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ADDRESS

THE HONORABLE CHARLES J. HITCH

ASSISTANT SECRETARY OF DEFENSE (COMPTROLLER)

JUNE 1963

OPERATIONS ANALYSIS IN THE DEPARTMENT OF DEFENSE

It has been said, provocatively but I think rightly, that we tend to overestimate the progress that we will make in the short run, and that we tend to underestimate the progress that we will make in the long run. Not only is there wisdom in that statement from a general point of view, but I also think that it applies to military operations analysis in particular.

For those of us who work in this field on a day-by-day basis, progress often seems agonizingly slow. We are aware of all the room there is for improvement, of all the things we would like to do that we haven't had time for, and of all the problems that we haven't yet really learned how to handle. But -- perhaps we are overestimating the progress that we expect in the short run.

To see if there is some truth in this, I would like first to give you my impressions of where we stand today in the field of military operations research: what we have learned, what we are trying to do, and what we hope for in the future.

Much of what has been said and written recently has centered on the "computer approach" to defense problems. One might almost believe that the denizens of the Pentagon no longer need to tax their brains with questions of national defense -- that, buried down in some dark corner of the Pentagon, we have built an omniscient computer -- that any time a problem of choice comes up, all we have to do is ask this electronic brain, and then just sit back and read the answer as you would the card from a penny weighing-machine.

Of course, this is far from so. In fact, this impression is almost diametrically opposite to the true picture.

The selection of weapon systems, the design of forces, and the choice of the level of the national defense effort have always been essentially artistic, rather than scientific. I wish today that I could tell you that we have found a way to change all that -- but I cannot. These things still involve far more art than science, but we are taking increasing advantage of the art of operations research.

The aim of this art is to assist the decision-maker by furnishing him, insofar as possible, with quantitative estimations of the cost and effectiveness of each of the various alternative courses which he could choose. The distinguishing feature is that these estimations are based not on opinions, or on simple extrapolations of earlier doctrines, but on quantitative analyses. The thesis is so obvious and simple that it seems trite; but the facts are that, in the past, we have not always been as rigorous and analytical as we might have been.

It isn't hard to see why this analytical tool has become important on the higher levels of military planning. While it may well be that earlier

modern science can offer has enormously complicated our problems of choice. There is hardly a military task which cannot be accomplished in a multitude of ways -- and many capabilities which we take for granted today have been wholly impossible over most of the span of military history. Further, the price tags associated with each of the alternatives are tending to become so large that choices must be made. It is becoming less and less feasible to hedge against improper selection.

Given such a situation, it is hardly surprising that we have turned to analytical techniques to assist us in our choices. But let me hasten to add four remarks with regard to these techniques. First, they do not constitute anything like a panacea. Second, their use constitutes a hazard; The potential danger in improper use and unwarranted confidence can be just as great as the potential benefit from proper use and appropriate confidence. Third, as I noted before, proper application involves rather more art than it does science, and fourth, the state-of-the-analytical-art is still quite embryonic; our proficiency in the field is improving.

Discussing these four points in greater detail, and starting with the one about analytical techniques not being a panacea, I think it is absolutely clear that there will always be considerations which bear on the very fundamentals of defense which are simply not subject to any sort of rigorous, quantitative analysis. It is not even possible to draw a line between those which are and those which are not. The gamut encompasses a wealth of considerations which are more or less subject to analysis. Thus, there will necessarily be some questions which are outside the scope of our analytical technique. For this reason alone, we cannot expect a panacea. It is as if some of the digits had been left off the input keyboard of our computer. As an example, I might mention considerations of the morale of the armed forces. How do you quantitatively distinguish between men who are highly motivated, and those who are demoralized? In fact, how do you quantitatively predict what it is that motivates or discourages a man? And which man? The fact that we cannot quantify such things (and there are many other similar examples) does not mean that they have no effect on the outcome of a military endeavour -- it simply means that our analytical techniques cannot answer every question.

That such is the case is widely recognized, particularly by those in the military profession who have had to live with these realities. But does that mean that all analysis becomes meaningless? I think not. Every bit of the total problem that can be confidently analyzed removes one more bit of uncertainty from our process of making a choice. While I can hardly believe that any significant military problem will ever be wholly susceptible to rigorous analysis, I feel just as certain that analytical techniques can allow us to make significant choices with a very real increase in confidence.

On the second point -- that such techniques constitute a potential hazard as well as a potential benefit -- there is a well-known human tendency to believe the printed word. By the same token, in the design of military forces, there is a tendency to associate analysis with credibility, particularly if the magic word "computer" is mentioned. In fact, of course, the Machiavellian analyst can "prove" the most outrageous theses. But he is

In this field, we like to think of the analyst as a disinterested observer, a scholar who has no preconceived opinions, a human calculating machine whose sole motivation is the search for truth. But this is a goal rather than a representation of reality. Persons who have worked in a field long enough to become competent tend to have formed opinions on the basis of their labors, and keeping an open mind requires great moral integrity and intellect.

