SOME COMMENTS ON COST-EFFECTIVENESS

E. S. Quade

March 1965

P-3091

SOME COMMENTS ON COST-EFFECTIVENESS

E. S. Quade*

The RAND Corporation, Santa Monica, California

My assignment is to say something about the limitations, the pitfalls, and the advantages of systems analysis, as it applies to the cost-effectiveness function.

First, I need to make a distinction between systems analysis and cost-effectiveness.

As commonly used in the defense community, the phrase "systems analysis" refers to formal inquiries intended to advise a decisionmaker on the policy choices involved in such matters as weapon development, force posture design, or the determination of strategic objectives. A typical analysis might tackle the question of what might be the characteristics of a new strategic bomber and whether one should be developed or not; whether tactical air wings or carrier task forces should be substituted for U.S. ground divisions in NATO; or whether we should modify the test ban treaty now that the Chicoms have nuclear weapons and, if so, how. Each such study involves as one stage a comparison of alternative courses of action in terms of their effectiveness and cost. When, as often happens, this stage requires major attention, the entire study is sometimes called a cost-effectiveness analysis.

---

*Any views expressed in this paper are those of the author. They should not be interpreted as reflecting the views of The RAND Corporation or the official opinion or policy of any of its governmental or private research sponsors. Papers are reproduced by The RAND Corporation as a courtesy to members of its staff.

These remarks were prepared for delivery as part of a panel on "Cost/Effectiveness" at the Fourth U.S. Army Operations Research Symposium, Redstone Arsenal, Huntsville, Alabama, 31 March 1965.

The distinction is not one of principle but of emphasis. Let me try to make this clear with a homely example.

Suppose a family has decided to buy a television set. Not only is their objective fairly clear, but, if they have paid due attention to the advertisements, their alternatives are well-defined. The situation is then one for cost-effectiveness analysis. The only significant questions the family need answer concern the differences among the available sets in both performance and cost. With a little care, making proper allowance for uncertainty, they can estimate, say, the five year procurement and operating cost of any particular set and do so with a feeling that they are well inside the ball park. They will discover, of course, that finding a standard for measuring the effectiveness of the various sets is somewhat more difficult. For one thing, the problem is multidimensional—they must consider color quality, the option for remote control, portability, screen size, and so forth. But ordinarily, one consideration—perhaps color—dominates. On this basis, they can go look at some color sets, compare costs against color quality, and finally determine a best buy.

Now suppose the family has simply decided to spend more money and thus increase their standard of living—a decision similar to one to strengthen the U.S. defense posture by increasing the military budget. How can they decide how to allocate the money between various possibilities? This is a situation for systems analysis. They first need to investigate their goals or objectives, and then establish criteria, determine measures of effectiveness, and look into the full range of alternatives—a new car, a piano, a trip to Europe. Here, because the alternatives are so dissimilar, determining what they want to do is the major problem; how to determine what it costs may become a comparatively minor one.

Don't misunderstand me—the determination of even the dollar costs of a military action is not simple. The cost of a change in force posture, say, is measured by a price that typically includes the costs of R&D, initial investment, and annual operation, and these costs will have to cover the various weapons, equipment, and vehicles involved, and also the whole materiel and manpower structure that underlies the entire lifetime of the system. Consequently, it requires a great deal of sophistication to learn how to cost a force posture—to learn what goes into making up a posture and how their costs can be found. It takes even more know-how and research to estimate the costs of weapons and forces that are as yet only concepts. But with care and experience, once we decide <u>what</u> we are costing, we can do fairly well.

In summary, what I am saying is that to qualify as a system analysis a study must look at the entire problem and look at it as a whole. Thus, characteristically, a systems analysis will involve a systematic investigation of the decisionmaker's objectives and of the relevant criteria; a comparison—quantitative where possible—of the costs, effectivenesses, and risks associated with the alternative policies or strategies for achieving each objective; and an attempt to formulate additional alternatives if those examined are found wanting. I regard the typical cost-effectiveness analysis as just one stage in this process.

Now let me say a few words about the limitations of any analysis, even the best, when used in defense planning. Following that, I'll discuss some of the pitfalls of such analysis and conclude with a few remarks about its advantages and its future.

