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CLOUDS, CLOCKS, AND THE STUDY OF POLITICS

By GABRIEL A. ALMOND and STEPHEN J. GENCO*

IN its eagerness to become scientific, political science has in recent decades tended to lose contact with its ontological base. It has tended to treat political events and phenomena as natural events lending themselves to the same explanatory logic as is found in physics and the other hard sciences. This tendency may be understood in part as a phase in the scientific revolution, as a diffusion, in two steps, of ontological and methodological assumptions from the strikingly successful hard sciences: first to psychology and economics, and then from these bellwether human sciences to sociology, anthropology, political science, and even history. In adopting the agenda of hard science, the social sciences, and political science in particular, were encouraged by the neopositivist school of the philosophy of science which legitimated this assumption of ontological and meta-methodological homogeneity. More recently, some philosophers of science and some psychologists and economists have had second thoughts about the applicability to human subject matters of strategy used in hard science. It may be useful to bring these arguments to the attention of political scientists.

POPPER'S METAPHORS

Karl Popper, who along with R. B. Braithwaite, Carl Hempel, and Ernest Nagel has argued the thesis of meta-methodological homogeneity, more recently has stressed the heterogeneity of reality, and its unamenability to a single model of scientific explanation. He uses the metaphor of clouds and clocks to represent the commonsense notions of determinacy and indeterminacy in physical systems. He asks us to imagine a continuum stretching from the most irregular, disorderly, and unpredictable "clouds" on the left to the most regular, orderly, and predictable "clocks" on the right. As the best example of a deterministic system near the clock-extreme, Popper cites the solar system. Toward this end of the continuum we would find such phenomena as pendulums, precision clocks, and motor cars. As an example of a system near the other, indeterminate, end of the continuum, he cites a cluster of gnats or small flies in which each insect moves randomly except that

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it turns back toward the center when it strays too far from the swarm. Near this extreme we would find gas clouds, the weather, schools of fish, human societies and, perhaps a bit closer toward the center, individual human beings and animals.

The Newtonian revolution in physics popularized the notion—which was to persist for approximately 250 years—that this commonsense arrangement was in error. The success of Newton's theory in explaining and predicting a multitude of celestial and earthbound events by his laws of motion led most thinkers—although not Newton himself—to embrace the position that the universe and all its parts were by nature clocklike and in principle completely predictable. Phenomena that had the appearance of indeterminacy were viewed as being merely poorly understood; in time, they also were expected to be found regular and predictable. Thus, the reigning model of science after Newton affirmed that all nature was governed by deterministic laws or, to put it in Popper's metaphor, "*all clouds are clocks*—even the most cloudy of clouds."¹

In the 1920's, the development of quantum theory challenged this clocklike model of nature and supported the view that indeterminacy and chance were fundamental to all natural processes. With this discovery, Popper's metaphor was inverted; now the dominant view held that "to some degree *all clocks are clouds*; or in other words, that *only clouds exist*, though clouds of very different degrees of cloudiness."² Many scientists and philosophers greeted this change of model with relief, since it seemed to free them from the nightmare of determinism that denied the efficacy of human choices and goals.

But Popper goes on to argue his central point, that "*indeterminism is not enough*" to account for the apparent autonomy of human ideas in the physical world. "If determinism is true, then the whole world is a perfectly running flawless clock, including all clouds, all organisms, all animals, all men. If, on the other hand, Pierce's or Heisenberg's or some other form of indeterminism is true, then sheer *chance* plays a major role in our physical world. But is chance really more satisfactory than determinism?"³

Popper answers in the negative. Although physicists and philosophers have tried to build models of human choice based upon the unpredictability of quantum jumps,⁴ he rejects these as being too circumscribed.

¹ Karl R. Popper, "Of Clouds and Clocks: An Approach to the Problem of Rationality and the Freedom of Man," in Popper, *Objective Knowledge: An Evolutionary Approach* (Oxford: Clarendon Press 1972), 210; emphasis in original.

² *Ibid.*, 213; emphasis in original.

³ *Ibid.*, 226; emphasis in original.

⁴ Arthur H. Compton, *The Freedom of Man* (New Haven: Yale University Press 1935).

He acknowledges that “the quantum-jump model may be a model for . . . snap decisions. . . . But are snap decisions really so very interesting? Are they characteristic of human behavior—of *rational* human behavior?” He concludes: “I do not think so. . . . What we need for understanding rational human behavior—and indeed animal behavior—is something *intermediate* in character, between perfect chance and perfect determinism—something intermediate between perfect clouds and perfect clocks. . . . For obviously what we want is to understand how such non-physical things as *purposes, deliberations, plans, decisions, theories, intentions, and values*, can play a part in bringing about physical changes in the physical world”⁵

Popper’s method of arriving at a solution to this problem seems, like the problem itself, to be relevant to politics and political science. His conjecture is that the problem is essentially one of *control*; i.e., the control of behavior and other aspects of the physical world by human ideas or mental abstractions. Thus, he states that “the solution must explain freedom; and it must also explain how freedom is not just chance but, rather, the result of a subtle interplay between *something almost random or haphazard*, and *something like a restrictive or selective control*—such as an aim or standard—though certainly not a cast-iron control.” Accordingly, he restricts the scope of acceptable solutions to those that “conform to *the idea of combining freedom and control*, and also to *the idea of ‘plastic control,’* as I shall call it in contradistinction to a ‘cast-iron’ control.”⁶

Popper reaches an evolutionary solution to this problem—one that stresses trial and error elimination, or variation and selective retention.⁷ Only such a theory can accommodate plastic control, and thus human freedom. Once this is seen, the problem of the relationship between ideas and behavior becomes solvable: “For the control of ourselves and of our actions by our theories and purposes is *plastic* control. We are not *forced* to submit ourselves to the control of our theories, for we can discuss them critically, and we can reject them freely if we think that they fall short of our regulative standards. Not only do our theories control us, but we can control our theories (and even our standards): there is a kind of *feedback* here.”⁸

Popper concludes: “We have seen that it is unsatisfactory to look upon the world as a closed physical system—whether a strictly deter-

⁵ Popper (fn. 1), 228, 229; emphasis in original.

⁶ *Ibid.*, 231-32; emphasis in original.

⁷ See Donald T. Campbell, “Variation and Selective Retention in Socio-cultural Evolution,” *General Systems Yearbook*, XIV (1969).

⁸ Popper (fn. 1), 240-41; emphasis in original.

ministic system or a system in which whatever is not strictly determined is simply due to chance; on such a view of the world human creativeness and human freedom can only be illusions. . . . I have therefore offered a different view of the world—one in which the physical world is an open system. This is compatible with the view of the evolution of life as a process of trial-and-error elimination; and it allows us to understand rationally, though far from fully, the emergence of biological novelty and the growth of human knowledge and human freedom.”⁹

Thus Popper tells us that the models of explanation appropriate to the physical sciences will not enable us to come to grips with human and cultural phenomena, and that while we can increase our understanding of them, we cannot explain them fully because of their creative and emergent properties.

THE ONTOLOGICAL PROPERTIES OF POLITICS

Popper’s essay presents us with three ways of conceptualizing social reality—as a clock, as a cloud, and as a system of plastic controls. Political reality, which it is the task of political science to explain, is clearly best captured by the third conceptualization. It consists of ideas—human decisions, goals, purposes—in constant and intense interaction with other ideas, human behavior, and the physical world. At the center of this complex system are *choices and decisions*—decisions to command, obey, vote, make demands. The political universe has organization; elites make decisions to command or not to command, what to command, how to implement commands. Citizens and subjects make decisions to comply, how to comply or not to comply; to make demands, how to make demands, or not to make demands. That is the heart of politics, the subject matter our discipline is committed to exploring and understanding.

The relations among these events are not simply reactive, as are the encounters of physical objects; they are not readily amenable to cause-and-effect “clocklike” models or metaphors. Basically, this is because the behavioral repertoires of elites and citizens are not fixed repertoires. The actors in politics have memories; they learn from experience. They have goals, aspirations, calculative strategies. Memory, learning, goal seeking, and problem solving intervene between “cause” and “effect,” between independent and dependent variable.

Political decisions are not made and implemented in a vacuum; they are subject to a complex array of *constraints and opportunities*. These

⁹ *Ibid.*, 254-55.

constraints—the necessities of politics—range from the relatively hard variety represented by environmental or ecological limits to the quite soft variety illustrated by passing fashions and fads. Constraints define the “operational milieu” of political actors¹⁰ and exhibit varying degrees of manipulability. Some, like geography or the level of technology, are difficult to alter even in the long run; in the short run, they are practically nonmanipulable. Others, like cultural values and public opinion, are relatively easy to manipulate in some circumstances, more intractable in others. But manipulation is very rarely impossible in principle. Even relatively hard environmental constraints—such as the relation between material resource needs and population—can sometimes be altered as a consequence of man’s creative, adaptive capacities. The agricultural revolution some 10,000 years ago multiplied by many times the number of people capable of being sustained in a given space, and the industrial revolution of the last two centuries multiplied it by many times again.

