The Political Psychology of Rational Choice Theory

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EDITOR’S NOTE: It is with some sadness that we publish the following article, one of the last pieces written by William Riker. Bill did more than any other single person to encourage the use of rational choice theory within political science. This article reflects Bill’s belief that rational choice theory provides the only scientific basis for social theory. My thanks to Ken Shepsle and Bernie Grofman for editing the final manuscript.

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I. SCIENCE AND THEORY

The goal of science is to utter accurate generalizations about nature. For the most part, scientific generalizations are statements of class inclusion: that members of the subject class are members also of the predicate class. The human value of such statements is that, when validated, they admit of prediction: Upon observing a member of the subject class, one then can infer, on the basis of the generalization, that it is also a member of the predicate class. Of course, “when validated” is a highly restrictive condition. Given the subjectivity of our demarcation of classes and the fallibility of our observation, all statements about nature are always contingent and can be validated empirically only to some degree of confidence.

The value of a generalization is that it allows for prediction. Unfortunately, however, empirical generalizations cannot explain why a predicted effect occurs. Yet science is aimed at explanation as much as at prediction. To explain a generalization is to state a necessary and sufficient condition for the subject class to be in the predicate class. An explanation thus requires that the generalization be embedded in a theory from which the necessary and sufficient conditions can be deduced. To embed a generalization in a theory gives the generalization both empirical and deductive support and increases confidence in its accuracy.
Theories begin with axioms about motions (physical science) and actions (social science) of the fundamental units assumed to exist in the scientific subject matter. From the axioms theorists infer theorems which describe motions and actions similar to the events described in empirical generalizations. Insofar as a theorem and a generalization are roughly equivalent, the theory provides deductive support for empirical description, though theorems, as abstractions, are never completely accurate empirically. Nevertheless, once a theory exists, it can be used to criticize empirical generalizations, especially the definition of classes. And revised empirical generalizations themselves force the revision and improvement of theory.

II. RATIONAL CHOICE THEORY

From this brief description of science, it is apparent that theory is scientifically essential. In the social sciences, where actors are the basic units, theory necessarily concerns actors and so begins with axioms about their actions. Believing that actions and outcomes vary according to the range of institutions and the kinds of actions guided by them, social scientists have separated their theories roughly by these considerations. Economics thus concerns mainly the processes and outcomes produced by voluntary exchange, where of course, all participants benefit. Politics, on the other hand, mainly concerns processes and outcomes produced by group decisions which are practically binding on those who cannot resign from the group. Hence, in politics there are losers as well as winners—and it is politics, not economics, that is the dismal science (Riker, 1988).

Contemporary political theory, the so-called rational choice model, starts with the assumption that actors know what they want and can order their wants transitively. (These formulations are probably equivalent. To know what one wants requires one to choose the best from among several goals and, failing to attain it, to choose the second best, etc. To order three goals is to decide that one is better than either of the other two and that a second is better than a third, which is exactly a transitive ordering.)

Note that this assumption does not specify any particular goal as humanly universal. In many earlier political philosophies, writers have specified some particular goal by fiat: Aristotle specified the avoidance of *status*; Hobbes, the avoidance of violent death; Locke, the security of property. What has in the past often been called political psychology amounts to little more than the attribution of some such oversimplified goal. In the rational choice model, however, there is no particular goal. Rather, there is a set of possible goals that must be ordered.

There is some dispute about how to define this set of goals. Some would include all conceivable possibilities. Others would limit the set to those options
believed to be worth searching for and including (so-called bounded rationality). In my opinion this dispute is futile. No one ever knows all options, so I interpret the set as those choices known to the chooser.

It is more important to point out that whether the goals in the set are good or evil or neither is irrelevant, as long as the participants order them. This does not mean, however, that the postulate lacks substance. The reason we assume ordering is that we also assume actors are purposive and their actions depend on their ordering of goals. The substance of the postulate is thus the teleological premise about human life.

Note, however, that the assumption about purposiveness does not confine the goals to ones that have external consequences. Nothing in the assumption precludes goals like pleasures in the act itself. Nevertheless, the assumption does imply that actors try to choose the best means to their ends. But while we assume that they can order their goals, we do not assume that they necessarily know the appropriate means. There are degrees of difficulty in choosing instruments. Probably everyone knows that to make an omelet one has to break eggs. But many reasonable people may be quite confused about, ardently dispute about, and even strategically lie about the effect on ends of alternative means (Austen-Smith & Riker, 1988, 1990). Hence the model does not require instrumental accuracy, although it does require that actions not be randomly related to ends and that people do try to choose instruments that they believe, sometimes mistakenly, will achieve their goals.

The choice of means is much complicated by the fact that actions by one actor produce outcomes in conjunction with actions by others. In choosing among possible actions, therefore, each actor must consider what others may choose and must evaluate the potential equilibrium outcomes from the several individual choices. This means that given the potential equilibria when the actor and others choose, the actor chooses action leading to the most satisfactory of the anticipated equilibria actually available, given others’ choices.

Consequently, just as the choice of means is complicated by the interperson-al character of equilibria, so the choice among goals is complicated by limits on potential equilibria. Utopian goals, by which I mean goals that are not embodied in any feasible equilibrium outcome, are necessarily excluded from the set of goals that actors order.

III. THE RATIONAL CHOICE MODEL AND POLITICAL THEORY

In economic theory it is relatively easy to assign goals and even to identify appropriate instruments and the resulting equilibria. But in political theory the simple identification of goals is the crux of the problem. The theorist can neither believe what actors say are their goals nor trust his or her own attributions of
them. Many people hide aggrandizement under the cloak of self-proclaimed altruism. The theorist may be gullible or cynical or both. Furthermore, utopian goals are necessarily modified in real interaction, so that action itself affects the form and order of goals. For the theorist, it is important to distinguish between idle wishes and feasible choices.

