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The culture of the industry and the culture of research

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Culture is increasingly cited as being in need of change if the UK construction industry is to improve its efficiency and productivity. The paper argues that the concept of culture is amenable to radically different treatments and that the research community must recognize the consequences of this choice if it is to make a useful contribution to bringing about the desired change. The dominant research paradigm in construction management is examined and compared to an alternative approach. The consequences attendant upon the choice between these two are explored with reference to four phenomena: a study of quality in the construction industry, Japanese innovation in management, Deming's concept of total quality management and the situation of the site engineer. It is concluded that the dominant rationalist paradigm tacitly endorses existing attitudes and that if researchers are to have a role in changing the culture of the industry, then the culture of research must change also.

Keywords: Quality, methodology, research paradigms, culture.

Introduction

The publication of the Latham Report (Latham 1994) has again turned attention to the institutions of the construction industry and to the culture that has developed within them. The report reads very much like an ultimatum. Given overcapacity and foreign competition, attitudes must change or else! In this paper we wish to consider the role that the community of researchers can play in the change that is asked of the industry. Our argument is that the research community is largely dominated by a set of methodological assumptions which we will call the rationalist paradigm. These have the effect of tacitly endorsing those very attitudes that are said to be in need of change. We propose, therefore, that if we are to illuminate what is going on in the industry, we are in quite as much need of reviewing our culture, as are the practitioners whom we research.

The rationalist paradigm consists of several methodological assumptions and techniques which are largely implicit and unchallenged in conventional construction management research. Chief among these is the attempt to import into management research the distinction which is drawn in natural science between subjective experience and objective reality. The use of this analytic device is intended to produce a neutral description of some field of human activity. Such a field is conceived of as a natural system. The aim is to identify the constituent elements of the system, show how they interact and show how the system as a whole interacts with its environment.

The attraction of this paradigm, applied at both the individual and organizational levels, is that it seems capable of providing specific and scientifically valid statements about the probability of human effectiveness and of organizational success and failure. It offers to show how key variables are related and how they can be managed to achieve successful outcomes.

In brief, our contention is that rationalist assumptions and ways of thinking about organization have become institutionalized, deeply embedded within the culture of research. This is not a novel view and much of the conventional wisdom referred to above has long been challenged by a vigorous, if minority, tradition within the fields of sociology, social psychology, philosophy and anthropology. In addition, a new wave of writers on organization and management is renewing the attack on rationalism, in favour of an interpretativist approach (Checkland and Scholes, 1990; Morgan, 1992; Hamel and Prahalad, 1994; Senge *et al.*, 1994).

Our argument is set out as follows. First, we will explore, in turn, the consequences for research of the rationalist and interpretativist paradigms. Having set out the choice before us, we will examine its consequences, with reference to four examples. First, we will comment upon a research project in which one of us was involved, demonstrating the limitations of the conventional methodology and showing how these can be overcome. We

then look at the revolution in Japanese manufacturing promoted by Ohno, highlighting the parallels between Ohno's devolution of responsibility to operatives and our own 'bottom-up' research perspective. Thirdly, we consider the career of Deming, showing how his innovative approach to quality fell foul of the rationalist paradigm. Finally, we will provide an example of the kind of view which an interpretative approach provides, by making some observations upon the situation of site engineers.

This paper is somewhat unconventional in construction management discourse. It is not a technical report, offering to further extend knowledge of the industry, nor is it even an attempt to outline a particular methodology. Rather, it is an attempt to bring into question some fundamental assumptions, which are accepted among the research community. What we are asking of our readers is not an easy thing; it is to re-examine some beliefs which are so fundamental to the practice of management research in this industry, that they are rarely even made explicit, let alone made subject to criticism. The difficulties of such a task have been often noted. Hamel and Prahalad (1994), for example, wrote of deeply ingrained habits of thought among managers, which they refer to as 'managerial frames'. Viewing the world through such frames, managers routinely exclude alternative ways of seeing a problem. Furthermore, 'Managerial frames become part of the organizational fabric as they are enacted through the firm's administrative structure and processes' (Hamel and Prahalad, 1994, p. 50). In this way, managers can blind themselves to future possibilities. Hamel and Prahalad (1994) conclude that managers need to unlearn much of their conventional wisdom and note that this is far more difficult than learning because, for most of the time, we are not aware of what we have learnt. We just act on it.

The dominant rationalist paradigm

We have observed that the chief assumption of the rationalist paradigm is the distinction which is drawn between subjective experience and objective reality. Within natural science, this distinction appears as unproblematic. This is because the concepts which natural scientists use are 'first order constructs' (Schutz, 1971). They constitute the imposition of meaning upon a natural order which, in itself, is meaning free. To put the matter simply, if we attribute the fall of an apple to the force of gravity, then neither the apple, nor gravity has a voice with which to contradict us. The situation of social researchers is altogether different. They come upon a world in which meaning is already made. Thus, their concepts must either reflect or rival those that they find – and if the latter, then by what authority can researchers claim the superiority of their own version of events?

This question is usually answered in terms of the second fundamental assumption, that the correct way to describe and analyse situations is through the employment of causal explanation. If 'variables' can be isolated and correlations established, then causal implications can be drawn. These formulations can then be tested through the application of the experimental method. We will be arguing in the next section, that such an approach is inimicable to the special demands of social research.

Thirdly, simplified models of reality are created out of these putative causal relations. These purport to show what functions each of the elements perform relative to the others and how they are coordinated to achieve specific objectives in the environment in which they occur. Thus, it is assumed possible to describe patterns of contingent relationships that hold between the variables that contribute to organizational survival or extinction. Organizational activity is conceived of in terms of systems, a standard distinction being between the 'technical' and the 'social' or 'human'.

