

## DUNCAN LUCE AS MEASUREMENT THEORIST

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**ABSTRACT.** This article records significant aspects of Duncan Luce's work as a measurement theorist. Its rather personal slant is based on our friendship of more than four decades.

### 1. FIRST ACQUAINTANCE

I first met Duncan in 1954 when he was a first-year fellow at the Center for Advanced Study in the Behavioral Sciences at Stanford. We talked some that year but our acquaintance really became much closer in the summer of 1957 when several of us, including Duncan, organized a family of summer institutes in various topics in mathematical social science at Stanford under the auspices of the Social Sciences Research Council. It was at this 1957 summer meeting that Duncan circulated the first draft of *Individual Choice Behavior* (1959).<sup>1</sup> At first I thought his formulation of the choice axiom was wrong, and we had some very intense arguments about it that summer. Out of our disagreement about this, and my recognition that I was wrong in my views about Duncan's formulation, we became not just acquaintances but close friends.

When Duncan, Bob Bush, and Gene Galanter decided to edit the *Handbook of Mathematical Psychology* in the early 60's, they invited me to write the first article on measurement, which I did jointly with Jerry Zinnes. In connection with that article, Duncan and I began discussions about the theory of measurement. As Volume III of the *Handbook of Mathematical Psychology* was organized, Duncan invited me to write jointly with him a long chapter entitled "Preference, Utility and Subjective Probability." This chapter actually contains a substantial amount of material on measurement theory, as is evident from the title, because its focus is an area in which a good deal of the modern research on measurement theory has been conducted. Duncan and I talked about this chapter a lot and out of that grew a strong common interest in measurement concepts.

### 2. THE THREE-VOLUME FOUNDATIONS OF MEASUREMENT

Following the publication of the *Handbook of Mathematical Psychology*, in 1968 Duncan and I wrote with Bob Bush the article on mathematical models in the social sciences for the *International Encyclopedia of the Social Sciences*, and in the same

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<sup>1</sup>The many books and papers of Duncan referred to here can be found in his bibliography given at the end of this volume.

year Duncan and I also wrote the article on mathematics for it. At about the same time, we got the idea of writing a substantial treatise on the foundations of measurement that would be aimed at covering most developments in the subject at that time. We agreed that we needed help in areas we did not know well, so we invited Dave Krantz and Amos Tversky to join us. Each of the four authors may have a slightly different recollection of how it got organized, but I think this is approximately correct. Certainly Duncan, as always, was the pacemaker in getting his parts of the treatise written. I have some further remarks about that in the last section.

Of the three volumes of *Foundations of Measurement*, Volume I appeared in 1971 – the only volume originally intended – Volume II in 1989 and Volume III in 1990. The final decision to have three rather than two volumes came quite late, as we realized there was too much material for a single second volume. Completing the work occupied all four of us over a good many years. Not certainly our only occupation, but still a major preoccupation to get it finished.

I can remember well the couple of weeks that Duncan moved in with me and my family to complete the chapters I was still lagging on in Volume I. This was probably sometime in the summer or early fall of 1970. He knew that one way to get them written was to come and stay until they were finished. Naturally the chapters, or parts of chapters, he had taken primary responsibility for were long since in finished draft. We had a somewhat similar experience in the writing of Volumes II and III. Duncan was always first off the mark with the parts he was committed to drafting and he was good at pestering the rest of us to get something finished. As is evident both from the articles referenced and from other features of the volume, most of Volume III was actually put together by Duncan and a good many parts of it reflect his particular interests in the theory of measurement that continue into the present, a subject which I remark on later.

Writing a joint work of the length of *Foundations of Measurement*, with a composition that extended over many years, is a strain without any doubt, but I am happy to say that Duncan and I are still talking in friendly terms and continue to see each other. We are of course all relieved, not just Duncan and me, but Dave Krantz<sup>2</sup> as well, that after many years the work is finally finished. It is clear that for each of us this will probably be the longest publication we participate in directly as an author in our entire academic career.

