The Contribution of Behavioral Economics to Political Science

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Abstract

Behavioral economics has become an important part of the economics profession. As a subfield, it tries to make sense of persistent violations of the standard model for economics. The major classes of violations involve social preferences (taking the well-being of others into account), time discounting (inconsistencies in valuing present and future commodities), and context (the effects of framing). Other violations involve well-known psychological heuristics such as overconfidence, constraints on strategic reasoning, emotions, and status differentials. These concepts are discussed in separate sections, and key experimental and empirical studies are noted.

INTRODUCTION

My long-time coauthor Catherine Eckel (who is an economist) tells me that "behavioral economics" is all about bringing humans back into economics. By humans she means attributes of people that are due to human genetic make-up, are embedded in our psychology, and are part of the long-term socialization processes. Betsy Hoffman (an economist who is a mutual friend), in a like-mannered way, characterized game theory as a wonderful theory of autism in which individuals think deeply and strategically about how they would play perfectly foresighted rational actors like themselves. But this means game theory is an analytic model of computationally introspective individuals who give little thought to the social world. Elinor Ostrom, in her Presidential Address to the American Political Science Association in 1997, cautioned the profession about elegant models that failed to make use of new findings in genetics and neuroscience, which pointed to systematic human biases leading to out-of-equilibrium predictions. She called on the profession to use such findings to better inform our theoretical models (Ostrom 1998). All these women make the case that to understand social behavior we need to account for social beings.

Behavioral economics has made enormous inroads into economics over the past two decades. It has established itself in the profession and has evolved a great deal since Camerer's (1997) call for action. Today there are popular books on the topic (Akerlof & Shiller 2009, Thaler & Sunstein 2009), businesses consult behavioral (and neuro-) economists to determine product placement, the Obama administration has appointed one well-known advocate (Cass Sunstein) as Administrator of the White House Office of Information and Regulatory Affairs, and the journals are filled with research under titles containing "behavioral economics."

Twenty years ago it would have been possible to thoroughly survey this growing field. Ten years ago the field was beginning to boom and a thorough survey would have been difficult. Today the field is enormous and I make no claims at giving it full treatment. Instead I focus on several topics that in which behavioral economics can claim successes—topics for which there have been breakthroughs and that have spawned new research areas. At the same time, I have selected the topics that will most resonate with areas of interest for political scientists. What I do not want to do is leave the impression that standard rational choice or game theoretic models have been supplanted. They have not. Such approaches to modeling strategic behavior remain valuable and provide considerable insight. As Ostrom (1998) notes, these models perform well when there are many actors (e.g., markets, elections), and where they begin to break down is when aspects of human behavior are not accounted for by the simplifying assumptions of our models. This means we need a different modeling strategy, not a rejection of modeling itself.

What properly constitutes behavioral economics is disputed. As in any growing field, divisions appear and seemingly fine distinctions are made. Khalil (2009), in his introduction to a three-volume collection of classic papers on behavioral economics, differentiates between those who are tinkering with the "objective function" (making assumptions about preferences) and those who are inserting heuristics into models. The former tackles the question of preferences directly. The latter harkens back to an older tradition of bounded rationality in which actors face cognitive costs that can be handled in much the same way as moving a budget constraint for an actor with fixed utility functions. In this sense, the division is between those who are trying to fundamentally revise the standard toolbox and those who are simply importing psychology into economics.

For those wanting to delve deeper into the topic, *Advances in Behavioral Economics* (Camerer et al. 2003) is a good place to start. This collection of 18 articles, originally published elsewhere, sketches the landscape of behavioral economics. In conjunction with the book, it is instructive to read its reviews, as they detail the concerns held by the profession (see

particularly Harrison 2005, Fudenberg 2006, and Pesendorfer 2006). For those wanting to push even further, there is the three-volume collection of articles by Khalil (2009). There are also numerous overviews of the literature that are geared for economists (see for example Camerer 1997 or DellaVigna 2009).

Generally, three major topics catch the attention of behavioral economists: social preferences, time discounting, and context. These are at the core of understanding preferences, and in this review I spend some time on each. A fourth topic pertains to psychological heuristics and has also been prominent in behavioral economics. Much of what has driven research on all of these topics comes from the experimental economics tradition, and this is what I best know. Throughout my discussion, I point to classic experiments that drove subsequent work, and I provide examples of new research that is carrying forward the work.

SOCIAL PREFERENCES

A rich area of research has focused on "other-regarding preferences." This work in economics has made inroads into political science, so I spend a great deal of this article discussing it. Standard game theoretic models start with axioms long ago presented by Luce & Raiffa (1957) (among others) stating that interpersonal comparisons are irrelevant in strategic choice. This seemed reasonable in that actors should care about their own utility and not that of their opponent in a game. This made most problems tractable. Yet a set of canonical experiments showed that subjects pay attention to something other than their own earnings, and this presented an increasingly interesting puzzle for theorists. I discuss four canonical games: the ultimatum game, the dictator game, the trust game, and the public goods game. (For excellent surveys of these canonical games, see the review articles in Kagel & Roth 1995, Camerer 2003, and a forthcoming second edition of Kagel & Roth 1995.) In this article, I point out several basic findings and what these meant for other-regarding preferences. I then return to a variety of models that have been proposed to add consideration of other people into standard utility theory.

The Ultimatum Game

The ultimatum game is a very simple two-person bargaining game. Player A is given an amount of money to split with player B. Player A announces the split, and B can accept or reject the proposed division. If accepted, the division is implemented and both parties go on their way. If rejected, both parties get nothing. The division is proposed only once (an ultimatum) and both parties know this. The theory is straightforward. In equilibrium, A will offer the smallest possible amount. B will accept whatever is offered, since any positive amount is preferred to zero. No positive amount proposed by A should ever be rejected.

In the first experiment to test this concept, Guth et al. (1982) found two peculiar things. First, almost all subjects gave more than the minimum—indeed, in the first experiment, the modal offer was 50% of the endowment. Second, nonzero offers were rejected. In a second experiment, subjects played the roles of both the proposer (A) and the responder (B). Subjects were first asked what they would propose and then they were asked what they would minimally accept. Again subjects proposed much more than the minimum (on average 45% of the endowment). Once again responders were willing to reject nonzero offers, with the median amount they were willing to accept being almost 36% of the endowment. Thaler (1988), in one of a number of influential articles that appeared under "Anomalies" in the *Journal of Economic Perspectives*, brought the game into the mainstream. The results were not warmly received. Economists and theorists suggested that the stakes were trivial, subjects did not understand the problem, the instructions were misleading, and the student sample was hardly of relevance. However, these results generated an enormous number of experiments designed to pick apart various explanations for the behavior. Some interpreted these results as

indicating that proposers were concerned with what the responder might do. In particular, was the proposer interested in a fair division or afraid of being rejected? Was the responder concerned with fairness or concerned with being insulted?

