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Review

Otis Dudley Duncan, quantitative sociologist par excellence: Path analysis, loglinear methods, and Rasch models

Leo A. Goodman

Department of Sociology and Department of Statistics, University of California, 410 Barrows Hall, Berkeley, CA 9470-1980, United States

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Abstract

This article has two main purposes. The first main purpose is to take note of some of Dudley Duncan's major contributions as a quantitative sociologist, as the leader in introducing path analysis and related structural equation models to sociology, as a pioneer in applying loglinear models and related models in many different sociological contexts, and as the leader in introducing Rasch models to sociology. The second main purpose of this article is to show the reader something of Dudley Duncan, of the person and his character.

Keywords: Path analysis; Structural equation models; Loglinear methods; Latent class analysis; Rasch models

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1. Introduction

Dudley Duncan and I arrived at the University of Chicago as assistant professors at about the same time a very long time ago. Dudley arrived there in 1951 in time for the beginning of the academic year 1951–1952, and I arrived there one year earlier. We were colleagues from the time when Dudley arrived in 1951 until the time when he left the University of Chicago in 1962 to take up work at the University of Michigan; but our colleagueship and friendship really took hold in the early 1970s, while Dudley was at the University of Michigan, then while he was at the University of Arizona, then while he

E-mail address: lgoodman@berkeley.edu.

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was at the University of California at Santa Barbara, and then, lastly, from the beginning of his retirement in Santa Barbara until his death in 2004. Our colleagueship and friendship grew in closeness over this very long period of time. I shall report here on some of Dudley's work as a quantitative sociologist, and I shall also describe here some of the experiences that Dudley and I shared over the years.

This report will take note of some of Dudley's work in three of the areas in which he made major contributions, during three different and distinct periods in his life, when he was (1) the leader in introducing path analysis and related structural equation models to sociology, and then (2) a pioneer in applying loglinear models and related models in many different sociological contexts, and then (3) the leader in introducing Rasch models to sociology. By also including in this report a description of some of the experiences that Dudley and I shared, I intend to show the reader something of Dudley, of the person and his character, as well as of his work.

With the intention of showing the reader something more of Dudley, the person and his character, I also make use of his own words in this report, frequently quoting from his writings. And, near the end of this report (in Section 5), I include there a humorous overview of Dudley's work and his impact, which he sent me a few years before his retirement. I also include, at the end of this report, a brief description of some of his activities and accomplishments during his retirement.

Earlier in this introductory section and in the title of this report, I refer to Dudley as a quantitative sociologist, but I have refrained from describing him as a "methodologist," despite the very important role that he has had in connection with three important methods of analysis [viz., (1) path analysis and related methods using structural equation models, (2) methods using loglinear models and related models, and (3) methods using Rasch models]. I have refrained from describing him as a "methodologist" out of consideration for the fact that there have been times when he has objected to being called a "methodologist." In 1974, he wrote about his contributions to methodology as follows:

> "I have myself written a good deal about methods. But I have not contributed notable advances in sociological methodology. My writing, when I have . . . [written] 'methodology', has been expository of methods that I learned about from others. I have been interested in applying good methods to research problems and in communicating what I have learned about methods to others. I would be happy if I could surmise that my influence in this respect was more beneficial than harmful, or if I could suppose that my research is noteworthy for its attempt to use methods appropriate to and effective for solving the problems under study."

The above comment by Dudley is a part of a longer statement by him, which was published in the *American Sociological Association Footnotes* (Duncan, 1974). Before it appeared in print, I was invited to comment on Dudley's statement, and my comment was published together with his statement. I argued then for a broader definition of "methodology." Here is a brief excerpt from my comment: "Empirical social research [needs, of course, to apply] appropriate methods, and sociological methods have merit to the extent that they contribute to the improvement of social research. In view of this intimate connection between social research and methods, I would like to recommend that, in the future, . . . [the purview of 'methodology' should be broadened to include] empirical social research [noteworthy for its use of methods effective for solving the problems under study] and/or the advancement of methods that can facilitate such research." I also stated in my comment that "with respect to Duncan's comment about his own work, I think that . . . Duncan's research work is outstanding, and it has greatly advanced the methods now available for sociological research . . .."

I am pleased to note here that, a few years after the 1974 statements were published in the ASA *Footnotes*, the American Sociological Association invited Dudley to receive the Samuel A. Stouffer Award in Methodology, for "sustained contributions to the advancement of sociological research, especially in the measurement and analysis of social change." He accepted this award.

