Abstract - Laboratory experiments have proven increasingly useful in all areas of economics. This paper discusses the methodology of experimental economics, highlights its strengths and weaknesses, discusses many of the applications of experimental methods to public economics, and suggests topics in which future applications may also prove useful.

“Argument is conclusive . . . but . . . it does not remove doubt, so that the mind may rest in the sure knowledge of the truth, unless it finds it by the method of experiment. For if any man who never saw fire proved by satisfactory arguments that fire burns, his hearer’s mind would never be satisfied, nor would he avoid the fire until he put his hand in it that he might learn by experiment what argument taught.”

—Roger Bacon (1928)

INTRODUCTION

Like other sciences, economics is a quest for truth, based on the development of theory and on the ability of that theory to explain observed activities. However, unlike many other sciences, especially the natural sciences, economics faces substantial difficulties in empirically testing the predictive power of its theories using data from the naturally occurring world. Given the dizzying array and complexity of forces that operate in the market systems studied by economists, economists can never be quite certain that they are “holding constant” the many factors that may be driving individual choices, so that they can focus on the “true” driving factors that are the object of empirical testing. Methods for achieving such identification have become increasingly sophisticated over time, especially with the use of so-called natural experiments. Even so, there are few instances in which such identification is uncontroversial.

Partly as a response to this inherent difficulty, economists have begun to emulate the methods of natural scientists, by conducting carefully controlled laboratory experiments. The methodology of experimental economics has matured significantly over the last few decades, and has yielded many

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insights across all fields in economics. This paper surveys the broad practice of experimental studies in one such field, public economics, attempts to identify some of the main results—and the main limitations—of these studies, and suggests areas in public economics in which experimental methods may be usefully applied in the future.

Public economics has profited significantly from the use of laboratory experiments, for several reasons. Econometric data on research questions obtained from the naturally occurring world can be unreliable, can fail to show the variation or distinctions of interest, or can fail to provide sufficient identification. Indeed, in some cases data simply cannot be assembled because the real world setting of relevance does not exist. Laboratory experiments, on the other hand, provide a controlled environment for testing predictions; that is, experiments allow one to examine the mechanisms of interest, as well as the changes in these environments and institutions, in isolation from each other. However, a laboratory experiment is only as good as its design: if the institutions and environments imposed in the lab do not parallel systems of interest in the world, the resulting experimental data can be useless or misleading. Finally, public economics often examines topics such as public goods or tax compliance where traditional models of *homo economicus* have fallen short. Experimental work has been used to explore, develop, modify, and test new theories in these areas, as well as to test the assumptions of these theories, in ways that field data do not allow.

We begin with a discussion of laboratory methods in economics, where we stress many of the design elements laid out in Smith (1976, 1982). We then examine several areas of experimental research in public economics. We do not pretend to be exhaustive either in our selection of areas or in our discussion of specific research contributions, but rather intend to give a flavor for the many and varied areas to which laboratory methods have been applied:

- **Tax compliance**—Theoretical analyses based upon expected utility theory have proven unable to explain much compliance behavior. Further, field data on tax compliance are limited in quantity and quality, for obvious reasons. Experiments represent one useful avenue by which economists can study compliance.
- **Public goods**—Experiments have allowed economists to understand how, whether, and why people contribute to public goods.
- **Political economy**—Economists and political scientists alike have used experiments to study many aspects of political economics, including voting, committees, and legislative behavior. Game theory has played a huge role in the theoretical and experimental work in this area.
- **Tax incidence**—Simple market experiments have been used to investigate theories of tax incidence.
- **“Other” experiments**—We discuss a selection of experimental papers in other areas of public economics, including behavioral responses to taxes and various macroeconomic questions.

We focus on experiments in public economics and, thus, we do not discuss a range of other areas, such as experimental analyses of industrial organization (e.g., market structure experiments, game-theoretic models of imperfect competition), environmental economics (e.g., externalities, valuation, especially the divergence between willingness to pay and willingness to accept), risk and uncertainty, labor economics (e.g., job search, work
effort, reservation wages), asymmetric information, and health economics. For comprehensive surveys of experimental methods, see Davis and Holt (1993) and Kagel and Roth (1995).

In the conclusions we suggest areas where the next decade could see laboratory experiments in public economics having a significant impact on theory and policy.

THE METHODOLOGY OF EXPERIMENTAL ECONOMICS

The use of laboratory experiments in economics began in earnest in the early 1960s with work on resource allocation under alternative forms of market organization. Growth in its applications came with the establishment of a well-defined framework for experimental work by Smith (1976, 1982), and laboratory methods are now widely accepted as a methodological approach in the analysis of theory and policy.

Laboratory experiments seem particularly well suited for the study of some aspects of public economics. Unlike theoretical work, experiments are not as constrained by the same degree of simplification required in analytical studies, which allows the impact of numerous factors not amenable to theoretical work to be examined precisely and unambiguously in a controlled environment. Unlike traditional empirical work based on naturally occurring data, experiments generate data under settings in which there is control over extraneous influences. As we discuss later, there are some obvious limitations of experimental methods. However, given the weaknesses of theoretical and econometric work, there are compelling reasons for the use of experiments. In fact, experimental work has examined a remarkably rich range of public economics issues in areas that to date have not proven fully amenable to either theoretical or empirical analyses.

Experimental economics involves the creation of a real microeconomic system in the laboratory, one that parallels the naturally occurring world that is the subject of investigation and one in which “subjects” (usually students) make decisions that yield individual financial payoffs whose magnitude depends on their decisions. The essence of such a system is control over the environment, the institutions, the incentives, and the preferences that subjects face. Of these, control over preferences is particularly crucial. As emphasized by Smith (1976), “[s]uch control can be achieved by using a reward structure to induce prescribed monetary value on actions”; that is, in experimental methods, “…it is important that one be able to state that, as between two experiments, individual values (e.g., demand or supply) either do or do not differ in a specified way.”

Smith (1982) identifies a set of sufficient conditions for control over preferences to be established:

- **Nonsatiation**: Subjects prefer more to less of the reward medium.
- **Saliency**: The rewards received by subjects are related to their decisions, so that subjects recognize that their actions affect their outcomes.
- **Reward Dominance**: The rewards are large enough to offset any subjective costs or benefits that subjects place on participation in the experiment.

