MANAGEMENT SCIENCE?

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We hold academic appointments at University-based schools of business/management in fields which come under the general description "Management Science." So, we are subject to review by our departmental, college and university Promotion and Tenure committees. We also work with industrial clients - consulting, teaching, etc. - where our work is subject to such immediate criteria as practicality, applicability and clarity.

Our two areas of experience — the academic and industrial settings — would seem complementary, providing encouragement and support for a mix of theory and practice. This is certainly true in that we are able to lace our classes with pertinent "war stories," and true too in that ongoing course development forces us to stay on top of "state of the art" to the benefit of our industrial clients.

However, as our colleagues know, there is an important unresolved conflict between what is required to advance through the academic ranks and what is required to conduct oneself successfully in the industrial setting. The recent commotion over Machol's letter in OR/MS Today [1] is one sign of this conflict: there the issue is focused on "Journal Readability."

In our opinion, this is but a symptom of a more serious problem: what constitutes legitimate research in the field of Management Science? We have tried to answer this question for ourselves and have come to this conclusion:

Academic-based management scientists can work on real or toy problems: in these contexts, they can conduct real or toy research.

So we have:

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It is our contention that the bulk of articles appearing in the refereed journals are in fact either Toy Research on Real Problems or Real Research on Toy Problems. It is further our contention that the recent changes in the TIMS/ORSA journals will encourage more and more of this and less and less Real Research on Real Problems.

We will elaborate on these categories, but before continuing will happily classify our own doctoral theses as Toy Research on Real Problems. We will

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not embarrass anyone by identifying a case of Toy Research on a Toy Problem, but we have both recently rejected articles for publication in one or another refereed journal because that is exactly what they were.

Real research on real problems? We know of several examples which will never see publication because they were done for clients with results which were sufficiently important that they are "company confidential." It is interesting to note that results like this do on occasion appear in print — disguised and generalized to the point that the methodology is extremely difficult to reproduce — in the "journal" reputed to be "all style and no content," the Harvard Business Review.

**Toy Problems (TP) and Real Problems (RP)**

We are all familiar with the art of modeling, so understand the necessary step of simplification of the real situation into tractable form. Bounding, inclusion/exclusion, determination of relationships, etc. — all are necessary to the accomplishment of research. The unfortunate end point of this exercise is the toy problem — a real problem simplified to the point that results cannot be intelligently integrated with other results.

Another class of toy problem arises when an extension to a known technique permits treatment of situations more complicated than previously. Whether or not any real problems correspond to the enhanced technique, the class of problems it can solve becomes fair game for the development of improved solution techniques. Consider, for example, the following fictional (but terribly familiar) abstract:

Glurtz and Smortzcrump have shown that, by treating certain simplex multipliers as imaginary numbers, the revised simplex method can be adapted to the cubic programming problem on the frequency plane. This paper shows that an algorithm based on Fibonacci numbers using group theoretic insights can yield efficiency increases of up to 8% for problems of this class.

Yes, TP/RP is a spectrum and we have identified the end points. Somewhere in the middle is the problem description which provides insight, permits solutions, displays previously unsuspected behavior.

The Real World has Real Problems which boggle the mind. Real managers work in this milieu and are understandably irritated at management scientists who exclude such intractable yet critical aspects as timing, cost of analysis, office politics and the like. For their part, management scientists continually go through the frustrating task of explaining that a particular model has value because it excludes "nonessential" elements.

We make regular use of Toy Problems in our teaching. The ubiquitous "find the optimal mix of tables and chairs" is invaluable in an introduction to linear programming. The first PERT network is often so simple that the critical path can be determined by inspection. Obviously, TP's have their uses. But even in teaching, one quickly moves to more realistic problems where the techniques are vital for solution.

Still, we often shy away from too much "reality." Here is the dilemma: we can expand the scale of the classroom example, add complications which will permit illustration of the technique's "bells and whistles," or we can introduce a realistic problem in which the student will have to compromise to formulate, will discover the inadequacy of the data, will be faced with a budget (time and money) constraint. (This last alternative is a fair description of the type of case commonly used at the Harvard Business School.)
Toy Research (TR) and Real Research (RR)

Research is the formal process of investigation; it is a quest for principles, for theory, for the “laws of nature.” Our friends in the physical sciences have set the standards for scientific methodology — hypothesis formulation, experimental design, test, analysis of results. Yet, even there, we know that the ideal investigation is rare: major breakthroughs are as often the result of sudden insight, accident, or inadvertence as they are the outcome of careful and diligent application of the scientific method.