Consistent with the aim of the rationalist paradigm – to abstract and model fields of social action as natural systems – two sets of concerns, commonly referred to as the 'technical' and 'contractual' systems have received considerable attention. Since the approach makes an essential separation between a technical system and a human system, the intention is to perfect the (abstract) technical system and then fit the human system to it. This is the equivalent of separating 'the dancers from the dance', as Scarbrough and Corbett (1992) described it. Despite choreographers, dances cannot exist without dancers. As we will attempt to demonstrate, this abstraction leads to confusion and impotence in the recommendations of researchers.

The assumption is that if the system designer works hard enough he can and should aim to make it 'idiot proof'. Thus, the search for 'efficient' checking techniques and tools is an important part of the agenda (for example, Parsons, 1972; Chase and Federle, 1992). This approach is nicely summarized by Welfare (1987):

> The road to perfection is by investigating the mystique which surrounds the physical placing of concrete and reducing it to a measurable, disciplined and easily repetitive activity. (pp. 32–33)

As will become apparent, in our discussions of Deming and Ohno, below, this emphasis runs counter to much management thinking, especially as it concerns the management of quality.

On the technical front, the problem is defined as the need to articulate in exact and comprehensive detail the intrinsic technical logic of the construction process. The attempt is to construct a blueprint of how the process can

be made to work most efficiently. The blueprint provides management with a control tool in two senses. It is intended to provide a detailed description of exactly what goes where, when and how, against which implementation can be checked and the necessary remedial action taken. In the more ambitious versions of this approach, the intention is to hone the tool so perfectly that nothing can go wrong. Secondly, as a consequence of its supposed performance of the first function, it acts as a device to legitimate management control. Offered as the pure expression of instrumental rationality it brooks no objection.

Proponents do make concessions concerning the completeness of the system that they can construct and the necessity of accommodating the technical system to the social or human systems. Nonetheless, many commentators in construction management research seem to hold this view in its extreme form, what we may call the 'engineering fix'. It holds that, given the vagaries of human nature and the uncertainties of human conduct, it is possible, in principle, to understand and describe the inherent characteristics of the technical system. With increasingly sophisticated tools to help, it becomes possible to at least get that right.

If concern on the technical front is to devise a system describing what should happen, on the contractual front, the concern is with how relationships can be controlled so that what should happen does happen. As is frequently noted, the contract assumes special significance in construction. This is due to the cost, complexity, novelty and uncertainty of many projects and, therefore, to the necessary involvement of several different parties experiencing different levels of risk. A central issue becomes how should rights and responsibilities be allocated.

It is significant that the point of reference or model for considering the nature of these conventions is often the idealized, unitary organization with a centralized source of authority and control, more or less completely responsible for designing and manufacturing a product. In other words, the model is that of the legal–rational organization or bureaucracy, classically described by Weber (1966) and which he saw as a shrine to the ascendancy of Western forms of cause and effect reasoning.

Thus, some participants in the perennial debate that goes on about the distribution of contractual rights that will best promote quality argue that it should be the contractor who is in control, i.e. should be at the top of the, in fact, non-existent hierarchy (Lombard, 1975; Breakwell, 1985). Others say it should be the design professionals (Issak, 1982) and yet others, the client (Bubshait and Al-Musaid, 1992). It is evident that every analysis of the shortcomings of the industry from Banwell to Sir Michael Latham himself, looks to new forms of procurement contract for a solution (Banwell, 1964; Latham, 1994).

Our concern here is that research on contracts can easily become dominated by the rationalist diagnosis, which dwells exclusively on tools of control. Its attention to the formal provisions of contract tend to ignore all the taken-for-granted conventions of everyday life which make any contract possible (Durkheim, 1933). As is frequently observed on construction projects, a good project is one where the formal provisions stay on a shelf gathering dust. Meaning is exchanged and shared without recourse to them.

The rationalists takes for granted the interpretative frameworks that are used to organize and communicate perception, thus effectively ignoring them. Instead of investigating the interpretations of others, they simply assert one of their own. Where the rationalist perspective is asserted by practitioners, supported by the researchers who have articulated it for them, it obstructs understanding of what is actually going on in the relationships between people. From the point of view of those trying to bring about change, possible futures are foreclosed. It is not surprising, then, that many of the practitioners we interview, express cynicism about management theory and our role as researchers.

It is important to appreciate that this explanatory framework is a consitituent of the current situation and not simply a description of it. It is a feature of the industry's culture. The kinds of answers it generates to the question, 'why does this practice persist?' are, 'because of incomplete control systems, inadequate supervision, weak management' (technical) or 'contracts do not adequately specify requirements and obligations and are not adequately enforced' (contractual). Thus, the answers are prescriptive and the remedies prescribed are tighter more elaborate regulatory systems.

From this paradigm has issued most of the conventional wisdom on the design and management of organization; it offers specific items of advice as to what managers can actually do. (How that advice is supposed to be taken and how it is to be interpreted in particular contexts is another matter.) The impact of the paradigm has been important in two ways.

Firstly, it has legitimated the control of some people over others. The point is taken up later in the paper but, for the moment, it is sufficient to say that the rationalist paradigm may provide a justification for a chosen course of action. It enables a manager, for example, to say 'I have considered the relevant variables which are related in the ways that research has established. I am simply enacting demonstrated principles. Science is neutral.'

Secondly, as Morgan (1986) has pointed out, the paradigm has provided a language for conceiving of and talking about organization. Researchers and managers have become accustomed to using such terms as 'struc-

ture', 'culture', 'motivation' and so on. These terms have entered the everyday experience of practitioners and are used to organize and communicate that experience. They serve a purpose and, insofar as they are used, one may assume that they are considered adequate. In contrast, given the more stringent requirements of reporting research, the way they are used within the rationalist paradigm is a matter of some confusion and will be considered below.

