R. Duncan Luce
Harvard University

Comments on Plott and on Kahneman, Knetsch, and Thaler

Commenting on papers by top researchers (Kahneman, Knetsch, and Thaler, in this issue; and Plott, in this issue) can be, and in this case has been, difficult because of the paucity of valid objections around which to organize one's comments. Of course, this may just reflect the fact that neither paper is close to my major areas of interest. In any event, when facing such a problem—because when asked to be a discussant some time ago I failed to anticipate what should have been obvious—three courses of action come to mind.

The most obvious, although cowardly, possibility was to call in "sick." On reflection, I realized that this task really would not resolve my problem because a conference publication typically lurks in the background, and the organizers would surely demand that I write out my undelivered comments anyhow. It is awkward to argue continued illness when one is up and about, attending other public meetings.

A second plausible tack is to think up a few questions that the authors of the paper may or may not be able to answer. Of course, if one expects to have anything left for the publication, it is wise to make sure that at least one of the questions is unanswerable using the present data or theory.

A third tack is either to extract a phrase or paragraph—the "lesson of the day," as the minister used to say when I was a boy—or to select another paper from the conference as a point of departure to discuss what one would have said at somewhat greater length had the organizers of the conference invited one to be a speaker rather than a discussant.

Having failed to adopt the first approach, I shall illustrate each of the others, first, by asking a few, hopefully sage questions of Kahneman et al. and, second, by using a phrase from Plott's paper and the entire nonassigned Tversky and Kahneman (in this issue) paper as two points of departure for what I really want to say.

(*Journal of Business*, 1986, vol. 59, no. 4, pt. 2)

0021-9398/86/5904-0015$01.50

For Kahneman et al., three questions occurred to me. As I said when I raised them at the conference, I was sure they could deal readily with the first two, but I doubted the third was answerable from the data. And indeed, they have rewritten their paper eliminating the classification scheme about which the first two questions were centered. So I am left with the last question I raised.

Although the results presented are very striking indeed, the fact remains that in many cases something between a quarter and a third of the respondents disagreed with the majority. This certainly suggests that we do not all have the same moral judgments in specific cases; but does it mean more than that? Is it pretty much a matter of chance whether an individual will be in a minority on a question, or is holding the minority view likely to be a behavioral pattern for all judgments of fairness? One can well imagine that there may be two or (probably) more patterns of morality in society and that, if one knew a person's answers to a few of the questions, their answers to the others would become quite predictable. If I understand how these data were collected, one can only use them indirectly to approach that question. Each respondent answered questions about only a small subset of the questions, whereas to verify strongly patterned behavior it would be necessary to have responses to all, or most, of the questions. It might be useful to find out if there is such patterning.

When I turn to Plott's summary of market experiments I am rather more at a loss to come up with a question, in part because I know little from first-hand experience about experimental markets. In this one, I have therefore decided to adopt a ministerial stance and seek out a theme on which to reflect—it is "rational choice." Plott remarks that the market models are based on individual rationality, and, despite the fact there is substantial evidence that rationality is not descriptively adequate, the market models are reasonably descriptive. In addition, he writes (p. S303): "If the following axioms are accepted, then preferences can be induced and controlled for purposes of experimentation.

1. More reward medium (money) is preferred to less, other things being equal (salience and nonsatiation).

2. Individuals place no independent value on experimental outcomes other than that provided by the reward medium (neutrality).

3. Individuals optimize."

From this he "derives" that the preference order over pairs (x, y), where x is the amount of a single commodity and y is money, is necessarily an additive conjoint structure with the representation $R^i(y) + x$, where R^i is an experimenter-determined conversion from the commodity to money. Since the experimenter controls R^i, a wide range of preference orders can be induced on the subject.

