PSYCHOLOGY AND ECONOMICS

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This is somewhat opinionated and out of date
in terms of what I now know and believe.
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I. Introduction

Before I attended my first professional psychology conference, a friend warned me that I would be surprised by the extent to which psychologists walk around talking about economics as being unrealistic and incorrect. These criticisms, which we also hear from our students, have had little impact on thinking in the economics profession. Except for an occasional reference to Keynes' "animal spirits," there is not much discussion of psychology in the hallways of this conference hotel. This asymmetry is equally astonishing to psychologists who believe that their experiments should be provoking a reevaluation in the economics profession. I do not believe that this situation is a one-sided conversation leading nowhere. Laboratory experiments done by a small minority of *economists* are finally having some impact on mainstream economic theory and policy. In addition, there is potential for much more interaction between experimental economics and psychology. This paper will discuss the kinds of things that economists can learn from psychology, and some ways in which psychologists can do a better job of developing and communicating results that economists will find useful.

Differences in methodology and perspective are major impediments to any conversation across disciplines, and therefore the paper will begin with a discussion of these differences. The various roles of theory are discussed in section II. Section III pertains to differences in the ultimate goals and focus of the two disciplines. Experimental procedures are discussed in section IV. The final section contains a conclusion.

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* Department of Economics, University of Virginia. This paper was presented at the 1995 ASSA Meetings in Washington, D.C., in a session organized by the Robin Cantor and Hal Arkes, program officers for the NSF program in Decision, Risk, and Management Science. Needless to say, the views expressed here do not necessarily (and often do not) correspond to those of NSF program officers and other panel members. My thoughts on several of the issues discussed here have been shaped by discussions with William Johnson.

II. The Role of Theory

Economics is unique among the social sciences in having a unified core of theory. This theoretical framework formalizes and generalizes insights that can be traced to Adam Smith, Cournot, and others. Models of the equilibrium interaction of maximizing agents are used in applications that range from economic development to finance. Much of the empirical work in economics can be characterized as the estimation of parameters of models that are special cases of the more general theories. The econometric techniques are primarily designed to deal with problems that arise with *non-laboratory* economic data: selection and simultaneous-equation biases, missing observations, measurement errors, unspecified functional forms, etc. In this manner, theories are tested in conjunction with lots of auxiliary assumptions about functional forms, error distributions, etc. The result is that few theories are decisively rejected with data from naturally occurring economic markets. Instead, theories are often evaluated on the basis of elegance, internal consistency, and the intuitive plausibility of the results.[1] As Kreps (1990, p.8) noted in one of the leading graduate theory books: "There is something of a 'market test' here: one's ability to convince others of one's personal intuitive insights arising from specific models."

The dramatic increase in the use of game theory and models with asymmetric information has produced many subtle developments and refinements that simply cannot be distinguished with data from naturally occurring markets. As a result, theorists and empiricists have become even more isolated from each other.[2] The material the textbooks has become increasingly theoretical. In the absence of empirical discipline, the theoretical refinements in the journals have become

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[1] Some of the names attached to theoretical concepts are illustrative: the "intuitive criterion" (Cho and Kreps, 1986), the "perfect sequential equilibrium" (Grossman and Perry, 1986), and the "divine equilibrium" (Banks and Sobel, 1987).

[2] For example, Tim Bresnahan (1993) in a recent conference address noted that the game-theoretic models of entry deterrence and strategic competition have had little impact on empirical work in industrial organization economics. He essentially said that it is fine for theorists to figure out what happens in all of the variations of these models; that this is what theorists should do, but that empirical researchers should be concerned with estimating cross elasticities of demand, etc. that have important implications for public policy. Similarly, theorists in industrial organization and other subfields have tended to ignore the empirical literature. Many of the most exciting theoretical advances have been in game theory, and very few of these developments have been evaluated with market data. Today graduate courses in industrial organization, for example, are likely to be organized around Tirole's (1988) theoretical book, which replaced the much more descriptive and empirical books used earlier.

so abstract and complex that they routinely draw ridicule from outsiders.