Before closing this introductory section, I will include here some comments by Dudley on the achievements of "great men" and on an important early intellectual influence on him: "From boyhood . . . I have had a tendency to idolize fine scientists as 'great men,' though I hope not in an uncritical way. It is just that there are orders of achievement that I feel I can never hope to comprehend, but [that I can] at best . . . appreciate." When considering people with whom he had had significant contact, Dudley also noted that he regards W.F. Ogburn as "something of an approximation" to a "great man," and he considers Sewell Wright a "great man." (Wright was one of the three major founders of modern population genetics; his invention of path analysis was just one of his many contributions to that field. Ogburn was described, at the time of his death in 1959, as "the last great social scientist who wished to know it all." His tireless advocacy of measurement, his writings on social change and culture, and

his insistence on the verification of social theories using quantitative methods, helped to move the emphasis in sociology at that time toward the development of a more exact science of social phenomena.) Dudley wrote his Ph.D. thesis under Ogburn at the University of Chicago, and he wrote about Ogburn's work in "An Appreciation of William Fielding Ogburn" (Duncan, 1959), which was published in *Technology and Culture*. Dudley also was the Editor of *William F. Ogburn on Culture and Social Change*, a volume consisting of some of Ogburn's writings selected by Dudley with an introduction by him (Duncan, 1964), which was published in the *Heritage of Sociology* Series by the University of Chicago Press. Ogburn was, in many ways, a role model for Dudley. Dudley has stated that all of the ideas of Ogburn that are summarized in Dudley's two essays (cited above) were influences on him, in one degree or another, and he also added the following comment on this matter: "Had I been closer to [Ogburn] in a personal sense, I might have become his disciple; as it was, I was his student and am no one's disciple."

I now end this introductory section by noting that, in my view and I expect in the view of others, Dudley was the most important quantitative sociologist in the world in the latter half of the twentieth century.

2. The path analysis period

Dudley's path analysis period began in 1963. According to Dudley, the "first honest path diagram in sociology" was published in an article called "Education and Occupational Mobility," which was written by him and his graduate student at that time, Bill Hodge (Duncan and Hodge, 1963). In commenting later on the path diagram in that paper, Dudley noted that "it was very rudimentary, just a three-variable diagram." By 1964, Dudley was applying non-rudimentary path analysis models in the preliminary examination of the data that he and Peter Blau had obtained for their *American Occupational Structure* research project. By 1966, Dudley's classic article on "Path Analysis: Sociological Examples" was published (Duncan, 1966). And, in 1967, the Blau-Duncan project was completed and their book, *The American Occupational Structure*, was published (Blau and Duncan, 1967). In 1968, Peter Blau and Dudley received the Pitirim A. Sorokin Award from the American Sociological Association for their 1967 book.

In commenting in 1973 on his earlier work on path analysis, Dudley stated that "acquiring the ability to work with path diagrams ... made all the difference in the way Blau and I accomplished the analysis of our ... data. We produced a 'basic model' which, primitive as it was by the standards of, say, econometrics, summed up a great deal of what sociologists were trying to get hold of in analyses of social mobility ... A lot of people [proceeded] to use the model and to modify it for their own purposes ... I long ago lost count of the number of books and papers that take the Blau–Duncan model as a starting point ... I contributed quite a few of them myself, during the six years or so at Michigan while completing the work with Blau and extending it with various other collaborators ..."

One of the collaborations was with the econometrician, Arthur Goldberger. Dudley and Goldberger were interested in examining the relationships between path analysis and the better developed procedures of econometrics and psychometrics, i.e., the causal models (path models) of the type Sewell Wright worked with, the simultaneous equations models of the econometricians, and the confirmatory factor analysis models of the psychometricians. A more comprehensive term that includes all these models is "structural equation models." Goldberger and Dudley were co-editors of *Structural Equation Models in the Social Sciences* (Goldberger and Duncan, 1973), and then Dudley wrote his own book, *Introduction to Structural Equation Models*, on this subject (Duncan, 1975a).

I return now to Dudley's comments in 1973 on his earlier work on path analysis and the related, more comprehensive structural equation models: "What we realize now is that a great many sociological issues that used to be approached in an undisciplined and intuitive way can be formalized for purposes of parameter estimation and hypothesis testing. This allows us economically and quickly to dispose of lots of false leads and bad guesses that formally would have polluted the literature for years on end. We have really achieved a major advance in regard to the amount of detail and level of sophistication that we can handle rigorously. Like all significant developments in the discipline, this one has many of the aspects of a fad. Thus, we can anticipate that a good deal of superficial and shoddy work will be defended on the ground that its results are expressed in the form of a structural equation model. But it is at least a merit of the approach via such models that questionable assumptions and procedures are fairly transparent to the disinterested critic."

