R. DUNCAN LUCE

SUPPES' CONTRIBUTIONS TO THE THEORY OF MEASUREMENT

A Philosophy of Measurement

More than any other living person, Suppes has affected contemporary presentations of theories of measurement. They bear the imprint of his views as to the appropriate axiomatic formulation of the intended empirical information and the nature of the theorems to be proved. I do not mean to imply that before him scientists were unaware of what needed to be done, but rather that he has stated the requirements more generally and more forcefully than others had. In essence, he formulated more clearly than anyone before him the common features of existing measurement theories – primarily those of Helmholtz (1887), Hölder (1901), Campbell (1920, 1928), Wiener (1921), von Neumann and Morgenstern (1947), and Savage (1954), as well as his own contributions of the 1950's – and he emphasized various relevant logical distinctions. Of course, philosophers of physics had earlier discussed the nature of measurement, especially important being the works of Bridgman (1922), Campbell (1920, 1928), and Cohen and Nagel (1934), but none had achieved a fully satisfactory mathematical treatment.

The first systematic statement of his views is found in the Suppes and Zinnes (1963) expository chapter in the *Handbook of Mathematical Psychology*, and it pervades the jointly authored volume *Foundations of Measurement* (Krantz, Luce, Suppes, and Tversky, Vol. I, 1971, Vol. II, in preparation). That it has been accepted by others is evidenced both by the nature of the articles that have appeared in the 1960's and 70's and by other expository works such as Pfanzagl's *Theory of Measurement* (1968).

The key elements of Suppes' approach to measurement can be briefly coded by these words: relational structures, representation and uniqueness theorems, invariance and meaningfulness, logical status of axioms, and finiteness.

Bogdan, R.J. (Ed.) 'Patrick Suppes' 93–110.

Relational Structures

It used to be somewhat vague in discussions of measurement exactly what were the empirical objects and observations to which numbers would be assigned, exactly what part of the number system would be employed, and exactly which numerical operations were representing which empirical operations. To a degree, these choices were clarified by discussion and example, but they were never so clear as in the ruthlessly formal, set-theoretic approach demanded by Suppes of himself, his students, and his collaborators (as one of the latter, I know whereof I speak). For Suppes, a qualitative structure is always a relational structure, i. e., of the form

$$\mathscr{A} = \langle A, S_1, \ldots, S_n \rangle,$$

where A is a set and each S_i, $i = 1, \ldots, n$, is a relation of some order k_i on A. (In practice k_i mostly is either 2, 3, or 4, corresponding to binary relations such as orderings, ternary relations such as operations, and quaternary relations of orderings of differences.) Because of the intended interpretation of an empirical structure, namely as something to be measured in terms of a concept of greater than, it is always the case that one of the relations – conventionally, S_1 – is some sort of an ordering relation and is usually written $\gtrsim$. Most often it is a weak order – connected and transitive – but sometimes something weaker is involved – quasiorder, interval order, semiorder, etc. In practice, of course, the set A must be identified with concrete objects or events, but so far as the theories are concerned A is treated as an abstract set and whatever structure it has empirically is embodied in the relations defined over A and the axioms they satisfy.

Equally well, the domain of numerical measures is interpreted as a numerical relational system

$$\mathscr{R} = \langle R, T_1, \ldots, T_n \rangle,$$

where R is a subset of the real numbers (or possibly something somewhat richer, such as a set of nonstandard reals) and the T_i are each relations of the same order, k_i, as the corresponding S_i. Again, because of the intended interpretation, T_1 is very special: it is $\geqslant$ or some modification thereof.

Representation and Uniqueness Theorems

The goal of the measurement theorist is to formulate constraints – axioms

– on the relations of the empirical structures fulfilling two requirements. First, each axiom should either be an (approximately) true empirical statement – a law of the most primitive sort – or it should be a technical constraint of one sort or another which we choose to accept for convenience. Second, the collection of axioms should be sufficient (and necessary if at all possible) for the existence of a homomorphism from the empirical relational structure to the numerical one. To be quite explicit, one requires axioms sufficient to prove the existence of a function ϕ from A into the set of numbers $\boldsymbol{R}$ such that for all $i = 1, \ldots, n$ and all k_i-tuples of elements $a_1, \ldots, a_{k_i}$ from A,

$$S_i(a_1, \ldots, a_{k_i}) \quad \text{iff} \quad T_i[\phi(a_1), \ldots, \phi(a_{k_i})].$$

Such a theorem is called a *representation theorem*, and the homomorphism ϕ is frequently called a *scale of measurement*.

Accompanying the representation theorem is another one, called a *uniqueness theorem*, which describes the family of all homomorphisms from the empirical structure to numerical structures. It can be divided into two parts, one of which is usually suppressed. The visible one assumes that the numerical relational structure is fixed, and it describes how to generate from one homomorphism all homomorphisms into that structure. Put another way, the uniqueness theorem describes the endomorphisms of the relational structure. Often it is easiest to summarize it by stating the number of values of the homomorphism that can be specified in advance so as to render the homomorphism unique: a unit for mass; a unit and a zero for non-absolute temperature or utility; etc.

