



University of
Salford
MANCHESTER

Construction management research and the attempt to build a social science

Seymour, D and Rooke, JA

Title	Construction management research and the attempt to build a social science
Authors	Seymour, D and Rooke, JA
Type	Article
URL	This version is available at: http://usir.salford.ac.uk/id/eprint/3002/
Published Date	1998

USIR is a digital collection of the research output of the University of Salford. Where copyright permits, full text material held in the repository is made freely available online and can be read, downloaded and copied for non-commercial private study or research purposes. Please check the manuscript for any further copyright restrictions.

For more information, including our policy and submission procedure, please contact the Repository Team at: usir@salford.ac.uk.

CONSTRUCTION MANAGEMENT RESEARCH AND THE ATTEMPT TO BUILD A SOCIAL SCIENCE

David Seymour and John Rooke

Published as:

Seymour, D. & Rooke, J. (1998) 'Construction Management Research and the Attempt to Build a Social Science', *Journal of Construction Procurement*, 4:59-73.

Abstract

The paper challenges the view that the major theoretical and methodological issues in the social sciences have been resolved and that positivism provides the only sound basis for research in construction. By examining the relationship between specialist discourses and natural language and Weber's failure to provide a basis for objective causal explanations of social action, it is argued that the kind of theorising that Runeson advocates is at best premature and at worst preempts the achievement of a more rigorous and thorough understanding of construction processes. Reporting some empirical research on the design and construction of reinforced concrete structures, the paper seeks to demonstrate some theoretical methodological and practical implications of an interpretive style of research.

Keywords Research methodology, theory, concrete cover, reinforcement.

Introduction

We thank the editors of this journal for the invitation to reply to Runeson's paper (1998). In this reply we will try to show to readers, variously concerned in the construction industry, the practical relevance of what may understandably appear to some as non-consequential, theoretical logic-chopping. We will do this by focussing on a particular item from our own empirical research since we think this is a better way of answering Runeson's criticisms of the concerns we have expressed in a number of articles (e.g. Seymour and Rook 1995, Seymour et al 1997, Rooke 1997). Our attempt will be to allay any suspicion that we are concerned only with methodological niceties. However, we do take leave to consider the criticisms 'head-on' because it will enable us to make some general points about the difficulties entailed in drawing on specialist academic disciplines to meet the ultimately practical aims of construction management studies.

We wish to make the following points about the arguments which go on in the social sciences each of which will be taken up in turn.

First, and on this we differ most profoundly from Runeson, the arguments in the social sciences about principle and method are unresolved and are likely to remain so.

Second, the arguments are necessary and, if conducted in the spirit of scholarship and the desire for greater understanding, are valuable and conducive to good research.

Third, while we have an absolute obligation to make ourselves understood to our readers, this is often not easy since they come with different degrees of interest in and familiarity with the different concerns of the various disciplines.

Fourth, and this point follows directly from the third, while there are specialist discourses developed to express specialist interest and concerns, they are all rooted in and depend on the language of everyday life. Failure to recognise the different logics inherent in different discourses leads to confusion. We will examine such a confusion which arises from Weber's project for a social science.

Finally, we will illustrate the relationship between specialist and every day discourses by examining some research on the problem of achieving adequate cover for reinforcement in concrete structures.

The unresolved issues and the necessity of argument

Runeson seems to evince confidence that all the important theoretical and methodological issues in the social sciences have been resolved and that positivist social science has achieved a level of development such that it is ready and waiting to provide the conceptual apparatus with which to solve the problems of the construction industry. We are less sanguine about this promise and have sought to explain why.

Nonetheless, he agrees with our main point that scientific method (as understood in the physical sciences) has its limitations, when it comes to researching human activities. We are in agreement too regarding the overuse of statistics in construction management research. He also agrees that research may have different kinds of topic and different purposes susceptible to different kinds of treatment. Yet, despite this, he seems to ignore the issue we are trying to raise: given the inappropriateness of the methods so often used, the variety of topics that construction management research addresses and the purposes it may have, how are scientific and non-scientific purposes to be identified and what principles are there for guidance on how research should proceed? In short, while we have tried to state some problems with current research and have proposed ways of addressing them, Runeson condemns our suggestions but offers none of his own.

However, we welcome his brief review of Weber's concept of *verstehen* as a useful contribution to the debate and we consider the importance and difficulties associated with *verstehen* below. He also correctly identifies a problem of definitions, such that terms remain

vaguely defined or subtly change their meanings as the debate progresses. But he does not recognise that this is an inevitable feature of such debates which progress largely by achieving successively clearer definitions of the terms in use. He seems unaware that the problem exists with regard to his own use of terms. Thus, for instance, it is not always clear whether he rejects or accepts the positivist view of science, which he seems sometimes to defend and at others to regard as superseded by later thinkers (Runeson 1997, Runeson 1998).

He usefully raises the moral issue involved with the injunction of "accepting other people's 'reality' (sic) without question". It must be clearly understood that this consists in a recommendation for research practice and not in a philosophy of life. There is all the difference in the world between dispassionately examining a set of beliefs and conceding their moral validity, not that the former task is easy.

The need for constructive argument

This observation takes us to the second point: argument is necessary and valuable so long as it is directed at mutual enlightenment. Unfortunately, Runeson obscures and devalues his contribution by his aggressive debating style. Perhaps the worst feature of this is the way he attributes views to us which we have not expressed, but which make easy targets for his own 'refutations'.

