## *Theory and Opposed-Systems Design* (1968)

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### *I. Madness in Methodology?*

When, after nearly a decade of study and work in the field, I left mathematical logic and the logic of science, I made a resolution not to write papers on the methodology or logic of social science—for fear I would never learn any social science. It was all too easy at the time to publish applications of Boolean algebra or the calculus of relations or the like that could just conceivably be relevant to some future empirical study, in, say, economics. But I had the uneasy feeling that in offering guides for new approaches to social science, I might never approach very closely myself. And I did want to learn something of the facts of life and the substantive issues whose powerful interest had dragged me away from the more chaste attractions of logic. I also had an uncomfortable suspicion that the devastating remark of the great French mathematician, Henri Poincaré, about sociology ("The most methods, and the least results") might only too accurately describe the way one might dally in the approach to any social science in order to avoid actually going in and getting lost in a very dense jungle. Maps, brochures, the purchase of compasses, machetes, bush jackets, and rakish tropical helmets can be used as a substitute for a hot and sweaty journey. In short, I sympathize with Johan Galtung's misgivings about theories about theory in a theory-poor field. (And with the feeling expressed by Burton Marshall since I first wrote these lines: reading the behavioralist literature in international relations seems a bit like sitting through an overture that never ends.[1] But I find that traditionalist critiques of behavioral essays on methodology, with rare exceptions like Marshall's own laconic contributions, have their own *longeurs.)*

Nonetheless, I find myself on the point of talking about an approach, and supposedly a distinctive approach, to the study of international relations—a notoriously impenetrable jungle. One customary way to begin such a discussion is to tick off all the other approaches, the wrong ones, and to end up with a shiny, colored brochure describing the right one—that sole hope for social science, your own. That is not the plan of this paper (though flesh being what it is, it might, of course, turn out that way).

The sort of study that has mainly engaged me for the last sixteen years has been pragmatic in purpose. Yet it seems to me that, from time to time, it has displayed traits of the relations among nations that are interesting and even important for theory. It has at any rate involved the extensive use of theory. That is to say, it has used mathematical models in "essentially general" form, models that refer to potential operations among states or other elements in the international system in a way that cannot be reduced merely to elementary statements about individual objects or to a finite conjunction of such singular statements.[2] It has also involved a great deal of grubby, highly specific empirical work on technologies, operations, costs, and potential interactions among states, factors that are plainly relevant for decisions of the governments of these states—or for citizens evaluating these decisions. It has required the cooperation of several disciplines and, in particular, a kind of close working together of natural science and social science disciplines which remains very unusual, if it exists at all, in universities. Hence, "a new approach."

On the other hand, it is quite clear to me that this line of attack hardly exhausts the approaches to the investigation of the relations among states or even the good approaches. And its novelties do not mean a total discontinuity with other ways of looking at the subject. I believe, in fact, that for all the obvious differences in its quantitative form from the classical or traditional writings in the field, with a bit of stretching of both, the approach I shall call "opposed-systems design" can be accommodated within the classical tradition quite as easily as within the more recent behavioral studies. It has indeed dealt with some matters at the heart of traditional international relations theory—namely, power relations among states—in a particularly operational and concrete way. Much behavioral theory does not. It differs from classical theory in subject as well as method.

Declaring yourself neutral in the war between the classicists and the behavioralists is probably about as safe as claiming neutrality between General Cao Ky and the Buddhists in Vietnam, and as little convincing to either side. Nonetheless, it is true that I have a high regard for a good many traditional studies of international relations—so far, for rather more of them perhaps than for the new studies. At the same time, I believe that some numerical relations are essential in understanding the changing relations among states; that they are frequently implicit in at least rudimentary form in the classical works and could stand more rigorous statement, imaginative extension, and systematic confirmation or disconfirmation by evidence. And I suspect that the specific quantitative methods that engage behavioralists today include some of those that might suggest fruitful theory. The current practices of traditional and behavioral studies do not exclude each other, nor do they together—or even in combination with the approach I shall describe—exhaust the possibilities. It is very easy to find miserable examples of any method, including, I would stress, the one I shall describe. There are no methods certain of result in a complex field of research. None is proof against a dim awareness of interesting problems or incompetence in formulating manageable and significant questions. The truth is that international studies are a hard line of work. The useful inquiries in international affairs that contrast in method, in good part, seem to me to complement each other, but to focus on different questions.

My purpose in this paper will be to describe the sort of study I have been concerned with, and then to try to locate it very briefly with respect to other studies in the field, some traditional and some (to use once again the current jargon) behavioral. The precision with which I can locate the method of opposed-systems design is limited by the fact that, while I have been actively concerned for quite a few years in the field of international affairs, I can claim no encyclopedic understanding of the literature. In any case the comparisons, as I have already suggested, are not invidious but orienting.

## *II. Opposed Systems*

### A. Questions for Decision-Makers

I shall use the phrase "opposed-systems design" to name a kind of study that attempts to discern and answer questions affecting policy—specifically affecting a choice of ends and of means to accomplish ends that stand a good chance of being opposed by other governments. The ends of any government are multiple and only partially incompatible with those of other governments—even very hostile ones—and of course such conflicts may be resolved without fighting. A peaceful resolution may depend in part on the risks involved in combat. In any case the conflict of aims raises the possibility of combat: and a major part of these studies is concerned therefore with the likelihood and the likely outcomes of such combat. In fact, they grew out of operational research as it had been practiced in World War II.

The positive reasons for my choice of this label will be made clear in what follows. On the negative side, "opposed-systems design" replaces several synonyms—some of my own devising—which have not quite succeeded in fending off casual misunderstanding. One workable synonym might appear to be "strategic studies"; but the phrase is at best ambiguous and at worst a militantly indiscriminate epithet used by antagonists of any study of potential military conflict. The most familiar serious candidate is E. L. Paxson's "systems analysis" and, in fact, this has the largest currency; there is now, for example, an able Assistant Secretary of Defense for Systems Analysis. But the word "system" is everybody's possession. It is used rather differently by engineers in "systems engineering," by theorists of international relations, and in particular by Mr. Kaplan in his "systems theory," and, rather mysteriously, by the general semanticists in their "general systems theory." As a short name for a complex of interdependent elements, the word "system" seems nearly indispensable, but not specific enough. Yet it is used without qualification to designate very different kinds of complexes of interdependent elements. I have tried in the past to discriminate the sort of study Paxson had in mind from many of these others by talking of "conflict-systems design," but that has the difficulty of suggesting that the goal of study is to generate conflict. "Conflict-worthy systems," modeled on "sea-worthy," is a more accurate term but even more awkward. Perhaps "opposed-systems design" is closing in on it. Potential opposition at any rate is an essential.

In both England and the United States during World War II, as is well known, a considerable and very fruitful effort was devoted to operational research, to the systematic analysis of alternative ways of accomplishing various proximate objectives. These analyses aided decisions on how to deploy and operate radars and coordinate them with interceptors in the Battle of Britain, how to pattern the movements of destroyers searching for submarines in the Bay of Biscay, how to determine the optimum altitudes for penetration and bomb delivery in the European theater, and a host of other matters. Studies of similar scope and intent continue today and are applied to aid or implement the decisions not only of national organizations but also of alliance and international (including interadversary) organizations. Among the latter studies are analyses of the instruments and sampling inspection procedures for an underground test ban or to prevent the diversion of material or equipment from peaceful to nonpeaceful uses in nuclear reactors operated under international agreements.

Present as well as past operational research had to do with how best to operate with given organizations and specified equipment in order to achieve various near-term goals. The operations studied have been essentially tactical. After World War II, however, broader analyses to aid decisions were made, dealing with a longer run in which a wider range of alternatives could be made available. New equipment could be designed, developed, and purchased, organizations could be expanded or contracted, and more numerous uncertainties were likely to affect the environment in which they operated and the goals they worked to achieve. Such cardinal choices, to borrow a term from C. P. Snow, might be illustrated by the decision on how to allocate resources for a strategic force that would not be operational until some years hence and that one might expect to constitute a major part of the operational force for the better part of the following decade. How much should one spend on increasing the size of this force and how much on protecting it and making it more subject to control? This specific choice was a vital one in developing a second-strike capability and in clarifying the objective of deterrence. Another question presently much debated, especially in connection with the decision on ballistic missile defense, concerns resource allocation between offense and active and passive defense. In an international environment that includes five countries that have made nuclear explosions and over one hundred and thirty that have not, still another cardinal issue today concerns the choice of military stance, formal or informal alliance commitments, and practicable international treaty arrangements among adversaries that may best reduce the expectation of nuclear war and the damage it would do. Such larger studies contrast with operational research mainly in degree, in the number of factors considered, and in the time perspective. In fact, they normally incorporate many operational research studies as components. They may be said to consider the larger "strategic" alternatives as distinct from the smaller "tactical" choices made in operational research, provided "strategy" is understood broadly enough to include a choice of ends as well as means.

All such studies, whether in the large or in the small, concern alternative systems involving both items of equipment and organizations of men using them. In this respect they are like the systems engineering studies of large complex systems in the public utility field, such as telephony, transportation, or postal systems. But in a public utility like the Bell Telephone system or the Japanese Super Hikari express train system, the principal obstacles to be overcome are natural ones: difficult terrain, storms, earthquakes, atmospheric disturbances, etc., with direct human opposition, such as sabotage, forming only a minor concern. In the field of arms and arms control both the peacetime and wartime decisions that will affect the safety and power relations among states must all be taken with potential man-made obstacles in mind; their success in good part depends on other decisions that may be taken by an at least partially hostile government.

