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# THE LOGIC OF POLITICAL INQUIRY:

## A SYNTHESIS OF OPPOSED PERSPECTIVES

*J. DONALD MOON*

### *1 INTRODUCTION: ALTERNATIVE MODELS OF POLITICAL INQUIRY*

In any almanac or book of records one can find a vast number of indisputably "political" facts: the number of seats in different legislatures, the number who voted in various elections, the type of political system enjoyed by particular countries at particular times, etc. Although it cannot be doubted that these are facts about politics, no one would argue that their presence qualifies such compilations as works of political science. Even if such repositories of miscellaneous information were limited to specifically political facts, they still would not qualify. Political science differs from the activities of professional fact compilers by its systematic character and by its concern for the *explanation* of political phenomena. We do not simply assemble information; we seek coherent accounts of political life.

So much is obvious—a part of the familiar litany we hear on the occasion of a presidential address or read in our textbooks. The interesting and controversial issues concern how this coherence is achieved. What constitutes a satisfactory explanation? On what principles can the data of political inquiry be organized? On what grounds ought we to accept a particular finding? What support does a finding give to a generalization, or what inferences can we draw from it? What kinds of concepts are admissible in political inquiry? Questions such as these pose the fundamental methodological is-

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sues of political science and of the other social sciences as well. They have been widely and often hotly debated, since they strike at the very heart of what we are doing—or ought to be doing—as political scientists. The answers one gives to these questions define a particular model of political and social inquiry specifying what can count as political or social knowledge, rules governing the formation of admissible concepts, etc.

Perhaps the most popular methodological position in political science is one that might broadly be called the "naturalist"[1] or "scientific" model, for it seeks to structure political science in terms of the methodological principles of the natural sciences. Indeed, this view of political science might also be called the "positivist" model, since it is inspired at least in part by an essentially positivist account of the methodology of the natural sciences. Adherents of this model deny the existence of any fundamental methodological differences between the natural and social sciences. For both natural and social science, the goals of the "scientific enterprise" are the explanation and prediction of natural or social phenomena. In both areas of inquiry, moreover, scientific explanation consists in showing that the particular event or state of affairs to be explained could be expected, given certain initial conditions and the general laws or regularities in the field. For example, we might explain the freezing of a pan of water on a winter night by reference to the general law that water freezes when it is cooled below 32°F and the fact (or initial condition) that the temperature that night was lower than 32°F. (This is a variant of the example Hempel, 1942, uses to illustrate scientific explanation.) Laws or regularities, in turn, can be explained by deducing them from more general laws, or sets of laws, which are organized into comprehensive theories that have implications for a wide range of phenomena. These theories, by giving an account of a multitude of diverse kinds of events and situations, represent the crowning achievement of science, for they provide coherence to, and the systematic character of, a particular area of inquiry.

Although it is sometimes said—or more often suggested—that the scientific model is the only model of political inquiry capable of providing either unity to a field or genuine explanations of some phenomenon, this claim is clearly false. If "social science is . . . simply a way of being careful about studying human affairs" (Polsby *et al.*, 1963, p. 10), it is certainly not unique in that respect. In particular, there is an alternative methodological position which underlies what might be called the model of an interpretative social or political science. In opposition to the naturalist ideal, adherents of this model, whom I will, with some trepidation, call "humanists," insist on the methodological distinctiveness of the human sciences. The phenomena of the social sciences consist of human actions, and these are fundamentally different from the phenomena of the natural sciences, for they are constituted by the ideas and self-understandings of the social actors themselves. In the natural sciences we are free to create categories, define concepts, or group events in any way we please, so long as it helps us to advance our explanatory aims. In the social sciences we do not enjoy such freedom, for the human actions we would explain are intentional, which is to say that their very existence depends on the ideas that the actors themselves have. To describe someone as "saluting," to take a simple example, is possible only if the actor himself has certain conceptions of hierarchy and respect in terms of which this practice is defined; and to understand what he did when he "saluted," we must first grasp these notions. Hence the investigator must understand the meanings of the actions and social forms he is studying, and to do so he must interpret their significance in terms of the actor's intentions and in terms of the conventions and the fundamental conceptualizations of the actor's society. Thus the social scientist is necessarily committed to the hermeneutical or interpretative techniques and perspectives which are characteristic of the humanities, and this fact marks a fundamental break between the philosophy of the social sciences and the philosophy of the natural sciences.

In the next two sections of this chapter, I will present the central elements of these two models of political inquiry. In Section 2 I will outline the "scientific ideal," focusing on the nature of scientific explanation as it involves the use of laws and theories. In Section 3 I present a number of criticisms of the naturalist model and then outline the essential elements of the interpretative conception.

My main purpose in this chapter, however, is to attack the opposition between these two positions by showing that the claims of both are unfounded. If we must concede that the social sciences are methodologically distinct from the natural sciences, the differences are not so great as adherents of the hermeneutical model insist, for some of the things that happen in social life are things no one *does*, and so we cannot think of them simply in terms of the categories of intentional action. Of course, we must understand the conventions of a society to recognize such events as a stock market crash or a war, but since these events do not (necessarily) express anyone's intentions, an analysis of their meanings will not explain why they occurred. For such an explanation a methodology modeled on the natural sciences will be more appropriate. In particular, we will have to make use of generalizations and theories analogous to those of the natural sciences, in order to explain how the events in question result from the kinds of situations that precede them.

In the fourth section of this chapter, I develop these criticisms of the interpretative ideal and offer the outlines of a methodological position which represents a genuine synthesis of these two equally one-sided accounts. Following recent works in the philosophy of science which emphasize the conceptual presuppositions of scientific theories, I show that theories in the social sciences make certain implicit assumptions regarding human motivation, sociality, and rationality, and that these assumptions underlie both the interpretative understanding that the humanist seeks and the explanatory generalizations and theories to which the naturalist aspires. Since the principal criterion of adequacy of an essay of this type must be its ability to illuminate political inquiry, especially its capacity to account for the variety of forms political inquiry can take, I have illustrated the argument of this section with a number of examples drawn from the literature of political science. Although the conception of political inquiry I develop is rather schematic, I hope that it can contribute to a more adequate methodology of political science, providing a better account and understanding of the processes and results of political inquiry.

