

## **Lean Construction theory as an exercise in practical reasoning**

John Rooke & Darryll Crook, School of Civil Engineering, University of Birmingham, Edgbaston, Birmingham, B15 2TT, UK.

E-mail J.A.Rooke@bham.ac.uk D.J.Crook@bham.ac.uk

### **Abstract**

Of the new production philosophy of Lean Construction (LC), Koskela (1992) has argued

*“The conceptual basis of construction management and engineering is obsolete. Formalization of the scientific foundations of construction management and engineering is a primary goal for the research community.*

*[...] The practical application of the new philosophy has commenced and diffused without any scientific, formalised basis [...] The theoretical and conceptual understanding of the new production approach is still limited. [...] [T]here is yet no unified, coherent and consistent theory.*

*[...] An explicit, preferably formalized theoretical basis is necessary for transfer of the new philosophy to new settings and for effective education.”*

Before successful efforts at development of this theoretical and conceptual understanding can be made, there is an *a priori* need to achieve a sound understanding of the philosophical underpinnings of what ‘Lean Construction Theory’ might consist of. We will argue that LC theorising is a form of social enquiry and hence empirical studies of the social interaction of managers involved in projects must be the starting point, the focus, the culmination and the ultimate and only basis of all theoretical activity in LC.

We examine the nature of the phenomena LC theorists are investigating and as a result dispute that LC theory will necessarily have a scientific foundation. We therefore undertake a preliminary attempt to sketch out some ground rules for a consistent and coherent approach to LC Theory. We suggest that just as construction management is the practical accomplishment of the situated social action of managers, so management theorising is the practical accomplishment of the situated social action of management theorists.

Drawing throughout on the respecification of sociology offered by ethnomethodology (Garfinkel, 1984), we will highlight the nature of theorising as an exercise in practical reasoning. Just as Lean Construction has respecified the construction process, ethnomethodology has respecified the process of social research and theorising. This approach highlights the similarity between the entirely practical exercise of theory-building and those activities the theory is about.

### **Keywords**

Lean Construction, theory, methodology, ethnomethodology, interpretive paradigm.

### **Introduction**

Howell and Ballard (1994) suggest that one of the features that constitutes construction production is uncertainty. In particular, there is uncertainty of both ends and means.

Construction activity can be seen as the constant, ongoing negotiation of these ends and means. They suggest that the success of a project may be determined by the ability and efficacy of, and speed with which the project participants can negotiate these ends and means<sup>1</sup>.

The main thrust of the Lean Construction (LC) movement has been to call for a change in the 'mental model' that is used to conceptualise construction from an emphasis on product to one on process. Howell *et al* (1996) argue strongly that construction should 'be reconceived from [its current emphasis on] the purchase of a product to a prototyping process'. The suggestion is that traditional construction management thinking has concentrated too much on contractual issues and regulating process outcomes (products) rather than on directly improving the processes themselves.

In its short history the LC movement has gone a considerable way to achieving this reconception, through the development of techniques such as the 'Last Planner' (Ballard & Howell 1994a, 1994b). The message from IGLC'95, however, was that these techniques remain only partial implementations of LC, and that a 'Theory of LC' is now needed to facilitate further reconception. Howell suggests that the problem of theory is not simply confined to LC, but to construction management as a whole:

'[...] construction management as a discipline suffers from want of a theoretical base adequate to explain the project process let alone how the industry works'. (Howell 1996)

Within the LC movement itself Howell and Ballard (1996) suggest 'advanced practice is now well out in front of theory, the lack of which now inhibits further progress in practice'. We take this to mean that theorising is now needed to aid the continuing development of the practical discipline of LC. Thus they seem to see theorising as a means to the end of informing further practical developments in the discipline.

Koskela has previously commented:

'Formalization of the scientific foundations of construction management and engineering is a primary goal for the research community.

[...] The practical application of the new philosophy has commenced and diffused without any scientific, formalised basis. The theoretical and conceptual understanding of the new production approach is still limited. [...] [T]here is yet no unified, coherent and consistent theory.

[...] An explicit, preferably formalized theoretical basis is necessary for transfer of the new philosophy to new settings and for effective education'. (Koskela 1992)

This constitutes a similar call for continuing development of LC, the 'end' of the exercise, to be carried out by theory-building (the 'means'). However, in addition, there is a clear call to

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<sup>1</sup> As well as in the two works cited, this point was made at the IGLC Workshop, held at the University of Birmingham, 23rd August, 1996 (Tape in authors' possession).

engage in the process of *scientific* theory building. This call contains a view of what constitutes scientific theory building: the formalisation of knowledge into a unified, coherent and consistent whole.

