I. Guide to Discussants

While most of George Stigler’s articles have received the attention they deserve, there is one piece that I think has been neglected, though its potential contribution to the knowledge transmitted at a conference like this one is enormous. The article I refer to is titled “The Conference Handbook” (Stigler 1977). In this incisive piece Stigler argues that conferences could be run much more efficiently if discussants could utilize a standard list of comments that could be called out by number, much as in the old story about the prisoners who told their jokes by number. Stigler offers several introductory remarks and 32 specific comments. For example, introductory remark F could be used nicely at an interdisciplinary conference like this one: “It is good to have a nonspecialist looking at our problem. There is always a chance of a fresh viewpoint, although usually, as in this case, the advantages of the division of labor are reaffirmed.” The specific comments begin with the classic 1: “Adam Smith said that.” Two others that might come in handy at this conference are 23: “The motivation of the agents in this theory is so narrowly egotistic that it cannot possibly explain the behavior of real people”; and 24: “The flabby economic actor in this impressionistic model should be replaced by the utility-maximizing individual” (pp. 442, 443).

While Stigler’s comments are insightful and quite versatile, I have found that conferences that combine psychologists and economists present a special set of problems to discussant and attendee alike, and so I am taking this opportunity to provide a customized list of comments that can be used in these situations. The comments I will mention are those that are most frequently offered by economists when discussing the work of psychologists. For the sake of fairness, a subject in which I have recently become interested, I will also offer brief responses.

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1. If the stakes are large enough, people will get it right. This comment is usually offered to rebut a demonstration of embarrassing inconsistency on the part of a group of undergraduate students participating in an experiment at one of our leading universities. Many such demonstrations have offered the subjects little or no incentive to think hard or to get the "right" answer, so it is reasonable to ask whether financial incentives might not eliminate less than fully rational answers. This, of course, is an empirical question. Do people tend to make better decisions when the stakes are high? There is little evidence that they do.

Some investigators have tested to see whether the introduction of moderate-sized financial incentives will eliminate irrational behavior. For example, Grether and Plott (1979) replicated Lichtenstein and Slovic's (1971) demonstration of the preference reversal phenomenon with and without financial incentives. They discovered to their surprise that the preference reversals were somewhat stronger when financial incentives were used. Of course, no one has received enough financial support to replicate preference reversal phenomena at very large stakes, but the assertion that systematic mistakes will always disappear if the stakes are large enough should be recognized for what it is—an assertion unsupported by any data.

2. In the real world people will learn to get it right. This comment, as is the first one, is derived from a reasonable concern that many experiments have not offered the subjects much if any opportunity to learn. The validity of the assertion again comes down to an empirical question. Do real world environments facilitate learning? Unfortunately, there is little reason to be optimistic. Accurate learning takes place only when the individual receives timely and organized feedback. As Einhorn and Hogarth (1978) have shown, many repetitive decision-making tasks do not provide this type of learning opportunity. For example, a common, well-documented decision-making failing is overconfidence. Subjects in many contexts have been shown to display this trait. Einhorn and Hogarth have shown that in decision-making tasks in which the decision maker usually succeeds, such as selecting students for admission into a highly selective college with a very attractive applicant pool, experience will tend to increase confidence regardless of the ability of the decision maker to discriminate good from bad applicants. Thus experience does not necessarily lead to learning.

3. In the aggregate, errors will cancel. This remark should be used with caution since the errors that have been discovered by the psychologists studying decision making are systematic. Similarly, the statement, If it is not rational, it is random and thus unpredictable, is incorrect. Behavior can be (and is often shown in the laboratory to be) purposeful, regular, and yet systematically different from the axioms of economic theory. I like the term "quasi rational" to describe such
behavior. Someone who systematically overreacts to new information in violation of Bayes’s rule is predictable yet only quasi rational.

4. In markets, arbitrage and competition will eliminate the effects of irrational agents. Markets can provide a unique context for agents to choose in an environment with both monetary incentives and learning opportunities. Moreover, the existence of other agents, ready to exploit the slightest slip, could create a situation in which mistakes are quickly eliminated. Under what circumstances will arbitrage and competition render the choices of quasi-rational agents irrelevant? This question is addressed in my recent paper with Tom Russell (Russell and Thaler 1985). We investigate the operation of competitive markets in which some agents are fully rational and others are quasi rational. We then find the conditions that are sufficient to guarantee that such markets will yield rational equilibria, that is, the equilibria that would obtain if all the agents were rational. We find that these conditions are quite restrictive and are unlikely to occur in any but the most efficient of financial markets. In goods markets, a mistake by one individual will generally not create an arbitrage or profit opportunity for someone else. In these circumstances, mistakes can persist. While it is wrong to assume that behavior discovered in the psychologist’s lab will necessarily survive in real world markets, it is also wrong to assume that markets will always eliminate such behavior.

