



**SWP 4/89 THE CASE STUDY: A VITAL YET
MISUNDERSTOOD RESEARCH
METHOD FOR MANAGEMENT**

**DR N.CRAIG SMITH
Cranfield School of Management
Cranfield University
Cranfield
Bedford MK43 0AL
United Kingdom**

**Tel: +44 (0)1234 751122
Fax: +44 (0)1234 781806**

28. NOV 88

ABSTRACT

This paper urges greater recognition of the case study as a research method for management. It acknowledges concern about the representativeness of case studies but by specifying the relationship between epistemology and research methods shows that this concern is misplaced. Representativeness is irrelevant for many research purposes, particularly when the distinction is made between logical and statistical inference. The validity of explanations or theory derived from case studies depends on the logic of the analysis and acknowledgement of *ceteris paribus* conditions, not on how typical the cases may be.

Typologies of case studies have been proposed and these are considered, together with recommendations for the conduct of systematic and rigorous case study research. It is stressed that research problems should be addressed using appropriate research methods. Research of the important problems within the management area frequently demands a qualitative research approach, though it would seem that such an approach is often ignored because of a positivist research orientation.

An assessment of the strengths and weaknesses of the case study method, which highlights its potential in the vital role of theory-building, leads to the conclusion that this method has much to commend it to management researchers.

In Recognition of the Case Study

Why should the research of management academics have intellectual authority and command the respect and attention of practitioners? An appropriate response to such a question would note the role of management academics as social scientists and that management research is therefore 'scientific'. It is not simply because management academics are usually to be found in universities and other 'centres of learning' and often have titles conveying eminence and wisdom! The legitimacy of management research must also be derived from the way in which it is conducted. In other words, the scientific method is employed to ensure that research findings are meaningful, both theoretically and practically too within an applied discipline, accepting that they may not have immediate application.

Einstein said 'the whole of science is nothing more than a refinement of everyday thinking'. The refinement comes from the methods which scientists employ. So it is the use of the scientific method which confers legitimacy on management research (and, as a consequence, much management teaching). However, what does the scientific method amount to in application to management? An examination of the content of many management journals containing empirical papers would suggest that being scientific means quantification within a hypothetico-deductive approach to science. Qualitative and inductive approaches are much less frequently reported, if at all (1).

Consideration of the contribution of this management research (the extent to which it is meaningful) is beyond the scope of this paper. It would raise a number of major questions, particularly about the agenda for management research and who sets it; though these issues should not be ignored (2). The concern here, however, is with whether in seeking to apply scientific method to management, researchers are using the most appropriate research methods and techniques. This clearly has some bearing on the potential contribution of research in management. The case study, it is suggested, is deserving of greater recognition as a research method. Such a claim has to be made within the context of an appreciation of what management research is trying to achieve.

Within social science generally there has long been criticism of positivist research orientations (3). This is gradually being acknowledged within the management literature (4). However, positivism continues to dominate, especially in the United States. Despite the leadership of the United States in management education, the traditional American research model would be a poor one to emulate. Being

scientific is not solely or necessarily the result of number-crunching. While this author would not deny a role for quantitative approaches within management research, qualitative approaches, including the use of the case study method, are often more appropriate for tackling the important research problems of management.

In using any research method it is helpful to understand its epistemological underpinnings. By examining the relationship between epistemology and research methods further support can be found for the use of qualitative approaches, such as the case study, in management research. It should be noted that all references to case study research in this paper refer to the development of case studies for research purposes. Teaching cases are developed to illustrate established theory. Research cases are used to build theory, though this does not preclude their later development into teaching cases.

Epistemology and Research Methods

'Although business researchers may need to learn even more about techniques, their notable weakness is their inadequate emphasis on the research process. Knowing how to use the tools and techniques of research does not in itself guarantee the effectiveness of an individual in carrying out a scientific investigation' (5).

Rigby's observation on the failure of management researchers to address fundamental methodological concerns seems almost as valid today as it was twenty years ago. Such a criticism is borne out by the reference above to the positivist orientations found in much published management research. Positivism can be simply defined as 'working as natural scientists are believed to' (6). It reflects, therefore, a belief that the social sciences can be investigated in the same way as the natural sciences. Many writers on research methodology have argued against positivism, the essence only of these arguments will be necessary here to demonstrate the relationship between epistemology and research methods and, as a consequence, the shortcomings of some of the more commonly adopted research approaches in management.

It should first be noted that positivists only rarely define themselves as such. Positivism is all but a term of abuse; though some would say rightly so, for in extreme cases it amounts to an ignorance of epistemological issues. Yet the waters beyond positivism are dangerous. They are best avoided by the faint of heart. In some respects, to operate within a positivist framework allows the researcher the luxury of not having to question whether the research is meaningful: the

methodological concern of such research often focuses on internal rather than external validity. Ultimately this is dysfunctional if social science is to advance.

There has been considerable debate about the 'scientificness' of the social sciences, including management (7). Science aims to create order, to make sense of facts. It seeks patterns or regularities. In so doing, a process of systematic observation, description, explanation and prediction is employed. At least this much can be agreed on. And all of this may be found within the social sciences. A reasonable position to adopt seems to be one of admitting the limitations to social science achievements while acknowledging the complexities of social science research. It may then be claimed that the social sciences are sciences insofar as they apply scientific method. But one must ask what form of scientific method (if any) is appropriate to social science.

