THE ANALYTICAL BASES OF SYSTEMS ANALYSIS

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INTRODUCTION

My assigned topic is "The Analytical Bases of Systems Analysis." This subject may be approached from several points of view. The usual one, I suppose, is to translate "analytical bases" into "tools and techniques," and then to proceed to talk about linear programming, Monte Carlo techniques, computer simulation models, and the like. Since a lot has already been written and said on these subjects, I prefer to take a different tack.

I want to focus on a discussion of the role of analysis in the decisionmaking process. This is important because different people in the analytical community have differing views on the matter and depending on which view is held, one can arrive at various alternative conclusions regarding the conceptual and procedural bases for analysis in support of the decision process. Let me say at the onset that my views are probably somewhat controversial, and that no doubt many attendees at this symposium will tend to take issue with some of my arguments. This, however, should help stimulate lively discussion!

Let me also emphasize at this point that my remarks are focused primarily on a long-range-planning military decision context; but hopefully many of the points made will have more general applicability.

WHAT IS SYSTEMS ANALYSIS?

Before launching into a discussion of the role of systems analysis in the long-range-planning process, I think I had best take a few

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moments to tell you what the term "systems analysis" means to me. This seems necessary because the term itself apparently has various meanings today; and also there are other terms which are sometimes used as being synonomous with systems analysis: e.g., cost-effectiveness analysis, cost-benefit analysis, operations analysis, and the like. Now I do not want to get tangled up in a semantics jungle here. So let me just say that in the context of my talk, systems analysis is an analytical process having the following major characteristics:

(1) A most fundamental characteristic is the systematic examination and comparison of alternative courses of action which might be taken to achieve specified objectives for some future time period. Not only is it important to systematically examine all of the relevant alternatives that can be identified initially, but also to design additional ones if those examined are found wanting.* Finally, the analysis, particularly if thoroughly and imaginatively done, may frequently result in modifications of the initially specified objectives.

(2) Critical examination of alternatives typically involves numerous considerations; but the two main ones are assessment of the cost (in the sense of economic resource cost) and the utility (the benefits or gains) pertaining to each of the alternatives being compared to attain the stipulated objectives.

(3) The time context is the future--often the distant future (five, ten, or more years).

(4) Because of the extended time horizon, the environment is one of uncertainty--very often great uncertainty. Since uncertainty is an important facet of the problem, it should be faced up to and treated explicitly in the analysis. This means, among other things, that wherever possible the analyst should avoid the exclusive use of simple expected value models.

(5) Usually the context in which the analysis takes place is fairly broad (often very broad) and the environment very complex with

*E. S. Quade, Military Systems Analysis, The RAND Corporation, RM-3452-PR, January 1963, p. 1.

numerous interactions among the key variables in the problem. This means that simple, straightforward solutions are the exception rather than the rule.

(6) While quantitative methods of analysis should be utilized as much as possible, because of items (4) and (5),* purely quantitative work must often be heavily supplemented by qualitative analysis. In fact, I stress the importance of good qualitative work and of using an appropriate combination of quantitative and qualitative methods.

(7) Usually the focus is on research and development and/or investment type decision problems, although operational decisions are sometimes encountered. This does not mean, of course, that operational considerations are ignored in dealing with R&D and investment type problems.

THE ROLE OF SYSTEMS ANALYSIS IN THE LONG-RANGE-PLANNING DECISION PROCESS

Given this general conception of systems analysis, let me now turn to a discussion of the role of analysis in the long-range planning-decision process.

I suppose, as analysts, we would always like to try to come up with "preferred solutions" when studying alternative future courses of action. Ideally this means determining "the optimum"--that is, the point on some well-defined surface where all the partial derivatives are equal to zero and the appropriate second order conditions prevail. I submit, however, that in most of today's long-range-planning decision problems of any consequence, it is rarely possible to even approach anything like a hard core optimization. Most likely we will be lucky if we can get some notion as to the signs of the partial derivatives--i.e., whether we are moving "up the hill," so to speak, toward the saddle point in a maximization problem, or away from the saddle point ("down hill"). In fact, I would even argue that in most studies that I have worked on in recent years, it is often difficult to determine

*And also because of inadequate data and information sources.

what "hill" we are on, or should be on! This rather crude analogy begins to convey the flavor of my thoughts on the role of analysis in the long-range decision process. Let me now be more specific.

