Operations Research

The Analyst's Bookshelf

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Reviews

RUFUS ISAACS, *Differential Games: A Mathematical Theory with Applications to Warfare and Pursuit, Control and Optimization*, John Wiley and Sons, New York, N.Y., 1965, 408 pages, $15.00

THE STUDY of differential games began in earnest in the early 50's at the Rand Corporation, motivated by military applications. DR. ISAACS developed his theory in a series of Rand memoranda[1] in 1954–55. The present volume is based on these groundbreaking investigations and on some later developments. Its appearance is particularly timely because of promising new applications in control theory.

A differential game concerns a differential equation

$$\dot{x}(t) = f[x(t), u(t), v(t), t], \tag{1}$$

where t is time, x is the state vector, u and v are the control vectors manipulated, within constraints, by the players. The game terminates when the point $[t, x(t)]$ encounters a given terminal manifold in (t, x) space, and the payoff is determined by the value at the terminal point of a function defined on this manifold. It is assumed that each player is aware of the current time t and state $x(t)$, at all times. Then the strategies of the players are described by functions $U(\cdot, \cdot)$, $V(\cdot, \cdot)$, which specify the dependence of the player's control upon time and state $u(t) = U[t, x(t)]$, $v(t) = V[t, x(t)]$. U must be selected by the minimizing player in a set $\mathcal{U}$ of admissible strategies and likewise V in a set $\mathcal{V}$. For each pair (U, V) the solution of the differential equations, from specified initial conditions (t_0, x_0), is presumed to exist and impinge on the terminal manifold.

This defines a payoff function $W(U, V)$, hence a game on the sets $\mathcal{U}$, $\mathcal{V}$. If this game has a saddle point, that is, if for some U^*, V^*,

$$W(U^*, V^*) = \min_{U \epsilon \mathcal{U}} W(U, V^*) = \max_{V \epsilon \mathcal{V}} W(U^*, V),$$

then this defines the value $J(t_0, x_0)$ of the game for the given initial conditions.

Furthermore, if $\mathcal{U}$ and $\mathcal{V}$ are sufficiently rich in strategies, then U^* and V^* can be chosen to be valid for all initial conditions in the region where saddle points exist.

Using the value function $J(\cdot, \cdot)$, the author goes through a dynamic programming procedure similar to the one used in some derivations of Pontryagin's maximum principle and obtains necessary conditions on the functions U^*, V^* and J. These have the form of a minimax principle, involving a costate p interpreted as the gradient of J and satisfying the familiar adjoint equations and transversality conditions. The values of U^* and V^* at given x, t, and p are obtained as a saddle point of the Hamiltonian (p, f) with u, v varying over their constraint sets.

These conditions allow backward integration from the terminal manifold to reconstitute U^*, V^* and J line by line, and this procedure can be interpreted as the solution by characteristics of a Hamilton-Jacobi equation.

This formal analogy with the ordinary (one-sided) variational problems can only be developed in regions where J is continuously differentiable, a fact of which the author is very much aware. Now it turns out that in differential games, singularities are more the rule than the exception and the main features of many games revolve around them. Thus, most of the book is concerned with the study of different types of singular surfaces, both in general and with the help of a beautiful collection of examples.

Some of these singularities (e.g., the 'equivocal' surfaces) have no exact parallel in one-sided variational problems. Therefore the reader familiar with optimal control should check his tendency to reason by analogy.

Now let us review some of the difficulties of differential game theory and the position taken with respect to them in the book.

1. *The Definition of the Game and of Solutions*

The most immediate way of defining differential games and their solutions is the one outlined above, following BERKOVITZ.[2] This approach is hampered by the following difficulties:

- It is not clear how the sets $\mathcal{U}$ and $\mathcal{V}$ of strategies should be defined in a given physical situation.
- It is necessary, in order to define W, that every pair (U, V) be a 'playable pair',[2] i.e., a pair that enables solution of (1) to terminate. No general procedure to check this assumption is known.
- Furthermore it is not clear in what sense the dynamic equations are to be integrated. It seems that it should be in the sense of FILIPPOV,[3] which would take care at least of the 'afferent' singular surfaces.

An alternative approach, originated by Karlin and followed in the book, is to consider only piecewise constant controls dependent on sampling of the state at discrete intervals. A strategy for one player is defined by a sampling grid and, for each sample time, a function defining the constant control over the next interval in terms of the last sampled state. W can be defined over the class of all pairs of such strategies, provided the dynamic equations can be solved for piecewise constant controls and the corresponding solutions reach the terminal manifold.

Now an upper value can be defined as the inf sup of W over such strategies and a lower value as the sup inf of W. When these are equal, the value of the game is thereby defined.

