INER 10,1_f11_131-148 4/5/05 5:06 PM Page 131

International Negotiation **10:** 131–148, 2005.
© 2005 *Koninklijke Brill NV. Printed in the Netherlands.*

The Method of Experimental Economics

RACHEL CROSON*
The Wharton School, University of Pennsylvania, 567 Huntsman Hall, 3730 Walnut Street, Philadelphia, PA 19104–6340 USA (Email: crosonr@wharton.upenn.edu)

Abstract. There are many similarities between experimental economics and psychological research, both substantive and methodological. However, there are important differences as well. This article discusses five methodological areas where experimental economists and experimental psychologists differ: incentives, context, subject pools, deception, experimental details and data analysis. Within each topic I present the economists' methodology and rationale and contrast it with current practice in psychology and management research. My hope is that this article will lead not only to a deeper understanding of each field's choice of methodology, but also to practical advice for researchers toward having their work read and accepted by their colleagues in related disciplines.

Keywords: Experimental economics, methodology, deception, incentives, context, subject pool, data analysis

Introduction

The 2002 Nobel Prize in Economics was awarded to Daniel Kahneman (an experimental psychologist) and Vernon Smith (an experimental economist). This award acknowledges an important trend in economics – the growth of experiments as a valid, accepted methodology and the influence of psychological research in that growth.[1]

There are many similarities between experimental economics and psychological research. Researchers in both fields are concerned with similar substantive areas; bargaining in economics and negotiation in psychology, public goods provision in economics and social dilemmas in psychology, just to name a few. The two fields also share many methodological practices. Both fields use convenient populations (like undergraduate students) as participants in their experiments. Both fields elicit decisions or introspections from their

* Rachel Croson received her PhD in Economics from Harvard University in 1994. She conducts research in experimental economics, judgment and decision-making, negotiation and social dilemmas. She is currently an Associate Professor at the Wharton School of the University of Pennsylvania and serves as the Psychology Head on the board of the Economic Science Association, the professional association for experimental economists.

INER 10,1_f11_131-148 4/5/05 5:06 PM Page 132

participants and use those to learn about the world. Both fields are concerned with careful experimental design, avoiding demand effects, and appropriate statistical analyses.

However, the *objectives* of psychology and economics experiments are often different. Economics experiments are designed to address economic theories; psychology experiments are designed to address psychological theories. This distinction may seem obvious at first, but it has important and often unforeseen implications for methodological differences in the two fields. These methodological differences are the subject of this article. I will discuss five methodological areas where experimental economists and experimental psychologists differ. They are ordered from most to least important *for economists*, although readers may disagree with my ordering.

This is by no means the first article on experimental economics methodology. However, most previous articles have been aimed at traditional (non-experimental) economists to argue that the experimental methodology is a relevant and valid one (e.g. Binmore 1987, 1999; Plott 1982, 1991a, 1991b, 1994; Roth 1986, 1991, 1994; Smith 1976, 1989, 1994). Friedman and Sunder (1994) have written the definitive text on experimental economics methodology aimed at economists who want to begin running or consuming experiments, and I highly recommend it for psychologists who want to see how economists think about experimental methodology. A very few papers explicitly compare economic and psychological methodology, Cox and Isaac (1986) present economic methodology for an interdisciplinary audience. Hertwig and Ortmann (2001) provide a well-documented comparison of the methodologies. Holt (1995) presents his perceptions of methodology in experimental psychology for economists. Croson (forthcoming) compares the two methodologies for law and economics scholars.

As many have noted, one main difference between the fields of economics and psychology is the existence of a unified core theory – expected utility theory.[2] Experiments in economics are explicitly designed to address these economic theories. These experiments have an important place in the dialectic of the scientific method. First a theory is constructed or described and predictions are generated from it deductively. Then, an experiment is run to test these predictions. Experiments designed to address economic theories need to have a high degree of *internal validity*. This requires the construction of a lab situation that exactly captures the theory's assumptions. If the experiment is not internally valid, the data it produces is not relevant to the theory's predictions. For instance, if the theory a researcher is trying to test assumes that a game is infinitely repeated, the experiment run must implement such a game.[3] Similarly one cannot test one-shot theories using repeated interaction among the same players.[4]

INER 10,1_f11_131-148 4/5/05 5:06 PM Page 133

Many (possibly all) of the ways in which experimental economics methodology differs from experimental psychology, discussed below, stem from this objective of internal validity. Economic theories predict how people will act in the presence of real, salient rewards; thus contingent *incentives* are critical for economic experiments. The theories are abstract, intending to apply to many different situations and individuals; thus there is little or no *context* in economic experiments and the *subject pools* used are also not of primary concern. The theory assumes that actors understand and believe the relationship between their actions and their payoffs; thus *deception* would endanger this belief and is almost nonexistent. *Experimental details* are designed to influence participant's perceptions of one or another of these areas. The final topic, *data analysis* is a bit different – variations in this methodology are based on the reference groups to whom researchers are appealing.

For each topic, I will introduce the methodological issue, describe the experimental economics practice and its rationale, mention psychological practice and rationale (much less thoroughly). My hope is that this article will lead not only to a deeper understanding of each field's choice of methodology, but also to some practical advice to psychologists on how to have their work read and accepted by economists. I begin with perhaps the largest gulf between economic and psychological experiments: incentives.

