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PREFACE

These papers on some aspects of national security were written in the period 1964-1968, when the author was a member of the staff of The Rand Corporation. Their continuing relevance extends beyond questions of defense to governmental planning and decisionmaking in general. Because of the many requests for these papers, which are now out of print, they are compiled here and reissued, in their original form.

The subjects of the papers fall into two broad categories. The first is arms control and the spread of nuclear weapons—the interactions between arms and arms control, and the costs and consequences of nuclear proliferation. The second is defense decisionmaking—the uses and limitations of systematic analysis, planning in the face of massive uncertainty, and centralization and decentralization of planning and budgeting.

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ARMS CONTROL AND THE SPREAD OF NUCLEAR WEAPONS

THE STRATEGIC CONSEQUENCES OF NUCLEAR PROLIFERATION

Rand Paper P-3393
June 1966

This paper was developed from the author's panel comments recorded at the Third International Arms Control Symposium, held in Philadelphia, April 1-3, 1966.

My responsibility in this Symposium is to examine the strategic consequences of nuclear proliferation. If we limit ourselves strictly to the strategic area—to the possible employment of additional nuclear capabilities against military or urban targets—one cannot avoid the conclusion that considerable exaggeration has crept into public discussion of proliferation's consequences. This observation rests, in part, upon a distinction between strategic and socio-political consequences, which some will regard as arbitrary and which in any event cannot be made precise. If we do isolate the strategic from the socio-political consequences, it is plain that the latter could be quite serious. The very countermeasures through which harmful strategic consequences can be avoided are likely to be viewed as undesirable on social or political grounds.

At the very least, the spread of nuclear weapons generates fear. When publics or governments become fearful, they can act in ways which seriously reduce the amenities of living in society. For example, one possible result of the spread would be for societies partially to close their borders and to police incoming goods and people more carefully than at present. This might easily be associated with the decline in the tolerance of dissent within the society. In the specific case of the United States, it is sometimes felt that the idealistic flavor characterizing much of its foreign policy would tend to disappear—to the disadvantage particularly of those who live in the underdeveloped world. These are consequences which few would view without some trepidation. But these are *political* consequences. They do not imply a major alteration of the military balance or for that matter the physical security of most of the world's population.

For these reasons I believe that most public discussion of proliferation's *strategic* consequences is seriously defective. However, nothing that I say regarding the exaggerations of these strategic consequences should be construed as a criticism of the basic objectives of U.S. policy or of the desirability of preventing further nuclear spread. I am dubious whether any such policies can be completely successful. There is a danger in expecting too much as well as in being too fearful. Moreover, since I believe the effects of proliferation would be less severe than currently anticipated, I would be inclined to set a lower price on what the United States should be willing to pay to prevent proliferation than would some other members of the Panel. In particular, I would be reluctant to pay a very high price in terms of offending friendly nations merely to get paper acquiescence to a nonproliferation treaty.

Nevertheless, despite the exaggerations of public discussion, it is plain that we should bend our efforts to avoid or to limit the spread of weapons. Proliferation adds to the problem of managing the world. It increases both the number of uncertainties and the number of variables that must be watched. One can put the menace of proliferation in another way, as has William C. Foster, the Director of the Arms

Control and Disarmament Agency: Further nuclear spread would lead to a reduction of the relative influence of the United States on the world scene. This appears like a self-serving plea, and for this reason, the argument may have less initial appeal to outsiders than to Americans. Nonetheless, the argument contains a surprisingly large element of altruism. A decline in the relative influence of the United States on the world scene may be more closely associated with increased difficulty in keeping the world relatively stable and peaceful than many non-Americans might be willing to concede at first blush. In a world in which nuclear weapons were more widely held and in which the United States sought to avoid "entanglements," the gravest misfortunes would be reserved for the populations in the unstable portions of the world rather than for the favorably situated publics of the nuclear superpowers.

SOME EXPRESSIONS OF CONCERN

If, then, we acknowledge that there are weighty reasons for opposing the spread, how much despair should we be prepared to feel, if our efforts at control turn out to be unsuccessful? It is on this point that prophecies of disaster appear to dominate public discussions and that public statements diverge most sharply from a sober assessment of the risks. The view that nuclear spread poses a single, overwhelming threat to the continued existence of mankind strikes me as a distortion of reality which, if taken seriously, could lead to a misallocation of our national efforts. I suggest that the noticeable discrepancy between the paramountcy nominally attributed to the problem and the policies we stand ready to adopt indicates that the more extreme expressions of alarm are not, in fact, taken too seriously. Among leading public figures Senator Robert Kennedy has most vividly dramatized the disastrous consequences to be expected from proliferation. In a recent statement he asserted that alongside proliferation control "nothing else means anything." This is a bit of political hyperbole, the force of which would appear to be weakened by the Senator's allocation of his own energies. He himself devotes intense effort to numerous other issues, but aside from public statements has given relatively little attention to the problems of proliferation.

To move from the survival of mankind to the survival of the United States, we have heard on even higher authority that the survival of the United States is at stake—if we fail to prevent the spread of nuclear weapons. Once again, for reasons that will be extensively developed below, this statement is simply misleading. The risks to the American society—and in this respect we must distinguish sharply between the American society and societies in the third world—are very much exaggerated. The United States is in a position to reduce the risks to itself to very low levels. The United States can both adjust its policies and adopt countermeasures which reduce the damage that limited nuclear capabilities could inflict. Such countermeasures would maintain or increase the already enormous gap between U.S. military capabilities and those possessed by non-superpowers. A package of such countermeasures could sharply reduce the risks of proliferation in several respects: (1) the ability to inflict damage on the United States would be kept low, (2) the United States, if it so desired, would remain in a position to deter attack by lesser nuclear powers against third countries, and (3) the incentives to acquire capabilities would consequently be altered—and possibly reduced toward the vanishing point. Countermeasures taken by the Soviet Union would, of course, reinforce the process.

I would argue that the ability to implement such countermeasures—to make crystal clear to all nuclear aspirants that acquisition of nuclear weapons cannot

significantly alter the strategic balance—provides the best hope over time of controlling proliferation and its consequences. My co-panelist, Hedley Bull, has characterized this position as one of "high posture." He has—on this occasion as on prior ones—expressed misgivings regarding its suitability. I will not at this time argue whether the psychological repercussions of augmenting the gap between the superpowers and other states will be of the sort that Mr. Bull foresees. My point at this juncture is to indicate (a) that such countermeasures are well within the capacity of the United States, and (b) that the survival of the United States is scarcely brought into question by the further spread of limited nuclear capabilities. This last spectre is one we had best put to rest.

I cannot imagine that any American President will fail to accept some of these countermeasures. However, let us place such issues to one side. Whatever their resolution, I think it plain that, if proliferation takes place, we shall go right on living with it. We may continue to complain about it, but we shall live with it, while continuing to enjoy the benefits—if that is the appropriate term—of a rising standard of living. The very leaders who now assert that nonproliferation is indispensable to our security will then find other subjects to dramatize.

I have been attempting to allay some of the anxieties regarding proliferation. This is necessary not simply because we should recognize that existence will continue to be quite tolerable, even if proliferation takes place. What is perhaps more important, an attitude of desperation regarding the spread of nuclear weapons is not merely inaccurate, but may also be counterproductive in terms of achieving control. By understating the difficulties of acquiring a serious nuclear capability and by exaggerating what a nuclear aspirant power may obtain through acquisition of a capability, we may strengthen the incentives for acquisition. The danger inherent in exaggerated chatter regarding the damage that additional capabilities can foster is that it revivifies the false notion of nuclear weapons as "the great equalizer" in international conflict. Hopefully, most nations will penetrate the smokescreen and perceive the difficulties. However, some may be lured into believing that nuclear weapons do provide an answer to their security problems. Others may be encouraged in the notion that acquisition will provide an instrument of threat or blackmail which can be directed toward the rest of the world.[1]

To illustrate the way in which such illusions can be fostered, Senator Kennedy has misappropriated some words of his brother, the late President, to the effect that "every man, woman, and child lives under a nuclear sword of Damocles hanging by the slenderest of threads, capable of being cut at any moment by accident or miscalculation or by madness." [2] The Senator's point is that each additional nuclear capability, no matter how limited, automatically creates an additional Damoclean sword. But this suggestion is simply not true. In relation to the indicated levels of destruction, the damage potential of small nuclear forces is too limited. In assessing the damage that might result from nuclear spread, it is essential to recognize that nuclear proliferation is as much or more a quantitative problem than it is a qualitative problem. The quantitative aspects are subject to calculation, but in the public excitement over the threat of proliferation these calculations are normally ignored. Such oversight seems indispensable in generating both needless anxiety and the nuclear-weapons-as-equalizers illusion.

[1] Fortunately, a number of those nations to which such a motive might reasonably be attributed appear to have such weak technical and industrial bases that it is doubtful whether they could develop a capability, even if so moved.

[2] The nuclear sword of Damocles in President Kennedy's United Nations address quite obviously referred to American and Soviet capabilities. While some poetic license seems understandable in relation to such vast destructive power, it does not seem particularly relevant in relation to the extremely modest forces that other powers could develop.

DIMENSIONS AND MEASUREMENT

Any serious attempt to assess the dimensions of the proliferation threat should begin with some calculations regarding the spectrum of strategic capabilities given varying levels of investment. Further development of the point that proliferation in certain essential respects is a quantitative problem is basic to our understanding. Proliferation is really quite unlike pregnancy, though in the intuition of many something akin to pregenancy is used as a rough analogue. It is frequently observed —usually by way of admonition—that there is no such thing as being a little bit pregnant. But this is because the results and the time involved in the process are pretty well defined. In size and weight, full-term babies tend towards a normal distribution; the variance is not a matter of great moment. But suppose that in pregnancy there were no tendency toward a unimodal distribution of the results and that the time involved in gestation were subject to enormous variation. Suppose again that the ultimate progeny could be Lilliputians or Brobdingnagians—or, for that matter, a varied assortment of misshapen dwarfs, possibly lacking essential organs, limbs, or faculties—and that the specific result depended upon not only the intake of the mother but her intelligence. This is really a more revealing analogy. It explains why being a *little bit proliferated* may be a meaningful concept, while being a little bit pregnant is not. In this area controlling the ultimate dimensions may be even more important than preventing conception or birth.

The range of possible nuclear capabilities is simply enormous. One must be aware of the importance of the distinctions to be drawn among capabilities—and how these distinctions relate to size and vulnerability. Consider the existing array of nuclear capabilities. The United States, which has invested most heavily, possesses a capability which is not only a solid deterrent, but which is not incredible in terms of a carefully controlled, countermilitary initial strike. The Soviet Union, which has invested less, has an impressive second-strike force, which is an effective deterrent. Britain possesses a much more limited capability, presently dependent for delivery on obsolescent aircraft, which is continuously waning in terms of influence and credibility. The French capability is even more limited in terms of damage potential against the Soviet Union, though it promises to exploit more advanced delivery systems indigenously produced. Finally, the Chinese capability—presently drawing the lion's share of attention—is barely past the embryonic stage. There is some question whether it should even be referred to as a *capability.*

The degree to which a nuclear capability is strategically exploitable—substantially dependent on the credibility of the threat to employ—is determined by its size and sophistication and by the vulnerability of the society it is designed to protect. Strategic posture ultimately depends upon the ability to inflict and to limit damage. All these are roughly correlated with the volume of resources the society has invested or is able to invest in its capability. Happily for the wealthy and powerful, this ability is subject to considerable variance. Somebody has observed: there is no cheap substitute for money. It is doubtful whether the inexorable requirement for money is anywhere more decisive than in relation to the development of a nuclear capability. Sophisticated nuclear weapons and sophisticated delivery systems are terribly expensive. The cost of developing a capability which could seriously disturb the superpowers (as opposed to one's unarmed neighbor) is staggering.[3]

Let me indicate roughly what kind of sums are involved. In order to develop a convincing second-strike capability against one of the superpowers, a nation must

[3] The potential of small nuclear capabilities for precipitating regional confrontations or regional destruction would remain as a major source of trouble. Its attenuation—in the case of continued great power involvement—will be treated below.

be prepared to spend billions of dollars annually—and these expenditures would continue for a decade and longer. Estimates differ; five billion dollars a year may be too high and three billion dollars a year might be adequate. These sums, however, run well beyond what most nations have been prepared to spend—including some that are present members of the nuclear club. Resources will be required not only for delivery systems and compatible weapons, but also for certain supplementary capabilities whose costs are rarely reckoned. Who includes such indispensable items as reconnaissance and intelligence in the list of required outlays? But any nation contemplating a confrontation with a superpower had better learn something about the location of targets and about the location and capabilities of its opponent's air defense and missile defense systems. The upshot is that only through very heavy outlays can a nation develop more than a very minimal threat against a superpower.

To illustrate the problem, let us consider some historic cost figures. Take the matter of weapons development and stockpiling. Down to this point in time the United States has invested on the order of $8 billion in the development of nuclear weapons. On AEC operations generally, it has now appropriated close to $40 billion. These are substantial sums. How many nations are in a position to spend even 20 percent or 25 percent of these amounts? Yet, for the creation of a serious capability, requiring deliverable weapons in the megaton range, heavy investment in weapons development is unavoidable.

Though the spread of *missile* capabilities is now a matter of increasing concern, the problem of compatibility implies that development of advanced weapons is preliminary to deployment of an effective missile force. To develop a warhead for an early-generation missile with limited thrust and size (the goal of a development program or the initial goal of a program for an aspiring nuclear power), there must be heavy investment in weapons testing in order to get yield-to-weight ratios to a point where a weapon adequate for target destruction can successfully be delivered in the vehicle. Moreover, there will have to be major investment in guidance technology simply to insure that missiles will be accurate enough to place weapons near the point targeted—whether military bases or cities. In this respect, it is vital to recognize the tradeoff between weapon size and weapon accuracy. With very large yields, considerable inaccuracy may be tolerated. However, with the very-low-yield weapons of the sort that can be developed with small amounts of money yet be delivered with limited-payload vehicles, the accuracy requirements become very severe. Yet, missile accuracy is neither cheap nor easy to obtain.

The implication is that no nation is going to be in a position to develop a strategic capability which is both sophisticated and cheap. In the absence of major investments or extraordinary outside assistance, the only option open to most nuclear aspirants is the aerial delivery of rather crude nuclear weapons. Though such capabilities can, of course, dramatically transform a regional balance of power (if the superpowers remain aloof), the superpowers themselves will remain more or less immune to nuclear threats emanating from countries other than the principal opponent. For the foreseeable future, only the Soviet Union will be able to deliver the requisite megatonnage to threaten major devastation in the United States. Threats from other quarters may be faced down.

The superpowers therefore will remain in a position in which they can dominate any nuclear confrontation. Only a superpower—and in this connection the term applies particularly to the United States—will be able to intervene in such confrontations in third areas. If it desires to pay the costs and is willing to run the risks, other nations—including the present three minor members of the nuclear club—will continually be deterred. Not only will they be precluded from implementing nuclear threats, but in the relevant cases, their capabilities will remain vulnerable to a

disarming first strike—unless they are given protection by an associated superpower. In any showdown with a superpower, a minor nuclear power relying on its own resources will simultaneously be deterred and be subject to disarming.

This asymmetrical relationship between major and lesser nuclear powers brings us back to a point raised earlier: why it may be counterproductive to talk in a panicky way about proliferation's threat to mankind-as-a-whole. If we are to dissuade others from aspiring to nuclear capabilities, what we should stress is that, if weapons spread, they are likely to be employed in third area contexts. The penalties for proliferation would be paid, not by the United States or the Soviet Union, but by third countries.

The likelihood that the first nuclear war, if it comes, will originate in and be confined to the underdeveloped world should play a prominent role in any assessment of proliferation's consequences. The tenor of the existing discussion of proliferation has led some in the underdeveloped countries to conclude that the major powers would be the chief beneficiaries of curtailing the spread. If nuclear spread is to be effectively opposed, it should be made crystal clear just whose security is placed at risk and whose is not.

COUNTERMEASURES

The problem of nuclear spread is not exhausted by the attempt at prevention. The effort to dissuade additional states from acquiring nuclear capabilities, while good in itself, is not likely to be wholly successful. Control includes much more than simple contraception. Influencing the character and consequences of whatever nuclear spread takes place should not be neglected out of disappointment with the "failure" to prevent proliferation entirely.

We should recognize that the long-run problem is how to live with the spread with minimum risk. This implies that control will require continuing effort over time; it is not an all-or-none problem to be settled in some particular time period. If we take the position that the issue is simply one of *counting* those nations claiming nuclear weapons status and that *if this number increases* we are undone, then we will fail to examine the second-stage opportunities for control.

Given a policy of minimizing the number of nuclear powers, there should be additional strings to the nonproliferation bow to be employed as the number of nuclear weapons states increases. What are these additional strings? First, if new weapons programs are launched, we may hope to keep the resulting capabilities as limited as possible. (This would reduce the damage potential of any nuclear wars taking place in third areas.) Second, we can take steps to reduce the risk that these capabilities, whatever their size, will actually be employed. Moreover, any actions which sharply reduce the size or the risk of employment of additional capabilities may also serve to weaken the motives for acquisition.

Under the heading of limiting the size of additional capabilities, the methods at our disposal are indirect ones. Recognizing the ordinary tradeoff between cost and quantity, our actions should be designed to keep the cost of strategic capabilities at a high level, thereby weakening the temptation to acquire larger capabilities. This implies no direct assistance to strategic nuclear programs of other nations—save in rare and unusual circumstances. Through rigorous strategic trade controls we may also hope to limit indirect assistance. Above all, we should make every effort to see that international assistance intended for the support of peaceful nuclear programs is not diverted to support of military programs.

These are not easily achievable goals, and we ought not pitch our definition of

success at too high a level. The instruments for control are imperfect. Moreover, costs of themselves cannot exclude other nations from seeking nuclear capabilities. Given the existing system of national sovereignties, the ability to influence the decisions of other states is quite limited. Nonetheless, something can be achieved. To whatever extent we can hold up costs, we can limit the size and the potential destructiveness of budding nuclear capabilities.

The second heading—reducing the risk that new capabilities will be actually employed or, if employed, limiting the potential damage—represents that aspect of living-with-proliferation-at-minimum-risk over which we ourselves have most control. There are certain hardware possibilities and other physical arrangements that can limit the potential for damage. One obvious possibility may be to buttress the air defense capabilities of threatened states. A more controversial possibility is the deployment of new systems that will sharply reduce the damage that Nth countries could inflict on the major nuclear powers. Elsewhere in this Symposium a panel discussion has been devoted to the most dramatic current illustration of this type of possibility: the deployment of an ABM[4] system. I would not wish my remarks to be taken as an endorsement of the ABM system, for that decision involves complex arms control, strategic, and cost-effectiveness calculations, which are beyond the scope of my responsibility. I should, however, like to stress one particular aspect that is relevant to the final decision: deployment of an ABM system or other systems that substantially reduce the damage that can be inflicted on the United States may serve to curtail the harmful consequences that could flow from proliferation. Through such damage-limiting measures, the willingness and the ability of the United States to intervene in third areas when the use of nuclear weapons is threatened is enhanced. Consequently the American ability to prevent the misuse of nuclear capabilities will be strengthened. The strongest deterrent to a lesser nuclear power's employing its capability is the possibility that a major nuclear power will enter the lists against it.

Given the existing preponderance of American power, the deployment of major new systems may not be essential to achieve this result. However, certain types of developments do appear desirable in order to exploit the discrepancy between major and lesser nuclear powers for the purpose of driving home the ill-advisedness of lesser powers initiating the use of nuclear weapons. For example, in a world of many nuclear powers in which anonymity is at least a hypothetical possibility, we should invest considerable effort in developing methods for "fingerprinting" nuclear weapons, and parallel systems through which we may in a crisis quickly ascribe responsibility for any detonation that occurs. Then, if we wish to offer protection to threatened nations, we could see to it that punishment for any such irresponsible act would be swift and condign.

An approach of this sort, which relies on superpower preponderance to withstand the potentially baleful effects of proliferation, is not one that is universally and automatically appealing. Hedley Bull has characterized this approach as "high posture" and has contrasted it with one that he prefers: the "low posture" in which the differences between the greater and lesser powers are muted. Let me therefore say a few words in defense of the so-called high posture.

First, phrases like high posture and low posture have a certain allure, but the question must be raised whether they accurately describe the underlying realities or the true alternatives. The gap in military nuclear power between the superpowers and other nations is enormous and will continue to be so. In fact, it is more likely

[4] Antiballistic missile—*Ed.*

to increase than diminish.[5] If we accept that the strategic gap will continue to be enormous, what seems desirable is that the character and width of the gap be sufficient to permit the superpowers to exert a stabilizing influence on the restless third areas of the world. Moreover, this stabilizing function needs to be perceived by those that may come to possess a minor nuclear capability. This potential stabilizing function should not lightly be discarded in the quest for a somewhat mythical low posture.

Second, the spread of nuclear capabilities into third areas will very much intensify the existing elements of instability and the danger in instability beyond what it is today. The nuclear capabilities will be unsophisticated and vulnerable. Given the existence of vulnerabilities and the temptation to exploit a temporary strategic edge, the likelihood of nuclear initiation through a hair-trigger response seems obvious. Most persons who seek a more peaceful world would find beneficial the ability of the superpowers to forestall the initial use of such capabilities. In seeking arms control arrangements we must keep in mind the bilateral U.S.-Soviet relationship, but we should also remember that increases in our capabilities, when matched by the Soviet Union, may serve to diminish the risks of dangerous outbreaks in third areas of the world.

Third, most nations, even when they strongly disapprove of specific aspects of U.S. policy, desire that the United States stand ready to counter nuclear threats against nations lacking in the means of self-protection. The United States, in particular, is being called upon to perform functions that other nations are not called upon to perform. If the United States is expected to play the role of a nuclear Galahad, risking nuclear retaliation and loss of population in behalf of others, it does not seem unreasonable for the United States to possess protective measures of a type not universally available. Nor does it seem wholly consistent for those who rely on U.S. protection simultaneously to urge the United States to accept a low posture *and* to stand ready to intervene in their defense. If a nation is expected to accept losses in behalf of others, it seems reasonable that the potential losses be held to a minimum. That those on whom the role of nuclear Galahad is thrust should desire thicker armor seems understandable.

CONCLUSION

The strategic importance of proliferation has tended to be exaggerated because the problem has been viewed qualitatively in terms of enumerating those nations that might acquire a small capability, rather than quantitatively in terms of the destructive potential of the capability that might be achieved. As far as we can see into the future, the strategic environment will continue to be dominated by the preponderant military power of the United States and the Soviet Union. It is possible that the spread of weapons will result in greater inhibition on the use of power

[5] Pious comments regarding diminution of the strategic-military gap separating the superpowers from other nations, nuclear and nonnuclear alike, is reminiscent of some high-flown discussions regarding the "income gap" which were particularly popular in the '50s. It was frequently stated at that time that it was essential to diminish the gap between the affluence of the developed nations and the poverty of the underdeveloped nations. In the intervening period per capita GNP in the United States has risen by more than $1,000, while there remains some question whether per capita income in the underdeveloped countries has risen at all. Diminution of the income gap was simply not a feasible objective. Similarly, in the strategic area we are not going to have any diminution of the gap between the superpowers and the rest for the foreseeable future. It is never sensible to base one's policies on hopes for the unobtainable. Therefore let us avoid repetition of this particular class of past errors. Whatever else our policy is based on, it should not be on an expected diminution of the strategic gap.

by the United States or the Soviet Union in regions of less than vital concern. The degree of inhibition depends upon the risks that we (or the Soviets) are willing to run. However, if we desire to accept the risks, we could, because of our preponderant power, continue to intervene in unsettled areas to diminish the risk of small-scale nuclear war.

With the spread of weapons there would be a greater likelihood of use or misuse, but the risk of use or misuse will be concentrated primarily in the third areas of the world. Given the current and prospective stable military balance between the United States and the Soviet Union, it is difficult to envisage conflicts in third areas escalating into exchanges between the ZIs[6] of the two major powers. This implies, of course, that proliferation would impose enlarged risks primarily on other nations. The superpowers will continue to be relatively immune; the threat to them will continue to come primarily from each other. In all analyses of proliferation this asymmetrical distribution of the risks should be stressed because of its possible impact on the incentives of aspiring nuclear powers.

A substantial diminution of the strategic gap between the superpowers and others is simply not in the offing. The only way in which reduction of the gap could be influential is if it undermines the credibility of intervention by a superpower to stabilize conditions in third areas being subjected to nuclear threat. This is not necessarily beneficial, and it is doubtful whether those in threatened areas would desire such an outcome, if they think seriously about the problem. What may be desirable is to make crystal clear that despite nuclear spread the major powers will retain the ability to intervene to deter nuclear threats or to punish nuclear irresponsibility without risking substantial damage to themselves. This does not necessarily mean that the major powers will be forced to deploy all those systems, like ABM, which hold some promise in this regard; it does mean that they shall be forced to work diligently so as continually to upgrade their ability to detect, deter, disarm, or punish the national source of nuclear irresponsibility.

While nuclear spread is basically destabilizing, its strategic consequences need not be too severe. Simple nuclear capabilities cannot play the role of "equalizers" in international conflict. The strategic position that the United States and the Soviet Union currently enjoy is so unassailable that even continuing action by third parties is unlikely to upset the central strategic balance for the next twenty years. Properly exploited, this central strategic balance could continue to provide some stability in regional conflicts—even in the face of nuclear spread.

[6] ZI = Zone of the Interior—*Ed.*

NUCLEAR SPREAD: THE SETTING OF THE PROBLEM

Rand Paper P-3557
March 1967

Prepared as an address to the regional American Assembly meeting at the University of Notre Dame, March 16-18, 1967, on the topic "A World of Nuclear Powers?," this paper was published in the Autumn 1967 *Yale Review* (Vol. 57, No. 1, pp. 66-84). A German translation by Wilhelm Cornides was published in Folge 14/1967 of *Europa Archiv*, under the title "Die Verbreitung von Kernwaffen: Möglichkeiten and Grenzen der Eindämmung der Proliferation" (pp. 479-492).

My purpose this evening is to discuss the likelihood and the dangers of further nuclear spread. What is said will, I trust, provide a sober assessment of the risks to the United States and the World Community—one which avoids some of the exaggerations which have crept into the public discussion of the problem. Such an appraisal may assist you in your deliberations, for it is my conviction that unduly alarmist statements hamper our ability to recognize the very elements of the problem which can make the largest contribution to keeping the risks manageable. My principal theme is that it is far harder than most people recognize to put together a serious strategic capability which could make a useful contribution to satisfying the security aspirations of most potential nuclear powers. Partly for security reasons, this consideration has been given insufficient prominence in public statements concerning the problem. The result has been a widespread belief that the nuclear "haves," and particularly the superpowers, are attempting to deny to nuclear aspirants a prize, which is not only highly valuable but is readily obtainable.

Public discussion has been characterized by a curious ambivalence, which stresses, not the dubious usefulness of feasible nuclear forces, but the potential devastation and the terror inherent in *any* nuclear capability, no matter how small or primitive it may be. Conveyed in such an attitude is the promise—the erroneous promise—that this terror-potential might be effectively employed to deter aggressive actions by militarily superior countries. To many nations this prospect offers an invitation rather than a threat. To cope with the proliferation threat we must disseminate a more sophisticated understanding of what specific nuclear capabilities can and cannot do. And to accomplish this, we must first deal with our own ambivalence regarding the military potential of even a few nuclear weapons.

The Hiroshima explosion in all its drama touched off not only a set of alarms, but also a set of predictions regarding the pace and the consequences of nuclear spread. At one pole was the optimistic government prognosis that it would be years before other nations could achieve the scientific breakthrough that would shatter the American monopoly. Concurrently there were a number of prophecies, many emanating from the scientific community,[1] suggesting that the pace of proliferation would be rapid and ultimately disastrous. It is important to recall that *both* sets of predictions have proved wrong. The technical problem, at least for primitive weap-

[1] The forecasts of a number of illustrious scientists have been as erratic as they have been dramatic. With the failure of the Baruch Plan in 1946, many scientists abruptly reversed their position—and the date both for nuclear spread and for doomsday began to recede.

ons, has proved to be less of a barrier than was hoped. On the other hand, the spread of weapons has been far slower than the scientific Cassandras anticipated. Great Britain, at an early date, and France and China, more recently, did acquire modest capabilities. Yet, not only has the actual pace been slower, but there are impressive military trends which, at least in logic, increasingly work against further acquisition. These forces I shall analyze presently. For the moment let me mention two reasons why the prophecies have gone awry. First, most nations have recognized that a minimum deterrent—that is, a suicidal threat against the cities of a potential foe—may not be much of an asset. Second, to acquire even a minimum deterrent requires efforts far more extensive than was perceived in the late '40s. But such underlying causes for past forecasting failures have rarely been explored by those observers who assert the inevitability of nuclear spread.

Despite the miserable record of this particular prophetic tradition, the standard alarms continue to be sounded—and the standard exaggerations reiterated. Perhaps the most familiar of these is the one expressed by C. P. Snow in 1960:

> We know with the certainty of statistical truth that if enough of these weapons are made—by enough different states—some of them are going to blow up. Through accident, madness, or folly—but the motives do not matter. What does matter is the nature of the statistical fact. . . . (This) is not a risk but a certainty. . . . The arms race between the US and the USSR not only continues but accelerates. Other countries join in. Within at the most six years China and several other states will have a stock of nuclear bombs. Within at most ten years some of these bombs are going off.[2]

This pronouncement attracted considerable attention when it was first issued. How has it fared—when compared with the actual events? Few nations, if any, show a serious inclination to join the arms race between the United States and the Soviet Union—to the extent that that is continuing. More important, six years have already passed. While China has had five tests, it is unknown whether and to what extent it possesses a stockpile. The "several other states"—which were supposed to acquire a stockpile—have not as yet made their appearance. To be sure, we still have three years to go before we can say whether or not the final prediction—that the weapons will be used either by accident or design—is wrong. Some of us are more diffident regarding precise statements of future events. However, analysis does suggest that, if weapons are consciously employed in clashes involving the superpowers, only the superpowers would have reason to initiate their use.

The purpose in reviewing past predictions in relation to actual developments is not so much to indulge in second-guessing as to shed light on current forecasts. Yet, whatever the extent of past and present exaggerations, it is clearly in the interest of both the United States and of mankind generally to limit the spread of nuclear weapons. The problem with the exaggerations is not that they point in the wrong direction, it is that they lead to a misunderstanding of the problem. We may not face the apocalypse, but proliferation will bring serious risks in its train. From the American standpoint, nuclear spread could lead to the disruption of nuclear strategies, to the political unsettlement of Europe, to the diversion of resources with a corresponding decrease in military security in Europe and along the Chinese periphery, to instability in third areas of the world which we would prefer to be serene, and, finally, to an added risk, however minimal, of a small-scale attack on the United

[2] Sir Charles P. Snow, "The Moral Unneutrality of Science," an address to the American Association for the Advancement of Science, New York City, Dec. 27, 1960, published in *Science,* Vol. 133, Jan. 27, 1961, pp. 256-259.

States. On balance, it is with good reason that American policy seeks to avoid further nuclear spread.

However, the benefits to us are relatively small as compared with the benefits to others. American military power makes us almost immune to foreseeable threats from countries other than the Soviet Union. The principal sufferers from nuclear spread would be the countries and the people in those areas in which the infection takes place. In order to deal intelligently with the problem, we shall make it perfectly clear that the principal beneficiaries of preventing nuclear spread are not the superpowers, but nations in the third areas. There has been much dramatic, but inaccurate, talk regarding the "equality of terror" which would similarly menace all nations. Let me emphasize that in the wake of proliferation, terror will be most unequally distributed. A disproportionately large share would be the lot of the weaker nuclear states and of their neighbors.

INCENTIVES AND CAPABILITIES

Let us examine the incentives and the capabilities for acquiring a nuclear force and—even more important—the relationship between the two. Under the heading of incentives it is crucial to distinguish between political prestige and military effectiveness. Unfortunately, considerable prestige can be extracted from possession of a few nuclear weapons, but, happily, military effectiveness is a different matter. In large measure the drive for acquisition turns out to be a quest for prestige masquerading as a quest for military security. The French desire to acquire weapons, for example, was strongly stimulated by the Suez psychology: the feeling that when a nation pursued its own objectives, it would lack the support of its major nuclear ally. The conclusion drawn was that a nation should pursue an independent nuclear capability with which to buttress its foreign policy stance. What was forgotten in this interpretation was that France's associate in the Suez venture, a nation perhaps more directly humiliated than was France, already possessed a nuclear capability. In fact, Britain's strength relative to the Soviet Union was then far greater than it is today—far greater than can be hoped for in the foreseeable future by any power of the middle rank. Yet, even then this nuclear force did not prevent her humiliation.

More generally, it is difficult to detect any close association between nuclear force and political success. Britain's power has steadily eroded despite the possession of a nuclear capability—perhaps, in part, because of the heavy cost associated with its acquisition, which diverted resources from other, more effective, employments. Save to the extent that France can employ her veto power within the Common Market or its disruptive power within NATO, her foreign policy has scarcely been crowned with success—in Africa, Latin America, or in Asia. Whatever the accomplishments have been, the nuclear force itself has been more of a "cause" than a source of strength. The period of Chinese nuclear testing since 1964 has coincided with a remarkable set of disasters. Even the stature of the Soviet Union is perhaps lower in the '60s, after acquiring a substantial intercontinental nuclear strike capability, than it was in the '50s.

One can, I think, conclude that the prestige associated with membership in the nuclear club is rather "atmospheric." This does not imply, of course, that it is unimportant. Prestige is inherently rather atmospheric, and yet it remains a potent incentive. Nonetheless, it seems desirable continually to call attention to the evidence—and to deflate, rather than inflate, the prestige associated with nuclear

weaponry. Few nations, fortunately, are likely either to run the risks or to make the investments in nuclear forces in pursuit of prestige alone. Most nations will also have to be persuaded, at least superficially, that nuclear forces can serve other, more concrete, security objectives.

We turn therefore to a scrutiny of military objectives that might serve as incentives. As a rough approximation, one can distinguish at least three classes of none-too-sharply definable objectives. As indicated in the display, these are (1) a deterrent against the Soviet Union, (2) a capability to deal with China, and (3) regional capabilities. For a number of European industrial nations the only immediate motive for acquisition is to deter the Soviet Union.[3] Whatever aspirations exist in West Germany and Sweden have centered on this objective; Italy and Switzerland have occasionally been mentioned in such speculations. To acquire a capability against the Soviet Union is a most demanding task. France's nuclear force is clearly inadequate—Britain's admittedly so. To acquire a serious and substantial second-strike force—for destruction of Soviet targets—would require expenditures of $4-5 billion per year and efforts extending over a ten- to fifteen-year period. Given the motivation and the immensity of the task, lesser capabilities are of doubtful interest. Efforts along these lines are of special concern to the United States because of the disruptive impact on European politics and because of the harmful consequences for the most effective nuclear strategy for NATO, i.e., a city-avoiding, countermilitary strike against Soviet strategic forces. But such considerations should provide powerful motivation for the European states to avoid further nuclear spread—other than for prestige purposes.

The second class of motivation, which has developed since 1964, is acquisition of an anti-China capability. The leading candidates are Japan and India—with Australia lurking more distantly in the background. Quite obviously, any Chinese capability, as compared with the Soviet Union's, will be sufficiently limited that a very impressive countercapability lies within the grasp of at least so powerful an industrial state as Japan. Moreover, one might argue that, taking this specific goal by itself, it is the one with which the United States could live most easily. But there are additional problems. First, while Japan might readily deter China, its aspirations could easily edge over into the first category—the difficult-to-attain anti-Soviet deterrent. If Japan depends upon the United States to deter Russia, acquisition of a lesser capability to deter a much more limited threat (which the United States can handle with comparative ease) would imply at a minimum the considerable wastage of resources. Second, in the Indian case, a primary problem is that Indian acquisition edges over into a third category, a regional capability that disrupts the local balance of power. Moreover, the limits on India's resources and the tightness of her budget indicate that a diversion of effort toward nuclear weaponry would in all probability result in an emasculation of the conventional military establishement, leaving India conventionally exposed not only to China, but possibly to Pakistan as well. If such an effort fails to provide an adequate deterrent against China, the result could be disastrous from the Indian standpoint.

The third class of motivation, encompassing a more heterogeneous collection of states, is associated with acquisition of a low-level, purely regional capability. So limited a capability, with primitive weapons and primitive delivery, could be thrown together with annual budgetary expenditures running in the range of $200 million, and could pose an impressive local threat—especially if the great powers choose to stand aloof. In this connection the Israeli-U.A.R. and the Indian-Pakistani confron-

[3] It is feared that further nuclear spread within Europe—particularly if West Germany acquired a capability—might touch off the regional motives for acquisition among neighboring states.

tations are the immediate sources of concern, though both Indonesia and South Africa have stirred interest as more distant possibilities. The risk implicit in such developments is the further undermining of a stable world order, to which the United States has dedicated itself. Continuing U.S. efforts to impose stability on unsettled areas would entail additional risks in the wake of acquisition of regional capabilities. But these risks would not be primarily to the United States proper or to its population. The principal risks would be borne by the population in those areas in which regional capabilities spread. Since the United States itself can handily survive such developments, we should recognize that simple altruism—so easily discounted in a cynical age—provides a not inconsequential component of U.S. nonproliferation policy.

In looking at the display in Table 1, one may perceive that the hope of avoiding further nuclear spread is buttressed by the fact that (aside from Japan) the nations with the best resource position also have the highest aspiration-thresholds for acquisition. In particular, the industrial nations of Western Europe both face stringent requirements and possess the bland alternative of relying on U.S. protection against the Soviet Union. Moreover, it is comforting that certain nations that may have aspired to regional capabilities and may have been sorely tempted to seek a local strategic advantage are the ones with the least adequate resources. This mismatch between levels of income and industrial competency and levels of nuclear aspiration is perhaps the most encouraging element in the attempt to prevent further nuclear spread.

Table 1

CLASSIFICATION OF NUCLEAR FORCE OBJECTIVES

Anti-Soviet	*Anti-China*	*Purely Regional*
West Germany	Japan	Israel
Sweden	India	U.A.R.
.....	.....	.....
Italy	Australia	Pakistan
Switzerland		Indonesia
		South Africa

There is, however, a considerable risk of feedback from the third category to the first category. If such weak states as the U.A.R. acquire weapons, it would make it more difficult for established powers like Italy or West Germany to stand aloof. They might be driven to acquisition for prestige reasons alone. The risk of contagion in nuclear spread is substantial. While the decision to go nuclear could remain dependent largely on the individual nation's perception of its own needs, there is unhappily some risk that the nuclear-prone states could become a group that moves more or less simultaneously toward acquisition.

Mutual stimulation by way of resource capabilities is inherently much less important than by way of incentives. When we examine the technical and economic resources of potential nuclear powers, we are struck by the contrasts among them. In Table 2, I have divided some of the nations in question into a number of resource categories. The display is more or less self-explanatory. West Germany and Japan are undeniably industrial powers of the first rank. The second category includes nations with a substantial GNP and at least a moderate-sized industrial sector. The third category includes nations with more limited total resources, but possessing

some modern technologies. The fourth category includes nations mentioned in connection with nuclear spread, but whose very weak technological bases make it improbable that weapons could be acquired without considerable outside assistance.

Table 2

ECONOMIC AND TECHNICAL LEVELS OF POTENTIAL NUCLEAR POWERS

Category A	*Category B*	*Category C*	*Category D*
West Germany	Sweden	Israel	U.A.R.
Japan	Australia	South Africa	Pakistan
	Italy	Taiwan (?)	Indonesia
	India		

Once again one notes the tendency for the more satisfied and less tempted nations to have the greater resource potential. There are recognizable points of potential combustion in each category. However, most of the nations about which serious concern has been expressed fall in the lower categories. It is difficult to see how even the most sorely tempted among them could pose a direct threat to the United States. The demands for R&D, weapons tests, secure and long-range delivery systems, and resource allocations generally are just too stringent. It is for this reason that the United States, if it wishes, will for a long time remain free to intervene in third areas of the world in such a way as to keep the probability of use of whatever nuclear forces are created to very low levels.

The spectrum of technical and economic capabilities casts light on another problem: the potential spillover of nuclear technology from peaceful programs into weapons programs. Considerable thought has been given to controlling the international flow of advanced nuclear technologies. On this question it is necessary to keep one's aspirations feasible. It would be both futile and counterproductive to attempt at this time to preclude development of a nuclear industry in a modern industrial state like, say, Japan. One may hope, through assistance, to lessen the risk that civilian programs will spill over into weapons programs. We should not persuade ourselves that the denial of assistance to Japan on, say, a plutonium separation plant will prevent her from acquiring one independently. For advanced industrial countries, denial of assistance, when they are able to proceed on their own, will not divert civilian programs. It may, however, cause sufficient resentment that these nations will be disinclined to accept U.S. suggestions regarding weapons problems.

By contrast, it is not infeasible to preclude access to nuclear technologies by the less-developed nations. This is an important distinction. The advanced industrial states cannot be denied pieces or components of capabilities—even though they will not be able to develop serious and substantial nuclear forces. On the other hand, for the less advanced states it may be possible to deny access even to simple types of nuclear facilities. This is significant because for such states simple capabilities may be all that they can obtain, may be all they desire, and yet would certainly provide an unsettling regional menace. A policy of denying to the less advanced states nuclear equipment and facilities that could be used in weapons programs may be feasible. Observe, however, that to achieve such denial we increasingly become dependent on cooperation from other industrial states—including such nations as France and perhaps even China. The point is that we shall have to define our objectives carefully. If by any chance we should desire the cooperation of other advanced countries in keeping dangerous capabilities out of the third areas of the world, we cannot afford to excite their ire through a policy of denying them the more advanced technologies.

PIECES OR COMPONENTS VERSUS COMPLETE SYSTEMS

There are a number of reasons to go more deeply into the issues raised by the spread of so-called peaceful nuclear technology. First, the recent achievements in nuclear power and the awareness that many nations will soon acquire major plutonium production facilities provide one of the main reasons for the recent flurry of concern over proliferation. Second, nuclear power facilities, while unquestionably of considerable specific importance, tie into certain broader misconceptions regarding acquisition of a serious nuclear force.

There is, of course, no question that plutonium will become increasingly more available than we might wish for reasons of international stability. By the year 2000, for example, it is estimated that several million kilograms will have accumulated in the plutonium inventory simply from power production. When the well-known fact that a nuclear weapon requires 5 to 10 kilograms of plutonium is recalled, it is easy to understand the basis for concern. Moreover, our anxieties reflect not only the growing substantive problem but also a feeling of guilt that we may have mistakenly betrayed our original intention to postpone the dissemination of specialized information and facilities until *after* effective international controls had been developed.[4] Unquestionably major efforts should be invested in devising safeguard systems to reduce the risk that some of this material will be diverted to weapons. But beyond our concern and our effort to establish safeguards we must learn to view the problem of plutonium production, by itself, in perspective.

In the future we shall be forced to diminish emphasis on preclusion of access to fissionable material as the principal barrier against nuclear spread—and to search for other deterrents. In the years since 1945 we have become habituated to regarding control over fissionable material as the indispensable barrier to the acquisition of nuclear capabilities. Such an attitude did little harm as long as fissionable materials might be regarded as both a meaningful indicator of interest in a nuclear capability and an effective point of control. However, it has led to an undue concentration upon and an exaggeration of the material ingredients in nuclear weapons—to the exclusion of complementary requirements which may be more important in the total decisionmaking context.

This reflects a more general problem: the tendency to draw conclusions about nuclear spread from studies of individual components of a nuclear force. In our research program at Rand we have made a substantial effort on what I shall call component studies—fissionable materials, weapons design, and delivery. Up to a point—and in some cases a fairly primitive design point—these component problems have proved solvable for many of the potential nuclear powers. There might be a tendency to draw unduly pessimistic conclusions from such results. But the crux of the matter is that we are dealing with bits and pieces of capabilities. When we come to overall systems design and to systems evaluation the inferences are more reassuring. The task of fitting together the available components into what is, by any stretch of the imagination, an operational force, is a demanding one. Moreover, when we evaluate the systems that can be put together at feasible budget levels in relation to the strategic objectives of individual countries, the result is normally a terrible

[4] In their joint declaration of November 15, 1945, President Truman, Prime Minister Attlee, and Prime Minister Mackenzie King stated: "We are not convinced that the spreading of the specialized information regarding the practical application of atomic energy, before it is possible to design effective, reciprocal, and enforceable safeguards acceptable to all nations, would contribute to a constructive solution of the problem of the atomic bomb. On the contrary, we think it might have the opposite effect."

In the same declaration it was asserted: "No system of safeguards that can be devised would of itself provide effective guarantee against production of atomic weapons by a nation bent upon aggression."

mismatch. In the final evaluation, system evaluations will take precedence over component studies, as they should. Yet, it would be desirable to have wider public recognition that the growing availability of components alone ought not lead to visions of disaster.

Fissionable materials alone do not constitute a nuclear capability. This basic proposition needs to be more widely understood. There are countless problems in weapons design and testing, reliability evaluation and safing, component development and weapon assembly—to say nothing of the delivery problem. These problems will not be swept away through the growing availability of plutonium. On the contrary, in some respects they may be considerably intensified.

Let me cite one important example. Power reactors under normal operations produce plutonium highly "contaminated" in the 240 isotope—up to 30 percent Pu^{240}. In the early days—at the time of the Baruch Plan, for example—contaminated plutonium of the type produced in power reactors was referred to as "denatured." There was high confidence that it would be useless in military programs. For some curious reason, at the very moment when reactors are becoming more widely available, the public view has shifted sharply. Not only is it believed that power-grade plutonium creates no insurmountable obstacles to military employment, but that it is virtually indistinguishable in utility from weapons-grade plutonium. This view is not only narrow, it is basically incorrect. Yet, we may read in as reputable a source as the London *Economist* that "any nuclear power station will turn out enough plutonium for a few bombs; and, as the French have shown, quite impure and low-grade plutonium will explode." [5]

Let me categorically assert that under these circumstances the weapons design and testing problems are awesome, especially for a nation with a limited technological base. At Rand we have studied closely the diversion possibilities from peaceful programs. These studies have confirmed the value of effective safeguards. Nonetheless, we should all recognize that diversion of plutonium from a peaceful program is not synonymous with the acquisition of weapons. It is but a short step on a long road.

To be sure, every available ingredient or component is a small step ahead, and may make it easier for nations to acquire a capability. Yet, the tendency to concentrate on components and to ignore overall systems requirements has been the cause of excessive apprehension. It has resulted in recent years in the habit of drastically understating the technical difficulties and the costs of acquiring a nuclear capability, associated with an habitual exaggeration of the willingness of other countries to invest resources in such a capability. In this respect technical experts have been particularly liable to error. No one should underestimate the invaluable assistance provided by scientists on technical questions. Nonetheless, scientists have tended to assume away what I would regard as the heart of the proliferation problem.

Put quite briefly, many scientists seem intuitively to believe that scientific matters which are now obvious to themselves and their American colleagues (after enormous outlays on R&D and training) will quickly (and almost costlessly) become apparent to their counterparts in technically more backward areas. A scientist, after all, is a scientist, and so-called scientific "secrets," once they have been discovered, are not particularly recondite.

Within the U.S. atomic community, scientists have operated in one of the most unconstrained resource environments in the history of mankind. Rarely having encountered any substantial resource stringency, U.S. scientists have little understanding of what it means or how it can cripple technical advance. Consequently

[5] *The Economist*, 10-16 December 1966, p. 1112.

they have little feeling for the fundamental difficulties that their counterparts in less favored nations will face. Furthermore, there is a strong penchant in the scientific community to discount those mundane (but terribly costly) problems falling outside the realm of scientific discovery. Once a problem has been solved *on paper,* the engineering problems are regarded as secondary. There is little perception of how primitive engineering may be in some countries with established scientific communities.

Finally and perhaps most important, assuming that the scientific and engineering problems can be solved, there is almost zero appreciation of the severe difficulties that must be surmounted in the areas of industrial organization and bureaucratic organization. These problems are frequently regarded as inherently negligible.[6] But in reality the problems of organizing production are likely to be the most intractable of all. At the lower end of the spectrum nations that have difficulty organizing factories that produce light bulbs or books of matches with adequate treatment of quality control are not likely to find the production of advanced weapons an easy matter. For more advanced industrial countries of the middle rank, the problems of inadequate resources, minuscule R&D programs, and short production runs, taken together, imply small, unsophisticated, and costly capabilities—inadequate for their presumed strategic objectives.

In the delivery area these problems may be even more intractable than in the weapons area. The costs of a serious, reliable, alert, and secure missile system, with good command and control, are staggering—especially if such ancillary requirements as reconnaissance and early warning are added in. For a nation like India, with so limited a base to draw upon, the financial cost alone may be infeasibly high. The costs of research and development in advanced areas, of acquiring the industrial facilities, of training, of deployment, of operations and maintenance (which are high for an alert and reliable force) are so awesome that I personally think it would be rash for India to make the attempt. While it is true that any particular element of the force is probably within India's competency, it is the overall system that I suspect would be just too much.

My own estimate is that developing a limited capability of satisfactory quality would require annual expenditures in excess of the existing Indian defense budget. This estimate stands in sharp contrast to another which is applicable to the Indian case and which was developed in the course of these American Assembly deliberations. Leonard Beaton has given an estimate of $300 million per year for a modest missile force with thermonuclear warheads ($250 million per year if only plutonium is used).[7] Even if we disregard the ancillary costs of reconnaissance or early warning, my estimate for the acquisition and operation of the basic hardware is higher by at least a factor of five.

One of the potential tragedies in the proliferation area is the possibility of nations being lured into military nuclear efforts through a drastic underestimation of the true costs. Contributing to this potentially tragic result is the fact that there has never been a good compilation of the cost of a missile program from the data available in the public realm. I for one think that the Indians not only will but should make their decision in line with what they consider to be their national interest. It would be unfortunate if the Indians in assessing their interests were to err on the basis of a come-on price. Viewing the matter as objectively as I can, when

[6] The fact that they are continually encountered in the real world is likely to be attributed to the incompetency or sheer obstinacy of the nonscientific types typically charged with the responsibility in these areas. Ultimately the problems of organization—in the scientific, as in the Marxist view—will "wither away."