Since the optimal solution requires, in general, continuous sensing of the state, these suprema and infima will not be attained. The optimal continuous-time policies U^*, V^* and corresponding trajectories must be obtained either by a limiting process or from the value function, by the necessary conditions.

2. *The Existence of a Value*

In either way of defining strategies, the upper value may turn out to be greater than the lower value, so that no pure value and, a fortiori, no saddle point exist. In the book it is assumed that a saddle point does exist and the necessary conditions are derived on this basis. The 'verification' of the purported solution (*see* below) will prove a posteriori that a saddle point exists. A priori, we only know that, if the Hamiltonian has no saddle point for the t, x, p of interest, then neither has the problem.

At first sight, the permanent availability of x seems to indicate that the game is of perfect information and hence should have a pure value. To see that this is fallacious, consider the sampled data game in which both players sense x and update their controls at the same grid of times. Then at each sample point their decisions are simultaneous and the game is not of perfect information. Hence in the limit of continuous sampling, infinitesimal differences between upper and lower values at each stage may compound into such a difference for the whole game. Some of these questions have been clarified by FLEMING.[4]

3. *The Verification Theorem*

This is the device used to check whether a purported solution is indeed correct. Suppose J, U^*, V^* have been found in an open connected region R of (x, t) space, and let T be a part of both the boundary of R and the terminal manifold. Suppose that for all (t_0, x_0) in R the trajectory when U^*, V^* are used stays in R and reaches T at a point where the payoff is $J(t_0, x_0)$. Then it is claimed that if J is the only continuously differentiable solution of the necessary conditions (Hamilton-Jacobi equation) on R, then the solution is correct.

In fact, it is also necessary that, if the first player follows strategy U^* and the second player any admissible strategy V, then the resulting trajectory from any (t_0, x_0) in R *stays in* R until it impinges on T. The symmetric requirement, for V^* and arbitrary U must also be fulfilled. Unfortunately, these requirements are not made sufficiently clear in the statement given in the book.

Note in this connection that to verify a saddle point by any method whatsoever one need only consider pairs of the form (U^*, V) and (U, V^*), and therefore a problem may have a meaningful solution even when the payoff is not defined for all pairs (U, V). Hence the requirements of Berkovitz are in a sense unnecessarily strong.

4. *Absence of a Pure Value*

If the upper value exceeds the lower value of the game, then the variational methods based on the saddle point condition break down. This can happen in the differential games as discussed above (when there is a lack of convexity), and is even more likely if the amount of information available to the players is decreased. Classically, game theory then recommends the use of randomized strategies, a concept briefly discussed in the last chapter of the book, but great technical difficulties lie in the way, since one must randomize continuous-time strategies.

It is regrettable that little attention has been given to the determination of the upper and lower values with the corresponding strategies for pessimistic players. Again dynamic programming provides an immediate approach to this question, but new types of singularities are to be expected.

In conclusion, this book with its lively style, its profound insight, and its fascinating examples, is a most enjoyable and worthwhile addition to the applied mathematics literature. A long time may elapse before all the results are rigorously justified and all the research problems proposed by the author solved; but this only enhances the value of this pioneering work.

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H. S. WITSENHAUSEN
Massachusetts Institute of Technology

ROBERT E. MACHOL, in collaboration with WILSON P. TANNER, JR., and SAMUEL N. ALEXANDER (editors), *System Engineering Handbook*, McGraw-Hill Book Co., New York, N.Y., 1965, 1085 pages, $29.50

A FEATURE of our society in recent years has been the growth and admixture of science, government, and business. Large complex systems are needed for defense, for transportation, for communication, and for many other sectors of modern society. From sheer necessity, to telescope time and integrate multiple components, a systems approach aiming toward the fulfillment of a total objective at an acceptable cost has developed. The *System Engineering Handbook* provides in a single volume an almost complete exposition of the many techniques and disciplines associated with the engineering aspects of this systems approach.

There is no universally accepted definition of systems engineering, nor is there a common understanding of just what it should encompass. The term is used to refer to various aspects of the definition, design, development, installation, and management of complex man-made systems. It is thus concerned both with the internal efficiency and optimal choice of a system. Hence, in addition to traditional engineering and mathematical skills, it demands a knowledge of economics, operations research, and the behavioral sciences.