An alternative: the interpretative perspective

One strand of the interpretative tradition originates in the work of Weber (1993) and his conception of *verstehende*. This German word translates into English as 'understanding'. It refers, however, to the understanding of another's point of view, rather than the kind of causal understanding which is the aim of the rationalist approach. It amounts to a recognition that to fully understand people's attitudes and beliefs, is to understand how they perceive the world (Weber, 1933). To attempt to build attitudes and beliefs into causal theories about people's behaviour, to create simplified models of that behaviour, to judge it from a putatively objective point of view, is to distort reality. It is to depart from the 'insider's view', which the *verstehen* method seeks to achieve. A great deal of Weber's work was an attempt to relate this *verstehen* understanding to the kind of conventional model building we have discussed. As will become clear, we feel that he failed in this attempt.

A second strand to the tradition originates with the work of the American social psychologist, G. H. Mead and continued through the work of the Chicago School of sociology. Even more than Weber, Mead places interaction between individuals at the centre of his analysis. Society is seen through the eyes of the people who live it. The valuable body of work which was produced directly by or under the influence of the Chicago sociologists, is too vast to review here. We will restrict ourselves to the consideration of one paper, Blumer's (1967) cogent critique of variable analysis in social science.

Blumer (1967) argues that variable analysis, such as that attempted in the study we consider below, is inadequate for the understanding of social processes. The kind of findings that are produced in the study of social situations are of a local nature and they have no general validity. In order to understand such findings, it is necessary to understand the context in which they are produced. Preparing data for statistical analysis, however, involves isolating variables and abstracting them from the context in which they are produced. This involves two major drawbacks. First, it overlooks the fact that a process of interpretation intervenes between the independent and dependent variables. As Blumer (1967) observes

> Interpretation is a formative or creative process in its own right. It constructs meanings which... are not predetermined or determined by the independent variable. (p. 87)

Secondly, the need for clear-cut, unitary definitions of variables leads to an oversimplification of complex phenomena. We shall be exploring the consequences of this latter point in the four examples that follow.

Since the 1960s, the interpretative perspective has been greatly enhanced by the study of ethnomethodology initiated by Garfinkle (1967). By studying the ordinary, everyday methods used by members of society to discover, establish and communicate the facts of social life, Garfinkle (1967) initiated a further radicalization of social research. Abandoning any attempt to utilize scientific methods of research or theory building, he concentrates instead on the study and utilization of these 'members' methods'. It is these methods which, along with Garfinkle (1967), we see as the only viable ones for social research. They are techniques in which every member of society is experienced and competent. It is the researchers' task to develop their own proficiency in using these methods and to increase the rigour with which they are applied continually. There is no space here to enter into an exploration of the consequences of this belief. For a penetrating study of members' methods, see Garfinkle (1967), in particular Chapter 5. For a fuller account of an interpretativist approach to construction management research, see Rooke and Seymour (in press).

Although ethnomethodology is a programme for empirical research and not a critical perspective, many of the insights it has achieved serve to undermine the rationalist paradigm. For example, Zimmerman's (1971) study, highlights the fact that members of an organization use the organization's rules in a selective manner. Judgements are made on an *ad hoc*, day to day basis, which are intended to maintain the smooth running of the organization and get through the work in hand. As Bittner (1973) observes

> formal organisational designs are schemes of interpretation that competent and entitled users can invoke in yet unknown ways whenever it suits their purposes. The varieties of ways in which the scheme can be invoked for information, direction, justification and so on, without incurring the risk of sanction, constitutes the scheme's methodical use. (p. 272)

Similar developments have taken place in the fields of anthropology and philosophy. Early European anthropologists saw the peoples that they began to study in the

nineteenth century as having primitive or undeveloped versions of their own cultures. Cultures that featured magic and rituals were expected to evolve in the same way as their own. The rational, scientific demonstration of cause and effect would replace magic and ritual.

However, as a result of ethnographic techniques which involved living amongst these people, it became clear that they were not victims of delusion. In the conduct of their everyday lives they were patently practical people who understood and used cause and effect in a way consistent with the rational explanations held by the anthropologists themselves. Ritual and magic was then seen as being functional at the affective level. In the famous case of the Trobriand Island fishermen, for example, since they made sure that their boats were seaworthy and that their nets were sound, they were seen to have a clear causal understanding of the consequences of these measures. The ceremonies which the fishermen undertook before going to sea, rather than being intended to have a direct causal effect on ensuring calm weather, performed the function of reassuring them and strengthening their sense of group identity.

More recently, philosophers and anthropologists have developed this point still further. In his seminal critique of both traditional anthropology and of Evans-Pritchard's more enlightened view, the Wittgenstinian philosopher Winch (1964) proposed that the practical (in Western terms) and the ceremonial or affective are all of a piece and it is to think unsystematically if one imposes a partial and selective explanatory framework. There is no objective, value-free point of view, argues Winch (1964), from which the rationality of a culture may be judged. On the contrary, non-scientific ways of thinking, such as bodies of religious or magical belief, have their own rationale, by which truth or falsity of an assertion may be judged. Thus, we may say that the true naïvety lies with the rationalist researcher, who presumes to select from a complex, integrated culture only that which they understand and to see the rest as mumbo-jumbo. While it is perfectly appropriate, in the context of the rationalist paradigm, to look for strictly causal relationships, it must be recognized when such analysis addresses only part of the problem. Moreover, while we in the West pride ourselves on being able to distinguish cause and effect, we too ritualize ways of dealing with events and make sense of them in non-causal ways. Some examples of this are provided below, in the exploration of the situation of the site engineer.

To summarize, the interpretative paradigm takes the points of view of individual practitioners as the focus of research. It recognizes that the standpoint and values of researchers have no logical priority and that these easily become the occasion for bias and special pleading. It further recognizes that this partiality on the part of the researcher cannot be entirely remedied, but must be kept continually in view, for the capacity to understand the values and beliefs of others is contingent upon being a social being with values and beliefs of one's own (Weber, 1966). Nevertheless, a deeper understanding of others is possible, given sufficient time and patience. Given knowledge of the beliefs and purposes of those whose activities we research, an accurate and useful account of them is possible.