These ontological properties of political affairs are plain for all to see; they are not matters on which reasonable persons can differ. Social scientists who—for whatever philosophical or methodological reasons—deny them and view human behavior as simply reactive and consequently susceptible to the same explanatory logic as “clocklike” natural phenomena are trying to fashion a science based on empirically falsified presuppositions. That becomes clear when their explanatory schemes are thought of in terms of their own behavior as scientists. Insofar as they acknowledge the importance of scientific memory, scientific creativity, calculative strategies, goal seeking, and problem solving in their own work, they must in some degree acknowledge these qualities in the human and social material they investigate and seek to explain.

The implication of these complexities of human and social reality is that the explanatory strategy of the hard sciences has only a limited application to the social sciences. Models, procedures, and methodologies created to explore a world in which clocklike and cloudlike characteristics predominate will capture only a part of the much richer world of social and political interaction. Thus, a simple search for regularities and lawful relationships among variables—a strategy that has led to tremendous successes in the physical sciences—will not explain social outcomes, but only some of the conditions affecting those outcomes.

Because the properties of political reality differ from those of physical

¹⁰ Harold Sprout and Margaret Sprout, *The Ecological Perspective on Human Affairs* (Princeton: Princeton University Press 1965).

reality, the properties of political *regularities* also differ from those of physical regularities. The regularities we discover are soft. They are soft because they are the outcomes of processes that exhibit plastic rather than cast-iron control. They are imbedded in history and involve recurrent “passings-through” of large numbers of human memories, learning processes, human goal-seeking impulses, and choices among alternatives. The regularities we discover appear to have a short half-life. They decay quickly because of the memory, creative searching, and learning that underlie them. Indeed, social science itself may contribute to this decay, since learning increasingly includes not only learning from experience, but from scientific research itself.

The softness and historical boundedness of political theories can be illustrated by a few examples. Political scientists are justifiably proud of their theory of voting behavior. It is the closest thing to a scientific theory that we have. It has generated a set of what appear to be “covering laws”—demographic and attitudinal correlates of the voting decision, inductively arrived at. The deductive Downsian model of the consequences for party systems of different distributions of voter attitudes looks like an even more basic law of politics. But even a casual review of the findings of voting research in the last thirty years shows how unstable these regularities are, and how far short of hard science our efforts to stabilize them must inevitably fall. Modern research on voting behavior made its greatest progress in studies of American elections in the 1950’s and early 1960’s, a period of rapid economic growth and low-intensity politics. Students of American voting behavior in that period maintained they could explain and predict American voting behavior on the basis of “party identification” and “candidate image”; issues seemed to play only a secondary role.¹¹ The result of this effort to produce a hard causal explanation was a psychological theory of voting behavior based on party identification and candidate image. But this theory was soon to be challenged by studies done in the early 1970’s which include data from the 1930’s and late 1960’s. These earlier and later periods show American voters as making their choices on the basis of candidates’ issue positions to a far greater extent than was true of the 1950’s and early 1960’s. Recent writers speak of the “decomposition” of the party system, of the individuation of voting behavior, and of the “ideologization” of American politics.¹² And one of the leading

¹¹ Angus Campbell and others, *The Voter Decides* (Evanston, Ill.: Row Peterson 1954); Campbell and others, *The American Voter* (New York: Wiley 1960).

¹² Norman Nie, Sidney Verba, and John R. Petrocik, *The Changing American Voter* (Cambridge: Harvard University Press 1976), 345ff; Walter Dean Burnham, *Critical Elections and the Mainsprings of American Politics* (New York: Norton 1970).

collaborators of the Michigan group which produced the original party-identification theory now acknowledges that the demographic and attitudinal correlates of voting behavior are only loosely related, and that the only kind of theory we can aspire to is "some orderly specification of the conditions under which they vary."¹³

Political socialization theory is still engaged in a futile effort to impute relatively fixed values and weights to agents of socialization—family, school, workplace, media of communication, adult experiences, and the like.¹⁴ Like voting research, socialization research in its thrust toward parsimonious scientific explanation has overlooked the larger historical context and the inherent instability of variables. Jennings and Niemi,¹⁵ in one of the most sophisticated studies of political socialization ever undertaken, report that the impact of parents and teachers on the political attitudes of high school seniors was surprisingly weak. They failed to register the fact that the high school seniors they were sampling were the class of 1965, the first cohort of the post-World War II baby boom. It was a generation which to a considerable extent socialized itself, and it turned socialization theory upside down in the late 1960's by providing the cultural innovators of the youth rebellion. Like voting behavior theory, socialization theory is now slowly acknowledging the inherent instability of variables. The impact of the agents of socialization varies with changes in demographic and social structure, technology, and political events and issues. All that we can aspire to is a collection of propositions specifying the conditions under which these impacts tend to vary.

Perhaps the most vulnerable of these thrusts into hard science were the efforts of students of American politics in the early 1960's to discover the relationships between politics and public policy. The problem had been set by earlier work which argued that characteristics of the political system—party competition, voter participation, apportionment, and the like—had important consequences for public policy as measured by the level of public expenditures, and particularly by welfare expenditures. A series of statistical studies comparing the political, economic, and public policy characteristics of the American states in the 1950's and early 1960's proceeded to demonstrate that these political

¹³ Philip E. Converse, "Public Opinion and Voting Behavior," in Fred I. Greenstein and Nelson W. Polsby, eds., *Handbook of Political Science*, IV (Boston: Addison-Wesley 1975), 126.

¹⁴ For a recent review of the literature, see David O. Sears, "Political Socialization," in Greenstein and Polsby (fn. 13), 93ff.

¹⁵ M. Kent Jennings and Richard G. Niemi, *The Political Character of Adolescence* (Princeton: Princeton University Press 1974).

variables had little independent impact on the policy variables. When controlled for level of economic development, the effect of these political differences was washed away. This finding led to the remarkable conclusion that economic and other environmental variables explain public policy much better than political variables.¹⁶

There are two aspects of this research in public policy that are noteworthy for our purposes. The first is the extraordinary constriction of the time and space perspectives in this effort to test a global proposition concerning the relationship between economics, politics, and public policy. The fact that these were the American states in the 1950's—a period of political stability—rather than in the 1930's, did not register as limiting the kinds of inferences that could be drawn. Political scientists studying these problems brought no historical perspective to bear on their research—no memories of war, revolution, and depression, and of their well-known relationships to politics and public policy. Second, there was no recognition of the fact that environmental variables cannot directly produce public policy, that political choice must in the nature of the case intervene between them, and that historically this intervention has been very large indeed.

Social mobilization theory has sought to explain and predict trends toward politicization, democratization, and de-ideologization from trends toward urbanization, industrialization, communication, and education—only to discover that when these relationships are examined historically, human intractability and inventiveness, as well as sheer chance, complicates these patterns enormously.¹⁷ The prophet of the end of ideology¹⁸ has become the prophet of the postindustrial society¹⁹ and, currently, the prophet of social disjunctions and cultural exhaustion.²⁰ Social scientists are finding that they do a better job of explaining when they follow the course of history, using sophisticated methodologies to isolate necessary sequences and constraints, but always aware of the role of chance and human inventiveness in producing the outcomes they are seeking to explain.

In their fascination with powerful regularities and uniformities that have the properties of causal necessity or high probability, social scien-

¹⁶ See Thomas R. Dye, *Understanding Public Policy* (Englewood Cliffs, N.J.: Prentice-Hall 1972), 243-48, for a review of this literature and a fuller formulation of these findings and inferences.

¹⁷ For a review of this literature, see Gabriel A. Almond, Scott C. Flanagan, and Robert J. Mundt, eds., *Crisis, Choice and Change* (Boston: Little Brown 1973), 8ff.

¹⁸ Daniel Bell, *The End of Ideology* (New York: Free Press 1960).

¹⁹ Daniel Bell, *The Coming of Post-Industrial Society* (New York: Free Press 1973).

²⁰ Daniel Bell, *The Cultural Contradictions of Capitalism* (New York: Basic Books 1976).

tists have overlooked the fact that much of social and political change has to be explained neither by strong regularities nor by weak regularities, but by accidental conjunctions—by events that had a low probability of occurring. The concatenation of particular leaders with particular historical contexts is a matter of chance—of fortune—rather than necessity. Scholars can explain why Russia was ripe for revolution in 1917; and they can explain some aspects of Lenin's personality and operational code; but they cannot explain *why* the two conjoined to produce the Bolshevik Revolution, only *that* they conjoined by chance. The problem is similar to that of the biologist seeking to explain the emergence of a new species. He can describe an ecological niche in terms of constraints and opportunities; but for the niche to be occupied, the chance occurrence of an appropriate mutation or set of mutations is required.

Although in some respects the problem is similar to that of the biologist, it differs in fundamental ways. The interplay between the constraints of the ecological niche and the randomness of the process of mutation, to be sure, is a matter of trial and error. The search process is a random one, and largely genetic. In human affairs, the search process in addition has important conscious, planful aspects. It involves not only the chance concatenation of a revolutionary political niche with a Lenin, but with a scheming, contriving, willing, improvising Lenin, constantly probing, testing, and learning about the constraints and opportunities within the niche he is striving to occupy. Once he does occupy it, he transforms the niche and the population occupying it in ways that will constrain (but again not *determine*) future adaptive efforts. If we are to understand political reality, we have to come to grips not only with its determinate aspects but, most particularly, with its creative, adaptive, problem-solving aspects. For it is this last characteristic which is the essentially human property, and which is the unique mechanism and explanatory challenge of the social sciences.

