

In the end, whether rational choice theory is thought of in covering-law or Friedman-instrumental terms, empirical testing cannot be escaped. On either view, a theory of politics has no payoff if its hypotheses do not survive empirical scrutiny. In this light, it is surprising that both defenders and critics of rational choice theory have paid so little attention to empirical testing. It is to that subject that we now turn.

## CHAPTER THREE

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### METHODOLOGICAL PATHOLOGIES

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Whatever may be said on behalf of the analytic elegance or heuristic value of rational choice theories, empirical applications have tended to suffer from two classes of methodological infirmities. The first encompasses what may be described as pedestrian methodological defects. Scholars working within the rational choice tradition from time to time misapply statistical techniques, overlook problems of measurement error, or rely excessively on inferences drawn from a small number of case studies. Although potentially serious, methodological shortcomings of this kind come with the territory in political science and are not the main focus of our critique.

More interesting is the syndrome of fundamental and recurrent methodological failings rooted in the universalist aspirations that motivate so much rational choice theorizing. These concern the ways hypotheses are conceptualized, the manner in which they are transformed into testable propositions, and the interpretation of empirical results when tests are conducted. We contend that these (often mutually reinforcing) mistakes stem from a method-driven rather than problem-driven approach to research, in which practitioners are more eager to vindicate one or another universalist model than to understand and explain actual political outcomes. More than anything else, it is this aspiration that leads to the errors that we describe here as the pathologies of rational choice theory. We make good on the claim that these are *characteristic* failings in Chapters 4 through 7, where we review in systematic fashion rational choice literatures on turnout, collective action, legislative behavior, and electoral competition. In this chapter we describe and illustrate these methodological failings, explaining why they are at odds with basic requirements of sound empirical research.<sup>1</sup>

1. It is not our position that every attempt to test rational choice models empirically goes

### POST HOC THEORY DEVELOPMENT

Many of the methodological failings of applied rational choice scholarship are traceable to a style of theorizing that places great emphasis on the development of post hoc accounts of known facts. Can a rational choice hypothesis explain the existence of seniority systems in Congress? The growth of deficit spending by governments? Why people vote for third parties? To answer such questions the theorist engages in a thought experiment designed to generate an explanation of a given phenomenon that is consistent with rational choice assumptions, somehow specified. Fiorina and Shepsle (1982, 63) offer a lucid description of this approach:

Our position is that scientific progress reflects (1) the scholarly *choice* of models that (2) possess equilibria that (3) correspond to observed regularities. This entails neither constructing equilibrium models *ex ante*, generalizing and refining subject to the constraint that equilibrium be preserved . . . nor retaining disequilibrium models only to be tongue-tied when asked to say something positive about the world. . . . To travel the first path is to say little that applies to the world of phenomena, and to travel the second is to say little, period. Instead, we recommend a third path, one termed "retroduction." . . . Put simply, the retroductive process begins with an empirical regularity X and poses the question, "How might the world be structured so that X is an anticipated feature of that world?" The answers (and there should be several) are models, all of which have in common the regularity X as a logical implication.

To be sure, striving to explain observed empirical regularities is preferable to fashioning theories according to the dictates of "neatness, or other aesthetic criteria" that otherwise guide rational choice theorizing in both political science and economics (Fiorina and Shepsle 1982, 63). But given the lack of specificity about what it means to be a rational actor, it is not obvious what sorts of behaviors, in principle, could fail to be explained by some variant of rational choice theory. Rational choice theorists have at their disposal a variety of assumptions about actors' objectives (wealth, power, moral satisfaction, etc.) and the extent to which individuals derive utility from the well-being of others, as well as the sorts of information and beliefs actors possess, their tastes for risk, the rate at which they discount future rewards, whether their decisions are informed by reasoning about strategic behavior of others, and, if so, the decision rules used when actors face conditions of uncertainty.<sup>2</sup> As

awry. But as we point out in the chapters that follow, in those rare cases when appropriate tests are appropriately conducted, the results seldom sustain any novel or counterintuitive propositions.

