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978-0-521-37848-2 - Putting Social Science to Work: The Ground between Theory and Use Explored through Case Studies in Organisations

Lisl Klein and Ken Eason

Excerpt

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# 1 Introduction

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This book has the difficult aim of bringing together systematic research and personal experience and reflection. The systematic aspect is represented by a collection of case studies. The aim of this empirical research was to examine the process by which social science is applied within organisations. We focussed on the utilisation process which occurs when one or more professional social scientists work directly with an organisation. The sample of cases is based on the institutions from which work was being carried out: in-house units, commercial consultants, academics doing applied work and a non-profit research institute. In the course of our work we also came across other approaches to social science utilisation, which were not in the form of specific cases or work with organisations. We present these (five practitioners and a national programme), but they are not included in the main body of the analysis.

Indeed, we did not want to be limited to the systematic analysis of the case material; to try to be systematic is necessary but not sufficient. We also want to discuss what we ourselves have learned from experience in the field. We hope that these two strands will fuse, for that is what professional practice is about. With some roots in systematic analysis and some demands on imagination, it is itself not quite an art and not quite a science. We think it may be akin to a craft. There is, however, little in the way of a craft tradition. The hard work of developing and honing and reflecting on methods is dispersed among small pockets of people, and even more dispersed are those who are in a position to give feedback on what is of greater or lesser value.

The practitioner of social science – we use the term deliberately, though at this stage with some apprehension – needs to achieve some synthesis between the knowledge that comes from research and the understanding that comes from experience and introspection. That requires, on the one hand, dispassionate curiosity, respect for rigour and a readiness to have one's favourite prejudices proved wrong and, on the other hand, empathy, imagination and the freedom to stray. It is a list of requirements which will explain at once why achievement will, in some sense, always fall short of expectation.

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The idea of professional practice in social science may well raise some hackles and some cries of 'élitism', so that is an issue we need to confront early: in relation to some professions, such as medicine or the law, non-professional clients are deemed not to be able to do the things the professionals do by virtue of the knowledge they do not have or by virtue of traditional property rights to that knowledge. In some areas these boundaries are beginning to be challenged, but there are, in any case, other fields where the problem simply does not exist. Gardening and cooking also have their professional practitioners, but that does not prevent people from doing it themselves. Both have extensive scientific bases, knowledge of which can prevent mistakes and enhance results, while mere reliance on the scientific knowledge can stultify. In both, some people benefit from professional help and some people do well without it. It does not seem to be such a big deal. The pleasure of it, even for the most experienced professional, is that there is always more to learn.

One theme that recurs in the book is the pervasive importance of funding structures, since he that pays the piper develops pipers. Thus research funding by the Anglo-German Foundation for the Study of Industrial Society brought with it a requirement to do parallel work in the two countries and two German colleagues, Edith Rost-Schaude and Rolf Kunstek of the Forschungsinstitut für Arbeit und Bildung in Heidelberg, collected a number of case studies in Germany. Also Klein, one of the UK authors, has experience of working in Germany in this field and reports on the work of the Commission for Economic and Social Change. There is therefore an Anglo-German thread in this work.

International comparison, however, is not an aspect about which we intend to be systematic. That would require a different and larger research frame and was not the primary aim of the work. Also, at the level of the specific projects in organisations there is no special reason to compare the German cases as such with the UK cases as such. The sample of cases is too small in either country for us to be able to say anything significant about differences in the amount or distribution of such attempts, or about country-based differences in the content of the work done. Some of the cases feature subsidiaries of multinational companies, and may well owe more to company culture than to the fact that they happened in Germany or the United Kingdom.

In any case, the primary aim of the research has been to learn about the processes and problems of social science utilisation. At this level of data collection, therefore, we are simply using the opportunity of the Anglo-German collaboration to obtain more cases and learn from them rather than to set up a systematic comparison. Each of the teams is producing its own accounts, with freedom to draw on the other's material. In our case, we include the German material in the cross-case analysis contained in

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chapters 10 and 11, but include only very brief descriptions of some of the German cases. Elsewhere, we refer to German experience where it seems that contrast or comparison contributes to a wider understanding of the general topic.