From the very beginning of his work on path analysis, Dudley was acutely aware of its limitations as a method for discovering causes. In his classic article on path analysis, published in 1966, he stated that "path analysis focuses on the problem of interpretation and does not purport to be a method for discovering causes;" and in the 1967 Blau–Duncan book, there is the follow-

ing statement: "Sociologists have only recently begun to appreciate how stringent are the logical requirements that must be met if discussion of causal processes is to go beyond mere impressionism and vague verbal formulations. We are a long way from being able to make causal inferences with confidence, and schemes of the kind presented here had best be regarded as crude first approximations to adequate causal models." With respect to the statement quoted above from the 1967 book, it is interesting to note that, almost two decades after the book was published, Dudley repeated this statement verbatim in an article that he was writing on a different subject (Duncan, 1985a), prefacing the statement with the remark that he was reiterating his earlier caveat that pertained to the use of path analysis in the 1967 book.

From the early 1970s on, Dudley no longer applied path analysis or structural equation models in any of the many diverse research projects that he carried out over those years and, in his 1975 text on structural equation models, he omitted all empirical examples. He moved on to other sociological areas of study, to other kinds of sociological data, and to other methods of analysis.

But before we move on to Dudley's next area of study and his next method of analysis, his loglinear methods period, here now is an excerpt from a summary statement that he wrote in 1971 about his contributions as of that date: "My main contribution has been to the problem of measurement in sociology. This is a theme which runs through otherwise diverse [sociological] studies . . . [that I have carried out]. I take 'measurement' to have broad significance, as perhaps Lord Kelvin did in the quotation engraved on the Social Sciences Building at the University of Chicago, '[When] you cannot measure, your knowledge is meager and unsatisfactory.' . . . I consider that I have made contributions [to the problem of measurement in sociology through the various studies that I have carried out], but perhaps most conspicuously in regard to structural equation models ('path analysis') as applied in the area of social stratification and mobility. This work has unified a vast amount of research whose structure was heretofore obscure, has provided instructive estimates of relationships hitherto suspected but not quantitatively evaluated, and has stimulated a very considerable number of currently ongoing projects widely scattered about this country and elsewhere in the world . . .."

Let us return now, for a moment, to the last sentence in the preceding introductory section, where I had noted that Dudley was no one's disciple, but he *might* have become the disciple of William Fielding Ogburn. With respect to the Lord Kelvin dictum quoted above by Dudley, I think it is interesting to note here that it was Ogburn himself, way back in 1929, who had selected that dictum to be carved on the Social Science Research Building. At the time when the building was being designed and constructed in 1928 and 1929, Ogburn was the Chairman of the Committee on Symbolism, and one of the responsibilities of the Committee was to select a relevant quotation to be carved on the building. His favorite quotation from the beginning was by Kelvin, but it was too long to fit the space allotted for it. So Ogburn trimmed the statement to fit the space and also to fit what he saw as the meaning of the statement (Merton, Sills, & Stigler, 1984; Stigler, 2004).

3. The loglinear methods period

Dudley left the University of Michigan in 1973 and moved on then to the University of Arizona, and, roughly speaking, it was during this period of time that he left his path analysis and structural equation models and moved on then to the loglinear methods period. I will now report on how the move from one methods period to another methods period took place.

During the period from 1972 to 1975, Dudley and I served on the Advisory Committee to the Social Science Research Council Center for the Coordination of Research on Social Indicators. When the Advisory Committee was formed in 1972, he was appointed its Chair, and he asked me to join him on the committee, which I did. The meetings of the committee took place in Washington, DC, and during the time when Dudley and I were in Washington for the first meeting of the committee in early 1972, in a conversation that he and I had about a new set of data that he was interested in analyzing, I suggested to him that he should try to analyze that particular set of data using the methods of research that I had been developing. He then took up this suggestion in earnest.

He studied some of my earlier articles on the analysis of occupational mobility tables and on the analysis of other kinds of cross-classified categorical data, and he sent me his questions and comments on that material. I then responded, and I also began to send him, one at a time, drafts of papers that I was writing at that time. He would then study these drafts and would send me his questions and comments on this material, and I would then respond to his questions and comments, and I would also take them into account in the exposition in the final versions of those papers. He and I proceeded in this way in 1972 and throughout his loglinear methods period. Also, starting in 1972 and continuing in 1973 and 1974, Dudley was using the various methods described in those papers to analyze various sets of data in which he was

interested, and in this way he was becoming an expert in the use of those methods.

