

## **Developing Theory in Lean Construction**

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### **Abstract**

The consensus at the Third Annual Conference of IGLC was that a Lean Construction (LC) philosophy and practice, distinct from existing construction praxes, are beginning to emerge. The need was identified to articulate and refine LC theory. In an attempt to contribute to the pursuit of this aim, the paper elaborates the proposition that a characteristic of existing organization and management theory is its inability to provide an adequate account of its own praxis. This is so because such theory is rooted in the rationalist or Cartesian paradigm which assumes the possibility of distinguishing between objective and subjective, and is, therefore, unable to address the actual processes, dynamics and so on that currently exist. In particular, there is a need to clarify, through empirical study, the relationship between research and its associated theory building and practice. Thus, there follow two issues which must form part of the LC agenda. First, a much more detailed understanding of current practice is necessary. Second, LC researchers, in pursuing their essentially action-oriented or developmental approach, in which the demonstration of practical benefit is at a premium, must be careful not to fall into the rationalist trap. This will be achieved to the extent that whatever theory they develop reflects the empirical realities that constitute the processes in which they are involved.

### **Keywords**

Lean Construction, rationalist philosophy, theory, method, socio-technical systems, practical reasoning.

### **Introduction**

From last year's IGLC conference, for me, two things stand out. One was my puzzlement about what people meant when it was generally agreed that the group had to break out of the conceptual straightjackets of conventional thinking and develop a new philosophy and a theory of lean construction (LC). The second was when Greg Howell took us to a fast food restaurant where we had breakfast. He drew attention to the way the process worked. It worked very well. Clearly, someone had thought about it. With the case of that restaurant in mind I want to raise the question of what we mean by theory.

The restaurant owner together with his staff, I take it, had devised a system that got good food efficiently and cheaply to customers. How far did theory come into what had been achieved? In pointing out how the system worked, with knowledge of what went on behind the scenes, did Greg theorise it for us or did he *show* us what was going on? He taught me to see what the system essentially consisted of. It could be expressed as a flow chart using the repertoire of symbols that the group has developed. It is possible, even probable that the restaurant owner had sat down with a paper and pencil and had carried out an equivalent mapping exercise. He may even have consulted with Greg thinking through questions like: where do we start? Does the customer go to a single start point and place his order regardless of the kind of food to be ordered or does s/he have to choose from a number of points depending on the food required - salads, hot or cold, drinks-only and so on - a method adopted in some restaurants?

Do we have here a case of theorizing? Perhaps. But I think it is more accurate to call it practical reasoning and this paper is about why I think this is so and about the importance of the distinction. The paper is, thus, about theory, too long by far, and I apologise. However, I don't apologise for the fact that there are weighty matters at issue because these are not of my making. My main point is that as practical people trying to improve the efficiency of construction there are important practical things to do. If we want to talk of philosophy and theory, and I think we should, this is deep water and we either learn to swim in it or stay on dry land.

So, how do we escape the conceptual straightjackets that construction management research and practice have inherited, since it was agreed at last year's Conference that a fundamental rethink is required in order to do so? I want to make some suggestions concerning the nature of these straightjackets.

Two of them derive from René Descartes (1596-1650). The first is the powerful philosophical influence within western science which, in broad terms, posits that there is a material world governed by causal laws which are accessible to scientific research and interpretation. This, so it goes, is the real world though it doesn't appear that way to our common sense experience. Human subjectivity - the mind and the senses - bestows on it qualities which are not actually there. The world as perceived by common sense is an illusion but scientific reason is capable of revealing the reality.

The second is that people are conceived of as both part of the material world and, therefore, subject to its physical laws, and distinct from it; the person, consciousness or the subjective being somehow inhabits the physical body - what Ryle (1963) called 'the ghost in the machine'.

A consequence of this dualism is to give science the task of looking for machine-like, cause-effect relationships in the material world, and this includes the working of the human mind. Certainly, the machines - both the world and the human mind - are very complex ones, but it is the task of scientific theory to make their working clear and, crucially, they stand apart from human consciousness.

I will suggest that to explore what we mean by theory, in developing the insights that lean construction thinking provides, we must escape this Cartesian dualism. In particular, I will consider the distinction between flow and conversion activities arguing that the notion of conversion is rooted in the Cartesian model and that there is a danger of casting flows in the same conceptual format as conversions. This would be a great loss since I take the concept of flow to refer to the much more subtle aspects of how the social world is constituted. It should be our concern to keep these subtleties available for enquiry and understanding. To do this we must be clear about the kinds of theory that we are looking for; what are possible and what are not.

### **Scientific Theory Versus Common Sense Theory**

The influence that Cartesian dualism has had can be seen, first, in the way the human or social sciences have made it their business to establish scientific versions, called theories, of what is really going on in the world as against what people think is going on. Second, is the attempt to discover just how the subjective - or consciousness - inhabits or is located in the physical body. Here, again, in this latter enterprise, we see the aim of providing a scientific version,

expressed in terms of the properties of physical matter - the body and particularly the brain - of what we experience as consciousness.

Having noted these two manifestations of Cartesian dualism, I will attend only to the first - that is - the tendency in the human sciences to look for scientific theory that somehow lies behind or informs human action and provides a truer account of it than the theories or accounts that people themselves offer. For the interested, a brilliant account of the second issue is given by Button *et al* in 'Computers, Minds and Conduct'. (1995)

I begin with an example of what I am driving at. 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)

My question here is: why didn't they ask the employees? Within the Cartesian paradigm, I suggest that the answers to this question are of the sort: The employees themselves don't actually know: That there are underlying patterns that can only be accessed by statistically valid, standardized questionnaires: That 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 individuals' consciousness of it.

Throughout Nystrom's review, which, I submit, is typical of the conventional view of the scientific enterprise, as applied to human organization, explanations are sought in terms of dynamics or forces which inhere in the social processes which we, as social beings, play out from day to day. All the great theorists, Marx, Weber and Durkheim, partake of this view of what it means to explain social processes and that, in short, is to provide theories *about* them. The three theorists that I have just mentioned were very much aware of the fact that people themselves have theories about what they are doing and that they are conscious of acting in terms of what is socially sanctioned or proscribed - call them theories-in-use - but, nonetheless, saw it as the scientific enterprise to look for the patterns and forces that lay behind them.

I want to propose an alternative to this conception of the scientific enterprise and that is to look more carefully at the theories that are actually there, in use; to find out how they are used in particular settings and for what particular purposes. That is, not to construct theories *about* people but to find out the theories *of* people (*their* theories) and to establish how they are used to create the patterns and regularities that we expect of social life.

To adopt this approach requires that the scientific enterprise itself be recognised as the social activity it is - no more and no less - and may, therefore, be subjected to the same scrutiny; how do scientists go about doing their research and constructing theories?

The alternative that I am proposing is demonstrated very effectively in a study by Lawrence Wieder (1974) of the code of conduct that exists amongst convicts in a penal institution. Though the 'convict code', known as such by the convicts themselves and all who have to do with them, undoubtedly exists, it is impossible to say exactly what it consists of. However, for those 'in the know' it is quite clear on any given occasion how the rules of the code are to be interpreted and applied. To understand what is going on in settings where the code is in operation it is necessary to understand the code. It thus has a kind of double status: it is both a fact of the situation and it is an interpretive device.