Early on Stevens (1946, 1951) emphasized these groups of endomorphisms, and he classed measurement as ordinal, interval, or ratio according as the set of endomorphisms includes all strictly increasing functions, or the positive linear ones, or just multiplication by positive constants. He was never very explicit exactly as to where these constraints on measurement scales came from, and he did not seem to accept fully its dependence on an axiomatic theory of measurement. For example, he insisted that the scale obtained by his empirical method of magnitude estimation was a ratio scale, but he never provided any detailed justification for that claim. By contrast, Suppes is very clear that such concepts of uniqueness rest explicitly on a detailed theory of measurement.

The (usually) unstated uniqueness theorem concerns alternative numerical relational structures. For example, the measurement of length and mass is almost always into $\langle Re^+, \geqslant, + \rangle$, where Re^+ is the set of positive reals. But by taking the exponential of that homomorphism, it is evident that the measurement could be into $\langle Re^+, \geqslant, \cdot \rangle$. Mostly, we do not

worry about this sort of nonuniqueness because it is agreed that it is purely a matter of convention which one we choose. So we have accepted the convention of using $+$ when it comes to combining within an attribute and $\cdot$ when it comes to combining between attributes, as for example momentum which is conventionally treated as the product of velocity and mass. Obviously, these conventions could have been different. In any event, it is a relatively trivial exercise to characterize all conceivable alternative numerical structures, and so there is not much interest in doing so (see p. 99–102 and 273 of Krantz *et al.*).

Invariance and Meaningfulness

When Stevens first focussed attention on the notion of scale type – the set of endomorphisms of the numerical representation – he pointed out an important connection between that set and the sorts of statistical statements that remain invariant under these transformations. There ensued a heated debate in both the psychological and, to a lesser degree, statistical literatures about the correctness of his proscriptions concerning statistical practice. To this day, the issues do not seem fully resolved, even among those very familiar both with measurement and statistics. For example, Suppes and I have never reached agreement about what limitations scale types impose on the use of statistical tests.

In the 1950's Suppes pointed out that what Stevens had said about the invariance of certain statistics was but a special case of a more general question, namely, which statements that one might formulate in terms of a measurement representation correspond to something meaningful in the empirical structure. For example, it is clear that it is not meaningful to assert that the mass of a particular object is 5, whereas it is quite meaningful to say that the ratio of one mass to another is 5.

Suppes' (1959) first approach to the problem was in terms of formal languages, but so far as I know that attack has not been pursued further by him or anyone else. An alternative, and much more comprehensible, set-theoretic approach was later suggested by Suppes and developed by Robinson (1963) and applied by Adams, Fagot, and Robinson (1965) to the statistical problems posed by Stevens. A summary of this work was given by Suppes and Zinnes (1963). The idea is that a statement formulated entirely in terms of measures and logical connectives is meaningful if and only if it is invariant under the endomorphisms of the numerical relational structure. A precise formulation of the word 'statement' either requires invoking formal languages, as in the 1959 paper, or treating a statement as defining a relation on the set R.

Of course, the notion of invariance under transformations plays an important role in many formulations of physics. A number of Suppes' papers concern the concept in various branches of mechanics. It also is a key idea in dimensional analysis, where it is required that physical laws be invariant under certain classes of transformations called similarities. Recently, I have shown (Luce, 1978) that within the richer context of measurement structures tied together, as in physics, by distribution laws, the concept of dimensional invariance is exactly the same as that of meaningfulness, namely, invariance of empirical relations under the automorphisms of the qualitative dimensional structure.

Logical Status of Axioms

Little has yet been said about the constraints imposed on the empirical relations S_i except that S_1 is always some species of ordering, $\gtrsim$. Obviously, other axiomatic properties must hold among the relations, and Suppes was one of the first to point out some logical distinctions among them.

First, there are the universal axioms that are logical consequences of the representation together with properties of the real numbers. One is that S_1 must be a weak ordering if T_1 is $\geqslant$. Another is that if S_2 is a ternary relation corresponding to a binary operation, say $\circ$, and if T_2 corresponds to $+$, then $\circ$ must satisfy the monotonicity property, for all a, b, c in A,

$$a \gtrsim b \text{ if and only if } a \circ c \gtrsim b \circ c.$$

Such axioms as these are called *necessary*.

Any axiom that is not necessary must, therefore, restrict the class of possible structures from the most general class having the representation. Suppes called these axioms *structural*, and that term is widely used. Usually the structural axioms involve some sort of existence statement. One example is the solvability axioms that assert the existence of a solution to an empirical equation. For example, in a structure $\langle A, \gtrsim, \circ \rangle$, we often assume that for all a, b in A with $a \succ b$, there exists c in A such that $a \sim b \circ c$, where $\sim$ denotes both $\gtrsim$ and $\lesssim$. Sometimes the existential nature of the axiom is masked and combined with another type of axiom into some form of topological continuity.

A second classification is into first and second order axioms. Because

the representation is usually some sort of ordinary numerical structure the cardinality of the empirical structure must in some way be restricted. The usual restrictions are either finiteness or the existence of a countable order-dense subset or some version of the Archimedean property, the assertion that positive numerical intervals are comparable. Such axioms are second order ones, and either one is included or the representation has to be modified. Recent studies (Narens, 1974a; Skala, 1975) have shown that it suffices to deal with versions of non-standard reals (Robinson, 1966).