2. Although rational choice theory is often advertised as a unified approach to the study of social, economic, and political behavior, we saw in Chapter 2 that there seem to be few constraints on the assumptions that underlie empirical accounts, and sometimes quite

Ordeshook (1993, 95) points out, those who craft post hoc explanations have not necessarily achieved much: "Even if such models fit the data up to an acceptable level of statistical accuracy, we must contend with the fact that we can establish nearly any reasonable outcome as an equilibrium to some model, provided only that model is sufficiently complex. . . . Designing assumptions so that a model's predictions fit the data is, in fact, little more than an exercise in curve fitting, albeit of a slightly more complicated sort than the type we generally hold in disrepute."

One indication of the ease with which post hoc accounts may be generated is that a great many sufficient explanations arise to explain phenomena such as nonzero voter turnout or differences between the platforms of the two American parties. Another indication is that sufficient explanations pop up to explain certain "stylized facts" that, on reflection, are not facts at all. McKelvey and Riezman (1992, 951), for example, set for themselves the task of explaining both why incumbent legislators tend to be reelected by wide margins and why legislatures have seniority systems. But neither of these premises holds for legislators or legislatures generally. The reelection rates of U.S. senators and representatives contrast sharply, and the strength of the seniority system in Congress has varied over time. Furthermore, statistical studies of Congressional elections (Feldman and Jondrow 1984; Ragsdale and Cook 1987) detect no evidence of the putative causal connection between seniority and incumbent electoral fortunes. Under these circumstances, it is difficult to know what to make of McKelvey and Riezman's analytic result that in equilibrium legislators adopt a seniority system and voters unanimously reelect all incumbents.<sup>3</sup>

One might at this point object that what we are calling post hoc theorizing might well be characterized as puzzle-solving, a legitimate scientific activity. It could be argued, for example, that the fact that voters go to the polls in large numbers despite the theoretical prediction that rational citizens abstain leads to the discovery of civic-mindedness. Our reservation about such "discoveries" (if they may be described as such) is that retroduction merely establishes the proposition that it is not impossible that some rational choice hypotheses might be true. Often rational choice theorists seem to regard this as the end of the exercise; that the post hoc account they propose indeed vindicates the approach of looking at politics as though it were populated by actors who approach "every situation with one eye on the gains to be had, the other eye on costs, a delicate ability to balance them, and a strong desire to follow wherever rationality leads" (Downs 1957, 7-8). Data that inspire a theory cannot, however, properly be used to test it, particularly when many post hoc accounts furnish the same prediction. Unless a given retroductive account is used to generate hypotheses that

contradictory motives are imputed to agents, depending on the domain of application (Mueller 1979).

3. McKelvey and Riezman (1992, 958) caution that their model implies more than one equilibrium. An alternative equilibrium is one in which "seniority is rejected by the legislature and all legislators are defeated for reelection."

survive when tested against other phenomena, little of empirical significance has been established.

For example, many rational choice theorists have sought to explain why, as Schumpeter (1942, 261) put it, "normally, the great political questions take their place in the psychic economy of the typical citizen with those leisure-hour interests that have not attained the rank of hobbies." The hypothesis of "rational ignorance" (Downs 1957) holds that citizens know little beyond what they can learn costlessly, because they have no incentive to expend resources to become knowledgeable about political affairs. In light of the small probability that any voter's ballot will prove decisive in an election, the rational citizen reasons that the benefits of casting a well-informed vote will not offset the expenditure of time and money spent gathering information. As we note in Chapter 5, this argument is widely touted as a successful explanation of what is taken to be widespread voter ignorance. But since other post hoc explanations for voter ignorance are imaginable, one must ask: Why should we put stock in *this* explanation? What else does this account tell us about the conditions under which voters will or will not invest in costly information?

Post hoc theories are not only tested inadequately, the manner in which they are developed tends to be in tension with the enterprise of empirical testing. To the extent that theorists exploit the ambiguity in the meaning of rationality to transform successive disconfirming instances into data consistent with a newly recast theory, one must question whether the succession of theories is susceptible to empirical evaluation in any meaningful sense. As we will see in subsequent chapters, rational choice theorists seldom set forth a clear statement of what datum or data, if observed, would warrant rejection of the particular hypotheses they set forth or, more generally, their conviction that politics flows from the maximizing behavior of rational actors.<sup>4</sup>