Since this time, hundreds of experiments have tried to understand behavior in the ultimatum game. Blount (1995) demonstrated that the intentions of the proposer matter. In her experiment, subjects were paired with proposers who were either humans or a computer (and were told which). She showed that low offers from humans were rejected, but not low offers from a computer. Cameron (1999) looked at the effect of stakes and showed that even very large stakes (up to three times an individual's monthly income) did not markedly change proposer behavior, although responders were more likely to accept offers. Eckel & Grossman (2001) found that the gender composition of pairings makes a difference, with males more likely to accept lower offers from women (chivalry) and women more likely to accept lower offers from women (solidarity). Roth et al. (1991) pointed to cross-cultural differences among students, and Henrich et al. (2001) found cross-cultural variation among people in small, primitive societies. Bahry & Wilson (2006) showed that strong norms can persist in societies, with offers above 50% being rejected. Sanfey et al. (2003) demonstrated neurological correlates with behavior in the ultimatum game, with additional support by Koenigs & Tranel (2007), who used the game on subjects with lesion damage to the ventromedial prefrontal cortex. By and large, these ultimatum game results, in which unexpectedly large offers are made and low offers are rejected, persist.

Political scientists have not ignored the ultimatum game. Hibbing & Alford (2004) have used it to demonstrate that individuals are willing to cooperate when they feel that they are not likely to be "taken for a sucker." If the process awarding the proposer an advantage is considered fair, low offers are accepted. If the proposer's intention is clear (and considered fair) then low offers are also accepted. Smith (2006) used the same idea of "wary cooperators" and looked at proposers who were put into a position where they could act for themselves or on behalf of others (mimicking a version of representation). He found that there is a propensity to act on behalf of others in the ultimatum game. Bahry & Wilson (2004) used the ultimatum game to measure the degree to which norms are followed in a transitional society. Even though younger Russians employed very different strategies than their older counterparts, the young conform to a well-known norm of sharing equally when it comes to the ultimatum game.

Generally the ultimatum game is a useful, well-established tool to understand bilateral bargaining. Political scientists have made considerable use of the Baron & Ferejohn (1989) legislative bargaining model, which is nothing more than a repeated shrinking-pie version of the one-period ultimatum game. Adding institutional complexity to the stark bargaining arena of the ultimatum game is a fruitful avenue for future work. Varying who is assigned to each role (and how) could provide insights into institutional legitimacy. Manipulating the pairings of players could shed light on implicit attitudes toward racial and ethnic others. There are numerous possibilities for using this well-studied game.

The Dictator Game

A second canonical experiment is the dictator game, designed to test fairness. It is similar to the ultimatum game, except that the second player has no move at the second node of the game. The proposer, A, is given an endowment and then allowed to split that endowment with the recipient, B. Player B has no choice and must accept whatever A sends. In this sense A is a dictator; B has no action. All play is anonymous, thereby wiping out fear of postgame retaliation. The equilibrium for this setting is obvious: A should keep everything and send nothing to B. If something is sent, this is thought to be due to some sense of fairness or an act of altruism on the part of A.

The first such experiments were run by Kahneman et al. (1986) to explicitly test underlying assumptions in economics in light of findings under the ultimatum game. The results were straightforward in that almost three quarters of the subjects gave something to the second player. Forsythe et al. (1994) then tested the same game under a variety of conditions and found that it was impossible to move all subjects to keeping all of their endowment.

Subsequent researchers were puzzled by the results and pressed on several fronts. On the one hand, there was skepticism about A's actions. Did A believe that there would be no postexperiment interaction? Did A believe that the choices were completely anonymous? Hoffman et al. (1994) ran a series of experiments that sought to strip away the fears that A's actions could be identified. Their experiment implemented a strong single-blind design and then moved to a double-blind design in which no one, including the experimenter, could possibly infer what A kept. These design changes were intended to increase the "social distance" between A and everyone else. As A's actions were increasingly masked from B's, the splits decreased. By contrast, Eckel & Grossman (1996) made the recipient a charity—increasing the social distance even more—and yet found considerable altruistic behavior.

A recent meta-study of 129 papers that have used the dictator game finds that dictators give about 28% of their endowment (Engle 2010). In looking at the covariates that change altruistic behavior, Engle notes a number of experimental parameters that affect what is sent in the dictator game. Old age, having more than a single recipient, and making the recipient "deserving" are all predictors of positive amounts sent to the recipient. Increasing social distance, making decisions in groups, repeating the game, moving to double-blind procedures, using student or child populations, giving the recipient an endowment, and having the dictator earn the endowment are all predictors of decreased amounts sent to the recipient. A number of covariates make no difference, including the type of incentive, the social cues that are provided, whether there is an option to take money from the other player, and whether subjects are from a developing country or a "primitive" culture. In short, it remains the case that there are positive contributions in the dictator game.

Political scientists have used the dictator game in a variety of ways. Whitt & Wilson (2007a) used the experiment as a measurement tool to understand discrimination in postwar Bosnia. They found that most people adopt a simple heuristic that divides the amount when paired with an individual from the outgroup. Those who hold extreme nationalist views were more willing to decrease what they send to the outgroup. Fowler (2006), using the dictator game, found that strong partisans are more altruistic and thereby more likely to bear the cost of voting. Fowler & Kam (2007), also using the dictator game, uncovered a correlation between sending more and participating in politics and other forms of social behavior. They regard this as evidence that social preferences matter for political behavior.

Although a large number of variations of the dictator game have been looked into, there remains plenty of scope for political scientists to use the same game. The core of the dictator game involves taking a costly action on behalf of someone else. The dictator game may be useful for understanding a part of the representative relationship. For example, under what circumstances will a representative be willing to sacrifice her own interests on behalf of others? Or under what circumstances might a representative be willing to sacrifice local interests for national interests? The dictator game may be valuable when thinking of terrorists willing to sacrifice their own lives on behalf of others. It may give insight into which institutions foster altruistic acts. In short, a number of questions that are of interest to political scientists have not been explored and could easily use this simple experimental game.

The Trust Game

A third canonical experiment, based on the "investment game" developed by Berg et al.

(1995), is commonly known as the trust game. In this experiment there are again two players. Both are given an equivalent endowment, and player A has the first move. Player A can take any portion of the endowment and send it to player B. Whatever is sent is tripled in value by the experimenter and given to B. Player B then decides how to split the tripled amount plus the endowment. Like the ultimatum game and the dictator game, this is a one-shot game in which, once the decision is completed, the players walk away with their earnings. Under backward induction, the equilibrium is obvious. Player B will keep her endowment and whatever was sent (and tripled). Player A, anticipating this, will refuse to send anything at the first move. If A trusts that B will be trustworthy, then both parties can be made better off.

