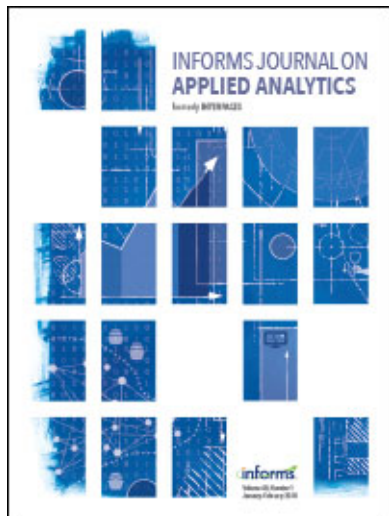




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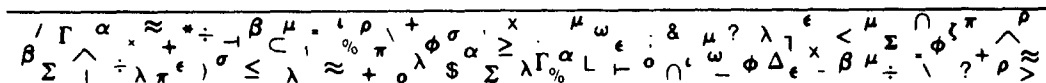


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Book Reviews

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The range of books reviewed is wide, covering theory and applications in operations research, statistics, econometrics, mathematics, computers, and information systems (no software is reviewed). In addition, we include books in other fields that emphasize technical applications. Publishers who wish to have their books and proceedings reviewed should send them to Professor Benjamin Lev. We list the books and proceedings received; not all books received can be reviewed because space and time are limited. Those who would like to review books are urged to send me their names, addresses, and specific areas of expertise. We commission all reviews and do not accept unsolicited book reviews. Readers are encouraged to suggest books that might be reviewed or to ask publishers to send me copies of such books.

ARROW, KENNETH J.; COTTLE, RICHARD W.; EAVES, B. CURTIS; and OLKIN, INGRAM, eds. 1996, *Education in a Research University*, Stanford University Press, Stanford, California, xxxv + 494 pp., \$60.00.

Gerald Lieberman served as the first chairman of Stanford's Department of Operations Research, as chairman of the Faculty Senate, as dean of graduate studies and provost of Stanford, and as president of ORSA. This book is dedicated to him

and might better have been titled *Essays in Honor of Gerald Lieberman*. It contains 30 individual chapters, many of them fascinating and many by extremely eminent authors. All of the authors appear to have some sort of tie to Lieberman, including some who were students of his. Most of the chapters deal with topics related to operations research or statistics. The rest deal with university education. The book is divided into three parts. Part A is entitled "Administering a research university." It

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INTERFACES 27: 1 January-February 1997 (pp. 155-166)

contains 10 chapters, only three of which are related to operations research. Part B is entitled "Teaching and learning." All but one of its 14 chapters concern operations research or statistics. All six chapters of part C, entitled "Research reaching society," concern operations research or statistics.

In part A, the highlights for me included David Hopkin's report on the operations research models Stanford used in, among other things, managing its tenure policies. It is largely reprinted from *Interfaces* with a contemporary retrospective. The chapter by Stephen Fienberg, a statistician who has been a dean and an academic vice-president, on the uses of statistics in academic administration also held my attention. Among the chapters not related to operations research or statistics, I was most intrigued by the account of Jean H. Fetter, a former dean of admissions at Stanford, about affirmative action graduate admissions at Stanford, and by Patrick Suppes's forecast of educational technology at Stanford in the 21st century. I would love to hear an extended discussion between Suppes and Clifford Stoll whose book *Silicon Snake Oil* [1995] was reviewed here in the March–April 1996 issue.

Part B contains a number of chapters describing the evolution of courses or programs in operations research or statistics that have worked out well. These include the Stanford OR department's modeling and project courses, a computationally intensive statistics course, a quantitative methods course for MBAs, the MIT leaders for manufacturing program, and the applied mathematics program at Clemson University. There is also a report of a sur-

vey of Lieberman's PhD graduates who went to work in industry and an account of his post-PhD education by one of them.

Several other chapters in Part B are interesting. Frederick S. Hillier writes about writing and revising the Hillier and Lieberman [1967] textbook. Sam L. Savage has a must-read chapter on the ergonomics of tools for operations research applications and the implications of the huge changes in this arena for the operations research profession. Two chapters particularly appealed to me. One is a short discussion of the role of judgment in high level policy making by Alexander George, an emeritus professor of international relations. The other is a discussion by Nobel prize winner Kenneth J. Arrow of the evolution since the 1940s of the role of operations research in the graduate education of economists. It helped me understand a bit better why economists and operations researchers think differently about the same phenomena.

