Developing a More Empirical[1] *Approach to Culture, Attitude and Motivation in Construction Management Research: A critique and a proposal.*

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Abstract

This paper addresses the problem of achieving adequate empirical accounts of culture, motivation and attitudes in construction management research. The usual association of adequacy with objectivity, causality and quantification is criticised and it is suggested that the concept of *verstehen* provides an alternative guide to study. Within this conception, explanations are not primarily causal in form, but concerned with the investigation and explication of meaning. Criteria developed within the discipline of ethnomethodology (EM) and founded in the logic of *verstehen* are offered as alternative standards of rigorous analysis. These consist in a requirement to remain faithful to the empirical phenomenon under study and to eschew speculation and *ad hoc* abstraction.

Introduction

This paper addresses the problem of achieving adequate empirical accounts of culture, motivation and attitudes in construction management research. Usually, in construction

management research, empirical accounts are associated with the notion of objectivity - to be truly empirical, an account should also be objective. It will be argued that in researching concepts like culture, attitude and motivation, it is not possible to sustain a stable distinction between objective and subjective accounts. This is because, in such research, explanation is concerned primarily with conveying an understanding of the points of view of other people (*Verstehen*) (Weber 1933) and not with the isolation of variables and the establishment of correlations between them. Explanations are thus, not primarily causal in form, but concerned with the investigation and explication of meaning.

To put the matter concisely: it is the ideas which managers have, rather than any hypothesised causal variables which directly account for their behaviour. These ideas have three important characteristics:

1. they contain a moral dimension;
2. they are not necessarily in the form of causal explanations, though they may be;
3. they are resistant to measurement.

It will be further argued that, given that this is the nature of the phenomenon, formal methods of investigation and reporting are inappropriate and distorting. To be clear, although formal techniques may be successful in producing data which is statistically reliable, this same data becomes so abstract and empty of content that it loses any claim to empirical validity.

Since the rigour of formal research is necessarily gauged by its conformance to the formal methods it purports to adopt, this poses a further problem: how can we ensure that research is both rigorous *and* empirically grounded? It will be argued that one way this may be achieved is through employing an ethnomethodological (henceforth, EM)[2] approach to construction management research (Garfinkel 1984, Sharrock and Anderson 1986).

Two Problems of Objectivity

The idea of a systematic empirical investigation of a phenomenon is one which is associated with the practices of science. The concept of scientific objectivity denotes an attitude of neutrality or indifference to the data, a suspension of our ideas about what-should-be, in order to better investigate what-is. In this section, two problems of objectivity, as they occur in researching culture, attitudes or motivation, are discussed.

First, although the notion of objectivity is assumed to underlie most studies in construction management research, in practice it is often neglected. This happens when some notion of rational, or efficient behaviour, for instance, is introduced as a standard against which the actual behaviour of managers is judged. Such a confusion between decisions of fact and decisions of value, detracts from the supposed objective nature of the study. It is not suggested that management researchers should not make value judgements or recommendations. But these should be: a) clearly distinguished from the factual study; b) recognised as having the same *logical* status as managers' own judgements.

Point b) requires amplification: it is not suggested that all accounts have the same status. After all, if a researcher has spent three years thinking about a phenomenon, talking to people about it, reading what others have written on the subject, then his/her understanding of that phenomenon is likely to be better than the understandings of someone who has not done these things. Thus, his/her recommendations are likely to be better grounded. However, the choice of methodology gives no *a priori* truth status to the findings. If, after three years research, the researcher's claims to knowledge are challenged by a manager who has spent thirty years living and working with that same phenomenon, then who is to be believed? No generic solution to this problem is available: all claims to the truth value of an assertion need to be evaluated individually.

Secondly, objectivity entails that the world is observed and described from no-particular-point-of-view. Another way of putting this is to say that an objective description is true from any point of view. Descriptions in management studies cannot have this quality. Any description which is proposed is necessarily from a particular point of view. This is because descriptions of the social world must inevitably be composed from within that world. We cannot stand outside society to observe it, in the way we can observe the behaviour of physical bodies from the outside. Furthermore, our descriptions of the social world are composed of the same stuff from which the social world itself is constructed; that is to say, words, concepts, or meanings. Thus, in providing a description of management studies, words, concepts, or meanings are both the topic and resource of that description.

