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Book Reviews

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BOOK REVIEWS

FRISCH, RAGNAR. *Planning for India: selected explorations in methodology.* Bombay and Calcutta: Asia Publishing House and Statistical Publishing Society, 1960, $6.00, 86 pp., double spread flow chart.

SANDEE, JAN. *A demonstration planning model for India.* Bombay and Calcutta: Asia Publishing House and Statistical Publishing Society, 1960, $4.75, 60 pp.

During his stay (1954–55) at the Indian Statistical Institute, Dr. Frisch, a distinguished scholar from the Institute of Economics of Oslo University, wrote a number of memoranda on national planning, some of which are incorporated in this scholarly work under review, which aims to present the basic methods of planning from a technical and statistical standpoint. While the broad generalizations are very readable and sound in all respects, the heavy mathematical intrusions may deter the reader from following the work clearly. In fact, this is virtually the first publication that clearly exposes the myth of orthodox economists that the banking, money, and money-cost approach forms the only environment for planning. They fail to realize that behind these symbols there is a world of realities which persists no matter what sort of economic institutions man devises. Planning in terms of these real entities is the main aim of this text.

Step by step, Dr. Frisch leads the reader into the scientific problems in methodology. These, stated in a nutshell, are: statement of desired ends, specifying the list of activities, and determining the optimum combination of targets. But a mere listing of these items serves no purpose; thus in the list of activities and their associated developments the author very rightly points out the *modus operandi*. For each specific development project the idea is not merely to estimate how large the effective demand for that product would be and then work out the detailed capacity-plans accordingly; this is not real planning but only a specific sort of guessing, with a subsequent computation of the details corresponding to the overall guess adopted. The list should contain not only the mere labor saving and capital intensive projects like steel and aluminium plants, but also a number of labor intensive devices and activities, such as road building, slum clearing, minor irrigation, and handloom weaving. Only by including a sufficiently large spectrum of such activities will it be possible at a later stage to find optimal solutions that really satisfy the desired conditions on employment in a transition period.

Scientific planning, as advocated in Dr. Frisch's work, thus lies in coordinating everything in one simultaneous and integrated piece of analysis and doing it on some optimum basis. It is like solving the whole nexus as one simultaneous problem where everything determines everything, much in the same way as all the unknowns in a system of equations determine each other simultaneously. These aspects are discussed in mathematical detail, including the "logarithmic potential method of linear programming," which has been successfully used on medium-sized problems. For bigger problems on electronic computers the

author relies on his multiplex method, a flow chart for this method being given at the end of the text.

The applications of Frisch's versatile mathematical methods are then discussed, and it is in here that the planners of underdeveloped countries like India should find rich and meaty material. Thus solutions to problems can be had from the computational analysis. One such discussed is whether the country should have one steel plant built entirely by Indian personnel and finance and without foreign loans, or two steel plants of the same capacity but with foreign loans. Another vital aspect treated is the way in which the population and economy may develop in the course of time, partly under the impact of objective natural forces, and partly as the result of planned activity. That Frisch's methodology has not been used in the overall planning in India is clear between the lines. While the country's First and Second Five-Year Plans have achieved a fair measure of success, it may be questioned whether it would not have been possible to achieve still more or to achieve the same results with less human suffering and lower financial burden if the individual parts of the Plan had been geared into one another in a better way. The ideal state would be a detailed scientific analysis for evaluating the best possible combination which could be realistically conceivable at the time of formulation of the Plan.

While Dr. Frisch has no doubt succeeded in evaluating this ideal mathematically, the imponderable variables may baffle the practical worker into arriving at a suitable combination of targets. It is here that the ultimate analysis may go off at a tangent and yield different results. However, within the framework set by the author, this study is a valuable record of the methodology of planning in its widest perspective, and it would be worthwhile to utilize it for medium-sized projects for all its worth.

The Sandee report presents a model designed to demonstrate the possibilities of planning models in India. In view of the fact that too much realism in the model might interfere with detached judgment, the author has restricted his study to treatment of long-term problems instead of the more pressing questions of the day. Essentially, the model demonstrated is based on an assumed economic situation in 1960 and analysis of the Indian economy in 1970. The year 1960 formed the last year of India's Second Five-Year Plan and as such revealed the general trends in the growth throughout the Plan period. Although a fully professional report exists on the demonstration model, Sandee gives a much fuller account of the construction of the model, including an elementary introduction to input-output and linear programming theory.