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1 Smith (1976) goes on to write that a maintained assumption in experimental economics is the following: “[g]iven a costless choice between two alternatives, identical except that the first yields more of the reward medium . . . than the second, the first will always be chosen (preferred) over the second.” It is in this sense that Smith (1976) refers to experimental economics as “induced value theory.”
Privacy: each subject knows only his or her own payoffs, so that they do not receive any subjective value from the payoffs of other subjects.

Nearly all recent experimental studies invoke these conditions.\(^2\)

Several other procedures should also be followed in experimental economics. For example, the experiment should be administered in a uniform and consistent manner to allow replicability. The experiment should not be excessively long or complicated, since subjects may become bored or confused. Subjects must believe that the procedures described to them are the procedures actually followed. The instructions provided to subjects should be understandable, should avoid the use of examples that lead subjects to anchor on certain choices that are the focus of the experiment, and should be phrased in “neutral” rather than “loaded” terms, in order to mask the context of the experiment and to avoid direct reference to the real–world phenomena under investigation. Neutrality increases the experimenter’s control over subject preferences, and avoids leading subjects to invoke different “mental scripts,” which may enable them to fill in (potentially) missing information in the instructions but which also may unpredictably influence their choices. It is sometimes claimed that the use of neutral instructions limits the ability to generalize from the experimental to the naturally occurring setting. In fact, however, as argued by most experimental economists, it is not possible to generalize beyond the laboratory unless one uses neutral instructions, since the experimenter cannot control (or induce) the values that subjects associate with loaded terms.\(^3\)

The main methodological advantages of laboratory experiments are, therefore, control (including data generation and replicability from this control) and flexibility.

First, laboratory methods allow the investigator to control the institutions and incentives facing subjects in order to investigate the main issue of interest. Relatedly and importantly, laboratory methods allow the investigator to generate data on individual and group choices in settings where these institutions and incentives can by varied singly and independently in order to examine responses to separate changes in these factors. In the naturally occurring world, such control is seldom if ever available. Data on, say, individual tax compliance decisions may not exist or, even if available, is likely to be imprecise, incomplete, and unreliable. Further, data on the responses of individuals to the myriad factors thought to influence the compliance decision may confound these many influences, making it difficult to disentangle their effects on compliance. Finally, it is likely that independent variation in many of these influences is simply not possible in the naturally occurring world, making it impossible to measure their effects on individual and group behavior. Laboratory methods allow such data to be generated in a setting in which each relevant factor can be separately manipulated and outcomes of subject choices reliably observed. They also allow other investigators to replicate more–or–less identical laboratory settings in order to determine whether the results are robust, an essential attribute of any science.

Second, laboratory methods give the investigator substantial flexibility in examining the effects of supposedly relevant considerations on individual and group decisions. It is difficult to imagine real–world settings in which, say, notions of fairness, altruism, and trust can be

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\(^2\) There are, however, some exceptions to these conditions. For example, one does not want to impose Privacy in an experiment on, say, altruism because the payoffs to others are an essential part of the research issue.

\(^3\) See Davis and Holt (1993) for examples of instructions for a variety of experiments.
manipulated in a reliable manner. Carefully designed experiments give the investigator precisely this flexibility, thereby allowing theories based on these notions (and the assumptions of these theories) to be tested in a controlled environment.

The next section discusses some of the main areas in public economics that have employed laboratory experiments.

APPLICATION OF EXPERIMENTAL METHODS TO PUBLIC ECONOMICS

Tax Compliance

Tax compliance is an area that seems especially amenable to laboratory experiments, given limitations in both theoretical and empirical approaches. Theoretical models are not able to incorporate fully, appropriately, or tractably many factors deemed relevant to the individual compliance decision. In a sense, the theory is both too complex and too simple. It is only in the simpler models that clear-cut analytical results can be generated on the compliance impact of basic policy parameters; when more complex dimensions of individual behavior are introduced, the theoretical results generally become ambiguous. Paradoxically, the theoretical models of individual choice are also too simple. There are numerous factors that affect the reporting decisions of individuals, but theoretical models are capable of including only a few.

Of perhaps more importance, the main difficulty in empirical studies of tax compliance is the absence of reliable information on individual compliance decisions: it is difficult to measure something that by its very nature people want to conceal.5

In contrast, laboratory methods allow many factors suggested by theory to be introduced in experimental settings. Also, experiments generate precise data on individual compliance decisions, which allow econometric estimation of individual responses in ways that are simply not possible with field data. For example, penalty rates do not vary exogenously across individuals and, thus, it has not been possible to estimate individual responses to changes in penalty rates. Independent variation in audit rates across individuals has also proven difficult to generate in natural settings. Examining with field data the impact of other factors often deemed important in compliance decisions (e.g., public good provision, audit selection rules, social processes and institutions) is even more problematic. Indeed, laboratory methods have examined a wide range of factors in the compliance decision, factors that have not proven amenable to either theoretical analyses or empirical analyses with field data.

The standard economics-of-crime model of compliance is based upon the work of Allingham and Sandmo (1972).

4 See Torgler (2002) for a survey of experimental analyses of tax compliance. Also, see Andreoni, Erard, and Feinstein (1998), Alm (1999), and Slemrod and Yitzhaki (2002) for surveys of the broader compliance literature.

