Liberating the Industrious Tailor: The Case for Ideology and Instrumentalism in the Social Sciences.

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Many political scientists are concerned with understanding the philosophy of science and incorporating its teachings into their methodology.[1] This is, at least partially, because of the promise of theoretical and practical progress this branch of philosophy extends. We consider and evaluate both this concern and this promise in light of some annoyingly persistent problems. In addition, we suggest an alternative approach to "doing political science" calculated to turn these problems to the advantage of practitioners in the discipline.

I. PHILOSOPHY OF SCIENCE AND THE DEFINING OF POLITICAL METHODOLOGY

Most philosophers of science have described their task as identifying and systematizing those elements of scientific procedure accounting for the successes of the natural sciences. For example, Carl Hempel (1966:3-8) begins his popular introduction to The Philosophy of the Natural Sciences, with an account of the way that testing the logical implications of generalized hypotheses allowed Semmelweis to isolate the cause of vertain infections. Hempel's example is compelling. It illustrates the manner in which scientific analysis and scientific method lead to progress in the accumulation of knowledge and human well-being.

Now consider, for example, Cohen and Nagel's (1934: 195) statement about the practical concerns the scientific method is designed to address:

> If we wish clarity and accuracy, order and consistency, security and cogency, in our actions

> and intellectual allegiances we shall have to resort to some method of fixing beliefs whose efficacy in resolving problems is independent of our desires and wills. Such a method, which takes advantage of the objective connections in the world around us, should be found reasonable not because of its appeal to the idiosyncrasies of a selected few individuals, but because it can be tested repeatedly and by all men.

Again, Nagel (1961:Chapter 1) argues that the methodological principles of science lead to ever more precise and extensive accumulations of knowledge. Finally, Karl Popper (1958:34-39) virtually identifies the philosophy of science with the methodological rules that lead to the demarcation of the natural sciences. For Pepper, these rules constitute the defining characteristics of the scientific enterprise (1958:59-77).

To many political scientists, these claims of clarity, accuracy, cogency, and order are intoxicating. Faced with nearly intractable problems and with only limited success in producing knowledge, they devour the philosophy of science. Seeking to improve their own discipline, they turn the philosophers' descriptions of scientific method into prescriptions for political analysis. Consider, for example, Farquharson (1969:3-4), who idealizes both the scientific concept of theory as a mathematical formulation of experience, and the scientific requirement of theory selection through testing the specific implications of generalized covering statements against reality. Certainly the prescriptions for "rigorous" political analysis by the great "approach builders" were couched in terms of this "received view." Easton (1952:52-7), for example, tries to justify his constructs in these terms, and Merton (1977), Singer (1968), and many others take similar positions.

This "received view"[2] is so intoxicating because of its interpretation of the relationship between values and knowledge. Values in the "received view" are, in important ways, separable from analysis. Of course values motivate the scientist in the selection of research interests, in the initial choice of variables and indicators, and even in the preliminary understanding of the data collected. Thus the motivation for doing research is not value free. In fact, research in the natural sciences (as in political science) is often aimed (directly or indirectly) at resolving problems of violence, poverty, and inefficiency. Similarly, the researcher's concerns (usually arrived at through normative argumentation) are reflected in both the

researcher's approach and choice of variables. Given a particular interest and particular problem, the researcher may seek a theoretical approach that permits solving the problem by manipulating a particularly accessible variable. But even then the motivations of research <u>need</u> not have implications for the manner in which it is conducted. Most particularly, one would want the empirical basis for the acceptance or rejection of a theory to be independent of the values of the researcher. As Cohen and Nagel indicated, the "received view", from its very beginnings, sought to extend this objectivity of the natural sciences to other fields.[3] These fields were understood to have values and objectives different from the natural sciences. Consequently, progress was to be achieved by developing a methodology that could significantly reduce the impact of any researcher's values on theory selection. Once this methodology was available, all fields of study could choose their theories in the same way, regardless of a researcher's values and motivation. In brief, methodology would check bias.

Politics (needless to say) is rife with divergent values. More importantly, the predominant approach to understanding politics prior to the general acceptance of the "received view" worked to incorporate these divergent values into the very analysis itself. C. A. McClelland's "wisdom approach" stressed a long-term, direct approach with a narrow topic of concern; a study of the topic's history; and

> an "understanding" that is not quite intuitive; it is more a synthesis constructed privately from both particular facts and general meanings. Each student must build up such understanding by his own individual intellectual effort sustained over a long period of time (1969:4).[4]

All of this, of course, led to choosing theory on the basis of unchecked individual experience and of the researcher's personal recall of what was subjectively perceived as most significant. This, in turn, made generalization (theory construction) and testing highly suspect, and frustrated political scientists' attempts to identify any progress in their discipline. The philosophy of science, promising relief from these difficulties and frustrations, consequently became a preeminent concern of the discipline.