Input-Output practice comprises two separate activities; the construction of the input-output table, and the use of the input-output model. The first relates to the economy of a country in a certain specified year and gives the value of flow of goods and services between productive sectors. It may give other things as well, but the inter-industry flows are essential. Examples of input-output tables are given where all productive sectors of the economy are listed as rows and again as columns. The rows show how the output of the sectors are distributed over the productive sectors (where they serve as "input") and over

the other destinations, such as consumption, investments, or exports. On the basis of this type of table, the classical input-output model is drawn up. The basic assumption of this model is that the inputs are proportional to outputs, which can be expressed in matrix notation as $x = Ax + y$, where x is the vector of total output, A is a matrix of input-output coefficient, and y is the vector of the totals of consumption, exports, and investment. This is an open static model. On the basis of a Leontief model it could be closed, partly or entirely, by relating the components of the vector y, that is, the "final bill of goods," to the production levels by other relations than the input-output relations of the open model. Consumer demand by commodities can be related to the income level which itself is a function of the output. Further, the model becomes dynamic when it is closed by the linking of the investment part of the final bill of goods to output. In the present study, Sandee lucidly explains these operations and discusses the various production sectors on which the model is set up.

These are agriculture; large-scale food manufacturing; steel, electric power, mining, fertilizer and heavy engineering industry; transport; construction; and housing. All these are individually explained and commented upon, together with a discussion of "capital-output ratios." The amount of fixed capital needed to produce a unit of a certain commodity per annum is called the "capital-output ratio" of the production in question. The derivation of these ratios is difficult. The ideal source, but one not yet available in India, would be a well-digested set of projects and realization reports of both new plant and extension of existing facilities. Without this source, difficulties arise in evaluating these ratios, for for one is forced to take into account observations of existing assets and the output obtained from them.

What then is the net result? Sandee summarizes the final facts which emerge from such studies with their inherent limitations. In the present model the level of material consumption in 1970 is used as the only criterion. Due to the peculiar structure of the model, this simple target is wider than it seems. As investment and welfare go together, maximizing the one means maximizing the other. Further, incorporation of employment in the target function would have probably led to a different plan, since some material welfare would have to be sacrificed to ensure more employment. Equality has not found a place in the model either. In fact, the overall "perspective" consideration starts from the experience that investment requires more imports than consumption does, and investment might therefore be limited by balance-of-payments difficulties rather than by saving. To prevent this, some of the possible growth in the first ten years might be sacrificed to enlarge import substitution possibilities in later periods. This, of course, requires serious quantitative examination.

Based against this background and the limitations of data and availability of information, the Sandee study offers a critical and penetrating analysis of the Indian economy as envisaged in 1970, an analysis which though striking in its assertion, may be questioned by many.

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PECK, MERTON J. AND SCHERER, FREDERIC M. *The weapons acquisition process: an economic analysis*. Boston: Graduate School of Business Administration, Harvard University, 1962, $10.00, 736 pp.

Next to the hearings of Congressional investigating committees, this book by Peck and Scherer of the Harvard Graduate School of Business Administration is one of the most comprehensive books ever written on the defense production industry. They have done a monumental and thorough job of trying to define the characteristics of this industry from a number of points of view. This first book is termed "An Economic Analysis," and there are other volumes pending from the project. The size of the project can be indicated by the number of pages—this first volume, over 700, and the remark that there are over 1700 additional unpublished pages of textual material in their files that they did not include in the book.

The authors give a great deal of proper attention and stress to the uncertainty that beclouds the weapons acquisition process. This applies to the ultimate value of the weapon as it is affected by the changing military environment and the activities of the opposing sides, the possibility of attaining the technological goals required to develop the weapon, the feasibility of the time schedule, and the realization of the cost estimates. In each of these areas, the authors present case study material to show by how much the expectations and actual experience differed. The core of their case study work was twelve advanced aircraft and missile weapons, including most of those that are important in today's military force.

They also give ample attention to the peculiar nature of the market structure governing the defense industry. Thus, the essential monopsonistic nature of the market structure is stressed, as well as the central role played by the buyer (the government) in determining and governing the product produced. They also provide a lot of useful and unique data and statistical analyses of other significant market characteristics, such as the geographic mix of defense companies and plants, the sizes of defense companies, the supply of technical and scientific personnel, the entry and exit experience in the defense industries and the influence of profits, economies of scale and risk taking on the entry and exit behavior of companies from the defense industry.

They devote a considerable part of the book (about 375 pages) to the Execution of Weapon Programs (Section III), and it is here that the primary interest of management scientists would be found. The authors put great stress on the tradeoff between time, resources, and weapon performance. To discuss this tradeoff, the authors make use of the classical economic device of indifference curves, and they then indicate how the marginal rates of substitution of these three factors can be equated. They recognize that the use of a static analysis, such as indifference curves, leaves out a great many important considerations, such as the types of uncertainty the authors stressed as complicating the weapons development and acquisition process, and ignores the highly-dynamic character of the military environment, with its intelligence uncertainty, technological breakthrough by one or the other country, etc. They presumably justify this

highly over-simplified approach on the grounds that they are using this static and graphical technique as a pedagogical device. However, this is where one feels that the authors are in a sense talking down to their audience. Granted the audience is not characteristically an economics class, and granted that the authors have shown a great deal of sophisticated understanding of the complexities of the process, it still does not follow that it is appropriate to bring in such a simple view of the world at a point in the book where it would appear that prescriptions or more helpful approaches to the problem should be offered to the reader.

