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Laboratory experiments are a widely used methodology for advancing causal knowledge in the physical and life sciences. With the exception of psychology, the adoption of laboratory experiments has been much slower in the social sciences, although during the past two decades the use of lab experiments has accelerated. Nonetheless, there remains considerable resistance among social scientists who argue that lab experiments lack "realism" and generalizability. In this article, we discuss the advantages and limitations of laboratory social science experiments by comparing them to research based on nonexperimental data and to field experiments. We argue that many recent objections against lab experiments are misguided and that even more lab experiments should be conducted.

The social sciences have generally been less willing to use laboratory experiments than the natural sciences, and empirical social science has traditionally been considered as largely nonexperimental, that is, based on observations collected in naturally occurring situations. The first lab experiments in economics were not conducted until the late 1940s. Fewer than 10 experimental papers per year were published before 1965, which grew to about 30 per year by 1975 (1, 2). Starting from this low level, experimentation in economics greatly increased in the mid-1980s. In three well-known economics journals—*American Economic Review*, *Econometrica*, and *Quarterly Journal of Economics*—the fraction of laboratory experimental papers in relation to all published papers was between 0.84% and 1.58% in the 1980s, between 3.06% and 3.32% in the 1990s, and between 3.8% and 4.15% between 2000 and 2008 (authors' calculations). The percentages were much higher in more specialized economics journals. The first specialty journal, *Experimental Economics*, was founded in 1998. A similar increase in lab experiments has taken place in other social sciences as well, for example, in political science (3).

Many social scientists are still reluctant to rely on laboratory evidence. Common objections are that student participant pools are unrepresentative and that sample sizes are small. There is also a widespread view that the lab produces unrealistic data, which lacks relevance for understanding the "real world." This notion has its basis in an implicit hierarchy in terms of generating relevant data, with field data being superior to lab data. We argue that this view, despite its intuitive appeal, has its basis in a misunderstanding of the nature of evidence in science and of the kind of data collected in the lab. We also

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> **Samuelson and Nordhaus**
> *Principles of Economics 12th ed.*
> *McGraw-Hill New York 1985, p. 8*

> " Experimental economics is an 'exciting new development' . "
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> **Samuelson and Nordhaus**
> *Principles of Economics 14th ed.*
> *McGraw-Hill New York 1992, p. 5*

argue that many of the objections against evidence from the lab suggest the wisdom of conducting more lab experiments, not fewer. Although most of our examples and topics are taken from economics, the methodological points we discuss can be applied to all social sciences.

The Lab Provides Controlled Variation

Controlled variation is the foundation of empirical scientific knowledge. The laboratory allows tight control of decision environments. As an illustration, consider a simple experiment, the gift-exchange game, which tests the theory that employment relationships are governed by a gift exchange; that is, that workers reciprocate fair wages with high effort. A positive relationship between wages and effort is the central assumption of efficiency wage theories that have important implications for the functioning of labor markets and that can explain rigid wages and involuntary unemployment (4). Testing this class of theories with field data is notoriously difficult. For example, in firms, worker effort is not easily observed or measured, and workers are confronted with a mix of different incentives. This makes interpretation of different effort levels difficult. An observed variation in wages may not reflect generosity but may be due to firm size, self-selection of workers, or simply productivity differences. Even if a relationship between wages and effort is detected, it may not necessarily reflect a fair-wage–effort relationship; instead, it could reflect strategic considerations based on reputation and repeated interactions. In the laboratory, these factors can be varied in a controlled fashion. The experimenter observes effort and wages and can rule out confounding effects such as multiple incentives, selection, productivity differences, and repeated interactions.

