

On the Genesis of Nuclear Strategy:
Letter to Michael Howard (1968)

Albert Wohlstetter

Excerpted from Albert Wohlstetter, *On the Genesis of Nuclear Strategy*, unpublished, expanded version of Wohlstetter's unpublished November 6, 1968, letter to Michael Howard, with additional materials and commentary by James Digby and Arthur Steiner, and a note by Michael Howard, revised circa April 1986, available from Hoover Institution Archives, Albert and Roberta Wohlstetter Papers, Writings, Box 187, Folder 22. Courtesy of the Wohlstetter Estate, the Digby Estate, and Arthur Steiner.

November 6, 1968
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Michael Howard
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Dear Michael:

Let me begin with some comments on a few specific points in your paper on the history of nuclear strategy. I shall deal with the timing and logical content of concepts and doctrine, the role of physicists, "academic" historians and social scientists, and the then "unacademic" systems analysts whose work used actual military deployments, plans and operations; also with the actual relations of nuclear forces in the 1950s. My comments concern not only those concepts and strategies in whose development I was personally engaged, but also some earlier history that is traceable in the Special Collection on atomic scientists in the Harper Collection at the University of Chicago and similar collections at the U.S. Atomic Energy Commission.¹ I shall be using my as yet unpublished lecture notes on the history of nuclear strategy – and especially the notes relevant to the statements in your paper about

how nuclear weapons were seen to affect and how they affected the stable deterrence of war; and the genesis of the first-strike, second-strike distinction.

First, your pages 4ff:² You contrast the lay notion that nuclear weapons would transform the entire nature of war with the judgment of professionals: ". . . for the professional they made remarkably little difference. . . ." You suggest that the professional, including not only the military but also scientists who had long experience of military planning, held the latter view, and cite Blackett and Bush as examples.³ Then you contrast a few "academics" who were thinking ahead of what you assume to have been the state of the art for the ten years following the initiation of planning in NATO at the end of the 1940s.

However, the very first contrast made—that between the professional and the layman—will not sustain examination; and the state of the art in the 1950s was not what you suggest. The physicists connected with the Manhattan Project (including some fitting your description as experienced with military planning) were the first to see that nuclear weapons made a great difference—though their understanding was understandably deficient. The "difference" made is actually multiple and complex. Some differences were critical much earlier than you suggest. NATO plans in 1949 and later did not recognize the impending technical environment in which they would operate in the 1950s. Finally, academic social scientists and historians, like Viner, Brodie, and Fox,⁴ did indeed have important insights in 1945 and 1946, but they did not foresee the possibility that nuclear attacks on nuclear strategic forces raised an entirely new order of problem requiring a major distinction between "first-strike" and "second-strike" forces. Indeed, in some respects, they were even further from seeing the problem than the physicists—who caught inconsistent glimpses of it.

As my brief talk at Oxford indicated, it was mainly those rival institutions who didn't have the bomb at the war's end (such as the Navy, the ground Army—and the Russians) who then said it made little difference. And politicians and professionals associated with these bombless ones said the same. Military professionals connected with the Army Air Corps, and those concerned with strategic bombing in particular took an opposite view. (The War Department public statements uneasily tried to bridge its air and ground advocates' views.)

The physicists connected with the Manhattan Project at first almost unanimously held that the bomb changed things completely. Item 1 in the four-point "Creed" of the Federation of American Scientists read: "1. The bomb is a revolutionary weapon." But then after 1946 these scientists began to associate themselves with one side or another in the factional disputes. The majority gradually reversed the absolutist position they had previously taken that there was no defense against nuclear weapons, that it was "one world or none."⁵ But this was after the end of 1946 when the Russians turned down the Baruch Plan and it was clear that there was not going to be one world. The physicists then looked more soberly at the "many-world" alternative to none. For the first half of the 1950s, in fact, the majority faction of physicists swung to the opposite of their first extreme. Vannevar Bush, whose 1949 views you cite, illustrates perfectly both the initial position and the change. His memoranda⁶ on September 30, 1944, stated that nuclear weapons were of world-shaking importance, that they would soon place every population center in the world at the mercy of the nation that struck first, etc.

Let me expand a little on the initial position of the natural scientists and engineers connected with the Manhattan Project. And then let me treat the views of Viner, Brodie, and Fox in relation to those of the Manhattan Project scientists. I think it is clear that each of these groups had vital insights. Neither, however, can genuinely be said to have understood "the whole concept of a stable balance of second-strike forces" (your p. 5)⁷ in the plain sense in which the phrase is used today and in which it was defined. Moreover, when looked at historically it is possible to see why, for all the honors they deserve, they were not likely to have foreseen the relations of forces that called forth the distinction in the early 1950s.

THE MANHATTAN PROJECT SCIENTISTS

A good place to begin with the early views of the atomic scientists is the "Prospectus on Nucleonics" by a committee headed by Zay Jeffries that included Enrico Fermi, James Franck, T. R. Hogness, R. S. Mulliken, R. S. Stone, and C. A. Thomas. It was dated September 1944. It contains several ideas that became commonplace immediately after Hiroshima.⁸

The first was the recognition of the enormous increase in destructiveness enabled by nuclear weapons, and, coupled

with this recognition, the insight that simply overmatching an adversary's bombs is not strictly in point.

A nation, or even a political group, given the opportunity to start aggression by a sudden use of nuclear destruction devices, will be able to unleash a "blitzkrieg" infinitely more terrifying than that of 1939-40. A sudden blow of this kind might literally wipe out even the largest nation—or at least all its production centers—and decide the issue on the first day of the war. The weight of the weapons of destruction required to deliver this blow will be infinitesimal compared to that used up on a present day heavy bombing raid. . . .

The second was the idea of the prospect of nuclear retaliation as, it is to be hoped, something that might paralyze an aggressor.

The most that an independent American nucleonic rearmament can achieve is the certainty that a sudden total devastation of New York or Chicago can be answered the next day by an even more extensive devastation of the cities of the aggressor, and the hope that the fear of such a retaliation will paralyze the aggressor.

On both counts, the Jeffries Committee deserve very early credit. Yet, if one examines the statements closely, both analytically and in their historical context, some essential limitations emerge.

First, like almost everyone else for years to come, members of the Jeffries Committee were thinking primarily of production centers and cities as the natural targets for nuclear attack. Your quotation from Vannevar Bush in 1949 (and Bush's 1944 memoranda as well) display the same presumption: "They could undoubtedly devastate the cities and the war potential. . . ." For a good many reasons, some of which I have described elsewhere,⁹ the notion was ingrained very early that an atomic weapon is essentially a weapon of "mass destruction" or "terror" to be used as the Americans used it at Hiroshima. Eugene Rabinowitch, the editor of the *Bulletin of the Atomic Scientists*, and two other physicists from the Metallurgical Laboratories in Chicago wrote that "Atomic bombs are weapons used only against large cities and industrial centers. Therefore, if both sides in a conflict have enough atomic bombs to wipe out each other's cities, they are in

approximately equal position, even if the one has three times more bombs than the other." (*Life*, October 29, 1945, p. 46.) The famous Franck Report, which was dated June 11, 1945, proceeded on the same assumption: "Atomic bombs containing a larger quantity of active material but still weighing less than a ton may be expected to be available within ten years which could destroy over ten square miles of a city. A nation able to assign 10 tons of atomic explosives for the preparation of a sneak attack on this country can then hope to achieve the destruction of all industry and most of the population in an area from 500 square miles upwards." (Signed by J. Franck, D. J. Hughes, J. J. Nickson, E. Rabinowitch, G. T. Seaborg, J. C. Stearns, and L. Szilard.)¹⁰ Whether cities were the only targets or just the preeminent ones, phrases like "weapons for mass destruction" came to be used as synonyms for "atomic weapons." So they entered the language and so they continue to color our thought, even though we have long since come to see the critical importance of other targets quite detached from masses of people. As might be expected, Oppenheimer in 1945 summarized with characteristic eloquence the essentially universal view of an atomic weapon: "Surprise and... terror are as intrinsic to it as are the fissionable nuclei."¹¹

Oppenheimer's understanding had been formed in the circumstances of the original use of the weapon. The Interim Committee and the Scientific Panel, of which Oppenheimer was a member, were seeking as a target "a vital war plant employing a large number of workers and closely surrounded by workers' houses."¹² As was not infrequently the case in the strategic bombing debate between the wars, there was a certain ambivalence about the purpose of destroying "closely surrounding" civilian workers and their houses in addition to the "vital war plant." The flow of products from a war plant, however "vital," supported the war only by way of a pipeline of material to the fighting. Interrupting the material flow would reduce stocks and have an indirect and delayed effect. So also for the plant workers taken simply as a factor of production. But the sudden act of annihilating the plant and the workers could shock and inspire terror and so have a direct and immediate effect on the popular and governmental will to continue the war. Standard doctrine of strategic bombing, both English and American, stressed not only the destruction of war-supporting industry, but also the weakening of an adversary's will to resist. Though the Interim Committee and the Scientific Panel agreed that "the United States ... could not concentrate on a civilian area," it chose a war plant closely surrounded by workers'

houses “to make as profound a psychological impression on as many of the inhabitants as possible,” to administer “the maximum surprise shock.”

