

PART VI

What have we learned?

Has public choice contributed anything to the study of politics?

The human understanding when it has once adopted an opinion draws all things else to support and agree with it. And though there be a greater number and weight of instances to be found on the other side, yet these it either neglects or despises, or else by some distinction sets aside and rejects, in order that by this great and pernicious predetermination the authority of its former conclusion may remain inviolate.

Sir Francis Bacon

In the fifty some years since the field of public choice was launched with the works of Black (1948a,b), Buchanan (1949), and Arrow (1951), it has grown tremendously in both breadth and depth. A comparison of the lengths of *Public Choice* published in 1979, *Public Choice II* published in 1989, and *Public Choice III* actually *understates* the growth of the field, since the current text leaves uncovered or only lightly covered a far greater fraction of the literature than did the one published in 1979.

The growth of the literature also reflects a growth in the number of people working in the field. This growth has been particularly conspicuous in political science. Three of the major figures in the field – Kenneth Arrow, James Buchanan, and Amartya Sen – have been awarded Nobel prizes. It would seem reasonable to conclude from these developments that the experiment of introducing rational actor models into the study of politics has been a success.

Not all observers would agree, however. From early on, the public choice or economic approach to politics has had its critics [for example, Stokes (1963) and Barry (1965, 1970)], and if anything criticism of the public choice approach has become more strident in recent years. It seems warranted, therefore, before closing this review of public choice to address some of the criticisms that have been launched against it. The weary reader will be happy to know that I shall not try to take up *all* of the criticisms that have been made against the public choice approach – that would require a book at least as long as the present one. Instead, I shall focus upon the attack of Green and Shapiro (1994), two political scientists, since theirs is a frontal assault on the public choice approach, and it subsumes many of the criticisms leveled by others.¹ Any reader who has been persuaded that public choice does have something to offer to the study of politics and is not interested in methodological disputes should skip to the final chapter.

¹ From here on I shall refer to the authors as simply G&S with apologies to fans of Gilbert and Sullivan. All unidentified page references in this chapter are to their book.

28.1 The failures of rational actor models of politics

G&S

... contend that much of the fanfare with which the rational choice approach has been heralded in political science must be seen as premature once the question is asked: What has this literature contributed to our understanding of politics? ... To date, a large proportion of the theoretical conjectures of rational choice theorists have not been tested empirically. Those tests that have been undertaken have either failed on their own terms or garnered theoretical support for propositions that, on reflection, can only be characterized as banal: they do little more than restate existing knowledge in rational choice terminology. (p. 6)

To support these claims, G&S focus on three of the classics in the public choice field: Arrow's *Social Choice and Individual Values*, Downs's *An Economic Theory of Democracy*, and Olson's *The Logic of Collective Action*. Clearly, if these three works have not contributed to our understanding of political processes, it is unlikely that lesser works have done so, and thus it pays to consider whether G&S have indeed made their case. Have we learned little or nothing over and above what was already known in political science from these works and the literatures that they spawned?

G&S examine the empirical support for four predictions, which they claim emerge from the above three books: (1) that cycling will be widespread in legislatures (Arrow), (2) that rational citizens will vote only when the expected, instrumental gains from the outcome of the election exceed the cost of voting (Downs), (3) that candidates compete in issue spaces and converge upon identical platforms in two-party systems (Downs), and (4) that rational individuals will not join groups that provide public goods without selective incentives, that is, that they free-ride (Olson).

G&S argue that the public choice literature has failed to produce much in the way of empirical support for any of these predictions and, more generally, "has yet to get off the ground as a rigorous empirical enterprise." The reason for this is, according to G&S, that empirical testing of the implications of rational choice models has suffered from several, fundamental "methodological pathologies" (p. 33). These include: (1) post hoc theorizing (pp. 34–8). When confronted with evidence, which is inconsistent with the predictions of a model, the rational choice scholar introduces some auxiliary assumption which "rescues" the theory from being rejected by the data. G&S use the introduction of a "taste for civic duty" into the Downsian rational voter model to avoid the awkward prediction that no one will vote as one example (p. 50 ff.); (2) formulating untestable theories. "Those who seek to derive testable propositions from rational choice models frequently find ... that these theories are constructed in ways that insulate them against untoward encounters with evidence" (p. 38); (3) selecting and interpreting the evidence. Rational choice scholars are accused of searching for evidence that will confirm their theories (pp. 42–3), of projecting evidence from their theories (pp. 43–4), and of placing arbitrary restrictions on the domain over which their theories are applicable (pp. 44–6). An example of the latter would be an argument that the prediction of free-riding in

the voluntary provision of public goods is not refuted by individuals participating in mass demonstrations, since this behavior is “irrational” and thus falls outside of the theory (p. 88).

Before discussing these criticisms of G&S, it is perhaps worth pausing to consider what the methodology of the rational choice approach is exactly, and its potential and limitations.

28.2 The rational choice approach to modeling

The fundamental assumption of the rational choice approach to modeling is, of course, that people are rational. In most applications of the approach this translates into assuming that they are *maximizers*. To be a maximizer you have to have something to maximize. Thus, before the rational choice analyst begins to model human behavior, she must decide what it is that the people whose behavior she wishes to explain are maximizing. She must postulate an objective function.