Thus, we are held to believe that a \$5 hamburger can be cheaper than a \$2 hamburger! Though this is impossible what Runeson's talent for parody obscures is that a \$5 hamburger may be more *desirable* than a \$2 hamburger. There might be all sorts of reasons for this, but one possible reason is that the \$5 hamburger is more expensive, and therefore 'must be better', another is that the hamburger, though no better, is 'more exclusive' - simply by paying more than we need to for an item, we can demonstrate our ability to do so. It is such views as these, which Runeson may wish to dismiss as illogical, that account for real people, in the real world performing real actions - witness, for example, the popularity of designer clothes. It is the explication of such views (and not a set of hypothetical causal regularities which Runeson claims that science has discovered but which he does not specify) which allow us to predict human behaviour.

Though the general thrust of Runeson's paper is to convey the impression that we are *ignorantes*, it is difficult to see, in that case, why he bothers to reply to us at all. Is it because he believes the rest of the Construction Management community are also *ignorantes* and therefore likely to be misled by us? This certainly seems to be implied in some of the comments he makes. Thus, it seems, we are guilty of hiding our true intentions "among logical somersaults and emotive language". This is an entirely unsupported charge Runeson's own use of emotive words and phrases seems designed to hide the fact that his

assertions are unsupported by argument or evidence. An example of this is his use of a review of *Studies in Ethnomethodology*. This review may well contain reasoned arguments, if so, Runeson neglects to tell us what they are.

Another inadequacy in his paper is the attack on Garfinkel. Runeson asserts that Garfinkel has retreated from his earlier position. This is not the case. We take it that Runeson is referring to the opening passage of the paper to which he refers (Garfinkel 1996) where Garfinkel attempts to situate EM in relation to mainstream social science (ibid p6). Referring to the latter's achievements, he writes: "EM does not dispute those achievements." (loc cit). Indeed, Garfinkel has *never* sought to dispute these achievements. For instance, in what is regarded as EM's foundational text, *Studies in Ethnomethodology*, he writes: "Ethnomethodological studies are not directed at formulating or arguing correctives [...] They do not formulate a remedy for practical actions, as if it were being found about practical action that they were better or worse than they are usually cracked up to be." (Garfinkel 1967/84 p viii). Rather, EM treats these achievements of social science like any other practical achievement as topics for EM analysis in their own right.

For some reason, probably the difficulty of his writing occasioned by his putting precision before ease of understanding (a problem which all of us need to struggle to resolve), Garfinkel attracts dismissive comments. Runeson mistakes Garfinkel's courteous acknowledgement of the social science movements 'undoubted achievements' (Garfinkel 1996) as a retraction and no one should read *Studies in Ethnomethodology* in such a way. Garfinkel has always been highly complimentary about the achievements of conventional social science, particularly the work of Talcott Parsons, which "remains awesome for the penetrating depth and unflinching precision of its practical sociological reasoning". (Garfinkel 1967/84 p ix).

We are also puzzled by Runeson's remarks on Rooke (1997), a paper which seems to have relatively straightforward implications. To set these out formally, the paper proposes two premises and a deduction:

1. for statistical data to be valid, definitions must be consistent over the study (every time we count a duck, we must be sure it is a duck and not a crow)
2. in the transcript of questionnaire interview reproduced, the definition of 'manager' did not remain consistent (a manager may be a supervisor, or a professional)

If these two premises are accepted (and we cannot see on what grounds they could be refuted), then the deduction is inevitable - the statistical datum generated by the interview is not valid. It is true that this does not prove that all statistical data are invalid, but it does

demonstrate that the validity of any particular piece of statistical data is open to question. The practical consequence of this is that any piece of statistical evidence which contradicts our own common sense beliefs can be readily dismissed.

Natural Language and Specialist Discourse

The purpose of our papers (Seymour and Rooke 1995, Seymour et al 1997) was to draw attention to the fact that members of the construction management research community were drawing on and uncritically applying concepts and methods about which there was much disagreement within the disciplines whence they came. In particular, it was our view that there were whole traditions of social enquiry which are ignored or slighted because they do not conform to a naive view of science. It was our aim to defend and promote these traditions especially those which take very seriously the problems of achieving a genuinely scientific status.

It was precisely our effort to encourage this debate and to query the superficial 'scientific' criteria that were being applied to guarantee quality in research that led to our being accused of being anti-scientific. We accept a large part of the responsibility for this misunderstanding having perhaps been guilty of misleading by oversimplification. However, we stress that this problem is endemic and exposes the complicated relationship between every natural language and the demands placed upon language in the pursuit of precision and accuracy. Thus, as we embark again on an effort to clarify the contribution that we believe sociology (rather than various trivializations of it) can make to construction management, we emphasise the need for clarity and precision, recognising at the same time the difficulties created in effectively communicating what needs to be said if it is to be found accessible and useful.

We note that our difficulty here is to communicate to non-specialists the results to date of an on-going and often complex process in which scholars refine their thinking in the course of critical discussion. It is through this process that a better understanding of the world is achieved. However, such a debate also results in the creation of specialist discourses which are not readily accessible to outsiders. At the same time, these discourses are rooted in and cannot escape from the natural language of everyday life. We will now consider some implications of this important fact.

