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The problem of 'methods'

A challenging look at research methodology is necessary because of the confusion of terminology that has come about by the accretion over the last century of research methods and the vocabulary for describing them.

Probably the most ambiguous term of all is the term 'methods' itself. Many different meanings of this word can be found within the literature. Indeed, one text book that has a title 'Research Methods in Psychology' may be totally different in its coverage from another book. The term 'methods' can be used to describe statistical manipulations. By contrast it can be seen as an exploration of the issues of research planning and design. Yet another usage is to describe the instruments that are utilised to collect the actual data that is at the heart of the study. There are yet other usages that put the term 'method' into a more ideological framework by contrasting phenomenological methods, for example, with positivist ones. Many other different usages can be found in the literature, but even these few serve to illustrate that there is no clear framework as to what really is the appropriate focus to any discussion of research procedures.

Research questions

At the heart of research is the answering of questions. This rather looser form seems to me to be fruitful, instead of 'the testing of hypotheses', because the hypothetico-deductive approach assumes a very particular framework for scientific investigations. In a situation in which the research study may be aimed at formulating policy guidelines, or for example disentangling the particular constituents of a therapeutic activity or clarifying the most useful ways of combatting memory deficits, there will usually not be any formal hypotheses in the conventional scientific sense. What hypotheses was Darwin setting out to test when he went on his voyage on 'The Beagle'? If he was just exploring the varieties of

animals around a loosely formulated notion of speciation is it right to dismiss his work as 'unscientific'?

If research is about answering questions, methodologies need to be considered in terms of how appropriate they are for answering a particular question. To understand the appropriateness of a methodology, it is useful to recognise the many different types of research questions that can be asked.

Five broad types of research questions can be distinguished.

Causal explanations

These are research questions of the classical form in which distinct influences on particular outcomes are to be identified. A lot of therapeutic research is formulated in these terms, on the lines of 'does a particular therapy really produce a change in the patient'. Psychophysiological studies are also typically of this form, whereby the effects are examined of particular mechanisms on behaviour. However, such formulations often come unstuck when the complexity of the ongoing situation is explored, for example the ways in which alcohol interacts with cultural expectations to produce its effects. As a consequence, the strongest causal questions would appear to come from highly controlled, laboratory based, biologically oriented studies in which the impact of particular organic or pharmacological states are being examined.

Probative influences

The term probative is stolen from the legal framework because this describes studies in which the specific causal mechanisms are not open to study but some indication of the directions of influence can be disentangled. This is the type of research question that is typically studied using 'field experiments'. Changes are introduced into some component of an ongoing system and their effects monitored. This is a model that is frequently upheld as appropriate for studying the effects of therapy, environmental design, or changes in training and other organisational practices. However, the amount of control that it is necessary to introduce into a field situation - in order to get clear indications of the broad influences of a particular type of intervention - are usually so demanding that they are likely to distort the organisations to the extent that the results may not be generalisable. As a consequence practical constraints tend to make extensive field experiments very rare and those that are done tend to be small scale or superficial, with a few noteworthy exceptions.

Relational studies

Here we have questions about the patterns of relationships between variables. Surveys of the relationship between IQ and school achievements, stimulus complexity and preference, or what sorts of patients seem to be benefiting from what sorts of treatment would fall into this category. Curiously this framework for

research is often despised by people with an experimental frame of mind who are looking for direct causal links, yet it is by far the most common approach especially for applied research. I think the reason for its common use is that not only is it essentially cost effective, but the strategy can also establish relationships on which predictions can be based. Predicting which group of patients are most likely to benefit from what sort of treatment or what level of IQ is appropriate for university education, is often of more immediate utility than knowing why such effects may actually be occurring.

Descriptive studies

When I was taught research methods as an undergraduate, the word description was used as a term of insult commonly associated with the term 'merely'. However, anybody who has studied anthropology, the law or many areas of management science, or of course medicine and large areas of biology, will realise that giving a clear and coherent description is challenging and often crucial. To know exactly what is being delivered in a particular service, the sorts of people who are making use of it and what they are actually experiencing may be the key to understanding how that service can be improved or what its general values are. Just as a full description of a habitat can help us to understand how animals may survive within it, or the identification of memorising strategies can help in establishing how people cope with reduced memory capability.