A third sort of question about the axioms is their consistency and independence. In principle, one could ask about their categoricity, but measurement structures are in practice never categorical. Consistency is usually evident since the intended numerical representation is a model of the axioms. Independence is of course established in the usual way by exhibiting models that satisfy all but the axiom in question. For example, Suppes in his first publication (1951) took pains to give a system of axioms for extensive measurement which are independent. (He also improved on Hölder's system by weakening the structural axioms, but his system has long since been superseded by better ones.) Often, however, as for example in axiomatizations of Boolean algebras, the most economical set of independent axioms is less transparent than a slightly more redundant set of axioms, and so some degree of overlap among the axioms is permitted, including sometimes dependent axioms. (For example, in a theory of extensive structures it is a lot easier to include commutativity of $\circ$ rather than to deduce it.)

Finiteness

Most of the axiom systems found in the literature of measurement force the set A of the empirical relational structure to be infinite. At the same time, these axiom systems involve a finite number of relations and a finite (usually quite small) number of axioms characterizing these relations. Although such structures often seem like plausible idealizations of reality, from two points of view they are not descriptive. First, most theories of the universe say there are only finitely many objects and so any infinite structure must not be an accurate description. Second, any set of data we deal with must be finite, and perhaps the theory of measurement should be developed only for data structures. Suppes has strongly argued, both in person and by example, that we should develop finite measurement systems for, at least, the latter reason. As we shall see, he seems implicitly to have rejected the former reason. I should point out that many theorists, and I among them, have never been persuaded that the theories should be confined to the data one happens to have collected, and success in approximating the finite universe by infinite theories is adequate justification for using the infinite theories.

Basically three tacks have been followed in developing theories for finite structures. The first is to suppose that the finite set A is selected in some *a priori* way, as in a factorial experimental design, and then the data are simply the empirical inequalities that are observed. The difficulty of this approach was made clear in the very fundamental paper of Scott and Suppes (1958) in which it was established that such structures cannot be characterized by any fixed set of first order axioms. Scott (1964) and independently Tversky (1964) pursued that tack using a kind of axiom schema that increases the number of axioms with the size of A. If I have understood Suppes' reaction correctly, the logician in him was repelled by this approach. So another avenue had to be followed.

His second approach supposed somewhat implicitly that the finite data set can be selected from an unaxiomatized empirical universe in such a way that certain very special structural relations hold. Put another way, it is assumed that certain equations can be solved and the elements involved are just those solutions. In practice, the elements selected are those that end up being equally spaced in the representation or, put another way, that the integers constitute a suitable representation or, put still another way, the structure axiomatized is what is called a standard sequence in the more general theories. A systematic presentation of a number of these axiomatizations is Suppes (1972a).

They have the great virtue of being rather simple to state and the representation theorems are quite easy to prove, so for many teaching situations they are useful. Nevertheless, they are very incomplete theories. One would like the general theory to include as subsystems any set of data one might, for whatever reason, choose to collect, but one should not necessarily expect to be able to construct a representation of every subsystem. Recently Suppes has shown a way to do this for subjective probability if one is willing to accept approximate measurement for all but the standard sequence. I describe this in some detail below.

Decision Theory and Probability

Although Suppes' first paper concerned the theory of extensive measure-

ment, all of his subsequent work on specific theories of measurement has had to do with rational decision making: subjective expected utility theory, empirical testing of these theories, and axiomatic probability.

Subjective Expected Utility Theory

During the 1950's, a number of economists, statisticians, and philosophers were trying to understand better and to generalize the theory of rational decision making that had been sparked by von Neumann and Morgenstern (1944, 1947, 1953); special attention was given to the axiomatization of expected utility. The most important development was pioneered by Savage (1954). Under plausible axioms, his very rich decision structure (all functions from finite partitions of the states of nature into events with their range a set of consequences) was adequate, first, to derive a unique subjective probability measure over the states of nature and then, using that, to construct a utility function for which subjective expected utility preserved the ordering of decisions. The latter construction paralleled closely that of the original von Neumann and Morgenstern development.

The objections to Savage's approach are by now well known – many of the most telling criticisms were first made by Suppes (1956, 1960). Perhaps the most important ones are, first, the postulation and heavy use of constant acts, i.e., functions with a single consequence, which in most contexts seem highly unrealistic, and second, the structural assumption of arbitrarily fine partitions of the states of nature into equally-likely events, which also usually is highly artificial. Thus, a strong motive for additional work during this period was to overcome these major difficulties. With the exception of Davidson, McKinsey, and Suppes (1955), which provided an alternative formulation of the von Neumann-Morgenstern model, Suppes' work at this time concentrated on working out an alternative idea which had originally been suggested by Ramsey (1931).

