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COMMENTARY

Commentary on Aspects of Lola Lopes' Paper

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Lopes (1996) clearly has limited patience with the concept of "rationality" as a basis for, or descriptor of, human decision making. I, in contrast, find some of the principles of rationality reasonably compelling—as do subjects when the situation allows them to see the structure of the decision they face—and so it strikes me as relevant to understand in some detail just which principles of rationality are and are not being violated. This, I believe, is the thrust of a good deal of the contemporary work, some of which she mentions, relating data and theory.

The first question is just what does the behavioral evidence against subjective expected utility (SEU), which surely is overwhelming, tell us is wrong with that theory. I have argued (Luce, 1990, 1992; Luce & von Winterfeldt, 1994) that it is very important to distinguish between two types of rationality embodied in SEU. The one, called *preference rationality*, includes such principles as transitivity and consequence monotonicity (i.e., replacing a consequence in a gamble by something better can only make the gamble better). Violations of these properties typically are viewed as errors when they are pointed out to subjects. The other type, called *structural rationality*, says that one's preferences should not be altered by changing the way a gamble is formulated so long as the bottom line remains unchanged.[1] It may well be that people agree to this in principle, but most unaided people simply fail to implement it successfully. Try to work through with undergraduates the equivalence

$$(x, E; (y, D; z)) \Leftrightarrow ((x, E; y), D; (x, E; z)), \qquad (1)$$

where the symbol $(u, E; v)$ means one receives u if event E occurs and v otherwise and the underlying chance experiment is independently run twice to realize the events D and E. I believe that the empirical evidence is heavily against the behavioral accuracy of most structural rationality properties, but it is much less so against preference rationality.[2]

The rank-dependent utility (RDU) models of the 1980s (for a summary see Quiggin, 1993) have exactly the feature of maintaining preference rationality and abandoning most of structural rationality. Their most complex structural property is what has been called *event commutativity:*

$$((x, D; 0), E; 0) \sim ((x, E; 0), D; 0). \qquad (2)$$

Here the bottom line is reasonably clear: x if D and E both occur and 0 otherwise; they differ only in the order in which D and E are realized.[3] This has been tested and although the results are somewhat mixed, the most careful laboratory study (Chung, von Winterfeldt, & Luce, 1994) supports Eq. (2) for a large proportion of subjects. Adding further, more complex conditions of structural rationality, such as Eq. (1),

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[1] Economists often, seemingly unwittingly, invoke the fully strength of structural rationality by assuming that lotteries can be modeled as random variables. The convention of probability theory is that two formulations of random variables that have the same distribution are to be treated as identical.

[2] There is a current debate about consequence monotonicity with Birnbaum (1992) and Mellers, Weiss, and Birnbaum (1992) claiming evidence against it and von Winterfeldt, Chung, Luce, and Cho (submitted) arguing that the apparent violations can be attributed to the noisiness of the data.

[3] Lopes has raised the question of what happens when on the first run the outcome 0 occurs: is the second run carried out anyway? The theory is neutral on this point, but it might make an empirical difference. So far as I know, it has never been studied.

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to the rank-dependent model eliminates the rank dependence and so reduces it to SEU and that automatically forces a vast array of structural equivalences (Luce & Narens, 1985).

The Samuelson example in the Lopes article raises another aspect of rationality not touched on at all by SEU or RDU. His argument is this: Suppose g is a gamble with large gains and large losses but $EV(g) > 0$. An "irrational" person prefers the status quo to having g but, for some sufficiently large n, prefers playing it n times independently,[4] g^n, to the status quo. A rather more common example, exhibited by millions of people every day, is to prefer to the status quo a lottery for which, when the purchase price is taken into account, $EV(g) < 0$. Yet, for most people, there is some sufficiently large n—usually rather small—such that they will not buy n or more such lotteries, and so the status quo is preferred[5] to the convolution g^n. Clearly, as Samuelson observes, such behavior is totally inconsistent with expected value calculations, but it is not clear one way or the other whether it is inconsistent with RDU, SEU, or even EU. The difficulty is that the classical theories do not provide any formula for calculating $U(g^n)$ from $U(g)$. This aspect of rationality/nonrationality debate seems to have received less attention than one might expect given its ubiquitousness.