The book is in three parts:

Part I, 'Issues in social science utilisation'. These chapters (chapters 2–4) describe the issues as we saw them before we considered the empirical material;

Part II, 'An empirical study of social science utilisation'. In this part (chapters 5–13) we first describe the historical context of the application of social science, in the United Kingdom and in Germany (chapter 5). Next come brief accounts of the cases in organisation, seven UK and seven German (chapters 6–9). Then come two chapters of cross-case analysis (chapters 10–11). The final part presents some additional case material which is not about organisations. It goes inwards to the individual practitioners (chapter 12) and outwards to a governmental strategy (chapter 13).

Part III, Discussion. In this part (chapters 14–15) we discuss what contributes to successful practice.

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## **Part I    Issues in social science utilisation**

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## 2 Issues of content: the use of knowledge and the dynamics of action

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We said in the preface that application in the social sciences is beset with problems. From the conceptual point of view a major problem is that there are two distinct frames of reference, which the professional needs somehow to integrate. The first is the framework of taking an 'item' of knowledge or a research finding and translating it into use. The second, which criss-crosses and intertwines with the first, is a framework about the dynamics of action. In this chapter we discuss some of the issues raised by these two frameworks.

### Knowledge-into-use

#### *The validity and replicability of research findings*

The first relevant question concerns the validity of research findings in the social sciences: how 'valid' does a finding have to be before it is legitimate (in the eyes of social scientists) to make use of it, or before it is found useful in practice, which is not the same thing.

From the social science world come ideas, with varying degrees of verification. Maslow's hierarchy-of-needs concept, Herzberg's two-factor theory of motivation, are examples of ideas which in parts of the social science community are regarded as inadequately verified,<sup>1</sup> while in parts of the user community they are seen as useful products of social science. There are probably no findings so respectable as to be completely unchallenged.<sup>2</sup>

<sup>1</sup> See, for instance, Blackler and Williams (1971, pp. 283–303).

<sup>2</sup> The problem of the assessment of quality in science is very usefully discussed by Ravetz (1971, p. 275): "To give a rough idea of the sorts of assessments that are made, I can distinguish several classes of scientific work, and the standards by which they are defined. The best of all scientific work is that which survives, through all the many testings and transformations, to become genuine scientific knowledge. But this can be known only in retrospect; and so a sober assessment of any new result will not place it in the "immortal" class. However, one can reasonably predict that any given result is likely to yield "enduring facts" which will survive the demise of the original problem: if that problem was a deep and difficult one, and the result is capable of development and extension, then it is entitled to be considered as first class. Now, even a temporary fact is no mean achievement, for its

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This absence of consensus about what are valid findings is partly due to the essential incompleteness of all knowledge. It is also partly due to the politics and career structure of academic life: to some extent the identity and worth of one's own work or that of one's team or school are established by challenging the work of others; and this may not necessarily be done by replicating the work, but by using different methods, which are in any case likely to lead to different findings.

There is, however, another consideration which is more intrinsic to the subject-matter: the distinction between an idea and a valid finding may be in principle less fundamental in the social sciences than in the natural sciences because of the greater influence of differences in context. Even in the same historical period different ideas are put forward simultaneously which are not apparently compatible but concerning which one can find evidence if one looks carefully enough. Even more, a different geographical or historical context will affect the evidence on the same idea. Differences in context mean that some investigations are not repeatable, or not repeatable in the same form. Differences in context and time possibly constitute the greatest problem in establishing any fundamental parallel between the social and natural sciences.

This does not imply a total relativism. There are underlying generalities, over which different concrete manifestations cluster. In the field which is generally known as behaviour in organisations, three such underlying generalities, having more than temporary validity, are:

- (i) The structural characteristics implied by technology, markets, laws, size, ownership and control systems will have some behavioural and organisational consequences.
- (ii) Within narrower system boundaries, the structural characteristics of tasks and work-roles have some psychological consequences for the people doing them; in turn, people will influence the structure of their environment to some extent. The technical and social systems are interdependent.
- (iii) Process is relevant as well as content. This implies, firstly, that how things are done, for example, how changes are introduced, will be relevant to outcomes as well as the content of what is done; and it implies, secondly, that groups or organisations can learn to review themselves and modify how they operate.

existence shows that the solution to the original problem had depth rather greater than the explicit statement of the conclusion would guarantee; work which achieves this success is certainly good. Even this grade is not the minimum quality necessary for a result to be worthwhile in the advancement of its field; so long as it meets the appropriate standards of adequacy, and shows its value by being put to use by others, however briefly, in subsequent work, the research which produced it can be considered competent.'

