The Case for Strategic Force Defense (1969)

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THE ROLE OF ABM IN THE 1970'S

Since I believe the Safeguard program warrants the sums involved, and I support it, perhaps I should begin by saying that I am entirely sympathetic to a rigorous review of the Defense Budget. I favor getting our safety as cheaply as we can. Moreover, I believe the Defense Budget has a good deal of fat that can be cut without substantial harm. I would recommend, for example, a careful look at the equipment and support costs of our ground forces, and at our tactical air forces, both land and sea-based. Some of these seem ineffective, or leveled at threats that are poorly defined or not grave enough to be worth the cost.

Sensible efforts to reduce the Defense Budget, however, would not center on the strategic offense and defense force. There are, of course, arguable choices about strategic offense and defense. But the eight billion dollar plus strategic budget makes up a small part of the total Defense Budget. It has a paramount importance for the safety of the country and, indeed, of international society. Deterring nuclear coercion and nuclear attack on ourselves and our allies, [and] reducing the damage done in case deterrence fails, are complex and uncertain functions; but because they are crucial, the part of the Defense Budget devoted to them has been the most studied and is better understood than any of the rest.

Nonetheless, sizable uncertainties are intrinsic. They affect the predictions of scientists as well as the military and limit the reductions we can make without excessive risk. The strategic forces will need continuing adjustment to predicted and to some unanticipated changes in the state of the art. But such adjustments need not entail drastic changes up or down in long term levels of spending.

A start in deploying ABM [anti-ballistic missile defenses], I believe, is a prudent response to changes in the state of the art

available to ourselves and to our adversaries. As strategic systems go, it is a modest program. It is subject to review and can be halted or stretched out. The average annual cost of the completed program on a five year basis is less than one-fifth of what we were spending for active defense against manned bombers at the end of the 1950's. Nor is it at all likely to start a quantitative arms spiral. Indeed, despite the stereotype, there has been no quantitative arms race in the strategic offense and defense budget, no "ever-accelerating increase," nor, in fact, any long term increase at all. The budget for strategic offense and defense forces in fiscal 1962 was 11.3 billion dollars.[2] The proposed fiscal 1970 budget, as of June, comes to about 8 billion dollars. Adjusted for price changes, the 1962 figure was well over fifty percent higher than that for 1970, perhaps even as much as two-thirds higher.

There is an important difference between making qualitative adjustments to technical change and expanding the number of vehicles or megatons or dollars spent. The difference has been ignored in a debate on ABM that seems at the same time impassioned and very abstract, quite removed from the concrete political, economic, and military realities of nuclear offense and defense and their actual history. For example, one alternative to protecting Minuteman is to buy more Minutemen without protection. But adding new vehicles is costly and more destabilizing than an active defense of these hard points, since it increases the capacity to strike first. A one-sided self-denial of new technology can lead simply to multiplying our missiles and budgets, or to a decrease in safety, or to both.

Active defense against ballistic missiles in the 1970's will have an important role to play in maintaining a protected and responsible second-strike capacity. The projected Safeguard defense of the national command authority and of the bomber and Minuteman bases are directed to this end. And it has a useful function in providing an area defense against attacks involving modest numbers of apparent incoming missiles.

There have been so many charges that the Safeguard program was invented in bad faith in March of this year as a gimmick to answer critics of the Sentinel city defense that I would stress that in 1967, long before the present Administration quite independently decided on Safeguard, the evidence of advancing technology convinced me that ABM in the 1970's would have essentially the uses the Administration suggests for Safeguard, and in the same order: to defend the offense and, given this, at a small extra cost

to provide a light area defense of population.[3] In fact, there is a substantial continuity between the ABM decisions of the present and past Administrations. The last Administration called for an ABM area defense but said it would furnish an economic basis for defending Minuteman if the threat grew. It had been weighing and it continued to weigh this decision for some time—indeed itself requested some funds for hardpoint defense in its own version of the 1970 fiscal budget.

Like the Republicans now, the Democrats in 1967 were charged with directing their ABM decision against the opposing party. I would recommend to opponents of ABM that they contemplate the possibility that the decisions were made in good faith in both cases, and that we turn to the substance of the issues.

There are other political and military functions of an ABM system than protecting the offense and offering an area defense of civilians against light attack. I would like to say something about each of these two latter roles and also something about the doctrine of Minimum Deterrence on which much opposition to the ABM is based, but time permits comment mostly on the protected offense function.

ABM as a Part of a Second-Strike Force in the 1970's

For one superpower as against another, getting and keeping a responsible second-strike force is feasible but hard. It requires thought, effort, and continuing realistic adjustments to technological change. Minimum Deterrence theorists, who call for no defense of our civilians and nearly total reliance on a threat to bombard enemy civilians, have always claimed that the attacker inevitably must expend many strategic vehicles to destroy only one of the vehicles attacked. No such generalization holds. It has depended and always must depend on the changing capabilities of the offense and on the kind and degree of protection of the force attacked. At one time, for example, both we and the Russians had very many unprotected aircraft concentrated on a base within the lethal radius of a single bomb. On a two-wing base, for example, we had as many as one hundred thirty aircraft; on a one-wing base sixty-five medium bombers and tankers. And the planned response time was too slow for the reliable warning likely to be available. Small numbers of vehicles could have destroyed much larger numbers of the vehicles they attacked. Under some realistically determined conditions, the ratio would have favored

the attacker by one to eight or more. These vulnerabilities had nothing to do with the supposed missile gap. In fact they preceded such predictions.

