SOME COMMENTS ON SYSTEMS ANALYSIS

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INTRODUCTION

Our concern in this session today is the current state of systems analysis and its possibilities for the future, including potential problem areas. The primary focus is the Department of Defense, so my remarks to follow will be confined exclusively to the national security context. However, some of the subject matter may have some applicability to other problem areas.

First, let me try to make clear what systems analysis means to me, because the term has different meanings for different people, and its role in the decision process is viewed in a variety of ways--even among the practitioners themselves. These differing conceptions of what the subject is and what it is supposed to accomplish can in turn lead to differing assessments of the current state of affairs and the prospects for the future.

WHAT IS SYSTEMS ANALYSIS?

Let us start out by listing a few of the major characteristics of systems analysis:

(1) A fundamental characteristic is the systematic examination of objectives in a given problem area and of the alternative ways of achieving these objectives.

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(2) Systems analysis typically involves an iterative process of formulation, testing, reformulation, re-testing, and so on. If, as is often the case, the process does not generate a "preferred" set of alternatives, the results may nevertheless be very useful to the decisionmaker--for example in the form of re-thinking and clarification of 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--usually great uncertainty, which must be faced and treated explicitly in the analysis. This means that the analyst should avoid the exclusive use of simple expected value models. Such techniques as sensitivity analysis, _a fortiori_ analysis, and contingency analysis should be utilized wherever possible.

(5) Usually the context in which the analysis takes place is fairly broad (often very broad) and the environment very complex, with numerous interactions among the key variables in the problem. This means, on the one hand, that simple, straightforward solutions tend to be the exception rather than the rule. On the other hand, even the most comprehensive systems analysis can never be really complete. But an analysis does not have to be complete to be useful.

(6) While quantitative methods of analysis should be utilized as much as possible, many facets of a typical systems analysis problem cannot be quantified. Thus, purely quantitative work must often be supplemented by qualitative analysis. I stress the importance of _good_ qualitative analysis and of using an appropriate combination of quantitative and qualitative methods.

So much for the characteristics of systems analysis. Let me now list a few points concerning what systems analysis is _not_:

First and foremost, it is not a panacea for solving the major resource allocation problems in the Department of Defense or any other part of the government. It cannot possibly replace the contextual intuition and judgment of the decisionmakers and/or planners. The primary objective of systems analysis is to enhance that intuition and judgment--especially with respect to the quantifiable nucleus of the decision process.

Second, systems analysis is not a science in the strict sense of the word, and it is not likely to become one in the foreseeable future. It is essentially an art; but in practicing this art a serious attempt is made to use scientific concepts and modern analytical methods and techniques in the process of examining alternative future courses of action.

Third, systems analysis is not an activity unduly dominated by economists and mathematicians preoccupied with building esoteric models and playing endless series of numbers games on computers. Economists and mathematicians do typically make major contributions to the systems analysis process; but so do members of other disciplines--engineers, physicists, military strategists, political scientists, and the like. Quantitative analyses are made, but this does not mean that systems analysis tries to assign numbers to every facet of a problem area. Computers are often used, but their role is restricted largely to easing the burden of computation--particularly where large numbers of alternatives have to be examined, and/or where it is necessary to test the sensitivity of final results to values of key input parameters about which we are uncertain. The latter is especially significant, since we must almost always engage in a sensitivity analysis in our all important search for dominant solutions.

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

Given this general conception of systems analysis, what is the role of analysis in the long-range planning decision process? Just as the term systems analysis itself has different meanings to different people, the role of analysis in the long-range planning decision process is often interpreted in various ways. (These two subjects are not independent, of course.)

Contrary to what some of the more enthusiastic advocates of quantitative analysis may think, systems analysis should be visualized 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 through the more precise statement of problems, the discovery and outlining of alternatives, making comparisons among alternatives, and the like. In practically no case should it be assumed that the results of the analysis will "make" the decision. The really critical problems are too difficult, and there are too many intangible (e.g., political, psychological, and sociological) considerations that cannot be taken fully 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. And in many instances a small amount of sharpening of intuition and judgment can have a high payoff.

Let me sum up this part of the discussion with a quotation from a statement 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.†

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*There is a body of current research examining some of the physiological and psychological aspects of the intuition and judgment process. Will this be a future major area for systems analysis?

