Invited Presentation

Experimental Methods in Agricultural and Resource Economics: How Useful Are They?

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Experimental economics has the potential to fill some of the gaps in the economist's tool kit. This article describes experimental economics, its advantages and disadvantages, and why this tool might be a good choice in some situations. The article summarizes the history of its use by agricultural and resource economists. An illustrative example compares laboratory experiment data with survey data.

I. Why Would Agricultural and Resource Economists Want to Use Experimental Methods?

Economics traditionally has been defined as outside the purview of controlled experimentation. These words from a first-year graduate textbook are typical: "Economics is a non-experimental science . . . [in economics there is] a lack of controlled experimentation" (Kennedy, p. 7 and p. 1).

Economists generally estimate or infer demand and supply on the basis of variations in market prices and quantities sold. This inference is necessary because economists cannot observe directly the relationships that govern demand and supply.

Sometimes there are problems with available market data. One problem is confounding by extraneous variables. Econometric control to account for this confounding is limited by what we think those extraneous variables are. Another problem is that records sometimes are not kept for market data of interest, and it often is difficult or expensive to collect the relevant data.

Market data just are not much help for many interesting questions. For example, economists' more sophisticated models have subtle behavioral assumptions for which "natural" markets provide little evidence (either pro or con). Agricultural and resource economists increasingly are asked for insights about a whole range of nonmarket goods and services. Innovative methods are needed to estimate demand and supply for such goods, and the welfare implications of policies that affect their provision. Economists have explored hedonic, travel cost, and contingent valuation techniques for addressing such issues. These techniques have been shown to be useful in some situations. However, their limitations suggest the need to continue searching for other approaches. Some researchers have turned to Monte Carlo simulations for testing theories. In comparison, experimental economics provides a way to generate real data from real people for situations where there are no market data.

Experimental economics is a possible complement to the economist's existing set of tools. Two directions seem especially important with respect to the potential for agricultural and resource economists to use experimental economics. One is oriented toward valuation, including work to define mechanisms that reveal preferences for goods such as visibility and clean air. Another looks at the efficiency implications of alternative mechanisms to control externalities effectively (such as tradeable permits vs. taxes vs. standards), or of different methods for providing public goods.

What Is *Experimental Economics?*

Definitions and dividing lines vary. A useful distinction to keep in mind is that scientific experiments examine the impact of various changes that are introduced by the scientist, while holding all other factors constant. Controlling the variations permitted in an experiment allows more accurate inferences about cause-and-effect relationships.

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Economists often try to approximate this condition by using econometrics to account for extraneous factors, but without having the ability to control changes in the variable of interest. Because economists study the behavior of real people, experiments must not overstep what it is ethical to do to people, and they must consider the cost of using human subjects. The result often is relatively simple experiments that are not expected to harm subjects, and small numbers of subjects. However, experimental economics differs from other empirical economics by being designed so that the experimenter actually does control the changes in specified variables, while holding the other variables constant. This can be accomplished using pencil and paper in a classroom, using group settings, using computers in a network, etc.

In an early experiment, Edward Chamberlin *induced* the demand and cost structure by giving each graduate student subject a card with a value (or cost). Their hypothetical reward was the difference between what the card said and the negotiated price. This example illustrates the advantage that experiments do *not* require assumptions about functional forms and product homogeneity; such assumptions would be needed to test whether a natural market yields competitive price and quantity predictions. (In Chamberlin's experiments, prices did *not* converge to the theoretical equilibrium. One explanation was that the students did not know what the equilibrium price should be. Others were the lack of monetary incentives meant that they did not try to reach the equilibrium and absence of repeated market experience.)

What Can Be Done with Experiments in Economics?

There are several types of objectives for experiments (Davis and Holt, p. 19; Roth, 1987b):

- Testing and modifying formal theories. This is what Roth calls "Speaking to Theorists." The researcher must specify a theory's structural assumptions so that the behavioral hypotheses can be tested.
- Providing direct input into the policy-making process, or "Whispering into the Ears of Princes." Sensitivity tests, or stress tests, can make the setting more like the real world.
- Gathering data on interesting phenomena and important institutions, or "Searching for Facts." Documentation of empirical regularities can help in understanding how people and economies operate, even when satisfactory theories are not available.

Hey (p. 225) emphasizes three types of contributions that experiments can make: 1) tests of dynamic and equilibrium behavior under different forms of market organization (where there currently is little theory), 2) examining auctions and bargaining (where the wealth of theory leads to indeterminate predictions), and 3) examining decision making under risk (which has lead to the near-overthrow of subjective expected utility theory).

Market data are not available for filling many of the gaps in our understanding of behavior and how economies operate. Charles Plott (1991, p. 909) echoes the sentiments of many experimental economists: "The only practical source of data that can be obtained within an appropriate time frame and serve as a guide for many of the newly developed theories is the laboratory." Hey (p. 2) argues that economic data generated under controlled conditions lead to better understanding of existing theories and stimulate new ones. He sees such data as being of better quality (and possibly of a lower cost) than data currently available. "We need to know both that the economic theory works in its own environment and that it survives the transition to the real world. Experimental methods allow us to separate out these two stages." (Hey, p. 15)

II. What are the Pros and Cons of Experimental Economics?

"Although no panacea, laboratory techniques have the important advantage of imposing professional responsibility on data collection, and of allowing more direct tests of behavioral assumptions." (Davis and Holt, p. 4)

Advantages

Shogren and Nowell (p. 107) cite five reasons given by Hoffman and Spitzer (1985) for why economists are turning to experimental methods:

- By controlling for extraneous factors, they isolate basic behavioral characteristics to provide the cleanest possible tests for fundamental theories.
- They examine the predictive power of a theory.
- They can test the predictive power of alternative theories.
- They can suggest directions for a new theory.
- They are inexpensive and can be conducted quickly, compared with gathering data from natural markets.