THE LIMITATIONS

One limitation of many analyses is self-imposed. Analysts often take too narrow an approach to a complex problem, forgetting that the record is full of instances where the failure to consider the whole has had serious consequences. For example, before Pearl Harbor, the Japanese made extensive use of war gaming to plan an almost perfect attack. But more attention to the broader problem of what could happen to them two or three years later might well have changed their recommendations.

But even the best systems analyses have many limitations that further analysis cannot remove. I'll confine my remarks to just two types: the necessity that measures of effectiveness be proximate and that analysis be incomplete.

One of the principal problems of systems analysis results from the fact that not only are objectives multiple, but their attainment must be measured approximately. Consider deterrence, for instance. It exists only in the mind—and in the enemy's mind at that. We cannot, therefore, measure directly the effectiveness of alternatives we hope will lead to deterrence, but must use instead such approximations as the potential mortalities that we might inflict or the square feet of roof cover we might destroy. Consequently it is clear that even if a comparison of two systems indicates that one could inflict 50 per cent more casualties on the enemy than the other, we cannot conclude that this means the system supplies 50 per cent more deterrence.

Moreover, we can't be as confident that our estimates of effectiveness are essentially correct as we are about our cost estimates. It is the opinion of one analyst who has studied the problem of estimating casualties that if a pre-World War II estimator had worked analogously to his brother of today, had known his trade exceptionally well, had been knowledgeable about the means by which

World War II military actions produced casualties, had known the probabilities associated with each weapon, and could estimate the number of people subject to each weapon—then such an estimator would have underestimated the total cost in human lives of the war to the Soviets by a factor of between three and four.

Such an error in the measurement of effectiveness may not be too important if we are comparing two systems that are not radically unlike one another—two ground attack aircraft, say. But at higher levels of optimization—tanks vs. aircraft or missiles—gross differences in system effectiveness may be obscured by gross differences in the quality of damage assessment.

Next, incompleteness. Limitations on time and money obviously place sharp limits on how far an inquiry can be carried. Typically, other costs have the same effect. For instance, we would like to find out what the Soviets would do if we should put an armed Minuteman on Moscow. One way to get this information would be to launch a Minuteman. But while this might be cheap in dollars, the likelihood of other costs precludes at once this type of investigation.

Still more important, however, is the general fact that analysis can never treat all the relevant factors. No matter how thorough, it always leaves something for the decisionmaker. For at best, calculations by themselves give us, for each set of specific assumptions—about the political and economic state of the world, the actions of the enemy, the outcome of various technological investigations, and so on—a somewhat less than objective appraisal of, say, the effectiveness, for a fixed cost, of proposed forces or weapons for attaining given goals. Such appraisals are not good enough; they must be supplemented by informal and considered judgment.

For example, such qualities of a system as its flexibility, its compatibility with other existing systems, its contributions to national prestige abroad, and its impact on domestic political constraints—all these and others can play as important a role in the choice of alternative force postures as any idealized war outcome calculations. In addition, the analyst and the decision-maker must take into account certain even less tangible considerations, such as the perception each side has of its own or its enemy's strengths or the extent to which a superiority in residual forces can be effectively used to coerce an enemy to discontinue a conflict. Ways to measure these things even approximately don't exist and they must be handled subjectively.

Finally, there are the difficulties that arise because the future may take many forms. We can design a force structure for a particular war in a particular place, but we have no surefire way to work out a structure that is good for the entire spectrum of future wars in all the places they may occur.

Thus, systems analysis is not without its defects. It is rich in the kind of analysis that tells what damage could be done to the United States given a particular enemy force structure (or, to put it another way, what enemy requirements would be to achieve a given destruction); but it is poor in the kinds of analyses that evaluate how we will actually stand relative to the Soviets in years to come.

PITFALLS

And now for the pitfalls. There are many, particularly of the kind that the analyst himself may meet, such as underemphasis on problem formulation, overemphasis on the model to the neglect of the question, or concentration on the statistical uncertainties rather than the real uncertainties. But I will discuss just two that the sponsor and the user, as well as the analyst, must guard against.

