages they offer. Although it is highly probable that any significant action will cause reactions and interactions with almost every person and device on earth, time and ignorance constrain us to establish more or less arbitrary limits to any given engineering problem. The value of the solution is strongly dependent upon the rational selection of such arbitrary limits. All too frequently, insufficient attention is paid to this, and the job becomes essentially infinite in scope, necessitating an incomplete, obsolete, or chaotic solution. Most of our city planning efforts have followed this pattern. The effectiveness of a system is generally sensitive to various key component performance characteristics which in turn have to be assumed. Inevitably a large element of discretion must be employed here. Perhaps the most difficult hurdle to the use of the systems approach, and systems engineering in general, lies in the fallacy that precise simulation of elemental characteristics in a system model incorporating assumed interactions will inevitably lead to acceptable systems solutions. The ability of the modern digital computer to handle massive amounts of postulated performance data for such simulation has led to a tendency to accept elaborate computerized simulation as a substitute for development. This is, of course, fallacious. We can only crank into the computer information assumed on the basis of current knowledge. Any complex advanced device - be it an airplane, an engine, or a space vehicle - will involve complex interactions of which we may be too ignorant to experimentally test. Undue stress on

a systems approach can, moreover, act heavily to the detriment of a development program. The ostensible necessity to demonstrate compatibility of a postulated device with all contiguous elements tends to inhibit innovation and condone complacency. The plight of the American railroad is in some measure due to this.

During recent years, "cost effectiveness" has become the sine qua non for evaluating the worth of a proposed development program. Here again we find that this is necessarily a relatively crude tool and its manipulation must be handled most carefully. The term "cost effectiveness" is philosophically more satisfying than other criteria. It takes the form of a quotient which we instinctively tend to feel is appropriate to this type of function. Since we have become adroit in estimating manpower and material requirements in terms of dollars, the cost element of the criterion can be evaluated and expressed in simple units. The term "effectiveness" is more difficult to define. However, we have generally chosen to calibrate this also in terms of dollars, which presumably relate to the incremental value of what is to be presented to mankind. Thus we divide dollars by dollars and generate a dimensionless parameter which through implied definition should provide a very basic and valuable tool for evaluation and selection of proposed major development activity. Since this has become the foremost yardstick for selection of prospective major engineering development programs in this nation, it is highly appropriate to examine this very carefully. Let us take "cost" first. The problem of estimating the costs involved in the vast development undertakings of this era is exceedingly complex. In deference to this fact, the nation has established a complex ritual through which the prospective price of most major development programs is developed, refined, and approved. Inevitably such a ritual must depend heavily upon experience. Initial cost estimates for the development of some complex device are developed by costing and summing events depicted in the development plan. The archives are then studied carefully for corroboration of the validity of costs derived.

How good are the archives? The attitudes and procedures within government agencies concerned are not static, and this in turn profoundly affects what appears in the archives. From the close of World War II until the mid-1950s, it was a firmly established practice to charge off the great preponderance of development costs for major programs against production funds. As a consequence of this, these figures must be viewed most critically when employed as precedents. The mid-1950s witnessed a discontinuity in this procedure. The urgency which the nation belatedly brought to bear on the rapid development of long-range ballistic missiles led to disruption of established procedures, adoption of new techniques, and the discard (perhaps unjustifiably) of many old ones. The organizational patterns adopted strongly favored "systems" engineering vs. component development. This led directly to a vast drying up of the funds

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surreptitiously, during the previous period. As a consequence of this, essential component developments quite often could only be funded as intrinsic elements of major system development programs, an unhappy fact, which raised the costs and reduced the effectiveness of such projects markedly. Thus early records are wholly misleading because of cost category juggling, and later figures are worse because of grossly inefficient conduct of advanced development.

During the last decade, reliance upon ad hoc committees for evaluation of technical validity, schedule accuracy, and prospective costs of great development programs has reached new heights. Typically, a designated program director develops specific sets of plans which are then repeatedly reviewed by successive echelons of eminent committees. The effects of this procedure have been profound. The programs approved tend to have a common coloring in technical content and are almost always tagged with unrealistic costs. Eminent technical committees tend to agree on the feasibility of only that which is understood in some detail by every member. Since the members generally represent diverse skills, only elements which fall within the relatively mundane portion of the technical disciplines involved can be agreed upon as worthy. Thus, generally only technical goals of a quite short-range nature can be approved. The fact that the members of such evaluating committees feel strongly that their technical reputations are involved, further inhibits any proposed venture into new fields of technology. Only a small percentage of new development ever pays off as planned, and therefore the prophet who predicts disaster for all novel departure will be right most of the time.

In addition to these technical inhibitions, the technique of successive committee review suffers intrinsically from financial myopia. The rules strongly proscribe any allocation of funds to unforeseen contingencies by the program director in his basic plan. Nevertheless, any worthwhile development must involve such contingencies because if it did not do so, all would already have been known and development would be unnecessary. If the total program as visualized by the program director involved a billion dollars expended over five years, contingency expenditure would necessarily relate to this figure since most of such costs stem from the fact that major elements of the program have to mark time while unforeseen technical difficulties are solved in the afflicted areas. In this instance, the cost of a pause would approximate \$1 million per day. During successive review, such preliminary plans generally have been treated in an almost standard way. The committee involved duly notes that previous similar development plans finally cost two or three times the original estimates before realization and required perhaps 50% more time than anticipated. This has led to the committee arbitrarily increasing the director's estimated costs, usually by a factor of two, and a stretchout of his schedule by perhaps 50%. In the course of repeating this process by successive committees, original estimates are frequently multiplied by 5 or 6 and the time scales substantially distorted.

happily, this provides no solution to the dilemma of accurately planning development work. The unforeseen contingencies are still not pinpointed, and the unhappy program director is asked to prepare new plans embodying the much higher costs and stretched time scales specified. If we assume that at some point the new plans are accepted, a bit of reflection will indicate that the contingencies now encountered will cost many times the originally foreseen values. The program director, in compliance with direct orders, has augmented his program plan by extensive study, redundant approaches, back-up components, etc. This means that the cost per day of his program at any point in its history will have increased by some substantial factor. Thus, marking time when some unforeseen difficulty arises will be several times as expensive. As a result of all this, the amount by which actual expenditures tend to exceed planned expenditures is greater than ever. It is easily seen that such a philosophy leads to continually rising costs for ever less significant achievements. In summary, we are faced with the difficulty that recorded expenditures in the era up to the mid-1950s are incorrect because the project engineer of that time knew that honest disclosure of development costs would lead to termination of his program, and he therefore recategorized such costs as production. Costs encountered since that time are even less trustworthy, because of a rigidly enforced regimen of unrealistic administration. Thus, for prospective major development programs today, admirable though the philosophy of cost effectiveness be, the derivation of meaningful cost data is exceedingly difficult.

Effectiveness, the other major element of this prime criterion, is perhaps even more difficult to identify. As long as we discuss phenomena occurring within a continuum, the derivation of prospective effectiveness by extrapolation of past performance seems valid. Today, with our greatly augmented ability to modify great segments of our ecology, such continuity of effect cannot be automatically assumed. In dealing with massive exploitation of space, we are surely facing a great discontinuity in terms of potential benefits to humanity, and therefore we must apply the "effectiveness" element of "cost effectiveness" most carefully. As with the cost element, careful interpretation of past experience can alone provide clues to the techniques which must be employed. Perhaps the most radical aspect of the character "effectiveness" is its potential persistence. The effect of great developments persists indefinitely, and thus the magnitude of such "effectiveness" may approach infinity. When we attempt to render this quantity more tractable by arbitrarily limiting its duration to perhaps 5 or 10 years, we are indeed able to generate finite values for "effectiveness" - but are they meaningful? Is it of any significance to assess the value of the invention of the wheel in terms of its return to humanity during the first 10 years after its advent? Perhaps on this basis our remote ancestors would have concluded that it was not worth the effort that went into it.

One of the first massive investments in technology in the course of the development of this nation was the effort poured into the establishment of rail transportation. This involved many billions of dollars during the 19th Century when such magnitudes were quite impressive. The driving force for much of this was facilitating the movement of supplies for the Civil War armies, and consequently philosophic debate was limited. It is interesting to conjecture what might have happened had there been no Civil War. Based upon extrapolation of existing overland traffic, all horse drawn, it would have been impossible to justify building these railroads which proved vital to the development of the nation. Millenia of such extrapolated traffic would have been required to justify the expenditures involved. Had the philosophic moods of the present prevailed, it would no doubt have been conclusively proved that railroads should not be built because they could not be justified on the basis of cost effectiveness. In recent years the nation's advanced industries have been undergoing what has been aptly termed the cybernetic revolution. In the course of this, the large general-purpose digital computer has occupied an evermore important role. One typical use of the computer has been the parametric exploration of proposed designs. As an example of this in the shipbuilding world, the effectiveness of ocean-going freighters has been explored by computers as a function of the variation of a large number of key parameters. These include such things as length, beam, type of powerplant, gross weight, speed, etc. In a totally unrelated series of activities, the aircraft industry has indulged in studies which have led to the development of the so-called jumbo jets. There is no doubt that these jumbo jets will be able to accommodate transoceanic cargos at much lower costs than preceding aircraft generations, and that at some speed of surface vehicle, and some unit value of cargo, they will become more cost effective than ocean-going freighters. Nevertheless, regardless of how we caress the computer, and in spite of the fact that some companies now claim they can generate several thousand designs per hour this way, it is quite evident that the computer will never come out with a ship design even remotely resembling the 747 or C5A. While effectiveness can be explored with extreme thoroughness today within regions of a technical continuum, transcending discontinuities require human discretion. Perhaps one of the most interesting examples of improperly assessed effectiveness is the airplane itself. Who, during the historic first flight at Kitty Hawk, could have predicted the effectiveness of the airplane, in myriad affairs of humanity, during the next 60 years? Had the fate of the airplane been subjected at that time to the clear, cool, unimpassioned process of evaluation characterizing today's choice of what offering of technology is to be assailed, it would have died aborning. Throughout all of these examples, and the many which we do not have time to consider, the same troublesome thread runs. Effectiveness, in terms of use to mankind, of truly worthwhile development persists through centuries and millennia and consequently assumes a value, in terms of dollars for human effort equivalence, which approaches infinity.