The authors do introduce some aspects of operations research and management science, but primarily in terms of the criterion question, and they use Hitch's views on optimization and Simon's concept of "satisfiscing" to indicate the trials and tribulations of finding or even hoping to find optimal solutions to highly complex problems. They also bring in the work of B. Klein on development strategies (i.e., to undertake parallel development of highly uncertain subsystems, and to concentrate on subsystem development as a hedge against the uncertainties of future system requirements). They imply a criticism of the Klein work on the grounds that it is non-operational, but this is clearly a criticism one can bring against the applicability of an indifference curve approach to the development process.[1]

Granting that the book under review is not intended for a highly technical audience, it would still have been useful both to the authors and the audience (which is probably a large consumer of operations research and management science capability) to have tried to bring in much more of the current state of the art in dealing with uncertainty, dynamic relationships, bargaining, and management control. It is not clear from the description of the project's publication plans that this will be done in any other of the books, and this book does seem to be a reasonably appropriate place for it, in view of the very description of the institutional setting presented.

Without trying to be exhaustive, the work on dynamic programming of Bellman, Fulkerson and Ford, Johnson, etc., come to mind in their efforts on production scheduling under uncertainty. The work of Simon, *et al.*, at Carnegie Tech, and the book of Dorfman, Samuelson, and Solow on linear programming are also relevant on production planning, inventory control, and the work force. Then, there are the studies in gaming, and the applications of game theory by Baumol, Shubik, and Feeney to the market mechanism. There are also a great many papers in *Management Science*, *Management Technology*, and *Operations Research*, among other journals, that deserve review and possible mention.

My point is that the authors build up the audience's expectations, and what they present in the way of normative prescriptions are feeble indeed. In order to do this, the authors impute a lower order of sophistication and capability

[1] The undersigned presented a similar oversimplified view of the weapons-logistics tradeoffs in 1957, and he is required from time to time to apologize for its naivete and non-operational limitations. But this was over five years ago, and the field has advanced a great deal in that time.

to the present system than perhaps they intend. From my experience and knowledge of the process under study, it is characterized by a large number of people trying to make a very difficult system work. There is obviously a good deal of heuristic activity involved in the current system, but for much of the process, this may be the current state of the management art. However, the system is marked by a great deal of checks and balances through organizational arrangements, information flow, and the use of conferences and meetings as a means of trading off and interacting possibly conflicting effects. This is not to say that the industry could not benefit very much from a greater infusion of management science, but I believe that the presentation of indifference curves as an entry to the "New World" is not sufficiently representative.

This reliance on a highly oversimplified model forces the authors into other weaknesses. For example, in computing the value of the alternative weapon systems for the indifference analysis, the authors are required to use the notion of "probability of war," and this makes the probability purists cringe. They are also required to develop value scales for time, quality, and cost, and although this seems to be an interesting idea, and the scaling technique is quite sophisticated, the effort still begs the question of dynamic change and uncertainty, both on the demand and the supply side.

What I have said up to this point constitutes my main criticism of the book, but there are a few other points worth mentioning for the record. First, because of the highly aggregative nature of the model used, the authors completely ignore the weapons-logistics tradeoffs, and these deserve some mention in a book devoted to efficient use of resources and to increasing the weapons capability per dollar of system cost. Without belaboring the point, there is a good deal of available logistics research which indicates the significant leverage of this tradeoff, and also the extent to which logistics policies can help in hedging against the uncertainties of resource commitment.

Then, in Chapter 6, the authors carry through a detailed statistical analysis of the relationships between the variance in development cost and variance in development time factors for the twelve major weapon systems studied, and various indices characterizing the difficulty and innovational characters of these programs. Aside from the difficulties of scaling such characteristics that have already been mentioned, I find fault with the statistical techniques used. The authors use standard linear regression, and make probability statements about the statistical significance of the regression and correlation coefficients. There are a number of grounds for disagreeing with the appropriateness of the use of this form of regression with these data. First, it is unlikely that the separate weapon systems can be considered statistically independent, in view of the interactions among these weapon systems. Second, there are "errors" in both the dependent and independent variables, which is inconsistent with the assumption of standard linear regression theory; and third, there is no indication that the errors in the dependent variable are distributed normally. All of these conditions affect the estimate of regression coefficients, the resulting correlation coefficients, and the usability of the statistical tests of significance. Although the authors

mention the limitation of lack of randomness in one of the footnotes, it is not clear what they really could conclude from this analysis in view of the even more extensive inapplicability of the regression technique to the data. Although I may be too critical, I believe that the use of simple statistical techniques is no substitute for a more operational and rational behavioral model, and it is here that the authors fall short. Perhaps their first attempts with statistical analysis will lead the way for others to try to model this process more dynamically.