My impression is that psychological studies of decision making are primarily experimental, and in many cases with only loose connections to what an economist would consider to be theory (more on this below). This may explain why economists have not noticed these results. Keynes, Samuelson, and others prefaced their influential books with remarks about the impossibility of conducting controlled experiments in economics. Chamberlin (1948) conducted the first market experiment, but the results were barely mentioned in a footnote in subsequent editions of his classic *Theory of Monopolistic Competition.* The *Journal of Economic Literature* did not list a separate category for experimental economics until 1986, and even today, only about 2% of empirical research in economics is experimental (Heck and Zaleski, 1991). This statistic probably overstates the impact of experimentation because there are no active economics laboratories at the major Ivy League, West-coast, and upper-Midwest departments that train the graduate students who obtain most of the top teaching and research positions.

I have heard it said that economic theory is in a stagnant phase; the exciting developments that arose from rational expectations, applied game theory, and information economics have pretty much played out.[3] There is a disconcerting array of alternative theoretical developments that have not been subjected to tough empirical testing. I believe that experimental work by economists and psychologists (much of which is funded by the NSF) can have a very large impact in the coming years. Experimental economics papers already have a disproportionately large presence in the top economics journals.[4] In contrast, the work of experimental psychologists often misses the mark because they choose the wrong audience and and the wrong way of presenting their results.

First, consider the audience. The dominance of the maximizing/equilibrium paradigm of economics has been challenged by many groups of economists. In addition, there is always a political demand at the university level for arguments that "sciences" really are not as scientific

[3] These observations are due to Andy Schotter of NYU.

[4] Experimental economists are concentrated in the U.S. and Europe. About 130 experimentalists attend the fall meetings of the Economic Science Association, and a somewhat smaller group attend the spring meetings, joint with the Public Choice Society. For details, contact the current president, Robert Forsythe, at: Department of Economics, University of Iowa, Iowa City, 52245, or by e-mail: rforsyth@scout-po.biz.uiowa.edu.

as they pretend to be. Dissenting groups (e.g., radical, rhetorical, and Austrian economists) are actually very marginal in terms of their following and impact on the profession, and as such, are the wrong audience. Attacks in the popular press, which are almost always on a superficial level, just reduce the chances of any productive interaction. Psychologists are most likely to read related papers by experimental economists, who can sometimes be perceived as defenders of established theory, but this is also an inappropriate and relatively unimportant target. Experimental economists are basically sympathetic to the general approach taken by psychologists, despite major methodological differences discussed below. It is the powerful economic theorists who have the most use for results of carefully designed experiments.

Next, consider the presentation of results. The typical psychology paper on decision making will characterize the results as being inconsistent with maximizing models of choice and conclude that the maximization-based theories are simply incorrect. The qualitative features of the discrepancy are sometimes presented as an alternative theory. Yet it is often the case that maximization theories predict well in sufficiently simple contexts.[5] Then it is the alternative theory of biased behavior fails in simple cases. This line of research is much more convincing to economists if there is a model of behavior that explains both the rational choices in transparent situations and the biases in richer environments. Lacking this, a better way to proceed may be to present the anomalous behavior patterns as "empirical regularities" that serve as data to challenge and guide further theoretical work. I do not mean to imply that psychologists do not come up with what economists would call theories. (McFadden’s widely used logit model is a generalization of the discrete choice theory developed by Luce (1959), a mathematical psychologist.) All that I am suggesting here is a little care in the use of the word theory, which raises the question of what economists really mean by good theory.

It is useful to begin with a specific example, the theory of "melioration" proposed by psychologists Herrnstein and Mazur (1987). This theory essentially states that individuals adjust decision variables to equate average payoffs, instead of using marginal payoffs as implied by

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[5] Shafir, Simonson, and Tversky (forthcoming) note: "Indeed, it has been repeatedly observed that the axioms of rational choice which are often violated in nontransparent situations are generally satisfied when their application is transparent (e.g., Tversky and Kahneman, 1986). There are many exceptions to this statement. The compromise and attraction effects, discussed in Simonson (1989), show up in very simple contexts.

maximization (marginal payoffs are equated in an interior solution with a continuous choice variable). For example, suppose you have $3 which can be invested in either project A or project B, with a gross payout indicated:

dollars invested in project A or B:	$0	$1	$2	$3
dollars returned from project A:	$0	$1	$2	$3
dollars returned from project B:	$0	$1.50	$2	$2.50

Notice that project A has a constant average and marginal product of 1; it returns the amount invested. Project B has marginal return of 1.50 for the first dollar invested and of only .50 for the second and third dollars invested. Therefore, the optimal decision is to put $2 in A and $1 in B, which raises final wealth to $2 + $1.50 = $3.50.[6] In contrast, melioration requires that the average returns be equated, which occurs when only $1 is put in A and $2 is put in B. This yields an average product of 1 for each project, and the final wealth is only $3. My guess is that melioration would fail to predict well in this simple example. Melioration is not a very general theory, since it has no clear implication when there is only a single choice variable. I would not characterize melioration as a theory, but rather as an empirical regularity that has been observed in some contexts to be discussed next.