Surprise at Hiroshima had then a function quite different from its role in surprise attack on nuclear forces. It reduced the probability that the active defenses would be alerted and the single unescorted plane carrying the A-bomb intercepted. But even more important, since delivery could have been assured by other devices—for example, by an escort of hundreds of planes—surprise was an intrinsic element in the terror and shock aimed at and achieved. It is easy to see why terror, and surprise in relation to terror, were seen not only by prominent members of the Scientific Panel, but also by the Manhattan Project physicists and by a wider public, as the essentials after Hiroshima and Nagasaki. The A-bomb was preeminently a weapon to be used against population centers or against industry embedded in population centers.

That this was an almost universal view of the Manhattan Project physicists I can confirm on the basis of an examination of hundreds of their statements made from 1944 to 1946. This view led to other consequences I cannot elaborate on in this letter. For example, it displaced the matching of weapons against weapons with an equally mechanical numerical matching of bombs against cities. This in turn led to the stereotypes of “overkill” in which numbers or total yield of bombs are compared with total population (now usually the population of the world). And it led natural and social scientists to take degree of urbanization as the measure of a country’s vulnerability. In 1945 and 1946 this was taken to imply the intrinsic disadvantage to the United States in an arms race with the Soviet Union; and when after 1946 physicists began to think of defense as an alternative to world government, they thought of defending cities and began by talking especially of one most costly and implausible measure—namely to outrace bomb stockpiles by multiplying and dispersing cities.

However, what is essential for this letter is the way their view of the bomb as preeminently a city-destroyer blinkered them as to the possibilities of using it to destroy strategic nuclear forces. Perhaps the most revealing testimony in this respect is that of the Nobel Laureate Irving Langmuir.¹³ I mentioned it in my talk at Oxford. He pictures four stages in an arms race:

1. We alone have atomic bombs. We are then secure at that time. 2. Other nations also have atomic bombs, but they haven't enough to destroy all our cities; but we have enough to destroy all of theirs. We are still relatively secure, and nobody is likely to start an attack under those conditions. 3. Two or more nations have enough bombs to destroy all cities, perhaps 10,000 bombs of the kind that we have now. That will probably come in an armament race. Retaliation, however, would be expected and that would be a deterring factor, but perhaps not decisive.

As was mentioned yesterday, and I think discussed by General Groves, 40,000 people might be wiped out in the United States by an attack of that kind, and it would not help us much to destroy 40,000,000 people in the nation of attack. . . .

There is, however, a fourth stage which would automatically come sooner or later in any unlimited armament race. We can confidently assume that there are going to be discoveries made in this field. They may be made 4 to 5 years hence. They may be made 10 or 15 years hence, but it is almost certain that we will have atomic bombs a thousand times as powerful as those that now exist by means that are now undiscovered.

It could be done by a cheaper means of production. Instead of producing 10,000 bombs, it is conceivable that by cheaper means of construction you could have 300,000 bombs.

That would be enough to treat every square mile in the United States the way Hiroshima was. There would then be no retaliation. There wouldn't be 60 percent of the people left; there might be 2 percent of the people left, and under those conditions you can see what happens in the world.

In short, so fixed was the notion that cities or production centers were the primary targets for nuclear weapons that Langmuir could only foresee nuclear damage to nuclear retaliatory forces when there would be enough bombs to cover the entire country – and so inevitably, as a by-product, nuclear strike forces too! The 300,000 bomb calculation is quite typical of the gross computations of the time. Langmuir took the 10 square mile damage area sometimes¹⁴ roughly estimated for Hiroshima and, assuming square bombs,

divided it into the 3 million square mile area of the United States. A good many of Blackett's calculations in his first book are of the same order of precision.

This suggests some limitations on the physicists' understanding of the problems of retaliation and its virtues. If, in fact, nuclear weapons to all intents and purposes were usable only against cities and industry, nuclear retaliatory forces would be intrinsically quite safe. Effortlessly safe, since they wouldn't be attacked. Retaliation would be assured. There would be, essentially, no distinction between striking first and striking second (and therefore no need for a first-strike, second-strike distinction.) And this automatically suggests, especially today, the prospect of deterrence. And, especially today, this doesn't seem the worst of all possible worlds. To us, it suggests at least a limited but important kind of stability. However, it would be a mistake to read our views into the writings of the physicists at the time. In fact, the Jeffries Report didn't think much of "the hope that the fear of such a retaliation will paralyze the aggressor." It went on to say, "The whole history of mankind teaches that this is a very uncertain hope, and that accumulated weapons of destruction 'go off' sooner or later, even if this means a senseless mutual destruction." The Jeffries Committee, then, uttered one of the earlier versions of the apocalyptic argument about the inevitability of nuclear war through some irrational act: "sooner or later." (Observe that Langmuir, too, refers to the deterring prospect of retaliation, but without enthusiasm.)

Moreover, in between the two paragraphs [of the Jeffries report] I have cited earlier, which drew the picture of a nuclear war as a sequence in which an aggressor destroyed the cities and production centers of his adversary, who in turn inflicted a similar mass destruction on the aggressor, the authors of the report included a fascinating analogy of the nuclear dilemma with the situation of two men equipped with machine guns in a room of 100 x 100 feet. The first to attack would not only destroy the other but, provided he attacked soon enough, emerge unscathed. Here the difference between striking first and striking second is all important, and there is an enormous incentive to preempt, a maximum of instability. (The close machine gun duel analogy has been attributed to Eugene Wigner¹⁵ and used by other physicists as well.)

The Jeffries Report, then, contains side by side two incompatible pictures of the revolution wrought by nuclear weapons. In the one picture, striking first is of no advantage, since the other side

inevitably could retaliate in kind. In the other picture, striking first is decisive, since neither side could retaliate. These incompatible pictures were not *seen* to be incompatible. Each existed, so to speak, by itself. And, by itself, neither would indicate any urgent need for a first-strike, second-strike distinction of the kind that grew out of the Base Study.¹⁶ Neither picture called for basic choices and difficult efforts in the design and construction of a nuclear force specifically to survive nuclear attacks.

If we were to use the latter distinction, we might say that, in the one picture of the nuclear world, *neither* side could have a capacity for striking second; in the other picture of the nuclear world, with both sides directing their attacks on cities, *each* side with nuclear weapons had a capacity for “striking second,” that is, retaliating against the other *automatically*. However, that is to use the words quite differently from the way they were defined when I introduced the distinction at the start of the 1950s. There *the capacity to strike second plainly referred to the ability of a nuclear force to strike back after the force itself had been subject to nuclear attack*. To find it urgent to make the distinction, one had to perceive both that it was possible and useful to get a second-strike capability and that it was neither inevitable nor easy. In a sense, the Manhattan Project physicists missed the target on both sides, as the Jeffries Report and many other documents illustrate. The world of the two close machine gunners missed it on the left by failing to see the measure of stability that might be brought by making even sudden attack highly risky. And a world in which one nuclear country would open a war with a nuclear attack only on a second nuclear country’s cities and production centers missed it on the right by making nuclear retaliation automatic, or a minor problem. Surprise and striking early were vital in the first world; they were secondary in the second world of terror bombing of cities.

The fascinating thing is that these two worlds existed side by side without jostling, not only in the Jeffries Report, but for more than two years following, in the statements of the Manhattan Project physicists. In nuclear weapons, [as noted above, Robert Oppenheimer said] in 1945, “the elements of surprise *and* of terror are as intrinsic to it as are the fissionable nuclei.” But if the element of terror were primary, the element of surprise would be important only insofar as it seconded the shock of terror visited on the population attacked. A city nuclear attack, unlike the surprise at Pearl Harbor which the physicists frequently cited, would ignore direct military targets, except as incidents or by-product.

It might destroy war industry and so prevent mobilization. But it would not prevent an already mobilized strategic force, separate from the victim's own cities, from retaliating against the attacker's cities and war-supporting industry. The temptation to aggression is then hard to see. "They are weapons," said Oppenheimer, "of aggression, of surprise, and of terror." There was a latent contradiction in the physicists' view.