Natural Language as the Basic Matrix for specialist Discourse

We all live in the common sense everyday world and make sense to each other in the language available to us - the natural language of everyday speech. Even the most refined and specialised languages, say that used in physics, has its roots in and continually has recourse to natural language. For example, there is currently a problem with achieving

adequate concrete cover to the rebar in reinforced concrete (RC) structures. This is very important for without adequate cover, salts and water get to the rebar, they rust, expand, the concrete cracks, falls away, exposing the the rebar to futher deterioration and ultimately an unserviceable building. Why does it happen and how can it be prevented? Is it a matter of poor workmanship? Poor design? Poor communication? Poor supervision?

Any normally competent person can understand this problem though as a matter for specialist study the issue is construed in a variety of specialist ways - by industrial chemists, materials scientists, engineers and so on. Thus, in terms of the substantive themes that are brought under examination and the kinds of factors that are held relevant to understanding them, there is a basis in ordinary language for communicating this understanding as between laypersons and specialists. 'It is in this agreement - agreement as to the fundamental and ordered existence of the phenomenon independent of its having been addressed by *some* method of enquiry - that [specialist] and lay[person] are mutually oriented to a common factual domain.' (Zimmerman and Pollner 1971 p81)

Scientific ways of thinking are well established and efficacious for predicting the behaviour of concrete in specified conditions (see for example DOE, 1997). At various times, experiments are conducted and observations recorded; generalisations are achieved through inductive reasoning or sudden insight; hypotheses are derived and tested. Experiments are designed to be reproducible and results published in a language understandable to any member of the specialist scientific community concerned. However, these methods of enquiry, intended to produce empirical generalization, are not so effective in studying the organizational contexts in which concrete structures are erected

Of course, in a common sense sort of way, for anyone associated with the process of RC construction, there are general patterns, series of events that tend to happen in broadly predictable ways. Our lives would be quite literally senseless if we were not able to count on such regularities and patterns. But there is a world of difference between the everyday accomplishment of recognising patterns of conduct and the scientific enterprise of generating rigorous law-like generalisations from which can be derived precise and definite predictions, given stated conditions. The research discussed below and reported more fully elsewhere (Seymour et al 1997, Shamma-Toma et al 1997) concluded that failure to take account of this difference undermines the whole specification procedure used in the UK construction industry with respect to RC. This procedure is based on the assumption that it *ought* to be possible to determine these patterns of conduct with scientific accuracy.

For example, the assumption in design practice when specifying the required cover is that there are consistent patterns in the distribution of cover achieved. Based on this assumption cover surveys have been conducted and their findings used to establish values and tolerances

that need to be complied with on site (Clear 1990). Specifications are therefore stated in terms of nominal cover (the target) plus or minus what is deemed to be acceptable by the designer in following the Standards.

However, it is recognised that there will be some variation in site conditions and that the construction of in-situ concrete is a highly interactive process. An attempt is therefore made to address some features of this issue in BS 5606 (Seymour et al 1996). In recognition of the distinctive features of the construction process, whilst preserving the logic of specification through the use of tolerances, it states that tolerances should be specified which reflect the following:

- 1 the dimensional tolerance needs of the design;
- 2 the particular requirements of the various elements and components of the construction.

According to these criteria, the tolerances for concrete cover are to be determined from the assessment of combined deviation limits. In other words, the criteria are intended to take into account the deviations that occur at the various stages in the construction of an element, say a wall or a column. Deviations may occur as follows:

1. The accuracy of setting out and positioning the kicker. This is specified on some sites as 'the position of structural elements shall be accurate to $\pm 3\text{mm}$ '.
2. The accuracy of the thickness of the structural element is stated in many specifications to be within $\pm 4\text{mm}$.
3. The plumbness of the structural elements is stated to be within $\pm 6\text{mm}$ per storey.
4. The bending of reinforcement is recommended to be $\pm 5\text{mm}$ for closed dimensions.

However, in the view of some practitioners this does not go far enough. For example, it is held that there is incompatibility of the cover tolerances with other construction tolerances of the structural element. Thus;

"Over-tightening of bolts of the forms that is within tolerance but thinner than the specified thickness of the wall. We can over-tighten the bolts by 5mm or something like that and this reduces the cover."

"We have tolerances on the kicker. We set out the kicker separately from the starter bars. We sometimes end up cranking bars to get the cover on the kicker."

(Seymour et al 1996, p 13)

BS5606 provides an equation to calculate the combined tolerance of the total deviation of a specific design or interface;

$$DL_t = \sqrt{(DL_1)^2 + (DL_2)^2 + \dots + (DL_n)^2}$$

DL_t = Total deviation limits

DL_n = Component deviation limits

Using this equation to calculate the cover tolerance, the tolerance that needs to be applied is $\pm 10\text{mm}$ for vertical structural elements. However, this does not take into account the movement of the shutters or reinforcement during placement and compaction of the concrete. Further, the tolerance will change depending on the changes of the individual tolerances of the component features introduced in particular contract specifications. Theoretically, then, the construction process should comply with a cover tolerance of $\pm 10\text{mm}$ for vertical structural elements, assuming that it is physically possible for the design to be erected within the tolerances specified.