THE CLOCK MODEL OF POLITICAL SCIENCE

The now dominant, "behavioral" tradition in political science tends to rest on three epistemological and methodological assumptions which it has taken from the hard sciences: (1) that the purpose of science is the discovery of regularities in, and ultimately laws of, social and political processes; (2) that scientific explanation means the deductive subsumption of individual events under "covering laws"; and (3) that the only scientifically relevant relationships between events in the world are those which correspond to a physicalistic conception of

causal connection. These assumptions are highly interrelated, and each carries important substantive implications for the study of politics.

(1) The emphasis on generalizations in political science must first be understood in historical context. When David Easton argued in 1953 that “knowledge becomes critical and reliable when it increases in generality and internally consistent organization, when, in short, it is cast in the form of systematic generalized statements applicable to large numbers of particular cases,”²¹ he was speaking against a tradition of ideographic, descriptive, noncumulative, and institutional case studies that had dominated much of the discipline (with a few notable exceptions) for several decades. A similar concern animated the behavioral polemics of Truman and others in the early 1950’s.²² The long-term result of this praiseworthy attempt to shift emphasis from description to explanation, however, has been the enshrining of the notion of generalization as the *sine qua non* of the scientific aspirations of the profession. This is perhaps most readily apparent in the recently burgeoning “scope and methods” literature. For example, Scarrow, in his *Comparative Political Analysis*, announces that “Generalizations are the hallmark of all scientific endeavor,”²³ while Conway and Feigert, in *Political Analysis: An Introduction*, declare that “the function of science is generally perceived as being the establishment of general laws or theories which explain the behavior with which the particular discipline is concerned.”²⁴ Even a sophisticated study, such as Przeworski’s and Teune’s *Logic of Comparative Social Inquiry*, states somewhat dogmatically that: “The pivotal assumption of this analysis is that social science research, including comparative inquiry, should and can lead to general statements about social phenomena. This assumption implies that human and social behavior can be explained in terms of general laws established by observation. Introduced here as an expression of preference, this assumption will not be logically justified.”²⁵

The substantive impact of this emphasis on generalizations is to focus the attention of research on regularities, uniformities, and stable

²¹ Easton, *The Political System* (New York: Knopf 1953), 55.

²² David B. Truman, “The Impact on Political Science of the Revolution in the Behavioral Sciences,” reprinted in Heinz Eulau, ed., *Behavioralism in Political Science* (New York: Atherton 1969).

²³ Howard A. Scarrow, *Comparative Political Analysis: An Introduction* (New York: Harper & Row 1969), 33.

²⁴ Margaret Conway and Frank B. Feigert, *Political Analysis: An Introduction* (Boston: Allyn and Bacon 1972), 17.

²⁵ Adam Przeworski and Henry Teune, *The Logic of Comparative Social Inquiry* (New York: Wiley 1970), 4.

patterns of association in political processes at the expense of unique or low-probability events or political outcomes. As Frohock expresses it in *The Nature of Political Inquiry*, “Science is concerned with establishing causal relations and general laws. To do this the social scientist must concentrate on systematic patterns of human conduct. Only as an event is a recurring instance of a general class can it be treated scientifically.”²⁶

We are not arguing here for the view that regularities do not occur in political processes or that valid generalizations cannot be made. As we noted above, political regularities—albeit soft—clearly exist and are crucial to political inquiry. Rather, our criticism is aimed at positions that see regularities and generalizations as the *only* proper objects of scientific political inquiry. This seems to us an unnecessary delimitation of the scope of the discipline’s subject matter. If political reality is best viewed as a conjunction of choice and constraint, and as a source of both regularity and innovation, then political science should not be limited to a consideration of only part of this reality. A pure focus on generalizations as “the hallmark of all scientific endeavor” would seem to condemn it to just such a limitation.

(2) The concern with generalizations and regularities—and the concomitant willingness to limit the scope of political science to only those aspects of political reality that are generalizable—is closely associated with a particular conception of *explanation* in political inquiry. This position is also reflected in the “scope and methods” literature. Alan Isaak, in his *Scope and Method of Political Science*, declares that political scientists must accept the “scientific fact of life” that “every sound explanation and prediction contains at least one generalization; without generalizations there could be no explanations or predictions.”²⁷ Similarly, Conway and Feigert argue that “Explanations in science require . . . laws or theories which are well established. . . . Explanation occurs when the facts to be explained can be deduced as a logical consequence of the laws or theory and . . . other known facts.”²⁸

The model of explanation alluded to here is the so-called “covering law” or deductive-nomological (D-N) model developed in the philosophy of science by R. B. Braithwaite,²⁹ Carl Hempel,³⁰ and others. The

²⁶ Fred M. Frohock, *The Nature of Political Inquiry* (Homewood, Ill.: Dorsey 1967), 141.

²⁷ Isaak, *The Scope and Method of Political Science* (Homewood, Ill.: Dorsey 1969), 80.

²⁸ Conway and Feigert (fn. 24), 27.

²⁹ Braithwaite, *Scientific Explanation* (Cambridge: Cambridge University Press 1953).

³⁰ Hempel, *Aspects of Scientific Explanation* (New York: Free Press 1965); see also Ernest Nagel, *The Structure of Science* (New York: Harcourt, Brace and World 1961).

basic idea underlying this model is that something is explained when it has been shown to be a member of a more general class of things. "To explain something is to exhibit it as a special case of what is known in general."³¹ This is achieved, according to the model, when the particular case is deduced from a more general law (or set of laws) that "covers" it and all other relevantly similar cases. That is why generalizations play such a fundamental role in deductive explanations.

The explanatory power of the D-N model derives from the fact that deduction from covering laws *logically necessitates* that which is deduced. The deduction "explains" by telling us that, on the basis of what we already know (the generalization), the case in question was to be expected: it *had* to occur the way it did.³² This notion of "it was to be expected" stands at the center of the deductive conception of explanation, and accounts for the close association between explanation and prediction in the model.³³ For adherents of the D-N model, an explanation that would not be equally capable of supporting a prediction would not qualify as a true explanation.³⁴ It is not surprising, therefore, that closed deterministic systems—"clock models" in Popper's terminology—are most amenable to D-N explanation. As Hempel puts it: "The best examples of explanations conforming to the D-N model are based on physical theories of deterministic character. . . . [T]he laws specified by such a theory for the changes of state are deterministic in the sense that, given the state of that system at any one time, they determine its state at any other, earlier or later, time."³⁵

It is clear that the D-N model loses its usefulness to the degree that there are *exceptions* to the law or laws warranting the explanation in question. If we cannot legitimately maintain that "all *A*'s are *B*'s" and must settle for a law asserting only that "some *A*'s are *B*'s," then the deductive link is dissolved and our explanation of the occurrence of *B* continues to be problematic. This state of affairs, however, is just what is implied by the notion of plastic control. Plasticity means that we can

³¹ Abraham Kaplan, *The Conduct of Inquiry* (San Francisco: Chandler 1964), 339.

³² *Ibid.*

³³ Paul Diesing, *Patterns of Discovery in the Social Sciences* (Chicago: Aldine Atherton 1971), 164.

³⁴ See Hempel (fn. 30), 367, where this position is maintained while its obverse—that a valid prediction must also qualify as an explanation—is put aside. This modification of the so-called "symmetry thesis of explanation and prediction" has not always been appreciated by political scientists. See, e.g., Oran Young, "The Perils of Odysseus: On Constructing Theories in International Relations," in Raymond Tanter and Richard Ullman, eds., *Theory and Policy in International Relations* (Princeton: Princeton University Press 1972), 183.

³⁵ Hempel (fn. 30), 351; see also Nagel (fn. 30), 323.

expect, in principle, that there will be exceptions to any generalizations we might form about the phenomena that are of interest to us. Thus, the more our subject matter exhibits plastic control, the less it will be amenable to simple D-N explanations.

(3) The notion of causality is closely associated with the idea of covering-law explanation by both political scientists and philosophers of science. R. B. Braithwaite, for example, describes causality strictly in terms of covering laws: “[T]he statement that some particular event is the effect of a set of circumstances involves the assertion of a general law; to ask for the cause of an event is always to ask for a general law which applies to the particular event.”³⁶

This formulation is echoed by political scientists. Thus, Robert Dahl argues that “If we wish to explain an event, *E*, in a strictly causal manner, we consider *E* as an effect and bring it under some generalization of the form: ‘Every event *C* is accompanied later by an event *E*.’ . . . The *C* is called the cause, *E* the effect.”³⁷ Similarly, Isaak maintains that, “If saying that ‘*A* causes *B*’ is tantamount to ‘*B* always follows *A*,’ then they are both reducible to ‘If *A*, then *B*.’ In other words, we can express what is traditionally known as a causal relationship without using the term cause.”³⁸

All of these characterizations rest on the notion of causality as an *explanatory* concept. But how is this explanatory status acquired? As can be seen from even a cursory exposure to the literature on causation and conditions,³⁹ the concepts “cause” and “effect” are broad and ambiguous. One element of their meaning seems to stand out in any account, however: the principle of “same cause, same effect.”⁴⁰ As Hempel puts it, “as is suggested by the principle ‘same cause, same effect,’ the assertion that [a given set of] circumstances jointly caused a given event implies that whenever and wherever circumstances of

³⁶ Braithwaite (fn. 29), 2; see also Hempel (fn. 30), 348-49.

