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COSTING TOMORROW'S WEAPON SYSTEMS

David Novick

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SANTA MONICA • CALIFORNIA

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PREFACE

This paper was presented at the Air Force Systems Command's Management Conference held in Monterey, California, from May 2 to 5, 1962. It discusses some of the results of a continuing study of military costing problems being conducted for the United States Air Force by the Cost Analysis Department of The RAND Corporation.

For a more detailed discussion of the problems of costing advanced weapons and program budgeting, the reader is referred to the following unclassified publications by D. Novick: RM-1759, A New Approach to the Military Budget, June 1956; RM-2116, Economic Considerations in Air Force Decisions, February 1958; RM-2695, System and Total Force Cost Analysis, April 1961; P-2222, New Tools for Planners and Programmers, February 1961; and P-2556, The Cost of Advanced Weapons, February 1962.

> Because of the technical complexity of modern-day weapons, their lengthy period of development, their tremendous combat power, and their enormous cost, sound choices of major weapon systems in relation to military tasks and missions have become the key decisions around which much else of the Defense Program revolves. But the full cost implications of these decisions, present and future, cannot be ascertained unless both the programs and their cost are projected over a period of years.

These words, which may have a familiar ring to some of you, were spoken by Defense Secretary McNamara in recent testimony before the Senate Armed Services Committee. As most of you are well aware, the Air Force, along with the Army and Navy, is now required to estimate the cost implications of individual weapon systems out through FY 1967; and in my opinion the desirability of projecting costs at least that far into the future is indisputable.

Effective planning and programming require a full understanding of the long-range implications of decisions to allocate resources. A decision to procure a given quantity of hardware carries with it an obligation for facilities, acquisition and training of personnel, personnel housing, support equipment and a host of other related items, all of which must be paid for. In addition, a procurement decision implies a decision to incur annual recurring costs so long as the system remains in the inventory. A full identification of the timing and cost of these requirements is essential to a full understanding of the resource impact of a given decision. It is to provide the Secretary of Defense and his military advisors with this understanding that the new programming/budgeting system has been instituted.

As I stated earlier, I consider the desirability of projecting resource requirements five years or more into the future to be axiomatic. Criticisms

of the procedure are generally not on the grounds of desirability, however, but on the grounds of feasibility. Is it really possible to estimate development, procurement and operating costs of weapon systems, many of which are still not completely defined, with sufficient accuracy to use these estimates for making major program decisions? Persons posing this question can put together a hair-raising story based on case histories of past programs -- the Falcon missile, BOMARC and Snark are frequently cited -- on the magnitude of error possible in estimating the cost of advanced systems.

In defending a policy of attempting to estimate advanced weapon system costs I am sometimes tempted to quote Samuel Johnson's comment on women preachers. He observed that a woman preaching is like a dog's walking on its two hind legs. It is not done well but you are surprised to find it done at all. However, since Johnson made that remark women have learned to preach very well and, similarly, we are learning how better to estimate costs. But before discussing methods of improving resource estimates we should attempt to settle the question of whether there is any real alternative to costing future weapon systems. There are people, it is true, who prefer not to know the total cost of a system in advance because large costs tend to prejudice the changes that system has for development. In justification of this point of view they point out that many useful, perhaps essential, programs have started this way. Saturn was sold as a $72 million feasibility demonstration, but the program was expanded into a full-fledged development effort and is now expected to cost almost a billion dollars. The Mercury program was originally estimated at about $200 million, something like $300 million off the currently expected actual. It would be an exercise in metaphysics

to try to decide whether or not these programs would have been started if the ultimate costs had been known, but I suspect that for every situation of this kind that has turned out well, it would be possible to find two that have turned out badly. The Hughes flying boat, Rascal, and the ANP program are a few examples of where a knowledge of total cost might have saved the nation hundreds of millions of dollars.

In essence, the alternative implied here is one of "muddling through," forsaking analytical tools, and trusting to chance and individual judgment instead.

There is a "back to the good old days" ring to this that may appeal to some, but even in the good old days when national security problems were far less complex than they are today, human judgment was fallible and chance could be capricious. World War I is replete with examples of how reliance upon mystique rather than analysis proved disastrous.

No, I think the only real alternative is to develop better analytical techniques, to improve the accuracy of cost estimates so that the planner can have confidence in the cost inputs to his analysis and the programmer will be spared the costly and painful reprogrammings that result when the money runs out.