Consider the nature of this particular human activity known as science. Hughes notes that 'scientific methods seek deliberately to annihilate the individual scientist's standpoints and are designed as rules whereby agreement on specific versions of the world can be reached: a distinction, in short, between the producer of a statement and the procedure whereby it is produced' (8). The outcome of these methods is scientific knowledge: 'a systematic body of concepts, theories, principles and laws or law-like statements designed to explain phenomena' (9). This outcome is achieved where plausibility is recognised or where, as Hughes puts it, there is agreement on specific versions of the world. The problem for the social sciences (and hence some of the complexity of social science research as acknowledged above) is that this involves a human attempt to explain human phenomena. This is problematic because it is doubtful as to whether method, can ever 'annihilate' the individual scientist's standpoint. Medawar admits this problem within the natural sciences, he quotes Whewell: 'Facts cannot be observed as facts except in virtue of the conceptions which the observer himself unconsciously supplies' (10). Such is the dilemma posed for the social sciences, that Hughes feels obliged to ask: 'is a science of social life impossible?' (11).

In reference to Schutz, Hughes explains the dilemma of the social sciences in terms of the social construction of reality: 'like all sciences they make objective meaning claims, or at least aspire to do so, but in the case of social sciences these have to be within the context of the human activity which has created them and which cannot be understood apart from this scheme of action' (12). As Berger and Kellner put it, in a different context, 'direct access to facts and laws... is never possible, no matter what one's standpoint... there is no magical trick by which one can bypass the act of

interpretation' (13). This is the basic epistemological problem of social science. How can the human world be objectively known in subjective, human terms?

One may, indeed, go further, for scientific activity and what is associated with it, including the status of scientists and scientific knowledge is, after all, like the phenomena studied by social scientists, a social construction. As Ford neatly observes, 'when academics take off their white scientific coats and funny philosophical hats they turn into ordinary people' (14). This is the problem, and one that social scientists cannot escape. Moreover, to echo an earlier and vital theme, if they didn't have their white scientific coats and other accoutrements of scientific activity - including titles and ivory towers - would the outcome of such activity, scientific knowledge, still have intellectual authority? For, as Hughes observes, 'it is necessary to ask what it is about the procedures and methods used by sociological researchers or economists, psychologists, historians, etc, which makes them superior, gives them greater intellectual authority than those used by, say, the man or woman in the street, the journalist, the racial bigot, the politician, the revolutionary, or a Trobriand Islander'.

Clearly, at this juncture, the analysis of epistemological issues surrounding social science research has reached a point well within the maze of research methodology. The problems facing social scientists seems intractable. One might, therefore, take heart in the following words from the sociologist George Homans, quoted by Denzin in *The Research Act*:

'The most important advice I can give the contemporary sociologist has nothing to do with the validity of my arguments. It is this: you do not have to believe anything about theory and methodology that is told you pretentiously and sanctimoniously by other sociologists - including myself. So much guff has gotten mixed with the truth that, if you cannot tell which is which, you had better reject it all. It will only get in your way. No one will go far wrong theoretically who remains in close touch with and seeks to understand a body of concrete phenomena' (15).

It is very easy to end up in a methodological maze. Providing the researcher has a basic grasp of the issues and remains close to the phenomena studied, meaningful research is likely to be conducted. Yet, within this sensible conclusion, lies a key to some resolution of the problem identified above as well highlighting the principal weakness of positivist research. The superiority of scientific knowledge, its greater

intellectual authority, stems from whether the scientist *qua* scientist was able to 'stand back a bit', achieve some measure of objectivity. Morally at least, there is an obligation on the scientist to do this if claims of superiority are to be made. But even though social science involves human attempts to investigate human phenomena, a natural science, positivist approach to the social sciences often ignores the inevitable act of interpretation by the scientist. It then becomes invalid because the attempt at objectivity is illusory. Moreover, because of this artificial distancing, the researcher is not sufficiently close to the phenomenon under investigation to understand it. So, just as there is a requirement 'to stand back a bit', there is an equal requirement not to stand back so far that the findings are distorted by distance as well as by the act of interpretation (16).

This argument about distance from the phenomenon under study and its impact upon objectivity and whether research is meaningful can be expressed another way. It was earlier noted that science involves a systematic process of observation, description, explanation and prediction. In applying natural science methods and techniques to social science problems, positivist approaches assume that social science is at a point of development whereby methods and techniques appropriate to explanation and prediction may be employed and that much of the complexity of social phenomena can be ignored. For much of social science, observation and description, with possibly limited explanation, are the requisite modes. Certainly this is true of management. Accordingly, methods appropriate to this phase of development need to be employed.

Bonoma, in one of the few papers in the management literature on the case study method, covers this problem of positivist research orientations by referring to a trade-off between 'currency' and 'data integrity' (17). Currency pertains to generalisability of results, an amalgam of what is elsewhere termed external validity and pragmatic or ecological validity. Data integrity refers to those characteristics of research that affect error and bias in research results, an amalgam of internal validity, statistical conclusion validity and reliability. Bonoma notes that, ideally, high levels of both data integrity and results currency should be sought, but that it is not possible for any single research method simultaneously to minimise multiple threats to both data integrity and currency. So, for example, laboratory experiments offer high data integrity but low currency, in contrast to case research which offers high currency but low data integrity. He explains:

'the study which seeks a high degree of data integrity requires a precise operationalisation of the research variables, a relatively large

7

sample size and quantitative data for statistical power, and the ability to exercise control over persons, settings, and other factors to prevent causal contamination. In contrast, a study which seeks high currency typically demands situationally unconstrained operationalisations of variables to allow cross-setting generalisation, and observations within natural, ecologically valid settings - "noisy" settings - where large samples, quantitative measures, and control are more difficult to achieve. Often, the latter kind of study demands a greater use of subjective or, at best, clinical analysis.'