³⁷ Dahl, “Cause and Effect in the Study of Politics,” in Daniel Lerner, ed., *Cause and Effect* (New York: Free Press 1965), 87.

³⁸ Isaak (fn. 27), 95.

³⁹ See, e.g., Ernest Sosa, *Causation and Conditionals* (Oxford: Oxford University Press 1975); Myles Brand, ed., *The Nature of Causation* (Urbana: University of Illinois Press 1976).

⁴⁰ There are many disputes concerning the philosophical status of causality that go well beyond this consensual element of its meaning—for example, the problem of whether the causal connection represents a constant conjunction, logical necessity, or “natural” necessity; and the problem of the temporal ordering and contiguity of causes and effects. For a discussion of these in terms relevant to political science research, see Georg Henrik von Wright, *Explanation and Understanding* (Ithaca, N.Y.: Cornell University Press 1971).

the kind in question occur, an event of the kind to be explained takes place."⁴¹ Or, in Abraham Kaplan's slightly more cautious formulation: "Causal connection is usually analyzed in terms of some relation of implication: the grammar of the 'if-then' conjunction is at least a starting point. If the cause occurs, then its effects occur."⁴² It is this element of "same cause, same effect" that confers explanatory power on causal relations in the world. Without it, "causality" becomes simply another problematic and essentially unexplained relationship between two or more things, events, or processes.

This philosophical characterization of the relationship between cause and effect is closely related to Popper's notion of cast-iron control. The cause *produces* the effect, and the existence of the cause is the *explanation* of the effect. A world of pure cause and effect, as narrowly defined by this identification of causality with covering-law explanation, would be a world without exceptions, a world that could not be other than what it is. Such a world, we feel, is completely alien to the world of politics, in which the potential for surprise and innovation is inherent in many, if not most, situations.

In spite of the inflexibility and aridity of the explanatory concept of causality, however, many political scientists have attempted to couch their analyses of political phenomena in terms of the notions of cause and effect. The result is often an odd mixture of formalized definitions and unrelated empirical substance. As an example of such a mixture, we might take a brief look at one branch of political analysis that has made considerable use of causal formulations—the literature on the concept of power. Here the relationship of cause and effect is explicitly invoked as a metaphor for a necessary, dependent connection between events. For example, Herbert Simon has stated that "for the assertion 'C has power over R,' we can substitute the assertion 'C's behavior causes R's behavior.' If we can define the causal relation, we can define influence, power or authority, and *vice versa*."⁴³ Similarly, Andrew McFarland asserts that "definitions of power or influence based on such concepts as force, incentives or utilities, and minimum winning coalitions are . . . reducible to causal terms."⁴⁴ More recently, Jack Nagel has defined power as follows: "A power relation, actual or potential, is an actual or potential causal relation between the preferences of an actor regarding an out-

⁴¹ Hempel (fn. 30), 348-49.

⁴² Kaplan, "Noncausal Explanation," in Lerner (fn. 37), 146.

⁴³ Simon, *Models of Man* (New York: Wiley 1957), 5.

⁴⁴ McFarland, *Power and Leadership in Pluralist Systems* (Stanford: Stanford University Press 1969), 29.

come and the outcome itself.”⁴⁵ And Robert Dahl, in the latest edition of his *Modern Political Analysis*, seems to maintain (although with some caveats) his long-standing view that causation is fundamental to understanding power and influence relations: “when we single out influence from all other aspects of human interaction in order to give it special attention, what interests us and what we focus attention on is that one or more of the persons in this interaction get what they want, or at least get closer to what they want, by causing other people to act in some particular way. We want to call attention to a *causal relationship* between what *A* wants and what *B* does.”⁴⁶

How is the word “cause” being used in these definitions? Clearly it is not being used as an explanatory concept, in the sense described by philosophers of science. For an explanation to be truly causal in that sense, as we have seen, the relationship in question would have to be (1) cast-iron, (2) generalizable, and (3) amenable to covering-law explanation. None of these properties would seem to apply to power relationships. There is no “necessity” inherent in the outcome of an attempt to assert power over another person, as there is in a causal connection between two physical objects. The target of the power attempt may, for any number of reasons, act differently than the power wielder would have him act. This is because a power relationship does not involve cast-iron control; instead, it is an interaction of two choosing and mutually constraining individuals, each with his own resources, goals, purposes, interests, and strategies. The intentions and resources of the first certainly constrain the choices and actions of the second, but they do not determine those choices and actions in any sort of cast-iron sense.

This “looseness of fit” between the behavior and intentions of actors involved in an attempt to exercise power means that their relationship is not readily generalizable; neither is it particularly amenable to strict covering-law explanation. As Hart and Honoré have put it: “The statement that one person did something because . . . another threatened him, carries no implication or covert assertion that if the circumstances were repeated, the same action would follow; nor does such a statement require for its defense, as ordinary causal statements do, a generalization. . . .”⁴⁷ These considerations lead us to conclude that the power

⁴⁵ Nagel, *The Descriptive Analysis of Power* (New Haven: Yale University Press 1975), 29.

⁴⁶ Dahl, *Modern Political Analysis* (3rd ed.; Englewood Cliffs, N.J.: Prentice-Hall 1976), 30; emphasis in original.

⁴⁷ H. L. A. Hart and A. M. Honoré, *Causation in the Law* (Oxford: Clarendon Press 1959), 52.

relationship is not causal, at least not in the explanatory sense of the term.⁴⁸

This conclusion would seem, in one sense, to be shared by Dahl and many of the other political scientists who use causal language in their definition of power. If we examine their empirical analyses of power relations in real-world political situations rather than their definitions, we find careful and precise examinations of the complex interactions that contribute to outcomes, without reliance on simplistic notions of “same cause, same effect.” In such substantive analyses—as opposed to definition making—plasticity is recognized and indeterminateness is often handled with sophistication and insight.

What we seem to observe in this particular area of political research, then, is a rhetorical or metaphorical—rather than explanatory—usage of causal language in formalizations and definitions. This accounts for the lack of a subsequent commitment to actual causal analysis in substantive research. The somewhat incongruous gap can perhaps best be explained as an attempt on the part of political scientists to create a “halo effect” around their theoretical formulations. Our longing for full scientific status has led us to create a kind of “cargo cult,” fashioning cardboard imitations of the tools and products of the hard sciences in the hope that our incantations would make them real.

These three elements of the implicit logic that informs much of political science research today appear to imply a substantive model of the political world which closely resembles the deterministic “clock model” outlined by Popper. That is not to say that any political scientists actually *see* the political world this way; no doubt we would all agree that it often *appears* to be quite porous, irregular, and unpredictable. Rather, it is to say that the arsenal of meta-methodological principles and procedures we have borrowed from the physical sciences—or, more correctly, from a certain philosophical perspective on the physical sciences—has come to us with an array of substantive assumptions that all proclaim the principle “all clouds are clocks.” If we search only for generalizations and regularities in political processes, if we couch our explanations only in terms of the covering-law model, and if we view political relationships as ultimately causal in nature, we are committing ourselves—whether we recognize it or not—to a disciplinary research program designed to strip away the cloudlike and purposive aspects of political reality in order to expose its “true” clocklike structure. If politics is *not* clocklike in its fundamental structure, then

⁴⁸ For further arguments along similar lines, see Terence Ball, “Power, Causation and Explanation,” *Polity*, VIII (Winter 1975), 189-214.

the whole program is inappropriate. We believe this to be the case: the current quandary in political science can to a large extent be explained by the fact that, by themselves, "clock-model" assumptions are inappropriate for dealing with the substance of political phenomena.

THE ADOPTION OF THE CLOCK MODEL AND ITS EFFECTS ON POLITICAL
RESEARCH AND PEDAGOGY

The movement toward hard science in the study of politics is a phase in the scientific revolution of the last several decades. The great breakthroughs in physics and biology, and the extraordinary increases in research funding as science became a national asset, created a mood of sanguine expectations. It is not surprising that political scientists sought to share in this exciting and remunerative adventure.

Political science was invited to imitate the hard sciences by some of the more influential philosophers of science on the grounds that political reality lent itself to the same powerful methods that had proven so effective in physics and biology. That is one of the basic tenets of the logical positivist tradition in the philosophy of science,⁴⁹ and has been a starting point for many books and articles designed to show the social sciences and history how to achieve a "truly" scientific status.⁵⁰ In addition, there was immediate evidence of the success of the hard-science strategy within the social sciences themselves. Psychology and economics had been the first disciplines in the social sciences to move in this direction, demonstrating the possibilities of experimental methods, sophisticated quantitative methods, computer simulation, and mathematical modelling. The combination of philosophical legitimation and the demonstrated progress of psychology and economics was impossible to resist.

As a consequence of these legitimations and demonstration effects, the incentive structure of political science began to encourage an orientation modelled on the physical sciences. The pressures for conformity can be measured in terms of prestige, journal publications, fellowships, and grants. Major sources of research funding and graduate fellowships, such as the National Science Foundation, have been dominated by the hard sciences; the social science divisions have been junior

⁴⁹ See von Wright (fn. 40), chap. 1.

