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Prospects in Aeronautics Research and Development

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Transportation has become an increasingly important ingredient to societies in achieving success in a modern technological world. Many of the transportation problems appear solvable only through full exploitation of air transport. Although this mode of transportation has made tremendous strides in the last two decades, it seems to have fallen far short of what is required or of what might be expected. The question is raised as to whether aeronautical research and development has been fully effective in providing the base for a rapid and successful development of necessary air transportation. To answer this question, a gross review is made of the growth of aeronautical research and development (R&D). It is concluded that an imbalance exists between experimental and theoretical efforts, and that a great need exists to refine and extend the scientific theories of importance to aeronautics. It is concluded also that aeronautics R&D has neglected to examine the socioeconomic impact of its activities and to account for these in developing its programs.

Introduction

THERE seems little question that by now the last "doubting Thomas" has stopped arguing the importance of air transportation. This transportation mode has become a major factor in world development and it is likely that it has or will reach the point where its continuing availability is absolutely necessary. As Fig. 1 illustrates, air transportation is rapidly eating into service provided by other modes, and this can be expected to increase as larger and hungrier aircraft come into service. It is heartening to recall, as shown in Fig. 2, that not too many years ago old aircraft were being turned into highway hamburger stands, whereas today we see air transportation turning ocean liners into hotels; more recent events show that this trend will continue. Studies that have examined the potential of air transportation show how far we are from realizing full exploitation of air transportation, and that to do so will require development in years ahead far greater than anything yet realized. They show also that aeronautics has reached a new status—that of being a major factor in the future socioeconomic development of the world.

Presented as Paper 68-217 at the AIAA Aircraft Design for 1980 Operations Meeting, Washington, D.C., February 12–14, 1968; submitted March 1, 1968. *Note:* The author considered at length the question of references to related material. The usual reference list was abandoned for two reasons. First, the various aspects of the problem have been the subject of so much discussion recently that even a representative cross section of views produced an unacceptably long list. Second, the author did not wish to imply, by any particular reference or references, that any one of the views expressed had been accepted or even had majority support. Many conflicting views remain to be resolved; it is hoped that this discussion will contribute toward attracting the increased attention necessary to arrive at a proper definition for the future role of aeronautics R&D.

This will represent a major change in a field that reached its present position more by the tenacity of its supporters than through explicit recognition by society of its importance. Now that society has recognized the importance of air transportation, it is also questioning whether or not the direction and pace of development of air transportation has been guided properly. A recent study made for the U.S. Senate described aeronautics as being overly concerned with the narrow aspects of its own survival as a separate element of society, and insufficiently concerned with its present major role in socioeconomic development. The study emphasized that aeronautics R&D required a carefully organized group of objectives to achieve the essential capabilities in air transportation in the shortest possible time. Although perhaps broadest in scope, this study is just one of many which have arrived at generally similar conclusions. Invariably at some point in these studies R&D comes under scrutiny and the question is raised as to whether it is measuring up to the challenge confronting it. The purpose here is to examine aeronautics R&D and see if the criticism is merited, and if so, what could be done about it. In attempting to do this, it was useful to trace briefly the growth of aeronautical science to its present stage. With this in mind, current weaknesses and omissions became more apparent and, hence, requirements and scope for the future more obvious.

Growth of Aeronautics R&D

Although starting as a largely experimental activity, aeronautics quickly developed a good theoretical base. Prandtl, Glauert, Lanchester, Bairstow, Joukowsky, and others laid the theoretical groundwork in the early 1900's which led to orderly experimental verification of theory and subsequent continued refinement of aircraft. As shown in Fig. 3, Betz

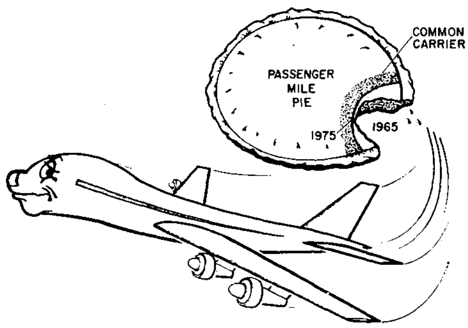


Fig. 1 Intercity transportation.

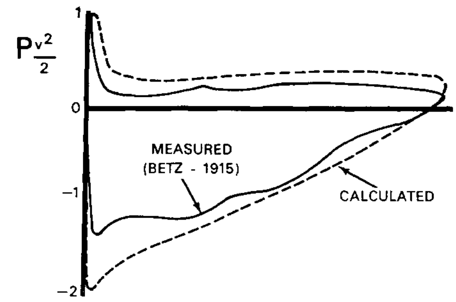


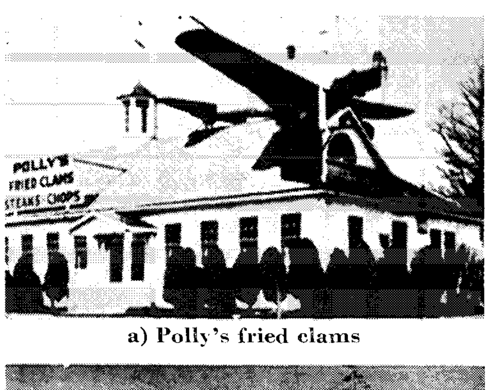
Fig. 3 Joukowsky airfoil pressure distribution.

was conducting carefully controlled wind-tunnel experiments in 1915 to verify Joukowsky's theoretical predictions of chordwise pressure distribution. These theories were expanded and refined in the 1920's and 1930's by such people as Theodorsen, Allen, and Jones, for example, to account for the subtler aspects of the fundamental theories laid down earlier by others. Experimentation was continued at an expanding rate to provide information on the variation of important parameters involving higher-order effects.

By 1940, aeronautics as a science was considered by many to be rather completely exploited. The aeronautical engineer lived in a comfortable world where science had set ultimate limits which appeared very close at hand. He looked forward to a continued refinement of existing designs through experimental tailoring of details.

As Fig. 4 shows, the generally accepted operating range for aircraft in the 1930's was relatively confined. Theory had thrown up the sonic barrier as a limit to maximum speed. Experiment had driven performance closer to this limit but speed increases were coming in small increments. The single major departure from continued evolutionary development of conventional aircraft was found among the helicopter enthusiasts.

The coming of the second World War, with its demand for many types of specialized aircraft, provided the impetus to expand experimental development in order to wring the last shred of performance out of each design. As Fig. 5 shows, one consequence of this demand for experimental capability for development was a very rapid build-up in national experimental facilities.



a) Polly's fried clams

b) Queen Mary is sold

Fig. 2 Reversal in attitudes towards aircraft.

