The Rational Choice Controversy

Economic Models of Politics Reconsidered

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INTRODUCTION: ECONOMIC APPROACHES TO POLITICS

The debate over Green and Shapiro's Pathologies of Rational Choice Theory *sustains their contention that rational choice theory has not produced novel, empirically sustainable findings about politics—if one accepts their definition of empirically sustainable findings. Green and Shapiro show that rational choice research often resembles the empirically vacuous practices in which economists engage under the aegis of instrumentalism. Yet Green and Shapiro's insistence that theoretical constructs should produce accurate predictions may itself lead to instrumentalism. Some of Green and Shapiro's critics hint at a better approach, which would eschew predictively "testing the validity" of rational choice theory in favor of testing the* applicability *of the theory to particular cases. For this approach to work, however, the theory cannot be* assumed *to apply to any particular case.*

Politics has often been portrayed as a preeminent arena of the accidental, the emotional, the ideological, the habitual, and the traditional. Yet in recent years, growing numbers of scholars have attempted to apply to politics the postulates of rational choice theory. How can politics and, by implication, history be regarded as products of "rational choice"?

Public Choice Theory and Empirical Reality

To answer that question, a distinction should first be drawn between two

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terms that are often used imprecisely or synonymously: *rational choice* and *public choice*. One understanding of the difference holds that public choice theory applies economic analysis to political (i.e., "public") decision making, while rational choice theory goes even farther, applying economics to other nonmarket realms, such as family life. This distinction, to adopt John Ferejohn's (1991) terminology, attributes to both public and rational choice theory a "thin" understanding of the economists' rationality postulate: individuals are assumed to have only the inclination to satisfy their stable and ordered preferences, whether these are selfish or not. But outside the academy, public choice theory has a decidedly "thick" connotation, referring to the alleged propensity of political actors to pursue their material self-interest. This understanding of public choice theory, associated with its "Virginia school," is used here.[1] Accordingly, I mean by "rational choice theory" the much broader claim that, regardless of what sort of ends people pursue, they do so through strategic, instrumentally rational behavior. If public choice theory is false or its applicability is restricted, rational choice theory may yet be completely vindicated; but if rational choice theory fails, public choice theory goes down with it.

By systematically examining whether political actors are primarily motivated by selfish ends, public choice theory has raised crucial questions about the advisability of previously accepted policies, institutions, and political systems. Not only does public choice theory forcefully remind empirical scholars of Machiavelli's and Hobbes's suggestion that self-interest may animate putatively public-spirited policies, but it impels normative scholars to ask if any given substitution of political for market processes depends unrealistically on selfless voters, legislators, or bureaucrats.

Like most important ideas, however, public choice theory is liable to polemical oversimplification. The main danger is that the *possibility* that people are as self-interested in their political as their economic behavior may be treated as the *assumption* that self-interest is always and everywhere the real fountainhead of politics. This tendency is present even in the work of one of the most careful public choice scholars, James Buchanan, who writes, for example, that "the burden of proof should rest with those who suggest that wholly different models of man apply in the political and the economic realms of behavior" (1984, 13–14; cf. Buchanan and Tullock 1962, 19). The validity of Buchanan's argument is far from obvious. Scientifically speaking, the notion that people are as selfish in politics as in their economic dealings should be treated as skeptically as any other "model." That is, it should be treated as a hypothesis—even if it has *always* turned out to be true—simply because no claim about empirical reality can be assumed to be true a priori.

As a practical matter, of course, one may want to treat *as if* it were true, a priori, a theory that seems to explain an empirical regularity; that is, one may want to use such a theory as a rule of thumb (cf. Ordeshook below, 187) to guide one's future actions—but always with the caveat that new circumstances may alter the applicability of the "rule." Rules of thumb are not laws, and while, in a given time and place, one may cautiously generalize from observed self-interest, neither the prevalence of self-interest nor its magnitude when compared to other causal factors is an ahistorical given. Therefore, while political actors (as opposed to scholars) may justifiably treat past regularities as starting points for future predictions, such predictions remain hypothetical, and in the consideration of new cases, competing hypotheses—while perhaps less initially plausible—should bear no greater *evidentiary* burdens than old regularities. To insist on a double standard here would be dogmatic.

It is easy to see why public choice scholars may view their theory as self-evident rather than contingent. Most public choice scholars are economists, and the thick-rational picture of human beings as members of the species *Homo economicus*, while supposedly hypothetical even in economics textbooks, is, in the practice of economics, taken for granted as an obvious truth. We should be cognizant, however, of Buchanan and Gordon Tullock's admonition that "the ultimate defense of the economic-individualist behavioral assumption must be empirical. . . . The only final test of a model lies in its ability to assist in understanding real phenomena" (Buchanan and Tullock 1962, 28; cf. ibid., 21). Otherwise, models are likely to be mistaken for real-world phenomena even when there is little evidence that the models actually apply. And empirical researchers are liable to take for granted that finding a correlation between private interests and the adoption of a given public policy—which establishes the mere *possibility* that the public choice explanation is valid—actually demonstrates causality, and, moreover, causality of universal scope.

Perhaps for those reasons, the effort of comparing public choice hypotheses against alternatives frequently falls to non-public choice scholars. One such effort is Leif Lewin's *Self-Interest and Public Interest in Western Democracies*, published by Oxford University Press in 1991. Reviewing in detail the empirical literature on a variety of public choice claims—almost all of which was written by non-public choice researchers—Lewin found that in no case does public choice theory withstand scrutiny as a general hypothesis about the ubiquity of self-interest in politics. Thus, voters usually fail to "vote their pocketbooks"; when economic concerns are salient (which is not always the case; cf. Wattenberg 1991, 27), they tend to vote not egoistically, as public choice theory predicts, but "sociotropically," favoring the candidate or party they think likeliest to benefit the economy of their society as a whole (Lewin 1991, ch. 2; cf. Kiewiet 1983; Kinder and

Kiewiet 1981; Fiorina 1981; but see Nannestad and Paldam 1994). Nor is there plausible evidence for the allegedly pervasive "political business cycle": for the most part, politicians appear to have primed the pump before election day only in the mid-twentieth-century United States (Lewin 1991, ch. 3). And the growth of the public sector cannot be convincingly attributed to bureaucrats who maximize their agencies' budgets for personal gain; even if bureaucrats are assumed to be primarily self-seeking—an assumption persuasively disputed by Mark Kelman (1988, 218–20)—it is unclear how budget maximization would, even in theory, serve their selfish ends (see Blais and Dion 1991). Not surprisingly, then, little empirical evidence supporting the budget-maximization claim has been unearthed (Lewin 1991, ch. 4).

