THE AMERICAN SCIENTIFIC STRATEGISTS

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October 1964

P-2979

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Publications on military strategy have always in the past been rather rare. Most of the few people who wrote them were career military officers. Perhaps that is one reason why the publications were so few. The specific qualifications and virtues required of a military officer have always left little room for what is essentially a scholarly, analytical, and preferably also a literary activity.

Within the last dozen years the situation has been vastly changed, at least within the United States. Articles and books on strategy or strategic problems have become relatively abundant, not as compared with other

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A shorter version of this paper was published as a chapter in Scientists and National Policy-Making, edited by Robert Gilpin and Christopher Wright, Columbia University Press, 1964. The present expanded version has been prepared as the introductory chapter in a volume of selected essays on strategy by various American writers, collected and edited by the author for the French publishing house, EDITIONS STOCK. The volume, in French translation, will be published in Paris early in 1965.

intellectual fields but certainly as compared with former times. Moreover, the writers of these publications are almost always civilians, and of a particular type. They are highly trained in the formal academic sense, and their training is basically scientific. Especially since the coming into office in January, 1961 of the Kennedy-Johnson Administration, they have risen spectacularly in importance.

These people, though usually with little military experience themselves or occasionally none (some are too young to have served in World War II) must nevertheless be regarded as fully professional in the field of military strategy. The skills in which they are so highly trained are relevant skills, and their full-time labors over a period of years have been devoted to strategy, to which they seem to have made a wholehearted and permanent commitment. How else does one describe a professional? This is not to discount the importance of appropriate military experience, of which we shall say more later. But we should notice here that the special abilities of these "scientific strategists" have been recognized and fully used by the military services, often even with enthusiasm, and the relations between these civilians and the military officers whom they have served is on the whole quite close. Although their numbers are still small compared to the numbers in the ranks of other scientific disciplines, they have been collectively of enormous public influence. It is no exaggeration to say that all the distinctively modern concepts of military strategy, most of which have been embraced by the military services themselves, have evolved out of their ranks.

Contrast with the Past

Despite the still relatively small number of these specialists, the contrast between the situation today and that existing before 1950, or at least before World War II, is spectacular. Theoretical strategy, like ancient Gaul, was divided before World War II into three parts. In the field of naval strategy the figure of the American Rear-Admiral Alfred Thayer Mahan, who died in 1914, was still supreme. Mahan, though a career naval officer, became in later life essentially an historian, and his strategic ideas represented mostly the rediscovery of principles that had held sway in days of sail. His several volumes of naval history, beginning with his famous The Influence of Sea Power upon History, 1660-1783, had an unbelievable impact on their times. Another distinguished though less-known figure was his civilian British contemporary, Sir Julian Corbett, also a naval historian. In France the outstanding figure in naval strategy was Admiral Castex, who wrote mostly of the submarine experience of World War I. The very few books on naval strategy published after Mahan, Corbett, and Castex were mostly efforts to bring their work up to date.[1]

In the new field of air strategy there was only one noteworthy writer, the Italian General Giulio Douhet, whose slender body of writings appeared in the decade following

[1] So I regard my own book on naval strategy, which originally appeared in 1941 and which was published in a French edition under the title La Strategie Navale, et Son Application dans la Guerre 1939-1945, Paris (Payot), 1947.

World War I. The American Brigadier General "Billy" Mitchell was an air propagandist, but not in any true sense a writer on air strategy. Whether or not the British and American air forces that were later to carry out strategic bombing owed much directly to Douhet's views (the British have tended to deny it) there is no doubt of the large measure of identity between the views of those services and Douhet. The same is true of the German Luftwaffe of World War II, some of whose leaders subsequently acknowledged their indebtedness to Douhet. In land strategy the situation was more chaotic. There had been no really great writer on strategy since the towering Karl von Clausewitz, who died in 1831, and the very influential though lesser figure of the Swiss mercenary, Antoine Henri Jomini, both career staff officers of the Napoleonic period.

In France an original mind appeared briefly in the slender but scholarly work of Colonel Ardant du Picq, who died of wounds received near Metz in August 1870 while leading his regiment into its initial engagement of the Franco-Prussian War. Du Picq had become entirely absorbed with the psychology of the soldier in combat. His realistic insights were the opposite of romantic, but his work nevertheless had an unfortunate influence in the pre-World War I school of military romantics of which Ferdinand Foch was the intellectual leader. Foch and his followers sloganized quotations from Du Picq and Clausewitz without troubling to understand the subtleties of their thought. For example, Du Picq's remark: "He will win who has the resolution to advance" reflected his convictions concerning the omnipresent factor of fear in battle. Foch quoted it as a clarion call to the attack. However great were to be

his abilities as a commander, Foch in his pre-1914 role as professor at the École Supérieure de la Guerre and as a writer on strategy was mostly a propagandist and a vulgarizer of his great predecessors.

The method of these men was mainly the scrutiny of military history to see what abiding lessons could be derived from the experience of the past. Some, like Du Picq and Mahan, had been careful and mostly objective historians; others, like Foch, were not averse to distorting history, with which they had little enough familiarity, to serve their pre-existing convictions. We should notice also that none of these figures, including the most recent, had been at all interested in applying quantitative measures to their data. Douhet's neglect to apply even elementary arithmetical calculations to his concepts resulted in such gross exaggerations that we must account him a failure in his efforts to predict the character of World War II. He was indeed the prophet of strategic bombing, but he badly over-estimated the successes it would have in that conflict. Nor could Foch and his school bring any consideration of numbers to bear on their conceptions of what modern firepower would accomplish on the battlefield. If they had done so they might have had a better appreciation of the relationship between enemy machine guns and one's own casualties. Perhaps the modern school of strategists contrasts most sharply with the old in precisely this difference of attitude about numbers.

We should notice besides that even if we were to include the names of all the lesser figures whose writings on strategy make them worth mentioning at all, the total number in this field in the hundred years preceding 1950

would still be remarkably small. If we go back to the pre-Napoleonic centuries, beginning with the Renaissance, the landscape is even more barren. We are, of course, not speaking of writers of military history, who have always been much more numerous, but rather of those whose primary interest was to cull from allegedly good military history lessons of presumably enduring value. This, the theoretical study of military strategy, has been until now the most sparsely-populated of intellectual pursuits, despite the periodic recurrence of important wars.

The important military decisions were in the past the work of men who, though professional soldiers, were rarely specialists in strategy. The generals or admirals in top command were usually older men who had spent their lives in those varied pursuits that we call military, but much of it had to do with drill and very little of it with the conduct of war. The great Maréchal Maurice de Saxe, the foreigner who became the first-ranking soldier of France under Louis XV, observed in his Rêveries that most commanding generals displayed on the battlefield the utmost confusion, and he asked himself: "How does this happen? It is because," he answers, "very few men occupy themselves with the higher problems of war. They pass their lives drilling troops and believe that this is the only branch of the military art. When they arrive at the command of armies they are totally ignorant, and, in default of knowing what should be done, they do what they know." These words were written over 200 years ago, but for how long have they been obsolete?

De Saxe, incidentally, had along with his great talent the indispensable advantage also of high birth, though

illegitimate. That was an absolute requirement in the eighteenth century, and was still important in most of the armies of Europe down to World War I. The greatest single advantage which the French Revolution brought to the armies of France was that it permitted talent rather than birth to be the basis for selection to high rank. With time it was proved that holding in check the requirement for high birth did not guarantee the ascendancy of talent, but it was nevertheless a giant step in the right direction.

However, one must ask: What kind of talent was desired? De Saxe had called the higher conduct of war even for his own relatively simple times an "intellectual" art, but the talent that had always been -- and is still today -- the one most sought after for high rank was talent for leadership rather than for strategic insight. And surely no one can quarrel with the justification for this requirement. Nevertheless, strategic decisions had to be made, by leaders chosen for other skills. These leaders found it congenial to depend on their own personal experience, vivid but necessarily limited, which together with various simple axioms that were revered as "principles" and that were transferred from teacher to disciple through constant repetition provided them with the intuition or judgment by which all major military questions were answered. In each of the major military services of the world there were also important national traditions that helped guide the decisions.