Finally, all through the book the authors make reference to poor management such as "overmanning" (on p. 494) and "excessive cost of weapons programs" (on p. 594). These are serious charges, and the evidence presented is not particularly precise or convincing. To really substantiate such judgments or conclusions, the authors would have to produce the kind of normative model we would all like to have, and this they do not provide. Thus, in the face of uncertainty, competitive challenges, etc., it is reasonable to expect that there will be contingency pools of manpower, and that some of the best brains of the company would be continually withdrawn from work on current production problems, and assigned to work on new development projects.

In summary, the book contains a most impressive discussion and description of the institutional aspect of the weapons acquisition process, and as such, it will be very useful and instructive. Its efforts to indicate ways of improving the current state of affairs leave something to be desired. It is to be hoped that this important book will stimulate and facilitate some of the latter type of effort.

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ARGYRIS, CHRIS. *Interpersonal competence and organizational effectiveness.* Homewood, Illinois: Richard D. Irwin, Inc., 1962, $6.50, 292 pp.

This book belongs in the tradition of studies in which an analysis is made of a company and the results are fed back to the company for the purpose of improvement. It also is part of the always insightful, often penetrating observations of organizational behavior that we have come to expect of Argyris. In many ways it is a splendid study of some of the things that happen in the process of management. It would have made a fine long article or short monograph. It is a very bad book. It is pretentiously conceived, carelessly written, and even more carelessly edited. Since there is such a gap between the form of the presentation and the substance of the research, let us treat them separately.

A company president saw the possibility of improving sensitivity to feelings in the whole managerial group. Argyris, by interview, put together a description of the manner of managing, emphasizing rational, intellective, directive processes (and values). He feels that this is less desirable than a system that allows one to have negative feelings and, what is more, express them. His fairly detailed diagnosis was reported back to the executive group and the members were encouraged to re-think and re-discover the principles for themselves. Subse-

quently, what is here called a laboratory—a series of Bethel-type T-group sessions—was held to further the sensitization, re-discovery, and progress. At the end of this procedure most of the managers talked about management very differently from the way they did at the beginning. There is some reason to believe they also behaved differently. This finding is rarer and more noteworthy than the simple sentence suggests. In addition, a chapter by Roger Harrison reports a somewhat separate study showing a change in patterns of the perceptions of others as a result of the T-group sessions.

It is difficult to illustrate the things that are of value in Argyris' description of managers' thinking. Rest assured that they are there and look for them yourself. They are hard to pull out because they are not in the generalizations and conclusions, but in the penetration and selective reporting of the fabric of reality. It is really an artist's job. Argyris has an ear for dialogue like John O'Hara and a feeling for the pull between man and system like Marquand. His insights are in the tradition of the descriptions of what's going on of Whyte and McGregor. We actually understand the uniqueness of the human situation when we read it.

So much for the substance. There is much good there. The form, unfortunately, is another matter. Its deficiencies are too important to leave unremarked. Three responsibilities have been evaded—those of the author, the editors of the series, and the publisher's editorial assistants.

The book is cast in the jargon and frame of scientific method which fit it not at all. The book is full of suggestions that, for example, (p. 7) "One objective of this study was to explore the diagnostic validity of the model." This is heady stuff. It sounds as scientific as all get-out. Hypotheses are printed in bold face and italics just like the big boys do, though they are not rigorously derived or tested (or, in many cases, easily testable). There is talk of an experimental and control group and the classic before-and-after measures, although the before-measures are given to the experimental group as treatment and the after-measures ask the same things. The factitiousness of seeming scientific without being so is so marked that both the author and the (professional) series editors bear a responsibility.

For example, a series of hypotheses are presented. The following is representative of the treatment (p. 81 ff):

Hypothesis III

Since the executive system sanctions the inhibition of feelings, and since it requires dependence and develops fear of the superior's feelings and reactions,

Then,

A. if the subordinates develop negative feelings toward presentations that attempt to influence them

B. they will not tend to express them.

What evidence is relevant? Clearly it is an "if A, then B" case. We need evidence that (a) negative feelings have developed and (b) they tend not to be expressed.

Under a heading "Evidence for Hypothesis III" three cases (numbered)

are cited. One says a man was sent to make a presentation dealing with job-lots. Those who heard him said they thought his presentation was excellent. The man's superior said the "real reaction" was different (no source given for the implicit evaluation of the accuracy of this statement). Does this show negative feelings have developed? or been inhibited? It says positive feelings were reported. The superior said the feelings were negative.

Case two concerns demoting supervisors as part of a reduction of force. A dialogue shows some oppositon to the program and ends by the leader saying it will be difficult to carry it out but he asks the subordinates please to keep it in mind. Both A and Not-B seem to be present. No independent evidence is offered of negative feelings that are not expressed. No account is taken of the negative feeling expressed (by hypothesis, some of them may be expressed, since they will only "tend" to be inhibited).