The evidence adduced by Lewin (and Kelman) suggests that there is, in fact, a disjunction between the prevalence of self-interest in the economy and in other spheres, contrary to the public choice assumption. A plausible reason for this finding is not hard to come by. In the modern West, it is widely assumed that personal gain is the legitimate goal of economic activity, while it is thought to be illegitimate in other spheres, such as political and personal life. Indeed, the economic realm could be *defined* as the arena in which selfishness is considered legitimate. It is only to be expected, then, that—to some extent—people will internalize and be guided by unselfish norms in noneconomic realms (cf. Steven Kelman 1987, 244–45; Green 1992). The extent of self-interestedness is therefore likely to vary historically as perceptions of appropriate behavior change. It would be foolish to deny the *possibility* that public choice theory will be applicable in a given instance, but it is equally unwise to assert in advance that it *must* apply in all cases merely because it applies in some.

Green and Shapiro's Critique of Rational Choice Theory

Since the claim that people are instrumentally rational seems to be even more "obvious" than the claim that they are selfish, it is impressive that rational choice scholars tend to take more seriously than their public choice colleagues the need to test their claim against empirical data. That they do is shown by the attention they have paid to a work that questions whether rational choice theory succeeds in explaining real-world phenomena: Donald P. Green and Ian Shapiro's *Pathologies of Rational Choice Theory: A Critique of Applications in Political Science* (Yale University Press, 1994).[2] While Lewin's *Self-Interest and Public Interest* made barely a ripple in public choice circles, Green and Shapiro's book was the subject of well-attended panels at successive meetings of the American Political Science Association and heated debates at dozens of American, Canadian, British, and Australian universities. In response to the controversy and to the important issues it raised, not only concerning rational choice and public choice theory but social science methodology, *Critical Review: An Interdisciplinary Journal of Politics and Society* published a double issue on Green and Shapiro's book in 1995, republished here.

Most of the contributors to the present volume agree that rational choice theory should be empirically tested rather than treated as an a priori truth. While some contributors applaud *Pathologies* or urge more uncompromising criticisms of rational choice theory (Abelson, Lane, Murphy, and Taylor below), others argue that Green and Shapiro misunderstand the theory (Lohmann, Schofield below), overlook its achievements (Fiorina, Kelley, Ordeshook, Shepsle below), or adhere to naive methodological standards (Chong, Diermeier, Ferejohn and Satz below). If a debate this complex can be summarized briefly, perhaps it can be said that most of their critics concede Green and Shapiro's main contention: that the applicability of nonobvious rational choice hypotheses to real-world political phenomena has yet to be demonstrated empirically —but only if we accept Green and Shapiro's view of what counts as an empirical demonstration. For the most part, then, the debate below tends to focus on the approach to empirical science that underpins Green and Shapiro's list of methodological "pathologies."

The first of these pathologies is "post hoc theory development" (*Pathologies*, 34–35)—known to statisticians as "curve fitting." Green and Shapiro contend that rather than formulating bold predictions that are falsifiable by empirical evidence, rational choice theorists tend first to look at the empirical evidence, then design a rational choice model that fits it. If disconfirming data should later come in, a new version of rational choice theory is concocted to redescribe the anomaly as "rational." A related vice, according to Green and Shapiro, is the tendency to compare rational choice theories against "either untenable alternatives or none at all" (ibid., 37)—as is often the case in public choice scholarship.

Second, Green and Shapiro contend that those rational choice predictions that are not amended post hoc are spared modification only because they rely on unmeasurable ("unobservable") entities, such as "equilibrium," making it difficult to detect whether the antecedent or "initial" conditions from which a predicted result should be expected to follow did, in fact, obtain.

Third, rational choice theorists allegedly engage in "arbitrary domain restriction." It is one thing, Green and Shapiro contend, to specify in advance *particular* reasons why a theory may not explain a certain class of phenomena. It is another thing, however, to suggest that rational choice theory is applicable "wherever the theory seems to work" (*Pathologies*, 45)—that is, that rational choice theory is valid *ceteris paribus*, where the scope of the caveat is unspecified by the theory.

Fourth, Green and Shapiro charge that rational choice predictions often only vaguely specify the magnitude of the effects being predicted.

Finally, they argue that rational choice theorists frequently search for confirming rather than falsifying evidence—or misconstrue the latter as the former.

The first three chapters of *Pathologies* and its conclusion, where Green and Shapiro describe rational choice theory and the methodological basis of their critique of it, may be the most important parts of the book. While there is insufficient space to reproduce the subtleties of those chapters here, we can illustrate the "pathologies" by surveying the middle chapters of the book, where Green and Shapiro substantiate their indictments by examining the best-developed applications of rational choice theory to American politics. A summary of their conclusions in these chapters may be useful to readers unfamiliar with the empirical issues to which many of the essays in this volume refer. Readers interested in the detailed argumentation and evidence for Green and Shapiro's conclusions, however, must consult *Pathologies* itself.

In Chapter 4, Green and Shapiro examine the phenomenon of voting—a major paradox for rational choice theory. In a real-world election with a large electorate, it is instrumentally irrational for anyone to cast a ballot, since no single vote has more than an infinitesimal chance of deciding the outcome. Whether one favors selfish or selfless ends, virtually any activity in pursuit of those ends would be more effective than the time spent on voting (and on educating oneself about candidates and issues). Yet hundreds of millions of people do vote. For rational choice theory, this would appear to be a gigantic anomaly.

Green and Shapiro point out that the usual response of rational choice theorists has been to modify their theory, post hoc, such that the "selective incentives" to vote come to encompass not only the material benefits that might flow to a voter from the election of a friendly candidate, but also, for instance, the psychic benefits public-spirited citizens feel when doing their duty by voting—regardless of the chance that their votes will affect the outcome. Hence the rational choice "point prediction" changes from no turnout to massive turnout. Another approach Green and Shapiro condemn is to declare that rational choice theory does not apply to cases (such as mass elections) where an individual's action is so inefficacious that instrumental considerations should not be expected to predominate in the first place. In Green and Shapiro's estimation, then, rational choice theory only avoids falsification by the phenomenon of voting by being either arbitrarily restricted to other domains or modified beyond recognition.

In Chapter 5 Green and Shapiro turn to other cases of "free riding." Voting in mass elections is but one instance of a larger problem for rational choice theory: why should anyone—again, irrespective of whether or not the goals she pursues are self-interested—devote resources of time or money to causes she favors, but has little chance of decisively assisting? Why not, instead, catch a free ride on the efforts of others to help the cause succeed? Rational choice theory would seem to be refuted not only by people who vote, but by those who contribute small amounts of money to political campaigns, attend rallies, and engage in other forms of collective action designed to secure goals whose achievement is independent of the efforts of any single participant.