Davis and Holt (p. 14) and Hey (p. 222) stress reliability and control as advantages of the experimental approach. Reliability occurs because the possibility for independent verification of findings with a new data set encourages careful collection of relevant data in an experiment. This makes it less likely that the researcher will be shown to be in error. Reliability is enhanced by procedural regularity, which makes it relatively inexpensive to replicate lab experiments. Computers can make this easier, but hardly are necessary.

Control comes from the ability to manipulate the lab conditions and evaluate alternative theories and policies—and be sure what influences have changed across alternatives (compared with the usual hope that correct specification of the econometric model allows the researcher to keep everything else equal). Of course, keeping constant those variables that you want kept constant and getting the right type and magnitude of variation in the others means that the researcher must carefully specify what the changes are. Davis and Holt (p. 19) emphasize that this "forces the theorist to specify models in terms of observable variables, and forces the data collector to be precise and clever in obtaining the desired control."

One important control feature is motivation. Control is more complete if the rewards are salient and correspond to the incentives assumed in the relevant theory. For this purpose, salient means that changes in a subject's decisions affect his/her rewards. Another control feature is unbiasedness, to avoid "leading" subjects, even inadvertently. This often is implemented by avoiding references to economic jargon or any particular commodity. A third control feature is calibration, for establishing a clear basis for comparison. An example might be baseline data collected without the experimental "treatment." As is the case for other empirical tests, failure to reject a maintained hypothesis is stronger when competing models can be rejected. The strength is greater to the extent that the experiment is designed so that it will parallel the structure of a naturally occurring economic process (often called external validity in other applications).

A major advantage is the ability to test a general model in a simple case. If it fails to work in a simple case, then it is unlikely to work in a more complicated (real world or natural) setting. Using the laboratory, it is easy to change the model to see how it might be made to work better in the simple case—and perhaps in the field as well.

Experiments can be used to evaluate the efficiency of alternative institutions or market structures *even if there is no existing good theoretical representation of the processes* that occur. Perhaps most important, the laboratory approach points out the importance of *trading rules* and *institutions* to market outcomes. In an early demonstration of this, Smith got rapid convergence to the equilibrium price when he repeated Chamberlin's experiment, but with a buyer's or seller's bid announced to the entire group (double continuous auction) rather than just having pairwise negotiations. More recently, Smith and Williams (1992) simulated stock markets to show that providing common information does *not* assure that all subjects will have about the same expectations regarding future prices. Instead individual valuations depend on their expectations about how others will behave. This leads to bubbles and crashes, and *slows* the convergence predicted by the rational-expectations model.

Disadvantages

In practice, any economist's tool has both advantages and disadvantages. Some disadvantages are inherent problems that reflect differing philosophical approaches. Others are less crucial, and can be solved by additional research, conscientious implementation, and judicious wording when describing procedures and results.

A primary disadvantage of laboratory experiments is that they yield insights about theory, but not about the details of real economies (Plott, 1991). There is a temptation to forget this, and interpret too much about the applicability of lab results to natural markets. Of course, other techniques also suffer from the temptation to overinterpret results.

The relationship between laboratory manipulations and the real world has been the subject of much debate and limited consensus. Some observers criticize the artificiality of the laboratory environment, whether it is a pencil-and-paper exercise or a computer network. They worry that subjects are not "real" decision makers, in the sense that they are not making real decisions for real rewards. This disadvantage can be minimized by 1) replicating experiments with experienced decision makers, 2) making the structure of experiments more like decisions subjects face in real life (for design parallelism, or external validity),[1] and 3) using bigger rewards (and losses). These steps have not been implemented widely, mostly because of the costs of doing so. Some researchers

[1] Of course, it is extremely difficult to have lab experiments represent all the political and other pressures of any real-world policy decision.

are using such steps in the design stage to increase the accuracy and validity of contingent valuation surveys.

The use of student subjects is a shortcoming only if typical economic agents in relevant markets actually think differently from students. Most tests with different groups show similar results (Davis and Holt, p. 17). Even so, the laboratory approach needs additional validation by using subjects who are representative of the group of interest. There are many issues in experimental design (e.g., making sure the outcome is not "created" by the way the experiment is designed), implementation (avoid experimenter bias in providing instructions, keep good records, etc.), and interpretation (e.g., being careful not to over-generalize, especially when cultural and social class effects might be important). Such issues could explain why Davis and Holt sometimes do find differences in performance, for example from graduate students compared with undergraduates.

