Choice, Decision, and Measurement
A. A. J. Marley (Ed.),

THE PAST SEVEN YEARS: 1988-95

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ABSTRACT. In 1989 my then scientific autobiography (Luce, 1989) concluded: "At that point [early 1988] I did take early retirement from Harvard, but the next five to ten years promise not to be idle." These few pages take up the ensuing seven years, which indeed have not been idle. (Perhaps the problem is that I've never taken up golf.) For example, 32 papers have appeared in journals and book chapters, half a dozen are in various pre-publication stages, and three books have appeared: the much belated volumes II and III of the *Foundations of Measurement* (Luce, Krantz, Suppes, & Tversky, 1990; Suppes, Krantz, Luce, & Tversky, 1989) and a textbook, based on the core course given at Harvard and repeated several times at UCI, *Sound & Hearing* (Luce, 1993). Although favorably reviewed, it has not spawned the courses I had hoped it might.

1. RESEARCH THEMES

During the period from 1975 through the late 1980's roughly half of my work centered on the representational (or axiomatic or algebraic) theory of measurement. Although we are far from completing that program of research - witness the list of 15 open problems suggested in Luce and Narens (1994) - much of my research energy has shifted from the abstract themes to more concrete attempts to apply what we have learned about measurement theory to empirically interesting problems. Several things lay behind the shift: criticism of representational measurement as seemingly irrelevant to substantive issues, e.g. Cliff (1992), but also see Narens and Luce (1993), my own scientific disposition to develop theory that makes empirical predictions and to test these, and perhaps some attenuation of my abstract skills which is said to occur with age - although occasionally it seems to be less of an issue than is commonly believed, witness, e.g., Patrick Suppes. In any event, beginning with Luce (1988), I began participating in the rather lively developments in our understanding of how to measure utility.

Key words and phrases. Scale type, joint receipt, rank-dependent utility, sign-dependent utility, relation of riskless and risky utility, weighting functions.

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Applications of Measurement Theory to Utility Theory. Much recent economic literature on individual decision making under risk[1] and under uncertainty has focused on how to adapt the subjective expected utility (SEU) model of Savage (1954) to some of the empirical findings that seem to undermine the basic tenets of rationality embodied in that model. Two main avenues have been pursued. One, mostly followed by economists and well summarized by one of its originators J. Quiggin (Quiggin, 1993), focused on weakening what economists call the independence axiom, which is explicated below, and deriving from modifications of that axiom and the other axioms new forms of the numerical representation. The other tack, mostly pursued by psychologists, involved directly modifying the SEU representation to accommodate some of the empirical anomalies. The most important example of this approach was D. Kahneman and A. Tversky's prospect theory (Kahneman & Tversky, 1979) which, beyond doubt, has been more widely cited than any other paper in this area in the past few decades. It seemed to me that both approaches had significant difficulties, and part of my effort was to try to overcome them.

Economists often begin by restricting the domain of alternatives to lotteries: money consequences with known probabilities. They interpret such lotteries as random variables with probability distributions over money. That seems innocent enough, a mere mathematical notation for a lottery. But, empirically, it isn't innocent at all for the following simple reason. Suppose $\mathbf{X}$ and $\mathbf{Y}$ are random variables and λ is a probability, i.e., a number between 0 and 1. Then

$$\mathbf{Z} = \lambda\mathbf{X} + (1-\lambda)\mathbf{Y}$$

is the mixture random variable having the distribution

$$\Pr[\mathbf{Z} = z] = \lambda \Pr[\mathbf{X} = z] + (1-\lambda) \Pr[\mathbf{Y} = z].$$

The mathematics of random variable theory automatically reduces any compound mixture lottery to its equivalent first order form. No distinction is made among compound random variables having the same bottom line. It is unlikely that people treat them as the same, and substantial empirical data support differential treatment except for the simplest reductions. Nonetheless, that strong assumption is built into the meaning of random variables.

