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WHERE IS SYSTEMS ANALYSIS?

By C. J. DiBona

CNA Research Contribution No. 30

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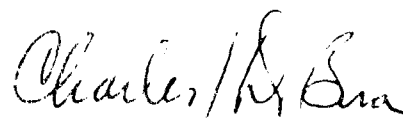
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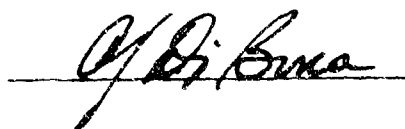
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CNA RESEARCH CONTRIBUTION NO. 30

WHERE IS SYSTEMS ANALYSIS?

By C.J. DiBona



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ABSTRACT

This discussion of the present state of systems analysis was delivered as a talk at the 20th Military Operations Research Symposium, held 12 December 1967, at the National Bureau of Standards, Gaithersburg, Maryland.

This paper is published as a Research Contribution to provide as early as possible the results of CNA supporting analysis to the Naval, Marine, and analytical community.

WHERE IS SYSTEMS ANALYSIS?

Most practitioners of the art of systems analysis seem to agree that it has succeeded well in some areas. In others, they see hopeful signs. In still others, the feeling is that a long time may pass before real progress is made. It is in keeping with the traditions of this art to explore critically and try to understand why there is this significant consensus among practitioners about the usefulness of systems analysis.

Let us consider the framework of systems analyses. First, these analyses are, almost exclusively, "force-package"-related. It is no coincidence that program budgeting and system analysis have succeeded together. The existence of a specific set of programs, with associated costs and outputs, has made it possible - or, at least, easier - to focus the systems analysis effort.

At the same time, however, analysts have become, to some extent, prisoners of the categories of output. For example, though strategic mobility forces are farther than strategic nuclear forces from final output, analysts treat both in much the same way. Few studies of general-purpose forces treat rigorously the implications of alternative systems for strategic mobility forces.

Little systematic analysis has been conducted on the choices among major force packages or, in the case of general-purpose forces, among their separate components. Nor has much study been devoted to such obvious questions as the relationship between active and reserve forces of a given type. Perhaps the areas of analytical achievement would be different if the forces were packaged differently. Perhaps more would be known about the air/ground trade-off if tactical air and ground forces were not packaged separately.

In addition to being related to force package problems, most systems analyses are assessed in terms of the ability to answer adequately a hierarchy of questions of practical interest to decision-makers at the level of the Department of Defense and the services. In descending order, they are:

First, questions about the level of force in a package;

Second, questions about the mix of land- or sea-based systems for a given level of capability or budget. This is a special set which is logically little different from the third set of questions, but the obvious service interests give it special emphasis. Not surprisingly, we have concentrated a good part of our efforts on trade-offs between Polaris and Minuteman, carriers and land-based air, and C-5 aircraft and FDL ships.

Third, there are questions about the choice for procurement of specific systems or subsystems, such as an aircraft, tank, ship, or rifle type. Notable examples are the F-111, the main battle tank, nuclear powered ships, and the M-16 rifle.

The concern with these large and immediate procurement and force-size questions has relegated a fourth category to relative obscurity. These are questions about "R&D strategies" on which much work used to be done. How much should we be willing to pay to find out more about alternative systems before we make system choices? How many systems should we simultaneously develop in the face of uncertainty about their relative costs and productivity? In other words, how big a menu of alternatives should we preserve for the future, and how long should we delay to gain information before making decisions? The problems are tractable. There is at least a partial body of theory to work from. Moreover, good solutions very probably have greater leverage to help the American taxpayer in the long run than the questions we have concentrated on. Finally such analyses should help to reduce part of the multi-faceted uncertainty that confounds us in dealing with certain force packages.

We shall return to this point, but first, what is the assessment of systems analysis? And why? There seems to be general agreement that in the strategic nuclear offensive and defensive force packages we have things fairly well in hand; we contribute significantly to questions of level, mix, and subsystem choice. In the area of strategic mobility we also feel comfortable about making recommendations not only about the mix of systems but the level of capability the U. S. should procure. But this widely accepted favorable assessment of the analytical contribution to these programs descends when we begin to consider tactical air and ASW. And finally, there are very few positive statements about what systems analysis has done to clarify decisions on questions about conventional ground forces. In this area we can barely make PERT charts for procurements associated with force buildups.

