

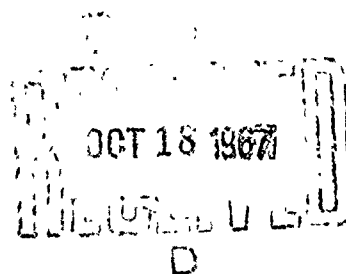
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AUGUST 1967

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INNOVATION AND
MILITARY REQUIREMENTS:
A COMPARATIVE STUDY

Robert L. Perry

PREPARED FOR:
UNITED STATES AIR FORCE PROJECT RAND



The **RAND** Corporation
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PREFACE

This RAND Memorandum examines the interaction between technological innovation and military requirements with the object of suggesting how the effectiveness of the research and development mechanism can be improved. A better understanding of the ways in which requirements are related to or derived from innovative technology is obviously desirable in times when technological advances are so crucial to the maintenance of an effective military force. The possibility that important advances in military capability may arise in an earlier and more appropriate recognition of innovative technology has several interesting implications.

Using as examples two cases of major innovation in military aeronautics -- the turbojet engine and variable-sweep aircraft -- the study considers the relevance to the military case of certain traits generally ascribed to innovation in a civil sphere. It is concerned with likenesses and differences and with the influences of those characteristics on the pace and outcome of research and development.

As part of RAND's continuing work on research and development policy for the United States Air Force, this Memorandum should be of special interest to that part of the technical community concerned with plans and requirements, particularly at the level of exploratory development. But it also has implications for such varied functions as laboratory management, research and development contracting, and the governance of major development programs.

SUMMARY

A great many factors, including a large element of chance, influence the reciprocal interaction between technological innovation and military requirements. This study is most immediately concerned with the circumstances that ultimately cause some branch of the military to adopt a very novel embodiment of new technology. In examining those phenomena, the Memorandum considers innovation to be a three-phase process that includes invention or conception, demonstration of feasibility, and acceptance or adoption. On the strength of an examination of the characteristics that theory attributes to innovation in a military setting, the Memorandum suggests that the decision to proceed with an innovation, or not to proceed, resembles in the broad the classical investment model and proposes a modification of that model to fit the case of innovation in a military setting. Two major innovations in aeronautics, turbojet engines and variable-sweep aircraft, serve to illustrate the thesis and provide an empirical base for several observations and speculative conclusions.

The patterns of innovation that characterized the evolution of turbojet engines and variable-sweep aircraft generally resembled one another except in their feasibility demonstration phases. The prospective utility of each device was demonstrated relatively soon after invention, though the delay in recognition of its practicability was greater for variable sweep than for the turbojet. In both instances, demonstrators (or prototypes) had obvious deficiencies. But although efforts to overcome the deficiencies of prototype turbojets began immediately after feasibility demonstration, in the case of variable-sweep aircraft the known problems were treated either as intractable or as not worth solving. The potential of variable sweep seems to have been neglected because apparently satisfactory alternative means of compensating for the basic limitations of fixed-sweep wings were available, and because of the general belief -- quite erroneous -- that the variable-sweep installation was responsible for several serious flight deficiencies of the demonstrator aircraft. Even when independent

scientific inquiry subsequently found a way of overcoming the residual problems of wing-sweep variation, the military was slow to acknowledge the value of or the requirement for an operational application.

The wartime environment in which demonstrations were conducted certainly encouraged recognition of the potential of turbojet propulsion. So did the absence of equally attractive alternative ways of improving aircraft performance. Variable-sweep concepts were first demonstrated during peacetime and against a background of several other promising advances in aerodynamic design. Such differences have obvious importance. They suggest strongly that perceived need encourages the early exploitation of innovations even when technical feasibility has been indifferently demonstrated and that in placid times evidence of technical feasibility may have to be compelling before the military will seriously invest in devices that are strikingly novel.

In the case of variable sweep, several abortive episodes of pre-development occurred before the concept was finally accepted. Given that basic technical feasibility was not in question, in each instance the absence of a compelling requirement or the absence of understanding that a valid requirement could be stated caused a lapse of interest.

The advantages of having an improved awareness of the impact of innovations on military requirements, and of requirements on innovation, are obvious. On the evidence, it seems particularly important to conduct technical feasibility demonstrations as quickly and cheaply as possible once an innovation has reached the stage where feasibility appraisal is appropriate. Thereafter, matching the innovation to an appropriate requirement is more a matter of taking into account the inherent characteristics of the innovative device than of forcing the innovator to satisfy some remotely applicable statement of requirements.

As in so many other aspects of research and development where innovative technology is concerned, uncertainty is the chief constraint on the decision process. Deciding whether the value of a novel device

would be worth the investment needed to perfect it, and whether its usefulness would be at least as great as that of alternative devices attainable at the same or lesser cost, is largely a matter of reducing uncertainties to manageable dimensions. Lessening technological uncertainty is a first step toward moderating the influence of policy uncertainties, and hence of insuring that the interaction between innovation and requirements is a proper one.

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I. INTRODUCTION: THE CHARACTER OF INNOVATION?

At present there are three general viewpoints on the question of how technology could most effectively be employed in generating good weapons. Without too much oversimplification it can be said that the first view calls for using only thoroughly proven technology in new systems; the second for a systems emphasis without much regard for the uncertainties of new technology; and the third for emphasizing the opportunity factor in technology, encouraging the evolution of systems from a base of broad technical development rather than from abstractly conjured system requirements. The flaw in all these viewpoints is that they tend to ignore the reactive influence of innovative technology on requirements, and of requirements on the handling of innovations.

It seems reasonable to assume that the elements of innovation constitute a three-step sequential process that begins with concept or invention, proceeds to demonstration of feasibility, and ends with acceptance, adoption, and imitation. Development occurs as part of acceptance and adoption. These characteristics have been ascribed to innovations in a competitive market.* Whether they hold for the military case is not certain. Although parallels might be drawn, some of the issues that are of considerable concern to an analysis of civil innovation -- growth rate of the national economy, or market factors, for example -- are clearly irrelevant to the military case. But as a working hypothesis, it may be suggested that there is an important resemblance to the classical investment decision, even though the disposition of a proposed innovation in military aeronautics may be more dependent on the character of existing military requirements than on circumstances ordinarily arising in the free working of economic forces.

Although the intensity of inventive activity may be responsive to a variety of economic and sociological factors, success is in many

¹See, for example, J. A. Schumpeter, The Theory of Economic Development (Harvard University Press, Cambridge, 1949).

respects a random event that does not conform to any standard pattern of behavior.* To paraphrase Sir Solly Zuckerman, evaluations of the prospective worth of untested inventions are not the stuff of which one can yet see a predictive science being born.** Hindsight is still the only sure way of identifying "significant" inventive activity. Interesting ideas are not difficult to come by and it is quite common to find that a crucial event later characterized as "the invention" has been preceded by a vast number of arid proposals and abortive experiments that are of interest mostly to historians of the obscure. It may be romantic to credit invention to a spark of genius, but it is more accurate to treat it as the final product of a great deal of illuminative energy.

"Demonstration of feasibility" has two aspects: technical feasibility and economic feasibility. It is entirely possible to have one without the other; indeed, that may be the most common situation. In any case, for the purpose of this study "economic feasibility" will be equated with something more commonly called "military feasibility" or "the potential of satisfying a valid military requirement." The concept is not very satisfactory because military feasibility is a poor surrogate for economic feasibility. There are no reasonable standards for defining it. What is militarily feasible to one group, or culture, may seem stark madness to another. But in the broad, when either development or adoption of a new device is being considered, the advantage of benefit over cost need not be so obviously or completely favorable for military devices as for their commercial equivalents. In a time of pronounced national stress almost any product or process that promises a momentary military advantage stands a good chance of adoption. The costs of innovation tend to appear inconsequential as against the penalties for having to use unapt combat

* See J. Jewkes, D. Sawers, R. Stillerman, The Sources of Invention (Macmillan, London, 1958), and The Rate and Direction of Inventive Activity, a report of the National Bureau of Economic Research, (Princeton University Press, Princeton, 1962).

** Scientists and War: The Impact of Science on Military and Civil Affairs (Hamish Hamilton, London, 1966), p. 120.

equipment; objective cost-and-benefit analysis becomes less likely as wartime stresses increase.

Conversely, the temptation to endorse or to sponsor a risk-laden military innovation is lessened in peacetime. Very powerful nations facing no obvious threats tend toward a stagnant military technology between wars. Military leaders may push for new or better weapons during peacetime because that is in the nature of their calling, and has been for thousands of years. But as often as not, "new" becomes "more" and "better" becomes "more reliable." Political leaders, particularly if they are subject to dismissal by a tax-conscious electorate, tend to treat whatever is in the existent inventory as entirely satisfactory. It is the "outs" who clamor for new weapons or new policies and the "ins" who are obliged to defend the status quo. And, given a situation in which no grave threat to a nation is apparent, or acknowledgeable, high risk innovations usually receive no more than cursory consideration when need is not obvious and benefit is uncertain. High cost is a major consideration. Innovation may be very costly in devious ways: a successful innovation can cause an entire inventory of weapons to become obsolete, vastly increasing the price of maintaining a predetermined military effectiveness. Under conditions of high satisfaction with whatever is currently being produced or stocked or used, the conditions that would encourage a favorable reception of a proposed innovation will rarely occur.

Interestingly, the institutional reaction of a military service to an innovation that threatens the existing structure or allocation of functions, resembles in many respects the reaction of a very large industrial firm to an innovation that promises to change existing markets or production processes.* If the status quo is threatened, as when small firms are innovating successfully, or (in the military sphere) when another country is known to be intent on new and more effective weapons, both big industry and its military counterpart

*See, for example, Walter Adams and Joel B. Dirlan, "Big Steel, Invention, and Innovation," in The Quarterly Journal of Economics, May 1966.

presumably will be more receptive to innovation. Even in that case, however, powerful opposition from supporters of the status quo can be anticipated. The decision of the United States to invest in ballistic missiles in the mid-1950s is probably the classic example.

A comparable problem of acting on an innovation in industry is illustrated by United States Steel's delay in adopting the basic oxygen process of steel making until 1964, a decade after the first small American firms had done so. Apparently the firm perceived no real threat to its share of the market until then. The fact that earlier adoption presumably would have increased earnings and lessened depreciation and replacement costs seems not to have influenced the corporation; the lack of a threat to the status quo or (to put it another way) the absence of a valid requirement for change, seemed more important.*

Economic feasibility, or military feasibility, is a concept that must be handled circumspectly in any consideration of the motivation for or the rationalization of innovation. The degree of demonstrated need seems to be the most vital consideration in deciding what is militarily feasible and what is not. It seems reasonable to suggest that appreciation of need may be inversely proportional to the stability of the status quo -- both in military situations and in their civil equivalents.

What constitutes a convincing demonstration of technical feasibility (the prerequisite for proceeding to the third stage of innovation) is decided in much the same way as a determination of economic feasibility. When pressures for change are most extreme, as in war-time or when the general market for an established product is declining or a new market is emerging, it seems reasonable to expect the stringency of requirements for a technical feasibility demonstration to lessen. In such circumstances, some evidence of technical feasibility might be accepted that in other, less strained conditions, would be dismissed as insufficient.

*Adams and Dirlan, pp. 105-107.

The third phase in the innovative process (as here considered) is that of acceptance, adoption, and imitation. Whether acceptance follows feasibility demonstration, and how closely, may be dependent on a number of variables. For military projects, the forcefulness, vitality, and urgency of a military requirement is an obvious determinant. In the same sense, the potential of an innovation in the commercial world may go unrealized if no one sees an unfilled need for a new product or process. The key may well be the balance between the perception of a valid requirement and the conviction of feasibility imparted by a demonstration.

To create a model of the innovative process that puts much emphasis on feasibility establishment is a risky undertaking if only because the values that must be assigned various factors are so heavily dependent on the environment and on the outlook of the participants. As an example, technical feasibility meant one thing to the Germans and quite another to the Allies. Caught up in the desperation of 1944 and 1945, the Luftwaffe put into regular service rocket-powered interceptor aircraft that the Russians, the British, and the Americans subsequently adjudged too dangerous even for experienced experimental test pilots. The Germans put the V-2 ballistic missile into production so early that more than 65,000 engineering orders were subsequently needed to make it operational. On the other hand, the British went about wartime development of the Hawker Tempest so cautiously that when the aircraft eventually entered service late in 1943 it was inferior in performance to improved models of the Spitfire it had originally been intended to replace. In the immediate postwar period the British decided not to attempt manned supersonic flight experiments because of danger to the test pilots. In the same vein, it is worthwhile to recall that the United States made no serious effort to develop an operational supersonic fighter until the Korean War spurred interest, while the British, though having an appreciable advantage in turbojet technology in 1952, neglected to develop a comparable aircraft until the late 1950s. Both, however, invested in the development of jet-powered strategic bombers, that, though

they subsequently proved difficult to perfect, seemed more "technically feasible" at the time.

One classic illustration of the ways in which viewpoint influences judgments about economic feasibility in a military setting was the Allied reaction to early indications that the Germans were developing a ballistic missile. After calculating the resources required for development and production and the probable military potential of the product, Allied intelligence concluded that no German rocket would ever be fired at England. The British assumed that no warhead smaller than five tons could be militarily influential and that a "large" missile could not be completed in time to affect the course of the war.* The second assumption was quite sound, but the first had no meaning at all to the Germans, whose standard of values was different. Lacking any other way of bombarding England, the Germans decided that a ballistic missile costing \$45,000 in production lots was quite as sensible an investment as a \$50,000 bomber that was almost sure to be shot down. The point is that economic feasibility can be very introspectively interpreted when military needs are pressing.

Such examples strongly suggest that the strictures implied by the phrases "economic feasibility," "military feasibility," and "proof of technical feasibility" can cause different reactions in different institutions; can vary in importance with time; and can be given one connotation when applied to commercial developments, another when applied to routine or noncritical military requirements and still another when a military situation is desperate.

One would not be surprised to find significant differences in the ways the civil and the military establishments respond to proposals for major technical innovation. A rather obvious distinction is that the military presumably would tend to emphasize intangibles more than would a commercial firm: threat and prospective tactical worth, to note two prominent factors, probably would be given greater weight by the military, while absolute cost might in many circumstances be viewed

*David Irvine, The Mare's Nest, (Kimber, London, 1964), pp. 44-45, 127.

as a less dominant consideration. Still, the approach taken by the military to the assessment of the anticipated value of an innovation and the anticipated cost of its development seems broadly analogous to the way civil firms assess investment opportunity. In both cases the strength of the justification for innovation (or investment) tends to increase as the predicted worth of the product exceeds the sum of probable costs over time.*

The predicted value of an innovation would tend to be low if its worth only moderately exceeded the cost of acquiring it, particularly if there were any notable uncertainties about its application. That is, worth would be low if the requirement were marginal, perhaps because of the lack of a perceived threat or because of alternatives that seemed less risky or costly. Obviously, transitory judgment or perception would have a more important role for the military than for the civil case: risk and threat, in the military sense, are elusive qualities that resist measurement. The military would presumably react more positively to a perceived threat if only because the penalty for failing to act could be extreme, whereas for the civil case a threat might have to be appreciably larger before it prompted a strong reaction. Being second or even third to innovate does not normally

*A simplified way of stating the equation is that the expected value would be equal to the expected utility (or worth) of the project discounted at the prevailing market rate of interest. The analogy can be expressed as:

$$PV = \sum_{t=1}^n \frac{E(W_t)}{(1+r)^t} ,$$

where E is the expectation operator and PV is the present value of the expected benefits arising from innovation at a particular year, t, in the future; W is worth; and r is the rate of interest. W, it should be noted, represents the customer's evaluation of the net worth, for period t, of such factors as the need for the product, the product's cost, the availability of alternatives, the apparent threat, lead time prospects, and uncertainty, among others. Having a comprehensive, precise mathematical notation of the functions at hand is less important, however, than appreciating the general functional relationships among the crucial variables.

condemn an industrial concern but in some circumstances a nation might not survive being a poor second.*

The most that can be said of worth expectations is that predictions of requirement, cost, alternative choices, lead time, and the like need be off very little to change the value of the equation radically. Uncertainty of worth or uncertainty of cost makes an assignment of expected value very risky. For that matter, a relatively slight difference in the value assigned to expected worth by two authorities can make remarkable differences in the present value factor, as witness the crucially different British and German estimates of the value of the ballistic missile. Estimating a substantial advantage of worth over cost would generally increase expected value and enhance the desirability of an innovation.

As the time needed (or predicted) for the perfection and introduction of an innovation increases, or as the assurance of a considerable time requirement becomes more pronounced, the apparent value of the innovation will be comparably lessened. Thus, for example, the German decision in 1941 to put off an investment in advanced aircraft because they expected to end the war in a single summer's campaign, and the later decision to opt for all sorts of striking innovations in aeronautics when it became apparent that several more years of fighting were ahead.

