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SUGGESTIONS CONCERNING THE PREPARATION OF MANUSCRIPTS FOR THE QUARTERLY OF APPLIED MATHEMATICS

The editors will appreciate the authors' cooperation in taking note of the following directions for the preparation of manuscripts. These directions have been drawn up with a view toward eliminating unnecessary correspondence, avoiding the return of papers for changes, and reducing the charges made for "author's corrections."

Manuscripts: Papers should be submitted in original typewriting on one side only of white paper sheets and be double or triple spaced with wide margins. Marginal instructions to the printer should be written in pencil to distinguish them clearly from the body of the text.

The papers should be submitted in final form. Only typographical errors may be corrected in proofs; composition charges for all major deviations from the manuscript will be passed on to the author.

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Mathematical Work: As far as possible, formulas should be typewritten; Greek letters and other symbols not available on the typewriter should be carefully inserted in ink. Manuscripts containing pencilled material other than marginal instructions to the printer will not be accepted.

The difference between capital and lower-case letters should be clearly shown; care should be taken to avoid confusion between zero (0) and the letter O, between the numeral one (1), the letter l and the prime ($'$), between alpha and a, kappa and k, mu and u, nu and v, eta and n.

The level of subscripts, exponents, subscripts to subscripts and exponents in exponents should be clearly indicated.

Dots, bars, and other markings to be set *above* letters should be strictly avoided because they require costly hand-composition; in their stead markings (such as primes or indices) which *follow* the letter should be used.

Square roots should be written with the exponent $\frac{1}{2}$ rather than with the sign $\surd$.

Complicated exponents and subscripts should be avoided. Any complicated expression that recurs frequently should be represented by a special symbol.

For exponentials with lengthy or complicated exponents the symbol exp should be used, particularly if such exponentials appear in the body of the text. Thus,

$$\exp\,[(a^2 + b^2)^{1/2}] \text{ is preferable to } e^{(a^2 + b^2)^{1/2}}$$

Fractions in the body of the text and fractions occurring in the numerators or denominators of fractions should be written with the solidus. Thus,

$$\frac{\cos\,(\pi x/2b)}{\cos\,(\pi a/2b)} \text{ is preferable to } \frac{\cos\dfrac{\pi x}{2b}}{\cos\dfrac{\pi a}{2b}}$$

In many instances the use of negative exponents permits saving of space. Thus,

$$\int u^{-1} \sin u\, du \text{ is preferable to } \int \frac{\sin u}{u}\, du.$$

Whereas the intended grouping of symbols in handwritten formulas can be made clear by slight variations in spacing, this procedure is not acceptable in printed formulas. To avoid misunderstanding, the order of symbols should therefore be carefully considered. Thus,

$$(a + bx)\cos t \text{ is preferable to } \cos t(a + bx).$$

In handwritten formulas the size of parentheses, brackets and braces can vary more widely than in print. Particular attention should therefore be paid to the proper use of parentheses, brackets and braces. Thus,

$$\{[a + (b + cx)^n]\cos ky\}^2 \text{ is preferable to } ((a + (b + cx)^n)\cos ky)^2.$$

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Bibliography: References should be grouped together in a Bibliography at the end of the manuscript. References to the Bibliography should be made by numerals between square brackets.

The following examples show the desired arrangements: (*for books*—S. Timoshenko, *Strength of materials*, vol. 2, Macmillan and Co., London, 1931, p. 237; *for periodicals*—Lord Rayleigh, *On the flow of viscous liquids, especially in three dimensions*, Phil. Mag. (5) 36, 354–372(1893). Note that the number of the series is not separated by commas from the name of the periodical or the number of the volume.

Authors' initials should precede their names rather than follow it.

In quoted titles of books or papers, capital letters should be used only where the language requires this. Thus, *On the flow of viscous fluids* is preferable to *On the Flow of Viscous Fluids*, but the corresponding German title would have to be rendered as *Über die Strömung zäher Flüssigkeiten*.

Titles of books or papers should be quoted in the original language (with an English translation added in parentheses, if this seems desirable), but only English abbreviations should be used for bibliographical details like ed., vol., no., chap., p.

Footnotes: As far as possible, footnotes should be avoided. Footnotes containing mathematical formulas are not acceptable.

Abbreviations: Much space can be saved by the use of standard abbreviations like Eq., Eqs., Fig., Sec., Art., etc. These should be used, however, only if they are followed by a reference number. Thus, "Eq (25)" is acceptable, but not "the preceding Eq." Moreover, if any one of these terms occurs as the first word of a sentence, it should be spelled out.

Special abbreviations should be avoided. Thus "boundary conditions" should always be spelled out and not be abbreviated as "b.c.," even if this special abbreviation is defined somewhere in the text.