The same ordering of the contributions of systems analysis is present in the evidenced attitude of decision-makers toward each force package.

In the strategic nuclear and strategic mobility areas, decision-makers are both able and willing to make very explicit judgments about the objective or criteria in terms which are susceptible of calculation, given the state of the art of systems analysis. For example, with respect to strategic forces, decision-makers are willing to say more than merely that we should be able to deliver a second strike attack of sufficient magnitude to deter the enemy from a first strike. They are actually willing to spell out what that level is in terms of the percentage of enemy population and industrial capacity destroyed. While these assessments of the specific level of destruction needed for deterrence have changed over time, this has not shaken the decision-makers faith, or ours, in their ability to make the appropriate explicit judgments and to use analysis in arriving at those judgments. The important point is that where the decision-maker is willing to stick his neck out, the analyst's problem is radically simplified.

In the case of rapid deployment forces, decision-makers are willing to spell out in some detail the specific time profiles of delivery of forces to particular geographic areas and to talk about how many simultaneous contingencies we should be able to handle. Decision-makers do this in spite of the fact that our knowledge

of conventional warfare is crude. We have a poor understanding of how much it is worth to deliver a division in, say 7 days instead of 4. Consequently, specifications of delivery profiles can only be based upon the roughest of judgments. Yet, once given, they do make detailed calculation possible.

In going through the other force packages - those packages in which the assessment of the contribution of systems analysis is lower - the character of the judgment decision-makers can or are willing to make is different. For tactical air forces, for example, there are few useable objectives or criteria, other than very crude ones, for determining how much force we should have. This is also true about land forces. The conceptual problem of assigning explicit objectives or criteria in these areas is no different, in a broad sense, than it is for strategic nuclear forces. Yet they are not assigned.

The actions of decision-makers shouldn't surprise us. The analysis of strategic forces is at least sufficient to indicate how changing expenditures on weapons will affect the percentage of surviving enemy populations or industrial capacities. For general-purpose forces, we poorly understand what desirable outcomes are or how changes in the forces affect outcomes. In these areas, the primary form of analysis used in force level decisions is a comparison of friendly and enemy forces on several different standards. For example, in an analysis of tactical aviation we depend on such comparisons as the total payload capability of the two opposing total tactical air forces computed for a single sortie on a mission of fixed radius. For ground forces, the comparisons are closer to simple personnel or equipment counts, with some account taken of qualitative differences.

It is perfectly reasonable to ask: What is so peculiar about the areas where we believe systems analysis has made a contribution? Why are they arrayed this way?

In trying to address these questions, and at the risk of appearing facetious, it may be appropriate to relate one of Parkinson's Laws. Parkinson observed that in the boards of large corporations multi-million dollar decisions were made rather quickly while decisions about such trivial matters as putting a roof over the employees' bike shed provoked long and spirited debate. This is an example of the "Law of Triviality," which states that the time spent on any item on the agenda will be in inverse proportion to the sum involved. The explanation is that events and magnitudes beyond the realm of personal experience may seem considerably less complex than those which can be related to individual experience.

This human trait does not appear to provide a complete explanation of the differential attitude to problems in the various "force-program" areas, but there may be a grain of truth in it. Current opinion as to the success of systems analysis is very negatively correlated with the amount of human experience in each area. For example, there has been no experience with massive nuclear exchanges and very little with rapid deployment, but we believe systems analysis has led us to understand these programs. It would appear that opinion is split on how well

we do now or will do in the immediate future with the analysis of tactical air forces; we've had a little experience there. The same applies to ASW forces.

Finally, at the other pole, we find ground combat. The verdict of experts is that it is nearly intractable. Experience there, in one form or another, has been piling up since the African genesis. On its face, this is a strange correlation. Most sciences are based on observation and are designed to permit us to extend experience.

But then, this explanation is based upon simple correlation. Although it is an extremely close one, there is always the possibility that it is spurious and that the contribution of systems analysis to the understanding of each force package is really explained by causal factors other than lack of evidence.

For example, some areas may be neater or inherently more tractable than others. The more the subjects of investigation are pre-planned and the more they depend on the characteristics of large pieces of equipment with few operating modes, and the more they are determined by early large events, and the more likely they are to be analytically tractable. These areas are less likely to be influenced by unpredictable behavior and states of training or to be dependent upon a myriad of small encounters. Our assessment of those characteristics in each of the force packages also tends to correlate nicely with how we feel about the state of analysis in each.

