

The Affect Effect

DYNAMICS OF EMOTION IN
POLITICAL THINKING AND BEHAVIOR

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Politics and the Equilibrium of Fear: Can Strategies and Emotions Interact?

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Political scientists seek improved explanations of political behaviors and outcomes. Improvement comes not only from the promulgation of new concepts for thinking about politics but also from refined understandings of the conditions under which more established concepts apply. Political psychologists engage in such explorations. So do game theorists. We argue that these two groups have something to offer one another, something that can improve explanations of some of the social behaviors on which these groups focus. To set the stage for this offering, we begin with a brief description of what each group of scholars does.

Political psychologists use research concerning human thought and perception from other disciplines to inform and motivate their work. In this field there are no widely accepted guidelines for what it means to engage in the practice. Some political psychologists follow standard social psychological practices, designing research from a laboratory-based stimulus-response paradigm and running experiments whose relation to specific scientific questions is simple and clear. Others follow practices that are common to the study of public opinion and voting behavior. They draw inferences from regressions conducted on answers to multipurpose questions placed on large surveys. Still, political psychologists embed experiments in surveys. So instead of being defined by use of a single method, political psychology is defined by the use of an expanding range of methods.

Game theorists seek precise explanations of the causes of individual behaviors and collective outcomes. They use mathematized premises and conclusions to draw logically coherent inferences about when and why

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people behave as they do. Since the 1980s and 1990s, when an increasing number of scholars learned to design and solve games of incomplete information, game theorists have expanded their inquiries into questions of how thoughts, perception, cognition, and learning affect social phenomena. To date, however, political psychology and game theory have had very limited interaction.

Can these two endeavors converge in a constructive way? Yes, they can. Integrating aspects of game theory and political psychology can create valuable knowledge that neither approach can generate alone. In this chapter, we support this conclusion by focusing on the part of political psychology that focuses on emotions. With the publication of studies by Kinder (1994), Lodge and Taber (2000), Rahn (2000), Marcus, Neuman, and MacKuen (2000), and Brader (2005) has come greater interest in the emotional basis of political interactions (see also Lerner and Keltner 2002). We build on these efforts.

We work against the null hypothesis that game theory and the study of emotions are completely irrelevant to one another. This null hypothesis is no straw man. To see a rationale for believing it, consider the following statement by LeDoux (1996, 19): “[E]motions are things that happen to us rather than things we will to occur . . . external events are simply arranged so that the stimuli that automatically trigger emotions will be present. We have little direct control over our emotional reactions.”

It is hard to disagree with the claim that emotional responses have a strong subconscious component. It may even seem reasonable to conclude that game theory—with its focus on incentives, strategic decision making, and goal-oriented learning—cannot clarify emotional aspects of politics. Such ideas are consistent with Elster’s (2000, 692) conclusion: “The social sciences today, however, cannot offer a formal model of the interaction between rational and non-rational concerns that would allow us to deduce specific implications for behavior. As mentioned earlier, the idea of modeling emotions . . . is jejune and superficial. The fact that emotion can cloud thinking to the detriment of an agent’s interests is enough to refute this idea.”

Although there is much to disagree with in this claim, such as the separation of emotion and reason and the tendency to confound game theory as a method with very narrow notions of rationality, a critique of such ideas is not our focus. Instead, we offer a constructive attempt to yield improved inferences about important political phenomena.

It is not helpful to claim improvement without a standard against which to measure it. We choose applicability and argue that insights from game-theoretic studies of strategic decision making can clarify the conditions

under which empirical claims about emotions in politics apply to particular situations. At the same time, we contend that replacing standard game-theoretic assumptions about how people react to particular stimuli with premises that are more realistic empirically can clarify the conditions under which important ideas about strategic behavior apply to politics.

A basic version of our argument is as follows: emotional responses have subconscious aspects. These aspects are beyond the purview of strategic decision making, incentives, goal-oriented learning, and other phenomena for which game theory is an effective explanation. But there is more to emotions and politics than what happens at the subconscious level. In thinking about the extent to which a game-theoretic logic of emotion in politics is possible, we find Damasio's distinction between primary and secondary emotions useful: "Primary emotions (read: innate, preorganized, Jamesian) depend on limbic system circuitry, the amygdala and anterior cingulate being the prime players. . . . But the mechanism of primary emotions does not describe the full range of emotional behaviors . . . they are followed by mechanisms of secondary emotions, which occur once we begin experiencing feelings and forming *systematic connections between categories of objects and situations, on the one hand, and primary emotions, on the other*" (Damasio 1994, 134, emphasis in original).