In the study of work and occupations the fact that it is difficult or impossible to articulate tacit or taken-for-granted knowledge has been extensively commented on. Jamous and Peloille (1970), for example, talk of professional practice as comprising *technical* knowledge and *indeterminate* knowledge. While the former is codified and to be found in manuals and textbooks, the latter is recognised in such terms as 'feel', 'flair' and 'aptitude' and is gained through the experience of practice in the profession. Similarly, Argyris *et al* (1985) and Schon (1983) have respectively developed the notions of *action science* and *reflective practice*. However, despite the similarities between what these writers observe about the tacit and taken-for-granted nature of practical knowledge, Wieder's point is that in the code itself is to be found the 'theory' of what is going on. The code is both an empirical fact and an explanation. They do what they do *because* they are complying with the code. There is no need for any theory which offers to explain why they are really doing it. In contrast Jamous and Peloille and Argyris and Schon do seek to provide higher order theory. They try to theorise the theory that they see as being used by the practitioners. The danger with such an attempt is that the empirical reality (i.e. the theories in use), that it should be their business to report, is lost or obscured, or, worse still, is supplanted. The theorist's theory is offered as the true theory! Thus, for example, in Schon's description of a teacher of architecture working through a design problem with a student, fascinating as far as it goes, Schon, as it were takes a pace backwards and gives his *own* commentary on what is going on. The empirical facts of the situation -how architect and pupil make sense to each other - are cut short and submerged by Schon's version of what went on. To be fair to Schon, he goes much further than most in tackling that vital question of how people *do in fact show what they know beyond what they can say*.

In contrasting Wieder's radically empirical intention with that of conventional sociological theory building, Anderson and Sharrock (1986) write:

"Does the Code explain the behaviour of convicts? Such an argument is likely to be a specific form of the more general debate about what kinds of thing can explain human actions. Can rules do this? Rules are a kind of idea and there is an open question as to whether ideas can really explain actions. Put another way, rules, as a kind of idea are properly counted as a part of culture, and the question then is: does culture rather than structure (i.e. the arrangement of social relations) determine what people do?" (p 49)

These are the kinds of question that sociology conventionally poses, however such questions beg a prior question of whether patterns of conduct and the rules that inform them have been adequately identified and described in the first place.

"It furthermore assumes that whether or not the rules do have an explanatory connection with the actions making up the pattern they do have another more

basic connection with them i.e. they are the *rules for that setting* [sic]. The convict code is identified by its very name as a set of rules which putatively regulate the lives of convicts. It is not a theoretical construct, a set of rules contrived by a sociological theorist for the express purpose of generating the observed behaviour.” (*ibid* p 50)

There are two points of note here. The emphasis on *rules for that setting* is to stress that the concern of the researcher is to understand how *that setting* works. Whether or not we can find in it general patterns that may be found elsewhere is not the concern. Second, the force of the phrase *generating observed behaviour* is that, if we, as researchers look at behaviours in terms of our previously formed theories, we see what we see as instances of them. This is not to say that in the *natural attitude* as opposed to the *scientific attitude* we don't do this. In this distinction the former refers to the fact that in matters of everyday affairs we accept things as they come for all practical purposes. Adopting the scientific attitude, our concern with respect to the object of our enquiry, is to ask questions about it that we would not normally ask. However, it is to be emphasized that in doing science we adopt the natural attitude towards everything else that is not the direct object of our enquiry. Thus in conducting an experiment on the physical composition of a piece of wood we take for granted the lab bench on which we are doing the experiment and everything else around us. Thus, in the natural attitude we do look for similarities and differences as between this and that kind of situation. We do generalize. But to do so in the scientific attitude prevents us from seeing how this particular situation is constituted. This point has profound consequences for construction management research.

In short then, what is being proposed here is that we abandon the search for the kind of general theory which has been the conventional objective of the social sciences for two reasons. The first is scientific/methodological the second is practical.

### **Scientific/Methodological Reasons for Re-evaluating Conventional Research Agenda**

Assuming that the purpose of management and organizational research is to provide an account of what managers do in their working lives and to bring the powers of science and rigorous research to help them do it better, the consensus seems to be that this purpose is not being achieved (see the reference to Nystrom and Starbuck (1984) below). I suggest that though there is, in principle, the possibility of distinctive, scientifically valid theories of social life, it remains unrealized and is probably unrealizable. This means that what researchers do and what the people they research do must be looked at as *strictly equivalent processes*. Certainly, people do different *kinds* of things subject to different sets of rules. Thus, notably, as described above, the contrast may be made between what has been called the *natural* and the *scientific* attitudes.

In adopting the scientific attitude towards social matters, however, it is considerably more difficult to keep it separate from the natural. Nonetheless, most researchers or theorists of research method, especially those who seek to justify so-called qualitative research (e.g. Yin 1994), offer rules about how research should be carried out which derive from a particular conception of the relationship between researcher and physical phenomena found in the natural sciences and which has no place in the study of social phenomena. With regard to these phenomena, we must accept that as researchers we approach them in the same way that we approach any practical problem of everyday life with one important proviso: we can try to dispense with the preconceptions and methods (as provided for example by Yin) which are

intended to *imitate* the methods of the natural sciences. As imitations, as tools not fit for the purposes for which they are required, these methods can only result in bad science. Also, we can adopt the scientific attitude and not take for granted what is going on in a given setting, as we normally would, nor look for general patterns and comparisons as we normally would, rather, to ask how it is constituted. The despised notion of going in with an open mind is precisely what is required. It is difficult, but the scientific challenge is to elucidate the conceptual organization of the worlds we observe and not impose our own conceptions on them. To the extent that we wish, actively to participate in these worlds with the conventional view of research methods, the researcher's role becomes strictly equivalent to that of 19th Century missionaries: 'putting the natives right'. As participant, the researcher has the same right as anyone else to offer her/his view as worthy of consideration. However, s/he cannot justify that worldview as being more truthful or scientific than theirs. The scientific part comes in elucidating their worldview. To the extent that the researcher wishes to share his/her worldview with them, this becomes a collaborative, normative process in which we say: "let's get together, let's see if we can establish what **our** problems are and find ways of tackling them in ways which **we** think are better". Any notion of science or 'real' theory doesn't come into it.

In contrast to this view of research, the conventional management research agenda seems to be that:

- i) through 'systematic, careful enquiry or examination' it is possible to look through what is going on to what is **really** going on;
- ii) the researcher **discovers** something the researched did not know about;
- iii) the researcher (now theorist) builds up a corpus of knowledge and procedures for establishing its **veracity**.
- iv) though there may be a range of specific objectives in the studies undertaken, **generalizability** is always an ultimate goal.

I will now consider the viability of this agenda. I pose the question of what this research sets itself to do by asking what are the standards against which research is required to defend itself in order to qualify as methodologically sound. Anderson and Sharrock (1986) drawing on Kaufmann (1958) suggest that these standards are:

1. that there should be some kind of conceptual or theoretical framework
2. that the terms or concepts used in the theory should be clearly defined
3. that the research should be rigorously conducted within this framework and other undefined concepts taken from other frameworks should not be smuggled in
4. that the theories should be about real phenomena -things, events, actions which have identifiable characteristics which can be observed
5. that the researcher point out the limitations concerning the inferences that may be drawn when the framework is used in research; the scope of their generalizability
6. that the researcher should explain the procedures used so that the reader can judge the validity of the data offered

It is standardly conceded that it is difficult for some research to meet these stringent standards. Particularly where qualitative data are used, there is preparedness to weaken them

and advice is given about how to, as it were, limit the damage, how to control bias and the intrusion of self, how to apologise for the study's having fallen short of the ideal and so on.

The point is, however, that **no** study in the whole corpus of sociological literature, let alone the management literature, comes anywhere near meeting these standards and I challenge the reader to cite one. The conclusion, following Garfinkel (1952), is that there is a mismatch between what is **assumed** to be possible (and therefore attempted) and what **is** possible. Here, then, we are presented with another challenge: why not try to find out what is possible whilst meeting the standards that have been laid out? Garfinkel took up this challenge by subjecting the work of Talcott Parsons (1949), whom Garfinkel saw as a brilliant theorist, to the test. If Parsons can't do it, nobody can. Parsons was tried and found wanting.

