Operations Research

Publication details, including instructions for authors and subscription information:
http://pubsonline.informs.org

The Analyst's Bookshelf

To cite this article:
(1971) The Analyst's Bookshelf. Operations Research 19(2):551-555. https://doi.org/10.1287/opre.19.2.551

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Reviews

ROBIN FARQUHARSON, *The Theory of Voting*, Yale University Press, New Haven, Connecticut, 1969, 90 pages, $5.00

TO THE reviewer the title seems a bit misleading, since many possible theoretical analyses of voting behavior find no place in this excellent small monograph. Voting is considered to be an adopted means for decision making within the framework of n-person games. The study of the voting process parallels formal military tactics and strategy in that it long preceded game theory yet is much illuminated thereby. Voting, as a procedure for determining and validating decisions, is a notable invention as important in its place as is the concept of a weapon (e.g., a club or stone instead of teeth or claws). A cogent analog of the 'social contract' is presented in an appendix as is also a letter from Pliny on voting. The viewpoint is claimed to be between that of NASH'S uncooperative n-person games, in which there are neither coalitions nor side payments, and the VON NEUMANN-MORGENSTERN games which admit coalitions but exclude side payments.

The advantages claimed for such a viewpoint (p. 71) are that domination can be asymmetrical and transitive, thus permitting freedom from the multiplicity and nonuniqueness of the von Neumann-Morgensterm solutions

What the author does in his short treatise is well worth digging into. There are several valuable insights available. Sincere voting is that in which an individual's vote is cast for the alternative, among those available for choice, that contains the preferred possible final outcome. In sophisticated voting, one may opt for a less preferred alternative in order to achieve a better final outcome than sincere voting would secure. Such voting requires knowledge of the preferences of other participants in the voting. Circumstances in which sophisticated voting and naive voting are the same are called 'straightforward.'

If a voter does not have a straightforward strategy, and especially if the other voters do not either, then his best behavior depends on what others will do. Since predictions of others' choices depend on his own, the voter faces a dilemma familiar to game theory students. Knowledge of a voter's preferences and the assumption that only primarily and secondarily admissible strategies will be chosen allow a layering of strategies so that what happens is found by successive elimination of what will not happen. In cases where all choices are binary and preferences are strong, this reduction proceeds to a single outcome called an 'individual equilibrium.'

If coalitions form the noncooperative equilibrium, solutions based on sophisticated voting, indicated above, must be reconsidered. A solution of the kind indicated above is called vulnerable if there exists a coalition whose members, by joint action, can secure another outcome that all members of the coalition would prefer. Simple answers are not available for this case, but one can say that an individual

equilibrium is collective if no coalition has both the will and the power to make a change. There does not necessarily exist a collective equilibrium, as the author shows.

At the end, the author devotes two pages to the analysis of information conditions. This is indicative of the brevity of the volume. Much is concentrated in a small compass so that the result is not easy reading. Nevertheless the reader will be well repaid if he devotes the time he would have spent if the work were spelt out in greater detail and thus made easier to read.

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D. R. COX, *Analysis of Binary Data*, Methuen and Company, London, England, 1970. 142 pages, $6.50

IT IS ALWAYS a pleasure to read a book by D. R. Cox, on at least two grounds. First, the aims and results are clearly and unambiguously set before the reader. Second, when the topic is one in statistics, the reader feels that he is in the hands of a statistician who has dealt with real problems in everyday space rather than Banach space. On both of these counts this latest monograph is a success.

The theme of this book is the use of logistic models for the analysis of binary data depending on one or more parameters. Applications for which the analysis is appropriate include contingency tables, bioassay, Markoffian models for time series, and data on paired preference. Much analysis of continuous data in statistics is based on the assumption of an underlying normal distribution. Although this assumption does not necessarily correspond to reality, its use may be justified on one of several grounds; it leads to simple computations, results may not be sensitive to the exact distribution, the real distribution may be inaccessible so that one might as well use the simplest model. The statistician who has a feel for his data and has not committed too much of it to a computer will usually realize when the assumption of a particular distribution can lead to trouble. Professor Cox suggests that the logistic model might very well play the same role in the analysis of binary data. Certainly, the calculations that follow from this assumption are simple. There is also some indication that the theory is relatively robust as well, which tends to argue for Professor Cox's suggestion.