Case three tells of a luncheon in what is described as an atmosphere of crisis. The division head entered looking upset and preoccupied. His appearance heightened the tension. In a later interview, the division head saw this fact. He described the things he felt as he entered the room—his worries and the like. Asked if it would not have helped the others at the luncheon to tell them what he was worrying about, he said "Oh, no . . . ." Does this show the existence of "negative feelings towards presentations that attempt to influence them?" I think not. None were reported expressed, but in the absence of A, B is not compelling.

This is shoddy. I gloss over whether three selected bits of behavior are a proper test of such an hypothesis no matter what the facts. I gloss over whether hypotheses that say "If A, B will tend to happen" are precise enough to be testable. I gloss over the derivation of the hypothesis from more general principles. But I cannot gloss over packaging as analytic experimental method what is naturalistic observation. It is good observation, in the tradition of Darwin and Fabre and Lubbock, but it is not empirical testing in the tradition of Percy Bridgman and Clark Hull, although it is made to sound so. What are called hypotheses are descriptive summaries; what is called evidence is illustration. Repackaging is misleading.

One special phenomenon occurs when the social sciences other than economics come into the field of business. They bring with them a vocabulary, a set of traditional defense reactions, and an apparently new approach. Psychology, particularly, has been defensive about scientific method. For a variety of reasons which need not be detailed here, psychology has developed a considerable concentration on the logic of testing hypotheses, and terms like the hypothetico-deductive method, operationism, intervening variables, and hypothetical constructs are rife. Probably useful to psychology, this vocabulary has a peril when the social scientist moves to another field. The vocabulary above may carry an aura of logical tightness, empirical substantiation, and close reasoning that is not necessarily true. This book is an example of the phenomenon.

There is still a matter of detailed style. Often this is idiosyncratic and allowable. Cumulatively it becomes important. The economics of publishing now are such that there is great pressure to dash off a manuscript. Even so, the author

should re-read the manuscript, and perhaps, also someone should besides the typesetter at the publisher's. For example:

1. There are a host of vague referents. On some I had to call in a colleague and my secretary to help me find them. One dangling "it" I have never found. Argyris speaks (p. 84) of a luncheon which the division head was tempted to cancel because of an atmosphere of crisis. He goes on to say "The crisis climate remained until the end of the luncheon when the division head called it off." Called what off? The luncheon? Retroactively? Or the "crisis climate"? If so this is the most remarkable finding in the book. Surely someone read this sentence between the typist and the typesetter. Whoever did should have wondered where the "it" was attached.

2. The book is full of italicized words and words enclosed in quotation marks. The following examples are reproduced exactly from the book:

> . . . the shortage of time and a hurried agenda "centralizes" the meeting . . . (P. 81)
>
> The hope was that . . . they would begin to "unfreeze" and desire to change. (P. 7)
>
> . . . the *effective* subordinate was harmed by centralization. (P. 110)
>
> The plant managers *heard* that they ought to accept more responsibility . . . (P. 79)

There are hundreds of such cases. I presume italics and quotes mean that the word has a meaning somewhat different from the accepted one. Presumably "centralize" is different from centralize, else why the typographical distinction? I think I catch the fine ironic difference between "*heard*" and "heard," but how much more effective an *effective* subordinate is than an effective subordinate, I do not know. Years ago my daughter used to write us that, "I've had a real *fun* time. I met a real 'cute' boy." I worked hard to cure *her* of this schoolgirlish prose (last italics mine).

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BUFFA, ELWOOD S. *Modern production management.* New York: John Wiley & Sons, Inc., 1961, 636 pp.

This book is intended as a text for a basic course in production management. The general field of production management is at the moment in a unique position. In the past decades a great deal of research has been undertaken by management and social scientists on problems of production. The results of this research have lead to a much greater understanding of, and ability to deal with, many of the difficult problems in the field. Therefore the author of any new book in production management is faced with the problem of determining the proper content for this field. It has been suggested that production can be viewed from a historical-descriptive, quantitative, technical, or human viewpoint. Life being short, and books being long, it usually is not possible to deal with all these considerations in any one volume. Unfortunately this book lacks a central con-

cept, or set of concepts, of what the student should learn about production, but rather has attempted to present a comprehensive discussion of all approaches.

The book is divided into four sections: introduction, analytical methods in production management, design of production systems, and operation and control of production systems. This division is both logical and convenient as a conceptual framework on which to base such a work. The author is to be congratulated on this general outline which permits the inclusion of materials which would not conveniently fit into other frameworks.

