Racing Forward? Or Ambling Back? (1976)

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Not long ago the *Bulletin of the Atomic Scientists*, which since 1945 has kept time on the arms race, moved its famous clock ominously closer to midnight. The familiar reasoning is that American and Soviet negotiators at Geneva have failed to reach agreement on limiting strategic arms and so the race continues. The United States has forced the pace by overestimating the Soviet threat, and then, to play safe, spends more resources than are needed to meet even a menace so inflated. In this way we have given the U.S.S.R. no alternative than to react by spending in its own self-defense—which, in turn, we meet by still more "worst case" analyses, increased spending, and so on and on in the deadly "action-reaction cycle." The superpowers are engaged in a mortal contest, each provoking the other into piling up arms endlessly, wasting scarce resources, increasing the indiscriminate destructiveness of weapons, lessening rather than adding to their security, and moving the world closer to nuclear holocaust.

Secretary of State Kissinger has recently adopted one variant of this reasoning that puts the blame on technology. He has said that military technology has developed a momentum of its own, is at odds with the human capacity to comprehend it, is simply out of control, or is in imminent danger of getting beyond political control. Thus we must restrain not only the number of arms but their qualitative improvement. For it seems that the very effort to design new and better techniques to protect ourselves against adversaries makes things worse for both sides and mankind.

All this is familiar, but is it true? Is it true, for example, that we chronically overestimate what the Russians will deploy and that this is the source of an "action-reaction" chain, driving the Russians and ultimately ourselves to disaster? Whatever is the case for the Soviet strategic budgets and forces, has the United States in any clear sense been racing at all? Is it true, as is claimed, that U.S. technical innovation, in particular, has spurred us to

higher and higher levels of strategic spending, destructiveness, and instability?

In fact, none of this is true. Starting in the early 1960s, we systematically *under*estimated how much and how rapidly the Soviets would increase their strategic offense forces. Moreover, for an even longer time, our own spending on strategic forces has been "spiraling" down rather than up. U.S. strategic program budgets ("Program I" as it is called) in real terms fell from a plateau at the end of the 1950s that was three and a half times the present size. In fact, the peak in strategic spending occurred in fiscal year 1952 when the budget was about 4.25 times the fiscal 1976 level (in 1976 U.S. dollars the strategic program budget in FY52 was 32.6 billion compared to 7.7 billion in FY76). Finally, the net effect of major innovations in our strategic force since the 1950s was to reduce not only its cost but also its indiscriminate destructiveness, and its instability or vulnerability to attack. These actualities seem to contrast so sharply with the standard saying about Soviet-American competition that we need:

> First, to recall and document what the stereotypes about the strategic arms race have been;
>
> Second, to contrast the standard view that we chronically overestimate Soviet offense deployments with the facts about what Soviet offense forces we predicted in the 1960s and how these predictions turned out;
>
> Third, to contrast the theory that our strategic spending has been going up with the actual declining costs;
>
> Fourth, to consider briefly the concrete effects of qualitative improvements on U.S. strategic forces and budgets.
>
> Finally, to ask how we could have been repeating obvious untruths for so long without embarrassment. Answers to this last question must necessarily be speculative. I'll suggest some as I go along.

I

The Standard View of the "Arms Race"

Contemporary stereotypes about the strategic arms race resemble the arms-race doctrines of Lord Grey, Bertrand Russell, Lewis Fry Richardson, and other doctrines that flourished in England between two world wars and can be traced back at least to Cobden in the mid-nineteenth century. These doctrines suggested that each side in an arms race sees as a threat an increase in arms by the other side that is intended merely for defense. Lord Grey, who had been Foreign Minister when the Great War broke out, wrote:

> The increase of armaments, that is intended in each nation to produce consciousness of strength, and a sense of security, does not produce these effects. On the contrary, it produces a consciousness of the strength of other nations and a sense of fear.... The enormous growth of armaments in Europe, the sense of insecurity and fear caused by them—it was these that made war inevitable.[2]

The Quaker physicist, Richardson, put such views into differential equations relating the rate of increase in defense budgets, on one side, to the level of spending on the other with a resulting exponential increase of budgets for both.

The doctrines of the strategic "race" that have prevailed for more than 15 years add a few new twists to the old theory. First, they talk not simply of an exaggerated fear about the intent of an opponent in amassing armaments, but about exaggerated estimates of the size of these armaments and about plans to meet the opposing side's increase which would be overcautious (assuming the "worst case") even if the estimates of the range of possibilities were correct. Second, the British theorists between the wars adopted a certain Olympian even-handedness in describing the reciprocal fears generating the race. (Richardson talks of the mistaken fear of the "Minister of *Jedesland* [every country].") But current American doctrines, like revisionist history, frequently place on America the main responsibility for the rate and scale of the arms race. Third, the current doctrines stress the instabilities brought about by technology. And fourth, they locate the source of the race especially in efforts to defend civilians and destroy

offensive military forces, and see the force driving the quantitative spiral to be not merely qualitative military change, but, in particular, improved technologies for destroying not people but weapons, whether in place or already on their way to target. This perverse doctrine, widely prevalent among theorists of the arms race since Sputnik, has been summarized by a sympathizer to the view in the "frosty apothegm": "Killing people is good; killing weapons is bad."[3]

Arms race dogma about "runaway technology," "exaggerated threats," "worst case analysis," "explosive increases," "uncapped volcanoes," "action-reaction," "treadmill to nowhere," etc., so pervades the statements on SALT and strategic interaction by Cabinet members, Congress and its staff, public interest lobbies, the academics, and the news media, that selecting a few out of a mass of citations may seem redundant; it risks bruising individual sensibilities.

But as Leon Festinger, a student of apocalyptic prophecies, reminds us, prophets and their disciples often deny they meant what they said, or even that they said it. So also, the apocalyptic prophets of the race to nuclear oblivion, when confronted with an empirical test and refutation of their beliefs: they have responded by denying that they or anyone else hold the dogma.[4] Here then is a sample of views documenting the points challenged.

Take the exaggerated threat "worst case" dynamic. In its more moderate form, this dogma holds that our planners have a systematic bias towards exaggerating—expecting our adversary to do more than he does—and that they compound this error by designing our force to meet a force greater than we expect—a "worst plausible case." It is this minimal form I show to be in error, not only the more obviously wrong extreme that talks of "invariable overestimation" or "worst possible case."

Morton Halperin and Jeremy Stone, as if arguments can be directed only at the extreme, say the notion that "arms race analysts believe in a myth of invariable U.S. overestimation" is a "straw man." It is "obviously unlikely," they say, that "analysts believe anything is invariable." They want quotations.

For the extreme, one can introduce the flesh and blood Jeremy Stone to the straw Jeremy Stone, who has written:

> The department invariably exaggerates the Soviet threat to obtain public and congressional support for weapons that will undermine the Soviet deterrent.[5]

And less or equally extreme:

> **Jerome Wiesner**—We always underestimate our own capabilities and overestimate those of the other fellow.[6]
>
> **Leonard Rodberg**—Even though the Soviets invariably lag far behind these predictions, our own programs go forward as if the forecasts were accurate....[7]
>
> **Herbert Scoville**—We should not again fall into the trap of perennial, compulsive reaction to timeworn exaggerated threats.[8]
>
> **Leslie Gelb**—The common practice, as I think we all know, has been to exaggerate and overdramatize.[9]
>
> **Robert McNamara**—...a strategic planner must be "conservative" in his calculations; that is, he must prepare for the worst plausible case.[10]
>
> **Stanley Hoffmann**—The whole history of the postwar arms race is one of... preemptive escalation based on a worst case hypothesis which assumes the adversary's capacity and will to go ahead full speed.[11]
>
> **Paul Warnke**—... in determining relative strategic balance, the other side, just as we do, must use worst case analysis.... They are not going to overestimate their potential and underestimate ours. If any, the error will be in the other direction.[12]

Such a belief is distinct from, but frequently associated with, a view that the United States is the catalyst for the race. Halperin and Stone observe sagely that the two views are distinct, but seem to doubt the currency of the second view as well. We might begin the list once more with a characteristically temperate quote from Stone:

> The Department of Defense has become an inventor and a merchandiser of exaggerated fears ... an unscrupulous lobbyist to get the weapons to answer these fears. Worst

of all, through the action-and-reaction phenomenon, its aggressive pursuit of the arms race has greatly undermined the security of the nation by unnecessarily stimulating Soviet efforts to keep up.[13]

Edgar Bottome—It is my contention that with minor exceptions, the United States had led in the development of military technology and weapons production throughout the Cold War…. The Soviet Union has been placed in a position where all it could do was react to American initiatives in bomber or missile building programs. This American superiority, along with the highly ambitious nature of American foreign policy, has placed the United States in a position of being fundamentally responsible for every major escalation of the arms race.[14]

William Epstein—American scientists seem to have the edge in technology and to lead the way in developing new weapons, particularly in the nuclear field, but Soviet scientists follow close behind in the action-reaction chain.[15]

Bernard Feld—History guarantees that new American technology will certainly be followed ... by Soviet emulation.[16]

Marshall Shulman—This commitment ... has led us to force the pace of the strategic arms race, and it inescapably leads to an uncontrolled military competition with the Soviet Union.[17]

