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Goran Runeson

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NOTE

The role of theory in construction management research: comment

GÖRAN RUNESON

School of Building, University of New South Wales, Sydney 2052, NSW, Australia

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Seymour *et al.* claim that positivist research methods are unsuitable for research into construction management. On the contrary, mainstream methodology has been modified to deal with the special demands of such research and conventional research methods have been instrumental in substantial advances in science. Seymour *et al.*'s argument, ostensibly about research methods, is essentially anti-scientific, and, although it has been around for a long time, there are no positive achievements to suggest that we would benefit from adopting it. Contrary to Seymour *et al.*'s claims, positivist research methods are our best insurance against bad research.

Keywords: Research methods, methodology

Seymour *et al.*'s note on research into construction management (1997) argues in favour of 'interpretative' (qualitative?) and against 'rationalistic' (quantitative?) research methods in construction management research, against a positivist epistemology and for a nominalist ontology. It is an argument that would be considerably easier to accept if there was some indication of the purpose of the research and the nature of the theories that would result from adopting the authors' views.

However, before dealing with the main arguments of the paper, it is worthwhile examining the idea, explicit and implicit, in the note: that construction management is a discipline, based on a theory or a science (terms used interchangeably in various places on p.118, always in the singular form). Construction management is a set of functions where different techniques are employed. Some of the functions are based on, or can be explained by, various scientific theories, some of the techniques have a theoretical background.

In respect of performing several different functions, a construction manager is not very different from a house wife/husband or a theatre director. However, it is difficult to accept the suggestion that there is a single separate theory that covers the construction manager when he/she acts as a buyer of materials, as a motivator of personnel or as a producer of work schedules, and that, somehow, that theory is totally different to the theory that applies when he/she is performing exactly the same functions, but for a different purpose.

While this position is not inconsistent with the epistemological and ontological position advocated by the authors, it leads to a very complicated perception of the world, and without any evidence to the contrary, it is much easier to believe that economic activities are covered by economic theory, motivation by psychology and planning, if done well, by probability theory, no matter who performs them or why. The consequence of adopting the latter attitude is, of course, that most of the methodological battles that Seymour *et al.* want

us to join have already been fought in the respective sciences and that this is, at best, a rearguard action against poor research that has been performed in the name of 'rationalistic' research methods.

There are not only many different theories but also many kinds of research, varying in scientific content and ranging from finding a telephone number to establishing the Great Unifying Theory. A lot of research into construction management is directed towards finding better work practices or improving decision making. Such research includes such elements as finding the most suitable computer program for estimating or implementing structural change in an organization or constructing check lists for various purposes.

This is what Seymour *et al.* presumably refer to when they suggest that "the discipline intends (sic) to produce advice for practitioners." (p.119). However, producing normative advice is not science, at least not in the positivist tradition (e.g. Lipsey, 1963) and is therefore not subject to judgement by scientific methodology. It is judged on results, and results alone. Research methods or philosophies do not really matter. It often helps if normative models have a good scientific basis; it has been advocated that scientists should provide an input (Putnam, 1974), but it is certainly not necessary to have good science even when scientific issues are involved and there are many examples to illustrate that point. Navigators continued to use Hipparchus' *Catalogue* and Ptolemy's *Tables* long after the publication of Copernicus' *De Revolutionibus* because they were more accurate than the more scientifically correct calculations based on a heliocentric solar system (Koestler, 1959); builders setting out foundations for small buildings have no real reason to take into consideration that the earth is not flat; one can construct a manual for how to operate a television without using any theories of electronics, and so on.

There has been very little corresponding progress in 'the science of construction management' and one of the probable reasons is the widespread insistence that it is one separate, or a set of unique sciences. Unfortunately, this has in practice resulted in numerous works where the researchers appear to be unaware of existing theories and there is no science at all, no theory, implicit or explicit, so that any selection of variables that shows a statistical correlation or can be fitted into a regression model seems sufficient for a paper. When Seymour *et al.* state that Betts and Lansley (1993) and Betts and Wood-Harper (1994) make a distinction 'between research that does not contribute to theory and research that does' and take that to mean a rejection of one research method in favour of another, they are misinterpreting the very appropriate distinction that is made in these papers between meaningful and not so meaningful scientific research.

Provided that the purpose of science is to create useful theories, and Seymour *et al.* seem to agree with that proposition, meaningful science is either testing theories, testing the applicability of theories, constructing theoretical models or building theories (e.g. Kuhn, 1970). This has very little to do with the difference between quantitative and qualitative scientific methods.

Seymour *et al.* suggest that there are examples of poor research, and they are certainly right in that. A typical example of not very meaningful research, and I have been asked to review or referee papers on this topic more than once, is the popular topic of establishing why things go wrong on construction projects by asking construction managers what they think the reasons are. It has possibly some, although limited, merit as normative research, but as scientific research it suffers from two serious draw-backs: there is no theory either being tested or developed and the results are so subjective and so biased in favour of construction managers and against designers, subcontractors and trade unions as to be totally meaningless. This has nothing to do with the question of whether information was derived from survey questionnaires and subjected to all kinds of descriptive statistics disguised as analyses, or from in-depth interpretative interviews presented some other way. Either way, it is bad research, and there is a lot of it around.

Papers based on such research projects often have a heavy emphasis on statistics indicating that the authors know how to use their statistical computer programmes, but not why or when. My absolute favourite example of non-theoretical science is the research project that triumphantly establishes, from impressive-looking statistics, that existing houses are now so expensive that no-one can afford to buy them, when the slightest attempt towards analytical thinking would make it obvious that houses are that expensive exactly because so many are prepared to pay so much for them. Sample selections, objectivity, theories and analyses are important.

