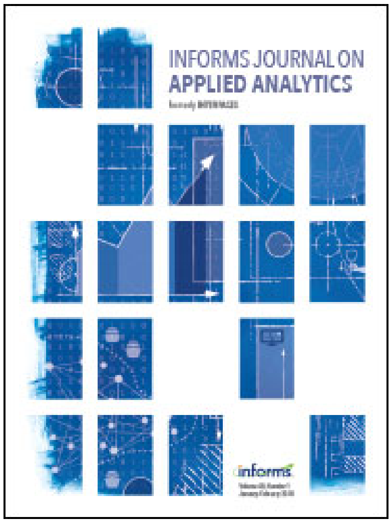

Interfaces

Book Reviews

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Book Reviews

Benjamin Lev
School of Management, The University of Michigan–Dearborn, 4901 Evergreen Road,
Dearborn, Michigan 48128-1491, blev@umich.edu

The range of books reviewed is wide, covering theory and applications in operations research, statistics, management science, 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 reviewed should send them to Professor Benjamin Lev. We list the books received; not all books received can be reviewed because space is 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.

The authors or editors of the books reviewed in this issue are Michael Bartholomew-Biggs, Saul I. Gass, Arjang A. Assad, W. Li, Massimo Paolucci, Roberto Sacile, Suresh Sethi, Hanqin Zhang, Qing Zhang, and William T. Ziemba.

BARTHOLOMEW-BIGGS, MICHAEL. 2005. *Nonlinear Optimization with Financial Applications*. Kluwer Academic Publishers, Dordrecht, The Netherlands. 261 pp. $69.95.

The subject of financial mathematics attracts considerable popular and academic interest. Educational institutions have begun to offer academic degrees in this subject, and all kinds of computer packages now purport to help analysts provide quick solutions to all types of financial-management problems that can be posed mathematically. *Nonlinear Optimization with Financial Applications* consists of 23 chapters, and the author's avowed purpose is to "be concerned with the more general [optimization] problem in which the [objective] function and/or the constraints are nonlinear" (p. 2). By sampling from this book's contents, I hope to convey the intellectual contributions of this book.

One salient motif is that many problems in financial management can be posed and analyzed as nonlinear optimization problems. To this end, Bartholomew-Biggs introduces such pertinent notions as the return and the risk in a particular financial portfolio and the variances and the covariances that can be computed from the history of financial returns. Following this introduction, the author poses the basic minimum risk problem and uses fairly straightforward mathematics to demonstrate how to compute the minimum risk for a given return and how to optimize the expected return given an acceptable level of risk. In this introductory material, the author points out that not all problems in finance can be posed and usefully studied as quadratic optimization problems.

He spends considerable time developing this point. He explains that for nonquadratic optimization problems, it will often be necessary "to use iterative methods...." (p. 43). What kind of iterative methods? The methods include the secant method and Newton's method; these iterative methods incorporate gradient techniques, in which one frequently computes both the first and the second derivatives of the apposite functions. When discussing finite difference approximations to derivatives, the author helpfully notes that a poor choice of the so-called differencing step size can give "inaccurate results and hinder the ultimate convergence to [the optimal solution]" (p. 46).

How does one compute minimum-risk and maximum-expected-return financial portfolios when short selling is impermissible? In Chapter 9, the author explains that one can first make a simple transformation of the old problem variables and then restate the original optimization problem as a minimize-risk problem and as a maximize-expected-return problem. This transformation ensures that the amounts to be invested are all nonnegative. In this informative discussion, Bartholomew-Biggs emphasizes that, although this minimize-risk problem can be solved numerically, relative to the minimization problems discussed earlier in the book, it is a more difficult optimization problem "because it involves a non-quadratic function" (p. 102).