### 3. LUCE'S RESEARCH IN MEASUREMENT

I have already remarked on Duncan's central and leading role in the writing of *Foundations of Measurement (FM)*. I want to stress now the importance of the research in the theory of measurement that he has done over many years, much of which is included in *FM* in systematic review of the subject. But *FM* is primarily a report of research already completed and so it is appropriate to give a separate account of Duncan's many papers in measurement. I will not try to cover everything. There are really too many to comment on each one in depth. What is

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<sup>2</sup>Sadly for all of us Amos Tversky died May 26, 1996.

important is to comment on those that in my view have had a significant impact on research in the field.

The place for me to begin is his famous 1957 paper on semiorders. The idea of just noticeable differences had been around in psychology for a good many decades, going back at least to early work of Thurstone, but a qualitative non-statistical theory of just noticeable differences, particularly at the level of simple orderings, had not been well worked out prior to Duncan's paper. He carefully developed the relation between just noticeable differences and utility discriminations. The algebraic work on semiorders now probably has a research literature of as many as 500 papers. Duncan's article is the beginning of that rich and fertile concept.

There followed in 1958 the article with Ward Edwards on the derivation of subjective scales from just noticeable differences, and in 1959, the article "A probabilistic theory of utility and its relationship to Fechnerian scaling" in a volume on measurement theory edited by Churchman and Ratoosh (1959). And then in the same year his important paper "On the possible psychophysical laws" which like the paper with Edwards a year earlier appeared in *Psychological Review*. Duncan's work on these related topics of subjective scales, psychophysical laws, and closely related matters such as Fechnerian scaling has contained some of his most original and interesting ideas. He has subsequently published several papers on the possible psychophysical laws. I have always felt myself that this work is among the most interesting from a theoretical standpoint of any that has appeared in psychophysics.

In fact, I think one of the problems I have in writing about Duncan's work on measurement is not to try to cover a large part of the research papers he has written, because so many of the things that he has focused on in psychophysics and decision theory, both theoretical and experimental, have been closely related to issues of measurement. For example, many of his experimental papers on preference and choice have tested particular measurement axioms for preferences, utility or subjective probability.

A theoretical paper of Duncan's with John Tukey in 1964 is another example of a paper squarely in the theory of measurement that has had a significant impact both on measurement theory and several other areas of psychology. I refer to the well-known article of that year on conjoint measurement, which appeared in the first volume of the *Journal of Mathematical Psychology (JMP)*. Duncan has continued to write papers on conjoint measurement, but perhaps more important, a large number of other people have as well. The literature on conjoint measurement, taken in both its theoretical and experimental aspects, is even larger in my judgment, though I have not attempted a serious quantitative comparison, than that on semiorders.

As another aspect of Duncan's fundamental work on possible psychophysical laws I mention also his 1965 paper that appeared in the journal *Philosophy of Science* entitled "A 'fundamental' axiomatization of multiplicative power relations among three variables." Duncan's work in this area is extraordinarily interesting. As written up by Duncan in Chapter 10 in Volume I, it is some of the most permanent material in terms of significance to be found in *FM*.

Another direction of Duncan's work in measurement is reflected in his 1967 paper in the *Annals of Mathematical Statistics* on sufficient conditions for the existence of a finitely additive probability measure. The axiom that really does the

work here is complicated, but it is typical of Duncan that he kept at the topic until he got to something that indeed provided a reasonable sufficient condition. This was followed up in 1968 by a related article in the *Annals of Mathematical Statistics* on the numerical representation of qualitative *conditional* probability. In many ways the article on conditional probability, because of its greater complexity, marked a more substantial advance in the subject. In the same year, 1968, Duncan published in *Synthese* with Fred Roberts an article that I have always admired, and consider fundamental, on axiomatic thermodynamics and extensive measurement. Again, much of this work was included in Volume I of *FM*.

Then in the next year, 1969, Duncan published with Tony Marley another extension of extensive measurement, namely, extensive measurement when the concatenation operation is restricted and maximal elements may exist, a subject of considerable conceptual interest from the standpoint of the actual practice of extensive measurement. Again, the core results were included in Volume I of *FM*. Then in 1971 Duncan published three measurement articles, with results that mostly were also reorganized and put in Volume 1 of *FM*. I have in mind, first, the article on periodic extensive measurement, in itself an interesting conceptual problem. This article appeared in *Composito Mathematica*. Next was the article on conditional expected utility with Dave Krantz, which appeared in *Econometrica*, and third was Duncan's article "Similar systems and dimensionally invariant laws" which appeared in *Philosophy of Science*.