Research in the field of management is in its infancy if compared to that in, say physics. “Scientific management” is a creature of the 20th century; at its start it consisted almost exclusively of Taylor’s studies of work. By World War I, when our first schools of business were in operation, the question of business research appeared, largely because of the necessity of course development and for the training and development of appropriate faculty.

The Taylor studies and the first cases written at the Harvard Business School were the “research” available to students of business only five decades ago. Since then, we have evolved from schools of business to schools of management, and from single “business” faculties into increasingly disparate disciplines of which MS is but one.

How has our “research” evolved? In our opinion, evolution has occurred along three dimensions: data exploitation, experimentation, and abstraction.

Data Exploitation

Early studies were largely focused on particular industries. Data from the operation of firms within these industry categorizations were analyzed in an attempt to improve understanding of the workings of these major industrial segmentations. (Some of our older schools of business still have residual departmental groupings from this era — e.g., the Insurance Department at Wharton and the Railroad Room at Harvard.)

The data was experiential, not experimental; the analyses were cross-sectional. While these forms persist, more recent research tends to treat experiential data as if it were experimental data in an attempt to discover underlying relationships. Further, there has been increasing specialization both by functional area and by discipline. It is not coincidence that this development has been paralleled by the increasing specialization along the same lines within schools of management — but the direction of causality is not clear.

Experimentation

True controlled experimentation is not available to management researchers. The expense (not to mention risk) is generally too great for a company to permit its use as a laboratory. As a result, it has been necessary to devise experimental universes for experimentation — an exercise which can culminate in an assumption that the behavior of sophomore psychology majors is a valid representation of the behavior of corporate managers.

A peculiar form of experimentation is available to us; this is the application of theory to existing (real) situations, with analysis of results and some combination of empirical support of the theory and/or indications of the need for further development of theory. This mode of experimentation is in fact engineering in the best sense of the word, and is a valid description of the role we play in the industrial setting.

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We believe that much of the criticism aimed at the Applications section of Management Science is caused by unfulfilled expectation. It is the place where one expects to find descriptions of these “engineering” jobs. All too often, however, it is impossible to determine what “theory” (if any) is being “applied” to what setting. Worse yet, the necessary link from results back to theory is sometimes as hard to find as Judge Crater.

**Abstraction**

In the development of highly simplified models of reality, we come closest to the work of the physical scientists. However, while they tend to concentrate on testing their models, we usually slight this step and focus on optimizing ours. Indeed, this difficult objective of optimality tends to force us towards the grossly over-simplified model to allow the full power of an associated technique to be displayed.

The alternative — the more complex and hence (hopefully) more realistic model — drives us towards simulation where optimality becomes impractical, if not impossible, and model validation can become the key issue.

One form of abstraction which we share with other scientists is taxonomy — the sorting and classification of phenomena. This extremely valuable activity is usually relegated to Chapter II of doctoral dissertations. Its most common appearance is in the Preface and Table of Contents of textbooks. In neither case does taxonomy carry much prestige.

**Toward a Definition of Real Research**

If we set as our standard the definition of research as it is understood in the basic sciences, we are doomed to failure at the outset. Lacking the capability for true controlled experimentation, we have developed our own research methodologies, most of which are wholly foreign to the classical scientist.

Our colleagues who are practicing managers are quite disinterested in the development of research methodologies. Our university colleagues, however, are definitely interested; their interest expresses itself in University-level promotion and tenure considerations where the work of management scientists is necessarily compared with that of, say, chemists or mathematicians. After all, we are the ones who have tagged ourselves with the title “scientist”: who can blame them for holding us to classical research standards?

By these standards, our research appears inadequate. The question is this: should we strive to meet these standards or should we develop a set derived from the realities of the focus of our discipline?

