OPERATIONS RESEARCH AND BUSINESS DECISIONS*

THAT there has been a very rapid growth of interest in the operations research technique within the past decade is well known. Apparently the origins of operations research are to be found in the cooperative efforts of scientists during World War II. However, in the process of becoming, or attempting to become, an independent discipline, operations research has developed its own historians who are prepared to trace the roots of the new discipline to Thomas Edison[1] and, in one case, to Plato.[2] My own interest is narrow by comparison and centers upon the usefulness of operations research in business decision making.

It is my impression that there are two matters that deserve attention if the sole point at issue is the usefulness of operations research as an aid to management. The first is the extent to which operations research can legitimately be regarded as a new discipline with essential unity of method and purpose. The second is the extent to which operations research can achieve results of genuine importance in the industrial environment. I believe that operations research fails to qualify as a unique, self-contained discipline, and yet embodies enough that is new and helpful to be worthy of managerial attention. In support of these contentions, I will present, first, a critical appraisal of the operations research discipline; and second, a brief assessment of the practical value of the operations research technique in industry. A critical appraisal of the operations research discipline is essential to any assessment of its value in industry because the particular functions that operations research can per-

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form effectively for business managers depend upon the very nature of the discipline and its techniques.

An outsider seeking an appreciation of the nature and scope of operations research turns, quite naturally I believe, to the growing literature of the discipline and to the statements of its advocates. A modest amount of research in this direction has convinced me, at least provisionally, that operations research has more to fear from its friends than from its enemies. A statement or two by the friends of operations research may be sufficient to establish this contention.

We are told, for example, that:

> The Theory of Value reveals that operations research, as the science of decision, embraces not only the content of all the sciences—physics, social sciences, economic sciences, the philosophies including ethics, the political sciences—but also that it is intimately concerned with the postulates of these sciences as well. In this respect operations research can be regarded as the most fundamental of all sciences.[3]

Should one concede that operations research *is* this broad, it is not difficult to understand why the article from which the passage was cited required four authors, or to appreciate why operations research is, in general, a group effort.

Another instance of what I take to be an overly ambitious view has been expressed by the Earl of Halsbury. In an essay entitled (amazingly enough), "From Plato to the Linear Program," Halsbury states:

> I shall be concerned in this paper with a still wider field of operational research, a field standing in relation to any single operational study that might be proposed, somewhat as the complex number field stands to any finite number ring. This problem arises when we consider society as a whole and ask how its performance can be optimized.[4]

Halsbury's remarks on the optimization of the social process are of no special interest. But, imbedded in his essay is one passage which economists will find to be quite revealing. Halsbury writes:

> . . . We have in England at the National Physical Laboratory a simulator that will demonstrate Keynesian theory to the non-mathematician. The spectator can raise the bank rate on one dial setting and watch the consequences thereof upon unemployment represented by a calibrated volt- or ammeter at another point of the instrument. He can couple two of these simulated systems together in an importer-exporter relationship and watch the effect of a tariff barrier upon their reciprocal trading. Having thrown the pair into oscillation by some such device he can play with the controls in an endeavor to restore stability and, on failing to achieve it, can be shown by the demonstrator how to do so.
>
> Controversial economics are not possible under these circumstances; one cannot be partisan with respect to the reading of an instrument.[5]

It would appear that we have here an instance of model building with a vengeance. Competence in circuitry design is commendable; but its relevance to economic matters is, I think, not great. The advocates of operations research weaken their case immeasurably when they couple the most superficial of observations on economic matters with sweeping analysis of the whole social process. I would conclude that the scope of operations research (as written about, if not as practiced) is often too broad to be manageable; and that in the process of assimilating the functions of many separate disciplines, operations research takes on tasks beyond its apparent capabilities.

