Construction research: a field of application

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Construction management research: a field of application

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Abstract: Research in construction management is diverse in content and in quality. There is much to be learned from more fundamental disciplines. Construction is a sub-set of human experience rather than a completely separate phenomenon. Therefore, it is likely that there are few problems in construction requiring the invention of a completely new theory. If construction researchers base their work only on that of other construction researchers, our academic community will become less relevant to the world at large. The theories that we develop or test must be of wider applicability to be of any real interest. In undertaking research, researchers learn a lot about themselves. Perhaps the only difference between research and education is that if we are learning about something which no-one else knows, then it is research, otherwise it is education. Self-awareness of this will help to reduce the chances of publishing work which only reveals a researcher's own learning curve. Scientific method is not as simplistic as non-scientists claim and is the only real way of overcoming methodological weaknesses in our work. The reporting of research may convey the false impression that it is undertaken in the sequence in which it is written. Construction is not so unique and special as to require a completely different set of methods from other fields of enquiry. Until our research is reported in mainstream journals and conferences, there is little chance that we will influence the wider academic community and a concomitant danger that it will become irrelevant. The most useful insights will come from research which challenges the current orthodoxy rather than research which merely reports it.

Keywords: research methods, research discipline, academic development.

Introduction

Research in construction management is diverse, in content and in quality. There seems to be a tendency among some researchers to imagine that construction is so very different from other fields of endeavour that no account needs be taken of knowledge from outside of the construction literature. This is a mistake. Construction is a part of human activity, a sub-set of it. Therefore, construction management is a sub-set, not a different set of management. It is extremely important to draw upon the accumulated expertise from outside construction management. Only if this is done properly, can we hope to influence thinking outside of construction. Until the construction management research community begins to influence the rest of the academic world in some way, we will always find ourselves a derivative and secondary area of research. I have been asked to provide a keynote paper for this event based upon my experiences as an editor of **Construction Management and Economics**, the foremost international refereed journal in our field. I am delighted to take part in this way and offer you this discourse in the spirit of an opening for debate.

There are several aspects to the problem of the place of construction management in the wider academic community: first the influences into and out of construction management research, second, the personal experiences of researchers within our community, third, the notion of a "construction management discipline" and fourth, the impact of our publications.

Influences on construction management

Many of the best works in our field are influenced by sound work from a more mainstream discipline. The weakest researchers seem to disregard the fact that the problems we face in the construction industry are usually examples of problems already faced elsewhere. One of the perennial questions that I ask authors and my own researchers is *what is the general class of problem to which this specific problem belongs?* The answer to this question brings with it a battery of tools and techniques instantly recognizable to someone experienced in a more fundamental discipline, whether it is systems analysis, organizational theory, motivational theory, environmental psychology or whatever. The fact is that a reasonably good piece of work in one of these more general disciplines will always be recognized as such by a sufficient number of peers. This is the kind of justification needed for publication or for the award of a research degree. Without this kind of endorsement, our work may suffer from serious methodological weaknesses.

The difficulty in defining the field of construction management stems from the fact that our influences are very diverse. Papers in our journal contain many references to the literature on project management, or on cost modelling, but these, again, may often be derivative works which have built upon some more fundamental theory. In the process of developing such general ideas for application to a specific construction problem, compromises need to be made. If subsequent work does not trace the antecedents of the theory back upstream, there is a danger that the compromises, rather than the real theory, become the basis of research. It is therefore extremely important that researchers in our field trace ideas back upstream to their (often) non-construction origins. Indeed, Betts and Lansley (1993) lamented that there was an increasing trend among authors of papers in Construction Management and Economics to refer only to other papers in the journal, or to construction literature. This is a trend that needs to be reversed. Otherwise, we will grow increasingly introverted and of no real interest to academics outside of our immediate circle. During this conference, I would like to explore the potential for theory building and theory testing which will influence fields wider than our own.

Research as self-education

The best work is that which contributes to our understanding. This holds for research and education, the dual themes of this event. Research and education are frequently paired, perhaps because the two are so difficult, in reality, to separate. Perhaps the only difference between research and education is that if we are learning about something which no-one else knows, then it is research, otherwise it is education. This is an important point. There are many researchers who are simply re-treading well worn paths. Since they are generating no new insights to our general understanding, they are engaged in a process of self-education. This is a valuable process, but should not be confused with research. Education in this context is perhaps not what was meant by the conference organizers. However, it does flag the indivisibility of education and research. It also highlights a perennial problem for a journal editor! A paper which goes through the motions of research, apparently making discoveries and having all the hallmarks of a good paper, may simply be a record of one person's learning, rather than a contribution to our general understanding.