5 For example, most empirical work for the United States has utilized data provided by the Internal Revenue Service (IRS) through its Taxpayer Compliance Measurement Program (TCMP), which contains a detailed line-by-line audit of a stratified random sample of roughly 50,000 individual tax returns conducted on a three-year cycle. These audits yield an IRS estimate of the taxpayer’s “true” income so that a measure of individual tax evasion can be calculated. However, until recently most researchers have not had access to the individual data, and instead have been forced to use TCMP data aggregated to the three-digit zip code level, an aggregate measure likely to comprise disparate elements of underreporting that reflect very different motivational factors. TCMP data also have some well-recognized deficiencies: the audits do not detect all underreported income, nonfilers are not captured, honest errors are not identified, final audit adjustments are not included, and there are few noneconomic factors to which the data can be linked; the TCMP has also been discontinued, and recent data are not yet available. Other data, such as aggregate measures of evasion or individual surveys, also present methodological problems.
In its simplest form, an individual is assumed to receive a fixed amount of income $I$, and must choose how much of this income to declare to the tax authorities and how much to underreport. The individual pays taxes at rate $t$ on every dollar $D$ of income that is declared, while no taxes are paid on underreported income. However, the individual may be audited with a fixed, predetermined probability $p$; if audited, then all underreported income is discovered, and the individual must pay a penalty at rate $f$ on each dollar that he or she was supposed to pay in taxes but did not pay. The individual’s income $I_c$ if caught underreporting equals $I_c = I - tD - f(\left(I - D\right)]$, while if underreporting is not caught income $I_r$, is $I_r = I - tD$. The individual chooses declared income to maximize the expected utility $EU(I)$ of the evasion gamble, or $EU(I) = pU(I_c) + (1 - p)U(I_r)$, where $E$ is the expectation operator and utility $U(I)$ is a function only of income. Numerous extensions to this basic model have been made since the original Allingham and Sandmo (1972) paper, extensions that enrich but also complicate the analysis and that render clear-cut analytical results quite difficult.6

Applying this framework to the study of tax compliance is straightforward, and, beginning with the seminal experimental study of Friedland, Maital, and Rutenberg (1978), the basic design of most compliance experiments has been similar. Human subjects in a controlled laboratory are told that they should feel free to make as much income as possible. At the beginning of each round of the experiment, each subject is given income and must decide how much income to report. Taxes are paid at some rate on all reported, but not on underreported, income. However, underreporting is discovered with some probability, and the subject must then pay a fine on unpaid taxes. This process is repeated for a given number of rounds. At the completion of the experiment, each subject is paid an amount (the accumulated earnings) that depends on his or her performance during the experiment. Various policy changes can easily be introduced, such as changes in audit probabilities or audit rules, in penalty rates, in tax rates, in public good provision, in institutions that affect tax equity or social norms, and in many other relevant policies or institutions.

The main fiscal variables of interest in the compliance decision are audit, penalty, and tax rates. For example, nearly all experimental studies have found that a higher audit rate (i.e., a greater probability of audit) leads to more compliance, with an estimated reported income–audit rate elasticity that varies within the narrow range of 0.1 to 0.2 (Friedland, Maital, and Rutenberg, 1978; Beck, Davis, and Jung, 1991; Alm, Jackson, and McKee, 1992a; Alm, Cronshaw, and McKee, 1993). Alm, McClelland, and Schulze (1992) showed that this impact appears to be small and nonlinear, so that the deterrent effect of a higher audit rate eventually diminishes; their results also suggested that many subjects appear to substantially overweight the probability of an audit, so that there is far more compliance than is predicted by expected utility theory, a result that is nearly universal across all experimental designs. Also, Spicer and Hero (1985) and Webley (1987) found that individuals who have been audited report more income post–audit than individuals who have not been audited, a result that contrasts somewhat with empirical evidence based on field data (Erard, 1992).

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6 Again, see Andreoni, Erard, and Feinstein (1998), Alm (1999), and Slemrod and Yitzhaki (2002) for discussions of the theoretical literature and its many extensions. Note that some recent work has incorporated aspects of “behavioral economics,” in which insights from other social sciences (especially psychology) are incorporated. See, for example, Yaniv (1999), Bernasconi and Zanardi (2004), and Dhani and al–Nowaihi (forthcoming).
Most studies have found that compliance increases but only slightly with increases in the fine rate on unpaid taxes (Friedland, Maital, and Rutenberg, 1978; Beck, Davis, and Jung, 1991; Alm, Jackson, and McKee, 1992a; Alm, McClelland, and Schulze, 1992). The estimated reported income–fine rate elasticity is typically less than 0.1.

Experimental evidence also suggests that a higher marginal tax rate often leads to less compliance, with an estimated reported income–tax rate elasticity of roughly –0.5 (Friedland, Maital, and Rutenberg, 1978; Alm, Jackson, and McKee, 1992a). However, Beck, Davis, and Jung (1991) and Alm, Sanchez, and de Juan (1995) found that reported income rises with higher tax rates, a result consistent with the standard theory.

Other factors have also been investigated. Higher (true) income leads to higher reported income, with an estimated reported income–income elasticity of roughly 3/4 (Alm, Jackson, and McKee, 1992a). In experiments that link the voluntary contributions and the compliance literatures, the presence of a public good financed by voluntary tax payments has been found to increase subject tax compliance in a nonlinear manner (Becker, Buchner, and Sleeking, 1987; Alm, McClelland, and Schulze, 1992; Alm, Jackson, and McKee, 1992a). Like group rewards, individual rewards (e.g., eligibility for a lottery if an audit indicates full reporting) can provide a significant positive inducement for greater compliance (Alm, Jackson, and McKee, 1992a). The presence of taxpayer uncertainty about either taxable income or the various fiscal parameters has an ambiguous impact on subject compliance (Spicer and Thomas, 1982; Friedland, 1982; Beck, Davis, and Jung, 1991; Alm, Jackson, and McKee, 1992a). The impacts of only a small number of sociodemographic variables have been examined in the laboratory. Friedland, Maital, and Rutenberg (1978) and Baldry (1987) found that older individuals are more compliant than younger ones; Baldry (1987) also found that women evade less than men.

Some other experimental studies have examined various aspects of tax administration. Audit selection methods that utilize information provided on the tax return, such as a “cutoff rule” (in which any subject who reports income lower than a cutoff level is audited with certainty) or a conditional back or forward audit rule (in which subjects found to be noncompliant in the current period are audited in past or in future periods with certainty) have been found to be far more effective in generating tax compliance than purely random selection methods, even when the random audit rate is 20, 30, or 50 percent (Collins and Plumlee, 1991; Alm, Cronshaw, and McKee, 1993). Compliance is affected by the uses of tax revenues and the decision process by which these uses are chosen. Alm, Jackson, and McKee (1993) found that subjects pay more in taxes when they choose the use of their taxes by voting than when the identical use is imposed upon them, that compliance is somewhat greater when the vote is decisive than when the vote is close, and that compliance is significantly and dramatically lowered by the imposition of an unpopular program. Surprisingly, Martinez–Vazquez, Harwood, and Larkins (1992) observed that compliance does not seem to be significantly affected by withholding systems, at least beyond the obvious channel that withholding reduces the opportunities for evasion. Subjects who find themselves unexpectedly underwithheld do not behave much differently than subjects who correctly anticipate the underwithholding, and there is little difference in compliance between taxpayers who are under– or overwithheld. Alm, McKee, and Beck (1990) found that a tax amnesty lowers post–amnesty tax compliance; however, they also showed that an amnesty that
is accompanied by greater post-amnesty enforcement efforts can overcome and even reverse the typical post-amnesty decline in compliance. In fact, their results showed that compliance is higher when an amnesty is followed by greater enforcement than when enforcement alone increases by an equal amount.

