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THE NEW GREAT DEBATE

Traditionalism vs. Science in International Relations

By MORTON A. KAPLAN

OVER the past decade traditionalists have launched a series of attacks on scientific approaches to international politics. Most of the arguments employed against the scientific approach stem from those used earlier by E. H. Carr in *The Twenty Years' Crisis*.[1] The general arguments that have been employed include these among others: that politics involves purpose in a way that physical science does not; that scientific knowledge is applicable to facts, but understanding, wisdom, or intuition are required for areas where human purpose is involved; that those pursuing scientific models tend to mistake their models for reality; that scientific method requires high precision and measurement and therefore is incapable of coping with the most important elements of international politics; and that the practitioners of scientific method can never be sure that they have not left something out of their model.

I

According to Carr, "The laboratory worker engaged in investigating the causes of cancer may have been originally inspired by the purpose of eradicating the disease. But this purpose is, in the strictest sense, irrelevant to the investigation and separable from it. His conclusion can be nothing more than a true report on fact. It cannot help to make the facts other than they are; for the facts exist independently of what anyone thinks about them. In the political sciences, which are concerned with human behavior, there are no such facts. The investigator is inspired by the desire to cure some ill of the body politic. Among the causes of the trouble, he diagnoses the fact that human beings normally react to certain conditions in a certain way. But this is not a fact comparable with the fact that human bodies react in a certain way to certain drugs. It is a fact which may be changed by the desire to change it; and this desire, already present in the mind of the investigator, may be extended, as the result of his investigation, to a sufficient number of other human beings to make it effective."[2]

The two cases cited by Carr are different, but Carr has mistaken the nature of the difference. Carr's inapt distinction results from a

[1] 2nd ed. (London 1956). [2] *Ibid.*, 3-4.

prior failure to distinguish between the facts he initially holds constant (system) and the facts he allows to change (parameters). It is a fact that rattlesnake venom injected into the blood system will normally kill a person. It is also a fact that the proper antidote administered in time will negate the destructive action of the venom. The cancer worker also desires to change some facts, namely, those relating to the development of cancer. He does this by changing, perhaps by drugs or perhaps by irradiation, the system in which the cancerous cells are embedded. The politician who desires to change the world must also change the state of a system—in this case, the political system. He may do this by the use of force, by the allocation of resources, or by means of verbal persuasion. The system may undergo radical change. Its characteristic operation may be different from what it was before the new inputs, including information, were embedded in the system. But then a similar kind of change in characteristic behavior occurs when, for instance, opium is injected into the human physiological system or flowers are hybridized (step functions)[3].

Systems embodying purpose cannot be studied by the methods ordinarily used by physicists. Suitably defined, however, purpose need not distinguish the physical from the human with respect to the problems raised by Carr. Consider an automatic pilot in an airplane. If the plane moves from level, the automatic pilot reverses the direction of change. Reverse the wires from the pilot to the ailerons and elevators so that now the automatic pilot will introduce positive feedback into the cycle of movement and throw the plane into a spin if it deviates from level. Now reconstruct the automatic pilot system so that it becomes what Ashby calls an ultrastable system.[4] Move the plane from level. The pilot, with its wires reversed, will increase departure from level. The sensors of the pilot system will detect this consequence of the operation and adjust by reversing procedures. Although the operation of the automatic pilot in this case differs from human purpose in two important respects—lack of consciousness and simplicity of the system—it has much in common with it. We can even carry the analogy one step farther. We can think of a tic-tac-toe-playing computer, attached to an information retrieval system, which plays against a human player; receives information from spies about the moves he will make, or, alternatively, extrapolates from his past moves;

[3] W. Ross Ashby, *Design for a Brain* (New York 1952), 80 ff.
[4] *Ibid.*, 99.

and attempts to anticipate the moves of the human player and to frustrate them by the appropriate countermoves.

All of the systems we have described can be investigated by scientific methods. When one says this, however, one does not necessarily mean that these systems can be investigated by the procedures of physics. The equalities of physics lack explanatory power to account for the behavior of homeostatic or ultrastable systems. Specific explanatory theories must be developed for particular systems. And, in the case of the game-playing computer, one cannot use the calculus but must use some variation of the type of set theory used in game analysis. Thus, though the theories, explanations, and tools used may differ from those of the physicist, they are part of the general arsenal of science.

There are a number of important differences between mechanical systems and ultrastable systems which have not been discussed, and which cannot be discussed, for lack of space. Human psychological systems and human social and political systems differ in still other important ways from Ashby's ultrastable systems and from each other. Our object here is not to carry out a critical examination of these differences but to show the extent to which traditionalist arguments confuse the issues.

II

If the traditionalist has confused the distinction between the facts of physical science and the purposes of politics, then it is clear that he must also have confused the relationship between intuition and scientific knowledge.