So the threshold problem of political theory is to identify feasible goals that people actually use to guide action. The only way I know to do this necessitates the use of the rational choice model. It is sometimes said that behaviorism as adapted from psychology can serve as a political theory. But it is just here that behaviorism fails. Its fundamental assumption is that intentions are unobservable and unknowable. All the scientist can do is observe behavior and outcomes, which tells nothing about intentions or how people choose actions. This method makes sense when observing bees or pigeons or even chimpanzees. We do not understand their thought processes and so we cannot imagine their intentions. But with human beings we have the advantage of empathy and introspection, which allow us to infer intentions from behavior.

But, as noted, the scientist cannot simply equate his or her own intentions with the intentions of others. Nor can he or she arrive at their intentions entirely by empathy. So it is necessary to work back and forth among outcomes, actions, and intentions to discover goals people actually have. The procedure runs as follows: The scientist assumes goals for relevant subjects and then predicts choices and outcomes on the basis of assumed goals. If the scientist then observes that predicted actions and outcomes occur, he or she has in fact explained the outcomes and the research is complete. If, however, the scientist fails to observe the predicted outcomes and actions, then he or she must reconsider the attribution of goals. With a new attribution, a new prediction, if observed, completes the research. If, however, prediction fails again, the process must be repeated and repeated, until finally the prediction is correct and the explanation is satisfactory.

Such testing and revision is not possible for behavioral generalizations divorced from theory. The most well-known behavioral generalization in politics is the relation between party identification and vote intention, as self-reported in sample surveys (Campbell, Converse, Miller, & Stokes, 1962). Unfortunately, this generalization is not entirely accurate because some who say they favor a party disclaim an intention to vote for its candidate. Furthermore, as party identification grows weaker, the fit of the subject class into the predicate class worsens. But the behaviorist cannot do much about revising the generalization because the method forecloses examination of voters' intentions. This is an example of why behaviorism is at a dead end.

The method of rational choice is, by contrast, alive and flourishing, because it does admit of revision and testing. I offer one recent example: The prevailing generalization about legislative organization (mainly in Congress) describes the
committee structure as facilitating, as intended, the legislators’ distribution of gains from trade to their interest group clients (Weingast, 1981; 1987). The theory is that legislators desire re-election and seek to achieve it by service to clients. While a large amount of evidence about committee assignments, conference committees, and so on, supports the generalization and the theory, it is nevertheless true that membership on committees is not severely skewed toward legislators whose constituents and clients can benefit greatly from committee decisions (Krehbiel, 1990). Thus the assumed goals do not fit perfectly into the observed organizational outcome. This raises doubts, therefore, about the goals attributed to legislators in their organization of committees. So Krehbiel derived an alternative set of goals from the fact that legislators are deeply uncertain about the probable consequences of potential policies. Does a particular weapons system improve defense? Does an increase in the minimum wage generate more loss of wages by inducing unemployment than gains of wages to those who remain employed? To answer questions like these, legislators need good guesses about what outcomes follow from policies chosen. But only well-informed people can make good guesses. So another goal of legislators in structuring committees is, so Krehbiel argued, to improve the flow of adequate information. The committee system thus serves two kinds of instruments, distribution to interests and information for legislators. Both instruments have the goal of re-election, but one serves the goal by serving minority interests, while the other serves the goal by serving majority interests. Krehbiel, following Benno (1978) calls this latter provision “good public policy,” a usage unfortunately misinterpreted as idealism when in fact it refers to a form of self-interested re-election.

Of course, the incompatibility of some, perhaps jointly held, goals greatly complicates scientific analysis. If Krehbiel is correct about his observations, the fact of majority rule on the floor suggests that in the long run committees are likely to be oriented more toward information than distribution. But then I am sure that the dispute about observation is not over, so it is not yet clear how scientists will resolve this question, except to complicate matters by including both instruments of re-election.

Because the use of the rational choice model to refine scientific sentences is central to this paper, I offer another example: One of the main books in the development of the rational choice model is Olson (1963), in which it is assumed that group members are myopically concerned with maximizing private goods and the collective good produced by the group. In large groups, however, myopic members are expected to be unwilling to pay for the collective good (which, by definition, they can always use), free-riding on the efforts of other group members instead. This, of course, is a formula for group disaster. In Olson’s theory groups solve this problem either by producing private goods of sufficient value to induce membership or by coercing persons to join. With respect to labor unions,
which are large groups in Olson’s terms, private goods are not enough to attract members so unions must use coercion in the form of the union shop contract or the closed shop contract. Olson remarked that right-to-work laws (prohibitions of union or closed shop contracts) would bring about the death of unions. In fact, however, right-to-work laws have only marginally reduced membership. This is a clear counter-example to Olson’s theory. How must the theory be revised to fit this evidence? Johnson (1987) proposed to revise members’ time horizon in calculating benefits. Olson assumed that members compare costs and benefits in a single period. Johnson, however, assumed that members compare costs and benefits discounted over a lifetime of economic periods. Thus discounted, future benefits may outweigh initial and continuing costs. Johnson changed decision-makers from myopic to hypermetropic calculators. There is still some debate about the factual evidence, but I reiterate that the resolution must proceed by the method of rational choice analysis, though the complexities of goals always makes resolution difficult and accounts, I believe, for the fact that political theory is much more difficult than economic theory.

