LEGENDS OF THE STRATEGIC ARMS RACE

PART I: THE DRIVING ENGINE

PART II: THE UNCONTROLLED UPWARD SPIRAL

by

ALBERT WOHLSTETTER

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ALBERT WOHLSTETTER

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STRATEGIC REVIEW is pleased to present Dr. Albert Wohlstetter's important paper *Legends of the Strategic Arms Race* as a significant contribution to the debate on strategic arms policy.

This paper was prepared for the California Seminar on Arms Control and Foreign Policy, Conference on Arms Competition and Strategic Doctrine, June 1974. "Part I: The Driving Engine" was published, without the Appendix and with some deletions, in the Summer issue of *Foreign Policy* under the title, "Is There a Strategic Arms Race?". Part II, published in the Fall issue of *Foreign Policy*, will be presented in the Winter issue of STRATEGIC REVIEW.

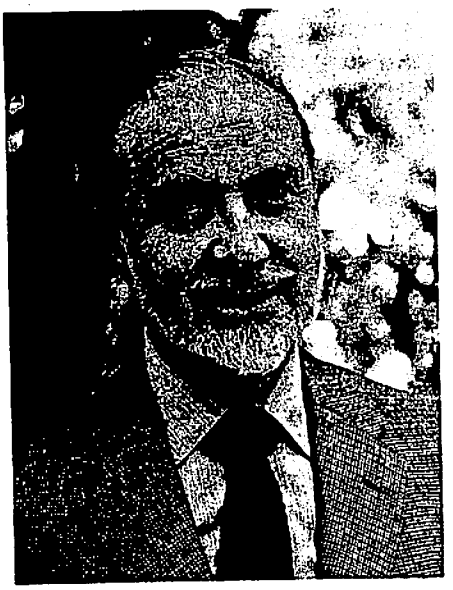
The accuracy of intelligence estimates of opposing military force is a matter of vital import. But in the final analysis, they are only estimates, subject to error and misinformation. Deliberate distortion of such assessments for domestic political effect has, on occasion, been alleged by some observers. Therefore, this analysis of recent experience is a salutary adjustment to reality.

These intelligence estimates reflect a consistent pattern of underestimation of the pace of Soviet deployment of strategic weapons and refute the widely held myth that we have been overestimating Soviet capabilities.

A. G. B. METCALF
Strategic Studies Editor

LEGENDS OF THE STRATEGIC ARMS RACE, PART I: THE DRIVING ENGINE

ALBERT WOHLSTETTER



THE AUTHOR: Dr. Albert Wohlstetter is University Professor at the University of Chicago, a Fellow of its Center for Policy Studies, and Sometime Fellow of All Souls College, Oxford University, England. Trained as a mathematical logician and economist, during World War II he worked on problems of reliability and quality control in electronic and electrical equipment. He has been a member of the Rand Research Council, Professor in Residence at UCLA and Ford Professor at the University of California at Berkeley. For the last twenty-three years he has done research on problems of defense and political-military affairs and specifically on protecting strategic forces and stabilizing deterrence. He is the author and co-author of numerous articles and several book-length studies.

For a notion so central to contemporary debate on arms policy, the phrase "strategic arms race" remains remarkably unclear. When we talk of "arms" are we referring to the total budget spent on strategic forces? The number of strategic vehicles or launchers? The number of weapons? The total explosive energy that could be released by all the strategic weapons? The aggregate destructive area of these weapons? Or are we concerned with qualitative change—that is, alterations in unit performance characteristics—the speed of an aircraft or missile, its accuracy, the blast resistance of its silo, the concealability of its launch point, the scale and sharpness of optical photos or other sensing devices, the controllability of a weapon and its resistance to accidental or unauthorized use? When we talk of a "race" what do we imply about the rate at which the race is run, about the ostensible goal of the contest, about how the "race" is generated, about the nature of the interaction among strategic adversaries?

Arms race theorists are charged with an urgent message But what is it? Not merely that a government constructing an armed force has in mind the possibility of conflict. That will startle no one. To build a national defense is to recognize serious differences, potentially incompatible goals of possible adversaries. Military forces then are at least partially competitive: What one side does, whether to defend itself or to initiate attack or to threaten attack or response, may be at the partial expense of another side. (Weapons are not by nature altogether friendly.) This means in turn that *some* connection is only to be expected between what one side does and the kind and probable size of a potential opponent's force.

Arms race doctrines plainly want to say much

I draw on the forthcoming book, *Competition or Race: Innovation and the Changing Size of Strategic Forces* by Albert Wohlstetter, David McGarvey, Fred Hoffman and Amoretta Hoeber. I am greatly indebted to my co-authors and to Roberta Wohlstetter for advice on testing for bias in intelligence predictions.

more than these simple truths. They suggest that the competition results from exaggerated fears and estimates of opposing threats, and therefore is not merely, or even mainly, instrumental to the partially opposed objectives of each side. The competition takes on an explosive life of its own that may frustrate the objectives of both. Explosive in two senses: (a) it leads to "accelerating" (or "exponential" or "spiraling" or "uncontrolled" or "unlimited" or "unbridled" or "infinite") increases in budgets and force sizes; (b) it leads inevitably to war, or at any rate makes war much more likely.

Such doctrines strongly resemble views that were widespread among statesmen like Lord Grey between the two World Wars. Lewis Fry Richardson put these views into his famous equations relating the rate of increase in defense budgets on one side to the level of spending on the other. Current theorists of an explosive quantitative race, however, have added some odd twists during the last fifteen years. Perversely they regard an ability to attack cities as relatively benign. They locate the source of the race especially in efforts to defend civilians and destroy offensive military forces, and characteristically see the force driving the quantitative spiral to be qualitative military change, in particular, improved technologies for destroying weapons, whether in place or already on their way to target. According to the present nearly universal dogma, a major innovation announces a "new round" in the arms race, another turn in the irreversible "ratchet" of increased budgets, leading to "new levels of nuclear overkill" and leaving both sides inevitably worse off than before.

Now in protecting one's own independence or that of one's allies or in preserving a coalition or even a relation of dependency, almost anyone would want to reduce the chance that there will be an actual war; and if the war should occur, most of us would like it to destroy as little as possible. Moreover, we want to buy safety and independence as cheaply as we can. Such considerations affect unilateral national decisions on defense as well as arms negotiations with potential adversaries. And negotiations with adversaries are more likely to complement usefully the necessary process of national decision-making, if they are based on an objective appraisal of what has been the actual, historical—rather than a hypothetical and legendary—competition between the adversaries and on an unprejudiced assessment of the net advantage or disadvantage in any proposed quantitative or qualitative change.

Theories of the strategic weapons race, however, are blunt instruments in weapons debate; not tools of analysis and appraisal so much as words wildly aimed to counter some equally misleading slogans by proponents of increased budgets. When precise enough to be wrong, they are massively in error. Far from illuminating changes in the strategic forces on both sides and so aiding thoughtful national choice or agreement with adversaries, they cry panic. They also blind us to what should have been obvious to an unprejudiced eye:

(1) That in spite of the myth of invariable or systematic U.S. overestimation, we systematically underestimated the number of offense vehicles the Russians would deploy. The duration of this period of underestimation dwarfs the three and one-half years starting at the end of 1957 when we expected a "missile gap." The myth of invariable overestimation grew with the fact of underestimation and has lasted until now.

(2) That U.S. strategic budgets and the destructiveness of U.S. strategic forces have been going down, not up. U.S. strategic budgets have declined nearly exponentially from the high plateau of 1956–1961.

(3) That the net thrust of major qualitative change in the strategic field has been to redeploy and cut rather than to increase resources devoted to the strategic force; to increase political control of the force; to reduce its vulnerability; and therefore also to reduce the instabilities that could lead to a nuclear holocaust. Almost the exact reverse of the stereotype.

This paper, the first of two parts, treats the ambiguities of theories of strategic arms interaction, and tests one major feature of the presumed dynamics of that interaction—the claim of invariable or systematic U.S. overestimation —by confronting it with over fifty U.S. predictions of the number of missiles and bombers that the Soviets would deploy.

Strategic Arms Race: Metaphor or Model?

A survey of the literature indicates that the most frequent view since Sputnik presumes accelerated spending on strategic offense and

defense, but especially on new armaments. The spending has an ostensible goal of increased safety but, ironically, an increasingly probable end in war. In fact, an excessive concern for safety is supposed to be the root of the trouble.[1]

Uncertainties are intrinsic. But as the theory goes, they especially affect any U.S. attempt, in case deterrence fails, to take out insurance by active or passive defense against weapons launched at our cities, or by a capability to destroy adversary military weapons before they are launched. Uncertainties are much smaller for retaliation against a small number of unprotected population centers, where at least the targets are not only easy to destroy but also stationary, fixed in number, or change only very slowly.[2] The uncertainties in attacks on weapons are very large, even in estimating how many weapons an adversary will deploy. U.S. planners systematically resolve these uncertainties by playing safe, assuming "the worst case" and building up to take care of that. But this forces the Soviet Union to do the same, etc. The initiative in the large majority of cases has been ours. It is the United States, holders of the doctrine seem invariably to feel, that has "set the rate and scale for most of the individual steps in the strategic arms race."[3] (A view quite close to that of revisionist historians.)

1. Herbert York, *Race to Oblivion*, New York, Simon and Schuster, 1970, p. 237; Ralph E. Lapp, *Arms Beyond Doubt*, New York, Cowles Book Company, Inc., 1970 *passim;* Nancy Lipton and Leonard S. Rodberg, "The Missile Race—The Contest with Ourselves" in *The Pentagon Watchers*, New York, Doubleday and Co., 1970, pp. 299–300.

2. Cf. G. B. Kistiakowsky and G. W. Rathjens, "The Limitations of Strategic Arms," *Scientific American*, Vol. 222, No. 1, January 1970, p. 24. "*The uncertainty about the effectiveness of damage-limiting capabilities will be considerably greater than about assured destruction capabilities.* . . . the characteristics of the target against which assured-destruction capabilities would be used (population and industry) will be known with some precision and will change only slowly with time." (italics theirs).

3. Herbert York, op. cit., page 230; Cf. also Marshall Shulman, Statement before U.S. Senate Committee on Foreign Relations, *Hearings on Strategic Arms Limitation Agreements*, 92nd Congress, 2nd Sess., U.S. Government Printing Office, 1972, page 139; William Epstein, "Will the Russians Play 'American Roulette?' ", *Saturday Review World*, June 29, 1974; Bernard T. Feld, "The Sorry History of Arms Control," *Bulletin of the Atomic Scientist*, Vol. XXVI, No. 7, September 1970, page 26; Jeremy Stone in *American Militarism 1970*, New York: The Viking Press, 1969, page 68; Edgar M. Bottome, *The Balance of Terror: A Guide to the Arms Race*, Boston, 1972, pp. xv–xvi.

In the writings of almost any proponent of the current doctrine, ambiguities and inconsistencies abound as to just what is accelerating. As for how the acceleration and its disastrous consequence are generated, the vagueness and unclarities loom even larger.

Before commenting on the obscure mechanism that is supposed to lead to spiralling arms spending, some things need saying about the mechanism that is supposed to lead from spiralling arms to war. The latter is as unclear in contemporary doctrine as it was in Richardson's. Some eighteenth century writers, such as Immanuel Kant, held that nations undertook wars of aggression to escape the financial burden of maintaining a standing army. It is hard, however, to take that seriously as a motive for starting World War III, with its enormous potential costs in blood and treasure. (It is hard to take it seriously as a motive for starting World War I or World War II.) Another alternative suggested by contemporary theorists of the strategic arms race refers simply to the increased tension that comes with rising arms expenditures. Once again, I know of no convincing elaboration of such a view. It is sometimes indicated that the chance of accidental war rises proportionately with spending on arms. But that is clearly not so. The chance of war occurring by mistake or through some unauthorized act depends, for example, on arrangements for a responsible, protected command and control, and for vehicles so protected that they need not be launched while signals of an attack are still substantially uncertain. Improving such arrangements costs money. In fact many of the most reckless strategies, i.e., those calling for launch-on-warning and the like, have been propounded by advocates of nuclear forces reduced in cost and in size to very small numbers.

But whatever disasters might follow an accelerating quantitative race, the race itself would be undesirable. Even if it did nothing more than drain resources, an exponential drain would be no laughing matter. The arms race doctrine, however, seems to offer little more than a metaphor about the factors that generate decisions on arms. If we want to go beyond metaphor, we need to develop models reflecting several aspects of reality that are usually omitted in theories of a self-enclosed, spiralling interaction between development and

procurement choices on the two sides.

First, a realistic model would reflect the fact that the multiple objectives of potentially opposed governments may include more than simply an interest in defending their own current territorial boundaries without any encroachment on or defense of the independence of other nations. And decisions on armaments will respond to political acts outside of the cycle of weapons innovation and expansion. The arms decisions of the two superpowers cannot be taken simply as unfortunate cases of reciprocal failure by both superpowers to see that all their important interests are held in common. They are not.

Second, a model, as distinct from a metaphor, that hoped to explain strategic arms decisions, would have to reflect institutional forces within each country that shape its response—if any—to changes in another country's military posture; or to political acts. Close students of this decision process, like Loftus and Marshall, have stressed that when we consider the actual institutions and operative doctrines of those who affect weapons decisions of both superpowers, we find the interactions to be not explosive, but "muffled, lagged and very complex." [4]

Third, such a model would note that governmental decisions on strategic arms are constrained both by resource limits at any given time and by the fact that the government has many civilian as well as military objectives besides those of the strategic force. This forces trade-offs among differing objectives. The point is obvious enough, but it has important implications for the supposed exponential process; and, obvious or not, the point tends to get lost.

4. A. W. Marshall as quoted in Graham Allison, *Essence of Decision: Explaining the Cuban Missile Crisis*, Boston, Massachusetts: Little, Brown & Co., 1971, p. 98. Some more popular, recent versions of bureaucratic politics sometimes suggest a kind of explosive competition among factions within the government that drives budgets up exponentially. However, the serious studies suggest neither hyper-responsiveness nor simply a mad tossing about of funds, but substantial bureaucratic inertia as well as budgetary constraints. Bureaucratic factors are essential, but their existence hardly implies a spiral. Moreover, if as the first point stresses, changing resources available for strategic forces respond to political acts outside the cycle of arms decisions on the two sides, they are even more obviously affected by political acts outside the intramural rivalries of one side.

To illustrate this neglect, one might take a classic early source for Minimum Deterrence and strategic arms race doctrine: The National Planning Association (NPA) study *1970 Without Arms Control* (1958). The authors observed that no more than 200 warheads would be needed to destroy "a large nation-state" (i.e., its major population centers). But a "counter-offensive," mutually pursued, must accelerate. This reasoning, now standard, is nonetheless bizarre. After all, for centuries non-nuclear forces that could be greatly expanded were purchased to deal with opposing non-nuclear forces. And no one so far has held that only aiming them at a fixed number of civilians can avoid a spiral. The authors of the study, however, took off from a calculation of General Gallois, theorist of small nuclear forces for small and medium powers to replace alliances. Gallois claimed that, at a range of 2,000 miles, 12 missiles would have to be expended to destroy one hardened missile; at 3,000 miles, 18; and at 4,500 miles, 26. The NPA Committee supposed that 50,000 to 60,000 Soviet missiles would be needed to destroy 4,000 Western launchers, which might drive the West to build a half-million missiles to destroy the Soviet ones, and so on. This calculation would have looked even more horrendous if the Committee, taking Gallois at his face value, had used intercontinental ranges, and a 15 to 1 exchange ratio. Half a million missiles would have been horrendous enough; at the going rate of cost per missile, it exceeded the American GNP.

But of course even though each government were to aim at reducing the harm done to its civil society in the event of war, it would not be its only aim and it would be willing to sacrifice only so much of its other aims for that one purpose. Long before the GNP was exhausted in the effort, the opportunity costs of a decision to expand the missile stockpile would seem excessive.

This point has many implications for the current doctrines about explosive arms races. One concerns the stereotype that an overestimate of an adversary threat generates an accelerating increase on one's own side. Why should this be so? If one's aim to counter a given threat is made extremely costly by expected adversary moves, because the threat is very large and the advantage is all on the other side, the game may not be worth the candle. This was in fact Secre-

tary McNamara's chief argument against undertaking a thick ABM defense against the Soviets. In short, the larger the threat, the more futile a response may seem. Inflated threats then can discourage response rather than stimulate an arms race. On the other hand, in the past an understatement of adversary capabilities has sometimes been used to justify ambitious programs that might have looked futile if a more accurate estimate of the capability had been made. This was the case with some of the estimates of the ICBM and the significance of fusion technology assumed in the Lincoln Summer Study in the early 1950s. Depending on the trade-offs with other objectives, overestimates or underestimates might discourage or stimulate a response. If one side anticipates a major program by the other, it might be discouraged from action of its own. And if it anticipates inaction by its adversary, it may be tempted itself to act.

In short, we can have both action-inaction and inaction-reaction sequences. The very phrase "action-reaction" has an aura of mechanical inevitability. Like Newton's Third Law: For Every Action There Is An Equal and Opposite Reaction. Only here, since the mechanism is explosive, it seems the law is supposed to read: For Every Action There Is an Opposing Greater-Than-Equal Reaction. If on the other hand the term "reaction" is understood broadly enough, as sometimes seems the case, to include responses that decrease budgets or hold them the same, rather than only to increase them, the action-reaction phenomenon is simply a portentous tautology.

