

THE EMPIRICAL CONTENT OF RATIONAL CHOICE THEORY

A REPLY TO GREEN AND SHAPIRO

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ABSTRACT

Green and Shapiro have argued that rational choice theory has produced virtually no new propositions about politics that have been carefully tested and not found wanting; and that an empirically successful rational choice theory would be no more universal than the middle-level theories that they advocate. In this essay I argue four main points. First, *Pathologies of Rational Choice Analysis* was much better designed to illustrate methodological failings than to sustain a global claim that rational choice theory has made no empirical contributions. Second, there is empirically confirmed content specific to rational choice theory, enough to make it the vital and exciting research program that it is. Third, there is a sense in which rational choice is more universal than its predecessors. Fourth, to provide a full evaluation of the scientific value of any theory one needs to consider both theoretical and empirical success.

KEY WORDS • philosophy of social science • rational choice theory • scientific method

Of the many recent criticisms of rational choice theory, none has attracted as much attention, debate and commentary as Donald Green and Ian Shapiro's *Pathologies of Rational Choice Theory* (1994). One indication of this interest was the publication of a special edition of *Critical Review* devoted to various comments on their book, together with their responses.¹ In this article I survey where I think we are in the debate.

Green and Shapiro (henceforth G&S) and their critics turn out to agree on a number of points. For example, both G&S and most of their critics seem to hold a partial universalist view of rational choice theory, to agree on a similar list of conditions under which rational choice theory might in principle be useful, and to agree that all research paradigms exhibit the various practices that G&S labeled 'pathologies' in their book. The main points of contention appear to be two:

I thank Daniel Diermeier, John Huber, Skip Lupia, Mathew McCubbins, Samuel Popkin and Michael Thies for their helpful comments.

1. *Critical Review* 9(1-2), Winter-Spring 1995. Page citations without a year all refer to this special edition (which later appeared in book form) (Friedman, 1996).

1. G&S believe that rational choice theory, properly defined, has produced virtually no new propositions about politics that have been carefully tested and not found wanting. Rational choice theorists disagree.
2. G&S argue that an empirically successful rational choice theory, were such a thing to emerge, would not be any more universal than the middle-level theories that they advocate. Rational choice theorists might disagree, depending on how one defines various terms.

In this essay I argue four main points. First, *Pathologies* was much better designed to illustrate methodological failings than to sustain a global claim that rational choice theory has made no empirical contributions. Second, there is empirically confirmed content specific to rational choice theory, enough to make it the vital and exciting research program that it is. Third, there is a sense in which rational choice is more universal than its predecessors. Fourth, to provide a full evaluation of the scientific value of any theory one needs to consider both theoretical and empirical success. Before turning to these points, I first provide a selective review of the debate as it has unfolded thus far.

The Previous Debate

Many of G&S's critics charge that they employ inappropriate standards in judging rational choice theory, standards that not even successful theories from the hard sciences could survive. Daniel Diermeier (1995), for example, reads G&S as: (1) stipulating a list of methodological pathologies; (2) arguing that rational choice scholars are more likely to exhibit these pathologies than are non-rational-choice scholars; and (3) further arguing that these pathologies are rooted in the universalist aspirations of rational choice theory. Given this reading, part of Diermeier's response is to point out that each of the pathologies that G&S identify is present even in the most successful research paradigms – the running example being Newtonian particle mechanics. We should not reject theories merely because they exhibit these pathologies, says Diermeier, otherwise we'll get rid of the good ones as well as the bad (and everything in between). Show me something better than rational choice theory and maybe I'll change. Shepsle (1995: 217) gives his own version of this Lakatosian advice in his First Law of Wingwalking: 'Don't let go of something until you have something else to hold on to'.²

G&S agree that their pathologies appear in successful research para-

2. Lakatos's (1970) well-known argument is that a theory should only be rejected when another theory comes along that explains everything the old one did and, in addition, makes some new testable predictions that in fact survive careful testing.

digms and that one would not want to throw out Newtonian particle mechanics because there was a bit of *post hoc* theorizing about a newly discovered planet here or an anomaly about Mercury's orbit there. But, they say, why would anyone suffer these pathologies on behalf of a theory that has virtually no identifiable empirical content? 'Shepsle's appeal to the First Law of Wingwalking might be easier to take seriously', they say, 'if the aircraft in question were in fact airborne.' Diermeier, they say, 'assumes just what is in dispute here: that rational choice theory is a successful theory.' 'What is there besides anomalies', they ask, 'that is not obvious or banal?' (Green and Shapiro, 1995: 250, 260-1).

The disagreement is thus not really about criteria of judgement in the philosophy of science, at least at this point. The crux of the matter is simply that Diermeier, Shepsle and others believe that rational choice theory has empirical content that is worth preserving, while G&S see very little.

The second part of Diermeier's rejoinder notes that G&S do not demonstrate by comparative analysis that rational choice theory is *more likely* to exhibit pathologies than are non-rational-choice theories. G&S agree that they do not provide such a comparative analysis. But if rational choice theory has (virtually) no empirical content, then the existence of any confirmed empirical content anywhere in traditional political science directly shows the empirical superiority of non-rational-choice theories – and indirectly indicates their superiority in avoiding pathologies. So, again, the issue comes back to the near-zero empirical content of rational choice theory.