Consultancy (action research)

The first four questions are truly research questions in the sense that they can be answered independently of any detailed understanding of what is likely to be done with the results. However, there are many important research challenges in which the research is being conducted precisely to change the organisation, or individuals on whom the studies are being conducted. I prefer to call this 'consultancy' because it implies a very different mode of relationship to the research activity than 'action research', which still implies the researcher has some distance from the organisation. The crucial point about consultancy is that it must be carried out in a way that enables the organisation to act on the results of the work. It therefore needs to take into account the way in which the organisation does indeed act. Consultancy is always a part of the decision process and therefore has a quite different relationship to the topics of study from the other types of research activity. In effect, the central question of consultancy is 'how can we enable the people who we are advising to change for the better?'

My proposal is that all five types of research question imply different types of research framework, which might be called research strategies. I use the term strategy very deliberately taking it from the military metaphor in which the strategy is the overall framework for describing how the war is to be conducted. It covers the overall planning and logistics of the research activities, embracing what is often thought of as research design, but really going beyond that to cover all the related

issues of how the research is to be approached. This will take account of the major limitations on research which are carefully hidden from undergraduates. The published literature always describes research as if it happens in an unconstrained universe. The limits of time, money, the particular skills and resources available, that are all necessary to conduct the research are never ever indicated. The whole operation is described as if it were still being carried out by nineteenth century gentlemen of private means who could do whatever was necessary to achieve their particular objective. However, an appropriate research strategy would take very real account of the cost effectiveness of different approaches to answering the research questions as well, of course, as establishing what is the appropriate set of research design procedures for answering the particular question.

Another important point about recognising that each type of research question has its own appropriate form of strategy is that it also recognises that there is not one pure form of research design - the laboratory, controlled experiment - compared with which all other research designs are more primitive and less effective. The general discussion of good and bad research that puts so much emphasis on control groups and experimental groups, dependent and independent variables, tests of the null hypothesis and so on, are, within the framework I am outlining, only one type of research design and a very limited one, really only appropriate to experimental studies of causality.

Once it is accepted that there are other questions that it is legitimate to answer, beyond focused questions about cause and effect, then it can be seen that each research strategy has its own rules and procedures. There are good and bad ways of doing descriptive case studies, or surveys of relationships, or field experiments, just as there are good and bad ways of doing controlled, laboratory based experiments. What researchers need to understand are the different criteria that are relevant for evaluating each of the different strategies. Not the narrow view that all studies are some form of experimental test.

This broadening of the consideration of the criteria for evaluating research also broadens the range of psychological theories that can be developed. The experimental study of causality really assumes more or less mechanical models of cause and effect. However, if it is accepted that we are often trying to build up an account of interrelated systems of influence, then a research strategy that allows us to identify what the major constituents of the system are, is going to be more productive. The experiment has the distinct limitations of only testing influences at a few levels and usually only on a few output, dependent variables. Furthermore, we often want to know how a system operates in its natural setting. It is, therefore, necessary to be sure that the research procedure does take account of naturally occurring psychological and social processes.

As well as the different criteria, that can be brought into to play for evaluating different research strategies, there is also a possibility for identifying more clearly the skills that are needed for answering different research questions. The most important and obvious example of this is that the skills associated with consultancy, in which some change is desired in an organisation, clearly relate to persuasion and communication, not solely to computer literacy or ability with arcane statistics. A

commanding presence may be the difference between a piece of consultancy having some influence on an organisation and that same advice falling on stony ground.

