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Political Conjecture in Military Planning

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Prepared for

Office of Naval Research
Washington, D. C.

March 1968

Contract No. N00014-67-C-0516
NR 047-067/1-18-67
NR 274-086X/1-18-76
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I. The Task

We have been asked to extend and deepen the analysis and prescriptions presented in Science and Defense.[1] More particularly, we are expected to explore methods and administrative concepts for determining military requirements to meet future contingencies, five, ten, fifteen years hence, that take into account expected or possible changes in the relevant world environment. Though this evironment has several aspects (e.g. technological, economic, geographic), politico-military factors are the component with which this paper is chiefly concerned.

The importance of these factors follows from the fact that the use of military power is essentially a political process - unlike the use of military forces which is a military process, although one which has usually political purposes and is usually subject to political conditions. As Clausewitz put it, war is the continuation of politics by other means. Strictly speaking, military power is present when the use of military forces influences the behavior of one's opponent.

While this has always been so throughout the history of organized warfare, it seems that, in recent decades, military power has

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[1] Klaus Knorr and Oskar Morgenstern, Science and Defense: Some Thoughts on Military Research and Development, Center of International Studies, Princeton University, Policy Memorandum No. 32, February 18, 1965.

become more "politicized"[2] than before. In relations between nuclear powers, this change is attributable to first, the new emphasis on strategic deterrence, a psycho-political process, itself the consequence of revolutionary advances in arms technology, which aims at the avoidance of a strategic military clash; and second, the use, in crisis situations, of military threats and other actions under the restraint of avoiding uncontrolled escalation to strategic war.[3] In relations between nuclear and nonnuclear powers, the change results from, first, the strong stigma that has attached itself to the use of nuclear weapons and, second, the diminished international legitimacy of the use of military force, especially in relations between larger and smaller powers.[4]

This increased politization of military power is easily illustrated with reference to the Cuban missile crisis in 1962, in the course of which the United States attempted carefully, and successfully, to achieve the removal of Soviet strategic missiles by the minimum use of military moves and threats. It is also illustrated by various restraints on American military action against North Vietnam

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[2]Cf. Robert E. Osgood and Robert W. Tucker, Force, Order and Instice, Baltimore, Johns Hopkins Press, 1967, p. 28.

[3]Cf. Thomas C. Schelling, Arms and Influence, New Haven, Yale University Press, 1966, esp. Chapters I and V.

[4]Cf. Klaus Knorr, On the Uses of Military Power in the Nuclear Age, Princeton, Princeton University Press, 1966, Chapter III.

in 1966-67 as, for instance, in the limitations placed on target lists, or in the fact that pilots trained in, and aircraft designed for, nuclear missions were employed extensively, and in the face of a mounting casualty rate, to destroy truck convoys and bridges by means of conventional explosives, that is to say, by technically very inefficient means. The much more efficient nuclear arms were not considered usable.

Clearly, if one is interested in the design of weapons, forces, doctrines and strategies for future use, it is crucial to visualize, as much as possible, the politico-military environment in which these capabilities may find employment. Therefore, our key questions are: What is the nature of the politico-military inputs required? How can these inputs be produced? How must the people occupying roles in producing and employing these inputs be organized in order to achieve satisfactory results?

To inquire into these questions is directly in line with the strong administrative trend of the past six years toward applying more analytical rigor in military planning. Much of this trend is expressed in the emphasis on cost-effectiveness studies for elucidating choices. This technique is very valuable when applied with proper restraint - a restraint induced by an awareness of its shortcomings. The main trouble with its application arises when more or less different outputs involved in choices must be evaluated

comparatively not only in strict terms of technical military performance but also with reference to the future military-political environment of the United States. The political context will in part, and not rarely in large part, determine the very relevance, and if relevant, the usefulness of different kinds of military forces.

II. Plan of Study

In order to perform our task, we will, first, briefly restate some pertinent observations made in Science and Defense on the scope of R &D decisions. Second, we will, also briefly, note the relationship between factors involved in determining the military requirements of the future. Third, we will analyze at length the problem of politico-military inputs, that is, the nature of the business of conjecturing about the future conditions of the military function, and of anticipating and deciding on requirements on the basis of the conjectures. Fourth, we will identify the personnel roles involved in these processes of conjecture, anticipation, and decision, the personnel capabilities to match these roles, and the problem of recruiting such personnel. Fifth, we will speculate on administrative concepts that should help to structure the relationships between the role players, that is to say, indicate the manner by which their concerted effort is likely to yield satisfactory results. Finally, we will raise the questions concerning criteria for evaluating results.

III. The Scope of Military Research and Development

In Science and Defense (pp. 1, 38), we proposed that the scope of R & D choices and decisions include military doctrine and military strategy as much as military hardware. Doctrine specifies the manner of deploying and using weapon systems; strategy concerns the choice of military means for influencing an opponent in time of formal peace or war, or for managing a particular military confrontation.

This deliberate and systematic extension of the concept of R & D seems to us very important. The utility of weapons depends obviously on appropriate doctrine and the exercise of strategic choice. New weapons often require adjustments or radical innovations in doctrine and strategy. Thus, the development of tactical nuclear weapons called for doctrinal innovation and offered new strategic options. Contrariwise, changes in military strategy, and especially the development of new strategies point to the need for new weapons and associated doctrines or for new uses of existing arms. For instance, as the emphasis on strategic deterrence increased in the 1950's, or on a counter-insurgency posture in the 1960's, new weapons and doctrines were urgently required. Thus,

if the United States government decided in the future to make substantial use of nonlethal, temporarily incapacitating weapons in counter-insurgency situations, the decision would require not only further R & D on suitable weapons, but also the development of an appropriate doctrine, and the consideration of their strategic fit in particular situations. Moreover, history is full of examples of weapons designed and developed for one specific purpose, finding new and unexpected uses. Such re-assignments call usually for doctrinal adaptation or suggest a new strategic fit. We are not suggesting that--viewed temporally--innovation starts clearly in one area--whether it is weapons and doctrine, or strategy--and then induces demands for changes in another. This happens occasionally, especially in a crisis when the need for innovation is discovered suddenly. More frequently, new developments in the several areas of military effort go hand in hand, involving reciprocal stimulation, although one hand may be leading the other at any one time.

Despite these facts, there is a strong inclination in the Defense Department and the military services to regard R & D as something overwhelmingly concerned with hardware. This is not to say that the business of adapting and developing doctrine

and strategy fails to be taken seriously, and that choices in these areas are not considered and exercised with care. But there is a lesser disposition to regard appropriate R & D as a proper preparation for these decisions. In practice, a considerable amount of research on matters of strategy and doctrine does in fact take place. Research contracts given to private defense industry, universities and such research organizations as RAND and IDA indicate the range and volume of this sort of work.

Nevertheless, these research endeavors are generally not regarded as co-equal with weaponry in military R & D. This attitude may be understandable in view of the necessarily much larger expenditure of funds on hardware-type R & D compared with doctrinal and strategic studies. Yet this disparity in financial outlay is not an index of the relative importance of the various activities in fashioning satisfactory military postures. Existing practice is in line with a strong American penchant to seek technological solutions or "fixes" for essentially non-technological problems. It may also be felt that hardware problems lend themselves more to R & D than doctrinal and strategic problems, or that the nation's resources for weapon development are far richer than those on hand for development of doctrine and strategy.

But it is doubtful that the research resources available for doctrinal and strategic studies are being fully exploited. And if they should be in insufficient supply for certain purposes, little is done to expand the supply. Finally, the overemphasis on hardware research and development may be attributed to the fact that it is, or is thought to be, much easier to specify the properties of the desired hardware than it is to define the desired product of non-hardware research and development. Yet it is not clear that, if this difference exists, it needs to exist, or that it could not be greatly decreased. More important, sheer administrative inconvenience in specifying the desirable properties of a new strategic option, compared with the convenience of defining those of a new tank or rocket, hardly justify a corresponding neglect of the more difficult researches. In principle, it seems to us, R &D in all areas should be recognized to be of equal and inter-connected importance. In the following two sections, we hope to demonstrate the special value of non-hardware research and development.

IV. The Overall Framework of R & D Choices

In order to approach our task within the proper framework, we must begin by noting the overall complex of reality within which the problem of developing military capabilities and postures must be appreciated. Speaking abstractly, it is the purpose of national military power to produce intended effects in the behavior of other nations, usually antagonistic. To put it differently, military systems and their employment are meant to contribute, sometimes decisively, to the achievement of national policy objectives in the international arena. Now, the three main referents of this statement--military systems, policy objectives, and international arena--concern realities that are, first, interdependent, second, highly complex, and third, subject to change, often rapid change, over time. And change in any one reality is apt to impinge in a complex manner on the others.

While the following sections focus on the relationship between changes in military capabilities and changes in the international environment, we briefly emphasize here that, in doing so, we must not lose sight of this larger framework. Whether national military forces and strategies contribute to the achievement of national goals, and the extent of the contribution, depend upon the nature of foreign-policy objectives as well as on the properties of the international arena and the suitability

of military systems and their use. In short, the utility of military postures is a function of their suitability to both policy and international environment.

Thus, policy objectives may be unrealistic and hence court failure. They will be unrealistic if the military and nonmilitary means of international influence available to the government are insufficient to produce desired policy results. Or the use of military means fails because employing military force in certain ways is inappropriate to a particular policy goal, e.g. achieving a reputation as a power which acts with justice and restraint. For various reasons, it is far from easy to foresee the relevance and adequacy of military means to achieving certain policy objectives. One reason is that the effectiveness of these means is always conditional on particular circumstances. A nation may have plenty of military means for use in certain situations (e.g. strategic deterrence) but not for use in others (e.g. counter-insurgency in a distant theater of operations). In any case, successful military performance demands that capabilities, objectives, and international environment are properly reconciled.

As military systems and the international environment are subject to change, so is national policy; and this fact has an obvious bearing on the choice of weapons, doctrines, and strategy. To give a recent example: in 1964, the United States was clearly unprepared,

and doing little to prepare itself deliberately, for the massive military intervention in the Vietnam conflict that took place in 1966 and 1967. United States policy changed suddenly. Similarly, the experience of massive intervention in Vietnam might now suggest the military requirement of further developing and maintaining adequate capabilities for future contingencies of this kind. Yet we cannot take for granted that the United States will want to practice such large-scale intervention in local conflict far from its shores in the future. If the eventual outcome of the Vietnamese War proves very unsatisfactory, the United States might modify the foreign-policy stance that led to it, or it might do so for other reasons.

But--relevant as they are to military R &D--such policy changes and their probability are hard to foresee, even a few years ahead. This difficulty does not mean that this part of the total problem can be ignored. But looking at the overall framework for analyzing R & D choices, the total job of analysis is formidable indeed. It is so formidable precisely because the relevant realities, and their interaction, are complex; because these realities undergo change that is hard if not impossible, to foresee; because the total problem must, for purposes of analysis, be decomposed into manageable parts; and because the results of these analyses must be brought together and sensibly integrated for proper decision-making.

Since the overall framework is large, and the total analytical task intimidating, the traditional tendency to escape this daunting complexity by concentrating on the immediate properties of military technology and systems and their monetary costs is perhaps hardly surprising. This tendency can and will be favored more or less on the explicit or, more often, tacit assumption that the other realities are not changing much and can therefore be neglected. In the past, this tendency caused the behavior characterized by a penchant of the military at any one time to prepare for the last war rather than for the next.

Today the military realize that this disposition will not do. The rapidity and consequences of rapid change in all the related components of reality are too impressive not to be recognized. Hence the urge to re-think the problem and search for improved practices.