Recently Luce (1995) examined such issues theoretically and Cho and Luce (1995) studied them experimentally. Under fairly well confirmed assumptions, I showed that if CE denotes the certainty equivalent of a gamble, i.e., a sum of money for which $CE(g) \sim g$, then a person who is monotonic[6] in convolution $*$ has to exhibit the property

$$CE(g*h) = CE(g) + CE(h), \tag{3}$$

and, conversely, Eq. (3) implies monotonicity of $*$. Obviously, a person for which $CE(g) = EV(g)$ satisfies Eq. (3) because convolution is nothing but the distribution of the sum of independent random variables. Clearly, those buying lottery tickets must violate Eq. (3) for sufficiently many convolutions because were they monotonic we would have the contradiction $CE(g) > 0$ and $CE(g^n) = nCE(g) < 0$.

To study this issue empirically, Cho and Luce (1995) first attempted to classify subjects according to their disposition to accept gambles with negative EVs. We confronted them with 10 gambles whose EVs were slightly negative—about −\$2—and we determined their CEs. A subject was classed as a "gambler" if 7 or more[7] of the CEs were positive and as a "nongambler" otherwise. Our conclusion from the partitioned data was that the gamblers tended to violate Eq. (3) whereas nongamblers appeared to satisfy it.

Closely related to this work, Luce and Fishburn (1991, 1995) argued that one really should enrich the domain of certain consequences and gambles to include receiving several things—gambles or certain consequences—at once. Denote by $g \oplus h$ the *joint receipt* of the independent gambles g and h. As noted above, from a rational perspective—e.g., Samuelson's—joint receipt should just be convolution, i.e., $\oplus = *$. Cho and Luce (1995) found this equality not to be rejected for their gamblers, but it was for their non-gamblers. So both types of people appear to be nonrational, but in different ways: the gamblers see that $\oplus = *$, which is rational, but fail to be monotonic, which is not; the nongamblers are monotonic in $*$, which is rational, but fail to see that $\oplus = *$, which is not.

To illustrate further the usefulness of the concept of joint receipt consider the following properties. The first is the highly rational property of *segregation*, i.e., for x and y of the same sign,

$$(x, E; 0) \oplus y \sim (x \oplus y, E; y). \tag{4}$$

The second is that the status quo is singular, i.e.,

$$U(0) = 0. \tag{5}$$

And the third is binary rank dependence for the special gamble $(g, E; 0)$, i.e., taking into account Eq. (5),

$$U(g, E; 0) = U(g)W(E), \tag{6}$$

where W is a weight (between 0 and 1) assigned to the event E. In general, the weights are not probabilities. Furthermore, different weighting functions are used depending on whether g is seen as a net gain or a net loss.

Luce and Fishburn (1991, 1995) showed, first, that these three assumptions (plus some assumptions on the richness of the domain) imply that for gains,[8] there is a positive constant C such that

[4] Technically, lotteries are independent random variables, and the distribution of their sum is the convolution of their distributions.

[5] As Lopes has pointed out to me, this apparent reversal of preference may, of course, be dictated by some, partially self-imposed, budget constraint. The actual underlying cause of the reversal is, however, immaterial to my argument.

[6] Monotonicity of $*$ means that $g' \succeq g$ if and only if $g'*h \succeq g*h$.

[7] The conclusion does not change if one uses 6 or more or 8 or more in constructing the partition.

[8] For losses, the same equation holds except that the constant (a different one) is negative.

$$U(x \oplus y) = U(x) + U(y) - \frac{U(x)U(y)}{C}. \quad (7)$$

If $\oplus$ is monotonic, which seems to be somewhat in doubt for gains from data of Cho and Luce (1995),[9] then U must be bounded by C. Second, using Eq. (7) recursively with the obvious generalization of segregation to any finite gamble of gains (or of losses), they derived the general rank-dependent representation for gains (and losses). There is empirical evidence both in Cho, Luce, and von Winterfeldt (1994) and in Cho and Luce (1995) favoring segregation. The separation property of Eq. (6) is common to many theories and was tested experimentally using additive conjoint measurement methods by Tversky (1967). So Eq. (7) and the rank-dependent form appear to be a very plausible properties of utility. [Note that Eq. (7) differs greatly from the formula proposed by Thaler (1985) called the *hedonic rule,* whose properties were worked out by Fishburn and Luce (1995).]