Of some importance, several recent studies have examined the role of various societal institutions and processes in tax compliance. Elster (1989) defines a “social norm” as a pattern of behavior that is judged in a similar way by others and that, therefore, is sustained in part by social approval or disapproval. Consequently, if others behave according to some socially accepted mode of behavior, then the individual will behave appropriately; if others do not so behave, then the individual will respond in kind. A social norm is, therefore, process oriented, unlike the outcome orientation of individual rationality. In the context of tax compliance, the existence of a social norm suggests that an individual will comply as long as he or she believes that compliance is the social norm; conversely, if noncompliance becomes pervasive, then the social norm of compliance disappears.7 Torgler (2003) and Gërxhani and Schram (2006) found experimental evidence that this social norm can be affected by the institutions that face individuals, by individuals’ attitudes toward these institutions, and by individual participation in the selection of those institutions. Other experiments also demonstrate the role of social norms in compliance. For example, Alm, Sanchez, and de Juan (1995) demonstrated that there are significant differences in the compliance behavior of student subjects in similar experiments but in different countries (Spain versus the United States), thereby suggesting that societal attitudes toward compliance exert a measurable impact on tax compliance.8 Webley, Robben, Elffers and Hessing (1991) found that individuals who have a negative attitude toward government comply less as a result.

In other work on institutions and processes, Alm, Jackson, and McKee (1992b) found that institutional uncertainty about tax, penalty, and audit rates can systematically affect compliance. Alm, Jackson, and McKee (1993) demonstrated that government can affect compliance by ensuring that individuals have a say in the decision process by voting on the specific way in which their taxes are to be used (e.g., donations to the student health center versus the university president’s office) and by spending taxes in ways consistent with citizen preferences. Similarly, Alm, McClelland, and Schulze (1999) showed that compliance is decreased, often collapsing virtually to zero, when there is a social expression via group selection of the fiscal regime of a willingness to tolerate tax evasion, such as a majority vote in favor of reduced enforcement on detected evasion; however, they also find that compliance can be increased when there is a social expression of an unwillingness to tolerate tax evasion. In earlier work, Spicer and Becker (1980) conducted experiments that suggested that perceptions of “fiscal inequity” affect compliance: compliance is lower (higher) among subjects who are told that their tax rate is higher (lower) than that of others. More recently, Alm and McKee (2004) conducted experiments in which one’s chance of audit depends on

7 There are other concepts that describe the same basic phenomenon as social norms, such as “psychic cost” (Gordon, 1989), “tax morale” (Pommerehne, Hart, and Frey, 1994), “moral sentiments” (Érard and Feinstein, 1994), “group conformity and social customs” (Myles and Naylor, 1996), and “intrinsic motivation” (Frey, 1992).

8 Compliance was significantly lower for Spanish subjects than for U.S. subjects, even though the experimental design was similar. These results are consistent with the estimation results of Alm and Torgler (2006), who found a much lower social norm of compliance in Spain than in the U.S.
one’s own compliance but also on one’s compliance relative to that of others in the audit pool; that is, the audit agency audits only those taxpayers thought to be the least compliant in an audit class, so that there is a “coordination game” among the taxpayers, and taxpayers will have an incentive to coordinate their reporting behavior. Their experimental results indicate that individuals typically find it difficult to coordinate their compliance decisions but that communication (or “cheap talk”) among the subjects facilitates such coordination.

Public Goods

One of the first areas in public economics to which experimental methods were applied is voluntary contributions to the provision of public goods. The standard theory of public goods concludes that the Nash equilibrium is characterized by zero voluntary contributions to public goods as individuals attempt to “free ride” on the contributions of others. However, there are numerous examples from the naturally occurring world in which many individuals do contribute positive amounts to public goods (e.g., public radio, volunteer work, charitable donations). Experiments have been employed to determine the circumstances under which individuals will contribute and to determine mechanisms that will allow the free rider problem to be overcome.

A typical “voluntary contributions” experiment begins by assigning student subjects to groups. Subjects are endowed with tokens and are isolated from each other. Each subject is offered two possible “investments,” one in a private good and one in a public good. The private good investment has higher returns for the individual, but the public good investment provides a return to all members of the group; that is, the public good investment is both nonrival (because all members of the group derive the same benefit from any member’s contribution) and nonexclusive (because subjects who do not contribute to the public good investment still receive the same group return). Subjects are then asked to choose an allocation of tokens between the investments. Because the total social return is greater for the public good than for the private good, the socially optimal outcome is for everyone to contribute fully to the public good. However, as with the standard prisoner’s dilemma game, each individual has an incentive to keep all of his or her tokens because the private good investment has a higher personal return than the public good investment. This contribution game is then repeated for a number of rounds, and the level of actual contributions is compared to the Nash equilibrium prediction (zero contributions) and the socially optimal outcome (full contributions).

