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G. De Soete, H. Feger, and K.C. Klauer, Eds., *New Developments in Psychological Choice Modeling*, Amsterdam: North-Holland, 1989, pp. 356.

This book consists of 15 papers. Some of them have already been published in a 1988 special issue on unfolding of the *Zeitschrift für Sozialpsychologie*, and some have been published in a 1987 special issue of *Communication and Cognition* on probabilistic choice models. Since both journals are not very widely available, and both fields are obviously related, it was decided to bundle revised versions of the papers in a single edited volume. I think that was a good idea. The book gives a good overview of the field of psychometric choice modeling. It is useful to have such an overview, because it clearly shows that at least part of the field is somewhat of a mess. The book costs $105, which is $7 per paper. Almost nobody will buy it. Of course the publisher has taken this into account.

I shall start this review with a remark about the area covered by these papers, and then I shall briefly discuss them all individually. All papers pretend to discuss choice modeling. Some of them use unidimensional and some use multidimensional models. Some of them are probabilistic and some are deterministic. But these are not the basic distinctions. It is more fundamental that some of them illustrate the famous psychometric law that everything that one can think of can also be turned into a computer program. I converge, therefore I exist. The field keeps generating more and more general techniques, as a sort of automatism, ignoring the fact that people have long ago stopped using the more simple versions, because these were already much too complicated. Other papers stick much more to the basic Thurstonian and Coombsian models, sometimes with slight extensions, and figure out how well these normative models fit actual choice behavior. I think this approach to error is much more fruitful than the approach of drowning it in the parameter sea. The distinction is the same one, by the way, that made people give up the Spearman model in factor analysis and turn to multiple factor analysis, because that gave a better fit. They did not realize that almost all the theory, all the stability, and all the cumulative functions of the normative model disappeared at the same time.

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Heiser's paper is firmly in the multidimensional scaling approach to choice modeling. He tries to breathe new life into nonmetric unfolding, a technique that never really got off the ground because of great computational difficulties. The solutions that come out of the algorithms are often partially or completely degenerate, in the sense that they only show a single dominant qualitative and uninteresting aspect of the data. For instance that one preference is smaller than all the others. Heiser tries to solve these computational difficulties by imposing smoothness conditions on the pseudo distances, basically by bounding the distance between successive values. He then applies his program to two examples, and shows that it does not produce degenerate solutions. There is some interesting historical discussion in the paper, but it is quite unrelated to the main contribution, which is algorithmic.

Feger also discusses unidimensional and multidimensional unfolding. In the true Coombsian tradition he points out the ordinal constraints the model imposes on the data, and he mentions some heuristics that can be used to find exact solutions if they exist. The paper looks curiously outdated, and the only reference is to Coombs' book.

DeSarbo and Rao discuss GENFOLD. The technique combines internal and external, conditional and unconditional, metric and nonmetric, weighted and unweighted unfolding. The paper describes the model briefly, the algorithm in detail, and it has its perfunctory example. This type of paper drives the interested researcher crazy, because it does not provide any useful guidelines. It is like presenting a seemingly sophisticated, glittering product, without telling anybody what the product is actually good for.

Bossuyt and Roskam present a new probabililistic unfolding procedure. Probabilistic techniques are sometimes presented as having major advantages over deterministic (scaling) techniques, because it is possible to estimate parameters by imitating maximum likelihood loss functions. The authors present their theory in axiomatic form, but a brief perusal of the axioms and theorems shows their triviality. The mathematics is almost completely hollow, because it stays entirely on the level of definitions.

Croon's paper is more interesting. It ably reviews one-dimensional choice theories for rankings (which have a great deal of arbitrariness, especially if the number of objects that is ranked is larger than, say, three or four). Croon then goes on to make the choice models multidimensional by using latent classes, and by computing maximum likelihood estimates by the EM algorithm. Of course latent class analysis, even in the case of binary outcomes, is a problematic technique, because of the very flat likelihood functions. Thus again we see the pattern here of making a complicated and poorly understood technique even more complicated. Also the additional rigor provided by using likelihood methods is largely illusory, because they work conditional on the truth of the model and on the infiniteness of the sample. Croon compares his latent class example with a nonmetric unfolding solution, which also illustrates nicely (although unwittingly) some of Heiser's remarks about degenerate solutions.

De Soete, Carroll, and DeSarbo present the Wandering Ideal Point model. The recipe is the same as in the DeSarbo and Rao paper, but there is more information on the model here, and the model also happens to be more interesting. Again choice probabilities are modeled, and thus maximum likelihood type algorithms can be used. There is much emphasis on the algorithm. There is almost no historical context. But there is a burning question after reading this paper. Who is going to use it? And why? Because it's there?