According to Green and Shapiro, rational choice scholars avoid this problem by discussing only confirming evidence for their theory or by comparing it only to unchallenging null hypotheses. Oftentimes, rational choice accounts of collective action also "expand what counts as a selective incentive in order to evade problematic evidence" (*Pathologies*, 87), interpreting collective action as benefitting individual participants by enhancing their reputation, allowing them to express their convictions, entertaining them, or satisfying their sense of duty. As with post hoc attempts to make rational choice theory compatible with voting, here the criteria of rationality are so all-encompassing that rational choice theory becomes an unfalsifiable tautology.

In Chapter 6 Green and Shapiro take up rational choice models of legislative behavior. Rational choice theory shows that any parliamentary majority in favor of a set of legislative policies is unstable, in that a different majority can be found that will support a different policy combination. This should lead to aimless parliamentary "cycling" between different legislative equilibria; since this does not often occur, it could mean that manipulated or brokered legislative outcomes are masquerading as the unique will of the parliamentary majority. The unsettling consequences for democratic theory are apparent. But, Green and Shapiro ask, how well are rational choice models of parliamentary behavior borne out in practice? Such writers as Tullock accept that cycling is infrequent, but account for this anomaly by imputing parliamentary stability to logrolling, coalition building, and a host of other rational-choice-friendly post hoc possibilities that sidestep the question of verisimilitude by relying on ambiguities in their depiction of legislative "equilibrium" and "stability." Moreover, when rational choice work that compares the degree of instability produced by different legislative rules—since the theory suggests rules should be decisive—does not produce the hypothesized results, the theory is altered tautologically, such that "*when the assumptions of the model hold*, the prediction holds" (*Pathologies*, 144, emphasis original).

Finally, Chapter 7 considers "spatial theories" of electoral competition—in other words, theories that model candidates' issue positions by comparing them to graduated voter preferences that can be mapped as points on a line segment. Spatial electoral theories start with the postulate that candidates will converge toward the position of the "median voter," portraying themselves as *nearly* indistinguishable moderates so as to capture all the votes to the left or the right of the political center-point. Green and Shapiro complain that, despite the many analytic complications that have been added to this simple picture, spatial voting hypotheses have not been tested against posited stratagems that are not readily captured by such models, such as the manipulation of candidates' personal images; and that when, all too infrequently, spatial theories are tested, they do not seem to explain many American election campaigns. On the other hand, spatial models that seek verisimilitude tend to predict all possible outcomes, including the *divergence* of candidates' positions; and they leave unanswered the question of the *magnitude* of the various forces driving candidates away from equilibrium. Moreover, as with rational choice models of parliamentary behavior, spatial behavior models are often tautologized, Green and Shapiro contend, by their use of unmeasurables such as "risk aversion, discounting of future outcomes, beliefs about the likely behavior of others, [and] utilities derived from outcomes other than electoral victory" (*Pathologies*, 175-76). With such vaguely defined forces at work, "it is unlikely that spatial models . . . can be estimated statistically" (ibid., 176).

A few remarks might bring some perspective to the debate over whether such practices are indeed blameworthy by highlighting parallels between them and the practices, and methodology, of economics—where rational choice theory originated (but see Murphy below).

Rational Choice Theory in Political Science and Economics

An appropriate starting point is the objection, made below by Ferejohn and Debra Satz, Morris Fiorina, and Kenneth Shepsle, that Green and Shapiro's list of pathologies seems to require that rational choice theory be subjected to "state-of-the-art statistical analysis" (Fiorina, 90) in order to qualify as empirically substantiated. Observers of the debate may find this argument surprising: if anything, one might have expected the *critics* of rational choice theory to champion humanistic approaches to social science against rational choice reductionism. Yet it turns out that rational choice theory is itself seen by some of its most prominent proponents as more humanistic (that is, as suffering fewer of the distorting effects of scientism) than are the critics' standards of proof.

In reply, Green and Shapiro point out that *Pathologies* does not "advocate the exclusive use of quantitative methods in political science" (below, 245). To equate science with statistics would, Green and Shapiro suggest, be as wrongheaded as the rational choice pathologies they identify: in both cases, scholarship would be driven by researchers' precommitment to particular methods rather than by their determination to understand political phenomena. All they ask, Green and Shapiro write, is that scientific attitudes toward data selection, measurement, and reasoning prevail, "whether the mode of inquiry be ethnographic or statistical" (ibid.; cf. *Pathologies*, 172). Is it possible that their critics have, nonetheless, correctly detected a hyperpositivist tendency that, despite Green and Shapiro's intentions, is somehow implied (if not entailed) by their argument?

Here recourse to the recent history of economics may be useful. Until the middle of this century, the mainstream methodological view, derived from both classical and Austrian economists (see Hausman 1992b, 123, and Caldwell 1982, 103-4), emphasized "the self-evident nature of the basic postulates of economic theory" (Caldwell 1982, 99). And basic economic assumptions were not only seen as obviously true; they were unfalsifiable in principle, since they were held to apply only *ceteris paribus*—that is, in the absence of countervailing factors, which John Stuart Mill called "disturbing causes." Since such disturbances are frequently or always present, economic theory was thought unable to predict exactly what will happen in a given instance. On the other hand, economic theory *was* held to be realistic in the sense that its assumptions do hold good when no disturbances are present. Two independent claims are at work here: first, that economic assumptions identify efficacious initial conditions that are universally present in human affairs—the self-evidence postulate; second, that the predictions following from these assumptions are inexact, because other conditions may counteract them in a given instance—the *ceteris paribus* postulate.

In the 1940s and 1950s economists began to abandon the classical/Austrian orthodoxy. Terence Hutchison's *The Significance and Basic Postulates of Economic Theory* (1938) pioneered the new, positivist approach that would eventually lead to Milton Friedman's enormously influential "The Methodology of Positive Economics" (1953). Hutchison argued that basic economic assumptions (such as instrumental rationality), being untestable in principle, are devoid of scientific merit. "The price of unconditional necessity and certainty of propositions of pure logic and mathematics (and of propositions of pure theory) is," he wrote, "complete lack of empirical content" (Hutchison 1938, 27). For

> the *ceteris paribus* assumption makes out of an empirical proposition that is concerned with facts, and therefore conceivably can be false, a necessary ana-

> lytical-tautological proposition. . . . The *ceteris paribus* assumption sweeps all the unknowns together under one portmanteau assumption for a logical "solution." (Ibid., 1938, 42)

Norman Schofield points out below that the prepositivistic method criticized by Hutchison is still practiced by microeconomists—who continue to derive postulates about economic interactions from rational choice assumptions (the very ones that have, in turn, been borrowed by some political scientists). But while microeconomists may be accused of relying on the *ceteris paribus* self-evidence of their models as a substitute for extensive empirical research, macroeconomists, who study economy-wide aggregate phenomena, adhere more closely to the instrumentalist version of positivism that is widely accepted as the "official" economic methodology. According to instrumentalism as formulated by Milton Friedman, a theory should be judged by its predictive accuracy, not the realism of its assumptions. Thus, macroeconomists tend to assess the validity of generalizations that have relatively weak theoretical foundations by seeing how well they account for statistical data, or they simply make predictions based on statistical regularities that have no theoretical foundation at all.[3]