The problem is illustrated vividly by the following example of consequence monotonicity and its reduced form in the case of random variable representations. Suppose g, g', and h are lotteries (not interpreted as random variables) and λ, $0 < \lambda < 1$, is a probability. Then a compound lottery can be formed as $(g, \lambda; h)$, meaning that when the chance experiment underlying λ is run, one receives g with probability λ and h with probability $1 - \lambda$. Let $g \succsim h$ denote a weak preference ordering, where $g \succ h$ denotes that g is strictly preferred to $h, g \sim h$ denotes that g and h are indifferent, and $\succsim = \succ \cup \sim$. A major assumption of rational behavior

[1] *Risk* is the term used when the consequences in the gamble have known probabilities of arising, whereas *uncertainty* refers to cases involving chance events for which their probabilities are either not known or appear to be inherently unknowable. *Riskless* choice concerns cases where chance plays no role in the consequence received.

in many theories, called *consequence monotonicity*, is:

$$g' \succsim g \text{ if and only if } (g', \lambda; h) \succsim (g, \lambda; h). \tag{1}$$

In words, replacing one consequence by another that is viewed as better improves matters. Now, suppose we think of g, g', and h as random variables, say $\mathbf{X}$, $\mathbf{X}'$, and $\mathbf{Y}$, respectively. Then in the usual random variable notation we write the above condition as:

$$\mathbf{X}' \succsim \mathbf{X} \text{ if and only if } \lambda\mathbf{X}' + (1-\lambda)\mathbf{Z} \succsim \lambda\mathbf{X} + (1-\lambda)\mathbf{Z}. \tag{2}$$

This property is called the *independence axiom*. The problem is that although it seems equivalent to consequence monotonicity, it really is not because it also assumes that one can reduce the compound form to its equivalent first order form. No such property holds for consequence monotonicity unless it is explicitly assumed.

Early in the history of utility theory for lotteries, M. Allais (Allais, 1953) described a compelling thought experiment where people violate independence; subsequently this and variants on it were confirmed experimentally (for summaries of much empirical work concerning the SEU model, see Schoemaker, 1982, 1990). Despite the fact that others, e.g. Brothers (1990) and Kahneman and Tversky (1979), provided evidence that, when the compound lotteries are presented without reduction to their first order form, consequence monotonicity holds, economists have continued to model alternatives as random variables and attempted to weaken the independence property. It seemed obvious to me that one should be very cautious, indeed, about invoking the reduction property of random variables, in which case it might be possible to retain the highly rational property of consequence monotonicity[2]. That observation has been one of three foundation stones of my work.

A second foundation stone, hardly original, is that the distinction between gains and losses matters greatly. The economist H. Markowitz (Markowitz, 1952) and the psychologist W. Edwards (Edwards, 1962) were among the first to emphasize that fact and to point out that the extant theories ignored it. But once again the paper that made a real difference on this score was Kahneman and Tversky (1979). The issue of how gains and losses are to be defined is still far from resolved. In experiments we typically treat no exchange of money to define the status quo, and any addition to the status quo is a gain and any reduction from it is a loss. But we are acutely aware that this is inadequate. For a person desperately in need of \$100, a "net win" of \$50 in an evening at a casino may functionally seem more like a loss than a gain. More commonly, a choice set of gambles may define a temporary status quo somewhere between the smallest and largest possible consequences.

[2] This is not the place to go into much detail, but it should be mentioned that M. H. Birnbaum and B. A. Mellers (Birnbaum, 1992b; Mellers, Weiss, & Birnbaum, 1992b) have argued from their data that consequence monotonicity fails when one of the consequences is no exchange of money and the consequences are evaluated in terms of a judged monetary certainty equivalence - the sum of money the person judges to be indifferent to the gamble. von Winterfeldt, Chung, Luce, and Cho (1997) questioned their use of judged certainty equivalents and provided choice-based data that cast in some doubt the conclusion that monotonicity fails in these cases. Various sources of evidence should make one very wary of assuming judged certainty equivalents are really the same as monetary indifferences to gambles.

