Theory and Evidence in Comparative Politics and International Relations

Edited by

Richard Ned Lebow

and

Mark Irving Lichbach

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Theory and Evidence[1]

Mark Irving Lichbach

In their debate on neopositivism, while Kratocwil danced with relativism, Pollins stressed the value of positivism to qualitative researchers, and Hopf recognized the importance of making interpretations more rigorous. Listening to their stimulating debate, Chernoff tried to clarify the nature of naturalism, and Waldner probed the idea of causal mechanisms. Trying to understand the implications of the debate for social science practice, Lawrence subjected the empirical claims of the democratic peace literature, and Levy international relations research programs more broadly, to scrutiny.

Bernstein, Lebow, Stein, and Weber summarize and extend these analyses by making a plea for case-based reasoning. We need to understand these debates in terms of three principles of the traditional positivist philosophy of science.

1. *Theory* is deductive-nomological: it begins as abstract, axiomatic, and foundational; it becomes subsuming, integrating, and unifying; and it ends as organized, comprehensive, and encyclopedic.
2. *Evidence* is oriented toward falsification: scientists attempt to reject a hypothesis; after one possible explanan is discarded, they investigate another to see if it can account for the explanandum.
3. *Evaluation* is therefore based on deductive and nomological laws that resist falsification: these laws establish the ever-expanding domain of a theory; science therefore succeeds when it discovers universal laws that are true.

This philosophy of science might have suited social scientists a few decades ago. Today's more modest philosophy of science that consists instead of three different principles.

1. *Theory* consists of research programs that contain nuts and bolts; these causal mechanisms are combined into models of a theory that suggest lawful regularities.
2. *Evidence* establishes the applicability of these models of a theory for the models of data that exist in particular domains; the elaboration of a theory thus delimits the theory's scope.
3. *Evaluation* grapples with the problem that the science that results from following the first two principles is prone to nonfalsifiability and to self-serving confirmations. Confrontations between theory and evidence are thus evaluated in the context of larger structures of knowledge.

This final chapter moves the debate (Lichbach 2004) forward by dealing with the problem of evaluation. For pragmatists who work with a thin version of one paradigm, Lakatos's (1970) "additional and true" standard, which lets them explore rationalist, culturalist, and structuralist approaches on their own terms, is applied. For competitors who employ alternative paradigms, Popper's (1968) "different and better" standard, which lets them conduct competitive evaluations among alternative rationalist, culturalist, and structuralist explanations, is employed. And for hegemons who synthesize the different paradigms into one thick paradigm, "nested models" that combine the two standards, and thus lets them compare syntheses to their components (models and foils), is used.

Evaluation

Hirschman (1977: 117) recounts the following: "In an old and well-known Jewish story, the rabbi of Krakow interrupted his prayers one day with a wail to announce that he had just seen the death of the rabbi of Warsaw two hundred miles away. The Krakow congregation, though saddened was of course much impressed with the visionary powers of their rabbi. A few days later some Jews from Krakow traveled to Warsaw and, to their surprise, saw the old rabbi there officiating in what seemed to be tolerable health. Upon their return they confided the news to the faithful and there was incipient snickering. Then a few undaunted disciples came to the defense of their rabbi; admitting that he may be wrong on the specifics, they exclaimed 'Nevertheless, what vision!' "

Are scientists different? Physical and biological scientists who cling to the heuristic power of their "vision" of the world often fit models of their theories to the world so that they might "save" the phenomenon in question. Curve-fitting—aligning theory and observation—is easy, and Davis and Hersh (1981: 75) describe the classic scientific parallel to the disciples of the rabbi of Krakow:

> In the Ptolemaic system, the earth is fixed in position while the sun moves, and all the planets revolve around it. Fixing our attention, say, on Mars one assumes that Mars circled about the earth in a certain eccentric circle and with a certain fixed period. Compare this theory now with the observations. It fits, but only partially. There are times when the orbit of Mars exhibits a retrograde movement which is unexplainable by a simple circular motion. To overcome this limitation, Ptolemy added to the basic motion a second eccentric circular motion with its own smaller radius and its own frequency. This science can now exhibit retrograde motion, and by careful adjustment of the radii and the eccentric periods, he can fit the motion of Mars quite well. If we require more precision, then a third circle of smaller radii still and a yet different period may be added.

The Duhem-Quine thesis explains why such things happen in science and hence why science can be no different than religion: when a theory is found wanting, it is not clear what has gone wrong. Scientists can thus use ad hoc explanations, post hoc adjustments, and tautologizing alterations to immunize their theory from falsification by inaccurate predictions. Analysts, eager to prove their pet theories correct, ignore the facts and instead turn to these fudged factors: arbitrary domain restrictions, empty prevarications, face-saving linguistic tricks, and exception barring. Scientists who claim to know the cases before they see them eventually interpret cases in terms of theory, conflate evidence and generalizations, and equate the empirical and the analytical. Even when they consider plausible rival hypotheses, such scientists often engage those other theories on their own terms, carefully privileging their own theory by setting up their opponents as a straw figure.

Rapoport (in Weintraub 1985: 35) thus argues that

> mathematically, you can cook up anything. You can imagine any sort of situation and represent it by a mathematical model. The problem becomes that of finding something in the real world to fit the model . . .
>
> There was a man who liked to fix things around the house, but the only tools he could use were a screwdriver and a file. When he saw a screw that wasn't tight, he tightened it with his screwdriver. Finally, there were no more screws to tighten. But he saw some protruding nails. So he took his file and made grooves in the caps of the nails. Then he took his screwdriver and screwed them in. To paraphrase Marshall McLuhan's famous remark, "the medium is the message," the mathematician could well say, "the tool is the theory."

