ON THE INTERPLAY OF RISKLESS

AND RISKY UTILITY

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In developing this paper, I re-read some of Ward Edwards major contributions to the
study of behavioral decision making (Edwards, 1954, 1962, 1992; von Winterfeldt and
Edwards, 1986) to remind myself about how he has viewed the developments of a
half century of work on individual decision making that began with von Neuman and
Morgenstern's (1947) theory of expected utility. I focused on three aspects:

- The relation between normative, prescriptive, and descriptive theories.
- The importance (or perhaps not) of some concept of reference level and the
  resulting concepts of gains and losses.
- The relation of riskless and risky choice[1].

Although I will concentrate mostly on the last topic, in part because it is the least
satisfactorily developed in the literature, I do want to make a few observations about
the interrelation of the first two.

Reference Levels and Theoretical Approaches

Ward was one of the first, at least among decision theorists, to emphasize the role of
reference levels and the resulting concepts of gains and losses. I won't attempt to
summarize the history of this concept. Suffice it to say that, with varying degrees of
enthusiasm, most of us have had to acknowledge that reference levels play a descrip-
tive role, and many of the more influential attempts at descriptive theory have included
some inherent notion of zero utility (Bell, 1982; Coombs, 1964; Kahneman & Tver-
sky, 1979; Luce, 1991, 1996, 1997; Luce & Fishburn, 1991, 1995; Markowitz, 1952;
Tversky & Kahneman, 1992). Most psychologists working on judgment and decision

making are convinced that, in general, people do not treat gains and losses in the same way. The concepts of risk aversion for gains and risk seeking for losses arise only if such differential treatment occurs.

Despite this widespread belief, little serious research exists on the concept of reference level itself for preferences—in particular, how it seems to be determined in particular contexts. Indeed, I know of no study in which it is measured or manipulated in any direct fashion nor do I know of any serious attempts to build a theory for its determination. Luce, Mellers, and Chang (1993) formulated a possible direction for its determination as part of their demonstration that some of the empirical findings concerning possible non-monotonicities of consequences (Birnbaum, 1992, 1997; Mellers, Chang, Birnbaum, and Ordóñez, 1992) could result from reference levels that depend on the choice alternatives presented for choice. However, I viewed that specific model as no more than a demonstration of the sorts of things that changes in reference level can produce, not as a theory likely to stand up to further tests. In particular, I am very suspicious of the rule we used for determining reference levels, namely, the minimum gain or, when there are no gains, the minimum loss among the alternatives under consideration. To date, my students and I have been unable to devise an empirical way to check reasonably directly the adequacy of that—or any—theory for reference levels. In most applications of theories in which the status quo or reference level plays a role, we simply take it for granted that the reference level is the current state of wealth and that receiving money is a gain and losing it is a loss. But we know full well that under some circumstances, the reference or aspiration level can be quite different from the current state. A familiar example is the man who desperately needs $1,000 to cover the mortgage payment and who has only $250 and tries to solve the problem by gambling. He bets the $250 and recovers it plus $500. As this leaves him shy of his aspiration by $250, it is effectively a loss. The analysis of Allais's paradox given below is another example of a shifting reference level.

Because gains and losses are such pervasive concepts, it really is quite difficult to avoid their playing a role in prescriptive uses of decision analysis. It must be very difficult indeed to convince a CEO that what is called zero utility is totally arbitrary and that decisions should ignore whether the monies involved are called gains (profits) or losses. Cost-benefit analyses are based on such a distinction. Estimating utility functions has proved problematic and so, for the most part, they are avoided. I will return later to a possible reason for these difficulties.

The question also arises whether one should, as in the descriptive theories, assign different weights or subjective probabilities to uncertain events contingent on their leading to a gain or a loss. To those analysts who adhere to the standard normative theory of subjective expected utility (SEU)—the "old time religion" of Howard (1992) which was generally agreed to be *the* normative model by the participants at Edward's 1989 "revival meeting" held in Santa Cruz and excellently summarized in Edwards (1992)—the idea that one might weight uncertainty differently depending on the sign of the outcome is anathema. The normative axioms have no room for such heresy.