It is this contradiction that Jacob Viner observed. It is easy for us to see it today. It was by no means easy then. Viner deserves great credit. By the same token, the physicists whose lack in this respect Viner observed nonetheless deserve high honors for having generated some of the basic issues and above all for having recognized that nuclear weapons were revolutionary. It is not, after all, surprising that they understood only a small part of what this revolution meant. They did not see that if nuclear forces were, as they assumed, safe from nuclear attack, surprise was by definition of little advantage. Neither did they pursue the line of analysis suggested by the machine gunners in a small room. The analogy is notable precisely because in it the gunners are not safe and there is no distinction between the safety of the "population" and the retaliatory force. The scattered insights of the Manhattan Project physicists did not penetrate any significant distance into the possibility that nuclear forces themselves were not easily made safe from one another, and that in *their* case being surprised might be fatal. However, Viner and the other social scientists and historians in the late 1940s did not see this either, and in fact they were in some ways further from seeing it than the physicists because they followed only the "unattacked-retaliatory-force" branch of the physicists' thought, with its implicit relative optimism.

THE SOCIAL SCIENTISTS AND HISTORIANS

Viner's extraordinary paper on "The Implications of the Atomic Bomb for International Relations" took off from the physicists' assumptions that nuclear weapons were city-destroyers, but rejected their apocalyptic conclusion — since

the atomic bomb, unlike battleships, artillery, airplanes, and soldiers, is not an effective weapon against its own kind . . . it does not much matter strategically how much more efficient the atomic bomb can become provided

superiority in efficiency affects chiefly the fineness of the dust to which it reduces the city upon which it is dropped. . . . There seems to be universal agreement that under atomic-bomb warfare there would be a new and tremendous advantage in being first to attack and that the atomic bomb therefore gives a greater advantage than ever to the aggressor. I nevertheless remain unconvinced. . . . What difference will it then make whether it was country A which had its cities destroyed at 9 a.m. and country B which had its cities destroyed at 12 a.m., or the other way round?

Viner read his paper on November 16, 1945.¹⁷ He must have written it only a couple of months after he first heard of the bomb when it exploded over Hiroshima, and on all counts this paper, to which Brodie and Fox acknowledged their indebtedness, must be seen as one of the landmarks in the history of the development of strategic doctrine.¹⁸

What is more, Viner not only detected one crucial strand of inconsistency in the strategic thinking of the Manhattan Project scientists; he brought to the political issues a kind of sophisticated awareness of the character of the international system which was quite beyond the physicists. His remarks on the dim prospects of early world government are the sort of thing that one might have expected from a distinguished student of both international relations and international economics. And Bernard Brodie and William Fox, and several others of like training, made very important similar points, points that were very rare at the time. There are many other matters of interest in Viner. Viner's is the first, and in some ways still the best, statement of Pierre Gallois's position on the stabilizing effect of the spread of nuclear weapons.¹⁹ Wrong, I think. Its error is, of course, pardonable in November 1945; it flowed from the fundamental assumption of great stability because of the automatic or nearly automatic invulnerability of strategic forces, and from a belief that they would therefore be "equalizers," restoring in fact essential features of the 18th and 19th century international system.

Viner's insights were limited by the scant information he had derived from the physicists. He was aware of this and said specifically that he was working with "a few facts and a few surmises about the military effectiveness and the cost of atomic bombs": information that he deliberately exposed to his audience,

including many of the most famous physicists associated with the Manhattan Project.

The bomb has a minimum size, and in this size it is, and will remain, too expensive—or too scarce, whether expensive or not—to be used against minor targets. Its targets therefore must be primarily cities, and its military effectiveness must reside primarily in its capacity to destroy urban population and productive facilities. Under atomic bomb warfare, the soldier in the army would be safer than his wife and children in their urban home.

In this set of assumptions and in drawing inferences from them, Viner observed one inconsistency of the physicists but shared some inconsistencies with the physicists. He assumed that the bomb would be too expensive for even the superpowers to acquire enough of them to use against targets other than cities. Yet he assumed that they would be cheap enough so that even small powers could acquire them in substantial numbers. (In fact, the physicists sometimes explicitly talked of the bomb as cheap, especially when they were stressing the dangers of the spread of nuclear weapons.) But the principal upshot of Viner's analysis was to suggest that nuclear weapons would, *in the nature of the case*, be rather stabilizing, that they would *reduce* the importance of surprise and restore military significance to the weaker countries.

Viner has one sentence that refers in passing to the possibility of atomic or other attack on nuclear forces. But his perfunctory dismissal of this possibility is entirely characteristic and displays as much as anything else how far he was from recognizing the essentials of surprise nuclear attack. He says, "No country possessing atomic bombs will be foolish enough to concentrate either its bomb-production and bomb-throwing facilities or its bomb stockpiles at a small number of spots vulnerable to atomic bomb or other modes of attack." In fact, a policy of simply multiplying the number of points containing these nuclear facilities could hardly hope to match the means of destroying these facilities, among other reasons because such simple multiplication if very extensive is very costly. However, if Viner did not think seriously of the problem of nuclear attack on nuclear forces at that time, it is hard to find anyone else who did.

Brodie starts from Viner's notion that since a nuclear exchange would be directed at cities and industry, the element of surprise is not as important as the physicists assumed. In fact, he cites Viner as having first suggested and elaborated the idea (see pp. 73, 74 of *The Absolute Weapon*).²⁰ And, as he says, his paper is plainly in debt to Viner in numerous ways. Like Viner, he is thinking of nuclear weapons as being primarily directed at cities and industry, and for much the same reason.

The enormous concentration of power in the individual bomb, irreducible below a certain high limit except through deliberate and purposeless wastage of efficiency, is such as to demand for the full realization of that power targets in which the enemy's basic strength is comparably concentrated. Thus, the city is a made-to-order target, and the degree of urbanization of a country furnishes a rough index of its relative vulnerability to the atomic bomb (p. 99).

His First Postulate, in the preceding chapter, reads that:

The power of the present bomb is such that any *city* in the world can be effectively destroyed by one to ten bombs . . . (p. 24, emphasis added).

Any damage done to a retaliatory capability he thinks of as a by-product of the nuclear attack which the aggressor would have directed at cities. This is plain on pp. 88 and 89, but at many points elsewhere. And he is thinking of the problem of retaliation essentially as that of maintaining the nuclear retaliatory force in isolation from the disaster areas that the cities would become under nuclear attack; and of protecting it [the retaliatory force] from conventional ground forces.

The ability to fight back after an atomic bomb attack will depend on the degree to which the armed forces have made themselves independent of the urban communities and their industries for supply and support. The proposition just made is the basic proposition of atomic bomb warfare. . . . (p. 88, emphasis in the original).

In fact, Brodie considers and dismisses the “private arguments” of “certain scientists” that nuclear attack on nuclear launch sites might be effective without ground force seizure of the launch sites.

Certain scientists have argued privately that . . . a nation committing aggression with atomic bombs would have so paralyzed its opponent as to make invasion wholly superfluous. It might be alleged that such an argument does not give due credit to the atomic bomb, since it neglects the necessity of preventing or minimizing retaliation in kind. If the experience with the V-1 and V-2 launching sites in World War II means anything at all, it indicates that only occupation of such sites will finally prevent their being used. Perhaps the greater destructiveness of the atomic bomb as compared with the bombs used against V-1 and V-2 sites will make an essential difference in this respect, but it should be remembered that thousands of tons of bombs were dropped on those sites (pp. 91 and 92).

However,

An invasion designed to prevent large-scale retaliation with atomic bombs to any considerable degree would have to be incredibly swift and sufficiently powerful to overwhelm instantly any opposition. Moreover, it would have to descend in one fell swoop upon points scattered throughout the length and breadth of the enemy territory. The question arises whether such an operation is possible, especially across broad water barriers, against any great power which is not completely asleep and which has sizable armed forces at its disposal (pp. 92 and 93).

And

The invasion and occupation of a great country solely or even chiefly by air would be an incredibly difficult task even if one assumes a minimum of air opposition (p. 93).

Brodie regarded ground force occupation of strategic air bases as necessary to prevent retaliation, but infeasible. However, he

regarded ground force invasion as both feasible and necessary to consolidate the effects of atomic bombardment of cities and industry. Much the same view is reflected in the official Air Force position expressed by General H. H. Arnold at about that time.²¹

The realistic insights of Viner, Brodie, and Fox are best appreciated as a contrast with the utopianism of the scientists at the end of the war. The Manhattan Project physicists (and a good many others who knew about the Manhattan Project early and felt that it had revolutionary implications for warfare) believed that it made both necessary and possible a revolutionary change in international relations. They were thinking of something like world government, or at least very extensive international control, and frequently said that it was feasible just because it was urgent and necessary. The apocalyptic predictions they made tended, therefore, to have a hortatory character. They were appeals for a soul change in world statesmen. Publicists like Norman Cousins in his *Modern Man Is Obsolete* accepted the essentials of their apocalyptic view.²² Viner, Brodie, and Fox were particularly discerning and incisive in their perception that, on one hand, ways of organizing the world for perfect peace were not then available, nor would be in the foreseeable future, and that, on the other hand, the alternative of nuclear annihilation was not inevitable, that there were some elements of stability implicit in the scientists' own picture of nuclear relations, or rather in one of their pictures.