IV. THE ROLE OF UTILITY THEORY IN RATIONAL CHOICE MODELS

So far I have described the rational choice model entirely in terms of the ordinal ordering of goals and the selection of appropriate instruments. Often, however, the model is complicated by the assumption of cardinal orderings. Suppose a pair of alternative actions each serve, in varying degrees, two goals in the individual ordering. To choose between the actions, one must combine their effects on goal 1, and the value of goal 1, with their effects on goal 2 and the value of goal 2. The arithmetic for this combination is the theory of cardinal utility. Given an ordinal ordering of goals, the observer can construct (by a Von Neumann-Morgenstern experiment) the subject’s cardinal distance among goals. That is, the ordering of three goals, a, b, and c, by the relation of preference and indifference, R—for example, aRb, bRc and aRc—can be translated into a cardinal utility number: u(x). Then one can write: \( u(a) \geq u(b) \geq u(c) \), for example \( u(a) = 1 \), \( u(b) = 0.3 \), and \( u(c) = 0 \). In this sense, utility is a measure on preference. Having measured preferences, one can compare two actions in terms of the probability by which each attains the several goals. Thus, if action 1 achieves a with \( p_1(a) \), b with \( p_1(b) \), and c with \( p_1(c) \), and if action 2 achieves a with \( p_2(a) \), b with \( p_2(b) \), and c with \( p_2(c) \), then one can calculate expected utilities:

- expected utility of 1 = \( v_1 = p_1(a)u(a) + p_1(b)u(b) + p_1(c)u(c) \)
- expected utility of 2 = \( v_2 = p_2(a)u(a) + p_2(b)u(b) + p_2(c)u(c) \),

and of course \( v_1 > v_2 \), \( v_2 > v_1 \), and \( v_1 = v_2 \) are all possible.
Expected utility calculations are good enough for prediction and explanation in many real world applications. If actors regularly choose action 1 over action 2 and if \( v_1 > v_2 \), then one can straightforwardly derive both prediction and explanation. (For a good example in political science, see Bueno de Mesquita, 1981, where expected utility calculations turn out to be very good postdictors of decisions to make war.)

Nevertheless, expected utility is an abstraction from the full circumstances of preference and choice and for that reason, may, as I pointed out in Section I, occasionally be inaccurate. So revision is often appropriate and is as true of expected utility theory as of any other. For one such inaccuracy, discovered by Allais (1953), suppose action 1 results in a reward for certain, while action 2 results, with some probability, in zero reward. Then, even if \( v_1 < v_2 \), an actor, who otherwise acts in accord with expected utility theory, may choose \( v_1 \), because he or she prefers the certainty of the smaller reward. By ignoring the comfort of certainty, the expected utility calculation is, in this case, misleading. A similarly confusing abstraction (discovered by Ellsberg, 1961) is that probability numbers may themselves be ambiguous (i.e., \( p_1 \) may unambiguously equal some known value or it may range ambiguously over many values). Analogous to the preference for certainty, some people may prefer risks with unambiguous probabilities (Einhorn & Hogarth, 1986).

These abstractions have been much discussed in recent years. See, for example, Kahneman and Tversky (1979), Tversky and Kahneman (1981), Hogarth and Reder (1987), Cook and Levi (1990), Monroe (1991). But the emphasis in this discussion is on the existence of puzzles and their effect on the validity of axioms. Unfortunately, the significance of these discoveries for scientific research itself has mostly been ignored. Should these puzzles lead us to discard expected utility theory as a tool of investigation? For reasons discussed later, I am inclined to answer negatively. Instead we should, I think, go through the same kind of attribution, testing, and revision on methods of calculation as we go through on the specification of goals.

As an example, consider the puzzle of why people vote. In the initial, expected utility formulation of the question (Downs, 1957), the generalization was that, if \( R \) (the reward from voting) were a positive number, then the potential voter would actually vote. Otherwise not. The calculation of \( R \) is: \( R = pB - C \), where \( B \) is the net benefit from voting (i.e., the voter's gain from the victory of his preferred candidate less the gain from the victory of another candidate), where \( p \) is the chance that the voter be decisive (i.e., break or make a tie), and where \( C \) is the cost of voting. Since \( p \) is a tiny number in most elections, it is almost certain that \( pB < C \), even when \( C \) is very small, for example, one dollar. Inference: most people do not vote. Of course, as the number of voters declines to render \( p \) larger and \( R \) positive, more would vote and there would be a pendulum-like variation around a small number of voters. Of course, this infer-
ential outcome does not accord with experience, and there is no very good way to repair the defect by tinkering with p or B.

One way of resolving the puzzle without abandoning expected utility is to postulate additional goals unrelated to the outcome of the election. Ordeshook and I (1968) added a term, D, to measure the satisfaction obtained from the act of voting itself (e.g., pride in citizenship, satisfaction in taking sides, etc.) and we showed that, as D increased, so did voting, a result that has often been duplicated. Many scientists, however, disliked our emendation on the ground that the additional term violated the principle of consequentialism, namely, that the utility of actions resides in their results. I cannot see any very good reason for insisting on consequentialism, at least in this case. Human beings, and indeed mammals generally, do many things simply for fun, excitement, and self-expression; and voting may very well be one of them. Furthermore, other tests besides Ordeshook’s and mine have shown that a variety of nonconsequential motives are involved in voting (Barzel & Silberberg, 1973; Brunk, 1980; Thompson, 1982). Still, I do grant that, if voting is to be treated as a serious act in the context of democratic theory, our solution of adding a nonconsequential goal may well be disputed.

So Ferejohn and Fiorina (1974) proposed to resolve the puzzle, not by revising goals, but by resolving utility calculations. Instead of maximizing utility, people were, they said, minimizing the maximum regret. (For example, one’s maximum regret might follow from failing to vote and then discovering that by one’s vote one might have prevented the election of one’s least liked candidate. To minimize the maximum regret means, therefore, to choose to vote.) The minimax regret calculation, of course, leads to a different result from the expected utility calculation and thus resolves the puzzle.