Systematic (or even invariable) overestimation then need not lead to an arms spiral. Nonetheless, it is important to ask whether the U.S. government has in fact systematically overestimated Soviet missile and bomber deployments: an assertion central to the dogma of a spiral driven by exaggerated estimates and mistaken fear.

U.S. Predictions and Soviet Realities

The "missile gap", as is well known, was a U.S. overestimate after Sputnik of the number of ICBM launchers that the Russians would deploy in the early 1960s. Indeed, the trauma of discovering the error formed the basis of many of Mr. McNamara's generalizations about our tendency to exaggerate and then respond to anticipated larger threats rather than to what the Soviets actually turned out to do. The missile gap has also generated a substantial confessional literature on the part of current proponents of the doctrine of an explosive arms race about their own role in creating the myth of the missile gap, and a substantial academic industry in doctoral theses and articles explaining this particular overestimate and the supposedly general and plainly evil habit of overestimating. A few comments, therefore, are in order on the missile gap before making a broader test of the habit. (Perhaps it is worth saying that I am on record, before and after Sputnik, as having steadily opposed evaluating force effectiveness on the basis of bomber or missile gaps.)

First, the "missile gap", a brief period in which the Soviets were expected to but did not deploy ICBMs more rapidly than we did, was an ICBM gap rather than a general missile gap. During the same period, in fact, we regularly and greatly underestimated the number of *intermediate and medium range* ballistic missile (IR/MRBM) launchers that the Russians would deploy at the end of the 1950s and in the early 1960s. For example, our underestimate of the number of IR and MRBM launchers that the Russians would deploy by 1963 roughly offset our overestimate of the number of ICBM launchers they would deploy. In short, we misunderstood or reversed the priorities the Russians assigned to getting capabilities against the European as distinct from the North American part of NATO. This piece of ethnocentrism on our part was characteristic. We also greatly underestimated Soviet aircraft systems directed primarily at Europe rather than ourselves.

Second, predicting the size and exact mixture of a potential adversary's weapon deployments several years hence is a hard line of work. It is intrinsically uncertain, reversible by the adversary himself between the time of prediction and the actual deployment. Moreover, an adversary may want his opponent to estimate wrongly, either up or down. In the specific case of the missile gap, Khrushchev did what he could to make the U.S. and the rest of the world believe that the Soviets had a larger initial program of ICBMs than they actually had; and he succeeded.

Whatever the source and nature of our misestimation, it helped generate the belief that we

invariably expect the Russian programs to be larger than they turn out to be, that we compound this overestimate by deliberately designing our programs to meet a Russian threat that is greater even than the one we expect, and then, when the Russian threat turns out to be less rather than greater than expected, the damage is done; the overlarge U.S. force is already a reality or irreversibly committed.

It is a good idea, then, to subject to systematic test this claim of regular overestimation. It is a major element of the current dogma, repeated endlessly since 1961. In fact, the nearly universal acceptance of this belief has emerged from constant repetition of tags like "the mad momentum", "we have invariably overestimated" or "we are running a race with ourselves", etc., etc. rather than from any systematic numerical comparison with reality.[5] Figures 1 to 3 and Tables 1 and 2 sum up the results of a search for all of the long-term predictions of Soviet strategic missile and bomber deployment that could be found in the annual presentation of programs and budgets to Congress by the Secretary of Defense from the start of 1962 to the start of 1972, and a comparison of these predictions with what the Russians actually deployed by mid-1972—the last date referred to in the predictions that could be checked at the time the analysis was completed.

Aside from their comparative accessibility, several reasons governed the choice of these predictions from the Defense Secretaries' formal statements rather than from Army, Navy, Air Force, CIA, Bureau of Intelligence Research in State, or other estimates

First, during this extended period the Secretary of Defense did, regularly, every year, make predictions precise enough to be proved wrong and precise enough for measuring how much they had missed the mark. The possibility of determining error here requires not only that the predictions be specific as to time and quantity, and not excessively hedged by "might" or "may conceivably," but also that the adversary realities referred to in the predictions be open to observation and highly reliable measurement by the U.S. *after the fact*. Not all objects nor all characteristics predicted nor all predictors meet these requirements. Far from it.

Second, these predictions of the Secretary of Defense form a well-defined, substantial population of estimates—which is not the case for intelligence predictions in general.

Third, these estimates were presented as authoritative and official.

Fourth, they were given particular prominence in the programming and budgeting process by the fact that the Secretary used them directly to support his programs. And finally, these particular forecasts relate directly to the Secretary's judgment and that of the Congress on the five-year defense program. They are therefore most relevant for analyzing possible relations between defense programs and defense budgets and the impetus these programs might be given by forecasts as to the future enemy force deployments. Defense systems take many years to become operational, and the forces they will confront are necessarily the subject only of long-term conjecture. In presenting these estimates the Secretary emphasized this point. For example, in 1963 he testified:

> Because of the long leadtimes involved in making these weapon systems operational, we must plan for our forces well in advance of the time when we will need them and, indeed, we now project our programs at least five years ahead of the current budget year. For the same reason we must also project our estimates of the enemy's forces at least five years into the future, and for some purposes, even beyond. These longer range projections of enemy capabilities are, of course, highly conjectural, particularly since they deal with a period beyond the production and deployment leadtimes of enemy weapon systems. Therefore, we are, in effect, attempting to anticipate production and deployment decisions which our opponents, themselves, may not yet have made. This fact should be borne in mind as we discuss the intelligence estimates and our own programs based on them.[6]

5. e.g., Nancy Lipton and Leonard S. Rodberg, "The Missile Race—The Contest with Ourselves," in *The Pentagon Watchers*, New York: Doubleday & Co., 1970, p. 303; Dr. Jerome B. Wiesner, *ABM: Yes or No*, Center for the Study of Democratic Institutions, Fund for the Republic, Santa Barbara, California, 1969, p. 18; Dr. W. K. H. Panofsky, "Roots of the Strategic Arms Race: Ambiguity and Ignorance," *Bulletin of the Atomic Scientists*, Vol. XXVII, June 1971, p. 15.

6. *Statement of Secretary of Defense Robert S. McNamara before the House Armed Services Committee, the Fiscal Year 1964–68 Defense Program and 1964 Defense Budget*, Office of the Secretary of Defense, January 1963.

The first eight charts, Figures 1a to 1h, compare U.S. predictions of Soviet ICBM launchers to be deployed with the actuality as estimated after the fact.[7] The vertical arrows indicate the date at which the prediction was made (e.g., February 1962 in Figure 1a). The dashed line or lines indicate the range from high to low of what was predicted. (In Figure 1a, a high of 650 and a low of 350, by mid-1967, five and a half years later.) Later projections usually included, (as in Figure 1b), a high and a low for more than one year. This is shown in the shaded portion. The steeply rising solid line which is the same in all the charts shows the number the Russians actually completed, as estimated after the fact.

Though the claim about invariable overestimation posits that at least the middle of the range between high and low always exceeds the reality, it will be apparent that even the high end of the range seldom did that, and then only at the start of the period—and even then just barely. For ICBMs the "highs" reached as high as reality only twice in eleven times. The prediction made in 1965 (Figure 1d) is quite typical. Figures 2 and 3 illustrate analogously typical long-run predictions of future Soviet submarine-launched missiles deployed and future Soviet bomber deployments. The middle of the predicted range of the number of sub-launched missiles deployed was about three-quarters of the eventual reality. In the case of the bombers, we continued to believe that the Russians were going to phase them down and most drastically in the case of the medium bombers; but the Soviets never came down to our expectations. Tables 1 and 2 sum up some principal results. Out of fifty-one predictions, the low end of the range *never* exceeded the actual; the mean between the high and low exceeded it only twice in fifty-one times; our highs reached reality only nine times! Hardly a record of overestimation. Moreover, the ratios of projected to realized future values of the Soviet strategic force in operation display the fact that the underestimates were very substantial and that even the average of the highs was under the reality. It will be evident also that there was no systematic learning from the past as information accumulated.

7. Predictions in Figures 1a through 1h exclude short-term estimates that are limited essentially to the completion of launchers already started.

In fact, since the numbers shown refer to estimates of the *cumulative* number of strategic vehicles in operation at future dates, and since the later predictions were based on much more extensive knowledge of what was already deployed or at least started in construction at the time of the prediction, the degree of bias can be made even plainer. There are several points.

First, our means of acquiring information improved greatly over the period. Second, in the later years a much larger proportion of the cumulative total in operation was already in operation at the time predictions were made. And third, we had information not only about the number of launchers completed and in operation (displayed in the rising curves of Soviet ICBM and SLBM launchers) but also about the substantial numbers of launchers that had been started but not completed at the time the prediction was made. We knew that ICBMs started would generally be completed, say, in about a year and a half, and submarine-based missile launchers in about two and a half years, but in any case well before the dates in our long-run predictions. In fact, estimates of the missile launchers already started that were expected to be completed by a given time were, at the midrange, only 3% below the actual number for ICBMs and 2% above it for submarine-launched missiles. If we make a rough adjustment for this fact on the one hand and on the other allow for some delay in acquiring and processing information by the date predictions were made, if we assume generously a seven-month delay, the degree of understatement will be more apparent. In effect, what was being predicted was an *increment* in the force then in operation or under construction. It is appropriate to compare that increment with the actual amount newly started and completed in the ensuing interval. These figures are indicated in the parentheses on Table 2. With this adjustment, it is clear that *the actual change was three times the mid-range of the predictions in the case of ICBMs and double in the case of sub-launched missiles.*

How explain this systematic underestimate over so extended a period? And how explain what seems even more startling, the long-term peaceful coexistence of such systematic understatement with the generalized claim by exponents of the doctrine of an exploding arms race that the U.S. invariably *over*estimates?

Fig. 1a

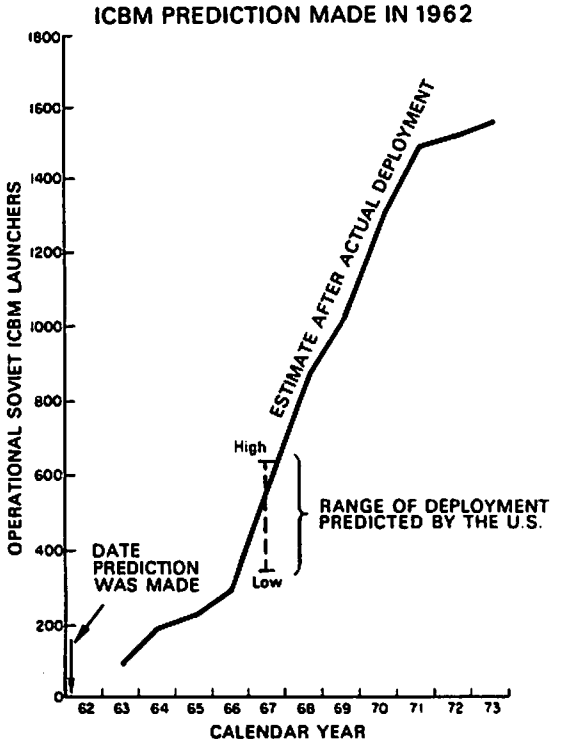


Fig. 1b

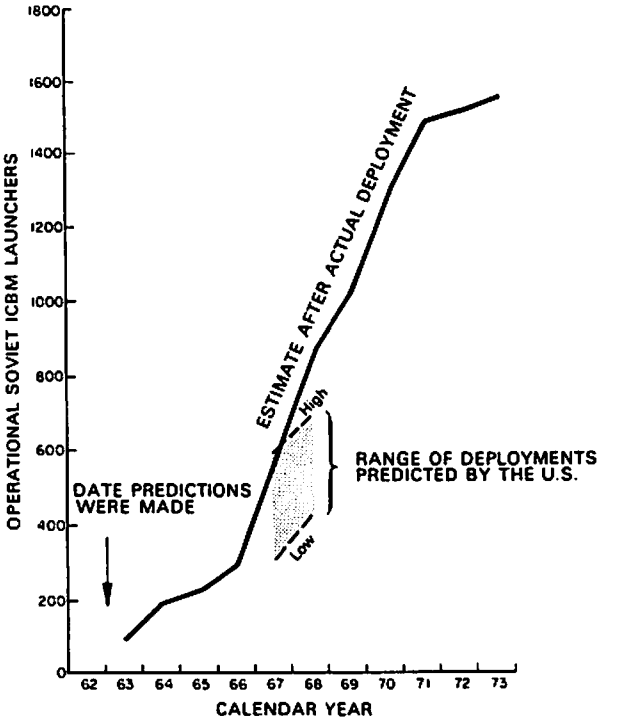


Fig. 1c

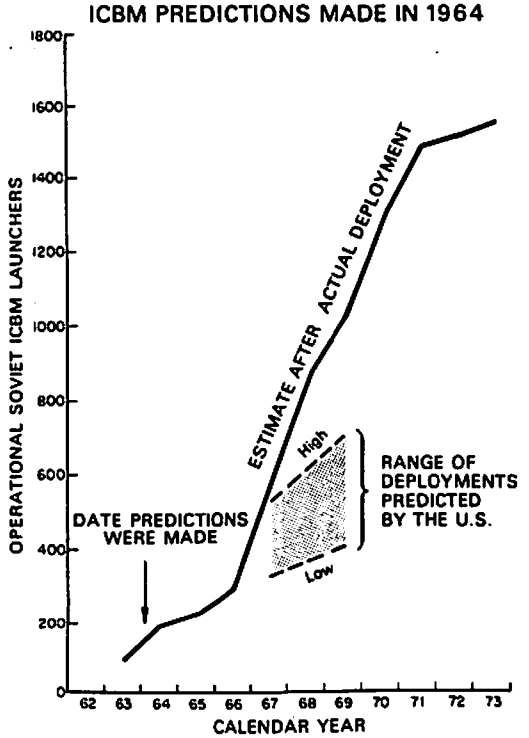
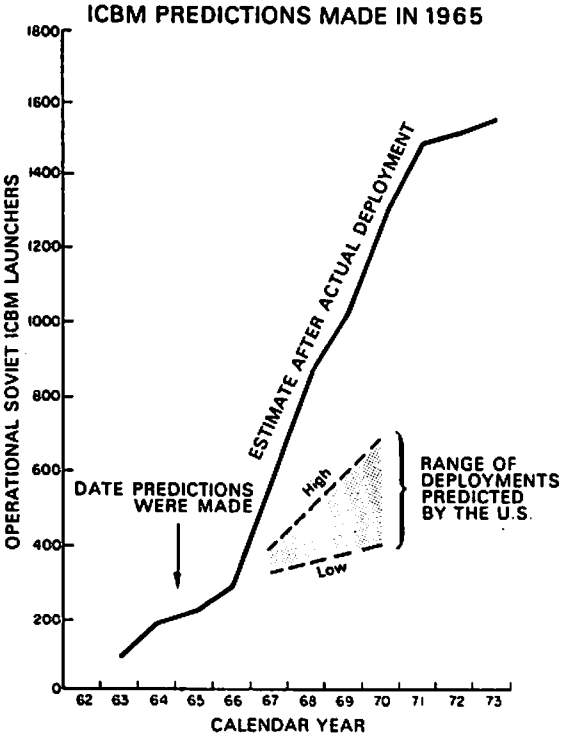


Fig. 1d



Predictions in Figures 1a through 1h exclude short-term estimates that are limited essentially to the completion of launchers already started.