There may even be a more simple explanation. Given the present state of the art of analysis, we can adequately analyze conflicts which happen with such great speed that there is no real opportunity to re-assess, re-deploy forces or change tactics. Conversely, the analysis of longer conflicts in which each side has the opportunity to learn from, and react to, the experience of the war cannot be convincingly done today.

When one considers the most widely used analyses of conventional wars, a remarkable number are high intensity, extremely short wars. For example, most of the analyses which deal with a NATO war look much more like the recent Arab-Israeli conflict than World War II revisited. All decisive action is taken and the outcome is decided in about one week. Similarly, in ASW, the wars analyzed are incredibly quick. But the users of the analysis believe that neither we nor the enemy would act in the specific ways suggested. As the length of the conflict grows, this objection gains much more force. Both we and our enemies would continually re-adapt our tactics and strategies to what is happening, and the problem of analysis becomes much more complex. It is not surprising that the analysis of "broken-back" nuclear exchanges is less advanced than that of massive exchanges. The difference between strategic nuclear and strategic conventional wars is that in the latter case the massive, rapid, single-strategy conflict is considered to be a good deal less likely. In any case, it is a good deal less convincing.

There is another explanation which is, in part, related to the correlation between experience with a type of force and the ability to analyze it. That is, the people who do systems analysis probably have taken the trouble to learn a great deal more about the details of strategic nuclear forces and are as expert about the fundamentals as any other group. This may be because there is little opportunity for field experimentation, and what little is done is carefully catalogued. As a consequence, it is amenable to a form of research that analysts are more accustomed to. With conventional forces, systems analysts are probably a good deal less expert than the operators. Given the competitive environment in which most analysis is necessarily done, this lack of expertise is a weakness that is likely to be fatal.

Of course, there has been more opportunity for experience with general-purpose forces to disprove an analysis. The number of carefully constructed and executed tests of weapons systems in appropriate environmental conditions is and has been very small. As a result, there can be little doubt that our peacetime estimates of the wartime performance of systems are very poor. Before the Vietnamese conflict we badly misestimated our capability with certain weapon systems. Even for certain narrowly defined tactical air missions, such as the number of sorties to achieve a kill on a specified target with a given weapon, we found some estimates off by an order of magnitude.

This is nothing new, although we've improved somewhat. Herman Goering claimed that no enemy bomber would ever attack Berlin. His estimate of the number of anti-aircraft shells required per aircraft kill was off by a factor of 1,000. The guns used in the German tests were manned by men who might be characterized as athletes with PhD's in physics.

It doesn't take a war to demonstrate a misestimate of performance. Systems analyses are generally directed at the procurement of new, or relatively new, systems. We tend to accept the estimates of the potential producer, who has a strong incentive to make the system look good. The alternative source of data is the "in-house" project manager, who is also not likely to be very ill-disposed to the system even if he has any real uncertainty about it.

When the system actually gets into use, the analyst is often, for a number of reasons, the last person to find out how well or badly it performs. Given the way we operate now, there is often a significant delay before the analysts know what's happening in operational exercises. This is more evident for conventional general-purpose forces, because more widespread and unreported training and practice occurs with them, and the analysts are not present.

I personally believe that the difficulty experienced in dealing with conventional general-purpose forces is a composite of these factors. The situations are significantly more complex, tactics and strategy changes brought about by derived information during the conflict must be addressed; and finally, systems analysts are relatively inexpert in these areas and insufficiently oriented empirically.

As a result, there are large and recognized uncertainties operating in several dimensions which drive through many of the large general-purpose force problems we attempt to deal with. If we explicitly recognize each uncertainty and try to do sensitivity analyses, the value of the analysis as a predictor of outcome may not be meaningless, but as an aid to decision-making, its utility is very low. It has been observed in these studies that there are reasonable assumptions which provide results of totally different character, not only as to outcome of war situations, but also as to the relative attractiveness of different systems tested. What we often end up with is either a myriad of possible outcomes, with little ability to select the most reasonable ones, or with a few point estimates that we must qualify so broadly that they lack credence. It's not surprising that when it comes to general-purpose forces the decision-makers are unwilling, or more accurately, unable to state a specific objective, as they do for strategic nuclear forces. They can state only the most general rules for choice.