The interpretative paradigm generates several kinds of answers to the question, 'why does this or that practice exist?' The values of those who perform these practices are their conscious reasons for maintaining them are made clear. Their reactions to attempts to change these practices and values may be explored. Their own views on how improvements might be made can be elicited. Views about the practices of others might also be sought. Thus, the answers are descriptive rather than prescriptive, the description providing a sound empirical basis from which prescription can then be made.

The rationalist paradigm as an obstacle to effective research

The study with which we will be concerned examined specific structural features taken from a sample of 25 projects. It was designed to establish the quality of the finished construction, defined as the extent to which it conformed to the specifications provided in the design and what organizational features, work practices and the like, correlated with these outcomes. It is not our purpose here to report the results of the study, some of which have been published in Shammas-Toma *et al.* (1994) where a fuller account of the study is given. We merely wish to highlight some methodological issues, for the purpose of discussion.

The projects varied in type of structure, size and contract used and were located throughout the UK. The research was carried out by a team of engineers and a social scientist. A sample of structural elements on each site was monitored throughout the construction process. The completed structural elements were subjected to a series of tests and measurements. Photographs were taken of work in progress by the fieldworker. Informal interviews were conducted with operatives, foremen, engineers and managers during the course of the work, about what they were doing, why they were doing it and so on. The period of investigation for each site was, on average, 1 month.

On the basis of the evidence established through the monitoring process, defects and the reasons for them were identified. Sites were allotted scores for quality achieved (incidence of defects) and causal explanations of the defects were established. Defects were distinguished into two classes depending on their origins.

Within the first class, referred to as 'management-controllable defects', were placed those defects that were judged to have originated in impractical design, such as a clash in the position of components, components having been despatched to the site by suppliers wrongly dimensioned or out of tolerance and inappropriate methods of construction adopted by contractors or the supply of inappropriate materials.

A second class was 'operative-controllable defects', judged to result directly from operative action, such as misplacement or omission of components. In other words, defects were classified as those which appear during construction but originate before it (management-controllable) and those which occur during it (operative-controllable). The categorization of defects into the four types of cause, was carried out by an expert panel comprising two structural or civil engineers.

The panel's task was to reconstruct what had happened, using the field researcher's measurements, observations and reported discussions with participants. This procedure, we suggest, has two fundamental weaknesses. First, while they may have been able, on matters of technical or engineering judgement, to accomplish this without too much distortion, to establish the sequence of decisions and events that had led to the technical defects and causes presented a different order of problem. Reconstructing what had happened took the panel far beyond purely engineering considerations into the realms of organizational practice, contractual liability, human motive and so on. They had no explicit methodology for dealing with these issues.

For example, a typical event needing to be evaluated was the construction of a reinforced concrete lift shaft on a building project. The reinforcement box was constructed with the appropriate placement of spacers. The form fixers (who provided the plywood for the forms) had difficulty fixing the form because the box was slightly over size. Was this the designers' fault or the suppliers? Should the contractors be blamed for not checking? In the process of making the box fit, the form fixers knocked off most of the spacers. As a result, the finished construction did not meet the specifications. Again, should this be attributed to poor workmanship, to ignorance or to carelessness on the part of the operatives? There was no apparent consultation between or attempt to coordinate the steel fixers and form fixers. The researcher also observed that although the site agent saw the event which led directly to the defect, no corrective action was taken.

Resolving these issues calls for judgements of right or wrong. The rationalist paradigm, derived as it is from the methodology of rational science, is concerned primarily with judgements of is or is not. It makes no explicit provision for answering questions with an ethical or political dimension. Such necessary judgements are not provided for within the rationalist paradigm. This raises awkward questions. What significance can be attached to the conclusions that the panel was able to arrive at? What kind of basis can be established for the kind of judgements at which they arrived?

Secondly, even the technical decisions made by the panel were made in the abstract, with no real involvement in the situation and no real consequences attending them. The technical decisions made on-site are made by people very much aware of possible consequences, especially contractual and financial ones. It was found later through interviews with site staff that achieving the specifications with regard to the particular feature under investigation was not seen as a priority. Greater priority was given to finishing the job in as short a time as possible. This was attributed to the fact that site staff were under pressure from head office and that, anyway, they did not have enough engineers. Further, it was found that steel fixers were employed on an individual basis as labour only and that far from there being consultation between them and the form fixers, there was no possibility of it. By the time the latter started work, the steel fixers had left for another site. Finally, it was alleged that even if it had been the right size it was a 'damned stupid' design which would have been difficult or impossible to comply with anyway.

Even insofar as the panel was able to empathize, they were faced with the problem that the meaning of the data is dependent upon the context in which they are produced. Since the methodology involved forced the panel to work with data which were decontextualized, their grasp of the problems involved was necessarily vague. In order to compensate for this, they had imaginatively to reconstruct the situations by drawing on their own experience. Imaginative construction and reconstruction of events is a necessary part of the documentary method. However, the whole texture of informal arrangements and tacit agreements that characterize a work-place and particularly a construction project were largely unknown to the field researcher and entirely unknown to the panel. Viable assessments of these matters can be made only with the situated knowledge of those actively engaged or, alternatively, by the committed ethnographic researcher, who has taken considerable time to get to know the participants and their situation.

It will come as no surprise that the accounts, explanations and rationalizations offered by different individuals varied widely. People blame each other: accusations are made about incompetence, laziness, dishonesty and so on. While it was possible at times to verify the factual accuracy of what a respondent claimed had occurred, why it had occurred and whose responsibility it was supposed to be, for much else of the time, there was no way in which the accuracy or truth of a given account could reliably be established. In such circumstances, the

only safeguard against bias is the rigorous application of *verstehen*. There is no single, objective or neutral account to be had.