In sum, Viner, Brodie, and Fox made many cogent points of great importance. But none seriously considered the problem of designing a nuclear force to survive a major nuclear surprise attack, nor did they show any awareness that this was a problem at all, much less a basic one. In fact, they were further from seeing this than *some* of the scientists — inconsistently to be sure, in writing and in "private arguments" — were at least *some* of the time.

THE MILITARY VIEWS AND THE MILITARY STANCE

As I have mentioned, the Manhattan Project physicists, once they had abandoned hope for early agreement on international control of atomic energy, tended to line up with one faction or another of the military. After the Russians turned down the Baruch proposal, some physicists, like Edward Teller, thought about fusion weapons and improvements of the strategic offense. Many more, like Bush, Oppenheimer, Rabinowitch, and Rabi,²³

turned to the defense of cities and the problems of battlefield war. Project East River considered civil defense. The Lincoln Summer Study focused on the active defense of the industrial heartland of the United States and on providing early warning for fighter interceptors.²⁴ Project Vista proposed battlefield nuclear weapons for the defense of Europe. It is familiar now that the factional disputes among the scientists, and the corresponding ones within and among the services, were bitter and destructive. Perhaps the most fascinating aspect of these disputes (one that has not been observed) is their total neglect of the increasingly serious problem of the vulnerability of strategic forces, of the problem of obtaining a defended offense. This neglect affected both the military and the scientists, including all the principal factions of each.

The service positions on the A-bomb in the immediate postwar period were predictable. The War Department held that the A-bomb "has given the offensive a marked advantage, at least for the time being, over the defensive." (*Bulletin of the Atomic Scientists*, June 1947, reprinting *Army Navy Journal* for April 12, 1947.) The Navy Department, on the other hand, had it that "the present technological trend is decidedly in favor of the defense." It "decidedly favors the defense of large centers of population and industry." (*Bulletin of the Atomic Scientists*, July 1947.) This disagreement deepened and culminated in the B-36 Hearings at the end of the decade, where the Navy said that the bomb had little chance of getting through and that it would do little harm if it did. (I mentioned the brave naval officer who said one could stand at one end of the runway at Washington National Airport "with no more protection than the clothes you now have on, and have an atom bomb explode at the other end of the runway without serious injury to you.")²⁵ And LeMay²⁶ affirmed that the bomber always gets through.

However, the Navy never brought up the subject of the liability of nuclear bombers to be destroyed before takeoff on the ground by enemy nuclear bombers; and neither did the War Department, nor its Air Force split-off. General Arnold (then Air Force Chief of Staff) early in 1946 argued for the possibility, though not the certainty, of an atomic stalemate through mutual fear:

Now the arguments given above are not intended to comfort us with the thought that, if all nations had atomic weapons no nation would use them for fear of retaliation. All they show is that there is a *possibility* of

stalemate with respect to destruction of cities by atomic bombs (*One World or None*, p. 32, emphasis in the original).

He *was* thinking of cities, though with the usual ambiguities about industrial support of military strength, and the “will to resist” (see p. 27). His view was less downright and abstract, though not unlike that of Viner:

Our defense can only be a counteroffensive; we must be prepared to give as good as we take or better. Should we ever find ourselves facing an aggressor who could destroy our industrial machine without having his destroyed in turn, our defeat would be assured. Thus our first defense is the ability to retaliate even after receiving the hardest blow the enemy can deliver. This means weapons in adequate numbers strategically distributed so that no enemy is better situated to strike our industry than we are to strike his (*One World or None*, p. 31).

The war would be an exchange of blows against cities and industry.

I had intended to describe in some detail the characteristic developments in the nuclear doctrines of each of the services. Unfortunately, there isn't time for that. Nor is there time to say much on the history of the actual plans and operations of the Strategic Air Command and the Air Defense Command.²⁷ However, I will say a little about actual deployments, operations, and plans for most of the 1950s.

A rough way to characterize the nuclear *offense* stance is to say that it was focused on the problem of coordinating an immense attack capable of penetrating Russian area and local active defenses in order to deliver a decisive blow, primarily to the industrial heartland of the Soviet Union. The planning for this was ingenious and efficient—given time to get the attack under way undisturbed. And almost the only sorts of disturbance that had been seriously considered were those that might have been by-products of a Russian attack on American cities, or sabotage, or conventional ground attack. Such “by-product” disturbances to SAC were, correctly, not anticipated to be large or extremely difficult to overcome. In this respect, the active offense stance reflected a view similar to that of Viner and Brodie.

A rough way to characterize the *defense* stance against nuclear attack would parallel this focus of the offense force on the enemy active defense of the enemy heartland. Our defense was focused on the problem of intercepting Russian bombers before they reached the bomb release line over American cities and war-supporting industry. The contiguous radars were deployed primarily in the Northeast industrial heartland and near our coastal cities. Though they had a variety of problems, including that of saturation by a large raid employing electronic countermeasures, the radar and air defense bomber system was able to detect and track bombers and guide fighters toward the interception of a massive raid in particular.

Our offense and our defense stances changed over time as our own and the Russian stocks of nuclear weapons swelled. But in some essentials they changed not at all. Viner, an excellent economist, had derived from the physicists and chemists in 1945 the assumption that A-bombs would always be expensive and scarce because fissile material was scarce. (Eugene Rabinowitch's writings at the time offer examples.) However, an elementary economic operation—raising the price of uranium—offered incentives to a great many uranium prospectors and it soon became clear that bomb stockpiles could be greatly expanded and that there were bombs enough for military targets in addition to cities and industry. As our stockpile expanded, military targets, including strategic bases, were added to our attack plans. And in a symmetrical way the Air Defense Command assumed that with expanding Russian stockpiles a massive Russian attack directed at the American industrial heartland would add on some bombs and bomb carriers directed at our nuclear force.²⁸ However, in both cases these extra targets were attachments to attacks directed basically at cities and industry. This was a quite natural way to see the problem, given the history of views I have already outlined, but it is important to note that it had a critical effect on the chance that the vulnerability of SAC would be observed. For the U.S. defenders anticipated a massive Russian attack of anywhere from 500 to over 1000 bombers directed at cities and industry and using techniques of saturation rather than the methods of minimizing warning possible for a smaller force directed solely or mainly at SAC.²⁹ So massive a Russian attack was likely to provide strategic warning and would quite reliably have given extended tactical warning—enough tactical warning, perhaps, to be useful to even a very ponderous and complex strategic force. This was by no

means true, however, for an adversary who designed his first raid specifically to disrupt and destroy a strategic retaliation. Surprise turned out to be as important as the Manhattan Project physicists had assumed, but for very different reasons.

SAC bases, unfortunately, were located primarily outside the radar cover that had been designed for the defense of cities and industries: they were mainly not in the Northeast, but in the South and West, where the flying weather for training is good, and for the most part where they would not be engulfed in a disaster of the cities. The bombers and tankers were concentrated on a few crowded and shelterless bases (some holding a total of about 120 bombers and tankers). The bases in the continental United States expanded slowly in number, reaching about 28 or 29 in 1956. Other, equally indispensable elements of the force, such as the stockpiles of bombs and command and control, were even more concentrated. The bombers normally were stripped down and in maintenance, a state that enabled very high availability rates, given notice of a day or two, but extremely low readiness for the first six hours after receiving warning. However, even an improved warning network, designed specifically for the protection of SAC, could not have assured anything like that much warning. But the warning network had been designed primarily for the protection of cities and industry. If the strategic force could have survived a modest attack on its home bases, the plans called for an immensely complex operation of coordinating slow tankers and bombers, picking up [nuclear ordnance] at bomb stockpile sites generally far from the home base, and finally deploying to overseas bases, which were far more vulnerable than the home bases left behind. Fred Hoffman remarked during the Base Study that the problem of the analyst looking soberly at the vulnerability of SAC sometimes boiled down to propping SAC up over one barrier so that it could be knocked down at the next, so many were the alternative, entirely feasible, ways of destroying it.