V. Prediction versus Conjecture

Prognosis is indispensable to good planning of any kind. In order to make proper military plans for the future, governments and the military must take into account future technology, future economic costs, and the future politico-military environment. Predictions or conjectures about future technology are inherent in R & D projects at various stages. The inexorable pressure of resource restraints makes cost estimates an indispensable element of planning. In fact, as presently practiced in the United States, estimates of technology and costs are intimately related in systems analysis applied to major military projects.

It is the thesis of this paper that politico-military forecasting should be regarded as equally important to military plans that have a major bearing on the military capabilities and posture of the United States. It is of particular importance in R & D planning which will affect these capabilities only with a considerable time lag.

According to public discussion, the United States faced in 1967, when this essay was written, several important choices regarding the future of its military capabilities. Should the United States proceed to the small, or large, scale deployment of BMD (Ballistic Missile Defenses) on its territory? Should it develop BMD systems deployable at sea? Should it develop a follow-on

supersonic bomber? Should it develop a new generation of land-based ICBM's to succeed the Minuteman system? Should it plan a substantial and highly mobile tactical force for quick development far from its shores? No doubt there were other such choices not recorded in the public domain.

All these devices were technologically feasible; provided the public was persuaded of their merits, they were economically feasible, though perhaps not while the war in the Vietnams had pushed defense expenditures to an unusually high level. Yet how these choices should be exercised, obviously depended also on the kind of military posture which this country decided it needed in the future. This desired posture, in turn, depended upon two interacting considerations. One concerns the purposes to which the United States expected to put its military power in the future. For deterring attack on itself and on its closest allies? For containing Communist aggression at many places or everywhere? The other consideration concerns the expected politico-military environment five, ten or fifteen years hence within which the resort to military power might take place. It is the estimate of this environment which is the subject of this paper.

This environment can be divided into three parts: probable opponents, probable allies, and the rest of the outside world.

Regarding the first category, it makes a difference whether or not the Soviet Union and China will be very aggressive, in terms of indirect as well as direct aggression, and whether the relationship between the two is one of antagonism or cooperation. Regarding the second category, it makes a difference whether the United States will have many allies or few, whether NATO will crumble or remain cohesive, or whether the western European nations will develop a considerable indigenous center of power, or whether Japan will once again become a substantial military power. Regarding the third category, it makes a difference whether or not many of the less developed countries will be enfeebled by poverty, rent by civil strife, attracted by Communist ideology, and susceptible to indirect aggression. And regarding each category, the implications of possible configurations depend on the configuration arising in the others. For example, the significance of a crumbling NATO would depend upon the posture and policy of the USSR.

Surely, if it were possible to predict the properties of this environment ten or fifteen years hence, those engaged in military planning would like to know:

(1) The military capabilities and intentions, if not decisions, of all states powerful enough singly or in alliance, to present a direct threat to the security of the United States, or to present a substantial military threat to the stability and security of any area in which the United States has a major interest.

(2) Since local international conflicts can engender great-power involvement, they would be also interested in predicting the military capabilities and intentions, if not decisions, of lesser nations (for example, the U. A. R., Israel, or Indonesia), and the kinds of intervention from outside the region to be expected in the event of local wars.

(3) Since local internal conflict can lead to foreign military or para-military intervention, we are also interested in predicting the revolutionary potential, and the political direction and politico-military strength of the forces arrayed against one another in many unstable states, great or small, and the kinds of intervention from outside which may be launched against countries involved in civil strife.

The planners would want to foresee above all future events which will drastically change the external politico-military environment.

In the past, the military planning of governments was, even for great powers, much simpler than it is now. As long as weapons technology developed only very slowly, and defensive weapons were a good match for offensive ones, governments could decide to maintain relatively small but expandable forces while peace prevailed, and expand these forces only after an adversary power had began to do so, or after war had broken out. This classical posture is, for well-known reasons, obsolete at the present time, and no great power is resorting to it. The need for prediction has become far greater.

(1) The Hopelessness of Prediction

Unfortunately, reliable prediction of all such future events, behavior and even capabilities is impossible. Of course, someone may make a highly pin-pointed prediction and turn out to be right. But there is no capacity for such prediction which justifies enough confidence to be taken seriously. To realize this fact is so important that we will first demonstrate and then explain it.

We begin with some examples of major events that drastically changed or threatened to change the politico-military environment. During recent decades, the United States became involved in four important wars: World War I, World War II, the Korean War, and the war in Vietnam. Which United States involvement was predictable, or predicted with any degree of confidence, especially at the official level, even five years ahead, let alone ten or fifteen years? None of them was.

For another example, let us divide the past into fifteen-year intervals beginning with 1967. On the left side of the following table, we indicate the time periods; on the right side we refer to major politico-military events that occurred during each period but were not predicted, and not predictable, at its beginning.

Time Period	Unpredicted Events at Beginning of Period
1907 - 1922	World War I; kind and length of war; outcome; communist revolution and take-over in Russia.
1922 - 1937	Rise of Nazi Germany; German rearmament; rise of Japan as militarily aggressive power; the great economic depression.
1937 - 1952	World War II; configuration and outcome; nuclear weapons; decolonization under way; communist take-over of China
1952 - 1967	Sino-Soviet split; decline of NATO cohesion; Vietnamese conflict; China a nuclear power; Arab-Israeli War of 1967
1967 - 1982	What are the big, surprising events we do not, and cannot, predict in 1967?

A study of this list is hardly encouraging.

For another demonstration, we refer to two present policy questions important to policy officials in the United States and elsewhere. One question or series of related questions is this: will the present antagonism between Peking and Moscow continue indefinitely, deepen, or decline and perhaps even disappear for a time? Which conditions will determine these outcomes? To what extent can the United States affect these outcomes? The other series of policy questions are: is it true, as is widely

assumed, that the Soviet military threat to Europe has declined in recent years? If so, did it decline because Moscow is deterred by the fear of United States retaliation? Or is it because Soviet leaders have usually been disinclined to run dangerous military risks? Or is it because, as a result of internal changes in the Soviet Union, Soviet leaders are no longer interested, if they ever were, in forcibly capturing Western Europe for Communist rule? In any case, is the present military stability in Europe likely to last five, ten, or fifteen years? Answers to both sets of questions are obviously important when it comes to planning the military posture of the United States. But it is equally clear that different people have different answers to these questions, that it is hard to know which answer is likely to be right, and that some cautious and modest people will say that these questions cannot be answered with any degree of confidence.

The impossibility of high-confidence prediction about the external environment relevant to military planning cannot only be illustrated; it can also be explained. If we are interested in certain relevant conditions in that environment--e.g. the foreign and military policy of an important state--it is readily understood that these conditions are determined by a host of other factors, each of which is itself the result of still other factors. The possibility of predicting the foreign policy of State X would have to rest either on the fact that all conditions of interest and the underlying factors involved are constant, or can be

treated as constant because they change only very slowly over time, or on the fact that, though some conditions and factors are significantly variable, their identity is known, their change is predictable, and the relative weight factor is also known.

Regarding the essential constancy of conditions and underlying factors, it is a historical fact that certain conditions of interest---e.g. the foreign policy of Britain or France did not change appreciably over some past period of ten or fifteen years (e.g. during the nineteenth century) in relevant respects. But at other times this condition did change. Whether it did or not is, of course, known in retrospect; it was not known in advance. Moreover, if confidence in the constancy of such conditions was risky in the past, it is riskier now because, viewing the life of societies, change in many factors---technological, economic, social, etc.---has become more rapid and more pervasive than it was in the past.

But if one cannot rely on the constancy of conditions, of interest to military planners, and of the underlying factors, one cannot rely either on the ability to predict the course of change. It would be difficult enough to identify all the factors which have effected such changes in the past, and might conceivably do so in the future. It would be literally impossible to attach weight to these changing factors. Although we are often able to observe relevant trends, and

sometimes even measure them in a rough fashion, trends are affected by other trends in ways and with a strength which it is impossible to predict. These difficulties are inherent in the desirability to predict events which are essentially unique.* The fact that reasonably successful prediction is possible in certain aspects of economic, social and demographic file does not entitle us to optimism. Prediction in these matters is easier because they concern statistically large numbers of component events (in other words, classes of events), very few variable conditions, and little or steady change in the parameters. Thus, some basic characteristics of social life have exhibited considerable regularity over appreciable periods of time. But these kinds of conditions do not take us far in predicting the military capabilities of nations and are decidedly unimportant when it comes to predicting behavior which partakes of the nature of unique events. These kinds of relatively stable conditions contrast strongly with most factors of obvious and direct relevance, but of greater changeability, such as military capabilities and decisions in crisis situations, and still more sharply with matters of mood rather than attitude. Historical events have shown frequently that the moral-political moods in a nation can have a striking effect on crisis behavior and, more generally, on

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*Social scientists develop hypotheses which have considerable predictive power. These hypotheses employ the ceteris paribus clause ("all other things being equal...") and then proceed to predict: if A and B, then X. But other things are not, of course, equal in the real world. Hence, such hypotheses can help us to understand the genesis of events in the real world, but we cannot predict these events in particular instances.

the military power which a nation is able to exert. Yet those factors of mood and morale are patently among the most unpredictable phenomena.

It must be understood that, despite the great progress made by the physical sciences, they are also unable to predict single discrete events as, for instance, the spot where a particular leaf falling from a particular tree in Fall will come to rest on the ground. In this case too, the number of variables involved in bringing about the result is itself a variable. Thus, at the time the leaf is dropping, there may or may not be a breeze or storm; the leaf may or may not be wet from rain; there may or may not be other leaves or objects which the leaf may strike in the course of its descent. Nor is the relative weight of these variables known in advance. Thus, the wind may change velocity or direction while the leaf is tumbling.

The simple fact is that, certainly at the current state of knowledge, we are even unable to give definitive explanations even of past events or past change. To furnish a compelling explanation would require the historian to have a clear grasp of the relevant universe of causes. Since he can never attain this mastery, all explanations of past changes are essentially hypothetical. This explains why there has been continuous controversy about the origin of such events as World War I or Hitler's rise to power in Germany.

The fact is that though a historian may be able to explain how some sequence of events was possible, he is unable to demonstrate that it was necessary.* If we are incapable of explaining the past, about which much is known, we are hardly able to predict the future.

Historical, and presumably also future change, can also be distinguished in terms of rapidity. Sometimes change in conditions highly relevant to military power comes about very gradually and, after some considerable time, the cumulative results have brought about a drastic change. For example, an alliance may slowly wither and decay and, in the end, perhaps end in enmity. Such change may be dangerous just because it lacks clear signals and may be inperceptible for a while to anyone not specially sensitive to it.

However, there is also the further fact, demonstrated in the tabulation on p. 19, that change in conditions of interest to military planning, may not only be slow and steady, but at times also vast, sudden and thus abruptly upsetting the external environment of many states. That is to say, the future must not only be expected to be full of surprises, it must also be expected to feature sudden surprises of great consequence. In these respects, the universe of conditions of interest to the military planner is not essentially different from the universe of the stock market of interest to the investor. The

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*Cf. Robert Waelder, Progress and Revolution, New York, International Universities Press, 1967, p. 193.

investor has access to a stream of information, much of it of high quality and this information is studied by thousands of qualified people with a substantial stake in the stock market. Yet only by chance are a few of them able to predict the course of the market. On the basis of present knowledge, stock market prices are in principle unpredictable.