Naturally, not all services were alike; the German Imperial Army, especially under the thirty years reign of the extraordinary elder von Moltke and later the comparably

able von Schlieffen, placed an important emphasis on precise mobilization and deployment planning. That was enough almost by itself to account for the spectacular German victory in the Franco-Prussian War of 1870-71. But in a service where aristocratic connections were so important, the number of serious, dedicated persons ruminating on strategic problems could never be considerable. Perhaps the need for them was easily saturated. As a German corps commander once put it to an over-enthusiastic young staff officer: "His Majesty keeps only one strategist [Schlieffen], and neither you nor I is that man."

At any rate, careful military analysis after 1871 remained tied to "war plans" that were essentially plans for mobilization and for initial deployment or attack. What to do if enemy initiative should spoil one's plan was hardly ever considered. And so far as new weapons of war were concerned, the comprehension of their tactical effects was left entirely to intuition constrained by a professional bias for discounting the results of change. One of the favored maxims of the hundred years before World War II was derived from Jomini: "Methods change, but principles are unchanging." To see change going on was easy; to absorb its often terrible implications seemed always to be extremely difficult.

In so far as the system worked prior to World War II it did so because (a) it was fairly universal, so that each side, despite the exceptions already noted, tended to work under more or less comparable disadvantages, and (b) changes in military technology, which began to speed up about the middle of the nineteenth century, were confined to instruments having only tactical significance. For

example the naval gun, though it underwent drastic development in the thirty or forty years following 1850, remained from any strategic point of view essentially the same instrument; it was simply a means by which one ship could deliver blows to another. It was only with the submarine, the bomber airplane, and later and much more importantly the atomic bomb that instruments came forward that had profound strategic as well as tactical significance.

Nevertheless, by any pragmatic standard we have to concede that the system worked very badly for those whom it was supposed to serve. The tactical and strategic "lessons" presented by the experience of successive wars often had to be learned over and over again, always at great cost and frequently at the risk of defeat. For example, cavalry charges against intact lines of infantry proved useless at Waterloo, and both sides in the American Civil War learned that the proper way to use cavalry was to exploit the horse for mobility but to do the major fighting dismounted. However, this experience had no effect on the French tactics in the Franco-Prussian War. The French repeatedly resorted to cavalry charges against strong infantry, almost invariably with complete futility and considerable losses. In World War I cavalry were not sent charging against intact lines, but up to 1917 large cavalry detachments armed primarily with lances were kept behind the lines in each Allied offensive, ready to dash forward to exploit any "breakthrough" -- which, mercifully for the cavalry if not for the infantry, never took place. When in 1916 Premier Lloyd George remonstrated with the British supreme commander in France, Sir Douglas Haig,

for keeping so much manpower and horses unused in the form of cavalry, Joffre, who was standing by, supported his British colleague, saying that he too planned to use cavalry in the forthcoming battle of the Somme.

Comparable examples could be given over and over again. The most horrendous failure was that which basically characterized World War I, where the simple lesson provided in the American Civil War and reconfirmed in the Franco-Prussian and Russo-Japanese Wars -- that direct frontal attacks on entrenched infantry armed with modern hand rifles were bound to be extremely costly and usually fruitless unless extremely well planned -- was not taken seriously even against strong forces armed with machine guns. Even in late 1915 Haig delivered himself of the opinion that "the machine gun is a much overrated weapon," and that "two per battalion is more than sufficient." By that time the British and French armies had already suffered from that one weapon untold casualties and the repulse of every offensive effort they had attempted -- and worse was to come. It was the German machine gun more than any other weapon that was to cause the complete failure of the horribly bloody Somme and Nivelle offensives.

Why had not the most obvious lessons of combat experience been absorbed by commanders who were to send great new armies into battle? Because for the most part experience not personal to him was not communicated to the leader of new forces, who was likely to pride himself more on being a man of action than on being a student of history, even of military history. What was worse, in World War I the unprecedented separation of high command from front lines made it possible for the commander to be

isolated even from his own experience. When on the first day of the Somme offensive one British division was thrown back to its starting point after terrible losses, Haig wrote in his diary that it had probably not even left its trenches. This kind of misevaluation of events at the front was the rule rather than the exception.

One must remember too -- and the military are not the only ones of whom this is true -- that people wedded to dogmas will often continue to cherish them undiminished after direct personal experiences which should prove them wrong. It is so often possible to explain awkward events away as being due to special circumstances not likely to be repeated. In a profession where rank means so much, and where promotion is so much at the mercy of one's seniors, who will question or challenge the dogmas of his elders? Actually, this latter factor is not so important as one might think, because the young officers, selected for their willingness to absorb the military virtues, have always been eager to believe their elders and to acquire the same "doctrine" -- to use the word which is the favored military word for "conceptions" or "ideas."

Changes Wrought by World War II

World War II had altogether a different character from World War I, with, in the main, a different and more flexible kind of military leader. But among the more conspicuous factors of change, two deserve special mention. The first was the introduction at the very end of the conflict of the atomic bomb, which not only presented in itself a basic strategic change of totally unprecedented importance but also signalled the beginning of an era of extremely rapid and also

extremely costly technological development. The second was the heavy reliance upon scientists to assist not only top military commanders but even heads of government to reach critical tactical and strategic decisions. The use of such people was particularly prominent in the new field of strategic bombing, where previous war experience was completely lacking and where a flow of new inventions was drastically affecting the capabilities of the bombing forces. The outstanding application of analytical skills was in target selection, where economists especially proved invaluable; but even in such tactical matters as whether or not bombing forces should use "chaff" or "window" to confuse enemy radar, the matter was, at least among the British, largely debated by the scientists and decided by the prime minister, with the military acting mostly as interested observers of the debate.

The atomic bomb differed from all previous military inventions from remotest antiquity in that its effects clearly went far beyond the tactical. The bomber airplane had already taken war beyond the battlefield; but nuclear weapons guaranteed that those operations, which in World War II we called strategic bombing, would be all-important in any war in which they should again occur. The effectiveness of strategic bombing in World War II had been disputed by many uncommitted observers, but with nuclear weapons there could be no reasonable doubt of it. On the contrary, one began to hear expressions of the fear that even in their initial form those weapons were far too effective for the user's good, if reciprocal use also had to be considered.

It is one of the more noteworthy aspects of the coming of nuclear weapons that almost everyone immediately understood that something tremendously important had happened. By contrast, those who lived through the first military use of gunpowder some time in the first quarter of the fourteenth century seemed to have been quite unexcited about it, and failed to record the occasion. Actually, they were right to be unexcited, because it took another four centuries before field artillery became really important in battle, which is to say about the first quarter of the eighteenth century, and still another century after that before we entered the age of modern firearms -- with breech loading, rifling, and explosive projectiles. By contrast, the development of nuclear weapons and the vehicles to carry them has been terribly swift. Today, not yet two decades after the first atomic bomb, we are already living in an age of vast and numerous thermonuclear weapons with rocket-type delivery vehicles of the most fantastic accuracy.

The tremendous novelty of the new military conditions was naturally bound to throw some doubt on the pertinence of the professional soldier's military experience. Also, the destructiveness of nuclear weapons meant that total or "general" war (which for the first decade of the atomic age was practically the only kind one thought about) would have to be fought with forces in being at the outset -- a change that effectively pushed all the major strategic decisions, including choice of weapon systems, of deployments, and of targets, into the pre-war period, when the military man has not yet "taken over" from the civilian.

