



Management Science

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In a highly pragmatic, action-oriented society such as the United States, one of the most distressing things that can happen to a person is to be relegated to the sidelines during a crisis, watching someone else set things right. This is the unfortunate situation of the contemporary management scientist. In spite of his training, his potential contribution—even his hopes and aspirations—he is still very much on the “sidelines” in modern America.

Management science, operations research, applied quantitative methodology, or whatever you choose to call it, began to emerge as a distinct body of knowledge in the United States around 1950. The ensuing years were busy and exciting for those of us who were involved in this “new” field. It was stimulating and satisfying to make studies and prepare elegant reports full of analyses and recommendations for the industrial managers of that period. The fact that these reports gathered dust by the dozens on many an executive desk neither deterred nor even seemed to bother us particularly.

It was a time when the large corporations were scrambling for talent to staff the “applied mathematics” or “operations research” groups that were blossoming like flowers on organization charts all over the country. Consulting assignments had never been more plentiful. The appearance of commercially available electronic computers was reinforcing the promise of a wonderful future for the field.

Then, suddenly, sharply, and with devastating effect, came the post-Korean War recession of 1957. Although this was a relatively short but severe recession, the ruthless response of the industrial corporate community to the budding field of management science was appalling. At the very time when the contributions of simulation analysis, applied statistics, O.R., and the like should have been called upon for help and guidance in dealing with the harsh problems of an economic decline, the groups that could have supplied such services were terminated summarily.

Many an engineer-turned-management scientist found himself once again wielding a slide rule in a design department. Many a systems analyst found himself grinding out payroll and cycle billing programs. Many a young Ph.D. found himself back on a college campus teaching courses in work measurement or quality control. (Whoever heard of zero defects in those days?) It was a traumatic experience for the members of today’s older generation of management scientists.

Have we learned the lesson of that bitter and deflating experience? I submit that we have not learned that lesson, and further, that many of us have never even recognized the lesson that was there to be learned.

What is the lesson? It was implicit in the way the top managers of the day dealt with the problem situations that confronted them. The lesson is: when the chips are down, managers behave on the basis of intuition, not on the basis of formalized logic.

A decade has elapsed since then. Would the top managers of today operate in the same way? Would they respond to a recession in exactly the same manner as did their predecessors of ten years ago?

All of us at one time or another have had the privilege of working with a truly competent manager. What qualities characterize these top-flight executives? High caliber, experienced executives have the ability to size up situations almost at a glance. Their perceptive powers enable them to penetrate to the heart of matters in an unbelievably short time. On the basis of rapid diagnosis, these men seem to know immediately what needs to be done. Generally speaking, they do not need exhaustive reports complete with analyses and documentation to tell them what to do.

To be sure, the top men do not ignore such information if it is available. However, they do not rely upon it as a basis for action. These are decisive men. They are willing to make decisions on the basis of their diagnoses and intuitive analyses. Without doubt, they are men of action. Worst of all, from our point of view, these experienced managers operate with an implacable aura of confidence. They are not egotists, just confident of their ability to manage. That they manage successfully is obvious. Otherwise they would not be where they are.

In the recession of 1957, these men felt sure that they did not need us, the "management scientists." Not only did they not need us, they fired us! It has taken our discipline a decade to recover. Has the lesson been learned? Could the same thing happen again? Can our field suffer a second humiliating repudiation?

The probabilities of such a thing happening again are remote. This prediction is based on three factors. First, modern management science embraces a powerful body of knowledge which has much to offer managers at all levels. Second, the real and potential contribution of management science is much more widely recognized now than it was five, ten or fifteen years ago. Third, for the first time management scientists are trying to establish communication with the important people they must serve—the managers.

Management scientists have never really established communication with the management practitioners. The proper conditions for effective communication rarely exist when the two come face to face. The necessary mutual respect and mutual interest give way to impatient inarticulateness on the one hand and a combination of fear and indifference on the other. The O.R. man resents having to sell his wares, while the executive fears looking foolish. Communication cannot occur where such attitudes exist.