CONTENTS

BOOK REVIEW SECTION

Einführung in die Unternehmensforschung, Vols. I and II. By R. Henn and H. P. Künzi. Springer-Verlag, Berlin, Heidelberg, New York. 1968. viii + 154 pp.; viii + 201 pp. $2.70; $3.20.

Professors Henn and Künzi, well-known to German-speaking operations researchers, have written a popular introduction to operations research in paperback format. Volume I surveys the principal mathematical tools of a general nature, including set theory, linear algebra, probability theory, statistics, and simulation techniques. The treatment of probability is very brief and stops short of stochastic processes. The reader encounters neither the Poisson distribution nor least-squares regression. The chapter on simulation introduces random numbers and discusses some elementary but significant applications (newsboy, random walk, replacement). Volume II brings more specific techniques in short expositions of linear programming (with emphasis on the simplex method), mathematical programming and dynamic programming, the barest elements of two-person zero-sum game theory, and finally graph theory. The authors are at their best when discussing their special fields: nonlinear programming and graph theory. The subject matter of operations research is presented in chapters on inventory control, replacement, queuing, and critical paths.

The books never deviate from a conventional course. Their main shortcoming is that they attempt too much in too limited a space. They whet one's appetite without satisfying it. However, the writing is lucid and occasionally fascinating. These texts should be welcomed by students of economics, business, and social science who look for an introduction to operations research which is uncompromising as far as it goes, which presents the basic ideas, and which does not get too deeply involved in mathematical technique.

MARTIN J. BECKMANN (*Providence, R. I.*)

Mathematische Hilfsmittel des Ingenieurs, Vol. III. Edited by R. Sauer and I. Szabó. Springer-Verlag, Berlin, Heidelberg, New York, 1968. xix + 535 pp. $24.50.

This is the second to be published of the four volumes which will form the complete work. Much more than the earlier Volume I, it addresses itself to the very special brand of engineering scientists who depend on very sophisticated and rather new fields of mathematics. Volume I, published in 1967, was reviewed previously in this journal.

The present volume contains: algebra, by F. L. Bauer and J. Stoer; geometry and tensor calculus, by R. Sauer and T. P. Angelitch; interpolation and approximate integration, by R. Burlisch and H. Rutishauser; approximation of functions: part I, basic theory, by G. Aumann, and part II, representation of functions in digital computers, by R. Burlisch and J. Stoer; linear and nonlinear optimization, by H. P. Künzi. In addition there is an appendix on digital computing systems by K. Samelson.

The chapter on algebra consists essentially of set and matrix theory. The authors state that in this handbook proofs had to be omitted, and refer the reader to a number of standard texts. The reviewer shares their feeling that some of the presentation is so compact that without knowledge or extensive study elsewhere little help can be derived, particularly from the part on set theory.

The chapter on geometry, which precedes the tensor calculus, shows the well-known clarity of R. Sauer's writings; many figures support concepts. Affine and projective geometry, nomography, spherical trigonometry, vector algebra and calculus are treated, as is differential geometry, which is so important in the study of many engineering problems in which flow or stress fields in regions with very general boundaries are considered. The chapter on tensors starts in great generality, in a space of n dimensions without a metric, and gradually narrows the subject down to three-dimensional, Euclidean space. Applications to mechanics of continua and electromagnetic fields are touched upon.

In the following chapters the reader sees the extension of old familiar formulae and methods. Interpolation and integration are presented with the assumption that the user will have an electronic computer.

The spline interpolation seems particularly interesting. In some cases even the ALGOL program is indicated. Although computer languages still are changing, many readers may appreciate these samples, since transition from one computer language to another is a relatively easy operation. The chapter on approximation of functions will be welcome to many who know that in order to avoid storage problems in computer operations, one often needs good analytic representations of functions. The consideration of functions with singularites will be useful, as will the presentation of functions by continued fractions.

One wonders why the chapter on linear and nonlinear optimization was made part of this volume; however, it may have been hard to find a better place in the plan of the handbook. The simplex method for linear programming is explained in detail, with examples. The difficulties of the numerical treatment of the nonlinear optimization are outlined.

This reviewer expects that this third volume will be a handy tool on the desk of many engineers.

I. FLÜGGE-LOTZ (*Stanford, Calif.*)

Current algebras and their applications. By B. Renner. Pergamon Press, New York, 1968. xiv + 177 pp. $9.00.

It has often been observed that the information explosion is becoming worse, not better. The field of elementary particle physics suffers perhaps more than any other in physics in this respect: nearly all research areas and fads are immortalized as the subjects of innumerable review articles, monographs, lectures, summer school proceedings, topical conferences, international conferences, etc. The topic of current algebras, one of the newest and most exciting areas in the field of high energy physics, is seemingly no exception, for the book by Renner is one of three major reviews of current algebras published in 1968, and more are predicted.