Firstly, the standards require that the phenomena we are researching should be considered only in terms of the framework we have declared and what steps we have to take to apply that framework. All other considerations must be controlled out or bracketed off, and we must apply only that framework. To use the metaphor of playing a board game, say chess, once we have chosen the game we are going to play we must abide by its rules and not arbitrarily import those of another, say draughts (checkers). We must ignore all the other possibilities of what we might do with the pieces - take a queen by jumping over it with a bishop. Similarly, once we have decided on the theoretical framework we are going to use in our research and defined its terms, we must ignore everything else we know or might do.

Secondly, we need to apply the standard of rigour and state the scope of generalizability. This implies i) moving logically and consistently from one element in our theory to another as we use it and that all the elements fit coherently together; ii) we should be able to say what steps have to be taken to get from level to level of generalization.

Within management research for example it is known that individual decision making behaviour somehow interacts with, constitutes, yet is constrained by, what organizations do, while, in turn organizations interact with markets and environments. However, we await the theory that articulates these relationships according to the rules laid down. With regard to rigour and levels of generalization, then, theory in management research simply fails to comply with the standards it sets. It has been unable rigorously to show how individual conduct relates to organization and organization to society. To the extent that it has tried, the theory/methods it offers are grossly clumsy and primitive. And of course this is constantly acknowledged - "This is only intended to be heuristic", "We know it is a gross oversimplification" and so on. Since this is so, and if one is really trying to apply scientific principles, why not apply them where there is some chance that you realistically can?

This might mean bringing to light the way daily life is organized. How do we, how do others create and sustain orderly social life? The question of how social order is possible has been asked for a long time, but not until fairly recently has anyone attempted to look for an answer at the point at which social order is accomplished - people doing everyday things. Instead, all the everyday evidence of order being accomplished is taken for granted, with the researcher trying to look beyond it to provide general propositions of how it is all possible - what the real and underlying structures and dynamics are. In trying to assemble a theoretical framework which will enable us to link, in a coherent and rigorous way, individual perception to social structure, the question is not asked how do we actually do it though the remarkable *fact* is we do.

Recalling my first point, it is also to be noted that in the search for the reasons for or causes of social order the researchers have *used* all the humdrum accomplishments of everyday life (the natural attitude) to manage their enquiries and to interpret what they see. In terms of the metaphor used above they are importing into the game that they are committed to playing, rules and procedures that they have not declared and have no place in that game. They are breaking the very rules which *they* have established to guarantee their research as scientific. Their ambition exceeds their grasp.

The third point concerns the relationship between theory and the frameworks they provide, on the one hand, and the empirical research that accompanies them on the other. Practical, common sense reasoning consists in a continual process of acting with reference to some idea (theory) of what you are doing and finding confirmation of that idea or modifying it having done or tried to do what you intended. It is to be noted, however, that we don't do this all the time. Indeed, most of the time, we just do what comes next. In science the process is basically the same as practical, common sense reasoning. The difference is in the rigour with which we apply the standards set out above.

It will be seen that there are, in essence, two ways in which the relationship between theory and action can be treated: you can use the framework which the theory provides to study what goes on. The framework enables you to make visible that which is tacit and taken for granted and not normally a matter for enquiry. The framework should indeed provide a *framework* for the better study and understanding of the phenomena that are being addressed. Overwhelmingly, however, this is not what happens. Thus, the second way in which the relationship between theory and action is conducted is that studies -empirical observation - are carried out to validate, verify or test the theory. When the object of study has constant properties and has an existence independent of the researcher, as in the classic conditions of natural scientific enquiry, this procedure is viable (though some critics might dispute this as well). When, however, the object of study is constantly changing and this is the *essential* character of social *processes*, the attempt to validate the theory becomes futile.

The futility of trying to develop valid measures, criteria for guaranteeing objectivity, replicability of a given study, and so on, was clearly signalled in Cicourel's *Methods and Measurement in Sociology* (1964). While the book was taken as a manual on how to refine research techniques so as to bring them in line with the 'theory validation' school of thought, its purpose was in fact to show that its objectives could not be met.

These two tendencies in the way the relationship between theory and action may be treated each implies a distinct research agenda. In the first the primary concern is with the substance of management practice - what is going on here? The question is to be answered in terms of the concerns, priorities, values, beliefs, objectives and so on of the actors themselves. That is, it is not a matter of what I, the researcher, thinks is going on, but what the manager thinks is going on. In the second, the intention is to be able to say what is **really** going on. The actual substance of the phenomenon itself -management practice - disappears and gives way to a single, abstracted version of it, whose veracity or accuracy it becomes the researcher's concern to justify or defend.

### **Practical Reasons for Re-evaluating the Conventional Research Agenda**

Of the practical reasons I note three. The first is that the conventional attempt to provide theories about people and organization has not been very successful in delivering what it



promised. The Nystrom review that I referred to above is one of many in the Handbook of Organizational Design (Nystrom and Starbuck 1984). All the reviews, covering many aspects of organization theory and design, agree that theory to date is unsatisfactory, incomplete and primitive. In other words, there is essential agreement with the conclusion offered above (under scientific/methodological reasons) regarding the success, to date, of applying standards of scientific rigour in theory building. Nonetheless, they still pursue it.

In his conclusions, Nystrom (*ibid* p291) makes some significant comments which bear on this point. He contrasts the failure to develop theory with the fact that organization developers and designers have availed themselves of the research that has been done. He is rather dismissive of this fact, downgrading what he refers to as the 'smorgasbord approach' of organization developers who pick and choose from what is available. That is, this is a second best approach to doing whatever they do and that there should be a sounder basis for it, to be provided by integrated theory. My point, as stated earlier, is that this sound, scientific basis in the terms envisaged by conventional theorists is a mirage. Instead of pursuing it, it seems to me that there are a whole number of important empirical questions to be asked here of practitioners about what they find useful and why; how they evaluate research; what they make of the general theories on offer and so on. My finding on this is simply that it has provided a vocabulary which they put to any number of uses and of course it is inevitable they do so.

Thus, the second reason is that in the pursuit of general theories and model building, so abstract do the theories and models become, that they serve very little practical purpose and, indeed, perhaps do disservice. With Crook and Rooke, I have argued elsewhere (Crook *et al* 1996a, 1996b) that in the field of IT applications, the model builders make such arbitrary simplifications in order to construct their models that they are useless for practical purposes. The justification is always that we need to develop the models first and then we can tinker with them in order to make them fit user requirements. The extensive COMIC programme of research (1996) is demonstrating very effectively that the top-down or waterfall conception of software design is the wrong way up. It is much more effective to develop IT *with* the users rather than intendedly *for* them. I think that exactly the same relationship stands with building theories about organizational behaviour and what goes on in organizations.

Third, by recognising researchers as just other practitioners, they can get on with two valuable tasks. In addition to adopting the scientific attitude in explicating social processes, they can exercise rigour and systematic thought in what is called practical reasoning. This is to be distinguished from theorizing in the sense discussed, of looking for some true, intrinsic logic to be found in the social world from which may be derived guides to action.

### **The Cartesian Framework and Conversions**

I think it is illuminating to see the story that is told by Womack *et al* in *The Machine That Changed The World* (1990) and the elaborations by Koskela, Ballard, Howell and others on the thinking and experiences that it describes, in terms of an effort to break out of the Cartesian bind. The dominant logic that is evident in the development of the organization of manufacturing throughout the 19<sup>th</sup> and into the 20<sup>th</sup> Century is that of Descartes. It is not just that the technical processes of production take precedence over the human and social considerations, but that the entire process is conceived of as a machine which converts raw materials into products. What I think needs to be emphasised in the changes in the engineering of production, described by Koskela, where the concern with flows was

subordinated to the concern with conversions, is that processes which lean construction thinkers now refer to as flows were *thought of as conversions*.