The experiment was designed by Berg et al. (1995), and they find that few individuals play the Nash equilibrium under backward induction. Johnson & Mislin (2008), in a meta-analysis of the trust game, show that, on average, trusters send 50.8% of their endowment (based on 84 experiments). Trust pays (barely), in that 36.5% of what is sent is returned (based on 75 experiments). Contrary to game theoretic expectations, trust is widespread and it is reciprocated.

The general findings of the trust game and its many variations are dealt with by Wilson & Eckel (2011). Their findings are consistent with the point that individuals are generally trusting and generally trustworthy. Yet a number of studies point to human traits that are related to trust and are unaccounted for by standard game theoretic models. For example, Kosfeld et al. (2005) show that administering the hormone oxytocin enhances trust (but not trustworthiness). The behavior is a response to a biological reaction. Eckel & Wilson (2008) show that skin shade has an independent effect on trusting decisions. When subjects can view their partner, they send less to darker-skinned partners. In a similar vein, more attractive partners are trusted more, but more attractive trusters are penalized by trustees (Wilson & Eckel 2006). The roots of these two behaviors have to do with stereotyping and expectations. As with the other canonical experiments discussed above, there has been considerable research on the trust game that moves beyond a narrow sample of college students. Much of that work deals with population samples cross culturally (Bahry & Wilson 2004, Naef et al. 2009) or with convenience samples of adults in a variety of countries and settings (Carpenter et al. 2004, Cronk 2007, Karlan 2005). In short, results from the trust game do not go away.

The concept of trust is central for political science. Many claim that trust is critical for both the formation of social capital and for institutional legitimacy. Others claim that trust among citizens comes from stable institutions, in effect proposing the opposite causal direction (for a discussion of this point, see Wilson & Eckel 2011). The investment game provides a laboratory model for settling issues of causality. As well, issues of trust go directly to the heart of the representative relationship in democracies. In a like vein, given that it is impossible to write all possible contingencies into a contract or write legislation that covers all possibilities, principal–agent relationships are grounded in trust. A good deal of research has focused on the conditions under which a principal's trust can be maintained. Yet few have exploited the investment game to explore the conditions under which this occurs. It might seem that the design of institutions is a natural place to start, but a few studies point to a perverse finding that institutions crowd out trust (see Wilson & Eckel 2011). In a very different domain, trust is important in bargaining settings in which actors cannot credibly commit—a hallmark of many agreements studied in international relations. In short, the issues involved with trust are important for political scientists, and the findings from the investment game ought to be informative.

The Public Goods Game

The final canonical experiment is widely known in the social sciences as the public goods game, a social dilemma, or the voluntary contribution

mechanism (in its repeated form). Often this experiment is used to test for social cooperation. In effect, this experiment is an *n*-person version of a prisoners' dilemma. The usual design has more than two actors (ranging anywhere from four to several hundred) who make a simultaneous decision. Each actor is given an endowment and chooses to allocate it between a private pool and a group pool. The private pool is similar to a private investment that returns at a rate of 1–1 and is returned directly to the individual. By contrast, whatever is put into the group pool is multiplied by the experimenter and then divided equally among all members of the group. So long as the marginal per capita rate of return (MPCR) is less than 1, then individuals have an incentive to "free ride" and contribute nothing. The dilemma is that everyone is better off contributing everything if the MPCR is sufficiently large, as that ensures a socially optimal outcome. Everyone makes more than if everyone invests in the private pool. Yet any individual always makes even more by investing in the private pool and free riding off the efforts of others. Under backward induction, the Nash equilibrium in a one-shot game is for everyone to invest in the private pool.

Early experiments suggested that subjects did not play Nash (Dawes et al. 1977, Marwell & Ames 1979). Economists doubted these results because the stakes seemed low (or in some cases nonexistent). Isaac et al. (1984) ran what has become the canonical voluntary contribution mechanism (VCM), in which the public goods game was repeated a number of times. Under highly controlled and paid conditions, they found much the same. Subjects did not play Nash in the first period (average contributions ranged between 40% and 60%), but over time, contributions to the public good went nearly to zero. Subsequent experiments focused on the resilience of these results to group size and changes in MPCR (Isaac & Walker 1988), communications (Sally 1995, Sell & Wilson 1991, Wilson & Sell 1997), restart effects (Andreoni 1988), punishment (Fehr & Gächter 2000), and switching to a common pool resource (Ostrom et al. 1994). Generally the findings do not support the Nash prediction.

Two influential surveys detailed the findings from this literature. Ledyard (1995) elaborated the research up to 1995 for the *Handbook of Experimental Economics* (a second edition of this influential book is forthcoming). Ledyard noted many of the persistent anomalies and suggested routes for future research. Ostrom (2000) took stock of the main findings from this literature, noting seven regularities. First, subjects contribute 40%–60% of their endowments to the public good in the first round of play; second, after that round, contribution levels decay and there is considerable free riding in the final period of play; third, beliefs about the positive cooperation of others increase one's own cooperation; fourth, gaining experience in the experiment (learning) results in more, not less, cooperation; fifth, face-to-face communication and other forms of "cheap talk" enhance levels of cooperation; sixth, when allowed, subjects will engage in costly punishment to sanction those who are not contributing; and seventh, the VCM is very sensitive to institutional features that define it (e.g., the communication or punishment structures).

The public goods game is well known to political scientists. It has its foundations in Olson's (1965) book *The Logic of Collective Action*. That work was central for those concerned with interest groups and political mobilization. The free rider problem spurred a good deal of rational choice theory and made the case that political institutions were necessary to overcome problems of collective action. Yet early experimental work by political scientists called into question the inevitability of free riding. Orbell et al. (1984) and Dawes et al. (1986) pointed to the power of group identity in building social cooperation. In later work, Ostrom et al. (1994) introduced the common pool resource version of a public goods game. In doing so, they noted that Hobbesian solutions were not inevitable for overcoming free riding and that groups could endogenously design their own institutions, were skilled in communications, and could deftly use punishment. More

recently, Levine & Palfrey (2007) have used a variant of the public goods game to tackle the question of why people turn out to vote when voting is costly and free riding is cheap. Holding beliefs about being pivotal is crucial, even if those beliefs are off the mark. Habyarimana et al. (2009) effectively use the game to test why ethnic heterogeneity leads to decreased provisioning of public goods. It appears that the networks developed by coethnics are important for ensuring monitoring and thereby ensuring the provisioning of public goods. These are only a few of the many studies by political scientists on the topic.