## 2 THE "SCIENTIFIC" IDEAL

### *2.1 Explanation, Theories, and Theory-Testing: An Overview*

At least since the time of Hobbes an important school of political research has endeavored to place the study of politics on a "scientific" footing. The attractiveness of such an undertaking is obvious. Indeed, the success of the natural sciences in providing explanations and descriptions of the world is so great that some have even taken science to be the paradigm of rationality: "science . . . is the only method available for responsibly assigning maximally reliable truth status [to statements]" (Gregor, 1971, p. 27). From such a perspective, the desirability of the scientific ideal requires little argument: to be concerned with what actually goes on in the political world, that is, to be interested in the empirical study of politics, requires that we follow the procedures of science.

Apart from its intellectual attractions, the scientific ideal answers to our practical interests as well. Hume, along with many classical thinkers, hoped to reduce politics to a science in order to moderate political conflicts. Once we understood the "general truths" of politics, Hume reasoned, many political disputes could be settled, and the factitious struggles which threaten to "change a good administration into a bad one" could be controlled or ended (Hume, 1952, p. 23). More ambitiously, Gabriel Almond sees the need for "an explanatory, predictive, and manipulative political theory" that can be used to solve "the problem of violence and coercion in human affairs" (Almond, 1967, pp. 7, 18). Science, by providing us with the causal laws or mechanisms which operate in a particular field, tells us which variables or conditions we must manipulate in order to achieve results that we desire: "Prediction implies the possibility of control. . ." (Falco, 1973, p. 55). Scientific theories are validated in the context of successful instrumental action, whether in experiments or in technological applications (Habermas, 1966; 1971, Part II). As our manipulations of the "independent" variables produce desired changes in the "dependent" variables, we justify our account of the underlying causal structure while advancing our control over the world. We might say that technological control is imminent in experimental procedure, and that science therefore holds the key to our acquiring greater control over our environment and over the processes of our own society.

Although the appeals of a scientific study of politics are clear, just what such a study requires is not so obvious. As our understanding of the methods and procedures of science has changed, our conception of a science of politics has also varied. Hobbes's vision, for example, featured an axiomatic deductive method modeled on geometry, whereas John Stuart Mill is celebrated for his emphasis on the primacy of observation and induction. At the present time there are a number of partially competing, partially overlapping accounts of scientific procedure that have gained acceptance among political scientists who have written on these issues. A central feature of most accounts, however, is the concept of scientific explanation, for the goals of understanding and control which the scientific enterprise advances are realized in the explanations science provides. The concept of explanation is also crucial, for it is this idea which separates the two models of political inquiry, the "scientific" and the "interpretative," which I am contrasting in this chapter.

The standard account of scientific explanation is known as the "covering-law" model. According to this model, scientific explanation requires generalizations or laws, and so most of Section 2.2 is devoted to an explication of the nature of these laws. The analysis of the role of laws in explanations however, leads directly to a discussion of scientific theories, for the conditions which must be imposed on laws in order to account for their explanatory force can be justified only by going beyond the laws themselves to a consideration of the theories in which they are "embedded," so to speak. In Section 2.3, therefore, I discuss the structure of scientific theories. I first outline the different uses of the term "theory" and then offer a very abbreviated account of the "orthodox" view of theories—the model of scientific theories which was developed by the logical empiricists and which has dominated much of the discussion of theories since. This model is briefly criticized in Section 2.3.2. The criticisms, which emphasize what might be called the "conceptual foundations" of scientific inquiry, lead at once to a consideration of the problems of testing and evaluating scientific theories, and these questions are taken up in Section 2.4. Finally, the argument is summarized in Section 2.5.

### 2.2 Scientific Explanation

To explain why something occurred is to show why, given the circumstances, it *had* to occur—to show that nothing else could have occurred under these conditions. If I want to know why an event, $p$, occurred and am told that, under these circumstances, $p$ or $q$ or $z$ might have happened, my curiosity will be some-what abated, but it will certainly not be satisfied—especially if the differences between the alternatives were significant. For example, one would be dissatisfied with an explanation of the fall of the French Fourth Republic in terms of its high level of governmental instability if the author of the account was quite prepared to admit that the Fourth Republic, like the Third, could have continued in spite of this disability, though at a declining level of performance. Surely we would be entitled to object that what we were promised was an account of why it *fell,* but all we were in fact told was that it was unstable.

Of course, to the extent that the range of possible events or outcomes has been limited, this account could be considered helpful. Moreover, it suggests a line of further inquiry: given the set of possible outcomes, what further factor(s) present in the situation led to the actual collapse of the system, rather than to its continued erosion? Carrying the example further, we might point to the threatened military revolt: it was this, given the background of ministerial instability, that precipitated the collapse. We could say that ministerial instability and the threatened coup jointly account for the fall of the Fourth Republic.

Quite apart from any question of its truth, this account is still not satisfactory since it does not show that the Fourth Republic fell *because* of the military coup in conjunction with ministerial instability. Obviously the collapse *followed* these earlier events or conditions, but if they are also to *explain* it, something more is required. And this "more" is a generalization or scientific law to the effect that if a political system has a high level of instability and is threatened by a military coup, it will collapse.[2] This generalization, together with the singular statements already provided, logically implies the statement that describes the event to be explained and thus shows that it was necessary. It is the generalization or law that provides the *force* of the "because" in our putative explanatory account and transforms it from a description of a mere sequence of events into a genuinely causal account.

It must be stressed that some generalization (or set of generalizations) is necessary if an account is to be explanatory—if we are to show that the event to be explained was necessary, given the initial conditions.[3] This is evident from a simple logical consideration: no set of singular statements denoting the initial conditions could logically imply the singular statements denoting the event to be explained, without the addition of some universal premise. From such statements as "This $x$ is a $y$" we could never deduce "This $x$ is a $z$" unless we could add the universal premise "All $y$'s are $z$'s." And it is essential that we deduce the (description of the) event to be explained (the "explanandum") from the description of the initial conditions if we are to say that the explanandum was necessary, given these conditions.[4]

Because of the critical importance of laws or generalizations in this analysis of scientific explanation, it is often called the "covering-law model" of scientific explanation or, simply, "nomological" explanation (from the

Greek "nomos," meaning "law"). Similarly, the generalizations which figure in such explanations may be called "nomological statements." One of the major issues regarding this pattern of explanation concerns the nature of these nomological statements. Although generalizations of some sort are required, it is obvious that not just any kind of general statement will do. Consider, for example, the following putative explanation of Canada's relatively painless progress to autonomy and independence.