Thus, the esoteric aura surrounding the word "analysis," coupled with the fact that analyses are conducted by human beings with human failings, presents us with a potential hazard. I do not consider this a valid reason for dismissing the utility of analyses, any more than the fact they are not a panacea. But it does indicate that we must never be casual in our approach to analysis--we have a tool of great power in our hands, and it must be treated with proper respect.

On the third point -- that analysis involves more art than it does science -- let me point out that there is virtually no bibliography of standard analytical technique, save for some rather specialized cases. There are branches of mathematical technique which are sometimes of great assistance, but, as an example, there is no standard method of evaluating the military work of a tactical aircraft, or even of comparing two types of tactical aircraft on a relative basis. This is not to say that we cannot make such comparisons -- but that there is no single correct way. Accordingly, it is inevitable that judgment is involved in determining how the analysis shall be conducted.

The art of analysis involves a sensitivity to the military task at hand, an insight into the proper definition of figures of merit, an understanding of the pitfalls, an ability to differentiate between what is significant and what is trivial, an appreciation of the degree of confidence warranted in the conclusions by the underlying analysis. By and large, this art, as any art, is perfected only through experience. It is not something that can be swallowed in pill form. It is not something that you can look up in a handbook of formulae. There is no threshold of capability, and no upper limit of excellence. But I can tell you that a proper analysis of a previously incomprehensible problem -- one which sorts out a tangle of facts and points the way to understanding -- can well exceed the artistic limits of any electronic computer.

Finally, on the fourth point -- the state-of-the-analytical-art being embryonic -- it is only quite recently that the use of analytical techniques has begun to have a major effect on our defense structure, as opposed to the relatively detailed tactical application of existing forces. I mentioned before that there is no bibliography of standard analytical techniques. This is characteristic of the early state of our development. I suspect that before too much longer, we will begin to see wider acceptance of certain standard analytical techniques of some military tasks than we have in the past. But, far more important than this, I am sure that the number of people who are versed in this sort of analysis will increase very rapidly.

The significance of this trend should not be underestimated. At present, we have far more questions which are amenable to analysis than we have analysts to work on them. But groups of analysts are being formed and gathering experience in places where none existed before. In addition to the well-established centers for operations research, many smaller groups are springing up in this country at many levels in the Services as adjuncts to military contractors, as small firms offering analytical facilities, in association with universities, and so on.

I look to many beneficial effects as a result of this increase in our analytical resources. For one thing, the philosophy underlying operational research techniques should not be considered as applicable only at the tactical level, or only at the highest level of military decision-making. It is equally applicable on all levels, as well as to all the chronological stages in the development of weapon systems.

After all, what this sort of activity attempts to do is to reduce the uncertainties involved in making choices between alternatives. It is clear that choices are made at all levels of the military structure -- not only in the Office of the Secretary of Defense but also in the Service staffs, the theater commands, the operational unit headquarters, and so on.

Furthermore, choices must be made continually as any given weapon system is developed -- not only the basic decision to embark on the project, but, later, as the "paper" design is translated into hardware -- still later, how to employ the system to best advantage, and, in many cases, how to modify the original system to take advantage of new technology, or to extend its useful service life.

Thus, with choices having to be made at all levels and on a continuing basis, it is clear that the opportunities for benefiting from analytical techniques are extremely wide. It is for this reason that the generation of additional numbers of qualified analysts, and the development of the art of analysis, hold so much promise.

But, assuming that the use of operations research techniques will tend to become more pervasive in the Services, what about its use in our Office of the Secretary of Defense? Will we eventually, as some apparently fear, usurp the design of military forces, and leave the Services only the "housekeeping" responsibilities? To answer these questions, let me take a moment to outline the place of operations research in the Office of the Secretary of Defense.

In the first place, I feel that the most significant contribution that we have made in the Office of the Secretary of Defense since 1961 is to organize military planning, programming, and budgeting in a way that facilitates the use of analytical techniques as an aid in decision-making. The classical form in which defense budgets had been presented in past years did not lend itself to the application of analytical techniques. Categories were not generally associated with any single military task. For example, personnel were grouped together, with no distinction as

forces required for the conduct of limited war, or of general support, etc. On that basis, what we proposed to spend for manpower was clear enough, but what we expected to accomplish with the expenditure was quite obscure. Under those conditions, the application of analytical techniques as an aid toward improving our military posture would have been immeasurably complicated.

Furthermore, budgeting on a year-by-year basis is, in most areas, quite unrealistic. For example, expenditures in the initial phases of the development of a new weapon system do not necessarily bear any relation to the total expenditures involved in the expected life of the system, and a rational choice between alternative systems can hardly be made with only part of the cost data at hand. It is necessary to consider more than the R&D expenditures; we must also estimate the costs associated with procurement of the developed article with the spares and support equipment, with operation, with training the necessary personnel, possibly with the ordnance required for it, and so on, over its entire life cycle, from initial concept to retirement. While the accuracy with which we can predict such costs may leave something to be desired, it is clear that at least trying to take such factors into account is preferable to ignoring them.