## B. Theoretical Models

In elaborating an analysis of the capabilities in the 1950s of either of the two major nuclear powers for striking back after nuclear attack on its strategic force, or in analyzing the feasibility and cost in the 1970s for one of the two major powers to limit potential damage by using active defenses against an initial ballistic missile attack, mathematical models embodying a theory of these interactions are necessary. Sometimes large-scale computer models are required. Sometimes a small analytic model will catch essential features of the subject matter. A study of the protection of strategic forces in the early 1950s[3] used differential equation models capable of analytic solution on a slide rule, as well as Monte Carlo computer models for some component studies. Optimal solutions found by partial differentiation required fixing in advance the values of a great many variables (numbers of targets struck, the number of vehicles forming a "cell" to saturate defenses, the number of warheads, the number of kilograms per warhead, the overpressure resistance of elements on bases and their dispersal in space, deployment and delay times in the active defenses, approach and penetration routes and altitude profiles, and peacetime costs that varied for alternative readiness choices among others). Though some simple analytic models have been useful, their realism and utility have depended on their being associated with a painstaking empirical examination of variations much too complex to be represented by a well-behaved analytic function or a smooth curve. For example, the losses to be expected by aircraft penetrating distant defenses and many other costs that vary with distance are seldom essentially continuous or linear or monotonic-increasing. They may not even be steadily nondecreasing. Nor are they derivable from common experience.

It is worth observing that, contrary to current legend, opposed-systems analyses have made little or no use of game theory, and while they normally require many map exercises, they have not been heavily dependent on formal games or experimental simulation. I would guess that games and game theory have played a much smaller role in serious studies whose main aim is to aid specific decisions on opposed systems than they do in the more general academic behavioral literature on international conflict appearing in such magazines as the *Journal of Conflict Resolution.*

In the more successful studies, mathematical models of potential military interaction have played a rather pragmatic role, but they are essential. On the other hand, so is a great deal of elementary arithmetic; and much study of data derived from state-of-the-arts studies, theoretical analyses of equipment design, tests of existing equipment and components of future equipment, peacetime operations and logistics; and also political data permitting at least rough judgments of such contingencies as the loss of various overseas base areas. (Political catastrophes such as the loss of bases may affect aircraft and tanker requirements quite as much as technological factors like specific fuel consumption.) Since the choices to be affected extend years into the future, the alternatives compared and studied empirically may include not simply the received or existing alternatives but also invented ones. The invention of operations, organizations, or equipment has, in fact, been crucial in the studies that have worked out best.

### C. The Time Span Covered

These theories and the policies they serve deal with a future that is long compared to the models and choices in traditional operational research, which aims at proximate goals for forces substantially fixed in size and composition. On the other hand, their scope in time has been modest by comparison with that of attempts to construct theories of international strategy, as in

Schelling, or systems theories, as in Kaplan, or theories of the balance of power like Deutsch and Singer's highly general semi-quantitative construct; or many of the more traditional, less explicitly quantitative theories. The sort of opposed-systems design of which I speak studies technologies, operations, political interactions, and economic costs stretching perhaps for as much as a decade and a half into the future, and designs alternative systems to operate within that period, which has seemed to be about as far in advance as the technological and political context can be foreseen or parameterized with enough constraints to yield conclusions. In fact, though hope and salesmanship may spring eternal for eternal final solutions to our troubles, the best practice is quite self-conscious about the finiteness of the life of measures proposed, and will estimate their end. Thus, at the beginning of the 1950s, it was possible to design a system of deterrence for the rest of the decade in the United States, which used tactical warning to permit alert response as an essential part of a complex set of arrangements. But by the time the system was designed and some of its elements adopted in principle, while it had seven years or so to run, it was also foreseen by the designers that travel times for attacking vehicles in the early 1960s would be so sharply reduced that warning and alert measures, while still useful, would no longer have a decisive importance. They would not, at any rate, be adequate. Measures that did not depend on warning and fast response, such as shelter for strategic vehicles or a mode of operation which kept vehicles on the move, would be an essential both for survival under attack and for reducing the likelihood of a fast and irrevocable response to a false alarm. At the start of 1954, a second study which designed a deterrent system for the 1960s suggested the methods of hardening that were later adopted, but explicitly anticipated that the adequacy of such measures would not outlast the 1960s, when guidance technology could be expected to reduce the inaccuracy exploited by protective construction.[4] (The first sketch of the study was entitled "Defending a Strategic Force After 1960" and had a subsection entitled "After After 1960" which dealt with technological changes likely in the 1970s.[5]) In both studies, estimates of the length of time at the end of which the design measures would no longer suffice turned out to be quite accurate. It is interesting to observe that ambitious smaller powers developing nuclear forces have chugged along, ten years out of phase, just in time to develop first- and second-generation forces capable of meeting the past but not the contemporaneous threat.

The perspective of ten or fifteen years or so may not be an essential permanent feature of such opposed-systems design and analysis. But neither is it accidental. It has been connected with the fact that some of the major technological changes take that long to come into effect, once they are visible. It has taken about that long for some of the potentially decisive changes in the state of the art to go from the stage of well confirmed principle through research, development, engineering, and procurement to operation on a considerable scale. After that they are likely to remain in operation for some time. In the summer of 1953, for example, Bruno Augenstein and (a while later) the Gardner Committee perceived the implications of high-yield, relatively small, light fusion payloads for transforming the performance of the intercontinental ballistic missile program then under desultory development for over a half a dozen years. However, even the crash program that resulted, and many billions of dollars, could not advance the time to a date earlier than the 1960s, at which ballistic missiles would make up the bulk of the force of the two major powers. It was possible in 1951 and 1952 to recognize that vehicles travelling at ballistic speeds *might* appear in the force in the sixties decade; and by 1953 to recognize they *would* be; and in both cases to take such impending changes into account in designing systems of deterrence. Years before forces are in operation, it is possible to analyze their interactions with some success, and frequently also to recognize the time limits in which the analysis is valid. It is not solely, of course, a matter of the technological state of the art. Some of the conditions of the analysis will also concern the rate of change at which political arrangements may take place. So at the start of the 1950s, with base rights in two dozen or more countries, one could safely assume that while some rights would be withdrawn, not all nor, in fact, most of these rights would be lost by the end of the decade. One could, moreover, test alternative base systems for how they would fare under a variety of reasonably likely contingencies; but, beyond a decade, the variety of possibilities multiplies very fast.

I would not exclude the possibility of dealing with longer-run futures. Indeed, some sorts of gross technological and political change may be visible in outline decades off and yet require so long an incubation period that they need some actions now to bring them into being or to prevent some desirable futures from being foreclosed. Even designs for Bell Telephone must sometimes be planned on a time scale involving decades. Changes in urban development and population concentrations are extremely slow, and some of the time constants in urban and regional design need also to be quite long. It is apparent that some major features of the international environment will change only over a period of decades, and, while attempts at increasing safety must be open to the wide variety of contingencies implied by such a scale, some gross limitations on this variety may be decipherable. There are a number of attempts now current to look at such long-run futures, or proposals to do so (Bertrand de Jouvenel's *Futuribles;* the Commission for the Year 2000 of the American Academy of Arts and Sciences; the Hudson Institute Project on the Year 2000; the Institute for 21st Century Studies at Ball State University in Muncie, Indiana; Olaf Helmer's projected Institute for the Future; and many others) and such activity may yield useful guides for designing systems for very long-term changes in international affairs. However, for the time, the empirical success of such studies of the long-run future lies in the future; we may hope in a shorter-run future.

The upshot of the foregoing is that, at best, the theories developed so far in opposed-systems design cover a self-consciously restricted interval of time in which the critical, potential, dynamic interactions are mainly contained within the span of less than a decade and a half, sometimes considerably less.

## D. Means

What I have said already makes clear that an opposed-systems design deals with a complex variety of means and conditions including various technologies, modes of operation, organizations, and economic and political factors. Most important, such factors have to be dealt with simultaneously, since there is a great deal of feedback. Take the critical role of technology, for example. If you look at economic treatises you will find statements like "We assume as given the maximum amount of output *x*, which can be produced from any given set of inputs ($v_1 \ldots v_n$). This catalogue of possibilities is the production function and may be written $x = \Psi (v_i \ldots v_n)$".

For an opposed-systems design a procedure of taking the technical coefficients as fixed or as undetermined parameters will not do. A central part of the inquiry must look at the current and impending state of the art and at feasible and useful changes. In the past two decades in which such inquiries have grown up, nuclear, electronic, propulsion, and transport technology have changed massively. The problem is not just to predict such changes, however. Since this is a work of design, it must explore how—in the light of interdependencies with military, political, and economic events—the changes may usefully be bent.

Technology with its enormous changes presents not only essential problems for the analysis, but also some of the major distinctive opportunities for such an analysis. For, along with the uncertainties, a system with a large technological component, like the highly organized warning, command, control, communication, and reaction systems of aircraft and missiles, inevitably displays many regularities and predictabilities, and the changed relationships brought about by order-of-magnitude increases in a critical technical variable will also be accessible to theoretical analysis. (Thus changes of three orders of magnitude in the explosive yield of a given volume and weight of payload, and by an order of magnitude in the speed of vehicles, or by an order of magnitude in delivery accuracy, can be expected to have decisive and analyzable effects on the economic and operational variables.)

Analyses of opposed systems have worked out best where the technical component has been large and where, as a result, the problem of predicting the outcome of operational interactions has been more manageable. (Yet not without its surprises: some of the greatest successes have come where large changes in the technical components impend, but the ramified consequences of these changes are obvious only after an analysis of considerable sophistication.) Analyses have worked least well where the systems analyzed have been determined by minutely varied local characteristics, such as terrain, morale, training, etc., with no gross technical components dominating the result. Operational analyses of the interaction of ground forces are seldom convincing for this reason, except where there are many obvious disproportions between the components of strength of opposing sides. The formal models they employ—usually some simple differential equations of a type introduced by F. W. Lanchester near the start of World War I[6]—have not often provided very persuasive or useful representations of these highly variable, locally determined phenomena. In their simplest form, Lanchester's equations state that the rate of change or dissipation in a military force is proportional to the absolute size of the opposing military force. The constant of proportionality in this negative term represents the rate of destruction that can be brought about by a unit of the opposing military force.

$$\frac{dX}{dt} = -k_y Y$$

$$\frac{dY}{dt} = -k_x X$$

Partly, perhaps, because these equations have a simple analytic solution, a vast literature has grown up elaborating them and applying them to a very wide variety of cases.[7]

There are some actual cases which approximate these equations. In this respect there is quite as much to say for them as for Richardson's formally similar equations, now rather popular for representing arms races. In fact, some rather better fits have been found for the Lanchester equations.[8] But they do not represent a universal law governing all combat. And Lanchester himself was aware of situations in which they did not apply at all. They have not been much help in predicting the outcomes of classical war between large armies.