Rule (1988: 86) agrees: "For many thinkers, seeing one's theory 'fit' (any slice of reality that catches his/her fancy) is reason enough for acceptance of the theory, indeed for preferring it to others. But . . . we need a more rigorous standard. If one embraces a theory on the grounds that it 'fits' evidence that might as well support a version of other theories, the choice is more a statement of one's own inner world than about a shared exterior one." A determined theorist can always locate supporting illustrations and rationalize belief in the face of contrary evidence by reinterpreting an appropriate set of stylized facts. As Tolstoy (1968: 771) caricatured the point: "He was one of those theoreticians who so love their theory that they lose sight of the theory's object—its practical application. His passion for theory made him despair all practical considerations and he would not hear of them. He positively rejoiced in failure, for failures resulting from the theory only proved to him the accuracy of his theory."

If physical and biological scientists are eerily like the disciples of the rabbi of Krakow, social scientists may be worse. Consider rational choice theory. Some economic theories are comprehensive, unified, and elegant: Arrow-Debreu general equilibrium theory is the most prominent example. Other economic theories are frameworks or toolboxes that organize our thinking by housing many different models for analyzing various problems. Dixit (1996: 35) suggests that oligopoly theory is such an example:

> There are so many different issues that arise in the study of competition among a small number of firms that there is no hope of constructing a single analytical model of oligopoly on par with the standard elegant model of perfect competition. However, most people would agree that oligopoly remains a useful conceptual umbrella for sheltering the large variety of models that examine specific issues such as tacit collusion, strategic commitments, and preemption.

Arrow (1987: 226)—the general equilibrium theorist—identifies a problem with this perspective:

> I think there is a tendency in . . . this methodology to say, "here is a particular problem, I will make a set of assumptions, and here are the consequences; ah! yes in this case they worked out well." But I say, if these assumptions are true, they should be true for the next problem. In other words, there is a tendency to look only at the consequences that one happens to be studying at that moment, and not asking whether these assumptions can imply something quite different, whether they can be used in another field. In other words, it is not enough to test the assumptions in one field, one has to test them in others as well—something that Popper, for instance, would insist on.

A related foible in economic methodology is what Schumpeter (1954: 472) refers to as the Ricardian vice: strong exemplar cases and self-evident truths lead to a theory that assumes too much and produces tautologies, yet is applied to the world to make theoretical and policy inferences (Pheby 1988: 17). Green and Shapiro (1994; 1996) locate another related problem: rational choicers use a style of theorizing that advocates arbitrary a priori and a posteriori domain restrictions, so when their theory does not fit a case, the case is not treated as a disconfirming instance. Rational choice theorists, in other words, treat ceteris paribus conditions as "open ended escape clauses" (Kincaid 1996: 63).

The twin dangers of self-serving confirmation and nonfalsifiability bedevil rational choice theorists for yet another reason: their models are often deliberately heuristic rather than realistic. They may have, that is, instrumental value in probing the world rather than intrinsic value in mirroring it. Suárez (1999: 174) draws a valuable distinction between the two approaches to approximating theory to the world:

> They are, broadly speaking, two methods for approximating theory to the world. One is the approximation of the theory to the problem situation brought about by introducing corrections into the theoretical description—the theory is refined to bring it closer to the problem-situation . . . this is a form of approximation toward the real case: the corrections introduced into the theoretical descriptions are intended to account for the imperfections that occur in the problem situation.
>
> The other is the approximation of the problem-situation to the theory by means of simplifications of the problem situation itself . . . We idealize the description of the problem-situation, while leaving the theoretical construction unaffected . . . this process can come in either of two forms. It can come first in the form of conceptual redescriptions of the problem-situation, performed only in thought, and not in reality. In such "thought-experiments" complications are idealized away and the result is a simplified description of the problem-situation. Secondly, there is also the possibility of physical "shielding" of the experimental apparatus.
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> In the latter case the theory is left untouched, while the problem-situation is altered; in the former case the converse is true: the problem-situation is left untouched, while the theoretical description is corrected.

Such approximations to a problem-situation may have great heuristic value but at the cost of realism.

In other words, a rationalist model might not approximate the world at all. Indeed, it might be counterfactual to the average occurrence of reality (Reuten 1999: 198). Gibbard and Varian (1978: 667, 676) suggest that economic models often deliberately distort in order to emphasize some part of reality. They (1978: 665, 673) thus speak of caricature models that present "even to the point of distorting—certain selected aspects of the economic situation . . . Often the assumptions of a model are chosen not to approximate reality, but to exaggerate or isolate some feature of reality." Mayer (1993: 126) comments that "the value of such a model is that it brings out an important feature of the economy that was previously not given enough attention, and that it is robust with respect to the exaggerated assumptions." Modeling in economics is thus often heavily dependent on interpretive context, emphasizing significant features of the world and not describing it as a whole.