An empirical test of the two methods of calculation is possible (Cain, 1978; Thompson, 1982). As Ferejohn and Fiorina point out, strategic voting “is never minimax regret optimal.” The reason is that, when the probability that a first choice wins is less than the probability of a second, the voter’s maximum regret occurs after strategically voting for the second choice when a sincere vote would produce victory for the first choice. Hence strategic voting must be rejected under minimax regret. In surveying the literature on strategic voting, I found (1982) a huge amount of it, especially in three candidate plurality elections by voters whose first choice was the weakest candidate. In the most careful such study, Black (1978, 1980) found that such voters voted strategically or sincerely almost entirely in accord with expected utility calculations. So I conclude that, however puzzling the expected utility calculation may be, the best available evidence indicates that, on the whole, voters actually choose as if they carried out this calculation.

Although there is strong evidence against the assumption that voters choose as if using minimax regret calculations, I do not think it is yet appropriate to
discard this assumption for all decisions. Indeed, I have found one situation in
which elite participants apparently believe that some ordinary voters use mini-
max regret. In a study of the rhetoric of the campaigns for and against the
ratification of the United States Constitution, I found that rhetors on both sides,
overwhelmingly, presented highly negative arguments, castigating their oppo-
nents’ position, not praising their own (Riker, 1991). Clearly they were acting as
if the citizens to be persuaded were highly risk averse, enough so to decide by
minimax regret rather than expected utility. Of course, the fact that rhetors
emphasized negative arguments does not mean that they believed all voters were
minimax regret calculators. It made perfect sense for them to be highly negative
if they believed that only marginal voters so calculated. Indeed, I suspect that
was the case: those who knew what they wanted, probably the great majority of
the voters, were extremely likely to base their votes on expected utility. But self-
assured voters are not the targets of persuasion. Rhetors are interested only in
those whom they have some chance to switch, the marginal voters. Insofar as one
can infer from the action of rhetors what they believed the persuadable voters
were like, there is evidence for minimax regret calculation, or at least evidence
for an implicit belief by rhetors that some other people carried through minimax
regret calculation.

So altogether I think the question of how people calculate choices is open.
This means that, just as scientists must discover intentions by use of the rational
choice model, so also they must discover methods of calculation by the same
devices of attribution, testing, and revision within the same model.

V. HOW THE DEFECTS OF EXPECTED UTILITY
ARE EXAGGERATED

Where Allais and Ellsberg have raised serious questions about expected
utility, the fact that expected utility works quite well in analyzing political actions
suggests that the theoretical criticisms, while important, are not sufficient to give
up expected utility when no other model of calculation has been shown to work
as well in explaining political choices and outcomes. In fact the defects of the
expected utility model have been exaggerated in the following ways:

1. Most of the experiments identifying defects are pencil and paper tests
with minimum rewards for participation. It is hard to believe that sub-
jects regard such tests seriously, especially since they typically have
learned to be skeptical about psychological testing. Some of the exper-
iments identifying defects involve game situations and real bets with
money that subjects have been allowed to win. (I know of no experiment
in which subjects have been required to put up their own money in bets.
Indeed, in most states such an experiment would be illegal. Yet it is in
just such a situation that one would expect serious and careful calculation.) Nevertheless, game experiments are certainly better than paper and pencil tests, though not as good as experiments in which subjects bet their own money.

2. Probability theory is a creation of the last 350 years. Until very recently, only highly specialized academics knew very much about it. Even today most Americans learn a little bit about it in secondary school and a few learn quite a bit more in college. In this circumstance, one can hardly expect ordinary subjects of experiments to grasp the complexities of probability theory, especially when the experimenters are trying to mislead them. In an earlier era, when mathematical training was modest, some brilliant thinkers made elementary mistakes: Hobbes thought he squared the circle and Bernouilli thought that complementary probabilities summed to more than one. Children today often have trouble with dividing by zero. Perhaps when probability theory is as well studied in childhood as division now is, adults will have as little difficulty with probability as they now have with the concept of zero. Certainly adult subjects are now quite naive about probability, which is not surprising when we read in the current press that professors of mathematics err on the Monty Hall problem. That subjects are naive about probability puzzles does not mean, however, that they are also naive about figuring out what is good for them. On subjects that they think about a lot, I suspect that most people turn out to be fairly good at expected utility calculations.

3. None of the numerous experiments confirming Allais’s and Ellsberg’s discoveries have, in my opinion, been conducted in such a way as to convince social scientists of the applicability of the experiments to social science problems. Most of these experiments have used naive subjects. (True Allais and Ellsberg did not, but then they did not do proper experiments either.) Indeed, most psychologists seem to believe that they can arrive at accurate description of human choice only if the subjects are naive. But in decision-making in political and economic votings, the decision-makers are usually experienced and often are professional elites trained to think about the subject matter of the decision. Experimenters ought to, but usually do not, use trained subjects if they want to convince social scientists of the usefulness of the results.

(Tversky and Kahneman, 1981, apparently believing that they were using sophisticated subjects, used physicians for a paper and pencil test of a form of Allais problem dealing with preferences about inoculation. But, of course, their subjects, while sophisticated about inoculation,
were as naive as most people about probability and policy-making. So this ill-designed experiment added nothing to what was already known.