So the problem has been partitioned into developing theories in which the status quo is assumed to exist and gains and losses are carefully distinguished and in working out theories to describe how the status quo or reference levels are constructed. The former theories are by now rather well developed. But, for all practical purposes, no work on the latter has begun in any serious way. I have been unable to come up with a useful empirical way to estimate reference levels and, beyond what is in Luce, Mellers, and Chang (1993), I know of no theoretical proposals. L. L. Lopes has studied experimentally the impact of various distributions of money on reference levels (Lopes, 1984, 1987).

The third foundation stone is the observation that utility can be constructed for riskless consequences[3] using a binary operation rather than studying the tradeoff between consequences and chance, which has been the basis of all theories of weighted or expected utilities. The operation, which I have called *joint receipt*, is the simplest thing in the world. You often receive two or more things at once: checks and bills in the mail, gifts on birthdays and holidays, purchases when shopping, etc. The key fact is that if x and y are valued objects, then their joint receipt, which I denote by $x \oplus y$, is also a valued "object." This basis for measurement is much like that underlying the measurement of mass, and indeed the pan balance analogy seems quite close.

These are the ideas that I have been able to pursue with some success. The details are far too complex to cover here in detail, but the main outlines are describable. Let e denote the status quo and consider for the moment just gains, i.e., consequences x such that $x \succsim e$. We assume, in all cases, that $e \sim (e, E; e)$ and that the utility function U has the property $U(e) = 0$, where e is the status quo. (In many cases this is forced by assumed properties.) Observe that we have potentially four distinct ways to construct such a utility function:

(1) The first measure is based on binary gambles of gains. Much of the recent literature, including experimental papers, suggests that a *rank-dependent weighted average* form works in which the weight assigned to an event depends not only on that event, as in SEU, but also on the rank order position of its consequence among all of the consequences of that gamble. In the binary case, where $(x, E; y)$ means a chance device is run and x is received if the event E occurs and y is received if E fails to occur, the utility for gains x, y ($\succsim e$) has the rank-dependent form

$$U_1(x, E; y) = \begin{cases} U_1(x)W_1^+(E) + U_1(y)[1 - W_1^+(E)] & \text{if } x \succ y \\ U_1(x) & \text{if } x \sim y \\ U_1(x)[1 - W_1^+(\neg E)] + U_1(y)W_1^+(\neg E) & \text{if } y \succ x, \end{cases} \tag{3}$$

where $\neg E$ denotes the complement of E relative to the chance device. P. P. Wakker has provided an axiomatization (Wakker, 1989) of this form which basically rests

[3]Such a riskless utility measure can be extended to gambles by finding their certainty equivalents, the sum of money perceived by the subject as indifferent to the gamble. But as Cho and Luce (1995) showed empirically, this may not be without difficulties for it appears that the joint receipt of two gambles may not be indifferent to the joint receipt of their respective certainty equivalents. We do not yet know whether this is a significant finding or simply evidence that our estimates of certainty equivalents are biased, which I suspect can occur for skewed psychometric functions and/or skewed gambles. This possibility is under investigation.

on assuming the usual SEU-type axioms separately for the two regions of $x \succ y$ and $y \succ x$.

Generalizations of the rank-dependent form are found in many articles, many of which are summarized in Quiggin (1993). Others include Liu (1995), Luce and Fishburn (1991, 1995), and Tversky and Kahneman (1992). The treatment by Liu is especially economical.

(2) The second measure is also based on binary gambles, but now of a gain and a loss. Here two different weighting functions arise, one when the consequence attached to an event is a gain and a different one when it is a loss:

$$U_2(x^+, E; y^-) = U_2(x^+)W_2^+(E) + U_2(y^-)W_2^-(\neg E) \tag{4}$$

Because in general $W_2(E) + W_2(\neg E) \neq 1$ and $e \sim (e, E; e)$, it follows immediately that Equation 4 forces $U(e) = 0$. The form in Equation 4 was first postulated in Kahneman and Tversky (1979). It, together with a generalization of Equation 3 to gambles with finitely many consequences, has been axiomatized in Luce and Fishburn (1991, 1995), and Wakker and Tversky (1993). The trick used in the latter paper is basically to assume the SEU axioms hold in the regions defined by gains and losses and by order among the consequences. The former one builds the axiom system in terms of joint receipts (see below).