There is a large literature on the subject of intuition in physical science and mathematics. Great discoveries, when they do not occur accidentally or as a consequence of trial-and-error procedures, are the product of scientific intuition. If the best statesmen are usually those with the best intuitions and judgments concerning politics, so the best scientists are often those whose scientific judgment or intuition is the best. There are cases in which scientists have been repeatedly right although the reasons they have given to support their theories have turned out to be faulty. The reasons for the superiority of intuition are not hard to find. The brain is more sophisticated and complicated than any computer we can construct. It can scan for variations in ways for which directions cannot yet be coded; it can reason below the level of consciousness in ways that neither numbers nor verbal logic can articulate. As John von Neumann pointed out in his posthumously published Silliman lectures, even if we used in its construction the smallest components available, and even if we knew (as we in fact do not) how to link the system up, it would require a housing 10^8 or 10^9 as large as the brain casing (or roughly as large as the Empire State Building) to house an analogue to the brain.[5] Even though miniaturization has made profound strides since von Neumann's death, this gives some indication of the scope of the problem.

The skill of a tea taster gives one indication of the capacity of the human brain to scan for "fits." Computer recognition is hopelessly primitive by comparison. Similarly, the human capacity to find parallels in history defies our ability to code or to articulate. The brain's coding apparently differs from that of mathematics and verbal logic.[6] Its code is apparently less precise but more reliable. And it apparently, along with the scanning capacity, plays a major role in intuition.

The humanist who wants to substitute in human events a verbal process called reason or understanding for a verbal and/or mathematical process called science has confused intuition with the articulation of communicable knowledge. The source of the confusion may possibly lie in the Aristotelian distinction between science and art. Science, according to Aristotle, must be certain, for it derives true conclusions from necessary—not merely true—premises.[7] Thus hypothetical knowledge cannot be scientific, for its premises, even if true, are not known to be necessary. One cannot intuit the necessity of the premises in human events; therefore art rather than science governs knowledge of human events. Modern science, however, insists upon the hypothetical character of all empirical knowledge. The test for communicable knowledge depends on replicability even if only in principle. Thus there is no distinction between the physical and human with respect to the need for confirmation and communication. There is a distinction between subject matters with respect to the degree to which theoretical knowledge is possible and to which warranted belief or precision is possible.

Science requires an articulated secondary language that permits reasonable precision and replicability. Unless scientific procedures are followed, to the extent the subject matter permits, intuitions cannot be falsified and science cannot grow. Even intuition requires the techniques of science to prepare the base on which new intuitions

[5] *The Computer and the Brain* (New Haven 1958), 50.
[6] *Ibid.*, 90-92.
[7] *Organon: Posterior Analytics, Topica*, Loeb Classical Library (London 1960), 33-55.

develop. If Einstein's intuition produced both the special and general theories of relativity, that intuition operated within a framework of previous discovery and research—e.g., non-Euclidean geometries and Lorentz transformations (based on the Michelson-Morley experiment) —that created an order within which the procedures of his unconscious mind could generate the intuitions that led to relativity theory. Newton could not have had Einstein's intuitions.

III

There is one other way in which traditionalists sometimes assert that human purpose can be apprehended by methods different from those used by the sciences. Motives, they say, are subjective and can be intuited by introspection. The purposes of past civilizations or eras can be seen into by means of introspective, subjective wisdom. We are long past the period when psychological behaviorists insisted upon the exclusion of the concept of consciousness from the realm of psychological discourse. There are no doubt differences between subjective awareness of one's own purpose, at least, and subjective awareness of external phenomena. Yet, however much some of us may reject the more speculative aspects of Freudian psychoanalysis, we also see quite clearly that a number of human actions depend upon unconscious motivations that are often inconsistent with the conscious motivations. It is a rare man who is willing to assert that his own actions have never surprised him or that he has never discovered motivations other than those he thought he possessed. Although these unconscious motivations are sometimes confirmed by bringing them to consciousness, they are more often confirmed by careful observation and analysis of the behavior patterns of people and of attempts to explain these behaviors. Even introspection, through the examination of behavior, often brings to subjective awareness a previously unperceived motivation. In any event, our certitude as to our motivations has long since been discarded as valid evidence of their actuality. The normal tools of careful and controlled scientific observation are invaluable in assessing hypotheses concerning motivation.[8]

[8] The phenomenon referred to has been discussed in a more corrigible sense by psychologists. Psychologists have discovered that the unconscious biases of investigators may determine the responses of those being investigated. The very fact that this has occurred, however, has been discovered by further scientific investigation in which controls have been added for the biases of the givers of the tests. Where the entire macrostructure of politics changes, controlled experiments in this exact sense cannot be carried on. The two situations are different in practice rather than in principle; however, it is this last phenomenon to which the discussion above has reference.