However, during this same period, a new era of aeronautics began. Several imaginative individuals were about to blow this comfortable world apart, like John Stack who didn't believe that the sonic barrier of theory really existed, and Frank Whittle who didn't believe the automobile engine should power an aircraft. Within a few years after the war, the aeronautics horizon had enlarged enormously as shown in Fig. 6. The X series of aircraft had shown the sonic barrier to be only a penetratable annoyance. Turbojet engines gave increasing thrust with speed, enabled much greater excess thrust at low speed, and offered high reliability. The aeronautical engineers' world was no longer constrained, but extended from zero speed up to some undefined limit. Analysis of the potential of atmospheric flight had defined possible flight corridors, as illustrated in Fig. 7, showing the relation of speed and altitude as bounded by too little lift, too high dynamic pressure, and unacceptable aerodynamic heating. Although sufficient experimental evidence existed to prove that these flight capabilities did indeed exist, no adequate theories existed to guide research and development toward practical and useful vehicles. As Fig. 8 shows, in an effort to obtain earliest possible realization of these new flight capabilities, a second vast expansion in experimental facilities occurred. In stark contrast to the wartime expansion, which was to enable further refinement of existing aircraft, these new facilities enabled study of whole new flight capabilities. Although a number of aircraft were built on this largely experimental base, for example, the early X series of aircraft shown on Fig. 9, the lack of an adequate theoretical understanding left a number of them with deficiencies which prevented full realization of their potential performance.

In part, the lack of an adequate theoretical base can be traced to a limitation in available analytic capabilities. Although the equations presumably describing any physical process could be written, their solution in many cases was so tedious that only infrequently could an experiment be designed to test the validity. Even less often did the more advanced theoretical analysis find its way into applications. A close coupling between theory and experiment is necessary, of course, if either is to advance on a solid basis.

The engineers, however, required solutions to real world problems. To supply these, aeronautical R&D activities were asked to produce vast amounts of experimental data.

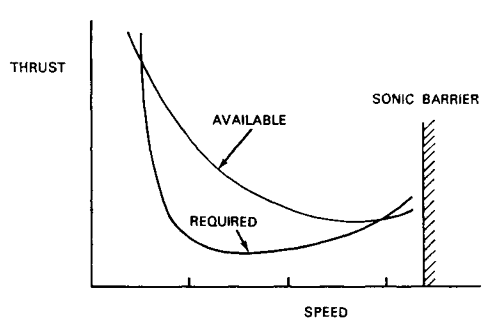


Fig. 4 Aircraft operating limits, pre 1940.

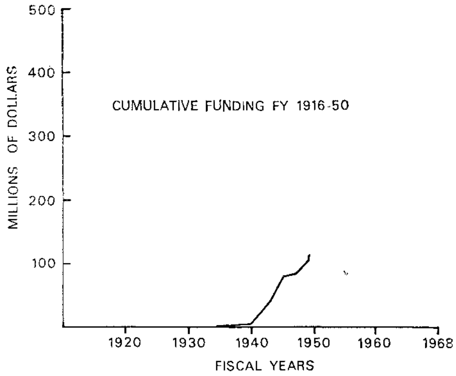


Fig. 5 NASA (NACA) aeronautical research facilities.

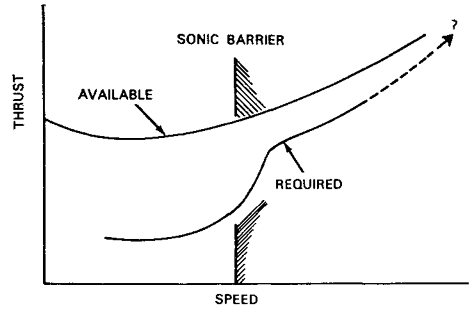


Fig. 6 Aircraft operating limits, post 1950.

From some of this it was possible to correct older theories. Other parts were used to establish semiempirical limits. Figure 10 shows a well-known example of this type. From a large number of experiments on swept wings, it was possible to define the relation between wing sweep and wing aspect ratio which separated those wings exhibiting pitch down or stability at high lift, from those showing pitch up or instability, but there were many cases for which no adequate theory existed and aeronautics R&D was asked to produce sufficient data so that the engineer could find within it the answer to his problem. One only needs to recall the vast amounts of data collected on various wing and airfoil combinations, to form a picture of the situation.

A serious consequence of this demand for experimental data was a breakdown in good communication between theoretical and experimental research. Often the theoretician did not recognize the full implication of his analysis. Often the experimentalist solved problems by trial and error when an answer existed in theory, had he been aware of it. The disastrous roll-yaw coupling problem experienced in flight was foretold theoretically but not recognized by the designer. A well-known case is that of the area rule. Hayes' theories had predicted this effect in 1946, but its significance was not recognized by the designer because no special attention was drawn to this important aspect of a rather complex theory. Whitcomb demonstrated the area rule experimentally, but was led to this point by physical reasoning, not by Hayes' analyses. Connection between the two was not made clear for several years.

All the fault did not lie with the experimentalist. In removing the limitations in theory, which resulted from oversimplification to allow easy use, the theorist presented an increasingly complex picture to the designer. The latter tended to be frightened away simply through computational difficulty and turned toward "back-of-the-envelope" empiricism. Sporadic attempts were made to bridge this gap. Falkner, Weissinger, and DeYoung's work on span loading, for example, were efforts to reduce awkward theories to a form amenable to everyday use.

Relief from these computational difficulties appeared with the electronic computer whose use, as shown on Fig. 11, expanded rapidly in postwar years. The computer greatly extended the capabilities of the theoretician and provided the designer with the tool necessary to apply the most complex theories to the solution of his problem. However, before aeronautics could begin to exploit this new potential, space flight appeared. In the absence of any real experience in space, it was necessary to turn to theoretical analysis to solve almost every problem. The complexity of these problems left no alternative but to use the computer as a primary tool. The direct relation between aeronautics and space flight sciences in many areas served to take from aeronautics a very large proportion of its strength in theoretical analyses and devote it to space. From the standpoint of achievements in space flight, these events must be considered an outstanding success. However, in aeronautics, the effect was to slow the development of technology. Full advantage was not taken by aeronautics of this explosion in analytic capability. The theorists who remained in the field were few and not well coupled with the experimentalist. Only infrequently were graduates trained in these new analytic capabilities attracted into aeronautics. Consequently, the experimentalist remained to dominate the field.

At the same time, aeronautics had arrived at a point where these analytic capabilities were sorely needed. A bewildering range of possibilities was available in flight vehicles. The turbine engine with its high thrust-weight ratio made possible

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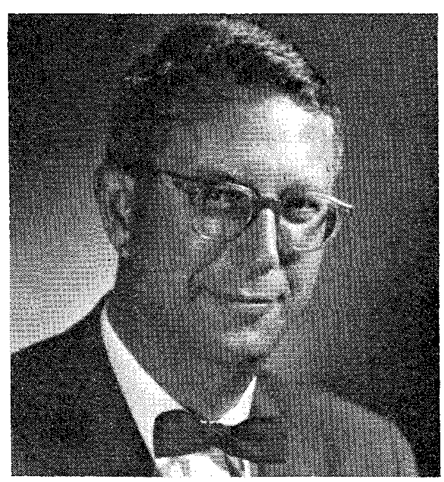
Charles W. Harper



Charles W. Harper was appointed to the position of Deputy Associate Administrator (Aeronautics) in the Office of Advanced Research and Technology on May 3, 1967, when the office was created. In this position he is responsible for the coordination and management of over-all NASA research and technology work related to aeronautics. Cooperation and coordination between NASA's Office of Research and Technology divisions and industry, and other government agencies, are among his paramount responsibilities.