At several points in your paper you suggest that the Russians had no capability for attacking the United States until rather late—until after they had acquired a stock of thermonuclear weapons, or after they had acquired very long-range aircraft or possibly after Sputnik and the intercontinental missile. In fact, before 1955 the Russians had enough planes with adequate range and enough bombs with adequate yield to have done a great deal of damage to American cities, if not intercepted. This was understood and displayed in all of the intelligence estimates during a period when

intelligence generally underestimated the Russians. *Even more important, only a fraction of their estimated capability was enough to dispose of SAC.*

Finally, the Russian force itself was even more concentrated and vulnerable than the American strategic force. Neither side had a second-strike capability, in the sense in which I defined it.

I said at the meeting in Oxford that the vulnerability of SAC had nothing to do with Air Force stupidity, or folly, or anything of the sort. Nuclear weapons were new; their implications were little understood; and the strategic force planners tended to examine the meaning of nuclear weapons to see how they affected the answers to the questions of strategic bombing as these questions had been understood previously. (See my comments in "Analysis and Design of Conflict Systems," pp. 109ff and 125ff.)³⁰ Moreover, the Navy and ground Army themselves failed to see that nuclear weapons raised new questions of the vulnerability of retaliatory forces before launching. And the limitation was not simply military; it affected natural and social scientists as well. Nor were these matters obvious to able systems analysts, who had access to data and worked on other closely related questions.

SYSTEMS ANALYSTS

At the end of the 1940s and in the early 1950s there were a good many analysts working in operational research organizations attached to the Strategic Air Command or the Air Defense Command. They dealt with important but relatively restricted questions that had to be answered to improve decisions by the operational commanders: questions such as techniques for the offense for penetrating defenses; alternative ways of releasing weapons over target, such as high altitude versus low altitude bomb release; and techniques for the defense system for sifting out potentially hostile attacks from the normal air traffic patterns displayed on radar. For this purpose they used actual data on the performance of men and equipment and the actual detailed geography.

At Rand this sort of study was extended to include a much wider and longer range of choices, involving choices among equipments that would be available several years hence and that would alter significantly current operational performance, such as speed, altitude, range of bomber aircraft, performance of defense radars, and a host of other matters. Some of these studies were

excellent. The systems studies for active defense led by Barlow and Digby (R-227 in 1951 and R-250 in 1953)³¹ were particularly impressive. Impressive and serious treatments of the functioning of immensely complicated systems of interdependent elements were done in very realistic and objective fashion on the basis of a very large effort by many researchers closely aware of current military operations as well as of impending changes in the state of the art. These persistent and careful efforts contrasted greatly with crash campaigns like the Lincoln Summer Study, which exploited the famous names of Manhattan Project physicists to sell some gadgetry such as the DEW line and the Whirlwind computer, or later the SAGE system, as handy-dandy solutions to the problem of getting nearly perfect active defense of cities and industry. If the subject of this letter were the problem of limiting damage in case deterrence fails, there would be a good deal to say about the Barlow-Digby study. Moreover, unlike minimum deterrence theorists, I regard active defense as a subject of continuing interest.

However, I am dealing here mainly with the development of our understanding of problems of stable deterrence. The most important observation to be made in this respect about the offense bombing systems analyses—such as that of Quade-Shamberg-Specht, *A Comparison of Airplane Systems for Strategic Bombing*, September 1950 (R-208)³² and the defense systems analyses led by Barlow and Digby—is that, as far as the problems of deterrence and retaliation were concerned, these studies exhibited exactly the same tunnel vision as did the military plans and the informal utterances and essays of the natural and social scientists. The offense systems analyses examined systematically alternative equipments and methods for American bombers to penetrate Russia's defense of her heartland. They matched American bombers against Russian fighters and surface-to-air missiles. The defense systems analyses, on the other hand, essentially matched Russian bombers against American interceptors and local defenses; moreover, even when they added our SAC bases in the United States to the Russian list of offense targets, as in the case of our military plans, this was done simply as a perturbation of an attack directed essentially at crippling population and industry. The defense analyses never therefore considered attacks specifically designed for the purpose of surprising and destroying the strategic force.

The systems analysis embodied in the Base Study addressed that problem. It observed that surprise had a different and greater

significance for the possible nuclear destruction of the retaliatory force than it did for an attack on cities and industry.

The advantages of mounting the first surprise attack of a war (little or no warning of city populations, confusion of defenses) have been generally recognized. The surprise attack is doubly important for attack on strategic bases, since many of the most vital and vulnerable elements on these bases are mobile, and, if the attack comes as no surprise, aircraft, personnel and essential material may have been evacuated from the bases before bomb release. . . . The surprise attack, large or small scale, must be regarded as a major threat to SAC survival (p. 233).³³

Exploiting the information, expertise, and methods that had been developed in the offense and defense analyses, it matched enemy offense and defense against our own offense and defense in potential attacks designed specifically to destroy an inadequately defended offense. The results were a shock. The authors knew the results in a preliminary way by the start of 1952 but spent the following fifteen months systematically checking and testing the conclusions, as well as refining them and designing improvements. When I briefed the results internally to the Rand Management Committee at the end of 1953, even though there had been quite a few rumors and preliminary indications, the shock was quite as great. Though the results seem painfully obvious now and were overwhelmingly evidenced then, the fact that the study caused this shock suggests how completely the prior strategic focus and assumptions had precluded an understanding of how hard it was to design a strategic force capable of surviving a nuclear attack directed at its destruction. For the same reason, the results of the study had to be briefed over 90 times to the military and particularly to audiences of specialists in related plans and operations.

There are a few observations worth making. First, these studies deliberately understated the vulnerability of the programmed systems. R-244S and R-266³⁴ showed the deadly results of attacks using as few as 30 bombs of 40-kiloton yield, at a time when intelligence estimated that the Russians would have 400 bombs, many in the megaton range. Against the programmed system it employed mostly medium-range Russian piston-engine bombers, the TU-4, modeled on our own B-29 and B-50. These were times

when intelligence, moreover, was underestimating the Russians. (After Sputnik, intelligence frequently went to the opposite pole; but before that it underestimated how rapidly the Russians would get the A-bomb, jet-fighters, the H-bomb, long-range turbo jet and turbo-prop bombers, and how rapidly they would expand their stockpile of weapons.) The conservatism is also illustrated by comparing the forces presumed by the Base Study for 1956 with those actually revealed by intelligence in 1956 to be operational. This comparison can be made by examining R-290's section on "current vulnerability" (that is, 1956 vulnerability).³⁵ It can also be shown by comparing the 1961 capabilities presumed in R-290 with the capabilities which were public knowledge by 1961. For example, the best accuracy assumed in R-290 to be available to the Russians for attacking the programmed strategic force in 1961 was 2 nautical miles (see p. 27). But President Eisenhower revealed before 1961 that the Russians and we had achieved accuracies of 1 mile. This makes quite a difference. The number of weapons needed to destroy a target varies essentially as the square of the median miss-distance. This means roughly that when circular error probable, or CEP (that is, median miss-distance), is halved, the salvo needed for destruction is divided by four. The curve on p. 27 with this adjustment looks even worse. But R-290 showed that it was feasible to destroy the force programmed for 1961 using only *manned aircraft*. The results in no way depended on a "missile gap." The entire method of both studies, however, was to show that even with the most favorable assumptions for our side, the situation was extremely bad. We therefore omitted in the printed version and for large audiences some even more extreme vulnerabilities.

Third, we studied attacks on all nuclear forces capable of retaliation, including carrier task forces. (See pp. 11ff and 30ff of R-290.)

Fourth, the data and the reasoning of the study were subjected to intensive review by experienced military officers, who were by no means eager to accept these painful conclusions. This was done not only in the course of the long series of briefings for the specialists but during months of examination by an ad hoc committee of the Air Staff which included as members officers from Plans, Logistics, Operations, and other parts of the Air Force. Even if we had been able to guess *a priori* the results of the study, we would never have been able to persuade any substantial number of military men whose *a priori* guesses had been quite the contrary.

But finally an examination of the Base Study, and of the study that followed it after another three years of work, should make clear that *a priori* reasoning on these matters could hardly hope to yield convincing conclusions.