Now the first thing to note about the necessity to posit an objective function is that this must come from *outside* the theory to be tested. Most often the rational choice analyst chooses the arguments to go into the objective function by introspection or by simply using the objective function, which has become standard in the literature (firms maximize profits, workers maximize utility, which is a function of income and leisure). But she might also consult sociology or psychology to see what is a reasonable assumption to make about the particular group she is studying. While *some* economists might study the Catholic church by positing that it maximizes profits, and the behavior of priests assuming that they are only concerned about income and leisure, other more daring and ambitious scholars might try to determine from other sources what are reasonable assumptions about the goals of these actors. The rational choice approach does not require that the rational choice scholar refrain from using knowledge from other disciplines that might help her specify the goals of individuals.

The second step in constructing a rational actor model is to specify what, if any, constraints exist within which the actor must operate. Once again there are standard assumptions regarding the choice of constraints in economic modeling (consumers have limited budgets), but in applying the approach in new areas the analyst will need to find out what are reasonable assumptions. Here again, appeal to other branches of the social sciences may be in order.

Once the rational choice analyst has specified the arguments of the objective function and the relevant constraints, she can maximize this function. This gives her one equation – the first-order condition from the maximization problem – with which to make predictions. In some cases, the second-order condition may give her some additional, predictive power. Still more analytic power can be brought to bear on the problem if the analyst can assume that the aggregated behavior of all individuals in the system leads to an equilibrium outcome. This gives the analyst two equations instead of one, and increases the possibility of deriving refutable propositions. The great interest of the rational choice analyst in determining whether political competition is likely to produce an equilibrium or not arises from the greater predictive content of models with equilibria.

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Armed with these two equations, the rational choice analyst can, ideally, derive predictions from her model and test them with the relevant data. Very often, however, the fairly simple objective function specified for the actors and the other elements of the model lead to rather general predictions. For example, the usual assumptions that economists make about consumer utility functions lead to the prediction that consumer demand schedules have negative slopes. Consumers buy more of a good at a lower price. This is certainly one of the “banal” predictions from rational choice models that “do little more than restate existing knowledge.” Moreover, any estimate of the elasticity of demand for a good between -0.001 and -1000 could be interpreted as “consistent with the theory’s predictions,” and thus support for rational choice models in general. Such flexibility in empirical testing is part of what disturbs G&S. To derive more precise predictions, however, one must build more into the model. How one chooses to modify the model to obtain more accurate predictions will depend on the questions one wishes to answer with it.

Consider the following example: suppose that an economist at the University of Iowa decides to estimate the demand schedule for pork in Iowa, and the fraction of consumers’ food budgets that goes to the purchase of pork. He gathers data on consumer incomes, prices and quantities of pork sold, prices of substitute products like beef, and so forth and estimates the parameters of his model. The fit to the data is so good that he has great confidence in the accuracy of his estimates. He sets up a consulting firm and begins to use the model to predict pork sales in other states and countries. He has great success in some states and countries, but the model does very poorly in explaining purchases of pork in Israel and Egypt. A sociologist friend of the economist suggests that this may be because the Jewish and Moslem religions forbid eating pork.

What to do? One possibility would be to refuse to use the model in countries with large Jewish or Moslem populations on the grounds that it is “irrational” to allow one’s religious beliefs to affect one’s food consumption and the economist’s model of demand assumes rational individual behavior. This would be an example of the kind of domain restriction of which G&S are highly critical. A more pragmatic response would be to introduce some variables, like the percentages of the population that are Jewish or Moslem, to account for the differences in “tastes for pork” by these consumers. G&S would also be unhappy with this amendment to the rational actor model to improve its explanatory power, and would dismiss it as just another example of the kind of post hoc theorizing that rational choice scholars resort to when their models fail to perform well empirically. Their position seems to be that once the rational choice theorist has constructed a simplified model to explain one phenomenon – demand schedules have negative slopes – she cannot modify this model to improve its performance in specific applications. If the only relevant variables in the simplified model were income and prices, then income and prices must explain equally well the demand for all groups for all sorts of goods.²

Many economists would agree with them. Extreme in this regard is the position of Stigler and Becker (1977), whom G&S cite, who regard any appeal to

² See the discussion of Chong (1996) and Diermeier (1996).

changes in preferences to explain anomalous empirical findings as unscientific. Many economists also regard any assumption about the motives of managers, other than that they maximize profits, as ad hoc. But some managerial decisions, like paying high premia to acquire firms in unrelated industries, seem difficult to explain as attempts by managers to maximize profits. The investment and merger activity of large corporations is easier to explain by assuming that managers maximize an objective function that includes the growth of the firm in it, and thus some economists, like myself, have chosen to posit this sort of objective function when modeling corporate investment and merger activity. Such models are no less a part of the rational actor literature than are models that assume profits maximization. There is nothing in the rational actor methodology that demands that we assume that there is only one argument in an actor's objective function, and that the analyst is constrained in her choice of what this one argument should be by the choices made by previous analysts.

This point is particularly important to keep in mind when considering the application of rational actor modeling to politics. What, for example, should the rational choice analyst assume goes into the objective function of a rational bureaucrat? To answer this question she can contemplate what goals she would pursue if she were a bureaucrat, she can consult sociology and psychology books dealing with bureaucracies, read Franz Kafka or other novelists who have written about bureaucracy, and so on. Niskanen (1971), having worked in the Defense Department, came to the conclusion that bureaucrats maximize the size of their budgets and developed a theory of bureaucracy based on this assumption. This behavioral assumption is obviously similar to that mentioned above used to explain the investment and merger activity of large firms. But it may not explain the behavior of all bureaucrats in all settings. Perhaps if Niskanen had worked in a different bureaucracy with different constraints and opportunities, he would have concluded that bureaucrats maximize leisure, or the probability of not being fired.