In making the trade-off, choosing the right method, Bonoma suggests the researcher has to consider the purpose of the research and nature of the phenomenon under investigation. On the former, Bonoma, in essence, notes that high data integrity methods (and, therefore, with low currency) cannot be efficiently applied to theory-building research; that is, research at the description end of a research continuum of description, classification, comparison, measurement/estimation, establishing association, and determining cause and effect. This is because 'either the power of deductive methods is underutilised, or theory and/or method are prematurely pressed into service when their underlying assumptions cannot be met'. Of course the converse applies to high currency, inductive methods. In considering the phenomenon under investigation he suggests the key issues are whether the phenomenon can be studied outside its normal setting (often requisite for high data integrity) and whether it is amenable to quantification. On the latter point, Bonoma gives the example of good practice in marketing management as a research topic which currently, at least, defies quantification.

In looking at the research conducted in marketing, Bonoma concludes that 'the apparent research bias towards types of investigation that preserve data integrity at the expense of currency results in a methodological one-sidedness that may impair the development and testing of sound theories'. This reiterates the concern earlier expressed about the research agenda for management. Some areas of management can quite legitimately be investigated using quantitative and hypothetico-deductive approaches. In such circumstances one might conclude that positivism is acceptable. However, to what extent are these areas more worthy of investigation than those demanding more inductive and qualitative approaches? One must certainly question a research agenda should it be determined by a requirement to use particular research methods.

In sum, an understanding of the epistemological issues surrounding social science research point to the requirement to use an appropriate method for the research problem; in other words, 'horses for courses'. The debate about positivism has illustrated the limitations of traditional research methods when applied to many social science problems. An alternative and seemingly more potentially fruitful path would employ qualitative and inductive approaches. The case study is included in such approaches.

Epistemological issues as discussed above seem frequently to be considered irrelevant to the practice of research - to be ignored if the researcher can latch on to an appropriate research method, appropriateness usually stemming from prior use in similar circumstances. Yet they have a direct consequence for the meaning which may be attributed to the research; meaningful research demands a sound epistemological base to the research methods. Epistemology and research methods are interrelated in a complex way. Despite the assumed division between the theory and practice of research, the two cannot be considered in isolation. There is, so to speak, a two-way street. Much of the criticism of the use of case studies in research stems from this misapprehension, the view that the relationship between epistemology and research methods is unidirectional, a one-way street.

In Reply to Questions of Representativeness

The principal criticism of case studies in research is that they are unrepresentative. Theoretical conclusions derived from case studies are not considered to be valid unless the cases can be demonstrated to be 'typical' of the phenomena under investigation. The very word 'representative' implies recourse to survey research methods to demonstrate, via quantitative procedures, that the theoretical conclusions derived from the cases are applicable to the population as a whole. Qualitative research, according to the canons of positivism, is fine for exploratory studies, but quantification is necessary to establish the validity of any findings. The special issue of *Administrative Science Quarterly*, on qualitative research, contained as a preface a neat illustration of this point under the heading 'The Seminar':

Qualitative researcher:

'Many people these days are bored with their work and are....'

Quantitative researcher
(interrupting):

What people, how many, when do they feel this way, when do they work, what do they do. why are they bored, how

long have they felt this way, what are their needs, when do they feel excited, where did they come from, what parts of their work bother them most, which...'

Qualitative researcher:

'Never mind.'

So concerns with representativeness may be irrelevant. Some would argue this irrelevance is absolute. Others that it is only temporary, that - for the moment - representativeness can be ignored, but that it must be attended to eventually if generalisations - valid theoretical conclusions - are to be made. The next section, in examining theory-building and the case study method, is largely concerned with the former proposition that representativeness is absolutely irrelevant. Such a proposition rests on accepting the two-way street concept of the theory and practice of research, that there is an interrelationship between epistemology and research methods and hence concern with whether cases are typical or not is epistemologically erroneous. However, before explaining why this should be so, it is useful to consider the proposition that representativeness is only temporarily irrelevant. In so doing, the more conventional argument for the use of the case study method may be briefly explored.

Perhaps surprisingly, there is not a great deal of literature on the use of the case study method, at least not under that title. Yet many researchers refer to case studies. This imbalance seems to reflect the low status of case study research (because of the representativeness issue) and the view that it is not a method as such. One can find references to research methods such as repertory grid or comparative analysis which then *produce* case studies, but they are not conceived as forming a part of case study method. Consequently, McClintock *et al* refer to case study 'strategies' rather than 'methods': 'The differences between case study and sample survey strategies in the analysis of organisations reflect a broader distinction in the social sciences between qualitative and quantitative methods' (18). This would indicate that case studies are an approach, rather than a method. As Goode and Hatt put it over thirty years ago:

The case study, then, is not a specific technique. It is a way of organising social data so as to preserve the *unitary character of the social object being studied*. Expressed somewhat differently, it is an approach which views any social unit as a whole. Almost always this

means of approach includes the development of that unit, which may be a person, a family or other social group, a set of relationships or processes... or even an entire culture (19, their emphasis).