The responsibility for inadequate communications between practicing managers and management scientists must rest on the latter. Too many aspects of their behavior inhibit the free interchange of ideas. For instance, in trying to show

what he has to offer, the management scientist often fails to come out from behind the formalism of his methodologies. Furthermore, he does not seem to realize that communication is hindered by the use of too much technical jargon ("payoff matrix," "strategies," "heuristic," "programming," etc.).

In addition to devoting effort to improving communication with members of management, management scientists might consider a number of other strategies to encourage managers to become more interested in the field. They should give priority to strategies which actually involve managers in management science activities whenever possible. The implication of this concept is that more heuristic techniques should be developed in the next few years.

We know that it is easy for a manager to lay aside a report which contains the statement of a problem, an elaborate analysis, and a set of recommendations. He finds it easy to ignore this report, even if he commissioned it. Why? The reason is that he did not participate in any way in the analysis or in the formulation of the recommendations. We have seen this happen over and over again.

Such difficulties can be avoided if the manager can be brought into the study from time to time so that he can make his contributions. Merely receiving periodic progress reports is not enough. He must participate in the project, even if his contribution is small. If this is done, the manager will not casually lay aside the final report.

This brings us to the task at hand. We do not want the manager to be so quick about laying aside his copy of *Management Science* when it comes across his desk. We want the Journal to be more than a reference used by master's and doctoral students in documenting their work. We hope managers will develop the same attitudes of anticipation and interest towards this publication that they manifest towards such periodicals as *Business Week*, the *Harvard Business Review*, and *Fortune*.

This can be achieved only through participation and involvement. Just as managers must be brought more into the application of management science, they must also take part in the creation of the literature. They can bring to the field a new and lively dimension by identifying unfamiliar problem areas, interpreting developments and trends, criticizing results, and providing encouragement.

The development of this new dimension will bring the *science* of management into a more active role in relation to actual management practices, making the profession more current, effective and relevant to our fast-moving way of life. When this happens, management scientists will no longer be standing on the sidelines.

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Letter to the Editor—Free-for-All

In a recent paper, "A Decision Theory Approach to Portfolio Selection" (*Management Science*, Vol. 12, No. 8, April 1966), James C. T. Mao and Carl Erik Sarndal discuss the use of Donald E. Farrar's objective function (*The Investment Decision Under Uncertainty*, Englewood Cliffs, New Jersey, 1962) as a

means of choosing optimal portfolios. Several other writers have commented on or used this objective function in papers on the subject of portfolio selection. This letter demonstrates analytically and by means of a numerical example that Farrar's objective function for portfolio selection violates a well-accepted axiom of decision-making. Therefore Farrar's criterion cannot possibly be the basis of rational investment decisions (or rational decision-making under uncertainty in general).

Farrar's objective function is defined as follows: If one is faced with a choice of possible portfolios, each characterised by its expected payoff μ_i and the variance of its payoff σ_i^2 , then the value of each portfolio can be measured by $U_i = \mu_i - A\sigma_i^2$ (where A is a parameter of the decision maker, the "co-efficient of risk aversion") and one should choose that portfolio for which this measure attains its maximum value.

Suppose we are considering three risky investments $I_i (\mu_i, \sigma_i^2) i = 1, 2, 3$. Suppose further that I_3 is simply a lottery ticket giving I_1 as a prize with probability p , and I_2 with probability $(1-p)$. Then surely to a "rational man" the "utility" or value of I_3 should be somewhere between those of I_1 and I_2 , and any function which purports to measure this utility should reflect this if it is to be an acceptable measure. The following analysis demonstrates that Farrar's measure does not necessarily satisfy this requirement.