The problem of the disappearing phenomenon

In this section, an EM analysis of an interview transcript will be employed. The intention is twofold. First, to compare the interview transcript data with the more conventional questionnaire-generated data upon which statistical operations are performed. Contrary to those who would see statistical material as 'hard data', it is argued that such data does not reveal, but rather conceals important features of the phenomenon under study. In the reduction of the data to a statistical form, the empirical phenomenon disappears. Secondly, in performing the analysis, the intention is to show what such an analysis looks like and to demonstrate some features of the empirical phenomena which it is capable of revealing.

The transcript is taken from an interview in which a questionnaire was administered. The transcript attempts to reproduce the interview as spoken, including hesitations, false starts and grammatical idiosyncrasies. The respondent is an architect:

[I.1] So are you a manager of people who are not managers themselves, or a manager of other managers?

[A.1] I think I'm a manager of other managers really.

[I.2] Yeh, because they manage people themselves.

[A.2] Because they're self actualisers aren't they? They're people who… manage… jobs, teams, everything.

[I.3] So they would have subordinates under them?

[A.3] Not in a ss, not, not necessarily in a strict.. office structure, but in terms of the way that they do their job. They're...

[I.4] In terms of like the contractors?

[A.4] Yes, the contractors, suppliers, I mean they wouldn't like to think of themselves as subordinates, but I mean in the way that… an architect has to instruct.

Examining a transcript like this will be a new experience for most construction management researchers and many will be surprised to discover how much information is contained in such a short piece. However, such information is only available with a careful and detailed study. In filling out the questionnaire form, it was entered that the respondent was a manager of managers. Such an unambiguous categorisation is necessary for the purposes of such a survey, in order that the responses can be counted to produce the statistical data. In this way, the additional information is lost, in the preparation of the data. An attempt will now be made to demonstrate what some of the features of this information amount to.

The interviewer's first utterance [I.1] is read directly from the questionnaire form based on Hofstede's value survey model. The architect's response [A.1] is sufficient to allow an answer to be filled in on the questionnaire. However, the architect's first utterance possesses two features which, in effect, qualify the answer as not definitive. 'I think', at the beginning of the utterance, denotes that he considers the answer he has given as opinion, rather than fact. It can be read as indicating that the question does not have a clear, indisputable answer - that the answer offered is an *attempted* answer. 'Really', at

the end of the utterance reinforces this impression. It seems to imply that some work has gone into formulating the answer. The meaning of 'really', in this context, would seem to be something like this: 'it could be said that I am a manager of people who are not managers themselves, but on balance this would be wrong'. What the 'really' also shows is that he is designing his response to correspond with the terms of the question. Rather than reject the question out of hand as inappropriate, or misleading, he is saying something like “well, given that I have to answer the question using one of the candidate answers that you suggest, 'a manager of managers' would be the preferred option”.

Two immediate objections might be made to the forgoing analysis. First, it may seem to some that such detailed examination of a casual remark amounts to little more than nit-picking and any conclusions drawn from such an analysis could have no possible significance. In response, it may be pointed out that a growing body of work, dating back to the early 1970's, has demonstrated the intensely organised nature of ordinary conversation and the fact that conversationalists orient to this organisation (Sacks 1972, 1974 & 1995; Atkinson & Heritage 1984; Button & Lee 1987; Cuff 1993). In the face of such detailed organization existing in ordinary talk, it is arguable that it would be wrong to dismiss any utterance, or part of an utterance, as being produced by mere chance.

Secondly, the correctness of the interpretation of this utterance may questioned. This would be a valid criticism, if it were accompanied by an alternative interpretation. Two reasons are offered for accepting the interpretation given above. First, it is an interpretation which makes sense in the context of the rest of the conversation reported. Second, no other viable interpretation is apparent to the author. These reasons apply also to the analysis which follows.

How does the subsequent conversation confirm the interpretation of the architect's first utterance? The interviewer's second utterance [I.2] offers a possible reason for the architect's answer. The architect does not accept this reason. However, instead he offers an alternative [A.2]: that he manages 'self actualisers'. Notice that the architect is

not suggesting that the people he manages do not manage others, on the contrary, they do 'manage teams'. However, this would not seem to be what he regards as most important about them, in the context of the question. For this respondent, it seems, it is not so much position in a hierarchy which defines a manager, but something about the quality of their working experience. After all, as architects, they are principally designers, rather than 'managers', in the narrow sense put forward by the interviewer.