Some of the properties of binary systems with $U_1 = U_2$ are discussed in Luce and von Winterfeldt (1994). Of course, an obvious question is when does $U_1 = U_2$ and $W_1^+ = W_2^+$? This is easily seen to come down to consequence monotonicity of the gambles.

There is another property of binary gambles, which is appreciably weaker than Equations 3 and 4, that plays an important role. A utility function U is said to be *separable* if there exist functions $W^{(k)}, k = +, -$, over events such that

$$U(x, E; e) = U(x)W^{(k)}(E), \quad k = \begin{cases} + & \text{if } x \succsim e, \\ - & \text{if } x \precsim e. \end{cases} \tag{5}$$

If $\mathcal{C}$ denotes the set of consequences and $\mathcal{E}$ the set of events, Equation 5 means that $\langle \mathcal{C} \times \mathcal{E}, \succsim \rangle$ satisfies the axioms of additive conjoint measurement (Krantz, Luce, Suppes, & Tversky, 1971, see Chapter 3). Tversky (1967) provided a direct test of this representation and found it held; however, W was not finitely additive, which of course was inconsistent with SEU. The key axioms of additive conjoint measurement are monotonicity and the Thomsen condition. The latter can be shown to be equivalent to the empirically testable condition called *status-quo event commutativity*[4]:

$$((x, E; e), D; e) \sim ((x, D; e), E; e)) \tag{6}$$

The empirical literature on this property is somewhat mixed. Using a forced choice procedure between the two sides, Ronen (1971, 1973) showed a pronounced preference for the side in which the first event is more probable. Since the choice is forced, the only distinction available is the order of events: apparently most subjects agreed to resolve the quandary in the same way. Results of Brothers (1990) are complex, but using choice-based certainty equivalents, Brothers found some

[4]It is called just *event commutativity* if Equation 6 holds for an arbitrary y in place of e.

support for Equation 6. And pursuing the same approach more carefully, Chung, von Winterfeldt, and Luce (1994) found support for it in 22 of 25 subjects. One advantage of using certainty equivalents is that one avoids forcing subjects to make a choice.

(3) A quite different way to measure utility of gains is based on the ordering and joint receipt of just gains. Assuming that joint receipt, denoted by $\oplus$, is commutative, Luce and Fishburn (1991, 1995) have modeled joint receipts of gains as an extensive structure – the same type of structure as underlies much physical measurement such as mass, length, and charge. This is surely the case if, for money consequences, it is true that $x \oplus y = x + y$, as Tversky and Kahneman (1992) claimed and as Cho and Luce (1995) established experimentally for gains and losses separately. However, Thaler (1985), using a classroom questionnaire, found evidence that additivity of money held for losses but not for gains. I do not really understand the inconsistency in these empirical results, although I suspect that the scenarios used in Thaler (1985) carry a lot of extraneous meaning beyond the simple concept of joint receipt.

Although the assumption of an extensive structure means there is an additive numerical representation, we do not take that to be the utility measure, but rather a representation of the form

$$U_3(x \oplus y) = U_3(x) + U_3(y) - U_3(x)U_3(y)/C, \qquad C > 0 \tag{7}$$

where $0 \leq U_3(x) \leq C$ follows from monotonicity of $\oplus$. It may seem odd to use a bounded, non-additive representation when an additive one exists, but the reason is that the non-additive one is compatible with other measures of utility and the additive one is not. Note that by assuming $e \sim e \oplus e, U(e) = 0$ is forced by Equation 7.