The nature of the problem: the relationship between research and practice

At this point in the research, one of us was introduced to Womack *et al.*'s (1990) *The Machine that Changed the World*. In particular, he was struck by the book's description of Ohno's insight regarding the consequences of engineering-dominated, mass-production thinking. This thinking results in a production system where no reference is made to those who operate it apart from how they have to be controlled in order to fit it.

The book (Womack *et al.*, 1990) is primarily a study of the changes that the Japanese brought to automobile production. One of the key figures in these changes was Taiichi Ohno who saw the inefficiencies built into the mass-production system. For example, the system as a whole could only be overseen from a synoptic point of view. Furthermore, it was assumed that if the workers were allowed to stop the line, they might do so for various personal and nefarious reasons. Ohno recognized how disastrously inefficient this was, with errors piling upon errors till the system boundary was reached. He introduced a facility for stopping the line at each work station so that problems could be remedied as they occurred. He went further. Womack *et al.* (1990) wrote

> In mass production plants, problems tended to be treated as random events. The idea was simply to repair each error and hope that it didn't recur. Ohno instead instituted a system of problem-solving called 'the five why's.' Production workers were taught to trace systematically every error back to its ultimate cause (by asking 'why' as each layer of the problem was uncovered), then to devise a fix, so that it would never occur again. (p. 57)

There is a clear parallel here to the research problem described above. No engineer or panel of engineers was able properly to adjudicate on why defects occurred; it required the situated knowledge of those on the spot. The research procedure was the product of a particular mode of thinking whose negative consequences Ohno had astutely noticed. It was perfectly respectable within the rationalist paradigm. So long as due care is taken according to its conventions, the researcher is expected to establish the facts of what happened. That is what research is supposed to provide and, to an extent, it does so. However, the procedure was severely limited in its scope, since it could not address the reality of the data, which far from constituting a single, describable course of events, consisted of a variety of partially conflicting versions. From a methodological point of view, how these different versions are managed, negotiated, resolved and so on, is the stuff of culture and should be the object of our attention. From a practical point of view, the rationalist perspective directs attention away from a conclusion like Ohno's, which points to the need to authorize operatives to diagnose and act on their diagnosis. The alternative, not adopted by Ohno, is a putatively objective viewpoint, offered, in fact, from an engineering perspective and one which would have marginalized alternatives.

Whilst not wishing to make too much of the Japanese initiatives in re-engineering, we see within them an implicit recognition of the limitations of the rationalist paradigm, its abstractness and partiality, when used in the attempt to understand and explain complex human processes. To accomplish this, it is necessary, to employ a methodology that addresses the totality.

In considering Ohno's innovation, care must be taken in interpreting what it means and what inferences for practice can be made. We believe the following inferences can be made. Firstly, someone who is not a member of an organizational setting, risks not understanding what is going on and, thus, imposing an irrelevant or inaccurate interpretation of it. Secondly, although a work process was radically re-engineered, in a particular setting, we need to know much more about that setting before making generalizations about practice. In particular what was it about that setting which enabled the adoption of the change? Thus, we need to know much more about the circumstances of change. For example, was the pursuit of causes in the 'five why's' so firmly directed at the technical, as is implied or were there instances of buck-passing, covering of backs, denials of alleged causes and so on, such as were present in the cases our research considered? If so, how were they adjudicated? Given the probability that many answers are provided in the mass of research carried out by Womack *et al.* (1990) and point, say, to a highly cohesive corporate culture (Dore, 1973) where there is great incentive to look only for technical causes or where finger-pointing is untainted by the suspicion of personal or political motive, there remains the problem of the genesis of such a culture. To what extent could it arise in the context of different traditions, expectations about work, democracy and individual freedom (White and Trevor, 1983)? It is reported, for example, that there is a less acceptable face to Japanese industry, where dissent is met with intimidation and an appearance of harmony masks the resentment and jealousy of fellow workers (Junkerman, 1982).

We will illustrate the importance of this second point with reference to the career of W.E. Deming.

Deming and the limitations of the rationalist perspective

Deming's career can be seen as a struggle to come to terms with the issues referred to above concerning susceptibility of national cultures to change and so provides a highly significant comment on them. Deming is frequently cited as the proverbial prophet in his own country. A lukewarm response to his ideas at home in the US contrasted with a warm reception in Japan. Then, due to the reputation his success had brought, Americans too clamoured to adopt them. This enthusiasm waned, however, as the radical nature of his approach became apparent. He is now widely recognized as an originator of the concept of total quality management (TQM) and a key figure in the remarkable post-war industrial success of Japan. How far the full extent of his message will register only time will tell.

As a statistician, his abiding and central idea was that the key to quality is to control variation in the production process and so reduce variation in the finished product. This statistical approach to quality lies firmly within the rationalist paradigm. However, the task of controlling the variation, which the use of statistics makes it possible to identify, opened up a view of organization, its relationships with customers and suppliers and the role of management, with quite revolutionary implications. The concern with the systemic causes of variation shifted the emphasis from the structure of organizations to the processes that they were intended to perform. This involved asking radical and often unpalatable questions about the efficacy of existing structures and the values and expectations of the people, their attitudes (especially those of managers) and the need to change them. The famous 14 points are an attempt to articulate this realization; to state the key principles for a philosophy of change. Though statistics and the other products of the rationalist paradigm provide the tools for this revolution, their potential can only be realized to the extent that the organizational expressions of the paradigm are overturned.

The relative failure of Western companies to adopt Deming's ideas has been attributed to the power of vested interests. This diagnosis is consistent with what we have suggested – people who have power, authority and status within existing structures are not anxious for them to be shaken. However, we are drawing attention to the more subtle and deep-rooted reason for resistance to change: the very ways we think about, investigate and understand organization forecloses certain conclusions about them and veils the possibilities of what they might become.