John Newhouse—America's forces apparently served as both model and catalyst for the Russians…. Such is the action-reaction cycle as perceived by many scientists and bureaucrats.[18]

Newhouse adds that other scientists argue, "It is the impulse of technology, not an action-reaction cycle, that drives the arms race...." Most scientists in my collection see the impulse coming from us *and* technology. So, to quote Rodberg, "...we have used our own superior technology to drive the arms race forward."[19] But the malign role of technology is particularly important in the

dogma and deserves illustration. "Is Jerome Wiesner," Michael Nacht has demanded, choosing an evidently far-fetched case, "a modern-day Luddite?" Consider the following from a committee Wiesner headed:

> It is, after all the *continuing competition to perfect and deploy new armaments* that absorbs quantities of time, energy, and resources that no static environment would demand; that exacerbates U.S. and Soviet relations with unreal considerations of strategic advantage or disadvantage; that keeps political leaders in both great powers off-balance and ill-prepared for far-reaching agreements; that fixes the attention of both sides on the most threatening aspects of the opposing posture; and, especially, that provides heightened risks of a violent *spasm of procurement—one spurring to new levels the cost, distrust, and the explosive dangers of an unending competition in arms* (italics added).[20]

The explosive dangers feared, Wiesner makes clear elsewhere, involve "an ever-increasing likelihood of war so disastrous that civilization, if not man himself, will be eradicated."[21] Anyone who holds that military innovation has a *net* bad effect (my definition of a Luddite in the military field)—let alone the effect of ultimate catastrophe—should want to impose general restraints on it. So, to quote Herbert York:

> The recent small successes in controlling the quantitative side of the arms race also call for renewed efforts to control its qualitative side, to slow down the rate of weapons innovation, and hence to reduce the frequency of introduction of ever more complex and threatening weapons.[22]

Examples could be multiplied. But we need not leave Cambridge, Massachusetts. Consider George Kistiakowsky and George Rathjens:

> ... any understanding that slowed the rate of development and change of strategic systems would have an effect in the right direction.[23]

And take Harvey Brooks, who argues that "the most promising lines of action for controlling the qualitative arms race probably lie in mutually agreed limitations on testing," but also suggests agreements to forgo specific improvements and general declarations against destabilizing developments, even if both would be hard to interpret or verify—particularly "in closed societies."[24] Even unverifiable agreements would provide arguments in internal bureaucratic debate to those who oppose such developments—at least in open societies. Or take Paul Doty:

> ... even better would be the adoption of a generalized set of restraints that would slow the whole development and deployment process.[25]

These would have an effect in the right direction, if qualitative change has a net bias toward making strategic forces more costly, more indiscriminately destructive, more vulnerable, and harder to control. But if not, you wouldn't slow things down generally. Nor try merely to stop "unfavorable" developments (always a good idea). You would encourage the development with all deliberate speed of technologies that reduce costs, increase discrimination, and make forces less vulnerable and easier to control.

I will present evidence that, whatever the false starts and mistakes in detail, the net effects of our major technological choices from the 1950s to the present were exactly the reverse of the Luddite stereotype. Generalized restraints would have been a bad idea.

II

U.S. Predictions and Soviet Realities

Systematic or even invariable overestimation need not lead to an arms spiral. 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 Secretary 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. The logic that overestimating an adversary drives one to race him is not compelling. 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.

The "missile gap," as is well known, was a U.S. overestimate after Sputnik of the number of intercontinental ballistic missile (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 to respond to anticipated larger threats rather than to what the Soviet leaders 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 (IRBM/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 IRBM 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., rather than from any systematic numerical comparison with reality.[26] Figures 1 to 3 sum up[27] 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 program. 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 lead-times 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.[28]

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.[29] 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 11 times. The prediction made in 1965 (Figure 1d) is 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 51 predictions, the low end of the range *never* exceeded the actual; the mean between the high and low exceeded it only twice in 51 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.

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.

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.

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 percent below the actual number for ICBMs and 2 percent 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.

Figure 1a
ICBM Prediction Made in 1962

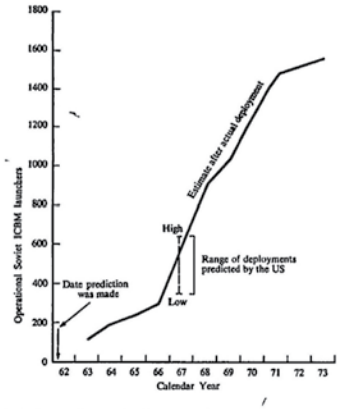
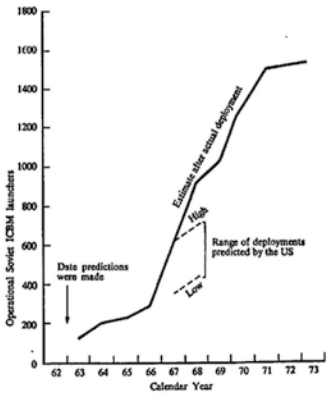


Figure 1b
ICBM Predictions Made in 1963



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

Figure 1c
ICBM Predictions Made in 1964

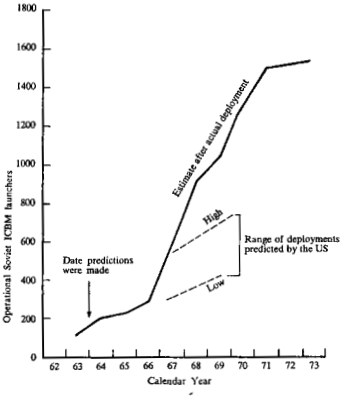


Figure 1d
ICBM Predictions Made in 1965

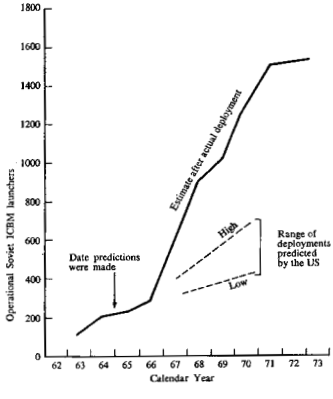


Figure 1e
ICBM Prediction Made in 1966

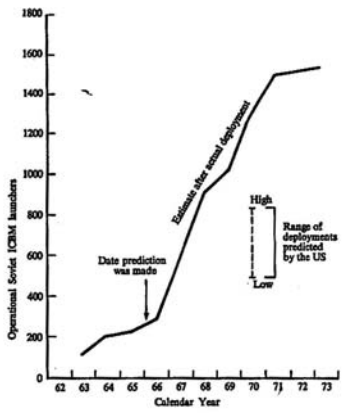


Figure 1f
ICBM Prediction Made in 1967

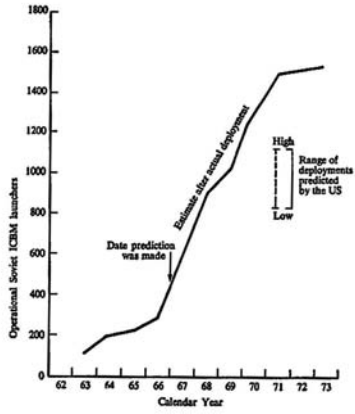


Figure 1g
ICBM Prediction Made in 1968

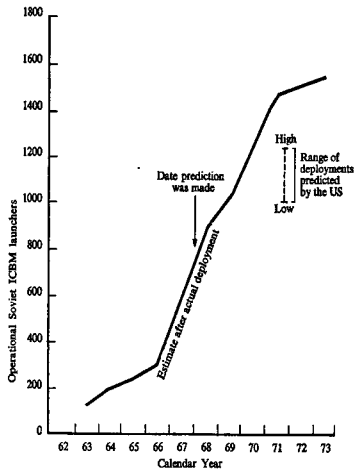
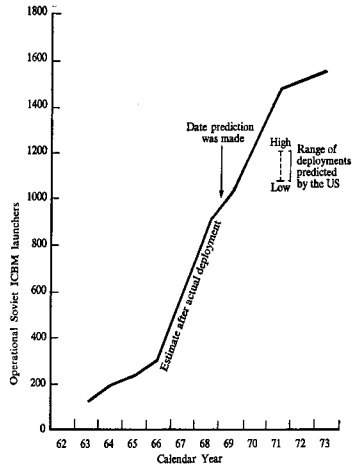
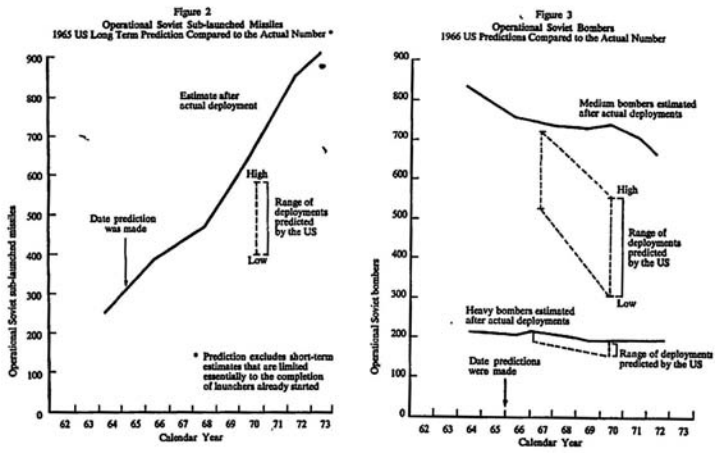


Figure 1h
ICBM Prediction Made in 1969





Burying Wrong Predictions in the Known Past

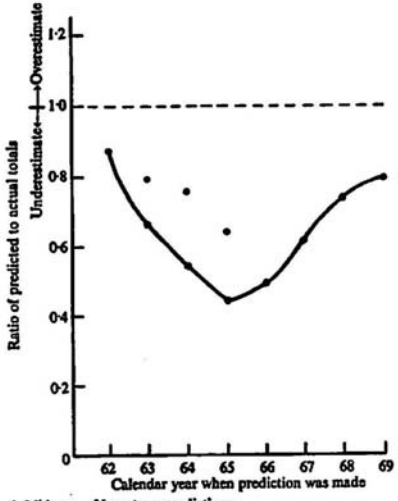
Our longer-term predictions about the Soviet strategic triad were under the mark for 11 years. The long-term ICBM projections presented in Figures 1a-1h were made during the eight years from 1962 to 1969. (Later ones referred to dates well after SALT I numerical limits on missiles took effect.) Did these eight years of long-range ICBM predictions show systematic learning?