Other social scientific research communities also face the twin dangers of self-serving confirmation and nonfalsifiability. With respect to the culturalists, Durkheim often said "the facts are wrong" when confronted with evidence that contradicted his theories (Lukes 1985: 33, 52). With respect to the structuralists, the explanatory modesty of structuralists regarding the scope of their "historically concrete" arguments comes down to the assertion "that the theory should apply only where the evidence happens to fit, while instances of discordant evidence should simply be ignored" (Rule 1988: 71). While a theory with a limited domain is common in science, one that "holds only for certain special cases is not very exciting *unless* we can specify in advance what those cases will be. Perhaps what Weber said of the materialist theory of history holds for theories in general: They are not conveyances to be taken and alighted from at will" (Rule 1988: 89, emphasis in original). Hence, the Marxist theory of revolution is often saved by lengthening the time span: the revolution is *always* coming.[2]

Thus social science—whether practiced by rationalists, culturalists, or structuralists—often displays "built-in justification" (Boumans 1999) that produces the ex post validation of ad hoc modifications of failed theories. In other words, if a social scientist acts as if he or she can neither accept nor reject a theory, but rather acts as if the boundary, scope, or domain of the theory is defined in its application and elaboration, there is a danger of self-serving confirmations and nonfalsifiability. Unless scientists have a way to evaluate the application and elaboration of a theory, a science consisting of models of a theory (nuts and bolts) that mesh with models of data to explain particular problem domains is no science at all.

Why is one story preferable to another? How do we know when we have improved an existing story? What does a set of stories generated from one or more approaches tell us about the approaches?[3] Unless social scientists can separate real knowledge from mere opinion, every social scientist can claim that his or her story is guided by data and evidence and that his or her opponents' stories, too invested in misguided theories or ideas, are driven by dogmatic beliefs. Social science therefore needs criteria of theory appraisal that stand somewhere between positivism and relativism.

To clarify the problem of induction: empirical inquiry faces two fundamental and interrelated logical difficulties. First, facts, data, or observations are overdetermined by theories, hypotheses, or propositions and the supply of plausible rival hypotheses that can fit the same body of evidence is in principle infinite—social scientists, after all, can readily invent different theories to explain the same piece of reality (e.g., lots of causes of capitalism in the West or of economic success—and now failure!—in Japan). Hence, there are several alternative and incompatible ways to account for the facts: one can explain 100 percent of the variance in more than one way, divide the sample space with more than one approach, and locate multiple paths to the same outcome.

Second, theories are underdetermined by empirical evidence. On the one hand, we cannot conclusively verify theories or prove them true. The fallacy of affirming the consequence means that there will always be the possibility of committing a Type I error—rejecting a true null hypothesis. On the other hand, we cannot conclusively falsify theories either. The Duhem-Quine problem of auxiliary hypotheses means that we test theories as whole; if a test fails we do not know whether the test hypothesis or an auxiliary hypothesis is false. Hence, the probability of committing a Type II error—accepting a false null hypothesis—also cannot be reduced to zero. Any theory thus can be reconciled with some evidence.

The implications of these twin problems of induction run deep. Hume ([1739] 1984: 189, emphasis in original) writes that "*we have no reason to draw any inference concerning any object beyond those of which we have had experience.*" King, Keohane, and Verba (1994: 79) offer a modern restatement: "We can never hope to know a causal effect for certain." In other words, extrapolation from experience, from the present case to another case, is never completely justified. Probabilistic and nonprobabilistic theories of confirmation and falsification (Howson 2000) and the very notion of verisimilitude (Brink 2000) have deep philosophical problems. And there are even empirical grounds for this skepticism about empiricism. The pessimistic induction (Newton-Smith 1981: 14)—"any theory will be discovered to be false within, say, 200 years of being propounded"—or alternatively, science is "one damn theory after another" (Rouse 1987: 4), seems to fit the history of science quite nicely.

We are left with Maher (1993: 218): "The history of science is a history of false theories, and yet we want to say that science is making progress." Nagel (1953: 700), hoping to overcome Hume's problem, thus writes that the "basic trouble" with the philosophy of science is that "we do not possess at present a generally accepted, explicitly formulated, and fully comprehensive scheme for weighing the evidence for any arbitrarily given hypothesis so that the logical worth of alternative conclusions relative to the evidence available can be compared." The twin problems of induction tell us that just like there can be no methodical routine for doing creative work (i.e., a master science of discovery), there can be no inductive science of justification either. Nagel's (1953) hope for a unique scientific method for making the uniquely rational choice among the limitless number of contending theories, and hence a method that could explain and justify the change in scientific theories, is an impossible dream: The search for a computer program that can conclusively decide between competing theories is a chimera; the search for Algorithor, the philosopher of science who discovers the one true method, cannot succeed (Newton-Smith 2000: 4); and the search for "the methodologist's stone" (Newton-Smith 1981: 77) is fruitless. Scientists cannot quantify "degrees of confirmation," in effect "adding the weight" of many studies so that propositions are established "more or less." There is no algorithmic way to assess verisimilitude: how one hypothesis is close to the truth or closer to the truth than is another hypothesis.

This then is the fundamental indeterminacy of empirical work: important questions can not be entirely arbitrated by the sciences of deductive and inductive logic.[4] Logical empiricism, the positivist, received, or syntactic view favored by rationalist hegemons, overestimated the power of formal logic and measurement strategies to clarify the nature of theoretical claims.

So we must ask: How can observational data give us reasons for accepting or rejecting[5] a hypothesis that transcends the data? What principles can scientists use to weight evidence and make inferences that allow them to accept hypotheses that are true and to reject hypotheses that are false? If we cannot answer these questions, the disciples of social science are no better than the disciples of the Rabbi of Krakow.