Incentives

Economic theories describe and predict decisions individuals will make in the presence of payoffs. Typically, theories specify the payoffs from taking one action or another. For example, individuals contributing to a public good (cooperating in a social dilemma) incur some private cost from doing so which outweighs the private benefit they receive from the public good. Since earnings are higher when individuals defect than when they contribute, theory predicts they will defect.

It is critical for theory testing that the participants actually face the payoffs assumed by the theory. The fact that individuals cooperate in social dilemmas when there are no payoff consequences from their actions is simply not informative. Economic theory makes no predictions of what individuals will *say* they *would* do, only what they will *actually* do when faced with a given decision and the resulting payoffs. For example when studying ultimatum bargaining it is important to have participants actually earn the amounts that their decisions imply (e.g. Croson 1996, Forsythe et al. 1994).

This has led to the practice of *induced valuation* in experimental economics (Smith 1976), where participants' compensation is not simply positive, but

INER 10,1_f11_131-148 4/5/05 5:06 PM Page 134

importantly is responsive to the choices they make in a way that is consistent with the theory being tested. This practice replaces the flat-fee payments more common in psychology experiments, where individuals are paid some amount for their participation (perhaps zero, or perhaps earning extra credit in their course) which is not contingent on the decisions they make.

For example, when two psychologists first identified the disjunction effect (nonconsequential reasoning), their papers included data from surveys and hypothetical questions (Shafir and Tversky 1992; Tversky and Shafir 1992). Participants were asked about gambles they might take after having won or lost a previous gamble or vacations they might purchase after having passed or failed their exams. When an economist wanted to further explore the boundaries of the effect, participants played games (prisoner dilemmas, public goods, and games of iterated dominance) and were paid based on their decisions and the decisions of their counterparts (Croson 1999, Croson 2000). This is not to say that all psychologists use unpaid participants – many run experiments with incentives.[5] But *all* economics experiments involve salient payments to participants. This practice is considered critical to the validity of the experiment and the objective of testing the theory being addressed.

Typically, payments in economics experiments are made in cash directly after the experiment. There are a few reasons for the use of cash. First, everyone values it, in contrast with extra-credit points or other grade-related rewards which may be valued only by students who are grade-conscious and/or whose grade may be affected by the outcome. Second, cash is one good that is non-satiable – more is always better.

That said, some notable experiments use forms of payment other than cash. All, however, use payments that are contingent on individual's decisions and argue (successfully) that the payments involved are consistent with the theories being tested. For example, in Boyce et al. (1992) the authors want to willingness-to-pay and willingness-to-accept for specific environmental damages. They use dollars for the WTP and WTA measures, but for the environmental damage they bring a baby tree into the lab that the participant will chop down if the damage must be incurred. This tree-killing action captures nicely the theory's assumptions of incurring environmental damage and is an excellent example of non-financial rewards that are nonetheless contingent on the participant's actions.

A second issue in the area of incentives is the amount participants receive. A number of papers have argued that participants must be paid enough to compensate them for their time (thus, the average payoff should translate into an hourly wage roughly equal to the salaries of on-campus jobs), and for the thinking costs they incur during the experiment (Smith and Walker 1993).

INER 10,1_f11_131-148 4/5/05 5:06 PM Page 135

One natural question is the extent to which paying participants contingent on their actions affects outcomes. There are now a number of meta-analyses on this question. Camerer and Hogarth (1999) review 74 studies and conclude that financial incentives have a large effect on judgment and decision tasks, less in games in markets. Hertwig and Ortmann (1998) review 10 studies from JBDM with and without incentives. They find that incentives decrease framing effects, bring auction bids closer to optimality and eliminate preference reversals. They also identify contexts when payment had no effect, specifically confidence judgment and information acquisitions. In summary, for some tasks contingent payment does not seem to affect the mean outcomes, while for others it does. There is also general agreement, however, that contingent payment reduces the *variance* of responses one receives. Smith and Walker (1993) survey 31 experimental economics studies varying incentives and conclude that "in virtually all cases, rewards reduce the variance of the data around the predicted outcome" (245).

Paying contingent (and sufficient) payments can get expensive. There are a number of "tricks" that economists play in order to stretch limited funding. First, if participants in the experiment are playing the same game repeatedly, researchers sometimes choose one round at random for real payment. Of course, participants are told in advance that one round will be randomly chosen (see the section on deception, below), but not which one. This payment design has the advantage of avoiding *wealth effects* in which participants who are accumulating money throughout the session begin making riskier and riskier decisions. Similarly, for experiments with large numbers of participants, researchers sometimes choose one participant (or one dyad or one group) at random for real payment. Again, participants are told in advance that one will be chosen at random. While both these techniques have the potential to reduce expenditures, experimental economists using these designs typically make the *expected* earnings of any given participant equal to the wage rate.