Here I shall take as a text for my remarks the following statements by the Assistant Secretary of Defense, Systems Analysis:

> Ultimately all policies are made ... on the basis of judgments. There is no other way, and there never will be. The question is whether those judgments have to be made in the fog of inadequate and inaccurate data, unclear and undefined issues, and a welter of conflicting personal opinions, or whether they can be made on the basis of adequate, reliable information, relevant experience, and clearly drawn issues. In the end, analysis is but an aid to judgment ... . Judgment is supreme.*

> The analyst at this level is not computing optimum solutions or making decisions. In fact, computation is not his most important contribution. And he is helping someone else to make decisions. His job is to ask and find answers to the questions: "What are we trying to do?" "What are the alternative ways of achieving it?" "What would they cost, and how effective would they be?" "What does the decisionmaker need to know in order to make a choice?" And to collect and organize this information for those who are responsible for deciding what the Defense program ought to be.**

The Assistant Secretary's remarks pretty much reflect my own views on the subject. I would put the argument in the following manner:

Contrary to what some of the more enthusiastic advocates of quantitative analysis may think, I tend to visualize systems analysis as playing a somewhat modest, though very significant, role in the overall decisionmaking process. In reality most major long-range-planning decision problems must ultimately be resolved primarily on the basis of intuition and judgment. I suggest that the main role of analysis should be to try to sharpen this intuition and judgment. In practically no case should it be assumed that the results of the analysis will

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*A. C. Enthoven, quotation contained in an article in Business Week, November 13, 1965, p. 189.

**A. C. Enthoven, article in The Armed Forces Comptroller, Vol. IX, No. 1, March 1964, p. 39.

"make" the decision. The really critical problems are just too difficult, and there are too many intangible (e.g., political, psychological, and sociological) considerations that cannot be taken into account in the analytical process, especially in a quantitative sense. In sum, the analytical process should be directed toward assisting the decisionmaker in such a way that his intuition and judgment are better than they would be without the results of the analysis.

We might say that there are two extreme positions regarding the role of analysis in the decisionmaking process. On the one hand, one might argue that the types of long-range-planning decision problems under consideration here are just too complex for the current state of analytical art to handle. Decisions must be made purely on the basis of intuition, judgment, and experience: i.e., the zero analysis position. At the other extreme are those who tend to think that all problems should be tackled in a purely quantitative fashion, with a view to essentially "making" the decision. Such a view implies explicit (usually meaning quantitative) calculations of cost and utility for all the alternatives under consideration. This may be possible, at times, for very narrowly defined, low level sub-optimization problems; but even this is questionable.

More generally, in dealing with major decision problems of choice, if the analyst approaches the analytical task in an inflexible "hard core" frame of mind, he is likely to be in for trouble. For example, he may soon give up in complete frustration, he may wind up with such a simplified model that the resulting calculations are essentially meaningless, or the result might be that his conclusions are presented two years after the critical time of decision and therefore useless to the decisionmaker.

My viewpoint is that in most cases the relevant range is between the extremes mentioned above, and that in such a context there is a wide scope of analytical effort that can be useful. Furthermore, even when only a relatively incomplete set of quantitative calculations of cost and utility can be made (probably the general situation), much can be done to assist the decisionmaker in the sense that I am using the

term assistance. To repeat: The objective is to sharpen intuition and judgment. It is conceivable that only a small amount of sharpening may on occasion have a high payoff.

One other point seems relevant here. In that rare circumstance when a fairly complete set of calculations of cost and utility is possible and a resulting conclusion about a preferred alternative reached, it just may be that the conclusion itself may not be the most useful thing to the decisionmaker. In the first place, as pointed out earlier, the analysis usually cannot take everything into account--particularly some of the nebulous non-quantitative considerations. The decisionmaker has to allow for these himself. But more important, most high-level decisionmakers are very busy men, with the result that they do not have time to structure a particular problem, think up the relevant alternatives (especially the subtle ones), trace out the key interactions among variables in the problem, and the like. This the analyst, if he is competent, can do, and should do. And it is precisely this sort of contribution that may be most useful to the decisionmaker. The fact that the analysis reaches a firm conclusion about a preferred alternative may in many instances be of secondary importance.