Judging the outcome of potential classical combat is a problem not simply for analysts, of course, but for decision-makers, too. The Israelis, for example, feel themselves menaced in a world in which their hostile neighbors outnumber them by a factor of twenty-five. They regard their own superiority in morale, training, education, and technical skill as making up for some of this numerical difference in population and even in the number of tanks and other equipment, but have made clear that there are some changes in Arab military equipment and even some political changes that they will not tolerate. They believe that such changes would presage a successful Arab attack. But how does one estimate the outcome of such complex interactions in which so many of the variables that influence the result are hard to measure?

Just before the Suez campaign of 1956, Czech and Russian arms arrangements with Egypt drastically increased Egyptian superiority in tanks and jet aircraft to a ratio of four to one. According to General Dayan, "In artillery, naval vessels and infantry weapons, the Israel picture was no better. It was not only the disparity in quantity but also the superiority in quality which decisively upset the arms scales."[9] A maxim attributed to Napoleon is that the moral is to the material as four is to one. It is, however, difficult to establish a unit of moral, and it is therefore rather hard to know how to trade it against jet aircraft and tanks. In any case, the Israelis decided not to wait until the increase in Egyptian armaments had become operationally effective. Again in 1967, on the basis of published figures,[10] it appears that the Egyptians had about 430 combat aircraft, not counting jet trainers, and the Israelis had about 200, not counting their 60 Magister Fouga trainers. There were large discrepancies in other arms, and if one counted in the Iraqi, Syrian, and Jordanian air forces (these countries had all joined Nasser in the week preceding the outbreak), the odds looked again to be close to four to one. Such gross order-of-battle figures are hard to interpret. And in 1967 it is clear that the Israelis and the Egyptians interpreted them differently. Intuition had to serve. But it did not serve the two sides equally.

Intuition plays a role in all theorizing, too, but in a successful systems analysis the theory can do a good deal to support and sharpen and sometimes correct intuition. The Israelis have recently, along with the Swedes and some other of the smaller powers, done a good deal to develop systems analysis and opposed-systems design. But it appears so far that their analytic successes, like those of the NATO countries, have been not as much in large-scale ground war as in very small unit interaction and in the more technologically determined areas, such as those involving aircraft. Air war was Lanchester's starting point a half-century ago, even though he applied it more broadly. So far I know of no convincing opposed-systems analysis in the large (i.e., strategy) of warfare between large armies.

The growing importance of technology and the gross changes in performance effected by new states of the art assure an increasing range of application for the sorts of theory used in opposed-systems design. It is a paradox that we can do better in analyzing the potential outcomes of some sorts of conflict that have never occurred than we can do with conflicts of the sort that have been endemic for ages. This does not mean, of course, that the new sorts of conflicts would not have their surprises. It remains true that anticipating the course and result of a war between armies of men with variable intensities of political motivation and skill on terrain whose multiple surface deformations strongly influence outcomes of separate local engagements over a period of weeks, months, or even years is extraordinarily hard—except possibly for cases where an opponent grossly outclasses the other in all relevant respects. And, by comparison, one can with relative ease predict the consequence of 50 or 100 fusion-tipped intermediate range ballistic missiles with known accuracies and yields exploding on ten or so aircraft bases containing vehicles without benefit of tactical warning or blast protection. A relatively few measurable variables determine the outcome. This "easy" analysis, however, is not trivial. It has substantial contemporary relevance, for example, to an estimate of the second-strike capability of the first-generation French nuclear force based on some ten points in south and southwestern France. Against a small force of Russian rockets used appropriately, it has no significant probability of survival. Slightly more complicated analyses of their second-generation force yield similar results. Neither the Mirage IV bomber force nor the hardened missile force in Haute Provence which will succeed it could survive an attack from the more advanced contemporary forces whose threat they are supposed to deter. Such an analysis would be reinforced by considerations of the problem of protecting centers of command and the flow of information to and from them; and of the cumulative obstacles that can be interposed inside the territory of a major antagonist. Uncertainties qualify all empirical analyses, but in these cases they are much reduced, so gross are the determinants of the cumulative interactions. Rather more complex but quite reliable analyses can be made of the third-generation French force. These analyses of the military performance do not say all there is to say about the *force de frappe,* or the broader questions of incentives and drawbacks to the spread of nuclear weapons, but they say some things of great importance.[11]

Finally, though the regularities introduced by technology have played an important and even a critical role in opposed-systems design, such analyses nonetheless are not purely technological, though some technologists are in the habit of saying so. There are essential interactions and feedbacks, as I have already said, with operational, economic, and political events.

### E. Ends

One of the disabilities of the phrase "strategic studies" as a description or title of the opposed-systems analyses that have grown up since World War II is that at least some of the dictionary definitions of "strategy" limit the word to the study of alternative means to attain fixed ends. These wide-ranging studies, looking ahead for many years, differ from operational research by taking as a salient objective the clarification and revision of the objectives maximized.[12] This point is worth stressing not simply because of the possible misleading associations with the word "strategy," but because of some current semi-comic misunderstandings on the subject. Unlike operational research on tactics, opposed-systems design of major alternatives tends where it is successful to involve a careful critique of constraints and objectives. A government's ends cannot be accepted as the final deliverances of authority or intuition. They are subject to revision as the result of an analysis that frequently displays incompatibilities with other ends of that government, or that indicates means so costly that the game is not worth the candle. Moreover, even when an opposed-systems design does not set out to revise objectives, it is quite likely to end up that way.[13]

The tentative character of the objectives examined in an opposed-systems design and the importance of questioning ends as well as means are not merely minor qualifications in a general practice of finding the best means to fixed, unquestionable ends. They are major points of difference from operational research, stressed from the start by the principal practitioners of opposed-systems design. The need to take objectives only on trial is imposed, in the case of actual research on broad policy issues extending over years, by the very breadth of the inquiry. There is no authoritative or intuitive set of goals perfectly compatible with each other and with content enough to furnish guidance. In fact, there is always a multiplicity of goals in partial conflict. Political circumstances and technologies alter, making the old goals partially irrelevant and sometimes offering opportunities to satisfy several desired objectives simultaneously that had been previously incompatible, or vice versa. The well-defined preference function establishing at least a weak ordering among all possible alternatives, which is a convenient assumption in much of economic theory, is never realized in fact even for individuals, much less for nations. "All possible alternatives" are not in general definable and not all of the possibilities we might specify are strictly speaking "connected," subject to a weak order: there are some complex pairs of alternatives we don't know how to compare, how to establish one member of the pair as no worse than the other. While there are, of course, some *partial* orders among our preferences, frequently we learn how to compare them only in the process of an analysis.[14]

Of course, a government agency seeking aid in its decisions may have quite firm ideas to start with as to what it wants to accomplish by a specific decision and may hope for succor only in the choice of means. Nonetheless, precisely because governments have limited resources and more than one objective, there is always the possibility that the initial objective will be bought at too great a sacrifice of other goals. And, from the standpoint of the sponsoring agency itself, one critical advantage of objective research on policy is that it can aid decision to avoid irrational sacrifice of important goals by pointing to the need for revising ends. Moreover, in governments such as that of the United States, questioning goals need not be terribly dangerous to the questioner; there are always enough factions espousing varied ends to provide some safety in dealing with a short-sighted or dogmatic leadership.

One may ask, however, whether there are not limits in the method of opposed-systems analysis which prevent the questioning of some objectives. Isn't it tied to the "power structure"[15]—whatever mist that hazy phrase designates? If the conclusion of a systems analysis were to propose the overthrow by force of a government sponsoring it, it would be rather unreasonable to suppose that the sponsor would be overjoyed: "Yes, indeed, the analysis has not met my original objectives, but it has hit on something more important: my violent overthrow." Or, "It has met my original objectives, and even better, it involves my violent overthrow." But few foreign-policy objectives of government in the United States seem to be so fundamentally at odds with the realities that they require overthrow of our government for their accomplishment; and if they are, this is hardly a limitation characteristic of the method of analysis. Let me expand on this a little.

So far we have talked about governments and nations. Most of the problems normally considered in international relations have to do with the relations among states. This is, to be sure, somewhat artificial—an approximation useful for some purposes, like the treatment of stars as point-masses in astronomy. However, the internal structure of states may critically affect conflict or cooperation among states, the start or ending of wars, and many other matters. Specific peace terms may look less tolerable to the ruling faction than continuing to fight; concluding the war may then require dealing with a faction previously not in control. Dealings with governments to end World Wars I and II provide several examples. An analytic understanding of alternatives in civil wars is of interest, therefore, to the international theorist as well as to the decision-maker.[16]

I refer to the decision-maker both in and out of the government. There is no reason why a revolutionary might not find it handy to use the tools of opposed-systems design himself. Mao, Giap, Guevara, and many others have worked out theories of how best to overthrow some sorts of government, complete with suggestions as to the technical equipment for conducting guerrilla war as well as the political devices that seem to have worked out best. A careful reading of their manuals suggests they might use a more tentative and systematic self-correcting mode of theorizing themselves, and there is nothing in the character of opposed-systems design that gives capitalist governments a patent which cannot be infringed. Though revolutionaries normally require rather rigid adherence to their programs, it should be observed, of course, that the ends of revolutionaries are multiple and often turn out to be in conflict, too, and therefore cannot safely be regarded as final. A good opposed-systems design to bring about a revolution would not be too rigidly tied to the unanalyzed goals of the revolutionary power structure.