What makes current algebras so exciting? The answer is that they provide a means for investigating the properties of strongly interacting particles through their weak or electromagnetic interactions, the latter being expressed in terms of "currents", analogous to the electromagnetic currents familiar from electrodynamics. These currents are postulated to obey certain dynamical rules, one result of which is that the set of "charges" derived from the currents are closed under the operation of commutation; hence the name current algebras. Extremely useful relations between weak or electro-magnetic properties and strong interaction properties can be derived from expressions (sum rules) obtained by evaluating the matrix elements of the "charge" commutators taken between specified physical states. Many of these relations are in good agreement with experiment.

Since few theoretically derived results on strongly interacting particles had been available, the excitement generated by current algebras is understandable. Given the publication explosion and need for information dissemination, it is not surprising that monographs and reviews on current algebra have begun to appear. Unfortunately, Renner's book is the only one of three very recent general reviews with which I am familiar and which I am unable to recommend for one's personal library. The other two, *Current algebras* by Adler and Dashen (W. A. Benjamin, New York, 1968) and the article "Current algebra" by Bjorken and Nauenberg in *Annual Review of Nuclear Science*, Vol. 18 (Annual Reviews, Inc., Palo Alto, 1968), are each in their own ways better reviews of the subject and the ones I can recommend. Renner's book seems to be useful only for practicing elementary particle theorists, while the other two are admirably suited to a more general audience. In particular, the review of Bjorken and Nauenberg provides an excellent introduction for non-specialists who wish to learn the main ideas, results and physical content of current algebras, while the monograph of Adler and Dashen is a good introductory text on the subject, enhanced by selected reprints of important papers in the field.

All three reviews cover the same main ideas. These include a historical introduction, definitions and properties of currents, brief discussion of SU(3) symmetry, low energy theorems, sum rules, partially conserved axial currents (PCAC), gradient or Schwinger terms, and discussions of applications to a variety of elementary particle phenomena. In short, each review covers the field, although in very different ways.

At first glance, *Current algebras and their applications* seems like an extraordinarily well annotated set of references, with the annotations (in this case the main text) preceding the apparent main text, here the collection of references to the literature up to 1967. Since the book is an outgrowth of a Rutherford Laboratory Report, RHEL/R 126, which was based on a series of lectures given in 1966 at the

Rutherford Laboratory, and which included 219 references, the present format of total inclusion of all relevant material is understandable but disappointing. The book fails as an introduction to the subject for the non-specialist or beginning graduate student in the field because it is insufficiently selective: it tries to cite all ideas and references yet succeeds only in presenting too large and minutely detailed a picture. A consequence of this for me was that the book became for the most part dull reading, an unpardonable sin when one considers how exciting the field of current algebras has been. Neither of the other two reviews suffered from this fault. A further point is that Renner himself seems uncertain as to the makeup of his intended audience. For example, unimodular, covariant equations and the notation $K(l3)$ are defined, even though any second-year physics graduate student should know these terms, yet he fails to define Poincaré group and spurion, and hardly elaborates the notion of an interpolating field, concepts which are generally in vogue only in the current research areas of high energy physics. By contrast, both of the other reviews are highly selective, and maintain a sense of excitment about the results of current algebras because of this. As noted above, they provide excellent introductions to the main ideas, results and physical principles involved. The style and choice of pedagogic material of these other authors is to be commended.

Apart from these caveats relating to style, readability and feeling of excitment, the book as a review is adequate. The entire field up to publication time is surveyed and many applications are included. For the person already familiar with the subject of current algebras, not only is relevant literature cited for nearly every application and theoretical development, but articulate comments are to be found as well, e.g. on p. 95 following eq. (6.42) where the status of non-leptonic weak decays is succinctly discussed. Nevertheless, it is not a book I could recommend to the general reader and in particular those non-high-energy-theorists who normally peruse this journal. As an archive of references and a guide to the literature, it belongs in university or institutional libraries, but compared to the two other reviews mentioned above, particularly when it comes to price, it would have been better to keep it as an expanded RHEL Report, available in the preprint or report library of research groups for its fine bibliography.

F. S. Levin (*Providence, R. I.*)

Topics in regression analysis. By Arthur S. Goldberger. The Macmillan Company, New York, 1968. x + 139 pp. $6.95.

This book presents an unconventional approach to regression analysis and least squares. The results presented are standard ones found in most books on correlation and regression, but the heuristic methods of deducing them are novel. One difficulty in assessing this book comes from deciding to what audience it is addressed. For, although the preface states that readers would be expected to have some familiarity with multiple regression, statistical inference and matrix algebra, the derivations make little use of this information.