WHY IS OPTIMIZATION SO DIFFICULT?

At this point you may well ask the question: "Why is hard core optimization so difficult for the class of decision problems under consideration here?" Part of the answer, of course, is that in dealing with long-range-planning problems major uncertainties are always present; and the theory of choice under conditions of uncertainty does not always give us a definite set of rules to follow. Should we use expected values and ignore variances? Should we take variances into account; and if so, how? Should we use a minimax rule? And so on.

Another part of the answer concerns the basic nature of the decision questions themselves--their complexity and the scenario dependency of assumptions that must be made to formulate and to deal with the questions. Let me illustrate this point by referring to an area where in the past some of us thought that something approaching

optimization might be attained. (We could have been wrong, of course; and I suspect we were!) I have in mind the general war problem area.

In the past--and still to a large extent today--general war decision problems have been formulated in terms of a "spasm response" scenario; and analyses of these problems have for the most part been conducted in that context. Spasm response involves a fairly mechanistic set of considerations. One side lets go with all (or a major part) of his strategic forces, and the other side retaliates in kind. Many of the major facets of this problem can be modeled, and numerous sets of quantitative results can be calculated--and have been calculated. Now I do not suppose any of us would argue that we have ever attained "hard core" optimization in dealing with the spasm response case. There are still major uncertainties involved. But I do think some of us thought we were moving in that direction.

Now let us see what happens when we move away from the spasm response case (today usually called the pure assured destruction case) and begin to consider other strategies and scenarios. While not at all underplaying the importance of having an assured-destruction deterrent capability, many people today feel that in addition, other concepts of general war involving controlled response capability should be seriously considered. The main idea here is that in future time periods it might be desirable, even mandatory, that the national leaders have a wider range of options available to use in dealing with crisis situations--that is, a wider range than that available from a force mix tailored primarily to the notion of spasm response. Examples of these other options may be summarized under headings like:

(1) Damage limiting capability.
(2) Coercion and bargaining capabilities to be used in an escalation process stemming from a crisis situation.
(3) Intrawar deterrence of countervalue exchanges.
(4) War termination.

In addition, we have the problem of the proliferation of nuclear weapons in the future--the "N-country" problem--and the question of what this means for the future force posture of the United States.

Now I obviously cannot get into a detailed discussion of these topics here. Each is a complex subject in itself. But the main point I am trying to make is, I think, clear. Once controlled response strategies and scenarios are taken into consideration, the analytical problems increase astronomically when compared with the spasm response case. The uncertainties compound, scenario dependencies abound, a force mix that might seem preferred in one case might not look so good in another, non-quantifiable variables (e.g.,political and psychological factors) are just very important, and the like. In sum, any notion of anything approaching hard core optimization goes out the window and we begin to wonder whether classical systems analysis can contribute very much in dealing with problems of this kind.

Perhaps something can be done in the way of very low level sub-optimizations for small pieces of the total problem. And some of this may be useful to a limited extent. But the long-range force planners have to grapple with the general war force mix problem (including force size) for future time periods; and the real question is whether analysis of some kind can sharpen the intuition and judgment of the decision-makers in this complicated area.

I think the answer is probably "yes," but I confess that at this time I cannot be specific as to how analysis might help. At RAND we have just recently launched a major study of the whole question of controlled response in general war. Initially we shall probably experiment with various combinations of the more conventional methods, including war gaming (manual), war game simulations, classical systems analysis, and the like. Here we recognize that any one method alone will not do the job, and we also realize that the political scientists will have to provide a substantial input, particularly in the form of a menu of rich scenarios of various controlled response environments. Our main goal, of course, is to try to come up with some new concepts, methods and techniques of analysis, as well as to say something substantive about the complicated issues involved. We may fail, of course, but we think it is very important to give the problem a good try.