To say that the philosophy of science was a preeminent concern is not to say that it was everywhere accepted as defining the proper method for political science. Consistently, this "received view" was challenged by a "humanistic," "interpretive," or "hermeneutic" approach. Often using ideas about the scientific enterprise popularized by Kuhn, Polanyi, and Feyerabend,[5] political scientists argued for a fundamental break between the philosophy of the natural sciences and the practice of the social sciences.[6] From this perspective, political science does not uncover "truths" but "ideologies" (fundamental conceptualizations, conventions, etc.) inextricably tied to notions of human purpose and the nature of the good life.[7] Consequently, value neutrality in political research is limited in much the same way (but even more so) as it was in medical research (Taylor, 1967:47).

The emphasis and aims of the "received view" and the "humanist-ideological approach" identify a continuum. In fact, any given analyst's approach is likely to be mixed, incorporating aspects of both poles. Some philosophers of science and political theorists even argue for a conscious, dynamic interplay between these positions as the proper method in the social sciences (Taylor, 1967: or earlier, Pierce, 1931:paragraphs 319-337). Of course, there are research situations in political science where such an interplay makes a great deal of sense. To the degree that political research is not based on hypothetical-deductive constructs, but involves "approaches" and less clearly linked hypotheses, it is increasingly difficult to restrict the role of values. When research proceeds less on the basis of its inner logic and more on an exploratory basis, the researcher's freedom increases. There are, in brief, recognized forms of research in political science to which the philosophy of science is relatively inapplicable. Many political scientists simply do not play the science game. Thus, some critics of the received view (like Charles Taylor) broaden the object of their analysis from political science and "political theory" to political scientists and their "approaches to political theory" (Taylor, 1967). Under these circumstances, it is reasonable to define "doing political science" to include the entire "received view"/"Humanist-ideological" continuum.

But what if we confine "doing political science" to research seeking the same sort of rigor as the natural sciences? Must values creep in? Is there necessarily an ideological basis to the pursuit of theory in the social sciences, as the "humanists" maintain? Can we so confine our concept of "doing political science"?

We shall try to answer these questions. Specifically, we argue (1) that ideology necessarily plays an integral role in the most scientifically rigorous methodology; this role goes beyond the role ideology plays in motivating research interest, defining variables and indicators, and interpreting results (i.e., <u>ideology is an inseparable part of the formal logic of scientific method</u>); (2) that <u>it is not only the nature of the social sciences that makes ideological concerns important</u> to the inquiry; and (3) that given (1) and (2) it is not accurate to depict "doing political science" (or to prescribe what ought to be its method) in terms of the "received view"/"humanist-ideological" continuum. As an alternative, we argue that "doing political science" is best placed on a continuum, defined in terms of an interplay between social values or ideology and problem solving instrumentalism. This reconceptualization of "doing political science" does not diminish the work of those following the "received view." Rather, it liberates them from certain supposed requirements of an empirical social science impossible to fulfill. Further, it obviously places political scientists' conclusions on a rather soft epistemological foundation.

II. A PERSISTENT DIFFICULTY: THE PROBLEM OF THEORY SELECTION

Any convincing claim for a method of inquiry or for a procedure to accumulate knowledge accurately, must account for what is added and what is discarded. If a scientific procedure leads to a value-free accumulation of knowledge, it must provide sound criteria for choosing among competing hypotheses, theories, and explanations. It is only through the consistent use of such criteria that systematic progress can be achieved. Thus, this aspect of method is central to the epistemological and practical questions of procedure and method in political science.

The roots of such choice criteria are, of course, in epistemology, and the study of these roots has recently concerned philosophers of science. Although they are not of one mind, generally they agree that the "goodness of fit" between a theory and reality should dictate theory choice.[7] Reality is considered to be quite independent of the observer who determines what are "better" theories. The notion of value-free scientific investigation is thus preserved, and there is no role in the choice for cultural biases, ideological values, or subjective predispositions.