[7] The American Assembly, *A World of Nuclear Powers?,* Prentice-Hall, 1966, pp. 32-36.

I consider all the things that India would have to do to acquire a serious capability, I believe that the Indians would be making a very bad bargain, if they made the attempt. Moreover, what applies to India, applies in a more general way to other nuclear aspirants. The case for Indian acquisition is perhaps somewhat less unsatisfactory than is the case for acquisition by others.

SOME CONCLUDING OBSERVATIONS

On balance, what I have said can be construed as cautiously optimistic regarding the chances of limiting further spread. This optimism depends upon developing a greater public and international awareness of the enormous gulf between individual components for primitive forces and serious and substantial strategic systems. If it were appreciated how little a nation can extract from a primitive capability, the appetite for nuclear forces would diminish. In addition, while admittedly little good can be expected from acquisition, happily such acquisition need not lead to disaster. The consequences of further proliferation remain controllable. Thus, when the capabilities available to nuclear aspirants are carefully scrutinized, a question arises whether the actual dimension of the threat merits the note of desperation that has occasionally crept into public discussion.

Permit me to be speculative for a moment. Taking a long-term view, the air of foreboding regarding nuclear spread which is expressed in Washington and elsewhere should be taken as an indication that the far more serious threat of central war with the Soviet Union has been reduced to manageable proportions. Governments are inclined to view current problems as posing dire threats to their publics. They also wish to give the appearance of manfully grappling with these problems. With the waning of the central war threat, only the spread of nuclear capabilities seems to carry with it the risk of upsetting the measurable degree of military stability that has been attained. Yet, we should retain a sense of proportion regarding proliferation. As compared with central war, the risks involved—particularly for the United States itself—remain of a much lower order. Perhaps like Bunyan's Pilgrim, we should be aware of the dangers and obstacles that lie in our path, but we should also recognize when we have emerged from the Valley of the Shadow of Death. Speaking pragmatically, moreover, it is only by retaining cool heads that we shall hold the risks of the spread to the minimum level.

One Washington observer, viewing with some skepticism the tendency to view with alarm, has commented that coping with the proliferation problem required the services of only two men—one to count and the other to wring his hands. The reference to hand-wringing aptly characterizes the prevailing atmosphere of concern, while the reference to counting underscores a widespread conviction of inevitability. Neither the feelings of desperation nor the conviction of inevitability strike me as appropriate. We must keep in mind that the threat of nuclear spread has been a cloud looming on the horizon for twenty years. Yet it has gone on looming—in the face of repeated predictions that the multiplication of nuclear powers was but a few short years away. Such evidence—the disinclination of most nations to opt for nuclear capabilities as soon as it was feasible—should be recalled in assessing the inevitability of the spread, for it points to barriers to acquisition, the efficacy of which has not disappeared and which could be better advertised. When examined carefully, these barriers reveal certain difficulties in the military and political exploitation of minor nuclear capabilities, which, if it does not remove concern, suggests that the spread of capabilities, even if it does materialize, will be a more manageable problem than the more dire prophecies would have it.

A sharp increase in the number of nuclear powers may come, but it may not. There is nothing inevitable about such a development—save in the sense that the existence of the nuclear option in conjunction with the continuance of unrestricted national sovereignty implies a long-run possibility of the slow expansion of the number of nuclear-armed nations. Yet within this configuration there remains ample scope for international agreement. Despite our continuing differences with the Soviet Union, limited understandings are feasible, which would help to limit the proliferation threat.

ARMS INTERACTIONS AND ARMS CONTROL

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This is an unclassified version of a paper originally presented on December 13, 1967, at the Military Operations Research Symposium meeting in Gaithersburg, Maryland.

The set of issues suggested by the phrase "interactions" is generally recognized to be fundamental to arms control; yet these issues are very troublesome and intractable to deal with. A good model of interactions is indispensable to intelligent arms policy, but a naive model could both lessen security and intensify the arms competition in unprofitable directions. In my opinion, certain features of the interactions problem are not widely understood, and consequently I shall be attempting to redress the balance. I shall deal with (1) some general aspects of the interactions problem, (2) the ABM problem, and (3) some related aspects of the proliferation problem.

CONCEPTUAL PROBLEMS

The rationalistic approach to arms control, which dominates the public discussion, tends to rest upon speculation and syllogisms. It presupposes that arms decisions made by a rival are formulated by a national decisionmaking process, governed by something like a unified intelligence. There is, for example, a good deal of casual (one might say half-baked) speculation on what the Soviets might do; the resulting hypothesis is gradually transmuted into the certainty that this indeed is what the Soviets will do—with little attempt made to temper the speculation by considerations of bureaucratic or economic feasibility. The result is to impute tremendous flexibility to the Soviet posture. Little attention is paid to resource availability or to the deadening and stultifying effects of the bureaucratic process. Diabolically clever measures and countermeasures follow one another, and we are off into an ingenious—and, incidentally, a costless and frictionless—arms spiral.

By contrast, consider the real-world limitations on such a process. First, extraordinarily long lead times are encountered before a new weapon system is deployed and operational. Typically in public discussion these lead times tend to be ignored or truncated. On the other hand, in good research studies lead-time considerations are prominently displayed, though even here it is my impression that the times involved are substantially understated. Nonetheless, one effect of sensibly introducing lead times is to stretch out the arms race. A second is that the weapons systems being designed today will be introduced a decade hence into what may be an altogether different strategic environment. Thereby hangs a tale to which we shall shortly return.

But there are other constraints, perhaps of greater importance, on a hypothetical arms race in which scientific marvels succeed scientific marvels. I almost entitled this paper "Budgets, Bureaucracies, and Blindness: The Three Hidden Weapons in Arms Control." It is these key ingredients that explain the slowness of arms responses, the lost opportunities, and the perseverance of vulnerabilities. Budgets

after all are not unlimited, and major deployments, which in speculative design could occur simultaneously, compete for the same resources. For example, with limited budgets, it may be infeasible to project the expansion and upgrading of offensive forces at the same time that a major ABM deployment takes place. Moreover, for good bureaucratic reasons budgets, when limited, tend to be inflexible. Each bureaucratic element fights to preserve its fair share of the budget. New programs encounter resistance to obtaining ample funding. This bears on the issue of blindness, a term I use without pejorative intent since it is inherent in the human condition. Accepted doctrine tends sharply to limit permissible action and moderately limits permissible views. At any one time there is a somewhat stereotyped view of the threat, of proper strategy, and of acceptable armaments. Proposals falling outside of this circle, no matter how ingenious, encounter seemingly endless resistance before obtaining acceptance.

Two bits of evidence may be mentioned to support this contention. The work of Loftus and Marshall on Soviet military allocations underscored the ability of established Soviet military units to obtain the lion's share of appropriations. The astonishing result was that in a period that American researchers were discussing, and even forecasting, a surprise attack, the Soviets were doing remarkably little to build up their intercontinental strike forces. These forces were neglected, not only in terms of the American surprise attack fixation, but in terms of a minimal deterrent against the overwhelming U.S. strategic capabilities.

The second piece of evidence contrasts the high degree of flexibility in military postures obtained in playing the SAFE game, which contrasted sharply with the much lower degree of flexibility experienced in the real world. Nations demonstrate a persistent reluctance to phase out deployed equipment, which game players or researchers would tend to regard as obsolescent. As a result the roll-over cycle in military posture tends to be slower than a number of analysts have suggested. Incidentally, the willingness to incur substantial operations and maintenance charges on older equipment is not simply irrational, it reflects an understandable desire to hedge against the failure of new and untested equipment by standing with the tried and true.

In contrast to this *observed* sluggishness in changing military postures is a picture of rapid-pulsed technological revolutions presented by some weapons scientists and arms controllers. Although diametrically opposed morals are drawn, these two groups present surprisingly similar images. Technology represents the primary constraint—organization, production, bureaucratic, and resource barriers pale into insignificance. When the technological barrier is breached, the transformation to a new posture is achieved through comparative statics. The lags are brief, and any resource requirements are assumed, like manna, to be supplied by a Kindly Providence. That a variety of alarming conclusions are drawn is not surprising (nor should it be regarded as simply accidental). One view suggests that the world will quickly be destroyed through an unfettered arms race unless political constraints are imposed that rein in technology. The opposite view suggests that an opponent will come into a position in which he can destroy U.S. retaliatory capacity and major cities in one surprise attack—unless U.S. technological advance is further stimulated. Nonetheless, there is an underlying similarity in the acceptance of the ease and speed of technological change.

My own conviction is that the use of rational models, presupposing quick perception, development, and absorption of new technologies and a high degree of interaction based on astute moves and countermoves, leads to a misunderstanding of the arms control problem. Nations, as they determine arms policies, should be viewed organizationally—as sluggish organisms, dominated by doctrines based specifically

on obsolescent strategic views, and comprised of contending bureaucracies that create major obstacles to the instituting of serious change. Major changes come as a result of political shocks. Smaller changes, even though they be cumulatively important, are ignored for long periods of time. National powers of perception are quite limited—for large numbers of people must be persuaded that their previous judgments are outmoded. This is a time-consuming process at best.

Thus, I would argue that, rather than arms interactions being a game of subtle move and countermove based on high sensitivity to the logical implications of opponents' actions, it reflects the erratic and occasional reaction and overreaction to dramatic and shocking achievements by the opponent. The elements of surprise and humiliation may be crucial in determining the response. The first Soviet ICBM launching in the summer of 1957 caused nary a ripple in U.S. defense programs; the impact of Sputnik in the fall was far greater.[1]

In the Soviet case, one might hypothesize that the anger and frustration felt by Soviet leaders regarding the U-2 flights after 1956 provided a powerful stimulus to the development of the SA-2, which is reasonably effective at high altitudes. (It may also have intensified the traditional Soviet emphasis on air defense.) For low altitude defense, however, no such stimulus existed, and the Soviets' lag in this respect has been noticeable, despite its equivalent importance for effective air defense. But, more recently, the performance of Soviet SAMs in North Vietnam has been such as to suggest that a new and powerful stimulus may be operating regarding low altitude Soviet defenses. Other examples might be cited, including the ABM case to which I will turn presently. The point to recognize is that rational or pseudorational optimizing procedures may be far less important in determining an opponent's (or our own) responses than what organization theorists call attention cues.

If we are to deal intelligently with the arms control problem, we shall have to invest far greater effort than heretofore into what are the true attention cues for particular national organizations relying on imperfect information and achieving imperfect adaptation. What are the actions on our part that are likely to elicit a serious response by the opponent? Among a number of alternative responses, which one is he likely to choose, and can we influence that choice? (In the tendentious discussions that comprise public argumentation it is frequently assumed that there is but one route the opponent can follow.) What is the likely magnitude of the response in terms of resources invested—and in the face of conflicting budgetary demands? On the other hand, what are the classes of actions that we can take—and these are probably the majority—which the opponent will ignore or to which he will respond only after long delay?

Let us contrast the method of analysis stressing the uncertainty of the character and magnitude of the opponent's response with one that is frequently encountered, based on the casual extrapolation of logical tendencies. Let me cite the specific case of civil defense and raise some issues that will bear on the discussion of ABM in the next section. Consider the class of argument regarding passive and active defenses that suggests defenses create instability by undermining an opponent's confidence in his residual deterrent, thereby possibly inducing him to "go first" in a confrontation (or, at a lesser level, to further expand his strategic offensive forces). The argument is based upon a strategic concept, probably obsolescent, that deterrence is ultimately based upon the ability to destroy some specific fraction of an opponent's population. In regard to civil defense, so the argument runs, action on our part would either induce instability by tempting the opponent to a first strike or touch off a new

[1] Even here the spending implications—as opposed to the psychological and political consequences—tend to be much exaggerated. On a seasonally adjusted basis, defense expenditures in *constant dollars* held steady at roughly a $46 billion annual rate through 1957 and 1958.

phase of the arms race as the opponent attempts to maintain his kill potential. For this reason, it is argued that it may be wisdom to deny ourselves civil defense. Although the line of reasoning is in a sense impeccable, it is in conflict with what I would regard as the realities of interactions.

The effect of civil defense programs is difficult for a foreign observer to define precisely, and the ambiguity alone may lead an opponent partially to disregard them. Just as an aside, in our own calculations of Assured Destruction with present and programmed forces, we disregard the impact of a variety of Soviet civil defense programs. Would the Soviets be more responsive than we have been, especially along the lines of the destabilizing response? There are a number of reasons for thinking not. In contrast to offensive weaponry, civil defense is more subtle and less conspicuous, and I suspect that the most probable Soviet reaction would be to do little. Moreover, the argument reflects the American preoccupation with kill potential, which the Soviets may not share. The Soviets have steadily argued, as in ABM discussions, *that defensive measures should not be viewed as provocative or as a stimulus to the arms race.* It seems uncertain, to say the least, that *our* acting on *their* doctrine would necessarily generate the destabilizing consequences projected above. In addition, the Soviet offensive forces program, reflecting lengthy internal bureaucratic decisionmaking, is probably inflexible, and is likely to be played out, whether or not we take action on the civil defense front.

More fundamentally, the belief that both sides must and will insist on maintenance of the ability to destroy some preset fraction of the opponent's population is probably erroneous. It rests on a concept of strategic bombardment in spasm war, which becomes increasingly dubious as the two major antagonists deploy staggeringly large offensive forces. Do both sides need to maintain kill potential against, say, 25 percent of the opponent's population? Would deterrence not remain effective if both sides could reciprocally inflict lower levels of population destruction? Would the ability to destroy more than 50 percent of industrial capacity plus *only* 10 percent of the population, for example, be so inadequate a deterrent that the Soviets or ourselves would be tempted to initiate city-busting warfare? If we question, in this manner, the value of catastrophic threats to population as an unalloyed and stabilizing asset, it becomes unclear how the Soviets might respond. The response might reasonably be civil defense activities of their own. If this were the type of interaction, the net effect would be to reduce population destruction on both sides, if war should come, yet not significantly impair the effectiveness of deterrence.

From this civil defense example, I would suggest the danger of formulating arms control policies by the simple spinning-out of logical tendencies without deeper inquiry into the likelihood of response and the character and magnitude of a response, should it occur. But I would add one more element: the tendency, in the face of the very long lead times for the procurement of new systems, to decide on the acquisition of new hardware a decade or more hence with the objective of implementing the strategic concepts of today.

Future forces should have sufficient flexibility built in so that they can perform something more than that strategic mission which appears dominant today. In the present context, with Assured Destruction as the sole criterion, we have been selecting hardware essentially for the purpose of population kill. Yet, with the buildup of major devastation potential on both sides, it becomes increasingly doubtful whether such capabilities are the most reassuring or desirable to possess or whether they could serve any useful purpose in that range of contingencies which are becoming increasingly probable.

ABM AND ARMS CONTROL

Let me go now in some detail into what is so vexed a question: the implications of ABM deployment for arms control. Initially I shall deal with two sharply contrasting and simple formulations of the effect of ABM on arms control. Subsequently I shall raise some questions that will amplify and extend the list of formulations, and then discuss some considerations that I feel add realism to the role of ABM in the more probable nuclear clashes. Let me emphasize that I am trying to deal with ABM systems that we will encounter in the real world, that is, systems that are subject to enormous variance in actual performance and ones in which all of the ingenious improvements or retrofits that the engineers can develop will not have been incorporated.

At the outset let us examine the two views that have dominated the arms control debate. First, that expressed by Secretary McNamara in his San Francisco speech of September 1967; second, the one presented by Donald Brennan in a recent issue of the *Bulletin of the Atomic Scientists.* [2] (For the time being I am restricting my observations to the bilateral case: the Soviet Union and the United States.)

The Secretary argues that ABM deployment is disadvantageous for both powers. Each power can and will maintain its Assured Destruction capability, i.e., the ability to destroy its principal rival in retaliation after absorbing a first strike. At lower cost each major power will spend whatever sums are necessary to maintain its offensive forces so as to compensate for the attrition imposed by the defense. ABM deployment thus implies heavy and wasteful expenditures which will add to the stock of weapons of mass destruction, but ultimately it will leave both powers in the same relative position. By contrast, Brennan argues that a "Soviet freeze," i.e., one on offensive systems and with no limit on deployment of defensive systems, is superior to the previously proposed "American freeze" applying solely to defensive systems. He argues, quite rightly in my view, that both sides could continue to deter even as their Assured Destruction capabilities shrank with the buildup of defensive systems. If both sides retain the ability to destroy say five to ten percent of their rival's population, mutual deterrence will continue to be as effective as with 25 to 30 percent of the respective populations at risk, but we should all be better off—if deterrence were to fail. I believe this is a fair representation of the two views.

Certain obvious questions may be raised. First, Brennan has little to say regarding the inducing or the policing of the freeze on offensive capabilities. My own view is that such policing continues to be infeasible, especially since the development of the MIRVed[3] missile. One would have to have access to launching tubes, silos, and pads, and to examine the missile payloads in order to prevent the expansion of offensive capabilities. From the first the Soviet Union has with unusual consistency opposed any such prying activities on the part of the West.

Second, the Secretary's view presupposes a substantial elasticity of budgets accompanied by a total inelasticity of demand for second-strike population-kill capabilities. In view of the very substantial sums involved, this is subject to question. It would require a remarkable and extraordinarily smooth process of adjustment, if such major activities, with all the attendant uncertainties, were to take place, and everything remain the same as before. Let me mention a few additional propositions which could drastically affect the probable outcome even if we remain within the context of Assured Destruction capabilities.

[2] D. G. Brennan, "New Thoughts on Missile Defense," *Bulletin of the Atomic Scientists*, June 1967, pp. 10-15.

[3] Equipped with multiple independent warheads.—*Ed.*

One rather disconcerting possibility, sometimes stressed by political scientists, is that ABM deployment would be far more destabilizing than the Secretary indicates. It is suggested that each nation will fear that after an opponent's initial strike, it could strike back only with a *ragged* attack which could readily be handled by its opponent's ABM system. Therefore, so the argument runs, each nation will be tempted to strike *before* its own forces have been attacked. The ABM deployment thus would recreate the vulnerability problems that existed with the not-well-protected offensive forces in the 1950s. ABM deployment therefore may be highly destabilizing. I personally do not put much credence in the argument, for it implies that the temptation to go first is strong despite the assurance of one's own destruction, and that the disadvantages of waiting will appear overpowering. In my view the attractiveness of initiating and the weaknesses of deterrence are overstated, but I believe that the position should be mentioned.

A second possibility is somewhat less disquieting, but also more realistic in my view. Suppose that, in practice, ABM systems will perform quite badly. Yet, each side apprehensively overstates the capabilities of its opponent's ABM and each side consequently hedges by overbuilding its offensive forces so that the "requisite damage" can be inflicted—in the face of very effective performance by the opponent's ABM. Thus, each side overresponds—and each side acquires even greater devastation potential than it originally thought was necessary. Moreover, the weapons obtained may be designed solely or primarily for the Assured Destruction mission and be useless in other roles. There is at least some evidence that this is taking place. The United States has espoused a doctrine of "offensive-conservative" and may be overbuilding in relation to the real capabilities of the Soviet forces. If the Soviets were to do likewise, the result would be as hypothesized in the model: a superfluity of weapons designed for the mass destruction mission.

Third, consider a more reassuring possibility. The evidence from the past hardly demonstrates that the Soviets' demand for Assured Destruction capabilities is inelastic. Throughout the '50s the Soviets were niggardly in providing resources for the intercontinental mission. Their retaliatory capability was limited to perhaps 5 to 10 million Americans, and they seemed satisfied with that condition. In the past the Soviets have not been captivated by the strategic bombardment doctrines that took hold in the West during World War II. Unfortunately, there is some evidence that the Soviets have recently been "learning" from us. Their strategic doctrine has tended to lag 5 to 10 years behind our own, and there are a number of indications that the current discussion within the Soviet Union is couched in terms of the Massive Retaliation notions popular in the United States in the late '50s. Nonetheless, the Soviets have historically been defensive-minded. When faced with the enormous budgetary drains represented by the development of an ABM system, is it certain that they will simultaneously accept the need for investing even greater sums in offensive forces in order to counter the effect of the U.S. ABM? Instead of moving to offset the U.S. ABM entirely might they not rather downgrade or ignore the capabilities of the system in their planning?

Fourth, to expand further on this point, much of the domestic debate on arms control has been couched in a hypothetical manner that disregards the budgetary and bureaucratic constraints facing any nation. It presupposes a degree of responsiveness to the deployment decisions of a rival that is historically questionable. What if deployment decisions and plans are, in fact, unresponsive? I would argue that the Soviet program for offensive forces was designed years ago, that it will be completed irrespective of arms control initiatives from the United States, and that it might not be further expanded unless American activities "shock" the Soviets out of a preconceived mold. If our activities are such as to tranquillize the Soviets, it

becomes unlikely that they will face up to the painful budgetary and bureaucratic adjustments implied by a substantial expansion of the offensive forces program.

It must be kept in mind that the defensive forces, other military components, and other bureaucracies will be jealously safeguarding their claims to a rightful share of total Soviet resources. There is no evidence that Soviet analyses are as competent as those in the DoD, or that there exists the equivalent of a Secretary McNamara who could impose a reallocation of resources, or even that the Soviets are as dedicated to maintenance of Assured Destruction as are we. When these budgetary and bureaucratic limitations are recalled, one might suggest slow responsiveness to a U.S. ABM buildup with a consequent Soviet willingness to permit some erosion of their Assured Destruction capabilities.

I believe that these factors, described as "non-rational," deserve careful attention. It may appeal to both our pride and our sense of logic to treat the arms policies of nations as if they were logical in the game-theoretic sense. Nonetheless, in the determination of arms policy the behavior of states is more rigid and their objectives are less sharply defined than it is customary to admit. If this is difficult to believe for the Russians, consider our own arms decisions here in the United States: the ABM decision, for example. The Secretary of Defense indicates that deployment may be useless, and is undoubtedly premature. Nonetheless, the United States decides to deploy. Why? It seems doubtful that the United States would have deployed, if the Soviets had not "preempted" by initiating deployment of a well-advertised system. Did electoral calculations, looking forward to 1968, have something to do with the decision to go ahead? Such calculations strike me as a non-negligible influence.

What this suggests is that the Soviet decision to deploy, working through political factors, precipitated the U.S. decision. Yet, irony of ironies, the rationale publicly offered features defense against the *Chinese* threat. Does this not dramatically underscore the role of mechanical, imitative, and unanalytical influences in national decisionmaking? To go on, although the only reasons we ascribe for deploying ABM are (1) defense against China, and (2) hard point defense of Minuteman sites, some statements have been made about deploying ABM in Europe—to which neither of the reasons could possibly apply.

The ways of political decisionmaking are wonderful to behold—and really lie beyond reproaches of the rationalist critic. But, it appears to me that, knowing what we really know about political decisions, it is inappropriate for us to pretend in our analyses that such decisions emerge from rational models.

Some of the major implications of ABM deployment are obscured by the type of debate indicated earlier: first, because the role of uncertainties is ignored; second, because the argument is couched in terms of Assured Destruction; and, third, because quite frequently only the bilateral case is considered. Uncertainties, which get shunted aside by assumption, affect the results in ways other than simply inducing both parties to build up redundant offensive capabilities. The fact that uncertainties regarding the actual performance of the opponent's defense are ineradicable might provide an important stabilizing element. If one can never be sure, whatever temptation may exist to go first in a *surprise* attack will be further reduced. Only the most desperate circumstances would induce one of the major powers to alter its conviction that waiting is better than going.

Perhaps an even more important consequence of ABM deployment is obscured by confining the debate to the Assured Destruction case. One of the apparent casualties of the ABM deployment is damage limiting through counterforce. The argument seems to be: we cannot be sure we could penetrate the defenses in a timely manner; therefore why should we invest major resources in counterforce? It is better to put

our resources into what is assumed to be the real task: Assured Destruction of the civil fabric of the opponent's society. Given the present American predilection for "offensive-conservative" and the belief that we must exhaust the opponent's defenses (the rival is assumed to be able to discriminate decoys), the result is the proliferation of small RVs that can be relied on ultimately to destroy the rival's cities, but are of limited utility for counterforce. I fear that at some future date the historical conclusion may be that the most significant result of ABM deployment was to turn our thoughts away from city avoidance and minimizing collateral damage to the civil fabric of our opponent's society and toward employment of military capabilities in their most destructive mode.

Admittedly ABM should make a difference; under any circumstances it would have major implications for arms control. But perhaps its most important impact has been to augment the perceived difficulties in attempting to do clean counterforce and to turn our attention and concentrate our inventiveness, rather obsessively, on the city-destruction task. I question whether this is a healthy development. In the event of war the most desirable thing that can happen to weapons of mass destruction is that they be destroyed before they inflict damage. For the reason indicated earlier, I would question whether Soviet procurement policies will be much influenced by our non-procurement of counterforce capabilities. The direct response to U.S. procurement decisions is probably not close, but in any event the Russians will ascribe counterforce capabilities to us whether or not we procure them.

Perhaps more fundamental is the need to question the view that the Soviet ABM will necessarily frustrate a U.S. counterforce mission. In satisfying our compulsion regarding "offensive-conservative," we have been ascribing astonishing capabilities to the Soviet ABM system, while remaining keenly aware of the deficiencies of our own ABM. Yet, ironically, we are persuaded that our own defense systems are superior. Is this offensive-conservative? To me it does not seem to be even simple prudence. There must be some consistency; if it is so easy for the Soviets to penetrate our system, it seems rather odd to assume that theirs will be leakproof.

When one considers the numerous, alternative ways of degrading or penetrating an ABM system, the more unwise it seems to me it is to become discouraged about counterforce. I cannot go into details, but far too little attention and effort has gone into designing a system intended to degrade an enemy ABM system. *Any real-world system is going to have vulnerabilities.* The possibilities for circumventing the defenses are substantial. Quite frequently, these possibilities are defined away because paper studies by our engineers design for the Soviets highly versatile systems with no vulnerabilities. Real-world systems are not like that. The initial deficiencies are only irregularly cured. This seems especially pertinent since the Soviets have been even more prone to living with the deficiencies of systems as initially configured than have we.

If war can be kept at a low level and directed toward military rather than urban targets, it would seem to me to be most consistent with the objectives of arms control. The aspirations of arms controllers should be higher than to deal simply with the numbers of weapons. Ideally, arms control should deal with strategies as well as weapons. Arms controllers should examine whether arms postures, which would permit conflict to be carried on between military forces rather than to be bent toward urban destruction, are not more consistent with their long-range goals. In the arms control context the connection between strategies and numbers of weapons deserves much more serious attention.

BEYOND THE BILATERAL CASE

One of the difficulties with many arms control analyses has been that, when the complexities of an n-person world become too staggering, the tendency has been to slip back into the bilateral case. When one considers the importance attached to nonproliferation in arms control, the basic inconsistency of confining analyses to the bilateral case becomes apparent. By and large, arms controllers have sought a world in which the two superpowers reduced their offensive forces to much lower levels and in which proliferation, preferably beyond the original two nuclear powers, either did not take place or was reversed.

Is there not a basic inconsistency in this set of desires? Is it not the dominance of the superpowers—and their ability to control lesser conflicts—that provide the principal deterrent to the rapid spread of nuclear weapons? Without the essential bipolarity existing in the military realm, the tendency toward acquisition would be strengthened. To discourage nuclear spread, and to control the spread that takes place, the military capabilities under the control of the superpowers may have to be *disproportionately* large. This applies not only to counterforce capabilities, which sharply reduce the potential threat embodied in third forces, but also to ABM deployments. The existence of even moderately effective ABM systems will diminish the ability of the third forces of Britain, France, and China to destabilize the international political balance.

Arms controllers, like other groups, tend to deal with those parts of a problem that they consider subject to manipulation. It is interesting therefore to note that in recent years arms control proposals have focused exclusively on what the United States, the Soviet Union, and Britain can be induced to do. But the activities of others, most prominently those of China, will not go away and are too important to ignore.[4] The discussion in the Wiesner Committee's report some years ago concentrated primarily on the bilateral case: an ABM moratorium and a freeze on strategic delivery vehicles for the United States and the Soviet Union. China was dismissed as essentially a trivial influence in the international power balance.[5] In some respects this was a healthy step, for there has been a marked tendency on the American scene to inflate the potential Chinese threat. Nonetheless, China cannot be reduced to triviality. In this connection let me cite an *earlier* paper by Jerome Wiesner in which he stated:

> Any comprehensive arms-control agreement will of necessity involve the participation of the government of the People's Republic of China. . . . It is obvious that if we fail to gain their participation in arms-control agreements, we will have to accept a serious shortcoming in any security system we create.[6]

I suggest that it is unwise to truncate or abandon analyses simply because the real-world possibilities do not match our aspirations. We must carefully study what the inability to capture China within the context of arms-control agreements implies, for China's nonparticipation may alter the ideal shape of such agreement. We

[4] The Department of Defense has not, of course, ignored third parties. China, if not the broader proliferation problem, was stressed in Secretary McNamara's recent announcement of the ABM decision. My references here are to the views emanating from the arms control community rather than to the actual policies of the U.S. Government.

[5] The White House Conference on International Cooperation, National Citizens' Commission, *Report of the Committee on Arms Control and Disarmament,* November 28-December 1, 1965.

[6] Jerome Wiesner, *Where Science and Politics Meet,* New York: McGraw-Hill Book Company, 1965, p. 235 (originally published in the Fall 1960 issue of *Daedalus).*

cannot afford to attempt to limit only the two superpowers because other nuclear states refuse even to participate in arms control discussions.

Arms control policy is faced with a special dilemma. The military dominance of the United States and the Soviet Union has served to keep the proliferation threat under control. If arms control is successful in reducing the relative advantages of the superpowers, their ability to stabilize third areas of the world will be substantially reduced. The effect may be to turn the third areas into even more of a mess than they are today and to set off a scramble for nuclear weapons.

It is reasonable for arms controllers to aspire (1) to reduce superpower armaments, forestall ABM deployment, and so on, and (2) to prevent proliferation and maintain stability in the third world. I am not sure that both objectives can be pursued at the same time. In any event, the conflict between the two objectives must be studied, and whatever choice is necessary should be explicitly made. Unless there is some underlying consistency in the objectives that we choose to endorse, we are indulging in pieties. Indulging in pieties is not the path to serious influence on national policy decisions.

DEFENSE DECISIONMAKING

THE CHANGING ENVIRONMENT FOR SYSTEMS ANALYSIS

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Whenever we look at the changing environment and its implications for decisionmaking, that commonplace English word "uncertainty" is certain to be introduced into the discussion. Unfortunately it is doubtful whether its introduction will serve to illuminate the knotty problems of choice implied by constant change. No word is used more eagerly or more glibly. All too frequently, however, "uncertainty" is employed as an incantation to exorcise from the analysis the troublesome questions that it cannot resolve,[2] the result being a genuflection toward what remains unknown before decisions are made on the basis of the limited picture of the unfolding environment, as it is perceived at a point in time.

For this reason I might be inclined, following Kierkegaard, to begin with a panegyric on uncertainty, but this would be misleading, in a sense, for the most interesting consideration in dealing with national policy is not so much uncertainty as certainty. In both analysis and planning we are too prone to ignore the certainty that things change over time: that a number of years hence national objectives and strategies will be different from what they are today. In planning for the future the appearance of uncertainty at a given time is perhaps less interesting than *the certainty that changes in objectives and strategies will take place over time.*

What is true for ourselves is also true for others. As we look at earlier analytical efforts, I think we will recognize the failure to appreciate the eroding effect that time inevitably has on all perceptions of the environment. Part of this failure undoubtedly has reflected the inability fully to anticipate the consequences of success. In conditions of conflict even partial success implies that one's foe will be forced to divert his energies to actions designed to exert pressures on those vulnerable points now made relatively more lucrative by one's previous successes. For the game theorist such a development is perfectly predictable, but in the real world the tendency of governments to concentrate attention on immediate and pressing concerns makes it difficult to give adequate weight to this insight. More allowance can be made in future work for the alteration over time of opponents' objectives and strategies (partially in response to our own moves) and for our own adaptation to these anticipatable, if not predictable, changes in behavior.

There are other important, if more narrowly technical, reasons for the past failure to appreciate the erosive impact of time. First, systems-analytic work has been overly tied to the rather unique conditions that originally gave rise to the art. It has been too much dominated by those relatively simple strategies appropriate for the early nuclear period—which permit the quantitatively precise evaluation of mutual destructiveness in a showdown clash, actual or hypothetical, between the

[2] Or to provide a form of personal insurance so that, if events go awry, the analyst need not be held accountable.

two superpowers.[3] Second, and perhaps more fundamental, an illusion developed that systems analysis as a technique was not itself subject to change and that the basic methods were more or less impervious to the eroding effects of time.

This identification of the evanescent with the permanent has introduced certain rigidities into analysis. These are in the process of disappearing, not only because of the changes in the environment per se, but because the environment has changed in a particular direction: decisionmaking in the '60s is increasing in its complexity and this carries with it implications, not only for the decisionmaker, but for the analyst as well. A clear moral is implied for future systems-analytic work: there is a growing requirement for care in the design of studies or analyses and for ingenuity and flexibility in the design of systems. In developing this theme I shall attempt to do three things: (1) to indicate those trends that have intensified demands on the analyst, (2) to illustrate the general issues raised, by reference to two specific analytical and choice problems confronting defense analysts, and (3) to draw a few inferences regarding future analytical work.

MAJOR ENVIRONMENTAL CHANGES

Since the developmental years of systems analysis, changes have occurred in the strategic, political, and technical environment which raise the level of sophistication required of the art. Four specific factors may be cited, which, operating together, have accentuated demands on the analyst. As we examine them individually, we will see that none of them is wholly new. Yet, growing weight must be assigned to each, and jointly they considerably augment the challenge to the imagination and insight of the analyst.

The Increase in (Perceived) Political Fluidity

In the past, the assumption of Soviet malevolence, accompanied by estimates of future Soviet capabilities based on production possibilities, led to the designation of specified threats in particular time periods. The challenge to the analyst was to design a broad system to deal with the assumed threat. This approach was employed in 1950 shortly after the first Soviet atomic test.[4] The year 1954 was specified as one of "maximum peril" on the hypothesis that by then the Soviets would have produced atomic weapons in sufficient number to neutralize the U.S. atomic deterrent. By that year, it was argued, conventional forces would be required to prevent an outthrust of Soviet power. Whatever the merits of the contemplated posture, the argument for it was temporarily eclipsed by the "new look" decisions of 1953-1954.

Again, in the "missile gap" controversies following the Soviet development of the ICBM in 1957, projections were developed indicating 1961-1962 as the period of maximum danger of a Soviet strike possibly—or even probably—from the blue. The degree of risk was properly regarded as dependent on the U.S. posture. The main objective of the analyst was to devise means for countering a clearly specified threat —one which implied the "worst possible" consequences. In such an environment, the

[3] This bias is reflected in the continuing tendency for high DoD officials to present their rationale on the acquisition or continuation of the strategic systems in Packages I and II (and even to evaluate such systems) largely in terms of the destructiveness of a nuclear war in which the Soviets strike in an unconstrained manner.

[4] See Paul Hammond's study, "NSC-68: Prologue to Rearmament" in Schilling, Hammond, and Snyder, *Strategy, Politics, and Defense Budgets*, Columbia University Press, 1962.

"minimax rule" compelled attention for guiding analysis. Once again, the existence of an acknowledged and specific threat simplified the task of the analyst.

It would be improper to suggest that a procedure of specifying and responding to a single dominant threat is simply wrong. As a simplifying assumption in periods of revolutionary changes in military technology, it has its uses. Indeed, if once again the nation were confronted with another such revolutionary change, we might well return to using this analytical device. But consider how different our situation is today. Instead of having to deal with a dominant threat in a specific time period, we deal with *a spectrum of vaguely perceived and more modest threats which may develop at some indefinite time in the future.* No longer do we feel that the Soviets can negate our second-strike capability without signs of buildup—and the absence of a dominant threat cancels out the "minimax rule" as a guide to action. We are less inclined to view the Soviets as either implacable foes or rational game-opponents or to attempt to anticipate their actions on such a basis. No longer do we seem to place much confidence in our ability to predict what the Soviets will do. As a result, we can no longer concentrate our resources on countering a single maximum threat. Instead, we must allocate resources so that we can deal with many (preferably all) of the broad array of vaguely perceived threats—threats that can be posed by the Chinese and others as well as by the Soviets.

Greater Sophistication Regarding the Character of Nuclear War

A second factor, interlocking with the first, that intensifies demands upon analysts, is the growing sophistication on both sides regarding the character of nuclear war. We will go into this subject in more detail below; here we might simply note the major changes.

First, we now recognize both a number of conflicting objectives and major uncertainty as to which of these objectives would be dominant in nuclear war. As a consequence, even greater attention must now be devoted to the criterion problem. In the fifties, analyses of possible nuclear exchanges did not get much beyond two-sided spasm wars.[5] In both the first-strike and second-strike variants, the objective was clear-cut: destruction of the maximum number of enemy targets, either cities or highly vulnerable strategic air forces. The targets were known, immobile, and soft. Destruction was to be achieved without worrying about collateral damage (which was, at that time, a "bonus" effect). Damage limitation for the United States emerged principally through the reduction of Soviet offensive capabilities either by preemption or quick retaliation subsequent to the Soviet first strike.

Retrospectively viewing these early-vintage studies of broad strategic systems, we are probably struck by the relative simplicity of the problem as it then was seen. Only a single type of weapon system was involved. Penetration was a manageable problem. The focus of attention was on survivability, but it was the short-lived survivability necessary to hit back in a spasm-war strike. Thus, in the early fifties, the relative advantages of ZI[6] basing versus overseas basing in the two main contingencies would correctly become a major issue.

By current standards, the defects of this outlook are numerous—even if we confine ourselves to analysis of central war with the Soviets as the main foe and disregard complexities introduced into strategic planning by the rise of China. Forces and analysis have advanced. Conceptions regarding both objectives and

[5] To assure that, if a Soviet attack came, the war would be at least two-sided, was the dominant concern—and in view of the character of the existing forces, it was then the appropriate concern.

[6] Zone of the Interior—*Ed.*

strategy, if not wrong then, are now obsolete. Manipulation of a single offensive weapon system as the main variable for both deterrence and warfighting is now wholly inappropriate. The image of how nuclear war might come about—in massive initial strikes with little or no warning—must now be modified. As a consequence, a sharp line can no longer be drawn between strategic forces and general purpose forces. Today we are concerned with damage limitation, and with combinations of offensive and defensive systems to attain that end. A variety of strategic offensive systems exist which potentially may contribute to our strike capabilities, but the optimal choice will depend upon the circumstances.

Partly because of the inherent difficulties if the Soviets take sensible countermeasures, partly because of the absence of really hard thinking on the part of the Services, partly because of high-level policy decisions, it is now generally accepted that the option of a highly successful disarming strike is not open to either side. A principal consequence of this view is the reinforced emphasis on avoiding collateral damage to the Soviet Union. If the Soviet offensive forces capable of inflicting drastic damage on the United States cannot be eliminated, the counsel of wisdom suggests that we provide every incentive to the Soviets to exercise restraint in the use of their surviving forces. This implies keeping the Soviets aware of how much remains at risk, if they behave rashly. This concern about collateral damage means that we must now give careful attention to balancing objectives to which we gave no thought in earlier systems studies—for example, knocking out a hardened target with a weapon of the smallest possible yield. A half-decade ago, as far as collateral effects were concerned, we were indifferent as to the size of weapons to be dropped on point targets.

Two major consequences of this growing awareness of the complexities surrounding nuclear war should be underscored.

The first stems from the fact, mentioned earlier, that emphasis has shifted from the operating characteristics of individual weapon systems to the combination of numerous systems into an integrated package. The stress has shifted to the complementarities among weapon systems, and *a major goal in tradeoff analysis is to improve compatibility between systems,* even if some otherwise desirable characteristics must be sacrificed. This trend is epitomized by major DoD studies in recent years, and perhaps most revealingly in the decision to combine Package I and Package II for analytical purposes.

This new emphasis on complementarities requires a major adjustment in studies for the Air Force. Historically such studies have relied on the weapon-system concept pioneered at Rand. But now the Air Force will have to grapple with the same intractable problems in integrating weapons systems that the other Services have always faced—at least implicitly. For the Army, the meaning of the weapon-system concept has always been rather fuzzy. Center stage has been something like the infantry division, which ties together a large number of separate capabilities. What meaning could there be, for example, in a "howitzer weapon system" or an "armored car weapon system"? In the strategic field, at least, the Air Force has been fortunate in the past in having relatively clean-cut analytical devices at hand. But this increases the difficulty of adjusting to a package-oriented environment, and there is small comfort in having to pay the penalty for past success.

The second consequence that we must now recognize involves a related set of complicating factors, which stem from our changed image of how nuclear war may come about. Given the prospective strategic balance, with the potential for devastation embodied in the forces that would survive a disarming attack, it becomes very hard to envisage nuclear war being initiated suddenly with all-out strikes. If it were to come, it is most likely to come in a sequence of escalating steps from a lower-level

confrontation. This implies the need for careful study of how best to mesh general purpose forces and strategic forces. Strategic forces will either serve to control or fail to control the process of escalation—"keep the lid on," in the current parlance. The less advantageous the strategic balance is to the United States, the bolder the enemy may be in any specific crisis. On the other hand, limited war forces, including selected nuclear forces, may through their existence or through their employment serve to control conditions which could escalate to central war. Thus, limited war forces—with or without firebreaks[7]—are part of the mechanism of deterrence of central war, and the complementarities and tradeoffs between the two types of forces must be carefully analyzed.

If crises and potential crises are the seedbed of central war, and if effective crisis management constitutes a principal means for reducing the risk of central war and for obtaining settlements on terms favorable to us, then some of the confidence generated in the past by systems studies must perforce disappear. Crises involve so many unpredictable elements—boldness, resolve, and determination in the pursuit of one's objectives; rapid and unforeseen adaptation or improvisation of military capabilities—that neatness and precision, if obtained in systems studies, will not be consistent with the messiness of real-life conditions.

Both the increased stress on packages, complementarities, and mission tradeoffs (under conditions in which we hope that central war, if it comes, will be characterized by restraint), and the increased tie between strategic and limited war capabilities (stressing the recognition that central war, if it comes, will come via escalation) diminish our ability to get a quantitative handle on strategic problems. The role of assumptions in providing a royal road to quantitative conclusions has increased by something like an order of magnitude. While varying degrees of confidence will be placed in such assumptions, the problem of analysis is markedly different from what it was in the fifties, when contingencies could be mapped out in advance. Whatever the confidence placed in assumptions, the probability that they will be wrong is very high.

Increased Emphasis on Highly Specialized Weapon Systems

The demands on the analyst, particularly in force-structure determination in an environment in which mission tradeoffs have become critical, is heightened by the highly specialized nature of many modern weapon systems. In part, specialized systems may be required because of our altered objectives in nuclear war. Given a desire to avoid collateral damage, the option of going to higher levels of violence to achieve target destruction disappears. One must attain higher performance levels in target destruction. The end of reliance on big yields may imply a variety of highly specialized delivery vehicles.

Perhaps more importantly, specialized weapon systems are also needed to counter enemy advances or to exploit enemy vulnerabilities. But because of their inflexibility, such weapon systems will be required in the force structure only if the enemy adopts certain courses of action rather than others. A highly specialized system, by definition, invests major resources in a specific kind of capability. On a cost-effectiveness basis, however, the allocation of resources for a highly specialized purpose is warranted only if the enemy chooses to procure and deploy just those capabilities for which the highly specialized system is a countermeasure.

Let me illustrate this problem by reference to the growing variety of delivery

[7] The nuclear firebreak is the threshold between nonnuclear and nuclear war. The first use of a nuclear weapon is "crossing the nuclear firebreak." —*Ed.*

vehicles and concepts. A decade ago there was only one delivery vehicle: the bomber. Before the order-of-magnitude improvement in air defense capabilities, the principal problem was to get the aircraft over the target. A system analysis concentrated on tradeoffs among range, weight, payload, and speed, and paid some additional attention to basing concept, alert status, vulnerability and the like. However, with the arrival of surface-to-air missiles, the picture began to change. Against extensive air defenses, even with defense suppression, high-altitude attack looked less promising. To circumvent Soviet SAMs,[8] low-altitude penetration became the accepted concept. In principle, bombers optimized for low-altitude operations became attractive. At the same time, long-range missiles, free of any initial problems of penetration, were being deployed, and they had major cost advantages over bombers for most tasks involving known immobile targets.

Yet the possibility of effective ABM defenses has lurked in the background. Given the improvement in low-altitude air defense, major difficulties in penetration could develop with existing U.S. delivery vehicles. A case may exist for developing, as a hedge, advanced systems like SLAM,[9] designed to circumvent such defenses through low-altitude penetration at Mach 3. But that case does not extend to procurement and deployment—until such time as Soviet action makes so specialized and costly a system an attractive buy. If Soviet defenses do not improve to such a degree, other less costly measures will suffice—improved penetration aids, for example.

The point is that much of U.S. research and development activity should be devoted to developing specialized capabilities designed to counter Soviet developments which would exploit vulnerabilities in existing U.S. systems. Yet the Soviets cannot acquire capabilities to exploit all our vulnerabilities; they will have to choose. Thus, many of our own specialized systems need never be procured. On cost-effectiveness grounds, appropriate actions for us depend upon those routes the Soviets actually choose to follow. And since force-structure decisions, more than R&D decisions, are critically dependent upon intelligence, the broader the menu of capabilities from which we must choose, the more vital good intelligence becomes in analysis.

One additional consideration must be added. Development of specialized systems implies, almost by definition, that there are fewer hedges against the failures of *close substitutes.* In the fifties, the F-101, the F-104, and the F-102/106 programs were in some sense substitutes. The opportunities for transfer of subsystems from one program to another were sizable. But in recent years the increased specialization of systems and subsystems reduces this kind of hedge. Increased specialization means less opportunity for partial overlaps among programs. This imposes greater demands on the R&D program, a fact that leads us to our last point, which concerns the growing financial strains in R&D.

Rising Costs of Research and Development

That costs of developing military systems since the early fifties have been rising rapidly, perhaps exponentially, may be taken as a datum. Opinions vary regarding causes and possible cures, but the fact itself is beyond dispute. Given relatively stable budgets, either the number and variety of systems both developed and procured will fall, or else a number of systems successfully designed will not be carried through the full and costly development cycle as it is now known.

At the same time that the supply of new weapons is under downward pressure,

[8] Surface-to-air missile—*Ed.*

[9] Supersonic low-altitude missile—*Ed.*

the demand is rising for varied weapon development to hedge against uncertainty. As mentioned earlier, we live in a period in which the strategic balance is likely to change slowly and in no clearly predictable manner with respect to new weapons. Yet, though the direction of advance is not clearly charted, instability and change do lurk in the background. We must use our R&D resources to counter a number of potential Soviet threats.

Happily, the possibility does exist for directing the R&D more toward hedging against a large number of possible surprises and less toward developing a smaller number of operational systems. There are two contrasting approaches that can be taken to hedging against uncertainty. The first is to have in development a number of complete systems—one of which, as a threat crystallizes and a need is perceived, has qualities which make it adaptable to a new mission. This is the now traditional approach to aircraft development. It characterized bomber and fighter development during the fifties. It was comforting to discover, for example, that the B-52 did possess a low-altitude capability or that the F-104 could be adapted to the role of an attack aircraft.

The second approach is quite different. The stress is on a well-stocked R&D menu, with numerous specialized projects which can rapidly be moved into the procurement-deployment stage, if the need arises. The focus of the program is shifted away from *full systems* development to exploratory and advanced development *stages*. The goal is to create, in effect, a shelf of advanced weapon hedges. The key concepts here are technical building-blocks, preliminary compatibility studies, and system design. This may be called an option-creating and option-preserving strategy for R&D. It contrasts with a strategy in which the major effort on the scientific-technical base represented mainly feedback from the objective of full systems development. It involves recognition that successful development does not necessarily involve procurement, and that procurement and deployment will probably not follow from successful development in the majority of cases. A major problem with this strategy, of course, is that the willingness to cut off a successful program goes against the grain for both the technologists and the organizations responsible for its development.

If we are prudent in allocating our energies, there seems to be little reason to wonder which is the appropriate kind of hedge strategy to pursue in the foreseeable future. Given the rising cost of systems, the falling supply of new weapon systems, the growth of specialized potential threats which existing systems are unlikely to prove sufficiently flexible to counter, and the need for more specialized capabilities to counter specific threats, it appears that the appropriate means for hedging against surprises is through an enhanced R&D program, in which individual projects are austerely conducted—a program designed to create and preserve a multitude of options. As the Soviet Union gives indications of pursuing particular lines of attack, we could move with moderate speed to counter those actions. We must be aware that such a strategy involves the quick response of the American economy, when production and deployment prove necessary. But in light of the proved flexibility of U.S. industry and technology and the historical sluggishness of the Soviet economy, we can have a measure of confidence that, in the final race for completely operational forces, we would come out ahead.

Yet we should be aware of the greater challenge this strategy represents for system design and system analysis. The designer must deal more than he would like with preliminary work on incomplete systems. The analyst, as he looks to the future, must deal with more or less hypothetical forces on which it is extremely difficult to get even a rough quantitative handle.

TWO CONTEMPORARY ANALYTICAL PROBLEMS

Up to this point, the discussion has run largely in terms of generalities—generalities which help to explain how the character of analytical studies has been altered since the mid-fifties. To provide additional substance, let us examine two recent analytical and decision problems: (1) the optimal resource allocation for damage limitation, and (2) the choice of a specific offensive force-mix and the limits this choice may impose on strategic options in subsequent time periods. The former is intended to illustrate the points made earlier regarding our growing sophistication about nuclear war and our changed perception of the threat. The latter is intended to illustrate what has been said regarding the problem of covering a broad spectrum of threats in light of the increasing cost and decreasing flexibility of new weapon systems.

