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Strategy as an Art and a Science

Bernard Brodie

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I HAVE BEEN ASKED TO SPEAK on the subject of *Strategy as an Art and a Science,* and it is perhaps a measure of my eagerness to return to these halls that I have accepted this assignment. It is effrontery enough that I, a civilian, should talk to this professional military audience on the subject of strategy, but that I should also do so in terms that might imply a mastery of the artistic as well as the scientific approach to the subject borders on the preposterous. I must disclaim that implication, and then proceed to try to do my best with whatever is left of the subject.

After all, a lecture title, like a book title, has the dual object of communicating some meaning concerning content and also displaying some sex appeal. It is a point of manners not to examine it too clearly for its meaning. On the other hand, it does help for the lecturer in beginning his lecture to know what in general he is going to talk about.

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The first thing that occurs to me when we talk about strategy as an art and a science is that we seem to some degree to be alluding to two different eras of time. The kind of scientific approach to *strategic problems* represented by my own organization, the RAND Corporation—and by similar organizations associated with the Army and the Navy—dates only from World War II. Notice I said "strategic problems" rather than "strategy." Inasmuch as the latter term suggests something comprehensive, coherent, and on a level of high-policy decision, we are still far from having found out how to do it scientifically.

Nor do I wish to suggest that the approaches to strategy of the pre–World War II era were essentially unscientific. On the contrary, they were good or they were bad in the degree to which they reflected scientific values of objectivity, realism, comprehensiveness, and imagination.

Let me, however, caution you that except for some gifted individuals, who have been historically scarce—and who may or may not have had much influence on their own and subsequent times, both art and science have generally been lacking in what presumed to be strategic studies. Whether we have much to crow about now I shall leave to a later point in my talk. But we should not be deceived by our own fine words, and when we are talking about strategy either as an art or as a science we should be clear in our own minds that we mean a study as ideally conceived but only infrequently pursued.

One other distinction I must make clear at this time is that between the study of strategic theory and strategic problems, on the one hand, and the actual practice of strategy by the general or the admiral on the other. The difference is not quite as sharp as it sounds, especially now that the important strategic decisions are made not in the heat of battle but during peacetime in relatively quiet offices. Nevertheless, within the limits of my assignment, I have elected to talk mostly about theory. This perhaps betrays my own bias, for the national interest (and I am sure your own professional interest) is in the practice, not the theory, of strategy.

On the other hand, it seems historically confirmed that when theory has declined so has practice. How could it be otherwise? Generals and admirals have to learn their art somewhere, but it makes a good deal of difference whether they have been trained in an atmosphere of live inquiry about strategy or [have been] simply handed down some stereotyped axioms. The terrible example of World War I in its land phases should be enough to convince us of that. I think it is fair to say that while good theory will not guarantee good generalship, bad theory will certainly guarantee the reverse.

One of the first things that strikes the serious student of strategic thought is how small is the band of really significant contributors to the field. In the strategy especially of ground warfare, the most commanding figure by far is [Carl von] Clausewitz, who has, after all, been dead for over a hundred years. His contemporary, [Henri de] Jomini, was also a respectable figure, though not on the same plane as Clausewitz. However, he has, I think, been far the more influential of the two. He used French, which is a more international language than the German of Clausewitz; even in translation, he is easier to read and understand; he wrote much more; and, above all, he lived much longer. He lived to be a very old man and was prolific throughout all his life. It was Jomini who was read by the men who directed our Civil War, and it was Jomini rather than Clausewitz whom Mahan acknowledged to be his best friend among writers. I think that this is historically an important point because the influence of Jomini has certainly been made more apparent in our own time than that of Clausewitz.