This is a fine distinction but it is the crux of the entire argument being presented here and I hope to make its vital importance clear. I agree with Koskela, as an observation on history, that the conversion model has dominated organizational practice. However, those sequences of work which are recognisable *as conversions* by virtue of the organizational arrangements in which they take place, are not fully captured as phenomena by characterizing them as conversions. To assume that this is so is to fall into the trap of assuming that actual work processes can be adequately described in terms of a theoretical construct. The conversion model or the Cartesian paradigm is a way of thinking about organization which, as a matter of historical fact has dominated and still dominates practice, at least in western society. That is, the way of thinking is expressed in organizational strategies, policies, regulatory systems, arrangement of workflows etc. However, actual organizations do not simply embody what organization designers, policy makers and so on intend. The reality of organizations is much more than that. I take the importance of the concept flows to LC thinkers to signal the recognition both that the empirical consequences of the application of conversion (Cartesian) thinking are inefficient and that there is much more that goes on in organization construction that is not captured by the conversion model.

I take recognition of the importance of what are called flows as recognition of both these facts. However it seems to me that there is a danger that the concept flows becomes just another theoretical construct which aims to theorize activities so that the actual substance of those activities is obscured. This is why I have suggested that the concept points towards a matter for concern which can be practically addressed and managed. It must not attempt to translate them into a theoretical abstraction.

To explain what I mean I will consider the work of researchers at the Tavistock Institute , which was to become known as the Socio-technical Systems approach. I will suggest that though they made an effort made to break from mechanical thinking present in the Cartesian framework, they do not succeed. The metaphor of mechanical conversion remains dominant and prevents them from genuinely addressing the matter of flows which they began by trying to do.

In the classic study of technological changes in the British coal-mining industry (Trist *et al* 1963), which largely set out the scope and concerns of the approach, it was stressed that in designing technological processes they must be adapted to the needs, interests, values, culture and so on of the people who would be employed within them. That is, they recognised that by concentrating on material or physical conversions, crucial features of organizational process and of vital importance to management are obscured.

Thus, a key term was ‘organizational choice’, the title of Trist *et al*’s book which reported the mining study. The point that was being made was, of course, that technology did not determine culture and social structure. Any given technology was not to be identified by its ‘hardware’. A technology is, as it were, the hardware-in-use. As Scarbrough and Corbett (1992) were later to put it, it is impossible to separate the dancers from the dance. Thus, the early socio-technical theorists sought a conceptual synthesis between the technical and the human.

However, progressively, throughout the work of, for example Miller and Rice, the principle of choice gets lost and the supposedly intrinsic requirements of technology as hardware dominate thinking and conversion remains the key concept.

Miller and Rice were keenly aware of what LC researchers would call flows. I draw attention to the mechanical way in which they conceptualised them. In *Systems of Organization* (1967), in which they set out their theoretical framework, they made the distinction between operating activities (conversions) and maintenance activities and regulatory activities (flows). Operating activities are those which contribute directly to the achievement of the primary task by converting inputs into outputs - are value- adding. In an operating activity, human agents act on the throughput *causing* it to change in some required way. It is converted. This mechanical conception of causality is the foundation of the entire theory.

The other two sets of activities contribute to this end. Maintenance activities ensure that all necessary input resources are available. Regulatory activities are of two kinds; those that occur within a conversion and those designed to coordinate conversions by maintaining control at the boundaries of conversion or operating activities. To design effective organizations, they argued that, first, they must be designed with reference to their primary task 'the task that it must perform if it is to survive'. Second, the boundaries around conversions should be drawn as broadly as possible to facilitate flexibility within self regulating workgroups but the imperatives of Technology, Time and Territory had to be observed in deciding where boundaries were to be drawn.

They emphasise that conversions are carried out by people who have interests, values and so on which are formed through their relations within so-called sentient groupings both within and outside the organization. Sentience may impede or facilitate the achievement of the primary task and has to be managed.

Finally, and crucially, Miller and Rice insist that all activities, including maintenance and regulatory activities, should be seen as conversions. Thus, explaining the concept of conversion ('transforming a throughput') they talk of the work of, e.g. a ticket office, as that of converting people without tickets into people with tickets or of a passenger transport firm, converting people in location A into people in location B. While the physical nature of these activities make the application of the word conversion plausible, more worrying is that they also talk of having to manage people's perceptions in terms of conversion e.g. it is the task of cabin staff to convert people nervous of being in an aeroplane to people who are not nervous. And of course it is a crucial task of management to convert people who come into an organization into a usable resource.

This general theoretical framework is used for two purposes: to provide a theory which explains what goes on in organizations and which can therefore be used to manage them more effectively. This reflects the conventional assumption that theory should be about empirical reality and insofar as it is it aids our understanding of it and our ability to act effectively with respect to it.

The theory they develop attempts to address both the technical and the social. However, this very separation at the *theoretical* level prejudices the ways in which, the technical and the social constitute each other at the *empirical* level. In other words, *theorizing organization stops Miller and Rice from genuinely seeing the facts of organization*. Organizations are attributed with certain definitional characteristics; they are claimed to be empirically there

e.g. primary task, system boundaries. However they also wish to address the fact that there is choice in these matters which are influenced by human interests, values and so on. The question which they want to address but cannot is how, what they call the technical and social, influence each other. What they do, therefore, is to ask how do the two impinge on each other *as they concern outcomes for the organization*. But at the same time they are trying to accommodate their commitment to the view that organizations are social creations which admit choice and are not to be evaluated purely against technical criteria. They therefore cut through the problem by introducing the *construct* or *principle* of management and attribute to it the role of choice-maker. Management becomes the proxy for 'concern with the social'. This construct again obscures what actual managers actually do.

To enlarge on this evaluation of Miller and Rice, first, consider the conceptual status of the primary task. It may be taken as an inferred statement of scientific fact within a Cartesian framework - a machine must have a purpose and its parts may be evaluated with reference to that purpose. This indeed was the view that Miller and Rice took in their early work; that it was an immanent quality of the organization itself. Later, recognising the determinist implications, this was revised and defined as a matter of management policy. However, it is still offered as a matter of scientific fact, albeit the choice of Primary Task is given to the ghost in the machine (management) rather than to the machine itself (organization). Management is a theoretical invention. In other words, human choice is first recognised and then subsumed into the Cartesian framework. Now, this is not to say that there are not real people who are appointed as managers and who are given the task of deciding on policy, regulating departmental boundaries and so on, but these are all empirical matters. Who decides policy? how is it decided? how is it implemented? and so on. These matters can only be known from intimate knowledge of what goes on.

Second, how do the Time Technology Territory imperatives, these limits to organizational choice, originate? We are faced with the same choice: either they originate in the immanent characteristics of the organization or they are the result of management decisions. Surely, all of these common-sensically recognisable constraints exist only to the extent that they have significance to people and the ways they are or are not experienced as constraints? The sole significance given to them by Miller and Rice is how they impact the boundaries of conversion processes relative to other conversions as perceived by an abstract management in pursuit of an abstract primary task.

As to their second, practical or organization design/development (OD) purpose, how effective is their theory in generating a set of organizational design principles? This is an empirical question which it is difficult to answer. In their many studies where Miller and Rice were involved as consultants in an OD capacity, not surprisingly, they argue its usefulness. I will return to this issue of the role of theory in organizational design and other kinds of change initiative; for the moment I make the following observations.