Criteria for Resource Allocation for Damage Limitation

The first problem is, of course, the subject of the ongoing DoD studies on optimizing Packages I and II. Certain methodological aspects of the existing studies could be criticized—in particular, the deficiencies of parametric analysis for long-range force-structure planning in light of the absence of time-phasing and the impossibility of identifying or analyzing critical decision points. But it is perhaps better to concentrate attention on a single issue: that the quantitative results rest upon inherently subjective estimates. In the final analysis *the optimal allocation will depend upon assumptions regarding a subjective parameter: specifically, the probability and duration of a period of mutual restraint and city avoidance in nuclear war.* This is a case in which what previously was called "the royal road to quantitative conclusions" must be based upon some rather questionable initial assumptions. Either explicitly or implicitly, some estimate of the probability of city avoidance will enter into the analysis and determine the results. But since this highly subjective element will influence both resource allocation and strategic choice, it should be considered explicitly—rather than be ignored in the quest for deceptively firm quantitative conclusions.

Broadly speaking, current studies attempt to reveal, for various budget levels, the optimal point on constant-damage tradeoff curves for allocating funds between strategic offensive forces, on the one hand, and optimized civil defense and terminal bomber defense, on the other. (See Fig. 1.) But the optimal point is dependent on the degree of mutual restraint assumed—in that for a war which is primarily counterforce, it will be advisable to invest relatively greater resources in expanding, diversifying, and protecting our offensive capabilities.

Let us consider two extreme and hypothetical cases, and explore their implications for resource allocations. In the first case, Mr. Brezhnev and his successor, if any, as well as the American President, repeatedly emphasize in their public statements that nuclear war is a terrible thing: that it would be disastrous were it to come; that if it does come, the loss of life must be held down; and that "our" side would never initiate a strike at enemy cities, but reserve its "invulnerable" forces for retaliation should the foe strike cities first. Such declarations, if made repeatedly, would certainly influence our view as to the nature of nuclear war and the preparations we should make for it.

In the second case, we feel quite sure that the bulk of the Soviet missile force is pointed at our cities, and that the decision to launch will follow immediately upon any substantial U.S. strike—either because the missiles are, in effect, wired for an automatic response or because the authority to fire descends automatically to lower command levels when U.S. warheads impact on Soviet soil.

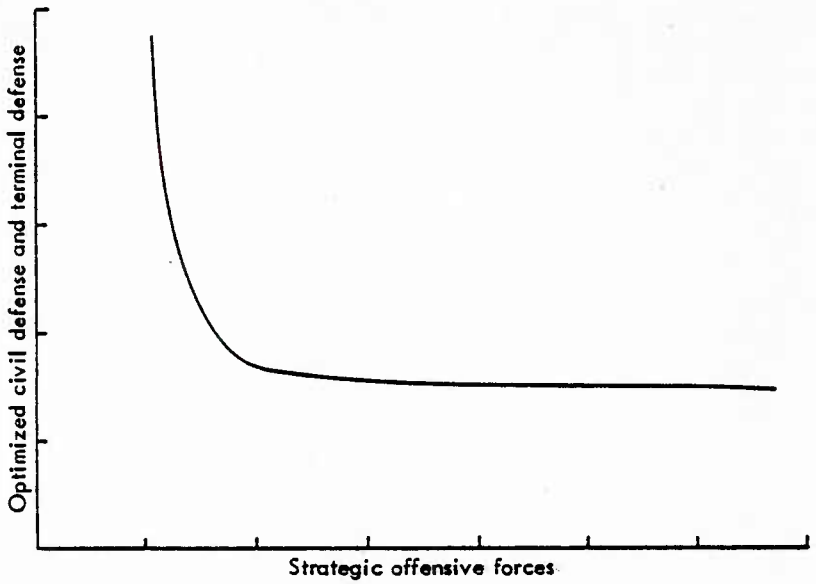

Fig. 1—Tradeoff curve for damage-limiting

Clearly, under these two sets of hypothetical conditions, we would assign very different subjective probabilities to the existence and extent of a period of mutual city avoidance. But it should also be clear that, given these alternative probabilities, we are dealing with *two different tradeoff functions* between strategic offensive and strategic defensive capabilities. Thus, in Fig. 2, Curve A represents a tradeoff function in the case of spasm or near-spasm war. For damage limitation in such a war,

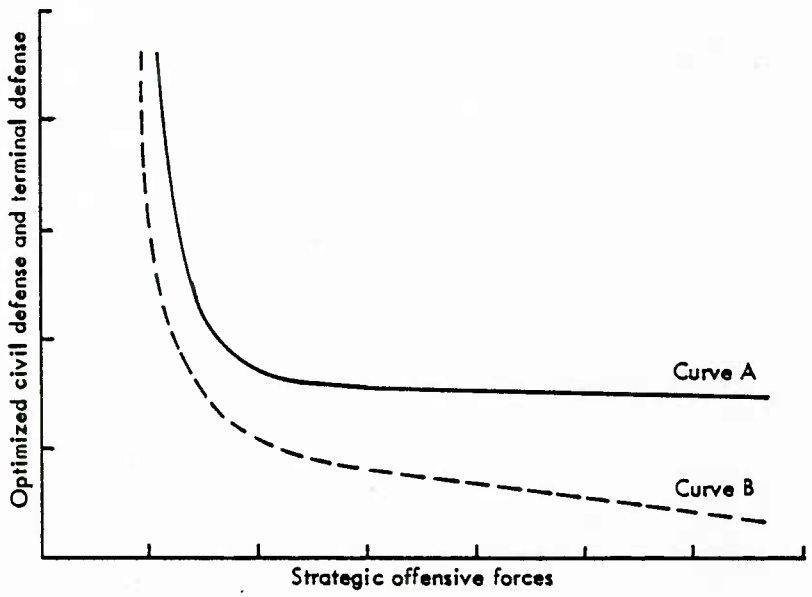

Fig. 2—Tradeoff curves for damage-limiting

our strategic offensive forces, designed to deal with time-urgent targets, would consist largely of missiles. The force would tend to be limited, since very large additional expenditures on offensive capabilities would not buy much in the way of damage limitation. This is indicated by the elbow in Curve A. On the other hand, the payoff to additional defensive forces would be moderately high, so that relatively modest outlays for defensive capabilities would be the equivalent of very large expenditures on offensive capabilities.

By contrast, Curve B indicates the tradeoff function on the assumption that there will be an indefinite period of city avoidance. The entire curve is shifted downward and to the left, indicating that limiting damage to a *given* level can be obtained with lower outlays on Packages I and II. Under these conditions, the returns for additional outlays on strategic offensive forces may be moderately high. By contrast, beyond a limited initial investment in defensive capabilities, very large additional outlays on such capabilities may buy comparatively little—and relatively modest outlays on offense will be the equivalent of very large outlays on defense.

Why does this sharp divergency exist? The answer is that defensive capabilities (aside from fallout shelters) perform a somewhat different and more limited function than do offensive capabilities. This difference in function is obscured in many existing studies, which examine optimization in terms of alternative war outcomes based upon potential damage at a single point in time, rather than provide a time-sequential analysis of the war that recognizes the possibility of a period of mutual restraint. To illustrate this difference in the nature of the two force-sets, let us turn to Fig. 3. As the arrows at the left of the figure suggest, both the offensive and defensive capabilities of the United States serve to reduce the Soviet *potential* for damage. But there is a difference which could prove to be very important. While both sets of forces from the very beginning do limit potential damage, *the United States could employ its strategic offensive forces immediately on the outbreak of war in order to alter the character of the Soviet threat.* The longer the period of restraint, the more extended is the *intra-war* opportunity to alter that threat. Moreover, the longer the period, the more options may be open to us to make such an attempt. By contrast, the defensive capabilities are, in a sense, "withheld." They perform their "active" role only in the relatively brief period required for a Soviet strike against cities. *A period of restraint does not result, therefore, in an expansion of the list of interesting defensive options.*

Thus, if restraint is preserved for a period, the actual employment of defensive capabilities to blunt the Soviet attack would occur only after what may be a considerable lag—during which the strategic offensive forces could be employed to perform their function of reducing the possible weight of the attack that the Soviets must ultimately decide whether or not to launch.[10]

There is a need, therefore, to consider carefully how long such a lag might last, and to include the best probability estimates among the parameters used to optimize force allocation. But this important factor is neglected in many studies, which more or less implicitly assume near-spasm war. The reason for this is that only in the context of a near-spasm war can the strategic offensive and strategic defensive forces be compared simply and without qualification. Once the possibility of restraint is introduced and the contrasting functions of the two categories of forces are underscored, the complexity of the calculations is increased many times. And this complexi-

[10] There is the added possibility, depending on the hardware characteristics and the command and control arrangements of the Soviet forces, that they could also be used to degrade Soviet targeting capabilities.

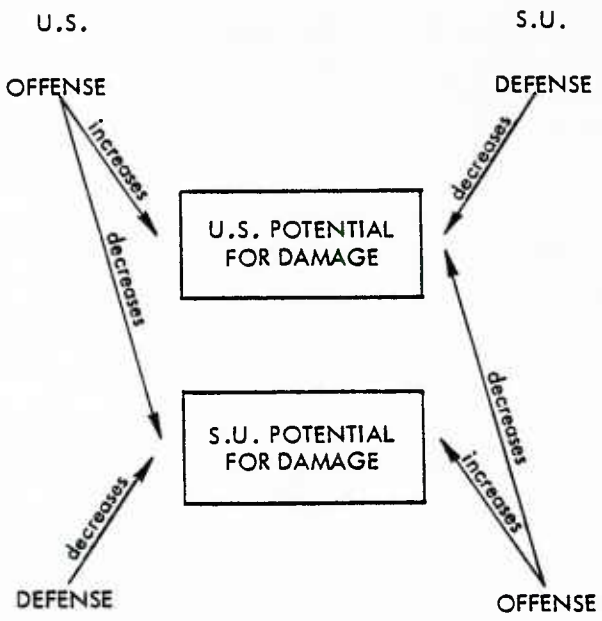

Fig. 3—Measuring war outcomes

ty occurs not only in the calculations of systems effectiveness. One must ask himself searching questions regarding what portion of the population he is willing to risk (for a limited but indeterminate time period) in order to provide greater capabilities for reducing long-run Soviet damage potential. To such a question there is no objective answer. Yet, if one avoids such questions, the result may be that the force structure is optimized for dealing with what may be the least likely type of central war.

To illustrate the way such calculations may influence the optimal force structure, let us consider three hypothetical systems—distinguished in accordance with the speed of effective reaction. System A (say, missiles) can react immediately to destroy Soviet damage-inflicting capabilities. System B (say, reconnaissance-strike capabilities) reacts more slowly, its maximum effectiveness occurring from twelve hours to two weeks after the outbreak of war. System C (say, ASW[11] capabilities) requires several weeks or even months to accomplish its mission. To limit damage in a near-spasm war, one would want to rely primarily on System A. But procurement would be relatively limited, with the balance of funds going into defensive capabilities because the *marginal cost* of killing additional Soviet offensive capabilities would rise rapidly for this system. However, if there is a lengthy period of restraint, Systems B and C may become attractive for reducing Soviet damage potential. If the period of restraint lasts for a week, for example, the marginal cost of destroying surviving Soviet land-based missiles through reconnaissance-strike capabilities may be relatively moderate—and this option *could* become interesting. If the war goes on for months, ASW capabilities designed to seek out and destroy

[11] Antisubmarine warfare—*Ed.*

Soviet missile-launching submarines might be very interesting. In a brief period, the marginal cost of destroying Soviet SLBMs[12] could be infinite. But over an extended period, it might be moderate enough to be highly attractive, especially when it is remembered that knocking out a submarine represents a bargain in terms of missiles destroyed. In an extended counterforce war, all of the enemy's capabilities can be made vulnerable.

The moral of this story is that where enough time is available, a slow-reacting system may be relatively cheap in terms of the *marginal cost* of destroying additional enemy capabilities. The high marginal cost of damage limitation through strategic offensive forces applicable in a near-spasm war may cease to be relevant if an extended period of restraint occurs.

As a simple example of these points, let us examine several situations, defined by the data in Table 1, in which the Soviet forces consist entirely of missiles. There are 800 missiles, of which 200 are elusive targets that can be discovered only after some time has passed. If the war is essentially over after an initial exchange, the use of strategic offensive forces to reduce enemy damage potential becomes too costly. The marginal cost of taking out enemy capabilities may be very high in relation to saving the lives of, say, a million people. Enough enemy missiles (350) survive, in any event, so that one should invest heavily in defensive capabilities. If, however, the war goes on for several months, one may be able, through the use of time-consuming offensive systems, to reduce enemy forces to the point that the surviving missiles (55) represent a much more modest threat to civil society. From the standpoint of damage limitation, the optimal mix in this case is skimpier defensive preparations and far heavier investment in strategic offensive forces—a solution that means little more than that the allocation of resources depends on the ratio in the final showdown between enemy vehicles and one's own lucrative—that is, civil—targets. If one cannot reduce this ratio substantially through extended counterforce operations, then the payoff of heavy outlays on defensive measures will be much greater than if one can.

Table 1

EFFECTS OF DIFFERENT ALLOCATIONS OF STRATEGIC FORCES

Type of Missile Attacked	Initial Force Size	Size after Initial Strike	Size after One Week	Size after Several Months
Targetable	600	150	40	25
Not initially targetable	200	200	200	30
Total	800	350	240	55

To demonstrate that for optimal resource allocation we are interested in the ratio, in the final showdown, between enemy vehicles and one's own civil targets, let us turn to Fig. 4. The figure indicates (1) that the measure in which we are ultimately interested in studies of damage limitation is the Soviet potential for damage, and (2) that both offensive and defensive capabilities affect this variable, the offense by directly reducing Soviet offensive capabilities, the defense by blunting the effects of an attack, so that what is finally filtered through is Soviet potential for

[12] Submarine-launched ballistic missiles—*Ed.*

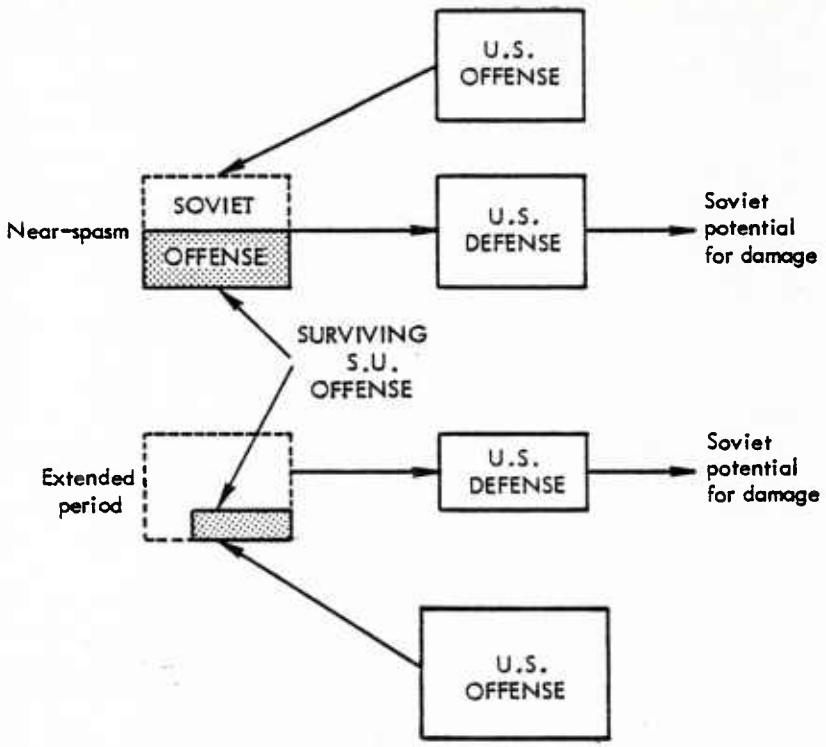

Fig. 4—Variation in optimal resource allocation depending on assumed character of war

damage. The questions are: How much to U.S. offense, how much to defense? In the near-spasm context, the United States may be unable to make a very substantial dent in Soviet offense, and the optimal strategy would be to invest heavily in defense. Where, on the other hand, there is an extended period, U.S. offense, through a variety of measures, may reduce Soviet offense to a very low level—so low that much less should be allocated to defense to blunt the now much-reduced Soviet attack. The size of the boxes in the figure indicates that in the second case an entirely different allocation may be appropriate for the United States—one in which much more is invested in offense and far less in defense.

It should be acknowledged that the studies of damage limitation in the near-spasm context are unquestionably useful in providing an *initial* basis for analysis. From them one can speculate on the sequence of interactions as each side responds to the perceived outcomes by altering its intentions and capabilities. However, from the foregoing discussion, we can conclude that raising the issue of complementarities between missions enormously complicates analytical work and raises questions regarding the confidence that we can place in the results. To design an analysis which points to a single and unequivocal set of conclusions regarding strategic-forces resource allocation is well-nigh impossible. The point that should be remembered is that the ultimate decision regarding resource allocation must rest on nonquantifiable or subjectively quantifiable elements and that it cannot rest solely on presumably quantifiable technical data. Thus, an essentially unknowable parameter becomes critical in determining the ultimate results.

Constraining Future Strategic Options by an Early-on Force-Structure Decision

Let us turn now to our second illustration, which involves a problem in choice with which the Department of Defense is continuously struggling, that is, determining the composition of U.S. missile forces. The purpose of this example is to underscore the desirability of maintaining flexibility in planning in order to cope with a gradually unfolding threat environment whose precise future character cannot be predicted. It was suggested earlier that the way to retain flexibility in such an environment is through an aggressive R&D program designed to develop multiple options and through avoidance of force-structure decisions until such decisions are forced upon us by the flow of events. The present example stresses the advisability of delaying major force-structure commitments until long-lead-time elements force a decision. The case for such a decisionmaking pattern is quite strong *when one has moderate confidence that delay will permit the resolution of major uncertainties regarding the future strategic and technological environment.* This kind of flexibility may be contrasted with the premature foreclosing of strategic options implicit in commitments which are made at too early a date.

To indicate the advisability of such a decisionmaking pattern, let us examine the particular set of decisions, made early in 1961, which determined the character of our missile forces. Needless to say, the purpose in going back to these earlier decisions is not to indulge in some pointless second-guessing, but to learn what we can for the future.

The decisions made in March 1961 were advertised as part of a "quick and dirty look" at the force-structure program inherited from the Eisenhower administration. Major conclusions were to expand greatly the projected Minuteman (and Polaris) force and to reduce the size of, and the emphasis upon, the Titan II force. The effect of these decisions was to determine that the intermediate-run U.S. forces would be overwhelmingly composed of small-payload missiles. Two background aspects of these decisions should be kept in mind. First, they were made before the new intelligence then becoming available had been fully absorbed. While fears of a major "missile gap" were being dissipated, we were still unaware how great our strategic superiority was. Consequently, substantial emphasis remained on a quick buildup of a second-strike capability. Second, there existed certain political pressures for making changes (particularly in light of the preceding campaign) that would dramatize and highlight the shift away from the policies of the preceding administration. The atmosphere was one calling for decisiveness in a period of presumed crisis, as is perhaps suggested by the phrase "quick and dirty look." In any historically fair assessment of these decisions, these considerations must be kept in mind.

Nevertheless, we should ask ourselves a question: What can we as analysts learn in retrospect from these decisions? It now is clear that major difficulties existed in formulating the early-on cost-effectiveness studies which served as their basis. On the one hand, there were major uncertainties regarding the size and character of prospective Soviet forces and also regarding the strategic concept that we would adopt. In addition, major technical uncertainties regarding both Minuteman and Titan II remained unresolved. As a consequence of these deficiencies in information, it was inevitable that only the crudest observations could be made regarding the effectiveness component of the decisionmaking schema. Cost considerations, therefore, became dominant. Yet, even here, because of the unresolved technical problems, not much confidence could be placed in the cost calculations. As it turned out, these calculations were strongly biased against Titan II because of the drastic underestimation of missile operations and maintenance costs. This favored the missile with the lower initial capital cost, that is, Minuteman.

The upshot was that, as a result of calculations based mainly on cost, decisions were made, in effect, against large-payload missiles and for small-payload missiles. At the time, there was, to be sure, a developing emphasis on the desirability of avoiding collateral damage through the use of weapons of lower yield (which may have been associated with a stress on small-payload vehicles), but it seems fair to say that cost was the main consideration pushing in the direction of a force composed of small-payload missiles. Had it been necessary to make that decision at that time, of course, it would have been equally necessary to have based it on whatever information was then available. But we can say with reasonable confidence that there was then no compelling reason why it had to be made. It could have been delayed, and—in retrospect—we can see several still unresolved strategic and technical issues which suggest why it should have been delayed. Let us examine six of these issues.

Counterforce Strategy and the Prospective Hardening of Soviet Missiles. Perhaps the most important has been the elaboration of the counterforce strategy in its controlled response variant, with its emphasis on initial targeting of military targets exclusively and the attempt to avoid major damage to the fabric of Soviet civil society. In addition, although Soviet ICBMs were then soft, there was the prospect, later emphasized by Secretary McNamara in Congressional testimony, that in the future their ICBMs would be hardened. The extent of hardening and, a fortiori, the degree of hardness could not be anticipated with any precision. Particularly as the Soviets hardened their missiles, the United States might require higher-yield weapons to destroy Soviet capabilities. The degree to which our own CEPs[13] could be lowered was unknown, and consequently, higher-yield weapons might be needed to substitute for targeting inaccuracies. Moreover, if the Soviets failed to press the development and procurement of missile-armed submarines, the pressure upon us to avoid the use of high-yield weapons out of concern for collateral damage would be much reduced, because we still might be able to achieve a major disarming blow. In light of the still unknown parameters, the prospective Soviet moves toward hardening should have led us to emphasize the large-payload hedge rather than commit ourselves too early to a force composed largely of small-payload missiles.

Possible Soviet ABM Deployment. Since we were aware of the possibility that the Soviets might deploy an ABM system, the implications of such an eventuality for a U.S. missile force composed primarily of small-payload missiles might well have been considered. Reentry vehicles with small-yield weapons appear to be particularly vulnerable to ABM systems. Large-payload vehicles represented a hedge against Soviet ABM deployment in that the reentry body could be toughened up, much higher-yield weapons could be employed, and a wide assortment of penetration aids could be packed into the vehicle.[14] In short, the commitment to a force composed primarily of small-payload missiles should be regarded as an inadequate hedge against the possibility of the Soviet ABM system.

Test Moratorium and Test Ban. In early 1961, it could not be assumed with any confidence that the Soviets would break the test moratorium during the summer of 1961, thereby permitting our own test series and possible improvements in yield-to-weight ratios. In subsequent reviews, the possibility of the test ban treaty, which materialized in the summer of 1963, should have been kept in mind. The treaty now inhibits our ability to reduce the size and weight of warheads in Minuteman or to increase the yield with a warhead of given size. With given weight and size constraints in the reentry vehicle, a reduced ability to vary the physical size of the

[13] Circular error probable, a measure of accuracy—*Ed.*

[14] Cf. *The New York Times*, June 20, 1966, pp. 1, 9.

weapon implies a lessened possibility of doing such things as toughening up the reentry vehicle or packing in additional penetration aids.

Limitation on Numbers. Limiting the number of missiles available to both sides has been discussed at Geneva. Moreover, even in the absence of a formal agreement, some implicit bargaining has taken place between the two camps with the intention of holding numbers down. If numbers are held down, for whatever reason, much larger capabilities are provided by a force composed of large-payload as opposed to small-payload missiles. The large-payload vehicle represents a hedge to offset the effects of the likely inclination to hold numbers down.

Multiple Warhead Options. For quite obvious reasons, the possibility of a missile carrying multiple warheads, each individually delivered, very much increases the utility of a large-payload vehicle.[15]

Questionable Systems Reliability. The fact that the technical characteristics of missile systems and subsystems were still unknown in 1961 points to one final possible advantage of the large-payload vehicle. In such a vehicle, if reliability problems were encountered, subsystems such as guidance packages could have been placed in parallel, thereby reducing the risk of unreliability—and possibly economizing on operation and maintenance costs.

The ultimate influence of any one of these considerations could have been such as to make it advisable to press forward with large-payload vehicles; yet decisions made earlier, largely on the basis of cost considerations which were crude in themselves, had already inclined the United States in the direction of small-payload missiles. The entire episode illustrates the need for flexibility, for hedging, and yet for *timely* decisions respecting force structure. It also demonstrates what should be obvious, how cost-effectiveness studies conducted at one point in time may become irrelevant as strategic objectives and circumstances change. The point here, however, is that the decision did not have to be made so soon. When the external environment permits delay, and when one has moderate confidence that major uncertainties will be largely resolved, delaying the decision is likely to be the wisest course of action.

These conclusions seem inescapable, but they still leave room for the observation that, for strategic purposes,[16] the Minuteman decision has not worked out too badly. This is due particularly to the slow buildup, hardening, and dispersal of Soviet forces and to the slow advance on ABM systems. The favorable resolution of technical problems and the brief resumption of testing by the United States have been helpful. But the point is, if we have been right, we have been right because of developments that could not have been predicted with high confidence. Whatever analysts may say, it is undoubtedly better to have been right for invalid reasons than wrong for the right reasons. Yet, since we cannot count on such good fortune in all cases, we are well advised to see what we can learn from earlier experiences. One lesson this example demonstrates is the desirability of maintaining options and of putting off critical force-structure decisions until forced to make them by long-lead-

[15] Cf. *The New York Times*, June 20, 1966, p. 9.

[16] With respect to the cost-effectiveness of the investment, on the other hand, the decision may turn out far less favorably than anticipated. In a hardened and dispersed missile system with sophisticated command and control, the ground environment rather than the missiles themselves represents the main cost. In moving toward more advanced missiles, we most certainly would desire to take advantage of such costs, and have the existing silos service several generations of missiles. But the existing silos were designed for the relatively small Minuteman. This implies that, if the United States moves toward larger-payload missiles, we shall be facing the tough—and expensive—compatibility problems involved in squeezing larger vehicles into the existing space-limited configuration. Substantial retrofitting costs over time make the original decision more questionable—though, admittedly, this is easier to see in hindsight.

time items. Another is that the dominant role of uncertainties in this example (like the role of assumptions regarding highly subjective parameters in the previous examples) indicates that undue expectations regarding precision in systems studies are likely to be self-defeating.

SOME FINAL INFERENCES

The purpose of this concluding section is to draw some inferences from the discussion that may prove helpful in future analytical work. We can group these inferences under six headings.

Uncertainties: State of the World, Objectives, and Strategies

Inevitably the first issue to be raised is that which normally falls under the heading "uncertainty." Three points in particular should be emphasized. First, in a number of important ways the environment has indeed become more fluid, more uncertain. This reflects the perceived reduction in the probability of all-out nuclear war and the depolarization of international politics. Second, we are now more aware of "uncertainties" that have always existed in the environment, but which earlier perceptions of the state of the world precluded our recognizing. Third, we should now be more willing to acknowledge the certainty of change—with its corollary that only the absence of surprises would be surprising.

Heightened awareness of continuous change and the uncertainties it entails is reflected in our perceptions of the nature of opponents, the character of nuclear war and how it may be initiated, and the future array of military capabilities and the degree of coherence attainable within the array. But these uncertainties regarding the environment, objectives, and strategy vastly complicate the decisionmaking schema. One view of the decision process, taken from formal decision theory, has attracted some attention among systems analysts. The procedure is to assign subjective probabilities to possible states of the world, array them accordingly along one axis, examine strategies or alternative lines of action along another axis, and then make a choice among them by means of some decision rule.

This is a neat and intellectually elegant way to structure the problem, but, if improperly understood, may create more problems than it solves. While a useful first approximation, such a model is inadequate in at least two respects. First, the assignment of probabilities to perceived possible states of the world will inevitably be misleading, because the chances are very great that the state of the world that does materialize will be one which was not perceived in advance. Second, both the optimal strategy and the strategy finally chosen are likely to be different from those which were arrayed in the payoff matrix. In assessing decision theory, it is important that we keep in mind the distinction that exists between *risk* and *uncertainty*. For risk, anticipation is possible and appropriate calculations, even if subjective, can be made. By contrast, how uncertainty will be resolved is impossible to foresee, and its existence will partially destroy the relevance of all advance calculations.

Disparate Approaches to Analysis

This matter of uncertainty raises the question of how to approach analysis. In the past, both within and without Rand, there have been two disparate points of

view. A first group, whom we might call the contingency *planners*,[17] has felt some confidence in our ability to chart in advance successful policies for the unknown future. Their method has been to designate the probable states of the world and to design a system which can deal adequately with each of them. A second group, whom we might describe as *contingency* planners,[18] has tended to emphasize the uncertainties and our limited ability to predict the future. Those who hold this view have consequently stressed the need for sequential decisionmaking, for improvisation, for hedging, and for adaptability. Heightened awareness of inevitable change should tend to make us more sympathetic to the latter approach.

Developing and Selecting U.S. Military Capabilities

We face a wide spectrum of threats, but we cannot tell which, if any, will actually materialize. Given the cost and specialization of our own weapon systems, we cannot afford to procure all the systems necessary to deal with such a multitude of threats. We may actually buy fewer systems, each of which is designed to deal with a relatively limited threat. For central war purposes the effectiveness of any given U.S. system depends on what is in the Soviet force structure. Many major force-structure decisions will have to be delayed until we have clear evidence of the direction in which the Soviets will proceed. On the other hand, although specialized weapon systems deal with narrow threats, we must protect ourselves against a broad range of threats. The chief way to do this is through a wide-ranging, austerely-conducted R&D program, in which it is fully recognized that many successful developments will not lead to procurement.

Although the chief way of building flexibility into the future force structure should be an R&D program which provides a rich menu, we ought not to neglect the possibility of building flexibility into individual systems. In a continuously changing strategic environment, it must be kept in mind that the choice of a weapon system does not simply optimize—it also constrains the choice of future strategy. The adaptation of strategy should not be unduly limited by the selection of weapon systems through the choice of criteria which inherently reflect a single set of strategic conditions.

The Problem of Cherished Beliefs

A determination to stress adaptability and the avoidance of premature commitments in the future implies that we must be on our guard against the cherished beliefs that are carried forward from previous conditions and previous battles. I am aware that this admonition represents a counsel of perfection, one that sounds naive when extended toward frail human nature as it must perform in a bureaucratic environment. I am convinced, however, that counsels of perfection are necessary in providing warning flags for the kind of error into which we fall through seduction rather than through bungling.

Let me illustrate this matter by referring once again to decision theory and to one of its basic tenets. In principle, our choice of force structure and strategy should be dependent upon, and subsequent to, our estimates of the probabilities pertaining to various states of the world. In practice, however, this is rarely the case. Partly,

[17] That is, those who believe that the array and character of future contingencies can be specified in advance, and that *detailed advance* planning can be done to deal with whichever one does occur.

[18] That is, those who believe that future developments will have a large element of the unforeseen, that contingencies cannot be specified precisely in advance, and that *whatever planning one does must be done so that it may be adapted to the contingent and the unforeseen.*

this may be ascribed to the long lead-time associated with the purchase and deployment of weapon systems—which implies that our strategic choices must be made well in advance of any hard knowledge about the state of the world. Much more important, however, in imposing obstacles to logical choices, are bureaucratic pressures and the fact that in most human beings there is a proclivity to decide a question more or less on the basis of intuition, and then to adjust one's assessment of the state of the world accordingly. Rather than the state of the world determining one's strategy, as in the model, the assessment of the state of nature is not even arrived at independently, but all too frequently is merely a reflection of strategic choice.

This is how analysis is distorted by—or, more properly, is made to reflect—preconceptions. If we are to achieve true adaptability and suppress cherished but obsolete beliefs, we shall all have to try both individually and organizationally to control such tendencies.

Complementarities and Mission Tradeoffs

Recognition of complementarities among major missions means that the problems of overall system design have become increasingly intricate and that more attention must be paid to them. More emphasis must be placed on force integration; less attention can be concentrated on the individual weapon system. As a result, the opportunity for traditional systems analysis—in the sense of analysis to assist a simple choice between several given systems for accomplishing a single objective—has diminished. This shift implies that in analytical work, choice becomes more dependent upon parameters which are only implicitly or subjectively quantifiable—and which may even be unknowable. Under these circumstances, it is probably preferable to have an acknowledged imprecision in systems studies rather than a spurious precision.

Quantitative Precision

Finally, we might observe that the imprecision in analytical results, at least in the quantitative sense, stems not only from the growing role of mission tradeoff studies which rest on parameters at best only subjectively quantifiable, but also from the passing sharpness of the distinction between capabilities for central war and limited confrontations, from the absence of dominant threats, and from the general growth of uncertainty. But this may imply that in military systems we are passing through a great transition. Previously, there may have been an overemphasis on intuition, but now that the battle for the recognition of quantitative studies has been won, the current problem may be an overemphasis on those objects of analysis that can be readily quantified. The stress on the quantitative element is not obsolete, but now that its importance has been recognized, we should be increasingly aware that it does not represent the whole story. We should be more inclined, perhaps, to recognize the element of art in systems analysis—and to stress what the best practitioners have always known: that judgment and intuition (in handling quantitative considerations, to be sure) are the critical inputs.

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ORGANIZATIONAL STRUCTURES AND PLANNING

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The author is indebted to B. H. Klein and R. N. McKean for their contributions to a number of the major themes in the paper. In addition, helpful criticisms of an earlier draft were supplied by G. R. Hall, V. Taylor, and C. Wolf, Jr.

The purpose of this paper is to consider the influence that organizational structure has upon planning. In particular, it is intended to examine the type of structure most conducive to successful planning—given variation in the size of organization, the speed of change, and the uncertainty regarding means and ends. Stress will be placed on structures and planning in large organizations, with the underlying premise that problems in this context differ in important respects from those applying in small organizations.

Large organizations suffer from a geometric increase in the difficulty of (a) successfully communicating intentions and procedures, (b) establishing an harmonious system of incentives, and (c) achieving adequate cohesion among numerous individuals and subunits with sharply conflicting wills. Herein lies a partial explanation of why large organizations are given to control by doctrines, which impress the outsider as rigid and arbitrary and which inevitably grow stale before being abandoned.

In large organizations it is hard to preserve channels for and open-mindedness toward dissent, for organizational distance permits keeping dissenters sufficiently far away that only a garbled version of their message is heard. Yet, large organizations may be forced to coexist with dissent in its most crippling form, for their size permits the preservation of pockets of bureaucratic resistance, which may frustrate organizational purposes through overt or tacit noncooperation. This risk of organizational stalemate is especially relevant in relation to those large governmental organizations with massive requirements for trained personnel[1] and legal restrictions on expediting desired personnel turnover.

Discussion of improving large organizations is frequently marred by the concealed assumption that aside from scale their differences from small organizations are trivial. In planning, the procedures and attitudes that an individual or a small, closely-knit group might establish are unconsciously accepted as the appropriate standard. This instinctively anthropomorphic treatment—in which the large organization appears as simply the (presumably) rational individual or small group writ large—is perhaps the most eminent of the intellectualist fallacies that appear in the debates on organizational structure.

It leads almost automatically to advocacy of flexibility and open-mindedness and to extolling the virtues of sequential decisionmaking with its attributes of avoiding commitments to specific goals, willingness to abandon obsolete plans, and preservation of multiple options—without due appreciation of the constraints that organizational life per se places on the use of such maneuvers. Nonetheless, the constraints

[1] While it is conceivable that a staff adequate to perform postal services could be gathered in months or even weeks, it is staggering to think of the problem that would be posed in replacing all the personnel of the Air Force or Navy.

are impressive and the room for maneuver may be relatively small. The problem is really one of discerning and achieving optimal organizational adaptability by weighing the costs against the gains of flexibility. This would never be easy, but the problem is made even more intractable because of the variation over time of optimal adaptability in response to internal dissension and external pressures.

Though the issues of organizational structure and planning represented the focus of the older-style political economy, modern economists, in deference to the prevailing penchant for precision, have tended to shy away from the problem. It is too complex—and consequently is ill-adapted to treatment by model-building. But there are other reasons why talking about planning and structure is hard, and ironically one of them is that it is so easy.

A good many of us carry over the emotional sets from the more or less ideological disputes of the '30s and '40s. The attitudes are only partially buried, and the instinct to return to the old simplicities is strong. Consequently, words like "centralization" and "decentralization " can elicit a strong reaction—and a vigorous discussion of first principles. I would not deny that such discussion can provide guidelines with respect to long-run objectives, but it cannot shed much light on short-term organizational structure. Attacking current organizational decisions by reference to slogans regarding long-term objectives may provide the basis for a rousing argument. It will hardly be analytically profitable.

In assessing modifications of organizational structures, the issues are to a large extent ad hoc. It is natural to view organizational change in terms of the small-end-of-the-wedge-establishing-trends, but in organizational life, more than in politics, the appropriate image is that of a pendulum swinging to correct existing ills. No organization is quite right, and, without prodding, organizations will grow typically less sound over time. To avoid growing stale, any organizational structure needs an occasional shaking-up or breath of fresh air. Organizational structures are the right ones only for a specific set of problems and for a specific distribution of talent. Under different circumstances a variety of organizational forms will be most suitable.

As problems and personnel change, organizational forms should be altered—and such alterations ought not be blocked by neuroses concerning the establishing of long-term trends. It is true, for example, that qualitatively the costs and the benefits of tightly controlled, centralized organizations do not change much. Nonetheless, the weights assigned to these costs and benefits undergo constant alteration, so that on occasion movement toward tighter control will become appropriate. More generally, changing conditions will make major alterations desirable—even aside from the stimulative effect of a shake-up.

This paper examines some implications for planning and organizational structure of the experience in the Department of Defense since 1961. This is not intended primarily as a detailed analysis of the problems of defense organization, but rather it is intended to provide some generalizations and to raise some questions for a broader audience of professionally trained people. The broad objectives of defense reorganization, which prominently featured increased control by the Office of the Secretary of Defense, are widely understood. One objective was to achieve better coordination of interrelated decisions than that which "bargaining" among the Services could provide. The other main objective was to improve choices in general: (a) by looking at full costs rather than down-payment implications of alternative policies, (b) by costing in terms of programs or "outputs" rather than inputs, and (c) by systematically considering alternatives and tradeoffs in terms of cost-effectiveness.

For these objectives there has understandably been widespread sympathy and support. However, there have also been certain side-effects, which have not been

given the same attention, and these have major importance for those interested in the efficient and successful employment of resources within organizations. Our purpose is to point up some of these neglected considerations and to see what lessons can be gleaned from this experience.

Since my purpose is, in part, to raise questions, and since some of my comments will be in a critical vein, it is proper to make my position clear: by underscoring my wholehearted, if not unique endorsement of the view that Robert McNamara has been one of the nation's great public servants. Any proper criticisms must start from this basis. The reason that we are in a good position to analyze and to criticize is that McNamara has provided new substance. His aspirations were high. Much has been achieved, even more has been attempted. In handling defense issues, the United States has broken out of the prior mold in which rigid preplanning oscillated with impulsive ad hoc changes in programs.[2] This is an impressive accomplishment. Nonetheless, complete success always remains elusive, so it should not be surprising that there are loose ends and bits of debris that remain to be straightened out.

SOME PERSPECTIVES ON PLANNING

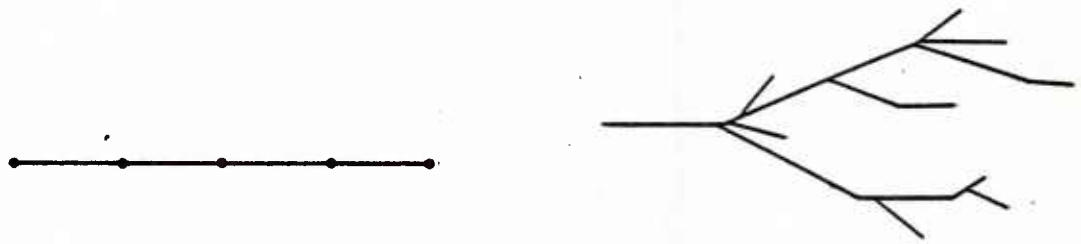
At base, planning implies the programmed commitment of inputs over time to provide instrumentalities (in a sense, higher-level inputs) which can handle anticipatable future demands. Planning procedures and structures, however, should be adapted to the degree of confidence that can be attached to one's forecast of the future. The higher the degree of confidence, the more explicit can advanced planning be, and the less need be the concern in committing large organizations to specific types of activities. Uncertainties always exist with respect to the investment of inputs and the creation of instrumentalities. These uncertainties are especially important in R&D, where not only costs but the ultimate achievements are in question. Additional uncertainties exist regarding whether the planned-on instrumentalities can adequately handle the anticipated demands and, indeed, whether one can accurately forecast what such demands will be.

In defense, the latter problem is especially frustrating, for national objectives are continually changing, partially reflecting internal alterations of preferences and partially the behavior of rivals on the international scene. Here, the "uncertainty" is misleading, for it is not so much uncertainty as the certainty that objectives will change over time, and that some years hence they will look considerably different from those perceived at the start of the planning period. As we shall see, such considerations should have a major bearing on the planning process.

[2] This is not intended as a denigration of McNamara's predecessors. The Eisenhower administration, I would argue, has been given scant justice in its treatment by the intellectual community—especially in questions of defense management. It was concerned with overlapping efforts and duplication, and grappled with the problems of improving control (McNamara's powers, after all, were instituted under the Defense Reorganization Act of 1958). Eisenhower himself had experienced and was scornful of the demands generated through military planning procedures (see his acid reference to "the so-called military requirements" and the need for procedures to bring them into bounds, Emmet Hughes, *The Ordeal of Power*, p. 75).

Nonetheless, defense programming continued to be handled in the traditional way with its rigid and unsatisfiable "requirements" based on simplistic quantitative impulses. Though deeply concerned, the administration was never able to exploit analytical tools in dealing with the problem. This was Eisenhower's failure in defense, rather than the issues that bothered the public and the intelligentsia. On those questions Eisenhower has turned out to be right more frequently than his critics—even on the limited set of issues that were real problems rather than caricatures. It remains an historical irony that an administration, which had presided over the build-up of America's decisive military edge, should stand condemned at its close for "neglecting the national defenses."

Very roughly, one can distinguish between two general approaches to planning. Cook's-Tour planning rests, implicitly or explicitly, on the supposition that the future is sufficiently certain that we can chart a straight course years in advance. In it, direction, speed, size of commitment, and achievement milestones (not decision points) are indicated with, at least, rough precision. By contrast, what may be termed Lewis-and-Clark planning acknowledges that many alternative courses of action and forks in the road will appear, but their precise character and timing cannot be anticipated. Neither the size of commitment nor even the direction of movement should be stipulated too far in advance. At the end of a period one can retrospectively examine the paths pursued, which include many abandoned initiatives or experiments and many hard (and possibly erroneous) choices. Only limited confidence could have been placed in advanced predictions regarding which options would be chosen, when the choices would be made, or how long alternative courses of action would be pursued before abandonment. Retrospectively one may map (as in the diagram) what has taken place, but the planning function is not to chart a precise course of action. Rather it is to prepare to cope with the uncertain terrain of the future, to note the signs in the environment that a decision point has been reached, and to respond in a timely fashion.



Cook's-Tour planning

Lewis-and-Clark planning

Wherever uncertainties are substantial the balance should shift in the direction of Lewis-and-Clark planning. Despite its messiness, its relative advantage then increases. The appropriate planning concept is one that is conducive to (1) facing uncertainties (not pushing them aside) and (2) hedging against uncertainties (i.e., not biased against hedging). Nevertheless, in all bureaucracies there are strong pressures to go too far in the quest for Cook's-Tour planning. In part, this is inevitable in large organizations as a concomitant to the need for cohesion and the cost of communications. In part, the pressure is understandable since it may permit committing others to our view of the world, our objectives, and our strategies.[3] In part, it is a form of casual laziness. Characteristically the tendency towards precise planning goes much too far, and appropriately these pressures will be resisted by insightful decisionmakers.

The cost of acquiescence is neglect of uncertainties, lost flexibility, neglected and suppressed options, and less-than-optimal adjustment to changing opportunities and threats existing in the external environment. In evaluating planning procedures, one must guard against exaggerating the extent to which the future can be foretold

[3] At the highest level, leaders have a natural inclination to limit the ability of their successors radically to alter the policies they will inherit. However, at a point in time the strongest pressures for commitment normally come from below, particularly just below the top, where officials are close enough to have a keen interest in high policy and a keen sense of their own limited ability to control decisions. The decisionmakers may be even less sympathetic in the future, so why help provide the hedges, options, and elbow room, which make it easier for them to change their minds? At the highest level there may be little sympathy with this motivation, but it is impossible to eradicate it.

and planning for it precisely charted. Modifications, which permit greater adaptability in the face of change, should be introduced. Planning should be based on an *accurate* view of the future. Where an accurate projection implies recognition of the unpredictable and the unknowable, this should be taken into account.

How should organizations decide on a general planning approach? Aside from the intelligence, reasonableness, and perceptiveness of personnel,[4] three major factors influence an organization's ability successfully to plan in a precise and monolithic manner. These are (1) the size and internal structure of the organization, (2) the dimensions of the planning problem and the stability of functional relationships, and (3) the existence and responsiveness of rivals and the organization's ability to anticipate or to perceive such responses and to make the necessary adjustments.

Examination of these factors will reveal why the national security realm is inherently less tractable for planning purposes than certain other functions like highway planning or the provision of postal services. Nonetheless, planning for defense is unavoidable, and, despite the difficulties, greater success has been achieved in defense than in some other areas which appear better to lend themselves to planning. However, inherent difficulties are very great. The final measure of success is to stumble fewer times and in less important ways than one's national rivals. In evaluating recent defense planning one should keep in mind therefore that a .400 batting average is a very impressive achievement.

Organizational Size

The problem of the size and internal structuring of organizations has already been raised. Achieving cohesive action among large aggregations of people is itself no mean accomplishment. Communications are very costly, and are not certain to be successful. Directions must be simple, and this simplicity, at a higher level, will be reflected in organizational doctrine that strikes outsiders as appallingly unsophisticated and lacking appreciation of the nuances. Change consequently can only be introduced slowly. Without careful preparation the attempted change will simply introduce chaos. In light of the problem of successful communication, even a carefully prepared change must be followed by a shakedown in which the bugs are eliminated. In operating commands, flexibility at any one time can involve only several agreed-on options. Discipline is at a premium. Individuality must be suppressed. The functionaries are operators, not seminar students. The Strategic Air Command, to take one example, simply cannot tolerate the personal iconoclasm, frequently irresponsible, which has so high a payoff in a university or a research organization.

In moving towards smaller, less operations-oriented organizations, the constraints become less onerous. Partial communication of divergent ideas becomes increasingly feasible, though complete understanding inevitably remains elusive. For relatively small groups the sharply reduced cost of communication permits a relatively full exchange of concepts without necessarily absorbing all energies in

[4] Since all organizations recognize that, in principle, it is better to have more competent rather than less competent personnel, and since effective organizations normally do strive to acquire competent talent, it would appear that nothing more need be said on this subject. Unfortunately, what is so readily endorsed in principle (as indicated by the omnipresent bromides regarding "quality" and "the best talent" in thriving organizations) is about the most difficult thing to achieve in real-world behavior. In all organizations the measures of "competency" tends to become, to a greater or lesser degree, adherence to the "party line." In addition, there is the delicate problem of the personality structure of leading administrators or recruiters, with the selective bias that this introduces in recruitment and advancement. Reflecting the psychological tendencies of leading figures, organizations will tend to acquire specific personality types with particular intellectual bents—and will mold other recruitees into the desired pattern. I will say no more on this subject, despite its importance, other than to recommend to the reader Harold Lasswell's insightful study, *Power and Personality* (Compass Books edition, 1962).

communications and leaving none for operations. Small groups *can* adjust quickly (though they need not necessarily do so). Only within relatively small groups is there much opportunity for real flexibility. Small groups can change plans, can avoid advance commitment, can *easily* maintain options until decision points are reached. However, once again there is no assurance that they will do so, especially when defending bureaucratic interests.

Whatever the internal flexibility, a subordinate smaller organization can find good reasons to attempt to get the parent organization to commit itself to the options it favors. The impulse to tie down outsiders, while retaining internal freedom, is understandable. Nevertheless, it is crucial to recognize that *only* in small groups can there be free internal communications with preservation of most energies for work on substantive problems. Substantive work outside of the preestablished mold can be efficiently accomplished only in small groups.[5] Use of small working groups is, of course, no guarantee of productive work. Yet for the set of production groups, the yield of substantive new work will decline as the group grows significantly beyond some critical mass.

Herein lies the case for major devolution of responsibility whenever the promise of change and the existence of uncertainties makes it inappropriate to indulge in precise planning, Cook's-Tour manner. Major devolution of responsibility to small or lower-level groups should be utilized for that work which, especially in the long run but even in the short, does not involve significant spillovers for other components of the organization. In defense, this includes the bulk of operational assignments, most analytical and developmental efforts, and even some specific decisions on weapon systems. Delegation of responsibility is not easy to practice at the apex of an organization. The natural inclination is to want to keep control of everything. Nevertheless, one indicator of success in large organizations is the willingness to delegate relatively far down the pyramid where compact work groups can be found. U.S. experience suggests that this is most crucial in the development area.

Massive organizations with extensive communications at high, medium, and low levels will run up costs staggeringly. With given budgets, high costs mean less output. Reduced cost through devolution of responsibility could improve the payoff from R&D manyfold. Within the Department of Defense, such a structure has not had enormous appeal. Yet, to achieve high efficiency, very large organizations must exploit the special advantages that small groups can provide. In the DoD one route to further improvement will be a willingness to abandon detailed control in the cases where it is costly and to sift out its problems, delegating responsibility more freely in the many cases in which it is feasible.