Furthermore, the rapidity of change that resulted from the development of a wide range of nuclear weapons, combined

with the development of fabulous new vehicles for carrying them, diminished greatly the utility of simple professional judgment in selecting the appropriate systems, especially in view of the tremendous sums of money involved in the choices. The military man had all he could do to keep abreast of current technological developments pertinent to his work. But decisions had to be made currently about weapon systems for which "lead times" were very long, that is, which would actually not be available until six or eight years had passed, bringing with them a totally new technological environment. It was not altogether a new problem for the military to have to think ahead technologically, but certainly the dimensions and complexity of the problem were totally new. One had to rely on technicians who could be counted on to maintain an alert understanding of the evolving "state of the art" in any one technological field. There was a clear need also for intensive methods of analysis in order to appraise one weapon system against a competing one, to determine which promised the most return or "payoff" for the tremendous investments of money involved. The military might not wish to accept in any one instance the specific conclusions of such an analysis, but they knew they could not do without the exploratory work and without the people who possessed the special skills for carrying it out.

The Founding of the Research Institutions on Military Affairs

Deriving not so much from these realizations among the military, which came somewhat later, but rather from the new prestige of scientists as a result of the nuclear developments, and also from certain sobering experiences in World

War II, a development took place in the United States that was to have consequences far beyond those expected by the sponsors. This was the founding of a number of institutions closely associated with but outside the military services, where people with various kinds of scientific training and access to classified information would devote themselves on a full-time basis to the consideration of military problems.

The prototype of these organizations and the best known among them has been The RAND Corporation, originally set up by a contract between the United States Air Force and the Douglas Aircraft Corporation (the word RAND means simply Research and Development). RAND, which has long since severed its connection with Douglas and which has grown considerably during the fifteen years of its existence, possessed at the beginning of 1964 a component of professional staff members (about 470, besides a larger number of administrative and secretarial personnel) equivalent to the entire faculty of a large university. It has indeed played a role very much like certain great universities and research centers in the past, where in each case some dedicated scholars and their students opened up whole new fields of knowledge. Other institutions have done the same on a smaller scale.[2]

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[2]Prominent among those other autonomous institutions in America where strategic analysis is carried on today are the Army's Research Analysis Corporation, the Navy's Operations Evaluation Group, the Joint Chiefs' Institute of Defense Analyses, and the unattached Stanford Research Institute. Within the Defense Department and attached directly to the Director of Defense Research and Engineering and the Joint Chiefs of Staff is the Weapons System Evaluation Group. In the university world the few relevant

The movement has thus far been almost entirely an American one, there being essentially no institution abroad comparable to those we have been describing. The Institute for Strategic Studies, with an international membership and headquarters in London, provides a valuable forum for public discussions of strategic questions and has sponsored certain very useful publications, but it is in no wise a research institution after the RAND pattern. In France the recently founded Institut Francais d'Études Stratégiques, directed by Général André Beaufre, represents an effort to undertake what organizations like RAND have done in the United States, but it has thus far been organized on a very small scale. One must mention also the Group d'Études Mathématiques des Problémes Politiques et Stratégiques, which is a university research group of some six persons, under the direction of Professor G. Th. Guilbaud, attached to l'École Pratique des Hautes Études.

The use of scientists for assistance in making tactical decisions goes back at least to World War I, where such distinguished British scientists as Ernest Rutherford were employed to think up new ways of dealing with the German U-boats, and where the British mathematician F. W. Lanchester laid the groundwork for what later became known as operations analysis. The latter's book entitled Aircraft in Warfare, published in 1916, is a landmark in the field.

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centers tend to be very small but also to contain some distinguished names; one must mention especially the Harvard University Center for International Affairs, the Columbia University Institute of War and Peace Studies, the Princeton University Center of International Studies, the Georgetown University Center for Strategic Studies, and the Washington Center of Foreign Policy Research of the Johns Hopkins University.

Employment of scientists for military work was carried very much farther in World War II, especially, as we have seen, in the novel field of strategic bombing. No doubt also several unhappy experiences of the air war helped convince the then Chief of Staff of the U.S. Army Air Forces, General H. H. Arnold, that scientists should be retained on a continuing basis in peacetime to assist the Air Force with its many tactical and strategic problems. For one thing, the U.S. Strategic Bombing Survey, carried out on the heels of the advancing or occupying armies both in Germany and later Japan, had disclosed that the bombing attacks might have been much more effective if certain modest and easily achieved improvements had been made in weapons and techniques. Perhaps, with the right kind of research, these could have been anticipated. Even more immediately disturbing, the allied air forces had been behind in several important technological races. The Germans had developed the self-propelled missiles -- first the HS-293 glide bomb used in the Mediterranean, then the V-1, "air-breathing" missile (to use present terminology rather than that of World War II), and finally the rocket-type ballistic missile V-2. They had also developed the ME 262, a jet bomber of remarkable performance, and the rocket-powered fighter ME 163. The German wind-tunnels were experimenting with swept-wing aircraft models that anticipated American F-86's and the Soviet MIGs. German jet engines alone, if they had been applied to fighters rather than, at Hitler's insistence, to bombers, might have been enough to stop entirely the Anglo-American air offensive (the ME 163's had too brief a cruising endurance to do the job, even if they had existed in larger numbers).

The Air Force interest in helping to found The RAND Corporation was no doubt primarily for the sake of technological assistance. But those who were given the task of organizing RAND included in it almost from the outset divisions of mathematics, economics, and social science, as well as the more-to-be-expected engineering and physics divisions. The inclusion of an economics division turned out to be of especially critical importance, and the inclusion of a social science division made it possible to undertake, among other things, a thoroughgoing study of the Communist enemy or enemies of a kind that had too long been lacking from the strategic studies.

The vital influence of the institutional framework in guiding and developing the interests as well as the skills of the participants is demonstrated by the fact that most past and present members of RAND who are today publicly distinguished in the field, for example men like Malcolm Hoag, Herman Kahn, and Albert Wohlstetter, had done no work in military problems and had shown little interest in the subject at the time they joined RAND, which they were invited to do simply because of their general technical competence. Others, like William Kaufmann and Thomas Schelling, had shown interest and some accomplishment in the field before entering or associating themselves with that organization, but their now-terminated full-time association with RAND clearly helped to focus their interests, apparently permanently, on strategic problems. However, the present writer happens to be practically alone (partly as a result merely of seniority in age over all of the aforementioned except Wohlstetter) in having published a substantial amount of work on strategy, including several books,

before joining The RAND Corporation.

Mutually useful contacts are also maintained between the several strategic research institutions, and also between them and those people who have continued to contribute significantly to the field while remaining in or having returned to the university world. In that connection we must recognize that the American university system, which has often been criticized as having too much curricular flexibility, does make a place for scholars interested in modern strategy which the more conservative European universities, especially in the United Kingdom, may be reluctant to do. One is of course aware of Professor Raymond Aron, who teaches a course in nuclear strategy at the University of Paris, but Professor Aron would in any country be exceptional for his superabundant talents.

At RAND an institutional learning process characterized the first few years of endeavor. Early work depended heavily on assumptions or "hypotheses" offered by the Air Force concerning strategic needs to be met by the weapon systems to be examined. It was inevitable, however, that with time these assumptions and hypotheses would be increasingly challenged. One could have expected that in putting together talented and highly-trained persons to work full-time on military problems, some of them would surely become interested in re-examining the basic assumptions of the field, which is to say interested in the comprehensive problems of strategy rather than in sharply delimited tactical or technological problems. That, in any case, is what happened, though it must be emphasized that the number of persons who have taken that step is very small compared to the whole number of persons who have been in RAND.

Conceptual Parallels between Strategy and Economics

In an article published as long ago as 1949, the present writer called attention to the remarkable similarities in both method and objectives between the science of economics and what could become a science of strategy.[3] Both fields are concerned essentially with the optimum utilization of scarce resources for the efficient achievement of certain objectives of society. Most of the concepts basic to economics, the writer argued, could be transposed to the field of strategy with considerable resulting clarification -- for example, the concepts of "marginal utility" (or "diminishing returns") and of "opportunity cost" (i.e., whenever we buy something with our limited resources, we are paying for it the opportunity to buy something else of comparable importance and value for the same money -- a simple idea but a frequently overlooked one). The arguments of that article have been confirmed by events to a degree which the author never expected.