### F. Uncertainties, Simplifications, and the Role of Inequalities

Statements about new approaches tend to be both programmatic and excessively hopeful. I believe there have been some successes in the analysis and design of opposed systems. But I have tried to suggest, as I have gone along, some of their limits. In fact, very large uncertainties affect both the ends and the means dealt with in an opposed-systems analysis; and the models used, while solving some problems, introduce others. Inevitably they simplify, and therefore introduce error. Simplification is a problem for all theory. I can say just a little about both the uncertainties and about how opposed-systems design has dealt with them and with the biases of theoretical simplification.[17]

First, on the uncertainties. The long period between the gestation of a technology and its birth, operational life, and death has a double aspect, so far as uncertainty is concerned. It means that the system as originally conceived will have to face a great many eventualities that were unlikely to have been foreseen at the time of conception. On the other hand, it confers some element of stability and predictability that can be used in an analysis.

The B-36 took some seventeen years from the idea of it to the time at which it was phased out of the strategic force. It was conceived shortly after the fall of France as insurance against the contingency that Britain might fall, too. Its proponents thought of it as a way of reaching Germany with high explosives from bases at intercontinental distances, if no bases nearby were available. It was at the beginning a propeller plane, designed to operate against defenses consisting of guns and propeller-driven fighter planes. Its designers did not consider the opposition of surface-to-air missiles or jets and knew nothing of the Manhattan Project which was shortly to develop nuclear explosives. In fact, they learned of the Manhattan Project only when most of us did, with the explosion at Hiroshima. By the time the B-36 was phased into the force, after many vicissitudes, it had four jet engines as well as six propeller engines. It was expected to carry a nuclear payload over quite different routes to quite different targets against a different enemy with markedly different active defenses, and an offense that might make even bases at intercontinental distances unsafe.[18] The history of tactical fighter planes seems even more regularly to display disparities between initial conception and actual operating conditions. This can be illustrated by the story of the P-47 Thunderbolt and the P-51 Mustang in World War II.[19]

Such large and ineradicable uncertainties present problems in plenty for analysts, but even more for dogmatists. And large bureaucracies teem with dogmatists. Of necessity most of the bureaucracy will be engaged in the complexities of day-to-day decision of the sort that keeps a bureaucracy afloat. Intelligence tends to be expended in the short run, while frequently very large changes are gathering and—to the persistent eye—are already visible just beyond the short run. The familiar trait of inertia that characterizes large and complex organizations confers an especially great marginal productivity on realistic analysis of the basic changes impending and their significance. New technologies involving dramatic order-of-magnitude improvements take a considerable time to become operational realities; this fact limits the range of uncertainty, making it possible to look ahead. The characteristics of decision-making in large organizations frequently insure that, without a systematic effort at analyzing the distant consequences of coming changes, programs will be obsolete by the time they come into effect. Inventive and realistic systems design has been useful not so much because it is intrinsically so good as because the alternative of routine decision is so bad.

The strategy for dealing with uncertainty is related also to the method of treating the biases introduced by theoretical simplification. The equations of the physical sciences typically simplify: they hold only under ideal conditions. However, in contrast to the empirical associations found in most quantitative social science inquiries, inequalities or differences between predicted and actual values can frequently be explained (by the physical scientist) as due to deviations of the experiment from the ideal conditions assumed in theory. Differences or inequalities, as distinct from equations, have played another role, but a crucial one, in opposed-systems analyses. This role has to do with the prominence of arguments of an *a fortiori* sort, running "even if . . .; then more so, since in fact...." In comparing alternative systems with one programmed, one cannot eliminate uncertainty, but one can assume that they will be resolved favorably from the standpoint of a dubious programmed system. One cannot avoid theoretical simplification, but one can design a model to favor the programmed or other losing systems and to give them the benefit of the doubt. Then if the comparison shows that, even with all the favors bestowed by the model's assumption, the system programmed or otherwise likely to be chosen is vastly inferior to an alternative, this offers substantial ground for choice. Moreover, it should not be surprising that bureaucrats exhibit enough inertia to make such *a fortiori* analyses possible and very useful, as some opposed-systems analyses have been.

## *III. Links to Other Theories in International Relations*

### A. Theories of Decision in International Affairs

Opposed-systems designs have looked at the choices available for government decision-makers where such decisions are interdependent with decisions of other governments. This concern connects them in an obvious way with theories of decision-making in international politics of the sort associated with Richard Snyder, H. W. Bruck, B. Sapin, and J. A. Robinson. However, not just these scholars but most theorists of international relations are, in one way or another, concerned with the foreign-policy decisions of governments, or the decisions of international organizations. A good many such theorists, including many of the behavioralists, take decision processes and decision-makers as their main subject matter: for example, they study how decision-makers behave in crisis. Indeed, Rosecrance and Mueller, in a sympathetic and knowledgeable but critical review of academic quantitative studies of the last decade (those using factor analysis, content analysis, international simulation, and the measurement of communication flows) make the point that these studies cannot be dismissed as they are by the classicists because they sometimes use rather indirect measures, since the "truly relevant information" for both the classical and the newer studies would be data on the processes of government planning and decision and is "scarcely ever available until long after the event."[20] Rosecrance and Mueller assume, in other words, that the proper subject matter for study is the decisions themselves—that theory should be mainly, so to speak, meta-decisional. It seems doubtful that as much of the focus of inquiry in the traditional literature has been meta-decisional. On the other hand, an opposed-systems design will deal with the factors that affect and are the subject of decision rather than only or mainly the decision process and the decision-maker. It will deal with such matters as the deployment of radars, the amount of warning available along various routes against various attacks and how this might be changed, or with the number of tons per day that can be lifted to support a blockaded population, like Berlin's, or with the number of kilograms of fissile material that might be diverted from peaceful uses in a nuclear power plant designed to generate electricity, given specified inspection arrangements under an international atomic energy authority. It will be concerned with analyzing and designing methods of control and response in crises. Crises in fact are likely to be taken as a test of deterrent systems. It will also look at patterns of behavior of various decision-makers, including inert and other irrational forms of behavior. But unlike most of the social-psychological studies with which I am familiar, an opposed-systems design would be likely to concentrate on the substantive consequences of the various alternative decisions that might be taken, and how these consequences might satisfy or disappoint the multiple ends of the governments concerned.

### B. Potential Wars

Opposed-systems analyses have focused on how our national, alliance or interadversary choices might affect the likelihood and likely outcomes of various sorts of combat. This focus is clearly related to a main, historic way of looking at relations among states at least since Hobbes and Rousseau, who viewed the anarchy of sovereign independent nations as a state of war—actual fighting or perpetual anticipation and preparation for it. In the United States the powerful tradition of realism in international theory has, of course, shown a large concern with military power relations among states. But in one way or another almost all approaches to international affairs must cover this ground *en route.*

Realist geopolitical theories of the balance of power have been useful in calling our attention to the interests and aims of nation-states and the way such interests might be realized or bounded by their relative military strengths. Not only the theoretical essays, but some of the theoretically-oriented realist historical works—such as Tang Tsou's monumental study of the *American Failure in China, 1945-51*—have been persuasive and illuminating. But realist theories are often content to dichotomize interests into "vital" and "nonvital." For some purposes such gross distinctions may be serviceable. The functionalists in international law use this rough division to suggest areas which states will not entrust to international adjudication and those they might.[21] Postal service and cultural exchange seem clearly not vital. However, for purposes of weighing actions that might lead to war, such a simple dichotomy is hardly enough. In this connection, as often as not, a "vital" interest is simply defined as one that a nation would fight for. This definition has crowned many a tautology in which, for example, some respected foreign-policy expert warns Congress that it would be a mistake to suppose that China would not fight if it felt its vital interests were at stake; or perhaps reassures Congress that China will not fight unless its vital interests are at stake—two pieces of wisdom derivable by definition rather than by long experience as a China hand. A great many aims of a nation-state may be incompatible with aims strongly held by other nations or coalitions of nations, and actions in furthering such aims may risk war. But just how much they risk war and how much war itself would put at risk can vary from the insignificant to the catastrophic. Much more explicit and systematic treatment of goals and interests, and the costs of fulfilling them, is needed for purposes of policy decision, and is needed in an opposed-system analysis to aid decision.

Balance-of-power theories have come in for a flood of criticism, much of it centering on the term "balance."[22] While the many ambiguities in the notion of equilibrium used in such theories are worth pointing out, I do not think that they are very hard to clarify and correct. A concept of equilibrium and the associated notions of stability and instability have been useful in social as well as biological and physical science. Handled with care, they can be fruitful in theories of international relations. The notion of "power" itself, which in these contexts has had considerably less critical scrutiny, is something else again. Even when it is conceived as military strength, rather than in the broader and vaguer terms of any capability to "affect" the behavior of others, it bristles with alternative meanings, and sometimes seems bereft of all. These lacks sharply limit the uses to which the traditional theories of the power relations among states can be put.

Among traditional theorists even acute critics of balance-of-power theories implicitly take power as if it were measurable by a simple arithmetic quantity. In this respect they are like the objects of their critique. Case studies of the balance of power have frequently described quite concretely the military forces arrayed on opposing sides: the numbers of army divisions, tanks, aircraft, ships of various types, and so on; and also the broad geophysical setting: oceans, land masses, ranges of mountains, and so on. However, such specifics are inputs, not outputs of "power," which, even though it may be tacitly assumed to be a single quantity, is undefined. These inputs offer only impressionistic grounds for judging the outcome of any concrete conflict. But in international affairs we are interested in the possible outcomes of a great many conceivable interactions among nations. These vary from subversion and guerrilla actions, through classic naval or ground engagements in the homeland of major antagonists or in some distant theater of war, to the results of nuclear exchanges under a variety of circumstances of outbreak. A country with few classical military forces and no nuclear capability might be able to manipulate covert force effectively. The delivery range and destructive radius of weapons and the problems of supporting operations logistically vary for different circumstances and kinds of conflict, and at various times. No single, one-dimensional quantity will characterize the range of capabilities usually intended when we talk of military "power." Strength, in short, is a vector with many components. It takes a good many numbers to describe the outcomes that interest us. And systematic analysis may be needed to project even one.