Niskanen was the first person in the public choice field to develop a model of bureaucracy, and many who have followed him have also assumed that bureaucrats are budget maximizers. A number have also claimed to find empirical support for this hypothesis. We shall reexamine one set of studies that provides this support below. It would be wrong to argue, however, because Niskanen was the first to model bureaucratic behavior and he assumed that bureaucrats are budget maximizers, and because several other studies have made the same assumption, that the application of the methodology of rational choice *requires* that one assume that all bureaucrats maximize their budgets, and only their budgets. It would be equally wrong to interpret any evidence that is inconsistent with the predictions of a model of budget-maximizing bureaucrats as rejecting *both* the assumptions of this model and the rational choice approach to the study of bureaucracy.

A good positive theory derives strong and refutable predictions from a relatively small set of assumptions. Arrow's essay is not intended to be a contribution to positive theory, and it is a bit misleading to argue against it on the basis of empirical studies as I shall explain in the following test. Downs's and Olson's contributions

are positive theories and they meet the criterion for good positive theory – they make strong predictions from a few assumptions – no one votes, two candidates adopt identical platforms, no one contributes anything voluntarily to the provision of a public good.³ G&S appear to want to reject both these theories and the rational choice approach to politics, because the theories' strong predictions are not born out by the data. But such a step is premature. Neoclassical economics also makes strong predictions. Under the assumptions that firms maximize profits and markets are competitive, for example, all firms should earn zero profits. Should one reject all neoclassical economics and the rational choice approach to human behavior if one observes some firms earning positive or negative profits?

Before doing so one must answer three questions: (1) Are the predictions of the theory so wide from the mark that it is impossible to believe that the theory accounts for the data? (2) Are there additional, plausible assumptions that one can make which are consistent with the rational choice methodology and that will account for the discrepancies between the model's predictions and the empirical evidence? (Some markets are not perfectly competitive, some managers maximize objective functions that include additional or other arguments than profits.) (3) Is there an alternative theory that explains the data better? Only if the answers to these three questions are yes, no, and yes is one justified in abandoning the rational choice approach. G&S claim that this is the case for the theories of Arrow, Downs, and Olson. Let us examine their arguments further.

28.3 The prediction of cycling

G&S cite Arrow (1951) as the source of the prediction that democracy is unstable, and in particular that legislatures will be plagued by cycles. As noted earlier, Arrow's book is a contribution to normative theory. It makes *no* predictions about what one might observe in practice. The impossibility theorem states that it is not possible to devise a process for aggregating individual preferences that both defines a social ordering and satisfies the famous five axioms (see Chapter 24). The only empirical prediction from this theorem that is possible is that any actual system for aggregating individual preferences must violate at least one of the axioms, *assuming, that is, that the system defines a social ordering*. Since it is unlikely that any political system truly defines a social ordering, that is, a ranking of all feasible social states, no "prediction" from the theory is truly testable. If, on the other hand, we assume that all political systems are capable of defining a social ordering, then the theory still only predicts that at least one of the axioms is violated. It is just as legitimate to claim that Arrow's theorem predicts that all political systems will be dictatorships as it is to claim that it predicts that all legislatures will get caught in voting cycles. Since until very recently most of the world's political systems have been dictatorships of one form or another, it might be argued that this "prediction of the Arrow theorem" is well supported.

³ Strictly speaking neither Downs's model of the rational voter nor Olson's model predict zero participation. The voluntary-contribution-to-a-public-good model presented in Chapter 2, for example, predicts *positive* contributions when the group's size is finite. See Lohmann (1996).

When the underlying preferences of the members of a committee are such as to produce a cycle over all of the feasible outcomes, it would be possible for the committee to cycle endlessly over these outcomes. Since rational people will not want to spend endless amounts of time voting on a given set of issues, one expects a committee made up of rational people to establish procedures that reduce, if not eliminate, the probability of a cycle. The questions Arrow's theorem raises about these procedures are, do they in fact eliminate cycles, and if so how? Do they eliminate cycles, for example, by producing arbitrary outcomes – the outcome chosen depends on the chance order in which issues come to a vote in a constrained agenda; dictatorial outcomes as the result of agenda manipulation; or Pareto-dominated outcomes because the agenda has been so restricted to avoid a cycle that a Pareto-optimal outcome cannot come to a vote? G&S are highly critical of the efforts by rational choice scholars to answer these questions. Some of their criticisms are well taken, but they do not undermine the importance of determining the answers to these questions. Had Arrow (and Black) not alerted us to the potential for cycling and its dangers, we would not even be in a position to pose the right questions about the impact of legislative procedures, let alone answer them.

It should also be noted that cycling *can* be observed in situations in which political institutions have *not* been designed to prevent them. The formation of a cabinet in a multiparty system has the characteristics of a zero-sum game. There are a fixed number of cabinet positions, and a majority coalition must form to determine how the positions are allocated. When an election results in the possibility of three or more assignments of cabinet seats, the stage is set for a cycle. The frequent turnover of governments in countries like Italy and Fourth Republic France looks a lot like what coalition theory leads us to expect in the absence of a core.

Simulation studies indicate that the probability of a cycle rises with the degree of heterogeneity of the preferences of the committee members, and with the size of the issue set (see Chapter 4). Thus, the findings that cabinet stability declines as the degrees of fractionalization and polarization in multiparty parliaments increase is consistent with what one expects from rational choice theory.⁴ Cycling can and does occur in more unstructured settings, and thus it is important to find out if it occurs in legislatures like the U.S. Congress, and if not why not.