These problems of empirical evaluation are compounded by the fact that rational choice models of a given phenomenon are difficult to evaluate vis-à-vis alternative theoretical perspectives that are not rooted in the assumption of utility maximization. In principle as well as in practice, rational choice models may be constructed from a wide assortment of assumptions about beliefs, tastes, and environmental constraints. Not surprisingly, rational choice models may generate diametrically opposing predictions. Some rational choice accounts, for example, predict that collective political action will collapse under the weight of the free-rider problem; others suggest that such movements may be sustained by solidary incentives. Some variants of rational

4. It is not hard to understand why rational choice theorists might be reluctant to relinquish the propositions that they advance. Leaving aside rare instances in which theorems rest on flawed proofs (e.g., Austen-Smith and Riker 1987), these propositions *are* true as analytic statements. Rational choice theorists, therefore, often regard empirical setbacks as indicating a given theorem's limited range of application. As we point out in Chapters 5 to 7, theorists in this position often cling to the notion that the forms identified in a theorem are fundamental and operative, even if they are offset in specific applications.

choice theory predict that candidates in a two-party system will adopt identical platforms, while others assert that candidates will adopt divergent political stances. That constructions of rational choice theory predict X and Not-X creates vexing problems for those seeking to compare the performance of rational choice models against competing perspectives. The predictions of one rational choice model will invariably overlap with those derived from another kind of theory.

Alternative theoretical accounts, it should be noted, occupy a small pedestal in the rational choice pantheon. The drive for sufficient accounts of political phenomena often impels rational choice theorists to focus instead on what the theory *does* seem to explain. As Russell (1979, 11) notes, this style of analysis is often accompanied by a striking disregard for alternative explanations, leaving open the question of whether the data conform equally well to the predictions of competing theoretical accounts. Sometimes the failure to consider the relative strength of rational choice versus alternative explanations stems from mere sloppiness or parochialism. More often, however, it results from a faulty approach to theorizing that stresses the formulation of sufficient explanations. Ironically, the insistence on pressing one form of explanation to the exclusion of others has the effect of diminishing the persuasiveness of rational choice accounts.<sup>5</sup>

Because of the lack of interest in competing explanations, research is seldom designed with an eye toward rejecting a credible null hypothesis, a conjecture accorded presumption of truth by the researcher, in favor of a rational choice-derived alternative. The null hypothesis that the researcher seeks to reject is frequently rather prosaic—for example, the hypothesis that experimental electors vote randomly (McKelvey and Ordeshook 1984) or that behavior is unresponsive to changes in price (Wittman 1975).<sup>6</sup> Just as overcoming an adversary like Grenada does little to attest to the military might of the United States, one's views of politics are not much influenced by the fact that a rational choice proposition vanquishes a trivial or implausible null hypothesis. This is not a critical failing, but we should accord explanatory power to rational choice theories in proportion to the credibility of the null hypotheses over which they triumph. More often than not, rational choice scholars consider either untenable alternative explanations or none at all.

In sum, when post hoc theorizing is used to come up with possible rational choice

5. Olson's rational choice explanation for the economic decline of Britain (1982), for example, surely would have been more compelling had he compared (or even mentioned) any of the more than half-dozen competing explanations (see Cameron 1988). Much the same may be said of the large literature that places the blame for inflation and the growth of government at the doorstep of democratic institutions and the incentives they engender (see Barry 1984; Mueller 1989, chap. 17).

6. Wittman (1975, 738) offers (though does not test) the hypothesis that those given paid time off work in order to vote will be more likely to do so. He also suggests that turnout will be higher, all things being equal, among citizens in good health.

explanations of observed phenomena or to reformulate rational choice hypotheses in ways that evade or appear to account for anomalous instances, the rational choice theorist may believe that the theoretical approach has in some significant way been "saved." In reality, the specific hypotheses in question have yet to be tested.

This critique of post hoc theorizing is not meant to foreclose the possibility of genuine theoretical innovation. Our point is not that theoretical predictions can never be changed to accommodate new evidence. Rather it is that the "innovations" that typically emerge do not involve new predictions as such; they involve mere redescription of the processes by which a previously known outcome obtains. Having recast their hypotheses to encompass known facts—and, in particular, anomalies—rational choice theorists typically fail to take the next step: proposing a coherent test to gauge the empirical adequacy of the newly revised hypothesis. Even less often do they take the step after that: gauging the empirical power of their preferred theoretical formulation against that of alternative explanatory accounts.