It is well known that financial portfolio management problems vary in complexity. Consistent with his how-to-solve perspective, the author ably shows how to accommodate one kind of complexity, the presence of transaction costs, in his risk-minimization and return-maximization problem frameworks. Specifically, one can accommodate transaction costs by solving what he calls "rebalancing problems." In this context, one would expect that incorporating transaction costs would make the pertinent optimization problems nondifferentiable, and the author shows that this is indeed the case. How then do we address this nondifferentiability issue? Bartholomew-Biggs persuasively argues that one way "is to formulate the re-balancing problem in terms of *inequality* constraints" (p. 182, emphasis in original).

The author analyzes the advantages and the disadvantages of various numerical methods of solving optimization problems, including interior-point methods. As he rightly notes, these methods are popular in nonlinear programming in part because they are very useful in solving linear-programming problems. In using interior-point methods, one first converts a nonlinear optimization problem with inequality constraints to one with equality constraints by introducing slack variables. Next, one bounds the slack variables by introducing a barrier term and then solves the transformed problem involving a specific parameter numerically. The author notes that the increasing success of interior-point methods has brought them into direct competition with sequential quadratic programming methods and that "it still seems an open question [as to] which of these two approaches is 'better'" (p. 226).

The writing in this book is somewhat dry in parts and the book would have been improved by careful proofreading. However, the discussion in this book is thorough and competent. Although a careful reading of the book will benefit those interested in financial mathematics, to get the most out of it, readers should be familiar with linear algebra at the level of, say, Kolman (1984) or Strang (1988). In addition, it would help to be cognizant of linear-programming theory at the level of, say, Bradley et al. (1977). This latter group of readers will profit most from Bartholomew-Biggs' detailed discussion of the pros and cons of alternate numerical methods for solving nonlinear optimization problems.

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Amitrajeet A. Batabyal
Department of Economics, Rochester Institute of Technology, 92 Lomb Memorial Drive, Rochester, New York 14623-5604

Gass, Saul I., Arjang A. Assad. 2005. *An Annotated Timeline of Operations Research: An Informal History*. Kluwer Academic Publishers, Dordrecht, The Netherlands. 213 pp. $89.95.

In this interesting book, Gass and Assad attempt to show how the methodology of operations research and management science (OR/MS) developed over time. They deal with the methodology more than with the practice; with the theory rather than the application. This book is not a conventional history of OR/MS. The authors rarely describe the questions or problems that led investigators to search for the theory. The authors have done exactly what they set out to do as described in the preface: recount "how the methodology of OR developed in some detail" (p. xi). There is more to OR/MS than methodology. In the early days of OR in World War II, the OR people had to be involved at the operational level. Blackett (1962, p. 171) wrote, "Operational staffs provide the scientists with the operational outlook and data. The scientists apply scientific methods of analysis to these data, and are thus able to provide useful advice.... Thus such scientific analysis, if done at all, must be done in and near the operations rooms."

It seems that during its early days, OR was more an experimental and experiential science than a mathematical science.

According to the authors, the time line begins not in 1936 but in 1564, when Girolamo Cardano published *The Book on the Games of Chance*, and they end with the entry "MIT Operations Research Center 50th Anniversary" in 2004. The authors have divided the intervening time span into eight periods. Only the last six of these periods contains the conventional history of OR, that is, the period from 1936 to the present.

For the first period, 1564 to 1873, the authors list major contributions to mathematics and probability (and hence, to OR) that arose in gambling situations together with other major mathematical discoveries. They list the ancestors, so to speak, of OR—Bernoulli, Cardano, DeMoivre, Huygens, Pascal, and Waldegrave, all of whom belong to the gambling, chance, and probability family. Other ancestors are Bayes, Euler, Fourier, Gauss, Graunt, LaGrange, Laplace, Legendre, Newton, and Poisson. This first period was an era of mathematical invention without which OR (and physics too) would be almost entirely an experimental science.