The disagreement can be clarified with the help of a Venn diagram. In Figure 1 are three circles, each representing a set of theoretical propositions that have been empirically confirmed or corroborated. The bottom circle represents obvious and boring theoretical propositions that do not pass G&S's sexist Grandma Test (roughly, 'if even Grandma would have

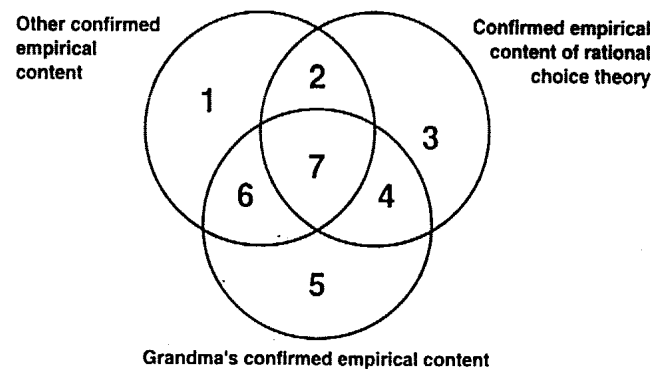


Figure 1. Confirmed Empirical Content from Three Sources

thought of it, it is not interesting'). The circle on the left represents theoretical propositions emerging from non-rational-choice theories. Finally, the circle on the right represents rational choice's theoretical propositions. The numbers in the diagram identify various kinds of proposition. For example, the region labeled with a '3' – which I shall call 'A3' for 'area 3' – represents theoretical propositions that rational choice theorists have made, that non-rational-choice theorists have not made, and that are not obvious. G&S's claim is simply that A3 is nearly empty (while A1 is not).

So why is A3 (nearly) empty in G&S's estimation, while rational choice theorists apparently think it is teeming with interesting propositions? One important reason is that G&S limit the search for empirically corroborated rational choice propositions to relatively more formal and deductive models. Many rational choice readers of *Pathologies* probably ran through a list of what they viewed as rational choice empirical accomplishments – from such scholars as Morris Fiorina, John Aldrich, Elinor Ostrom, David Laitin, David Rohde, Mathew McCubbins, and so on – concluding that G&S had simply missed the boat.

But G&S exclude much of what these and other empirically-oriented rational choice scholars have done, on the ground that their models are not sufficiently formal and deductive. For example, several critics having pointed to Fiorina's *Retrospective Voting in American National Elections* (1981) – a work not discussed in *Pathologies* – as an example of excellent empirical scholarship from a rational choice perspective, G&S respond by saying that Fiorina's model is not formal enough to be rational choice. They do hedge a bit on this issue, saying that it is 'unclear' whether it is rational choice. And they do also note (1995: 242) that 'his central empirical claim has not, in our view, survived subsequent challenges', which would remove this claim not just from A3 but from the whole diagram in Figure 1. But the central argument, the one they would presumably use to banish the rest of Fiorina's work from A3, is the first: his work is not sufficiently formal and deductive to fall into the category of rational choice theory as they define it.³ Elinor Ostrom's work (e.g., Ostrom, 1990), to which other critics had pointed, is similarly ruled out of bounds, as is the work of several others. Thus, G&S's response to Fiorina's complaint that 'if G&S are correct ... what I have done that is rational choice makes no empirical contribution, and any empirical contributions I have made are not rational choice' (Fiorina, 1995: 85) is essentially: 'Yes,

3. Of Fiorina's work, the only candidate for inclusion in A3 that G&S have specifically flagged (1995: 190) is his work on the professionalization of state legislatures (1994), which they apparently view as more formal than *Retrospective Voting*.

that's about right – and a similar rule applies to other less formal rational choice scholars'.

What is Rational Choice?

Many scholars regard as 'rational choice' any argument that takes actors and their goals as primitives, then proceeds to explain how the given actors' pursuit of their posited goals leads to systematic tendencies in observed behavior. Such arguments, no matter how formal, are contrasted with those that explain behavior as a consequence of socialization-instilled values, habit or other grounds that have little to do with the pursuit of goals.

G&S's formalism requirement explicitly excludes from the category of rational choice (hence from A3) work that, albeit 'rational choice' by the above definition, is relatively informal and inductive. In G&S's view, a proposition seeking admittance to A3 needs to pass both an operational verification hurdle (as high as that applied to A1) and a separate deductive validity/generalizability hurdle (*higher* than that applied to A1).

Why do G&S restrict admittance to A3 in this way? Recall that a major part of their project is to explode the universalist ambitions of rational choice. Their position seems to be as follows. Rational choice work that is truly universal has not produced any new empirical content. What some people call 'rational choice scholarship' that has produced interesting empirical findings is no more universal than the middle-level theories that G&S advocate. Therefore, *formal* rational choice theorists must either admit that their work has no empirical content as of yet or abandon their universalist pretensions. I have several reactions to this line of argument and to the formalism requirement that goes along with it.