Much further work on robustness will have to be done before the logistic model plays as central a role in statistics as does the normal model, but Cox's monograph does argue persuasively that the effort should be made.

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KARL SEILER, III, *Introduction to Systems Cost-Effectiveness*, Wiley-Interscience (Publications in O.R. Series, No. 17), New York, N.Y., 1969, 108 pages, $9.95

IN THE deliberations preceding major public and private-sector decisions—typically involving choices amongst alternative 'systems' or system configurations or programs for achieving some goal—the loosely-defined body of outlooks

and techniques known collectively as 'cost-effectiveness analysis' continues to play an increasingly prominent role. This book's preface properly notes both the difficulty "of collecting material from many sources in order to get an integrated understanding" of how such analyses are structured and executed, and the resultant need for a "concise yet comprehensive textbook on cost-effectiveness theory and applications."

Such a goal is too ambitious for so slim a volume, nor is it seriously attempted. There is no connected account of any specific applications, and the text's descriptions of methods and procedures generally do not reach the level of 'theory.' What *does* appear, arranged in quite reasonable sequence, are expositions of the various steps and processes involved in the relatively ritualized cost-effectiveness (C-E) analyses encountered in the military sphere. Chapter I introduces the types of cost factors and considerations to be treated. Chapter II takes up the assembly of these factors into "system cost models"; here I applaud the recommended use of the triangular distribution for cost estimates, though the writer cops out in omitting an explicit formula for variance. Chapter III takes up "system effectiveness models"; the effectiveness measure emphasized is the product of the expected values of the system's basic performance, availability, reliability, and survivability, each factor modified by a multiplier representing an associated "statistical confidence." Such formulas are indeed common in C-E analyses, but normally lack much solid justification beyond the practical need to specify *some* reasonable-looking function of the indicated factors so that one can get on with the analysis and/or the associated trade-off studies. The concluding Chapter IV ("System Cost-Effectiveness Models") performs a final integration, describing how measures of cost and effectiveness for various systems can be brought together to aid in the selection decision. This was to my mind the strongest of the chapters, especially in its endorsement of the use of full probability distributions (not just mean values) whenever possible despite the extra data and effort required.

By the single criterion of *scope*, then, the book promises to be useful both as a substantial introduction to the practice of C-E analyses for absolute or relative newcomers to the field, and as a kind of checklist for the professional. Its literature references, though idiosyncratic, do guide the reader to deeper treatments of particular points. Unfortunately, the volume's reliability and readability are critically degraded by the unacceptable prevalence in it of (a) poor writing and (b) loose thinking.

Failing (a) rises to occasional heights (like p. 37's "usually always," or p. 95's "pragmatic practice"). But mainly it attains only a grating persistent infelicity of language, a steady willingness to settle for some second cousin of the proper word or phrase (e.g. "exclude" for "omit" on page 6, "technological" for "technical" on p. 101), which makes reading quite unpleasant and creates a constant threat of misunderstanding.

Flaw (b) is illustrated in cameo by Section 3.6 (less than two pages long), which early on reverses dependent and independent variables, has its first equation marred by an extraneous symbol, fails to explain why its second equation might be an "approximation" of its first, and offers no words as to when and why the proposed alternate use of its "weights" as exponents would be superior to their normal deployment as multipliers. A further *sparse* sample of faults includes:

(a) an assertion that average fixed and variable costs *increase* with the number of units produced (p. 10);

(b) a garbled formulation of discrete and continuous discounting (pp. 17–18);

(c) a dangerously naive reference to regression (pp. 19–20);

(d) botching of the equations on p. 30 and p. 37, which are intended to "facilitate the computer programming process";

(e) the trivial zero solution to the cost minimization problem of (p. 31);

(f) citation only of Lanchester's *linear* law, thus missing the major point of this contribution (pp. 58–59);

(g) omitting all multivariate aspects from pp. 61–64's discussion of problems with optimization techniques; and

(h) the assertion that linear programming encompasses problems with discontinuous side conditions (p. 84).

