# Applied research in management

C. Orpen
Deakin University, Victoria, Australia

The dominant model for research in management is the scientific one developed in the natural sciences. This ideal model is shown to differ in important ways from that followed by most applied researchers working on real-life management problems. It is argued that the dominant or ideal model has serious deficiencies when used to research many of the problems investigated by applied researchers. For improved understanding of management issues, it is suggested that the model followed by applied researchers be adopted more often than is currently the case.
*S. Afr. J. Bus. Mgmt.* 1985, 16: 116 – 118

Die dominante navorsingsmodel in bestuurstudies is die model wat ook in die Natuurwetenskappe gebruik word. Hierdie ideale model verskil in belangrike opsigte van die modelle wat deur toegepaste navorsers, aktief betrokke by werklike bestuursprobleme, gebruik word. Die skrywer voer aan dat hierdie dominante of ideale model ernstige tekortkominge het wanneer dit toegepas word op probleme waarby toegepaste navorsers betrokke is. Vir 'n verbeterde begrip van bestuurskwessies, word voorgestel dat die model wat deur toegepaste navorsers gebruik word, meermale gebruik word, eerder as slegs die gevestigde model.
*S.-Afr. Tydskr. Bedryfsl.* 1985, 16: 116 – 118

**C. Orpen**
Deakin University, Victoria 3217, Australia

Accepted February 1985

As is apparent from reading any orthodox text, management is typically studied on the basis that it is a science, which is likely to advance most rapidly if it employs essentially the same methods and procedures as other natural and human sciences. This claim is typically defended in a rather defensive manner, not so much by the results already achieved, but rather because management research relies upon the scientific principles of controlled observation and empirical testing of hypotheses, mainly through experiments or surveys (e.g. Stoner, 1982; Schemerhorn, 1984). However, besides being a scientific discipline carried out mainly, although not exclusively, by academics at universities (Orpen, 1983), management is also an applied discipline conducted by practitioners and consultants, whose aim is to improve the performance of individuals and organizations in specific situations. It is the argument in this article that the procedures which are in fact followed by these applied researchers differ in important ways from those followed by their academic colleagues. Specifically, it is maintained that academic researchers can learn a lot by following the example set by applied researchers working in the field, often quite independently from universities.

When following the scientific model, academics are typically advised to go through a series of discrete steps. First, they are told to study the existing literature for problem areas that are worth investigating. Having done this, the next recommended step is to design and conduct an experiment or survey to test the hypotheses developed from the literature search. Finally, the data produced by the study are tested statistically to establish whether or not the hypotheses are valid. If the hypotheses are supported by the results, then all is well. However, when (as so often happens) the results fail to confirm the hypotheses, what is the management researcher to do?

A common response is to look for *post hoc* reasons for the way in which the experiment or survey has been designed and carried out. A brief glance at the literature will reveal how ingenious academics can be in 'explaining away' failures to confirm hypotheses by these means (e.g. Argyris, 1968). Another way of accounting for the fact that results do not confirm hypotheses is to alter the hypotheses themselves. Since this, in a sense, amounts to the researcher admitting he or she was in 'error' it is seldom employed. Yet another approach is to show that some aspect of the particular experimental or survey procedure is responsible for the failure to confirm the stated hypotheses. This is done quite frequently, but typically within narrow limits, as Hall (1976) has shown so clearly. Finally, it is possible to admit that the traditional scientific model itself may need to be revised or altered to make it suitable for the real-life problem under investigation. In this article it is argued that this possibility has been insufficiently recognized and that there is a lot to be gained if, for some problems, management researchers deliberately adopt the procedures currently followed by most practitioners and consultants working in real-life organizations, instead of slavishly following the 'ideal' recommended by scientific texts.

In order to appreciate what is implied by this, it is necessary to indicate some of the important ways in which the 'model' followed by applied researchers differs from the 'ideal' which most academic researchers attempt to follow. First, the main concern of consultants and practitioners working in the field is with helping a particular organization solve an immediate problem, rather than with the development of law-like generalizations applicable to a variety of situations. It is something that concerns the organization that provides the reason for the research, and not a theoretical issue that puzzles the investigator. As a result, the research is necessarily relevant, if only to a particular organization. In both these respects applied research often differs from that conducted by academics following the traditional scientific model.

Secondly, the initial step in applied management research is to analyse the organizational context, especially its constraints and the opportunities it provides for doing the research in a way the investigator would like. Practitioners and consultants typically do not even *start* to design their research study until they have secured the full support of organizational leaders, and know that what they want to do is both feasible and regarded as potentially useful and important. Academic researchers, on the other hand, typically design their study so that their hypotheses can be tested as fully and completely as possible, before subsequently 'looking around' for the best place to conduct the experiment or survey. Only when they fail to find an ideal site, do they start compromising in what they want to do. Because it is unpleasant to compromise in this way, what often happens is that the scientific research ends up being done in a setting that is not really appropriate for the particular survey or experiment. As Kaplan (1964) and Argyris (1978) have shown so convincingly, it is the inappropriateness of settings that frequently lead to difficulties in interpreting the results of academic management research.