Therefore, while cost effectiveness in terms of value to humanity per dollar expended is an attractive concept, it is also a very dangerous one. It can legitimately be applied to poor development work, where the gains evolved will indeed be limited to the 5 or 10 years considered by most studies. Or it may be applied to relatively insignificant development work where we do not transcend the limits of some technical continuum, and where extrapolation of past experience will provide an accurate mirror of what is to come. Where does the development of low-cost space transportation fit into this spectrum? In the case of the railroad and the airplane, the postulation of effectiveness in terms of potential contribution to the well-being of humanity for a limited period, as based upon extrapolation of past activities, would have doomed them for eternity. Yet in space, which in many ways represents a greater discontinuity with the past than either of these great inventions, we are proceeding along these very lines. In the face of this, it is remarkable that our efforts in space persist even with minuscule advances. In spite of the great cybernetic tools with which we have analyzed all of the obvious facets of the potentials of space, we have not been able to justify the costs involved in developing low-cost transportation. Is it possible that with all of these wonderful instruments at our disposal, our activities in space have been defined almost entirely fortuitously and that the tools have been largely employed to rationalize this course of action? There is some good evidence to this effect.

The first payloads were necessarily and appropriately boosted into space by slightly modified ballistic missiles. Since there was no alternate mode of accomplishing this, and much technical curiosity could be categorically satisfied in this way, this was highly appropriate. To encourage the growth of our space capabilities while lacking any real feel for eventual usefulness, a widely understood objective was established, capable of focusing the energies of a substantial segment of our technical population. The goal of placing a man on the Moon by 1970 has served indeed to guide engineering and science into development of analytical techniques, fabrication methods, and operational procedures pertinent to space activity. The goal itself, I feel, was of little importance. With success having been achieved in the coalescence of a respectable national effort, perhaps the time has come to select more meaningful goals, in the sense of contributions which space can make to the well-being of humanity. Since the inception of the nation's space effort, all missions undertaken have had to be compatible with the modified ballistic missiles which have served and continue to serve as our space launchers. As a consequence of this, because of the very high cost of launch and the inaccessibility of the device after it has been launched, consideration has been given only to the execution of missions involving orbiting of relatively small, very high cost equipment of great assumed reliability and of some minimum persistence of activity in space. The bounty of space is so great that even with these severe limitations, some useful returns have been gained. An impact has been made in the fields

of communication, navigation, and observation including, particularly, weather reporting. It is important to recognize that these were all established when the technology of space launch vehicles was in its infancy and indeed represented simply the modification of existing military devices. Limitation of space missions to constraints consistent with simple extension of this line of technology would in some ways be comparable to diverting the efforts of the nation for perhaps 50 years to improvement of the horse car as the advanced form of the railway train, or the massive exploitation of the triplane as representing the future of air transport. In spite of our magnificent tools of analysis, or perhaps because of them, very little effort has gone into the development of true low-cost space transportation systems.

The necessity to coordinate large numbers of technically prominent organizations and people in the execution of modern major development programs has focused a great deal of attention on the art of management. Surely the ability to apply discretion in the application of such things as systems engineering, planning, and cost effectiveness falls within the purview of this art. One may perhaps regard management as an adaptive control system, sensing a series of output functions and processing and acting upon these data. This makes a nice block diagram, but it is important to appraise what it senses and how it treats the data collected. Years of observation have convinced me that Figure 1 (A) & (B) below illustrate the major elements of management technique in large American development programs today:

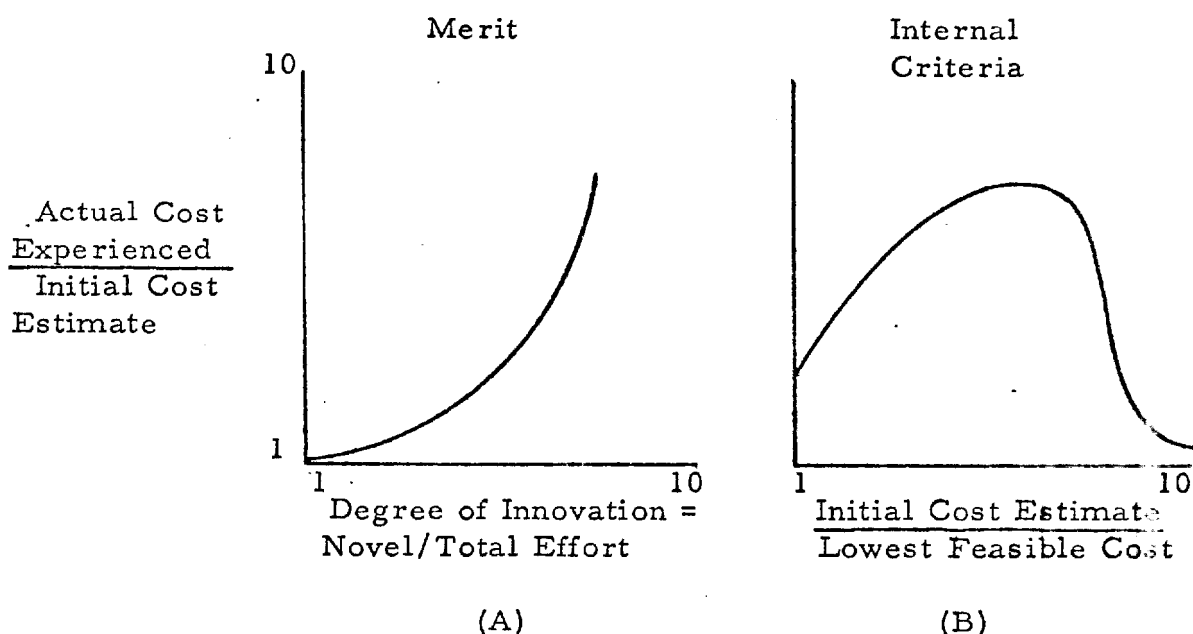


Fig. 1 Internal Management Criteria of Merit

It seems clear that if indeed Figure 1 represents major elements of successful management, the best development program manager is he who eliminates innovation and multiplies anticipated costs by ten. This ridiculous conclusion is reinforced by the unfortunate circumstance that when one is flying on Figure 1 (B) beyond the abscissa value of 5, the controls are reversed; and the more outrageously badly initial costs are estimated, the better the manager looks. Although it must be admitted that this includes a small element of the facetious, the need to use merit indicators relating to phenomena outside of the project or organization involved is clear. Cost effectiveness, systems engineering, and planning as practiced incestuously by program staffs, their consultants and affiliated eminent committees, do not meet this need.

It is difficult to identify any substitute for real competition to provide the incentive to accept risk, court failure, and tolerate mistakes. The learning process is all but completely stifled without these three harpies, and we in this country have gone to great lengths today to cage and hide them. Progress in terms of erudite papers presented is striking; in terms of effective development, it is surprisingly stagnant. A great deal of good development evolves from ill-understood innovation cast in the role of an invitation to serendipity. Rigid adherence to management, based upon internal criteria, goes far toward the elimination of this as well. There is no question that competition is expensive, permits overlap of effort, and engenders short tempers; but it alone provides the priceless incentive to seek out and meet real needs before the other fellow. The Air Force and Navy have been castigated for rivalry in the development of airplanes. We have said with justice that this is expensive and wasteful. I firmly believe that this nation has developed the best airplanes on earth because of this rivalry and if it cost many times what it has, it would be well worth it. How does one fit into cost effectiveness the value of such real incentives as fear of being outdone? The converse of this wasteful story is that of the U.S. Army tank. In this, the history of development is not tainted by passion, inefficiency, and rivalry. Competition has not been permitted. And yet, I do not think that the world regards the American Army tank as a thing of beauty and a joy forever. To be effective, unhappily, competition must be real - it cannot consist of heavyweight brochuremanship, libraries full of urbane studies, incestuously amplified responses to RFPs, nor subtle attempts to substitute elaborate simulation for development effort.

The competition of our space program with that of Russia is inadequate. The industrial potentials of the two nations are not comparable, the objectives of the rivalry are politically selected, and the status of the race is too easily clouded by emotional nationalism and the ease of suppression and distortion of news and its implications. At least two actually independent agencies, prosecuting independent and rival programs, should be established within the U.S. to exploit space (perhaps NASA and USAF). Absolutely minimal efforts to "coordinate"

should be made, and the competitive spirit should be strongly encouraged. This could be one of the few ways to render space activity sensitive to society's needs and to provide management and the nation with external, meaningful, easy-to-interpret indications of progress. It could be hoped that such an approach would result in efforts to contend with real foreseen needs of humanity rather than extrapolating directions of development, fortuitously adopted and rationalized indefinitely, as constituting lowest risk and greatest cost effectiveness. It could go far toward placing in limbo the philosophy of "How do we achieve greatest savings (of what)?" and substituting "How do we most happily employ our constantly expanding pool of trained manpower and superior equipment?"

On the technical side, it is important to recognize the nature of the cost structure of boosting and operating payloads in space today. The approaches we have selected are so complex that without a grasp of what constitutes the major contributors to the extremely high cost of space transport, any attempt to economize is doomed to failure. The actual energy involved in placing a payload into low orbit is about the same as flying it across the Atlantic in an airplane, and thus the intuitive feeling is fallacious that great improvement in costs necessarily await development of stunning new powerplants. The two major costs which dwarf all others today stem from extremely poor utilization of very expensive launch facilities at Cape Kennedy and Vandenberg Air Force Base, and the pattern of repetitive, relatively ineffectual testing which has come to characterize space hardware. It seems clear that we have fallen into this situation by extending the technology of the single-shot ballistic missile and fighting off any temptation to adopt a rational alternative by massive philosophic roadblocks mostly in the form of "cost-effectiveness" studies. Currently, the annual payroll of Kennedy plus Vandenberg is approximately \$400 million. By extrapolating this and factoring in anticipated space launch rates, one has to conclude that this item alone will continue indefinitely to absorb hundreds of dollars for each pound of payload launched.

All good transportation systems involve a careful balance between safety, reliability, capacity, and cost. The air transportation system of this nation might illustrate how this compromise is achieved. Safety is attained when hazard to passengers, crew, payload, and the nation at large is minimized. Commercial airlines do not seek this solely, or even largely, by placing undue stress upon reliability. Reliability simply denotes the ability to perform as predicted. It is the rare transcontinental flight during which no component failures occur. Thus, reliability need not be extremely high. Safety is primarily assured by rigid adherence to the principle that components shall not fail catastrophically, and that the device be capable of mission completion or intact abort after failure. I don't think the profit and loss statements of the airlines would be attractive if the passengers knew that the failure of a component would probably destroy the airplane. I think this

would be so even if they were told that a rocket would pull them out of the comfortable airplane fuselage in an escape capsule (sometimes) a full 76.2 milliseconds before the explosion occurred.