Theoretical Innovations

Although these findings were at first ignored by the theoretical community, the robustness of the experimental results has motivated new theoretical development. An early model by Rabin (1993) proposed that individuals might value fairness in others. For example, those who behave kindly may be rewarded with kindness and those behaving unkindly might be punished. In effect, this model was concerned with the intentions of the other actor. The importance of intention was driven home by Blount's (1995) experiment on the ultimatum game, in which subjects responded very differently when the proposer was a human versus a computer. Rather than kindness, Levine (1998) posed a model of spitefulness. Again the agent is aware of the actions and intentions of another and responds in a manner that harms the agent as well as the other. Andreoni (1990) and Andreoni & Miller (2002) turned to whether subjects have a utility function that is increasing with the payoffs to others.

In the late 1990s, two key models focused on the distribution of outcomes, incorporating intentions, and relying on fairness and reciprocity. Fehr & Schmidt (1999) proposed a utility function in which an agent cares about not only her own payoffs but also the payoffs of her counterpart. The utility function is kinked in the sense that an agent cares about her own payoffs and cares about the other's payoffs relative to her own. When the counterpart gets less than the agent, the agent may suffer and give up some utility to make an adjustment—hence giving money to another in the dictator game. However, if the agent gets less than the counterpart, this comparison may result in negative utility—for example, leading to rejections of positive amounts in the ultimatum game. Bolton & Ockenfels (2000) proposed a model of utility that is quite similar. In both cases, individuals compare their own payoffs with those of their counterpart and respond to inequalities. Generally this approach has been noted as inequity aversion. The two models differ in their functional forms (Fehr & Schmidt take a linear form and Bolton & Ockenfels use a nonlinear form) and in their basis of social comparison (Fehr & Schmidt use a comparison of the agent with every other counterpart in the game, whereas Bolton & Ockenfels use a comparison of the proportion of the payoff based on the average of all other payoffs). The models lead to differences in predictions depending on the number of players in the game and the extent to which there are inequalities in outcomes. These models take the experimental data seriously and have pushed researchers very hard to refine their experiments to test the robustness of these models.

There have been dozens of experiments trying to figure out what type of other-regarding utility function holds up. For every competitor, new experiments are designed to test (and break) a proposed model of social preferences. The classic article by Charness & Rabin (2002) began the task of testing different models of social preferences. For recent variations on models of social preferences, see Dufwenberg & Kirchsteiger (2004) and Engelmann & Strobel (2004). A useful starting point is the recent issue of the *Journal of Economic Behavior and Organization* (2010, 73:1) which is partly devoted to a discussion of the Fehr & Schmidt (1999) model of inequity aversion.

Moving away from standard rational choice models may provide useful insights for political scientists. The idea that actors in some settings may impute motives to others, may be spiteful,

or may worry about relative status is not far fetched. Whereas many of our models assume that political leaders carefully weigh all possible strategies before committing to conflict, anecdotal evidence points to leaders who are driven by hatred or disdain for their counterpart. Knowing when to incorporate other-regarding preferences into our models will expand our explanations. Building in such preferences may also help unlock who gets which ministries under coalition bargaining. It may not be just a matter of seat strength and ideological proximity—the shared friendships or enduring personal animosities may affect the ways in which deals are reached. Social preferences may be useful when explaining why voters may vote for candidates who espouse bold ideas of fairness, even though such policies run contrary to self interest. Incorporating social preferences may also help explain why politicians sometimes take principled stands that may cost them re-election. And a theory of social preferences may be useful for understanding the darker side of human cooperation. For example, what propels individuals to mobilize to engage in ethnic cleansing? What drives individuals to kill themselves on behalf of the group? These are costly actions that require coordination and cooperation.

None of these issues can be understood without accounting for strategic behaviors between actors. The current toolbox of game theoretic models can get to the core issues that we think cause the phenomenon. My chief concern is whether incorporating social preferences makes a sufficient difference for the explanation to be worthwhile. My sense is that building in other-regarding preferences will be more important when actors can easily draw inferences about their counterparts when considering strategic choices. Such preferences will be less important in large-scale competitive settings in which it is difficult to draw inferences about others. I note that my strategic considerations are quite different when voting in my academic department than when voting in a national election.

TIME DISCOUNTING

A second major concern for economists is how people treat current versus long-term gains. This is a central question for understanding savings and investments. It would be a simple matter if people valued current assets in exactly the same way they value future assets. For example, if having $100 today is equivalent to having $100 in six years, then for purposes of analysis I could treat current and future value as equivalent. Of course, if true for all people, then this would be a remarkably different world, in which interest and savings would be irrelevant. Most people (and all models) assume that present value is different from future value. If I am offered $100 now, how much would it take to get me to forego that amount now and instead accept some amount a year in the future? I surely would not agree to $99 a year from now. How about $101? (At current interest rates in my savings account, this might be a good deal.) How about $110? The point at which I am willing to accept a future payoff in place of a present payoff defines the extent to which I discount the future. To a psychologist it may also say something about my patience.

If humans are hardwired in similar ways, then we should display similar forms of discounting, and the standard way of representing it is through an exponential rate of discounting. Suppose that one wanted to capture the utility for a bundle of consumed goods over time (some vector of x's). A convenient way of doing so is to represent this as

$$U^t(x_t, \mathsf{K}, x_T) = \sum_{k=0}^{T-t} D(k)u(x_{t+k}),$$

where $D(k) = \left(\frac{1}{1+r}\right)^k$.

In this representation, one's utility at some period can be calculated, where *D(k)* is the discount function and r is an individual's discount rate. The discount rate affects the degree to which an individual values the future. Note that this model could easily allow for heterogeneity by allowing r to vary for each individual. Even so, everyone discounts exponentially.

The chief problem with this approach is that psychologists and economists have documented many instances in which people make inconsistent choices. The nature of this inconsistency is expressed in the following way. Suppose I have a choice between $100 today and $105 tomorrow, and I prefer the $100 today. Next I am given the choice between $100 in six months and $105 in six months plus a day, and I prefer the $105. This constitutes a preference reversal that is inconsistent with the standard form of exponential discounting. I am not the only one guilty of such a reversal; this result has been replicated many times (for a general survey, see Frederick et al. 2002).

Brown et al. (2009) look at consumption patterns in the laboratory using a mechanism in which subjects live multiple lifetimes, earn period-by-period payments, and can experience "income shocks." This means subjects not only have to save for the future (it is possible to run out of cash before ending a lifetime), but subjects also get to learn from lifetime to lifetime. (In a different treatment, they were able to learn from one another—but for the present discussion, I set aside that aspect of the experiment.) The basic result, as many know from experience, is that most subjects overspend early on and do not save enough. Brown et al. (2009) include a second study, identical to the first except that the reward medium is a beverage rather than money. Subjects were told not to drink anything for four hours before coming into the lab. Upon arriving, they were given salty snacks (but no liquids), and they received 45 minutes of instructions before starting the experiment. Subjects earned points, rather than dollars, and points were converted to liquid. Care was taken so that subjects did not become satiated. Again, subjects overspent in the early periods and failed to save. These findings (and those by many others) point to problems with standard models of discounting.