The nature of the difficulties encountered in prediction makes it clear that none of the new techniques, recently developed, can increase our confidence. Projection from recent trends is at best an indication of what is possible. Similarly, gaming and simulation may turn up possibilities, perhaps interesting ones, but the quality of the results is no better than the inputs, and for the reasons indicated, these are even at best of a low order. The crucial limitation on our ability to identify and attach weights to the variable conditions determining unique events, does not permit the programming of computers for the kinds of prediction we are interested in. The Delphi Technique* may produce possible futures on which the participant experts are agreed. But---aside from the fact that the results again depend on the competence of the inputs (i.e., the participants)--- the plausible future may turn out to be far off mark. In fact, according

*The Delphi Technique essentially involves ranking of alternatives by a group (or committee) of experts. Results may be "fed back" to the members, possibly repeatedly, in order to clarify agreements and disagreements.

to our tabulation on p. 19, the chances for this are very great indeed. This does not mean that these techniques are worthless. They may stimulate thinking, but they do not make the problem of prediction appreciably more tractable.

(2) The Planning Dilemma

Unless a particular war is in process or recognized to be imminent, military planners are compelled to make commitments that will shape future military capabilities long before the nature of future conflicts is clear. These commitments consist of choosing the development weapons, forces, bases and associated doctrines. Some equipment, forces and bases may be useful for a great variety of uses and conflicts; other equipment and forces are highly specialized and will be worthless for other conflicts than the type for which they were designed. It would be ideal to possess a military posture which is instantly adaptable to all possible kinds of conflict. But this is impossible under present conditions since modern technology enforces a high degree of specialization. This makes the military accomodation of some situations very difficult, for developing and maintaining a great variety of types of forces would be a strain impractical even for the richest country.

Military planners also face the choice of how to allocate available resources---men and treasure---between the production and maintenance of military forces on the basis of tested technology, on the one hand, and R & D designed to prepare the military for a variety of future contingencies, on the other hand. At any one time, they can do more of the one, or more of the other. If they choose more research and development, they may leave the country less prepared to cope with military challenges in the present and immediate future. If they over-prepare for the present and the immediate future, they run the risk of leaving the country ill-prepared over the longer run. To strike the right balance is obviously difficult. The decision must clearly be sensitive to the behavior, capabilities and plans of potential adversaries. The decision will also be influenced by new technological opportunities that open up; for instance, a weapon becomes available which is either fundamentally new (as was the case with the ballistic missile) or clearly superior in performance to existing ones. But the burden of this chapter is to insist that the decision should also be guided by expected changes in the politico-military environment. Without such guidance, many weapons, forces and military doctrines will prove unsuitable when future emergencies arise.

It stands to reason that if political and military leaders are responsive to this requirement, and provide themselves with the

administrative resources to meet it, their country may be able to neutralize the military power of another country disposing of appreciably larger resources, but insensitive to the requirement; and vis-a-vis an opponent of equal material wealth, they can achieve a decisive military superiority if this opponent is administratively unable to adjust itself to a world which is in the process of rapid and pervasive change. It is a world in which the danger of building Maginot lives has grown accordingly.

The impossibility of high-confidence prediction of the politico-military environment presents the planners, therefore, with a seemingly intractable dilemma. They have adapted to it chiefly in two ways. One is to deemphasize this kind of political input and concentrate on military choices in terms of their technological and economic implications. Cost-effectiveness analysis, and systems analysis in general, are difficult enough when confronting technological and economic uncertainties. It is far more difficult to cope with the politico-military implications of choices that, when exercised, become fully effective only with a delay of several years. Implicit in or equivalent to this reaction is the assumption that the future politico-military environment will be essentially as that environment is constituted at present.

The other response to the dilemma is to regard the future politico-military environment as unpredictable and, bowing only

to immovable economic restraints, base planning on the assumption of the worst imaginable future. In practice this means that the United States must be prepared against the worst which recognized or potential adversary powers may be able to do by military means. This posture does not entirely escape conjecture about the future, for even the shape of the worst imaginable contingency is not given as a datum, and is subject to doubt. Moreover, since it discards guesswork about the intentions of governments, and focuses on capabilities at their disposal, conjecture on future capabilities cannot be avoided.

Serious drawbacks are associated with both postures. The assumption that the politico-military environment will remain unchanged is, as we have shown, very likely to prove false, and could turn out to be extremely wrong, perhaps disastrously so. This posture certainly courts surprise. The risk is that the United States will find itself ill-prepared for unforeseen military contingencies and that it has wasted valuable resources. To be sure, the assessment of the environment will be revised as time goes on, and evidence of change is received and recognized as such. But there may be too little time to adapt military plans, force structures, weapons, etc., with sufficient dispatch to escape grave risks of unpreparedness.

In view of the fact that high-confidence predictions about the politico-military future are impossible, taking out insurance against the "worst possible future" is a rational method for dealing with uncertainty. The critical question concerns the amount of insurance, and the form it should take. Over-insurance against one risk is waste. Indeed, if military plans are based primarily on the worst possible future, waste and possibly inadequate preparation for other contingencies will occur if the worst possible future does not happen.* If preparing for the worst possible future requires simply more forces and weapons than required for lesser contigencies, only waste--but no inadequate preparations result in this case. Inadequate preparation as well as waste will result if concentration on the worst contingency leads to the accumulation of forces and weapons which are of little, or no, use in other contingencies. The second disadvantage is that planning on the worst possible future may, like a self-fulfilling prophecy, bring about that very contingency. This may happen because states hostile to, or fearful of the United States, structure their own military plans in part as a response to American

*Strictly speaking, we should say: if the worst fails to happen regardless of American military planning. If the United States plans in terms of the worst contingency, it may, of course, deter the kind of hostile action involved in it. In this case, we made a "self-defeating" prediction which caused us to act so as to nullify the predicted event. The difficulty is that it is hard to know whether such deterrence is or was required. For example, since the Soviet Union did not invade western Europe after World War II, was this because its leaders were deterred by American threats or because they were never interested in invasion? We do not know the answer to this question.

action. Armament may beget counter-armament, and an arms race ensues. In that case, the military preparations undertaken with reference to the worst contingency will, of course, be useful in coping with it.

The drawbacks of the two postures raise the question of whether more refined postures are not possible even though the politico-military environment is strictly speaking unpredictable. It is our thesis that there are useful ways for conjecturing about this future that can form a basis for such refinement, and provide valuable political inputs for the process of military planning, especially in R & D.

(3) Conjecture about the Future

The claim of conjecture is more modest than the claim of prediction. Conjecture is reasoned inference from admittedly defective evidence. That is to say, to conjecture is to form an opinion or judgment on what is recognized as inadequate evidence. It is distinguished from tacit intuitive judgment by two essential elements: competent use of such evidence as there is, and the use of explicit reasoning.

There is no sharp dividing line between conjecture and prediction. Conjecture indeed may be regarded as a low order of prediction. We prefer the term "conjecture" precisely in order to distinguish this activity from the ambitious forms of prediction.

The person engaging conjecture is, or should be, aware of the impossibility of hig. confidence prediction of a vast range of phenomena. He will distinguish between objects in terms of conjecturability.

Among the objects of "prediction" of interest to the military planner who wants to consider the implications of changes in the external world environment, we may distinguish between first, certain kinds of events, such as the outbreak of severe international crises, wars or revolutions; second, certain predispositions, such as attitudes toward military risks, with which governments or elites (i.e. groups from which governments are recruited) approach decisions on matters of foreign and military policy, and third, relevant capabilities (such as military forces and military potential, available to governments in making foreign-policy and military decisions).

We infer from experience the propositons that, among these objects, the class of events is less conjecturable than the classes of predispositions and capabilities; and that, within the class of capabilities, tangibles are more conjecturable than intangibles, and also more conjecturable than predispositions, which are also intangibles. Two supporting hypotheses are, first, that tangible objects are more easily observed (and often measurable) than are

intangibles, and second, events--in the genesis of which predispositions and capabilities play a part--are more unpredictably contingent than either predispositions or capabilities. That is to say, the interaction of many factors is characteristic of the determination of all three classes of objects. But, in the causation of our class of events, factors are apt to intervene which are hard, or impossible, to foresee (i.e. to identify, associate and weigh in advance). It is this special characteristic which renders events less conjecturable than predispositions and capabilities.

We also propose that objects tend to be the less conjecturable, the more remote in time they are from the present. This proposition is derived from the fact that many conditions--economic, cultural, demographic, technological, etc.--change only slowly, at least usually. This is clearly important in conjecturing about capabilities. In general, next year's technology is more like today's than is technology five of fifteen years hence. This is especially true in the modern world in which change has become accelerated, prevasive and highly interactive. Therefore, we can estimate a state's population growth more closely over the near than over the longer term. For conjecture of directly or indirectly measurable objects, projection is therefore a valuable tool of conjecture. Straight-line projection assumes, of course, that a present trend

will remain unchanged. Since this is unlikely to prove true, multiple-choice projection, resulting in a range of estimates, is often employed.

Conjecture about the predispositions of governments is more feasible over the short run than over longer periods because the behavior of elites and governments (including the bureaucracies on which government leaders depend), is observable in various international situations, and usually does not change abruptly. Abrupt changes, however, may occur, especially as a result of revolution and coup d' etat. Thus, between 1931 and 1935, bureaucracy and elites as well as government leaders underwent drastic change in Germany, and the predisposition of the Nazi leaders was utterly different from those of governments during the Weimar regime.

It must be appreciated that conjecture is simultaneously subject to the limits inherent in both propositions. Hence, conjecture about certain objects over a long period (e.g. ten years) can be undertaken with more confidence than conjecture about more recalcitrant objects over a shorter period. It may also be noted that conjecture about the same objects may, in certain cases, be more difficult over the shorter than over the longer term. This holds true--- as in the case of the national income or the balance of payments---when short-term fluctuations occur with some

frequency around a steady trend.

To sum up, conjecture about the future involves what the Germans call Vorausdenken (literally, "thinking ahead") rather than Voraussagen, which is prediction.

VI. Kinds and Purposes of Conjecture about the Future

In discussing different kinds and purposes of conjecture, it is useful to distinguish between short-term (up to five years) and long-term periods (more than five years), and also--whenever feasible and interesting--between the conjecturability of different phenomena.

(1) Intelligence: Short-Term Conjecture

In the intelligence "community," the U. S. Government has established a complex bureaucratic structure, and developed professional personnel resources, for conjecturing about the near future. These services pay close and continuous attention to the major powers, especially adversary powers, and will also pay close attention to lesser powers in critical areas of instability and great-power involvement. They attempt to ascertain present, and "estimate" future, government dispositions and policies, and national capabilities. They are also expected to forewarn government consumers of important events, such as the outbreak of war or revolution.

Their record for predicting events of this kind is necessarily poor, and should not be expected to be otherwise. Acquisition of advance information occurs rarely, and conjecturing about these events is extremely difficult because the pattern of conditions bringing them about is insufficiently iterative, and because information

about the conditions is usually crude, or unreliable, or unobtainable. For instance, early in 1967, the intelligence services should have possessed very good information about the military forces of Israel and the various Arab states; they should have been able to conjecture with considerable confidence on the outcome of a military clash between Israel and the Arab states surrounding her. Given the policies pursued by the various governments in the area, they should have considered the outbreak of war possible. But they should not have been expected to forecast the outbreak of hostilities in June 1967, or the precise circumstances that led up to it.