The first of these is the "cherished belief." It leads to an "attention bias," which is the most frequent cause of failure to look at a full range of alternatives. The cherished belief often takes the form of an unconscious adherence to a "party line." All organizations foster one to some extent; RAND, the military services, and the DOD are no exception. My feeling is that Herman Kahn was right when he called the party line "the most important single reason for the tremendous miscalculations that are made in foreseeing and preparing for technical advances or changes in the strategic situation."

As I've pointed out elsewhere, the history of strategic bombing studies since World War II illustrates the workings of this influence. In World War II, the bombing problem was to penetrate the defenses, bomb accurately, and return. The bomber's concern was with enemy fighters, antiaircraft guns, and missiles—not with enemy bombers. For years, even in studies for time periods long after the Soviets were expected to have nuclear weapons, no serious attention was paid to the possibility that our bombers might be vulnerable on the ground, and we did not plan an attack on theirs as a prime objective. Requirements and specifications for future bombers hardly considered the problem of surviving the enemy offense. This was not stupidity on the part of anyone. For instance, in the Navy-SAC controversy over the B-36 in 1949, the Navy questioned the B-36 on every basis it could think of—including the argument that strategic bombing was immoral—but the question of its vulnerability on the ground did not come up. RAND's strategic bombing studies, even after the seriousness of the problem of bomber survival on the ground had been pointed out, continued to concentrate on such questions as cruise speed and altitude, low vs. high penetration, supersonic dash, bombing altitude, small vs. large planes, and what targets to select.* Thus, in a 1952 comparison of aircraft with missiles for strategic bombing, we ignored any effect on the comparison of possible differences in ground survivability—and no one we briefed took us to task for this omission! Fortunately, the Russians took even longer than we did to recognize ground vulnerability. It took extensive briefings of a study devoted to proving that base vulnerability was a serious threat to national security to get major attention on the problem.

What can happen is that, during the early cycles of the study, the participants and successive reviewers become aware that some of the alternatives or certain assumptions being considered are frowned on by higher-ranking officers. It then seems useless, even hazardous, to support such unpopular views strongly, and gradually they may be stressed less and less or even forgotten. Alternatively, after a period of successful work, a group of analysts may become so entrenched in an organization that they lose their independence of view.

Another pitfall is for a busy administrator to attempt to ease his job by transferring a portion of his decisionmaking function to a model. For example, it has been proposed a number of times (even to the extent of writing a study contract) that a general computer model for a strategic air war be set up to supply weapons designers with a systematic evaluation of their design concepts and to enable the Department of Defense to

---

*E. S. Quade (ed.), Analysis for Military Decisions, Rand-McNally & Co., Chicago, 1964, pp. 306-307.

evaluate the worth of alternative "design solutions" developed by competing contractors.

> One argument for such a model notes that "the choice of assumptions, the forecast of the future, and the methods of analysis have a marked influence on the performance and physical characteristics of the weapon system set forth as preferred or optimal"; therefore, a uniform framework would mean that "the results obtained by the various contractors would be comparable since the effects due to variation in the assumptions they might have chosen to form their models would have been eliminated." This may indeed be the case, but will the end result be desirable? A rigidly specified framework may mitigate one sort of undersirable bias—by making it difficult for an analysis to be used to rationalize conclusions already otherwise derived—but only at the severe risk of introducing other biases.
>
> A fundamental objection is that a uniform framework necessarily conceals or removes by assumption many extremely important uncertainties, therefore tending to lead to solutions that disregard the value of hedging against those uncertainties. Another is that even if efforts were made to keep the model "up to date," this would turn out to be impossible, for the analyst must be able to modify his model in the terminal stages of his study to accommodate information acquired during the early phases. Indeed, in a problem involving the struggle between nations, there are so many factors of shifting importance, and such radical changes in objectives and tactics are likely, that most models are obsolete long before the recommendations from the study can become accepted policy.*

Finally, if a model or a mathematical formula were used to indicate which proposal to select, the proposers' emphasis would soon focus on how to make his design look good in terms of this analytic definition, not on how to make it look good against the enemy—a much harder problem.

The analyst "sets up" the decisionmaker for this pitfall when the analyst begins to believe that there are

---

*Ibid., pp. 312-313.

"universal" models which can treat every aspect of a complex problem simultaneously. This occurs when he forgets that the questions being asked, as well as the process being represented, should determine the model.