Part C contains the only chapters with technical content. In one of these chapters, Alan S. Manne presents a model for evaluating greenhouse gas abatement policies. In another, Samuel Karlin and Volker Brendel discuss probability models for computational molecular biology. The chapters in this part that will most interest INFORMS members are George B. Dantzig's discussion of the early history of linear programming and the short final chapter by Lincoln E. Moses on how to communicate technical data effectively.

Different readers will undoubtedly prefer different parts of this collection, but anyone interested in either operations research or university education will find lots of interesting, valuable, and well-presented

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material here.

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DASKIN, MARK 1995, *Network and Discrete Location Models, Algorithms, and Applications*, John Wiley and Sons, New York, 498 pp., \$74.95.

Location, location, location. As Mark Daskin states in the introduction to this new book, facility location can have an important impact on the success or failure of both private and public sector enterprises.

The author intends this book to be a hands-on guide to using and developing facility location models, and it is targeted to both students and practitioners. His goals are to introduce the reader to a number of classical facility location models and applications of those models, to describe key methodologies used to solve location problems, and to help the reader develop skills in modeling and solving location problems. Daskin provides review questions at the end of each chapter, as well as a disk with software (IBM-compatible) that allows the reader to apply facility location algorithms to problems of realistic size. The book is intended as both a textbook and a reference book.

Daskin begins the book with an introduction to facility location problems, discuss-

ing what they are and why they are important, and provides a taxonomy for location problems. He assumes only very basic knowledge of linear algebra, so, in chapters 2 and 3, he reviews the OR/MS techniques and concepts that underlie many location models, including linear programming, transportation problems, shortest path problems, and complexity analysis.

In chapters 4 through 7, Daskin focuses on four main types of location problems—covering problems, center problems, median problems, and fixed charge location problems. In contrast to Francis, McGinnis, and White [1992], which divides their presentation of location models according to the locational topology, Daskin has divided his presentation according to the objective function of the model (set covering, minimizing maximum distances, minimizing average distances, and fixed-charge problems). The latter appeals to me as a natural way of thinking about location problems in practice: the decision maker first identifies an appropriate objective to optimize, then determines how to model the locational topology (for example, whether to model the road network as a tree network or a general network).

In chapter 8, Daskin discusses extensions of the four basic types of location models; these include multiobjective location problems, hierarchical problems, models of interacting facilities, location/routing problems, hub location problems, and models for the location of undesirable facilities. He concludes the book with a discussion of how the use of optimization models for facility location fits into the overall facility location planning process.

The book is clear and easy to follow.

Each chapter begins with a verbal introduction to the problem considered. This is followed by mathematical development. Presentation of the models is enhanced by numerous examples and illustrations. Each chapter concludes with a verbal summary. Exercises are included at the end of each chapter.

The software that comes with the book can be used to solve a number of types of location problems. Chapters in the book contain exercises to be done with the software. The exercises rely on preset data tables within the software or require manual input. They are intended to familiarize the user with the full setup behind the location models. The software also includes a large library of examples that can be run instantaneously. All of the input files for these examples can be modified; it is especially nice that one can modify the data piece by piece.

The software is well explained and easy to use. I got it up and running an example in about five minutes. Everything is menu driven; data requirements and options are explained as they appear in the menus. The results menus are great for a beginner but may be a bit limiting for the expert. The software also provides excellent graphics. There are several graphics options, several printing options, and many configuration options. The package seems very flexible.

Overall, I found this book to be well written, interesting, and useful. I can recommend this book with no reservations to anyone teaching an undergraduate or master's-level course in location modeling, and I can also recommend it as an excellent reference book to anyone interested in location modeling. With its mix of mathemati-

cal development, intuitive explanation, examples and illustrations, Daskin's book is similar to another text and reference book that I particularly like, *Urban Operations Research* by Larson and Odoni [1981].

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HAUSCH, DONALD B.; LO, VICTOR S. Y.; and ZIEMBA, WILLIAM T., eds. 1994, *Efficiency of Racetrack Betting Markets*, Academic Press, San Diego, California, 648 pp., \$65.00.