In 1974, Dudley and I were informed by the American Sociological Association (ASA) that we were selected to share the Samuel A. Stouffer Methodology Award. When I heard this news, I felt pleased and honored and I especially liked the idea that Dudley and I would share the award. Dudley had a quite different reaction to this news. He wrote a description of some of my research work – a much better description of this work than I would have been able to write – and he included this description in an open letter in which he turned down the award. Dudley's open letter was published in the ASA *Footnotes*. An excerpt from his statement was included in the introductory section earlier herein, and now here is an excerpt from a different part of his open letter:

"It would be a great honor to share the award with Leo Goodman, but . . . I feel strongly that Goodman should be the sole recipient of this year's award, and that his honor should not be diluted . . .. [His] contributions to methodology are too many to enumerate, but it is possible to characterize them briefly and unequivocally, mentioning some outstanding examples:

"His work on methods for analyzing mobility tables solved a problem that had plagued research workers in this field for at least two decades . . .. In solving this problem, he rendered a substantial corpus of previous work. . . obsolete—no softer word will do.

"Goodman's collection of models for survey analysis does somewhat the same thing, but in an even more striking manner. He has provided *for the first time* a set of statistical methods that are adequate to the tasks posed by the 'language of social research' hitherto associated with the Columbia school and kindred approaches to survey analysis. The practiced user of Goodman's methods can accomplish with ease everything that this school attempted, and a great deal more . . .. It is . . . no doubt portentous that almost any complex body of data previously analyzed by even a skilled practitioner of survey analysis yields different conclusions by Goodman's methods . . . It is easy, moreover, to see after the fact how the practitioner fell into . . . error . . .. Many survey researchers are not yet aware of the magnitude of the revolution that Goodman's methods are producing . . ..

"In similar fashion, Goodman has put panel analysis on a sound footing for the first time and, as a consequence, we can now ignore a substantial body of misguided literature that provided erroneous, misleading, or merely useless procedures for manipulating panel data.

"In his most recent contributions, . . . Goodman has provided a substantial statistical foundation for the latent structure model of Lazarsfeld and the scaling model of Guttman. It is notorious that for the 25 or 30 years that these models have been discussed and applied . . . the estimation and testing procedures suggested for the models by their inventors and employed, *faute de mieux*, by research workers [have not been satisfactory] . . .. The statistical problems had defeated . . . some very eminent statisticians. Now, thanks to Goodman, [by using the methods presented in his statistical foundation] we can begin to understand correctly what is at stake . . .. Prestidigitation will no longer suffice as a legitimation for some *ad hoc* procedure . . ., nor will incantation of a rule of thumb . . ..

"It is characteristic of Goodman's best work, then, that it *solves* problems. . .. Moreover, it solves problems that are *important*. . .. Finally, it solves the problems in a *definitive* . . . *way* . . .. The solutions actually supersede and do not merely compete with previous procedures, recipes, and rules of thumb . . .."

I mentioned earlier herein, that, starting in 1972 and continuing in 1973 and 1974, Dudley was becoming an expert in the use of the methods that he referred to in his 1974 statement (quoted above) about my work. During this period of time, he and his wife Beverly were also beginning to use these methods to analyze many different sets of data that were part of their joint research project on *Sex Typing and Social Roles*. In 1975, they published *three* articles in which they applied the methods for analyzing survey data to some of the sets of data in their project: Two of the three articles were written by Dudley (Duncan, 1975b, 1975c), and one was written by Beverly and Mark Evers (Duncan and Evers, 1975). Dudley later referred to his 1975b article and to the article by Beverly and Evers as "among the first applications [of these methods] in sociology."

Also in 1975, Dudley wrote a review of a 1973 book on *Panel Analysis*, whose author, someone who was associated with the Columbia school, had applied Columbia school latent class methods to analyze different sets of panel data. In his review (Duncan, 1975d) of the book, Dudley used what he calls "Goodman's algorithm" for latent class models to reanalyze several sets of panel data that were analyzed in the book, and he noted in the review that he had obtained conclusions that are quite different from those in the book. But he did not fault the author of the book. Instead he stated that the author "cannot be faulted for not having made use of this method, since the publication of Goodman's algorithm postdates that of the work under review. Nevertheless, in the light of Goodman's contribution [published in 1974], *Panel Analysis* will have to be reworked from beginning to end before any of its empirical results can be taken seriously

. . .. It would, I think, be in the spirit in which [the author] offers his work to accept it only as a point of departure for more serious investigations."

