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THE AIR FORCE AND OPERATIONS RESEARCH

A COMMENTARY ON I. B. HOLLEY'S PAPER

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PREFATORY NOTE

The basic paper on which the following remarks are a commentary is "Operations Research and the Air Force: A Case History in Doctrine and Organization, 1942-1968," by I. B. Holley, professor of history at Duke University. It is a brief account of the evolution of operations research within the USAF and a critique on that evolution. He concluded that the function had not been fully exploited, perhaps could not be in the circumstances, and that in part the failure occurred because the function became too narrowly concentrated about the mathematical sciences and ignored the potential contributions of such disciplines as economics, sociology, and history.

The following pages approximate a commentary that was presented to the Third Annual Military History Symposium of the Air Force Academy on May 9th, 1969. I use the cautionary "approximate" because in the event about one-sixth of the following was dropped from the spoken text in the interests of staying on schedule, while a few remarks that referred to earlier events of the symposium are not reproduced here because they have only transient interest.

The reader who did not attend the symposium or who has not read Professor Holley's paper will infer from what follows that I did not unreservedly accept his interpretation of the events and their consequences. It is perhaps essential to observe that I have not gone behind his carefully researched account of events themselves, have accepted -- as given -- his objective and candid summary of developments, and chiefly urge here that the conclusions he suggested should be stated more plainly and broadened to include alternative explanations of what must be described as a most puzzling failure of Air Force management.

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This paper was prepared for the Third Annual Military History Symposium of the Air Force Academy.

Professor Holley's account of the 25 year tribulation of operations research in the Air Force fairly allots operations research to the special class of "good" things (like the American Revolution and Queen Victoria) designated in that paradigm of military history, 1066 and All That. He assumes that operations research has a high intrinsic value. And although the assumption is probably correct, it is advisable to recall R. D. Schmerl's classic observation that "to make knowledge an end in itself, . . . is very close to doing things for the hell of it."*

Which should suggest that I do not in all respects agree with Professor Holley's generally cheerful conclusions or with the reasoning that produced them. Or, indeed, with his way of describing his reasoning.

I take exception to Professor Holley's analysis and his findings on various grounds. First, it would appear that he has been entirely too charitable to the Air Staff and too forgiving to the practitioners of operations research at Air Force headquarters. He has not stated plainly some adverse conclusions and he has not set down some harsh judgments that -- to me, at least -- are implicit in his resumé. Second, but perhaps not entirely independent of the first, his careful account of how the Air Force has reacted to the reality of operations research mentions few names except those of the dead or the long retired -- and not all of those. It is not difficult for a reasonably diligent reader-between-the-lines to discover that people named Zimmerman and LeMay were among the anonymous principals, although intuition will not tell anyone what was so controversial about their respective roles. Individuals and their actions are the corpus of history; numbers and abstractions have become the province of mathematicians and the like. We owe it to ourselves as historians, I maintain, to spit in the occasional eye that wants spitting in. Otherwise, we might as well become political scientists.

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*Schmerl, "The Scientist as Seer," in A Stress Analysis of a Strapless Evening Gown and Other Essays for a Scientific Age, R. A. Baker, editor, Prentice-Hall, Englewood Cliffs, New Jersey, 1963.

Next, I do not agree with Professor Holley or the Operations Research Society of America that there is a special brand of analytical thought which occurs at the knee of some peculiar curve and becomes purer than something else called Operations Analysis, or Systems Analysis, or even -- if you will pardon the phrase -- Cost-Benefit Analysis. It does not make a great deal of difference whether one gets his Monte Carlo distribution by throwing dice or by reading between the beeps of an IBM Model 360. The numbers don't care. And since the point of it all is to recommend solutions to specific individual problems, there would appear to be some native advantage to assigning dimensional values to as many relevant uncertainties as can be identified in each problem. I know Professor Holley essentially agrees with that doctrine, even if he does not say so here, because he has explicitly used it in one of the finest studies of Air Force -- or Air Forces -- decisionmaking yet written, his Buying Aircraft: Materiel Procurement for the Army Air Forces, in the Army historical series.*

Finally, to paraphrase Oscar Wilde, it does not matter much whether an analysis is conducted in the Pentagon or on the third level below Offutt Air Force Base or in Santa Monica, if it is done well. That is all that matters. It is plain from Professor Holley's account, although he has been extremely careful to avoid unfounded criticism, that headquarters United States Air Force was spectacularly unskillful in exploiting the potential of operations research, but it is also apparent that one must exercise extreme care that the implications of such findings do not unfairly prejudice evaluation of the analysis operations of other agencies, institutions, or headquarters which have not been explicitly examined for this case study.