Group, social, or political behavior cannot, in any event, be derived directly from individual motivation. There are too many group invariances that themselves determine the pattern of individual motivations. Americans do differ from Frenchmen who in turn differ from Chinese. These differences are not merely biological. The behavior of the members of the Joint Chiefs of Staff during the Korean War was different from the behavior of the field commanders; most of these differences depended more upon social roles and information flows than upon personality factors. Thus, even were the assertions concerning individual motivation and purpose correct, no reliable inferences could be drawn concerning the analysis of group behavior.

Traditional opponents of scientific method have one other argument against the analogies between ultrastable but nonhuman systems and human social systems. They argue that ultrastable systems such as the Ashby automatic pilot are constructed by men and therefore have the purposes of men built into them. The logic here, however, is faulty. An external observer could detect the use and purpose of the automatic pilot by observing its effect upon the behavior of the airplane. He would not require any knowledge of or insight into the purposes of the designer. Alternatively, if the biological revolution permitted us to synthesize the ovum and sperm cell, to fertilize the egg, and to grow the fertilized egg in an artificial culture, we could produce an equivalent of a human being. This may or may not be beyond the ingenuity of men, but in principle it illustrates the point. The appropriate distinction is not between the unconscious designs of nature and accident on one hand and the conscious and purposeful designs of men on the other but between the kinds of systems to which the generalizations are applied. The artificially created human would differ from Ashby's ultrastable automatic pilot in exactly the same ways as do natural human beings. If these differences were overlooked, incorrect conclusions would be drawn and inapplicable generalizations applied. If the likeness of the artificially created human to natural humans were overlooked, the moral consequences would be monstrous. But it is the traditionalist rather than the scientist who would be more likely to make this mistake.

IV

The distinction between determinism and free will that is offered by Carr can be refuted succinctly, for the elements of the refutation have appeared already in the previous sections. There is surely a distinction between systems capable of anticipating the actions of others and trying to trick them and systems, such as that of inanimate nature, which can never (however much we may mistake their character) attempt to trick us. The deterministic models of physics obviously are inappropriate to the first kind of systems, but there are scientific methods for studying such systems. This does not imply that science possesses the solutions for all problems of this kind; surely it does not. The point here is only that there are formalized scientific procedures for dealing with these problems and that where these procedures are not successful, it is not merely because purpose is involved. The problem may be too complex for any procedure we have developed, or even for any that we can develop. Or it may be that no solution exists, e.g., some bargaining cases in which rationality cannot be defined and the social and political constraints do not "fix" behavior either. Marginal cases of this kind do arise. To the extent that they do, the procedures of science can provide neither explanation nor prediction. Many of the major problems of macroscopic international politics, however, do appear to be manageable. In any event, the question of manageability can be decided only on the basis of practice and not on the basis of faulty philosophical argumentation.

V

The traditionalist asserts that those who aspire to a "science" of politics insist upon precision, rigor, quantification, and general theory. The traditionalist further claims that the complexity of international politics is such that these goals cannot be attained nor the important questions of international politics be investigated by these means. Whether the charge is correct cannot be answered in general. The appropriate degree of theory and of precision depends both on the state of the discipline and on the subject matter.[9] Since I am most

[9] The assertion that my *System and Process in International Politics* (New York 1957) attempts a completely deductive theory has been made both by Hedley Bull and by Stanley Hoffmann. Hoffmann apparently quotes *System and Process* to this effect ("The Long Road to Theory," *World Politics*, XI [April 1959], 357). And Bull, apparently relying upon Hoffmann, then uses the admitted fact that not all assertions of the models are rigorously deduced as a disproof of the claims made for the models ("International Theory: The Case for a Classical Approach," *World Politics*, XVIII [April 1966], 366-67, 371-72). Yet the first page of the preface—the page from which Hoffmann takes his quotations—which contains the paragraph describing what an ideal deductive theory would look like, includes as the last line of that paragraph the following sentence: "If 'theory' is interpreted in this strict sense, this book does not contain a theory." It then goes on to say, "If some of the requirements for a theory are loosened; if systematic completeness is not required; if proof of logical consistency is not required; if unambiguous interpretation of terms and laboratory methods of confirma-

familiar with my own work, I should like to consider it first in some detail and then to examine a number of other scientific approaches criticized by traditionalists. I shall try to show that fundamentally different enterprises are involved and that blanket analyses obscure more than they clarify.

The conception that underlies *System and Process* is fairly simple. If the number, type, and behavior of nations differ over time, and if their military capabilities, their economic assets, and their information also vary over time, then there is some likely interconnection between these elements such that different structural and behavioral systems can be discerned to operate in different periods of history. This conception may turn out to be incorrect, but it does not seem an unreasonable basis for an investigation of the subject matter. To conduct such an investigation requires systematic hypotheses concerning the nature of the connections of the variables. Only after these are made can past history be examined in a way that illuminates the hypotheses. Otherwise the investigator has no criteria on the basis of which he can pick and choose from among the infinite reservoir of facts available to him. These initial hypotheses indicate the areas of facts which have the greatest importance for this type of investigation; presumably if the hypotheses are wrong, this will become reasonably evident in the course of attempting to use them.