Prior to his current assignment, he had served since October 1, 1964, as director of the Aeronautical Vehicles Division, also at NASA Headquarters, Washington, D.C. From 1959 to October 1964, he served as Chief of the Full-Scale and Systems Research Division at NASA Ames Research Center, which specialized in such aerodynamic areas as wind-tunnel and flying research, directed at achieving low flight speed; the development and use of ground-based simulation as an aerospace research technique; guidance, navigation, and control system studies for manned and unmanned aerospace vehicles; and the physical research associated with such systems.

Mr. Harper was born in Winnipeg, Canada, September 24, 1913, and became a United States citizen in 1941, the same year he was graduated from the University of California at Berkeley. He is a member of Tau Beta Pi and Sigma Xi, an Associate Fellow of AIAA, and the author of numerous technical papers.

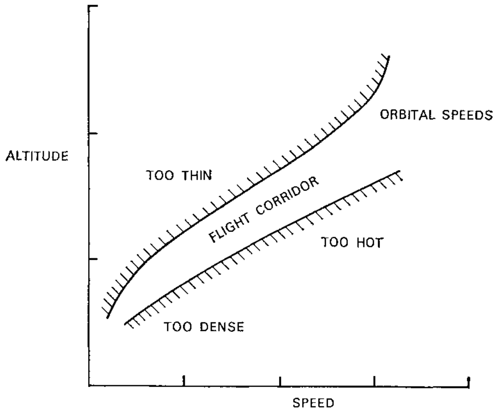


Fig. 7 Atmospheric flight limits.

a multitude of V/STOL types. The supersonic transport (SST) seemed possible in various forms. The hydrogen-fueled hypersonic aircraft showed potential. Because these possibilities could not be analyzed adequately to identify the critical problems, the only approach was to examine as many as possible through experiment, in wind tunnel and in flight. Under these circumstances, it should be no surprise that some of the effort was wasted on unsound ideas, some critical problems were overlooked, and some effort wasted on trivia. Consequently, progress towards achieving these new capabilities has been slow.

Those advanced concepts that did reach flight status showed that a problem, once minor in nature, had reached major proportions. In the past, it had been possible to assume that a vehicle assembled from satisfactory elements would integrate to form a satisfactory whole. Experience with these new vehicles of the interaction between external aerodynamics and propulsion, between structural deformation and external aerodynamics, showed that a combination of satisfactory elements no longer assured a satisfactory whole. The pilot-airplane integration problem became increasingly difficult; as Fig. 12 indicates, a design that appeared clearly defined and satisfactory to the engineer often became less defined and unsatisfactory when the characteristics of the pilot were introduced into the system. Successful integration of aircraft elements presented a new and major challenge for aeronautics R&D; only limited guidance from theory existed, and experimental research entailed high cost and high technical risk, since a complete system was involved. This dilemma had a major inhibiting effect on the pace of air transport development.

While aeronautics R&D was struggling with the technical problems of advanced aircraft, a tremendous and unpredicted growth of air transport took place along evolutionary lines. Instead of the light plane in every garage and the V/STOL

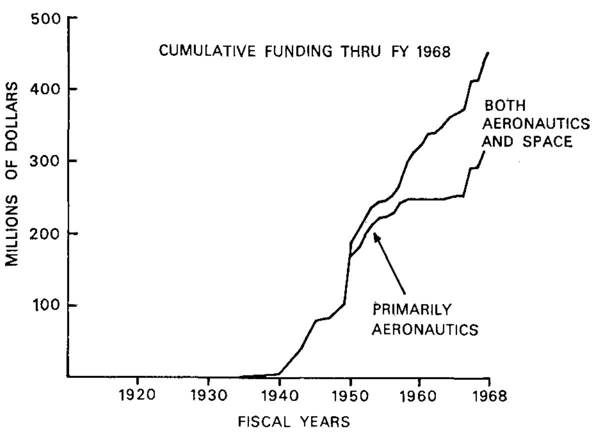


Fig. 8 NASA (NACA) aeronautical facilities.

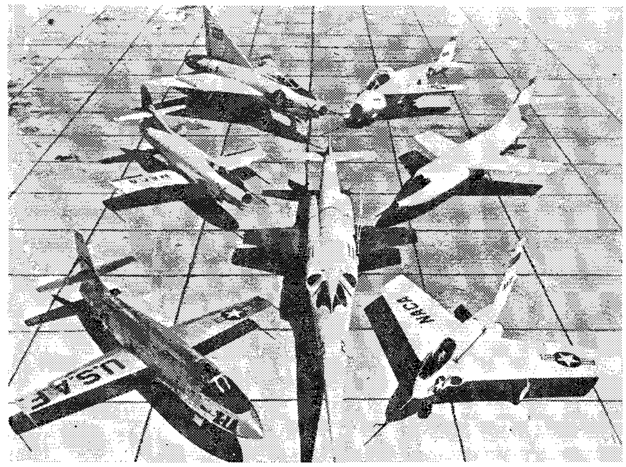


Fig. 9 Early series of X airplanes.

transport hopping around the megalopolises, increasing numbers of conventional jet transports were waiting to take off or land at every major airport. Criticism was directed at aeronautics R&D for having failed to recognize the full socioeconomic impact of air transportation, and to take account of this in defining R&D goals. It was argued that air transport goals were simply higher, faster, and further without any special consideration of its interaction with many socioeconomic factors and, in turn, their effect on R&D requirements. Even such an obvious problem as that of aircraft noise was given scant attention by aeronautics R&D before it became an unacceptable nuisance. Certainly, it is clear that air transportation is having many major socioeconomic effects. Airports, initially placed far from the cities, have grown enormously and have become the hub of business activities. The growth of the Los Angeles International Airport from 1950 to 1967, shown in Fig. 13, is representative of that of many major airports throughout the world. Air transportation has had a major impact on the automobile rental business. Industrial, scientific, and professional conventions are now a continuous and major source of income for larger cities and are totally dependent on rapid air travel. Because of air travel, tourism has become the world's largest international business, as well as adding a new word to every language. International industrial investments have flourished because of air travel. International politics have undergone a major change because of air travel. It is clear that air freight is changing inventory requirements in a major way. Whether any of these or other similar factors would have a significant effect on aeronautics R&D was not carefully examined. Obviously such an impact should have been examined if the maximum benefit was to be expected from research and development expenditures.