Fig. 1e

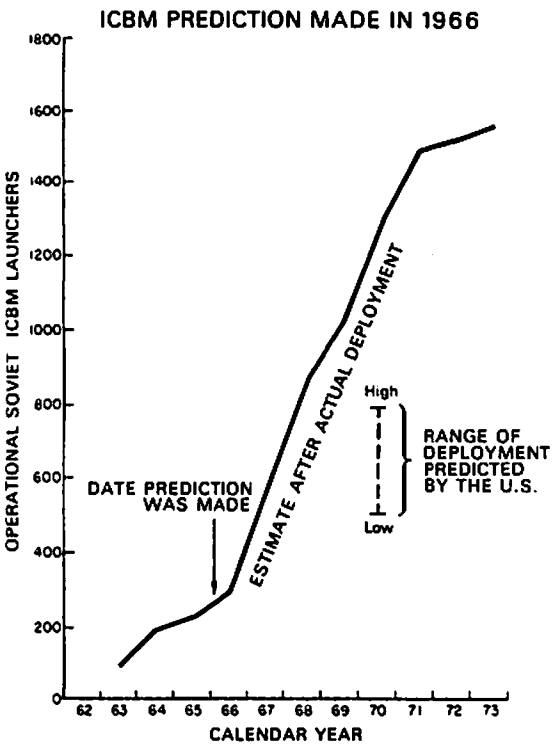


Fig. 1f

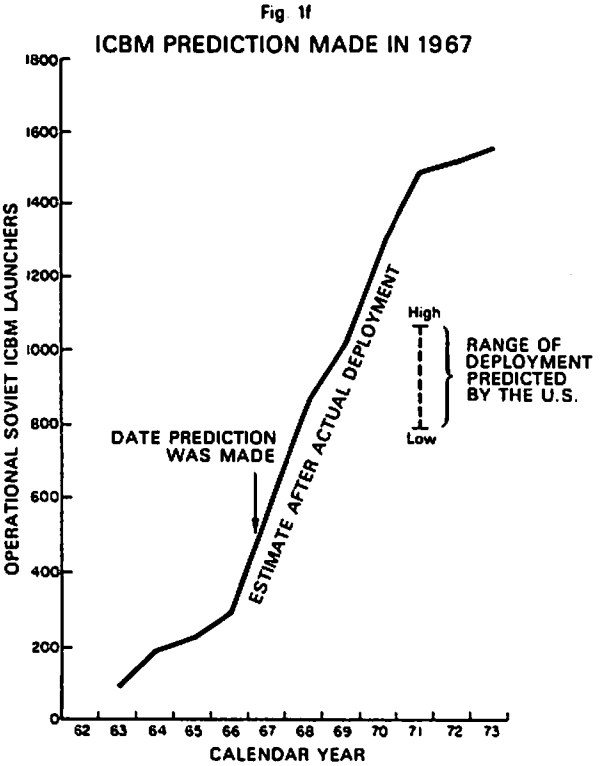


Fig. 1g

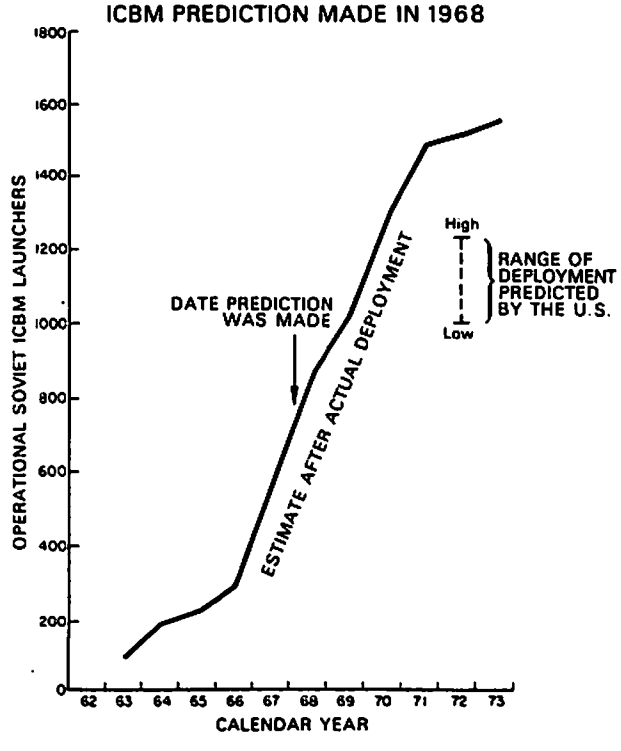


Fig. 1h

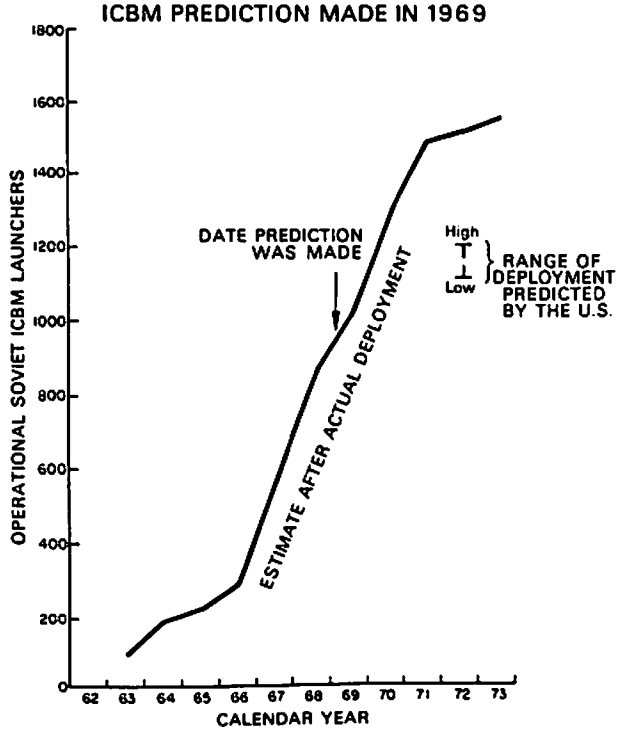


Fig. 2

OPERATIONAL SOVIET SUB-LAUNCHED MISSILES
1965 U.S. Long Term Prediction Compared to the Actual Number*

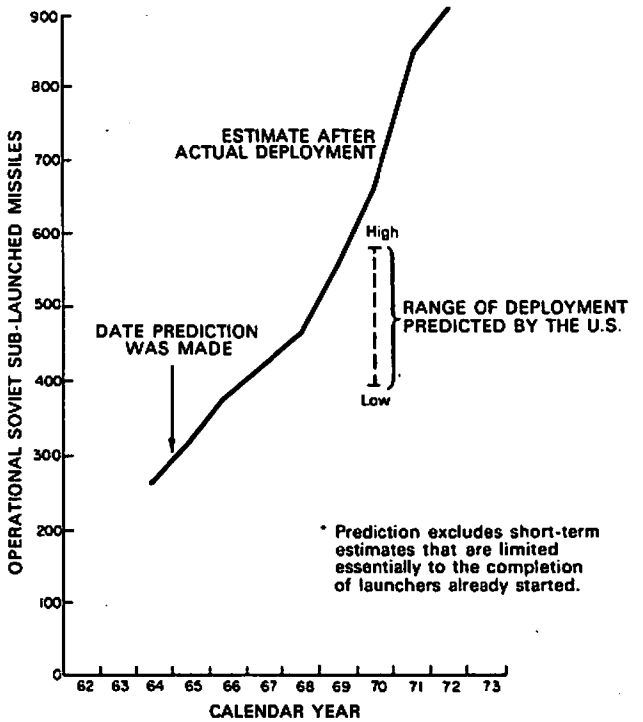
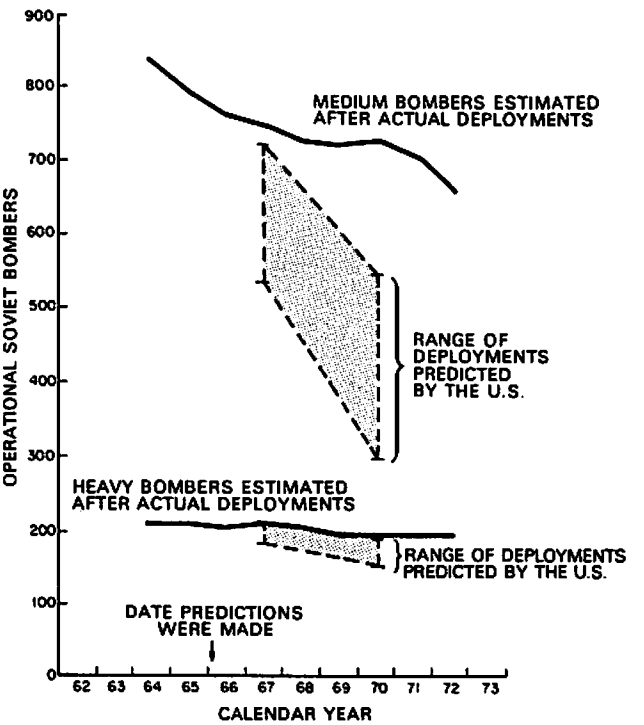


Fig. 3

OPERATIONAL SOVIET BOMBERS
1966 U.S. Predictions Compared to the Actual Number



The first question is a little easier to answer. For one thing, long-range predictions *are*, as I have said earlier, inevitably a hard and uncertain task. Errors are only to be expected and unless heavily entrenched by dogma, when they are publicly exposed, as in the "missile gap," the spectacle encourages a swing to the opposite extreme. In fact, the overestimation after Sputnik of ICBM deployments itself reacted to an earlier underestimate of the speed at which the Soviet Union would be able to develop and test their first ICBMs. Sputnik had only underlined in public a previous error of underestimation that had been found in secret earlier in 1957 about how soon the Soviets would test their first ICBMs. Sputnik, however, was spectacularly public and inevitably fed a political debate inside and outside the government about the relative position of the U.S. and the Soviet Union.

My own view of the matter,[8] by no means the symmetrical opposite of the overestimation theory, has been: Our officials sometimes overestimate, and sometimes underestimate, and sometimes even get it right; in any case neither mis-estimate means univocally expanding budgets or military adventurism. Underestimates persisted for an extraordinarily long time after the error of the missile gap, fortified by an American strategic view that Americans often attributed also to the Soviets. (These were "projections" in a double sense.) That view suggested that the Soviets did not need a large expansion of forces in order to be able to

8. See my "On Vietnam and Bureaucracy," in *Great Issues of International Politics*, M. A. Kaplan, ed., Chicago: Aldine Publishing Co., 1970; and my testimony before the Senate Armed Services Committee, 1969 and 1970: ". . . 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 turbo-prop bombers, airborne intercept radars, and large-scale fissile material production."

Table 1

1962–1971 U.S. PREDICTIONS THAT EXCEED THE ACTUAL SOVIET STRATEGIC DEPLOYMENT*

	ICBMs	SUB-LAUNCHED MISSILES	HEAVY BOMBERS	MEDIUM BOMBERS	TOTAL
LOW PREDICTIONS THAT EXCEED ACTUAL	0 OF 11	0 OF 15	0 OF 14	0 OF 11	0 OF 51
MID-RANGE OF PREDICTIONS THAT EXCEED ACTUAL	0 OF 11	1 OF 15	1 OF 14	0 OF 11	2 OF 51
HIGH PREDICTIONS THAT EXCEED ACTUAL	2 OF 11	3 OF 15	2 OF 14	2 OF 11	9 OF 51

Table 2

AVERAGE RATIOS OF PREDICTED-TO-ACTUAL CUMULATIVE NUMBERS*
(Numbers in parentheses compare predicted to actual *change*)

	ICBMs (11 ESTIMATES)	SUB-LAUNCHED MISSILES (15 ESTIMATES)	HEAVY BOMBERS (14 ESTIMATES)	MEDIUM BOMBERS (11 ESTIMATES)
LOWER PREDICTIONS	0.53 (0.16)	0.64 (0.12)	0.85	0.67
MID-RANGE OF PREDICTIONS	0.67 (0.33)	0.74 (0.47)	0.91	0.77
HIGH PREDICTIONS	0.80 (0.50)	0.84 (0.82)	0.98	0.87

* Predictions exclude short-term estimates of ICBMs and sub-launched missiles that are limited essentially to completion of launchers already started.

destroy a few American cities and therefore did not intend to undertake it.[9]

In 1964–1965 the Soviet force was roughly at the 200 ICBM level in vogue with "Minimum Deterrent" theorists. Then many, including Mr. McNamara, suggested that the Soviet Union had no intention of catching up.[10] In the next two years the force jumped to 570 at mid-year. Then it was commonly said "Inevitably, the Soviet leaders have been pressing to catch up. They may even labor under the illusion that they can obtain a margin of strategic superiority. . . ."[11] The January 1968 public Posture Statement said that Soviet operational launchers from October to October grew from 340 to 720. (This one-year increment was nearly double the canonical 200.) However the statement opined that the Soviets would slow down; and the classified prediction for 1972 quantified this judgment. In the event, the prediction fell far short of the mark. Finally as the Soviets exceeded U.S. missile numbers, "equality" was said to be all they had in mind. The

9. That view was never consistently adopted by Mr. McNamara. He came to use action-reaction language, and often talked as if the adequacy of strategic forces could be measured solely in terms of their use to destroy cities. However, he brilliantly attacked the overkill theory and continued through his last Posture Statement to insist that we keep the objective of limiting damage in case deterrence failed.

10. See, for example, "The Soviets . . . are not seeking to engage us in . . . the quantitative race. . . . There is no indication that the Soviets are seeking to develop a strategic nuclear force as large as ours." "Interview with Robert S. McNamara, Defense Secretary," *U.S. News and World Report*, April 12, 1965, p. 52. This view was held by men with little else in common. So, Hedley Bull: ". . . The Soviet Union did not embark upon the massive programme of intercontinental missile construction that had been anticipated, but seemed to settle for the sort of capability that in the United States is associated with the policy of 'minimum deterrence.'" *The Control of the Arms Race*, Frederich A. Praeger, New York, 2nd ed., p. xxii; and Richard J. Barnet and Marcus G. Raskin: ". . . Where we once believed that the Soviets were bent on surpassing the U.S. in military power, it now appears that '. . . they are quite willing to put up with a missile gap: Indeed, we have been running much of the arms race with ourselves." *After Twenty Years: Alternatives to the Cold War in Europe*, New York: Random House, 1965, p. 4.

11. Editorial, *The New Republic*, November 18, 1967.

dogma and the climate encouraged underestimating and discouraged its correction.

A distorting myopia followed from the close polemical focus of factions in and out of government on the very latest incremental change in Soviet force dispositions and its implications for the current year's U.S. budget, as compared to that of the preceding year. Momentary pauses in Soviet construction of launchers for one missile type, perhaps because new improved systems were being readied for deployment or because of bad weather, were seized on by outside advisors and by unnamed "highly placed officials" as an indication that Soviet programs were "tapering off", "levelling off", "slowing down", "petering out", "grinding to a halt".[12] Since, characteristically, massive Soviet efforts in research, development, testing and evaluation parallel a countercycle in deployment, and since Russian weather is notoriously intemperate, especially during their long winters when our budget debates start, there was plenty of room for confusion, ambiguity and self-deception inside and outside the U.S. government.

As for the public view, it was only to be expected that statements about increased Soviet missile deployments would be dismissed with a kind of naive cynicism: the slickers in the Pentagon are using their annual scare tactics in support of bigger budgets. Some outside advisors protested the government's " 'most outrageous' statements about the alleged buildup by Russia", whereas in fact, we were told, "the Soviet arms capability actually is tapering off." Dissonant sounds of reality were hardly audible in Establishment study groups meeting in Washington, Cambridge and New York. The successful attempt to save the predictions and the dogma on which they were based is quite as instructive as the performance of Sabbatai Zevi's followers, a sect that managed to survive and reinterpret a public prediction that the world would end in 1648 and even to acquire new more enthusiastic adherents; or the Millerites who gathered new followers after the world failed to end as Miller had predicted by March 21, 1844.[13] Students of the subject have observed that when predictions fail, this may only increase fervor and proselytizing for the dogma that led to the prediction. After all, it is in just such adversity that a dogma needs all the recruits it can get. Editorials and articles appear with ritual regularity in the *New York Times*, the *New Republic*, the *Christian Science Monitor*, *Scientific American* and elsewhere warning of the Pentagon's ritual exaggeration of the threat and presenting in full-blown form a generalized doctrine that it is just such exaggerations that accelerate the fatal spiral.

Though holders of the dogma of regular U.S. overestimation protested excessive secrecy, they were in good part protected by it. Exact quantitative comparisons of past predictions with reality take time and would have met much resistance even in private; in public a systematic long-term check was impossible. However, enough has long been public to undermine the theory of regular overestimation. We have had open official statements reflecting classified estimates that the Russians would not try to get as many missiles as the U.S., that they were stopping or slowing down; and equally public figures on the actual growth of Russian strategic forces. The contrast was plain, or rather would have been plain, if only we had been taking a long hard look; or even looking. More important, the reality of understatement should have destroyed the generalized theory of overstatement, but it did not.

It would be unfortunate if we should now swing from understatement to the opposite extreme. It would be nice, though far from easy, to get it nearly right. Even if we do, the implications for our strategic budgets will by no means be simple. Sober consideration, however, will discount the threat that invariably overestimating Soviet threats drives us to exponential increases and the notion that only throwing caution to the winds can stop the "race." The threat of invariable overestimation is one that is plainly exaggerated.

12. For this focus on the momentary or partial pauses, see, for example, the *New York Times*, April 27, 1969; the *Chicago Sun Times*, April 22, 1970; the *Milwaukee Journal*, April 26, 1970; *SIPRI Yearbook of World Armaments and Disarmament*, 1969–70, New York: Humanities Press, 1970 p. 53; the *Wall Street Journal*, December 17, 1970; the *Manchester Guardian*, November 7, 1971; *Survival*, September/October, 1972.

13. These two cases of failed predictions are described in Leon Festinger's *When Prophecy Fails*, Harper Torch Book, 1964 and in his *Theory of Cognitive Dissonance*, Stanford University Press, 1967. Festinger's model of cognitive dissonance fits the history of the theory of systematic overestimation rather well.

APPENDIX TO LEGENDS OF THE STRATEGIC ARMS RACE, PART I: THE DRIVING ENGINE

ON TESTING FOR BIAS IN FORECASTS OF ADVERSARY FORCES

I. Looking for upward bias, not simply random error, however large.

In "Legends" my purpose in comparing pre- and post-deployment estimates of Soviet strategic forces was not to establish the fact that there were errors or that the errors in prediction were—given the uncertainties and the circumstances in which they were made—unreasonably large. As the paper stresses, errors in predicting deployments, including deployments not visibly begun at the time of prediction, are nearly inevitable. Such predictions are intrinsically hazardous. They anticipate adversary decisions that may not yet have been made and that, once made, might be reversed. They are guesses, often informed guesses, that might conceivably be improved by more systematic backward looks. But they will remain conjectures.

The analysis of past errors does suggest some methods of detecting and adjusting for systematic bias earlier than in the past. However, such improvements were not a primary goal of the paper. The analysis in the paper is part of a larger look at strategic arms competition, and this part was mainly directed at testing the hypothesis that U.S. strategic spending had rapidly increased in good part because of a chronic tendency to overestimate adversary forces. In this connection it is important to see whether in fact the errors of estimation were mainly in the direction of exaggerating and to do this over a substantial period during which the claim was made that such exaggeration was the driving engine of the arms race.