Just as we can be reasonably sure that postal services don't engage "vital" interests of sovereign nations in conflict, so some questions about the relations of force between nation-states are gross enough to be settled on the basis of the impressions about air and naval power and oceans and continental land masses. But a good many others cannot, though they are susceptible to subtler and more systematic analysis.

On military power relations among states, the behavioral studies and the quantitative approaches that are usually contrasted with traditional theories of international relations do not seem to me to be a decided advance. On power relationships the empirical work has been slight; the theory has been too general to be both meaningful and true. Perhaps the slightness is due to a kind of shunning of the subject. For, as I have suggested, though behavioralists may contrast their approach with the traditionalists mainly in terms of method, there seem to be differences in subject matter as well. With a few exceptions, the empirical quantitative work with which I am familiar has been concerned with international organization and integration, and where it has been concerned with conflict, the social-psychological analyses have dealt with subjects like national and international images that might create tension, or decision processes in crisis, or the tendencies of individual decision-makers to distrust the governments of other countries or to see them as threatening. I know of little work, however, on the actual military potentialities of the various states in relation to others and how these might affect the threats as well as how the threats are perceived.

As for theory, let me take by way of illustration the question of how military strength varies with distance. I have treated this at length elsewhere[23] and here can indicate only schematically the results. Nonetheless, this example may serve to display some of the characteristic continuities and differences among (1) traditional theories, (2) the rather general "behavioral" theories, and (3) opposed-systems analyses of power relations among states.

(1) In traditional theories of international relations, some references to distance or proximity and their effects are implicit. Sometimes they appear in describing the possibility of conflict itself. The abundance of Rousseau's idyllic state of nature had something to do with the fact that enough space separated men to enable them to satisfy their desires without seriously clashing with each other. And in the much less idyllic condition of anarchy among the states in Europe, Rousseau's vivid description of their unstable configuration is made in terms of their close juxtaposition, touching "each other at so many points that no one of them can move without giving a deadly jar to all the rest." A casual survey of classic writings on the anarchy of independent states turns up a multiplicity of references to problems of equilibrium of unconnected sovereigns "in the same neighborhood."[24] The power to do harm has limits in range, and so space would seem to provide not only more room for satisfying goals without jostling but also a cushion of safety. Of course, "neighborhood" is a qualitative term, and it is apparent that vicinities are elastic and have stretched in several dimensions with time and improvements in communication, transport, and optimal weapons range. The qualitative condition assumes only that states are close enough to have reason for conflict and means to fight each other. However, not infrequently traditional balance-of-power theories are talking

about essentially quantitative relationships, even though they present them informally and in everyday language rather than in symbols. This is true, for example, of the geopolitical treatments of the way military strength varies with distance, that underlie some of the familiar notions of spheres of influence. So, for example, Spykman: "Power is effective in inverse ratio from its source"[25]; and Kennan: "... the effectiveness of the power radiated from any national center decreases in proportion to the distance involved."[26]

The assumption of a sharp weakening of strength with distance underpins much of the recent discussion of the need to reduce American commitments (though, of course, the motivation of the debate has less to do with theory than with the frustrations of the Vietnamese war). The theory runs: Great powers can use force to keep distant great challengers at a distance from areas near their border, their "sphere of influence"; this makes possible a balance which is best left alone; it protects at the same time as it limits the interests of opposing states, and in any case it cannot successfully be upset.

It is both a strength and a weakness of this traditional theory of a proportionate weakening of strength with distance that its purity is marred by qualifications about differences in the variation of strength over air, sea, and land distances. References to "air powers" or "naval powers" versus "land powers" make evident that the pure theory needs qualification, but do not make clear just how such qualification can he effected. Some of the more formal quantitative theories on the other hand are quite pure.

(2) Kenneth Boulding has formulated a general theory of conflict and defense that is intended to comprehend the relation to distance of both classical or conventional strength and the strength of current forces of "world-wide range."[27] (The traditional theory I have outlined contemplates classical strength only.) His theory states in brief: In the classical case the amount of strength provided out of given resources decreases, or the cost of maintaining a fixed amount of strength increases, linearly with distance; stable equilibria between widely separated large and small powers are therefore possible; but in the case of contemporary delivery technology, the loss of strength with distance vanishes, as does also the chance of stable systems of national defense.

Boulding's mathematical model is derived from models developed by Harold Hotelling and Arthur Smithies for the analysis of the spatial competition between economic firms distributed in a line. It involves some simple linear differential equations, for which he offers as one interpretation: two countries, with their homes at points A and B respectively, each have a certain number of men who can be devoted to fighting; at a point outside the home countries, say between A and B, some out of the total number of men that each can muster have to be devoted to supporting the fighters, leaving fewer to fight; the farther out from A and nearer to B the fighters from A go, the more bearers are needed and so there are fewer fighters. ("Bearing" or "supporting" can be used inclusively to mean all activities other than fighting needed to make fighting possible.) If the forces available to A at home are larger than those available to B at home, they may still reach some point of equality in number of fighters at some point in between that is nearer to B. Though the theory is essentially a logistic one, it is assumed that at the point of equality the conflict is going to be a tie, hence an equilibrium point.

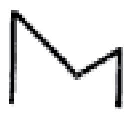
Boulding's model is static as well as linear. It has the virtue, however, of being more precisely simple than the traditional theory, which it generalizes slightly. Like the traditional theory, it assumes that strength is one-dimensional. (Boulding recognizes at one point that strength is really multi-dimensional, but dismisses this as a second-order effect,[28] as he dismisses deviations from linearity as minor.[29])

(3) It is possible to look more closely at various components of strength and how they vary with distance and to pay attention to a host of variables absent or implicitly held constant in a simple model, formal or informal. For either classical or nuclear strength one can examine not merely logistics or combat delivery, but also the attempts to interdict supplies and to use offense or defense to blunt opposing fire. And even so far as logistics is concerned, one can look at the alternative systems of transport available at any given time, at the result of varying allocations of resources to the purchase of lift or other support capabilities, and at changes in the technology of transport and communications at a distance. If one does this, in realistic, empirical detail, it is apparent that the linear picture of one-dimensional strength declining with distance is not merely a vast oversimplification of reality; it is wrong. In the first place, at any given time, and especially today when the range of possible sorts of conflict has increased dramatically, strength (as we have suggested) cannot be measured by a single arithmetic quantity, but by a sequence of many; and so for loss of strength. This is by no means a second-order effect. Equilibrium points that balance the strengths of two nations with respect to one component of the vector will not in general coincide with points that equalize

strength with respect to other components. And problems of the stability of equilibrium are much more complicated for both theory and practice.

Second, even when we look at components of strength, neither nuclear nor nonnuclear components behave like the simple linear picture. I shall sketch the results of some relevant close analyses of nonnuclear cases in the 1960s: the support capabilities in possible wars in Himalayan India and in Thailand by China on the one hand and the United States on the other. And I shall also outline a few of the results of an extensive nuclear study— the variation with critical distances of various sorts of nuclear strength during the 1950s.

Take the nonnuclear cases. Following Boulding, the linear model of decrease of strength with distance may be represented in the case of two powers with unequal home strength as a kind of lopsided M with legs of different heights representing the strengths of each of the two powers at home, and with the two slanting members meeting at a point nearer the shorter leg. Something like:



The vertical legs represent the strengths at home of the two countries; the slanting lines show how the strength declines at various distances away from home toward the adversary. The point at which they meet is their equilibrium point.

This simple picture, I believe, is a fair representation of what a good many columnists and members of Congress have in mind when they talk of comparative disadvantages to the United States in fighting eight or ten thousand miles away from home against an adversary whose home base is near the scene of conflict. A curve representing the lift capability of the United States from its borders to the China-India border in the Himalayas and a Chinese capability from Cheng-tu-Szechwan to the same points in the Himalayas looks very different. It is both nonlinear and discontinuous. One such curve is shown in the accompanying Figure 1. Another such curve in Figure 2 shows the change in support capability of each side as a function of distance from home to battle on the Thai-Laos border.

The most striking fact displayed by these figures, however, is that the long-distance lift capacity of each side massively exceeds their short-distance lift inside the theater, especially in the very short ranges in which the battle would be joined. But these bottlenecks inside the theater are to a very considerable extent determined by local factors: harbors, ports and loading facilities, railroad and road capacities, etc. They are not a function of the long-haul distances. The dramatic sweep of the curves showing, for example, the first 8,100 miles of hauling from the United States, while it catches the headlines and affects intuitive judgments, hardly determines the results. The bottlenecks are inside the theater. The important factors are the unimportant-looking little ripples in the cascade at the bottom of the chart which are so small that, in the Indian case, we have used a balloon within a balloon to magnify them enough to be visible. Nearly the same is true in the Thai-Laos case, where the United States from 8,500 miles away can lift four times as much to the Thai-Laos border as China can from 450 miles off; and U.S. capability in the combat zone is a small fraction of its long-haul capability.

If one looks at it in cost terms, the minor importance of the long haul appears even more vividly. It can be shown that adding several thousand miles to the distance at which remote wars are fought adds a very tiny percentage to the cost of fighting such wars.

The curves displayed, it should be stressed, are the result of a great deal of grubby, inglorious empirical work using a variety of detailed operational models to calculate the capacity of road nets in various seasons and a host of other laborious but necessary inquiries. One might be tempted to dismiss such labors as of little theoretical importance. However, they are important both for policy and for theory. Intuition on such matters is not enough, even when presented in formal mathematical dress. The curves show this.

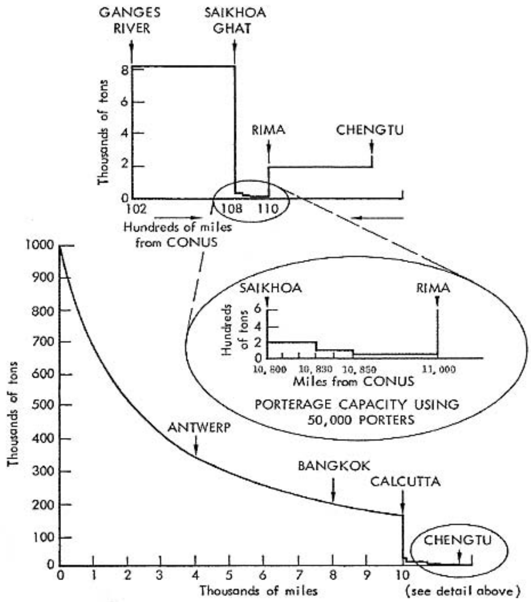



FIG. 1. Lift from United States mainland and from Szechwan to Orient-India border. (Wohlstetter and Rainey, 1966.)