From this review of aeronautics R&D growth, it is possible to identify several conditions that should be the object of

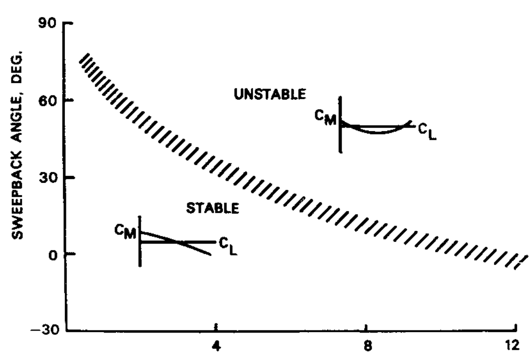


Fig. 10 Effects of wing sweep and aspect ratio on longitudinal stability.

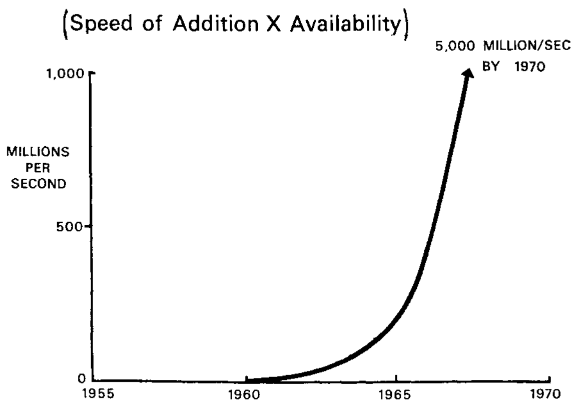


Fig. 11 United States computing power.

corrective action in the future. 1) The growth of the theoretical base for aeronautics R&D has fallen short of that required; new capabilities in theoretical analysis have not been exploited fully. 2) Because it lacked a strong theoretical base, aeronautics R&D has become overly involved in data acquisition and problem solving for specific designs. 3) Sufficient recognition has not been given the fact that air transportation is having a major socioeconomic impact, and consequently the latter should have a major influence on aeronautics R&D. If the preceding criticisms are valid and the object is to remove them, it becomes possible to speculate on the future of aeronautics R&D.

Basic Goal for R&D

In making such speculations, it is necessary to establish a long-range goal for aeronautics R&D as a basis for making decisions regarding the elements of current R&D programs. As indicated on Fig. 14, R&D, including aeronautics, should have as a goal that of providing all information required for a designer to go from paper designs to production with complete confidence of success. No experimental development would be required. Obviously this is a distant goal. In losing sight of it, R&D often becomes a series of short-term exercises in specific problem solving. When this occurs, problems similar in their fundamental nature but differing in detail are solved over and over. Experimental opportunities are lost which could provide a basic understanding leading to solutions for a whole class of problems and important extensions of theory. If the aforesaid goal is taken as an objective, it is clear that a great deal of ignorance exists in the aeronautical sciences, and it will be many years before the aeronautical research engineer works himself out of a job.

Some of the areas of ignorance in aeronautics which are subjects for R&D were described in Schairer's Wright Brothers Lecture of 1964. In concentrating just on areas of potential aircraft performance increase, some 38 separate opportunities were identified. Very few of these opportunities have been realized in practice in the four years since that lecture. It would seem that the R&D goal of providing the designer with information required to convert concept to practice with confidence is not being approached rapidly.

The very number of promising ideas available for study presents a dilemma to aeronautics R&D. Obviously it is not possible to pursue all of them with a major effort. Some rational basis for making a choice must be established which is more valid then the interest or intuition of the individual research man.

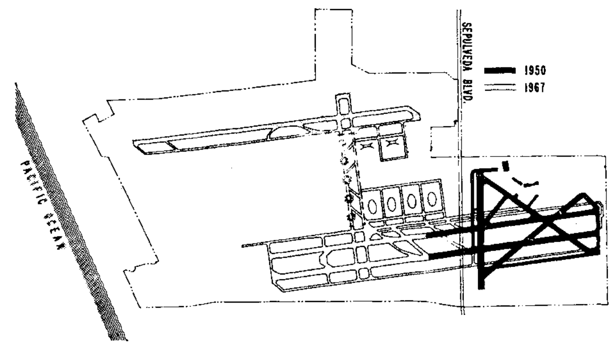


Fig. 13 Los Angeles International Airport.

Requirements for Transportation Systems Analysis

The obvious basis for making a choice is a measure of the effect any given technological advance will have in satisfying requirements for future air transportation. A starting point to define an aeronautics R&D future, then, would seem to be an examination of the role air transportation could play in the potential socioeconomic development of the world.

Clearly there is no current lack of transportation system studies; they are appearing in every form of publication. From the standpoint of aeronautics R&D, however, these have not been particularly useful. Many are limited to an examination of wider application of state-of-the-art technology. Many assume solutions to technical problems whose achievement is not at all evident. Only a few represent the combined efforts of technical and transportation experts to bring together possible advances in technology with key transportation requirements. Rather, in air transportation the pattern of progress has been more one of expediency. A new technological capability surfaces from R&D and a search is made for an application, or alternatively, an existing operation highlights a problem, usually one that could have been foretold, such as aircraft noise annoyance, and a frantic R&D effort is generated to solve the problem. Efforts to establish future air transport requirements are seen in the formation of the Department of Transportation and in the many recent studies by states, cities, and economically interdependent areas. Because of the many nontechnical factors involved, aeronautical R&D groups alone cannot make such studies but their participation is invaluable in assuring that the conclusions are based on sound physical reasoning and not science fiction fantasy. Without these transportation system studies, it is impossible to be definitive in choosing fruitful R&D programs; but even qualitative analysis brings to light some interesting possibilities. Some examples will be considered next.

Megalopolis

It seems fair to assume that one major factor in the development of megalopolis, was that heads of industry, and/or government required frequent face-to-face contact and they

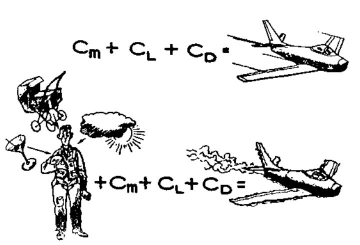


Fig. 12 Pilot-airplane integration problem.

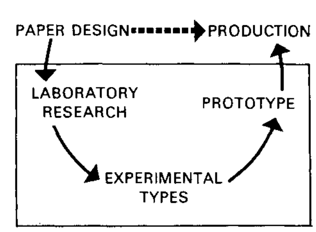


Fig. 14 Eliminate through R&D.

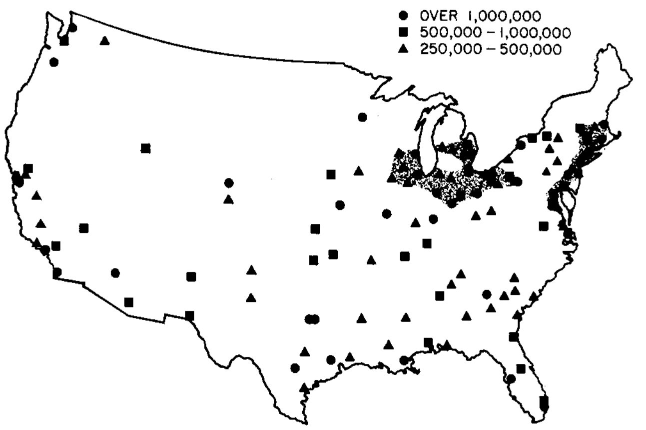


Fig. 15 Population centers, 1980.

