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AN APPRECIATION OF SYSTEMS ANALYSIS,

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Charles Hitch ✓

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AN APPRECIATION OF SYSTEMS ANALYSIS*

Charles Hitch

Military systems analysis is an extension of operations research techniques of World War II to problems of broader context and longer range--e.g., force composition and development as well as operations decisions.

Greater complexity is inevitable as we attempt analyses to aid decisions affecting more distant time periods: growth in number of relevant variables; compounding of uncertainties; increased importance of enemy reactions; complications of time phasing; the need for a broader concept of criteria.

Techniques available to the systems analyst for dealing with these complexities are less than satisfactory. "Factoring" is inevitable. All routine or mechanistic approaches are deficient; e.g., expected value and minimax criteria. Design and criteria problems are of dominant importance.

The systems analyst may have to be content with better rather than optimal solutions; or with devising and costing sensible methods of hedging; or merely with discovering critical sensitivities. He has an important role as inventor of systems.

The difficulties of systems analysis are rooted in the nature of the military problems. Other methods--e.g., piecemeal analysis; intuition--do not escape them and have limitations of their own. Military systems analysis provides a framework for combining the knowledge of experts in many fields to reach solutions which transcend any individual expert's judgment.

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The first widespread and explicit use of scientific method or analysis as an aid to military decision making was made by operations analysis (or operations research) teams in World War II. Small-scale and partial use of similar analytic techniques had long been common. Thucydides describes examples of their use by the Athenians in the Peloponnesian wars.

(21) *This paper is a condensation of lectures prepared for Air Force audiences; it was presented at the Operations Research Society of America ~~at the~~ Los Angeles meeting, August 15, 1955, ¹

But even World War II operations analyses were limited in character. They related to operations in the immediate future, not to force composition or the development of equipment which would affect operations in the more distant future. Partly in consequence, they were simple, in the sense that they considered only a small number of interdependent factors. They were able, as a rule, to use some proximate, obvious straightforward objective or criterion as a basis for choosing one operation over another.

A typical example of a World War II operations analysis problem was what bomber formation to use in attacking targets deep in Germany. This problem had few variables and an obvious criterion: minimize losses in achieving given target destruction.

Since the war, at RAND and elsewhere, attempts have been made to use analysis as an aid to military decisions in problems of immensely greater difficulty and complexity. There has been a tendency to use the term "systems analysis" to describe these more complex analyses, but there is no line of demarcation. Both operations analysis and systems analysis are attempts to apply scientific method to important problems of military decision, even though the problems are not particularly appropriate for scientific method and would never be selected for the application of scientific method by a truly "academic" researcher.

Both operations analysis and systems analysis have the same essential elements:

An objective or objectives which we desire to accomplish.

Alternative techniques or instrumentalities (or "systems")

by which the objective may be accomplished.

The "costs" or resources required by each system.

A mathematical model or models; i.e., the mathematical or logical framework or set of equations showing the interdependence of the objectives, the techniques and instrumentalities, the environment, and the resources.

A criterion, relating objectives and costs or resources, for choosing the preferred or optimal alternative.

THE DEVELOPMENT OF MILITARY SYSTEMS ANALYSIS

Developments and extensions of operations analysis since World War II have taken the following forms:

- 1.. The use of analysis to aid in force composition and development as well as operations decisions.
2. A great increase in number of interdependent factors considered.
3. The explicit treatment of problems of uncertainty.
4. The explicit treatment of enemy reactions.
5. The explicit treatment of time-phasing.
6. A broader concept of objectives and criteria appropriate to the broader and longer range problems of decision being analyzed.

We will discuss these in turn, considering in an impressionistic and superficial way the analytic techniques appropriate for each extension.

1. Force Composition and Development Decisions

Models used for force composition and development decisions need not be formally different from those used for operations decisions. Their form is likely to differ, however, because of the greater number of interdependent variables, the increase in uncertainty, and the broader criteria appropriate

to the longer time horizon considered. As an historical fact, the attempt to apply models to development problems has sparked important developments in systems analysis methods in all these areas.