When I suggest that a systems analysis was necessary, I am referring not so much to the sort of training in a specific traditional discipline that was required; I refer rather to a kind of activity or function. The systems analysts in studying the potential interactions of military forces to aid military decision, used extensive data on peacetime operations and logistics, data on the actual geographical and temporal distribution of forces and equipment, and data derived from state-of-the-art studies and theoretical analyses of equipment design, including data both for ourselves and for our adversary. Some first-class systems analysts, like Jim Digby and Ed Barlow, were electronic engineers; or like Robert Lutz, a co-author of the Base Study, an aeronautical engineer; like Bruno Augenstein, a physicist. Some were mathematicians, like Ed Quade, or mathematical statisticians, like Andy Marshall, or mathematical logicians, like myself and Norman Dalkey. Some had training in more than one discipline; Marshall and I had worked in economics, Marshall also had done work in physics, and I in industrial engineering. Harry Rowen was trained in engineering and economics. Or, like Fred Hoffman, they were trained mainly in economics. Economists played a key role. But at least one sociologist-demographer, Fred Iklé, did a partial systems analysis of great importance on the problem of reducing the chance of nuclear accidents and unauthorized acts. Bill Kaufmann, so far as I know, is the only political scientist with traditional training who undertook and successfully executed a concrete analysis of the potential operations of actual military systems, and he did this around 1960. It is not strictly correct to contrast systems analysts with physicists or social scientists or engineers. Some of each of these have been systems analysts.³⁶

What these men had in common is that they were dealing with actual operational, design, and plans data. They were not basing evaluations on simple models and *a priori* guesses as to the performance of the interacting strategic offense and defense of both sides.

This line of attack stemmed from operational research during World War II. Pat Blackett deserves an honored place as progenitor of this method in his work during World War II, along with Harold Larnder and several others in the United Kingdom and in the United States.³⁷ He used methods that later were greatly

extended for the more complex military decisions on equipment and operations in the postwar period, when many more variables were open.

Blackett, in a well-known paper on operational research³⁸ written during World War II, gave some illustrations of why it was absurd to hope to reach reasonable conclusions on the typical problems of potential interactions among military forces unless one had access to operational data—data, he was constrained to point out, that even physicists working on secret research and development problems normally could not and did not have. He considered the case of the anti-U-boat operations by aircraft. “The yield of the operations . . . will depend at least on the following variables: *U-boats* – number operating, tactics, defensive strength, offensive armament, geographical distribution, state of training and morale of crews, efficiency of look-outs; *Aircraft* – number and duration of sorties, search tactics, height of patrol, attack tactics, bomb load, accuracy of bombing, geographical and temporal distribution, performance, camouflage of aircraft, performance of radar, site of training and fatigue of crews; *Weather Conditions* – state of the sea, cloud height and amount, visibility.” He concluded: “To attempt an *a priori* solution of this problem is clearly absurd.” One needs data.

But it is even more plainly absurd to suppose that one can determine *a priori* whether the American strategic force in the mid-1950s, say, was vulnerable to attack by a Russian strategic nuclear force, or whether the strategic force planned for the 1960s was likely to be easily destroyed. The absurdity is plainer because there are many more variables involved in a systems analysis of the problem of nuclear retaliation than the twenty-one listed by Blackett for the anti-U-boat operation. And, in fact, some of the individual *components* of the second-strike problem are of an order of complexity like that of the anti-U-boat case.

The point that Blackett made about the need for classified operational data for wartime operational research can easily be misunderstood, as can my similar point about the design and analysis of complex opposed systems in the postwar period. It might be taken as a sort of obscurantism, a suggestion that the people with “inside dope,” and only such dopesters, have sound conclusions about the critical, potential interactions of military forces in the period under discussion. However, that is not his meaning; nor is it mine.

First, one can have access to secret “inside information” and make no use of it—or make very poor use of it. Just as a library card or other access to the *Bibliothèque Nationale*, the British Museum, and the Library of Congress does not automatically assure that its holder has made a study of the historical documents they contain, or, if he has, that he has done it competently and written an able history; so a clearance providing access to secret operational as well as design data is very far from assuring that its holder has used such data in a competent, serious analysis. In fact, the frequent assurances by an electronics engineer such as Jerry Wiesner or a physicist such as Herbert York in October 1964 that no important technological changes were likely to affect the strategic offense or the strategic defense demonstrate that even design and development data, not to say operational complexities, may be ignored by people with access, especially when they have an ideological axe to grind.³⁹

They said this when it was already clear that multiple, independently aimed reentry vehicles and increased precision could work revolutionary changes in the offense, and that phased array radars, advances in the computer art, and new weapons effects that greatly increased the lethal volume of defense warheads, could revolutionize active defense against ICBMs. Moreover, these changes did not simply cancel each other, they affected the relations between large, sophisticated and small, less-advanced nuclear forces. Even more than such failures to use access to development data to anticipate technological changes, there are failures to analyze the strategic military consequences of such technological changes—even when participants in the strategic debate have, so to speak, their “library card,” that is, could get access to the data but do not go through the laborious analysis required. To me, it has been simply appalling how much of the debate proceeds in terms of the scholastic absurdities of *a priori* models, whether the debaters have access or do not. Among those who do not have access, Blackett has the smallest excuse for such *a priori* reasoning since, when not consumed by political passions, he knows better.

Second, the point against simple *a priori* models that pretend to cover interactions involving several dozen variables can be made in another, somewhat more explicit, way. No conclusion at all is possible except by picking values for the many variables involved. One has to determine the range, the speed, the altitude, the radar cross-section of the offense vehicles, their precision in

navigation and bomb delivery, the yield of the weapons, and many other matters on the offense side; and one must determine the location in space and time of the vehicles under attack, their degree of readiness, protection against blast and other weapons effects, etc. If one determines these values arbitrarily, one can get any conclusion desired. It will depend simply on the arbitrary choice. If one determines it by rumor—the rumor that the B-47s used three or four hundred bases (see Raymond Aron, *Paix et Guerre*),⁴⁰ or the rumor that one-third of SAC was armed, combat ready, and in the air at all times (see Patterson and Furniss, *NATO, A Critical Appraisal*),⁴¹ or, even more farfetched, that there was, in the 1950s, a continuous air-alert of short-range fighter bombers, as Blackett suggests—then one can emerge only with a conclusion as valid as the rumors themselves.⁴² All these and many other rumors, however, were quite false. One must agree with Blackett's original position that it is hopelessly absurd to judge the outcomes of such complex interactions without access to actual operational data, plans, and deployments. Such access is a necessary, though not a sufficient, condition for *concrete* judgments about the stability of nuclear deterrence at any particular time. There seems to me to be a very grave lack of understanding of this point today in the European and British discussions of strategy, not to say the American ones.

I do not by any means reject the importance of the more philosophical and conceptual analyses of strategy, but they are severely limited by a lack of empirical concreteness as to what they can say about the actual relations among opposing military forces in any given historical period. I am sure that as a historian, you find no difficulty in distinguishing essays on the philosophy of history by Isaiah Berlin or E. H. Carr or M. G. White, however valuable, from concrete historical studies such as your monumental work on the Franco-Prussian war, or Carr's history of Russia.⁴³

I would distinguish my own essays on matters of principle and basic concepts, such as "The Delicate Balance of Terror"⁴⁴—which was *not* about the vulnerability of strategic forces in 1958—from the detailed, empirical studies, consuming years for their completion, of the operations of deterrence in the 1950s, or the operations of deterrence in the late 1950s and the 1960s.

I say this even though the concepts elaborated in my own public essays were developed for the most part as working tools—e.g., the second-strike concept, the idea of deterrence as a matter of comparative risks, and the recognition that a stable

deterrence was feasible, but hard, and that its stability was subject to technological upset. When "The Delicate Balance..." stated that:

it is not fruitful to talk about the likelihood of general war without specifying the range of alternatives that are pressing on the aggressor and the strategic postures of both the Soviet bloc and the West. Deterrence is a matter of comparative risks. The balance is not automatic. First, since thermonuclear weapons give an enormous advantage to the aggressor, it takes great ingenuity and realism at any given level of nuclear technology to devise a stable equilibrium. And, second, this technology itself is changing with fantastic speed. Deterrence will require an urgent and continuing effort.⁴⁵

It reflected a concrete judgment made earlier in R-290, pp. 40-41:

The attacks described here, and many others studied, clearly indicate the present vulnerability of our strike force. They do not, of course, imply that a Russian attack is imminent. Nor do we think it is. That is a matter of Soviet intention rather than Soviet capability, and such intent would be affected in the first instance by Soviet knowledge of our vulnerability and in the second instance by the comparative gains and risks of alternatives to central war. Nonetheless it is a painful fact that the risks to the Soviets of attempting a surprise attack on the United States are much lower than are generally estimated. We would like this course of Soviet action to be a worse alternative to almost any other they might contemplate—including, for example, the acceptance of defeat in some limited or peripheral war. But the sober and careful scrutiny of the present vulnerability of our strike force to feasible Russian attacks, and realistic tests of the plans for its future defense, show the seriousness of the problem.