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

THE STATE OF SYSTEMS ANALYSIS TODAY

With my previous remarks as a backdrop, consider the question of the state of systems analysis today in the national security realm. To do justice to this question would require both more knowledge on my part and more time than is available to me today. Besides, I am most anxious to get to the next question: What about the future? I shall therefore be very brief in talking about the present situation.

One's assessment of the present state of affairs is very much dependent upon the criteria used as a basis for judgment. If volume of activity is the criterion, then I think we must conclude that systems analysis is in great shape--at least relative to, say, ten years ago. There are certainly more people engaged in the activity today than before. However, it is not clear what this means in a substantive sense. In fact, some would conclude that having more people engaged in systems analysis activity is by definition most unfortunate!

Let me pose a more relevant question: As a result of systems analysis, do we have a relatively clearer understanding of the substantive national security issues today than ten or fifteen years ago? I am not sure that anyone can really demonstrate whether the answer is yes or no. To my knowledge a scholarly survey of this question has not been done; so the answer must be primarily a matter of subjective judgment.

My own feeling is that in some areas a good deal of progress has been made in the way of clarification of issues and providing a basis for sharpening the intuition and judgment of the decisionmakers. Examples of a few of the more important of these are the following:

(1) The mix of airlift, sealift and prepositioning.
(2) The force mix of land-based and sea-based tactical airpower.
(3) The mix of manned aircraft and missile forces in the strategic offensive mission area.
(4) The balance among different damage-limiting measures (both offensive and defensive) in nuclear war preparedness.

The ultimate question, of course, is whether systems analysis as practised today is helping to promote better national security decisions than would be the case without this specific type of analytical input to the process. I do not know whether or not such a question can be answered definitively by anyone. I do know that I am not qualified to give such an answer. My contacts with the decision process have not been frequent enough and intimate enough to permit me to attempt an informed assessment. However, as an outsider who on occasion has had an opportunity to observe parts of the total decisionmaking procedure, my feeling is that at least in some areas decisions have tended to be more informed than they would have been without an explicit systems analysis contribution. The implication is that the resulting decisions are better; but I would not try to defend this definitively.

Now my remarks should not be interpreted to imply that all is well with systems analysis today. Much remains to be done--both in terms of concepts and methods of analysis, and in terms of applications to subject matter areas. This points us toward the future. Let me now turn to that subject specifically.

FUTURE PROSPECTS AND PROBLEM AREAS

The future prospects for systems analysis depend upon a number of factors, many of which are self-evident and would thus show up on almost everyone's list of items about the future. While these factors may be painfully obvious, they are nevertheless very important, and for this reason I want to list a few of them for the record:

(1) High-quality people working in an interdisciplinary research environment are the prime requirements for further progress in systems analysis in the future.

(2) The responsible decisionmakers and/or planners of the future must have a proper appreciation of what systems analysis can and cannot do for them, and they must be willing to make use of systems analysis inputs to the decision process. (This, of course, does not mean accepting uncritically the results of all analyses.)

(3) We must continue to try to develop new concepts, methods and techniques of analysis--advancing the analytical "state of the art." While most of this should probably be done as part of the research effort applied to specific problem areas, it seems desirable to allocate a modest amount of resources to the development of methodology *per se*.

So much for examples of "necessary conditions" for the advancement of systems analysis in the future. Let us now consider specific problem areas. Because of time constraints today, I can discuss only a few examples.

The strategic offensive and defensive forces area would seem to be a most interesting one from a systems analysis point of view. For one thing, the excellent work done to date in this area provides a good foundation for making further advances in the future. Here I particularly have in mind recent past and current work which makes a serious attempt to consider the strategic offense and defense together, and which examines a wider range of strategic scenarios than was the case previously--specifically, for example, damage-limiting capabilities in addition to pure assured destruction.

Future work on the strategic offensive/defense force mix problem might well take off from these studies, and attempt further explorations along the following lines:

(1) Focus considerably more on joint intent/capability analyses (multi-sided) rather than on extreme intent cases where the adversaries use their maximum capabilities.

(2) This implies considering subjects like the following (in addition to assured destruction and damage limiting):

   (a) Coercion and bargaining capabilities to be used in an escalation process stemming from a crisis situation (to the extent that this is not automatic from damage-limiting analyses).

   (b) Intrawar deterrence of countervalue exchanges.

   (c) War termination.