I have tried to show that Miller and Rice's theory is essentially mechanistic, despite its claim to be socio-technical. In the lived reality of organization the technical and social constitute each other, which is not to say, of course, that people don't or shouldn't talk about 'social aspects' and 'technical aspects'. However, such talk takes place within the natural attitude where people are free to make sense to each other in any way that they find is appropriate to them. Within the scientific attitude, where Miller and Rice's concepts are put to work, the claim made for them is that they have clear empirical referents. The distinction between social and technical cannot work in this way. What is most troubling from an OD point of view is

that by putting them to work in this allegedly scientific way, claims are being made about the empirical nature of phenomena that they do not have. A false account of what we are dealing with is presented. From a design point of view this would seem to be unhelpful. This is especially true in the use of the concept of conversion. Any process can be considered as a conversion but this may have the effect of obscuring all the other things it is as well

What I am drawing attention to is the way the multiplicity of human conduct is being expressed as a physical conversion. The attraction, I suggest, is that it thereby provides a practical guide for action but for many purposes it is a bad guide for action insofar as it misconstrues the nature of what needs to be managed - i.e. what people do, their reasons for doing it and so on.

It is also attractive because, implying that the concern is with essentially physical processes, they can be seen to have tangible and measurable outcomes. While it is useful to measure **things** that can be measured in situations where it is known what the measures mean, it is very hazardous to try to measure 'things' which have no physical referents, like attitude, motivation, satisfaction and sentience. And, again, this is not to suggest that such terms cannot convey meaning in adequately common sense ways. The problem is in assuming that they have a finite referent.

Thus, the characteristic of a conversion is that the value-added patently occurred; it is the difference between the inputs and outputs. By definition, what happened can be inferred from the objective difference. Whatever else happened is deemed irrelevant.

Now, of course, there are a multitude of other things that go on both within conversions and within flows. As a practical issue, to consider an activity as a conversion with a finite outcome may be appropriate. Some aspects of flows *may* usefully be treated in this way - the delivery of materials from point A to B for example - but what I take to be the crucial nature of a flow, especially in the context of construction, is that it is a process in which *meaning is constituted*. It *articulates* the connection between conversions whose character is that their meaning is more clearly established. This point will be elaborated in the next section.

In summary, in addressing the meaning that people attach to work, relationships and so on, which Miller and Rice begin by trying to do, they end by subordinating it all to the instrumental perspective of an abstract 'management' in charge of the organization as machine. The meanings that people (including managers) actually attach to things is thus rendered unproblematic. In the case of nervous passengers, for example, they have to be managed so that they accede to the definition of the situation as provided by the managers (the cabin staff). The passengers' definition of the situation constitutes a problem that has to be dealt with.

My argument is that talk about and reflection on the concerns of the construction industry are dominated by Cartesian or mechanical conceptions of causality. It does not provide a conceptual apparatus which deals adequately with questions of human meaning. Only the consequences of human action for the machine (organization) are admitted. The meaning of that action for the actor is simply reconstituted in terms of those consequences. LC research must develop a conceptual apparatus which avoids this reductionism and beware the tendency to subordinate or construe 'people' issues in terms not appropriate to them. Thus, in doing so it must beware falling into the Cartesian trap of misconstruing the nature of the empirical world it addresses i.e. that which is constituted by the meanings of people. In pursuing its

*developmental* purposes and trying to encourage *a new way of seeing*, it must recognise that this is a social process and that the use of any new conceptual apparatus can only evolve within the context of on-going practice and the way it is conceptualized.

### **Empirical Science and Practical Reasoning**

In what follows I consider the distinction that is made in LC research between conversions and flows. I will develop the idea that the concept 'flows' points to a way of escaping the Cartesian straightjacket. To do so I wish to try to keep in focus the aim of the scientific enterprise which is to attend to what is empirically there in the world of construction organization, keeping it separate from the concern with how this relates to the practical business of improving organization and processes.

This is to restate the alternative view of the purpose of human science, described at the outset of this paper, which is to discover how social life is constituted by the people who live it. The practical purposes and conduct of managers is just one strand in this process and so has no special status *within the scientific attitude*. How far the pursuit of this scientific, objective knowledge is of use to managers is for the moment an open question. We must escape the assumption, present in conventional theorizing, as shown with reference to Miller and Rice, that one leads directly to the other. We must learn to understand the phenomena before us without prejudice. The theory is to be found in what is there; explicating *that* theory is one of our tasks, as with the code that Wieder explicates for us. This is my proposed alternative to saying: let's *assume* that that is what is there and if the assumption/theory is seen to enable *us* to make sense in terms of *our* assumptions and theory then it therefore constitutes both a valid scientific finding *and* provides basis for practical action.

As already noted several times, finding out what is going on the terms in which it is going on is the scientific part. To sharpen our own practical reasoning to bring about practical ends is a *different enterprise*.

I have said (as an empirical observation) that to refer to an activity as a conversion gives it a special status: as an activity in which there is a difference between inputs and outputs and that this difference contributes to the purposes of an organization as defined by its management. It is conceptually constituted; it is a construct. Other things happen. These may themselves be constituted as conversions. But this is to subordinate them or reconstitute them within a single 'management' perspective which is itself a construct.

While this is perfectly legitimate as an instance of practical reasoning i.e. the manager construes the significance from his/her point of view, there are other versions equally valid and equally a constituent of the circumstance under consideration which, from an empirical point of view, are variously consequential for what goes on and must not be lost from view. These are the matters that a truly scientific account are concerned with; these are foundational matters.

The enterprise of constructing or engineering the reality, creating conceptual constructs to be used (constituting an activity as a conversion, for example), is a legitimate management action. How well managers' conceptualizations serve their purposes are practical matters for them to judge. We must be clear about how our purposes as researchers relate to theirs as managers and, as I have said, I see two kinds of purpose for researchers.

- i) To document how the world is empirically constituted by people and that includes the constructions used by managers, how they try to get people to 'buy into' their conception and so on.
- ii) To promote, themselves, distinctive ways of thinking about construction. This is what I have called practical reasoning.

I submit that this second purpose is more likely to be achieved if we don't confuse it with the former.

### **Flows as the Exchange of Meaning**

What, then, do we mean by flows? Getting the right plant and material in appropriate quantities etc from the point of origin to the point of production. This can be conceived of as a conversion and indeed Miller and Rice encourage us to do so. Plans, specs, etc., considered as conveying a purely instrumental set of information, can be similarly conceived. However, furnishing a workforce who will be disposed to work in a way consistent with the objectives of their employers, achieving the compliance of suppliers etc etc **can** be seen as yet another kind of conversion and, again, Miller and Rice do - they talk of socialization - recent commentators would talk of getting people 'to buy into the culture'. Thus, if we think of the management of flows as the management of a different kind of, but still essentially a conversion process, the promise of the concept flows is lost. As I have said, I think the concept is reaching out to matters which lie beyond the scope of theory building and modelling as conventionally conceived.

To see what these matters might be, perhaps it is useful to ask why do flows appear so important and particularly so in construction? I think the answer lies in the direction of variability and uncertainty, and the failure of conventional theorizing adequately to address them. The growing attraction of the concept culture, I think is evidence of this. That is, in order to address the complexities of articulating many different organizations, of creating a meaningful whole from many disparate parts in the pursuit of realizing a complex project, it is increasingly recognised that we need to know how people variously understand and make sense of things. We need to concern ourselves with meaning.

Ballard's point regarding the need in construction to accept the changing nature of client requirements as a fact and to learn how to manage the construction process's response to it, is a good starting point. I see a parallel between:

- i) (what I take to be) his argument that client requirements are not finite requirements which are an input to one large conversion process (i.e. the project) and
- ii) Weider's point that human science is about discovering what is going on in the terms of those engaged in whatever it is they are engaged in.