After Clausewitz and Jomini, we find various contributors to special studies in strategy, some of whom were quite good. In Germany, there was the elder [Field Marshal Helmuth] von Moltke, who was Chief of Staff in the Imperial Army for something over thirty years (apparently they were not very dedicated to rotation in those days). Then, of course, there was [Count Alfred von] Schlieffen. In France, we had the distinctive work of Ardant du Picq, while in Britain there was the name of that Colonel [George F. R.] Henderson who wrote so brilliantly of our own Civil War, and especially on the campaigns of Stonewall Jackson. I could continue to mention other respectable names, of course. Yet, it seems to me there was a profound decline from the time of Clausewitz in the quality of strategic thought. The decline finally took the form of a search for axioms which were simple and easy to grasp, something that Clausewitz himself had scrupulously avoided.

In the field of naval strategy we have one great name: that of the man who was so closely and productively associated with this institution, and whose name has been given to the building [Mahan Hall] adjoining this one [Pringle Hall]. He tends to overshadow a contemporary of his who would otherwise have been much better known—the British civilian naval historian Julian S. Corbett.

In the field of air strategy we have one name, the Italian general Giulio Douhet. His writings can all be put together in a book of rather small size; in fact, they have been so put together. Literally, there is no one else. You may think of names like Billy Mitchell and Alexander de Seversky, but Mitchell, though he was full of tactical ideas, really never gave evidence of having any strategic sense at all, while de Seversky, it seems to me, simply rewrote Douhet without acknowledgement and with much less sense of responsibility.

You perhaps feel I am treating very cavalierly a large amount of writing on strategic theory that has continued into the present day, as published in the various professional journals, for example. Let me however assure you, first of all, that the amount is not large; secondly, what amount there is tends (with few exceptions) to be of rather poor quality. I am not speaking of works in naval or military history. These are probably better done today than they ever have been done before; some very fine works in naval and military history have been produced over the last decade. I am talking instead about theoretical works in strategy, works which presume to explain rather than merely to describe the past, or which address themselves to present and future conditions. These tend to be repetitious, stereotyped, unimaginative, and, I am especially sorry to say, usually propagandistic. By "propagandistic" I refer, of course, to the warfare between the services.

Before we go on to speculate upon the aridity of strategic theory in our own times, let us consider the method of the great leaders of strategic thought I have referred to thus far. Let us first take Clausewitz, who represents what we might call the "philosophic interpretation of military history," and who is certainly the greatest figure in that tradition. Clausewitz was himself a professional officer and also a profound student not only of war but of the science and philosophy of his times. He was a great admirer, for example, of the philosopher Hegel, who was ten years older than he and who died in the same cholera epidemic of 1831. His admiration caused him, unfortunately, to imitate the characteristic Hegelian dialectic in his own writing. Thus, like Hegel, he presents first the thesis of his argument; then, the antithesis; and, finally, the synthesis. This is the characteristic which makes Hegel so difficult to read, and such is also the case with Clausewitz. We see it, for example, in the first chapter of his book entitled *On War*—the only chapter he edited and considered completed before his death—in which he sets forth, first, the proposition that war in its pure form scorns any modifications of violence. This is the theme on which the book opens, and it is developed with considerable eloquence. Then, suddenly, after a few pages, he begins to develop the opposite theme: that war, however, never exists in its pure form but is rather a phase in the political activity of states. This brings him to qualify considerably everything he said previously about war being pure violence.

Because of his dialectical method, Clausewitz is very difficult to understand by anyone who tries to read him casually. But he is easy enough to quote, and some of the sentences in his opening pages have quite a lot of blood and thunder in them. The authority of his words has therefore been used to underline the absurdity of trying to moderate war when, in fact, the whole tenor of his book is that war is a political act and must therefore be governed by the political objective. He returns to this theme again and again throughout the book. Clausewitz has been called "the prophet of total war," when in fact he is almost the very opposite: he is almost "the prophet of limited war."

His deductions on strategy were derived from a close reading of the military history especially of his own times, which embraced the Napoleonic wars, but also the wars of the preceding two centuries. Of the ten volumes into which his posthumous works were gathered, seven are devoted to monographs in history. His treatment of military history is comprehensive, careful, and, above all, objective. This, I submit, is still the key to the good utilization of history and strategic studies.