If these issues are to be explored in depth, the question arises as to whether present systems analysis concepts and methods alone are sufficient for dealing with the total problem. The answer is likely to be no, and in that case we might be tempted to say: "Let's game it." "Gaming it," however, has typically suffered from some or all of the following disadvantages:

(1) Unless the game is overly simplified (and hence perhaps useless or misleading), one run-through has taken an inordinately long time, and therefore was very expensive in terms of man-days of effort.

(2) In a given "play" of the game so many key factors were varying that interpreting the significance of a single play has been most difficult. Repetitions, under controlled conditions, might help solve this problem; but in the past the length of time (and hence the cost) required for one play made a large number of run-throughs infeasible.

(3) It has been difficult to make readily available to the players the large body of information (including a kit of analytical tools to utilize this information) required to permit them to adequately assess the relevant range of alternatives before deciding upon a move. Similar difficulties have arisen with respect to control and evaluation teams in their recording, assessing, and directing the play. In sum, there have been difficulties in getting the desired analytical substance into the games to prevent them from degenerating into a series of isolated plays which are too little subject to meaningful interpretation.

For these reasons, and others, merely "gaming it" does not seem to be the answer to the strategic offense/defense planning problem. Neither does the use of present systems analysis methods alone. Apparently some innovation in analytical concepts and methods is called for in the future. However, methodological innovation is not easy to come by; and we might ask whether something useful can be done in the near term future which will "tide us over," so to speak, until a more dramatic breakthrough is attained. On the basis of current experimentation,* it would appear that some interesting possibilities exist.

Consider the following statements:

(1) We have a body of knowledge and experience in conventional gaming.

(2) We have a body of knowledge and experience in current systems analysis methods and techniques.

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*Present work at The RAND Corporation in this area is under the direction of E. W. Paxson.

(3) We have computers, and there have been recent advances with respect to information storage and retrieval, display techniques, and, perhaps most important of all, on-line, time-sharing computer systems designed for use by analysts with no background in computer programming.

(4) We have the beginnings of techniques to systematize the interactions of a group engaged in a joint judgmental endeavor in the context of a dynamic sequential decision process.*

Now any one of these items by itself may not help very much toward solving our problem. But what might happen if we combine all four in a very deliberate manner, with a view to having each one mutually reinforce the others? Might the result be a whole that is in a sense greater than the sum of the parts?

In the case of the offense/defense force mix planning problem, I think that such an outcome might be possible because of reasons like the following:

Primarily because of the multisided controlled-response scenarios that have to be examined, some sort of gaming activity seems called for. Conventional gaming, however, tends to have limitations, as noted previously, with respect to analytical content. Perhaps this disadvantage can be partially offset by providing the players and the control and evaluation teams with a body of data and analytical models which would permit the participants to engage in a considerable amount of analytical activity during the play of the game. The trouble here is that attempting to increase the analytical content of the play would tend to lengthen a process that is already too long. This is where the computer technology comes in--particularly on-line, time-sharing computer systems. Through judicious exploitation of advanced computer technology, it appears possible to <u>readily</u> make available to the game participants a considerable amount of the necessary data base and modularly constructed analytical models required to increase substantially

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*In part, this involves an extension of "The Delphi Method." For a discussion of the method itself, see Olaf Helmer, <u>Analysis of the Future: The Delphi Method</u>, The RAND Corporation, P-3558, March 1967.

the analytical content of the game. Conceivably this might be done without making the game itself unduly mechanistic and without increasing the playing time significantly. If anything, playing time might hopefully be reduced.

Let me give you just one example of what has been done in this area as a part of one of the experiments with which I am familiar. At certain stages of the game the players have to engage in a planning exercise to structure their respective offensive/defensive forces for the distant future. Numerous alternative systems and mixes of systems are available for consideration, and the force-structure choices have to be made subject to a budget (resource) constraint. This means that the players have to engage in a considerable amount of cost analysis activity in the process of trying to arrive at the most effective force that might be obtained from the stipulated overall cost level. Using conventional cost analysis methods and techniques would take an inordinate amount of time. So a series of quick-response cost analysis models (for both individual weapon systems and total force structures) were developed and programmed for an on-line, time-sharing computer system. Use of these models permits the participants to very rapidly assess the resource impact of alternative force structures that they want to consider year by year over a period of years into the future. The result is a marked increase in the cost analysis activity that can be engaged in by the participants, without a significant increase in game play time. Also, the results of the players' deliberations are automatically made a matter of record for post-game review and evaluation from the computer printouts.