The introductory chapter gives a brief summary of the contents of the book. This is followed by a discussion of relationships and regression, in which conditional expectations and uncorrelated variates are defined. The linear regression of y on x is estimated from the condition that the deviation from estimated regression function be uncorrelated with x. Although the author asserts that this bypasses the method of least squares, and that "the present method may be viewed as less arbitrary, since it has some inferential content", it should be pointed out that the choice of x as the variable with which the deviations are to be uncorrelated is just as arbitrary; there is nothing in the author's formulation to preclude the choice of x^2, $\log x$ or any other monotonic function of x. In fact, his choice leads to the familiar least-squares estimates, so that least-squares methods are introduced implicitly.

Chapter 3, regression coefficients, is a rather lengthy discussion of partial and gross (i.e. simple) regression coefficients. This may be of interest to economists concerned with the biases that may be introduced into regression equations if some relevant variables are omitted. Much of the discussion covers what must be old ground for anyone familiar with regression theory. Similarly, Chapter 4, coefficients of determination, provides nothing new, some space being devoted to showing that if additional regressor variables are introduced, R^2 is necessarily increased.

Chapters 5, 6 and 7 consider in a leisurely fashion the determination of the standard error of regression coefficients. For some students it may be of value to learn in one chapter that the standard errors depend on elements of the inverse of the product matrix of the regressor variates, in the next that they

depend also on the residual sum of squares of the y-variate, and finally that they depend on the degrees of freedom associated with this sum of squares. For many, however, the prospect of such a lengthy discourse will be daunting.

Chapter 8, functional forms, considers various types of nonlinear regression. It describes the use of dummy variables for the "cell-mean approach" (analysis of variance model), and the use of transformations of the regressand for such nonlinear models as the Cobb–Douglas function. There is a sensible discussion of the precautions to be taken when data are transformed.

Chapter 9, choice of functional form, discusses the various criteria for choice of a particular regression function. It points out the limitations of formal criteria such as significance tests and the multiple correlation coefficient, and indicates the value of theoretical subject-matter criteria.

The final two chapters of the book are the most valuable and readable, because they are written at the level of the intended readership. The rest of the book suffers from the defect that the author has striven for simplicity by avoiding mathematics, but has actually made his and his readers' task harder. The lesson to be drawn from this is that mathematics, like electricity, makes life easier.

E. J. Williams (*Parkville, Australia*)

Dynamic programming of economic decisions. By Martin J. Beckmann. Springer–Verlag, New York, Heidelberg, Berlin, 1968. 143 pp. $7.00.

This attractive little book is intended as an introduction to dynamic programming for a reader who wants a general survey of the field. The emphasis is on basic principles rather than on the detailed techniques of dynamic programming. The author's approach is mathematical and the treatment is rigorous but not pedantic. Much of the theory is motivated through simple and concrete examples which should be helpful in bringing out its relevance to economic decision making in sequential situations.

The first part of the book deals with finite problems and introduces the reader to the principle of optimality, value and policy iteration as well as to stability questions. These topics reappear later on in more sophisticated versions. Risk and uncertainty are discussed in a lucid manner and stochastic decision processes are exemplified by applications to machine care and inventory problems. Adaptive programming is discussed briefly.

In the later part of the book the author turns to continuous problems in which either the decision variables, or time, or both are continuous. This, of course, requires a somewhat different mathematical approach, but the author succeeds in keeping the analysis comparatively simple without sacrificing the rigor of the argument. The reader will find a surprising amount of information in the less than 150 pages of the book.

The most attractive feature of the book is the didactic manner in which the basic principles of dynamic programming are exhibited and shown to be natural and simple. The many examples given in the text help to do this; one could have wished, though, that some large-scale dynamic programming problem of real economic significance had been included and dealt with numerically.

This book can be strongly recommended as an introduction to the mathematical theory of dynamic programming.

Ulf Grenander (*Providence, R. I.*)

Computational solution of nonlinear operator equations. By Louis B. Rall. John Wiley & Sons, New York, London, Sydney, Toronto, 1969. viii + 225 pp. $14.95.

This is a brief introduction to the two topics in numerical functional analysis which are most frequently used: the contraction mapping principle and Newton's method. They are presented here in a more or less self-contained setting which requires of the reader little more than a background of the usual facts from undergraduate mathematics. The title is presumably motivated and justified by the inclusion of a computer program for Newton's method.

Chapter I on linear spaces, operators and equations contains the most elementary results from linear functional analysis which are needed in the sequel. Chapter II deals with the contraction mapping

principle for operators in a Banach space. In Chapter III on differentiation of operators the Frechet derivative and higher derivatives are discussed briefly. Chapter IV presents Newton's method and its applications, following closely the treatment by Kantorovich. A short appendix by R. E. Moore gives an elementary introduction to the Lebesgue integral. Each section closes with a set of exercises, and each chapter ends with notes and a list of references.