Dimensions and Interrelationships

The less well known the future terrain, the greater the losses in planning by simple Cook's-Tour methods. To deal with a not wholly ascertainable future, one wishes to introduce planning options and hedges. Under some circumstances simple hedging is possible without doing much violence to the spirit of Cook's-Tour planning. The critical factor is the number of dimensions to the problem. When one has high confidence that the important variable is one-dimensional and quantitative, relatively simple hedging becomes feasible. Consider highway construction as an example. Though we are not certain, we have high confidence that transportation in the future will make considerable use of some kind of ground-based automotive

[5] A classic statement of the proposition, not generally known to economists, may be found in R. B. Kershner's "The Size of Research and Engineering Teams," *IRE Transactions on Engineering Management*, Vol. 5, No. 2, June 1958, pp. 35-38.

vehicle requiring roads. We are uncertain regarding numbers of vehicles and relative usage. We do know that a major cost in expanding existing highways is the tearing down and rebuilding of overpasses that constrain traffic flow. A simple, if underutilized, hedge against expanded traffic flow is to make the original overpass sufficiently wide that additional lanes of traffic can be provided without demolition. Such a hedge adds measurably to initial construction costs, but provides insurance against the much higher cost of rebuilding the overpass much sooner than contemplated in the original plans. An option is introduced, which may or may not be taken up. There is an implicit decision point, though not precisely timed.

Such one-dimensional hedging against quantitative change is normally available only for low-order problems, which are mere subelements in the comprehensive plan of a large organization. Whenever sharp qualitative changes may be in the offing, thus destroying the problems' unidimensional quantitative nature, hedging becomes far harder. And, as one adds to the dimensions of the plan by increasing the number of issues covered, planning ceases to bear any relation to a prescription of activities that will be undertaken in the future. Instead planning appropriately becomes a vast hedge, indicating the character, the means of acquisition, and the use of certain instrumentalities—if certain sets of circumstances should materialize. Rather than providing an exact prescription of activities, a good plan will admittedly provide no more than the roughest guidelines.

To provide a prescription of activities when there are numerous dimensions, one would have to know how an ever-changing environment will influence the relative values of the several activities. What weights will be assigned to different activities and how will they influence the distribution of effort? What are the tradeoffs, as they will be seen in the future, among these various categories of activities? (The tradeoff functions undergo continual change in response to the changing valuation of activities.) Thus, if plans for future activities are adhered to, the results inevitably will be less than optimal.

We are not clairvoyant. Prescription of future activities requires us to have more knowledge of the future than we possibly can. In multidimensional planning, where future weights and even some future dimensions remain unknown, planning of future activities can, at best, be only roughly indicative. A detailed program can be provided only in connection with strategies for developing new instrumentalities, for providing options. A good plan can highlight those variables which will importantly influence the ultimate decision and can anticipate decision points. But a good plan should be viewed as a complicated structure to foster intelligent hedging. It ought not be viewed as a prescription of future activities.

Defense is the most dramatic example of multidimensional planning. Important qualitative changes are relatively frequent and potentially devastating. Consequently, simple quantitative hedges of the overpass type are only a small part of a solution to the hedging problem. In dealing with an unknowable future, much of the burden of hedging falls on the R&D program, preferably one of wide-ranging character. The purpose of R&D is not to provide for the future force structure per se, but rather to develop and to preserve options, which may or may not be taken up. Viewed as a system of hedging, R&D activities cannot be made fully compatible with the definition of a program of future activities. Correctly viewed, a phenomenal R&D success does not necessarily imply acquisition and deployment, whereas, depending on the strategic situation, a partial R&D failure may be followed by acquisition and deployment.

The purpose of R&D is to buy options. It should be recognized as the first phase of a sequential decisionmaking process. Its precise purpose is to reduce the time that would be required before the achievement of an operational capability. The low costs

of preproduction R&D are accepted as insurance against a future military demand—without any commitment to the force structure. Through an austerely conducted program, an impressive array of options can be provided. At least in principle, planning in this form—as a system of acquiring hedges—is relatively simple.[6]

Within the force structure, there is less opportunity for hedging. The importance of this point is hard to overstate. Numerous errors and incalculable waste can come from premature commitment to a system that turns out to be unnecessary. The moral is to delay such decisions until long-lead-time items force the decision. The adaptation of the force structure to deal with as-yet-undetermined future contingencies is the most costly, and, therefore, the trickiest part of the planning process. Since post-decision hedging is so cumbersome, it is important to avoid forfeiting flexibility by preapprehending a decision point.

The Response of Rivals: Perception and Counterresponse

The existence of major rivals intensifies planning difficulties—particularly so on the international scene. An organization's perception of the nature of its rival is based on an oversimplified and partially distorted interpretation of the rival's earlier behavior. Organizational momentum and insensitivity make difficult the recognition of gradual alteration in the rival's conduct, which makes the predominant perception increasingly obsolescent. Only shocks bring major changes in the prevailing perception, which therefore is adjusted only erratically and with lags. Furthermore, the conduct of the rival is influenced by a utility function hard for outsiders to comprehend, and is determined by a bureaucracy, no less cumbersome than our own, which persistently twists behavior in directions that we—as "objective" outsiders—regard as irrational.

Since we have little appreciation of the cross-currents and pressures within the extensive bureaucracy of the opponent, we are periodically subjected to surprises. Nonetheless, when it is ultimately perceived that the previously prevailing image of the rival's behavior has been embarrassingly inaccurate, it can be drastically revised in official circles with astonishingly little questioning. A specialist's services facilitate the process. There are tribal soothsayers who concoct a new rationalization of the rival's behavior which explains (away) all of his unanticipated actions.[7] Off with the old, on with the new, and push to one side any lingering doubts.

[6] Unfortunately, existing organizational arrangements raise imposing barriers to accomplishment of what seems desirable in principle. These arrangements create strong pressures—once again from below—making it difficult to stop a successful or even semi-successful program. One large and long-lived office—the System Program Office (SPO)—is established to handle the individual weapon system on an *integrated* basis. R&D, source selection, acquisition, introduction into the force structure, and early operations are therefore treated as parts of an integrated program. The result is strong pressure to avoid shelving any program. Moreover, OSD pressure toward a broader framework for costs—so-called life cycle costs—adds to the problem. If analysis and decisions are based on presumption of a full life cycle, this further militates against recognizing the legitimacy of termination or shelving of systems. Given its objectives regarding expanding options, OSD would be well-advised to accept some supplementary costs in order to more firmly establish decision milestones.

[7] Needless to say, this is not a condemnation of Kremlinology per se, but a plea for better Kremlinology and its like. In existing work too much emphasis is placed on personalities and policies they are presumed to represent and on public statements and their exegesis. Too little attention is paid to the broader picture of the decisionmaking process, to internal pressures, and to the real trend in capabilities. Also, too little attention is paid to past interpretative errors of our own. If the United States continues to obliterate defective images of its rivals without careful examination of just where we went astray, we shall never learn from our mistakes.

On the other hand, offsetting our own debacles, in this respect we may draw some wry amusement from the role assigned by Moscow and Peking to their own soothsayers, who are obliged to explain American behavior while developing or exploiting acceptable Marxist terminology. They have, no doubt, been quite busy since the start of the Johnson administration.

The planning function must take into account the unanticipated behavior and responses of the rival and our own lagging perception of and reactions to them. Precise long-range plans of, say, the force structure are drawn up on the supposition that the rival will take certain actions, which likely as not fail to materialize. For successful planning this further barrier to planning ahead must be taken into account. If we adhere rigidly to long-term prescriptions of our own capabilities and actions, the results are bound to be less than optimal. Not only is a wholly satisfactory understanding of the character of the rival's behavior unattainable, but this behavior, whatever it is, is subject to periodic adjustment. There are two reasons for this.

First, there is what may appear like a game-theoretic response (though, particularly in international rivalries, it will be marred by bureaucratic sluggishness and by the erratic trial-and-error surges by which a change in policy is actually achieved). The rival will divert his energies from those points that one's own actions have made potentially less lucrative to other, less-well-covered points, which will now appear relatively more lucrative to him. Abstractly speaking, such a response should be anticipated and preparation for adjustments made. In the real world, however, it may be quite difficult to deal with, because the governing doctrine of large organizations inevitably will be simplistic and within that doctrine it is difficult to accommodate any large number of threats.

Second, the rival's view-of-the-world and strategies will be independently changing—as will our own. To obtain long-range plans, we are inclined to freeze both our rivals' and our own view of things. The certainty of change is overlooked, or, more precisely, it is advantageously disguised through mislabeling it as a type of "uncertainty," which we proceed to discount.

Large organizations find it hard to anticipate, to recognize, or to adjust to change. It is hard for them to focus on a large number of threats simultaneously, or to anticipate the possible penalties and consequences of their own successes, or to acknowledge the changeableness of their own and their rivals' behavior. Changes in the environment can only be appreciated by small groups initially. To influence a large organization—to get the prevailing doctrine changed—is a time-consuming process, and by the time it is accomplished the new views will themselves be on the verge of obsolescence. This may account for the organizational propensity to zig and zag. Though prevailing doctrine may collapse with surprising suddenness, normally it has lasted too long. Fashions change, but the new doctrine is unlikely to be substantially more sophisticated than the old.[8] Though it is perhaps unfortunate that this is the way things are, the realities must be taken into account.

SOME IMPLICATIONS FOR DEFENSE PLANNING

The changeableness of the world scene and attitudes toward it, and the multidimensionality of defense problems, severely restrict the potential effectiveness of long-range planning in defense relative to certain non-defense areas. Yet, concomitantly, there is a pressing need for such planning, especially in light of the long-lead-

[8] Seemingly the United States has found it difficult to settle on some reasonable middle ground between the atheistic-materialistic-Communist-world-conspiracy view of the Soviet Union and the I-like-old-Joe-but-he's-a-prisoner-of-the-Politburo view (a later version being, after-Cuba-we-can-trust-Nikita). During Phase II we come to expect the Soviet leaders to pull our chestnuts out of the fire.

A similarly oscillating tendency seems to be developing with respect to the U.S. view of China. From the image of China as a woefully weak, fearful, and easily-deterrable state we appear to be veering to an image of China as the paramount threat to world peace, hellbent on world conquest in the seventies.

time items involved in procurement. Moreover, much of the near and intermediate term force structure is determined by what is already in inventory: for long system life and the heavy outlays on capital equipment preclude very rapid turnover. The factors previously enumerated therefore hardly permit us to discard advance planning. Rather, they provide warning flags which, by discouraging us from harboring too high expectations, may facilitate superior planning over the long haul.

One major implication is that changeableness in objectives, in strategic views, and in the utility of instrumentalities, by underscoring the Knightian distinction between risk and uncertainty (the latter dealing with conditions for which probability distributions are unknown), brings us to a view of planning which is essentially Schumpeterian. *Successful* planning in the long run may be set in partial contrast to *efficient* planning in the short run. Excessive concern with the latter may involve some sacrifice of the former. An overweening concern with the micro-detail of efficiency in the small may lead us to overlook what constitutes efficiency in the large, i.e., success. This appears especially germane for forces designed for war-fighting as opposed to deterrence. In this connection the *general purpose* forces, as the name perhaps suggests, deserve special attention.

In force planning the paramount question remains: In what war or conflict will the forces be engaged? Are the forces to be optimized for a specific kind of conflict, and, if so, which one? Optimization of forces for what appears to be the most probable or most threatening conflict will, no doubt, enhance the capability for that type of engagement, but may do so at the expense of their *general purpose* utility. Despite the vast improvement of the general purpose forces in the past five years, there is some question whether the OSD has been sufficiently alert to this point. Encouraged by the proved role for systems-analytic techniques, the OSD has appeared eager to optimize in situations in which the underlying realities provide an inadequate foundation. Specifically there has been a disinclination to recognize the flattening effect that time and changing contexts have on tradeoff curves. It is hoped that systems analysis can provide a precise answer to force structure planning. As between program elements it is assumed that there is a sharp elbow in the tradeoff curve, which analysis will uncover.

Considerable energy has been devoted to such work. By contrast, insufficient attention has been given to examining the implications for force composition of the wide range of conflicts in which the United States might become engaged. Implicitly, it is accepted that forces optimized for one kind of war will be suitable for other kinds of war.[9] Forces designed for a major struggle (for example, an all-out Soviet assault in Europe) will prove quite adaptable for lower-order conflicts. As between types of conflict, the relevance of optimization seems to disappear, because it is assumed that lower-order capabilities are automatically provided as spillovers from capabilities for major conflicts. In short, forces are viewed as highly complementary for certain major conflicts, but the same forces are seen as highly substitutable in different conflicts. For a specific conflict, optimization is crucial, but among conflicts it is insignificant.

The point may be indicated briefly. In Fig. 1, Part A pertains to force planning for a specific major conflict. Two program elements are indicated on the axes—say,

[9] I have couched this discussion in terms of the OSD, which has accepted this premise in much of its analytical work. However, the Services have been even less inclined to consider wide conflict variability in determining the characteristics of forces. In certain cases the OSD has urged the Services to take conflict variety into account in designing equipment and organization. Design of divisions is a major case in point, and intermittent OSD pressure for a small, inexpensive counterinsurgency aircraft is another. Part of the problem can be attributed to the OSD desire to have major initiatives come from the Services. Nevertheless, the OSD itself apparently has been too ready to accept the proposition that forces are highly substitutable as between potential wars.

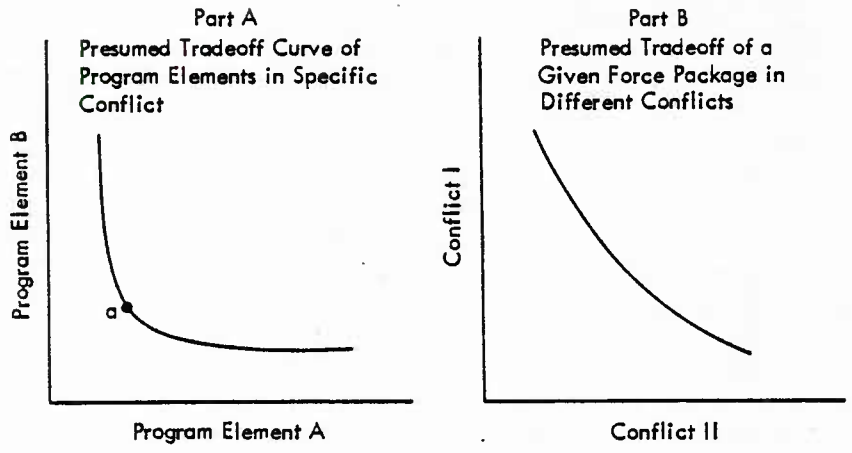


Figure 1

tactical air and infantry divisions. It is presumed that there is sharp curvature of the tradeoff function providing a clear-cut point of optimization at small a. Part B indicates the role assigned to the same forces in a variety of conflict situations. Different hypothetical conflicts are places on each axis. In this case it is assumed that the tradeoff function shows little curvature. In other words, forces optimized for one type of conflict can be substituted in other conflicts with little penalty in terms of potential payoff. On the face of it, there would appear to be little reason to presume that optimization is crucial for one set of circumstances but has little relevance for the other set in which conditions are, by definition, more varied. The ultimate effect of any such line of thought, it should be noted, is that optimization of forces is achieved by contemplating a single type of conflict—and ignoring the rest.

Such a procedure appears dubious in concept. The typical fare of the present world struggle is not the expected wars, but rather the crises that erupt at times and in places where they are not anticipated. In Cuba and in Vietnam, U.S. forces have been deployed to deal with conflict situations other than those for which they were optimized. U.S. forces must be developed to deal with the unexpected. A principal objective is to provide the physical means for crisis management. And crises almost invariably come as a surprise to one of the participants—and must be handled by a quick response.

Evidence literally abounds of our inability to plan the future force structure for contingencies that actually materialized. When the Kennedy administration came into office in 1961 there was concern that our posture did not deter a belligerent Soviet Union from major military action—either a bolt-from-the-blue or a major conventional assault in Western Europe. In retrospect, it seems clear that the build-up of strategic missile forces was more rapid than necessary. What is perhaps even more revealing was the focus on developing conventional forces capable of dealing with an all-out engagement in Western Europe. This goal has more recently been deemphasized for a number of reasons, not the least of which is that we no longer consider the Soviets to be that bold. I would be the last to suggest that the administration's concern was unwarranted. Nonetheless, energies devoted to this kind of war did divert attention from the creation of capabilities for other types of wars.[10]

[10] The OSD did push the development of the Special Forces. While good in itself, this represented, however, a very small proportion of the total effort.

(At that time there existed a dominating reluctance to contemplate American involvement in a major ground war in Asia.)[11] Forces designed for the major-European-war role might not be readily adaptable to other contingencies. If divisions are designed to fight a major foe in the North German plain, they may prove to be an unwieldy instrument for coping with a scattered and hidden foe in the marshlands or forests of Vietnam.

Turning to tactical air, it is notable that major effort was devoted to the TFX fighter, the principal new weapon system authorized during the early years. This was a costly vehicle, obviously inappropriate for a lengthy war of attrition against a foe with weak air armament. Its principal justification lay in its role for major war—possibly nuclear. Though hardly appropriate for a steady-attrition conventional war with limited air opposition, the F-111 (TFX) and the F-4 were planned increasingly to dominate our tactical air posture. In Vietnam, however, we have become engaged in such a steady-attrition conventional war. We had programmed neither the appropriate type nor number of tactical aircraft to carry on that war and fulfill our other commitments.[12]

The program adjustments that have become necessary illustrate how actual developments may come as a surprise to planners. In discussing the 1965-1969 Defense Program in early 1964, Secretary McNamara indicated that both the B-57's and the F-102's would be quickly phased out.[13] The plans later were altered drastically. The B-57's were not phased out, but were shipped to Vietnam and became for a time our principal tactical bomber. The F-102's were also retained in service, though "for planning purposes," it continued to be indicated that they would all be gone by June 1966.[14] Such adjustments merely illustrate the infeasibility of precise advance planning. Plans belong in a hypothetical loose-leaf folder, so that when adjustments are desired, they can readily be made.

The tactical air situation, however, reveals a more fundamental problem in planning. For many years the selection of equipment and the training of personnel in the Air Force have presupposed the use of nuclear weapons in major conflict conditions. Nuclear weapons permitted the number of required sorties to be held down. Aircraft were expensive and designed for nuclear delivery. High investment per aircraft, as well as the expected firepower per sortie, tended to hold down the number of aircraft. Attrition—with the low sortie rates expected—would be bearable in terms of total cost and the drain on aircraft inventory.

On the other hand, the DoD has repeatedly signalled its intention to keep wars conventional, if possible. Nonetheless, the aircraft inventory permitted was adjusted to a war different from the one being fought in Vietnam. Expensive and vulnerable aircraft are flying repeated sorties against heavy ground fire. The cumulative attrition is high. Not only are costs far higher than they would be with more appropriate aircraft, but the attrition in this peripheral war is draining the inventory at an unanticipated rate. More important, the DoD, which had explicit notions regarding how such a war should be fought, did not see to it that appropriate equipment was provided to fulfill its strategic intentions.[15]

[11] This may illustrate the problems inherent in accepting concepts or doctrines implicitly closing options—that we may ultimately wish to take up.

[12] The unveiling of the B-52 bomber in its tactical role may represent the kind of serendipity on which policymakers count. Cost-effectiveness calculations, however, raise some question whether this really was an appropriate solution.

[13] Statement of Secretary of Defense Robert S. McNamara before the House Armed Services Committee on the Fiscal Year 1965-69 Defense Program and 1965 Defense Budget, January 27, 1964.

[14] Statement of Secretary of Defense Robert S. McNamara before the House Armed Services Committee on the Fiscal Year 1966-70 Defense Program and the 1966 Defense Budget, February 18, 1965.

[15] The bureaucratic aspects of this problem will be discussed below.

The deficiencies of precise advanced planning can also be illustrated with respect to the strategic forces—though perhaps less dramatically than in the case of tactical air. From 1961 to 1963 bitter controversy was generated in the Pentagon regarding the size of the Minuteman force five years or more in the future. Figures which had tentatively been approved by the OSD had subsequently been revised downward. The point is not only that the plans were changed, but that the energies expended in the controversies were used in an unproductive manner. Not only did the OSD seem to be taking back capabilities on which it had committed itself, which caused some bitterness, but the issue need never have been decided in the first place. Given some stability in strategic objectives,[16] U.S. strategic capabilities will be determined by the forces deployed by potential foes, primarily the Soviet Union. Since predictions of how rapidly the Soviets would deploy strategic capabilities have repeatedly turned awry, it would seem the better course of wisdom *not* to precipitate such questions prematurely. Decisions regarding the size and composition of the strategic forces should not be determined until the major uncertainties regarding the enemy's posture have been resolved—or until long-lead-time items force the decisions upon us. Planning too far in advance means the forfeiting of options.

Ultimately we must accept that advance planning necessarily entails partial failure. The instrumentalities are never quite right for the circumstances in which they are employed. But a partial failure also means a partial success. A typical reason that the instrumentalities provided prove distinctly suboptimal is that one has successfully deterred the type of conflict for which they were originally intended. In the contemporary power struggle the United States should expect to fight "the wrong war, in the wrong place, at the wrong time." It may do so reluctantly, as did the Truman administration in Korea, or more committedly, as does the Johnson administration in Vietnam. Nonetheless, as long as deterrence is largely successful, the wars in which we become involved are likely to occur in places for which we are least well prepared. Although in its nature this consideration is difficult for planning to take into account, it is essential that it not be overlooked in planning. Overoptimization—by designing forces for the most obvious possible wars—may be the surest way to hampering the use of our potential power.

The gulf between the wars for which we prepare and the wrong wars that we fight at the wrong times and in the wrong places brings us back to the organizational image of the outside world and the nature of foes which, as indicated above, tends to ossification and to obsolescence. Sudden changes in image frequently coincide with changes in administration, which in the United States are typically associated with "new looks in defense" of one sort or another. While on the outside, the opposition broods on capabilities, possible wars, and the nature and behavior of foes, which the incumbents are presumed to be willfully overlooking. Not uncommonly the opposition is correct, but these views, nurtured on the outside, become dominant on the arrival in power. When they become dogma, they may not only be out of date, but become increasingly more so, particularly when adhered to rigidly.

This provides an explanation, not only of why we fail to prepare for the war that we actually fight, but also why zigs tend to follow zags in military policy. The more quickly obsolescent views, which gestated on the outside, are jettisoned, the better. It is to the credit of the Kennedy administration that the bomber gaps, missile gaps, and early rhetoric were so quickly forgotten. The tendency to provide another zig to follow the previous zag was certainly there. Much of the credit undoubtedly

[16] Differences between the OSD and the Services regarding objectives—what strategic superiority means and what margin of superiority is desired—would inevitably breed some controversy. However, the level of controversy can be reduced—if premature discussion of specific numbers is avoided.

belongs to McNamara's introduction of analytical techniques, which has transformed the discussion of military problems, and somewhat alleviated the tendency for new military policy simply to represent the replacement of one set of prejudices by another.

BUREAUCRATIC PROBLEMS

The introduction of radically different methods of planning and management has, on balance, significantly improved the quality of defense decisionmaking. Yet, as must be expected, in grappling with the old problems, certain new problems have been created. Partly this reflects that certain consequences could not be foreseen when the scheme of reform was initially formulated. In addition, it will be recognized that any major redistribution of power implies "losses" for particular groups and individuals. Inevitably this leads to resistance, conflict, and misunderstanding. In this section we shall explore both the unanticipated developments and the dilemmas that have emerged.

First, let us examine the impact of the introduction of cost-effectiveness analyses in the determination of force structure decisions. Aside from issues of prerogatives and power involved in the question of who must ultimately decide, it should be understood that an underlying difference exists between the conceptual approaches of the new breed of systems analysts and the professional officer corps. Let us consider this question aside from which approach is right or wrong, for we shall discover that each position has something that could be said for it. By training and inclination, the military officer is reluctant to drift too far from the concrete, and in large measure the concrete implies the fruits of past experience. This helps to account for the stress placed on campaign histories in military institutions and the "lessons" presumed to be gleanable from such historical analyses. Experience helps one to distinguish between superficially plausible hypotheses and the capabilities that will survive in the heat of battle. Command experience makes one keenly aware of the miseries of command and control and the criticality of the human factor. The weaknesses of men will surface in battle, and only discipline and organization, not analyses or irrelevant pep talk, will shore them up. In examining new equipment or concepts, officers are not disposed to ignore man or man-equipment relations subsumed in organizations.

By contrast, the method of systems analysis is to look at the hypothetical future rather than at experience, to assess objectives, and to suggest how best to accomplish these objectives given the available and developable techniques. The past is disregarded save for the investment carryover represented by sunk costs. The human factor—how men are likely to perform—tends to be deemphasized, since it is difficult to draw within the analytical structure. Stress is placed on such elements as weapon effectiveness, gross firepower, vulnerability, survivability, and communications equipment—to the exclusion of the more intangible elements. The greater the rate of technical change, the more appropriate is such an approach. Yet, the weight is increasingly placed on hypothesis and conjecture. The concrete experience, which the military perhaps mistakenly regard as providing something less hypothetical, receives little attention.

Experience provides the basis of military intuition or judgment, which has not infrequently been treated derisively by civilians. Once again, however, the military approach contains a kernel of truth. The military professionals have recognized,

quite correctly in my view, that all decisions must ultimately be based on intuition.[17] This insight did lead, unfortunately, to a downgrading of analysis and neglect of its role in "educating" intuition, with the result that analytical efforts were not pushed as far as possible. By contrast, the defense intellectuals, imbued with the sense of their own rationality, tended to push analysis further than it reasonably could be pushed. In the early years of enthusiasm an image was created that somehow systems analysis led directly to the appropriate decision. This is not, of course, the case.[18] The only result was the mislabeling by the civilians of their own intuitions and judgments as "analysis."[19]

This divergency in view led in the early years to misunderstandings which happily are now fading in importance. It is now more widely recognized that analysis cannot lead directly to decisions. Its purpose is to gather evidence, to improve the quality of discussion, and to sharpen the intuitions of the decisionmaker. By themselves the analyses ignore complexities and intangibles, which the military rightly stress, and the decisionmaker will appropriately take into account. While every effort should be made to reduce dependence on sheer intuition, the role of intuition at the conclusion remains embarrassingly large.[20]

There is a second problem, the resolution of which still requires major action. The alteration in the structure of power within the Department of Defense since 1960 has resulted in a slicing up of authority and responsibility in ways that do not mesh. The Office of the Secretary of Defense has acquired the authority to specify major objectives and policies in planning, but their implementation requires *positive* motivation and action on the part of the Services. Proposals normally come up from the Services. The OSD is reluctant to introduce major new components in the force structure solely on its own initiative. But the proposals coming up from the Services may not be appropriate for the implementation of OSD objectives and policies. And, when such a failure occurs, controversy ensues regarding just where the responsibility lies.

I have already mentioned that the tactical air forces, both in numbers and design, were developed with nuclear delivery in mind. On top of the existing force structure the OSD has superimposed a preferred strategy of U.S. acceptance of the nuclear firebreak with initial recourse, wherever possible, to conventional weaponry. To achieve major impact with such a strategy requires greatly increased sortie generation, more numerous, appropriately designed, and presumably cheaper aircraft, and much greater logistical capabilities. The Air Force has continued to stress relatively small numbers of high-quality aircraft. But, more important, the OSD has

[17] I am here dealing with the most insightful of military observers. That much commentary has been something less than insightful cannot be denied. Many officers have employed "intuition" as a kind of incantation without appreciating its proper role—and have tended to confuse it with rules of thumb and very tired old formulas.

[18] No doubt this was in part the hoopla that surrounds any major reform. In the heat of controversy it was easy to fall back on the argument that "our" solutions flow from scientific study, "yours" do not. At Rand and elsewhere, however, the better practitioners of systems analysis have long recognized the dependence of analysis on an irreducible minimum of intuition.

[19] There may be a real dilemma at this point. Too early acknowledgment of the final role that intuition must play may inevitably lead to impairment of analysis. Yet, postponing recognition of its role may preclude perception of the point at which analysis slides over into debatable policy judgments.

[20] It could not be otherwise. Pressures of time preclude the chief decisionmakers from examining and assessing the detail of the many analyses presented to them. The decisionmakers must exercise judgment regarding which analyses (and whose interpretation) they will accept. They can only dip into the analytical details in a most cursory manner. It has been observed—in a questioning tone—"that the higher one goes in the Pentagon, the less rigorous is the analysis." This is true, but, given pressures of time, little can be done about it. Structurally, nothing can ultimately substitute for the decisionmaker's intuition, though hopefully these intuitions will be consistent with careful analytical work done at lower levels, the spirit of which the decisionmaker has absorbed.

been reluctant to face either the bureaucratic or the resource implications of its strategy. It has not been prepared to insist that the force posture be changed—nor has it clearly been willing to provide the necessary resources while requiring that the Air Force make the major investment which will permit an extended campaign of conventional bombing.

It has been a steady complaint within the OSD that the Services have been uncooperative. Five years after the new accretion of authority in OSD this refrain provides, at least in my view, an increasingly flimsy excuse for inaction. If the authority to determine overall strategy falls to the OSD, that implies that it also acquires the responsibility to see to it that the instruments and financial resources are provided to implement its preferred strategy. Perhaps the reluctance to face the bureaucratic or cost implications of strategic choice is understandable, but that then raises a question of the appropriateness of treating the Services as handy scapegoats.

A third element of conflict is introduced, when we consider the issue of responsibility in its broadest terms. In principle, within the Department of Defense a proposal can be initiated by one or more of the Services, the OSD, and other major DoD subelements, and that proposal will be evaluated on its own merits. In practice, it does not work out this way. Such a system requires a disinterested judge, but the OSD fills the roles of both judge and plaintiff, which quite naturally raises questions about its degree of detachment. On many issues the OSD is obliged to make the decision. There is no suitable alternative. Nonetheless, the facade of objectivity—as a mask for constituted authority—may reasonably stir resentment. The OSD sets defense objectives, but it seems inappropriate then scornfully to dismiss Service proposals on grounds that the proposal is more in accord with the Service's notion of strategy than its own. The OSD establishes the ground rules for debate. But if it interprets or restricts the application of those ground rules so as to further strategic notions of its own, it should not be surprised when such actions are regarded as an unfair exercise of authority.

Let me give a specific illustration. Since 1961 much stress has been laid on creating multiple options. All informed persons will agree that such an approach is desirable in principle, not only for the DoD but for other organizations as well. Unfortunately, options have a habit of running up against constraints, and the decision must be made whether the option will be forfeited or the constraint relaxed. In organizational life, despite the lip service to multiple options, the observed tendency is to be deeply concerned regarding the options the dominant group desires, but to be indifferent or hostile to the options valued by other groups. This is the way the world is, but we should recognize that options conflict and that leaders are normally more concerned about *their* options than about options generally.

A clear example is the present endorsement in national policy of a nuclear firebreak. My personal view is that the Government has been more concerned to establish the firebreak than it should have been. That, however, is not my point, which is that the firebreak represents a constraint cutting across the multiple option approach. The Government has refrained from making certain investments in the capabilities for low-level nuclear warfare, not only because of the expense but because the creation of the options might tempt us to go through the firebreak, and would certainly give others the impression that we were willing to do so. This is national policy. It may be sound national policy. Nonetheless, it represents the deliberate exclusion, through a self-denying ordinance, of a whole range of options.

Policy discussion should acknowledge that we are frequently as interested in creating constraints as in creating options. Just as we may avoid the costs of creating some options, so we may readily incur the costs of creating constraints, i.e., option denials. From public discussion it has not been at all obvious that we are willing to

invest major resources in establishing constraints. If greater clarity were attained, controversies could focus on the real issues. In the case of nuclear weaponry, the real issue is: Do we wish to deny ourselves certain options? It is certainly legitimate for leaders to be interested in *their* options, and to wish to avoid other options. If that is the goal, discussion should focus on its validity, and it should not be obscured by inspirational talk on generalized option creating.

Such considerations have broad applicability to long-range planning under conditions of uncertainty. Unless a plan consists primarily of the indication of numerous options by which one may respond to the unknown, it must represent something of a corset. Time forces certain decisions, but wherever a plan represents premature commitment to arrangements that may be far from optimal, it becomes an instrument for option-denial. At best, the plan will be readapted—at the cost of energies which might more profitably have been invested elsewhere. At worst, the commitment to less-than-optimal arrangements will be honored. Normally the results will fall between the two poles, but bureaucratic pressures push in the direction of the latter pole.

It has already been mentioned that plans elaborated in 1962-1963 for the missile forces of 1967-1968 had to be adjusted downward in 1963-1964 with a considerable investment of energy in bureaucratic fighting. The decision reflected changes in intelligence estimates and in strategic concept. The point is that it required strength to change programs that need not have been formalized. A Defense Secretary with less courage and strength than Robert McNamara possesses might well have hesitated to alter plans which he had previously accepted. In that case defense allocations would admittedly have diverged from the optimum. This is, of course, only one illustration. More generally, the Five-Year Force Structure and Financial Program, developed in DoD since 1961, may readily lead to premature commitments, tending to reduce the number of options. There is a bias against readjustment, which only strong personalities can override. In some cases, as with the nuclear firebreak, option-denial can be understood as a reflection of national policies which seek constraints on particular types of violence. In other cases, option-denial may flow from the inflexibility of bureaucratic processes. In the latter case, it is unintended, and therefore much less justifiable. To the extent that the Five-Year Force Structure and Financial Program leads in this direction, it cuts against optimal long-range planning.

The fourth bureaucratic issue to be examined is the intelligent handling of dissent. In the nature of organizational life, this is perhaps the hardest problem of all. Since much resistance is blind, it is all too easy for leadership to treat all opposition as if it were blind. How can one provide an effective channel for dissent without providing a forum for obscurantism? The ultimate difficulty arises, quite unavoidably, from the stringent limits on the individual's capacity to communicate with others—but is intensified by problems of access. One learns normally only from the people with whom one talks about policy issues, if not theory. Beyond the umbra of organizational intimates, there is a penumbra of others whose ideas become distorted and oversimplified as they are carried over distances. And beyond the penumbra is the contemptuously treated darkness represented by the reprobates and the unknowns. Under the best of circumstance a really creative idea, because it is new, is hard to sell. It is likely to bear some semblance to some older heresy, and may casually be dismissed on those grounds. However, if one worries continually about dissent, all one's energies will be dissipated. Effective organizations are not debating societies given over to dispute regarding nagging intellectual doubts or

been competitively used in the Pentagon as a weapon in bureaucratic controversies, there is little doubt that at base it is the most powerful technique yet devised for improving governmental decisionmaking. The details of particular analyses will, of course, continue to be subjected to rigorous examination. Moreover, because the technique provides a radical means for questioning prevailing ideas and methods of procedure, we must anticipate a continuation of bureaucratic resistance. When active resistance becomes unpromising, recourse will be had to passive resistance. Nevertheless, cost-benefit analysis is here to stay.

These new management tools may be the primary *technical* contribution of the McNamara regime. In defense, what is perhaps even more important is the *psychological* contribution. McNamara has succeeded in stirring up a very stale millpond dominated by old habits, prejudices, intuitions, and good ideas which had not been rigorously formulated. For this the nation will remain forever in his debt. But it is important to keep in mind that, although the new techniques help, the stimulating impact of the stirring up of the stale millpond would have come even in the absence of these techniques. The contribution of the new regime goes well beyond the specific institutional reforms that one can list.

Despite the improvements, problems remain. The Department of Defense is a far-flung organization with a highly variegated structure of activities. Such organizations have difficulty in preserving flexibility and creativity and in preventing organizational doctrine and other constraining influences from snuffing out creativity. No doubt a major part of the answer is the better exploitation of the unique characteristics of the small group with its opportunities for low-cost communication and escape from organizational doctrine. In view of the diversity within the organization, this may appear as an obvious part of the solution, but large organizations find it difficult to provide *real* freedom to small groups or to give an attentive hearing to new ideas that such groups may produce. More will have to be done on this problem. And more will have to be done on the closely allied problem of providing a channel for the better understanding and utilization of dissent.

This issue of correctly handling dissent poses something of a dilemma. Those who are most tolerant of dissent are frequently those who place the least value on it and are consequently impervious to its pinpricks. The Eisenhower administration, for example, had a deep-seated contempt for sensation-mongering critics, and in the nature of things these critics wound up on the outside. Yet, it allowed external criticism to flourish without reproof, if without much interest. The administrations since 1961, more politically aware, perceived quite clearly the penalty that the Eisenhower administration had paid for its lack of alertness to criticism. There is no inclination to make the same mistake. Every effort is made to counteract criticism, and this has overflowed into the overpowering of dissent. It is a seeming irony that the tolerance for criticism has diminished. The present regime is more responsive to criticism, can comprehend its message more precisely, but just because it takes it so much more seriously, it has tended to be more resentful. Here is a point of danger. Is it possible to have an alertness and sensitivity to criticism and at the same time not have that sensitivity lead to a querulous reaction, which results in the attempt to push the criticism aside?

It is the higher-order objectives which in the long run are likely to prove the most important and the most controversial. But for all the sensitivity on the subject, one has the feeling that talents and expertise are concentrated elsewhere, and that less attention is paid than is appropriate to higher-order objectives and the changes they must undergo as the environment changes. The Department of Defense has done an effective job in considering appropriate middle-level inputs for middle-level objectives. These are the issues that lend themselves to relatively precise answers.

Moreover, these are the current issues that absorb so much energy that comparatively little is left for considering such issues as the shape of the world and how it is changing and the appropriate higher-order objectives and how they will be influenced by external change.

Achieving efficient management, while desirable in itself, may not be the most important thing in the long run. Some relief from the distractions of current pressures must be given so that energies can be devoted to study of higher-order objectives. In this regard an institutional arrangement within the DoD for focusing attention on the elements of change and the longer-run issues could be invaluable. As managers of their Departments, American leaders find it difficult to cultivate the attitude of detached questioning, which Walter Bagehot regarded as the most fruitful role for a Minister. In principle, some institutional means for fostering constructive criticism can be created. In practice, however, the problem would be to prevent such internal institutions, ostensibly devoted to study of the long run, from being turned like everything else to the pursuit of short-run objectives.

ON RELATING NON-TECHNICAL ELEMENTS TO SYSTEM STUDIES

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Prepared for presentation in the Panel on Social, Economic, and Political Factors in Military Systems Analysis at the annual meeting of the Military Operations Research Society, at El Paso, April 25-27, 1967, this paper was published in the November 1968 *Bulletin of the Atomic Scientists* (Vol. 24, No. 9, pp. 12-17) under the title, "The 'Soft' Factors in Systems Studies." The author would like to express his appreciation to A. W. Marshall, R. R. Nelson, and C. Wolf, Jr., for their helpful comments.

In the past six years systematic use of quantitative analyses has resulted in a significant improvement of the U.S. military posture. The effectiveness of military forces has increased substantially in relation to the resources invested, and in some areas large increases in capability have been associated with declining costs. In terms of military power in the limited sense, there is little question that the usefulness of systems analysis has been amply demonstrated. On the other hand, there have been a number of prominent policy failures during this same period, and it is sometimes suggested that at least some of these failures are attributable to the narrow range of factors that are incorporated in military systems studies and to the singlemindedness with which the conclusions of such studies are implemented by policymakers.

My responsibility in this panel, as one whose main activity has been to examine military capabilities in the narrower sense, is to speculate on the inherent limitations of analyses that neglect nonmilitary factors and to consider whether the structuring or the implementation of such studies has indeed contributed to policy failures. In so doing, one inevitably becomes concerned with those social and political factors which admittedly are overlooked in such studies. To the extent that such omission causes serious damage, it is incumbent on critics to suggest methods for addressing such elements—either in the studies themselves or in their subsequent use in policy formation. Systematic analysis, after all, is still in the early stages of development, and there remains ample room for improvement in our procedures.

I.

There are five major themes that I will develop in this paper. First, the omission of certain broader political and social factors does imply an inherent limitation in military systems studies. Second, to a certain degree such self-imposed limitations are defensible in the attempt to deal (suboptimally) with a critical portion of the overall problem. Third, the chief difficulties arise in the transition from these analytical studies, admittedly suboptimized, to decisionmaking. Fourth, *major* systems studies, though assumed to be suboptimized, inevitably incorporate a number of nontechnical assumptions, though typically in a tangential and implicit fashion. Fifth and finally, in many decisions the use of specialized studies without buttressing

by the explicit and extensive examination of the non-technical factors does lead to incomplete results and inadequate conclusions—which may be highly embarrassing with respect to national policy.

II.

It seems appropriate to start with a statement of the semi-orthodox position regarding systems analysis. Whenever one considers the entire complex of decision-making, it is apparent that everything is ultimately related to everything else. The ramifications of a single issue spread out to other issues. What appears at one level to be a precise and dominating objective becomes, as one moves to higher-order problems, only one of several alternative inputs, and the choice among them becomes dependent on higher-order objectives.[1] The layering and tying of issues makes fully complete analytical treatment of even a single problem wholly infeasible, since there is an exponential growth in the complexity and cumbersomeness of the total problem. In principle, policy formation would require the solution of an infinite number of simultaneous equations, a large proportion of which are unknowable and almost all of which inevitably must lie outside the attention span of a single human mind. To deal with any problem in its entire complexity, one is ultimately faced with such global and *unworkable* questions as "where is the society going?"

One cannot do everything at once. To attempt to do so is the road to analytical impotence. If anything at all is to be accomplished, one must break into the total problem at some point. Not only does one have to start somewhere, but one will probably have to leave something out. But the point is to start somewhere—and the most productive place to start is on those elements which are fundamental to the final results and which are relatively easy to manipulate. In analytical work there is a visible preference for the so-called "hard" elements—hard in the sense of being susceptible to measurement rather than in the sense of being intractable, for the political or organizational factors are undoubtedly the most difficult of all. One starts with the elements of technology, cost, and (admittedly inadequate) measures of output or effectiveness, because these provide an easy-to-grab handle to the problem. The underlying premise is that the economic factor—seemingly implicit in the phrase, cost-effectiveness—should outweigh political, social, and other factors. Within the original scope of the analysis, these last are often given zero weight.

While clearly acceptable in lower-order instrumental problems, it is apparent that for higher-order strategies or policies the whole procedure becomes increasingly questionable. For higher-order problems the measure of effectiveness must be based on some broad strategic criteria, in which political or psychological assessments unavoidably play a role, whether this be admitted or not. Thus, even in straight systems studies the effectiveness concept is likely to be tainted by political estimates that make up the overall strategy. It is fervently, if secretly, hoped that these "soft" elements will have a minimum effect on the results.

Good reasons exist for avoiding the soft elements at the outset. Political assessments, for example, are based on opinions which are subject to change, and may fluctuate wildly with changes in fashion. What is worse, they may rest on the gossamer support provided by intuition, whose range and variety is limited only by the number of people involved. To the extent that intuitions converge, the focal point[2] tends to be some sort of unchallengeable old wives' tale—or, to put it more

[1] On this point, see Albert Wohlstetter, "Analysis and Design of Conflict Systems" in E. S. Quade (Editor), *Analysis for Military Decisions*, Rand McNally and Co., 1964, esp. pp. 111-122.

[2] In this case opinions may harden into political myths, impervious to the adjustment required by changing circumstances.

politely, the badly tested corollary of some past experience, which may no longer be relevant.

Aside from the difficulty of absorbing political factors into the analysis, the reasons for exclusion in the early stages of analysis relate to the quality of available political inputs. Characteristically the quality is quite low. Sophisticated political insight is rare—and is usually unavailable to those doing the studies. What is available turns out, as frequently as not, to be demonstrably wrong—despite the many evasive techniques used to rationalize past mistakes. When so-called political insight turns out to be a collection of Delphic utterances, subject to a variety of interpretations, it becomes a questionable basis for making decisions.

Moreover, in the realm of the political myth, early use of "political wisdom" is likely to introduce the type of stodgy or obsolescent ideas produced by "wise men." The classic and frequently derided example is provided by "old China hands," but "old X hands" provides a better clue to the generic case. The underlying premise in such assessments is that the best guide to the future is some mild perturbation of previous experience, sometimes highly limited and subjective. A procedure for dealing with national entities on the basis of gradually digested subjective experience is reliable only under traditionalist conditions. In a world characterized by high-speed communications, by the ability to mobilize resources rapidly, by changing technologies, and by the possibility of rapid development of impressive new capabilities, the traditional sort of "political wisdom" becomes increasingly dubious. Reliance on what may be ossified experience will lead to irrelevant, and possibly disastrous, conclusions.

The record of achievement by these means is very spotty indeed. To choose one of the most responsible and highly commended examples, the U.S. decision to organize NATO in 1949, however desirable some of the consequences, was based on some rather obsolescent notions regarding the strength and importance of the European nations and the direct contribution that they could make to the security of the United States. There was a striking failure to recognize the revolutionary impact that nuclear forces would make with respect to the earlier beliefs regarding European defense.[3] While European security remained important to the United States, its importance existed in different and more indirect ways. Not recognizing how dominant the strategic nuclear balance was to be, not only in U.S. security but European security as well, the United States attempted to organize European defense in the traditional pattern of the grand coalition.

The need for free nations "standing together" in a sacramental bond against tyranny had been "learned" by the United States in the two world wars. The standard alliance hopes regarding resource pooling, as expressed for example at the Lisbon conference, were to have relatively little relevance for the actual military conditions that the Western nations were to face. Though the Soviets imitatively established a similar structure in the East, it has become increasingly apparent that both alliances are facades weakly masking the overwhelming dominance of each by a single partner, in a way that prior wisdom conspicuously failed to predict. Putting aside the *political* desirability of NATO, its relevance from the standpoint of military force is entirely different from what was anticipated.

Though this may be a case in which the wrong premises led to what is, at least in part, the right political result, it should make us uneasy regarding the early introduction of "political wisdom" in force structure positions. Over the years the

[3] Cf. Warner R. Schilling, "The H-Bomb Decision," *Political Science Quarterly,* March 1961, pp. 26-27. Schilling comments that U.S. policies to restore the European balance of power were "pre-nuclear in their rationale. . . . Nor were nuclear weapons believed to have qualitatively altered the military problem of achieving that objective."

total loss resulting from the misallocation of resources to and within NATO has been truly impressive,[4] and these wasted efforts reflect in no small part the faulty original concepts. It is, I believe, a striking case which suggests that more effective forces could have been obtained at far lower cost—if there had been continuing force structure analyses,[5] which initially excluded the soft political factors and their accompanying shibboleths.

Though one may reasonably conclude from such occurrences that the standard methodology—with its initial concentration on the hard elements—is quite defensible, at least in principle, it should simultaneously be emphasized that the initial exclusion of the soft factors is not intended to lead to their permanent exclusion. Sensible systems analysts have always recognized that the deciding factors frequently go beyond those originally incorporated in the analysis, and thus the decisionmaker must supplement the original analysis with the excluded considerations in reaching his decision. (Pride in their efforts may tempt the same analysts to oppose in practice those qualifications that they accept in principle.) Analysts are inclined to believe, with some justification, that the real world is overrun with political or bureaucratic pressures, which make it improbable that analytical efforts will be pushed too far without political review. The danger, it is believed, lies in analysis being ignored rather than its sweeping all before it.

Nonetheless, this assumption, made very loosely, that the appropriate adjustments will be "done later," does represent a major weakness in the methodology. There is some element of irresponsibility in ignoring how studies may subsequently be used or manhandled in the bureaucracy. The need for political review of limited-purpose analyses is not something that analysts should mention and then blithely ignore, for there is every likelihood that the obligation will get lost somewhere in the bureaucracy. While it is not their primary role, analysts should pay additional attention to the bureaucratic issues, for this could lead to a more satisfactory accommodation between their own efforts and institutional reality.

There are two prominent reasons for studying the problem. First, in light of the way bureaucracies work, a careful technical study is likely to become a Departmental position. A Department may regard its responsibilities as limited—and advocate a policy optimized solely for cost, economic, and technical factors. It then assumes that the responsibility for considering other elements lies with other Departments. If the bureaucratic position of the first Department is sufficiently powerful, it may carry the day with its recommendation without receiving the review that it itself considered necessary. Something of this sort seems to have occurred in the case of Skybolt, in which what emerged as the strongest forces supported a strictly technical analysis, with inadequate political coordination taking place after the decision.[6]

There is a second and more fundamental reason for encouraging among analysts a better appreciation of the norms in bureaucracy. In much of the work of systems analysts there is an underlying premise that policy is determined by a single deciding unit. In developing alternatives for one organization or in predicting the behavior of other organizations, this assumption is invalid. Within the government the unit to which advice is tendered will typically lack control over the resources included in the study—and may even lack knowledge of the activities and objectives of

[4] Cf. A. W. Marshall, *Determinants of NATO Force Posture*, The Rand Corporation, P-3280, January 1966.

[5] Of necessity, these would have to have been high-quality analyses, which were relatively objective—i.e., free from the institutional biases and interests of individual Services or Commands.

[6] The irruption of the Cuban missile crisis in October 1962, diverting attention and absorbing energies from the issues symbolized by the Skybolt case, may have prevented an appropriate review and contributed substantially to the political debacle.

supposedly cooperating units. The results may be particularly deficient when the advice is tendered either directly or indirectly to a committee of equals or to an alliance. Given the resistance and the bargaining which is inevitable in light of the disparity of objectives, perspectives, and information, the results will be sharply at variance with those anticipated on the premise of a unified and rational controlling force.

In such an environment the excuse that "the best laid schemes o' mice and men gang aft a-gley" is invalid, for the neglect of the real character of the environment[7] implies that the plans may be the worst laid. A number of phenomena might be considered in this light. One fascinating case was the attempt to extract expanded conventional forces from our European allies in the 1961-1962 period—with little appreciation of the real structure of NATO or the divergency of interest and attitude among its members or their component bureaucracies, including the U.S. military. More generally, any attempt to get a *formal* change in NATO strategy, given the need to obtain approval from the North Atlantic Council (NAC), will run into much greater resistance than one would anticipate on the premise that the decision will reflect the unified interest of the members viewed collectively.