We suggest that formalisation *per se* does not necessarily give us a science. We shall argue below that science is a very specific type of formalisation implying a number of explicit methodological procedures and a distinctive 'set of rules' for theorist's practice. For a researcher to suggest that s/he is doing science is to make a claim about his/her activities. If LC theorists wish to make this claim, they must establish the nature of that claim so that it can be seen to be vindicated. Thus our critique is directed at the level of the claims that may be made about the status of theoretical work in LC, not at the level of the actual practice of LC theorists.

Within the suggestion that the new philosophy needs to be codified by means of theories that are scientific, although Koskela does not explicitly mention it, there must be the implied suggestion that these theories will be produced by the application of theory-building methods that are scientific. The implication is there by virtue of the fact that scientific theories are (by definition) produced using scientific methods. These methods are those upon which the community of scientists agree and which constitute the 'rules of entry' to the scientific 'corpus of knowledge' (Kaufmann 1958).

We suggest that in parallel to Ballard & Howell's call for there to be negotiation between means and ends in the practical activities of the *construction* process, there should be similar negotiation between ends and means in the *theorising* process. As such, there may be other, alternative means to the theory-building end, which are not necessarily scientific.

As we will suggest, all theorising is carried out for a particular purpose, and the very fact that some of our theories are not scientific may be what makes them fit for certain (non-scientific) purposes. However, we wish to stress that the fact that these theories are not scientific cannot justify any lack of rigour, logic, coherence or consistency in the methods employed for their production.

If, however, LC researchers do wish to produce theories that are scientific; that is theories that comply with the 'rules of science' (as defined by the community of scientists), that are produced using scientific methods, and that can be admitted to the corpus of scientific knowledge, we wish to examine the nature and implications of that endeavour. The main theme of this paper therefore is to examine what a scientific theory of LC would look like. Before discussions of this, we discuss some general issues appropriate to any type of theory, the most important of which (and which underlies all types of theory) is the need for prior *description* of the phenomena about which we are theorising.

In starting a discussion on the nature of theory, immediate questions arise: why do we want to build theory? for whom? what will the theory look like? etc. It seems to us that these questions are rarely asked in traditional construction management thinking and have not yet been asked, let alone answered, in LC. Further, there are many other questions in addition to those above we must first ask and issues we must first clarify before starting the theory-building exercise, including another fundamental question: what do we have as a resource from which to build the theoretical base? It is these questions that we will consider now.

## Why do we want to build LC theory?

As we have suggested above, this question is rarely considered in construction management. We will deal here with two possible reasons for wanting to build theory, both of which we see implied in Koskela's call: first to develop a scientific theory of LC; second, to develop a set of heuristics to enable the efficacious communication of LC practice.

We have already addressed these two aims in Seymour, Crook & Rooke (1996) where we argue that construction management research serves two sets of 'customers': academics and practitioners. In that work, the metaphor of a customer-supplier relationship is used to explicate and clarify a number of issues surrounding the praxis of construction management research. We suggest that overwhelmingly in construction management the question is answered by reference to two reasons (either directly or in terms corresponding to them): these reasons for building theory are *truth* and *usefulness*.

Examining these two concepts, as they were employed in the work of Bennett (1983) we suggest that they are used erroneously. Truth and usefulness are seen (by Bennett) as lying at either end of the same scale, implying that, in research, truth is somehow compromised by usefulness (and therefore, as this is a 'fact of life', it is permissible to compromise truth for usefulness). Using the concepts in this way has given rise to confusion in much construction management research, resulting in researchers arbitrarily stepping outside the strict empirical programme that, as we shall see in the next section, they have committed themselves to.

The question, 'why do we want to build theory?' does, in fact, contain *two* elements that must be considered in parallel and that cannot be separated. The second element is 'for whom do we want to build theory?' It is because construction management researchers have selectively neglected this second part of the question that the scale above and the resultant confusion in construction management practice has evolved. As a result, we suggest that this single factor notion of truth be abandoned and replaced with the notion of 'fitness for purpose'. Thus in answer to this second question, we suggest that the theorist builds theory for two different 'customers'—other members of the community of academics and members of the community of practitioners (from whom the theory is, incidentally, derived)—and that the purposes and requirements of the two sets of customers are very different. Hence the criteria used for judging both the truth and the usefulness of the research depend strongly on whose requirements are being considered.

For *practitioners* 'fit for purpose' might mean something like 'useful to me in my situation: relevant to the practical problems I face in my work on a daily basis'. In other words practitioners may not be concerned whether the 'product' they are being offered by theorists has undergone sustained critique or peer review by the theorist's academic colleagues, but do insist that it has a practical pay-off. It is here that LC theorists, in meeting practitioners' needs, may wish to provide a set of heuristics, tools, and the like.

Fit for *academics*' purposes means precisely that the research *can* survive the rigours of sustained logical critique—it is not, however, necessarily required to be of immediate practical use to managers.

Though the different purposes for the theory imply different types of theorising, we show in the next section how there are some foundational issues common to all theorising.