What is offered in this formula, then, is derived from an ideal-typification (cf Weber); a simplification intended to act as a model or guideline based upon inductive generalization. It may well act as a rough and ready guide but, as was pointed out in the quotes above, it is very rough and ready indeed and has to be supplemented with knowledge of situationally specific conditions.

These observations on the nature and use of typifications become ever more significant the more complex the processes they aim to represent become and yet similar theoretical formulations using abstract concepts, sometimes in the symbolic terms as used in the equation above, are employed to express them. Indeed, the research referred to above has been criticised for not expressing our findings in such terms. We have, for example, been asked what correlations there are between cover achieved and such contextual features as size of firm involved, contractual form used and so on. While we have supplied such information (Shammas-Toma 1998), we have stressed that any inferences based on them must be highly speculative. Of much greater significance, in our view is, to understand the actual processes involved in particular cases, the point we will discuss in the final section of this paper.

The problem identified here points to the limitations of applying to a social process (the design and construction of RC) a logic which is appropriate to understanding physical phenomena where the impact of different variables can be manipulated in controlled ways.

Consider what is being attempted in applying such a logic to understanding/explaining a social process. As researchers, our understanding of particular phenomena (eg designing and constructing a concrete element) is made possible by a mass of common sense recognitions and capabilities which we share with people whose conduct is of interest to us (designers, site managers, formwork carpenters, steel fixers, concretors etc). As observers of RC construction practice we make any number of common sense assumptions about what those in the process are doing, why they are doing it and so on. In providing descriptions and analyses of this process we convert all the understanding that we have gained through our use of common sense, shared knowledge into formalised concepts. These abstractions act as a kind of short hand or code which needs to be decoded by those to whom we convey our understanding. They, in turn, do so by drawing on the very same kinds of understanding that we have used in providing the abstracted account. Thus, the formula referred to above is a shorthand which conveys in an encoded form information which is to be interpreted (or decoded). The comments reported above were that the formula did not cover the eventualities likely to be encountered and so forth. This is inevitable. As Wittgenstein pointed out, though we have rules to guide us in our activities, these must *always* be interpreted. And though we may make rules to guide our interpretations, these rules must also be interpreted. A major concern in our own research is with the methods of interpretation.

What can we expect from a social science?

Here then is the major problem that a social science faces in trying to provide an objective, factual account of the structure and dynamics of social processes (say, constructing RC buildings). There is, of course, an understandable desire amongst practitioners for accurate and factual accounts of the way things are; for analysis which cuts through of the incidentals of things to reveal the essentials, the underlying causal patterns as the basis for re-synthesis and more reliable guides to action. This is understandable because the natural sciences have achieved and continue to achieve such accounts and many within the social sciences (including Runeson) promise that their own principles and methods of enquiry will deliver such accounts.

We have argued throughout that insofar as people do find such offerings useful, well and good. However, what we have criticised are the claims that any given set of findings *ought* to be useful *because* they are based on the application of science. In our view (and Runeson's too), what too frequently happens is that because certain procedures have been

followed, the truth or accuracy of a finding is thereby assumed to be guaranteed. However, in addition to this (and here we part company with Runeson), besides this rhetorical appeal to science which simply ignores the methodological difficulties, we are also sceptical of many other (more serious) claims to being scientific and have sought to show the basis of our scepticism.

For example, we have been accused of anecdotalism in using quotes from practitioners such as the ones used above. It has been demanded of us that we say how typical the response was; how many people said it, and so on. However, our purpose was not to try to provide the evidence for such generalisation. Our prior concern was to be sure that we understood what we were seeing and that we could provide an adequate description of the phenomenon before us instead of counting arbitrarily dissected and identified features of it. As we commented above, before counting different kinds of bird it is important to be able to distinguish ducks from crows. While we were satisfied that we understood what the respondents quoted above were referring to, by virtue of the context in which the reference was made, to assign numerical scores to *what* they were talking about would have been arbitrary in the extreme.

The problem with attempting to make this kind of topic the basis of scientific study is that to do so sets up a dichotomy between everyday common sense and the criteria of evidence, reason and logic acceptable to a scientific community. As we will now try to show, Weber's struggle with the notion of *verstehen* (the issue raised by Runeson) may be seen as an effort, ultimately unsuccessful, to resolve this difference.

Weber and *verstehen*

The social world is self-evidently not random but regarding the regularities which it displays as a sufficient basis for a science is premature. There are regularities which enable us to understand what is going on and predict what is likely to happen next. This is the conscious, mundane achievement of every ordinary member of society. Weber thought that it ought to be possible, by applying methods of systematic observation, reason and logic to turn the everyday knowledge of how social processes work into a science - that is, in Weber's conception, a method of enquiry that will reveal the objective nature of the phenomenon under study, untainted by one's own subjective experience and values.