Let us turn now to a different problem area: the question of the mobility of the general purpose forces. Here again is a case where one might, at first glance, think that something approaching a reasonably good optimization might be possible. I haven't time to even outline all of the issues involved in this problem. So I shall discuss a few of them (staying within the bounds of an unclassified discussion) to illustrate my point.

Until recently most studies tended to focus on the "big lift" part of the total problem--that is, the intercontinental transportation question. The central issue here, of course, is the preferred mix of airlift, sealift, and prepositioned supplies and equipment. Some very good work has been done in this area, and I think something fairly close to good <u>suboptimizations</u> has been attained. However, when one begins to think more deeply about the <u>total</u> problem--the problem the force planners have to grapple with--then questions begin to arise.

Two key factors in the big lift problem are: (1) the high cost of airlift vs. sealift and prepositioning; and (2) the payoff in terms of <u>very</u> rapid response time available from force mixes containing a relatively high proportion of expensive airlift capability. So the question of the value of very rapid response is a dominant consideration. However, if one wants to get serious about delving into the matter of quick response, it is immediately obvious that the boundaries of the original problem have to be broadened. Total response time is made up of intra-Z.I. mobility and intra-theater (or objectives area) mobility in addition to the big lift. And there are <u>interactions among all three</u>. So we have to look at the total before we know what kind of a response we really have for various alternatives. Here the problem begins to get very complicated. For example, when the intra-objectives area is added to the analysis, things get particularly messy. The ground battle cannot be ignored, nor can the questions of re-deployment and resupply. Furthermore, the final outcomes are just <u>very scenario dependent</u>.

Although I have barely scratched the surface, I think I have said enough to illustrate my point: that hard core analysis of the mobility problem facing the long-range planners is very difficult--

particularly in an optimization sense. Does this mean that all the study effort expended in this area to date is worthless to the decision-makers? I think nothing could be further from the truth. Recently, some very interesting work has been done on the intra-theater (or objectives area) mobility problem, to supplement the studies already done in the big lift area. While no over-all "preferred solutions" have been forthcoming, these studies have provided major insights into the key variables involved, some of the more important interrelationships among the variables, the sensitivity of results to variations in key parameters and assumptions, and the like. As a result, I feel that the decisionmakers have a much better basis for their judgments regarding future mobility force mixes than they would have had without the studies. But in my view, this is the real value of analysis. That is my main point.

WHAT CAN BE DONE?

If you agree with even half of what I have said up to this point, you may wonder about whether systems analysis can contribute very much to the long-range planning process in the area of national security--or any other realm, for that matter. Perhaps systems analysts had best apply for job re-training and transfer to other occupations!

I do not think such a conclusion is warranted. Given the appropriate view regarding the role of analysis in the planning decision process, I think that the analyst can pull his own weight many times over in assisting the decisionmakers to sharpen their intuition and judgment. Let me try to illustrate my point by offering some simple examples.

For these examples, I have deliberately chosen "sticky" (but not atypical) problem areas. I have also deliberately chosen what some people might call fairly "low level" examples of analytical work in these problem areas. Actually, I think that in many cases, much more in the way of analysis can in fact be done than is indicated in my illustrations. However, I want to try to make an *a fortiori* argument, so to speak. If I can show that relatively simple type analyses can

be useful, then I shall be well on the way toward demonstrating my point without having to resort to arguments based on more sophisticated forms of analytical effort. Another point is relevant here. In most fast-moving decision environments, the analyst is quite often called on to try to do something useful in a relatively short period of time. Usually this means that he does not have time to structure and carry out a complicated, complete analysis of the problem. He will have to settle for much less if he is to have any impact at all on the decision process.