All four of Green and Shapiro's "pathologies" are (they contend) objectionable for reasons that are strikingly similar to both Hutchison's positivism and Friedman's instrumentalism. That is, they are "pathological" inasmuch as they inhibit the testing of rational choice theory by definite predictions. When Green and Shapiro allow only those post hoc, falsification-avoiding expansions of rational choice theory that can survive subsequent attempts at predictive falsification; when they demand that the theory predict magnitudes and specify observables in ways that can be tested; when they disavow ad hoc shrinkages of the domain of application because this averts falsification by inaccurate predictions; and when they condemn searches for confirming rather than falsifying evidence, they are attempting to secure predictions that can serve as tests of the theory's validity. Given the association in economics between Friedmanite predictivism (see Caldwell 1992) and (macroeconomic) statistical testing, it is understandable that Shepsle should characterize Green and Shapiro as "political statisticians . . . contemptuous of anything short of . . . systematic, comprehensive, and sophisticated data analysis" (below, 219).

Strictly speaking, however, Green and Shapiro's methodological posture is agnostic. They compare rational choice claims against a range of methodological criteria—Hempelian, Popperian, Friedmanite, and Lakatosian—that mirror the variety of methodologies to which rational choice theorists appeal. Green and Shapiro's enumerations of methodological "pathologies," then, merely hold rational choice theory to its own advertised standards. What these standards share, however, is a commitment to judging theories by their predictive accuracy. An unintended consequence of Green and Shapiro's immanent methodological critique is to expose a tension between this predictive criterion for the success of rational choice theory and the implicitly prepositivist defenses of the theory adumbrated by its most astute advocates below.

For example, several contributors to this volume contend that, instead of being vilified, post hoc theorizing should be applauded by those interested in the empirical merits of rational choice theory—since post hoc amendments are designed to incorporate empirical findings into the theory. Only if one's aim is to test a theory against its predictions will one object to post hoc theory expansions (or ad hoc shrinkages)—since such amendments make the theory's predictions unfalsifiable. And, like the prepositivist economists, Fiorina (below, 88) emphasizes "the importance of *ceteris paribus*."

Yet most of the defenders of rational choice do not repudiate the aspiration to discover social-science laws testable by their predictions; indeed, several explicitly endorse this aspiration. If one accepts that laws, or lawlike regularities, can be falsified by incorrect predictions, how can one oppose predictivism?

One way is to endorse one of the central tenets of the prepositivist economic approach: the postulate of the *self-evident applicability* of the laws in question. Thus Fiorina, Shepsle, Ferejohn and Satz, and Dennis Chong express something like a conviction that rational choice assumptions are obviously true—like the basic postulates of microeconomics, in the old view. Chong, for instance, writes that "there is an essential degree of rationality in most behavior" (below, 39), and Fiorina interprets Mancur Olson's *The Logic of Collective Action* as arguing that (*ceteris paribus*) there is a *universal* tendency for people to behave in ways that will frustrate large-scale collective action in the absence of selective incentives. These sentiments echo those of Mill; of the Austrian economists Friedrich von Wieser (see Hutchison 1994, 213) and Ludwig von Mises (1981); and of the great expositor of the prepositivist orthodoxy, Lionel Robbins—all of whom held that "the propositions of economic theory . . . are obviously deductions from a series of postulates" that "are so much the stuff of our everyday experience that they have only to be stated to be recognized as obvious" (Robbins 1935, 78–79).

The Future of Political Science?

Ironically, the parallels between rational choice theory and prepositivist economics are even clearer when one notices the *official* hegemony of instrumentalism across *both* levels of economics. Just as Green and Shapiro show (e.g., *Pathologies*, 30–31) that rational choice theorists invoke a variety of predictivist doctrines in support of practices that are, according to those very doctrines, pathological, even microeconomists profess allegiance to Friedmanite instrumentalist predictivism—despite their failure to investigate the accuracy of the predictions generated by their assumptions (not to mention the accuracy of the assumptions themselves). Although microeconomic assumptions are quite similar to rational choice theory in their abstraction from reality (and their content), microeconomists accept in principle that their predictions must, to be worthwhile, be tested—some day, and preferably by someone else.

Why did instrumentalism supplant the prepositivist economic orthodoxy? Less than a decade after Hutchison's first assaults on the classical/Austrian view, Richard Lester conducted a survey showing that businesspeople do not necessarily try to maximize expected returns (see Hausman 1992b, 159)—contradicting an "obvious" microeconomic assumption. In response, Fritz Machlup took up the task of defending microeconomics on predictivist grounds. Like Friedman, Machlup suggested that economists should learn from up-to-date positivism (logical empiricism) that good theories need not use realistic assumptions. Realist forms of positivism should be discarded, he argued, and theories should be measured against the success of their predictions. The only apparent alternative would have been to accept that Lester's results *falsified a theory* that, however flawed in its *assumptions*, did generate "fairly good results in many applications" (Machlup 1956, 488; cf. Ordeshook below, 184). But Machlup's and later Friedman's instrumentalism left open the possibility that economics would be falsified by inaccurate *predictions*. (How likely is it that an unrealistic theory will make accurate predictions?) So instrumentalism came to be interpreted as licensing economic theorists to bracket not only the realism of their assumptions but that of their predictions, too.

Most of the contributors to this volume, in contrast, argue that rational choice assumptions are realistic, but may not be predictive because of disturbing causes. In short, rational choice assumptions are both self-evidently true and unfalsifiable. Like prepositivist economics, this view is vulnerable to Lester-like evidence against the assumptions' realism—such as that amassed below by Robert P. Abelson and Robert E. Lane. It is far from inconceivable, then, that an instrumentalist version of rational choice theory (e.g., Downs 1957, 21) will supplant the realist view in political science, as it has in economics. On that day, we can probably expect that the political science "stars" will be those who produce formal models of extremely limited empirical relevance, and that researchers concerned with empirical data will be thought "dunces" who aren't bright enough to do the really important work: theorizing (Mayer 1993, 70). As Thomas Mayer writes, "the typical academic economist . . . feels less like a scientist when gathering data, or thinking about data, than when doing the mathematics of a paper" (ibid.). Even though it should encourage hypothesis testing rather than formal speculation, economists have treated instrumentalism as "a carte blanche for making whatever assumptions provide a tractable model. Formalists have found this highly convenient, since it seems to imply that their 'if-then' reasoning can by itself solve empirical science problems" (ibid., 51–52).