In 1972 and in the years that followed, with Volume I of *FM* out of the way, Duncan in many ways accelerated his publications in the theory of measurement. First in 1972 appeared "Conditional expected, extensive utility" in *Theory and Decision*. Then in 1973, the paper "Three axiom systems for additive semi-ordered structures" in the *SIAM Journal of Applied Mathematics* and the article "Measurement and psychophysics" in the volume *Notes of Lectures on Mathematics in the Behavioral Sciences*, published by the Mathematical Association of America. This is a long article reviewing much of the work that Duncan had done on measurement in psychophysics.

In the second half of the 70's Duncan began publishing an important series of articles with Louis Narens on the theory of measurement. In 1976 two articles appeared with Narens, first "The algebra of measurement" in the *Journal of Pure and Applied Algebra* and "A qualitative equivalent to the relativistic additive law for velocities" in *Synthese*, and then two years later, again with Narens, "Qualitative independence in probability theory" in *Theory and Decision*. Duncan and Louis come at the theory of measurement from a somewhat different perspective. Duncan, with a broad scientific experience in mathematical psychology, and originally Louis with a background in mathematics and a focus on abstract theory of measurement as he moved into the mathematical social sciences. Certainly it has been a very fruitful and productive collaboration. I will not have space to mention all of their joint work but it has been one of the important collaborations in the theory of measurement in the last two decades.

To mention some of the important papers in this long collaboration, there is a 1983 paper with Louis on interpersonal comparability of utility in *Theory and Decision* and the long paper entitled "Symmetry, scale types, and generalizations

of classical physical measurement" in *JMP*. Then in the next year, 1984, with Louis "Classification of real measurement representations by scale type" in *Measurement*, and in 1985 with Louis "Classification of concatenation structures according to scale type" in *JMP*, and in 1986 "Uniqueness and homogeneity of real relational structures" in *JMP*. I like to think in many ways of this leading up to a central interest of both Duncan's and Louis', namely, obtaining very general theorems about the scale type of the relational structures that have numerical representations and satisfy a few quite general conditions. This tendency is very much brought out in the 1987 article with Louis "The mathematics underlying measurement on the continuum" in *Science*, and Duncan's article "Measurement structures with Archimedean ordered translation groups" in *Order*.

Toward the latter part of the 80's Duncan became intensely concerned with revising the classical axioms for subjective expected utility to provide an axiomatization reflecting in a more sensitive way many of the descriptive aspects of choice behavior that had been found in a number of experimental studies, several of which Duncan was himself involved in as author or co-author. I have in mind for example his 1988 article "Rank-dependent, subjective-utility representations" in *The Journal of Risk and Uncertainty*. This same direction of work is to be seen in his 1991 article "Rank- and sign-dependent linear utility models for binary gambles" in the *Journal of Economic Theory*, his article in the same year with Peter Fishburn "Rank- and sign-dependent linear utility models for finite first-order gambles" in the *Journal of Risk and Uncertainty*, and a number of articles in the 90's that continue this line of investigation. I mention especially Duncan's 1992 article "Where does subjective expected utility fail descriptively?" in the *Journal of Risk and Uncertainty*, and even more recent work that is still in the process of being published at the time of my writing this article.

During the last few years in the 90's Duncan has also continued the line of investigation with Louis I mentioned that I thought was particularly important in terms of reaching for the greatest level of generality in characterizing measurement structures. I mention here their 1992 article "Intrinsic Archimedeaness and the continuum" in the measurement volume edited by Savage and Ehrlich (1992).

Although I will not really finish this survey, I will just stop with the comment that it is evident enough from looking at my file of papers soon to be published by Duncan, and by talking with him, that his interest in measurement continues unabated. We may expect a host of papers in the years ahead, particularly now that he has retired and has even more time to pursue the many measurement ideas I know he feels are not yet put in proper order.

