Contents

1

Laws and Theories

I write this book with three aims in mind: first, to examine theories of international politics and approaches to the subject matter that make some claim to being theoretically important; second, to construct a theory of international politics that remedies the defects of present theories; and third, to examine some applications of the theory constructed. The required preliminary to the accomplishment of these tasks is to say what theories are and to state the requirements for testing them.

I

Students of international politics use the term "theory" freely, often to cover any work that departs from mere description and seldom to refer only to work that meets philosophy-of-science standards. The aims I intend to pursue require that definitions of the key terms *theory* and *law* be carefully chosen. Whereas two definitions of theory vie for acceptance, a simple definition of law is widely accepted. Laws establish relations between variables, variables being concepts that can take different values. If *a*, then *b*, where *a* stands for one or more independent variables and *b* stands for the dependent variable: In form, this is the statement of a law. If the relation between *a* and *b* is invariant, the law is absolute. If the relation is highly constant, though not invariant, the law would read like this: If *a*, then *b* with probability *x*. A law is based not simply on a relation that has been found, but on one that has been found repeatedly. Repetition gives rise to the expectation that if I find *a* in the future, then with specified probability I will also find *b*. In the natural sciences even probabilistic laws contain a strong imputation of necessity. In the social sciences to say that persons of specified income vote Democratic with a certain probability is to make a law-like statement. The word *like* implies a lesser sense of necessity. Still, the statement would not be at all like a law unless the relation had so often and so reliably been found in the past that the expectation of its holding in the future with comparable probability is high.*

By one definition, theories are collections or sets of laws pertaining to a particular behavior or phenomenon. In addition to income, for example, associations may be established between voters' education, their religion, and their parents' political commitment, on the one hand, and the way they vote, on the other hand. If the probabilistic laws thus established are taken together, higher correlations are achieved between voters' characteristics (the independent variables) and choice of party (the dependent variable). Theories are, then, more complex than laws, but only quantitatively so. Between laws and theories no difference of kind appears.

This first definition of theory supports the aspiration of those many social scientists who would "build" theory by collecting carefully verified, interconnected hypotheses. The following story suggests how most political scientists think of theory:

Homer describes the walls of Troy as being eight feet thick. If his account is true, then millenia later one should be able to find those walls by careful digging. This thought occurred to Heinrich Schliemann as a boy, and as a man he put the theory to empirical test. Karl Deutsch uses the story as an example of how new-style theories are tested (1966, pp. 168–69). A theory is born in conjecture and is viable if the conjecture is confirmed. Deutsch regards theories of the simple if-then sort as "special theories," which may "later on become embedded in a grand theory." He then gives other examples and in doing so shifts "from a yes-or-no question to a how-much question." We should try to find out how much of a contribution "different variables" make to a given result (1966, pp. 219–21).

What is possibly useful in such a pattern of thinking, and what is not? Everyone knows that a coefficient of correlation, even a high one, does not warrant saying that a causal relation exists. Squaring the coefficient, however, technically permits us to say that we have accounted for a certain percentage of the variance. It is then easy to believe that a real causal connection has been identified and measured, to think that the relation between an independent and a dependent variable has been established, and to forget that something has been said only about dots on a piece of paper and the regression line drawn through them. Is the correlation spurious? That suggests the right question without quite asking it. Correlations are neither spurious nor genuine; they are merely numbers that one gets by performing simple mathematical operations. A correlation is neither spurious nor genuine, but the relation that we infer from it may be either. Suppose someone propounds a law, for example, by carefully establishing the relation between the amount of push imparted to a cart and the amount of its movement. The relation established, if conditions are kept constant and measurement is careful, is simply a fact of observation, a law that remains constantly valid. The *explanation* offered for that relation of push and movement, however, is radically different depending on whether we consult Aristotle or Galileo or Newton. The uncritical acceptance of a number as indicating that a connection obtains is the first danger to guard against. To do so is fairly easy. The next problem is more important and harder to solve.