Smith (1976) discusses the theoretical basis for inducing values in experimental settings and the conditions under which it is valid to do so. It is more difficult to *elicit* preferences in a lab setting than to *induce* preferences. Part of the reason it is more difficult to elicit values is because auctions usually have been used in elicitation experiments and the dynamics of auctions are not completely understood. In these auctions, economists rely on several assumptions, such as presuming that experience will eliminate unprofitable behavior. There are further behavioral issues that are not completely understood; for example, whether agents have a bequest motive (Davis and Holt).

Although not an inherent limitation, jargon has delayed acceptance of experimental economics. For example, consider the connotation of "induced values." Like "induced labor," this often is seen as forced, unnatural. Some readers might think of "seduced," and accuse experimenters of warping the minds of subjects—who ought to be called participants (they are not subjected to pain or discomfort as might have been the case in early experiments in other disciplines). Smith and Williams (p. 118) use language that is less likely to be misconstrued: "Supply and demand curves in an experimental market are set by giving each experimental subject a different internal cost or value for one unit of a hypothetical commodity." Here, "internal value" or "assigned value" has a better connotation than "induced value."

"Control," and "manipulate" can be misinterpreted as meaning that the researcher controls the outcome (and therefore contributes nothing to knowledge). A "game" usually is something not to be taken seriously. "Treatment," "trial," and "experiment" have negative connotations for some people. Hey (p. 4) points out that even the word "laboratory" conjures visions of bubbling test tubes. The economist's lab could be a classroom, a networked computer room, or the world at large. Aside from connotations that can lead to unintended perspectives, special terms often have different definitions across studies in this relatively new field of study. Examples include trial, period, session, and round, which sometimes are used interchangeably. This can be corrected by careful definition of terms when describing an experiment.

As is true for other empirical economics approaches, practical concerns can make it difficult to implement experiments and get useful results. These can be minimized by careful attention to the following list:

- experimental design
- human subjects review
- (computer) programming
- pretesting to make sure the experimental design is appropriate (i.e., it does what you want it to do) and robust (i.e., it survives exposure to experimental subjects). (Hey pp. 17, 27)
- arrangements for and in the (computer) lab
- choosing the number of subjects, usually 10–20 per "treatment"
- recruiting enough subjects to avoid having to reschedule, getting them ready for experiments
- practice and/or questions before the experiment starts to test understanding of instructions
- enough assistants to cope when the computer crashes, and to make participant payments in a timely fashion
- type and amount of subject payments

Hey (p. 17) stresses that successful laboratory experiments are heavily dependent on painstaking and careful attention to detail. For example, Harrison and Morgan mention the need to make sure that changes in information presented to subjects are large enough to exceed their perceptive threshold. This can ensure that decisions resulting in losses smaller than the perceptive threshold are not misinterpreted as inconsistent with theory.

Perhaps the loudest criticism of the laboratory approach is that it is simplistic, and that support from such experiments does not mean the theory works in the real world. But if a theory that works in the lab fails in a "natural" market, this implies that the theory omits an important feature of the economy. As indicated above, if it does not work

Table 1. Number of articles published using experimental techniques, 1985–1993, selected journals.

Journal	1985	1986	1987	1988	1989	1990	1991	1992	1993[a]
AJAE	3	1	0	0	3	0	0	0	0
AJAE*	3	1	0	0	3	0	0	0	0
JEEM	1	1	1	0	0	2	1	2	1
Land Econ	0	0	0	1	0	0	0	0	0
Risk Anal.	0	0	1	2	1	3	1	4	0

in the lab, the theory is unlikely to work in a more complex natural market setting. Charles Plott (1991, p. 905) responds to the criticisms about simplicity and lack of reality:

> [E]conomies created in the laboratories might be very simple relative to those found in nature, but they are just as real. Real people motivated by real money make real decisions, real mistakes and suffer real frustrations and delights because of their real talents and real limitations. . . . simplicity is an advantage because it allows the reasons for a model's failure to be isolated and sometimes even measured.

III. What is the Trend in Use of Experimental Methods by Agricultural and Resource Economists?

We examined journal articles to determine the extent to which agricultural and resource economists have been using experimental techniques. The *American Journal of Agricultural Economics* (AJAE), *Journal of Environmental Economics and Management* (JEEM) and *Land Economics* represent major publication outlets for agricultural and resource economists. We omitted the journals published by the regional associations, as well as international journals, although they also are outlets for experimental research (e.g., Forster and Roberts; Menkhaus et al; Ruppel and Fuller). For comparison, we included *Risk Analysis*, an interdisciplinary journal that includes many social science articles.

Table 1 shows the pattern of experimental articles from 1985 through May 1993. Table 2 shows this as the percentage of the articles in each journal. The row marked AJAE* excludes the December issue, which contains the Proceedings of the association's annual meetings. The articles in that issue are based mainly on invited addresses and papers. Although the papers presented at the meetings contain original data, these papers are abstracted for the December AJAE. The result is that the papers in the December issue tend to be general and contain little data; they may not be representative of presentations at the meetings. Therefore, the data for AJAE are presented both with and without the December issue.

These tables account for most papers: full articles, notes, comments, replies, presented papers, and discussions. Information about original data is more likely to appear in a full length article. Therefore, the tables probably understate how much experimental methods are used based on appearance in the sample of journals. This contrasts with previous articles on data types and analysis methods, which counted only longer articles (Leontief; Morgan; Debertin and Pagoulatos).