The capacity of a transport system is a sensitive function of utilization, load factor, ratio of payload to gross weight, speed, and size. These are not completely independent parameters and indeed, because of this, flexibility and versatility are cardinal requirements. Because the airplane can accommodate a wide variety of payloads and fly in and out of airports all over the world on demand, utilization and load factors are good. Economy has improved with size, but this is largely a reflection of lower labor costs per passenger or unit cargo, in both direct and indirect categories. Many current commercial airliners are approaching 40% utilization and this, coupled with reasonable load factors, yields excellent economy of operation in spite of rapidly increasing capital costs per vehicle. Speed, a function of technical refinement, contributes to economy (cost effectiveness) by increasing passenger and ton miles produced per hour at a greater rate than that at which operating and fixed costs climb. Payload to gross weight has frequently declined as a function of time during the last two decades, but here again system economy has improved, a phenomenon which would be impossible except for reductions in relative test and maintenance costs and substantial gains in amortization of investment in terms of ton miles produced per lifetime. Utilization is incredibly poor for all major elements of our space programs. Launchers serve in the role intended a very few hours in any given year. Launch vehicles are somewhat difficult to place in this context; but if one considers that a functional flight involving less than a half hour of boost is the total effective contribution of such a device, then this divided by the time elapsed from the moment of its acceptance test at the contractor's plant seems to be the quantity we seek. It is, of course, appallingly low. The load factor of our present generation of vehicles seems reasonably good. We seldom launch them without a full load of something, but when this something is sand or other inert material, does this contribute to low cost operation? Because of the severe constraints to which space payloads must accommodate in terms of geometry, weight, power requirements, etc., it is a time-consuming process to nurse all payloads to meet operational criteria simultaneously. As payloads rise with the introduction of larger boosters, this situation will deteriorate and I expect load factors of the future to decline perceptively. The payload-to-gross-weight ratio of current equipment is very poor, and this does indeed reflect limitations of technology, the specific impulses offered by current propellants and mass ratios reflecting today's structures. It is difficult to raise this parameter much, and it will be necessary to contend with this situation for years to come. It is not, however, necessarily a crippling deficiency in terms of gross costs encountered. Rather, it merely underscores the absolute necessity to amortize the structure of these boosters over many flights, a technique to which of

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slightly modified single-shot ballistic missiles. All of these characteristics impose a sharp limit upon capacity. Although during 1966 the investment of the United States in space reached \$45 billion from the date of its inception in 1955, which is more than the total value of the rights of way of American railroads, we are taxed to place a half million pounds into orbit annually. As transportation systems go, this is not even a prototype figure, and the effectiveness of current payloads reflects this. It will be seen, then, that because of adherence to a fortuitously chosen technology we have, in space, warped the balance between safety, reliability, capacity, and cost. We rarely meet our scheduled launch dates. We can place very little payload into space per year. It is extremely costly to do this. And it is evident that we have sacrificed all of this for the achievement of very marginal safety.

If we can overcome our carefully laid philosophic barriers, is there a technical solution to the acquisition of a mature space transport system? Every major aerospace contractor in this nation has answered this question affirmatively. The solution takes the form of a reusable vehicle. The word "reusability" is intended to imply a device which takes off and lands repeatedly with the necessity for no more than the type of inspection that characterizes airplanes between flights. The mere recovery of the components that make up the device, with the need for elaborate re-assembly and inspection, would accomplish little or nothing. We must invest in these vehicles the versatility and flexibility which characterize the airplane in order to enable it to transport diverse payloads over varying routes in many types of adverse environments. To accomplish this, it will be essential to return to established engineering principles universally applied to development of economical, critically loaded vehicles.

Historically, two approaches to this have been successfully pursued, the choice being dependent on the cost, size and complexity of the device in question. For relatively simple, low-cost items such as aircraft forward firing rockets, the establishment of a qualified design through thorough, sequential, and destructive testing of a large number of missiles is followed by continuing destructive acceptance tests of a significant percentage of all units produced during the lifetime of the project. Such a procedure is regarded as inappropriate for space boosters on the grounds of economy and industrial capacity. The classic alternative to this procedure, adhered to by airplane builders, involves establishment of a qualified design through subjecting the entire assembly to a broad envelope of combinations of adverse circumstances, to establish a well-defined region of safe flight. Such a flight test program, thoroughly exploring the interaction of all major subsystems and components over wide ranges of ambient parameters, frequently involves thousands of hours of flight test, and thousands of engineering changes prior to the qualification of a specific design. Each individual item produced to this qualified design must then pass an acceptance test to

establish that its deviation from approved standards in all significant respects is not excessive. In sharp contrast to this, the flight test program for a typical space booster may involve less than two hours of highly stressed powered flight with deviations from design point operation generally only occurring inadvertently. Under such circumstances, no true envelope of safe flight operability can be established. For this reason, comparisons of flight test costs for expendable as compared to reusable vehicles are generally unrealistic. The contention that it costs less to ineffectually flight test expendable vehicles than to effectively qualify reusable vehicles is indeed true, but has little pertinence. Such rump qualification procedures inevitably result in the prolongation of flight test over the life of the program in the form of greatly augmented acceptance and operations costs. A key quality of the typical reusable device which permits it to be subjected to thorough and extensive flight test, is the ability to perform intact abort after failure of key components and subsystems. This characteristic deriving from a careful reliance upon selected redundancy and stern adherence to the requirement that component failure modes be noncatastrophic, permits thorough exploration of operational envelopes without the penalty of unacceptable risk. It is difficult to see how this quality can be engendered into the type of space booster selected for current and prospective space operations by this nation.

Exorbitant costs in dollars and reliability for achievement of minimal safety follow directly from these considerations. The acceptance test of the reusable vehicle is thorough and realistic, since the interaction of essential components and subsystems can be directly observed under true, extreme flight conditions. Over a period of time acceptance test elements which are found to be irrelevant are omitted and information from these tests as well as that deriving from field operation is employed for the progressive refinement of design which characterizes this class of vehicle, and tends to cut operational costs profoundly as a function of time. In sharp contrast with this, each flight of an expendable device is preceded by an extremely elaborate acceptance test. Because such a vehicle has never been flown before and cannot abort intact, recourse must be made to elaborate simulation methods in assessing the complex interactions of major subsystems and components. Such acceptance testing is far more expensive than that associated with reusable devices because it must be completed in full before each flight and because much more elaborate equipment is required for simulation than free flight. It is much less effective than that associated with reusable devices because the simulations involved are necessarily far from perfect, and the feedback process whereby the design of reusable vehicles is progressively refined in the course of time is almost lacking; or to an extent, reversed. This unfortunate reversal of diminishing test costs with time occurs when unanticipated problems are observed in the course of operational flights. Since these are rarely supported by analysis and resolution, they generally

result in further augmentation of the initially elaborate and relatively ineffective test procedures followed.

Some recent studies have indicated that the cost of space payloads has now become so overwhelmingly greater than the cost of construction and operation of the boosters associated with them, that efforts to improve space transportation economy should be focused on these payloads rather than on boosters. The relative costs quoted in these reports are accurate, but a careful consideration of the derivation of these may be in order.

Since no official projection exists of the United States space program beyond a few years, a Rand study (AIAA paper 66-862) will be employed as a possible model. This prognostication places 72 million pounds into low earth orbit between 1970 and 1999 employing current and advanced versions of Saturn boosters. Payloads include both manned and unmanned spacecraft, and indeed the unit costs of these are surprisingly high - about \$27,000 per pound (average) for unmanned and \$10,000 per pound for manned. These data are based on information provided in Tables 1 and 2 (pages 15 & 16) derived mostly from Aviation Week. If we assume that 50 million pounds will be placed in low orbit and 22 million in equivalent orbital weight will be employed to place 5 million pounds in escape trajectories, and that spacecraft will continue to cost the same per pound as current species; total spacecraft cost would be 635×10^9 dollars. Booster cost given by Rand is 109×10^9 dollars but presumably does not include support operations. If we add such support costs at \$2 billion over the 30-year period and the construction of three more launch site 39's at another billion, total launch costs would approximate \$112 billion. These launch costs then would amount to 112/747 or just about 15% of the total. This seems to confirm the assertions that launch costs constitute a relatively negligible element of total cost, and that efforts to reduce costs of space operations should be concentrated on spacecraft.

But is this reasonable? Have we perhaps lost our sense of proportion and are depending too heavily upon internally developing criteria of merit? It will be seen from Table 1 that the established Lunar Orbiter program will end with five spacecraft costing \$43,500 per pound. This is about six times the cost of an equal weight of pure gold. Is there anything intrinsic in the make-up of this device that assures such costs into the indefinite future? Almost certainly the answer is "no." The extremely high costs indicated in the two tables result primarily from efforts to achieve mathematical indications of almost impossible reliability and the perpetuation of extremely elaborate but nonetheless questionable testing. An indication of this is found in the fact that according to Technology Week of February 13, 1967, components built, but not used, could be assembled into a sixth spacecraft for about \$2.5 billion.