Even if we have satisfied ourselves in various ways that a correlation points to a connection that reliably holds, we still have not accounted for that connection in the sense of having explained it. We have accounted for it in the way—and only in the way—that Aristotelian physics accounted for the relation between push and movement. From a practical standpoint, knowledge of the high correlation between push and movement is very useful. That descriptive knowledge may suggest clues about the principles of motion. It may as easily be grossly misleading, as indeed it turned out to be. Numbers may describe what goes on in the world. But no matter how securely we nail a description down with numbers, we still have not explained what we have described. Statistics do not show how anything works or fits together. Statistics are simply descriptions in numerical form. The form is economical because statistics describe a universe through manipulation of samples drawn from it. Statistics are useful because of the variety of ingenious operations that can be performed, some of which can be used to check on the significance of others. The result, however, remains a description of some part of the world and not an explanation of it. Statistical operations cannot bridge the gap that lies between description and explanation. Karl Deutsch advises us "to formulate, or reformulate, a proposition in terms of probability and to say *how much* of the outcome could be accounted for by one element and how much of the outcome could be accounted for from other elements or is autonomous and free" (1966, p. 220). If we follow that advice, we will behave like Aristotelian physicists. We will treat a problem as though it were like the one of trying to say to what extent a cart's movement results from push and slope and to what extent its movement is impeded by frictions. We will continue to think in sequential and correlational terms. By doing so, results that are practically useful may be achieved, although students of international politics have disappointingly little to show for such efforts, even in practical terms. And if useful information were uncovered, the more difficult task of figuring out its theoretical meaning would remain.

*One must be careful. The above statement is law-like only if it can be verified in various ways. Counterfactual conditions, for example, would have to be met in this way: Person *b* is in the income category of likely Republicans; if *b*'s income were reduced to a certain level, he would probably become a Democrat. More precisely, the law-like statement establishes these expectations: If *b* is an *R* with probability *x*, and if *a* is a *D* with probability *y*, then if *b* becomes *a*, he thereby becomes a *D* with probability *y*.

The "inductivist illusion," as structural anthropologist Lévi-Strauss terms it, is the belief that truth is won and explanation achieved through the accumulation of more and more data and the examination of more and more cases. If we gather more and more data and establish more and more associations, however, we will not finally find that we know something. We will simply end up having more and more data and larger sets of correlations. Data never speak for themselves. Observation and experience never lead directly to knowledge of causes. As the American pragmatist, C. S. Peirce, once said, "direct experience is neither certain nor uncertain, because it affirms nothing—it just *is*. It involves no error, because it testifies to nothing but its own appearance. For the same reason, it affords no certainty" (quoted in Nagel 1956, p. 150). Data, seeming facts, apparent associations—these are not certain knowledge of something. They may be puzzles that can one day be explained; they may be trivia that need not be explained at all.

If we follow the inductivist route, we can deal only with pieces of problems. The belief that the pieces can be added up, that they can be treated as independent variables whose summed effects will account for a certain portion of a dependent variable's movement, rests on nothing more than faith. We do not know what to add up, and we do not know whether addition is the appropriate operation. The number of pieces that might be taken as parts of a problem is infinite, and so is the number of ways in which the pieces may be combined. Neither observationally nor experimentally can one work with an infinity of objects and combinations. In the following example, Ross Ashby offers an apt caution. Astrophysicists seek to explain the behavior of star clusters with 20,000 members. The beginner, Ashby observes, "will say simply that he wants to know what the cluster will do, i.e., he wants the trajectories of the components. If this knowledge, however, could be given to him, it would take the form of many volumes filled with numerical tables, and he would then realise that he did not really want all that." The problem, Ashby concludes, is how to find out what we really want to know without "being overwhelmed with useless detail" (1956, p. 113). The old motto, "knowledge for the sake of knowledge" is an appealing one, perhaps because one can keep busy and at the same time avoid the difficult question of knowledge for what. Because facts do not speak for themselves, because associations never contain or conclusively suggest their own explanation, the question must be faced. The idea of "knowledge for the sake of knowledge" loses its charm, and indeed its meaning, once one realizes that the possible objects of knowledge are infinite.

Today's students of politics nevertheless display a strong commitment to induction. They examine numerous cases with the hope that connections and patterns will emerge and that those connections and patterns will represent the frequently mentioned "reality that is out there." The hope apparently rests on the conviction that knowledge begins with certainties and that induction can uncover them. But we can never say with assurance that a state of affairs inductively arrived at corresponds to something objectively real. What we think of as reality is itself an elaborate conception constructed and reconstructed through the ages. Reality emerges from our selection and organization of materials that are available in infinite quantity. How can we decide which materials to select and how to arrange them? No inductive procedure can answer the question, for the very problem is to figure out the criteria by which induction can usefully proceed.