This is not to say that economists never use any data collected from a survey or hypothetical responses. Many experiments in economics use a post-experimental questionnaire where participants are asked to introspect as to why they made the decisions they made. But economists are very cautious when reporting and interpreting this data, and always ask these questions *after* they have collected the actual (real-money) decisions of interest. The perception is that responses to questions like this are "cheap talk" and may have no relation to what is actually in the minds of experimental participants. Personally, I consider this data like any other, and consider the motivations of the subjects including honest reporting, impression management and other factors in interpreting these results. This data is often particularly useful in going

INER 10,1_f11_131-148 4/5/05 5:06 PM Page 136

beyond documenting a particular outcome and digging deeper into the motivations causing it.

My recommendation to psychologists who want economists to use their work and to cite their results is simple – pay your participants. And pay them not a flat fee, but an amount contingent on their decisions. This is particularly important if the experiment's results are inconsistent with economic theory. It is far too easy (and common) for economists to simply disregard experimental results inconsistent with their theories because the participants weren't appropriately incentivized.

Context

The second domain in which economic and psychology experiments differ is in their use of context in experiments. Economics experiments are primarily context-free (or context-neutral), even those that purport to be about something in the real world (e.g. Croson and Mnookin 1997). Participants are not told they are providing a public good (or, even more specifically, cleaning up the water in their city) as in psychology experiments. Instead they are asked to allocate tokens between two accounts that offer varying payoffs (e.g. Croson 1996b).

There are three main reasons why economists use little context. First, the theory being developed is supposed to apply generally – it should predict behavior in any context that involves the appropriate payoffs, so the experiments to test the theory should not rely on a particular context. Second, context often adds variance to the data. For example, if some participants think that going to court is a good thing and others think it is a bad thing, then describing the experimental decision as 'going to court' as opposed to 'choosing option A' could add noise to the data (Croson and Johnston 2000). This additional noise might not change the average or aggregate decision, but it can impact the variance of those decisions, reducing the likelihood of detecting statistically significant differences between treatments of the experiment. Finally, and most importantly, context can add systematic bias or demand effects. For example, if participants in aggregate think there should be fewer court cases or want to be seen as kind, gentle types by their professor, then describing the decision in terms of going to court might reduce everyone's likelihood of choosing that option. This would change the responses in a systematic way based on the context. Such systematic changes in the data will significantly change the conclusions reached, so economists try to avoid context in their experiments. There are some costs of avoiding context (see below), but in

INER 10,1_f11_131-148 4/5/05 5:06 PM Page 137

economics experiments they are not high, and context is in general considered a nuisance variable rather than a variable of interest.

However, there are arguments in favor of context as well. As Loewenstein (1999) points out, even abstract instructions contain context, albeit unfamiliar. Additionally, experiments with context have more external validity, cueing subjects to behavior that we might more often observe in the real world. For example, the well-known result that Wason's-task errors significantly decrease when context is added, suggests that the importance of cognitive errors may have been overestimated. For psychologists who want their work to be accepted by economists, the use of context is not as serious a methodological deviation as a lack of incentives. However, papers aimed at economists need to argue that the results are not being driven by the particular context chosen or, even better, demonstrate the same effect in multiple (sufficiently different) contexts.

Subject Pools

Just as economic theories are intended to apply generally to varying contexts, they are designed to apply generally to varying subject pools. While some economic theories focus on individual differences (e.g. risk-preferences), the goal in economics is to develop and test simple theories that explain behavior by many people in many contexts, even if only imperfectly, rather than developing more complicated theories that explain behavior by a subset of people (maybe only one) in a smaller class of contexts more accurately. Thus there is limited concern by experimental economists about demographics. Many early economics experiments did not even collect demographic data of the participants and very little analysis was done examining how different individuals acted differently. More recent research in experimental economics has begun to focus on individual differences, especially gender differences. Eckel and Grossman (forthcoming b) provide a nice review of this literature. Other experimental economics research has started to investigate cultural differences as well. For example, Buchan, Croson and Dawes (forthcoming) use experimental economics methodology to look at cross-cultural direct and indirect trust.

That said, economists are concerned with other dimensions of their subject pool. First, economists typically recruit volunteers as participants in their experiments, only rarely using students from the researchers' own course (or other courses in the department). The latter is a common practice in psychology, where students in introductory psychology courses participate in

INER 10,1_f11_131-148 4/5/05 5:06 PM Page 138

experiments as part of their educational experience and/or for extra credit in their courses. Experimental economists are particularly concerned with inducing demand effects by having students who have learned about the theories they are trying to test in class participate in experiments testing them. Some recent research suggests that there are systematic differences between decisions made by these "true" volunteers and by "pseudo" volunteers who are students in a class (Eckel and Grossman, forthcoming a).

Second is the ongoing (and unresolved) debate about whether economists are different than other professionals. Papers have demonstrated that economic students (undergraduate and PhD) are more likely to free-ride in social dilemmas (Marwell and Ames 1981; Frank, Gilovich and Regan 1993, 1996) and to offer (and demand) less in ultimatum games (Carter and Irons 1991). However, these results are by no means unchallenged. Yezer, Goldfarb and Poppen (1996) present evidence that economics students are *more* cooperative than other students in a lost-letter experiment (contrasting with the Frank et al. papers which were hypothetical survey results). Similarly, Laband and Biel (1999) show that professional economists cheat less on their association dues than professional sociologists and political scientists. My take on this debate is twofold. First, the jury is still out on whether economists are different than non-economists. Second, given this unresolved debate, the experimental economics practice of using volunteers from all over the university as participants in their experiments rather than students from their own (economics) classes is a sensible and conservative one.