The first experimental test of the existence of gift exchanges in the framework of a formal game-theoretic model was designed to mimic an employment relationship. Participants assumed the roles of workers and firms (5). Firms made (binding) wage offers, which workers could accept. If a worker accepted, he or she then had to choose a costly effort level. Labor contracts are generally incomplete contracts; that is, effort is not fully contractually enforceable. In the experiment, this is reflected by the fact that workers were free to choose any effort level above the contractually enforceable level. In this framework, it is possible to test the gift-exchange hypothesis against the self-interest assumption commonly made in economics: that a self-interested worker would always choose the lowest possible effort because effort is costly and there is no punishment for minimal effort. Anticipating this, the firm has no incentive to pay an above-minimum wage, because self-interested workers work no harder if given a higher-than-minimum wage. Nevertheless, the results of numerous gift-exchange experiments in the lab revealed that higher wages induce workers to provide higher effort levels. The experiment is a good example of the many experiments that have challenged the assumption of a universally selfish and rational *Homo economicus*. Systematic lab evidence shows that people are boundedly rational and prone to behavior such as loss aversion, present bias, or judgment biases (6). Phenomena such as reciprocity or social approval, which have been largely neglected by mainstream economics, have been shown to be important in affecting

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economic outcomes in bargaining and market interactions (*7, 8*).

These experiments illustrate that the lab offers possibilities to control decision environments in ways that are hard to duplicate with the use of naturally occurring settings. In the laboratory, the experimenter knows and controls the material payoffs, the order in which the different parties can act, the information parties possess when they make choices, and whether the game is repeated or one shot. This control allows for the testing of precise predictions derived from game-theoretic models. Participants are randomly assigned, and decisions are rewarded. Payment ensures that participants take their decisions seriously. For example, if a firm pays a higher wage or a participant provides higher effort, costs are higher and final earnings are lower. In this sense, behavior in the laboratory is reliable and real: Participants in the lab are human beings who perceive their behavior as relevant, experience real emotions, and take decisions with real economic consequences (*6, 9–12*). Lab experiments can be used for testing theories and to study institutions at relatively low cost (*9*). This is particularly interesting for policy questions where the proposed program intervention has no counterpart in reality and where constructing the counterfactual states of interest may be done more easily in the lab than in the field. Moreover, whereas existing institutions are adopted endogenously, rendering causal inferences about their effects difficult, the lab allows exogenous changes in institutions. Lab experiments have turned out to be valuable in solving practical problems that arise in implementing matching markets (*13*), government regulation (*14*), or airport time-slot allocation (*15*). Economic engineering, a combination of theory and experiments, has improved design and functioning of many markets (*16*).

Lab or Field Is Not the Choice

Resistance concerning laboratory evidence often centers on an appeal to realism. This skepticism has recently manifested itself in a lively debate in economics about field versus lab experiments. Whereas some scholars have argued passionately in favor of laboratory experiments where controlled manipulations of conditions on carefully documented populations are performed (*17–19*), others have argued in favor of field experiments where conditions are more "realistic," although perhaps less tightly controlled, and where more realism also implies greater relevance to policy (*3, 20–22*). These controversies appear throughout the social sciences (*3, 23–26*).

The casual reader may mistakenly interpret arguments about realism as an effective critique against the lab, potentially discouraging lab experimentation and slowing down the production of knowledge in economics and other social sciences. The issue of realism, however, is not a distinctive feature of lab versus field data. The real issue is determining the best way to isolate the causal effect of interest. To illustrate this point

and to structure the debate about field and lab data, we suggest the following simple model.

Consider an outcome of interest Y (e.g., effort supplied by a worker) and a list of determinants of Y, $(X_1, …, X_N)$. For specificity, suppose that

$$Y = f(X_1,…,X_N) \qquad (1)$$

which is sometimes called an all-causes model (*27, 28*) because it captures all possible causes of Y in $(X_1, …, X_N)$. The causal effect of X_1 on Y is the effect of varying X_1 holding fixed $\tilde{X} = (X_2,…,X_N)$. In the pioneering field experiments (*20*), X_1 was the tax rate on wages. In the laboratory gift-exchange experiment, the values of X_1 are the different wage levels paid by firms. Unless f is separable in X_1, so that

$$Y = \phi(X_1) + g(\tilde{X}) \qquad (2)$$

the level of Y response to X_1 will depend on the level of $\tilde{X}$. Even in the separable case, unless $\phi(X_1)$ is a linear function of X_1, the causal effect of X_1 depends on the level of X_1 and the size of the variation of X_1. These problems appear in both field and lab experiments and in any estimation of the causal effect of X_1.