There is always a temptation in such circumstances to resort to responses that are automatic or that bypass national command. Advocates of sole reliance on city bombardment forces have from the time this doctrine first gained currency been tempted to prove that response was certain by making it automatic, by shortcutting responsible political decision.[4] But the decision to launch ICBM's against Russian cities would be perhaps the most momentous choice ever made in all of history. It would be the decision for World War III. If this awful decision is ever made it should be based on as much information as we can get and it should be made by as high a political authority as possible. It is the last decision we should contemplate delegating to a computer.

The revival today, by several distinguished senators and some able physicists opposing ABM, of the suggestion that, rather than defend ICBM's [intercontinental ballistic missiles], we should launch them at Russian cities simply on the basis of radar represents a long step backward. If we were willing to do this, we would dispense with silos or Poseidon submarines or any other mode of protecting our missiles. And we would increase the nightmare possibility of nuclear war by mistake.

Understanding of the complex problems of designing a protected and responsible nuclear strategic force has grown slowly among scientists as well as laymen, civilians as well as soldiers, Democrats as well as Republicans. But it has grown, and decisively. The United States has designed and deployed a second-strike force capable of riding out an attack, and there have been large improvements in protecting responsible command. This was accomplished not by merely expanding nuclear bombardment forces, but in essence by shifting to forces with protection against the changing threat. The stereotype repeated throughout the 1960's that our security has declined while our strategic force grew at an accelerating rate is grossly wrong on both counts. In the past some key programs increased the protected second-strike capacity of the force, while cutting at the same time billions of dollars from the spending projected.

In the 1970's unless we continue to make appropriate decisions to meet technological change, once again the viability of a large part of our second-strike force will be put in question. Several related innovations, but in particular the development

of a rocket booster carrying many reentry vehicles each aimed precisely at a different target, raise once again the possibility of attack ratios favoring the attacker. One reentry vehicle may kill a booster carrying several. One booster can carry the means of destroying many boosters.

Raising a question about the future second-strike capacity of any part of our strategic force implies nothing about the present intentions of an adversary to strike first or even to be able in the future effectively to strike first. The recent debate on whether the Soviet missile, SS-9, is a "first-strike weapon" or whether the Russians intend it to be seems beside the point. If by maintaining our second-strike capability we can make the risks of striking very great, this can affect an adversary's intentions favorably to ourselves. It can deter him even in a crisis, like the one over missiles in Cuba, when the alternative to striking may look bad, but not, if we are careful, as bad as striking. Moreover, we ought not to talk of "first-strike weapons" and "second-strike weapons" as if this could be settled simply by looking at the weapons on one side. Whether or not a weapons system can preclude substantial retaliation will depend on many uncertain future performance characteristics of the forces on *both* sides. The test of whether one has a responsible second-strike capacity is whether one can, under nuclear attack, preserve vehicles, decision centers, and the flow of communications among them, whether one can transmit the order to retaliate and penetrate adversary defenses to reach targets. If we were unwilling even to entertain the hypothesis of a first-strike, we would do nothing to protect any part of our strategic forces or its control centers by making them mobile or hard or defended by ABM. Some leading scientists who oppose currently deploying ABM say they will favor it for the defense of Minuteman when precise MIRV's [multiple independently targetable reentry vehicles] and the related offense technologies are likely to be available to the Russians. That calendar date, and not present Soviet intent, is then a major substantive issue for these opponents. And their position recognizes that we want to maintain the second-strike capacity—not of just one, but of all major vehicle types in our strategic force: Minuteman, bombers, and Poseidon.

In designing a second-strike force, there are excellent reasons for making it a substantial mixture of vehicles of several quite different types: land as well as sea-based, manned as well as unmanned, each with its own mode of protection. Such systems

have differing limitations, are subject to varied and independent uncertainties, require distinct modes of attack and, if each type is protected, greatly complicate the attack. It is a serious matter, then, if a large part of this mixture is badly affected by changing adversary forces and technologies. The forces deployed and the state of the art available to the Russians will influence other parts of our strategic force than Minuteman silos. And ABM has a role to play, for example, in protecting the important fixed elements of a mobile force, including the politically responsible command centers. Preserving command, control and communications is always hard, and particularly so for mobile sea-based systems.

My remarks, however, center, so far as the second-strike function of ABM is concerned, on the problem of protecting Minuteman. We have good cause to preserve the second-strike capability of so large a proportion of our strategic force. Even if it were true that the United States needed only a few strategic vehicles surviving, buying and paying for the operation of a great many that had become vulnerable to attack would be a very poor way to obtain those few surviving. There are safer and cheaper ways of getting a force of a given size than to buy a much larger one, most of which is susceptible to annihilation.

How does the planned timing of our ABM deployment compare to the date when it is reasonably likely that Russian offense technology could badly worsen the effectiveness of our projected Minuteman III? The first point to note is that the proposed Safeguard deployment has extended lead times. It can stretch out further if continuing review of intelligence suggests it should, but the shortest schedule calls for completing this program early in 1976. If, as ABM opponents stress in other connections, there is likely to be a substantial shakedown period, we are talking of 1977 or later. If, as has been suggested, we delay decision for another year or more and then proceed to design and develop an entirely new ABM, we are talking of the 1980's.

Second, predicting exact calendar dates at which technologies will be available to adversaries and what their strategic significance will be is very hard, and we are not very good at it. Moreover, we have erred not only on the side of overestimating Russian capabilities, but often by underestimating them. At earlier dates we were surprised by the rapid Soviet achievement of the A-bomb, the H-bomb, advanced jet engines, long-range turboprop bombers, airborne intercept radars, and large-scale fissile-

material production. And scientists have been surprised, not only military men.[5]

Third, the public discussion has not stressed how sensitively the accuracy of attack affects the viability of the hardened force attacked. Accuracy affects the number of weapons required to destroy a hard target very much more than the bomb yield or the overpressure resistance of the target. Roughly speaking, for such targets, improving accuracy by a factor of slightly more than two is the same as increasing bomb yield tenfold and serves essentially to offset a tenfold increase in overpressure resistance.