One question, which I have never fully resolved in my own mind, is whether the pervasive distinction between gains and losses, which everyone I have ever encountered makes except, possibly, Ronald Howard should be viewed simply as a failure of rationality—in much the same way that rather common errors in drawing inferences

are simply seen as violations of logic or probability—or whether we have simply over-simplified our concept of rationality.

Let me illustrate my concern by once again looking at Allais's (1953) famous paradox. Recall that most people when confronted with the choice

$$(\$5M, 0.10; \$1M, 0.89; \$0, 0.01) \text{ versus } \$1M$$

typically choose $\$1M$; whereas when offered the choice

$$(\$5M, 0.10; \$0, 0.90) \text{ versus } (\$1M, 0.11; \$0, 0.89)$$

they typically choose the left gamble with the $\$5M$ consequence. It is not difficult to show either by calculating expected utility or by using two of the usual rationality axioms, namely monotonicity of consequences and the reduction of compound gambles to equivalent first order ones, that the rational decision in the second is to choose the right-hand gamble if the $\$1M$ is chosen in the first situation. So the behavior, which most people exhibit, is alleged not to be rational.

Yet, there seems to me a very good "rational" argument for doing what people do. The first choice is immediately recoded as a choice between:

$$(\$4M, 0.10; \$0, .89; -\$1M, 0.01) \text{ versus } \$0.$$

The reason for this recoding is that because $\$1M$ can be assured it is reasonable to absorb it, virtually instantly, into one's wealth, which is done by subtracting it from both alternatives. Doing so, makes very clear what the risk of the gamble is an improbable, but large, loss. Many of us see this gamble as less desirable than the new status quo. But when we are faced with the second of Allais's alternatives, there is no "natural" sure amount to subtract away, and so no losses appear. Of course, a normative theorist will not only say that this is a silly analysis—framing of a choice should not matter—, but also will note that there is nothing in the usual decision setup that permits one to talk about the "subtraction" of $1M from a gamble.

My response to that claim is that I do not see why one type of reduction or reframing, e.g., reduction of compound gambles, is clearly acceptable and rational whereas another type of reframing, e.g., subtracting a common consequence, is not. Surely the mere fact that for 50 years we have restricted ourselves to the framework of just preferences among gambles should not be taken as holy writ. Indeed, is it plausible to say that people are irrational because they work outside the usual setup, or are we better off acknowledging that the usual decision theory apparatus is unnecessarily restrictive? I take the latter position, a view that is explicated in the balance of this paper.

Riskless Utility

Like many others[2] during the past five decades, Ward has somewhat skirted the issue of riskless utility. The initial section of his 1954 survey discusses how classical—pre-World War II—economics routed out the idea of cardinal (interval or ratio scale) utility and showed that most of what they were doing rested only on ordinal considerations. One slight difficulty of this approach was that it rendered meaningless the concept of

diminishing marginal utility, which did seem a bit odd. It was only with the advent in 1947—exactly 50 years ago—of studying risky and, in 1954, uncertain alternatives that interval-scale utility became not only respectable, but the totally dominant concept (Savage, 1954; von Neumann & Morgenstern, 1947).

von Winterfeldt and Edwards (1986) devoted several chapters to riskless utility. They began by declaring it to be, in reality, a vacuous concept. No alternative is ever really riskless—the money you receive may be counterfeit, the automobile you buy may be a lemon, the book arriving in the mail may have a 32 page signature replaced by one from another book (as was true for my copy of the Russian translation of *Games and Decisions*), and so on. Following that decidedly reserved start, they go on to explicate one of the riskless ideas that has been pursued, namely, that we can usually think of riskless objects of value as having multi-attributes and so we can use trade-offs among attributes as a means of measurement (Keeney & Raiffa, 1976). At the least, this approach has the drawback that it cannot be used for money consequences.

Another approach that has received some attention is to add the new primitive judgment of "greater utility difference" between two pairs of objects (Krantz, Luce, Suppes, & Tversky, 1971, p. 492-493; Krzysztofowicz, 1994; Wakker & Denefee, 1996). This, it seems to me, has the decided drawback of being an unnatural judgment. When in our ordinary life do we judge that one pair of objects differs more in preference than does another pair?