Among the $\tilde{X}$ in the gift-exchange experiments described above are concrete details of market institutions such as the number of firms and workers, the order of moves, the choice set, payoff functions, information available, whether or not interactions are one shot, and whether or not they are anonymous. More generally, $\tilde{X}$ could be demographic characteristics of the participants, the level of observation of actions by third parties, individual preference parameters (e.g., morality, persistence, self-control, and social and peer influences), and other aspects of environments.

Many laboratory experiments like the gift-exchange experiment have provided evidence of gift exchange and social preferences in lab settings for certain values of $\tilde{X}$, usually, but not always, with use of populations of undergraduates and different bargaining and market institutions (*7, 29*). The relevance of these findings has been questioned in recent field experiments analyzing behavior in a population of sports-card traders in a "natural setting," that is, for another set of conditions $\tilde{X}'$ including, for example, different institutional details, payoffs, and a different participant population (*30*). In this particular market, the evidence for social preferences is weaker. If one is interested in the effect of social preferences under a third condition ($\tilde{X}''$), neither the undergraduate nor the

sports-cards field study may identify the effect of interest. It is not obvious whether the lab $\tilde{X}$ or the field $\tilde{X}'$ is more informative for the third condition unless a more tightly specified economic model is postulated or a more precisely formulated policy problem is specified. Recent evidence suggests that empirical support for the existence of social preferences such as reciprocity or gift exchange is not a matter of lab or field but of the prevailing conditions such as the details of agent interactions (*31–33*). When the exact question being addressed and the population being studied are mirrored in an experiment, the information from it can be clear and informative. Otherwise, to transport experimental findings to new populations or new environments requires a model (*34*).

Field methods are able to obtain a universally defined causal effect only if the special functional form (Eq. 2) is specified and the response of Y to X_1 is linear. If this is the case, however, lab experiments are equally able to obtain accurate inferences about universal effects. Observing behavior in the field is in general not more interesting or informative than observing behavior in the lab. The general quest for running experiments in the field to obtain more realistic data is therefore misguided. In fact,

> " While laboratory processes are simple in comparison to naturally occurring processes, they are real processes in the sense that real people participate for real and substantial profits and follow real rules in doing so. It is precisely because they are real that they are interesting. "
>
> **Charles R. Plott**
> *Journal of Economic Literature*
> *vol. 20, 1982, p. 1486*

the key issue is what is the best way to isolate the effect of X_1 while holding constant $\tilde{X}$. No greater reality is conveyed by one set of $\tilde{X}$ than another unless the proposed use of an estimate as well as target populations and settings are carefully specified. The pioneering field experiments (*20*) defined the target population and the questions sought to be answered very precisely.

The usefulness of particular methods and data is ultimately a matter of the underlying research question. Lab experiments are very powerful whenever tight control of $\tilde{X}$ is essential. This applies in particular for testing (game theoretic) models and behavioral assumptions. The lab can also easily implement many different values of $\tilde{X}$, for example, the number of buyers or sellers in market experiments, and in this way explore the issue of robustness of an estimated effect. Tight control of $\tilde{X}$ also allows replicability of results, which is generally more difficult with field data. The field on the other hand offers a large range of variations in $\tilde{X}$, which are potentially relevant but hard to implement in the lab. This way field experiments can provide important complementary insights to lab findings, for example, in the area of development economics, or by addressing specific policy questions such as testing antipoverty programs that are targeted to, and need to be evaluated on, populations in poverty (20).

Other objections. In addition to the lack-of-realism critique, other objections concerning lab evidence have been put forward. Ironically, most objections raise questions that can be very well analyzed with lab experiments, suggesting the wisdom of conducting more lab experiments, not fewer. One common objection is that lab experiments with students do not produce representative evidence. For the purpose of testing theories, this is not a problem because most economic models derive predictions that are independent of assumptions concerning participant pools. On some aspects of behavior, however, students are not representative of the general population or a target population of interest, and we would agree that a richer variation in context, populations, and environments $\tilde{X}$ should be used in future lab experiments. In this vein, the gift-exchange game has been run on nonstudent samples (35), yielding results similar to those obtained by using samples of students. Other studies have shown the relevance of social preferences for CEOs (36), for professional financial traders (37, 38), or for the general population (39, 40).

It has also been noted that (i) stakes in experiments (money paid for decisions taken) are trivial, (ii) the number of participants or observations is too small, (iii) participants are inexperienced, (iv) Hawthorne effects may distort experiments, or (v) self-selection into experiments may bias results.