Thus, the qualities that make Clausewitz great are first of all his philosophic penetration and breadth, which make him examine the place of war in the lives of nations and which thus save him from the error which is common to so many lesser figures in the field—the error of considering war as though it were an isolated act, serving no purpose outside itself.

Another aspect of Clausewitz which makes him great is his insistence upon looking at the particular subject he is discussing from all sides. He is just as determined to make clear the exceptions to any rule as he is to set down the rule itself. It is for the latter reason that Clausewitz insists that there are no principles of war; that is, there is no system of rules which, if pursued, will guarantee success.

His contemporary Jomini scolded him for that position. Clausewitz has been criticized on the grounds that he left no "system" of strategy, no method which can be indoctrinated by teachers and learned by students. The observation is true, but I consider it to his great credit rather than a ground for criticism.

Clausewitz, notice, was living near the end of an era in which military technology was changing scarcely at all. Whatever changes in tactics and strategy we can attribute to the Napoleonic wars did not involve changes in materiel. The smoothbore flintlock musket was the hand weapon throughout the entire Napoleonic era, just as the horse-drawn smoothbore gun was the standard field piece. This puts Clausewitz's position in considerable contrast with that of Mahan, who began to write on naval strategy during a period of the most rapid and radical change in naval armament. Sail had given way to steam, the wooden ship to iron or steel construction and armor, the smoothbore piece to the rifled turret gun, and so forth. Yet the interesting thing about Mahan is that he turned his attention away from these changes that were going on in his own time to what he considered to be the enduring conditions of war at sea.

Like Clausewitz, only more so, the bulk of his writings are histories—naval histories of the days of sail. His great hero is [Admiral Horatio] Nelson, of whom he also wrote a biography. His precepts on naval strategy are found mainly in his histories, though he finally wrote (towards the end of his career) a volume called *Naval Strategy* (which was published in 1911), which he himself considered intellectually not very successful.

Like Clausewitz, he is interested not only in how men fight but also in why they fight. The articles and essays gathered together in the volume entitled *Armaments and Arbitration* reveal him as having a very considerable sophistication in international politics.

Very different from either Clausewitz or Mahan is Douhet, the prophet of air power. To begin with, he is not only not a historian (as the others were), but he explicitly and vigorously rejects the idea that one can learn from a study of history how wars should be fought. He rejects especially the doctrine, derived from Jomini and which I am sure you have all heard many times, that "methods change, but principles are unchanging." In fact, he turns that doctrine upside down, and insists that an invention as radical as that of the airplane must change everything about war.

I, personally, feel that Douhet deserves great credit for his boldness in this respect. I recall that Mahan, in one of the few instances that he let himself utter a dictum, stated that "the *guerre de course* (i.e., commerce raiding) can never be by itself alone decisive of great issues." This he based mostly on a reading of the War of 1812. But when Mahan died in December of 1914, the submarine was already at hand to suggest otherwise—that perhaps the *guerre de course* can be decisive of great issues; however, Mahan failed utterly to predict the enormous potential of the submarine as a commerce raider. Certainly no one would have predicted the present potential of the submarine as a strategic bomber.

I think the so-often-repeated axiom that I quoted a moment ago—"methods change, but principles are unchanging"—has had on the whole an unfortunate influence on strategic thinking, encouraging, as it does, the lazy man's approach to novel problems. It has certainly slowed down our adaptation to atomic weapons. If we attribute it to Jomini, we must bear in mind that the kind of changes Jomini witnessed in his lifetime bear no comparison at all with those we see in our own. I think, also, that Douhet deserves credit for scoffing at the kind of encapsulation of knowledge we encounter in the usual treatment of the so-called "principles of war."

Nevertheless, I suspect also that Douhet's ignorance of military history helps to account for one of his more disastrous errors. You remember it was a cornerstone of his philosophy that henceforward the defense would be so much superior to the offense on the ground that lines would be static, even if the defending army was much inferior to the attacking opponent's. This idea he based on a rough reading of World War I (I think a more careful reading of World War I would have shaken that opinion), and it was the kind of error that a person who was as brilliant as he but also a better student of history would probably not have fallen into.