The second period, 1881 to 1935, finds engineers, mathematicians, statisticians, and economists providing methods and tools important to OR. In the section on this period, Gass and Assad include the work of Henry L. Gantt, Lillian and Frank Gilbreth, and Frederick W. Taylor—all based on the recognition that work can be measured. Once one measures work, one can, through analysis, improve work performance and process efficiency. With the exception of these four people and Ford W. Harris, who published the well-known economic-order-quantity (EOQ) formula in 1915, the balance of the authors cited dealt with mathematics and statistics, including Frederick W. Lanchester who developed the model of warfare and A. K. Erlang in Denmark, E. C. Molina in the United States, and others in France, Germany, Sweden, Holland, and the U.S.S.R. who developed the models of congestion in telephone traffic. The citations on the time line clearly show the development of modern statistical theory over time.

Beginning with Chapter 3, "Birth of operations research," the authors trace the development of OR as we know it, from its birth in 1936 in Britain to the present day. They list three papers of major importance published in 1936 that were completely independent of the work begun by the founders of OR: the papers by T. S. Motzkin (1936), W. W. Leontieff (1936), and A. M. Turing (1936) became of major importance to OR.

Gass and Assad's coverage of events between 1939 and 1945 is rather heavy on US research and rather light on the British contribution. This imbalance persists in the postwar period. The authors pay less attention to work of significance done in other countries than to work done in the United States.

For example, they have no citation for the work in search theory carried out by the Royal Air Force (RAF) Coastal Command from 1940 on as part of the campaign against submarines (Waddington 1973). Thomas Edison proposed using aircraft to find and attack submarines in 1917, but aircraft endurance was only a few hours then. The operations anti-U-boat group section of the RAF developed methods of searching for U-boats visually and using radar, proper settings for depth bombs, night operations, and direction of attack. They were successful in reducing the probability that a submarine would escape from the Bay of Biscay into the Atlantic until the enemy introduced the snorkel tube.

What is most interesting to me is that the time line shows how rapidly people developed major OR models and methods. The rapid rates of development are similar to those in physics between 1900 and 1930 (Gamow 1985) and in biophysics somewhat later. This book reminded me that scientific progress is sometimes slow and sometimes rapid.

Some years were years of great accomplishments. For 1950, Gass and Assad cite key work in statistical decision theory (A. Wald), quality control (W. E. Deming), the prisoner's dilemma (A. W. Tucker), the Nash equilibrium (J. F. Nash), dynamic programming (R. Bellman), and agriculture (C. W. Thornthwaite). In fact, the 1950s were a decade of great progress in OR; the seeds planted during World War II bore heavy fruit.

I started my OR career in that decade; the time line shows that progress was faster than I remembered. In the 1950s and into the early 1960s, I tried to read every issue of *Operations Research*, *Management Science*, and the *Operational Research Quarterly*, hoping to keep up with the field. Now I realize why I kept falling behind. There were just too many important contributions for one person to understand and master.

1961 was an amazing year; Gass and Assad discuss simulation programming languages (G. Gordon and H. Markowitz), decision trees (H. Raiffa and R. O. Schlaifer), Little's law (J. D. C. Little), packet switching (P. Baran), goal programming (A. Charnes and W. W. Cooper), geometric programming (C. Zener), auction theory (W. Vickrey), and industrial dynamics (J. W. Forrester).

The authors have performed a great service in creating the time line (or calendar) of events that made and shaped OR. They provide more than a time line, also citing references and describing the inventions and individuals who made things happen. They also discuss many of the important books, the founding of national and international OR societies, OR journals, and some of the inventors.

Every OR professional should spend some time with this book. It tells us where we came from and how we got here. It is humbling to learn that some approaches or techniques were known, at least in theory, centuries ago. With careful reading, the reader will be able to trace the development of a field of OR or seek knowledge in the older literature.

Have Gass and Assad omitted or misconstrued events of importance? Most readers will answer Yes. When pressed to specify omitted events, they will have different lists. For example, the authors call Herbert Solow's article in *Fortune* magazine (April 1951, pp. 105–106, 146, 148) "the first article on OR that appeared in the U.S. popular press." The March 1951 *Scientific American* contained H. C. Levinson and A. Brown's article, "Operations research" (pp. 15–17).

