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PROBLEMS OF THEORY BUILDING AND THEORY CONFIRMATION IN INTERNATIONAL POLITICS

By MORTON A. KAPLAN

THERE is a great demand for theories in international relations. The term "theory" has become so honorific that hypotheses, statements of fact, and intuitive guesses are often dressed up as theories. In part this longing for theory can be ascribed to a desire for the status of a "hard science" like physics, since the "hard sciences" are often viewed by laymen as the theoretical sciences par excellence. They have displayed their power in revealing the secrets of nature and, when applied to the affairs of men, have achieved notable practical successes like the construction of the atomic bomb.

On the whole this demand for theory is probably good. We cannot reason without generalization and, where matters are complex, the web of reasoning logically takes the form of a theory. Most historical investigations and case studies employ theories inexplicitly—often in the belief that the generalizations follow from the straightforward presentation of "purely factual" material. There is usually no recognition that interpretations of factual material can always be presented in a form isomorphic with theories from the sciences of economics, psychology, sociology, and so forth.

While it is doubtful that theories in social science, and in international politics in particular, can ever have the power of theories in physical science or be applied with the success achieved by physical scientists in making applications from their disciplines, the present sorry state of social science is no proof in itself that social science cannot attain such predictive power. Theoretical physical science lay fallow from the time of the Greeks until Galileo. The theory of international politics may indeed be awaiting its Galilean revolution. However, only an inquiry into the nature of the subject matter can inform us whether such expectations are justified on the basis of present knowledge.

According to some modern scientists, Galileo succeeded in revolutionizing physics as a theoretical science because he set himself a simpler problem than the Greek physicists set themselves. For instance, the Greeks tried to investigate the nature of perfect motion; only circles and straight lines fell within this category. Every other type of motion was treated as a deviation from perfect motion. Galileo set himself the simpler task of discovering where a body would be at time 2 if he knew its position, direction, and momentum at time 1.

Modern theoretical physical science has reared its present lofty edifice by setting itself problems that it has the tools or techniques to solve. When necessary, it has limited ruthlessly the scope of its inquiry. It has not attempted to predict the path a flipped chair will take, the paths of the individual particles of an exploded grenade, or the paths of the individual molecules of gas in a chamber. In the last case, there are laws dealing with the behavior of gases under given conditions of temperature and pressure, but these deal with the aggregate behavior of gases and not the behaviors of individual particles. The physicist does not make predictions with respect to matter in general but only with respect to the aspects of matter that physics deals with; and these, by definition, are the physical aspects of matter.

Every prediction of physics can be expressed as a consequent following upon certain antecedent conditions. Where the influence of extraneous considerations is minimal, as in matters of astrophysics, the distinction between the formal character of a physical prediction and a historical prediction can be neglected without causing any serious problems. The general behavior of astral bodies thus can be studied directly; physicists can measure the red shift of the sun's light in order to confirm relativity theory; and soon an atomic clock may be placed in a satellite to confirm certain relativity propositions concerning time. In the last case, however, we must rest our conclusion on a faith that, among other things, the scientists involved are not careless or dishonest, and that no malicious or fun-loving outer planetary race substitutes a different clock in the satellite. These may be extraordinarily reasonable assumptions—so reasonable that no sane man would doubt them; yet they reinforce our assertion that the prediction refers only to what will happen under specified antecedent conditions. The real world conditions may be different; but the physicist's prediction, except in engineering applications of physics, ignores this.

We have specified two factors that help the physical scientist to achieve the predictive power manifested by modern physics. He deals with a simple problem, or, put another way, with a problem in which only a small number of important variables are operative; and he carries on his studies and experiments in a laboratory that is closed to outer-world or historical forces.[1] Now it appears that the less these

[1] See the discussion in Morton A. Kaplan, *System and Process in International Politics*, New York, 1957, pp. xi ff. (referred to hereinafter as *System and Process*);

factors aid the physicist, the less developed theoretically is the particular phase of physical science to which he applies his talents. There seems to be a hierarchy of biology, chemistry, and physics within the physical sciences, with the degree of theoretical development ascending from one to the next; each science appears bound by the laws of the former as it adds new laws or propositions that distinguish its particular subject matter. On the other hand, each science gets less theoretical as we move from laboratory generalizations to engineering applications and to the complexities and uncertainties of the real world. The problem of engineering a theory of international politics to real world conditions is fundamental and itself requires to be understood at a theoretical level.