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|---|---|
| Initial conditions: | Canada was a white (or white-ruled) British colony during the nineteenth century. |
| General law: | All countries that were white (or white-ruled) British colonies during the nineteenth century achieved independence without a prolonged struggle. |
| Explanandum: | Canada achieved independence without a prolonged struggle. |

Although this explanation looks superficially like an example of the covering-law model, the appearance is deceiving. The subject of the generalization in the example—the set of white-ruled British colonies between 1800 and 1900—contains only a finite number of examples, namely, Canada, Australia, New Zealand, and South Africa. Hence our "general law" is equivalent to the following series of statements (I have abbreviated the longer phrases):

Canada is a WBC and Canada achieved independence painlessly,

and

Australia is a WBC and Australia achieved independence painlessly,

and

New Zealand is a . . . ,

and

South Africa is a . . . .

When we put our "general law" in this form, it is obvious that the "explanation" in question is no explanation at all, because the conclusion of the argument simply repeats one of the premises. Far from being given an account of *why* Canada achieved independence painlessly, we are simply told *that* she did—once as a premise and once again as a conclusion!

The problem here is not that a proper name (British) appears in the putative "law," for there are other recognized laws which include proper names. Galileo's law mentions the earth (the acceleration of bodies in free fall in the vicinity of the earth is constant), and Kepler's laws specifically refer to the sun. But both laws can be interpreted as applying to an unrestricted class of objects which might satisfy the conditions of the law. Thus Galileo's law holds for any object which might become unsupported near the earth, and Kepler's laws enable us to predict that all planets which have been, are, or might come into the "vicinity" of the sun will revolve about it in elliptical orbits. What is essential is that a generalization be an unrestricted universal statement, that its subject term designate an open class of objects or instances. Only then can it be said to explain or predict its instances, rather than merely summarizing our observations. (See Nagel, 1961, p. 63; Hempel, 1965, pp. 342–343.)

Although universality in this sense may be a necessary condition of a law, it is hardly sufficient since some unrestricted generalizations may be merely "accidental." Consider two time-honored examples, "All swans are white" and "All crows are black." Apart from questions of truth or falsity, neither statement provides a satisfactory explanation of its instances because, one might say, there is no "necessity" to either of them. It is often held that, in order to provide a ground for extension to new cases, as a law must do if it is to provide an explanation of *any* case, a law should support subjunctive conditional statements, including statements contrary to known or presumed facts. For example, if it is a law that water, under normal pressure, vaporizes when heated, then we should be able to form the subjunctive conditional statement "If there were anything that is a sample of water and if it were heated, then it would vaporize." Now suppose that we had ascertained that some given sample of liquid was indeed water, and suppose that we then destroyed it by electrolysis; then this physical law should justify the counterfactual statement "If that sample of water had been heated under normal pressure, then it would have vaporized." Generalizations which do not support such conditionals may be called "accidental" generalizations, as opposed to laws. Because the generalizations about swans and crows do not support such conditionals as "If there were swans in Australia, they would be white," or "If there were crows in Anarctica, they would be black," we would not call them laws.

This test can be used to determine whether some of the generalizations which are or have been advanced in political science are actually laws. Consider, for example, the generalization[5] "If a democratic country has a single-member, plurality electoral system, then it will have only two electoral political parties." (See Duverger, 1950, for the classic discussion of this "sociological law" and Grumm, 1958, and Rae, 1971, for empirical tests of it.) If this is a sociological law, then it should permit us to say that if countries not now democratic were to become so, and if they were to have single-member electoral systems, then they would all have two-party systems. For example, writing in 1950, we might have said, "If India should adopt this electoral system, it will have only two electoral parties," or perhaps, "If (only!) France would adopt this electoral system, it would then experience (the blessings of) a two-party system." If we were unwilling to affirm such statements, we would betray our belief that this famous law was no law at all but merely an accidental generalization of some sort. It may be a "spurious correlation" or a result of the contingent fact that the logically and empirically possible combination of factors producing multipartyism had not occurred in countries which happen to have such electoral systems (such as, perhaps, a federal system marked by intense, regionally based cultural, linguistic, or religious cleavages).

The laws which figure in explanations, then, must be unrestricted universals, and they must "support" counterfactual and subjunctive conditionals. These requirements arise from the need to make sense of our intuitive idea that laws must apply to all possible cases and not simply reflect an "accidental" concomitance of events. If an explanation shows that the event in question *had* to occur, that it could not have been otherwise, then the generalization on which the explanation is based must not simply be a summation of some set of particular instances or express the coincidence that the empirically and logically possible combination of factors which would falsify the generalization does not happen to occur.

Because of these requirements, it is often held that laws must express some kind of "necessity," but the idea involved here is difficult to explicate. One unambiguous interpretation would be to hold that laws are *logically* necessary, i.e., that laws are necessarily true in view of the meanings of the terms they contain. In this case, laws would be analytic statements, and their truth would be guaranteed by the conventions of the language in which they are stated. Thus, if "All swans are white" is a scientific law, then finding a black bird which in all other respects resembles a swan would be grounds not for denying the "law" but for recognizing a new category of bird. Indeed, if laws were logically necessary, their denials would be self-contradictory, and no amount of empirical evidence could possibly bear upon their truth. This, however, is a major difficulty with this position, because it makes the empirical character of science problematic. If laws express "necessary truths," then, as Nagel (1961, pp. 53–54) points out, scientists should proceed like logicians and mathematicians. They should develop demonstrative proofs for their scientific laws, rather than setting up experiments or engaging in other forms of controlled observation.

Diametrically opposed to the logical-necessity or "conventionalist" view of laws is the Humean or "empiricist" position, according to which a law simply states the existence of some uniformity that happens to exist in the world but which is in no way "necessary." For example, in the Humean view, the law that water under normal pressure vaporizes when heated simply expresses the fact that all past, present, and future cases in which water under normal pressure is heated also happen to be cases in which the water vaporizes. But there are deep problems with this view as well, for it does not seem to meet the intuitive requirement that laws apply to all possible cases. In this view, it is difficult to see how laws can be said to "support" counterfactual or subjunctive conditionals. If laws simply express the fact that a particular relationship holds, how could they support our belief that, in possible instances which did not actually occur, the relationship *would have held*? For example, how could such laws support our belief that if we had heated a certain sample of water yesterday, it would have vaporized?