Experiments have been conducted in much more complex environments than the one described above. In particular, the payoffs for each investment are random and depend on the frequency with which that investment option has been chosen in previous trials. As the authors note, this situation is like tennis, where a frequent choice of the same strategy (lob or pass) will cause the opponent to be more likely to defend against that strategy. Thus the probability of winning a point with a lob (or pass) is a decreasing function of the frequency with which one has lobbed (or passed) in the past. As far as I can tell, subjects are not told the mechanisms that governed the distributions of payoffs over time, so any attempt to behave rationally must involve both learning and experimentation on the part of the subject. If the outcome is approximated by

[6] The marginal returns are equated in the sense that the discontinuous marginal return for project B falls from 1.50 to .50 as investment is increased from $1 to $2, so this marginal return crosses the constant marginal return of $1 for project A at this point.

equating average returns, an economist would want to know whether there are rational learning and experimentation strategies that can produce significantly higher expected payoffs. The answer to this question is probably impossible to determine, since we do not know what prior beliefs subjects bring to the investment decision. My guess is that complexity and randomness causes subjects to think in terms of linear relationships, so that average and marginal products are equivalent. Herrnstein and Mazur (1987, p.43) respond that "One may wonder about a theory of behavior that fails to handle the equivalent of a simplified tennis game,..." My reaction is that the tennis interpretation suggests a two-person, zero-sum game, where a mixed-strategy Nash equilibrium is calculated precisely by equating average payoffs! This is because a player would only be willing to randomize if the two alternative decisions had the same expected payoffs. For an economics audience, it is essential that the experiments be structured so that the predictions of standard economic theories can be calculated.

I would guess that most psychologists, after reviewing the data, would still prefer the melioration theory, and most economists would certainly prefer the maximization theory. One explanation is that "theory" has a different role and meaning in the two disciplines. Economists have a sense of what is a good theory, and this sense is usually acquired from listening to a lot of seminar discussions and arguments over coffee. They prefer theories that are operational and general, since these are "portable" to different applications. General theories must perform well in simple environments, and experimental tests in those environments should usually precede stress tests in complex settings. Finally, the worst thing an economist can say about a theory is that it is *ad hoc*, which usually means that its assumptions are expressed as descriptive patterns of decisions rather than in terms of the underlying causes of those decisions (objectives, constraints, etc.). Melioration is clearly *ad hoc* in this sense. The feeling here is that a theory based on more primitive assumptions will have a wider reach than the particular context being studied.

Theory in psychology is sometimes more of a description of the qualitative pattern of behavior in a specific situation, and in this sense it may not be portable to other situations.[7] The

[7] Gordon Tullock once remarked to me that theory for a psychologist is like finding a regular pattern of large designs on the carpet of the conference hotel.

focus is often on rich environments where biases or deviations from rational behavior are likely to be found. This is natural since these are precisely the situations where the mental process of simplifying hard problems will be revealed most clearly. One of the important contributions of psychology is the observation of actual decisions making to come up with "mental models" and decision making heuristics. Economists will admit that they do not know much about learning and adjustment in unstable "out-of-equilibrium" situations. Models of adjustment are especially when there may be multiple equilibria, some of which are worse for almost everyone (e.g., high unemployment equilibria). Data from experiments can provide useful guides to theorists who are otherwise reluctant to step out from the cozy world of perfect rationality.

Unfortunately, many possibilities for useful interchange are lost because of rigid attitudes and misunderstandings. Psychologists precede the words "bias" or "effect" with the same types of juicy adjectives that economists use before the word "equilibrium." To an economist, the psychology of judgement and decision making often seems like a bag of tricks, with no coherent theory and no innovative statistical modeling.[8] To an outsider, economics often seems like a branch of theology, where rational behavior is worshiped and sinners are ignored. A recent commissioner at the Federal Trade Commission characterized economists as a "flat earth society." Richard Thaler recently quipped that "when an economist says the evidence is mixed, this means that theory says one thing and the data say another."[9] These views are exaggerated for effect, but they do indicate both a problem with perceptions and a potential for more useful interaction.