⁵⁰ See, e.g., Nagel (fn. 30); Hempel (fn. 30), chap. 9; May Brodbeck, "Explanation, Prediction, and 'Imperfect' Knowledge," in Herbert Feigl and Grover Maxwell, eds., *Minnesota Studies in the Philosophy of Science: Vol. 3* (Minneapolis: University of Minnesota Press 1962); Richard S. Rudner, *Philosophy of Social Science* (Englewood Cliffs, N.J.: Prentice-Hall 1966); Rudner, "Comment: On the Evolving Standard View in Philosophy of Science," *American Political Science Review*, Vol. 66 (September 1972).

partners in these agencies, and the political science section has been the most junior of all. Projects that have the appearance of hard science have had the inside track for gaining substantial research support.

Perhaps the most important consequence of this imitation of hard science has been an emphasis on method as the primary criterion for judging the quality of research in political science. Today, the leading research traditions tend to be defined by their methodologies rather than their substantive foci. One result of this principle of organization—although certainly not a necessary consequence of it—has been that the value of this work seems to be measured primarily by its technical virtuosity, and only secondarily by the importance of the problems treated or illuminated.

In the last two decades there has been a tremendous drive toward quantification in political science. Riker celebrated this trend in a recent communication to the *American Political Science Review* when he commented that some two-thirds of the articles in recent issues of that journal were based on quantitative analysis employing sophisticated statistics.⁵¹ Quantification has undoubtedly contributed to major advances in political science and other social sciences. But it has also led to a significant number of pseudo-scientific exercises that exhibit the form but not the substance of research in the physical sciences. Such studies become more prevalent when the use of quantification is treated as an end in itself rather than as a means toward understanding concrete political problems. Irrelevant quantification has recently been the subject of searching critiques in international relations,⁵² comparative politics,⁵³ policy studies,⁵⁴ and elsewhere.

Quantitative analysis in political science has moved increasingly toward more sophisticated statistical methods. But the structure of the data in social science research often comes into conflict with the assumptions underlying confirmatory statistical theory. The problems involved in applying complex statistical methods to nonrandom, nonlinear, or nonadditive data should not be minimized.⁵⁵ Much of the

⁵¹ William H. Riker, quoted in "Editorial Comment," *American Political Science Review*, Vol. 68 (June 1974), 733-34.

⁵² Edward R. Tufte, "Improving Data Analysis in Political Science," *World Politics*, xxi (July 1969).

⁵³ Andrew Mack, "Numbers Are Not Enough," *Comparative Politics*, vii (July 1975).

⁵⁴ Ralph E. Strauch, "A Critical Look at Quantitative Methodology," *Policy Science*, ii (Winter 1976).

⁵⁵ See, e.g., Hayward R. Alker, "The Long Road to International Relations Theory: Problems of Statistical Nonadditivity," *World Politics*, xviii (July 1966); Hubert M. Blalock, "Correlated Independent Variables: The Problem of Multicollinearity," in Edward R. Tufte, ed., *The Quantitative Analysis of Social Problems* (Reading, Mass.: Addison-Wesley 1970).

inferential power of these methods is lost when the structure of the data does not conform to the rigid requirements of the theory. These difficulties have proven formidable enough to lead some statisticians, such as John Tukey, at Princeton, to devise alternative data-analytic techniques that, although not nearly as powerful as the most advanced statistical methods, are more compatible with the idiosyncratic characteristics of social and political data.⁵⁶ Here we seem to have fallen into a trap comparable to that of the early phases of Third-World development when “high technologies” were introduced into poor agricultural countries without regard for their disruptive consequences. We are discovering that an intermediate level of statistical technology, which takes into account the special characteristics of social data, is more appropriate to the social sciences than are the very sophisticated methods.

Running parallel to this emphasis on statistics in political science is an interest in mathematics and the construction of simple, logically rigorous models. This approach has been advocated in comparative politics by Holt and Richardson, who argue that “political scientists must turn to mathematics” if the discipline is to progress scientifically. They are careful to distinguish this path from the statistical one: “In making an appeal for more mathematics, we are not talking about statistics. . . . [S]tatistics provides a science with a basis for rigorous *induction*. Our critique suggests that the crying need in comparative politics is for more rigorous deduction and this is where mathematics, not statistics, is relevant.”⁵⁷ This statement is echoed by A. James Gregor, Oran Young,⁵⁸ and many others.

The difficulty with mathematical models is that they usually measure up poorly to the complexities of the phenomena being modelled. For example, Oran Young, who strongly advocates the use of modelling methods in international relations, has candidly observed that “The inherent hazard of this procedure is that its products may display little relevance to the real world of international relations for the indefinite future.”⁵⁹ Holt and Richardson, on the other hand, argue that a mathematically oriented political science must necessarily take a radically

⁵⁶ Tukey, *Exploratory Data Analysis* (Reading, Mass.: Addison-Wesley 1977); David C. Hoaglin, *A First Course in Data Analysis* (Reading, Mass.: Addison-Wesley, forthcoming).

⁵⁷ Robert T. Holt and John M. Richardson, Jr., “Competing Paradigms in Comparative Politics,” in Holt and John E. Turner, eds., *The Methodology of Comparative Research* (New York: Free Press 1970), 70.

⁵⁸ Gregor, “Political Science and the Uses of Functional Analysis,” *American Political Science Review*, Vol. 62 (June 1968), 425-39; Young (fn. 34).

⁵⁹ *Ibid.*, 196.

circumscribed view of political reality, cutting itself free from problem solving: "A science that is heavily committed to dealing with socially and morally relevant problems finds little use for this kind of paradigm or for the commitment to mathematics that it requires. For political science to advance, it must shed this professional commitment to solving social and moral problems."⁶⁰

One aspect of the mathematical approach to politics deserves special mention: the use of rational-choice models to explain political behavior. These models are particularly interesting because they take the most intractable elements of political processes—the individual and collective choices of political actors—and try to treat them deterministically. Some analysts have argued that if political science is ever to be a true science, the notion of rationality must be its central concept. For example, Riker and Ordeshook draw an explicit analogy between rationality on the one hand and the notion of mechanism on the other:

. . . it is clear that the assumption of rationality and the assumption of mechanism play comparable roles in the explanation of the social and physical world. The mechanical assumptions assert that there is something about things that assures us they will (usually) move regularly, and the rationality assumption asserts that there is something about people that makes them behave (usually) in a regular way. In each case, the function is to generalize about the regularity.⁶¹

The kind of regularities Riker and Ordeshook are concerned with here are of a special type—"postulated" as opposed to "observed" regularities. Granting that choices in empirical situations usually fail to exhibit the degree of regularity necessary for warranting deductive explanations and theories, Riker and Ordeshook choose to build a theory of politics on the foundations of how people *would* act if they were rational utility maximizers. This, of course, leads to a theory that fails to model political reality well. But the substantive loss is considered acceptable in light of the methodological gain: "The method of postulated regularity is positively more efficient, because it permits the easy generation of hypotheses and offers a single and parsimonious explanation of behavior."⁶²

⁶⁰ Holt and Richardson (fn. 57), 70-71.

⁶¹ William H. Riker and Peter C. Ordeshook, *An Introduction to Positive Political Theory* (Englewood Cliffs, N.J.: Prentice-Hall 1973), 11. A sympathetic yet sober evaluation of the utility of rational choice models for explaining and predicting coalition behavior is offered by Abram De Swann, *Coalition Theories and Cabinet Formations* (San Francisco: Jossey-Bass 1973).

⁶² Riker and Ordeshook (fn. 61), 11-12. By "explanation," we can only assume that Riker and Ordeshook mean "definition," since the postulation of rationality *defines* a (hypothetical) type of behavior, but does not *explain* it in any way.

The popularity of rational-choice models in political science would be puzzling to anyone who was not familiar with the current hierarchy of methodological and substantive priorities in the field. But with this hierarchy in mind, some particularly perplexing exercises become understandable. For example, in the recently published *Handbook of Political Science*, J. Donald Moon contributes a piece on “The Logic of Political Inquiry.”⁶³ This article begins very promisingly by articulating the D-N model of explanation as well as an important alternative to it, the interpretive model, which explains behavior in terms of motives, intentions, rules and norms, etc. Noting serious defects in both models, Moon turns to the task of synthesizing the two in order to create a more comprehensive framework for political explanation. But the “synthesis” turns out not to be a synthesis at all; instead it consists of a *substitution* of a rational actor “model of man” for the interpretive model of explanation. This eliminates the “looseness” and lack of regularity of empirical choice that is captured by the interpretive model and substitutes for it “presuppositions [that] specify the decisional premises of the actors which, together with descriptions of their situations, provide the rationale for the actions which bring about the overall pattern of social behavior . . . that . . . theorists desire to explain.”⁶⁴

Like the regularities of interest to Riker and Ordeshook, these “presuppositions” are postulated (specified) *a priori*. They replace the contingent aspects of empirical choice and action with causal and lawlike assumptions. Thus, choices are reduced to an algorithm specifying a necessary outcome from a necessary utility calculation. The net result of this substantive reduction is a definition of choice in terms of cause-and-effect relationships; which is to say, a definition of choice that denies the existence of choice! Certainly this conclusion would appear strange if we were not familiar with the current priority of method over substance in political science. As it is, we can see that Moon is struggling with the task of fitting his recalcitrant subject matter to the strict exigencies of a methodological notion of necessity that bears little resemblance to the realities of political choice.