First, although I have never thought that rational choice theory (formal or otherwise) provides a universal theory of all human behavior, I do not see how it follows that empirically successful rational choice work is therefore no more universal than middle-level theory. The universality or generality of a proposition can vary continuously but G&S argue as if there are no stops on the universalism scale between 'true universalism' and 'middle-level theory'.⁴ Perhaps rational choice propositions that fall just below G&S's formalism cut-off are both empirically sustained and more universal – or less universal – than propositions in traditional political science.

4. 'Middle-level theory' is not precisely defined but entails a substantially greater willingness to contextualize one's arguments, bringing in habit, socialized values and other non-instrumental considerations more readily than an overarching universal rational choice theory like 'wealth maximization, always' would permit.

Second, imposing a formalism requirement for admittance to A3 means that any empirical contributions made by works that qualify as rational choice only by the broad definition appear in A1 (the set of non-obvious empirically sustained propositions from non-rational choice theories), not A3. For example, Fenno's *Congressmen in Committees* (1973) contributes only to A1. Although Fenno's work is not formal and has a large inductive component, it clearly and self-consciously departed from a prior theoretical tradition in congressional studies that drew inspiration from sociology, and it clearly and consistently worried about the goals that members of Congress pursued and how this affected the institution they inhabited. So it is odd to see Fenno's work count as a one hundred percent non-rational-choice success in Figure 1.

If we adopt uniform criteria for admitting propositions to Figure 1, allocating them to A3 if the theories from which they derive focus primarily on goal-seeking behavior, then propositions from Fiorina's *Retrospective Voting* must tip back over into A3 from A1, where they currently reside. So too would propositions from Fenno's *Congressmen in Committees*, Popkin's *The Rational Peasant* (1979), and other examples of empirical research from a rational choice perspective, tip over into A3. Whether these propositions are superior or inferior to some other propositions on a combined accounting of theoretical rigor and empirical success is not something that G&S address.

Third, G&S's real target is formal rational choice theory, not rational choice theory broadly defined. They divorce the more contextualizing brand of empirical rational choice theory from the more purely theoretical stuff, evaluating the latter on its own as if it has no meaningful influence on the former. I do not think that this divorce always makes sense, but leave that aside for the moment.

Even if one accepts G&S's definition of what rational choice theory is, they do not give very good evidence for believing their central proposition: that formal rational choice theory has not produced interesting and empirically sustained propositions about politics. In the next two sections I defend this opinion, first by criticizing G&S's selection of rational choice literatures to survey, then by arguing that, even in the literatures they selected, there are interesting and empirically confirmed rational choice propositions that satisfy their minimum requirements.

A Methodological Critique

If the main goal of *Pathologies* was to demonstrate the poverty of rational choice theory's empirical accomplishments, then it should have focused on models with reputations for strong *empirical* performance. By my reading,

however, *Pathologies* focused (in its main review chapters, 4–7) on one model with such a reputation (the Olsonian model of collective action) (Olson, 1965), two models notoriously without such reputations (the pivotal voter model of turnout, the majority-rule cycling model), and two models that have remained largely theoretical (the spatial model of candidate competition, the structure-induced equilibrium model). Had I been asked to defend the empirical accomplishments of rational choice theory, I would never have dreamed of basing a defense on the pivotal voter or majority-rule cycling models, for the obvious reason that both are associated with well-known empirical paradoxes. Nor would I have chosen the spatial model of candidate competition or the early structure-induced equilibrium models as advertisements for the empirical success of rational choice theory, valuable though I think they are theoretically.⁵

What would I have chosen? Instead of the pivotal voter model of turnout, which everyone already agreed was not empirically successful before G&S wrote their book, I would have chosen other rational choice approaches to studying turn-out, such as those focusing on the mobilizational incentives of elites⁶ and on the institutionally conditioned costs and benefits of voting.⁷ Instead of the majority-rule instability models, which everyone already agreed, before G&S wrote their book, were not empirically successful, I would have chosen the various rational-actor models of coalition government formation (see Laver and Schofield, 1990, and Laver and Shepsle, 1994, for extensive reviews of this literature). Instead of spatial models of candidate competition, which I think most agreed, before G&S wrote their book, were difficult to operationalize and test (cf. Ordeshook, 1976), I would have chosen Romer and Rosenthal's well-known spatial model of agenda setting, which has generated new predictions and tested them (Romer and Rosenthal, 1978, 1979; and see below). Finally, instead of structure-induced equilibrium models and theoretical

5. For a recent consideration of the theoretical value of the majority-rule cycling and structure-induced equilibrium models, in the context of a careful empirical investigation of French legislative politics, see Huber (1996).

6. G&S deal briefly with Morton's (1991) and Uhlaner's (1989) theoretical work on the mobilizational incentives of elites, deriding it as just another empirically unsupported attempt to elude the paradox of voting. They do not deal with empirical work that takes the same theoretical approach, such as Berch (1993), Cox and Munger (1989), or Matsusaka and Palda (1993); with empirical attacks on the pivotal voter model from a strategic elites perspective, such as Matsusaka (1993); or with empirical work that flows from a theory wedding rational choice and other approaches, such as Rosenstone and Hansen (1993).