The first-generation systems engineer can probably be characterized as someone who, starting with a sound education in science or one of the branches of engineering, has acquired the necessary working knowledge of economics and operations research. No clear pattern for the training of systems engineers has yet emerged, and I found no particularly impressive example of a coherent program among the courses taught at a number of universities. This Handbook, however, is not the ordinary engineering handbook that contains tables and formulas and which the practicing engineer (sometimes even the student) can pick up, use, and set aside. Rather, this volume is designed to be studied, for it contains a theoretical treatment of the engineering tools and information background needed for the practice of systems engineering. Hence it should be very helpful to practicing engineers and scientists of various specialties desiring to learn something about systems engineering.

The *System Engineering Handbook* is a tremendous work involving forty-one chapters prepared by fifty-eight authors. I could not imagine anyone collecting a list of contributors more distinguished or better qualified (BELLMAN on dynamic programming, BOULDING on economics, DANTZIG on the simplex method, EDIE on urban areas, GERLOUGH on simulation, HOWARD on probability, KORN on computers, MORSE on queues and Markov processes, THRALL on game theory, and many others in fields less familiar to an operations-research audience). I prevailed on some of my colleagues to look at a few of the many chapters for which I have no competence. They universally reported that the presentations are well written but concise and require both study and a greater mathematical sophistication than suggested in the Preface. Many of these chapters are, of course, more a source of general orientation than of data, but the excellent index and good bibliographies that are provided should be helpful there. It is a tribute to the editors that there seems to be little duplication.

The forty-one chapters are separated into six parts. The first of these contains a single chapter, "Methodology of System Engineering," by the senior editor, ROBERT E. MACHOL. In nine pages it contains essentially everything said in the volume about the systems viewpoint and the general principles of systems design. Here is found the only mention of the phases and steps in systems engineering and a too brief discussion of such terms as criteria, suboptimization, measures of effectiveness, and cost-effectiveness.

The titles of the other five parts and some sample chapter headings are as follows: "System Environment (six chapters: 'The Ocean,' 'Land Masses,' 'Urban Areas,' . . .)"; "System Components (fourteen chapters: 'Radar,' 'Infrared,' 'Guidance,' 'Propulsion,' . . .)"; "System Theory (nine chapters: 'Information Theory,' 'Game Theory,' 'Decision Theory,' 'Feedback Theory,' . . .)"; "System Techniques (seven chapters: 'Simulation', 'Reliability', 'Economics,' 'Management,' . . .)"; "Useful Mathematics Associated with Systems Engineering (four chapters: 'Probability,' 'Numerical Analysis,' . . .)".

The chapters on environment are almost unique in a work of this type. The material is very condensed; it is impossible to do more than brush lightly the fields of astronomy and space together in thirty-six pages. What is remarkable is that this section appears at all.

Because the book looks so complete, one may think that except for greater depth and detail everything one needs to know about system engineering is here. This is not the case. In the definition of systems engineering used in this collection, the significant word is "design." The emphasis in the various chapters is on design for better performance with little attention paid to design for lower cost. There are no chapters on cost analysis or cost estimating. Whatever qualms I have about this work are associated with the statement attributed by Boulding to an unnamed economist (p. 35–1), "the engineer is a man who spends his life finding out the best way to do something that should not be done at all." The managerial aspects of systems engineering and the problems of optimal choice—how to show a project is indeed worthwhile—are slighted; and there is almost nothing about criteria. The subject of how one builds models—for example, the process of turning assumptions about environment, components, costs, and communications into mathematical equations to be programmed for the computer—is not treated within the volume but left to the references. Nothing is here about how to handle the interface between social policy and the typical systems engineering project, nor about techniques to handle intuition and judgment, and neither are there references for these.

E. S. QUADE
The Rand Corporation

PAUL H. COOTNER (editor), *The Random Character of Stock Market Prices*, The M.I.T. Press, Cambridge, Mass., 1964, 522 pages, $15.00

HAVING had a go at stock market prediction in 1956,[1] this reviewer's initial reaction to a book on the *Random Character of Stock Market Prices* was similar to that of the Bostonian lady in one of J. P. Marquand's books who asked her husband what their daughter was reading. "It's a book by Freud," he answered, "and it's about sex." "How in the world," she asked, "can anyone write a whole book about That?"

As it turns out, the papers presented in this book constitute a lively and scholarly treatment of stock market randomness, combining the clinical detail of Masters-Johnson with the variations on a common theme of the mithunas of Kandarya Mahadeo.

Most of the twenty-one papers deal with the single question: Given historical data on the price of a stock, will this data help predict future price movements? COOTNER observes that few "market technicians" would doubt that historical data can be used to aid predictions, while few "academicians" believe that "such a backward look is of any substantial value."