The next paper is by Takane. It is quite different from some of the others, because it is scholarly, it is modest, and computation is deemphasized. Thurstone's contributions are discussed, and it is shown that there is some room between the extremely strong case V assumptions and the extremely weak case I assumptions that can be filled by interesting intermediate models. One of them is the Wandering Ideal Point model of De Soete and others, which looks much more interesting after reading Takane's paper. Takane fits his computations into standard structural equation techniques, which is an interesting exercise.

Carroll, DeSarbo, and De Soete discuss stochastic tree unfolding. This illustrates the degree of predictability of the developments in this field. There has been the tendency, in the past, to extend everything we could do to low-dimensional Euclidian spaces in such a way that we could also inflict it on trees. The paper has a nice (although slightly irrelevant) discussion of the metric properties of trees. It then extends the Thurstonian Wandering Ideal Models to trees. It is not difficult to predict that in subsequent years we shall see generalizations to trees with individual differences, and to arbitrary graphs with path length distance. Very little attention is paid to local minimum and stability problems.

Zinnes and MacKay do not develop anything like a new model, fortunately. They use the Hefner model, which is already 30 years old, combined with the unfolding model, which is even older. The Hefner model leads to a doubly noncentral F distribution for the choice probabilities. Instead of sitting down in disgust after discovering this, as many people have done over the years, Zinnes and MacKay start the heroic task of actually estimating the multidimensional scale values, using various kinds of approximations. They compare their procedure with more classical metric and nonmetric scaling, and discover that metric scaling does quite well.

Bossuyt and Roskam give a general discussion of probabilistic choice theories, and the ways they can be tested. The article is mainly didactic, and generally useful. I happen to think they overestimate the power of the axiomatic method, and of maximum likelihood as well, but I guess they have other prejudices than I have.

Orth gives an axiomatization of an unfolding structure, based on the notion of betweenness, which guarantees the existence of a quantitative J-scale. He then applies his axioms to some data structures to see how far they are justified. This approach can be contrasted with constructing more and more complicated computerized models, which will (by definition) give a better and better fit, and a more and more unstable representation as well. I think it is far more useful, and far more instructive.

Van Blokland also takes the unidimensional unfolding model seriously. There is a nice scholarly discussion of the model, and the best J scale is defined as the scale with the smallest number of violations. Combinatorial procedures are used to find it. Linear programming techniques are then used to construct scale values. And likelihood methods are used to test a particular choice model. Although piecing the three parts of the analysis together is still a problem, again a classical simple model is taken seriously, and applied with a lot of ingenuity to a serious example.

Van Schuur unfolds German political parties.The paper is quite long, and has a rather verbose introduction. Again one-dimensional unfolding is taken seriously, and search procedures similar to those used in Mokken scale analysis are used to construct the unfolding scale. The error theory used by Van Schuur's MUDFOLD is rather mysterious (to me, at least). The example is interesting. One gets the impression that Orth, Van Schuur, and Van Blokland could very well have come to similar conclusions, in the unlikely case that they would have analyzed the same examples. It is clear that such an actual independent comparison would be very interesting.

DeSarbo, De Soete, and Jedidi apply the battery of stochastic multidimensional unfolding and vector models to a small number of analgesics. The different procedures give different results. The authors compare the results by computing canonical correlations. This is typical. If one would perform, say, 15 analyses, then one would even have sufficient output to do an unfolding analysis comparing the outcomes of the 15 techniques. They claim that these techniques are useful, basically because the dimensions could be interpreted, and the fit could be compared by quasi-likelihood methods. I do not think that this is sufficient proof. Moreover they will partly invalidate their own claims by developing even more useful techniques within the next year.

Gaul reviews the Thurstonian model, and the Wandering Ideal extensions. He then applies the techniques to three examples, to indicate that they give useful additional information. We have to be careful here, more careful than Gaul is. It is clear, by definition, that the techniques give more information. But is not at all clear, without a lot of additional study, that this additional information is indeed useful (stable, repeatable, not dictated by peculiarities of algorithm and loss function).

In summary, the quality of the papers is uneven. Some are worth their $7, and some are not. The papers that take the simple one-dimensional choice models seriously seem to be the most valuable ones. Other papers illustrate the current crisis of psychometrics, which I think is caused partly by the fact that the computer has run away with both scientific content and statistical stability.

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