So far as I know only one experimenter educated subjects before testing their choices (Reilly, 1982). The “preference reversal” phenomenon, discovered by Lichtenstein and Slovic (1971) is that some subjects, when choosing between bets P and D, prefer a bet P (with a high probability of winning a small amount) to a bet D (with a low probability of winning a large amount), but then, when assigning a price on these bets, assign a higher price to bets D. (These subjects were, of course, very close to Allais subjects who preferred certain winning when it was available—minimax regret—but who chose by expected utility when all gains were probabilistic. That is, Lichtenstein’s and Slovic’s subjects appeared as minimax regret choosers when betting, but as expected utility calculators when selling.) When Grether and Plott (1979) also found that from one-third to three-eights of the choices in their experiment were inconsistent (i.e., displayed preference reversal), Reilly repeated it after first instructing the subjects in expected utility theory. He reduced the incidence of inconsistency to about 10%, but he did not eliminate it. Reilly’s experiment shows that a lot of the evidence for Minimax regret choices arises from the naivety of the subjects. Perhaps all of it does because Reilly did not also instruct subjects on price theory. Yet the pricing assignments were, implicitly, for competitive markets, where buyers would simply be price takers. The greediness in high prices on D bets might well disappear if the subjects understood price theory also. Since social science involves repeated interactions, often among sophisticates, criticisms of expected utility based almost exclusively on naive subjects are not likely to impress social scientists, especially when just a little bit of education seems to eliminate a lot of the puzzling behavior.

4. The use of naive subjects is not a trivial defect. But it pales to insignificance beside the fact that, in none of the experiments, have experimenters allowed subjects to hear debates by elites on appropriate action. In the real world of political and economic life, disputes by elites form a constant background to decision-making. Through media and conversation, buyers learn daily about prices; sellers constantly monitor others’ prices; most citizens know what the law requires of them and, if they regard requirements as onerous, they read and listen to discussions of them; voters can hardly escape some knowledge and dispute in campaigns; policy-makers never cease to receive advice; etc.

By contrast, in all the experiments I know of, the experimenter has
not allowed a competing experimenter to criticize the set-up of the experiment to the subjects. Scholars like Tversky and Kahneman have indeed proved that they can manipulate a large proportion of subjects with trick questions and clever framing. Indeed the most important result of their experiments may well be their practical proof of heresthetic manipulation. (I coined the word heresthetic, intending it to parallel the word rhetoric, from a Greek root for choosing, selecting, and voting. Rhetoric refers to persuasion and heresthetic refers to the manipulation of the decision situation so that participants decide as the manipulator desires, despite an initial disinclination to do so. Typical kinds of heresthetic are agenda control, framing, introducing new dimensions, etc.) But herestheticians in the real world do not have exclusive access to the persons manipulated, nor do the manipulators have exclusive control of information, nor the exclusive right to formulate issues. As might be supposed, real world heresthetical manipulation is sometimes successful, sometimes not. In a widely discussed instance of attempted manipulation (Pliny, 1963; Farquharson, 1969; Riker, 1986), Pliny the Younger, presiding over the Roman Senate, selected an unconventional voting method that, if followed sincerely, would bring about the outcome he desired. Immediately grasping Pliny’s secret purpose, his opponent directed his supporters to vote sophisticatedly. This produced the outcome they preferred to the one Pliny favored. It was also the outcome that would have occurred if Pliny had used the customary procedure. The moral of this story is that Pliny framed the issue, but his opponent unframed it. Such dueling is almost universal in the real world but unknown in the laboratory.

In my book, The Art of Political Manipulation, 1986, I collected a dozen examples of heresthetic, quite a few of which involved framing. Most of them display similar dueling, and the results are mixed. Senator DePew was able to delay the 17th Amendment for about five years with a killer amendment—but only for five years. Whigs and Republicans successfully added the dimension of slavery to the ordinary economic issues of politics in order to divide the Democratic party. They succeeded, but it took them 40 years—from the Missouri compromise to the Civil War (see also Riker, 1982). Senator Magnuson prevented the shipment of nerve gas back to the United States from Okinawa by transforming a defense issue into an issue of the dignity of the United States Senate, but perhaps he was able to do so because the whole issue lasted only an hour or so and certainly took his opponents by surprise. Indeed, he really had no direct opponent on the Senate floor.

Psychologists studying and elaborating on the Allais and Ellsberg puzzles err by insisting on naivety and by disallowing dispute about
prospective action. This is perhaps understandable because psychologists regularly consider the psyche in isolation. They study individual vision and sensation, individual thought processes, individual decisions. For the abstractions of their science they are perhaps properly insistent on individual-level study. But social scientists study what people do together. And interaction may well produce a difference in choices. If so, then what is learned about isolated individuals may have very little significance for social science or the study of individual choice in a social context. An elegant example of this irrelevance is the discovery by McKelvey and Ordeshook (1985a, 1985b, 1990) that very little information is enough to result in outcomes predicted by spatial models of politics, which are, incidentally, the most widely used form of the rational choice model. While the conventional theory of democracy has assumed that voters are well-informed, many recent discoveries, beginning with the earliest survey research (Berelson, Lazarsfeld, McPhee, 1954) have shown that voters are poorly informed. McKelvey and Ordeshook required each subject to vote in a contested election and then to observe only the winner’s payoff to the individual subject, to vote again in a second election on the basis alone of this knowledge (that is, without any information by the alternative candidate), and to repeat this process about 20 times. This interaction between successful candidates allocating amounts of payoff to voters and voters subsequently choosing between the candidates resulted in some change of winners and ultimately in the convergence of winner’s positions (payoffs to the group) fairly close to that predicted in the spatial model. In short, just a tiny bit of information was enough to modify the choices of voters and candidates until the rational choice emerged. Yet the typical experiments (as those by Tversky and Kahneman) used to justify attacks on the rational choice model do not allow even this tiny bit of interaction to distribute information. So I wonder very much if these experiments have any relevance at all for the study of social science.