A convincing feasibility demonstration would appreciably enhance confidence in the predicted worth of whatever innovation was being

*The fact that there are few striking examples of fatal consequences arising from having been surprised by enemy innovation may say nothing at all to the point. U.S. willingness to innovate was a prime factor in the outcome of the Pacific war, even though the Japanese had lost before the first atomic bomb was dropped. Given a quicker perception of possible worth, the Germans might have radically changed the outcome of the European war by 1945 -- but they failed to exploit the potential implicit in such weapons as submarines, missiles, and reaction-powered aircraft. Improper initial use of the tank, the submarine, and poison gas are notorious examples of badly managed military innovations of the 1914-1918 period. The case of the machine gun is almost a classic.

evaluated. Indeed, it is difficult to conceive of any single event that could so markedly change the value of the entire equation, particularly if doubt about the technical feasibility of the innovation had been prevalent earlier. A feasibility demonstration, if successful, immediately raises the value of the worth factor and reduces one uncertainty inherent in the cost factor (costs are easier to calculate once one knows what is being costed), thus encouraging a more favorable view of the innovation. Moreover, because a convincing feasibility demonstration serves to narrow the range of uncertainty about the time needed to complete development, it also tends to reduce the value of the time reciprocal and to further enhance estimated value. If technical feasibility can be shown, and military feasibility thus becomes more certain, it may also be said that some determination of economic feasibility has been made.

Other effects of varying the factor values in the equation can readily be seen. On the whole, it would appear that the investment equation provides a base for an interesting analogy to the military evaluation of proposed technical innovations. At the least, it provides a vehicle for assigning subjective values to the several considerations discussed earlier in terms of "economic feasibility," "military feasibility," and "technical feasibility," and it describes the interrelationship of the main considerations.

It is obvious that the expected value of a proposed innovation could vary greatly as the innovation progressed from idea to perfected item -- which is to say that time usually reduces uncertainty and makes prediction somewhat less risky. Given nothing more than a reasonable preliminary statement of invention, an extremely high value would have to be assigned to probable worth before the low values of the other factors could be overcome and a high expected value assigned to the innovation. During the invention stage, before any real feasibility demonstration, the calculation of probable costs is very nearly impossible -- a point that most inventors seem incapable of understanding. No real assurance that values assigned individual factors are other than hypothetical is available in advance of a

feasibility demonstration -- and not always afterwards. Nevertheless, a demonstration of technical feasibility (and of general military feasibility) could serve as an initial point for estimates of economic feasibility or military value. Earlier evaluations would have little credibility.

In the sections that follow, an effort will be made to establish the degree to which actual events in the cases of two major aeronautical innovations are representative of the conjectural model, derived from investment equations. Once that has been done, general means of evaluating the need or desirability of proceeding with the development of items that can rationally be characterized as major innovations in military technology may be suggested. What is being sought is a way of evaluating the real requirement ("expected value") for a specific item of innovative technology. Difficult to quantify, however, are such factors as perception and interpretation of threat, commitment to an existing inventory of weapons, or intellectual resistance to a proposed innovation. And all these, plus other intangibles, influence the assignment of values to the various factors.

II. TURBOJET ENGINES^{*}

Propulsion devices based on the principles of Newton's third law of motion were proposed as early as the 17th century and the proposition that an internal combustion engine using petroleum-base fuels could be made into a turbojet was advanced in the 19th century. Early in the 20th century various inventors patented machines that in one way or another compressed air, forced it into a combustion chamber where it was mixed with a fuel and ignited, and exhausted the expanding gas through discharge nozzles. Neither piston compression nor a compressor driven by a separate, internal combustion engine could be made sufficiently efficient, however, and it was not until the appearance of a rotary compressor interconnected to a gas turbine that a reaction engine became a reasonable prospect.

Although some attempts were made early in this century to use the exhaust products of a conventional piston engine for propulsion, early efforts were almost all based on mechanically coupling a turbine section to a propeller. Industrial gas turbines, on which a fair amount of work was done between 1919 and 1930, were designed to deliver mechanical force to an electrical generator or other rotating machine. The principle of using the exhaust of a gas turbine for propulsion was recognized at least as early as 1921 but for quite respectable reasons was not used at that time. British researchers concluded in 1920 that turbine machinery powerful enough to drive an aircraft propeller would have a weight-to-horsepower ratio at least twice that of the 2.5:1 of

^{*} Unless otherwise indicated in footnotes, the facts here summarized were drawn from the standard accounts of turbojet engine development: (1) Robert Schlaifer, Development of Aircraft Engines (Harvard University Graduate School of Business Administration, Boston, 1950), pp. 321-509; (2) M. M. Postan, D. Hay, and J. D. Scott, Design and Development of Weapons (H. M. Stationery Office, London, 1964), pp. 135-236; (3) Oliver Stewart, Aviation: The Creative Ideas (Praeger, New York, 1966), pp. 156-169; (4) John Jewkes, David Sawers and Richard Stillerman, The Sources of Invention (Macmillan, London, 1958), pp. 314-321. I am solely responsible for any interpretation that arises in the necessity of reconciling differences between accounts.

contemporary reciprocating aircraft engines. These estimates were based on experience with industrial turbines and made no allowance either for the lightweight construction of aircraft engines or for the fact that a limited-life engine might be entirely acceptable for aircraft use. More than 15 years later the U.S. Navy, attempting roughly the same evaluation, came to a conclusion even less favorable. In any case, during the 1920s compressor design and turbine blade materials were not far enough advanced to support any meaningful development effort.

BRITISH EFFORT

Dr. A. A. Griffith, a scientist at the Royal Aircraft Establishment, was the first to approach the problem of turbine and compressor blade design from the standpoint of an aerodynamicist. On theoretical grounds he concluded in 1926 that he could design a high-efficiency compressor that would make the gas turbine a practical device for driving a propeller. The Aeronautical Research Committee, approximately the equivalent of the American National Advisory Committee for Aeronautics, subsequently approved the conduct of a small scale research program intended to prove or disprove the validity of some of Dr. Griffith's concepts. Although the results of preliminary wind tunnel testing and compressor turbine experimentation were encouraging, the effort came to nothing. In 1930, Dr. Griffith was assigned to an Air Ministry laboratory where there were no facilities for wind tunnel research, and by the time he returned to the Farnborough station the depression had set in and funds for such ambitious projects were not to be had. Between 1929 and 1936, the R.A.E. did no further work toward the development of a gas turbine propulsion system.

At about the time that Dr. Griffith was reporting the results of his early experiments to the Aeronautical Research Committee, Flight Officer Frank Whittle proposed and then patented the use of a gas turbine for jet propulsion. Whittle, who was unaware of an earlier patent in France and of Griffith's work, did not anticipate using his turbine as the drive unit for a propeller and consequently was

able to propose a much less complex machine than the axial-flow turbine with gearbox connection to a propeller that interested Griffith.

When Whittle first submitted his proposal to the Air Ministry in 1929, it was sent to Dr. Griffith for appraisal and was rejected as impractical. In many respects the assessment was fair, for at that point Whittle's design was relatively crude and it did not seem likely that materials capable of withstanding the temperatures and stresses he contemplated could be made available in the near future. Whittle also attempted to interest an industrial turbine manufacturer and an aircraft engine manufacturer in his ideas, but was unsuccessful. The consensus was that contemporary metallurgy was not up to the task asked of it, that a sufficiently high compressor efficiency could not be obtained, and that in any case jet propulsion was unlikely to be wanted because of the relatively low speeds that could be extracted from the lightly stressed wood and metal aircraft of the period.

In a sense, evaluation of the Whittle proposal in 1929 represented the first attempt to assign a value to the proposed innovation. The term of development promised to be long, the cost of obtaining suitable materials and shaping them was sure to be high, and to most prospective supporters the worth of the investment must have seemed slight. Whether Dr. Griffith was unduly influenced by his own precommitment to what he considered a more feasible alternative -- the prop-jet -- cannot be estimated. But in any case the need for a turbojet engine was not apparent and its application could not be predicted. Together these factors would give Whittle's proposal a very low expected value. On the other hand, some theoretical basis for Whittle's claims certainly existed and the cost of verifying them need not have been great. Had a very careful appraisal of these circumstances been made at the time, it seems likely that some provision for subsidizing further research might have resulted. After all, Griffith's project was no sounder.

Like Griffith, Whittle was convinced that current compressor and turbine efficiencies could be improved if an aircraft application were kept clearly in mind. He appreciated that the forward motion of an aircraft would of itself tend to enhance apparent compressor efficiencies,

and he was certain that as aircraft speeds increased the turbojet engine would become the only sensible means of propulsion. However, he could do little to correct the defects of his original proposal until he had more time for research than a junior flying officer in the Royal Air Force was ordinarily permitted, and he was entirely aware that he needed a better base of knowledge on which to proceed. An opportunity for more formal training arose in 1934 when the RAF assigned him to Cambridge University, where he took the mechanical sciences course. Still, by 1935 he had so little residual confidence in the commercial prospects of his invention that he allowed his original patent to expire, and neither he nor anyone else in England seems to have sensed a possible military application.

Late in 1935, Whittle obtained tentative assurances of financial support for further research from a small group of private investors who had concluded that in time a turbojet engine might turn a very good profit. The only application that was seriously advanced was for a high-speed mail carrier.

Some of the expectations on which the 1935 appreciation was based seem odd 30 years after the fact, but they were not as farfetched as they seemed. First off, in the mid-1930s the market for high-speed mail planes of moderate range was quite good. Bristol and de Havilland in England; Boeing, Lockheed, and Douglas in America; and Heinkel in Germany were profitably selling such aircraft. Second, it was not unreasonable to conclude that an engine could be developed for \$250,000, the sum Whittle thought it necessary to raise. And finally, if an appropriate engine could be developed it would return enormous profits because in its intended application it would monopolize the market; competition from reciprocating engines was all but unthinkable. Given Whittle's optimism and the willingness of his investors to risk relatively little capital over a period that was not expected to exceed five years, the return seemed most promising and the expected value of the innovation, in consequence, surprisingly high.

On the other hand, the prospects of a military application were discouraging. Schlaifer has suggested that Whittle did not think of

applying the power plant to an interceptor-fighter because fighter aircraft of the time were being designed for relatively long endurance at medium and low altitudes.* That is not an entirely convincing argument in light of the fact that the Spitfire, the Hurricane, the Heinkel He.112, the Messerschmitt Bf.109, the Seversky P-35 and the Curtiss P-36 were all in early development in 1935, and each was in essence a short-endurance, high-speed interceptor. A more plausible explanation is that Whittle unquestioningly accepted the Trenchard premise, "the bomber always gets through," and that fighter projects of that era were invariably designed for short-term development. A five-year investment would represent unacceptably high time costs -- interest equivalents -- and even if direct costs seemed prospectively low and returns prospectively high, the expected value of the undertaking would be lower than the value of a similar, short-term investment in more quickly available conventional fighters more certain to be available when required.

Again, the logic of the decision to concentrate on a commercial application of Whittle's turbojet was not faultless, but in the environment of the moment any other outcome would have been surprising. Given an appreciation of the long-term promise of the turbojet for military aircraft, the project would have been valued more highly and some glimmer of Air Ministry interest could have been anticipated. But the fact was that in the circumstances the Air Ministry was quite incapable of such long-term forecasting of need.

Owing to his status as an RAF officer, Whittle had to submit the details of his design and proposed engine to the Air Ministry before making any binding agreements for its commercial exploitation. Ordinarily, if such proposals had any such apparent military potential, the Air Ministry imposed a secrecy restraint and reserved all rights in the invention. In this instance, Ministry officials expressed no more than an academic interest in Whittle's work and interposed no objections to its being developed for civil uses.

*Schlaifer, p. 338.

Given his experience to that time and a realistic apprehension of the difficulties that still faced him, Whittle estimated that he could design the engine and build a flight prototype for an expenditure of about \$250,000 over a period of five to seven years. Finding capital was something of a problem, but by the end of 1935 he had joined with a group of optimistic engineers and investors to form a new company called Power Jets, Ltd., organized for the express purpose of developing and commercially exploiting his jet engine design. Not much could be done to further improve the design without some experimentation, but owing to the scarcity of funds the group decided to forego extensive component testing and proceed directly to the construction of a bench test engine. Perhaps the decision was inevitable; the highly specialized facilities that would have permitted trials of the turbine and compressor were not available in any case. But there can be no quarrel with the judgment that Whittle and his supporters chose the course that was most nearly correct -- a feasibility demonstration at the least cost and in the least time that circumstances would permit. In June 1936, therefore, Power Jets authorized the British Thompson-Houston Company to build a complete engine to Whittle's current specifications.

The group knew that building an engine would be a costly process. An effort to obtain additional financial support from the Air Ministry was unsuccessful, leading officials there having concluded that Whittle was unlikely to succeed where so many predecessors had failed. But encouragement from Henry Tizard^{*} and the vague prospect that funds might be obtained from the Aeronautical Research Committee proved enough to attract the capital needed to pay for the bench test model, and in April 1937 it was operated for the first time. (Power Jets had spent about \$30,000 to that point, almost all on the bench-test engine.) That the engine worked, even though at a lower output than anticipated, was enough to induce the Air Ministry to reconsider its

^{*}Tizard had more influence in Government at that time than any other English scientist, being almost solely responsible for the British decision to invest in air defense radar.

earlier ruling and to promise \$25,000 in research funds. In order to obtain the money, however, Whittle had to agree to rebuild the original engine and operate it at a higher output for at least 20 hours, a task that ultimately occupied him until 1939.

Frugality remained the watchword through July 1939. The total income of the company to that time was less than \$100,000, of which about 85 percent had been obtained from private investors.* Air Ministry support consisted of partial progress payments on work being done, and less than \$16,000 had actually been paid over. In July 1939 the imminence of war, a record of constantly encouraging progress, and a better impression of possible military applications was enough to induce the government to assume full financial responsibility for further development.

Development progress was probably the most important factor in the July 1939 endorsement of Whittle's engine. The bench-test model had operated inauspiciously in the summer of 1937, turbine efficiency being particularly disappointing, but that it operated at all encouraged optimism. And there were outside influences too. Griffith had earlier been induced to take up his turbine research once again, and by 1937 the Swiss firm of Brown-Boveri was guaranteeing delivery of gas turbines that could be used as blowers for refineries. In light of these events some of the earlier quibbling about technical feasibility began to seem less pertinent. Funds were made available to support a new program of turbine research by the Royal Aircraft Establishment, the object being ultimate development of an axial-compressor turboprop engine. However, the first R.A.E. test bed engine was not completed until October 1940, so the main burden of development remained with Whittle. A residual difficulty was that the Ministry still did not think the Whittle design entirely practical, having greater faith in the approach supported by Griffith and the experts of the R.A.E. Nevertheless, with the encouragement of funds provided

* Whittle received no salary from Power Jets, his only income being his service pay, although from July 1937 onward he was permitted to devote his full time to the engine development project.

by the Ministry, Whittle proceeded to build a second version of his engine in which turbine efficiency was about 25 percent better. The combustion system still was defective, however, and after less than five hours of bench stand operation the engine failed. Undismayed, Whittle constructed a third version, again using many reworked parts from the original, and resumed testing in October 1938.

By that time the main problem was plain to all: combustion system inadequacies. Although experiments using the third version of the Whittle test engine continued for 30 months, it was not until October 1940 that a satisfactory burner and flame tube became available -- designed, appropriately enough, by a petroleum company engineer Power Jets had called in. Much remained to be done in the way of improvement, but finding a working solution to the combustion problem made the Whittle engine a practical device and made possible its further development into a flight article.

When, in July 1939, the Air Ministry had somewhat reluctantly agreed that the theoretical feasibility of the Whittle engine had been validated there was no evading the next step -- commitment to a demonstration of its actual flight potential. Power Jets was thereupon promised the funds needed to build a flight version of the engine and the Gloster Aircraft Company was instructed to design and construct an appropriate airframe. Although intended primarily to be an experimental vehicle, the Gloster aircraft was to be designed to the general specifications of an interceptor fighter, the most obvious and urgent application that could be conceived. Late in 1939, after the war had actually begun, the Air Ministry authorized the development of a still more advanced version of the flight engine. Two models were known respectively as the W-1 and W-2.

Early in 1940, several months before assembly of the first W-1 but at a time when Germany was crumpling Britain's continental allies one by one, Gloster was told to begin the design of an operational fighter based on the W-2, work to start as soon as the original experimental aircraft (the E 28/39) had been laid out. This eventually became the Meteor, the specification being approved in September 1940,

at the height of the Battle of Britain. Two months later the government made available to Power Jets a large blower that could be used to test combustion chambers apart from the main engine. Earlier experimentation had been inhibited by the need to use a bench-test engine in combustion section development, a most inefficient procedure and one that certainly slowed progress.

During the summer of 1940, while the W-1 engine was being fabricated and assembled, reasonable solutions were found for both the combustion section design defects and the turbine blade problems of the basic engine. The first was a design innovation, the second a materials and fabrication process improvement. Together they made the W-1 a thoroughly practical proposition. Although an ever widening band of possibilities was being explored, until that time there had been no real assurance of such a fortunate outcome.

Ground tests of the W-1 began in December 1940, a modified version was completed in April 1941, and on 15 May 1941 the E 28/39 was flown on the power of a Whittle jet engine. At relatively low altitudes it proved faster than the Spitfire, the best fighter the British then had, and it was not greatly inferior at middle altitudes. A further turnabout in opinion was signaled by decisions on the part of Rolls Royce, Bristol, Vickers, and de Havilland to invest in turbojet development and on the part of the government to undertake quantity production of both the W-2 and the Meteor. The most important indicator of the changing climate was an order to the Rover Company for a small number of W-2B engines incorporating design changes that Whittle proposed after he became convinced that the original W-2 had serious faults. Preparations for large-scale production were also begun.