Clearly the case study is not a technique, it is not a means for obtaining data. Yet it may be described as a research method insofar as it is a method for organising data. (One may also refer to case study methods (20), such as participant observation, content analysis, or repertory grid, by which data for case studies is obtained. However, it is simpler to distinguish between techniques and the method, particularly as techniques such as content analysis or repertory grid are not exclusive to the case study method.)

Case studies, as qualitative research, may be employed within a positivist perspective. One may seek to involve numbers and counting, as Jauch *et al* suggest in advocating the structured content analysis of cases (21). Or, as McClintock *et al* propose, apply the logic and method of survey research (22). The latter paper considers some of the literature on the use of case studies and qualitative vs. quantitative approaches. It suggests a choice between 'thick', 'deep', and 'holistic', and 'thin', 'narrow', but 'generalisable'. In response to the question: 'What do you do if you prefer data that are real, deep *and* hard?' (raised by Zelditch in 1962), McClintock *et al* favour the invention of research designs that incorporate qualitative and quantitative strategies. They seek (quoting Warwick), 'to wed the qualitative and historically attuned case study with representative coverage and quantification'. By incorporating elements of positivist research design (sampling, quantification, etc.), they absolve themselves from the charge that their cases are unrepresentative.

Theory-building and the Case Study Method

So the problem of representativeness may become temporarily irrelevant either by choosing to view case studies as appropriate to exploratory work only, or by making them representative through the application of quantitative procedures. However, both solutions still accept the epistemological requirement for representativeness. One may, alternatively, view it as absolutely irrelevant.

There are two reasons for this. Firstly, one may have different intentions when using case studies as opposed to survey research. One's purpose, for example, may be with description rather than correlation. Consequently, Benson and Hughes distinguish between the different intentions in ethnomethodology and what they choose to term conventional sociology (read positivist):

'What we are *not* saying is that 'conventional' sociology and ethnomethodology are in competition with each other; that, for example, the ethnomethodologist's approach to the study of crime is a better alternative to those exemplified in criminology or sociology... under ethnomethodological treatment the topic as conceived by 'conventional' sociology tends to dissolve or disappear, suggesting, to put it no stronger than this, that ethnomethodology's interest lies in a different direction pointing to different phenomena' (23).

Secondly, and perhaps most importantly, there is the recognition that representativeness is irrelevant because it can be a spurious basis for claiming validity. Worsley and others write:

'The general validity of the analysis does not depend on whether the case being analysed is representative of other cases of this kind, but rather upon the plausibility of the logic of the analysis. The generality is of the same kind that enabled Sir Ronald Ross to announce the 'cause' of malaria when he found the malaria parasite in the salivary gland of a single female *Anopheles* mosquito in 1897 (24).

Clyde Mitchell has expanded on this argument in a recent article which presents a particularly thorough and convincing submission for the case study method (25). As he shows, 'logical inference is epistemologically quite independent of statistical inference'. How he comes to this conclusion is worthy of close consideration. Mitchell starts by referring to an eclipse of interest in case studies as a method of sociological analysis, which he attributes to the tremendous increase in quantitative studies following the development of statistical techniques and powerful computer technology. He suggests there is a consequent confusion about the use of case studies, as indicated by the challenge frequently addressed to those who have chosen to pursue the deviant path of case studies: 'How do you know the case you have chosen is typical?' Mitchell responds to this challenge by explaining the difference between making inferences from statistical data and from cases. In so doing, he provides guidelines for the use of case studies in social investigation and theory building.

A simple definition of the case study is at first presented: 'the basic descriptive material an observer has assembled by whatever means available about some particular phenomenon or set of events' This however would also apply to the

'cases' of physicians and other practitioners trained in the systematic recording of information, such as psychiatrists, social workers and so on. It is necessary to emphasise the distinction between their purposes and that here of using the material to infer theoretical principles. Moreover, the term observer is better replaced by analyst because of these analytic intentions. So Mitchell's definition characterises a case study as 'a detailed examination of an event (or series of related events) which the analyst believes exhibits (or exhibit) the operation of some identified general theoretical principle'. Different types of case study may be identified according to their complexity and their use (these are considered in the next section). Following his classification of the types of case studies, Mitchell turns to his central concern and the 'fundamental problem' in case studies: 'the basis upon which general inferences may be drawn from them'.

Mitchell asks how ostensibly unique material can form the basis for inference about some process in general. The very word 'case' connotes this uniqueness and the implication of a chance or haphazard occurrence. Yet most social anthropological and much sociological theorising is founded on case studies. He suggests the difficulties in the practice of the case study method arise out of the common assumption that the only valid basis of inference is that which has been developed in relation to statistical analysis. However, as Mitchell goes to great lengths to explain, statistical analysis merely permits the inference that characteristics within the sample may be expected within the population. Theorised relationships between the characteristics are the result of a separate procedure and not substantiated by statistical analysis. Having described the purposes of statistical analysis, Mitchell writes:

In so far as the descriptive features of the sample (and therefore of the parent population) are concerned the validity of the inference is probably sound. The distribution of age of a representative sample drawn from a parent population probably reflects reasonably accurately - given sampling errors - the distribution of ages within that population. A difficulty arises however when the relationship *between* characteristics is considered. In the sample analysed a relationship - a correlation - in fact may be noted between say age and the probability of being married. In terms of the canons of statistical inference the analyst may assume that the same relationship exists between the same characteristics in the parent population. Note, however, that the inference from the sample in relation to the parent population is simply about the concomitant variation of two

characteristics. The analyst must go beyond the sample and resort to theoretical thinking to link those characteristics together... The inference about the *logical* relationship between the two characteristics is not based upon the representatives of the sample and therefore upon its typicality, but rather upon the plausibility or upon the logicity of the nexus between the two characteristics (his emphasis).