The clear implication is that analyses of strategy, force structure, or other questions might be better designed to take into account the organizational impediments to their implementation. Rational resource allocation may be the proper initial benchmark for military systems studies, but modifications of those studies should also be produced to take into account organizational feasibility. It is perhaps necessary to stress that considerations of feasibility must not be permitted to drift into excessive accommodation with the status quo. The proper goal of the analyst is not so much to choose between existing alternatives but to design new and better, but feasible, alternatives. Only in this way can one have some confidence of obtaining that second-best solution which is the best among the feasible alternatives. Not only is it wise to recognize that the best is often the enemy of the good; where organizational reality enters in, the best may be a deadly enemy.

III.

We have looked at some reasons for initially excluding the soft factors in military systems studies and for concentrating on the suboptimization of the hard factors. We have also underscored some of the problems that arise in the transition from the analytical stage to the decisionmaking stage. It is now necessary to consider why, even under the best of circumstances, it is impossible in analyses of higher-order problems to avoid the introduction of those soft factors, which are both variable and subjective. A major point is that some measure of effectiveness must be designed to compare alternatives, but that any *serious* measure of effectiveness inevitably becomes tainted by the political or psychological assessments that make up overall strategy. Unless we are dealing with a system that is purely instrumental, we must recognize in any conflict situation that overall systems effectiveness is dependent upon the response of the opponent.[8] Instrumental capabilities are useful in certain specific roles, not in others. Unless one can disarm an opponent more or

[7] Curiously, the better management consultant firms do take the organizational environment carefully into account in tendering advice.

[8] Wherever a system appears on the surface to be instrumental (based on hardware and its exploitable performance), there is some tendency to slide past so-called "human factors" in the equation. It would be generally conceded that central war forces are more instrumental in some sense than are general purpose forces. That is why it is both easier and more congenial for analysts to study the former rather than the latter. Nonetheless, even the central war forces are critically dependent on such elements as command and control systems, which are so tricky to assess. It is impossible to say whether the men who

less completely or unless one can crush his powers of resistance, one must be forced to recognize this simple fact: ultimately it will be necessary to have recourse to policy, which (to turn Clausewitz on his head) is a continuation of conflict by other means.

Whenever a disarming strategy is feasible, the application of force may remove most of the burden from policy and from policy assessment.[9] The cases in question never are perfectly clear-cut, but they are sufficiently different that we can distinguish them from those in which policy assessment becomes the heart of the problem. For example, in the '50s a nuclear war waged against the Soviet Union might have sufficiently disarmed the Soviets that they were quite susceptible to our pressures. Communist China is likely to remain in this position for an indefinite period into the future. However, the buildup of Soviet forces in the '60s has been such that we must carefully consider how the Soviets might employ their residual forces. As a result, any projection of a nuclear war and *any plan for conducting such a war* must rest on a highly subjective assessment of enemy responses. Similarly, a war of attrition against powerful industrial states like Germany or Japan, fielding well-organized and well-equipped forces highly dependent on substantial logistical support, is likely to have a specific terminal date. By contrast, we are becoming increasingly aware that in an engagement like the one in Vietnam, in which the opponent's forces are amorphous and difficult to discover, suppress, or destroy, to devise military systems studies which ignore the organization and psychology of the foe and of the population that he desires to control, is likely to be the basis of continuing self-deception.

Consider the problem of measuring progress in Vietnam. In such conflicts it is almost a truism that more than technology and economics are involved in assessing the effectiveness of forces. In fact, it has become almost a bromide in public statements on the question that in Vietnam "the political factor is dominant." Even though such assertions are questionable, at least in the sense that they are intended, it should be apparent that military studies will depend upon something more than the hard factors that analysts prefer to employ. Yet, observe the unsatisfactory set of mechanical indicators employed as criteria of military performance in Vietnam. Moreover, one might observe that the United States is rather vacillating in its choice among the set of mechanical criteria. A regular and uniform employment of all the criteria on a comparative basis might itself underscore the soft elements in the estimates.

In Vietnam, the use of "body count" [10] is intended to provide hard evidence of enemy mortalities—though some question persists regarding the count's accuracy. (The circumstances are hardly such as to invite careful statistical procedures, and inevitably there is considerable phony counting, double counting, and counting of extraneous bodies.) Nonetheless, from the data acquired in this way and from our own reports, an estimate can be made of *relative* casualties, and this is presumed to be a very revealing figure. Why the *ratio of casualties* is supposed to be dramati-

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are supposed to be alert, will be alert, or whether they will perform the actions that they should, or whether the actions will be correctly performed and at the right time. If these questions are ignored, the analyst may be able to fight a neat and well calculated war. On the basis of previous experience, however, one wonders whether such hypothetical conflicts, which appear to be drawn from a Platonic world of ideas, would have much correspondence with reality.

The clear inference is that, as soon as one gets away from the relatively simple type of production problems, it becomes increasingly misleading to view capabilities as purely instrumental.

[9] Even in these cases there is no indication that to rely completely on force ("unconditional surrender") is the wiser strategy; it is only a feasible strategy.

[10] All warring nations try to keep track of enemy dead. The only thing unique in our current behavior is that we are the first nation to advertise this procedure, not for purposes of propaganda, but as part of our data collection service.

cally revealing, but the *ratio of costs* (in Vietnam alone) has no significance at all—remains something of a puzzler. In offense-defense calculations, including such questions as deployment of ABM, cost ratios are presumed to indicate what is worthwhile or not worthwhile for ourselves or for the Russians. Currently in Vietnam cost ratios favor our opponents at something like 50 or 60:1 (and may go as high as 100:1), levels which certainly have been regarded as precluding other lines of activity. While in this case we may prefer to ignore cost ratios, we would be ill-advised to assume that other members of the international audience were displaying a similar propensity to ignore these unfavorable ratios.

The point is that whenever something is sufficiently important to us, we are prepared to jettison calculations on cost ratios. But our opponents in Vietnam, since their undertakings are important to them, may be equally prepared to disregard *casualty* ratios.[11] I believe the moral is clear: the valuations placed by each side on the outcome are more important than ratios, per se. Ratios by themselves are a poorish benchmark of how performance is perceived. High valuation of an objective may make unfavorable cost ratios inconsequential, whereas low valuation may make the most favorable ratios wholly unsatisfactory. Moreover; as has been shown, the ratios themselves are likely to lack internal consistency.[12]

In terms of capabilities the critical variable is one of scale rather than one of loss ratios. It is *absolute* losses that determine how long the insurgent forces can maintain the size, cohesion, and effectiveness of their units. To analyze staying power is a tricky problem involving estimates of motivation, dedication, and natural efficiency of enemy manpower. We have a few crude rules-of-thumb indicating the number of casualties a unit can absorb without losing its effectiveness, but these reflect regular fighting units of industrial nations in regular battles. There is no reason to assume their applicability either to covert warfare or to pure guerrilla warfare. To shed more light on Vietnamese conditions, we must deal more directly with the aggregative sources of enemy strength. Aggregate enemy casualties are an important input, but must be reinforced by studies of recruitment requirements, indoctrination requirements (especially relating time to effectiveness), training requirements (particularly the impact of the ratio of veterans to new recruits) and the like. Better understanding of these problems may reveal the critical points in the enemy's operation. In the past we have not known what was critical. Our actions in both North and South may have concentrated on items that were marginal to enemy efforts, and may have missed those places in which our action might be expected to bite into the foe's minimum capabilities.

When we turn to the higher-intensity wars, we can see once again how strategy and force posture become dependent upon one's political assessment of the enemy—once the feasibility of crushing the enemy's resistance is discounted. In analyzing central war, for example, the tendency has been to deal solely with spasm wars or minor variations of spasm wars, because the analysis of extended counterforce wars is inherently difficult and, in addition, depends upon speculative and unverifiable assumptions regarding enemy conduct. The willingness of the enemy to avoid cities and the length of the period in which restraint would be exercised are two crucial ingredients in designing forces and strategy. If city avoidance is to take place and a long-endurance counterforce war is the prospect, then the best allocation of the

[11] They may be buoying up their spirits by contemplating cost ratios, and gloating over the number of dollars it costs us for each dollar that they invest. On this matter there is an interesting table included in *The People of Vietnam Will Triumph!: U.S. Aggressors Will be Defeated!*, Foreign Languages Press, Peking, 1965, which suggests a capital-intensive objective function (brilliant victories against the imperialists through destruction of planes, ships, vehicles, trains, and structures).

[12] Even without doctoring the statistics—it should be noted that we do not include desertions from South Vietnamese forces in the overall casualty ratios. Our adversaries may take a different attitude.

strategic budget shifts from defense toward offense. One might invest more in both highly accurate missiles and in hunt-and-kill capabilities—the latter having a payoff only if the enemy reserves his forces. One would be correspondingly less inclined to allocate extensive resources toward ABM or toward blast shelters in urban areas.

To extend the case somewhat farther, suppose the United States were forced to consider retaliation against the Soviet Zone of the Interior (because of an invasion of Europe, for example). Should assault be inaugurated by a time-urgent attack against the main elements of the Soviet strategic forces or should one start with very modest, essentially demonstrative, attacks, hoping that the Soviets will cease their provocation? Perhaps the critical element is whether one believes that a heavy time-urgent attack would automatically trigger Soviet retaliatory forces against our cities—and whether a much lower-level attack would or would not elicit the same Soviet response. These questions are so difficult, not only because Soviet intentions remain obscure and little light is necessarily shed on them by Soviet declaratory statements possibly issued with the intent to deceive, but because the Soviets themselves may not know how they should behave in such a contingency. "Knowing your enemy" becomes a particularly challenging assignment, if the enemy does not know himself. One may not exactly require second sight, but the existing odd bits of political wisdom do not provide much illumination.

At a level of violence just below that of ZI exchanges is the question of European defense. Once again, differences regarding strategies both within the United States and among the members of NATO reflect in large part different assessments of the potential foe. The present complacence in Europe reflects the view, held more strongly there than here, that the Soviets are just not very aggressive and the detente is here to stay. An earlier, but widely advertised, continental view regarding a strategy of immediate nuclear response reflected a particular view of Soviet character and Soviet ambitions. It was held that (a) the Soviets would put sufficient credence in the threat that (b) they would be deterred. The Americans have been more inclined to fear that the Soviets could be adventurous, and, since this was a possibility, more eager to obtain capabilities that we would not be frightened to employ or that would not necessarily crumble.

Part of European resistance to the buildup of conventional forces and European disinclination to accept American manpower and equipment estimates reflects a vastly different image of the consequences of an engagement in Central Europe. European experiences with multinational forces have not been reassuring. Some NATO members recall that Allied forces on the Western front in 1940 were equal in number to the German forces, yet they were simply swept away. When there are basic disagreements regarding the potential of one's own side and the foe, it is hardly surprising that different strategic conclusions are reached.

On these fundamental issues of strategy, plans and hopes are so dependent on varying images of the foe.[13] Not only is it difficult to get a good handle on the problem (let alone a quantitative handle), but the images of the foe seem subject to rather sharp oscillation—much greater than the change in the foe's position would warrant. At one time the accepted image of the Soviets seemed close to that of "Commie rats who only understand force." Since the Kennedy years it has become more fashionable to accept an image of the Soviets in which there are counterparts on the other side who are just as urbane, civilized, and intent on the eradication of differences as are those on our side. These contrasting images point to contrasting policies

[13] Obviously, though perhaps unfortunately, the reverse is also true. Interactions are strong, and there exists an embarrassing tendency to envision as the true foe the one that existing plans are intended to deal with.

and military postures. Neither of these images, however, fully represents the character of Soviet society. The Soviets are neither willing to sacrifice all their achievements in the quest for world domination, nor are they urbane and civilized in the Western sense. Soviet behavior, no less than our own, is subject not only to alteration over time, but to oscillation as well. For the foreseeable future, we would be as ill advised to base our policies on the belief that the Soviets are inherently peace loving, as on the premise that they are inherently and ideologically aggressive.

Such shifts in the assessment of the foe are likely to have considerable impact on strategic decisions, force structure decisions, and even research and development decisions. For example, certain classes of capabilities have been rejected because of the *intuitive* feeling of the decisionmaker that such capabilities would create arms control difficulties and intensify the arms race.[14] There is, of course, nothing inherently wrong with basing decisions on such values. It has already been stressed that political elements both properly and inevitably play a role in higher-level decisions. What is desirable is that decisions not be made intuitively, and that the political assumptions be analyzed explicitly—in relation to the relevant data and conditions —rather than accepted implicitly. Higher-level analytical efforts normally suggest specific decisions, which should be scrutinized on the basis of their political implications. Yet, in moving from a technical analysis to a political decision, it is clearly appropriate to go through additional stages of analysis. Before a class of capabilities is rejected because it would upset the arms balance, for example, there should be careful analysis which convincingly demonstrates that in fact such capabilities would upset the arms balance.

There is a latent danger for analytical work at the point at which it links up with the decisionmaking process. Within the policymaking structure there is a natural reluctance to encourage or to permit extensive scrutiny of debatable points of policy. There is a tendency to smother the controversial problems, on which differences are fundamentally irreconcilable, under the heading of "science" or "method." In political debate one does not wish to admit the existence of speculative imponderables. One prefers to imply that one's opponents are committing incompetent errors because of an inadequate methodology. Unfortunately, systems analysis provides something of a facade of pure objectivity for political debate. While insiders know better, systems analysis has been presented to the public as an instrument that somehow "solves" problems. The upshot has been to obscure the unavoidable role of political imponderables in decisionmaking, and to discourage analysts from dealing explicitly with these imponderables.

IV.

Let us now examine the way that the economic factor interacts with the other elements—particularly the political and organizational questions. I am not here using "economic" in the sense that it is commonly employed in military systems studies. That is the sense of *efficiency* in which one strives for the least-cost method of obtaining a specific objective, given some moderately well defined isoquants and identifiable cost ratios. Here our concern is the "economic factor" in its broadest possible role: how a nation decides what it wants to achieve (in the sense of being willing to commit resources) and how it goes about achieving those objectives. We are concerned with how a nation identifies its utility function and how its *actual* production possibility curve is revealed—and how it may be predicted before it is

[14] In some cases the rejection may have reflected the view that money simply should not be made available, and the arms control rationale served as satisfactory cover.

disclosed. Thus, we are dealing with allocative questions in the oldest sense of political economy, issues about which we would say—in light of the tendency toward specialization—that the political and economic factors are inextricably mixed.

In these cases the economic factor, which is an independent input in the efficiency case, both qualifies, and is qualified by, politics, the specificity of resources, and the problems of organization. These will affect (a) the decision, the ability, and the costs associated with the development and deployment of a specific kind of capability, (b) the possibilities for actually implementing the necessary resource reallocation, and (c) the overall allocation of national resources. Let me address these subjects in ascending order of complexity.

(a) Consider first of all national acquisition of specific capabilities. Over the years we have been pretty consistent in exaggerating either the pace or the extent of buildup by other nations of specific military capabilities. The tendency of acquisition and deployment to lag has been true despite the *occasional* tendencies for foreign R&D achievements to take place sooner than anticipated. The Soviet bomber buildup, the Soviet missile buildup, the buildup of British, French, and Chinese strategic nuclear forces, all of these have been either slower or less extensive than anticipated. The consistent bias in our estimates should provide some impetus toward reflection: it seems reasonable to suggest that in developing new capabilities the costs tend to be higher and the organizational problems more intractable than relevant national planning groups characteristically anticipate. Not only does this apply most forcefully to other countries' planning, but even more frequently to our appraisals of the ability of others to surmount time and cost obstacles. We remain oblivious, in the case of others, to what we have begun to see, at least dimly, in our own case. In our developmental experience we have noted that both money costs and time costs tend to be disproportionately higher than predicted, the more we are attempting to push the state of the art.[15] Moreover, even when developmental problems have been solved and we are dealing with well-established technologies, both production and construction costs have tended to outrun the anticipated level.

The unanticipatedly high costs in time and money for both ourselves and others have an obvious connection with military buildups which are slower or less extensive than predicted. The United States has an abundance of resources, and a relatively high degree of flexibility in allocating those resources. Not infrequently our response to the discovery of overruns is to redouble our efforts (and expenditures) and thereby to surmount the cost obstacles by financial *force majeure*. The projected capability is then obtained at higher than predicted cost. On other occasions we have scaled down our force objectives. The point is that other nations, lacking our abundance and our flexibility, are likely to be faced with the same question in a form more cruel. A program that may have initially been sold by its patrons as a low-cost program has been transformed into one of high costs.[16] The nation in question will be forced to ask itself whether it really wanted so ambitious a program in the first place. By contrast, we ourselves, as we survey the program from afar, are likely to continually exaggerate the other nation's willingness to invest substantial resources.

The clear inference is that we should avoid the prediction of a specific buildup by another nation on the simple premise that the *resources could be made available.* There must be evidence not only of a general desire to acquire a capability, but of a dominating intention to override the cost, organizational, and technical problems

[15] Cf. A. W. Marshall and W. H. Meckling, "Predictability of the Costs, Time, and Success of Development," in *The Rate and Direction of Inventive Activity,* Princeton University Press, 1962.

[16] For some rather dramatic evidence in the case of British missile development, see David Divine, *The Broken Wing,* Hutchinson of London, 1966, pp. 311-341.

that inevitably will be encountered. Moreover, preferably one should be making predictions regarding the acquisition of a capability sufficiently limited in scope and total cost that it does not encroach on entrenched bureaucratic baronies or drain off too much in the way of resources.

In addition, to the extent that one is attempting to forecast for relatively backward countries, it is essential to avoid simply transferring to them the cost factors relevant only to the advanced industrial economies. Less developed countries are characterized by massively unproductive labor. For technically advanced and complex products their costs typically outrun those of, say, the United States by a factor of three.[17] Under the circumstances it is inherently deceptive to develop cost estimates for such countries through the use of U.S. analogues.

These problems are particularly intense with respect to nuclear spread and the evaluation of strategic capabilities—issues that have been my principal interest for some years. The tendency, at least in recent years, has been to understate the difficulties, underestimate the costs, and exaggerate the resources nations are willing to invest in developing nuclear capabilities. The scientific community has been particularly prone to error. No one ought to underestimate the invaluable assistance provided by scientists on the technical matters. Nonetheless, scientists tend to assume away what I would regard as the heart of the problem. Put quite briefly, many scientists seem intuitively to believe that scientific matters which are now obvious to themselves and their American colleagues (after enormous outlays on R&D and training) will quickly (and almost costlessly) become apparent to their counterparts in technically more backward areas. A scientist, after all, is a scientist, and so-called scientific "secrets," once they have been discovered, are not particularly recondite.

Within the U.S. atomic community, scientists have operated in one of the most unconstrained resource environments in the history of mankind. Rarely having encountered any substantial resource stringency, U.S. scientists have little understanding of what it means or how it can cripple technical advance. Consequently they have little feeling for the fundamental difficulties that their counterparts in less favored nations will face. Furthermore, there is a strong penchant in the scientific community to discount those mundane (but terribly costly) problems falling outside the realm of scientific discovery. Once a problem has been solved *on paper,* the engineering problems are regarded as secondary. There is little perception of how primitive engineering may be in some countries with established scientific communities.

Finally and perhaps most important, assuming that the scientific and engineering problems can be solved, there is almost zero appreciation of the severe difficulties that must be surmounted in the areas of industrial organization and bureaucratic organization. These problems are frequently regarded as inherently negligible.[18] But in reality the problems of organizing production are likely to be the most intractable of all. At the lower end of the spectrum nations that have difficulty organizing factories that produce light bulbs or books of matches with adequate treatment of quality control are not likely to find the production of advanced weapons an easy matter. For more advanced industrial countries of the middle rank, the problems of inadequate resources, minuscule R&D programs, and short production

[17] For some relevant numbers, see Jack Baranson, "Transfer of Technical Knowledge by International Corporations to Developing Economies," *American Economic Review,* May 1966, p. 262.

[18] The fact that they are continually encountered in the real world is likely to be attributed to the incompetency or sheer obstinacy of the nonscientific types typically charged with the responsibility in these areas. Ultimately the problems of organization—in the scientific, as in the Marxist view—will "wither away."

runs, taken together, imply small, unsophisticated, and costly capabilities—inadequate for their presumed strategic objectives.

It is not only the tendency to transfer or ascribe to other nations production frontiers for advanced weapons equivalent to those of the United States; the problem goes beyond that. There is a strong tendency to attribute to other nations cost factors which are *more* favorable than those in the United States, to assume that a large proportion of the costs that the Americans (for some inexplicable reason) encountered can be avoided.

The overall result is drastically to understate the costs of acquiring a serious nuclear capability—and here comments by public pundits greatly compound the offense. Rather than full system costing, little bits and pieces of capability are examined by themselves, such as the cost of plutonium (frequently highly contaminated with Pu^{240}), the R&D cost of very primitive design, and the like—and the alarms are sounded. The possible lack of correct technical concepts, the possibility of false leads, the time and money costs and the possible errors implicit in bureaucratic decisionmaking, the costs of acquiring production facilities, the complexity of adequately efficient industrial organization, the uncertainties of imprecisely or inaccurately defined objectives, and the heavy costs of deployment, maintenance, and operations—all these are understated or even ignored.

To estimate the chances for national acquisition of a specific capability, we must at a minimum make the effort to get *full systems costing*. In addition, we must do more than vaguely ascribe a desire to acquire the capability. To do so means inappropriately to isolate a single desire, to pretend that the nation in question does not have offsetting desires, and to assume that policy will fall under the influence of those pushing for the specific capability rather than the many other groups which will be resisting it. In practice, we must discern a determination (and an ability) to carry through a program irrespective of difficulties. On the nuclear front, there are very few countries for which we can say with certainty that this is the case—and that is the reason the pace of nuclear spread has been so much slower than has repeatedly been predicted.

(b) In predicting the extent of a specific deployment and the corresponding willingness to invest resources, the bureaucratic organization in the other nation should be recognized as supplying a critical and potentially decisive ingredient. Yet remarkably little serious work has been done on the bureaucratic aspects of national decisionmaking. On the level of political gossip, almost everyone recognizes the importance of factional conflict; we ascribe particular causes to particular personalities and groups and watch carefully who is rising and falling. Yet, whenever an attempt at prediction is made, it becomes convenient, emotionally satisfying and possibly bureaucratically rewarding to fall back on a monolithic interpretation of what the national actors on the international stage will do. Among analysts from various disciplinary backgrounds there exists a strong inclination to treat policymaking in other nations as if it were governed by a rational and unified deciding unit.

The reality is very different. Decisions are *nominally* made by senior political figures who are harried, have insufficient time to study problems in detail, who are gripped by emotions of their youth or by prior experiences, and who are susceptible to claims made by subordinate groups which are couched in a way to appeal to their prejudices. Below them are a set of mutually jealous and warring bureaucratic groups, clamoring for resources and anxious to protect established preserves. To the extent that they are not closely watched, the subordinate bureaucratic groups will attempt to achieve their objectives quietly or even surreptitiously. Moreover, their capacity for resistance to high-level objectives enunciated from above, but to which

they take exception, is breathtaking. Actual programs and allocative decisions will consequently diverge quite sharply from those that would be predicted on the assumption of a rational intelligence. Instead they will be strongly influenced by prejudice, sheer incompetency, and by outright fights, deviousness, and bootlegging within the bureaucracies. Changes which appear rational and desirable will be compromised half to death, and the compromises themselves will be slow in coming. Traumatic events—like Sputnik, an initial nuclear detonation, or the invasion of Korea—may speed up the process of change, but the capacity for resistance will remain formidable.

The decisive battleground in the competition between new proposals and continuing programs is not on the level of ideas but on the level of budgets.[19] New ideas may be accepted, in principle,[20] and even thundered in national policy statements. The national leadership may believe it endorsed a new concept and may believe that the concept is being implemented. But the place for the established bureaucracies to cut ambitious new programs down to size is in the budgetary infighting. It is a truism that the demand for resources far exceeds the supply. To cut old programs sharply would be a radical move. A program for a new capability finds it difficult to obtain a Darwinian niche in the budget structure and to expand upon it—in the face of established programs which may become masked under imitative labels.

Even when a program is objectively obsolescent, its established position indicates that it satisfies some deep-seated emotion in the society. New, if doubtful or fraudulent, stories can be cooked up to play on the old emotions. For example, one can readily imagine that the never-underfinanced Soviet air defense forces may for years have attempted to protect their appropriations by pointing to the B-70—and demanding the resources to cope with this new but inherently so-easy-to-manage threat.[21] It hardly needs mentioning that similar examples could be recounted for the United States. This is the way that bureaucracies operate: they concoct some issue, possibly fallacious, and thereby preserve or expand their historical share of resources. New growths find it tough going in a badly weeded garden.

To ignore these realities of bureaucratic life will repeatedly lead the analyst into error. It is inadvisable to predict the introduction of new capabilities or the restructuring of forces on the basis of the obvious desirability of such restructuring or on the basis of public statements by high officials that such restructuring will take place. Bureaucratic resistance, either overt or covert, is likely to make these predictions invalid. This much, at least, we can say right now. Beyond that, we can encourage careful research on bureaucratic structure and its operation in particular countries.

(c) Economic aggregates, including broad resource allocation and the growth rate, are reflections of countless microeconomic decisions. Assessing overall resource allocation for other countries becomes easier—to the extent that we can correctly estimate the flow of resources into specific sectors or capabilities. In one sense predicting overall resource allocation is riskier because of the possibility of compounding errors in summarizing numerous individual decisions. In another sense, such prediction is less risky because of the limited number of degrees of freedom and because of the realistic limits in varying such an aggregate as "consumption."

[19] For a revealing study in the Soviet case, see J. E. Loftus and A. W. Marshall, *Forecasting Soviet Force Structure: The Importance of Bureaucratic and Budgetary Constraints* (U), The Rand Corporation, RM-3612-PR, Secret.

[20] To resist new ideas, in principle, may be politically imprudent.

[21] On February 20, 1967, General Pavel A. Kurochkin indicated that "missiles fired at the Soviet Union would never reach their targets." In response to a question he asserted that "detecting missiles in time and destroying them in flight is no problem." *(The New York Times,* February 21, 1967, p. 5). Observers of bureaucracies recognize that the hard sell is not confined to commercial wares.

Nonetheless, some assessment of trends among the broad aggregates is essential for estimating future capabilities and attitudes, and the relative stature of nations.

Resources flowing into investment help to determine the growth rate (whose long-run importance needs no stressing), and also the buildup of specific critical industries. Resources flowing into defense determine, at least potentially, the nation's military power. Resources flowing into consumption may determine the level of satisfaction and stability in the society—and may indicate how contentious a nation will be on the international scene. Moreover, the trends over time are of equal significance, for they strongly influence the number of new opportunities that can be seized and the quality of resource employment. The value of good estimates on overall resource usage hardly needs to be stressed, but there are a number of pitfalls to which attention should be called.

First, there has been within economic doctrine a time-honored view, expressed quite clearly by the mercantilists, that nations obtain strategic advantage from their economic activities in accordance with the size of some "surplus." In more recent times this notion has been formalized as the doctrine of Economic Potential for War. The underlying notion is that some minimum of resources must be devoted to keeping a society going—in the form of consumption, ordinary government expenditures, and the like—but that all above this minimum can, in principle, be devoted to strategic purposes. The limitations to such an approach to immediate conflict are obvious in the present era due to (a) the overriding importance of forces-in-being[22] and (b) the limited range of potential product, given specific industrial structures associated with the long lead time necessary to change those structures. On the other hand, the Economic Potential for War still seems to have considerable relevance for assessing possible trends in strategic rivalries over the very long run. In light of the structure of international power and politics at the present time, these long-run trends in strategic rivalries appear to be of increasing importance. It is this economic feasibility aspect of overall resource allocation, rather than the obvious limitations of EPW for short-run rivalries, that I want to assess.

Put quite bluntly, the limits of economic feasibility, especially in the completely hypothetical sense as derived from the national income accounts, is a misleading basis for making force estimates. What one is doing, in effect, is to select one rather extreme case out of an infinite number of possible cases of resource allocations, and to concentrate on it exclusively. It is extreme in the statistical sense that its probability of occurrence is out beyond the three sigma point.[23]

To assume, for example, that in a totalitarian society consumption will be held at some prevailing low level or permitted to rise by only a fraction of the growth rate—and that all the rest of society's resources will be devoted to strategic purposes —is to assume something for which there is very little evidence in modern history and none outside the demanding period of war. The difficulties are numerous. First, it is to assume that the sole concern of a society is external conflict, and that the society either has no other objective or those other objectives are without internal strength. Second, as recounted above, it ignores the rather stringent bureaucratic limitations. Any attempt to achieve massive reallocation of resources is bound to encounter frustration. When the real economic bite comes, one can count on lengthy debate, delay, and covert resistance from established bureaucratic entities.

Third, the hypothetical statement of how much could be diverted to military purposes, broadly construed, will very much overstate what can *effectively* be diverted outside of a war situation. It is sometimes suggested that the "surplus" can be

[22] Save for very-low-intensity conflicts.

[23] Less than one chance in a thousand (more precisely, less than a probability of .00135)—*Ed.*

diverted in large measure to the production of military equipments. However, if we examine the case of World War II, when some forty or forty-five percent of the GNP in various countries was being diverted to war purposes, we discover that a very large proportion of those expenditures was devoted to operations and maintenance and logistical purposes of a type differing little from peacetime. Much of U.S. military expenditures went to support the ten percent of the population that was in the armed services, to support a worldwide transportation net, and the like. In such cases the "military purposes" were not much different from "civilian purposes." To suggest that a high proportion of GNP can be devoted on a continuing basis in peacetime to military equipments, per se, strikes me as simply misleading.

A principal reason is that in extended periods of peace, as opposed to brief periods of major war, there is a clear-cut conflict between allocations to military equipments and support and investment for future strategic *and* civilian purposes. It is improper to set up an economic model which ignores the impact of military allocations on investment and growth—especially so for those countries that have not reached a high level of industrial attainment. A study which stipulates a likely rate of growth, which uses the stated growth rate as the basis for projecting national production available in the future, and which then *freely* proceeds to allocate a very high proportion to military purposes and especially military end-products—without reexamining the growth rate—misses a main element in the problem. *Ceteris paribus* simply cannot be assumed. In principle, one should pair off higher military allocations with lower growth rates. Thus, in looking toward future years, a given percentage of GNP going to military could imply lower aggregate resources going to the military function.

Finally, one errs, if, in estimating the growth rate and in pressing the limits of economic feasibility, the high probability of setbacks is ignored. If we had been sensible, we should not have been surprised that the Chinese would encounter institutional difficulties—given the enormous pressures that their system generates. My former colleague, G. W. Nutter, may overstate the case when he argues that the period of War Communism and the liquidation of the kulaks were inherent reflections of the Communist system—so that no adjustments should be made for such "inevitable" occurrences in assessing Soviet economic performance. Nonetheless, all systems have their "contradictions." To disregard the likelihood of setbacks, when making long-range predictions, is to indulge in a bland form of utopianism. Particularly is this the case when—as in Communist China—managerial talent is short, central direction frequently primitive, and ideological emotionalism high.

For all these reasons, caution should be the keynote in making long-range projections of the growth of the major economic aggregates.

V.

What this long discussion has established, I trust, is the expanding risk of applying the semi-orthodox methods of systems analysis as one moves up the ladder from low-level decisions and instrumental capabilities to problems which increasingly involve the total society. The chances that really excellent studies may result in misleading policy decisions increase greatly. But technical studies are not one's sole support; they ought fully to be supplemented by additional studies on policy issues taking into account a wider range of factors. The point of danger, the point to be carefully watched, but also the point of maximum opportunity, is the one at which the transition occurs from analytical studies to decisionmaking.

Even in this phase the role of traditional analysis is not negligible, for analysts then become a highly useful, if another, pressure group. Their function is to see that

appreciation of the technical factors is not washed away in what may become the emotional catharsis of policymaking.

There are, of course, dangers in a partial view, however well grounded in logic it may be. On the big issues a society's response may be erratic, even compulsive, but the elements of change are likely to be interdependent rather than stochastic. The analyst's habits of mind, stressing the disentangling or separating-out of supposedly independent factors, may here be irrelevant under conditions such that the premise of *ceteris paribus* does not apply. Realism about policies frequently demands that we recognize when hypothetically separate elements are hopelessly entangled. At such times, the analytical intelligence may point to more options than naturally exist. When the nation is caught in some situation, an analyst may propose certain alternatives in the intent to improve our position—while assuming that we can back off, if the attempt fails. In such cases the analyst, in his search for options, fails to recognize that ill-considered attempts to increase flexibility may decrease flexibility. When a nation is committed to some course of action, the sucking-in process may be intensified by the search for improvements. Certain policies—the bombing of North Vietnam, for example—cannot be taken up and dropped on an experimental basis, for these are not tools alone, but great historical events that determine the total response of society.

Issues of this sort will be of growing interest to us. Analysis remains in its infancy. There is much to be learned regarding how better to take non-technical factors into account. To the extent that such factors cannot be taken directly into consideration, we can learn to be more alert to occasions of suboptimization—and to qualify analytical conclusions accordingly.

SYSTEMS ANALYSIS AND THE POLITICAL PROCESS

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My purpose in this paper is to evaluate the role for systems analysis—particularly as it functions in a highly politicized environment. I shall not devote any attention to discussing whether cost-effectiveness procedures are hypothetically desirable. Far too much attention—in Congress and elsewhere—has been wasted in this strange dialectical tilting ground. Viewed abstractly, systems analysis implies rigorous thinking, hopefully quantitative, regarding the gains and the resource expenditures involved in a particular course of action—to insure that scarce resources are employed productively rather than wastefully. It is almost tautological therefore to state that systems analysis effectively employed will be beneficial. The real questions arise when we descend from a high level of abstraction and begin to grapple with the practical issues. Attention must be given to such questions as (1) the quality of information bases and analyses, (2) methodology, (3) bias, (4) the impact of politicized environments on analytical efforts and analytical results.

These issues cannot be treated wholly in isolation. The quality of information, for example, is very much influenced (and biased) by the structure of and alliances within the bureaucracy. The methodology chosen for analytical efforts will in itself introduce a specific form of bias. These, in turn, reinforced by the specific interests and functions of separate sections of the bureaucracy, will increase tensions within the Government and make more costly the introduction of changes which might objectively be regarded as desirable. Nonetheless, the effort to sort out different classes of issues must be made. One may categorize issues (1) and (2) as "mechanical" and issues (3) and (4) as "organizational." Without implying a judgment regarding the relative importance of these problems, it is plain that a paper directed to political scientists should concentrate on the latter class of problems. After a few words on the way in which the data base and methodology may influence the quality of analysis, the balance of the paper will be devoted to the implications of these broader organizational issues.

Where gross wastage and irrationality have flourished it is relatively easy (in principle) to indicate very much improved patterns of resource allocation even in the face of rather skimpy data. In all other cases the quality of the underlying data will determine the quality of analysis. The fact must be recognized that the data presently available to the Government for analytical work are not in good shape. One of the reasons for the success of systems analysis in the Department of Defense under McNamara is that considerable prior effort had been invested in the development and study of the data relevant to defense problems. For most of its other functions the Government faces an uphill fight simply in developing useful data.

In part, this problem will yield to steady effort, especially as more trained personnel become available. However, it would be utopian to expect agencies automatically to provide data useful for analytical purposes. Knowledge is a form of power, and most institutions exhibit an understandable reluctance to dissipate this power in the absence of compensating advantages. While newer or favored agencies,

which anticipate expanded budgets, are likely to prove cooperative, the old-line agencies, especially those that have established a degree of independence, are likely to prove obdurate. In many cases, data of appropriate quality can only be obtained through the wholehearted cooperation of the relevant agencies. Since the indicated tactic for many agencies will be to hide some information and to release much of the balance in warped form, many decisions will continue to be based on deficient information, with only limited confidence being placed in the results.

The problems that established methodology can create ought not be ignored, even though a sense of proportion suggests that in relation to the enormous potential payoff of systems analysis the errors attributable to methodological bias should be relatively small. While, at its best, systems analysis insists only on "rigorous thinking," the background of systems analysis in lower-order operations research problems has resulted in a lingering preference for formal models, preferably mathematical. In numerous cases this leads to the neglect of important variables which are not readily subject to manipulation by the existing methods. The normal association of model-building and simplification cannot be avoided in analytical work in the social sciences, but there is cause for concern if such analytical work becomes the sole basis for decisionmaking.

The stress on quantifiable elements is particularly risky in cost-benefit work where objectives are hard to define or subject to change. In most cases the cost elements can be reduced to money terms. By contrast, objectives may be numerous, mutually incommensurable, and reducible to money terms only on the basis of rather arbitrary and subjective judgments by the analyst.[1] The result is that what started as a cost-benefit analysis becomes primarily a crude cost comparison—with inadequate attention either to a number of the potential benefits or to the adaptability of the preferred alternative to a number of unforeseen contingencies. Countervailing tendencies toward prodigality in pursuit of misconceived or ill-defined objectives may bulk larger overall, yet there is no assurance that such tendencies will serve as direct offsets to the biasing of specific analyses toward the choice of the low-cost alternative. When and if systems-analytical work becomes routinized, the risks implicit in methodological bias will rise.

As distinct from methodological bias, the more general forms of bias reflect the pressures of a large and variegated organizational structure. Among the causes of bias are: asymmetry in the sources of information, disproportionate attention by the analyst to preferred information sources, prior intellectual commitment on the part of the analyst, selectivity in organizational recruitment, and other bureaucratic pressures. From these sources a great deal of bias, reinforced by slipshod and mechanical work, inevitably slips in, even on those occasions that it is not deliberately introduced. It scarcely needs saying that in so complex an organization as the United States Government, viewed from its highest levels, the deliberate introduction of misinformation and distortion is no insignificant problem in itself, as will be seen below. The point being made here is that a very large proportion of total bias springs from honest conviction rather than the attempt to deceive, and it is particularly difficult to compensate for bias in this form.

Contrary to a widespread hope, the solution does not lie in the training and upgrading of personnel—in getting more honest (or more intelligent and capable) personnel. The most damaging forms of bias spring from an honest, if misguided, conviction of the correctness of one's own views. Where biases clash they may be

[1] It is infeasible to go into the criterion problem at any length. Suffice it to say that for most higher-order problems adequate measures of merit have yet to be devised. Even if some improvement may be hoped for, the irreducibly subjective element in such measures will remain substantial.

viewed with less apprehension under the classification of the "competition of ideas." But all too frequently biases are mutually reinforcing. And, in any event, the introduction of bias (inevitable in all save the lowest-order decisions) contaminates the detached and quantitative analysis which a widespread myth holds to be attainable.

The final question bearing on the effectiveness of systems analysis for governmental decisionmaking is the impact of politicized environments on analytical efforts and analytical results. The deliberate introduction of distortion and fuzziness to improve the competitive position of one's own agency or division is an unavoidable and dominant feature of the bureaucratic landscape. At lower levels the tendency to pick and choose those data which support one's position results in analyses which may be uncritically accepted at higher levels, if the conclusions are palatable. Only if the conclusions are unpalatable, will searching questions be raised regarding the underlying data. Not infrequently, the very agencies whose premises are most questionable, are the very ones which are most adept in handling the new quantitative tools, and in developing a superficially convincing presentation that may beguile those charged with responsibility for review.

The techniques of deception are legion; the effectiveness of intelligence operations and the available sanctions, frequently low. In the variegated structure of the Government (with innumerable agencies and subagencies), deliberate distortion is reinforced by honest conviction, bias, recruitment, limited information, and the structure of power. It becomes impossible to separate one such element from another. In a perpetual rutting season, these mutually reinforcing tendencies coagulate in their separate sectors of the lattice structure of the Government. How much systems analysis can do to counteract the pernicious results of such coagulative tendencies remains an open question. Certainly it can accomplish something—hopefully a great deal. Nonetheless, the resistances to the application of systematic and rigorous analysis in a highly politicized environment are sufficient to make even the stoutest heart grow faint. Our purpose is to examine how analytical techniques will fare in this political environment. Let us consider four aspects of the problem: (1) the general limitations, (2) the relevancy of experience in the Department of Defense, (3) bureaucratic problems in a wider compass, and (4) what systems analysis can accomplish.

GENERAL LIMITATIONS

With perhaps a tinge of self-satisfaction on the part of its practitioners, systems analysis has been advertised as the application of logical thinking to broad policy issues. The implication is that logic comes in only one guise. Yet, whatever the doubts of those who seek to rationalize politics, the political process is dominated by a species of logic of its own, one that diverges from the brand germane to systems analysis. The domain of politics is a far broader system than that to which systems analysis is typically applied. Systems analysis applies to substantive issues susceptible to definition, where linkages exist among costs, technologies, and closely related payoffs. The criterion is some substantive (and presumably measurable) utility which is more or less directly relevant to the enhancing of national security or citizen well-being. The pride of systems analysis is its ability to take a long-run view and to disregard prior commitments, if they are too costly or nonproductive.

By contrast, in politics one is concerned with more than the substantive costs and benefits involved in a specific decision area. One is engaged in mobilizing support by words and by actions over a wide range of ill-defined issues. The ultimate criterion will remain the psychological and voting responses of the general elector-

ate and of important pressure groups. Positive responses in this realm are only irregularly correlated with those actions preferred on the basis of cost-benefit criteria. The focus of political action tends to be the short run. The wariness with which the approaching election is watched is tempered only by the precept that the half-life of the public's memory is approximately three months.

Put quite briefly, political decision operates under the normal constraint to avoid serious risk of the loss of power. The tool of politics (which frequently becomes its objective) is to extract resources from the general taxpayer with minimum offense and to distribute the proceeds among innumerable claimants in such a way as to maximize support at the polls. Politics, so far as mobilizing support is concerned, represents the art of calculated cheating—or more precisely how to cheat without being *really* caught. Slogans and catch phrases, even when unbacked by the commitment of resources, remain effective instruments of political gain. One needs a steady flow of attention-grabbing cues, and it is of lesser moment whether the indicated castles in Spain ever materialize. The contrast to the systems-analytic approach with its emphasis on careful calculation of resources required to implement real alternatives could not be greater. In political decision, the *appearance* of effort, however inadequate, may be overwhelmingly more remunerative than the costly (and thereby unpleasant) implementation of complete programs.

Consider two of the guiding principles of systems analysis: (1) the avoidance of foot-in-the-door techniques leading to an unintended commitment to large expenditures, and (2) the orientation of analysis and allocation decisions toward output rather than input categories. These go to the heart of systems analysis with respect to the quest for the proper relating of resources provided and goals adopted. Output-orientation is designed to measure the extent to which adopted goals are actually achieved. Avoidance of foot-in-the-door is designed to prevent the preliminary wastage of resources on purposes for which one is unwilling to pay full costs. These are laudable principles, but they conform poorly to the realities of political decision.

Politics, it was hinted above, requires the systematic exploitation of foot-in-the-door techniques. One wishes to attract current support from various voting groups by indications or symbolic representations that the government will satisfy their aspirations. One wishes to attract the support of many groups, but there are limits to the size of the budget. Consequently, resources are applied thinly over a wide array of programs. The symbolism of concern is enough and the last thing that is desired is the totting up of the full costs of a program with the implication that one should not go ahead unless willing to incur the costs involved.

Similarly, in the real world of political decision it is immensely difficult to concentrate on outputs rather than inputs. A very large proportion of political pressure is concerned with the sale or preservation of specific types of socio-economic inputs. The preservation or expansion of vested interests implies that political decision will be much concerned by and may be overwhelmed by inputs rather than outputs. No doubt, the behavior of politicians reflects a total disregard of Kant's categorical imperative, but that viewed realistically is the name of the game. Classical liberals may stress the desirability of advancing one's component of the general interest rather than one's special interests, but it requires no great amount of shrewdness on the part of politicians to see that such behavior will not lead to political success. The systems analyst may search for new and more efficient means for achieving objectives, but these new means are by definition likely to have little political support both within and without the Government, depending on the affected groups. Both within and without the Government (depending on the locus of affected interests) the opposition to new methods will be powerful. Consequently political leaders who are interested in maintaining a consensus (as all political

leaders must be) must continue to pay close attention to input-oriented interest groups.

As a result, there is an inevitable note of paradox when systems-analytic techniques are endorsed at the highest political level. For such an endorsement implies, in principle, the partial renunciation of the most effective tools of the politician. That systems-analytic techniques are being diffused throughout the Federal bureaucracy in response to a directive[2] of President Johnson is both understandable and ironical. It is understandable in that the pressures for sensible use of resources will be most keenly felt during an administration with high aspirations and expanding programs (much more so than, for instance, in the Eisenhower administration). Yet, it is also ironical in that no recent administration has been more alert to the direct political implications of domestic programs.

Lyndon Johnson prides himself on the widespread recognition of his superb political instincts—and on his understanding of what makes the electorate click. Repeatedly he has extracted political gain through the announcement (during the low-cost initial stages) of new programs—before the costs have been thought through or the bills presented for payment. Though this be the political replica of what the analyst decries as foot-in-the-door techniques, few political leaders will be restrained by such an observation. Politics is geared to the hopes of the voters rather than to the calculation of the cost accountant. In politics one is almost driven to overstate the benefits and understate the costs of controversial programs.

The keynote of the Great Society has been the launching of new programs associated with substantial increases in Government expenditures. Goals have been announced (like the elimination of poverty) before the means of achieving them have been developed. Neither alternative policies nor the costs have been studied until *after a decision has been reached.* No one would suggest that such programs as "demonstration cities" [3] or rent subsidies have been carefully analyzed with respect to benefits and cost, especially in relation to the alternative employment of the same resources. My point here is neither to ascribe praise or blame to what is effective politics, nor is it to raise questions regarding the merits of the programs themselves. Rather it is to suggest the inherent difficulties of reconciling such procedures with the precepts of systems analysis.

These problems are not new ones. For generations men have sought methods for introducing more "rationality" into the government allocations. Systems analysis is a powerful technique, but like all techniques, it will be germane only when there is a willingness to employ it systematically in dealing with issues of public policy. In fact, systems analysis is only the latest in a series of attempts to achieve more rational allocation. Moreover, prior attempts bear at least a family resemblance to what we now propose to do with systems analysis. For example, Public Law 801, passed in 1956, required the presentation of five-year cost estimates when new programs were adopted. The five-year cost estimates have a familiar ring, but the law is a dead letter. It has been ignored, not because it is undesirable, but because it expresses a pious hope but disregards the underlying realities of political life. Once

[2] For details see Bulletin No. 66-3, issued by the Bureau of the Budget on October 12, 1965.

[3] "Doing more for the cities" has become the latest arena for political competition. The new programs are to be superimposed on the old without too much study. Indicative of the preexisting casualness in the attitude toward costing (one of the two legs of cost-benefit analysis) are two recent items bearing on the Federal Government's urban programs. First, in testifying on New York City's budgetary problems, Mayor Lindsay was unable to indicate "what is the total Federal figure?" (Senator Kennedy's words)—in assistance to the City. No one was able to establish whether Federal contributions were closer to the half-billion-dollar mark or the billion-dollar mark. Second, Senator Abraham Ribicoff, whose subcommittee is investigating the problem, stated in an interview: "No one really knows how much we are spending on the program to help cities. . . . What are these programs doing? What should they be doing? Have the cities the men to spend this money properly? What have they duplicated, what have they wasted?"

again it suggests the barriers of imposing upon political decision a method for efficiently using resources to provide direct, substantive benefits.

There is an old yarn concerning a farmer who was approached by an enthusiastic extension agent pushing a new technique which allegedly would raise the farmer's output by 10 percent. The farmer is supposed to have replied: "I'm only farming half as well as I know how to, right now." It was just too much bother to take advantage of opportunities for improvement. There is a moral in the story for the improvement of the operation of the Government. In many, perhaps most, lines of activity, we already know—even without systems analysis—how to improve efficiency and shave costs by eliminating obsolescent activities. In principle, we could easily do far better. The problem is not absence of knowledge; it is rather that appropriate actions are constrained by political factors reflecting the anticipated reactions of various interest groups.

In such lines of activity, if analysis is to be useful, it will not be by contributing to knowledge, but rather by serving as a political instrument through which the relevant political constraints can be relaxed. This is both a more modest and a more ambitious objective for systems analysis than is generally stated, but it is suggestive of the true role that analysis can play once we recognize the serious limitations imposed upon it by the political process.

THE RELEVANCY OF DEPARTMENT OF DEFENSE EXPERIENCE

The application of cost-effectiveness techniques in the Department of Defense since 1961 is regarded as a model for reform. While unspoken, there exists an underlying premise that "what's good for DoD is good for the rest of the Government bureaucracy." While this is, of course, true with regard to the *role* of analytical probing, it is not necessarily the case with regard to the *implementation* of analytical results. It is necessary, therefore, to explore certain differences between the Department of Defense and other elements of the bureaucracy. Moreover, we should examine the actual workings of the new procedures in the DoD, for a somewhat idealized picture has been disseminated which diverges in part from the reality. In so doing we shall be stressing the structural and political aspects of decisionmaking rather than the substantive issues that have been in controversy since 1961. In a sense, this represents an injustice to Secretary McNamara and his aides, for omitting reference to the substantive issues ignores the truly remarkable way in which the new team took hold with respect to the main strategic and postural issues in 1961.

Controversies regarding budgetary allocations in defense are fought out *within* a single Department. Outsiders, even the Congress itself, have only a nominal influence on allocation. Since the Defense Reorganization Act of 1958, the Secretary of Defense has had sufficient authority to impose his will on the Services. Moreover, the DoD does not supply final goods and services highly valued by influential portions of the electorate, nor is its use of specific inputs such that affected interest groups are normally in a position to block specific allocative decisions. In the United States the military has a relatively weak political position. In the absence of influential public support, the traditional tactic of cultivating Congress is inadequate. When the Executive Branch stands firm behind its budgetary decisions (whether based on sound analysis or not), the military has no real alternative to accepting the

decision.[4] The means of direct resistance, available to other components of the bureaucracy, are largely denied to the military.

Consequently the Department of Defense, relative to other components of the bureaucracy, has provided an abnormally easy place to apply program budgeting and systems analysis. Only in the case of the closing of the obsolete or redundant bases were vested interests sufficiently involved to require major political courage to override. With the support of the President, the Department of Defense can follow *internally-generated* guidelines, rational or otherwise, with only ineffectual resistance from below or outside. Moreover, the bulk of Defense's allocative decisions are internal to the Department. The linkages to allocative decisions by other Departments or agencies are relatively weak, by contrast to the major civilian programs.