The bottom panel of Table 3 gives the cumulative percentages for experimental articles from the journals that we checked. Our numbers tend to underestimate the proportion of experimental articles relative to the data (in the top panel) for the *American Economics Review* (AER) and the *Economic Journal*, because we included short articles while Leontief and Morgan did not. Furthermore, their category for those two journals is "Empirical

Table 2. Articles published using experimental techniques, expressed as a percentage of total articles published, 1985–1993, selected journals.

Journal	1985	1986	1987	1988	1989	1990	1991	1992	1993[a]
AJAE	1.5	0.5	0	0	1.8	0	0	0	0
AJAE*	2.3	0.7	0	0	2.4	0	0	0	0
JEEM	3.2	3.2	3.4	0	0	3.9	2.8	4.9	5.6
Land Econ	0	0	0	2.6	0	0	0	0	0
Risk Anal.	0	0	1.9	2.7	1.5	4.1	1.0	6.6	0

[a]Through May 1993

Table 3. Percentage of articles using experimental techniques, cumulative years, selected journals.

AER[a] 1972–1976	AER[a] 1977–1981	AER[b] 1982–1986	Economic Journal[b] 1982–1986
0.5	1.9	6.0	2.0

AJAE 1985–1993[c]	AJAE* 1985–1993[c]	JEEM 1985–1993[c]	Land Econ. 1985–1993[c]	Risk Analysis 1985–1993[c]
0.5	0.7	2.7	0.3	2.2

[a]Leontief 1982
[b]Morgan 1988
[c]Through May 1993

analysis based on artificial simulations and experiments," which might include articles that do not use experimental results (such as Monte Carlo methods). Thus agricultural and resource economists appear to use experimental methods at approximately the same rate as other economists, as represented in this small selection of journals. These tables demonstrate that it is relatively uncommon for economists in general to use experimental methods. Apparently their use also is uncommon in two other social sciences: political science and sociology. For the 1982–86 period, Morgan reports that experimental methods were used in 6 percent of the main articles in the *American Political Science Review* and 3 percent of articles in the *American Sociological Review*. A quick look at the 1991 issues of the *Journal of Marketing Research* shows much more reliance on this approach: 17 percent (8 of 46 articles) used experimental methods. Presumably, a similar pattern would emerge if we examined the entire 1985–93 time period for this journal.[2]

Although these tables do not reveal a time trend in the quantity of research using experimental methods, Plott (1991, p. 901) points out that the number of experimental papers has grown from 2–3 per year in the early 1970s to nearly 100 per year, in economics at large. They are scattered in a wide range of journals. Agricultural and resource economists are applying experimental methods to a wide range of topics. Agricultural economists have examined market behavior and efficiency (Ruppel and Fuller; Forster and Roberts; Rhodus et al.; Beilock et al; Buccola), utility functions (Bennett and Smith), agricultural lending (Stover et al.), subjective probabilities (Nelson and Bessler), agricultural marketing (Menkhaus et al.), and willingness to pay for safer food (Shin et al.).

Shogren and Nowell define two main areas of experimental application to environmental economics: institutional and valuation. We follow this distinction for resource economics. It is based on Smith's (1982, 1989) triad of experimental economics: the environment (agents, commodities, etc.), institution (rules governing communication and exchanges), and actual behavior. The institutional work compares different institutions and environments to examine their impact on behavior. Valuation experiments control institutions and behavior (using accurate demand-revealing mechanisms) to elicit underlying preferences from the environment.

Institutional experiments have examined the Coase theorem and the effects of relaxing its assumptions (Kahneman et al. 1990; Norton and Patrick; Hoffman and Spitzer 1982; Hoffman and Spitzer 1985 for review), the exploitation of common-pool resources (Walker and Gardner; Walker et al.), and the provision of public goods (Prince et al.; Brookshire and Coursey; Coursey and Smith; Marwell and Ames).

The goal of valuation experiments is to obtain values or preferences for nonmarketed goods. Resource economists have applied the contingent valuation method (CVM), in which survey respondents are directly asked for values, for this purpose. Experimental methods have been used to improve and validate the CVM (see Bergstrom and Stoll 1989 for a theoretical overview). The first step in this process is to have an accurate demand-revealing mechanism, which is then applied to a survey population. Experiments have been conducted to design these mechanisms (Brookshire et al.; also Prince et al. and Brookshire and Coursey, which were public good experiments designed explicitly for application to the CVM). Laboratory experiments can then be used to inexpensively refine the survey instrument, until a "best set" of

[2] Part of these differences can be explained by the assumptions various disciplines bring to their research. Those in marketing and psychology assume the importance of individual decision makers. Sociologists and political scientists tend to see individuals as "created" by the system of which they are a part. For these disciplines, organizational and group decision making is viewed as more important than individual decision making.

questionnaires has been developed for field application (Coursey and Schulze). There is a serious interest in this technique (Coursey and Schulze; Gregory and Furby; Coursey; Kealy et al. 1988, 1990; Bergstrom and Stoll, 1990; Bergstrom et al.; Kahneman and Knetsch).