And also in 1975, for the *Social Indicators Newsletter*, Dudley prepared a statement on "The Statistics of Leo Goodman" (Duncan, 1975e). This was prepared at the request of the Social Science Research Council Center for Coordination of Research on Social Indicators, "in the interest of encouraging social indicators researchers to acquaint themselves with the measurement and analytic techniques developed by Leo Goodman." Dudley's statement included excerpts from his open letter in the 1974 ASA *Footnotes*, and it also notes that "work [by Goodman] on a large number of other topics [not included in the 1974 open letter] could be mentioned. Research workers in the social indicators field would perhaps be especially interested in [Goodman's] papers on contingency table analysis, time series analysis, Markov processes, and demographic models . . .."

During the time period from 1975 until 1983, Dudley wrote many articles, and he also wrote a book with his wife Beverly, *Sex Typing and Social Roles* (Duncan and Duncan, 1978), applying various methods of analysis that had been referred to in his 1974 open letter that had described some of my work. In addition, some of his articles also developed further some of the statistical techniques to which he had referred in the open letter. And during the same time period, while he was working at the University of Arizona, he regularly taught a course that he described as his "specialty of statistical methods of survey analysis." I am sure that it was an excellent course. He was an extraordinary student of this subject, and he turned himself into an expert in and a valuable contributor to the field. He also turned himself into someone who was well versed in the literature of the other experts in the field (not just well versed in my work).

In 1981, Dudley also wrote a very interesting and thorough review (Duncan, 1981a) of a 1979 Festschrift in honor of Paul Lazarsfeld that was edited by Bob Merton, Jim Coleman, and Pete Rossi. The Festschrift consisted of articles by various authors, including one article by Jim Coleman and one by me. In commenting on the analysis of some panel data in Coleman's chapter, Dudley stated that, when data of this kind are analyzed now, "the whole cafeteria of causal models for panel data and statistical tests of hypotheses about such a data structure (Goodman, 1978, chapter 6; Duncan, 1981a, 1981b) becomes relevant." And, with respect to my chapter in the Festschrift, Dudley wrote as follows: "The only chapter of this book that deals systematically with latent structure analysis – surely one of Lazarfeld's big ideas – is Goodman's explication and illustration of models of . . . latent structure . . .. This [chapter] compactly summarizes a substantial development of statistical methods that Goodman [introduced earlier]. It provides a valuable review (and some extensions) of his earlier work [on this subject] . . .. We are finally in a position to exploit latent structure models in serious empirical research . . .."

And in 1982, Dudley wrote a "Review Essay: Statistical Methods for Categorical Data" (Duncan, 1982), which was a very thorough review of my book on *Analyzing Qualitative/Categorical Data* (Goodman, 1978). He also wrote a very thorough Foreword (Duncan, 1984a) for my book on *The Analysis of Cross-Classified Data Having Ordered Categories* (Goodman, 1984), which ended with the following remark: ". . . I do want to urge the reader to persevere in the study of these remarkable contributions to statistical methods. Few efforts will be more richly rewarded."

Before closing the present section, I return now, for a moment, to Dudley's comments on his contributions to methodology, which I quoted in the introductory section herein. In those comments, Dudley stated that he would be happy if he could suppose that his research "is noteworthy for its attempt to use methods appropriate to and effective for solving the problems under study." In my view, his research does satisfy this criterion. Many examples of this could be given. For the time period covered in the present section, I will report here on only one of these examples, namely, Dudley's article on social roles and indicators of sex typing (Duncan, 1979).

This article analyzes survey data obtained in a study that was carried out three times within a 23 year period. In each of the three surveys, a sample of mothers was obtained, and the data were the responses by the mothers to four questions on the assignment of household tasks to boys and girls. Each of the four questions asks the respondent whether task X should be done as a regular task by a boy, by a girl, or by both. Task X is different on each of the questions. The "traditional" response would be "boy" on the first two questions, and "girl" on the last two questions, and the "egalitarian" response on a question would be "both." (For more details about the survey data, see the 1979 article.) There are two kinds of respondents who are entirely consistent—those who give a *traditional* response to the four questions and those who give an *egalitarian* response to them. By applying a special latent class model to the survey data, it is possible to estimate what is the proportion of "ideological" respondents and the proportion of "situational" respondents among those who give an entirely consistent *traditional* response; similarly, it is possible to estimate the corresponding proportions among those who give an entirely consistent *egalitarian* response. The terms

ideological and *situational* do not refer to the directly observable features of the response to one item or the pattern of responses to all four items. The terms refer to two latent classes in the special latent class model. (This is an example of what I refer to as "statistical magic"—a term that I use in an approving sense.) With respect to the special latent class model, Dudley notes that it "has not been described hitherto, although it is related to models suggested by Goodman (1974, 1975, 1979)." The three articles cited here by Dudley are all concerned with latent class models, but each article deals with a different aspect of the subject, and, in his own ingenious way, Dudley is able, in his article, to make use of the results, presented in each of the three cited articles, pertaining to the three different aspects of the subject. He is a complete master of all of this material. Dudley's article can serve as an excellent example, just one of the very many such examples, in which his research uses "methods appropriate to and effective for solving the [particular] problems under study."