For those civilian programs in which improved-performance-through-analysis is hoped for, the situation is far less favorable. A number of the newer Departments represent a gathering-in of preexisting entities, with the tradition of independence and outside sources of support serving to sustain that independence. The Secretary is in a weak position to impose decisions; he is rather like a weak feudal overlord attempting to control some ill-governed baronies. The equivalent of the Defense Reorganization Act of 1958 does not exist to establish the authority of the Secretary. This condition applies, moreover, to some of the older Departments in which nominally subordinate units are in reality independent baronies.

The services provided by the various bureaus and agencies regularly create clienteles within the electorate, whose interests it is politically risky for the President to override in preparing his Budget. These interests are strongly represented in Congress, and even a bold President could not afford to take on too many of them within a brief span of time.

The weakness of the Departments, relative to the DoD, implies that their allocative decisions cannot be based upon *internally-generated* guidelines. Consequently guidelines must be imposed from above, which is both difficult and politically risky for the President and his principal aides. More important, the appropriate analytical and decisionmaking domain is much broader than the individual bureaus and agencies in question. There are important linkages and spillovers in costs, in technologies, and particularly in payoffs across agency lines. The improvements to be obtained by intraorganizational changes are small relative to those obtainable by interorganizational adjustment. This is particularly dramatic, for example, in the natural resources area. Here the Bureau of Reclamation, the Corps of Engineers, the National Park Service, the Forestry Service, the Bureau of Land Management, and the Bureau of Mines are only the more prominent among the *Federal* Agencies involved (whose activities must be reconciled with such State entities as the Texas Railway Commission). Each has a position to maintain and a "suboptimizing" mission to perform, and as we shall see later, the concept of that mission is frequently based upon obsolescent views and obsolescent professional functions. Each, moreover, is involved in a symbiotic relationship with a clientele, which it partially supports and from which it gains significant political backing.

The "systems" to which "analysis" should be applied are far broader than the ones which are the concern of the existing entities. Yet, the existing organizational structure makes it virtually impossible to implement the recommendations which

[4] While this judgment conflicts sharply with the interpretation represented by General Eisenhower's "military-industrial complex" or C. Wright Mill's "power elite"—to say nothing of the standard Leninist view—I believe that the evidence will bear out that only in periods of national hysteria does the "complex" have much influence on broad defense allocations. One need inquire only into what has happened to the Strategic Air Command under McNamara, and compare the results with the many long-lived and obsolete civilian programs.

would come from good analyses. Thus, the underlying question remains: How strong is the will and ability to achieve a modernization of the structure of the Federal Government?[5]

To this must be added one final point. Both intensive and extensive research had been done on the problems of defense before 1961. This body of research was available to Secretary McNamara when he began to introduce his reforms in 1961, and the reforms underlay many of the decisions regarding allocations. For most of the civilian programs, very little policy-oriented research bearing on allocative decisions has been done. In some areas the problems have not even been formulated. Consequently, there is no capital of preexisting research to be milked. It may be years before adequate analyses have been performed. While in no way does this suggest that analytical effort should not be pushed, it does suggest that our expectations should not be pitched too high with respect to immediate benefits.

Let us turn briefly to consider the other relevant aspect of DoD experience: the actual workings of the evaluative procedures as opposed to the idealized model. In understanding the results we must bear in mind that analytical work is performed and decisions are reached, not by disinterested machines, but by individuals with specific views, commitments, and ambitions. The normal bureaucratic tendencies may be weakened, but will not disappear. We might anticipate the following.

- Where centralized evaluative procedures are applied, certain proposals, towards which the reviewers are predisposed, will be subject to less rigorous scrutiny than will other proposals.

- An administrator will have powerful incentives to preserve his own options by vigorously suppressing foot-in-the-door attempts by *his* subordinates. He may have a strong desire to commit his superiors or his successors to those policies that he personally favors. Moreover, there may be a weak impulse to preserve options favored by subordinates, but which he opposes.

- Finally, while the impulse to justify the commitments or disguise the errors in judgment of subordinates may be weak, the impulse to justify policies and programs to which one's own name has become attached may be correspondingly strong. Consequently, the hope that prior commitments can be disregarded appears utopian. Over time, current decisionmaking may increasingly be influenced by prior decisions.

Manifestations of such tendencies have not disappeared in the DoD since 1961. The Department's leaders have been capable men—and their preferences quite defensible. Yet, one must examine how such bureaucratic tendencies might influence the results, not only if the DoD's decisions were in the hands of men of lesser caliber, but also when the tendencies are exhibited in the more politicized environment affecting the civilian programs. For example, under the first heading above, contract definition procedures require the judgment that the relevant technology is in hand before signing. It is rumored that the Office of the Director of Defense Research and Engineering takes a far more tolerant view of "technology in hand" when it wants a contract than when it does not. While I cannot confirm this assertion from direct observation, I would not find it surprising.[6]

[5] The recent refusal of the Congress to sanction the transfer of the Maritime Administration from the Department of Commerce to the new Department of Transportation is symptomatic of the broader problem of achieving a more coherent structure for Federal Government activities.

[6] Such an attitude of easy tolerance could be disastrous in the civilian programs. As we shall see below, certain civilian agencies take quite readily to the language of systems analysis and are able to construct superficially plausible, but basically misleading, analyses. Where strong political pressures are involved,

On the second point, it is plainly desirable to suppress the attempts of subordinates to commit a Department or the Government to certain courses of action, even when this does not preclude such attempts at higher levels. The point we must keep in mind is that outside the DoD there may be a closer identification of senior officials with the proposals made by subordinate units in their Departments. There may be less ability to control and suppress attempts to gain Departmental support. In that case, the willingness of senior administrators to push for commitments at higher levels would not imply a willingness to suppress such pressures from below. Consequently, the Departments could become transmission belts to move the pressure for commitments from lower units to higher political levels.

Enough has been said to suggest that there is some discrepancy between the theory and the practice of systems analysis. While the theory is unexceptionable, the practice is subject to the temptations and distractions that characterize the real world. Actual experience in the DoD ought not be treated as synonymous with the idealized theoretical statement of the procedures. Perfection and elegance exist but rarely in the real world. When the natural impediments to implementation, which were encountered in DoD experience, are extended to the more raucous and politicized environment of the civilian programs, we should not be too surprised if the DoD experience proves to be a rather inexact model for what will actually take place.

THE ENCOUNTER WITH THE BUREAUCRACY

In predicting how systems analysis will fare as it encounters the passive resistance of the bureaucracy, one might start with E. L. Katzenbach's observation in his classic study of the Horse Cavalry that "history ... is studded with institutions which have managed to dodge the challenge of the obvious." [7] The reference is to military history, but observers as diverse as Thomas Jefferson and C. Northcote Parkinson suggest that the dictum may also be relevant to the civilian bureaucracy. For the military, as Katzenbach indicates, the difficulty of serious interwar testing of the effectiveness of forces partially accounts for the longevity of obsolescent institutions. But Katzenbach wrote prior to the impact of systems analysis, and it is arguable that the new techniques have eased the problem of testing and have made it more difficult for obsolescent institutions to withstand the challenge of the obvious.

In civilian activities, however, the problem is less one of devising suitable instruments for testing than of overcoming inertia and the political strength of supporting constituencies. It is rare that the obsolescence of civilian functions becomes *obvious.* The dramatic evidence of an opponent's military capability is absent. The civilian agencies make contributions to the well-being of portions of the electorate, and it is difficult to make a persuasive case that the functions or technologies in question have been superseded. Perhaps only dramatic, interest-arousing events are sufficient to persuade the public that the productive period of an institution's life is near its end.[8]

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there may be no inclination to scrutinize and challenge superficially plausible analyses, and consequently costly and ineffective programs may win easy acceptance.

[7] E. L. Katzenbach, Jr., "The Horse Cavalry in the Twentieth Century: A Study in Policy Response," *Public Policy,* 1958, Graduate School of Public Administration, Harvard University, p. 121.

[8] Conceivably the Bureau of Reclamation's invasion of the Grand Canyon may be such an event, bringing to the attention of the public that (a) the supply of suitable dam sites in the United States is nearing exhaustion and (b) hydropower has in large measure been superseded in its economics by both coal-fired and nuclear plants.

The barriers to the effective utilization of analysis are formidable. The older agencies, anxious to preserve their traditional orientations and functions, will be reluctant to view problems in terms of "broader systems." Given the narrow perspective of most agencies, the spillovers are already large and growing. Yet, if the spillover problem is seriously attacked, it would certainly imply radical change in the well-established ways of doing business and could imply a shrinkage of budgets. By contrast, the DoD has energetically dealt with the issues of spillovers between the Services. Spillovers from the DoD to the outside are perhaps another matter, but these are relatively small—in camparison to those existing at the relevant decision-making level in the civilian agencies.

Collectively the programs of the Government are like an iceberg with only a small portion appearing above the surface. Most of the existing arrangements continue from year to year; in a brief period only relatively minor perturbations are feasible, whereas to implement analytical conclusions may require radical modernization. Thus, the difficulties are substantial. The older agencies will resist either the imparting of information or the development of analyses which would cut into their treasure troves. Unhappily, the new agencies, from which better things might be hoped, are put under unremitting pressure to produce glamorous new programs—before the necessary analysis has been performed.

These are the "obvious" obstacles, but there are others more subtle and less obvious.

First, there is the ease with which all parties may fall into describing as "end use" or as outputs what are essentially inputs. The temptation is strong to continue to describe as an output what it has always been the agency's purpose to produce. The organization of the Government for providing "outputs" has normally been on an "input" basis.[9] The Forestry Service produces forests; the Bureau of Reclamation builds dams; the Corps of Engineers creates canals and flood control projects; the Atomic Energy Commission is charged with the responsibility aggressively to push the development of nuclear power. What is needed is a broader view of power development or water resources development or land use—with the evaluation of the relative benefits that component programs could provide on an integrated basis. But the existing organizations are in no position, either structurally or temperamentally, to provide such an evaluation.

Even where an agency is organizationally charged with a broader responsibility, confusion may remain regarding just what the "output" is. The Forestry Service is charged not only to manage the forests efficiently for production purposes, but to provide recreation for the public. However, the Forestry Service is dominated or strongly influenced by professional foresters, sometimes known as "timber beasts." Foresters certainly love trees and productive forests as such, and may view the town-dwellers who invade their forests as a nuisance to be tolerated. Consequently, the suggestion is hardly surprising that the Forest Service has overinvested in timber production and underinvested in recreation. Moreover, the Forest Service is interested in *timber* rather than in *lumber*. Yet, from the national standpoint, it is arguable that small sums invested in research and development on sawmill operations would have a much higher payoff than much larger sums invested in expanded tree production.

This leads into the second difficulty, which may be the most baffling and intractable of all. This is the orientation of research personnel in the agencies to prevailing notions of professional standards and scientific integrity. This orientation tends to

[9] The establishment of single-function agencies is both a reflection of and a promoter of what may be called "resource ideologies"—in which "water," "nuclear energy," "timber," and the like become valued for their own sake and become the measure of value.

overshadow a concern for the broader policy objectives of the agency. Reduced payoffs in this case reflect the highest rather than the lowest motives, but the impact on government efficiency may be the same. Researchers who respond mainly to the interests of their professional peers in universities and elsewhere may keep the research shop so pure that it is of little use to the agency in developing improved techniques or policies. This is the opposite extreme from use of research as an unimaginative and low-level tool for management, but it can occur within the same organization.

A portion of the Forestry Service's research personnel are primarily concerned with maintenance of professional status among foresters located in large measure outside the Service. Perhaps a more interesting example is the Geological Survey, which played so large a role in stimulating hydrological research in this country. In any attempt to achieve a coordinated water research program in the Government, the Geological Survey would be a key element. However, Survey personnel have been reluctant to be included in any such plan for fear that the Survey would become embroiled in policy issues and lose its identification with pure science. One is not without sympathy for such an attitude. Yet, effective policy research—at an intermediate level between high science and prosaic managerial research—must be carried out somewhere in the Government, if the new analytical techniques are to be exploited. The reorientation and broadening of professional attitudes is an essential ingredient for the more effective performance of many governmental functions. Yet, it is a problem that is easier to indicate than to solve. At best, many years will be required before the professional bodies are appropriately reoriented.

Third, there exist certain fundamental issues of choice, which even complete modernization of the governmental structure cannot resolve. Analysis cannot bridge the gap between irreconcilable objectives. At its best, analysis can shed some light on the costs of accepting one objective at the expense of others. But there is a danger that analysis may help to disguise fundamental choice problems as efficiency problems. Analytical techniques have been most successful in obtaining efficient mixes through the compromising of several objectives. But some objectives are not susceptible to compromise, and such objectives could easily be ignored in the simpleminded quest for efficient solutions.

Consider one important form of land use, that of wilderness preservation. The now dominant approach to land use analysis is that of multiple use with utilities balanced at the margin. But, by definition, a wilderness cannot be "improved" for other purposes. The preservationist impulse is one of exclusionary use of unique ecological or geological settings. One must face the fundamental choice issue *before* one seeks efficiency, or the issue of choice will be prejudged. The difficulty in the extended discussions of improved managerial or analytical tools is that it distracts attention from these more fundamental questions which deserve study in depth. By establishing efficiency as a goal one is deflected from examining those positions in which the question is: How much "efficiency" should we sacrifice in order to preserve a particular style of life or physical environment?

These are examples of the less obvious obstacles in the path of improved-government-service-at-lowered-unit-cost through analysis. But enumeration of these problems should not be taken to imply that we should be deterred from pushing ahead with the development and the exploitation of analytical techniques. These problems will yield to persevering effort. In the long run, they may prove to be less of a barrier than the more obvious one embodied in the formidable powers of resistance represented by the existing organizational structure and division of labor within the Government.

Without modernization of the bureaucratic structure, a large portion of the

potential gains of the broad application of systems analysis will be foregone. The existing structure, organized in large measure around inputs and supported by clienteles with sizable political influence, may become adept at presenting drastically suboptimized (input-oriented) or misleading analyses, which it is more convenient to accept than reject. To accept the spirit of systems analysis is exceedingly hard, but to learn the language is rather easy. There is a danger that the same old programs will be presented in new costumes. In this regard our little experience is not altogether encouraging. A number of the agencies which were early users of cost-benefit techniques have demonstrated a proficiency in presenting questionable cost-benefit analyses for questionable programs. Quantitative documentation is presented in full, but with a willing audience it appears subject to easy manipulation.

One glaring example is in water resources, for there Congress early required responsible agencies to justify proposals in terms of cost-benefit calculations. But Congress displays a willingness to be persuaded, even when the calculations are only pro forma. In developing the case for the Marble Canyon dam, the Bureau of Reclamation calculated costs on the assumption that the load factor would be 80 percent. More recently, in response to certain criticism, the Bureau has indicated that the dam would be used for firming power—and the estimated load factor has slipped to 50 percent. No one has insisted that the Bureau go back and recalculate its estimates of costs on the basis of the adjusted figure. When there is a willingness to be persuaded, fundamental changes in the data may be treated as minor perturbations.

Another example, happily more straightforward, is the case presented by the Atomic Energy Commission to keep in operation the three gaseous diffusion plants at Oak Ridge, Paducah, and Portsmouth—which are no longer required for military production. The Commission's argument is that there will be a strain on production facilities around 1980, and there should be "preproduction" [10] of slightly enriched uranium to provide for power reactors some 15 years in the future. Given any reasonable rate of discount, 5 percent for purposes of discussion, the Commission's argument says, in effect, that it will be unable 15 years hence to perform separative work at less than double the present cost. Since work is going forward on improving gaseous diffusion and other technologies; since it may be more efficient (given the pattern of demand) to scrap the present plants and build new ones at a later date; and since a main cost item in the gaseous diffusion process is the cost of electric power (which the Commission repeatedly has insisted will be reduced), it would seem that one might reasonably forecast a fall in the cost of separative work rather than an increase. Nonetheless, it would not be wise to assume that the Commission will be unsuccessful in pressing its case or that the diffusion plants will, in fact, be closed down when the existing power contracts have been terminated.

These cases may indicate the shape of things to come in the future. It should come as no great surprise that Government agencies, like other entrenched interests, will fight vigorously to preserve their activities.

WHAT CAN SYSTEMS ANALYSIS ACCOMPLISH?

The number of apprehensions that have been expressed might make it appear that I am indifferent, or even opposed, to the attempt to introduce systems analysis throughout the Government. On the contrary, I am hopeful and even, within moderation, enthusiastic. This is a case of two and a half cheers for systems analysis. But

[10] The term "stockpiling" has acquired some unfortunate connotations, and is going out of favor.

before we begin to cheer we should be fully aware of what systems analysis cannot accomplish as well as what it can.

In the first place, systems analysis cannot achieve wonders; it cannot transmute the dross of politics into the fine gold of Platonic decisionmaking, which exists in the world of ideas rather than the world of reality. Political decisions in a democratic society can hardly be more "rational" than the public, the ultimate sovereign, is willing to tolerate. All of the old elements remain: the myths and ideologies, the pressure groups, the need for accommodation and compromise, the decision made under duress. Systems analysis may modify, but it cannot extirpate, these elements. Analysis is not a substitute for any form of decisionmaking, but for political decisionmaking it will be an even less effective guide than in narrower decision contexts.

As long as the public displays an insatiable appetite for "constructive new ideas" (whether or not they have been systematically designed), democratic politics will inevitably revolve around the foot-in-the-door techniques that the analysts criticize. As long as interested clienteles will support inefficient or counterproductive Government activities, obsolescent functions will be preserved. Democratic politics will remain unchanged: a combination of pie in the sky and a bird in the hand. Tokenism, catch phrases, and cultivation of various interests will remain the guideposts.

What then can systems analysis accomplish? The question is perhaps most relevant for the long run, since we must recognize the problem of transition. The qualities that make for good analysis—detachment, breadth, interdisciplinary sympathies—do not appear like manna from heaven. It will take time to train an adequate supply of personnel and to produce good analysis. One cannot put new wine into old bottles. Even though the language of cost-effectiveness analysis is adopted by the agencies, one cannot expect a miraculous change of attitudes. At best, it will be years before analysis begins to have a significant influence in many agencies.

Nonetheless, even in the shorter run analysis will serve an educative function. In ways that may go unrecognized, analysis will begin to reshape the way that agencies view their own problems. While the desire to preserve empires will not disappear, the concept of the agency's functions will undergo change. Perhaps this is the major accomplishment of analysis: it sharpens and educates the judgments and intuitions of those making decisions. Even when analytical drapings are employed consciously or unconsciously as a camouflage for prejudged issues, the intuitions will have become sharper.

In the early stages, this educative function may be reinforced by the shock effect. The need to respond to probing questions will shake up many a stale millpond. An advantage of all new techniques of managerial decisionmaking is that it forces management to think through its problems anew. In an environment so readily dominated by routine, this cannot help but have a favorable impact.

The other major function of analysis is to smoke out the ideologies and the hidden interests. By introducing numbers, systems analysis serves to move arguments from the level of ideology or syllogism to the level of quantitative calculation. Of course, numbers alone are not necessarily persuasive. The ideologies and the established interests may not be rooted out, but the whole character of the discussion is changed. There will be a far greater awareness of how much it costs to support programs revolving about particular interests or resources. The public may be willing to pay the price—at least temporarily—but such a program is put on the defensive. Ideology alone will no longer suffice. In the longer run, less resources are likely to be committed to the program and less will be wasted than if the cost-effectiveness calculations had not been done.

Finally, we must remember that there is a certain amount of gross wastage in the Government, which serves nobody's purpose. These situations reflect not differences of opinion, not interests, nor ideologies, but simply the failure to perceive dominant solutions. It is in this realm that McNamara achieved his great savings within the Pentagon. With the elimination of these obvious sources of waste, analyses have had to become more subtle and recondite, but they are not necessarily as productive. Sources of gross waste may have been more common and certainly easier to get at in the Services than in the civilian programs. But within the civilian programs there remains a margin which can be squeezed out—even without the modernization of the Government's administrative structure.

USES AND ABUSES OF ANALYSIS

April 1968

This is the text of a memorandum prepared by the author at the request of the Subcommittee on National Security and International Operations of the Committee on Government Operations, United States Senate. It was included in the record of the Subcommittee's study on planning-programming-budgeting (PPB) in the national security departments and agencies, as a separate Committee print booklet (U.S. Government Printing Office, 1968, 12 pp.).

The Subcommittee's invitation to assess the role that analysis may play in governmental decisionmaking is gratifying for a number of reasons. In its current stocktaking, the Subcommittee is accomplishing something of a turnabout: the analysis of systems analysis. This evaluation takes place at a critical time. Like other offspring in American life, analysis has been absorbed into an environment which has been at once both too permissive and too resentful. There is ample evidence that such a pattern is beneficial to neither the offspring nor the environment. Currently there is a risk that reaction against what may be termed the exuberance of certain claims and activities of analysis could result in the discarding of the substantial benefits that analysis does offer. I shall be attempting to bring out the instances of undue gullibility as well as undue skepticism, but in so doing I should perhaps make my own position clear. My attitude has long been one of two-and-a-half cheers for systems analysis. I recognize—and have emphasized—its limitations. I will make no excuses for offenses committed in its name. But despite the limitations and distortions, I remain an unabashed, if qualified, defender of the value of analysis in policy formation.

In the pages that follow I shall deal with some salient issues regarding the role of analysis: its relation to decisions and decisionmakers, its functioning in a political environment where conflicting objectives exist, and its utility for improving the resource allocation process.

THE AUTHORITY OF ANALYSIS

Systems analysis has been variously defined. In the most ambitious formulation it has been described as "the application of scientific method, using the term in its broadest sense." Certain attributes of science—objectivity, openness, self-correctability, verifiability, etc.—are alleged to apply to systems analysis. Would that it were so, but realistically speaking such assertions must be rejected. Even for science—as those who are familiar with the history of scientific investigations will recognize—this represents a rather romanticized view. In science, however, competition takes the form of establishing hypotheses regarding the workings of the natural order. Evidence and experiments are reproducible, and institutions and personalities consequently play a smaller long-run role. In scientific investigations the search for truth is by and large unfettered. By contrast, in the search for preferred policies, such encumbrances as social values and goals, constraints, institutional require-

ments (both broad and narrow) pertain. Truth becomes only one of a number of conflicting objectives and, sad to relate, oftentimes a secondary one.

An alternative definition described systems analysis as "quantified common sense." By some expositors this definition has been treated as the equivalent of the earlier one, but is really quite distinct. However high the regard in which common sense, quantitative or otherwise, is held in the American community, it never has been regarded as synonymous with scientific method. Nonetheless, the definition is far more apt. Common sense, for example, will accept that within a complicated bureaucratic structure, distortions inevitably creep into the process of acquiring and organizing evidence. What one sees depends upon where one sits—an earthy way of describing what is more elegantly referred to as cognitive limits. It may be inferred that a systems analysis shop attached to the Office of the Secretary of Defense will be quite responsive to the perceptions and prejudices of the Secretary and the institutional requirements of his Office. This should be no more surprising than that the Operations Analysis shop at Omaha will be influenced by the doctrine, present activities, and aspirations of the Strategic Air Command.

In the early years of the introduction of PPB into the Department of Defense, faith in the ease with which scientific objectivity could be attained tended to be high in OSD. For Service staffs, this was a rather painful period, for rather invidious distinctions were drawn regarding *their* objectivity. In recent years an enormous change has taken place regarding the nature of the analytical dialogue. Undoubtedly this new attitude reflects experience and the growing awareness that past decisions and past commitments limit the openness and the freshness with which the OSD staff can address issues in controversy.

This new realism has been reflected in a number of ways. Especially in private appraisals, analysis has been justified with increasing frequency and frankness as part of an adversary proceeding. But such an interpretation is symptomatic of a substantial change. Whatever the merits of an adversary procedure—and these are substantial where there exist clashes of interests and goals and where evidence is difficult to unearth—no one has ever suggested that adversaries seek to be wholly objective. One may hope that the result will be the elucidation of the best possible case for and the best possible case against. But, unfortunately, the emphasis tends to shift to a search for the winning argument as opposed to the correct conclusion. In view of the uneven distribution of debating skills, one cannot fail to have qualms about the probable outcomes. One senior official has observed, only half facetiously, that experience in debate is the most valuable training for analytical work.

Acceptance of the tug-of-war concept, as opposed to the objective-scholar concept, of analysis has coincided with recognition of an even greater limitation of analysis as a guide to policymaking. In recent years it has been recognized in public statements (as well as the textbooks) that analysis is not a scientific procedure for reaching decisions which avoid intuitive elements, but rather a mechanism for sharpening the intuitions of the decisionmaker. Once again this is right. No matter how large a contribution analysis makes, the role of the subjective preferences of the decisionmaker remains imposing. Analysis is, in the end, a method of investigating rather than solving problems. The highest strategic objectives, the statement of preferences or utility, must in large part be imposed from outside. Poor or haphazard analysis may contribute to poor decisions, but good analysis by itself cannot insure correct decisions. This implies two things. First, whatever the complex of decisions, legitimate differences of opinion will persist. Second, disagreement with the decisions should not automatically cast doubt on either the role of analysis in general or on the quality of specific analyses. These must be examined in and of themselves.

To be sure, the judgment of the decisionmakers regarding major objectives and what is or is not important is likely to feed back and influence the analysis. This is not always true, but there are strong pressures to make it come true. Studies are driven by the underlying assumptions, and these may be imposed directly or indirectly from above. Specific terms of reference may indicate which scenarios are acceptable, which unacceptable, and which contingencies should or should not be considered. It is perfectly appropriate, if not obligatory, for the analyst to point out deficiencies in study assumptions or terms of reference. Yet, many will lack the perception or the inclination, while others would regard such action as personally imprudent. In these cases the analysis will only play back to the decisionmaker a more sharply defined version of what was already implicit in his assumptions. The role of analysis then becomes not so much to *sharpen* the intuitions of the decisionmaker as to *confirm* them.

Under these circumstances analysis is not being used in its most fruitful form, that of raising questions. But analysis is a tool that can be used in a variety of ways. Much depends upon how the decisionmaker decides to employ it. Considerable fear has been expressed that analysis will usurp the decisionmaking role, that the decisionmaker will become passive, and let analysis (implicitly) make the decisions. This is possible; it is also improper. But whether the decisionmaker will control the tool rather than letting it run away with him strikes me as a less important question than whether he will employ it properly in another sense. Will the decisionmaker tolerate analysis—even when it is his own hobbyhorses which are under scrutiny?

How many hobbyhorses are there? Are they off limits to the analysts? Dr. Enthoven has quite properly objected to the canard that analysis is somehow responsible for what are regarded as the mishaps of the TFX decisions, pointing out that the new procedures were only tangentially involved. A more penetrating question, it seems to me, is: why did the analysts steer away from the issue?

A slightly different issue arises in the case of Vietnam. Numerous blunders are alleged to be chargeable to analytic errors. But analysis has been employed in the Vietnamese context in only the most cursory fashion. Neither the high-level civilian nor the military authorities have been eager to exploit the full potentials of analysis. The degree of the success of the Vietnamese venture provides scant recommendation for reliance on untutored intuition or the standard rules-of-thumb. Once again, rather than blaming analytic efforts for the failures, the appropriate question should be: why has analysis been so little employed?

An acquaintance, who has been deeply involved in analytic activities in one of the Departments, recently commented to me on his experiences. Analysis he felt had been relevant in only a small proportion of the decisions. Half the time a decision had been foreclosed by high-level political involvement: a call from the White House, interest expressed by key Congressmen or Committees. In an additional 30 percent of the cases, the careers of immediate supervisors were involved. Analysis could not influence the recommendations; it could serve only as an irritant. But, he argued, in something like 20 percent of the issues, analysis was unfettered and contributed to much improved overall results. This was only the experience of one individual. In other cases the proportions might be quite different. The point is that analysis should be judged on the basis of only the minority of cases in which its influence is in some sense instrumental. Analysis is a useful tool, but it is only a tool. It would be a mistake to turn over a new proverbial leaf—and generally find fault with tools rather than craftsmen.

PRACTITIONERS VERSUS INSTRUMENTS

Accepting that analysis only sharpens the intuitions of decisionmakers, that its powers may be curtailed by unquestioned (or question-begging) assumptions or by imposed terms of reference, and that it is increasingly viewed as a contest between adversaries, permits us to be more realistic about analysis in a number of ways. The inflated claims, periodically made in its behalf, may be rejected—along with the misplaced criticisms made in response. Questioning of decisions is turned into questioning of decisionmakers' judgments rather than the role of analysis. And analysis itself can be employed more effectively in clarifying the underpinnings of policies, thereby creating the potential for designing more effective ones. We should understand that analysis provides no formula for solving problems, no prescription for sensible policies. It cannot and should not be employed to "demonstrate" that one's own policies are so right and those of others, so wrong.

What analysis provides is an exercise in logical coherence, hopefully with knowledge of and respect for the underlying technical, economic, and organizational data. Coherence does not insure the "correctness" of policy. In fact, an incoherent policy will sometimes be closer to correct than a coherent one. But the incoherence itself scarcely makes a contribution. It is almost invariably a source of waste, and typically of policy muddles.

Analysis may make a contribution, but we should be very clear what it cannot do. It does not provide an instant cure for pigheadedness. In fact, it does not provide an instant cure for anything—not because of its theoretical deficiences, but because it has to be employed by people and by organizations with divergent goals and views and with stringently limited information about actual conditions.

It is a mistake to identify analysis with the particular judgments, prejudices, or arguable decisions of some of its major proponents. Especially is this so when analysis has been employed as a weapon of political conflict. The political process being what it is, it is hardly advisable to admit error in public; that would prove too costly. Human emotions being what they are, it is also unlikely that error will be admitted in private. This does not gainsay the value of analysis before policy commitments are made—or when they are being seriously reconsidered. What it does say is that we should avoid tying analysis to the personal proclivities of the particular individuals who were instrumental in introducing it into government. To do so may be flattering to the individuals. Some may even be inclined to treat their own attitudes and commitments as synonymous with analysis. It would be a serious error for others to accept this view.

Disciplined, orderly thought is the characterization given to analysis, but disciplined, orderly thought suggests certain traits: reflectiveness, self-criticism, and willingness to reconsider past commitments without self-justification. However rarely or frequently encountered in the general human population, these are not traits characteristic of the action-oriented, incisive individuals who reach policy-making positions. Questioning and self-doubt lead to Hamlet-like decisionmakers.

Analysts themselves may be self-doubting, bemused by uncertainties, frighteningly candid, but different tactics have been required of the missionaries who have proselytized in behalf of analysis. I do not need to develop this point at any length. It should be plain, for example, that the actual decision to introduce analysis on a government-wide basis (as previously within the DoD) required an act of judgment and courage passing beyond the confines of analysis. Some analysts found the manner in which analytical procedures were instituted disquieting. This no doubt reflects a certain naivete on their part regarding political processes. But analysis was introduced rather suddenly. There was little advance preparation, little attempt to

assess resource availability or calculate short-run costs. There was no "program definition phase." What occurred was that the political conditions were ripe,[1] and the opportunity was seized—for analysis.

I have perhaps belabored the distinction between analysis and judgment and the fact that the act of deciding occurs in the nonanalytical phase. These matters need to be emphasized right now. It is important that analytical procedures in the DoD or elsewhere *not* be identified with particular sets of policies, decisions, or individuals. If analysis comes to be confused with the idiosyncrasies of a few dominant personalities, there is some risk that it will disappear along with its original proponents. Its potential benefits for U.S. policy would then be lost for some time to come.

Admittedly there have been overstated claims, planted stories, and an impression generated among the cognoscenti of a new, scientific means for grinding out decisions. Admittedly the limitations appeared in the footnotes and not in the fanfare. But these are just the accoutrements of attention-getting. Analysis itself should scarcely be discarded on these grounds. Even if some decisionmakers or analysts have failed to display the mental elasticity that analysis in principle demands, this is only a reflection of the human condition. Why throw the baby out with the bathwater?

PAYOFFS

What is the baby? I seem to have devoted most of my attention to the reasons for refraining from the last half-cheer for analysis, and virtually no attention to the reasons for the two and one-half cheers. In part this is due to the excellent set of papers and comments that the Subcommittee has published. Therein the potential benefits of program budgeting and analysis are fully presented. Lengthy reiterations of either the potential advantages or the accomplishments seem unnecessary. However, there are some points on which I should like to add a few words.

First, analysis has great value in turning debates over resource allocation toward the realities and away from simple statements of noble purpose. Analysis is not scientific method. Neither will it necessarily be objective in an organizational context. Yet, within the adversary relationship, analysis at least focuses the debate on what particular systems can accomplish and what numbers are required. The emphasis is on the real rather than the symbolic function of weapon systems. Disappointed as many in the Services have been with major policy decisions of the OSD, I believe most knowledgeable officers would agree that the new methods have been beneficial in this respect.

Second and closely related, analysis is oriented toward outputs rather than toward inputs. In this way expenditures can be tied to specific goals, and those expenditures which satisfy primarily the traditions or well-being of individual agencies are brought into question. There are difficulties with goal or output orientation, particularly since we so frequently lack complete understanding of the mechanism that ties inputs to outputs. But the orientation is correct. The government structure is subdivided into agencies that typically concentrate on inputs. Dams, warships, trees, post offices, bombers, nuclear power, supersonic transportation, and, I may add, research expenditures are often treated as ends in themselves—with little examination as to how these instruments serve public purposes. Conscious output

[1] This episode suggests why the politician in his role may find analysis both incomplete and frustrating. Analysis deals in a rather abstract way with resource usage and efficient allocations. It does not deal with the attitudinal issues of support-generation, coalition-gathering, or with timing, which are so important in the political context.

orientation, with as much quantitative backup as possible, points in the right direction. It forces agencies to shift attention from their beloved instruments and to explain the goals they serve rather than the functions they perform—and this at a level more practical than the usual rhetoric of noble purpose.

Third, the attempt is made to design systems or policies with practical budgetary limits in mind. The time-honored gap between the planners and the budgeteers has been widely discussed, along with the difficulties it causes. There is little point in plans too costly to be implemented or systems too expensive to be bought in the requisite quantity—if some reduction in quality will provide a feasible and serviceable, if less ideal, posture. (Here we are discussing capabilities and postures which would be effective, if bought—keeping in mind that so many expensive proposals serve little purpose at all.)

Fourth, an attempt is made to take spillovers into account and to achieve better integration between the several Services and Commands. Once again, this is more easily said than done. For example, we are belatedly becoming aware of the spillovers and the integration problems between the strategic offensive force under Air Force management and the new Sentinel system under Army control. This indicates that the attempt to take spillovers into account has not been overwhelmingly successful, but the goal is a correct one. The nation would not wish to duplicate SAC's capabilities for SACEUR or the Polaris force for CINCSAC.[2]

Fifth, the attempt is made to take into account the long-run cost implications of decisions. Perhaps, it is more appropriate to say . . . the attempt *should* be made. There has been a certain inconsistency on this account. The costs of some systems have been carefully investigated, before a choice is made. For other (preferred) systems this has not been the case. The Program Definition Phase was originally introduced to insure that technology was in hand and the long-run costs considered before force structure decisions were made. Yet, curiously, in the programmed forces for the '70s our strategic forces are scheduled to become increasingly dependent on MIRVed[3] vehicles, even though the technology is not yet in hand and we have only an inkling of the ultimate costs. The appropriate review of alternatives and hedges did not take place. But this represents, not a criticism of the objective, but a plea for more consistency in its pursuit. It hardly negates the desirability of the careful weighing of alternatives with the long-run cost implications taken into account.

These attributes and precepts of analysis seem unexceptionable. They are. An appropriate inference is that many of the complaints couched in terms of "too much analysis" or "the errors of analysis" should be altered into "better and more consistent analysis." In this connection, an editor and friend recently suggested a paper on the impact of systems analysis: "Not the general appraisals, we've had enough of that; tell us whether systems analysis has ever really been employed in the Department of Defense." An exaggeration perhaps, but as the MIRVing case suggests, analytic techniques have not been consistently applied.

Bernard Shaw observed somewhere that the only trouble with Christianity was that it had never really been tried. An epigram is at best a half-truth, designed, as someone has commented, to irritate anyone who believes the other half. In DoD, systems analysis has at least been tried. But there is an element in Shaw's remark that needs to be taken into account. In assessing the success of analysis, both the incomplete implementation and the resistance should be kept in mind.

[2] That is, to duplicate Strategic Air Command capabilities for NATO, or the Polaris force for the Strategic Air Command—*Ed.*

[3] That is, missiles fitted with multiple independently targeted warheads—*Ed.*

BUDGETS

Military posture is determined in large measure by the total volume of resources the society is willing to divert from non-defense to defense uses. Yet, understanding the determinants of this resource flow presents a most perplexing problem. No good mechanism or rationale exists for deciding what diversion is proper. Some analysts have shied away from the problem, arguing that the main objective should be the efficient employment of whatever resources are provided. A limited feel for appropriate diversion may be obtained by asking such questions as how much more is needed for defense than is needed for other purposes. In principle, senior policymakers may find it no harder to decide on allocation between damage-limiting and urban renewal than between damage-limiting and assured destruction. They will certainly find it no easier. For a number of practical reasons, they may find it far harder actually to bring about such a resource shift.

The amorphousness of this decision area combined with the repudiation of what were regarded as the rigidities of the Eisenhower years led to some bold words in 1961: there would be no *arbitrary* budget limits; in addition, every proposal would be examined on its own merits. These guidelines have since been regularly reasserted—with perhaps somewhat falling conviction. Originally they might be attributed to sheer enthusiasm; now they can only be taken as either propaganda or self-deception.

However, no matter the source, they will not stand up to analysis.

At any time there exists a rough political limit on defense expenditures. For members of this Subcommittee—in fact for any practicing politician—such an assertion will seem like a truism. Something like a consensus develops regarding proper levels of defense expenditures—and in the absence of external shocks this sum will not be substantially augmented. Of course, the *arbitrary* limit is always the *other fellow's*. One's own limit is only proximate and is wholly reasonable. Yet, defense expenditures do tend to become stabilized for years within rather narrow limits. Inevitably, new pressure for funds leads to the sacrifice of programs previously desirable on their own merits. That is as simple as arithmetic.

The only time that budget limits are not pressing (and more or less arbitrary) is when, as during the early Kennedy years, a political decision has been made that much more can be spent on defense. After a brief period of exuberance, the old constraints reappear. The decision does not have to be announced by the President or the Budget Bureau. The Secretary of Defense may get a feel for what is feasible, or he may be trusted to bring in a reasonable figure. But within a rather narrow range he will face a limit, which he may not transcend without either creating a minor fiscal crisis or straining his own credit with the President of the United States.

Save in the rare periods of budgetary relaxation, this, rightly or wrongly, is the way the system works. There is no point in kidding oneself. One may erect a facade intended to demonstrate that there are no arbitrary budget limits and each proposal is examined on its own merits. The pretense can be partially successful, but only because the criteria for choice are so imprecise. Standards can be made increasingly stringent, yet no one can prove how large was the role of budgetary pressures.

Nonetheless, no one should be deceived. What happens is that various alternatives and hedges are discarded; programs become less pressing and are stretched out. The practices are well-known from the bad, old meat-axe days. Under budgetary pressure (arbitrary or not) it is truly remarkable how many options one discovers one can do without. Multiple options just become less multiple. Before uncertainties are resolved, commitments are made and hedge programs are terminated. In the well-advertised adversary relationship, the negotiator-analysts become much harder to persuade. If they are not directly instructed, *they know.*

These are not hypothetical possibilities. With the intensification of budgetary pressures stemming from the Vietnamese war, there has, for example, been a wholesale slaughter of programs in the strategic area. It is important not to be misled regarding the critical role of budgetary pressures—and thus come to believe that so many programs, previously regarded as meritworthy, have suddenly lost their merit. Otherwise, we might gradually come to believe that we are doing far better than is actually the case. One should remain aware that the decimation of a program has long-run postural implications. That is, after all, the message that PPB attempts to convey.

These are elementary propositions. I do not dwell on certain theoretical problems and inconsistencies bearing on the relationship of overall defense spending to the optimality of programs. Suffice it to say that the *quality* of what one buys depends upon how much one wants to spend. This connection between level of demand and cost/effectiveness creates a dilemma in that *neither* the character of the programs nor the size of the budget can be determined initially. But that is a theoretical nicety, the direct consequences of which may not be of major importance.

The vital point is the way in which budgetary limits may control force posture and therefore strategy. Shifting sands seems the best way to characterize the strategic rationales of recent years. In 1961 the suicidal implications of massive retaliation were underscored: the United States would be faced with a choice between humiliation or holocaust. Interest then developed in damage-limiting and coercion. But there had been little willingness to invest money in either. Since 1965 the merits of Assured Destruction have been emphasized—with little attention paid to the suicidal implications found so distressing in prior years. The principal rationale for the current emphasis on Assured Destruction reflects certain recently-developed notions of arms control. It clearly falls within the province of the decisionmakers to adopt a strategy of measured response to any Soviet buildup with the long-term objective of preserving U.S. Assured Destruction capabilities. One should note, however, that to accept this particular guide to action implies that the Minuteman buildup of 1961-62 was a mistake. These newer arms control criteria may be the preferred ones, but they rest on the judgments and intuitions of the decisionmakers. They certainly do not emerge by themselves from analysis.

May one infer that the oscillations in strategy have something to do with budget limits, or in this case something more specific: a preconception regarding how much this nation should spend on the strategic forces? I find the conclusion irresistible. The evidence antedates the current phase-down in the face of the Soviet buildup. Once again, these lie within the decisionmaker's prerogatives, but particular beliefs regarding budget limits or the "adequacy" of specific strategies should not be attributed to, much less blamed on, analysis.

A USEFUL IF OVERSOLD TOOL

Whatever resources are made available to defense (or any other mission), choices will have to be made.

Allocative decisions inevitably are painful; many claimants will be sorely disappointed.

Few will find fault with their own proposals, almost all with the machinery for selection.

Any procedures for allocation will be criticized—even in a hypothetical case in which the conceptual basis is unarguable and no errors are made. Analysis provides the backup for a selective process. What does it contribute? How does it compare

with real-world alternatives—not with mythical alternatives in which all claimants get their requests and no one is disappointed?

It has been emphasized that analysis cannot determine the appropriate strategy. It can shed light on costs and tradeoffs. But the choice to press arms control or arms competition or to rely on tactical nuclears or nuclear firebreaks must be determined by the decisionmaker, sustained primarily by hope, conviction, and prayer. Even if a decision could be demonstrated as correct at a given moment in time, there is the certainty that objectives will change over time. For these higher-level problems analysis is an aid, but a limited aid. The toughest problems, dominated as they are by uncertainties and by differences in goals, do not yield to analysis.

Happily many problems are more mundane and more tractable. Where analysis has proved its highest value is in uncovering cases of gross waste: points at which substantial expenditures may contribute little to any stated objective. It might be thought that a problem of diminishing returns exists for analysis in that the cases of gross misuse of resources are likely to be uncovered at an early stage. Thus, as the opportunity for major savings through elimination of irrational forms of waste theoretically recedes, analysis would be forced into the more ambiguous areas in which strategic choices become intimately involved. In some cases, where information is readily available and objectives and conditions relatively unchanging, this could prove to be true. The very success of analysis would then undermine near-term expectations of additional returns. However, in defense this turns out to be irrelevant, since the problems are so volatile and information so difficult to unearth.

To say that analysis works best in cases of gross waste should not be taken to imply that analysis accomplishes little. The simple cases involving so-called dominant solutions may involve billions of dollars. The volume of government resources that may be lavished on the care and feeding of white elephants is simply staggering.

Here we have "quantified common sense" in its most direct form. In bureaucracies, units at all levels are concerned with organizational health. Rather than making the hard choices, the tendency is strong to maintain morale by paying off all parties. Analysis provides a means for coping with this problem. The big issues may not be directly involved, though they are likely to be dragged in by the proponents of particular programs.

Should the assessment of analysis be much influenced by the annoyance felt by those whose proposals have failed the tests? Certainly not in the general case. No more than should the decisionmakers be permitted to hide their judgments behind the camouflage of analysis, should the patrons of doubtful proposals be encouraged to argue that acceptance would and should have come—if *only* analysis had not been employed. Budgets are limited and hard choices must be made. If nobody were annoyed analysis would not be doing its job—of questioning both routinized practices and blue-sky propositions. Disappointment is unavoidable. The question is not the existence of annoyance, but to strive to annoy in the right way and for the right reasons.

In this light it may be desirable to examine the issue of the generalist versus the specialist which has been touched upon in the Hearings. In the nature of things specialists become committed to particulars: a piece of hardware, a technological criterion, a disciplinary blind spot. It is a case of suboptimization run wild. Proponents of specific capabilities or gadgets tend to become monomaniacs. In a sense that is the way they should be: totally dedicated to their tasks. But one does not turn to them for detached judgments. There is no substitute for the *informed* generalist. There is a recognizable risk that the superficiality of the generalist may match the monomania of the specialist. However, that need not be the case. Although the generalist's knowledge cannot match that of this specialist in detail, analysis can

once again play a useful role, by permitting the organization for the generalist of more specialized information than he alone could master.

How does this relate to the limits of the analyst's role? Two distinctions should be kept in mind: that between the technical specialist and the analytical generalist and that between the analyst and the decisionmaker. The analyst's tools are not circumscribed by discipline or even by subject matter. But general tools are not immediately convertible into broad policies. Many analysts are, in some sense, specialists in the use of general tools. Being a good analytical generalist does not necessarily imply possession of such additional qualities as breadth, judgment, and political attunement. These latter qualities are what many have in mind when they speak of the generalist as policymaker.

CONCLUSION

In closing I should like to underscore three points.

First, the position of the decisionmaker employing analysis is somewhat ambiguous. For tactical purposes this ambiguity may be deliberately augmented. Intermittently he may choose to stress *analysis* or *judgment*, and to shift hats according to the tactical requirements of the moment. His policy judgments may be obscured or defended by cryptic references to detailed analyses which allegedly force the policy conclusions. On the other hand, if any limitations or inadequacies in the analysis should come to light, these can be waved away with the reminder that all issues are ultimately matters for the decisionmaker's judgment.

Moreover, the pattern is in reality far more complicated than the standard exposition in which the analyst produces an *objective* study, and the decisionmaker's judgment enters at a later stage erected on the foundation of these objective results. That makes the analytical and judgmental stages seem clean-cut. Few studies are that pure. The decisionmaker's judgments quite typically are dumped in at an early stage in the form of guidance, assumptions, and terms of reference. The more political a study, the less likely is it to be pure. In fact, the process can be (and has been) far more corrupted, when questionable (phony) numbers are introduced. Since judgment and analysis are thoroughly intertwined in all but a few studies, the attempt of decisionmakers to shift roles by referring to fundamental analyses should be treated with some skepticism. The decisionmaker should not be permitted to escape the full burden of responsibility by the invocation of analysis.

The temptation for those who have introduced analytical techniques into the government to treat their own positions or careers as identical with analysis is understandable. No outsider should yield to the same temptation. The roles and even the temperaments of decisionmaker and analyst are quite distinct. The confusion tends to disguise the heavy personal burden borne by the decisionmaker. More important, if analysis is treated as synonymous with particular decisions or personalities, there is a risk that it will be throttled or abandoned after their departure. From the standpoint of public policy this would be a major loss.

Second, we should avoid the erroneous belief that the performance or potential power of analysis will be uniform in all contexts. If a town is considering building a bridge, a number of difficult analytical problems must be addressed: does demand warrant construction, where should the bridge be built, what should be its capacity, and so on. But once these questions are resolved the engineer falls back on a solid technical base. By contrast, for such goals as deterrence, assured destruction, controlled nuclear warfare, damage-limiting, to say nothing of welfare benefits, we fall

back, not on a firm technical base, but on what may be scientific mush. The distinction is not always appreciated. The difficulty is sometimes dealt with by referring euphemistically to *the model problem.* But our ability to formulate models depends upon our knowledge of the mechanics of the real world. For many problems our knowledge is meager, and the profferred models are misleading or downright erroneous. The lack of good models in many problem areas simultaneously limits the power of analysis, while increasing the burden placed on judgment. In treating analysis as a uniformly efficient problem-solving technique, the variability of analysis, which reflects the variability of the knowledge base, is ignored.

Though analysis is a powerful tool, specific analyses vary greatly in quality. Some are little more than trash. But we need to discriminate, rather than to reject analysis in toto. At the present time there is some risk that we will do the latter. In an address some years ago Secretary Enthoven observed: "My general impression is that the art of systems analysis is in about the same stage now as medicine during the latter half of the 19th century; that is, it has just reached the point at which it can do more good than harm." That was a frank and realistic, if somewhat pessimistic, assessment of the state of the *art.* Scientifically speaking, there are numerous blind spots in medicine. Yet, most of us ultimately are inclined to accept the doctor's diagnosis, if not his advice. Quite plainly at the present time Congress and the public are having second thoughts regarding how much trust to put in systems analysis. No doubt it is necessary to develop a greater ability to discriminate. Nonetheless, I suggest that policy will benefit substantially from the analysts' diagnoses.

Third, there is little doubt that analysis has been oversold. That strikes me as a rather standard result in matters political. But the reaction against the overselling could be more costly than the overselling itself. Analysis is a powerful instrument: with it our batting average has been far higher than without it. Analysis is also an adaptable instrument. The McNamara regime has in many respects been a highly personalized one. Its performance should not be taken as defining the limits of this flexible tool. Admittedly, analyses vary substantially in quality. Each should be taken with a large grain of salt. On the other hand, if one does not demand too much of it, analysis will prove to be a most serviceable instrument.

DEFENSE PLANNING AND BUDGETING: THE ISSUE OF CENTRALIZED CONTROL

Rand Paper P-3813
May 1968

Also published as a monograph by the Industrial College of the Armed Forces in 1968.