In addition, neither of these approaches provides a way to formulate what happens when a gamble is reframed by subtracting off a fixed amount. So, I turn to a formulation of that operation.

Joint Receipt—The experience of receiving two or more things at once is ubiquitous. We may call the resulting entity their "joint receipt." It is perhaps simplest to begin with the binary concept, so if g and h are two valued entities, then let $g \oplus h$ denote the joint receipt of g and h. The only limitation on g and h is that both they and $g \oplus h$ be valued by the decision maker. For the special case where g is a riskless alternative, e.g., a sum of money which is identified as an element of the real numbers $\mathbb{R}$, the notation is changed to $x, y, z \cdots, x \oplus y, \cdots$.

Two things are excluded in the concept of joint receipt: First, two valueless things together do not become valuable and, second, two valued things together do not become valueless. It is not difficult to think of examples that fail to meet these requirements. For example, in the wilderness a gun alone or a clip of bullets alone are virtually worthless, but when possessed jointly (and assuming the clip fits the gun), they could be quite valuable. Other more homey examples exist[3] such as single shoes or ear rings versus pairs of matched ones—at least, this is an example for older generations. Clearly no such problems arise if one restricts attention to money gambles, which covers a substantial portion of the literature. The non-monetary counterexamples have to be bypassed by the experimenter much as similar cases are in physics. For example, in carrying out weight measurement using a pan balance one has to be cautious about exactly what materials are put together in the same pan of a balance if one wishes to avoid a disaster.

Possible Properties of Joint Receipt and Preference—The analogy of joint receipt to weights on a pan balance is, in fact, very close, especially if one considers weighing in air using containers (matched in weight) of substances whose densities are both greater and less than that of air.

In particular, let $\mathcal{G}$ denote the collection of valued objects with e a distinguished entity representing the status quo. Given that the entities in question are valued, we postulate a preference relation $\succsim$ over $\mathcal{G}$. Of course, $\succ$, $\precsim$, $\prec$, and $\sim$ have their usual definitions.

Ignoring e to begin with, we assume that, for all $f, g, h \in \mathcal{G}$, $\oplus$ and $\succsim$ satisfy:

1. *Closure:* $f \oplus g \in \mathcal{G}$.
2. *Weak Order:* $\succsim$ is a connected and transitive relation.
3. *Weak Associativity:* $f \oplus (g \oplus h) \sim (f \oplus g) \oplus h$.
4. *Monotonicity:* $f \succsim g$ if and only if $f \oplus h \succsim g \oplus h$ if and only if $h \oplus f \succsim h \oplus g$.

Testing associativity depends exactly on how $\oplus$ is implemented empirically, and so far no direct studies have been reported. The concept is so transparent, it is difficult to imagine it will not be satisfied in any natural realization. For example, associativity might have to do with how things are packaged at a store for shipping. If it takes two boxes, do you care which item is alone in a box and which two are packaged together? Of course, one can imagine circumstances where it might matter, but not for reasons having to do with preferences per se among the entities: It probably is better to package the Limburger cheese alone and away from foods that absorb odors, and for ease of carrying it may be desirable to balance the weights of the two packages. But for the most part, the parentheses seem totally artificial. Certainly they do normatively.

Monotonicity of joint receipt is just as normatively compelling as monotonicity of consequences is for gambles. However, Cho and Luce (1995) showed that either it fails descriptively or that the method they used to determine certainty equivalents of gambles[4] is not perfectly order preserving. Subsequent work by Cho and Fisher (submitted) seems to sustain monotonicity and raises some doubt about the order preserving property of PEST-determined certainty equivalents.[5] The possibility of bias in our version of PEST was investigated using simulations by Sneddon and Luce (submitted).

Adding the status quo e gives rise to three conditions. The first is that e is an identity whose behavior is similar to 0 in the additive real numbers, i.e.,

5. *Identity:* $\forall g \in \mathcal{G}, g \oplus e \sim g \sim e \oplus g$.