In utterance [I.3], the interviewer makes a second attempt to clarify the architect's answer, in terms of his definition of a 'manager' - someone who 'would have subordinates under them'. Again, the architect demurs [A.3]: 'not necessarily in a strict office structure'.

In utterance [I.4], the interviewer finally offers a formulation which is accepted: they manage contractors. In accepting this formulation, the respondent elaborates it: they manage suppliers as well, but neither contractors nor suppliers would 'like to think of themselves as subordinates' [A.4]. Thus, he points out the ambiguous nature of hierarchy which he perceives in a contractual relationship. There is much that could be said about this relationship, but in the interests of brevity it will left to the reader to supply the features which this ambiguity amounts to.

The analysis above should be sufficient to demonstrate two important deficiencies in the questionnaire data generated by the interview. First, the definition of 'manager' used in the questionnaire was ambiguous. For the respondent, it seemed to refer as much to such professional roles as planner, designer, negotiator, or whatever, as to a direct supervision of personnel. Second, although an architect issues instructions [A.4], the context in which this takes place in not an unambiguous hierarchy. It is not suggested that these facts constitute a discovery. On the contrary, it is assumed that any member of the industry will be entirely familiar with them. Rather, the systematic explication of these features is offered as an alternative to a formal research methodology which tends to make such features disappear.

The Problem of Measurement

No one has ever seen an attitude. We may have witnessed someone 'striking an attitude', or 'adopting an attitude', perhaps. More often, as researchers, what we observe is someone describing their attitudes, or the attitudes of others. (Such descriptions are apparent in the transcript: architects are self actualisers; contractors don't like to think of themselves as subordinates). A similar observation may be made about culture and motivation: these are not objects in the world, their existence is inferred from events which are taken to be evidence of that existence.

Given the nature of these phenomena, the intention to measure them is problematic. When the attempt is made to measure motivation or attitudes, using attitudinal surveys, the research method creates its own phenomenon. A usual procedure is to request the respondent to grade their responses on a scale of, say, 1 to 5. Thus, a statement is given and the respondent is asked to give a graded response ranging from ‘disagree strongly’ to ‘agree strongly’.

It is true that to some extent all management research, indeed all social research, creates its own phenomenon in some manner. Thus, interviewers must inevitably ask questions and the questions asked will be used by informants to formulate their answers. William Foote Whyte (1955) observed in the appendix to his classic participant observation study, that living among the people whose lives he was researching taught him the answers to questions which he would never have thought of asking at the outset. However, as Wagner (1981) has pointed out, even in the case of such intensive direct observation, the phenomenon is created between researcher and informant in a communicative process. In the case of a questionnaire survey, the balance of creative endeavour is heavily on the side of the researcher. The questions are predetermined before the two have met, if indeed they ever do meet. The respondent is instructed how to answer so that the replies are suitable for statistical purposes. The most naturally occurring data, the stories and comments of managers cannot be presented in

mathematical form. Thus, in preparing statistical data, they are often dismissed as merely anecdotal material; seen as, at best, peripheral to the serious business of principled and systematic research.

Yet, what is the phenomenon under study? If it is the attitudes and motivations of managers (and not the preconceptions and speculations of researchers) then these comments and stories represent a strong form of empirical data regarding the understandings which managers have about their situations and the methods they use for achieving their aims within those situations.

Moreover, these understandings, rooted as they are in the real life day-to-day experience of these managers, represent the unforgiving standard against which our own thinking as academics must and will be judged.

Induction and the Documentary Method

Another feature is the problem of induction. How many instance must one witness before one can say that a generalisation has been proved? This problem of logic has been fully explored for the purposes of scientific study by Popper (1959)[3], who rejects the inductive method in favour of the method of falsification. This method, of developing only hypotheses which are capable of being proved false, whatever its utility in scientific research, is not one of the primary methods with which members conduct the day to day business of learning about the social world. Nor is it one which is practically available to the ethnographer. Again, as Garfinkel (1984) has shown, anyone who conducts their social interaction according to the principle of scientific scepticism invites, at best a creative response, at worst an angry one. Instead, researchers and other members use the documentary method to make sense of the social world. This method of seeing events as instances of an overall pattern is again sufficient to its purposes, but does not confer upon its findings any special scientific warrant. Features of this method are elucidated below.