As Frederick et al. (2002) note in their survey, there are numerous ways to interpret this anomalous behavior. My own example above might lend support to a view of "hyperbolic" discounting (more on this below). A different possibility is that many anomalies are due to "magnitude effects" in which individuals discount small sums differently from large sums. I might treat amounts in the hundred-dollar range differently from amounts in the tens-of-thousands-of-dollars range, being more patient for the latter than the former. Alternatively, it could be that people exhibit greater discounting to avoid delay than to expedite receipt of a good. For example, the amount I would pay to receive an iPad now rather than six months in the future would be much less than what I would accept if I were offered an iPad now but had to delay receiving it for six months. Frederick et al. (2002) illustrate a number of other variations of these anomalies.

Like the literature on prosocial behavior, these anomalous results on time discounting have given rise to a number of proposed theoretical models. The most popular of these models suggests hyperbolic discounting. Laibson (1997) develops what has become known as a beta-delta model designed to account for preference reversals in discounting. Taking the discount function noted above, discounting now has two parameters:

$$D(k) = \begin{cases} 1 & if\ k = 0 \\ \beta \left(\dfrac{1}{1+r} \right)^{k} & f\ k > 0 \end{cases}.$$

In essence, there is increased discounting between the current period and the next, with a constant rate of discounting thereafter. In line with the experiment by Brown et al. (2009), this captures a phenomenon of too little savings in the earliest periods. Functional magnetic resonance imaging (fMRI) studies suggest different regions of the brain are used to assess short-term and long-term decisions (McClure et al. 2004), and proponents of the beta-delta model point to this for support.

A second approach, a "dual-self" model, has been proposed by Fudenberg & Levine (2006). They imagine two selves involved in some multistage game. The "short-run" self is myopically interested in each single stage of the game. The "long-run" self shares the same stage-game preferences as the short-run self but chooses

some commitment device to influence the myopic self's action in order to achieve a better outcome over the long run. An example might involve a consumption problem. Suppose I am heading out to a local ice house (a Texas version of an outdoor bar). My long-run self says I only want to spend $20 on beers. My short-run self agrees, but I know that I'll probably want yet another beer after my money dries up, and that before I know it, I'll have spent $40 on beers for myself (and no doubt my prosocial self buying rounds for others). My long-term self adopts a simple device—I bring $20 in cash and leave behind my ATM card and all credit cards. This way, when my money runs out, I'm done. Fudenberg & Levine's model deftly handles this self-control problem and covers much the same ground as hyperbolic discounting (imagine that the beta component of the beta-delta function characterizes the degree to which the long-term self is involved in a choice).

A number of other models have been proposed to deal with time inconsistency. Gul & Pesendorfer (2001) detail a model of temptation and self control. Bernheim & Rangel (2004) provide a model also dealing with temptation, using insights from addiction. All of these models are designed to deal with persistent empirical results that show that people are inconsistent in how they value the future.

Time discounting is not limited to economics and finance. The same concepts have application in political science. First, almost all theoretical models with repeated play have some form of time discounting in them. A simple parameter that discounts future payoffs yields tractable solutions for infinite-horizon games. Yet such studies rely on a very standard form of time discounting that does not match the anomalous behavior that troubles economists. Does it matter for our models? My sense is that it does. How much would theoretical predictions for going to war vary if there is heterogeneity among actors—with some being very foresighted and others being extremely impatient? How do models of legislative bargaining change when there is an influx of hyperbolic discounters into the legislature? Are there interesting institutional mechanisms built into political systems that can accommodate dual-self models?

Second, from the empirical side, the problems associated with varying levels and types of time discounting may give us insights into why voters prefer to hear short-term solutions to problems or why politicians and political parties may only offer short-term solutions when campaigning. It may explain why some issues persist and others die before ever getting onto the policy agenda. Undoubtedly there are other possibilities that can inform political science.

CONTEXT

The final major puzzle for behavioral economists pertains to context. There are three parts to the puzzle: gains and losses, status quo bias, and framing. Kahneman et al. (1991) provides an easy, but old, entry point to these issues. In this section, I treat each one separately.

Gains and Losses

When I offer advice to people designing their first experiment, I caution them to avoid losses for subjects and I tell them to avoid a payoff of zero as one of the outcomes. Invariably, the objection is either "in the real world people face losses" or "the model normalizes payoffs between 0 and 1 and I want to test the model." I then advise them to scale up the payoffs and when asked why, my justification is that subjects act weird in losses, and zero is a focal point that subjects avoid. The core of my observation involves loss aversion by subjects and the fact that zero is an important reference point.

Kahneman & Tversky (1979) opened up the discussion in economics when questioning whether standard forms of expected utility held up. Their 1979 article on prospect theory developed a powerful critique of expected utility (and some of its weighted variants), proposed a utility function in which individuals were risk seeking in gains and risk avoiding in losses, and illustrated the issues with a large number

of hypothetical examples. This article set off a substantial and ongoing debate in economics over the understanding of risk (for reviews, see Camerer 1995, 2000; Laibson & Zeckhauser 1998; Starmer 2000).

Grether & Plott (1979) did not believe the Kahneman & Tversky results. They thought such an anomaly could be easily wiped out using the appropriate experimental design. They complained that the Kahneman & Tversky results were supported by experiments using hypothetical statements. This meant that the subjects had little incentive to think hard about the problem. In a clever set of laboratory experiments with money on the table, Grether & Plott (1979) found the same phenomenon—preference reversals. "Needless to say," they admitted, "the results we obtained were not those expected when we initiated this study" (p. 634). This spawned a large number of experiments designed to do away with the problem. But it has not gone away—although a recent survey (Berg et al. 2010) points out that incentives may limit reversals.

Recent work has tried to "marry" expected utility and prospect theory. Harrison & Rutstrom (2009) conducted an experiment over many gambles in a gains frame, a losses frame, and a mixed gains and losses frame. They explicitly account for subject heterogeneity (sex, race, and age differences) and point out that both models (expected utility and prospect theory) survive. However, both models appear to operate in conjunction with heterogeneous traits brought into the lab and in specific task domains. Loomes et al. (2010) take a different strategy by designing an experiment to see if market forces erode problems of preference reversals. The prediction is that the market disciplines those who do not adhere to standard assumptions. However, they, too, find that preference reversals remain.