All five types of research question interrelate and therefore all the different strategies need to be drawn upon in different circumstances. There may even be an evolution of stages in the explanatory process that is relevant to formulating large research programmes. But until these issues are carefully and fully debated within the framework of applied psychology methodology, we will continue to have a lot of neat experiments published in our journals by a few academic researchers, but which have very little impact on policy and practice. By contrast there will continue to be a whole range of rather poorly conducted pieces of action research that are taken notice of by health authorities, government departments, commercial organisations and newspapers, but which embarrass academic psychologists . 'We didn't have much time and just did what we could' is usually taken as an excuse or defence for work that the psychologists believe is not as neat and tidy as the paradigm models of controlled experiment that they think they must be aiming for. My argument is that these limitations should always be understood within the research framework and that studies should be conducted in full knowledge of what is the most effective way of answering the question *within* the resources available.

Research tactics

Just as the *strategies* for research imply a formulation of the types of psychological theory that are appropriate to develop, so research *tactics* carry implications for the models of human beings that it is assumed legitimate to formulate.

Putting it at its most extreme, studies that use instruments that observe the individual at a distance, or test some performance that the person can produce under specific conditions, are assuming models of humanity that has people as essentially mechanical objects that can be understood from 'the outside'. By contrast, any procedure that involves a direct questioning and interaction with a respondent, not only assumes that the individual has a special perspective on his or her own experiences but also that individual is able to understand the framework that the researcher is bringing to the situation. The respondent, furthermore, must understand the questions well enough to be able to answer appropriately. It therefore follows that at the one extreme we are likely to have theories of behaviour that are biological in orientation and look for processes that are beyond the individual's control or awareness, whereas at the other extreme, we have theories that give pride of place to the interpretation and understanding of the person who participates in the research. There are, of course, gradations in between these two extremes, but they all carry with them implicit assumptions about the nature of people. Research tactics are no more neutral about the types of theories to which they pay homage than are research strategies.

It therefore is no surprise to find that the research strategies which are essentially mechanical and simply causal in the types of questions they attempt to answer, also tend to draw upon research tactics that equally look for direct and relatively

straightforward causal mechanisms within the individual. By contrast the strategies that look for the complex interrelationships between existing systems of experience are likely to use tactics that put the subject of research into the position of expert on the process being studied. Of course, there are many hybrid procedures and there are many attempts to break through these different barriers, but I believe that it is only from an understanding of the theoretical assumptions that are implicit within strategies and tactics that really innovatory procedures and theories will be able to evolve.

Appropriate use of resources

In order to understand more fully the ways in which research strategies and tactics can be harnessed to the resolution of the many problems faced by psychologists doing research, it is useful to consider more directly some of the constraints and possibilities that are prevalent in most areas of psychological research.

I think it is productive to characterise a fruitful approach here as one of 'organic data collection'. What I have in mind here is the notion of understanding fully the context within which the data is being collected and drawing upon that context in a clear-sighted way, rather than planning research in relation to pure and abstract notions of elegant design.

There are many issues in developing organic research projects, but three points are illustrative of the general approach that I believe is worth considering.

The first is the most obvious organic notion of developing research in a way that makes it natural to its context. Setting up special procedures that can only be operational under particular experimental conditions with specialist resources needs to be very carefully evaluated. In contrast, taking advantage of circumstances that already exist will often have greater potential. This means for example that physiological studies using extensive equipment may be more appropriate within hospitals that have such equipment regularly in use, but is less appropriate when trying to study people in their own homes or to take another example, getting coal miners to complete a lengthy questionnaire at work may prove far more artificial than asking civil servants in a tax office who are used to filling in forms.

A second aspect of the 'organic' approach is to think of the possibilities of recycling existing materials; reusing old data. Most organisations are now buried under large amounts of records and other summaries of activities and experiences. Furthermore, there exists large amounts of data that has never been either fully analysed or reanalysed in the light of new understandings of psychological processes, and ways of studying those processes. It might even be suggested that there ought to be a moratorium on all new data collection until we have really made proper use of all the data we have already collected! But even the possibility of this as an approach to research has implications for research strategies and tactics, because it pushes the research in the direction of description and understanding and away from the study of precise causal mechanisms. One spin off of this way of thinking about research data is that the records that are kept can be developed in a

more systematic way so that they are of more utility to the organisation as well as being of value to the research process itself.