## What is the nature of the theorist's enterprise?

What we wish to suggest is that LC theorising is a form of enquiry, an investigation of the world or a part of the world. Further, as academics and LC theorists, what we are engaged in is a form of *professional* enquiry<sup>2</sup>.

Further, as professional enquirers, we suspect that most, if not all, LC theorists would not claim to be engaged in metaphysical speculation, and would instead claim precisely the opposite, that their work is informed by the empirical world of ordinary, everyday human experience. Blumer suggests that an empirical investigation, the research enterprise, 'designed to yield verifiable knowledge of human group life and human conduct' (1967:21) must meet a fundamental criterion. He suggests:

[...] an empirical world exists as something available for observation, study, and analysis. It *stands over against* the scientific observer, with a character that has to be dug out through observation, study and analysis. [...] It is the testing ground for any assertions made about the empirical world. "Reality" for empirical science only exists in the empirical world, can be sought only there, and can be verified only there [...]' (*op cit*:21, italics in original)

Thus the viability of the theory-building exercise rests upon the ability of the theory to be verified in the empirical world, on the question, 'can the assertions of the theory be tested against the empirical world?'

## What is LC theory?

Sharrock and Anderson (1986:7) suggest that if we are to successfully enquire into the nature of theory, as we seek here to do, we must

'begin [our] inquiries in a location outside the frameworks provided by established theories. One such location is in the world our pre-theoretical experience, with the world as we ordinarily experience it *before* we start to theorize about it.'

In answering the question of 'what is theory?' we wish to follow their advice. What we shall do is to appeal to nothing more than our ordinary, everyday membership of a language speaking community and our competence at using that communities' language. What do we mean by this? What we wish to appeal to is what we would ordinarily, normally, unproblematically, unthinkingly do and say in the everyday world (the *Lebenswelt*) of pre-theoretical experience.

In this world of our pre-theoretical experience, we wish to highlight here two common uses of 'theory'—theory as *explanation* and theory as *instruction*. For example, David arrives late for

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<sup>2</sup> Though we wish to make an important distinction between professional enquiry (the theorist's enterprise) and what we will call 'lay enquiry'—the kind of enquiry we make on a daily basis using our common sense knowledge of the world as ordinary, everyday members of society—we do not wish to make this distinction in any way to imply that there is anything lacking in the processes of lay theorising, but to make entirely the opposite point, that even in this professional role, we do not, and cannot, eschew, transcend or override this *Lebenswelt* against which our theorising is performed.

the conference. I might ordinarily say, “Well I reckon that he’s been held up by traffic”. Here, I am theorising; I am producing a ‘theory-of-(David’s)-lateness’; my theory provides an *explanation* of David’s late arrival. Putting aside all issues of reliability, validity, generalizability, etc. for the moment, the first common, pre-theoretical use for ‘theory’ that we wish to highlight is thus that of providing an explanation of some phenomenon. Returning now to David; when he does eventually arrive, I might say to him, “I think that to avoid delays due to traffic congestion, it is best to use your bicycle”. Here, again, I am theorising; I am producing a ‘theory-of-avoiding-lateness’. In this case, my theory consists of a set of *instructions* for avoiding lateness (‘use your bicycle’). Hence, here is our second common usage of ‘theory’—a set of instructions (heuristics).

However, before we can embark on the theory-building exercise, either for explanatory or instructive purposes, there is a prior need to *describe* the phenomena we are explaining or for which we are giving instructions. We can only seek explanations of LC practice from accounts of what that practice is, from accounts of what people in construction actually do. Currently, throughout the vast construction management (CM) literature (and in the smaller body of LC literature), perspicuous accounts of CM practice are notable by their absence. Within management literature as a whole, we would point to examples such as Watson’s (1994) *In Search of Management* and Dalton’s (1959) *Men who Manage* as attempts to provide the kind of accounts of management practice that we see as a prerequisite for any theory-building about that practice. Indeed, Womack *et al*’s (1990) book, *The Machine that Changed the World* could be seen as an attempt to provide a perspicuous pre-theoretical account or explication (not explanation) of Lean Production<sup>3</sup>. Rather than theorising the new philosophy, the work serves as a detailed resource upon which any theory-builders can draw, and upon which it would seem entirely advisable to draw.

### **How would we produce theory that is scientific?**

As we have discussed above, Koskela calls for a ‘unified, coherent and consistent theory’ and ‘an explicit, preferably formalized theoretical basis’ to LC. If, by our theorising, we are attempting to build a body of findings, a ‘corpus of knowledge’ (Kaufmann 1958), it is assumed that there will be some codified ‘rules’ for admission to that corpus. Kaufmann ‘points out that science knows only that which has been admitted to its body of findings by the appropriate procedures’ (Sharrock and Anderson 1986:44). What are these rules? Sharrock and Anderson suggest ‘it is very difficult to define these rules. Certainly they are not regulations and are more like a set of commitments which [...] scientists seem to respect and advocate’ (1986:18). They do, however, outline them in the form of three maxims (*op cit*):-

- ‘1. When employing a theory, use only the terms defined in the theory.
2. Use only coherent, consistent and rigorous theories.
3. Use only theories which yield studies of real phenomena.’