And the reference to the possibility of technological upset was not hypothetical. It was based on the fact that by the time the Base Study was finished and some, though not all, of its principal recommendations were accepted, I knew that it had no more than

seven years or so to run. In the 1960s, vehicles traveling 4 miles per second would make warning and alert measures inadequate. Fred Hoffman and I wrote "Defending A Strategic Force after 1960" (D-2270)⁴⁶ and put it out on February 1, 1954, as the first rough cut at the problem of protecting SAC in the ballistic missile era. It was the precursor of R-290, which was not issued for two and a half years, but this precursor of the second study showing the technological limits of the first study was put out before the final report of the first study was issued in April. Moreover, it proposed the system of hardening adopted for the 1960s, but foresaw that hardening would be enough for only a finite time—that in the 1970s precision was likely to have increased enough to make it inadequate even though still useful. (Today an ABM defense of hardened ballistic missiles seems a very likely way of maintaining stability of the deterrent in the 1970s, but that can be accepted or rejected only on the basis of a detailed system analysis.)

It is conceivable that these particular concepts might have been arrived at *a priori* but I'm rather skeptical. In any case, it should be plain from the history I have tried to document why the discovery of the vulnerability of SAC, the development of the first-strike/second-strike distinction, and the recognition of the feasible but limited and difficult stability of deterrence, owe substantially nothing to the strategic writings of the natural and social scientists. I was not familiar with these writings, and if I had been they could hardly have led me to make the conclusions that emerged from empirical study. I am afraid that your footnote 41, p. 15, and your paragraph beginning "Not until thermonuclear weapons . . ." on p. 6 are misleading.⁴⁷ The work at Rand that you refer to did not study the implications of Brodie's ideas. The work was quite unconscious of these early ideas of Brodie and Viner. Moreover, the study came to precisely the opposite conclusions from those implied by Viner and Brodie. The timing and direction of influence suggested in your footnote 41 and your p. 15 seem then in error. The analysis of the vulnerability of strategic forces was clear to the authors of the Base Study by the beginning of 1952, and the first summary printed report (R-244-S) was formally presented to the Air Force on March 1, 1953. Morgenstern, Schelling, and Brodie all had read, as consultants or staff members of Rand, some or all of the sequence of papers and reports on the subject.⁴⁸ This is by no means to minimize the great importance of Schelling's keen analysis of the relations of the problems of surprise attack, deterrence, and disarmament. His essay was an illuminating

example of the sorts of basic clarification that can proceed without new empirical effort on the foundation of intuition, common sense, and previous empirical work. But the discovery of the vulnerabilities of strategic forces owes its primary debt to the tradition of operational research and empirical systems analysis. Hence, the acknowledgments at the beginning of the Base Study to J. F. Digby, E. J. Barlow, E. S. Quade, P. M. Dadant, E. Reich, *et al.*⁴⁹ Because their contributions to strategy have been classified, they are largely unknown. This is true even of the important contributions of men like Fred Hoffman and Harry Rowen which are a little better known.⁵⁰ They are largely unsung heroes of strategy in the nuclear age.

I must apologize for the extreme length of this "letter." And for the corresponding length of time it has taken me to get it off. It is focused on one central problem, that of the stability of nuclear deterrence. Your paper quite rightly deals with many other problems besides this one. I hope, however, the material I have drawn from my lectures on this one subject will be useful.

ENDNOTES - Wohlstetter - On the Genesis of Nuclear Strategy

Note: Unbracketed endnotes are Wohlstetter's. Endnotes in square brackets were added in by April 1986 by James Digby and Arthur Steiner. Endnotes in double-square brackets were added in 2008 by Robert Zarate.

1. [These collections are now in the Joseph Regenstein Library at the University of Chicago, the Historian's office of the U.S. Department of Energy, and the U.S. National Archives.]

2. [In the Adelphi Paper version this becomes pp. 19ff.] [[See Michael Howard, "The Classical Strategists," Adelphi Paper No. 54, London, UK: Institute for Strategic Studies, 1969; and "The Classical Strategists," in Howard, ed., *Studies in Peace and War*, New York, NY: The Viking Press, 1971, pp. 154-183.]]

3. [P. M. S. Blackett, British Nobel Laureate physicist, pioneer in operational research, was author of *Fear, War, and the Bomb*, published in the United States by McGraw-Hill in 1949. Vannevar Bush, electrical engineer, was head of the Organization for Scientific Research and Development during World War II, making him, in effect, the nation's chief scientist for the war effort.

He wrote *Modern Arms and Free Men*, published by Simon and Schuster in 1949.]

4. [Refers to Jacob Viner, Chicago economist and specialist in international trade; Bernard Brodie, political scientist at Yale, 1945-51, and later at Rand; and William T. R. Fox, political scientist and associate of Brodie at Yale in the late 1940s, later at Columbia.]

5. [The title of a book edited by Dexter Masters and Katharine Way, *One World or None*, New York: McGraw-Hill, 1946.]

6. These documents are in the Atomic Energy Commission collection. See p. 329 of the Hewlett and Anderson official history. [Richard Hewlett and Oscar Anderson, *The New World: Vol. 1 of a History of the U.S. Atomic Energy Commission*, University Park: Pennsylvania State University Press, 1962. One of the three similar memoranda of that date, signed by Bush and James Conant, was published as an appendix to Martin J. Sherwin, *A World Destroyed*, New York: Knopf, 1975. The three original memoranda can now be found in the National Archives.]

7. [Adelphi version, p. 21. See editors' preface, above, for a note on how this was changed by Howard.]

8. [All signers of the Jeffries report were senior scientists at the Metallurgical Laboratory, University of Chicago. For background on the "Prospectus on Nucleonics," see Hewlett and Anderson, *op.cit.*, pp. 324-325, and Alice Kimball Smith, *A Peril and a Hope*, Chicago, IL: University of Chicago Press, 1965, pp. 19-24 and 539-559. Most of the text is reprinted in Smith, *ibid.*; the full text is in the National Archives.]

9. [Here Wohlstetter was referring to unpublished writings that are still not generally available.]

10. [The Franck Report, a report to the Secretary of War, is reprinted as an appendix in Smith, *op.cit.*, pp. 560-572. For background, see *ibid.*, pp. 41-52, and Arthur Steiner, "Baptism of the Atomic Scientists," *Bulletin of the Atomic Scientists*, February 1975, pp. 21-28.]

11. "Atomic Weapons," paper presented at a Symposium of the American Philosophical Society on Atomic Energy and Its Implications, Philadelphia, November 1945. [See *Proceedings of the American Philosophical Society*, Vol. 90, No. 1, January 29, 1946, The American Philosophical Society, Philadelphia, 1946.]

12. [Quoted in] Hewlett and Anderson, p. 358. [The Interim Committee was set up by Secretary of War Stimson in May 1945 to advise him on atomic energy policy. Its scientific panel was composed of Oppenheimer, Fermi, Lawrence, and Arthur H. Compton. The minutes of the meeting that included the Scientific Panel have been published as an appendix in Sherwin, *op.cit.* The original is in the National Archives.]

13. [The quoted passage is from *Atomic Energy, Hearings on S. Res. 179*, Special Committee on Atomic Energy, U.S. Senate, 79th Congress, First Session, Part I, U.S. Government Printing Office, 1946.]

14. Sometimes 4 [square] miles was estimated for total destruction. [The U.S. Strategic Bombing Survey report, *The Effects of Atomic Bombs on Hiroshima and Nagasaki*, U.S. Government Printing Office, 1946, pp. 3 and 30, cites "4.4 sq mi which were almost completely burned out" at Hiroshima and 9.9 sq mi as the area within which wood frame buildings were damaged at Nagasaki.]

15. [Wigner was a Manhattan Project physicist, later a Nobel Laureate.]

16. [Albert Wohlstetter, Henry S. Rowen, Robert Lutz, and Fred Hoffman, *Selection and Use of Strategic Air Bases*, Rand Report R-266, 1954 (declassified in 1962). Publicly released in 1985, a number of copies have been in scholarly hands for some time; for a description of its methodology, see E. S. Quade, *Analysis for Military Decisions*, Chicago: Rand McNally, 1966, Chap. 3, pp. 24-63. See also Bruce L. R. Smith, *The RAND Corporation*, Cambridge, MA: Harvard University Press, 1966, Chapt. VI.]

17. At the same symposium that included Oppenheimer's paper on "Atomic Weapons," and also a short, less explicit version by Langmuir of his four-stage atomic arms race. ["The Implications of the Atomic Bomb for International Relations,"

Proceedings of the American Philosophical Society, Vol. 90, No. 1, January 1946. Viner's paper is also available in his *International Economics*, Glencoe, IL: The Free Press, 1951, pp. 300-309.]

18. Not that it was unanticipated. The University of Chicago scientists had met on September 20, 1945, and concluded that "The atomic bomb makes surprise an unimportant element of warfare. Retaliation in equal terms is unavoidable, and in this sense the atomic bomb is a war deterrent, a peace-making force" (Box 28, Folder 25, Harper Collection). Viner, however, unlike the physicists, noticed the inconsistency with some of the principal themes these same physicists were advancing at the time. [Viner attended the conference and read an early version of his paper. The Harper Collection is now in the Regenstein Library, University of Chicago.]