Professors are not immune to the excitement of a day at the races, although they sometimes show up in their thinking caps and academic robes. This book collects 54 papers about horse racing written by mathematicians, psychologists, economists, and management scientists over nearly half a century. The authors exhaustively analyze statistics about racetrack betting and discuss strategies to exploit the "inefficiencies" in such wagering.

All 54 chapters originally appeared as journal articles. None have been changed in this volume: the editors did not try to achieve a common typeface, let alone a common notation. But this diversity is not really frustrating because the papers are singularly well written. And it is fun to watch an arduous mathematical derivation

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evolve into a review of the fortunes of (the four-legged) Mrs. Twiggywinkle.

The authors describe several striking regularities in betting behavior. There is a persistent "long shot" bias: when betting odds assign a particular horse a remote chance of winning, its actual prospects are even more remote. This bias gets stronger late in the racing day, as bettors faring badly try in one fell swoop to recoup their losses. Correspondingly, heavily favored horses win even more often than the track odds imply.

The volume suffers from a surprising lapse: the absence of an incisive introductory chapter. The editors limit themselves to the most minimal narrative comments. They offer us no overview of what is known about betting behavior and what remains to be understood, and of how far this volume goes beyond what Gordon Newell called "mathematicians working for their mutual entertainment." (Perhaps here it is parimutuel entertainment.)

The editors indeed suggest that the work has broad significance, but they hardly demonstrate the point. They assert, for example, that the racetrack provides "an excellent forum for a psychological investigation of investor risk behavior." But not everyone goes to the racetrack and, among those who do, the frequent patrons and the heavy bettors have a disproportionate effect on the track odds. To put it simply, big racetrack gamblers and big venture capitalists are probably not the same people. Thus, it is not clear how much the behavior of the former tells us about the behavior of the latter.

Indeed, the editors go on to inform us that "financial experts" see the racetrack as

allowing "cleaner" studies of risk taking than are possible in financial markets. But if stock market gambling is not already understood, how can we be sure that the horse race is a suitable metaphor for it? It is disappointing that the book says so little about why racetrack betting is relevant beyond the racetrack. Perhaps the editors have inadvertently called attention to a serious gap in the literature.

The New York Times used to advertise that "you don't have to read it all, but it's nice to know it's all there." Many of us are happy that this book is "all there," although not everyone will wish to express his joy by putting this volume on his bookshelf. Some will await the penetrating review article that synthesizes the work in this collection. Perhaps the editors—who are all leading researchers in this domain—will wish to purge their present sins of omission by preparing such an article themselves.

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RHYNE, RUSSELL 1995, *Evaluating Alternative Indonesian Sea-Sovereignty Systems: An Exercise in Soft Technology Transfer*, Institute for Operations Research and the Management Sciences, Linthicum, Maryland, iii + 264 pp., \$23.00.

In the 1970s, civilian analysts attached to US military forces in the Pacific engaged in a program to teach PPBS (Planning, Programming, and Budgeting System) and systems analysis to the military staffs of several Far Eastern nations. Dr. Rhyme headed the Indonesian effort, which consisted of leading several Indonesian military officers through a pilot study to show the country's

military leaders how analysis might help them with their resource decisions. In this book, Rhyne describes the results of the effort and includes a detailed account of the pilot study itself. It is must reading for anyone who attempts a similar effort in soft technology transfer to a nation with a different language and different traditions of policy analysis. Rhyne also spends considerable time on a subtext that I found even more interesting, pointed criticisms of some of the ways that systems analysts have been practicing military analysis.

Rhyne delivers a remarkably candid account of his efforts and failures in getting the Indonesians to institutionalize systems analysis in their military staff. Analytical technique was not in short supply: by 1974, a dozen Indonesian officers had received degrees in operations research from the Navy's Postgraduate School in Monterey, California. The problem was that these officers lacked practice in applying the tools they had learned to real problems. The pilot study was designed to fill this gap. Despite his best efforts and the fact that similar efforts in Japan and South Korea had met with success, Rhyne was unable to get the Indonesians to establish the organization and career tracks to carry on the work.