What these maxims indicate is that the nature of our theory is dictated by the nature of the phenomena under study. This is to say no more than that said above by Blumer (1967:21), when he asserts ‘[...] [the empirical world] is the point of departure and the point of return in the case of empirical science. It is the testing ground for any assertions made about the empirical world’.

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<sup>3</sup> We owe this point to David Seymour (1996a).

As we have pointed out above, a necessary prerequisite for any theory is description. We suggest that it is possible (though not unproblematic) to produce a *description* that meets these three maxims. We would not wish to suggest that the description so produced is a scientific theory. To suggest that a scientific theory is doing nothing more than describing phenomena is a contravention of the logical grammar of language, whether that language is the language of ordinary, everyday members of society, or is of members of a community of professional enquirers.

We therefore suggest that the criteria above are incomplete for circumscribing the notion of a scientific theory. We wish to add to these the notion that an essential feature of scientific theory is the notion of *causality*, and therefore, by implication, the notion that the theories are also predictive (N.B. the opposite is not true—predictive theories do not necessarily imply causality; see Sacks *et al* (1974) for an example of a predictive but non-causal model). In fact, we would suggest that this, above all else, is the *essence* of a scientific theory. We support this statement by drawing the reader's attention to the established theories (laws) of natural science, specifically those of physics. All these theories, about which there would be no dispute (we hope) concerning their scientific status, are more or less immediately recognisable as causal theories. For example, Newton's Laws embody a deterministic, Laplacian notion of causality. In other scientific theories the causal elements may be less obvious, but even in such areas as 'fuzzy logic' and 'neural nets' they still remain.

We will now examine in more detail the third of the maxims given by Sharrock and Anderson. By discussing the phenomena that LC theorists are studying, we will show that LC theorising and LC theorists are engaged in a programme of *social* enquiry; LC theorists are studying social phenomena and, as such, any scientific theories they produce must comply with the postulates and methods of a *social science*. This implies that to adopt either a conception of 'scientific theory' in the *natural* science sense of the term, or the methods of enquiry used in the natural sciences, will be logically untenable.

We will discuss three postulates given by Schutz as minimum criteria that must be met for production of a social science. Many sociologists much greater than ourselves have grappled with the issue of a social science before us; the status of their achievements in producing a social science are problematic. Consequently the possibility of a *social science* is a difficult proposition.

We suggest that, given the phenomena we are studying, a more achievable objective would be to formulate a set of coherent, practically-oriented heuristics that address the same problems that practitioners themselves address. If, on the other hand, LC theorists insist on the hazardous project of scientific theory-building, then they must still make the necessary beginning of producing adequate empirical descriptions of their phenomena. Producing such descriptions, as we have suggested, is more problematic than might be immediately apparent.

### **What do we have from which theory might be built?**

We have suggested above, following Blumer that the resource from which our theory will be built (and indeed the topic of that theory) is the empirical world of everyday experience. In the case of LC, what we have available for study are the lived experiences and achievements of those members of a language speaking community who are doing LC. That construction management researchers are doing sociology may seem strange (and may come as a shock!)

to members of a discipline who have traditionally looked for theoretical, methodological and philosophical guidance in their work to engineering (with its theoretical roots in physics).

Some researchers might argue that they are studying only the 'technical' parts of the construction process, and are leaving study of the 'social' aspects of it to people like ourselves (we also note the characterisation of 'socio-technical' that is commonly used). The best example of this way of thinking is found in the various literature on systems analysis where construction or organisational activity is divided respectively into the 'social system' and the 'technical system' (Newcombe *et al* 1990) or the 'socio-technical system' (Checkland & Scholes 1990).

Our objection to this is based on our assertion that a prerequisite for any theoretical activity is description of the phenomenon under study. The socio-technical dichotomy is itself a theoretical construct. It pre-supposes the existence of an adequate empirical description of the phenomenon. Such a description, we suggest, is not available. Indeed, we would follow Scarbrough and Corbett (1992:1) on this point, maintaining that the distinction attempts the impossible: to 'separate the dancer from the dance'. What we are suggesting is that by adopting the labels of 'social' and 'technical', researchers have already characterised the phenomena under study without first describing them 'as they are'.