7. For work that deals with the institutionally conditioned costs and benefits of voting see e.g. Powell (1986); Jackman (1987); Blais and Carty (1990); and Jackman and Miller (1995). Lijphart (1997) provides a recent review.

studies of the core I would have chosen more recent and applied legislative models (e.g., Krehbiel, 1991; Kiewiet and McCubbins, 1991; Cox and McCubbins, 1993; Huber, 1996; Tsebelis and Money, 1997), several of which G&S discuss briefly in their concluding chapter.

G&S clearly had to select only a fraction of rational choice works to review in their book because, as they point out (1994: 7), 'anyone who sets out to conduct a fully comprehensive evaluation of the literature might never finish'. Had G&S targeted five models or published works that could arguably be represented as containing the most successful empirical work from a rational choice perspective, and found no new empirical content, then they could more reasonably have argued that a comprehensive search would have turned up little. But how does a chapter devoted to attempts to wriggle out of the paradox of turn-out show the unsoundness of empirical work in rational choice analyses of, for example, interest-group contributions to politicians (e.g., Grier and Munger, 1991; Grier, Munger and Roberts, 1994), language politics (e.g., Laitin, 1989, 1991, 1994), trade politics (e.g., Rogowski, 1989), or progressive ambition (e.g., Rohde, 1979)? How do several chapter sections devoted to the majority-rule cycling model and theoretical instability results show the unsoundness of empirical work in rational choice analyses of, for example, political choice of electoral laws (Bawn, 1993), shirking in public bureaucracies (Weingast and Moran, 1983; Brehm and Gates, 1993; Olson, 1996), partisan cycles in congressional elections and the macroeconomy (Alesina and Rosenthal, 1989), or French coalition politics (Tsebelis, 1990)? If one believed that rational choice theory was inherently prone to exhibit a certain rate of pathology, then I suppose that one could examine any rational choice literature, verify the rate of pathology there, and generalize to the rest. But G&S explicitly deny believing that rational choice theory is inherently pathological. So why should one believe the general claim that A3 is empty?

A3 is not Empty

Assuming that G&S had surveyed the four 'substitute' models suggested in the previous section, would they have found anything of empirical interest? In this section, I sketch empirical work testing one of these models, leaving aside rational actor models of coalition formation (already amply surveyed), recent models of Congress (already surveyed by G&S and by Shepsle and Weingast, 1995), and rational actor models of elite mobilization and turnout (for a partial review of which see Cox et al., 1998). I will not confine my survey entirely to works that appeared before *Pathologies* did in 1994 but, as will be seen, there would have been something to survey even without the more recent work.

The Setter Model and Empirical Research

The Romer-Rosenthal setter model began life as an attempt to explain a particular empirical problem involving Oregon school bond referenda (1978, 1979). Although this model is extremely prominent in the area of spatial modelling, and was developed as a direct response to the use of Downs' median voter theorem in empirical studies of local public finance, G&S do not review it. Is the setter model any more successful empirically than the area of spatial theory upon which G&S choose to focus?

Yes. The basic model is a very simple one that features an agenda setter, A, who can either make a take-it-or-leave-it policy proposal to a chooser, C, or refrain from making any proposal. If C rejects the proposal, or no proposal is made to begin with, then a well-defined reversionary policy results. Otherwise, the policy proposed by A and accepted by C comes into force. Typically a unidimensional spatial model is used to characterize the available policy options, but multidimensional models have been used as well.

The original application of the model to school bond referenda in Oregon put school administrators in the role of the agenda setter and voters in the role of choosers, with school budgets being the policy at issue. The reversionary budget was well defined in state law and varied substantially from school district to school district, allowing Romer and Rosenthal to test key predictions of the model concerning when school administrators would seek to get a budget via the referendum process, rather than stick with the reversionary budget, and how much they would ask for. One non-obvious prediction that has been subject to several empirical tests runs as follows. Suppose that administrators in a set of school districts are budget maximizers (as might be plausible in the larger and more bureaucratized districts). If administrators in such districts decide to ask for a new budget, they will naturally want to get as large a budget as the voters will accept. Given some uncertainty about what voters will grant, administrators should ask for a budget that is likely to command something a bit more than a bare majority. In calculating what budget to put on the referendum, the setter will

... take account of voters' indirect preferences for education. In turn, these preferences reflect income, tax price, children per household, and other variables used in [previous] cross-sectional studies by social scientists. Because the setter 'takes out' the effects of these variables in 'aiming', we should find that the level of 'Yes' voting in a cross-section of districts holding elections should appear as random variation about a mean level, statistically unrelated to income, price, or other exogenous variables. (Rosenthal, 1990: 228)

Romer and Rosenthal (1982) have confirmed the 'take-out' hypothesis for Oregon school districts, while Rothstein (1988) and Romer et al. (1992) have confirmed it for the Michigan and New York cases. (See Rosenthal,

1990, for a review of other empirical findings relevant to local public finance based on the setter model.)

Kiewiet and McCubbins (1988, 1991) use a setter model to analyze a substantially different issue: presidential influence on congressional appropriations decisions.⁸ The key prediction from their model is that presidents will be able to exert substantially more influence on congressional decisions when they prefer to appropriate less to an agency than does Congress. They test this 'asymmetric influence hypothesis' using a switching regime regression model (estimated with appropriations data for 43 federal agencies from 1948 to 1979), finding substantial support for it. Their setter model also predicts something similar to Romer and Rosenthal's 'take-out' hypothesis: that variables tapping congressional preferences will be poor predictors of final appropriations levels when the president is strategically advantaged, but good predictors otherwise. This hypothesis is also supported in their analysis of the data.