The problem of indexicality

Conventional accounts of the industry, like all such descriptions of social life, are constructed out of a series of generalisations. These generalisations, it has been argued, are achieved in ways which are, in principle, no different to those employed by any member of society to understand and describe her/his social world. In employing descriptions supplied by other members, whether these descriptions are formally, or informally elicited, we take the topic of study and employ it as a resource of that study. As Wieder (1974) makes clear, the use of such methods confers upon the resultant accounts a logical status equivalent to the activities of those they describe. Such generalisations and inferences, though entirely adequate to the purposes for which they are usually employed, do not amount to scientific procedures. They have inbuilt features which are generally recognised and allowed for by members in their use of them, but which tend to be obscured by implicit or explicit claims to the possession of a scientific method.

One of these features is the indexical nature of language. We regularly conduct our communication on the basis that intended meanings will be heard as qualified by all sorts of unstated assumptions. These assumptions are integral to the context in which those meanings are expressed. There are examples of such assumptions in the transcript reproduced above. Thus, the architect assumes that the interviewer will understand terms such as 'self-actualiser' and 'contractor'. We can also see from this transcript what happens when such assumptions become problematic: a repair is undertaken. Thus, from a situation in which the term 'manager' holds a different meaning for the architect and interviewer, a sequence of talk is developed in which the meaning of the term *as it is being used by the architect in this context*, is explicated.

The Problem of Introducing Rigour Into Informal Research Procedures

If, as has been argued, we are to abandon formalised research procedures, we present ourselves with a further series of problems.

First, such qualitative or ethnographic research methods as remain available are time consuming and expensive. In practice, this means that fewer examples of a phenomenon can be studied. Thus, a possible objection to this approach is that it does not demonstrate what statisticians term reliability - the assurance that a similar study among different managers would produce similar results. This is a version of the logical problem known as the problem of induction. However, the problem of induction is logically insoluble. That is to say, there is no magic number of cases, whether eight, or eighty, which can be sampled to give guaranteed reliability (Popper 1959). The only method of achieving an entirely reliable picture of a population is to include the whole population in the sample. Nor is this merely a problem in logic; as political opinion polls continually demonstrate, even the most carefully chosen samples can be misleading.

The demand for reliability then, cannot be taken as an unproblematic criterion of rigour. Consequently, EM analysis rejects this demand, in favour of a more modest one: that the analysis represents an adequate description of the case, or cases, that have been studied. Any attempt to generalise from these cases must be left to our common sense judgements. Thus, the analysis of the transcript above does not establish that attitudes expressed represent typical attitudes among architects. The reader can only draw upon his/her own knowledge of architects to decide the issue of typicality. It is true that some EM studies, particularly in the field of Conversation Analysis, have achieved generalisability (see particularly, Sacks, Schegloff and Jeffferson, 1974). However, this is due in many ways to specific qualities of the phenomenon under study and the type of generalisation attempted; it is not intended to explore this issue here.

The shift of emphasis from generalisability to specifics may seem altogether too modest to many researchers. However, from the point of view of EM analysis, any successful generalisation will depend upon the detailed analysis of very many individual cases, so

that, to put the matter bluntly: we may be sure what it is we are counting. The transcript analysis above demonstrates how a supposedly unproblematic definition ('manager of managers') turns out to be less than precise in an interview situation.

The other quality which statisticians look for in a study is validity. That is to say that what is purported to have been measured has in fact been measured. An abandonment of measurement is advocated here; however, in respect to validity, informal approaches have a distinct advantage over more formal methodologies, as the analysis of the transcript shows. It is suggested that the phenomena highlighted in that analysis are resistant to measurement. Moreover, they are accessible through ordinary methods of social discourse. The ordinary methods by which we converse, discuss ideas and learn about the world are however various and largely unstudied. They are, of course, effective. To abandon the flexible use of these methods, in order to don the straight jacket of a formal research method can only reduce that effectiveness.

Informal methods are, of course, fallible. Their inherent problems are well known, though rarely examined. The research may, for instance, display prejudice, speculation, or misunderstanding. If, as it has been argued, formal methods fail to remedy these limitations, it is no solution to either ignore or accept them. Moreover, the skills involved in applying these methods, albeit possessed by all, are nevertheless variably distributed. The problem is that these skills are taken for granted and used as a matter of course. We have not examined them. We have not attempted consciously to develop these ordinary skills. EM analysis provides us with ways of analysing these skills, thus opening up a new field for study: an enquiry into methods of enquiry.