For the reasons developed in the foregoing, conjecture about government dispositions (and policies), and about national military capabilities is much more feasible. However, even over the shorter run, the conjecturability of these objects is subject to severe limitations whose bearing, moreover, cannot be specified in advance. Regarding capabilities, for instance, the U. S. intelligence services warned the government in 1959-60 that a gap favoring the U.S.S.R. in ICBM's might even open up within a few years. This did not happen, partly because the Soviet Union built fewer missiles than it was believed capable of, and willing to produce, and partly because, in response to the warning, the United States accelerated its own missile program. To give an example regarding government dispositions, in 1962, when intelligence data indicated a build-up of Soviet missiles in

Cuba, the U. S. intelligence services doubted that these were ballistic missiles because such an action did not fit into the estimate of Soviet government dispositions.*

Despite these inherent limitations, short-term intelligence estimates are extremely useful, particularly if the difficulties of conjecture are recognized by the consumers and producers of intelligence alike. In fact, intelligence on the military capabilities, including the military R & D efforts, of the great powers are indispensable to military planners. However, as the history of intelligence proves abundantly, the usefulness of good intelligence is conditional on the receptivity of the consumer.

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*Cf. Klaus Knorr, "Failures in National Intelligence Estimates: The Case of the Cuban Missiles," World Politics, XVI (1964), pp. 455 ff.

(2) "Typing"

If government officials and military officers could look forward to no other products of conjecture than those derived from customary intelligence operations, the planning of military systems for the future would receive scant support for decisions that must look far into the future. Fortunately, there are other avenues open for useful conjecturing; these approaches could be utilized to a greater extent, and to better effect, than they are now.

One of these avenues for conjecture involves an operation we call "typing." In order to make proper preparations for meeting the hostility of all but very powerful states, it is not important, even if perhaps deemed desirable, to study and conjecture about their particular behavior and capabilities. In fact, the less powerful a state, the lesser the importance for the U.S., for making good decisions on military R and D, of anticipating its peculiar policies and forces. Below the level of co-equal power, or certainly below the level of a handful of relatively great powers, our proposal is essentially to conjecture about types of opponents, types of allies, types of military conflicts and actions, types of theaters of military action, types of political conditions apt to affect military operations significantly, and--we add hopefully--types of United States

foreign-policy postures that may require military backing. By conjecturing not about single events, or about the capabilities of individual states, but about classes of events, of missions and of militarily significant conditions, we make the job of conjecture a great deal more feasible and credible, and yet produce results immediately relevant to most choices of military R & D. Maximizing our understanding about types of situations--rather than unique cases is obviously easier.

We will demonstrate this method in connection with counter-insurgency operations. To identify in advance, that is, to predict the country in the throes of civil war in which the United States would want to intervene militarily, is virtually hopeless. First, there are literally scores of states lacking political cohesion and efficient government, afflicted with the instabilities and frustrations generated by political and economic change, or by the lack of and demand for such change. Many of these states of doubtful viability may become the target of Communist activity, whether home-based or emanating from abroad. Second, even the United States, powerful as it is, cannot hope to intervene militarily in all insurgency situations that may erupt. How many South Vietnams can it afford in succession, let alone simultaneously? On the other hand, the

countries concerned, and the small aggressive intervening power* whom the United States might want to oppose, should violence flare up, are of a kind. And a recognition of the properties these countries have in common is sufficient for determining the military capabilities which the United States might want to have for purposes of effective intervention. Thus:

(1) Excepting the Caribbean region, all these countries are located at a considerable distance from the United States.

(2) Most of the countries are accessible from the sea.

(3) The large majority are located in the sub-tropical or in the tropical zone.

(4) Most of these countries have difficult terrain of one kind or another: mountains, jungle, swamps, desert.

(5) All of these countries are economically underdeveloped, more or less, and hence lack a dense network of transportation and other communication facilities, capacious ports, etc.

(6) Most of these countries are inhabited, wholly or largely, by non-white populations.

(7) In most of these countries, loyalty and responsiveness to the central government is weak.

(8) Many of these countries harbor radical political movements that are Communist as well as nationalist, or willing to accept Communists as allies.

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*If the aggressive intervening country is a great power, the situation is different. Conjectures about the policies and capabilities of great powers do not lend themselves to typing for the purposes we have in mind.

(9) In the event of revolution, the radical and nationalist movements in many of these countries can expect support from sympathetic other states (but rarely--alas!--from the United States).

It seems to us that these and other properties that could be identified are sufficient to give R & D a great deal of guidance in the fashioning of appropriate military capabilities. By way of example, we indicate some of the important implications of the properties we have listed, and the military requirements to which these implications point:

(1) Adequate means of mobility to the area, into the area, and within the area of insurgency.

(2) Equipment and supplies that can stand up to exacting conditions of climate and terrain, including equipment that has terrain-versatility.

(3) A police-type capability to cope with terrorist acts.

(4) Border-sealing techniques and devices in order to minimize the use of foreign sanctuaries and the influx of men, weapons, and supplies from abroad.

(5) Improved capabilities for identifying enemies who are able to conceal themselves in the civilian population.

(6) Non-lethal temporarily incapacitating weapons in order to minimize civilian casualties among the friendly or neutral populations.

(7) Capabilities for conducting counter-insurgency operations in urban areas, for the rapid growth of urban conglomerates is characteristic of nearly all the countries concerned, and it is a mistake to focus only on guerrilla warfare in the countryside.

(8) Suitable political competence among the military. Although the vital political tasks in an insurgency situation are not within the province of the military, there is a complex interface between political and military activities that calls for corresponding resources in the military services.

We conclude that this approach to the business of conjecture is useful regarding the design of counter-insurgency capabilities. The very same demonstration could be undertaken with reference to possible U. S. intervention in regional conflicts between small states. In fact, since such a war in Europe would almost certainly involve great powers from the start, thus constituting a different kind of situation, the characteristics of nearly all other states that might be involved in such local conflicts, are the same we identified in the discussion of counter-insurgency operations. The military implications, however, are different.

But, as we suggested, the typing method of conjecture is applicable to other kinds of phenomena whenever the attempt to conjecture on unique cases is close to hopeless and not indispensable. Thus, it would be possible, and useful, to conjecture

systematically about types of future opponents and allies, types of future theatres of operation, types of military actions, and types of restraints imposed on future military actions.

Two considerations affect the utility of typing from the viewpoint of long-range military planning. First, some objects lend themselves to typing more than others, for example, events (such as limited military conflicts) are less "typable" than theatres of war. Second, objects for typing should be chosen which are demonstrably close bearing on military problems.

(3) Alternat e Futures

T ing is an unsatisfactory method of conjecture when there is a high premium on foreseeing the specific properties of individual phenomena, such as the behavior, predisposition and capabilities of particular states. The Soviet Union hardly lends itself to "typing," and its future politico-military posture is obviously a matter of vital concern to American military planners. To a somewhat lesser extent, this holds true of all actual and potential middle powers--such as France, China and Japan--and at times even of lesser states--such as Israel and India--which occupy a key position in an unstable area of potentially dangerous great-power involvement.

With reference to these key states, the temptation is strong to apply to longer-range speculation the kind of intelligence work which has a much better chance for performance when put to short-term conjecture. As we pointed out, however, such conjecture is unable to inspire confidence. Just what can we predict, with any degree of credibility, about the Soviet Union as it will be ten or fifteen years from now? On the supposition that it will be spared the devastation of large-scale thermonuclear war, we can predict Communist rule, territory, and population with considerable confidence. Perhaps we can even predict GNP. But conjecture

about these sorts of objects are of little value to our purpose. For instance, even if we had confidence in the projection that the Soviet GNP will have grown by x percent ten years hence, we could not flatly assume that Soviet defense expenditures will have expanded in proportion. Even if we did, this would tell us little about the use of these expenditures —or at least that part which is not a necessary consequence of commitments made in preceding years— which it would be really interesting to know. For instance, Soviet leaders might use increased expenditures to build redundant capabilities for strategic deterrence, or instead they might develop a large capability for tactical military intervention far from its home base.

Faced with these recalcitrant difficulties, we can play down, if not give up, the attempt to predict a conjectured Soviet Union, with specified characteristics, to the future emergence of which we attribute a high subjective degree of probability. Instead, we can use all our knowledge of the present, and of recent trends, and construct a whole set of possible Soviet futures. One of these might be very favorable, and one very unfavorable to the security of the United States and its allies; one or two futures might be intermediate. Thus, it is compatible with our present knowledge that a Soviet Union could develop which is in charge of military adventurers, less afraid than she is now about its own security, increasingly preoccupied with the

solution of domestic problems, less eager to export Communist ideology, more and more a status-quo power. Or one can imagine a Soviet Union which is increasingly preoccupied with an external threat emanating from China, and eager to keep its relations with the United States and western Europe stabilized. At the other extreme, it is also compatible with present knowledge that a Soviet Union could develop which, grown militarily much stronger, re-aligned with Communist China, and increasingly confident of its ability to deter or curb United States intervention, is eager to extend the sway of Communist rule in the world, and willing to run military risks on behalf of this goal. These alternative patterns of the future could be provided with considerable detail in political, economic and military matters.

Another set of hypothetical futures could be designed with reference to China. And the alliance environment of the United States could be subjected to the same kind of exercise. For example, we might hypothesize that NATO will remain as it is, or grow more cohesive, or turn appreciably less cohesive, or give way to one or another form of truncated NATO. We might associate different assumptions on the military capabilities of our European allies with these alternative futures and we might relate these NATO futures to our set of Soviet futures since the two sets must be obviously connected. Thus, a peaceful Soviet Union might be associated with a crumbling NATO. Similar conjectures could be applied to Japan and perhaps other large countries, e.g., India.

The chief purpose of this kind of conjecturing is to broaden and enrich the perspectives of military planners, and to alert the decision-makers to the possible worlds any one of which they may have to cope with. For reasons we have already noted, the purpose is definitely not to induce planners and decision-makers to act solely or primarily on the assumption of the militarily worst possible future, for this might bring the worst future about.

Even if it is deemed advisable to envisage "the worst possible future," in order to prevent it from happening to us, one encounters difficult value problems. There is no way - in this area - to describe the "worst possible." Is it the death of all persons in the United States in a nuclear attack? But there might be some who would say that this would be preferable to their survival under enslavement to foreign rule. Is the worst, in a far more realistic sense, a coalition of the Soviet Union with China, the rise of this immense complex to dominant industrial-military power? Or is the "worst" the internal collapse of our civilization, our will to live as free men and our deliberate subjugation to others, this subjugation not even felt as one?

One could go on describing such (highly implausible?) situations. But we would not be able to choose from among them and even if we could, there might be some we could do nothing about. The point of this observation is that projections which are based on

notions of "the worst", "the best", etc. involve value judgments, and values differ. Hence there can be no specification which - even if technically possible - would be acceptable to all.

It is not undesirable, and is in fact unavoidable, that the producers and consumers of sets of multiple futures will attach subjective feelings of probability* to the development of each future. This is indeed being done by the intelligence services which usually couch their "estimates" in such language as: "there is an even chance...," or "more than an even chance," or it is "virtually certain" or "highly improbable" that X will happen, or that B will happen if A occurs. A set of at least two alternative futures is implicit in this phraseology, which assigns a higher "probability" to one future rather than the other.

Even regarding longer-range conjectures, this assignment of "probabilities" is not undesirable provided it is done with an appropriate caution that is clearly understood by the consumers as well as the producers of the conjectures. Caution insists that the assignment of "probabilities" is based on information about past and present, and that this information should therefore be regarded to be highly obsolescent. To refer once more to our tabulation on p. 19, and

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*We are aware of the fact that, technically speaking, "probability" is meaningless when applied to single events. Historical events either happen or they do not. Therefore, when we use the concept of probability in this paper, we refer to subjective estimates. They are of the kind that would lead a person to bet on one outcome rather than another.

to our analysis of its implications, what appears to us as the most plausible in a set of hypothetical futures has an excellent chance of proving wrong, indeed spectacularly wrong. Any assignment of probability, therefore, should be regarded as highly tentative and always subject to revision as the future unfolds and conflicting evidence is received. Planning must be based entirely or primarily on neither the most plausible nor the worst-possible of a set of futures. In fact, it seems to us that the great value of the set as a guide to rational military planning lies in the following suggestion: to insure against the worst possible future, though not to an extent precluding a military posture suited to other futures (including the plausible future), and in such a way as to influence developments in the direction of the best possible future.