### FORMULATING TESTS

To test a theory, one needs to know in advance what the theory predicts. From time to time, certain rational choice theorists have expressed discomfort with the lack of attention devoted to this aspect of applied rational choice scholarship. For instance, in 1978 Fiorina and Plott observed that "game theoretic and social choice—theoretic models . . . are developed and advocated without a hint of possible operational definitions—one can find proof upon proof, but one searches in vain for a detailed discussion of exactly how and when a model should be applied" (575–76). Concerns of this kind, however, have had surprisingly little impact on the evolution of rational choice scholarship, and the imbalance between analytic exposition and application remains marked.

Those who seek to derive testable propositions from rational choice models frequently find, moreover, that these theories are constructed in ways that insulate them against untoward encounters with evidence. This problem turns up in various forms. Those who advance models so parsimonious or abstract that recognizable features of politics are all but absent (for example, models of policy making that omit mention of political parties and treat each branch of government as unitary actors [Banks 1989; Spiller and Spitzer 1992]) deflect empirical scrutiny by describing their theories as simplifications or first cuts at thorny theoretical issues. Others assert that their models capture general truths that need not coincide with specific applications, as when Calvert (1985, 87) defends a model of candidate strategy "because it reveals the properties that underlie all electoral competition, even though these properties may be counteracted by the particular conditions of a real world situation" (see also Strom 1990, 11).

Arguably the most important source of slipperiness in model building is the multiplication of unobservable terms, which causes the complexity of a theory to outstrip the capacity of the data to render an informative test. This general problem is compounded by the specific difficulties that attend the ambiguous translation from equilibrium models to empirical tests.

*Slippery Predictions* Rational choice explanations typically comprise an array of unobservable entities. Tastes, beliefs, decision rules, and, at a higher order of abstraction, equilibria, form the essential ingredients of most rational choice models. The problem is not the positing of unobservable terms per se, but rather the ratio of latent constructs to observable measures in rational choice accounts.<sup>7</sup> As this ratio grows, it becomes increasingly difficult to establish whether a set of data confirms or disconfirms a rational choice explanation.

Consider, by way of illustration, a game in which two players must divide \$14 between them. If the players can agree on how to allocate the money, then that agreement becomes binding; if no agreement is reached, then player 1 receives \$12, and player 2 receives nothing. "Cooperative game theory," note Hoffman and Spitzer, "predicts that the subjects will cooperate and divide the rewards \$13 to \$1 (the Nash bargaining solution: an even division of the \$2 gain from trade). Under no circumstances should [player 1] settle for less than \$12, according to game theory" (1985, 259). Suppose that after repeated observations of this game actually being played, one encountered a substantial number of resolutions in which the players divided the \$14 evenly.<sup>8</sup> What may be inferred from this pattern of results? That the dollar amounts were too small to induce preferences over and above preexisting tastes for fairness? That despite the proscription of threats, player 1 feared physical retaliation from player 2? Mistaken understanding of the game? A temporary departure from equilibrium that would be rectified through greater exposure to the real world of cutthroat negotiations?

As this example indicates, rational choice hypotheses that meet with unanticipated facts may be resuscitated by recourse to a variety of unobservable thought processes

7. The problem is exacerbated to some degree by the skepticism with which rational choice scholars regard "psychological" measures of tastes and beliefs. Although tastes and beliefs figure prominently in rational choice explanations, many scholars working within this tradition question the validity of measures other than behavior—actual choices—as indications of preference. As we note in the chapters that follow, however, this skepticism about soft data has not prevented rational choice theorists from voicing speculations about psychological processes based on no data.

8. Indeed, Hoffman and Spitzer (1985, 260) report that all of their experimental subjects do precisely that when the roles of players 1 and 2 are assigned by coin flip. Under these conditions, the subject in the role of player 1 always "agreed to take \$5 less than the \$12 that he could have obtained *without* the other subject's cooperation." See also Hoffman and Spitzer 1982.

for which there are insufficient direct or indirect measures. When faced with discordant results, it may be difficult, therefore, to distinguish empirically among three different claims about the principal unobservable term, equilibrium:

- The preferences assumed by the model are accurately represented in the setting one observes, but some or all of the actors lack the strategic acumen to play the game as rational choice recommends, and hence predicts.
- The model accurately captures the actors' objectives, but, perhaps owing to the particular characteristics of the equilibrium itself, there is a temporary departure from this predicted outcome.<sup>9</sup>
- The model does not capture one or more features of the observed game, and the outcomes conform to the equilibria (or lack thereof) associated with some other game.

