The Analyst's Bookshelf

Hugh J. Miser, Editor

Reviews

DAVID BRAYBROOKE and CHARLES E. LINDBLOM, *A Strategy of Decision: Policy Evaluation as a Social Process*, The Free Press of Glencoe (a Division of the Macmillan Co.), New York, N. Y., 1963, 280 pages, $5.95

IN operations research we all recognize that in addition to developing strategies for our clients, we have the internal problem of developing strategies for operations-research teams. Thus we have to be very careful about the questions we ask, the problems we work on, and the way in which we attempt to implement the results. No one that I know of has tried to formalize the OR 'strategy space' or to suggest calculable methods of optimizing in this space, but there is a respectable literature on the subject.

Perhaps the most common strategy we adopt consists of partitioning our activities into two parts. On the one hand, the research team feels the responsibility of attaining as comprehensive an understanding of the client's world as is possible. In order to accomplish this first aim, it tries to construct a model that will approximately satisfy the criteria of scientific rigor and accuracy. On the other hand, the research team tries to establish a satisfactory social relation with the client, in order to maximize its opportunities of obtaining information and eventually of exerting influence.

This partitioning itself may not be a wise way to view the matter, and apparently BRAYBROOKE AND LINDBLOM think not. They start with another kind of partitioning of the world of the researcher into four parts, representing the intersection of "high vs. low understanding" and "incremental vs. large change." They then try to show that the so-called responsibility of attaining a comprehensive understanding of the client's problem is appropriate at best in the "quadrant" of high understanding and low change. Perhaps they feel that this is where most of OR belongs, though they never mention OR as such. When one wants to accomplish a small but significant change in the area of "low understanding," i.e., high degree of complexity, he must give up the comprehensive objective and resort instead to "disjointed incrementalism." This is the strategy of restricting oneself to policies that differ only incrementally from the status quo. In other words, every 'status quo' has a close neighborhood of alternative states, and the analyst should restrict himself to this neighborhood, never getting too far from home. One is reminded of current 'search procedures' in OR mathematics, though the authors might feel very uncomfortable with the analogy. They would feel uncomfortable largely because they never view their strategy of change in the context of

a strategy space, and hence can at best resort to constructing convenient strawmen whenever the issues become difficult.

The rigorous proponent of the comprehensive viewpoint will have no difficulty at all in questioning the authors' advice to use disjointed incrementalism. It's the same as the old confusion about SIMON's 'satisficing'. To use Simon's overquoted example of finding the best needle in the haystack, it is surely common sense to stop with a satisficing, and even a satisfactory needle, rather than try to examine them all. But this piece of common sense is based on a comprehensive view of the haystack, the cost of search, and the types of needles (if one needle is an ancient possession of Cleo's, I might never be satificed until I found it). So too, the authors' close neighborhood concept only makes sense in terms of a comprehensive account of the strategy space and the cost of search. If all they tell us is that we must limit our effort in terms of our resources to conduct research, they tell us—I hope—nothing we did not know before.

However, these more formal criticisms are trivial compared to a much deeper concern that this book generates. It is a very serious matter to change anything, almost as serious as not changing it. Furthermore, all change is adopted in the darkness of extreme uncertainty, especially because we must assume certain characteristics of the whole system that embeds the part we want to change. We are always a long distance from our comprehensive ideal, no matter what we claim. All this amounts to saying is that we would like if possible to change again if our first change is not successful. Suppose we say that a change is teleologically irreversible if, once it is made and is unsatisfactory, we cannot erase its evil effect. Obviously, there are degrees of irreversibility, but the point is that we ought to adopt the strategy of avoiding irreversible changes. Will the strategy of incremental change do this most successfully? At first sight, yes, because if we only move a 'small' distance, we can 'easily' move back. But the more astute will see that this is not necessarily so, especially in the political world. The reaction to an unsatisfactory change may be such as to consolidate the conservatives or arouse the radicals. Furthermore, some easily reversible changes can easily be erased by the opponents, long before they have been tested and institutionalized. The authors are afraid of comprehensive and calculating people like TINBERGEN, ARROW, and BENTHAM. They fear that the proposed calculi of these men would irreversibly ruin the race, if they could feasibly be adopted. They could also have cited less quantitative but more comprehensive writers like SPENGLER, TOYNBEE, or E. A. SINGER. But it is not obvious why disjointed incrementalism is any blessing, either; while we are carefully moving to neighborhood status, the entire business may collapse. Someone, somehow, has to say something about what the whole problem is like, and how badly off the target are our present aims. Perhaps the healthiest way in which humans can live in large, complex, little understood systems is to carry on an endless debate between the realism that defends small changes while being unconscious of general aims, vs. the idealism that defends its *Weltanschauung* while being unconscious of immediate states.

I can understand the authors' fear of the formal model builders as well as those who will not move until all the data are in hand. But I cannot appreciate their wish to keep assumptions about the whole system unconscious, because whether one

likes it or not, any recommendation to change or not to change entails a commitment about the whole system. How can we possibly justify the position that these whole-system commitments should be suppressed?

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THOMAS L. SAATY and JOSEPH BRAM, *Nonlinear Mathematics*, McGraw-Hill Book Co., Inc., New York, N. Y., 1964, 397 pages, $12.50

THIS IS a different kind of book. Ordinarily a mathematics book may be described as 'vertical' when it treats a rather specialized mathematical subfield, such as topological groups, in depth, or 'horizontal' when it is a survey of a whole field of mathematically related topics, such as an introductory text on statistics and probability. In the former case the book is definitely for the specialist; in the latter it is for the novice. *Nonlinear Mathematics* seems to be a 'horizontal' book, but at an advanced level, cutting across all types of nonlinear phenomena and seeking to relate and expose a common fabric. It would like to provide a unifying perspective for nonlinear problems such as has been provided for linear phenomena by the theory of linear operators on Banach spaces.

The first of six chapters, "Linear and Nonlinear Transformations," carries the reader quickly through the definitions of vector space, linear transformation, inner product space, Hilbert space (finite and infinite dimensional), Banach space, and linear functional, and proves some fixed-point theorems. Diverse problems of linear mathematics are seen to resolve into questions of uniqueness or existence for particular linear operators on linear spaces. For example, the basic theorem on existence of a complete orthonormal set of eigenvectors for a self-adjoint, compact operator A on a Hilbert space is used to show the existence of eigenvalues and eigenfunctions for the classical Sturm-Liouville system.

The basic purpose of the book, to find a unifying view for nonlinear problems, is partially realized in connection with the contraction mappings $A(x)$ on Banach spaces. These (not necessarily linear) mappings send open subsets into themselves and satisfy a Lipschitz condition. Such mappings are shown to have a fixed-point property which is the basis for the iterative procedure $x_{n+1} = A(x_n)$ for solving the equation $A(x) = x$, an equation noted in the preface to be at the heart of many nonlinear problems.

The pace of Chapter I forces the reviewer to conclude that the chapter will only be appreciated by the experienced mathematician or graduate student already familiar with the basic concepts. It relates these concepts delightfully, but does not contain enough bread-and-butter examples to teach the beginner *both* the concepts and the 'horizontal' tissue of the relations. The closing section, which reviews all of Lebesgue integration in less than four pages, exemplifies this fast pace.

Chapter II deals with nonlinear algebraic and trancendental equations. The Newton-Raphson technique is explained, and the Ostrowski-Kantorovich theorem on existence and convergence of solutions is proved in a form that implies the result for abstract finite dimensional spaces. Even so standard a work on numerical