(4) Trends in the World Environment

Another —and very fruitful— approach to conjecture is to focus on strong and relevant trends in the world environment. For our purposes, a trend is "strong" if it is firmly anchored in a part of reality which, judging from past experience, has substantial inertia, that is to say, is not subject to sudden change, and has not spent itself. A trend is "relevant" if it has a direct bearing on military power and its use. This approach once again avoids conjecture about unique events or individual members in a class of phenomena. It sticks to an analytical endeavor for which present competence is fairly high and capable of further improvement.

Once again, we demonstrate the value of this approach by way of an example. The chosen example relates to the future availability of overseas bases located on the territory of other countries for United States military operations in various parts of the world. The projection of United States military power over far distances has depended on military technology, the supply of finance for applying technology, and the availability of overseas bases. Advancing technology has been offering new opportunities for increasing the range of weapons and the range of the forces equipped for launching weapons on the target. (Actually, range is not the only consideration in the exertion of military power over distance. Speed of delivery and volume of fire power are likewise important.

What may have to be increased is the product of range times speed times size of forces.) Overseas bases help to extend range, increase speed of delivery, and facilitate volume of delivery, even though they do so at costs in terms of manpower, expenditures, and political commitments.

The United States emerged from World War II with a far-flung network of military bases overseas, notably dense in the Pacific, and subsequently it added to their number as numerous alliances and base agreements were concluded. In more recent years, the maintenance of this base system has become increasingly difficult. Adverse pressure from host countries has raised financial and other costs of base maintenance (e.g. Morocco, Panama), restricted the use of bases for military operations (e.g. Spain, Saudi Arabia, Japan), or led to the closing of bases either by the United States itself or the host country (e.g. France). This trend has gathered strength over the past ten years. Since it is based on powerful and persistent political forces (e.g. local nationalism), which have also caused the weakening and crumbling of alliances, it is reasonable to expect at this time that the trend will continue and perhaps increase in strength, during the next ten or fifteen years. As a result, the United States must at this time reckon with the possibility that the number of its overseas bases will contract and that the use of remaining bases will become more restricted.

It would be extremely difficult to predict with any confidence what will happen to any particular bases. It is much easier--for anyone able to discern recent trends and to evaluate the forces begetting the trends--to predict that the overall availability of United States bases overseas will probably decrease. This is not to say that recent trends are sure to persist and that this conjecture is therefore certain to prove correct. The conditions producing the trend could change over time. This type of conjecture, therefore, should not be exercised once for all, but rather should involve a continuous scanning of the international environment so that trends predicted earlier can be revised in the light of new evidence.

Continuing with this example, we will now indicate how much of a guide for military R & D can be gained from this sort of conjecture. The first question is whether there have been changes in United States foreign policy and military strategy, in technology and in the world environment that have modified the United States dependence on overseas bases, or will so modify this need in the future. If the need had declined, a contracting availability of bases would be less serious, or perhaps not matter at all. Thus, it is obvious that, regarding the employment of strategic nuclear forces, advances in military technology have rendered the United States far less dependent on distant bases. With the

development of the ICBM, the POLARIS system and the intercontinental bomber, the need for bases has not disappeared but very substantially diminished. The development of nuclear-powered surface ships offers another technological opportunity for reducing the need for overseas bases. On the whole, however, base requirements for tactical war and counter-insurgency operations have not declined. The question of base requirements is also sensitive to foreign policy. If the United States continues to act as a policeman against aggression the world over, the need for bases will remain great. If it cuts back this role, the need decreases. Military strategy enters the equation because, concerning limited local war, the United States has a choice, in a crisis, of having more or less recourse to raising the risk of escalation to the strategic level. The more it decides to resort to this strategy, the less its need for overseas bases will tend to be.

If we assume that United States base requirements will continue to be great, particularly for tactical and counter-insurgency actions, because no major changes in foreign policy are anticipated and that the supply of overseas bases will shrink in the future, what are the implications for military R & D, broadly conceived? We say broadly conceived since adaptations in foreign policy and military strategy may afford a partial solution of the

problem. But several lines of military hardware development also suggest themselves as worthy of study:

(1) Military technologies designed to reduce the need for bases by designing weapon systems of greater range (e. g. naval task forces powered by nuclear engines) or lessening qualitative base requirements within a theater of operations (e. g. aircraft capable of very short take-off).

(2) Improvements in the design of air and sea lift with a view to increasing speed and volume of deployment.

(3) Technologies for ocean-floating bases that can be stationed overseas and can accommodate military forces and supplies.

(4) The technology of quick base and port construction. Even if the supply of United States overseas bases will contract in the future, this refers usually to the maintenance of bases in areas not involved in an ongoing military conflict, or facing a severe military threat. As experience shows, however, foreign countries under direct military pressure may seek military aid from the United States and are then ready to accommodate United States base needs on their territory (e. g. South Vietnam, Thailand). Yet since this will happen usually only when a military crisis has become acute, there is then a premium on speedy base construction. It is therefore interesting to explore technologies that facilitate rapid base construction, for example, quick building of docks and pre-stocking of machines and components.

To give some other examples of trend analysis more briefly, there is, first, the powerful stigma attached to the use of nuclear weapons, especially their tactical use against non-nuclear countries. This stigma tends to become reinforced with every year in which nuclear arms have not been fired in combat. It affects military strategy and force requirements for tactical operations, the need for overseas bases, etc. It suggests the continued merit of R & D looking toward the improvement of non-nuclear armaments. And it suggests, furthermore, that if a nuclear power experiences severe military pressure in a local conflict, and contemplates the employment of nuclear arms against a non-nuclear enemy, it may want to defy the stigma as little as possible; and this will place a premium on very limited, and primarily defensive, applications, that is to say, on weapons of high accuracy, low yield and minimal fall-out, on defensive rather than offensive systems (e.g. ASW, anti-aircraft, demolition explosives), and on military doctrines, and command-and-control arrangements, appropriate to such carefully restricted employment.

For another example, we refer to the growth of population and particularly of sprawling urban and semi-urban conglomerates in the less developed countries of Asia, Latin America and Africa (as well as in highly developed societies), and to their common characteristics as potential theaters of military operations, especially in the

case of counter-insurgency. The capture, control, and defense of such areas, teeming with population, obviously present a military problem quite different from countering guerrilla warfare in the sparsely settled countryside or from waging conventional war which, in terms of weaponry and doctrine, is traditionally focused on operations in the countryside. As a potential theater of operations, the urban conglomerates suggest interesting questions about suitable weapons, forces and doctrines. For instance, the development and use of non-lethal temporarily incapacitating weapons, more effective than tear gas, is an interesting question in this context.

There are also trends in the world environment which impinge on the feasibility of foreign policies, and hence affect military postures only indirectly. Thus, the past several decades, and especially the period since World War II, have witnessed the increasing development of restraints on the international employment of military force.** These restraints can be defied only at the expense of arousing political resentment and hostility, and endangering the reputation of a country even among its friends and allies. The use of military force that is aggressive, or looks aggressive to third countries, has lost in international legitimacy. The exercise of military power by a big, rich and powerful country against a small, poor and weak country has come to provoke widespread resentment. To give another example of this kind, it can be anticipated, at least at

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**For an analysis of these developments and their consequences, see Klaus Knorr, On the Uses of Military Power in the Nuclear Age, Princeton, Princeton University Press, 1966.

present juncture, that world communism will present a less and less monolithic front, and that the conflict-generating capacity of the postwar struggle between Communist countries as a class and "democratic" countries as a class will become more muted, while tensions between highly-developed, rich nations and under-developed, poor countries are likely to become acerbated, and breed international conflicts on the outcome of which military strength may be of some consequence.

These examples demonstrate the relevance of conjecture about general trends in the future world environment within which the United States expects to exert military power. This conjectural focus relates changes in the expected world environment to military systems by paying the closest attention to the uses of military power. It is functionally close to the problem of deciding on future military postures and their worth. It avoids the diffusion and confusion resulting from an attempt at casting the conjecturing net too far afield; and it also avoids problems excessively beset with uncertainties and resulting in very low-confidence estimates.

(5) Conclusion on Conjecturing Approaches

In the foregoing analysis, we have shown that a great deal of military planning, especially R & D planning of a non-routine character, is highly sensitive to the assumptions made on the future politico-military environment within which the United States is expected to maintain and employ military power. We have demonstrated that, and explained why, prediction of phenomena particularly relevant to such military planning is impossible with anything but the lowest degree of confidence, especially the farther away the future we try to speculate about. We have described several methods of conjecture about the future which rank higher in feasibility, and yet can produce useful politico-military inputs for employment in the planning process.

The usefulness of this sort of Vorausdenken resides in three promises. First, it can minimize the risk that United States military preparations for the future are based on the wrong pattern of contingencies, and hence leaves the country ill-prepared. Second, for the same reason, proper conjectures can prevent waste of resources. Successful politico-military conjecture is thus a valuable ally of proper cost-benefit analysis. In fact, the inexorable pressure of resource restraints alone argues powerfully for placing greater emphasis on politico-military conjecture. Third, to the extent that the United States is at this time the most

powerful nation in the world and can influence the development of the future, that is to say, to the extent that it can choose the future--conjecture about the future politics--military environment is indispensable.

We did not, however, apply these methods in a systematical way since to do so would be a different and vast task. We are also convinced that the operational use of these methods are capable of considerable refinement by dint of both trial and error and appropriate researches. In the following section, we address ourselves to some aspects of the administrative problems that are posed by the objective of inserting politico-military inputs in the military planning process.

VII. The Political Expert in Military Planning

In this section our subject is the expert who can provide political inputs concerning the future world environment. We begin by defining various roles in the process of military planning; particularly R & D management.

(1) Roles in R and D Management

In Science and Defense our principal focus was on how the skills of scientists and engineers could be best exploited in the design of effective military postures. We identified the roles of the inventor and the innovator, and their relationships. To repeat briefly, the inventor has a new idea capable of military application. While the inventor can be an engineer or a scientist, the scientist also produces (i. e., invents) new basic knowledge from which new technological ideas can be derived. The innovator weighs various technological choices, compares their expected utility, costs, and risks. He decides on the worthwhileness of developing and adopting a new idea and assumes responsibility for his decision. His creativity is in choosing the most promising combination of military end-products. In short, the innovator is an entrepreneur who gets things done and who is ever alert to ways of modifying his output

and his production methods.*

We also looked at the balance of stimuli flowing between innovator and inventor. The flow of ideas from inventor to innovator concerns proposals of improved or entirely novel weapons. The stimulus is: this is what we can do, or think we can do. The flow of ideas from innovator to inventor concerns new military problems and related needs that are more or less conceptualized. The stimulus is: here is a problem, can you do something about this? We noted that, in the past, the flow of stimuli from inventor to innovator had been richer than the reverse flow from innovator to inventor. We wondered whether the balance of flows could and should not be improved; whether there could and should not be more or better direction of the inventor by the innovator.