The propagation of theoretical terms that are either unmeasurable or difficult to measure creates a situation akin to underidentification in statistical models involving latent variables (Bollen 1989). Under these circumstances, data cannot furnish a convincing test. When any hypothesis fails, the researcher is always in a position to argue that a successful prediction was thwarted by an offsetting tendency or temporary aberration. In this respect, empirical discussions in rational choice scholarship are reminiscent of debates about the declining rate of profit that once preoccupied Marxists. Having convinced themselves by analytic argument that the rate of profit in capitalism must fall over time but failing to find evidence to support this contention, Marxists for decades devoted their energies to identifying masking, fleeting, and countervailing tendencies that obscure this alleged phenomenon. Declining profitability was believed to be going on just beneath the surface on the strength of a theory that insisted that this must be so (compare Roemer 1979a; Van Parijs 1980).

The underidentification problem may be addressed in two ways. One is to set limits on the range of theoretical arguments that may be used in the construction or resuscitation of a theory. This kind of restriction, however, proves difficult to maintain against the impulse to defend the universal applicability of the rational choice approach. Often these restrictions are endorsed by such figures as Downs (1957) and Olson (1965), who introduce rational choice inquiry into a given domain of politics. But over time these constraints are relaxed by subsequent authors seeking to preserve a model in the face of discordant evidence. Another approach is to gather additional data so as to give the number of measures a sporting chance to catch up with the

9. Fiorina and Shepsle (1982) offer a lucid typology for various kinds of equilibria. Some, like "black holes," attract and retain outcomes in a social system. Others are retentive but not attractive, or vice versa. In the latter cases, it may be impossible to determine empirically whether a system is temporarily or permanently out of equilibrium.

number of theoretical terms. Rational choice scholars tend to shy away from this approach, perhaps a tacit admission that the formal precision of rational choice models greatly outstrips political scientists' capacity to measure.

*Vaguely Operationalized Predictions* A second common pathology related to hypothesis testing concerns the fit between the hypotheses advanced and the empirical tests used to evaluate them. Since equilibrium analysis is at the heart of much rational choice scholarship, many rational choice propositions are stated in the form of point predictions. Sometimes that point prediction is a rate or proportion, as in the case of Olson's conjecture that in the absence of selective incentives or coercion, members of large groups will not engage in collective action to advance their joint interests (1965). In other cases, the point prediction involves a particular outcome, as in the case of a specific majority rule equilibrium point in a cooperative bargaining game. Such propositions are invariably false to some degree; strategic blunders sometimes occur, producing nonequilibrium outcomes. The argument then shifts to the often expressed "hope that enough people act rationally enough of the time in their political behavior for economic theories of politics to yield descriptions, explanations, and predictions which are frequently useful approximations to the truth" (Kavka 1991, 372).<sup>10</sup>

It is unclear whether a rigorous test of a point prediction can be constructed in the form of an approximation. If several millions of dollars in small contributions are collected by referendum campaigns, is that evidence in support of the free-rider hypothesis (Lowenstein 1982, 572–73), given the paltry ratio of contributions to public concern over the outcome of these elections, or against it (Tillock and Morrison 1979), given the presumed irrationality of absorbing personal costs on behalf of a broadly diffused public good?

The match between theory and evidence becomes more ambiguous when rational choice hypotheses move seamlessly between point predictions and marginal predictions. The former concerns the location of an equilibrium under static conditions; the latter—derived from "comparative statics" analysis—concerns the direction in which an equilibrium is expected to move in response to exogenous changes in goals, beliefs, or environmental constraints. It is logically possible that only one sort of prediction will survive empirical testing, but the availability of two standards of evaluation affords defenders of a model more opportunity to claim support for its predictions. In particular, predictions at the margin are often hailed when static predictions fall into trouble. Whatever the defects of rational choice explanations of why citizens bother to go to the polls, Grofman (1993a) argues, rational choice theory does predict correctly that people are less inclined to vote in bad weather.