Let x represent the cash value of the return from an investment. Suppose two investments I_1 and I_2 are being compared, for which x has the distributions $f_1(x)$ and $f_2(x)$ respectively. Let the expected values and variances of these distributions be $\mu_1, \sigma_1^2; \mu_2, \sigma_2^2$. Consider investment I_3 which is a lottery ticket giving I_1 as prize with probability p and I_2 with probability $(1-p)$. Then the distribution of the final outcome of I_3 is

$$f_3(x) = pf_1(x) + (1-p)f_2(x).$$

From which

$$\begin{aligned}\mu_3 &= p\mu_1 + (1-p)\mu_2 \\ \sigma_3^2 &= p\sigma_1^2 + (1-p)\sigma_2^2 + p(1-p)(\mu_1 - \mu_2)^2.\end{aligned}$$

Applying Farrar's measure, we obtain

$$\begin{aligned}U_1 &= \mu_1 - A\sigma_1^2 \\ U_2 &= \mu_2 - A\sigma_2^2 \\ U_3 &= pU_1 + (1-p)U_2 - Ap(1-p)(\mu_1 - \mu_2)^2.\end{aligned}$$

Consider

$$\frac{\partial U_3}{\partial p} = U_1 - U_2 - A(1-2p)(\mu_1 - \mu_2)^2.$$

If we set $p = 0$, this becomes

$$\left. \frac{\partial U_3}{\partial p} \right|_{p=0} = U_1 - U_2 - A(\mu_1 - \mu_2)^2.$$

If $U_1 = U_2$, this expression is negative. This implies there must be some values of p for which the lottery ticket is imputed to have a smaller value than the (equal) values of its two possible outcomes. Even if $U_1 > U_2$ this expression can still be negative. This would imply that for small values of p , increasing the chance of getting the more preferred outcome decreases the value imputed to the lottery ticket, which is absurd. As a numerical demonstration consider the following:

$$A = 0.5; \quad \mu_1 = 6, \sigma_1^2 = 3; \quad \mu_2 = 4, \sigma_2^2 = 1;$$

then $U_1 = 4.5; U_2 = 3.5$.

If we take $p = 0.25$, it is easily seen that $u_3 = 3.375$, which is less than the value attached to the poorer of the two possible outcomes of the lottery.

It would thus appear that Farrar's measure can give results which do not accord with our intuitive requirements of a "good" decision-making criterion. The importance of this is well expressed by R. Duncan Luce and Howard Raiffa (*Games and Decisions*, John Wiley, New York, 1957). In summarising their approach to the problem of finding a decision-making criterion they state, "... we culled from our intuition a number of reasonable desiderata which a respectable decision criterion ought to fulfill. Naturally, we hoped as a result to be better able to discriminate among suggested candidates for a definition of optimality. . . ." In this way Luce and Raiffa demonstrate that a number of suggested criteria, e.g., minimax, minimax regret, pessimism-optimism index, etc., are unsatisfactory. It is Luce and Raiffa's axiom 9 (convexity which both the pessimism-optimism index and Farrar's criterion fail to satisfy). For similar reasons, Farrar's criterion is unsatisfactory and ought to join the ranks of those which have been investigated and found wanting.

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Letter to the Editor—Free -for-All

After successfully bringing to an end the second great conflagration of the twentieth century, operational scientists were faced with the loss of their principal laboratory—military problems. Casting around the contemporary scene for a new workshop, the scientists decided that management and administration would be the object of their reforms. Consequently, the ensuing two decades saw the operational scientists giving aid and succor to the confused ranks of managers and administrators—and the learned advice of the scientists was to be found in the cherished journals of their profession.

At the end of the second decade, it came to pass that the operational scientists wished to take the pulse of the rank and file. One of their cherished journals begot an epistle—free and available to all—with the hope of finding out whether or not their subjects were happy. But the response was not positive. Worse yet, it wasn't even negative. The sad result was no response at all, no cries for bread or, for that

matter, even cake. The number one apostle in charge of this epistle was soon to report that he was "bored with the lack of mail, tired of not receiving sparkling and witty words and clear perception—but mostly sad that there are so many who talk so much but cannot write a word." (*Management Science*, October 1966, p. B-109.)

Perhaps no one had been listening!

Perhaps the rank and file had long since become bored because there had been so many who wrote so much but had so little to say!

Perhaps it was because the operational scientists had forgotten their subjects and had developed a science for scientists rather than a science for management!