It would not be surprising if they did, or even if, after eight years of trying, ICBM predictions finally touched reality. Programs do, in the end, level off; and the forecaster who year after year predicts they will, sooner or later, like a stopped watch, will be right. What is surprising is that these forecasts got worse, not better.

Some analysts now grant that we underestimated, but claim that we improved with time.[30] They ignore the important difference between predicting a *cumulative* total of vehicles that will have been deployed at some future time, most of which are known to be already completed or in process at the time when the prediction is made, and predicting a *change* from this known state. This accurately-known past makes up an increasing portion of the cumulative total. Nonetheless, those who detect an improvement in forecasts compare predicted with actual totals, not predicted with actual change from what was known; and so swamp unpredicted new starts in the steadily increasing total of launchers known to be started or completed.

Suppose every year a forecaster regularly predicted that during the next 12 months an adversary was going to add 10 more missile launchers; and every year, without fail, the adversary added 100. At the end of 10 years, the adversary would have built up a force of 1,000 launchers. But in the beginning of the tenth year, with 900 in place, the forecaster, undaunted, might predict, once more, that in the *next* period the adversary would build only 10 more, so reaching a cumulative total of 910. If one used Nacht's ratio of the predicted-to-actual cumulative number deployed, it would appear that the forecaster's skill in prediction was steadily improving. In the first year the predicted-to-actual ratio was 10/100, in the second year 110/200—and so on until the great success of the tenth year, when the predicted-to-actual ratio would be 910/1,000. A success ratio of 0.91 seems a marvelous improvement over 0.10. However, year after year he would have been undershooting reality in the same way. The *difference* between the predicted and actual cumulative numbers would have been the same—namely 90—and the *ratio* of predicted-to-actual *increments* would have continued to be one-tenth. The forecaster would have learned nothing about how better to anticipate the future. The cumulative ratios, as in Figure 4a, miss this essential point.

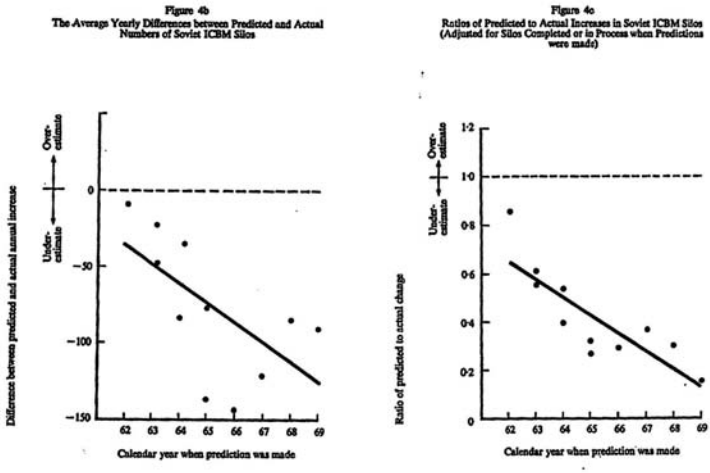
Figure 4a
Ratios of Predicted to Actual Cumulative Totals of Soviet ICBM Silos (Burying the Future in the Known Past) *



* Midrange of long term predictions

Moving from hypothetical to actual history, if we exercise a little care, it is easy to see that our long-run predictions of net future change were getting no better, that if anything they were worsening. The most direct way to establish that fact is suggested by our hypothetical example, where the difference between prediction and reality remains constant while the cumulative ratios suggest an apparent improvement.

Figure 4a[31] presents a scatter diagram that buries errors about the future in statements that are mostly about the known past. It shows ratios of predicted-to-actual cumulative totals of finished silos. The Secretaries made these long-term predictions during the eight-year period 1962 to 1969. All refer to dates no later than mid-1972. Each dot represents one such cumulative ratio calculated at the mid-range of each prediction. In each of three of the years, the Secretaries made two long-range predictions. I have connected the subset of eight dots that maximize the impression that the worsening was reversed.[32]



In the more appropriate Figure 4b each dot represents the mean amount per year by which the mid-range between high and low of a long-term prediction missed reality. All of the dots throughout the entire period are below zero. All undershoot reality. The average difference between predicted and actual silos was -80.1. Second, the dots drift downward quite steeply; that is, the underestimates tended to get much worse year by year. A trend line fitted in the standard way to the points representing

underestimates slopes downward at the rate of -12.59 silos per year. For the period as a whole the evidence indicates not "learning," but "unlearning." During the later subperiod starting in 1965 (the year some analysts think of as the worst), tests do not show improvement: there is no statistically significant trend towards reducing the differences between predicted and actual. A variety of statistical tests indicates worsening.[33] Moreover Figure 4b still neglects knowledge of launchers in process. On the whole, then, the evidence provided by a study of differences between predicted and actual numbers of silos suggests both underestimation and *increasing* underestimation.

That evidence can be greatly reinforced by a closer look at ratios, provided however that one looks at ratios of predicted-to-actual *changes* from the accurately known past. At the time when predictions were being made, the forecaster had hard data not only on (a) silos completed at that time, but also on (b) those that were in process of construction. Figure 4c presents ratios adjusted both for silos completed and for those in process of construction. Since the predicted numbers were less than the actual numbers, the ratios are all less than one; all are underestimates. The predictions averaged roughly a third of the actual number. The median ratio is .34. The ratios drift downward with time, worsening at a rate of about eight percentage points a year.

In sum, the long-term U.S. projections of Soviet ICBM silos were not only underestimates, but also deteriorating underestimates. The phenomenon cries out for explanation.

The distinction between predicting cumulative totals and predicting changes in these totals may explain not only recent errors in analyzing history; it may also be part of the explanation for the slowness of the forecasters themselves to recognize a drift away from reality while it was happening. For even though the use of cumulative totals of finished launchers (and especially of ratios of predicted-to-actual totals) has its hazards in an analysis of the success of predictions, such totals have an obvious current operational importance for those who are charged with planning for the contingency of combat. Adversaries must fight with the stocks they have ready at the time a war breaks out. "Orders of battle" are given in terms of such total stocks. For many current purposes, therefore, it is entirely natural to formulate predictions in such terms.

Nonetheless, when predictions are formulated mainly in this way—as they are—systematic forecasting errors will tend

to be buried in the larger totals, and corrections are likely to be discovered later than if forecasts were made in terms of the changes expected during the prediction interval. Someone planning to buy additional forces or to phase some out, should focus on long-term *changes* in adversary forces. Failure to center on change is only part of the explanation. Much remains to be explained. But underestimation of bomber and missile deployments for a very long time plainly persisted. That is the main point

So far I have focused on the important set of predictions cited by the Secretaries of Defense. While these plainly played a key role in the planning and budgeting process, one might well ask whether they were typical of the intelligence community. Those reluctant to give up the myth of chronic overestimation in particular ask this question, and have in mind the official consensus and, even more, the widely reported excesses of the Air Force. In fact it is familiar that during the "missile gap" Army and Navy estimates were under, and the Air Force over, the consensus. To judge how widespread underestimation became during the 1960s, it is worth comparing Air Force long-range ICBM predictions with the official consensus starting in the autumn of 1961, and comparing both with the Soviet realities counted in post-deployment estimates.

The Air Force, the Consensus, and Reality

In the first two years (Figures 5a and 5b), the Air Force did indeed exceed both the consensus and the reality. In autumn 1962 the mid-range of the consensus was below the 1967 reality and the "high" barely reached it. In autumn 1963, the Air Force predictions still greatly exceeded the consensus, but the two began to converge. There was some overlap between them in the early years referred to in the prediction, and in the more distant years, when the Air Force outbid the consensus, even its high dropped below reality. In autumn 1964 the Air Force and official predictions came close together and overlapped for the first time in predictions about the more distant years. For these more distant years, even the Air Force highs were below reality, though the Air Force still exceeded the consensus. In autumn 1965 and 1966 (Figures 5e and 5f) underestimation worsened with further convergence. Finally, in autumn 1967, convergence was total. The Air Force endorsed the consensus on condition that the Soviets would deploy MRVs (Multiple Re-entry Vehicles—unlike MIRVs, *not* aimed independently), which they did. The highs of the long-

term forecasts in these last years till mid-1967 were invariably under reality, and both the consensus and the Air Force assumed an ultimate leveling off of the Russian program well below what happened. In autumn 1968 the Air Force concurred with the consensus on the assumption, now clearly conservative, that MIRVs would be deployed by mid-1978.