10. As we note in Chapter 5, when empirical failures occur, this "approximation" notion accompanies attribution of anomalies to the behavior of an irrational few.

We have no objection to the use of comparative statics to generate hypotheses. To the contrary, we find tests that focus on change at the margin much more amenable to traditional quasi-experimental methodology than those involving point predictions. Our concern is with the notion that the rationality of certain actions can be rescued on the grounds that the actors are to some degree responsive to changes in costs or benefits. Take, for example, the study of why politically inexperienced candidates challenge incumbent members of the House of Representatives. The behavior of these challengers is something of a mystery, since their chances of defeating an incumbent are nothing short of dismal. Like most puzzles of this sort, the behavior of challengers may be explained by reference to such ancillary factors as self-delusion, eagerness to promote legal practices while on the campaign trail, belief that *somebody* should contest the incumbent, and so forth. Banks and Kiewiet (1989, 1007) try to salvage the notion that rational, election-seeking behavior accounts for the decisions of weak challengers by arguing that "weak challengers can maximize their probability of getting elected to Congress by running now against the incumbent" rather than waiting for an open-seat contest in which they may have to defeat other strong opponents in both the primary and general elections. As the authors note dryly, "This probability may not be very high, but they are maximizing it." The study of whether weak challengers are *more* attracted to races against incumbents or to open-seat contests may be a worthy endeavor in its own right, but it is unclear how the results speak to the question of whether weak challengers are rational to oppose House incumbents, so long as rationality requires that the benefits of doing so outweigh the costs (1000).

### SELECTING AND INTERPRETING EVIDENCE

Another set of characteristic pathologies concerns the manner in which rational choice hypotheses are tested. The first has to do with the biased fashion in which evidence is selected. The second deals with subtler ways in which evidence is projected from theory rather than gathered independently from it. The last involves the strategic retreat from domains in which the theory is found to perform poorly. All three methodological defects undermine the theoretical claims they are intended to support, as it is the structured search for disconfirming evidence that is essential to scientific testing.

*Searching for Confirming Evidence* When reading applied rational choice scholarship, one is struck by the extent to which advocates of rational choice models permit their theoretical commitments to contaminate the sampling of evidence. The procedure of adducing instances that confirm a hypothesis is perhaps most transparent in such domains as regulation and bureaucratic politics, where the ideological stakes are

high. This practice, reminiscent of advertisements that show one brand's achievements while mentioning neither its failings nor the comparable achievements of its competitors, is not limited to these ideologically charged domains, however. In its more qualitative manifestations, rational choice scholarship tends to ruminate over confirming illustrations combed from the political landscape, memorable moments in history, and biblical texts (Brams 1980, 1993; Riker 1982, 1986). Elsewhere, this pathology leads researchers to dwell on instances of successful prediction, be they the phenomena of strategic counteramendments by committee leaders on the House floor (Weingast 1989, 810) or the suboptimal provision of collective goods (Olson 1965). The tendency to adduce confirming instances also manifests itself, though in subtler form, in quantitative research that goes through the motions of contrasting treatment and control conditions en route to a conclusion that follows trivially from the research design. McCubbins (1991, 107), for example, finds that time-series analyses of federal data for the period 1929 to 1988 "strongly support" his game theoretic account of how divided party control of Congress leads to budget deficits. Granted, his statistical estimates suggest that "since 1929, divided government has yielded sizable increases in the national debt" (102), but the period studied contains just two such episodes: the advent of supply-side economics under Ronald Reagan, and the drought of federal revenues during the waning days of the Hoover administration.

A variant of this methodological problem surfaces in studies that use laboratory behavior to support rational choice propositions but fail to build a control group into the experimental design. As we argue in detail in Chapter 6, successful experiments of this sort at most suggest that a laboratory setting can be constructed to approximate the conditions presupposed by a theorem; a researcher seeking to defend a rational choice hypothesis need only engineer a confirming illustration. Generated without a control group, the results give no indication of whether the observed outcome would have obtained anyway for reasons unrelated to the theory in question, nor does the experiment tell us whether this theory predicts successfully under other circumstances. Experiments crafted in this way illustrate rather than test.