I have tried to reconstruct various numerical proofs recently presented or distributed to the Congress that purport to show that Minuteman will be quite safe without any extra protection; these proofs depend heavily on optimistic estimates of limitations in Russian delivery accuracies, reliabilities, and associated offense capabilities and sometimes on very poor offense tactics.[6] Suppose, however, that by 1976 when Safeguard is deployed, or by 1977 when it may be shaken down, the Russians have:

1. accuracies like those of the systems we are deploying now[7]
2. over-all reliabilities currently attributable to them
3. methods familiar to us for using extensive and timely information as to which missiles have failed so that others can replace them
4. continued production of SS-9 boosters at past rates
5. modest numbers of MIRV's per booster (e.g., the three five-megaton reentry vehicles stated by Secretary Laird for the SS-9).

Then the percentage of the Minuteman force that would be destroyed, if undefended, comes to about ninety-five percent.

These results are based on quite moderate assumptions about Russian capabilities. Better accuracies, for example, may be expected in the late 1970's, and higher degrees of MIRVing. Reliabilities of any given offense missile system improve with use. Do those who favor a hardpoint defense but would postpone a start really consider these Russian capabilities I have outlined "extremely implausible"? Or at *all* implausible?

There is a striking inconsistency in the way ABM opponents treat the Chinese and the Russians. In contemplating the possibility of a Russian offense against our Minuteman, they assume that Russians who cannot by 1976 or 1977—twenty years after Sputnik—do what we know how to do now. When considering the

ability of the Chinese to penetrate an ABM defense, they attribute to them penetration systems that cost us many billions of dollars, a dozen years of trials and many failures to develop, and they assume this for the first generation Chinese missiles. These are rather backward Russians and very advanced Chinese. Moreover since in the Russian case we are considering a potential threat to our second-strike capability and we want this to be highly reliable, we want particularly to avoid underestimating the threat. But we should undertake a modest defense of population if it works in the expected case, even if on extremely pessimistic assumptions it might not. Here again it seems to me the ABM critics get things exactly backwards.

Finally, the fact that such impending developments in Russian offense may make it necessary to do something more to protect the fixed elements of our force should come as no surprise. It was the sensitive effects of missile inaccuracy that in the early 1950's suggested to the original proponents of programs for hardening strategic vehicles against ICBM attack that

a. hardening would be an important and effective method of protection against ICBM attack in the 1960's; and that
b. by itself hardening would not be adequate for much past the 1960's.

The ICBM's then expected in the 1960's were, of course, enormously faster than manned bombers, and therefore would out-mode some programs that served very well in the 1950's; but the early ICBM's were likely to be much less accurate than the manned bombers. They were expected to have inaccuracies measured in miles, perhaps, it seemed then, as large as five miles, compared to the quarter of a nautical mile or fifteen hundred feet median miss distance associated with manned bombers. Since just doubling inaccuracy could affect weapons requirements by a factor of four, hardening clearly seemed a good idea. The paper proposing hardening for the 1960's was entitled "Defending a Strategic Force after 1960" and was put out on February 1, 1954. That paper included a very short section called "After After 1960" that is quite relevant for understanding why we should expect that we will have to adapt the current Minuteman to impending changes in opposing offense technology. The section read in full:

> The foregoing also suggests that even against the ballistic missile this defense would have a finite life. The

> missile might improve drastically in accuracy and payload. However, the date at which the Russians will have a missile capable of carrying a 25 MT bomb with a 1500 ft. CEP [circular error probable] appears sufficiently far removed to make the defense good, let's say, until the end of the Sixties (p. 91).

That the numbers cited in this paper of February 1954 so closely match some of those being talked of for the SS-9 is, of course, purely a coincidence. They were performance characteristics of bombers then current. However, the quotation illustrates that, from the outset, it was to be expected that sooner or later and probably in the 1970's, hardening would not be enough by itself. The discussion also suggests that to depend merely on further hardening would make the system vulnerable to further improvements in accuracy.

Hardening can be outpaced by further development in precision. This does not mean that for some possible threats a combination of ABM and extreme hardening might not be useful. It might. But as a complete substitute for ABM extreme hardening has drawbacks. It is subject, in my opinion, to much larger uncertainties as to both performance and costs than the ABM.

The major components of the Safeguard system have received elaborate study and testing. Ideas for brand new ABM systems to defend hard points that I am familiar with are not serious competitors in this time period. We should start deploying the system now on the schedule suggested and we should expect, as in the case of every other offense and defense system, that we shall learn a great deal from operational experience, make some changes and retrofits. This seems to me a sound way to supplement the protection of the Minuteman in a period when we can expect it to be endangered.

ON THE COUNTERFORCE CALCULATIONS OF SOME PROMINENT ABM OPPONENTS[8]

In preparing the preceding portion of this chapter on the role of ABM in the 1970's, I undertook to review and test my past views on the subject and once again to form my own independent judgment. I, therefore, did not rely on calculations of either the government or its critics. I took the relevant classified and public data and performed my own analysis.

The kind of analysis involved in obtaining a protected and responsible strategic force has been my principal concern for eighteen years starting with the study that gave rise to the first-strike/second-strike distinction and to a good many other concepts and modes of protecting and controlling strategic forces cited by both sides in the present debate. The ABM has other functions that I support, but my chapter in the space available focused on its role in defending Minuteman. As I stressed there, these are complex and intrinsically uncertain matters. Where scientists differ on them, laymen may be tempted simply to throw up their hands and choose to rely on the authority of those scientists they favor. I feel, however, that the substantive differences among the scientists, if carefully explained, are quite accessible to interested readers and that such careful explanation can help them form their own judgment as to which conclusions are sound.