I hope that this brief description gives you some idea of how existing bodies of knowledge might be combined to produce an analytical procedure which will enable us to better tackle complicated defense planning problems in the future. Current experimentation is not far enough along as yet to permit assessment of the potential utility of such procedures; but at least they hold promise of representing a modest step forward.

As a second problem area example, let us turn to a subject that is important now and is likely to remain so in the future: the question of <u>the mobility of the general purpose forces.</u> I haven't time to outline all of the issues involved in this problem; so I shall discuss only some of them to illustrate a few selected points.

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. As I indicated earlier, some very good work has been done in this area, and I think something fairly close to good <u>sub</u>optimization 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.

A central issue in the big lift problem is the high cost of airlift vs. sealift (including advanced design logistics ships) and prepositioning in relation to the payoff in terms of <u>very</u> rapid response time and flexibility 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 interactions among all three. 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 very scenario dependent. For example, the results are sensitive to speed of development of the potential threat, the value of forward vs. rear defense in the light of U.S. commitments to various areas, and the like.

Although I have barely scratched the surface, I think I have said enough to illustrate my main point: that substantive analysis of the <u>total</u> mobility problem facing the long-range planners is very difficult--

particularly in attempting to seek out preferred alternatives in a broad context. Does this mean that the study effort expended in this area to date is worthless to the decisionmakers and that further work should not be initiated in the future? 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 overall "preferred solutions" have been forthcoming, these studies have provided 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, and in my view, this is the real value of analysis.

But what about the future? The key problem for future work seems to me to be similar to that in other areas: trying to find a way to bring together in some sense the work already done on components of the total problem. Here, it is tempting to say that everything is related to everything else, therefore let's set out immediately to build the grand "general equilibrium" model. While this may be correct in principle, experience to date indicates that trying to tackle the "big model" all at once usually does not produce substantive research results. Here is where some carefully executed ingenuity by systems analysts might produce useful results in the future. For example, if we can establish just a few key threads of linkage among intra-Z.I., inter-theater, and intra-objectives area mobility systems, a significant increment of understanding might be achieved.

Also, in some cases at least, a wider range of alternatives might be examined in the future. For example, there may be a trade-off between economic and military aid to indigenous forces on the one hand and expensive, very quick response time on the part of U.S. general purpose forces on the other. If indigenous forces can be given the capability to hold out against potential enemy attacks for a little bit longer period of time, perhaps the mobility capability of U.S. general purpose forces can be placed below the very high cost portion of the mobility cost curve. In other words, force mixes containing very large elements of expensive airlift might be avoided. While this would no doubt not be possible in some cases, there may be enough of such instances to make a significant difference in the future posture of the U.S. general purpose and lift forces.

All in all, it seems that in the problem area concerning mobility of the general purpose forces, many issues remain to be explored in future systems analysis work. While much has been accomplished to date, numerous unexplored interrelationships need examination in the future.

So much for examples of specific problem areas. Let me now turn to the subject of systems analysis methodology in the future. Here there is a "standard list" that people tend to recite when asked about what conceptual and methodological work needs to be done. Included on the list are, for example, the need for better ways to deal with uncertainty in systems analyses, further work on the criteria problem, better methods for treating problems associated with time, etc.

These, of course, are important subjects. However, today I would like to discuss a somewhat different (though related) set of issues.

In a general sense systems analysis has two main components: that part dealing with effectiveness (or utility) considerations, and that part concerned with cost (or resource impact) considerations. Now we are often told that the effectiveness side of the coin is the really difficult area, where most of the future conceptual and methodological work needs to be focused. Some people even say that for the most part the cost or resource impact area is in great shape and that little remains to be done. I agree that the effectiveness realm is the more difficult of the two, but I do not think that the cost analysis area is as tidied up as some people would lead us to believe. In my remarks to follow I hope to make this clear. I shall argue that there may be important problems in the cost analysis area that need working on in the future, although I must admit at the outset that I do not know just how they should be tackled.

Most cost analysts who really know the subject will readily admit that there is plenty of room for improvement in the future. The question is, in what direction? In the recent past, considerable attention has been focused on one problem area in particular--that concerning problems associated with time. More specifically, a lot of attention has been given to the issue of what discount rate to use in systems analysis studies to "equalize" future cost streams through time--that is, to equalize them with respect to time preference. We have had conferences where much time was spent arguing about whether the discount rate should be the current interest rate on U.S. government bonds, the current rate of growth of GNP, the current rate on AAA industrial bonds, or some other rate. Some people apparently feel that this is an important area for future research.