The stress on reductionist explanation, quantification, and formalization has also led to an overloading of graduate curricula. If a political scientist must be a statistician, psychologist, and sociologist, then some of the traditional curriculum has to be set aside in order to make room

⁶³ Moon, “The Logic of Political Inquiry: A Synthesis of Opposed Perspectives,” in Greenstein and Polsby (fn. 13), I.

⁶⁴ *Ibid.*, 194.

for these newer disciplines and techniques. Anyone who has taught in a major graduate department of political science in the last twenty years will recall this inexorable process of narrowing and technicizing of the curriculum; the foreign-language requirements have been reduced, the field examination requirements have dropped from five to four to three, perhaps even to two. By the mid-1960's, it had become possible for someone to become a Ph.D. in political science with little if any knowledge of political theory, political history, foreign political systems, international relations, and even much about American politics and government. As Hayward Alker has recently remarked: "Training graduate students intensively in multivariate quantitative methods such as factor analysis makes less time available for developing a sophisticated awareness of what has classically been thought and said about political life. . . . Thus modern training is particularly inappropriate for understanding modern politics in which many questions about systems restructuring are continually raised."⁶⁵

Accompanying this narrowing and technicization of the graduate curriculum has been a demoralization of the older intellectual traditions in the social sciences and in political science. Political theory and philosophy, public law and public administration, and descriptive institutional analysis have all become defensive, peripheral, and secondary subject matters. As a result, a large part of the political science tradition is no longer being transmitted effectively to younger generations.

What we suggest here is that "science" is not a set of methods extracted from mathematical physics, as the neopositivist philosophers might have us believe; it is ultimately a commitment to explore and attempt to understand a given segment of empirical reality. The means employed in pursuing this goal should be secondary: in "good" science, methods are fit to the subject matter rather than subject matter being truncated or distorted in order to fit it to a preordained notion of "scientific method." This is the lesson that social scientists should have learned from the physical sciences. Instead, they have ignored it and, in the process, have undermined what Abraham Kaplan has called the "autonomy of inquiry."⁶⁶ If social science is to redeem itself, "Social scientists need to construct their own notions of 'good science,' their own methodological approach appropriate to their particular subject matter. . . . This view implies giving up the notion that there is some

⁶⁵ Alker, "Polimetrics: Its Descriptive Foundations," in Greenstein and Polsby (fn. 13), VII, 157.

⁶⁶ Kaplan (fn. 31), 3.

close analogy in the social sciences to basic research in the physical sciences.”⁸⁷

SECOND THOUGHTS IN PSYCHOLOGY AND ECONOMICS

Much of the knowledge our discipline has acquired of “scientific method” has been filtered through the two “pace-setting” disciplines in social science—psychology and economics. If we look closely at the present state of these disciplines which have pioneered in the use of statistical methods, mathematical models, and experimentation, we find evidence of some doubt and disillusionment.

Psychology, much like political science, has over the last couple of decades entertained a nearly constant “great debate” concerning the conceptual and methodological principles underlying the discipline. How should man, as the subject matter of psychology, be conceptualized? What kind of knowledge should psychology hope to acquire, and how can this knowledge best be pursued? Lately, some participants in this debate have become more and more critical of the established orthodoxy and have begun to question previously sacrosanct assumptions. These critics are not the inevitable dissenting minority in any discipline, but include some of the recognized leaders in the profession—leaders who, in fact, have been instrumental in creating the very conceptions they now question.

The problem of the “image of man” in psychology has been taken up many times. A particularly trenchant and lucid discussion was offered by Isidor Chein in his 1962 presidential address to the Society for the Psychological Study of Social Issues. Chein argued that “among psychologists . . . the prevailing image of Man is that of an impotent reactor, with its responses completely determined by two distinct and separate, albeit interacting, sets of factors: (1) the forces impinging on it and (2) its constitution (including in the latter term . . . momentary psychological states).”⁸⁸ He held that this image is obviously false, that psychologists can cling to it only “by violating our cardinal obligation as scientists—to maintain faith in our subject matter, to support scrupulously that which we observe, and to observe fully without willful bias.”⁸⁹

What this image denies, and what observation clearly attests to, is

⁸⁷ Marc J. Roberts, “On the Nature and Condition of Social Science,” *Daedalus*, Vol. 103 (Summer 1974), 61, 62.

⁸⁸ Chein, “The Image of Man,” *Journal of Social Issues*, xviii (October 1962), 3.

⁸⁹ *Ibid.* Similar arguments are made in Rom Harré and P. F. Secord, *The Explanation of Social Behavior* (Totowa, N.J.: Rowman and Littlefield 1972).

that man is “an *active, responsible agent*, not simply a *helpless, powerless* reagent.” Chein continues: “I am saying that we should not permit ourselves to be seduced, as so many of us have been, by those pretentious high order conceptualizations of Psychology that would deny Man the quality that is inalienably his, the quality of freedom—and, in the denial, make Man, as a psychological agent, inaccessible.”⁷⁰

This argument bears a strong resemblance to Popper’s. The determinist assumption of “cast-iron control” over choice and action is rejected for a conception that allows for the autonomy of human action in creating, as well as in responding to, the world. Interestingly enough, Chein claims to be a determinist—in the sense of viewing every event as having necessary and sufficient conditions—but argues that motives and purposes share in the determination of human actions, thus bringing them under direct human control. Like Popper, therefore, Chein is concerned with the question of how “mental events” such as purposes, deliberations, plans, etc., can play a part in bringing about change in the physical world.

In his presidential address before the American Psychological Association in 1975, Donald Campbell called on psychologists to show a bit of epistemic humility, and to recognize that “all scientific knowledge is indirect, presumptive, obliquely and incompletely corroborated at best.” He went on to argue that reductionism in psychology must be seen as a first step in a long-term research strategy, not as an end in itself:

Considering the complexities of our field and our models from the history of the successful sciences, a strategy of deliberate initial oversimplification has to be recommended to psychology. But this guarantees that in the early stages of development the theoretical orthodoxy will be misleadingly reductionistic, will portray humans as more simple machines than they actually are. If psychologists at such a stage were to lose the perspective that this view was a product of their long-term strategy, were instead to exaggerate the degree of perfection of their current theories, and were to propagate these immature theories as final truth, the net result could be destructive of popular values. . . . Here again, a science requiring the strategy of deliberate initial oversimplification may recruit scholars overeager to adopt a demeaning, mechanistic, reductionistic view of human nature.⁷¹

Today, at least some psychologists have managed to move beyond the mechanistic image of man, and are pursuing research based upon a more realistic and useful conception. Among the newer approaches in

⁷⁰ Chein (fn. 68), 2; emphasis in original; 18.

⁷¹ Campbell, “On the Conflict Between Biological and Social Evolution and Between Psychology and Moral Tradition,” *American Psychologist*, xxx (December 1975), 1120, 1121.

social psychology, for example, is “attribution theory,” which examines the assumptions and working hypotheses that constitute the “naive psychology” of ordinary people as they interpret their own behaviors and the actions of others. Lee Ross, one of the leaders in this field, has summed up the significance of this approach:

The current ascendancy of attribution theory in social psychology culminates a long struggle to upgrade that discipline’s conception of man. No longer the stimulus-response (S-R) automaton of radical behaviorism, promoted beyond the rank of information processor and cognitive consistency seeker, psychological man has at last been awarded a status equal to that of the scientist who investigates him. For man, in the perspective of attribution theory, is an intuitive psychologist who seeks to explain behavior and to draw inferences about actors and their environments.⁷²

What of psychology’s second problem, the kind of knowledge it can expect to attain about man? That issue has recently been given careful consideration by the educational psychologist Lee Cronbach. Reflecting on his experience in experimental social psychology over the last two decades, Cronbach asks the question, “Should social science aspire to reduce behavior to laws?” He observes that “Social scientists generally, and psychologists in particular, have modelled their work on physical science, aspiring to amass empirical generalizations, to restructure them into more general laws, and to weld scattered laws into coherent theory. That lofty aspiration is far from realization.”⁷³

The essential difficulty with this methodology, Cronbach argues, is that social science laws, unlike physical laws, seem to be highly mutable. As he puts it, “Generalizations decay.” Further, “At one time a conclusion describes the existing situation well, at a later time it accounts for rather little variance, and ultimately it is valid only as history. The half-life of an empirical proposition may be great or small. The more open a system, the shorter the half-life of relations within it are likely to be.” He compares the task of building theories in this way with a mechanical assembly problem: “It is as if we needed a gross of dry cells to power an engine and could make one a month. The energy would leak out of the first cells before we had half the battery completed. So it is with the potency of our generalizations.”⁷⁴

⁷² Ross, “The Intuitive Psychologist and His Shortcomings: Distortions in the Attribution Process,” in L. Berkowitz, ed., *Advances in Experimental Social Psychology*, X (New York: Academic Press 1977), 174.

⁷³ Cronbach, “Beyond the Two Disciplines of Scientific Psychology,” *American Psychologist*, xxx (February 1975), 116, 125.