A final aspect of organic research, that may seem a little paradoxical, is the proposal to avoid 'operational' definitions of what is being studied. By defining what is being studied in terms of the procedures that are used to study the phenomena we are quite directly separating the issues being studied from their context. Issues being studied need to be defined in terms that relate to the context and then procedures found which are valid and reliable ways of exploring the issues that have been identified as being relevant. Perhaps the classic example here is the study of intelligence. As long as it was defined in relation to abstract measurements from IQ tests theoretical progress was slow and application dogged with confusion. It was only by looking at what the intellectual demands were on people in a variety of situations that the study of intelligence took a leap forward.

This point, of putting emphasis on the definition of what is being studied, is a plea for a much closer link between the theoretical formulations of research and the ways in which measurements are being made. This emphasis on the closer link between the theoretical formulations and the procedures also, perhaps again paradoxically, makes it much more possible for the research to have an impact. An understanding of the ways in which the conceptualisation of the research problems are the basis for the effectiveness and utility of the research, can best be gleaned by considering the research process and how it integrates with decision making. It is to this linking of research and action that I shall now turn.

Linking research to action

If we think of research as having three broad stages, namely

1.   the formulation of a research problem
2.   the collecting of data and information to help answer that problem
3.   the results and conclusions of research

then it can be seen that the most usual model for the application of research findings is one in which the results and conclusions are fed to which ever group may be interested in those results. However, if it is recognised that decision makers who must act on research also go through a sequential process then a more integrated link becomes feasible.
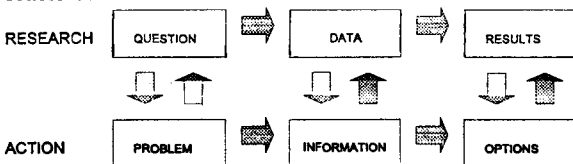
Integrating research and action

Decision makers must also identify the task that they have to deal with. They must further collect appropriate information to help deal with that task. Finally, they must make decisions about the most appropriate ways of dealing with the task given the information that they have.

In the simple model, then, the conclusions are fed in to the decision choice phase, or may possibly be part of the information that is drawn upon. So, for example, knowing that certain classes of phobic patients may benefit from a typical type of intervention may be a research conclusion that is drawn upon when service provisions are being considered.

However, psychological research shows us over and over again that people absorb into their conceptual systems those constructs that connect with their existing framework of understanding. It therefore follows that the research questions themselves should be formulated in ways that connect directly with the salient conceptualisations with which the decision makers have to deal. This will then allow a much closer integration between the research and the action.

Similarly, the data that researchers collect may or may not have links to the sorts of information that the decision makers feel appropriate. For example, where the researcher may wish to have very precise measurements about the detailed impact on behaviour of particular forms of treatment, the policy makers may only be interested in the number of people who make use of that treatment and how much it costs to deliver.

One consequence of these considerations is that the formulations of research questions and their mode of test needs to be open to a variety of presentations and to allow different emphases to be drawn out for different purposes.

In relation to the strategies and tactics discussed above the implications here are that descriptive material that provides a coherent account of what is happening in a situation may often have more utility then a more precisely controlled experimental study. Furthermore, the possibility of illustrating the conclusions with graphic examples cannot be underestimated. Policy makers who need to take information from a variety of disciplines, who will not be specialists in any one particular discipline, will nonetheless make judgements based on their own understanding of the material presented to them. Graphic examples will often enable them to understand the implications of any conclusions, even though those examples cannot be taken as strong empirical evidence within the strict framework of scientific hypothesis testing.

It is worth emphasising that it has been well established, in the realms of expert testimony in the courts, that visual summaries of research findings often carry far more weight with non expert audiences than do numerical, and especially statistical, summaries of those results. As psychologists we all therefore need to become more adept at representing our ideas visually if we are to get our message across.

The increasingly available family of multi-dimensional scaling techniques may offer a new range of ways of summarising research material that allows decision makers to conceptualise the results in a form that is salient to them, while still staying close to the original empirical results. These visual summaries of relationships within sets of data, though, often demand a broader range of

explanatory systems than the cause and effect explanations that might be typical of studies modelled on laboratory experiments.