Unlike primary emotions, secondary emotions can be learned and inhibited. For questions of applicability, the question is "How?" An answer comes from research concerning emotions at the level of the neural substrate. It reveals important connections between emotional responses and goal orientation. As Kandel, Schwartz, and Jessell (1995, 610) describe it: "[T]he amygdala is required for the conditioning of an organism to the environment (or context) in which it lives. The survival of an organism depends on behaviors that maximize contact with biologically safe environments and minimize contact with dangerous environments. Many of these dangers are subject to modification through experience." Because many of the emotions that are relevant to political contexts are of the secondary variety (after all, it is hard to imagine someone fearing Bill Clinton from the womb) and because some political actors may attempt to evoke emotions as part of a persuasive strategy, the conditions under which political phenomena will induce or be affected by emotional responses will be a function of goal-oriented decision making (for example, directed learning), at least in part. In this sense, we follow Damasio (1995, 124), who argues:

Culture and civilization could not have arisen from single individuals and thus cannot be reduced to biological mechanisms and, even less, can they

be reduced to a subset of genetic specifications. Their comprehension demands not just general biology and neurobiology but the methodologies of the social sciences as well. In human societies there are social conventions and ethical rules over and above those that biology already provides. Those additional layers of control shape instinctual behavior so that it can be adapted flexibly to a complex and rapidly changing environment and ensure survival for the individual . . . in circumstances in which a preset response from the natural repertoire would be immediately or eventually counterproductive.

Our argument continues with the premise that the kind of goal-oriented learning that Kandel et al. and Damasio describe can be affected by incentives and strategic decision making. As a result, incentives and strategy can affect the conditions under which emotions affect politics—and vice versa. Because game theory has proved to be an effective way to understand how incentives and strategies affect behaviors and outcomes in other contexts, we conclude that it can help researchers clarify the conditional relationship between emotional responses and political circumstances.

In the rest of this chapter we present the longer form of the argument, proceeding as follows. First, we focus on what the study of emotion can bring to game-theoretic analyses of politics. We do so by first breaking down what game theorists do and then showing where findings about emotion can make constructive contributions. Second, we reverse the question, examining what game-theoretic practices can bring to the study of emotion. We argue that the relevance of emotional phenomena to a given political situation is likely to depend on strategic factors. We then describe an example of new research in which game-theoretic and emotional considerations are integrated to constructive scientific ends.

CAN THE STUDY OF EMOTIONS IMPROVE POLITICAL GAME THEORY?

In this section we have two objectives. First, we debunk two widely held views of game-theoretic political science that, if true, would limit the value of attempts to integrate insights from the study of emotions. Second, we offer a framework for such integration. To accomplish both objectives, we begin with a brief description of what game theorists do.

Noncooperative game theory, the dominant form of game theory in use today, is a method of representing and explaining behaviors and out-

comes in contexts where participants can act strategically. In this context, *strategy* means “plan of action.” Game theory generates insights by allowing researchers to evaluate the extent to which one player’s plan of action is a best response to the plans of others. If all players in a game perceive their plan of action as a best response to others’ plans, then the game reaches a steady state—not one person has an incentive to change his or her plan of action at any point in the game. Such steady states are called equilibria.

Equilibria are focal in game theory because they constitute more reliable representations of social decision dynamics than do situations in which at least one player would want to change a plan of action. In other words, if we have described the game correctly and we offer every player in the game a prediction of what everyone in the game will do and every player truthfully responds “Yes, you have described my plan correctly,” then the situation is in equilibrium and the prediction will be accurate. If, by contrast, one or more players were to say, “Now that I know what you have told me, I am going to change my plans,” then the situation is out of equilibrium and the prediction will be inaccurate. In game-theoretic contexts, and all else constant, equilibrium means more reliable inference.

The credibility of equilibrium statements in noncooperative games depends in large part on a set of practices that game theorists follow when developing their models. A principal goal for game theorists is to offer logical clarity and precision regarding the topic of study. This goal induces scholars to state premises and conclusions in explicit terms and to make their relations transparent. Indeed, such transparency is required in the sense that when a game theorist draws a nonobvious conclusion from a set of premises, he or she is expected to prove (in the literal sense) that the conclusion is a direct logical implication of the premises. Merely waving one’s hands at the relationship tends not to be credible. Moreover, for those who have sufficient mathematical background, such practices make the logic of game theoretic arguments replicable—which can reduce misunderstanding, increase the efficiency of scholarly debates, and hasten the accumulation of knowledge.

The same properties of game theory that increase the effectiveness of some efforts cause problems in others. For example, making models precise and analytically tractable usually requires the use of simplifying assumptions. Critics of game theory are quick to point out that some of these assumptions are unrealistic. Such criticisms are often fair and sometimes constructive. In two of these critiques, we see an opportunity for the empirical study of emotion to improve the applicability of political game

theory. In one critique, game theory is criticized for imposing rationality. In the second, it is criticized for the way it treats information and perception. In what follows, we demonstrate that understanding the validity of these critiques provides a useful way to see how the introduction of emotion-related concepts can improve a model's applicability.