As each of us learns more about the things we deal with, we find that our previously accepted models of the world are no longer appropriate. It is important not to confuse one's own learning curve with the general utility of past research. For example, in coming to grips with the physics of heat transfer through walls, we learn about various formulae which help in

calculating heat loss. If it subsequently turns out that these formulae are inaccurate or unrealistic, this does not detract from their value in helping us to come to a better understanding of thermal transfer and putting us in a position where we can understand better the full complexities of the processes. As individuals acquire more sophisticated skills, they become aware of the limitations of what they learned earlier. This process is to be expected since we use simplified models to develop our understanding. Therefore, I would expect there to be a continuous stream of individuals experiencing the gradual realization that all that they had previously learned is not as reliable as they once thought it. I recall my own sense of disillusionment, during my PhD studies, when I had to acknowledge the limitations of what I had previously thought of as "known". None of this devalues the quality of the work in our field. It is an inevitable result of the need to develop simple models in order to help make sense of a very complex reality. Runeson (1997) cited an interesting example of this phenomenon when he mentioned sailors' use of tables for calculating their position, even though these tables were known to be inaccurate.

Holistic vs fragmented approaches

Within the general picture of personal voyages of discovery there is an interesting problem related to the difference between a holistic approach to knowledge and a fragmented approach. I once took a small part in a degree course for architects and noticed that from day one, students were solving whole problems and then having their work subjected to criticism by the tutors. This project-based approach taught the students that they had to discover for themselves the parameters to a problem and had to choose their own methods for solution. It struck me that this was very different from the disaggregation witnessed in education for surveyors and builders. Here, the knowledge deemed to be required is listed, codified, classified, ranked and split up so that it can be drip-fed and particular methods allocated to particular problems for the students. I do not know how typical my observations are and I would be pleased to learn more about this from contributors to our conference.

Models of scientific research

One big problem in our work is the interface between the Natural Sciences and Social Sciences. Models of scientific enquiry are very useful to us in defining research projects and in acquiring data. But some of the work is based on simplified models of enquiry learned at the laboratory bench in school. This is not the same as the kind of enquiry conducted by real scientists (Gleick 1993).

Science proceeds on a basis of replicability and detachment. The social sciences, focusing on people for their analysis, have had to confront the difficult problem of observer influence. When the researcher turns out to be one of the things that it being researched, there is clearly no detachment and replicability relies upon an identical human following the footsteps of the first researcher. This impossibility renders many of our attempts at the application of science redundant. One interesting point about this phenomenon is that it is not confined to social science. Quantum physicists face similar problems in their work. For example, how can you use light to look at something smaller than a photon? The very act of looking changes the thing being observed. Observer influence, then, is not a unique problem confined to social science, but a general problem. It has to be addressed in our work, whatever branch of the sciences we seek to apply. It is good to see this kind of debate emerging in the provocative papers of Seymour *et al* (1997).

The difference between undertaking and reporting research

A third strand in this discussion of the relationship between researcher and research is the importance of separating the doing from the writing. Research is not undertaken in the same sequence and manner to that in which it is written up. Writing up (when done well) tends to follow the text book science pattern of:

- describing what is already known,
- identifying the gap in knowledge,
- discussing the kind of data that will be needed to plug this gap,
- choosing an appropriate method for collecting and analysing the data,
- presenting examples of data,
- discussing the findings,
- developing conclusions in the context of limitations of the particular study and
- relating these back to what was presented as already known.

There is absolutely no need to actually undertake these tasks in this sequence. Many good research projects begin with a clear vision of what the conclusions *might* be. Other projects may be undertaken with a particular research method in mind. The researcher is frequently simply making all the ducks line up to be able to present some insight or finding so that others can see how it fits in, in the researcher's view. Thus, working models and rules of thumb can gradually be developed into more robust and general theories. This is the important point about the process; the generalization of the theories we use. A theory which only explains one event involving one person is an interesting anecdote, perhaps, but until we can pull some generalization out of it, it remains an anecdote. The orthodox rigidity of publication is a useful technique for demonstrating the generalizability of research findings.

The notion of discipline in construction management

I am aware that other papers in this conference are dealing with the notion of construction management as a "discipline", so will do no more than mention it in passing. The notion of a discipline may be connected to a notion of exclusivity. Economists and lawyers have little problem moving between industries in applying their theories. While the particular facts change from one industry to the next, the methodological applications do not. As Hillebrandt (1985) pointed out, the distinguishing features of construction are not individually unique, each having a similarity or parallel in other industries. It is only their combined effect which is unique. Similarly, in law, there is no distinction made between civil engineering and building. If a case is applicable, it is applicable, regardless of its origin. There is much to be learned in the way that economists and lawyers develop generalities from specifics. The ultimate test of generalizability is the extent to which findings from construction research can influence researchers in the more fundamental disciplines.