28.4 The predictions of spatial models

The Downsian model of two-party competition with a single-dimensional issue space predicts that both parties adopt the position favored by the median voter (see Chapter 11). Most probabilistic voting models of two-party competition also predict that both parties adopt the same position, although now it is some sort of *mean* of the voters' ideal points (see Chapter 12). Almost everyone probably agrees that U.S. presidential candidates and the leading two parties in Britain do not adopt identical platforms. Once again the rational actor model is hoisted with the petard it has created by making too precise of a prediction. But before tossing aside these

⁴ See Grofman and van Roozendaal (1997) and Chapter 13, this book.

models and the rational actor methodology that they employ, let us try to answer the three questions just posed: (1) Are the predictions so wide from the mark that it is impossible to accept the theory? (2) Are there additional, plausible assumptions that one can make that are consistent with the rational choice methodology and will account for the discrepancies between the model's predictions and the empirical evidence? (3) Is there an alternative theory that explains the data better?

Let us start with the first question. How close must the platforms of two candidates be for us to say that they are close enough to accept the theory? Now the first problem in answering this question is, of course, that differences between candidate platforms cannot be measured as easily as, say, distances between vendors on a beach. Closeness is to some extent in the eye of the beholder, and G&S emphasize that rational choice scholars are not of one opinion on this matter (pp. 153–4).

Let us leave this measurement problem aside, however, and assume that we can measure distances between candidates objectively. One way to try and answer the question of whether the two candidates' platforms are close enough to one another to accept rational choice theory is to compare its prediction to that of a competing theory that does not assume that voters and candidates are rational actors. What is a reasonable alternative model of two-candidate competition? What is a reasonable null hypothesis?

Perhaps a good way to begin to answer these questions is to consider the problem first addressed by Hotelling (1929) in his classic article on spatial competition. Although Hotelling's article is often cited as the first spatial model of voting, he did not set out to examine this problem, but rather a seemingly simpler and yet more intriguing question – the choice of location of sellers in a spatial market. Imagine bathers evenly distributed along a straight stretch of beach of length d . Two ice cream vendors set up stands along the beach. Where does one expect them to set up their stands?

The simplest hypothesis would be to assume that they choose locations at random. This would lead us to expect that we would find each vendor at a different location on the beach each day. The distance between them, b , would be a random variable, but over time we would predict that the mean of b would equal one half of d .

An alternative hypothesis would be that the vendors seek to minimize the distance bathers must walk to purchase an ice cream. This might be called “the public interest theory” of vending.⁵ This hypothesis leads to the prediction that the vendors locate their stands one-fourth of the distance from the two ends of the beach. Now we would predict that the vendors choose the same locations every day, and that $b = d/2$ every day.

What other predictions are reasonable? If one described this problem to a random sample of people passing through Times Square, I doubt that many of them would predict that the two vendors would locate adjacent to one another at the center of the beach. Yet this is the prediction of the Hotelling model. Suppose now we went to a beach that was 100 meters long, and observed two vendors located adjacent

⁵ Hotelling (1929, pp. 52–3) discusses this possibility as the outcome one would expect under socialism and characterizes it as “an argument to the socialist side.”

to the center of the beach five meters apart. Would we reject Hotelling's model because they were not located literally next to one another, or would we say that the observed b is so much smaller than $50 (d/2)$ that we accept the model over its rivals, leaving open of course the possibility that some other model may come along that outperforms Hotelling's in predicting the locations of the two vendors?

Returning to the problem of two-party competition, I suspect that estimates of b and d for two-party systems would reveal that b tends to be significantly less than $d/2$. Is this sufficient for us to accept the simplified version of the Hotelling/Downs model of two-party competition? For some it will be, for others not. Some will wish to predict b more accurately and thus will choose to modify the simple version of the model.

The Hotelling/Downs model assumes that there is but one election, and that the candidates are free to choose any position in the issue space. In the United States a person must win two contests to become president – the one to become the candidate of her party and then the one to become president. A direct extension of the Downsian model to take into account the nomination process leads to the prediction that candidates would adopt the position favored by the median voter *in their party* to win its nomination, and then move to the median voter's position in the full electorate. If one adds the reasonable auxiliary assumption that it is not possible for a candidate to move all of the distance between the median for her party and the median for the country during the short span of time between party conventions and presidential elections, then one reaches the following predictions: (1) prior to being nominated candidates adopt positions far removed from one another, (2) after their nominations they move toward the center, and (3) at the election they are located nearer to one another than when they were nominated, but they still do not adopt identical positions.⁶ These predictions would seem to fit the facts of American presidential elections fairly well. G&S do not discuss these extensions of the Downsian model by rational choice scholars, but I expect that G&S would also dismiss this work as “post hoc theorizing.”

In Chapter 19 we discussed one attempt by rational choice scholars to develop a model of two-party competition that explicitly took into account the ideological differences between parties, which led them to adopt different policies – namely, the work of Alesina (1988b) and Alesina and Rosenthal (1995). The Alesina and Rosenthal model makes some very precise predictions about the patterns of income growth over the electoral cycle under Republican and Democratic administrations, and not all their predictions are supported by the data. Yet the model accounts for a number of phenomena, like the midterm cycle, that other observers of politics have had trouble explaining.