19. [Gallois retired from the French air force and became an advocate of a French nuclear capability. For a summary of his early views, see Howard's Adelphi version, p. 29.]

20. [Bernard Brodie, ed., *The Absolute Weapon: Atomic Power and World Order*, New York: Harcourt, Brace, 1946. Brodie contributed Chaps. I and II, pp. 21-107.]

21. [See Masters and Way, *op.cit.*, pp. 26-32.]

22. [See Norman Cousins's editorial in *The Saturday Review of Literature*, August 18, 1945, pp. 5-9. Cousins's editorial preceded the scientists' public statements.]

23. [I.I. Rabi, Nobel Laureate physicist, was a consultant to the Manhattan Project and a member of many governmental advisory committees after World War II.]

24. East River and Lincoln came at the start of the 1950s and were accompanied and followed by a great flurry of concern in the universities and in the intellectual community about urban defense. It was reflected in the *Bulletin of the Atomic Scientists* and in the liberal and popular magazines and newspapers at the time, and it culminated in the disasters of the Oppenheimer Hearings. [U.S. Atomic Energy Commission. *In the Matter of J. Robert Oppenheimer*, MIT Press, Cambridge, MA, 1971. See index

for many references to Lincoln, East River, and Vista.] [[Digby and Steiner's index is not included in this edited volume.]] Your statement, p. 14, about the timing of the public concern is almost the reverse of what actually happened. Sputnik occurred near the end of the campaign for civil defense by the MIT, Harvard, Cal Tech, and etc., faculty members, who then switched back to the notion that there was no defense and that defense indeed might be destabilizing. Teller and others tended to change places with their physicist opponents in a kind of minuet. But they never commanded the support of the intellectual community that Oppenheimer, Bethe, Rabi, Rabinowitch, *et al.*, had. [The reference to p. 14 was to material on p. 26, Adelphi version.]

25. Statement of Eugene Tatom, Commander, U.S. Navy. *The National Defense Program-Unification and Strategy*, Hearings before the Committee on Armed Services, House of Representatives, 81st Congress, First Session, October 1949, U.S. Government Printing Office, Washington, 1949, p. 170. Compare Blackett, a Navy opponent of strategic bombing, in 1948: "The power of human beings to 'stick it' is immense; a determined folk will learn to stand atomic bombardment, if that is their fate, just as Germans learnt to stand ordinary bombing on a scale up to fifty times larger than that which the enthusiasts for strategic bombing thought would bring about the collapse of their war effort." (*Military and Political Consequences of Atomic Energy*, London, 1948, p. 56.) Blackett, of course, was also then a supporter of the Soviet position on atomic energy at the United Nations.

26. [General Curtis E. LeMay, former Commander-in-Chief, Strategic Air Command and Chief of Staff of the Air Force during the early 1960s. LeMay was noted for his enthusiastic advocacy of long-range strategic bombers.]

27. [In recent years, several secondary sources have explored the declassified original documents. See David Alan Rosenberg, "The Origins of Overkill," *International Security*, Spring 1983, Vol. 7, No. 4, pp. 3-71. See also Fred Kaplan, *The Wizards of Armageddon*, New York: Simon & Schuster, 1983. A complete annotated war plan of the late 1940s can be found in Anthony Cave Brown, *DROPSHOT*, New York: Dial Press/James Wade, 1978.]

28. [For example, in "Summary Evaluation of the Net Capability of the U.S.S.R. to Inflict Direct Injury on the United States up to July 1, 1955," NSC140/1 (May 1953), it is assumed that a mid-1955 attack by the Soviets would allocate 80 atomic weapons against the U.S. atomic air defensive capability worldwide, 151 against urban industrial targets, and hold 60 in reserve. Printed in U.S. Department of State, *Foreign Relations of the United States, 1952-1954*, Vol. II, part 1, Washington, U.S. Government Printing Office, 1984, pp. 328-349.)]

29. [For example, JCS 1924/76, October 30, 1953, in the National Archives, assumed a Soviet attack with 700 aircraft at the end of 1957.]

30. [The page numbers refer to the version included in E.S. Quade, ed., *Analysis for Military Decisions*, Chicago: Rand McNally and Company, 1964.]

31. [Edward J. Barlow and James F. Digby, eds., *Air Defense Study*, Report R-227, The Rand Corporation, Santa Monica, October 15, 1951; Edward J. Barlow, *Active Defense of the United States 1954-1960*, R-250 (Abridged), December 1, 1953. R-227 is not yet publicly released.]

32. [According to the Rand Publications Department, R-208 is not yet publicly released. Edward Quade was a mathematician, Richard Schamberg an aeronautical engineer, and Robert Specht a mathematician at Rand.]

33. [See R-266, *op.cit.*]

34. [R-244-S is not yet generally available. It reported in summary form the conclusions of Wohlstetter's team. For a discussion, see Bruce L.R. Smith, *op.cit.*, pp. 218-219. R-266, previously cited, was a more comprehensive report.]

35. [Albert Wohlstetter, Fred Hoffman, and H.S. Rowen, *Protecting U.S. Power To Strike Back in the 1950s and 1960s*, Rand Report R-290, The Rand Corporation, Santa Monica, 1956. This report is not yet generally available.]

36. [All of the people referred to in this paragraph were colleagues of Wohlstetter at Rand during much of the 1950s.]

37. [Harold Larnder was an English scientist who had worked on Britain's early radars before immigrating to Canada after World War II. He saw much of Rand analysts during the development of the North American Air Defense Command. Wohlstetter held him in especially high regard.]

38. [Patrick M. S. Blackett, "Operational Research," *The Advancement of Science*, Vol. V, No. 17, April 1948; reprinted in Blackett, *Studies of War*, Edinburgh: Oliver and Boyd, 1962, p. 188.]

39. [Jerome B. Wiesner and Herbert F. York, "National Security and the Nuclear Test Ban," *Scientific American*, October 1964, pp. 27-35.]

40. [Raymond Aron, *Peace and War*, trans. by Richard Howard and Annette Baker Fox, New York: Praeger, 1967, p. 422.]

41. [Gardner Patterson and Edgard S. Furniss, Jr., "NATO, a Critical Appraisal," Princeton University Conference on NATO, Princeton, June 1957, p. 32 (cited in "The Delicate Balance of Terror," *op. cit.*, p. 218).]

42. [Others may not interpret Blackett's words in quite the same way. Cf. Blackett, *Studies*, p. 133.]

43. [E. H. Carr was a British diplomat turned historian; Berlin is a British, and White an American, philosopher.]

44. [*Op. cit.*, pp. 211-234.]

45. [*Op. cit.*, p. 222.]

46. [This internal Rand document has not been formally released, but copies have been in the hands of some scholars. It is cited, for example, in Fred Kaplan, *The Wizards of Armageddon*, pp. 118-119. Wohlstetter himself quoted from this document in his testimony favoring the proposed "Safeguard" anti-ballistic missiles system. Today he continues to believe that stability would be enhanced by active defenses, and therefore supported proposals

for active anti-missile defenses. (See U.S. Senate, Committee on Armed Services, Hearings, *Authorization for Military Procurement Research, Fiscal Year 1971 and Reserve Strength*, Ninety-First Congress, Second Session, Part 3, May 19, 1970, pp. 2249-2250.) Herbert York, an opponent of Wohlstetter on the ABM issue, referred to D-2270 as "This remarkably prescient study" in *Race to Oblivion*, New York: Simon & Schuster, 1970, p. 183.]

47. [See pages 27 and 21, respectively, of the Adelphi version. Howard made no change in footnote 41. The paragraph on old page 6 was slightly changed to read ". . . the full implications and requirements of his ideas, and others current in the United States' academic community were to be exhaustively studied." (The italicized phrase was added in the Adelphi version.) In footnote 11, Howard added the sentence (referring to Brodie), "He did not, however, deal with the problem of vulnerability of retaliatory forces and the consequent dependence of stability on an effective second-strike capability."]

48. [Oskar Morgenstern was an economist at Princeton and a Rand consultant; Thomas Schelling, a Harvard economist, spent a year at Rand in 1959-1960. This led to his important book *The Strategy of Conflict*, Cambridge, MA: Harvard University Press, 1960.]

49. [Digby, Barlow, and Dadant were engineers at Rand who worked on air defense analyses in the early 1950s; Quade and Reich were mathematicians.]

50. [Hoffman and Rowen, both economists, were and are Wohlstetter's long-time collaborators.]