Figure 5a
Air Force and Official Consensus Predictions Made in Autumn 1961

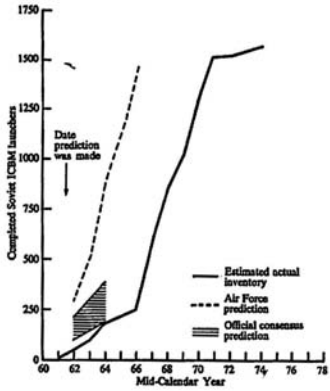


Figure 5b
Air Force and Official Consensus Predictions Made in Autumn 1962

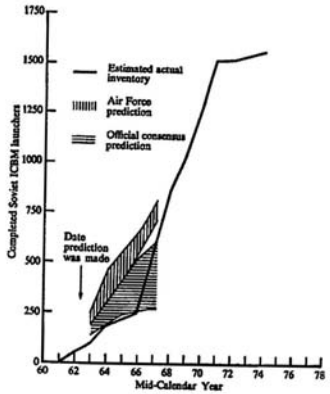


Figure 5c
Air Force and Official Consensus Predictions Made in Autumn 1963

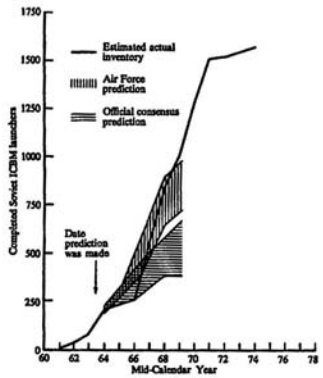
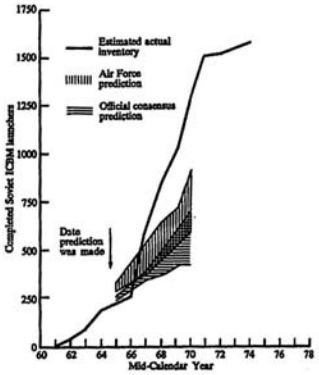
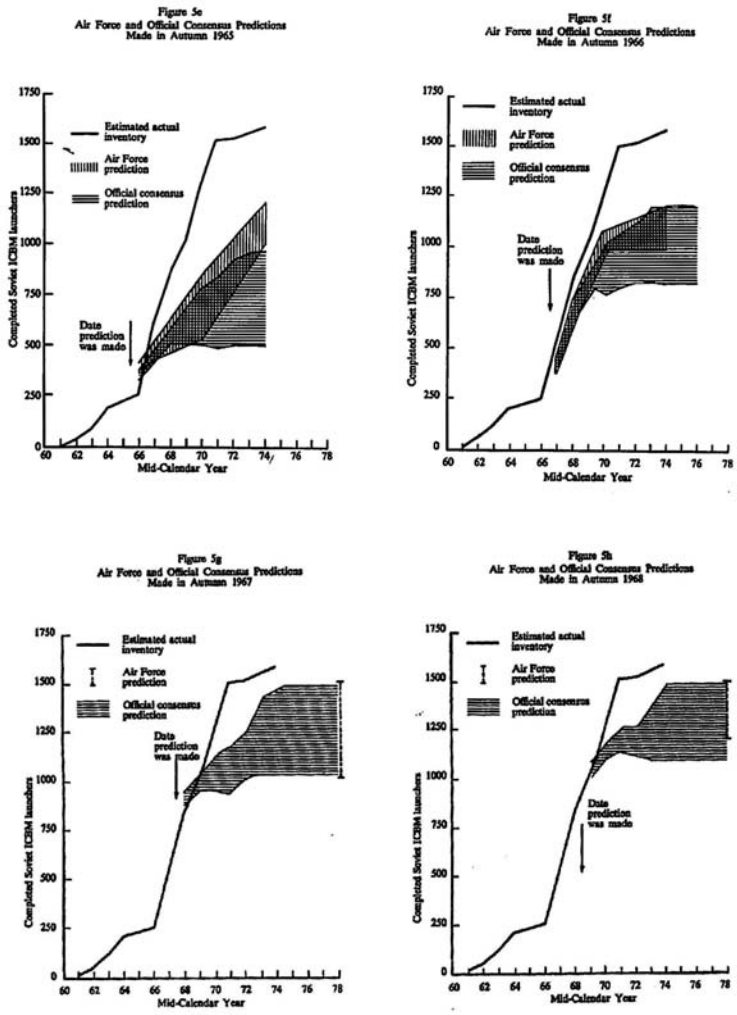


Figure 5d
Air Force and Official Consensus Predictions Made in Autumn 1964





The steady movement towards the official forecasts suggests the power of consensus. That power is particularly impressive since final convergence occurred in autumn 1967, which (as McNamara observed the following January) marked a 380-silo jump from autumn 1966. Deviation from the consensus on the high side went out of style just as it became objectively most plausible.

Why?

Pressures for conformity in the 1960s tended to operate against overestimating offense deployment. Overestimating rather than error had become disreputable. For example, the Secretary, in January 1964, stressed that "these longer-range projections of enemy capabilities must necessarily be highly uncertain," but, "indeed the record shows that in the last several years we have consistently *over*estimated Soviet ICBM strength" (italics added). He then cited three forecasts made in 1959, 1960, and 1961, during the "missile gap," about Soviet ICBMs expected in mid-1963. All three, of course, were far above the mark. He warned, "These facts should be borne in mind as we discuss the estimates for the 1967-69 period." But the 1964 estimate about 1967, to which he attached this caveat, turned out to be not above but way below the mark—120 silos below at mid-range. Moreover, while in the preceding two years predictions about 1967 were also below, the 1964 prediction was worse. And the 1965 prediction about 1967 was worse still. As 1967 got closer, our aim at it sank steadily further beneath the bull's eye.

Part of the pressure to conform by underestimating was very likely a reflex, over-correcting for the "missile gap" that had publicly embarrassed the intelligence community. But this could hardly explain the extraordinary persistence and even worsening of the errors, as evidence to the contrary began to pour in. It is interesting that the Secretary brought up the "missile gap" in 1964 to reinforce his caveat against overestimation. The "gap" had been given public burial in the autumn of 1961. The Defense Report had not bothered to mention it in 1962 or 1963. The Report revived the horrible example as part of the budget battle and issued ominous strictures against exaggeration as a way of cutting the ground from under importunate service demands based on anticipated large Soviet capabilities.

As for Soviet "capabilities," when the Secretary used that phrase, or "Soviet ICBM strength," as in the passage quoted, he referred explicitly to the *number* of vehicles deployed. These numbers are what the forecasts were overwhelmingly about, just as the forecasts during the "missile gap" had been. It was only when the number of Soviet silos completed or in process came close to catching up with the ceiling we had chosen for our ICBM force that the Secretary began to put some stress on "qualitative

superiority." In effect, he asserted by way of comfort, the Soviets may get nearly as many missiles, but ours will be better. But his FY 1968 Report insisted that especially if we counted in the SLBMs, we were still ahead even in numbers—"as of now." "As of now, we have more than three times the number of intercontinental ballistic missiles (i.e., ICBMs and SLBMs) the Soviets have. Even by the early 1970s, we still expect to have a significant lead over the Soviet Union in terms of numbers... and," the Secretary added, in a vague but dazzling phrase, able to comfort even today, "a very substantial superiority in terms of overall combat effectiveness."

But in 1971, the Soviets had the lead in numbers. Looking on the bright side—"quality"—may have dazzled perceptions of our failure to predict the numerical shift. The Defense Reports in fact contain a treasure trove of methods of bucking us up while blurring our view. Their very vagueness soothes. "By and large," said the Secretary in 1965, "the current estimates… projected through mid-1970 are of the same order of magnitude as [last year's] projections through mid-1969." And in 1966, with reassuring familiarity: "By and large the current estimates projected through mid-1970 are of the same general order of magnitude as those which I discussed here last year." In 1967, he reported that the current estimates were "generally in line" with the preceding year. "Order of magnitude" is particularly mind-boggling, but strictly implied only that this year's estimates were within one-tenth to 10 times as much as last year's. Which is less reassuring. In any case, the estimates were wrong and getting worse.

In 1968, after the huge 380-silo jump in one year, McNamara said, "We believe the Soviet ICBM force will continue to grow over the next few years, but at a considerably slower rate than in the recent past." But the rate specified fell far below the one later observed. In 1969, Secretary Clifford continued in the same cheery vein. The Soviet force has grown "well over threefold in a ... little more than two years. The rate ... has been somewhat greater than estimated a year ago. However, we believe [it] will be considerably smaller over the next two or three years." But once again the expected rate of new starts formed a small fraction of the actual. Such muffled disappointments scarcely perturbed the theory, pushed hard in 1969 and 1970, that exaggerations drove a race.