So who was first? Is *Scientific American* part of the popular press? Did the March issue appear on newsstands before the April issue of *Fortune*? Such questions are mere quibbling. What is important is that by 1951 the general public was made aware of OR and its effective applications by business and government.

We could find some other examples, but they would not diminish the value of the book. I think this book is as good and true a time line as we can expect.

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Ernest Koenigsberg
2345 Divisadero Street, San Francisco, California 94115

Li, W. 2005. *Risk Assessment of Power Systems: Models, Methods, and Applications*. John Wiley and Sons, New York. 325 pp. $110.00.

The field of risk analysis—which goes by several names—has grown dramatically. Advances are being made in analytic techniques, and computers have become more powerful and less expensive. Society is less and less willing to accept even the smallest risks from increasingly complicated systems.

Governments and corporations seek to improve risk analysis of the electric power grid. According to the Electric Power Research Institute (EPRI), the US economy alone loses between $104 and $164 billion a year from power system outages. Citizens of Canada, England, Italy, New Zealand, and the United States have all endured recent large-scale blackouts.

The political response to this problem has been a mantra of "reliability at any cost" or short-lived commitments to making sure blackouts "never happen again." One analytical response is to improve the risk-assessment capabilities that enable managers and regulators to understand how different investment decisions, reliability policies, and operational protocols affect the trade-off between the cost of reliability and the cost of unreliability. We need more risk assessment of bulk power systems, and Li's objective is to close the gap between power-system risk theory and actual applications in the industry. Li, who works for the British Columbia Transmission Corporation, draws on his academic and industry experience and has based this book on many of his technical papers and reports.

This book is in three parts. In the main text, Li devotes four chapters to modeling and methods. This includes a chapter on parameter estimation in outage models, which typically receives less attention in the power-system-reliability literature than it deserves. In the last section of the main text, Li covers eight applications, including risk evaluation in generation and transmission planning, substation configuration, and maintenance. The perspective for these applications is the utility, and the industry context accounts for some nonutility generation but not for a full-fledged wholesale market administered by an independent system operator. Li ends the book with five appendices that cover critical background material, such as basic probability concepts, Monte Carlo simulation, power-flow models, optimization algorithms, and some probability-distribution tables.

Li discusses probabilistic transient stability analysis, an application that exemplifies the ever-expanding role of risk analysis in power systems. Following on the heels of probabilistic risk analysis in steady-state adequacy analysis, probabilistic transient stability assessment and the associated reliability criteria could replace deterministic transient stability criteria. Instead of the system being able to withstand the worst case scenario (more accurately, the somewhat well-defined, extreme scenario), the system would be designed and operated to satisfy risk criteria. To ease the policy transition from deterministic-based requirements to risk-based ones, analysts can develop risk-informed criteria, as they do for the safety regulation of US commercial nuclear power plants.

Even with Li's effort, we need a lot more work to complete the program of quantifying and analyzing the risks associated with bulk power systems. First, we need more work on common-cause and cascading failures, which Li does address. Second, we must incorporate into risk analysis the change in the industry from a vertically integrated structure to one with competitive-generation markets in many parts of the world. In principal, the effects of competition on reliability are ambiguous. During times of scarce resources, increased wholesale prices should increase supply even in the short term by improving unit availabilities while decreasing demand, assuming some level of price-responsive load, and both of these effects improve reliability. On the other hand, de-integrating the electric power system makes coordinated planning and operations difficult and could prompt market participants to cut reliability corners. Third, we should extend the risk-analysis program to incorporate operator error and nonbulk power systems, such as computer and information systems that are critical for planning and operating bulk power systems. Finally, we need to incorporate the costs of unreliability and reliability into risk assessments.

Li has advanced the application of sophisticated risk analysis to electric power systems, which we hope will continue to expand to incorporate the changing industry.