Some of the uncertainty results from incompletely known technical performance, some reflects our ignorance of how systems interact in conflicts, and some is a result of uncertainty about how information derived from the conflict affects the contestants' actions.

These are new techniques which would permit us to deal with this third kind of uncertainty by radically expanding our ability to game problems while avoiding the previous limitations of analytic content. This is very interesting. However, such a method is subject to the same explosion of results and effort, and most of the necessary data base and modularized analytical models are themselves subject to wide variations of the first two kinds. In other words, we now have problems of great uncertainty in dealing with a fixed single, preplanned decision process.

If this is all true, then our primary task may be to find some way of reducing some of the uncertainty. The well-known task for the sensitivity test is to help find out what uncertainties matter most. The next step is to determine which of those uncertainties that matter are ones we can really hope to narrow - and then do something about them.

It is the latter task - of doing something about the uncertainties that matter - which we have tended to ignore. It is probably because systems analysts like to think of themselves much more as "conceptualizers" rather than as empiricists.

For example, how can anyone have much confidence in the validity of a model for predicting certain outcomes of tactical air encounters while simultaneously admitting that this knowledge of the inputs is poor? The lack of agreement about inputs may be no fault of the model, but it raises questions as to its validity.

Models are abstract representations of reality which help us to perceive significant relationships in the real world, to manipulate them, and thereby predict outcomes. The real test of their utility or validity is whether they are good

predictors. With the admittedly great uncertainty about tactical and technical inputs or outcomes, it is essentially impossible to determine the validity of a model. We simply can't make that judgment now.

Systems analysis does need more empiricists. We need more people who are involved, not only in structuring complex problems, but who bring this understanding to the development and monitoring of tests. Further, the tests must be conducted in an organizational framework which at least eliminates the most obvious bias-forming incentives. Moreover, the test should be conducted under a spectrum of operating conditions, not "ideal" conditions alone. The single most important need for the future success of systems analysis in dealing with general-purpose force issues is the acquisition of good empirical data. It is a task we cannot leave to others.

The responsibility for the inadequacies of the present state of knowledge can hardly be laid exclusively on the community of systems analysts. There has been a real conflict between the use of operational units in training exercises and their use in monitored testing. Commanders of operational forces have tended to prefer training to testing, a preference which may well be shortsighted.

Finally, we turn to a largely ignored but important task mentioned earlier - work on R and D strategies. This is a special case of the general problem of dealing with uncertainty. It is generally recognized that our predictions of the costs, time to develop, and success of developments of new weapons systems have been poor. Cost increases of 200 to 300 percent and extensions of development time by 30 to 50 percent are the rule. Moreover, the size of the error has been very variable.

Recently, CNA reviewed some predictions made in 1958 by the best available experts about ASW system performances in 1965. The 1965 actual data indicated wide errors. For example, the estimate of the ability of a CODAR pair to localize a conventional submarine target was off by a factor of 10. Only one system was projected with reasonable accuracy.

The purpose of the estimates of cost and performance is to permit better decisions. Given the history of the accuracy of these estimates, we may be failing in this task. There is undeniable optimistic bias, but the effect is that the systems where we make the worst errors tend to be the ones we favor. That is, the systems where we overestimate the performance and underestimate the cost are those we choose to develop and procure. The worse the error, the more likely we are to choose the system.

The long and short run implications of the way we handle R and D decisions was pointed out by A. W. Marshall and W. H. Meckling as long ago as 1959 and by Burton Klein even earlier. Meckling and Marshall remarked that the R and D decisions made today affect tomorrow's menu of systems and that the decision as to how many we continue with ought to be a function of our confidence in the estimate. The more confident we are in the estimate, the less we should duplicate.

because we can choose earlier. Given our experience with the estimates, the chances of providing an inadequate or inappropriate menu are apparent. Finally, they point out that short-run decisions about what to procure this year or next are influenced by what we believe will be available 2 or 3 years from now. In ASW forces, this is a serious and important consideration.

In spite of the significance of these issues, it is not apparent that there is a systematic examination of these important allocation questions. The handling of R and D is not unrelated to my earlier remarks about uncertainty. We will never eliminate it. But we must address much more directly the question of how much we are willing to pay in time and resources to reduce it.

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