The pilot study itself was successful. The subject was maintaining control in one of Indonesia's archipelagic seas. Rhyne's officer study group constructed several alternative sea-control forces consisting of patrol boats, aircraft, land forces, and support resources at a constant 10-year cost of \$250 million. The effectiveness of the alternatives was evaluated in a variety of missions (that is, against a variety of threats) in several scenarios. Judging from the book, the treat-

ment of the various steps was quite uneven; there is much discussion of scenarios, threats, and the emphases, or general features of the alternatives, but almost nothing on the numerical alternatives and the estimation of costs.

Although the outlines of the study are familiar, Rhyne uses some controversial techniques that he feels avoid the major pitfalls he cites. Among Rhyne's complaints are that analysts often ignore the real-world context in constructing scenarios for analysis. Future threat projections are based on maximum enemy capabilities rather than on plausible intentions, and fancy OR techniques are overused in calculating effectiveness. (Rhyne argues that preparing for the worst threat has become too expensive even for the US; moreover, our preoccupation with Soviet nuclear attack left us open to defeat in Vietnam. Some would argue, however, that our problems in Vietnam were not caused by inappropriate technology—Vietnam was, indeed, one of the "lesser included cases" that we could handle if we were prepared for the Soviets—but US policy was designed to avoid Chinese and Russian involvement.) A more serious methodological criticism is that in comparing alternatives that yield more than one output, analysts need to give decision makers more than the standard effectiveness results. Effectiveness vectors must be translated into single overall measures of value. Finally, analysts do not take enough time to understand the systems (human and machine) they are comparing, and too often (especially in the case of the early McNamara systems analysis office) use cost effectiveness to counter professional military experience.

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Rhyne is certainly on the mark in criticizing overinvestment in detail and mathematical techniques. Operations research was invented by Philip Morse and others in the early years of World War II to solve such real problems as how to convoy goods to Europe against the German submarine threat. Although these early operations researchers were physicists with the highest credentials in mathematical analysis, the techniques they developed for attacking real-world problems were simple, as befits problems with little data and much uncertainty. This perspective has been lost, however. Many of the OR journals, university operations research departments, and entire careers are devoted to developing technique for its own sake ("a hammer looking for a nail").

Rhyne's strong views against McNamara's whiz kids, however, seem overdone. Much of the conflict between the analysts and the military arose from the shift in power that Rhyne himself admits was taking place at the time. Rhyne, in fact, echoes the same theme in praising the Sea-Control Study ". . . as a means of enhancing Indonesia's policy makers' control over the budget share assigned to each of . . . [the] . . . four services." Rhyne is certainly correct in saying that it would be foolish to dismiss military judgment in conducting defense analysis, but the services often have different judgments about the same problem, and they cannot all be right. (In a recent article in the *Naval War College Review*, Peter Tarpgaard [1995] argues that the TFX, widely regarded as one of the major errors of the McNamara years, was not a failure of methodology at all, but the fact that the methodology was ignored. Many

studies at the time indicated that a single design could not fill the needs of both land-based and carrier-based aircraft, but McNamara ignored the analysis. It's hard to imagine an analytical methodology that would keep people from ignoring its results!)

That brings us to Rhyne's proposals for injecting the political and economic realities ("context") into the analysis. He borrows a technique from social field theory called field anomaly relaxation that involves working through a "sector/factor" matrix. In the scenario analysis, for example, the columns of the matrix (the sectors) are seven broad categories, such as the worldwide power balance and raw material availability. The rows under each sector (the factors) span the range of possibilities that Indonesia might face in the 20 years following the study. Each horizontal path through the matrix (one factor from each sector) is a scenario, and there are 226,000 combinations. The next step is to apply various "filters" to throw out the combinations that failed to pass a variety of tests: there was more than one marginal (unlikely) factor, some of the factors were incongruous, and other combinations just wouldn't "jell." Similar approaches for constructing threats and alternative emphases yielded 27,000 and 4,536 possibilities, respectively.

Although the sector/factor technique depends on the insight of the study members, Rhyne argues that the approach makes the analysis more systematic and thus guards against the possibility of missing important cases. There are some drawbacks, however. The approach is very time consuming; in fact, working through the scenario analysis left too little time for a complete analysis of

the threats and alternatives. (Shortage of time also led to analyzing only two alternatives. The study team excluded the status quo, which “. . . had grown mostly through accretion and was hardly worthy of investigation.” Including it, however, would have given planners some solid information about whether the current force levels were really all that bad!) Another problem is that immersion in detail often leads to loss of perspective. More important, the approach seems to violate Rhyne’s own dictum that analysis should bring judgments up front.