Although it appeared that technical feasibility had been adequately demonstrated (and the war situation served to lessen the importance of the "military feasibility" issue), such an assumption became somewhat less tenable during late 1941. The first W-2Bs completed by Rover proved subject to violent compressor surging that severely limited their thrust output, and even though a separate program of compressor development was undertaken by the company, the problem

persisted. Moreover, the turbine blade material used in the Rover-built engines proved insufficiently durable; not until July 1942, when the British obtained from the General Electric Company blades built for the American version of the W-2B, was turbine life extended past 25 hours. Then in March 1942 Power Jets began the development of a new engine, the W-2/500, which incorporated a succession of compressor improvements proposed by Rolls Royce on the basis of its previous experience with turbosuperchargers. The W-2/500 also included a turbine design featuring much improved engine airflow.

During its initial test in September 1942 the W-2/500 exceeded its design thrust requirements without any surging. Subsequent trials indicated that reliability and operating life had also been improved by the redesign. The Rover version of the W-2B survived 25 hours of full thrust (1600 pounds) running in March 1943, and the following month a model developed by Rolls Royce (later the Welland) passed a 100-hour test. During the first quarter of 1943 Rolls Royce took over full responsibility for what had been Rover's role in the development-production program.

The Welland engine was put into production in October 1943. Service deliveries began the following May. Meteor fighters equipped with Wellands were delivered to operating squadrons in July 1944 and were immediately pressed into service against the V-1 buzz bombs. The service engine weighed 850 pounds, had a thrust output of 1600 pounds, and pushed the Meteor to a maximum speed of 410 miles per hour.

Several factors changed both rapidly and repeatedly in the period between 1937 and October 1940, when a very firm commitment to the Whittle turbojet finally emerged. In 1937, operation of the first bench-test engine provided marginal validation of Whittle's main claims to technical feasibility. The probable worth of the engine immediately increased. The existence of the Brown-Boveri turbines encouraged hope that something useful would come of the Whittle project. As affairs had progressed pretty much the way Whittle had predicted, officialdom came to have greater confidence in his predictions of cost and probable development time. The need for a high

speed interceptor became acute after the Munich crisis of 1938 and by July 1939 had brought on a general subsidy of further development. The time required for completion of development had decreased merely by passage of the months, and the cost of investment had lessened, relatively, with the beginning of general rearmament in Britain and the consequent abundance of comparatively "cheap" funds.

The 1939 decision to accelerate development of the Whittle turbojet was eminently sound. Its chief ingredients were the improved credibility of Whittle's expectations (arising largely in the increasing convincingness of the feasibility demonstrations) and the steadily mounting valuation of the worth of a successful turbojet. Although the cost was clearly going to be higher than initially estimated, the range of cost uncertainty was smaller and in terms both of funds available and the cost of alternative ways of achieving the same (now more desirable) end, cost was no longer the dominant factor it had been.

GERMAN EFFORT

When the first British Meteors entered active service they were about 110 miles an hour slower than the Messerschmitt Me.262 jets the Germans were operating. That disparity was, however, less the result of better engines than of superior aircraft design; the Me.262 was designed for a maximum speed of 582 miles per hour at sea level, a figure established on the premise that anything greater would cause severe compressibility problems. The engine was another, and even more interesting matter.

Quite independent of Whittle, and without any knowledge of previous British work on aircraft turbines, Hans von Ohain,* a student at the University of Göttingen, had begun studies of jet propulsion during 1934. The design he proposed was in principle like Whittle's but in important details it was appreciably different. It perhaps owed something to earlier work conducted by Professor Albert Betz at

*Who is quite inexplicably called "Dr. von Chain" throughout the Postan, Hay, and Scott study.

Göttingen, but the Ohain patents were quite as original as were Whittle's. And, as with Whittle, once the principle had been worked out Ohain was obliged to face the question of finding the money to fund development.

On the strength of a recommendation from one of his professors, Ohain was approached by Ernst Heinkel, who had convinced himself that something better than the reciprocating engine would be required to attain the high flight speeds of which new airframe designs seemed capable. (Even before taking on Ohain, Heinkel had begun a collaboration with Wernher von Braun in experiments coupling an early liquid-rocket engine to a light airframe. The work eventually led to the He.176, the first aircraft specifically designed to fly on rocket power.) Early in 1936, Heinkel put Ohain in charge of a company-funded turbojet development program. With two or three assistants and the grudging assistance of several Heinkel engineers, Ohain succeeded in building a bench-test engine that first operated in March 1937, a couple of weeks before Whittle's prototype engine started its sequence of initial testing.

Ohain's success had even more striking results than did Whittle's. Heinkel immediately authorized the construction of a flight engine and began the design of an operational fighter to contain it. Ohain encountered much the same sort of technical difficulty as Whittle, particularly in compressor and combustion chamber efficiency, and his first flight engine was not up to its assignment. Redesign followed, an improved engine was assembled and bench tested in the spring of 1939, and on 27 August of that year it first flew in the He.178.

Heinkel had kept the results of the work entirely secret until mid-1938, when Hans A. Mauch of the Air Ministry accidentally learned of the development. Mauch visited the Heinkel plant, inspected both the engine and the He.178 airframe, investigated the question of probable aircraft speeds, and concluded that the German government should encourage the further development of jet engines. Information of that verdict reached Herbert Wagner, chief of airframe development at the Junkers Airplane Company, which had separately sponsored a program of

turbojet development since 1936. Like Heinkel, Junkers had conducted its work without advising the government or, for that matter, the Junkers Engine Company, a separate corporate entity. Convinced that the reciprocating engine would not produce the speeds of which new airframes were capable, and lacking confidence in the foresight of the very conservative Junkers Engine Company, Wagner had induced the airframe company to allow one of his staff, Max Mueller, to undertake development of an axial-flow turboprop engine that in many of its essentials resembled the concept earlier endorsed by Griffith in England. Within a year, by 1937, preliminary experiments had convinced Mueller that the shortest path to high aircraft speeds led through turbojets rather than turboprops. A Junkers team of about 45 people centered their efforts on an axial-flow engine completely different from the centrifugal-flow turbojets of Whittle and Ohain. The native advantages of axial flow and some very ingenious design expedients conceived by Mueller made the Ju.006 (as the engine was labeled) appreciably lighter and smaller in diameter than its contemporaries. It reached the bench-test stage during 1938, still funded entirely by Junkers, but proved unable to run under its own power. At that point Wagner disclosed the existence of the program to Mauch and, like Heinkel, asked that the government subsidize further work.

Although very enthusiastic about the future of turbojet propulsion, Mauch was convinced that meaningful progress toward operational engines could best be made by the existing engine concerns. To that point none had indicated awareness of, much less interest in, turbojets. The Aeronautical Research Establishment at Göttingen had invested in additional research on compressors in the interim, however, and some of the earlier objections to the practicality of turbojet development had been dispelled by the findings. Although Mauch offered government financing, only the Bavarian Motor Werke (BMW) of all the major engine firms in Germany was certain enough of turbojet feasibility to undertake immediate research. The Junkers engine people ultimately established a program based on the Mueller concepts but discarded virtually all of the components developed for the initially unsuccessful Ju.006.

In its stead, the engine group began development of a new axial-flow engine later called the Ju.004.

Convinced that the feasibility of turbojet propulsion had been sufficiently demonstrated by Heinkel and by Junkers, the Air Ministry in the fall of 1938 instructed Messerschmitt to design a fighter around engine specifications derived from the research at the Aeronautical Research Establishment -- which meant small frontal area axial-flow engines. The design that emerged moved to the fabrication phase in 1939; it became the Me.262, the first operational jet fighter in the world.

By the spring of 1939 five separate commercial firms were developing turbojet engines of one sort or another, all but Heinkel under government sponsorship. Mauch decided it was time to organize the effort and to eliminate duplications. He was unsuccessful in efforts to induce Heinkel to turn over the Ohain engine to Daimler Benz and most of the Mueller group went either to Heinkel or to other engine builders rather than transfer to the Junkers Engine Company, but he managed to involve each of the existing engine firms in at least one development project and he insured that several alternative approaches were adequately covered. By the end of 1939 Junkers was at work on a relatively conservative axial-flow engine (Ju.004), Bramo on a more powerful axial turbojet (003), Heinkel (which had finally obtained government support) on a new centrifugal engine designed by Ohain (001) and a very advanced axial-flow design by Mueller (006), and Daimler Benz on a counterrotating compressor engine incorporating a ducted fan (007). Not only was the program considerably more comprehensive than its British counterpart, but in most respects the Germans seemed to be both more thorough and more aware of advancement opportunities than their British opposites. They had equally as much trouble with turbine blade design and fabrication as the British, however, and were not so fortunate in having access to the rare-metal alloys to which the British and the Americans turned in overcoming the difficulty. As Göttingen had actually done the basic research in compressor characteristics that the R.A.E. had decided was not worth

attempting, the Germans were much better prepared to move from concept to final design. Moreover, owing to the native disorganization of their early effort and its lack of commitment to a preferred design course, the Germans had several attractive options available when they decided to move toward an operational turbojet-powered aircraft.

Even though the Germans had a very substantial lead in research and development by 1940 (at which time the British still had not solved either the turbine blade or the combustor problem), they failed to exploit it during the next three years. Heinkel's He.178, comparable to the E 28/39, flew nearly two years sooner than the British airplane, but the prototype Me.262 did not fly until July 1942, only a year before the first Meteor. (The He.280, Heinkel's intended alternative to the Me.262, had flown in mid-1941 on the power of two Ohain-designed 001 engines, but the airframe had defects that made production very unlikely.) Equipped with Junkers 004 engines, the Me.262 was in almost every respect a better aircraft than the Meteor, but a decision to put the Messerschmitt aircraft into production was not made until June 1943 and the subsequent production program was badly handled. Nearly a year was expended in stumbling over decisions on materials and priorities and on a controversy over the most suitable operational role for the fighter. Consequently the aircraft did not enter the operational inventory in any numbers until the late months of 1944, by which time small numbers of Meteors were also in squadron service.

In large part the dilatory pace of German engine development after 1941 was the consequence of the Air Ministry's conclusion that "the most pressing problem ... was not the development of a flyable turbojet but the design and development of an engine suitable to become an ultimate service type."* The only turbojet that saw appreciable service use was the Junkers 004, and that was because the Junkers people stuck to conservative engineering practices in an attempt to create a serviceable engine suitable for large-scale production. By early 1944 the Air Ministry had frozen the design of the 004 and had essentially canceled all the earlier turbojet programs except the

*Schlaifer, p. 403.

BMW 003 in favor of developments aimed at second generation engines that could not possibly be ready in time to influence the course of the war. Moreover, although the 004 design was frozen early in 1944 and both plant facilities and production workers were allocated to Junkers, there was no large-scale production for several months thereafter owing to difficulties arising from technical defects of the interim engines.

As Schlaifer has pointed out, the differences between the turbo-jet programs of Britain and Germany are mostly explained by the differences in the conditions under which development took place and by differing objectives of development. The lack of certain materials in Germany clearly did much to influence the pattern of German development and largely explains why German engines were less durable than British engines. The Germans needed high performance fighter aircraft to stand off Allied bombers and therefore developed an appropriate engine; the British saw no need for a fighter that sacrificed range for speed. Thus, says Schlaifer, the British developed

... a thoroughly reliable engine of reasonably good fuel economy, whatever the cost in time of development or in difficulty and expense of manufacture, whereas the Germans rushed into quantity production an engine which had been intended only as a preliminary stage in a longer development program, and to some extent sacrificed the performance and reliability of even this engine in order to make possible very rapid production with a minimum consumption of strategic materials.*

An understanding of German reaction to the Ohain and Mueller projects requires a more complex statement of the environmental influences than is needed for the British case. First, it is apparent that two airframe manufacturers, Heinkel and Junkers, put a very high valuation on the prospective worth of a turbojet engine. The best explanation seems to be their common belief that only after the creation of an exceptionally powerful engine would they be able to build aircraft superior to those being developed in other countries. German engines were distinctly inferior to British aero-engines through the

* Schlaifer, p. 439.

1930s and promised to remain so into the 1940s. (Heinkel had to put a Rolls Royce Kestrel in his prototype He.112 because a sufficiently powerful Junkers engine was not available.) The Ohain proposal may have had a low probability of success (or a high probability of extreme cost) but Heinkel and Junkers seem to have concluded that depending on conventional engines from German industry would be at least as costly, or risky, or both. It was not so much that the prospective value of the Ohain or Mueller engines was great, but that the value of the promised conventional engines was less.

For Mauch, a different set of values applied. He concluded that although the promise of turbojet development was great, the cost of entrusting it to other than experienced engine firms was unacceptable. He probably concluded, as well, that the time-phased cost of a continuing investment would be too high unless development could be vested in more experienced engine builders. There was no real dispute among Heinkel, Junkers, and the Air Ministry (personified by Mauch) on the probable value of a successful development or even on the probability of success. But to ensure the dominance of his ideas on the best way to certify the success of development Mauch uprooted the original projects and gave them to new custodians. Those custodians, the traditional engine builders of Germany, had still a third set of values. Their judgment was that the accelerated development of conventional engines was a preferable alternative: Junkers Engine assigned a lower expected value to the turbojet than did Junkers Aircraft, for example.

In any event, the Germans put a perfectly good -- for the time -- turbojet into operational service, and by dint of better aeronautical engineering housed it in an aircraft far superior to either of its Allied counterparts, the Meteor or the P-80. That the Germans did not more successfully exploit their early lead in turbojet development seems a matter of misjudgment rather than bad fortune. Even though the British muddled through, they managed to stay roughly in time with the Germans. The British estimate turned favorable later than did the German estimate, but was not subsequently re-juggled to the disadvantage of the total program.

Finally, it is clear that in the German case, as in the British, an early demonstration of the technical feasibility of the turbojet engine made enormous leverage available to those who favored its development. Even though the prototype Junkers engine would not operate under its own power, there seems to have been no question of the soundness of its design. Development engineering was the requisite. The Ohain engine had many of the faults and troubles that characterized Whittle's engine, but its ability to operate overcame qualms.

U.S. EFFORT

The potential advantages of gas turbines for aircraft propulsion were understood in the United States at least as soon as in England and Germany, but nothing meaningful was done to advance the concept during the 1930s. The principal reason seems to have been the unquestioned assumption that a turbojet power unit would weigh as much as a reciprocating engine of comparable power output and would, therefore, need to have about the same rate of fuel consumption. Computations of required efficiencies for such components as turbines and compressor sections suggested that operating temperatures would be too high for any existing materials. Alternatively, experience with industrial turbines and some consideration of their use in naval vessels suggested that turbines would weigh appreciably more per unit of horsepower than existing aircraft engines. Finally, until about 1940 there was no widespread appreciation of the possibility that airframes could be designed for speeds in excess of 400 miles per hour, above which reciprocating engines became inefficient. All in all, the Americans seem to have overstated difficulty and underestimated worth on every possible occasion.

There were only two serious proposals for gas turbine propulsion development in the United States before word arrived of the success of the Whittle engine. Each was sponsored by an airframe company, and each began in 1940 after an indeterminate period of preliminary discussion. Northrop Aircraft Corporation invested about \$25,000 in study of a high efficiency turboprop engine before approaching the

military services with a request for a development contract, estimating that development would cost about \$1 million. After a considerable delay Northrop was authorized to design a 2500 horsepower engine and to build the compressor section, but the program was badly thought out and started slowly. In 1943 the Air Force abandoned the novel concept that a good compressor could be designed in the absence of an engine to test it and authorized the construction of a complete engine. It was completed and bench tested in December 1944, becoming the first American turboprop engine to operate, but it still had not flown when the war ended and it was abandoned shortly thereafter.

Lockheed Aircraft Corporation took an approach that was unique in the United States. Concluding that an engine-airframe combination would shortly be needed that could substantially outperform anything then contemplated, Lockheed put a turbine specialist to work on the question of an appropriate engine. Early in 1941 the designer, Nathan C. Price, decided that a turbojet was the obvious answer. Lockheed then began the detailed layout of both the engine (the L-1000) and an airframe (the L-133) to complement it. In much the fashion of the Germans some three years earlier, Lockheed set a high performance goal and designed toward it: 625 miles per hour at 50,000 feet! Unfortunately, Price had designed the engine to conform to the efficiency concepts then fashionable in America, so it was a very complicated and difficult development project. Rather than proceed with the program on its own, Lockheed approached the Army with a request for a development contract but was unable to find a receptive audience. Inconclusive discussions continued for nearly two years before Lockheed learned that since 1941 other companies had been working on jet engines of both American and British origin. Although funds for development of the L-1000 were ultimately provided, there was a clear understanding that the project was aimed at long-term applications rather than use in the war.

The only other native development of any consequence was a turbojet based on a Turbo Engineering Corporation supercharger program. Intended to provide boost thrust during combat, the engine was a

Navy-sponsored development that did not begin until October 1942, was delayed for several months by events that had nothing to do with the technical features of the engine, and was eventually dropped because work on the turbojet was interfering with the production of badly needed turbosuperchargers.

The importation, improvement, and production of the Whittle engine under auspices of the Army Air Forces had considerable significance for the long-term prospects of U.S. turbojet development, but in the context of this study chiefly serve to indicate the acceptance of the British design. In one important respect American engineering influenced the wartime course of turbojet development: because of experience with turbosuperchargers extending over two decades, General Electric was able to make several changes to the original W-1 engine that improved either its performance or its reliability. Notable among these was the development of turbine blade material that permitted the British to satisfy performance requirements their own experts had been unable to cope with. Subsequent to the initial transfer of the Whittle engine, General Electric's production engines tended to depart from the British model in many details, but the changes introduced by the American company were probably no more important than similar changes suggested by Rolls Royce, with a somewhat similar background in supercharger development.