Little of this abbreviated discussion should surprise political scientists. We have long been aware of prospect theory. Influential surveys by Levy (2003), McDermott (2004), and Mercer (2005) point to its importance for international relations. They regard risk-seeking behavior as important for explaining military gambles. On the other hand, risk aversion may help explain the relative stability of international systems. As Mercer (2005) makes clear, prospect theory has not made much progress in other fields of political science. Political economists (who commonly assume risk neutrality in models) could benefit from accounting for different responses to gains and losses. Political psychologists ought to be interested in differential cognitive processing under gains and losses. As to the latter, recent work by McDermott et al. (2008) leverages a foraging model to ground prospect theory in evolutionary theory, thereby posing a potential explanation for the underlying psychological mechanisms driving different responses to gains and losses. Because it seems likely that risk preferences are dramatically different in gains and losses domains, these differences ought to be accounted for in our models. Perhaps a sticking point for many is that the reference point on which such models are dependent is incompletely understood.

Status Quo Bias

A second puzzle has been characterized as the status quo bias or the endowment effect. The problem was originally noted by Knetsch & Sinden (1984) and made more widely known by Kahneman et al. (1990). The puzzle is best represented by a classic experiment from this latter article. Subjects were randomly assigned to receive a coffee mug or not. After handling their coffee mug, subjects were asked how much they were willing to accept to part with it. Those without a mug were asked how much they were willing to pay for one. The median price for sellers was $5.75 and the median price for buyers was $2.25. The difference could be due to what experimentalists call a "wealth effect," so another experiment was run in which there were buyers, sellers, and choosers. The two former roles were the same as before, while the choosers made a set of choices between whether to accept a specific amount of cash or accept the mug. Under these conditions, the median price for sellers was $7.12, for buyers it was $2.87, and

for choosers it was $3.12. The interesting finding is that subjects overvalue the item that they have in hand. This endowment effect persists in many environments, providing a basis for the old adage "a bird in the hand is worth two in the bush." In effect, individuals value those things that they hold.

The endowment effect is not limited to adults. Harbaugh et al. (2001) demonstrate that the endowment effect carries over to children. Chen et al. (2006) provide evidence of it in capuchin monkeys. A quasi-field experiment over the trading behavior of sports card memorabilists indicates a preference for the status quo, although the effect diminishes with the experience of the trader (List 2003). A recent model by Kőszegi & Rabin (2006) points out that the reference point held by actors is crucial, and that more experienced actors may have better estimates of the likelihood of retaining the endowment. In short, the endowment effect seems to persist. With experience it tends to dissolve, but as DellaVigna (2009) points out, even traders hold onto some stocks too long.

For political scientists, accounting for status quo bias can be quite important. It can be useful when considering why incumbents enjoy an advantage with respect to voters. For example, Patty (2006) uses status quo bias to help model congressional midterm losses for the president's party. Status quo bias provides insight into the stickiness of legislation. It might help explain why institutional rules are not continually rewritten. Status quo bias may help explain why trade agreements are so tortuous and why interest groups can thrive once they gain a foothold. In short, this is a puzzle that political scientists are familiar with but rarely include in models.

Framing

The final puzzle involves framing and was pointed out by Tversky & Kahneman (1981). The puzzle is tied with loss aversion, since many of the examples (and experiments) change the frame from gains to losses. However, individuals should not switch their choices when given inconsequential changes in the description of the choices. In the classic example, subjects are presented with the possibility of an unspecified "Asian disease" that is expected to kill 600 people and are asked to choose between two different policies. If policy A is chosen, 200 people will be saved; if policy B is chosen, there is a one-third chance that 600 people will be saved and a two-thirds chance that no people will be saved. Typically people prefer policy A, which indicates risk aversion. Another group of randomly selected subjects is given the equivalent policy options, but framed in a different manner. If policy A* is adopted, 400 people will die; if policy B* is adopted, there is a one-third chance that no one will die and a two-thirds chance that 600 people will die. Subjects now favor policy B*, the risk-seeking policy. Aside from the framing, the pairs of policies (A,A*) and (B,B*) are equivalent. Yet behavior varies with the frame.

Fehr & Goette (2007) find strong evidence of reference effects that can easily be manipulated through framing. Their field experiment with bicycle couriers in Switzerland has the authors manipulating wages and using lotteries that are similar to the framing problem noted above (but with cash incentives). They find evidence of switching behavior in the lotteries and link this to labor and effort decisions.

Framing effects are not unusual anomalies for political scientists. The discipline was introduced to the problem by Quattrone & Tversky (1988). That article was presented as a challenge to rational choice theory and provided evidence from experiments in which subjects made choices based on hypothetical settings—very similar to those of Tversky & Kahneman (1981), mentioned above. However, it had a muted impact on game theorists in political science. This does not mean that framing is unfamiliar to the discipline. Chong & Druckman (2007) catalogue the numerous ways in which framing effects have been exploited: detailing how issues are presented to the public, how support for national institutions varies under different frames, and how competitive frames play out in public opinion. Given that interest

groups and politicians are constantly competing to bring issues to the fore, it is no surprise that framing should be important in political science.

Generally, the anomalies relating to context have been at the forefront for political scientists. A brief sampling of the political science literature turns up overviews of context effects for shaping public opinion (Druckman & Lupia 2000, Jones 1999, McDermott 2001); concerted efforts at understanding prospect theory and its implications for modeling risk in international relations (Boettcher 2004, Levy 2003, Mercer 2005, O'Neill 2001); and a good deal of work on framing and its relevance for political decision making (Baumgartner & Mahoney 2008, Druckman 2004, Druckman & McDermott 2008). In short, political science has absorbed these lessons and applied them to its own distinct problems. However, empirical work has outpaced theoretical attention to many of these issues.

Psychological Irregularities

Outside the trinity of major behavioral puzzles for economists, there is a cluster of psychological mechanisms of concern. Whether these are considered heuristics or psychological peculiarities does not matter for the purposes of this review. They are well known, well documented, and pose problems for standard economic theory. I treat four problems: overconfidence, levels of thinking, emotions, and status. There are others. I could just as easily have chosen probability over- and underweighting (it turns out that people are not very good at judging low-probability events; we overweight the likelihood that the event will occur). Or I could have turned to projection bias, in which people use their current situation to project future action ("I'm in good health now, so I plan on being in good health in the distant future"). Or I could have detailed neuroeconomics, which as a subfield tackles many of these issues [for an interesting discussion of the value of neuroeconomics, see the special issue of *Economics and Philosophy*, 24(3), 2008].

Overconfidence

Psychologists have long noted the Lake Wobegon effect, named after Garrison Keillor's town in which "all the children are above average." Svenson (1980), for example, reported that 80% of all drivers believed their driving ability was above average. For behavioral economics, the question is whether this finding persists in the face of market structures that ought to drive out overconfidence. There appears to be plenty of evidence that entrants into markets are overconfident about their likelihood of success, investors are overconfident about their ability to read the market, and males are overconfident about their skills in competitive settings.