One particular area of concentration in the valuation experiments has been in the discrepancy between willingness-to-pay (WTP) and willingness-to-accept (WTA), which was first observed in CVM studies (see Mitchell and Carson, or Fisher et al. for examples). A similar difference was observed in laboratory experiments (Coursey et al.; Knetsch and Sinden 1984, 1987). This was in conflict with the understanding of the theory at the time, which stated that, except for a small income effect, the two measures should be close for price changes (Willig) and for quantity changes (Randall and Stoll). However, Hanemann has since shown that, for quantity changes, the difference between the two measures also depends on the ease of substitutability for the good. If a good has no close substitutes, the two measures can differ greatly. This has been supported experimentally by Shogren et al. According to Smith (1989) this process of testing theory, improving the theory, and testing it again should be what economic methodology is all about, but is not what the profession does well.

IV. Illustrative Results: Comparing Experimental and Survey Prices

While the introduction of actual monetary incentives does not necessarily change behavior, some studies have found systematic differences between what people say as survey respondents and what they do in the context of a laboratory market (e.g., Coursey et al.; Knez et al.).

For example, Kachelmeier et al. used an experimental market to test findings by Kahneman, Knetsch, and Thaler (1986), based on survey data, that a producer price increase is judged acceptable or fair only if it is attributable to an increase in producer costs. Price increases that increased producers' profits rather than being justified by cost increases are considered unfair and are resisted. Kachelmeier et al. found that the effect of fairness on prices and seller profits declined over time, suggesting that market fairness may be a short-lived phenomenon and illustrating the importance of repeated market experience. They suggest that market participants' continuing concern for monetary consequences eventually overcame their initial fair response to a cost increase.

In the same spirit we have tested the hypothesis that responses from what people say as survey respondents and what they do in the context of a laboratory market differ, especially when the scenarios are rather complex. Further, we wished to determine which behavioral models best predicted outcomes. The typical benchmark model is the non-cooperative, game-theoretic solution called subgame perfect equilibrium (SPE). For any round (i.e., subgame), this requires that any set of strategies (i.e., one strategy for each player) be in Nash equilibrium (i.e., every player is happy with his/her decision, given the decision(s) by the other player(s)).[3] We examined how various models performed when we added two complexities to the situation: outside options and bargaining costs. A full description of the experimental conditions, including a detailed discussion of the results, can be found in Zwick and Weg.

Subjects in the survey condition were given the scenario presented in the appendix.[4] The parameters were chosen so that there were eight versions, shown in Part C of the appendix. Sixty-eight (different) subjects participated in the survey. These subjects were asked "What do you think would be the negotiated price of the used textbook if . . ." The end of the sentence described specific parameters of the situation corresponding to one of the eight versions.

Each of the 96 subjects in the experimental condition also saw one of the eight versions. They played that version six times, three times as a buyer and then three times as a seller. The actual payoff to a subject was the average net profit of two randomly selected games.

All subjects were undergraduate students at Penn State University. Subjects for the experimental sessions were recruited through classified advertising placed in the campus newspaper promising monetary reward contingent on performance in a bargaining study. Subjects in the survey conditions were undergraduate marketing majors who participated in the survey as partial fulfillment for a course credit.

The experiments were set up to examine how the division of gains is affected by the relative power held by the buyer and seller. One source of power comes from the availability of outside options. In this case the seller's outside option is the (fixed) price B_b at which the bookstore will purchase their

[3] Note that a Nash equilibrium could be Pareto-dominated by other outcomes (i.e., sets of strategies) as in the Prisoner's Dilemma.

[4] The title of the book differed between the lab experiments and the classroom survey. Otherwise, the scenarios were the same, except for obvious changes to explain the role playing scenario.

used text. The buyer's outside option is the (fixed) price B_s at which the bookstore will sell the book to them. A second source of power comes from differential bargaining costs. For example, the buyer and seller might incur different costs of making offers and counter offers because of telephone calls, legal advice, or database search. In this experiment, the bargaining cost is called a rejection fee, and must be paid by both buyer and seller whenever a price offer is rejected. The seller pays a rejection fee of c_s and the buyer pays a rejection fee of c_b.

The outside options, rejection fees and bargaining rules are known by all subjects. This is an example of what Hey (p. 24) calls a well-defined experiment. The experiment provides data on the extent to which subjects exploit their strategic power from an advantageous outside option or from relatively lower bargaining costs. This can be compared with what division would occur under alternative models that include fairness considerations.

Figure 1 illustrates the bargaining problem. Each participant's return is given on an axis; in our example the diagonal line would intersect the axes at \$20. The possible bargains are represented by the dashed portion of the line. The buyer (under the rules we consider) would not bargain for a price higher than the bookstore's selling price, B_s, while the seller will not bargain for a price lower than the bookstore's buying price, B_b. The outcome based on the split-the-difference (STD) rule