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Table 1

SPACECRAFT UNIT COSTS

SPACECRAFT	WEIGHT (lb)	NUMBER	TOTAL SPACECRAFT COST (\$ millions)	TOTAL WEIGHT	SPACECRAFT UNIT COST (\$/lb)
SURVEYOR	2750	7	488.9	19,250	25,400
LUNAR ORBITER	800	5	174.6	4,000	43,500
OSO	490	8	94.8	3,920	24,200
OGO	1000	6	219.1	6,000	36,500
OAO	3900	4	436.9	15,600	28,000
BIOSATELLITE	1200	6	136.5	7,200	19,000
IMP	158 (avg)	10	56	1,580	35,400
ATM. EXP.	495	2	6.8	980	6,900
ISIS	110	13	28.6	1,430	20,000
Totals			1,642.2	59,960	

Calculation of Average Unit Costs

$$\text{Average Spacecraft Unit Costs} = \frac{1642.2 \times 10^6}{59,960} = \$27,400/\text{lb}$$

Source of data: Space/Aeronautics January 1967

Table 2

GEMINI COSTS
(Millions of Dollars)

<u>FISCAL YEAR</u>	<u>SPACECRAFT</u>	<u>LAUNCH VEHICLES</u>	<u>SUPPORT</u>	<u>TOTAL</u>	<u>SOURCE</u>
1962	39.20	13.60	2.16	54.96	AvWk 1/21/63
1963	205.05	79.11	3.94	288.10	" 1/27/64
1964	280.52	122.70	15.68	418.90	" 2/1/65
1965	165.30	115.40	27.70	308.40	" 1/31/66
1966	98.87	72.90	25.50	197.27	" 1/30/67
1967	9.15	2.90	9.55	21.60	" 1/30/67

Totals 798.09 406.61 84.53 1289.23

Percentages 61.9% 31.6% 6.5% 100%

Calculation of Unit Costs

10 vehicles \$79.8 M per vehicle
8000 lb/vehicle 80,000 lb total spacecraft

$$\text{Spacecraft Unit Cost} = \frac{\$79.8 \times 10^7}{80 \times 10^3} \approx \$10,000 \text{ per lb}$$

ability to inspect, maintain, and repair these devices in orbit could provide another order of magnitude of reduction in basic cost. Such a potential could only be provided through employment of reusable launch vehicles in operational patterns analogous to those of aircraft.

If all spacecraft costs could be decreased by an order of magnitude as apparently those of Lunar Orbiter can, the total program cost for the Rand 30-year effort would drop to \$175 billion and the launch cost would constitute 64% of the total. Reduction by two orders of magnitude would render spacecraft costs relatively negligible and boost costs heavily predominant. Moreover, if the nature of space missions changes, with long-term economically meaningful orbital operations becoming predominant; the replenishment, maintenance, and repair of vehicles will involve most transport effort. It is difficult to see how exploitation of space for the advantage of society can proceed in any other way; and under those circumstances, the cost of boost and alighting operations becomes all important. Thus it can be seen that the argument against development of low-cost boosters on the grounds that this constitutes an insignificant element of space expenditure is by no means unassailable; and, indeed, unless the nation's space activities have led to a new "breakthrough" (the first event to my knowledge worthy of this peculiar term) which guarantees that spacecraft fabrication will be orders of magnitude more expensive than that for comparable devices, into the indefinite future, the assumption is probably groundless.

Can low-cost, flexible, versatile boost and alighting systems be developed? Are there technically insurmountable barriers to their evolution? These questions cannot be answered categorically at this time, but I firmly believe that the nation's continuing unique concern with the development of ever larger, solely expendable booster systems will prove an impenetrable barrier to the advent of any economical approach to space transportation. Masses of literature have been written to demonstrate conclusively that reusable systems will inevitably prove less economical than projected expendables. I believe that this persistent contention represents an outstanding instance of confusion between ephemeral considerations and fundamental technical limitations. When we strip off the costly aspects of an immature unchallenged transport system which stem from low utilization, inefficient test procedures, cumbersome patterns of operation, and inefficient use of manpower; we are left with fundamental considerations of which two become all important: the amortization of structure and the cost of energy conversion.

With respect to these two elements, we have already seen that amortization of structure for single-shot devices is inevitably extremely high and always seems to exceed \$1,000/lb of payload placed in orbit. The other fundamental element of cost, energy conversion, has generated confusion ever since the advent of space activity. The one excludes economically worthless exotic varieties, is of the order of

\$2.00/lb of payload placed in orbit. When one regards this as an index of propulsion effectiveness, it is easily seen that the persistent effort to develop high-performance rocket engines, in the sense of improvement in specific impulse, simply reflects a way of contending with the very high cost of structures associated with space transportation. The actual cost of propellants involved is insignificant. The direct key, then, to achievement of low-cost space transportation, after one disposes of ineffective excessive test and poor utilization, lies in the ability to amortize structure much more effectively. To date, substantial effort has been focused upon work to improve ratios of payload to gross weight or, better, ratios of payload to dry weight of prospective boosters. Since this figure for today's devices is abysmally low, substantial improvement is possible. However, it is doubtful whether we can improve on current values by more than perhaps 100%. As opposed to this, when we consider dollars per pound in orbit as a function of numbers of flights during the life of the boost vehicle, through employment of rational assumptions one can conceivably cut this cost of amortization by two orders of magnitude or better. A plot of dollars per pound in orbit as a function of numbers of flights per lifetime of boost vehicle could show the effect of potential improvement in the ratio of payload to gross weight as simply a slight broadening of the curve produced.

During the past decade every major aerospace organization in the United States has studied reusable boosters which alone promises massive reduction in this enormous cost of amortization of structure. In a recent study sponsored by the government intended to indicate directions of propulsion development which should be sponsored for reusable boosters, 36 "advanced" composite engine configurations were carefully investigated with the conclusion that none was significantly better than powerplants employing well-established technology. A study of the reusable designs submitted over the past five years drives one to the conclusion that with no great advance in technology, two-stage, chemically powered, completely reusable devices can provide the means for releasing the inhibitions which threaten to starve our space programs into the indefinite future. A normalization of the majority of the designs available for study indicates that a two-stage, all-reusable booster with either airbreathers or mixed airbreathers and rockets for first stage, and rockets plus sufficient airbreathers for systematic re-entry for second stage, with a gross weight of approximately one million pounds, can carry about 30,000 pounds of payload into low orbit and return. More highly refined versions of this - embodying advanced lifting structures, droppable tanks, and almost single-stage-to-orbit configurations - might better this, but the simple pedestrian design embodying least advanced technology is revealing. In terms of amortization of structure, this would cost about \$25.00/lb payload if we write off the equivalent cost of each booster over 100 flights, or it would amount to \$2.50/lb if we perform 1000 missions for the equivalent basic hardware cost. At this point the ratio of cost of energy transformation

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to amortization of structure falls into the general ballpark characterizing all good transportation systems.

What is wrong with the reasoning? The major contention of the opposition is that a "refurbishment" rate of 10-50% is all that could ever be achieved for such systems. This translates into 2-10 flights per booster lifetime; and if this were so, the approach indicated is obviously invalid. I think this assumption could well be realistic if one talks in terms of recoverable devices; that is, vehicles which somehow struggle down to the ground in a graceless attitude to a slightly controlled crash and are then torn apart, carefully examined by little men with microscopes, and reassembled and elaborately tested before the next flight. This is not the concept to which I am alluding. It is essential that if such devices are to prove economical they must take off and land on a pattern similar to that of commercial airliners. In achieving this, one will not merely profoundly reduce today's astronomical costs of amortization of structure but deeply cut into the tremendous costs associated with excessive, ineffective acceptance testing, pre-flight testing, exceedingly poor utilization of launch facilities, sacrifice of performance for achievement of specified trajectories by orbital transfer in space, recovery of men and equipment after picturesque but expensive splashdowns, a degree of unreliability which has frequently delayed scheduled missions for many months at enormous cost, etc., ad infinitum.

It is interesting to note that the commercial airliner of today has a refurbishment cost well under 1/10 of 1% per launch, and to consider why a space transporter could not approach this figure. The typical powered flight time and the duration of the period when the structure is subjected to high temperatures are both much shorter than those associated with typical aircraft missions. It is true that the temperatures to be experienced are much higher; but it must be recognized that the stresses involved in this segment of the flight will relate simply to envelope materials which do not constitute the major element of the cost of such vehicles. We have the choice of cooling critical elements of this structure by radiation, ablation, or the use of propellant heat sinks in various limited ways. Why are we so pessimistic regarding the potential effectiveness of such systems when the little pertinent experience we have had with the X-15 looks reasonably optimistic? The nation has committed itself to the development and operation of the SST. The first-stage vanes and blades of the turbines for this vehicle will experience conditions relative to temperature, stress, and oxidation, somewhat similar to those postulated for key parts of the outer surface of the space transport. No one involved in this program, however, expects these engine elements to have lives of minutes, necessitating their replacement after each flight. Any company postulating such a design would be laughed out of the competition. Yet the time per flight during which the surfaces of the space transport will be subjected

to these stringent conditions will be very much lower. I think those who feel this to be an impossible problem are encouraged in their convictions by the uninspiring attitude toward development risk which has characterized our space programs.

In spite of the fact that significant efforts to develop adequate, refractory, high-strength skins for radiation-cooled re-entry structures have so far met with only partial success, I believe that several promising solutions to this problem are apparent. It is only in recent years that knowledge of solid-state physics has reached the point where it can contribute heavily in these difficult areas, and full advantage has yet to be taken of this new capability. Unfortunately, even a relatively superficial treatment of this subject would warrant many pages of discussion, and so I will limit myself to an indication of directions which look inviting. Fiber technology and its end product, composite structures, have advanced markedly in the last few years. Unfortunately, a great deal of stress has been concentrated on a material which promises little by way of high-temperature strength and diffusion resistance. Other fibers much more promising with regard to these two properties are known, and the task of transforming these into engineering materials has not been vigorously pursued. Inevitably, the matrices incorporating these fibers will require highly refractory properties. Techniques permitting these materials to operate under high-temperature oxidizing conditions without surface deterioration or fiber diffusion will require much improvement in the art of creating and positioning diffusion barriers.

Use of such materials as graphite for structural members could provide another solution to improving the life of re-entry structures. Although little success has been achieved in the development of coatings for refractory metals to date, here again improvement in diffusion barrier techniques could change this picture profoundly.

With regard to propulsion requirements for economical, safe, reliable space boosters, relatively little effort has been devoted to achievement of the performance required. The heavy stress placed upon exploration of exotic composite engine studies during the last five years seems to have masked the fact that employment of refined, relatively conventional powerplants in discreet rather than composite units will provide at least adequate and probably best solutions for the reusable space transport. If such vehicles are ever to achieve the flexibility and versatility of operation which alone can improve load factor, utilization, and test and inspection costs to the point where space can be exploited effectively; high performance, in the sense of thrust-weight ratios and specific impulse, is of relatively secondary consideration.