G&S concentrate on the attempts by rational choice scholars to explain outcomes in two-party systems. The Downsian spatial model has been adapted to study competition in multiparty systems, however, with considerable success (see discussion in Chapter 13). van Roozendaal's (1990, 1992, 1993) prediction that “central parties” will always be part of a coalition government is a fairly straightforward extension of

⁶ See discussion and references in Chapter 11, Section 11.1.

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the logic of the median voter theorem to cabinet formation, and one that has proven to be accurate roughly 85 percent of the time (Laver and Schofield, 1990, p. 113). Is 85 percent a sufficiently high success rate to vindicate the use of spatial models and coalition theories to predict cabinet compositions in multiparty systems? Is there a nonspatial model that does better?

Laver and Shepsle's (1996) model of cabinet formation extends the median voter model to a multidimensional issue space, and Schofield's (1993a,b, 1995) concept of the "heart" is yet another development using spatial theory and rational choice models to predict which parties will form the governments in multiparty systems. The predictive power of these models seems sufficiently strong to warrant retaining the rational choice approach and spatial theory in the study of multiparty systems – pending the appearance of models with greater explanatory power that do not employ this methodology.⁷

28.5 Predicting voting and free-riding

In Chapter 14 we reviewed the public choice literature explaining voter turnouts. As the reader will recall, the simple Downsian rational-voter model, in which the voter weighs the expected benefits from bringing about the victory of his favored candidate against the cost of voting, does not offer an adequate explanation of why people vote. Moreover, some of the attempts to modify the theory by proponents of the rational choice approach raise more questions than they answer. G&S make much of these failures of the rational choice approach and many of their points are well taken (pp. 50–68).

G&S also question the empirical support for the prediction, often associated with Olson (1965), that people will not voluntarily contribute to the provision of a pure public good, as their receipt of the benefits from its provision are independent of their contribution. Among the evidence that they cite against the free-rider hypothesis are the many public good-provision experiments that find participants making far greater contributions than the rational choice hypothesis predicts.⁸

There is no question that these sorts of results constitute a great challenge to the rational choice approach to politics. Many practitioners of this approach have been disturbed by these findings and have gone to great (excessive) lengths to explain them away. But the proper response to such contradictory evidence is neither to dismiss it as irrelevant nor, as G&S would seem to have us do, to discard the rational choice approach in its entirety. The proper reaction is to reconsider this approach's premises and try to determine which of them is sufficiently far from reality to account for the predictive failures. Once again, it is also necessary to compare the predictive power of the rational actor model with that of alternative approaches.

What prediction, for example, would a student of politics who is not a follower of the rational choice approach make as to the likely contribution of someone in a public goods experiment? One possibility, of course, would be to assume that people *do not*

⁷ See also Schofield (1996b).

⁸ See G&S (pp. 88–93) and our discussion in Chapter 2.

free-ride. They pursue the public interest rather than their own, narrowly defined self-interest. If the contribution that maximizes the payoffs to the group is 100, and the contribution that maximizes the payoff to an individual contributor is 1, this public interest model would predict an individual contribution of 100. Since the typical outcome from a public goods experiment is an average contribution of around 50, the prediction of the public interest model is as far off the mark as is the selfish-individual, rational choice model. Both need to be significantly modified to account for the findings in public goods experiments.

Note that both the public interest and the rational choice models assume that people are maximizing some sort of objective function and thus yield very precise predictions. Because of their preciseness they are very easy to reject. But before discarding either model, we must again ask what the predictions of the alternative models are. If one claims that the nonrational choice approach would predict some contribution between 1 and 100, one stacks the cards in favor of this approach. And even this vacuous interpretation of the alternative to rational choice models would not help us to predict *which* people would contribute more than 50, which less.⁹

In Chapter 14 I proposed modeling individual behavior under the assumption that people acted *as if* they were maximizing an objective function of the following form:

$$O_i = U_i + \theta_i \sum_{j \neq i} U_j. \quad (28.1)$$

Many sorts of behavior, like individual choices in market experiments, can be adequately explained assuming θ equals zero. A contribution of 30 by one person and 60 by another in a public goods experiment would, on the other hand, imply both positive and different θ s for each person.

Of course, such a model would merely allow us to offer a post hoc rationalization for deviations from the predictions of the selfish, rational actor model, unless we are able to explain why one person has a θ of 0.3 and someone else 0.6. Moreover, to construct a general theory that can predict human actions, we would need to be able to explain why a given individual might behave in one situation as if θ were zero, and in another as if it were one. Such explanations are more likely to be found in the field of psychology than in the rational choice literature. The key difference between such a behavioral approach and the usual application of rational actor models is that it forces the investigator to examine the *past* histories of the people whose behavior he wishes to explain, and not just focus upon the entries in the different cells of the game's payoff matrix. Human behavior is viewed as being adaptive and only approximates the purely forward-looking behavior depicted in rational actor models.

This adaptive approach would remove some of the surprise from experiments such as those of Marwell and Ames (1981), who found that graduate students in

⁹ Strong proponents of the rational choice approach can take solace from the fact that mixed-strategy equilibria to this sort of game exist in which some participants make contributions greater than one (Lohmann, 1996). G&S (pp. 125–8) will only lament that this is just another example of the failure of game-theoretic approaches to generate refutable hypotheses.

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economics contributed significantly less than other students in public goods experiments, or of Blais and Young (1999), who found that Canadian students were significantly less likely to vote after having listened to a lecture explaining the Downsian voter model.