Those who believe, oddly, that knowledge begins with certainties think of theories as edifices of truth, which they would build inductively. They define theories as hypotheses that are confirmed and connected. But empirical knowledge is always problematic. Experience often misleads us. As Heinrich Hertz put it, "that which is derived from experience can again be annulled by experience" (1894, p. 357). Nothing is ever both empirical and absolutely true, a proposition established by Immanuel Kant and now widely accepted at least by natural scientists. And since empirical knowledge is potentially infinite in extent, without some guidance we can know neither what information to gather nor how to put it together so that it becomes comprehensible. If we could directly apprehend the world that interests us, we would have no need for theory. We cannot. One can reliably find his way among infinite materials only with the guidance of theory defined in the second sense.

Rather than being mere collections of laws, theories are statements that explain them (cf. Nagel 1961, pp. 80–81; Isaak 1969, pp. 138–39). Theories are qualitatively different from laws. Laws identify invariant or probable associations. Theories show why those associations obtain. Each descriptive term in a law is directly tied to observational or laboratory procedures, and laws are established only if they pass observational or experimental tests. In addition to descriptive terms, theories contain theoretical notions. Theories cannot be constructed through induction alone, for theoretical notions can only be invented, not discovered. Aristotle dealt with real motion, that is with the ratios of effort to movement that are matters of common experience. Galileo took bold steps away from the real world in order to explain it. Aristotle believed that objects are naturally at rest and that effort is required to move them; Galileo assumed that both rest and uniform circular motion are natural and that an object remains in either of these conditions in the absence of outside forces. Newton conceived of a uniform rectilinear motion. The theory he devised to explain it introduced such theoretical notions as point-mass, instantaneous acceleration, force, and absolute space and time, none of which can be observed or experimentally determined. At each step, from Aristotle through Galileo to Newton, the theoretical concepts became bolder—that is, further removed from our sense experience.

A theoretical notion may be a concept, such as force, or an assumption, such as the assumption that mass concentrates at a point. A theoretical notion does not explain or predict anything. We know, and so did Newton, that mass does not concentrate at a point. But it was not odd of Newton to assume that it did, for assumptions are not assertions of fact. They are neither true nor false. Theoretical notions find their justification in the success of the theories that employ them. Of purported laws, we ask: "Are they true?" Of theories, we ask: "How great is their explanatory power?" Newton's theory of universal gravitation provided a unified explanation of celestial and terrestrial phenomena. Its power lay in the number of previously disparate empirical generalizations and laws that could be subsumed in one explanatory system, and in the number and range of new hypotheses generated or suggested by the theory, hypotheses that in turn led to new experimental laws.

Aristotle concluded that, within limits, "a given body can be displaced in a set time through a distance proportional to the effort available" (Toulmin 1961, p. 49). Whether by ancient or modern mechanics, the high correlation of push and movement holds true. But how is it to be explained? Such facts have remained constant; the theories accepted as adequate for their explanation have changed radically. Laws are "facts of observation"; theories are "speculative processes introduced to explain them." Experimental results are permanent; theories, however well supported, may not last (Andrade 1957, pp. 29, 242). Laws remain, theories come and go.

Since I see no reason for wasting the word "theory" by defining it as a set of two or more laws, I adopt the second meaning of the term: Theories explain laws. This meaning does not accord with usage in much of traditional political theory, which is concerned more with philosophic interpretation than with theoretical explanation. It does correspond to the definition of the term in the natural sciences and in some of the social sciences, especially economics. The definition also satisfies the need for a term to cover the explanatory activity we persistently engage in. In order to get beyond "the facts of observation," as we wish irresistibly to do, we must grapple with the problem of explanation. The urge to explain is not born of idle curiosity alone. It is produced also by the desire to control, or at least to know if control is possible, rather than merely to predict. Prediction follows from knowledge of the regularity of associations embodied in laws. Sunrises and sunsets can be reliably predicted on the basis of empirical findings alone, without benefit of theories explaining why the phenomena occur. Prediction may certainly be useful: The forces that propel two bodies headed for a collision may be inaccessible, but if we can predict the collision, we can at least get out of the way. Still, we would often like to be able to exert some control. Because a law does not say why a particular association holds, it cannot tell us whether we can exercise control and how we might go about doing so. For the latter purposes we need a theory.

A theory, though related to the world about which explanations are wanted, always remains distinct from that world. "Reality" will be congruent neither with

a theory nor with a model that may represent it. Because political scientists often think that the best model is the one that reflects reality most accurately, further discussion is needed.