Following van Fraasen (1980), "constructive empiricism" does not aim at "true" theories but only aims at empirically adequate theories—everything it says about observables is "true." As best as they can, scientists rely on judgment to establish that a model of a theory is consistent with a model of the data. Newton-Smith (1981: 232) thus writes that

> a practicing scientist is continually making judgments for which he can provide no justification beyond saying that is how things strike him. This should come as no surprise in a post-Wittgensteinian era. Wittgenstein repeatedly drew attention to the fact that we cannot specify usable, logically necessary and sufficient conditions for the application of many commonly employed predicates.
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> The time has come to model at least some aspects of the scientific enterprise not on the multiplication tables but on the exercise of the skills of, say, the master chef who produces new dishes, or the wine blender who does deliver the goods but who is notoriously unable to give a usable description of how

it is that he does selection the particular portions of the wines that add up taste-wise to more than the sum of their parts.

Since it is so difficult to assess the epistemic value of a theory, scientific judgment involves "beauty" and "justice" in addition to "truth" (Lave and March 1975). Pragmatic or aesthetic values, including consistency, parsimony or simplicity, and fruitfulness, fertility, scope, or unifying power, thereby enter science.

Let us focus here, however, on empirical criteria of theory evaluation.[6] Since facts are overdetermined by theories and theories underdetermined by facts, something else must determine our choice of explanatory theories. Absolute standards of theory evaluation are not available, so relative ones must be found. I therefore offer a broader approach to evidence that focuses on larger structures of knowledge. For those pragmatists who work within one paradigm to fit its models of theory (nuts and bolts) to models of data in a new problem domain, Lakatos's (1970) additional and true standard should be used; for those competitors who use the nuts and bolts that come from alternative paradigms, Popper's (1968) different and better standard is appropriate; and for those who synthesize different paradigms into one favored paradigm, nested models that combine the two standards are relevant.

Lakatos

The philosopher of science Imre Lakatos (1970) proposes a standard for evaluating a single research program. He suggests that scientists characterize each modification of a research program (i.e., an attempt to apply a program's nuts and bolts to a new problem domain) as "progressive" (1) if it can account for previous findings; (2) if it can predict "novel content" or some hitherto unexpected or counterintuitive observations; and (3) if some of these excess predictions resist falsification. A modification of a research program is "degenerative" if it is merely patchwork to explain an internally generated anomaly of the program and offers no new substantive insights into the new problem domain. Degenerative programs are, accordingly, autonomous and self-perpetuating, farther and farther removed from reality.

Consider an example from the rationalist approach to collective action. Lakatos's additional and true criteria ask the following: What, besides that protest groups do form and that their participants are rational, does the collective action research program tell us about a new case of collective dissent? Each new application of an existing solution to the free-rider problem must tell us something additional and true about the protest.

Solutions are potentially rich in their implications, focusing as they do on the group's actions (e.g., rhetoric, deeds), internal organization (e.g., membership characteristics, entrepreneurs), and external relations (e.g., competition with enemies such as the regime, cooperation with allies such as patrons). Showing that the collective action research program can tell us more about a new conflict than simply that rational people rebel demonstrates the heuristic value of the approach. It reveals the range of observations or the multiple outcroppings (Webb et al. 1981: 66–68) about conflict that the approach can explain. And it enables us to take a fresh look at existing theoretical arguments and empirical evidence.

Consider, for example, the selective incentives idea. The application of this solution to the Rebel's Dilemma could reveal many stylized facts about a new instance of protest or rebellion:

1. Rioters typically loot stores.
2. Voluntary members of a dissident group often attempt to become paid staff and make a career out of their participation (i.e., over time protest is professionalized).
3. Long-lived dissident organizations usually become oligarchical, with leaders receiving the majority of the benefits.
4. Government commonly co-opts leaders and "buys off" followers, and thus long-lived dissident organizations regularly become deradicalized.
5. Organizing manuals written by protest leaders frequently stress appeals to self-interest, and hence to immediate, specific, and concrete issues, rather than to altruism, and hence to ideology, programs, and self-sacrifice.
6. Organizational meetings and protest demonstrations routinely include food, drink, and entertainment.

These ideas tell us more than that participation in the new instance of collective dissent is rational. The existence of selective incentives determines what the various actors (e.g., participants, opposition leaders, government, patrons) do and how opposition groups become corrupt and change over time. While the selective incentives solution to the free-rider problem was initially designed to explain why rational people participate in rebellion, it explains much more—why protest and rebellion take particular courses and have particular consequences. The focus upon additional and true statements about protest is thus a particularly useful perspective for evaluating the new application of this old nut and bolt.

The Lakatosian approach is not without its critics, however. Many would argue that the deductive fertility of a research program—the variety of propositions that it can yield about a new problem domain—is a necessary but not sufficient condition for its value.[7] While valuable and important, there are four reasons why one should not overestimate the significance of any research program's ability to produce additional and true observations about a new domain of inquiry.[8]

First, a research program is only one of many research programs. Each has a more or less fertile agenda of topics for study. Some parts of the agendas of different research programs do not coincide. "Breakdown theories" of protest, for example, tell us that protest will occur during periods of personal pathology and antisocial behavior; the concomitants of protest will therefore be suicide, divorce, alcoholism, drug abuse, and vagrancy. Nothing in the collective action research program leads one to study these phenomena as covariates of protest. Other parts of the agendas of different research programs do coincide. Both collective action and grievance theories, for example, have been used to explain the same observations about the impact of economic inequality on collective dissent (Lichbach 1989, 1990).