On the Safely of Minuteman

In my statement to the Senate Armed Services Committee on April 23, I said, "I have tried to reconstruct various numerical proofs recently presented or distributed to the Congress that purport to show our Minuteman will be safe without any extra protection; these proofs depend heavily on optimistic estimates of limitations in Russian delivery accuracies, reliabilities, associated offense capabilities, and sometimes on poor offense tactics." In response to questions from members of the Committee, I illustrated several troubles with these attempted proofs of the safety of Minuteman, but there was no time to explain their defects adequately. I would like to try to do that now, and to comment specifically on the calculations of Dr. Rathjens, Dr. Lapp, and of the Federation of American Scientists. Some of the comments, particularly those of Dr. Lapp, bear also on some unevidenced statements on this subject by Prof. Chayes and Dr. Panofsky and, more recently, by Dr. Wiesner.

Though my own calculations were based on classified as well as public data, my summary of results, like that of Dr. Rathjens, was unclassified and so are the comments I am about to make. This will prevent explicit specification of some of the numbers assumed by Dr. Rathjens and by myself and inevitably it forces some roundaboutness of expression. I am able to state, for example, that Dr. Rathjens and I assume the same accuracy for the Russian SS-9 in the mid- and late 1970's. I can say that the SS-9

is now expected (and, before the Nixon Administration, was expected) to achieve that accuracy years in advance of this late time period. And I can say, as Dr. Rathjens did, that the accuracy we have assumed for the Russians, in this late time period, is essentially the same as that estimated for our own MIRV carrying missiles, namely Poseidon and Minuteman III.[9] But I cannot say what that accuracy is.

I, therefore, submitted a classified statement in which the essential numerical assumptions are explicit and related to intelligence estimates. However, even without the classified statements, some essential defects of the calculations of Dr. Rathjens, Dr. Lapp, and the Federation of American Scientists can be made clear.

Dr. Rathjens' Calculations

Dr. Rathjens has stated, "Even if the Soviet SS-9 missile force were to grow as rapidly as the Defense Department's most worrisome projections, even if the Soviet Union were to develop and employ MIRV's with those missiles and even if they achieved accuracies as good as we apparently expect with our MIRV forces (according to figures released in late 1967 by former Deputy Secretary of Defense Nitze), a quarter of our Minuteman force could be expected to survive a Soviet preemptive SS-9 attack. That quarter alone would be more than enough to inflict unacceptable damage on the U.S.S.R."[10]

My own parallel calculations for the mid- and late 1970's, using what I described as moderate assumptions, show about five percent surviving. What explains the difference? Since Dr. Rathjens and I compared notes on April 22, I am able to fix quite precisely where we agreed and where we differed.

Our assumptions agreed in the accuracy assumed for the SS-9, in the overall reliability rate, in the numbers of SS-9 boosters (500) and in the use of several independently aimed reentry vehicles in each booster. Our assumptions differed on three key points: in the degree of blast resistance assumed for our Minuteman silos, in the yield of the Russian reentry vehicles, and in the use or non-use by the Russians of substantial information about what missiles are unready at launch or fail in early stages.

On the first point, I have explained that Dr. Rathjens assumed that Minuteman silos were two-thirds more blast resistant than I did, and two-thirds more blast resistant than they are officially

estimated to be. He derived his assumption by reading several points off an unclassified chart showing the probability of a Minuteman silo being destroyed as a function of accuracy for various bomb yields. Then by using standard rules for weapons effects he inferred the overpressure resistance of Minuteman silos. However, the curves on the unclassified chart cannot be correctly read to imply the overpressure resistance Dr. Rathjens infers. His reading of the curves was in error.

Second, I assumed three 5-megaton reentry vehicles for each SS-9, as in Secretary Laird's public statements. Dr. Rathjens assumed four 1-megaton reentry vehicles. More than four reentry vehicles can be fitted on the SS-9, if the payload is only one megaton. However, the three 5-megaton reentry vehicles, given the accuracy we both assume, and given the actual blast resistance of the Minuteman, do enough for the attacker. Using his lower Russian bomb yield and his overestimated Minuteman blast resistance, Dr. Rathjens derived a probability of about sixty percent that one arriving Russian reentry vehicle would destroy one Minuteman silo. If he had used the officially estimated 5-megaton reentry vehicle and the actual blast resistance of the Minuteman silo, the probability would have been nearly ninety-nine percent. If he had used three 5-megaton reentry vehicles per booster for the SS-9 and the correct estimate for blast resistance, he would have found only sixteen percent, instead of twenty-five percent of the Minuteman force surviving. Alternatively, if he had used the classified estimates of the number of 1-megaton reentry vehicles that can be fitted on an SS-9 booster, his calculations would have shown about 7.3 percent surviving. The combined significance of these first two points of difference between Dr. Rathjens and myself is then considerable.

The third point of difference between our calculations is that Dr. Rathjens assumes that the Russians would have to salvo all of their missiles with no information as to which had been unready or failed in time to be discovered, or at any rate with no use of such information. However, it is familiar that better methods are available and are of considerable utility for an offense that wants to assure a very high percentage of destruction of the force attacked. Most missiles that are counted as "unreliable" (excluded from the figure of overall reliability) are either not ready for launch or fail at launch, and this information can be made available immediately. A substantial additional fraction that fail do so at burnout, and information as to whether burnout velocity is within expected