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Frank A. Felder
Center for Energy, Economics and Environmental Policy, E. J. Bloustein School of Planning and Public Policy, Rutgers, The State University of New Jersey, Rutgers, New Jersey 08901, ffelder@rci.rutgers.edu

Paolucci, Massimo, Roberto Sacile. 2005. *Agent-Based Manufacturing and Control Systems: New Agile Manufacturing Solutions for Achieving Peak Performance*. CRC Press, Boca Raton, Florida. 269 pp. $79.95.

We all know that globalization and the information technology revolution have put manufacturing systems on a path of rapid and remarkable transformation, but it takes a good book like Massimo Paolucci and Roberto Sacile's to give us a peek into the even more remarkable transformations that are on their way. Paolucci and Sacile are professors at the University of Genoa, in Italy, and both have impressive expertise in agent-based techniques for designing decision-support systems.

In their book, they look at decision-support systems in manufacturing. The fundamental transformations that manufacturing systems are going through are largely in flexibility, adaptability, and responsiveness to change, and the techniques leading these transformations are agent-based techniques. In the opening pages of the book, the authors explain what *agent* means in this context and what multi-agent systems

(MASs) are. They make the point that manufacturing systems are likely to be the most fruitful field for applying agent and MAS technology.

The book has seven chapters covering MAS architecture and how to design MASs for manufacturing, details of techniques the authors and others developed for planning, scheduling, and controlling manufacturing operations, the use of simulation in designing MASs, and their implementation. The book also includes a chapter on real-world applications of MASs in manufacturing and a concluding chapter on future trends and prospects. The authors close almost every chapter with a case study of a hypothetical company, PS-Bikes, and its use of the MAS techniques discussed in the chapter.

Although all chapters are extremely comprehensive, the chapter on agent-based simulation is particularly detailed and informative. Simulation is one of the most powerful tools in agent research, and the authors have wisely devoted a chapter to this subject. They begin by explaining the differences between agent-based simulation and non-agent-based simulation. First, the entities of an agent-based simulation model are modeled as agents. Second, these entities communicate with each other, perceive changes in their operating environment, and are capable of proactive behavior. Third, agent-based simulations tend to be intrinsically distributed, because agents have autonomous behavior built into them.

Paolucci and Sacile make it clear that agent-based simulation focuses on behaviors because the agents are entities with defined behavior. Therefore, one of the first considerations in judging whether agent-based simulation is appropriate in a given situation is whether the system can be represented in terms of entities with specifiable behaviors. The authors give the example of a supply chain, an area in which agent-based simulation is increasingly used. The supply-chain environment is a natural for agent-based simulation: The entities are the production and distribution enterprises in the supply chain, and the behavior of any given entity is influenced by the behavior of other entities. Therefore, one can view the resulting supplies, inventories, costs, and so forth as a complex-system behavior arising out of the decisions and behavior of the individual entities.

The authors cite the work of Parunak et al. (1998), who studied the well-known bullwhip effect in supply chains with both agent-based and traditional equation-based modeling. They recommend agent-based modeling for situations with a large number of distributed decision makers, and see equation-based modeling as better suited to centralized decision-making systems whose unfolding can be stated in terms of definite laws rather than information processing.

The authors provide a very useful discussion of some platforms that support multiagent-based simulation; for example, SWARM, developed in the mid-1990s, and MAST, developed around the turn of the century. MAST was developed especially for simulating manufacturing environments, and in fact, the acronym stands for "manufacturing agent simulation tool."

When it comes to implementing agent-based systems, many researchers originally designed their own frameworks to suit their own purposes. But as the field matured, they felt that they needed standards to insure interoperability of various systems. The authors describe the role of the Foundation for Intelligent Physical Agents (FIPA) in developing such standards.

Researchers have designed many successful multiagent systems that provide manufacturing systems with the agility that the modern business environment demands, and the authors have devoted Chapter 6 to describing some of the outstanding ones. They discuss two models that provide a supply-chain-oriented solution to planning and scheduling in manufacturing and describe other examples in planning, scheduling, and shop-floor control in manufacturing. But the authors lament that, in spite of the abundantly demonstrated potential of agent-based techniques in manufacturing, there are very few actual industrial applications.