Empiricists are quick to recognize this difficulty, but they insist that it can be met by viewing laws and the explanations in which laws figure, in terms of their functions and places in scientific theories. Nagel (1961, pp. 68–73) argues that subjunctive and contrary-to-fact conditionals are asserted not simply on the basis of some particular law but on the basis of other laws and theories one accepts. One cannot propound a particular universal statement—all ravens are black—and then ask whether it supports a subjunctive conditional statement—if ravens were found in polar regions, they would be black—for even if we accept the statement as a law of nature, the conditionals it supports depend on the set of other laws, assumptions, and theories we accept. In this case, the conditional appears to conflict with some of our beliefs regarding the color of plumage, and so we would not assert it. But this does not mean that the putative law is not a law *because* it fails to support such conditionals, since it fails only to support *this* conditional, and the reason is that the "law" in question is only part of the evidence that bears upon our accepting or rejecting particular statements. Of course, we may still decide for other reasons that "All ravens are black" is not a law of nature, but such a decision will, once again, depend on the role this statement plays in the "system of explanation" constituting a particular science. Thus, to understand the structure of scientific explanation, we are led from a consideration of laws to a discussion of theories and the relationships between laws and theories.

### *2.3 Scientific Theories*

That adequate explanations ultimately rely on theories is a cardinal tenet of modern political science. Van Dyke expresses a widely shared belief when he argues that

> Explanation by reference to a . . . law is ordinarily incomplete. Among other things, it is incomplete in the sense that we are likely to want to know . . . why the law holds. For this purpose we seek a theory. Having explained the event by reference to . . . a law, we seek to explain the . . . law by reference to a theory. (Van Dyke, 1960, p. 41)

One of the reasons why so much attention has recently been paid to the development of theories (or "conceptual frameworks" which can provide the basis for the development of theories) is the general belief that only a systematic theory of politics, or some aspect of politics, will provide adequate explanations of an accumulating body of empirical material. For example, Deutsch (1963, pp. 3ff) describes the history of science as a dialectic of "philosophic stages," in which the development of theories is emphasized, and "empirical stages," in which theories are tested and refined. In this subsection I will present a brief characterization of the structure of scientific theories, and in the next subsection (2.4) I will discuss some of the issues involved in the testing and acceptance of a theory.

The subject of scientific theories has probably given rise to greater contention and dispute than has any other topic in the philosophy of science. To outline the contrasting positions regarding the structure and meaning of theories would require another essay of this length and would contribute little to an explication of the actual work of political scientists. It is possible, however, to discern three different uses of the term "theory" which are relatively unproblematic. The first use is clearly informal, as when the term "theory" designates a set of basic ideas about a subject—a fundamental conceptualization of a field or of a set of phenomena. In this sense, the term "theory" could be replaced by such words as "paradigm" (Kuhn, 1970b; Masterman, 1970), "research program" (Lakatos, 1970, 1971), or simply "conceptual framework." Conceptualizations of politics in terms of cybernetics (e.g., Deutsch, 1963), systems (e.g., Easton, 1965a, 1965b), functions (e.g., Almond, 1960; Almond and Powell, 1965), and the "rational choice" behavior of individual actors (e.g., Downs, 1957) are examples of "theories" in this sense. These fundamental conceptualizations play an important role in the testing and construction of theories, a role that will be discussed in Section 2.4 and Section 4 below.

A second informal use of "theory" is so vague that it might be replaced by "conjecture" or even "hypothesis." Any set of loosely articulated reasons for expecting a particular outcome may be called a "theory" in this sense. For example, one might have a theory that education is related to democratic stability, since "democracy" requires voting, which requires the ability to choose alternatives, and that ability is probably strengthened with education. This usage may be the most common, and I will occasionally use the term in this way, especially when constructing illustrations. No doubt, scientific theories may actually grow out of such vaguely formulated conjectures, but in this form they do not represent the systematic qualities which are required in a genuinely explanatory theory.

### *2.3.1 Hypothetico-Deductive Theories: The "Orthodox" View*

The third use of "theory" is more formal, referring to such well-developed, systematically related sets of propositions as the kinetic theory of gases or the special theory of relativity. Although there is little agreement regarding the analysis of these theories or regarding their relationship to informal theories, one very influential position is developed in the positivist or logical empiricist tradition within the philosophy of science. This conception can be called "the 'orthodox' view of theories," to use the title of the excellent article by Feigl (1970), which provides a remarkably succinct summary and exposition of this model and of the debate it has triggered. This view, which is offered as a "rational reconstruction" of theories and *not* as an account of how theories are actually discovered and developed, sees theories as consisting of an uninterpreted postulate system or "pure calculus," together with a set of semantical rules which provide an empirical interpretation for some of the symbols of the postulate system. An uninterpreted postulate system consists of a purely formal set of symbols or elements, a set of axioms, and a number of "transformation rules," or rules of inference. The set of symbols consists of two kinds of terms. Some of the symbols of this system are explicitly "defined" in terms of other symbols in the sense that they could be replaced by the other symbols in any formula in which they occur. Obviously, however, not all the elements of the system could be explicitly defined in terms of others, without at least some of the definitions being circular. Hence there must be a set of symbols which are undefined within the system, and these elements are called "primitives." Some of the symbols of the system, including all the primitives, will be used in formulating the axioms or postulates of the system. Then, with the use of the rules of inference which the system allows (such as the standard rules of elementary logic), "theorems" can be derived from these axioms. "Theorems," then, are the formulas which can be logically derived from the axioms of a system.

So far this construction is a purely logical affair—it consists of a completely formal set of symbols and the rules for manipulating them. But this sytem can be "interpreted" by semantical rules which assign empirical meaning to some of the symbols of the system. The semantical rules say that certain elements of the calculus are to designate certain objects in the world, such as empirical entitites, relations, etc. These rules are formulated in what is called a "metalanguage," which is generally some natural language, such as ordinary English. For example, suppose that we have a system composed of the following elements: *a, b, c,* and *R*. Let the system have an axiom which reads: $(aRb)$ & $(bRc) \rightarrow (aRc)$. This system can be interpreted by using the following semantical rules: *R* designates the relation "is preferred to," and *a, b,* and *c* designate positions a political party could take on an ideological spectrum. The axiom, then, would say that if one position is preferred to a second (e.g., "conservative" to "liberal") and the second to a third (e.g., "liberal" to "radical"), then the first is also preferred to the third (i.e., "conservative" to "radical").

Once semantical rules have been specified, the "pure calculus" becomes a system of *statements* and no longer simply a system of formulas. Now that the symbols have been given an interpretation, the "formulas" in which they figure are not simply abstract "formalisms" but are assertions about the world. An empirically interpreted calculus, then, is a theory.