In the preface to this volume Buffa deals with the ever present problem of teaching production management to students who lack adequate mathematical training. He states, "In preparing this book, I have adopted a general outline which I believe makes possible the inclusion of much of the new material in a way that is comprehensible to the average, diligent, undergraduate student, without placing heavy emphasis on mathematical skill." This objective leads the author to introduce such topics as decision models, inventory models, linear programming, assembly line balancing, and other such analytical techniques. This is correct, for a book on production should include these and other related topics.

Unfortunately Buffa was faced with the problem of how to deal with analytical models without placing heavy emphasis on mathematical skill. He resolved this dilemma by typically giving examples of the use of the model, or technique, or by presenting a formula. In the first treatment the reader is taken through the solution of a problem but is given no general formulation of the method. When presenting formulas the volume does not generally provide a derivation or discussion of the underlying assumptions of the model. As a result the analytical models are dealt with in an inadequate and superficial manner.

The balance in the book can also be questioned. The author has devoted approximately 100 pages to the subject of production processes and job design while he finds that the entire field of inventory and production control can be dealt with in a short chapter of 33 pages plus some material in an earlier chapter on mathematical and simulation models.

The chapter on inventory and production control illustrates still another deficiency in this text. A number of inventory models are presented, but the important problem of determining policy under the conditions of uncertainty (of both lead time and demand during lead time) is dealt with in summary fashion, no model being developed or even presented. The section of this chapter dealing with production control is, to a large extent, a summary of the work of Magee without benefit, again, of any generalized models. As such it is quite confusing and, perhaps more important, misleading.

The author has done very fine work in some sections of this text. His chapters on job design are quite good. He has presented the linear programming models (both the simplex and transportation) in a manner that can rapidly be grasped by students. The chapters on maintenance and quality control are also good.

It is suggested that the author has suffered by trying to make this book all

things to all people. It attempts to encompass the engineering, human, mathematical, and traditional concepts of production management. We hope that in the author's next book he will restrict his efforts to a more unified concept and he will undoubtedly have a fine volume.

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LLOYD, DAVID K. AND LIPOW, MYRON. *Reliability: management methods, and mathematics*. Englewood Cliffs, New Jersey: Prentice-Hall, Inc., 1962, $11.25, 528 pp.

This is a very good book from my point of view. The statistical aspects of reliability interest me, and here the authors present a wealth of material on the subject. I particularly liked the detail used to carry out the various mathematical derivations. As its title implies, the study covers a wide variety of topics, starting with the Management, Organization, and Communication of Reliability, which takes a matter of fifty pages. The bulk of the text deals essentially with reliability in its statistical and mathematical aspects.

To digress a moment, there was obviously not a great deal to say about the first set of topics, since there is no science or even very good rationale for this kind of activity. But it is useful to have one point of view presented and discussed. This is not to minimize or delimit the importance of this area. Large sums of money and large decisions hinge on reliability issues. The mathematical purity of a Weibull distribution or a Greco-Latin design must be mixed and interacted with the complexity of demonstrating reliabilities of 0.99999, or the definitional problems of "success" and "failure" or that "all test objectives were achieved."

Undoubtedly there is much reliability theory. This book amply demonstrates its existence and sophistication. However, the question remains: was the theory developed to show off the power of mathematical and statistical techniques, or was it developed with due regard for the difficulties of collecting valid reliability data and for the recognition of the differences between laboratory, factory, repair facility and field conditions? The reliability issue entails much more than launching a missile or space vehicle under the special and unusual conditions of a development test program. Other significant factors involve maintaining these vehicles in field conditions on-alert, keeping them available for long periods of time, and developing policies about the frequency of checkouts, replacement, and maintenance. There is also the importance of relating the reliability or test specifications used in production to the specifications determined for the field. There have already been instances where electronic systems have passed inspection in the factory or repair facility, only to be rejected in large frequencies when delivered to the field.

I have said enough about the complexity and great importance of this area. The Lloyd-Lipow book represents a welcome addition to the reliability literature. It has captured much of the available mathematical and statistical published

material as evidenced by the topics treated and the ample bibliographies presented at the end of each chapter. It discusses all the currently popular statistical failure models, including the exponential, Weibull and negative binomial distributions, and presents methods for estimating their parameters mostly from the point of view of large sample techniques, such as maximum likelihood. The importance of dealing with small sample methods in this work is also obvious.

The book contains a good discussion of reliability structure models but it would have been desirable to have supplemented the comparatively simple analytic discussion of this topic with the possibilities of simulation and Monte Carlo techniques. These methods are being used increasingly to treat the very complicated circuit and system design requirements called for in the very complex electronics of the space and missile age.

The treatment of experimental design and sampling inspection is quite thorough and sophisticated. It was most pleasing to see the extensive discussion devoted to the response surface techniques developed by Box and Hunter. Such multivariate methods, although really in their infancy in terms of model complexity, should be exploited to the utmost; they deserve a prominent place in reliability and similar applications requiring much experimentation with large numbers of determining variables.