None of the men whom I have mentioned thus far used anything which remotely resembled modern systems analysis, but only in Douhet's case do you see it resulting in really grave error. The others were looking for what remained unchanged in war. This obliged them to depend heavily on historical research. But Douhet was looking at what was essentially new, without having much to go on except his hunches. Thus, another cardinal error of Douhet's was that he grossly overestimated the amount of physical damage that could be accomplished with each ton of bombs, and grossly underestimated the amount of damage that any great nation could absorb. It was, in other words, his numbers or his quantitative judgments that were wrong. In some instances the error resulted from a failure to use elementary arithmetic.

Finding the correct numbers is what modern systems analysis particularly stresses, and Douhet would have greatly profited from it. To be sure, the atomic bomb came along to rescue Douhet from some of his worst estimates, but I think we are not being excessively purist if we deny him credit for that. Anyway, and more important, the atomic bomb and its nuclear successors went much too far in helping to redeem him from his errors. They have created new problems today which his philosophy fails to accommodate.

This makes us realize that the situation confronting us today points up Douhet's greatest deficiency: he forgot that war fits into a political context and must have a political function. He has not been alone in that respect, however. That error puts Douhet at the opposite end of the scale from Clausewitz, and it makes the philosophy of the latter in some crucial respects more pertinent to our own times than that of Douhet. However, I do not wish to imply that Douhet should be treated with anything other than considerable respect. His thinking was both imaginative and fiercely logical—after all, it was some of his premises that proved wrong, not his logic—and he was, of course, fearless in his opinions. Those are traits that will always deserve admiration and emulation.

I have tried thus far in this hour to help you recall something of the content as well as the method of the leading figure, or figures, in each of the three major fields of strategy. With your indulgence, I should like now to speculate on the reasons for what I consider the relatively low state of strategic study over the years, now being somewhat improved by the introduction of important new methods of scientific analysis.

One of the first thoughts that comes to mind is that Clausewitz may very well have exhausted broad speculation in his field, just as Mahan later did in his. There is certainly something to that. It was difficult to say original and profound things after Clausewitz. I know, from personal experience in making the effort, that it was difficult to say important and original things about naval war after Mahan and Corbett. And yet, that cannot tell the whole story. Clausewitz did not pre-empt the field of strategy any more than Adam Smith pre-empted the field of economics, yet compare what has happened in each of these fields in subsequent generations: in the latter case the tremendous and still vital growth of theory and knowledge, and in the former case very little growth or development.

Of course it could also be true that people like Clausewitz and Mahan were accidents, anyway—brilliant and original thinkers and scholars in a profession which, let us face it honestly, has never attached too much value to these qualities. The very infrequency with which such men have appeared would argue as much, but the examination of Mahan's career tends to confirm it.

We have to recall that Mahan never received any career benefits from his superb contributions to the strategic thinking of his time, except for assignment to this College. He was retired as a captain and promoted to rear admiral only in retirement, along with every other retired captain who had lived long enough to see service in the Civil War.

Mahan, in his autobiography, tells us that he came to regard himself as temperamentally unsuited to the career he had chosen. And, as a matter of fact, we know that he was not too well thought of as a ship's officer by some of his seniors at sea, the last of whom gave him a bad fitness report as commander of the cruiser *Chicago*.

The story tells us two things: first, that the Navy did not then place a very high value on strategic thinking *per se;* and, secondly, that Mahan himself largely dissociated his career as a scholar and writer from that as a naval officer. He seemed to feel that his naval career was important to him only in giving direction to his scholarly interest. Perhaps it also assisted him in his mastery of his subject, but we cannot be too sure of that. After all, a number of persons who did not have that background have also contributed to extremely important work, like Corbett and like that very interesting eighteenth-century figure, John Clark of Edinburgh, whose treatise on naval tactics had an important influence on the tactics at Trafalgar.

