ORSSA Presidential Address

Theo Stewart, 1978

Research and operations research

Of recent years there has been an increasingly acrimonious debate in some circles as to the value of modern academic research within OR. In view of the number of well-known names who have participated in this debate, it is with some trepidation that I enter it now. Nevertheless I will venture to do so in the belief that my background of having moved from line production management to OR, and to research in the methodology of OR must be a near-unique qualification for this purpose.

Now a certain level of rivalry between practitioners and researchers (missionaries and methodologists, as some have termed it) can be constructive. When it gets to a level where the two groups no longer talk to each other — practitioners dismissing researchers as irrelevant and incomprehensible, and being dismissed in return as unscientific ignoramuses — this is disastrous for OR as a force in modern decision-making.

I am thankful that we are nowhere near this situation in this country. Judging from recent discussions by the presidents of (the American) ORSA and TIMS, however, there seems to be some fear that such a situation is being approached in the USA. It is thus a danger we cannot ignore and should actively avoid, for our manpower resources in this country are far too limited to afford any suggestion of anything other than optimal communication and goodwill between researcher and practitioner. It is my contention that our society, ORSSA, can fulfil a vital function in maintaining our currently good level of communication and goodwill.

In developing my theme, I want to discuss firstly the role of research in OR, then future research needs in OR and finally the function of the OR Society in optimising contact between practitioners and researchers.

In discussing the role of research, I will concentrate on algorithmic development, or what may be termed mathematical OR methodology. The reason for this is that this is the area of most present research activity. This is not to deny that other research needs exist — but these I shall rather discuss when dealing with future research needs, constituting prospective rather than current research activity.

I strongly believe that the development of this mathematical OR methodology has a two-fold benefit for future OR practice.

The first is the evident benefit of providing new and more powerful tools. One may expect this basic fact to be undisputed by practitioners (although criticism that the wrong tools are being developed may be justified). Nevertheless amongst some there is, in my opinion, an unjustified arrogance in insisting that no more tools are required. A recent letter to the Journal of the

Operational Research Society suggests that further algorithmic development is unnecessary in that "the tools we already have allow us to give convincing quantified answers in the time required." My reaction is that this situation pays tribute to past research, and should encourage further research.

Clearly the OR profession cannot rest on its laurels. It is up to the researchers to identify future OR trends and to have the right tools ready at the right time. Here in South Africa we actually have a further potential problem. Threats of computer embargoes can lead to severely restricted computer facilities. It is not impossible therefore that there will arise in OR practice an urgent requirement for computationally highly efficient, although probably approximate, algorithms. Perhaps we should be giving this possibility more attention at present.

The second benefit of research in algorithmic development is perhaps somewhat less evident. This is that the analysis of perhaps over-simplified real-life situations can help in developing sound intuition. As someone has said: "The purpose of mathematical programming is insight, not numbers". Consider, for example, queuing theory. It is justifiably claimed that there are no Poisson processes, service times are not exponential and queue disciplines are seldom a simple FIFO. This is probably often true (although sometimes mere nit-picking), but this does not imply that queuing theory is of limited or no practical use. Let me quote from the presidential address of Professor Simpson to the British OR Society: "...in queuing situations, while the arrival and service distributions, the queue discipline and of course the context may vary very substantially from one situation to another, they all have a basic form which is reflected in the mathematical methods used and in the resultant models emerging. The ability to draw and to exploit such analogies is one of our strengths ... while the techniques as such may never be really applicable, they can often be used in the initial stages of a study to give valuable 'back of envelope' estimates..." Actually my experience is that decisions have often to be made on the basis of studies that don't get beyond this stage, making the back of envelope vitally important in its own right.

Thus, while I agree that some mathematical development is of academic interest only, generalising existing models to a level of abstraction unmotivated by practical needs, and has no place in OR, there is, I submit, a range of mathematical OR methodological research which we discard at our peril. Having said this, however, we must immediately qualify it by a call for conscious effort on the part of practitioner and academic alike to ensure optimal utilisation of our resources in this area to achieve maximum realization of the two-fold benefit of research. Let me therefore pass on to what seems to be perceived future OR research requirements, not only in the more mathematical research, but in OR research in general.