Relative to e one defines a gamble g as a *gain* if $g \succ e$ and as a *loss* if $g \prec e$. We assume that the domain of valued objects is rich enough that for each loss there is a gain that will just negate it and for each gain there is a loss that will just negate it, i.e.,

6. *Inverses:* $\forall g \in \mathcal{G} \; \exists g^{-1} \in \mathcal{G}$ such that $g \oplus g^{-1} \sim e$.

The next, and last, basic property is, by its non-finite nature, inherently untestable in a direct sense. Only the failure of indirect consequences would make it suspect. To state it, we define inductively for each $g \in \mathcal{G}$ and integer n, $g^{(1)} = g$ and $g^{(n)} = g^{(n-1)} \oplus g'$, where $g' \sim g$. In words, $g^{(n)}$ is the joint receipt of n copies of g. Because of associativity, the apparently asymmetric construction is immaterial.

7. *Archimedean:* $\forall f, g \in \mathcal{G}$, if $g \succ e$, then there exists an integer n such that

discussed by Pfanzagl (1959), which he called "consistency" and is a generalization of one of the "pre-editing" principles discussed informally by Kahneman and Tversky (1979) and which they called "segregation." I will use the latter term and formulate the concept as follows. Let $\mathcal{E}$ denote a set of chance experiments and, for $E \in \mathcal{E}$, let $(f, C; g, E \backslash C)$ denote the uncertain alternative in which f is the consequence if the event $C \subseteq E$ occurs and g is the consequence if C fails to occur when the experiment is conducted. When there is no ambiguity about the experiment in question, the notation for the gamble is simplified to $(f, C; g)$.

Definition 1. *Segregation (for gains)* is said to hold if for all $x, y \in \mathcal{G}$ with $x \succ e, y \succ e$ and $C \subseteq E \in \mathcal{E}$,

$$(x, C; e) \oplus y \sim (x \oplus y, C; y). \tag{4}$$

We see that the two sides amount to the same thing, and so it is indeed a highly rational axiom[8]. It has been studied empirically in two papers, Cho and Luce (1995) and Cho, Luce, and von Winterfeldt (1994), and it appears to be sustained within the noise level of their methods.

The introduction of $\oplus$ allows one to define a concept of "subtraction" in the usual way:

Definition 2. For all $f, g, h \in \mathcal{G}$,

$$f \sim g \ominus h \iff g \sim f \oplus h. \tag{5}$$

Note that if $\oplus$ is additive over money, i.e., Eq. (1) holds, then for x and y both money gains or money losses

$$x \ominus y = x - y, \tag{6}$$

which with segregation is exactly the property invoked above in the analysis of the Allais paradox and invoked by Kahneman and Tversky (1979) as pre-editing.

The Negative Exponential Representation—I will not attempt to give a precise statement of the result about F, but only the gist of it (for details see Luce, 1996, 1997, Luce & Fishburn, 1991, 1995). Suppose that U is the simplest rank-dependent representation of binary gambles, i.e., for $x \succsim y \succ e$,

$$U(x, C; y) = U(x)W(C) + U(y)[1 - W(C)], \tag{7}$$

that U is *weakly subadditive*[9] in the sense that

$$U(x \oplus x) < 2U(x), \tag{8}$$

and that segregation holds. Then one can show mathematically that for some constants $\delta > 0, \Delta > 0$,

$$F(z) = \Delta[1 - e^{-\delta z}], \tag{9}$$

whence

$$U(x) = \Delta[1 - e^{-\delta V(x)}]. \qquad (10)$$

Thus, U and V are related through a negative exponential transformation. Clearly, Δ and U have the same unit and U is bounded from above by Δ, which intuitively seems somewhat plausible.

From Theorem 1, Eq. (10), and the additivity of V, it is easy to show for gains x, y that

$$U(x \oplus y) = U(x) + U(y) - \frac{U(x)U(y)}{\Delta}. \qquad (11)$$

Observe that U is unique up to multiplication by a positive constant. These transformations, it should be noted, are completely distinct[10] from the ratio scale transformations of V. The above result has a converse, but I will omit it here because it is somewhat complex (Luce, 1996).