Some of the differences in perspective between the two disciplines may be due to the journal review process. I get the impression that some psychologists try lots of decision

[8] One of my colleagues at Virginia recently told me that "psychology seems too easy; they have no theory, they just do a simple experiment and look at what happens."

[9] This comment was made in a presentation to the Judgement and Decision Sciences Meetings in Washington, DC in 1994, with reference to the effect of IRA tax incentives on saving behavior. (Many economists in the 1980's said that IRAs would not affect marginal incentives to save, and Thaler's analysis of mental accounts predicted that there would be an increase in saving.) After the laughter died down, I began thinking that I had not heard too many people congratulating themselves on the high savings rates in the US since the mid 1980's. In fact, savings rates seem to have peaked at about the time the first IRA laws came along. There is, however, some indirect evidence that IRAs increased savings. But here is a case where the predictions of theory are also mixed (the "two-handed economist problem"), since IRA incentives can have an effect in dynamic life-cycle models, where consumers anticipate that IRA incentives will have an effect at the margin in later, low-saving periods. In any case, the point of including the quote is to document common perceptions of economists' attitudes.

problems out on their classes in search for anomalies. This search produces a bias to find new biases, which are of interest to colleagues and journal editors. In contrast, editors and referees of economics journals are often skeptical of experimental methods, and it is important to show them one treatment where the standard theory really predicts well. Otherwise they may suspect that the "mistakes" are caused by distracted undergraduate subjects who do not understand the procedures and incentives. This calibration process often tilts the scales toward papers that tend to support the predictions of standard economic theory.

Economists tend to think of deviations from rational behavior as random "errors" rather than systematic biases. The distinction is subtle and important when the equilibrium or optimal behavior is at a boundary of the choice space, and any treatment that injects noise into the decisions will bounce all deviations in the same direction. For example, it is very tempting to design an experiment in which the Nash equilibrium is one in dominant strategies, since the calculations then do not depend on assumptions about risk preferences. But such equilibria are often at the a boundary. For example, it is a dominant strategy to contribute nothing to the public exchange in the standard voluntary contributions experiment. Positive contributions can be due to systematic altruism or to errors caused by random unobserved variations in utility. Any treatment that increases the error rates might be incorrectly interpreted as increasing altruism. For example, Andreoni (1988) unexpectedly found that subjects in stable groups contributed less to a public good than when they were rematched into new groups in each period. This strangers/partners effect may just be due to the increased randomness in decisions when subjects are being rematched (Palfrey and Prisbrey, 1994). Of course, many biases away from optimal decisions at a boundary can also be replicated when the optimal decision is in the interior of the choice space.[10]

To summarize, psychologists should address their work to mainstream economists, not to the popular press or anti-scientific elements on the fringe. Therefore, experiments should be designed in a way that permits calculation of the predictions of maximization-based decision or game theory. Although economists prefer general theories based on primitive assumptions, they

[10] Ball, Bazerman, and Carroll (1990) reported a winner's curse bias away from an optimal (boundary) bid of zero. Holt and Sherman (1994) modified the design so that the optimal bid is above zero. The observed bids were still too high in their winner's curse treatment.

should remember that it is sometimes only possible to devise descriptive theories that explain biases in specific contexts. In any event, the word "theory" should be used with care, and some anomalous results might be presented as "empirical regularities" or "stylized facts." Economists should remember that anomalies and heuristics can be very useful in constructing good models of behavior in unstable phases of adjustment and disequilibrium.

III. Differences in Perspective

If you look at the first half of a microeconomic theory book, just about all you see is maximization, with various constraints and applications to consumer and producer decisions. This creates the impression that economists are primarily concerned with optimal solutions to individual decision problems. Indeed, one of my retired colleagues has the personalized license plate: MAXIMIZE (but his former secretary has a license that reads: NT TRUST). These perceptions are misleading; economists are primarily interested in the equilibrium interaction of individuals in markets. Their perspective is much more from a social point of view, with an eye to social welfare and policy recommendations. Since psychologists are more interested in learning and bounded rationality, their insights will be most useful in the study of adjustment to equilibrium.