It is important to note that, in this account, not all the elements of the uninterpreted system are provided with empirical interpretations or semantical rules specifying the observable properties, relations, or entities which the term is to designate. Some of the terms of a theory will not be assigned an empirical interpretation and are called "theoretical terms." Similarly, some of the sentences of the theory will be made up only of empirically interpreted terms, and so they will be equivalent to sentences in the observation language, or the language with which we describe the empirical world. Other sentences will include only theoretical terms, and so we can say that they will be sentences in the language specific to the theory, or the theoretical language. Finally, there will be sentences which include both kinds of terms. These sentences are called "correspondence rules," and they serve to provide a partial interpretation of the theoretical language by linking it to the observational language.

Because this "orthodox" view of theories draws a sharp distinction between the "theoretical" and the "observational" languages of a theory, it has been called the "dual language" conception of theories. In this view there are two crucial components of a theory: a set of theoretical principles and a set of correspondence rules which link the theoretical principles to observations, thereby providing an indirect or partial empirical interpretation to the theoretical principles. A theory of this type is what is often called a hypothetico-deductive system.[6]

Theories constructed on this model are said to "explain" laws by permitting us to deduce them from the basic principles of the theory.

> . . . empirical science raises the question "Why?" also in regard to the uniformities expressed by . . . laws and often answers it, again, by means of a deductive-nomological explanation, in which the uniformity in question is subsumed under more inclusive laws or under theoretical principles. (Hempel, 1965, p. 343)

A theory provides a higher "level of explanation" than laws provide, because it "construes . . . phenomena as manifestations of entities that lie behind or beneath them, as it were." These entities "are assumed to be governed by characteristic theoretical laws, or theoretical principles, by means of which the theory then explains . . . empirical uniformities" (Hempel, 1966, p. 70). In general, however, a theory will not enable us to deduce the precise form of the laws which had been discovered before the theory was developed. Rather, the theory will frequently show that the laws in question hold only within certain limits, and/or it will provide a more accurate version of these laws. Moreover, theories provide an account of how a number of diverse laws and types of phenomena are systematically related, and so we can say that they do much more than simply enable us to explain laws.

If the explanatory work of the theory is done by the theoretical entities and the theoretical principles a theory posits, the correspondence rules are equally vital. Without the correspondence rules, the theoretical principles would have no meaning, since they do not refer to observable things. Hence the correspondence rules are necessary if the theory is to have empirical import, because they link the theory to statements which can be tested by experiment and observation. Indeed, without correspondence rules the theory would have no explanatory power either, for the phenomena or laws we wish to explain are described or stated in the observation language, and so the theory could explain them only if there were some means to link the language of the theory to the language of our observations.

From this brief and unfortunately abstract discussion of the structure of scientific theories, it must be obvious that we do not have such theories in political science. There are no theories which have actually been put into axiomatic form, and with the possible exception of some "rational choice" theories borrowed from economics, none of our theories is even a candidate for such axiomatization. Nonetheless, the development of such theories represents the highest aspiration of the "scientific" ideal—what many hope will grow out of the loose formulations of basic ideas which presently constitute the "theories" of our discipline. Without theories such as these, the explanatory power and systematic coherence promised by the scientific ideal will not be realized.

### 2.3.2 *Some Criticisms of the Orthodox View of Theories*

Before we go on to a consideration of the process of theory-testing and acceptance, it is necessary to consider some of the criticisms that have been leveled against the orthodox view of theories. Some of these criticisms are important in that they bear upon the process of theory-testing, and because they have implications for the way in which "informal" theories are related to well-developed ones.

Although a great variety of objections have been urged against the received view of scientific theories, those I will consider here all emphasize what might be called the "conceptual presuppositions" of scientific theories, or scientific inquiry in general, and they all bear upon the distinction between the "theoretical" and "observational" terms of a theory. Spector has shown that this distinction does not withstand analysis, for these categories are not mutually exclusive. He shows that there are a number of distinct uses of the terms "observable" and "unobservable" which "make different points about a term or entity. No one of these [is] such that all and only those terms which [adherents to the orthodox view] have called observable will turn out to be observable, nor will they be such that direct meanings can be given only for those terms which turn out to be observational" (Spector, 1966, p.

3). Moreover, the same is true of the notion of "theoretical terms" (Spector, 1966, p. 89).

In a similar vein, Hempel has argued that the orthodox view of theories should be reformulated. Theoretical principles should not be viewed as an uninterpreted calculus but as the "internal principles" of a theory, and correspondence rules should be reformulated as "bridge principles" linking the internal principles to an antecedently understood vocabulary, which may include previously understood theories.

> . . . the phenomena to which bridge principles link the basic entities and processes assumed by a theory need not be "directly" observable or measurable: they may well be characterized in terms of previously established theories, and their observation or measurement may presuppose the principles of those theories. (Hempel, 1966, p. 74)

Moreover, theoretical terms will often be understood before they are used in a new theory, since they will have been used in other theories, and therefore it cannot be said that they derive their meanings simply from the correspondence rules of the new theory. Hence "it is misleading to view the internal principles of a theory as an uninterpreted calculus and the theoretical terms accordingly as variables, as markers of empty shells into which the juice of empirical content is pumped through the pipelines called correspondence rules" (Hempel, 1969, p. 31).

If theories are not composed of two distinct kinds of terms, then the testing of a theory is rendered problematic. In particular, if the bridge principles do not link theoretical concepts to observables, and if theoretical terms can be given meaning directly, then it would be possible to use a theory to criticize and reject the observational statements themselves. But the observational statements are supposed to be independent of a theory so that they can be used to test it. If they are not independent but are subject to theoretical criticism, then how can we ever test a theory? And since the explantory power of a theory is dependent on its being testable, the very status of theories in the scientific enterprise seems to be called into question. This issue is taken up in the next subsection.

## 2.4 The Choice and Testing of Theories

### 2.4.1 The Difficulty of Falsifying a Theory

Political scientists have often been critical of the "theories" in our discipline because they have been "informal," consisting of sets of basic ideas about, or conceptualizations of, politics. Such informal theories may be vital to the discovery of genuine theories, but they are no substitute for the latter, because they are not testable, and therefore they cannot be explanatory.