A Parallel in Physics—So, despite the fact that the operation $\oplus$ has an additive value representation, the representation corresponding to the rank-dependent theory, which includes SEU as a special case, is not additive, but rather has the form of Eq. (11). This situation closely parallels one in physics, namely, the representation of velocity and the concatenation of velocities in the relativistic framework. One way to measure velocity is based on the conjoint structure involving distance and time and that leads to the usual formula for velocity, namely,

$$v = \frac{s}{t},$$

where s is the distance elapsed in the time t of an object in unaccelerated motion at velocity v. Equally well, as Einstein showed, one can build a theory of velocity measurement based on the concatenation $\bigcirc$ of velocities. In the one dimensional case, this forms the positive quadrant of an Archimedean ordered group and so has an additive representation, which is called "rapidity." Rapidity spans the positive real numbers with the rapidity of light, c, mapping to ∞, and it is quite distinct from the usual measure of velocity above. In that measure and for velocities all in the same direction, concatenation has the well known relativistic representation

$$v(a \bigcirc b) = \frac{v(a) + v(b)}{1 + \frac{v(a)v(b)}{v(c)^2}},$$

where c denotes light in a vacuum and so $v(c)$ is the velocity of light in the v−measure. Again, this non-additive representation is bounded by $v(c)$, whereas rapidity is unbounded.

So what we have found in the utility context is far from unprecedented, and it may well be a prototypical example of how bounded measures can arise in the behavioral sciences. One difference is that the upper bound on velocity is realized by light, whereas no valued object seems to achieve the upper bound on utility.

Of course, if U is convex instead of concave, the expression of Eq. (10) is changed to an exponential and U is unbounded.

Segregation for Losses—The definition of segregation for losses is exactly as in Eq. (4) except the assumption now is that $x, y \prec e$. Assuming that U is weakly superadditive, i.e., $U(x \oplus x) > 2U(x)$, for losses, one finds that the representation is for some $\gamma > 0$, $\Gamma > 0$

$$U(X) = \Gamma[e^{\gamma V(x)} - 1],\qquad(12)$$

and so for losses

$$U(x \oplus y) = U(x) + U(y) + \frac{U(x)U(y)}{\Gamma}.\qquad(13)$$

Thus, U is not bounded from below.

It should be noted that if U is concave for losses, the expression changes and it is bounded from below. That bound may be realized for some people, namely, death.

Issues in Estimating Weights—Tversky and Kahneman (1992) used the rank-dependent representation of Eq. (7) for risky gambles to estimate the form of the weighting function for $(x, p; y)$, where p denotes the prescribed probability of the event. They had the subjects provide certainty equivalents $CE(x, p; e)$ of the lottery $(x, p; e)$ and they noted that, because $U(e) = 0$, the rank dependent model exhibits the property called *separability*:

$$U(x, p; e) = U[CE(x, p; e)] = U(x)W(p).\qquad(14)$$

They assumed as a plausible form for U a power function, i.e.,

$$U(x) = \alpha x^\beta,\qquad(15)$$

which with Eq. (14) yields

$$W(p) = \left[\frac{CE(x, p; e)}{x}\right]^\beta.\qquad(16)$$

So, once β has been estimated, W is determined[11]. Their data, so processed, exhibited an inverted S-shaped form with $W(p) > p$ in the region of about (0,0.4) and $W(p) < p$ in the interval (0.4,1). They and others have made much of this form.

Three things about this conclusion make me uneasy. First, suppose segregation, Eq. (4), holds as it seems to empirically; that rank dependence, Eq. (7), holds as Tversky and Kahneman certainly assumed; and that joint receipt of money gains is additive, Eq. (1), as they clearly believed and for which we have some supporting empirical evidence. Then it follows from these assumptions that the correct form for U is not a power function but a negative exponential one, i.e.

$$U(x) = \Delta(1 - e^{-\delta' x}), \quad \delta' = \delta c. \tag{17}$$

The estimation problem is a good deal more complex for this function. Such an attempt is currently being carried out by my graduate student R. Sneddon.