The precepts of operations research are not new. Liddell Hart observed in October 1937 that "the way that decisions are reached on

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*Irving Brinton Holley, Jr., Buying Aircraft: Materiel Procurement for the Army Air Forces, Department of the Army, Washington, 1964 (in the series of special studies, "United States Army in World War II," prepared by the Office of the Chief of Military History).

questions of strategy, tactics, or organization, etc., is lamentably unscientific." He urged that "...the investigation of problems be given to a body of officers who can devote their whole time to exploring the data on record, collecting it from outside, and working out the conclusions in a free atmosphere."* Liddell Hart had more to say and much that was equally pertinent, but that is the crux of what may be the first and certainly is one of the best statements of a requirement for operations research.

As the British saw it, operations research had two initial and two subsequent aspects. First there was the evaluation of the operational performance of an equipment or a weapon, and second an analysis of the relationship between tactics and weaponry to see to what extent one influenced the form of the other. Two extensions of operations research appeared later. One concerned the prediction of the course of future operations which might be either tactical or strategic, with the object of influencing policy. The last had to do with the study of the efficiency of organizations in actual operations.**

It seems evident from a comparison of (a) the British notion of what matters were within the purview of operations research with (b) the actual experience of the postwar USAF in these matters, that the British view was very much the broader. Headquarters, United States Air Force, seems to have kept its beak pretty much on the first line of inquiry opened by the British, and although the Tactical and Strategic Air Commands may have tried excursions into the relationship between tactics and weaponry, they were field commands and dared not venture into issues affecting changes in strategy, or organizational evaluation. Was there some peculiar element in the British experience that led them to such a generous view and something else in the American experience, or the American establishment, that caused quite a different

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*B. H. Liddell Hart, Thoughts on War, Faber, London, 1944, p. 125.

**This explanation of the span of operational research is taken from a speech made in 1952 by E. C. Williams, Director of Operational Research at the Admiralty, and cited in, The Origins and Development of Operational Research in the Royal Air Force, H.M.S.O. (Air Ministry), London, 1963, xviii.

perspective to result? These are legitimate questions, but it is not likely that they can be fully and satisfactorily explored here.

Still, something can be gained by a quick look.

Before the start of the European War the R.A.F. had three fundamental experiences of operational research. The first involved the influence of radar, newly developed, on air tactics. The second was an attempt during the special bombing trials of 1937-1938 to discover the accuracy of bomber attacks on various targets and the effect of anti-aircraft fire on low altitude and dive bombing attacks. The third involved experiments with methods of controlling the interception of intruding bombers and ultimately led to the creation of control room or operations room procedures.

Significantly, in all cases the principal inquiries were conducted by civilians who were mostly specialists in the engineering sciences, and the results were in all cases contrary to the hopes and beliefs of principal military figures and many senior civilians. There has been some discussion here of the difficulties that occurred when traditional military authorities of earlier centuries were obliged to face unpalatable technical realities. An observation of Sir Solly Zuckerman bears on this point, in part because he says in four sentences what others have taken forty pages to recite:

> The soldier must have faith in his weapons. Someone, somehow, must make 'the man at the sharp end' believe that the weapons with which he has been provided are at least as good as those that the enemy or potential enemy has at his disposal. . . . This world of faith and belief, of service, loyalty and discipline, is the very antithesis of the one in which science thrives.