⁷⁴ *Ibid.*, 122-23.

At the end of this article, which recounts two decades of aspiration toward a nomological psychology, Cronbach writes:

Social scientists are rightly proud of the discipline we draw from the natural science side of our ancestry. Scientific discipline is what we uniquely add to the time-honored ways of studying man. Too narrow an identification with science, however, has fixed our eyes upon an inappropriate goal. The goal of our work, I have argued here, is not to amass generalizations atop which a theoretical tower can some day be erected. . . . The special task of the social scientist in each generation is to pin down the contemporary facts. Beyond that, he shares with the humanistic scholar and the artist in the effort to gain insight into contemporary relationships, and to realign the culture's view of man with present realities.⁷⁵

Economics, like psychology and social psychology, has also been having its troubles in recent years. The critical themes have been surprisingly consistent; the field is seen as isolated and inbred, with its formal models bearing very little resemblance to the empirical world with which economists are supposed to be concerned. These criticisms have for quite some time been the stock-in-trade of such established gadflies of the profession as Gunnar Myrdal and John Kenneth Galbraith. Myrdal, for example, has argued that economists have failed to produce relevant knowledge because of an inappropriate commitment to the methods of the simpler natural sciences:

In recent decades . . . there has been a strenuous, even strained, effort among my economic colleagues to emulate what they conceive of as the methods of the natural sciences by constructing utterly simplified models, often given mathematical dressing. . . .

It should be clear, however, that this adoption of a form, which the natural scientists, in more simple, pointed questions, can use for analysis and presentation, does not really make the social sciences more scientific, if that form is not adequate to social reality and therefore, not adequate for the analysis of it.⁷⁶

Similarly, Galbraith used the occasion of his 1972 presidential address to the American Economic Association to chide the profession for its failure to come to grips with practical economic problems: "Neo-classical or neo-Keynesian economics, though providing unlimited opportunities for demanding refinement, has a decisive flaw. It offers no useful handle for grasping the economic problems that now beset the modern society. . . . No arrangement for the perpetuation of thought

⁷⁵ *Ibid.*, 126.

⁷⁶ Gunnar Myrdal, *Against the Stream: Critical Essays on Economics* (New York: Vintage 1972), 143.

is secure if that thought does not make contact with the problems that it is presumed to solve.”⁷⁷

These doubts and concerns have lately become a bit more widespread. Marc Roberts, a younger economist, asserts that “a significant proportion of recent theoretical work in economics has been of little scientific value. Many papers explore questions posed not by the world itself, but by someone else’s model.”⁷⁸ These views seem to be shared by some of the most respected leaders of the economic establishment. Oskar Morgenstern, in an important paper published in 1972, argues that economics is in a crisis because it lacks the concepts, methods, and philosophy it needs to deal adequately with social and political reality. Following a discussion of current equilibrium theory, Morgenstern observes:

The contrast with reality is striking; the time has come for economic theory to turn around to “face the music.”

There is, of course, always the possibility and the temptation of proving all sorts of theorems which have no empirical relevance whatsoever . . . Yet the ultimate criterion is whether what the theorem asserts is what is found in reality. One cannot help but be reminded of Hans Christian Andersen’s story of the Emperor’s clothes.⁷⁹

Wassily Leontief, who won the Nobel Memorial Prize for the invention of input-output analysis, has struck an even more pessimistic note. In his presidential address to the AEA, given two years before Galbraith’s, Leontief argued that “The uneasiness [in economics] is caused not by the irrelevance of the practical problems to which present-day economists address their efforts, but rather by the palpable *inadequacy* of the scientific means with which they try to solve them. . . . Uncritical enthusiasm for mathematical formulation tends often to conceal the ephemeral substantive content of the argument behind the formidable front of algebraic signs.” He concluded that “In no other field of empirical inquiry has so massive and sophisticated a statistical machinery been used with such indifferent results.”⁸⁰

The problems in economics, as in psychology, would seem to be primarily substantive. Morgenstern, sounding much like Popper, points

⁷⁷ John K. Galbraith, “Power and the Useful Economist,” *American Economic Review*, Vol. 63 (March 1973), 2.

⁷⁸ Roberts (fn. 67), 60.

⁷⁹ Morgenstern, “Thirteen Critical Points in Contemporary Economic Theory,” *Journal of Economic Literature*, x (December 1972), 1164-65.

⁸⁰ Leontief, “Theoretical Assumptions and Nonobserved Facts,” *American Economic Review*, Vol. 61 (March 1971), 1, 2, 3; emphasis in original.

to the failure of economics to deal seriously with the nonphysical aspects of economic processes:

. . . the overwhelming emphasis on the physical aspects of the economic process . . . seems one-sided when we realize that it is plans, decisions, preferences, states of information, expectations, etc., etc., that determine the movement and significance of the physical components of the whole economic phenomenon. We are far from having more than broad notions of how to describe and measure their share in a concrete situation. Do we even have a good methodology we could apply?⁸¹

It used to be, and apparently still is in much of economic theory if not practice, that these decisions and expectations could be discounted because they tended to cancel one another out in the classical market situation. Today, however, many economists attribute a large part of the discipline's empirical dilemma to a failure to appreciate how extensively political decisions now override the mechanisms of the market. Galbraith observes that "in place of the market system, we must now assume that for approximately half of all economic output there is a power or planning system."⁸² The effect of this injection of planning into the economic process has been to upset the predictive capabilities of economic theory. Robert Heilbroner, in commenting on the inability of economics to predict the course of a national economy, remarks that "it may be that this is less possible than it was, because the economy itself now is so much more a creature of decision making, and so much less the outcome of sheer interplay of impersonal forces, that prediction becomes inherently more difficult."⁸³

This major problem in economics would seem to have important implications for political science. For what the economists are saying is that to the extent their subject matter is becoming more *political*, it is becoming less susceptible to scientific and formalistic methodologies. The impact of decisions, of the possibility of shifting the economy in new directions, undermines the regularity of the impersonal forces that previously allowed for successful predictive and modelling exercises. This conclusion does not augur well for those who envision an eventually formalized political science. Indeed, the tendency seems to be in the opposite direction; economics may be becoming more like political science!

A second and related problem economists have had to deal with deserves mention: the problem of decaying generalizations. Like psy-

⁸¹ Morgenstern (fn. 79), 1187-88.

⁸² Galbraith (fn. 77), 4.

⁸³ Quoted in Wade Greene, "Economists in Recession," *New York Times Magazine* May 12, 1974, p. 64.

chology, economics has been unsuccessful in its attempt to build lasting empirical models of its subject matter. As Leontief puts it:

In contrast to most physical sciences, we study a system that is not only exceedingly complex but also in a state of constant flux. I have in mind not the obvious change in the variables . . . that our equations are supposed to explain, but the basic structural relationships described by the form and the parameters of these equations. In order to know what the shapes of these structural relationships actually are at any given time, we have to keep them under continuous surveillance.⁸⁴

These second thoughts in economics and psychology illustrate the degree to which the two bellwether disciplines are now reassessing their earlier explanatory strategies and meta-methodological commitments. Clearly, their attempts to deal with the complexities of social reality in terms of a model of scientific method borrowed from the physical sciences has run into more difficulties than they had expected. The ambivalence of this effort to bring the human enterprise under the categories and logic of the hard sciences has been captured by the economist and social philosopher Albert Hirschman, who points out in a recent book—in a section entitled “A Passion for the Possible”—that “Most social scientists conceive it as their exclusive task to discover and stress regularities, stable relationships, and uniform sequences . . .” rather than recognizing “the multiplicity and creative disorder of the human adventure.” He maintains that the social scientists would be surprised and even “distraught if their search for general laws were crowned with total success,” and concludes, “Quite possibly . . . all the successive theories and models in the social sciences, and the immense efforts that go into them, are motivated by the noble, if unconscious, desire to demonstrate the irreducibility of the social world to general laws! In no other way would it have been possible to affirm so conclusively the social world as the realm of freedom and creativity.”⁸⁵

The philosophy of science itself is experiencing a process of re-evaluation and reorientation similar to that taking place in psychology and economics. The article by Popper which we have used as a metaphorical guide for our own thinking is but one example of a more general trend in the field exemplified by his work⁸⁶ and that of Polanyi,⁸⁷ Hanson,⁸⁸

⁸⁴ Leontief (fn. 80), 3.

⁸⁵ Albert O. Hirschman, *A Bias for Hope* (New Haven: Yale University Press 1971), 27.

⁸⁶ Popper (fn. 1); *Conjectures and Refutations* (New York: Basic Books 1963); *The Logic of Scientific Discovery* (New York: Basic Books 1959).

⁸⁷ Michael Polanyi, *Personal Knowledge* (Chicago: University of Chicago Press 1958).

⁸⁸ Norwood R. Hanson, *Patterns of Discovery* (Cambridge: Cambridge University Press 1958); *Observation and Explanation: A Guide to Philosophy of Science* (New York: Harper and Row 1971).