Plott argues that assumption 2 is where economists and psychologists part company. That is certainly the case if the nonmonetary commodity has any intrinsic worth to the subject. But if the experiment is so contrived that it is simply a recoding for money, then the assumption is true, but the procedure seems artificial, if not trivial. Let me not get stuck on this point, for it is assumption 3, I believe, that really separates the economists from the psychologists. It is here that something having to do with rationality appears. Just what does that mean, and just what have the experiments shown us? That is, of course, the main topic of the paper by Tversky and Kahneman (in this issue).

I think we will all agree that a major keystone of rationality is transitivity. We must have that if we are to have any kind of numerical theory that associates numbers to individual alternatives and permits us to describe behavior as equivalent to maximizing value. But we all know (thanks in large part to the careful experimental work of Tversky [1969], Lichtenstein and Slovic [1971], Grether and Plott [1979], and Slovic and Lichtenstein [1983]) of circumstances in which transitivity fails. This continues to strike me as one of the most surprising results in the area and one that I believe needs continued investigation.

Recall too that when it is brought to a person's attention that he or she has made strictly intransitive choices, considerable discomfort is exhibited. People seem to feel it to be an inconsistency they do not like, and with good reason since it can be made to bankrupt them. What is not at all clear to me is whether these sorts of intransitivities are in fact relevant to the market situations discussed by Plott. If they are, I would expect that considerable advantage could be taken of the person exhibiting them. Let me leave, albeit uneasily, transitivity and turn to what else is involved in rationality.

Here we enter a great morass of examples and experiments that are, in my opinion, less than perfectly clear in their significance. Tversky and Kahneman (in this issue) conclude that these findings imply that the development of rational and descriptive theories are separate enterprises. I am not yet convinced, in part because there is a theory (Luce and Narens 1985, sec. 7, p. 56) that seems potentially able to encompass both.

Let us take as the domain of discussion an algebra, $\mathscr{E}$, of events and a set, X, of gambles that is closed under binary operations of forming gambles from the events. This is often called a mixture space. More specifically, if A is an event in $\mathscr{E}$ and x, y are gambles in X, then the following gamble is also in X: if the experiment underlying event A is carried out, then x is received when A occurs, and y is received when $\bar{A}$ occurs. I use an operator notation to denote this gamble, namely, $x \mathrm{o}_A y$. The more complex gamble $(x \mathrm{o}_A y) \mathrm{o}_B z$ is interpreted to be a two-stage gamble in which the experiment underlying B is first run, and, if B occurs, then independent of that experiment the one underlying A is run to select between x and y. In particular B may equal A, in which case two independent realizations of the same experiment are involved, as in generating a random sample. Now, assuming a transitive preference ordering $\gtrsim$ on X, I formulate for this structure three additional principles, each of which is true in subjective expected utility (SEU). These are all discussed by Tversky and Kahneman.

PRINCIPLE 1—*Monotonicity of composition* (which is an example of what Tversky and Kahneman call "dominance"). For all x, y, z in X and A in $\mathscr{E}$,

$$x \gtrsim y$$

if and only if

$$x\text{o}_A z \gtrsim y\text{o}_A z$$

if and only if

$$z\text{o}_A x \gtrsim z\text{o}_A y.$$

(In a more careful presentation, I would have to restrict A to be nonnull in some suitable sense.) As we are all aware, this property is really very compelling, and it underlies numerous social dilemmas, such as versions of the prisoner's dilemma. Let me not comment on it until I lay out the other two principles.

PRINCIPLE 2—*Irrelevance of framing* (which is an example of what Tversky and Kahneman call "invariance"). Given any two gambles that are equivalent in the sense that the several outcomes occur under exactly the same conditions, except possibly for the order in which independent experiments are carried out, the gambles should be judged indifferent in preference.