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*Schumpeter, who was a keen student of the innovating function, pointed long ago to an important change in the mode of innovating enterprise. While innovation was previously crucially dependent on the personal genius of the innovating entrepreneur, the recent growth of specialized knowledge and large bureaucratic structures in government as well as business enterprise has made innovation more routine. That is to say, it now depends less on the flash of intuition and the quality of personal judgment, and more on the systematic and rigorous study of alternative courses of action and their estimated consequences. (Cf. Joseph A. Schumpeter, Capitalism, Socialism and Democracy, 3rd. ed., New York, Harper, 1950, p. 132.) However, Schumpeter was inclined to overestimate the magnitude of this change. The new resources and methods have made innovation appreciably more routine than before, but scarcely routine altogether. Specialized knowledge and its concerted use have their limits, and judgment remains an important element in much decision-making.

While in Science and Defense our main focus was on hardware problems in R & D, we nevertheless also pointed to the need for the infusion of political expertise into the R & D process (especially pp. 52, 57). In the present paper, our concern is chiefly with the role of political expertise in the military planning process, with particular reference to R & D. Before discussing this matter, we must - in order to place our analysis within the overall context - note the role of one more expert: the economist. His role is obviously important. It is a crucial condition of military planning that economic resources available fordefense are always scarce in relation to what those in charge of the defnse effort might wish to get and this scarcity situation prevails also for practically all sub-problems of military allocation. Choices must be made on this basis as well as other bases. The economist is skilled in conceptualizing and solving problems of optimization, identifying, procuring, and utilizing the data necessary for solutions.

There can be no doubt that the process of military planning for the future, notably including R & D, would be ill-served by a scanty and haphazard use of political expertise. In conceptualizing the function of the political expert, we exclude political expertise about which the American people and their elected representatives will demand from, and give to the military establishment at any one time. The problem of this kind of goal-setting is outside our purview. We are only concerned with political expertise about those aspects of the

international environment that are germane to military problems. While the need for this kind of knowledge has existed in the past, even if insufficiently recognized until recently, and insufficiently met even now, it is a need that - as we have already observed - has been growing in importance, certainly as far as the United States is concerned.

Military power has always been used not only for waging war but for threat-making, whether for purposes of deterring attack or of what has been called "compellence";* conducting the diplomacy of violence has always been a business for which political expertise is an obviously important input. However, with the advent of nuclear offensive weapons enjoying a vast superiority over defensive systems, deterrence —which is credible threat-making— dominates the use of military capabilities on the strategic level between nuclear powers; like all military deterrence, this kind is not only a matter of military forces but also of communications between opponents. The design of these communications requires political expertise. Moreover, whenever tactical local war involves a risk of escalation to the strategic level, the choice of military initiative and riposte demands continuous political inputs of a high order. Furthermore, as long as the United States is determined to intervene in local wars on behalf of its conception of a tolerable world order, choosing the time, place, and mode of intervention is not just a

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* Thomas C. Schelling, Arms and Influence, New Haven, Yale University Press, 1966.

military problem but a delicate task for diplomacy backed by political expertise. The need for political expertise is also acute and demanding as long as the United States will consider armed intervention in civil war situations. Civil wars are only partly military conflicts; they are usually primarily political contests. This mean that resort to military means must be concerted intimately with the use of political and economic means.

The fact that change of all kinds, including political change, has become accelerated the world over, puts a further premium on the insertion of political expertise in the process of military planning. As long as change was slow, it could be ignored, or recognized and acted upon with delay, without serious consequences. This is patently not the case in the modern world.

It follows that military strategies, doctrines, and forces require continuous review in relation to the politics of various conceivable conflict situations, and also of the political factors that tend to make foreign countries foes, allies, or neutrals in struggles involving the United States. Hence, our examination of the business of conjecturing about the future was concerned throughout with the future politico-military environment.

Inputs of political expertise are not, of course, equally important for all kinds of military R & D. It is of little, or no, importance to R & D concerned with upgrading established weaponry as, for instance, the development of a somewhat better tank or missile. Such projects raise only questions of economic and technological merit.

Political expertise is of great, and not seldom central, importance when it comes to the most forward-looking kind of R & D, to projects concerned with new conditions, greatly improved weapons, or entirely new types of armament. To refer to examples of the past, this was the case with the development of the POLARIS and SKYBOLT systems, with the development of nuclear-powered ships, or with the decision to demote naval gunnery in favor of missiles. At present, political factors loom obviously large regarding such systems as ABMs, CB weapons or new airlift systems. Political expertise is extremely relevant to the development of new military strategies, such as the stress on multiple options for the defense of Western Europe in the early 1960's, or to adaptation to new strategic conditions - such as the development of mutual deterrence on the strategic nuclear level, or various international arms control arrangements.

(2) The Role of Political Expert

Whenever political inputs are important, if not crucial to making sound R & D decision on military hardware, or the development of force structures, military doctrine and strategy, who should supply the inputs? The obvious answer, but one often disregarded in practice, is that these inputs must be produced by people commanding political expertise as experts, that is, as professionals. For reasons we will spell out

below, it would not do, as has often been done in the past, to rely casually on available civilian bureaucrats, military personnel, economists, and physical scientists who are willing, or are made, to double as political experts. It should be noted in this respect that, as with other professional roles, two requirements are attached to that of the political expert. He is to provide expertise, of course, but also he is to provide it responsibly. That is to say, one requires a professional to be responsible both in terms of being sensitive to his professional limitations and in terms of justified accountability for his work. If the amateur bungles, he has the ready-made excuse that, after all, he could, and should, not have been expected to be more than an amateur.

But what exactly are the professional qualifications of the political expert? Regarding short-term intelligence work, we assume that the intelligence services have established a set of qualifications and professional standards which are reasonably well defined, though we are not sure that these standards have been defined sharply and codified so that an inquiry would elicit quick, clear-cut, and agreed-upon answers. Regarding longer-range conjecture, there has been no attempt, to our knowledge, at defining the conditions of political expertise. Nor is it possible at this point to do so as satisfactorily, as it can be done for expertise in economics, statistics, or physics. Indeed, to identify the appropriate set of qualifications definitely seems to us important enough, at this stage, to warrant a special R & D study. At this point, we can do no more than offer some suggestions.

To say that the expert on world politics must be perceptive, knowledgeable, imaginative, articulate, and courageous would be saying next to nothing. The questions are with regard to what he should have these admirable qualities, and how their presence can be recognized and tested, and their magnitude graded. Nor would it be useful to say that all we need is a pragmatic test, that we simply pick those people whose forecasts -- judged with the benefit of hindsight -- yield the highest batting average. The trouble with this notion is that, in the case of long-range conjectures, batting averages can be calculated only with the great delay and that, without knowing much about expert qualifications, nothing but mediocre candidates may have been sent to bat.

If we look at the several approaches to long-term conjecture about the politico-military environment, we can infer some of the qualities of expertise that should be in demand.

First, the political expert in question must have a broad knowledge of world affairs, not in the sense that he is intimately familiar with the minutae of the many national governments, economies, societies and policies, but in the sense that he understands the developing patterns of population and economic growth, and the main thrusts of political and ideological forces at work the world over, the evolving nature of modern-day nationalism, socialism and communism, the syndroms of aspirations and frustrations, especially in the developing countries, the nature of political revolutions, etc.

Second, in order to make such far-ranging knowledge both possible and usable, the political expert must possess sophisticated conceptual equipment which permits available information to be ordered. He need not have the full conceptual competence of the professional economist, demographer, political scientist, sociologist, and historian. But, cutting across these specialist competences, he needs to understand the key concepts which explain such common processes as economic growth and political revolutions.

Third, based on a broad knowledge of world affairs and a tool-box of concepts, plus an imaginative mind, the political expert should have a considerable ability to recognize patterns of problems, activities and events in the contemporary world. Such pattern recognition means a capacity for seeing the composite picture of what is happening in the world at large and in particular regions. It means an ability to synthesize as well as analyze.

Fourth, while the political expert cannot and need not be a specialist on any particular country or geographic area, on the economics of development, on the intricacies of socialist doctrines, or other subjects of this kind, he does need the ability to make use of a range of specialists, drawing upon them as assistants, or upon their work as raw material. He must be a specialist in using specialists; and the wide-ranging conceptual sophistication described above should insure this competence.

Fifth, the political expert must be free from ideological bias since such bias colors the complexion and distorts the structure of the outside world as he perceives it. Thus, anyone who, in the mid-1960's, believes communism to be a monolithic world movement, or an irresistible threat to the integrity of most underdeveloped countries, is seriously reduced in his ability to comprehend present-day world reality.

Sixth, the political expert, though he need not be a military expert, should be familiar enough with military realities - forces, technology, doctrines, strategies - so that he is enabled to turn his own expertise on problems of high military relevance. Since the objects of conjecture in the political environment are multitudinous, it is essential that the conjecturing under discussion here is closely related to military problems.

Finally, the political expert should have the ability of "reasoned" conjecture. It is not good enough for him to be oracular on the basis of good intuition and smart judgment. He must be able to formulate the analysis which supports his conclusions. This is necessary so that his output can be subjected to the criticism of other experts, which is to say that his output must withstand the challenge of contrary knowledge and the usual tests of logic.

On top of all this, it is clear that the experts must also command good judgment in sifting the stream of evidence, often ambiguous if not contradictory, in choosing conceptual frameworks, in estimating the strength of trends and forces, and in deciding on their relevance to military problems. But at the present stage of knowledge, the bases of good judgment are close to inexplicable.

This list of qualifications may look like a "tall" order which only supermen are able to fit. Yet this is so only if one has unreasonably high expectations. The list lays down a set of qualifications which is by no means more extensive than the set we would want a professional intelligence officer or a policy-planner in the Department of State to possess. Securing a reasonable level of competence turns on questions of recruitment and training.

This description of qualifications is still somewhat rudimentary. A substantial research effort drawing upon various specialists could be directed toward its refinement and, as a further step, toward designing appropriate and measurable indicators for the battery of qualifications. Such indicators would be administratively very important since they would permit a degree of rigorous testing of personnel. It is unlikely that any set of tests would be good enough to obviate altogether resort to the trial-and-error method of establishing adequate expertise. But it would seem undesirable, because inefficient, to rely on trial and error entirely. To discover a proper set of indicators is a big task which itself merits consideration as a major project in R & D.

(3) Recruitment and Training

As long as we lack a fully developed and tested conception of the qualifications with which the political expert should be endowed, we are unable to develop appropriate procedures for his recruitment and training. However, the foregoing analysis has gone far enough to shed light on some aspects of the problem.

Anyone who is alive to the need for the kind of political expertise under review, and who is familiar with the relevant institutions, has come across individuals who, one is rather confident, answer to the basic requirements of the role we outlined. They are to be found in the intelligence services, among the military, among civilian officials in the Pentagon, in the Department of State and the Foreign Service, in research organizations such as RAND, at the universities and occasionally among newspapermen. But one has not met many such individuals anywhere. Whatever the institutional base or profession, their density is very low.