Many proponents of rational choice modeling, like Stigler and Becker (1977), would reject any attempt to improve the predictive power of a rational choice model by allowing preferences to be malleable, and Riker (1990), whom G&S cite (pp. 185–6), explicitly rejects behaviorism as an alternative to rational choice modeling. Thus, anyone who would follow the route that I have suggested would take a long step away from the pure forms of rational choice analysis, and indeed a step in the direction of the approach advocated by G&S, which tries to identify “cognitive or social-psychological factors that affect the degree to which actors follow impulse, habit, or the lead of others” But this approach would not force one to abandon the search for a universal theory of human behavior, which G&S would have us do, nor would one have to sacrifice the potential for analytical rigor that comes by modeling individuals as maximizing explicitly defined objective functions.

28.6 Can public choice contribute to the positive study of political institutions?

G&S’s book is filled with examples of empirical studies by public choice scholars who, G&S claim, made fundamental methodological errors that robbed their work of scientific value. In closing their book, they offer these scholars the following advice:

More fruitful than asking “How might a rational choice theory explain X ?” Would be the problem-driven question: “What explains X ?” (p. 203)

In this section we describe a few studies that have employed the rational choice approach to explain X and, I believe, have done a relatively good job of it.

In the state of Oregon local school boards are free to spend any amount of money up to an amount defined by a specific formula. This formula-set limit is called the reversion budget, R . If a school board wishes to spend more than its R , it must seek the approval of the voters in its district. Some school boards propose amounts above their R , some do not. Some proposals are greatly above R , some are not. How might one predict when a school board would call a referendum to approve a higher budget, and by how much it would deviate from the reversion level?

Romer and Rosenthal (1978, 1979b, 1982) addressed these questions using the public choice approach.¹⁰ They first had to posit an objective function for the school board. Following Niskanen (1971) they assumed that school board members were bureaucrats who maximized their budgets. Romer and Rosenthal then utilized the median voter model to predict the maximum possible budget a school board could get approved in a referendum. With these two elements of the model in place, they

¹⁰ Their work is discussed in more detail in Chapter 16.

were able to generate several quite specific predictions as, for example, that the amount by which a proposed budget exceeds that favored by the median voter will be higher, the lower R is relative to the median voter's preferred expenditure; and that referenda will not be called when R exceeds the expenditure favored by the median voter. Their predictions were supported by the data.

Note that Romer and Rosenthal committed all of the sins outlined by G&S. They assumed that bureaucrats were budget maximizers and voters were utility maximizers. They assumed that the collective choice problem could be analyzed using a spatial model with a single-dimensional issue space. They invoked the median voter theorem.

How should one proceed if one does not want to commit these sins? Does one posit an objective for a school board, and if so, what is it? Lane (1996, p. 123) criticizes the rational choice approach for assuming "that managers of public enterprises are motivated by personal self-interest." Instead, he claims that they internalize the goals of their organization, citing Wolf (1988) in support. Applying this assumption to school boards one might assume that each seeks to provide the students in its district with a "good education." If so what model does one use to predict the amount needed in each district? Does one proceed inductively, and set up a probit model to predict when a school board calls a referendum and collect data on all possible relevant variables (number of school-age children in a district, income of the district, and so on). With considerable diligence and luck one might come up with enough variables to provide a reasonable fit to the data. But one would not really understand why the school boards behaved as they did. One would also not be able to pass judgment over whether the school budgets were larger or smaller than they should be. Indeed, through one's choice of motivation for the school board, one would have essentially already assumed that each school budget was at its optimal level.

One of the advantages of a rational choice approach over a purely inductive approach to modeling is that the rational choice approach often can identify whether policy outcomes are inefficient or suboptimal in some other way. If one adheres to the view that the government ought to do that which the median voter desires, then one must conclude from Romer and Rosenthal's work that school budgets in Oregon are systematically larger than they should be.

In *The Theory of Political Coalitions* (1962) Riker followed exactly the procedure G&S recommend – he chose as the subject to be analyzed a puzzle from the real world. Why are grand coalitions so short lived? He deviated from their recommendations by applying rational choice analysis to this problem. Using this approach he developed his "theory of minimum winning coalitions." I know of no better analysis of this question. G&S are critical of Riker's applications of rational choice analysis to the study of politics at several places in their book, but curiously they omit any reference to this application, Riker's most famous contribution to the literature.

Observers of U.S. politics have long believed that congressmen trade votes on bills. How can one test whether this in fact is true? Does logrolling occur on all bills, or only on some? If only on some, how does one determine which issues are the result of vote trades and which are not?

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The public choice analysis of logrolling provides a rigorous means to test whether it occurs. From the definition of a logrolling situation we predict that issues X and Y would lose, if individuals would sincerely state their preferences, and pass as a result of vote trades.¹¹ This precise definition of logrolling leads to the prediction that trading should only occur on issues where the vote is close *and* the votes of the traders are crucial to the victory of the winning issues. Thus to test for the presence of logrolling one first needs to construct a model to predict how representatives will vote in the absence of a trade. This, in turn, requires us to model the voting behavior of representatives, and thus to make some assumption about their motivation. The work of Stratmann (1992b, 1995) indicates that logrolling occurs on some issues, but not on others. It allows us to provide precise answers to the above questions. How one would answer these questions without employing the analytical tools provided by public choice is difficult to imagine.

One could cite other examples from the literature on voting-with-the-feet (Chapter 9), rent seeking (Chapter 15), campaign contributions (Chapter 20), government size (Chapter 21), and still more.¹² However, I hope that these examples will suffice to convince the reader that the methodology of public choice is capable of providing rigorous empirical tests of hypotheses about politics, and that at least *some* practitioners in the field have conducted such tests.