John Huber (1992) examines the use of two take-it-or-leave-it procedural devices available to the government in France – the package vote and the guillotine (or confidence) procedure – based on previous formal analysis of the use of somewhat different restrictive procedures in the USA. His logit analyses of the government's decision to use the package vote and the guillotine on a sample of 356 government bills considered between 1978 and 1989 shows substantial support for several of the key predictions of the US models – e.g., that restrictive procedures are used to protect logrolls within the majority party/coalition and are thus more likely to be used when majority interests are more diverse. His work illuminates the use of restrictive procedures in general and their role in the French system in particular (both topics elaborated upon in his excellent 1996 book).

Pablo Spiller and Rafael Gely (1992) use a modified setter model to examine the interplay between executive agencies, the Supreme Court and Congress. Their model yields empirically refutable predictions about the conditions under which congressional preferences will influence the Supreme Court, which they test via an econometric model of the Court's decisions on industrial labor relations. Statistically, their model outperforms purely legalistic models of Court decision-making. Substantively, it illuminates the important issue of political influence on judicial decisions in a fresh way.

More recently, Charles Cameron (n.d.) has based his pathbreaking book on presidential vetoes on the setter model. Cameron compiles a compre-

8. Here, of course, the strategic situation is more complicated than in the case of school bond referenda, because presidents can veto appropriations bills and Congress can override those vetoes. In addition, the rules governing reversionary spending levels are more complex. Nonetheless, the basic structure of the model is similar.

hensive data set on vetoes, veto overrides, attempts to pass a vetoed bill again, veto threats, and concessions that occur during the bargaining process between president and Congress. His theory predicts or helps explain over a dozen specific features observable in the data, such as that: (1) the probability of a veto increases with legislative significance during divided government, but not under unified government; (2) the probability of an override attempt increases in later rounds of 'veto chains' (i.e., sequences similar to 'bill passed, bill vetoed, bill passed again with changes, bill vetoed again, etc.');

(3) the probability of a second veto is higher after a failed override attempt; and (4) vetoes without prior veto threats are rare. One prediction in particular that emerges and is tested is the analog in the veto override case of the Romer–Rosenthal 'take-out' hypothesis. Sure enough, once a decision to attempt an override has been made, none of the covariates predicts the success of that attempt.

Other recent work using setter models to launch empirical work include Liz Gerber's (1996) investigation of how the possibility of legislation by initiative affects the incentives of elected legislators (using parental consent requirements for teenage abortions as a test case), Dion and Huber's (1996) study of the conditions under which the House Rules Committee grants restrictive rules, Cohen and Spitzer's (1996) examination of judicial deference to agency action (using data on the Supreme Court's grants of *certiorari* and subsequent upholding or reversal of lower-court decisions), Lupia and McCubbins' (1998) study of democratic delegation (using a variety of experimental evidence), and Cox and Katz's (n.d.) study of the reapportionment revolution (using data on congressional elections).

In my judgment, the empirical work in the books and articles cited above is clearly superior to the bulk of the empirical work that G&S choose to exhibit as characteristic of rational choice scholarship and does not suffer by comparison with empirical work elsewhere in political science. Moreover, I view these works as more characteristic of recent empirical work in rational choice circles than the range of studies upon which G&S focus in Chapters 4–7.

Summary

Pathologies is well designed to illustrate methodological failings. For that purpose, it makes sense to review research into the pivotal voter and majority-rule cycling models, with their associated paradoxes. But *Pathologies* is not well designed to demonstrate what now appears to be G&S's central assertion – that rational choice theory above a minimum universality lacks corroborated empirical content. To support that claim, the natural research strategy would have been to avoid models that produced well-known empirical paradoxes or were primarily theoretical, in order to

concentrate fire on models with stronger empirical reputations. I have suggested substitutes for four of the five models that G&S reviewed and briefly surveyed one of them here.

Is Rational Choice Theory More Universal than Others?

I turn in this section to the question of rational choice theory's universality. In particular, I shall focus on the universality of formalized rational choice theory – G&S's target.

While rational choice theory more broadly defined might be a paradigm, what is now the hegemonic approach to formalizing rational choice theory – viz., game theory – is a methodology. I am not sure exactly how methodologies differ from paradigms. But I know that they are not substantive theories of anything in particular. Game theory has only a little more substantive content than does statistical theory – or what I shall call econometric theory in what follows.