American experience in turbojet development before 1945 was dominated by an extreme aversion to risk. Neither Lockheed nor Northrop proved willing to make any substantial research and development investment in their own designs without assurance that the War Department would underwrite virtually all expenditures. It is difficult to say whether that aversion was grounded in a low confidence of success or -- more probably -- in the continued presence of opportunities for a higher return on investment elsewhere. In the case of the War Department, the absence of appreciation of the potential of turbojet development cannot validly be cited as the reason for inaction because there is no indication that anyone influential in that department had ever been exposed to a prospectus on the engine until General H. H. Arnold *

heard of the Whittle turbojet. In the circumstances, it seems most unlikely that anything approaching a demonstration of the feasibility of an American design could have been conducted before 1944, largely because they were based on invalid engineering concepts that derived, in their turn, from an insufficiency of sound basic research.

The American experience chiefly serves to demonstrate how unlikely it is that a striking innovation in military aeronautics can get a hearing in the absence of a sound technical foundation. But something may also be said about the existence of dominant alternatives, for in the realm of highly efficient air-cooled reciprocating engines no country was better provided than the United States. Finally, a subsidiary reason for disinterest in turbojet engines may deserve casual notice: until actually embroiled in the war the U.S. Army and Navy air forces concentrated their attention very largely on the sorts of aircraft -- long endurance bombers and shipboard fighters -- that could least profit from the availability of a reaction engine.

OTHER EFFORTS

In addition to the German, British, and American experiments with turbojets there were various concepts at large in the world that might, with proper fertilization, have led to something useful. An American, R. E. Lasley, actually assembled and operated a turbojet in 1934, but it had very low efficiencies, his design objective was unsound, and his expectations were too far in advance of reality. In 1930, Secondo Campini, an Italian engineer, patented a propulsion device that used a conventional engine and a ducted propeller as a compressor section while burning fuel in the exhaust duct. The Italians built and in 1940 flew (at least once) an aircraft featuring that system. Between 1936 and 1940 the U.S. National Advisory Committee on Aeronautics performed some desultory experiments involving the Campini system, and the effort was accelerated early in the war years. But impossibly high fuel consumption ratios doomed the concept; it was abandoned everywhere by 1943. René Anxionnaz of France filed patents on a complete turbojet engine in December 1939, basing his design on the

results of work with gas turbine power plants for naval vessels. Several prototypes were actually built by Société Rateau in 1940, but they were taken by the Germans. In 1944 Rateau secretly began the construction of another set of prototypes that was finished after the war.*

In the Soviet Union many years of investigation and experimentation had led to some small, poorly conceived turbojets capable only of augmenting the performance of conventional aircraft. Nothing powerful enough to serve as a prime engine was built in Russia until captured Junkers and BMW turbojets became available to serve as models.**

None of these random projects had any influence on the main course of turbojet development. The concept of using the exhaust of a gas turbine for jet propulsion had been proposed at least as early as 1921, it was discussed and evaluated in France, Germany, England, and the United States during the 1920s and the 1930s, and each of several nations numbered among its engineers several who had a reasonably clear understanding of what had to be done to reduce the concept to practice. How the transition was to be made could not have been predicted before 1939; it is clear that the impulse of the war was the chief motivant of the development that followed.

EVALUATION

How evaluating authorities looked on the prospect of developing an operationally useful turbojet engine has been discussed. The dominant consideration, in each instance, seems to have been the initial tendency to conclude that the value of a turbojet would be less than the value of improved conventional aero engines. Sufficiently convincing feasibility demonstrations were carried through in England and in Germany by 1940; thereafter the prospect of acquiring

* Financial Times (London), 2, 3, 5, 8, 10, 11, 15, 22, 23, 24 November; 2, 6, 8 December 1966.

** Vaclav Nemecek, "Turbojets and Tribulation," Flying Review International, April 1966, p. 490.

operationally desirable turbojets and the expected worth of such units were assigned considerably higher values. The circumstances that influenced that change of viewpoint were dominated by the success of various feasibility demonstrations, each success tending to increase the anticipated value of the product, but the influence of wartime urgency cannot be discounted. First, it made the relative cost of the investment seem less important, if only because enormous sums were being spent on many risky projects, and second, the very great advantages of having turbojet-powered aircraft (or the equally great disadvantages of not having them when the enemy did) became more apparent as the prospect of their appearance became plainer. In brief, the increasingly greater value of anticipated worth as compared to predicted cost tended to encourage acceleration of development. It seems equally clear, however, that a more comprehensive analysis of the prospective value of turbojet propulsion following early feasibility demonstrations might have further accelerated the availability of the turbojet engine.

III. VARIABLE-SWEEP AIRCRAFT

Means for changing the angle of wing-sweep in flight were provided in an airplane built by Clement Ader in 1890, and though most of the English-speaking world holds that the Wright Brothers were the first to make and fly a powered aircraft, most Frenchmen and not a few Englishmen are convinced that Ader anticipated the Wright Flier by some 13 years.* Whether it flew at all is arguable, and it certainly could not have flown well, but the Ader airplane was perhaps capable of flight under ideal conditions. Variable sweep was Ader's way of controlling pitch or longitudinal stability. In many other respects, the Ader aircraft shared features with the Wrights'; they both used vertical rudders interconnected with the wing-warping controls to provide roll and turn control, for example.**

Ader made at least one further attempt to apply the variable-sweep concept, but it accomplished no more than his first. (He was, incidentally, a well respected and successful inventor whose influence on the evolution of the telephone may have been as important as Bell's.) In February 1904 Ader applied for a patent on what would today be called a hydrofoil with air-cushion features, a vehicle that resembled nothing

*The evidence for an Ader flight in 1890 or 1891 or 1898 is flimsy at best, resting largely on the fortuitous finding of two pieces of coal, supposed to have been used as markers of the start and end of free flight, some 27 years after the event. Nothing was made of the claim before 1906, not much more thereafter. The issue is of no moment here, in any case; curious readers can easily enough find arguments on all sides of the controversy. What is interesting, however, is that it seems to be the first instance of intense technological chauvinism affecting aeronautics. The average American who is interested in aeronautical history is unlikely to be aware of Ader, much less of the German claimant, K. Jatho, or the candidate generally advanced by the British, Sir Hiram Maxim (who was, embarrassingly, an American citizen when his machine was "operated," in 1894, in something less than free flight), or the Russian's I. N. Golubev. The issue is not "who was first," which tends to be a magnet for pedants, but who took the first significant step in the innovative sequence that extends from concept through general adoption.

**Oliver Stewart, Aviation: The Creative Ideas (Praeger, New York, 1966), pp. 17-35.

of later vintage. It embodied lifting surfaces -- "near-wings" -- that folded back along the hull when the vehicle was at rest or moving slowly, but extended to a 90 degree angle with the hull when high speed was required. Though the relationship of wing angle to speed was the opposite of that later adopted for aircraft, a vague awareness of the relevant principles was apparent in Ader's original design. If the Ader aircraft did not fly, it can at least be said with assurance that his hydrofoil operated moderately well. In so doing, it presumably performed the first in-motion wing angle change.*

The Wright airplane proceeded from concept through supportable theory to demonstration, and so did the Ader hydrofoil. The airplane was further developed and used widely. Ader's hydrofoil was not. Nor were various features of the Wright airplane that in their own right were innovative -- specifically, the canard elevator system and wing warping. But on such grounds alone there is insufficient justification for concluding that one device -- the airplane -- was successful while another -- the hydrofoil or wing warping -- was not. It is enough to say that one found a ready acceptance, having displayed some technical attractiveness, while another did not. For both reasons and significance one must seek further.

As understanding of basic flight principles became more general, it was inevitable that someone should hit on the idea of asymmetrically varying wing sweep as a means of providing attitude control. Experiments to that end were carried on in France as early as 1911, and in 1914 a patent application covering the principle was filed by Edson F. Gallaudet of Norwich, Connecticut. He concluded correctly that sweeping back the tip of either wing would reduce the lift forces on that surface and thus induce the aircraft to bank in that direction. Nothing came of the Gallaudet patent, but some ten years later Professor G. T. R. Hill of England built an aircraft embodying very similar principles. Like Gallaudet, Hill was chiefly attracted by the fact that asymmetrically

*H. F. King, "Swing Wing, Variable Incidence, Air-Cushion Hydrofoil," Flight International, 18 November 1964, pp. 68-70.

variable sweep was a way of managing the attitude of an aircraft.* Although the approach had some theoretical advantages over ailerons, for two reasons it did not provoke much interest. First, available control devices perfectly satisfied immediate needs. There was no ready market. Second, and perhaps more important, though technically feasible, a wing-sweep control system was immensely more complex than an aileron system, and it had no compensating practical advantage.** Still it was not entirely lacking merit: NACA reinvented it in 1945.***

GERMAN EFFORT

None of the early work had any lasting importance. The cornerstone for what came later was the conception of wing sweep as a device for lessening the effects of compressibility induced by high-speed flight. Although moderately swept wings had been used on aircraft as early as the 1920s, chiefly as a way of improving the longitudinal stability of short-coupled airframes, a high-speed application was first proposed by Dr. Adolf Busemann, a young German physicist, to a 1935 conference of scientists in Rome. His abstract mathematical treatment of the properties of various wing forms created no special stir, but that evening the conference sponsor, General G. Artur Crocco, teased Busemann for having evaded the question of practical application. On the back of the dinner program, Crocco, who was director of scientific research for the Italian Air Force, sketched a high-speed swept-wing airplane. But except for Busemann, all those present shortly

* Hill began with the idea of designing a tailless aircraft and this led him to the study of alternative means of controlling flight attitudes. All the Hill-designed pterodactyl aircraft embodied wings having moderate sweepback, there being no better way of stabilizing the longitudinal axis of flight once the conventional tail was dispensed with.

** Although Hill is generally assumed to have concentrated on moving the wing tips to control the aircraft, he patented one design (January 1930) that called for differentially varying the sweep angles of the complete wings.

*** E. F. Gallaudet, U.S. Patent 1,200,098, granted 3 October 1916; M. M. Alexander, "Structural Problems Associated with Variable Geometry," AIAA Paper 65-774, November 1965; D. Kieth-Lucas, "Professor G. T. R. Hill," Journal of the Royal Aeronautical Society, LV, March 1956.

forgot the discussion and its implications. Theodore von Karman, who had heard the exchange, ignored wing sweep when he was called on to help overcome compressibility limitations that affected such high-speed fighters as the P-38. He later chided himself for his forgetfulness. Still, there was no reason for taking special note of the theory Busemann had advanced. Considering the propulsion devices available or realistically predictable in 1935, compressibility problems seemed unlikely to become troublesome enough to require such an extreme solution as wing sweep.*

Late in 1937, at a meeting of the German Academy of Aeronautical Sciences, Busemann predicted the occurrence of severe control problems when straight-wing aircraft encountered the compressibility effects characteristic of flight at high subsonic speeds, and mentioned his earlier findings on the theoretically desirable effects of sweepback. Dr. Albert Betz of the Aerodynamics Research Institute at Göttingen was by that time also doing research in the field.** Dr. Woldemar Voigt, chief of aerodynamics and preliminary design for Messerschmitt, heard of the Betz-Busemann work through one of his junior engineers and, sensing that the concept might have an application to several high-speed aircraft with which Messerschmitt was then concerned, induced his company to sponsor the first comprehensive wind tunnel tests of wing-sweep effects. By 1942 Messerschmitt had laid down the

* Adolf Busemann, "Überschallgeschwindigkeit," in Convegno di Scienze Fisiche, Matematiche e Naturali; Tema: Le Alte Velocità in Aviazione (Rome, 1936), pp. 315-347; comments by T. von Karman following R. Smelt's "A Critical Review of German Research on High-Speed Air Flow," Journal of the Royal Aeronautical Society, L, December 1946, p. 927; T. F. Walkowicz, "Birth of Sweepback," Air Force, XXXV, April 1952, pp. 30-33, 72.

** Betz had considered the stability characteristics of the swept wing much earlier; see A. Betz, "Applied Airfoil Theory," Aerodynamic Theory, W. F. Durand (ed.) (reprint, Cal Tech, 1943, first printing: 1934), IV, 99-110. Others who dealt with sweepback in prewar years included M. M. Munk, "Note on the Relative Effect of the Dihedral and the Sweep Back of Airplane Wings," NACA Technical Note No. 177, 1924; H. G. Kussner, "General Airfoil Theory," NACA Technical Note No. 979, 1941; and H. Schlichting, "Airfoil Theory at Supersonic Speed," NACA Technical Note No. 897, 1939.

first of several airframe designs based on wings swept at 20 to 35 degrees and other firms were beginning to express interest in the swept-wing concept.

Among the Messerschmitt scientists who became acquainted with the Busemann hypotheses and the Betz research was Dr. Alexander Lippisch, who had earlier and independently concluded that a delta wing with a moderate angle of leading edge sweep represented the best design approach to very high-speed flight. As early as the mid-1930s he was well along the path that was to lead ultimately to the rocket powered Me.163 aircraft of 1944.

An imaginative designer of enormous intellectual capacity, Lippisch sensed the inherent shortcomings of swept-wing aircraft more quickly than did most of his contemporaries. Several of his delta-form aircraft had actually flown before 1940, so he had practical experience with swept leading edges. Although his basic design was adequately stable in the form it had by 1941, he departed momentarily from the main course of his research during 1941 and 1942 to design and patent a variable-sweep aircraft having better low-speed handling characteristics than the usual modified delta of the period. Lippisch certainly was not alone in appreciating that the low aspect ratio wing best able to overcome high-speed compressibility effects possessed undesirable low-speed characteristics. He did not provide for changing the sweep angle of the entire wing, however. Roughly the outboard 40 percent of the wing swung forward for landing and takeoff. Perhaps Lippisch understood the stability problems that would arise from a combination of a straight-out wing with a fuselage that lacked horizontal tail surfaces; perhaps he sought only a modest improvement in his basic delta-form design. He appears, nonetheless, to have been the first to propose combining the advantages of swept and straight wings in a single, variable-sweep aircraft.*

*Walkowicz, loc. cit.; interview of Prof. A. Lippisch by David Sawers, Princeton University, 1964, communicated to author by private correspondence; Theodore von Karman, Aerodynamics--Selected Topics in the Light of their Historical Development (Cornell University Press, 1954), pp. 133-134; Reichspatentamt M 152 348 XI/62b, by Messerschmitt

Both the low speed instability of swept-wing aircraft and structural problems arising in the thin airfoils and sharp attachment angles of highly swept wings were recognized in Germany by 1943. Lippisch favored the delta as much on structural as on aerodynamic grounds, although the Lippisch designs actually built had swept trailing edges that made them structurally troublesome too. Another group in Germany that included Voigt, Betz, and Busemann, advocated fixed-angle swept wings augmented by such variable-geometry accessories as slats, slots, fences, spoilers, and air brakes. Lacking convincing evidence that one approach should be pursued to the exclusion of the other, the Germans attempted both. A decision to concentrate attention on a single configuration probably could not have been enforced in any case. Oddly enough, considering the German proclivity to invest heavily in a variety of high-risk R&D projects, there is no indication that the Lippisch-proposed variable-sweep aircraft was ever seriously considered for development. Indeed, even Lippisch seems to have lost interest after the first essay, though it certainly posed no insurmountable technical problems. From 1943 to the end of the war he worked almost exclusively on delta-form research aircraft intended for flight at extreme altitudes and supersonic speeds. When the war ended, both Heinkel and Messerschmitt had in development turbojet fighters with high degrees of wing-sweep, but apart from the Lippisch-designed Me.163 no swept-wing aircraft reached operational forces.

Many inquiries into the applicability of wing sweep to operational aircraft were begun once the availability of turbojet engines was assured, however, and some of them edged into the question of sweep variation effects. Owing to the dispersion of German records at the end of the war, it subsequently became difficult to distinguish with any certainty between design proposals and prototypes actually built. One proposal was the Blohm and Voss P.202, a twin-jet fighter unique in that its straight wing rotated in the horizontal plane, giving sweepback to the right section and sweepforward to the left section at the discretion of the pilot. Aerodynamically, it appeared that one sort of sweep would

A.G., 2 September 1942, based on preliminary drawings submitted November 1941 and May 1942, in H. M. Patent Office files, London.

have much the same effect as the other.* There are some indications that the aircraft was built and flown by the end of the war, but if so, the results were not widely disseminated and there is no evidence that the design influenced later events.**

A Messerschmitt design of mid-1942, numbered P-1101, was another of the many proposed fighters incorporating wing sweep. It remained a paper project until September 1944, when the Air Ministry approved the construction of one prototype. Fabrication started in October. Originally laid down as a single place interceptor, the prototype was intended to demonstrate the military potential of a mid-fuselage wing swept at 40 degrees. But so rapidly were new aerodynamic data accumulating, even in the last months of the war, and so susceptible were the Germans to promises of ever better performance, that by early 1945 the P-1101 had lost its priority to a later design with still greater speed potential. Although plans for a production version of the P-1101 were abandoned, the original prototype was so far along in construction that Messerschmitt decided to complete it as a flying test bed. Voigt, who by that time was chief of the Messerschmitt engineering and research establishment at Oberammergau, where the P-1101 was being built, seems to have been responsible for a January 1945 decision to make the aircraft a test bed for a pivoting wing that could be pre-set on the ground at any of three preselected sweep angles.