Looked at from the perspective of the rationalists, Deming's 14 points seem like a series of contradictions and indeed he has been criticized precisely for not providing clear-cut programmes for how his ideas are to be realized (Flood, 1993). Such criticisms demand the impossible. Deming's (1986) fundamental propositions are not intended as a set of rational cause–effect procedures or algorithms; they do not offer a clear set of instructions. On the contrary, they contain paradoxes. From the rigid logic of the control loop, where objectives are clearly stated, methods monitored and shortfalls remedied, this is an anathema. Constancy of purpose and a philosophy of change, for instance, are difficult to reconcile. Yet, both are integral to the first of Deming's 14 points:

> Create constancy of purpose toward improvement of product and service, with the aim to become competitive and to stay in business, and to provide jobs. (p. 23)

For Deming, the intention to innovate should itself be a constant purpose. Furthermore, constancy of purpose (to stay in business) will provide the secure atmosphere in which innovation can flourish. These terms, then, do not identify measurable variables, which are causally related. The logical relations between them are of a different nature.

The questions that Deming's work raises are numerous. For instance, point 12 recommends the removal of barriers to 'pride of workmanship'. On the other hand point 5 calls upon companies to 'Improve constantly and forever the system of production and service, to improve quality and productivity, and thus constantly decrease costs' (Deming, 1986, p. 23).

Is pride in workmanship consistent with the collective pressures of continually refining procedures? It is a much noted aspect of innovation that old skills become redundant. To what extent can the two prescriptions be reconciled? Which should take precedence when they cannot? These are questions which can only be answered in particular contexts in particular firms.

Furthermore, the answers must be measured against the injunction to 'Drive out fear, so that everyone may work effectively for the company' (Deming, 1986, p. 23).

Again, point 13 calls for 'education and self improvement' (Deming, 1986, p. 24). But is education merely to mean indoctrination, as sometimes appears to be the case as, for instance, in the work of Peters and Waterman (1982). If it is not, of what does it consist and for what purpose? Clearly, there are value judgements to be made.

Deming's increasing attention to the fact of organization as a human process, replete with conflict and contradiction and, in all this, the centrality of meaning and ways of thinking, were simply too difficult to reconcile with rationalistic Western conceptions of organization and management's role in it. He was forced to address the multiple, perceived realities that constitute organizations and the intrinsic contradictions that

this view reveals. His advice is in terms of paradoxes because that is all it sensibly can be. His later American experience caused him to widen his perception, something he had not been required to do in the atmosphere of receptivity he found in Japan. He was forced to look beyond the more operational aspects of his work and consider exactly what it was about the Japanese situation that had led to this receptivity. How far Deming was himself constitutionally capable of pursuing the research agenda – the totality of Japanese culture, to which his experience pointed, it is not necessary to speculate.

What we insist upon is that it is this same challenge which confronts the community of construction management researchers today. Charged with the task of reporting upon the very culture from which it originates and armed with methods developed for a very different purpose, it faces an impasse. Previous training leads researchers and sponsors alike to believe that research can and should produce a body of objective facts. The nature of social process is resistant to such an endeavour. Inasmuch as such facts are produced, they represent a distortion of the true picture, necessarily constituting a partial account. This distortion has a double effect, as well as preventing insights into the nature of the management problems it seeks to address, it acts to reinforce existing attitudes. Thus, instead of facilitating the change for which Latham (1994), for example, has called, it acts as a profoundly conservative force. Breaking out of this circle will not be easy. In order, even to question the existing culture, it is necessary to escape the restraining bonds of allegiance, interest and habit of mind.

Unless culture is treated in the terms that it requires rather than in the terms of the rationalist paradigm, culture will be projected as an entity that can be managed and controlled just as it was once believed that organizations could be controlled, like rationally designed machines. Deming was faced with the fact that a culture is a way of interpreting and constructing a social reality. Statistical tools, which lay at the core of his approach, neutral as they seem in themselves, take on different meanings depending on the culture and context in which they are put to use; who uses them, for what purposes, their assumed limitations, the inferences that may be made from them and so on. Similarly, in diagnosing what is currently happening in the UK construction industry, we must take great care about the ways in which the tools of the diagnostic process, themselves an expression of that culture, imply the remedies which the culture already has to hand. Breaking the mould is difficult and there are already ample signs that, in responding to the challenge of national cultures which seem to avoid the problems of our own, the preferred conclusion is that culture must be managed and controlled. This rationalistic view of what culture is, presages a conceptual and practical error as great as the belief that organizations themselves can be controlled like rationally design machines.

An interpretativist view of the engineer

We have stated that, in the interpretative approach, the researcher's aim is to report the perspectives of the participants in particular settings. The concern is with how and why they do things as they do. The aim is not to report any single truth. Rather, it is recognized that any particular report or account of how and why things happen is produced for particular purposes, audiences and circumstances and is tailored accordingly. This assumption holds as much for the accounts of a researcher as the people s/he is researching. The accounts that the researcher offers are intended to reveal how people construct their world, what meaning they attach to things and events. S/he is concerned with what people think they are doing, not what they may be said to be doing from any, claimed neutral standpoint (Vaill, 1975). From within the interpretative paradigm, what people think they are doing is seen as the very crux of what is meant by culture. Culture is, first and foremost, a method used by people with reference to other people (Eglin, 1980). It is not to be treated as some objective totality or system 'out there'.

With respect to the research material presented above, regarding the achievement of structural quality, the interpretativist researcher is interested in knowing what meaning the events which the research considered have for the participants involved. What methods do they employ to manage and cope with them? How do these meanings and methods contributes to what might be called the culture of the industry?

