

Research Statement – Roberto Weber

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I. Introduction

Described broadly, my research consists of behavioral and experimental studies of decision-making in economic and organizational contexts, frequently using economic games to study interaction. My work lies at the intersection of economics, game theory, organizational behavior, business strategy, and political science.

One important goal driving my research is to develop novel laboratory paradigms that bring experiments closer to how we observe economic and organizational phenomena in the real world. Thus, I often attempt to find ways to introduce potentially important features of naturally-occurring economic, organizational, and social environments into laboratory experiments, in order to study how such features influence decisions and outcomes. I also strive to incorporate insights from psychology and other social sciences into the kinds of simple environments studied by economists.

This approach has produced several important research contributions. My work appears in leading journals in economics (Weber, 2006, *American Economic Review*; Hamman, Lowenstein & Weber, forthcoming, *American Economic Review*; Kogan, Kwasnica & Weber, forthcoming, *American Economic Review*), organizational behavior (Weber, Camerer, Rottenstreich & Knez, 2001, *Organization Science*), management (Weber & Camerer, 2003, *Management Science*), information technology (Hsieh, Kraut, Hudson & Weber, 2008, *Proceedings of Computer Supported Cooperative Work*), and game theory (Weber, 2003, *Games and Economic Behavior*; Rick & Weber, forthcoming, *Games and Economic Behavior*).

In what follows, I describe some of my contributions and briefly discuss my on-going research. I divide my contributions into three broad areas: a) organizations and institutions, b) behavioral game theory, and c) social preferences and behavior. In each area, my research primarily studies how the collective actions of multiple individuals interact to yield social and organizational outcomes.

II. Organizations and institutions

A significant part of my work deals with organizations, the phenomena that tend to occur within them, and the functioning of related social systems such as political institutions. As with most of my research, my work in this area often involves experiments that bring complex organizational or political phenomena into controlled laboratory settings in order to study precisely how such phenomena occur or what drives them.

1. Summary of previous work

Much of my research in this area deals with the coordination of activity among interdependent actors. Coordination is important in economic and organizational contexts, and my work identifies features of a situation that make it easy or hard to achieve efficient coordination. For instance, some of my research explores the influence of leadership in coordinating activity and how situational features can influence the effectiveness of leaders (Weber, Camerer, Rottenstreich & Knez, 2001; Kuang, Weber & Dana, 2007). Other research explores coordination problems that arise when merging organizations or integrating new entrants (Weber & Camerer, 2003; Weber, 2006; Salmon & Weber, 2009). Additionally, I also explore how coordination problems are influenced by factors such as incentives and

communication (Weber, Rick & Hamman, 2006; Kogan, Kwasnica & Weber, forthcoming; Blume, Kriss & Weber, 2009; Hsieh, Kraut, Hudson & Weber, 2008).

I also study organizational topics other than coordination. For instance, one of my papers explores the phenomenon of escalation of commitment, or chasing sunk costs, among managers in the National Basketball Association (Camerer & Weber, 1999). Other research explores the characteristics of trust within organizations (McEvily, Weber, Bicchieri & Ho, 2006).

Finally, I also study the functioning of political institutions and the behavior of individuals within those institutions. For instance, I have examined the formation of voter beliefs (Glasgow & Weber, 2005; Patty & Weber, 2007) and the decision-making processes of political institutions (Patty & Weber, 2006). In another recent paper, I examine how delegated democracy can help solve free-rider problems (Hamman, Weber & Woon, 2009).

2. Illustrative example

One of my most important papers in this area explores the relationship between firm size – or the size of any social collective – and coordination problems (Weber, 2006). Many previous experiments document a strong negative effect of group size on the ability of group members to coordinate their activity (Van Huyck et al., 1990; Weber et al., 2001). This raises a puzzle concerning why we observe large, efficiently-coordinated firms in the real world when they are so difficult to obtain in the laboratory. I show that the growth process a firm undergoes is critical to determining the extent to which that firm is able to maintain efficient coordination. Firms start small, when solving coordination problems is easy. The way in which they grow can allow them to add new members, while maintaining the efficient coordination they were able to obtain when small. If firms grow too quickly or if they fail to provide new entrants with adequate information about firm practices, then they are likely to experience coordination problems.

This finding is important, as it represents a very rare instance in which several large laboratory groups are able to independently obtain efficient coordination, and it presents a possible mechanism whereby the puzzle of large-group coordination failure might be solved. In more recent related work (Salmon & Weber, 2009), I explore alternative methods for integrating new entrants.

3. On-going research

I have several current projects dealing with the study of organizations and political institutions. I present two examples.

Determinants of effective leadership. Building on my prior work on leadership, I am currently pursuing two projects that explore the determinants of a leader's effectiveness in obtaining coordination. One project (with Jordi Brandts and David Cooper) explores how the process of selecting a leader influences that leader's credibility among followers and therefore the leader's effectiveness. A second project (with David Cooper and John Hamman) explores how a leader manages her credibility, in order to have followers continue to respond to her directives.