The Navy, like any military service, finds itself obliged to place a high value on certain other qualities besides scholarship in its officers. Notice that I am not criticizing this—far from it. Qualities like loyalty, physical courage and, especially, leadership are very high on the list. Intelligence is, of course, necessary to master the now very involved techniques, but it also can be fully absorbed in doing so. Since talents of any specific kind are always scarce, the more we emphasize one kind over another the more drastically we degrade our chances of getting the latter kind by any sort of inadvertence.

There is another characteristic of the military profession that I think is relevant. Unlike most of the esoteric professions, the military profession is rather averse to specialization. It is accustomed to specialization in technological fields, but from the career point of view on the basis of tolerance rather than encouragement. Compare this situation with that in the medical profession, for example, where the spectacular advances are the work not of the practicing physician, however specialized he may be in his practice, but of a relatively small corps of workers who are specialized in research.

The next question is: Why do the military services place such a low valuation on strategy (as I submit they do), which is to say on strategic insight and imagination and on the special kinds of knowledge that contribute to it? The answer falls into two parts, and I should like to say something about both.

One answer tends to be that the Navy, or whatever service it may be, does not need many strategists. After all, how many slots are there for commander-in-chief, anyway? As one German officer in the days of Schlieffen said to a young subordinate who was trying to develop his own ideas on strategy: "His Majesty retains but one strategist [Schlieffen], and neither you nor I is that man." However, this answer does not explain why the man who rises to the top—and thus who gets to be the practicing strategist—should be expected to do a successful job at it when he has been selected upward for other talents.

The other reason, I am sure, is the general conviction that strategy is easy. This statement may surprise you, but I submit that the conception of strategy being easy is implicit in all your training. Also, explicit statements to the same effect are not wanting. One good example is by the late Field Marshal Lord [Archibald] Wavell, who, in taking exception to a statement by Captain [Basil] Liddell-Hart, wrote the following paragraph:

> I hold that tactics, the art of handling troops on the battlefield, is and always will be a more difficult and more important part of the general's task than strategy, the art of bringing forces to the battlefield in a favourable position. A homely analogy can be made from contract bridge. The calling is strategy, the play of the hand tactics. I imagine that all experienced cardplayers will agree that the latter is the more difficult part of the game, and gives more scope for the skill of the good player. Calling is to a certain degree mechanical and subject to conventions; so is strategy, the main principles of which are simple and easy to grasp. . . . But in the end it is the result of the manner in which the cards are played or the battle is fought that is put down on the score sheets or in the pages of history. Therefore I rate the skillful tactician above the skillful strategist, especially him who plays the bad cards well.[1]

Many generals, from Napoleon to Eisenhower, have asserted in one form or another the idea that the main principles of war "are simple and easy to grasp," but it is remarkable that even Lord Wavell should have joined in that chorus. After all, the one fatal mistake of his own career resulted from an error in strategic judgment. In one place, he candidly admits as much. In the early part of 1941—only one year before he published the passage I just quoted—he lent his military authority to approving the British expedition into Greece, and committed a considerable portion of his forces to that purpose, without having first disposed of [General Erwin] Rommel in the desert. He excuses himself on the ground that the action would have been justified against any ordinary commander, and then he adds: "I had not reckoned on a Rommel."[2] And he has nothing to say about the fate of the expedition in Greece.

To return to the quoted statement, one notices also the traditionally narrow conception of strategy as "the art of bringing forces to the battlefield in a favourable position"—a conception which falls far short of allowing for any consideration of the ultimate objectives of the campaign—and especially of the war itself—on which considerations Wavell might have sought to excuse the intervention in Greece (as others had done). Above all, the idea that strategy, like bidding in bridge, "is to a certain degree mechanical and subject to conventions" betrays the almost universal assumption that the ends or objectives of the military effort are always given or somehow obvious. Certainly in war (as in many other critiques) it must be true that if one knows without question what one has to do, then deciding how to go about doing it (which is Wavell's definition of "strategy") makes far less demands on the leader than the actual doing. But, as the Korean War indicated, the question of what one has to do may be a quite confounding one the moment it is admitted to be a real question. For wars of the future, it may well be the greatest single question facing us. Also, for a variety of reasons, it is a question on which the politician can give us very little guidance, and we must not expect it of him. Let me digress for a moment to amplify that point, for I think it is a very important one. Remember one thing: you, and a very few civilians like myself, are able to spend most of our working hours brooding about the next war, but the politician is not able to do this.