The small number of variables to which theories of international politics are restricted necessarily abstract from a far richer historical context. The theories therefore can be used for the derivation of consequences *only* under explicitly stated boundary or parameter conditions. For instance, the statements concerning alignment patterns of the "balance of power" model in *System and Process* apply only at the level of type of alignment, and do not specify the actual actors who participate in specific alignments. And they specify even this broad consequence only for stated values of the exogenous and endogenous variables. The first attempt to bring the models closer to the richness of history occurs in Chapter 3. In this chapter the models are varied for specified differences in the internal political and regulatory structure of nation-states. It is specifically recognized that the "structural features chosen to classify national actors are quite gross and, therefore, are not sufficient for any analysis aspiring to high predictive power" (p. 56). Even so, these gross features result in an enormous number of matrix boxes and cannot be used in general theoretical formulations, but only in formulations where the international systems aspects of the models are held constant as parameters. That is, as we come closer to reality—and this is still at a high level of abstraction—we lose generality. We begin to employ procedures closer to the step-by-step engineering applications of physical theory than to generalized theoretical statements of physical theory.

Even these gross characteristics of national actors are far removed from their historical complexity. "Any attempt to describe the actual

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and in idem, "Toward a Theory of International Politics," *Journal of Conflict Resolution*, II (December 1958), pp. 335-47. The skepticism expressed in the present paper was stated clearly and repeatedly in *System and Process*, but apparently was not understood by some commentators.

actor systems would founder under the weight of the parameters which individualize these systems—*even when their structural characteristics are similar* [italics added]. Such things as capability factors, logistic factors, and information, including history of the past, are specific to the system . . ." (p. 54). When we include the important factors that are contingent from the standpoint of theory, such as personality factors (an effort is made in Chapter 6 of *System and Process* to relate such factors to the models in a generalized sense), economic and political conditions, technological developments, invention, and other intranational and transnational factors, the complexity becomes so great that serious efforts to discuss them all and relate them all to models systematically would founder under the detail. If we want to apply our models to concrete cases, we must choose just those factors and just those factor values that we have some reason to believe operate in the particular instance we wish to understand and explain. In the endeavor, as our analysis gains in richness of relevant detail, we face a continuing loss of generality and a growing vagueness and lack of specification concerning the weight that each factor contributes to the total event or situation. This is the price we must pay when we deal with actual history. The models are useful in making these applications, but "do not correspond with reality except at the indicated levels of abstraction" (p. 2). They can be applied only in a step-by-step process, holding certain factors constant while attempting to work out the effects of additional factors not included in the models.

To repeat, we require models to test the generalizations we must employ at the level of international systems. There is no alternative—no other method to state or to analyze these generalizations. Such models can include only a restricted number of variables. In using such models, we pay the price of abstracting out many of the factors affecting the concrete course of events. When we wish to employ the model for more detailed engineering—not to test the broad generalization but to relate it to the historic context in which events are embedded—we lose theoretic generality. We come closer to step-by-step engineering applications. The practical and not the theoretic tends to dominate. We gain in historic richness, in the adequacy of our explanations, and lose in terms of precise understanding of how the variables under analysis are related. Our predictions become cruder in the sense of being less clearly related to an analytic process of reasoning and deduction. We pay a price whether we use models or engage in "historic" investigations. Neither method is "wrong" or "right" in the abstract. Both perform important functions and both contribute to each other if properly understood.

Which is to be employed as the primary tool of application depends upon the objective the researcher prefers to achieve.[2]

If the preceding assertions are correct, they should give the social scientist pause, for they indicate that the factors inhibiting the development of a powerful, predictive, theoretical social science are fundamental and that it is not merely a matter of waiting for a Galilean breakthrough. This does not imply the absence of predictability. We are all familiar with predictions of suicide rates based upon the assumption that, within a society, the forces operating for suicide will not change greatly from year to year; with sufficiently large numbers involved, statistical predictions will have a reasonable degree of accuracy. Similar predictions are made in regard to rates of automobile accidents. Sampling and interview methods have been used with reasonable accuracy to predict voting behavior and consumers' responsiveness to new products. In cases of this kind, however, although the statistical theories involved may be complex and powerful, the applications are not matters of complex social theory. Complex theories have also been applied to the measurement of certain human or social capacities. Factor analysis and Guttman scales, for instance, have been employed in such tasks. Again, however, the theories appear more methodological than substantive.

Substantive social theories in disciplines other than international politics appear to be of a number of different kinds. In economics, input-output analyses and linear programming reduce problems more or less to matters of methodological engineering. Beginning with Adam Smith, the mainstream of economic theory consists of substantive social theory of a highly abstract nature. To treat it with extreme brevity, the characteristics of certain kinds of markets are delineated and, on the assumption of economic rationality or profit maximization, the conditions of market equilibrium are specified.