Philosophers of the "received view" do not pretend that scientists actually choose in this fashion. Rather than describing behavior, these philosophers try to specify a set of sufficient conditions for scientists to generate knowledge we might accept as "scientific." For Hempel and Nagel, as well as for many others, the core of the methodological argument is the choice criterion: comparing theory to reality can lead to progressive development in our store of knowledge. Given this agreement, the question becomes, how might we devise a criterion so that "reality" dictates our choice of theory?

Alternative Choice Criteria

Theory selection based on truth (the verificationist approach) would be most satisfactory, but this criterion is too severe. The universal statements and law-like generalizations of theories are open-ended. We cannot, for example, prove decisively that "all x are y," until each "x" is examined. Even if "all x" constitute a knowable and finite set, "x" might vary across time and space, and it is, therefore, at least questionable to say that since every x is now y, it will be y in the future. This is the general problem of induction, with us at least since Hume (1962:45-91 first published in 1777).

Interestingly, even if one argues that there is actually no problem of induction (Swineburn, 1978:9-17) there are still at least two difficulties with the verificationist approach. First, there are Hempel's well known paradoxes of confirmation, or the problem of what counts as evidence (1965:13-16). For example, a theory that asserts that only groups "appropriately" organized or privileged can supply themselves with collective goods (Olson, 1965) can be corroborated (using common conceptions of confirmation) (1) by any collective good not being supplied to any group regardless of its state of organization, etc., (2) by any "appropriately" organized group that either does or does not receive collective goods, (3) by any objects in the universe that do not constitute "an appropriately organized group." The problem stems from the fact that if (as is usually the case) a law takes the logical form of a universal conditional (e.g., all crows are black), then confirming any or all logically equivalent forms should serve to confirm the original form. For example, one logically equivalent form of "all crows are black" is "all non-black objects are not crows." Now if black crows are confirmatory evidence for the original form because they fulfill the antecedent crowness, and the consequent, blackness, then any non-black, non-crow object (e.g., a red pencil) must also be confirmatory

evidence.

The second problem with the verificationist approach is the general underdetermination of any probable reality. One perplexing problem of statistical accounts, for example, is that they make it possible (on the basis of true premises) to establish pairs of incompatible conclusions from the same body of available evidence (Hempel, 1965:53-67). To see this, consider the statements (1) "80 percent of American Jews vote Democratic" and (2) 90 percent of American stock brokers vote Republican." Now imagine Jacobi, an American Jew and stock-broker. Does he vote Democratic with a probability of .8 or Republican with a probability of .9? Both inferences are obviously problematic. In this manner, it becomes difficult to state precisely what confirmatory evidence would look like. Consequently, the most we can speak of are "degrees of confirmation" (Carnap, 1947-8) relative to the total available evidence, and this evidence is often so comprehensive and complex as to defy expression in terms clearly confirming any probabilistic, law-like statement (Hempel, 1965:66-7).

It would seem, then, that all theories are equally unverifiable, and that choice among theories on the basis of proven truth is impossible. But problems of induction and corroboration may be generally avoided by adopting a falsificationist approach.

If science cannot prove, it might nevertheless disprove theories, avoiding some problems raised by the use of a verification criterion. Though it may take an infinite set of observations to verify the truth of a statement, as long as a statement is cast in universal conditional form it takes but one replicable observation to falsify it.[9] The idea is to use the empirical base to choose from among law-like general statements that are fair game for falsifying attempts. Just as Hempel did not focus on the generation of hypotheses but on their confirmation, falsificationists are not so concerned with <u>how</u> generalization occurs (Popper, 1958:107-8) as with getting on with "scientific" falsifying procedure. Induction is not an integral part of the falsification process and avoiding its logical problems is, at least in part, a strength of this philosophical school's procedural prescription. Thus, induction is not a "problem" in the sense it would be for a verificationist, who must justify the step from empirical base to choice of hypothesis through an inductive process.

Finally, the criterion of falsifiability allows one to avoid the paradoxes of confirmation.[10] A single-minded falsificationism reduces ambiguity about what data are appropriate so long as the theory can be stated as a simple universal conditional.

Still, the criterion of falsifiability is unsatisfactory. First, it leads to the rejection of theories when we have no well formulated alternatives. Second, laws are not always of universal conditional form. Third, since Popper's falsificationist program tells us nothing about the generation of hypotheses, the rejection of "near misses" is final, even though all that might be called for is a refinement of the theory. Finally, theories by themselves absolutely forbid any given occurrence or given state of affairs. Theories usually contain understood or tacit certeris paribus clauses, which logically deflect the impact of apparently falsifying instances from the central theory.[11] It seems, then, that theories are not only unverifiable but also unfalsifiable.