Since the papers in this book are by academicians, it is not surprising that a convincing case is made for the unlikelihood of price prediction based on price history alone. Unlike the market technicians, however, most of whom seem to look to the future for validation of their methods, the academicians have diligently documented their case. The conclusion is that future price changes are *almost* completely independent of past changes. Most of the first half of the book is devoted to tests of the hypothesis of independence, and the hypothesis is supported to a convincing degree. However, before the reader has abandoned all hope of market predictability, evidence is given for the existence of a weak coherent structure susceptible to prediction and underlying the dominating Brownian motion.

In the course of these investigations, the reader is exposed to the full battery of modern statistical tools for the analysis of time series. The tools themselves come in for examination, and their limitations are exposed. For example, much standard methodology requires that the variance of the process under examination be finite: This may not be true for stock market price changes. One view is presented as the "stable Paretian hypothesis"; an opposing view has recently been printed in *Operations Research*.[2] The need for new methods of statistical testing and several examples form important portions of the material presented.

Thus the book exhibits not only the sophisticated application of existing correlation and spectral analysis methodology, but also new directions of research when these methods are not appropriate.

Workers in military operations research, where a small amount of data is often made to bear the support of an impressive edifice of theory, can hardly do better than to read this book as an example of the interplay between theory and data analysis that constitutes the proper application of the scientific method. Not the least instructive are the papers by OSBORNE (both printed originally in *Operations Research*) in which the perceptive analysis of a great deal of data is used to (1) partly confirm a prior hypothesis, (2) modify and improve the existing theory, (3) reveal a fine structure of market movements that suggests new models of market action, of which one has been presented by Osborne elsewhere.[3]

The book is organized in four parts. Each is preceded by an introduction by Cootner which discusses the constituent papers and their place and significance in the historical development of the subject; these essays are worth the price of admission in themselves. It is easy for a collection of papers to have in common only a roughly defined area of interest, but here Cootner's commentary not only welds the selections into a coherent whole, but by referencing and discussing related studies that are not included, it extends the scope and comprehensiveness of the book.

The four parts of the book are:

I. Origins and Justification of the Random Walk Theory
II. Refinement and Empirical Testing
III. The Random Walk Hypothesis Reexamined
IV. The Statistical Analysis of Option Prices

All of the papers are of high quality. In this limited space only a few can be singled out for comment.

In Part I the basis for the "random walk theory of stock market prices" is presented. A paper by H. V. ROBERTS demonstrates that a random (and unpredictable) process can generate a time series that looks startlingly like a graph of stock prices. The classical and brillant paper by LOUIS BACHELIER on "Theory of Speculation" is presented in translation from the French. As Cootner points out, the Bachelier paper, published in 1900, anticipates the modern theory of stochastic processes, and derives, on a heuristic basis, many results that were later rediscovered in the process of a rigorous development of the theory of random processes. Bachelier hypothesizes what is now known as a "Brownian motion" model of price movement and solves it to obtain probability density functions for diffusion in the presence of an absorbing barrier, and first passage times. Like Oliver Heaviside, his interest is in applications; and his model is tested against market data, then applied to the valuation of stock options. This model, spectacular in its simplicity, can, after sixty years, be improved only to a small degree in providing a close statistical description of market movement.

In Part II, market structure is examined in greater detail, the paper by M. F. M. OSBORNE standing as a landmark in data analysis and organization. A correlation analysis paper by M. G. KENDALL and a spectral analysis paper by C. W. J. GRANGER and O. MORGENSTERN reveal that, within the limits of resolution of these analytical tools, the past of the market bears negligible relation to its future, although the latter paper admits the possibility of a very weak long-term (years) cyclical component which "will rapidly disappear as it is being made use of."

Having established the close correspondence of market price movement to that of a random walk, successive papers concentrate on the (small) observed discrepancies in this model.

The initial Brownian walk model treated traders as a homogeneous aggregate. The paper by ARNOLD MOORE builds upon prior formulations by Holbrook Working and F. W. Taussig to postulate a market that consists of two kinds of traders—the well-informed and the inept: the former have early access to information that will affect future stock prices; the latter "go with the market." This model is carried forward by Cootner, who suggests that these well-informed "professionals" buy when a stock has dropped sufficiently below their estimate of its value to justify the transaction and sell on a comparable basis, but that between these two extremes stock motion is a random walk generated by the transactions of the inept traders. The motion is therefore a random walk with reflecting barriers, but the barriers themselves are generated by changes in the expectations of the informed traders, and, Cootner's computations indicate, tend to have a measurable persistence. As Cootner points out, this model has much in common with the lore of the Wall Street chartists—the difference is that Cootner is concerned with validating the hypothesis rather than obtaining subscriptions for a market letter.

The possibility of prediction is examined by S. S. ALEXANDER as well as by Cootner; the market is indicated to be not completely random, but the predictable component is slight.