In the reviewer's opinion the selection and presentation of the material is somewhat dated, which is unfortunate because of the current wide interest in the subject.

H. A. ANTOSIEWICZ (*Los Angeles*)

The theory of price uncertainty, production and profit. By Clement Allan Tisdell. Princeton University Press, Princeton, N. J., 1968. ix + 197 pp. $6.50.

Virtually all economic models of the firm assume that all variables are non-stochastic in nature and are known to the firm when it makes its operating decisions. This book outlines a way to approach a model of a competitive firm when the price received is a random variable. The randomness of the price is the only departure from the usual competitive firm model which is assumed here.

The first question considered is the criterion used by the firm for picking its optimum output. If price is a random variable, so are the profits earned by the firm. The usual deterministic rule of maximizing profits has no clear meaning in this situation. A good discussion is given of the available criteria based both on ordinal and cardinal measures of success. The cardinal group consists of maximizing certain moments or combinations of moments of the probability distribution of profits. The simplest criterion is to maximize the expected value of profits, but risk-taking or desire to avoid risk-taking may also be important to firms so that higher moments are also considered in the decision-making criterion. The ordinal group consists of the maximum, maximax, other criteria following from the strategic notions of game theory. These, too, reflect the varying attitudes which the firm might take toward risk situations. While the technical aspects of these criteria are well covered, one could wish for more adequate discussion of the economic implications of adopting a specific criterion.

Tisdell's contribution in this area is to show how some of the ordinal criteria can be converted to straightforward maximization problems. He assumes that the production possibility set is strictly convex, and uses the properties of convex sets to derive the firm's decision process as a linear programming problem. The shadow prices in the imputed linear profit function uniquely reflect the type of decision criterion chosen. Further, there is a one-to-one correspondence between each set of shadow prices and the vector of outputs for the firm. Thus the whole vexing problem of criterion choice can be put aside and attention given to the central problem of the firm's behavior when faced with random prices.

There are two effects of price randomness which must be introduced into the firm's decision process: uncertainty and instability. Uncertainty measures the inability of the firm to predict future prices accurately. Instability refers to the size of the price variance. The main thrust of the analysis is to determine the effects of uncertainty and instability on the profits of a firm. Further work is done on the relationship between price uncertainty and instability and the flexibility of production techniques chosen by the firm. A flexible technique is defined as one with a relatively flat cost curve, i.e., production costs will not rise very much when output is quite different from the minimum-cost output. Inflexible techniques may have lower costs at the optimum output level, but much higher costs at output levels lower or higher than the minimum-cost one.

The general method Tisdell uses is to form an expected profit function where two arguments are price variance (instability) and the mean square difference between actual prices and the predicted prices (uncertainty). The predicted prices are the ones used as shadow prices in the optimum output calculations. He has developed a more complete formulation of the problem than the more narrow models used by previous workers.

The main results of the analysis are that profits are generally directly related to price instability (though the relationship may be reversed under some circumstances) and inversely related to the amount of price uncertainty. Inflexible techniques are likely to be chosen with a high degree of price uncertainty. While the effect of price instability is generally to induce the use of more flexible techniques, this is not certain. All of these results follow from the assumptions of convexity in the profit function with respect to price and concavity in the profit function with respect to output.

The later chapters suggest ways to aggregate the results for firms into implications for industries and for the economy as a whole. One simple dynamic element is introduced (that output can be changed after plans have been formulated, but only at a cost), but a fully dynamic model in the spirit of recent control theory or calculus of variation firm models has not been developed. One intriguing policy implication is discussed. Uncertain prices may reduce the aggregate output of the economy. This implies that the government might usefully support some type of institutional scheme, such as universal provision for hedging operations, so that the apparent price uncertainty facing firms is reduced. While these extensions and implications are suggestive, they are not worked out in any detail. One is left with the feeling that Tisdell's approach may be useful, but much theoretical work needs to be done. Empirical work in this area has hardly begun, despite the fact that even the simple models worked out by Tisdell and others already contain testable empirical implications.

A major flaw in the analytical sections is the unfair and confusing treatment given to two other workers who have considered this problem, Professors Baumol and Oi. Tisdell arrives at opposite conclusions from Baumol and Oi regarding the effect of price instability upon choice of techniques and profits respectively. He implies that there must be errors in the analyses of Baumol and Oi. Even a casual reading of the Baumol and Oi works cited by Tisdell shows that the differences in results come not from differences in analysis but from differences in fundamental assumptions about a firm's behavior. Tisdell assumes throughout the sections in question that a firm decides upon its output for a particular period before it knows the price for that period. It cannot change the output later when the actual price becomes known. In addition, price prediction is not perfect. The result is that output in any one period is only by accident going to be the optimal output, given the price which actually occurs. On the other hand, Baumol and Oi assume that output is optimal each period, either because instantaneous costless output adjustment is possible or price prediction is perfect. With these assumptions, Oi is correct when he argues that higher price instability always implies higher profits and Baumol is correct that higher price instability will likely induce more flexible techniques. These are not the results which follow from Tisdell's rather different assumptions.