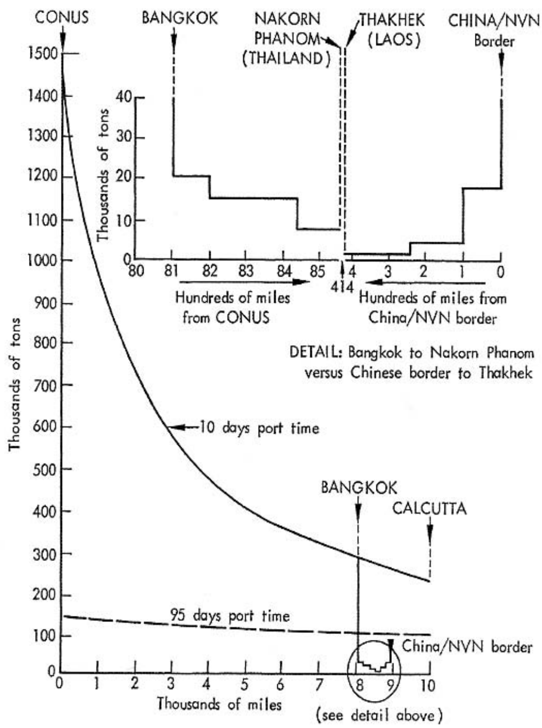



FIG. 2. Lift from United States mainland and China border to Thai-Laos borders. (Wohlstetter and Rainey, 1966.)

The nuclear case also behaves quite differently from the assumption. First, if we neglect opposition by offense or defense, the costs of nuclear strength on the linear model should not increase significantly with distance. In fact, they do, and more than linearly; that is, more than the model suggests even for nonnuclear strength. (The formal linear model of strength weakening with distance also neglects opposition.) Cost curves for the 1950s generation of subsonic turbojets have an J-shaped form, rising asymptotically at points less than the maximum base-target distance, and costs of tanker refueling systems increase in steps at an increasing rate. Ground refueled systems increase in steps modestly. (Among other things, this suggests the wide variation *at any given time* in cost-radius curves depending on the choice of system.)

In the nuclear case, if one takes into account opposition by offense and defense—which means examining a very large number of potential conflicts and the interdependent choices of both sides in these conflicts—then the situation is reversed; it is even further from the simple linear model. Then the costs of a nuclear second-strike capability in the 1950s *decrease* sharply and effectiveness *increases* if operating bases are kept far back at intercontinental range. The decrease in costs and the increase in effectiveness, however, are not monotonic. While an overseas base system close to adversary attack was vulnerable, as well as difficult to support, an intermediate operating base system was even more costly and almost as vulnerable, with nearly all of the defects of the overseas base system plus a good many others of its own (extremely high aerial refueling costs, etc.). In fact, the intermediate operating base system combined the defects of the vulnerable overseas operating base system with the defects of an extremely high-cost, exclusively air-refueled intercontinental system. The latter was considered and rejected as an alternative to an intercontinental ground-refueled system. Against moderate enemy offense the least costly system was the intercontinental ground-refueled system. The advanced overseas base system was some 50 percent higher, and an intercontinental air-refueled system was roughly double the cost. The intermediate system was nearly triple the cost. Against a more formidable enemy offense the advanced overseas operating base system became about as expensive as the exclusively air-refueled intercontinental system. The intercontinental ground-refueled system remained cheapest and the intermediate system remained worst, being more than three times as costly for a given performance.

The importance of distance for the determination of nuclear strength is not merely a phenomenon of the 1950s. While the nature of the dependency changes, some large country examples (like the American extended-range Minuteman III and the enormous expenditures to increase the range of submarine-launched missiles) show the continuing importance of such complex dependency in the 1970s. And the troubles to be experienced by the medium-sized and smaller *n*th countries illustrate the continuing importance of distance even more vividly.

Sociologists and students of international politics have frequently referred to the maximum range of individual aircraft or missiles and the growth of this maximum range over calendar time as an indicator of the increasing capabilities for projecting military strength or civilian transport and travel and the consequent increasing interdependence of the world. Boulding's use of this parameter is then a familiar one.[30] However, while maximum delivery range or maximum speed of individual aircraft or maximum destructive radius of current explosives are suggestive, they are inadequate measures of strength. They deal with performance only crudely and leave out costs altogether. There is, for example, no direct connection between the maximum range of individual vehicles and national capabilities to do battle at a distance. Even if one neglects the subtler considerations of performance affected by interactions with adversaries, the factor of cost is essential. The nuclear propelled airplane, for example, a vehicle of very extended range, could be established in the 1950s as a poor way of projecting strength, one that would lower capabilities for fixed resources. This became obvious when one considered even a crudely measured performance for an entire system to be bought and operated out of a given budget. The unit costs were so high that adopting the system would have meant, for a fixed expenditure of resources, a decided reduction in the strength we could project even nearby.

Finally, the belief that stable nuclear equilibria are impossible owes its origin to some of the hoariest conceptions of the nuclear age. It neglects, among a good many other critical matters, the difference between first- and second-strike capabilities. Such stabilities are feasible, but limited and uncertain and not automatic.

### C. Specifics and the General: Imaginary Gardens with Real Toads

I have tried to describe some of the features of opposed-systems analyses, and some of their chief limitations to date, and I have used as illustration some results that bear on variations of strength with distance. The models used in opposed-systems design are plainly not intended to cover all the characteristics of all possible relations among nation-states from the Treaty of Westphalia on, nor all of the data that have been generated by agencies reporting on one or another aspect of the various nation-states or their intercourse. They are limited and partial. It is sometimes suggested by writers on some future international theory that one has the alternative of constructing a partial or limited theory on the one hand, or a total or general theory on the other. However, no theory is "total" in the sense that it deals with all possible traits of any given subject matter, and the notion of "generality" is an ambiguous one. Sometimes when one says that theory $T_1$ is more general than theory $T_2$, one means that $T_2$ is a special case of $T_1$ and deducible from it. $T_1$ is more powerful, has more content. On the other hand, sometimes one says $T_1$ is more general than $T_2$ when it is a proper part of $T_2$—as a geometry may be a proper part of a physical theory, and so may have less content. Or one may call a theory general because it has some undetermined parameters. In that case it is not an empirical statement. It might become one if operationally meaningful constants are substituted for the parameters, or if the parameters can be "bound," that is, said to hold for all or some values. For such parameters are, of course, really variables. They are blanks, pronouns without antecedents. Like some economic models, some of the formal models in international politics may be of this character.

Boulding's own general theory is general in this sense. A great deal of it consists in elementary truths of analytic geometry. These identify various regions of a quarter plane as regions of stability or instability. Though such statements yield categories of possible systems, they have no empirical content specific to international conflict. And the curves that divide the quarter plane, like the straight lines we have examined, have slight empirical relevance. In fact, the notion of "strength" as such is given no operational content. A typology of possible systems may be of use, but it is important to be clear that one is dealing with taxonomy, not with theoretical laws (much less "the great law of diminishing strength with distance"[31]). It is all too easy in constructing such a model, as I have remarked elsewhere, to get the exhilarating feeling that one is filling holes when one is only outlining them. Boulding contrasts his own theoretical bias with the sociological and taxonomic bias of political scientists working on types of international systems. He exaggerates the contrast. There is nothing wrong with taxonomy. It can be a most useful stage in preparation for the formulation of laws, but for this purpose one has to be clear about the difference.

The work of the Quaker physicist, L. F. Richardson, after some vicissitudes of attempted statistical testing, tends now also to be reduced to typology. Richardson started out by formulating differential equations of a very simple form, relating the rate of increase of arms expenditures of each side in any arms competition to the amount of the expenditures of the other side. The equations are essentially the same as the Lanchester equations described earlier except that the variables refer not to initial forces of each side but to the annual arms expenditures, and the right-hand side of the equation is positive. The familiar solution is an exponential, suggesting that arms expenditures lead to explosive arms races and (with some lacunae in the inference) to wars. Richardson began with this simple relationship about the time of World War I, but in the course of the interwar debates introduced extra terms and parameters into his equations to take into partial account such countervailing influences as budget constraints. There are enough terms and parameters in the equations to make them fit just about any actual configuration of arms expenditures. And the theory, which has been revived in recent years and is now rather frequently cited,[32] has become essentially a taxonomy, a way of classifying stable and unstable parameter values.

Richardson was an original and able research man. But there are some rather large drawbacks in the typologies obtained from the use of his equations. Constraints like those of a budget are introduced only in a very inadequate and unrevealing way, with no explicit reference to alternative choice. On the other hand, I know of no persuasive historical example of the simple sort of explosive arms process he had originally in mind where the extra terms are of minor importance. The one historical case that some contemporary commentators have called a "fairly successful"[33] application involved, among other substantial defects, only five observations in all on annual differences in arms expenditures before World War I. Hardly enough to be convincing. It will be no fault of Richardson's if, out of our madness for method, we accept the forms of these equations as a substitute for substance, and make them a permanent addition to our gadgetry.

Fundamental theories with a very wide range of reference may be based on common experience rather than on systematic empirical tests, and they may say very little about any particular subject matter. But they nonetheless can have great importance. It is not my intention to disparage them. On the contrary, several theories with a much wider range than any we have discussed throw light on the structure of interdependent choices much more fundamentally and inclusively than any study of national or international choice. For example, the mathematical developments of von Neumann, or more recently of Lloyd Shapley and others in game theory, or the less rigorously formal theories of bargaining and strategy of conflict in the sense of Schelling: these again are much more general than a study of international politics.