What does this imply for our theoretical work? The crucial point is that as social researchers, we are in a very different position to natural scientists, in that the world we research is already characterised. Sharrock and Anderson suggest (following Schutz) 'the phenomena of sociology, [...] are *already and intrinsically* typified [by the members themselves], and sociology itself must therefore be counted as a 'second order discipline', building further typifications on and in addition to those already available in society itself.' (1986:9). Hence it is the reasoning of managers in construction that must be the central topic of our inquiries; the theorist's reasoning, typifications, schemes of interpretation, theories are secondary to and are subject to test by that reasoning. As such any theoretical work that attempts to transcend or correct that reasoning by overriding or ignoring it can only ironise the practitioner's world. Further, any theoretical schema of the researcher's own making can only be incomplete, partial reflections of that world. In a different context, Sharrock and Button (1995) use the term 'simulacrum' to denote that the kinds of models of social life produced by researchers are only highly simplified simulations of 'real life'.

One of the best descriptions we have found that sums up the central problematic of social enquiry is provided by Dr John Lee. He suggests:-

The world is thoroughly and continually characterised by any language speaking community and the question for sociology is what consequences follow from the fact that we live in a world already characterised. The point, of course, being that there is a massive, monumental priority involved in persons' characterisations or descriptions of their life. That priority is established by the fact that it is *their* descriptions, or *their* characterisations that provide for their actions. That is the key point: not ours, theirs. (Dr John Lee, 1995).

To explain in a different way, what we are suggesting is that an empirical programme of social research must adopt a *Verstehen* approach to its work. This German word, adopted many years ago by Max Weber (1933) translates into English as 'understanding'. However it does



not refer to a causal understanding, but to understanding meaning. A *Verstehen* approach means an approach that seeks to understand the other person's point of view—what her actions mean *to her*.

Thus, as social researchers, we are not in a position to unproblematically characterise the world of study, in the same way natural science can. Why is this? Simply put, it is because our 'objects of study' can and do attribute meaning to their world; they are conscious, sentient beings (for instance, when we see an apple falling from a tree, we attribute significance to that phenomenon—the apple is ripe, perhaps; the wind has shaken it loose; the law of gravity holds, etc.). In the natural sciences, the objects of study do not attribute meaning. The researcher is free to impose his/her meanings on phenomena, without fear of contradiction (for instance, in the previous example, neither the tree (nor the apple) knows nor cares about the falling of the apple, neither can the tree contradict the researcher who attributes its falling to gravity, an act of God, a freak typhoon in Albuquerque or whatever).

Schutz also points out that social life is *intersubjectively* organised, that is organised as an ongoing dialogue *between and among* its subjects. This leads him to formulate three postulates for scientific models (theories) of social life. In the next section we will outline these postulates, and discuss their implications for LC theorising.

### **What are the postulates for a scientific theory of LC?**

Given the above discussion, we can now make two programmatic statements about LC theorising:-

1. LC theorists are engaged in a programme of social research; and
2. the resource that programme will employ in its work is the lived experience and activity of those people who are doing LC.

With these in mind, what would be needed for a scientific theory of LC to be possible? To state the problematic of LC another way, what would a scientific description of LC practice look like? Schutz (1971:43) provides three postulates for scientific model constructs of the social world which can be seen as providing only the necessary minimum criteria for a social science<sup>4</sup>. These are:

*The postulate of logical consistency* The system of typical constructs designed by the scientist has to be established with the highest degree of clarity and distinctness of the conceptual framework implied and must be fully compatible with the principles of formal logic. fulfilment of this postulate warrants the objective validity of the thought objects constructed by the social scientist [...].

*The postulate of subjective interpretation* In order to explain human actions the scientist has to ask what model of an individual mind can be constructed and what typical contents must be attributed to it in order to explain the

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<sup>4</sup> We are grateful to Professor Wes Sharrock for pointing out that the requirements Schutz' criteria lay down may be seen as a fairly liberal interpretation of what constitutes a science.

observed facts as the result of the activity of such a mind in an understandable relation [...]

*The postulate of adequacy* Each term in a scientific model of human action must be constructed in such a way that a human act performed within the life-world by an individual actor in the way indicated by the typical construct would be understandable for the actor himself [...]

What are the implications of these postulates for researchers attempting to build a social science? As we have said, the first, critical step for building a scientific model or theory is adequate, empirical description of the phenomena it is modelling (in the case of LC, we have said that these phenomena are what people are doing). Schutz suggests how the first of these postulates provides the terms from which the models are built, ensures the models are objective and is important for distinguishing the scientific model from the common sense thinking it is designed to supersede. The first postulate therefore refers to the 'building blocks' of our theories. These terms must be comprehensively, logically and accurately described before the theorising commences. Once this description has been done, the theorist must use these terms and these terms alone in his/her model and must use the terms consistently, to refer to the 'same' thing. In particular, s/he should not speciously import undefined terms into the model in an arbitrary manner (thus making up new terms as the theorising progresses). Hence descriptions used for scientific models must be constructed from previously-defined terms.