Much Ado about Rational Choice

Many game-theoretic efforts in political science are criticized for the minimal way in which they represent how people think. Game theorists respond to such criticisms by saying that people may not actually do the kinds of mathematical calculations attributed to actors in the model, but they act "as if" they think about the world in that way (see, for example, Satz and Ferejohn 1994). To judge the extent to which game-theoretic approaches satisfy "as if" standards, it is important to understand that theorists model individual psychologies in different ways.

Models vary in their assumptions. Some famous models such as the prisoner's dilemma or the median voter theorem are games of complete information, where all players are assumed to know everything about every aspect of the game. In recent decades, theorists have become facile with the mathematics of games of incomplete information. In these games, players may not know everything and may act on the basis of their beliefs. As a result, theorists now model a growing range of psychological processes (see, for example, Lupia, Zharinova, and Levine 2007).

That game theorists can model phenomena such as attention, perception, and learning is interesting because it runs counter to a common belief, namely, that game theory as an analytical method and the phenomenon known as rational choice theory are one and the same. This belief is mistaken.

Game theory is, above all, a method. Across all existing models, the intersection of assumptions is quite minimal; actors with all kinds of worldviews and psyches are allowed. What the modelers do share are basic rules for drawing inferences—those described above.

Rational choice theory, by contrast, means something less useful because extant definitions of rationality vary widely. By *rationality* some people mean wealth maximization, others mean selfishness (which may be nonmonetary), some mean omniscient decision making, and others mean something completely different (see Lupia, McCubbins, and Popkin 2000, 5–9 for a longer list). Therefore, many of our discipline's debates about whether people are rational are as much about failure to coordinate on semantics as they are about the substance of human decision

making. Henceforth, we follow this advice on how to think about how people think.

If we can distill these many definitions of rationality into one that is sensible empirically and widely applicable, we can avoid much of the confusion currently associated with the concept of rationality and, as a result, craft better explanations of why people do what they do. We will now argue for such a definition. The basis of our argument is that there is at least one issue on which these many definitions of rationality agree. The issue is that people have reasons for the choices they make. That is, regardless of people's genetics or socialization, if they are able to make choices, then reasons will precede these choices. Therefore, we conclude that *a rational choice is one that is based on reasons, irrespective of what these reasons may be.* (Lupia, McCubbins, and Popkin 2000, 7 emphasis in original)

Most psychologists focus on reason rather than rationality, where reason is nothing more than the normal functioning of the mind. Behavioral economists are moving toward a similar focus. The segment of political science that concerns itself with offering improved explanations of individual behavior and collective choice should do the same. As the game theorists who reside within this segment focus on reason instead of rationality, a productive joint venture with scholars of emotions becomes increasingly possible.

So if narrow notions of rationality and all ways of representing individual psychology in game theory were equivalent, and if emotion and reason were antithetical, then a constructive integration of game-theoretic methods and substantive insights about emotion would be impossible. Above, we explained why the first antecedent (equivalence) is false. We now do the same for the second antecedent (antithesis).

Many scholars, particularly since the wide dissemination of Damasio's *Descartes' Error: Emotion, Reason, and the Human Brain*, have shown that emotions can enhance information processing and improve decision quality. The correspondence between emotions and reason runs deep. As Kandel et al. (1995, 600) report, "An animal whose sympathetic nervous system is experimentally eliminated can survive as long as it sheltered, kept warm, and not exposed to stress. Such an animal cannot, however, carry out strenuous work or fend for itself." Emotions and what many people regard as reason are not antithetical. Indeed, as Phelps (2006, 46–47) concludes in her recent review of neuropsychological research, "[t]he mechanisms of emotion and cognition appear to be intertwined at all stages of stimulus processing. . . . Examining cognitive functions

without an appreciation for the social, emotional, and motivational context will result in an understanding that may be limited in its applicability." Therefore, a joint venture between game theory and emotions in politics is potentially instructive. To realize this potential, however, we need to be more specific about how to incorporate emotional phenomena into a game-theoretic model.

Utility and Information as Entry Points for Emotions

The second criticism of game theory concerns its psychological adequacy. Consider, for example, the common practice of assuming that players have quadratic utility functions (for example, in a model where preferences and outcomes are represented by points in a space, utility declines in the distance between a player's ideal point and the outcome of the game—squared) or the assumption that players have uniform prior beliefs (that is, they believe that every possible state of nature is equally probable). We know that such assumptions are chosen for mathematical convenience (that is, uniform priors often allow universal inferences about game attributes without the use of complex derivations). We know that both practices make it easier for scholars to solve the models they construct. We also know that such assumptions are rarely, if ever, based on even a cursory examination of the decision makers' psychology.