To recap: in a questionnaire survey the method employed is simply the everyday one of asking questions. By restricting the answers to replies on a printed form, a vast amount of data is eliminated from the research process. Thus, for instance, the mood and context, as conveyed by body language, tone of voice and subsidiary explanation are lost. So also is the opportunity to ask further questions to clarify or extend understanding. The ability of managers to contribute their own ideas, indeed to point out new directions

which research should take, is severely restricted (see Garfinkel 1984, pp 193-197 for a further elaboration of these points). The weakness of informal methods is that they are largely unstudied. The logical next step, then, is to study these methods.

Rediscovering The Phenomenon: The Criteria of Ethnomethodological Indifference and Unique Adequacy

Ethnomethodology is the study of the ordinary methods used by members of society to make sense of the social world in which they live. Thus, an ethnomethodological study of management consists in a study of how managers make sense of the world of management in which they work.

EM pursues the logic of *verstehen* to its conclusion, taking inter-subjectively established meaning as its sole object of study. Such inter-subjective meaning, as a topic of study, encompasses everything which is meant by the realities of organisation, management, economics, research procedure, etc. Such realities are achieved and recognised by members through processes of communication. It is in these processes, which comprise members sense-making methods, that the empirical existence of these realities is to be found. The study of empirically existent organisation (etc.) is for EM identical to the study of these communicative sense-making methods. This study requires the practice of ethnomethodological indifference, both as method for and a criterion of successful study.

The difficulties associated with the use of the scientific notion of objectivity, when applied to studies of culture, attitudes and motivation, have been explored in this paper. But if the notion of objectivity is abandoned, then this might seem to give researchers a license to say anything they like. It may be seen to open the door to a relativist free-for-all, where anyone's opinion is as good as anyone else's. The neutral stance which objectivity is intended to guarantee, is established in ethnomethodology (EM), in the policy of ethnomethodological indifference:

> "to refuse serious consideration to the prevailing proposal that efficiency, efficacy, effectiveness [...] *i.e.* that the rational properties of practical activities —be assessed, recognised, categorised, described by using a rule or a standard obtained outside actual settings within which such properties are recognised, used, produced, and talked about by settings' members." (Garfinkel 1984 p.33)

This formulation explicitly rejects the kind of bias discussed above, where judgements of the 'rationality', or 'effectiveness' of managers behaviour are smuggled in under the cover of objectivity. Moreover, it recognises that accounts are a product of the social setting in which they are produced and inevitably contain value judgements. EM indifference insists that such judgements should originate from and thus be pertinent to, the situation which they describe.

The criterion of unique adequacy insists, similarly, that the methods used to produce a description of a situation, should be those which originate from the situation they describe. Garfinkel gives two unique adequacy criteria, a strong one and a weak one. The weak requirement is that:

> "the analyst must be *vulgarly* competent in the local production and reflexively natural accountability of the phenomenon" (Garfinkel & Wieder 1992, p182)

Thus, to analyse a management setting adequately, we must know what any member to that setting would ordinarily know about that setting. This might be taken as a criteria for adequate ethnography - the author of an ethnographic account of a setting can produce an adequate account only to the extent that s/he appreciates the understanding of that setting which any other member to that setting would know.

A properly ethnomethodological account should also meet the strong requirement of unique adequacy, however. This criterion is founded on the discovery that a phenomenon,

> "already possesses whatever as methods methods could be of [observing], of [recognizing], of [counting], of [collecting], of [topicalizing], of [describing] it, and so on" (Garfinkel & Wieder 1992, p182)

In other words, the methods which members to a situation use to make their meanings clear to other members to that situation are sufficient to the purpose of describing that situation. Furthermore, the use of any other methods must involve some distortion of the phenomenon. The task of EM, rather than to produce an alternative description of the situation, is to describe those methods, as they are used by members, as a means of producing, managing, maintaining the orderliness of, acting in, manipulating, directing, or whatever, that setting, to each other. Such an enterprise represents the final and complete working out of the logic of *verstehen.*

The analysis of the transcript presented above can now be assessed by EM criteria of adequacy. First, the judgements explicated in that analysis are those expressed in the transcript data. The analysis should contain nothing which any competent member of the industry cannot see in the data. Secondly, the methods used in the analysis to make clear what is being said in the transcript are simply an explication, a making clear, of the methods used in the conversation reported.