Weber was conscious that to achieve this a major problem had first to be tackled. The social world is crucially different from the physical world in that the social world is composed of people acting out their subjective experience in their everyday lives. People act with some consciousness of what they are doing and, if asked, are expected to be able to say what they are doing. In other words, it is assumed that the social world is composed of people acting

rationally and not randomly. Furthermore, in order to understand this essential feature of the social world, the observer must call on his/her own experience; the capability for understanding others lies in oneself. The problem that follows from this is how to hold one's own experience in abeyance; to use it but to neutralize its effect on what is observed; to provide an objective account of what is 'out there' as distinct from what one is merely subjectively perceiving. This, in a nutshell is how the problem of how it is possible to have a science of society is posed by Weber. It is important to stress, in view of Runeson's critique, that the question is unresolved. However, what we can do is inspect, on their merits, attempts to study society on the basis of various kinds of working resolution. Broadly, there are, as stated above, two touchstones against which to evaluate these working resolutions: common sense and the specialist principles and methods of the social sciences. We will consider each in turn (in reverse order).

First, we will rehearse a central argument concerning what it takes to construct a social science. Second, with an appeal to common sense, in the last part of the paper we will inspect the question: what do we know about concrete structures which will enable us to build them more efficiently?

To construct a *social* science we are not simply concerned with what people do. We are concerned with what their action means; what they intended by it or what they sought to achieve by doing it. Thus, somebody waving her arms above her head might be a signal of alarm, an attempt to attract attention, the expression of a greeting, taking part in a Mexican wave. To understand what any such arm movement means is usually supplied to the observer by the context. Somebody else notices the danger that is being signalled and runs for safety; somebody waves back, smiles are exchanged and so on and so on. Being familiar with such series of events, for the most part, 'what is going on' is 'obvious' it is common sense- we understand; *wir verstehen*.

Verstehen, then, in Weber's words (translated) is:

'the subjective interpretation of a coherent course of conduct when and in so far as, according to our habitual modes of thought and feeling, its component parts taken in their mutual relation are recognised to constitute a 'typical' complex of meaning.'
(Weber 1966, p99)

He called this adequacy at the level of meaning. This is the first and indispensable step to understanding the workings of the social world. However, according to Weber, the sociologist's concern is to go beyond mere understanding and to be able to establish causal connections in any series of events; that not only is one event expected to follow from

another but that one event *necessarily* does follow from another (causal adequacy). Thus, causal explanation:

'depends on being able to determine that there is a probability, which in the rare ideal case can be numerically stated, but is always in some sense calculable, that a given observable event (overt or subjective) will be followed or accompanied by another event.' (loc cit)

Following Schutz (1967) we take it that the import of this distinction for Weber is that the aim to establish a causally adequate explanation is the special concern of the social *scientist*. But, of course, if the social scientist assures us that he can indeed show a causal or necessary connection between one event and another, then we may well be expected to sit up and take note. If I wish to achieve x and science can show that there is a strong probability that I should do y, this is knowledge worth having. However, Schutz invites us to inspect the basis of the social scientist's assurance on this matter.

Weber tries to transmute *subjective* understanding which derives from a person's recognising on the basis of his own experience of typical series of occurrences (seeing someone waving her arms and someone else running to her rescue) into *objective* understanding. This is to be done by showing that there is a probability that this will happen on any particular occasion or with sufficient frequency for it to be inferred that there are necessary connections in a series of events. Thus, causal adequacy has been established.

Schutz disputes the validity of this attempt arguing that the two kinds of understanding (subjective and objective) are qualitatively and logically the same. The difference is that the former represents the interpretation of one's own experience, the latter an interpretation using 'a scientific complex of knowledge', that is an inter-subjectively agreed upon interpretation that satisfies the social science community. In other words, we establish the objectivity of an account by comparing our subjective impressions with the subjective impressions of others.

'A sequence of events is causally adequate to the degree that experience teaches us it will probably happen again. The concept of causal adequacy relates therefore to that objective context of meaning which is social science itself.' (Schutz 1967 p231)

In both cases the generalization (this is the *kind* of thing going on here) is achieved through 'a synthesis of recognition'. He means by this that Weber's attempt to objectify the phenomenon fails since one (as social scientist) is still *importing* one's own experience

(however carefully assembled it may be) of a typified series of events which exists *before* whatever it is one is trying to explain.

'He [Weber] postulates as the task of social science the discovery of intended meaning -indeed the intended meaning of the actor. But this 'intended meaning' turns out to be a meaning which is given to the observer and not to the actor' (ibid p234)

In summary then, the discovery and demonstration of causal patterns and regularities with regard to physical, non-social phenomena is patently achievable sufficient for the enormous impact that this knowledge has had on our everyday lives. Quite what is known and how it came to be known is for the various communities of physical and natural scientists to decide. If, however, causal patterns and regularities which constitute the body of social science knowledge are those that are agreed by the community of social scientists, what status do they have relative to the causal patterns and regularities that the actors themselves recognise, in terms of which they act and whose action constitutes the very phenomena to be explained?

Consider this example. We all know of the interest there is in the connections between job satisfaction, motivation and performance and of the desire to find some causal patterns in them. Nystrom (1984), in an extensive review of this literature, discusses some so-called 'unexpected results' of job redesign. It appears that employees in a number of studies had been more satisfied or less satisfied with the redesigned jobs than the theories on which the jobs had been redesigned had led the researchers to expect. He reports, for example, Billings et al's attempts to explain the findings of one of the studies where it was discovered that employees' perception of job characteristics changed prior to the actual technological conversion itself.