In order to be able to see what we expect to accomplish with our expenditures, and to account for more than any single year's budget, we have established a different process of planning, programming, and budgeting. At the heart of the concept lies a publication called the "Five-Year Force Structure and Financial Program." In this document, one can see at a glance just what has been approved in each functional category (such as strategic retaliatory forces) for the next five years. These categories are further broken down into individual systems. For example, you can see just how many B-52's we plan to have in any given year, how much investment is involved, and what the expected operating costs are. You can find out how many personnel are involved in each system, and so on. This "bible" represents the currently approved program. There is not a single item in it that has not been approved by the Secretary of Defense, and, with this approach, it is now possible for all those involved in the decision-making process to have at their fingertips an authoritative statement of just what our current plans are.

I stress the word "current" because this document is not meant to be inflexible -- far from it, since its purpose is to facilitate rational choices between alternatives. The current plan can be changed at any time, but, in order to do so, a formal procedure has been established. Any proposed change is submitted in a standard form, written in the "language," if you will, of this basic planning document. The proposed changes in the force structure, year by year, are documented; the associated costs are detailed; changes in manpower, both military and civilian, are stipulated. In addition, of course, these proposals are accompanied by quantitative analyses which justify the reasons why the changes are felt to be beneficial.

These documents are reviewed by the Secretary of Defense. If he approves, appropriate changes are made in the Five-Year Force Structure and Financial Program, and the new project is implemented. If he disapproves, the Service sponsoring the proposed change has an opportunity to submit additional justification if it feels that significant factors were not adequately considered in reaching the decision.

Thus, I feel that one of our most important contributions with respect to operations research in the military establishment has been this attempt to create an environment in which quantitative analysis can flourish and be employed effectively. In addition to this, my Systems Analysis staff also contributes to the effective use of such techniques. Their utility is threefold: first, as an integral part of the Office of the Secretary of Defense, they are in an excellent position to view the problems of national defense as a whole. This qualifies them, with other parts of the Secretary's office, to suggest profitable areas for analytical consideration. In this function, they work closely with the Services in the conduct of their individual studies with a constant view as to how they will fit into the larger picture. In this role, I would like to point out that the studies are conducted by the Services, and not by members of my staff, who act only in an advisory capacity, and not as overlords.

The second function of the Systems Analysis staff is to participate with other parts of the Office of the Secretary of Defense and the JCS in the review of proposed changes in the Five-Year Force Structure and Financial Program. In this role, they attempt to determine that the basic inputs and assumptions are reasonable, that the objectives conform to national policy, and that the suboptimization has not been too narrow. I would also like to point out that my staff does not render decisions on such matters -- that is the exclusive right of the Secretary of Defense. The Systems Analysis staff merely offers comment. And I am sure that none of you suspects Mr. McNamara of being a rubber stamp.

In their third function, my Systems Analysis staff occasionally conducts studies of subjects which cut across Service lines. I might also point out that the great majority of such studies are performed in the Joint Chiefs of Staff area which, in addition to its organic Special Studies Group, also relies on the Weapons System Evaluation Group.

Thus, we are in no way attempting to usurp the Services' function of the design of our forces. Rather, our aim is to provide a climate in which the benefits of quantitative analysis can be fully realized as an inherent part of the force design process. In this context, our aim, rather than usurping such analyses, is to encourage them -- to provide a forum where they can be appreciated -- to assist however we can in expanding their use throughout the military organization.

So much for the quick summary of where we stand today. What about the long-run progress? Do we, in fact, tend to underestimate it?

I would hesitate to predict where we would stand in, say, 20 years from now. 1983 is a long way off, and, according to my thesis, I would only underestimate the expected progress. But I can turn back 20 years to 1943 which, I think most of you would generally agree, marked the first easily recognizable attempt to use operations research techniques as an aid to military planning and decision-making.

In those days, the pioneers in this field consisted of a relatively few small groups whose attention was turned to the immediate problems of winning a war. We in the United States were particularly indebted to predecessors in the United Kingdom for their cooperation in our early efforts. But activities in the field were far more limited than they are today. Most of the analysis was devoted to problems of tactics and -- even more strikingly -- to the problem of making best use of the weapons at hand. In certain areas -- ASW springs quickly to mind -- these efforts met with notable success. But the difference between then and now is one of scope. There was no attempt made to use operations research techniques as an aid in making major decisions affecting entire national defense strategies. Only a very few people were even aware of the existence of such activities as there were then. The proliferation of analytical groups that we see today in the great independent organizations, such as RAND, in all of the Services, and in all of the major weapons contractors' facilities was unheard of.

In short, times have changed. From little more than a good idea, operations research has grown into a big business. I sincerely doubt that many of the pioneers of 20 years ago would have been so bold as to predict that operations research techniques would have the influence they do today, and I will not be so bold today as to predict the next 20 years. I only hope that the further growth in scope and influence which we may achieve during the next 20 years will be matched by an at least equal advance in the state of our technological art.