*Projecting Evidence from Theory* A profound desire to establish rational choice theory's breadth of application from time to time opens the door to a tendentious reading of the empirical record. Sometimes this is a simple matter of authors imagining a datum consistent with economic logic (for example, bad weather depresses voter turnout) and assuming this datum to be empirically verified. At other times, one finds rational choice theorists asserting almost by way of afterthought that some eccentric feature of a model mirrors reality. For example, McKelvey and Riezman's legislative model (1992) hinges on the assumption that those with seniority are more likely to be recognized on the floor in the initial round of voting but not in subsequent rounds. The authors insist that this characterization provides a "realistic description

of the seniority system for the U.S. Congress" because seniority-influenced committees get first crack at making proposals, and "once the bills go to the floor, the committees lose most of their power" (958). Suffice it to say that this is a rather sparse depiction of the process by which legislation is proposed and amended in Congress (Weingast 1989).

Even when a full-blown empirical study is undertaken, the theoretical convictions of the authors may guide what they infer from a set of observations and how they reconstruct the data for presentation. For example, an obscure set of House votes on the Powell Amendment to a 1956 measure authorizing school construction has been offered up time and again as evidence of how legislators vote to strengthen a proposal they dislike in an effort to make the amended bill unpalatable (Riker 1965, 1982, 1986; Denzau, Riker, and Shepsle 1985). A dispassionate examination of the historical record, however, shows that the facts surrounding the Powell Amendment are at best ambiguous with respect to the phenomenon of strategic voting (Krehbiel and Rivers 1990). Indeed, the omissions and factual distortions that Krehbiel and Rivers detect in previous accounts (556–60, 574) suggest that earlier writers were unable to assimilate data that did not conform to their theoretical expectations.

*Arbitrary Domain Restriction* On occasion, rational choice theorists will concede that there are domains—such as voter turnout and organized collective action—in which no plausible variant of the theory appears to work. Some theorists are then inclined to withdraw, choosing to concentrate on applications in which these theories appear to have better success. For instance, in trying to make the case that his wealth-maximization hypothesis explains the evolution of the criminal law, Posner (1985) is forced to come to accept that he cannot explain the existence of laws against such "victimless crimes" as prostitution and drug abuse. He therefore abandons this domain, insisting nonetheless that wealth-maximization provides a powerful explanation of the rest of the criminal law.

Such a move might at first sight seem reasonable, even modest, but there is more at stake here than meets the eye. Suppose it transpired one day that red apples did not fall to the ground as other heavy bodies do. One would not be much impressed by the physicist who said of the theory of gravity that, though it seems not to work for red apples, it does a good job of explaining why other things fall to the ground and that consequently from now on he was going to restrict his attention to those other things when using the theory.

What we are calling arbitrary restriction to domains where a theory seems to work is not to be confused with two nonarbitrary forms of domain restriction that scientists engage in routinely. First, as Moe points out (1979, 235), testing of all scientific theories involves the insertion of *ceteris paribus* clauses designed to exclude omitted factors, so that a proper test of the hypothesis that objects of unequal mass fall to earth

at the same rate presupposes wind resistance to be held constant.<sup>11</sup> Second, theories may properly include an account of what are conventionally termed "interaction effects," factors that limit or enhance the influence of the independent variables of theoretical interest. Indeed, the value of a theory in the eyes of those who wish to understand and influence politics may hinge on a clear account of the conditions under which it is held to apply. Arbitrary domain restriction occurs when an empirically testable set of limiting conditions is lacking but retreat is sounded anyway. There is, in other words, a critical difference between specifying the relevant domain in advance by reference to limiting conditions and specifying as the relevant domain: "wherever the theory seems to work."<sup>12</sup>

The problem of arbitrary domain restriction is thus the obverse of the tendency to adduce confirming instances. The latter involves fishing for supportive evidence; the former, draining lakes that contain problematic data. While the practice of adducing confirming instances produces misleading tests, arbitrary domain restriction renders problematic the enterprise of testing. If the appropriate domain within which a theory is to be tested is defined by reference to whether the theory works in that domain, testing becomes pointless.