Second, almost all the proponents of a given research program claim that the program can explain much of the empirical world by subsuming the important parts of competing research programs. Consider, once again, the case of collective dissent. Gurr (1970: 321) is obviously correct, from one philosophy of science perspective, when he argues that "one determinant of the adequacy of theoretical generalization is the degree to which it integrates more specific explanations and observed regularities." But claims about the deductive fertility and integrative capacity of the core ideas of research programs in conflict studies have been heard too many times. Gurr (1970), for example, too easily integrates status discrepancy, cognitive dissonance, value disequilibrium, and relative deprivation ideas under the frustration-aggression rubric. Tilly (1971: 416) thus likens *Why Men Rebel* to a sponge and maintains that "the sponge-like character of the work comes out in Gurr's enormous effort to subsume—to make every other argument, hypothesis, and finding support his scheme, and to contradict none of them." Students of conflict are thus justifiably suspicious about claims by supporters of the latest research program that the program is the key that "unlocks all conceivable doors" (Hirschman 1970: 330). Exaggerated claims succeed "only in provoking the readers' resistance and incredulity" (Hirschman 1970: 331). The derivation of innumerable "true" propositions from a research program is thus seen as a breathless search for cognitive consistency between new information and old perspectives, with all the inevitable elements of gimmickry and gadgetry. Hirschman (1970) understandably counsels modesty in the difficult search for truth and understanding. In fact, only a simple-minded positivism would lead one to try to subsume all theories under a single favorite theory (Lloyd 1986: 216).

Third, it is always easy to make deductions that support theories. Hence, accounts of the beginning of protest always seem to confirm culturalist theories and accounts of the end of protest always seem to confirm rationalist theories. If, for example, collective dissent occurs, culturalist theories conduct an ex post facto search for grievances while rationalist theories look for collective action solutions. If instead collective dissent does not occur, culturalist theories conduct an ex post facto search for the weakness of grievances and rationalist theories look for the Rebel's Dilemma. An example from Thompson (1966: 572) illustrates the point: "Yorkshire Luddism petered out amidst arrests, betrayals, threats, and disillusionment." Collective action theorists are trained to read "selective disincentives" and the "improbability of making a difference" into Thompson's diagnosis of why Yorkshire Luddism failed. Runciman's (1989: 367) warnings against "self-confirming illustrations pre-emptively immunized against awkward evidence" are quite relevant here.

Finally, it is easy to produce numerous deductions by adding numerous assumptions. Much "like a conjurer putting a rabbit in a hat, taking it out again and expecting a round of applause" (Barry, cited in Hechter 1990: 243), it is an approach that deserves no honors. Research programs in the social sciences often appear deductively fertile only because of an inelegant eclecticism: their assumptions are hedged so as to be able to account for much of the empirical world. But unless the assumptions behind a research program are parsimonious and precise, nothing of value has been accomplished, for anything can be derived from everything.

The consequence of eclectic theories is therefore that testing becomes impossible. Eckstein (1980) discovered this truth in conflict studies when he tried but failed (not *his* fault) to separate two important research programs, Gurr's (1970) version of culturalist theories and Tilly's (1978) version of rationalist theories, by determining which theory better explains the known facts about how social cleavages, the economy, repression, urbanization, and so on influence collective dissent. Eckstein points out that Gurr and Tilly surrounded their core assumptions with a "protective belt" by arguing that grievances and mobilizable resources are required for collective dissent. Both theories thus turned out to be eclectic.

Popper

Given these difficulties with the additional and true criterion, scientists often try to answer a second question about models of theories derived from their pet research program: Compared with other approaches, does my approach tell us things that are unique and more valid about the new problem domain under investigation? Scientists must show, in other words, that the implications of their pet theories are (1) original and pioneering and hence unexpected and counterintuitive, given other traditional wisdom in the field and (2) more valid than that traditional wisdom. The additional and true propositions about the new conflict derived from rational choice theories, for example, must also be different and better than those offered by alternative theories of conflict.

Truth, Popper (1968) tells us, comes out of the confrontation of ideas. A research program's models must therefore be tested against those of the competition. A scientist is consequently less interested in finding the best hypothesis from his or her pet program than in comparing the competing theories from different programs in a subject domain, a point well recognized by both philosophers of science[9] and practicing social scientists.[10]

The different and better criterion is particularly relevant because of the imperialistic tendencies of research programs. Many rational choice theorists have tried to push back the limits of their explanations (Bates and Bianco 1990: 351; Miller 1990: 343). Rapoport (1970: 300) thus comments on Riker's minimum-winning coalition prediction of the election of 1824: "How serious are we to take these calculations? Are not the conventional political interpretation of the Clay-Adams alliance more convincing? I do not know. If we could find situations where the predictions of 'conventional' political theory and the behavioral scientist's interpretation of n-person games seem incompatible, we could pit one against the other. In the above instance they are compatible and the question remains open."

In attempting to be integrative and eclectic, scholars often miss the value of Popperian-type crucial tests among paradigms in advancing middle-range theories and concrete explanations (Lichbach 1995: sect. 9.3). In their widely cited studies, however, Eckstein (1980) and McAdam (1982: chap. 4) develop "explanation sketches" of two or three alternative models of contentious politics and then explore several substantive domains to discover competing test implications. How do the paradigms differ? Do they yield competing predictions? Can we develop and test the predictions in a new problem domain, whether a broad sample, a carefully chosen set of comparisons, or a crucial case study?

