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Organizational Structures and Planning

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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 a 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 co-exist 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

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large governmental organizations with massive requirements for trained personnel¹ 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 decision-making 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 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 in 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 dissensions 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 30's and 40's. 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 discus-

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.

sion 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 the 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. My 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 have been 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.

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. The Eisenhower administration was alert to the major issues in 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*, New York, 1963, p. 75).

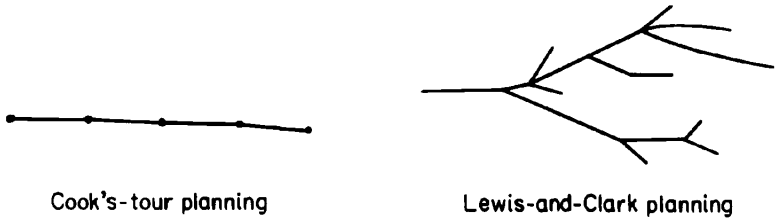
Nonetheless, defense programming continued to be handled in the traditional way with its rigid and unsatisfiable "requirements" based on simplistic quantitative impulses. Though deeply concerned, the administration was never able to exploit analytical tools in dealing with the problem. This was Eisenhower's 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 buildup of America's decisive military edge, should stand condemned at its close for "neglecting the national defenses."

I. Some Perspectives on Planning

Basically planning implies the programmed commitment of inputs over time to provide instrumentalities (in a sense, higher-level inputs) which can handle anticipable 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 advanced planning can 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 and 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 in reflection of internal alterations of preferences and partially of 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.

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. With this approach, 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 con-

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 showing that a decision point has been reached, and to respond in a timely fashion.

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 us to commit others to our view of the world, our objectives, and our strategies.³ In part, it is a form of laziness. Characteristically the tendency towards precise planning goes much too far, and appropriately these pressures will be resisted by insightful decision-makers. The cost of acquiescence is neglect of uncertainties, lost flexibility, neglected and suppressed options, and less-than-optimal adjustment to changing op-

³ At the highest level, leaders have a natural inclination to limit the ability of their successors to alter radically the policies they will inherit. However, at a point in time the strongest pressures for commitment normally come from below, particularly from 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 decision-makers 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.

portunities 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 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 realm of national security 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 to lend themselves better 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.

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 people, 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 measure 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 recruits 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*, New York, 1962.

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 in 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 shake-down 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 such a high 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 illusive. For relatively small groups the sharply reduced cost of communication permits a relatively full exchange of concepts without necessarily absorbing all energies in 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 for attempting 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 while preserving most energies for work on substantive problems. Substantive work outside of the pre-established 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 authority 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 and 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 would be a willingness to abandon detailed control in the cases in which it is costly and sift out its problems, delegating responsibility more freely in the many cases where 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. In order 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

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*, June 1958, pp. 35-38.

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 vehicle requiring roads. We are uncertain regarding the 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 restrict the flow of traffic. A simple, if underutilized, hedge against expanded traffic flow is to make the original underpass 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 it is 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, which destroy the problems' unidimensional quantitative nature, may be in the offing, 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.

In order 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 trade-offs, as they will be seen in the future, among these various categories of activities? (The trade-off 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-and-D program, preferably one of wide-ranging character. The purpose of R-and-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-and-D activities cannot be made fully compatible with the definition of a program of future activities. Correctly viewed, a phenomenal R-and-D success does not necessarily imply acquisition and deployment, whereas, depending on the strategic situation, a partial R-and-D failure may be followed by acquisition and deployment. The purpose of R and D is to buy options. It should be recognized as the first phase of a sequential decision-making 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 and D are accepted as insurance against a future military demand—without any commitment to the force and structure. Through an austere 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

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 semisuccessful 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 and D, source selection, acquisition, introduc-

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 postdecision hedging is so cumbersome, it is important to avoid forfeiting flexibility by being too quick to anticipate a decision point.

THE RESPONSE OF RIVALS: PERCEPTION AND COUNTERRESPONSE

The existence of major rivals increases planning difficulties—particularly 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 this conduct 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 crosscurrents 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, this image 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

tion 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.

unanticipated actions.⁷ Off with the old, on with the new, and brush aside any lingering doubts.

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 look like a game-theory 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 his 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

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 the policies they are presumed to represent and on public statements and their exegesis. Too little attention is paid to the broader picture of the decision-making 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.

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. Influencing 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 the prevailing doctrine may collapse with surprising suddenness, normally it has already 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.

II. 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 nondefense areas. Yet, concomitantly, there is a pressing need for such planning, especially in light of the long lead-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.

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.