The history of scientific market analysis began in 1900 with Bachelier's desire to provide a basis for option values. This book closes, in Part IV, with a series of applications of the developed market theories to the valuation of derivative assets such as puts, calls, warrants, and convertible bonds. But having been brought full cycle, the analysis of market structure is by no means complete. These papers set a high standard for those that will yet appear, in objectivity, creative insight, and conscientious verification.

It is unlikely that the readers of this book will find in it a basis for early retirement as a result of stock market trading, but it can hardly fail to hold the interest of anyone engaged in constructing models of the real world and interested in their validation. It raises interesting and possibly alarming issues. Cootner says of MANDEBROT's paper and the "stable Paretian hypothesis:" "If he is right, almost all of our statistical tools are obsolete—least squares, spectral analysis, workable maximum-likelihood solutions, all our established sample theory, closed distribution functions." It suggests interesting psychological states: ". . . no rational man with a quadratic utility function would invest in stocks," and "buyers of puts and calls may have cubic utility functions."

And finally, it suggests that there are in fact at least two kinds of people trading in the market, the inept and the ept. There are many kinds of people in the world. Modelling two levels of competence is (as I have developed elsewhere[4]) an improvement over dealing with the "average" man. Human variability is one of the most interesting characteristics properly to account for in any systems analysis, and it is dealt with directly in portions of this book.

Modelling the stock market is only one example of the problem of determining the structure of a process when it is not possible to perform controlled experiments on it. This book displays the increasing penetration possible by the progressive utilization of sharper and more definitive analytical tools. If the market is, in fact, a "fair game," one may expect that the discovery of predictable elements will lead to the obliteration of their predictability. But in the process, the understanding of market structure, supply-demand relations, the associated decision process, and the behavior of people will be further clarified.

One final remark in a larger context. The book fairly disposes of the "technicians" who look only at price patterns. The "fundamentalists," who believe that future stock prices can be determined by other determinable characteristics of the company issuing a stock, are yet to be reckoned with.

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HERBERT K. WEISS
Palos Verdes Peninsula, California

CARL F. J. OVERHAGE and R. JOYCE HARMAN (editors), ***INTREX: Report of a Planning Conference on Information Transfer Experiments,*** **The M.I.T. Press, Cambridge, Massachusetts, 1965, 294 pages, $3.50 (paper)**

THIS IS a report of a planning conference at Woods Hole in August of 1965, at which a selection of librarians, scientists, and engineers were exposed to and wrote about the 'library problem.' It is gratifying to see a wider spectrum than the usual coterie of Washington scientists, who control so much of the nation's research and development, yet in this instance are too senior to have to use a library themselves, and whose attitude is expressed by the editorial in *Physical Review Letters* of 27th September 1965, which is supercilious and naive about what library automation could do for a scholar. (Even so, one wonders how many of the participants might have been, as a user, in a library within the last twelve months.) The result is disappointing, perhaps unavoidably so, in that the same old ground is gone over, with hardly a new idea or observation.

The major criticism of the main text is its preoccupation with Project MAC, now in its infancy at MIT—or perhaps, we should say, the "Cambridge intellectual community" (p. 41). The features of MAC applicable to the library problem, namely user consoles connected with files by a communication system, have been identified before, e.g., in *Automation and the Library of Congress*, edited by GILBERT W. KING, a survey sponsored by the Council on Library Resources, Inc. (available from the Superintendent of Documents, U.S. Government Printing Office, Washington, D.C., 20402, 1963, 96 pages, $2.00). What is wrong about the emphasis on MAC is the emphasis on numerical computing. The word "computer" appears on nearly every page in *INTREX*. Perhaps the editor would say he means "data processor," but does he sincerely believe this? Computers are no more applicable to the library problem than an abacus is to teaching spelling. One cannot add or multiply words, nor analyze grammar by Boolean algebra. A computer is not a data processor, as shown by the various DOD attempts to apply them to intelligence problems. Computers demand stringent formatting of data, a constraint foreign to the library task. The argument expressed in *INTREX* (p. 32) is that there is a great body of programming a MAC-like system will provide to the scholar. Is this programming applicable? It is largely numerical, its executive features primarily manipulate fixed-length words and highly formatted entries. The argument that this effort should be salvaged for the library is equivalent to have said you build jet engines because the gasoline industry is geared to high octane. Programming is a sorry mess of ad hoc, empirical, undocumented, theoryless, expensive, tendentious sorcery. It would have been refreshing to see an explicit recommendation that a new MIT look be made at the programming (if indeed this is even the right word) needed for the search, maintenance, and generation of files—which, after all, is the central theme of a library system.