He added:

> Perhaps . . . it is to the professionalism and isolation of the military establishment . . . that we have to look more than anywhere else in order to understand the fact that until quite recent times the military mind has been suspicious of the changes which are provoked by technological advance -- and correspondingly suspicious of scientists.*

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*Zuckerman, Scientists and War; the Impact of Science on Military and Civil Affairs, Hamish Hamilton, London, 1966, p. 8-9, p. 13.

It is reasonable to suggest that here and in the reaction of operations research specialists to the expression of such suspicions lies one source of the shortcomings of postwar operations research as practiced in Air Force headquarters. Operations research became part of an established organization: it should be quite obvious even to the most insensitive observer that no organization ever succeeds in reforming itself. Yet in matters of strategy analysis and evaluation of organizational effectiveness as well as the probable relationship between weapons and tactics, a part of the organization -- the operations research function -- was nominally charged with forcing the head of the organization to consider actions he would instinctively reject.

Prudent men do not take such positions, and prudence seems to have been characteristic of most postwar operations analysis in the USAF. Operations research tended to confine its attention to matters that were highly quantifiable and to avoid the doctrinal controversies implied in the British definition of the function. Whether that was the consequence of organizational placement, as Professor Holley suggests, or of the preferences of those who guided the function, or of the sociological setting of operations research in the military society that surrounded it cannot be readily determined. But these, too, are legitimate questions that must ultimately be answered.

Some years ago, Charles Poore commented that ". . . a great measure of the historian's trade lies in expertly pointing out what was inexpertly done long ago. Or not done."* Professor Holley has explicitly denied any such intent, but nonetheless he has indirectly and somewhat too gently told us what the Air Force has not done or has done quite inexpertly during twenty-odd years of tinkering with operations research in Air Force headquarters, both as a function and as an institution.

Operations research as it was conceived and practiced during World War II represented a means for performing more effectively or more

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*New York Times Book Section, 29 December 1964.

efficiently tasks that the military services would somehow ultimately be obliged to perform in any case. In those earlier and more violent days, "effective" and "economical" implied lesser casualties and slighter wastage of materiel than would otherwise occur. As tends to be true of all military establishments, everywhere, and at all times, the Air Force, having discovered that operations research was a particularly useful technique for specific applications, decided to enfold it in the existing structure of a permanent organization. But the Air Force seems to have been blind to the reality that both the circumstances that made operations research initially valuable and the characteristics of the discipline were perishable.

It is an interesting commentary on the character of operations research as used by the United States Air Force and as remarked by Professor Holley that its first significant contribution was to improve the bombing effectiveness of B-17 and B-24 aircraft in 1944, and its most recent accomplishment to recommend ways of improving the bombing effectiveness of B-52 aircraft over Vietnam. It would seem that in 25 years the designations of the aircraft and the targets have changed, but not much else.

Operations research began by addressing quite small issues -- or at least issues that could be addressed in rather small terms. Bombing accuracy, gunnery practices, maintenance concepts, supply and inventory problems: these were the wartime topics. And although such topics remained important to the postwar Air Force, they were overtaken and subordinated to much larger issues of weapons choice, strategic doctrine, procedures of research and development, methods of ensuring interservice cooperation in combat conditions, and such matters. Operations research in the Air Force generally has not sought out such larger questions, or, in approaching them, has attempted to narrow the uncertainties by excluding consideration of items that are difficult to quantify. Here is a sub-aspect of the problem: the difficulty of handling large policy issues in an organization designed for smaller questions. Moreover, Professor Holley observes, the operations research organization in Air Force headquarters preferred to deal

with matters that lent themselves to quantification rather than those in which judgment factors had to be substituted. Over the long term the operations research function seems to have avoided any broad commitment to do broad-issue analysis.