Until American troops overran Oberammergau in April 1945, the Allies had no intimation that the Messerschmitt plant existed.*** Convinced that their conquerers would recognize the unique value of the exotic design and engineering work underway there, Messerschmitt personnel abandoned the facility in perfect order and prudently

* Eiichiro Sekigawa (ed.), German Military Aircraft in the Second World War (Kantosha Co., Tokyo, 1959) I, p. 201, and II, p. 141.

** Mr. A. Satin, TRW Corporation, in a conversation with the author, 29 May 1967.

*** Oberammergau had no runway, no landing strip, not even a taxi ramp. It is not surprising that aerial reconnaissance never found the plant. Had some photo interpreter been brash enough to suggest that the principal Messerschmitt aeronautical research station had no airfield he would probably have been laughed out of the room.

withdrew to nearby villages. As a precaution against either the accidental or the unthinking destruction of the experimental shops, they carefully bundled up most of the plans, data, and working drawings and trucked them off to secure areas. The air technical intelligence team that arrived shortly after the first combat patrol was one of the most distinguished of its kind in all of Germany. Operating under the direction of R. J. Woods, chief designer for Bell Aircraft Company and one of its founders, the investigating team included Dr. H. B. Hawkins, one of the foremost British aerodynamicists; Jack Woolams, a very highly regarded engineer and test pilot who later joined Bell Aircraft; Dr. Clark B. Millikan; and Col. Randolph Lovelace, perhaps the most highly talented aeromedical researcher then in American uniform.

Under what authority they did so it is difficult to say, but Woods and his people somehow reassembled not merely the senior technical staff of the Messerschmitt research establishment, but enough workers to man a skeletonized prototype shop. Woods also induced the local military authorities to persuade the Germans to dig up the missing plans and data. Most were recovered, although it appears that a French intelligence team was first on the spot at one cache. Fortunately, the P-1101 was about 80 percent complete and was in perfect order, while conveniently at hand were the designers, engineers, and fabricators who had brought it that far.*

The P-1101 never flew, which was perhaps fortunate all around. There was a general feeling among Americans that in flight the airplane would have characteristics that would thoroughly test a pilot's skill. But the chief reason for keeping it grounded was that the prototype HS-011 turbojet engine installed by the Germans could be neither operated nor replaced, and there was no comparably high-thrust axial-flow turbojet to substitute. So the completed aircraft was crated and

*Report of the Combined Intelligence Objectives Sub-Committee, SHAEF: "Messerschmitt Engineering and Research Facilities-Oberammergau" (Item No. 5/49), 1945; Air Technical Intelligence Review No. F-1R-6-RE, "Survey of Messerschmitt Factory and Functions, Oberammergau, Germany," August 1946; letter, J. C. Trotter, Bell Aerosystems Company, to the author, 28 December 1966.

shipped to Wright Field for exhibition and evaluation during the late summer of 1945. Later, both Lippisch and Voigt followed, two among the many German scientists brought to the United States immediately after the war.

Authorities at Wright Field apparently looked upon the P-1101 as a freak of less immediate interest than most of the other rocket and jet aircraft that came to America at the same time. It fell somewhere between the operational jets which most intrigued combat people and the very advanced experimental designs which fascinated aerodynamicists and design engineers. The P-1101 attracted no such attention. In any case, the aerodynamicists were preoccupied with reconciling the tons of theoretical and test data brought from Germany with the most recent products of American research. Only three months before the German collapse, R. T. Jones, an American researcher at NACA's Langley Research Laboratory, had independently worked out the aeronautical principles of wing-sweep technology. Jones disclosed his preliminary findings in February 1945 and the NACA undertook a full-scale test of them in the spring of that year. What with the flood of German data on the swept wing and in the near wake of the revolution caused by turbojet introduction, there was quite enough novelty to unsettle the world of aerodynamics and to occupy the full attention of most engineers and designers.*

That no variable-sweep aircraft completed initial development under German auspices was not owing to any reluctance to design and build an airplane with flight-adjustable geometry. The Blohm and Voss P.202 was mentioned earlier; the Messerschmitt P-1114, a prototype jet that was nearly complete in April 1945, had a swept-wing structure that could be shifted fore and aft along the fuselage to compensate for center of lift movement as flight speeds increased.** It is interesting

* R. T. Jones, "Wing Plan Forms for High-Speed Flight," NACA Technical Note No. 1033, March 1946.

** AAF TR No. 540, "Description of Project P01-114 Under Construction," 6 March 1946. The P-1114 had more than a casual resemblance to the X-4 tailless aircraft built by Northrop for the Air Force and NACA four years later. Based on a Lippisch design, the P-1114 was intended to fly in the Mach 1.2 to 1.5 range at altitudes above 45,000 feet.

that no such compensations were made for the P-1101, although Voigt appreciated that changing the wing angle would induce a corresponding change in the center of gravity. Because the wings of the P-1101 were adjustable only on the ground, ballast was the logical compensation for any shift in the center of gravity. Nevertheless, the lack of some means of fore and aft wing movement was a significant defect of the P-1101 design, one that would almost certainly have made the aircraft very tricky to fly.

A fair number of German aircraft with more obvious shortcomings than the proposed variable-sweep Lippisch fighter were pressed through advanced development and into flight test during the final years of the European war, so lack of interest in the design may have arisen in the simple fact that Lippisch himself was preoccupied with other aircraft concepts. Or it may have been no more than a peculiar side effect of the personal differences that caused Lippisch to leave the Messerschmitt organization in 1943.* In light of the profligate German investment in a variety of radical designs of dubious merit, it may not be important that in 1944 and 1945 there existed no valid military requirement for an aircraft having the special characteristics that Lippisch predicted for his design. If interceptors capable of operating from small fields were wanted, the Germans had the short-takeoff Me.163 and the vertically launched Natter, both rocket powered, though the Natter was certainly more dangerous to its pilot than to American bombers and the Me.163 was a bit too much for any but a veteran flier. Not merely the Me.262, but several more advanced turbojet aircraft could have been made available if a high performance air superiority fighter was wanted. Finally, of course, the engines available by 1945 simply were not powerful enough to generate speeds that would justify the use of a variable-sweep wing. In summary, the concept had no determined and influential supporter, there was no experimental proof that the current variable-wing design was sound (as indeed, it was not), and no valid

*The Lippisch variable-sweep patent was assigned to Messerschmitt, so it might well have been either inconvenient or legally impolitic for Lippisch to have done further work on it after he went back to the Glider Institute.

requirement for that sort of aircraft appeared. So variable sweep remained in the conceptual stage through the end of the Second World War.

In the immediate postwar period, 1946 and 1947, relatively little was done with the variable-sweep concept. German assets had been distributed among the British, French, Americans, and Russians. If the latter were at all interested, no evidence on that point has come to light. The French apparently had a set of plans for the P-1101, the Americans had the airplane and its principal designers, while the British had a smattering of information. The French were in the least favorable position, having come out of the war with a gutted aircraft industry and a six-year hiatus in their fund of aerodynamics. The British were somewhat better off, but decided to concentrate on improving their quite good conventional aircraft rather than invest heavily in new concepts. One of many consequences was that until August 1954 the British had no aircraft that could go supersonic in level flight.

EARLY U.S. EFFORT

In the United States there was very little immediate interest in variable sweep. Although Bell Aircraft Corporation and NACA-Langley paid it some peripheral attention, both were more concerned with compensating for the known deficiencies of the fixed swept-wing in a new generation of American turbojet aircraft. For that matter, although approving the use of wing sweep in the B-47 and F-86, the Air Force was cautious enough to buy alternative straight-wing designs as well. The U.S. Navy initially concluded that the relatively high landing and takeoff speeds of swept-wing aircraft and their instability at low speeds made them unsuitable for carrier use.

The use of slots, flaps, spoilers, and wing fences plus an improved understanding of the structural problems of swept-wing aircraft largely overcame the deficiencies complained of during their early employment. But the first lot of postwar aircraft operated subsonically, and many researchers agreed that a quite different solution might be required once supersonic aircraft were laid down. In July of 1948, less than eight months after the X-1 had made the first supersonic

flight, Smith J. DeFrance of the NACA Ames Laboratory publicly suggested that variable-sweep wings offered the most sensible way of overcoming the takeoff and landing problems that supersonic aircraft were certain to encounter.*

The views of DeFrance were common to both the Ames and the Langley groups. At frequent intervals, starting in 1946, Ames had recommended to NACA headquarters the advantages of a variable-sweep wing. During 1947 and 1948, Ames also attempted to interest several aircraft manufacturers in exploring the variable-sweep principle. Of those contacted, Lockheed, Grumman, and Bell were most attentive -- although Lockheed early concluded that the weight of a fully reliable wing-sweep mechanism would be so great as to offset any advantage provided by the variable geometry.**

Bell, under the spur of R. J. Woods, was appreciably more optimistic than Lockheed about the possibility that an operationally useful variable-sweep aircraft could be built. Bell engineers, drawing heavily on both German research and the more recent NACA studies of the characteristics of swept wings, began a detailed analysis of the potential of variable sweep in 1947. The company, which had designed and built both the first American jet aircraft and the rocket-propelled X-1, stood in no special awe of untried or novel design concepts.

Independent of Ames, Langley scientists in 1945 essentially reinvented the concept of a variable-skew wing -- the technique by which one wing swiveled back to provide roll control through asymmetry of lift. By the end of the year, however, the Langley laboratories had begun wind tunnel tests of a symmetrical variable-sweep wing and in 1946 proceeded to test some subscale free-flight models. In 1947, one of the Langley people suggested adding a variable-sweep wing to the X-1 to improve its performance. While examining that possibility, NACA aerodynamicists first identified the center of gravity/center of lift

* Aero Digest, 1 July 1951, p. 86.

** Memo, H. J. Allen, Chief, High Speed Research Section, NACA-Ames, to the Laboratory Director, 21 April 1949; C. L. Johnson, U.S. Patent 2,794,608, filed 19 April 1949.

relationship problem that was to plague designers of variable-sweep aircraft for another decade. They concluded that a fore and aft wing translation would be needed to compensate for changing longitudinal stability as a wing was swept toward its most extreme angle.

The X-1 proposal was dropped on the grounds that modifying an existing aircraft would not be sensible. But it was conceivable that the addition of a variable-sweep wing to the still embryonic X-2, the first swept-wing rocket-powered research aircraft, might be highly beneficial. Work toward that end was not abandoned until Bell came forward with the proposal that was to lead to construction of the X-5, the first aircraft actually to incorporate a variable-sweep mechanism.*

After moderately intensive work on the possible applications of variable sweep to operational aircraft, Bell in the fall of 1948 proposed to General K. B. Wolfe of the Air Materiel Command at Wright Field that the Air Force buy 24 copies of an improved interceptor version of the P-1101 aircraft. The design that Bell proposed very closely resembled the original P-1101, differing mostly in its incorporation of a mechanism, conceived by Woods, that would permit the wing angle to be varied during flight and the wing structure to be moved fore and aft along the fuselage. Wolfe thought highly of the idea, particularly because it seemed to offer an attractive exit from the flight difficulties then being encountered with early fixed-sweep aircraft. Engineering Division personnel at Wright Field were of another mind, however. They objected that the addition of a sweep-varying mechanism would severely constrain use of the P-1101 as an interceptor. There was no reasonable way of equipping the aircraft with either guns or rockets and, as laid out, the aircraft could not carry enough fuel to make it a good interceptor.

Such objections killed the interceptor proposal. Bell then very sensibly suggested building two experimental aircraft to demonstrate the variable-sweep technique and to provide a full-scale free-flight

*D. D. Baals and E. C. Polhamus (NASA-Langley), "Variable-Sweep Aircraft -- Past, Present, and Future," Astronautics and Aerospace Engineering, June 1963.

vehicle for research on several current problems involving wing-sweep effects.* NACA expressed interest, as did Air Force headquarters. Taking heart, Bell formally proposed such a program in February 1949; the Air Force approved the proposal less than a week later.

The Bell X-5 design started at about the point reached by the Germans some four years earlier. Indeed, Bell took pains to obtain the services of the principal designers of the P-1101, including Voigt, and to get copies of all available German data.

Bell honestly expected that speeds "to at least [Mach] 1.1" could be explored by the X-5. Because the aircraft would be no more than a somewhat sturdier version of the P-1101 with an added variable-sweep mechanism, the Air Force was less optimistic. Air Force officials had no expectation of going on to a production version, but Bell obviously hoped that a variable-sweep interceptor might evolve from the original design. Thus some differences in objective were introduced into the program from its start.

Although the X-5 program was favorably regarded by Air Force headquarters, the Aircraft Laboratory at Wright Field saw little merit in the program. The laboratory could foresee no operational future for a variable-sweep wing and held that data on wing-sweep angle effects could be acquired more cheaply by other means. Fortunately, NACA was greatly interested in testing a highly swept wing in flight, although subscribing to the general belief that such an aircraft would have nasty flight characteristics at low speeds. As Walter Williams of NACA later put it, a main reason for going ahead with the variable-sweep X-5 was that "we didn't have enough nerve to build a 60 degree airplane."**

* Engineering Division, AMC (USAF), "Memo for General Swofford," 24 August 1950.

** Notes on Experimental Aircraft, from tape recording of Los Angeles Section, AIAA meeting 25 January 1965; Engineering Division Memo report 4302-73-5, "Design Comments on the X-5 Airplane," 29 August 1949. Interestingly, at almost the same time the English Electric Company laid down an aircraft with 60-degree wing sweep and a Mach 2.0 level flight capability, the P-1 Lightning.

NACA's wind tunnel research, conducted in advance of Bell's X-5 proposal, had confirmed that if the wings were merely pivoted rearward about a fixed axis, as the P-1101 design provided, the aerodynamic center of lift of the aircraft would shift to a point appreciably behind its center of gravity. Without some compensation elsewhere, the aircraft would tend to become progressively over-stable, and thus less controllable, as its wing angle narrowed. Bell had originally proposed to shift the entire wing section slightly forward along the fuselage, concurrently changing the dihedral of the wings from positive to negative as sweep angle increased. Initially, NACA agreed that the combination of these movements would tend to keep the center of gravity and the center of lift in their proper relation to one another regardless of wing-sweep angle. But before anything in the nature of a formal agreement had been signed, NACA concluded that the center of lift of the proposed aircraft would shift three feet more than the center of gravity as the wing was swept back. Bell rechecked the data and decided that NACA's misgivings were unwarranted but as insurance decided to provide for at least 27 inches of wing travel.

Although the principles of the variable-sweep mechanism conceived by Woods were retained, the design eventually used was developed by J. C. Trotter and R. H. Dufort and was ultimately patented in their names. Dispensing with the variable dihedral feature because it too greatly complicated the design, Trotter and Dufort specified a box structure that moved longitudinally within the fuselage and contained an actuating mechanism to control the sweep angle. Regulation of the relationship between center of gravity and center of lift was dependent on the fore and aft movement of the movable box structure.*

In the light of aerodynamic knowledge that existed at the time, the variable-sweep arrangement designed by Trotter and Dufort probably represented the best attainable compromise of mechanical simplicity with aerodynamic effectiveness. Yet even as designed it was complex,

*Trotter letter, 28 December 1966; W. E. Greene, The Bell X-5 Research Airplane, Wright Air Development Center case history, March 1954; AIAA Meeting Tape, 25 January 1965: J. S. Trotter and R. H. Dufort, U.S. Patent 2699300, filed 20 April 1950.

bulky, and relatively heavy. And by the time it was installed in the X-5, the wing-sweep mechanism weighed about 50 percent more than had originally been proposed. Perhaps more important, the layout of the wing-sweep mechanism inhibited design freedom in other respects. The nature and location of the pivot made it impractical to put fuel cells in the wings, and if the landing gear were hinged to any part of the wing structure the aircraft could be landed only when the wings were fully forward. In any case, unless the original P-1101 design was to be abandoned, both the fuel tanks and the landing gear had to be put in the fuselage. The main landing wheels had to retract into wells aft of the wing in order to stay clear of the wing mechanism, and the struts had to be unusually long because they straddled the underslung engine. The only location left for the pilot and the fuel supply was the upper fuselage, above the engine. For the pilot the placement did not matter too much, but there was a good deal of queasiness about nestling the fuel tank against the combustion section of the engine. The net effect was to give the X-5 a severely limited fuel capacity and one that could not be appreciably enlarged without completely redesigning the aircraft.