One may, of course, object that any discipline, and especially one in its formative stages, is very likely to find some of its devotees given to exuberance, even intemperance, in evaluating and defining their subject matter. But having made this allowance, and having applied some appropriate discount factor to the more outrageous claims made for operations research, I am forced to the belief that extravagance remains. Nor can I draw much comfort from Philip Morse's 1953 statement as retiring president of the Operations Research Society of America that:

> We should no longer have trouble explaining the scope and methods of operations research to the layman. We already can say: *Operations Research is the activity carried on by members of the Operations Research Society; its methods are those reported in our JOURNAL.*[6]

I would not pursue this line of attack at all were it not the fact that, superficially at least, disturbing parallels can be drawn betwen operations research now and "scientific management" then. Serious practitioners of operations research could well be disturbed by excesses committed within their own ranks. Much that was path-breaking in the "scientific management" movement was lost, at least temporarily, in the inevitable reaction to extreme statements as to the power and significance of the new approach. Operations research may be running the same risk.

Irrespective of the more extreme claims made from within the operations research profession, is there anything in the methodology of operations research that prevents the new discipline from attaining independent status? I believe that there is. I am not concerned here with the *techniques* developed (or appropriated) by operations research such as linear programming, information theory, queueing theory, game theory, and the like. While the value in application of some of these techniques may be questionable, there can be no objection to their use and further development if they prove to be effective. Rather, I refer to the difficult problem as to the selection of appropriate criteria by which a solution achieved by operations research methods is to be judged.

Operations research in attempting the solution of either military or industrial problems must make, or have made for it, a decision as to the objective or objectives to be attained. This decision as to objective implies the selection of an appropriate criterion. Selection of an inappropriate criterion can result in suboptimization; that is, the objective of a part of the organization will be achieved to the possible detriment of overall organizational success.[7] For example, in the economic theory of the firm, it is often assumed that an appropriate criterion is the maximization of total profits over some time horizon. Yet, as I read the operations research literature relating to the theory of the firm, there is little recognition of the implications of profit maximization. Amidst a welter of inverted matrices and symbolic logic, one too often finds a piecemeal approach applying clearly inappropriate criteria. I submit that so long as some operations research practitioners continue in their industrial efforts to ignore the lessons of sophomore economics one must agree with Charles Hitch who has written from within the profession:

> Where money values are used to measure effort and merit, a weak but highly significant kind of optimizing process results in an economy—one with which Adam Smith was familiar, and with which operations researchers ought to be but generally are not.[8]

I do not suggest that operations research techniques are necessarily unsuited for managerial application. Nor would I contend that economics provides such powerful guides to business decisions that no useful area remains where operations research can make a contribution. Quite the opposite. But it is discouraging to find operations research techniques applied uncritically and in apparent ignorance of well-established findings in theoretical and applied economics, industrial administration, and industrial engineering. Indeed, in the bulk of the operations research literature, the attention given to the selection of appropriate criteria by which alternative courses of action in the industrial environment are to be judged is extremely cursory and disjointed.

This omission would not be so objectionable were it not for the fact that operations research purports to embrace the selection of appropriate criteria and thereby to provide definitive guides for business decisions. At the worst, operations research implies a substitution of something for the executive function. As to precisely what it is that is to be substituted, I am uncertain, and I find little enlightenment in the literature. We are told by Bernard Koopman in an analysis of the fallacies of operations research that:

> "Authorititis" is that regression to logical infantilism which believes that the missing links in one's solution of a problem, as well as the common sense required for relating it to reality, can be readily supplied by the uniformed officer or the company executive who must eventually use the result.[9]

Along similar lines, Ellis Johnson has written:

> . . . Its most distinguishing characteristic [referring to operations research] stems from the fact that it has been concerned, since its inception, with the overall aspects of action systems. In general, the older professions that exist to serve management have made a sound, healthy application of scientific principles to *separate* elements of action systems. Operations research is more concerned with "optimizing" the operations of the whole organization than with improving operations within one division thereof. Obviously, in order to study and understand the overall system, operations research must study and understand all of the components, and so is interested in serving management at all levels within the system. Only thus can it serve the whole organization.[10]

While it is eminently proper for Koopman to decry the evils of "authorititis" and for Johnson to note that "optimization" must refer to the activities of the entire firm, it is far from clear to what extent operations research activities complement traditional decision-making processes, and to what extent operations research attempts to *substitute* for executive functions. My own view is that the successful application of operations research techniques is definitely contingent upon an operations research group working subordinate to and under the direction of management. Specifically, I am less disturbed by the dangers of "authorititis" than I am by the possibility that the choice of appropriate criteria by operations research groups will be excessively naive.