> The complex theoretical structures of Plato, Aristotle, Aquinas, Harrington, Hobbes, Hegel, and Marx attest to no lack of imaginative abilities. What have been lacking have been the casting of theory into universal, empirically falsifiable form, and the provision of criteria of falsification. (Goldberg, 1963, pp. 34–35)

Theories are genuinely explanatory only if we can use them deductively to infer statements cast in observational terms, which can then be seen to be true or false. If such observable consequences cannot be deduced from a theory, it is not explanatory. In the absence of such consequences, the theory could not be used to show that some phenomenon could be expected (given the initial conditions), and so it could hardly be said to "explain" the event in question.

The particular conception of theory-testing involved here stresses the "falsification" of theories. According to this view, theories can be *rejected* by empirical tests, since theories entail observational statements which can be found to be false, and this finding implies that the theory itself is false. But a theory cannot be completely confirmed in this way, even if all its observational consequences are found to be correct—roughly because a false statement (or a set of statements, some of which are false) may validly imply a statement that happens to be true, as in the example, "If the Sears Tower in Chicago is over two miles tall, then it is the tallest building in the world." Once a theory has been proposed and has been shown to have observational consequences that are false, the theory must be rejected or at least reformulated. Furthermore, according to this view, it is necessary to put theories to increasingly severe tests, for it is only in this way that we can discover a theory's limitations and see where it must be changed in order to strengthen it.[7]

Unfortunately, this whole conception of how theories are tested is thrown into doubt by the considerations presented in Section 2.3.2 above. If theories do not consist of sharply distinguished observational and theoretical vocabularies, then any test of the "internal principles" of a theory is also a test of the various theoretical assumptions and principles which are part of the observational or experimental component of the theory. Hence it is always possible to save a theory by arguing that "observational" statements describing the alleged counterexample are themselves defective, and so they cannot be used to refute the internal principles of the theory.

Such moves can take two basic forms. One is to argue that there are factors extraneous to the theory operative in the situation in which the theory is tested, and so this situation does not satisfy the assumptions of the theory. For example, one might have a theory of political coalitions which predicts that only groupings of a certain sort will form. If, in a given instance, one finds that the prediction fails, the theory could be saved by

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discovering a further factor, such as imperfections in the structure of communication in the test situation, which shows that the theory is not applicable to this case. Hence what appeared to be a refutation turns out to be an irrelevancy.

A second move is to deny the truth of the observational statement which contradicts the theory. This can be done in several ways. One is to deny some of the theoretical principles which are part of the observational language in question. For example, if one has a "theory" that an increase in political participation by people with authoritarian personalities will undermine the stability of a democratic system by increasing the strength of extremist parties, and if this theory is not borne out by studies correlating such participation and stability, then one might save the political theory by rejecting the psychological theory used to measure or classify people as authoritarians.

A radical variant of this move would be to deny what are apparently purely "factual" statements—those which seem only to report our "rock-bottom" observations and not to "interpret" them at all. Even these elementary statements, however, do not constitute a "fancyless medium of unvarnished news" (Quine, 1960, p. 2), for they must make use of the conceptual structure of our language. What we "see"—in the sense of what statements we are prepared to affirm or deny in response to a particular stimulus—is already structured by the conceptual system we hold. Even these "rock-bottom" statements reflect "natural interpretations" (Feyerabend, 1970a, pp. 48ff), a structuring of perception (or, rather, perception-reports) so immediate that we are not aware of any structuring at all. But we can come to be aware of these natural interpretations by criticizing them from the vantage point of an alternative set of concepts, which may be the framework of a scientific theory. Hence, instead of rejecting a theory in response to certain factual statements we make, we might decide to revise our "factual" statements in light of our new theories. For example, we might come to say that the sun only *appears to rise,* rather than that it *rises*.[8] (See, e.g., Hanson, 1958, p. 5 and Chapters 1 and 2.)

It must be emphasized that it is not open to us to get around these problems by declaring such methods of saving a theory to be *ultra vires,* for the theories which underlie our observations may be wrong, and the theories which we have proposed may be correct. The desire to ensure the empirical character and testability of our theories may prompt us to forbid these strategems, but such a move would be self-defeating. Not only would this suggestion have foreclosed a number of the most important scientific developments, but it would also effectively preclude the criticism of those elements of our observational language which may genuinely be in need of criticism. It would remove from empirical investigation and control the auxiliary sciences and theories needed to generate observational statements bearing on a theory under test, and it would therefore reduce the empirical content of science by investing our observational language with an unquestionable, "metaphysical" status. (See Feyerabend, 1963, 1965; Lakatos, 1970, pp. 93–116.)

Enough has been said to indicate that the problem of falsifying a theory or finding evidence that supports it is extremely difficult. Indeed, that problem raises fundamental questions regarding the cognitive status of science and whether or not rational grounds can ever be given for the choice of one particular theory over another. Instead of pursuing those issues,[9] however, I will present the solution to this problem which has been developed by Lakatos. In addition to showing that there are rational grounds on which theories can be evaluated, it has the merit of bearing upon the kinds of theories we actually have in the social sciences.

### 2.4.2 *The Criticism and Choice of Theories*

At first glance, the discussion above seems to have left us in a real dilemma. On the one hand, it seems to be possible to save a theory from refutation by using any one of a number of possible strategems, and the use of these strategems cannot be ruled out without reducing the empirical content of our knowledge. But on the other hand, if there is no way to refute a theory, then it must have lost its empirical character since its immunity from refutation can be purchased only at the cost of depriving it of any determinate implications regarding specific, observable events, processes, entities, or whatever.

In order to escape this dilemma, one must recognize that a theory is not abandoned simply because it is inconsistent with some well-confirmed observations or data. To give it up is rational only when there is a better theory to replace it. Tests of theories "are—at least—three-cornered fights between rival theories and experiment," not confrontations of isolated theories with particular facts. Indeed, inconvenient facts are never allowed to "shoot down" elegant theories, for all theories are "born refuted" (Lakatos, 1970, p. 121n). All theories face an "ocean of anomalies" (Lakatos, 1970, p. 135) or data which, at least superficially, contradict the theory, but such evidence bears on our decision to reject a theory only when we have a better theory at hand.

This observation enables us to shift our attention from the problem of evaluating or testing an individual theory to that of evaluating or testing a series of theories. Placed in this context, criteria for evaluating such a series can be stated which preserve the testability and empirical character of theories, but which do not ignore or dismiss the various strategems that can be used for "saving" them. These new criteria will "impose certain standards on the theoretical adjustments by which one is allowed to save a theory," thereby providing room for using such strategems when that use can be justified.