A final issue in the subject-pool debate concerns the use of students as opposed to professionals (or "real people" as critics of experiments sometimes say). In terms of economics experiments that test theories, this is not a problematic criticism – the economic theory is supposed to be general and to apply to anyone facing a decision like the one described in the theory, not simply people who are above 30. However, there are some experiments, particularly those that are aimed at testbedding policies, where the use of professionals as participants makes sense. For example, Cummings Holt and Laury (2002) use farmers to test competing designs for mechanisms to allocate water rights in Georgia. Dyer, Kagel and Levin (1989) and Dyer and Kagel (1996) test theories of auctions with contractors who submit competitive bids for a living.

Once the researcher moves away from students to professionals, the incentives used in the experiment become both more important and more difficult. An undergraduate student can be induced to think hard about a problem if the difference between making the right decision and the wrong one is around twenty dollars. A professional whose income (and opportunity cost of time) is higher may require significantly more money to participate. More troublesome is that high-earning professions may not be motivated by money, at least not

INER 10,1_f11_131-148 4/5/05 5:06 PM Page 139

on the scale most experimentalists can pay. Nonetheless, a growing number of economics experiments have attempted to demonstrate effects among professionals as well as among student participants.

For psychologists who want their work to be accepted by economists, the use of Psych I and similar subject pools is indeed problematic. If incentives are being offered, recruiting participants from across the university is a relatively painless way to avoid selection biases that may result from using only students in psychology courses, and demand effects from using one's own students as participants in experiments.

Deception

One methodological difference between experimental psychologists and economists which has recently received attention is the use of deception in experiments. A series of papers examines (and criticizes) standard practice in both fields, including Ortmann and Hertwig (1997, 1998), Bonetti (1998a, 1988b), McDaniel and Starmer (1998) and Hey (1998).

One of the strictest rules in experimental economics is that the researcher may not deceive their participants. This prohibition on deception includes deception about the purpose of the experiment, the payoffs the participants will earn, or the characterization of the participants' counterparts. As described above, the validity of an economic experiment rests on the link between behavior and payoffs (incentives). If that link is weakened, the experiment becomes an inferior test of the economic theory it is designed to address. Similarly, the reasoning goes, if participants are deceived about that link, the validity of their decisions is called into doubt.

A second reason deception is disfavored in economics has to do with the public-goods nature of trust in the experimenter. If participants are routinely deceived in experiments, for instance, by being told they will take home their earnings in the game, and then actually receiving a $5 flat fee for their participation, they will begin to distrust the experimenter's statements. This lack of trust could lead the participants to change their behavior in future experiments.

In contrast, psychology experiments often deceive participants about the purpose of the experiment, the payoffs that will be earned and the existence (or nonexistence) of counterparts.

Sometimes this deception is necessary for the experiment. For example, deception about the purpose of the experiment can aid in honest elicitation and overcome presentation effects; participants who know an experiment is about racial discrimination may act contrary to the way they normally do. In addition, deception is often used to examine situations which would not occur

INER 10,1_f11_131-148 4/5/05 5:06 PM Page 140

naturally, for example, how individuals respond to low ultimatum offers. That said, many of these benefits arising from deception can be enjoyed by simple omission (not informing the participants of the subject of the experiment, or doing so only very generally) rather than by commission (explicitly lying to the participants).

After deceptive experiences, experimental psychologists often debrief their participants, revealing the deception and asking them not to tell other potential participants about their knowledge. Although this is recommended and often required by human subjects committees, to economists this practice only enhances the likelihood of recruiting participants who enter the experiment expecting deception and further weakens the link between actions and earnings.

The norm against deception is quite strong in experimental economics – an economic journal will not publish an experiment in which deception was used, while in psychology journals, deception is commonplace and accepted. The closest the experimental economics field has come to allowing deception is surprising participants with additional decisions when they initially believed the experiment had ended (e.g. Andreoni 1988; Croson 1996; Boles, Croson and Murnighan 2000; Croson, Boles and Murnighan (forthcoming)). Note that this does not involve deception by *commission*; participants are never told anything that is not true. Instead, it involves deception by *omission*; participants are not told everything about the experiment when they begin.

For psychologists who want their work to be read and accepted by economists, deception will prove to be a difficult barrier to overcome. If it's simply a matter of saving money, economists react very skeptically to experiments that use deception. If the deception is necessary for some other reason, an explanation and justification will be needed in order to have the data taken seriously.

Experimental Procedures

These last two topics differ from the previous four. Here, I describe a collection of my practices in the implementation of economics experiments, rather than globally-accepted wisdom about experimental design. Many of these procedures are commonly used both psychology and economics.

Many experimental projects begin with an application to one's human subject committee. Experimental economists tend to have relatively little trouble with human subjects committees, as many of the usual "flags" are not present in their designs. The experiments do not involve deception, there are rarely any consequences other than financial, and participants earn money for their par-

INER 10,1_f11_131-148 4/5/05 5:06 PM Page 141

ticipation. Little or no demographic data is collected, and participants are paid privately (see below), reducing the impact of social comparisons on earnings. Thus many projects receive expedited review and quick acceptance.