Thus, for example, this principle implies, as is easily verified, that each of the following indifferences should hold:

$$(x\text{o}_A y)\text{o}_B y \sim (x\text{o}_B y)\text{o}_A y, \tag{1}$$

which is called "commutativity in the mixture space";

$$x\text{o}_A y \sim y\text{o}_{\sim A} x, \tag{2}$$

which is called "complementation in the mixture space";

$$(x\text{o}_A y)\text{o}_A z \sim (x\text{o}_A z)\text{o}_A(y\text{o}_A z), \tag{3}$$

which is called "right autodistributivity" in the mixture space.

I shall refer to such equations where the only real difference is one of framing as "accounting equations." Again, let me hold off on comments.

PRINCIPLE 3—*Event monotonocity* (which Tversky and Kahneman call "cancellation" and the "sure-thing principle"). Suppose $x > y$ and A, B, C in $\mathscr{C}$ are such that $A \cap C = B \cap C = \emptyset$. Then

$$x \mathrm{o}_A y \gtrsim x \mathrm{o}_B y$$

if and only if

$$x \mathrm{o}_{A \cup C} y \gtrsim x \mathrm{o}_{B \cup C} y.$$

Violations of this property are often called the Ellsberg paradox.

Note well the difference between principles 1 and 3. The former concerns monotonicity under formation of new gambles from old ones, whereas the latter concerns monotonicity when the outcomes associated with the events are systematically altered by changing the outcome on both sides over an event, C, that is disjoint from both A and B. These two principles are not to be confused since, as will soon be clear, they play very different roles in utility theory. The fact that the term "sure-thing principle" has been applied to both suggests that they may not always be clearly distinguished, although they certainly are distinguished by Tversky and Kahneman.

As was emphasized by Tversky and Kahneman, SEU satisfies all three properties, and it is descriptively wrong. Are all three principles wrong, or are only some of them? According to my reading of the literature we know that the Ellsberg paradox is exhibited by many people, and thus event monotonicity, principle 3, is not descriptively valid in general. We also know, as is well summarized in Tversky and Kahneman, that versions of principle 2, invariance, and versions of principles 1 and 2 combined, dominance and invariance, are not descriptively correct. They assert that when dominance is made transparent, as it is in the statement of principle 1, people abide by it. I think, however, I am correct in saying that we do not have any true experimental test of that claim. But let me assume that it will be confirmed. Then it is interesting to ask what sort of theory can be based on just transitivity and monotonicity of composition (dominance). Ignoring possible failures of transitivity such a theory might be descriptively adequate and, with additional postulates such as principles 2 and 3, reduce to SEU.

Such a theory, called "dual bilinear utility," can be found in Luce and Narens (1985). I will not attempt to recount the argument, which is intricate and depends in part on deducing the form of the most general concatenation structure that has an interval scale representation onto the real numbers. Suffice it here to give the representation that results from these considerations: there is a utility function, U over X, and two weighting functions, S^+ and S^-, on $\mathscr{C}$ such that U is order preserving and

$$U(x\text{o}_A y) = \begin{cases} S^+(A)U(x) + [1 - S^+(A)]U(y), & \text{if } x > y, \\ U(x), \quad \text{if } x \sim y, \\ S^-(A)U(x) + [1 - S^-(A)]U(y), & \text{if } x < y. \end{cases}$$

The utility function is an interval scale, as is easily verified. Note that this form degenerates into SEU provided that $S^+ = S^-$ = a probability measure; otherwise, it has some differences of importance. Let me point out five.

First, given the way the representation was derived, it necessarily satisfies monotonicity of composition (principle 1), a fact that is easily verified directly.

Second, this representation satisfies the framing principle of commutativity in the mixture space that is embodied in equation (1). This corresponds to the idea that the order in which experiments are conducted does not have any special significance. Personally, I am a bit more suspicious of this than I am of monotonicity of composition, and it certainly needs to be studied in isolation.

Third, this representation satisfies complementation in the mixture space (eq. [2]) if and only if, for each A in $\mathscr{E}$,

$$S^+(A) + S^-(\bar{A}) = 1.$$

This is not yet the same as saying that S^+ and S^- are identical.