The emphasis on existing programming is entangled with a de-emphasis of the memory requirements. True, at some points in the book, the topic is a scientific and technical library for MIT, where the number and duration of the items of the collection is relatively small. On the other hand, there seems to be an intent to create a large network of libraries (p. 1 and Chapter III) as suggested in *Automation and the Library of Congress* where the estimate of required memory size (10^{12} bits at least) was more realistic. The disaster of not facing up to memory requirements is that lack of memory has always imposed restraints on the information being put into it—e.g., the use of initials instead of full author's names, of only the first of a multiple author, or of only upper case letters.

"The presently fashionable computer retrieval methods are probably only a partial answer," to quote S. GOUDSMIT (*Phys. Rev. Letters, loc. cit.*), because the imposition of numerical programming and inadequate memory will constrain scholars to the language and methodology of computers rather than force data processors to become more like scholars. A minor example of this train of thought is the typography of the book under review. Thankfully it does have upper and lower case, but only two font styles, and consequently is not easy to read or refer to. Unquestionably, this was chosen in the interests of speed and cost of publication. If this is already the attitude of the leader of Project INTREX we can see a subjection of the library and publication standards to the law of the computer.

The serious intent of the conference was to plan experiments from which to design a system, and in this respect it was singularly unfruitful. There are laundry lists of things to look into, but little which thrills a potential user. The reason for this may be that there are no test-tube experiments in systems design. For example, the corpus on which to do the experiment must be large, larger than the 10,000 or so documents an individual personally can be at home with, in order for the experimenter to do something he could not do alone. Another dampening effect is the immense cost of prototype equipment that has adequate human-factor design so that the result of the test is not overwhelmed by some extraneous factor. For example, if a console is used to display typical library catalog cards (p. 46), and the mechanism only allows the user to 'turn' a card every few seconds, the whole test is jeopardized by impatience with an irrelevant feature. Further, the tests should be done by a real user—one who has to write his thesis, or a report, by a deadline. Such users with a real job to do will not use a gadget.

The words 'system design' are overworked these days, but refer to an important ingredient in projects of this kind, in an era where technology is largely under control, and individual experiments are a matter of routine. A good system is larger than its parts. *INTREX* seems very weak in appreciating that planning a program needs total system design.

A deeper critique of experiments for design was made by FUSSLER (p. 161) who points out the need for economic evaluation—for example, the trade-off of publication style and speed in printing this book, already mentioned—or even the real value and need of an information transfer system, or an automated Library of Congress.

Human beings do a remarkable job of learning in spite of the deplorable state of our libraries. But is the computer the desirable cultural tool to re-orient learning? Do we appreciate that computers, as distinct from possible data-processing systems, tend to emphasize things—items that can be labelled, formatted, put on punched cards or in memory locations—a tendency to which the Western civilization is already too prone. There is an argument that the scholarly writing of encyclopedias, source books, treatises and reviews may do more to preserve, organize and distil human knowledge, in sufficient detail. Nevertheless, there is no question that data-processing systems will be part of our intellectual activity, much more deeply than anyone yet can visualize, and let us hope the first seeds will be sown by Project MAC-INTREX at MIT.

GILBERT W. KING
The Aerospace Corporation

MANFRED KOCHEN (editor), *Some Problems in Information Science*, The Scarecrow Press, Inc., 257 Park Avenue South, New York, N.Y. 10010, 1965, 309 pages, $8.50

THIS BOOK, which consists of contributions by a number of authors, including the editor, should probably never have been written. It seems to me that an author (or, in this case, editor and author) should ask himself what audience his book addresses and what it has to contribute to that audience. Providing he has an answer to these questions, a book is in order. However, so far as I can see, this very spotty book does not measure up to this test.

It claims to address a study of generalized total information systems, subsuming under that heading such diverse items as a university considered as a total information system, and a completely automated computer-centered information system—with the presumption that both can be treated in the same way. A general description of the total information system is presented that attempts to rationalize these diverse entities within this single framework. While this attempt does not fail completely, it seems to be almost tautological. In fact, the articles within the book that deal with this general class of problem suffer from slight content, and will probably offer little to the average reader.

There are, however, some articles that deal with various elements within this descriptive framework, primarily from a mechanized point of view, and in these we find most of the book's content. Considerable emphasis is placed upon structured networks, where the network may be considered as a set of nodes (typically representing facts, articles, or other items that may be considered equivalent to nouns in ordinary speech). The nodes are connected by relations to form a network. Such networks are discussed almost solely from the point of view of graph theory. Within this discipline, some interesting relations are derived, although it remains to be seen whether they will, in fact, have any practical implications. The graph theory approach, if it is to bear fruit, depends upon these networks resolving into almost disconnected elements (the leaves and lobes of the graph theory). The world of library problems, on the other hand, does not seem to give rise to clean-cut clusters of documents or other equivalent groups.