2. More Variables

The application to force composition and development decisions means that we are concerned with a military force two or three to ten or fifteen years in the future instead of with present forces. This fact alone vastly increases the number of interdependent variables which we have to consider. All sorts of things which are given in the short run become variables in the long run. For example, in the bomber formation operations analysis in World War II, the planes were B-17's, their number was given, the targets were given, the bombs were given, the enemy defenses were given, etc. In the longer run these are not given. They are unknown. They become variables. Some are variables subject to our control, some are variables subject to the enemy's control, some are subject to nobody's control. But all are variables. Moreover they are all interdependent.

To illustrate how this can increase the number of interdependent variables and systems to be compared, consider a simple bomber or missile development problem. Suppose we ruthlessly simplify aircraft characteristics to three-- speed, range, and altitude. What else do we have to consider as interdependent variables? At least bomber formation, flight path, base system (two variables-- number and general location), target system, bombs, enemy defenses (two variables?). This may not sound like many (in fact, it is far fewer than are probably necessary), but if we go no higher than ten, and if we let each variable take only two alternative values, we already have 2^{10} cases or systems

to calculate and compare ($2^{10} > 1,000$). If we let each variable take four alternative values, we have 4^{10} cases ($4^{10} > 1,000,000$).

In the World War II bomber formation problem, on the same assumptions, everything else is given except possibly flight path. The number of cases, with the same degree of simplification, would be two, four, or at most sixteen. So just moving into the future puts us in a different ball park.

What do we do when we are confronted with a million or billion or decillion cases to compute and compare? Of course we develop higher speed computers every year and greater skill in using them, and I do not want to belittle the significance of this accomplishment. But even the capacities of modern high-speed computers are limited. We still have to cut down most of our problems for this reason. And there are usually more confining limitations. We have to get relevant data and relations to feed into the machine on all these systems. The more variables we deal with, the more time this takes and the more people--both scarce and valuable resources.

So somehow, for one practical reason or another, big, broad problems have to be cut down in size. We have to "factor out" a practical problem--i.e., factor out those variables which are especially important for the decision with which we are concerned, and "suppress" or "aggregate" the rest. How do we do this? To a limited extent by preliminary analyses and tests, but for the most part by sheer judgment. It is hard to do; it amounts to no less than deciding, in designing the analysis, what is important and what isn't.

Probably most systems analyses that go wrong, go wrong here. Either they include a mass of data and calculations which are just excess baggage, or they exclude some really critical factor on which a good decision depends.

3. Explicit Treatment of Uncertainty

Uncertainty is present in operations problems, and it needs to be dealt with there. But uncertainties multiply, as an almost invariable rule, as we look further in future.

We need to distinguish several kinds of uncertainty:

- (a) "Planning factor" uncertainty. Planning factors--attrition rates; average bombing errors, etc.--are always uncertain, but less so in the present than in the future.
- (b) Uncertainty regarding the enemy and his reactions. Like (a), this is always with us and increases with time.
- (c) Strategic uncertainty. Will it be war, cold war, or peace?

If war, when? General or local? With what political constraints? To achieve what political objectives? Who will be our enemies, who our allies? Will our allies make bases available to us?

In the very short run, strategic uncertainties of this kind are sometimes trivial--e.g., in the typical operations analysis problem in World War II. In the longer run they may quickly become dominant. No force composition or development analysis can ignore them. For example, the whole composition of the Air Force may be drastically affected by our opinion about the relative likelihood of big and little wars. Who can tell us? The answer is not easy or obvious. No present Administration or Joint Chiefs of Staff can make strategic decisions binding on a future Administration or Joint Chiefs of Staff.

- (d) Technological uncertainty. This is small in the present; in the future it can be vast. For example, until recently there was real uncertainty as to whether the H-bomb would work, and if so, when;

this profoundly influenced the structure of many systems analyses. There is always technological uncertainty of some degree attached to research and development; otherwise it wouldn't be research and development.