In closing my survey of Duncan's research on measurement I should mention some areas in which we have had many discussions and about which we do not entirely agree.

To begin with, Duncan has been very much more attracted to the continuum as a framework for theories of measurement than I have. He has made a good case for his views, especially in much of his recent abstract work with Louis Narens. Our very disagreement has, on the other hand, inspired me to further work and I have been motivated to take the long route of setting up a constructive system of axioms for the foundations of mathematics within which one can prove a certain

kind of isomorphism between standard continuous models and finite models. I will not try to give a technical description of this work (see Suppes & Chuaqui, 1993; Chuaqui & Suppes, 1995; Sommer & Suppes, in press a, in press b). The important point is that one does not have to agree with Duncan about what are the very best approaches to the theory of measurement to recognize how important his work has been in pushing forward the frontiers of the subject, especially in a general mathematical setting.

The second remark is that we have also had many discussions about the theory of error, and we both agree that this is without question the biggest omission in the kind of work on the foundations of measurement that he and I have engaged in for many years. Where we do not entirely agree is how the theory of error should be approached from the standpoint of the theory of measurement. Again, I will not try to enter into the details of where we have a different kind of emphasis. It may well be that when the problems are really straightened out we will find we very much agree about their solutions.

Third, although I have respected and admired Duncan's detailed work on rank-order models and the like for preferences and decision theory, my own view is that the deficiencies of decision theory and of subjective expected utility theory are to be found more in the thinness of the psychological assumptions about preference at a more general level. In this case, Duncan and I have not really discussed our differences of view with any thoroughness, mainly because my own ideas about how to modify the classical approach have only recently begun to head in a new direction. Duncan would certainly be entitled at this stage to tell me to put some axioms on the table because I have not as yet done anything like the detailed work he has on modifying the classical framework. I do hope to do so and to bring in such matters as the way in which unconscious associations have an effect on choices immediately following them, and how such matters of association can be integrated into a theory of what it means to be rational. Some skeptics may call this the Freudian view of the ideal consumer. But elaboration of these ideas must be left for another time, when I am sure Duncan will be there to give me a proper set of criticisms.

#### 4. SOME PERSONAL REMARKS

From what I said at the beginning, it is evident that my friendship with Duncan extends over more than forty years, and so I feel entitled to comment on my perceptions of Duncan as a working scientist. The remarks I want to make are similar to ones that I made at a dinner in honor of his retirement at Irvine on August 3, 1995. As everybody knows, Duncan is smart and hard working. He has been one of the most influential mathematical social scientists of his generation.

He is compulsive in two ways, both of which are almost necessary for a really successful scientific career. How is he compulsive? First, he is the best person I have ever met at meeting deadlines. My own list of collaborators is long and no one in that list is his equal. He simply gets the job done in a quicker and more thorough fashion than anyone else I know. Secondly, he is also compulsive about making headway on a problem once he has decided to work on it. He does not give

up easily. If he knows that there is something to be done that should be done and that is important for a given area, whether it is a very general theorem in the theory of measurement or new axioms for choice behavior in preference situations, you can count on Duncan to stay with the matter for a very long time to get something that is workable and useful.

Third, he is stubborn. He has very definite ideas of his own and he is not easily persuaded that other ideas are better. This is, it seems to me, again an excellent quality for a good scientist to have, although it may not be the best quality for a parent or spouse. I will say that Duncan is willing to listen and is not so stubborn as to hold out against all odds.

A fourth quality is that he has had a large number of collaborators, many of them former students, working with him. This extensive network has amplified considerably the total amount of work he has done, especially in the experimental direction. It is my impression that his collaborators have found Duncan highly constructive in his approach to joint work, even if he is at times demanding and impatient at the lateness of delivery of work by others. He really understands well the psychology of collaboration, he brings a lot to the table, and he has the capacity to appreciate what others are offering as well. These to me are the important characteristics of someone who has a long and successful career of collaboration with many different kinds of colleagues.

Finally I will mention that he has been a good and loyal friend over many years. It has been an honor and a pleasure to work with him and to enjoy his friendship for more than four decades.