Some Comments on the Papers by Kagel and Battalio and by Smith

Experimentation and Tests of Economic Hypotheses

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Economists are justly proud of the increasing role which they play in the formation of practical public and private economic policy, and in great part this role has become available to us through the development of sophisticated methods for establishing quantitative counterparts to the abstract concepts which characterize economic theory. I believe that an equally important contribution to contemporary economics can be made in the laboratory, but that this contribution must take a rather different form: instead of establishing numerical measures for well-known processes, experiments enable us to test the underlying behavioral hypotheses upon which all economic theory rests. It is from this perspective that I would like to discuss the Kagel–Battalio and Smith papers, so that my concern will be more with the methodology and theoretical underpinning of these papers than with the specific experimental results.

Both of these papers stress the principle that behavioral laws which apply in experimental settings can be expected to apply with equal force to less limited "real world" circumstances. Smith treats this "parallelism" virtually as an axiom, while Kagel and Battalio go even farther and extend the principle not only beyond the limits of the laboratory but across the boundaries of the human species as well. Unfortunately, as conceptual premises, neither statement of principle is made as precise and unambiguous as one might wish, and, indeed, each contains something of an "escape"

ISBN 0-12-416550-8

clause. Smith's proposition specifies *ceteris paribus* conditions without naming the variables which are required to be held constant. Given such broad residual powers to restrict the applicability of the principle, counter-examples to the proposition would certainly be hard to defend. Kagel and Battalio leave themselves two such openings in one sentence, arguing that "principles of economic behavior would be *virtually* unique among behavioral principles if they did not apply, *with some variation, of course*, to nonhumans." (Emphasis added.) In spite of the uneasy feeling that such truisms do not provide an adequate foundation for basic research, I am strongly inclined to accept the authors' view that there do exist fundamental behavioral laws which apply equally well to both experimental and ordinary environments.

Acceptance of the proposition that the same laws underlie both laboratory and "real world" behavior does not, however, imply acceptance of a proposition that the same *behavior* will characterize both circumstances. The two environments are quite different, and even if we confine ourselves to a single species (human), there are many reasons for expecting actual behavior to differ in the two alternative situations. The following are just a few of the problems which are frequently encountered in experimental practice:

(1) Experimental situations often project a gamelike atmosphere in which a "subject" may see himself as "matching wits" against the experimenter–designer of the game. Even with relatively large payoffs, a subject may derive personal satisfaction from perceived "victories" which are not necessarily correlated with the performance indices used by the experimenter.

(2) Experimental subjects are often cast in roles, such as "seller," "dealer," or even "monopolist," and the subject may act in accordance with his own (mis)perceptions of these roles rather than in accordance with other incentives which may have been incorporated into the situation.

(3) "Real world" behavior has usually been learned over many trials or over many years. The relatively short time horizons of experiments cannot hope to capture more than the behavior of the most naïve and inexperienced actors who are found in the wider system.

(4) Among the biological species, human beings are foremost in their capacity to control their own behavior through the implementation of abstract rules. Since these rules can be applied to a variety of different situations, human subjects usually carry many of them into the laboratory. Which of many possible alternative rules is to be applied in the experiment then depends on the background and experience of individual subjects and on their short-run interpretations of the nature of the situation. Subjects in "prisoner's dilemma" experiments, for example, are found to behave in widely different ways, depending apparently on whether they first perceive the cooperative or the competitive aspects of the situation.

Considering the potential severity of these pitfalls, it seems to be extraordinarily optimistic to assume that behavior in an artificially constructed "market" game would provide direct insight into actual market behavior. Nevertheless, these authors are inclined to take their experiments quite literally: Professor Smith, for example, describes his "auctions," "intertemporal equilibria," and "public good" experiments as though they were immediately comparable to the economic phenomena after which they are modeled. Smith shows sensitivity to the interpretation problem early in his paper. This is illustrated in his discussion of "complexity" in which he argues that experimental validity increases with the simplicity of the task which is set for the subjects. But sometimes it is hard to see where Smith's principles are to be applied. He describes one auction experiment (see Fig. 8) in which the seller is a monopolist, and while he seems ready to accept the relevance of buyer behavior which corresponds to his theory of bidding, he fails even to mention that the same experiment reports pricing behavior which is inconsistent with the theory of monopoly. I think that it would be useful if the "precepts" could be invoked to explain to the reader why the experimental bidding behavior is significant while the experimental pricing behavior is not.

Kagel and Battalio confine their discussion to more elementary economic phenomena than does Smith, and to that extent, their conclusions are less subject to the pitfalls noted above. However, they pay a price for this conservatism in that their conclusions reflect phenomena which are already extremely well documented in wider (and more relevant) market environments. New empirical techniques are unlikely to gain wide acceptance among economists if their major products are the conclusions that people will not work hard if they can get the same pay without it, that supply and demand elasticities are not zero, or that market price and quantity are inclined to settle in the vicinity of a supply–demand intersection.

There is a methodological problem in all of these experiments which concerns me and which is brought up particularly clearly in Kagel and Battalio's reference to token economy experiments. Originally, token economies were designed by practicing psychologists as a means for modifying human behavior in institutional settings. These "economies" were patterned after the laboratory experiments which have been used to test the psychological theory of reinforcement learning, and it was hoped that the principles of learning behavior which have been established in those experiments would prove to have practical use in bringing about human behavior change. The procedure in fact met with great success, and it is now very widely used. Moreover, even the earliest token economy experiments encountered the phenomena which economists recognize as upward-sloping, and even backward-bending, supply curves, demand functions, and market equilibria, so that these simple market phenomena are now familiar to all token economy practitioners.

The point is that token economies are simply extensions of experiments designed to test learning models, and like the original experiments themselves, they strongly support the implications of reinforcement learning theory. Now the psychologists' theories of learning have very little in common with the economists' concepts of optimizing behavior. In fact, the static model of deliberate utility maximization using full information or Bayesian techniques is completely alien to the psychologists' dynamic trial-and-error *ex post* reward mechanisms. As competing behavioral hypotheses these are readily distinguishable. How then is it possible for the same experiments to "confirm" both of them? In effect, Kagel and Battalio are pointing to successful learning experiments as support for the predictions of an incompatible optimization theory.

It appears that Kagel and Battalio have confined their discussion to a class of phenomena in which the implications of the two behavioral paradigms are identical. As I have shown elsewhere (Cross, 1973), under static full-information equilibrium circumstances the properties of models of reinforcement learning would be empirically similar to the properties of optimization models, but if we were to introduce uncertainty or dynamic adjustment processes, the implications of the two theories would be wholly different. Considering the general success of laboratory experiments in replicating learning phenomena, one suspects that had Kagel and Battalio extended their work to saving behavior or to decision making in risky situations, economists would feel much less comfortable with their empirical results.

All of this is not meant to be a criticism of the experimental paradigm. It seems only that the authors of both of these papers have been more concerned with finding experimental support for what, as economists, we already believe than with seeking phenomena which will challenge these views and thereby expand our horizons. To my mind, the great value of laboratory experimentation is not that it confirms what we already think we know about static equilibria, but that it provides a means for investigating the much more difficult dynamic problems which confront us in our analyses of saving and investment behavior, insurance purchases, and the adjustment processes which characterize disequilibrium.

REFERENCE

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