(e) Finally, there is statistical uncertainty--the kind that would still exist even if we could predict technological progress, the dates of wars, enemy strategies, and the central values of all important parameters. Statistical uncertainty stems from chance elements in the real world. Statistical uncertainty is the least of our worries in systems analysis: not only have we made great progress in Monte Carlo techniques for dealing with statistical fluctuations when we have to, but these are usually swamped by uncertainties regarding central values and states of the world in long-range problems. The use of elaborate methods to reflect statistical uncertainties in such problems is likely to be an expensive frill.

What do we do in a systems analysis to take account of the proliferation of uncertainties resulting from our ignorance of the future? We can't ignore them. To base our decision on some single set of "best guesses" could be disastrous. For example, suppose that there is uncertainty about ten factors (e.g., will overseas bases be available? will the enemy have interceptors effective at 60,000 feet?), and we make a best guess on all ten. If the probability that each best guess is right is sixty per cent, the probability that all ten are right is one-half of one per cent. We would be ignoring futures with a 99.5 per cent probability of occurring.

The problem breaks down into two parts:

- (a) How compute all the "interesting" contingencies? This raises problems very similar to those we have already discussed. Mere computation time is important, but usually much more so is deciding which cases deserve to be computed and assembling the data and structure for each.
- (b) Once we have computed the alternatives, we will almost inevitably discover that one strategy is superior in some contingencies, another in others. How does the systems analyst choose the preferred strategy? What decision does he recommend?

For example, suppose analysis shows that a strategic bombing system dependent on overseas bases will be most effective for 1960-1965. Suppose further that we regard it as quite likely, but not certain, that we will have overseas bases in that time period. Suppose finally that if we should not have the bases, the system would be very bad. What do we do in such a case?

Ingenious efforts have been made to find an answer, but with limited success. The shortcomings of merely maximizing the expected outcome are too well known to elaborate upon: because it incorrectly weights high and low outcomes, it can lead to the choice of a reckless strategy and possible catastrophe. Maximizing the expected value or "utility" of the possible outcomes is not subject to the same criticism of recklessness, but it is rarely possible to assign objective utilities to the outcomes, even if one somehow surmounts the theoretical and practical obstacles of assigning probabilities.

Expected outcomes or values, in any event, are appropriate only in playing against nature, not in playing against an intelligent opponent; and in few military systems analyses is it legitimate to ignore completely the possibility

of intelligent opposition. Game theory suggests that in such circumstances we should max-min; i.e., choose the system which minimizes the worst that can happen to us. This would be good enough if we were really playing a two-person constant sum game, but we never are--except perhaps at the lowest operational level. And if our opponent is anything less than completely wise and resourceful (i.e., always) max-mining is too conservative; it forfeits opportunities to exploit enemy mistakes.

Some advocates of max-min have argued that a special variety of it--max-mining "regret" or "loss" rather than outcomes--is an appropriate criterion or approximately rational rule of thumb for the case when we play against nature and can in principle calculate probabilities but in actual practice have only vague estimates. It at least does not lead to foolishly conservative strategies, as ordinary max-mining does in these circumstances. But, as we have seen, we never or rarely find systems problems in which the game element is completely absent.

There simply is no satisfactory general answer to the problem. Different people take different views of risks--in their own lives and as decision-makers for the nation. Some play boldly, some play for safety. So what does the poor systems analyst do? He frequently calculates some expected outcome or minimaxes or both, but in interpreting his results he is aware of their biases and inadequacies. But he doesn't stop there. There are other tricks of the systems analysis trade where uncertainties are grossly important:

- (a) He tries to invent a system which is as good or almost as good if overseas bases are available, but still pretty good if they aren't. We call a system which is best in any circumstances a "sure thing" or "dominant." We can seldom find a truly dominant

system, but sometimes we can come close. Such a system in the above example might be a bomber force which would normally use overseas bases, but if they were not available would have a substantial capability with air refueling from ZI bases. It is arguable that the most valuable function of systems analysis is the stimulation of systems invention.