Econometrics is a method of making valid causal inferences, not a substantive theory of some particular (class of) causal relations. Game theory is a method of analyzing strategic interactions, not a substantive theory of some particular (class of) interactions. What allows econometrics to work on prices, plant heights, cardio-pulmonary pressures and *coups d'état* is the flexible and abstract concept of a (random) variable. What allows game theory to work on lizards' sex lives, social norms, market structure, and candidate strategy are the flexible and abstract concepts of utility and belief. Econometric theory has developed by producing a huge cookbook full of abstract models that empirical researchers may apply to particular problems; each model specifies a set of assumptions under which valid causal inferences can be made. Game theory has developed by producing a huge cookbook full of abstract models that empirical researchers may apply to particular problems; each model specifies a set of assumptions under which internally consistent predictions about the interplay of different actors' preferences, beliefs and actions can be made. The most popular econometric models, such as the Gauss–Markov model underlying ordinary least squares regression, are widely applied across the social sciences – and beyond. The most popular game-theoretic models, such as dilemma and coordination games, are widely applied across the social sciences – and beyond.

Those who use econometric theory to guide their empirical work are not engaged in a fundamentally different enterprise than those who analyze data without the benefit of econometric theory. Both parties are interested in making valid causal inferences. Perfectly sound research designs have been constructed by scholars operating without all the rules and regula-

tions that econometricians are so fond of. Nonetheless, all the rules and regulations evolved precisely to deal with threats to the validity of causal inference, so that nose-to-the-grindstone application of these rules can supply the defect of insight. Some theorists believe that the rules of causal inference laid out in econometric theory apply, *mutatis mutandis*, even to those who work with qualitative data (cf. King et al., 1994).

Those who use game theory to guide their empirical work are not engaged in a fundamentally different enterprise than those who analyze strategic interaction without the benefit of game theory. Both parties are interested in constructing internally consistent theories of how unobservable preferences and beliefs relate to observable actions. Perfectly sound theories have been constructed by scholars operating without all the rules and regulations that game theorists are so fond of. Nonetheless, all the rules and regulations, especially those involving the core concept of equilibrium, evolved precisely to deal with threats to the internal consistency of theories of reasoned action and interaction, so that nose-to-the-grindstone application of these rules can supply the defect of insight. Some rational choice theorists believe that the rules of equilibrium analysis laid out in game theory apply, *mutatis mutandis*, even to social situations in which there is no rational thought of any kind involved (e.g., evolutionary biology), or there is a substantial component of non-instrumental utility (e.g., social norms; cf. Ullmann-Margalit, 1977; Sugden, 1986; Coleman, 1990; Mackie, 1996), or there are mistakes, misinformation, and learning rather than errorless perfect-information deduction.

Game theory does not provide a universal substantive theory of human behavior. It does, however, provide a universal method of analyzing strategic interaction, a methodology that has been fruitfully applied from evolutionary biology to operations research to economics to sociology to political science and beyond. Is there a more universally applicable or otherwise superior method of analyzing strategic interaction on offer somewhere in middle-level theory? If not, then why would eclectic theorists – those willing to borrow from all relevant theoretical domains in order to explain the particular phenomena that interest them – not use game theory?

Scientific Value in the Social Sciences

Most game theorists probably believe that game theory subsumes and improves many previous modes of strategic analysis: that it clarifies the murky, relates the unrelated, and exposes the counterfactual before it even gets to the data. I value game theory – the *formal* analysis of strategic interaction – for two reasons. First, as a rational choice theorist (broadly

defined), I already believed that there was ample evidence of the importance of strategic interaction in politics. Second, given this substantive position, I accepted game theory as a purely conceptual or methodological advance, even before I noticed that it had the sort of Lakatosian pay-off that G&S demand (and mistakenly insist is absent).

Lakatos (1970) dealt mostly with theories from the hard sciences that took a hypothetico-deductive form. As theoretical virtues were held roughly constant, he defined scientific progress *purely* in terms of empirical progress. One might also imagine scientific progress in which the empirical evidence is constant but theories become more general and rigorous. Presumably any such notion of scientific advance would open a hornets' nest of disagreement if applied across paradigmatic boundaries. But theoretical advances are clearly identified and valued within paradigms in the social sciences, where many of our models are not formally developed. In this section, I consider the interplay of theory and evidence in producing 'scientific value' in the social sciences.

Theories are conventionally valued not just for their ability to generate predictions that survive empirical testing but also for their parsimony, rigor, and generality. Should one be willing to trade off some empirical success for more theoretical success (e.g., conceptual clarity or generality)? One might be willing to entertain such a trade-off because both empirical and theoretical success are continuously variable. The evidence that a particular prediction is borne out in the data varies widely in its power to convince. Similarly, some theoretical arguments are internally inconsistent, some compel but are not apodictic, some are highly formal but narrowly based, some are more general, some less, and so forth.

There is a whole series of potential disagreements, not just between G&S and rational choice theorists, but between individual scholars of whatever stripe, that might arise at this point. One way to organize these disputes would be via a simple formula to evaluate the scientific value (denoted V) of a theory: $V = eE + tT + ET$. Here E is the 'empirical success' of the theory: have its predictions been tested and not found wanting? how many and demanding were the tests? and so on. T is the 'theoretical success' of the theory: how many testable predictions does it generate? are they derived rigorously from general premises? and so on. The coefficients t and e reflect one's valuation of pure theory and pure empiricism, respectively. Finally, ET is simply E multiplied by T .