Model is used in two principal ways. In one sense a model represents a theory. In another sense a model pictures reality while simplifying it, say, through omission or through reduction of scale. If such a model departs too far from reality, it becomes useless. A model airplane should look like a real airplane. Explanatory power, however, is gained by moving away from "reality," not by staying close to it. A full description would be of least explanatory power; an elegant theory, of most. The latter would be at an extreme remove from reality; think of physics. Departing from reality is not necessarily good, but unless one can do so in some clever way, one can only describe and not explain. Thus James Conant once defined science as "a dynamic undertaking directed to lowering the degree of the empiricism involved in solving problems" (1952, p. 62). A model of a theory will be about as far removed from reality as the theory it represents. In modeling a theory, one looks for suggestive ways of depicting the theory, and not the reality it deals with. The model then presents the theory, with its theoretical notions necessarily omitted, whether through organismic, mechanical, mathematical, or other expressions.

Some political scientists write of theoretical models as though they were of the model airplane sort. For example, they first criticize the state-centric model of international politics because it has supposedly become further and further removed from reality. Then they try earnestly to make models that mirror reality ever more fully. If their efforts were to succeed, the model and the real world would become one and the same. The error made is the opposite of the one Immanuel Kant so cogently warned against, that is, of thinking that what is true in theory may not be so in practice. As Kant well understood, his warning did not imply that theory and practice are identical. Theory explains some part of reality and is therefore distinct from the reality it explains. If the distinction is preserved, it becomes obvious that induction from observables cannot in itself yield a theory that explains the observed. "A theory can be tested by experience," as Albert Einstein once said, "but there is no way from experience to the setting up of a theory" (quoted in Harris 1970, p. 121). To claim that it is possible to arrive at a theory inductively is to claim that we can understand phenomena before the means for their explanation are contrived.

The point is not to reject induction, but to ask what induction can and cannot accomplish. Induction is used at the level of hypotheses and laws rather than at the level of theories. Laws are different from theories, and the difference is reflected in the distinction between the way in which laws may be discovered and the way in which theories have to be constructed. Hypotheses may be inferred from theories. If they are confirmed quite conclusively, they are called laws.

Hypotheses may also be arrived at inductively. Again, if they are confirmed quite conclusively, they are called laws. Ebb and flood tides were predicted by ancient Babylonians with an accuracy unsurpassed until the end of the nineteenth century. Highly reliable knowledge of the law-like movement of tides did not enable one to explain them. Hypotheses about the association of this with that, no matter how well confirmed, do not give birth to theories. Associations never contain or conclusively suggest their own explanation.

Though in itself induction leads to a theoretical dead end, we nevertheless need some sense of the puzzling connections of things and events before we can worry about constructing theories. At the same time we need a theory, or some theories, in order to know what kind of data and connections to look for. Knowledge, it seems, must precede theory, and yet knowledge can proceed only from theory. This looks much like the dilemma suggested by the Platonic proposition that we cannot know anything until we know everything. Take this thought literally, and one is driven to despair. Take it instead as a statement of the strategic problem of gaining knowledge, and no more is suggested than the difficulties in any field of getting onto an intellectual track that promises to lead to some progress.

If induction is not the way to get onto a useful track, what is? The leap from law to theory, from the fashioning of hypotheses to the development of explanations of them, cannot be made by taking information as evidence and seeking more of it. The leap cannot be made by continuing to ask what is associated with what, but rather by trying to answer such questions as these: Why does this occur? How does that thing work? What causes what? How does it all hang together?

If theory is not an edifice of truth and not a reproduction of reality, then what is it? A theory is a picture, mentally formed, of a bounded realm or domain of activity. A theory is a depiction of the organization of a domain and of the connections among its parts (cf. Boltzman 1905). The infinite materials of any realm can be organized in endlessly different ways. A theory indicates that some factors are more important than others and specifies relations among them. In reality, everything is related to everything else, and one domain cannot be separated from others. Theory isolates one realm from all others in order to deal with it intellectually. To isolate a realm is a precondition to developing a theory that will explain what goes on within it. If the precondition cannot be met, and that of course is a possibility, then the construction of theory for the matters at hand is impossible. The question, as ever with theories, is not whether the isolation of a realm is realistic, but whether it is useful. And usefulness is judged by the explanatory and predictive powers of the theory that may be fashioned.