What goes wrong currently in construction is that, according to the conventional model, a finite input is required and, of course, this is not available. The alternative is for those in business to meet client needs to find out and to continue to find out how the world appears to their clients who, in turn, should be similarly concerned to find out about their 'partners'. The emphasis is thus on finding out how it looks to the other fellow. There is no definitive meaning to be had; only that which is agreed between the parties involved in that setting.

There are ways of facilitating this and that is to work out a lexicon, specialized vocabularies, conventions for mapping, protocols etc. However, these are languages and languages are *used*. The point here is that a community can self-consciously work out a code for itself which expresses its members' specific concerns. But they must not credit this process with being anything more than what, say, street kids do, when they work out an argot for themselves. This is the point that Morgan (1986) makes when he says that all organization 'theory' has done is to provide a language for managers. This language must not be seen to provide a scientific description of organizational phenomena in the same way that, say, physics provides a terminology which has finite referents and specifies the relationships that exist between them. Specialized languages, like all languages, are tools which are used to create and maintain social order. These tools and their use are *part* of social order. If sight is lost of this fact then we are back with the conventional view of theory as capable of standing outside the phenomena it purportedly describes with all the attendant problems and confusions that I have discussed. We can get better at practical reasoning and develop tools and techniques with which to share and communicate it, but the test of its efficacy is always on the ground, in use. Abstract theory building leads inevitably into the Cartesian cul de sac. To seek intrinsic patterns in the expectation that they are there to be found like discovering how atomic structures intrinsically are is a simple category mistake. The undoubted regularities and patterns that there are in the social world **contain** or **comprise** the expectation of and the *communication* of regularities and pattern. The former cannot stand apart from the latter. As Ryle put it, the man who asked to be directed to Oxford University and, being pointed to Keble College, replied that, no, he wanted to see Oxford University, was making a similar category mistake.

In common with Koskela, Ballard, Howell and others, I have said that the conversion model has dominated organizational practice. However, I have stressed that in using the concept conversion as part of their own theoretical lexicon they must be careful not to 'reify' it. That is, they must not fall into the trap of assuming, in characterizing work processes as conversions, that actual processes can be adequately described in terms of a theoretical construct. I take flows as being in danger of becoming just another theoretical construct which aims to theorize activities so that the actual substance of those activities is obscured. This is why I have suggested that the concept *points* towards matter of practical organizational concern; it does not try to translate them into a theoretical construct.

### **Indexicality and the Management of Meaning**

As I see it, the important contribution the LC thinkers make is to focus on the future and how it is to be managed rather than to theorize the past. As a process of practical reasoning it addresses actual situations with the view to managing them more effectively - what *will* happen. To theorize what *has* happened inevitably invites thinking in terms of conversions; in terms of net outcomes and therefore risks misconstruing all the things that actually happened.

Conversion is a readily understood concept because, I think, it evokes a physical metaphor; changing some *thing* into *something* else. For the most part it is thought of in terms of a simple cause-effect model of how what was done *was* done. But of course an account of this process is always a *selective* reconstruction adapted to the purposes for which and the circumstances in which it is called. This reconstruction may be carried out either according to the rationalist/Cartesian model or commonsensically, both of which draw on a wealth of tacit knowledge and shared assumptions.



The concern with flows, in the sense of managing meaning, is to make these assumptions and knowledge *explicit* because much of the time, as evidenced in 'poor communication', misunderstandings, etc, assumptions and knowledge are only partially shared -they admit a great deal of slack. They are sufficient for all practical purposes, but, of course there are times when the practical purposes require greater exactitude. Thus, for example, much of the time it is adequate to say 'I'll see you at about 2 o'clock' or 'we'll need about a dozen brackets'. On other occasions we need to say 'we will meet at 1400 hrs let's synchronize watches', or 'we need 13 brackets of XYZ specifications'. It is the concern to articulate the taken for granted or to make explicit what might otherwise, at best, not need to be made explicit, which is central to the concern with flows -how are acts to be interpreted or how are conversions to be situated.

I am suggesting that there is a parallel in what happens on an everyday basis and what happens in Cartesian theorizing. Both are indexical. In the former we supply all the necessary information that we have about the circumstances to make sense of what someone says or does. In the latter, we supply all the necessary taken-for-granted to interpret what the theory means and are required to accept the assumptions on which the theory is built. What the theory actually states is, as it were, only the tip of the iceberg and that tip concerns only outcomes.

For example, consider the way motivation is standardly theorized. By and large, we assume that the people we have to do with make the same assumptions about, say, causality as we do; that they will make the same inferences about information as we do; that they will attach the same meaning to events as we do. When anybody does something, therefore, he/she is not normally called upon to give an account of what they are doing. i.e. the meaning of what they do is inferred and we expect that to happen. As socialized beings, by and large, we successively adjust our conduct to what is expected - events pass unremarked upon - they are normal. If there is a risk of ambiguity or misunderstanding we can explain or ask for an explanation and there is a standard vocabulary or repertoire of meaningful motives or 'causes' of our conduct. From time to time, an event or action **is** remarked upon as not normal, exceptional or indeed, remarkable, as warranting remark. Only then is the question asked: what does it mean? (why did he/she do that?) Thus, a manager notes that an individual's productivity is lower or higher than is expected. In such circumstances the question is asked: Why? All sorts of theory regarding human motivation may then be invoked, but essentially an explanation is invoked that satisfices. The adequacy of an explanation is a function of the situation. It makes adequate sense to say that he is working harder because he is on a bonus and that the bonus is having the intended causal effect, or whatever.

Do the theories of motivation (in this case) give us a better more accurate understanding? I don't think so. And I don't think, either, that this is a matter of there happening not to be any good theories of motivation. *There are none to be had which can be empirically validated* (see Sharrock and Watson 1984). This is because all kinds of common sense assumptions (characteristic of the natural attitude) are put to work to enable the statement of the various theories of motivation to make sense. It is encoded. It is indexical. To understand it, you have to reverse the process to decode it and remedy indexicality. It is not surprising that managers (in my experience) regard 'theories' of motivation with contempt. If they mean anything at all they mean what we knew anyway. Again, (to try make this clear), managers certainly do talk of motivating. They may also talk of hygiene factors and motivators, the 'theoretical' distinction given to managers by Herzberg. But insofar as managers find it a useful distinction, it owes nothing to scientific theory.

Juran also uses the iceberg metaphor about the causes of quality defects and I think it instructive to consider his use relative to the 'causes' of action. The point he makes is that there is a multitude of causes that result in the immediate cause of what happened. Some of them cannot be assigned - the common causes. I want to draw attention to the fact that there is both a multitude of causes of, and a multitude of reasons for a human act. That is, the attribution of causes and the attribution of reasons are crucially different. They both respond to the question: why did it happen? In the first case the answer is in terms of conversion in the second it is in terms of flow. In the former the 'act-or' is conceived of as a physical entity to which causal explanations can be applied. Within the Cartesian framework of cause and effect this is what is done. The actor's acts are 'stimulated' by a number of psychological and social needs. What is seen to be important is the outcome (effect or output), as determined by 'management' for the 'organization', as discussed in my analysis of Miller and Rice. This is the so-called 'behaviorist' or 'black box' treatment of human conduct and is the second manifestation of the Cartesian legacy, referred to at the outset of this paper.