The third postulate ensures that the descriptions the scientist uses, correspond with the common-sense experience they are describing and that they are recognisable by the actors whose activities the model describes, thus accommodating the point made in the quote from John Lee above. It makes the entirely reasonable stipulation that an adequate description of the actors' world implies a description that is recognisable by the actors themselves. What this implies for the theorist is that s/he can describe things (in the terms provided by the building blocks) that actors do not describe, or redescribe things (in the same terms) that actors do describe, but always the descriptions that the scientist uses must be recognisable to the actor. David Seymour (1996b) provides an interesting example of 'how not to do it', of a scheme where the model's terms were not recognisable by the actors the model purported to be about. He says:

'I was at a seminar [...] given by Ron Baden Hellard on company culture. He asked delegates to fill out a questionnaire relating to Wendy Hall's 'points of the compass' typology. [...] In answering one of the questions on the list somebody asked what the question meant. 'You know (said RBH) like a Japanese firm'! So, for 'northern firm' read a firm 'like Tarmac' for an 'eastern' firm, read 'Laing'. So what is the use of the labels? To understand what they mean, you resort to common language and experience. The terms are then supposed to specify the objective characteristics of different firms. The whole process is logically fallacious and does not help in any practical way.'

Taken together these two postulates have important implications. It is important to note, as we did above with reference to the social-technical dichotomy, that one of the activities the postulates preclude is using members' ongoing descriptions as scientific descriptions. If the scientist were to attempt to do this, s/he would be attempting to simultaneously use as a

*resource*, for his/her descriptions in the scientific model what are also the *topic* of those descriptions. This would be in violation of the postulate of logical consistency.

What therefore can the scientist use for his/her descriptions? We have said above that social science is a 'second order discipline', building its typifications on and in addition to the typifications already available in society. It is necessary to build these typifications because actors' descriptions, while fulfilling some kind of (as yet unexplicated) logic, do not fulfil the principles of *formal* logic specified by Schutz. Hence these scientific constructs 'are designed to supersede the constructs of common-sense thought' (1971:5) and are thus 'constructs of the second degree, namely constructs of the constructs made by the actors on the social scene, whose behavior the scientist observes and tries to explain in accordance with the procedural rules of his science.' (*ibid*:6).

Here we encounter a problem. There is an essential tension between the requirements of the first and second postulates on the one hand and the third on the other. This because producing the scientific account because fulfilling these postulates involves positing the second order constructs of the scientific account as improvements on actors' constructs. These may therefore be unrecognisable to the actors whose activities they purport to describe thus violating the third postulate. What the scientist is doing is necessarily giving an account of the actors' practice that stands in competition to the actors' own account. Resolving this competition is the way to dissolve the problem. Dissolving the problem is fundamental if the scientist wishes to proceed. In addition, as a consequence of Schutz' location of objectivity within the first postulate, the social scientist must address the issue of indexical expressions.

Garfinkel describes the difficulty: he shows how indexical expressions—'expressions whose sense cannot be decided by an auditor without his necessarily knowing or assuming something about the biography and the purposes of the user of the expression, the circumstances of the utterance, the previous course of the conversation, or the particular relationship of actual or potential interaction that exists between the expressor and the auditor' (1984:4)—are an inescapable feature of everyday life. For example, 'he' refers to a different person, depending on the context of use. He discusses how the programme of scientific theorising is to replace indexical expressions for objective (context-free) expressions, but that in social science this programme remains unfulfilled, and that its results remain 'unreasonably programmatic' (*ibid*:5). The problem of indexicality which has bedevilled logicians since the birth of the discipline: objective terms retain the same meaning in any context; indexical terms do not do this.

The description provided by fulfilment of the first and third postulates facilitates construction of the model specified in the second postulate. Schutz' second postulate implies that the model that is constructed must be sufficient to explain all the observed facts of behaviour 'in an understandable relation'. In other words it must be a comprehensive, causal and predictive model. Thus it must specify the logical relation between the terms described above; it must cover every 'what if' scenario that is logically possible between its terms. It is within this model that the essential notion of causality is provided.

In Schutz' work, the model constructed is a model of the mind. What this model of the mind specifies is what causes the behaviour that has been accounted for in the description above. We acknowledge that researchers may not want to provide a model of the mind in order to fulfil the causality requirement. However, if they do not, it is a requirement of the scientific

enterprise that they provide an comparable alternative that adequately meets the scientific need for causality.

What, in summary, do Schutz' postulates specify? What, according to Schutz, constitutes a social science? Within these postulates, we have traced the necessity for two things: i) a logically consistent account of the social actor and ii) interlocking with this account, a model of the actor's mind. The account results from the stipulation of the first and third postulates. These specify the necessity for an account of what social actors do—an account of social practice—given in terms that, or as a description that, these social actors would recognise (with the consequent implication that the account be produced using *Verstehen* or comparable methods). The second part, the model of the actor's mind such that the model accounts for the things the actor does, results from the second postulate and provides the essential causality of the scientific endeavour.