Some of the interaction of the two disciplines along these lines has been especially productive, but in unintended and surprising ways. For many years, the most stimulating interdisciplinary mixes have been in graduate business schools. Robert Lucas recently recounted a conversation with Milton Friedman, who wondered why he did not think of rational expectations, lamenting that he had been so close.[11] Lucas' response was that Friedman had been in the wrong place. He recounted that the assistant professors at Carnegie-Mellon had been exposed to "all of this behavioral stuff," coming from Herb Simon and others. Jack Muth's reaction was to try to cut through the difficult problems of learning and adjustment by specifying what must be true in equilibrium. Lucas attributed the Modigilani-Miller theorem to the same

[11] This paragraph is based on my recollections of Lucas' remarks at a panel discussion for the conference in honor of Richard Cyert's retirement from Carnegie-Mellon in 1993.

type of reaction against the prevailing views at Carnegie-Mellon in those days.[12]

A different reaction to behavioral anomalies is that they do not matter much in the aggregate. In particular, there is a strong belief and some experimental evidence that biases at the individual level tend to diminish in the context of market trading. See Smith (1994) for discussion and references. Many of the contexts in which biases (representativeness, preference reversals, etc.) diminish are in double auction trading. This institution is highly centralized, with all improving bids, asks and transactions being displayed in real time, as on a ticker tape. The surprising resilience of this trading institution should be more of a comfort to those who study financial markets that to those who study consumer behavior in retail markets for products that are not purchased on a daily basis.

Economists are going to be relatively unconcerned with behavioral anomalies that do not seem to have important welfare effects or policy implications. It is hard for me to get very excited about something like hindsight bias, but the possibility of money illusion is another matter. But lets move on to the widely studied effect of the menu of choices on choice probabilities. In the Simonson (1989) experiments, for example, subjects were asked to imagine hypothetical choices between three products, each of which is described by a point in a two-dimensional attribute space (e.g. picture quality and reliability). In one treatment, product A was the point (60, 80) and product B was the point (70, 70). In a two-way choice, some subjects selected A and others selected B. But when a third (undominated) option C was added at a point like (80, 60), the proportion of B choices increased. This has been labeled a "compromise effect," since the middle option B was more likely to be chosen when it could be rationalized as a compromise between the other two. If the added option C had been (60, 75), it would have been dominated by A. In this type of experiment, the addition of a dominated option tends to increase the choice probability of the dominating option, which is called the "attraction effect." The dominated option itself is sometimes chosen.

These decision patterns are clearly inconsistent with utility maximization, but what are

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[12] I came to Carnegie later and took many of my courses from Lucas and Ed Prescott. Prescott was one of my thesis advisors, and he later urged me not to get into laboratory experimentation ("it was a dead-end in the 1960's and it will be a dead end in the 1980's). My other advisor, Morris DeGroot, and our joint coauthor, Richard Cyert, were much more interested in models of learning and adaptive behavior.

the implications? Simonson suggests that the compromise effect may offer a reason for a firm to try to position its product between those of its competitors. As a consultant, I would have a hard time charging much for this advice. Any deeper analysis of this situation must take into account the location decisions of the two firms that entered first, possibly anticipating future entry. I have also heard it argued that these choice paradoxes might by useful for policy makers who are making up a list of coverage options for a national health care plan. I cannot imagine that there are big issues associated with deciding how many *dominated* health-care options to add, and I do not trust experts to limit my choices. Think of this situation a little differently. Suppose that any combination of attributes that sums to 140 is feasible without subsidy, so that options A and B above are on the efficiency boundary. Adding randomly located options will increase the chances that a dominated option will be selected, but it will also increase the range of undominated choices available. Even with some randomness in consumer decisions, the addition of options will not necessarily reduce individuals' welfare. Moreover, exclusion of competitors is likely to facilitate tacit collusion among providers who may try to move away from the efficient boundary.

IV. Experimental Procedures

The first presentations that I heard at a psychology conference was a report on an experiment in which the authors had elicited monetary values for being released from the task of writing a sonnet about autumn leaves or your first week in college. When I went up to one of the authors afterwards and asked her how those sonnets turned out, she admitted that they did not really make anybody write a sonnet. After responses to the elicitation question were safely obtained, they told subjects that the procedures announced before would not be followed, but rather, that everyone would be paid $5 and released. Most of the other presentations in this session either involved deception of subjects or the use of hypothetical payments. It is hard to take such studies seriously, despite the fact that when asked, the speakers asserted that someone else had replicated the finding with real incentives.