In our opinion, much of the dissatisfaction with our current academic situation stems from the fact that our academic reputations can well depend upon our ability to appear at least as sophisticated as our cousins in the Math Department; this may give credence to our claim of “science,” but it neglects our responsibilities to the field of management.

As an aside, we should point out that we are not alone in this problem. It is pervasive in schools of engineering where mechanical engineers, for instance, are moving closer and closer to applied physics. Doctoral dissertations in the engineering disciplines display the resulting schizophrenia: good engineering is too frequently driven out by poor science.

So where does this leave us? Can we now distinguish between RR and TR?

We have tried out this RR/TR spectrum on several colleagues, using articles from Management Science. It is very easy to get agreement on which
articles are and are not RR. But, it is practically impossible to get the same agreement about what RR is and is not. Taxonomy is trivial; definition is difficult.

We are thus driven back to first principles: RR, whatever the problem setting, is that research which confirms, denies, or leads to the development of theory. The analogy with engineering is complete if we remember that the aim of engineering research is to develop theory so robust that its consequences are plain to sophomores, boring to juniors, open to question by seniors, and moving targets for graduate students.

The traditional image of the engineer is of an individual, sufficiently trained to keep track of the research of the mathematicians and physicists, who grasps available theory as real problems require, and when theory fails (or doesn't exist) makes a chewing gum/baling wire patch to bring the job in. The quick fixes, even the failures, become both the goals to the scientists for more research and, sometimes, the earliest stages of new theory development.

This, we believe, is what practicing management scientists are doing today. It is grubby yet exciting work; it involves the best and the worst of office politics, disastrous failures, expense over-runs, hunch-playing, and, happily, occasional jobs well done (sometimes for reasons no one can explain). However, this is not what appears in print. To the reader of the journal which bears the name of our profession, MS appears increasingly dull and trivial; yet we know that most of us enjoy what we are doing, and we think it is important.

**What's a Management Scientist To Do?**

So far, we have followed the path of our colleagues in the physical sciences, particularly those in the engineering disciplines. They too really enjoy their work, and late-night sessions abound with war stories — research gone awry and bailed out at the last minute, methodological screw-ups, races to finish "before the boys in Houston," blind alleys pursued, breakthroughs stumbled onto when the 360/ broke down.

But in print, the research appears thus: it was observed, it was hypothesized, it was tested, it was concluded. The fact that it bears no relationship to the actual chronology is a secret we all withhold from our graduate students until late in the thesis process.

Thus we observe the trend in our own journals toward reports of what appears to be more and more scientific but is unarguably more and more trivial. We agree with Machol that, intentionally or not, the least important work is couched in the most opaque prose. In a way this is fortunate since it practically guarantees that no manager will waste his time reading it.

To say this is to damn our refereed journals. Remember their purpose, it is to spread the word to those who need to know. Failure to remember this inevitably leads to Bad Science driving out Good Engineering. And, in our opinion, this is what is happening in Management Science.

Science and Engineering can coexist and thrive only through a creative tension, each demanding better and better of the other, egging one another toward better theory and better practice. It is a conflict never resolved but always shifting in emphasis, played out on the stage of real-world practice and proclaimed in the forum of the professional societies.

This is simply not happening in Management Science. The bulk of our science never finds application and the bulk of our engineering is never
brought to our professional associations. This is our own fault, at least in part. We don’t even try to put our “real” work into print; we are apologetic when we execute a successful job that doesn’t fit with current theory; we refuse to get involved when the pages of our journals are filled with increasingly trivial research on even more trivial problems.

Meanwhile, managers have deserted TIMS and practice-oriented Management Scientists are increasingly uncomfortable. We fear that Klein and Butkovich are correct in their prediction that “The profession will survive in its academic version and stabilize where academic output will just match academic demand” [2]. Before we write an epitaph for Management Science — “Move over Philosophy, MS is here” — are any last-ditch steps available?

We have a painful alternative: preach what we practice. Those of us who straddle the groves of academe and the real world can begin by bringing our most fascinating work out of the closet and into print. We can begin the “war story of the month” so that Professor Woolsey need no longer stand alone. Most important, we can point out — in print — where current theory appears to fail (at the risk, of course, of revealing the problem as our misapplication). Finally, we can insist that at least one of our journals become the forum the field requires. Our nominee is Interfaces.

References