Posner, in our example, pushes the case for wealth-maximization as far as he can and cuts and runs when he has to. Yet he neither considers any alternative explanation nor sees the need to offer an account of why the theory breaks down in the domain of victimless crimes. For domain restriction to be adequate, the relevant domain must be

11. It is important to note that *ceteris paribus* provisos must refer to confounding factors, such as wind resistance, whose effects are in principle testable. One cannot take the position that only when all the logical assumptions of a theorem are satisfied empirically do the theorem's empirical predictions follow.

12. In much the same vein, arguments about when and where to apply a theory must be advanced in a consistent fashion. For example, in an effort to bolster their claim that House "leaders will be chosen in such a fashion that their personal reelection is not too incompatible with the duties of office," Cox and McCubbins (1993, 130) point out that one rational choice argument, based on the idea of the "uncovered set" (see Chapters 6 and 7), predicts "definite limits to the policy platforms that those seeking leadership positions will adopt" and, in particular, rules out successful bids by noncentrist candidates (130). Although Cox and McCubbins wish to embrace this prediction, they note that it is open to the objection that decisions enacted by majority rule are inherently unstable, that "there will always be some majority, all of [w]hose members could be made better off if its policies were changed" (131). Cox and McCubbins respond that this objection about the inherent vulnerability of House speakers rests on the assumption that actors incur no transaction costs when identifying or forming new majority coalitions. When these costs are taken into account, they contend, the instability problem no longer applies to the choice of speaker. They neglect to mention, however, that their preferred predictions based on the uncovered set also presuppose the absence of transaction costs.

specified independently of whether the theory explains the phenomenon within it. Furthermore, the hypothesis about the limiting conditions of rational choice explanations must itself stand up to empirical testing. As we noted in Chapter 2, rational choice theorists such as Brennan and Buchanan, Fiorina, and Satz and Ferejohn have suggested some hypotheses about the conditions under which rational choice explanations are likely to apply. It will become plain in subsequent chapters, however, that these recommendations have not yet had much impact on the design and application of rational choice models.

### CONCLUDING COMMENTS

Although widespread among empirical applications, the methodological problems identified in this chapter are not inextricable features of rational choice theorizing. Indeed, the larger message of this book is not that rational choice models of politics should simply be abandoned. Rather, the rational choice approach must be rethought fundamentally, and its relations with the existing stock of knowledge and theory in the social sciences should be reevaluated. It is therefore necessary to understand what the recurrent methodological problems are, why they turn up, and how they might be remedied. In this spirit, we turn to the literatures on turnout, collective action, legislative behavior, and electoral competition.

## CHAPTER FOUR

### THE PARADOX OF VOTER TURNOUT

At the foundation of democratic politics stands the act of voting, accompanied by a paradox. Starting with Anthony Downs (1957), rational choice theorists have characterized voter turnout as a collective action problem in which individuals are asked to sacrifice time and transportation costs on behalf of a public good, the election of a particular candidate or party. Although rational citizens may care a great deal about which person or group wins the election, an analysis of the instrumental value of voting suggests that they will nevertheless balk at the prospect of contributing to a collective cause since it is readily apparent that any one vote has an infinitesimal probability of altering the election outcome. Why take the time to vote when the election outcome will be unaffected by one's ballot? Unless rational citizens find the act of voting gratifying—because, say, they enjoy democratic participation or seek the status rewards of being seen at the polls—they will abstain and foist the costs of voting onto others.

In situations where voting is optional and altruism rare, the equilibrium posited for voter turnout in large electorates is one in which very few people, if any, bother to go to the polls. Many scholars, including several working within the rational choice tradition (Tullock 1967; Hardin 1982; Brennan and Buchanan 1984; Satz and Ferejohn 1993), therefore view voter turnout as a case in which rational choice theory fails empirically. For our purposes, the case of voter turnout is interesting not because it is a failure but because it illustrates the characteristic ways that rational choice theorists have reacted to discrepancies between theory and observation. In their resolute determination to declare some variant of rational choice theory victorious over the evidence (or, alternatively, to declare peace with honor through artful domain restriction), rational choice theorists have trotted out an astonishing variety of conjectures about the costs and benefits of voting, in the process generating an enormous literature, possibly larger in terms of academic citations and sheer bibliographic length