The models of *System and Process* provide a theoretical framework within which seemingly unconnected kinds of events can be related. A few examples of these can be given. For instance, it is asserted in the traditional literature that the framework of European international law is the product of a common civilization, culture, set of values, and personal ties. Our hypotheses indicate that the "balance of power" type of system is likely to motivate and reinforce the kinds of norms that were observed during the modern European "balance of power" period. If the traditionalist hypothesis is correct, then one would expect that international law would have been strongest in the earliest part of the modern European "balance of power" period, when, as a

tion are not required; then this book is, or at least contains, a theory. This theory may be viewed as an initial or introductory theory of international politics." This qualification is repeated in the conclusion (pp. 245-46): "A complete and systematic statement of these assumptions has not been offered. One reason for this gap lies in the belief of the author that international politics, and social science generally, is so poorly developed that the construction of a precise deductive system would be more constrictive and misleading than enlightening, that, at this stage of development, some ambiguity is a good thing." I did believe, however, that the ambiguity could be reduced and that more disciplined reasoning and scientific method could be introduced into the study of international politics. *That* was what *System and Process* tried to do.

consequence of a common Catholicism and interrelated dynasticism, the cultural factors making for uniformity of norms would have been strongest. If the systems model is correct, then one would instead expect the norms to develop over time as the actors learned how these norms reinforced their common interests. One would also expect on the basis of the systems model that a number of these norms would receive less reinforcement in a loose bipolar system. No systematic study of these hypotheses has yet been carried out. Peripheral results from comparative studies directed to other aspects of "balance of power" behavior, however, indicate the likelihood that the systems explanation will account for the historic evidence better than the traditionalist one. The early evidence indicates that the norms were weaker in the earlier phases of the period. Such results are not conclusive. We may find still other "balance of power" systems in which our initial expectations are falsified. This would then create a new problem for investigation. However, the systematic nature of the systems hypotheses would make this kind of comparative analysis easier by providing a framework within which questions could be generated and research carried on. It is perhaps no accident that the first set of comparative theories of international relations was developed within a systems framework and not within a traditionalistic framework.

An illustration of the way in which systems models may be used to connect or to explain seemingly discordant facts may also be offered. According to the systems model of the "balance of power" system, alliances will be short in duration, shifting as to membership, and wars will be limited in objectives. The reason offered for this is that the need to maintain the availability of potential alliance partners is greater than the need for the additional assets that would result from the destruction of the defeated foe. If one looks at Europe after 1870, however, one finds a set of relatively permanent alliances centered on France and Germany which produced a war that, according to the standards of the time, was relatively unlimited. The models, however, are closed in such a way that public opinion does not interfere with the rationality of external decision-making. The seizure of Alsace-Lorraine by Germany after the war of 1870, as Bismarck foresaw, produced in France a desire for revenge that, despite German attempts to buy France off, made it impossible for France and Germany to be alliance partners in any serious sense. For this reason, Germany considered a preventive war against France. That Germany and France became the hubs of opposing alliances therefore is consistent with the model if the parameter change is taken into account. Since neither France nor Germany viewed the other as a potential alliance partner, the motivation that served to limit war would not have been operative with respect to these two nations. Although this is surely not a complete—nor even a "proved"—explanation of the events leading to the First World War, it does establish a consistency between the predictions of the model suitably adjusted for a changed parameter and the actual course of events. Thus the systems model has some additional explanatory power even for some nonconforming events.[10] It may be possible to offer similar explanations for other parameter changes. One would not expect that this could be done with respect to problems of system change involving the transformation rules of the system. If this were possible, we should have a general theory of the system rather than a set of comparative theories. Although it cannot be demonstrated that a general theory is impossible, the reasons for its lack of likelihood have been stated by me elsewhere."[11]

In addition to empirical investigations, the systems theory of international politics calls for the use of models. The reason for this is quite simple. Even statesmen make statements about the relationship of states. From what assumptions are such statements derived? This is often unclear. Are they correctly derived? Only a much more systematic statement of the assumptions and of the conditions under which they are proposed to apply permits any kind of answer. Under what conditions do the generalizations apply, if at all? How much difference does it make to add one state or two states to a five-state system and under what conditions? Is Arthur Burns correct in asserting that five is the optimal number for security, with declining security both below and above that number,[12] or is Kaplan correct in believing that five is the minimal lower bound for security but that security increases as the number of states is increased up to some as-yet-undiscovered upper bound? How many deviant states can a system tolerate? What degree of deviance is tolerable? Can deviance be accommodated so that deviant states are forced to behave as if they were merely security-oriented? How will changes in weapons systems affect

[10] It was long known that certain poisons produced death. It was not known, however, how they did so. Eventually chemists learned that when certain poisons entered the blood stream, they combined with the oxygen in the blood and thereby deprived vital organs of the oxygen necessary for life. Although the end result of the poisoning was long known, the chemical explanation contributes to knowledge. Under some circumstances it has important utility. For instance, if one knows the mechanism involved, it may be easier to find the antidote.