Deception that is revealed to subjects will dilute the effects of incentives in later experiments, by the same researcher and by others in the same department. Even when deception is not revealed directly, subjects may be able to discover it by comparing experiences after the experiment ends. Most economics referees will never recommend publication of a study that involves significant deception. I always go to great lengths to convince subjects that they will be paid and that all procedures are being followed. Often I have a randomly selected student monitor looking watch procedures that involve throwing dice, pairing subjects in games, telling subjects about others' payoffs, etc. One of the reasons that I have a hard time getting other economists to take experiments seriously is that many of them feel like they have been tricked in psychology experiments.[13]

The use of real (monetary or otherwise) incentives is, in my opinion, essential for most studies of economic behavior. Our theories are about how people actually behave when they have to face the consequences. This point of view goes back at least to Adam Smith's *Wealth of Nations*; he limited his discussion to "effectual demand," noting that even a poor man would have a demand for a "coach and six" if no price had to be paid. Not using salient incentives usually increases the "noise" in the subjects' decisions, and it can sometimes dramatically affect the results. Psychologists had been doing probability matching experiments for twenty years before Sidney Siegel and his coauthors reported an experiment in which a high proportion of decisions were rational predictions of the more likely event when monetary rewards were used.[14] But when subjects were told to "do your best" with no financial rewards, the proportion of correct choices almost exactly matched the probability of the more likely event.

I am not arguing that real incentives are needed for studies of thought processes that are unrelated to economic decisions. Nor am I claiming that experiments with hypothetical payments are never informative. In some contingent valuation studies, for example, the questions will necessarily be hypothetical, but it is still useful to try to calibrate answers to hypothetical and real questions in scaled down versions of the problem being studied. I have less patience with the attitude of one bright young psychologist who told me that it was just not worth the trouble to use real incentives just to convince economists. Its true that sometimes the results turn out the

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[13] Deception is probably not as much of a problem as it used to be. The Psychology Department at Virginia has a committee that reviews procedures and even requires the researcher to reveal the purpose of the experiment after each session. I was recently an outside reader for a psychology thesis defense, and the student had explained to the subjects *ex post* the predictions of the theory being tested. This seems risky since there is nothing to prevent students from discussing these predictions with others in the class who will attend later sessions.

[14] See Siegel, Siegel, and Andrews (1964).

same either way, but sometimes they do not. Forsythe, Horowitz, Savin, and Sefton (1994) find that the use of money payments has a significant effect on behavior in dictator bargaining games (in the predicted direction). They report no significant effect on the first offers made in ultimatum games, but Hoffman, McCabe, and Smith (1993b) conclude that irrational rejections of positive offers are more common in ultimatum games with hypothetical payoffs. Smith and Walker (1993) survey the use of monetary rewards in economics experiments and conclude that sometimes money payments seem to matter and sometimes they do not. I see too many experimental economics papers come across my desk in a month to be able to spend much time trying to assess the few that use with hypothetical payments.

Another worrisome practice in psychology is the recruitment of subjects from the same class or from the investigator's own class. Simonson (1989) reported that deviations from rational behavior (with hypothetical payoffs) were affected a manipulation of the possibility of having a student's decisions discussed later in class. Hoffman, McCabe, and Smith (1994b) have report that increases in anonymity increase the aggressiveness (rationality) of initial offers in ultimatum games (for $10 payoffs). In addition, it is very hard to replicate the degree of social interaction between students and an instructor, or among students in the same class.

Indeed, the procedures used in many current psychological studies of decision making continue to surprise me, since it was a psychologist, Siegel, who first studied economic interactions with careful laboratory methods (in combination with an economist, Fouraker).[15] His work always contains detailed descriptions of procedures, instructions, money payments, subject pools, etc.[16] When the Economic Science Association decided to begin giving a dissertation prize for work in experimental economics, one of the board members suggested that we call it the Siegel award. I hope that a renewed interaction between psychology and economics will have similarly beneficial effects.

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[15] See Siegel and Fouraker (1960) and Fouraker and Siegel (1963).

[16] For a more detailed list of what most economics referees look for, see Davis and Holt (1994), chapter 1, or the Palfrey and Porter (1991) "Guidelines for Submission of Experimental Manuscripts" in *Econometrica.*

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