Actually, however, economic theory can be viewed from two different perspectives. It can be viewed as a prediction of what will happen when economic actors behave according to the specified parameters of the theory. It can also be viewed as a prescription for maximizing profits or minimizing losses. Thus the marginal cost curve may be viewed as predicting that output will increase until marginal cost and price are in equilibrium, or as prescribing that production be increased until marginal cost equals price. Nothing in classical economic theory will account for the behavior of the old Chinese merchant who, having only

[2] For a recent detailed discussion of this problem, see Joseph J. Schwab, "What Do Scientists Do?" *Behavioral Science*, v (January 1960), pp. 1-27.

three items of a particular type, wants \$1 apiece or \$7 for all three, on the ground that if he sells all of them, he will lose face when someone else asks for the item. Nor would classical economic theory account for situations where attempts to maximize profits by lowering selling price resulted not in competitive behavior but in economic retaliation, including the withholding of supplies, by an infuriated economic community.

The mainstream of economic theory does not deal with real businesses or industries, or real markets. It deals with representative firms and abstract markets. It deals with generalities like the interest rate or the flow of money. It deals with aggregates of happenings and not with individual transactions. It does not predict individual behavior but general behavior and even here, if we want to be precise, it is necessary to add that it predicts not what the behavior will be but what the consequences of different kinds of behavior will be under certain specified assumptions.

The economist studies different kinds of markets—for instance, competitive, oligopolistic, and monopolistic; perfect and imperfect. Luckily for him, the economy simultaneously presents him with close analogies of most kinds of markets. If economic developments transform a competitive market into an oligopolistic one, he can look elsewhere for another competitive market to study. His theories concerning transitional developments from one kind of market to another are weaker than his theories concerning the behavior of a specified type of market.

In a society in which social constraints on the profit motive are minimal and economic rationality, as defined by economics, is a significant, if not the sole, motivating factor in economic behavior, the economist's predictions are likely to be confirmed. It is true that the economy is more complex than any economic model, and that feedback factors are so involved and complex that the most highly abstract theories concerning rate of economic growth and the economic consequences of different kinds of monetary management are difficult to demonstrate satisfactorily. But it is less difficult to see that production does tend to increase, under properly specified conditions, until price equals marginal cost, and so forth. Money is a reasonably tangible and countable commodity. Calculations concerning costs and so forth are fairly easy to make. If most firms behave according to principles of economic rationality, those which fail to do so tend to disappear and can be ignored.

Let us repeat: economic theory consists of models that abstract from the real world certain selected variables, and concerns itself with their interrelationships. One of the variables is motivation and thus the theory has a normative element when viewed from one aspect. The theory is based upon monetary units. Thus the operations of arithmetic can be used and all participants can come to reasonably similar conclusions at least with respect to certain critical aspects of economic processes. In some societies, the key boundary conditions like motivation can be determined independently with reasonable accuracy; and empirical investigations, using the monetary yardstick, can thus investigate the corollary predictions of economic theory. Although the theory is not highly confirmable with respect to other aspects of economic reality, such as innovative gambles, the fact that the economy persists and that it includes many different kinds of markets provides economic theory with a continuing relevance. We will see, for reasons to be specified later, that such models are not as promising when applied to problems of international politics.

There is a second major type of social theory which has had some significant success—although, for reasons that will become clear, this type of theory has less relevance to the most important and broadest problems of international politics. This is the type utilized by G. P. Murdock in working with the materials of the Human Relations Area Files and by S. N. Eisenstadt, particularly in his recent and as yet unpublished study of the modern historical bureaucratic states. This type of theory is less abstract and is more closely linked to empirical materials than that associated with the mainstream of economic analysis. To simplify Eisenstadt's theory almost to the point of distortion, he attempts to show that rulers can carry out certain kinds of policies only when there is a certain level of resources available to them, that these resources can be only of a kind developed by various types of free rather than feudal strata in society, and that these resources can be utilized and these free strata encouraged to engage in their activities only when there is a particular kind of bureaucratic development in the society.

It is relatively easy to show the level of resources required for particular kinds of policies. To show that feudal elements cannot provide this level of resources, and indeed might interfere with the implementation of these policies, requires a combination of social theory and comparative empirical research. To show that bureaucratic development is a necessary requirement for the flourishing of these free resources requires the application of social theory. Comparative research can then investigate the empirical relationships between the continued existence of such free resources and the development and maintenance of the historical bureaucracies. It appears from Eisenstadt's research that a direct relationship exists. Work of this kind emphasizes an extremely close relationship between theoretical structure and empirical materials. But its success depends both upon the existence of sufficient comparative materials and upon the fact that the relationships involved take on the characteristics almost of a force of nature. Although cultural factors might stifle the free resources and consequently stifle the bureaucracy, or vice versa, it must not be within the limits of variability for a bureaucracy to persist in the absence of the free resources, or for the free resources to continue to exist in the absence of the bureaucracy. Human misjudgment or differences in cultural patterns and objectives must be quite indifferent. For reasons that will emerge below, this particular kind of method is not easily applicable to the most important problems of international politics.