II. Problems in finding predictions that are refutably definite.

A. Excessive Hedging

If we are going to find out whether certain predictions were right or wrong and how far off the mark they were, we have to deal with those that are not so hedged that they are irrefutable even in principle. Some hedging is not only prudent, but a candid and essential recognition of the range of uncertainty. But intelligence documents are often thickets of cautious hedges. "It is possible that . . .", "The adversary may conceivably . . . , but . . ." and so on. For example, in the summer of 1941, when a debate was raging over the issue of an aggressive move north or south by Japan, the estimate of Army Intelligence was that Japan would continue its policy of avoiding war, but that if she went to war, she would move either north or south. The estimate is phrased with enough verbiage to make it sound substantial:

> . . . Japan will probably continue to assemble, by gradual withdrawals from China, a field force for possible employment either in Southeastern Asia or against Russia. Her hopes of empire are bound up with an Axis victory and she is subject to strong German pressure to attack Russia at once; nevertheless it is believed that she will avoid precipitate action and will continue her policy of avoiding war with Russia on the one hand and with the United States and Great Britain on the other. If forced, or if selecting to choose between action against Russia or to the Southwest, she will be influenced by Germany's success against Russia . . . and by America's action, particularly as regards the distribution of United States' naval strength, and as regards attempts to send supplies to Russia through Vladivostok. Should the choice be the southward advance, it will probably consist of a containment of Hong Kong and the Philippine Islands while attacking British Maylaya via Thailand and Indo-China.[1]

Some statements about the future are too vague to be wrong. Others are numerically precise but turn out on examination to be tautolo-

1. *Hearings before the Joint Committee on the Investigation of the Pearl Harbor Attack*, 79th Congress, United States Government Printing Office, Washington, D.C., 1946, Part 3, p. 1039.

gies, algebraic or arithmetic truths rather than ventures in forecasting. A good many fall under the heading that includes, according to Austrian folklore, the meteorological example, "if the cock crows at precisely twelve o'clock, it will either rain or not rain." So it may be said that "if the Russians continue to build SS-9s at the current rate, by 197_ they will have deployed __ more than they presently have," a statement which appears to, but does not, add any new information to the factual estimate of what their current rate is. It cannot be refuted by future developments. It is true that some tautologies have an emotional impact (one thinks of "business is business" and "war is war") and this is in itself an interesting subject, but it is not the same subject as determining how far off the mark and in what direction are the refutably definite predictions.

B. Dubious Realities

To find out whether predictions are right or wrong, we need not only a prediction which is refutably definite; we have also to make some comparison of the prediction with the event predicted after it has occurred. But the event then has to be subject to observation and measurement with sufficient reliability to remove any substantial doubt and disagreement in the community of observers. Building underground intercontinental missile silos of great blast resistance involves massive construction activities that go on for a year and a half or so, and the silos themselves are embedded in a great deal of continuing support activity once constructed. They are subject to identification and counting by an adversary with modern reconnaissance equipment.

This is not to say that there are not some important uncertainties even here. Indeed reconnaissance experts such as Amrom Katz have long suggested that a substantial number of missiles might be hidden if the government doing the hiding wanted to depend on concealment rather than elaborate blast resistance for their safety.[2] Nonetheless one can at least say that silos of the kind that have been the subject of forecasts about Russian ICBM deployments appear to be quite visible to "the national technical means of verification" of the United States (to quote the SALT agreements). (If in fact there were in addition to these observable, countable complex blast resistant silos a substantial number of hidden missiles, then a finding that a prediction understated the reality would only be reinforced.) And the results of such observations can within fairly narrow limits be identified and counted with reasonable objectivity and with extensive agreement by the defense community. It is clear that a count made after mid-1967, say, of the number of such silos already deployed is vastly more reliable than a prediction made in 1963 as to the number that will be deployed by mid-1967, four and a half years hence. Even these current or historical estimates have some uncertainties of course. The number of silos already completed in mid-1967 was estimated in the fall of that year using a spread from 536 to 566: 551 ± 15. This was later adjusted to a point estimate of 570. These small uncertainties in interpretation of the numbers already in place at a given time are of a different order from the uncertainties of prediction. They contrast especially with the uncertainties of long-range predictions which, as Secretary McNamara said, depend not only on "present deployment trends", but on "economic, strategic and technical considerations". And these are necessarily much more hazardous than the sorts of uncertainties that enter into photo interpretation and the like.

High officials have frequently stated that our means of verification have greatly improved since the 1950s and this surely has also increased the assurance with which we can count elaborate complex silos with long construction times. In the 1950s before these improvements, the contrast in certainty between predictions and historical estimates was by no means as sharp even for counting silos.

Moreover there are a number of predictions, unhedged and precisely quantitative, which unfortunately refer to realities that are not likely to be open to verification and precise determination after the event. For example, many predictions in the early 1950s referred to the future size and mixture of Soviet bomb stockpiles. A prediction in 1951 of what the Soviets would have in 1956 might be compared with an estimate made after deployment in 1956, but the latter would itself be only a very hazardous guess. In general one would not expect stocks

2. See his "Hiders and Finders," *Bulletin of the Atomic Scientists*, Vol. XVII, No. 10, September 1961.

of nuclear bombs to be kept where they can be seen from high altitudes and counted by adversaries.

There are still many other cases today where the uncertainty for post-deployment estimates is so large that they are very far from a definitive test of pre-deployment estimates. This is particularly true for complex unit performance characteristics (like average system delivery errors or blast overpressure resistance). That is, post-deployment estimates of technical qualitative traits are likely to be much less reliable than those for numbers of vehicles in place. We can measure such technological traits only imperfectly even for our own systems. They are statistical, physical characteristics measurable only by a sequence of physical experiments under known controlled conditions. For example, the "Circular Error Probable" (CEP) of an ICBM, the median system delivery error, is a resultant of random and systematic errors from many sources: errors in guidance attributable to drifts in gyroscopes; to imperfections in the measurements of acceleration or to the approximations used by computers; non-guidance systematic errors or bias derived from flaws in geophysical and geodetic information; from the effects of winds, etc. on re-entry, and so on. When an adversary designs statistical experiments to determine such errors we would be unlikely to know all the relevant experimental conditions even if we knew the results. Post-deployment estimates of such complex realities can hardly be taken as a definitive test of the validity of predictions.

Comparing estimates before and after deployment in such cases does not deserve the title "comparing predictions with reality". It is more like comparing one informed guess with another, possibly better informed guess. The range of uncertainty will remain large and the estimates after deployment are still likely to be subject to substantial disagreement.

Predictions of adversary performance characteristics should also be regarded in a gingerly way. They should at any rate be scrutinized for familiar prejudices about the technical competence of the foreign power in question. For example, before Pearl Harbor American Intelligence underestimated both production rate and equipment performance of the Japanese.

> On December 1, [1941] Army Intelligence placed Japanese aircraft production at "200 per month for all combat types, both army and navy." The actual rate was 426 per month. It was also usual to consider Japan's pilot training inferior to ours, although their cadets averaged 300 flying hours as compared to 200 for U.S. cadets; their first-line pilots averaged about 600 flying hours; and their carrier pilots, about 800. Our descriptions of the Zero single-engine fighter underestimated its range (800 instead of 900 miles), its speed (250 statute miles per hour instead of 300—it was faster at high altitude than our P-40), and its maneuverability. The majority of U.S. naval officers believed that the sonar gear in Japanese destroyers was inferior, when it was actually four or five times more powerful than our own; and it was commonly believed that their ships were somewhat top-heavy, when they were not. The aircraft capacity of their carriers, the efficiency of their direction-finding stations, etc. were also underestimated. Even the common notion about poor Japanese eyesight seems to have been an unconscious factor in making performance estimates.[3]

Our errors in gauging Japanese skill and ingenuity were matched by equally serious errors on Japan's part in measuring the United States. Their efficient network of spies had a high record of accuracy in reporting quantitative data, but their planning staffs underestimated American tenacity and resolve, as well as the consequences of a superior productive capability.

III. Problems in data for testing bias in predictions.

Another principal difficulty with testing intelligence predictions is finding them in the first place. It is a little like the old-fashioned recipe starting, "Take 16 wild turkeys. . . ." Intelligence material is, for good reason, closely held and much of it is inaccessible even to those with clearances. While old forecasts and estimates of the kind we are using are less sensitive than current ones, this is only a modest comfort. Precisely because old intelligence has little direct, current use, it is also less likely to be on

3. Roberta Wohlstetter, *Pearl Harbor: Warning and Decision*, Stanford University Press, 1962, p. 337.

file in accessible spots. Then, of course, some sources of intelligence are more sensitive than others.

But there are other troubles with the data. These are complex matters involving many distinctions among the events predicted. For example, the vehicles counted in predictions and those in post-deployment estimates have to be comparable. Submarine-launched missiles may be ballistic or cruise. Their launch tubes may be on diesel or nuclear submarines. The submarines themselves may be started but not completed, or completed but not "deployable", and so on. The ICBMs have, of course, to be distinguished from intermediate range missiles, but then there is an hermaphrodite called "variable range ballistic missiles" which sometimes is included with the ICBMs, and sometimes not. Similarly, there are some ICBM launchers on the Russian side, as on our own, which are used as test beds. They are much more numerous on the Russian side and these *could* be used in actual operations. They are included in the predictions of some predictors, but not in others. In general, we have systematically excluded them from both forecasts and from estimates after deployment. However, the conventions as to inclusion and exclusion of various categories of missile and bomber vary from one year to another and do so especially in the Secretaries' prose. The fact that the Secretaries' predictions are a large part of the time embodied in prose rather than tables makes a search more difficult.

Furthermore, the Secretaries' prose contained, along with the bold refutably definite predictions, some that were rather ambiguous, and some that were so hedged as not to be refutable at all.

Finally, a major difficulty may be connected with one of the factors underlying the persistence of error in forecasts. This difficulty has to do with the familiar problem in government bureaucracies that most of the members of the organization are concerned with current problems, some urgent. An interest in history seems a luxury. A formal prediction by the Secretary dating back ten years is rather ancient history. Such limitations in the interest of individuals in history are compounded by the fact that the tour of duty of men who make long-range or medium-term predictions and estimates covers a much shorter span of years than the predictions themselves. Institutional memory may then be weaker than that of individuals.

Of course the *short*-range forecasts of ICBM deployment, etc. are more likely to be kept in the files long enough to be compared with reality. But these, where they are definite enough to be refuted by observation in a highly reliable way, are also the ones which don't go far beyond such observation of what is already completed or already started. Short-term forecasts are quite close to the actual. In sum, the intelligence predictions likely to be on file check out fairly well. The long-range predictions which are much more frequently in error are not kept in the files long enough to remind their owners that they were wrong.

Some of the same difficulties that plague our comparisons then, are a partial explanation of the persistence of a systematic bias. They also suggest that the situation can be improved by a systematic effort to keep checking the long, as well as the short, run predictions for drifts upward or downward.

IV. Focus on the uncertain element in forecasting cumulative deployments completed and operational at a future date.

Even in a program of rapidly increasing adversary deployment, some part of the cumulative future deployment of vehicles is likely to be well established by past and current observation at the time when forecasts are made. In 1969, with about a thousand ICBMs operational, a prediction about the total number cumulatively completed in mid-1970, can be quite securely based not only on the thousand or so already completed, but also on the numbers that had already been started in 1969, and which would in the normal course of construction be completed and operational by mid-1970. These silos that are started but still in the process of construction are subject to observation too, as is the normal construction time. A short-range prediction of cumulative missile deployments at an advanced stage of massive programs is then not likely to be far off the mark. Such massive programs have enough "inertia" to limit the disparities. Of course even short-term predictions of a program being rapidly phased out might be somewhat hazardous since phase-outs do not have the long visible

gestation periods common to the completion and operational deployment of ICBM silos and submarine missile launch tubes (about a year and a half for ICBM silos and about two and a half for submarines). Even relatively short-run prediction of the number of bombers in place can then be chancy if one expects a rapid phase-out when there is none, or vice versa.

Predispositions towards exaggeration or understatement have room to come into play to the extent that forecasts are uncertain, that is, they go beyond what is observable. They are therefore the most relevant. We are interested specifically in predictions that depend on "economic, strategic, and technical considerations" and that attempt "to anticipate production and deployment decisions which our opponents, themselves, may not yet have made."[4] Such predictions are frankly presented as "highly conjectural".[5]

This paper "Legends of the Strategic Arms Race" has therefore concentrated on predictions that go beyond what was observable on the date the prediction was made. Even a long-range prediction that goes beyond observables, if it forecasts cumulative deployments to be completed at a future date, will have a large component that does not go beyond what has already been observed to be completed or started. A prediction made in 1969 about what will be cumulatively deployed in 1972 will have a large "inertial" component. On the other hand it will also contain the more hazardous component of silos or launch tubes not yet visibly started at the time the prediction was made, but forecasted to be started and completed by the date referred to in the prediction; and bombers which are expected to be phased out, though there is no visible indication at the time of the prediction that they will be.

To isolate the *change* from what is observable to what is hazarded as likely to happen, it would be convenient also to subtract out the inertial component in the long-range prediction. Four sets of tables therefore are included. The first set (Tables I-1 to I-4) deals with cumulative deployments that go beyond the observable. The second set (Tables II-1 to II-3) deals with the cumulative short-range predictions that are based only on observed completions and visible starts, and a third set (Tables III-1 to III-2) deals not with cumulative deployments but the change in deployment. A final fourth "set" (Table IV-1) consists in a single table presenting the estimates made after actual deployment of the number of ICBM and SLM launchers and heavy and medium bombers.

The second set of short-term cumulative predictions is, as would be expected, much closer to the mark than the more hazardous cumulative forecasts that go beyond observation. Their mid-range is on the average within two or three per cent. They are of interest here mainly for their contrast with the predictions that go beyond observables and as an aid for isolating errors in predicting changes in deployment as distinct from cumulative deployments. They are useful in separating the historical element in forecasts of future deployments. The third set of tables deals with such changes from what is reasonably well known to what can only be conjectured.

Two methods might be used for isolating the future change from the historical element in the cumulative forecasts. The method used in the tables presented in "Legends of the Strategic Arms Race" approximates it roughly by taking the long-run forecasts and making certain plausible or generous assumptions about (a) the last date preceding the Secretary's prediction when he would have had observations on what had been already completed or visibly started, (a seven or eight month lag was assumed) and (b) the normal construction time (about thirty months for submarine launch tubes and about eighteen months for ICBM silos).

A second method that is likely to be a better approximation makes use of the short-term estimates. Ideally what we would want in order to isolate the predictive, uncertain element in cumulative forecasts would be the predictor's estimate at the time of the prediction as to:

(a) What was already completed at the start of the interval.

(b) The number of units already in process at the time of the prediction expected to be completed sometime within the interval.

The sum of (a) and (b), however, is essentially what is supplied in the short-term estimates. If therefore the short-term predictions

4. Statement of Secretary of Defense, Robert S. McNamara, before the House Armed Services Committee on FY 1965–69 Defense Program and 1965 Defense Budget. January 27, 1964.
5. Ibid.

are subtracted from the corresponding long-term predictions, the remainder is a better measure than our first approximation of the portion of the cumulative deployment that was expected to be newly started and completed in the interval. The measure so computed approximates the *net* number started and completed by the end of the period, since the difference between the short-term and long-term predictions is clearly a forecast about a change in inventory between the years for which the two predictions are made (the "target years" of the two predictions). The measure therefore is net of the estimated withdrawals between the target years.

LIST OF TABLES

Table I-1

ICBM LAUNCHER PREDICTIONS THAT GO BEYOND OBSERVABLES
Predicted Operational Soviet ICBM Launchers Compared to the Actual Number

		Predicted and Estimated Actual Inventory				
Date Prediction Was Made (First quarter of year)	*Date Referred to in the Prediction (mid-year)*	*Predicted Number*	*Estimated Actual Inventory*	*Ratio: Predicted to Estimated Actual*		
				Low	*High*	*Mid-Range*
1962	1967	350–650	570	0.61	1.14	0.88
1963	1967	300–600	570	0.53	1.05	0.79
	1968	475–700	858	0.55	0.82	0.68
1964	1967	325–525	570	0.57	0.92	0.75
	1969	400–700	1028	0.39	0.68	0.54
1965	1967	330–395	570	0.58	0.69	0.64
	1970	410–700	1299	0.32	0.54	0.43
1966	1970	505–795	1299	0.39	0.61	0.50
1967	1971	805–1080	1513	0.53	0.71	0.62
1968	1972	1020–1251	1527	0.67	0.82	0.74
1969	1972	1158–1276	1527	0.76	0.84	0.80
			Average:	0.54	0.80	0.67

By "predictions that go beyond observables" we mean those extending far enough into the future to include in the cumulative estimate (besides estimates of launchers already completed and those started but not yet completed at the time when the prediction was made) those that were expected to be newly started after the time when the prediction was made and completed by the future date referred to in the prediction. For ICBM launchers these are the predictions for more than 18 months into the future. (Since these predictions, short- and long-range, were presented in January or February and always referred to mid-years, they are for 4 or 5 months ahead, or for that plus some whole multiple of twelve, i.e., 16–17 months, 28–29 months and so forth.)

Only predictions referring to mid-1972 or before are included in the Table since data on estimated actual inventories beyond mid-1972 were not available at the time of the analysis.

