Some Reflections on Operations Research in 1960

By JOHN S. DE CANI, Fulbright Professor, Norges Handelshøyskole[1])

The 'Fifties were the decade of Madison Avenue, of grey flannel suits, and of the soft sell. They were the decade of the mediocre but sincere. Too often, the "Fumbling 'Fifties subsituted Public Relations for solid scientific achievement.

The 'Fifties also saw the introduction of operations research on a large scale into non-military government agencies and private business. Unfortunately, operations research as it is preached and practiced today, especially in the United States of America, shows many of the characteristics of the decade in which it reached full flower, the decade which has just passed. It has been both oversold and overdamned, and it is high time we took a more reflective look at it, seeing it for what it really is and questioning some common misconceptions concerning it.

The first of these misconceptions is that operations research is a new scientific approach to the solution of management problems. Until the late Nineteen Forties, operations research was a military secret, but it was the kind of secret that could not be kept for long once World War II had ended. Wartime operations research teams were disbanded, and members of these teams found their way back to private industry, where they introduced many of the methods of operations research to the solution of problems arising in reconversion from wartime to peacetime production. Soon the secret was out, and the "New Science" burst upon us.

Of course, it was not new. As "operational research" it had been in existence since 1939, when it was used by the British in the study of the best means for utilizing early warning radar. Lanchester in England and Edison in America had applied scientific principles to the study of warfare in World War I, although their discoveries were not applied at the time. Even the ancient King af Syracuse recruited Archimedes to help him fight the Romans. The techniques of F. W. Taylor in the Eighteen Eighties and Nineties and of Erlang in the first and second decade of this century were quite modern by operations research standards. Since the source of these historical statements is a standard work on operations research (4), it is clear that at least some practitioners are not claiming that it is new.

The novelty of operations research soon disappears when we attempt to define it. It has been variously defined as "the study of operations with a view to improving them" and "the maximization of an objective function subject to constraints." The latter

[1]) On leave of absence from the University of Pennsylvania during the academic year 1959–60.

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definition is a restatement of the central problem of neo-classical economics: the allocation of scarce means to competing ends so as to maximize some utility function.

The unique contribution of operations research is really to broaden the scope of the application of the scientific method to organizational problems. Operations research introduced the concept of sub-optimization: the concept that improving one operation need not necessarily result in an improvement in the efficiency of the whole complex of operations which make up an organization.

A second major misconception regarding operations research is that a special kind of training is required of those who would apply operations research effectively. There are those who claim that the only people who have a right to practice operations research are those who have been admitted to the cult by sacred rites conducted at certain institutions of higher learning. "If you ain't in the club, you can't play."

Those who make this claim defeat themselves in the very next breath when they assert that operations research is best practiced by a team of experts, each trained in a different discipline and each bringning the unique point of view of his discipline to the problem. The requisite disciplines are usually listed as mathematics, statistics, economics, and one or two representatives of the physical and biological sciences. Engineers are tolerated, provided they are not too "narrow".

The contradiction becomes apparent when we realize that, if one is to become "expert" in both mathematics, for example, and operations research, the qualifications for "expert" in one of these disciplines can't be too severe.

Basically, the main qualifications for a good operations analyst are imagination, initiative, receptiveness to new or different ideas, and training in any of several scientific disciplines which emphasize the spiral sequence of research: hypothesis, experiment, observation, test, and up to the next cycle of the spiral with a new hypothesis.

This brings up the third major misconception: Operations research must be practiced by a team. While it might be a lonely occupation in some business organizations, it is possible for one man to apply operations research effectively to the problems of an organization. It has been my own experience that operations research teams are not exempt from Parkinson's Law: Work increases to fill the time available for its completion. Most of the "work" thus produced turns out to be busy work, and the effective work of the team is accomplished by one or two members. The other members tend to obstruct progress with seemingly endless and pointless argument. A committee has been defined as a group of the unwilling selected from the unqualified to do the unnecessary: a similar definition might be applied to many operations research teams.

The story is told that when Galileo discovered sun spots his father refused to accept their existence because they were not mentioned in Aristotle. Things haven't changed much since poor Galileo told his father of the wonders he saw through his telescope. We have too often been confronted with statements like, "There is nothing in Keynes that isn't in Alfred Marshall." In fact, the preceeding argument shows all of the earmarks of such an attitude. Therefore, before I am embraced by the reactionaries and executed for treason by my colleagues, let me assert my conviction that operations research can make a valuable contribution to management.

Textbooks in operations research are of necessity devoted mainly to descriptions of the "tools" of operations research. Their tables of contents list such things as inventory models, linear programming, queueing theory, and scheduling models, and operations research is often regarded as defined by such cellections of techniques. Yet most of

the techniques used in operations research were not devised by people who regarded themselves primarily as operations analysts; they are developed by mathematicians, economists, and engineers.

What is it, then, which makes operations research unique? Basically, it is the attitude of the investigator and the scope of the problems he considers. The pre-war management consultant was concerned primarily with minutia. If, for example, he considered a problem in inventory control, the bounds of his problem usually ended with the inventory operation. He was little concerned with the effects of his recommendations on the production or purchasing operations which preceded the inventory operation, or on the sales and distribution operations which followed it. In contrast, the modern operations analyst considers particular operations in relation to the goals of the total organization. He is continually apprehensive that improvement in one operation create problems in other opreations and result in a reduction in the efficiency of the total organization. His bugaboo[2]) is sub-optimization. He is, therefore, concerned with ways in which the collection of operations which define the total organization may be simultaneously or sequentially improved. To define operations research as a collection of techniques is misleading, for it draws attention to single operations and small details which are of only secondary importance in operations research.

The operations analyst has at least one other attribute which distinguishes him from the pre-war management consultant: he is suspicious of "the tried and the true". His first question is likely to be, "What is there about this problem which makes it unique?" and not, "What is there about this problem which makes it possible for me to use the X-technique in solving it?" Of course, asking such a question is part of a research technique, but this technique has the advantage that the limitations of standard methods of solution when applied to the particular problem at hand are made explicit at the outset. The operations analyst knows that he must rely on his own ingenuity to solve those aspects of the problem which make it unique.

Hence, it is not in the application of a specific set of techniques that operations research makes its contribution; it is in the state of mind of the operations analyst. Some textbooks on the subject have tried to present this elusive idea. One of the best discussions is in Saaty (3) in Chapter 12, "Some Thoughts on Creativity". This chapter should be required reading for anyone seriously considering operations research as a career.

After more than ten years of living with, if not utilizing, operations research, we ought to be accustomed to it and to accept it neither as a universal panacea nor as "nothing new". We should accept it for what it is: a state of mind and a way of asking questions in research directed toward optimizing the behavior of complete systems rather than of the individual operations of which they are composed.

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