It has become almost routine for prominent economists to bemoan the distance between economic theory and reality. As long ago as 1971, Nobel laureate Wassily Leontief condemned "the ephemeral substantive content of the arguments behind the formidable front of algebraic signs" generated by the "unbridled enthusiasm for mathematical formulation" (quoted, Mayer 1993, 2). In the previous year, Frank Hahn had referred to the "scandalous . . . spectacle of so many people refining the analyses of economic states which they give no reason to suppose will ever, or have ever, come about" (quoted, ibid.). More recently, Arjo Klamer wrote that "most members of the profession . . . will confess, usually at unguarded moments, that their highly sophisticated research produces ultimately meaningless results," and he asked how long "irony and cynicism [can] sustain the economics profession" (quoted, ibid., 3). And Franklin Fisher has deplored, as producing the "games that economists play," the "strong tendency for even the best practitioners to concentrate on analytically interesting questions rather than on the ones that really matter for the study of real-life industries" (quoted, ibid., 63). The same problem afflicts public choice theory. The originator of the bureaucratic budget-maximization hypothesis, William Niskanen, now writes (1993, 151) that "much of the [public choice] literature is a collection of intellectual games. Our specialty has developed clear models of first and second derivatives but cannot answer such simple questions as 'Why do people vote?'" While macroeconomists use instrumentalism to justify predictions bereft of theoretical underpinnings, microeconomists—and public choice theorists—use a degenerate form of instrumentalism to sanction implausible theories with scant predictive power.

Uniting Theory and Data

Instrumentalism bridges a gap that confronts virtually all science: the distance between facts and our understanding of them; precise laws and inaccurate predictions. There are at least two important reasons for this fissure.

The first problem is that outside the laboratory (and even within it), the world is usually too complex to allow the complete isolation of the variables in which one is interested. Only a unified and true theory of everything, encompassing all the more limited theories human beings can understand, would apply with precision at all times and in all places. Thus, even if the hypothesized independent variable is present in a given case, other possible causes of the dependent variable are likely to be present as well, ruining predictions about the magnitude produced by the hypothesized cause. Moreover, "disturbing causes" may offset the action of the independent variable, diminishing the dependent variable or making it disappear. While a true theory may predict that X will cause Y to occur, not-Y may yet occur in the presence of X because of the overpowering effect of other variables. Self-interested voters might tend to vote their pocketbooks, but their nationalistic education might obviate this tendency. The question is which tendency will, in a given instance, predominate, and the answer may very well have to wait until after the fact.

The problem of complexity encourages prudent scholars to attach *ceteris paribus* clauses to their theories. But this can obliterate their predictive power, making them suspect in the eyes of positivists such as Hutchison and Friedman. Yet in theory, if not in practice, the instrumentalist alternative, like Green and Shapiro's critique of rational choice theory, sacrifices theoretical realism to the pursuit of predictions. This is why Green and Shapiro are taken to task for (allegedly) privileging the statistical over the plausible. As Ferejohn and Satz write, an understanding (*Verstehen*) of human behavior in terms of human intentions is "necessary for any explanatory . . . rather than merely predictive or descriptive" science (below, 74).

The second problem is the unobservability of some real causes, such as gravity. In the physical sciences this problem can be solved by instrumentalism: one can ignore the verisimilitude of one's assumptions about unobservables in favor of worrying exclusively about the predictions they generate. But in the social sciences, theories that provide *Verstehen* must include assumptions about unobservable mental states, and these are not consistent enough across all people at all times to produce accurate behavioral predictions unless they are defined so broadly that the "predictions" are empty tautologies.[4] Similar actions—such as the votes cast by several different electors, or the budget-maximizing actions of several different bureaucrats—cannot simply be treated as equivalent for predictive purposes, for they may have different motivations. Even when disturbing causes *are* held constant, we want to know whether the agents in a given real-world instance actually have the particular attitudes that constitute the initial conditions of a nontautological social-science theory.

That is what Lester asked about microeconomic theory. His answer was, Not always. The response of Machlup and Friedman was to render the question nugatory by severing any direct link between observed behavior and

plausible independent variables. This accomplished, theoretical and empirical research became dissociated from each other: microeconomic model-building now takes place without regard to the verisimilitude of the posited initial conditions, and realistic theoretical assumptions no longer check macroeconomic predictions.

The middle chapters of *Pathologies* show why this process has not yet been completed in political science. True, one category of rational choice research consists of formal mathematical derivations of equilibrium solutions to largely imaginary problems, based on admittedly unrealistic initial-condition assumptions. But in a second category is rational choice research into observed behavior. Since rational choice assumptions are no more universally applicable than are those of any other nontautological model, research of this type sometimes fails to make accurate predictions. In response, researchers who, were they in economics, would tend to disregard the theoretical implications, instead either (1) expand the theory toward tautology, so it can predict more (or all) outcomes; or (2) resort to ad hocery, admitting that nontautological rational choice theory tends to apply unpredictably, not universally. Green and Shapiro deride the first stratagem as post hoc theory development and the second as arbitrary domain restriction.

A more radical, yet constructive critique of rational choice theory could be imagined, however, based on the belief that social science should offer realistic (in the sense of *verstehende*) explanations of human actions. Since unrealistic modelling implies a repudiation of this proposition, rational choice theorists who want to avoid the cynical irrelevance that characterizes modern economics should be congratulated, not chastised, for modifying their theory, whether (2) explicitly (by means of arbitrary domain restriction) or (1) implicitly (by further tautologizing the definition of rationality, subsuming behavior previously studied by social psychologists, "ethnographers," and the like).

Such modifications, however, fly in the face of the assumption that nontautological rational choice theory is universally applicable. Tautologizing rational choice or restricting its applicability means renouncing a priori assumptions about what causes will be at work in any given instance. Precisely because we cannot know in advance whether people will be instrumentally rational, we cannot predict their behavior. Post hoc or ad hoc rational choice theory allows Weber's categories of intrinsically valuable, traditional, and emotional action; Green and Shapiro's "habit, blunder, and the like" (*Pathologies*, 27); Abelson's and Michael Taylor's "expressive" action; and so forth, (1) to be incorporated into rational choice theory, or else (2) to set its limits.

Yet Green and Shapiro oppose this approach because, committed (at least for purposes of argument) to one or another form of predictive testing, they must condemn post hoc and tautologizing alterations that immunize a theory from falsification by inaccurate predictions. Their alternative (and that of Taylor and Stanley Kelley below), which would narrow the predicted applicability of rational choice theory by spelling out a list of theoretically justified (hence predictable) domain restrictions, will inevitably encounter the very indeterminacies they criticize in "universalistic," a priori rational choice theory. For while such lists increase the relevance of the theory's initial conditions in the remaining domains and reduce the need to appeal to the *ceteris paribus* clause, we can assume that some degree of unobservability and complexity will always remain, continuing to debar accurate point predictions (or retrodictions) or even comparative statics. Although rational choice theory's accuracy will have improved, it will still be "falsified" in real-world applications; both its assumptions and its conclusions will remain unrealistic (as with all valid but incomplete theories), and if economics continues to show the way, this may well lead, in reaction, to instrumentalism.