Given what we know, it is worth stating that universal rejections of game theory on the basis of blanket critiques about the method's psychological inadequacy are of limited value, especially if the alternative is either unstated theory or brands of theorizing in which premises, conclusions, and their logical relations are stated imprecisely. Indeed, oversimplified assumptions about cognition in political science are not unique to game theory. Many attempts to explain political behavior, including those grounded in case studies or regression analysis, are based on premises with no apparent connection to concrete empirical findings from fields such as psychology. For example, when people use OLS or Probit to draw an inference about political psychology from survey data, they incorporate into their argument implicit assumptions about allowable relations between the included variables. When some of these variables are meant to represent behavioral phenomena and others are meant to represent psychological phenomena, the choice of a particular estimator implies concrete assumptions about the mechanics of reason whose relation to well-documented psychological insights are rarely, if ever, clearly established. As a result, one way to characterize the difference between game theorists' assumptions about psychological phenomena and those

of many other scholars is that game theorists are more likely to state their assumptions clearly enough to be scrutinized. Put another way, for many descriptions of political behavior other than game theory, assumptions about peoples' mental states are less controversial only because they are less clearly articulated.

The challenge for us, then, is to clearly present in game-theoretic terms some of the emotion-relevant phenomena that are increasingly prevalent in political psychology. Such a task is made easier by the fact that all noncooperative games are built from a common list of conceptual elements. A brief review of this list reveals the places where emotions can be brought in.

Many game theory textbooks define the components of a game as follows: players, actions, strategies (plans of action), information (what people perceive and believe about various aspects of the game), outcomes (the consequence of their actions—games are most interesting when the outcome depends on multiple players' actions), payoffs (how people feel about various outcomes), and the equilibrium concept (the manner by which equilibria are determined). Of these, three are obvious candidates for introducing emotions: payoffs, information, and equilibrium concepts. We explain each in turn.

Payoffs are measured in terms of utility functions. A utility function represents how a person feels about a particular outcome. From the literature about emotions we can adopt the idea that a person's feeling about a situation can depend on whether emotional subsystems are activated. For example, if we hold the stimulus constant but vary whether the stimulus induces fear (where the variance may be due to differences in previous experiences with the stimulus), then we can expect a difference in how the person feels about outcomes that are associated with the stimulus.

What a player in a game knows is typically modeled as a probability distribution across important aspects of the game. A voter who is uncertain about a candidate, for example, is modeled as thinking about the candidate as if she assigns probabilities to various kinds of personality attributes or policy preferences that the candidate might have. We can use a similar representation to incorporate emotion. Consider, for example, that fear can be evoked by a conscious or subconscious association between an object and a painful outcome (one that provides sufficiently low utility). As Kandel et al. (1995, 608) describes it, "In fear conditioning, an initially neutral stimulus that does not evoke automatic responses can be paired with an electric shock such that, eventually, just the neutral stimulus will produce autonomic responses associated with fear." Therefore, if a player were to assign a particular object a sufficiently high probability of corresponding to a bad outcome, then it would be reasonable to expect

that player to react as a fearful person would. By contrast, a player who viewed exactly the same object but assigned it a lower probability of leading to bad outcomes might not react fearfully. In other words, the object provides the players with information about the potential outcomes of the game, but players' experiences with the object lead them to react to it in different ways.

A third game component, equilibrium concepts, can also serve as a channel for introducing emotional phenomena into game-theoretic models. An equilibrium concept entails assumptions about how people react to the strategies and beliefs of others. We contend that variations in emotional responses can affect these aspects of a person's decision calculus. If fear motivates people to pay greater attention to a particular aspect of their environment, as Marcus, Neuman, and MacKuen (2000) conclude, then it may also lead them to generate different counterfactual assessments of their own behavior or that of others than would be the case if they were not fearful. This, in turn, may affect the extent to which they are willing to maintain a particular strategy in the presence of other strategies. It may be that a behavior that an actor perceives to be her best response given a particular set of facts about the strategies and beliefs of others may not be her best response if she receives the same facts in a fearful state. Varying equilibrium concepts, from, say, the Nash equilibrium concept to one that allows greater variations in counterfactual reasoning, such as the self-confirming equilibrium concept, can allow us to integrate a wider range of emotional content into game-theoretic models (for a detailed discussion of this topic, see Lupia and Zharinova 2004).