Another reality of the interaction between technology and its military applications is cost -- the economic factor. There may have been a time in the postwar world when questions of military choice could be decided without weighing cost consequences, but they probably were very small issues. Certainly there has never been a goal "worth any price." That is a preposterous exaggeration of need. But it has become very difficult to induce the services -- including the Air Force -- to make hard choices that require giving up one desirable objective in order to finance another. Perhaps the internal structure of a military society cannot endure the continued shock of making choices that are quite unacceptable to some part of the society -- as in deciding to invest in missiles rather than bombers, for example, or armed helicopters rather than close support fighters. In any case, the service that would be obliged to live with the consequences has habitually been reluctant to make broad value judgments in matters that affect choices between weapons and -- hence -- force structures. Force structure decisions hinge on prior choices of strategies. Or should, although in fact strategy choices are definitely limited by present force structure realities -- the very high probability that Soviet assured destruction forces cannot be destroyed, for instance -- and by force structure expectations that frequently are dominated by technological uncertainties. But these are precisely the sorts of matters that operations research practitioners in Air Force headquarters were least anxious, and perhaps least ready, to consider. For reasons that are beyond the province of this and Professor Holley's paper, the services (all three, not the Air Force alone) tried to avoid making unpleasant force structure recommendations, prefering to let others have the responsibility, and the onus. One consequence has been an increasing intrusion of secretariat-level authorities in questions

that once were decided by operating commands. Such intrusions have occurred, and have subsequently been institutionalized, either because a service refused to make choices, or because a service made such irrational choices that senior authorities concluded that they could no longer trust in service judgments.

Here are issues and questions to which the techniques of operations research might properly have been applied. At least they are not foreign to the interests of the function. But instead separate systems analysis organizations were created during the late 1950s and the 1960s at the secretariat and air staff or command level. In some respects that may have been a minor tragedy of organization. But it may also have been inevitable, given the nature of organizations and the character of the assignments of systems analysis organizations. In any event, the trend definitely cut away one branch of operations research.

The plain facts seem to say that the postwar Air Force appreciated the worth of operations research and the advantages of keeping it alive. So the function was institutionalized and the capability preserved against some future emergency. Not much more seems to have been contemplated, and owing to (a) the preferences of the operations research specialists and (b) the pressures of ordinary bureaucracy, scarcely that much resulted. Operations research did not take on the varied tasks suggested by early experience, but the tasks had to be done and ultimately other organizations tried to do them.

Professor Holley has gently discussed one of the reasons that such "other organizations" within the Air Force were also only modestly successful in performing such difficult assignments of analysis. The systems analysis organizations were in many instances used as resources to generate evidence that could be used to counter the findings of analysis performed by groups outside the Air Force. Put more baldly, they frequently served as protectors of the status quo, or of the preferred status, whether quo or not. They were no more able than any other part of the larger organization to bring on major changes, however necessary. The usual source of such change in a thoroughly

institutionalized organization like the Air Force was (a) an investigative body, (b) a consulting organization, or (c) an executive committee.

Whether such realities were acknowledged or not is in some respects immaterial. The creation and growth of RAND and of later organizations which purported to do about the same kinds of research was one consequence of the failure of organizations native to the Air Force to bring about essential changes.

RAND was called into being to consider the weaponry implications of new technology, a set of questions operations researchers of the late 1940s were extremely reluctant to attack. In the succeeding five years, RAND ventured cautiously into a consideration of the doctrinal and force structure implications of new weaponry. After 1953, it was unlikely that any internal group of 10 or 20 Air Force headquarters people who called themselves operations researchers would be able to recapture and cope with such complex, difficult, and fascinating problems.

Nor were the immediate intra-organization alternatives accepted. Cost-effectiveness analysis and scenario analysis were treated merely as the new faces of operations research, and Air Force headquarters does not appear to have agreed that these new faces belonged at the military conference table with all of the older faces. There lies another difficulty.

Sir Solly Zuckerman put it this way:

> Basically, operational analysis implies a kind of scientific natural history. It is a search for exact information as a foundation for extrapolation and prediction. It is not so much a science in the sense of a corpus of exact knowledge, as it is the attempted application of rigorous methods of scientific method and action to new and apparently unique situations. The less exact the information available for analysis, the less it is founded on experience, the more imprecise are its conclusions, however sophisticated and glamorous the mathematics with which the analysis is done.*

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*Zuckerman, p. 18.