Third, throughout their research, practitioners and consultants are *forced* to take the complex reality of the world in which they are working into account. For one thing, they are constantly dealing with people from different parts of the organization who do not allow them to forget for whom they are working and what is expected from them. For another, their clients typically do not permit them to assign subjects randomly to experimental conditions or go through the various steps necessary for developing scientifically-validated survey instruments (e.g. Webb, Campbell, Schwartz & Secrest, 1966). Because experiments and (to a lesser extent) surveys provide the investigator with a large degree of control over what happens, this may be considered a disadvantage of applied research. However, there is another side to the coin: Both experiments and surveys achieve control at the cost of 'simplifying' what is being examined, so as to make it amenable to rigorous research, as Argyris (1968) and Harre & Second (1980) have shown. For our present purposes, what is important is the fact that people are *not* randomly assigned to experimental conditions in real-life and will be suspicious if this occurs. This feeling of suspicion is likely to affect their responses to the experimental conditions to a significant extent. Again, the trouble with standardized instruments used in surveys is that the questions predetermine how people can (and will) respond. As a result, subjects are prevented from providing their own interpretations of what things *mean* to them — which is what we need to know to understand their behaviour.

A fourth characteristic of applied management research is that it is usually conducted by persons who belong to the organization whose problem they are investigating. As a result, they typically develop a more intimate grasp of what is involved than can be provided by examining the results of experiments or the aggregate responses of individuals to surveys. In addition, they usually have more opportunity than their academic colleagues for taking repeated measures over a long period of time. This helps them to avoid the dangers of relying on cross-sectional data, dealing only with what happens at a single point in time — which is often the only kind of data available to academics.

In the fifth place, in applied research the concern is initially with a management problem that needs to be solved. It is hoped that the solution, or at least the principles involved, will have a wider applicability and perhaps some theoretical significance; it is a question, in a sense, of research generating theory. In management research based on the scientific model, however, it is *theory* which generates the particular piece of research. The important aim of such research is not to solve a particular problem (although it may do so), but rather to test some aspects of a theory in the hope of establishing its validity or refining or elaborating it further. A sixth difference between academic research and applied research in management concerns the flexibility of the procedures which are followed, and the criteria for judging results. In research guided by the 'scientific model' certain well-established procedures are automatically followed — once the research is under way the investigator lets it run its course, and remains as neutral or objective as possible. In contrast, in applied research, the 'procedures' are more like strategies that can be revised and altered, within certain limits, as the research proceeds, depending on whether they prove useful or not. In addition, the investigator realized that he is an integral part of what he or she is studying, and that there are times when it may be counter-productive to try to remain completely objective or neutral.

In the seventh place, largely as a consequence of the differences identified so far, management practitioners and consultants doing applied research are much more willing than their counterparts following the 'scientific model' to employ techniques which allow the investigator some scope for interpretation, such as depth interviewing, survey feedback, role playing, process consultation and team-building interviews (Schein, 1975; Bennis, 1966). Finally, applied researchers are typically concerned, not just with reporting their results to the scientific community, but also with selling them to the organization. In addition they are usually involved in helping to implement the 'solution' which they propose on the basis of their research results. Their job consists, not just of doing the research, but seeing that their results are used in a way that assists the organization (Bennis, 1966). Even if the results do not appear valuable in this way, the applied researcher looks for useful by-products or ideas for future management research from his major findings.

It is my contention that management research which follows this model frequently does more justice to the complexities of the real world than research conducted according to the 'ideal' described in the 'scientific model'. 'What is' research conducted by management practitioners and consultants may be messy and untidy, but this is because the world it seeks to describe possesses just these features. It is probably because of this fact that when we read accounts of such research we are immediately struck by its relevance and familiarity. What we read is *understandable* because it describes things as we know they are from our own experience. Unfortunately, this is not something that can be said for much of the management research conducted according to the 'ideal' model prescribed by those who insist that only one sort of management research can be 'scientific'.

It would be quite wrong to suppose that management researchers have not been aware of some of these difficulties. In fact, as recent events testify, they have made numerous attempts to improve the extent to which their scientific models can cope with realities of organizational life. For instance, ingenious attempts have been made to develop quasi-experimental designs that do not require random assignment to conditions (e.g. Campbell & Stanley, 1966); statistical techniques have been developed to deal with the restriction of range problem (e.g. Lord, 1969; Kenny, 1973); it is becoming increasingly acceptable to employ unstructured interviews along with experiments/surveys within single research efforts, as recommended nearly 40 years ago by Adorno, Frenkel-Brunswik, Levinson & Sanford (1950) in *The Authoritarian Personality*; and there has been a tendency of late to de-emphasize complex factorial designs for multiple base-line longitudinal studies that are better suited for applied research in single organizations (*cf.* Komacki, 1977; Bourdon, 1977). What these attempts have in common is the fact that they have occurred within the framework of the 'scientific model' advocated for management research. There have been adjustments and refinements, but the 'scientific model' is still insisted upon as the model which should be followed by researchers. As should be obvious from what has been said so far, it is my view that this insistence on a single definition of acceptable research has been detrimental to the progress of the discipline of management in a number of ways. Of these five stand out as potentially more serious than the rest.