The basic standard governing our evaluation of a series of theories is that each subsequent theory in the series should enable us to predict "some novel, hitherto unexpected fact," and that at least some of these predictions should be corroborated. A series of theories which satisfies this criterion can be called "progressive," and a series which fails to do so can be called "degenerating." This idea can be illustrated by returning to the examples presented earlier. In one case I suggested that a theory of coalition formation might be saved from refutation by introducing a new variable, "communication imperfections," into the description of the test situation, thereby showing that it failed to meet the assumptions of the theory. According to the criterion developed here, this strategem would be permissible only if adding this variable to the theory would enable us to predict new facts. Otherwise, it would be prohibited as a "content-decreasing" move which must be rejected as pseudo-scientific. Similarly, in the case of the theory relating increased participation by "authoritarians" to political instability, any attempt to save the theory by rejecting the psychological theory on which the measures of "authoritarianism" are based would be progressive only if it led to a new theory which not only removed the anomaly in question but also predicted additional findings, such as other features of the behavior of "authoritarians" or, possibly, other consequences of their political participation. In both of these cases we are concerned with evaluating possible theoretical adjustments. Until one of the adjustments we try "works," there is no need to reject the theory—at least, there is no need to reject it simply on the grounds that it contradicts known facts.

The latter example is of additional interest since it bears directly on the idea of a theory being "confronted" with and tested by the "facts." In this case and in all such cases, this is a misleading formulation—at least, once we have given up the orthodox conception of theories which sharply distinguishes between "observational" and "theoretical" statements. What is to be counted as the "facts" depends on the perspective we decide to adopt. If we focus on the theory of instability, the factual statements are those describing the effects of increased participation by authoritarians. But those statements amount to two different sets of claims. One is that certain individuals behaved in certain ways, and the other is that they had certain characteristics, $c_1$, $c_2$, etc., which imply that they are authoritarians. Hence, accepting those statements as "factual" amounts to accepting the psychological theory according to which characteristics $c_1$, $c_2$, etc., are indicators of authoritarianism. If instead we should decide to accept the political theory as "factual," or "correct," then the statements describing the behavior of those particular individuals would serve to refute the psychological theory. Hence, what we have here is an inconsistency between two theories, in light of certain observations.

> . . . experiments do not simply overthrow theories, [and] no theory forbids a state of affairs specifiable in advance. It is not that we propose a theory and Nature may shout NO; rather, we propose a maze of theories, and Nature may shout INCONSISTENT. (Lakatos, 1970, p. 130)

This conflict or inconsistency is resolved by determining which adjustment in either or both theories leads to the greatest increase in empirical content.

By setting up the standard of increasing empirical content to govern theoretical adjustments, and by conceptualizing the problem of theory-testing in terms of evaluating a series of theories, we preserve the empirical—and therefore explanatory—character of scientific theories. By developing standards which we can use to evaluate successive or proposed theories, we avoid the dilemma which the criticism of the orthodox view of theories posed. We no longer need to choose between, on the one hand, blocking off some areas of empirical inquiry in order to preserve some statements as "factual" so that they can be used in testing theories and, on the other hand, permitting our theories to become trivial by opening up the flood gates to a multitude of strategems for saving a theory. Since we are not trying to test a single, isolated theory but are evaluating a series of theories, we can permit the floodgates to open and allow all theory-saving strategems to be used. But we insist that these moves count as creating a new theory, and we insist that this new theory have a greater empirical content than the original one.

The demand that we continuously increase the empirical content of our theories may, at first glance, seem to get us out of one thicket only to land us in another. Although it bars both those ploys (usually verbal reformulations) which simply remove an anomaly without predicting new facts, and those theoretical adjustments which predict new "facts" that turn out to be wrong, it fails to stop those theoretical adjustments which simply amount to adding another hypothesis to a theory when that hypothesis has no real relationship to the rest of the theory. For example, this standard does not rule out a "research strategy" which consists in selecting one's "independent variables" from a correlation matrix simply with a view to maximizing the "variance explained" in the dependent variables.

In cases such as these, the theoretical adjustment is merely "empirical" or "formal"; it provides us with a better "fit" between theory and data but at the cost of increasing the "complexity" of the theory to the same degree that the fit is improved. Thus the resulting theory fails to provide unity and coherence to a field by representing diverse phenomena as the result of a few basic, underlying theoretical principles. Since this is one important purpose of scientific theories, our standards for evaluating theories must be strengthened to prevent such (non)theoretical growth by agglomeration. Lakatos has formulated such standards in terms of what he calls "research programs."

### 2.4.3 *Scientific Research Programs*

The basic way to prevent theories from growing as a result of the piling up of a number of unrelated hypotheses is to require that there be some continuity between them. This rather vague requirement can be clarified in terms of the idea of a "research program." A new theory which has more empirical content than the preceding one will be acceptable only if it meets one of two conditions: (1) it is developed in accordance with the same underlying program of research as the preceding theory, or (2) it launches an altogether new program. From this point of view, a research program "consists of methodological rules: some tell us what paths of research to avoid (*negative heuristic*), and others what paths to pursue (*positive heuristic*)" (Lakatos, 1970, p. 132; emphasis in original). These rules reflect or provide an "implicit definition of the conceptual framework" or the "language" of the scientific theory. That is, the research program may be said to define the fundamental conceptualization of the phenomena within an area of study and the system of concepts and basic principles which are to be used in ordering and explaining those phenomena. Lakatos provides several examples of research programs in the natural sciences, including Newton's gravitational theory. In the political and social sciences there are several such research programs, or "proto-research programs," since some have yet to be very well defined. Perhaps the most successful is the conception of politics in terms of the "rational choice" model, a program I will analyze in some detail in Section 4. Similarly, "functionalism," "group theory," Marxism, and "systems theory" could all be considered research programs, or nascent research programs, for they have provided the conceptual frameworks for some research within political science.

Lakatos argues that research programs may be characterized by a "hard core," which he calls the "negative heuristic," and a "protective belt" of auxiliary hypotheses and theories, which he calls the "positive heuristic." The negative heuristic consists of those basic assumptions which the theorist refuses to change, irrespective of the evidence. For example, the hard core of Newtonian mechanics, according to Lakatos, consisted of the "three laws of dynamics and the law of gravitation" (Lakatos, 1970, p. 133). In the rational choice research program, the hard core consists of the conceptualization of social phenomena in terms of the rational choices of individual actors responding to the structure of incentives they face (see Section 4). The negative heuristic comprises the theorist's basic ideas about a field, and if a test goes against a theory, or if mathematical or logical difficulties arise in the course of articulating a theory, the theorist will refuse to make adjustments in these basic principles, but will alter some feature(s) of the protective belt in order to "save" the hard core. One of the factors that make a series of theories continuous, then, is that each theory in the series has the same "hard core" as the others.