AUTHOR'S NOTE: This study was initiated in 1963 and completed early in 1964. At that time the new Department of Defense procedures had just begun to take hold. It was Rand's purpose to help clarify the objectives and mechanics of the new procedures, to assist the Services in adjusting to the developing system, but above all to anticipate some of the longer-run implications of these changes. The completed study did receive some circulation within an audience limited primarily to the Air Force. It is gratifying that the interpretations and the judgments offered in it have stood the test of time sufficiently well that the Industrial College of the Armed Forces now wishes to publish it for use in its instructional program.

In retrospect, I might alter the phrasing of certain points, but the basic conclusions would remain unchanged. There is one partial exception, and it is part of a larger problem. In recent years I have become less sanguine regarding the efficacy of inter-Service rivalry and criticism (useful as it may be) or the potential of the major Commands for flushing out new alternatives or criticizing obsolescent activities. Large hierarchical organizations, whether characterized by centralization, or by partial decentralization in tripartite manner, or even by greater responsibility devolving to the Command level, tend to be remarkably efficient mechanisms for the suppression of new ideas and alternatives. In part, this is inevitable. Conceptual innovations are *disorganizing*. The Services, and especially the operating Commands, place a premium on *organization*. Nonetheless, momentarily putting aside the question of the appropriate degree of centralization, the alternative-generation process within the Services has not been as effective as we would wish. The Services will have to do more in improving the incentives for generating alternatives and the receptivity given to innovations. The more effective the performance of subordinate hierarchies, the more powerful is the case for decentralization.

On an intimately related point, the prognostications regarding the impact of small-group coalescence around a limited range of views have turned out to be distressingly germane for subsequent developments. As an example I cite the primacy placed upon Assured Destruction within the Office of the Secretary of Defense in recent years. It has not only dominated force structure decisions, but has strongly affected both the evaluation of alternatives and the emphasis in the R&D program. Within a more limited arena, the incentives created have not been much different from those that came within the charmed nuclear circle that applied during the era of Massive Retaliation. Perhaps the alternative-generation process within the Services has been adequate, but the incentives profferred both to the Services and to industrial contractors have been clearly marked. I believe the consequences of this narrowing range of acceptability to be harmful. To be sure, it is suggested that we buy more than enough for Assured Destruction, and the margin can be devoted to Damage Limiting. But this rests on the presupposition that weapon systems bought for one role can perform effectively in another role—in other words, that a high degree of substitutability applies.

This presupposition strikes me as unwarranted. The particular set of decisions involved may be defended or questioned, but they do point to the risks inherent in the limited perspectives of a single group.

While I would like to draw renewed attention to these points, I have done little to alter the main text in the light of subsequent developments. Certain changes in nomenclature have taken place: Subject Issue is now Program Budget Decision; Program Change Proposal is now Program Change Request, and so on. In the large the system has been only slightly altered in the intervening years, and these changes are of little importance. I have not updated certain references, such as those to the TFX.* Rather than seem to be indulging in hindsight I leave the material in its original form—to exploit whatever clairvoyant value it may be deemed to possess.

This note would be incomplete, were I not to express my indebtedness to Roland McKean, who was an original participant in the study. His contributions are reflected throughout, even though I am prepared to take responsibility for any deficiencies. McKean's decision to return to academic life precluded his involvement at the close. Of greater importance, from the standpoint of defense analyses his departure remains an even more regretted loss.

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* Later the F-111—*Ed.*

I. HIGHLIGHTS OF THE REVOLUTION IN DEFENSE MANAGEMENT

The main feature of the revolution, as most people see it, is the centralization of control over many matters that were formerly controlled by the individual Services. That decisions in so far-flung an organization as the Department of Defense must always be made in the light of a variegated pattern of advice and bargaining pressures is a fact that should never be ignored. Yet in recent years the Office of the Secretary of Defense has clearly played a stronger role than formerly in major force-structure and development decisions and also in many lesser choices.

Through his influence on the budget, the Secretary of Defense has in the past always been able to exercise a great deal of *negative* control—by withholding approval, by vetoing suggestions, even by bargaining. Of late, however, the *affirmative* power of the OSD to implement its own ideas while blocking rival ideas has increased markedly. Concurrently, the power of the Services to propose, to persuade, to implement, has shrunk. Though the trend toward centralization was noticeable before 1961, a marked acceleration occurred at that point. Mr. Robert S. McNamara has been an unusually forceful Secretary of Defense. The offices of the Director of Defense Research and Engineering (DDR&E) have monitored research and development programs more closely than before—eliminating, cutting, deferring, and occasionally initiating or increasing individual R&D projects (or even tasks). Other parts of the Office of the Secretary of Defense have strongly influenced various choices. The Services, on the other hand, have had less influence on most of these decisions.

What event yielded this outcome or made it possible? Probably the main thing was not changes in the formal institutional structure but a massive shift in power resulting from (1) the appointment of a Secretary of Defense who had the will and ability to exert a greater degree of control, and (2) the existence of considerable support from the White House and Congress (in good part reflecting dissatisfaction with the way defense planning had previously been managed). Given this basic shift in the structure of power, decisionmaking roles have altered. Concurrently, a number of institutional changes emerged. Some of these changes may have played at

least a partial supporting role in the centralization process, yet the case for them is in large measure independent of their connection to centralization. Even if the power structure were to shift again, many such changes would almost certainly remain. In this study we will give special attention to some of these institutional changes and the effects they may have when accompanied by marked centralization.

One of the most significant institutional modifications is the new budgeting-programming system. It is comprised of several more or less separable elements: (1) the new format in which plans and budgets are presented in terms of Programs and program elements, categories that are at least somewhat more like outputs than the conventional Appropriations categories; (2) extensive use of special papers and cost-effectiveness analyses to provide additional aid in deciding how resources should be allocated among Programs and program elements; (3) bookkeeping devices for checking on the Services and Commands to make sure that the OSD allocations and decisions are being implemented.

The first element alone, the new format, is essentially a supplementary information system—an attempt to show the cost implications of Programs and of certain changes in them. The second element, cost-effectiveness studies, aims to provide additional and particularly relevant information about alternatives that are deemed to be particularly important. These studies have apparently played significant roles in budgeting-programming decisions. The third element, a mechanism to see that the Services adhere to the accepted Programs, is a vital part of the existing system. Without such a mechanism, OSD would make decisions, but the Services could then take their budgets and, within limits, do what they pleased with them.

The particular mechanism that is used involves several devices: (1) a five-year plan—the approved Five-Year Force Structure and Financial Program (now the Five Year Defense Program)—that provides a base line; (2) a system of progress reporting; (3) reprogramming procedures for seeking changes in the current year and the fiscal year shown in the published budget; (4) mechanisms for seeking changes in the subsequent "program years"—Program Change Proposals or PCPs (now called Program Change Requests or PCRs). The latter are formal requests for permission to depart from the accepted Program and are also a means of keeping score on accepted changes.

In connection with these devices several points should be noted. First, the five-year plan or Blue Book is becoming an effective base line, at least in the sense that unexplained revisions will no longer appear. At first there was much confusion, and no "audit trail" that could show exactly how the base line plus approved changes yielded the updated base. Completely new figures were sometimes introduced as the updated version. Now, however, the updated Blue Book will be precisely the old base line plus approved changes and officially noted lists of below-threshold changes. Second, most significant proposals require reprogramming requests or, if future "program years" are affected, PCPs. Under specified conditions, a Service can make minor, i.e., below-threshold, resource transfers or substitutions, but significant shifts of resources from one program element to another, as well as proposals that would increase total obligational authority, require OSD approval.

In the Financial Programs, and of course in the proposed changes, operating costs are, quite properly, segregated from investment costs, and the system functions differently with respect to these different costs. Departures from authorized investment or major procurement costs can rather easily be detected in most program elements. Aircraft and submarines and Honest John battalions are easily counted (although, as noted later, this is true with or without the new system). Deviations from approved allocations of operations and maintenance (O&M) costs, however, are not so easily detected, because the estimates *for individual program elements* are

exceedingly rough and are not linked to identifiable resources. If they wished, therefore, the Services could probably shift O&M items from one program element to another without penalty.[1]

R&D costs are also quite properly segregated (properly because it would be misleading to show investment or operating costs as necessarily being entailed by development decisions).[2] In basic research and exploratory development the five-year plan, or Blue Book, provides for a rising level of effort. When the annual budget is prepared, OSD has usually trimmed the amount for the coming year below the programmed figure, and Service requests have seldom pressed against the ceiling on total obligational authority. As a consequence, at present PCPs rarely figure in the process. Nevertheless, DDR&E exercises control in this case by means of detailed review of the R&D Programs. In Advanced Development, however, considerable control is exercised through the PCP mechanism. In addition, deferrals can make sure that the Services are unable to do things without the approval of DDR&E. For acceptance in Engineering or Systems Development, a proposal must survive an exhaustive screening process prior to entering Program Definition—another decisionmaking point at which OSD exerts control.

Several other points about the functioning of the system should be mentioned. First, while the Five-Year Financial Program is somewhat helpful to the decisionmakers in deciding upon resource allocation among program elements, it is appropriate to supplement it by special analyses for important decisions. Collecting costs in terms of program elements reveals only the roughest of clues to the information that is needed. What one typically should have is the cost of an increment or a decrement, not the cost of the entire existing Program. Moreover, one should have the cost of a particular kind of increment, not a "representative" slice, because there is no unique program-element cost that is appropriate under all circumstances: Are new facilities required, or are part of them on hand? Will bases be available because something else is being phased out? In what climate will the extra activity be conducted? Any cost analyst will ask dozens of questions before trying to estimate the relevant costs of a proposed change.

Also, of course, the Blue Book gives no clues to effectiveness, and wherever possible one needs ad hoc analyses (and judgments) to provide such clues. Again the interrelationships between program elements make this depend upon the particular circumstances. The gain in effectiveness attributable to an increment to C-141 (Troop Carrier) depends upon what is happening to C-141 (Air Transport) and to C-123 (Air Transport, Reserve and Guard Forces). The loss in effectiveness due to a reduction in Surveillance and Warning Systems depends upon what is being done to the B-52 program element. These are some of the reasons why the new budget format per se has a somewhat limited impact and why ad hoc cost-effectiveness analysis and "special papers" are important parts of the OSD system.[3]

[1] Within the Air Force the P-documents give a basis for internal control, but they were never drawn up in terms of the program-element structure and do not offer an easy means of enforcing allocations, program element by program element.

[2] However, to the extent that weapons systems are introduced into the operational packages before R&D has been completed (or before executing the Program Definition Phase), the distinction becomes quite blurred. The handling of the TFX is a case in point.

[3] Although we feel that program budgeting should be supplemented by cost-effectiveness analyses, it should be emphasized that in principle the two are separable. One could have the analyses in the absence of program budgeting, and one could have the formal machinery without analysis. In either case, however, some of the benefits would be lost. Cost-utility analysis has not been *created* in recent years; it has only been formalized. In a rough and ready manner the military services have always made use of cost-effectiveness assessments. Within the Services deep misgivings have arisen which reflect (1) the belief that on occasion the formalized techniques have been pushed too far, and (2) disagreement in measuring effectiveness with those in OSD. The Services should not permit doubts of such a nature to cause them to reject the substantial benefits that cost-effectiveness analysis can provide.

The strong interrelationships between Programs, between program elements, and between PCPs, have affected the evolution of the system considerably. At first it was thought that PCPs might be reviewed singly with the Services submitting them at any time during the year. Important proposals often reached OSD late in the budget cycle, however, after changes of less worth had already been approved and "budgetary limits" had already been stretched. Moreover, in preparing proposals, the Services knew too little about the fate of earlier and related proposals. Because of such difficulties, it was decided to review the PCPs in related batches, but this still does not cope adequately with interdependencies or "ricochet effects." There are complaints that too few persons get to consider programs in their entirety, leading to "irresponsible" approval of PCPs and ultimately to deep Subject/Issue (now Program Budget Decision) cuts in terms of the old Appropriation categories. The growing awareness of the implications of interdependencies is indicated by the fact that Programs I (Strategic Retaliatory Forces) and II (Continental Air Missile Defense Forces) have been combined and are now considered as a single Program (Strategic Forces).

There is growing pressure to authorize application of single PCPs to related groups of program elements or even to entire Programs instead of having separate PCPs for each element. There is also pressure to authorize single PCPs for "issues" that affect particular resources, for it seems wasteful to have a series of bookkeeping "corrections" rather than one summary correction. Interdependencies also yield constant dissatisfaction with the program element structure, and it will no doubt be altered gradually. This structure is presumably to make use of categories having minimal interdependence, and it can no doubt be improved, but we should recognize that *no* format can fully eliminate interrelationships among Programs and program elements. They are there mainly because life is complicated, not because the program element structure creates them. If one postponed decisions until all interdependence could be taken into account, decisions would never be made.

Perhaps a more important factor shaping the system has been the following fact: to consider only the changes proposed in the PCPs is to look mainly at proposals that the Services believe it is judicious to submit. To get at the iceberg beneath the surface, pressure will grow to have annual reviews of Programs in their entirety. While this is more easily discussed than done, the functioning of the system may alter noticeably. For one thing the PCP may come to be explicitly regarded as a record-keeping and control instrument rather than an instrument to aid decisions. Despite efforts to improve PCPs, sequential and informal exchanges, special analyses, and nonquantifiable considerations are almost inevitably more important than formal PCPs in reaching decisions. Also if the Services are unable to provide Program reviews in a manner that is acceptable to OSD, one likely response would be the belief that there is no alternative to still more affirmative control by OSD.

Constraints on the Services are nothing new, of course. Congress and OSD have never given the Services free rein and could not conceivably do so. Congress has long required a type of PCP—the reprogramming requests for certain departures from enacted Appropriations. Economy-minded Secretaries of Defense have slashed budgets and vetoed Service proposals. Within the Services there have long existed networks of controls, with reporting systems to make resource shifts difficult and to help detect deviations from approved activities. But the new "revolution" in defense planning has added another layer of constraints, instituted a markedly greater degree of affirmative control by OSD, and extended the period of control from one year to five or more years.

Perhaps we should emphasize once more that the budgeting-programming system is not itself responsible for the centralization of authority. The new budgeting-programming arrangements and also the use of cost-effectiveness analysis by OSD

have received a good deal of publicity and may have become symbols of the new regime. But the shift in power structure was clearly the main factor. The crucial decisions could have been made (though perhaps not as wisely) and enforced without program budgeting. Indeed, some monumental decisions were made in 1961 before the system was in operation, and since that time important choices have sometimes been reached before the relevant PCPs and the formal procedures had even presented the matters for decision. So the Services should not blame the mechanism for all the things they have complained about, nor should one credit the procedures for all the improvements that may have emerged. At the same time the budgeting-programming arrangements, and other institutional reforms, *do* have consequences. Any change in organizational procedures will alter, to a greater or less degree, the structure of incentives within the system—and, over time, will alter the ultimate outcomes. The five-year plan, for example, could have this result—predisposing, even though not compelling, certain outcomes.

A few other highlights of the new order in defense planning should be mentioned, because they influence the impacts of centralization that will be discussed later. First, in judging the extent of centralization, it is probably significant that contractors seem to regard OSD as their real customer now. In internal documents prepared to help the management of one firm, it is bluntly stated that (1) OSD, not the Services, must be regarded as the customer, even when dealing through a Service; (2) all sales efforts should be oriented around cost-effectiveness analyses *in terms of a specific mission or defense requirement.* Second, OSD is centralizing (narrowing) the sources and the flow of military intelligence. The intelligence chiefs of the Services have lost their formal membership on the U.S. Intelligence Board. In addition, there is the more extensive use of independent agencies like the Defense Supply Agency, Defense Communications Agency, and Defense Intelligence Agency to manage certain activities centrally, with attendant reduction of Service responsibilities.

II. INTENDED BENEFITS OF INCREASED CENTRALIZATION AND INSTITUTIONAL CHANGES

No one would deny that, prior to 1961, there were many things wrong with defense planning. Improvements *were* desirable. And few would deny that the OSD "revolution" brought improvements from the Nation's standpoint. Some of the reasons for the increased centralization and institutional changes, or in other words some of the benefits that OSD aimed to produce, are the following:

BETTER COORDINATION OF INTERRELATED DECISIONS

In the nuclear age, interrelationships among decisions have become more and more important. What was decided about Polaris affected what should be done about Minuteman and the B-52. What was done about carriers could affect the costs or effectiveness of Army units. Decisions about the advanced development of Thor affected the worth of the development of Jupiter. Most persons, including voters and Congressmen, agreed that force-structure and systems-development choices needed better coordination than Service bargaining provided. Perhaps there were other ways to make the bargaining process work better, but to most people steps toward more affirmative OSD control over these decisions seemed to be the best, or possibly

only, way to proceed. Yet, even if this conclusion is accepted, it must be kept in mind that there is a wide spectrum of possible arrangements that would constrain OSD in different ways. At present, for example, Congress, the White House, and other Departments clearly put one sample set of constraints upon OSD choices. Many other patterns of checks and balances can be visualized.

BETTER CHOICES IN GENERAL

There was reason to believe that more efficient choices in general could be made. At least there were plenty of things wrong with the information that was available, the criteria that were used, and the decisions that emerged. OSD hoped to make better choices by doing a variety of things—among them, (1) looking at the full-cost implications of alternative decisions, (2) costing and thinking in terms of "programs" rather than "input categories," and (3) considering alternatives and tradeoffs systematically.

1. Looking at Full-Cost Implications

It is clearly sensible to look at the total costs entailed by alternative actions when choosing among them. When one considers selecting an item to be bought on the installment plan, one should look at the full-cost streams, not merely at the down payments. Yet with the conventional budget, the Defense Department had been choosing purchases frequently with the main emphasis on the "down payment," that is, on the first year's cost. In part the neglect of full costs by the Services and other persons was inadvertent, but in part it represented the deliberate attempt to get a foot in the door. In either case, once a system or a program was partially paid for, only the incremental costs were relevant, and these would naturally be smaller than the full costs. At that point the costs relative to the gains might make it economical to buy the program, even though the full costs relative to the gains would have made it an inefficient use of resources. The five-year financial plan was intended to compel defense planners to take *full* costs into account when making their choices. How much emphasis has really been given or should be given to those "program years" is debatable, but they clearly receive more attention than in the past.

By reducing the number of opportunities to use the foot-in-door tactic, OSD presumably hoped also to improve incentives within the Services. It was hoped, no doubt, that the Services would now find efforts to seek greater military effectiveness from budgets of given size more rewarding than devoting present energies and resources to the quest for larger future budgets. For, except for formal changes, the financial plan is set for five years ahead, and Program Change Proposals have to include their full-cost implications and be reviewed by OSD.

2. Costing in Terms of Programs

Presumably better judgments can be made about the size of Programs or capabilities (which might be called "intermediate outputs") than about the number of particular inputs to buy for an entire Service. Costing and thinking in terms of Programs and program elements is no panacea, of course—judgments are still difficult to make. But one can more nearly answer questions like, "Should we spend more or less for airlift or retaliatory capabilities?" than questions like "Should we spend more or less on military personnel for broad categories within the Air Force?" The

latter decision depends upon what mix of capabilities is desired. Blue Book costs alone provide only moderate assistance because, as noted earlier, they do not show the costs of increments or decrements to various capabilities, and these are the costs that are relevant to most decisions. For this reason, OSD and the Services keep seeking ways to get prompt, even if rough, cost estimates for perturbations in the Blue Book programs.

3. Considering Alternatives and Tradeoffs Systematically

Another aim of OSD, intended to contribute to improved choices in general, was to consider tradeoffs systematically—in light of costs and Programs. Previously it had seemed that requirements or needs were often specified without regard for costs, and cost limits or budgets were often specified without regard for needs. Now, it is possible that bargaining between the need-firsters and the budget-firsters resulted in a better set of choices than would alternative arrangements; but to most people it appeared that there must be a better way. OSD's intent after 1961 was to reject the notions of unique requirements and firm ceilings for the individual Services (or components thereof) and instead to consider quite consciously the gains and the costs of alternative defense proposals.

If each Service proposal was to be examined on its own merits (in terms of costs and gains as compared with other alternatives), rather than in terms of a Service's total expenditures, then no Service should be able to count on some specific share of the total defense budget. OSD may have had ball-park budgetary limits in mind, of course, but at least ceilings for the individual Services were not announced, and each one could hope to get a larger share if its proposals were persuasive. After a time the five-year financial plan imposed a firmer budgetary constraint than had ever existed before, but it was still not supposed to represent in any mystical sense an inviolable budget or requirement. If the gains from a change were judged to exceed the costs, in principle, at least, the change was to be made.

Again there is no denying that the previous system was imperfect. Alternatives and substitution possibilities were not compared systematically. Proposed programs were stated to be absolute requirements, implying that tradeoffs did not exist. Priority lists often put obviously important activities last on the list, again implying that everything was required and no tradeoffs were possible. Part of the difference between the old system and the new was semantic, because under the old procedures the bargaining process *did* yield departures from "absolutely necessary" budget ceilings and "absolutely essential" requirements. But the difference was not *all* semantic, and the new system did represent a change in attitude as well as a change in who made the decisions.

III. POSSIBLE COSTS OF THE NEW SYSTEM

There were then many unsatisfactory features to the old system. However, it would be invalid to reason that imperfections in the old arrangement *automatically* imply that the new one is better—or that it is better in every way. One is obliged to look critically at the costs or imperfections of the new system as well as the old. Even though one regards the new system as superior, it is necessary to examine some negative repercussions of the new system which have received inadequate attention. While in light of an overall appraisal, some may regard these as second-order effects, they should not be neglected. However rewarding existing reforms may be, further improvements can still be made.

In assessing change many observers are inclined to stress the more dramatic shifts in policy or even the stated goals of policy. They are perhaps especially prone to ignore certain costs—e.g., of impairing incentives, of neglecting uncertainties, of placing heavy demands on delicate communications networks—probably because these costs are so hard to measure. If such costs are neglected, people are in effect insisting that performance be improved or efficiency increased—no matter what the cost! However, in such a view there is an underlying fallacy. Unless we look at *full* costs, we can easily be penny-wise and pound-foolish.[4]

The issue most critically susceptible to the neglect of full costs is the degree of centralization in decisionmaking. The apparent benefits (in *future* avoidance of errors experienced in the past) are such that the tendency over time is to acquire additional layers of controls. The benefits are immediately obvious, but the costs—in money, in time and energy, and ultimately in lost options—are far less apparent, especially in the short run. Since this issue of "how much control?" is crucial to the discussion of costs, it is necessary to consider certain aspects of centralization which bear on striking the best balance in distributing responsibility between OSD and the Services.

• While terms like centralization or decentralization are freely employed, any particular arrangement within the spectrum of possibilities must involve some degree of centralization or, to look at the other side of the coin, some degree of decentralization. Wherever there is the possibility that subunits may undermine overall organizational objectives, there ought to be coordination.[5] Yet, on the other hand, no authority is so powerful that it does not have to bargain with anyone. Even aside from external organizations or higher levels (with their allies in one's own organization), any organization must motivate subordinate levels by providing an opportunity to influence policy. In the absence of such exchanges, the chances for eliciting the necessary cooperation and support are slight.

• In any given circumstances, there is no precise balance between centralization and decentralization which is "correct." In fact, there is an advantage in altering the balance from time to time simply to prevent organizational staleness from setting in. All that one can say is that wherever there is even a moderate risk that overall organizational objectives will be *substantially* undermined through "suboptimization" on the part of subordinate units, the case for sufficient centralization to reduce the risk of counterproductive clashes to an acceptable level becomes very strong. On the other hand, if there is little risk that a suboptimizing unit will substantially undermine overall organizational objectives, the opportunity for substantial decentralization exists.[6] Action will depend on the effectiveness with which

[4] Several illustrations may be given of how the single-minded emphasis on a particular aspect of performance or cost may result in reduced efficiency or performance overall. In portions of the value engineering effort, to cite one aspect of the cost reduction program, it is almost explicit: we aim to make contractors more efficient (in producing particular components) no matter how much it costs overall. Incentive contracts, while valuable in many applications, can bring heavy costs in others. For example, tying incentive payments to a delivery date results in what the Russians call "storming" —frantic efforts to meet the delivery date at the expense of anything not clearly constrained by the specifications (e.g., reliability, future deliveries, storage costs). Again the existence of bad effects does not tell one what to do—but it does suggest that we must keep struggling to look at the *full* costs and *full* effects.

[5] While the General Motors Corporation is frequently cited as representing an ideal of efficiency through decentralization, in point of fact the degree of centralized control in that case is usually understated by protagonists of decentralization.

[6] The fact that a lower level organization suboptimizes rather than acting in direct response to highest organizational objectives does not necessarily lead to harmful results. Officers working on a particular project or for an operating command need not ask themselves each morning how that project or that command relates to overall national security objectives, no more than a businessman need ask each day what he is doing for GNP. It is certainly not essential, and may well be harmful, for someone with the overall view to make all decisions. Bargaining processes in large organizations (like the price mechanism in the economy at large) can frequently turn private (suboptimizing) "vice" into public virtue.

subunits could perform their duties. One should note that the lower the costs of operations relative to the costs of communication or coordination under such circumstances, the more powerful is the case for decentralization. Speaking more concretely, for the expensive and interrelated force-structure decisions, major control at the center appears essential, but for inexpensive and independent R&D decisions which need not damage overall objectives, the case for decentralization is powerful.

• Implicit in any organizational structure are risks of errors, both of omission and commission. The particular set of risks varies depending on the organizational structure. It is therefore not sufficient to show that a particular organizational structure creates risks; one must specify the area of risk and alternative arrangements which will reduce those risks. We feel that too much control may too easily be exercised over R&D activities—control which is *very costly in relation to total resources involved in the activities per se.* Peculiarly in the development of new ideas must every effort be made to prevent the crushing of inventiveness—and avoiding creative talent being devoted either to anticipating what higher authority would like to hear or the attempt to communicate upward an unjelled idea. In R&D one wants great diversity and to avoid "consistency" like the plague.

• While, in such areas as R&D, decentralization should be fostered and exploited, the specific form that decentralization takes is not a constant. In particular, though the *historical form* of decentralization has been through the Services, it should not be assumed that this is the only form that decentralization could take in the future. Nevertheless, partly because of size and established structure, it is an obvious form. Since the predominant stress since 1947 has been in the direction of unification, integration, and coordination, it is perhaps desirable to recall that the original impetus toward decentralization reflected the inability of established authorities fully to exploit new military hardware and concepts. The establishment of the Air Force, for example, reflected the unwillingness of the older Services to take aircraft seriously as a new strategic system. New systems may be accepted too late, but one should not assume that decentralization will *delay* that acceptance. Germany's failure to press rocket development energetically may have cost dearly in the Second World War.

• The Office of the Secretary of Defense, it should be recognized, is not monolithic. Substantial decentralization (and controversy) exists within it. DDR&E is visibly a center of independent power, and between it and the Comptroller's Office, for example, there appear to be frequent divergencies in view. Nevertheless, one would be wise never to assume too wide a divergence within any single organization. Most public or external discussion will inevitably constitute a defense of the policies adopted by the organization. To be sure, *internal* discussion can reveal significant differences. Under the best of circumstances, these differences will be keenly marked. Nevertheless, for the most part, individuals within any organization will reflect either the personality of the organization (if based on a permanent cadre) or the views and the personal style of existing leadership (if there is mobility into and out of the organization). In the long run divergences based on differing interests are most effective in divulging differences in ideas.

• Just because activities are controlled by the separate Services does not necessarily imply that there need be decentralization—unless appropriate steps are taken. The Services separately and even jointly may be dominated by one view. In the fifties this was roughly the case. Decentralization through the Services is feasible, but it is not inevitable. Existing controls, in fact, make beneficial decentralization difficult to achieve.

Given these observations, let us turn to the new OSD system, keeping in mind

that it is not a single package to be considered on an all-or-none basis. Though many parts of the system may provide substantial advantages, others may yield net disadvantages. It is these features in which we are interested, for here the opportunity exists to obtain further improvement—through the modification of costly or disadvantageous techniques.

Our major concern is with longer-run effects, including effects one might anticipate as successive sets of OSD officials take over the reins. It cannot be foretold how much military insight future generations of civilians in OSD will possess. If forced to hazard a guess, one should assume that familiarity with military issues will remain at a much higher level than has historically been the case. What does seem certain, however, is that the energy and imaginativeness of OSD personnel will decline. What has occurred in OSD is not dissimilar from what occurred during periods of creation or transformation in other government agencies and departments. The challenge of change attracts individuals of extraordinary merit. When creative fervor wanes, such individuals go elsewhere. As persons with lesser overall ability inherit the system, lacking experience in its creation and in the reasons for change, the nation may reap fewer of the benefits and begin to incur heavier costs. To be sure, under any system a decline in ability will tend to reduce performance, although de facto decentralization, flowing from reduced ability at the center, may mitigate the effects. Nevertheless, a system placing a disproportionately high premium on the imaginativeness of a few critically placed men is peculiarly vulnerable to a decline of ability in men in key positions.

Moreover, many benefits of the new procedures have depended upon considerable indifference to politics on the part of Pentagon decisionmakers—a condition which cannot be assumed to be permanent. On the contrary, major Cabinet positions have on occasion gone to purely political appointees, and rarely to men who cannot be swayed by political considerations. Moreover, when men enter office determined to resist political pressures, as the years progress they acquire commitments which very much reduce their flexibility.[7]

Since the emphasis in this Paper is on long-run costs, more of them remain as yet either invisible or barely discernible. For the time being we are in no position to indicate how important these costs are, or in some cases whether they really exist. Indeed, even in the future, no one will be able to demonstrate rigorously how serious these costs are. They will take such forms as "too few" alternatives being considered or "too few" hedges against uncertainty being adopted, and since nobody can show conclusively what is correct, they cannot show conclusively what constitutes "too few." For these various reasons, we cannot present measurements—we can only present the arguments for consideration. Also, even if, on the basis of these arguments, one judges that modifications of the OSD system are in order, how far to go requires a still more difficult judgment.

[7] The Navy version of the TFX fighter is reported to be overweight, causing considerable difficulty in both takeoff and landing on carrier decks. The Navy is reported as wishing to acquire the F-4C in larger numbers as a substitute. Given the earlier commitment of the OSD both to the concept of what was originally the tri-service fighter (later the Air Force-Navy fighter) and to "commonalty" as the criterion for selecting a contractor—in addition to the lengthy and heated controversy touched off by the award to General Dynamics—the possible embarrassment to OSD makes it improbable that OSD would accept the Navy's view irrespective of cost-effectiveness considerations. As a general rule, one may observe, it is easier to cancel commitments made by earlier decisionmakers than it is to cancel one's own.

THE THREAT ENVIRONMENT AND THE ORGANIZATIONAL STRUCTURE

Moreover, it is important to recognize that the optimal decisionmaking structure for the military establishment varies with circumstances. In this respect the determining element is the way in which the threat environment is perceived. For planning under all but the simplest conditions, by far the most important considerations are the existence of uncertainty and the mechanisms designed to deal with it. In defense planning, lack of precise knowledge regarding present or contemplated actions on the part of potential rivals, reinforced in recent years by the rapid change in military technology, accentuates the role of uncertainty even above that prevailing in ordinary planning.

Under some conditions the relative stress placed in military planning on the ever-present uncertainties may diminish. Whenever a specific threat has very high probabilities ascribed to it or whenever such a threat despite lower assigned probabilities appears overwhelming in its consequence, a concentration of effort on that specific threat is both likely and defensible. Something of this sort did take place during the middle and late fifties and continued into the early years of the Kennedy administration when widespread concern existed that the Soviets were striving to attain strategic superiority. Under these conditions many uncertainties were neglected, including those revolving around "lesser" threats, and the bulk of American activities centered around the countering of what was perceived as the main threat. When such considerations apply, centralization may have a larger role to play, and the suppression of alternatives that may accompany centralization may be viewed as a lesser cost.

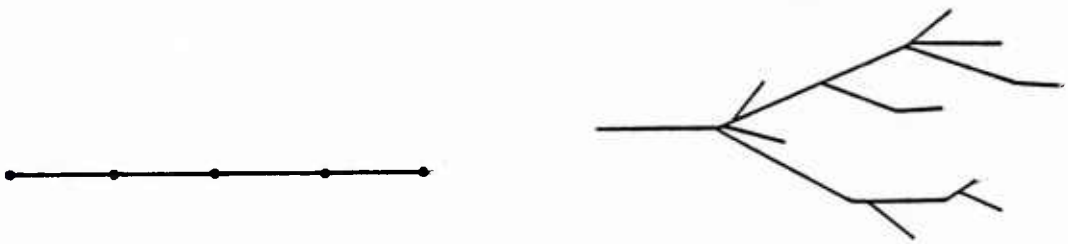
The current threat environment bears little resemblance to the one just described. There exists a variety of threats which could be posed by the Soviet Union, China, or possibly others. None appears sufficiently probable, or potentially so overwhelming in its consequences before we could respond, as to justify the procurement of expensive new systems as countermeasures. With time and the improvement of our ideas, it seems probable that more effective countermeasures to any such threats can be devised at a later date. The existing threat environment calls not for costly crash efforts to counter dominant threats but the maintenance of an across-the-board program consisting of numerous and austerely-conducted projects providing us with future options quickly to counter whatever specific threat does materialize. Such an objective can be undermined by too much centralization, which results in a premature screening process and/or in disproportionately heavy outlays for organization and communication. Particularly in an environment dominated by a range of relatively distant and imprecisely formed threats is *decentralization* desirable, for that permits investigation of the technical feasibility, processes, and costs of new hardware ideas before subjecting the latter to the withering blast of specific mission requirements or conformity to *existing* doctrine.

THE APPROPRIATE PLANNING CONCEPT

Given the grave uncertainties which are ubiquitous in defense choices, what kind of attitude should be taken toward planning by decisionmakers at *all* levels (save in the operating commands)? What concept of planning is appropriate not only in OSD, but in the Air Staff, the Air Force Systems Command, and the like? Above all, it should be a concept that is conducive to (1) facing uncertainties (not pushing them aside) and (2) hedging against uncertainties (i.e., not biased against hedging). Plans should be based upon an *accurate* appraisal of the future—and an accurate

projection is that future strategic contexts are unclear, future technology is uncertain, human judgments are fallible, performance of systems is never certain.

In such a world we need not Cook's-Tour planning, which rests on the view that the future is sufficiently certain that we can chart a straight course five years ahead, but rather what we shall call Lewis-and-Clark planning. In the latter case we recognize that there will be many forks in the road and many alternative courses of action, but their precise character and timing cannot be anticipated. *At the end of a period* we can look back on the paths pursued, which include many abandoned experiments, many hard (and possibly erroneous) choices, and the like, but we could never have anticipated which options would be chosen, when the choices would be made, and how long alternate courses of action would be pursued before abandonment. Retrospectively what took place may be mapped (as in the diagram), but the planning function is not to chart a precise course of action; rather it is to prepare to deal with the uncertain terrain of the future, to note the signs in the environment that a decision point has been reached, and to respond in a timely fashion.



Cook's-Tour planning

Lewis-and-Clark planning

In bureaucracies decisionmakers are continually tempted to go too far in the quest for Cook's-Tour planning. This can be done only at the cost of neglect of uncertainties, lost flexibility, neglected and suppressed options, and less-than-optimal adjustment to changing opportunities and threats existing in the external environment. In evaluating any planning procedures, we must be on guard against the tendency to exaggerate the extent to which the future can be foretold and planning for it precisely charted. Modifications can be introduced into a system which will permit greater recognition of uncertainties and enhanced flexibility in the face of change. At times organizational requirements may necessitate the tentative outlining of a Cook's-Tour plan, but under such circumstances a major effort should be made to highlight in the minds of all concerned that the proposed arrangements remain tentative.

IV. CONSEQUENCES OF CENTRALIZATION: SOME HYPOTHESES AND PRELIMINARY EVIDENCE

In this section two questions are dealt with: (a) a hypothetical treatment of the impact of centralization, and (b) a tentative analysis of the impact of the new procedures in specific decision contexts. The first issue is itself subdivided under three headings: (1) the ultimate suppression of alternatives, (2) neglect of relevant impacts on cost or effectiveness, and (3) neglect of uncertainties. It should be stressed, however, that these three are not separate elements but rather facets of the overall problem of centralization. They are thoroughly interconnected and no precise dividing line can be drawn. Because of these interrelationships no attempt is made to classify the subsequent discussion of the effects of centralization in

specific decision contexts—i.e., studies, R&D force structure, etc.—under these subheadings. In each case the precise weight that might be attributed to (1), (2), or (3) will vary. Finally, while the discussion is couched in terms of the effects of OSD centralization, it must be emphasized that the tendencies described exist in *all* cases of centralization. The Services themselves have in the past provided striking examples of these tendencies.

THREE HYPOTHETICAL EFFECTS OF CENTRALIZATION

1. Ultimate Suppression of Alternatives

Although OSD may set out to examine alternative actions systematically, there is danger that the system will work to shorten the list of alternatives that are *seriously* entertained.[8] Consider the way issues are decided when major (and many minor) choices are made by OSD and when the bargaining strength of dissenters is drastically reduced.[9] First, the views of one group will face fewer checks and balances and will play a greater role than before. In most of the big choices, there are no demonstrably correct answers eliminating the need for heroic judgments. Judgment has to be decisive. Any small group will be inclined toward certain views—their exact substance does not matter. Suppose, as hypothetical examples, the small group favored increasing our commitments in a particular theater, or suppose it favored decreasing our commitments. Suppose it was stubbornly convinced that a particular weapon systems concept was the only answer or, on the other hand, was doggedly opposed to it. Centralization of authority would tend to foreclose serious consideration of alternatives that inter-Service bargaining would air more seriously.

Since "inter-Service rivalry" is treated in this paper as one route through which benefits of decentralization may come, it seems advisable to say a few words regarding what such rivalry may reasonably be expected to accomplish and *what it should not be expected to accomplish.* If one were to expect thoroughgoing and rigorous competition among the three Services—along the lines of the traditional competitive model—one would be doomed to disappointment. The three Services should be expected to exhibit the kind of competition which typically exists "among the few" when there is recognition of mutual dependence. One would expect the "competitors" to recognize elements of both rivalry and cooperation in their relationship, to establish a pattern or code which limits rivalry, and to be forbearing in their relationships in the sense of avoiding disturbance of that pattern through frequent and direct assaults on one another. Moreover (and this runs against the grain of OSD's intention of fostering competition among the Services), the existence of a powerful "outsider" who intends to exploit divisions between the "competitors" may actually encourage them to close ranks. Particularly is this so when conflicts of interest can be passively passed up to higher authority for decision, thereby avoiding open ruptures.

[8] Reflecting extensive experimentation by social psychologists, it has now become almost a commonplace that a centralized communications and decisionmaking structure permits more rapid achievement of consensus and more rapid making of decisions *through the suppression of disagreement and of deviant expressions of opinion.* On this point, see for example, P. M. Blau and W. R. Scott, *Formal Organizations,* 1962, Chandler, pp. 125-128, 131, 134, 139. This manner of eliminating alternatives is reinforced in a hierarchical structure by self-censoring and the suppression of ideas on the part of lower echelons.

[9] Many of these points were discussed several years ago in the very perceptive paper by Alain C. Enthoven and Henry S. Rowen, "Defense Planning and Organization," in *Public Finances, Needs, Sources and Utilization,* A Report of the National Bureau of Economic Research, Princeton, N.J., Princeton University Press, 1961, pp. 365-417.

What one can expect from such rivalry, even when modified by recognition of mutual dependence, is that each Service will harbor diverse and divergent ideas on major national security issues. Such conflicts may normally not result in overt clashes, but are nonetheless valuable. Such pools of concepts diminish the risk that dominant but obsolescent ideas will persist too long without challenge. On rare occasions conflicts will be brought into open controversy—with enormously beneficial effects on defense policy. The B-36 carrier controversy is, of course, an example frequently cited. A more recent example, and one perhaps more revealing of the potential benefits, is the resistance of the Army throughout the later fifties to the heavy emphasis on general war capabilities and especially to the sharp reduction in capabilities for limited warfare. Leading defense officials since 1961 have repeatedly underscored their belief that the Army's earlier contention was correct and of utmost importance. Yet, the effect of restructuring the Department of Defense certainly is to diminish the chances that a position like the one regarding limited war capabilities can be presented with similar effectiveness in the future.

Finally, one should not assume that the kind of limited rivalry exhibited by the Services is one that applies exclusively to them. There is plenty of evidence that the subunits of the OSD have recognized, as they must, the interplay of rivalry and cooperation in running themselves and govern their behavior accordingly. There are exactly the same kind of tacit agreements to avoid continued controversy and to recognize areas of dominant concern that exist among the Services—or under any organizationally similar conditions.

In the long run, inter-Service rivalry and criticism are probably better than good intentions at flushing out new alternatives and criticizing obsolete activities. Numerous illustrations may be cited. The most dramatic example, however, is that of the Polaris weapon system. The conception of the system and the determination exhibited in its development is in large measure attributable to the Navy's special interest in developing a competitive strategic system in the missile age. The beneficial effect on *overall* security is clear. Yet, there is reason to doubt whether under the new system, approval could have been obtained and development energetically pursued. Especially is this so in light of the exacting scrutiny on the basis of cost-effectiveness calculations of new mission concepts very early in the development cycle. The heavier the reliance on one group's views, the greater the risk of neglecting alternatives.

Second, with greater centralization, simplification of the task of choosing and controlling becomes imperative. Affirmative control or the attempt to exercise such control in an organization as large as the Department of Defense requires arraying the alternatives quickly, focusing on the main considerations quickly, and making choices quickly. All this means the rapid screening and disposition of alternatives and the use of rules of thumb to help with this task. In other words, the pressures on a small group at the top make the cost of fully exploring numerous alternatives high, and eventually the quest for alternative solutions is likely to become less eager.

Third, the incentives at lower levels, i.e., within the individual Services, to invent alternatives, may be dulled.[10] They are constrained by lack of authority to

[10] That the military services have not always displayed their present enthusiasm and vigor in pursuit of new military technology is suggested by the following quotation from James B. Conant, *Modern Science and Modern Man*, Doubleday Anchor edition, 1952, p. 116:

> In 1940, those of us who were in Washington as civilians were concerned mostly with the technological conservatism of the men in uniform. I will relate no stories to prove the point. The conflict between the professors and the "brass" is too well known. Most of the versions do less than justice to the military man and give too much credit to the professor. Be that as it may, what I am concerned with is not the technological conservatism of the men in uniform

make innovations, and, if they are reasonable, will find it less rewarding than before to think about innovations. Change is inconvenient, and designing possible innovations takes effort; the lower the rewards for this activity and the greater the pressures for other activities, the less the effort that will be devoted to designing alternative courses of action. In these circumstances, it may eventually seem more rewarding for the Services just not to rock the boat.

2. Neglect of Relevant Impacts on Cost or Effectiveness

In choosing among weapon systems or other alternatives, one always risks neglecting relevant impacts on costs or effectiveness. At best they are hard to perceive and measure. A decade or so ago many people thought the effectiveness of U.S. retaliation forces was adequately reflected by their ability, undisturbed by any enemy strike, to inflict damage on that enemy's installations or economy. Gradually it was recognized that vulnerability of U.S. forces had much to do with their effectiveness or deterrent capability, and also that the character of nuclear striking forces had much to do with the deterrence of minor aggressions, with the course and outcome of a war should deterrence fail, even with the chances of reaching acceptable weapons-control agreements. Thus crucial impacts on effectiveness, and also on costs, are often difficult to anticipate.

How might centralization—in the long run—add to this difficulty? First of all, if the views of one group ever dominate the debate sufficiently to make the expression of dissenting views ineffective, it could reduce the likelihood of all effects of alternative systems being aired. For it was mainly voices of dissent, not sophisticated management, that finally made us face additional impacts of alternative strategic capabilities. Just as one group is likely to consider a narrower menu of alternatives than would a multiplicity of groups, one group is also likely to recognize a narrower range of effects than would a multiplicity of groups.

Now so far a variety of views are unquestionably being expressed. The Services forcefully present their positions, pointing out considerations regarding strategic or tactical forces that they fear are being neglected. Outside criticism still calls attention to effects that outsiders believe are underemphasized. But in the long run centralization could diminish the extent to which dissenting positions are presented. How much dissent is it desirable to preserve? Presumably no one knows in any precise fashion. All one can say is that the dominance of one set of biases instead of competition among several sets of biases could be very costly.

What about systems analysis or cost-effectiveness analysis?—won't such comparisons of alternatives insure that we perceive *all* costs and *all* effects? Properly used, systematic comparisons can help enormously. Systems analysis was and is intended to help reveal all impacts on costs and effectiveness, not merely by quantifying some of them but by forcing us at least to recognize the others. It does not guarantee this, of course. Biases, e.g., by the Services, can distort the analyses, though inter-Service competition may help expose distortions or gaps. Also quantifying certain effects may blind the unwary to extremely important nonquantifiable considerations.

There is always a legitimate question as to whether quantitative analysis is

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in 1940 but the almost fanatic enthusiasm for research and development of their successors in 1952. It is a phenomenon not unlike that of an old-fashioned religious conversion.

It should be a continuing concern of the Department of Defense to maintain initiative in the Services and forestall a return to a traditionalist attitude toward changing military technology. A key element in *preserving* an initiative attitude may be the *full* participation both in R&D and in weapon-system selection.

helping or hurting. To that question one leading defense official has provided the following answer: "My general impression is that the art of systems analysis is in about the same stage now as medicine during the latter half of the 19th century; that is, it has just reached the point at which it can do more good than harm, on the average." [11] In this light it is clear that analyses should serve only as an aid to decisionmakers, who should avoid applying or appearing to apply analytical findings mechanically. This is particularly true since analyses, like medicines, vary in quality around "the average," and the quality of individual analyses is hard to assess, and, also, since quantitative analyses, like medicines, may be helpful in some problems and damaging in others.

A review of any good cost-effectiveness analysis will show how analysts must grope their way toward relevant costs and gains of the alternative systems.[12] No one says, "Here is the objective, these are the three alternatives, one part of the team will cost these alternatives, and another part will trace out their effectiveness in achieving the objective." What really happens is constant interaction that keeps modifying ideas about objectives, the alternatives, and the costs.

As the work proceeds, dissent and discussion reveal that objectives other than those first considered are affected by the various systems. As a consequence, measures of effectiveness change, and the alternatives are redesigned. As rough cost estimates are prepared, someone may realize that particular features of a system increase costs greatly yet may not contribute much to effectiveness, and the alternative systems are redesigned. As effectiveness is explored, it is realized that certain new features might contribute greatly to effectiveness yet cost little, and the alternatives are redesigned. Indeed, it may appropriately be said that the real contributions of systems analysis have been the invention of new ideas and the appreciation of additional impacts (growing out of the examination of clashing views)—rather than the final cost-effectiveness exhibits.

Viewed in this light, systems analysis can be of considerable value in uncovering well-hidden considerations bearing on cost or effectiveness. But systems analysis provides no panacea. First, there must be full appreciation of the role of groping and criticism in providing relevant and up-to-date answers in a complex and ever-changing world. Moreover, there must be recognition that measurement is not the final criterion and that many important questions elude quantification. This skepticism regarding the quantifiability of defense problems is unfortunately not always present; sometimes the rhetorical question is absent: "What else can one do but *measure* to the best of his ability? What is a better alternative?" The answer is that of course we should try to make use of relevant measurements yet also to recognize that measurements have varying degrees of relevance, that there are nearly always relevant considerations that cannot be quantified, that life is tough and choices are hard. To use a simple easy basis for making inherently hard choices is to use a wrong basis and make wrong choices. Cost-effectiveness should be regarded only as an aid, sometimes only a slight aid, in reaching decisions.

It may reasonably be asked whether the current generation of OSD practitioners will forget the limitations of systems analysis. Insofar as the new techniques are their own creation and insofar as it may be assumed that quantification can be pushed beyond its proper boundaries, there is some danger. Undoubtedly, however, the main danger lies in the longer run, when the public or some new OSD management may have formed an erroneous concept regarding how much cost-effectiveness

[11] Address by Deputy Assistant Secretary Alain Enthoven before the Naval War College, Newport, Rhode Island, June 6, 1963.

[12] For one of the classics, see A. J. Wohlstetter, F. S. Hoffman, R. J. Lutz, and H. S. Rowen, *Selection and Use of Strategic Air Bases*, The Rand Corporation, R-266, April 1954.

analysis can accomplish. By then analytical techniques may have become formalized —and may be regularly misapplied. There is even danger that studies will retrogress toward the asking of the very type of partial and oversimplified questions that provided the impetus for the development of systems analysis.

Even in the short run, problems of this type are fostered by centralization. There is cause for concern in the fact that OSD serves in the twin roles of advocate and judge. Every organization exhibits a natural tendency to put its best foot forward. When the Services in the past wanted to sell or defend a program, they bent every effort to make it look good. This effort included a judicious amount of undercosting, but rivalry and criticism often brought this to light. However, OSD itself may have favorite systems that it wishes to introduce or, once introduced, to defend. Officials may overstate effectiveness or understate costs.[13] Since the relations between the OSD and the Services are not symmetrical, the latter are both reluctant and more restricted in their ability to expose erroneous calculations. The less the extent of criticism and competition, the less likely it is that these errors will be brought to light. Shielding analyses from effective bargaining and criticism would in the end result in their being used to justify particular preconceptions and emotional commitments.

3. Neglect of Uncertainties

One extremely troublesome and important problem which exists in all decision-making structures, but which *could* be intensified by centralized planning, is the treatment of uncertainties—uncertainties about future strategic contexts, about the functioning of systems, about actual operations, and so on. Might not such uncertainties be neglected in the long run? The same forces noted earlier could help bring this about. The judgments of one group would not be likely to take seriously as many contingencies and uncertainties as the judgments of several groups. One group might have particular convictions regarding Missile A's performance or about the likelihood of war in Europe and might make choices as though there were relative certainty. If other groups have bargaining power and can therefore make their judgments felt, more of the true uncertainties may be forced into the picture, and decisions may be made with full awareness of their existence.