28.7 Has public choice contributed anything to the normative study of political institutions?

Just as challenging for the student of politics as the question of why government sizes differ so much across countries, is the question of why the choice of voting rules differs *so little*. Why does virtually every legislature and other sort of committee employ the simple majority rule for most, if not all, of its collective decisions? This question obviously *cannot* be approached empirically, because there is little or no variation in the variable that one wishes to explain. The answer must be sought in a normative analysis of the simple majority rule. The simple majority rule must be presumed to be the best voting rule because it is the rule preferred by all forms of committees. But in what sense is it best?

Public choice has offered several answers to this question (see Chapters 4 and 6). The most elegant of these is May's (1952) proof of the equivalence between the simple majority rule and four axioms. If one believes that a voting rule should satisfy these four axioms, then one should advocate the use of the simple majority rule for making collective decisions.

May's theorem is valid only for binary choices. Should a committee need to decide among three or more alternatives, it must consider the possibility of cycles under the simple majority rule. The normative case for the simple majority rule is weakened, and we enter the realm of Arrow's (1951) impossibility theorem. Even if we assume with G&S that cycling is not often observed in some legislatures, like the U.S. Congress, this observation in no way detracts from the significance of the

¹¹ For a full statement of the definition and discussion, see Chapter 5.

¹² See also the examples cited by Fiorina (1996, p. 90), Ordeshook (1996, p. 176), Shepsle (1996, p. 218), Cox (1999), and discussion in Mueller (1997b).

Arrow theorem. This “fact” merely alerts us to another fact – that one or more of the five axioms of the theorem must generally be violated.

New democracies appear from time to time and must decide which voting rule(s) to inscribe in their constitutions. Old democracies sometimes amend their constitutions. Should the student of politics recommend the simple majority rule, some venerable rule like the Borda count, or some newly invented rule like approval voting, the demand revelation process, or voting by veto? I do not see how one can offer an adequate answer to this question unless one understands the formal properties of each voting rule.¹³

28.8 Conclusions

Suppose that you were asked to explain why the OPEC countries sometimes agree to cut petroleum output significantly and oil prices rise to great heights, and at other times they expand their outputs driving petroleum prices into great troughs. These are decisions by governments and thus by definition political decisions. A good student of politics ought to be able to explain them. How should a good student proceed?

The good student might first inquire as to the likely motivation behind the decisions for each government. Oil revenues are higher when oil prices are higher, and so one might posit that the OPEC countries are trying to increase their revenues when they cut outputs to raise prices. A reasonable beginning would be to assume that each OPEC country is a revenue maximizer, and that the periodic meetings of representatives from each country in Vienna are attempts to set outputs so as to maximize the joint revenues of the OPEC members.

The astute student might next observe that cartels have the characteristics of a prisoners’ dilemma, and thus are vulnerable to free-riding if each country is maximizing its revenues. A first start to the problem would be to develop, or locate within the literature, a model of cartel behavior that predicts that cartels will sometimes succeed in restricting output and raising revenues, but then breakdown as individual members engage in free-riding behavior.

Should one not also allow for the fact that Saudi Arabia is an Arab, Moslem country and Venezuela is Catholic; that Kuwait is rich and Nigeria poor; that some country leaders are empire-builders, while other leaders appear content to preserve their monarchies? Perhaps, but before doing so most rational choice scholars would first want to see how well the simpler model explains the data, the model that assumes only that each country seeks to maximize its revenue, and together they are caught in a recurring prisoners’ dilemma. Only if this model fails to account for the observed pattern of prices and quantities adequately would it be necessary to bring in other factors.

Great theorists provide clear answers to important questions that often arise not as a result of their reading of the latest issue of a technical journal, but from reading the latest newspaper and observing the world around them. Riker (1962) was puzzled by the short lives of grand coalitions, and developed a theory to explain why they so quickly come apart. Olson (1982) was puzzled by the relative economic success

¹³ See also Schofield (1996b, pp. 190–1).

of the countries which lost World War II compared to the winners, and developed a theory to explain the losers' superior performance. In both cases they found answers to the questions they tackled through the application of rational choice analysis.

John Maynard Keynes (1936) was puzzled as to how widespread unemployment could arise and persist. He did not find an answer in the prevailing economic models with their predictions of market equilibria. He compared the premises of these models with the facts of the world around him and found some of the premises wanting. Wages were not as flexible as the competitive model assumed; interest rates sometimes got stuck in a "liquidity trap." Investors were not the rational, cool, calculating individuals who appeared in economic models, but rather mortals whose "animal spirits" sometimes got the best of them. By abandoning some of the assumptions contained in the reigning paradigm, Keynes created a model of the economy which could account for the existence of widespread and persistent unemployment. His modification of the reigning paradigm was attacked by its adherents from the start, and debate continues to the present day over how best to model the macroeconomy. Regardless of one's views on this question, one should recognize that Keynes's methodological approach is the one to follow. Stick to the prevailing model so long as it is able to explain the phenomena which one wishes to explain. Reexamine its premises when it cannot explain these phenomena, and substitute other premises that fit reality more closely. Continue to modify the existing model until it can adequately account for the data. Abandon the old model (paradigm) in favor of a new one if one comes along which offers a better solution to the puzzle.