There are some other (much less damning) limiters of the book's appeal, which should also be pointed out. One is the failure to communicate, to any appreciable extent, the very real intellectual challenge and stimulus of C-E analysis. A second stems from the fact that much of the current excitement over C-E analysis lies in its potential application to areas *other* than those of military security or private profit; the text stresses the first of these spheres almost exclusively, with an occasional and sometimes confusing shift to the second (e.g., p. 33's reference to cash inflow in an otherwise DOD-oriented discussion). This restriction will be not only disappointing, but downright repugnant, to many of the new generation of potential analysts; the text's few allusions to using C-E analysis in other fields are blatantly afterthoughts, and the bland assurance (p. 2) that "there is no fundamental reason why it is not equally applicable to other fields" will bring bitter smiles to the faces of those involved in such undertakings.

Finally, the preface notes that C-E analysis has proven "successful," although "errors have been made," but the book does not even try to provide a basis or criteria relative to which findings of either 'success' or 'error' can be made—an especially embarrassing omission in view of the subject-matter! The only evidence offered for the claim (p. 1) of "impressive accomplishments" is that of wide-spread *adoption* of the approach, but of course cannibalism, slavery, the 'sack' dress, and other unpleasant practices and ideologies could offer similar evidence in their own behalf.

The flaws and failings of this book, inveighed against above, to my mind outweigh its very real and substantial informative values. I could not recommend it, in particular, to what strikes me as a most important type of potential reader, namely the bright young student of economics or science or engineering who is seeking a *pro bono publico* use for his trained talents and is wondering whether "this cost-effectiveness stuff" might not be the right path to follow.

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M. M. BOTVINNIK, *Computers, Chess, and Long Range Planning*, Springer-Verlag, New York, N. Y., 1970, 89 pages, $3.50 (paper)

IF THIS work had been authored by an ordinary person, it would probably be quickly dismissed as an over-optimistic appraisal of realities. For in this work, originally titled "An Algorithm for Chess," Botvinnik proposes a method for

building a master-level chess-playing computer program. The reason that this seems ludicrous is that only in recent years have researchers been able to build chess programs that can play at Class B level. In the chess hierarchy, a Master would beat a class B player well over 99 per cent of the time.

But Botvinnik is no ordinary person. He probably is the greatest chess player that ever lived and, in addition, holds a Doctor of Sciences degree in Electrical Engineering. What Bovinnik has clearly noted is that today's chess playing programs are tactical opportunists. That is, they are very good at taking advantage of opportunities that present themselves, but have little ability at creating opportunities. This is because standard tree-search methods are used wherein the universe of the program—all it *can* know—is totally contained in the tree and in the evaluation function applied to the bottom nodes of the tree. However, living chess players are able to deal in concepts that are far beyond the moves in a tree search. Botvinnik notes that these lie mainly in the realm of strategic possibilities. Strategy, as he clearly develops the theme, is the finding of optimum locations for one's pieces, the finding of paths to relocate pieces to their optimum squares, and the assessment of the possibilities of doing this successfully.

Most of the book is devoted to developing the notation of paths and the concepts related to the accessibility of points on a path. Botvinnik shows methods of constructing paths by machine, and then develops rules for traversing these paths. All this is rather formidable and one might have to convene a congress of mathematicians to prove its correctness. However, one thing is quite clear: his method—the idea of analyzing paths of pieces apart from direct possibilities in a tree search—is quite sound and will enable a computer to play a much better game of chess when properly programmed. Thus the book is valuable to those who are interested in chess programming and, to the extent such path analysis of nonimmediate possibilities transfers to other fields, it could be valuable to others. The essence of the method is that it avoids the local maxima problem to the extent that a tree of possibilities, when minimaxed, can only guarantee a local maximum. Thus the taking into account of possibilities well beyond the scope of a tree search greatly increases the probability of finding a global maximum.

From the point of view of someone who is directly involved in chess programming, Botvinnik's suggested implementations show a certain lack of sophistication in the use of computers. However this is not terribly important. The translator has done a great deal to make life easier for those not too familiar with chess and computers. Here is an altogether worth while and important book, although I cannot necessarily advocate it for those interested in long-range planning.

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Books Received

Adolf Adam, Elmar Helten, and Friedrich Scholl, *Kybernetische Modelle und Methoden*, Westdeutscher Verlag, Opladen, West Germany, 1970, 172 pages, DM 42 (DM 29 in paper).

Arnold E. Amstutz, *Computer Simulation of Competitive Market Response*. M.I.T. Press, Cambridge, Mass., 1970. 457 pages, $17.95 ($4.95 in paper).