If I understand G&S's view properly, $t = 0$. They state that 'a theory of politics has no payoff if its hypotheses do not survive empirical scrutiny' (1994: 32). In terms of the formula offered above, this statement defines 'empirical success' solely as 'generating hypotheses that survive empirical scrutiny' and says that if empirical success so defined is lacking (i.e., $E = 0$), then the theory has no scientific value (i.e., $V = 0$). But if lack of empirical

success ($E = 0$) implies no scientific pay-off ($V = 0$) regardless of theoretical success (T), then t must be zero in the formula for V given above.

Whether t ought to be zero is debatable and depends on further details of how one defines 'empirical success.' For example, in G&S's definition ('generating hypotheses that survive empirical scrutiny') what does 'generating hypotheses' mean? If a pure theory does not directly produce testable hypotheses but does influence empirical researchers who do generate testable hypotheses, does this redound to the credit of the pure theory? How tight must the relationship be between the pure theory and the empirical research it influences? G&S think the the relationship must be rather tight: it is on that ground that they give Olson's theory of collective action no credit whatever for Ostrom's empirical work. If one thinks that Olson should get some sort of credit for the empirical content of Ostrom's work, there are two ways to give it to him: (1) define 'empirical success' more broadly than G&S do; (2) stick with G&S's definition but decide that $t > 0$: pure theory has value in setting empirical research agendas, for example. Either way, one would be disagreeing with the assertion that 'a theory of politics has no pay-off if its hypotheses do not survive empirical scrutiny.'

Another question about G&S's definition of empirical success ('generating hypotheses that survive empirical scrutiny') is 'hypotheses about what?' G&S consistently define empirical success in terms of an ability to explain real-world political phenomena. Thus, for example, they discount rational choice experimental work as providing much empirical verification, partly on the grounds that such experiments have limited external validity: 'the fact that experimental gatherings of potential contributors are brought together by an outside party, available options are highly regimented, the collective cause lacks ideological content, and face-to-face negotiations tend to take place in demographically homogeneous groups renders uncertain the external validity of laboratory studies' (1994: 93; see also 1994: 124, 139). If one defines empirical success solely as success in explaining the real world, however, and adopts the standard for judging external validity suggested by the above passage, and also insists that $t = 0$, then many propositions from the hard sciences would not qualify as scientific. For example, the proposition P that 'a feather dropped in a vacuum will accelerate at 981 ms^{-2} ', would have no scientific value, even if experimentally confirmed, because the experiment has no surface external validity: feathers never meet vacuums in the real world and the fact that they fall just like ball bearings in some contrived laboratory setting is a very bad guide to predicting their behavior out there in the real world. The theory of gravitational force that predicted P might be supported by dropping heavy aerodynamic objects from some tower but the observation

P would not contribute to the theory's scientific success. If one thinks the observation P does have scientific value, then again one either has to define empirical success more broadly than G&S do or (more peculiarly) include the ability to predict experimental results as one value of pure theory (so that $t > 0$).⁹

Let us accept G&S's stipulation that $t = 0$ and their definition of empirical success. Even with these restrictions, theoretical success (T) contributes to the scientific value of a theory in a way that G&S largely ignore. They take for granted that the contents of A2 – confirmed predictions that are made both by formal rational choice models and by informal rational choice models or non-rational-choice models – redound almost entirely to the credit of the older informal rational choice or non-rational-choice theories. The formal rational choice theories are mere redescriptions that never really add anything in the way of insight, rigor, generality or parsimony; so the *only* sense in which they might prove themselves empirically would be via the production of novel predictions that are confirmed. Sometimes this may be fair. But sometimes rational choice theories (whether formal or informal) may displace non-rational-choice theories because they provide more satisfying explanations of things that both theories predict. The reverse can happen too. In judging the scientific value of a theory, the production of new empirical content is not the only criterion; old empirical content is often in play too.

McCubbins and Schwartz's (1984) model, for example, provided a quite different interpretation of the widely acknowledged rarity of explicit legislative oversight of executive agencies than had previously reigned in the literature. The facts were not in dispute; their interpretation was. Their work was valuable even before empirical investigations arose that bore on their ideas (e.g., Aberbach, 1990; Aberbach and Ogul, 1990; for a survey, see McCubbins et al., 1997). To take another example, Schelling's (1978) reinterpretation of white flight as a coordination game was valuable, even before it inspired a careful empirical examination that took this tack (Gamm, n.d.).

G&S also take for granted that the contents of A4 and A7 – corroborated empirical predictions that are made by formal rational choice models but that G&S deem to be obvious or banal – do not redound to the credit of

rational choice theory. I would make two points here. First, what is obvious sometimes changes from generation to generation, in response to the latest theories: think, for example, of western ideas about disease. Second, it is highly problematic to claim that what is obvious to some stereotypic aged person is obviously well-understood theoretically, so that there is no room for theoretical, hence scientific, advance.¹⁰

By putting all of A2, A4 and A7 in the category of 'stuff we already knew,' G&S are able to say that any conceptual advance from rational choice theory is a purely theoretical matter unconnected to empirical success. But this puts asunder what are inextricably linked aspects of the research process. Even if $t = 0$, so that $V = eE + ET$, every conceptual advance enhances the value of the empirical results to which it is attached ($T + e$ is the coefficient that converts empirical success into scientific value), just as every empirical advance enhances the value of the theory with which it is associated (E is the coefficient that converts theoretical success into scientific value).

