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Understanding Basic Statistics, by Harvey W. Kushner and Gerald DeMaio, Holden-Day, 1980, xiii + 381 pp., $12.95.

Finite Mathematics, A Self-Teaching Guide, by Ronald I. Rothenberg, John Wiley, 1980, xii + 283 pp., $7.95 (paperbound).

Finite Mathematics technically requires only a knowledge of intermediate algebra and is suitable as a supplemental text or a review. The text has chapters entitled "Logic," "Properties of Sets," "Calculational Methods," "Introductory Linear Programming," "Matrix Algebra and Linear Systems," "The Simplex Method of Linear Programming," and "Probability and Applications" (Game Theory and Markov Processes). A substantially higher level of mathematical sophistication is needed for this book than for *Understanding Basic Statistics.*

Understanding Basic Statistics is meant to be used in a one-semester introductory course, or in a statistics-for-social-scientists course for undergraduates or re-entry graduate students. Some of the topics, such as regression and one-way Anova are presented traditionally, and others, such as various types of graphs, percentiles, and association measures (phi, lambda, gamma, etc.) are presented in the framework of behavioral-science application.

Many statistics texts, such as those by Freund, by Huntsberger and Billingsley, and by Mendenhall can be taught by instructors with a good mathematical, but minimal statistical, background. If anything, the reverse is true for this text.

The material in *Understanding Basic Statistics* is covered at a slower pace and a lower level than the preceding texts. More advanced books, such as the one by Snedecor and Cochran, are suggested for the reader wanting more details on a more advanced technique.

The reading style is interesting and includes occasional historical anecdotes. Appendices give lists of symbols and formulas and the page where they were first defined. Problems are generally at the same level of difficulty as the text.

Some general comments indicate the social-science flavor.

Initial instructions for choosing class intervals did not specify ways to choose the number of classes or what to do if the range plus one is a prime number, and so on.

One is shown how to calculate the index of dispersion, and it is never mentioned again.

The treatment of probability does not include the theorem for probability of a complement.

Hypothesis testing is introduced through a binomial example before coverage of the central limit theorem.

A divisor of n is used for s^2.

The F test is motivated by using it to check the homoscedasticity of variances in the two-sample t. (The chapter on the t test is unusual and needs skill in presentation).

In summary, *Finite Mathematics* is a supplemental, but not programmed, guide. *Understanding Basic Statistics* is a well-written, elementary undergraduate text with social-science applications.

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The Enterprise of Knowledge: An Essay on Knowledge, Credal Probability, and Chance, by Isaac Levi, The MIT Press, 1980, xvii + 462 pp., $27.50.

1. *Synopsis.* In *The Enterprise of Knowledge*, Isaac Levi develops a general theory about how we should organize and, when the need arises, revise our knowledge. And he tries to explain statistical inference in terms of this theory.

The Enterprise of Knowledge is an outgrowth of Levi's earlier book, *Gambling with Truth* (1967). In *Gambling with Truth*, Levi explored the idea of using expected utility calculations when we are concerned not with practical decisions, but rather with the problem of deciding whether a certain proposition is true. Levi's idea was that we should assess our utility for the new information we would acquire by correctly deciding that the proposition is true, assess our disutility for the error we would make in incorrectly deciding it is true, and then combine these utilities with our probabilities for its truth and falsehood, thus obtaining an expected utility for the decision that it is true. In *The Enterprise of Knowledge*, Levi develops this idea in two directions. First, he abandons simple Bayesian decision theory for a generalization in which the Bayesian's probability distribution and utility function are replaced by sets of probability distributions and utility functions. And second, he embeds the simple problem of deciding whether a proposition is true in an elaborate framework for organizing and revising knowledge.