The book concludes with three examples of reliability and evaluation programs. All of these are in the engineering development phases and the authors more or less walk us through some of the statistical treatment used. There is no discussion about how significant these statistical considerations were in the reliability results produced, the satisfaction of the development contract conditions, and the subsequent experience in operational field conditions. I am sure this part of the reliability study would make even more fascinating reading.

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BATCHELOR, JAMES H. *Operations research, an annotated bibliography*. St. Louis, Missouri: St. Louis University Press, Vol. I (through 1957), 1959, $10.00, x+866 pp., Vol. II (1958–1959), 1962, $12.50., xi+628 pp., Vol. III (1960), 1963.

These two volumes contain a listing of 6723 articles on operations research published through 1959 by authors from Ablahat to Zwinggi. This reviewer naturally turned first to the page on which Machol was alphabetized and was pleased to discover on the same page a charming abstract of an article by Niccolo Machiavelli, published in Rome in 1531, entitled, "What Dangers Are Run by One Who Takes the Lead in Advising Some Course of Action." I do not know whether this is the earliest article on operations research.

The book is organized alphabetically in each of the volumes by senior author. The index does not list senior authors, but does list junior authors, a device which has cleverly saved some money in preparation and publication, but which requires one to look in four places (two in each volume) to find the works of a

given man. This is typical of the book, which has been prepared for publication in the least expensive fashion by typing and photo-offset. The proof-reading leaves a little to be desired, but this is not serious in a work of this type. The price per page is unusually low.

The indices occupy over 160 columns (approximately 12,000 items) in each of the two volumes, list subjects as well as junior authors, and seem complete as regards the indexing of items included in the titles of articles (although Flood's article on the travelling salesman problem is not included) and also fairly complete on key words included in the abstracts. The system of underlining in the index is, however, unexplained. The list of abbreviations in this book, including all abbreviations used for journal titles, totals exactly twelve. This and other techniques make the book particularly easy to read. Furthermore, the straightforward organization gives this work the special virtue that no special instructions are required for its use.

Many of the titles cited in this bibliography were only abstracts in the original and these, together with some other articles, are given by title only. For approximately half of the titles cited "abstracts have been prepared to indicate the nature of the article and the views and ideas of the authors or reviewers as they presented them and are really quite full digests". While one might carp with individual abstracts, on the whole the abstracting has been excellent. "The criterion for inclusion has been that the author or editor has indicated that the work concerns operations research, or that it has been cited in support of such work . . . some exceptions have seemed desirable." Again, the editor seems to have chosen well.

It seems pertinent to compare this with the two other principal bibliographies on operations research. (There have been a number of others, including ASTIA document #AD269750, with several hundred citations, and a recent profit-making venture entitled *Literature Digest Service in Operations Research/Management Science*.)

The first of these two principal bibliographies, resulting in the volume *A Comprehensive Bibliography on Operations Research*, was done by a Case Institute of Technology group. This volume, one of the Publications in Operations Research series of the Operations Research Society of America, covered the literature through 1956. A supplementary volume covering literature through 1958 is being prepared for publication, but editorial effort has ceased. These volumes are designed to be of special utility for compiling bibliographies on special topics. They do not contain any abstracts or summaries, but merely the author, the title, and the citation, and in addition, a ten-digit number coding each article in a number of ways. While such a bibliography is clearly not for light reading, IBM cards are available for each entry, and by re-sorting them the operations analyst can compile his own specialized bibliography in any way. For a further comparison of this work with the Batchelor volumes, see *Operations Research*, *8*, 283–6 (1960).

The second consists of the periodical *International Abstracts in Operations Research*, published by the Operations Research Society of America for the

International Federation of Operations Research Societies. It covers literature from the beginning of 1961 and is a continuing effort. This periodical, with its quick response time, makes it possible for the OR worker to have quick coverage of the current literature. In addition, it is keyed by various subjects, making it easy to find papers in one's special field of interest without going through all of the abstracts. This periodical has been published for only a short time and its eventual utility is hard to judge. As of this writing,[1] 348 citations to the 1961 literature have appeared in its first volume, which is to be compared to more than 2500 Batchelor citations to the 1958–59 literature. Presumably, it will be supplied with cumulative indices from time to time. For a further discussion of these abstracts, see the ORSA *Bulletin, Operations Research*, Vol. 9, Supplement 1, pp. B-48 to B-54 (1961).

These three bibliographic efforts supplement one another nicely. In particular, the Batchelor books and the *International Abstracts* will jointly provide continuous coverage when Batchelor's Vol. III (1960 literature) is published in 1963. The serious operations research worker will want all three on his shelves. This reviewer predicts that he will refer most frequently to the Batchelor volumes.