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Though empiricist criteria are dealt a serious blow by these considerations, the wound is not fatal. Such "absolutest" views of science and its choice criteria are not really necessary. The point of science and the scientific method might rather be to search, as Popper (1958:50) says, "for mistakes with the serious purpose of eliminating as many of these mistakes as we can in order to get nearer the truth." Using this approach, experiments would be designed to indicate which theories portray reality most accurately. Rival theories are tested against each other and the one that provides maximum corroborated content is chosen. In this way, theory selection is predicted on "verisimilitude," and science is concerned primarily with "close" instead of "exact" fit. "Better," rather than "best," theories would be chosen. Indeed, this seems to be the pattern in political science. Consider for example, the evolution of theorizing about the act of voting. Beginning with Downs' (1957) derivation of the logic of non-voting, we see an effort continuously to improve the theory's fit with reality.

This choice criterion precludes some of the problems of verification and falsification dealt with so far, and this portrayal of science probably squares quite nicely with the views of most scientists. First of all, addressing verisimilitude in this manner stresses the deductive implications of theories, and thus avoids the inductive perplexities of the verificationist approach. Second, one

always keeps the "best" theory despite falsifying instances, and one can accept the theory as "best" in light of all its auxiliary theories (theories about what constitutes a "fact," the "real-world," "unbiased perceptions," etc., and theories about the nature and scope of the tacit ceteris paribus clause) without claiming it to be ultimately the most "truthful" that one might possibly devise. Thus the fundamental objections to the falsification approaches are met. Finally, even such basic introductory treatments of scientific method as Nagel's describe science as motivated by "the desire for explanations which are at once systematic and controllable by factual evidence." These treatments also seek to explain how science differs from common sense, to the extent that it constantly, consistently, and closely critiques its own arguments in order to increase their fit with experimental observations (Nagel, 1961:7-10, 12-13). It is not too foreign, then, to speak of maximally reliable theories rather than "true" theories.

It would seem, then, that we can develop logical and systematic principles for choosing between rival theories, consistent with the objectives of the "received view." Thus, the received view would seem to be a useful antipode in explanations of what constitutes "doing social (and therefore political) science." Lakatos (1970) has, in fact, set about formulating and defending such principles for choosing maximally reliable theories (those with maximally corroborated content) according to his "doctrine of sophisticated methodological falsificationism":

> For the sophisticated a scientific theory T is falsified if and only if another theory T' has been proposed with the following characteristics: (1) T' has excess empirical content over T: that is, it predicts novel facts, that is, facts improbable in the light of, or even forbidden by, T; (2) T' explains the previous success of T, that is, all the unrefuted content of T is included (within the limits of observational error) in the content of T' and (3) some of the excess content of T' is corroborated. (Lakatos, 1970: 116. Italics in the original.)

These criteria seem to provide standards for empirically evaluating theories while recognizing that no theory can be conclusively falsified. Thus, they provide for theory choice on the basis of "verisimilitude" or closest fit, given the corroborated content of rival theories. This position has been appealing to political scientists. As Moon says, "although there are certain difficulties with Lakatos' program. . . it is sufficient to demonstrate the testable, and therefore explanatory, character of scientific theories." (1975:153).

Recent developments in the philosophy of science and in political methodology, however, can be used to challenge the "Lakatosian Resolution" of the traditional problem of theory selection. For example, David Miller (1975:159-191) has argued that choosing between two theories (both non-isomorphic with reality) on the basis of relative verisimilitude is impossible. His basic argument may be illustrated quite simply. Imagine two theories (A and B) each predicting certain aspects of reality (e.g., X and Y). The nature of X and Y is free to vary widely: these may be constants (e.g., a society's marginal propensity to consume), or parametric function (predicted response rates of one society to another's arms levels). Imagine further that one theory (A) is always at least as good, and at times better, in predicting aspects of reality than the other (B). This situation is graphically exemplified in Figure 1, and summarized in Table 1.