It may do some violence to the varieties of systematic social theory to claim that the two methods just discussed exhaust the universe of possibilities. But in a broad sense they do. Different techniques of analysis may be employed and more or less elaborate and sophisticated theoretical structures built and certainly different substantive conclusions may be reached, but every type of systematic social theory that is substantive rather than methodological in nature can be cast in one form or the other.[8]

It is now our task to investigate the peculiarities that attend a theoretical investigation into the most important substantive problems of international politics, and the difficulties which this involves for both theory construction and theory confirmation. It appears to me that the main task of a theory of international politics is to investigate the institutional regularities that attend the course of international political life, just as political science in general investigates the institutional regularities of national political life. As political scientists, we are not interested in the solution of a particular cabinet crisis as an isolated problem. We are interested in such affairs for the light they shed on the

[8] I do not contend, of course, that these types of theory exhaust the kinds of analysis that might be made. Obviously, the historian who attempts to explain a concrete sequence of events or the genesis of a specific event proceeds in ways that differ in part from those analyzed. The student of voting behavior interested in prediction of voting trends usually adopts a different form of analysis, although this need not invariably be the case. Attempts at statistical correlation of historic events are also of a different order. A case in point is Teggart's *Rome and China*, although one might contend that he used his statistical information to build a theory of a strictly biographical-causal type. Similarly, attempts to relate international activities to form of government or to trade rivalry may be restricted to explanations that are based on correlations, or may extend to causal sequences and to theories of one of the two specified types. I would contend, however, that for the kinds of problems this paper attempts to deal with at the theoretical level—that is, problems involving systematic social theory—the two broad types of theory specified roughly exhaust the field.

generalities of such occurrences; and, if there are no generalizations to be made, there is no political science, although we may still have a journalistic interest in political affairs.

If we are interested in institutional regularities—and, as a consequence, in comparative differences[4]—in international politics, we must recognize certain peculiar features of the subject matter. Although political science is dedicated to the study of the state as the source of political authority, international politics deals with relations between or among these ultimate or "sovereign" bodies. Nations are built of hundreds or thousands of cross-cutting social roles manifesting themselves in behavior. There are labor unions, religious organizations, industries, and so forth. The web of relationships produces a host of organized pressures, some of which can be likened to forces of nature. The individual and his decisions become lost, "averaged out," in the flow of decisions. In the international system there is only a small number of major actors or nation-states. During the nineteenth century they could be counted on

[4] Regularities may be thought of in many ways. For instance, the fact that 2 is the first integer greater than 1 is a regularity of our ordinary number system. That winter follows fall is a regularity of weather in temperate climates. That the candidate with a majority of the votes takes office is a regularity of our political system. That a system of at least five essential actors goes along with the essential rules of the "balance of power" system is a regularity of my "balance of power" model. Attention to regularities directly implies attention to differences. For instance, in *System and Process* six comparative types of international systems are specified and differences postulated with respect to three different sets of variables: the essential rules, the endogenous variables of the system, and the parameters or exogenous variables. The search for regularities in *System and Process* was itself responsible for the construction of a comparative international typology for perhaps the first time in the discipline.

I do not know how to separate the search for regularities from the search for differences in political science. Nor can I understand the assertion that the search for regularities must operate "only at the level of wholes" (Stanley J. Hoffmann, ed., *Contemporary Theory in International Relations*, Englewood Cliffs, N.J., 1960, p. 42). Regularities in my theory always involve the values of variables of systems, not "wholes," just as differences do. Of course, there are irregular events and processes, and also random ones. Reality cannot be forced into preconstructed molds. But the existence of regularities (and thus of regular differences) is necessary for the development of theory. The predisposition to search for regularities is essential to finding them and is desirable, provided it does not involve a dogmatic rejection of evidence to the contrary.