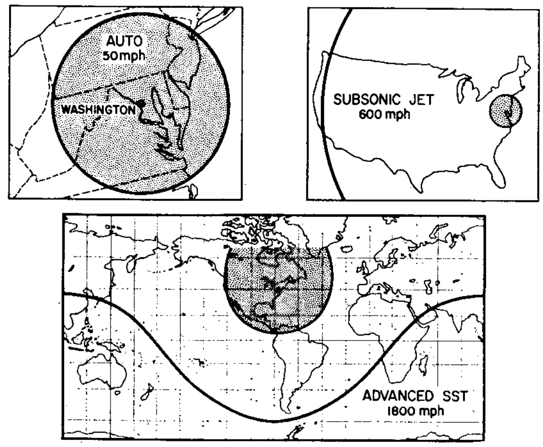


Fig. 17 All in a day's work.

therefore settled along existing modes of transportation. Quite naturally, the labor force that required management settled with it; population concentration was an inevitable result. Saturation of the original land or water transport systems has been a major factor in promoting air transport. This gave good communication between distant cities and probably aided growth of megalopolises around these cities. By 1980, population growth will create new centers, as indicated on Fig. 15, which can be expected to coalesce into new megalopolises using long-range air transport but hampered by local transport problems.

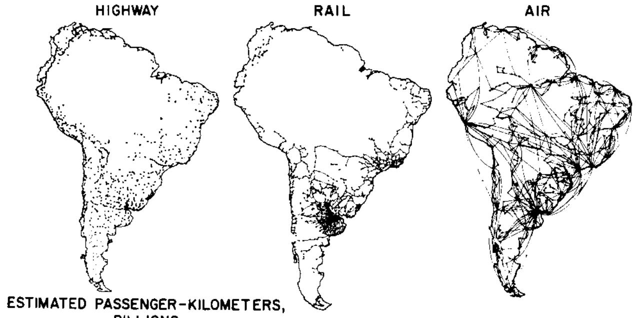
It has been suggested that a fine network of air transport could be laid over megalopolis to solve its transportation problems. It is not difficult to generate counterarguments showing that, whereas some specific transportation problems can be solved this way, the major problems of megalopolises will not be affected. One solution could be to dissolve megalopolises by spreading out the population. This could be done by enabling the management to separate in distance, but not in time, and take their support with them. Here a local air transport system, properly developed, could have a profound and beneficial effect on future development, but the kind of analysis that will define in detail the technological advances required to produce the necessary vehicles does not yet exist.

Underdeveloped Areas

Proper economic development of underdeveloped areas in the world is certainly fundamental to the continued well-being and development of the remainder. History shows that adequate transportation is a requirement for development; in fact, a lack of existing water transport or difficult-to-develop land transport can almost always be associated with lack of economic development. Air transportation could put all areas of the world on an equal transportation basis if proper vehicles existed. In some areas of the world, for example in South America, as shown on Fig. 16, where modern ground transport is only partially developed, air transport already is providing major communication links, and if properly exploited, could circumvent permanently the need for widespread, expensive, inflexible ground systems. It would appear from the figure that the opportunity exists to achieve just such a circumstance in South America. While not many years ago, it was absurd to think of air freight as anything but maximum value items such as passengers or mail; this is no longer true. Scarcity of raw or semiprocessed materials has raised their value to the point where lost time in transit becomes important, and air transport makes sense. Certainly, this point has been reached in many processed goods where a large inventory in a slow delivery system pipeline is unacceptable. Again, the analysis does not exist which would define clearly what these vehicles should be.

High-Speed Transport

Although the advantages of speed have been a prime selling point for air transportation, it is not apparent that the full implication has been realized as yet. High speed makes possible frequent interchanges over long distances, but the value of speed does not increase progressively with distance. Because man lives by a metabolic cycle, it is important to avoid overnight stops in widely separated time zones. Suppose a 6-hr work period is required and a 14-hr day is acceptable where travel is involved; then the traveler has 8 hr for a round trip. As Fig. 17 shows, at 50 mph, the traveler is confined to a 200-mile radius; at 600 mph, his potentially useful daily work range has reached 2400 miles; at speeds of 1800 mph, he can reach 7200 miles. With such high-speed air transport capa-



ESTIMATED PASSENGER-KILOMETERS, BILLIONS

YEAR	HIGHWAY	RAIL	AIR
1950	5.5	26	1.7
1967	52.5	32	9.5
1980	238	30	40

Fig. 16 Transportation networks South America, 1967.

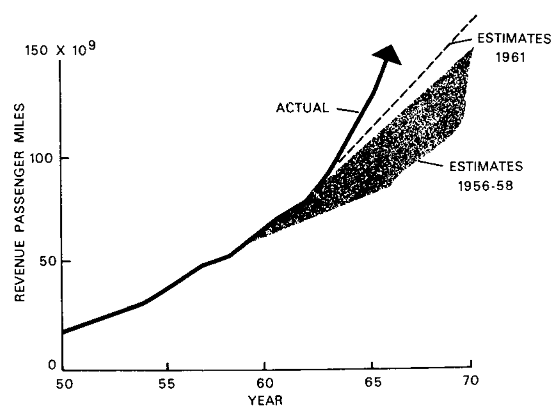


Fig. 18 Commercial air traffic free world travel.

bilities, ground transit times become of critical importance. It is easy to see why our airports are becoming centers for management meetings. The ground time problem becomes more critical, of course, as useful working ranges increase, since a requirement for overnight stay causes serious disruption of the traveler's metabolic cycle. Clearly, studies of the extent to which very high-speed transport will develop 1-day long distance trips in the manner the present subsonic jets have done for short distances, would provide aeronautics R&D with strong guidance for some elements of its technical program.

These examples serve to indicate only the kind of questions to be answered if aeronautics R&D is to apply its efforts most profitably. Before the questions can be answered properly, thorough transportation systems analysis must be made. These must include consideration of the ultimate use of the system as a transportation element, and hence, of all of the socioeconomic factors of travel habits and requirements as projected for the future. As anyone knows who has tried to collect such information for use in projecting short-haul aircraft requirements, very little information exists. This is emphasized in Fig. 18, which compares earlier predictions for growth in air travel by jet transport with the growth that has been realized; not enough is known about the effect of new travel capabilities on travel habits to predict the future safely.

These systems analyses, to be valid, depend heavily on what can be done in a technical sense to meet travel requirements, and it is in this regard that the R&D man must work closely with the socioeconomists. At the same time, these studies will highlight for the research man the most profitable lines of endeavor. They prevent R&D from overlooking a technological requirement that must be satisfied to make a new capability of value.