All formal ICBM predictions of the Secretaries of Defense from 1962 through 1972 satisfying the above conditions are included in the Table.

Table I-2

SLM LAUNCH TUBE PREDICTIONS THAT GO BEYOND OBSERVABLES

Predicted Operational Soviet Submarine-Launched Missile Launchers Compared to the Actual Number

Date Prediction Was Made (First-quarter of year)	*Date Referred to in the Prediction (mid-year)*	*System(s) Predicted*	*Predicted Number*	*Estimated Actual Inv.*	*Ratio: Predicted to Est. Actual*		
					Low	*High*	*Mid-Range*
1962	1966	SLBMs and SLCMs	174	371–399 [a]	0.45	0.45	0.45
1963	1966	SLBMs and SLCMs	306	371–399 [a]	0.79	0.79	0.79
	1967	SLBMs and SLCMs	342	427	0.80	0.80	0.80
1964	1969	Total SLBMs	185–236	196	0.94	1.20	1.07
1965	1970	Total SLBMs	157–248	304	0.52	0.82	0.67
		SLBMs and SLCMs	401–628	674	0.59	0.93	0.76
1966	1970	Total SLBMs	120–220	304	0.39	0.72	0.56
		SLBMs and SLCMs	440–615	674	0.65	0.91	0.78
1967	1971	Total SLBMs	185–229	448	0.41	0.51	0.46
		SLBMs and SLCMs	565–645	852–854 [a]	0.66	0.76	0.71
1968	1972	SLBMs—Nuc.[b]	267–318	440	0.61	0.72	0.66
		Total SLBMs	340–391	500	0.68	0.78	0.73
1969	1972	SLBMs—Nuc.[b]	286–494	440	0.65	1.12	0.89
		Total SLBMs	356–564	500	0.71	1.13	0.92
		SLBMs and SLCMs	712–920	920–922 [a]	0.77	1.00	0.89
				Average:	0.64	0.84	0.74

[a] In those cases where the estimated actual inventory is a range, the midpoint of this range is used in computing the ratios of predicted to estimated actual inventories.

[b] SLBMs on nuclear-powered submarines.

For the meaning of "predictions that go beyond observables" here, see notes to Table I-1. For SLM launchers these are the predictions for more than two and one-half years into the future.

Only predictions referring to mid-1972 or before are included in the Table since data on estimated actual inventories beyond mid-1972 were not available at the time of the analysis.

All formal SLM predictions of the Secretaries of Defense from 1962 through 1972 satisfying the above conditions are included in the Table.

Table I-3

BOMBER PREDICTIONS THAT GO BEYOND OBSERVABLES

Predicted Operational Soviet Heavy and Medium Bombers Compared to the Actual Number

Date Prediction Was Made (First quarter of year)	Date Referred to in the Prediction (Mid-Year)	Bomber/ Tanker System Predicted	Predicted and Estimated Actual Inventory: Predicted Inventory	Estimated Actual Inventory	Ratio: Predicted to Est. Actual: Low	High	Mid-Range
1965	1967	Heavy	170–210	210	0.81	1.00	0.90
		Medium	540–755	750	0.72	1.01	0.86
		Total	710–965	960	0.74	1.01	0.87
	1970	Heavy	140–180	195	0.72	0.92	0.82
		Medium	290–510	730	0.40	0.70	0.55
		Total	430–690	925	0.46	0.75	0.61
1966	1967	Heavy	185–215	210	0.88	1.02	0.95
		Medium	540–725	750	0.72	0.97	0.84
		Total	725–940	960	0.76	0.98	0.87
	1970	Heavy	155–195	195	0.79	1.00	0.90
		Medium	300–550	730	0.41	0.75	0.58
		Total	455–745	925	0.49	0.81	0.65
1967	1968	Heavy [a]	140–155 [a]	155 [a]	0.90	1.00	0.95
		Medium	475–580	730	0.65	0.79	0.72
		Total [a]	615–735 [a]	885 [a]	0.69	0.83	0.76
	1971	Heavy [a]	105–130 [a]	145 [a]	0.72	0.90	0.81
		Medium	300–425	710	0.42	0.60	0.51
		Total [a]	405–555 [a]	855 [a]	0.47	0.65	0.56
1968	1969	Heavy [a]	140–155 [a]	145 [a]	0.97	1.07	1.02
		Medium	600–675	725	0.83	0.93	0.88
		Total [a]	740–830 [a]	870 [a]	0.85	0.95	0.90
	1972	Heavy [a]	105–130 [a]	145 [a]	0.72	0.90	0.81
		Medium	425–550	635–690 [b]	0.64	0.83	0.74
		Total [a]	530–680 [a]	780–835 [a, b]	0.66	0.84	0.75
1969	1970	Heavy [a]	135–140 [a]	145 [a]	0.93	0.97	0.95
		Medium	625–725	730	0.86	0.99	0.92
		Total	760–865 [a]	875 [a]	0.87	0.99	0.93
	1972	Heavy [a]	115–135 [a]	145 [a]	0.79	0.93	0.86
		Medium	500–600	635–690 [b]	0.75	0.91	0.83
		Total [a]	615–735 [a]	780–835 [a, b]	0.76	0.91	0.84
1970	1970	Heavy [a]	135–140 [a]	145 [a]	0.93	0.97	0.95
		Medium	675–760	730	0.92	1.04	0.98
		Total [a]	810–900 [a]	875 [a]	0.93	1.03	0.98
1971	1971	Heavy	175–195	195	0.90	1.00	0.95
	1972	Heavy	165–195	195	0.85	1.00	0.92
1972	1972	Heavy	190	195	0.97	0.97	0.97
			Averages:	Heavy	0.85	0.98	0.91
				Medium	0.67	0.87	0.76
				Total	0.70	0.89	0.79

[a] Heavy tankers not included.

[b] In those cases where the estimated actual inventory is a range, the midpoint of this range has been used in computing the ratios of predicted to estimated actual inventories.

Throughout the period analyzed the Secretaries of Defense predicted level or reduced heavy and medium bomber forces. Since the withdrawal of bombers from a strategic force is not preceded by a substantial period of visible preparation, all the bomber predictions "go beyond the observables" in the sense defined for Tables I-1 and I-2, and all formal bomber predictions by the Secretaries from 1962 through 1972 referring to mid-1972 or before are included in the table. Data on estimated actual inventories beyond mid-1972 was not available at the time of analysis.

Table I-4

RATIO OF PREDICTED TO ESTIMATED ACTUAL INVENTORY—SUMMARY STATISTICS
Predictions that Go Beyond the Observables

	ICBMs			*SLMs*			*BOMBERS*								
							Heavy			*Medium*			*Total*		
	Low	*High*	*Mid-Range*	*Low*	*High*	*Mid-Range*	*Low*	*High*	*Mid-Range*	*Low*	*High*	*Mid-Range*	*Low*	*High*	*Mid-Range*
Mean	0.54	0.80	0.67	0.64	0.84	0.74	0.85	0.98	0.91	0.67	0.87	0.76	0.70	0.89	0.79
Standard Deviation	±0.12	±0.17	±0.13	±0.15	±0.21	±0.16	±0.09	±0.05	±0.06	±0.18	±0.14	±0.15	±0.16	±0.12	±0.13
Upper Quartile	0.62	0.95	0.80	0.77	1.03	0.89	0.93	1.00	0.95	0.84	0.99	0.89	0.85	0.99	0.91
Median	0.55	0.82	0.69	0.66	0.80	0.76	0.87	0.99	0.94	0.72	0.91	0.83	0.74	0.91	0.84
Lower Quartile	0.39	0.66	0.53	0.50	0.72	0.64	0.76	0.93	0.84	0.42	0.73	0.57	0.49	0.80	0.64
Number of Predictions	11	11	11	15	15	15	14	14	14	11	11	11	11	11	11

Table II-1

NUMBER OF SHORT-TERM PREDICTIONS OF MISSILE LAUNCHERS EXCEEDING AND NUMBER BRACKETING THE ESTIMATED ACTUAL INVENTORY

Predictions that Depend only on Observed Missile Launchers Completed, Observed Launcher Starts, and Estimated Rates of Completion

	ICBMs	*SLMs*
Low Predictions that exceed actual	4 of 11 (36%)	9 of 25 (36%)
Mid-Range of Predictions that exceed actual	6 of 11 (55%)	12 of 25 (48%)
High Predictions that exceed actual	7 of 11 (73%)	19 of 25 (76%)
Number of Predictions that *Bracket* Actual	4 of 11 (36%)	12 of 25 (48%)

Table II-2

RATIO OF SHORT-TERM PREDICTIONS OF MISSILE LAUNCHERS TO ESTIMATED ACTUAL NUMBER—SUMMARY STATISTICS

Predictions that Depend only on Observed Missile Launchers Completed, Observed Launcher Starts, and Estimated Rates of Completion

	ICBMs			*SLMs*		
	Low	*High*	*Mid-Range*	*Low*	*High*	*Mid-Range*
Mean	0.95	1.03	0.99	0.96	1.11	1.04
Standard Deviation	±0.11	±0.11	±0.11	±0.18	±0.19	±0.18
Upper Quartile	1.06	1.12	1.09	1.10	1.16	1.12
Median	0.98	1.01	1.00	0.91	1.07	0.99
Lower Quartile	0.86	0.92	0.89	0.82	1.00	0.92
Number of Predictions	11	11	11	25	25	25

Table III-1

NUMBER OF PREDICTED *INCREASES* EXCEEDING AND NUMBER BRACKETING THE ESTIMATED ACTUAL INCREASE

Predictions that Go Beyond the Observables

	ICBMs	*SLMs*
Low Predicted Increases that Exceed Actual	0 of 11 (0%)	0 of 12 (0%)
Mid-Range of Predicted Increases that Exceed Actual	0 of 11 (0%)	0 of 12 (0%)
High Predicted Increases that Exceed Actual	1 of 11 (9%)	3 of 12 (25%)
Number of Predictions that *Bracket* Actual	1 of 11 (9%)	3 of 12 (25%)

Table III-2

RATIO OF PREDICTED *INCREASES* COMPARED TO ESTIMATED ACTUAL INCREASES—SUMMARY STATISTICS

Predictions that Go Beyond the Observables

	ICBMs			*SLMs*		
	Low	*High*	*Mid-Range*	*Low*	*High*	*Mid-Range*
Mean	0.24	0.60	0.42	0.35	0.69	0.52
Standard Deviation	±0.14	±0.26	±0.19	±0.23	±0.32	±0.26
Upper Octile	0.49	1.09	0.76	0.61	1.25	0.93
Upper Quartile	0.33	0.81	0.56	0.61	1.00	0.74
Median	0.23	0.48	0.34	0.36	0.63	0.48
Lower Quartile	0.14	0.41	0.28	0.12	0.40	0.24
Lower Octile	0.05	0.33	0.20	0.05	0.32	0.22
Number of Predictions	11	11	11	12	12	12

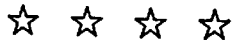
Table IV-1

OPERATIONAL SOVIET ICBM LAUNCHERS, SLM LAUNCH TUBES, AND BOMBERS ESTIMATED AFTER ACTUAL DEPLOYMENT

	ICBMs	SLBMs and SLCMs					BOMBERS				
			SLBM Launchers					Heavy			
Mid-Year	Total Launchers*	SLCM Launchers	Diesel	Nuclear	Total	Total SLBM and SLCM Launchers	Medium Bombers	Tankers	Bombers	Sub-Total	Total Heavy and Medium (inc. tankers)
1963	91	–	80	27	107	–	925–950			195–215	1120–1165
1964	188–191	152–160	80	27	107	259–267	800–875	40–45	160–175	200–220	1000–1095
1965	224	208–216	80	27	107	315–323	770–820	45–55	160	205–215	975–1035
1966	250	264–292	80	27	107	371–399	745–785	45–55	155	200–210	945–995
1967	570	320	80	27	107	427	750	50	160	210	960
1968	858	348	78	43	121	469	730	50	155	205	935
1969	1028	368	76	120	196	564	725	50	145	195	920
1970	1299	370	72	232	304	674	730	50	145	195	925
1971	1513	404–406	72	376	448	852–854	710	50	145	195	905
1972	1527	420–422	60	440	500	920–922	635–690	50	145	195	830–885

* Excluding test-site and training launchers.

End of Part I



LEGENDS OF THE STRATEGIC ARMS RACE, PART II: THE UNCONTROLLED UPWARD SPIRAL

by

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STRATEGIC REVIEW, Fall 1974, offered Part I, "The Driving Engine," of the Wohlstetter essay on *Legends of the Strategic Arms Race* which dealt with the view that the United States persistently overestimates, by a wide margin, the size of future Soviet offensive forces. In fact, the essay showed that from the early 1960s the United States projected smaller Soviet forces than those actually deployed.

Part II, "The Uncontrolled Upward Spiral," presented herewith, documents a more serious defect in contemporary views of U.S. defense planning: namely that the U.S. has persistently expanded its strategic budget, strategic forces and the indiscriminate destructiveness of its weapons in response to this supposed overestimate of the adversary. Part II documents the fact that strategic budgets have decreased drastically.

Legends of the Strategic Arms Race makes plain that projections of Soviet forces below the levels actually deployed were accompanied by decreasing strategic budgets. Thus, American defense and foreign security policies are subjected to double jeopardy, which raises fundamental questions about the viability of the military intelligence process and the soundness and adequacy of national security planning.

One can find a dangerous parallel between the current situation and that which prevailed on the eve of World War II when the Free World failed to perceive the ominous events which were in motion, and pursued policies which invited aggression.

(*Parts I and II, somewhat abridged, were published in the Summer and Fall 1974 issues of* Foreign Policy.)

A. G. B. METCALF
Strategic Studies Editor

LEGENDS OF THE STRATEGIC ARMS RACE, PART II: THE UNCONTROLLED UPWARD SPIRAL

ALBERT WOHLSTETTER

According to a principal element of post-Sputnik doctrine on the strategic "race," systematic overestimation of future adversary strategic forces is the driving engine of the arms spiral on our side: We invariably expect the Russian programs to be larger than they turn out to be; we compound this overestimate by "worst case" analysis, cautiously overdesigning our programs to meet a Russian threat greater even than the one we expect—only to find, when the Russian threat turns out to be less than expected, that we have irrevocably committed ourselves to new and higher levels of spending on strategic forces. So according to the received doctrine.

But not in reality. The first part of this article showed that after the brief period of the "missile gap," a theory of regular overestimation grew with the fact of underestimating the size of future Soviet offense forces. In annual presentations of programs and budgets to Congress by two Defense Secretaries, fifty-one predictions go beyond the observable to include expected changes in offense deployments that had not yet been visibly started. In general such extended predictions are most relevant for American decisions about development and deployment, since these take many years to come into effect. Such predictions that go beyond observation moreover leave the most room for the exercise of judgment or prejudgment, and so room for any tendency to exaggerate or understate Soviet force plans. In forty-nine out of fifty-one cases the eventual Soviet deployment exceeded the mid-range of the Secretaries' estimates. In forty-two of the fifty-one, it exceeded the Secretaries' high.

Moreover, the underestimates were substantial. If one considers not the cumulative deployments predicted, but the expected change from what had already been observed, the difference between the reality and the estimates was very large indeed. The actual increase in missiles was, on the average, double the expected number or more.

But the trouble with received doctrine on strategic action and reaction lies not only in its factual error about regular overestimation. It has even more to do with the reasoning that presumes that overestimation inevitably means overreaction; that if one side, say the United States, expects a large increase in adversary capability, it will decide to meet or exceed that added capability. The iron law that is supposed to govern strategic action (For Every Action, There Is An Opposing Equal Or Greater-Than-Equal Reaction) is made in fact of plastic. Even if the United States had overestimated or merely correctly estimated the rapid rise in Soviet strategic forces, it might or might not have responded by rapidly increasing its own strategic forces. That would depend on whether the effort seemed worth the sacrifice of other goals. To take one major case, it was the growing substantial size and potential further expansion of Soviet offense forces that McNamara identified as the reason for *not* going ahead with a thick ballistic missile defense of American cities. Here one side anticipated major action by the other; and chose inaction.

And there are also cases where anticipating adversary inaction leads to action. So a government that prefers a favorable force ratio compared to its adversary, but does not regard this as a good beyond price, might undertake programs to achieve it if the price is right—which it might be if the adversary (perhaps through fear of an arms race?) was expected not to offset the numerical advantage. (Opposing sides may not equally fear an arms race, as can be documented in the case of the British and the

This article, as the first, is based on the forthcoming *Competition or Race? Innovation and the Changing Size of Strategic Forces* by Albert Wohlstetter, Fred Hoffman, David McGarvey, and Amoretta Hoeber.

Germans in the 1930s.) This might in part explain the Russian decision to increase their missiles beyond the numbers deployed by the Americans.

But we have less speculative examples. A historic case where Americans plainly discounted future Russian capabilities and where that low estimate led to large-scale spending is the massive continental defense programs we undertook in the 1950s. These were based, among other things, on understatements of the future significance of adversary fusion and strategic rocket technologies. The initial influential studies assumed that our continental defenses would not have to cope with ICBMs before the late 1960s, and that fusion weapons had little or no strategic importance for either side. Fusion weapons were assumed to be strategically redundant (not merely morally questionable), since it was believed that (a) they were usable only against cities; (b) except for the very few largest cities, they exceeded in destructiveness what was needed for their demolition; and (c) any one of these large cities could be leveled in any case by a small number of fission weapons.