"Best fit" criteria would seem to dictate that we choose theory (A) over theory (B), as (A) apparently is uniformly more accurate. But as Miller demonstrates, unless (A) is always true (never errs), (A) can not <u>always</u> be more accurate than (B). That is, we can always construct other combinatorial aspects of the reality the theories explain that reverse the relative degrees of accuracy. To illustrate, consider Table II. Here theory A predicts both X and Y more accurately than B. However, we can construct a new aspect of reality (V). Defining V (for illustrative purposes) as the arithmetic sum of X and Y, note that B predicts V better than A does. Similar manipulation is possible for parametric functions. Miller notes that such reversals undercut the possibility of choosing among theories on the basis of verisimilitude. No ordering of theories on such a basis is possible. Miller goes on to establish the existence of constructable variables that can reverse the verisimilitude ranking of any two imperfectly fitting theories. Thus, among false theories there can always be found predictions reversing any order of accuracy derived from any other set predictions. This means that if we have any theory that predicts many things better than any other, we can construct a new domain of comparable size, in which the relative accuracy of the two theories is reversed.

If Miller's findings are correct, much of the force is taken from the "Lakatosian Resolution," and the problem of theory choice persists. This, in turn, calls into question once again both the usefulness of the "received

Table 1. The relative accuracy of two theories

	X	Y				
reality	X	Y	EITHER:	X_b	$\geq X_a$	$\geq X$ or
predictions by A	X_a	Y_a		X_b	$\leq X_a$	$\leq X$ where,
predictions by B	X_b	Y_b		In either case, one or both inequalities are strict.		

Figure 1: Where the theories generate parametric functions, Z(t)

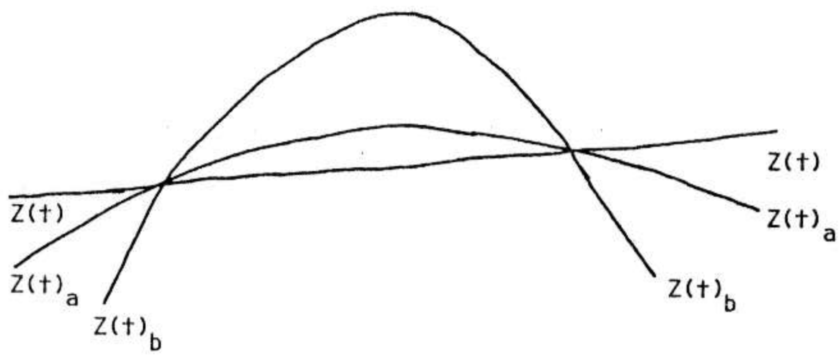

Table 2. Reversing the accuracy of the theories

	X	Y	V = f(X,Y) = X + Y
reality	8	0	8
predictions by A	7	2	9
predictions by B	4	4	8

view" as a source of techniques for political scientists, and the validity of defining the practice of political science in terms of such a perspective. Of course, the strength of Miller's argument depends on the status of the constructed variables or constants. He argues forcefully that the constructed variables are not of lower status than the original variables. But there are, it would seem, some differences. After all, one can construct variables reversing the accuracy of theory prediction only when one knows where the competing theories are inaccurate. And in the natural sciences there are theories (even if only a few) for which the existence and extent of inaccuracy (if any) is unknown. In those cases, one cannot demonstrate that any given ordering among theories is arbitrary. Theorists might continue to use one theory as "better" than another despite profound suspicions that the "better" theory will be found false at some point. Without known falsifying instances, in other words, scientists will continue using the "better" theory even though, once falsification occurs or anomolies develop, the merits of the previously chosen theory could be characterized as dubious.

Social scientists, on the other hand, continuously use, and choose among, theories known to be poor predictors of some aspects of reality. Consider, for example, the theory of rational choice. Data have shown that under replicable laboratory conditions the theory fails in a very high percentage of trials. For instance, the notion that one can identify the preferences of individuals independently of a structural environment of choice (i.e., only relative to the alternatives from which the choice has to be made) has been challenged by a number of psychologists. Experimental evidence is dramatically one-sided in showing that certain environmental structures induce unstable, or inconsistent, preferences for an extremely large subset of subjects (Grether and Plott, 1979; Tversky and Kahneman, 1981). Still, public choice (in political science) and most of microeconomics rest on this falsified base.

For another illustration, consider how the theory of rational choice has been used to derive certain strategic models, such as those employed in game theory. The game theoretic model most common in political analysis is the prisoner's dilemma. As the various models and applications have proliferated, so has experimental evidence. Not only are many results counter to the simpler models, but also some defy all the models. Thus, for example, experiments by Marwell and Ames (1979 and 1980) cast doubt on

virtually all major propositions regarding the behavior of individuals in n-prisoner games. Indeed, these sorts of observations appear frequently. Hence, social science theories are probably more directly subject to Miller's critique than the theories of the physical sciences.