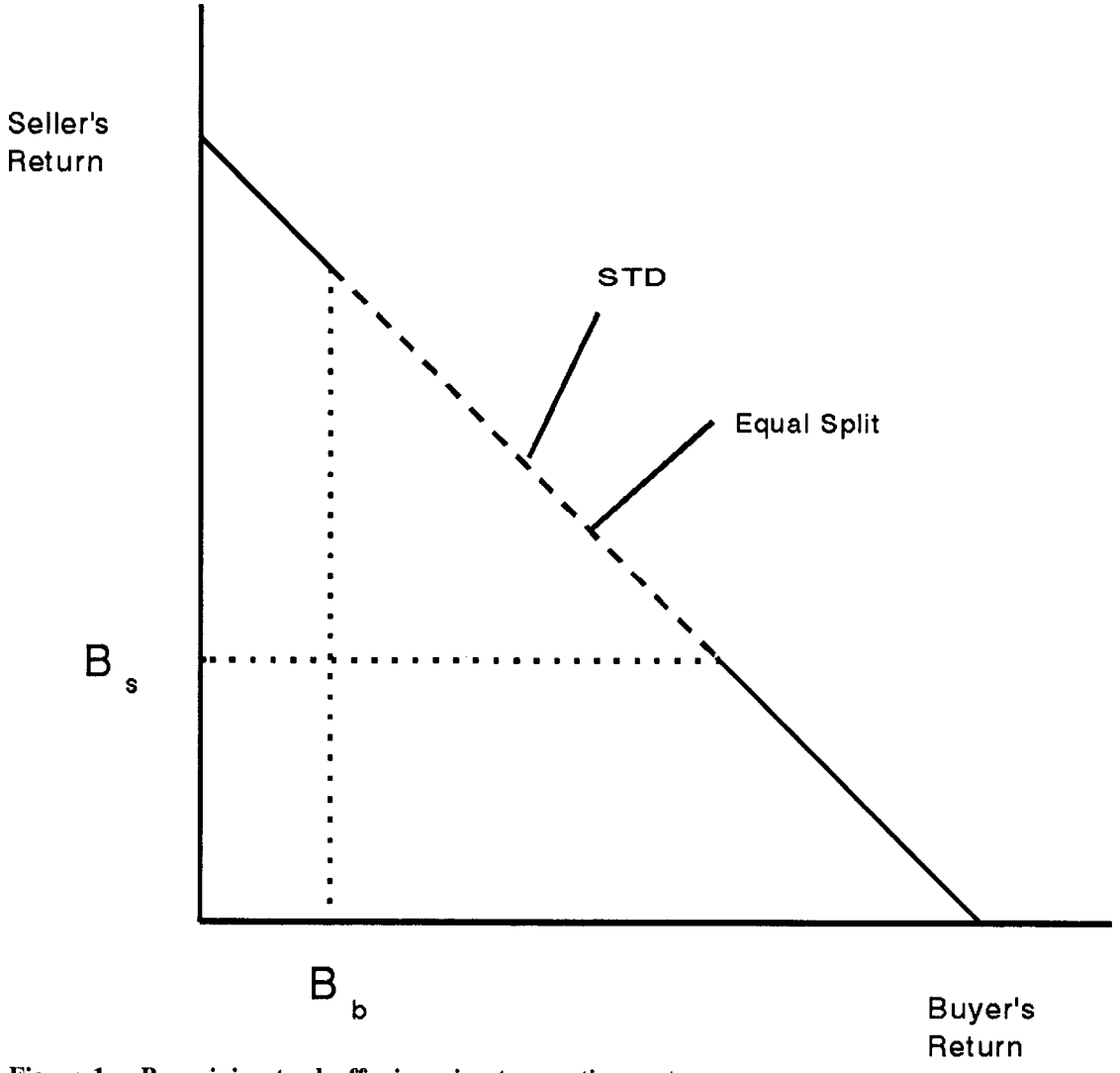

Figure 1. Bargaining tradeoffs, ignoring transaction costs.

Table 4. Means (and standard deviations) of final accepted prices

Bookstore Prices		Role Playing		Survey	
		$c_b < c_s$	$c_b > c_s$	$c_b < c_s$	$c_b > c_s$
Selling	Buying	Mean (STD)	Mean (STD)	Mean (STD)	Mean (STD)
20	6	10.44	14.94	10.85	13.00
		(1.54)	(1.41)	(3.15)	(3.57)
20	14	15.98	17.02	15.85	17.00
		(1.00)	(0.51)	(1.94)	(1.77)
14	0	6.82	9.91	6.97	8.74
		(3.74)	(0.89)	(3.93)	(3.29)
6	0	3.17	5.50	3.26	4.00
		(1.23)	(1.12)	(2.25)	(2.24)

is indicated as the midpoint of the dashed section.[5] The deal-me-out (DMO) outcome will either be "equal split" or the bookstore price, if one of the bargainers can get a better price there.[6] This diagram cannot display bargaining costs, so SPE is not represented.

Results:

Table 4 presents means (and standard deviations) of final accepted prices in the third iteration of the experimental session and the corresponding survey prices.[7] For example, the first row shows outside options where the bookstore will pay \$6 for a used book and sell it for \$20. When the buyer's rejection fee is lower than the seller's, the mean price in the experiments was \$10.44, compared with \$10.85 in the survey. When the buyer had a higher rejection fee, the experimental mean was \$14.94, compared with \$13.00 in the survey.

Zwick and Weg found that no single model considered in their study can account for the observed prices in all of the experimental conditions. However systematic patterns of behavior do emerge, and indicate that the laboratory prices were based on the relative advantages of the buyers and sellers.