Tisdell should have explained the effects of changing the assumption about adjusting output, both in fairness to Baumol and Oi and to clarify the crucial nature of the assumption for his own argument. More generally, one is left with the somewhat uncomfortable feeling that Tisdell has not really thought long enough or deeply enough about the implications of the assumptions or methods used in the model. The models and analyses he uses seem technically correct and do yield interesting answers. However, little is known about the sensitivity of the results to particular economic or mathematical assumptions. It is not even clear how his model relates to other similar formulations by other workers or what economic interpretations should be given to the differences in models. Lastly, the connection between these models and the important questions for which firm models have recently been used is virtually neglected. Some of these questions include analyses of market structure, pricing policies, investment policies, research and development, the internal decision-making process of a firm, attitudes towards risk, and entry barriers raised by such things as scale economies and advertising. Uncertainty is an inherent part of much of market phenomena; it will be treated adaquately only when theories incorporating uncertainty are broadened to incorporate more of the economic forces at work.

Despite the rather narrow view taken both in his own models and in considering the works of others, Tisdell's work is a useful step towards bringing uncertainty into firm and market models in a meaningful way. However, it is just one step along the way.

MARK B. SCHUPACK (*Providence, R. I.*)

Strategy for R & D. By Thomas Marschak, Thomas K. Glennan, Jr., and Robert Summers. A RAND Corporation Research Study. Springer–Verlag, New York, Inc., 1967. xiii + 330 pp. $14.20.

Research and development projects inherently contain a great deal of uncertainty. The essence of such projects is that they are attempting to make processes or goods which have never been made before. It is not certain at the beginning of the project which method of approach is best or what the cost will be in terms of time and money. This uncertainty must be dealt with in some rational way if

we are ever to devise analytical tools to help in the planning and accomplishment of research and development projects.

This book is concerned with ways of describing and characterizing the development process. The ultimate goal is to build a realistic framework within which we can analyze ways of making decisions about development projects when faced with great uncertainty. The book is essentially a collection of four essays, two by Marschak and one by each of the other authors. They all are about the general topic of development strategy, but differ widely in scope and method. All of the data used is from military development projects. Aside from natural use of such data by people associated with the RAND Corporation, these data do represent some of the single largest development projects for which data are available.

Briefly, Glennan attempts to provide a description of the development process. His description is based on wide observations of military development projects. Marschak gives a number of detailed case studies of development projects. He attempts to generalize from the case observations. Summers observes the discrepancy between actual and estimated costs for a number of military projects. He explains the discrepancy using a single-equation regression model. Finally, Marschak, in the last third of the book, develops a normative theory of development. Much of the analysis is couched in a type of decision-theory framework. We shall give more details about these essays.

Marschak's theory is the most important contribution in the book. His object is to provide a way of looking at the development process which will allow the formulation of rules for determining optimum development decisions. He severely limits the scope of his theory by concentrating upon the optimum way to develop one item. He gives little or no attention to the questions of selecting a group of projects within a given R & D budget or the optimum size of the R & D budget when production, marketing, and investment activities compete for funds. However, even the theory of single-item development is too complex for anything approaching complete solution.

The theory assumes that there are many approaches possible, any one of which could lead to the development of the item or process in question. The cost in time and money of each approach is known only with uncertainty. Each cost is a stochastic variable whose expected value is assumed known. The object of the decision making is to pick the approach or approaches which will minimize some combination of time and cost. One or more review points are provided at which time the decision maker picks the combination of approaches which he will carry forward to the next review point. At the last review point, he will choose the one approach which will be used to complete the project. There is a cost involved in carrying an approach from one review point to the next. This cost for bringing forward an additional approach must be balanced against the possibility that the additional approach will turn out to have a true cost lower than all other approaches.

What decision rules can be formulated to decide how many approaches to consider at first, and how to narrow or change the set considered at each review point? It turns out that the answer depends crucially upon the probability distribution of the costs for each approach. To make any analytical progress at all, the cost probability distribution must be rather restricted. For example, assuming a kind of unbiasedness in the stage-to-stage estimates and that the Markov property holds for successive estimates will insure that estimates of the true final costs will get better (smaller variance) as we move from one review point to the next.

More drastic restrictions are necessary if even the simplest rules of thumb are to be valid. Some rules which would be convenient to validate include: 1) If the cost of carrying an additional approach to another review point is low enough, you should use more approaches at first, not less. This will yield more information, and thus a higher probability of finding a very low-cost approach. 2) At each of the several review points, pick the subset of approaches to carry forward which has the lowest expected values of final cost. 3) Avoid heavy early commitments to any approach for which the degree of uncertainty is high.