III. THE IMPLICATIONS FOR THEORIZING IN POLITICAL SCIENCE

The understanding that theories may increase in practical verisimilitude and yet never approach truth appears more germane to the social sciences than to the physical sciences. In social science, virtually all theories (excepting perhaps those of demography) are understood to be false. As mentioned, microeconomics' shakey foundations include the theory of profit-maximizing firms and utility-maximizing consumers. But were we to throw out these and similar theories, we would not even have "rough first approximations of reality," and, therefore, very little to work with. Thus, it is not surprising that social scientists employ less demanding (non-absolutist) criteria for choosing among theories. Social scientists cannot choose a theory with an unblemished record. Indeed, their expectations are such that they apologize for those rare instances where theories remain unblemished.

Typical of the comments in political science, for example, are those of McKelvey and Ordeshook:

> The results of the seventeen 5-person experiments in Table 9, moreover, are a bit embarrassing. We cannot report a single failure of the competitive solution. . . . In Fact, we might even hope for a few failures to render these results more believable (1978:28-29).[12]

There is almost an explicit assumption here that data producing such good results must be biased in some way the authors do not understand. They "know" their theory is only "mostly true" at best. Political scientists, then, are forever choosing "second best" theories: one falsified in the strict sense, yet generating "more useful" explanations and "superior predictions" as compared to other available theories.

In brief, it might be said that progress in social science (and most certainly in political science) seems to be realized by choosing among false theories according to their verisimilitude or improved empirical fit. This in turn serves to justify reducing to a minimum normative considerations in theory formation and choice. (Normative considerations are thought to act as excess baggage, frustrating the attainment of improved empirical fit and thus inhibiting if not preventing progress.)

Miller's findings, however, block any such hope for empirical progress. We can not choose among theories on the basis of their relative (unbounded) verisimilitude. Denied this generally accepted criterion, we are forced to look for conditionally acceptable standards. Baldwin (1979) has discussed one effective strategy. If we limit the domain of any theory in some useful manner--to a set of interesting or important dimensions of reality given our present concerns, needs, and institutional goals--we can avoid many of the difficulties Miller (1975) exposes. Of course, any limitation must be arbitrary to some extent and hence difficult to defend philosophically. This difficulty, however, arises from the tacit assumption, in most theoretical endeavors, that theories must be "holistic," or isomorphic with reality. But where this is recognizably not the case, choice must depend on another standard (e.g., its relevance to important social concerns or its efficacy in resolving immediate problems).

If theories may be <u>chosen</u> to fit only part of our world, so theories may also be <u>developed</u> to satisfy certain limited ends. The objectives motivating the theory (and implicit in its development) then help identify its domain. (Perhaps, as Beardsley [1976] argues, this cannot altogether be avoided). But such teleology is perhaps <u>too</u> effective. It could so powerfully limit our domain that the development of new domains (new applications) for old theories is either retarded or precluded.

Predicating theory selection on evidence from a subset of that available and relevant, or deliberately restricting theories to particular problems may be seen as forms of instrumentalism. Indeed, there are many who reluctantly turn to instrumentalism as a method of limiting theory domain. Instrumentalism calls for theory selection on the basis of the <u>best fit to a problem area of interest to the researchers,</u> thereby combining Baldwin's approach and Lakatos' principles. This weak approach (is it the only survivor?) has interesting implications for (1) the role of values and ideology in social scientific endeavor, (2) the definition of what counts as "doing political science," and (3) the role of "scientific" or "empirical" techniques in political science. Let us consider each of these.

Instrumentalism leads to an independent role for values in the theoretical process, since problem selection is no longer necessarily implied by the objective selection of theories. Rather, the choice of "best" theoretical structure comes to depend on the choice of problem. But other aspects of instrumentalism lead to a more interesting role for the scientist's values.

Generally, the lack of determinate choice, or the existence of severe ambiguity, creates a large role for values in a science wedded to instrumentalist criteria of theory choice. An instrumentalist perspective does not define the problems from which the researcher is to choose. Consequently, nothing necessarily prevents the ever-increasing proliferation of problem areas, each with its own theoretical structures and paradigms. Indeed a "pure" instrumentalist perspective would foster this proliferation of theory domains. (Let us label such a product "rampant instrumentalism.") Moreover, little consistency would be required among problem areas. After all, it is the efficacy of a theory <u>within</u> its domain that counts, not its consistency across domains. The question becomes: what definition of problem areas is relevant for purposes of theory choice? Does one allow for continual division into sub-problems? Does the status of each sub-problem (or set of sub-problems) equal the status of the more general problem, permitting increasing proliferation of theories? Without a technique for problem definition, we have an indeterminate or ambiguous criterion for selection among competing theoretical structures.