[11] *System and Process*, xvii-xviii.

[12] Arthur Lee Burns, "From Balance to Deterrence: A Theoretical Analysis," *World Politics*, IX (July 1957), 494-529.

the problem of stability? What of geographic constraints? To what extent do internal decision-making organs, either by facilitating or impeding concentration on problems of external concern or by influencing the speed of reaction time, affect the stability of the system?

Some of these questions can be explored at a theoretical level in terms of the consistency and implications of the basic assumptions. Computer realizations are helpful to this end. The relevance of the questions for the real world can be explored by means of historical comparative studies. If the theoretical model is stable and the historical system is not, this is an indication that some factor not taken account of in the theory is operating. If both systems are stable, it is possible that this may be so for reasons other than those contained in the assumptions. Possible responses to this proposition may be obtained either through more thorough research into particular systems or by means of additional comparative studies that may permit discrimination of the cases. Elucidation of the constraining parameters would likely require a large series of comparative studies. The degree of confidence we place in our studies will never approach that which the physicist has in the study of mechanics (although other areas of physics may present problems as bad as those of politics); but without theoretical models we are unable even to make the discriminations open to us and to explore these questions to the same degree of depth.[13]

International systems theory is designed to investigate problems of macrosystem structure. It is not, for instance, easily adaptable to the investigation of microstructural problems of foreign policy. Techniques in this area would involve closer analogies with histology than with macrosystem analysis. This is an area in which extensive knowledge of a specific course of events, immense accumulations of detail, sensitivity and judgment in the selection of relevant factors, and intuitive ability of a high order are extremely important. We cannot easily use comparative evaluation, for the large number of variables involved in such events would not be even closely paralleled in other cases. In this sense, histology has an advantage over political science, for the histologist can at least examine generically similar material time and time again. Although elements of these problems can be subjected to scientific analysis, in many cases the use of intuitive judgment outweighs that of demonstrable knowledge. In these last cases, the conclusions can often be communicated, though usually in poorly articulated

[13] The problem of confirmation of systems models is explored in greater depth in Kaplan, "Some Problems of International Systems Research," in *International Political Communities* (New York 1966), 497-502.

form, but the means by which they were reached can be only badly misrepresented.

International systems theory, however, is only one of the scientific approaches to the subject matter of international politics. I hesitate to speak of the research of other scholars because I have not examined their work with the care required of a serious critic. Yet even superficial analysis would seem to indicate that the scientific approaches discussed together by Hedley Bull, for instance, have little in common.[14] They address themselves to different questions and use different methods. I shall try to indicate what some of these differences are—and my own attitude toward these other approaches—with the understanding that I do not consider myself an entirely competent judge.

Hedley Bull discusses Kaplan, Deutsch, Russett, Schelling, and various others as if they represented a sufficiently common position that similar criticisms would apply to all of them. Whereas I begin with a macrosystem analysis, however, Karl Deutsch proceeds with an inductive analysis based upon the quantification of the parameters of systems.[15] Whereas I study general system behavior, Deutsch studies the growth of community. Hedley Bull criticizes Karl Deutsch for counting all communications as if they were equal in some respect. Yet surely that is a most economical initial hypothesis. Unless Deutsch makes that assumption or a similar one, he can not discover whether such an item count will provide him with meaningful indicators for the growth of community.

In any event, it is rather discouraging to find Deutsch attacked because he does not differentiate messages according to criteria of importance. Deutsch developed his indices on the basis of a sophisticated set of hypotheses and after elaborate historical studies. If the indices prove not to be exceptionally useful, this will likely be uncovered by further empirical work. If further categorizations prove necessary—as they have, for instance, in assessing group differences in intelligence—empirical scientific work will no doubt establish this fact. If Haas is right that elite activity that produces institutions is more important than an increased flow of communications in establishing a pluralistic security community, the empirical evidence will likely in-

[14] Pp. 361-77.

[15] "Toward an Inventory of Basic Trends and Patterns in Comparative and International Politics," *American Political Science Review*, LIV (March 1960), 34-57. See also Deutsch and others, *Political Community and the North Atlantic Area* (Princeton 1957); Deutsch, *Nationalism and Social Communication* (New York 1953); and Deutsch, *Political Community at the International Level* (Garden City 1954).

dicate this also.[16] If differentiation of flows according to the kinds of systems they develop within—a systems orientation—is likely to make for finer discrimination, it is again the empirical scientific evidence and not abstract literary considerations that will establish this point.[17]