My second source of uneasiness is a very simple rational argument suggesting that W, not U, should be a power function. This prediction follows almost immediately from separability, Eq. (14), coupled with the simplest reduction of compound gambles, namely,

$$((x, p; e), q; e) \sim (x, pq; e). \tag{18}$$

Third, the data fitted are medians over subjects, which is only really justifiable for either linear functions, which these clearly are not, or for identical subjects, which is most unlikely. Indeed, I have shown in a numerical example that the average of two people both having power function weights, one with an exponent less than one and the other greater than one, exhibit an inverse S-shape under their analysis.

These three reasons make me very uncertain about what we can confidently say at this time about the form of the weighting functions.

Extensions to General Finite Gambles—Equation (7) is the rank-dependent representation for binary gambles of gains. An obvious question is how to extend it to arbitrary finite gambles. From 1982 to now, a series of axiomatizations have been given of what is called the rank-dependent or cumulative prospect representation. Quiggin (1993) summarized the efforts of the 1980s. To that we may add the co-monotonicity approach of Wakker (1989) and Wakker and Tversky (1993), the inductive use of an accounting equivalence by Liu (1995) and Luce and Fishburn (1991, 1995), and the unpublished inductive use of the highly rational property of coalescing (Luce, 1998). Starmer and Sugden (1993) claim to have empirically rejected coalescing, which they call "event splitting." Because of peculiarities of the design, I am not convinced. Lack of space does not permit giving the details.

Linking Joint Receipts and Gambles for Mixed Gains and Losses

Associative Joint Receipt of Mixed Consequences—For the associative model, one uncovers what the utility for mixed consequences is by using the additivity of V (Theorem 1) and the two representations of Eqs. (10) and (12). It is fairly straightforward (Luce, 1997) to show

$$U(x \oplus y) = \left(\frac{U(x)}{\Delta} + \frac{U(y)}{\Gamma}\right) \begin{cases} \frac{\Delta}{1+U(y)/\Gamma}, & x \succsim e \succsim y, \ x \oplus y \succsim e \\ \frac{\Gamma}{1-U(x)/\Delta}, & x \succsim e \succsim y, \ x \oplus y \prec e \end{cases}. \tag{19}$$

Note that because $U(y) < 0 < U(x)$ the "correction" factors on the right are both > 1.

Assuming $\oplus$ is additive over money gains and over losses, and so Eq. (2) holds, it is not difficult to show that $\oplus$ over mixed gains and loss is linear, but not simply

additive. In fact, one deduces that for $x \succsim e \succsim y$

$$x \oplus y = \begin{cases} x + \frac{k}{c}y, & x \oplus y \succsim e \\ \frac{c}{k}x + y, & x \oplus y \prec e \end{cases}.$$ (20)

This prediction has not yet been checked empirically.

Extensive-Conjoint Joint Receipt of Mixed Consequences—The model of Luce and Fishburn (1991, 1995) coincides with the above associative model for gains and losses, but for the joint receipt of mixed consequences they postulated it to have a form different from Eq. (19), namely,

$$U(x \oplus y) = U(x) + U(y), \qquad x \succsim e \succsim y,$$ (21)

which certainly is simpler. However, that simplicity is bought at a severe axiomatic price. For the U of Eq. (21) to agree with the U of Eqs. (11) and (13), which was derived for gains and losses separately, a quite complex condition must be satisfied [see Luce (1996), Definition 12 and Theorem 7]. The reason Luce and Fishburn chose this representation will be made clear in the next subsection.

So far, no experiment has been reported that is designed to choose between the associative and the conjoint representation of mixed joint receipt. We will see below that there is some indirect evidence favoring the associative model, but I am not yet confident about the situation.

Duplex Decomposition—Kahneman and Tversky (1979) and Tversky and Kahneman (1992) assumed in both versions of prospect theory that the utility for mixed binary gambles takes on the following *sign-dependent* form:

$$U(x, E; y) = U(x)W^+(E) + U(y)W^-(\overline{E}), \qquad x \succsim e \succsim y,$$ (22)

where W^+ is the weighting function from gains and W^- that from losses.

Observe that by separability, Eq. (14), the right side of Eq. (22) is

$$U(x, E; e) + U(e, E; y).$$

The question I raised in the early 1990s was how one might rationalize this assumption. That, along with Tversky's and Kahneman's informal editing, led me to consider joint receipts as a needed extra primitive. If we assume U to be additive over $\oplus$, as in Eq. (21), then the following testable property leads to Eq. (22).