Let aEb denote a gamble in which a is the consequence if the event E occurs and b otherwise. Suppose that E^* is an event for which the decision maker is indifferent between aE^*b and bE^*a for all a and b, then it is easy to see that if the subjective expected utility property holds, this event must have subjective probability 1/2. Moreover, for all consequences a, b, c, d,

$$\begin{aligned} aE^*b \gtrsim cE^*d \quad &\text{iff} \quad u(a) + u(b) \geqslant u(c) + u(d) \\ &\text{iff} \quad u(a) - u(c) \geqslant u(d) - u(b). \end{aligned}$$

Thus, if such an event be found, the whole problem of utility construction is reduced to the question of when do orderings of gambles based on this event have a representation in terms of utility differences. So, in sharp contrast to Savage, Suppes began by constructing the utility function and only after that did he get deeply involved with the subjective probability function.

Suppes and Winet (1955) provided an axiomatization of a (quaternary) relation over $A \times A$ for which a relatively unique representation in terms of utility differences obtains; for later (and simpler) versions of this theory, see Chapter 4 of Krantz *et al.* Davidson and Suppes (1956) generalized this so as to construct both a utility function u and a subjective probability function P over events such that the following restricted subjective expected utility property holds: for all consequences a, b, c, d and all events E,

$$aEb \gtrsim cEd \quad \text{iff} \quad u(a)P(E) + u(b)P(\bar{E}) \geqslant u(c)P(E) + u(d)P(\bar{E}).$$

And, by invoking the existence of constant decisions, which did not please him at all, Suppes (1956) gave an axiomatization of the general subjective expected utility property.

As a theoretical program to replace the Savage structure, this effort was only partially successful. Suppes was able to get away from the infinite states of nature, but in the final analysis he was not able to bypass the constant acts. Moreover, the Ramsey context was as narrow as the original von Neumann-Morgenstern one. Not until Krantz and I (1971) (see also Chapter 8 of Krantz *et al.*) developed a theory of conditional expected utility did an alternative exist to Savage which is at the same level of generality, does not invoke infinite states of nature, and does not require constant acts to construct a utility function over acts. [However, for criticisms of that structure, see Balch and Fishburn (1974) and a reply by Krantz and Luce (1974), and Spohn (1977)].

Experiments on Subjective Expected Utility

Unlike many theorists, Suppes has always insisted that a scientific theory be put to empirical test. In particular, his work on decision models was interactive with an experimental program. At the time, the only empirical work in the area was that of Mosteller and Nogee (1951) who had experimented on the von Neumann and Morgenstern model. Together with the experimental psychologist Sidney Siegel, Davidson and Suppes (1957) reported a number of careful studies based on the Ramsey paradigm. In particular they first found a chance event with subjective probability 1/2

– a die with one nonsense triad on three faces and another on the remaining three. Next they selected two sums of money and arbitrarily fixed their utility, and then they successively searched for other sums that were equally spaced in utility. A variety of cross checks were possible. This is not the place to detail these studies, except to note that they were very carefully conducted, they were extensive, and they ran afoul of the pervasive problem of error and inconsistency. The latter had been evident in the Mosteller–Nogee experiment, and it has remained a major stumbling block in evaluating all algebraic measurement theories.

At the time, Suppes attempted to deal with it by introducing an error threshold and using methods of linear programming to solve the resulting set of data inequalities. That had its faults – perhaps the most severe being that the sure-thing principle need not hold – and so a modified model and new experiment were reported in Suppes and Walsh (1959). In a closely related paper, Royden, Suppes, and Walsh (1959) studied the utility for gambling. Valiant though these experimental efforts were, they did not lead to a clear decision as to the adequacy of the expected-utility property and I do not believe that others were persuaded that this was a suitable way to handle error and inconsistency.

The problem of error has remained formidable, though recently some positive steps have been taken. One of these is the work of Falmagne (1976), and the other is the approximate probability model of Suppes discussed in the next section.

In closing this section, let me remark that the whole issue of testing decision theories remains quite murky. Suppes' approach represents one attack: fit the representation to the data as well as possible and then evaluate that fit. Of course, the questions are how best to estimate the huge number of parameters (functions) and how to evaluate the goodness of fit, neither of which has been satisfactorily answered. Moreover, assuming the model is shown to be unsatisfactory, what then? Do we simply reject the rationality axioms that lead to the subjective expected-utility representation, or do we try to modify them? An alternative approach is to study selectively various qualitative properties implied by the subjective expected-utility representation in order to discover in as much detail as possible the nature of the descriptive breakdown of the model. I tend to favor that approach, although I would be less than candid not to admit that so far it has only focussed attention on failures of the extended sure-thing principle without informing us about acceptable substitutes and different representations. Of course, many economists and statisticians argue for the (non-structural) axioms on grounds of rationality, and certainly they are compelling canons of rational behavior. For those people there is no need to study the failures empirically. Rather, as with logic, one attempts to teach rational behavior without particularly caring to describe exactly a student's failures.