Rhyne’s method for translating effectiveness into value terms also leads to problems. It involves using a technique called magnitude estimation scaling in social psychology. For the Indonesian sea control study, questionnaires were used to solicit the views of military officers on the relative values of different levels of effectiveness, both within and across missions. These relative values are used as a double-weighting, or scaling scheme to translate the effectiveness of a given alternative against the various threats into a single output measure. (Although Rhyne states that the approach was an innovation in 1973, double-weighting techniques have been around at least since the 1960s.)

Rhyne’s interest in developing a single value measure is certainly understandable. Comparing effectiveness vectors does create problems of interpretation. However, Rhyne’s argument that planners need a linear ranking implies that analysts have not found satisfactory ways of exhibiting multi-dimensional results in the studies they have conducted over the last 35 years.

Attaching values to effectiveness raises

an even more important conceptual issue at the heart of policy analysis. Effectiveness measures are certainly not indicators of ultimate value, but the questions are whose values are important, and how are the values to be measured? The first question is simple; at least in democracies, the values that count are those of the citizenry, the ultimate beneficiaries of defense. How to measure values, however, is not so clear. Because military goods are not sold in a market, there is no price mechanism to measure the values that citizens attach to various defense alternatives. Since we can’t measure effectiveness in dollar terms, we employ the standard cost effectiveness approach of holding cost or effectiveness constant. (See, for example, E. S. Quade’s [1975 pp. 25–27] discussion. Rhyne’s discussion of methodology reflects a misunderstanding of economics. He says that econometrics deals with the question of value versus effectiveness, but it cannot be used in determining subjective valuations because it assumes that price equals value, an assumption that isn’t valid unless the market in question involves very many, similar transactions. Econometrics is not, however, a way of assigning values, but only of relating whatever variables the analyst chooses. The connection between price and value has meaning for individuals, not just markets. The more I value something, the larger the price I am willing to pay. The theorem about markets is simply that where there are many buyers and sellers, price is set by the crossing of the demand and supply curves, where the value and cost of the marginal unit are equal.)

Rhyne’s approach is to use military judgment. Military and civilian officials are, af-

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ter all, the agents of the citizenry in matters of defense. However, the purpose of studies is to provide information to such officials at the highest decision-making levels, where visibility creates the strongest incentives for good decision making. The effectiveness scaling technique suffers from the same problem as the sector/factor approach discussed earlier: it buries the judgments of the analysts (including the lower-level military people on the study), rather than making them explicit. It would be difficult for decision makers to know how the results of the sea control study depend on the judgments the study team had made on scenarios, threats, and alternatives.

Rhyné's book is well written overall, but the reader's attention is diverted by spelling and grammatical errors ("McNamara" is misspelled, curiously enough), and some terms are used without definition and consistency. Despite its conceptual problems, however, Rhyné's book is a fascinating, "warts-and-all" account of the problems in soft-technology transfer. His discussion of such problems would be an excellent starting place for anyone who is undertaking a similar, cross-cultural effort.

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YOON, K. PAUL and HWANG,

CHING-LAI 1995, *Multiple Attribute Decision Making: An Introduction*, Sage Publications, Thousand Oaks, California, 75 pp. (paper), \$9.50.

Decision makers often find it hard to make the best choice. In selecting an alternative they may spend lots of time on what to choose; however, they might do well to use multiple attribute decision making (MADM) methods. Yoon and Hwang formally define MADM as making preference decisions, over available alternatives characterized by multiple, usually conflicting attributes. MADM methodology is aimed at improving decision quality.

MADM procedures can be applied to a wide range of human choices, professional, managerial, and political. The authors identify 13 MADM methods, using such applications as site selection for a waste management plant, budget reduction, business manager selection, plant location, and automobile purchase.

The authors also present efficient normative MADM methods, including different types of numerical examples. The examples they describe are simple enough to be understandable but complex enough to illustrate the computational procedures and characteristics of the method. They also analyze a number of real-world cases.