"Billings et al conjectured that employees are sensitized by researchers' questionnaires and rate their jobs low in anticipation of job changes expected to follow the technological conversion." (Nystrom 1984 p279)

Our question here is: why didn't they ask the employees? The answer is likely to be of the sort (we suggest): The employees themselves don't actually know; that there are underlying patterns or forces at work which can be discovered if individual cases are ignored and the aggregated behaviours studied, these to be accessed by statistically valid, standardized questionnaire surveys. That is, if it is possible to establish patterns and statistical probabilities, we don't actually need to know what individual motives are at work: That it is

possible, in short, to explain human conduct either without reference to or by transcending an individual's consciousness of it. This view would not have satisfied Weber himself. A *sine qua non* of his attempts to formulate the grounds for a science of social action was the need to incorporate the actors' own understandings in the explanations of their action.

In light of the argument above we do not think he succeeded. Our view is that the 'scientific' procedures employed to sift out incidentals from fundamentals amount to no more than common sense practices, which we all do all the time in establishing the regularities and patterns in our everyday life. What is offered as the result of these procedures can never be anything more than plausible. It cannot be scientific in the way claimed for the reason that it does not *discover* patterns; it *supplies* them. This may be acceptable so long as one recognises what is being done. In our view, however, it continually risks preempting, denying or ignoring the sense-making patterns that are actually being used by actors to constitute the phenomenon itself.

So What? What has all this got to do with building durable concrete structures?

The RC study, discussed above, was designed to find out if adequate cover was achieved and, if not, why not. The simple methodological choice was: i) do we look for explanation in the way the process was constituted by the actors themselves, bringing to it their various understandings of the situation or, ii) do we cut through this to offer an account of what was 'really' going on, that is, a causal explanation, *our* explanation which we would submit for judgement as true or false by other social scientists?

The study was carried out on twenty-five construction projects in the UK and is more fully reported elsewhere (Shammas-Toma, 1995, Seymour et al 1996). It was designed to establish, first, how the cover achieved to reinforcement in a sample of concrete structures measured up with the specifications and, second, the reasons for the standards of cover that were achieved. It was found that cover did not comply with the tolerances given in a significant number of cases despite the fact that the twelve main contractors who undertook the projects were all highly reputable and all but two of them were quality assured to ISO 9000. We believe that how this finding can be explained has important implications for both research and practice.

We use the study to consider the following questions:

In what sense was it possible to establish a *causal* explanation for the finding that much of the cover was inadequate?

Why was it important not to look for causal explanations where they were not to be found?

Concrete Cover - The Study

The standard method of communicating design requirements to site is in terms of nominal cover and specified tolerances. It is expected that the tolerances will reflect the functional needs of the structure and be achievable in the circumstances in which it is built. However, (as was noted earlier) in construction the tolerances set are derived from standard codes of practice, these codes having been prepared on the assumption that there are consistent patterns in the distribution of cover achieved. The reality, confirmed by this research, is that there are wide variations within and between sites even in identical structural members. This may, of course, be the result of poor workmanship and/or lack of supervision, however, the research concluded that though such reasons cannot be discounted, more fundamental factors are at issue, amongst which is the assumption that construction produces standardized products in controlled conditions where the expectation of consistent patterns would be warranted. However, as Ballard (1994) points out, the vast bulk of construction is in the nature of prototype development where the consistency assumption is not warranted. As such, the processes involved are craft-like in nature in the sense of requiring continuous response and adaptation to new information, and, given this, are labour intensive and therefore not amenable to the fine-tuning, calibration and control, characteristic of stable production systems.

Thus, the design and detailing of concrete elements is standardly carried out 'blind' with no reference to the organizational circumstances in which they are to be built, simply because these are not known. Nonetheless, current contractual practice requires that contractors comply with the specifications regardless of their appropriateness or buildability. There are two consequences. First, it encourages the contractor into a 'detection mode'. That is, he attempts to ensure conformance by checking and remedial work. Second, there is no realistic reference point against which improvements can be made. Subcontractors for the different phases - steel-fixing, formwork and concreting - recruited for the most part on price, are not expected 'to do it right first time' and there is little communication and coordination between them. A third consequence is that where, as sometimes happens, REs are prepared to cooperate with contractors' staff and adjust technical requirements appropriate to the conditions encountered, they risk running foul of the letter of the contract. Many, therefore, adopt a policy of requiring strict conformance to specifications, regardless of their

appropriateness. However, it is to be noted that where this was the policy, there was no marked superiority in the quality achieved and, besides, it was reported that it tended to sour the quality of relations between the contractor's and RE's staff (Seymour et al, 1996).

Some Implications for Research and Practice

The data generated by the research, on which the above findings are based, were of two kinds. First, the cover achieved and its immediate causes, such as missing spacers and rebar wrongly bent, were matters of physical measurement and direct observation. However, all that had occurred before the technical processes which directly resulted in construction (fixing steel, placing concrete), but had had an impact on how the technical processes were carried out (eg procurement policy, communication) were not similarly available for observation and measurement. The researcher was reliant on what he was told by those involved. Not surprisingly, many reasons were given for the resultant quality and these varied depending on who gave them, the nature of their involvement, their perceived contractual liabilities and so on. This was not a question, however, of who was or was not telling the truth. It was not possible to separate out the cumulative and interactive chains of events which had led to the measured results in any absolute way though, from any given point of view, it was possible to provide plausible versions of what had happened.