Those who were for large continental defense programs and against fusion weapons clearly premised their judgment on underestimates of the importance for an adversary offense of fusion and also of rocket technologies. However, the political-military significance of such technologies is complex and uncertain, and the difficulties are not partisan matters. It is an interesting fact that those who felt that deploying fusion weapons was important nonetheless shared some of the same mistaken beliefs as to what their role was to be. They also believed that fusion technology meant essentially much bigger bombs. (In fact it made medium- and low-yield bombs smaller, lighter, and cheaper, and this in turn made it feasible to use them in missile and other systems more easily capable of surviving attack and penetrating defenses.) So far as strategic rockets were concerned, some initial and transient limitations in their physical performance, in particular their great inaccuracies, shaped some of the basic presuppositions about the alternatives for strategy and arms control that are still very widely held.

It is worth elaborating somewhat on these early expectations—as to the technical facts of rocketry and fusion—since they were the premises from which most men, even those of widely differing predispositions, derived quite durable judgments as to whether there are policy alternatives. The premises have eroded steadily over time, but the policy inferences drawn from them persist.

The Initial Debate . . .

The initial debate on fusion weapons inside the government talked of weapons with an explosive yield equal to 40 or 25 million tons (megatons) of chemical explosives. A traditional strategic target like a steel mill might be destroyed by a 40 megaton weapon if it were anywhere within a circle of 87 square miles; and brick houses not targeted would collapse within an area of 416 square miles around the point of detonation. "Like it or not," even its proponents were in the habit of saying, "the H-bomb is a city buster." No one, of course, for or against it, really "liked" it. And specifically no one liked what seemed to be its inevitable indiscriminate destructiveness.

Even if powerful first impressions about the implications of a technology were easier to change than they are, the initial inferences about targeting as well as collateral damage drawn from the debate on fusion weapons might not have altered with improvements in our understanding of fusion technology. For the inferences were soon reinforced by the apparent implications of the inaccuracy of strategic rockets. The U.S. strategic rocket program in the mid-1950s was made feasible by a drastic loosening of the requirements imposed for accuracy. The inaccuracies then permitted greatly exceeded those of manned bombers dropping gravity bombs. We expected median delivery errors in our ICBMs of three to five nautical miles, which would have meant that, out of a large number of bombs so aimed, half would have fallen outside of a circle of twenty-eight to eighty square miles—*and this neglecting "gross errors."* The initial design for Polaris implied that half its shots would lie outside of a fifty square mile area. And at the end of the decade, while we were overestimating the initial Soviet ICBM deployment, we were still understating its initial accuracy. We assumed an eighty square mile median circle of error for the Soviets.

Even the first American and Soviet strategic rockets were more accurate than we had expected. It is clear now from public information

that the area of the median circle of error for strategic rockets has long been measured in tenths of a square mile; it will, I believe, soon be measured in hundredths, and in the long run, in thousandths or less.

Nonetheless the first impressions of enormous inaccuracy and wholesale destructiveness most powerfully influenced our views as to where we have political choices and where we face a bare unalterable technical condition. We need now to rethink the basic technologies and the developments and directions that they have taken since our first understanding of them. I believe our present conceptions are in great disarray as to what military alternatives are feasible, the political sense of these military alternatives for alliances, for the control of arms, and for the long-term interest of world order. Not the least affected by the transient technical context in which it was formed is the characteristic doctrine of the strategic arms race that has flourished since Sputnik.

The "invariable overestimate," "worst case" dynamic is only one of three distinctive components of recent strategic arms race theory. Perhaps the most remarkable and uniquely new element of the post-Sputnik doctrine, distinguishing it from the arms race theories of the 1940s as well as those of the interwar period, was the idea that an exponential race could be avoided only by tying strategic forces to the destruction of population rather than to opposing military forces. The origins of this paradoxical view are visible in a study issued one month after Sputnik by the Naval Warfare Analysis Group, then at the Massachusetts Institute of Technology, and in its 1960 follow-up study. According to the 1957 study, the objective of strategic forces should be to destroy "the softest target system that will do the job of deterrence, *viz.*, at present population." Enemy population targets, according to the authors, are "a particularly easy, and possibly the only practical, form of targeting for long-range missiles." (And indeed they were, given the inaccuracies then anticipated.)

Deterrence in these documents meant not simply a second strike capability, as that concept was originally defined years before Sputnik and offensive missiles. It meant retaliation in a sense that made it inappropriate to direct strategic weapons at anything other than population. Moreover it enabled one to fix a definite ceiling on requirements:

> Retaliatory (revenge) war force requirements: at most 1,000 megatons. Target: enemy population. Revenge against inanimate objects is senseless, hence, people are the target of retaliation. Urban concentration strongly reduces attack force requirements for decimation and complete social disorganization.[1]

But, the study said, if one aimed strategic weapons at opposing strategic forces (typically it conceived only these two alternative targets: population or strategic forces), the *floor* under requirements would be at least 10,000 megatons. And the follow-up study suggested that there would be no ceiling. Attacks on enemy striking forces would "require practically unlimited forces and practically unattainable Intelligence information for their meaningful implementation; and they guarantee an unstable arms race by tying our own offensive force requirements to the enemy's."[2] The only way out is to cut the tie to opposing enemy forces and to aim strategic weapons exclusively at populations.

That this link to the destruction of population rationalized an apparent inability of the initial strategic rockets to do anything else is suggested by the fact that for every other variety of military force the studies called for a policy of graduated deterrence based on "possession of a spectrum of nuclear weapons down to the lowest yield and/or improved conventional weapons." Postulate I of the study concerned massive retaliation. Postulate II, on graduated deterrence, had it that "either opponent can meet the application of limited destructive force with effectively equal or with greater force." Clearly Postulate II "ties" this extremely broad spectrum of American military force to the kind and size of opposing military forces. Such a connection, of course, is traditional. One might just as well have reasoned that (in parallel with Postulate I) if we bought conventional military forces to destroy adversary military forces, our adversary could always buy additional forces to offset our increased capability, and we in turn would have to buy

1. Study 5 of the Naval Warfare Analysis Group, November 1957, p. 12.
2. Study 62-60 of the Naval Warfare Analysis Group, July and October 1960, p. 3.

more forces to offset these, and so on ad infinitum. Interwar arms race theories did presuppose an explosive connection between the decisions of two states to acquire arms, leading to just such a non-nuclear arms spiral. However, the theory had little relation to reality, and never before or after Sputnik did it lead strategists and opponents of arms races to the extraordinary suggestion that opposing theater forces should be aimed exclusively at villages rather than at each other.

The Naval Warfare Analysis studies were done by able operations analysts. Yet it is easy to identify parochial bureaucratic elements in their work. The expected shift in the pattern of warfare, according to Study 5, implied "a growing importance of the 'old-fashioned' services. The burden of supporting national policy falls again (or still) on ships and soldiers, which must be available in adequate strengths to implement Postulate II." However, in the aftermath of Sputnik, the support for population bombing as a way to avoid a strategic arms race came from a very wide range of persons. There were Army versions of the argument (that made an exception for Nike missiles) and versions in the Weapons Systems Evaluation Group of the Joint Chiefs. A National Planning Association study group presented essentially the same view in *1970 Without Arms Control* (1958). The group was headed by Colonel Richard Leghorn, formerly an Air Force Development Planner, and included three senior members of RAND, W. C. Davidson (a Quaker physicist), Norman Cousins, John Loosbrock (editor of *Air Force*), and David Riesman. And the view continues to underlie a very wide range of opinion on arms races today.

Quality vs. Quantity

The third essential element in the post-Sputnik arms race doctrine is the peculiarly destabilizing role assigned to technological innovation. It is qualitative change especially that is supposed to set off a new round in the race, leading to new and higher force and budget levels. In a kind of reversal of the Hegelian dialectic, *quality*, so to speak, *becomes quantity*. This idea is not quite as unique as the notion that targeting anything other than a fixed number of population centers would generate an arms race. However, in the post-Sputnik version, the two are closely related. For it is innovation in weapons aimed at other weapons that is supposed to be peculiarly dangerous. This applies with particular force then to innovations in active defense, such as ABM, since unlike offense vehicles, these can *only* be aimed at incoming weapons, not at population.

In fact, actual American practice has always included strategic targeting of military forces, and it has never abandoned technical improvements in the ability effectively to destroy opposing military forces. According to the theory then, this practice should have generated exponential increases in arms, at least on the American side, if we were racing with ourselves in the guise of imaginary Russians. And on the Russian side too, unless they had adopted the policy of targeting only a small number of population centers, as used to be suggested in the mid-1960s. The results of this exponential race, according to the theory, should have been not only (a) an increase in U.S. strategic budgets, but also (b) a steady increase in the sheer indiscriminate destructiveness of our strategic weapons, (c) a decrease in our security, and (d) an increase, driven by a technology that has lost all relation to human purpose, in a tendency of our forces to get beyond political control.

Some variants of bureaucratic theories of the arms competition discount any tight interconnection between U.S. and Soviet weapons choices of the sort posited in the standard action-reaction theory, but do suggest exponential increases, at least on our side, as the result of an explosive intramural race among the services. In fact, the extreme variant is at the opposite pole from the standard action-reaction theory (even though the two are sometimes held by the same person simultaneously). At the extreme, the "race with ourselves" is taken to mean no connection at all between our weapons decisions and Russian behavior. According to Congressman Aspin, "The competition, always, in our Defense Department is never the Soviet Union. It is the offense vs. the defense; it's the Army vs. the Navy. That's where the real competition is."[3]

There is no doubt about the great importance of bureaucratic factors in understanding decisions to develop, buy, and deploy military

3. Telecast on the Public Broadcasting Service, "Firing Line," May 26, 1974. Copyright Southern Educational Communications Association, transcript p. 7.

forces. However, the importance of bureaucratic factors does not imply an exponential—or in fact any—rise in strategic spending. Many other parts of the defense and nondefense bureaucracy compete for the budget and some are devoted to cutting it. Nothing in the fates decrees that advocates of increased rather than decreased strategic spending invariably or usually win that competition. Moreover, I know of no well-established part of bureaucratic theory that suggests hyper-responsiveness, or mad tossing about of funds, or systematically innovative behavior rather than sluggishness and resistance to change.

In any case, whatever the explanation offered for the strategic race, there is a prior question as to whether there has been a race to be explained. To justify the term "race," any side that is racing has at least to be rapidly increasing its strategic budgets and forces. Even if the increase does not proceed at an increasing rate, for the name "race" to make any sense at all, there would have to be at the very least an increasing trend. An examination of American strategic budgets and forces since the mid-1950s suggests that on the principal relevant measures the trend is down. And an examination of the net effect of qualitative innovation in the strategic forces over the same time period equally refutes the stereotype.

A Quantitative Spiral?

Total Explosive Energy And "Overkill": The total explosive energy that could be released by the strategic stockpile is a measure frequently used to compare U.S. and Soviet forces by conservative organizations, such as the American Security Council. It also appears in the popular vivid comparisons of the total explosive yield of all the bombs dropped in Korea (200,000 tons) or in World War II (5,000,000 tons) with the explosive yield (measured in tons of some non-nuclear chemical explosive such as TNT) of a single nuclear warhead, several of which might be carried in one vehicle today. However, the drawbacks of such a measure are clear and most obvious in the vivid comparisons. A single bomb releasing five million tons of explosive energy (i.e., a five megaton weapon) is incapable of doing anything like the damage done worldwide from Japan and Burma to West Europe and Russia by the many tens of thousands of bombs exploded in World War II, even if the total energy yield were the same. In general, one large warhead with twice the energy yield of two smaller weapons, unlike them, cannot be used to attack two very widely separated targets.

Moreover it was understood at the dawn of the atomic age that, even though the Hiroshima bomb had roughly one thousand times the explosive yield of one of the largest World War II blockbusters, it would not do structural damage to an area one thousand times the size, but roughly one-tenth that. By comparison with the smaller bomb, some 90 per cent of its energy would be "wasted" in "overhitting" or "overdestroying" or "overkilling" the nearby area.[4] For that comparison then, not 1,000, but its two-thirds power, 100 is a roughly correct approximation for determining relative structural damage. And even in comparing the destructive effect of stocks of bombs that are less varied in yield, some such adjustment is essential.

However, it is not only conservative polemic that exploits the misleading measure of gross "megatonnage" of explosive energy. Some of the crudest polemical uses are by opponents of increases in military budgets. In talking of "overkill," they usually divide the total population of the world into the aggregate explosive energy in the stockpile to arrive at some such figure as ten tons of TNT equivalent for every man, woman, and child in the world. Such a measure makes exactly the confusion that the original discussions of overhitting or overdestruction of the area near the target were designed to avoid. And it adds several other more potent confusions besides. It implies that the purpose of stocks of weapons is and should be exclusively to destroy population, that what is wrong is not the killing of populations, but their overkilling. It is not strictly related to hypotheses about a spiraling increase in total explosive yield, or still less a spiral in the damage that might be done. However, by suggesting that the stocks are now far too large, it makes plausible the notion that there has been a steady exponential increase. In fact, nuclear weapons are directed at any of a large variety of military targets, and there is no simple rule for deciding

4. For an early appreciation of this point, see, for example, P.M.S. Blackett, *The Political and Military Consequences of Atomic Energy*, London: Turnstile Press, 1948.

Figure 1
Combined U. S. Strategic Offense and Defense Megatons
Years 1945-1972
Vertical index relative to 1972. 1972=1.0

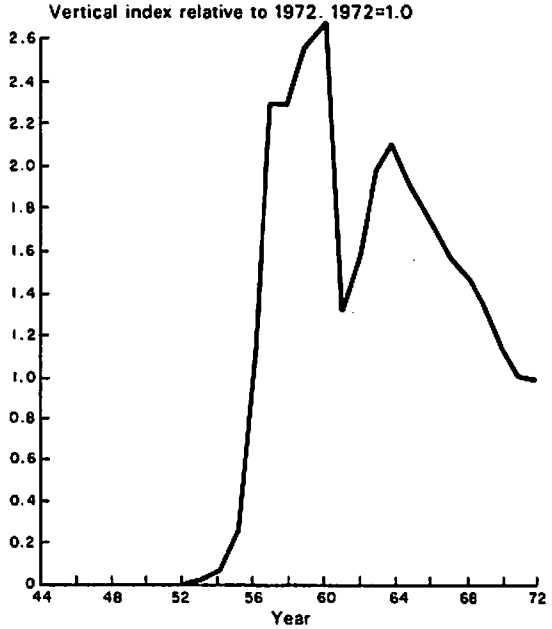
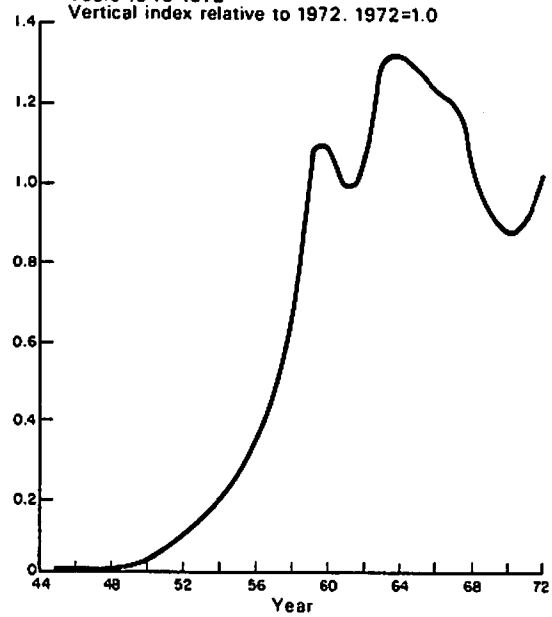


Figure 2
Combined U. S. Strategic Offense and Defense Warheads
Years 1945-1972
Vertical index relative to 1972. 1972=1.0



whether one has too many or too few. That is a problem we need not address here.[5] The question we are asking is whether on this measure there has been an exponential increase.

The answer indicated in Figure 1 is "clearly not." After an initial sharp increase, the total explosive energy yield declined from a peak two-and-a-half times the 1972 figure. And 1972 was about at the level of 1955. While this aggregate includes, appropriately for contemporary arms race theories, strategic defense as well as offense warheads, the decline is about the same for the aggregate explosive yield of the offense warheads alone.

The Number Of Strategic Warheads: At the opposite extreme from totting up the energy releasable by all strategic warheads is a measure that ignores the yield altogether and counts simply warheads. The smallest strategic defense warheads differ from the largest strategic offense warheads by many orders of magnitude, but even if we were to limit ourselves to strategic offense warheads, merely counting warheads while neglecting yield involves an heroic distortion. In fact, the largest offense nuclear warhead is roughly a thousand times the smallest offense nuclear warhead[6]—the same as the difference between the Hiroshima bomb and the largest non-nuclear blockbusters of World War II! Counting the largest and the smallest each as one—with evenhanded justice—would then be exactly like dismissing the first two nuclear weapons as of negligible importance since they increased the stocks of "blockbusters" by only a fraction of a per cent.

While there is no adequate single common measure for so heterogeneous a collection of vehicles and weapons, clearly something better

5. I address it briefly in *Pacem in Terris III, Vol. II, The Military Dimensions of Foreign Policy*, Fred Warner Neal and Mary Kersey Harvey, eds., Santa Barbara: Fund for the Republic, Inc., 1974. I favor a U.S.-Soviet reduction to equal lower totals. That is quite independent of the question as to whether the U.S. totals have increased exponentially or at all.