In sum, research concerning emotion conducted over the course of the past decade reinforces the idea that emotions are necessary for goal-oriented behavior. As scholars such as Rahn (2000), Marcus, Neuman, and MacKuen (2000), and Brader (2005) argue, emotions can affect the kinds of environmental stimuli to which people attend, the manner in which they react to what they see, and what they recall. Game-theoretic methods, if properly developed, can help us understand the logical consequences of such variations. Therefore, being more explicit about the integration of emotional content can improve the extent to which game-theoretic models provide reliable descriptions of strategic political interactions.

CAN GAME THEORY IMPROVE EMOTION-FOCUSED POLITICAL RESEARCH?

Contributors to this book are among those who have helped establish that emotion need not be the antithesis of reason. Such findings were helpful

in explaining why research concerning emotions can improve the applicability of political game theory. Now we turn the tables and ask, "How can the use of game theory possibly improve the applicability of existing research regarding emotions and politics?" The answer, in short, is that previous work regarding emotions is largely silent about the topic of strategic interaction and about its implications for the conditions under which we should expect emotional phenomena to be relevant to political contexts.

In many studies, for example, a fear-relevant political variable is presented. Few scholars question the conditions under which the stimulus becomes fearful or the conditions under which it can cease to be so. And yet we know that people vary in the feelings they have about political phenomena and that these feelings can change over time. We also know that some people stake their political careers on the likely presence or absence of fear in political contexts. We know that creating a sense of fear can change the amount of discretion that citizens are willing to give to their political leaders or the concessions that one country or political faction will make to others. Some political leaders may perceive a personal benefit in creating fear. If the potential targets of such fear also know this, then they may have an incentive to be skeptical of stimuli that are meant to induce fear. Situations such as those described above are endemic in politics. They also have gamelike attributes such that the outcome depends on the beliefs and strategies of multiple players.

The presence of such attributes in politics is the reason why it is important to build on previous work by focusing on the extent to which aspects of goal-oriented learning and other strategic behaviors affect the political relevance of emotional phenomena. If it seems incorrect to discuss strategy, incentives, and emotions in the same sentence and to present their relationship as endogenous, we ask you to consider the following facts. To be certain, emotional responses have a strong subconscious element. There are aspects of emotional response over which we have no control. So we can agree that some aspects of emotional response are beyond the domain of goal-oriented learning. We can also agree that emotions are not applied entirely at random. Were this true, we could not accurately anticipate the emotional responses of others. But we can anticipate others' emotions—in their presence and in their absence—precisely because we come to learn that emotional responses have a contingent element. For example, what we fear depends in part on what we have experienced in the past and in part on where we are at the moment. Two manifestations of this contingency are that we learn to fear some things in our environment and that we can learn to suppress other fears. As Damasio

(1994, 130) states. "In many circumstances in our life as social beings, however, we know that our emotions are triggered only after an evaluative, voluntary, nonautomatic mental process. Because of the nature of our experience, a broad range of stimuli and situations has become associated with those stimuli which are innately set to cause emotions. The reaction to that broad range of stimuli and situations can be filtered by an interposed mindful evaluation."

Learning, in turn, can be affected by goal orientation. We are more likely to pay attention to and remember certain stimuli if we believe or observe that such attention correlates highly with our ability to achieve a particular goal. As Cacioppo and Gardner (1999, 199) state, "motivational strength increases as the distance from a desired or undesired endstate decreases." As a result, the conditions under which emotions affect some political interactions will be a function of incentives. Responses to such incentives, in turn, will be governed by a mix of conscious and subconscious phenomena. In some situations, the stimuli, incentives, learning algorithms, and subconscious activities will settle into a steady state—by which we mean that the relations between these phenomena will be predictable. Such steady states are what game theorists call equilibria, and they use these equilibria to characterize conditions under which some factors affect others. Therefore, the applicability of research concerning emotions and politics can be improved by examining the conditions under which emotional phenomena play particular roles in such steady-state relations.

The difference between our approach and that of current scholars of emotion can be seen with respect to the following conclusion (Marcus et al. 2000, 63–64): "Anxious voters will, in most instances, act very much like the rational voters as depicted by theories of public choice. However, when complacent, voters will in most instances look very much like the value protecting voters depicted by the theory of symbolic politics." We believe that this conclusion is true but only under certain conditions—conditions that are affected by strategic phenomena. Our point of departure is the premise that there is anything approaching a one-to-one correspondence between strategic, goal-oriented behavior and heightened anxiety. We consider such a relation very unlikely for the following reasons.

We know that emotions such as anxiety can cause a range of reactions. Anxiety can make some voters very attentive to environmental stimuli. It can, as Marcus and his colleagues describe, induce people to commit substantial mental energy to surveillance of their surroundings. We also know, however, that there are people for whom anxiety triggers with-

drawal. If improved applicability is the standard against which we want to measure scientific progress, then game theory can be a powerful ally because it can help us understand the conditions under which each kind of outcome occurs.