For a change of pace, let us start out with an example outside the national security area. Suppose that we are concerned with deciding among alternative proposed water resources projects, and that we have a given budget to spend on such projects in the future. The budget is such that all of the proposed projects cannot be undertaken. We therefore want to choose the "preferred mix." Suppose further that we have an analytical staff and that it comes up with a summary of results of systems analyses of the problem in the following format:

Analytical Factor	Proposed Projects 1	2	3	4	...	n
(1) Present worth[a] ($):						
(a) Discounted @ 2½% (50 yr)						
(b) Discounted @ 5 % "						
(c) Discounted @ 8 % "						
(2) Possible variability of outcome:						
(a) "Most likely" range of present worth (low-high $)						
(b) Range of present worth outside of which outcome is "very unlikely" to fall						
(3) Effect on personal wealth distribution:						
(a) Number of farms affected						
(b) Average value of land and buildings per farm in the watershed ($)						
(c) Average net benefit per farm owner ($)						
(4) Effect on regional wealth distribution:						
(a) Average increase in per family income in the Basin ($)						
(b) Percentage increase in average income in the Basin due to project						
(5) Internal rate of return of project (%)[b]						

[a] Present value of estimated benefits minus present value of estimated costs.

[b] The rate of discount which reduces present worth to zero.

Assume that in addition to the quantitative data presented in the table, the analytical staff has supplemented the numerical calculations with qualitative discussion of some of the more relevant non-quantifiable issues involved in the decision: e.g., political factors, non-quantifiable "spillover" effects, and the like.

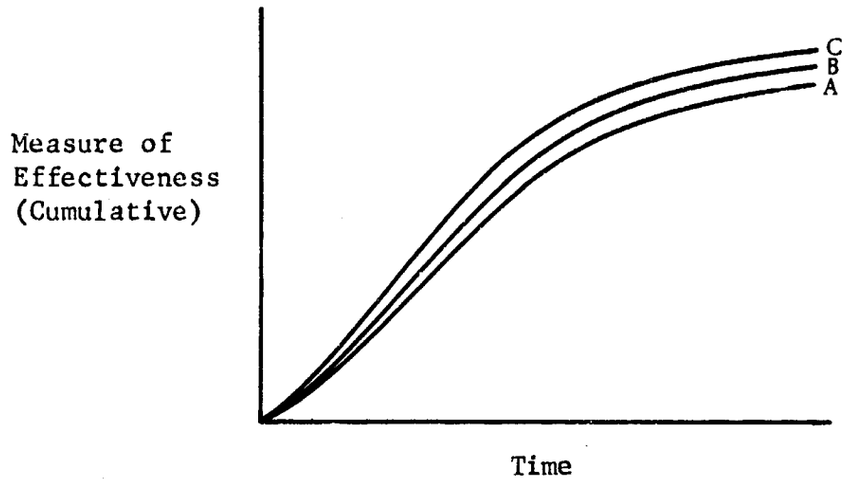
Now decision problems regarding alternative water resources projects are usually very complex. The analyst can rarely come up with a preferred solution--particularly in the sense that one mix of alternatives completely dominates all others. I submit, however, that even in such a context, analytical results of the type portrayed above can go a long way toward sharpening the intuition and judgment of the decisionmakers. I think you will all agree that in the above illustrative case, the decisionmakers would be better off if they had the results of the analytical effort than if they did not have such information. Their decision is likely to be a more informed one.

Let me now turn briefly to another example to illustrate a somewhat different point. Actually, I want to show two things with this example: (1) how wrong conclusions can be drawn from a systems analysis--particularly in the face of uncertainty; and (2) how the results of the same study can be interpreted differently, and how the second interpretation can be of assistance to the decisionmakers.

I have in mind here the results and conclusions of a study that was actually performed by one of the military departments in the Department of Defense and submitted to the Secretary of Defense and his staff. We need not get into the substantive national security issues involved in the study to illustrate the methodological points that I want to emphasize.

The structure of the analysis was an equal cost comparison of several alternative future courses of action; that is, for a specified budget level to be devoted to a particular military mission area, the alternatives were compared on the basis of their estimated effectiveness in accomplishing the stipulated task. The final quantitative results took the following form.