Russett uses still a different technique.[18] I believe that his fitting curves to data by means of quadratic equations is not suited to the data he uses. This, however, is true, if true at all, not on the basis of some general philosophical principle, but on the basis of a specific evaluation of the use of the technique in terms of the subject matter to which it is applied. I am also, for instance, skeptical of the techniques employed by Zaninovich in his *Empirical Theory of State Response*, with respect to the Sino-Soviet case.[19] Although I find his conclusions unexceptional—for instance, the conclusion that when two states are involved in a critical relationship, each will misperceive the intentions of the other—I do not find them particularly useful in the form in which they are applied. The phenomenon of mistaken perception is well known. As a mere phenomenon, it does not require further documentation. Nor in this abstract form does it add much to our understanding of the political process. It is not very useful for policy-makers either. It does not tell them what the misperceptions will be or the particular kinds of responses they will produce. Moreover, since most of the analysis is based upon the coding of public statements and editorials in the party newspapers, there is the additional danger that the public stance of the state will be misperceived by the investigator as its private one. Whether my judgment of the procedure is right or wrong, however, depends not upon the crude general propositions enunciated by the traditionalists but upon a specific analysis of the application of the methodology to a specific subject matter.

One may desire to raise questions about some of the simulations of international politics that are being carried on. Whether small group simulations reveal more about small groups simulating international relations than about the more complex pattern of international politics is, at the minimum, an open question. If simulation is a quite useful tool for generating hypotheses, it is likely much less useful for confirming them. Here the reader must be warned: I am not here offering

[16] Ernst Haas, "The Challenge of Regionalism," *International Organization*, XII (Autumn 1958), 440-58.

[17] For a responsible discussion of Deutsch's categories and techniques, see Ralph H. Retzlaff, "The Use of Aggregate Data in Comparative Political Analysis," *Journal of Politics*, XXVII (November 1965), 797-817.

[18] Bruce M. Russett, *Trends in World Politics* (New York 1965).

[19] Martin George Zaninovich, *An Empirical Theory of State Response: The Sino-Soviet Case* (Stanford 1964), mimeographed.

an analysis of whether this is the case or not, and may merely be asserting my own prejudice.

Much of the criticism of the work of Thomas Schelling seems misguided. It is generally agreed that there are many interesting insights in Schelling's work;[20] but the traditionalists, e.g., Hedley Bull, sometimes object that the insights are not derived from game-theoretic methods. This argument is misleading; Schelling rarely uses mathematical game-theoretic methods. Most of his analysis is sociological; that is the root of his assertion that he desires to reorient game theory. On the other hand, although his insights in the usual case are not rigorously derived from game theory, it must be admitted that insights of this kind did not seriously begin to enter the literature until the questions posed by game-theoretic analysis directed attention to them.

Schelling is so identified with game theory by the traditionalists that he is credited with contributions he has not claimed. According to Hoffmann, "Until now game theory has . . . weaknesses that Schelling reviews. The main flaw is that game theory has dealt *only* [italics added] with zero-sum games. . . ."[21] It is not entirely unexpected that a political scientist would commit a technical error in the area of game theory. It is surprising, however, that one who presumes to evaluate the utility of that theory would make this elementary a mistake. The point is covered in every treatise on the subject (and by Schelling), and there is a large literature on the subject. The mixed-motive game is one of the basic classifications of mathematical game theory. However, Hoffmann does not rest there. He continues, "Therefore, game theory applies only to a marginal and paradoxical case: pure conflict with limited stakes, i.e., the characteristic conflicts of moderate, balance-of-power, international systems."[22] Unfortunately, the "balance of power" case is neither paradoxical nor zero-sum. Moreover, although there are many mixed-motive games for which there are appropriate game-theoretic models, the "balance of power" case is not one of them. Game theory has only limited applicability to most problems of international politics, but we are hardly likely to learn from the traditionalists what these limits are and why they exist.

Although traditionalists quite often have accused those using scientific method of neglecting Aristotle's dictum to use those methods appropriate to the subject matter, I would contend that it is the user

[20] Thomas C. Schelling, *The Strategy of Conflict* (Cambridge, Mass., 1960).
[21] Stanley Hoffmann, *The State of War* (New York 1965), 205.
[22] *Ibid.*, 206.

of scientific method who has more often observed the dictum. This is illustrated by the fact that so intelligent a student of politics as Hedley Bull, who openly recognizes the danger that he might be talking about discordant things, nonetheless falls into what I would call the trap of traditionalism: the use of overparticularization and unrelated generalization. Thus Bull lists highly disparate methods and subjects with minimal discussion and inadequate or nonexistent classification and applies to them extremely general criticisms. Such broad and universal generalizations are extremely difficult, if not impossible, to falsify. Who would deny that the complexity of the subject matter places constraints on what can be said? But different subject matters and different degrees of complexity require different tools of analysis and different procedures. The traditionalist, however, as in the case of Bull, does not discuss how or why the complexity of a specific subject impedes what kind of generalization, or how and in what ways generalizations should be limited. The traditional literature in international relations, even when it is directly concerned with the subject matter, is of much the same order: a great mass of detail to which absurdly broad and often unfalsifiable generalizations are applied. Thus traditional "balance of power" theory is asserted to apply regardless of the number and kinds of states, variations in motivation, kinds of weapons systems, and so forth. Remarkably the same generalizations are asserted to apply not merely to the macrostructure of international politics but to the individual decisions of foreign policy. The generalizations are applied indiscriminately over enormous stretches of time and space. They are sufficiently loosely stated so that almost no event can be inconsistent with them.