Different scholars will put different values (t and e) on 'unempirical theory' and 'atheoretical empirical work,' and will differently define 'empirical success' (E) and 'theoretical success' (T). Regardless of one's personal values and definitions, if one is attacking a bunch of theorists, the natural points of attack are at E and t . If these can be driven to near-zero levels, the scientific value of the theorists' work falls to near-zero levels too, unless T is 'really big.' If the point of such an attack is to show that the theorists are straying too far from the positivist path, then the natural strategy is to define the relevant terms above in a fashion that is consistently favorable to theory, and show that even with generous values there is reason for concern. G&S, however, do not do this.

Rational choice theorists were already aware of the imbalance between theory and empirical testing in their field (see, for example, Palfrey, 1991; Enelow and Morton, 1993; or Mueller, 1993). I agree wholeheartedly with G&S that there is a need for more and better empirical testing of rational choice models – whether rational choice is broadly or narrowly defined. But agreeing with Palfrey, Enelow and Morton, G&S, and others that there is such a need is a far cry from agreeing with G&S's overall judgment of rational choice's contributions. There is a whole series of judgment calls that must be made in order to evaluate the scientific value of a research

9. The point I make in this paragraph is similar to that made by Campbell (1969: 361) concerning Nicholson and Carlisle's 'uninhibited generalization' from a few hydrolysis experiments with a 'very parochial and idiochronic sample of Soho water'. I am not prepared to go as far as Campbell did in dismissing concerns about external validity, however, because the hard sciences have a much longer and better track record of overcoming threats to external validity; my position is closer to that expressed in Klinder and Palfrey (1993: 26–31).

10. Among the conceptual advances that a formal approach can offer is to show that many obvious propositions, each previously 'obvious' on the basis of a separate theory, can be generated from a consistent and relatively small set of assumptions. Even this 'organizational' function of theory can have substantial scientific value – if one accepts the accounting of scientific value proposed here.

program, and minor and sometimes not-so-minor disagreements at a number of those points can cumulate into a substantially different bottom line.

Conclusion

Judging from the responses, several scholars interpreted the main argument of Green and Shapiro's *Pathologies of Rational Choice Theory* as being that rational choice theory exhibited certain pathologies more often than other theories and should be rejected or debunked on that basis. Some, including even non-rational-choice figures such as Stanley Kelley (1995), thought G&S viewed the pathologies they recounted as inherent features of rational choice theory. Given the above reading, several defenders of rational choice theory responded with something like the following argument: first, the pathologies G&S list are present in all scientific research programs, so that their mere presence cannot be used as an argument for rejection; second, because G&S do not provide an explicit comparison with other theories, one cannot tell from their book whether rational choice theory is more pathological than some other theory; third, in any event one should not reject a theory because it exhibits pathologies but because it has been superseded by a better theory, à la Lakatos.

G&S, responding to their critics, agree that pathologies appear in all scientific research programs; that their book does not specifically contrast the rate of pathology in rational choice and non-rational-choice political science; and that rational choice theories will not inevitably exhibit pathologies. Their book did not turn on or deny these points – and none of them entitle rational choice theorists to claim a Lakatosian defense.

Why not? Because rational choice theories could have properly superseded other theories and emerged as the ones to beat on proper Lakatosian grounds *only if* they had produced new empirical content. Thus, G&S's main line of attack was, and still is, that rational choice theorists have never exhibited much, if any, new empirical content. Rational choice theory may have subsumed previous empirical content and redescribed it, but 'the empirically supported contributions of rational choice to our understanding of important political subjects are ... virtually non-existent'.¹¹

G&S arrive at this conclusion as follows. First, they define 'an empirically supported contribution' as a formally derived hypothesis that has

been tested against real-world data (and not found wanting). Informal rational choice hypotheses do not count as 'rational choice', even if inspired by more formal work. Experiments without external validity do not count as 'empirical'. Second, they confine attention to *new* hypotheses generated by rational choice theories. If a rational choice model predicts the same things that an older non-rational-choice model did, perhaps on entirely different grounds, then this is merely a reinterpretation and there is no sense in which it counts as an empirical contribution of rational choice theory; the prediction redounds solely to the credit of the older theory. Third, they also confine attention to what they consider to be non-obvious hypotheses. Fourth, when searching for new, non-obvious, and formally derived rational choice hypotheses that have been sustained against the real-world empirical record, G&S look long and hard at the pivotal voter model and the majority-rule cycling model, two models already notoriously unsuccessful in empirical terms, and avoid well-known and empirically more successful models, such as those developed to explain patterns of coalition government or behavior in take-it-or-leave-it bargaining situations. Indeed, of the 100 pages in their main review chapters (4–7), 41 are devoted to these two models. Although G&S do take on one model with a good empirical reputation (Olson's model of collective action), on the whole they do not provide the sort of review that could sustain a global claim that rational choice theory has produced virtually no interesting and empirically sustained propositions about politics, much less the broader claim that they announce at the beginning of *Pathologies*, whereby virtually nothing has been 'learned' about politics from the rational choice approach.

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11. This is how one of G&S's colleagues at Yale characterizes their contention. See Smith (1997: 269).

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