The probabilities in Levi's generalization of Bayesian decision theory are to be interpreted roughly like the personal probabilities of the usual theory, except that instead of committing himself to a unique probability distribution, a person may suspend judgment between all the distributions in a convex set, say B. (Levi claims that Ramsey and de Finetti's arguments for additive probabilities are correct but can be interpreted to justify such suspension of judgment. He bases the requirement that B be convex on axiomatic arguments that are sometimes advanced for using weighted averages to compromise disagreements about probabilities.) In addition to B, a person also has a convex set, say G, of utility functions. To decide how to act, he first calculates expected utilities for all possible actions using all pairs (Q, u), where $Q \in B$ and $u \in G$. He eliminates from consideration all actions whose expected utility is not optimal for some pair (Q, u). If one of the actions that remain amounts to refusing to choose among the other actions that remain, then he takes this action. Otherwise he chooses from the actions that remain one that maximizes the minimum expected utility.

Levi's framework for organizing and revising knowledge involves a "corpus of knowledge" and a "credal state." A person's corpus of knowledge consists of all the propositions the person knows to be true, together with their logical consequences. His credal state is a convex set of probability distributions over all the propositions he is uncertain about. When the person adds a new proposition to his corpus of knowledge (either because of a deliberate decision based on the expected utility of deciding it is true or because of a policy that automatically accepts reports from certain sources as true) he changes his credal state by conditioning all the probability distributions in the convex set on the new proposition.

Levi stresses that a person should, at any particular time, be absolutely certain of the propositions in his corpus of knowledge; he is supposed to have a similar commitment to changing his credal state by conditioning. But he is allowed to change his mind on both points. New evidence may cause him to abandon some of the propositions in his corpus, and "contextual considerations" may cause him to change his credal state in ways other than conditioning.

Levi recognizes the fantastic nature of his theory; he admits that no human being could possibly work out the logical consequences of all the things he knows or keep track of probabilities for all the things he is uncertain about. But he considers the theory "normative"; a person should try to follow it insofar as he is able.

In the latter part of his book, Levi discusses his theory's implications for parametric statistical inference. (Nonparametric methods do not seem to fit in and are not mentioned.) To do parametric statistics within Levi's theory, a person needs to have infallible knowledge that his parametric model is correct, and Levi does not say much about how we might come to acquire such knowledge. To obtain useful results, the person also needs a credal state for the parameter that consists of relatively few prior distributions. (If a set of prior distributions is too broad, then the application of Bayes theorem will produce an equally broad set of posteriors.) Levi does make a suggestion about how to narrow too broad a credal state. Consider a prior Q that gives so little probability to a parameter value θ that a person would, if Q were the only prior in his credal state, have positive expected utility for deciding θ is false. Then Q represents, Levi feels, too extreme an opinion to be included in the credal state, and should be left out. If the utilities for correctly deciding θ is false are sufficiently uniform for the various θ, then this turns out to result in eliminating all priors that are very different from a uniform prior.

Levi concludes by using his theory to criticize various other approaches to statistical inference. He rejects Fisher's randomization argument because it cannot be fit into his generalized Bayesian framework. He disinters Fisher's fiducial argument and gives it a couple of new funerals. He forces Kyburg's and Dempster's ideas on upper and lower probabilities into his framework and refutes the results. He discusses Neyman's attempt to replace inductive inference by "inductive behavior." And he criticizes the Rasmussen report on nuclear reactor safety, suggesting that it erred on the side of optimism because it used precise probability estimates instead of more justifiable upper bounds.

2. *Is our knowledge infallible?* Few statisticians will have much sympathy with the thesis that our knowledge is infallible. Statisticians are accustomed to thinking of almost all knowledge as tentative. In particular, they usually accept parametric models in only a provisional way.

It might be thought that Levi's demand that we consider our knowledge infallible is cancelled, or at least reduced to merely an odd way of speaking, by his demand that we also consider it corrigible. But there is a fundamental difference between his view and the view to which statisticians are accustomed. When a statistician finds his model fits the facts poorly, he usually expects to modify the assumptions that had seemed most questionable on the basis of prior evidence. But Levi considers this inappropriate. Once we have accepted the model's assumptions as part of our knowledge, they are all on an equal footing. All are infallible, none are more questionable than others. If we are forced to modify our model, the appropriate criterion for deciding what assumptions to drop is, according to Levi, the utility we attach to the information represented by these assumptions.