Theories, though not divorced from the world of experiment and observation, are only indirectly connected with it. Thus the statement made by many that theories can never be proved true. If "truth" is the question, then we are in the realm of law, not of theory. Thus the statement made by James B. Conant, a chemist, that "a theory is only overthrown by a better theory" (1947, p. 48). Thus the statement made by John Rader Platt, a physicist, that "the pressure of scientific determinism becomes weak and random as we approach the great unitary syntheses. For they are not only discoveries. They are also artistic creations, shaped by the taste and style of a single hand" (1956, p. 75). And these statements can all be read as glosses on the famous proof of the mathematician Henri Poincaré that if one mechanical explanation for a phenomenon can be given, then so can an infinity of others.* Theories do construct *a* reality, but no one can ever say that it is *the* reality. We are therefore faced with both an infinity of data and an infinity of possible explanations of the data. The problem is a double one. Facts do not determine theories; more than one theory may fit any set of facts. Theories do not explain facts conclusively; we can never be sure that a good theory will not be replaced by a better one.

I have said what theories are and what they are not, but I have not said how theories are made. How are they made? The best, but unhelpful, short answer is this: "creatively." The word sets the problem without saying how to solve it. How does one move between observations and experiments and theories that explain them? The longest process of painful trial and error will not lead to the construction of a theory unless at some point a brilliant intuition flashes, a creative idea emerges. One cannot say how the intuition comes and how the idea is born. One can say what they will be about. They will be about the organization of the subject matter. They will convey a sense of the unobservable relations of things. They will be about connections and causes by which sense is made of things observed. A theory is not the occurrences seen and the associations recorded, but is instead the explanation of them. The formula for the acceleration of a freely falling body does not explain how the body falls. For the explanation one looks in classical physics to the whole Newtonian system—a package of interconnected concepts, an organization of the physical world in which the pertinent happenings become natural or necessary. Once the system is understood, once its principle of organization is grasped, the phenomena are explained. All of this is well summed up in words that Werner Heisenberg attributes to Wolfgang Pauli: " 'Understanding' probably means nothing more than having whatever ideas and concepts are needed to recognize that a great many different phenomena are part of a coherent whole" (1971, p. 33).

By a theory the significance of the observed is made manifest. A theory arranges phenomena so that they are seen as mutually dependent; it connects

*The proof is simply presented by Nagel (1961, p. 116n). One should add that the explanations will not be equally simple and useful.

otherwise disparate facts; it shows how changes in some of the phenomena necessarily entail changes in others. To form a theory requires envisioning a pattern where none is visible to the naked eye. The pattern is not the sum of the substance of our daily world. Scientific facts are highly special and relatively few as compared to all of the things that could conceivably be brought within explanatory systems. A theory must then be constructed through simplifying. That is made obvious by thinking of any theory, whether Isaac Newton's or Adam Smith's, or by thinking of the alternative—to seek not explanation through simplification but accurate reproduction through exhaustive description. Simplifications lay bare the essential elements in play and indicate the necessary relations of cause and interdependency—or suggest where to look for them.

Even by those who have authored them, the emergence of theories cannot be described in other than uncertain and impressionistic ways. Elements of theories can, however, be identified. The difficulty of moving from causal speculations based on factual studies to theoretical formulations that lead one to view facts in particular ways is experienced in any field. To cope with the difficulty, simplification is required. This is achieved mainly in the following four ways: (1) by isolation, which requires viewing the actions and interactions of a small number of factors and forces as though in the meantime other things remain equal; (2) by abstraction, which requires leaving some things aside in order to concentrate on others; (3) by aggregation, which requires lumping disparate elements together according to criteria derived from a theoretical purpose; (4) by idealization, which requires proceeding as though perfection were attained or a limit reached even though neither can be. Whatever the means of simplifying may be, the aim is to try to find the central tendency among a confusion of tendencies, to single out the propelling principle even though other principles operate, to seek the essential factors where innumerable factors are present.

In addition to simplifications, or as forms of them, theories embody theoretical assumptions. Imagining that mass concentrates at a point, inventing genes, mesons, and neutrinos, positing a national interest, and defining nations as unitary and purposive actors: These are examples of common assumptions. Theories are combinations of descriptive and theoretical statements. The theoretical statements are nonfactual elements of a theory. They are not introduced freely or whimsically. They are not introduced in the ancient and medieval manner as fictions invented to save a theory. They are introduced only when they make explanation possible. The worth of a theoretical notion is judged by the usefulness of the theory of which it is a part. Theoretical notions enable us to make sense of the data; the data limit the freedom with which theoretical notions are invented. Theorists create their assumptions. Whether or not they are acceptable depends on the merit of the scientific structure of which they are a part.