Problem definition

In order to embrace effective research activities that will connect with policy making, it is helpful to consider more closely the three constituents of effective integration. The first is the issue of how the research question should be defined.

The traditional approach is to define problems solely in terms of the existing literature and the hypotheses that might be derived from it. But in order to contribute to the activities of policy making it is fruitful to look at other issues that may limit and give shape to the definition of any research question.

1. Where can solutions be found?
    Most organisations have only the possibilities of acting in a relatively small solution space. It is, therefore, necessary to have some idea of the range of solutions to problems that may be feasible in order to draw on those to shape the questions that may be asked.
2. Are there other ways of considering the problems?
    Often the problems posed by an organisation or by the research literature can be regarded in many different ways. A flexibility of attitude towards what is the real nature of the problem can therefore be very productive. For example, rather than looking at the successes of different therapies it may be more feasible to look at the mechanisms by which patients get assigned to different therapists. It may not be possible to change therapists' approaches to the therapy that they provide but it may be possible to assign patients to different therapists.
3. What are the limits within which the solution(s) must be established?
    This deals with the question of whether there are implicit restraints on costs or time scales that are crucial to solving the problem.
4. Acceptable forms of explanation?
    Some forms of explanation of the phenomena under study may be acceptable within an organisation, for example, those relating to the physical layout or the use of resources, but others may be quite unacceptable, even if scientifically more valid, such as incompetent management or inappropriate training. However, changes in layout or location may require new management procedures that are acceptable and bring in a discussion on training.

There is a central difficulty that comes out of these considerations. Significant psychological research usually implies organisational change. It implies comments on *how* things are done not on *what* is done. But organisations typically prefer options that require them to continue doing things the way they always have. This means that advocacy and indeed rhetoric are important skills in summarising

research and its strengths.

Appropriate information

One of the key ways in which the results of research can be advocated is around the information that is presented to support the work. I have already mentioned the value of strong visual summaries and powerful images and have indicated the implication this has for doing at least some qualitative research that will generate good, memorable examples. But there are other aspects of the way the information is presented that are worth consideration.

One of the strong components of the information is the fact that it can be presented as relating to very special preferably unique, 'expertise'. Decision makers will often regard themselves as capable and well informed. They need to be convinced that some extra and additional forms of 'expertness' are being presented to them if they are to take note of it.

However, this expertness cannot be seen as too esoteric or unavailable to direct understanding, otherwise not only is it psychologically threatening to many people who must act on the information, but there will often be some real doubt as to its veracity. The material must therefore be presented as having some degree of face validity. As psychologists know, what seems obvious to a recipient of a piece of information depends to a large extent on the context within which that information is presented. Research findings should, therefore, make a connection with what is seen to be obvious so that its impact can be absorbed.

However, this obviousness should push understanding beyond 'what we know already'. Often there will be a few key findings and a few key illustrations that are a little surprising, although quite understandable in the context presented. It is this balance that enables the policy maker to feel that the research really has moved forward but not too far to be threatening.

Evaluating outcome

The other set of considerations that need to be part of the whole research programme are those relating to the way in which the research itself will be evaluated and especially the results of the research. The criteria do also relate directly to the evaluation of the outcome of any other process such as the delivery of a particular service or a particular type of treatment.

Salience of criteria

As I have mentioned the ways of thinking about the problem, and the room for manoeuvre that is considered to be available, will be brought to bear when looking at the criteria that have been used to reach any conclusions. These criteria therefore have to be carefully evaluated to ensure that they are appropriately salient to the people who will make the decisions. Average life expectancy of patients who have

gone through major surgery is often not considered nearly so important, for example, as the proportion of them who live for one or two years after the operation. The number of people who complete a course of training may be more important than the grade they obtain on completion.

Risks of failure

Another important way of evaluating the conclusions of research is to consider what the risks would be that are associated with any failures that result from implementing the research. Most people are cautious in their actions especially if they are in senior civil service positions. The main things that they want to avoid is failure. Successes carry less weight in terms of their future progress through the administrative hierarchy than having particular failures on their career reviews. This essential conservatism is therefore brought to play when evaluating whether major new systems should be implemented.