5. My strictures against blind application of experiments on naive and isolated subjects to choices of people in social situations are important for yet another reason. In the real world, decision-makers do not make a single inconsequential decision on a paper and pencil test and then, pocketing their fees, depart from the world of the laboratory forever. Instead, real decision-makers choose again and again. The very right to continue to make decisions often depends on the quality of the decisions made. The seller who does not set his price at the intersection of marginal cost and marginal revenue is very likely to bankrupt himself and thus to disappear from the economic scene. The candidate who refuses to
seek the median on a one-dimensional distribution of preferences leaves
the scene in defeat (in the fashion of Goldwater and McGovern). The bill
manager who disdains compromises for coalitions carries neither bills
nor himself forward. At any moment the realm of decision-making is
occupied for the most part by survivors, those who have chosen suc-
cessfully in the past and presumably wish to do so in the future. This
means that those who make mistakes such as those in Allais’s (and
Tversky’s and Kahneman’s) puzzles are gradually excluded from politi-
cal and economic decision-making. Of course, each age cohort brings
new supplies of people with erroneous intuitions, but they get pruned
out, either by learning or by failing to learn.

The conclusion is that while many people may make mistakes in
laboratory puzzles, such people are probably fairly rare in significant
decision-making situations.

6. And this leads me to my final observation about the exaggeration of the
defects of expected utility theory: None of the experiments displaying
inconsistencies in choice portray all subjects as inconsistent. For experi-
menters to recommend the abandonment of expected utility theory when
the experiments themselves show that many people—often well over
half, as in the preference reversal experiments—are indeed expected
utility maximizers is to ignore the evidence that the experimenters have
themselves created. Instead, the properly scientific attitude toward such
results is to look at every case of choosing alternatives as a new problem
determining just what people in fact do when placed in the constraints
of culture and institutions. Just as the scientist must work back and forth
between goals and outcomes in particular situations, so also he or she
must work back and forth between methods of choice and outcomes in
particular situations. In most situations that scientists have examined up
to now, the model of expected utility maximization works quite well,
which fact is itself a reason not to be hasty in abandoning it simply on the
basis of puzzles about axioms. Doubtless there are cases in which it does
not work and some other method of calculations must be hypothesized.
But not many such cases have as yet been identified. The only one I feel
easy about in politics is negative appeal of campaigns, which, as I have
already indicated, probably applies only to a small proportion of the
voters, those who are uninformed and marginal between parties.

VI. THE VARIETY AND COMPLEXITY OF HUMAN GOALS
AND RATIONAL CHOICE THEORY

As I have already pointed out, a profound error of many earlier political
philosophers is that they attributed a single overriding goal for political actors.
The very variety of the goals so attributed is, of course, persuasive evidence of the depth of the error. So I conclude with some observations on the scientific desirability of an open mind about and continuous reinvestigation of actors’ intentions.

One thing rational choice models cannot do is describe human character as a whole. The models are about the relation between goals and outcomes in categories of events. The events do not span whole lifetimes or even, indeed, whole persons. So the goals discovered are situation-specific and there is no reason to infer that they carry over from one category of event to another. Scientists using the model aim at finding circumstances so precise and specific that the scientist can then attribute the same goals to reasonable people placed in these circumstances. Consequently, it makes no sense in terms of rational choice theory to attempt to attribute consistent character traits to actors.

Sometimes critics say that rational choice theory assumes that people are self-regarding in all their actions. Substantively this is completely false, though in one technical sense it is true. The model assumes that people are purposive, though of course, their goals need not be consequentialist. Since there are always goals in the model and since action is always aimed at satisfying them, the model is, in a technical sense, always self-regarding: People do what they want to do.

Professor Jane Mansfield nicely describes this situation by saying that, while all actions are by definition self-interested, some self-interested goals are self-regarding and other self-interested goals are other-regarding. In short, what people want to do varies widely, ranging from private advantage, power over others, or greed for wealth, to the intention of helping others even to the extreme of wishing to sacrifice one’s own life. These goals are not character traits. A so-called economic man is not always concerned with maximizing income—only a tiny class of misers qualify as consistent money maximizers. Rather an economic man in a competitive market is one who, as a buyer, acts as a price-searcher and a price-taker, or who, as a seller, seeks to equate marginal revenue with marginal cost. In other circumstances, say, family life, this economic man is likely to sacrifice self-satisfying pleasures in order to increase pleasures of spouse and children.

Conversely, apart from a few saints, no persons are consistently benevolent—only deceased saints qualify as consistent because surviving saints have not yet given their food to the starving. It is very likely that many species have a genetically determined propensity to extreme altruism (such as sacrifice of the actor’s own life). But this altruism is only displayed in genetically advantageous ways. Thus, honeybees defend the hive with suicidal stings, birds lead predators away from nests, mammals defend offspring (even siblings) to the death, and chimpanzee warfare probably counts as defense of the extended family (Wilson, 1975; Goodall, 1986). But none of these creatures nurture a weakened or aged adult, apparently because there seems no genetic advantage in doing so.