Nonetheless, it would be a rather arbitrary usage to limit application of the term "theory," still more of "explanation," to works of such a high degree of generality; still more arbitrary to limit it to models with undetermined parameters, sentential functions rather than completed universal "if..., then..." statements. Discussions of this subject tend to be muddled by a dichotomy sometimes used between the "nomothetic" or law-like and the "idiographic" that concerns particular, named objects, "some." However, it is apparent that no statement—not even a singular statement about individual objects—is idiographic in the sense that it only concerns particulars. We say, for example, that such-and-such an individual object bears some relation to such-another one: "Jill tumbles after Jack." If we aspire to say something rather than mutely to point, we have to ascribe properties, class membership, relationships. And, on the other hand, very many quite respectable laws contain references to particulars. They contain the operator "all" in uneliminable fashion. But they also use the operator "some," and may name individual objects. Kepler's laws for solar orbits, for example, make up an important theory, even though they refer to a subject restricted both in space and time. Moreover, though this theory seems obvious to us now (every new idea, Whitehead reminds us, has been called obvious by someone who did not discover it), it was in fact a most precarious inference from the astronomical observations available at the time. Peirce found that there were 79 alternative theories that Kepler tried before hitting on one that worked. Kepler's theory is less general than Newton's, whose inverse-square law later showed that bodies would move in an orbit that is a conic section, with the origin of the central force at the focus. But even then, to derive the elliptical form of solar orbits one needs to know the relative masses and the relative velocities of the sun and the planets—all individuals—and to neglect perturbing effects of some distant masses. It is even possible, if we accept a hypothesis propounded by the French physicist Duhem and also by Peirce, that all the laws of nature, such as Newton's and quantum mechanics, hold within the margin of error of our observations only for very, very long historical epochs. It is conceivable that the relations are slowly changing. In that case, of course, all laws would be restricted in space and time.

The word "theory" is used in the field of political science rather differently from its most familiar usage in the natural sciences, or in economics. It is frequently reserved for very basic studies in the philosophy of politics, and sometimes for studies in the history of the philosophy of politics. These seem to me to be valid enterprises, interesting and rewarding. And, though the word "theory" has, at least in academic circles, a eulogistic character, it would be a waste to spend much time arguing for the title.

Like some of the more general empirical theories, and unlike some of the crude empirical statistical associations, the models used in opposed-systems analyses are essentially general. A good many of the statements in them refer to domains of potential operations and cannot be reduced to statements about individuals. They are idealizations. They are hypothetical, like some of the more general theories. However, if I may borrow a phrase from Marianne Moore, these are "imaginary gardens with real toads in them." The restriction in time permits great specificity in input, the use of laws with bound variables and genuine constants rather than sentential functions, and a richness in detailed conclusions.

Very general theories and some simplified small-group experiments are sometimes used to justify policy conclusions, even though some essential specifics are lacking. Much of the discussion of the state of strategic forces on the international scene is discouragingly innocent of an awareness of even the relevance of specific information, not to say of the information itself. It makes a good deal of difference whether a strategic force is based on 400 bases or on 28. It makes a difference whether a third of the force is in the air at all times, with fuel tanks full enough to complete a combat mission, armed with all the necessary electronic equipment and other preparations; or only four percent of the force, and that almost entirely on training missions, unarmed, and on the average inadequately fuelled for combat. It makes a difference whether the tactical warning provided by radars along feasible routes and profiles of attack matches the degree of readiness and speed of response of the forces warned. In the 1950s a great many members of the academic community, as well as journalists and members of governments, were in error on each of these and a good many other essential factors affecting capacities for second strike, and yet spoke rather blithely about policy on the subject. Many analyses of the Cuban missile crisis are affected by the same carefree indifference to essential features of the military stance of each side. Some of these data are, of course, governed by rules of secrecy and, even with all the data available, inference must be uncertain. However, such uncertainty can be reduced with information, and on a good many critically important military relations among states, the effects that dominate results are gross enough to show up in public data, provided these are gathered diligently and analyzed systematically and with care. Some very interesting things can be said, for example, about *n*th countries only on the basis of such empirical analysis, and on the basis of using a logical apparatus considerably more refined than a few bare distinctions like that between "vulnerable" and "invulnerable" force.

An opposed-systems analysis is at the level of generality appropriate to policy choice. This is, of course, not surprising, since that is how opposed-systems analysis got started. I have said very little about the relationship of policy to valid theory. In the field of international politics, an interest in policy hardly needs justification. Just about everyone in the field is interested in policy. I am using the word "interest" in both of its meanings: they are fascinated by it and have a stake in it. I believe that the likelihood of useful analyses for the choice of ends—and of means for achieving such ends—is enhanced if the analyses are systematic and explicit about objectives as well as instrumentalities; for one thing, they are then open both to self-criticism and to public examination. How analyses performed to aid policy might affect policy is a subject that has received extended comment. I would like to close with a speculation on the theoretical potential of policy designs.

There is, of course, an old academic snobbery about applied science in general. Applied science is distinctly lower-class. Such snobbery affects the social sciences, too. It is clear that work on policy needs theory. The fact that this can be a two-way street, while sometimes recognized in the natural sciences, seems much less frequently, if at all, to be recognized in the social sciences. It is familiar to historians of science that, in the words of the philosophical biologist, L. J. Henderson, thermodynamics owes more to the steam engine than the steam engine owes to thermodynamics. This is evident in the work of Sadi Carnot. It seems plausible to me that something of the same sort might happen in social science. It may be that well-evidenced generalizations will be easier to come by where they concern or stem from alternative designed operations and social structures—especially where these structures involve complex interdependencies of men and machines—than where they stem from the haphazard reports of the workings of unpremeditated institutions that have grown mostly without intent. In the latter case, research men are sometimes reduced to correlating each time-series so gathered with every other time-series in their possession. Though designed social structures or policy alternatives are normally quite complex in the field of political-military affairs, they may be rather better understood or more accessible to understanding than the unpremeditated complexities normally dealt with in the social sciences. On the other hand, they may be more interesting because they are complex and have more direct social relevance than small-group experiments. While such experiments are, of course, the work of design, and may be of great interest, it is sometimes rather hard to make the inferential jump from the small experimental group to the large social or political groupings that concern us.

There is no single best path through the tangle of international politics to basic theory. One useful trail may lead through the analysis and design of complex systems that are viable in a world of partially hostile and independent states.

**ENDNOTES - Wohlstetter - Theory and Opposed-Systems Design**

*Note: The original version of this essay contains in-text citations and a list of works cited at the end. In this version, in-text citations were converted into endnotes. Text bounded by square brackets generally indicates such a conversion.*

1. [Burton Marshall, “Waiting for the Curtain,” *SAIS Review*, Vol. 10, No. 4, Summer 1966, p. 21.]

2. The term “essentially general” is adapted from C. G. Hempel. [Hempel, *Aspects of Scientific Explanation*, New York, NY: Free Press, 1965, pp. 338*ff*.] An elaboration of this initial statement is made later in the section entitled “Specifics and the General: Imaginary Gardens with Real Toads.” For those familiar with the symbolism of mathematical logic, a partial and summary statement can be made at this point: essentially general statements include, besides those containing no individual names and only universally bound variables, some that may refer to individual objects, as Kepler’s laws refer to the sun and its planets: they may for example, be of the form “( x )Rxa.” Or they may use existential quantifiers such as the word “some” or the symbol “( ∃x)”: they may be of the form “(x) (∃y)Rxy.” But they also irreducibly involve universal quantifiers like the words “all” or “every” or the symbol “( x).”

3. See [Albert J. Wohlstetter, Fred S. Hoffman, Robert J. Lutz, and Henry S. Rowen, *The Selection of Strategic Air Bases*, R-244-S, special staff report, Santa Monica, CA: RAND Corporation, March 1, 1953, TOP SECRET, declassified on July 1, 1963, available from *www.albertwohlstetter.com/writings/19530301-AW-EtAl-R244S.pdf*]; and [Albert J. Wohlstetter, Fred S. Hoffman, Henry S. Rowen, and Robert J. Lutz, *The Selection and Use of Strategic Air Bases*, R-266, final report, Santa Monica, CA: RAND Corporation, April 1954, TOP SECRET, declassified circa 1961, available from *www.rand.org/pubs/reports/R0266/*].

4. [Albert J. Wohlstetter, Fred S. Hoffman, and Henry S. Rowen, *Protecting U.S. Power to Strike Back in the 1950s and 1960s,* R-290, staff report, Santa Monica, CA: RAND Corporation, September 1, 1956, TOP SECRET, declassified circa 1960s, availabl e from *www.albertwohlstetter.com/writings/19560901-AW-EtAl-R290.pdf*]

5. [Albert J. Wohlstetter and Fred S. Hoffman, *Defending a Strategic Force After 1960: With Notes on the Need by Both Sides for Accurate Bomb Delivery, Particularly for the Big Bombs*, D-2270, Santa Monica, CA: RAND Corporation, February 1, 1954, available from *www.albertwohlstetter.com/writings/19540201-AW-FH2270.pdf*.]

6.[F. W. Lanchester, *Aircraft in Wartime: The Dawn of the Fourth Arm,* London, UK: Constable, 1916.]