There are numerous variants on this basic design. While early work focused on “one-shot” games, the standard approach in recent work is to examine subject responses over multiple rounds. The public good return is often linear with respect to the contributions, but it can be based on a “threshold” (or a “provision point”) in which the group return will only be provided if total contributions exceed some designated threshold; relatedly, incentive compatible mechanisms (Groves and Ledyard, 1977) have been examined. The return on the group investment can be varied. Subjects with different levels of experience in the contribution experiments can be examined. Similarly, subjects can remain in the same group for the entire experiment or the assignment of subjects can be randomly varied from round to round. Group size can also vary, from “small” to “large” groups, and the subject “type” can also change in order to

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* See Ledyard (1995) and Janssen and Ahn (2004) for comprehensive surveys of public good experiments.
examine whether, say, men behave differently than women or economics students behave differently than other students. In most experiments, privacy is maintained in the sense that individual subjects’ contribution choices are not revealed to other subjects, subjects are not allowed to communicate with one another, and subjects do not know anything about other members of the group. However, others’ choices can be revealed, group communication (e.g., “cheap talk”) can be allowed, and individual identities can be shown to all subjects. Alternately, privacy can be further enhanced by using a “double blind” procedure to shield the identity of the subjects’ contributions, not only from other subjects, but also from the experimenter, so that the data on subjects’ contributions cannot be linked to the subjects’ identities by anyone.

All of these experiments aim to answer the core questions of public goods provision. How much are people willing to contribute to a public good? Why do they contribute at all? What institutional and environmental factors can increase contributions?

It is not possible—or necessary—to detail all the results from these studies, but it is useful to highlight some selective studies. In some of the earliest and most cited papers in this area, sociologists Marwell and Ames (1979, 1980, 1981), who were suspicious of economists’ theories of rational choice, embarked on a systematic study of the public goods problem, with titles such as “Economists Free–Ride, Does Anyone Else?” These papers and other important early works (e.g., Schneider and Pommerhene, 1981) found surprisingly little free riding, with contributions much closer to the Pareto efficient level than the theory predicted. For example, Marwell and Ames (1981) found that most groups made significantly higher contributions than economists would predict—except for groups made up of economics students!

Since these early papers, there have been many additional studies, often devised either in an attempt to resurrect the standard prediction of complete free riding or in an effort to demonstrate why individuals contribute and what factors help overcome free riding. For example, Isaac, McCue, and Plott (1985) demonstrated that the “high” levels of contributions in earlier experiments diminished over time if subjects are examined over repeated rounds, and Isaac, Walker, and Thomas (1984) found that experienced subjects contribute less than inexperienced subjects. Other types of experiments have been performed, with different group returns (Laury, Walker, and Williams, 1999), with varying group size (Isaac and Walker, 1988), without confidentiality of subjects’ decisions (Andreoni and Petrie, 2004), and with “threshold” (or “provision point”) mechanisms (Bagnoli and McKee, 1991; Bagnoli, Ben–David, and McKee, 1992). In the last studies, game theory predicts that there may be several non–cooperative equilibria. Results have been somewhat mixed as to whether thresholds in and of themselves directly affect contributions, but even so there is some evidence that such institutions matter.

These newer studies have also allowed new theoretical models to be tested and existing models to be revised. These models introduce such factors as altruism (Becker, 1974; Andreoni and Miller, 2002; Cox and Sadiraj, 2007), impure and warm–glow altruism (Andreoni, 1989, 1990), inequality aversion (Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000), confusion versus altruism (Andreoni, 1995), and reciprocity (Sugden, 1984).10

10 Cox and Sadiraj (2007) also rigorously examine two models of public goods contributions: the inequity aversion model and the egocentric other–regarding preferences model. They find that inequality aversion cannot be reconciled with the stylized facts, but egocentric other–regarding preferences can.
In surveys of voluntary contributions experiments, Ledyard (1995) and Janssen and Ahn (2004) conclude that the results from the varied experiments are often contradictory. Even so, they suggest a range of somewhat different “stylized facts” arising out of all of this work, which can be summarized as:

- Many subjects contribute to a public good, sometimes in significant amounts.
- There is substantial individual variation in contributions.
- At least at the outset, subjects contribute halfway between the Pareto efficient level and the free-riding level.
- Contributions decline with repetition.
- Individuals change their contributions from round to round in ways influenced by the experimental structure. For example, subjects tend to contribute at a higher rate if there is:
  - Face-to-face communication (e.g., “cheap talk”),
  - A larger group size,
  - A higher marginal return on the group investment,
  - A larger individual endowment.

There is also some recent work that combines laboratory experiments on public goods with field experiments. For example, Laury and Taylor (forthcoming) found that individuals’ contributions to a public good in the laboratory were not always able to predict their willingness to contribute to a public good in the naturally occurring world.

**Political Economy**

As in many other areas of public economics, experiments in political economy are multidisciplinary. In this case, the political science, the public policy, and the management literatures have interacted productively with the literature in economics over the last 30 years.

Palfrey (2006) surveys much of the work that has been done with laboratory experiments in political science. He classifies experiments into four categories: committee decision making, elections and candidate competition, information aggregation and committees, and voter turnout and participation games.

Committee decision making experiments are generally performed by bringing a group of subjects together and allowing them to make decisions in a policy space, with values for the policy options induced in the laboratory. The results are viewed through the lens of bargaining: who was able to get what they wanted, and how? For example, Fiorina and Plott (1978) developed a theory of the core for small committees, and explored the sensitivity of committee performance to details of committee procedure. Other work on committees includes Plott and Levine (1977) and Levine and Plott (1978).

Subsequent experiments by Plott (1991) and others have changed the committee structure (e.g., size, agenda control, agenda setting, conveners, number of policy options, communication and voting rules), and observed the resulting behavior and performance of the committees. Most experimental results have supported the original conclusions of Fiorina and Plott (1978): given the existence of a core, committee outcomes will tend to be within the core, and, in the absence of a core, the distribution of committee outcomes varies continuously with the committee members’ preference profile. Other results have shown that costs assessed when proposals are passed expand the core set of outcomes but interfere with the ability to achieve the core, that conveners can influence outcomes in some cases, and that there is full rent extraction from committee members.
Work on elections and candidate competition has examined theories about Condorcet winners in competitive elections and the median voter, often using spatial competition models (McKelvey and Ordeshook, 1990). Subjects typically play the roles of voters and candidates. Information transmission mechanisms vary, and experimenters observe whether voters behave optimally (e.g., vote for their own “best” option). Frequently, candidate competition is modeled in a one-dimensional world with single peaked preferences and various informational conditions. In these settings, platforms in competitive elections are observed to converge to the Condorcet winner (McKelvey and Ordeshook, 1982), and even limited information allows voters to behave as if they are fully informed (McKelvey and Ordeshook, 1985). Multicandidate elections present a further challenge. As Palfrey (2006) puts it, “. . . almost anything can happen in equilibrium” because of the presence of multiple equilibria. These elections have been modeled as coordination failures, and coordination devices such as polls have been tested for efficacy (Forsythe, Myerson, Rietz, and Weber, 1993, 1996). In the absence of a coordinating device, results typically find a coordination failure, but coordinating devices like polls and alternative voting procedures (e.g., Borda counts) can alleviate the problem. Aragones and Palfrey (2002, 2004) have also examined asymmetric contests, such as incumbent advantage, and have found that voter preferences determine the effect that candidate advantage bestows and that candidate advantage can lead to divergence in the platforms of candidates.