In their concluding chapter, Paolucci and Sacile look into the future. They discuss a number of problems that they think must be addressed before agent-based systems can find widespread use, including some political or human-interaction problems. Software engineers promoting agent-based techniques tend to exaggerate their benefits and minimize the difficulties of designing and implementing these systems. The solution, the authors point out, lies in developing a widely accepted justification methodology for such systems. Protagonists of these systems can seldom say why agent-based systems should be used. What they need is a cost-benefit-based method of making a watertight case for such systems in a given situation.

The book's readability would have been considerably enhanced had the authors included an index or glossary of acronyms. They use quite a few, many of which cannot be considered standard or well known. Although one can find some of the acronyms in the index and trace them back to the page of their first use, many others are not listed. Readers must thumb through the book to locate where they are first used and explained.

All in all, the book is an impressive and thought-provoking compendium of things to come in the development of manufacturing management and control systems. The authors cover thoroughly a very complex field in which software, hardware, and artificial intelligence can play mutually supportive roles to change the structure of manufacturing systems for the better.

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Jamshed A. Modi
S. P. Jain Institute of Management and Research, Andheri (W), Bombay 400 058, India

Sethi, Suresh, Hanqin Zhang, Qing Zhang. 2005. *Average-Cost Control of Stochastic Manufacturing Systems*. Springer, New York. 324 pp. $79.95.

In their book, Sethi, Zhang, and Zhang focus on optimal controls for manufacturing systems that consist of machines subject to breakdown and repair. The overall objective is to choose the rate of production properly to meet the demand at minimum cost. This book is a sequel to Sethi and Zhang's (1994) *Hierarchical Decision Making in Stochastic Manufacturing Systems*, a popular and frequently cited book on production planning and stochastic control. In that book, they deal with discounted costs, whereas in the current book, they deal with long-run average costs exclusively.

Standard operations research courses have included inventory theory for years. The subject matter in this book originated with inventory theory, but it is a substantial generalization of the traditional setup. The systems under consideration range from a single-machine model to flow shops and job shops. The costs under consideration include production, inventory, and shortage costs.

The essence of the book is the idea of near-optimal control, rather than exact optimal control. Loosely, in the limit system, the randomness of the machines' capacity is replaced by an average capacity of the machines together with modifications of the cost function. Thus, the limit system is much simpler than the original system and can be solved relatively easily. One then devises production strategies based on the limit system and applies such strategies to the original system. Such an approach leads to a fulfillment of the objective that is close to the exact optimal one. This alternative is of great practical value. It is particularly useful for large-scale and complex production-planning systems. A subtitle for the authors' book could have been, "Near-optimal control of manufacturing systems under long-run average costs."

Long-run average cost criteria, the focus of this book, are more desirable in practice than the corresponding discount cost criteria. In long-run average cost problems, one can replace time-varying characteristics with steady state ones, so they are practically more appealing. Mathematical analysis of long-run average cost problems is notoriously more difficult than that of the discount cost counterpart. Thus, from a mathematical point of view, the analysis the authors develop is highly interesting in its own right. The results obtained and techniques developed may be applicable for control problems in other areas.

Consisting of 11 chapters and six appendices, the book is divided into five parts. Part 1 is a prologue, containing descriptions of a number of manufacturing models. Part 2 covers optimal strategies for parallel machine systems, dynamic flow shops, job shops, and those under risk-sensitive criteria. Part 3 is devoted to near-optimal controls of the systems introduced in

Part 2. Part 4 contains further discussion and some open problems. Part 5 contains six appendices, including background material on such subjects as Markov chains and viscosity solutions.

The authors intend the book for people working in manufacturing systems, operations research, applied probability, and systems theory. Although some proofs require advanced mathematics, the results presented are accessible to practitioners. Although there have been several excellent books on manufacturing systems (Arrow et al. 1958, Bensoussan et al. 1983, Gershwin 1994) (with emphasis on economic theory, mathematical results, and systems engineering, respectively), this book is one of a kind, and it deals with long-run average costs exclusively.