In all probability, over the next two decades attractive concepts for space transports will involve two classes of engine: airbreathers and rockets. By operating within the atmosphere, airbreathers can

provide economy in take-off, flexibility in selection of points of embarkation and return, and marked improvement in ratio of payload to gross weight as hypersonic airbreathers are refined. These objectives can be achieved without recourse to supersonic combustion which could probably provide only marginal improvement, if any. The type of device visualized for this work has been broadly characterized as the turboramjet, and careful studies have shown that foreseeable improvements in currently known approaches can easily lead to better than a doubling in ratio of payload to gross weight as well as providing enormous flexibility in greatly extended downrange and crossrange capabilities. The second stage operation will almost certainly be heavily dependent upon rockets although airbreathers will be needed here, too, to assure adequate landing capabilities in conformance with air traffic control regulations. The rockets required for this task, however, should bear little resemblance to those employed by the current generation of single-shot space boosters. The major requirements for such rocket engines follow:

1) Broad Intrinsic Stability. The engine should operate stably over very broad limits of chamber pressure, mixture ratio, fuel composition, oxidant composition, and pertinent contamination concentrations. This stability should prevail under both steady-state conditions and a wide range of transient patterns. The stability required here, indicated as freedom from undue pressure oscillations, should be achieved through the design of its intrinsic basic elements and their relation to each other. Dependence upon external sensing and correcting servo equipment introduced solely for this purpose should be absolutely minimized. Past experience has demonstrated that extensive reliance upon such systems invites unreliability through the inevitable fragility and precision of setting required for sensors, which lead to numerous instances of false signal-induced shutdown.

2) Benign Failure Modes. As contrasted with reliability, the safety of any transportation system heavily depends upon noncatastrophic failure of major elements. Achievement of high reliability levels alone could simply imply that a fraction of payloads equal to one minus this reliability, as well as a similar fraction of the vehicles involved, would steadily be destroyed during the course of operations. It is strongly felt that no transportation system should accept such a handicap. As an unfortunate heritage of the nation's ballistic missile experience, many people are convinced that failures of rocket engines are necessarily catastrophic. This belief is so strong that most rocket-powered transports developed or contemplated have paid small attention to improvements in safety and reliability achievable through ability to operate with one or more engines shut down. In large measure, this mental coupling of unanticipated shutdown and catastrophe in the case of rocket engines derives from the operating characteristics of the liquid-liquid impinging jet, gas generator fed cycle which has dominated rocket development in recent years. With such engines, a relatively small percentage of

design point values in many operating parameters tend to induce catastrophic failure. A rocket engine suitable for transport service should be required to demonstrate hazard-free shutdown or compromised safe operation under many conditions of single critical component failure. The scope and pertinence of tests required to confirm the benign shutdown quality will greatly contribute to transportation system safety achievable.

3) Transient Overload and Self-Healing Capabilities. Propulsion systems intended for transportation functions have traditionally been endowed with the ability to accept transient overloads with minimal damage and hazard. Typically, such an experience simply results in the shortening of the anticipated period between overhauls. Ideally, it would be desirable to confer a self-healing quality upon the engine to minimize incidence of failure due to progressive weakening by transient overloads. The typical tubular-walled regeneratively-cooled thrust chamber and throat of most of today's rocket powerplants constitute an interesting instance of lack of this property. Adequate heat transfer demands that these tube walls be thin and fragile. Any asymmetry of injector discharge due to distortion, partial failure, or ingestion of foreign material tends to focus higher heat fluxes on concentrated areas of these tube walls. Typically, this can result in rapid burnout and catastrophic failure. Substitution of a structure in which local regions of high heat flux will simply cause erosion to a new configuration of equilibrium heat transfer without structural failure would be highly preferred.

4) Efficient Throttleability. Achievement of the broad intrinsic stability requirement heading this list will assure the technical feasibility of throttling. However, ability to perform the transport functions indicated in the manner characteristic of all accepted transport systems will require more than simple technical feasibility of partial power operation. The ability to attain mission objectives in spite of single or more engine failure has been regarded as essential to military aircraft systems of the bomber and transport category. It would seem that this type of requirement has not been established for transport booster systems primarily because of an intuitive feeling that uncompromised performance capabilities would suffer unduly. The development of adequate rocket engines, however, will profoundly affect this assumption. Necessarily, orbital boost vehicles will incorporate high mass ratios and low structural weights. Also, the rocket engines employed will discharge the major source of mass on board the vehicle - the propellants - at a very high rate. As a consequence of these considerations, achievement of the ability to safely abort or attain one's mission objectives with single or more engine outage may be simpler than for typical airbreathing aircraft. When the space booster is constrained to follow an acceleration-limited trajectory, as economy and physiology will inevitably demand, within relatively short time intervals, the ability to safely abort or attain one's mission objectives with single or more engine outage may be simpler than for typical airbreathing aircraft.

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the point where the thrust deficiency arising from failure of a single unit can be made up simply by raising the thrust produced by the remaining engines to their rated values. This capability is a strong function of the number of engines employed, vehicle-engine geometric configuration, and throttleability. This important aspect of orbital boost can be exploited most effectively if performance loss in terms of degradation of specific impulse is minimized with throttling even at relatively low altitude. In addition to providing the capacity to meet mission objectives with partial powerplant failure, adequate throttleability should enable the pilots of such booster craft to check out their engines before each flight, systematically and without excessive propellant loss, in conformance with intelligent established practices. While it is true that such takeoff procedures will decrease spectacular display and minimize heroic qualities demanded of the crew, they are essential for transportation system effectiveness.

5) Performance. Although four key transportation qualities have been designated as indispensable, in some ways they may be summed up as simply contributing to cost per ton or cost per ton-mile delivered. For terrestrial transportation systems where amortization procedures for financing hardware elements are well developed, cost of fuel and ton-mile per hour capacity per capital dollar invested are established criteria of merit. Unfortunately, gross expansion of many less fundamental elements of cost has distorted space booster financing to a point where performance criteria are obscure. If one assumes, however, that patterns of orbital boost operation will shake down in the future to something resembling conventional transportation systems, similar criteria to those of terrestrial aircraft will apply. Here economic constraint immediately eliminates consideration of very expensive propellants, and the requirement to develop many ton-miles per hour for a given dollar investment in structure eliminates low performance propellants. Similarly, for orbital booster transport rocket engines, a great culling of propellants to be considered can be achieved by elimination of obviously uneconomic candidates. Engines employing the selected propellants remaining should be capable of operation at values of specific impulse reasonably close to theoretical as established by specifications. The major conventional element of performance other than specific impulse should be treated with care. Whereas thrust-to-weight ratios of typical airbreathing engines clearly constitute an index of superior performance, this may not be the case for rocketry. Envelope geometry, ruggedness, and amenability to effective incorporation into superior vehicle designs are often more significant. Frequently, moreover, simple thrust-weight ratios are hard to determine because of arbitrary weight assignment of elements which might be either vehicle or engine.

6) Life. Perhaps at no other point of philosophy does the air-breathing pattern of operation diverge so profoundly from that of rocket-

powered vehicles as in concept of engine life. Here again, primarily as a heritage from powerplants designed to perform single-shot missions for ballistic missiles, there has been a tendency to accept limitations on rocket life expectancies which are not intrinsic in the engine. As in the case of other powerplants, many elements of life must be evaluated. Total life expectancy in terms of numbers of starts and cumulative duration, time between overhauls, average time of flight, and maximum single flight time requirements have all been considered. Although the heat fluxes encountered in some aspects of rocket operation exceed those of more conventional energy exchange devices by a wide margin, stresses and temperatures experienced in structural materials involved are all within realms of past experience. As a consequence, the life of a rocket engine should have certain similarities to that of other powerplants. This should be particularly so when the first four requirements discussed are achieved. Although it has become customary to discuss life and single mission time of rocket engines in terms of seconds, based primarily upon past missile experience, this seems an unfortunate habit in the context of transport applications. Surely rocket engine lives can be extended to magnitudes of hours and tens of hours based upon past limited experience. While numbers of starts have been regarded as a more appropriate unit of life than simple duration (as for instance hours in the case of a turbojet), this derived in large measure from the consequences of the hard start characterizing most of these devices developed to date. It is probable that if reciprocating engines were always started at full throttle, their lives would also be more a function of numbers of starts than of running time. Because of the duty cycles typically associated with space boost operations, average and maximum running times per mission can be expected to be on the order of minutes rather than hours. It should be recognized, however, that this pattern stems from the inevitably propellant-limited configurations of space boosters, the thickness of the atmosphere, the fact that the earth has a diameter of 8000 miles, and that its rotation is only one revolution per day. The rocket engine itself should be capable of far greater durations of average and maximum operating time per mission than will ever be required. These two requirements, although they have been discussed extensively, are probably of little significance and may well be set far beyond the need of any currently anticipated missions. Time between overhaul, however, is a very real criterion of merit and can contribute significantly to economy and capacity. Since there has been no substantial employment of this criterion in past rocket engines, the approach to be followed initially will have to be reasoned, but arbitrary. It seems entirely reasonable that engines designed to accommodate the first four requirements discussed should be capable of times between overhaul on the order of many hours. It should be recognized that this number, as in the case of airbreathing engines, will inevitably rise profoundly as refinements in design occur which can be accomplished only through the process of inspection, reuse, analyses, and improvement.

7) Restart Ability. On various occasions, established transportation systems have attempted operation with powerplants of limited restart ability. The shotgun and cartridge starters applied to reciprocating and turbojet engines were examples of this. These instances simply served to emphasize the great attractiveness of unlimited restart ability, and most operations have reverted to this condition as soon as technology permitted. In the case of the rocket engine, the feasibility of unlimited restart ability has been heavily obscured by the general character of the powerplants which have been developed. Most of these, because of very limited regions of steady-state stability and lack of efficient throttleability, have been constrained to adopt starting transients of very short time duration with exceedingly closely controlled mixture ratios as chamber pressure rapidly rose. Under such fast start circumstances, ignition energy requirements have tended to be so high that simple electrical devices employed for most other engines have been largely eschewed. Instead, a great deal of reliance has been placed upon carefully sequenced pyrotechnic devices, the use of auto-igniting propellants, or employment of auto-igniting slugs simply during the starting transient. There has been much confusion over the meaning of the word "auto-igniting" and its odd, generally obfuscating, semantic variations. In fact, all rocket propellants are auto-igniting providing the temperature is high enough. Similarly, all have ignition delay lags; and, unfortunately, even with those capable of auto-ignition at relatively low temperatures, this time delay persists and tends to sharply limit equipment design and environmental temperatures permissible. Fortunately here again the ability to engender Items 1) through 4) into the basic design of future engines will greatly simplify the ignition problem. With engines meeting these requirements, simple electrical systems should be capable of providing the unlimited restart characteristic which has proved so attractive to other transport systems. Under these circumstances, the balance between positive and negative aspects of operation with auto-igniting propellants should be restudied. As contrasted with ease and safety of ignition at high ambient temperatures, and ability to contend with momentary flame-out, the hazards of unintentional ignition inside and outside the engine as well as ignition limitations should be considered for auto-igniting propellants.