Comparison of Equal Cost Alternatives



The stated conclusion of the study, based almost exclusively on these quantitative results, was that alternative C is preferred over A and B for a wide range of circumstances and contingencies. (The context of the study, I should point out, involved a time period some 10 to 15 years into the future.) Yet the differences in estimated effectiveness of the alternatives (for a constant budget level) was at most 15 percent! Now my point is simply that the context of the problem was clouded by so many uncertainties and the model used in the analysis was so aggregative, that calculated differences among the alternatives averaging less than 15 percent just cannot be regarded as significant. Thus, the stated conclusions of the study, if taken literally, could in a real sense be misleading to the decisionmakers.* In decision problems of this type where uncertainties are very great, the analyst is generally looking for much larger differences among the alternatives being examined. How great? There is no general rule. However, I can say that in the past when experienced analysts have been dealing with problems of this type, differences in the neighborhood of a factor of 2 or 3 have been sought. I personally feel that in most

*Needless to say, the Secretary of Defense and his analytical staff were not misled in this case. They are too experienced in interpreting the results of analytical studies to be overly impressed by small differences.

long-range planning problems where major uncertainties are present, quantitative differences among alternatives must be at least a factor of two before we can even begin to have any confidence that the differences are significant. In any event, when they are smaller than that, the analyst must exercise extra caution in interpreting the results, and he must not make statements that are likely to mislead the decisionmakers.

There is another side to the coin, however. When quantitative differences among alternatives fall within a relatively narrow range, does this mean that the study is of no use to the decisionmaker? Not necessarily. If the quantitative work has been carried out in a reasonably competent manner and the differences among alternatives do tend to be relatively small, this fact in itself can be of considerable interest to the decisionmaker. This is especially true if sensitivity analyses have been made showing that as key parameters in the problem are varied over their relevant ranges, the final results are still within relatively narrow ranges. Given results of this kind, the decisionmaker can be less concerned about making a mistake regarding the quantitative aspects of the problems, and he may then feel somewhat more comfortable about focusing more of his attention on the qualitative factors--political, psychological, sociological considerations. In fact, if the analyst has done a reasonably thorough job, he might include a discussion of these factors in a qualitative supplementation to the purely quantitative part of the study.

The main point here is that while one of the main goals of analysis is to search for "preferred alternatives" characterized by quantitative results significantly different (better) from other alternatives, the fact that a strong case cannot be made for a preferred alternative does not mean that the study is worthless. The results, and the sensitivity analysis supporting the results, can still be very enlightening to the decisionmaker. And again I emphasize that this is the main purpose of analysis.

As a final example, let us consider a military decision environment where the analyst is called upon to come up with something in a relatively short period of time in a rather complex problem area. The question is what can be done, if anything? If we take the position that the objective is to provide something that will help sharpen the decisionmakers' intuition and judgment, I think a great deal can be done. Something far short of a type of analysis involving a relatively complete set of calculations of utility and cost may be very useful. For one thing, a mere enumeration of all the relevant alternatives may be very helpful. If the analyst can go beyond this and furnish data and information bearing on utility and cost of these alternatives, so much the better.

One thing that can be done is to develop summary analyses of cost and utility and present them along with a qualitative statement of some of the key implications. Examples are contained in Fig. 1 and Table 1.