(b) If he fails to find a dominant solution, he calculates the cost of providing insurance against the chance of catastrophe--e.g., by buying a mixed force with a substantial number of very long-range aircraft. Then the Air Force has to make a command decision, but at least it can do so knowing what the insurance costs.

(c) If he is concerned with development decisions, he recommends the development of aircraft and missiles only some of which depend on overseas bases. The situation may be clearer when decisions about quantity procurement have to be made several years hence. We can afford to develop more types of equipment than we procure in quantity, and given the relatively low cost of development, we ought to. Insurance is cheaper at this stage. The systems analyst must guard against the implicit "either/or." Mixed systems frequently are right for procurement, even more frequently right for development.

4. Explicit Treatment of Enemy Reactions

In some problems, what the enemy does is obviously crucial in making the right decision, e.g., the need for ECM, or the best allocation of funds between offense and defense. But it can also be crucial in many less obvious instances. For example, the choice between offensive bombers and missiles is extremely sensitive to their vulnerability to enemy attack--in the air and especially on

the ground. So there is great interest in developing models appropriate for problems of this kind.

Two kinds of models are available:

- (a) Game theory models.
- (b) Games (i.e., war games).

Game theory is a branch of mathematics which studies situations of conflict. Two-person constant-sum game theory models are useful for some simple military problems; but satisfactory theories for non-constant-sum and multi-person games have not been developed, and the whole theory is in its infancy. Moreover, the difficulty of calculating solutions to game theory problems severely restricts the number of variables which can be included. Consequently there is a strong tendency to substitute games for game theory, and games have been developed which, unlike traditional military war games, permit many plays, so that a number of possible strategies can be tested against enemy counter strategies. Games, like game theory, are not completely satisfactory, but for other reasons. Different players play differently--some probably too well. Sensitivity analysis is usually impossible. Results are therefore very difficult to interpret.

5. Explicit Treatment of Time Phasing

In many military decisions the sequence of events is of critical importance. For example, should we go into production now on some particular missile defense or wait two years until a better one is developed?

To handle such problems we need "dynamic" models, i.e., models in which the variables bear dates. We have such models, but introducing time explicitly is neither easy nor painless:

It complicates the computation by multiplying the number of variables. If we put time in we have to take something else out.

It complicates the selection of a criterion. Solution A may be better for '58, worse for '60; Solution B, vice versa.

It raises in acute form the question of our ability to predict, e.g., will a much better missile really be ready only two years later?

6. Broadening of Criteria

The selection of a criterion or criteria is frequently the central problem of the design of any systems analysis.

We have, characteristically, numerous alternative ways or "systems" for achieving our objectives (positive values). All involve costs of some kind (negative values). If we had some common measure for all the positive and negative values (as a business firm does, more or less), the answer to the criterion problem would be obvious. You would choose the system which yielded the greatest excess of positive values over negative ones.

Unfortunately, we can't do this. Objectives and costs are usually incommensurate. Objectives are likely to be such things as enemy targets destroyed, enemy planes shot down, probability that war will be deterred. Costs may be crews lost, aircraft lost, millions of budget dollars expended. I don't want to exaggerate. Frequently we can find common denominators, especially on the cost side. But also frequently, especially on the objectives side, things just won't add.

We have seen that in the typical World War II operations analysis problem fairly simple, obvious ways out of these difficulties could usually be found. So many things are fixed or given. Thus in the bomber formation example, we can choose the formation which maximizes target destruction for given aircraft

losses, or minimizes aircraft losses for given target destruction. These criteria, although they sound different, are logically equivalent, and so give the same answer (for the same scale of attack).

Consider, at the other extreme, the mixed force composition and development problem of determining the optimum composition of the Air Force, 1956 to 1966. What are Air Force objectives? It does little good to say that the Air Force objective is to promote such national objectives as to win a war if there is one, or to deter war. What kind of war? When? At what cost in lives and dollars? The optimal Air Force for fighting a thermonuclear war is not optimal for fighting peripheral wars. Concentration on missiles may be just right for our 1966 capability, but may weaken us for 1960. While our criteria must be consistent with national objectives, they must be defined much more concretely, or "operationally." No one has written (or will write) a real long-range war plane to guide us.