Even if one accepts for the moment Wavell's own limited definition of strategy, one cannot help marveling at the cavalier way in which he dismisses strategic decisions as not only less difficult but also less *important* than tactical ones. Less difficult (within the limits he applies), they certainly are. The "main principles" of war of which he speaks, and which he so obviously overevaluates, represent for the most part modest refinements upon common sense, and to the thoroughly sensible man the making of a sensible decision upon a line of conduct might be quite easy. In contrast to tactical problems, which make heavy demands on technical skill and which in war are always multiple and often presented under great stress, the strategic decision is as a rule simple and gross in its content, is usually made in relative freedom from the heat and vicissitudes of battle, and it may be of a kind which is made once and for all for the campaign or for the entire war. But how crucial it is that it be correct!

To give you an example from a rather lower level—something which you might call "grand tactics" rather than strategy, but having some of the same characteristics—when Admiral [William F.] Halsey in the supreme test of his art at Leyte Gulf threw his vast Third Fleet against a decoy force, he effectively nullified both his own incomparable qualities as a leader and as a fighter and also the American advantage in possessing by far the superior fleet. The American landing forces—whose protection was his first responsibility—did not suffer the disaster his action invited, but he did lose the opportunity the Japanese had placed in his hands to destroy their main fleet (to be sure, it was destroyed subsequently and very soon). He made this error, notice, because of his rigid adherence to what he considered to be a "principle of war"—namely, the principle of concentration.

One thinks also of the arresting sentence with which Sir Winston Churchill qualified an otherwise harsh criticism of Sir John Jellicoe's conduct at Jutland: "Jellicoe was the only man on either side who could lose the war in an afternoon." What a world of trenchant meaning lies in that one admission!

Or, to use an example from the air campaigns of World War II, let us remember the decision of the RAF Fighter Command not to engage the German fighter sweeps sent over London at the onset of the Battle of Britain. A fairly obvious decision, to be sure—or was it? It took some stomach to refuse the German bait and let enemy planes fly unopposed over one's capital.

Let us remember, too, that the Allied strategic bombing campaign in World War II is rarely criticized for its tactical handling, which, on the whole, is generally admitted to have been magnificently done. All the important and voluminous criticisms of the effort center around questions which are essentially strategic. Were the basic military resources absorbed by it too great in view of the returns, or vice versa? Could not the air power involved have been better used, even as air power, for other military purposes? Were not the wrong target systems selected? And so forth. Whatever convictions one may have about the answers to these questions (or the spirit behind the questioning), the questions themselves are neither irrelevant nor unimportant.

Finally, on the very topmost level of decision was the Allied election, upon the entry of the United States into World War II, to concentrate on defeating Germany and Italy first, rather than Japan. This, in view of the attack on Pearl Harbor, was emotionally, I am sure, not an easy decision. But what could have been more simple and more obviously correct? We know this commitment was resisted, and we also know how fortunate it is that the basic resolve behind it never faltered.

It is hard to escape the conclusion that Lord Wavell's view reflects a peculiarly professional bias. There is no doubt that tactics and the administration of military forces are the areas in which the soldier is most completely professional. The handling of battles, whether of land, sea, or air—the maneuvering of large forces, the leadership of men in the face of horror and death, and the development and administration of the organizations which must effect these purposes—is clearly not a job for amateurs. In these tasks there is no substitute for the hard training and the experience which the services alone provide over long years.