The frustrating failure to move V/STOL transport off dead center is a good illustration of this. First efforts to develop such vehicles pursued vigorously the VTOL performance capability in a multitude of ways. Despite the fact that some 20-odd of these were built and flown, it was only recently that the critical importance of also having a rapid and easily controllable descent capability was recognized in a quantitative sense. Results of one systems analysis show the difference in direct operating cost (DOC) resulting from increased cruise speed and reduced descent time, that is, from cruise to unloading. On Fig. 19 is shown the reduction in direct operating cost resulting from either a decrease in descent time or an increase in cruise speed for a particular V/STOL design. At ranges of 100 miles it is evident that DOC is more sensitive to reductions in terminal maneuvering time than to increases in cruise speed. It is clear that research on requirements for short descent times should have had as much attention as that on performance. Only recently have the dynamic flight studies been undertaken which will define those aircraft characteristics having major control over flight descent times. With all these analyses finally being available to guide R&D, the research programs can be directed with maximum

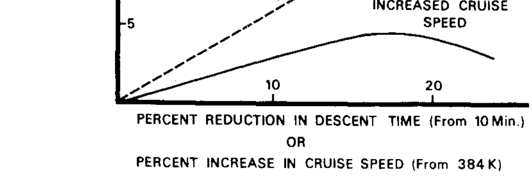


Fig. 19 Effect of cruise speed and descent time on short haul air transport DOC.

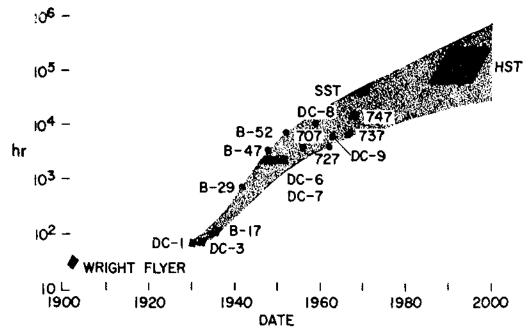


Fig. 20 Wind-tunnel hours.

effectiveness. A very close interplay and interaction between economists, social scientists, and physical scientists will be necessary to define the R&D program required for each to produce the data that will make these analyses valid.

Requirements for Development of Aeronautical Theories

Critical as these transportation systems analyses are to the orderly progress of R&D, even in their most sophisticated form they are meaningless unless based on scientific facts. However, given well-established technical theories for the physical processes involved, the systems analyses can be kept confined to the real world and still explore beyond the limits of experimentally demonstrated capability.

The value of developing broadly applicable theories for the scientific disciplines in aeronautics is becoming of critical importance to the aeronautical experimentalist as well. In the past, where adequate theoretical treatment of a problem was lacking, it was possible to provide ground test facilities or flight research programs which enabled an ad hoc solution to be found. It appears unlikely that this principle can be followed to the same degree in the future; Fig. 20 shows a comparison of the wind-tunnel test time devoted to development of an early aircraft and a modern one, which clearly illustrates the point. Further, the wide scope of flight possibilities of interest would require a very great increase in the number of ground or flight facilities. Considering the nature of the problem, for example, airframe-engine integration for V/STOL or for hypersonic aircraft, the cost of facilities for ad hoc experimentation can be anticipated to be very high. Therefore, to the maximum extent possible, experimentation must be viewed as a means to verify and extend the necessary theories; as will be noted later, even the best progress to be expected leaves a very great challenge for the experimentalists. The lack of adequate theoretical treatment can be found in any of the scientific disciplines underlying aeronautics technology, aerodynamics, materials, structures, flight environment, flight dynamics, etc. A closer look at aerodynamics and flight dynamics is taken here simply to illustrate the nature of the requirements future R&D must satisfy.

One aspect of existing aerodynamic theories which should present a challenge to the scientist is the very large number of theories required to describe basically the same process, namely, fluid mechanics. Theories exist for flow around two-dimensional airfoils, three-dimensional wings, bodies of revolution, propellers, rotors, and so on, ad infinitum. Many of the theories involve empirical constants that simply lump together areas of ignorance. Some "theories" should be considered little more than curve fitting of experimental data glorified through the inclusion of obvious aerodynamic parameters such as Mach numbers or dynamic pressure. To study a range of aircraft geometries and operating conditions, then, the aerodynamicists must use a succession of theories properly adjusted by a large number of empirical "constants." For example, as shown on Fig. 21, if it is desired to calculate the

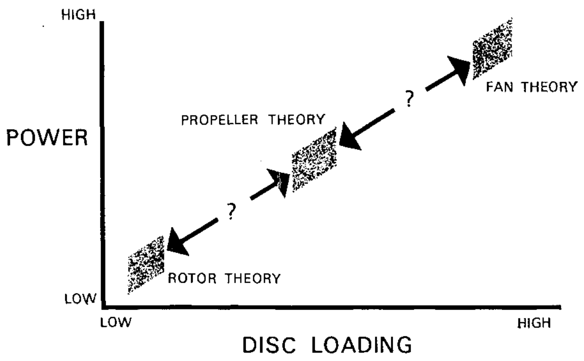


Fig. 21 Power for equal thrust.

power required to create a fixed level of thrust, the theory used depends on the disk loading, that is, whether a rotor, propeller, or high disk loading fan is involved; no single theory exists for the whole range which gives equivalent accuracy. As noted earlier, this situation was excusable when applied analysis was limited by hand computation, but with modern computational capabilities, progress can be made towards achieving unified theories. Some indication of the potential of the modern computer to extend analytic capabilities in aeronautics has already been demonstrated successfully. For example, as shown on Fig. 22, just recently the computer has enabled analysis of nonequilibrium flow conditions, such as temperature and density variation, behind a normal shock wave. Prior to this, calculations were limited to regions just behind the shock wave and at the body surface. Other examples are found in supersonic aircraft aerodynamics, in hypersonic boundary-layer theory, in airfoil design, and even in flow analysis involving the Karman vortex street; however, much remains to be done in both extending the application and bringing it into widespread use.

For many desired advances in aeronautics, however, no proper theory exists. In only some of the cases discussed by Schairer in 1964 was it possible to provide a theoretical target performance and compare this with practical experience. In other cases, it was possible only to present experimental data and speculate on the possibility of attaining improved performance. His discussion of maximum lift and the effect of wing sweep on this are good examples. In the first instance, he points out we do not yet know the upper limits we should be striving to reach; this is despite the fact that probably more ad hoc research has been devoted to maximum lift than to any other single aerodynamic parameter. In the second instance, he points out that $C_{L_{\max}}$ of swept wings has already exceeded predictions made by existing theories and again it is not known what a realistic upper limit should be.