What is needed is a form of scientific method that uses theory to explain facts; restrains theory with facts; yet allows for an imperfect fit between the two. Such an approach would both encourage domain restrictions *and* accept that untheorized inexactitude or inapplicability does not falsify theories. It would thereby sanction rational choice research that uses ad hoc criteria of applicability—or tautological formulations of "rationality." By allowing the domain of rational choice to shrink when its predictions prove false, the needed approach would recognize that any theory that could completely specify its conditions of inapplicability with no trace of ad hocery would be part of a perfectly comprehensive Laplacean determinism, of a sort to which human beings cannot aspire (see Murphy below). With such godlike knowledge, no empirical research would ever be necessary (cf. Chong below, 45–46; Ferejohn and Satz below, 82). By allowing its domain to expand to the point at which many "irrational" behaviors are redefined, on the other hand, the needed method would recognize that nothing rides on whether a given behavior is described as "rational"—so long as it is clear what one means by the term.

Such a method requires treating theories as ideal types[5] (cf. Lane below, 124) which are derivable from any source (but which, in the social sciences, provide *Verstehen*); which are devised to explain imagined patterns of behavior; *but which are not falsified by inaccurate predictions of actual behavior*, because variations in initial conditions, and disturbing causes, may always interfere. Like the positivism Daniel Diermeier discusses below (62–63), the ideal-type view denies that the purpose of prediction is to falsify *theories*, rather than to falsify the hypothesis that a certain theory applies to a certain case. The purpose of empirical research, therefore, is to confirm or falsify the possibility that a *particular* event is an instance of the operation

of a certain "law"—that is, research may demonstrate that the posited cause is present and has not been undone by countervailing forces (cf. Shepsle below, 220). Ideal-type formal models have no scientific value until empirical research begins to assess the extent of their applicability and the magnitude of their effect in particular cases. (One may, of course, define a "particular case" as broadly as one wants.) The main goal of social science is the determination of how far, in given cases, the initial conditions of theoretically derived hypotheses hold good and are not "disturbed."

In this view, only empirical research can falsify or verify the hypothesis that a certain ideal type explains a certain phenomenon in the real world. *All* logically coherent theories are true in the sense that if their initial conditions hold, all else being equal, then the posited consequences will always follow (cf. Chong below, 49). But if the purpose of theory building is to develop ideal types whose hypotheses are relatively *accurate*, the question is whether the initial conditions *do* hold and whether all else *is* equal. In social science, morever, these constraints must not be merely fortuitous, as under instrumentalism; the accuracy of one's hypotheses must flow from the accuracy of one's assumptions. The ideal-type approach does sanction a certain amount of theoretical fantasizing, since any number of logically coherent assumptions (even when subject to the *verstehen* requirement) *might* prove applicable in the real world, and it is vital to think through the consequences of one's assumptions (i.e., the consequences if one's ideal-type initial conditions are present, are capable of producing the posited effects, and are not counteracted). But the ideal-type approach also builds into theorizing a bias toward interaction with empirical research—for any theory, that is, which is intended to be scientifically relevant.

It is a peculiarity of *verstehende* theories that unless counteracted, their initial conditions automatically produce the effects posited by sound deduction. If one is self-interested or instrumentally rational then, *ceteris paribus*, certain behavioral effects follow as a matter of course. There is no question as to whether the unobservable force (the mental state) is capable of producing the consequent behavior, as there might be a question about whether gravity can pull two bodies toward each other; no empirical investigation is needed to determine the efficacy of mental states in producing behavior if they are present and are not defeated, say, by ignorance about appropriate means. Thus, any internally coherent *verstehende* ideal type, no matter how fanciful, may be said to be applicable (to *all* of reality) if it relies heavily enough on the *ceteris paribus* clause and on the mere assumption that its initial conditions are "obviously" present. But only some *verstehende* ideal types will turn out to apply (to a specific *part* of reality) without heavy reliance on these assumptions. Empirical research is required if we are to distinguish whether, in a given instance, a valid theory is of the first or the second type.

The advantage of the ideal-type approach is that, by placing the inexact fit of theory and reality at center stage, it redirects scientific research away from attempting to use empirical data to verify or falsify laws, instead using it to show how close or how far competing "laws" come to explaining actual behavior in a given case. This obviates the dilemma social scientists must face when their theories are, inevitably, unable to predict behavior: either come up with endless excuses about the inaptness of the test, or beat a quasi-instrumentalist retreat from the empirical fray into ivory-tower theorizing. The ideal-type approach circumvents the dilemma by recognizing from the outset that scientific laws cannot be held to all-or-nothing standards in the real world. No humanly devised theory can predict the frequency or magnitude with which ideal-typical behavior, or an approximation of it, will appear in reality. A theory cannot be falsified by a single bad prediction, or even a string of them, once we recognize that each falsification concerns only the application of the theory to a particular case, and shows merely that the theory is less than universally applicable to the real world.

Since no negative result can be considered a definitive "test" of anything but a theory's applicability in the specific instance examined, the assumption that a theory that has always produced relatively accurate predictions in the past will continue to do so in the future is unwarranted; until we achieve perfect knowledge, conditions may unexpectedly change. The most accurate explanations will tend to be retrodictions (unless we unexpectedly achieve omniscience); the best social science will be historical. However, once one has discovered a theory whose initial conditions (which may include path-dependent historical circumstances) obtain and are not counteracted in the given cases, one may make the theory the basis not only of scientific retrodiction, but of practical predictions of future likelihoods. Upon these, one bases the design of one's legislation or one's bridges (cf. Ordeshook below). But the difference between "engineering" and science always should always be kept in mind.

The ideal-type approach has many similarities to the prepositivist economic orthodoxy. Chief among these are that they both emphasize what Daniel M. Hausman (1992b) calls the "inexact nature" of social science; and that in both, "empirical studies are used to suggest plausible subsidiary postulates, and to check on the applicability of the theoretical framework to given situations" (Caldwell 1982, 103). There is one key difference, however. The old view, especially in its Austrian version, vitiated its commitment to check the empirical applicability of a framework by assuming, a priori, that economic assumptions were not just ideal types, but laws that were (largely if not perfectly), in fact, reflected in the real world. The tenet of self-evidence militates against doing empirical research to determine the extent of a theory's accuracy in a given instance. This tenet, in other words, is the source of the universalistic apriorism Green and Shapiro attack.

Yet by holding rational choice theory to its predictivist methodological canons, Green and Shapiro leave instrumentalism as the only viable alternative to universalism. No "partial universalism" can undo the gap between data and predictions—unless it is a "universalism" so partial that it institutionalizes the gap, refusing to assume that theories bear any resemblance to reality beyond that which is proven, case by case. In this way the ideal-type approach incorporates Green and Shapiro's antiuniversalism, without their predictivism—and the antipositivist sentiments of many of Green and Shapiro's critics, without their apriorism.