8) Propellant Jettison Ability. Ability to successfully abort with structure and cargo intact during essentially all of most booster flight profiles may depend heavily on the engine's capability to off-load propellants rapidly. By careful consideration of engine stability limits and cycle employed, it should be possible to jettison propellants rapidly, with controllably limited thrust, by employing the turbines and pumps of the basic powerplants when conditions demand this.

9) Environmental Limitations. The nation's experience to date in boosting payloads into space contrasts very sharply with that of a typical transportation system.

maintained under carefully monitored conditions for periods of about a quarter of a year prior to launch. In the sense that reliability is the ability to schedule events, this is of course appalling. However, with respect to powerplant environmental limitations, such pampering is highly misleading. If boost operations are to progress to a point where economy and capacity will permit major exploitation of space, powerplants must be capable of operating under the broad ranges of environmental conditions characterizing other transportation systems. Without recourse to extensive, very expensive, specialized ground installations, these engines should be capable of starting and operating over wide temperature and humidity ranges, large variations of attitude, incursion of salt air and spray, and with the ability to accept rapid transients in all of these.

10) Instrumentation. The typical air transport displays a series of engine operating parameters on its engine instrument panel. The purpose of this instrumentation is to allow the discretion of the crew to be employed as a contribution to transport effectiveness. Progressive deterioration may be noted and approaching characteristic failures may be sensed before they tend to become catastrophic. Contrasted with this approach, it has been proposed that many rocket-powered boosters be equipped with "malfunction sensors," the purpose of which would be to anticipate failure and initiate an engine shutdown transient prior to its occurrence. Here again the philosophy involved merely reflects the lack of benign failure mode which has characterized most rocket engines to date. Superficially, it would seem that the combination of sufficient malfunction sensors and the development of adequate high-speed servo-loops to initiate shutdown would permit engines with catastrophic failure modes to be employed happily. This is far from the case and unfortunately provision of ever larger numbers of malfunction sensors eventually simply assures a reliability of zero for the system. The larger the numbers of these devices, the greater the probability of failure on the part of the sensor rather than on the part of the engine. This of course is precisely the reasoning behind the type of instrumentation furnished with piloted aircraft. If Items 1) through 4) are incorporated into future rocket engine designs, the functions of rocket engine instrumentation should be very similar to those of airbreathing instrumentation and should probably consist of displays reasonably similar to those of jet aircraft.

Putting together existing and slightly advanced technology in the key fields of refractory high-strength structures and propulsion, we can visualize in the near future types of vehicles well adapted to perform space transportation tasks. Three vehicle types will be covered in the following paragraphs to present some idea of potentials maximizing employment of existing and realizable hardware and covering a range of sizes. In turn, each of these is broken down to show the effects of varying

In the first of these a concept is considered, termed the small recoverable booster/manned hypersonic test vehicle (SRB/MHTV), which is designed to fulfill a very high percentage of currently foreseen launch requirements and to provide a logical step for development of major second-generation reusable boosters. To minimize development cost and difficulty, this vehicle is designed to operate within the flight envelope of the advanced X-15. Moreover, technology required for the fully controllable benign failure mode rocket engines involved has been demonstrated by work on the RL-10 and RL-20 engines. The general configurations of this type vehicle are presented in Figure 2 below:

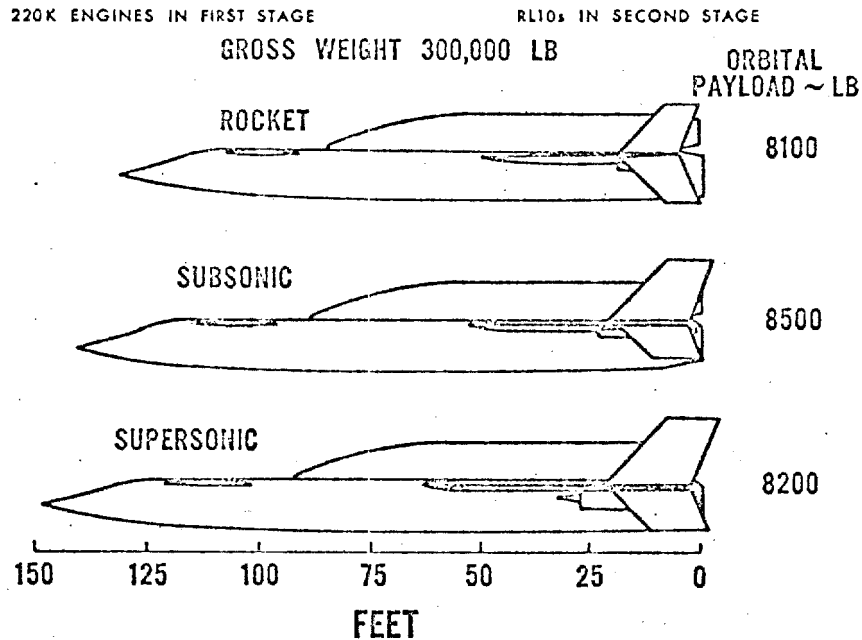


Fig. 2 Basic Small Recoverable Booster Configurations

Each of these vehicles employs horizontal landing and take-off of first stage; grosses at approximately 300,000 lb; and can place something more than 8,000 lb into low orbit. Some penalty (about 15%) in payload was accepted to permit use of engines in existence or under development. The configuration designated "Rocket" employs two 220,000-lb rockets and two TF-30s for first stage, and three uprated RL-10s (20,000-lb thrust) for the second stage. The "Subsonic" configuration used six TF-30s plus four RL-20s (220 K) for first stage propulsion, and three uprated RL-10s for second stage. In the subsonic vehicle, the TF-30s are operated as primary propulsion for take-off and acceleration to Mach 0.9. Rocket engines are then started and accelerate the first stage to 7,000 fps when the second stage is released. Since the TF-30s have a supersonic capability, they are run simultaneously with the rockets to their limiting Mach number and then closed off to protect

them from the high-temperature hypersonic environment. The rocket vehicle is accelerated to 7,000 fps on its RL-20s, and the TF-30s are simply used for flyback. The "Supersonic" vehicle employs two RL-20 rockets plus four J-58s (F-12 aircraft engine) in its first stage and a set of three uprated RL-10s again for second stage. Here the J-58s accelerate the first stage to Mach 3, when the rockets are started to continue acceleration to 7,000 fps. While the supersonic small recoverable booster does not show significant payload advantage, its growth potential, as more advanced airbreathing engines become available, is most attractive. A summary of the configurations discussed is shown in Figure 3 below:

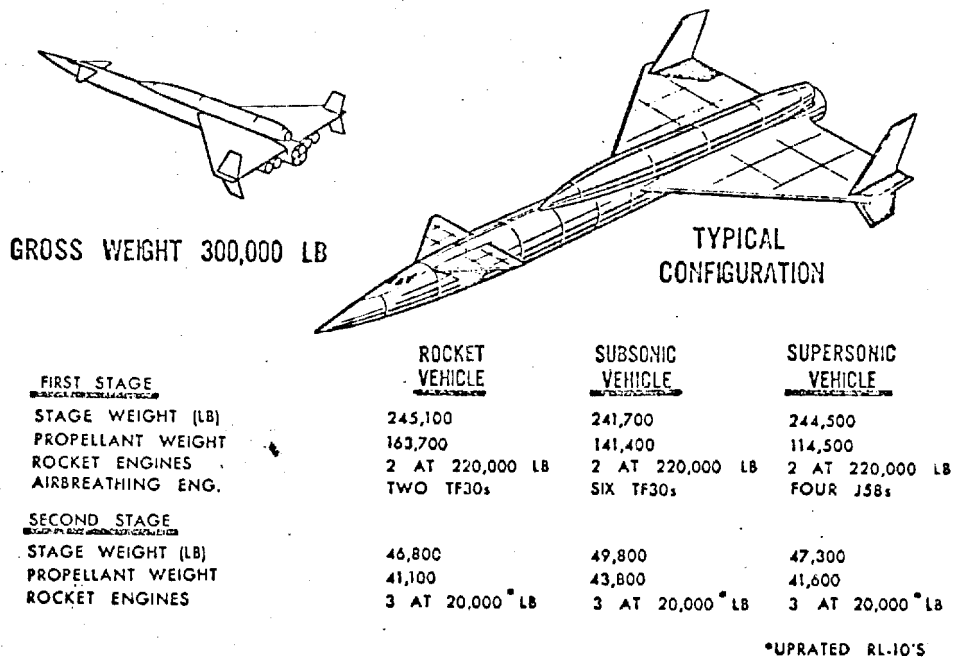


Fig. 3 Summary of Configurations

The optimization studies of the SRB rocket thrust level are summarized in Figure 4 on the following page.