But can research that integrates game-theoretic and emotion-based insights provide unique insights? We now address that point directly. Above, we built an argument about how attention can improve game theory by working from several criticisms of the ways in which game theorists draw inferences. The point was not to diminish the game theorists' effort but to point out places in the method where insights from the study of emotions can be most useful. We now construct the reverse argument. We offer a critique of the methods by which recent insights about emotions and politics have been generated—a kind of critique that is more likely to be offered by people who are not political psychologists—as a way of detailing how and why game-theoretic insights can help emotion-oriented scholars better achieve some of their primary objectives.

We can all agree that the post-Damasio literature in political science has been insightful. We can also agree that the insights come primarily from three kinds of studies: laboratory experiments, survey experiments, and general election surveys. The extent to which findings from such methods provide reliable inferences about the role of emotions in particular political situations can be questioned.

The main critique of laboratory experiments is well known: most are run using convenience samples of undergraduates or of residents of the communities in which political psychologists' primary employers are located. Questions arise about the extent to which student responses to focal stimuli accurately represent how others in the population would react.¹ Questions also arise about the extent to which stimuli presented in the laboratory environment evoke the same kinds of reactions that they would evoke in the same subjects in the field.

Because survey experiments are used less frequently in the study of emotions, the critiques against it are less familiar. These experiments are

1. We refrain from using the term *external validity* in this context. Like *rationality*, it is a term that means very different things to different people. For us, a claim about validation is most useful when it is anchored on a well-defined standard. Such a standard allows transparent and replicable comparisons. Our experience, however, is that many claims of external validity are of not of this kind. Instead, they are offered without clear reference and as the basis for a blanket rejection of experimental methods. Such critics may indeed have in mind a concrete standard that many experimental studies do not surpass, but the standard is seldom stated explicitly, which means that the validity claims themselves are difficult or impossible to validate.

typically housed within large surveys. Subjects in many prominent survey experiments are recruited via a process called random-digit dialing, a technique that leading survey houses use to acquire nationally representative subject pools in a cost-effective manner. When a survey house succeeds in acquiring a representative sample, survey experiments do not face the “convenience sample” critique. But because such experiments are usually conducted over the telephone or via the Internet, concerns about the extent to which reactions parallel those that researchers would see in less contrived circumstances remain.

Important insights about emotions in politics have also been drawn from nonexperimental components of national surveys. Such studies sample the perspectives of a broader range of people than do most laboratory experiments and, thus, provide shelter from the convenience sample critiques. The book *Affective Intelligence and Political Judgment*, for example, makes extensive use of the American National Election Studies (ANES). The ANES is valuable in many ways and has been used to refine the scientific understanding of numerous concepts and relationships. Yet several attributes of such studies stack the deck against their ability to serve as the basis for strong causal claims about emotions. Unlike experiments, the survey is designed and questions are written to serve many purposes. To date, few if any questions on these general-purpose surveys have been written to obtain the best possible measures of emotion-related concepts or with a specific emotion-related hypothesis in mind. Moreover, of the survey questions used in emotion-related research, most are based on self-reports of mental states. Across the scientific community, there is a general agreement that such reports are of questionable reliability, as Churchland (1995, 22) explains: “Humans are famously bad at describing their sensations—of tastes, of aromas, of feelings—but we are famously good at discriminating, enjoying, and suffering them. . . . And yet, while we all participate in the richness of sensory life, we struggle to communicate to others all but its coarsest features. Our capacity for verbal description comes nowhere near our capacity for sensory discrimination.” Davidson (2003, 131) draws a similar conclusion, though in a different way:

Much of the psychological literature on emotion implicitly assumes that emotions are conscious feeling states. A vast number of studies depend upon self-report measures to make inferences about the presence of emotional states. Such self-report measures are often outcome variables in studies on emotion and they often serve as “manipulation checks” to confirm the presence of an intended emotional manipulation. Failure to

find detectable change on self-report measures is sometimes offered as evidence that emotion was not elicited, and more frequently, the presence of self-reported emotion is taken as evidence that emotion has been activated. While the experiential side of emotion is unquestionably important and provides useful information to an individual that can be harnessed for adaptive functioning (e.g., Damasio 1994), it is also clear that much of the affect that we generate is likely to be non-conscious.

Such skepticism is shared in ongoing research about the kinds of personal attributes about which people can report reliably in a survey (see, for example, Tourangeau et al. 2000, chaps. 3, 6, 11.3) and work concerning the ways in which conventional survey interviewing techniques sometimes produce atypical and unrepresentative responses (Schwarz 1994).