Rather than claiming that the application of operations research technique magically enables one to ask the right questions in addition to providing the right answers, is it not possible that the operations research profession could be fully and efficiently employed in the solution of industrial problems given to them by separate operating divisions and, perhaps less frequently, by top management? No one, I am sure, would wish to proscribe an operations research group from serious thought about, and analysis of, as Johnson puts it, entire "action systems." But, until this infant discipline demonstrates more maturity, I doubt very seriously that substantial responsibility for decision making should be removed from top levels of management.

I will proceed now to consider briefly whether the operations research technique can be of value in business situations. Earlier remarks as to the extravagance of some estimates of operations research and references to the difficulties inherent in the selection of appropriate criteria could be taken to mean that operations research is valueless in the industrial environment. I would judge that the operations research technique *can* be of definite value in industry. Doubts arise in my own mind only to the extent that operations research practitioners view their discipline with such enthusiasm that they fail to appreciate difficulties inherent in the industrial environment.

To be somewhat more specific I would contend that most of the barriers to effective decision-making in industry occur not for want of knowledge as to how a problem might be analyzed if needed information were available. Aside from difficulties of internal communication and the irreducible uncertainties of the future, the major barrier is the embarrassing wealth of irrelevant information and the deplorable scarcity of relevant data. Even the fabulous capacities of modern electronic computers may be overtaxed in some industrial situations with the digestion of trivial and misleading information. Some comfort may be drawn from the fact that these machines seem to survive in the military and governmental establishments.

It is distressing that in crucial areas of industrial budgeting and control, financial data eminently suitable for stockholder consumption and internal revenue purposes continue to provide only the most imperfect guide to managerial decisions. However one views the growth of operations research, it does seem inescapable that the application of very refined techniques to aid business decision-making will necessitate prior improvement in the quality of the basic data. Perhaps the interim period in which superior data reporting systems are being developed will allow the operations research profession time in which to adapt its methods more closely to the needs of industrial situations. One can hope that in the process of this adaptation, the findings of established disciplines will not be ignored entirely.

Indeed, there are, or should be, no vested interests in this regard. Wherever operations research can add to existing tools of analysis or contribute to the development of more relevant data, that contribution is welcome. There have been tangible achievements by operations research and the future may bring further progress. Operations research is best viewed as a means of bringing the undoubted skills of applied scientists to bear upon specific industrial problems. In the last analysis, only operational experience can determine the extent to which operations research will be of value to industrial managers.

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NOTES

1. William F. Whitmore, "Edison and Operations Research," *Journal of the Operations Research Society of America* (referred to subsequently as *Journal*), February, 1953, I, 83-85.
2. Earl of Halsbury, "From Plato to the Linear Program," in *Operations Research for Management,* McCloskey and Coppinger (eds.), Vol. II, (Baltimore, 1956), pp. xvii-xxxvi.
3. Nicholas M. Smith, Jr., and others, "The Theory of Value and the Science of Decision: A Summary," *Journal* (May, 1953), I, 103-113.
4. Halsbury, *op. cit.*, p. xviii.
5. *Ibid.*, p. xxii.
6. Philip M. Morse, "Trends in Operations Research," *Journal* (August 1953), I, 159 (italics in the original).
7. On this point see Charles Hitch, "Sub-optimization in Operations Problems," *Journal* (May, 1953), I, 87-99, *passim.*
8. Charles Hitch, "Comments," *Journal* (August, 1956), IV, 430.
9. B. O. Koopman, "Fallacies in Operations Research," *Journal* (August, 1956), IV, 424.
10. Ellis A. Johnson, "The Executive, the Organization, and Operations Research," in *Operations Research for Management,* McCloskey and Trefethen (eds.), Vol. I, (Baltimore, 1954), pp. xiii-xiv.