In summary, this is an excellent book devoted to a fast growing area that has a major impact on the economy. Sethi, Zhang, and Zhang have made substantial contributions to the operations research and control systems communities. Like its companion work (Sethi and Zhang 1994), this book should stimulate further research in production planning, systems theory, and stochastic control.

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G. Yin
Department of Mathematics, Wayne State University, Detroit, Michigan 48202, gyin@math.wayne.edu

Ziemba, William T. 2003. *The Stochastic Programming Approach to Asset, Liability, and Wealth Management*. The Research Foundation of AIMR, Charlottesville, Virginia. 192 pp. $50.00.

William Ziemba's monograph for the Research Foundation of the Association for Investment Management and Research (AIMR) is a welcome publication by one of the world's experts in financial data and investment techniques. The book covers some of the most sophisticated discussions in finance over the past 50 years, including capital-asset pricing, options pricing, utility theory, and sequential games. But Ziemba's starting premise is what can investors do with such sophisticated technology in markets that include momentum investors, bettors on mean reversion, mutual fund buyers, covered call strategists, and risk arbitrageurs—that is, real markets.

This book is not an academic tome with careful beginnings, logical middles, and tidy endings. It reads like a series of vignettes. In each section, Ziemba provides a brief, sophisticated preparation for a basic topic and follows that with a deep dive into its implications, densely illustrated with volumes of real financial data and discussions of practical consequences. Ziemba's thesis is that investors need to supplement many of the basic investment models in finance with careful analysis of investment strategies and careful construction of extreme scenarios to stress those strategies. Furthermore, of all the basic modeling frameworks in finance, stochastic programming is the only one that fully provides the flexibility needed for modeling strategies and generating scenarios.

The book is short (192 pages—a welcome relief in this era of word-processor fattened tomes), and so it does not cover every aspect in full detail. Ziemba does not intend to teach people how to make and solve stochastic programming models, but rather to educate them about modeling and data issues in wealth management. Fortunately, some reasonably good texts are available in stochastic programming. The Society for Industrial and Applied Mathematics (SIAM) and the Mathematical Programming Society, for example, are planning to publish a book on applications of stochastic programming edited by Wallace and Ziemba, that contains many articles with computer codes and well-developed applications. (Up-to-date information about the field, its literature, and computational infrastructure can be found on the stochastic programming community home page at http://stoprog.org/.)

A pioneer of stochastic programming and its application to financial modeling, Ziemba has written a very useful book. He writes for an MBA and master's level audience in a simple chatty style, and he provides deep insights. For more technically oriented readers, a 72-page mathematical and technical appendix is available. None of this material is too mathematically complicated for the average graduate student in operations research or financial engineering. But while the level of mathematics is standard, the level of thinking is very advanced.

In Chapter 1, Ziemba reviews static portfolio theory, introducing mean-variance analysis, capital asset pricing, Arrow-Pratt risk aversion, stochastic dominance, the Sharpe ratio, and portfolio management expected utility portfolio theory. He covers the importance of the accuracy of mean estimates and provides the financial economics background readers need to understand the static and multiperiod stochastic programming models in later chapters.

In Chapter 2, Ziemba discusses extreme events and the use and estimation of scenarios from an economic and mathematical point of view. His discussion on the bond-stock crash prediction model is worth the price of the book! This model focuses on periods when long-bond yields are much higher than earning yields of stock. Whenever the ratio was in the danger zone in Japan between 1948 and 1988, there was a crash (a 10 percent or greater decline within one year). The 1990 crash in Japan and the recent 2000–2002 crash in the United States, Canada, and Hong Kong are similarly predictable. When billions are at stake, such predictions are very useful. Generally speaking, prediction models like these imply a shifting of probability distributions over scenarios. In strategies, such as fix-mix, a stochastic programming approach should lead to superior results.