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When it comes to application, context again makes replication and comparison difficult. Whether an organisation is the first pioneer to apply a new concept, or the twentieth follower, or whether it is compelled into application because this is decreed by policy or law, is dynamically different and will lead to different outcomes even though the content is the same. On the one hand, the enthusiasm with which something is being introduced may be the most influential factor; on the other hand, sophisticated subjects may become immune to the Hawthorne effect, and some 'change strategies' may not be effective when tried repeatedly.

***The usability of findings***

Another set of questions surrounds the usability of findings or concepts, even the most respectable ones. It is one of the most dysfunctional consequences of neglect of the development part of the R and D spectrum that the relevance, and therefore usefulness, of even highly valued findings or concepts may not be apparent. An alternative explanation could be that an intervening step is generally omitted. This is the vital step of internalising-and-converting-into-use, which is an act of imagination.

For example, a proposed study was being introduced to the division of anaesthesia in a large general hospital (this study is referred to again in chapter 3). The members of the division explained their policies: they serviced the general operating list, and also provided a service to the departments of obstetrics and gynaecology, dentistry and psychiatry. But their absolute priority was the general operating list – 'If you are scheduled to have an operation you'll have your operation, no matter how short-staffed we are.' The researcher asked, 'Does that mean that I might come across an obstetrician who says, "You can never damn-well find an anaesthetist when you need them"?' With dawning realisation one of the consultants looked up from his coffee and said, 'Good Lord! Yes, I suppose you might.' The concept being applied was a fairly obvious instance of structural perspective. The insight generated by its internalised use was instrumental in getting agreement for the study.

The application of a general finding to particular circumstances itself requires a quality of understanding both of the finding and of the situation. There is a danger that findings may be thought inappropriately to apply in situations which do not match the ones which produced the original research. It is only too easy to draw false conclusions, or to draw correct conclusions from the research and misread the situation itself. On the one hand, therefore, one can say with confidence that our understanding of the functioning of individuals and organisations has increased very greatly during the last sixty years; on the other hand, nobody could claim that the social sciences can supply from stock easy answers to complex problems.

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Internalising-and-converting-into-use modifies a finding, or may at least make it look different.

In the absence of general attention from the social science profession to this process of internalising-and-converting-into-use, individual practitioners find themselves doing it privately, in those areas of knowledge with which they are most familiar. In other words, they work out for themselves how to make theory and findings operational. As an example, research on the behavioural consequences of control systems (Woodward, 1970) turns out to be most useful in understanding situations during the diagnostic phase of applied research, and in predicting the likely outcomes of alternative organisational design decisions. This is part of a contingency framework, which is concerned with how the environment affects behaviour choices, and therefore involves looking at organisations in terms of the interplay between behavioural and structural factors. It is not only substantively useful in predicting the behavioural consequences of aspects of structure (for example, technology, markets, and so on), and therefore as an aid to organisation design; it also has the function of demonstrating a scientific – in the natural science sense – way of operating with social and organisational phenomena, and therefore of establishing credibility. Even on the basis of a simple example, like the different behavioural consequences according to whether maintenance is costed with production or is a separate cost centre, people in industry can without much difficulty be helped to enter into this framework, and then to continue to work with it themselves.

Another example of the usefulness of the contingency framework arises from experience of the recent wave of interest in 'job enrichment', job design, and so on. In recent years a number of organisations have tried to decide whether to embark on such an exercise. It helps to understand that, where an organisation is in a rapidly changing market (for example, office equipment), which demands bigger product ranges, shorter product lifetimes and frequent modifications, a flexible mode of production, which also involves more highly skilled and flexible people, is congruent with the needs of the market and increases the organisation's ability to absorb variances (Butera, 1975). Where the market does not make such demands (for example, banking), one may still decide to change work organisation in the direction of greater skill, or greater flexibility, or greater autonomy for work people (or all three). But this will be a decision on ideological grounds rather than functional ones. It will therefore require much greater commitment and consensus, the differences of view between different interest groups are likely to have greater salience, and it may not survive difficulties in implementation. It is a quite different kind of decision.