Improvement in the costing of tomorrow's weapon systems can result from several different kinds of actions. You men, as executives, might be able to accomplish something by administrative action alone. In this connection, I am reminded of the article that appeared in the Wall Street Journal several years ago which told of the methods a number of business concerns had used to go from a poor earnings situation to a good one in a one-year period. Some of the companies had instituted profit-sharing plans, others had

installed electronic data-processing equipment, others had centralized or decentralized depending on which they had done last. One concern, however, a national grocery chain, had a much simpler solution than any of these -- the manager of every division that was losing money was fired. I do not suggest this procedure will work in all or even in many situations, but in the matter of cost estimates, it may be that the incentives to estimate low are much greater than the penalties, if indeed there are any penalties. If the situation were reversed, I would expect estimates to improve.

How much they would improve is hard to say, because our studies suggest that the major cause of poor cost estimates is neither incompetence nor deliberate understatement but an endemic inability to appreciate the difficulty of the job to be done, or to put it another way, the perennial optimism so characteristic of Americans. This optimism pervades estimates of the number of engineering hours required to develop a piece of hardware, the amount of test hardware required for a test program, the performance of the equipment, the personnel required to man and maintain it, and so on. Estimates of system reliability have a particularly poor record for accuracy, as do estimates of system weight, and weight growth can have a surprising impact on cost, reliability and performance.

Some six or seven years ago we completed a small study of the weight growth of manned aircraft that indicated about a 10 per cent increase throughout the life of a system, and this percentage has increased sharply for missile and space systems. In the manned aircraft case, these increases have had the effect of significantly degrading performance; i.e., range and/or payload, although no case has been recorded where the weight increase has made takeoff impossible. For missile and space systems weight increases

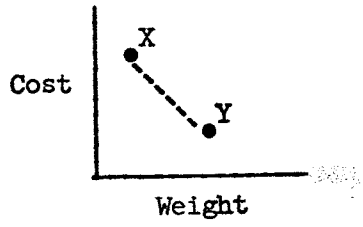
have not only degraded performance but in several important cases have seriously threatened the entire mission.

Then there are the weight increases that do not occur but are prevented by redesign, and here it is not performance that suffers but the budget and system reliability. While it is difficult to isolate this cost, winning the weight battle is never cheap. It has probably cost hundreds of millions in R&D costs and five to ten points of reliability on a number of weapon and space systems. So let us be charitable and say that all of us suffer from myopia when trying to peer into the future. The important thing to remember is that this condition is aggravated, not corrected, by wearing rose-colored glasses.

For cost analysts a corrective lens is close at hand -- cost data from previous weapon system programs. There is an old saying among cost analysts that an ounce of data is worth a pound of methodology. The only way future weapon systems can be costed is on the basis of how much things have cost in the past, and the person who ignores this dictum, believing past experience to be irrelevant, does so at his own peril. The more futuristic in design or the greater the advance in manufacturing state-of-the-art required, the more valuable a library of data on past systems costs and on understanding of the interelationships between cost elements. Most of the individual components of the space systems of tomorrow have their analogs in the ballistic missile, supersonic aircraft, and ground electronic systems in existence today. The old rule of thumb, "20 per cent new, 80 per cent old," is still a pretty good one, and a skillful estimator can project the cost of the 20 per cent new on the basis of existing cost histories. System integration and system testing requirements are admittedly more difficult

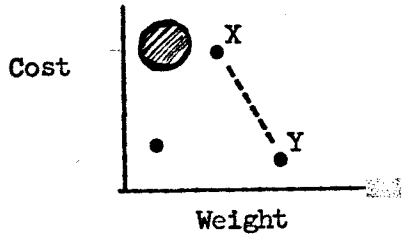
to estimate, especially for space systems because of the high costs and uncertainties associated with space simulation. But this situation will improve as our fund of experience grows. In the meantime we can do a much better job than has been done in the past just by making use of data already recorded.

To illustrate this point I would like to cite several examples of estimates during the past few years which appear to have been made in a vacuum; that is, data and experience from previous and existing programs were ignored. These are real examples but to avoid some of the problems which arise when talking about actual costs I am going to keep the discussion as general and the systems as anonymous as possible. The first example involves an inertial guidance system for a ballistic missile. At the time this guidance was proposed, two other comparable systems existed and their costs were known. It had also been pretty well established that for specified accuracy and range requirements you could not make a guidance system cheaper by making it smaller. In fact, the assumption was just the reverse. On the basis of the costs of Systems X and Y it appeared the relationship between cost and weight was of the nature shown on the sketch below:



A new system was proposed that would have greater accuracy and a much longer mean time between failures. In addition it would withstand higher g loadings, and weigh only half as much as System X. Best of all it was to cost only one-third as much as System X. Instead of being somewhere in this shaded

area where a prudent estimator might place it, the cost was down at the point indicated by a black dot. As it turns out, the prudent estimator would have been right. The cost today is in the upper half of the shaded area.