THE CURRENT STATUS OF SYSTEMS ANALYSIS

It is easy to exaggerate the degree of assistance that analysis can offer a policymaker. Using value judgments, imprecise knowledge, and intuitive estimates of enemy intent, gleaned from specialists or from the policymaker himself, a study can do little more than assess some of the implications of choosing one alternative over another. This can help the decisionmaker make a better decision than he would otherwise make, but the man who has the responsibility must interpret such assessments in the light of his own knowledge, values, and estimates, and assess other implications himself. The decision must become his own.

Let me draw an analogy between the decisionmaker using a study team for advice and a medical doctor using a clinical laboratory.

Suppose, for example, our doctor is trying to decide whether to send his patient to a surgeon to have his stomach resected or to treat him medically for a gastric ulcer.

The doctor is influenced by:

1. The technical findings of the laboratory crews. Like the military decisionmaker, he might or might not be able to carry out these investigations himself, but it would not be economic for him to do so. He depends, therefore, on laboratory reports, some of which will be on cold slips of paper without comment or nuance—numbers alone. Others from the laboratory might write paragraphs or talk to the doctor or bring x-ray plates to discuss with him.

2. Observations or analyses the doctor makes himself. Some of these he puts in the form of written notes; those he can't write out he holds in his head.

3. Impressions of the risks and possibilities of success with various treatments. Some of these impressions are from his experience, others from medical reports.

Finally, like the military decisionmaker, the doctor must make a judgment based on whatever facts or analyses he has. This judgment is the ultimate synthesis the doctor makes of the numerical tests, the written out but relatively diffuse notes, the unrecorded conversations with technicians, and his own introspection. It is not a mere calculation, but is made on intuitive grounds. Sometimes a factor is overriding, but on the whole he just doesn't know. He could do more analysis, sometimes even risk the patient's life in order to guard it—call for a liver puncture or other dangerous procedures—but his inquiry can never be complete. His judgment, like that of the military decisionmaker, must be made with uncertainties in mind. Analysis will have helped him by answering some of his questions, sharpening his intuition, and broadening his basis for judgment. But in very few cases should we expect it to prove that a particular course of action is best.

Many people, and some of you may be among them, are uneasy about the increasingly important role such analytic aids as cost-effectiveness analysis and computerized games play in military planning and skeptical of advice from such sources. Some of this skepticism may be justified, for the work is not always competently done or used with its limitations in mind. But those critics who hold that national defense decisions are being made today solely by consideration of the calculations are not only premature in their belief (to say the least), but probably have a basic misunderstanding of how such decisions must, in fact, be made. As you should know, this process is today rampant with dogma, service rivalries, special interests, and horse-trading—so much so that, in the opinion of some analysts, a computerized solution based on a relaxation of these human constraints might lead to something better.

As one RAND analyst put it:

> Consequently, we must adopt a modest view of the importance of analysis and of studies in the decisionmaking process. To say that analysis does not make decisions is to repeat a truism. But it is useful to remind ourselves that we may not know all of the inputs to a decision—analysis and other facts and judgments and guesses.
>
> Let me take one example. The most drastic change in recent years in the "general purpose forces" did not result from a formal systems analysis. In 1961 in the light of the Berlin crisis and the relatively small size of U.S. conventional forces, President Kennedy and Secretary McNamara decided to increase the number of U.S. divisions from 11 to 16 and to increase air and sea lift capabilities.

Incidentally, the interservice rivalries and bargaining provide an automatic review; this has the virtue of insuring that the work is carefully scrutinized and that alternatives and considerations not noticed in the study are brought to attention.

How can we summarize any danger that there might be in reliance on systems analysis, or any similar approach to defense decisions? First, since many factors fundamental to national defense problems are not subject to rigorous, quantitative analysis, they may possibly be neglected, deliberately set aside, or improperly weighted in the analysis itself or in the decision based on such analysis. Second, an analysis may, on the surface, appear so scientific and quantitative that it may be assigned a validity not justified by the many subjective judgments involved. In other words, we may be so mesmerized by the beauty and precision of the numbers that we overlook the simplifications made to achieve this precision, neglect analysis of the qualitative factors, and overemphasize the importance of idealized calculations to the decision process. But better analysis and careful attention to where analysis ends and judgment begins should help to reduce these dangers.