Nonetheless, there is a problem with the different and better criteria. Researchers who consider themselves "problem driven," "puzzle directed," or "question oriented" often argue that synergisms of research traditions are valuable. Since this type of social scientist is interested in developing middle-range theories in some substantive domain (e.g., protest cycles) or historically concrete explanations of empirical happenings (e.g., fascism in Germany and Italy), he or she wants to draw freely upon rationalist, culturalist, and structuralist approaches to develop a single comprehensive theory or explanation.

Nested Models

As such scientists mine different approaches to construct explanations that address concrete problems and puzzles, they create the possibility for substantive syntheses. Similarly to how Weber used ideal types, scientists can draw on the nuts of bolts available in different schools to explain a historical puzzle. The combination can, to use a chemical metaphor, be a compound, mixture, or something in between like a colloidal suspension; or the combination, to use a biological metaphor, can range from true symbiosis to mutual coexistence. However intellectually cohesive the result, creativity comes from the reconciliation of differences and the attempt at synthesis. Modest rational choice theory, for example, moves cautiously from thin to thick rationality—incorporating cultural and structural alternatives into an individualistic decision-calculus—in an attempt to establish baselines and boundaries.

There is a research methodology that allows scientists to evaluate the results: nested models. In this approach, splitters develop a set of competing predictions that complement the set of predictions produced by the synthesizers. For example (Lichbach and Seligman, 2000: chap. 4): What do rationalist theories predict about regime transition? Or culturalist theories? Or a rationalist-culturalist consortium? Nested models enable scientists to evaluate the limitations of the pure theories and the value added of the combined one. This approach therefore allows creative competitions in new problem domains. Some combinations might work better in some of these domains than others.

At the level of paradigm, middle-range theory, or empirical explanation, such creative confrontations are to be preferred to flabby and facile syntheses. Rationalist and culturalist paradigms, for instance, can be used to generate ideal-type theories about regime transition that can serve as the models and foils that make theoretical and empirical work interesting and worthwhile. The dialogue between paradigms should therefore stress struggle over synthesis and competition over consortium; even syntheses and consortiums, via the nested models, can enter the struggle and the competition. Contending theories should always guide our research.[11]

The critical assumption here is that metastandards of evaluation exist and hence transparadigmatic connections can be fashioned. Popperians and those who use nested models thus search for this neutral language—an Archimedean point of the ideal observer, a transparadigmatic norm, and a theory-independent standard of comparison— that does not privilege one tradition over the other. Partisans counter, however, that there are often real differences among research schools, that competing theories do not share meanings, and that different theories cannot be translated into one another. The incommensurability or otherness of theories, that is, dooms interparadigmatic translations and transparadigmatic syntheses and researchers are trapped in their particular self-contained discourses. Since there is no metaframework, higher order language, first philosophy, foundations, or independent tribunal that can facilitate comparison of the separate local languages, the only standards are within-paradigm standards. Hence there can be no conversations among traditions, competitions among communities, and rational choice among paradigms.[12]

Davidson (1973–4) challenges this dogma of the separation of conceptual schemes and maintains that a conceptual scheme can be made intelligible to someone else. Wittgenstein (cited in Bhaskar 1997: 8) adds that one can see the fly in the fly-bottle only if one's perspective is different from that of the fly. Following Weber's point that one does not have to be Caesar to understand Caesar, one does not need to speak with Caesar to understand Caesar. More generally, there are analytical and empirical arguments against relativism and for the kind of transparadigmatic comparisons advocated by the synthesizers.

The analytical argument is that conflict implies mutual understanding or a common language within which disagreement can occur—a metastandard of comparability and translatability. As MacIntyre (1988: 370) puts it: "A precondition of the adherents of two different traditions understanding those traditions as rival and competing is of course that in some significant measure they understand each other." MacIntyre (1990: 5) continues: "To be able to recognize some alien system of belief and practice as in contention with one's own always requires a capacity to translate its terms and idioms into one's own. The adherents of every standpoint in recognizing the existence of rival standpoints recognizes also, implicitly if not explicitly, that those standpoints are formulated within and in terms of common norms of intelligibility and evaluation." Implied in incommensurability and incompatibility, or in disagreement and conflict, is some mutual understanding.[13]

The empirical argument against relativism is also straightforward: where is the evidence that scientists on opposite sides of a theoretical fence fail to comprehend one another? Common sense observation implies the exact opposite: Scientists often understand their disagreements and conflicts quite well. To write a history of science, moreover, is to assume that conceptual frameworks different from one's own can be made understandable.

Gellner (1998: 187) thus maintains that traditions or "cultures are not terminal. The possibility of transcendence of cultural limits is a fact; it is the single most important fact about human life." He (p. 191) continues: "Organic, self-contained social and conceptual cocoons cannot cope with either their internal or external conflicts. The notion of a culture-transcending truth emerges partly to cope with the resulting problems, partly to help explain the culture-transcending achievements of science."[14]

Summary

A common complaint about theories is that what is new is wrong and what is right is old; we therefore want models of theories (nuts and bolts) that grow out of a single research program to be additional and true (i.e., new and right) explanations of a new problem domain. Another common complaint about theories is that what is different is wrong and what is right is the same; we therefore want our models of theories to confront alternatives and to be different and better (i.e., different and right) explanations of a new problem domain. A model of a theory thus must ideally satisfy two criteria: it must account for some additional and true observations about a subject matter; and it must explain these observations differently and better than competing models of theories. If the theory is synthetic, both criteria should be applied to its component parts and the resulting consortium.

Modest positivists therefore should elaborate their favorite research program to discover its utility in explaining new problem domains; they should also compare its deductions to a stylized version of an alternative research program and to a synthesis of the two programs.