We believe that their scarcity results not from lack of talent, but from the fact that the incentive structure and training opportunities at all the institutions we mentioned are ill-designed for producing the kind of expert we have in mind. As matters stand now, the rewards for accomplishment are uncertain and small. Thus, the orientation

in the intelligence services is overwhelmingly toward short-term estimates of future conditions and to developing the professional skills for this kind of work. The military —who still aspire to the ideal of the well-rounded officer, and hence frown on too much specialization— demand a great many qualifications from their members: top-level leadership, command in the field, administration, teaching and training, development of military strategy and doctrine, and other innovation. It would be hard to add political expertise to this long list. Moreover, the way the military are familiarized with world politics at the advanced service schools is too sketchy to develop competence in this area.* In the Foreign Service and the State Department, the overwhelming pull is in the direction of attending to immediate problems, and taking the longer-run view is hardly encouraged. Even when a structure is created to supplement this strong preoccupation with the problems of the day —for example, the Policy Planning Council— the incentive of its members is to insert themselves in the dominating problems of the present. This is where rewards beckon, while the products of long-range analysis are likely to be consigned to dusty shelves. At the universities - where much

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*Cf. Edward L. Katzenbach, Jr., "The Demotion of Professionalism at the War Colleges," United States Naval Institute Proceedings, vol. 91, March 1965, pp. 34-41. More hopeful, incidentally, is the enrollment of military officers for a regular two-year, and occasionally three-year, program of training in the social science departments of universities.

useful conceptual work is under cultivation - the prevailing push is nevertheless toward specialization along lines that do not tend to produce deliberately and directly the kind of expertise, skilled in synthesis as well as analysis, we have in mind. That the political expert can be found in all these institutional settings and in a broad range of professions (even among economists) suggests that the development of the appropriate perspective and combination of skills results from the accident of personal inclination, and a personal response to a personally perceived challenge. This makes for a haphazard production of the expertise in question.

Under these circumstances - that is, a small supply of qualified experts, scattered over many institutions and professions, and lacking a definition of professional qualifications, and proper institutional incentives - it would be surprising if their recruitment for the role of infusing the processes of military planning, including R & D, with a necessary kind of expertise, were other than haphazard, a hit-and-miss business. What has kept this state of affairs from becoming glaringly obvious is that deliberate recruitment for the role has been on a small scale and half-hearted to boot. Under the same circumstances, one also would not expect a deliberate institutionalized attempt at training personnel for this function which we believe to be vital but has not been generally judged to be so.

The development of productive training methods depends, however, in large part on prior research on the specific qualifications of the political expert we have in mind. As suggested above, there are some tough problems, perhaps insoluble for some time. One can hardly train for imagination and judgment as long as their elements are not well understood. Yet these obstacles scarcely argue against any training or recruitment for this role, or against the feasibility of the role altogether. These are problems also encountered in training other professionals, such as teachers, physicians and soldiers. Thus, we noted that the politico-military expert requires the ability to synthesize as well as analyze. Universities have become quite successful in teaching analytical skill. But they have not so far developed a good basis for teaching synthesis. This is, for example, a well known problem in teaching engineers. As Lord Bowden, the Principal of the College of Science and Technology at the University of Manchester, declared recently: "You can train young men to do analysis, but I don't know how to train them to synthesize."*

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* "Nations and Professionals," Science and Technology, No. 72, Dec. 1967, p. 39.

VIII. Utilizing Political Expertise

Even if the need for political expertise is recognized, its standards defined, the necessary skills developed, and procedures formulated for matching skills and role, the question remains of how this role should be integrated into the structures to which the task of military planning for the future is assigned.

Though we will discuss some crucial aspects of this problem, there are several germane matters which are excluded from the present paper.

First, it does not present a description of the present administrative system for utilizing political expertise. To some extent - but to an extent we believe inadequate - political experts (or sometimes pseudo-political experts) are employed in producing inputs for the planning process. This happens within the military services, in the Department of Defense, and also in the work of outside contractors hired to undertake particular studies. But we are not sufficiently familiar with these practices to give a worthwhile description of the existing system. This does not, of course, imply that it would not be useful to have such a description, or indeed an analytical historical study of how the problem has been coped with in the past.

Second, we do not intend to propose full-blown administrative blueprints, i.e., organization charts, that might be considered if resort to political expertise is to be broadened and improved. This is a job for management and public-administration experts who command the skills we do not possess. The contribution we hope to make is conceptual.

Third, it is well known that the process of deciding on military plans for the future, and of acting on these plans, is in large part not a problem of expert planners formulating rational choices for achieving desired objectives, but a matter of intra-institutional and inter-institutional politics, that is, of negotiations and bargaining among officials and officers (also legislators and lobbyists) which derive a powerful motivation from the desire to serve particularist career, service, agency, and business interests.* Although this competitive pluralistic system, and the factionalism that inspires it, are of central importance, this is another subject with which we are insufficiently familiar. Besides, whatever the influence of such politics on decisions —and we do not want to suggest that this rivalry does not involve favorable as well as wasteful effects— decision-making is also responsive to the goal and modes of rational problem-solving. In any case, it is this latter part of reality with which this memorandum is concerned.

Fourth, we ignore pertinent problems encountered more or less in all large-scale organizations —such as centralization versus decentralization, and their respective merits and demerits— since these problems are for the most part not specific to the insertion of politico-military inputs into the planning process.

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*On this see James R. Schlesinger, Systems Analysis and the Political Process, The RAND Corporation, P-3464, June 1967.

In the following we present certain prerequisites of a rational solution for enriching the military planning process by the infusion of political expertise.

(1) Some Administrative Problems

First, any substantial improvement over present practices requires doctrinal innovation. The doctrine must recognize, and enforce the recognition of, the dependence of sound planning, including R & D, on the use of the political expert. To be effective, the new doctrine should be diffused throughout the relevant organizations; it must influence behavior up and down the line in the military services and in the Department of Defense. Of course, the hold of the doctrine should be especially strong in specialized structures entrusted with the analysis of problems of choice. But it is equally important that this hold be firm at the executive level where decisions are made, that is at the top of the military services, the Joint Chiefs of Staff, and the civilian bureaucracies in the Pentagon and in the Department of State. If it is not, the occupants of subordinate levels will tend to de-emphasize this function.

Second, at the present time, the relevant attitudes and practices at the executive level are not conducive to the adequate use of politico-military inputs referring to the future. The use of such conjectures is being played down, or they are ignored, even if, as occasionally happens,

the inputs have been prepared in one place or another of the organization. This blockage occurs for several reasons. Thus, the inputs are regarded as so "soft", i.e., lacking the reassuring quality of (apparently) "hard" facts, as to be worthless. Or, because regarded as soft, they are easily shoved aside when conflicting with existing preconceptions or when running counter to particular interest. Their present use also suffers from an anti-intellectual, narrowly pragmatist climate characteristic of much American organizational life * which has perhaps yielded recently to the intrusion of the economist, but is as yet inhospitable to the political expert. Basically, moreover, conjecturing will tend to increase, rather than diminish, the recognition of uncertainties and, in response to uncertainties, "... the executive tends to rely, not on the expert, but on precedent, trial and error, short-run feedback."**

Third, part of a proper administrative doctrine would, of course, be a realistic appreciation of the limitations of the political inputs. Unless these limitations are doctrinally defined and generally understood, the temptation to make excessive demands on the political expert might be as great as the present inclination to ignore, if not scoff at, him. The result would be to invite error and disappointment. Unavoidably, these inputs will be speculative and "iffy" where certainty is vastly

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* Cf. Harold L. Wilensky, Organizational Intelligence, New York, Basic Books, 1967, pp. 63, 80

** Ibid, p. 80.

preferred. In short, they will be "soft", although often not appreciably more so that the "facts" introduced in technological and economic considerations. In any case, assumptions about the future are inescapable in any planning, and the inputs we have described are a great deal less soft than the tacit, implicit assumptions that are sure to guide planning in their absence. They are less soft precisely because they are reasoned, because they are presumably the fruit of a systematic and continuous activity, because, not being tacit, they are subject to reasoned criticism, and because they would be produced increasingly by professionally competent personnel.

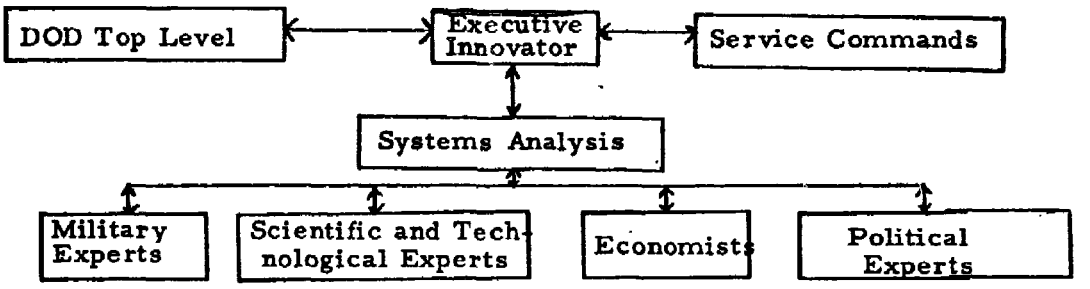
Fourth, at the present time, the demand for political inputs is usually organized on an ad hoc basis. There is nothing wrong with varying the demand on political expertise with the importance and susceptibility of different planning problems. But since ad hoc decisions on the need for these inputs are usually made at a level where the appreciation of their potential value is underdeveloped, there seems to us need for rules that raise the applicability of political considerations as a matter of routine.

Fifth, there seems to us a great deal to be said for the Department of Defense, and perhaps, in addition, even each military service, to maintain a first-rate specialized structure engaged in the business of anticipating the future conditions of the exercise of military power. Since for shorter-range forecasting, this structure could rely largely on the established intelligence services, its work should emphasize the more speculative conjecturing about the longer run. Such a specialized structure would, of course, serve as a common resource to be drawn upon by various other agencies, bureaus, task forces, etc.,

concerned with military planning for the future.

Sixth, if in the case of major R & D decisions we think of systems analysis as a technique for preparing the decision-maker, as a rigorous synthesizer of various considerations bearing on the act of choice, political expertise should be included deliberately and co-equally along with expertise on military, technological and economic consequences. In this connection it might be noted that economists have, since 1961, perhaps been too dominant in systems analyses carried on in the Department of Defense. Historically viewed, it is the economist's skill in conceptualizing problems of optimization that accounts for the emergence of this dominance. However, there is no logical reason why economists should be unduly prominent in the application of this tool if the problem of military choice is taken as broadly as we think it should be taken. The overall job might just as well be directed by qualified military men or political experts.

The following diagram illuminates the essential role relationships.



An alternative schema would be to insert an office of political expertise between the level of systems analysis as conducted now and the level of the "executive-innovator." In this case, the recommendations proceeding from systems analysis would reach the decision-making level along with a document of reasoned political advice.

Seventh, another and crucial relationship represented on the diagram is that between the innovator and the top level of service commands, joint commands, and the DOD. In order to be able to devote himself fully to his demanding role, the innovator must be insulated to a degree from the other types of activities in the services, special commands and the DOD. He must also be free from traditional modes of operation that, though effective in the rest of the defense establishment, interfere with performing the creative functions assigned to him. But although the innovating function is separable and requires a good deal of autonomy, its product must nevertheless be capable of prompt review and acceptance by those parts of the overall structure that are meant to be the consumers of innovation. At the same time, the innovator must be highly responsive to problems brought to him by the prospective consumer. The way the relationship between the institutional roles is structured will condition the success with which the innovative function is performed. This is, of course, part of the general problem posed by administrative specialization and compartmentation. This practice is inevitable if the advantages of specialization are to be secured and if complex problems must be broken up into manageable parts. Yet the practice of division also creates interface problems and the neglect of relationships that must be taken into account whenever problems of choice require compromises and hence a thorough examination of trade-offs between different values. If the consideration of problems is divided with reference to military, technological or economic factors, there is always a danger that the application of political expertise is neglected.

An administrative problem which is bound to be raised is how to proceed when the political experts disagree. The assumption that leads to the question, and seemingly makes it into a key problem, is that disagreement would be frequent; that even if on political matters one opinion were not as "good" as another, there would be no way of telling the "good" from the "bad"; and that, if this were so, and no acceptable procedure for the settlement of disagreement existed, the political inputs would be depreciated, if not ignored.