The most troubling constraint for experimental economists imposed by human subject committees is guaranteeing positive earnings. Economists often want to explore decision-making in the face of losses rather than gains, but human subject committees (and other concerns) prevent us from taking money from our participants. There are a few alternative responses which have been successful at various institutions. The easiest is to pay a large "show-up-fee" and then have losses from the experiment deduced from it. While it is not entirely clear that this induces the "loss frame" in participants, it is usually acceptable to human subject committees. One human subject committee has allowed researchers to take money from participants provided they agreed to the risks they were to take. Another allows participants to "work off" any debts they incur by photocopying articles at an appropriate wage rate. These are, however, the exception rather than the rule, and often experimental economists are called to construct creative experimental designs to both induce the appropriate incentives to test the theory and ensure the participants will all make money.

A second set of concerns involves interactions with the participants. Economics experiments tend to be run with groups of participants rather than one at a time. And since there are no confederates, when the participants arrive in the lab there is often a period of waiting for their counterparts to arrive. Providing newspapers, magazines and an internet connection helps participants pass the time quietly, and without external discussion.

During the experiment, instructions are read aloud while participants follow along silently. This is useful for three reasons. First, it ensures that participants have been exposed to the instructions, and have not simply skipped them and started the experiment. Second, it creates *common knowledge*, one of the theoretical conditions necessary to test economic theories. This means not simply that everyone knows the game they will play, but that everyone knows that everyone knows the game, and that everyone knows that everyone knows that everyone knows, Reading the instructions out loud also serves a third purpose – it reassures the participants that everyone in the room has indeed been given the same instructions and reduces suspicion of deception.

Instructions often include examples to help participants keep track of financial transactions. However, examples have the opportunity to induce demand effects – participants use the example as a signal of what the experimenter would like them to do. There are a few solutions to this. First, one can construct examples that are far outside the possible range of behavior. For example,

INER 10,1_f11_131-148 4/5/05 5:06 PM Page 142

when deciding how much to allocate to a public good out of 20 tokens, the examples can involve allocating 5000 tokens in some manner. Second, one can use variables like X and Y to substitute for numbers in the examples. This works well with mathematically sophisticated participants, but is often confusing for others.

My personal preference is to use a quiz rather than examples. As in examples, it is important to make quizzes as unbiased as possible, either by using different scales in the quiz and in the experiment, by making the quiz abstract rather than concrete, or by allowing participants to fill in their own numbers for their decisions. Then we ask participants to calculate their earnings (and sometimes, their counterpart's earnings). Experimental monitors circulate throughout the room and check the answers, correcting and explaining for anyone who completed it incorrectly.

A second issue that arises during experiments is how to deal with participant questions. Especially while reading instructions aloud, participants will often have questions about the procedures. These questions you would like to have asked and answered publicly. However, other participants ask leading or contaminating questions (e.g. "why don't we just all cooperate so we can earn more money?"). These are attempts to influence the actions of others disguised as questions, and these the experimenter doesn't want asked and answered publicly. One tactic I find useful is to ask those with questions to raise their hand. The experimenter can then go to the participant and hear the question privately. If the question is the type that should be publicly addressed, the experimenter can repeat the question and answer it publicly. If not, the question can be answered privately.

A final issue that often arises during experiments involves randomization. While it is now technologically very easy to use computers to generate random numbers, participants are often skeptical about the unbiasedness of this method. Thus economics experiments typically use actual randomizing devices (e.g. dice as in Croson and Johnston 2000) to implement random outcomes.

Once the experiment is over, participants receive their earnings (in cash) and are asked to sign a receipt before they leave. In many economics experiments it is important that these earnings be paid privately. Theories often assume that individuals value money absolutely, without social influence or comparative preferences. If we want to test the implications of such a theory, it is important to implement an experimental design where these other considerations are not present. Of course, one can also test the assumptions of such a theory, at which point knowing the earnings of others is an important design considerations. When payments are private, participants often want to know how well

INER 10,1_f11_131-148 4/5/05 5:06 PM Page 143

they did relative to others. I try hard to avoid answering that question, reminding them that earnings are private information.

A final end-of-experiment consideration involves debriefing. As with our first topic (human subject committees) extensive debriefing is not typically needed when experiments involve no deception. Economists are concerned about communicating the nature of the experiment to participants when others will be coming to the lab later in the day or the week to participate in another session or treatment. I usually ask participants to leave their names on a sign-up sheet if they are interested in a summary of the experimental objectives and results. I can then send them a writeup once all the data for the experiment is collected. This sheet also provides a vehicle to collect the contact information of participants who would like to participate in further experiments.

The procedures described in this section are by no means critical for psychologists (or even economists) to use in their implementations. No economist will object to a psychological experiment on the basis of the debriefing procedure used, for example. Hopefully it will provide some advice and insight into the nitty-gritty of running economics experiments and perhaps even some good ideas for psychologists to use in their procedures.

Data Analysis

A final methodological dimension along which the fields differ is the data analyses they employ. In my publications in both economics and psychology/management journals, I have found referees and editors surprisingly parochial about their favored statistical methods.