Such events are constituted by situated, meaningful social action. In their nature they have to be understood in the terms of those who took part in them. The notion of 'cause' simply does not apply since it implies underlying forces at work of which the participants themselves were not aware and there is no way of empirically validating the existence of such supposed forces as we argued above with reference to Weber. However, this kind of causal explanation is regularly applied (eg the use of *motive* as an explanatory device and the example from Nystrom given above). In other words, some intrinsic dynamic is *conceptualized* and hypothesised as cause in the same way that, say, gravity is invoked to explain why objects fall. This way of explaining and thinking about social processes has massive repercussions for the quality of both research and practice.

Research: Accounts and explanations of what happened and why it happened will always depend on why the questions are asked. It is a matter of situated practice. To ask what happened, regarding, for example, the construction of a faulty column, is a practical question for which we may legitimately look for a practical answer to satisfy the practical reasons for asking the question: e.g. what do we need to know in order to remedy defective work? who pays for remedial work - ie who, in a contractual sense, could be deemed to be at fault? What do we need to do in order to prevent such a defect from occurring again? and so on.

Thus, though there may be selective versions of 'what happened' which satisfy engineering, contractual, legal or any other set of criteria which reflect why one wants to know, there is no way of providing an account which does not reflect a particular, situated standpoint. And note, that in satisfying any given criteria, such criteria are set and maintained by particular communities of specialists - engineering, legal and so on.

Now, the warrant claimed by Runeson on behalf of the community of social scientists goes rather further than this. They claim to search for underlying, real or scientifically valid causes. This seems to us is a mistake in two senses. First, it is a conceptual mistake. Whatever happened in a particular circumstance was a *socially* achieved event: *the phenomenon was constituted by the meanings that participants gave to it*. Second, it is a mistake because it represents a lost opportunity. Thus, instead, of trying to answer the question: 'what *really* happened?' as though there were a single scientifically valid account to be had, a useful research question is: how are different perspectives communicated, negotiated and so on in the *process* or the production of the event? To ask the latter question has supremely practical implications.

Research: Thus, we find ourselves in agreement with a number of commentators (e.g Ballard and Koskela) on the need to think about the improvement of construction efficiency in terms of *processes* rather than *products* (and, besides, expect to contribute to the exploration and clarification of this distinction). For the moment, we note that the concern with products endorses and compounds a version of the design/construction process which is simplistic and causal, i.e. input resources are acted upon within a system. Some agency *causes* them to be converted. Value is added and the product is exported from the system. Thinking about management in terms of this causal or conversion model has a number of consequences. First, it ignores or obscures all that goes on between conversions where meaning and significance is communicated and negotiated between all the parties involved-what, in Lean Construction theory is referred to as 'flows' (Koskela 1992, Seymour 1994). Second, it encourages equating a *technical* conversion with a *contractually*-defined package of work and, therefore, prevents doing what is technically expedient for the sake of contractual considerations. The expectation of general patterns and generic causes impedes attention to the specifics of given situations. Thus, for example, this study concluded that it was precisely *contractual* differentiation that impeded attention to the specific needs of *technical* coordination and improvement. Far from there being causes (aside from the immediate technical causes), the concrete quality achieved was the *result* of failure to find ways of negotiating and resolving different perspectives and *perceptions of causes*.

In summary, we see a choice. The social scientist can try to provide scientifically verifiable versions of social affairs - or, as it concerns us, what goes on in construction - expressed in terms of underlying, general causal patterns. Alternatively, the social scientist can try to achieve a closer, more thorough understanding of the phenomena before him/her before attempting to theorise them. As to the former, we do not know of any that has succeeded. That is, we know of no social theory about construction that is anything more than a plausible version; none that comes close to meeting the standards of rigour set out, for example by (Kaufmann, 1958). Since it is a matter of choice, clearly, anybody is free to try to provide theory about construction which meets these criteria. We simply note some practical consequences. We have illustrated one of them with reference to our cover research. We think that the approach we adopted revealed some important features of concrete design and construction that would have been obscured or distorted had we not done so. Our contention that we were researching versions of particular instances of concrete construction and could not reliably establish any single, generalised version, has, we believe important practical implications for the design and production of concrete structures, raising the question - how can coordination in the process be more effectively achieved? We believe that this is more likely to the extent that it is recognised that we are concerned here with the communication of sense, meaning and significance. All this must be available for study and understanding. The imposition of a single version renders it invisible.

Conclusion

We have tried to show that in our concern with the difficulties of providing objective, scientifically valid versions of the regularities and patterns which so clearly exist in the social world, we do not conclude that we have nothing pertinent or useful to say about it and, in particular, the social world that we find in construction. Our central argument has been that all enquiry into construction activity would benefit from looking more carefully at that activity, in an attempt to achieve a more rigorous understanding of it before committing to theories about it.

We have acknowledged the powerful desire that all of us have to find meaning, regularities and patterns in the world we inhabit and the fact that we do indeed find such patterns. However, we have pointed out some major difficulties of achieving scientifically valid versions of them where they relate to socially organised activities. First, we noted that there are specialist discourses that make it possible to recast the constituents of the physical world in such a way as to explain the causal regularities, patterns and so on, which we normally

take for granted. But we also noted that the phenomena that these specialist discourses provide accounts of, always retain their everyday ordinary significance. For the purpose of her enquiry the scientist, as it were, suspends or brackets off what she ordinarily know about them.