It would be wrong, I think, to conclude that the Defense Reports display a conscious effort to obscure our failure to anticipate rapid Soviet increase. More likely, wishes and policy leanings

shaped—and lowered—consciousness. But much remains to be explained. Undoubtedly, various leanings—some to expand, some to cut or reallocate strategic spending—influenced estimates of contending factions. But then we need to ask not only "*cui bono* [to whose advantage]*?*" but which estimates matched reality. Factions in or out of government have *some* compatible interests. Aside from a joint interest in accurate assessment for the common defense, all factions have at least an occupational self-interest in *not* making forecasts that fail disastrously.

Underestimates persisted for an extraordinarily long time after the error of the missile gap in part because they were fortified by an American strategic view that Americans often attributed also to the Soviets. (These were "projections" in the psychoanalyst's, as well as the forecaster's sense.) That view suggested that the Soviets did not need a large expansion of forces in order to be able to destroy a few American cities and therefore did not intend to undertake it.[34]

It was common in and out of government through the mid-1960s to hold that the Soviets wanted only a minimum deterrent, a couple of hundred missiles aimed at cities (roughly the actual number of Soviet ICBMs in 1964-65), and that they would not try to catch up.[35] We clung to this belief after they had started enough launchers to make it untenable. Then we shifted to saying they wanted *only* to catch up, just as they were passing us on the way to getting 50 percent more. "Rough parity" can be quite rough.

Action-reaction language is vague enough to rationalize events after the fact. It was a glass through which we saw darkly. It not only led us to wrong predictions about the Soviet actions, but it made inaction on our part seem reasonable. The Russians would not act to catch up, because they knew we would react to counter them, and since they would not act we did not have to. But in fact, they acted and we did not. And sometimes the Secretary argued that if we were to increase our active defense, the Russians would inevitably react by vastly increasing their offense so that in the end we would not only have wasted the money, but would end up with a net increase in the number of fatalities we might suffer. In other words, if we acted, the Russians would react; therefore there was no point in taking action.

Unfortunately, a distorting and wishful 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 advisers and by unnamed "highly placed officials" as an indication that Soviet programs were "tapering off," "leveling off," "slowing down," "petering out," "grinding to a halt."[36] 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 advisers 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 and more enthusiastic adherents; or the Millerites who gathered new followers after the world failed to end as Miller had predicted by March 21, 1844.[37] 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 against 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.

Some of these policy decisions, I believe, were justified on other grounds. But prevailing doctrine offered a generalized rationale for cutting rather than expanding. That is what happened, but we didn't notice. Our perceptions of actual U.S. past declines have been as confused as our view of supposed future Soviet increases.

III

Mythical U.S. Increases and Actual U.S. Declines

Whatever the explanation offered for the strategic race—invariably overestimating and worst-case analysis, bureaucratic politics, technology out of control, etc.—there is a prior question as to whether or not there has been a race. 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-1940s 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 about the net destabilizing effect of technical change. First, look at our supposed quantitative upward spiral in the total explosive energy that could be released or in its capacity for indiscriminate destruction.

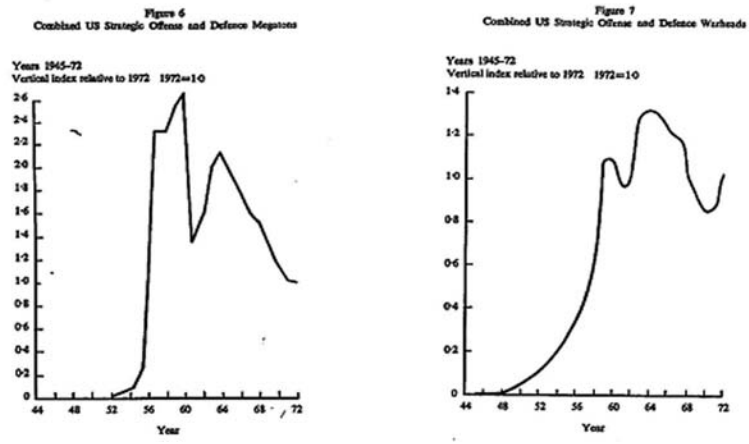
Total Explosive Energy and "Overkill"

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 the Second World War (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 the Second World War, 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 Second World War blockbusters, it would not do structural damage to an area one thousand times the size, but roughly one-tenth of that. By comparison with the smaller bomb, some 90 percent of its energy would be "wasted" in "overhitting" or "overdestroying" or "overkilling" the nearby area.[38] 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 10 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 whether one has too many or too few. That is a problem we need not address here.[39] The question we are asking is whether on this measure there has been an exponential increase.



The answer indicated in Figure 6 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 a heroic distortion. In fact, the largest offense nuclear warhead is roughly a thousand times the smallest offense nuclear warhead[40]—the same as the difference between the Hiroshima bomb and the largest non-nuclear blockbusters of the Second World War! Counting the largest and the smallest each as one—with even-handed justice—would then be exactly like dismissing the first two nuclear weapons as of negligible importance since they increased the stocks of "block-busters" by only a fraction of a percent.

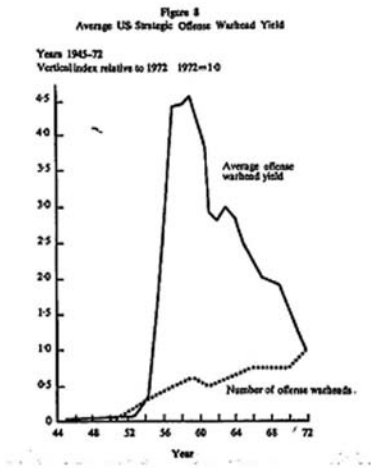
While there is no adequate single common measure for so heterogeneous a collection of vehicles and weapons, clearly something better is possible than a simple count of warheads.[41] That the latter is used so uncritically is one of the intellectual scandals of the current debate on SALT. Nonetheless one may ask whether the number of strategic offense and defense warheads has spiraled. And as Figure 7 shows, for this disparate aggregate, the answer is that it has not. It peaked in 1964 at roughly 30 percent higher than in 1972 which was about the 1960 level.[42]

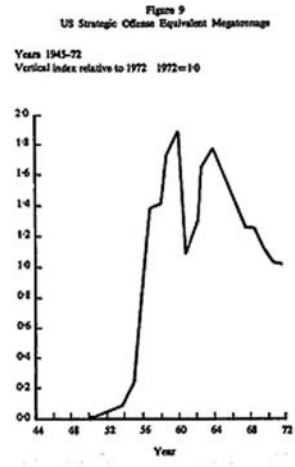
The sense of post-Sputnik arms race doctrine, with its central strictures against all weapons aimed at weapons and therefore against active defense as particularly destabilizing, plainly calls for including the Spartan, Sprint, Nike-Hercules, Falcon, and all other defense warheads in the total. However, 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 missiles, 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 1958-60 to 1972 they increased roughly by half. But their average yield was divided

by four-and-a-half (Figure 8). 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 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 over-concentration near the target. Taking a one-megaton weapon as standard, it measures any given stock of weapons 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.[43]

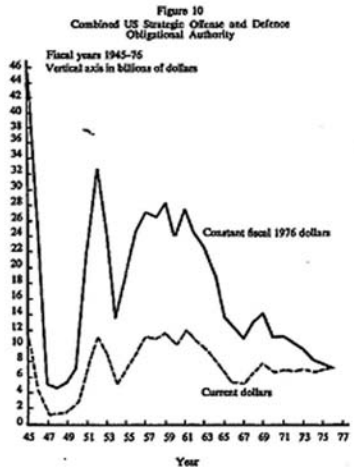




EMT, like all other indexes, has its limitations, but it captures some essentials missed in simply adding unadjusted megatons or warheads. Figure 9 shows a dramatic decrease since 1960 in the relative destructiveness, so measured, of the U.S. strategic force. At its peak it was nearly double the 1972 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 accelerating.



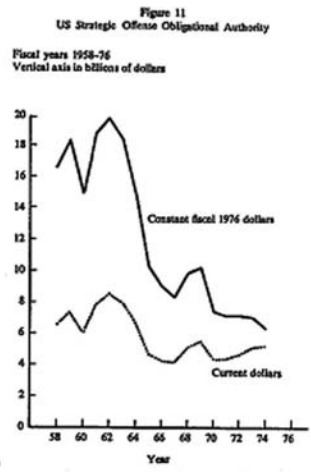


Figure 10 shows the total strategic budget as measured in the Defense Department Program I,[44] extending back to the Second World War. The top curve shows that the strategic budget in 1976 dollars declined from a peak of $32.6 billion in FY 1952 to $7.7 billion in FY 1976. Strategic expenditures have fluctuated, with a brief sharp decline and recovery after Korea, to very high levels varying between $24 and $28 billion in the seven years beginning in 1956; and then a more or less steady drastic decline to the recent low levels. In short, in real terms the strategic budget was well over four times higher during the Korean War and about three times as high at the end of the Eisenhower Administration as in 1976. This scarcely looks like an exponential increase in strategic budgets—more like an exponential decrease.[45] For the 24 years from 1952 to 1976, the average rate of decline was about 5 percent. For the 15 years from FY 1961 to FY 1976, there was a decline averaging 8 percent per year. I want to stress that this long-term decline is not simply [measured] as a percentage of GNP but in real terms. It is an absolute decline. Since real GNP was rising while strategic budgets in real terms were declining, strategic spending declined even more as a percentage of GNP. In percentage of GNP it was nearly seven times higher in the early 1950s and about five times higher in the late 1950s than in FY 1976 (3.2 percent and about 2.5 percent compared with .48 percent).