The implications of the iceberg metaphor are different in the two cases. In the first it is that we cannot identify and disaggregate causes, we must simply register their existence and cumulative impact on outcomes. As to the second, we are faced with all the taken-for-granted ways in which we make sense of events and communicate that sense to others. However, in both senses, there is no way to establish empirically what actually happened; what the causes or reasons definitively were. There is no way, for example, in which you can show that it was a higher or lower order of need at work that 'caused' a given behaviour. In actual situations, you can find out why someone did something if the atmosphere is such as to encourage him or her to tell you. However, for most of the time people do not have any particular motive for doing what they do. But, I submit that asking is the only way to find out. Theory and any number of reconstructions about why someone did something (the latter of which is invariably simply adjusted to the assumed reason for the question being asked), is not going to help establish the real reason whatever that is supposed to be

### **Managing Flows and the Application of Practical Reasoning**

The practical techniques developed for managing flows within LC seem to deal with this in a practical, future-oriented way. Thus, the concern with flows can be seen as an attempt to challenge these inadequate reconstructions. It recognises that reconstructions are embedded in a mass of taken-for-granted which make it difficult to find out for future purposes why what happened happened quite in the way it did. That is, inferences we can make about the past are suspect, subject to over-generalization, 'theorization', myth-making and so on. So, these practical techniques focus on the future not the past. They ask a series of purely pragmatic questions in a specific setting:

- i) where are we now? asked with reference to customer needs/requirements.
- ii) What have we got? Materials, plant, labour.
- iii) What can we do? Set objectives, do the job;
- iv) What did we do? Monitor, record methods and outcome
- v) what do we need to improve on what we did last time? Furnish it; do the job again; monitor and so on.

There arise from this two primary issues:

- i) the need to address the specific reality of a given project,

- ii) the need for a language/conceptual framework/model that adequately describes that reality sufficient for learning to occur in those circumstances.

There are two secondary issues

- i) How far can the model/conceptualization of (ii) above be generalized to other circumstances?
- ii) How far is it necessary/possible to claim that such a generalization has a yet more general validity; that in some way it seeks to describe some immanent or necessary dynamic?

The difficulty of persuading people, who hold a conventional (Cartesian) view of the relationship between theory and social reality, of the importance of the concept flow is, in part, that their mindset prevents them from seeing what it refers to. However, there is a paradox. On the one hand the bulk of the iceberg consists of people's sense-making activity; they constitute it. On the other, they use the conceptual schemes implied by 'the tip' with which consciously to explain it and therefore distort it. This is to say that we take for granted the vast bulk of capabilities that we use in making sense to each other and only address a few of them when we explicitly consider the matter of communication. It is the same, for example, with legal contracts. The explicit terms of the contract are embedded in and call upon a wealth of shared assumptions in order to interpret what they mean.

What, then, do flows consist of? Ballard writes:

'The fundamental fault of the conversion model is disregard of information and materials through space and time. That disregard conceals waste and the opportunity for waste reduction, but even more important, it conceals precisely what needs to be managed.'

To capture the essence of what is implied here, a parallel may be drawn with the way we habitually make sense of human conduct. For 'conversion' read an act -something is done; it has clearly observable consequences. For 'flow' read interpretation; the attribution of meaning or significance to what is done and choice about what reciprocal conduct is thereby implied. The parallel holds also for the transport of materials, for what is important about this process is to ensure that the use to which, say, a pallet of bricks or a ladder are to be put, is understood in the way that it is required to be understood. Thus, if the bricks arrive late or in the wrong place they are a signal for some compensatory action. If only one ladder is available and two are needed the meaning is beg, borrow or steal another ladder! To manage flows is to manage this attribution of meaning.

The way managing is formally talked about, in the textbooks and so on (there are number of exceptions e.g. Morgan) this quality is lost. Managing is about getting things done; causing things to change; converting. How it is done is of course a key practical concern. Theory has therefore been offered to explain what causes what. I have been suggesting that the nature of these explanations has been mechanical despite their claim not to be (e.g. Miller and Rice). I have suggested that the reason for this is the inevitable result of Cartesian dualism; the expectation of being able to find thing-like causes out there in the world. While the model may be appropriate to achieving change to the physical world, since management is precisely getting things done through people, it is not appropriate, despite trying to cast human conduct *only* in terms of its material consequences for the organization.

## Explanation and Understanding

I have argued that the emphasis on conversion is the result of the practical concern with instrumental action -something happens; visibly, palpably; A to B. Answers to the question: what caused it to happen? may be (within the present argument) of two kinds:

- i) a 'scientific', or, better, 'scientistic' account or explanation treats the social as though it were physical and external to the process of enquiry into it (the Cartesian paradigm). The adequacy of the explanation is judged with reference to the paradigm.
- ii) the explanation satisfies, i.e. it is an explanation which is adequate for the situation and for all practical purposes.

The question is what do we look for in an explanation? Does the scientistic explanation actually help to achieve our purposes? The kind of explanation offered by Miller and Rice purports to be able to do so. It may do. But as I have insisted throughout this is an empirical question. I have suggested that purportedly scientific explanations be regarded as any other kind of explanation. The key questions are: do they help? do they enable us to do anything in ways that better achieve our purposes? Do they enable us to understand the problem in hand sufficient for us to do what we intend? Do they enable us to say - now I understand how it works and the proof that I understand is that I can do it? I suggest that it is not explanation per se that we seek but understanding. We share understanding by showing.

For example, I refer to the restaurant briefly described in the Introduction. We can provide an account of this in terms of a formal explanatory model. The model may help us to understand how the restaurant operates, certainly, knowing how to apply the model enables us to provide an account of the restaurant in *the model's* terms to others who share the use of these terms. It provides a plausible explanation. But has it described what is going on? It is certainly a version of what is going on, but from the outside looking in. What we have not learnt is why what happens happens in quite that way; how it takes the particular form it does. What we will have done is make simplifying assumptions about what it all means to the people involved, but this is precisely what we need to know about and it is to this dynamic that the concept flows, as I see it, directs our attention.

What I am emphasizing is that there is a vast difference in learning to use a theoretical model to interpret or reconstruct the reality in its terms and using the model to help us see what is going on. The model is likely not to help us to see what is going on because it reconstructs the reality in its terms and precludes attention to the interpretive procedures that are in use.

Now there is an altogether different issue. It may be that we wish to propose the use of other interpretive procedures than those that are actually in use and say: "let's use my modelling procedures, explanatory schemes and so on". It seems to me perfectly reasonable to say that we can see a better way of doing things and in order to do so you need to look at things in a different way. I take it that LC 'theory' is being offered as a way of escaping the reductionist conversion/Cartesian model. It is saying that people are being blinded by looking at things in terms of mechanical conversions rather than looking at all the other things that go which make these conversions possible, that is, the flows. In which case, you can use the models and show their relevance, as you see it, to the problems that the people you are trying to help have. I think that this is precisely what Ballard and Howell, for example, are achieving

Thus, for example, consider their use of the concept buffers. I take them to be saying that time must be allowed so that it becomes possible to create a meaningful gestalt which can be achieved for any given package of work. In the conversion model this spells inefficiency which it would be if it were possible to specify precise outcomes in advance. But, of course, it is not possible, in fact, and therefore not inefficient but entirely appropriate in these circumstances. The trouble is that 'buffer' as with 'slack' is a physical metaphor. But consider what buffers are all about. I see them as being crucially about the need for time to negotiate a normative agreement, to communicate people's respective perceptions about what is possible and necessary. This is done with reference to certain communicative devices, measures of specific and measurable outcomes and so on. CAN is an agreement. It implies: meaning is understood and a meaningful commitment can be made to measurable outcomes -WILL. The conversion model with its black-box conception of the people involved in this process has no way of registering this crucially **normative** transaction.

I would call the tools they have developed for managing flows examples of practical reasoning. They are not theory as conventionally understood and have no need to be. With reference to what I proposed as the conventional constructional management research agenda (see above), they do not purport to be theory.

Nonetheless, they have important theoretical implications. The tools have been developed as a result of a not trying to *explain* the way the construction industry works but to *understand* it. To *convey* or *explain* (i.e. to make sense to other people) this understanding it can only be done by remedying indexicality, that is, by showing what they mean. To use theoretical constructs is the exact reverse of this process.