One of the more important implications of this synthesis is to legitimize not only inexact predictions and ad hoc/post hoc theory alteration, but searches for confirming evidence. If the task of empirical research is to see whether, and to what extent, a theory explains a particular slice of reality rather than to see whether reality falsifies a theory, positive evidence will be even more valuable than the negative kind.

The problem with many searches for confirming evidence is that they aim to prove the universal applicability of the assumptions underlying the theory. Accordingly, they illegitimately extrapolate from favorable cases and ignore unfavorable ones. The reviewer of Lewin's book in the journal *Public Choice*, for example, attempts to brush aside its troubling findings as "very selective," inasmuch as Lewin "fails to mention any experiments in public choice and rational choice theory" or to discuss the many cases in which self-interest does play a political role (Cain 1993, 379). As a response to the overly broad question with which Lewin opens the book—"Is it self-interest or public interest that predominates in public life?"—the reviewer asks legitimate questions. But, implicitly taking an even broader approach, the reviewer must ignore Lewin's evidence *against* self-interested voters, politicians, and bureaucrats.[6] The reviewer's preoccupation with (unidentified) confirming evidence could only be relevant to the disconfirming evidence Lewin presents if one views the purpose of public choice theory as the establishment of universally predictive laws, rather than the explanation of particular events.

Rational Choice Theory As an Ideal Type

Olson's *The Logic of Collective Action*, by contrast, is a model of ideal-type research.

Olson's argument is directed against "group theory" in political science, which held that individuals join groups (and, by implication, that they vote) on the basis of their collective self-interest. Olson points out that in groups with large memberships, such as labor unions and business lobbies, the contribution of each member to the achievement of collective goals is negligible, so the potential members' interests would better be served by free riding on others' contributions than by making pointless sacrifices for the collective benefit. Therefore, the observed existence of large groups requires some other explanation than collective self-interest. Olson finds this explanation in selective incentives that benefit group members, such as various services offered to the members of business lobbies; in the case of unions, "compulsory membership and picket lines are . . . of the essence" (Olson 1965, 71). Without such "incentives," the self-interest assumption of group theorists would generate a point prediction of zero collective action in large groups, and would therefore be falsified in virtually every case.

As Fiorina notes below, however, zero is not *Olson's* point prediction, even in the absence of selective incentives (cf. *Pathologies*, 82). Unlike group theorists—and public choice theorists—Olson notes that groups may consist of "altruistic individuals"; unlike rational choice theorists, he concedes that groups may contain "irrational individuals" (Olson 1965, 2; cf. ibid., 108). In such cases, selective incentives may not be needed to get people to join large groups.

By interpreting Olson's caveats as invocations of the *ceteris paribus* clause alone, Fiorina suggests that Olson posits an always-present *tendency* for large groups to require selective incentives, albeit a tendency that may be offset by "a host of other factors" (below, 88; cf. Chong below, 39). The a priori conviction that certain initial conditions always apply (while allowing that countervailing forces may offset them) entails the self-evidence of a theoretical assumption, and therefore violates the ideal-type method. An alternative would be to interpret Olson as saying that there may be different kinds of people, and that his theory does not apply to all of them: when considering altruistic and irrational people, in short, the initial conditions do *not* obtain. Undoubtedly Fiorina is right to point out that people often fail to behave selfishly or rationally *despite* satisfying the antecedent condition of having selfish or rational dispositions. In such cases, conflicting factors (such as ignorance or mixed motives) may need to be invoked by means of the *ceteris paribus* clause. But surely there can be no reason to assume, a priori, that people are always self-interested or rational, even *ceteris paribus*.

Olson qualifies his admission that the rationality and self-interest assumptions may not apply in some cases by suggesting that this is "usually of no practical importance" (1965, 2). Just how important it can be, however, becomes clear in the closing pages of his book, where Olson turns to discuss "noneconomic lobbies" and "groups that are characterized by a low degree of rationality" (ibid., 160, 161). In these cases, he proposes, "it would perhaps be better to turn to psychology or social psychology than to economics for a relevant theory" (ibid., 161).

Thus, by insisting that the presence of instrumental rationality and self-interest must be proven in each instance, Olson is drawn to agree—both with most of its defenders in this volume, and with such fierce critics as Lane—not only that rational choice theory can be compatible with other explanations, but that it can *facilitate* them. As Susanne Lohmann (below, 131) argues, "without a theory of how much people should contribute to a public good if they are rational and self-interested, there is no way of assessing whether empirical contribution patterns reveal altruism, systematic misassessments of the probability that a given contribution will be decisive, or total incomprehension on the part of the individuals involved." Were its practitioners less determined to protect rational choice theory ***in general*** from "falsification" by any single instance (or less prone to proceed as if such instances do not exist), they might more freely recognize that their theory lends great credibility to alternative approaches, such as social psychology, public opinion theory, and intellectual and cultural history, in the many *particular* cases in which its predictions are inaccurate (cf. *Pathologies*, 67; Weber 1949, 102).

Chong's paper exemplifies the other way to use rational choice theory as an ideal type: by expanding its boundaries to include "so-called extrarational incentives" (below, 40). Instead of restricting its domain of applicability, he deals with recalcitrant data by incorporating them into a broader version of it. The danger here is the temptation to ignore social-psychology constructs that cannot easily be interpreted as "rational."[7] If due caution is exercised, however, Chong's tautologizing approach would seem to be an acceptable remedy for the unrealistic assumptions and empirical irrelevance of rational choice theorizing discussed by Green and Shapiro.

Rational choice theorists should do more than tolerate alternative forms of explanation. Properly understood, their greatest contribution may be to demonstrate how *infrequently* political behavior exemplifies instrumental rationality—let alone the instrumentally rational pursuit of self-interest. But this is a matter for research, not speculation.

NOTES

1. I disregard the claim, made early in Buchanan and Tullock's *The Calculus of Consent*, that public choice theory assumes only instrumental rationality and

is agnostic about the types of ends (e.g., selfish or altruistic) that individuals pursue (Buchanan and Tullock 1962, 17).

As in most instances where this claim prefaces an exercise in economic theory, Buchanan and Tullock do, in fact, go on to assume self-interested ends, and accordingly they abandon their "praxiological" or thin-rational agnosticism (ibid., 29) in favor of thick-rational, *Homo economicus* assumptions. There seem to be three reasons for this retreat.