Avoiding trouble is a central theme in the rest of the book. The number of extreme events (those with realizations beyond the range of all previous data) is unfortunately on the rise. Ziemba advocates obtaining scenarios from all sorts of sources, such as past data analysis from econometrics and statistics, expert opinions, and economic models such as mean reversion, in addition to the crash measure. In the text and in the appendix, Ziemba provides a great deal of information about mathematical techniques for scenario generation, with many references to technical articles about this important and complex subject.

In Chapter 3, Ziemba discusses insurance models, such as the famous Russell Yasuda Kasai model and others implemented by the Frank Russell group, with whom the author had a long and distinguished consulting engagement. In Chapter 4, he discusses pension plans, and highlights the increased risk of investing heavily in one's own company's stock. He develops a case study for an Austrian pension fund. He compares continuous-time approaches with the discrete-time, scenario-based stochastic programming approach. Chapter 5 concerns individual asset/liability management for wealthy individuals and institutions, particularly for retirement and endowments of universities and other foundations. This is a vast new area of application and one in which the scenario-and-stochastic-programming approach has much to contribute.

Chapter 6 concerns hedge fund strategies—including typical trades, performance, risk control, and the Kelly or capital growth theory of investment over time. Many of the great investors, such as Buffett, Keynes, Soros, and Thorp, used such strategies. Ziemba also discusses the long-term capital-management disaster in 1998 and other similar failures. The recipe for disaster here is to overbet, that is, not to truly diversify and then to be hit by a bad scenario. A good stochastic-programming approach should prevent an investor from overbetting or failing to diversify in any scenario.

In the appendix, Ziemba adds technical details on portfolio management, how to estimate one's own utility function, how to generate scenarios, model formulations, and some case examples. The case examples include discussion on a Nikkei put warrant risk arbitrage, theoretical results for Kelly betting, blackjack and lottery betting, how to optimally choose fractional Kelly strategies to stay above a specified wealth path with high probability, and the good and bad properties of Kelly and fractional Kelly wagering systems.

The monograph is an easy way to learn about the stochastic-programming scenario approach to modeling complex asset-liability-management decisions. The advantages of this approach, as stated in the preface, are worth summarizing:

- Normality or lognormality is not an accurate representation of actual asset prices, especially for losses, and need not be assumed.
- Tail events are easily included. Downside probabilities estimated from actual option prices are 10 to 100 to 1,000 times fatter than lognormal.

• "Normal" and "crisis" economic scenarios (with higher and differing signed correlations) can be considered.

• Determining the exact scenario distribution accurately is not necessary to obtain model performance that is superior to that of other models and strategies, such as mean-variance or portfolio insurance.

• The best decisions are those determined in light of the decision maker's relevant constraints, uncertainties, and preferences.

• Most of the natural, practical aspects of asset/liability applications can be modeled well with the multiperiod stochastic-programming approach. Methods to solve such models are now highly developed and can be implemented on high-performance personal computers. Model output is easy to understand and interpret if one uses good graphical interfaces that are user friendly and easily understood by such nonoptimization experts as pension fund trustees. The models can be tested via simulation and statistical methods, and considerable independent evidence demonstrates their superiority to other standard approaches and strategies.

• The approach tends to protect investors from large market losses by considering the effects of extreme scenarios while accounting for other key aspects of the problem. The world today is very dangerous with many extreme scenarios. For example, the use of derivatives worldwide has grown into a US$142 trillion industry. Although much of this trade is for hedging and reducing risk, the sheer volume adds new risks.

• Determining whether investment positions are truly diversified and of the right size across time is crucial to protecting against extreme scenarios and ensuring that the results will be good in normal times and protected in extreme times.

Stochastic programming has always been recognized as having great potential in the field of asset liability and wealth management. Ziemba's book will make some of the key ideas of the field more accessible to operations research students and faculty and to their counterparts in the actuarial and investment sciences.

Alan King
IBM Research, PO Box 218, Yorktown Heights, New York 10598

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