To explain what *I* mean. There are theories - notably in physics which seem to have captured something of the material world. They enable us effectively to act on it. It would be comforting, perhaps, if similar theories were available for the social world. It might enable us to become more effective managers, for example. We look for the certainty that any proven theory provides.

However, as I concluded in my evaluation of Miller and Rice, what is offered as theory may be considered as examples of practical reasoning and useful as such. In which case it for the users to judge whether or not they are useful. As stressed throughout this paper, there is no way in which their validity or truth can be established through conventional research procedures.

As noted above, the importance of theory (duly validated) is that it offers certainty. Uncertainty inhibits action -we don't know what to do. Theory explains the connection between things and tells us what information we need to know about what kinds of things they are, what properties they have and in what degree. Theory plus information reduces uncertainty; it narrows the choice of appropriate action.

However, while this may be true of the material world (though some might doubt it) it is not true of the social world. In this uncertainty is decreased to the extent that people share the same 'theories' (in the sense of methods of practical reasoning) and are known to share them.

The entire problem as conventionally defined by theorists of organization of how to remedy the consequences of specialization and departmentalization, can be seen as the result of the dominance of the conversion model, as the LC commentators note. However, the attempts to theorize this problem by Thompson (1967), Lawrence and Lorsch (1967), Galbraith (1973)

and others, must be seen as exercises in practical reasoning, no more and no less. Their 'theories' cannot be validated as being a true picture of the world. Their general applicability cannot be established by comparative research. Their only test is in whether people find them useful in communicating to each other across specialisms and organizational boundaries their sense of what is. The models which, for example, contrast different forms of coordination (sequential, parallel, reciprocal) are a way of communicating a sense of various organizational choices. There is no valid empirical test of whether the phenomena which the models are supposed to represent are to be found in the world. They do not describe anything in reality. They are precisely constructs which may or may not foster understanding and learning about better ways of organizing. They do not *explain* anything, though this is what they are claimed to do.

Galbraith, for example, makes what I think is a useful distinction between an organizational strategy where rules, procedures and programmes are formulated in advance i.e. plans are prepared which state required conduct (characteristic of sequential and parallel forms) as opposed to the reciprocal form. In this third strategy people are trained/socialized into attributing the same or at least predictable meanings to events; they will interpret information in the same or predictable ways; they will apply the same or predictable theories of causality and so on. In other words, commonsensically recognisable alternatives are proposed: to reduce slack and minimize waste, either routinize or increase the probability that meaning and intentions can be communicated.

However, what must be recognised here is that the contrast is not between different *forms* of organization, which is how it is presented, nor even between the *spirit/culture* in which relationships are conducted. We face once again the impasse noted in the reference to Anderson and Sharrock cited on above. Galbraith is, in fact, offering an explanation These forms are organizational responses (i.e. empirically to be found) to different levels of environmental uncertainty. There is an intrinsic logic at work. Unless organization designers obey this logic the organization will fail.

To avoid this kind of theorizing perhaps it is useful to think about flows in terms of language? Words, phrases, sentences taken out of a particular context still have a kind of generalized meaning. We communicate particular meanings by articulating them to achieve particular communicative purposes. We manage the flow of meaning. Similarly, we take discrete elements, tools, materials, technical capability etc and assemble them into 'meaningful' elements of work -we manage workflow. The concern with flows thus, in other words, is about providing a gloss on a set of discrete generalized elements, to ask: how do they fit together on this occasion?

Extending the analogy. In a controlled cookie-cutter factory environment the text is fully prepared, it has been edited; ambiguities removed; meaning checked against event. In the prototype situation of construction we are permanently in the condition of discovering meaning. The central challenge for managing flows is managing that meaning. So, managing in-flow variation, for example, is about trying to ensure that adequate material resources are available for the job to be done and also that people are able to adjust their understanding of what needs to be done. As Morgan very usefully suggests, think of organization theorizing as the use of different kinds of metaphor, just as much available to people in organizations as theorists of them. It is a way of keeping alive the fact that we create organizations in the way we think about them.

A disastrous effect of the power of the Cartesian metaphor (the machine), which, I have argued throughout this paper, subtly dominates most thinking about organization, even when it seems to have been avoided, is that in construction we have inherited the view that a plan of a structure/facility can exhaustively describe what the reality will be like. Though it is conceded that modifications may have to be made in the event of manufacture or construction, this is seen as a deficiency of the particular plan, not that the relationship between plan and reality is basically misconceived. That is, the essentially indexical nature of a plan is not recognised; it must always be interpreted. Again, the inference is that managing the process of getting from client requirement through plan/design to constructed facility is primarily about managing the process of interpretation.

In contrast to this view, construction is infected by the dualist myth that the technical or substantive content can be planned. We have been led to think about construction in terms of a separation between the technical system and the human system, when the fact is that the distinction is senseless - as noted earlier, it is simply not possible to separate the dancers from the dance. I take the LC practitioners to be saying that the planning process is situated, from which I infer that planning cannot be done in the abstract, that is, without reference to the unique circumstances of the project in hand. You can choreograph, but the realization of the dance occurs through the dancers.

Within construction practice there is a paradox. It has become a truism that what characterises construction is uncertainty, yet planning is conducted as though certainty were possible. Uncertainty is inevitable in the same way that you can't know what somebody else is thinking. However, you can check it out and establish objective, real and shared criteria. Rather than assuming certainty and then bewailing its absence you can, to an extent, create it. A major factor that stands in the way of this, as I see it, is the unwillingness or inability induced by cause and effect reasoning to see this process for what it is. That is, one in which it is necessary to attend to meaning; to attend to how co-workers see things and how agreements can be made. Project Start Up (PSU) amongst other recent developments in project management, addresses this issue. Because of the obvious deficiencies of conventional planning, the intention is to establish how people will deal, in the event, with problems that they can't solve prospectively.

## **Summary and Conclusions**

I have argued that organization practice and ways of thinking about and researching organization are dominated by a powerful philosophical influence that largely derives from Descartes. Its impact on the human sciences has been to foster a view of the social world as amenable to objective analysis and that it therefore becomes possible to establish causal patterns which can provide a basis for more effective action. I have argued that this is a misconception of the nature of the relationship between human consciousness and action on the one hand and social reality on the other. This misconception is apparent in the efforts to develop empirically-based theory on the Cartesian model, where versions of social reality, composed of abstract theoretical constructs, are incapable of being empirically tested .

It is therefore proposed that in developing LC thinking and practice, first, genuinely empirical work can be done on the regularities and patterns that exist in construction by asking how people achieve it; how they make sense to each other. Second, there is every reason why, as a practical project, proponents of LC should demonstrate the benefits of the use of thinking

tools and techniques. These tools and techniques are likened to language inasmuch as they provide a vehicle for communicating, evaluating and negotiating purposes and intentions.

An obstacle to the use of these tools and techniques is the power of the Cartesian model, evident in both organizational practice and ways of thinking about it. This model is dominated by the metaphor of physical conversion. The concept of flows draws attention to those processes which the image of physical conversion necessarily distorts. In their essence these processes can find no place in the conversion model because they are about the significance or meaning of things and events. It is concluded, therefore, that the two purposes stated above have been signalled by the importance that LC has attached to flows. Those purposes are: i) empirically to establish how meaning is achieved in the social interaction that constitutes the construction process, and, ii) in the light of this understanding, to develop the tools and techniques of situated improvement. However, if this agenda is accepted, then, in understanding flows and developing ways in which they can be improved, the concept of flows, hitherto defined in contradistinction to conversions, needs to be abandoned, for it too risks being constituted as just another theoretical construct.

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