In many experiments in the area of information aggregation and committees, like-minded committee members are presented with a set of policy options, information, and communication rules (much as with jury models). Each policy has a payoff that depends on the state of the world, and the information provided may be decentralized or limited. In this setting, the committee must choose the “best” policy. Voting behavior is modeled using either a Nash equilibrium approach or a theory of “quantal response equilibrium” (McKelvey and Palfrey, 1995, 1998). A key paper here is by Guarnaschelli, McKelvey, and Palfrey (2000), who found some evidence that supports the quantal response equilibrium, in that voters pursue mixed (or stochastic) strategies. Other work (e.g., Goeree, Palfrey, and Rogers (2004)) has studied social learning and the dissemination of information within groups. Voters seem to learn from earlier elections, and, while “herds” may form as voter beliefs are stuck for some period of time, herds formed around incorrect beliefs collapse and become self-corrected.

Voter turnout is in some ways a public good issue. Citizens in a large population have very little incentive to vote, since any citizen’s vote is extremely unlikely to swing the election. Nonetheless, voter turnout is observed in nontrivial numbers. In the laboratory, this has been studied by assigning subjects to be voters and candidates, inducing valuations for the election outcomes in the voters, and allowing voters to vote or not vote as they choose. Schram and Sonnemans (1996) tested an alternative theory of voter turnout developed in Palfrey and Rosenthal (1983), focusing on voter turnout in a winner–take–all election versus a proportional–representation election. They found ambiguous results; that is, initial turnout was higher for winner–take–all elections than for proportionate–representation elections, but in both cases turnout declined to 20 percent after several periods. Other work in this area has looked at the effect on voter turnout of the size of electorate, relative party sizes, and the cost of voting. For example, Levine and Palfrey (2005) found that electorate size can matter and that Bayesian Nash equilibrium outcomes are supported.
Additionally, there is a growing literature on lobbying and its influence. Lobbying is often represented as a common agency game, where multiple “lobbyists” try to influence a single decision-making agent. For example, Kirchsteiger and Prat (2001) used a two-principal, three-alternative common agency game to examine two different kinds of equilibria: “truthful,” where campaign contributions truly represent the principal’s payoff function, and “natural,” where principals economize on offers. They found that, while the natural outcome is favored over the truthful equilibrium, most subjects do not use natural contribution schedules. Potters and van Winden (2000) tested a simpler signaling game, wherein the lobbyist sends a message (which may be true or false) about the state of the world to the decision-making subject, and found that professional subjects’ behavior is more closely aligned with game theoretic predictions than is the behavior of student subjects. They concluded that professional rules of conduct may account for some of these differences. Others have studied legislative bargaining. For example, Fréchette, Kagel, and Morelli (2005) performed a “divide the dollar” game with subjects divided into three legislative blocks where no majority exists. A subject is then selected who must propose a way of dividing the money, and the others have to either accept or reject that proposal, with a majority rule winning. Proposer power is observed, but it is not as strong as theoretical predictions, and tests of procedural rules support the prediction that there is a proportional relationship between votes and rewards.

These propositions have been subjected to experimental tests, generally via “double-auction experiments.”

Double-auction experiments were among the very first experiments to be examined in the laboratory. These experiments were initially designed to test whether buyers and sellers in a competitive market would generate prices and outputs consistent with competitive (or non-competitive) market predictions. This framework also allows tests of the tax equivalence proposition. Indeed, most papers using laboratory experiments methods in the tax incidence area test this equivalence result. There have also been experiments testing tax incidence in imperfectly competitive markets and in general equilibrium settings.

In a typical tax incidence experiment, subjects are designated as buyers or sellers in a standard double-auction setting. Buyers are given a schedule of unit valuations for a good, and sellers are given a schedule of unit costs. A buyer’s profit is the difference between her valuation and the price she pays for a good; a seller’s profit is the difference between the price he receives for a good and his cost. Subjects are encouraged to maximize profits, and subject payments are typically linked to their profits. Buyers post bids and sellers post offers, and transactions occur when a bid and an offer can be matched. Several rounds are often completed without taxes, and then taxes are imposed on one side or the other or both. Trading prices and quantities can then be studied to determine the resulting equilibrium and the incidence of the tax; the excess burden of the tax can also be examined.

Kachelmeier, Limberg, and Schadewald (1994) examined the incidence of three different tax implementations using a double auction with buyers, middlepersons, and sellers. They found some support for the equivalence theory. Borck, Engelmann, Müller, and Normann (2002) also found experimental evidence consistent with

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**Tax Incidence**

The theory of tax incidence has derived a number of very basic propositions, such as the equivalence between a tax imposed on buyers versus a tax imposed on sellers, at least in competitive markets.
equivalence in a similar experimental setting. Kerschbamer and Kirchsteiger (2000) used an ultimatum game to study incidence. Player 1 is the proposer and is given an endowment of money; Player 1 then chooses how much money to give to Player 2, the responder. Kerschbamer and Kirchsteiger (2002) imposed taxes first on Player 1, and then on Player 2. They found that statutory incidence did matter; that is, the proposers’ net offers were significantly higher when taxes were levied on the proposer than on the responder.

Since then, numerous papers have tested the standard theory in different environments and institutions, with varied results. In particular, the tax equivalence proposition seems to hold in some institutions (e.g., competitive markets), but not in others (e.g., situations involving bargaining where questions of fairness are emphasized). For example, Ruffl (2005) used a “pit” market, where buyers and sellers negotiate face to face and where both taxes and subsidies are imposed. He found that in “large” markets a tax and a subsidy both exhibit incidence equivalence, but in “small” markets market power seems to interfere with this mechanism.