Axiomatic Probability

Throughout the time I have known him, Suppes has thought much about the foundations of probability. His interest has taken at least three distinct routes. First, he has emphasized the pervasiveness of stochastic processes in the sciences, especially the social and behavioral ones, and he has spent considerable effort on Markov models for learning. Second, he has repeatedly emphasized (Suppes, 1961, 1963, 1966, 1974a) the anomaly that the single most important theory of physics, quantum mechanics, embodies a version of probability inconsistent with the widely accepted axiomatization of Kolmogorov (1933) which seems to be perfectly adequate for all of the rest of science. Third, from the Bayesian point of view, embodied in various rational theories of decision making, there is the interesting foundational question of finding a satisfactory axiomatization of qualitative (or comparative) probability that possesses a more-or-less unique numerical representation over a plausible algebra of events. This has proved to be a good deal more difficult than might, *a priori*, have been expected.

It is not relevant for me to discuss here his first interest and I have relatively little to say about the second one; I shall however discuss the latter more fully, as it is central to measurement.

Suppes (1966) took up the question of how to modify the Kolmogorov axiom system so as to make it agree with quantum mechanics. His suggestion, if adequate, is certainly simple: just restrict the definition of an algebra of sets to be closed not under all finite unions of events, but just disjoint ones. However, the fact that nearly 10 years later he is again struggling with the problem suggests that he is not satisfied with that solution. For example, on p. 771 of Suppes (1974a) we find

> . . . . [Q]uantum mechanics is not a standard statistical theory – it is a peculiar, mystifying, and as yet, poorly understood radical departure from the standard methodology of probability and statistics. There is as yet no uniform agreement on how the probabilistic aspects or statistical aspects of quantum mechanics should be formulated. But it is widely agreed that there are unusual problems that must be dealt with and that do not arise in standard statistical theories . . .
>
> The difficulty is that when the standard formalism of quantum mechanics is used the joint distribution of noncommuting random variables turns out not to be a proper joint distribution in the classical sense of probability.

These comments were made in a paper critical of K. R. Popper's study of these matters. Among other things, Popper (1959) proposed, without giving a careful mathematical analysis, a propensity interpretation of probability. Suppes (1973) suggested that axioms, much like those in Krantz *et al.* (p. 222), for qualitative conditional probability may provide a suitable axiomatization of propensity. Within that context, he is able to provide a qualitative axiom characterizing an event whose occurrence is independent of the past (e.g., radioactive decay), and to formulate a qualitative axiom for randomness.

Interesting though this may be, so far as I can see the deep issue of probability in quantum mechanics remains as problematical as ever.

Turning to the role of probability in theories of rational decision making, recall that Suppes was highly critical of the qualitative axiomatizations of Savage and de Finetti, because the structural axioms forced an infinity of events and were otherwise unrealistic. An alternative approach involving only finitely many events, due to Scott (1964) and Tversky (1964) is also unsatisfactory because of the ". . . combinatorial explosion that occurs in verifying the axioms when the number of events is large" (p. 166, Suppes, 1974b). So simply imposing finiteness by itself is not enough. A third problem is that of error and imprecision. His experimental work made it clear that the usual precision of measurement theories is unrealistic. Indeed, almost all real life uses of probability notions lack precision.

> It is this practical sense of leaving things vague and qualitative that needs to be dealt with and made explicit. In my judgment to insist that we assign sharp probability values to all of our beliefs is a mistake and a kind of Bayesian intellectual imperialism. I do not think this corresponds to our actual ways of thinking, and we have been seduced by the simplicity and beauty of some of the Bayesian models. On the other hand, a strong tendency exists on the part of practicing statisticians to narrow excessively the domain of statistical inference, and to end up with the view that making a sound statistical inference is so difficult that only rarely can we do so, and usually only in the most carefully designed and controlled experiments. (p. 447, Suppes, 1976).

His first new approach to the problem of axiomatizing qualitative probability very neatly combines the idea that there should be a finite set of events that are equally spaced and resolved very precisely with the idea that there are many other events which are irregularly spaced in probability and, indeed, are known only approximately. A little more precisely, the structure assumed is $\langle X, \mathscr{E}, \mathscr{S}, \gtrsim \rangle$, where X is a set (sample space), $\mathscr{E}$ and $\mathscr{S}$ are both algebras of subsets of X, and $\gtrsim$ is a binary relation on $\mathscr{E}$. Intuitively, $\mathscr{E}$ corresponds to all of the events to which probabilities in some form or another will be assigned, and $\mathscr{S}$ is the set of events to which precise assignments are made. It is assumed that $\langle X, \mathscr{E}, \gtrsim \rangle$ satisfies the usual deFinetti axioms – $\gtrsim$ is a monotonic weak ordering, $A \gtrsim \phi$ for A in $\mathscr{E}$; and $X \succ \phi$ – and that $\mathscr{S}$ is a finite subset of $\mathscr{E}$ with the two properties:

(i) if S is in $\mathscr{S}$ and $S \neq \phi$, then $S \succ \phi$.

(ii) if S, T are in $\mathscr{S}$ and $S \gtrsim T$, then there is a V in $\mathscr{S}$ such that $S \sim T \cup V$.