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[1] Since writing the above, Volume 1 of *IAOR* has been completed and contains 697 citations. I am indebted to the Editor of *IAOR* for the following additional data, which may shed light on the apparent discrepancy in the extent of literature coverage between the two publications: Of a random sample of 318 of the 2528 new Batchelor citations, 27% had pre-1958 dates; 11% were citations of individual chapters of books having individual chapter authors; 16% were papers presented at meetings with no further indication of publication; 2% were announcements; less than 5% were research reports issued by individual organizations. When the 38% which is dated 1958 is adjusted using these percentages so as to compare on a common basis with *IAOR*, the result is an inferred 687 citations for 1958 with a standard error of 21. While of the 697 citations in Volume 1 of *IAOR*, 228 would be dated by Batchelor as pre-1961, only 30 of these were items publicly available before 1961. Volume 2 of *IAOR* will contain more than 150 additional items which would be comparably dated as 1961.

BOOKS RECEIVED

(Inclusion of books in this list does not preclude their subsequent review.)

AKHIEZER, N. I. *The calculus of variations*. New York: Blaisdel Publishing Co., 1962, 247 pp.

ALLEN, R. G. D. *Basic mathematics*. New York: St. Martin's Press, 1962, $10.00, 512 pp.

BROWN, JAMES. *Cases in business communications*. Belmont, California: Wadsworth Publishing Co., Inc., 1962, 278 pp.

BURNS, TOM AND STALKER, G. M. *The management of innovation*. Chicago: Quadrangle Books, Inc., 1962, $7.00, 269 pp.

CASH, HAROLD C. AND CRISSY, W. J. W. *A point of view for salesmen*, Vol. 1, 1962, 54 pp.; *The use of appeals in selling*, Vol. 2, 1961, 52 pp.; *Motivation in selling*, Vol. 3, 1962, 60 pp.; *Personality and sales strategy*, Vol. 4, 1962, 70 pp.; *Tactics for conducting the sales call*, Vol. 5, 1962, 71 pp.; *The sales man as a self-manager*, Vol. 6, 1962, 79 pp.; (The Psychology of Selling Series.) Flushing, New York: Personnel Development Associates.

COLLIER, ABRAM T. *Management, men and values*. New York: Harper & Row, Publishers, 1962, $5.00, 235 pp.

COLMAN, HARRY L. AND SMALLWOOD, CLARENCE. *Computer language*. New York: McGraw-Hill Book Company, Inc., 1962, $3.95 (textbook ed.), $5.95 (trade ed.).

DANCE, FRANK E. X. *The citizen speaks*. Belmont, California: Wadsworth Publishing Co., 1962, $4.65 (trade list price), $3.50 (text list price), 224 pp.

DAUTEN, PAUL M., JR., (ed.). *Current issues and emerging concepts in management*. Boston: Houghton Mifflin Co., 1962, $2.75 (clothbound $4.75), 333 pp.

FERRIS, ROBERT G. AND FERRIS, WILLIAM H., JR. *Profits and profit evaluation sources*. Washington, D. C.: Econo Trends Publishing Company, 1962, $4.75, 101 pp.

FORD, L. R., JR. AND FULKERSON, D. R. *Flows in networks*. Princeton, N. J.: Princeton University Press, 1962. $6.00, 194 pp.

HALACY, D. S., JR. *Computers, the machines we think with*. New York: Harper & Row, Publishers, 1962, $4.95, 279 pp.

HIMSTREET, WILLIAM C. AND BATY, WAYNE M. *Business communications*. Belmont, California: Wadsworth Publishing Co., 1961, 419 pp.

LEVENSTEIN, AARON. *Why people work*. New York: The Crowell-Collier Press, 1962, $3.95.

MEANS, GARDINER C. *The corporate revolution in America*. New York: The Crowell-Collier Press, 1962, $3.95.

MILLS, EDWIN S. *Price, output and inventory policy*. New York: John Wiley & Sons, Inc., 1962, $7.95, 271 pp.

NADLER, GERALD. *Work design*. Homewood, Illinois: Richard D. Irwin, Inc., 1963, $12.00, 837 pp.

PETERSEN, ELMORE, PLOWMAN, E. GROSVENOR AND TRICKETT, JOSEPH M. *Business organization and management.* Homewood, Illinois: Richard D. Irwin, Inc., 1962, $8.65, 341 pp.

SIMONNARD, M. *Programmation lineaire.* Paris: Dunod, 1962, (68 NF), 420 pp.

SPRAGUE, RICHARD E. *Electronic business systems.* New York: The Ronald Press Company, 1962, $8.00, 168 pp.

STRAUSS, ANSELM L. AND RAINWATER, LEE. *The professional scientist.* Chicago: Aldine Publishing Company, 1962, $6.00, 282 pp.

WAGNER, HARVEY M. *Statistical management of inventory systems,* (Publications in Operations Research No. 6). New York: John Wiley & Sons, Inc., 1962, $8.95, 235 pp.