(4) The fourth and last measure is based on the trade-off of joint receipts between gains and losses. Let x^+ denote a gain and y^- a loss. Then we assume that the underlying operation has the properties of an additive conjoint structure and so has a representation

$$U_4(x^+ \oplus y^-) = U_4(x^+) + U_4(y^-). \tag{8}$$

As noted earlier, there is a well-known axiomatization for such a representation.

Some interesting properties of joint receipt coupled with certainty equivalents of gambles are described in Luce (1995b) and tested in Cho and Luce (1995).

The basic theoretical questions my colleagues and I have worked on are: What properties lead to the various possible equalities among the measures: $U_1 = U_3, U_2 = U_4$, and $U_3 = U_4$? And how do these properties fare empirically? Of course, completely analogous questions arise for the utility of losses, and the same empirical questions have to be explored. The mathematics for gains and losses is essentially the same, except for parameter differences, and so it need not be duplicated, but of course we cannot take for granted that gains and losses exhibit the same properties empirically. To outline fully all of the properties and what is known about them would take far too much space. An example concerns the relation of U_1 and U_3.

The following empirical property is called *segregation*: For gains x and y,

$$(x, E; e) \oplus y \sim (x \oplus y, E; y). \tag{9}$$

Note the highly rational character of this reduction condition. On both sides it says that $x \oplus y$ is the consequence if E occurs and $e \oplus y \sim y$ if E does not occur. Empirical evidence supporting segregation is provided by both Cho and Luce (1995) and Cho, Luce, and von Winterfeldt (1994).

Segregation was implicitly invoked in Kahneman and Tversky (1979), and Pfanzagl (1959) explored its implications without any constraint on the domain of consequences, leading to a result too strong for our purposes.

Consider now the following properties:

(a) Segregation (Equation 9).
(b) U_1 satisfies Equation 3 and weak subadditivity $U_1(x \oplus x) \leq 2U_1(x)$.
(c) U_3 is separable (Equation 5) and satisfies the representation of Equation 7.

Luce and Fishburn (1991, 1995) have shown that any two imply the third with $U_1 = U_3$. The fact that (a) and (b) imply Equation 7 is, of course, a good reason for using the non-additive representation rather than the additive one. This is analogous to the fact that physicists use a non-additive representation of relativistic velocity despite the fact that an additive one exists (in which the "speed" of light is ∞). They do this for several reasons, one being that a defining property of velocity (namely, velocity equals distance travelled divided by time taken) holds for the non-additive representation but not the additive one.

One feature of the above result is that (a) plus (c) provides an axiomatization of the rank-dependent form that does not presuppose knowing that rank dependence will arise. But as an axiomatization it has the major weakness that U is assumed to be separable as well as satisfying Equation 7. Why should this be? I show in Luce (1996) that if U is separable and satisfies Equation 7, then the following property holds: For any gains x, y, and event E, there is an event $D = D(x, E)$ such that

$$(x \oplus y, E; e) \sim (x, E; e) \oplus (y, D; e) \tag{10}$$

Moreover, using mathematical results of Aczél, Luce, and Maksa (1996), I have shown[5] that if Equation 10 holds and if there are separable utility functions and also ones that satisfy Equation 7 and a pair is related by a function that along with its inverse is differentiable, then there are ones that are both separable and satisfy Equation 7.

The case of separable utility for joint receipt of mixed consequences results in a condition similar to Equation 10.

Neither of these behavioral properties has yet been explored empirically, and it may prove difficult to do so because of their existential character.

The major linking property between U_2 and U_4, analogous to segregation, is called *duplex decomposition*:

$$(x, E; y) \sim (x, E', e) \oplus (e, E''; y) \tag{11}$$

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[5]As stated, this is misleading. I reduced this and two other problems like it to solving functional equations. After some very partial progress, I posed them to J. Aczél and after some e-mail and FAX correspondence he and Gy. Maksa did solve them.

where E' and E'' mean that E arises in two independent realizations of the underlying chance experiment. Slovic and Lichtenstein (1968) first noted that this property was sustained empirically, and Cho et al. (1994) reconfirmed that it held using somewhat different methods. Note that it is a decidedly non-rational property: on the left either x^+ or y^- but not both must arise whereas on the right there are additional possibilities, $x^+ \oplus y^-$ and $e \oplus e \sim e$. Nevertheless it seems to hold. It is worth emphasizing that this is the sole source of non-rationality, and so deviation from SEU, in this entire complex of ideas.