A majority of those who have made their mark today as theorists in strategy have been trained as economists, or at least have more than a bowing acquaintance with the concepts and principles in that field. Herman Kahn, for example, whose formal training was predominantly in physics, has also trained himself quite seriously as an economist, and was once even offered a professorial post in economics in a leading American university. Of the persons already mentioned, Hoag, Schelling, and Wohlstetter were intensively trained as economists, as also were Alain Enthoven,

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[3] Bernard Brodie, Strategy as a Science," World Politics, Vol. I, No. 4 (July 1949), pp. 467-488.

Charles Hitch, and Henry Rowen, who were formerly of RAND but now hold important posts in the Department of Defense. Other outstanding economists in the field are Professors Klaus Knorr and Oskar Morgenstern, both of Princeton University. Professor Knorr, however, has also had strong training in political science. A few other economists should be added who have made important contributions to strategic thinking but who are well known only to the experts and not to the general public, like Messrs. Daniel Ellsberg, Fred Hoffman, and Andrew Marshall. The present writer was trained primarily as a political scientist, but also with a leaning towards economics. Most of the above hold the degree of Doctor of Philosophy, and the few others have equivalent graduate training.

It is interesting that among those who have won public attention in the study of contemporary strategic problems almost none come from the physical sciences, Herman Kahn being in that regard quite exceptional. It is also interesting that virtually none come from the wide ranks of the military historians. Physical scientists seem to prefer to remain within their highly specialized fields; and as for historians, whatever the reasons for their hesitation in entering the field of strategic analysis, it is all the more remarkable in that until very recently the study of strategy was based overwhelmingly on the pursuit of lessons derivable from military history. Earlier generations of strategic writers used practically no tools other than history; few of the present writers in the field ever trouble to read history.

The modern training of economists, with its heavy emphasis on mathematics and other tools of quantitative

analysis, tends to fit one peculiarly well for grappling with certain characteristic problems of strategy, especially in what we call "cost-effectiveness" analysis. Besides, economists as a group seem to be rather more at home in the world of technology than do members of other branches of the social sciences. On the other hand, the usual training in economics has its own characteristic limitations, among which are the tendency to make the possessor insensitive to and often intolerant of political considerations which get in the way of his theory and calculations. We have already noted how weak he tends to be in either diplomatic or military history. One is sometimes amazed at how little some of the best-known strategic analysts of our times may know about conflicts no more remote in time than World War I, let alone earlier wars (the same is, however, true of professional military officers). It is not that they have no time for history but rather that the devotees of any highly developed science -- and economics is clearly the most highly developed of the social sciences -- tend to develop a certain disdain and even arrogance concerning other fields. It is a grave intellectual fault, but a very common one.

The importance of being sensitive to the political issues that are omnipresent in strategic questions would be hard to overestimate. Naturally, one accomplishes much by bringing representatives of the several relevant disciplines together to work jointly on the same problems -- along with physicists, engineers, and other technologists. This broad method of approach is one of the distinctive values of institutions like RAND. But the inter-disciplinary exchange of insights is not always easy to accomplish.

A major contribution to modern strategic thinking that emanated from the economists was the idea of using the dollar as a measure of value in comparing the relative performance of competing weapon systems, the dollar cost having the merit of being (a) always common to the systems compared, (b) always basically important, especially as the limiting factor which determines the necessity for choice, and (c) a unitary or single means of measurement. The only known way to examine objectively the claims advanced in favor of each of two or more competing weapon systems is to compare the two in terms of the respective costs to achieve a common objective, such as the destruction of a given number of targets in a given enemy area. The cost is measured not simply by the initial purchase price of the various weapons and vehicles involved but of the whole material and manpower structure that goes into the entire system of utilization, extended over some suitably prolonged period of time to include the important factor of peacetime maintenance. This is what we mean when we speak of comparing weapon systems (including bases, ancillary instruments, etc.) rather than simply the weapons themselves. This method is generally called the "cost-effectiveness" approach to weapons systems analysis.

Many factors which might not at first glance seem to be amenable to dollar-value comparison turn out on closer examination to be so. A simple example is the costing of reliability in a system, where the designer usually has some degree of choice between putting more money into each instrument to increase its reliability or increasing the number of instruments so that the failure of one will be offset by the success of another. This costing of a mission,

combined with application of the concepts of marginal utility and opportunity costs, also permits consideration of various kinds of "tradeoffs": for example, if X dollars are required in order to destroy with high confidence 80 per cent of a given target system, and if to destroy 90 per cent of that system requires 2X dollars, the analyst can offer for consideration the question whether the extra 10 per cent of target destruction is worth the doubling of expenditure. The policy-maker who is informed of this disproportion may want instead to consider putting the extra money into some other form of military enterprise.

However, the appropriate decisions will not usually be obvious or self-evident, which is to say that "good judgment," that is, intuition based on experience and insight, remains of enormous and indeed supreme importance in the decision-making process. That is true partly because some of the most important factors relevant to determining the performance of a system are <u>not</u> finally reducible to dollar comparisons, but more because there is always the larger context -- often with its special political and psychological overtones -- into which the weapon systems must be fitted. In fact, the more strategic or political a problem becomes, as compared with the merely tactical, the more we tend to outrun the limits of usefulness of cost-effectiveness analysis. Nevertheless, it is one thing to recognize the limits of the technique we have described, and quite another to deny or depreciate the great value of this and comparable techniques <u>within</u> the limits of their utility. Judgment is better for utilizing with discrimination the products of technical analysis rather than for setting itself up as an alternative to such analysis.

In view of the ongoing rate of technological change, such prediction can be a fearsome responsibility. For example, what kind of anti-missile missile systems will the Soviets have ten years hence, and how effective will they be? The kind of answer we give has to affect decisions we are making now about our offensive missile development, which may involve also the silos in which these missiles are housed. Even the estimates of cost of the individual factors in a system are themselves predictions which are likely to prove fallible. The sums involved in developing a major weapon system are likely to be enormous and also very difficult to predict accurately in advance. Often systems under development turn out before that development is completed to be obsolete because of new technological advances. Such systems then have to be abandoned and their heavy costs written off. Thus, at every stage decisions are made which are difficult at best and impossible to make wisely without utilizing all the tools that modern concepts of economic and strategic analysis have made available to us. This is a new and now permanent condition, a state of the world undreamed of in 1939.

The Complexities of Strategic Analysis

As we move to a larger context we see that we have hardly begun to measure the complexity of the major issues of strategy. We observe that a nation makes its strategic dispositions not against a more or less predictable state of nature but against an opponent or group of opponents whose present intentions and capabilities have to be analyzed to the best limits of our information and their

future course predicted. These opponents are cunning and have objectives in direct conflict with our own. The problems of decision in such a context are thus akin to "gaming" problems -- though the term is an unfortunate one for this situation. It is obvious, for example, that to make optimum use of a given amount of resources for, say, our own anti-missile defense, it is necessary to consider the kinds and numbers of offensive weapons that the opponent is likely to have at the time when one's defensive system will be ready for operation. Moreover, one has to adjust to a range of possible situations -- the single most likely situation does not necessarily have a high degree of likelihood. And, obviously, one's defensive preparations when detected will affect the offensive designs of the opponent and vice versa.

The example just described approaches what is called a "zero-sum game," which is to say a game in which one side's gain is the other side's loss. But not all strategic problems and probably not even most of the important ones, are of that character. The opponent's interests are not always in complete conflict with one's own.