One major implication is that changeableness in objectives, in strategic views, and in the utility of instrumentalities, by underscoring Knight's 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 microdetail 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 fighting wars 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 their 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-analysis 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 trade-off 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 trade-off 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 that forces designed for a major struggle

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

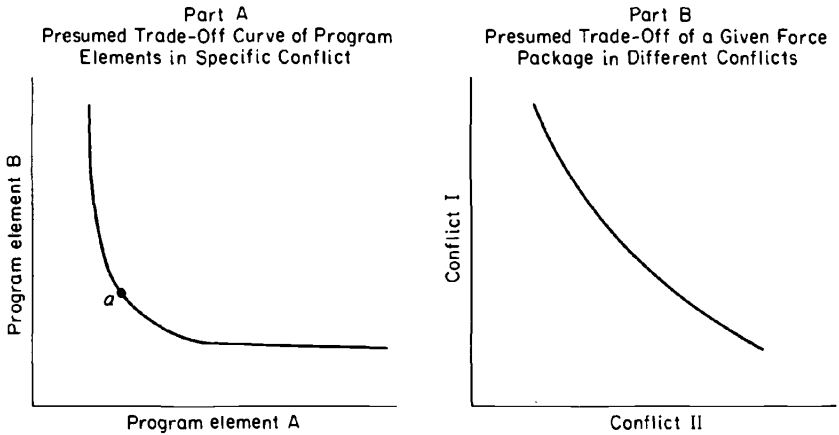


Figure 1

(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 Figure I, Part A pertains to force planning for a specific major conflict. Two program elements are indicated on the axes—say, tactical air and infantry divisions. It is presumed that there is sharp curvature of the trade-off 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 trade-off 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 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.

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 were 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 of our inability to plan the future force structure for contingencies that actually materialized literally abounds. 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 buildup 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 conflict in Western Europe. This goal has more recently been de-emphasized 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 (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 forces, it is notable that major effort was devoted

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.

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

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 to increasingly 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 dramatically. 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 forces 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 fire power 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 inten-

¹² The unveiling of the B-52 bomber in its tactical role may represent the kind of serendipity on which policy-makers 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 of 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.

tion 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

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 forces. 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 the fact 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 pro-

¹⁵ The bureaucratic aspects of this problem will be discussed in Section III.

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.

vided prove distinctly suboptimal is that the type of conflict for which they were originally intended has been successfully deterred. 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 with greater commitment, 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 by 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 of 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, an image which, as indicated above, tends to rigidity and to obsolescence. Sudden changes in this 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 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 to represent simply the replacement of one set of prejudices by another.

III. Bureaucratic Problems

The introduction of radically different methods of planning and management have, on balance, significantly improved the quality of defense decision-making. 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 the 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 without concern for 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 sunken costs. The human factor—how men are likely to perform—tends to be de-emphasized, since it is difficult to draw into the analytical structure. Stress is placed on such elements as weapon effectiveness, gross fire power, 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. 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 declining in importance. It is now more widely recognized that analysis cannot lead directly to decisions. Its purpose is

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.

to gather evidence, to improve the quality of discussion, and to sharpen the intuitions of the decision-maker. By themselves the analyses ignore complexities and intangibles, which the military rightly stress, and the decision-maker 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 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

²⁰ It could not be otherwise. Pressures of time preclude the chief decision-makers from examining and assessing the detail of the many analyses presented to them. The decision-makers 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 decision-maker’s intuition, though hopefully these intuitions will be consistent with careful analytical work done at lower levels, the spirit of which the decision-maker has absorbed.

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 to dismiss Service proposals scornfully 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 with establishing 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 circumstances 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 his energies will be dissipated. Effective organizations are not debating societies given over to dispute regarding nagging intellectual doubts or bureaucratic interests. Nevertheless, a

sufficient channel for dissent must be maintained, so that the views of the dissenters are not irretrievably lost.²¹

The normal difficulty is intensified by the tendency for organizations to become obsessed with their current problems and activities. Once again, this is natural, if counterproductive. At present there are indications that the Vietnamese war has led to a degree of brooding over China that is distorting not only our perceptions regarding China, but the rest of the world as well. When existing problems are frustrating and exhausting, little energy is left for detached appraisal. I suspect there is no wholly satisfactory solution, but it is important not to lose sight of the problem.

Perhaps some alleviation can be attained by the freeing of a number of high officials from day-to-day functional tasks. Walter Bagehot's concept of the cabinet minister as an outsider, detached from the routine processes of administration and free to ask probing questions, is probably inapplicable in the American context. The American system requires that secretaries both manage and represent their departments. Yet, there may be room for something like a Special Assistant for Devil's Advocacy charged with the responsibility to raise challenges to the prevailing concepts. Through a structurally supported position of partial independence, he may be able to save his leaders from longer-run slips arising from their preoccupation with current problems. The danger that doctrinaire or organizational blindness will be accepted as deep sophistication will be reduced. Useful dissent, which might otherwise be drowned in natural bureaucratic conflicts, would have greater chance to emerge.