4. The Rasch models period

Dudley left the University of Arizona in 1983 and moved then to the University of California at Santa Barbara. A year or so before leaving the University of Arizona, he began his study of Rasch models. His first work on this subject was "Rasch Measurement and Sociological Theory," the Hollingshead Lecture, delivered at Yale University in 1982. During the decade from 1982 to 1992, he wrote many articles on this subject, which were published in various journals and as chapters in books. It was also during this period that he wrote *Notes on Social Measurement* (Duncan, 1984b), which also includes material on Rasch models.

During a later period in Dudley's life, very close to the end of his life, when he was considering the very large body of work that he had created over his lifetime, he wrote that, in his own estimation, his "best book, the only one likely to be of enduring and not merely historical value, was *Notes on Social Measurement*," and he stated that he was also proud of his "most fully developed mathematical–theoretical article ["Probability, Disposition, and the Inconsistency of Attitudes and Behavior"], published [in 1986] in a specialized journal, *Synthese*, dealing with epistemology and methodology." He described this article as follows: "It presented a solution of a problem that had vexed some of the leading social scientists of the time . . .: Why do people's verbally expressed attitudes so often seem unrelated to their actions?" As I noted in the preceding paragraph, the Rasch model has its place in his 1984 book on measurement, and this model also has a central role in Dudley's solution to the problem considered in the 1986 article.

The 1986 article was concerned with the solution to a particular problem, the inconsistency of attitudes and behavior; but Dudley's exposition in that article could be directly translated by the reader in a way that would show him/her how the basic ideas of Rasch measurement could be applied in other contexts. In Dudley's article, he writes as follows: "Inconsistency of attitudes and behavior is due to the probabilistic connection between responses or actions and the (not directly observable) dispositions on which they depend. Latent variable [Rasch] models provide criteria for recognizing when attitude and behavior depend on the same disposition." He also notes that "the statistical methods deployed in the investigation are borrowed from the literature on log linear models for categorical data . . .." Here are some other excerpts from the 1986 article: "The strongest demand I have made [in this article] is that we take seriously the premises that human responses and actions are 'outcomes of irreducibly probabilistic processes' . . . and that the probabilities are not necessarily the same for any two persons. This demand arises as a scientist's hunch, conditioned by his inability to make sense of pervasive features of the data without these premises . . . I have embedded the premises in a discussion which further assumes a 'world of dispositions' . . . wherein an individual . . . will acquire . . . innumerable complex tendencies to respond to specific kinds of environmental . . . [conditions] with specific kinds of outcome behavior. But 'outcome' is related to 'tendency' in . . . a way . . . like . . . randomness . . . [enters] into actions; for example, if causes were identified only probabilistically . . .. When a process works the way the [Rasch] model says it should, the model explains how the process works. Among the important features of these models, of great interest when they do apply, is that they provide a separation . . . of effects due to differences among persons and those due to differences among 'items,' [where the meaning of the term 'items'] should be broadened [here] to take into account not only the wording of questions, but . . . any relevant features of the social situations in which responses are obtained or behaviors are recorded. No adequate illustration can be brief, so I must beg the interested reader to refer [also] to my other articles on this topic."

Dudley's 1986 article includes references to two other articles by him, namely, Duncan (1985a, 1985b). In each of these articles, the Rasch models are applied in the analysis of panel data. Both of these articles are outstanding. But I will report here on only one of them, Duncan (1985a).

This 1985 article sheds new light on the analysis of panel data in which each member of the panel is observed twice (thereby obtaining a two-wave panel) on two dichotomous variables (thereby obtaining data that can be described in the form of the so-called 16-fold table). Panel data of this kind, which were analyzed earlier by Lazarsfeld (1972), Coleman (1964), Berelson, Lazarsfeld, and McPhee (1954), Rosenberg (1955), Zeisel (1968), Hagenaars (1978), and by others, are reanalyzed in this article; indeed, new light is shed on these data. As was the case in Dudley's 1986 article, and also in his other articles in which Rasch models are applied, "the statistical methods deployed in the investigation are borrowed from the literature on log linear models for categorical data."