Need for 'structural' change

Once again it is worth pointing out that organisations, like people, are very reluctant to make major changes in their internal structures, how they think about things or how they do things. Proposals that lead to suggestions that they should do more or less of the same are therefore much easier for them to work with than radical recommendations of entirely new systems of working.

Inherent difficulties of psychological research

Some difficulties of doing psychological research go beyond the practical demands of research within organisations, especially when that research studies the activities and experiences of individuals. There are other problems that are inherent to the whole process of applying psychology. Four of these are worth considering in any research activity.

Individuals count

One of the central demands of much person-oriented psychological activity is to form a judgement about what is appropriate for a particular individual. Being able to demonstrate that there are broad differences on average between different groups of individuals is less valuable than knowing what to do about a specific person. Research procedures, therefore, need to be able to give findings precise enough that they can be linked to a particular individual. Often this means having a strong theoretical formulation that can be applied to the particular person, rather than group averages. It is often difficult to know how to relate the particular person that is the centre of the concern in therapy, selection, training or management to the 'normative' group on which the average was calculated.

Group heterogeneity

Many experimental studies assume that the variability in the sample is not of such a large scale that it will totally mask important differences between sub-groups. However, the ways in which people may differ from each other are legion. Therefore research has to define very carefully the sample to which the results may apply. Otherwise major variation in a particular population may unknowingly negate the relevance to other samples of the effects found in a particular study.

Practical constraints

As I have mentioned, in order for research to be possible it has to be carried out within the resources of time, skills and costs that are available to the researcher. It is, therefore, very critical to the success of a study that it is formulated within the possible practical constraints that exist. This will include access to appropriate data and appropriate contexts for doing research, as well as other limitations that come from the views of ethical committees and the particular points in time at which certain sorts of research is possible. It is one of the great practical problems of postgraduate courses that most studies are planned to be conducted during the summer months. This is precisely the time when many services are not fully operational or when there are limited levels of staffing because of people being away on holiday. As a consequence there are often not people available to help the research forward.

Qualitative concerns

For many real world problems there is not usually a concern with how *much* of a problem exists but rather *what* the problem is that can be described. Quantitative measurement procedures may therefore be very convenient for the desired levels of sophisticated analysis, but the real answers to pressing problems may be best answered by an indication of the mixture and variety of experiences that relate to the problems to be solved.

Conclusions

Central to all my comments above has been the desire to draw upon general psychological ideas to guide thinking about psychological research. One of the curiosities of research methodology teaching in psychology is that it is often devoid of consideration of the psychological issues that would be central to any other teaching in the curriculum. Typically, statistics are dealt with as a branch of mathematics and research design is treated as a cunning game of logic. Even the modes of data collections are usually presented as standardised procedures that have their own inner power, quite independently of any theoretical formulations which may have contributed to their creation.

The central themes I have emphasised for thinking about methodology for psychology has been that psychologists need to use their psychological skills in thinking about the research process. They need to consider why they are doing the research and what the likely consequences of this research will be. In effect, this is moving away from a purist, abstract approach to research and towards a more context aware approach. In advocating that people should understand the potential implications of their research I am not saying that they should avoid the strict logic of science. The opposite is the case. I am advocating a richer understanding of the ways in which scientific method has its impact. Science is not only about disentangling causal influences under controlled conditions. It is also scientific to examine the structure of relationships that exist in natural settings or to give a detailed description of particular patterns of behaviour.

The essence of science is the building of generalisable, theoretical models. It is first of all a clear and, wherever possible, formal, statement of the nature of the hypotheses that are being examined and the test of those hypotheses against appropriate data that makes a study scientific. But the test of hypotheses against data requires a clear sighted understanding of the contextual and resource limitations that are put upon the research.

The teaching of research methods in applied psychology, therefore, should be anything but dogmatic. I believe that it is most productive when it explores the issues I have listed above and many others. This will provide students with a rich, conceptual system that will provide them with a flexible enough range of ways of thinking about research to still be of value to them when they are commissioning their own research projects well into the next century.