To see whether the graph theoretic approach has any real contribution to make, we need some experiments involving a sizable collection of elements—say, several thousand adequately indexed documents or a few tens of thousands of citations from a reasonably homogeneous field. Without such experiments, one can hardly extrapolate to any meaningful information-science problem, since one usually does not really have a problem until his document collection grows into the tens of thousands.

A few of the articles in the book are provocative, and they do indeed stimulate thinking, though even in these cases it is not so much the results presented as the concepts expressed that lead to interesting speculation. In this group are the articles by ABRAHAM, REISNER, and FRAZER. While I have reservations about the extent to which graph theory will be useful in information science, it seems to me that this approach is at least worth following up. Miss Reisner's article on the patent citation index raises a number of questions, but it is provocative, provides some data, and presents a few interesting results.

In addition, the two articles by FLOOD and KOCHEN on automatic dissemination systems are descriptive and present noteworthy findings. The approach would seem to require more actual experiments to find the conditions under which this variety of dissemination technology will be useful, rather than further elaboration at the theoretical or conceptual level.

Finally, to close on a more positive note, I believe Mr. Kochen probably has something to say and that, if he can divorce himself from philosophy and present hard results, he could prepare an interesting book based on the same line of research he is attempting to put forward in *Some Problems in Information Science.* In particular, if he could aim for the presentation of results and abstain from speculation, we might all reap the benefits of his efforts.

DONALD L. DRUKEY
System Development Corporation

ARNOLD KAUFMANN, *Methods and Models of Operations Research*, Prentice-Hall, N.J., 1963, 524 pages, $16.00

THE DUST jacket states that this book "features a dual presentation of operations research—both with and without higher mathematics." The subjects that are covered—linear programs, queuing, inventory, and deterioration, replacement and maintenance of equipment—are in fact covered twice; once in Part I, which is titled "Methods and Models," and once in Part II, which is titled "Mathematical Development." The level of mathematics required for Part I is differential and integral calculus, including partial differentation and some acquaintance with probability and statistics. The mathematics needed for Part II is indicated by a chapter that contains a brief summary of matrix algebra, probability and statistics, and symbolic calculus (Laplace transforms).

In the foreword the author indicates that the book is intended to facilitate communication between engineers, managers, and accountants on the one hand, and analysts on the other, by being adapted to the mathematical level of both groups and by being concerned with factors currently utilized in industry for the examination and analysis of economic or management phenomena.

As a possible text, the book has some advantages and one major disadvantage. The advantage is that the arrangement of the material makes it possible for instructors to choose the mathematical level at which they cover the topics, a particularly useful option at the sophomore or junior level. The book's value as a text is improved by including standard statistical tables and a table of transforms. The major disadvantage is the absence of exercises.

The book was originally published in French in 1959, a fact that should not detract from its usefulness as a text.

DANIEL TEICHROEW
Case Institute of Technology

C. A. BAKER (editor), *Visual Capabilities in the Space Environment*, Pergamon Press, 44–01 21st Street, Long Island City, New York, N.Y. 11101, 1965, 211 pages, $10.50

THIS BOOK is a collection of articles sponsored by the Human Factors Society of America. All but three first appeared in a special issue of *Human Factors*, the official publication of the society. It is of interest to operations analysts not only because of its applications to operations in the space environment but to other operations that involve the use of the human eye. Most of the articles are interesting, instructive, and include rather comprehensive bibliographies at the end. The publication, therefore, is a good source book for recent work related to human vision. In an attempt to provide something of the flavor of the book, comments on some of the papers and on the book as a whole are presented in the paragraphs which follow.

"Visual Experiences of the Astronauts and Cosmonauts" is a descriptive paper that provides the reader with some appreciation for the general environment and the key problems to be faced in space flight. The four articles "Vision and Unusual

Gravitational Forces," "Visual Space Perception as Influenced by Unusual Vestibular Stimulation," "Space Cabin Atmospheric Trace Contaminants and Their Possible Influence on Visual Parameters," and "Vibration and Vision," taken together, constitute an excellent review of the known facts with regard to the effect of motion and other environmental conditions on the values of the pertinent visual parameters.