In chapter 1, the authors begin with a brief introduction. In chapter 2, they propose attribute generation, weighting, and normalization of attribute ratings. In chapter 3, they present noncompensatory methods, including satisficing methods, sequential elimination methods, and attitude oriented methods. Two scoring techniques—simple additive weighting (SAW) method and weighted product method—

are discussed in chapter 4. The authors developed the technique for order preference by similarity to ideal solution (TOPSIS) [1981], and they cover it concisely in chapter 5. In chapter 6, they review Roy's [1971] ELECTRE (Elimination et choix traduisant la réalité). Qualitative data can be easily transferred to either rank data or pairwise comparison data. Working with this type of data, they demonstrate two methods (median ranking and AHP) in chapter 7. The practical nature of the AHP method has led to its application in such diverse areas as education, politics, and sociology. As an example, Yoon and Hwang go over the application of AHP to President Carter's decision to try to rescue the American hostages in Iran. They first noted the close relationship between the AHP and SAW methods. Finally, they cover the extensions in chapter 8.

The MADM method permits only certain data inputs and therefore cannot be implemented in a soft data environment. The use of fuzzy MADM seems limited until researchers develop efficient algorithms and supporting software. The tremendous increase in the use of personal computers has made MADM analysis easily available to decision makers, and many MADM algorithms can be done on PCs. The electronic spreadsheet is an especially powerful tool for MADM analyses since there are many MADM methods for solving a particular problem. In fact, which method should be used to solve a particular problem is in itself an MADM problem. There is a need for a decision support system (DSS) that will offer advice on what MADM method to choose to obtain the best solution for a particular problem. Thus, MADM-DSS guides

a decision maker to choose the appropriate method based on the choice strategy.

Hwang and Lin [1987] created a prototype of an MADM-DSS using an expert system structure, which extracts the problem specifications and data from the decision maker and then converts the data if needed. After that, it identifies the problem type and a proper solution method. Finally, it solves the problem and displays the best solution.

This monograph is one of the best in the series "Quantitative applications in the social sciences." It will be a valuable reference for graduate students, research workers, and scientists in the social sciences and other fields. In a nutshell, it is a perfect book for those who want a basic knowledge of MADM procedures and applications.

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LEEMIS, LAWRENCE M. 1995, *Reliability: Probabilistic Models and Statistical Methods*, Prentice Hall, Englewood Cliffs, New Jersey, 319 pp., \$71.00.

The title implies that this is a text on probabilistic models and statistical methods in reliability. However, claiming that this book is appropriate for a course in reliability engineering is optimistic, since engi-

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neers would look for a more applications-oriented text. The book is most appropriate for a one-semester course for advanced undergraduates or graduate-level students in a mathematics or statistics department. It also requires a calculus-based introduction to probability and statistics. Within these limits, the book fills a need.

After an introduction that includes definitions of such terms as *reliability*, *coherent systems*, *structure functions*, and *reliability functions* in chapters 1 and 2, Leemis deals with probabilistic models in chapters 3 to 6, and statistical methods in chapters 7 to 10. The probabilistic models include standard distribution models and birth-death models. Statistical methods include both parametric and nonparametric estimation and the test statistics necessary for model formulation and life testing.

The book is somewhat inconsistent in terms of stated goals and the selection of topics and the nature of their coverage. For instance, it is debatable whether a text like this should include discussion of such concepts as information and efficiency, since classroom discussion can only be very cursory at this level. Some of the examples used are from biostatistics; since no biostatistician is going to buy a reliability text to gain an introductory background in statistical methods, this seems pointless and confusing. The instructor will be forced to supply alternative examples on reliability in place of those from biostatistics.

Compared to the depth of the discussion on statistical methods, which includes such topics as likelihood theory and life testing, the coverage of probabilistic models for repairable systems is very perfunctory. The short section on birth-death models does

not explore the richness of simple Markov models used in modeling repairable systems.

A text on reliability addressed to the engineer would require more exercises from the real world with real-world data. Since the bulk of the exercises in this book are theoretical in nature, the emphasis seems to be on the mathematical foundations, rather than on the applicability of reliability techniques in the industrial setting. Leemis states in the preface that the text should give students enough background to read most reliability journal articles and more advanced reliability texts. The book seems to satisfy this objective with the exception of its coverage on repairable systems.

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