TABLE I

CALCULATIONS ON THE VULNERABILITY OF THE MINUTEMAN FORCE IN THE LATE 1970's IF NO EXTRA PROTECTION

Difference Between Assumptions Used by Dr. Rathjens and Myself

Number of SS-9's	: Same (500)	
Over-all reliability	: Same	
Accuracy	: Same	
Minuteman Blast Resistance	Dr. Rathjens' :	2/3 higher than official estimate
	Mine :	Official estimate
SS-9 payload	Dr. Rathjens' :	4 reentry vehicles at 1 MT (less than SS-9 capability)
	Mine :	3 at 5 MT (SS-9 capability)
Use of partial information on missile malfunctions	Dr. Rathjens' :	Not used
	Mine :	Used

Effect of Assumptions on Minuteman Survivability

	Minuteman Surviving
Dr. Rathjens' result	25%
Adjust for correct Minuteman blast resistance and three 5 MT MIRV per SS-9	16%
Alternatively adjust for correct Minuteman blast resistance and number of 1 MT MIRV warheads the SS-9 is capable of carrying	7.3%
Using correct Minuteman blast resistance, three 5 MT MIRV per SS-9, and information as to missile malfunctions before or during launch only	8.7%
Using correct Minuteman blast resistance, the correct number of 1 MT warheads per SS-9, and information as to missile malfunctions before and during launch only	6%
Using correct Minuteman blast resistance and *either* the 5 MT MIRV or the 1 MT MIRV option, and information as to missile malfunctions including one-half those that fail after launch	5%

The table above summarizes the differences between Dr. Rathjens' and my calculations.

tolerances can also be made quickly available. For radio-guided missiles this is almost automatic, but inertial systems can also radio this information back, as the telemetering in a missile flight test program shows. Later flight information is also feasible. While some fraction of the failures will remain unknown, a large proportion can be known. Therefore, instead of salvoing all extra missiles blindly, to make up for all unreadiness and all failures without knowing where they occur, one can reprogram some extra missiles to replace the large proportion of known failures. Using a current planning factor for the proportion of the unreliable missiles that cannot be replaced on the basis of timely information, the calculations using three 5-megaton reentry vehicles show considerably greater destruction. Instead of sixteen percent surviving, the approximate five percent survival that I mentioned previously results. It should be observed that this ability of the 5MT force to destroy five percent of the Minuteman force presumes that only about one-half the failures after launching are replaced—a figure well within the state of the art. Moreover, even limiting the use of information to missile malfunctions before or during launch, the 5MT MIRV force would leave only eight or nine percent surviving.

Finally, such techniques of using substantial timely information as to which missiles cannot be relied on are less important for cases where smaller yields and larger numbers of reentry vehicles per booster are used. For the 1-megaton multiple reentry vehicle case I have referred to, the expected number of Minutemen surviving reduces from approximately 7.3 percent without using such techniques, to five percent using them. The errors in Dr. Rathjens' calculations are not amended simply by taking into account the possibility of reprogramming.

Dr. Lapp's Calculations

Dr. Ralph Lapp's calculations were not presented at a Senate Hearing. However, one set of his calculations was presented as a two page appendix to his statement called "The Case Against Missile Defense," and they were featured in front page stories early in April in leading newspapers, describing Dr. Lapp as science advisor to the Senate opposition. These calculations attacking the credibility of a threat to the Minuteman itself apparently achieved widespread credence. They contain several grave errors, some of which have been pointed out independently by myself on April

23, 1969, before the Senate Armed Services Committee, by Dr. Lawrence O'Neill before the House Armed Services Committee, and by Professor Eugene Wigner before the American Physical Society on April 29th. Yet these statements pointing out Dr. Lapp's errors have received little or no newspaper notice. It is therefore worth reviewing Dr. Lapp's calculations, particularly so since one of his most blatant errors appears to have been adopted uncritically by some of the other witnesses before the Committee, specifically Professor Chayes and Dr. Panofsky.[11]

Dr. Lapp states that his calculations are based on "maximum values" for Soviet capabilities. He shows seventy-six percent of the Minuteman surviving, compared to Dr. Rathjens' twenty-five and my five percent. Moreover, he has several assumptions that agree with my own:

1. Three 5-megaton reentry vehicles per SS-9, and
2. An accuracy estimate derived, like Dr. Rathjens', from public indications of the great precision of our Poseidon or Minuteman MIRV's.

His combined assumptions about the yield and accuracy of an SS-9 reentry vehicle and the blast resistance of the Minuteman result in very high probabilities that a single arriving reentry vehicle will destroy a Minuteman silo.

He suggests that two and one-half warheads of 5-megaton power with a half nautical mile inaccuracy or CEP[12] are needed to destroy a 200 psi target with a ninety-five percent probability, and 1.1 warheads would have that probability if the CEP were a quarter of a nautical mile. In fact, using standard methods of calculation, at a half-mile inaccuracy, two warheads would yield a ninety-six percent destruction probability and at a quarter of a mile inaccuracy one warhead would have a more than ninety-nine percent probability of destroying a 200 psi target. Either Dr. Lapp's calculations are based on some rather exotic and unspecified method, or they are in error. But in any case it is apparent that, even using his methods, he derives a very high single shot kill probability, roughly comparable to my own.

How then does Dr. Lapp's Minuteman force, faced by supposedly "maximum" Russian capabilities, come out so much better than even Dr. Rathjens' Minuteman force? First, Dr. Lapp assumes a much smaller number of SS-9's than Dr. Rathjens and I. He assumes three hundred thirty-three SS-9's. This is hardly a maximum force. It is less than the number that would be produced

at past rates by continuing production into the relevant 1976-77 time period. At three reentry vehicles per booster, Dr. Lapp's assumption would give the Russians about one thousand reentry vehicles.