The "positive heuristic" or "protective belt" is the second component of a research program. Whereas the negative heuristic tells us what assumptions or principles are *not* to be altered, the positive heuristic provides, as it were, the "plan" for future research on the program. Any theory or research program is always subject to a host of apparent counter-examples or anomalies, and it is impossible to deal with all of them at once. Rather, they must be attacked in an orderly fashion, and it is the function of the positive heuristic to create that order by providing directives to guide research. From this perspective, the positive heuristic "consists of a partially articulated set of suggestions or hints on how to change, develop the 'refutable variants' of the research-program, how to modify, sophisticate, the 'refutable' protective belt" (Lakatos, 1970, p. 135). By working on a research program in accordance with the positive heuristic, the theorist will generate "a chain of ever more complicated models simulating reality," in which each successive model is developed from the preceding one by altering some of the restrictive or simplifying assumptions of the earlier model in accordance with the directives of the positive heuristic. In the case of the Newtonian research program, for example, Newton began with a highly simplified model of the solar system consisting of only one pointlike planet orbiting the fixed pointlike sun, and as he developed a satisfactory analysis of this case, he relaxed these assumptions and developed a series of ever more complex models. In a similar fashion, Downs's theory of political action in a democracy began with a highly simplified model of perfectly informed parties, and those restrictive assumptions were gradually relaxed until a reasonably complex, explanatory theory emerged.

A research program can be called "progressive" so long as work in accordance with the positive heuristic continually yields theories which are progressive. But eventually a point will be reached where the program begins to "run out of steam." Theoretical innovations will be proposed which are degenerating, and it will become increasingly difficult to save the hard core of the program without resort to various ad hoc strategems. When this occurs researchers will—and ought to—gradually abandon the program in favor of one which is, at the time at least, progressive.

The hard core and the protective belt of a research program, then, constitute the continuities between successive theories which serve to rule out the use of auxiliary hypotheses which reflect a different way of conceptualizing the subject matter in question. This conception of a research program gives substance to the second principal criterion for evaluating, testing, and choosing theories. We require that successive theories, in the first place, have increased empirical content in the sense that later theories predict facts which are not predicted by earlier ones, and that some of these predictions be confirmed. Second, we require either that successive theories be continuous in the sense that they develop within the same research program, or that a new theory launch a new research program altogether. Finally, when we evaluate competing research programs, we judge one program to be better than another if it is generating a series of theories which are progressive, whereas the other is plagued with degenerating theories. These criteria provide us with standards which can be used in rationally evaluating theories, but which recognize that no theory can be conclusively falsified. Although there are certain difficulties with Lakatos's program,[10] and no doubt it will be amplified in the future, it is sufficient to demonstrate the testable, and therefore explanatory, character of scientific theories.

### *2.5 The Scientific Ideal: Summary and Conclusions*

In this section I have presented an outline of some important features of the methodology of the natural sciences. I have focused on the interrelated problems of scientific explanation and the empirical import or testability of scientific theories. These are certainly among the features which make scientific methodology so attractive to students of society and politics. Moreover, those who reject the naturalist model of social and political inquiry generally focus on the covering law pattern of explanation. They argue, as I shall show in the next section, that social and political inquiry requires a *different* model of explanation—one that is equally empirical but does not require laws or generalizations and does not lead to the development of theories. In order to appreciate this position, then, it is essential to outline as fully as possible the main features of the covering law model of explanation, and to show that it is related to other aspects of the scientific ideal.

The nomological pattern of explanation, as its name implies, requires the presence of general laws in any explanatory account. When we say that one thing—for example, military aggressiveness—occurs because of something else—for example, rapid economic growth—we require a general law linking these factors in order to "provide force" to the "because." But not just any kind of general statement can perform this explanatory function. Not only must laws be unrestricted universals (i.e., they must be in universal form and must apply to an unrestricted class of objects), but they must also support "counter-to-fact" and subjunctive conditional statements. For example, if it really is a law that rapid economic growth leads to military aggression, then we should be willing to say that if England's rate of economic growth had been a few points higher, she would have started World War I. But to make such an assertion requires a great deal more information than that conveyed by a particular law, and so in order to understand the explanatory force of laws, we had to examine them in relation to scientific theories.

The term "theory" is used in a number of different ways, and even when it refers to the fairly well articulated theories of the natural sciences, there is considerable controversy regarding their logical structure. The received or orthodox view of theories has the merit of being reasonably well specified, but it makes what appears to be an untenable distinction between "theoretical" and "observational" concepts. And when this distinction is dropped, the process of theory-testing becomes problematic. Once we admit that even our "rock-bottom" observation statements are conceptually structured and can therefore be criticized, we realize that "facts" cannot serve as an independent basis on which to judge "theories." This does not mean that our choice of scientific theories can be made in a purely subjective manner, however, and I outlined Lakatos's argument showing that there are rational standards for testing and evaluating theories. These standards presuppose the concept of a "research program," or a fundamental conceptualization of some phenomena, together with the basic concepts and principles which are used in ordering and explaining them. They require that theories be developed within such research programs in order to avoid an essentially ad hoc agglomeration of hypotheses passing as thoretical progress. Then, theoretical development within a research program is to be governed by the demand that new theories have more empirical content than the theories they replace. Finally, a research program can be evaluated by the extent to which it gives rise to a progressive series of theories.

Not only does this concept of a research program show that theories can be tested and therefore that they can have explanatory import, but it also helps us to see the manner in which science provides unity and coherence to a field of inquiry. Scientific theories represent a range of diverse phenomena and regularities as the manifestations of a small number of theoretical entities and their interrelationships, and they do so in terms of a conceptual structure with "heuristic power," which provides a basis for the further articulation and development of theories of even greater scope. This systematically progressive nature of science is one of its greatest attractions. But some have argued that the methods of science are unable to account for the kinds of phenomena which the social and political scientist seeks to understand. Their arguments and their ideal of political inquiry are examined in the next section.

## 3 THE INTERPRETATION AND EXPLANATION OF POLITICAL ACTION

### 3.1 *The Distinctive Character of Social Phenomena*

In this section I will outline the "interpretative" model of political inquiry, a methodological position opposed to the naturalist conception. The funda-

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