Bell was formally authorized to begin construction of the X-5 in March 1949, although no final contract was signed until two months later. A succession of technical difficulties and work stoppages delayed completion of the first aircraft until the spring of 1951, nearer 24 than the scheduled 12 months of fabrication time. (The circumstances were clearly quite different, but it is interesting that Messerschmitt allowed only six months for the construction of the P-1101 and was about on schedule when the plant was captured.)*

*Letter, P. J. Eli, Bell Aerosystems Company, to the author, 8 September 1966; remarks by I. L. Ashkenas, Systems Technology Incorporated, on AIAA Meeting Tape, 25 January 1965. The fate of the original P-1101 is of some interest. Wright Field had "loaned" the aircraft to Bell in August 1948 for use in studies supporting the original interceptor proposal. The following spring, Bell received permission to use the components of the Messerschmitt airplane in any way that would benefit the X-5 project. The records are silent about the remains, but the general recollection at Bell is that after both X-5s were delivered, what was left of the P-1101 was disposed of as miscellaneous

The first flight took place at Edwards Air Force Base on the morning of June 20, 1951. No effort was made to alter the wing sweep until the fifth flight, on July 27. In the course of the sixth flight the pilot experienced some minor difficulty with the manual crank that was to be used for emergencies, and on the seventeenth flight (in September) a small potentiometer drive gear failed, momentarily disabling the wing-sweep mechanism. As predicted, however, when flight speed was reduced the wing tended to return to its full forward position. Apart from some minor mechanical and electrical problems, no further trouble was experienced with the wing-sweep mechanism during tests of the X-5. It proved both sturdy and reliable.*

The experience of the X-5 program demonstrated that a reasonably satisfactory wing-sweep mechanism could be designed, built, and operated in flight. Flight tests also showed that changing the wing sweep of the aircraft in flight influenced its performance almost precisely as predicted. But nobody knowledgeable in the aerodynamics of the technique had ever been doubtful on that score.

Pilots who flew the aircraft found it delightful at speeds between about 300 and 500 knots. On at least one occasion it reached Mach 0.92 in a dive, but the experiment was not often repeated. It was normally limited to Mach 0.85 in level flight. Throughout its career the X-5 was powered by what had originally been intended to be an interim engine, a 4,900-pound-thrust J35. The afterburning J40 engine on which preflight performance predictions had been based never completed development. At higher speeds, the X-5 was susceptible to various instabilities that gave its pilots some uncomfortable moments. Most unsettling, the aircraft had vicious stall and spin characteristics.

scrap. That was in some respects a misfortune, because the X-5 really owed a considerable debt to its predecessor. As one knowledgeable observer remarked later, "there was a bit of conscious copying." Surviving pictures of the two aircraft show them to be nearly indistinguishable.

*Greene, loc. cit. Apart from an alteration in the gear ratio of the emergency handcrank, which needed 400 turns to move the wing through its complete range, no changes were made to the wing-sweep mechanism after the X-5 was delivered.

Sweeping the wing back accentuated its poor low-speed performance, but even with the wing fully forward pilots found it very difficult to regain control after the onset of a stall. These traits were discovered early in the test program, and there was a standing rule at Edwards that no pilot should stall the aircraft unless he allowed himself at least 30,000 feet of altitude for the recovery. The rule was violated only once, on October 13, 1953, with the result that both the number 2 aircraft and its pilot were lost.*

Perhaps the best reasoned assessment of the X-5, and almost certainly the briefest, was offered by John Stack when he described the program to Senators investigating the TFX contract award. "There were certain impracticalities in this airplane...", he observed. "The X-5 did, however, demonstrate in flight the possibilities of compatible configuration for subsonic and supersonic speeds. It indicated, finally, the possibilities for attainment of multimission aircraft..."** Unfortunately, glowing press releases about the aircraft gave the general public the impression that it had been a striking success, while those who knew something about the test program tended to be very dubious about the merits of variable-sweep wing.

More by coincidence than intent, less than a year after the initial flight of the X-5, a prototype of what was intended to be an operational, carrier-based variable-sweep fighter also began flight tests at Edwards Air Force Base. This was the XF10F-1, an airplane designed by Grumman in an effort to reconcile the Navy's need for a high-speed, swept-wing fighter with the very demanding requirements for operation from a carrier deck.

The circumstances that led the U.S. Navy to attempt development of a variable-sweep fighter originated in the exceedingly awkward

* Interviews with John Stack by the author, 23 and 30 August 1966; "Two Wings in One," Interavia, May 1962, pp. 617-619; T. A. Toll, E. C. Polhamus, and W. S. Aiken, Jr., NASA Variable Geometry Research, AGARD Report 447, April 1963, p. 5.

** TFX Contract Investigation (88th Congress, First Session, Permanent Subcommittee on Investigations, Committee on Government Operations, U.S. Senate) Part I, pp. 12-13.

situation in which the Navy found itself in 1946. By nature, turbojet-powered aircraft were not well suited to operation from the carriers the Navy had at the end of the Second World War. Their landing and takeoff speeds were appreciably higher than those of the propeller-driven aircraft they succeeded, and the maneuver options of jets during either takeoff or final approach to the carrier deck were few. Moreover, the first few jet aircraft acquired by the Navy for carrier use suffered from a variety of operational defects. Finally, the swept wing with its still more pronounced low-speed instability appeared before the Navy had succeeded in adjusting to the peculiarities of the early, modestly powered, straight-wing jet aircraft.

In July 1946, the head of fighter design for the Navy advised his superiors that a swept-wing fighter would have to be developed if the Navy was to stay abreast of either its prospective opponents or its sister service. The Grumman Aircraft Engineering Corporation responded by proposing to sweep the wings of an earlier approved design. A decision to proceed was put off, however, because funds to support the development could not immediately be found and because Grumman's engineering staff was fully occupied with other projects. The Navy subsequently argued that it would have been risky to proceed with such a "highly experimental" development until the performance of the Douglas D-588-II could be evaluated.* The argument had its specious features; by 1947 the F-86 had been successfully flown and several thousands of pounds of data on the nature of wing sweep had been accumulated. But a decision was delayed, nonetheless.

During the fall of 1947, Grumman proposed the development of a carrier fighter with swept leading edges. In December, the Navy agreed to purchase an engineering study, design data, and drawings. Designated XF10F-1, the aircraft was described as an intensive redesign of the basic XF9F-2, incorporating a delta-type wing and swept back

*Bureau of Aeronautics Scientific Historical Report, "Initial Steps in Development of XF9F-1, XF3D-1, and XF10F-1 Aircraft," 12 December 1955. The basic D-558 design was a straight-wing turbojet. The Model II ultimately added wing sweep and rocket power.

tail, and powered by an Americanized version of the British Rolls Royce Nene engine.

While XF10F development still was in its early phase, wind tunnel tests at Langley indicated that the aircraft would have highly undesirable flight characteristics both at high speeds and at the relatively low speeds required for carrier landing. Grumman reacted by proposing an alternate configuration with swept rather than delta wings. The basic aircraft was subsequently enlarged to accommodate fuel for a combat radius of roughly 650 nautical miles -- about double that of the original proposal. Provisions for a variable incidence wing were also incorporated to permit a high angle of attack during carrier takeoff and landing without putting the fuselage nose so high as to interfere with the pilot's vision. By January 1949, when the detailed specification for the aircraft was completed, the maximum speed requirement had increased from 580 to 636 knots and the takeoff gross weight from 21,000 pounds to 26,000 pounds. Changes required by the mockup board in April 1949 further increased the aircraft weight to more than 29,000 pounds.

The Navy did not look with great enthusiasm on the prospect of operating so heavy an aircraft from existing carriers. At low approach speeds it would have a high sink rate and marginal controllability, and the higher launch and landing speeds needed to overcome such shortcomings portended new problems equally grave. Grumman therefore undertook a new design, discarding variable incidence in favor of variable sweep. The change received Navy approval in December 1950. Although the wing-sweep device had hydraulic rather than electrical actuation, the mechanism of the XF10F-1 closely resembled that of the X-5. In both cases the wing was shifted fore and aft along the fuselage as wing angle changed. But the X-5 wing could be set at any angle between its two extremes, while the XF10F-1 was intended to take off, land, and perhaps cruise with its wing very nearly at right angles to the fuselage, sweeping its wing to a full 42.5 degrees for combat operations. Changes incident to and including the incorporation of the wing-sweep mechanism caused a further growth in the weight of the aircraft by

2,200 pounds. In partial compensation, a Westinghouse XJ40-8 engine was specified for the aircraft; it provided a theoretical maximum power of 10,900 pounds of sea level static thrust. One further change of some consequence was included in early 1951. Prompted in part by a desire to overcome the stabilizer ineffectiveness of several earlier jet aircraft, Grumman incorporated a delta-form horizontal tail balanced atop the fin. The incidence angle of the stabilizer was controlled by a canard placed forward of the main surface. In overhead view the delta tail looked like nothing so much as a miniaturized B-70 and the canard served roughly the same function for both.

The Navy ordered a total of 112 F10F-1 aircraft in the course of fleet expansion that accompanied the Korean War, but construction of the production version was postponed pending the outcome of early flight trials. Unanticipated difficulties in the development program delayed the first flight of the prototype until 19 May 1952. By the end of July, the aircraft had made 17 flights, had reached a maximum speed of Mach .8, and had been flown with wings both straight and swept. Again, wing sweeping had almost precisely the predicted effect on flight characteristics. But chiefly because the delta tail was ineffective at both extremes of the flight range, the aircraft had very poor directional stability. All attempts to correct the shortcomings of the original tail design proved futile, so in April 1953 Grumman and the NACA gave up and installed a conventional power-booster horizontal stabilizer.

For nearly a year uncertainty about a final configuration made it impractical to undertake production of the aircraft. Moreover, during the same period development of the J40 engine fell badly behind schedule. Concurrently, other aircraft that did not have the performance potential of the XF10F-1 but which had encountered less difficulty in development were gaining favor. Early in April 1953, the Navy canceled plans for a large-scale production program, and in June of that year, the contract for the 12 aircraft needed to complete development flight testing was also terminated. Further work on the XF10F-1 was disallowed shortly thereafter. The existing prototypes were destroyed.*

*"Narrative History of the Requirement and Development of the F10F General Purpose Fighter," U.S. Navy, BuAer, July 1957.

Apart from its unique and thoroughly unsatisfactory tail assembly, the XF10F-1 embodied several novel features not previously used in conjunction with variable sweep. Swivel-base pylons that hung from the wing permitted the bomb racks to be aligned with the center line of the airplane as the sweep angle was changed.* Although small aerodynamic ailerons were provided outboard on the wings, they were not power boosted and lateral control was initially dependent upon a series of eight paddle spoilers located in a slot in the wing immediately forward of the Fowler-type landing flaps. The spoilers, which extended 180 degrees above and below the wing, were intended to substitute for power-boosted controls. But the spoilers induced a pronounced flutter almost every time they were used. The addition of a power boost to the control system did no more than delay the onset of flutter. Because of the dangers involved, the system was finally disconnected and the ailerons were used for lateral control. Being originally intended to provide stick feel for the pilot and control augmentation when the wings were straight out, the ailerons were undersized for the airplane. So was the rudder. The faults of the horizontal stabilizer have been mentioned. Together, all these shortcomings made the airplane singularly unresponsive to the pilot's will.

Wind tunnel and simulator tests had early suggested that the lateral directional flight characteristics of the aircraft would be poor but nobody connected with the program really believed the prediction. In flight, longitudinal maneuvering qualities were reasonably good with the wings straight out and the aircraft flying at or near its cruising speed. But when the wing was swept back, stick forces increased by a factor of three, partly because of rudder ineffectiveness at high speeds. Yet, despite all these defects, the aircraft had a landing speed 20 knots lower than contemporary swept-wing fighters of the same weight and size and could be flown at a speed much closer to stall than any other swept-wing fighter. With allowances for control system inadequacies and the limitations of an undersized

*Insistence that stores could not be hung from the wing was one of the principal Aircraft Laboratory objections to Bell's original variable-sweep proposal.

engine, performance during cruise flight, with the wing swept back, fully satisfied expectations.*

Relatively little was made of it at the time, but the XF10F-1 also featured a fuel transfer system that controlled the shift of center of gravity during flight.** Kelly Johnson of Lockheed, who also held one of the early variable wing-sweep patents, had hit upon the idea of automatic fuel sequencing for balance in 1945, almost as soon as the center of gravity shift characteristic of swept-wing aircraft at increasingly high speeds had been noted by designers. Adoption of the expedient in the XF10F-1 represented another step toward solution of the major problems of stability and control encountered during trials of the first variable-sweep aircraft.

Although much closer to an operational configuration than the X-5, the XF10F-1 employed essentially the same principle of combined wing-sweep variations and fore and aft wing translation along the fuselage. It suffered from many of the same defects, complicated by a very unique tail configuration that on the whole proved less than satisfactory. Nevertheless, and with consideration of the fact that both were essentially research aircraft, the X-5 and XF10F-1 demonstrated that it was possible to maintain a reasonable degree of longitudinal stability even though center of gravity and center of lift relationships changed markedly as the wings were swept. That was a considerable achievement for the time.

Unfortunately, several disagreeable flight characteristics that really had nothing to do with their swing wing features gave both the X-5 and the XF10F-1 bad reputations. Additionally, by the time the X-5 and XF10F-1 had begun to return meaningful flight test findings, the requirements that had justified their construction were no longer valid. Acceptable compensations for the undesirable effects of wing sweep had been found elsewhere in aerodynamic theory. The Navy took advantage of

* C. H. Meyer, "Flight Testing of a Variable Sweep Wing Aircraft," AGARD Report 439, April 1963; letter, Meyer to author, 25 August 1966.

** C. L. Johnson, Patent No. 2,557,438, filed 18 June 1945.

the same technical advances and, by combining them with angled flight decks and significant improvements in steam catapult efficiency, acquired a family of swept-wing fighters and attack aircraft capable of operating satisfactorily from available carriers. Longer runways and more powerful engines did as much for the Air Force.

In large part because of the changing nature of the requirements, but also because of the rather poor records of the test aircraft, affection for the variable-sweep concept diminished in the United States after 1952 and the research investment decreased accordingly. During the preceding six years, progress past the level reached during World War II had been rather substantial. But substitute means of performing the functions for which variable-sweep aircraft were most suitable had been developed, and on balance the alternatives were more attractive. Flight stability difficulties seemed to be inherent in the concept, while the weight, bulk, and necessary positioning of the variable-sweep mechanisms generally offset any theoretical advantages of the installed systems.

It is probable that further research into the principles of swing-wing aerodynamics would have led, in time, to a configuration that did not have the undesirable flight characteristics of the X-5 and the XF10F-1. But alternate means of compensating for the shortcomings of swept wings had been found, and if they were in some respects cumbersome and troublesome, they nevertheless served a purpose. Characteristically, the engineering fraternity and military planners favored gradual modification and improvement of what was available to investment in something newer and riskier. Perhaps the main reason, in the case of variable sweep, was that the feasibility demonstration had not been successful. At the least, it had not been interpreted as a successful demonstration, and that, in the end, was all that mattered. Even though the aerodynamics of variable sweep had been worked out and shown to be sound, objections to the mechanism were sufficiently great to discourage further investment.

BRITISH EFFORT

Although the United States had been more attentive to the promise of variable sweep than any other nation, and had invested most heavily

in its exploration, the British had not by any means neglected the field. At least two British aircraft manufacturers, the Bristol Airplane Company and the Armstrong Whitworth Company, did some work on the concept in the late 1940s. Dr. A. E. Russell, Chief Designer for Bristol, proposed the development of a variable-sweep aircraft in 1951 but was turned down by the Ministry of Supply. Later that year, R. J. Woods of Bell and John Stack of NACA, both deeply involved in the X-5 project, briefed Armstrong Whitworth personnel on the aerodynamic and structural problems encountered in the course of X-5 development to that point.*

John Stack, NACA's best known aerodynamicist, was well acquainted with British airplane designers. He was particularly friendly with Dr. B. N. Wallis, then Chief Aerodynamicist for Vickers, whose reputation for design ingenuity was, in his own country, comparable to Stack's. Wallis had been one of the first of the English designers to study wing-sweep effects following the European War. His contributions have never been adequately documented, which may explain one of the most striking occurrences of technological chauvinism of this decade, for, by the British, Wallis is widely credited with having invented the swing wing.

Most British designers concerned with problems of supersonic flight early concluded that the delta wing had significant advantages over a fixed-angle swept wing. Wallis was one of those who held firmly to the minority view. He was early intrigued by the swept-wing work R. T. Jones had done at NACA-Langley in the mid-1940s and at about the time of the X-5 project turned his attention to variable sweep. Although having considerably fewer resources than NACA and lacking any substantial support from the British aircraft industry or from his own government, Wallis nevertheless succeeded in designing, constructing, and flying two small radio-controlled aircraft that incorporated simple variable-sweep mechanisms. He called the configuration the Polymorph. The results of his experiments were scientifically interesting but it does

* Interavia, May 1962, p. 618.

not appear that they had any influence on the later course of swing-wing research in England or abroad.

Although the Polymorph experiments fostered the later supposition that the British had muffed an early chance to exploit variable-sweep concepts, Wallis was not the only British airplane designer to work on the problem in this period. Perhaps the most elaborate variable-geometry proposal of the postwar era was that of L. E. Baynes, an Associate Fellow of the Royal Aeronautical Society and a noted glider designer, who began serious study of sweep variation effects in early 1947. Two years later, in March 1949, he applied for his first patent. (At about the same time, R. J. Woods of Bell and Kelly Johnson of Lockheed were putting the finishing touches to their own applications for patents on variable-sweep mechanisms.) Baynes, who appreciated the nature of the center of gravity and center of lift shift that occurred as the wing angle was made more acute, devised a solution remarkably different from those proposed elsewhere. He concluded that altering tail and wing angles simultaneously would be an effective way of overcoming the pitch-moment change that occurred as wing sweep increased. His design also provided for variable wing incidence and differential wing sweep. Finally, he proposed adjusting the angle of the hinge axis of the variable-sweep wings to change the dihedral, and making each tail assembly element variable in sweep and incidence. Differential movement of the tail sections would provide for directional control. All of these lift surface or control surface articulations were to be linked to the pilot's steering controls.