Costly communication and coordination. Prior research suggests that communication is an effective means for solving coordination problems (e.g., Devetag & Ortmann, 2007). However, this previous research uses communication in which everyone communicates and communicating is costless. In on-going research (with Andreas Blume and Peter Kriss), we explore the effectiveness of communication that is both costly and voluntary, as is often the case in real organizations. In a first paper (Blume, Kriss & Weber, 2009), we find that introducing

even trivial communication costs into a two-player game changes both the extent to which communication is used and its effectiveness for solving coordination problems. Surprisingly, we find that tacit coordination becomes much easier once communication is costly. We are currently extending our work to games with many players, to better understand how costly communication influences outcomes in organizations.

III. Behavioral game theory

I am also interested in game theory and how we can use the laboratory to test its theoretical assumptions and implications. A significant portion of my work deals with these kinds of tests.

1. Summary of previous work

Most of my work in this area identifies important assumptions in game-theoretic models, and then conducts laboratory tests of these assumptions. For instance, one paper shows that behavior is influenced by payoff magnitudes, which traditional theory assumes do not affect behavior (McKelvey, Palfrey & Weber, 2000). Another paper explores the assumption of common knowledge of rationality, and shows that it is not satisfied even in a setting where players want everyone to be rational (Weber, 2001). A third paper explores the extent to which changing the timing of moves matters in games, even when it is not accompanied by changes in information (Weber, Camerer & Knez, 2004). In all three cases, the results of experiments prove informative about the descriptive shortcomings of traditional theory, and also shed light on how theory might be improved.

2. Illustrative example

One of my main contributions to behavioral game theory is on the topic of learning. In two experimental papers (Weber, 2003; Rick & Weber, forthcoming), I explore a kind of learning that has been relatively understudied in economics. This more profound and cognitive form of learning involves meaningful acquisition of deeper knowledge that transfers more easily to novel situations.

In the 2003 paper, I demonstrate that people learn – that is, they adjust their behavior in the direction of Nash equilibrium – when playing games repeatedly without *any feedback*. Since most models of learning in games rely on a history of outcomes to produce learning, this evidence suggests a kind of learning not present in current models.

The second, forthcoming, paper further explores this alternative kind of learning. This paper begins by noting that the learning that occurs without feedback is likely similar to a kind of learning long-studied by psychologists, which involves greater cognition and a more profound understanding of concepts and meaning. This paper tests for this deeper kind of learning, and particularly for the extent to which it transfers to new games. Again using the paradigm in which subjects play games repeatedly without feedback, three experiments demonstrate that feedback-free repetition generates the more meaningful kind of learning in several different games. Perhaps most importantly, this kind of learning transfers to new games, even when feedback-based learning does not. The experiments in this paper represent one of the very few instances in which subjects regularly demonstrate transfer of learning across games. Moreover, we demonstrate that when subjects play a first game with feedback they fail to transfer what they've learned to a new game, suggesting that feedback-free learning does indeed produce something that is more meaningful.

The work in this second paper is particularly important, because it introduces to game theory a distinction between kinds of learning that has long been recognized by psychologists, and that has important implications for subsequent behavior.

3. On-going research

My current research continues to pursue important issues in the sub-field of behavioral game theory. I present three examples.

Strategic sophistication in games. A growing literature in game theory demonstrates that simple models of heterogeneously sophisticated players explain behavior in laboratory experiments (e.g., Costa-Gomes & Crawford, 2006). Such models assume that players' types correspond to how deeply they think about iterated best response, and thus make an implicit assumption about some underlying characteristic: strategic sophistication. In this project (with P.J. Healy and Sotiris Georganas), we explore the extent to which this strategic sophistication demonstrates stability across contexts. We find that it is generally unstable when one considers behavior across different types of games.

Time delays in strategic interaction. Following from my general interest in introducing realistic features into traditional experimental paradigms, I am currently exploring the effects of time delays in behavior in games. For convenience, laboratory strategic interaction usually proceeds very quickly, with players' decisions separated only by a matter of seconds, or at most minutes. However, real interaction often involves much lengthier delays. In one project (with Cleotilde Gonzalez and Wei-Siong Neo), we find that delays significantly affect behavior in ultimatum games but not in investment games.

Delay and randomness in punishment. Related to the work on time delays, I am interested in what happens to the effectiveness of punishment for inducing "good" behavior, as the punishment itself becomes slower and less certain. Several experiments demonstrate that punishment of non-cooperators is effective at inducing cooperative behavior (Fehr & Gächter, 2000). But an important question for moving such punishment to the real world involves the extent to which it continues to be effective when realistic features are added. Real-world punishment is often delayed by months or even years, may not always end up being received by those for whom it is intended, or may also affect innocent others. In two projects at early stages (with George Loewenstein and with Pablo Brañas-Garza and Fillipos Exadaktylos), we plan on exploring the introduction of such realistic features into punishment, to determine the conditions under which it is effective.