Of course, the criterion problem in the typical systems analysis is not as hard as in this case, which is extreme. It is extreme because a decision on force composition involves optimizing for the Air Force as a whole. It is a decision at the highest Air Force level. Decisions at this level are almost certain to require criteria based on complex and uncertain political and strategic factors. But many Air Force decisions, fortunately, can be made at lower levels--even force composition and development decisions. We can "factor out" a problem and the variables which are of first-order importance in its solution, ignoring the rest. And instead of optimizing for the Air Force as a whole, we "sub-optimize" for some sub-element of it, choosing an appropriate lower level criterion.*

*For a fuller discussion of this point see my earlier article, "Sub-Optimization in Operations Problems," Journal of the Operations Research Society of America, Vol. I, No. 3, May 1953, p. 87.

Distinguishing problems which we can successfully factor out is an art. Let me illustrate with an example from SAC operations. The choice between ZI and overseas bases (or what combination of both) won't factor. Overseas bases have tremendous ramifications throughout the Air Force and above and outside it. They have strategic and political values and vulnerabilities at other and higher levels than SAC operations that we would be stupid to ignore in making a decision. We can't sub-optimize, unless we frankly recognize that the sub-optimization deals only with some of the factors important to the decision.

On the other hand, given that we want at least part of SAC based in the ZI (and not even dependent on overseas staging), we think we can successfully factor out the problem: should range be extended by bigger, longer-range bombers, or by air refueling, or by what compromise between the two? This choice seems to have no first-order repercussions on air defense, on any theater operations, on international politics, etc. It can be sub-optimized by choosing as an appropriate lower level criterion the most efficient (in terms of target destruction) use of the budget or resources made available for strategic air.

But at best criterion selection is hard--harder in force composition than in operations problems, hardest in development problems. We have wrestled with criterion selection at RAND for eight years--with, at best, moderate success. We have found negative rules--criteria to avoid--but few positive ones of general helpfulness. There is no substitute for good judgment, and no substitute for exercising it. Working out a systems analysis with a bad criterion is equivalent to answering the wrong question. It is very easy to choose a criterion for a force composition or development problem that will insure our having the optimal system for the wrong war at the wrong time (to be fair, it is easy to make the same mistake without a systems analysis).

In some cases, indeed frequently, there is no single "right" answer to the criterion problem. The ultimate values of decision-makers differ as well as their image of the future. In these cases the systems analyst simply has to conclude: If you want an Air Force which will do A, choose Systems X; if B, choose System Y.

THE DIFFICULTIES ARE IN THE PROBLEMS

It is clear that despite our boldness and ingenuity in developing new techniques of analysis since the war, military systems analysis as it exists today is an imperfect instrument, an advisory art with many limitations.

Because we have so boldly tackled long-range, broad context problems of force composition and development, we have made drastic "factoring" and aggregation inevitable--with no good rules for either. We have learned to make extensive calculations of uncertain contingencies, but still don't know what to recommend (except as "sensible men") when, as is almost invariably the case, no dominant strategy emerges. We have developed some devices for dealing with enemy reactions and problems requiring time phasing, but only at the high cost of further factoring or aggregation and an apparent further complication of the already intractable criterion problem. In the matter of criteria we have to be satisfied with something practical which falls far short of perfection.

One may well ask, in view of this long catalogue of difficulties, dangers, and limitations, and the rather obvious possibilities of abuse they open up, whether military systems analysis is worth supporting and continuing.

The first thing to stress in answering this question is that almost all the difficulties we have discussed are inherent in the nature of the military problems that systems analysis is designed to help solve, and by no means peculiar to systems analysis, however we define it. This is obviously true of the "many variables" difficulty. In most of the military problems with which

we are concerned lots of things just are important. Systems analysis has trouble including all of them. But so does any other conceivable approach. Systems analysis is increasing its capability to handle larger problems every year. With present computers, for example, we can deal with far more intricate models than a genius can manage intuitively.