5. Where is the theory? The original contributions to what is now referred to as behavioral decision theory were simply empirical anomalies, such as those discovered by Allais and by Ellsberg. Even without theories, however, these results were very useful in showing where the existing theory (expected utility theory) made predictions about behavior that were systematically wrong. The papers presented in this session by Tversky and Kahneman (in this issue) and by Einhorn and Hogarth (in this issue) have taken these original anomalies and tried to develop descriptive theories that can account for the observed behavior. These explicitly descriptive theories cannot be derived from normative axioms. Nevertheless, they are theories, and they seem to do a good job of predicting behavior.

6. Economic theory has done very well so far, and if it is not broken . . . How successful is economic theory? The answer to this question depends on what constitutes a test. I propose the following ground rules. A test can be used as supporting evidence by the proponents of a theory if and only if the same test would have been accepted as a refutation had it come out the other way. Let me illustrate by example. If you look through a typical microeconomics textbook you will find few if any “tests” of the theory. However, there is one frequently reported test by Ray Battalio, John Kagel, and their colleagues, who perform experimental studies using animal subjects. (See, e.g., Kagel et al. 1975.) These studies have demonstrated that
rats and pigeons have downward-sloping demand curves and upward-sloping labor supply curves. Such results are cited as supporting the theory. However, if rats were found to violate the substitution axiom, would that count as a refutation of expected utility theory? If rat markets failed to clear, would we abandon the efficient market hypothesis in finance?

More productive than the selective citing of supporting evidence by both sides would be the adoption of the research procedure recommended by the Dutch psychologist Willem Hofstee. Hofstee (1984) recommends that scientists engage in reputational bets. Suppose that X thinks that rational models predict well and that Y thinks otherwise. Then X and Y must stipulate an experiment or other empirical investigation on which they agree in advance to disagree about their predictions of the outcome of the experiment. Hofstee has developed an incentive compatible method for eliciting probabilistic forecasts from each scientist about the outcomes of the experiment. Once the bets are made, a third scientist is brought in to run the experiment, and the three publish the results. A new bureau would be necessary to keep track of each scientist’s rating, as is done in chess. Perhaps a rule could be adopted that in order to maintain “grand master” status a bet has to be made every so often.

Economists and psychologists who genuinely made an effort to find some propositions to bet on might discover that there is less disagreement than was suspected. Perhaps economists do not really believe their models are descriptively accurate or psychologists do not believe their laboratory experiments would generalize to the market. Let us find out! My betting parlor in Ithaca is now open for business.

II. Positive Steps

Many of the results that have created the stimulation for this conference have been negative—counterexamples to the received theory. What positive steps can be taken? In many cases economic theory imposes restrictions on models by specifying variables that should not enter the analysis. For example, marginal analysis, the heart of microeconomic theory, specifies that only marginal costs and benefits should alter decisions. Historical or sunk costs should be irrelevant. Yet anyone who has tried to teach this concept knows that ignoring sunk costs does not come naturally to the uninitiated. Therefore it should not be surprising to discover that sunk costs often influence choices (Thaler 1980; Arkes and Blumer 1985). Positive theories of choice, then, will relax the restriction that sunk costs are irrelevant and will investigate the role they may play in actual decision making. The size of the first part of a two-part pricing scheme, for example, might affect utilization at the margin. (If I paid for it, I am going to use it!) The three papers
presented in this session (Einhorn and Hogarth, in this issue; Simon, in this issue; and Tversky and Kahneman, in this issue) provide other examples of opportunities for improving the descriptive validity of economic theories by relaxing the restrictions.

Herb Simon has stressed for years the importance of cognitive limitations on human decision making. One example of the way his twin concepts of bounded rationality and "satisficing" can be used to enrich economic theory is to incorporate task complexity into descriptive models. Research has demonstrated that, as tasks become more complex, individuals adopt simplifying decision-making strategies (Payne 1976; Russo and Dosher 1983). Economic theorists usually leave task complexity out of their models by assuming that any decision-making problem, no matter how complex, will be solved optimally. Descriptive validity would be increased by assuming that the use of simplifying rules and heuristics (with their accompanying biases) will be used more often in complex situations (even when the stakes are high).

Einhorn and Hogarth (in this issue) suggest relaxing the assumption that perceived ambiguity is irrelevant to choice. Since ambiguity is aversive in many (though not all) situations, the inclusion of ambiguity in a model of individual decision making under uncertainty may help enrich models of insurance purchases (see, e.g., Hogarth and Kunreuther 1985).

Kahneman and Tversky’s research has demonstrated repeatedly that even the most innocuous of assumptions, such as the invariance of choice to problem formulation, may need to be relaxed. Here the possibilities for enriching the economic model are endless. What are the comparative effects on consumption of a "temporary tax increase" compared to an equally large "temporary tax surcharge"? Can we be sure, without any empirical evidence, that the two are identical?

III. Two False Statements

I will end my remarks with the following two false statements.

1. Rational models are useless.
2. All behavior is rational.

I have offered these false statements because both sides in the debate that will be taking place at this conference and at similar conferences in the future have a tendency to misstate the other side’s views. If everyone would agree that these statements are false, then no one would have to waste any time repudiating them.

References


Einhorn, Hillel J., and Hogarth, Robin M. In this issue. Decision making under ambiguity.


Simon, Herbert A. In this issue. Rationality in psychology and economics.