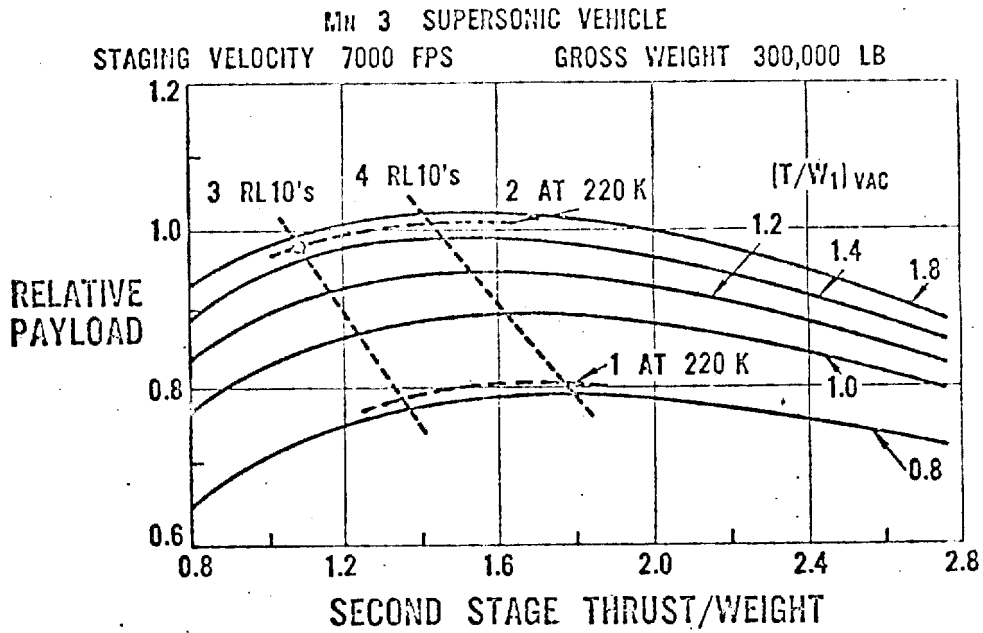


Fig. 4 Optimization of Small Recoverable Booster Rocket Thrust Level

A comparison of dynamic pressure and temperature loadings for the advanced X-15 and the small recoverable booster is shown on Figure 5 below:

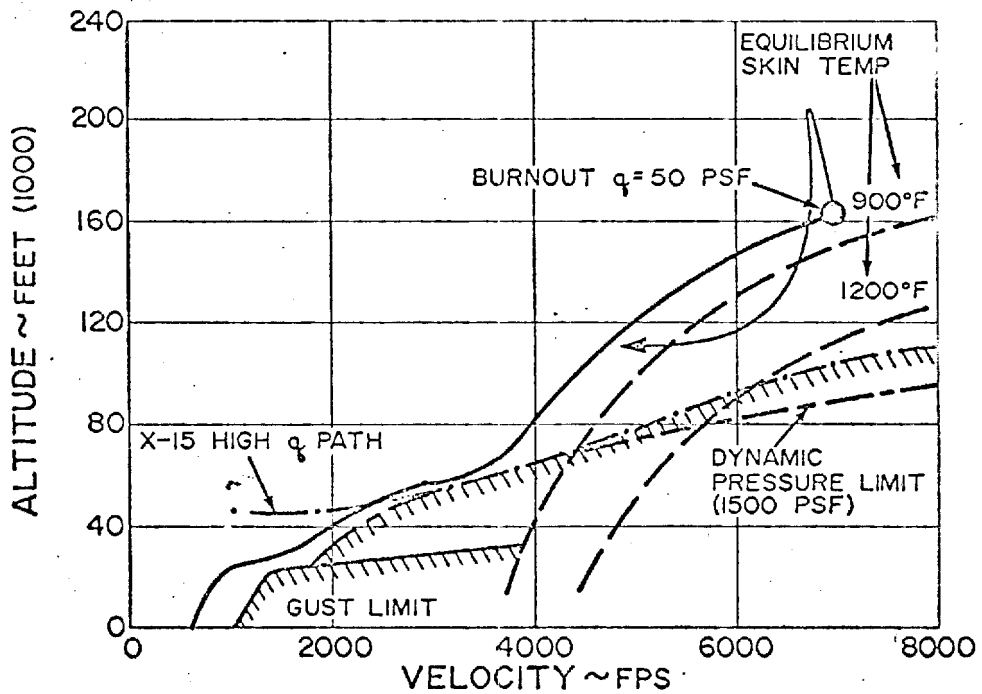


Fig. 5 Small Recoverable Booster Flight Path
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Aside from its role as a space launcher, the SRB/MHTV can be used to extend the flight regime being explored by the X-15. The first stage with no upper stage can attain a velocity of about 10,000 fps. This can be extended to about 12,000 fps if a fuel pod is substituted for the second stage. If the second stage is replaced by a manned research vehicle (a rocket-propelled lifting stage weighing roughly 60,000 lb), the flight velocity spectrum can be extended to beyond 20,000 fps. For all configurations except the rocket vehicle, the flight spectrum of a manned research vehicle can be further extended to orbital velocities if refueling or a more advanced airbreathing engine is used in the first stage.

In its role as a test vehicle, the SRB/MHTV can significantly increase the test time available at any sustained Mach number over that attainable with the X-15. This application, of course, requires throttling of the rocket engines to maintain a steady-state flight condition. Figure 6 below shows the test time available at a given sustained velocity. All three versions of the basic SRB/MHTV plus a fuel pod fall within the shaded band. At 4,000 fps, for example, the improved X-15 offers a little over one minute of sustained flight, while the SRB/MHTV can provide about 13 minutes of sustained flight. At 8,000 fps, the X-15 capability has diminished to practically zero while the MHTV can still provide seven minutes of operation. The test time available above 8,000 fps is still substantial, decreasing linearly with velocity until it finally goes to zero at about 12,000 fps.

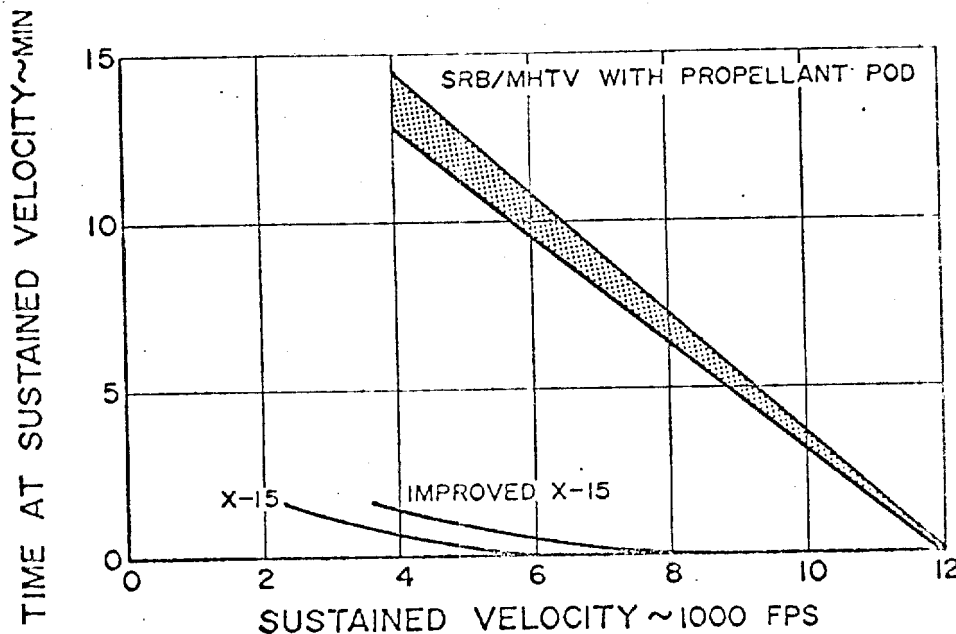


Fig. 6 Available Test Time

A supersonic enlarged version of the small recoverable booster using 220,000-lb thrust rocket engines in both stages has been explored. This is the most interesting of the three earlier configurations because of the great improvements possible through application of known air-breathing technology. A gross weight of 700,000 lb was selected which is compatible with four 220,000-lb rockets and six SST engines for first stage operation. Payload indicated in Figure 7 below is the weight delivered to orbit by an expendable second stage, but could consist of a reusable re-entry vehicle with cargo and passengers.

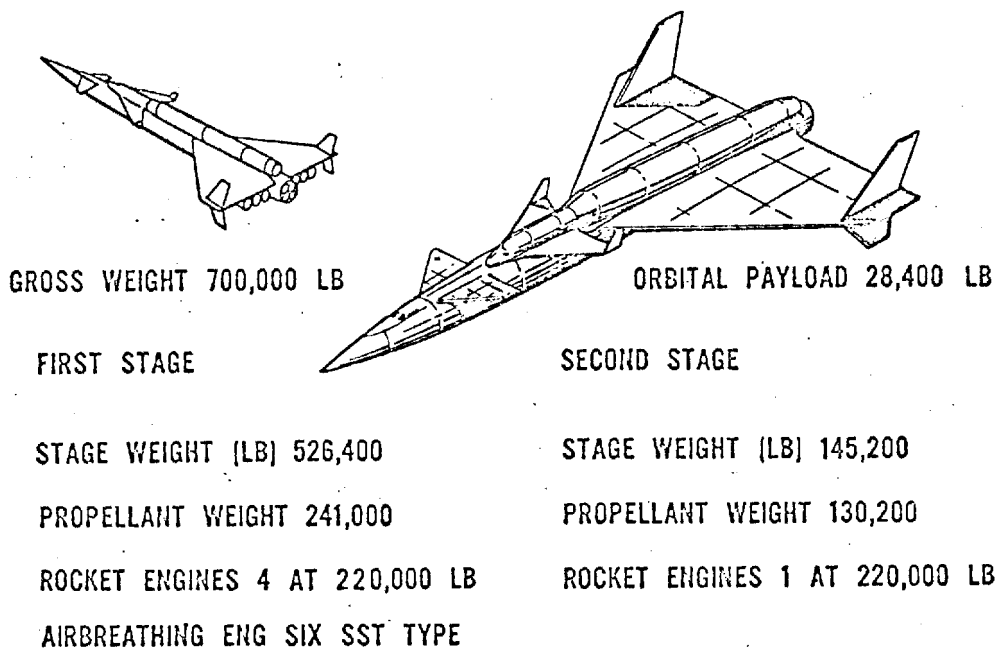


Fig. 7 Supersonic Medium Recoverable Booster Configuration

Figure 8 on the following page illustrates the gains in payload which can be achieved through application of advanced airbreathing engines, refuel, and sled launch techniques.