So, for example, one might expect the interpretativist researcher to make the standard observations that on a project there is a division of labour and authority, many firms are involved and people come and go. For site engineers or engineer–managers this is taken for granted as a necessary feature of their working life. They may envisage alternatives; know of the talk that goes on about 'stabilizing relationships', 'defragmenting' the industry, the need for attitudes to change and 'get the culture right', but for the present they have to work out ways of living with it.

One vexing problem is the very nature of their authority and the objectives it is used to achieve. One of the many ambiguities they must juggle with, is that they are expected to apply their engineering knowledge and secure the economic objectives of their employer. This is commonly experienced as having to trade off time, cost and quality objectives, since, given the tight profit margin the firm is working to, site staffing levels are

reduced, the quality of labour is unpredictable and so on. It is also experienced as having to administer formalized quality control or quality assurance systems which originate with people at head office whose motives for instituting them might be considered suspect. Are they really to control quality or are they to control site managers and thereby undermine their professional discretion and authority?

Dilemmas of this sort may be thought of in terms of two kinds of authority that they are expected to exercise and the ambiguities associated with each. One source of authority is that they are members of an occupation, one amongst several that they have dealings with and a special kind of occupation at that, a profession. Because of their profession's historical association with the development of concrete as a material they have special responsibilities in its use. To the extent that they are members of a professional association which controls entry through conditions of membership and, on that basis, seeks to guarantee competence, they carry certain legal responsibilities. In large part, the way concrete is supposed to be made and used is provided in the pure, experimental sciences and they are expected to apply the codes and conventions that are derived from them. But at the same time, they also know that some individual engineers are more competent than others; some have greater experience than others and that, depending on such factors, engineers (themselves included), expect, to be allowed 'elbow room' to exercise their judgement. Although concrete technology is derived from the exact sciences, using it on-site in variable and unpredictable conditions, it is far from being exact. And, crucially, the work has to be carried out by a number of occupational specialisms over whom engineers are expected to exercise their professional authority by providing the criteria for judging the quality of work. This is complicated by the fact that the norm is now that these specialists are likely to be employed by someone else.

They may well find all this is difficult to explain to people who are not engineers. On the one hand they have to preserve the view of themselves and their profession as dealing in hard demonstrable fact where unambiguous principles can be invoked and, on the other, they do things because they 'feel' it is the right 'engineering' thing to do, taking into account all the other contractual and commercial considerations noted. One of the ways they shield themselves from this dilemma is to have learnt from other engineers a number of ritual practices. They act, talk and dress *like* engineers. They conform to conventions, which go far beyond the technically necessary, to create confidence amongst the potential unbelievers and cynics who might be worried or critical if they were given reason to think that engineering practice is not all that it is supposed to be. It is a common experience of researchers to be asked to switch off the tape recorder with the words: 'Now I'll tell you what we really do'.

In this, the engineer is no different from any other professional. As Bagehot the nineteenth-century constitutional historian pointed out, with regard to politicians, there is an efficient part of government and a dignified part. All the pomp and ceremony is intended to sustain confidence that all is as it should be and that authority is being exercised effectively. How far the particular rituals that we have inherited in this country still succeed in sustaining this confidence is questionable but the point remains, that however it is managed, the professional in whom we necessarily vest our trust must work to maintain it. One of the ways in which this is achieved is by not revealing what goes on 'behind the scene'.

Gale (1992) has suggested ways in which overt and exaggeratedly 'macho' behaviour acts as a kind of bona fide, a ritual demonstration of a person's membership of the construction community. Thus, to act in a way consistent with standard expectations of what a particular occupational member should be like is a powerful cultural force. This is particularly important in those circumstances, typical of construction, where working relationships are often transitory. Relationships between people who do not know each other can be made easier if people conform to type. Typifications and stereotypes abound in construction because they facilitate transitory working relationships.

However, they can also impede the development of longer-lasting more personalized relationships characteristic of effective working teams. Thus, it is observed that stereotyping is a negative feature of the existing culture. The ambiguous nature of the engineer–managers' authority, referred to above, exacerbates this problem. For, besides maintaining their professional authority, they must promote the interests of the firm that employs them. Knowledge of this fact encourages people to attribute standard motives regardless of the dispositions of the actual individuals involved. So, in the abstract, 'everybody knows' that there are engineers who 'think up smart solutions that don't have a cat's chance in hell of working' or 'come up with impossible detailing because they don't know what it's like on site'. Equally, from the other side of the contractual fence, there are those who are 'always looking for reasons for claims and extras' or 'cowboys who are always looking to pull a fast one'.

Certainly, efforts will be made to cultivate people 'on the other side' who are disposed to act as fellow engineers; collaborators in a common problem rather than as adversaries who 'make your life difficult just for the hell of it' or to 'protect their own backs'. Attempts will perhaps also be made by contractor's engineers to try to develop good working relationships with subcontractors but, there may be uncertainty about their motives

and rationales. Contractor's engineers, then, are just as likely to treat subcontractors with suspicion, be resentful of their interference or be sceptical of their advice. Yet developing these relationships and, over time perhaps, generating a new set of typifications, is precisely the substance of cultural change.

We have briefly indicated some of the concerns of an interpretative treatment of site engineers' culture. The approach may be equally applied to any occupation in the industry, such as steelfixers or formwork carpenters. In these cases, engineers would appear as a feature in their lives, to be dealt with, given instructions by, negotiated with and so on. The answers to research questions like, 'why does this or that practice persist?' are in terms of the conscious views and experiences of these people, as reported to or observed by the researcher. We have very briefly indicated a number of such explanations from the study we considered earlier. Head office requires site engineers to carry out checking in a certain way, in the context of extreme commercial pressures, where the work is carried out by people over whom their authority is ambiguous. They must also perform other important tasks which compete for their time. They make a trade-off, perhaps feeling deprived of the reassurance that checking provides. These are the facts that it is the researcher's task to reveal, the very stuff of their attitudes and culture. If such things are not taken into account, efforts to effect change will inevitably be misdirected.