If $x \oplus y = x + y$, which many have conjectured and has been sustained in the experiment of Cho and Luce (1995) but violated for gains in a questionnaire study by Thaler (1985), then Eq. (7) implies that U has the following negative exponential dependence on money (Luce & Fishburn, 1995): For $x > 0$, there exists $k > 0$ such that

$$U(x) = C(1 - e^{-kx}). \quad (8)$$

Another use of joint receipt arises with gambles of mixed gains and losses. Consider the following property, called *duplex decomposition:* For $x > 0 > -y$,

$$(x, E; -y) \sim (x, E'; 0) \oplus (0, E''; -y), \quad (9)$$

where E' and E'' are realizations of the event E in two independent runs of the underlying chance experiment. Duplex decomposition violates the rationality condition that $\oplus = *$ because the convolution of the right two gambles clearly is not identical to the left gamble. Nonetheless, a fair amount of data sustain Eq. (9) (Slovic & Lichtenstein, 1968; Payne & Braunstein, 1971; Cho, Luce, & von Winterfeldt, 1995). That being so, it clearly is important to distinguish between $\oplus$ and $*$. Duplex decomposition coupled with the assumption that utility is additive over the joint receipt of gains and losses, i.e.,

$$U(x \oplus -y) = U(x) + U(-y), \quad (10)$$

for which we lack a strong argument[10] results in

$$U(x, E; -y) = U(x)W^{+}(E) + U(-y)W^{-}(\neg E), \quad (11)$$

here different weights are used depending on the sign of the consequence. Because in general $W^{+}(E) + W^{-}(\neg E) \neq 1$, U(0) must be 0 and so U is a ratio, not an interval, scale measure of utility. Equation (11) is a feature of rank- and sign-dependent theory (Luce, 1991; Luce & Fishburn, 1991), cumulative prospect theory (Tversky & Kahneman, 1992), and the earlier and much more restricted prospect theory (Kahneman & Tversky, 1979).

I have gone through these details in an attempt to make clear that some descriptive theories have been derived by carefully blending rationality and nonrationality assumptions that appear to be sustained empirically. The resulting models violate, as do untutored subjects, many—indeed, most—of the structural rationality equivalences.

Even so, as Lopes has emphasized, it is difficult to reconcile such theories with the reasonably clear evidence that subjects' reference or aspiration levels have a significant impact on behavior. In the rank- and sign-dependent theories, 0 (i.e., no exchange) plays that role: gains and losses in money are handled differently according to these theories. The problem is that we have a number of reasons, many due to Lopes, to believe that subjects do not always set their level at 0. In a choice between ($100, .5; $10) and ($50, .5; $25), a person selecting the former and receiving $10 is likely to see that consequence as, in some sense, a loss because at least $25 was guaranteed by choosing the other gamble. We still lack a satisfactory theory of how reference levels are established and, for that matter, good empirical ways of estimating them directly. Cho and I have attempted to elicit reference levels directly with little success. Luce, Mellers, and Chang (1993) suggested, none-too-seriously, one such theory which was based on the following principle: for gains, the reference level is the least CE of the gambles in the choice set and for

[9] They provide evidence that either $\oplus$ is not monotonic or CEs as they determined them using the up–down choice procedure called PEST are not order preserving over $\oplus$ or both. Neither alternative warms a theorist's heart, and so it continues to be under investigation. An attempt is being made to collect sufficient data from each subject so that they can be studied individually. This is essential if there are substantial individual differences which is suggested by the observed differences in the (perhaps gross) partitioning of subjects into gamblers and nongamblers.

[10] Luce and Fishburn (1991) argued for Eq. (10) on the grounds that it is the simplest form that is compatible with Eq. (7) and that maintains the monotonicity of preference over $\oplus$. This is not a very strong argument, especially if it turns out that preferences over $\oplus$ are not monotonic.

losses it is the least loss CE. We showed that recoding the gambles as gains and losses relative to such a reference level could in fact account for many anomalous phenomena. But we admitted this was a mere demonstration of the flexibility of such models and did not propose it as a serious theory. Until we understand a lot better than we now do how reference levels get established, it is doubtful if a good theory will appear.

Although I get there by a somewhat different route, I concur with Lopes final conclusion: "Although each paradox and axiom is attractive in its own way, none compels unthinking allegiance. Rather, each enriches our understanding of the evolving and imperfect constructions that humankind has created to describe and to aid decision making."

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