There is, of course, the important problem of level or precision of analysis. For instance, two thermostatically constant and equivalent temperatures may on analysis be discovered to have different patterns of variance around the mean temperature. Of two superficially identical ovens, one may have a concealed timer that halts its operation automatically after some given period of time. Of two presidential systems of government, one system may at a particular time have a strong president and the other a weak president; one may operate according to an item-veto principle and the other not. Some differences at the more precise level of analysis may affect the system in a way relevant to our inquiry; others may affect it but only after a certain period of time; others may affect it in no relevant way. In *System and Process* such problems are considered as related to the "levels of abstraction" problem and are linked to coupled systems and to engineering problems.

one's fingers. At the present time, the United States and the Soviet Union are the most important; there is a small number of nations of intermediary significance; and the total number of nations approximates one hundred. In systems of this kind, individual decisions are not canceled out in the mass. In some cases they may have a decisive effect. A change in the number of nations, particularly a reduction in the number of important nations, may have considerable effect upon the stability of the entire system of organized political relationships. Unlike the situation frequently encountered in economics, where a change in a particular market affects only that market and not the whole economy, a change in a part of the international political system often has an effect on the whole system. That means dynamic aspects of the process at the margin are of considerable importance, and that the factors making for stability or instability, or changing the number of participating actors, are also of great importance.

The cross-cutting social roles within nations produce a great number of solidary relations within the national web of relations; in general the relations between individuals and groups, on one hand, and the nation, on the other, are solidary. The relations of nations toward one another or toward the international system do not tend to be solidary, although nations may have instrumental reasons for supporting other nations or for helping to maintain the normative forms of the international system. The competition is not only for a "share of the spoils" but in addition may involve the most ultimate considerations. Although in domestic politics the form of political organization is usually taken for granted except during transitional periods, the very existence of the nation and the nature of the interrelationships among nations may be at stake in the play of international politics. The fact that there is only a small number of significant nation-states makes for a subsystem-dominant system of relations in the international arena—that is, a set of relations that exist not as parametric "givens" for the actors but as conditions that can be affected by their actions. Thus there is a highly strategic aspect to the central core of international political activity.

There is another difference between national and international politics that is significant for the task of constructing and confirming theory. Within national political systems, political organization is formal and durable. In the international arena, political organization is informal—at least with respect to those kinds of political action that historically have been the most important, like the alliance. This applies in particular to the methods of negotiation, bargaining, and conflict that characterize the interalliance activity governing the international political system.

Moreover, although particular alliances come into and pass out of existence with frequency in systems like the "balance of power" international system, the kind of alliance characteristic of the system endures for a considerable period of time. The kind of alliance system that is manifested in different and changing alliances through time is less tangible than the formal political organization characterizing the modern nation-state. This requires explanation at a high level of abstraction.

In addition, the international system is not a primary sphere of action in the same sense that national political systems are. Although we probably cannot understand the differences among the Italian city-state system, the nineteenth-century "balance of power" system, and the present bipolar system without knowledge of the number of essential actors, their relative capabilities, military instrumentalities, their relationships with the environment, and their modes of political intercourse, important aspects of international activity stem from intranational considerations. Internal political pressures—whether cultural and historical as in the anti-Westernism of the new nations, or whether stemming from the need to divert domestic discontent, the need for markets, desires for national aggrandizement, population explosions, technological innovations, and so forth—may have major consequences for international political decisions. That is, the focus of the decision may be as much on internal political and economic needs as on external ones.

We can now state certain things about the kind of theory we should have to construct to handle the central theoretical problems of international politics and the kinds of difficulties we are likely to run into in attempting to confirm such theories. Having learned our lesson from physical science, we will attempt in our theory to deal with only a limited number of variables. The central variables will include the major kinds of actors participating in international politics, their capabilities, including military capability, their motivations, their goal orientations, and their style of strategic and political activity. Even these central variables indicate great complexity in the theory. We must leave out, except as boundary conditions, all other variables, including intranational causes of international activity, although these may later be built into engineering applications of the theory. But it is quite clear that the set of variables to be included in the theory never exists in isolation in nature, as do the variables that the physicist deals with. As in economics, the central variables must be built into models that can be viewed either as normative or empirical, depending upon the way in which they are used. If the motivations and goal orientations are taken for granted, then the models are predictive for given specifications of the boundary conditions. If the boundary conditions are specified and the goal orientations or motivations left open, then the models may be viewed as prescriptions for maximizing certain kinds of objectives. Unfortunately there are certain differences from the situation in economics, shortly to be considered, that make the enterprise somewhat difficult.

For reasons already made clear, the model must be predictive or prescriptive with respect to activity at a high level of generality—that is, the *kind* of activity, whether political or normative—and not with respect to individual items of activity. It specifies what kinds of coalition patterns and goal objectives and limitations go along with given kinds of nations, capability ranges, economic and political systems, military forces, and so forth. It specifies the consequences which changes in certain of the internal or boundary conditions are likely to cause, but does not predict what will happen in a specific case. It predicts what kind of coalition should occur, and how its objectives should be limited if certain interests of the member nations are to be protected, but does not predict which particular nations will be members of which coalition.