As Professor Holley has pointed out, after 1948 operations research in Air Force headquarters rather completely became the province of numerologists. The humanities and the sciences of synthesis were mostly excluded from the discipline as practiced in the Air Force. But not because mathematicians do not like historians and such. Heed for a moment a comment by one of the leading advocates of the systems analysis approach. "Like operations research," said Alain Enthoven, "...[systems] analysis can and must be honest, in the sense that the quantitative factors are selected without bias, that the calculations are accurate, that alternatives are not arbitrarily suppressed, and the like. But it cannot be 'objective' in the sense of being independent of values. Value judgments are an integral part of the analysis: and it is the role of the analyst to bring to light for the policymaker exactly how and where value judgments enter so that the latter can make his own value judgments in the light of as much relevant information as possible."*

Here is a critical distinction. The heads of Air Force operations research in the Pentagon seem to have recognized intuitively the futility of raising issues the Air Force did not want to face. It is sometimes easier to avoid dabbling in certain classes of problems than to face the consequences of solving them. To suggest that the general ineffectiveness of the Air Force operations research organization can be explained largely by the absence of historians, economists, sociologists and the like is to oversimplify a very complex case. No doubt such people would be nice to have about. One is reminded of Alice's conversation with the white knight about mouse traps and bee hives and anklets on his horse, and the knight's remark that the mice kept the bees away or the bees kept the mice away, but that in any case the mouse trap was a necessary precaution against the possibility that mice would take over the horse's back and the anklets were necessary "to guard against the bites of sharks." It seems

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*Enthoven, "Operations Research and the Design of the Defense Program," Proceedings of the Third International Conference on Operational Research, Dunod, Paris, 1964, p. 53.

extremely unlikely that historians or economists could have helped much to answer questions that were never asked, and it is quite evident that operations research, for one reason or another, did not permit itself to become involved in large complex problems of policy that might conceivably have required the services of non-numerologists.

Once a function has been as carefully defined by its practitioners as was postwar operations research, the function tends to become the nucleus of an institution, and institutions are the stuff of which bureaucracies are made.

One of the dominant attributes of any ordinary bureaucracy like the Royal Navy, the German Post Office system, the Politburo, or the Roman Curia is that it accepts a stable set of values early in its existence and rarely, if ever, changes them of its own volition. Bureaucracies are self-perpetuating. They do not die of neglect -- as witness the continued vitality of the United States Indian Bureau -- and are decidedly difficult to kill: the Suez Canal Commission still lives, somewhere. Institutions change mostly in their response to outside pressures. If the pressure can be relieved elsewhere, as in the creation of alternative ways of doing essential systems analysis, an institutionalized operations research function will change little and the parent service -- here the Air Force -- will suffer thereby.

Consider a recurrent question that has perturbed the Air Force for two decades: What kinds of weapons should be selected for development emphasis. As early as 1945 the Air Force, still part of the Army, saw the need of developing and deploying bombardment missiles. Yet it was not until 1957 -- twelve years later -- that the Air Force gave up persistent efforts to develop aerodynamic cruise missiles in preference to ballistic missiles for the bombardment mission, notwithstanding that for several years the greater value of ballistic missiles had been established to the satisfaction of virtually all independent analysts. This question is further discussed in the Appendix to these remarks.

A friend of mine who is far better equipped than I to comment on the development of operations research in the Air Force, or on a paper about its development, has observed with considerable astuteness that

the really striking achievements of operations research in the Royal Air Force, where it had its first and greatest successes, occurred while England was losing the War, and at a time when radical notions and outspoken criticisms were listened to because radical measures were desperately needed.

Institutional change is rarely popular and institutional change is particularly unpopular if neither the institution nor its masters can find reason for dissatisfaction with matters as they have been. Let me close then, by repeating once more Charles Poore's injunction, ". . . a great measure of the historian's trade lies in expertly pointing out what was inexpertly done long ago. Or not done."

Let us begin.

Appendix

By 1952 or 1953, quantitative analysis had indicated that cruise missiles would be less accurate, less dependable, and more costly (in terms of combat effectiveness) than ballistic missiles. But virtually all of the research leading to such conclusions was conducted outside the regular Air Force, either by independent study groups or by committees created at the insistence of senior civilian officials. The Atlas ballistic missile program is perhaps the best known example of projects so affected. Although proposed as early as 1946, Atlas was continually subordinated to cruise missiles, at first because of assumed technological inadequacies, later because of technological misjudgments intermingled with shortcomings of doctrine. In each instance decisions were reflected in allocations of funds, or non-allocations.