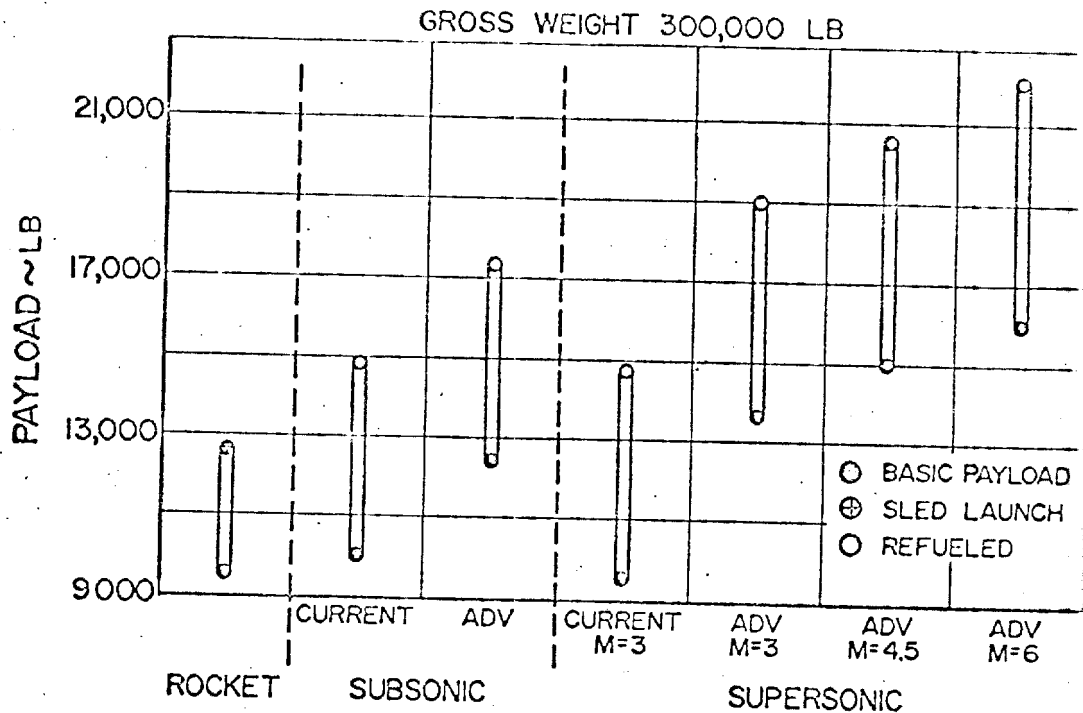


Fig. 8 Payload Growth Potential

The configurations discussed above are relatively mundane and were deliberately selected to illustrate advances in space transportation which can be achieved at low risk. Employment of refined lifting bodies, droppable tanks, and appropriately sized turboramjets and rockets could yield very significant improvements beyond those illustrated. Of major importance, however, is the conclusion that unless active programs are initiated to develop reusable elements, none of the attractive directions of technology indicated can be effectively investigated or exploited.

The primary obstacle confronting establishment of programs in this field is the conviction that the value derived from such vehicles will not equal the enormous development costs foreseen. To buttress this argument, large numbers of space operational models for the next 80 years have been prepared and discussed at length. This in spite of the fact that the key determinant of the character of these models is the cost and restrictive character of the space booster. It seems evident that such elliptical reasoning will resolve nothing.

Let us examine the potentials of the reusable booster in the broadest sense along the lines we have come to love, cost effectiveness. A wealth of analytical talent has been brought to bear on estimating the development costs involved. All of the techniques described in previous paragraphs have been extensively applied, and the conviction now exists that perhaps \$20 or \$30 billion would be required. I do not propose to quarrel with these figures because I think they have very little significance. The number could be \$40 billion, which is below what we have already expended on space, and still I think it would not significantly change the quotient of the cost effectiveness fraction.

The key criterion here surely is effectiveness. We have cast space in the role of providing reconnaissance, communication, navigation aid, and romance as its sole products into the indefinite future. On the basis of such a prognostication, there is indeed no good reason to proceed with development of economical space transportation devices. Is this all space has to offer?

With the explosion of population, erudition, and material demands which characterize our current society; vast problems are becoming apparent, stemming from unprecedented projected needs in transportation, power capacity, food supplies, and ecological contamination. The world's powerplant, in attempting to meet meteorologically rising electricity demands, is converting to nuclear power at a surprisingly high rate. Demand is doubling about every seven years, and the ability to provide energy to this schedule will tax the imaginations of humanity. Sufficient fertile and fissile ore exists to fulfill needs for centuries to come. Available techniques promise to contend successfully with contamination problems associated with spent nuclear fuels; but the rate of conversion of low quality and fertile fuels into fissile materials with breeder cycles now contemplated may prove a bottleneck, and the flexibility of application of the power produced at these large nuclear installations may substantially limit man's freedom of action. Fusion has been suggested as an alternative, and it appears favorable except for one characteristic. It now seems probable that the minimum size of a fusion plant is that of a small star. While at first blush this seems to present us with difficulty, in reality the solution it suggests may be most attractive. We have such a fusion machine just 93 million miles away, already proved highly reliable in character, which may be precisely the right place to put fusion machines. The output of this machine in the form of solar flux is approximately 3.6 gigawatts/square mile of projected earth surface. If we contemplate exploitation of this by orbiting large flimsy plane reflector arrays better than 5,000 miles above the surface of the earth, the results are interesting. Such arrays could concentrate solar flux to orbiting paraboloid reflectors focused upon laser converters which in turn could beam the coherent light, to which the flux had been converted, to any selected target. Even accepting the Carnot cycle and other losses associated with such a process, about 3000 square miles

of 1/3 mil aluminum mylar reflector could supply all the power needed by this nation well into the 21st century. If the frequency of the coherent light emitted is appropriate, we then have the possibility of impacting these beams on appropriately matched photovoltaic devices, the efficiencies of which can exceed 90%, and by these provide huge amounts of power at any point on the earth's surface with relatively small, easy to move converter stations. The perpetually annoying problems of fuel logistics, contamination, and inflexibility would disappear.

Let us look at the contamination problem as well in a new context. The conversion of the world's stationary powerplants to nuclear fission (or perhaps remote fusion) devices will eliminate most atmospheric and waterway contamination. However, one of the largest blocks of energy conversion used by man relates to transportation, and here nuclear application does not seem attractive. In spite of the recent spate of publicity concerning electrical propulsion for most transportation systems, this too suffers major handicaps. It is clear that the internal combustion engine, be it reciprocating or turbine, enjoys great advantages and that much can be done to alleviate pollution associated with its use. It is also clear that hydrocarbons are for many reasons economically attractive as fuels. It is probable that when all has been done which can be done, combustion of the best hydrocarbon - methane - will still inevitably result in the generation of tremendous amounts of carbon dioxide, but probably little or no other significant contaminant. During the last few years there has been a great deal of controversy regarding the influence of atmospheric carbon dioxide. Its relative opacity to infrared with consequent alteration of the earth's albedo certainly assures that increasing concentrations in the atmosphere would raise the surface temperature of our planet. This of course could be disastrous. Although about one-half of the carbon dioxide generated on earth today is still apparently the product of forest fires and vegetative decay, engine exhausts are surely and rapidly becoming predominant. We do not know all the details of the planetary buffering systems to control the atmospheric concentration of CO₂ but the oceans and green plants certainly contribute strongly. In spite of these, many investigators feel that carbon dioxide concentration in the atmosphere has increased significantly (by several percent) during the 20th century. If this is so, it will probably rise much more rapidly during the 21st century to become perhaps a major menace to humanity.

It is known that many species of bacteria exist capable of reducing tenuous atmospheric carbon dioxide to methane under the influence of light. We can postulate that as man's knowledge of his environment becomes more complete through matching the frequency of our orbiting solar-pumped laser to that required by such bacteria and by suspending these in sea water media saturated with atmosphere, mechanisms can be devised to control the atmospheric CO₂ providing humanity with a source of very high quality fuel (methane)

into the indefinite future. The mass of orbiting equipment required for such a job is not inconsistent with the abilities of humanity but is, of course, hopelessly beyond the capacities of any fire cracker operated transport system. Let us briefly turn our attention directly toward that other major and growing human problem, transportation itself. The airplane has emerged today as uniquely the best device for the transportation of reasonably valuable commodities over stage lengths exceeding a few hundred miles. Perhaps its greatest virtue is its long range character. Here, however, its attractiveness and utility are compromised by the unfavorable ratio of payload to gross weights stemming from the vast quantities of fuels required. A machine capable of operating supersonically or hypersonically over 5,000-mile stage lengths with payload-to-gross-weight fractions approximating one-half would manifestly be enormously profitable and of great value to mankind. Several approaches to this problem have been superficially explored employing the laser rather than on-board fuel as the source of energy for propulsion. Although the technical barriers confronting such an application are tremendous (and therefore most interesting), fundamental physical obstacles do not seem to exist. Above the tropopause, such vehicles would be powered by orbiting laser systems, while during take-off and landing, recourse could be had to small amounts of fuel or surface-based laser beams. The effect of applying such a technique to space transportation economy is obvious and great.

Although man has succeeded in deliberately modifying terrestrial weather to a very small extent, ability to cope with massive destructive atmospheric phenomena will almost certainly await the advent of advanced gigantic powerplants. It will be necessary to operate these for extended periods at tremendously high power levels to develop even a small fraction of the energy involved in destructive storms. Success in this application will also demand that such power be focused into limited areas moving rapidly along unpredictable trajectories. Alternatives to the space-based solar-pumped laser systems for this application are difficult to visualize. Mastery here could provide man with greatly augmented food supplies as well as innumerable other benefits.

We have touched briefly on postulated solutions to four of mankind's most urgent rapidly emerging problems - power, transportation, contamination, and food. I am certain that space can contribute to many others as well, including terrestrial excavation, wide range testing, high-temperature chemical processing, mining the oceans, etc., ad infinitum. Who is to gauge the "effectiveness" of such applications? If it is only through space that man can exercise control of the greenhouse effect and thus forestall his premature demise on this planet, is that worth 10¢ or \$20 billion, or should one avoid thinking about it because it is too expensive? For the most part these postulated projects differ significantly from what we have projected as uses for space by the great change in character of the time and space scales required. Although we have

no precedent for serious consideration of such vast engineering programs, it seems highly probable that we must break out of our self-imposed confinement of the imaginations and consider them. We can readily identify factors in our society which will compel this change. Our enormous unheeded growth in population, the fortuitous proliferation of technology, and the coalescence of all mankind into one society have rendered alternative approaches obsolete. Mass, energy, and social balance must be achieved internally within this society since external sinks and sources, postulated and real, have disappeared. It is highly probable that this profoundly new and different society cannot survive so long as we honor arbitrary tacit limits placed upon our technical vision. The magic of 10 years and \$20 billion as absolute limits for integrated engineering programs susceptible to serious consideration must dissolve. The nation's destiny demands that the scale of dollars and time contemplated for serious consideration now be extended to limits consistent with our resources and intellectual capabilities.

Within such a monumental but crudely defined matrix of felt needs and tentative technical solutions, specific limited development programs must, as always, be pursued along reasonably optimized plans. But it must be recognized that this process is a local suboptimization, and consciousness of recognized long-range objectives should confer flexibility to alter or eliminate development programs and approaches when appropriate.

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