Uncertainties make life difficult for the systems analyst, but this is so because the problem of intelligent behavior under uncertainty is really hard. Systems analysis at least permits one to explore systematically the possibilities of dominance and the cost of insurance.

Enemy reactions are hard for a systems analysis to incorporate. Game theory is in its infancy. War gaming in any form has shortcomings. But why? Because the problem itself is so hard. The enemy probably doesn't know himself how he will act or react in 1960. How can we outguess him? Not by abandoning game theory and war gaming, which for all their limitations carry us further along this road than any devices yet conceived.

Time phasing is so hard that few systems analyses attempt it explicitly. Why? Because of two difficulties: the choice of criterion when payoffs and costs occur in different years, and the prediction month by month and year by year of changes in technology and other parameters. Both are intrinsic. You can get rid of them only by escaping from reality. If the sequence of developments, or their speed, is crucial to your problem, you need an analysis with dates attached to every variable to find the right answer.

But note what a dynamic systems analysis can do even in the case where we can't predict the speed of development with accuracy. It can tell us what the critical development speeds are--if $> X$, wait; if $< X$, go ahead--and thus enables us to focus the intuition of experts on a manageable technical problem. It can

even, in some cases, yield a surprisingly unequivocal answer despite rather gross uncertainties about speed of development. We have encountered such cases at RAND, in which, while postponement might have resulted in startling improvement in some performance characteristics, these were shown by systems analysis to have trivial military worth.

And finally, there are all those troublesome problems of criteria. I can only assure you, or remind you, that they are equally troublesome in policy discussions on the Air Council, in the Joint Chiefs of Staff, in the National Security Council, and in Congress. You don't escape from them by escaping from systems analysis. You may be able to fuzz them up or conceal conflicts in a clever debate or essay (you can do the same in a clever systems analysis), but they remain for a clever opponent to reveal. The fundamental difficulty is that there does not exist a clear cut, definitive, operationally meaningful statement of national objectives or of Air Force objectives--even for the present, let alone for 1960-1965.

THE POSITIVE SIDE

Before we can say anything in general about the usefulness of systems analysis we must know what we are contrasting it with. If we define systems analysis broadly to include the various game techniques, etc., discussed in the preceding sections, what are the alternatives?

Let's consider two. Concentrating on the first word "systems," the alternative is unsystematic or piecemeal consideration of problems.

Concentrating on the second word "analysis," the alternative is, I suppose, intuition.

1. Systems Versus Nonsystems

This distinction has nothing to do, necessarily, with analysis. It is a question of breadth of context. In principle, one can attempt to intuit answers in a broad or narrow context or use analysis in a broad or narrow context.

It would be foolish to maintain that broad systems contexts are good, narrow contexts bad. It all depends on the problem. Is it factorable or not? How factorable is it? Systems contexts can be too broad, and when they are, they are wasteful. You pay a heavy price for a broad context. For anything you put in an analysis, something must come out. The broader the context, the less detail. If you are a scientist trying to develop materials to withstand the heat of rocket engines, your chances of success will be reduced to the extent that you devote time and energy to pondering the relative likelihood of big and little wars. As a matter of historical fact almost all scientific and technological progress has been achieved within very narrow contexts--by scientists wearing blinders. Let's continue a fruitful division of labor and not all become systems analysts. Some of us are a little concerned that a large proportion of our best design engineers in aircraft companies seem to be spending their time designing systems analyses instead of aircraft.

Nevertheless, there are cases where the systems approach--the systematic examination of broad alternatives--throws a flood of light on important problems.

Our previous example is such a case. What methods of range extension should be used to enable U.S. bombers to reach targets deep in Russia? The broad alternatives are: overseas operating bases with medium bombers; overseas bases for staging only; big, very long-range bombers; mother-daughter arrangements; air refueling, etc. Because the alternatives are broad, they need to be examined in a broad context. When we do we discover that some

systems have a superiority of two to one to five to one over other plausible systems with enthusiastic advocates. This is a tremendously important conclusion. It could not have been reached, or at least not demonstrated, without a comprehensive systems approach and systems costing.