Fourth, it satisfies right autodistributivity (eq. [3]) if and only if $S^+ = S^-$, in which case there is a single bilinear weighting rather than different ones that depend on the relative value of the components. I should point out that many other similar accounting equations, such as bisymmetry, lead to the same conclusion. Indeed it appears that any accounting equation that involves either more than two outcomes or more than two events forces this much of SEU. This means that, if we are to have the real flexibility of the dual bilinear model, the principle of irrelevance of framing must be invoked in a most limited way.

Fifth, the dual bilinear utility model satisfies principle 3 (monotonicity of events or cancellation) if and only if for each A, B, C in $\mathscr{E}$ with $A \cap C = B \cap C = \emptyset$ and for each $i = +, -$,

$$S^i(A) \geq S^i(B)$$

if and only if

$$S^i(A \cup C) \geq S^i(B \cup C).$$

This property holds, of course, when the S^i are probability measures.

I will not go into the calculations (they may be found in Luce and Narens [1985]), but the general dual bilinear model is closely similar to, and more general than, the prospect theory of Kahneman and Tversky (1979), which means that it can encompass many of the empirical results that are inconsistent with SEU. One of the major differences between the two theories, and one that no doubt can be exploited to distinguish between them empirically, is that the dual bilinear model exhibits a form of relativity internal to the gamble, placing different weights on the more preferred outcome than on the less preferred, whereas the prospect model has a form of relativity that is embodied in the form of the utility function, vis-à-vis present wealth.

In terms of empirical work, the dual bilinear model suggests that we should study monotonicity of composition in isolation from the framing questions, and such a study is currently under way. In addition, I think it would be interesting to examine carefully several of the accounting equations, in particular, equations (1) and (2). And, of course, it would be interesting to see what sorts of economic models would arise on the assumption of this somewhat more limited notion of rationality, one that does not invoke the strong analytic abilities implicit in the irrelevance-of-framing principle. In its present form the dual bilinear model is unsatisfactory in at least two important respects. First, it does not explicitly talk about gambles having more than two outcomes except to the extent that they can be decomposed into compositions of binary gambles. Second, no axiomatization of it has been developed in terms of preference as a primitive. I have shown how to axiomatize general interval scale concatenation structures (Luce 1986), but it is not immediately obvious how to use that to obtain an effective axiomatization of dual bilinear utilities. Considerable work remains to be done, but I think this model demonstrates the real possibility of evolving descriptive theories based on limited (bounded) forms of rationality, which, when sufficient properties are added, becomes SEU.

References

Grether, D. M., and Plott, C. R. 1979. Economic theory of choice and the preference reversal phenomenon. *American Economic Review* 69 (September): 623–38.

Kahneman, D.; Knetsch, J. L.; and Thaler, R. H. In this issue. Fairness and the assumptions of economics.

Kahneman, D., and Tversky, A. 1979. Prospect theory: An analysis of decision under risk. *Econometrics* 47 (March): 263–91.

Lichtenstein, S., and Slovic, P. 1971. Reversal of preferences between bids and choices in gambling decisions. *Journal of Experimental Psychology* 89, no. 1:46–55.

Luce, R. D. In press. Uniqueness and homogeneity of ordered relational structures. *Journal of Mathematical Psychology.*

Luce, R. D., and Narens, L. 1985. Classification of concatenation measurement structures according to scale type. *Journal of Mathematical Psychology* 29 (March): 1–72.

Plott, C. R. In this issue. Rational choice in experimental markets.

Slovic, P., and Lichtenstein, S. 1983. Preference reversals: A broader perspective. *American Economic Review* 73 (September): 596–605.

Tversky, A. 1969. Intransitivity of preference. *Psychological Review* 76 (January): 31–48.

Tversky, A., and Kahneman, D. In this issue. Rational choice and the framing of decisions.