You may accuse me of choosing a particularly egregious example to make my point here, and to an extent this is true, but examples of this sort are not hard to find. Consider some recent proposals by solid propellant manufacturers in the light of what we know from the Minuteman program. We know the cost to develop the Minuteman first-stage and the weight of that stage. We are told that it is possible to develop a new solid-propellant stage weighing ten times as much for one-tenth the cost. The only major difference between the two stages that would suggest an easier development program for the larger is that the requirement for a high mass fraction has been relaxed. The reason for the low estimate appears to be a belief that all the problems have been solved in previous programs, and therefore the costs of previous programs are irrelevant. This may be the judgment of engineers and propulsion experts and in any given case they may be right, but it is the statistician who will be right much more often. While he does not know what the problems will be any more than the engineers do, from examining the records of previous programs he has an idea of how the problems and their efforts will be distributed and can make an allowance for these.

The two examples above show how data can be useful in what might be

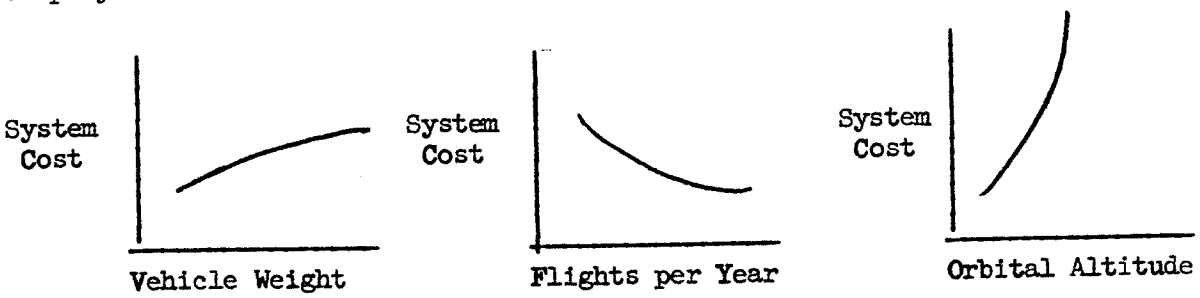
called go - no-go checks of someone else's estimate. The estimating process itself is generally much more detailed and, because people have been making estimates for a good many years, quite well understood. The cost to do a certain job in a certain way can usually be estimated to within 20 per cent of the actual cost, and if this amount of error can on occasions be catastrophic to a private concern, it is not insignificant in an estimate of the cost of a future weapon system. The order-of-magnitude underestimates that get the Pentagon and Congress so disturbed generally stem from "requirements uncertainty" rather than an inability to estimate correctly. "Requirements uncertainty" means that frequently the certain job cannot be done in a certain way, that it must be done in a different way, or perhaps that you just cannot get there from here. For example, with respect to a new aircraft, the original design may not produce the required performance characteristics and as a result the hardware configuration must be changed. Or, as is frequently the case, the certain job turns out to have been very uncertain -- hardware performance characteristics may be changed with a resultant change in hardware specifications, hence, cost. Or perhaps the method of deploying and employing a weapon system may be changed. A higher degree of dispersal, hardness or alert capability may be required to meet a new strategic situation. Too, a re-evaluation of the strategic situation may produce changes in system force size or in some cases a change in the number of years the system is planned to be kept in the operating inventory. And even if force size (when expressed in terms of number of combat wings or squadrons) is held constant, the total number of units of major equipment to be procured can still vary considerably due to changes in authorized Unit Equipment, expected attrition rates, or in the case of missile systems, changes in the number of authorized training firings.

There are only a few examples of the kinds of changes that contribute to requirements uncertainty. The key point is that requirements uncertainty can lead to wide variations in total system cost, even in the complete absence of cost-estimating uncertainty (if that were ever possible), and in costing tomorrow's weapon systems we must have a rational and defensible means of dealing with this problem. I emphasize "defensible" to rule out the fudge-factor approach, even though the consistent underestimation of costs in the defense industry seems to suggest some statistical manipulation as a means of improving the validity of cost projections. While it probably is true that adding some stochastic term to a cost model might improve many requirement estimates, we feel improvement should come from another direction.