Definition 3. *Duplex decomposition* is said to hold if for all events C, with independent realizations C', C'', and all consequences x, y, with $x \succsim e \succsim y$,

$$(x, C; y) \sim (x, C'; e) \oplus (e, C''; y).$$ (23)

Of course, this property is decidedly non-rational[12], but so then is Eq. (22) and we must expect something non-rational to give rise to it. It seems plausible to designate this type of condition as "quasi-rational" (Luce & von Winterfeldt, 1994).

Duplex decomposition was first proposed and studied empirically by Slovic and Lichtenstein (1968) who found it to hold. Moreover, they seemed to think of it as a plausible condition. It is the division of labor achieved by focusing separately on gains and losses as in cost-benefit analyses. A more recent study, Cho et al. (1994), has again supported it. On the other hand, Chechile and Cooke (1997) have brought into question *all* linear weighted representations for mixed gambles, in particular Eq. (22). This study is based on establishing for various x, y pairs the probabilities that render $(x, p; y)$ indifferent to a fixed gamble. Without careful side studies, we cannot know whether such judged probability equivalents actually yield choice indifferences. We do know that judged money certainty equivalents do not yield choice indifference. However, the effects they obtained are so massive that it doubtful these biases can account for the results.

Given that there are data supporting Eq. (23) and that the Chechile and Cooke data seem to mean that Eq. (22) does not hold, we are forced to conclude that the additivity, Eq. (21), postulated by Luce and Fishburn for mixed joint receipts must be wrong. So we turn to an alternative hypothesis.

Associative Joint Receipt and Duplex Decomposition—Perhaps the additivity of U over $\oplus$, along with its consequence in the presence of duplex decomposition that Eq. (22) holds, is where we have gone wrong. So, it seems reasonable to ask what the purely associative model together with duplex decomposition implies. For $x \succsim e \succsim y$, Luce (1997) has shown[13]

$$U(x, C; y) = \left(\frac{U(x)}{\Delta} W^+(C) + \frac{U(y)}{\Gamma} W^-(\overline{C}) \right) \begin{cases} \frac{\Delta}{1 + \frac{U(y)}{\Gamma} W^-(\overline{C})}, & (x, C; y) \succsim e \\ \frac{\Gamma}{1 - \frac{U(x)}{\Delta} W^+(C)}, & (x, C; y) \prec e \end{cases}$$

(24)

Clearly, this representation is not linear in U and so it has some potential for fitting the Chechile and Cooke data. According to Chechile (personal communication, July 1996) and as is easily verified it will do so only if certain signs are changed, which in turn is achieved only by assuming that U is convex for gains and concave for losses (in the limited range of values used). It is noteworthy that, of the several models they fit to their data, those that admit either concave or convex utility functions were always estimated to be convex over gains and concave over losses. For those that were restricted to be concave for gains and convex for losses, the estimates approached the linear case, which is as near to convex for gains and concave for losses as one can get in these models.

So far, no one has come up with predictions from Eq. (24) that can be followed up independent of fitting specific models to global data. But one thing is very clear. If Eq. (24) is correct descriptively, then the attempts to estimate utility functions based on Eq. (22) for mixed gambles are certain to lead to confusion. It may be useful to reconsider that approach to utility estimation and verification.

R. Sneddon, using data collected by Cho and her collaborators, is currently attempting to fit and compare Eqs. (22) and (24) by assuming that U is the negative exponential form of Eq. (17). The outcome is not yet known.

Conclusions

This paper attempts to illustrate four major points.

First, an interesting theory of riskless utility can be based upon the ubiquitous operation of joint receipt; it is structurally very like the theory of weight measurement in physics. Moreover, I believe that *a priori* arguments offered against such a theory are usually based on misunderstandings and so should not be taken very seriously.

Second, the issue of the relation between risky and riskless utility is more subtle than has typically been thought; it can only be understood theoretically and experimentally by means of linking laws such as segregation, Eq. (4), and duplex decomposition, Eq. (23). With such linking properties, which have been sustained empirically to a certain level of accuracy, we conclude that risky utility provides a non-additive representation of the joint receipt operation which is necessarily non-linearly related to its additive value representation, Eq. (10). Moreover, unlike the value measure, this one is bounded.