He has shown that there is a unique probability measure P on $\mathscr{S}$ that preserves the order $\gtrsim$ and that assigns the same probability to every minimal event of $\mathscr{S}$. For any element A of $\mathscr{E}$, one assigns upper and lower probabilities P^* and P_* as follows: if A is in $\mathscr{S}$, then $P^*(A) = P_*(A) = P(A)$; if not, then one finds S and S' in $\mathscr{S}$ such that $S \gtrsim A \gtrsim S'$ and $S \sim S' \cup V$, where V is in $\mathscr{S}$ and is minimal, and sets $P^*(A) = P(S)$ and $P_*(A) = P(S')$. These upper and lower probabilities can be shown to satisfy a number of properties previously proposed by Good (1962) and Smith (1961), and that $P^*(A) - P_*(A) \leq 1/n$, where n is the number of minimal elements in $\mathscr{S}$. Furthermore, if we define the relation $*\succ$ on $\mathscr{E}$ by:

$$A *\succ B \text{ iff there exists } S \text{ in } \mathscr{S} \text{ with } A \gtrsim S \gtrsim B,$$

then it can be shown that $*\succ$ is a semiorder on $\mathscr{E}$ and

$$\text{if } A *\succ B, \text{ then } P_*(A) \geq P^*(B),$$
$$\text{if } P_*(A) \geq P^*(B), \text{ then } A \gtrsim B.$$

This is derived in Suppes (1974b) and summarized in Suppes (1975, 1976). In addition, in Suppes (1975) these results are used to generate a comparable theory of approximate expected utility.

In my opinion, this is a most interesting development which has widespread potential for the whole theory of measurement. It captures quite neatly the idea that in measurement there is a precisely measured finite standard series which in turn is used to provide approximate measurement of other things of interest.

His second new approach, found in Suppes and Zanotti (1976), involves a quite different tack, namely to enlarge the scope of the problem. Often in mathematics this proves a more effective route than trying to axiomatize just the structures of interest. In this case we replace $\mathscr{E}$ by a closely related

family of random variables as follows. For any A in $\mathscr{E}$, its indicator function A^c is defined as:

$$A^c(a) = \begin{cases} 1 \text{ if } a \text{ is in } A \\ 0 \text{ if } a \text{ is not in } A. \end{cases}$$

Of course, $A^c + B^c$ is a function, but in general it is not an indicator function. Denote by $\mathscr{E}^*$ the algebra of *extended indicator functions* defined to be the smallest semigroup under function addition that includes all of the indicator functions of $\mathscr{E}$. The elements of $\mathscr{E}^*$ are obviously a subclass of all the random variables defined on X. The theorem proved is this: A necessary and sufficient condition for $\langle X, \mathscr{E}, \gtrsim \rangle$ to have a unique, order preserving probability representation is that it is possible to extend $\gtrsim$ to an ordering $\gtrsim*$ on the algebra $\mathscr{E}^*$ of extended indicator functions such that $\langle \mathscr{E}^*, \gtrsim*, + \rangle$ satisfies the conditions of a positive closed extensive structure (Krantz *et al.*, 1971, p. 73). It is not yet clear how useful this criterion will prove to be.

Concluding Remarks

Suppes' major contributions to the theory of measurement have been, in my opinion, four.

First, he laid bare, more clearly than anyone before him, the exact nature of a theory of measurement. He has been very exacting about stating what is empirical and what is mathematical, the types of axioms that are involved and the degree to which the structural ones can be avoided, and the limitations on meaningful numerical statements. To a degree this is didactic and expository, but it is my impression that the field has moved ahead more rapidly and surely because of his demand for logical clarity.

Second, he has focussed very sharply the distinction between finite and infinite structures. His original hope of finding finite systems of universal axioms was dashed by his fundamental paper with Scott (1958) and was not saved by Scott's (1964) axiom schema. Following that he persisted in pushing finite, equally-spaced structures (finite systems with extremely strong structural axioms), which I have never thought were very satisfactory until his recent work in approximate measurement of probability. An alternative tack, pursued by Narens (1974b), is to see the way in which increasingly large finite structures converge to infinite ones.

Third, he has and continues to contribute to the theory of qualitative probability and subjective expected utility. His first work in the mid 1950's involved cogent criticism of Savage's approach and the attempt to work out and to test empirically a substitute based on utility difference measurement. Although this, by itself, did not resolve the issues, it was surely an important intermediate step. Recently, he has developed a theory of approximate probability measurement involving a finite subsystem that is exactly measured; I find this work exciting and with a potential for wide generalization.

Finally, and by no means least, Suppes has been an important expositor of theories of measurement. His chapter with Zinnes (1963) was the first systematic statement of his outlook on measurement. Later I was involved in three expositions with him (Luce and Suppes, 1965, 1974, and Krantz *et al.,* 1971). Other papers of a largely expository character are Suppes (1960, 1961b, 1967, and 1972b). Often incorporated in these papers is a concern with history. Perhaps the purest example of this is Suppes (1971) in which he goes back to Archimedes' account of measurement and shows that much of it appears sensible if put into the framework of modern conjoint measurement theory.

Harvard University

References

Adams, E. W., R. F. Fagot, and R. E. Robinson: 1965, 'A theory of appropriate statistics', *Psychometrika*, **30**, 99–127.

Balch, M., and P. C. Fishburn: 1974, 'Subjective expected utility for conditional primitives', in M. S. Balch *et al.* (eds.), *Essays on Economic Behavior under Uncertainty*, North-Holland, Amsterdam, 1974, pp. 57–69.