Representational Theory of Measurement. Although utility has been my greatest focus during this period, general measurement issues have not been completely abandoned. First, Luce (1992a) presented a general theory of ordered structures with a monotonic[6] operation of order n and with finitely many singular points, where an element is singular if it is fixed under all automorphisms[7] of the structure. If the structure is homogeneous[8] between adjacent singular points, then one can show that there are most three singular points: a minimum, a maximum, and an interior one. Some of their properties were established, and a numerical representation was developed by patching together ones from the theory with no singular points (Alper, 1987; Narens, 1981a, 1981b). This work was heavily motivated by the models that had arisen in utility theory, and it offers a possible generalization of the linear weighted forms should they prove inadequate.

Second, a major finding in the representational theory of measurement was the formulation and partial results of Narens (1981a, 1981b), later completed in Alper (1987), to the effect that when a structure on the real continuum is homogeneous and finitely unique[9], then it has a numerical representation in which the admissible scale types lie between ratio (i.e., similarity) transformations and interval (i.e., positive affine) transformations. I have considered the question of whether one can dispense with the real continuum and replace it by Archimedean assumptions. With the help of T. M. Alper, this has been done. Let a *translation* be any automorphism with no fixed point and a *dilation* be one with at least one fixed point. The conditions are that, for the asymptotic order on the group of automorphisms, the order is connected, the set of translations is Archimedean relative to itself, the set of dilations is Archimedean relative to all automorphisms, and the structure is homogeneous. I had hoped that, as is true for theories of additive structures, the continuum case could be shown to be a special case of the Archimedean one, but so far this proof has eluded us.

Third, given our better understanding of measurement structures I reopened the issue I first approached in Luce (1959b), namely why are psychophysical matching functions of various sorts so often power functions. The trouble with the 1959

[6]The definition of monotonicity requires some delicacy at singular points.

[7]An *automorphism* is an isomorphism of the structure onto itself or what physicists refer to as a *symmetry*.

[8]Homogeneity means, intuitively, that elements cannot be distinguished by their properties. Formally, it is defined as follows: For any two elements x and y between a pair of adjacent singular points, there is an automorphism of the structure that takes x into y.

[9]A structure is finitely unique if there is an integer N such that whenever an automorphism has N fixed points it must be the identity.

paper, and more generally of many presentations of dimensional analysis, is a difficulty in distinguishing at the representational level between a change of units and a translation of the stimuli. These are far from the same thing – the one being a systematic change of stimuli and the other a systematic change of notation – and yet in ratio scale representations both appear at the representational level simply as multiplication by positive constants.[10] In Luce (1990b) I showed that if a matching relation exhibited what I called *translation consistency*, which was formulated entirely in terms of translations of the two physical continua involved, then a power function had to hold in the physical ratio scale representations. This result has been subsumed in the much deeper work that Narens reports in a chapter of this volume.

2. PERSONS, PLACES, AND EVENTS

Mathematical Behavioral Sciences at University of California, Irvine (UCI). From UCI's perspective, my major role in coming was to bring some structure to the existing interdisciplinary strength in mathematical modeling in the School of Social Sciences, to help augment it with new appointments, and to achieve greater national and international recognition. This effort began with the creation in 1988 of the Irvine Research Unit (IRU) in Mathematical Behavioral Sciences of which I was appointed director. These IRUs are five year creations of the campus that either sunset or are converted, via a somewhat tortuous process, to Organized Research Units (ORU) of the University of California, which are pretty much assured a 15 year or greater lifetime. A great many of them are focused on fairly explicit research topics, but some, like ours, are more generic. We went through the hurdles and became an ORU in 1992 with the name Institute for Mathematical Behavioral Sciences.