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 $12.1 billion in 1961 to $7.7 billion in 1976.[46]

Secondly, 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.

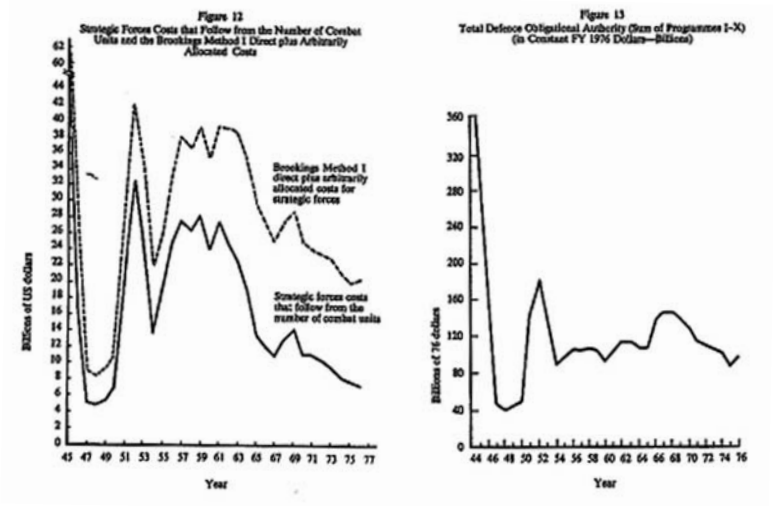
Thirdly, the drastic fall in strategic budgets measured in Program I may be partially obscured by adding in a rising but quite arbitrary "overhead" figure.[47] The program budgets for strategic or for general purpose forces aim to include all the cost of equipment, *matériel*, and personnel that can be directly attributed to the program mission, including all support costs that "follow directly from the number of combat units."[48] 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 for budgets that the Brookings Institution uses, which we call "Method I," would assign to the strategic forces an amount of overhead equal to less than half their direct costs during the 1950s, and over one-and-a-half times their direct costs in 1974.[49] 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 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 strategic forces budget was nearly double that in FY 1976 (this is displayed in the dashed line in Figure 12). The method Brookings applies to future projected budgets is less reducible to formula and involves more subjective judgment and even larger uncertainties.[50]



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 1976 dollars of over $2 billion in the late 1950s from a peak 2.5 times as high as the FY 1976 budget including overhead. With recently improved deflators the decline would be even larger.[51] Overhead allocations have their uses, but they are limited. All of them distribute some

unallocatable costs. When added to program costs without any breakdown, they obscure more than they illuminate change. Nonetheless, no overhead allocation with which I am familiar can hide the sharp declines in strategic budgets. Whether the decline is from a peak over four or two-and-a-half or twice recent levels, that should be fatal to the dogma about "ever-accelerating spending."

Nonetheless that dogma does die hard. Paul Warnke, for example, has agreed that some facts do damage the arms race figure of speech. But he talks of our continuing tendency "to spend these steadily increasing billions" and of our "formula for endless escalation in defense costs." Indeed, Warnke is so seized by the idea that the U.S. strategic budget and the defense budget as a whole have been steadily climbing that he can read a long document devoted to showing that both budgets have been sinking for years, with plunging graphs to illustrate, and not notice.[52] He did not, for example, notice the point of the article which painstakingly showed evidence of the drastic fall in the strategic program budget in real terms over the preceding 14 years. (The defense budget as a whole had been declining for a shorter time—since 1968.) He understands it to be saying that the United States and the Soviet Union have both been increasing strategic spending, but at different rates. Running at different speeds, he thinks, might still be a race. However, we have been moving not only at different speeds, but in opposite directions. If that doesn't do lethal damage to the arms race metaphor, nothing will.[53]

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 over three times its 1976 level.[54]

Fifth, I suspect the major reason for failure to observe the decline is that public debate usually concentrates intensely on the initial 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."[55] 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, along with or preceded by a drastic reduction in overseas strategic operating bases and a multi-billion 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 have 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.

The terms of the public debate have been scandalously loose and they have received very little critical attention from the media. SALT rhetoric and headlines linking new strategic programs to "Record Defense Spending" help the impression that strategic budgets especially must be out of control, since they are spotted as the main culprit in the general increase. In real terms, however, there has been no general increase in defense spending since 1968. Witness Figure 13. Picking on the strategic budget as the guilty party in the nonexistent general increase in the defense budget as a whole seems particularly absurd, since the strategic decline has been larger, more consistent, and more durable. But guilt by association has its effect because the smaller decline in total defense budgets is more easily obscured by neglecting inflation.

It is hard to fault the media when academics and politicians who specialize in defense and arms control matters themselves make such blunders, but even so the media's handling of the defense budget in recent years needs some comment. Take the distinction between real and inflated changes in dollar amounts. Although there are some sophisticated questions about methods of allowing for inflation, the gross sense of the distinction is not at all arcane. Newsman handle it all the time without stumbling. When in a recession year, 1970, the American gross national product neared $1 trillion ($970.1 billion) by comparison with $930.3 billion the preceding year, no headline greeted the news by announcing a record advance in production. On the contrary, the press observed that the GNP in 1970 was lower in real terms than it had been the year before. But year after year of Defense Department requests for budgets lower in "real" terms than the 1968 peak have been announced as "record budgets," apparently because in this case the media regard the distinction as unreal. And a press that with some justice prides itself on its energetic factual investigations is considerably weaker on analysis and reflection about even moderately complex matters. There, predisposition is more likely to hold sway.

The sloppiness is suggested in the largely unconscious predispositions implicit in the way the data are described or pictured. One can find examples among good journalists and excellent newspapers. Take the following case shown in Figure 14 of the *Los Angeles Times* announcing the new defense budget request in February 1974. The article headlines "Record Defense Spending" and suggests the primary cause for the increase in new strategic nuclear weapons of the kind that SALT is supposed to limit. Thus the lead paragraph states, "... a defense budget surpassing the peak spending period of World War II and laying the foundation for a new generation of nuclear weapons...." Only later in the article is it acknowledged that inflation might have something to do with the budget increases, and even then in wording that suggests this may just be a Pentagon claim—"While the research on new nuclear weapons systems could portend massive new spending several years hence, the $6.3 billion increase in the Pentagon's new budget largely was attributed to pay increases and in higher costs across the board for hardware and supplies."

LOS ANGELES TIMES FEBRUARY 5, 1974

Record Defense Spending Plan Includes New Nuclear Systems

BY RUDY ABRAMSON

WASHINGTON—President Nixon Monday sent Congress a defense budget surpassing the peak spending period of World War II and laying the foundation for a new generation of nuclear weapons as insurance against the failure of strategic arms talks with the Soviet Union.

"If negotiations fail and the Soviet Union seeks military advantage," Mr. Nixon said in his budget message, "the United States must be prepared to increase its forces quickly and effectively."

"Because the time required for development and deployment of major weapons systems is long, decisions made today will shape the ability of the United States to maintain its strength 5 to 10 years from now."

In asking a record defense outlay of $87.7 billion in the year beginning next July 1, the Administration proposed an $8.4 billion Pentagon research and development program plus more than $1 billion for the Atomic Energy Commission's nuclear weapons and propulsion projects.

Besides continuing work on the new B-1 strategic bomber and the giant Trident missile-firing submarine, the Defense Department asked Congress for $249 million for research and development on:

—Larger warheads for intercontinental ballistic missiles.

—Improved accuracy for ICBMs.

—A new missile-firing submarine smaller than those in the current Polaris-Poseidon fleet.

—Mobile ICBMs, which would be

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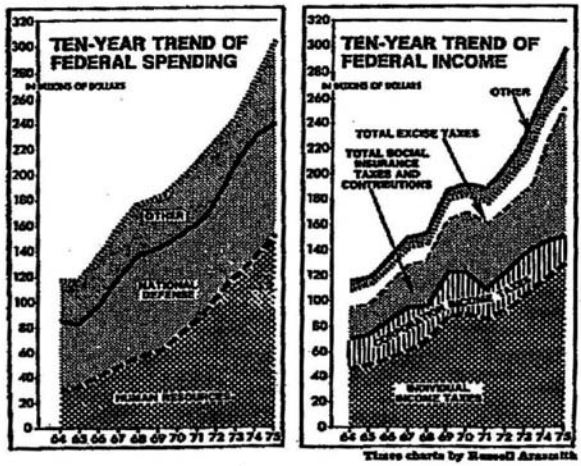


Times charts by Russell Arasmith

Figure 14

The graph, "Ten Year Trend of Federal Spending," accompanying in the article, not only reinforces the impression that national defense expenditures have been steadily climbing; it also suggests to the casual reader that they are the primary reason for the growth in the total federal budget. This effect results from piling the "National Defense" expenditures on top of those for "Human Resources."