Foils matter. We social scientists can begin with our research interests and then turn to our colleagues who can further those interests: "When I start a new piece of research, the first thing I ask myself is, 'Who should I take to lunch?' " (Bates, as cited in Shafer 1994: 4). We can also begin with our colleagues who can help us define our research identities: "He who walks with wise men becomes wise" (*Proverbs* 13: 20). Whether our research interests/identities are the goals and our colleagues the means, or our colleagues are the goals and our research interests/identities the means, we need wise colleagues to serve as our models *and* foils. They are the foundation of a modest philosophy of social science.

Notes

1. This chapter derives from Lichbach (2003), and appears by permission.
2. Similarly, the rational choice theory of protest is saved by shortening the time span: almost no one is rebelling *now*.

3. Blaug (1992: 110) writes that "storytelling makes use of the method of what historians call colligation, the binding together of facts, low-level generalizations, high-level theories, and value judgments in a coherent narrative, held together by a glue of an implicit set of beliefs and attitudes that the author shares with his readers. In able hands, it can be extremely persuasive, and yet it is never easy to explain afterwards why it has persuaded." Blaug thus wonders how one validate[s] a particular piece of storytelling. One asks, of course, if the facts are correctly stated; if other facts are omitted; if the lower-level generalizations are subject to counterexamples; and if we can find competing stories that will fit the facts. In short, we go through a process that is identical to the one that we regularly employ to validate the hypothetico-deductive explanations of orthodox economics. However, because storytelling lacks rigor, lacks a definite logical structure, it is all too easy to verify and virtually impossible to falsify. It is or can be persuasive precisely because it never runs the risk of being wrong.
4. Related problems in logic are that one can deduce identical conclusions from different assumptions and that one can deduce true sentences from false premises.
5. I do not have the space to discuss the problems with falsification. See, for example, Lakatos's (1970) critique. Since many traditional positivists see it as a panacea for empirical work, I will, however, mention three interrelated difficulties. First, Laudan (1996: 218–19) writes that "it leaves ambiguous the scientific status of virtually every singular existential statement, however well supported (e.g., the claim that there are atoms, that there is a planet closer to the Sun than the Earth, that there is a missing link." Second, "it has the untoward consequence of counting as scientific every crank claim which makes ascertainably false assertions. Thus flat Earthers, biblical creationists, proponents of laetrile or orgone boes, Uri Geller devotees, Bermuda Triangulators, circle squarers, Lysenkoists, charioteers of the gods, perpetum mobile builders, Big Foot searchers, Loch Nessians, faith healers, polywater dabblers, Rosicrucians, the world-is-about-to-enders, primal screamers, water diviners, magicians, and astrologers all turn out to be scientific on Popper's criterion—just as long as they are prepared to indicate some observation, however improbable, which (if it came to pass) would cause them to change their minds." Third, falsifications can be endless whereas we must ultimately believe in the truth our theories: "In the old story, the peasant goes to the priest for advice on saving his dying chickens. The priest recommends prayer, but the chicks continue to die. The priest then recommends music for the chicken coop, but the deaths continue unabated. Pondering again, the priest recommends repainting the chicken coop in bright colors. Finally, all the chickens die. 'What a shame,' the priest tells the peasant. 'I had so may more good ideas" (*Economist*, June 29, 1996: 19–21, cited in Saffran 1997: 208).
6. Indeed, "judgment" with respect to "truth" can be enhanced. Research methodologists accept the trade-off of Type I and Type II errors, recognize that neither error can be reduced to zero, and try to develop valid research designs that move the curve closer to the origin. Campbell and Stanley (1963) and Cook and Campbell (1979) thus develop checklists of challenges to internal and external validity in experimental designs, and King, Keohane, and Verba (1994) propose valuable methods for small-n studies. While these research design issues are an essential part of the evaluation to follow (one element in Lakatos's approach is

that theories resist falsification and one element of Popper's approach is that a theory provides a better fit to evidence than another theory), questions of research design are not explored here.

7. For example, a sufficient condition for the collective action research program to be progressive in Lakatos's sense is that it meet the above tests for, say, collective dissent. Such tests, however, are not necessary. There are many substantive areas, such as interest-group activity and voting behavior, where the collective action research program may yield insights. Whether the program is valuable for protest and rebellion says nothing about whether or not it is valuable for these other fields. A Lakatosian analysis of the collective action research program therefore cannot be limited to a single domain of study because limiting the empirical focus deprives the analyst of the most novel implications of the program. Focusing on a single field does not yield the full picture of the progressivity of a research program. A Lakatosian evaluation would, on the contrary, determine the impact of the program on a number of different fields: collective action theories are thus progressive if they yield many diverse implications in many different substantive domains. The question, "Is the collective action research program progressive or degenerative?" cannot be addressed with respect to a single substantive domain such as collective dissent.
8. Conciliation with old facts is even less desirable (Feyerabend 1988?):

> Why should an ideology be constrained by older problems which, at any rate, make sense only in the abandoned context and which look silly and unnatural now? Why should it even consider the "facts" that give rise to problem of this kind or played a role in their solutions? Why should it not rather proceed in its own way, devising its own task and assembling its own domain of "facts"? A comprehensive theory, after all, is supposed to contain also an ontology that determines what exists and thus delimits the domain of possible facts and possible questions . . . New views soon strike out in new directions and from upon the older problems.