(i) Most of what we know about the level of stakes on outcomes is derived from controlled lab experiments. The effects of varying stake size are mixed and seem to depend on concrete experimental contexts (41). Reciprocity does not vanish if participants in the gift-exchange experiment reported above are paid an equivalent of three months of income (42). Even if stake effects are relevant, however, it is not obvious what the "right" level of stakes should be; that is, what are the right levels of X_1 and $\tilde{X}$? We would ask in reply, how often do people make decisions involving monthly incomes, and how representative would such high-stake experiments be for the many decisions people make on a daily basis, which involve relatively small stakes? In any case, if one is seriously interested in how stakes affect behavior, one can run experiments with varying stake sizes.

(ii) The issue of sample size is a red herring. Effective methods have been developed for analyzing small sample experiments (43–45). Moreover, many experiments nowadays are run with samples of several hundred participants, sometimes with more than 1000 participants (46).

(iii) Experiments do not typically distinguish between experienced and inexperienced participants. It is an empirically interesting question how experience, learning, etc. affect behaviors. Failure to account for experience in some experiments is not an intrinsic weakness of the experimental method. In fact, it is common to run experiments with experienced participants and to study learning effects. Good examples are a study on ratchet effects and incentives with Chinese managers (47) and a study comparing the behavior of workers and students (48). In a recent field experiment on gift exchange, it was shown, for instance, that experienced donors, that is, donors who frequently donate for a particular charitable organization, reciprocally respond to gifts by donating more frequently (32).

(iv) Another concern often raised is scrutiny, that is, the possibility that participants in the lab behave differently because they perceive that they are observed. This is one version of a Hawthorne effect. [Parenthetically, reanalysis of the original Hawthorne data shows that no Hawthorne effect was present in the Hawthorne study (49).] It is a minor problem in many experiments, especially if the decision environment is interactive and "rich," such as in sequential bargaining or market experiments. Moreover, being observed is not an exclusive feature of the laboratory: Many decisions outside the lab are observed. Even on the Internet agents can be observed. In the lab, observers can be added to the experimental protocol. The lab allows the analyst to study the relevance of scrutiny by varying the degree of anonymity, for example, contrasting video experiments where participants are explicitly observed, with single anonymous (participant-participant anonymity) and double anonymous (full anonymity between participants and experimenter) procedures (50–52).

(v) Many scholars have expressed concerns about self-selection of particular participants into experiments. Self-selection is not necessarily a scourge. It can be a source of information on agent preferences (27, 28, 34). In the lab, one can collect detailed data on the backgrounds and personality traits of participants to control for selection or to explicitly study selection in a controlled way (53, 54). Selection is a feature of both field and social experiments (55) and is not a problem unique to lab experiments. Indeed, problems of noncompliance, attrition, and randomization bias plague many field experiments (27, 56).

Exploiting Complementarities

Experiments can be productive in complementing the information obtained from other empirical methods. One can combine lab and field experiments to better understand the mechanisms observed in the field. For example, this can be done by eliciting preferences and relating these preferences to observed behavior in the field (57, 58). Another example of exploiting complementarities is the experimental validation of survey instruments (59). Whereas surveys can generate large and representative data sets that provide statistical power, experiments allow the elicitation of preferences and attitudes in a controlled and incentive-compatible way because participants have to make choices with real money at stake. Such evidence is particularly important in securing better understanding of preference heterogeneity (60–63). The evidence that people are different clashes sharply with the widely used "representative agent" model that assumes that agents are homogenous or can be represented as if they are homogenous. Accounting for heterogeneity in preference parameters enables macro-economists to calibrate economic models in an empirically founded way (61).

We conclude by restating our argument. Causal knowledge requires controlled variation. In recent years, social scientists have hotly debated which form of controlled variation is most informative. This discussion is fruitful and will continue. In this context, it is important to acknowledge that empirical methods and data sources are complements, not substitutes. Field data, survey data, and experiments, both lab and field, as well as standard econometric methods, can all improve the state of knowledge in the social sciences. There is no hierarchy among these methods, and the issue of generalizability of results is universal to all of them.

References and Notes

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