We have now identified the postulates for a social science and their implications. As part of this project, we have described two practical difficulties the social scientist must overcome. First there is the tension between the necessity of providing an account—both logically consistent and at the same time recognisable to members—which results from the necessity to reconcile the first and third of Schutz' postulates. We have seen that in order to provide this account the theorist must make use of 'second order constructs', but that these are in competition with members own constructs. Second, Schutz requires the researcher construct a model of the mind to fulfil the scientific requirement for causality. This leads to a conception of the actor as a 'homunculi' whose conduct is determined but who, nonetheless, is supposed to act as if self-determining (1971:47).

Peter Winch provides a view of social enquiry and of the first of these difficulties which calls into question the very possibility of reconciling the first and third postulates. He criticises forms of social enquiry which involve the evaluation of actor's conceptions in terms of the investigator's own normative assumptions. In effect, this criticism amounts to a rigorous working out of the logic of *Verstehen*, which lies behind Schutz' third postulate. If this logic requires that an adequate account is one that captures the actor's point of view, then any concept which is intended as an improvement upon the actor's conception is also an obstacle to its fulfilment.

Winch (1970) makes this argument with regard to Evans-Pritchard's (1950) study of the Azande practice of consulting the oracle. In this study, Evans-Pritchard employs a *Verstehen* type method in his social enquiry. However, he is committed to a scientific model of causation in his understanding of the natural world. There is, of course, nothing wrong with this commitment, which is no doubt one which is shared by everyone here present. But when he attempts to evaluate Zande belief in terms of this scientific model he is, Winch argues,

'crucially wrong, in his attempt to characterise the scientific in terms of that which is 'in accord with objective reality' (1970:80)

Although Evans-Pritchard realises that Western scientists and Azande magicians have different conceptions of reality, he wishes to assert that the former are more accurate in their conception than the latter. If he is to do this he needs a concept of reality which is,

'intelligible and applicable outside the context of scientific reasoning itself'  
(*ibid*:81)

While recognising that we need to check our conceptions against an independent reality, he points out that even the nature of this independent reality is determined by our beliefs. Thus, for instance, religious beliefs are checked against the will of God. What is more, these beliefs are inherent in our use of language.

‘Reality is not what gives language sense. What is real and what is unreal shows itself *in* the sense that language has. Further, both the distinction between the real and the unreal and the concept of agreement with reality themselves belong to our language. [...] We could not in fact distinguish the real from the unreal without understanding the way this distinction operates in our language. If then we wish to understand the significance of these concepts, we must examine the use they actually do have in our language. [...] The general nature of the data revealed by the experiment can only be specified in terms of criteria built into the methods of experiment employed’ (*ibid*:82)

Evans-Pritchard is trying to use a conception of reality which is not determined by its actual use in language. This isn’t possible. In order to argue that scientific criteria constitute a link between reality and our ideas, which Zande magic does not, it would be necessary to refer to a discourse which supersedes both science and magic. To do otherwise is merely to construct a circular argument: of course magic makes no sense in scientific terms. Conversely, science may be nonsensical from the perspective of Zande magic.

The point is that the logic of *Verstehen*, just like the logic of scientific objectivity, demands that you do not contradict your data.

Ethnomethodology (Button, 1991; Garfinkel 1984; Heritage 1984; Turner 1975) provides an empirical alternative to scientific theory-building. This form of social enquiry, which preserves some of Schutz’ objectives, does not claim to constitute a social science. Instead, its studies aim to explicate actors’ own understandings of phenomena, thus pursuing the logic of *Verstehen* to its conclusion.

### **Ethnomethodology (EM) - the respecification of sociology**

Garfinkel’s respecification of sociology commenced with the insight that what conventional sociology had taken as its *resource* for its studies, could be taken as a *topic* of EM studies.

‘[For ethnomethodology], in contrast to certain versions of Durkheim that teach that the objective reality of social facts is sociology’s fundamental principle, the lesson is taken instead, and used as a study policy, that the objective reality of social facts *as* an ongoing accomplishment of the concerted activities of daily life [...] is, for members doing sociology, a fundamental phenomenon.’ (Garfinkel 1984:vii)

He suggested that conventional sociological enquiry took the existence of an ordered world, available for study and analysis, for granted and as such conventional sociology was not truly empirical, preferring (as we have shown above with reference to the social-technical distinction) to theorise the world rather than study that world. In an important sense, EM enquiries investigate what for conventional sociology, are unexplicated foundational issues. However ‘although they [EM enquiries] are directed to the preparation of manuals on sociological methods, these are *in no way* supplements to “standard” procedure, but are distinct from them (Garfinkel 1984:viii). Thus, EM is not ‘just another sociological

perspective’, but is entirely different from mainstream sociology. Garfinkel’s great insight was first to make the ‘uninteresting’ everyday activities of members a fundamental topic of study, and secondly to point out that members activities are at the same time methods used by members to make those activities accountable—that is ‘visibly-rational-and-reportable-for-all-practical-purposes’ (*ibid:vii*).