In the first place, there has been a tendency to minimize the *positive* aspects of conducting research in actual organizations, engaged in the real-life struggle of survival or death. Secondly, the dominance of the 'scientific model' has resulted in management researchers being insufficiently concerned with questions of implementation, and the impact their research has on the people being investigated and the organization for which they work. Thirdly, it has led to many valuable opportunities for collaboration between applied researchers and academics being either lost or not taken up with sufficient vigour. Fourthly, because the scientific model is still regarded as the 'ideal' in almost all circumstances, the results of applied research have not been sufficiently incorporated into the mainstream of texts and journals that define what the discipline of management is all about. Fifthly, in the other direction, because they see theory-generated scientific research as increasingly unrealistic and irrelevant, management practitioners and consultants are increasingly prone to ignore its results when deciding what should be done to improve organizations in real life. Even when the research seems appropriate, they typically complain that the results are not spelt out in a way that they can use or sell to their clients.

Finally, because of its dominant role, the scientific model not only determines the kinds of problems that are investigated by management researchers, but also the *attitude* to be adopted to do research on those problems. Specifically, it encourages attention to be focussed on those problems which are most amenable to investigation according to the scientific model, at the expense of others which appear less amenable. The trouble is that these problems are often of less concern to organizations, than those that are less amenable to such investigation because of their 'messy' and 'untidy' nature. In addition, the dominant role of the scientific model encourages the attitude that applied research, because it typically fails to meet the criterion of vigour and precision demanded of 'true' scientific research, is simply not worth bothering about. As has been argued in this article, the reverse is more often the case. It is because applied research in management deals with the 'messy' and 'untidy' world of organizational life as it is instead of as it 'should be', that applied research frequently fails to meet the criteria laid down by the scientific model. Put more strongly, it is precisely *because* applied research does not satisfy the conditions of the scientific model that it frequently improves our understanding of what management research seeks to investigate.

In the light of this, it should be clear why refinements and modifications to the scientific model, of the kind we are seeing at the moment, will often prove insufficient. For many problems, as this article has attempted to show, what is needed is a different model; one based closely on that usually adopted by applied researchers. If this does not occur genuine progress is not going to be made in improving our understanding of what needs to be done to manage organizations more efficiently and effectively.

## References

Adorno, T.W., Frenkel-Brunswik, E., Levinson, D.J. & Sanford, R.N. 1950. *The authoritarian personality*. New York: Harper.

Argyris, C. 1968. The unintended consequences of rigorous research, *Psychol. Bull.*, vol. 70, 185 – 197.

Argyris, C. 1978. *Organization and innovation*. Homewood, Ill.: Dorsey Press.

Bennis. W. 1966. *Changing Organizations*. New York: McGraw-Hill.

Bourdon, R.D. 1977. A token economy application to management performance improvement. *J. Organ. Behav. Mgmt.*, vol. 1, 23 – 27.

Campbell, D.T. & Stanley, J.C. 1966. *Experimental and quasi-experimental designs for research*. Rand McNally.

Hall, E.T. 1976. *The hidden language*. New York: Doubleday.

Harre, R. & Second, P. 1980. *The explanation of behaviour*. London: Routledge and Kegan Paul.

Kaplan, A. 1964. *The conduct of inquiry*, San Francisco, Ca.: Chandler Publishing.

Kenny, D.A. 1973. Cross-legged and synchronous common factors in panel data. In A.S. Goldberger & O.D. Duncan (Eds.), *Structural equation models in the social sciences*. New York: Seminar Press, 153 – 169.

Komacki, J. 1977. Alternative evaluation strategies in work settings: Reversal and multiple baseline designs. *J. Organ. Behav. Mgmt.*, vol. 1, 53 – 77.

Lord, F.M. 1969. Statistical adjustments when comparing pre-existing groups. *Psychol. Bull.*, vol. 72, 336 – 337.

Orpen, C. 1983. The uses of industrial psychology. *J. Mgmt.*, vol. 12, 25 – 36.

Schein, E. 1975. *Process consultation: Its role in organization development*. Reading, Mass.: Addison-Wesley.

Schemerhorn, J. 1984. *Managing for productivity*. New York, Wiley.

Stoner, A.J.F. 1982. *Management*. Englewood Cliffs, N.J.: Prentice-Hall.

Webb, E.J., Campbell, D.T., Schwartz, R.D. & Secrest, L. 1966. *Unobtrusive measures: Nonreactive research in the social sciences*. Chicago, Ill.: Rand-McNally.