Conclusions

We have outlined the main assumptions and implications of the rationalist paradigm. The persuasiveness of this paradigm stems from the immense power of science to explain the natural world and provides means for changing it. It is of little wonder, then, that many attempts have been made to apply scientific method to the understanding of human relations, in the hope of achieving similarly dramatic results. Engineers in particular, because their discipline draws heavily upon scientific methods and discoveries, are susceptible to the lure of this approach. The kind of explanation which engineers are professionally inclined and qualified to supply is a technical one and their methodology arises from this technical orientation. Among the community of researchers, however, it is not only engineers who have embraced the rationalist paradigm. In a society where the appelations 'science' and 'scientific' have such a high status, it is perhaps inevitable that members of almost every occupation have attempted to characterize their work as having a basis in science. These attempts to enhance professional status might remain relatively harmless if they were not accompanied by the abuse of scientific methods, in contexts for which they were not designed and in which they have no effective application.

In our consideration of the study of structural features in the fourth section and of the work of Deming in the sixth section, we have attempted to outline some of the limitations of the rationalist paradigm. The expectation implicit in the assumptions of this paradigm, is that research findings will be unambiguous. This approach, though it produces data which *looks* as though it fulfils the rationalist requirement, is not suitable for making assessments of social processes. We have introduced the reader to a small sample from the body of work which has criticized the rationalist paradigm and outlined an alternative approach. This approach involves returning to fundamentals, to examining the methods by which knowledge of social processes can be gained, methods which all members of society use as an everyday matter of course. The methods which researchers use can be essentially no different, though this does not excuse us from applying them as rigorously and self-consciously as possible.

The key to such an approach is that the researcher take seriously the world views of the people concerned. This involves resisting the temptation to dismiss these views in favour of some 'real' version of the 'facts', however self-evidently correct this version may appear. This principle was stated many years ago by Thomas (1964): 'If men define situations as real they are real in their consequences'. The problem that we see with the rationalist paradigm is that it does not require researchers to question their own position. Instead, rationalists put their faith in the use of particular methodological routines to guarantee their impartiality. The researcher's values are regarded as either irrelevant or self-evidently correct. In our view, this lack of self-consciousness results in methodologically unsound research and produces findings that are profoundly incomplete. Any programme of action based on such findings is of questionable practical value. We must, instead, ask genuinely empirical questions about what is going on in the situations we wish to understand, particularly when considering those admired innovations we wish to learn from. As we have tried to show, with reference to Ohno's innovations, without an intimate knowledge of the situations which occur, we will be in no position to offer either diagnosis or remedy.

We have attempted to demonstrate the kind of data this approach can generate, in our description of the situation of the site engineer. This material is, to be sure, relatively unremarkable in itself and is mainly provided to illustrate a methodological point. One significant fact which does emerge, however, is that practising engineers are faced with a disjunction between the world they live in and the idealized version of it provided by the rationalist paradigm. In the everyday performance of

their work, they recognize, research and resolve problems of human social process, with no recourse to rationalist prescriptions or the need for 'objective' accounts. This informal, unwritten and unsystemized knowledge should be a focus of research.

A subtheme to all of this is that practitioners and researchers participate in the same culture. If our methods are the same, then so are our assumptions. Thus, the relationship between production and research is a close and complex one. The judgement of engineers, whether on a research panel or in management, is accepted because it consists of members of an occupation who, through their skills in the application of instrumental rationality, have played a central role in creating the technology and institutions of the construction industy. There exists what Becker (1970) has called a 'hierarchy of credibility' in which certain kinds of information and certain kinds of informant carry more weight than others. This is what we mean, when we say that the rationalist paradigm itself is a constituent part of the culture of the industry and why a change in the culture of research must be our first consideration.

Engineers' roles, their self-conception and modes of thinking have been specifically shaped by the nature of their involvement in the construction industry. They have a viewpoint that might lead them, for example, either to distance themselves from the practicalities of construction or align their interests with the contractors. Either way, to expect them to provide an 'objective' account of a process in which members of their profession are variously involved as active participants is problematic. Their preferred procedures, diagnoses and remedies are inevitably selective and likely to be engendered by an engineering perspective.

The question then arises: how do research and practice relate? Our answer is the one given by Max Weber many years ago: they are crucially different. The objectives of practitioners, for example, quality, efficiency, productivity or profits, cannot be taken to be self-evident by the researcher. An essential purpose of research is to establish what participants in the situation under study, managers, engineers or steelfixers, mean by these terms and what values and beliefs underlie such meanings. Researcher may well share some of the understandings of some of the participants, but it is imperative that they suspend their own understandings. Only by doing so can they allow practitioners to speak for themselves.

Some recognition of these facts has occurred in more recent applications of systems theory to human organization. Senge *et al.* (1994) acknowledged the simplification that modelling entails, emphasizing the need to recognize the purpose that the resultant 'mental models' are intended to serve. They are tools which are used by people in context with the wealth of tacit meanings that those contexts have for the participants. It is pointed out that the work of earlier 'objective' systems theorists, who had modelled natural or mechanical systems was misapplied to human systems.

These Rationalist versions made the very dubious claim to offer objective versions of social reality. In similar vein to Senge *et al.* (1994), Checkland and Scholes (1990) suggest the term 'holon' to mean a-version-of-the-system, emphasizing the fact that any version of reality is provided by a situated person and will reflect that person's values, beliefs and intentions. It is vital, they argue, to recognize that the author of a holon has a particular world view. Significantly, Senge begins his most recent book, which concerns the applications of systems thinking, by extrapolating on the implications of an exchange of greetings characteristic of the tribespeople of sub-Saharan Africa. It is, 'I see you', with the response, 'I am here'. The point that he makes is that any form of social organization begins from the fact of a self, which attributes meanings to events and seeks to share them with others (Senge *et al.*, 1994, p.3).

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