For instance, the model of the "balance of power" system developed in *System and Process* predicts the kinds of shifts in membership according to short-term interests that occurred during the Congress of Vienna, but is not specific enough to predict the members of any particular alliance. It predicts that the stability of other variables of the system, such as the number of essential national actors or the limitations of objectives, depends upon a series of shifting, short-term, interest-oriented alliances, but does not predict that such alliances will occur in any particular case. It only predicts that if some factors external to the set of essential variables of the system persistently interfere with the alliance-patterning, then other variables of the system will also change in value.

The model must be strategically oriented. The small number of actors and the subsystem-dominant nature of the system entail this. Strategic play involves attempts to fool and to gain the better of opponents. The n-player nature of the system entails coalition problems. Game constraints on coalitions are weak. If the styles of play and strategies of the nations playing in the international political game are to converge to equilibrium, the analysis must be able to indicate the dynamic process which leads to this conclusion.

This has been done heuristically by the present writer. But in terms of a precise analysis sufficient to demonstrate the conclusion, the results so far are discouraging. No game model yet exists from which the conclusion can be derived. To prove that the equilibrium strategy is optimal—and thus rationally would be chosen by the players—by programming the game on a computer has so far been too complex to be practicable. All possible strategies and counterstrategies for all possible distributions of the spoils would have to be anticipated by the computer in making its decisions, or it would have to play through all the possibilities. As an alternative, Burns, Quandt, and Kaplan have constructed a table-stakes game of a simplified nature employing a remarkably complex computer, the human brain, in order to test some of the statements of the heuristic model.[5] We operate on the assumption that the human mind will eliminate the least likely strategies and divisions of the spoils, and that a series of plays by human players against one another will then perform the remaining eliminations necessary to discover the equilibrium strategies. The table-stakes game, however, is only a substitute for more desirable game or computer solutions and has, in any event, not yet been employed systematically.

Thus the problem of insuring the consistency and formal adequacy of the model is still unsolved. In addition, there is the danger that there are no optimal equilibrium strategies, in which case the whole theoretical enterprise would become murky—since the range of responses might be too great to handle—unless we discovered that there were in fact equilibrium strategies and styles of play employed by nations even though these did not follow from a consistent formal theory of strategy. That is, players might engage in non-optimal equilibrating strategies for cultural reasons, or there might be a formal solution that permitted a particular player at some stage of the play to secure dominance because of unavoidable and unpredictable momentary advantages. But as long as he did not have the advantage of the formal theory and did not know this, he might decide to employ the equilibrating strategy. And as long as disequilibrating strategies were not employed at these decisive points of time, the system would continue to function. There is another possibility: the disequilibrating strategy could be employed successfully by one of the players only at one of the aforementioned fortuitous stages of play. If a player in fact used the disequilibrating strategy appropriately, and if the opponents employed clearly optimal responses and could not prevent predominance by this player, we would in fact have a confirmation of the theory. If, however, there were no such thing as an optimal style of play, or if there were always a better line of play against any

[5] Morton A. Kaplan, Arthur L. Burns, and Richard E. Quandt, "Theoretical Analysis of the 'Balance of Power,'" *Behavioral Science*, v (July 1960), pp. 241-52. See also the article by T. C. Schelling in the present symposium.

particular set of equilibrium strategies, then the explanation of any empirical equilibrium would have to be found either in false conceptions of optimal play which were not challenged by deviant players, or in certain kinds of cultural inhibitions on styles of play. If our theory told us these should or should not produce equilibrium under specified conditions, we could then get some confirmations. Or, given a particular style of play as general, we might be able to make predictions concerning the consequences of individual deviance from the style, and confirm them.

Suppose that we are temporarily satisfied with the heuristic model, or that someday we possess a precise model which gives us greater confidence in the internal consistency of our theory. There still remain enormous problems with respect to the empirical confirmation of the theory. Here the differences from the situation facing the economic theorist assume first-order importance. There is not an easily calculable unit like money involved. If the concept of gross national product is rather fuzzy, the concept of national capabilities is even fuzzier. Nor is there a good measuring rod like that of profit to indicate the viability of the nation. Although there are considerations of capital strength such that a very big company might undersell its competitor uneconomically in order to insure its hold on the market, international political rivalry is much more direct than economic rivalry, since it involves not merely competition for a market but occasional forcible seizures of desired objectives. In the rare case that such things occur in the economy, they are not treated by economic theory. Motivation and rationality are reasonably evident when an entrepreneur increases production until marginal cost equals price. They are not so clear when a nation acts in terms of the precepts of the model of a system—for example, the "balance of power" system.[6] It is difficult to decide whether a particular action occurred because of strategically rational considerations or because of a particular ideological pattern of beliefs or because of internal political inhibitions. And even where a constraining pattern of beliefs can be demonstrated, it may be difficult to decide whether it accounts for the empirical pattern of activity or is merely a rationalization of activity decided upon for more strategic reasons.