Camerer & Lavallo (1999) began much of the discussion in economics by examining whether excess market entry can partly be explained by overconfidence in one's ability (a "competitive blindspot"). Camerer & Lavallo experimentally manipulated the payoffs in different markets, elicited whether subjects chose to enter each market, and then used a trivia quiz to determine performance. Performance was measured after entry decisions were made. As expected, overconfidence abounded, with subjects entering more frequently into markets when performance was based on their own actions (compared with a random assignment condition). Numerous other studies have replicated this finding about overconfidence in abilities (see, e.g., Clark & Friesen 2009).

In a survey of the gender gap in math abilities, Niederle & Vesterlund (2010) point to several experiments demonstrating overconfidence by males in competitive settings. In particular, they note an experiment in which subjects were given math problems to solve using two different payment schemes. The first was a piece-rate setting, in which a fixed amount was paid for each correctly solved problem. The second setting was competitive, with the person who got the most correct answers in their four-person group (two males and two females) being paid a winning amount and everyone else receiving nothing. In both payment settings, males and females, on average,

correctly answered the same number of math problems. Having experienced both payment settings, but not being informed of their performance or rank in either setting, subjects were asked which payment scheme they wanted for the next math task. Almost three quarters of the men and only 37% of the women chose the competition setting. To get at beliefs, subjects were asked how they thought they ranked in the competition setting and were paid for a correct guess. Seventy-five percent of the men and 43% of the women guessed they performed the best in the competition. Although both men and women are overconfident in their abilities, men are more so. No matter how hard the authors try to get rid of this gap, they are unable to do so.

Overconfidence is a well-known problem. It can lead to overbidding in auctions, it can explain too many entrants into market, and it might explain some differences between males and females in competitive settings. For political scientists, overconfidence may be useful for explaining why some candidates persist in campaigning even when they have no chance of election. The same underlying mechanism may explain campaign donations to a losing cause (and I conjecture there will be pronounced gender differences in those donations). In any competitive political setting, seemingly irrational behavior may be the result of overconfidence by one or more individuals. This may give insight into candidates who pour tens of millions of their own wealth into running for office. In a very different setting, overconfidence may help explain why some leaders enter into conflicts they cannot win. Sometimes "resolve" may simply be misplaced confidence.

Levels of Thinking

Most standard models assume economic agents are not doomed to myopia and, at a minimum, are capable of thinking ahead. This implies that backward induction for any structured game ought to be easy. Certainly top-rated chess players are capable of thinking many steps ahead, but it turns out that most people are not very good at multistage strategic reasoning. The classic example is the "beauty pageant" game, in which there is an array of contestants and the task is to pick the average of the choices of the judges. Assuming there are 100 contestants evenly spread on a number line ranging from 0 to 99, whom should any individual pick? A judge with no sophistication or beliefs about others can simply choose a random value. This would be someone with zero-order beliefs. A slightly more sophisticated judge might form (first-order) beliefs about the behavior of other judges and generate a best-response choice. Another may form second-order beliefs about the first-order beliefs of others and so on. Suppose my zero-order beliefs entail a guess of the average of the contestants on the number line (choosing between the 49th and 50th contestants). A first-order belief might anticipate this and lead me to choose some value below this, while also anticipating the response by other first-order beliefs. This continues for higher-order beliefs. If everyone holds higher-order beliefs, then the choice is the contestant at 0.

Nagel (1995) tests a variation of this game to determine whether there are levels of reasoning among subjects. Subjects repeat the task several times and are informed about the outcome following each play. She finds evidence for heterogeneity in the population but also finds that subjects learn with repetition. The modal subject engages in level 2 thinking, and the bulk of the subjects are at level 3 thinking or below. Ho et al. (1998) extend this study and find similar results. There is a large number of people who begin with level 0 thinking, but learn. Those who are sophisticated (beginning at level 1 or above) best respond to level 0 players. Everyone learns, but not everyone adjusts as fully foresighted agents (see also Costa-Gomes & Crawford 2006, Coricelli & Nagel 2009).

Such findings could explain the dearth of results on strategic voting in political science. Although theoretical models suggest the importance of strategic voting, my own experimental data point largely to sincere voting (Wilson 1986, Wilson & Pearson 1987). McKelvey & Palfrey (1992) find little evidence for higher-level strategic thinking in the

"centipede game." In this game, players have alternating moves in which they can either stop the game or pass to their counterpart. At each choice node, there is an increasing pie with an asymmetric split (e.g., a [$4,$1] split at the first node and a [$2,$8] split at the second node and so forth). The stakes get very high at the last node of the game, but subjects never get there. McKelvey & Palfrey (1992) do find strong evidence for best-response behavior to various levels of thinking, but little higher-level strategic behavior.

The depth of strategic thinking should be critical for the ways we model different environments. For phenomena in which many skilled agents contribute to strategic choice, foresighted strategic reasoning seems appropriate. Here I am thinking of decisions made by nation-states in the international arena when there is plenty of time. Diplomatic, military, and local political considerations will contribute as decision makers work through many levels of reasoning. By contrast, decisions made under time pressure by individuals with little information and little experience may be more fruitfully modeled as low-level strategic reasoning. For example, voting on obscure, nonpartisan, local candidates can be treated in this fashion. Overall our models would benefit from asking whether predicted outcomes would change as we shift from higher- to lower-order beliefs.

Emotions

Elster (1998) provides an important survey of the manner in which emotions may influence economic decisions. In particular, he points to the role of envy and indignation in motivating rejections in the ultimatum game and the place of love, hate, revenge, and contempt in a number of other settings. Prior work by Frank (1988) had established a basis for emotion in bargaining models. For Frank, difficult-to-disguise emotions like anger have the value of demonstrating commitment to a course of action.

It is well established in psychology that certain kinds of emotion can focus individuals on narrow goals and outcomes. Whitt & Wilson (2007b) use a one-shot public goods game to examine levels of cooperation among Hurricane Katrina evacuees housed in rescue shelters. All of the participants were very stressed (having been evacuated from New Orleans the week prior), but the most stressed were people whose family members were still missing. Those individuals contributed the least to the public good, and their behavior was consistent with what is expected of people who are highly fearful. Likewise, Eckel et al. (2008) report risk-seeking behavior among Katrina evacuees that reflects similar emotional reactions. In both of these articles, games drawn from experimental economics were used to measure behavior in light of varying emotional states.

Xiao & Houser (2005) use the ultimatum game to determine whether providing subjects an emotional outlet changes behavior. While a control group of subjects played a standard ultimatum game, those in the treatment group were given the opportunity to write a message to the proposer. The findings were clear. When given an opportunity to express an emotion, there were fewer rejections of low offers. Xiao & Houser (2005) interpret this to mean that costly punishment (rejections in the control condition) is used to express negative emotion. When another option is available to express emotion, costly punishment is not used. There are a handful of other articles on emotion and behavior (e.g., Fong 2007). However, although emotions are recognized as important for economic decision making, to this point few researchers have followed up on the promise.