My reaction is that I am not convinced that the subject is all that important. If a given national security decision is of such a nature that the decisionmakers are not (or should not be) indifferent with respect to time preference, the analysts can do a lot in the way of sharpening the intuition and judgment of the decisionmakers without necessarily determining the discount rate to use in the particular problem at hand. For example, the sensitivity of final results to a relevant range of discount rates can be calculated. Also, as part of this type of analysis, the rate that must be used in order for the ranking of the alternatives to be changed significantly can be determined. The decisionmakers can then make up their own minds about how they want to treat the time preference problem in the process of choosing among alternatives.

In any event, it seems to me that the discounting question *per se* is not one that represents a major problem area for future research. Furthermore, it would appear that discounting is but one facet of a more general question that *might* deserve some investigation in the future. That question is: Do the money cost inputs to systems analyses as generated by current cost analysis methods and procedures really represent what we think or hope they do? If they do not, are they still close enough for the purposes of systems analysis comparisons?

What do we hope these costs represent? Presumably we want them to reflect the economic costs to the Department of Defense and/or the Nation.

In its most fundamental sense the economic cost of a proposed future course of action is whatever must be given up or sacrificed in order to adopt that course--that is, other opportunities foregone. The interesting thing about the "opportunity cost" concept is that the cost of a given proposed alternative is thought of in terms of the utilities or benefits that would be foregone if the given proposal were to be implemented.

Current military cost analysis methods and procedures generate estimates of the cost of future courses of action in both physical units of measure (e.g., manpower) and money costs. Money costs can be calculated in a variety of ways; for example,

(1) Total system cost: R&D, investment, plus a span-of-years operating cost.

(2) Time-phased costs for systems and/or total force structures, usually measured in terms of total obligational authority and/or expenditures.

Money costs in one form or another are the most commonly used index of the economic cost of alternatives in systems analysis studies. The question I would like to pose is whether or not these money costs adequately reflect the "opportunity costs" of system and force structure proposals for the future.

The current generally accepted answer is perhaps best stated and argued most convincingly by Hitch and McKean.* I cannot take the time here today to go into the details of their argument; but let me state their main conclusion:

> As a consequence, money costs of future defense activities approximate the real alternatives that are foregone--the real sacrifices that are entailed--when one activity or weapon system is selected. This will be true for those problems in which a general monetary constraint is proper, that is, for problems pertaining to dates sufficiently in the future to permit the production and procurement of varying quantities of weapons and materiel.

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*Charles J. Hitch and Roland N. McKean, The Economics of Defense in the Nuclear Age, Harvard University Press, Cambridge, 1960, pp. 25-28.

Now this conclusion may well be correct, and as I stated before, the argument leading up to it is fairly convincing. Also, for the near-term future we have no alternative to using the currently established concepts, methods and techniques. However, for the more distant future I think that it might be worth while to try to investigate further the hypothesis that money cost estimates, as currently generated and used, are in fact an adequate index of economic (opportunity) costs for purposes of systems analysis. So far, the hypothesis has apparently not been rejected; but that it has been adequately tested is not clear. I am suggesting that it might be a good idea to try to do some more testing.

I must admit that at the moment I am not very clear in my own mind about the <u>specific</u> hypotheses that should be tested. One possible line of investigation might be the following: Economic theory tells us that under conditions of pure competition the money costs of the factors of production will in fact represent their true economic or opportunity costs. However, the U.S. economy might not be all that competitive, particularly in those sectors (like the aerospace industry) that are most relevant to national defense. Does this mean that money costs diverge significantly from economic costs? Is there a way that we might investigate this question quantitatively? For example, could we determine whether an appreciable drop in civilian space activities might greatly decrease the price of some highly specialized resources that are relevant for military systems?

Speaking of opportunity cost, the thought occurs to me that the cost to the audience might be inordinately high if I should talk for another five or ten minutes. The alternative is to stop now!

So let me sum up as follows: Systems analysis denotes the process of helping to sharpen the intuition and judgment of the decisionmakers through the more precise statement of problems, thorough identification (and often invention) of the relevant alternatives, measurement of their costs, identification of their probable benefits, explicit treatment of the uncertainties involved, and a serious attempt to estimate the effects of policy alternatives. These are commonsensical ideas, but actually doing them is often extraordinarily complex and demanding. Much progress has been made to date; but a great deal remains to be done in the future. Further progress will require ingenious thinking, and above all just plain hard work.