Both sides use nonparametric statistics, although economists favor Wilcoxon tests (Mann-Whitney U tests) after an influential paper demonstrating its power in ultimatum and dictator game data (Forsythe et al. 1994).[6] In contrast psychologists tend to use chi-squared and other tests that are appropriate for discrete data. Once the decision is made to move to parametric analysis, economists use regressions and psychologists ANOVAs. I have observed both these choices even when not appropriate – economists regularly use regressions even when the independent variables are discrete and psychologists regularly discretize their continuous independent variables in order to be able to use ANOVAs. These two techniques are, of course, quite related to each other and I have never experienced a situation where they yield qualitatively different responses. My interpretation of the source of these differences has to do with the reference disciplines to which the various

INER 10,1_f11_131-148 4/5/05 5:06 PM Page 144

experimentalists are trying to speak. For example, empirical and traditional economists use regressions; thus experimental economists use regression techniques to convince them that their results are valid.

A second methodological difference in analysis (and experimental design) is the concept of interactions. Economists are often uncomfortable with interaction effects (especially three-way or four-way interactions) while for psychologists interactions are the bread-and-butter of publication. My interpretation of the source of these differences involves the different objectives of the groups. In psychology, a result's cause can be understood only when one can make the result disappear. Thus interaction effects – treatments where the result is present and other treatments when it is absent ("now you see it, now you don't") illuminate the underlying cause of the result. In economics, the impulse is less to explore the cause of a given result than to explore its implications. Therefore the focus on interaction effects is minimal.

A final methodological difference concerns the ex-ante and ex-post hypotheses. This distinction is central in economics experiments. In part this stems from the existence of a core (deductive) theory which experiments test and from which ex-ante hypotheses can be easily drawn.

In contrast, Kerr (1998) describes a survey in which 156 researchers in social psychology, clinical psychology and sociology were asked how frequently they had personally observed some form of hypothesizing after the results are known (HARKing) in their professional life. Positive responses ranged from 32% to 48%. Respondents also advised using empirical inspiration for hypothesis generation 55% of the time, a similar rate as they advised the traditional ex-ante hypothesis testing methodology. Although no similar data exists among experimental economists, my interpretation is that this behavior occurs much less frequently, in part because of the existence of the unified body of theory. A hypothesis described as ex-ante which contradicted this core theory would be extremely suspicions. Second, an important contribution of experiments is seen in the profession to be highlighting the shortcomings and omissions of this core theory. Thus disproving the "hypothesis" is a perfectly acceptable and often sought-after outcome. Kerr (1998) presents an outstanding discussion of HARKing and its costs and benefits for individuals and the profession.

Conclusion

There are many similarities between experimental economics and psychological research. This article, however, was designed to illuminate some of the differences. I have discussed five methodological areas where experimental

INER 10,1_f11_131-148 4/5/05 5:06 PM Page 145

economists and experimental psychologists differ and have speculated as to the cause of those differences in each area.

As I hope I have conveyed in this article, there are no right and wrong answers. Each researcher needs to make their own methodological decisions based on the objectives of their experiment, the methods currently used in their field, and the audience they wish to address. My hope is that this article has described the methodology used in experimental economics to a non-economist audience and offered some insight into which of these considerations are viewed as non-negotiable (e.g. induced valuation) and which others may be relaxed (e.g. context). I have also offered some advice to psychologists who are interested in having their work read, cited and believed by economists.

That said, one might imagine the parallel article to this one; aimed at experimental economists and specifying what they need to do in order to have psychologists take their work seriously. My topic headings for that article would include context (adding more), interaction effects and mediation/moderation analyses as methods to eliminate alternative explanations for demonstrated phenomena.

I believe the questions that psychologists and economists are asking in their research have the potential to inform each other's fields deeply and profoundly. Understanding and accepting methodological differences is an important first step toward generating surplus-creating gains from trade, and I hope this article will help in taking that first step.

Notes

1. This article will be discussing the methodology of experimental economics and providing comparisons with that of experimental psychology. I mean the latter term to apply quite broadly and include research in management, dispute resolution and other related fields that use experimental data and experimental psychological methods.
2. While many (perhaps almost all) economists acknowledge that the theory does not predict and explain economic outcomes in *all* settings, the general consensus is that the theory does exceptionally well in predicting and explaining in a large variety of settings. Furthermore, there are costs and benefits of making the theory more complex. The benefits are clear, by adding extra parameters you can increase the predictive ability of the theory (e.g. by adding a parameter for the status-quo point around which the utility [value] function is asymmetric you can capture different risk preferences in gains and losses, or by adding others' payoffs into one's own utility function you can capture social factors like altruism, envy and inequality-aversion). However, there are also costs to adding extra parameters. The parameters need to be estimated in any given situation and when this is not possible the expanded theory makes no predictions. So expanded theory makes predictions they are more often correct, but there are fewer cases where any predictions are possible. Furthermore, the expanded theory is more complicated to work with and may not provide as many insights or unexpected

INER 10,1_f11_131-148 4/5/05 5:06 PM Page 146

predictions. Psychologists and economists have made different choices in this tradeoff between simplicity and descriptive ability.