Sources of power can be said to coincide when both the outside option and the rejection fee favor the buyer (or the seller). They conflict when one source of power favors the buyer and the other favors the seller. When both sources of power coincide, the experimental data show that the most common agreement is based on the split-the-difference (STD) rule. But when power cues conflict, the most common agreement is based on the deal-me-out (DMO) rule.

The SPE model did not perform as well as it has in similar bargaining games (Rapoport, et al; Weg and Zwick). SPE also has performed poorly in less similar games (see Shogren 1993 for examples). STD and DMO were much better predictors, as has been shown in other bargaining games (Binmore, et al., 1989, 1990).

Similar results emerge from the survey data with notable exceptions. First, STD cannot account for the \$14.94 average price in the experimental condition when a low-cost seller with an outside option of \$6 (i.e., bookstore buys for \$6) is facing a high-cost buyer with null outside option (i.e., used price at bookstore would take full \$20)—but the survey price of \$13.00 is exactly the STD price. The two prices are significantly different from each other; $t = 3.02$, $p < 0.004$.[8] Second, DMO accounts for both prices in the experimental condition when a high-cost buyer with outside options (i.e., used price from the bookstore) of \$14, and \$6, respectively, is facing a low-cost seller with null outside option (bookstore won't buy)—but the same is not true for the survey data. In both cases, average survey prices are significantly lower than the corresponding experimental prices (\$8.74 vs \$9.91 for a buyer with \$14 outside option, and \$4.00 vs \$5.50 for a buyer with \$6 outside option; $t = 2.20$,[9] $p < 0.03$; $t = 3.19$,[10] $p < 0.004$, respectively).

Discussion

The difference in prices between the survey and the experimental data are revealing. They agree

[5] *Split-the-difference (STD).* Bargaining costs are ignored by the subjects. Each player is assured the gain from his/her outside opportunity and the remaining surplus is split equally.

[6] *Deal-me-out (DMO).* Bargaining costs are ignored. The price is set to \$10 (half of the surplus available to divide), unless one of the bargainers can get a better price at the bookstore. If that is the case, the price is set to that better price.

[7] The third iteration is used because it represents the most experienced round with the subject in his/her initial role.

[8] Since the assumption of equal variances is rejected ($F(53), 10) = 6.43$, $p < 0.003$), we report the correction for unequal variance.

[9] Since the assumption of equal variances is rejected ($F(53, 9) = 13.56$, $p < 0.003$), we report the correction for unequal variance.

[10] Since the assumption of equal variances is rejected ($F(52, 9) = 4.02$, $p < 0.03$), we report the correction for unequal variance.

under the relatively simple scenario when the direction of power coincides in terms of costs and outside opportunities. Under this condition there is a strong socially accepted norm favoring split-the-difference as the common resolution. Both survey and experimental subjects adhered to this norm. A number of bargaining experiments have found that fairness is an important consideration to subjects (see Thaler for an overview). Apparently, subjects feel a sense of loyalty to both themselves and their opponent. Defining loyalty to the subject in other ways can alter behavior (Shogren 1989).

Prices are more complex in the conflicting condition, where the direction of power in terms of costs and outside opportunities differs, reflecting the intricacy of the situation. This is where survey data differ from laboratory data.

Subjects in the survey underestimated the power that can be derived from low bargaining costs. This is understandable given the fact that bargaining costs are harder to quantify in the survey situation. It suggests that real experience is needed to understand the importance of bargaining costs for determining negotiated prices. The difference between survey and experimental prices in the more complex situation demonstrates the possible fragility of estimating market prices based on one-shot survey responses.

V. What Next?

We have argued that experimental economics can be helpful for testing and developing theory, and for insights into empirical issues. Then why is this approach not used more widely by economists in general and by agricultural and resource economists in particular? One major reason is that graduate economics training does not leave room for the possibility of experimentation. The perspective is that it is unnecessary, impossible, rarely possible, or not as good a technique as econometrics. "Economics as currently learned and taught in graduate school and practiced afterward is more theory-intensive and less observation-intensive than perhaps any other science (Smith 1989, p. 151)." Kagel (p. 156) quotes a colleague: "I am a 'true believer' in microeconomic theory, and as a result I am perfectly willing to accept mathematical proofs without experimental verification." On the empirical side, a Penn State econometrics instructor dismissed experimental methods by saying: "We're not dealing with candy bars." Quoting McCloskey (p. 1126) ". . . when we seek the facts of the world, we pretend that only the 'experiments' suitable to regression analysis are appropriate. I once had a graduate student who thought that the very word 'empirical' meant 'regression on someone else's data.' "

Most experienced experimentalists (e.g., Smith 1989) can tell of hostile reviewers who hold experimental results to a high standard: they criticize the results of an experiment if it contradicts a theory, but say that the experiment "tells us what we already know" if it is consistent with theory. These attitudes and beliefs make it necessary, before undertaking experiments, to have a good understanding of procedures and methods. Two good sources are Davis and Holt, and Hey. Theoretical and applied reviews of the literature are Smith (1982, 1987, see also 1991 for his collected papers), Hoffman and Spitzer (1985), Plott (1982), and Roth (1987a, 1987b, 1988). Palfrey and Porter provide guidelines for manuscript submission (for *Econometrica* specifically, but they could be used for any journal).