Game theory does not solve these potential problems of experimentation or survey-based research, but if applied effectively, it can limit the critiques' impact. Scholars can use game theory to clarify the conditions under which empirical findings are robust to other circumstances—at least circumstances that can be represented in theoretical terms. Although many kinds of theorizing can play this role for scholars who want to generalize from empirical observations, game theory is particularly helpful when asking questions about the robustness of such observations to contexts in which strategic interaction matters.²

In most cases, when scholars conduct experiments, use surveys, or build theories, simplifying assumptions are required to produce findings that can be analyzed compactly. As a result, scholars who want their work to be broadly relevant are forced to make an “inductive leap” that projects their findings from a particular scientific context to a larger social domain. Different kinds of inference require different kinds of leaps (for example, a theorist needs to project in ways that an experimentalist does not). An advantage of research designs that integrate theoretical and empirical methods is that, in some cases, the inferential advantages of one approach can be used to counter the known limitations of other approaches. In political contexts where strategic behavior is not only possible but is witnessed with regularity, game theory can serve a supporting role for those performing studies of emotions and politics, whether experimental or survey-based. If properly developed, it permits strong,

2. Lupia and McCubbins (1998, chap. 6), for example, draw one-to-one comparisons between attributes of their formal models and attributes of their laboratory experiments to clarify what parts of the theory can be used to evaluate the generalizability of their empirical findings.

transparent, and logically coherent statements about the conditions that determine how and when potentially important emotional phenomena apply to politics.

CAN AN INTEGRATED APPROACH WORK? AN EXAMPLE

The link between emotions and game theory is just beginning to be explored. These initial explorations reveal important variations in emotional responses to different strategies. They also show how strategies depend on emotions.

A particularly promising set of activities can be found in economic-style experiments. Like experiments in psychology and other social sciences, these experiments gain their inferential power from the combination of experimental control and random assignment. They differ from other kinds of social science experiments, however, in that subjects are typically compensated on the basis of their performance during the experiment. In some cases, they are simply paid for choices they make. In other cases, and particularly in experiments motivated by game theory, subjects' compensation depends on the outcome of a game—their pay depends not only on what they do but also on the actions of others. Though promising in many respects, economic experiments do not provide a panacea when it comes to the study of emotions. Attempts to draw inferences from self-reports of emotional states or observed behaviors entail many of the same problems that vex other empirical researchers (as described above). Recently, innovative experimental designs, offered in the name of neuroeconomics, shed new light on the interaction between emotions and strategic behavior. They did so by getting around the self-reporting problem in a clever way.

Sanfey et al. (2003) use functional magnetic response imaging to track reactions of ultimatum game players at the level of the neural substrate. Ultimatum games are of interest to game theorists because early experiments showed that subjects play them in ways that the standard Nash equilibrium concept would not predict (see also McKelvey and Palfrey 1992, 1995 for a parallel demonstration on the centipede game). An ultimatum game involves two players and a finite sum of money, say, \$10. One player is given the responsibility of proposing a division of the money. The other player can only accept or reject the offer. If the second player accepts the first player's offer, the players are paid accordingly. If the second player rejects the offer, both players get nothing. Using the Nash equilibrium concept, the prediction of the game is that the first player will propose to keep almost all of the money and that the second

player, faced with a choice between being paid a small amount and being paid nothing, will accept. In laboratory experiments, however, the game is played differently. Many subjects propose a split closer to 50-50 than to the “almost everything for me” proposal predicted by Nash equilibrium concept. Moreover, subjects who are asked to play the role of the second player often reject offers that deviate from 50-50 although, strictly speaking, the rejection leaves them worse off financially (see, for example, Frohlich and Oppenheimer 2000).

Although many alternative explanations have been offered for such behaviors, Sanfey (2003, 1756) and his colleagues hypothesize that unfair offers (that is, those diverging from a 50-50 split) “would engage neural structures involved in both emotional and cognitive processing, and that the magnitude of activation in these structures might explain variance in the subsequent decision to accept or reject these offers.” To test this hypothesis, they had subjects play identical versions of an ultimatum game against human and computer partners. In their experiment, “[p]articipants accepted all fair offers, with decreasing acceptance rates as the offers became less fair” (ibid.; their null hypothesis was rejected at the 0.03 level or smaller). More interesting, however, were the underlying neuronal patterns. They found that “[a]mong the areas showing greater activation for fair compared with unfair offers from human partners were bilateral anterior insula[, which is] particularly interesting in light of this region’s oft-noted association with negative emotional states.”

An important part of this finding is its distinction from subject reactions when the computer rather than the human player made exactly the same offers. Sanfey and colleagues find that “[t]he magnitude of activation was also significantly greater for unfair offers from human partners as compared to . . . unfair offers from computer partners” (2003, 1756; the null hypothesis was rejected at the 0.03 level or smaller). This result shows that subjects’ emotional reactions to the play of the game were a function not of the payoff, because subjects did not respond *emotionally* when the computer made an unfair proposal. The extreme emotional reactions only followed unfair proposals from other players.