3. The way to implement an infinitely repeated game in the lab is to derive the theory's predictions under a discount rate of δ, then implement the discount rate by setting the probability of the game ending in any given period equal to (1-δ).
4. The way to implement a one-shot game is through a strangers or 'zipper' design where each participant meets each other participant at most once during the experiment. (Kamecke (1997), Andreoni (1988), Croson (1996b), Andreoni and Croson (forthcoming)).
5. Shafir and Tversky (1992) did include some studies on the prisoner's dilemma game in which participants were paid for their earnings. Morris, Sim and Girotto (1998) ran similar experiments with a flat fee.
6. This paper also demonstrated significant differences between behavior in paid and unpaid ultimatum and dictator games.

References

Andreoni, James (1988). "Why Free Ride? Strategies and Learning in Public Goods Experiments." *Journal of Public Economics*, 37: 291–304.

Andreoni, James and Croson, Rachel (forthcoming). "Partners versus Strangers: Random Rematching in Public Goods Experiments," in Plott, C. and Smith V., editors, *Handbook of Experimental Results*. New York: Elsevier.

Binmore, Kenneth (1987). "Experimental Economics." *European Economic Review*, 31, 1/2: 257–264.

Binmore, Kenneth (1999). "Why Experiment in Economics?" *Economic Journal*, 109, 453: F16–24.

Boles, Terry; Croson, Rachel; Murnighan, Keith (2000). "Deception, Retribution, and the Negotiation Process." *Organizational Behavior and Human Decision Processes*, 83: 235–259.

Bonetti, Shane (1998a). "Experimental Economics and Deception." *Journal of Economic Psychology*, 19, 3: 377–395.

Bonetti, Shane (1998b). "Experimental Economics and Deception: Response." *Journal of Economic Psychology*, 19, 3: 411–414.

Boyce, Rebecca; Brown, Thomas, McClelland, Gary, Peterson, George and Schulze, William (1992). "An Experimental Examination of Intrinsic Values as a Source of the WTA-WTP Disparity." *American Economic Review*, 82, 5: 1366–1373.

Buchan, Nancy; Croson, Rachel; Dawes, Robyn (forthcoming). "Swift Neighbors and Persistent Strangers: A Cross-Cultural Investigation of Trust and Reciprocity in Social Exchange." *American Journal of Sociology*.

Camerer, Colin; Hogarth, Robin (1999). "The effects of financial incentives in experiments: A review and capital- labor production theory." *Journal of Risk and Uncertainty* 19, 1–3: 7–42.

Carter, John; Irons, Michael (1991). "Are Economists Different, and If So, Why?" *Journal of Economic Perspectives*, 5: 171–177.

Cox, James; Isaac, Mark R. (1986). "Experimental Economics and Experimental Psychology: Ever the Twain Shall Meet?" in *Economic Psychology: Intersections in Theory and Application*, MacFadyen and MacFadyen, editors, North-Holland, Amsterdam: 647–669.

INER 10,1_f11_131-148 4/5/05 5:06 PM Page 147

Croson, Rachel (1996a). "Information in Ultimatum Games: An Experimental Study." *Journal of Economic Behavior and Organization*, 30: 197–212.

Croson, Rachel (1996b). "Partners and Strangers Revisited." *Economics Letters*, 53: 25–32.

Croson, Rachel (1999). "The Disjunction Effect and Reason-Based Choice in Games." *Organizational Behavior and Human Decision Processes*, 80: 118–133.

Croson, Rachel (2000). "Thinking like a Game Theorist: Factors Affecting the Frequency of Equilibrium Play." *Journal of Economic Behavior and Organization*, 41: 299–314.

Croson, Rachel (forthcoming). "Why (and How To) Experiment: Methodologies from Experimental Economics." *University of Illinois Law Review*.

Croson, Rachel; Boles, Terry; Murnighan Keith (forthcoming). "Cheap Talk in Bargaining Experiments: Lying and Threats in Ultimatum Games." *Journal of Economic Behavior and Organization*.

Croson, Rachel; Johnston, Jason (2000). "Experimental Results on Bargaining under Alternative Property Rights Regimes." *Journal of Law, Economics and Organization*, 16: 50–73.

Croson, Rachel; Mnookin, Robert. (1997). "Does Disputing through Agents Enhance Cooperation? Experimental Evidence." *Journal of Legal Studies*, 26: 331–345.

Cummings, Ronald; Holt, Charles; Laury, Susan (2002). "The Georgia Irrigation Reduction Auction." Working Paper, Georgia State University, Department of Economics.

Dyer, Douglas; Kagel, John; Levin, Dan (1989). "Comparison of Naive and Experienced Bidders in Common Value Offer Auctions: A Laboratory Analysis." *Economic Journal*, 99, 394: 108–115.

Dyer, Douglas; Kagel, John (1996). "Bidding in common value auctions: How the commercial construction industry corrects for the winner's curse." *Management Science*, 42, 10: 1463–1476.

Eckel, Catherine; Grossman, Phillip (forthcoming a). "Volunteers and Pseudo-Volunteers: The Effect of Recruitment Method on Subjects' Behavior in Experiments," *Experimental Economics*. Eckel, Catherine; Grossman, Phillip (forthcoming b). "Differences in the Economic Decisions of Men and Women: Experimental Evidence," In Plott, C. and Smith, V. *Handbook of Experimental Results*. New York: Elsevier.