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A further important question about this process of conversion from theory or research results to use is how near to the research base it takes place. Practitioners are themselves users of resources, i.e., of the researches of others. They are likely to experience difficulties similar to those of administrators in internalising the researches of others sufficiently both to be able to make the conversion and to recognise features in the context which will indicate appropriateness. Problems arise when the paths of continuing to develop and modify an idea within a research framework and developing it into use diverge, as at some stage they must if there is to be diffusion. The quality of understanding, knowledge about the limitations of the finding, and awareness of the context within which it was developed, all of which were present during the research, may not accompany the idea into use, especially use by others. This is one of the areas where use implies special training needs.

***Findings as products***

It is the above-mentioned divergence between the paths of, on the one hand, continuing to develop an idea in a research framework and, on the other, applying it, that underlies the anxiety of many social scientists about the use of 'packages'. There are by now a number of instances of strategies, developed in response to a particular situation, which have been routinised and formalised so that they should be capable of being applied in other situations and on a large scale. Insofar as they involve diagnostic methodologies, they may permit enough input from the local situation to prevent inappropriate solutions; but insofar as they make assumptions about knowledge which the social science community regards as inadequately tested and omit diagnosis of the local situation, they probably exemplify the widest part of the gulf that has arisen between research and consultancy. The distaste experienced by researchers is probably not uninfluenced by the recognition, success and financial rewards which have accrued to some of the authors of such packages, nor by the difficulty they themselves experience in letting go of ownership of a problem area. There is a big dilemma for the social scientist who wants both to see the diffusion of ideas and knowledge and also to exercise methodological caution. The dilemma, and the anxieties that go with it, become clear when one thinks of the phases of the product growth cycle which have been distinguished with regard to the products of manufacturing industry: the 'R and D phase', characterised by high skill and specialised methods in developing a prototype; the 'growth phase', when R and D appears to have succeeded and imitators begin to appear; and the 'mature phase', when the

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product is established through mass-production methods involving lower-level skills. An important dilemma for social science is whether it should – and wants to – aim to create ‘products’ analogous to those of engineering science.

The emergence of some products of social science in commercialised form has probably also reinforced the tendency among some people to see applied social science as a succession of passing fads. The research question here would be whether such fads leave behind some permanent residue – in other words, whether what is learned from a passing fashion becomes institutionalised. There are signs that this does in fact happen.

***Research as an action strategy***

Research is not only a strategy for developing knowledge which may be useful, it is also a strategy for investigating problems in the field directly. Often this is what is in fact meant by applied work.

Van de Vall and his colleagues made a study of this kind of work by examining 120 ‘applied social research’ projects in The Netherlands, 40 of them in industrial and labour relations (Vall et al., 1976, pp. 158–77). They did this by studying the research report and interviewing the social researcher and the policy-maker involved in each project. Interviews with the latter led to five indicators of policy impact and an ‘overall policy impact’ score for each project. They then related various characteristics of the projects to their ‘policy impact’.

The analysis shows that, even within the single framework of research, there are important differences in approach. For policy impact the analysis favours qualitative rather than quantitative methods; familiarity with the organisation’s decision-making processes; and grounded concepts of low abstraction and simple construction rather than formal ones. The following are their main conclusions; they find:

a widening chasm between the values and goals of academic social science and policy-oriented social research. A fundamental difference is that applied social research is guided by the dual norm of epistemological and operational validity. We found that external researchers had more difficulty in meeting this dual standard than did internal researchers.

A second difference relates to the theoretical level of the research results. While academic social scientists are primarily nomothetically oriented, searching for the highest level of abstraction, applied social researchers, seeking to solve a specific social problem, tend towards an idiographic orientation. We found a specific type of theoretical concept – grounded, continuous, and of low abstraction – to be best suited for social problem solving.

A third discrepancy is between the goals of academic social science and social policy making. When confronted with a conflict between method and