Bridgman, P.: 1922, 1931, *Dimensional Analysis*, Yale University Press, New Haven.

Campbell, N. R.: 1920, *Physics: the Elements*, Cambridge University Press, Cambridge. Reprinted as *Foundations of Science: the Philosophy of Theory and Experiment*, Dover, New York, 1957.

Campbell, N. R.: 1928, *An Account of the Principles of Measurement and Calculation*, Longmans, Green, London.

Cohen, M. R. and E. Nagel: 1934, *An Introduction to Logic and Scientific Method*, Harcourt, Brace, New York.

Davidson, D., J. C. C. McKinsey, and P. Suppes: 1955, 'Outlines of a formal theory of value, I', *Philosophy of Science*, **22**, 140–160.

Davidson, D. and P. Suppes: 1956, 'A finitistic axiomatization of subjective probability and utility', *Econometrica*, **24**, 264–275.

Davidson, D., P. Suppes, and S. Siegel: 1957, *Decision Making: an Experimental Approach*, Stanford University Press, Stanford.

Falmagne, J.-C.: 1976, 'Random conjoint measurement and loudness', *Psychological Review*, **83**, 65–79.

Good, I.J.: 1962, 'Subjective probability as the measure of a non-measurable set', in E. Nagel *et al* (eds.), *Logic Methodology and Philosophy of Science: Proceedings of the 1960 International Congress*, Stanford University Press, Stanford, 1962, pp. 319–329.

von Helmholtz, H.: 1887, 'Zählen und Messen Erkenntnis – theoretisch Betrachtet', *Philosophische Aufsätze Edvard Zeller Gewidmet*, Leipzig. English translation by C. L. Bryan, *Counting and Measuring*, van Nostrand, Princeton, 1930.

Hölder, O.: 1901, 'Die Axiome der Quantität und die Lehre vom Mass', *Ber. Verh. Kgl. Sächsis. Ges. Wiss. Leipzig, Math.-Phys. Classe*, **53**, 1–64.

Kolmogorov, A. N.: 1933, *Grundbegriffe der Wahrscheinlichkeitsrechnung*, Springer, Berlin. English translation by N. Morrison, *Foundations of the Theory of Probability*, Chelsea, New York, 1956.

Krantz, D. H. and R. D. Luce: 1974, 'The interpretation of conditional expected-utility theories', in M. S. Balch *et al.* (eds.), *Essays on Economic Behavior under Uncertainty*, North-Holland, Amsterdam, pp. 70–73.

Krantz, D. H., R. D. Luce, P. Suppes, and A. Tversky: 1971, *The Foundations of Measurement*, I, Academic Press, New York.

Luce, R. D.: 1978, 'Dimensionally invariant numerical laws correspond to meaningful qualitative relations', *Philosophy of Science*, **45**, 1–16.

Luce, R. D. and D. H. Krantz: 1971, 'Conditional expected utility', *Econometrica*, **39**, 253–271.

Luce, R. D. and P. Suppes: 1965, 'Preference, utility, and subjective probability', in R. D. Luce *et al.* (eds.), *Handbook of Mathematical Psychology*, III, Wiley, New York, 1965, pp. 249–410.

Luce, R. D. and P. Suppes: 1974, 'Measurement, theory of', *Encyclopedia Britannica*, 739–745.

Mosteller, F. and P. Nogee: 1951, 'An experimental measurement of utility', *Journal of Political Economy*, **59**, 371–404.

Narens, L.: 1974a, 'Measurement without Archimedean axioms', *Philosophy of Science*, **41**, 374–393.

Narens, L.: 1974b, 'Minimal conditions for additive conjoint measurement and qualitative probability', *Journal of Mathematical Psychology*, **11**, 404–430.

von Neumann, J. and O. Morgenstern: 1944, 1947, 1953, *Theory of Games and Economic Behavior*, Princeton University Press, Princeton.

Pfanzagl, J.: 1968, *Theory of Measurement*, Wiley, New York.

Popper, K. R.: 1959, 'The propensity interpretation of probability', *The British Journal for the Philosophy of Science*, **10**, 25–42.

Ramsey, F. P.: 1931, 'Truth and probability', in F. P. Ramsey, *The Foundations of Mathematics and Other Logical Essays*, Harcourt, Brace, New York, 1931, pp. 156–198.

Robinson, A.: 1966, *Non-standard Analysis*, North-Holland, Amsterdam.

Robinson, R. E.: 1963, 'A set-theoretical approach to empirical meaningfulness of empirical statements', *Technical Report* **55**, Institute for Mathematical Studies in the Social Sciences, Stanford.

Royden, H. L., P. Suppes, and K. Walsh: 1959, 'A model for the experimental measurement of the utility of gambling', *Behavioral Science*, **4**, 11–18.