Constructing theories involves more than the performance of logically permissible operations on observed data. By deduction nothing can be explained, for the results of deduction follow logically from initial premises. Deduction may give certain answers, but nothing new; what is deduced is already present either in theoretical major premises or in empirical minor premises dealing with matters previously observed. Induction may give new answers, but nothing certain; the multiplication of particular observations can never support a universal statement. Theory is fruitful because it goes beyond the necessarily barren hypothetico-deductive approach. Both induction and deduction are indispensable in the construction of theory, but using them in combination gives rise to a theory only if a creative idea emerges. The task of constructing theories becomes both more consequential and more complicated, and so does the task of verifying them. The relation between theory and observation, or between theory and fact, becomes puzzling.

As an example of this puzzling relation, consider the problem of defining the terms used in a theory. Think of the distinct meanings in different physical theories of space, energy, momentum, and time. Obviously such notions have no meaning outside of the theory in which they appear (Nagel 1961, pp. 17, 127f.). That theoretical notions are defined by the theory in which they appear is easily understood. In the field of international politics, think of the different meanings commonly attached to the words in the following list: power, force, pole, relation, actor, stability, structure, and system. The meanings of such terms vary depending on their user's approach to the subject. This is necessarily so in any field where theories are contradictory. The contradiction of theories creates differences in the meanings of terms across theories. In international politics, as in the social sciences generally, theories turn out to be weak ones. The weakness of theories creates uncertainty of meanings even within a single theory. In international politics, whether because theories are contradictory or weak, discussion and argument about many important matters—the closeness of national interdependence, the stability of particular configurations of power, the usefulness of force—are made difficult or useless because the participants are talking about different things while using the same terms for them. Movement toward a remedy is impeded by disinclination to treat the question of meaning as a problem that can be solved only through the articulation and refinement of theories. The tendency instead is to turn the problem of meaning into the technical one of making terms operational. That won't help. Any of the above terms can be made operational in most of the meanings our discourse assigns to them. "Poles" have clear empirical referents, for example, whether defined as blocs or as great powers. By either definition, "poles" can become descriptive terms in the statement of laws. The technical usability of terms is unfortunately a weak criterion.

Though it is easy to see that theoretical notions are defined by the theory in which they appear, it is easy to overlook that even descriptive terms acquire different meanings as theories change. Stephen C. Pepper refers to the "close interdependence of fact and theory" (1942, p. 324). Thomas S. Kuhn specifies what happens precisely in terms of the change of "similarity relations" in the transition from one theory to the next. Objects of the same or of different sets in one theory may be grouped in different or in the same sets by another theory, as with the sun, the moon, Mars, and the earth before and after Copernicus. As Kuhn remarks, if two men are committed to different theories, "we cannot say with any assurance that the two men even see the same thing, [that they] possess the same data, but identify or interpret it differently" (1970, pp. 266–76). Do we only know what we see, one may wonder, or do we only see what we know? Our minds cannot record and make something of all of the many things that in some sense we see. We are therefore inclined to see what we are looking for, to find what our sense of the causes of things leads us to believe significant.

Changes of theory produce changes in the meaning of terms, both theoretical and factual ones. Theories not only define terms; they also specify the operations that can rightly be performed. In the sense used a moment ago, the operational question is a minor or merely a practical one. In another sense, the operational question is fundamentally important. Theories indicate what is connected with what and how the connection is made. They convey a sense of how things work, of how they hang together, of what the structure of a realm of inquiry may be. If the organization of a realm affects the interactions of variables within it, it makes no sense to manipulate data until the question of how variables may be connected is answered. Nevertheless, correlational labors proceed as though in the international realm variables are directly connected without structural constraints operating on them—as though the phenomena we deal with are all at one level. Coefficients of correlation are amassed without asking which theories lead one to expect *what kind* of a connection among *which* variables.

Much pointless work is done because the three questions that should be asked at the outset of an inquiry are so often ignored. They are:

- Does the object of investigation permit use of the analytic method of classical physics—examining the attributes and interactions of two variables while others are kept constant?
- Does it permit the application of statistics in ways commonly used when the number of variables becomes very large?
- Does the object of study permit neither approach, but instead require a systemic one?