Even after noting these dangers and deficiencies, systems analysis may still look like a purely rational approach to decisionmaking, a coldly objective, scientific method free of the human attributes of preconceived ideas and partisan bias and judgment and intuition. Systems analysis may look like the scientific method applied to decisionmaking.

It isn't, really. Human judgment is used in designing the analysis; in deciding what alternatives to consider, what factors are relevant, what the interrelations between these factors are, and what numerical values to choose; and in analyzing and interpreting the results of the analysis. This fact—that judgment and intuition permeate all analysis—should be remembered when we examine the results that come, with apparent high precision, from analysis. The ideal is to keep all judgments in plain view and to make the logic behind the opinions explicit. For if these things are not explicit, we surrender the three great advantages that the analytic approach has over its competitors—namely, that someone else can examine the work, evaluate it, and modify it as new information or insight becomes available.

THE ADVANTAGES

Systems analysis falls short of being scientific research because its predictions ordinarily cannot be verified and the urgency of military problems forces the substitution of intuition for verifiable knowledge. But, in contrast to other aids to decisionmaking, it extracts everything possible from scientific methods, and its virtues are the virtues of those methods. Furthermore, its limitations are shared by its alternatives. And if we exclude intution or "unbuttoned" judgment, these alternatives are also analysis—but poorer analysis, less explicit, less systematic, less quantitative.

What is there about systems analysis that makes it better or more useful than other ways to furnish advice? To a large extent, systems analysis is a successful technique in areas where there is no accepted theoretical foundation (defense planning being an example), precisely because it is able to make a more systematic and efficient use of judgment than any of its alternatives. The essence of the method is to construct and operate within a "model" —an idealization of the situation appropriate to the problem. Such a model—which may a computer program, a war game, or a set of scenarios—introduces a precise structure and terminology that serve primarily as a means of communication, enabling the participants in the study to make their judgments in a concrete context, and through feedback—the results of computation, the countermoves in the war game, or the critique of the scenarios—it helps the decisionmaker, the analysts, and the experts on whom they depend to arrive at a clearer understanding of both the context and the problem.

At the very least, systems analysis can provide a way to choose the numerical quantities related to a weapon system so that they are logically consistent with each other, with an assumed objective, and with the calculator's expectation of the future. But systems analysis strives to do more than simply supply solutions that correctly follow from sets of arbitrarily chosen assumptions in narrow problems. It aspires to help the decisionmaker find solutions that experience will confirm in the broadest of problems.

What can we say about the future of systems analysis in defense and national security problems?

Resistance by the military to its use in broad problems of strategy is gradually breaking down. Military planning and strategy has always involved more art than science; what is happening is that the art form is changing from an ad hoc, seat-of-the-pants approach based on intuition and experience to one based on analysis supported by intuition and experience. Military planning itself has changed. With this change the computer is becoming increasingly significant—as an automaton, a process controller, a service trouble-shooting technician, a complex information processor, and a decision aid. Its usefulness in serving these ends can be expected to grow. But at the same time, it is important to note that even the best computer is no more than a tool to expedite analysis. Even in the narrowest military decisions, considerations not subject to any sort of quantitative analysis can always be present. Big decisions cannot be the automatic consequence of a computer program, of cost-effectiveness analysis, operations research, or any application of mathematical models, or of systems analysis.

For broad studies, involving force posture and composition or the strategy to achieve foreign policy objectives, intuitive, subjective, even ad hoc study schemes must continue to be used—but supplemented to an increasing extent by systems analysis. And as ingredients of this analysis, in recognition of the need for judgment, a greater use of scenarios, gaming, and other techniques for the systematic employment of experts can be expected, along with an increasing use of quantitative analysis for problems where it is appropriate.

The analytic method, in contrast to its alternatives, provides its answers by processes that are accessible to critical examination, capable of duplication by others, and readily modified as new information becomes available. As has been pointed out, however, much more is involved than the collection of information and its manipulation in mathematical models. But, whatever approach is used, asking the right questions, inventing ingenious alternatives, and skillfully interpreting the results of the computations and relating them to the many nonquantifiable factors are all part of the analytic process. Now, as in the foreseeable future, these steps may prove more helpful in decisionmaking than thousands of machine computations or a thorough knowledge of sophisticated operations research techniques.