Forsythe, Robert; Horowitz, Joel; Savin, N.E.; Sefton, Martin (1994). "Fairness in Simple Bargaining Experiments." *Games and Economic Behavior*, 6: 347–369.

Frank, Robert; Gilovich, Thomas; Regan, Dennis (1993). "Does studying economics inhibit cooperation?" *Journal of Economic Perspectives* 7: 159–171.

Frank, Robert; Gilovich, Thomas; Regan, Dennis (1996). "Do economists make bad citizens?" *Journal of Economic Perspectives*, 10: 187–192.

Friedman, Daniel; Sunder Shyam (1994). *Experimental Methods: A Primer for Economists*. New York: Cambridge University Press.

Gneezy, U., and A. Rustichini (2000). "Pay Enough or Don't Pay At All." *Quarterly Journal of Economics*, 791–810.

Hertwig, Ralph; Andreas Ortmann (2001). "Experimental Practices in Economics: A Methodological Challenge for Psychologists?" *Behavior and Brain Sciences*, 24, 383–451.

Hey, John (1998). "Experimental Economics and Deception: A Comment." *Journal of Economic Psychology*, 19, 3: 397–401.

Holt, Charles (1995). "Psychology and Economics: A Discussion of Methodology." University of Virginia, Discussion Paper.

Kamecke, Ulrich (1997). "Rotations: Matching Schemes That Efficiently Preserve the Best Reply Structure of a One Shot Game: Note." *International Journal of Game Theory*, 26, 3: 409–417.

INER 10,1_f11_131-148 4/5/05 5:06 PM Page 148

Laband, David; Biel, Richard (1999). "Are Economists More Selfish than Other 'Social' Scientists?" *Public Choice*, 100, 1–2: 85–101.

Loewenstein, George (1990). "Experimental Economics from the Vantage-Point of Behavioral Economics." *Economic Journal*, 109, F25-F34.

Marwell, Gerald; Ames, Ruth (1981). "Economists Free Ride, Does Anyone Else?: Experiments on the Provision of Public Goods IV." *Journal of Public Economics*, 15: 295–310.

McDaniel, Tanga; Starmer, Chris (1998). "Experimental Economics and Deception: A Comment." *Journal of Economic Psychology*, 19, 3: 403–409.

Morris, Michael; Sim, Damien; V. Girotto (1998). "Distinguishing Sources of Cooperation in the One-Round Prisoner's Dilemma: Evidence for Cooperation Decisions Based on the Illusion of Control." *Journal of Experimental Social Psychology*, 34, 494–512.

Ortmann, Andreas; Hertwig, Ralph (1997). "Is Deception Acceptable?" *American Psychologist*, 52, 7: 746–747.

Ortmann, Andreas; Hertwig, Ralph (1998). "The Question Remains: Is Deception Acceptable?" *American Psychologist*, 53, 7: 806–807.

Ortmann, Andreas; Hertwig, Ralph (2002). "The Costs of Deception: Evidence from Psychology." *Experimental Economics*, 5: 111–131.

Plott, Charles (1982). "Industrial Organization Theory and Experimental Economics." *Journal of Economic Literature*, 20: 1485–1527.

Plott, Charles (1991a). "Will Economics Become an Experimental Science?" *Southern Economic Journal*, 57, 4: 901–919.

Plott, Charles (1991b). "Economics in 2090: The Views of an Experimentalist." *Economic Journal*, 101, 404: 88–93.

Plott, Charles (1994). "Market Architectures, Institutional Landscapes and Testbed Experiments." *Economic Theory*, 4: 3–10.

Roth, Alvin (1986). "Laboratory Experimentation in Economics." *Economics & Philosophy*, 2, 2: 245–273.

Roth, Alvin (1988). "Laboratory Experimentation in Economics: A Methodological Overview." *The Economic Journal*, 98, 393: 974–1031.

Roth, Alvin (1991). "Game Theory as a Part of Empirical Economics." *Economic Journal*, 101, 404: 107–114.

Roth, Alvin (1994). "Let's Keep the Con out of Experimental Econ: A Methodological Note." *Empirical Economics*, 19, 2: 279–289.

Shafir, Eldar and Tversky, Amos (1992). "Thinking through uncertainty: Nonconsequential reasoning and choice." *Cognitive Psychology*, 24, 4: 449–474

Smith, Vernon (1976). "Experimental Economics: Induced Value Theory." *American Economic Review*, 66, 2: 274–279.

Smith, Vernon (1989). "Theory, Experiment and Economics." *Journal of Economic Perspectives*, 3, 1: 151–169.

Smith, Vernon (1994). "Economics in the Laboratory." *Journal of Economic Perspectives*, 8, 1: 113–131.

Smith, Vernon and Walker, James (1993). "Monetary Rewards and Decision Cost in Experimental Economics." *Economic Inquiry*, 31, 2: 245–261.

Tversky, Amos and Shafir, Eldar (1992). "The Disjunction Effect in Choice under Uncertainty." *Psychological Science*, 3: 305–309.

Yezer, Tony; Goldfarb, Robert; and Poppen, Paul (1996). "Does Studying Economics Discourage Cooperation?: Watch What We Do, Not What We Say or How We Play." *Journal of Economic Perspectives*, 10, 1: 171–186.