Mitchell offers an interesting example of a study for which the author claimed validity on the basis of statistical significance, but which was rejected because it was not plausible. The findings were rejected not because the variables failed to statistically correlate, but because they were not logically (or causally, if one prefers) related. The researcher had linked interpretations of Rorschach ink-blot with dietary disorders. It had been found that there was a statistically significant difference between those with dietary disorders and those without, in terms of the former reacting to the blot with a 'frog' response. This the researcher attributed to an unconscious belief in the cloacal theory of birth, which involves oral impregnation and anal parturition. The cloaca of the frog (its excretory and reproductive canals) are common, a biological fact providing, it is assumed, the rationale for this belief. The researcher hypothesised: 'Since patients should be inclined to manifest eating disorders: compulsive eating in the case of those who wish to get pregnant and anorexia in those who do not... such as patients should also be inclined to see cloacal animals such as frogs on the Rorschach.' The response of other clinical psychologists to this however was 'I don't believe it', even after having seen the experimental results. The theory proposed was rejected on the grounds of plausibility, regardless of unimpeachable method. As Mitchell explains, 'While the clinical psychologists may well have accepted that more people with dietary disorders saw the blots as frogs than those without, they could not accept the *explanation* of the relationship between the two characteristics' (his emphasis).

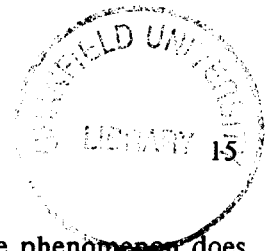
He is not, of course, the first to recognise such a distinction. Glaser and Strauss, for example, make the distinction between theoretical and statistical sampling:

'Theoretical sampling is done in order to discover categories and their properties, and to suggest the interrelationship into a theory. Statistical sampling is done to obtain accurate evidence on distributions of people among categories to be used in descriptions or verifications' (26).

Mitchell, in turn, recognises the commonly accepted distinction between statistical inference and scientific or causal inference. The former is 'the process by which the analyst draws conclusions about the existence of two or more characteristics in some wider population from some sample of that population to which the observer has access'; whereas, 'scientific or causal - or perhaps more appropriately - logical inference, is the process by which the analyst draws conclusions about the essential linkage between two or more characteristics in terms of some systematic explanatory schema - some set of theoretical propositions.' Importantly though, Mitchell recognises that the distinction is often absent in quantitative studies: 'In analytical thinking based on quantitative procedures *both* types of inference proceed *pari passu* but there has been some tendency to elide logical inferences with the logic of statistical inference: that the postulated *logical* connection among features in a sample may be assumed to exist in some parent population simply because the features may be inferred to *coexist* in that population' (his emphasis).

More importantly, this distinction paves the way for illustrating the irrelevance of representativeness in case studies, for the analyst, in using this method, is only concerned with logical inference. As Mitchell argues, 'the process of inference from case studies is only logical or causal and cannot be statistical and extrapolability from any one case study to like situations in general is based only on logical inference. We infer that the features present in the case study will be related in a wider population not because the case is representative but because our analysis is unassailable'. So in summary of Mitchell's position on the extent to which case studies have validity, Silverman writes, 'the claim, therefore, is not to representiveness but to faultless logic' (27).

Silverman points out that implicit in Mitchell's analysis here is the logic of analytic induction. Mitchell contrasts analytic induction and enumerative induction in reference to Znaniecki and to Robinson. Referring to the latter, he observes 'by its procedures analytical induction isolates the *necessary* circumstances for the manifestation of some phenomenon but does not in itself establish *sufficient* conditions... whereas enumerative induction, as exemplified by statistical procedures, establishes sufficient conditions for the phenomenon to occur' (his emphasis); though this is not to suggest a case for using both case studies and quantitative procedures, that is, as noted above, adopting the position that representativeness is only temporarily irrelevant. Robinson's argument assumes case studies concentrate on instances where the phenomenon under investigation actually occurs. To do such is, of course, eminently sensible, it is a far more efficient way of seeking evidence than the more random selection of cases. Yet this does not presume a requirement for



statistical procedures to take into account those occasions when the phenomenon does not occur, for these will have already been accounted for in the theorizing. As Mitchell puts it, 'the extent to which generalisation may be made from case studies depends upon the adequacy of the underlying theory and the whole corpus of related knowledge of which the case is analysed rather than on the particular instance itself'.