6. Even this fact (and not merely its implications for the incomparability of the elements in the aggregate of offense warheads) is not always recognized. It is sometimes said that U.S. strategic warheads in general are in the megaton range. See, for example: *Arms Control: Readings from Scientific American*, San Francisco: W.H. Freeman and Co., 1973, p. 179.

Figure 3
Average U. S. Strategic Offense Warhead Yield
Years 1945-1972
Vertical index relative to 1972. 1972=1.0

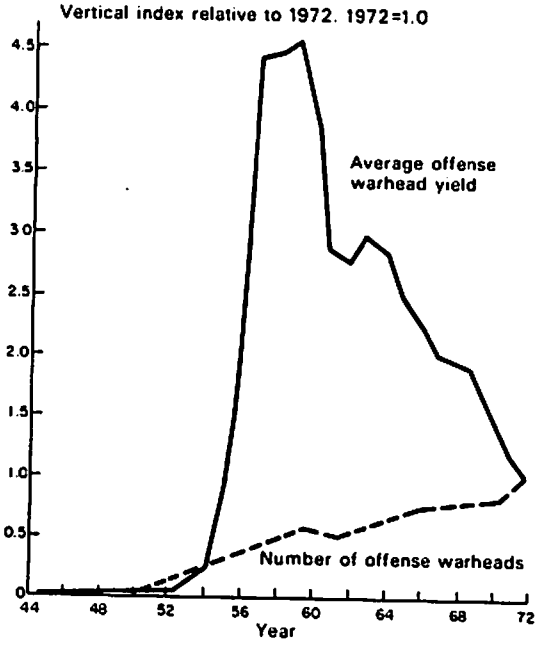
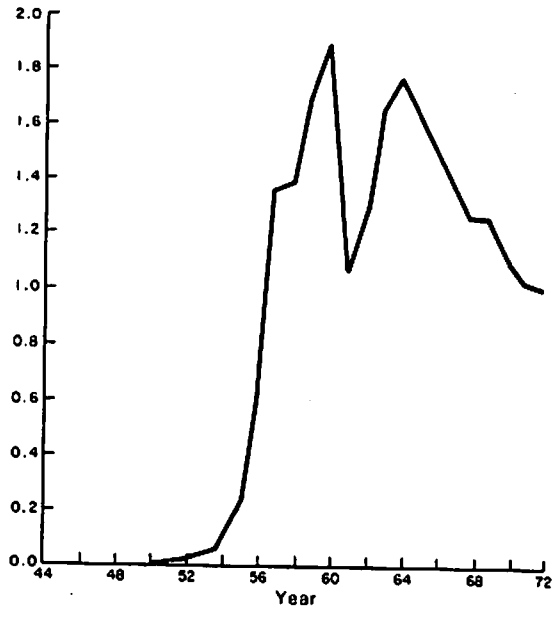


Figure 4
U. S. Strategic Offense Equivalent Megatonnage
Years 1945-1972
Vertical index relative to 1972. 1972=1.0



is possible than a simple count of warheads.[7] That the latter is used so uncritically is one of the intellectual scandals of the current debate on SALT. Nonetheless one may ask whether the number of strategic offense and defense warheads has spiraled. And as Figure 2 shows, for this disparate aggregate, the answer is that it has not. It peaked in 1964 at roughly 30 per cent higher than in 1972 which was about the 1960 level.[8]

The sense of post-Sputnik arms race doctrine with its central strictures against all weapons aimed at weapons and therefore against active defense as particularly destabilizing, plainly calls for including the Spartan, Sprint, Nike-Hercules, Falcon, and all other defense warheads in the total. However, given the opportunism of the current debate, it is hardly surprising that, when convenient, the distortion involved in counting warheads is compounded by excluding the supposedly most destabilizing —the defense warheads. In fact, one great oddity is that in spite of all the fire leveled at active defense, the debaters hardly notice that U.S. defense warheads, interceptor aircraft, surface-to-air, and air-to-air missiles have decreased drastically. The number of offense warheads has increased over time, but their average yield has decreased even more. From 1958–1960 to 1972 they increased roughly by half. But their average yield was divided by four-and-one-half (Figure 3). It is essential then to consider some measure in between counting megatons and counting warheads. We turn now to a measurement widely used for that purpose in the defense and arms control technical community.

Measures Of Relative Destructive Area ("EMT"): No single number adequately measures the destructive power of military weapons, still less other important attributes of military forces—their susceptibility to attack, their

7. One argument for simply counting warheads is the notion that the dangers of an accidental detonation increase linearly with that number. However, this is plainly false. The probability of an accidental, unauthorized detonation depends among other things on arrangements for weapons safety and for the centralization of control and command over these weapons.

8. The curves on numbers of warheads (Figure 2 and bottom of Figure 3) are smoothed in order to approximate the calculated data points, but closely enough so that deviations from the trends discussed are not significant.

Figure 5
Combined U. S. Strategic Offense and Defense Obligational Authority

Fiscal years 1956-1974
Vertical axis in billions of dollars

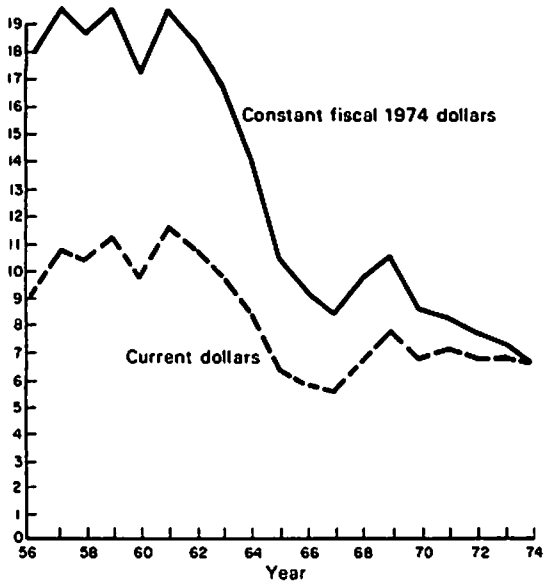
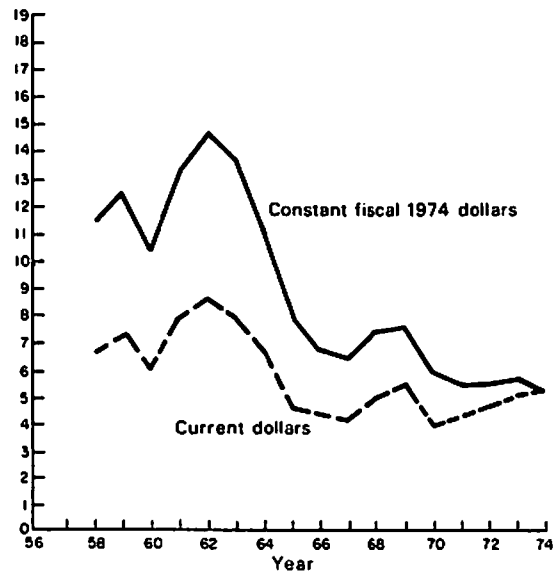


Figure 6
U. S. Strategic Offense Obligational Authority

Fiscal years 1956-1974
Vertical axis in billions of dollars



safety from "accidental" or mistaken or unauthorized use, their political controllability, their capability for discriminating between nonmilitary and military targets, and between friend and foe, their flexibility in a variety of political-military contingencies, etc. Nonetheless, as we have said, it is not hard to do better than counting warheads or counting megatons, and for comparing highly varied stocks of weapons at two different times or in two different countries, an index known (misleadingly) as "equivalent megatonnage" (EMT) has come into widespread technical use. It counts the number of weapons and their yields but makes a rough adjustment for the relative waste of explosive energy by the larger weapons through overconcentration near the target. Taking a one-megaton weapon as standard, it measures any given stock in terms of the number of such one-megaton weapons that under a variety of relevant conditions would do structural damage over an equal area.[9]

EMT, like all other indexes, has its limitations, but it captures some essentials missed in simply adding unadjusted megatons or warheads. Figure 4 shows a dramatic decrease since 1960 in the relative destructiveness, so measured, of the U.S. strategic force. At its peak it was nearly double the 1972 figure; and 1972 was roughly at the 1956 level! In any case, no spiral. This measure is relevant among other things to test the arms race argument that the uncontrolled destructiveness of U.S. strategic forces has increased. It has not. The area that might sustain structural damage has been halved and there has been a similar decline in potential fallout.

Offense And Defense Budgets

I could reinforce these results using curves on further physical measures. Instead I turn now to measures of the resources used in deploying a strategic force. Since these resources must be diverted from important alternative civilian uses, such measures are properly at the heart of the defense debate. In any case, they are central to arms race doctrines. Expenditures on strategic forces are most frequently identified as the variable that is supposed to be

9. The EMT of a weapon is computed by raising its yield, expressed in megatons, to the two-thirds power.

accelerating.

Figure 5 shows the total strategic budget as measured in the Defense Department Program I,[10] extended as far back in time—to FY 1956—as could be done using available unpublished computer runs. The top curve which corrects for inflation in military pay, materiel, retirement benefits, and the like, is the relevant one. It shows that the strategic budget in 1974 dollars declined from the very high levels of the period 1956–1961, which included three peak years well over $19 billion, to a 1974 level of $6.77 billion. In short, in real terms the strategic budget was nearly three times as high at the end of the Eisenhower Administration as in 1974! This scarcely looks like an exponential increase in strategic budgets. Rather more like an exponential decrease. For the thirteen years from 1961 to 1974 the average rate of decline was about 8 per cent per year.

How is it possible for the constantly expanding literature on ever-accelerating strategic budgets to ignore this increasing divergence between doctrine and reality?

First, exponents using the doctrine as a weapon in budget battles, handle rather carelessly the familiar distinction between real and inflated dollar costs. This can hide somewhat the drastic extent of the decline, but not the decline itself. Even in current, depreciating dollars the budget dropped from generally high levels in the 1950s and a peak of $11.5 billion in 1961 to $6.77 billion in 1974.

Second, the curves show minor local peaks and dips. Men concentrating on the immediate budget fight may easily take an ant's eye view. Looking forward from the bottom of a shallow local dip, the future looks all uphill. This opportune but myopic focus has tended to obscure the very trends that any arms race doctrine would have to confront. Such doctrines after all do not pretend to be concerned only with the brief rise, say, from 1960 to 1961. An intense focus on the current year's budget battle also leads to a related confusion: comparing the new budget request not with last year's request, but with the actual amount approved by Congress in the prior year—which can be considerably less. For example, for the defense budget as a whole, the total obligational authority approved in 1973 was $3.6 billion less, and in 1972, $4.1 billion less than the amount requested. For the FY 1974 strategic program the net difference between the requested and total obligational authority appears to be about $0.5 billion.

Third, the drastic fall in strategic budgets measured in Program I may be partially obscured by adding in a rising but quite arbitrary "overhead" figure.[11] The program budgets for strategic or for general purpose forces aim to include all the costs of equipment, materiel, and personnel that can be directly attributed to the program mission, including all support costs that "follow directly from the number of combat units."[12] Overhead allocations, whatever their accounting uses, are by definition arbitrary, and those now current have little or no causal relation to past or future reductions in the number of strategic combat vehicles. These arbitrarily allocated costs have tended to remain the same or to rise even though the strategic forces and their direct costs have been greatly reduced.

The formula that the Brookings Institution uses when dealing with past or current budgets would assign to the strategic forces an amount of overhead equal to less than half their direct costs in the late 1950s, and over one-and-a-half times their direct costs in 1974.[13] Meanwhile, direct costs of general purpose forces have varied in size from less than one-and-two-thirds to nearly five times the direct costs of the strategic forces, and the formula, year after year, splits the Intelligence and Communications budgets evenly between them. Of course, it has always been clear that some of these "overhead" costs may vary inversely with direct costs. Take Intelligence for example. Large SALT (or unilateral) reductions might call for greatly increased national means of monitoring variations in adversary forces, since marginal absolute changes make a larger proportional difference in small forces. (Dr. Wiesner in the past has suggested that inspection might have

10. Program I refers to Strategic Forces. Program II refers to General Purpose Forces. See below for what costs are included.

11. See, for example, "The Advocates," WETA-TV, Washington, D.C., February 14, 1974.

12. Martin Binkin, "Support Costs in the Defense Budget," Washington, D.C.: Brookings Institution Staff Paper, 1972, pp. 45–46.

13. The Brookings Institution uses a different method when estimating the effects on overhead of future reductions in the strategic combat forces. We are indebted to Barry Bleckman for generous help in explaining Brookings' methods.

to double if the forces were halved, and so on linearly.) But then one should expect future cuts in the direct costs of strategic forces to be partly offset by increases in Intelligence costs.

If one considers not merely what causes changes in "overhead," but also what the effects are of increases in overhead on an adversary, it is hard to see how these programs, many of which could well be classified under Human Resources or Social Welfare, would strike terror in the heart of an enemy. For example, CHAMPUS (Civilian Health and Medical Program of the Uniformed Services) includes such items as medical care for retirees, their dependents, and survivors. A drastic cut in the number of strategic combat vehicles would hardly decrease these costs and their increase should hardly seem menacing to the Soviet Union.

Nonetheless, even if these arbitrary costs are added on, they can only partially obscure the drastic decline. Using the formula Brookings applies to past budgets, the FY 1962 budget was about two-thirds higher than recent budgets. The method Brookings applies to future projected budgets is less reducible to formula and involves more subjective judgment and even larger uncertainties. If that method were applied to determine past trends, however, the decrease would be more drastic. Still other allocation methods, all necessarily arbitrary, show declines from a peak more than double the present budget. So, for example, a method used by the Department of Defense shows a decline in FY 1975 dollars of over $2 billion in the late 1950s from a peak 2.3 times as high as the FY 1975 budget including overhead. With recently improved deflators the decline would be even larger.[14] Overhead allocations have their uses, but they are limited. All of them distribute some unallocable costs. When added to program costs without any breakdown, they obscure more than they illuminate change. Nonetheless, all the allocations with which we are familiar show decided declines in strategic budgets, *not* an "upward spiral."

Fourth, in spite of the fact that arms race theorists take strategic defense along with counterforce as the villain in the piece and the principal force driving the race, they sometimes look for exponential increases in strategic budgets that cover only offense and allow for no compensating decreases in strategic defense. However, in 1962 the budget for offense taken alone was nearly three times its 1974 level.[15]

Fifth, I suspect the major reason for failure to observe the decline is that public debate usually concentrates intensely on the inital decision to buy and deploy a new system; much less on the operation and maintenance of the system once in; and hardly at all on its phasing out. In particular, the present exponents of arms race doctrines have had their gaze focused on the introduction of new systems—in line with their dominant preoccupation with innovation. As advocates they have been very much in on the beginnings, in favor of the new systems in the 1950s and generally against them in the 1960s. But the phasing out seems to escape their attention.

Systems starting from zero or near it are likely to grow very rapidly in the initial phases; they can scarcely go down. It is easy apparently to slip into the belief that there has been an "across-the-board growth of our own strategic forces."[16] However, an examination of the components of the strategic budget and an analysis of the entry into the force and the exit of various combat vehicles suggests the broad solution to the puzzle as to how this popular impressionistic doctrine can fit the facts so poorly.

U.S. strategic forces have not grown "across the board." On the contrary, as new systems were brought in, many others, including some very expensive ones, were taken out. At the end of FY 1956, for example, the strategic force included nearly 1,500 B-47 and RB-47 medium bombers, some 270 B-36 and RB-36 heavy bombers, a remnant of the B-50s and B-29s, and nearly 850 KC 97 and KC 29 tanker aircraft, all of which have since made their exit,

14. Recent improvements in deflators for Total Obligational Authority take into account the fact that a substantial fraction of the funds authorized in a given year are spent in later years.

15. Arms race theorists, faced recently with the divergence of strategic budgets from their theory of how they should behave, have suggested that the decline in the total strategic budget since it includes defensive forces merely displays the benefit of SALT I, which limited ABM. But the May 1972 agreements could hardly have affected anything before FY 1973, and the strategic defenses declined drastically many years before that. See, for example, "The Advocates," WETA-TV telecast cited above.

16. Nancy Lipton and L. S. Rodberg, "The Missile Race—The Contest with Ourselves," in *The Pentagon Watchers*, New York: Doubleday and Co., 1970, p. 301.

along with or preceded by a drastic reduction in overseas strategic operating bases and a multibillion dollar cut in overseas stocks for strategic forces. Between 1956 and the late 1960s the B-58 supersonic bomber, the Snark intercontinental cruise missile, the Atlas ICBM, and the Titan I ICBM have come and gone. So also has the Bomarc area defense missile, and most of the Nike-Hercules and fighter interceptors. In fact, air defense vehicles, promoted so vigorously in the 1950s by many who oppose them today as destabilizing, show an exponential decline from a peak of over 8,000 in 1959 to a force less than one-seventh as large in 1972; and to less than that now.

There is an amusing paradox, intelligible only in political debating terms, about the one-eyed vision displayed by exponents of arms race doctrines. On the one hand they fail to observe the increasingly obvious fact that in spite of their theory of invariable American overestimation of the size of Russian strategic forces, these forces have for many years systematically exceeded our expectation. Their one good eye in this case is focused on any momentary pause in the continuing deployment and expansion of existing strategic weapons systems. They turn a blind eye when the Russians start new systems. They see the Russians stopping, seldom starting. On the other hand, when it comes to U.S. strategic forces, they can barely preserve their belief that the American strategic budget is rising at an accelerating rate by fixing their gaze narrowly on the phasing in of new systems or their continuance and by neglecting the phasing out of the old. For the Americans, it seems, they notice the starts, not the stops.