Perhaps also the time has come for the chiefs of the operational scientists to parley with the indians. Possibly the few voices still crying in the wilderness represent the last of the interested rank and file. It might behoove the chiefs to pay heed to those cries and to accept them into their council.

Bigelow's plea is the most recent, albeit not the first, call for pragmatism (*MS*, October 1966, pp. B-109–110). Heany's comment that "What seems to be lacking is a willingness on the part of many in TIMS to tackle problems which appear insoluble or to move ahead in areas where their neat techniques of analysis are not useful" reinforces what may be a fairly common feeling among rank and file members (*MS*, September 1966, p. 153). It takes little more than a casual glance at the articles appearing in the cherished journals to realize that Bigelow and Heany are not just whistling in the dark. This is especially true with respect to marketing. Some marketing examples taken from recent issues serve as illustrations.

* In a study of market penetration, the author stated that the "results of this analysis are based upon selected situations which may not necessarily represent a real situation." (*MS*, September 1966, p. 29.)

One need only to read the article to understand why the article's content was not applied to a real situation.

* In a survey of the profession's achievements in advertising, the author stated that some of the early models made assumptions which "... were not testable. The results of these models were not reliable nor, perhaps, even rationalizable." The strongest statement made with reference to the success of later models is that "... some models began to predict with some measure of reliability." (Author's italics) The author did not comment on the assumptions of these later models. He concluded that "In the last analysis it is only the ground that has been broken, and some of the old guard vested interests that have been overcome." (*MS*, October 1966, p. B-25.)

The author listed sixty-five references. Surely this should give cause for a few moments of reflection on the profession's achievements in advertising.

* In a study of advertising in coupled markets, the author assumes an exponential form of sales response to advertising, a known sales saturation level and knowledge of the slope of the sales response at the origin. In his concluding remarks he stated that "The above model has not as yet been applied to real markets. It is advanced in the spirit of affording a theory of dynamic coupled markets which can serve as a forerunner to practice." (*MS*, August 1966, p. B-528).

This writer feels that it would not be unjust to ask whether such materials do not more appropriately belong in a journal for scientists.

- * In an article applying the Bayesian approach to retailing, the authors concluded that "Although our treatment assumes that the prior distribution is found from the beta family, this choice is made mainly for convenience of illustration and computation. Exactly the same analysis can be made for a prior distribution of arbitrary form, though only at the expense of introducing a numerical integration each of the many times a posterior distribution is needed." (*MS*, July 1966, pp. 796-797.)

There seems to be something in Heany's comment that applies to the above statement.

- * The "traveling salesman" problem is frequently treated in the literature. (See *MS*, November 1966, pp. 269-288 for the most recent treatment and a list of other treatments.) It unrealistically assumes that minimizing the cost or distance between a number of city-pairs to be visited once and only once is both the valid criterion and an accurate statement of the problem. Serious questions can be raised as to whether any part of the salesman population behaves according to the assumptions of this problem.

Here again the emphasis seems to be on techniques rather than a practical solution to management's problems.

This short review seems to lead to the conclusion that—at least with respect to marketing—there are many who write, but few who seem to have anything to say to management. The above examples suggest that one must ask at whom or at what groups are these articles directed? Since they do not seem to be directed at management, they must be directed at scientists. In turn, this conclusion suggests a matter to be contemplated by the chief operational scientists. That is, if this trend continues, TIMS may have to talk to itself because no one else will be listening.

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Letter to the Editor—Free-for-All

In the bulletin section of the February issue of *Management Science* I reported on a movement to found a TIMS Marketing College. The purpose and some suggested activities of such an organization as well as our progress in the organizational effort were reported at that time. The purpose of this letter is to appeal to interested practitioners to make known their interest in this activity while it is in its formative stages. Many letters to Free-for-All have charged that TIMS is talking to itself. This is an open invitation to all interested parties to participate actively in the development of the Marketing College so as to reduce the likelihood that such a charge will be justified in the future. Interest in the proposed College should be communicated to any member of the planning board, which is listed in the February issue.

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