The assumptions of technological inadequacy which hampered missile development from 1946 to 1953 arose in a set of value judgments accepted uncritically by Air Force analysts. The basic assumption was that ordinary evolution from a base of aircraft technology would lead most directly to an operationally capable missile. But there were important underlying assumptions. For example: (1) the assumption that some guidance system that was an extension of autopilot and autonavigator experience would be "easier" to develop than a closed-loop inertial trajectory system; (2) the assumption that derived or evolutionary advances in airframe technology would permit long-endurance, high-speed cruise missiles to be perfected before problems of high-stress launch and high-temperature re-entry could be solved for ballistic missiles; (3) the assumption that high-efficiency turbojet or ramjet propulsion systems would emerge from development much sooner than dependable large rockets; and (4) the assumption that the chief doctrinal modification required to move from bombers to missiles could be satisfied by substituting missiles for manned bombers in about a one for one ratio.

In time it became evident that each of these premises was thoroughly erroneous. They stemmed from assumptions about the value of experience in developing and operating the aircraft of World War II. From them were derived conclusions about the advisability -- and risk -- of depending on the evolution of missiles from aircraft progenitors, rather than investing in a ballistic missile program itself.

There were other considerations, too, of course. Until at least 1951 the Air Force was inherently incapable of accepting the commitment of any substantial part of its development-production budget to such exotic weapons as intercontinental ballistic missiles. The establishment of a separate Air Research and Development Command in 1951 removed that particular obstacle. Technology, or its uncertainty, remained an obstacle until 1952, after which time those who looked closely enough into the matter could find evidence that an intercontinental missile was no longer a particularly high risk investment in unlikely technology. In retrospect, it is quite plain that the difficulties of developing a ballistic missile were somewhat less appalling than the unacknowledged difficulties of developing a comparably accurate, reliable, and effective cruise missile. Put baldly, Atlas was much easier and cheaper to develop than Navajo would have been, or Snark, the evolutionary cruise missiles Atlas competed with. One is sorely tempted at this point to apply directly Professor Elting Morison's principal thesis about the resistance of a military society to major change. To people who had grown up with manned bombers before and during World War II and who had mostly stayed with them through the early part of the next decade, a cruise missile was a less painful and certainly a less abrupt departure from what they were familiar with than would be a totally alien ballistic missile. Those who favored the evolutionary approach to the creation of a new generation of weapons, predominantly missiles, were people to whom aircraft had a meaning as a way of life, a symbol, a preferred means of performing a military assignment. With minor exceptions, those who sought to bring on major or revolutionary change had no such commitments, being

primarily engineers and scientists of one sort of another, and only secondarily airplane commanders. It is not really important whether the opponents of change consciously recognized the possibility that the appearance of a ballistic missile might lead to the decline and ultimately to the disappearance of the manned bomber. It is enough that those concerned sometimes acted as if they foresaw that possibility. So cultural resistance to the innovation presented by the ballistic missile was one reason for the relatively slow initial progress of that development, and failure to take appropriate account of the unpredictability of technology was another.*

If the ballistic missile had, by 1952, become technically and financially and culturally conceivable, why was the requirement for it not strongly validated? In retrospect the answer seems plain enough: cultural resistance, or the extreme reluctance of a bureaucracy to change itself. If the analysis techniques developed through operations research and the experience of World War II ever had a promising utility, it should have been in a situation of this sort. What was required was an objective review of established but not widely understood facts and an analysis of the importance and relevance of those facts. That the Air Force had a doctrinal commitment to aerodynamic missiles derived from manned bombers was totally irrelevant to the issues which were clamoring for consideration.

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*These matters are discussed by E. E. Morison, Men, Machines and Modern Times (The M.I.T. Press, Cambridge, 1966), pp. 37-39, and also by Robert Perry, "The Ballistic Missile Decisions", The RAND Corporation (P-3686), October 1967.