However one explains the failure of arms race theorists to note the deviation of reality from their theory, it is quite plain that reality has diverged massively. Not only in the facts of *under*estimation that destroy a principal element of the supposed dynamics of the arms race, but also in the plain fact that the United States has not been running a quantitative strategic race.

It would be possible to present similar results for many other measures: for example, while strategic defense vehicles have declined for a decade and a half from a peak more than *seven* times their present number, offense vehicles have remained roughly the same for many years. The total of strategic vehicles therefore has gone down. The point should be very clear. There is no serious evidence of a quantitative strategic spiral.

That's quite a different point from saying that as a result of these declines, we are uniformly worse off. While I have differed with many specific development and deployment decisions, on the whole my view is that the net effect of changes over this long period, from the mid-1950s through the 1960s to the present time, has been an improvement in our force in key respects. *My view is indeed the opposite of the commonplace about the exponential arms race which has it that as we have spent more and more on our strategic forces, our security has steadily declined.* To evaluate the commonplace we need to consider the nature of the major qualitative innovations in strategic forces and their net effect.

The Net Effect Of Qualitative Change

Theories of the quantitative strategic race are an extraordinary muddle of errors and self-deceptions. Yet notions about "qualitative races" may be even worse off. In fact the Secretary of State recently expressed a longing for a "conceptual breakthrough" that would bring our understanding of qualitative races up to the present standard on the quantitative strategic race. Heaven forfend! The modesty of this desire, however, may measure the current confusion about qualitative competition.

Though discussion is far from rigorous, the kinds of changes usually thought of as "qualitative" are alterations in some relevant unit performance characteristic. The most obvious historic example is the thousand-fold increase in the average unit explosive yield accomplished by the first A-bombs. A second almost equally famous example is the introduction of the H-bomb in the 1950s which, as originally envisaged, was expected to multiply the yield of a single A-bomb again a thousand-fold. Another equally crucial case is the increase in the average speed of a strategic vehicle from about 500 to 13,000 miles per hour, made possible by the development of intercontinental rockets. Other unit performance characteristics affected by innovation have been mentioned earlier—blast resistance, concealability, accuracy, reliability, and controllability, or resistance to "accidental" or unauthorized use.

Some technical changes, it seems obvious, might worsen the position of everybody. Indeed, many now think that not rare but typical even of civilian technology, which is increasingly assigned all the hyperbolic traits recently attributed by the Secretary of State to military technology: it has "developed a momentum of its own," is "at odds with the human capacity to comprehend it," is, in brief, "out of control." Shades of Friedrich Juenger. Or Jacques Ellul who holds: "Technique itself . . . selects among the means to be employed. The human being is no longer in any sense the agent of choice," and "everything which is technique is necessarily used as soon as it is available, without distinction of good or evil. This is the principal law of our age."[17] The use of the A-bomb for Ellul only illustrates this law and is a symbol of "technical evolution" in general. Such symbols recall the cloudy determinism of Oswald Spengler's portentous "that which is a possibility is a necessity."

For environmentalists today, as for Juenger, a civilian technology out of control is the source more typically for polluting than humanizing the environment. We owe the environmental movement a debt for stressing that it is important in choosing among technologies to take into careful account the indirect, long-term, and public costs as well as the direct, immediate, and private costs of technical change. It has unfortunately also encouraged the revival of a more general Luddite view of technology as a threat to us all. The Luddite view moreover is particularly tempting when it comes to military technology. Most of us have little affection for weapons; and weapons improvements are likely to arouse a good deal less enthusiasm than technical advances in general. It is easy to believe that such "improvements" might make things worse all around.

However, just as in the civilian case one can only choose *among* technologies and it is highly unlikely that existing technologies are ideal, so also in the military case it is extremely implausible that current technologies are optimal, that they fit our political purposes beyond any possibility of improvement. We have to choose and we do. But the conditions of thoughtful choice are only obscured by the immoderate rhetoric, characteristic of Ellul, and also typical of the arms debate in the post-Sputnik era. So Lipton and Rodberg talk of the "mystique of technological progress within the defense establishment, where feasibility is equated with obligation, where if we can build it, we must."[18] A purple passage of that sort is expressive. But what is its meaning? It has no plain application to the real world in which a very long list of development projects were cancelled after much spending, but before deployment.[19] And many more development ideas were stillborn before any substantial money had been spent in their pursuit.

Moreover, it is clear that qualitative changes need not affect both sides badly. Some changes might benefit one side primarily (as radar favored the British more than the Germans in World War II). Still others might conceivably help both, since the two sides have some objectives in common. So, for example, fail-safe techniques that prevent a war from starting by mistake through a failure of communication or a false alarm, or Permissive Action Links that prevent local arming of weapons without a release from a remote responsible command center, and modes of protection that make it possible to ride out an attack and depend less on hair-trigger response. Neither side would like to see a nuclear war start by "accident" or through some unauthorized act.

The problem of judging the effect of a specific qualitative change in key performance parameters is complicated by the fact that it may be ambiguous. It may serve the interests of just one adversary in some particular respect and in another respect the interests of both. For example, improvements in reconnaissance may permit more precise location and destruction of a target, but also may reduce collateral damage and serve as a key national means of

17. *The Technological Society*, New York: Vintage Books, 1964, pp. 80, 99. Cf. Friedrich Juenger, *The Failure of Technology*, Chicago: Gateway Editions, Inc., Henry Regnery Co., 1956, pp. 163–4.

18. Op. cit., p. 302. Cf. Richard Barnet, "The National Security Bureaucracy and Military Intervention," delivered at Adlai Stevenson Institute, June 3, 1968, p. 27.

19. Nuclear propelled aircraft, started in 1951 and cancelled ten years later; the XB-70 bomber started in 1958 and cancelled in 1967; the Hard Rock Silo project, started in 1968 and cancelled in 1970; the SCAD Armed Decoys begun in 1968 and cancelled in 1973; the Navajo ramjet intercontinental missile begun in 1954, cancelled in 1957; the Rascal, the Skybolt, the mobile medium range ballistic missile, Regulus II, the Manned Orbiting Lab, and so on.

verifying that alterations in an adversary's force are no more menacing than is permitted by an arms treaty. The SALT agreements would be infeasible without precise national means of surveillance other than ground inspection. No case-by-case analysis of qualitative changes since the mid-1950s can be given. However, it is unnecessary for the purpose of evaluating the Luddite stereotype in the contemporary debate. According to that stereotype, major innovations (1) lead to new and higher levels of strategic expenditure, (2) make strategic forces more destructive, (3) make them less secure, and (4) make them harder to control politically. To test this familiar view, it is important to look broadly at the net outcome of such major technological innovations as the development of fusion weapons and strategic rocketry.

Before forming some judgment on this subject, it may provide perspective to observe that the view of innovation as generating an unstable arms race, though widespread in recent times, is by no means universal. One of the few serious studies of arms races, that by Samuel P. Huntington, held that military innovation was fundamentally benign, among other reasons because it enabled the redeployment rather than the increase of arms budgets.[20] Moreover, since it did not increase the share of national resources devoted to defense, it did not produce the strains leading to war, but in fact made war less likely.

Huntington's hypothesis about the effect of technological change, though it runs counter to the present fashion, is by no means implausible. A qualitative improvement has to do with some relevant performance characteristics of a weapon. Painting bombs blue, for example, would not generally qualify as an improvement. Increasing the explosive yield for a given weight or the accuracy of delivery would. Such changes mean that effectiveness per unit or per dollar is increased and this implies in turn that a given task might be done with fewer units or at less expense.

To meet an adverse change in a potential enemy's force, then, a government has the alternative, through qualitative change, to redeploy resources, just as Huntington asserts, rather than simply to multiply them. He also points out that a self-imposed or a treaty constraint on improving qualitative performance may impel a simple multiplication of units—that is, it may generate a quantitative race. Moreover, though it is possible that opposing governments may blindly introduce changes that worsen the position of both sides, and though it is surely true that governments make a lot of bad choices, they have plenty of incentives for looking beyond the immediate consequences of a procurement decision. And not all of their choices have been grossly wrong. It is not hard to dig up governmental analyses, good and bad, that look well beyond the next immediate step.

Conventional arms race theory presupposes a totally mechanical or instinctual behavior, that reacts only to the immediate move, never looking forward. But it is by no means clear that governments are as fatally concentrated on the immediate as arms race theorists debating the current budget. Both the U.S. and the Russians introduced (in good part independently) the revolutionary technologies of rocketry and fusion weapons. But we made adaptations in our force that exploited these technologies precisely to avoid the kind of deterioration the dogma suggests is automatic.

The main methods worked out in the early 1950s for protecting the strategic force based in the United States for the rest of the decade depended on tactical warning and a rapid, safely repeatable response by our force that did not commit it to war on the basis of substantially uncertain warning. These methods could work reasonably well, so long as the speed of attacking vehicles was that typical of manned aircraft. But it soon became clear that strategic rockets were likely to be a feasible operational component of strategic forces in the 1960s.

Rockets, because of their speed, might, in current jargon, have been described as "intrinsically destabilizing." However, no single performance characteristic taken in isolation, whether speed or accuracy or whatever, can be so established. If one had believed that speed was intrinsically destabilizing, one might conceivably have tried to get an agreement banning rockets altogether; or tried to increase their travel time by getting agreements to use extreme lofted trajectories; or—still more farfetched—an agreement to orbit them several

20. Samuel P. Huntington, "Arms Races: Prerequisites and Results," *Public Policy*, Vol. 8, Carl J. Friedrich and Seymour E. Harris, eds., Cambridge: Harvard University Press, 1958.

times before landing; or (as in the 1958 Surprise Attack Conference) to construct an elaborate international warning system shared with adversaries in order to preserve the possibility of timely, secure response. Instead of trying simply to stop or slow down technology, the tack taken to maintain an improved second strike capability was to make unilateral adaptations that exploited both the initial limitations of the new rockets, specifically their great inaccuracy, and also their substantial advantages for defense penetration and for developing new, cheaper, and better modes of protection against attack, including mobility. Useful adaptations of the new techniques were feasible, even though our understanding of them was only partial and uncertain. Our adjustments to them did not have to be made all at once. They were made incrementally as various pitfalls and opportunities presented by these techniques became plainer.

In short, in spite of the recent as well as the age-old romantic antagonism to technology and the belief expressed by such critics of technology as Jacques Ellul, we are not slaves to technique. We can and do make technical choices, and in doing so sometimes improve matters. The alternative is an indiscriminate hostility to innovation per se, but that rests on the implicit assumption that the point at which we have arrived cannot possibly be improved—a rather odd view for the critics of technology to hold, who otherwise stress the arbitrary and irrational process by which past decisions on development have been made. In effect, an antagonism to all innovation amounts to a sentimental attachment to older technology rather than a hostility to technique in general.

A study of the major changes in technologies from the 1950s to the present and their effects on the strategic force supports the view that whatever the false starts and mistakes in detail, on the whole the outcome was exactly the reverse of the stereotype in the four respects listed on page 45.

Much of this is implicit in the analysis of quantitative changes already offered. So I can be brief. First, strategic spending did not rise to new levels. From the late-1950s it fell almost by two-thirds. Second, the relative destructiveness of our strategic forces as measured by EMT declined. Moreover, in precise contradiction to the standard view, this decline responded in good part to the increased size and effectiveness of actual and anticipated Soviet active defenses. On the whole, the shifts in the American force from gravity bombs to air-to-surface missiles carried on strategic aircraft and to ICBMs and SLBMs themselves were in the first instance basically a response to the formidable growth of Russian air defenses. But these as well as later developments meant a drastic reduction in total and average explosive yield and in EMT. Third, through such devices as placing rockets on submarines moving continuously underwater or in highly blast-resistant complex silos, the strategic forces became less vulnerable than they had been in the 1950s—with a resultant increase in stability. In the mid-1950s our strategic forces were concentrated at a few points, were soft, slow to respond, inadequately warned, and inadequately protected by active defense.[21] The Soviet forces were even more vulnerable, and remained so much longer, but greatly improved in this respect in the mid-1960s. Fourth, the controllability of the force was improved by the very methods of protection adopted, which made hair-trigger response unnecessary; also by a variety of fail-safe devices and arrangements permitting positive control, and by improving the protection of the command and control arrangements themselves.

Finally, many of the measures that so improved the strategic force were adopted self-consciously as alternatives to simply multiplying the force and increasing budgets. They did not undertake the hopeless task of stopping qualitative change. Rather, they adapted qualitative change roughly to our purposes, not all of which are incompatible with those of potential adversaries.

Is There A Strategic Arms Race?

The post-Sputnik doctrine of the strategic race is clearly mistaken in all its principal tenets: the dynamics of overestimation (as outlined in Part One of this essay), the supposed accelerating increase in strategic spending and force levels, the steady rise in indiscriminate de-

21. For a contemporary analysis of the vulnerability of strategic forces in 1956, see, for example, Wohlstetter, Hoffman, Rowen, *Protecting U.S. Power to Strike Back in the 1950's and 1960's*, RAND, R-290, September 1956, pp. 30, 41. For earlier analyses by the same authors see *The Selection of Strategic Bases*, R-244S, April 1953 and *The Selection and Use of Strategic Air Base Systems*, R-266, March 1954.

structiveness, the decreased security of the force and the increased likelihood of war, the supposed movement of technology beyond the means of political control. In the sense that the doctrine claims, the United States plainly has not been racing.

But isn't there in *some* sense a "strategic" race? Obviously, depending on the sense. As Humpty Dumpty said, if you pay a word enough, it can mean anything you want it to mean. There is surely a military competition between the Soviet Union and the United States in the strategic field. And it is one related to the partially, but sometimes intensely opposed aims of the two governments in many parts of the world. Strategic forces are the ultimate back-up for alliance commitments.

However, that Soviet-American competition has been quite compatible with a rather steady rise in Soviet strategic spending—roughly in proportion to the growth in their GNP—during a very extended period when U.S. budgets rose, reached a plateau by the mid-1950s, and then declined by a factor of nearly three. A "race" in the ordinary sense involves a fast advance by the contestants. It is possible by ironic extension to talk of a turtle race. Or a race between a tortoise and a hare. And even a race in which both participants run backwards. But it is surely stretching it to talk of a "race" between parties moving in quite different directions. A competition perhaps of some complex and subtle sort, but hardly a race.

The trouble with most arms race theories has been that they start by assuming an accelerating competition and then look about for some mechanism that might conceivably explain it—a simple pair of differential equations with an exponential solution (as in Richardson), worst case dynamics, explosive interservice rivalries, etc. It would be better to start, however, with the actual gross behavior of the parties in the competition. Then a good many factors, each of which has enjoyed exclusive favor in various models, may be found indeed to have a limited role (but frequently a role quite opposite to that usually attributed—as in the case of technological change, which at key times may substitute for quantitative increase).

The gross shape of the U.S. curve of strategic spending, if extended back to 1945, would show a sharp drop after World War II, a surprisingly low level during the late 1940s when "atomic diplomacy" was supposed to have been in full sway, a rapid rise after Korea to a high plateau in the mid- and late-1950s, then another sharp decline beginning at the start of the 1960s. These gross changes in American, and the simultaneous quite different changes in Soviet strategic spending cannot be understood in terms of a closed cycle of tightly coupled interaction between U.S. and Soviet processes of decision to acquire weapons—as is assumed in the usual action-reaction theory. Still less can it be explained in terms of a closed cycle of competition among the services, though bureaucratic factors as well as opposing weapons deployments play a role.

The gross changes in American strategic forces have plainly been affected by political events outside the weapons acquisition process. For example, in the 1940s, the slow cumulative change starting well before the end of World War II in American perceptions, right or wrong, as to Soviet willingness to use implicit or explicit threats of force to encroach on the independence of neighbors; a growing recognition that the Soviets were not very interested in international ownership and control of all "dangerous" atomic energy activities, and so on. And in the 1950s, the gradual recognition, on the basis of actual experience, of the rigorous limitations of strategic (or any other) nuclear weapons as a substitute for classical military force (which changed the relative priorities of general purpose and strategic forces), the cumulative recognition of the limits of strategic defense, given the near term prospective state of the art, and improved technologies and better understanding of the requirements for protecting strategic offense forces. I believe the listed cumulative changes in the late 1940s are some of the things that brought about the reversal of direction after Korea and a sharp increase; and the listed changes in the 1950s are some of those that led to the decline in strategic spending in the 1960s. All that is another story—longer and more complex. However, the current doctrines of an accelerating arms race have little relevance for illuminating this complex competition, and in their apocalyptic and millennial character they hinder rather than help thoughtful national choice or agreement with adversaries.

Finally, some technologies reduce the range of political choice; some increase it. If our

concern about technology getting beyond political control is genuine rather than rhetorical, then we should actively encourage the development of techniques that increase the possibilities of political control. There will be a continuing need for the exercise of thought to make strategic forces secure and discriminatingly responsive to our aims, and to do this as economically as we can. Agreements with adversaries can play a useful role, but they cannot replace national choice. And neither the agreements nor the national choices are aided by the sort of hysteria implicit in theories of a strategic race always on the point of exploding.

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