Selecting cases for study will, as a consequence, not therefore rest on how typical the case may be, but on its explanatory power. Indeed, 'deviant' cases may be chosen, as analytic induction suggests, to demonstrate the limits to generalisation. The presentation of the case will be limited to that material which most effectively reveals the theoretical principle investigated, for just as the 'best' cases are employed, so are the 'best' elements within each case. This atypical, selective quality to case studies gives rise to their criticism as a basis for generalisation, but this is ill-founded. Irrelevant elements, just as irrelevant cases, would merely serve to confuse; providing the analyst meets the *ceteris paribus* criterion they should be ignored. Mitchell explains: 'in interpreting the events in any particular case theoretically the analyst must suppress some of the complexity in the events and state the logical connexions among some of the features which are germane to the interpretation'. And later, in reference to Gluckman, 'it is perfectly justifiable for the analyst to operate with a simplified account of the context within which the case is located provided that the impact of the features of that context on the events being considered in the analysis are incorporated rigorously into the analysis'. Much, of course, is left to the analyst, particularly his or her intimate knowledge of the circumstances of the case:

All cases are necessarily contextualised and generalisations made from case studies must therefore be qualified with a *ceteris paribus* condition. It is incumbent on the observer to provide readers with a minimal account of the context to enable them to judge for themselves the validity of treating other things as equal in that instance.'

Mitchell's observations on the logic of case studies has been usefully summarised in a table by Silverman. This is shown in the table below:

The Logic of Case Studies

	<u>Survey Research</u>	<u>Case Studies</u>
Claim to validity	Depends on representativeness of sample	Only valid if based on articulated theory
Nature of explanations	Correlations not causes	Logical/causal connections
Relation to theory	Theory-neutral	Theory-dependent

In sum, it should be recognised that epistemology and research methods are interrelated. A position on the former does not simply give rise to the latter. Accepting this two-way street prompts a reappraisal of the accepted wisdom that the case study method is inferior to quantitative methods because it lacks representativeness. Such a charge often prompts the response that representativeness is temporarily irrelevant; either because the case studies are exploratory, implying survey research at some future date, or that quantitative and qualitative procedures may be combined to provide the 'best of both worlds', which, while acknowledging the usefulness of case studies, still assumes the importance of representativeness. Alternatively, representativeness may be viewed as absolutely irrelevant. This position, contrary to accepted wisdom, reflects either an acknowledged difference in purpose, as in the concern of an ethnographer to describe a simple society as part of an anthropological study, or recognition of the epistemological distinction between statistical inference and logical inference.

Types of Case Study

Gluckman distinguishes between three types of case study, which may be viewed as falling along a continuum of increasing complexity (28). These are 'apt illustrations', 'social situations' and 'case studies'.

The apt illustration is 'a description of some fairly simple event or occurrence in which the operation of some general principle is clearly illustrated'. More complex is the analysis of a social situation, where 'some restricted and limited (bounded) set of events is analysed so as to reveal the way in which general principles of social organisation manifest themselves in some particular specified context'; for example, Gluckman's account of the official opening of a bridge in Zululand, which brought

together different sectors of the population, reflects the structure of South African society at that time. Then, finally, there is the extended case study, typically dealing with a sequence of events over a long time period and 'where the same actors are involved in a series of situations in which their structural positions must continually be re-specified and the flow of actors through different social positions specified'. The emphasis here is on a process. While apt illustrations and social situations are principally distinguished by their differences in complexity, social situations and extended case studies are distinguished by complexity and the duration of time spanning the events described. The extended case study is probably best exemplified by the lone anthropologist landing on a South Seas island, note-book and pencil in one hand, a large suitcase in the other, with the intention of staying for some time. Margaret Mead's study of the coming of age in Samoa would be a well-known, if highly criticised, example.

Eckstein has distinguished five categories of case study according to the way in which the case may be used as a contribution to theoretical thinking (29):

1. Configurative-idiographic studies.
2. Disciplined configurative studies.
3. Heuristic case studies.
4. Plausibility probes.
5. Crucial case studies.

Crucial case studies are comparable to the crucial test in the natural sciences. Plausibility probes are studies designed to tentatively test theory developed - which has perhaps resulted from heuristic case studies. Configurative-idiographic studies describe events and their circumstances which may indicate relationships, but not necessarily general theoretical interpretations. Finally, there are disciplined configurative studies, which do indicate general theoretical interpretations. They 'as their name implies are still configurations or patterns of elements but the observer does not look upon these as unique or "idiographic". Instead the analyst seeks to interpret the patterns in terms of general theoretical postulates.'

Conducting Case Study Research

The data collected frequently comes from both primary and secondary sources. Semi-structured interviews, using interview schedules often provide much of the primary data. The interviews should, with the permission of the respondents, be tape-recorded and subsequently transcribed in full. Transcribing interviews is a

lengthy process, but worthwhile in enabling the researcher to stay close to the data. Tape-recording ensures all data were noted and, in leaving the researcher free from the burden of making notes, allows concentration on the issues of concern and rapport to develop more easily. Data such as copies of letters, reports and so on, may also be obtained from interviewees.

Interviews are problematic in a number of ways, but principally:

1. Through the interviewer's values - reflected in the questions asked and the interaction, as well as non-verbal cues (raised eyebrows, for example) - which may bias response and interpretation.
2. In terms of validity - respondents providing answers the interviewer wants to hear, or that the respondent would like the interviewer to hear.
3. Inappropriate for the research question. Some research questions cannot be answered by interviews.

These problems and others as identified by many sources (30), must as far as possible, be controlled for; though the author has much sympathy with Silverman's position that there is no bad way of doing interviews, there is only bad analysis of interviews. As he writes, 'for positivists, interviews are essentially about ascertaining facts or beliefs out there in the world', whereas (as an interactionist would argue) interviews may also be seen as social events in themselves, involving interviewer and interviewee in mutual participant observation (31). Interviews are not just about asking questions and taking the answer given, but also about interactions, as, for example how they reveal feelings or fears. So finally, before reviewing the strengths and weaknesses of case studies, this section should make brief mention of the form of analysis which may be employed.