Quirmbach, Swenson, and Vines (1996) undertook a very different study of tax incidence. They framed the experiment in terms of general equilibrium tax incidence, as described by Harberger (1962). They also used a double auction, but subjects were assigned roles as sellers of capital, sellers of labor, corporate producers, and non–corporate producers. The results support the standard Harberger (1962) conclusions, in which a tax on corporate capital causes capital to flow from the corporate to the non–corporate sector.

"Other" Public Economics Experiments

Here we describe a handful of papers not easily classified but of potential interest to public economists. Several of them are in the area of behavioral changes caused by taxes.

Taxes can cause real behavioral changes by pushing taxpayers into evasion activities, as discussed earlier, and into other types of decisions; that is, the return on the taxed activity is reduced because of the tax, and people should tend to participate less in the taxed activity. Aside from tax compliance behavior, the type of real response that has been most studied is the labor supply response.

These experiments are essentially specialized versions of those used to study labor supply decisions in general. In some of these experiments, subjects are provided with a work task (e.g., typing numbers into a computer) and a leisure task (e.g., playing a game). During short work periods, they work, and they are paid by the productivity of their work (e.g., the number of keystrokes typed). They may be able to choose the length or frequency of breaks from work in which they perform the unpaid leisure task. Over subsequent periods, different taxes are imposed on the return from the work task. Work effort or time worked can then be studied.

Swenson (1988) first conducted these specialized work behavior experiments, with a focus on whether the Laffer curve exists. In his design, subjects are paid for their work effort, subject to exogenously determined taxes that varied across the work periods. His experimental results supported the existence of a Laffer curve; that is, work effort decreased with increases in the tax rate beyond some tax rate level. Sillamaa (1999a, 1999b) followed in the same area; however, in her design subjects are paired and the tax rate is endogenously determined by the worker’s partner. She found that a zero marginal tax rate increased work effort and that work effort is higher under a linear than a nonlinear tax schedule. Studies of the Laffer curve theory continue to be
pursued (Sutter and Weck–Hannemann, 2003; Lévy–Garboua, Masclet, and Montmarquette, 2005). Much of this work continues to find that tax collections decline with increases in the tax rate beyond some level, but this result is sensitive to experimental design. For example, Lévy–Garboua, Masclet, and Montmarquette (2005) found that reduced tax revenues with higher tax rates occurred only in treatments where the tax rate is chosen by a subject, but not when the tax rate is fully exogenous.

Other papers examining real responses to taxes have taken varied approaches. For example, de Bartolome (1995) investigated whether people respond to average or marginal tax rates. He showed tax schedules to students, and asked them to choose between an untaxed (but lower return) and a taxed (and higher return) savings vehicle, as returns are varied. His experimental results suggested that many subjects appeared to base their decisions on the average tax rate.

Also, Riedl and van Winden (2003, forthcoming) have performed macroeconomic experiments, studying questions like “What does it matter for unemployment if we tax inputs versus outputs?” Subjects play the roles of consumers and producers in two “countries” (with open or closed economies) and taxes are applied. Unemployment payments or labor subsidies, financed by taxes, are made available to consumers. They found that budget deficits can boost employment, that wage taxes and unemployment can reinforce each other, and that a sales tax may provide more effective financing for unemployment benefits than a wage tax. In addition, Cadsby and Frank (1991) examined whether Ricardian Equivalence holds in a laboratory setting, and Slate, McKee, Beck, and Alm (1995) extended these tests to allow for uncertainty about debt repayment. Both studies found some limited evidence for the equivalence between tax and debt finance.

SOME LIMITATIONS OF LABORATORY METHODS

Despite the demonstrated usefulness of experimental methods, there are sound reasons for caution in interpreting and generalizing experimental results. Some early experiments did not follow some now widely accepted procedures of the experimental paradigm, such as the use of repeated decisions and of neutral instructions. Much early work also lacked realism because values of the various policy parameters did not approximate real–world values.

Although more recent experimental research has generally addressed these problems, some concerns remain, some of which are more legitimate than others. A common criticism of experimental economics is that the student subjects typically used may not be representative of taxpayers. However, there is now much evidence that the experimental responses of students are seldom different than the responses of other subject pools (Plott, 1987). There is also no reason to believe that the cognitive processes of students are different from those of “real” people.11

Another common criticism is that it is not possible to control for many relevant factors in the laboratory. However, if one cannot control for such factors in the laboratory where the experimenter establishes the institutions, the rules, and the reward structure, then one cannot hope to control for these factors in the naturally occurring world.

11 An old joke among experimentalists addresses the use of student subjects: “Why use student subjects rather than, say, animals like pigeons or rats? Because students are cheaper than animals and because one does not get as attached to students as to animals.”
Of more legitimate concern, the results may well be sensitive to the specific experimental design, so that replication is crucial. It is also possible that subjects may modify their behavior simply because they know that they are participating in an experiment. Most importantly, there is a certain artificiality in any laboratory setting. A decision, say, to report three tokens of income in a tax compliance experiment is clearly different from a decision to report actual income on an annual tax return, even if the laboratory incentives are salient. In particular, the laboratory setting cannot capture a catastrophic loss such as jail, and it cannot capture the social stigma that some surveys suggest is an important factor in taxpayer reporting. Similar concerns reflect public good, voting, tax incidence, and other experiments.

In short, one must use the results from laboratory experiments with some care. However, such use depends largely upon the purpose of the experiment. According to Roth (1987), experiments can be classified into three broad categories that depend upon the dialogue in which they are meant to participate. “Speaking to Theorists” includes those experiments designed to test well-articulated theories. “Searching for Facts” involves experiments that examine the effects of variables about which existing theory has little to say. “Whispering in the Ears of Princes” identifies those experiments motivated by specific policy issues. To date, most experiments in public economics have fallen into the first two categories. Although this now seems to be changing somewhat, there remains a natural skepticism among policy makers about the ability of experimental analyses to illuminate some aspects of traditional public economics questions.12

CONCLUSIONS: SOME FUTURE DIRECTIONS

It is, we hope, apparent that laboratory experiments have considerably expanded our knowledge of many dimensions of public economics. We now know much more about why people contribute to public goods, about why they pay their taxes, about how taxes affect prices and incomes, about how political decisions are made, and about how individuals respond to tax incentives. We conclude by discussing several areas to which the future application of laboratory methods seems especially likely and valuable.13

First, there will certainly be experimental studies that extend the existing literatures in public economics. Research obviously answers some questions, but in the process research necessarily raises additional questions. For example, in the area of tax compliance, we expect that laboratory methods will be applied to such issues as:

- How is compliance affected in settings where individuals differ in the portion of their income that is “matched” (or reported to the tax authority via third-party information) versus “non–matched” (or not fully reported to the tax authority)?
- How is compliance affected when some individuals face a “certain” probability of audit and others are informed that they will not be audited?