It is understandable that these tasks should be much more carefully regarded and much more honored than those which seem to lie on the periphery of the profession, and indeed almost outside of it, which often cannot be tested until war comes—and perhaps not conclusively even then. And yet I feel that we are likely to make far more basic and costly errors in the field of strategy than we ever could make in tactics.

I now have time for just a few words about modern scientific method. First, let us remember that scientific method is useful and is being used in exploring alternative choices but not in making the *final* choice. The latter depends ultimately on good judgment, which is to say on the informed intuition of a person or of a group of persons who have been brought up in a particular indoctrination and whose approach to their work is fundamentally that of the artist, not of the scientist. I am not complaining about this—as a matter of fact, I don't see how it could be otherwise—I am merely observing it.

The scientific work which gets accepted tends to be that which affects tactical problems rather than strategic ones, or which affects lower-level strategic problems rather than higher-level strategic problems. As a matter of fact until rather recently my own organization, the RAND Corporation, kept out of strategic problems. Only recently has it really penetrated into this area. You may say that it refrained from doing so largely because it was invited to refrain from doing so.

The universe of data out of which reasonable military decisions have to be made is a vast chaotic mass of technological, economic, and political facts and predictions. To use the scientific method in bringing order out of this chaos is nothing other than the best we can do. When the method is true to its own tenets, it is bound to be more reliable by far than the traditional alternative method, which is to rely on the intuitive judgment of experienced commanders. One reason it is better is that it tends to incorporate in an orderly fashion whatever is good in strong intuition.

However, our experience thus far with scientific preparation for military decision-making warns us to appreciate how imperfect is "the best we can do." Those of us who do this work are beset by all kinds of limitations, including a limitation in talents. Above all, there are limitations in available knowledge. Where the object is to predict the future for the sake of appropriate action now, we simply cannot wait until all the relevant facts are in. Besides, we can make progress only as we cut off and treat in isolation a small portion of the total universal data confronting us. For that reason, almost every study is to some extent (and sometimes to a larger extent) out of context. In addition, we are dealing always with large admixtures of pure chance. These are sometimes difficult to take into full account without seeming to stultify our results, and that, of course, one is naturally loathe to do. The same is true of the large range of variables which deal with enemy intentions and capabilities. Finally, we are immersed in bias—our own and that of our clients and readers. To the latter we adjust in unconscious or semiconscious anticipation even when we try to be honest—and it is difficult always to be entirely honest.

A word on the great development in recent years of the gaming technique. A casual visitor at RAND might have the feeling that nothing goes on but games. RAND, I think, pioneered in the developments of all sorts of games, including games on a very high strategic level. I have in mind that the Naval War College has a considerable history in this respect, too, but I think it is largely on the tactical rather than the strategic level.

It seems to me that the technique of gaming does at least two things, both of which are extremely important. One is that it tends to make a reality out of the potential and also the intentions nominally ascribed to the enemy. I have had the privilege of studying over the years a number of so-called "strategic studies," and I have often been amazed at the degree to which they are permeated by what one can only call "wishful thinking." There will often be on the first page a list of stated assumptions or postulates which will say something like the following: "(1) The enemy is very intelligent; (2) He has the initiative." When you turn the pages, however, the enemy has ceased to be intelligent, and he has also ceased to have initiative. War gaming does not let you get away with that.

Secondly, and I think almost equally important, the list of the strategic studies which I have seen are really deployment studies. They are not war plans but deployment plans. War gaming forces you to push your thinking beyond the first step, and perhaps beyond the second and third steps.

Well, I see my time is up. What have I tried to present to you this morning? Certainly it was nothing that you can carry away in a convenient package—and let me say that this was my intention. I have tried to persuade you that strategy is not a simple study; that it is an extremely important one; and that there are no easy answers.

Thank you very much!

Notes

1. Archibald Percival Wavell, *Soldiers and Soldiering: Or Epithets of War* (London: Jonathan Cape, 1953), p. 47.

2. Ibid., p. 78.

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