For example, our applying adequate shelters to the vehicles of our retaliatory striking force is of great advantage to us, but it does not follow that the enemy's doing likewise is of corresponding disadvantage to us. There are some considerations which reconcile us to seeing him improve his defensive strategic posture. It may, for example, make him less "trigger happy" in a crisis, and therefore less unpredictable and exercising less constraint upon our actions. This gain is naturally offset in part, perhaps in large part, by the fact that his striking force

is now more difficult for us to destroy. He is less nervous, which is good, but perhaps more stubborn, which is likely to be bad. But even if we decide that his improvement in his defense posture is in the net disadvantageous to us, to leave our own missiles unprotected is so much more disadvantageous that we would proceed with protecting them even if we knew that our doing so would be directly responsible for his doing likewise (though usually the cause and effect relationships are not as clear-cut as that). On the other hand, one can think of many other possible actions which one is quite willing to forego if it means stimulating the opponent to do likewise.

We have already referred to the difficulties resulting from the fantastic speed with which military technology is presently changing. We have no real reason to foresee a future state of equilibrium even in the factors of most elementary importance, such as the nuclear weapons themselves. The layman feels that after a nuclear bomb has reached a certain level of power, a further increase in its power or other change in its characteristics is likely to be of little importance. In some categories of weapons that view is indeed justified, but if we are seeking to use a weapon as the warhead on a missile, we may be critically concerned with reducing its over-all weight without sacrificing yield. As the opponent installs and augments passive protection around his missiles, we not only have to make our own missiles more accurate but we may also have to fit them with more powerful warheads in order to restore some part of our capability to destroy his weapons. We may also have reasons for desiring to make our warheads "clean" rather than "dirty," that is, with little or no radioactive fallout; or we may

decide for comparable reasons, i.e., the avoidance of fallout, that we have to use air-bursts rather than ground-bursts. To get comparable effects with clean air-burst weapons as we get with dirty ground bursts may require still higher yields. Also as we see the opponent erecting fairly effective anti-missile missile systems, we may have to put counter-measures in our own missiles, such as decoys which are expelled from the missile to surround the warhead and thus confuse the enemy's radar screens. Decoys, like other counter-measures, involve weight, which competes with warhead weight. All these factors, and others as well, stimulate the search for ever more efficient weapons -- and incidentally make the test ban a meaningful retardant.

However, to consider any one category of weapons or instruments inevitably oversimplifies the problem. It is a broad spectrum of weapons in which change is occurring at so alarming a rate -- alarming because of all the elements of uncertainty, with resulting fallibility, that are bound to be introduced. In the nineteen years since the initial introduction of nuclear weapons, developments in their character, variety, and abundance as well as in the vehicles designed to carry them have effected not one but several distinct revolutions in military technology; and these are after all not the only instruments of military technology with which we have to concern ourselves.

As the "scientific strategists" have become more at home in their work, their greater maturity has resulted in important changes in basic approach. Where their tendency used to be to try to find the optimum method of dealing with a single most-expected contingency, the realization

that the enemy might play the game differently from the way in which we think we ought to play it has prompted us instead to seek that mixture of solutions which does rather well over a complex of contingencies. For example, a highly perfected defense against high-altitude bombers will be worth something even if it only drives enemy bombers to lower altitudes in their attack, because they are forced to burn up more fuel at those lower altitudes; but if we remain entirely open against low-level bombers, our overall vulnerability has been scarcely diminished. In other words, our total air defense investment has not paid off. The solution that does rather less well with the high-altitude craft but also has some capability against those entering at low-altitudes is likely to be decidedly preferable within any given budget. What we need are systems that will work well under widely divergent contingencies, including perhaps major catastrophe. We have learned too that in analyses aimed at policy making, it is far more important to consider carefully all the many factors and contingencies which may influence or change our problem than it is to achieve a high degree of mathematical or economic sophistication of the analytic techniques applied to solve it for any one set of assumptions. This is where we use "good judgment" and that disposition with which people are so unequally endowed to see beyond the limits of the assignment in hand.

The Uses and Limitations of Gaming

It is appropriate at this point to say a few more words about the use of gaming techniques in strategic

analysis, because among laymen the importance of this aspect of strategic analysis has been grossly exaggerated. The refinements of game theory as developed mostly by the late mathematician John von Neumann are generally of little importance to the strategist. Actually, many strategic analysts do important and excellent work in their field without having much understanding of game theory. What matters is the spirit of the gaming principle, the constant reminder that in war we shall be dealing with an opponent who will react to our moves and to whom we must react. It is amazing how little this simple conception has characterized war plans in the past. Those plans would often stipulate at the outset that the enemy was cunning, strong, and aggressive, and perhaps also that he would start the war, thus taking the initiative -- and then they would proceed to lay down a pattern of moves that would make sense only if the enemy were dimwitted and passive.

It is perhaps the greatest merit of the war-gaming technique that it overcomes these and like tendencies. Whether the particular game in hand be simple or complex, the important thing is that the enemy is represented by a player who will see to it that the initiative and capabilities accorded him by the explicit assumptions of the game will really mean something in the working out of the play. If he has been accorded enough advantages by the stipulations of the game, the chances are he will even win, which is to say achieve his objectives. That seems simple and natural enough, but it is not in keeping with war-planning traditions. It is really remarkable what immunities the usual war-plans designer has in the past taken for granted as belonging of right to his own forces (remember Plan XVII

with which the French General Staff entered World War I), and how quickly his roseate fantasies have collapsed the moment he has played a simple war game in which the enemy is assumed to have something like equal power and perhaps the initiative to boot. Another benefit of war games is that they force the players to consider the problem beyond the opening moves, which it is otherwise very difficult for a war planner (or any human being) to force himself to do. There are other advantages in war games -- and also some disadvantages, chief among the latter being the tendency of the play, especially in the more complicated games, to induce in the players the illusion that they have really tested their initial hypotheses or presumptions in a conclusive manner.

Thus, the greatest value of the gaming technique is in conditioning the analyst or decision-maker to ask himself spontaneously: "How is the enemy likely to respond if I carry out (or refrain from) this proposed action, and what new problems will that response create for me." If he considers possible enemy responses in pluralistic terms, that is, as involving any of a number of conceivable actions, he is already approaching his problem in a very sophisticated and unusual way. It remains important to distinguish between more probable and less probable enemy reactions, but the essential thing is to avoid the trap of forgetting that the enemy has choices. This example incidentally suggests that war gaming can be so simple as to be approximated even by two players without any equipment at all except their minds -- one is almost tempted to say a single player, but there are reasons why the game principle tends to collapse with only a single

player. Anyway, the idea that gaming is something that must be carried on with elaborate equipment like high-speed computers is a popular fiction, though that is not to deny that some highly specialized forms of gaming do make use of such computers.

The Imperfections of Scientific Strategy

We come now, inevitably to the limitations and imperfections of scientific method in strategic analyses and decision-making, and especially to the imperfections and limitations in the practitioners, whose greatest limitation is that they often fail to use scientific method when they should or when they pretend to. Such lapses are only one respect in which they are human, and thus disposed to sharing the infirmities of humanity. We shall mention others.

For one thing, it is unfortunately true that the most profound issues in strategy, those likely to affect most deeply the fates of nations and even of mankind, are precisely those which do not lend themselves to scientific analysis, usually because they are so laden with value judgments. They therefore tend to escape any kind of disciplined or searching thought altogether. Judgments about the behavior in a future crisis of the enemy, of one's allies, and even of one's own leaders tend to be inflexible and often naive. On these issues official judgments usually reflect simply the traditional thinking of military or civilian bureaucracies, heavily overlaid with national prejudice. Nor are such prejudices confined to stupid or unsophisticated men. They limit the thinking also of the most gifted and the most highly trained.

Those who do this work are naturally beset by all kinds of limitations, including limitations in talent and in available knowledge. The object is usually to predict the future for the sake of appropriate action, but one cannot wait until all the relevant facts are in. Besides, one can make progress only as one cuts off and treats in isolation individual problems involving small portions of the whole universe of data, and every research project is to that extent "out of context." We are also dealing always with large admixtures of pure chance. These are sometimes difficult to take into full account without seeming to make our results look foolish, and that human beings are naturally reluctant to do. The same is true of the large range of variables which deal with enemy intentions and capabilities. Finally, and most important, analysts are, like all other people, immersed in bias, their own above all but also that of their colleagues and their clients. Intellectual independence, like objectivity, is always a relative thing, and a high degree of it is extremely rare even among the most gifted.