In my judgment the basis for any approach to the problem is an acceptance of the idea that a certain amount of uncertainty is inevitable in any action occurring in the future. Once having admitted that, you can look at a weapon system description and single out the areas of greatest uncertainty and try to put some limits on these. For example, consider trying to estimate the total system cost of an aerospace plane -- a manned aircraft that can take off from a runway, fly into orbit, and after completing its mission there, fly back to earth and land. Among the characteristics that are unknown are the size of the vehicle, the number of flights it could make per year, the missions it would be used for and the attrition and wearout rates. The unknowns far outnumber the knowns, if indeed there are any knowns in a system as far-out as this one. Yet let us assume it would be desirable for planning purposes to compare the cost of using an aerospace plane for performing a number of missions with the cost of using several different expendable boosters. It is possible, using a range-of-values approach, to

come up with a range of cost estimates.

By a range-of-values approach I mean looking at a range of vehicle weights, a range of utilization rates, etc. Thus we would have a series of displays like these sketched below:



Further, an analysis of these will indicate those particular system characteristics to which total system cost is sensitive and those to which it is insensitive. In our aerospace plane example we might find that for a range of vehicle weights, utilization rates, missions, and attrition and wear-out rates, the total system cost is so great as to be meaningless. Closer scrutiny might reveal, however, that a major part of this variation comes from a single system characteristic, say, mission altitude. By limiting the system to low-altitude missions, the variation in cost might be reduced to a range small enough to make meaningful comparisons with other systems possible.

This process of looking at a range of values is sometimes called cost sensitivity analysis. It appears promising to us because it highlights the uncertainty inherent in future weapon system costs, and gives the planner a full view of the cost implications of decisions affecting system configuration and operations. In addition to program changes resulting from decisions there are, of course, the unexpected problems which invariably arise creating serious program delays, reliability degradation and increased costs. While these cannot be predicted in detail, sensitivity analysis should

instruct system planners on the relative magnitude of variation to be expected. Thus while sensitivity analysis does not per se point to unexpected difficulties, it can serve as a kind of weather map showing the likely approach of a cold front.

But while cost sensitivity analysis is useful in helping to cope with the uncertainty problem, it has its limitations. A practical difficulty is that these analyses require a lot of work, especially when compared to single-point estimates. A related difficulty is that the method generates a lot of numbers and the user may not see the forest for all the trees blocking his view. Probably a more serious criticism is that there is no guarantee that all the relevant alternatives will be included in any given analysis. And, I suspect, until we become more imaginative in choosing alternatives for analysis the final configuration of future systems will frequently fall outside the ranges of values examined. But here again the difficulties of analysis should suggest that analytical techniques be improved, not abandoned.

I am not presenting sensitivity analysis to you as the final solution to the problem of the uncertainty that pervades future weapon system costs. This meeting has been very aptly called a seminar, and taking the word "seminar" literally as a derivation of the Latin word "seed," what I hope to achieve with this paper is to plant a few seedlings either to flourish or wither in the discussion to come.

Before we begin our discussion, however, I would like to recapitulate briefly some of the main points of this paper. Costing tomorrow's weapon systems is a difficult and uncertain but essential process: essential for budget preparation and essential for major program decisions. As far as the

first application is concerned it is difficult to conceive of budgeting for a weapon system project without cost estimates, regardless of their validity. The decision-making application is more controversial. As a result of some very low estimates in the past, some analysts have taken the position that the use of cost estimates in systems analysis should be minimized or even eliminated. The basic flaw in this argument is that poor cost estimates are merely a reflection of the inadequacies of weapon system designers and planners. Cost analysts work from a program description provided by the other members of the team, and by far the most significant variance between initial estimates and final costs are due to changes in the original requirements rather than to an inappropriate price tag hung on an unchanging configuration.

In a world as dynamic as ours we cannot prevent changes nor would we want to. What is needed is a cost structure that will accommodate possible changes and show their implications before final decisions are made. Too, we must recognize that we frequently cannot predict how much an aircraft will weigh, how long a satellite will operate or how many test vehicles will be required for a given R&D program, and therefore must examine the cost implications of variations in program and system characteristics.

This really argues for a larger rather than a smaller role for future system costing and the trend seems to be in that direction. Certainly within AFSC the emphasis has increased markedly over the last year or so. It appears that within the foreseeable future AFSC will have a strong in-house cost analysis capability, and this will have a salutary effect on financial management throughout the command. It may not make planning and budgeting for systems acquisition any easier than it is today, but it should certainly make it better -- and that, I think, is what we are all striving for.