The restrictions to the probability distribution necessary to insure that these simple rules are valid for optimality are very complicated, and in some cases unknown. Complications increase manyfold as we get closer to reality by allowing for sequentially picking up previously discarded approaches, allowing each approach to be used with varying intensity, allowing for multi-component items where trade-offs of quality among the components are possible, and allowing for multi-valued preference functions instead of the simple two-valued one where an item is either satisfactory or unsatisfactory. Although the framework is here, the development of solutions has not been carried very far.

The use of the model as described is certainly an advance over what has been done previously.

One is still left with the uncomfortable feeling, however, that it ultimately may prove inadequate for the task. For example, Marschak notes that it is most difficult to translate the notion that wasteful duplication should be avoided into a policy prescription using this framework. This problem may be symptomatic of a general difficulty with the whole approach. We may not be able to characterize the decision process in an adequate manner by referring only to the probability distribution of the cost estimates for each approach. There may have to be more technological factors in the characterization. One of the major restrictions proposed for the probability distributions is that the several approaches be technically non-complementary. It is not clear that this is reasonable at all; parallel approaches carried on by the same firm on the same item may be highly interrelated technically. These technical properties may have to be an integral part of any useful theory.

The major use of a model like Marschak's is not only to suggest decision rules but to specify precisely when they are valid. In the process of setting up this framework we are forced to specify what types of knowledge we need to make analytical and empirical progress. Despite the restriction to non-complementary approaches and the analytical difficulties involved, a thoughtful start has been made in building up a normative theory. There are, of course, other possible approaches in the sense that the word approaches is used here.

Unfortunately, the rest of the book makes little use of this or any other framework. Glennan tries only to describe the development process as he has observed it. He claims that his work is nothing more than taxonomic, trying to characterize the process and pinpoint important variables. This method of treating the problem leads him to make many statements which appear to be important conclusions, but which, upon close examination, are very uncertain and imprecise in meaning and certainly are unsubstantiated either from considering some theoretical framework or from firm empirical data. For example: "The size of the error cost will be affected by the timing of the various tasks and by the interrelatedness of the components of the system being developed. But the critical question is the degree of uncertainty inherent in the project." Or this: "In some situations there is no alternative to making decisions in the face of major uncertainty. If such decisions must be made then the probability of mistakes may be substantial." None of these statements seem refutable in any sense.

In the introductory chapter of the book, Marschak describes Glennan's article as frustrating. It is. We do need to know about the real-world operations of the process we are trying to model. However, taxonomy is most useful when it is done with particular analytical frameworks in mind. There seems to be none in this case. One wonders why this article was included in a book where the major emphasis is rightfully given to the analytical problems involved.

Similar comments can be made about the detailed case studies presented by Marschak. He concludes, along with Glennan, that many possible development patterns can be observed. He also admits, which apparently did not concern Glennan, that the particular framework he chose for organizing the data may not have been the most relevant or revealing one. One question he tries to ask is whether it is better to make heavy commitments to a particular approach early in the life of the project or at a later time. He finds no conclusive evidence either way in his data. In his theoretical chapter Marschak does discuss some of the conditions which might determine whether early or late commitment of funds is best. None of the data collection seemed aimed at the variables which the theory suggests are important. Why was there no coordination between the theoretical and empirical chapters in the book? Hints in the book suggest that the empirical work was all done before the theoretical chapter was started. Thus the individual essays in the book preserve their historical integrity, but the blending and coordination of empirical and theoretical work which we should expect by the time research is published in book form is almost completely absent.

Although it is much less ambitious in its aim, Summers' article shows the most satisfactory wedding of empirical and simple theoretical work. He computes a single-equation regression model using the ratio of actual to estimated costs as the dependent variable and the following independent variables: timing of the estimate within the program life, the degree of technical advance desired, the length of the development program, and the calendar year. He uses rather *ad hoc* theorizing to explain the reason for including each variable in the equation and the sign of the regression coefficient which would be expected for each one. Data from case histories generally confirm this simple econometric model.

Even Summers' simple theory contradicts in one respect the picture generally given by Glennan and Marschak, both in the theoretical and empirical sections. The latter two assume that parallel approaches are used until the less desirable ones are weeded out and the one best approach remains. Summers envisions a process where the developer sequentially tries a number of approaches starting

with the approach which has the smallest cost estimate. Again, co-ordination among chapters seems absent.

Summers has a very good discussion of the welfare and policy problems involved when cost estimates are in great error.