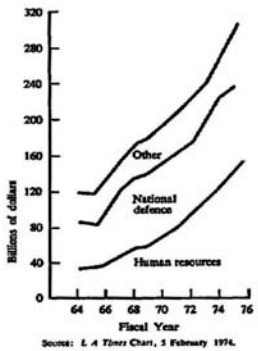
Figure 15 is a redrawing of this chart for clarity, and Figure 16 shows exactly the same data as Figure 15 at exactly the same scale.[56] The only change is that National Defense is now presented on the bottom rather than Human Resources.

The resulting chart gives quite a different and more accurate impression than that in the article. It shows that the major source of the increase in federal spending has been increases in Human Resources, not National Defense.

But even Figure 16 is misleading, since it is in current dollars and hence ignores the effects of inflation. Figure 17 presents the data of Figure 16 adjusted for inflation, i.e., in dollars of constant purchasing power. We now see a *downward* trend in National Defense spending that is more than overcome by an *upward* trend in spending for Human Resources. (In fact, more authoritative results indicate a sharper downward trend for National Defense expenditures than is shown in Figure 16. The data in the original article contain some anomalies. Retirement pay seems to have been included in the "National Defense" category, and this would help to explain the slower decline shown.)

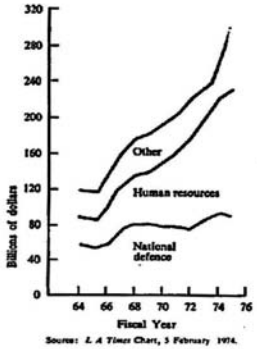
Belief in an exploding arms race is so ingrained by now in the way the media look at things that it seems even the chartmakers and layout men make their own *trompe l'oeil* [deceive the eye] contribution to its existence.

Figure 15
Ten-Year Trend of Federal Spending



Source: *L. A. Times* Chart, 3 February 1974.

Figure 16
Trend of Federal Spending—With National Defence Rather Than Human Resources on Bottom



Source: *L. A. Times* Chart, 3 February 1974.

However, the regular annual alarms in the press about an upward trend in the strategic budget can often point to economic projections *for several future years,* based on gleanings from testimony before Congress on Defense Department and service plans. Such indications of plans can mislead in the same way as comparing this year's budget request with the last enacted budget, but even more so, since the long-term plans are even more tentative and subject to attrition than requests formally submitted to Congress. They must run a recurring gauntlet through many stages of bargaining and review within Defense, Budget, the White House,

and Congress. It is appropriate to study the uncertain long-term costs implicit in various defense plans, but not to treat them as if they reflected the likely course of defense spending. Brookings says as much: "A note of warning must be emphasized. The projections should not be taken as predictions of future defense budgets. . . ."[57] As with drugs and cigarettes, however, users may ignore the warning label. (Even Brookings, normally more careful than its readers, sometimes forgets its own warning.) In any case, Figure 18 shows vividly that year after year Brookings' projections of strategic cost have sloped steeply upward, as year after year the actual budgets have continued to decline. This perpetual picture, so useful in budget battles, of a strategic budget on the point of exploding, sticks in our mind rather than any glimpse of actual history.

Figure 17
Trend of Federal Spending—Adjusted for Inflation
(Constant FY 1974 Dollars)

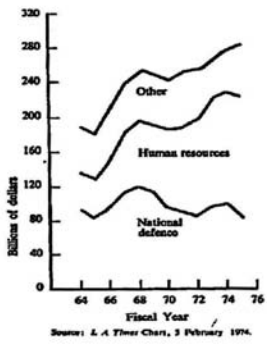
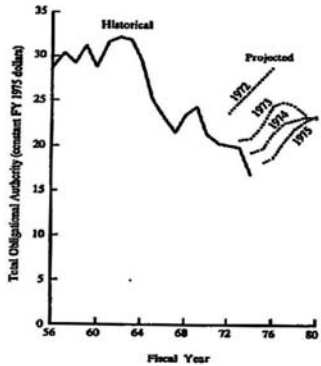


Figure 18
Brookings' Historical versus Brookings' Projected Strategic Costs
(including "Overhead")



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. If they cannot find a trend of increase in the plunging figures of the last 24 years, they find it in rosy service visions of the future, undampened by Executive or Congressional budget considerations.

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 others 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 is 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.

IV

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

forefend! 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 historical 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 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."[58] 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 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 generally. 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 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."[59] 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 was cancelled after much spending, but before deployment.[60] 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 the Second World War. 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 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.[61] 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 far-fetched—an agreement to orbit them several times before landing; or (as discussed 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 above.

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.[62] 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.

The combination of fusion weapons and missiles that enabled us to choose cheaper, safer, less destructive and better-controlled strategic forces were some of the very technologies that were thought at the time inevitably to have the opposite effects. Fusion warheads and the vastly increased speed of strategic rockets in particular made obsolete existing methods of protecting strategic forces, but they opened up new opportunities to increase the stability of the force. The principal effect of fusion technology was not so much to make weapons higher in yield, but to make low- and medium-yield weapons smaller, lighter and cheaper. This in turn made it possible to put them in rockets more easily protected by blast shelters or in constantly moving submarines. An attempt simply to stop or slow this technology would have reduced the survivability of deterrent forces and therefore diminished international stability.

Increasing the Choices

Perverse current dogmas center most of all on an attempt to stop or slow technologies of discrimination and control. However, the remarkable improvements in accuracy and control in prospect will permit non-nuclear weapons to replace nuclear ones in a wide range of contingencies. Moreover, such improvements will permit new forms of mobility for strategic forces, making it easier for deterrent forces to survive. More important, they will also increase the range of choice to include more discriminate, less brutal, less suicidal responses to attack— responses that are more believable. And only a politically believable response will deter.

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.

Language and the Present Political Chaos

> Political language—and with variations this is true of all political parties, from Conservatives to Anarchists—is designed to make lies sound truthful and murder respectable, and to give an appearance of solidity to pure wind.

Orwell, who said that, prescribed never using a metaphor you are used to seeing in print as his very first rule for reducing the decay. That would cut the vast clutter of images about racing and uncapped volcanoes that we use in order to hide from ourselves what has been happening and what the issues are. In the chaotic "debate" about Vladivostok, the proponents claimed it would put a "cap" or "lid" on the explosive increase. Opponents, from Senator Jackson to the Left, said it wouldn't: like SALT I it would only force the continuing of the spiral in strategic spending. But before and after SALT I, the spiral was pure wind; and it will be wind in the present political circumstance with or without SALT II. For the United States, one might conceivably talk about a "shoe" or a "floor," but hardly a "cap." Vladivostok also illustrates the absurdity of the exaggerated threat/"worst case" dynamic. Here, overblown estimates of future Russian programs may lend a specious urgency to rapid agreement—another "miracle" for the Secretary.

And when Secretary Kissinger asks, "What in the name of God is strategic superiority... at these levels?" he seems to be saying that it does not make any difference how many more missiles the Russians have than we—in which case it is hard to see any urgency in agreement. He sometimes explicitly means that it makes no difference, because each side now can—in the stereotype—kill every man, woman, and child several times over. But that is an example of exactly the use of language Orwell had in mind. For it implies in fine moral tones that we should measure the adequacy of our weapons in terms of the number of civilians

they can kill. The Secretary, however, does not believe that. He has also said that attacks on population are a "political impossibility, not to say a moral impossibility." I am all for probing the premises of thought on arms and arms control which the Secretary is said to want. But that can only start when we face up to evasions making "murder respectable" in such chaste phrases as "counter-value attacks" and in all the unreflective vocabulary of the arms race. This is an important part of rethinking policy about our relations with allies and adversaries, long overdue and essential for reducing the present chaos.

ENDNOTES - Wohlstetter - Racing Forward? Or Ambling Back?

1. I am indebted to many colleagues but especially to David McGarvey, Steven Honda, Gregory Jones, Robert Raab, Arthur Steiner, and Zivia Wurtele.

2. Sir Edward Grey, *Twenty-Five Years, 1892-1916*, Vol. 1, New York: Frederick A. Stokes Company, 1925, pp. 89-90.

3. John Newhouse, *Cold Dawn: The Story of SALT*, New York: Holt, Rinehart and Winston, 1973, p. 176.

4. See, for example, Morton H. Halperin and Jeremy J. Stone, "Rivals but No Race: Comment," *Foreign Policy*, No. 16, Fall 1974, pp. 88-92, and the views represented in Michael L. Nacht, "The Delicate Balance of Error," *Foreign Policy,* No. 19, Summer 1975, pp. 163-177.

5. Jeremy Stone in Erwin Knoll and Judith Nies McFadden, eds., *American Militarism 1970*, New York: Viking Press, 1969, p. 71.

6. Jerome Wiesner and Donald Brennan, eds., *Anti-Ballistic Missile: Yes or No?* New York: Hill and Wang, 1969, pp. 13-14.

7. Nancy Lipton and Leonard S. Rodberg, "The Missile Race—The Contest with Ourselves," in Leonard S. Rodberg and Derek Shearer, eds., *The Pentagon Watchers*, Garden City, New York: Doubleday, 1970, p. 303.