Agricultural and resource economists who are as interested in changing the way the world operates as in developing theory often wonder whether policy makers will pay attention to the results of experiments. Little evidence is available to answer this query, mostly because so few experiments have been conducted to examine policy questions. The work of Plott [1987] is an exception. Economists and others are beginning to fill this void. For example, researchers in Southern California are responding to Clean Air Act amendments by using laboratory markets to find the most efficient rules for trading the rights to emit specific pollutants (Smith and Williams, p. 121). Similar experiments are underway to develop trading rules for markets in natural gas and electricity. Smith has been using lab experiments to help develop market skills among utility managers and government officials from Eastern European countries.

Will policy makers listen? At this point, laboratory results showing that specific ideas will not work are likely to have more influence than results that support pet ideas. The reasons for this can be found among the advantages and disadvantages listed above. The lack of representative samples facing the prospect of sizeable gains and losses, along with scarcity of validation studies, are likely to make policy makers more skeptical of relying on positive results than on negative results. It should be kept in mind, however, that relatively new fields can experience a rapid increase in their influence, as more research results become available, to fill niches not covered by other approaches. The increased use of contingent valuation methods is an example of this, despite the continued controversy over what it can achieve and

how to accomplish its promise effectively. The increased cost of conducting representative surveys (of many types) suggests that there may be an important niche for experiments oriented toward explicit policy issues.

Finally, after conducting an experiment, progress can be made by answering the following questions:

Did the experiment achieve what we wanted it to achieve?

If not, why not?

What new, different, or stronger hypotheses are suggested?

Of course, similar questions should be answered for other economic methods. The beauty of experiments is that these questions often are easier to answer for this approach than for others.

Appendix

A: Scenario Given to Subjects

The Market for Used Textbooks

As you know students buy and sell used textbooks to the bookstore at a price determined by the bookstore. Also, students can buy and sell used textbooks directly with each other. We are interested in the price of a used textbook that is being exchanged between two students.

Consider the following situation:

The Seller

A student wants to sell his used textbook, *Marketing Communication. THE MONEY HE WILL GET FROM THE SALE IS HIS TO KEEP.* Thus he can either sell his used textbook directly to another student at a price determined by direct negotiation between the two of them or sell it to the bookstore at the bookstore's buying price (nonnegotiable).

The book by itself has no value to the Seller other than its selling price.

The Buyer

Another student is looking to buy *Marketing Communication.* This student's parents gave him $20 to buy this book. *HIS PARENTS AGREE THAT ANY MONEY LEFT OVER (IF HE BUYS THE BOOK FOR LESS) IS HIS TO KEEP.* Thus he can either buy the used textbook directly from the other student at a price determined by a direct negotiation between the two of them, or buy it from the bookstore at the bookstore's selling price (nonnegotiable).

The used textbook offered by the seller, and the used textbook offered by the bookstore are for all practical purposes identical.

Bargaining

The Buyer and the Seller negotiate the price of the used textbook. They take TURNS in proposing a price. The buyer proposes first.

The responding bargainer has three options:

(1) *Accept* the price offer. Here, the Seller sells the book to the Buyer at the agreed price.

(2) *Reject* the price offer. This choice signals an intent to continue the bargaining and it is the rejecting bargainer's turn to propose a price.

(3) *Quit.* Here, the Buyer buys the book from the bookstore for a fixed price, and the Seller sells the book to the bookstore for a fixed price.

Time is Money

As in real life, the bargaining itself incurs some costs. The costs are due to the time it takes to negotiate a deal, and as you well know "time is money."

Each time one of the bargainers *rejects* a proposal *both of them* must pay a certain rejection fee. We assume that rejection delays agreement, hence adds to the cost of time. These fees accumulate, of course, until an agreement is reached, or one of the bargainers chooses to quit.

Profits

The Seller's profit is the money he makes by selling the book, *minus* his rejection fees accumulated during the negotiation (if any).

The Buyer's profit is whatever is left of the $20 he got from his parents after paying for the book, *minus* his rejection fees accumulated during the negotiation (if any).

Summary

Two students bargain on the price of a used textbook. The bargaining procedure consists of alternating proposals about the price. Each bargainer may quit bargaining, the Buyer by buying the used textbook from the bookstore for a fixed price, and the Seller by selling his used textbook back to the bookstore at a fixed price. It is assumed that if one of the bargainers quits, it forces the other to do the

same. Any rejection involves a cost to both bargainers.

B: Survey Questionnaire

A generic question in the survey condition is the following: "Assume that the bookstore sells used *Marketing Communications* for $\$B_s$, and pays students $\$B_b$ for their used copies of *Marketing Communications*. What do you think would be the negotiated price of the used textbook if the rejection fees were $\$c_s$ to the Seller and $\$c_b$ to the Buyer?"

C: Parameters for Bookstore Prices and Per-period Bargaining Costs

For both the laboratory experiment and the survey, the parameters were:

Version	Bookstore selling price ($)	Bookstore buying price ($)	Seller's bargaining cost ($)	Buyer's bargaining cost ($)
1	20	6	2.00	0.10
2	20	6	0.10	2.00
3	20	14	2.00	0.10
4	20	14	0.10	2.00
5	14	0	2.00	0.10
6	14	0	0.10	2.00
7	6	0	2.00	0.10
8	6	0	0.10	2.00

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