The basic organization of the Institute is into five distinct (but somewhat overlapping) subgroups:

Axiomatic measurement theory and foundational issues.
Statistical modeling.
Individual decision making.
Perception and psychophysics.
Social and economic phenomena:
  social networks,
  public choice,
  macroeconomics and game theory.

We do not cover the important area of complex, adaptive systems. There is some work of this type on campus – in computer science, engineering, and psychobiology – but we have no such focus of strength in the Institute. This lack is one we have so far failed to overcome, due in part to budgetary problems discussed below and

[10] This is not always the case. For example, Equation 7 is invariant under a change of unit – multiplication by a positive constant – provided the dimensional constant C is also changed in this way. But this transformation does not correspond to an automorphism of the underlying operation which for this representation is of the form $U \to U' = C[1-(1-U/C)^k]$ for some $k > 0$. An analogous situation holds for relativistic velocity.

in part to the difficulty of beginning something new of this degree of popularity by making one appointment at a time.

An IRU or an ORU lies on one dimension of a matrix organization in which the other dimension is departments. Almost all appointments are in departments, which of course carry much of the burden of teaching, especially at the undergraduate level. Some teaching, mostly at the graduate level, arises from the existence of interdisciplinary Ph.D. programs. We proposed one in mathematical behavioral sciences, with membership nearly co-extensive to that of the Institute (which technically supports only research - as if teaching and research are clearly distinguishable at the graduate level), and it was approved in 1989. To get this started, we applied for and received in 1990 a five year NSF Research and Training grant.

One consequence of this matrix structure is that I report jointly to the Vice Chancellor for Research, who provides the budget, and the Dean of Social Sciences, who provides all sorts of support, including space, and who controls the allocation of faculty positions. I have been very fortunate to have extremely cordial relations with William Schonfeld, our dean. He is not the least bit quantitative - except for being a master of budgets - or mathematical, and yet he has been an enthusiastic supporter of the Institute. During the past seven years we have had three Vice Chancellors for Research. The position is currently held by an applied mathematician, Frederick Wan, who has some understanding of and, I believe, sympathy for what we do.

The major activities of the Institute have been a colloquium series (about 25 per year), 7 conferences (mostly in the summer) in various areas of mathematical behavioral sciences, partial support of visiting scholars, and the development and maintenance of a somewhat elaborate computer system. In addition, during the period of the training grant, we had a number of Institute postdoctoral fellows.

All of these activities were initiated in an era when the campus, and the whole UC system, was planning a then demographically plausible 40% expansion by the year 2005. We were hiring at all levels and some of those added strength to the Institute, for example, Chew Soo-Hong and Stergios Skaperdas in Economics and Barbara Dosher, Jean-Claude Falmagne, and George Sperling in Cognitive Sciences. No one at the time anticipated the collapse of the Soviet Union and the resulting massive cut back of jobs in California defense industries, which was a major factor in sharp reductions in state funds to UC – roughly a drop of 25%. Another major contributing factor was ballot initiatives, passed by the voters, that have long-term adverse consequences for the UC system by shifting the balance of funds away from higher education into K-12 education and into the criminal justice system ("Three strikes and you're out"). As this transfer increases, per force UC is increasingly becoming a private institution.

It is unclear to what degree UC, and UCI in particular, will be altered in accomplishing the necessary adjustment. The impacts of these changes and, more generally, of somewhat similar national trends are already significant. I will cite several that concern us. Until academic year 1995-96, faculty growth was decidedly attenuated and, what growth there was, was mostly at the junior level. The applicant pool of graduate students, except for foreign ones, seems reduced, especially in areas involving mathematics. And academic job placement is difficult.

This seems to be a significant problem for students with an interdisciplinary bent because departments seem to be narrowing their disciplinary foci.