Even in the areas in which strictly scientific analysis is appropriate, the complexity of the field, the impossibility of really testing conclusively one's suppositions or deductions, and the fact that one is constantly running up against value judgments or simply against the mysteries of the future mean that temptations always exist both to shortcut some of the analytical difficulties and to pretend to analytic objectivity where it has ceased to operate. Certainly one of the outstanding cases over the past few years has been the great dedication on the part of almost the whole community of American

civilian strategists to the idea of conventional, i.e., non-nuclear, warfare. The bias is manifested in the extraordinary passion with which their special theory is often urged.

Scientific method, in other words, is not what we have too much of but what we too often fail to achieve. The great merit of the scientifically trained analyst is that he tries more persistently than others, and he can perhaps be more easily made aware of his lapses when they occur. When we do succeed in using scientific method for the systematic exploration and comparison of alternative courses of action, we are doing simply the best we can do to bring some order into the vast, chaotic mass of technological, economic, and political facts and predictions which form the universe of data in which reasonable military decisions have to be made.

Relations Between the Analysts and their Military Clients

The relations between the civilian scientific strategists and their military clients have in the main been thoroughly good and mutually profitable. Where it has not been so, it is usually because we are dealing with character weaknesses on one or both sides. Most military officers know their own worth, and their special qualifications for indispensable service, and are content to recognize the limits of their training and experience. Some are not. The same is true on the side of the analysts. It has always been true that creative abilities are not necessarily combined in the same person with such character endowments as tact and modesty. As was to be expected, the growth in reputation and influence of the

civilian strategists, and also the development by them of ideas that were bound to be unattractive to certain of the services, were bound to result in some heartburning and even public complaint on the part of some military people. A few of them have voiced concern over the "usurpation" by the civilians of what was previously their own area of intellectual authority. Often this complaint has been expressed in rather undiscriminating condemnation of the new techniques. As we have hinted above, not all that complaint is unjustified. Some of it is even discriminating and shrewd.

Oddly enough, the most contemptuous judgments often come from European military officers, with whom it is a matter of cherished tradition to resist the intervention of the civilian, especially the politician, in military affairs. Writing in a professional military journal recently, a distinguished French officer, who happens to be much more tolerant than most, repeated the age-old military conviction that the intrusion of the politician into military affairs is usually bad and sometimes disastrous. In casting about for an example, he mentioned Abraham Lincoln's interference with the northern generals in the American Civil War. In doing so he only betrayed his ignorance of that war and of the quality as well as of the modesty of Lincoln's judgments. One hardly has to wonder why the author did not choose examples from wars more familiar to him, like the two world wars. The record on the military side is clearly not one to justify disdain of the civilian "interloper."

One painfully remembers also the episode, described to us, I believe, in one of M. Raymond Aron's books, in

which Pierre Laval was induced to make the most unfortunate comment of his career. When he showed to Marshal Pétain a speech he had prepared in which he made the observation that he "expected" a German victory over Great Britain, the Marshal told him that as a civilian he was not competent to express a military judgment. Laval then changed the word "expected" to "prefers," which the Marshal approved as a view which a civilian was entitled to have. Thus Laval went on record before the world as "preferring a German victory." One can hardly imagine an American military officer even of the Marshal's generation being quite so rigid in his distinctions.

The coming to office with the Kennedy Administration of the extraordinarily able, vigorous, and self-confident Mr. Robert S. McNamara as Secretary of Defense has had extraordinary consequences with respect to the relationships we have been discussing. Some of these consequences are likely to be enduring, and others will no doubt be but temporary.

The team that entered office with Mr. McNamara included, in positions of relatively high responsibility, a small group of persons who had made their mark as strategic analysts of the kind we have been describing. This nucleus of experts on the staff were able also to enlist the services, on a part-time consultative basis, of others who had had the same kind of training and experience. With such able and knowledgeable people on his staff or otherwise available to him, and speaking a kind of "cost effectiveness" language which as a former industrialist he had come to understand and appreciate, Mr. McNamara quickly developed so marked a confidence in

his own judgment that he was before long making decisions that ran counter to the unanimous advice of committees of his military advisors. He did so in the now famous TFX airplane controversy, and also in refusing to countenance the construction of more new aircraft carriers (besides the already-built Enterprise) with nuclear propulsion, insisting instead on the cheaper conventional propulsion. Even more important was his insistence on planning to fight even large-scale local wars with conventional rather than with tactical nuclear weapons.

It should be noted that no previous Secretary of Defense had ever made a decision of mostly military significance against a unanimous judgment of the Joint Chiefs. Some of them had reluctantly intervened when the Joint Chiefs were hopelessly split, but only after urging them to come to a meeting of minds if possible. What was new in Mr. McNamara's method was not only that he was willing to place his own judgment against that of his military advisors, but also that the kind of analytical investigations which the separate services had previously conducted on their own initiative and responsibility, which enabled them to reserve to themselves authority to accept or reject the advice they received from the institutions like RAND which they supported, were now being reported directly to the Secretary of Defense. In short, the military to a large extent lost control of the kind of analytical operations which they had themselves sponsored. It probably did not help matters that some of the civilians involved were very young and not inclined to be especially tactful or deferential to the military. The remarkable thing is that relations did not become more critical.

Certainly much of this situation is bound to be impermanent. There seems to be little chance for institutionalizing the pervasive and searching kind of civilian control of the whole gamut of important military decisions that a very special kind of Secretary aided by an unusual array of assistants has eagerly undertaken. Most future Secretaries will not want to shoulder that kind of responsibility. We can thus expect in the future to see the military chieftains or the Joint Chiefs regain much of their formerly unchallenged responsibility and authority for major military decisions and for providing their political superiors with appropriate advice.

On the other hand, a number of important trends already existing before the present Administration and brought to fuller fruition under Secretary McNamara represent long-term and largely irreversible changes. Among them no doubt is the new kind of budgeting according to strategic functions rather than to services, and also the tendency for the making of decisions on major weapons systems to be brought into the direct purview of the office of the Secretary of Defense. Moreover, it is probably unlikely that military leaders will quickly forget the salutary shock of being required to present closely reasoned justifications for their recommendations, rather than merely their sovereign judgment. And since their problems are not getting any simpler, we may expect that they will continue to rely heavily on the kind of intricate systems analysis to which they have by now become entirely accustomed.

II

Inasmuch as all the essays presented in this book have been selected from relatively recent publications -- in order to give the reader a sense for what is going on currently in American strategic thinking -- it might be well to outline the development of strategic thought to its present state, beginning with the dawn of the nuclear era less than two decades ago. That era has seen thus far not only an intensely rapid technological development but also a remarkable progression of strategic ideas. Some ideas that are conspicuously ascendant in the United States today were hardly thought of only a few years ago. One therefore gets a better understanding of where we are now from a review of where we have recently been.

The rapidity of the change incidentally helps explain the polemic character of certain of the contributions published here and elsewhere. The author of an essay on strategy has often felt it necessary not merely to state his point but to become its advocate and to argue it against what he knew to be strong existing opposition. Opposition to a new idea could always be counted on in the form of conservatism, especially where the new idea might be injurious to the self-esteem of a particular service. Sometimes there has also been a clash of ideas where neither side could be accused of being old-fashioned. The people who have had the original ideas have always been few in any field, and fewer in this field than in most others. Thus, there has been an added note of personal identification of persons with their own ideas, so that the clash of schools has frequently been only one short step removed from the clash of personalities.