The social scientist who wishes to explain the behavior of individuals as consumers, workers, voters, bureaucrats, priests, politicians, stockbrokers, soldiers, and drug addicts has a series of options. At one extreme is the universal, rational actor model – all individuals maximize an objective function (O). The starkest form of such a model would have a single variable in the objective function: all individuals maximize their own personal wealth (W),

$$O = W. \quad (28.2)$$

A slightly more general version of this model would be that all individuals maximize a utility function that includes wealth and one or two additional variables depending on the type of decisions being analyzed,

$$O = U(W, X_1, X_2, \dots). \quad (28.3)$$

Moving farther away from the strongest version of a universal theory we would have

$$O = U(X_1, X_2, \dots). \quad (28.4)$$

All arguments of the utility function are at the analyst's discretion. And moving still farther we have the approach suggested above to account for altruistic and similar sorts of behavior in situations where this behavior is anticipated,

$$O_i = U_i + \theta_i \sum_{j \neq i} U_j. \quad (28.5)$$

When one takes into account that the analyst is also free to choose the shape of the utility function and a set of constraints and auxiliary conditions under which the maximization process takes place, one sees that an approach to modeling human behavior that is universal insofar as it posits that individuals maximize an objective function can be quite flexible.

At the other extreme of the methodological spectrum is a pure inductive approach. The analyst who wishes to explain the behavior of individuals in the ten contexts just listed constructs ten different models, each one containing the set of variables that best explains the behavior of the group in question. The choice of variables in each case is determined from an examination of the relevant literatures in sociology and psychology, what has “worked” in previous studies, or simple trial and error. As one adds more arguments to the objective function, and more auxiliary assumptions, the power of the maximizing assumption is diluted and the model estimated under this approach begins to resemble that obtained by proceeding inductively. Where each scholar chooses to place herself along the spectrum running from (28.2) to the pure inductive model is largely a matter of scientific taste – one’s willingness to live with weak explanatory power in some situations for the cleanness and beauty of a simple, elegant model of human behavior versus one’s desire for high explanatory power in all situations at the cost of analytic consistency and clarity.

Earlier in this chapter we discussed several examples of behavior like voting and free-riding, which cannot be well explained with a simple version of the selfish, rational actor model. My proposal was to replace this model in these situations with a model in which individuals acted *as if* they were maximizing an objective function that included their own utility and a weighted sum of everyone else’s utility. This could be used to explain human behavior in all situations, even those where the traditional rational, self-interest model does well, since it allows for the possibility that the weight on other people’s utility is zero.

My proposal would constitute a step away from the pure rational actor model, but would retain some of the advantages of this approach in terms of making clear predictions that are subject to falsification. A more radical step is to abandon the assumption of maximizing behavior entirely. Simon (1947) won a Nobel prize for his studies of organizational behavior that built on the assumption that individuals are “satisficers” instead of maximizers. G&S seem sympathetic to Simon’s approach (pp. 22, 29, 186), and Lane (1996, p. 126) cites with favor an early application of it by Cyert and March (1963) who analyzed a single firm under the assumption that five different goals had to be satisfied. The model did extremely well at explaining the behavior of *this* firm, but it appeared that a different model might be needed for each firm in the economy, and the Cyert and March approach was not pursued by the economics profession. Applying the satisfying approach to, say, the study of public bureaucracies would seem likely to suffer a similar fate. Although with enough interviews and data, a team of economists, psychologists, and other social scientists might be able to construct a simulation model that would track decisions at the Defense Department accurately, one would not know whether it would do as well at other departments. A collection of ten simulation models, each tailored to a different government bureaucracy, *might* provide some insights to a general theory

of bureaucracy, but the likelihood seems small relative to the costs of putting such a collection together. The rational social scientist with scarce research time and resources seems well advised to avoid this research strategy.

Most parents exaggerate their children's accomplishments and overlook their failings. The same holds true for scientists with respect to their intellectual offspring. The same also holds true with respect to the scientific methodologies that they employ. It is perhaps some small comfort that these deficiencies appear to be as old as science itself, as the opening quotation of this chapter from Sir Francis Bacon reveals.

Several proponents of the rational choice approach to the study of both economics and politics can be accused of overstating the explanatory power of this approach, and of being blind to its shortcomings. G&S are justified in accusing them of hubris in the extreme. But in pressing their attack on the rational choice approach, G&S appear to ignore the weaknesses of the alternative approaches which they, implicitly, seem to advocate. They criticize rational choice scholars for seeking to develop and apply a *universal* model to explain the behavior of political actors. Although G&S make many valid criticisms of the way in which some researchers have tested such models, they offer no concrete, alternative research strategy. Thus, I expect that those working in the public choice field will continue to employ rational actor models to the study of politics modifying them as need be to explain individual actions in different situations. And I expect that many fine young scholars entering into the study of politics will continue to gravitate toward the rational choice approach precisely because it offers a more unified and compelling explanation of political behavior than do rival approaches.¹⁴

Bibliographical notes

Friedman (1996) has assembled 14 essays both supporting and taking issue with G&S, plus a concluding reply from them. Hogarth and Reder (1987) contains the proceedings of a conference in which several distinguished proponents and opponents of rational actor models made contributions. Mansbridge's (1990) anthology also includes a distinguished list of contributors who are mostly drawn from the critical side.

Both Frank (1988) and Thaler (1991) have sought to weaken or adapt the notion of rational behavior to make rational or "quasi-rational" actor models more consistent with the evidence on how humans behave arising from the psychology and experimental literatures.

Sen (1995) discusses both individual and collective rationality, and the importance of the distinction between social *preferences* and social *judgments* in the context of the Arrow Impossibility Theorem.

¹⁴ Both Ferejohn and Satz (1996) and Schofield (1996b) defend the scientific importance of universal theories.