Obviously a community's ideology (its inherent values, its fundamental conceptualizations, and its traditional categories of thought) will play a significant role here. Consider a situation where one problem (call it "<u>b</u>") is explained most accurately by theory "B", while another theory, "A", best predicts a subset of <u>b</u> (call that subset "<u>a</u>"). Now instrumentalism would say, chose A if you're interested in <u>a</u>, and B if you're interested in <u>b</u>. But a situation where the major interest was in <u>a</u>, with a residual interest in <u>b-a</u>, could lead to use of A in the analysis of b-a, even though it would not be as good a theory for <u>this</u> problem. Again, if the interest is in "a", but "A" requires a reordering of priorities or a radical alteration of fundamental concepts, where "B" does not, "B" might well be chosen though it predicts less well. In Beardsley's terminology, a community's anchoring point (what it is willing to consider as subject to inquiry and change) or its ideology and moral bias circumscribe any tendency toward "rampant instrumentalism."

On the other hand, interest in generality and desire to avoid paradox inhibits the tendency to allow ideology to define all the boundaries (a circumstance we could call "rampant ideology"). (Baldwin, for example, mentions the "paradox of power" arising from research on international pow- er relationships: e.g., America's failure in Vietnam.) Un- like metaphysics, instrumentalist science is still a game played with, and to a large extent governed by, data from a limited domain. One could be a "true believer" in a theory, but this belief would require a domain within which the theory instrumentally dominates its rivals; in addition the domain must be sufficiently important to justify the theory on the basis of the domain. In brief, there is apparently a "reflexive" relationship between instrumental- ism and ideology keeping each from running amok.[13]

Theories are discardable when they are no longer defensible ideologically or instrumentally in a domain. Note that this domain need not be "practical," but can be of purely theoretical interest: e.g., the interface of two theories predicting quite different things within one arena and hence establishing expectations of a "critical experiment." On the other hand, theories may coexist without generating the need for a critical experiment. Consider the case of voting behavior, where political scientists have advanced numerous psychological theories. Simultaneously, economists have developed numerous voting models based on simpler notions of choice, usually spin- offs of the theory of market choice that were not taken seriously by political scientists. As economists became more interested in the predictions regarding voting, their paradigm challenged that of the political scientists. Only when both communities of scholars became interested in the same phenomena did the conflicting bases for expla- nation manifest themselves as problems of theory choice.

We have argued (1) the "received view" of "doing political science" establishes criteria that are proving impossible to fulfill; (2) this leads to the adoption of a instrumentalist attitude toward scientific research, and (3) the instrumentalist attitude leads to a varied role for ideology (values, traditional categories of thought, conventions, etc.) in scientific endeavor. These three points argue strongly that "doing political science" leads in a particular direction--one in which the techniques of the "received view" still play an important but constrained (by the instrumentalist's views) role.

CONCLUSIONS

If the arguments in this paper are correct, the use of scientific methods in political science cannot be expected to lead to value-free inquiry, an ever increasing store-house of knowledge, or accurate predictions across all domains of inquiry. Furthermore, if the analysis is correct, only instrumentalism is likely to be acceptable as a criterion for theory selection, and thus political science is unlikely to be theoretically integrated. Because a host of different problems interest us as political scientists, we shall continue (in all likelihood) to have a host of different theories to explain phenomena. These theories are likely to compete, from time to time, but it is not likely that "conclusive" contests will be forthcoming. It is far more likely that political scientists will continue to choose theories on the basis of the relative weights placed on the integrating force of generalized theories vs. the explanatory power of narrower constructs. Such a forecast and analysis does not diminish the work done by scientists. In fact, this argument liber- ates them from the requirements of strict empiricism (which are impossible to fulfill, and insensitive to the social and cultural dynamics within which they function). And it liberates them, on the other extreme, from the dictates of pure ideology, which seeks to create reality in terms of its own fundamental premises. Scientists should be able to blend interest, need, and context in a manner reflecting the limitations of each.

FOOTNOTES

1. As evidence of this concern, consider (1) the listings of the Social Science Citation Index under well- known philosophers of science like Hempel (more than 90 listings in 1970, more than 120 in both 1978 and 1979), Nagel (more than 70 listings in 1970 and 1978, more than 90 in 1979), and Popper (more than 130 listings in 1970 and more than 350 in 1978 and 1979, and (2) the recent in- troduction and growth of new journals that specialize, at least in part, in the philosophy of science, and that aim primarily at political scientists (e.g., Political Methodology, Knowledge, etc.)