9. Given that theories are underdetermined by the facts, no amount of accumulated facts can lead to acceptance or rejection of a theory (Giddens 1979: 243). Only a better theory beats a theory. Feyerabend (1988: 24) thus advises scientists to "proliferate" inconsistent theories rather than eliminate rivals. He counsels pluralism and competition rather than authoritarianism and monopoly. Miller (1987: 140) offers the most extensive arguments here: "A theory is tested by comparing it with relevant current rivals. Very abstractly put, the question is which theory is a better basis for explaining phenomena . . . One confirms a theory by showing the best explanations of relevant phenomena appeal to instances of mechanisms in the repertoire of the theory rather than relying on rival theories." In other words, Miller stresses that each competing theory has a repertoire of causal mechanisms that can be applied to the relevant phenomena or subject domains it purports to cover. For each phenomenon or domain under investigation, the question is to find the theory that supplies the best causal mechanisms. And Most (1990) draws on Platt (1966) and offers a positivist

research-design to address the question of competing theories:

1. consider a phenomena or an existing result
2. devise as many alternative hypotheses as possible that might be capable of explaining it
3. for each hypothesis, specify additional predictive expectations that should hold if it is valid
4. devise a crucial experiment (or several of them) that will as nearly as possible exclude one or more of the hypotheses
5. move quickly to carry out the experiment to get a clear result
6. exclude the falsified hypotheses
7. recycle the procedure, making subhypotheses or sequential hypotheses to refine the possibilities, and so on.

10. Rule (1988: 43) argues that "rational choice models only move from the provocative and intriguing to the convincing by identifying sets of data for which the models provide better accounts than do alternative possibilities. We need more serious efforts to confront the models with such pertinent evidence." Mueller (1989: 193) maintains that "unless public choice-derived models can outperform the 'traditional, ad hoc' models against which they compete, the practical relevance of public choice theories must remain somewhat in doubt." Eckstein (1980) offers a classic test of rational actor versus deprived actor theories of protest and rebellion. Arrow's (1974: 65) comments about competition and authority systems also apply to competition and research programs: "The owl of Minerva flies not in the dusk but in the storm." Hence, social scientists should test their pet predictions of empirical regularities to see whether they are different from our existing understanding (i.e., preexisting theory) of the phenomena in question (Shapiro and Wendt 1992: 217): "This conformity with preexisting theories is important because in the realist view, all observation is theory-laden to a degree. Scientists compare theories not with 'the evidence' as empiricists claim but with alternative theories and background understandings of how the world works. Confirmation of a theory with those understandings is never a sufficient reason to accept it, but theory wildly at odds with them will inevitably bear a heavier burden of proof."
11. For an example of this approach in the field of domestic political conflict, see Lichbach (1997, 1998a, 1998b).
12. Pascal expressed this relativism or perspectivism in a way that is deceptively appealing to comparativists: "Truth is different on the other side of the Pyrenees." Or, as Norman (1983: 9), citing Protagoras, the most famous of the Sophists, put it "man is the measure of all things, of what is, that it is, and of what is not, that it is not.' Whatever seems to me to be the case, is true for me, and whatever seems to you to be the case, is true for you. No belief can be said to be true or false in itself, for there is no objective truth."

13. Kincaid (1996: 30–31) thus argues that Kuhn's (1970) relativism is self-defeating and self-referentially incoherent:

> If paradigms speak in entirely different languages, then they really never disagree. Since they share no meanings, they cannot assert what the other denies. Moreover, if meaning depends entirely on the overarching theory, then every difference in theory produces differences in meaning. So when any two individuals have different beliefs about the world, meanings will differ as well. According to Kuhn, however, differences in meaning preclude successful translation. Those who did not share Kuhn's theory of science should be unable to understand him.

The problem with pure relativism, the position that truth is bound in space and time (i.e., to cultures), is indeed self-referential incoherence: the only social scientific law is that there are no social scientific laws. This historicist doctrine has the same universalist pretensions as the social scientific doctrine. It is, however, self-contradictory and thrives only by exempting itself from its own conclusions. As Quine (1975: 238) puts it, "But if it were, then he, within his own culture, ought to see his own culture-bound truth as absolute. He cannot proclaim cultural relativism without rising above it, and he cannot rise above it without giving it up." One therefore cannot argue that all beliefs are unfounded, except this belief itself, or that all statements are biased, except this statement itself. Nihilism, relativism, perspectivism, and skepticism, goes the counterargument, are illusory and thus only lead to sophistry, casuistry, and historicism.

14. Since the incentives to strategically reveal their results and the collective action problem of monitoring their actions bedevil scientists, Moore's (1966: 356) "immunizing stratagem" is to be judicious and careful: "As one tries to grapple with the details of contradictory and fragmentary evidence, either of two things may happen. The certainty may evaporate into a chaos of ill-assorted facts, or else the evidence may be selected to produce an argument that runs too smoothly to be true." Moore (1978: xvi) thus promises:

> I had no intention of forcing the facts of German history though a conceptual sieve in order to "test" hypotheses. Historical facts have a certain patterned relationship to each other than such a procedure would obliterate and destroy. It is the task of the investigator to elicit this pattern through careful and critical attention to the evidence. It is necessary to proceed dialectically, patiently, listening for contradictory clues and signals, much as a skilled diagnostician tries to understand the set of organs and issues in a live human patient while searching for patterns that will reveal a state of health or a specific disease. Dissection and hypotheses are necessary in both forms of inquiry at certain points. But they are nowhere near enough.

Moore thus rejects relativism, or the idea that we are trapped in research communities.

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