It is even easier to identify the need for advances in theoretical flight dynamics than in theoretical aerodynamics. The term flight dynamics is used here to denote control of an aircraft about its desired flight path. Thus, it is concerned largely with short period motions of the aircraft. These could be imposed by the pilot to change the desired flight path or result from the pilot holding the aircraft on course against external disturbances. It is evident that these two conditions create conflicting requirements. To change course, the aircraft should respond quickly and positively to the pilot-imposed disturbances. On the other hand, to prevent constant pilot attention, the aircraft should resist course changes resulting from external disturbances. This becomes particularly critical in the landing maneuver, where the aircraft should not respond to low-altitude gusts, yet must be capable of rapid maneuvers required for traffic control, collision avoidance, or instrument landing system (ILS) misplacement, for example. The impossibility of achieving all the desired characteristics inherently in a single design is demonstrated by the steady progression toward automatic flight control. Figure 23 shows some of the more important steps already taken away from manual control towards automatic control; it can be expected that fully automatic control will be available to the pilot in the future, and he will be able to choose any degree of it that makes his over-all task easiest.

As implied in the last statement, these pilot aids have developed in the past largely to increase safety by easing the pilot's tasks. However, experience with some recent designs implies that pilot aids will be required not just to make the flight task easier, but to make it possible; this is a consequence of a direct conflict between those design features giving maximum performance and those giving ease of flight. It is not difficult to predict that proper pilot aids will become as vital to safe, efficient operation of many future aircraft as is the primary structure. This situation is creating new problems for the designer and hence for aeronautics R&D. As the pilot aids become increasingly important, they must become a fundamental part of the design and not just an added on feature; thus, dynamic analyses of the total system must be made, including the pilot, who will represent just one of the controlling elements, but who interacts with the others. Further, whether justified or not, past experience has led to a reluctance to relieve the pilot any more than necessary from the control functions; thus, the designer needs to know the limits of capability of the pilot in performing various tasks, singly and in combination, which is indeed a relatively unexplored area of aeronautics R&D.

Even without considering the human element, the theoretical study of flight dynamics is exceedingly complex because of the many degrees of freedom involved in an aircraft system, and the many interactions between them. The experimentalist has had only the grossest guidance from theory to aid in planning definitive experiments. Fortunately, the mathematical tools required to undertake these analyses are being developed in response to analytic requirements of spacecraft design. Techniques for stability and control analysis developed particularly for the unmanned spacecraft would seem to have much promise for analyzing the dynamics of aircraft flight. Such analyses would highlight very quickly the critical

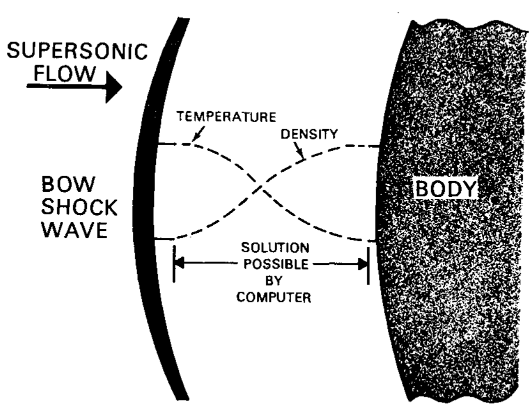


Fig. 22 Flow behind a shock wave.

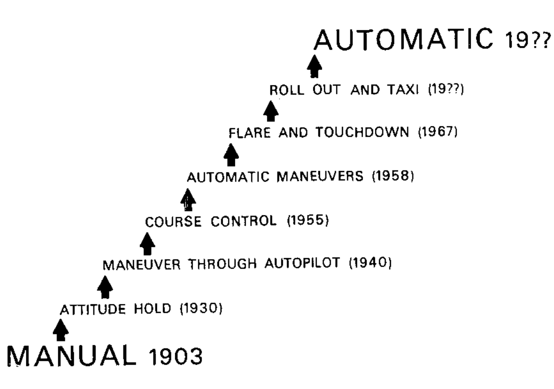


Fig. 23 Steps toward automatic flight.

factors in aircraft flight dynamics and provide the experimentalist the focus necessary to carry out definitive experiments.

Analysis of the aircraft system with the inclusion of the pilot as one servomechanism element has been successful to date in only a very limited degree. It is quite evident that although the analytic principles are sound, the aeronautical engineer does not have available all the information necessary to define, quantitatively, the human as a servomechanism. Most of the information that does exist is related to the capability of the highly trained pilot to operate under high physical stress; Fig. 24, for example, shows the degradation in a pilot's ability to perform a precise tracking task as he operates under increasingly high normal acceleration. Information of this type has proved extremely useful in guiding the design of military and research aircraft and space vehicles. However, the aeronautical engineer must also design vehicles for a different class of operators. These will include the nonprofessional pilot whose level of training may be no more than that of the average automobile operator; they will include the professional transport pilot who does not have the opportunity to fly critical maneuvers constantly for practice as does the military pilot, and whose operating stresses may likely be more psychological than physical; they will include pilots whose tasks involve repeated takeoff and landing only a few minutes apart; they will include pilots whose tasks involve long-range, high-speed flight where major stresses arise from complete distortion of the circadian rhythm. The task of defining the limit of capabilities of this wide variety of humans and conditions is obviously a formidable one. It will require the closest cooperation between the subject, psychologists, physiologists, and the aeronautics R&D man.

The Role of Experiment

Up to this point, attention of the future of aeronautics R&D has been focused on the need to begin transportation systems analyses in depth to provide aeronautics R&D with proper objectives, and to revitalize and extend the theoretical analysis of the basic scientific disciplines important to aeronautics. Experimental aeronautics R&D has been viewed in a somewhat critical sense as having become too involved in the gathering of data, and in the solution of specific development problems.

It has been suggested that one primary goal for experimental R&D would be that of supporting development of theoretical analyses. This does not mean a less important role for the experimentalist. As was noted earlier, the rapidly growing problem of integrating satisfactory elements of an aircraft into a satisfactory whole should provide ample challenge for the experimentalist.

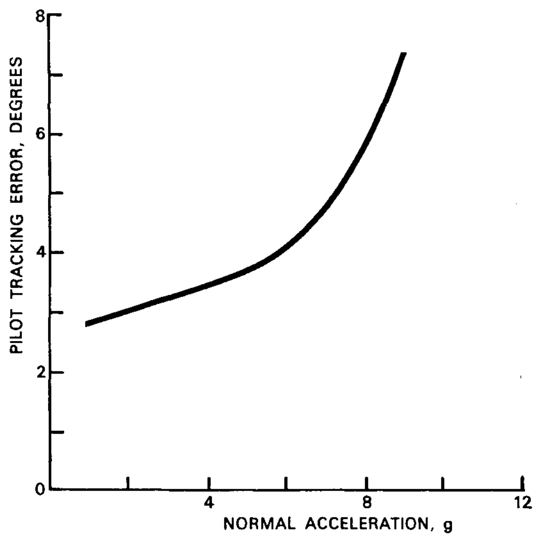


Fig. 24 Acceleration reduces pilot performance.