Second, he assumes that the Russians would use only three-fourths of their SS-9 force, that is, about two hundred fifty SS-9's (or 750 reentry vehicles). This extraordinary failure to use a fourth of the force most adapted to the purpose of destroying Minuteman is attributed to a supposed universal rule that military strategists always keep forces in reserve. This may or may not be true for tank battles or aircraft attacks in a conventional war. (The June 1967 war in the Middle East suggests it is not a sound generalization even about attacks with aircraft at the start of a non-nuclear war.) But as a universal rule for a nuclear first-strike? Dr. Lapp does not say for what these SS-9's would be reserved.

Most important, Dr. Lapp forgets that the Soviet Union has a great many intercontinental missiles besides the SS-9 and exceeding the SS-9 in numbers by a large amount. These missiles would seem to furnish a reserve that might satisfy a military strategist.

Third, he assumes overall reliabilities that are quite a bit lower than the reliabilities that Dr. Rathjens and I assumed, also lower than those attributed to the SS-9. As a result of the three assumptions, Dr. Lapp's Russians would have substantially less than half as many reliable arriving reentry vehicles as our thousand Minuteman silos. More than half the Minuteman force would then be untouched by SS-9 reentry vehicles.

Finally, Dr. Lapp makes an assumption that is plainly absurd. He supposes that even though each warhead has a very high probability of destroying a single silo, "any military realist" would fire two of his outnumbered attacking reentry vehicles at each silo that is attacked. This would leave three-fourths of the silos untouched. But if each warhead has a ninety-nine percent probability of destroying a single silo, firing two at one silo would merely increase the probability of destroying that specific silo to 99.99% but would make it quite certain that a silo that could have been destroyed will go unscathed. If a more sensible tactic were followed, namely to fire each of the two missiles at a different silo, there would be a probability of ninety-eight percent of destroying both silos and a probability of 99.99% that at least one of the two would be destroyed. (This latter is the same probability that Dr. Lapp would have achieved against the specific one that he was aiming at.) In short, Dr. Lapp's tactic would greatly reduce the

expected level of destruction achieved by the attack, and it would not increase the probability of achieving some minimum level of destruction. I know of no military realist who would regard Dr. Lapp's tactic as a sensible one for the attacker. I must agree with Dr. Wigner that Dr. Lapp has presumed that his adversary would be unbelievably stupid.

It should be observed that the absurdity of the tactic is not dependent on the roughly ninety-nine percent single shot kill probability implicit in Dr. Lapp's accuracy, yield and resistance assumptions. If one were to use a ninety-five percent shot destruction probability, the point is equally obvious. In this latter case, an adversary who assigned one missile to each of two targets would have a better than ninety percent chance of getting them both and a probability of 99¾% of getting at least one; and he could get no better than a 99¾% probability of getting one silo if he sent both missiles against one silo. In the latter case, however, he could destroy at *most* one silo.

Professor Chayes and Dr. Panofsky have made statements suggesting they also accept the principle of sending at least two missiles to each silo. Professor Chayes said in his statement to the Senate Armed Services Committee on April 23:

> ... it is agreed that the attacker would need at the very minimum 2,000 accurate warheads—two for every one of our silos—before being able to think about a first strike.

Professor Panofsky in his statement to the Senate Armed Services Committee on April 22 stated:

> Moreover, an attacker would have to compensate for the limited reliability of his force by targeting at least two and possibly more warheads against each of the 1,000 Minuteman silos.

The reason behind these two statements is less explicit than Dr. Lapp's. Dr. Panofsky is talking about compensating for unreliability rather than inaccuracy, but it seems plain that no such universal rule makes sense.

Dr. Lapp has a second set of calculations published on May 4, 1969, in *The New York Times Magazine*.[13] There he assumes the Russians may have five hundred rather than three hundred thirty-

three SS-9's. Since he again assumes three reentry vehicles per booster, this makes a total of 1,500 reentry vehicles. He apparently avoids the obviously bad strategies of reserving a quarter of the force, and then using the remainder to attack only half the targets they are capable of destroying with high probability. Nonetheless, once again his calculations show very high survival rates: "500 to 750 operable Minuteman." With these changed assumptions, how does the outcome continue to remain so favorable to Minuteman's survival?

Dr. Lapp has made some other changes. He has reduced the yield of the SS-9 reentry vehicles by twenty percent, increased his estimate of the hardness of the Minuteman by fifty percent, and, most important, he now uses very large inaccuracies for the SS-9, 3,600 feet in one case and 5,500 feet in the other. The latter great inaccuracy assures him his seven hundred fifty operable Minuteman surviving. But there is no justification for assuming such great inaccuracies in the mid- and late 1970's. One of the few constants in Dr. Lapp's various calculations appears to be his conclusion.

Calculations of Dr. Steven Weinberg and Dr. Jerome Wiesner (in *ABM: An Evaluation of the Decision to Employ an Anti-Ballistic Missile System,* edited by Abram Chayes and Jerome Wiesner, New York, 1969)

Dr. Weinberg and Dr. Wiesner present variants of the same calculation to show the safety of the Minuteman force. Dr. Weinberg supposes that at least 2,100 reliable arriving reentry vehicles "with megaton yield and high accuracy" would be needed to destroy all but 42 of our 1,050 ICBM silos. He appears to assume an eighty percent single shot kill probability. Dr. Weinberg doesn't indicate the exact blast resistance, yield, and inaccuracy assumptions that go into his eighty percent hypothetical kill probability, and the testimony of Deputy Secretary Packard that he cites in that connection offers no basis for such a determination.[14] Mr. Packard there shows for three different bomb yields a spectrum of probabilities varying from less than ten percent to one hundred percent as accuracy varies from a mile or so down below one-tenth of a mile. Mr. Packard does not say what the accuracy of any SS-9 reentry vehicle is expected to be so that no specific single shot kill probability can be inferred from his testimony.