And the vaunted sensitivity to history that the traditionalists claim—and that they deny to the modern scientific approaches—is difficult to find. Those traditionalists who have done a significant amount of historical research—and they are the exceptions—confine themselves largely to problems of diplomatic history that are unrelated to their generalizations about international politics, as in the case of Martin Wight, or to more specialized problems that are idiosyncratic. This is not an accident but is a direct product of the lack of articulated theoretical structure in the traditionalist approach. It is ironic that the traditionalists are so sure that they alone are concerned with subject matter that they are unaware of the extent to which those applying the newer approaches are using history as a laboratory for their researches. This development is unprecedented in the discipline and is a direct product of the concern of those using scientific approaches for developing disciplined and articulated theories and propositions that can be investigated empirically.

If those writers of the newer persuasion sometimes seem to ignore the traditional literature, it may not be entirely without good reason. Yet ignoring it is a mistake. There are honorable exceptions among the traditionalists, such as Raymond Aron, whose remarkable writings are surely useful to political scientists and whose methodology may not be quite so far removed from the newer scientific approaches as some traditionalists like to believe. Hedley Bull, one of the more vociferous critics of the newer approaches, has himself contributed a solid study of arms control to the literature.

VI

The traditionalist seems to feel that scientific models are inapt for a political world in which surprises may occur. He seems to feel that scientific theories must achieve generality and completeness or lack rigor. This seems more like a seventeenth-century view of science than like a modern view.

Physical science presents analogies to the surprises that stem from parameter changes in social or political systems. One of these is the phenomenon of superconductivity under conditions of extreme temperature and pressure. The phenomena associated with superconductivity had not been predicted by the then current physical theories. Only after experimentation with extreme temperatures and pressures were the phenomena noticed. And only then did it become necessary to explain them. Whether a highly general theory comprehending all novel phenomena, of which superconductivity is merely an example, can be developed by physical theory is still open to question. For reasons already evident, such a general theory would be even more questionable in the area of international politics. Were someone to suggest to a physicist that the discovery of novel phenomena such as superconductivity which had not been predicted by previous theory established either the lack of rigor of previous theory or the inappropriateness of the methodology employed, the argument would be dismissed.

VII

Another major charge made by the traditionalist against the newer methods is that since they use models, their practitioners are likely to mistake the models for reality. If the causal connection were not insisted on, I would not lightly deny the charge. There is a human tendency to reification. Surely the psychologists, sociologists, and anthropologists—and even the physicists, who know very little about politics—have a tendency to apply very simplified assumptions to very complex events. If, however, the traditionalist were to examine the propositions of the psychologists, for instance, he would find them no different from empirical generalizations—a category he likes. When a psychologist talks of projection or of a mirror image he is not, in the usual case, deriving these generalizations from an integrated theory, but is simply asserting an empirical generalization explicitly. The trouble with a generalization of this kind, apart from its general inapplicability, is that no context for its application is specified. Thus, as in the case of traditionalist arguments, it can be applied safely, for, in the form offered, it can never really be falsified.

On the other hand, it is natural to expect sophistication with respect to models from one who explicitly uses them. Only someone who has worked with models and the methodology of models knows how sensitive at least some models are to parameter adjustments. Thus a builder of models does not think of them as generally applicable. They are applicable only within a specified context; and it is extremely important to determine whether that context in fact exists. Moreover, the person who has worked with models usually has gone through the difficult task of trying to associate the parameters of the model with the real world. No one who has attempted this is likely to take it lightly.

I would argue that it is rather the traditionalist, whose assumptions are implicit rather than explicit and whose statements are made usually without reference to context, who is more likely to mistake his model for reality. Of course, even traditionalists are not likely to be as incautious as the historian Webster, who asserted that Castlereagh inherited his phlegmatic disposition from his mother who died when he was one year old. Yet the traditional literature of diplomatic history and international politics is filled with implicit assumptions as to motivation, interrelationships between variables, and so forth, that are implicit rather than specified, and the limits of application of which are never asserted. Even so careful and intelligent a traditionalist as George Kennan has made assertions about the likely effectiveness of United States aid in encouraging diversity and pluralism within the Soviet bloc which hardly seem to be sustained by the evidence.[23] Kennan did not explicitly articulate his model. He no doubt assumed that the provision of American aid provided the Polish

[23] "Polycentrism and Western Policy," *Foreign Affairs*, XLII (January 1964), 178.

government with an alternative to Soviet pressure. I would argue that had Kennan explicitly articulated his model, he might more likely have considered variables not included in his implicit model. Had he done so, he might have considered the possibility that the Polish government could argue to the Polish citizens that if the United States gave aid to Poland it must be a sign that the Polish regime was an acceptable regime. Therefore it would be unwise for the Polish citizen to oppose that regime or to expect even psychological aid from the United States in opposition. He also might have considered the hypothesis that the Polish leaders, as good Communists, and as a consequence of accepting American aid, might find it important to reassert at least some elements of Communist doctrine more strongly either to reassure themselves or to assure elements within the Polish Communist party whose support they needed that the leadership was not becoming a stooge for United States imperialism.