12 See Levitt and List (2006) for a recent discussion of the ability of laboratory methods to explain behavior outside the laboratory.
13 Laboratory experiments are proving to be a valuable teaching aid. For example, see the textbook by Bergstrom and Miller (2000), which uses experiments to teach microeconomics. See also EconPort, a digital library maintained by the Experimental Economics Center in the Andrew Young School of Policy Studies at Georgia State University, which emphasizes the use of experiments in all areas of economics including public economics, at http://econport.org/econport/request?page=web_home.
• How do individuals respond when the “productivity” of an audit (e.g., the amount of unreported income that is discovered in an audit) varies?
• How is information about enforcement disseminated among taxpayers, and how do taxpayers respond to this information?
• Does public disclosure of audit results affect compliance?
• Is compliance affected by the cost that any audit imposes on taxpayers?
• Can increased deterrence lead to less compliance because greater enforcement destroys “trust” in government?

Similarly, in the area of taxpayer responses to taxes, there are many dimensions of behavior that can be usefully examined in the laboratory but that remain largely unexplored. A particularly fruitful area here is intertemporal choice. The effect of taxation on saving decisions has yet to be thoroughly examined in a laboratory setting, despite the fact that experiments are able to examine individual choices over multiple periods. Various aspects of social insurance have also not been examined in the laboratory; that is, even though individual behavior in uncertain environments has been extensively studied in the laboratory, the broader social implications of government-provided social insurance have been almost completely neglected. In a related vein, there are many aspects of debt versus tax finance that remain unanswered with data from the naturally occurring world, such as the impact of distortionary taxation on tax and debt equivalence. The application of voting models to collective choice of tax structures is another potentially fertile area for research, as demonstrated by Ackert, Gillette, Rider, and Martinez–Vazquez (2007). Other areas in public economics have similar issues that could be examined in the laboratory.

Second, and perhaps more fundamentally, there will certainly be experimental studies that apply new frameworks and new theories to all areas of public economics. One such area is behavior under uncertainty. Nearly all theoretical analyses of taxpayer behavior under uncertainty are based upon expected utility theory. However, it is now well established that there are significant limitations in the ability of this theory to explain major aspects of individual behavior under uncertainty (Machina, 1987; Rabin, 2000; Cox and Sadiraj, 2006). Clearly, it is important to apply other theories of behavior. For example, in the area of tax compliance, theory needs to allow the introduction into the compliance decision of numerous factors beyond enforcement: overweighting of low probabilities, differential responses to gains versus losses, the presence of social norms and moral sentiments, notions of fairness, satisfaction with government programs, and so on. Approaches that seem particularly relevant for detailed investigation include prospect theory (Kahneman and Tversky, 1979) and generalized expected utility theory (Quiggin, 1993). Experimental tests of both the assumptions and the implications of these newer theories are an essential part of this research.

Related to the use of models of non-expected utility to individual choices is the application of “behavioral economics” to public economics. As discussed in the papers in Camerer, Loewenstein, and Rabin (2004) and in McCaffery and Slemrod (2006), behavioral economics can be broadly defined as an approach that uses methods and evidence from other social sciences (especially psychology) to inform the analysis of individual and group decision making. There is much evidence—often derived from laboratory experiments—that, contrary to the standard neoclassical approach to consumer choices, individuals are not always purely self–interested, they face limits on their ability to compute (e.g.,
“bounded rationality”), they systematically misperceive the true cost of actions (e.g., “fiscal illusion”), they face limits on their “self–control,” and they are affected by the ways in which choices are “framed” (e.g., reference points, gains versus losses, loss aversion). There is also much evidence that individuals are influenced by the social context in which decisions are made; that is, individuals are not always the outcome–oriented, egoistic, and selfish consumers envisioned by our standard theory but are affected in predictable ways by the processes by which outcomes are determined and also by notions of fairness, altruism, reciprocity, trust, and social norms (Cox, Friedman, and Gjerstad, forthcoming).

To date, insights from behavioral economics have been applied in some ways to the experimental analysis of public good provision, tax compliance, and voting. However, there is virtually unlimited scope for deeper application of these methods. Further, behavioral economics has yet to be applied in any systematic way to the analysis of individual responses to taxes; that is, do individual responses to taxes depend on the ways in which taxes are framed? For example, do individuals respond differently to a tax credit than to an equivalent tax deduction? Are they affected differently by a tax discount from a “high” tax base versus an equivalent tax surcharge from a “low” tax base, by a tax change expressed as a percent versus a change expressed as an absolute amount, or by a tax in which one’s own payments are compared to those of others? Does the timing of a tax change affect individual decisions (e.g., do individuals prefer many “small” tax reductions versus one “large” tax reduction, do they prefer withholding over a tax year versus a one–time payment at the end of the year)? Can individual savings decisions be affected by “commitment” or “heuristic” devices (e.g., forced payroll savings plans, “Christmas Clubs”) that simplify individual decision–making and that help overcome individual self–control problems? Is the behavioral response of an individual taxpayer affected by his or her perceptions of what others are doing or by his or her own notions of fairness or altruism? These—and many other—questions are difficult to examine theoretically or empirically, but can be brought into the laboratory for analysis. Indeed, the laboratory may represent the only avenue by which these questions can be tested.

In short, we believe that the future prospects for the application of experimental methods to public economics are exciting and unlimited. The challenge here is to design experiments that capture, or “parallel,” the essential elements of the naturally occurring world that are of interest. Such experiments have enormous potential to add further to our understanding of how government policies affect individual and group decisions.

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