With respect to social phenomena which, in their essence, exist in the everyday world this bracketing off of what one knows is that much more difficult to achieve. As we tried to demonstrate with reference to specification procedures, codified accounts derive their sense and meaning by having continual recourse to the uninspected, taken for granted, situated knowledge that we share and communicate through the use of everyday natural language.

Second, our discussion of Weber was intended to question the basis on which some versions of these regularities and patterns in the social world are given primacy over others as being more scientific. We concluded that we do not see in Weber an adequate basis for a social science that would merit this primacy. We argued, following Schutz's critique, that Weber failed to make the case that social science can explain causality. Rather, what passes as a valid explanation rests on the agreement amongst social scientists that it is valid. While this point is accepted by many commentators, for example Burrell and Morgan (1979), this acceptance itself presents a question: how does the community of social scientists actually go about achieving this agreement? In posing this question, we find that there is in fact a good deal of disagreement! And of course this disagreement is precisely about what constitutes science.

Our own response to this is to recognise the intrinsic difficulties of doing science with respect to social phenomena and therefore not to claim to do it. What we try to do, however, is to report as faithfully and accurately as we can how social processes are accomplished and in doing this hope to contribute to a better understanding of processes we study in construction.

In presenting the example of our RC construction research we have tried to demonstrate the nature of this contribution. Thus, in answer to the two questions we posed above, we consider it at the very least premature to offer causal explanations to account for the levels of cover achieved. To do so would have preempted the understanding which we believe we achieved and on the basis of which we are able to infer practical implications.

References

- Ballard, G. (1994) *On the Definition of Lean Construction*, Unpublished Memo to Members of International Group on Lean Construction.
- Burrell, G. and Morgan, G. (1979) *Sociological paradigms and organisational analysis*, reprinted 1994 by Athena, Athenaeum Press, Gateshead.
- Clear, C.A. (1990) *A Review of Surveys of Cover Achieved on Site*, Occasional Paper, Building Research Establishment, Garston.
- DoE (1997) *The Development of an Holistic Approach to Ensure the Durability of New Concrete Construction*, Report for the Department of the Environment, Project 38/13/21.
- Garfinkel, H. (1967) *Studies in Ethnomethodology*, reprinted 1984 by Polity Press, Cambridge.
- Garfinkel, H. (1996) Ethnomethodology's Program, *Social Psychology Quarterly*, Vol 59 pp5-21.
- Kaufmann, F. (1958) *The methodology of the social sciences*, The Humanities Press, New Jersey.
- Koskela, L. (1992) *Application of the New Production Theory in Construction*, Technical Report No 72, Center for Integrated Facilities Engineering, Stanford University
- Nystrom, P.C. (1984) Designing Jobs and Assigning Employees, in P.C. Nystrom and W. Starbuck (eds), *Handbook of Organizational Design*, Oxford University Press, Oxford.
- Rooke, J.A. (1997) Developing a More Empirical Approach to Culture, Attitude and Motivation in Construction Management Research, *Journal of Construction Procurement* Vol 3, No 2, pp 45-55.
- Rooke, J.A., Seymour, D.E. and Crook, D. (1997) Preserving Methodological Consistency: A Reply to Raftery, McGeorge and Walters, *Construction Management and Economics*, Vol 15, No 5, pp491-494.
- Runeson, G. (1997) The Role of Theory in Construction Management Research: Comment, *Construction Management and Economics*, Vol 15, No 3, pp 117-119.
- Runeson, G. (1998) Ignorantes Procedite: A Critique of the Movement for Methodological Renewal, *Journal of Construction Procurement* (in press)
- Schutz, A. (1967) *The phenomenology of the social world*, Northwestern University Press, Evanston, Il.
- Seymour, D.E. (1996) Developing Theory in Lean Construction, *Proceedings of the Fourth Annual Conference of the International Group for Lean Construction*, University of Birmingham.
- Seymour, D.E and Rooke, J.A. (1995) The Culture of the Industry and the Culture of Research, *Construction Management and Economics*, Vol 13, No 11, pp 511-523.

- Seymour, D.E., Rooke, J.A. and Crooke, D. (1997) The Role of Theory in Construction Management Research: A Call for a Debate, *Construction Management and Economics*, Vol 15, No 1, pp117-119.
- Shammas-Toma, M. (1995) *The Specification and Achievement of Cover to Reinforcement in Concrete Structures*, Unpublished PhD Thesis, University of Birmingham.
- Shammas-Toma, M., Seymour, D.E. and Clark, L.A. (1996) The Effectiveness of Formal Management Systems in Achieving Quality in Reinforced Concrete, *Construction Management and Economics*, Vol 14, No 4, pp 353-364.
- Seymour, D.E., Shammas-Toma, M. and Clark, L.A. (1997) Limitations of the Use of Tolerances for Communicating Design Requirements to Site, *Engineering Architectural and Construction Management*, Vol 4, No 1, pp 3-23.
- Weber, M. (1966) *The theory of social and economic organization* (translated by Talcott Parsons 1947) The Free Press, New York.
- Zimmerman, D.H. and Pollner, M. (1971) The Everyday World as a Phenomenon, in J.Douglas (ed) *Understanding Everyday Life*, Routledge and Kegan Paul, London.