7. [R. E. Bach, L. Dolansky, and H. L. Stubbs, "A Loss Minimizing Extension of the Lanchester Theory of Combat," *Sc. Rpt.*, No. 3, Contract AF 19 (604)-4573, AD 235019, AFCRC-TN-60-168, Northeastern University, January 31, 1960]; [P. M. S. Blackett, "Operational Research," *Quarterly Journal of the British Association for the Advancement of Science*, No. 5, 1948, pp. 26-38]; [H. Brackney, "Dynamics of Military Combat," *Operations Research,* No. 7, 1959, pp. 30-44]; [R. H. Brown, *A Stochastic Analysis of Lanchester's Theory of Combat*, ORO-T-323, AD 82944, Operations Research Office, Johns Hopkins University, December 1955]; [S. J. Deitchman, "A Lanchester Model of Guerrilla Warfare, *Operations Research*, No. 10, 1962, pp. 818-827]; [J. H. Engel, "A Verification of Lanchester's Law," *Operations Research*, No. 2, 1954, pp. 163-171]; [G. A. Gamow and R. E. Zimmerman, *Mathematical Models for Ground Combat*, ORO-SP-11, Operations Research Office, Johns Hopkins University, April 1957]; [T. Ganelius, "Mathematical Treatment of Combat," *Artilleri Tidskrift*, No. 3, 1955, p. 84]; [B. O. Koopman, *Quantitative Aspect of Combat*, Office of Scientific Research and Development, AMP Note No. 6, August 1943]; [F. W. Lanchester, *Aircraft in Warfare: The Dawn of the Fourth Arm*, London, U.K.: Constable, 1916]; [P. M. Morse, "Mathematical Problems in Operations Research," *Bulletin of the American Mathematical Society*, No. 54, 1948, pp. 619-621]; [C. Tompkins, "Probabilistic Problems and Military Evaluation: An Example," *Logistics Papers*, No. 2, George Washington University, 1950]; [H. K. Weiss, *Requirements for a Theory of Combat*, BRL Memo Report No. 667, Project No. TB 3-0102, Ballistic Research Laboratory, Aberdeen Proving Ground, April 1953]; [Weiss, "Lanchester-type Models of Warfare," *Proceedings of the First International Conference, Operations Research*, December 1957, pp. 82-99]; [Weiss, "The Fiske Model of Warfare," *Operations Research*, No. 10, 1962, pp. 569-571]; and [D. Willard, "Lanchester as a Force in History: An Analysis of Land Battles of the Years 1618-1905," AD 29735L 63-1-5, Div. 18A, Bethesda, MD: Research Analysis Corporation, November 1962 (Limited distribution: Request approval of: Office, Chief of Research and Development, Department of the Army, Washington, DC, Attn: Research Planning Division)].

For a critical statement, see [Albert Wohlstetter and Richard B. Rainey, Jr., *Distant Wars and Far Out Estimates*, unpublished paper prepared for presentation at the American Political Science Association (APSA) meetings in New York, NY, on September 8, 1966; and at the Strategic Studies Conference, sponsored by MIT, Endicott House, Dedham, MA, September 9 to September 11, 1966, revised 1967], and especially [Charles Bernstein, "Reconsidering Systems Analysis of Theater Air," paper presented at NATO Symposium on Defense Resource Allocation, Paris, France, September 20-22, 1966].

8. [J. H. Engel, "A Verification of Lanchester's Law," *Operations Research*, No. 2, 1954, pp. 163-171.]

9. [Moshe Dayan, *Diary of the Sinai Campaign*, London, UK: George Weidenfeld and Nicolson, 1966, p. 5.]

10. [*The Military Balance, 1966-1967*, London, UK: Institute of Strategic Studies, 1967.] There is an error in the addition in the figures for Israeli aircraft.

11. [Albert Wohlstetter, *Strength, Interest and New Technologies*, opening address before *The Implications of Military Technology in the 1970s*, the Institute for Strategic Studies' ninth annual conference, Elsinore, Denmark, September 28 to October 1, 1967, D(L)-16624-PR, Santa Monica, CA: RAND Corporation, January 24, 1968, available from *www.rand.org/about/history/wohlstetter/DL16624/DL16624.html.* The address was also published as Wohlstetter, *Strength, Interest and New Technologies*, in *The Implications of Military Technology in the 1970s*, Adelphi Papers No. 46, London, UK: Institute for Strategic Studies, March 1968.]

12. [Charles J. Hitch, "On the Choice of Objectives in Systems Studies," P-1955, Santa Monica, CA: RAND Corporation, March 1960, p. 2.]

13. See [Albert Wohlstetter, "The Non-Strategic and Non-Existent," in Kathleen Archibald, ed., *Strategic Interaction and Conflict: Original Papers and Discussion*, Berkeley, CA: Institute of International Studies, University of California, Berkeley, 1966, pp. 107-126., esp. pp. 112-13]; also [Wohlstetter, "Defense Decisions: Design Versus Analysis," IFORS Conference, Aix-en-Provence, 1960]; [Wohlstetter, "Analysis and Design of Conflict Systems," in E. S. Quade, ed., *Analysis for Military Decisions*, Chicago, IL: Rand

McNally & Co., 1964, pp. 103-148]; [E.S. Quade, "Introduction" and "The Selection and Use of Strategic Air Bases: A Case History," in Quade, ed., *Analysis for Military Decisions,* pp. 2-12, 24-64.]; [Alain C. Enthoven, "Systems Analysis and the Navy," *Naval Review*, Annapolis, MD: United States Naval Institute, 1964, pp. 98-117]; [Hitch, "On the Choice of Objectives in System Studies"]; [Henry S. Rowen, "Improving Decision-Making in Government," lecture for the Budget Bureau's 1965 summer seminar on systems analysis and program evaluation]; and [Fred S. Hoffman, "PPB, Its Place in the Sun," speech at the American Bankers Association, December 1967].

14. The point of view expressed here is developed at considerably greater length in [Wohlstetter, "Analysis and Design of Conflict Systems"].

15. Aaron Wildavsky, no radical critic, suggests the method *is* tied to the existing political structure. See [Wildavsky, "The Political Economy of Efficiency," *The Public Interest*, No. 8, Summer 1967, pp 30-48].

16. One might distinguish the theory or design of *strongly* opposed systems from that of *weakly* opposed systems. In this paper I have dealt mainly with the kind of potential opposition familiar among nation states. While such opposition is only partial it may lead to actual combat and this possibility is a classic defining trait of the anarchic system of nation states. (See the section, "Potential Wars.") Where internal dissent from the institutions of the nation is large and powerful enough to interfere in essential ways with their operation or possibly to effect a change in them by force, much the same sort of analysis we have described as opposed systems design is relevant both for the dissenter and for the authorities. We might better call the subject matter of such theory and design, in both the international and national cases, "strongly opposed systems." But in the national case there are some useful analogies even where governmental or factional policies are more weakly opposed, where they do not threaten internal war. Even then policy in some areas may be best formulated with possible counter moves in mind. So, for example, policies aimed at desegregating schools (by bussing nonwhites to white schools or the reverse, or reducing the grade span of the city schools so as to widen and make more varied the ethnic catchment

area from which pupils for any given school are drawn, and so on) may be met by a movement of the more privileged whites into private schools or out to the suburbs. Policies may be deliberately chosen so as to minimize the effects of such countermeasures. In designing systems that are "weakly" opposed, the opposition may be dealt with by methods that stress conciliation, compromise, and bargaining even more than in international affairs. But of course the difference is one of degree, and in the international case, too, compromise and conciliation are important modes of resolving differences.

17. A much more extended discussion is contained in [Wohlstetter, "Analysis and Design of Conflict Systems"].

18. For an extended analysis of the B-36 history, see [*ibid.*] .

19. For a detailed account, see [William Emerson, "Doctrine and Dogma," *Army*, June 1963, pp. 818-827]. See also my essay in the Kaplan volume referred to under Falk (to be published). [Albert Wohlstetter, "Theory and Opposed Systems Design," in Morton A. Kaplan, ed., *New Approaches to International Relations*, New York: St. Martin's, 1968, pp. 19-53.]

20. [R. N. Rosecrance and J. E. Mueller, "Decision-Making and the Quantitative Analysis of International Politics," *London Year Book of World Affairs*, 1967.]

21. See [Richard Falk, "New Approaches to the Study of International Law," in Kaplan, ed., *New Approaches to International Relations*, p. 27].

22. See, for example, the cogent critiques by [Ernst B. Haas, "The Balance of Power: Prescription, Concept or Propaganda?" *World Politics*, No. 5, July 1953, pp. 442-477]; [Inis Claude, *Power and International Relations*, New York, NY: Random House, 1962, esp. chs. 2 and 3]; and [A. F. Pollard, "The Balance of Power," *Journal of the British Institute of International Affairs*, No. 2, March 1923, pp; 51-64].

23. [Wohlstetter and Rainey, *Distant Wars and Far Out Estimates.*]

24. See, for example, *The Federalist No. 6,* by Alexander Hamilton: "To look for a continuation of harmony between a number of unconnected sovereigns, situated in the same neighborhood, would be to disregard the uniform course of human events." [*The Federalist,* Cleveland, OH: Meridian, Books, 1961, p. 28.]

25. [Nicholas John Spykman, *America's Strategy in World Politics*, New York, NY: Harcourt, Brace and World, 1942, p. 448.]

26. [George Kennan, *Russia and the West under Lenin and Stalin*, New York, NY: Mentor Books, 1962, p. 261.]

27. [Kenneth E. Boulding, *Conflict and Defense*, New York, NY: Harper and Row, 1962.]

28. [*Ibid.*, p. 241.]

29. [*Ibid.*, p. 231.]

30. See [*ibid.,* p. 272]. For a more or less standard sociological example, see [Hornell Hart, "Social Science and the Atomic Crisis, *Journal of Social Issues*, Supplement Series No. 2, April 1949]. Compare also [Bruce M. Russett, *Trends in World Politics*, New York, NY: Macmillan, 1965, esp. ch. 1]. Mr. Russett, however, avoids the extrapolations into the future characteristic of Hart and others.

31. [Boulding, p. 244.]

32. [Boulding, esp. ch. 2]; [Dean C. Pruitt, "Definition of the Situation as a Determinant of International Action," in Herbert C. Kelman, ed., *International Behavior*, New York, NY: Holt, Rinehart and Winston, 1965, pp. 422*ff*]; [Anatol Rapoport, *Fights, Games and Debates*, Ann Arbor, MI: University of Michigan Press, 1960]; and [Rapoport, "Lewis Richardson's Mathematical Theory of War," *Journal of Conflict Resolution*, Vol. 1, No. 3, September 1957, pp. 249-299].

33. [Boulding, p. 34]; [Pruitt, p. 423].