As we have already noted, the coming of the first atomic bomb was not greeted with indifference anywhere, and certainly not in the United States. Intellectuals in many fields and people in all walks of life knew that this signified a deeply important change in the long history of the world. The Truman-Attlee-King declaration of November 15, 1945, and the Baruch Proposals before the United Nations in the following year, reflected the anxiety provoked by this new device among the political leaders of the countries which had produced the bomb, especially the United States. The thought that it represented a fabulous and mostly American scientific and engineering accomplishment and that the United States possessed for the time being a monopoly on it seemed to cause no exhilaration among Americans.

Subsequent events did not undermine the early consensus on the importance of the new weapon, nor did they qualify the misgivings. On the contrary the first decade of the atomic age saw the collapse of the American monopoly, of the myth of inevitable scarcity, and of reasonable hopes for international atomic disarmament. It also saw the development of the thermonuclear weapon in both major camps. If at the end of that first decade one looked back at the opinions expressed so voluminously at the beginning of it, one found almost none that had proved too extravagant. Only the conservative guesses had proved to be hopelessly wrong.

The prevailing strategic concept of that first decade had been that in the event of war with the Soviet Union, the only available and in fact the only thinkable strategy for the United States was to attack immediately with its

nuclear weapons, carried in long-range bombers, the war-related industrial resources of the enemy, mostly gathered in the latter's cities. It would thus be a repetition of the strategic bombing of World War II, except what with the immeasurably more efficient nuclear bombs the campaign would be much more quickly accomplished with far more decisive effect. This view was of course especially favored by the Air Force. It was indeed occasionally challenged by members of the Army and the Navy, but it was unquestionably the dominant idea so far as the national strategy was concerned -- a fact that was reflected in the highly favored position of the Air Force in the national defense budget. The Army and Navy contented themselves with a conception of "broken-backed war," to use a phrase from the British Defence White Paper of 1954, which, however, accurately reflected American ideas. The phrase reflected simply the thought that after both sides had exhausted their stockpiles of nuclear bombs on each other, thus admittedly "breaking each other's backs," the armies and navies would carry on in accustomed fashion and one side or the other would force home a successful conclusion. How nations devastated by huge nuclear bombardments could nevertheless maintain large armies and navies in the field and at sea was a pertinent question that sometimes was asked but never answered.

With the formation of NATO in 1949, the umbrella of American strategic nuclear power was officially extended to cover also America's European allies. But because it was felt that the Red Army "could not be cornered" -- to quote an apt expression of Mr. Walter Lippmann -- that is, could not be kept within its peacetime confines even if

the source of its power in Soviet Russia was undergoing destruction, the general consensus was that an army was also necessary in Europe to stop the Russians from marching westward even while the bombs were raining on their homeland. Thus, the one-sided strategic air war would be attended by combat on the ground that was more or less conventional, except that a small proportion of the nuclear weapons in the stockpile could be used for "tactical targets." The latter were mostly fixed communications targets like railroad marshalling yards. These bombs would at least "retard" the Red Army, even though they would not be sufficient to destroy it. We should notice, however, that the conventional nature of the fighting envisaged for the ground was due simply and solely to the shortage of bombs, not to any conviction that it was better that way.

These were the ideas that held sway during the period of American monopoly, which incidentally ended at about the time of the outbreak of the Korean War in June, 1950. The Russians had tested a nuclear weapon in October, 1949, but we may be sure that by the time of the Korean War they had no real capability. Curiously, that war for a time seemed to have an effect on strategic thinking of the time. It was looked upon as entirely an aberration -- "the wrong war, at the wrong place, at the wrong time, and with the wrong enemy," to use the words of General Omar Bradley. Actually, the American leaders felt certain, one cannot say on what basis of information, that the Soviet Union was using the Korean War as a <u>ruse de guerre</u> to cause the United States to embroil its armies in a far-off land while the Russians prepared to attack in

Europe. For that reason among others, there was a minimal desire to use our quite limited supply of nuclear weapons in Korea. Also, there was the feeling, especially in the Air Force (which wanted to keep nuclear weapons for strategic use) that such weapons were not really tactically effective. These feelings supported an already pronounced aversion for political and moral reasons to using nuclear weapons in that war. The result was that they were not used even though American and United Nations forces found themselves at times in considerable difficulties. This non-use of nuclear weapons in this first of modern limited wars was to have a considerable effect on strategic thinking, but only much later on.

The famous "massive retaliation" speech of January, 1954 by the then Secretary of State, John Foster Dulles, was not a reply to opposing theories, which hardly existed at that time, but an effort to reject the Korean War as a pattern for future American strategies. American conduct in that war had been marked by unprecedented restraint, and Dulles and his political and military colleagues wanted to make it clear to potential aggressors that they could not rely upon a repetition of such restraint. Secretary Dulles wanted especially to make clear that even in the event of the recurrence of fighting in Korea, or of the outbreak of another war like it, the United States would feel free to use nuclear weapons if she felt it necessary, or even merely advantageous. "Local defenses," Dulles said, "must be reinforced by the further deterrent of massive retaliatory power." Another key assertion was: "The way to deter aggression is for the free community to be willing and able to respond vigorously at places and with means of

its own choosing." The American leaders, he went on, could now "shape our military establishment to fit what is our policy, instead of having to try to be ready to meet the enemy's many choices. That permits of a selection of military means instead of a multiplication of means. As a result, it is now possible to get, and share, more basic security at less cost." He added also that if the Communists renewed their aggression in Korea, "the United Nations response would not necessarily be confined to Korea."

It should be noted that the reasons that Secretary Dulles presented in justification of the new policy were in large part military and political -- as evidenced by the emphasis on "deterrence" and on keeping the initiative in American hands -- but also in large part economic. He emphasized that the United States could not continue indefinitely to develop large armies and maintain them in various parts of the world and at the same time also provide the clearly indispensable forces for nuclear warfare, especially the strategic force for general war. The Korean War had prompted a tripling of the American military budget. President Eisenhower, who as Chief of Staff of the United States Army in the years following World War II had worried about the strain on the American economy of a military budget of 12 to 13 billion dollars annually, was now appalled at one that was running over 40 billion dollars a year. He was determined to bring it below that figure, which he in fact succeeded in accomplishing, though he never managed to bring it below 38 billion dollars. To achieve even this figure, the Eisenhower Administration had had to commit itself to

what was then called the "new look" (after a then current style in women's clothing). The new look entailed heavy emphasis on the forces necessary for nuclear strategic bombing, and tight economizing everywhere else. The United States had, according to the Administration of the time, passed the stage where it could afford to delay the decision between conventional and nuclear arms. It had perforce opted for the latter -- no other choice was thinkable.

The Dulles pronouncement shortly began, however, to provoke criticism. The first criticisms were by politicians of the opposite party, like Messrs. Chester Bowles and Adlai Stevenson, who currently argued that the United States would not always feel it appropriate to use nuclear weapons in relatively minor aggressions in far-off countries and must therefore retain the capabilities to deal with such aggressions in other ways. At the same time certain more fundamental ideas were being developed and were reaching the stage of publication. These ideas had been stimulated less by the Korean War than by the shock of the advent of the first thermonuclear weapons.

The first American thermonuclear device was tested in November 1952, but its success was not made public until a year and half later -- at which time the Soviet Union, too, had already, some months back, succeeded in detonating a thermonuclear device. However, even before the existence of these terrible new weapons was made public, some among those who knew about them, and who had been appalled at the thought of wars being waged through the exchange of such monstrous charges, had begun to think

of the importance of limiting wars -- to limit them geographically if possible, and thus to require that they be limited in terms of the objectives of both parties. Only through such limitation, this small band of thinkers felt, could the threat of nuclear disaster to the world be avoided. It was no longer safe to rely entirely on deterrence; one also had to try to "control" war. The experience of the Korean War had not awakened this thinking, but it did serve as a heartening indication that even modern war could be limited. The Dulles pronouncement was directly attacked, mostly because it was felt that the kind of dread sanction he was trying to reinvoke would simply not be "credible" as a threat against relatively minor aggressions.