In this article on the 16-fold table, Dudley presents a whole cafeteria of Rasch models, thirteen in total, that can serve as possible hypotheses pertaining to the 16-fold table. In addition, he states that "the more important parts of the analysis developed from this theory [the Rasch latent-trait measurement theory] can be carried out by elementary methods and therefore require no great mathematical or statistical sophistication;" and he begins his analysis of the 16-fold table by first presenting his "elementary analysis" in one section, followed by a section on the "latent-trait [Rasch] model."

In the "elementary analysis" section, Dudley analyzes the 16-fold table by applying to these data, in his own ingenious way, the general concept of "quasi-independence" and the general method for partitioning the two-way table into its sub-table components. He refers the reader in this section to Goodman (1968) in which the general concept of quasi-independence, and the general method for partitioning the two-way table, are presented. Dudley also refers in this section on his "elementary analysis" to two other articles written by me (Goodman, 1962, 1969) in each of which there are different results that he applies in an effective manner. Dudley is the complete master of all of this material. The particular methods described in his "elementary analysis" section are applied by him in his analysis of 16-fold tables on, for example, the relationship between current drug use and depressive mood (examined in two waves about seven or eight months apart), on the relationship between the respondent buying or not buying a product that was advertised on a television program and the respondent having viewed or not viewed that program earlier (examined in two waves 4 months apart), on the relationship between the respondent (an undergraduate student) having a people-oriented or non-people-oriented preferred occupational choice and the respondent having people-oriented or non-people-oriented values (examined in two waves 2 years apart), and on various other 16-fold tables.

As I noted earlier, the "elementary analysis" section is then followed by a section on the "latent-trait [Rasch] model;" and these two sections are then followed by sections in which the thirteen different Rasch models pertaining to the 16-fold table are presented, and these different models are then applied, when appropriate, in the analysis of particular 16-fold tables. This 1985 article can also serve as an excellent example, again just one of the very many such examples, in which Dudley's research uses "methods appropriate to and effective for solving the [particular] problems under study."

Dudley's last graduate student, Magnus Stenbeck, who was also Dudley's last co-author on several of the Rasch models articles, wrote an article about Dudley for the *Encyclopedia of Social Measurement* (Stenbeck, 2005). He commented there as follows on Dudley's work on Rasch models: "Fifteen years after the conclusion of Duncan's work in this area, relatively little has come out of his efforts. Few followers seem to be active within the social science community. This stands in stark contrast to Duncan's strong impact on other fields of quantitative social science. At that late point in his career when Duncan devoted his work to Rasch measurement, he was in no position to organize ... [more research projects] with the Rasch model in mind." If Dudley had run into the Rasch models at an earlier age, I expect that he would have continued to develop further this area of study, the number of researchers who would have followed him, in the further development of Rasch models and their application in the social sciences, would have grown, and he would have had a strong impact in this field too.

Considering Magnus' comment above, it might be of interest to take note of the fact that the Inaugural Otis Dudley Duncan Lecture, presented at the 2004 Conference of the American Sociological Association Methodology Section, was on Rasch models—"Three different ways to view cross-classified categorical data: Rasch-type models, log-linear models, and latent-class models;" and a Keynote Lecture, presented at the 2005 Conference on Latent Variable Models in the Social Sciences, was also on Rasch models—"Some simple latent structure models, based on Rasch-type latent-trait models and on latent-class models, for the analysis of cross-classified categorical data." The 2004 and 2005 lectures were presented by me.

Since Dudley's Rasch models work is not as well known as some of his other work, I will present a list of his papers related to this subject later herein in the Appendix. (This list is a bit more complete than an ear-

lier list that was included with the 2004 Otis Dudley Duncan Lecture.)

5. A humorous overview of Dudley's work and his impact

Sometime in the mid-1980s, sometime before his retirement began, Dudley sent me a document that I will include in this section. He did not remember who wrote it. In commenting on it, he wrote as follows: "It is fun, but some may not find it funny as I do." And he asked me not to circulate it. I think that he may have been concerned, at that time, that someone might feel offended. But now, more than 20 years have gone by, and three out of the four people referred to in this overview (namely, Paul Lazarsfeld, Georg Rasch, Dudley, and me) are no longer alive. So here now is the overview, as it was presented to me by the "Prophet Dudley:"

For lo these many years the People were dwelling in the Land of Columbia with their wives and camels and computers. But the zero order correlations came upon them and the People were sore afraid. So they spake unto the Prophet Paul saying "Canst thou relieve us of this plague of zero order correlations?" And the Prophet Paul told them to buildeth tables of percentages and the correlations would go away. And the People did as he said and they builded many tables (and verily some obtained chairs) and some of the correlations went away. And the People of Columbia waxed fat and had many wives and many concubines.