Also, as noted before, with greater centralization it may become imperative to simplify the task of choosing, and one tempting route to simplification is to neglect uncertainties. This is particularly so when OSD (with its role as advocate) is making the case for a particular solution to a problem. But, even at best there is a very natural human reluctance to face inherent uncertainties—all of us try to find a number, a formula, a half-page summary without messy qualifications. We often need some kind of force to compel us to recognize contingencies and variable outcomes. Without enough bargaining pressures, a group can put too high a premium on simplifying choices.

Finally, the incentives of the Services to recognize and hedge against uncertainties may be dulled. For example, how would one expect the Services to react to the Blue Book rules? To a greater extent than before, the Services must live with their cost estimates. With OSD control they have an incentive to estimate costs accurately several years ahead and to avoid "overruns." From most standpoints, of course, this is desirable. But it does bring a cost. It is likely to foster the exploration of relatively

[13] The treatment of Minuteman could be a revealing case in point. There is reason for speculating that more than a coincidence exists between the fact that there has been continuing and very serious undercosting of the operations and maintenance costs for Minuteman and the fact that the latter has been a much preferred weapon system for OSD.

"safe" or "conservative" proposals and a neglect of radical ideas, hedging against contingencies (where the gains are highly uncertain), and long-range planning (where uncertainties abound).[14] No one can say just how much speculative thinking and hedging is "correct," but if we want to increase it over the long run, decentralization probably would work in that direction.

With costs supposed to be nailed down five years ahead, consider how a Service is likely to reason. Consider a proposal whose worth would be huge *if* certain contingencies occur or *if* technological problems can be solved or *if* certain loopholes can be filled. These facts mean that there are numerous possible outcomes. Some uncertainties can be resolved at a cost, e.g., by parallel R&D approaches, or by modifying the system to make it function better in a wider range of circumstances. But including these costs appears to make the proposal more expensive and also may make it sound as though one does not know quite what one is doing—which is true. This makes it less likely that the scheme will be accepted. Also, even if accepted, the proposal might later involve "overruns," which are not merely embarrassing but may entail cutting back on some other program. The Service's choice may be, not just to consider these uncertainties, which certainly should be taken into account, but to advance invariably the more cautious proposals. In deciding what combinations of proposals to press for, the Services will take into account the chances of acceptance, the effect on the prospects of the items they especially desire, the future difficulties that may be entailed.

The tendency to neglect uncertainties is deep-seated in all organizations. In so vast a structure as the Department of Defense, it must be resisted at all levels—not only in the OSD, but in the Services as well. The forces we have discussed were at work within the Services long before 1961. Uncertainties were neglected, but each Service was inclined to neglect a *different* set. Rivalry among them often forced each to worry about contingencies, to explore offbeat ideas and parallel approaches, to take a chance on risky proposals. The danger is not only that the OSD will neglect uncertainties, but that the new procedures will lessen the incentives for the Services to be alert to uncertainties and to develop measures for dealing with them. A *genuine*, though partial, dispersion of power—for example, giving the Services and possibly the Unified Commands some untrammeled funds for R&D—may in the long run be well worth the cost.

IMPACT OF THE NEW PROCEDURES IN SPECIFIC PROBLEM AREAS

Emphasis in the preceding section by necessity has been directed toward centralization's longer-run consequences. But the long run is not wholly separate from the short. It would seem plausible to assume, therefore, that on the basis of three years' experience with the new system, certain preliminary indicators of the suggested dangers would have appeared and that we should now be able to designate major problem areas. In this subsection the effects of the new procedures in specific decision contexts are examined in greater detail. Since the long-run problems previously discussed are wholly intertwined, no attempt is made to isolate cases in which alternatives may have been prematurely suppressed from cases in which uncertainties were ignored.

[14] This kind of "safe" bias in proposals may lead, of course, to highly "unsafe" gambles, as some of the contingencies do in fact materialize.

1. Studies

If the approval of studies is controlled too closely by one group—and it could be any group—the kinds of effects we have discovered should be anticipated. Judgments about what things are worth studying will reflect one group's appraisal of the future. Some ideas will be discarded because only certain pictures of disarmament, future strategies, or alignment of nations are believed to be in the cards; some technological advances will not appear to be worth thinking about because they are judged to be too costly or impossible. Yet there is vast uncertainty about most of these matters, and a multiplicity of judgments is likely to reflect that uncertainty more accurately.

Although relatively inexpensive, as defense alternatives go, studies are also relatively unattractive to most administrators, because individually studies are long-shots. If they are judged as a means of exploring a single group's ideas about strategy, the screening process can be rough. Nobody would think of applying cost-effectiveness analysis to studies, yet detailed justification and screening by one small group may have a somewhat similar effect. Studies will be chosen which in their view offer obvious prospects of success. Studies that involve costly attempts to resolve uncertainties or those that promise uncertain (even though large) gains are unlikely to survive. There will probably not be much sympathy for "pipe-dream" studies—say the possible new *military* implications of space. If OSD controlled study choices (or other choices) too closely, the Services might gradually lose incentives to make long-range studies.

Although mainly concerned about the future, some of these things seem to be occurring now. Money for the study of possible defense activities in space or of other futuristic ideas has apparently been rather hard to get. In the early years of the new administration, references, both dire and facetious, were made to the so-called "Study Gap." While the phrase may have been too melodramatic, it did point to an important reality: particularly in certain areas that officials judge to be unpromising or off limits, study approval is subject to both serious risks and delays. Already there is a tendency to concentrate on studies that look good, i.e., about which people already have enough information to show that they look good and about which there is therefore less uncertainty. Already the Services seem to be showing battle fatigue and losing some of their zest for long-range thinking.

There is an additional problem: within any bureaucratic structure pressures exist to force analyses of alternatives to become design studies. Within each Service, for example, after a study of alternatives is begun, participants and successive echelons of reviewers perceive that certain of the alternatives are frowned upon by higher-level officers. It seems useless, perhaps even hazardous, to make the full case for the unpopular alternatives. Gradually the case *against* these alternatives is stressed more and more, or they are dropped from the picture altogether. The project turns into a design study—the design of one "required" system rather than an objective comparison of alternative systems.

The existence of the several Services implies, however, that on major issues several alternatives may survive the screening process. If the Services exert little influence, however, OSD studies may ultimately become the only mechanism for exposing alternatives. But they too may turn out to be design studies. Participants and echelons of reviewers will recognize what top management wants. If the highest levels are indifferent, subordinates will support their own preferred courses of action and those preferences will be perceived by the ones who prepare and review the study. The long-run outcome may be, as it often is within the Services, design studies rather than objective comparisons of alternatives. The popular course of action may

be compared with "straw men," or the unpalatable alternatives may simply drop out of the analysis altogether.

2. Research and Development

If we look to the future and for the means to hedge against its uncertainties, the R&D program (at least in light of the existing strategic balance) may be considered even more important than immediate outlays on forces. In a sense the R&D program represents the future force structure. More than that, it provides the options for modification and adaptation of existing weapon systems to exploit new technology, to counter new threats, or to seize new strategic opportunities. When we look toward the future, any tendency to suppress alternatives becomes virtually synonymous with the failure to deal with certain classes of uncertainty. The R&D program should be designed to create and maintain as broad a spectrum of options as possible—some of which will prove to be optimal in as-yet-undetermined future contingencies.

If the costs of individual R&D programs were equal under OSD-centralized or Service-decentralized structures, it could be said with some confidence that in the long run the range of alternatives emerging from the study program and receiving serious R&D attention would be narrowed under centralization. For under centralization all proposals after going through successive screenings must pass finally through a one-group screening. If the Services had more authority over the use of R&D funds, proposals could be accepted if they passed through any one of three different screens. Whatever the disadvantages of the latter arrangement, it seems likely to have one advantage—the exploration of a wider range of alternatives.

However, there does exist the usual tradeoff between cost and quantity. The more costly the individual development activity, the fewer the number of developments that can be carried on. Thus the case for decentralized R&D activity is powerful only if the Services can conduct individual activities on an austere basis, thus permitting the exploration of multiple viewpoints. The belief is widespread that in fact the conduct of R&D activities under Service management has been the reverse of austere. It would appear that a substantial reduction in the costs of major R&D efforts is indispensable both in obtaining the benefits of decentralization and in providing a convincing political case for decentralization.

If one were persuaded that the costs and the supply schedule for R&D activities under unfettered Service management were under good control, one could argue more persuasively that demand "coordinated" by the OSD tends to be more narrowly restricted than the joint demand on the part of the Services. The problem of uncertainties is particularly serious. The tendencies discussed earlier—reliance on the judgments of fewer persons, the need for making hard choices simpler and easier, the incentives to avoid uncertainties within the Services and among contractors—are likely to yield more cautious proposals and more severe screening of proposals. Is this bad? Well, it depends on how far it goes. At the extreme, only well-understood ideas could be justified and explored. With enough central control, the Services too would turn even more diligently than at present to central screening; proposals and changes would be reviewed to death. No one would make obvious errors, but no "unsure bets" would be placed. We might be highly efficient as far as the existing range of possibilities was concerned, but doing little to increase the range of possibilities.

Again, it may be asked whether cost-effectiveness analysis and a detached scientific approach to R&D choice will save us. Wouldn't it be better than institutional arrangements to promote bickering and a diversity of explorations? Again it is a matter of how far things go. But some basic features of cost-effectiveness analysis

ought to be more widely understood. As an aid and only an aid to decisions, good cost-effectiveness analyses need not blind anyone to uncertainties. The greater the degree of uncertainty, of course, the less conclusive the findings of such analysis, and the more warily those findings must be employed. Nonetheless, there *is* danger of misusing cost-effectiveness analysis when applying it to R&D choices, and if one group has most of the bargaining power, the cards are stacked in favor of ultimate misuse. If employed carelessly or hurriedly, cost-effectiveness analysis can foster the neglect of uncertainty. It calls attention to costs and gains you know about, but cannot call attention to those you don't know about. Yet in R&D many costs and gains fall into the latter category.

For example, specifying objectives has to be done for cost-effectiveness analysis, yet it is hazardous. In choosing among deterrent postures, a good deal of stumbling occurred before it was realized that a second-strike, not merely a first-strike, capability was one of the main objectives. Only gradually was it then perceived that the deterrent posture also affects the achievement of other vital objectives. There is usually no "correct" mix of objectives, but one has to use particular mixes for analyses. Sensitivity analyses can help us avoid forgetting about tradeoffs and uncertainties, but that help is limited. Fully as important, no one knows exactly how proposed developments would affect the achievement of these objectives. Nothing is wrong. It is the best we can do, but we must remember that our best is none too good. There *are* multiple objectives, there *are* tradeoffs among these objectives, there *are* uncertainties, and heroic judgments *must* be made.

Along this same line, if the only way to justify even advanced developments is to link them with specific requirements or objectives, some worthwhile projects are certain to be eliminated. Insistence that missions be specified prior to development work will lead to too narrow an R&D menu. As others have noted, prior to the use of the wheel, people might have been hard pressed to spell out important requirements for it. We could appropriately tie R&D proposals to recognized missions if the latter could be fully spelled out. But given our ignorance of the future, two missions that ought to be recognized are (1) acquiring information, and (2) hedging against contingencies. These should be written into every cost-effectiveness study in capital letters, because in real life they are among the most important military requirements or objectives. Even engineering and systems developments affect these objectives.

Even after components are allegedly developed, putting them together often involves great uncertainty and yields valuable information. The Defense Department obtained important knowledge from Navaho, Skybolt, and many other "failures." That is not to say that they were the cheapest ways of getting the knowledge. Nor is it to say that stopping them was wrong. On the contrary, the point is that there are great uncertainties and there should be occasional cancellations. If there never were any, even in operational systems development, something would be wrong.

To come back to the main point, screening of R&D proposals that relies heavily on cost-effectiveness can bias us against developments that involve great uncertainties, yet some such projects are extremely important. The DoD directive regarding Project Definition (No. 3200.9) places considerable emphasis on the use of cost-effectiveness in deciding what projects can go into Project Definition and thence into engineering or systems development. There is nothing wrong with using good information in reaching this decision. Moreover, making it harder to get a system approved may, if other circumstances are propitious, induce the Services to give more attention to components in exploratory and advanced development. (Sometimes the Services may have felt that proposing certain component developments reduced, or

at least postponed, their chances of getting a full-system commitment.) Coupled with a slight dispersal of decisionmaking power, this project definition arrangement could work out well. Coupled with a high degree of centralized control, however, it is very likely to result in premature screening of alternative approaches.

Moreover, in addition to the direct effect of DDR&E screening of research proposals, tighter OSD controls over procurement, force structure, and operational decisions may ultimately make the Services less concerned in general about research. Will a Service continue to be as keenly interested as at present in basic research, systems analyses, or exploratory R&D—when the findings affect decisions that are to be made by someone else? Or will the Services gradually become more interested in studying choices that are still open to them? If so, the centralization may in the long run shift more of the exploratory (and other) R&D from Service management to direct OSD management under, say, ARPA.[15] This might still further limit the roster of alternatives that receive serious attention, for it would probably increase the tendency to pick the approach judged to be "best" and to ignore other parallel approaches.

3. Force Structure

The fewer the alternatives that are explored in the R&D program, the more restricted will be the ultimate options with regard to the force structure. Whatever the range of options, however, centralization may, through inadequate airing of divergent views and inadequate consideration of alternatives, lead to a less than optimal choice.

In considering force structure decisions, it is important to emphasize that centralized control regarding the *overall* force structure is desirable. To some extent it has always existed. Interdependencies among the programs of the separate Services are enormous, and these decisions need better coordination than loose bargaining among the Services can provide. By necessity the responsibility devolves upon the OSD. Yet, at the same time, if control become too tight, with the Services having a negligible impact on the final decisions, the latter may be far less than optimal. To be sure, a choice must be made: there can be only one force structure. Despite the preponderant role of the OSD in this choice, however, there should be a dialogue in which the voice of the Services makes the OSD alert to spillovers and other considerations it might overlook, though at the same time the Services should themselves try to recognize the interdependencies involved in the decisions that are made.

The need for centralized control regarding the overall force structure is sometimes interpreted as justifying another type of centralized control for which the case is far weaker. Control over the overall force structure should be distinguished from control over the choice of closely competing weapon systems, in which case the spillovers are much smaller at the margin, and therefore on which the voice of the individual Service can be proportionately stronger. OSD has tended in recent years to assert its primacy in the latter class of decisions. The most dramatic case has been that of the TFX. The evidence in that case provides no clear-cut demonstration of the superior insight of the OSD regarding specific weapon system choices—and that in a period when OSD officials were recognized to be unusually intelligent, well-informed, and energetic.

Unfettered choice of weapon systems by the Services may have led in the past to a proliferation of such systems in the force structure. However, unified control may lead to too few, and this raises once again the vexing problem of uncertainties.

[15] (Defense) Advanced Research Projects Agency—*Ed.*

To depend heavily on fewer systems implies a risk. It is impossible to indicate precisely what the optimal number of systems is. One cannot prove that whatever decision has been made is demonstrably wrong, but one can point to the imponderables. Diversity in weapon systems provides a hedge against uncertainty. Lessened diversity implies certain costs that must be solemnly weighed against the gains. And, as has repeatedly been stressed, it is important to reckon with the full costs of our choices.

The inclination of the OSD has been to concentrate on a limited number of weapon systems, which are regarded as highly flexible in dealing with a range of foreseeable contingencies. Particular emphasis, for example, has been placed on Minuteman, in preference to heavier-payload missiles, and on the TFX, which has been viewed as the appropriate instrument for an impressively large number of missions. This trend is sometimes referred to as "uni-weaponism"—with the implication that it has gone too far. Is this a case of too many eggs getting placed in too few baskets? One cannot give a definitive answer, but it is a question which should continually be raised.

Thus, OSD control now encompasses (a) major force structure decisions where interdependencies exist in terms of effectiveness (where it is essential), (b) the decision to limit the number of weapon systems where interdependencies in terms of cost have been stressed in relation to hedging against uncertainty (where the exercise of control is open to debate), and (c) the choice between competing weapon systems (where the case is weakest). How will this power be exercised in the future? There are two points of concern of a general nature to which we will now draw attention, reserving to the next subsection some specific mechanics of control which demand scrutiny.

First, there is the ever-present danger that the views of one group will prevail without adequate checks and balances. It should go without saying that even in force-structure choices, heroic judgments are required. Quantitative analyses can be tremendously helpful—far more so than in R&D—but any choice involves weighing some fearfully important nonquantifiable considerations. In addition to major differences regarding strategy, which must be explored if not reconciled, there are beliefs and intuitions regarding system effectiveness in combat, the likelihood of major limited wars and of escalation, and so forth. What one may want therefore is a mixture of views—not merely heard but also represented by some real bargaining power.

One way of crystallizing the issue is to ask whether the dialogue between the OSD and the Services on these matters is in a healthy and flourishing state. It is a sobering question. There is evidence of a growing tendency to disregard Service viewpoints. For example, in preparing for the fiscal '66 budget, guidelines were sent to the Services with instructions to reclama[16] *only* if new information had become available to them different from that on which the guidelines were drawn up. In the limited time period, this implied that reclamas were virtually excluded. But, more importantly, it may imply that in the future little attempt will be made to draw the Services into the forming of judgments on force structure—rather it suggests that the Services will be limited to the function of providing information. If, as we have suggested, what we require is a rather delicate set of checks and balances—neither too weak or too strong—we may be drifting toward a set that is too weak.

Second, looking toward the future there is no guarantee that OSD personnel will avidly search for alternatives. So far the new OSD has been an enthusiastic vigorous group, and it may have done far more searching for alternatives than we can expect

[16] "An action in contest of a decision by a panel, committee, Bureau of the Budget, or the like, to restore what has been taken away" —*United States Air Force Dictionary* (1956).

from OSD in the future. The pressures of routine work and a reduction in bargaining efforts by the Services may yield less emphasis on the examination of bothersome force-structure alternatives. Any agency is subject to developing stereotyped assumptions and models of the world which become unchallengeable.

If tacit "agreements" to avoid rocking the boat ever emerge, obsolete functions can last on and on. While inter-Service rivalry and criticism provide no guarantee for the prompt elimination of obsolete missions, it does provide one mechanism for bringing such obsolescence to light—as long as the Services are oriented toward the future and provided with an incentive to criticize. Bureaucratic inflexibilities have apparently produced some curious choices in the Soviet Union. Some major factors in producing these choices have been the privacy of communications and decision-making at the highest levels, the stilling of Service criticism and the encouragement of conformity, and a pattern of rewards and penalties which induced the Services to band together in covert resistance to pressures from above. A race with the Soviet Union in this respect is one we should seek to avoid.

4. Force Structure: The Blue Book and Reprogramming

Another source of concern regarding changing conditions and future receptivity to new ideas arises from an institutional device designed to bring order into defense planning, but which in itself will deserve careful scrutiny. Under the new system the approved financial plan for five years ahead and the approved force structure for eight years ahead are spelled out in the Blue Book. As a control device the latter may, however, discourage both flexibility and the search for new alternatives. If OSD is to exercise any kind of control, deviations from the program have to be difficult to effect and must be appraised by OSD. How does the system facilitate OSD appraisal and control? By reducing the number of alternatives that have to be considered. If each Service submitted an entirely new program and budget each year, it would be virtually impossible for a small staff to appraise and control it. OSD would then have to confine its attention to major decisions and to aggregative budgetary limitations. By confining the new proposals to formal PCPs—that is, by reducing the number and complexity of the alternatives—appraisal and control by top management becomes feasible.

This effect is so obvious that it has caused considerable concern. The OSD has recognized that dealing mainly with marginal changes under conditions of great uncertainty is a risky procedure, encouraging progressively less-than-optimal allocation. Therefore, despite its original inclinations, it is struggling against perhaps insuperable barriers to achieve program review rather than look only at the PCPs which the Services believe it is wise to submit or which OSD finds time to prepare. It may be, however, that to do this *effectively* requires, at least in part, program reviews by the Services, and that this requires nurturing Service incentives to criticize the full programs.

How difficult is it to effect changes in the Blue Book? To achieve changes in the current year or in the fiscal year shown in the published budget, one must go through the reprogramming procedures. Congress must grant permission for certain changes and at least be notified about many others. These rules laid down by Congress are shredded out into more detailed rules by OSD and various echelons within the Services. To achieve significant changes in the "program years," i.e., the five years beyond the published budget, one must go through the PCP procedure.

The effect, as far as examining alternatives is concerned, is to rule out many substitution possibilities. The budget is divided into pots of money, and future programs into categories; and shifts of resources among these pots and categories are

prohibited unless special permission is obtained. Sometimes (and this is nothing new) a laboratory has had plenty of funds to set up an experiment but has been unable to send anyone to observe it because the travel money had been exhausted. Sometimes there has been no telephone money even to inquire about the matter. Thus, to exercise central control, we preclude the consideration of numerous substitution possibilities—that is, of numerous alternatives.

Now some control of this sort must exist. The real question is: how much of this inflexibility yields more gain than cost? The new system adds a new echelon (OSD) checking on reprogramming and change proposals, induces more monitoring of changes within the Services, creates more rules and thresholds governing substitutions, adds new categories among which resources cannot be transferred. In the long run this may significantly increase the cost of exploring alternatives, and thereby reduce the number of alternatives explored.

Moreover, there is likely to be a trend toward proliferation of program elements —the compartments among which resource shifts are prohibited. When an OSD employee is asked to keep track of a set of program elements, he is not likely to feel an urge to provide flexibility to the Services. He will want more "visibility" for him at the expense of flexibility for the Services. Thus there is danger in the long run that pressures will come from OSD for shredding out more and more program elements.

Moreover, in the long run, the Services may become reprogramming-shy and especially PCP-shy. If their proposals do not seem to have much influence, efforts to urge changes will appear to be relatively unrewarding. Or at least the Services may find efforts devoted to distant changes less rewarding than efforts devoted to urgent reprogramming requests for the coming fiscal year. In other words, if the Services have too little influence, one original aim of program budgeting—getting the Services to look at the full costs of alternative choices—may be frustrated. There is another reason that the Services may become PCP-shy. Where future programs exhaust the funds that can be expected, a plus-PCP can be approved only if something else gives. With each Command's expectations built up according to the Blue Book, preparing a PCP may entail a great deal of conflict and trouble. Either the Service must find a minus to accompany the plus proposal, or it must be prepared for OSD to cut something. One of these days it may seem easier not to rock the boat. Now this sort of hard choice is caused by life, not by the Blue Book. But gearing expectations to an official program for several years ahead may actually make it harder to *face* these hard choices.

There is a more fundamental problem than the creation of procedural barriers to allocation. What is the effect of long-range planning on the attitude toward flexibility? Without a Blue Book there is presumption that the future is flexible except where the need for commitment has been demonstrated. This presumption that nonplanning means flexibility is somewhat deceptive in that standards may be virtually nonexistent for judging when a "need" for commitment has been demonstrated. Below the highest policy levels many commitments may be accepted tacitly, but nonetheless tenaciously. Thus the development of a plan, if it is merely indicative and its tentative nature is understood by all parties, may actually increase flexibility by making explicit (and vulnerable) commitments which otherwise would have remained hidden. On the other hand, a plan (especially if all the blanks are filled) can create the presumption that the future is fixed unless someone can demonstrate an urgent need for change. The burden of proof is placed on those recommending change. The costs of instituting change are greater than in the absence of a plan. But the real question is whether overall resistance to change is greater than when lower-level units are free to make tacit commitments—when framework planning does not exist.

Once again, this question is, in the main, one for the future. However, there are several factors in the environment surrounding decisionmaking that cause resistance to change. When anyone makes or even participates in a decision, he acquires an emotional commitment to it. It becomes much harder to change one's position once it is in print. If I am in charge, a change proposal that reverses my recorded decision has at least one if not two strikes against it. Apart from one's own emotional attachments, Congress and others look askance at vacillation and indecision. Frequent changes of mind make one look like either an oaf or a troublemaker. In Congressional hearings, legislators often say they are tired of all this reprogramming—a perfectly natural attitude that is no doubt shared by various high-level officials.

No one can fail to admire Mr. McNamara's determination to bring about change when he felt it to be necessary; however, there is a question whether the costs paid to overcome resistance to change had to be paid in several instances (or whether Mr. McNamara's successors as Secretary of Defense would be willing to incur such costs). This reflects an institutional question whether the costs incurred in reversing semi-commitments embodied in the Blue Book could not have been avoided. There is evidence that expectations are formed on the basis of the Blue Book—expectations that need never have been formalized. In 1963 the OSD decided to reduce the 1969 programmed level for Minuteman below the level previously approved. Undoubtedly major issues of this kind will arise again. The 1963 action touched off bitter (and time-consuming) controversy in the Pentagon, partly because the OSD reduction seemed to be taking away something that had been granted earlier. But why should such a controversy be permitted to develop in the first place? The 1969 force level particularly for a specific weapon system should remain flexible not only in 1963, but in 1964 as well.

Why should the DoD specify the force level for a particular strategic system so far in advance, when it may be altered not only by a major change in technology, but, more importantly, *by unanticipated changes in the assessment of the Soviet posture?* As subsequent events have amply demonstrated, the future posture of an opponent cannot be anticipated with any high level of confidence. A premature commitment provides little more than a source of nonproductive conflict and a decision which may have to be reversed but which may or may not be reversed. The purpose of such a commitment may be to provide a guideline, but the effect is to reduce flexibility. From their actions one may infer that both the OSD and the Services recognize that an entry in the Blue Book is indeed likely to reduce flexibility. For this reason we would like to stress our conviction that the main function of planning for the distant future should be to spell out the relevant information regarding future alternatives and that Blue Book commitments should be avoided until the appropriate decision point is reached.

A related mechanism for narrowing alternatives follows from the way decisions are reached, and to which the Blue Book lends some support. For the reasons described above, the Blue Book and centralization may convert what should be sequences of decisions into one-shot decisions. One reason is that a one-shot decision means that *you* make it, while a sequence of decisions means that someone else may make some of the choices. This makes it more vital than ever that the one-shot decision be right. As a consequence, one should feel the heightened urgency of taking interdependencies—the relation between other choices and this one, the significance of tomorrow's events and budgets and decisions for today's choice—into account.

Thus, alongside the tendency toward premature commitment regarding the long run, there are short-run pressures to delay a decision until there is high confidence it is the right one. The appropriate point for going ahead with some components in

the decision complex may have come earlier, and if decisionmaking were done sequentially, those components of the total decision might have been decided on in a more timely manner and have produced valuable information. Thus the tendency to delay for the "big" decision reduces flexibility by eliminating the options that might have been produced by earlier decisions on sections of the major problem.[17] Pressures to delay always exist, both to take more interdependencies into account and for other, less fruitful reasons, but those pressures may be greater if the procedures adopted push us toward one-shot decisions.

5. Force Structure: The New 5th and 8th Program Years

Each year, as an old year expires, a new year is added at the end of the five-year financial plan and the eight-year force structure. How are these additional years planned, and in the process are numerous alternatives seriously considered? It seems doubtful that anyone ponders alternative programs very arduously. The Services produce objective-force documents, the Joint Chiefs of Staff offer their suggestions, and the OSD makes the final decisions. The eighth year is likely to be an extension of previously programmed trends, the fifth year a slightly modified version of the old sixth year with detailed cost estimates now added.

In this case the thing to be concerned about is not the relative lack of attention given to the new fifth and eighth years. It would probably be foolish to fret much about or make detailed comparisons of alternatives for those distant time periods. The thing to be concerned about is the advisibility of committing ourselves to *any* program that far away. For entering these years in the Blue Book has more than token significance. From that time on, above-threshold changes must be made via PCPs, and, as noted above, some degree of inflexibility is generated. That is, entry in the Blue Book may make it more costly to examine alternatives.

Perhaps it should be reemphasized at this time that we are pointing out only the possible costs of the OSD system, in this section the possible neglect of alternatives that may one day result. We are not calling attention to the gains provided by the OSD arrangements or attempting to weigh the costs against the gains. We do this because we think the costs may not have received enough attention and because we hope steps can be taken to reduce the costs without greatly reducing the gains that the OSD system aims to provide.

V. THE SERVICES' RESPONSE IN THE NEW ENVIRONMENT

In the preceding sections attention has been concentrated on those long-run costs that centralization under the new system may entail. Against these costs must be set those benefits, both short-run and long-run, arising from better coordination of decisions and the introduction of techniques which may lead to better decisions in general. Such benefits, of course, reflect the objectives for which the system was established, briefly treated earlier. One may ask whether there are ways to avoid,

[17] Another way of putting this is to suggest that pressing for the "big" decision reintroduces some of the problems associated with concurrent planning. Both the Project Definition directive, and to a lesser extent the Blue Book, should force some care in using concurrent planning, by encouraging the resolution of major uncertainties prior to tooling up for production. But centralization may introduce some of the difficulties by causing dissenting judgments and therefore uncertainties to be overlooked. If you are led to believe there are no serious uncertainties, naturally you have a greater tendency to pick the best before you test or buy before you try.

No one can say how much concurrency is appropriate. It is clear, however, that either underestimating uncertainties, or failing to investigate multiple and relatively cheap possibilities to resolve those uncertainties, represents an aspect of the concurrency approach that is counterproductive.

reduce, or hedge against some of the costs of the system without seriously reducing the benefits obtained.

This section discusses some adjustments that the Services might make and others that they might urge upon OSD. Of the Service adjustments some are directed toward making for a more comfortable adaptation to an environment which has proved somewhat abrasive. Others are directed toward inducing OSD actions which will diminish centralization and its costs. This raises a fundamental point: the OSD attitude toward modification of the system will in large measure be shaped by the way the Services respond to it. The OSD (and the OSD alone) has the power to decentralize, just as it has the power to centralize. The Service response is all-important, for the OSD will surely not countenance decentralization until it is persuaded that the Services can be relied on not to utilize procedures which it regards as cumbersome, costly, option-suppressing, and leading to less-than-optimal decisions.

The adjustments suggested here would represent improvement, in the judgment of a number of Rand staff members. We cannot, however, estimate the costs and gains of each change. Whether or not any such step would truly be an improvement has to be a matter of judgment. Choices among alternative organizational structures or budgetary procedures are examples of decisions that are wallowing in uncertainty —and therefore decisions for which quantitative analysis can give pathetically little help.

SERVICE ADJUSTMENTS TO THE SYSTEM

1. The Attitude Toward Cost-Effectiveness Analysis

Previously the need for a continuing dialogue between the OSD and the Services was stressed—particularly in regard to force structure decisions where, in the nature of things, there can be little decentralization. To conduct a dialogue there must be a common language. Cost-effectiveness analysis provides such a language—one that is particularly illuminating in relation to output-oriented budgeting categories. For this reason we are disquieted by the lingering resentment in the Services directed toward both cost-effectiveness analyses and the new budgetary procedures. These institutional reforms accompanied a massive altering of power relationships in the DoD, but are not except in a fringe manner responsible for those alterations. The most controversial changes of recent years would have occurred in their absence. In the main both program budgeting and cost-effectiveness analysis should be viewed as technical and neutral instruments, and should not be blamed (or credited) either jointly or individually for other types of conflicts with which they happen to have been associated in time. These instruments should not be approached as the resented symbols of more fundamental conflicts, but rather as providing the appropriate means for communicating with OSD.

While we stress the appropriateness of cost-effectiveness analysis both as a tool in decisionmaking and as a vehicle for communication, there is need for a wider awareness in the Services of the ground rules of cost-effectiveness analysis. Such an awareness would forestall unnoticed manipulation of the ground rules by OSD and permit a better defense of Service interests. In particular, we have in mind three specific issues on which officers should be forewarned.

First, since the future is dominated by uncertainties and since both future strategic threats and opportunities can be discerned only in the grossest terms if at all, it is impossible to be even roughly accurate in indicating the nature of missions

assigned to new weapon systems or in measuring their effectiveness. The Services should resist attempts by OSD to obtain precise specification of the mission of systems right through the development cycle, pointing out that uncertainty regarding future strategic contexts precludes mission specification and that under the circumstances precise estimates of effectiveness are contrary to the ground rules of cost-effectiveness analysis. One may infer that the demand for precise mission specification at an early stage in the development cycle suggests an inclination to resist development under existing conditions.

Second, it should not be forgotten, particularly with respect to the pace of equipment replacement, that in strict logic cost-effectiveness analysis can never provide definitive answers *until the overall budget is determined*—even when cost and effectiveness parameters are known precisely. How much one wishes to invest in modest improvements in effectiveness depends upon the overall availability of resources. Just as a citizen with substantial resources may decide that a Jaguar or Cadillac provides enough additional benefits to justify its purchase despite its higher cost relative to a Chevrolet, so in the public realm the relative availability of resources determines the extent to which the Services are permitted to purchase "superior goods" or "inferior goods" [18] in order to achieve optimal allocation of funds. This is an elementary principle of economic analysis. To the extent that the OSD treats cost-effectiveness analysis in isolation from overall budgetary decisions, it is neglecting one of the ground rules of cost-effectiveness analysis.

Third, accuracy in cost estimation, while desirable in itself, is not so influential in the actual decisionmaking process as the governing image of cost-effectiveness analysis would suggest. While good ball-park estimates are useful inputs, only on rare occasions would errors of as much as 50 percent affect the results regarding which systems to develop and procure. Consequently painstaking effort at refinement of cost data is likely to lead to diminishing returns. To be sure, either presumed errors in calculating costs or different estimates of the relationship of cost to benefit will normally be cited as justifying the rejection of a particular proposal. The OSD on occasion will use cost-effectiveness calculations more as an instrument of controversy than of analysis.[19] Decisions which have been made on other bases will be rationalized in terms of cost-effectiveness calculations. But this should not lead either participants or bystanders to exaggerate the role of precise cost calculations (or effectiveness calculations) in the process of reaching the decision.

2. Improved Cost-Effectiveness Analysis for Communicating with OSD

Despite the need for alertness toward possible departures from the ground rules of cost-effectiveness analysis, there is no question that, in principle, more extensive use of *sound* cost-gain analyses is all to the good. Systematically examining *all* the gains and *all* the costs of alternative actions is the right way to look at problems of choice. It is important for the Services as well as OSD, i.e., for groups with different viewpoints, to make such analyses. This does not mean mechanical use of quantitative exhibits, of course, as there are many important nonquantifiable considerations and many difficulties in interpreting individual analyses. Needless to say, the desirability of this adjustment was apparent to the Services long ago, and they have been

[18] These terms are employed in their technical economic sense.

[19] In view of its adversary status, a certain amount of abuse by OSD of the tools it itself introduced is perhaps inevitable.

moving in this direction. We are merely reemphasizing the advisability of employing *careful* cost-effectiveness studies in communicating with OSD.

Our suggestion is no more than exhortation, however, for we do not know how to insure that careful studies will be made, that they give due emphasis to incommensurables and uncertainties, or that they will influence OSD. As noted above, it is difficult to imagine a Service (or any agency with a position) producing and submitting objective comparisons of alternative systems. There is a very human tendency to turn them into design studies pertaining to one system unless the comparison favors the Service position. Nevertheless, even studies that put a Service's best foot forward can provide valuable information. But incentives to make even these comparisons carefully will diminish if the role of the Services is weakened too much. Hence, while we exhort, we are not optimistic about the long-run prospects, if Service bargaining power continues to decline.

Along this same line, it seems to us that studies are the mode of communication to be emphasized. There may be little use in trying to refine PCPs. On important decisions, they have to be preceded by studies and informal communications; they cannot present complete analyses or review sensitivity tests and the arguments in full; and they are likely to become increasingly a device for keeping track of decisions and their cost implications.

3. Symmetry in Demands on OSD and the Services

Many questions may legitimately be raised regarding the structure and functioning of the new system. But reform must start from where we are, not from where we once were. Criticism must be constructive and it must be associated with an organizational structure and attitude within the Services which holds out promise that change will bring improvement. In particular, there is no point in criticizing habits of mind or tendencies in the OSD when the Services themselves exhibit those same tendencies. Let us bring out a number of illustrations. First, while a Service may legitimately express apprehensions regarding the consequences of centralization by the OSD, its criticism acquires logical force only if that Service itself is striving to avoid the pitfalls of centralization. If the penalties of centralization are to be incurred, it is not clear why this should occur at the Service level rather than the OSD level. Yet, Service procedures down to the present provide little assurance that the knowledge and the diversity existing at lower levels will be fully exploited.

Second, if concern is expressed regarding an OSD tendency to ignore the domination of the future by uncertainties and too early to narrow the range of future capabilities and strategies, then the Services themselves must give due emphasis to uncertainties and not treat the future as preordained. For example, in light of the uncertainties the Services cannot insist that missions or effectiveness of new systems be forecast in advance and that they will not be altered by changing events, strategies, or technologies.

Third, the dominance of the future by uncertainties carries with it implications regarding the R&D program. What the Services should press for is a program designed to counter a variety of possible threats—few of which may actually materialize. Given resource limitations, the United States cannot afford to *deploy* capabilities to counter all possible enemy threats. However, there should be in development projects designed to counter as many as possible of the discernible threats. This implies, however, that many completely successful developments will never be introduced into the force structure, because the particular threat each was designed to counter did not, in fact, materialize. Moreover, many developments will appear to be either unsuccessful or not worth additional outlays, and need to be cut off.

This places a considerable burden on the Services in managing their programs. Unless the Services can demonstrate a willingness to choose among existing programs and remorselessly prune out the least promising ones, they will invite intervention from above. One way or another programs will be cut off. Moreover, the Services must accept the fact that successful development does not necessarily imply procurement. They may legitimately criticize the reluctance of the OSD to sanction the bending of metal in the development program, but if successful metal-bending is taken to imply procurement, then OSD's reluctance becomes understandable. Finally, the Services must strive to cut costs on individual development projects so that more projects can be carried on. Major benefits of decentralization can be extracted only if Service-designed projects are conducted as austerely as DDR&E-designed projects.

The process of adjustment for the Services will not be easy. Their attention has been oriented toward systems acquisition, and the pattern of R&D activities reflects that emphasis. Heavy outlays, that could be avoided if development was not assumed necessarily to lead to procurement, were automatically included in the program. A few costly projects exhausted a substantial portion of the available funds, thereby narrowing the potential range of the R&D program.

If the Services are to reacquire much freedom of action in managing the R&D program, they will have to alter their way of doing business. They will have to conduct developments austerely, cut unpromising developments off without prodding from above, and not press the OSD to procure simply because of successful development. But altering old ways of doing business is exceedingly hard. For instance, partly because of its obvious conflict with traditional ways of doing business, there has been much resistance to the Project Definition Phase. But Project Definition has a useful role to play in an R&D program which is primarily designed as hedging against a broad range of threats rather than as representing the first step in system acquisition. Project Definition should not be restricted simply because it cuts against traditional methods for acquiring systems.

4. Defending a Moderate Degree of Confusion in Pre-Systems R&D

Most of the adjustments required in R&D activities came out in dealing with symmetrical demands on OSD and the Services, and the need for alteration in the Services' way of doing business. One additional point needs to be made: when we talk of austere planning we do not mean to support the frequently heard demand for the elimination of "waste and duplication" in R&D. To the contrary, this notion is among the most powerful forces pushing us toward Cook's-Tour planning of R&D. Ultimately, to avoid duplication and nonessential research, a high-level committee may insist that studies fit into exploratory development projects, which fit into advanced development, which fit into plans for systems development, which fit into presently recognized military missions. But planning to explore unknown territory should not be that neat. Things should not fit together well, there should be projects without a clear-cut objective, there should often be parallel approaches (i.e., partial duplication, though two projects should never be literally 100 percent duplicates).

Suggesting that the Services defend a moderate degree of confusion in exploratory and advanced development may appear to be superfluous and empty advice. However, we have the feeling that there is shame and defensiveness regarding any confusion or duplication in R&D. In such circumstances, pleading for a moderate shift in attitude may not be completely empty. Such a shift in belief is prerequisite to other steps. Moreover, the attitude being urged is a somewhat unnatural one. It

goes against the grain to believe in the virtues of messiness, checks and balances, dissension, planning for work *not* to fit together, and so on. These things have little glamor—and yet, in a world of uncertainty, they *do* have virtue (up to a point).

We have a little more in mind, however, than an oral defense of partial duplication in R&D. Decentralization is the surest way to avoid too much "dovetailing" and too little dissent in planning R&D projects. In connection with any defense of partial duplication, it would be appropriate for the Services to decentralize their own planning of studies, exploratory R&D, and advanced developments. (One might call this a "25-year plan" instead of a 5-year plan, because it is planning for uncertainty and the long run.) One possible offer would be to give AFSC *and its divisions* more authority, eliminating at least some of the constraints and reviews that are now imposed. This would reduce the extent to which exploratory ideas are reviewed and reviewed until they fit an Air Force position. (As has been suggested, OSD might then be willing to diminish its efforts to review R&D programs until they fit an OSD position.)

Dispersing authority a bit could encourage a wide-ranging exploration of ideas, which currently may be rejected if they appear to impair the chances of getting a particular system approved. The value of wide-ranging explorations—not tied to any military requirement (unless it is the "requirement" for information and flexibility) —is likely to go up if (1) systems development becomes more expensive, (2) operational systems become more specialized (i.e., less flexible), and (3) weapons control agreements ban various other activities. Under such circumstances, the value to the nation of some decentralization of R&D activities is likely to increase.[20]

OSD CHANGES THAT THE SERVICES MIGHT URGE

We now turn to consideration of those changes that the Services might urge upon the OSD. However, it is appropriate to reiterate at this point that the power to decentralize authority rests solely with the OSD. Barring a reversal of attitude in OSD, a fundamental change on the issue of centralization vs. decentralization will come about only if the OSD is convinced that past deficiencies in the way the Services conducted business have been eliminated. The kinds of adjustments previously discussed are, we believe, a necessary though not a sufficient condition for decentralization. The Services will have to demonstrate the ability for conducting their affairs at least roughly in the manner that the OSD regards as desirable. Viewed from the standpoint of OSD, the Services must *earn* the right to increased responsibility.

This implies that pressure for quick change will be fruitless and that change will come only gradually as the Services demonstrate to the satisfaction of OSD their ability to make adjustments. Keeping in mind the mechanics of the new system, there are several important routes to change within the system that the Services should have in mind as they seek to lessen the costs of centralization and premature commitment.

1. Higher Thresholds for Changes Affecting the Program Years

One change that would give the Services greater authority without really im-

[20] Perhaps it should be reiterated that we are *not* advocating the elimination of planning or control, or that scientists and firms be financed to do whatever they wish, happily unaware of military problems. We are suggesting the planning of such programs by a diversity of groups so the work will not be tied to one view of the future.

pairing OSD's ability to coordinate *important* choices would be to raise the thresholds, say to $100,000,000 (impact on total costs). Higher thresholds have been urged by the Services, and some of the thresholds were raised slightly in 1964. In our view, they should be raised quite a bit more. The aim would be partly to reduce the number of relatively small decisions that at present burden OSD and the Secretary of Defense, partly to make possible quicker and easier shifts of resources. But the aim would be principally to provide the Services with greater responsibility and authority and thus to keep the Services conducting their affairs in an aggressive manner.

A threshold of $100,000,000 would not bestow much bargaining power on the Services. They could make substitutions from one program element to another, and in that way take the initiative occasionally, but they could not exceed the limit placed on total obligational authority. Actually it is doubtful, in present circumstances, that they would bother much with the program years other than the one immediately ahead—the year that was at that time being converted into the next published budget. Even this much authority, however, would maintain greater incentive to criticize, to examine alternatives actively instead of becoming too passive, to review programs as well as submit judicious PCPs, to be concerned about long-range planning rather than short-run housekeeping.

To be sure, it might be less clean and neat, for it would result in some abuse and some mistakes. If you give anyone additional initiative, there will be a few blunders, a few transgressions. OSD could have an after-the-fact review of below-threshold decisions, perhaps on a sample basis. *Flagrant* abuse could be punished, either by withdrawing the powers in question, or refusing to move further with decentralization. It is likely that no greater deterrent would be necessary. OSD would need no new powers; the unstated threats would deter most of the obvious abuses. Our belief is that the gain would outweigh the cost—this little increase in bargaining power would help harness the self-interest of the Services to achieve the broader interests of the nation.

2. Higher Thresholds for Reprogramming

If Congress as well as OSD could be persuaded, the "thresholds" for reprogramming (i.e., changing the programs already enacted or about to be considered by Congress) should be raised also. With inflation and the present size of the Defense Department, they are a little out of date, and they do keep the Services from considering substitution possibilities and "looking for business" as much as they otherwise would. These thresholds help keep a Service from getting a foot in a door, and often, to be sure, the door is one that it would be inefficient to enter. It is our contention, however, that we should encourage the Services to keep opening doors and peering through them. Doing this is worth a few wasted trips up the stoop.

As before, after-the-fact reviews would be possible, and Congress has ample punitive powers, which should deter flagrant abuse.

3. Keep Blue Book as Incomplete "as Possible"

Having a five-year plan yet deliberately keeping it incomplete may sound like an eccentric proposal, but in many instances the best plan is a blank plan. When there is great uncertainty about the program, the most accurate television guide does not say "Perry Mason," it says "To be scheduled." Or consider Lewis and Clark again. It would not be a good plan if they had said, "We don't know what town we're going to hit that day, but we've got to fill these blanks—let's put down Sheboygan."

But put frivolous illustrations aside. In the Blue Book, we should have five-year costs spelled out only for program elements where relatively firm decisions really need to be made. Why should other parts be filled in? To be sure, it is helpful for high-level officials and others to write down and consider highly tentative decisions or "guesses." But this should surely take the form of alternative programs "to think by"—not a single officially approved program.[21] Another reason for filling in the blanks might be for the sake of neatness, but we hope such considerations are too trivial to consider. Perhaps another reason is in order to dispose of squabbles and reduce the number of issues requiring settlement or to make commitments now lest the decisions be made differently by someone else later on. This boils down to foreclosing alternatives, however—something that usually should be avoided.

There is no inherent necessity of treating all program elements alike in this respect. Some program elements are partially unfunded now (we point this out as a virtue, not a defect), and this could occur more rather than less often. The five-year program could concentrate on those items with long lead times or with long-range costs disproportionate to present costs. It could avoid specific plans (i.e., commitments, even if tentative ones) for those items that can be handled more flexibly, either due to short lead times or different cost patterns. It is true that a one-year time horizon is inadequate for many decisions, but a five-year time horizon may—in view of the uncertainties—be excessive for other decisions. What we should strive for is less rigidity, and avoidance of premature commitments. To keep the Blue Book a little less complete might make it a more meaningful exhibit and a more useful base line—and prevent attention from being shifted more and more to the preparation of the President's Budget.

More radical measures might be worth considering, though we are not advocating them. There is nothing sacred, of course, about a five-year period. Maybe a four-year plan is all we should have *as an approved program*, with other tentative projections being merely "programs to think by." Or possibly that is what the whole Blue Book should be—merely an information system to assist OSD in reaching major force-structure choices. It seems likely, however, that the Blue Book (or its equivalent) has to be used *as a control device* to ensure coordination of certain decisions that do indeed need coordination.

Given the present degree of centralization, the Services themselves are likely to want the Blue Book filled up. That is, it may appear to be the best tactic to keep their budgets from being eroded. If they acquire additional bargaining power, however, they might be willing to have the Blue Book incomplete and to urge OSD to move in that direction.

A corollary of this suggestion is "keep the Blue Book as imprecise as good cost estimation requires." With regard to the distant years, good estimation leads to imprecise cost estimates, because good estimation seeks to measure what really exists, and uncertainty is what really exists. Highly sophisticated, time-consuming estimates of costs five years hence have a spurious precision, not a real precision. An installation planned for the end of the coming five-year period, for example, can be "costed" right down to the last nail, but if things are managed properly, many features of that construction proposal are going to change. Refining such entries in the Blue Book is not worth a great deal of effort.

[21] Several members of the Rand Cost Analysis Department have suggested that various colors be used to distinguish between firm commitments and those that are merely tentative.

4. Decentralizing Authority over Studies, Exploratory Development and Advanced Development

As noted before, we believe there should be some degree of decentralization in the approval of studies, exploratory development, and even advanced development. If there were fewer reviews within the Services so that the program would have less conformity to Service positions, the OSD itself might come to exercise less control over these choices. As emphasized earlier, it would be worth some "duplication" and inefficiency in the small to keep lower levels thinking about contingencies and probing for alternatives.

5. Screen Proposals for Engineering and Systems Development as Development Proposals, Not as Procurement Proposals

Although proposals for engineering and systems development should be screened carefully, we should not behave as though deciding to procure and operate when deciding that a system should go into Program Definition. If future OSD personnel, Congress, or the public begin to regard this decision as a procurement decision, there is danger that they will want the options screened too carefully and that they will give too much emphasis to quantitative analysis. Even systems development decisions involve great uncertainties, and with a high degree of centralization there will develop pressures to simplify decisionmaking, to apply the same procedures to large categories of decisions, and to neglect uncertainties.

In our view it would be healthy insurance against this to keep systems and operational developments in Program VI (R&D) and to confine the programmed costs to development costs. To estimate potential payoffs one has to look at future procurement and operating costs (and this should be done), but there is no need to put these costs prematurely in the Blue Book or to make premature commitments.

6. Tie Program Decisions More Closely to the Annual Cycle

To give better guidance to lower levels and harness their self-interest more effectively, OSD ought to link the new systems more closely with the annual budget cycle. This would call especially for reviewing programs and all proposed changes—in effect, considering alternative budgets—in time to pare them down to the ceilings desired by the Secretary of Defense. Although firm cutoff dates are not ideal, they seem to be needed. If batches of PCPs are reviewed intermittently, the full impact on the budget cannot be anticipated, and, toward the end of the cycle, *deep* subject-issue cuts have to be made, cutting across and disrupting many program elements (and sometimes conflicting with simultaneous PCP decisions). In the end this may work to confuse lower levels, impair incentives, and make more centralization appear to be necessary.

If PCP-review were more closely linked with the budget cycle, the Secretary of Defense could give ball-park ceilings to his program offices, these ceilings could then be taken into account more effectively in reaching program decisions, subject-issue cuts could be modest in size, and these cuts could come *after,* not along with, PCP decisions. Also, if some degree of decentralization is desired (and we have argued that it should be), this kind of procedure and guidance would facilitate giving some additional authority to lower levels.

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