In 1949, Baynes submitted the results of some preliminary wind tunnel work to the Ministry of Supply, together with a design for a supersonic twin-jet fighter. Although he was invited to discuss his ideas with the Director of Military Aircraft Research and Development and subsequently with the Royal Aeronautical Establishment, his proposal was treated as too visionary and no funds were forthcoming. In 1956, after an interval in which he had succeeded in securing patents for his concept in Great Britain, the United States, France, and the British Commonwealth, he again submitted fighter design proposals to both the

Ministry of Supply and the United States Navy. Notwithstanding agreement that his approach had a good deal to recommend it, the evaluators concluded that the mechanical devices needed to perform all of the shape-changing functions were too weighty and intricate for practical use.*

Wallis, who had continued to work on swept-wing aerodynamics into the mid-1950s, had earlier come to some highly provocative conclusions regarding the applicability of variable sweep. Because his approach appeared to have both military and commercial potential, Vickers succeeded in obtaining a relatively small sustaining government research contract. By early 1958, Wallis had proceeded to the point at which it was clearly necessary to choose between investing larger resources and appreciably expanding the research, or dropping the project because its promise seemed not worth the cost of further exploitation.

The design he proposed was in its essentials an arrow-wing trailing behind a long slender fuselage. The objection to an arrow-wing aircraft was that at any reasonable touch-down speed, it had such a high angle of attack that its wing tips brushed the ground and its nose section was high in the air. To overcome the problems thus presented and to take advantage of the other attributes of a swing wing, Wallis proposed swinging the outboard sections of the arrow-wing forward for landing and takeoff. Partly because the long narrow wings and the slender cylindrical fuselage made a fuselage-centered pivot undesirable, Wallis located the pivots somewhat outside the fuselage walls, in stub wings. The resulting arrangement of outboard pivot points and small, fixed-wing sections was the source of the legend that Wallis had found the ideal solution to the stability problem that had plagued all earlier variable-sweep designs. He had indeed found a solution, or at least a partial solution, but it does not appear that he fully recognized the aerodynamic significance of the stub-wing, outboard pivot arrangement.

* Letter, L. E. Baynes to the Editor, Flight International, 3 May 1962, p. 217; Baynes, "Variable Sweep for the Extension of the Speed Range," Aeronautics (England), September 1955, pp 60-63.

Reluctant to build a "Swallow" prototype with company funds and unable to obtain government financing, Vickers Armstrong, with the concurrence of the British Government, proposed a development program partly supported by the United States. The evaluations group selected to examine the merits of the British proposal was the Weapons Development Steering Committee, an embodiment of President Eisenhower's desire to coordinate the R&D interests of the Western Allies. John Stack was one of its leading members. In midsummer 1958, Stack and General Albert Boyd, a retired Air Force officer who was one of the most skilled and experienced of American military test pilots, visited Vickers to examine the Wallis "Swallow" design. The British wanted to proceed immediately with the construction of a subscale flying model of a supersonic transport based on "Swallow" design concepts. Stack, who spent a week going over the details with Wallis, frankly told the British that in the climate of the time "we can't sell research aircraft in the United States." But an inter-governmental agreement for shared research was drawn under the terms of which NACA agreed to perform an exhaustive wind tunnel analysis of the potential of the "Swallow." Wallis was present at Langley during much of the test period.

One of the unique features of the "Swallow" was the placement of its engines. Two were attached to the outboard sections of each wing by swiveling pylons, one above and one below the main wing surface. Wallis had demonstrated to his own satisfaction, on small wind tunnel models, that the vectored thrust of these swiveling engines provided fully adequate three-axis control for the aircraft. He also contended that even should all four engines be shut down, the aerodynamic effect of changing the angle at which the long engine cells were presented to the air stream was sufficient to maintain controllability. But the necessity of yawing the engines with respect to the longitudinal axis of the aircraft and pitching them to provide control of the angle of attack created such a disruption of airflow that the lift-to-drag ratio obtained in the wind tunnel was but half of what predicted by Wallis. Moreover, at relatively low angles of attack in the high-sweep condition and at moderate angles of attack in a low-sweep attitude, the "Swallow" configuration proved to be longitudinally unstable. Finally, it appeared

that engine pod deflection would not insure the control responsiveness needed for normal flight maneuvers even when the engines were operating, while the possibility of controlling the aircraft in the event of an engine failure was dishearteningly low. Moving the engine pods closer to the fuselage tended to reduce longitudinal instability but further lessened control effectiveness. In the words of one of the principal American researchers who evaluated the "Swallow" design, there seemed to be "...major problems in making the 'Swallow' planform operational."*

In 1959 the results of the "Swallow" evaluation were reported to the British and, for practical purposes, further work on the configuration was discontinued. But when word of renewed United States progress in the refinement of variable-sweep concepts later reached England, the British press concluded that it was entirely an outgrowth of the "Swallow" design evaluation. Most Americans were quite unaware of the legend and its widespread acceptance. There appears to have been no open American publication of the "Swallow" findings before 1962, and then only in the middle sections of a United States patent.** No full statement of the events of the "Swallow" affair appeared in England at all, though informal accounts seem to have had widespread circulation. The legend formed thereby persisted -- as witness the following extract from The Economist in March 1965:***

The pivoting system was dreamed up here in Britain by Dr. Barnes Wallis, rejected here as impractical and expensive, and subsequently taken up by American government research agencies where the pivot now in use was developed and handed over to Boeing and General Dynamics when they bid for the F-111 contract.

* F. C. Polhamus in U.S. Patent 3,053,484, issued to Polhamus and W. J. Alford, Jr. (NASA), 11 September 1962 (application dated 7 July 1960). See also, Stack interviews, 23 August, 30 August 1966; "Two Wings in One," Interavia, May 1962; D. Sawers letter to the author, 20 March 1966. Some additional information was provided by various British Aircraft Corporation aerodynamicists in conversations with the author, April 1967.

** Alford and Polhamus, Patent 3,053,484, 11 September 1962.

*** The Economist (London), 20 March 1965, p. 1241. See also Flight International, 8 February 1962, p. 208: "The variable geometry wing, evolved by NASA's Langley Research Center from the original concept of Dr. Barnes Wallis...."

The legend was substantially at variance with the facts. That tests of the "Swallow" design in an American wind tunnel may have stimulated interest in some of its features seems undeniable, but the principal elements of the "Swallow" on which Wallis and his fellows rested their best hopes were precisely those rejected as impractical. It is no more than justice to acknowledge, however, that both the Baynes and the Wallis design approaches were ingenious attacks on the known deficiencies of variable-sweep aircraft. But in a manner faintly reminiscent of American experience with the XF10F, each was encumbered with so many design novelties that the prospective advantages of variable-wing sweep were obscured. Had the British honored Sir Robert Watson-Watt's advice to ruthlessly sacrifice "all refinements, elegancies, and versatilities" in favor of concentrating on "something to be going on with,"* there might have been another sort of outcome. But they did not, and there was not.

In the case of the Wallis "Swallow" design it appears that a reasonable number of influential supporters had been found, that the need for a feasibility demonstration was properly acknowledged, and that some of the design concepts embodied in the "Swallow" proposal were entirely valid. The principal defect of the Wallis design was aerodynamic instability, and that defect was pointed out to the Vickers design group by Farnborough (Royal Aircraft Establishment) scientists even before the Americans were called in. Farnborough was, however, notorious among British designers as a forum of aerodynamic conservatism. Whether the assumption was justified or not is of no moment; it seems to have been exaggerated, at best. More important, Wallis did not assign a high value to the Farnborough evaluation, perhaps partly because, like many inventors, he had a deep emotional commitment to his conception.** In this instance, the commitment colored judgment.

* Watson-Watt, quoted in L. N. Ridenour (ed.), Radar System Engineering, Volume I (M.I.T. Radiation Laboratory Series, Cambridge), p. 176.

** John Northrop's fondness for the all-wing aircraft and the dedication of Lippisch to the delta wing are pertinent examples of other such design commitments.

A second factor of some importance was the prototype tradition in Britain. The definitive analysis of the Wallis "Swallow" proposal was performed by scientists using wind tunnel tests that might reasonably have been conducted in England. Wallis was convinced that a flying prototype aircraft should be built to test "Swallow" design concepts, an approach that would have been both costly and lengthy. And, as matters turned out, futile too. The basic defect was that although Wallis' group had a keen appreciation of the need for a feasibility demonstration, there was insufficient consideration of the need for concentrating on fundamentals. What was needed was information that would lead to a workable variable-sweep aircraft rather than a prototype of a complex aircraft system. That the prototype approach was customary in Britain had no bearing on the main question, yet it became a dominant consideration.

Finally, later events made it clear that the basic "Swallow" configuration could not be adequately stabilized without some sort of rudder-elevator combination. Whittle had come to much the same point when he ran aground on combustion section difficulties. Whittle did not insist on his original design concept but accommodated to engineering realities. The "Swallow," the X-5, and the F10F-1 represent instances in which too much homage was paid to rigid design approaches and nonessential details when what was required was a demonstration of basic feasibility. Given a reasonably convincing demonstration that variable sweep might fit any of several applications -- that a requirement could be phrased on the strength of demonstrated technical feasibility -- the outcome could well have been quite different.

LATER U.S. EFFORT

All the meaningful research results that could be squeezed from the two X-5s had been obtained, recorded, and fully analyzed by 1953. One of the two was destroyed in October of that year, but the other was used as a chase aircraft until 1955 before being retired to a museum.

In some respects the fact that for two years the surviving X-5 served a highly useful role as a chase aircraft might have been taken as a favorable omen for the future of variable sweep. But a requirement that would justify further experimentation was lacking and the dominant problem associated with variable sweep had not yet been resolved. Notwithstanding all of the theoretical and applicational research conducted since 1942, no simple method of controlling the longitudinal stability variation as wing-sweep angle changed had been developed. And interest had largely lapsed. By 1953, none of the major aircraft manufacturers had any optimism about the future of the concept; Bell had proposed several versions of a tactical fighter based on the X-5 in the early 1950s, but disinterest on the part of the Air Force discouraged further research, while Grumman had discontinued work when it became apparent that the adoption of steam catapults and angled flight decks would allow a new generation of high performance fixed-sweep aircraft to operate effectively from carriers.

Between 1953 and 1957 further progress was made in improving the aerodynamic characteristics of aircraft with highly swept wings. The appearance of operational supersonic aircraft in those years and the absolute necessity of taming their flight behavior led to the addition of sophisticated slats and slots to swept wings, brought on significant improvements in wing flow control devices, and fostered a general solution to the problem of finding an appropriate location for the horizontal tail, one that would minimize the occasional longitudinal instability of aircraft with highly swept wings. By 1957 it was becoming clear that multi-mission aircraft capable of operating efficiently throughout the speed range from Mach 0.8 to something in excess of Mach 2.0 could well be the next essential requirement of the military. NACA-Langley, which had never entirely dropped its inquiry into variable-sweep aerodynamics, stepped up the tempo of experimentation. The Langley viewpoint was epitomized by John Stack, who interrupted an early briefing to remark, "You don't have to sell me on the advantages of a variable-sweep airplane. What I'm interested in is how to make it work."*

*Stack interview, 30 August 1966.

convinced that a workable solution to the longitudinal stability problem could be found and that when found it would be applicable to a variety of important operational requirements, even though the texture of the requirements might still be uncertain.

Spurred on by Stack's enthusiasm and by some encouraging early findings, a team of Langley aerodynamicists headed by Thomas A. Toll energetically undertook the analysis of an ever broader spectrum of wing-fuselage arrangements that seemed to hold special promise. Part of the work clearly was prompted by interest in the "Swallow" design. In early 1958, systematic wind tunnel and analytical studies using the 7 by 10 foot high speed tunnel at the Langley research center focused attention on one concept. By shifting the pivot point outboard of the main fuselage and moving it aft of its "normal" position, researchers were able to reduce the relative size of the movable wing section while still providing an adequate span when the wing was swept forward. (In all cases either a canard or a conventional stabilizer was part of the design.) Because only a portion of the wing lift surface rotated to the rear, while a significantly large lifting surface remained fixed, the shift in center of lift resulting from rearward motion of the wing was relatively slight. Moreover, as the angle of wing sweep grew sharper, the proportion of total lift provided by the fixed "glove" that housed the outboard wing pivot increased considerably, further counteracting the rearward shift in center of lift brought on by rotating the outer section of the wing. The increase in lift from the fixed "stub" resulted from the naturally greater angle of attack that occurred when the movable wing sections were swept toward the rear. Indeed, the researchers discovered that by juggling the proportion of movable panel to fixed stub they could actually reverse the direction in which center of lift moved with increasing sweep.

As a consequence of shifting the pivot outboard of the fuselage, the rapid and undesirable increase in stability that had characterized earlier inboard-pivot swept configurations was overcome. (Because of the relationship between center of lift and center of gravity, an increase in stability at high speeds meant a decrease in controllability.)

Indeed, at higher sweep angles a slight reduction in stability occurred -- with a consequent improvement in controllability. In essence then, it appeared that maneuverability deficiencies characteristic of the earlier variable-sweep aircraft would be generally overcome if the wing pivot were placed outboard of the fuselage. And of course the combination of a stub or glove wing plus an outboard pivot eliminated the need for fore and aft translation of the wing.

By late 1958, at about the time the NACA-Langley establishment became part of the National Aeronautics and Space Administration, work on configuration refinement had become intensive, quickly expanding from the original problem of finding a generally suitable configuration to the larger task of refining and translating into specific designs the information obtained earlier. By early 1959 there was conclusive evidence that the major obstacles to the successful employment of variable sweep either had been overcome or clearly were vulnerable. While refinement and design efforts were continued, the effort was again expanded to include research on sweep mechanisms. Although that aspect of the work continued for nearly two years, it early became apparent that there were several quite feasible ways of putting together a relatively simple sweep mechanism that satisfied both aerodynamic and structural requirements. For that matter, nothing much was wrong with the arrangement Wallis had proposed for rotating the outboard panels of the "Swallow" wings.

Two related but dissimilar aircraft configurations emerged from the preliminary studies at Langley. One, the work of E. C. Polhamus and W. J. Alford, Jr., embodied a conventional fuselage and empennage. Its moveable wing sections were attached to mid-fuselage stubs that made up about 20 percent of the total wing area. The other design, largely the conception of T. A. Toll, was based on a canard-stabilized fuselage with relatively large fixed wing panels extending from the rear half of the main body. Toll provided moveable wing sections pivoted well outboard of the fuselage and accounting for about half of the total lifting surface. The fixed stub wings designed by Toll were large enough to carry engine pylons. A great variety of

arrangements that did not prove so attractive were also studied in considerable detail. By early 1959 there was general agreement that either the aft-tail or the canard layout could be suitably tailored to specialized performance requirements.* Either suitably compensated for the stability defects of all the earlier variable-sweep aircraft designs.

Convinced that NASA-Langley had at last solved the central problem, Stack began actively seeking an appropriate application. He still was opposed to the idea of investing in another research airplane program, perhaps because he could see no hope of obtaining the substantial funds that would be needed but also because he did not believe that feasibility need be demonstrated by such an expensive process.

As one of the consultants frequently called upon by the Air Force to advise on weapons requirements, Stack knew that the Tactical Air Command had been unable to define its needs for an airplane to succeed the F-105 in the inventory. TAC, much influenced by the strategic concepts of the late 1950s, wanted a nuclear-capable fighter able to fly either the Atlantic or the Pacific unrefuelled, able to operate at low supersonic speeds at minimum altitudes, and also capable of flying air superiority missions at high altitudes and speeds above Mach 2.0. TAC also hoped to operate from dispersed European airfields, which meant a short-takeoff capability, although how short was an unanswered question.

Industry was confident of being able to satisfy the principal mission requirements, but only by an aircraft weighing upwards of 50 tons and thus incapable of operating from small, badly surfaced European airstrips. TAC wanted gross weight held to 20 to 25 tons,

* Polhamus and Alford Patent 3,053,484, 11 September 1962; T. A. Toll, Patent 3,064,928, granted 20 November 1962, application dated 23 August 1960; Toll, Polhamus, and Aiken, pp. 5-7; Baals and Polhamus, 10-14; Stack testimony in TFX Contract Investigation, I, pp. 12-13; Interavia, May 1962, p. 618; Stack interview, 30 August 1966; letter Polhamus to Jack Vogel. RAND, 27 October 1966.

implying quite another breed of aircraft. Design adjustments tended to limit the size of the aircraft by compromising the range, speed, or payload, alternatives that TAC refused to accept.

While matters were at that impasse, John Stack drove across Langley Air Force Base from the NASA laboratories to TAC headquarters, preliminary findings on the variable-sweep investigation in hand. He suggested to General F. K. Everest, the TAC commander, that the really crucial parts of the TAC fighter requirement could be satisfied by a variable-sweep aircraft built along the lines Polhamus and Alford were proposing. Although TAC would have to give a little on the weight, since 30 to 35 tons seemed the least that could be expected, all other specifications could probably be met -- including the short-field-operation requirement.