However, rejecting the 'rationalistic paradigm', as Seymour *et al.* do, because it concentrates on establishing causality (p.118), is rejecting science as something that can be used for predictions or can be tested. Science is about establishing causality, about formulating conditional statements that can be tested. This is how we differentiate between science and metaphysics.

That is not to imply that there is no role for interpretative research. For some kinds of normative research, it is as good a method as any other, provided it is not taken to be science. For some kinds of scientific research, it is sometimes the only method and in many other cases it can add valuable insight. It is primarily in building new theories and modifying

existing theories that interpretative research can help the derivation of alternative hypotheses that can then be tested through conventional research methods. Qualitative research, in the sense of using unquantified variables, also has an important function in many situations, especially where quantitative information is unreliable or simply not available, as in many types of historical research, and there are, as we know, brilliant examples of this in the social sciences.

It is easy to understand the fascination with interpretative research. The 'rationalistic research paradigm' creates 'black box' theories, where the process by which stimuli are converted to responses is hidden (Riggs, 1992), and if we, as many do, look at science as something that should produce theories which we can understand and believe in (van Fraassen, 1980), establishing exactly what happens during this process obviously is of interest. Hopefully, proper research methods do, over time, help to fill in the gaps in our understanding. However, in the meantime, I think we would be silly to reject, for instance, current research methods in physics, or worse still, reject physics, because it provides no interpretation of how electrons move from one orbit to another without passing the space in-between, or how they change from mass to energy and back. The problems of 'black box' theories are not confined to the social sciences and do not reduce the usefulness of theories. The debate between Einstein and Bohr and others about research into Quantum Theory is instructive in this context (Fine, 1991).

However, it seems that the current role for interpretative research is not sufficient for Seymour *et al.* According to them we can use only interpretative methods in construction management because:

Construction management is not amenable to such an approach [establishing global, objective and verifiable causal relationships]. The discipline of construction management is crucially different from that of a natural science in that our 'objects of study' are people (p.118)

The most important consequence of this sweeping reinterpretation of scientific philosophy is that medicine, dentistry and all social sciences deal with people and therefore, by application of this logic, cannot accommodate causal relationships. We can, in short, make no predictions about human behaviour. The ways most of us relate to our environment would indicate that there is a large number of people that would not totally agree with Seymour *et al.* Not even Popper, with his uncompromising emphasis on natural science, advocated these kinds of views (Popper, 1982).

That is not to say that there are no differences between social and natural science theories. There are, and the major difference originates in the generative assumptions which, in many social science theories, is

based on a motivation, e.g. profit maximization in economics. The problem with these kinds of assumptions is that they cannot be tested in such a way as to be falsified. This problem has been discussed by, among others, de Marchi (1988), Hausman, (1985, 1989), Klant (1984) and Redman (1991).

Motivations, unlike concepts such as 'gene' or 'electrical charge', are not labels of natural phenomena. They are not sets of items that behave in exactly the same way and share exactly the same, manageable, small sets of causes and effects (Rosenberg, 1994). Motivations require individual interpretations of the consequence of specific behaviour and therefore cannot be brought together in *unconditional* causal generalizations that enable us to predict and control *individual* human actions (Rosenberg, 1994).

The consequence of this is not, as Seymour *et al.* suggest, that we cannot formulate theories and economics is a good example of this. The consequence is rather that we cannot hope to improve the core assumptions through empirical testing and attempted falsifications as in the natural sciences. That, in turn, means that we have had to give up the idea of a unified humanistic theory that explains not only economic activities but also religion, love, hate and charity. Because the core assumptions cannot be improved, neither can the system of propositions built around them and the domain of individual theories may become quite narrow.

Testing of theories based on motivational assumptions is often restricted to establishing the validity of the auxiliary assumptions and verification frequently takes the form of illustrations (Machlup, 1978). In some of the social sciences, particularly economics, this has led to a *de facto* rejection of Popper (1959, 1972, 1983) and his prescriptions for scientific testing, in favour of Lakatos (1970, 1971, 1977), because of the many areas where there are substantial conflicts between falsificationism as postulated by Popper and the actual practice of most scientists (Hands, 1993). These practices include the acceptance of an unfalsifiable, metaphysical hard core and the preference for corroboration rather than falsification (Backhouse, 1994).

Hence, contrary to the impression given by Seymour *et al.*, the social sciences already have a modified methodology that takes into account their special characteristics. What is so fundamentally disturbing in Seymour *et al.*'s note, is not that it advocates a pluralistic attitude to research methods, but the basic anti-scientific attitude that it advocates. Seymour *et al.*'s real argument is not about one research method or another, it is, as indicated above, much more fundamental: they reject the existence of covering laws, universal causal relationships, in any area where people are concerned. This begs the question: if scientific

research is not about finding causal relationships, because there can be no such relationships; not about establishing general relationships; because there are no general relationships; not about verification, because there can be no verification, what is the purpose of scientific research? What is the nature of theories we should develop instead, and how can we use them if we can not generalize or predict or test? Seymour *et al.*'s argument has been around for a lot longer than they have, but so far there has been very little to recommend it, there has been no productive output, no theories, no scientific progress.

A positivist approach does not guarantee progress. It does not guarantee good research, because there will always be poor researchers. It does not guarantee objectivity, because there can probably never be any completely objective research. However, positivism currently offers the best way to reduce subjectivity and to discipline undisciplined researchers and the results, in terms of advancement of science, are nothing short of spectacular. Why reject it for something less useful?

The only positive indication in the note of what the authors have to contribute to research is that they have 'leisure to think'. May I suggest, if that is the reason for the note, that a game of Trivial Pursuit would be more meaningful than any attempt to turn the quest for science into a totally trivial pursuit.

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