An exception is the enormous literature on the topic of happiness. Much of that work is driven by responses to survey items concerning happiness (Clark et al. 2008). This voluminous survey literature often compares the aggregate levels of happiness across nations or places. The findings correlate everything from increasing country-level gross domestic product (which improves general happiness) to positive income shocks (which improve individual happiness only in the short run). Generally, happiness is taken to be an emotional mood and serves as a proxy for well-being.

Emotion has long been a part of political science. Work on "affective intelligence" (Marcus 2000) details how individual decision makers are more or less attentive to information depending on their affective state. Others demonstrate how people under threat evaluate candidates and policy issues (Brader 2005, Huddy et al. 2005). Opportunities for additional research abound. How elites use emotional frames to mobilize citizens is not well understood. The circumstances under which emotions are amplified within groups need to be addressed. For example, it appears that my fearfulness elevates when I am surrounded by others who are also fearful. Emotions are very much a part of our genetic makeup. In us, as in other animals, emotions dictate the four F's: fighting, fleeing, feeding, and mating. This message has not been lost on those concerned with biological foundations of politics (Oxley et al. 2008).

Status

Humans often look to others to decide whom to emulate or imitate. The decision of whom to follow is not random and is often associated with status. Status, of course, can be conferred for many reasons (Henrich & Gil-White 2001, Webster & Hysom 1998), but ordinarily it should have little economic consequence. However, as Mathur et al. (1997) show, when Michael Jordan announced he was returning to the NBA following a brief retirement and a stint as an unsuccessful baseball player, his client companies showed a 2% increase in stock prices, valued at over one billion dollars. Few of us are likely to "be like Mike." However, apparently many of us looked to him.

Although status should not matter for economic decisions, there is a good deal of evidence that it does. Ball et al. (2001) provide a beautifully designed experiment to test the effect of status in a market. They use a box-design market in which all sellers have the same cost and all buyers have the same reservation value. An equal number of units is demanded and supplied, which leads to a vertical overlap in the demand and supply curves. They use an oral double auction institution, which pushes the market to a uniform price. The equilibrium (market price) can be anywhere between the sellers' cost and buyers' reservation value. Status in this experiment was randomly awarded. Prior to beginning the experiment, half the group was given a gold star and those without gold stars were told to applaud those with. In one treatment, all those with gold stars (the high-status group) were assigned to be buyers, which everyone knew. In the other treatment, those with gold stars were assigned to be sellers. Ball et al. find that when buyers have higher status, the market price is lower than when the sellers have higher status. On average, high-status players earn 11.4% more. Markets should not be responsive to status, but they are. (For more experiments with bargaining games and status, see Ball & Eckel 1998, Eckel & Wilson 2007, Eckel et al. 2010.) In the end the question is why are people attentive to status: for reasons of imitation or for deference?

Status is a central concern for several issues in political science. A clear understanding of leadership eludes us. We can point to specific leaders, but we are hard pressed to predict who will become a leader or to prescibe how to enhance one's leadership. Many of these issues seem to be tied to status. Status, of course, is a signal, and it would be valuable to know how that signal is sent and read. Political scientists often talk about things that confer status on individuals, groups, or even nations. In some manner, having status leads to others responding in predictable ways. As with economic decisions, status should not affect political outcomes. It does, and it remains a puzzle for us.

In summary, numerous psychological anomalies pervade market economies—far more than have been touched on here. These same anomalies pervade political science.

MOVING BEYOND ANOMALIES

Political scientists are fortunate in that we are not bound to a particular approach to understanding the core issues of politics. Rational choice, social choice, and game theory

provide important insights into political behavior, but political scientists have not been afraid to borrow from psychology, sociology, and anthropology as well. What does behavioral economics mean for political science?

It is crucial to understand whether there are social preferences. Here the questions are many, complex, and perhaps daunting. Do people have a taste for fairness? Are people willing to punish others (at a cost to themselves) when norms are violated? Are people attentive to hierarchy and their position in it? Do people have a propensity to trust others? Are people naturally cooperative (and to what end: to help the group or to destroy another group)? These questions raise a number of issues. First, are many of these behaviors innate? Do humans have a propensity toward such behavior? If so, such dispositional traits should be helpful in getting the assumptions of our models right. A second issue is the extent to which these tastes are easily manipulated by leaders or redirected by institutional design. Causally, it may be that a taste for trust is a necessary condition for stable political institutions, or perhaps the causal relationship runs the other way. These and other issues need to be sorted out.

How can findings on time discounting be brought into political science? This is a serious question not only for theorists (who often rely on fairly simple models of time discounting) but also for empiricists. After all, voting and campaigning are importantly tied to promises about the future and how citizens view the future.

The puzzle of context is not so puzzling for political scientists. We are used to these problems and have accounted for them in a rich array of models and studies. However, as a discipline, we should not forget that gains and losses, status quo bias, and framing pose challenges to standard models and need to be accounted for.

There are a large number of psychological heuristics to consider when we model behavior or empirically test predictions. Not all of these heuristics will matter—many may be domain specific. However, they are important puzzles that are grounded in human psychology. In the next ten years cognitive neuroscience may provide important insights to the discipline, much as microeconomic theory did 30 years ago. I do not expect that the fundamentals of rationality will disappear, but rather that systematic findings from cognitive neuroscience will inform our simplified models of human behavior.

We know that peer effects help mobilize individuals. But we remain unclear about the mechanisms that push people to vote on this basis. They may have to do with imitation, social identity, overweighting the fear of social humiliation—the question is important, but remains unsolved.

Emotion needs to be brought into the study of social behavior. So far, political science has applied emotion to individuals. We have a good sense of what anxiety and fear mean for individual behavior. Yet, we suspect that many aspects of political life—including mobilizing people to harm others—require an emotional spark. It may be that such a spark works through anger, disgust, or fear. However it works, these emotions most likely are compounded by social interaction. Figuring out this mechanism will not be easy.

Finally, political science should take note of what has driven behavioral economics and imitate it. Many of the compelling findings have been derived from the laboratory. Skeptical researchers have created inventive experimental designs to understand where standard theory breaks down. The end is not to demonstrate that the theory fails. Rather, the aim is to pinpoint the circumstances under which it fails. Behavioral economics is not awash with researchers seeking to dismantle the standard model. Instead, those researchers begin with the premise that the model provides insight and are puzzled by their findings. This sense of puzzlement and honesty in designing new experiments to test the boundaries of failure have drawn the formal theorists into trying to explain the lapses. There are positive signs that the laboratory will be used more frequently for answering fundamental questions in political science. It will be important to engage formal theorists with those findings.

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