First, as do most economists, Tullock and Buchanan need to supplement the sparse requirements of instrumental rationality with the assumption that individuals are selfish "to a degree sufficient to make prediction and explanation possible"; otherwise, we would not be able to say whether prices reflected instances of "individual buyers deliberately pay[ing] to sellers higher prices than is necessary to secure the product or service purchased, [or of] . . . sellers deliberately accept[ing] lower prices than buyers are willing to pay" (Buchanan and Tullock 1962, 18). The praxiological/"thin"/rational-choice approach, Buchanan and Tullock argue, "cannot develop hypotheses about the results of political choice in any conceptually observable or measurable dimension. To take this additional step," they must therefore "move to . . . a more narrowly conceived submodel" in which not only rationality but selfishness is assumed (ibid., 29).

Second, Buchanan and Tullock juxtapose their "essentially economic approach to collective activity" against what they take to be the orthodox political science "assumption that the representative individual seeks not to maximize his own utility, but to find the 'public interest' or 'common good.'" The terms of this juxtaposition require Buchanan and Tullock to portray political actors as people who do *not* equate their own utility with the public interest or common good, and who *are* therefore "profit-seeker[s]" (ibid., 20).

Finally, Buchanan and Tullock seem to assume that, even granting that the same motivations drive people's economic and their political behavior, it follows that political agents must be motivated by self-interest. But this interpretation of the symmetry assumption overlooks the possibility that people could be consistently *selfless* in both the economy and the polity (which the young Marx envisioned), or that they could display a consistent *mixture* of selfish and selfless motives in both spheres.

My public choice/rational choice division also departs from Lohmann's (32) equation of rational choice theory with behavior that is both rational and self-interested; and from Dennis Mueller's (1989) treatment of literature on both thick-rational (e.g., rent-seeking) and thin-rational (e.g., legislative cycling) phenomena as falling under the rubric of public choice theory. This treatment contradicts Mueller's thick-rational equation of public choice with "the application of economics to political science" (rather than vice versa); i.e., the application to political science of the view "that all men pursue their private interests" (Mueller 1989, 1).

2. Page references to *Pathologies* in this introduction and in Green and Shapiro's reply are identified as such. Unidentified parenthetical page references in the introduction and the reply refer to other chapters in this volume, while unidentified parenthetical page references elsewhere in this volume refer to *Pathologies*.
3. This is a simplification; formal rational choice theory has, in fact, been applied not only to microeconomics but macroeconomics since the advent of the New Classical effort to provide microtheoretical, rational-expectations foundations for macroeconomics.
4. The latter problem can be illustrated by referring to the most aprioristic of the prepositivist economists, Ludwig von Mises, who criticized Weber's use of ideal types precisely because they are not universally applicable laws. Von Mises's tautological version of rational choice theory, "praxeology," appears to be what Buchanan and Tullock have in mind when they retreat from "praxiology," or rational choice theory as instrumental rationality in the "thinnest" sense, to the more definitely selfish assumptions of the *Homo economicus* ideal type (see n1 above).

Von Mises (1981, 81) defines as "the fundamental law of action" (on which economics is to be grounded) the apodictic certainty that people will act to achieve whatever it is that they "subjectively conside[r] mo[st] important," and he criticizes Weber (1978, 24–25) for failing to see that value-rational, affective, and traditional behavior are, by (von Mises's) definition, instrumentally rational. The difficulty is that such a thin definition of instrumental rationality generates a tautology that says little about human behavior (except that, as von Mises defines the term, it is always and everywhere "rational"). Without using distinctions among rational actions as von Mises defines them—distinctions of the sort provided by Weber's "thicker" ideal types—how, for example, are we to distinguish prices that reflect "individual buyers deliberately pay[ing] to sellers higher prices than is necessary to secure the product or service purchased" from prices that reflect sellers who "deliberately accept lower prices than buyers are willing to pay" (Buchanan and Tullock 1962, 18)?

Weber advises that in such cases—i.e., in all cases in which actual behavior is the topic of inquiry—we rely on empirical investigation (or on extrapolation from other investigations) into whether a given price was the outcome of instrumental bargaining or else exemplified, say, action taken for its intrinsic worth or for emotional or traditional reasons. Weber uses as an example "the generalization called Gresham's Law" (Weber 1978, 10), which is but "a rationally evident anticipation of human action under given conditions and under the ideal-typical assumption of purely rational action. Only experience," he writes, "can teach us how far action really does take place in accordance with it. This experience does in fact demonstrate that the proposition has a very far-reaching validity" (von Mises's translation of Weber 1978, 10, in von Mises 1981, 86).

In reply, von Mises denies that Gresham's Law applies only "under the ideal-typical assumption of purely rational action," but then he admits that Gresham's Law does not always produce an accurate prediction of people's behavior. While continuing to affirm that even behavior that is not predicted by the Law is "rational" in his sense of the word, von Mises allows several circumstances in which "the assumptions of the law do not apply," adding however that "experience teaches that for the mass of creditor-debtor relationships these assumptions do apply" (von Mises 1981, 87). As the reader may readily affirm, this is precisely Weber's position, in almost exactly the same words.

Von Mises and Weber agree that (1) *ceteris paribus* and (2) whenever its initial conditions apply, Gresham's Law accurately predicts that bad money drives out good. The question, then, is how to determine when these two conditions are satisfied—which cannot be done a priori. The apparent difference between their positions stems from Weber's effort to emphasize the importance of empirical investigation, which leads him to call the ideal type in question "so-called 'Gresham's law'" (von Mises's translation, 1981, 86). Von Mises's response is, in effect, that it is a *real* law. But this response obscures the fact that von Mises, too, in retrodicting actual behavior, must treat the regularity in question as an ideal type of purely contingent, a posteriori *applicability* to a given case.

It is evident from the chapter in which his critique of Weber appears ("Sociology and History," ch. 2 of von Mises 1981) that von Mises needlessly exaggerates the aprioristic implications of his view because in his mind, he is still fighting the *Methodenstreit* against the German Historical school of economics.

5. There are several differences between the methodology set forth here and Weber's view of ideal types (see Weber 1949, ch. 2, and Weber 1978, pt. 1, ch. 1). I have retained from Weber the idea of theoretical constructs with no necessary empirical referent; but, *inter alia*, the notions that these constructs may be wholly false or wholly true, and that they can be used to characterize natural as well as social science, may go beyond Weber's account.
6. The reviewer's only comment about this evidence is that it does not adequately "operationalize and test" the self-interest axiom in the areas it covers, by which he seems to mean that "self-interest" should be tautologized to include altruism, and that survey results probing mental states should be ignored in favor of overt behavior.
7. Chong (below, 42–43, 47) prefers this dangerous approach because of his fear that many non-rational choice theories are so "sociological" that they lack microfoundations. But see Schumpeter 1950 (cf. Prisching 1995) and Converse 1964 for methodologically individualistic accounts of the origin of irrational beliefs. I believe Chong may have an unnecessarily restrictive view of what constitute adequate microfoundations, leading him to reject ad hoc invocations of noninstrumental, nonselfish motivation.

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