[6] For example, in the prisoners' dilemma, if the prisoners acted irrationally and remained silent instead of talking because they misunderstood the strategic situation, they would in fact obtain a jointly more desirable result than in the case of a rational decision and might be led to believe that they *had* acted rationally. See Morton A. Kaplan, *Some Problems in the Strategic Analysis of International Politics*, Research Monograph No. 2, Center of International Studies, Princeton University, January 12, 1959; and *System and Process*, ch. 10.

There are many free variables in the type of model we have advocated both with respect to the variables internal to the model and with respect to those at the boundary. It may be especially difficult to determine which of the variables produced a particular result. When internal political factors, capabilities, military factors, strategic estimates, the credibility of opponents' offers and threats, and so forth vary simultaneously, there is the danger that almost any explanation can be fitted to the determinable facts, even though careful empirical investigation may eliminate with reasonable probability some of these possibilities.

Another important problem of empirical analysis concerns the vagueness of the criteria of the variables employed in the theory. The fuzziness of the concept of capability has been mentioned, as has the possibility of differing estimates of the factual situation. In addition, however, concrete actions like initiating a war, seizing booty or war objectives, entering into alliances, etc., have to be interpreted in terms of the more abstract variables of the theory. When is an objective limited? Clearly there may be vast gray areas here. When is a coalition designed to halt an actor with supranational objectives rather than merely to prevent military defeat? And does this difference have any importance as long as the same countercoalition is to be predicted in any case? What is an action to increase capabilities, and how do we distinguish between effective and ineffective action? England increased its capabilities during the Baldwin period although these actions were clearly inadequate, and engaged in at least some anti-German activities under Chamberlain although these, too, were clearly inadequate. We can obviously make reasonable judgments on these matters, but the determination is not specified by criteria presently employed in the theory. As a consequence, there is the danger that the theory can be fudged to explain almost any set of facts.

Unlike the economist, the student of international politics cannot examine simultaneously operating firms and markets of different kinds, and make detailed comparative or statistical analyses in order to determine which factors probably produce which result. He is not even in as good a position as the student of comparative politics. His only comparisons are comparisons in time, and in this case not one but many factors are varying simultaneously. For this reason analogies are quite perilous.

For the reasons analyzed previously, we must give up the hope that a theory of international politics can have either the explanatory or the predictive power of a "hard" science. Nonetheless we cannot study international politics theoretically without consideration of the constraints imposed by purely international factors on the international action process. The generalizations of historians and statesmen about the "balance of power" or the protection of national interests focus on such considerations. There are no tools other than the scientific tools to be applied, and their weakness in the particular case warrants skeptical caution rather than outright rejection.

It is in the nature of sophisticated and explicit theories that their careful statement reveals their weakness. The still greater weaknesses of ordinary common-sense generalizations are hidden by the implicit and inexplicit nature of the argument. If it is the case that generalizations of a scientific nature about the systematic properties of the international system cannot be avoided in significant analyses of international events, it is necessary to make theories concerning such events as explicit as possible. Although we may then recognize that our analyses are of a heuristic order—that is, that they permit us to order our experiences in a convincing but not highly demonstrable manner—they can still perform valuable functions. In addition, we may then be able to specify research designs that buttress the reasonable probability of our theoretical statements. For instance, the table-stakes game on which Burns, Quandt, and Kaplan are working is one such possibility, for it at least may give insight into the ways in which human beings in particular cultural settings succeed or fail in strategic undertakings that may not simulate international politics but resemble it with respect to certain key variables.

In such games, we may vary the number of players, the rate of economic development, relative and absolute military capabilities; we may instruct some players to use specified styles of play, some players to use deceit, and so forth; we may by varying the rewards put higher or lower premiums on "risky" moves or on attempts to gain hegemony, and try to establish the conditions under which this motivates players to pursue radically different styles of play. We can attempt to factor out as much as possible the cultural element in choice. We at least have in such methods a tool for the analogical testing of generalizations that are in fact made with respect to international politics and that are of the greatest importance in formulating theoretical systems or in determining practical policy decisions. In addition this is probably the only tool that will permit us systematically to investigate unstable systems as well as more stable systems. We are, however, presently inhibited from making ambitious generalizations concerning unstable systems by the paucity of our knowledge.