The more sophisticated sources on qualitative data analysis (32) refer to explicit coding and analytic procedures, whereby categories (concepts or relationships) and their properties are identified and analysed as they occur within the data. Some advocate quantitative procedures, from simply counting categories to statistical analysis. Ignoring the positivist overtones of coding and its analysis, there is the problem of specifying what to code. If the data alone is to generate theory then clearly categories cannot be specified *a priori*. This suggests either leaving the coding to the end of the data collection, which would deny flexibility such that interesting categories would only be recognised too late to prevent appropriate

investigation within the study, or coding everything, a burdensome (if not impossible) task, as Glaser and Strauss acknowledge. They propose the constant comparative method, which, they suggest, incorporates an ongoing explicit coding procedure and permits theory development during the study.

However, on the basis of this author's experiences of case study research this source and even the 'hands-on' manual by Miles and Huberman, seem remote from the practice of research. Research as lived, where interviews are a process of continual idea development; where theory is the outcome of a combination of studying other works, the data collection and chance occurrences and conversations; where as Latour and Woolgar comment on the natural sciences, order is brought forth from chaos (33); research as lived may not have the time or requirement for such fancy procedures. Coding, for this author at least, was satisfactorily achieved intuitively during the data collection. Only on writing-up the cases was it though necessary to code the data in any way, and then only to be certain of conveying key elements within the cases (34). This may, however, have had something to do with the partly deductive approach adopted or the research problem. However, the usefulness of explicit coding during data collection appears limited. If the researcher is close to the data, analysis and theorising is inevitably taking place. The value of such procedures may have more to do with making qualitative research appear acceptable and rigorous, than improving the method.

A further consideration is deciding how many cases to present, yet this may be out of the hands of the researcher. This author found that as each case progressed, as each interview was conducted, the data were found to be conforming to a pattern. In other words, a theory was emerging. The content of the data became 'predictable' because it conformed with expectations. This is common to qualitative research and is sometimes referred to as 'saturation' (by Glaser and Strauss, for example). When saturation is achieved, the researcher may claim to have a sufficient number of cases.

Strengths and Weaknesses of the Case Study Method

'case studies of whatever form are a reliable and respectable procedure of social analysis... much criticism of their reliability and validity has been based on a misconception of the basis upon which the analyst may justifiably extrapolate from an individual case study to the social process in general... The validity of the extrapolation depends not on the typicality or representativeness of the case but upon the cogency of the theoretical reasoning'.

The use of the case study method can lead to charges of anecdotalism. Yet for many research topics within management this method is the most appropriate. However, case studies need not be viewed as solely exploratory or tentative exercises in research. Their validity, when correctly understood, depends, as Mitchell indicates above (35), on how they are used and the logic of their analysis. Yet it would be foolish to understate some of the weaknesses of the case study method. As the earlier discussion has indicated, qualitative approaches do bring the researcher closer to the phenomenon under investigation and some might say too close. This raises two distinct problems. Firstly, the problem of the dependence on the researcher's skills of clinical analysis in maintaining objectivity. Yet as with quantitative research, judgement may still be passed on the validity of research results. (Indeed, the distinction between qualitative and quantitative research may be artificial in many ways. As earlier discussion suggests, one might agree with Ratcliffe that '*all approaches to inquiry are inherently qualitative in nature*' (36).) Secondly, there is the political consideration of the acceptability of case study research. As Bonoma somewhat drily observes - and is perhaps a fitting conclusion to this paper - '*because the major thrust of most published marketing research is towards deductive, numerate, and causally directed research, the researcher may have a greater challenge in demonstrating the benefits and necessity of qualitative methods for the problem studied*' (37).

Notes and References

1. Bonoma, for example, found that a random sample of ten issues (124 articles) of the Journal of Marketing Research contained no qualitative studies of any sort. See Bonoma, Thomas V, 'Case Research in Marketing: Opportunities, Problems, and a Process', Journal of Marketing Research, Vol. XXII (May 1985).
2. Few can deny the logic of the corporate requirement for a sense of mission, objectives and strategy. Management teachers regularly highlight the importance of corporate direction; after all, 'if you don't know where you are going, you may end up in the wrong place'. Yet this logic is so rarely applied to academic research and research in management is no exception. Is this a case of 'physician heal thyself'? Or is it that research is an activity which, because of the uncertainties that surround it and the loose organisation of those that conduct it, defies any sort of planning? Surely some direction for management research, particularly as it falls within an applied academic discipline, is both possible and necessary. There are therefore some key questions about the role and nature of management research which should be addressed by the management academic 'community'. The British Academy of Management annual conference provides the ideal opportunity to consider questions such as: (1) Who sets the agenda for management research? (2) How and to what extent does management research contribute to management teaching? (3) How and to what extent does management research contribute to management practice? (4) What efforts are being made to integrate management research findings, to produce a coherent body of knowledge? (5) What are the most appropriate methods and techniques for researching problems in management? These questions are clearly important, not least when the demand for management teaching at the undergraduate, postgraduate and post-experience levels grows apace, the substance of which is at least partly fed by academic research. This author would argue that the criteria for determining the management research agenda should be at least partly determined by practitioner and teaching requirements, though this is not to suggest that management research should be partisan and solely concerned with the interests of managers.
3. This criticism is well addressed in Silverman, David, Qualitative Methodology and Sociology (Aldershot, Gower, 1985).
4. See, for example, the special issue of the Administrative Science Quarterly on qualitative research, Vol. 24 (December 1979), or Bonoma, op.cit. (note 1).
5. Rigby, Paul H, Conceptual Foundations in Business Research (New York, John Wiley, 1965), p.vii.
6. Bell, Colin and Howard Newby, Doing Sociological Research (London, George Allen and Unwin, 1977), p.21.
7. See, for example, Dainty, Paul, 'Meaningful Management Research', Graduate Management Research, Vol. 1, No. 1 (Spring 1983); Hunt, Shalby D, 'The Nature and Scope of Marketing', Journal of Marketing, Vol. 40 (July 1976).
8. Hughes, John, The Philosophy of Social Research (London, Longman, 1980), p.12. Hughes presents a notably lucid and thorough critique of positivism.
9. Ibid., p.1.
10. Medawar, P B, The Art of the Soluble (London, Methuen, 1967), p.149.
11. Hughes, op. cit. (note 8), p.124.
12. Ibid., p.119.
13. Berger, Peter L and Hansfried Kellner, Sociology Reinterpreted: An Essay on Method and Vocation, (Harmondsworth, Penguin. 1981). p.68.