The institutional problems which have been discussed—experience versus a hypothetical future as a guideline to conduct, authority harnessed to responsibilities, the wise creation and preservation of options, and the appreciation of intelligent dissent—have been couched in terms of defense management problems of the last five years. Though others may disagree, I would submit that these are the issues that have demanded attention. It should be apparent, however, that their applicability

²¹ Preserving a channel for intelligent dissent is an even greater problem within the military establishment. What I have said earlier regarding the necessary paramountcy of doctrine in operating commands bespeaks a certain sympathy for the military in coping with their problems. Nonetheless, it is plain that customs and habits of mind developed in operational contexts have spilled over into the contexts in which they are inappropriate.

is not restricted to the Department of Defense. More generally, they would seem relevant to all large organizations, when the attempt is made to bring them under unified management.

IV. Final Remarks

Centrally controlled planning in the Department of Defense since 1961 must be regarded as one of the major planning experiments of all time. It is a commonplace that expenditures by the DOD each year exceed the gross national products of all but six or seven nations. Of course, the range of activities is circumscribed, so that planning DOD's activities is far simpler than planning economic activities in a modern industrialized state like France or Japan. On the other hand, the complexity of its activities may be roughly on a par with total activities in, say, India. (Complexity here refers solely to activity range rather than to the availability of specialized managerial personnel and technology or to the responsiveness of the total system to control from the top, both of which are markedly lower in India.) The DOD operates with limited market information, and must therefore rely on other techniques to provide the information for its basic allocation decisions. When the size and complexity of the effort is taken into account, the DOD does remarkably well. Given an equivalent degree of talent and sensitivity to central control, one *could* run an economy with the same management procedures.²² There is no reason why one should want to do so: the economy would function clumsily and relatively unprogressively. But it can be done. For this reason DOD experience does provide some insight into the problems of nations attempting major economic planning. In particular, national economic planning involves a multidimensional set of problems, which makes it unwise to attempt precise advance planning. With changing technology, sources of supply, tastes, and objectives, the proper inference would be to stress *rough, indicative planning* of the Lewis-and-Clark type. Attention should be given to the flagging of

²² "Running an economy" here refers to the technical instruments of control. The analogy to a planned economy is not intended to be a complete one—for in a planned economy the problem of choice is far more intractable than in the DOD. The DOD operates within a larger organizational framework; it receives guidelines from above; and it can accept the primacy of organizational goals. DOD therefore provides relatively little guidance regarding how a planned economy should choose, but, assuming that the choices have been made, it provides meaningful guidance regarding technical methods of control.

decision points and the building in of options. If planning is in the nature of prescription, it is bound to be costly—and will probably be inaccurate as well. For planning variegated activities under conditions of uncertainty, indicative planning—because it lacks precision and rigidity—is the appropriate means for attaining the best result possible, though not the best possible result. In this case, as in others, the hypothetical best can be an enemy of the attainable good.

In a sense what the DOD has been attempting is to develop a very broad quality control system—one that will statistically improve the quality of *decisions* in an organization with an enormous range of activities. Part of the intention is to hold down the goof rate. Far more important, however, is holding down the aggregate costs of goofs. There is no doubt that the McNamara regime has made at least as many errors, particularly errors of commission, as its predecessors, but that is no basis for criticism. In fact, it may be the reverse, for under conditions of uncertainty making numerous, small “errors” is the appropriate way that one gropes toward a set of decisions which most efficiently satisfy one’s aspirations. Even with a standardized product it is recognized that a quality-control system, which seeks zero defects, is reaching for a costly objective of questionable meaningfulness. For the DOD, holding down the average costliness of errors and aggregate error cost is the appropriate objective. And, it is in this respect that the McNamara regime represents so great an improvement over its predecessors, for enormous and wasteful activities of dubious effectiveness have been ruthlessly pruned. The *number* of “errors” may be permitted to rise—as long as stringent limits are placed on resources devoted to the care and feeding of white elephants.

The simple cost savings are perhaps the most obvious benefit stemming from the OSD-led managerial revolution. It reflects the more lucid articulation of objectives associated with the introduction of cost-effectiveness analysis and program budgeting. (The more lucid articulation of objectives is a gain in itself, even though the question whether the objectives ultimately chosen are the right ones must remain a matter of opinion.) There is no question that most of the programming-budgeting structure will survive, as it should. It is not only another way of structuring budgets, it is an improved way. I myself doubt whether the same attention will continue to be paid to the long-range program and the accompanying documentation requirements, as is at present. Nonethe-

less, the output-oriented ordering of expenditures represents an enormous structural improvement. In addition, the general principles of cost-effectiveness analysis are unlikely to be swept aside. Even if we recognize that "analysis" as a doctrine or battle cry has 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 decision-making. 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 taken 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 mill pond 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 mill pond 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. They have 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 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 atti-

tude 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.