The search problem, familiar to most operations analysts, is discussed for the visual case in "Visual Search in the Space Environment." The writer of this article presents the subject simply and understandably, although he omits the probability mathematics needed to compute any specific case. "Vigilance: A Review and Re-evaluation" deals with the departures from laboratory performance to be expected in operational situations. The rest of the papers are of less general application and more specifically oriented to the space flight mission. They deal with such questions as optimum visual systems design, and the use of the eyes for specific space flight missions such as surveillance, docking, and landing on the moon.

After reading the book and other articles in the same area it seems to this reviewer that human factors as a science has two outstanding needs. The first of these is for more care with definitions, so that ambiguities such as the use of the term "visual acuity" by different writers, or even by the same writer, to mean different things, can be avoided. The second, referred to by some of the authors of the book, is for more theoretical work to provide mathematical models such that the results to be expected from a given experiment can be predicted from the results of other experiments that have been done in the past.

EDWARD S. LAMAR
Arlington, Virginia

AN-MIN CHUNG, *Linear Programming*, Charles E. Merrill Books, Columbus, Ohio, 1963, 352 pages, $9.00

THIS introductory text can be conveniently divided into three parts. After an introduction there is a long chapter that summarizes the theory of matrices including, for some inexplicable reason, a rather extensive treatment of the theory of determinants.

The second, and largest, part of the book consists of six chapters devoted to a leisurely treatment of the general linear programming problem including such topics as the simplex method and its variations, post optimality and sensitivity analysis, and duality theory. The author carefully avoids the definition-theorem-proof approach of mathematics textbooks in favor of a more verbal style that should appeal to the nonmathematically oriented reader. Numerical examples with business or industrial content illustrate virtually all the important points discussed.

The treatment in this section is thorough, although, because of the attendant technical difficulties, a complete treatment of the question of degeneracy is omitted. A few rather subtle mathematical mistakes crept in; for example, the definition of a basic feasible solution given on page 73 is incorrect, and on page 181, in the discussion of changes in the columns of coefficients, the author ignores the possibility of the basis becoming singular.

The third part consists of two chapters on variations and extensions of linear programming. The first, on the transportation problem, includes a discussion of Vogel's method for finding an initial solution, and the treatment of the transshipment problem as a transportation problem, as well as a description of an algorithm for solving the transportation problem. The last chapter contains a treatment of an algorithm for solving linear programs with upper bounded variables and brief sketches of integer programming, quadratic programming, and convex programming by means of piecewise linear approximation.

This book should be an adequate introduction to linear programming for people with little mathematical training, although the verbal style and slow pace will probably make those with more mathematical background a bit impatient. Exercises at the end of each chapter, an index, and a bibliography add to the value of the book.

R. M. VAN SLYKE
University of California, Berkeley

Books Received

CLAUDE ABRAHAM AND ANDRÉ THOMAS, *Microéconomie: Décisions Optimales dans l'Entreprise et dans la Nation*, Dunod, Paris, France, 1966, 480 pages, 68 francs.

HAROLD T. AMRINE, JOHN A. RITCHEY, AND OLIVER S. HULLEY, *Manufacturing Organization and Management, Second Edition*, Prentice-Hall, Englewood Cliffs, N.J., 1966, 584 pages, $13.25.

EDMUND C. BERKELEY AND DANIEL G. BOBROW, editors, *The Programming Language LISP: Its Operations and Applications*, The M.I.T. Press, Cambridge, Mass., 1964, 392 pages, $5.00 (paper).

ROBERT T. BROWN, *Transport and the Economic Integration of South America*, The Brookings Institution, 1775 Massachusetts Avenue, N.W., Washington, D.C., 1966, 302 pages, $6.00.

ELWOOD S. BUFFA, editor, *Readings in Production and Operations Management*, John Wiley & Sons, New York, N.Y., 1966, 620 pages, $8.95, $4.95 in paper. An anthology of 34 papers aimed at supplementing the editor's two earlier volumes, *Modern Production Management, Second Edition*, 1965, and *Models for Production and Operations Management*, 1963, both published by Wiley. Eight of the papers were first published in the *Journal of Industrial Engineering*, seven in *Management Science* or *Management Technology*, six in the *Harvard Business Review*, four in this journal, seven elsewhere, and two appear here for the first time.

PHIL CARROLL, *Practical Production and Inventory Control*, McGraw-Hill Book Co., 1966, 352 pages, $9.50.

RENÉ CAVÉ, *Le Controle Statistique des Fabrications, Third Edition*, Éditions Eyrolles, 61 Boulevard Saint-Germain, Paris V^e, France, 1966, 543 pages, 75 francs.

GEORGE K. CHACKO, *Today's Information for Tomorrow's Products: An Operations Research Approach*, Thompson Book Co., 383 National Press Building, Washington, D.C. 20004, 1966, 239 pages, $11.00.