The Study of Members Ordinary Sense Making Methods (Documentary Method)

EM originated as an attempt to resolve, for sociology, similar problems to those discussed here, by respecifying sociology's topics and questions. Like management studies, sociology takes the descriptions which members of society provide and treats them as evidence for the existence of social phenomena (attitudes, culture, structure, organisation and the like). EM refuses to see these descriptions as evidence and instead treats them as data. Thus the questions becomes not, 'what is the nature of this phenomena which is being described?', but 'how is this description achieved?'.

Here, a similar respecification in management studies is proposed. This would allow us to answer much more precisely questions about the attitudes, motivation and culture of members of the industry. It resolves them into the general question of 'what do members of the industry do?', where this is taken to include what they say.

Already completed studies in ethnomethodology (Garfinkel 1984) have demonstrated the ubiquity of the documentary method of interpretation in the activities of both researchers and all other members of society.

Some features of the documentary method are:

1. Presently occurring talk can alter the understood sense of previously occurring talk.

2. The meanings of answers may be established by asking further questions.

3. Questions are often premised upon preceding answers.

4. Where the meaning of an utterance is unclear, interpretation may be suspended awaiting clarification in subsequent talk.

5. The meaning that a question has for a questioner may be altered in the light of the answer it receives.

6. Answers to questions may provide answers to further questions which were not asked.

These features and others like them, provide the basis for a systematic study of the informal methods of inquiry which it has been argued, must lie at the heart of thoroughly empirical research into culture, values and attitudes in the industry.

Conclusions

Several problems which face researchers into culture, attitudes and motivation have been outlined. These may be summarized as follows:

1. The tendency to introduce value judgements into the analysis under the cover of objectivity.
2. The necessarily situated nature of accounts of social phenomena.
3. The problem of producing adequate operational definitions for the purpose of generating statistical data.
4. The problem of induction.

An alternative approach has been suggested and demonstrated, with reference to a short interview transcript. This consists in:

1. A detailed analysis of singular phenomena.
2. Compliance with the EM criteria of indifference and unique adequacy.

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[1]The intention to develop an empirical approach should not be taken to indicate that the author subscribes to the philosophical doctrine of empiricism. The roots of the methodology outlined here lie, rather, in the phenomenology of Alfred Schutz (*Collected Papers*,vols. 1-3, 1962, 1964, 1966, respectively, Martinus Nijhoff, The Hague) and the later philosophy of Ludwig Wittgenstein (*Philosophical Investigations*, third edition, 1967, Blackwell, Oxford). Schutz's work, drawing on that of Husserl, attempts to provide a cogent basis for social scientific investigation and specifies the nature of empirical phenomena. Garfinkel's re-specification of Shutz's social science establishes ethnomethodology as a treatment of 'practical activities, practical circumstances, and practical sociological reasoning as topics of empirical study' (Garfinkel 1984, page 1). Wittgenstein, working in a different philosophical tradition, demonstrates that philosophical problems arise from confusions in the use of natural language. Thus, Wittgensteinian philosophy consists in a study of language use. This, the author would argues, is an empirical enterprise. The relevance of Wittgenstein's ideas to the issues examined here is explored in P. Winch, 1990, *The Idea of a Social Science and its Relation to Philosophy*, second edition, Routledge, London.

[2]Ethnomethodology should not be confused with ethnography. The latter, often referred to as participant observation in sociological texts, is an approach originating in the practices of C19th anthropologists. The term ethnomethodology was coined by Harold Garfinkel, circa 1960, to refer to the approach taken by himself and a group of colleagues. In some of its applications, at least, it may be regarded as a radicalising of ethnography (Weider 1974).

[3]It is not intended to imply that Popper's solution to the *logical* problem of induction represents the last word on the larger question of scientific theorising. Subsequent thinkers have been at pains to point out that, in practice, science neither does nor can conform to Popper's prescribed methodology. Nevertheless, inasmuch as scientific practice deviates from Popper's methodology, the problem of induction remains unresolved.