14. Ford, Julianne, Paradigms and Fairy Tales: An Introduction to the Science of Meanings (London, Routledge and Kegan Paul, 1975), p.5.
15. Homans, George C, Handbook of Modern Sociology, quoted at the beginning of Denzin, Norman K, The Research Act: A Theoretical Introduction to Sociological Methods (New York, McGraw-Hill, 1978).
16. Silverman identifies three assumptions central to theoretical critiques of positivism which are reflected here: '(1) Analytically, we cannot put our commonsense knowledge of social structures on one side in the misplaced hope of achieving an objective viewpoint. In an inter-subjective world, both observer and observed use the same resources to identify "meanings". (2) Methodologically, it should be recognised that a statistical logic and an experimental method are not always appropriate for the study of this inter-subjective world. Random sampling methods and the use of control groups derive from a logic which is not necessarily applicable to a post-positivist universe. (3) Practically, because we are dealing with an inter-subjective world, policy interventions based on a stimulus-response model of change are neither analytically nor politically acceptable. We can no longer, therefore, accept a picture of objective "experts" manipulating "variables" to produce "better" outcomes as tolerable for research practice' (op. cit. (note 3), p. ix).
17. Bonoma, op. cit (note 1).
18. McClintock, Charles C, Diane Brannon, and Steven Maynard Moody, 'Applying the Logic of Sample Surveys to Qualitative Case Studies: The Case Cluster Method', Administrative Science Quarterly, Vol. 24 (December 1979).
19. Goode, William J and Paul K Hatt, Methods in Social Research (New York, McGraw-Hill, 1952), p.331.
20. As in Cohen, Louis and Lawrence Manion, Research Methods in Education (London, Croom Helm, 1980), p.99.
21. Jauch, Lawrence R, Richard N Osborn and Thomas N Martin, 'Structured Content Analysis of Cases: A Complementary Method for Organisational Research', Academy of Management Review, Vol. 5, No. 4 (1980).
22. McClintock et al., op.cit. (note 18).
23. Benson, Douglas and John A Hughes, The Perspective of Ethnomethodology (London, Longman, 1983), p.30.
24. Worsley, Peter and others, Introducing Sociology (Harmondsworth, Penguin, 1970), p.112.
25. Mitchell, J Clyde, 'Case and Situation Analysis', The Sociological Review, Vol. 31 (1983), pp.187-211.
26. Glaser, Barney G and Anselm L Strauss, The Discovery of Grounded Theory: Strategies for Qualitative Research (New York, Aldine, 1967), p.62.
27. Silverman, op.cit. (note 3), p.114.
28. In Mitchell, op. cit. (note 25).
29. Ibid.
30. Useful sources are Hartman, John J and Jack H Hedblom, Methods for the Social Sciences: A Handbook for Students and Non-Specialists (Westport, Greenwood Press, 1977), chapter 11; and

Ackroyd, Stephen and John A Hughes, Data Collection in Context (Harlow, Longman, 1981), chapter 4.

31. Silverman, op. cit. (note 3), pp.161-2.
32. Such as Miles, Matthew B and A Michael Huberman, Qualitative Data Analysis: A Sourcebook of New Methods (Beverly Hills, Sage, 1984), pp.49-78; or Glaser and Strauss, op. cit. (note 26), pp.101-115 (for their constant comparative method).
33. Latour, Bruno and Steve Woolgar, Laboratory Life: The Social Construction of Scientific Facts (Beverly Hills, Sage, 1979).
34. Although it is worth noting that a journal was kept during research by this author as advocated by Mills, recording chronologically ideas on the research, including the data collection. See Mills, C. Wright, 'On Intellectual Craftmanship', appendix to The Sociological Imagination (New York, Oxford University Press, 1959).
35. Mitchell, op. cit. (note 25).
36. Ratcliffe, John W, 'Notions of Validity in Qualitative Research Methodology', Knowledge: Creation, Diffusion, Utilization, Vol. 5, No. 2 (December 1983).
37. Bonoma, op. cit. (note 1).