While altruism in these creatures seems limited to close family relations, in
humans benevolence even embraces members of large impersonal organizations. Perhaps the most extraordinary altruism occurs in warfare, as recorded in citations for awards of the Medal of Honor or the Victoria Cross. In civil life altruism means, at the extreme, dangerous rescues, dangerous protection for victims of unjust laws (as in the underground railroad). Civilian heroes may not usually expect to die. But in military engagements, the soldiers recognized for extraordinary valor and heroism know that death is either highly likely or certain, and yet they willingly undertake extremely altruistic action. For the sake of discussion, I attach in the Appendix, three citations chosen from a few adjacent pages of Medals of Honor Recipients, 1863–1978 (1978; Pfc. Caddy, 510; Sgt. Bolden, 499; Sgt. Carr, 513), which I have arranged from the most defensive to the most aggressive. These citations clearly indicate that self-sacrifice is situation-specific and that an adequate description of goals is very difficult:

1. It is sometimes said that extremely self-sacrificing acts are instinctive. And Pfc. Caddy’s almost instantaneous decision does perhaps seem so. Yet the citation tells us that he had taken a leading part in the advance and was, presumably, intently concentrated on the mission. While very few people, even in his circumstance, would smother a grenade with their bodies, he probably saw certain death as a necessary part of the goal on which he had concentrated all day. As for Sgts. Bolden and Carr, their units were, the citation tells us, pinned down by enemy fire. They had, therefore, some time to consider and plan their actions. So in these two cases, their decisions to place themselves at extremely high risk of death may easily have been an expected value calculation based on an overwhelming concern for their mission and their units.

2. In these three cases certainly, the self-sacrificing goal is situation-specific. Pfc. Caddy had no opportunity to display a similar concern in other circumstances. And in civilian life there are seldom opportunities either. Certainly the kind of altruism Sgts. Bolden and Carr displayed could not be tolerated in a peaceful setting and is, in fact, seldom observed. This is simply another indication that this benevolence was rationally calculated and situation-specific.

3. Furthermore, the exact nature of the altruism is hard to interpret in the case of Sgt. Bolden and, especially, of Sgt. Carr. Their actions approach in kind the actions of Viking berserkers, enough so that a strictly behaviorist observer would probably not be able to distinguish them, although a rational choice modeler, duty-bound to consider intentions, can tell them apart fairly easily. The berserkers, as described, for example, in Egil’s Saga, were an exceptionally efficient attack force. The Vikings’ military problem was, with a very small force in small ships, to attack and plunder coastal towns and villages all around the North Atlantic and
the North and Baltic Seas. Their solution was the berserker. Viking ships carried a small squad of berserkers who did not row and thus arrived fresh. With training and experience, they turned on, as they landed, a savage and perhaps trance-like fury. Heedless of their own safety, they tried to kill all defenders who did not flee. It was this quality that enabled the tiny population of Norsemen to conquer the coasts, even down to France.

The citation for Sgt. Carr reads like a berserker attack. But, of course, the situations were different. Sgt. Carr could expect only the satisfaction of eliminating the source of the danger and thus saving his platoon. The berserker could expect, first off, a long revel, then plunder enough to buy land and honor. Being a berserker was a way to get ahead in the world, for not all men were capable of becoming one. The berserker regularly practiced the art of savage fury and ultimately won great rewards if he survived. Sgt. Carr and Sgt. Bolden each obviously felt a high degree of responsibility for their endangered unit, but they had only this one opportunity to display their amazing valor. Because saving their units was so dangerous, neither one asked others to do it, but simply volunteered themselves, with no expectation of ultimate reward except the success of their missions and the safety of their units. In terms of the surrounding circumstances, therefore, Sgts. Carr and Bolden seem extremely altruistic, while berserkers seem to be self-aggrandizing. Nevertheless, we have insufficient data about any of these to be absolutely certain of motivation. Perhaps the sergeants simply didn’t trust others to do the job and perhaps berserkers were truly inspired by ethnic loyalty and pride. Furthermore the sergeants’ motivations seem, from the citations, to be mixed, a sense of responsibility to get the job done and at the same time a sense of self-sacrifice for their units. Truly it is difficult to untangle goals.

We can easily see that, in these three cases of extreme benevolence, the goal is rationally calculated and situation-specific. Our problem, typical of many political situations, is to decide just what the goals were—and in these cases we don’t have enough information for that. Nevertheless, one ought not, I believe, give up on disentangling them. It is, for example, misleading to describe any particular action as entirely altruistic. To do so covers up some relevant self-regarding motives. The loving mother endangering her own life for the sake of the child is nevertheless the carrier of genes that presumably direct such action for the survival of the genetic strain. The revolutionary at the barricades sacrificing himself or herself for social ideals is nevertheless also expecting to benefit in terms of office and honor when the revolution succeeds. Consequently, adequate description of behavior depends on disentangling such goals, determining how each one contributes to the outcome and so on.
But one cannot easily disentangle them when one contrasts rational choice (self-interested) with, supposedly, irrational models (non-self-interested) involving love, duty, honor, piety, etc. With such a sharp dichotomy, goals are always clear and do not need to be, indeed cannot be, disentangled. But when all goals are embodied in the rational choice model, one can then disentangle the components into self-regarding self-interested actions and other-regarding self-interested actions. Then one can determine, for example, the relative effect of other-regarding and self-regarding goals in any particular kind of action or institution.

The goal of social science is (1) to formulate sentences that show actions in a subject class producing results in a predicate class and (2) to show by test and observation that the asserted class inclusion cannot be declared invalid. It is helpful also to embed such a sentence in a theory that shows there is a necessary and sufficient reason for actions to produce the specified results. Reasons for actions make sense, so I believe, only in terms of the actors who act. It is true that some have tried to give reasons for actions in terms of zeitgeist or culture (Thompson, Ellis, Wildavsky, 1990). But these forces, if they exist, can only operate through actors, so it remains true that the reasons for action must be found in the decisions of actors. Reasons explain why actors decide the way they do.

So far as I know, only rational choice theory allows for such explanations. It allows the theorist to give reasons for decisions in terms of actors’ goals. Some writers seek to go beyond such mundane explanation to something supposedly deeper, to an explanation of why actors have the goals they have. That way lies infinite regress, for it then is possible to explain actors’ goals in terms of a higher order of goals. Elster (1989, 1990) suggests that perhaps “social norms” can provide alternative explanations. But social norms are themselves creations of actors for some purpose. So the process of explanation would then be to explain actors’ goals in adopting the norms to guide their decisions—an unnecessary convolution that complicates but does not eliminate the rational choice model. So I conclude that social theory must have a psychological base and the only available base is the rational choice model.