Kuhn,⁸⁹ Quine,⁹⁰ Lakatos,⁹¹ Toulmin,⁹² and many others. Today, the pre-eminent position held by logical positivism in the philosophy of science seems to be weakening. Philosophers of science no longer see their role as one of legislating the "rules" of science; they are more likely to pursue descriptive and explanatory modes of research. Science is viewed as an activity or a process, not simply as a logical product. Accordingly, an appreciation is beginning to develop for the degree to which science—humankind's loftiest intellectual achievement—is grounded and dependent upon basic common sense and informal as well as formalized substantive knowledge.⁹³ Philosophers are learning more about how science grows and how it prospers. The newer literature in the philosophy of science is rich in insights and implications for the enterprise of social science.

IMPLICATIONS

If the whole of social reality has distinctive properties rendering it unamenable to simple deductive-nomological forms of explanation, this is especially the case for the study of politics which, of all the social sciences, focuses most directly on collective goal-seeking and adaptive processes. A political science solely concerned with the search for regularities which constrain choice would miss the distinctive aspect of political reality, which is the effort to escape from constraints, to discover value-optimizing solutions to problems in the context of constraints.

The anthropologist John W. Bennett recommends an approach to anthropological theory and research which is oriented around the concept of adaptation:

Instead of abstractions from behavior, like culture or the reductive formulas of psychology or genetics, [adaptation] focuses on human actors who try to realize objectives, satisfy needs, and find peace while coping with present conditions. In their coping, humans create the social future in the sense of generating new problems or perpetuating old ones and may even modify the biological construction of the population in the process. . . . By analyzing the factors that guide the choice of strategies,

⁸⁹ Thomas S. Kuhn, *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press 1962).

⁹⁰ W. V. O. Quine, *Ontological Relativity* (New York: Columbia University Press 1969).

⁹¹ Imre Lakatos, "Falsification and the Methodology of Scientific Research Programmes," in Lakatos and Alan Musgrave, eds., *Criticism and the Growth of Knowledge* (Cambridge: Cambridge University Press 1970).

⁹² Stephen Toulmin, *Human Understanding*, I (Princeton: Princeton University Press 1972); *Foresight and Understanding* (New York: Harper and Row 1961).

⁹³ See Campbell (fn. 71).

we gain knowledge of the possibility and direction of change and the relation of human behavior to the milieu.⁹⁴

We would argue that what Bennett has to say about anthropology applies with even greater force to political science: “the important phenomena for an adaptational anthropology are dynamic human purposes, needs and wants. . . .” The emphasis ought to shift “toward strategic coping, that is, the attempt to realize individual and social objectives through the mobilization of social and material resources. This category of human behavior has become dominant in the contemporary world with its interdependence and growing constraints on free action.”⁹⁵

Duncan MacRae argues a similar thesis regarding the development of the social sciences in the last several decades.

They [the social sciences] have evolved from an earlier form of social analysis, less specialized and recondite, by imitating the natural sciences . . . many social scientists have become convinced that the most effective path to useful application lies through objective research and theory construction, free from the complications of ideological and philosophical dispute. They have thus developed distinct technical terminologies and methods of research, specialized journals and programs of graduate instruction. Through these devices they have separated the discourse of specialists from that of the general public, and the communications of the individual specialists from one another. The course of the social sciences during the past several decades has thus been guided by the model of natural science—however distinct they may seem from it to natural scientists themselves.⁹⁶

MacRae’s solution to this problem of the withdrawal of the social sciences from social problem solving is to introduce into the university a “discipline of policy analysis” which will combine social theories and analysis with disciplined ethical discourse. He believes that the present situation of cognitive and valuative fragmentation in the disciplines of social science can only be overcome by an institutional solution—the introduction of research and teaching departments of policy analysis and applied social science.⁹⁷

We have somewhat less faith in organizational solutions, and are convinced that the discipline of political science—which has tended to abandon the task MacRae now wishes to assign to a special discipline—

⁹⁴ Bennett, “Anticipation, Adaptation, and the Concept of Culture in Anthropology,” *Science*, Vol. 192 (May 28, 1976), 847.

⁹⁵ *Ibid.*, 850, 851.

⁹⁶ MacRae, *The Social Function of Social Science* (New Haven: Yale University Press 1976), 3.

⁹⁷ *Ibid.*, 277ff.

is still capable of reasserting a central role in the study and evaluation of public policy. The powerful attraction of the example of the natural sciences has begun to fade as our efforts have fallen short of our aspirations. Despite the prominence of the trend among our methodologists, in our leading journals, and in some of our leading centers of graduate instruction, the overwhelming majority of the profession in the United States and abroad either actively resists the model, experiences a sense of obsolescence because of its prominence, or is indifferent to it. Most of the published work in political science, settles for goals less ambitious than nomothetic explanation. This work includes descriptive or historical accounts or case studies making limited use of theoretical frameworks and generalizations, and contributes to the aims of understanding, interpreting, and exploring political reality and policy alternatives which MacRae identifies as crucial to policy analysis.

One might make the case that the search for greater rigor in our understanding of politics might have made more progress if its claims and expectations had been less extreme, less exaggerated, less difficult to square with a recalcitrant reality. A more cautious approach to scientific progress, recognizing the peculiarities of human and social reality, might have resulted in a more general acceptance of appropriate quantification, of the heuristic value of formal-mathematical formulation, experimental methods, and the like.

It is of interest that a quarter of a century ago, in the aftermath of World War II, when the movement toward science in the social disciplines was just beginning, this relationship between the search for regularities and man's efforts to discover value-optimizing solutions to his predicaments was more clearly understood. One has only to compare an early "scope and methods" book with the more recent ones cited above. Some twenty-five years ago, many of the pioneers of the behavioral movement in the social sciences contributed to a volume entitled *The Policy Sciences: Recent Developments in Scope and Method*. In the leading chapter, Harold Lasswell stated his priorities: "If our policy needs are to be served, what topics of research are most worthy of pursuit? . . . What are the most promising methods of gathering facts and interpreting their significance for policy? How can facts and interpretations be made effective in the decision-making process itself?" The same essay celebrated the introduction of scientific methods into the social sciences—statistics, mathematical modelling, and related approaches. But this scientific hardening of method was set in the context of problem solving, value clarification, and the enhancement of the human condition. Lasswell looked upon method as

making possible acts of “creative imagination” which might move mankind in constructive directions away from the tyrannies and catastrophes of the 1930’s and 1940’s.⁹⁸

The connection between the search for regularities and political creativity—clearly seen by that generation freshly returned from Washington and the military theaters of World War II—was gradually lost in the decades that followed. The “methods” message of Lasswell’s sermon was heard and acted upon with the mixed results we have reviewed, but the “policy science” message largely fell on deaf ears for reasons we have suggested above.

What is under attack here is the pecking order, and the particular set of priorities and resource allocations, which have come to dominate the profession in the last decades. These priorities and allocative policies, and this pecking order, are legitimated not by successes in the explanation of political reality, but by the example and the demonstration effect of the hard sciences. A pecking order in which mathematization and sophisticated statistical analysis are viewed as the only sources of “real” or “powerful” theory, while theories produced from the interplay of imagination and induction are treated as “heuristic” or “weak” theory, cannot be justified by the explanatory performance of the former. Theories are inherently weak in the human sciences—both those that look “strong” because they look like the theories of physics, economics, or psychology, and those that look “weak” because they derive hypotheses from the examination of individual cases or historical experience.

Another aspect of the pecking order which is under criticism here is the distinction between pure and applied political science. Even in the hard sciences, the comparative intellectual payoffs of so-called pure and applied research are not at all clear-cut. Important discoveries often emerge out of applied research. In the social sciences, including political science, this difference loses its meaning since the special characteristic of social reality is man’s adaptive behavior. The part of the discipline which calls itself pure political science, searching for powerful and enduring regularities, has missed the essential point of its subject matter. At best it illuminates the context of political decisions; but it leaves unexplored the adaptive searching process, the policy options, and their consequences. Surely the study of public policy—viewed as efforts to adapt to, cope with, modify, and overcome constraints—is as basic and pure an undertaking as is the search for constraining regularities.

⁹⁸ Daniel Lerner and Harold D. Lasswell, eds., *The Policy Sciences: Recent Developments in Scope and Method* (Stanford: Stanford University Press 1951), 3, 12.

Indeed, we might argue that the essence of political science—insofar as it is to be defined by the essence of the politics it studies—is the analysis of choice in the context of constraints. That would place the search for regularities, the search for solutions to problems, and the evaluation of these solutions on the same level. They would all be parts of a common effort to confront man's political fate with rigor, with the necessary objectivity, and with an inescapable sense of identification with the subject matter which the political scientist studies.

Our policies of research support and professional training need to be freed from imitating the hard sciences. Policy studies, institutional studies, and philosophically sophisticated evaluative studies are claimants on research support with as much legitimacy as is currently accorded mathematical, statistical, and psychological and sociological reductionist studies. Knowledge of political substance in its institutional, historical, and philosophical aspects has to be re-established on an equal footing with sophisticated methodologies and reductionist knowledge in our programs of graduate training. A whole library of meta-methodological handbooks and primers imposing the model of hard science on political reality has to be re-evaluated in a new light. These volumes do not represent the "true path" to scientific progress; rather, they are a historical deviation, a flirtation with mistaken metaphors that temporarily captured the imagination of social scientists. Their historical importance is thus great, but their relevance to practical research problems in the social sciences is limited. To progress scientifically, the social disciplines require their own philosophy of science based on explanatory strategies, possibilities, and obligations appropriate to human and social reality.