While Everest considered the NASA suggestion, Langley advised the Navy of its recent findings. Recognizing the possibility that variable-sweep might permit the development of a long-loiter supersonic fighter, the Navy contracted with North American and Douglas for detailed analyses of the concept. Boeing, which had for some years been very attentive to Langley's aerodynamic research, independently began considering how to apply variable-sweep to new aircraft. A series of preliminary studies confirmed Stack's contentions, so Boeing in late 1959 informally proposed the development of a variable-sweep fighter to serve TAC needs. Immediately thereafter, TAC headquarters, with assistance from NASA-Langley, began putting together a formal requirements statement that could be passed on to industry.*

Air Force headquarters and the Air Research and Development Command had to be convinced of the feasibility of using variable-sweep wing on a new fighter before the formal requirement could be approved. Aerodynamicists at Wright Field, in some respects influenced by the residue of X-5 and XF10F-1 experience, did not at first think

* Stack testimony, TFX Contract Investigation, I; Stack interviews, August 1966; E. C. Wells testimony, TFX Contract Investigation, IV, pp. 925-926.

the 1959 variable-sweep technology any more attractive than the approach that had been given up as hopeless seven years before. Challenged to provide more convincing evidence for their case, the research group at Langley designed and constructed four additional wind-tunnel models (each representing a slightly different version of the Polhamus-Aiken configuration), pushed them rapidly through three phases of aerodynamic analysis, and emerged with data on which objections from the Air Force technical cadre immediately foundered.

The next problem was to induce Air Force headquarters to ratify the formal requirements statement that TAC had composed. Not until February 1960 did General Everest succeed. The original System Development Requirement was followed, in July 1960, by System Operational Requirement Number 183 -- which officially committed the Air Force to what was now being called the TFX project. Although neither document specifically called out variable-sweep as a requirement of the proposed system, it was quite clear that the required performance would be dependent on recourse to that technique or another having similarly pronounced effects on flight characteristics.*

Development of the TFX did not immediately follow issuance of the operational requirement but was delayed by an effort to find a single design that would satisfy both Navy and TAC requirements. In June 1961 a bi-service requirement was defined, in January 1962 the prospective developers were reduced to two, and in December 1962 General Dynamics formally contracted to develop and build the F-111, as the fighter was designated. All of the configurations considered during the period of competition incorporated the essentials of the NASA-Langley variable-sweep design.**

By the time the F-111 program had begun, the validity of the new variable-sweep concepts was being widely accepted. Some of the

* Stack testimony, TFX Contract Investigation, I, pp. 13-14, 21, 36-37; Baals and Polhamus, 14-15; Interavia, May 1962, pp. 618-619; Stack interview, 30 August 1966; Polhamus letter to Jack Vogel, RAND, 27 October 1966.

** TFX Contract Investigation, passim.

early proposals for further application came to nothing, but by 1967 it was clear that both the French and the British were committed to developing and employing variable-sweep fighters other than the export F-111s, that the Russians had swing-wing aircraft, that a joint United States-West German variable-geometry fighter project was continuing, and that the Boeing supersonic transport would rely on variable sweep. All used the outboard pivot design originated by NASA-Langley.

EVALUATION

Perhaps the most provocative characteristic of the 1957-1962 transition from concept to development was that it occurred without resort to either research vehicles or prototypes of the classical sort. Feasibility was explored and demonstrated by wind tunnel experiments and their data products. In its final phases, once the correct principles had been established, the cost of wind tunnel research apparently came to under \$1 million. The cost of the earlier work with the X-5 and XF10F-1 was at least \$10 million.

For variable sweep, it is evident that the crucial event in the innovative process was the conjunction of a convincing feasibility demonstration with an urgent requirement, one that sweep-angle variation admirably satisfied. Requirements for variable sweep had been expressed at intervals from 1948 onward, but the technology had not been up to what was asked of it. And none of the variable-sweep arrangements suggested before 1958 satisfied current needs as well as did the various technical alternatives that made a swept-wing as tractable as an unswept wing.

Apart from general interest in improving the performance of aircraft, the NASA-Langley researchers seem to have had no particular applications in mind when they began the final phase of experimentation that led to the Polhamus-Alford configuration ultimately adopted. These circumstances suggest that a rather high degree of technical assurance had to be provided before variable sweep became attractive

to prospective users and that the eventual requirement was more directly influenced by the progress of research than by abstract notions of what was needed in the inventory. The requirement, then, can best be stated once the technology is reasonably well in hand. And "well in hand" appears to be another way of expressing the notion that a convincing demonstration of technical feasibility should precede any effort to apply the technology. Indeed, if the experience of the variable-sweep wing and the turbojet engine are at all representative, the military authorities who compose formal requirements cannot ordinarily be induced to take an innovation seriously until a feasibility demonstration has been carried through successfully.

The ability to perform a given function more effectively than can any alternative device or procedure -- or clear evidence of a potential for such performance -- is a dominant element in any feasibility demonstration. Here there is an interesting difference between variable sweep and the turbojet engine when both are treated as innovations. For in the case of the turbojet, the early feasibility demonstrations never quite succeeded in showing that a jet-propelled aircraft could actually fly or fight better than contemporary aircraft fitted with reciprocating engines, yet the potential was plain and the technical obstacles seemed relatively slight, so formal requirements for turbojet aircraft were approved. When feasibility demonstration was first attempted for variable sweep, in the X-5 and XF10F-1, the advantages of the technique were apparent (in crucial flight areas both aircraft performed better than conventional contemporaries) but the evidence also indicated that other, less costly, and less risky means of achieving the same sorts of performance gains could be provided. Not until variable sweep demonstrated a potential that other devices could not match did an appropriate requirement emerge. Mere expression of a requirement without much regard for the probability of satisfying it would seem to have no particular influence on the progress of technology. A requirements statement can, of course, encourage greater investment in a given area of research and development, but in the cases here examined,

investment alone does not appear to have been a decisive influence. Once the basic technological concepts have been shown to be sound, on the other hand, the investment of additional resources in research and development can be justified because the probability of a favorable outcome has improved.

IV. SOME CONCLUDING OBSERVATIONS

The process by which both variable sweep and the turbojet engine were carried from idea through general acceptance conforms reasonably well to the three-phase pattern observed in civil innovations. Although the interdependence of the sequential steps appears to be somewhat more pronounced than has been suggested for a free market case,* that does not seem to be particularly important. If there is a significant departure from the standard where innovation has obvious or immediate military applications, it occurs during the stage identified as "proof" or "demonstration of feasibility." Perceived urgency has a pronounced effect on the way in which a demonstration of technical feasibility is interpreted by those interested in applying the invention to military needs. Even when the uncertainty of application is relatively high, or when the "proof" of technical feasibility is relatively slender, an intangible that might be called prospective national need causes decisionmaking authorities to push the innovative process along quite rapidly. When there is a lesser perception of national need, as in the case of variable-sweep aircraft in the 1950s, a much more convincing demonstration of technical feasibility is demanded or, alternatively, a much plainer and more certain finding that the innovation will satisfy a "valid military requirement."

It seems clear that the reciprocity of innovation and requirement is unusually important during the period when demonstrations of feasibility are being attempted. The character of an innovation ordinarily influences the form of the "requirement" that authorized further development, but it does not appear that a statement of requirement influences the course of innovation unless the "requirement" has first been reshaped to reflect the technological implications of the innovation. Within the limits imposed by knowledge (the state of the art constraint),

* R. E. Johnson, "Technological Progress and Innovation," Oxford Economic Papers, XVIII, July 1966; J. A. Schumpeter, The Theory of Economic Development (Harvard University Press, Cambridge, 1949).

a statement of requirement can induce performance improvements -- as witness the remarkable advances in conventional propulsion and aerodynamics during the Second World War. But until a demonstration of capability has been carried through, there seems to be no way of anticipating the prospective applications of something essentially novel. A requirements statement specifying product characteristics rather than performance goals tends to have slight applicability to devices that qualify as innovations. There probably are exceptions, but as a general rule it may be assumed that the ordinary military requirement is not likely to be satisfied if it calls out innovative technology still to be demonstrated.

In each of the examples treated here, the "concept" or "invention" stage of the innovative process occurred outside the main stream of aerodynamics and propulsion. The inventors usually had no clear perception of prospective applications. Whittle and Ohain were interested in the jet engine of itself rather than in its ultimate use. Lippisch seems to have thought of variable sweep as a device for moderating the high landing speeds of swept-wing aircraft, an aerodynamic novelty, but to have been much more concerned with delta-shape aircraft. Baynes and Wallis attempted to apply to a complex aircraft form technology that had been imperfectly worked out. The Bell group was concerned more with establishing a requirement for an unproven device than with basic principles. Grumman tried to satisfy an existing requirement by whatever means seemed feasible. The only early researchers who focused on the problem of perfecting the technology before seeking out an application were the NACA scientists at Ames and Langley, and once the initial period of postwar experimentation had passed they were hard pressed to suggest a justification for continuing their work on variable sweep.

It may be asking too much of an inventor or a research group to require a clear statement of probable benefit covering a device of uncertain feasibility. The function of an inventor is to invent, and of a research laboratory to do research. But there is no obvious justification for failure to evaluate prospective applications and

their worth, once basic principles have been established, and to explore feasibility questions as they are raised. The difficulty seems to lie in a tendency to concentrate on evaluations of the feasibility of satisfying existing requirements that were generated without regard for the existence or pending availability of radically new technology. If there is no sure way of anticipating the technical feasibility of an innovation that is in the conceptual stage, there is no instrument -- other than sheer chance -- for matching it with a requirement. If, as is often the case, adoption of the innovation will cause changes in a great many operational or logistical arrangements, it is not even reasonable to state a specific requirement until quite a lot is known about the prospective implications. In the case of the turbojet engine, for example, thrust potential, fuel consumption, and durability characteristics had to be understood before there was any real prospect of discovering what military functions jet-powered aircraft would best serve. And these elements of knowledge had to be drawn from experimental evidence -- a demonstrator engine. For variable sweep it was nearly pointless to specify an operational application, much less a detailed design, until the center of gravity and center of pressure shift problem had been dealt with. That the solution would ultimately be found in aerodynamic research rather than mechanical engineering could scarcely have been predicted at the time the first flight experiments were conducted. What might have been anticipated, however, was that a feasibility demonstration involving reliance on a great many uncertain items distinct from the variable-sweep device had little chance of being interpreted as "satisfactory" unless most of the non-relevant items also operated in accordance with predictions.

Until feasibility had been convincingly demonstrated, most of the institutions, civil and military, that might have exploited variable sweep or the turbojet could not be diverted from their preoccupation with marginal, evolutionary improvements in the sorts of mechanisms they were familiar with. And that appears to be a common response. Auditory means of detecting the approach of intruding aircraft were favored over radar for several years after the listening devices had

become outmoded. Before 1938 only one large aircraft manufacturer (in France) made any effort to develop a helicopter, although all of the necessary principles had been established at least a decade earlier; as with jet engines, the principal research was conducted by isolated individuals and such unlikely sponsors as a marine engineering company. One of the classic cases of institutional resistance to a new concept involved continuous-aim firing from naval vessels; its adoption by the United States Navy was delayed until its chief advocate had taken the extreme step of going directly to the President of the United States.*

Inventions that could affect the equipment patterns of the military generally encounter greater resistance than inventions that can be enfolded into the establishment without disruptive consequences.** The causes are perhaps as much sociological as economic in origin, and they are not unique to military establishments of course. Commercial organizations are at least as averse as the military to making important objectives dependent on the outcome of very uncertain research programs and they seem at least as incapable of distinguishing between various degrees of development risk. The predictability of invention is notoriously difficult and the predictability of success in exploratory development most uncertain. Having particular familiarity with the technical area the invention will affect does not seem to improve prospects of successful prediction, as witness the general inaccuracy of "experts" in estimating the probability that a workable turbojet engine could be developed within a reasonable period of time.

The military should not be faulted for failing to anticipate the value of a new concept when its inventors can not, as is often the case. But if a newly conceived device has apparent military potential,

* E. E. Morison, "A Case Study of Innovation," Engineering and Science Monthly, April 1950.

** The reasons, as previously mentioned, include: institutional inertia; a preference for marginal improvements when the alternative involves abandonment of the familiar; a tendency to treat innovative devices as more risky than almost any less extreme alternative; and a general reluctance to disturb the status quo.

even if it is vague or uncertain, the military might well invest modestly in testing the technical feasibility of the concept or invention. The only object should be to discover whether the device is technically feasible, and to do so as cheaply as possible. Such an investment may not be made, of course, until the device approaches a status that permits feasibility testing. A formal evaluative model of the sort earlier suggested, akin to the classical investment equation, would appear to have considerable utility in making the decision to proceed to feasibility testing or, after a feasibility demonstration, to proceed with development. To insist that a formal requirement be contrived and approved at that stage is foolish.

Demonstration of feasibility has an immediate relationship to military requirements. In the case of the jet engine, once a demonstrator has been more or less successfully operated, means of employing it were quickly identified. In a sense, requirements were composed to accommodate the innovation. The central point is that the early turbo-jet engine demonstrations permitted a dispassionate appraisal of the military applications of the engine. Known and anticipated defects of the prototype engines were extensively investigated by research establishments, the original developers, and (for the first time) experienced engine manufacturers. The response was eminently logical.

Even while these events were in progress it was possible to consider more or less logically how the innovation meshed with both narrow operational objectives and broad national goals. The Germans decided to put into production an engine they knew to be imperfect and very sensibly concentrated development attention on projects for second-generation jet aircraft. The British moved more slowly toward perfecting the engines scheduled for their early operational jet aircraft, leaving longer term projects for later consideration. The Americans, less hampered by resource limitations than the British or Germans, invested heavily in improving the performance of the original Whittle engine and in newer designs while scheduling relatively large-scale production of the best model then available. In terms of the requirements, each nation recognized that each course promised to satisfy

perceived national needs. Whether such needs were correctly perceived is another matter.

The substance of the variable-sweep case is quite different. From any reasonable viewpoint the Germans had no wartime need for variable sweep. The first nominal statement of requirements was conjured up by Bell to support a proposal to build 24 variable-sweep interceptors for service test purposes. Although the requirement may have been valid in the abstract, the way that Bell proposed to satisfy it was not. The Navy requirement that led to the XF10F project had a high degree of initial validity, though not necessarily for variable sweep. It withered away over time, partly because the total XF10F design was of dubious worth, partly because improvements in the handling characteristics of fixed-sweep fighters made them eligible for carrier assignments, and partly because the Navy's adoption of angled flight decks and high-pressure steam catapults equated with the advantages claimed for a variable-sweep fighter. Given the expected cost, the time needed to solve technical problems, and the relatively high risk of project failure, the alternatives to variable sweep had to be assigned a higher expected value than the main program.

The pattern of investment in early work on turbojet engines and variable-sweep aircraft is very interesting. Both Whittle and Ohain carried their engines through the preliminary demonstration stages at costs that certainly did not exceed \$75,000* each. Lockheed, with what in retrospect seems to have been a reasonably attractive concept, invested \$25,000* in evaluative design and then spent two years trying to induce the government to fund a \$1 million feasibility demonstration. Virtually all of the early variable-sweep aircraft proposals, including both the X-5 and the "Swallow," were intended to lead into the production of specific aircraft. Probably \$10,000,000 was spent on the X-5 and XF10F. NASA-Langley, however, apparently spent less than \$1,000,000 in working out and demonstrating the feasibility of the stub-wing, outboard-pivot approach, largely ignoring the question of a specific

*In 1940 dollars.

application until the central questions of aerodynamics had been answered.

The importance of early and cheap demonstrations of the technical feasibility of novel devices is apparent in a number of other cases -- Watson-Watt and the radio-wave reflection detector, the Heinkel-von Braun experiments with liquid-fuel rocket aircraft, California Institute of Technology's experiments with JATO rockets, even Fermi's fission demonstration at Stagg Field. The experience of the United States Air Force with nuclear propulsion for aircraft, of the German Army with super-caliber rocket guns, and of the German Air Force with a great variety of ineffective anti-bomber weapons put into production late in the war would seem to support the proposition that attempting product development before demonstrating basic technical feasibility can have most unhappy consequences. All of these may have been badly managed programs, or they may not: the matter is of no special importance. What is important is that each program ultimately foundered because technology was not up to the task demanded of it -- but not solely because the state of the art was insufficiently in hand. For some purposes, the state of the art of nuclear propulsion, of rocketry, and of air defense may have been entirely adequate. In effect, in each instance the central difficulty lay in the fact that a demanding requirement had been established well before technical feasibility had been demonstrated and that the requirement itself had no special relevance to the demonstrated capabilities of the various innovative technologies.

These examples suggest that, given a concept or invention that is inherently sound, a demonstration of feasibility can be provided at relatively low cost if the necessity of demonstrating feasibility is not subordinated to some other (and probably improper) objective. Only then is it entirely feasible to evaluate the prospect that a valid requirement for the innovation exists -- or can be induced -- and to proceed accordingly.

It is plain that the interaction of innovation and requirements is not a static or sterile relationship, that it is very susceptible

to outside influences. For each innovation, with consideration of the requirements environment that it appears in or that it creates, there exists a correct balance between research that seeks to determine the feasibility of an innovation and the intensity of the need for it. All the evidence suggests that the most effective way of determining whether a given innovation can either satisfy an existing requirement or justify the issuance of a new requirement is to proceed toward a feasibility demonstration by the quickest and cheapest path that is consistent with the goal of making the demonstration sufficiently convincing and thereafter to trim the requirement to the demonstrated capability of the innovation. The tendency to do precisely that is very prominent during periods of pronounced national stress. A considerable improvement in the effectiveness of the innovative process may be obtainable if such a policy is honored during periods when stresses are less severe or less apparent.

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