APPENDIX

Caddy, William Robert

Rank and organization: Private First Class, U.S. Marine Corps Reserve. Born: 8 August 1925, Quincy, Mass. Accredited to: Massachusetts. Citation: For conspicuous gallantry and intrepidity at the risk of his life above and beyond the call of duty while serving as a rifleman with Company I, 3d Battalion, 26th Marines, 5th Marine Division, in action against enemy Japanese forces during
the seizure of Iwo Jima in the Volcano Islands, 3 March 1945. Consistently aggressive, Pfc. Caddy boldly defied shattering Japanese machinegun and small-arms fire to move forward with his platoon leader and another marine during the determined advance of his company through an isolated sector and, gaining the comparative safety of a shell hole, took temporary cover with his comrades. Immediately pinned down by deadly sniper fire from a well-concealed position, he made several unsuccessful attempts to again move forward and then, joined by his platoon leader, engaged the enemy in a fierce exchange of handgrenades until a Japanese grenade fell beyond reach in the shell hole. Fearlessly disregarding all personal danger, Pfc. Caddy instantly dived on the deadly missile, absorbing the exploding charge in his own body and protecting the others from serious injury. Stouthearted and indomitable, he unhesitatingly yielded his own life that his fellow marines might carry on the relentless battle against a fanatic enemy. His dauntless courage and valiant spirit of self-sacrifice in the face of certain death reflect the highest credit upon Pfc. Caddy and upon the U.S. Naval Service. He gallantly gave his life for his comrades.

Bolden, Paul L.

Rank and organization: Staff Sergeant, U.S. Army, Company I, 120th Infantry, 30th Infantry Division. Place and date: Petit-Coo, Belgium, 23 December 1944. Entered service at: Madison, Ala. Birth: Hobbes Island, Iowa. G.O. No.: 73, 30 August 1945. Citation: He voluntarily attacked a formidable enemy strongpoint in Petit-Coo, Belgium, on 23 December, 1944, when his company was pinned down by extremely heavy automatic and small-arms fire coming from a house 200 yards to the front. Mortar and tank artillery shells pounded the unit, when S/Sgt. Bolden and a comrade, on their own initiative, moved forward into a hail of bullets to eliminate the ever-increasing fire from the German position. Crawling ahead to close with what they knew was a powerfully armed, vastly superior force, the pair reached the house and took up assault positions, S/Sgt. Bolden under a window, his comrade across the street where he could deliver covering fire. In rapid succession, S/Sgt. Bolden hurled a fragmentation grenade and a white phosphorous grenade into the building; and then, fully realizing that he faced tremendous odds, rushed to the door, threw it open and fired into 35 SS troopers who were trying to reorganize themselves after the havoc wrought by the grenades. Twenty Germans died under the fire of his submachinegun before he was struck in the shoulder, chest, and stomach by part of a burst which killed his comrade across the street. He withdrew from the house, waiting for the surviving Germans to come out and surrender. When none appeared in the doorway, he summoned his ebbing strength, overcame the extreme pain he suffered and boldly walked back into the house, firing as he went. He had killed the remaining 15 enemy soldiers when his ammunition ran out.
S/Sgt. Bolden’s heroic advance against great odds, his fearless assault, and his magnificent display of courage in reentering the building where he had been severely wounded cleared the path for his company and insured the success of its mission.

Carr, Chris (name legally changed from Christos H. Karaberis, under which name the medal was awarded)

Rank and organization: Sergeant, U.S. Army, Company L, 337th Infantry, 85th Infantry Division. Place and date: Near Guignola, Italy, 1–2 October 1944. Entered service at: Manchester, N.H. Birth: Manchester, N.H. G.O. No.,: 97, 1 November 1945. Citation: Leading a squad of Company L, he gallantly cleared the way for his company’s approach along a ridge toward its objective, the Casoni di Remagna. When his platoon was pinned down by heavy fire from enemy mortars, machineguns, machine pistols, and rifles, he climbed in advance of his squad on a maneuver around the left flank to locate and eliminate the enemy gun positions. Undeterred by deadly fire that ricocheted off the barren rocky hillside, he crept to the rear of the first machinegun and charged, firing his submachinegun. In this surprise attack he captured 8 prisoners and turned them over to his squad before striking out alone for a second machinegun. Discovered in his advance and subjected to direct fire from the hostile weapon, he leaped to his feet and ran forward, weaving and crouching, pouring automatic fire into the emplacement that killed 4 of its defenders and forced the surrender of a lone survivor. He again moved forward through heavy fire to attack a third machinegun. When close to the emplacement, he closed with a nerve-shattering shout and burst of fire. Paralyzed by his whirlwind attack, all 4 gunners immediately surrendered. Once more advancing aggressively in the face of a thoroughly alerted enemy, he approached a point of high ground occupied by 2 machineguns which were firing on his company on the slope below. Charging the first of these weapons, he killed 4 of the crew and captured 3 more. The 6 defenders of the adjacent position, cowed by the savagery of his assault, immediately gave up. By his 1-man attack, heroically and voluntarily undertaken in the face of tremendous risks, Sgt. Karaberis captured 5 enemy machinegun positions, killed 8 Germans, took 22 prisoners, cleared the ridge leading to his company’s objective, and drove a deep wedge into the enemy line, making it possible for his battalion to occupy important, commanding ground.

REFERENCES


The Political Psychology of Rational Choice Theory