IV. Improved understanding of social preferences and behavior

In many economic, organizational, and policy domains, it is important to understand what makes people sacrifice self-interest for others' welfare. My work adds to this literature by introducing and demonstrating that much pro-social behavior results not from a preference for fair or equitable outcomes or behavior, but instead from individuals feeling compelled to comply with social norms that may vary across contexts. This is important, because it tells us that people often behave fairly not because they want to, but because they feel obligated to do so.

1. Summary of previous work

Much of my work in this area demonstrates that people who behave fairly in one context are often willing to seek out and rely on justifications for behaving self-interestedly in other almost identical contexts. My work also identifies several such justifications and therefore helps

shed light on the conditions under which people will and will not behave pro-socially. For instance, my work shows that, in order to behave self-interestedly, people often rely on uncertainty and ambiguity (Dana, Weber & Kuang, 2007; Haisley & Weber, forthcoming), the possibility to select into or out of environments (Lazear, Malmendier & Weber, 2009), and the ability to delegate agency to a third party (Hamman, Weber & Loewenstein, forthcoming) as justifications to avoid moral responsibility.

In a related line of research, I explore the influence of social norms on behavior. This work seeks to identify precisely how norms influence behavior, develop a model of these influences, and create a method for measuring norms that is applicable both in the laboratory and in naturally-occurring contexts. A first paper (Krupka & Weber, 2009a), builds on work in psychology to show that norms only affect behavior if an individual's attention has been drawn to the norm. This "focusing" influence yields counter-intuitive predictions, which we demonstrate in an experiment. In more recent work, also with Erin Krupka, I develop a method for measuring norms based on the extent to which people share agreement on the appropriateness or inappropriateness of given behaviors. We then show that such norms explain a considerable amount of behavior in experiments involving altruism (Krupka & Weber, 2009b), as well as in naturally-occurring behaviors such as tipping (Croson, Krupka & Weber, 2009).

2. Illustrative example

Dana, Weber and Kuang (2007) introduces the notion that people who appear to value fairness in one context will behave self-interestedly in other instances by relying on contextual justifications to do so. We term this phenomenon "moral wiggling," whereby people seek excuses to circumvent social obligations that they would prefer not to fulfill.

We first identify an environment in which a large majority of people behave pro-socially, at a personal cost, similarly to experiments on the well-known "dictator game." However, we then show that in almost identical contexts a majority of people behave self-interestedly. In each case, the new contexts are obtained by adding a feature to the baseline case that superficially obscures the relationship between actions and outcomes – so that a subject can justify to herself or to others that the bad social outcome was not really her fault. For example, in one modification we place subjects in a state where they are unaware of whether their self-interested behavior will help or harm others. Even though this uncertainty can be resolved at no cost, we find that almost half of subjects choose not to do so in order to behave self-interestedly.

This work is important, as it introduced a new way of thinking about the motivations for social behavior, in contrast with previous research that assumed that people behaved fairly because they valued doing so. Much of my subsequent work in this area proceeded by both demonstrating other ways in which people engage in moral wiggling, and by attempting to provide an improved understanding of why it occurs.

3. On-going research

Self-serving perceptions of others' risk. People often need to consider the extent to which they want to help insure others against losses. In a standard subjective utility framework with social preferences, the extent to which individuals care about others' probabilistic losses should be equivalent to how much they care about certain losses times the subjective probability they assign to the loss. My current research, with a Ph.D. student (Nathaniel Peterson), questions this assumption by eliciting subjective beliefs for probabilistic losses for self and others. We find

that people act as if the subjective probabilities of others' losses are lower than for themselves, even when the objective probabilities are fixed and known.

Delegation and social behavior. Building on my prior work showing that people behave unfairly when making choices through agents or delegates, I am exploring further how delegation plays a role in fair or pro-social behavior. In one project (with John Hamman and Jonathan Woon), we explore how delegation to elected leaders can help solve the free-rider problem. In another project (with Amy Chow, John Hamman and Ronald King), we explore how unfair behavior through delegates can be mitigated by institutional rules or features.

Affective influences on charitable behavior. In recent work (with Cynthia Cryder), we use a large dataset from a university telephone fundraising campaign, matched to local weather data for the recipient of the call. We find that weather effects previously linked to mood – in particular the degree of sunshine experienced that day – have large and significant effects on the amount of donations.

IV. Conclusion and other contributions to research

Finally, in addition to my original research projects and publications, I have contributed significantly to the synthesis and dissemination of knowledge in the above research areas. For instance, I have co-organized three conferences: the 2003 Economic Science Association Meetings in Pittsburgh, a 2004 conference on learning held at Carnegie Mellon, and the upcoming 2010 Behavioral Decision Research in Management Conference in Pittsburgh. I also have served editorial functions at several journals: I co-edited an issue of *Experimental Economics*, am currently on the editorial review boards for *Organizational Behavior and Human Decision Processes* and *Organization Science*, and serve as an associate editor for *Management Science* and the *Journal of the European Economic Association*. Finally, I co-wrote a chapter on behavioral economics for the *Handbook of Economic Sociology* (Weber & Dawes, 2005) and am currently co-writing book chapters for the upcoming *Handbook of Organizational Economics* and for the *Encyclopedia of the Sciences of Learning*.

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