Dr. Wiesner assumes five hundred reliable SS-9's, each carrying three MIRV's; or more exactly fifteen hundred reliable MIRV's. And he also assumes an eighty percent kill probability for each arriving reentry vehicle. He justifies this with the statement that a 5-megaton reentry vehicle would have to be used and that "at best the MIRV guidance system will be accurate enough to give only a 0.8 kill probability for the unit."[15] One can read directly from Deputy Secretary Packard's chart that Dr. Wiesner is thus implying that accuracies less than about 2,400 feet are not possible in the time period in question. Dr. Wiesner has given no technical argument to support this assertion; it is at variance with expected accuracies for our own MIRV systems, and it is at variance with the accuracy that the intelligence community has *for some time* expected the SS-9 to achieve years before the late 1970's time period, and with the accuracy assumed by Dr. Rathjens. At the 5-megaton yield and with the expected SS-9 accuracy the single shot kill probability for each reliable arriving reentry vehicle would be very much higher than eighty percent as I have already pointed out elsewhere.

If Dr. Wiesner had used three 5-megaton reentry vehicles, the expected accuracy of the SS-9's and, furthermore, had incorporated expected reliabilities, his calculations would have shown only sixty-three out of 1,100 hard targets surviving, that is 5.7%. Or if he had used the expected accuracy and reliabilities and the number of 1-megaton vehicles deliverable by the SS-9, he would have arrived at substantially the same result: sixty-eight out of 1,100 surviving.

There are a number of less critical flaws in Dr. Weinberg's and Dr. Wiesner's calculations. The essential, however, is that they both assume combinations of accuracy, yield, and number of reentry vehicles per booster that are less effective than intelligence expects (and for some time has expected) of the SS-9.

The Calculations of the Federation of American Scientists (FAS), March 8, 1969

These calculations of the FAS were published nearly a week before the President's decision on the Safeguard System was announced. The FAS statement was intended to refute in advance the need for extra protection of the Minuteman force. However, the calculations it presents are basically irrelevant since they use only the Russian force "at the present time," and they assume

larger inaccuracies than intelligence attributes to the Russians' SS-9's for the later time period. They do not use MIRV's and in fact, according to their author, they do not use the SS-9 at all.

In the first section of this chapter,[16] I said that the many confident assertions current that Minuteman will be safe without extra protection in the late 1970's are unjustified. These supplementary comments have illustrated and analyzed some essential flaws in these assertions: they depend on erroneous estimates about the blast resistance of our own forces or wishful estimates about Russian lacks either in accuracy or in other capabilities or in competent tactics in that time period; they do not, as they claim, use "the most worrisome projections" and the "maximum capabilities" for Russian forces. In fact even my own calculations showing that the Minuteman will be vulnerable if extra protection is not provided do not use "maximum" Russian capabilities. Greater accuracies, for example, are quite feasible in the late 1970's for the Russians. I have used the CEP attributed to the SS-9 in the early 1970's. If the SS-9's CEP should be two-hundred fifty feet smaller than that estimate, then only four-hundred SS-9's using megaton range reentry vehicles would destroy about ninety five percent of the Minuteman force. Or with the larger force even greater percentages of the Minuteman force could be destroyed if we do nothing to supplement its protection. As I emphasized in my statement on April 23rd, the expected vulnerability of a hardened force is extremely sensitive to the accuracy of the force attacking. The accuracy assumed by Dr. Rathjens and myself is not only attributed to the SS-9 in the early 1970's, it is also the accuracy we estimate for our own MIRV's. Programs for achieving still greater accuracies for some of our MIRV's have been drawn up though not funded.

I have focused on the problem of protecting Minuteman, because, as I have stressed, we need a mixed force and have good reason to preserve the second-strike capability of so large a proportion of our strategic force. Even if it were true that the United States needed only a few strategic vehicles to survive, buying and paying for the operation of a great many that had become vulnerable to attack would be a very poor way to obtain those few surviving. There are safer and cheaper ways of getting a force of a given size than to buy a much larger one, most of which is susceptible to annihilation. To maintain a force most of which could be used only in a first-strike, hardly contributes to stability.

It is sometimes said that such analyses of the potential vulnerability of Minuteman are like the talk of the bomber gap in the early 1950's and the missile gap at the end of the 1950's. Nothing could be further from the truth. Most of those who talked of bomber gaps and missile gaps raised these possibilities to argue for expanding the number of our own bombers or missiles to close the gap. They thought of the problem as one of matching first-strike forces. But how to maintain a second-strike force cannot be adequately understood in these terms. Whether or not we have it depends, as I have said, not simply on the relative size of two opposing forces, but on a great many characteristics of the attacking force and of the force attacked and its protection. It is the opponents of the ABM today who, rather than defend the offense, would simply expand it. Moreover, many of these same opponents of the ABM were among the chief propounders of the missile and bomber gaps in the past; some scientists are now willing to state that they helped "create the myth of the missile gap." My own record on this matter is quite clear. Throughout the 1950's I pointed out the essential irrelevance of matching first-strike forces and of all the gap theories that flowed from such matching. For example, in 1956 I wrote:

> Exaggerated estimates of Russian force size, for example, might be used directly to suggest emulation. But we have already made clear that determining who has the best or second best Air Force in being in advance of attack by simply matching numbers or quality is not to the point. Those who assert that we may have fewer and perhaps inferior planes than the enemy and still have a deterrent force must also recognize that we may have more and even better vehicles and yet have inadequate deterrence.[17]

The propensity simply to list Russian and American pre-attack forces measured in various arbitrary ways continues to be exhibited on both sides of the present debate. On one side, first-strike capabilities are sometimes matched against adversary cities in the discussions of "overkill." On the other side, first-strike forces of Russia and the United States are sometimes matched against each other to show "superiority" or "inferiority" or "parity" or the like. My point is quite different. Foreseeable technical change in the 1970's compels sober thought about improving the protection of

crucial elements in our strategic force. Such change can affect our second-strike capability. In that connection, I have centered my discussion on the protection of the Minuteman, but the problem of protecting our bombers is also important, and, even more, we must improve our protection of the national political command vital to the control of sea as well as land-based strategic forces.