8. Quoted from U.S. Congress, Senate, Committee on Armed Services, *Fiscal Year 1972 Authorization for Military Procurement: Hearings,* May 3, 1971, Part 2, 92nd Congress, 1st Session, p. 1767.

9. Quoted from U.S. Congress, House of Representatives, Subcommittee on National Security Policy and Scientific Developments of the Committee on Foreign Affairs, *National Security Policy and Changing World Power Alignment: Hearing-Symposium,* May 31, 1972, 92nd Congress, 2nd Session, p. 98.

10. Robert S. McNamara, "The Dynamics of Nuclear Strategy," speech, September 18, 1967, *Department of State Bulletin*, Vol. 57, No. 1476, October 9, 1967, p. 445.

11. Quoted from U.S. Congress, Senate, Committee on Foreign Relations, *Strategic Arms Limitation Agreement: Hearings,* June 28, 1972, 92nd Congress, 2nd Session, p. 193.

12. Quoted from U.S. Congress, Senate, Committee on Foreign Relations, Subcommittee on Arms Control and International Organizations, *Arms Control Implications of Current Defense Budget: Hearings,* July 13, 1971, pp. 205-206.

13. Knoll and McFadden, eds., *op. cit.,* p. 68.

14. Edgar M. Bottome, *The Balance of Terror: A Guide to the Arms Race,* Boston: Beacon Press, 1972, pp. xv-xvi.

15. William Epstein, "Will the Russians Play 'American Roulette'," *Saturday Review World,* June 29, 1974, pp 7-8. Epstein is the former Director of the UN Secretariat Disarmament Division.

16. Bernard T. Feld, "The Sorry History of Arms Control," *Bulletin of the Atomic Scientists,* Vol. 26, No. 7, September 1970, p. 26.

17.Quoted from U.S. Congress, Senate, Committee on Foreign Relations, *Strategic Arms Limitation Agreement: Hearings,* June 26, 1972, 92nd Congress, 2nd Session, p. 139.

18. John Newhouse, *op. cit.,* p. 133.

19. Nancy Lipton and Leonard S. Rodberg, *op. cit.,* p. 303.

20. Quoted from National Citizen's Commission, Report of the Committee on Arms Control and Disarmament, White House Conference on International Cooperation, November 28-December 1, 1963.

21. Jerome Wiesner, Foreword, in Donald G. Brennan, ed., *Arms Control, Disarmament and National Security,* New York: G. Braziller, 1961, p.14.

22. Herbert F. York, "Controlling the Qualitative Arms Race," *Bulletin of Atomic Scientists,* Vol. 29, March 1973, p. 4.

23. George Kistiakowsky and George Rathjens in *Scientific American,* Vol. 222, No. 1, January 1970, p. 27.

24. Harvey Brooks, "The Military Innovation System and the Quantitative Arms Races," revised draft distributed at Aspen Conference on Arms Control, August 1974.

25. Quoted from U.S. Congress, Senate, Committee on Foreign Relations, *Détente: Hearings,* September 1974, 93rd Congress, 2nd Session, p. 195.

26. E.g., Nancy Lipton and Leonard S. Rodberg, "The Missile Race—The Contest with Ourselves," in *The Pentagon Watchers, op. cit.,* p. 303; Dr. Jerome Wiesner, *ABM: Yes or No,* Santa Barbara, CA: Center for the Study of Democratic Institutions, 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.

27. For the data on the statistical distribution of predictions on which Figs. 1-3 are based, see *Strategic Review,* Fall 1974, U.S. Strategic Institute Report 75-1.

28. *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.

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

30. See, for example, Nacht, *op. cit.*

31. This figure is based on Nacht, *op. cit.*

32. In this, we follow Nacht.

33. Impressions from even a relevant picture, such as Figure 4b, can stand supplementing by the computation of a few statistics and the summary of the results of a variety of statistical tests on the differences between predicted and actual silos: (a) The mean underestimate of -80.1 silos per year is significant (using the Student's t-test) at the .001 level using the more rigorous "two-sided" criterion, that is, assuming appropriately that predictions can exceed as well as understate the reality. (b) The least squares trend line has an r^2 equal to .40, but its slope, -12.59, is significant at the .05 level. (c) There is no significant trend up or down at the .05 level for the sub-period 1965-1969. (d) The worsening displayed is confirmed by the Student's t-test for the difference between the sample means for 1962-1964 compared with 1963-1969. (The difference between predicted and actual is worse for the second sample at the .01 level.) (e) And finally, the Wilcoxon two-sample test, a robust "distribution-free" test, using only rank orders, also shows that later sample to be worse than the earlier at the .05 significance level. All of the above tests are two-sided.

34. Such a 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.

35. 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. Compare the *Military Balance, 1962-1963,* London, Institute of Strategic Studies, p. 2: "The Soviet Union thus appears committed to a policy of 'minimum' or counter-city deterrence in relation to the United States, though the large medium range missile force it has now developed and deployed against targets in Europe and Japan may serve as both." 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,* 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.

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

37. 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, 1967. Festinger's model of cognitive dissonance fits the history of the theory of systematic overestimation rather well.

38. For an early appreciation of this point, see, for example, P. M. S. Blackett, *Fear, War and the Bomb: Military and Political Consequences of Atomic Energy,* London, 1948.

39. I address it briefly in Fred Warner Neal and Mary Kersey Harvey, eds., *Pacem in Terris III, Vol. II, The Military Dimensions of Foreign Policy,* 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.

40. 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.

41. 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.

42. The curves on numbers of warheads (Figure 7 and bottom of Figure 8) are smoothed in order to approximate the calculated data points, but closely enough so that deviations from the trends discussed are not significant.

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

44. Program I refers to Strategic Forces. Program II refers to General Purposes Forces. We have used unpublished computer tabulations dated July 24, 1975, available from the Office of the Assistant Secretary of Defense, Comptroller (Programs and Budget) of Total Obligational Authority by Program, which were extended back beyond FY 1956 only recently.

45. The decreasing exponential fit is rather good. The r^2 for the period 1952 is .75, and the r^2 for the period FY 1961-76 is .88.

46. Recent Department of Defense computer tabulations have revised upward the 1961 current dollar estimate from its earlier-reported level of $11.5 billion.

47. See, for example, "The Advocates," WETA-TV, Washington, DC, February 14, 1974.

48. Martin Binkin, "Support Costs in the Defense Budget," Washington, DC: Brookings Institution Staff Paper, 1972, pp. 45-46.

49. The Brookings Institution uses a different method, which we call "Method II," when estimating the effects on overhead of future reductions in the strategic combat forces. We are indebted to Barry Blechman for generous help in explaining Brookings' methods.

50. If I were to suggest changes to Brookings, one would be to display separately, in past and future budgets, the overhead fraction. For future budgets especially this would help. It is easy to expect greater overhead savings from direct program cuts than are likely: total overhead and the direct program budgets have moved in opposite directions. Yet Brookings' overhead formula for past budgets, like most such allocations, assumes that overhead costs vary in a straight line with direct or operating costs without any time lag. And it loads an increasing proportion of total overhead onto the strategic budget. For such reasons I present figures on the strategic budget *with and without* Brookings' overhead allocation.

51. 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.

52. Paul C. Warnke, "Apes on a Treadmill," *Foreign Policy,* No. 18, Spring 1975, pp. 12-29.

53. Maybe nothing will. As an even more striking example of how hard the dogma dies, consider the following statement by Bernard Brodie written after I had assured Mr. Warnke that I was indeed saying that the total cost of our strategic force has gone down, not up. "We should note here Albert Wohlstetter's denial that there is an arms race between the two superpowers, though he concedes there is an arms competition which raises costs on both sides." Mr. Brodie cites not only my original paper which documented the decline in U.S. strategic costs, but also a reply to Mr. Warnke in which I said again that U.S. strategic costs had not "escalated" but had gone down. "On Clarifying Objectives of Arms Control," ACIS Working Paper No. 1. Program in Arms

Control and International Security, University of California at Los Angeles, April 1976, p. 22.

54. 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 program cited above.

55. Nancy Lipton and L. S. Rodberg, "The Missile Race—The Contest with Ourselves," in *The Pentagon Watcher,* New York, 1970, p. 301.

56. Figures 15 and 16 were drawn by first reading the data points on the graphs in the original article as carefully as possible using a ruler and set square. I do not know the source of the data used or kinds of budget moneys employed (e.g., obligational authority, expenditures, or other); or what definitions were used for the three categories displayed, and I have made no attempt to justify the data or explain the anomalies in it. In particular, I am at a loss to explain why National Defense expenditures show a decline from 1974 to 1975, even though the article talks about a $6.3 billion increase.

57. B. M. Blechman, E. M. Gramlich, and R. W. Hartman, *Setting National Priorities; the 1975 Budget,* Washington, DC: Brookings Institution, 1974, p. 306.

58. Jacque Ellul, *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. 163-164.

59. Lipton and Rodberg, *op. cit.,* p. 302. Cf. Richard Barnet, "The National Security Bureaucracy and Military Intervention," paper delivered at Adlai Stevenson Institute, June 3, 1968, p. 27.

60. Nuclear-propelled aircraft, started in 1951 and cancelled 10 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.

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

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