Let me give you another example of a different sort that cropped up in a RAND defense study of several years ago. It was at that time operational doctrine for certain interceptors to carry armament that, according to Air Defense Command estimates, gave each plane a fifty per cent probability of killing an intercepted bomber. Well, fifty per cent looked mighty good to most experienced Air Force hands. It was lots better than anything achieved in World War II.

What did we find when we examined this doctrine in a systems context?
Essentially:

(a) As was not the case in World War II, we were really preparing for defense against one (or at most a very few) massive atomic strikes.

(b) The total systems cost of procuring and operating the interceptors--to get them into position prepared to fire a rocket at an incoming bomber--was extremely high, so high that the most lavish expenditure on armament scarcely affected the total.

(c) It was therefore obvious nonsense to economize on armament.

(d) The single pass kill probability and the kill potential of the defense system could be increased by nearly fifty per cent simply by increasing the armament. The performance degradation of the interceptor resulting from increased weight was of the order of five per cent--which, at least in the period of interest--had a negligible impact on kill potential.

Now this again was a tremendously important result of looking at a problem in a systems context. A systems analysis wasn't really necessary. I am sure that some Air Force officers, using a broad systems context, thought their way through this one and reached the right conclusion without so much as using the back of an envelope. But many apparently didn't, and the doctrine wasn't changed until the systems analysis was produced and presented. Systems analysis forces both the systems analyst and his audience to think the problem through in a systems context.

2. Analysis Versus Intuition

Let us turn to the second part of my comparison: analysis versus intuition.

The main point I want to make might be called the inevitability of analysis. What we call intuition is a species of logical analysis. It uses models, in our sense of simplified conceptual counterparts of reality. Not surprisingly, in military problems as in so many others, it is sometimes useful to buttress our feeble minds with some external assistance: a pencil and the back of an envelope; a few equations; a desk calculator; sophisticated statistical and mathematical theory; high-speed calculators.

How far you go with such aids depends on the problem. But very frequently they enable you to find a solution you couldn't otherwise find, or to demonstrate that your intuitive solution was wrong--or, what is sometimes as important, right.

I am not selling intuition short. The unaided human mind is quite remarkably proficient at solving some kinds of problems. Let me remind you:

- (a) Some human beings play very good games of chess. No machine can yet give them a match.

(b) Human beings at RAND with intuition, pins, and a piece of string found the optimal route in the Traveling Salesman problem out of 10^{62} possible routes.

(c) On the evening of last November second the Columbia Broadcasting System used two methods for predicting the election results from the very early returns.

(1) The intuition of assorted political experts.

(2) A complex multi-variable model calculated on a UNIVAC.

The UNIVAC was grotesquely wrong: the experts did not do too badly.

The human mind has some great advantages over any machine--if we think of them as rivals or alternatives. It has, by comparison, a wonderfully capacious memory, which enables it to learn from experience. It has a marvelous facility for factoring out the important variables and suppressing or aggregating the rest. Closely related to this facility, it can build models highly appropriate to the particular problem it is considering. Big formal models computed on machines are much less flexible. These are the reasons human beings beat machines at chess or war games.

But, on the positive side:

(a) It is utterly wrong to look upon intuition and analysis or minds and machines as rivals or alternatives. Properly used, they complement each other. We have seen that every systems analysis is shot through with intuition and judgment. We have experimented at RAND with man-machine combinations which will play war games better than either men or machines.

(b) While unaided intuition is sometimes strikingly successful, as it was last November second, or in the Traveling Salesman

problem, it can also fall flat on its face. For example, in the election of November, 1952, when the UNIVAC was dead right, and the intuition of the experts so wrong that they suppressed the UNIVAC answer.