The probability that traditionalists will mistake their models for reality is further exemplified by Hedley Bull's criticisms of the new scientific approaches. Bull is so confident, on the basis of his premises, that those following the scientific method will engage largely in methodology both in their research and in their teaching, graduate and undergraduate, that he ignores the abundant evidence to the contrary. He himself admits that the other traditionalist critics of the new methods do not have adequate knowledge of these methods; yet he somehow fails to draw the inference from his own evidence that these critics have mistaken their implicit models for reality.

The traditional techniques with their inarticulated suppositions, their lack of specification of boundaries, and their almost necessary shifting of premises create a much greater danger that their implicit assumptions will automatically be applied to reality and a much greater sense of complacency than do scientific methods. I have no desire to be invidious, but, just as the traditionalists find it legitimate to characterize what they believe to be the inadequacies of the newer approaches, so it is equally legitimate to relate the defects of traditionalism to their sources. Bull, for instance, points out that English political science, as contrasted with American political science, remains committed to traditionalism. It is surely no secret that English political science is somewhat less than distinguished.

VIII

The traditionalists talk as if the newer methods have excluded philosophy as a tool for the analysis of international politics. Unfortunately few of them—again Raymond Aron is a conspicuous exception—have demonstrated any disciplined knowledge of philosophy; and many of them use the word as if it were a synonym for undisciplined speculation. There are many profound questions that in some senses are genuinely philosophical; the systems approach, among others, is related to a number of philosophical assumptions. The relationship between these philosophical assumptions and the validity of empirical theories is more complicated. It is entirely possible for an erroneous philosophy to furnish the ideas from which a valid empirical theory is derived. And it is dubious that the relationship between philosophical position and empirical theory is so direct—in either traditional or scientific approaches—that the arguments between or within competing approaches or theories can be settled by philosophical argument. There are, moreover, some important mistakes that ought to be avoided. Political theory ought not to be called philosophy merely because it is formulated by a man who is otherwise a philosopher unless the ideas have a genuine philosophical grounding. If the ideas are merely empirical propositions, as in the case of most philosophical statements used by traditionalists, they stand on the same footing as other empirical propositions. There is hardly much point in quoting one of the philosophers unless one understands him and can apply him correctly. I remember listening to a lecture by a well-known scholar, one cited by Bull as a good example of the traditionalist approach, who attempted to disprove Hegel's philosophy of history by showing that there were accidents in history. He was obviously unaware that for Hegel history was the realm of accident, that a major element of the Hegelian system involves the working out of necessity (often contrary to the wills of the actors) in a realm characterized by accident, and that, in any event, the whole matter was irrelevant to the point he thought he was making. Even if some matters of concern to international politics are profoundly philosophical, not all are. It is essential, if I may use that philosophical term inappropriately, to address the proper methods to the proper questions and not to make global statements about international politics, as do the traditionalists, which assume the relevance of the same melange of methods regardless of the type of question.

I have no doubt that the early attempts at a scientific approach to international politics are guilty of crudities and errors. It would be amazing—and I do not expect to be amazed—if the earliest hypotheses and models designed as tools for the orderly and comparative investigation of the history of international politics survive in their original form in the face of sustained empirical and methodological investigations. The self-corrective techniques of science will, however, likely sustain orderly progress in the discipline. The traditionalists are unlikely to be helpful in this task.

Having read the criticisms of the traditionalists, I am convinced that they understand neither the simpler assertions nor the more sophisticated techniques employed by the advocates of the newer methods. They have not helped to clarify the important issues in methodology; they have confused them. The traditionalists have accused those writers who advocate modern scientific approaches of using deterministic models despite explicit statements by those writers to the contrary. The traditionalists mistake explicitly heuristic models for dogmatic assertions. They mistake assertions about deductions within the framework of a model for statements about the open world of history. They call for historical research and do not recognize either that they have not heeded their own call or that they are merely repeating the words of the advocates of the newer approaches.

The traditionalists are often quite intelligent and witty people. Why then do they make such gross mistakes? Surely there must be something seriously wrong with an approach that devotes so much effort to such ill-informed criticism. One suspects that this sorry product is the consequence of the traditionalist view of philosophy as elegant but undisciplined speculation—speculation devoid of serious substantive or methodological concerns. Thus traditionalists repeat the same refrain like a gramophone endlessly playing a single record; that refrain is beautifully orchestrated, wittily produced, and sensitive only to the wear of the needle in the groove.