Moreover, the converse was also true—subjects’ play was a function of their emotional responses. This relation is demonstrated by the fact that even after accounting for variations in the size of unfair offers, “participants with strong anterior insula activation to unfair offers rejected a higher proportion of these offers” (Sanfey 2003, 1756–57; the null hypothesis was rejected at the .05 level or smaller). Studies such as this inform game theory, improving our understanding of the conditions under which the well-known Nash equilibrium solution applies. These

experiments, in other words, improve the applicability of the theoretical result. They also demonstrate that the relevance of subjects' emotional responses are conditional on strategic factors, including the extent to which the offer is perceived as unfair, and on whether they believe that such offers come from humans or from computer programs. In sum, in the Sanfey research agenda the empirical study of emotions and a game-theoretic representation of a social situation combine to provide superior insights about the relation between emotion and strategic behavior.

**CAN AN INTEGRATED APPROACH WORK?
POSSIBILITIES FOR THE FUTURE**

If a future generation of emotion-rich game-theoretic models comes to pass, their effectiveness will depend on the connection between the model's assumptions and the physical basis and mechanics of human cognition. Several of the contributions to *The Affect Effect* address the topic of emotions at this level and, in our view, provide potentially innovative starting points for scholars who want to better understand the conditions under which particular emotional phenomena apply to politics.

Consider, for example, Darren Schreiber's "Political Cognition as Social Cognition: Are We All Political Sophisticates?" (chapter 3 in this volume). Schreiber builds an argument about how political actors think about politics from empirical research on the properties of "mirror neurons." Mirror neurons facilitate mental representations of how other people think. They allow us to imagine how other people will respond. They also affect the extent to which we experience particular emotional responses simply by watching others have those responses. In game theory, premises about what people think about other people and how they expect them to respond play important roles. Once more is learned about the properties of mirror neurons, in particular the kinds of interpersonal attributes that these neurons are most likely to record and subsequently reduce in searches of memory, they could serve as a basis for analyses of strategic interaction that are more cognitively realistic. To this end, some game theorists have questioned the use of popular solutions concepts such as the Nash and the Bayesian-Nash concepts because of the rather severe assumptions they can imply about the quality of counterfactual reasoning. As mentioned above, alternative equilibrium concepts such as the self-confirming equilibrium concept have been proposed. A limitation of the concepts, however, is that they tend not to be psychologically rich. The self-confirming equilibrium concept, for example, allows for an expanded range of counterfactual assessments to be introduced into

game-theoretic reasoning, but it does not provide a standard for choosing among the counterfactuals people can and do run. Understanding more about the properties of structures such as mirror neurons may provide a more credible basis for such models in the future.

Another constructive possibility is apparent in the contribution of Spezio and Adolphs (chapter 4 in this volume). One of our chief concerns in this chapter has been to improve the applicability of empirical research about emotions. A key step in achieving such progress is a better understanding of the conditions under which emotions affect politics. Spezio and Adolphs provide a helpful way to construct such arguments through their use of the Yerkes-Dodson law. This law portrays goal-oriented decision makers as more effective when experiencing moderate levels of emotion than when very emotional or not emotional at all. This view is obviously helpful when thinking about a decision theoretic task in which the consequence of a person's decision depends only on that decision. We contend that it can also be helpful in more strategic situations. We can imagine cases where players vary in the extent to which they are affected by emotions. In a game featuring multiple players, the impact of emotions would depend on the distribution of emotions among the players. Two low-emotion players playing a game with one another could produce a different collective outcome than would occur if one low-emotion played the same game with a moderately emotional or highly emotional player. In such a case, we would expect the political consequence of emotions to be determined not only by the emotional status of any one person but also by interactions between the goal orientations and emotional states of multiple players.

CONCLUSION

Some scholars believe that emotions and reason are independent. Others believe that emotions and strategic decision making are mutually irrelevant. Both beliefs may have had credibility at one point, but now that studies of the brain at several levels of analysis reveal deep connections among emotion, reason, and strategic thinking, such hypotheses are no longer credible.

Emotions play a significant role in the outcome of our political processes. But so does strategic interaction between various political actors. Researchers who focus solely on one of these aspects can teach us many things. But researchers who integrate both kinds of insights can teach us much more. By endogenizing and strategizing emotional reactions to politics, a game-theoretic approach can clarify the conditions under which

politicians should seek to evoke fear among voters and the conditions under which people will respond to emotional appeals. To the extent that this approach increases our ability to connect our knowledge about the underpinnings of emotional life to real-world political phenomena, this union of game-theoretic principles and emotional research is beneficial.