This sad situation would indeed prevail if, instead of the modest types of reasoned conjecture described in this paper, it were the attempt to predict the unpredictable which would be institutionalized. In that case, it would certainly be impossible to distinguish the fool from the wizard; and if political inputs were not disregarded altogether, that input would be accepted which proved most convenient, that is, which encouraged the decision-maker to do what he wanted to do on other grounds. However, if "reasoned conjecture" is adopted, then one institutionalizes a professional function. And if this kind of political conjecture is professionalized, then there is no reason to expect more disagreement than from other professional experts, i.e., the military, scientists, engineers and economists. As in the case of these professions, the political experts engaging in trend analysis,

"typing" or the design of alternative futures would be likely to produce analyses and conclusions which agree much more than they disagree. Usually the area of conflict should be sharply delineated, and much disagreement should be a matter of emphasis rather than clear incompatibility.

This is not to say that minor disagreements would not happen often and that important disagreement might not occur occasionally. Differences of opinion might arise even concerning the relevant data, their quality and composition. Usually, the data do not simply exist just to be picked up and studied. They have to be gathered and concepts are required for guiding the acquisition and evaluation of information. Disagreement among experts is especially apt to arise when it comes to the recognition of patterns, the weighing of trends, the construction of types, etc. As in all professions, in the profession we have characterized, some practitioners will be better trained, more careful, harder working, more intelligent and imaginative than others. But because this kind of differentiation is a well-known administrative problem, it should not prove inhibiting in this case. It exists throughout government and among the military, and so it does in business and the civilian professions. Even though medical men may differ in diagnosis and prescription, we continue to consult them.

When, on occasion, crucial disagreements arise, it is important that they are fully as well as clearly stated, and that if the difference cannot be resolved, the consumer of advice design a policy which is sensitive to this lack of determination. If this is to happen, the disagreements must be placed before the chief decision-makers rather than have them excised or smoothed over on the way up. In any case, the problem of important disagreement between experts has been faced and dealt with by the intelligent services, the Department of State, and other government agencies. It is common in systems analysis as practiced up to now. Its employment also is an art, not a science. This means that it is often difficult to tell good from bad performance or results. Adding politico-military inputs systematically may complicate but does not fundamentally change the problem of management.

If military planning for the future, and particularly R & D management are to be upgraded in the United States, the reform of institutions and especially practices must be responsive, we believe, to the several points we have made. Only then will good planning become a matter of routine rather than of happenstance. Bringing such reform about calls for a truly innovative act. Regarding the planning decisions with which we are concerned, the output depends crucially on the kinds and quality of inputs; in this respect we have focused in this paper on the importance of adding proper political expertise to the mix of inputs. But the quality of the output also depends on the way in which different inputs are employed, and how their use is combined, in making choices. As one naval office put it: '<u>How</u> decisions are reached greatly influences

what decisions are reached."[1] One important problem of employing political expertise concerns the production, recruitment and institutional deployment of the experts. The other important problem is the development and effective propagation of a doctrine concerning the usability and use of the political expert. Such a doctrine is a vital condition of their proper use, and a prerequisite to solving the problem facing all bureaucratic structures: how to reconcile specialization with unity of direction.

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1 Captain Stanley M. Barnes, "Defense Planning Processes, An Unresolved National Problem," United States Naval Institute Proceedings, vol. 90, June 1964, p. 28.

(2) Military and Civilians

The problems with which we are here concerned, can be solved only if military and civilian leaders and experts act as a team. Not long ago —say, prior to World War II— military planning for the future, including R & D, was nearly entirely within the province of the military in fact, within the highly autonomous provinces of the military services. In each service, it was the military who developed strategic concepts, planned for various contingencies, and decided on the development of armament. In doing so, they had to interpret military security interests, and define and respond to various political requirements and restraints, both present and future. Obviously, this set of tasks demanded inputs of political expertise which the military were unequipped to produce and to utilize. They were, after all, not trained for this function and, given all their other training requirements, they hardly could have been trained for it adequately.

Our analysis suggests that this past scheme of doing things was primitive and unrealistic. Aside from the fact that, at the very top of the defense hierarchy, civilian leaders (e.g., the President, the Secretaries of Defense and State) must necessarily decide —though not without paying due attention to military advice— on foreign policy, overall military strategy, the application of military power, and overall defense expenditures, the planning process we have been reviewing,

calls for the proper infusion of various skills— those of the political expert, the scientist and engineer, and the economist —which only specialized civilians normally develop to a high level of proficiency. But this leaves the military with plenty of room for leadership and staff participation. In addition to the military command and training, and various administrative functions, which are their natural bailiwick, their place in military planning for the future should be as conspicuous and influential as it is indispensable. To begin with, military expertise is obviously one of several kinds of expertise required at the analytical level. Beyond this, it seems to us that the military should also occupy a leading (but not exclusive) position in the role of the innovator. It is, of course, true that the innovator as an individual authoritative leader has become rare, and may be disappearing, in all large problem-solving organisations, in this age of complex bureaucratic structures. Formally or de facto , the innovator will usually be a committee (structures which are not as doomed to sterility as the proverbial jibes imply). Committees need not necessarily be committees of equals, with initiating and veto powers evenly distributed. Nor should, or will, they be incapable of delegating responsibility and authority to individuals whenever personal leadership is a condition of success.

(3) Criteria for Evaluation

In order to do any job well, institutions need criteria for measuring performance. The task of military planning for the future, including

R & D, is no exception. This is why we raise the problem. In addressing ourselves to it in this concluding section, we end on one pessimistic and one optimistic note.

Unhappily, we are unable to propose a solution to the criterion problem for the line of activity under discussion. And we are convinced that no one else can do appreciably better at this time. One reason for our pessimism is the prodigious and perplexing complexity of the planning task. Another is the vast incidence of uncertainties regarding the future under which the task must be executed. There are, of course, the usual tests that are immediately applicable to all rational problem-solving. Are the assumptions clear and consistent? Have all parts of the problem been defined and considered? Is the analysis logical? Are the conclusions consistent? But these tests, valuable as they are, do not take us far enough. In view of the great difficulties inherent in conjecturing on, and coping with, future contingencies, there is only one definitive overall test, namely the retrospective test of measuring plans for the future against performance when the future has become the present. At any one time we can, beyond doubt, learn something from post-mortems of past decision-making once the results are "in." It is also useful that decisions taken in any year are reviewed in the light of new circumstances in subsequent years. But the full test comes necessarily too late. Occasionally, what other governments are doing may also serve as a test. At least, if their choices

differ from our own under comparable circumstances, we should be willing to re-examine our decisions.

On the other hand —and this leads us to our optimistic note— perfection would give us minimum loss of power, lives and treasure. But as it is unrealistic to expect perfection, so lesser performance is tolerable. United States decisions take effect in a world in which the decision-makers of other countries labor under similar handicaps. If the United States did a little better than governments and military leaders in other states, it would be relatively well off. Indeed, if it did only as well, or even a little worse, its security would not be compromised seriously as long as this country remains the nation richest in many relevant resources and therefore able to take quick remedial action in order to compensate for past errors.

After all, the United States has not, on the whole, done badly in recent decades. And it seems to us that our analysis points to ways for doing at least a little better than before. In fact, the process of military planning for future contingencies was performed by extremely primitive methods not so very long ago. These procedures have been greatly improved in recent years. Yet as we have demonstrated, they are still in many ways the result of quick and ad hoc improvisation and hence they are more haphazard in their effects than they need be. Under these conditions, even such procedural and institutional improvements, along the lines we have suggested, may well have a big pay-off. If we continue to muddle through, which is the inevitable nature of this business, we will then do it better.

IX. Some Recommendations

In line with our plea for broadening the application of military R & D to "soft" areas beyond hardware, including problems of strategy and doctrine, the principal recommendation is that the entire problem of conjecture about the future politico-military environment, and of how to mobilize and employ the relevant expertise, be made the subject of a large-scale and systematic R & D effort. An analytical paper such as the present can only raise questions, identify problems and suggest tentative conclusions. Hopefully it will stimulate responsible officials to take the problem seriously. But a large-scale and sustained effort, with participants from several disciplines and areas of experience is needed in order to provide a sound enough basis for recruiting and deploying personnel resources adept at the conjecturing business. Beyond arriving at conclusions about the case for systematic use of politico-military inputs in planning processes, a thorough study would have to pay particular attention to the formulation of a strategy or doctrine for politico-military conjecturing, and to the identification and training of appropriate political experts.

A second recommendation is for the Department of Defense to support relevant researchers at the universities and other research organizations. Of particular interest in this respect would be studies on indicators of change involving intangible phenomena. But equal

priority should probably be given to basic researchers in the social sciences, for the more progress is made in understanding processes of change and development in national systems and in the international systems, the more sophisticated conjectures about the future world environment will become. There is, in fact, increasing interest outside the government in tackling various aspects of conjecture about the future. The work of the Commission on the year 2000, sponsored by the American Academy of Arts and Sciences, is a salient example.*

The overarching plea of this paper concerns the use of political expertise, within a restructured administrative framework, in order to provide conjectures on the future politico-military environment which must be taken into account in military planning, and particularly in much of R & D planning. For the reasons spelled out, it will not be easy to adopt and implement this reform. On the other hand, it can hardly be regarded as a strange or disturbing proposal, for the production and employment of these political inputs on a professional and systematic basis is clearly in line with the great revolution that has taken place in military planning over the past half dozen years. This administrative revolution - which is frequently summed up under the symbol PPBS (i. e., the

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*Perhaps the most interesting documents produced thus far are the Working Papers prepared by the Hudson Institute under the direction of Herman Kahn and Anthony J. Wiener.

Planning-Programming-Budgeting-System in the Department of the Defense) marks a giant step toward making military planning a modern rational procedure provided proper standards are observed. Systems analysis is at the core of the new approach. And surely systems analysis calls for the proper consideration of all parts of a problem, political as well as military, technological and economic. The incorporation of political expertise, as defined, in this process of analysis is simply indispensable to good decision-making.

UNCLASSIFIED
Security Classification

DOCUMENT CONTROL DATA - R&D

(Security classification of title, body of abstract and indexing annotation must be entered when the overall report is classified)

1. ORIGINATING ACTIVITY *(Corporate author)* MATHEMATICA One Palmer Square Princeton, New Jersey 08540	2a. REPORT SECURITY CLASSIFICATION NONE 2b. GROUP
3. REPORT TITLE POLITICAL CONJECTURE IN MILITARY PLANNING	
4. DESCRIPTIVE NOTES *(Type of report and inclusive dates)* Final Report	
5. AUTHOR(S) *(Last name, first name, initial)* Knorr, Klaus Morgenstern, Oskar	

6. REPORT DATE	7a. TOTAL NO. OF PAGES	7b. NO. OF REFS
March 1968	i + 93	13

8a. CONTRACT OR GRANT NO. N00014-67-C-0516 new b. PROJECT NO. c. NR 047-067/1-18-67 (Code 436) d. NR 274-086X/1-18-76(Code 462)	9a. ORIGINATOR'S REPORT NUMBER(S) F-6203 9b. OTHER REPORT NO(S) *(Any other numbers that may be assigned this report)*

10. AVAILABILITY/LIMITATION NOTICES
This document is subject to special export controls and each transmittal to foreign governments or foreign nationals may be made only with prior approval of the Office of Naval Research (Code 436)

11. SUPPLEMENTARY NOTES	12. SPONSORING MILITARY ACTIVITY Office of Naval Research

13. ABSTRACT

DD FORM 1 JAN 64 1473

UNCLASSIFIED
Security Classification