In contrast to the Traveling Salesman problem, try your intuition on this: there are 25 persons in a group. What are the chances that at least two have the same birthday? Almost everyone without statistical training says--very small. In fact, they are better than even. If there are sixty in the group, the chances are 99.4 per cent!

(c) One of the troubles with intuition is that you never know whether it is good or not without an analytic check. For example, our intuition was good enough to solve the Traveling Salesman problem, but we didn't know it until we solved it analytically in a linear programming formulation.

MATS didn't know its assignment of aircraft to routes was within five per cent of optimal until we worked out a linear programming solution.

I've said there are good chess players. But we don't really know. Maybe even the best are as far from optimal strategies as expert opinion so frequently has been on military problems.

(d) Finally, analytic and computing techniques enable us to do things we otherwise couldn't. They may be poor on the memory side, but they have some capabilities unaided human minds don't.

Look again at the UNIVAC fiascos on election eves, 1952 and 1954. Here we had an elaborate model and a high-speed computer. In 1952

it was able to take the first few precinct returns from eastern states and trace their consequences--on its built-in assumption that similar trends were running in all precincts throughout the nation. They were, and the answer was dead right. The experts couldn't carry out such a calculation in their heads, and were inclined, like most experts, to err on the "conservative" side.

Or look at a different kind of example--of insights derived from theory. Take a brand new theory--one that I described as in its infancy and of very limited usefulness--viz., game theory.

In connection with RAND defense studies we have long been interested in the optimal deployment of limited defenses among targets, some of which are more valuable than others. Unfortunately, we have found no satisfactory general rule for deploying defenses, but game theory has given us valuable insights and hints about good and bad deployments we would not otherwise have had.

One striking example: Suppose you have your defenses deployed as well as you can. Now you get more defenses. How do you deploy them?

Well, my intuition told me (and so did most people's) that you deploy them mainly to protect additional targets--additional cities, harbors, airbases, etc., that you didn't previously have enough stuff to defend.

Game theory says no. You use additional defenses mainly to increase the defense of targets already defended. In fact over a wide range, the more you have the more you concentrate it.

Having been informed of this startling result, you think about it and begin to see the rationale. An increase in your defensive strength is equivalent to a decrease in the enemy's offensive strength. But as his strength decreases, he has to concentrate more and more on your most valuable targets to achieve anything worth while. These are the ones on which you therefore have to concentrate your defense.

But intuition alone would not have told us this. At any rate, not unequivocally enough to lead one to act on it.




CONCLUSION

Does analysis help more in the narrow context problems, where it has commonly been applied by scientists, or in the broad context problems, which are the special province of systems analysts?

~~I don't know.~~ On the basis of results, certainly one would have to say that the case for analysis in broad context problems is comparatively unproved. ~~Let me,~~ ^{it is} however, ^{that} suggest one reason why, when we are dealing with broad problems with broad systems analyses, explicit analysis using explicit models can be especially important.

We trust a man's intuition in a field in which he is expert. But in these cases we are dealing with a field so broad that no one can be called expert. A typical systems analysis depends critically on numerous technological factors in several fields of technology; on military operations and logistics factors on both our side and the enemy's; on broad economic, political and strategic factors; and on quite intricate relations among all these. No one is an expert in more than one or two of the sub-fields; no one is an expert in the field as a whole and the interrelations. So no one's unsupported intuitions in such a field can be trusted.



Systems analyses should be looked upon not as the antithesis of judgment but as a framework which permits the judgment of experts in numerous sub-fields to be combined--to yield results which transcend any individual judgment. This is its aim and opportunity. ~~✗~~ ↑

But we still have the question: Where is the "expert" in the field as a whole with the judgment required to design a systems analysis and interpret its results? We know there are not any real experts. But we think we can demonstrate that the degree of expertness required to design a systems analysis is less than the degree of expertness required to intuit a good answer without a systems analysis.

Let me put it in another way. We tend to be worse, in an absolute sense, in applying analysis or scientific method to broad context problems; but unaided intuition in such problems is also much worse in an absolute sense. Let's not deprive ourselves of any useful tools, however short of perfection they may fall.