is noticable that Yu. V. Prohorov takes a leading part in the original version of the Encyclopaedia; he is also a member of the translations arrangements committee; in any case he writes a fair number of articles himself. Naturally, the original articles were mainly based on sources originating from Soviet mathematicians. The editorial comments in the English translations are most valuable. So is the extensive cross-referencing and the inclusion of AMS classifications.

Let us look at two typical articles in order to observe some features of the presentation. Under "Bernoulli Random Walk" (BRW) we are first referred to "Bernoulli Trials" where we can learn about the Bernoulli measure, its applications, about references to the laws of large numbers and the central limit theorem. BRW decomposes into geometric representations, probabilities of returning, and maximum deviations. There is a detailed discussion of graphs of three BRWs each of one was observed during 200,000 units of time. As a consequence surprising properties of the walk can be explained. And this is, as it seems to the reviewer, a typical aim of an Encyclopaedia. The rôle played by the BRW within the general theory of random walks (on a group) has not been touched; F. Spitzer's book contains the necessary material and could have been cited here, but will possibly be cited unter "Random Walk". A second example: "Bayesian Approach" is discussed as a problem of statistical decision theory (naming also the disadvantages), later from the empirical point of view (supplementing the lack of a precise form of the apriori distribution). There is an additional article on Bayesian estimates. The entire treatment is mathematical without any ideological engagement; the quoted literature lacks up to date references: the books of J. O. Berger and of J. A. Hartigan (referring also to DeFinetti's work on the subject) should have been mentioned. Also some historical notes on the late Referend Thomas Bayes would have been in order. Here again the spirit of the presentation becomes clear: less history, just a small number of references, but extended mathematical explanation.

It should also be mentioned that the first three volumes are now published and the 10 volume work will be completed by the beginning of 1991.

Tübingen *H. Heyer*

Küsters U: Hierarchische Mittelwert- und Kovarianzstukturmodelle mit nichtmetrischen endogenen Variablen. Physika-Verlag, Heidelberg 1987, XII, 112 S, brosch, DM 49,–.

In this monograph, which is essentially the author's 1986 doctoral dissertation, a very general approach to defining, fitting, and testing econometric and psychometric models with unobserved latent variables and observed categorical variables is discussed. As the author observes, in his interesting historical discussion, his approach combines three different aspects of modeling multivariate data structures. In the first place the ideas from factor analysis, originally due to Spearman, which use unobserved latent variables to model correlation between observed variables. In the second place the classical simultaneous equation models of econometrics, due to Haavelmo and Frisch, with their distinction between endogenous and exogenous variables, and their emphasis on modeling causality. And in the third place the approach of Karl Pearson to model discrete varation by categorizing continuous latent variables. Of course many improvement and extensions of these basic ideas have been studied in the last twenty years. The approaches of factor analysis and simultaneous equation modelling have been combined by Jøreskog, Goldberger, Bentler, Browne, McDonald, Griliches and many others to simultaneous equation models with latent variables. The Pearsonian approach to categorical variables has been combined with classical econometric modelling by Tobin, Maddala, McFadden, Nelson, and others. It also has been combined with factor analysis by Muthen, Christofferson, Lee. Consequently it seems only natural to try an even more far reaching synthesis. This is what the author has tried to do, using the recent very general work of Muthen and McDonald as the starting point, and combining it with the censoring methods studied most thoroughly in econometrics.

In a sense we can consequently be brief about the basic idea of the monograph. Take the structural covariance model of McDonald, used in his program COSAN, take the idea of modelling the continuous latent variables and connecting them to discrete observed variables from the recent work of Muthen, used in his program LISCOMP, and we have a new program which is even more general than its two parents. This is all there is to it, the rest is mostly computational. It turns out that FIML estimation of all the parameters is generally not feasible, and consequently the author discusses limited information techniques based on the first and second order marginals. This is the standard practice, also used by Muthen, Olsson, Lee, and others. It leads to a three-step procedure, in which first the transformation parameters are estimated, then the covariances (assuming the transformations to be known), en finally the stuctural parameters (assuming both transformations and covariances to be known). Of course one loses information in each step, but this is dictated by computational feasibility.

The author gives a fairly detailed and precise study of the properties of his estimators. This is, not surprisingly, tedious and not very interesting. Under the usual regularity assumptions, however, the maximum likelihood estimates are continuous and differentiable F-consistent functions of the appropriate sample moments, and consequently they lead to consistent and asymptotically normal estimators, with a dispersion that can, at least in principle, be computed. The author also gives fairly detailed and precise suggestions about the numerical methods that can be used to optimize his fit criteria, and in the appendices he collects some of the necessary formulae and results. If somebody took the time to write a computerprogram using these suggestions, then (after a year of hard labour, and a great deal of fiddling with run-time parameters) it would undoubtedly work. If somebody took the time to carry out a large Monte Carlo study, with large sample sizes, then (after another year, and some more fiddling) the asymptotic results would undoubtedly be verified as well. These generally positive expectations do not explain the growing feeling of hopelessness that this reviewer had when reading the book.

Do we really want somebody to write this program? I don't think so. Do we really want somebody to carry out this Monte Carlo study? No, thank you. The author has undoubtedly done his job, and he has done it well according to the established criteria. But if somebody were to implement his idea in an actual computerprogram, he would create a monster. Another monster, I should say. I do not think that we really need programs that can do everything that exists, and even more. Statistical techniques are tools for data analysis, tools to help with prediction and explanation of empirical results. Every craftsman can tell you that good tools are necessarily specialized for the job they have to do, and that tools which try to do too many different things don't do anything quite right. I think the same criticism applies to the never ending quest for more and more general multivariate analysis techniques.

The principle of maximum likelihood (or weighted least squares) is of unlimited generality. So are the general purpose optimization techniques. There seems to be no reasons not to combine the three basic components of Küsters' approach with various forms of dependence between the observations, with various experimental designs for the exogenous variables, with various hierarchical structures in the sampling design, and with various forms of nonlinearity in the relationship between the variables. According to the basic philosophy behind this interpretation of progress, the more generality we introduce, the better our computer program will be. Presumably because we can better approach the Truth. Unfortunately, because of sampling errors, slow convergence, local minima, and computational instability, we will most certainly not be able to recognize it.

Los Angeles *J. de Leeuw*

Metrika (1990) 37:63-

Density Estim

By V. R. Prayag[1] ar

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1 V. R. Prayag, Depa
2 Dr. A. P. Gore, De

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