Levi is not the first philosopher to advance the opinion that our beliefs have at their base a body of propositions that we accept as all fully and equally certain. C. I. Lewis, for example, based his argument for the infallibility of sense-data on the maxim that "if something is probable, something is certain." Many philosophers have felt obliged to accept this maxim because of their agreement with John Maynard Keynes that probability is a relation between propositions. If a proposition h has probability only relative to another proposition e, called the evidence, then h can be said really to have this probability only if e is known for certain. Levi, though he does not argue for the infallibility of sense-data, seems to accept the idea that nothing can be probable unless something is certain. If nothing is certain, then we will have, Levi feels, no basis for our probable opinions and, in particular, no good reason for changing them. (See p. 15 of *Gambling with Truth* and p. 72 of *The Enterprise of Knowledge*.)

But why must probability be considered a relation between propositions? Why must the evidence on which a probability is based be expressed as a proposition? I believe that there is no good reason to suppose that evidence can always be translated into propositions and that the conviction that it can be has no basis other than philosophy's century-long fascination with symbolic logic.

As an antidote to the idea that evidence consists of propositions, it is useful to study simple, everyday problems in which we draw probable conclusions: a plant is probably going to die; a certain incident last week probably explains a friend's short-tempered behavior today; a story I remember reading recently was probably in the local newspaper. The evidence that the plant is going to die consists of the way it looks, our memory of how we have been treating it, and our experience with other plants. We sample from that evidence, look for tell-tale signs, make mental experiments in which we compare the plant to others we have known, try to invent arguments, and examine their strength. And it is from this process, not from infallible propositions describing the evidence, that we obtain our probability.

Like many philosophers and many Bayesian statisticians, Levi stresses the normative character of his theory. He repeatedly admits that he does not know how to prove that the various details of his theory are correct, but he insists that it tells people how they should try to organize their knowledge. And he insists that this imperative is largely immune to challenge from psychological investigations of how people actually think; challenges from psychology can only be relevant if they show people to be incapable of making any progress at all towards satisfying his demands. It seems to me, however, that once we escape from the idea that evidence has to be expressed in propositional form, there is little justification for calling a theory like Levi's normative. Statisticians and laymen both do their thinking in ways quite foreign to Levi's framework, and there is no reason to believe that the time and effort they can spend to improve their thinking would be best spent trying to put it into that framework.

3. *Evaluation.* Levi's treatment of statistical inference does not do justice to the depth and diversity of the subject. Instead of helping us understand the diversity of attitudes that we can take towards statistical models and diversity of justifications that can be provided for them, he offers us another sterile "normative" dogma. I share his attraction to the idea of using intervals of probability, or upper and lower probabilities, to better express uncertainty, but I found little in his book that will make this idea more popular among other statisticians.

The book is written in a staccato and repetitive style that is unattractive in general and particularly distracting in the mathematical passages. One never knows whether a given sentence is meant as a definition, a theorem, or an explanation. And Levi's approach to the work of other scholars is often irritating. He is so convinced of the correctness of his general framework that he considers it charitable to interpret other scholars' proposals in terms of that framework, even when this results in absurdities.

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Sample Path Properties of Stable Processes, by J. L. Mijnheer, Mathematical Centre Tract # 59, Mathematisch Centrum, 1975, 124 pp., approx. $7.00 (paperbound).

Random Walks With Stationary Increments and Renewal Theory, by H. C. P. Berbee, Mathematical Centre Tract # 112, Mathematisch Centrum, 1979, iii + 223 pp., approx. $12.50 (paperbound).

Branching Processes With Continuous State Space, by P. J. M. Kallenberg, Mathematical Centre Tract # 117,