Obviously, there are pressures which force us into an exclusive mind-set. Among these are the needs within education to specialise and carve out territory. This kind of pressure is reinforced by our close association with professional institutions with their inherent inertia and conservatism. Professions need to define and defend boundaries in a way that academics do not.

Of course, it is important that in acknowledging our multi-disciplinarity we do not fall into the trap of non-disciplinarity. The worst research in our field falls into this latter category. The best is more like work undertaken in the field of ergonomics where disciplines as diverse as psychology, physiology and engineering are brought to bear upon problems related to work in its most general sense. The science that is applied to these problems should not be watered down simply because it is part of a multi-disciplinary effort.

Boundaries need to be drawn around any study and one should beware of generalizing too widely from a strictly defined sample, such as whatever we may choose to define as the "construction industry". On the other hand, such dangers should not lead us to *start* from the view that construction endeavour is a separate category from human endeavour generally. Clearly, we already know much about humans in enterprises, in groups, in transactions and as individuals etc. There is no *a priori* reason to simply assume that none of this applies to construction. Such an assumption of exclusivity needs careful arguing, if it is not going to seem like an accident of omission.

The impact of our research

Research amounts to nothing unless it has an impact somewhere. First, it needs to be recorded so that it is "on the record" for future generations of researchers. Second, it should have an influence on our understanding, whether in terms of developments to theory or developments to practice. As researchers, the only really worthwhile publishing is in academic refereed journals. This is not because this is the "best" or the most widely read literature, but because of the peer review process which filters out work deemed to be unsatisfactory.

Why publish?

Publishing the results of research leads to peer recognition, career development and the oportunity to contribute to a "field" of knowledge. Contributions may be theory testing or theory building. But the purpose of a journal is to act as a record of what has been accomplished. The peer review process is there to ensure that what goes into the record has some validity and extends our understanding in some way.

As a practical, pragmatic industry, representing a large portion of any nation's economic activity, construction involves a diverse range of skills and materials in every conceivable physical environment. Every human interacts with buildings in some way, therefore construction cannot be isolated into some sub-set of human activity. Just as the ergonomists use any relevant skill to study problems related to work, those who study construction might draw upon any of the sciences to solve particular problems. Thus, construction is not a field or a discipline in its own right, but a rich source of problems and data which does not necessarily preclude one or another discipline.

Questions

Some of the most useful practical advice that I have come across about research design and implementation (e.g. Sapsford and Jupp 1996, Herbert 1990) is produced by researchers from the "caring professions" like nursing and psychology, or from criminologists. What is it about these fields of research that precipitates such useful contributions to the literature? What is it about the way that we conduct research into construction management that seems to preclude the production of such generally useful advice from construction management

researchers? I am often horrified when I hear a construction researcher tell me that they cannot use such a book as it "does not apply to construction research"! What is it about such construction management researchers that seems to preclude their use of such generic advice? Indeed, is the assumption underlying this final question valid, i.e. can we be sure that there really is a cult of exclusivity in construction management research?

One of the difficulties of producing work which is easily recognised as going through the "right" motions, is that therein lies the least chance of originality and insights. As George Bernard Shaw said - *the reasonable man adapts himself to the world, the unreasonable man adapts the world to himself; all progress depends upon the unreasonable man*. And progress in our understanding of construction management may depend upon counter-intuitive, unexpected ideas. This is a feature of discovery well explained by Wolpert (1992) and leads me frequently to consider very carefully those papers which get the most severe criticism. These may contain the seeds of special insights. It is this feature which also leads me to doubt the value of consulting practitioners and sifting through their combined thoughts about *the best things to do*. Some researchers seek to discover solutions to research questions by asking practitioners what they do, with a view to codifying and representing best practice. While this, to be sure, is a good way of discovering the current orthodoxy, it is unlikely to produce as many insights as seeking the less obvious, counter-intuitive solutions.

Conclusions

The work of construction management researchers is sometimes of a very high quality but there is a large proportion of work which lacks merit. If construction management researchers are to become an integral part of the wider academic community, it is essential that we do this on equal ground. We must harness the methods and techniques which have been shown to work in other applied fields. Of course, this is not to say that they need no adaptation or development. But until we adopt the approaches of mainstream scientists (social or natural) we will not be able to have the influence that many of us hope for. The ultimate goal will be publication in mainstream journals. The most useful insights will come from challenging the existing orthodoxy rather than merely reporting it.

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