The statesman faced with a choice between limiting his gains in order to preserve his future alliance potential or grabbing as much as he can at the moment ought rationally to be interested in which course better enhances his ability to maintain the independence of his country or to gain hegemony, etc., and whether and to what extent the answer is dependent upon cultural as distinguished from purely strategic factors. Is a revolutionary dictator such a threat to a "balance of power" system that other nations would be wise to gang up on him immediately and inflict extra punishment on the nation he represents, or is his ability to disrupt the system dependent largely on the spread of a revolutionary ideology that immobilizes the normal responses of the other members of the system? Would a system of "power-mad" dictatorships rationally be forced to adopt the rules of a "balance of power" system?

To what extent are the answers to questions such as these dependent upon the number of nations involved, their relative and absolute military capabilities, etc.? These are not obscure theoretical or practical questions. Even though answers to them framed in general terms do not fully determine answers to specific applications, because of boundary influences considered in engineering applications, they cut to the heart of questions concerning national activity and the advisability of common attempts to change the modes of international organization. If, for instance, the "balance of power" system is inherently unstable, there are more reasons for scholars and statesmen to consider alternative modes of international organization, and the plausible strategies to achieve them, than exist where the system is inherently stable. Strategic analysis thus raises explicitly questions essential both to policy and to theoretical understanding of international politics, even if it is not the key to all relevant questions. It is a tool that permits for the first time the explicit specification of certain key variables in the international political process and also permits for the first time the testing of hypotheses concerning these variables, even if only indirectly or by analogy. It is a tool for the development of more and more sophisticated tests and for the application of the elementary canons of scientific discourse to the analysis of the strategic aspects of international politics. The difficulties involved in formal strategic analysis—which we have discussed—are inherent in the nature of the subject matter. Less formal or non-strategic modes of analysis evade the problems involved in using strategic theory by evading the problems involved in the subject matter of international politics.[7]

[7] I do not disagree so much with the qualifications concerning game theory as it now stands that Burns makes elsewhere in this symposium as with his tendency to throw the baby out with the bath. For efforts to modify game theory to increase its relevance to international politics, see Thomas C. Schelling, *The Strategy of Conflict*, Cambridge,

In addition to gaming techniques for investigating the adequacy of statements derived from models of theories of international systems, we may turn to historical materials. We may examine cases that puzzle historians or for which historians do not have a convincing and sophisticated explanation, and see whether our theories seem to account for the state of affairs. The number of free variables involved may exclude dogmatism, but if, upon analysis, the theory provides a deeper and intellectually more satisfying explanation than the normal historical explanation, this is a mark in its favor. We can explore whether the theory is congruent with certain kinds of normative conduct and incongruent with others, as Katzenbach and Kaplan have been doing.[8] If the theory seems to specify the differences that actually do occur in the normative structure of international law during different historic periods, then this converging explanation gives us added reason to prefer the theory to alternative explanations.

Despite the weakness of comparative method with respect to problems of international politics, we might investigate differences in international behavior during different historic periods. For instance, we might study the ancient Greek system, the Italian city-state system, and the eighteenth- and nineteenth-century "balance of power" system—all of which have certain "balance of power" features in common. If some of these historic systems are stable and others not—for instance, a "roll-up" may occur—and if empirical study isolates the difference that seems to account for this, we can then hunt for additional examples where this specific difference seems to be the important one, and see whether the same difference in behavior occurs. We can also build the difference into our table-stakes game and see whether the same sort of different behavior is produced in the course of it. If so, we have a converging explanation. If not, we may experiment and see whether we can produce the behavior in the game in some other fashion, and then turn back to our historical studies to see whether enlightenment comes from this particular explanation rather than from the one we originally hit upon. Possibly some aspects of international theory are not dependent on strategic analysis and can have the solidity of theories like Eisenstadt's.

In any event, our explanations or theories can never have the authority of theory in physics, or its explanatory or predictive power. The impor-

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Mass., 1960; Kaplan, *System and Process*, ch. 11; and idem, *Some Problems in the Strategic Analysis of International Politics.*

[8] See Morton A. Kaplan and Nicholas de B. Katzenbach, *The Political Foundations of International Law*, New York, 1961.

tant problem is whether they can be stated in ways that permit additional analysis and investigation. Whether they are tautological dead ends or fruitful aids to historical and scientific imagination, whether the statements in them permit at least reasonable analysis and investigation or whether they are dogmatic fiats, the science of the discipline does not lie in absolute certainty but in reasonable belief, in definite canons of procedure and investigation, and in the attempt to permit confirmation or falsification even though of an imprecise order. The object is not to seek a certainty or precision that the subject matter does not allow, but to reject a dogmatism that the subject matter does not make necessary. The very difficulties of theory building and confirmation in international politics demand sincere dedication to scientific canons of procedure.