The answer to the last question will be "yes" if the object of study is both complex and organized. Organized complexity, to use Warren Weaver's term, precludes the use of traditional modes of investigation (1947, pp. 6–7). One must choose an approach that is appropriate to the subject matter. The rules by which one's inquiry proceeds vary from one approach to another. "Due process of inquiry," as Martin Landau has said, requires one to follow the logic and procedures that one's methodology prescribes (1972, pp. 219–21). Most students of international politics have not observed "due process of inquiry." Worse still, they have not been able to figure out what the due process of their inquiries might be. They have been much concerned with methods and little concerned with the logic of their use. This reverses the proper priority of concern, for once a methodology is adopted, the choice of methods becomes merely a tactical matter. It makes no sense to start the journey that is to bring us to an understanding of phenomena without asking which methodological routes might possibly lead there. Before setting out we need to ask what different theoretical maps of the subject matter might show. If we are not to waste time laboring without any idea of whether the labor is mere muscular exercise, theoretical questions must be raised at the outset of inquiry.

II

In examining international-political theories in the next two chapters, we shall rely on the above discussion of the meaning of theory. If we should find some constructions that look like theories, we will of course want to know how good the explanations they offer may be. I conclude this chapter, therefore, by examining the problem of testing theories.

In order to test a theory, one must do the following:

1. State the theory being tested.
2. Infer hypotheses from it.
3. Subject the hypotheses to experimental or observational tests.
4. In taking steps two and three, use the definitions of terms found in the theory being tested.
5. Eliminate or control perturbing variables not included in the theory under test.
6. Devise a number of distinct and demanding tests.
7. If a test is not passed, ask whether the theory flunks completely, needs repair and restatement, or requires a narrowing of the scope of its explanatory claims.

The apparent failure of a theory may result from the improper accomplishment of one of these steps. Several of them require special emphasis. Since a hypothesis derived from a theory is being tested (there being no way to test a theory directly), a hypothesis proved wrong should lead one to reexamine the second and seventh operations. Was the hypothesis rightly inferred from the theory? How, and to what extent, does the invalidation of a properly drawn hypothesis bring the theory into question? The unfavorable results of tests should not lead to the hasty rejection of theories. Nor should favorable results lead to their easy acceptance. Even if all tests are passed, one must remember that a theory is made credible only in proportion to the variety and difficulty of the tests, and that no theory can ever be proved true.*

Efforts by political scientists to infer hypotheses from theories and test them have become commonplace. Much of the testing is done in basically the same way. One effort to test propositions, an effort more careful than most, can therefore serve as an illustration of how the above requirements go unobserved. Singer, Bremer, and Stuckey (1972) set out to evaluate "a number of equally plausible, but logically incompatible, theoretical formulations" about certain conditions that are said to be associated with peace and stability, or, alternatively, with war and instability. Having consolidated the "viewpoints" of the opposing "schools," they offer "predictive models" in which concentration of capability within the set of major powers, changes of that concentration, and changes of capability among the powers are the three independent variables. They then reach conclusions about whether and when the "parity-fluidity" model or the "preponderance-stability" model makes the better predictions. The questions asked are these: Will international politics be more or less peaceful and stable if power is more or less closely concentrated and if the ranking of great powers changes more or less rapidly? What can one make of the answers given? Very little. The deficiencies that account for this disappointing answer are revealed by running down our list of rules for the testing of theories.

Many testers of theories seem to believe that the major difficulties lie in the devising of tests. Instead, one must insist that the first big difficulty lies in finding or stating theories with enough precision and plausibility to make testing worthwhile. Few theories of international politics define terms and specify the connection of variables with the clarity and logic that would make testing the theories worthwhile. Before a claim can be made to have tested something, one must have something to test. In testing their models, Singer, Bremer, and Stuckey fail to examine the theories they have attempted to model. The theories the authors apparently have in mind are contradictory and confused about whether it is war and peace, or conflict and harmony, or instability and stability that are the expected alternative outcomes. One may, for example, think of a stable system as one that survives the waging of wars. Singer and his associates nevertheless finesse the question of what outcome should be expected by identifying war with

*For consideration of testing procedures and explanation of their importance, see Stinchcombe (1968, Chapter 2).

instability and letting it go at that. They fail to explain how their expectations accord with expectations derived from any particular theory.