One impact, personally favorable, was that UC reacted to the budget cuts by offering generous "early retirement" to senior faculty, thus transferring them from the operating budget to the apparently over-funded retirement fund, and replacing them by less expensive junior faculty. Once retired, I and others were recalled to continue in various capacities, in my case, to continue directing the Institute, to teach some graduate seminars, and to continue supervising the four (now three after one received a Ph.D. in December of 1995) graduate students working with me. This strongly appealed to me because increasingly I found myself out of touch with the undergraduates. Nominally I am 49% time; functionally, full time.

National Scene. During this period I continued to participate in several national activities: 1987-90 as a member of the Executive Committee of the Society for Judgment and Decision Making; 1988-91, President of the Federation of Behavioral, Psychological, and Cognitive Sciences; 1989-91 on the Board of Directors of the American Psychological Society; 1992-94 as a member of the National Research Council's Committee on National Needs for Biomedical and Behavioral Research; 1993-95, Outside Visiting Committee for the Beckman Institute, University of Illinois; 1993-95 on the American Psychological Association's Board of Scientific Affairs; and 1994-97 on the NRC's Board for the Mathematical Sciences. Much of this effort has focused on creating and managing subgroups that worked on various intellectual problems having some sort of widespread implications. At times I found the lack of content and the political battles of various constituencies frustrating, but that seems to be in the nature of this kind of service.

Personal. Despite some of the problems arising from the UC cutback and some background anxiety about earthquakes, I have been very pleased by our move to southern California. I greatly enjoy the kind of outdoor living that is possible year round. Even in winter, unless it is raining, Carolyn, our cat Heidi[11], and I regularly lunch out of doors. Our garden and patio, although of modest size, has an expansive view of over 180 degrees looking out over all of Orange and (on clear days) Los Angeles counties and to the mountain range including Mount Baldy, some 80 miles away. Despite the garden's small size, it somehow manages to absorb more of our time tending and revising it than we had expected. Perhaps that is good given that gardening is my sole form of exercise.

I had been somewhat apprehensive about how Carolyn, a tried and true New Englander, would take to life here. After some months of adjustment, she began to take a lively interest in the incredibly varied landscape of California and the southwest. We quite regularly take automobile trips to various places, sometimes staying a week in one place and venturing forth from there. This has been fostered by our buying a time share condominium in Palm Springs. We never use it ourselves, but bank it for exchange with similar condominia located in spots we would like to visit - Kauai, Flagstaff, Santa Fe, and the like.

[11] Her name is a terrible pun on the fact that she, the shyest cat I've ever known, hides when anyone new comes into the house.

Since 1991 it has been marvelous to have my daughter, Aurora, in California, first at Marymount College in Palo Verdes and then St. Mary's College in Moraga. We have seen a good deal more of her than when she was in Brazil and, of course, she has become thoroughly acculturated.

A number of honors have been bestowed on me. In 1994 UCI awarded me a Distinguished Lectureship for Research and I was elected to the American Philosophical Society. That summer a conference was held in Keil, Germany, which jointly honored the memory of Herman von Helmholtz and me – hardly comparable contributors, but flattering. 1995 was my 70th year – a chronological fact decidedly at variance with my internal self image. In May, Carolyn hosted a lovely birthday luncheon of friends from the area. In August, UCI, in conjunction with the Society for Mathematical Psychology, honored me with a symposium and banquet at which some past students, A. A. J. Marley and Elke Weber, and some old professional colleagues, Richard Atkinson, David Green, William McGill, and Patrick Suppes, spoke, each in his or her own way reminding me of past associations and work. Jack Yellott of UCI served as the suave master of ceremonies. The present volume arose, in part, from this symposium. And in September, the European Mathematical Psychology Group, meeting in Regensberg, Germany also held a symposium and banquet in my honor, both arranged by Jan Drössler, and with charming remarks at the banquet by Edward (Eddy) Roskam. Although flattering and a source of lovely memories, all in all I'm happy this phase is over. Shy people become uneasy with such attention.