It should be remembered also that at this time, the middle of the 1950's, some decisive changes had occurred in the nuclear environment. The period of American nuclear monopoly had ended, and the thermonuclear weapon had come into existence in both camps. Moreover, especially because of the enormous stimulus that the Korean War had given to American nuclear weapons production, the period of nuclear "scarcity" that had characterized most of the first decade of the nuclear era was now ended. From now on one could talk about nuclear "plenty," with nuclear weapons being potentially abundant for all conceivable uses. Finally, the development of the long-range ballistic missile, or ICBM (intercontinental ballistic missile) as it was usually called, which shortly was to develop an accuracy far beyond the most optimistic predictions of the mid-fifties, was changing the whole picture of general nuclear war. It would now be

not a matter of weeks for bombers to finish delivering by repeated missions their deadly loads, but rather a matter of hours for the strategic bombing forces of a great power like the United States to accomplish their mission.

The arguments in favor of limiting war paid little attention at this time to the distinction between tactical nuclear weapons and conventional forces. The important thing was to avoid total engagement involving strategic nuclear bombing, or "general war" as it came soon to be called. Thus, the emphasis was on limiting the fighting locally to the exact area where a frontier had been violated.

It is difficult now to recall that these ideas were at first greeted as being simply harebrained. The axiom that "modern war must be total war" was too keenly ingrained to be easily shaken. "Limited war" smacked of something of the seventeenth and eighteenth centuries, or of the "Marquis of Queensberry rules," and it seemed idiotic to try to apply the idea to war.

It is interesting that all three military services initially opposed the idea of limiting war, though the opposition of the Air Force was more intense and more enduring. After all, general recognition of the decisive effectiveness of strategic bombing, in which the Air Force played a unique role, had been a long time in coming, and now that it had finally arrived there was beginning to be heard in the land the admonition that strategic bombing was <u>too</u> effective to be tolerable in view of the likelihood (or certainty) of an enemy reply in kind.

We should remember that the new conception of limited war was totally different from the idea as applied by

historians to the wars of the 18th century. If wars were limited in ages past, the reasons why they were so have little relevance for us today. Apart from the existence of moral, religious, and dynastic scruples, and the fact that anything as basic as national existence or even the survival of a dynasty was rarely at stake, wars were kept limited by the small margin of the national economic resources available for mobilization and by the small capability for destruction that could be purchased with that narrow margin. Today, on the contrary, we speak of limited war in a sense that connotes a deliberate hobbling of a tremendous power that is already mobilized and that must in any case be maintained at a very high pitch of effectiveness for the sake only of inducing the enemy to hobble himself to like degree.

No conduct like this has ever been known before. Nations and princes have often refrained from mobilizing their full potential strength while engaged in war, usually from lack of incentive. But to limit one's effort is not the same as to restrain one's available military power. The chief problem of limited war today is the problem of finding sanctions for keeping out of action, on a stable basis, just those existing instruments which from a strictly military point of view are far the most efficient -- and which have in the past tended to be dangerously vulnerable to attack while on the ground. However, by the time the decade of the 'fifties came to an end, ideas about the desirability or necessity of limiting war had made considerable headway. Then the coming into office in January, 1961 of the Kennedy Administration gave an enormous impetus to this movement. We must not forget

the considerable direct interest in the relevant issues of the new President, who as a senator had been a member of the Military Affairs Committee and had involved himself much more deeply in the problems occupying that committee than most of its other members. The appointment of the very forceful and determined Mr. McNamara, who continued on into the Johnson Administration, assured the continuity of the strategic ideas that the short Kennedy era had represented.

What has marked the Kennedy-Johnson Administration in the realm of strategy more than anything else has been the deep revulsion against the horror of nuclear war. The development of a Soviet strategic capability would have been enough to make officially respectable the formerly derided conceptions of "limited war," but there also developed within the Administration a great concern with nuclear "proliferation." Thus Administration leaders began among other things to express a keen disapproval of the French nuclear program for military purposes. There were a number of reasons why that program was bad in the Administration's view, but chief among them was the allegation that it would not only place France among the nuclear powers but would also stimulate others to undertake a similar program, which they might not otherwise do. The "others" about whom concern was felt included, at the head of the list, Germany.

The new Administration rejected vehemently the "massive retaliation" idea of the Eisenhower Administration. This was reflected not in any reduction of the strategic bombing forces, which on the contrary were accorded even more funds for intensive development, but in the

considerable building up of the ground forces and in organizing them into combat-ready divisions known as "special purpose" forces. The new Administration began to stress the importance not only of limited war, even in Europe where limited war had hitherto been considered impossible, but especially of limited conventional war. Nuclear weapons had to be provided in abundance and kept in reserve, but the more restrictions put about their use the better. It became almost an axiom that the main purpose of nuclear weapons was to keep the enemy from using them.

Finally, in the realm of general or strategic war itself, concepts of "controlled response" and avoidance of city bombing came into the ascendancy. Such means of fighting required very large strategic forces indeed, which the United States was able and willing to provide along with its special purpose forces. This thinking also stimulated the Administration to further public conflict with the French program, because it was felt that an independent French force de frappe would make impossible a sophisticated playing of a strategic bombing campaign in case of general war. The French Government was, in the American Administration view, simply spoiling things for others as well as itself.

The vigor with which the Kennedy Administration approached changes in strategic doctrine was also applied to the NATO relationship. In this respect the United States Department of State advanced as its own the views of the Defense Department, which had strong conceptions about improving the efficiency of the NATO military arrangements and especially about the importance of the

NATO allies in Europe building up conventional forces. After all, the American nuclear strategic forces were held to be more than large enough, to be ruled by the most modern doctrine, and to be ready beyond doubt to defend the allies of the United States. Inasmuch as the United States was already contributing nine or ten per cent of its great GNP (gross national product) to defense, and the European allies very much less, it was up to them, the argument went, to carry their share of the burden by expending more on defense. And in the Administration opinion, the only sensible way for them to expend it was on conventional armaments for the common good.

Apart from certain modifications in the official American view, due mostly to European recalcitrance, that is about where we stand today. And it is with the issues just described that most of the essays in this book concern themselves.

III

The selection of essays for this volume has been based on several criteria, no one of which could be satisfied completely or even satisfactorily. The first and most important criterion was that the leading and most original minds in the field of strategic thinking should be represented. This requirement has been satisfied in so far as essays from the right people were available for publication; but some leading figures have been absorbed into the work of the Administration to such an extent that they have not written for publication at all over the past three years or have published things which were from the point of view of subject matter simply not usable. Also, as I have already indicated above, there are a number of analysts who have made some noteworthy contributions to strategic thought over the years but who have done nothing for external publication and who are therefore virtually unknown outside the defense community itself.

Another criterion, second in importance only to that given above, was that the articles selected should provide a good coverage of the important issues of the day. This requirement proved astonishingly difficult to fulfill, especially if one tried to harmonize it with the criterion of using publications only from the more original contributors in the field. For example, although the great majority of the leading civilians in the American defense community have espoused the idea of fighting limited war with conventional arms alone if

possible, a good article advocating that point of view is hardly to be found -- though the paper by Alain Enthoven comes close enough to filling the need. Incidentally, my own paper presenting a rebuttal to that point of view is the only one I could find presenting that side of the case. Thus the coverage of subjects is unfortunately very spotty.

A third and final criterion was that the essays be reasonably readable, especially in view of the fact that they had to undergo translation. Many good and even original thoughts are buried in a language so obscure and difficult to read that it is beyond the grasp of the laymen (and sometimes the expert) even when he is at home in the original tongue. And although lucid articles can always be adequately translated into French, the difficulties of translation mount disproportionately as the original writing becomes turgid and jargonistic. This criterion has affected especially those papers dealing with methodology of research and of strategic analysis.

However, I trust that what I have been able to put together despite these limiting considerations will be of interest and profit to all Frenchmen interested in strategy and interested especially in understanding the American contribution.