I suspect that what has been done here is typical of what should be done elsewhere in psychology, especially in sensory measurement. To be more specific, if we are measuring attributes of intensity, such as loudness, we can manipulate both the superposition of physical intensity and independently other variables, such as frequency, that also affect loudness. The problem is to discover a suitable trade-off theory for the intensity and frequency factors and a law linking that to superposition as an operation. I have not yet seen how to carry this out.

Third, there are rational properties, different from those that have grown up around SEU, that are at least as compelling as the more familiar ones. The clearest examples are segregation and coalescing (not discussed in detail here). Of course, segregation falls outside the SEU framework, but coalescing does not. These properties appear to be useful in developing a descriptive theory that includes SEU as a special case but that in its general case avoids the descriptively questionable reduction (or accounting) equivalences.

Finally, I note that Ward Edwards (1992, pp.. 255-256) was not optimistic about our finding a descriptive theory that includes SEU as a special case despite the fact he remarks on p. 259 that "People do not maximize SEU, but they come close—close enough so that models intended to be descriptive must inevitably resemble SEU." I continue to feel that descriptive models should not wholly exclude the possibility of modeling rational actors in the same general framework. And this is true of the models just described.

Acknowledgement to Ward Edwards

Author Notes

Notes

[1] This "riskless" and "risky" terminology is common, and I will use it, but it would be more accurate to distinguish between "certain" and "uncertain" alternatives.

[2] For example, Arrow (1951, p. 425) remarked "First, the utilities assigned are not in any sense to be interpreted as some intrinsic amount of good in the outcome (which is a meaningless concept in any case)." I think some have interpreted this to mean, incorrectly in my opinion, that riskless utility is meaningless. The word "intrinsic" is key to Arrow's assertion, and the assertion is equally true for mass or length measurement. What makes physical measurement work is not intrinsic measures but relative ones, and the relative "amount of good" in two riskless outcomes is far from meaningless.

[3] I thank Barbara Mellers for suggesting this type of example.

[4] We modified for gambles a well-known sequential procedure from psychophysics called PEST. Basically, it involves homing in on the certainty equivalent of a gamble by a computer controlled series of choices between the gamble and amounts of money. Depending on whether the gamble or money is selected, the latter is increased or decreased on the next presentation of that gamble—which is separated by many trials involving other gambles. Each time the direction is reversed, the magnitude of the change is reduced.

[5] For many situations where two CEs are compared to establish an equivalence, it appears that the biases pretty much cancel out. However, as was true in Cho and Luce (1995), if there are two CEs on one side and only one on the other, then the bias can generate trouble.

[6] Technically, such a structure is called an *Archimedean ordered group*.

[7] In reality, one actually derives 8 from the other axioms as one step in the proof of Theorem 1.

[8] Luce and von Winterfeldt (1994) classed segregation along with duplex decomposition, below, as "quasi-rational" properties in part to distinguish them from the classical rationality axioms. I now consider this to have been a mistake for segregation which, after all, is fully rational in the sense that both sides have the same "bottom lines."

[9] In Luce (1996) I called this property *weak concavity*, but that really is misleading. Concavity implies subadditivity: $U(x \oplus y) < U(x) + U(y)$. And the present property is simply subadditivity with $y = x$.

[10] Technically, the ratio scale transformations of V correspond to the automorphisms

of the joint receipt structure on money whereas those of U correspond to the automorphisms of the gambling structure.

[11] The functions plotted in Fig. 3 of their paper, which are labeled $W(p)$, are actually $CE(x,p;e)/x$. The correct W plots are provided in Fig. 1 of Tversky and Fox (1995).

[12] The outcomes $x \oplus y$ and $e \oplus e \sim e$, which can arise on the right, do not arise on the left, only x or y but not both.

[13] There is a typographical error in Luce (1997) in Eq. (16b). In the denominator, the sign before $\frac{U(x)}{\Delta}$ should be $-$.

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