ENDNOTES - Wohlstetter - The Case for Strategic Force Defense

1. This chapter constitutes a slightly edited version of my Statement to the Senate Armed Services Committee, April 23, 1969, and a supplement submitted on May 23, 1969.

2. *DoD Appropriations for 1969*, Hearings, Part I. Financial Summary. Expenditures in the 1950's were not then broken down by mission, but strategic budgets were even higher in the late 1950's than in 1962. In constant prices, for example, 1959 was more than double 1970.

3. ". . . *First,* an offense force with such increased accuracies and reliabilities and with an extensive use of MIRV's is very much more efficient in attacking the fixed offense force or the important fixed elements of the mobile force of an adversary. . . . *Second*, one result of this sort of change in Russian offense forces is to make improved antiballistic missiles (rather than simply more hardening or more missiles) an economic way for the United States to protect the hard fixed elements of a strategic force. . . . *Third*, at a minor increment in the modest cost of a hard-point ABM defense, it is possible to make available a light ABM for defense of civil societies against a small submarine or land-based missile force or part of a large one launched by mistake or without authorization. . . ." See Albert Wohlstetter, "Strength, Interest and New Technologies," Address to the September 1967 Institute of Strategic Studies Conference on the Implications of Military Technology in the 1970's at Elsinore, Denmark, in *Adelphi Papers*, No. 46, p. 4.

4. See, for example, one of the first classic sources of Minimum Deterrence Doctrine: *1970 Without Arms Control*, Special Committee Report, Planning Pamphlet No. 104, Washington, DC: National Planning Association, 1958, pp. 32-33, and 44.

5. We have not been very good at predicting our own or our adversary's technologies. These matters are intrinsically uncertain. Eminent scientists at the end of the 1940's predicted that fusion weapons would be infeasible, and, if feasible, undeliverable, and, if delivered, of no strategic significance, since it was thought (erroneously) they could be used only against cities. Some of those who then thought the threat of fusion bombs against cities neither moral nor important strategically now take it to be both. Compare, for example, Hans Bethe's present views with those in "The Hydrogen Bomb," *Scientific American*, Vol. 182, No. 4, April 1950, pp. 18-23. In February 1953 an important scientific study group expected the Soviets would have no ICBM's before the late 1960's—a prediction plainly in error by the end of the year. See the final report of the Lincoln Summer Study, among whose prominent members were James Killian, Jerome B. Wiesner and Carl Kaysen. Writing in October 1964, Jerome B. Wiesner and Herbert York, "National Security and the Nuclear Test Ban," *Scientific American*, Vol. 211, No. 4, October 1964, pp. 18, 27-35, were quite sure that no technological surprises could substantially change the operational effectiveness of intercontinental delivery systems, and thus entirely missed the major strategic potential of precisely aimed MIRV's, a concept that had already emerged in the classified literature. These were able and informed men. But exact prediction on these matters defies confident assertion.

6. See [this essay's] next section, "ON THE COUNTERFORCE CALCULATIONS OF SOME PROMINENT ABM OPPONENTS," for elaboration.

7. Poseidon and Minuteman III have been test flown and are in the process of deployment (the first of these should be operational in about a year and a half).

8. This section is a slightly edited version of a May 23, 1969, supplement to my April 23, 1969, Statement to the Senate Armed Services Committee.

9. See endnote 7 above.

10. Testimony of April 23, 1969, before the Senate Armed Services Committee. See also Wohlstetter testimony of March 28, 1969, Part 1, p. 359, of *Strategic and Foreign Policy Implications of ABM Systems,* Hearings before a subcommittee of the Senate Committee on Foreign Relations.

11. It is an error that is repeated also in Abram Chayes and Jerome B. Wiesner, eds., *ABM: An Evaluation of the Decision to Deploy an Anti-ballistic Missile System*, New York: Harper & Row, 1969.

12. CEP is the acronym for "Circular Error, Probable," a commonly used measure of the inaccuracy of weapon systems. In repeated firings, 50% of the weapons would miss their targets by less than the CEP (or median miss distance) and 50% would miss by more than the CEP. A frequent misinterpretation assumes that all weapons miss their targets by a distance equal to the CEP—which is like assuming that all students score at the 50th percentile on an exam. A nautical mile is 6,080 feet. It, rather than a statute mile, is a standard dimension for measuring CEP or median miss distance.

13. Ralph E. Lapp, "From Nike to Safeguard: A Biography of the ABM," *The New York Times Magazine*, May 4, 1969.

14. Chayes and Wiesner, eds., *op. cit.*, pp. 86-93.

15. Johan Hølst and William Schneider added the following commentary in 1969: Professor Wohlstetter's critique is based upon the manuscript version of the book which was distributed prior to its publication. In book form, Dr. Wiesner replaced the explicit .8 kill probability with a vague reference to an "accuracy estimated by Secretary Laird." In the manuscript, he incorrectly calculated (on the basis of a .8 kill probability) that 270 missiles would survive (the correct number is less than 150). The book version retains the "conclusion" of 270 survivors but does not make any explicit probability assumption—and thus now assumes a kill probability of about .65. See Chayes and Wiesner, eds., *op. cit.,* p. 73.

16. I.e., my testimony on April 23, 1969.

17. Albert J. Wohlstetter and F. S. Hoffman, *Protecting U.S. Power to Strike Back in the 1950's and 1960's*, R-290, Santa Monica, CA: The RAND Corporation, September 1, 1956.