The authors claim to be systematically and quantitatively evaluating contradictory "theoretical formulations." In gathering their data they necessarily fix upon certain definitions of the variables involved. As their key independent variable they choose concentration of power or of capabilities. They mention no theory that in fact employs such a variable, and I know of none that does. The well-known theories dealing with these matters refer to numbers of great powers or to polarities. "Polarity," moreover, is variously defined in terms of countries or of blocs. "Poles" are counted sometimes according to the physical capabilities of nations or of alliances, sometimes by looking at the pattern of national interrelations, and sometimes by awarding or denying top status to those who get or fail to get their ways. Unless the confused, vague, and fluctuating definitions of variables are remedied, no tests of anything can properly be conducted. The authors have nevertheless arbitrarily introduced their new variables without even considering how they may alter one's expectation of outcomes. Though this crucial problem is not even discussed, Singer and his associates announce that correlations between power-concentration variables, on the one hand, and war, on the other hand, confirm or disconfirm the expectations of the two schools they so vaguely refer to.

Rules one, two, and four are thus blithely ignored. The theories being tested are not stated. How hypotheses may have been inferred from them is not explained. Observations are made and data are generated without any effort to define variables as they were defined in the theories presumably being dealt with. The authors may be accomplishing something, but that something cannot be the confirming or disconfirming of any school's expectations.

In the face of such failures, one finds it hard to believe that here, as so often in the correlational labors undertaken by students of international politics, no thought is given to the possible presence of perturbing variables. An exception does not prove a rule or a theory, but if something can be shown to be exceptional, it does not provide any disproof either. One would expect variation in results achieved to prompt a search for possible sources of perturbation omitted from the models. In the instance before us, the "findings" for the nineteenth century differ from those for the twentieth. The discrepancy leads the authors only to the barest speculation about what may have been omitted and to no speculation at all about what may have gone wrong in the way variables were originally defined and interconnected. Rule five is no more heeded than the preceding ones.

Rule six calls for a number of different tests and for demanding ones. One might think this instruction more than usually important since the model consists merely of three highly similar and arbitrarily chosen variables and since the results of the tests are inconclusive. The dubious quality of the results, however, does not lead the authors to devise or to suggest further tests that might challenge their models with some force.

The seventh rule calls for care in the drawing of conclusions from the negative results of tests. Do they defeat the theory, require its amendment, or call for a narrowing of explanatory claims? Singer and his associates fail to consider such questions. Instead they simply report the different correlations between power-concentration and war in the nineteenth and twentieth centuries. Their conclusions are modest enough, but then what more could they say?

A general word of caution should be added to the many words of caution just uttered. One would be scientifically most satisfied if rigorous, experimental tests could be made. If a theory is stated in general terms, however, and if it gives rise to expectations that fall within a range that is identifiable but unfortunately wide, then to draw precise inferences and to try to check them experimentally is to place more weight on the theory than it can bear. Rigorous testing of vague theory is an exercise in the use of methods rather than a useful effort to test theory. The early application of demanding tests may, moreover, cause poorly developed theories to be discarded before their potential has unfolded (cf. Rapoport, 1968).

What then can one do? Simply negotiate the seven steps set forth above in ways appropriate to the theory at hand. Ask what the theory leads one to expect rather than fixing arbitrarily on expectations that one's data and methods can cope with. Check expectations against one's (often historical) observations before trying for precise refinements and using elaborate methods. Unless a theory is shown to be logical, coherent, and plausible, it is silly to subject it to elaborate tests. If a theory is seen to be logical, coherent, and plausible, the rigor and complication of tests must be geared to the precision or to the generality of the expectations inferred from the theory.*

III

I have dealt so far with the meaning of theory and with theory construction and testing. Theories do not emerge from efforts to establish laws, even when those efforts succeed. The construction of theory is a primary task. One must decide which things to concentrate on in order to have a good chance of devising some explanations of the international patterns and events that interest us. To believe that we can proceed otherwise is to take the profoundly unscientific view that everything that varies is a variable. Without at least a sketchy theory, we cannot

*See Chapter 6, part III, for further thoughts about testing.

say what it is that needs to be explained, how it might be explained, and which data, how formulated, are to be accepted as evidence for or against hypotheses (cf. Scheffler 1967, pp. 64–66; Lakatos 1970, pp. 154–77). To proceed by looking for associations without at least some glimmering of a theory is like shooting a gun in the general direction of an invisible target. Not only would much ammunition be used up before hitting it, but also, if the bull's-eye were hit, no one would know it!

The trick, obviously, is to link theoretical concepts with a few variables in order to contrive explanations from which hypotheses can then be inferred and tested. Our problem in the next two chapters is to see to what extent, and how well, this has been done by students of international politics.