Garfinkel calls this feature of naturally-organised phenomena ‘reflexivity’ (1984:1). Reflexivity implies that I do not do something and *then* make it accountable. The two are ‘incarnate’ (*ibid*) and done simultaneously with no possibility of separation. Thus when I open the door, I (simultaneously and inseparably) *accountably* open the door. I do not subsequently make, nor have I previously made my opening the door accountable; the two are contemporary practices. If I subsequently describe my action, I am not making my ‘opening-the-door’ accountable either ‘again’ or ‘for the first time through’. What I *will* be doing is simultaneously (something like) giving-an-account-of-me-opening-the-door *and* (though not separately) making-‘me-giving-an-account-of-me-accountably-opening-the-door’-accountable -for-the-first-time.

It is in the reflexive practice of ordinary members of society that Garfinkel locates social order. EM enquiries are thus concerned with the achievement of social order

‘in that, and just how, it is every society’s locally, endogenously produced, naturally organised, reflexively accountable, ongoing, practical achievement, being everywhere, always, only, exactly and entirely, members’ work, with no time out, and with no possibility of evasion, hiding out, passing, postponement, or buy-outs’ (Garfinkel 1991:11).

He thus suggests that social order is to be found ‘in and as of the workings of ordinary, immortal society’ (1992). In other words, order is not only *in* members’ activities, order *is* their activities—through their activities members ‘do’, ‘are doing’, ‘have done’ and forever and immortally ‘will continue to do’ order. Hence, for EM, ‘Lean Construction’ is to be found not only in what LC people do, but also ‘Lean Construction’ *is* what people do. Thus not only do members activities provide the building blocks for LC theory, their activities *are* that theory.

### **Ethnomethodology and its respecification of Schutz’ postulates**

As we have discussed, Schutz’ second postulate requires a causal model of the mental to fulfil the scientific aim. EM does not attempt to meet this postulate; it does not attempt to give an account of the mind, let alone a causal account. EM studies do not undertake this ambitious project. EM is firmly rooted in the empirical; the problems of providing an adequate description of social life are more than enough for its (deliberately) limited ambitions.

We have already highlighted Garfinkel’s discussion of the problem of indexical expressions. He observes,

‘The properties of indexical expressions and indexical actions are ordered properties. They consist of organizationally demonstrable sense, or facticity, or methodic use, or agreement among “cultural colleagues”. Their ordered properties consist of organizationally demonstrable rational properties [...]’ (*ibid:11*)

He suggests that this ‘demonstrable rationality’, this invariant property of indexical expressions is sociology’s fundamental phenomenon and says, ‘I use the term “ethnomethodology” to refer to the investigation of the rational properties of indexical expressions and other practical actions as contingent ongoing accomplishments of organized artful practices of everyday life’ (*ibid*:11). In this way, Garfinkel respecifies the Schutzian problematic.

### **Conclusion: ethnomethodology can inform LC theorising**

As we have stated, a necessary prerequisite to theory-building is adequate description of the phenomenon. Whether what is being attempted by the LC movement is scientific theory-building, or the production of a set of practically-oriented heuristics, we believe that a programme of EM-informed studies, using ethnographic methods could provide the description that is required.

EM accounts of LC practice can serve as both a description and a definition of what LC is. An illustration of how this might be so may be found in a classic EM work, Weider’s book, *Language and social reality: the telling of the convict code* (1974, see also 1975), where he explicates the ‘code’ which is followed by inmates in a halfway house for offenders. The substance of the book, and the insights it provides are extensively discussed in Seymour (1996a). In Weider’s study, his account of the code-in-use serves as a description and, in an important sense, a definition of the convict code. By exploring common-sense concepts-in-use, practical reasoning and logic-in-use, EM accounts can provide this dual function.

While EM accounts can serve to assist in constructing both types of theory, we see the development of heuristics as being both more immediately beneficial and more do-able for theorists. We therefore propose two distinct areas of LC into which an EM-informed programme of work would provide insights:

- i) EM studies of the teaching practices of LC academics. These would facilitate assessments of the heuristics in use and the development of improved heuristics by LC theorists.
- ii) EM studies of the learning practices of managers attempting to identify the methods by which they put LC techniques into practice. Again this would facilitate development of improved heuristics, by ensuring that heuristics-development is firmly focused towards the ‘real world, real time’ (Hughes *et al* 1994:20) concerns of those ‘on the ground’.

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