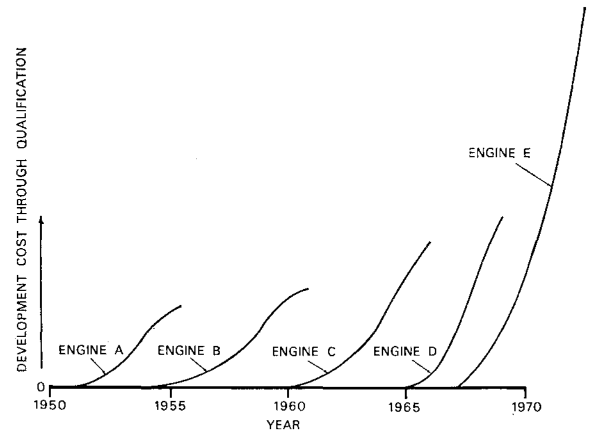


Fig. 25 Engine development cost vs development time.

As aircraft become more sophisticated, the interaction between the various elements increased and the identification of a successful design more difficult. The engine-airframe integration problem of supersonic aircraft is a current problem of this type. Through the development of the subsonic jets, it was possible for the buyer to make independent choices of airframe and engine without serious concern over the effects of interaction between the two on performance. Thus, we see similar models of Boeing or Douglas transport aircraft powered with different engines. In the case of the SST, however, the airframe designer found it expedient to design a slightly different aircraft for each of the competing engine designs, in part because of significant aerodynamic interactions. Another example is that of aerodynamic heating and consequent structural deformation. Until recently, the structures man was given a shape and some loads and his job was to find the lightest structure to hold that shape under the given loads. For speeds above Mach 3, structures must be designed to assume the desired aerodynamic shape when distorted due to heating; the heating, in turn, depends on the shape assumed. Thus, a strong interaction exists between structures and aerodynamics.

The challenge referred to arises from the very great difficulty of obtaining definitive knowledge about the important subsystem interactions without building a complete system. Development of any new turbojet engine presents a classic example of the inability to meet this challenge. Almost any significant design departure from an existing engine results in an R&D program leading to prototype construction and operation before confidence is had that the new system will operate; the cost and time development history for several turbojet engines is shown in Fig. 25. The lesson here is clear; not enough is being learned about successful integration of engine elements from past experience to outweigh the greater costs of increased sophistication in the engines. The cost of one of these programs reaches the tens of millions of dollars. The implications are obvious. New aircraft designs must be compromised to utilize existing engines until the cost of compromise is sufficiently great to justify the major expenditure of a new engine development.

The case of engine noise illustrates this. Although the knowledge that engines could be made quieter has existed for many years, the design changes from current military-based engines were recognized to be so great that a whole new engine system development program was involved; support for such a program became available only after aircraft noise was recognized as a national problem and one that was seriously inhibiting further growth in a critical air transport capability. Aeronautics R&D must show how to avoid these very costly experimental programs in system integration if rapid development in air transport is to be realized.

The challenge to the experimentalist of obtaining definitive information on the interaction between the various elements of a system is no small one. First, to devise an experiment such that information on the interaction between two elements can be used to predict interaction in other generally similar cases, is extremely difficult; the interaction between supersonic inlets and engines is a good example of the difficulty. Yet, if this cannot be accomplished, the permutations become hopelessly large, and research degenerates into specific problem solving. Second, to devise an experiment which does not require construction of a complete operating system, is also extremely difficult. It is here that the ingenuity of the experimentalist finds its premium. If he can find inexpensive means to show the nature of the interaction problem and obvious paths to its solution, aeronautics can move ahead rapidly; if each new revolutionary step requires the high risk and high cost of a complete experimental system, then progress will be retarded.

At least two paths, which have not yet been exploited fully, appear open to the experimentalist to accomplish this. The first of these is the principle of modifying an existing system, to incorporate an advanced concept, solely for the purpose of obtaining confirmation of prior analysis. Although this is often proposed and occasionally supported by research groups, too frequently the program evolves into one of solving the developmental problems of a new concept, a very costly and unnecessary activity for research. If aeronautics R&D is to exploit this principle, it must avoid the developmental aspects or costs will severely restrict the exploitation. The second approach involves use of the flight simulator as a research tool. The simulator is well established as a training tool. It is achieving increasing use as a developmental tool. Its use as a research tool to study inexpensively the systems integration problem, particularly where the human is involved, is not yet widespread; however, enough research work has been done to establish the very great potential of the technique. It is hardly necessary to point out that success in simulator research depends critically on sound analytic understanding of the physical principles involved; without this, the whole process reduces to nonsense.

In the aeronautics R&D program of the future, then, the experimentalist should play two principal roles: 1) to provide the information and guidance required to develop adequate theories in the basic sciences involved in aeronautics, 2) until adequate theory can be developed, to carry out those experiments necessary to define the interaction between various elements of the aircraft which as yet remain beyond the scope of any analysis, and yet whose compatibility is essential to success.

Conclusions

Perhaps the principal conclusion to be drawn from this look at the future of aeronautics R&D is that very much remains to be done. To accomplish this it appears that the level of scientific sophistication must be measurably increased over that existing during recent years. To realize the full potential of air transportation within desirable or necessary time spans will require a more organized and in depth attack than has been typical heretofore. Because of the very wide range of possibilities, it will be necessary to achieve the capability of eliminating unpromising avenues through analyses, retaining only those most promising for increasingly expensive hardware research and development. It is not too difficult to conclude that aeronautics R&D has not met fully the challenge facing it, and so has been, in part, responsible for the failure of air transport to realize its full potential.

To meet these challenges, aeronautics R&D must take positive action along the folllwing lines:

1) The aeronautics research man must take his knowledge with regard to physical possibilities of air transport and join with the socioeconomist in analyzing what kinds of possible air transportation are of most importance, nationally and internationally, in terms of continued world development. These studies should provide the basis for developing the most effective R&D program in aeronautics.

2) The emphasis placed on theoretical analysis in all sciences of concern to aeronautics must be increased greatly; in particular, full advantage must be taken of modern computational techniques to rid aeronautics of the multitude of disconnected, approximate, and semiempirical theories that have been the heritage of earlier theoretical analyses. A much closer tie between theory and experiment is required, with increased effort in experiment directed explicitly at validating new and extended theoretical treatments.

3) An increased experimental effort must be directed at the difficult problem of system integration. Methods must be found to attack this problem successfully, which do not require construction of complete new aircraft systems, and which ultimately will enable complete synthesis of the problem.

Air transportation has assumed today a position of great importance in world development. It has achieved this principally through exploitation of long-range, high-speed transport; but many important air transport capabilities have been exploited only slightly, or remain unexploited. The reason seems clear, high costs that are associated with high technical risks. The solution to this problem lies in the activities of aeronautical research and development. It will require a well organized, aggressively pursued R&D program, taking full advantage of all new technology, to reduce the technical risks to a financially acceptable level. This challenge is a large one, but, if met, major contributions to continued world development can be expected. Aeronautical R&D has within its reach the capability of meeting this challenge; there remains only the problem of exercising it fully and effectively.