And it came to pass that the Prophet Dudley arriveth from the sands of Oklahoma. And the Prophet Dudley spake unto the People saying, "Repent, ye sinners! Thou worshipeth false Gods!" And he told the People they should follow him down the recursive path, nigh unto the Land of Econometrics, whence he had been before them. And the People packed up their wives and camels and computers and followed him (as best they could) and the assistant professors found that many books could be written elucidating the miracle that Education is correlated with Occupational Prestige (when Prestige is estimated from Education) and the assistant professors became full professors, and the residual variance waxed while the man and maidservants decomposed covariances, and the journals were filled with graven images of what appeared to be road maps of Bulgaria. But the People cried out, "We know not what this Sociology is and can not read our journals nor yet find out any information about Society." But the Prophet and his disciples said, "Oh, ye of little faith, trust us." So the People stopped reading the journals and devoted themselves to Marxist agitation and all was well in the Land. But the remnant who tarried in Columbia were punished and the Lord smote the Bureau of Applied Social Research to show he/she was not kidding.

And it came to pass that the Prophet Leo came down from the clouds and saith, "Moses gave you tablets of clay, but I giveth unto you Tables of Counts plus plenteous notation." And the Prophet Dudley was moved, and he spake unto the People saying "Repent, ye sinners! Thou worshipeth false Gods!" and "Abandon ye those comfortable interval scales and follow me on the road back to tables." And the People despaired and they cried out, "Hey, we just came from there and it was a long walk." But the Prophet Dudley said, "Ye speaketh of tables builded from percentages; I speaketh unto you of tables builded from Logs, as told to me by the Wizard of Odds." And the People said, "Oh, I see, I guess" and they packed up their wives and camels and computers and they followed him (as best they could) back to the Land of Tables. And somewhat fewer assistant professors became full professors because it was a time of drought, and many fewer of the People could read the journals nor yet find out information about Society. But the Prophet and his disciples said, "Trust us, we were wrong about those covariances but we got it right this time." And those that dwelt in the Land of Sociology were known as odds fellows and all was well.

And it came to pass that the Lord spaketh to the Prophet Rasch and the Prophet Rasch spaketh unto the Prophet Dudley of local independence and the Prophet Dudley was moved. And he spake unto the People saying "Repent, ye sinners! Thou worshipeth false Gods!" And he saith unto them "Abandon ye not only regression but also causality and controls. Bow ye down before the mighty, invisible, unseen variable!" And the People saith, "I don't see any invisible variables out there, but anything to get away from those damn logarithms" and the wives and camels creaked to their feet and said, "upsy daisy, here we go again."

6. The period from the beginning of retirement until the end

In the beginning period of his retirement, Dudley spent his time in the intensive study of Just Intonation (a theory of tuning based on pure intervals), on which topic he published several articles. And he composed many pieces of music with computer and electronic instru-

ments based on that theory of tuning, creating a body of work amounting to an archive of some 20 CDs.

Having completed his explorations at the frontiers of music, Dudley turned to computer graphics and created hundreds of abstract images and designs. Then he returned briefly to quantitative research using online data sources, and he wrote articles on quite a few topics, for example, on the prevalence of creationism, the rising public toleration of atheists, the increasing number who specify "none" as their religion, the increasing public approval of euthanasia and assisted suicide for terminally ill persons experiencing great pain, and on some controversial statistics regarding gun use.

This work on quantitative research was carried out after Dudley and his wife, Bea Farwell (whom he had married in 1994, his first wife Beverly had died in 1988), had joined the Humanist Society of Santa Barbara. The social contacts provided by this organization were of great value to Dudley as a support for his writing and his thought during this time.

Dudley died in 2004 after struggling with advanced prostate cancer for 2 years. During this last period, he was also plagued by various other medical problems, and he experienced great pain at times. Nevertheless, in the months preceding his death, Dudley repeatedly proclaimed as his motto the stage direction, "Exit, laughing." He did what he could to adhere to this principle.

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Appendix. The Otis Dudley Duncan papers on Rasch models, 1982–1992

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Duncan, O. D. (1983). On a dynamic response model of W.F. Kempf. *Social Science Research*, *12*, 393–400.

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