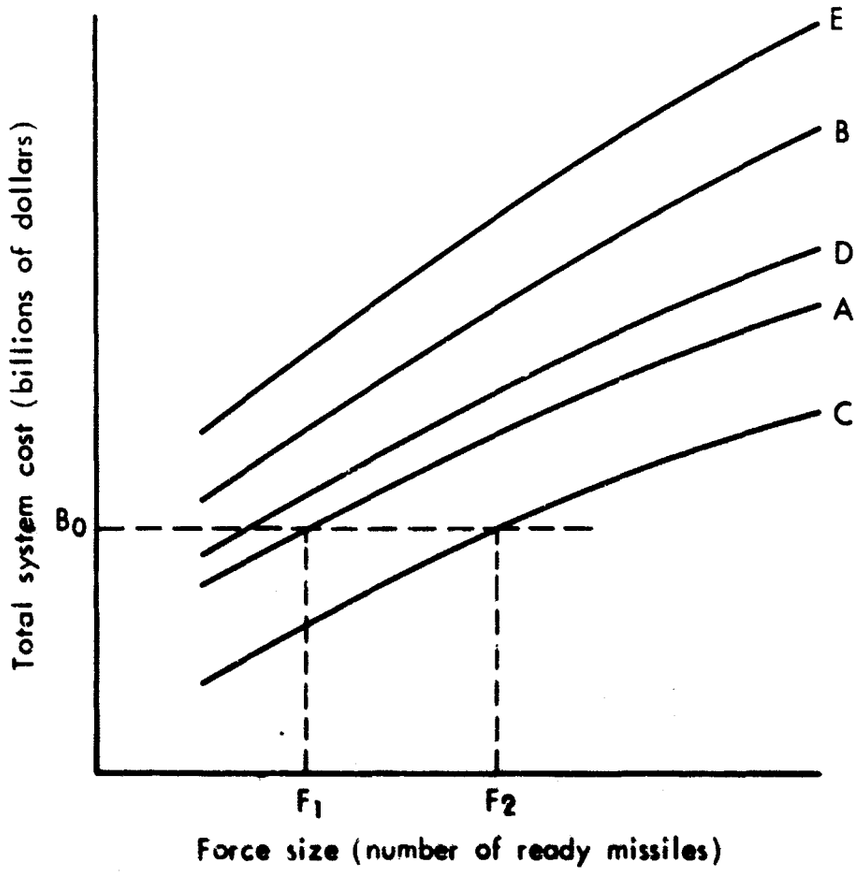


Fig. 1—Total system cost versus force size for alternative systems A, B, C, D and E

Table 1

SELECTED DATA BEARING ON UTILITY CONSIDERATIONS FOR ALTERNATIVE SYSTEMS A, B, C, D AND E

Description	Alternative System A	B	C	D	E
Quantitative Information					
Effective range (n mi)					
Cruise speed (kn)					
Penetration speed (kn)					
Warhead yield (MT)					
Circular error probability (CEP)					
Single-shot-kill probability					
Against soft targets					
Against hard targets					
Extended strike option time (days)					
. . .					
etc.					
Qualitative Information[a]					
"Show of force" capability					
Multi-directional attack capability					
Ground vulnerability					
In-flight vulnerability					
Controlled response capability					
. . .					
etc.					

[a]Some of these items have quantitative aspects to them; but they are very difficult to assess in a study with a short time deadline.

Figure 1 shows total system cost (research and development, investment, and operating cost*) vs. force size for several alternative systems. Here some of the alternatives are ground-based missile systems, others are airborne-alert, long-endurance-aircraft systems, with the aircraft serving as missile-launching platforms. In the case of the missile systems, force size means number of missiles in position ready to go. For the aircraft platform systems, force size means number of missiles continuously airborne on station ready to go.

*Operating cost is usually computed for a fixed period of years--say, 5 or 7.

Used in conjunction with data pertaining to utility (as in Table 1), system cost vs. force size curves can be useful. For example, suppose that alternatives A and C are in the same ball park with respect to certain key utility variables--say, penetration capability and single-shot-kill probability--but that C is clearly more vulnerable to an initial enemy strike than is A. The difference in the system cost curves for A and C in Fig. 1, then, essentially represents what we pay for getting reduced vulnerability. But there are other ways to play this game. Suppose the decisionmaker has a given budget (B_o in Fig. 1) to spend for supplementation of the already planned strategic forces. For B_o he can get a force size of F_1 for alternative A, or a much larger force (F_2) of system C. He may judge that the larger force of C may more than compensate for its higher vulnerability. Or he may decide that F_2 of C is roughly equivalent to F_1 of A and decide to go for C for other (qualitative) reasons: e.g., C may have more of a show-of-force capability than A, or be preferable from a controlled response point of view.

In any event, the decisionmaker is clearly in a better position to sharpen his intuition and judgment if he has the benefit of Fig. 1 and Table 1 than if he did not have them.* This is an illustrative example of what was meant earlier when I indicated that there are numerous things that can be done between the extremes of no analysis whatever and "hard core" cost-utility analysis. The above example is certainly far short of the latter; but it nevertheless may be useful.

CONCLUDING REMARK

I see that my allotted time has expired. I hope that my remarks have served to sharpen your intuition and judgment regarding the analytical bases of systems analysis. Thank you.

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*It is assumed, of course, that a textual discussion goes along with the figure and the table, so that the decisionmaker has the benefit of any interpretive comments that the analyst may have.

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