2. Rather than be overly narrow about the precise content of the received view, we lump together the varying postures of Hempel, (1965, 1966); Popper, (1959); Nagel, (1961); and Lakatos, (1970). While there are important differences among these philosophers, they agree on their goal: a theoretical description of science and, hence, an epistemological evaluation of a "theory" of scientific method.

3. One interesting short history of the received view is

John Passmore's (1967), especially 52-54.

4. Of course, McClelland is speaking specifically about only one branch of political science, but the approach is recognizable in the other branches as well. (See Lane, 1962; McClosky, 1960; Herring, 1965.)

5. Ball (1976) presents an analysis of this development, from a closely related viewpoint.

6. For an analysis of these arguments, see Moon (1975).

7. For an interesting discussion of these ties, see Taylor (1967:25-57).

8. When several available theories provide comparable fits, their secondary characteristics (e.g., simplicity) become relevant. A more fundamental reformulation may be required if the process reflects a scholarly linguistic consensus. Thus, for example, W. V. Quine (1960) sometimes talks as if the issue is linguistic, but elsewhere notes that the "perfect theory of truth is what Wilfrid Sellars has called the disappearance theory of truth. . . Truth hinges on reality. . ." (1970:11).

9. See Popper (1958:41, 59-71). There have been severe criticisms of this position. Hempel, for example, demonstrates that some scientific laws are of a form that cannot be falsified (Hempel, 1965:46-47). More generally, strict falsification is problematic for any probabilistic hypothesis, as Popper himself realizes (1958:189-191).

10. This is because falsifying data are unambiguously specified under this approach. But see the argument to the contrary in Scheffler (1963:269-291).

11. See the in-depth analysis of this point in I. Lakatos (1970:100-103).

12. A later expression of this view, without these qualifications was co-authored with U. D. Winer (1978), but they needn't fear. Recent experiments have generated replicable counter examples (Miller and Oppenheimer, 1981).

13. A more serious problem may lurk here, for a justification of instrumentalism requires that "our view of the problem" not change the underlying "facts". This is likely to be satisfied in the natural sciences. But consider the behavioral sciences: if ideology identifies a problem area (call it X) and leads us to theorize about human behavior within X (or to choose theories by behavior observed within X), we assume that had another ideology defined the problem differently (e.g., Y = X), behavior within X would not have changed. But if our interest in X is generated by an ideology that also modifies behavior within X, because behavior is a function of the same

system of values as is problem definition, then we could have trouble. Indeed, this abstract problem appears related to experimental evidence on preference reversal (Tversky and Kahneman, 1981). Tversky and others have dis covered that behaviors reflect the ways alternatives are "framed." The findings appear robust (Grether and Plott, 1979) and generalizable (Tversky and Kahneman, 1981). Hence, instrumentalism is left with a more complex, seriou reformulation problem in the social sciences than in the natural sciences. But the difficulties thus indicated must be the subject of another paper.

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Measurement Problems in Contextual Analysis: On Statistical Assumptions and Social Processes.

By Stephen Weatherford

The traditional division between political sociologists and political psychologists becomes less and less appropriate as the two schools develop operational, methodologically sophisticated approaches to similar political phenomena. Political sociology, broadly concerned with social groups and institutions, has tended to take a macro-level view of the political world, and to rely heavily on aggregate indicators of the social properties and political propensities of groups (classes, ethnic groups, counties, provinces, nations). Political psychology, more concerned with individual behavior, often assesses individuals outside their social and spatial surroundings, a tendency that is particularly pronounced when research is based on random sampling methods. Each approach is obviously incomplete, as attested by the voluminous literature on the various fallacies that make inferences from aggregate, as well as individual, data risky. The study of "contextual effects" represents an attempt to remedy these deficiencies by combining the strengths of both approaches.

Political scientists' interest in contextual effects studies is increasing, and the most recent national election surveys have been designed to facilitate such research. Precisely because contextual researchers work between two well-established areas of theory, they confront a number of peculiar problems for which previous research has few solutions. This paper builds on earlier theoretical and methodological work on contextual models (Przeworski, 1974; Hanushek et al., 1974; Sprague, 1976) and primarily concerns one fundamental problem. The origins and ramifications of the question are both