Savage, L. J.: 1954, *The Foundations of Statistics*, Wiley, New York.
Scott, D.: 1964, 'Measurement models and linear inequalities', *Journal of Mathematical Psychology*, **1**, 233–247.
Scott, D. and P. Suppes: 1958, 'Foundational aspects of theories of measurement', *The Journal of Symbolic Logic*, **23**, 113–128.
Skala, H. J.: 1975, *Non-Archimedean Utility Theory*, Reidel, Dordrecht, Holland.
Smith, C. A. B.: 1961, 'Consistency in statistical inference and decision', *Journal of the Royal Statistical Society, Series B*, **23**, 1–25.
Spohn, W.: 1977, 'Where Luce and Krantz Do Really Generalize Savage's Decision Model', *Erkenntnis*, **11**, 113–134.
Stevens, S. S.: 1946, 'On the theory of scales of measurement', *Science*, **103**, 677–680.
Stevens, S. S.: 1951, 'Mathematics, measurement and psychophysics', in S. S. Stevens (ed.), *Handbook of Experimental Psychology*, Wiley, New York, 1951, 1–49.
Suppes, P.: 1951, 'A set of independent axioms for extensive quantities', *Portugaliae Mathematica*, **10**, 163–172.
Suppes, P.: 1956, 'The role of subjective probability and utility in decision-making', *Proceedings of the Third Berkeley Symposium on Mathematical Statistics and Probability*, 1954–1955, **5**, 61–73.
Suppes, P.: 1959, 'Measurement, empirical meaningfulness, and three-valued logic', in C. W. Churchman and P. Ratoosh (eds.), *Measurement: Definitions and Theories*, Wiley, New York, 1959, pp. 129–143.
Suppes, P.: 1960, 'Some open problems in the foundations of subjective probability', in R. E. Machol (ed.), *Information and Decision Processes*, McGraw-Hill, New York, 1960, pp. 162–169.
Suppes, P.: 1961a, 'Probability concepts in quantum mechanics', *Philosophy of Science*, **28**, 378–389.
Suppes, P.: 1961b, 'The philosophical relevance of decision theory', *The Journal of Philosophy* **21**, 605–614.
Suppes, P.: 1963, 'The role of probability in quantum mechanics', in B. Baumrin (ed.), *Philosophy of Science, the Delaware Seminar*, Vol. 2, Wiley, New York, 1963, pp. 319–337.
Suppes, P.: 1966, 'The probabilistic argument for a non-classical logic of quantum mechanics', *Philosophy of Science*, **33**, 14–21.
Suppes, P.: 1967, 'Decision theory', in P. Edwards (ed.), *The Encyclopedia of Philosophy*, MacMillan and Free Press, New York, 1967, pp. 310–314.
Suppes, P.: 1971, 'Archimedes's anticipation of conjoint measurement,' *XIII International Congress of the History of Science*, "Nauka" Publishing House, Central Department of Oriental Literature, Moscow, 1–17.
Suppes, P.: 1972a, 'Finite equal-interval measurement structures', *Theoria*, **38**, 45–63.
Suppes, P.: 1972b, 'Measurement: problems of theory and application', in *Mathematics in the Social Sciences*, Australian Government Publishing Service, Canberra, 1972, pp. 613–622.
Suppes, P.: 1973, 'New foundations of objective probability: axioms for propensities', in P. Suppes *et al.* (eds.), *Logic, Methodology and Philosophy of Science IV: Proceedings of the Fourth International Congress for Logic, Methodology and Philosophy of Science, Bucharest, 1971*, North-Holland, Amsterdam, 1973, pp. 515–529.
Suppes, P.: 1974a, 'Popper's analysis of probability in quantum mechanics', in P. A.

Schilipp (ed.), *The Philosophy of Karl Popper*, Vol. 2, Open Court, LaSalle, Ill., 1974, pp. 760–774.
Suppes, P.: 1974b, 'The measurement of belief', *The Journal of the Royal Statistical Society Series B (Methodology)*, **36**, 160–191.
Suppes, P.: 1975, 'Approximate probability and expectation of gambles', *Erkenntnis*, **9**, 153–161.
Suppes, P.: 1976, 'Testing theories and the foundations of statistics', in Harper and Hooker (eds)., *Foundations of Probability Theory, Statistical Inference, and Statistical Theories of Science*, II, Reidel, Dordrecht, 1976, pp. 437–455.
Suppes, P. and K. Walsh: 1959, 'A non-linear model for the experimental measurement of utility', *Behavioral Science*, **4**, 204–211.
Suppes, P. and M. Winet: 1955, 'An axiomatization of utility based on the notion of utility differences', *Management Science*, **1**, 259–270.
Suppes, P. and Zanotti, M.: 1976, 'Necessary and sufficient conditions for existence of a unique measure strictly agreeing with a qualitative probability ordering', *Journal of Philosophical Logic*, **5**, 431–438.
Suppes, P. and J. L. Zinnes: 1963, 'Basic measurement theory', in R. D. Luce *et al.* (eds.), *Handbook of Mathematical Psychology*, I, Wiley, New York, 1963, pp. 1–76.
Tversky, A.: 1964, 'Finite additive structures', *Technical Report MMPP* **64–6**, University of Michigan, Michigan Mathematical Psychology Program.
Wiener, N.: 1921, 'A new theory of measurement: a study in the logic of mathematics', *Proceedings of the London Mathematical Society*, **19**, 181–205.