The 1977/78 president of TIMS, Gerald M Hoffman identifies two areas of great potential benefit to the practice of OR (OR/MS Today, November 1977). The first, I am glad to say, is one to which we at NRIMS have started to give attention, namely multi-attribute decision theory. Already here we are beginning to go beyond mathematics, and are keenly aware of the need, as Hoffman has pointed out, to see behavioural scientists contributing to the illumination of the behavioural assumptions which underlie the common multi-attribute decision models.

The second area identified by Hoffman is that of living with information systems. I feel this is an important point — in some areas at least, OR is going to have to contend with a situation not of insufficient data, but one in which efficient use has to be made of perhaps an over-abundance of data. What the precise nature is of problems arising in this area, and how they are to be solved are questions on which I would rather not comment at this stage.

Rather than dwell further on these methodological requirements, I want to draw your attention to another aspect of research. As I read in preparation for this address, I came across a recurrent theme: the need for OR to become more of an experimental science. Seth Bonder, in his plenary paper on "changing the future of OR", read at the recent IFORS conference, calls for the establishment of separate "Operational Science Research" aimed at "describing real world operational and management phenomena and developing associated causal dynamics", *i.e.* rather than developing models of processes from assumed axioms, the emphasis is on controlled experimental verification of general principles of such processes. We are thus called upon to move from the philosophical speculation of the Greeks to Newton's laws of mechanics. While I feel it dangerous to draw too sharp a distinction, as Bonder seems to do, between this operational science and mathematical OR methodological research, his description of this operational science research does sum up very well the opinions of others. Hoffman, TIMS president (OR/MS Today, May 1978) says, for example: "Learning to experiment is an important item on the agenda of Management Science." Simpson, in the address we have quoted, seems also to suggest something similar.

What is not at this stage clear, is the form which such "experimental" research should take. Hoffman has suggested an analogy with astronomy in that we cannot perform controlled experiments on management systems, but must rely on using models to make predictions and then to confirm these, or otherwise, through a systematic and exhaustive series of observations. Presumably thus, in decision analysis for example, we should be evaluating decisions using current models in a large number of widely different situations and then presenting them to the relevant decision makers. Where they are implemented the results must be observed, and where not the reasons for management rejection carefully noted. On the basis of information (some of it undoubtedly conflicting) built up in this way, the basics of our models will have to be modified. I refer here of course not to models of a particular situation, but to the axiomatic foundations of our art, for example, the axioms describing a rational decision maker.

The problem is that much of this observation has to be made on the job. Although in this country those in research also in general are active in OR practice, they have not the opportunity to personally make all necessary observations. They must rely on feedback from full-time practitioners — but this feedback must be in the form of scientific observation. To quote Hoffman again: "The theoretician needs the results of experiment to test his hypotheses about the world, and he expects to get these results from the community of practitioners. Instead he gets, at best, some anecdotal case histories". This leads up to my final point: We need thoughtful, meticulous and constructive criticism of existing models and methodologies from practitioners, based on valid experience in their use.

The theme which I have been leading up too, is that one very effective way in which this feedback of observation can be achieved, is by means of applications-oriented papers at our conferences — but applications papers which emphasize the strengths and weaknesses of the theory used — not simply relating success stories.

I know there are security problems in the best applications papers — I believe they can be overcome if the emphasis in papers is as I have described it. I know such papers require a substantial investment in time — but only by such investment can we hope to obtain any dividends. I acknowledge that such efforts place an awesome responsibility on researchers to protect this investment — I believe we can rise to the challenge.

This then is my message to the Society as I step down as president. Let us see greater use being made of our conferences and meetings in providing a continual two-way communication: practitioners lay open their problems, researchers reporting back on new tools, new approaches — in terms understandable to the practitioner. In his presidential address last year, Rob Eales emphasized communication between OR and management; I wish to emphasize communication within the OR community. Let me therefore call on the new executive, and on our membership as a whole, to give urgent attention to the promoting of a rich interaction between practitioner and researcher. We cannot afford to do otherwise.