In conclusion, the book is concerned with important issues of theory and policy. Many intriguing questions are raised and left unanswered. It is more diffuse and disjointed than it should have been, but is still provocative and should stimulate further work in the area. Good theoretical work about the R & D process has been extremely scarce in the past.

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Ship waves and wave resistance. By R. N. Bhattacharya. Jadavpur University, Calcutta, 1967. xxv + 156 pp. £2.

This monograph is not an attempt to give an exposition of the present state of the subject of the title but rather, for the most part, a gathering together of various of the author's researches on the subject. He assumes from the outset the linearized free-surface boundary conditions, without comment upon the relationship between this assumption and the configuration of the ship itself. Although some of the classical results concerning steadily moving pressure points and distributions, submerged sources, and thin ships are derived, the author's interest is chiefly directed toward accelerated motion, both rectilinear and along curved paths. Perhaps the strongest criticism the reviewer has concerning the author's derivations is the treatment of conditions at infinity. All mention of "radiation conditions" as either physically reasonable or mathematically necessary is suppressed in favor of a "fictitious viscosity," even in the case of accelerated motion. He thus misses the opportunity to show that the correct radiation condition is automatically satisfied by a steady motion obtained as the limit of a motion started from rest, and additionally misinterprets the rate of approach to the steady state because of the presence of the fictitious-viscosity term. There are also occasional incorrect statements. For example, on p. xvii: ". . . Michell's method has not the merit of Kelvin's in explaining the peculiar wave pattern due to the motion of ships. . .". In fact, Michell's method can explain much more than Kelvin's. There are also dubious formulas, such as one on p. 55 giving a time-dependent expression for the wave resistance of a pressure distribution moving with uniform angular velocity on a circular path. A final criticism is one over which the author has no control: It is obvious that he has had only limited access to the more recent literature on the subject in, for example, the *Journal of Ship Research*, *Schiffstechnik*, and also other journals and symposia.

The chapter headings give a good overview of the content.

I. Waves produced by a pressure system moving with an acceleration over the surface of deep water.
II. Wave resistance in deep water due to the accelerated motion of the pressure system.
III. Wave resistance in deep water due to arbitrary motion of a pressure area along any curved path on the undisturbed free surface.
IV. Wave resistance in deep water due to the arbitrary motion of any pressure area along any curved path on the undisturbed free surface.
V. Growth and decay of ship waves.
VI. Further analysis of ship waves.
VII. Wave resistance of a ship moving in a circular path.
VIII. Wave resistance of a symmetrical ship moving in a circular path in deep water.
IX. Shallow water effects on wave resistance of a ship moving in a circular path.
X. Wave resistance from the rate of dissipation of energy; some cases of uniformly moving point disturbances.
XI. Wave resistance and other forces and moments from the theory of attraction between sources and doublets.

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The calculus of observations: an introduction to numerical analysis. By Sir Edmund Whittaker and G. Robinson. Dover Publications, Inc., New York, 1967. xiv + 397 pp. $2.75.

"This Dover edition, first published in 1967, is an unabridged and unaltered republication of the fourth edition (1944) of the work originally published by Blackie & Son, Ltd., London, in 1924 under the title *The calculus of observations: a treatise on numerical mathematics.*" We are indebted to Dover Publications for making available, in an attractive bright blue cover, this numerical analysis classic. Still worth dipping into from time to time.

PHILIP J. DAVIS (*Providence, R. I.*)

Programming languages: history and fundamentals. By Jean E. Sammet. Prentice–Hall, Inc., Englewood Cliffs, N. J., 1969. xxx + 785 pp. $18.00 (students' edition $13.50).

This reviewer recently had the pleasure to sit on a panel with Miss Sammet at a computer conference. She criticized two of the papers in the session, each for presenting "just another language". She went on to question the need for computer languages which aren't really new or different and which adopt a syntactic structure apparently based on no particular predecessor. Her thought was that a computer language which is directed toward a special family of application areas can, in many cases, be designed as a dialect of some existing language.

When one examines her book, the motivation behind her comments is clear. There are indeed many languages; perhaps too many.

Miss Sammet has catalogued, abstracted and commented on what are the most important of these; some 120 in number. A book such as this will inevitably contain mistakes, but they are not so numerous as to reduce its value. The style is occasionally quite stilted, sometimes giving the impression that a section was written from a checklist covering each required topic in turn, rather than being a discourse about a language.

These minor points notwithstanding, this book is a tour de force, an unparalleled collection of history, concepts and opinions compiled by a highly respected professional. The references themselves are worth the price of the book, but the value of this work also lies in the vast wealth of information gathered in a single reference volume, some of it appearing publicly for the first time.

This reviewer is of the opinion that there will never again be an attempt to conceive a language containing "everything" as was done in PL/I. It may be that the same is true for a book of the scope of this one.

ROBERT F. ROSIN (*Buffalo, N. Y.*)