Book Review

Modern Construction Economics

Gerard de Valence (ed), Routledge, London, 232 pp. Hardcover, ISBN978-0-415-39706-3, USD 115.00 GBP 70.00

The editor, Gerard de Valence, points out in the preface, this book is neither a textbook nor a guide to what is done by construction managers and construction economists – read quantity surveyors and the like. Rather, de Valence notes it comprises a collection of chapters each of which focus on matters at the industry level and, in doing so, illustrates that a substantially improved understanding of the building and construction industry can be gained beyond the economics of delivering projects. Before giving some thought to how far each of the chapters achieve this, it's worth reflecting on the virtues of developing construction economics as its own discipline or sub-discipline in general economics and the bold manner by which de Valence is proposing we do this. That is, de Valence proposes partitioning industry and project economics - as explained in the preface and in Chapter 1. de Valence's view that "the time seems right" for these developments is also worthy of some consideration.

George Ofori in his article in *Construction Management and Economics* (CME) in 1994 held the view that construction economics cannot be described as a bona fide academic discipline. Towards developing this view, Ofori indicates some frustration that we're practically ignored as a discipline in general economic textbooks and just glancing through a few more recent general economic texts that I have to hand, I find some evidence that this continues to be the case today. With regard to why we might seek to change this situation and develop construction economics as a sub-discipline of general economics, de Valence offers essentially a business case based on construction related factors that contribute to gross domestic product. As important as we know this is, I suspect that this is insufficient motivation for many researchers who are prioritising the desire for an autonomous learning environment and other benefits associated with academe over any serious commercial rewards. In his CME article, Ofori goes on to give a rationale for establishing our own discipline comprising benefits from legitimacy in terms of attracting the best minds and research funding. This rationale for change resonates more than de Valence's business case for change – at least to me. However, Ofori's rationale may still be seen as falling short in terms of inspiring individual academics to make a real contribution to developing our discipline and in turn our industry - if various updates on our progress towards this goal are anything to go by. For example, Graham Ive and Chen-Yu Chang in their review of CME articles at the CME 25 Conference in 2007, concluded CME contains a small number of articles with theory references solely to economic literature. Given this lack of progress, it might be that de Valence's *the time seems right* statement is perhaps more clearly interpreted as *the time is up* on allowing more practical and project-based work to dominate our journals.

Moving construction economics away from projects to industry level analysis and onto more theoretical ground is, therefore, an ambitious suggestion given that updates on Ofori's article indicates that a number of our researchers may struggle to make this transition – as things currently stand. Beyond proposing our future direction and domain in construction economics, more fundamental thought might usefully be given to developing the culture and entire infrastructure surrounding research activity in our field, in order to increase the number of our academics that emphasise articulating problems over solutions; the long term over the short term; and the impact of output over the number of publications. All involved in construction economics have a role to play – not least of which our journals. For example, I was pleased to hear news of recent developments in the editorial policy of CME and which complements the direction proposed by de Valence. These developments include

encouraging papers that incorporate theory and draw more widely than the construction literature, as well as papers that engage more thoroughly with methodological issues and allow a longer discourse. If *all* of our key journals follow suit, then this could be a step towards changing the nature of reward regimes and selection criteria. In turn, this might more readily encourage academics to invest time in engaging theory from economic literature and which may allow a better understanding of the nature of many problems in construction that have been faced and addressed in other sectors. As the editor of AJCEB is also a chapter author, he is likely to sympathise with these developments and view as well.

I move on now from de Valence's preface and his first chapter, to look at the other chapters in terms of how much these other chapters focus on matters at the industry level and how much these chapters show the way in which an industry level approach can yield an improved understanding of building and construction activity beyond the economics of delivering projects. In Chapter 2 Jan Bröchner also develops a critique of project-based economics and, in particular, towards cost and time prediction. This time, however, the focus for change is not such much the issue of domain but rather the conceptual framework. More specifically, and in the face of technological advance and deregulation, Bröchner considers that the New Institutional Economics (NIE) based on some key departures from neo-classical economics including a semi-strong form of rationality (bounded rationality) can extend the reach of conventional neoclassicism and deliver new insights into how incentives for firms and individuals affect issues such as innovation and quality.

Bröchner goes on to illustrate this in terms of three topics that he feels should be given more attention, namely: integration and innovation, signalling in real-estate markets and developing public procurement. For example, with regard to integration and innovation Bröchner rightly notes that two microeconomic theories, firstly Transaction Cost Economics (strongly associated with NIE) and secondly Resource-Based Theory (that has much in common with NIE including bounded rationality) can be used to explain how construction firms integrate vertically and to explain the extent of subcontracting. My view is that Bröchner is spot-on here. Being able to understand and more fully explain these sorts of fundamental business decisions and in the context of construction projects that comprise a temporary coalition of independent firms, will unlock significant savings in reducing both transaction costs and production costs. This advanced understanding also holds the potential to prompt improvement in the delivery of projects and, therefore, increase the overall value of the constructed project from the client's perspective. It's interesting to link Bröchner's chapter to Chapter 5 by Lauri Koskela. Bröchner's message and approach obviates much of Koskela's concerns with mainstream or neoclassical economics. Moreover, I'm sure many of us have gained a great deal from the forum and exchange between Koskela; Glenn Ballard and Graham Winch in *Building Research & Information* in 2006 and for me Koskela is not adopting a forward looking view - in the context of this book and in terms of the issue being considered, by ignoring advances made by NIE and by focusing on limitations in the mainstream neoclassical economics. Moreover, of all the chapters in the book, I feel Koskela's chapter is the furthest away from de Valence's call for the development of theory to explain industry level phenomena, as the three-part theory of production is being advocated by Koskela on the basis that each part has produced practical methods, tools and production templates.

Chapter 3 by Christian Brochman also picks-up on NIE and this time applies the principal/agent relationship on the issue of collusion but with an interesting twist given that *ex ante* the contractor suffers from information asymmetry and assumes a role closer to the principal during the process of the client establishing/selecting the price whereas *ex post* the client then assumes the role of principal and may suffer from information asymmetry that favours the contractor. Brochman's comment that Zarkada-Fraser and Skitmore's work "has serious flaws" on the basis that estimators are the chosen group in the study when, as Brochman asserts, the players are the business unit managers, may fall short of

appreciating the full complexity involved in making the mark-up decision and in other cases may simply be incorrect when estimators and other site-based operational staff for example, engineers and quantity surveyors make these decisions - particularly in remote locations within the business unit. In the end, Brochman concludes that strong incentives exist in the construction industry to engage in collusion and a change in institutional arrangements is required to avoid collusion as legislation is not sufficient.

Derek Drew gives an easy introduction to his Chapter 4, with some simple examples, of the advanced topic of Auction Theory with some comments on its possible application to construction tendering. Quite correctly, he distinguishes between the standard independent private values (IPV) theory and common value (CV) theory and their contrasting assumptions. In short, the IPV assumption is that each bidder's cost estimate is exactly correct, while the CV assumption is also that each bidder's cost estimate is inexact, being a random variable with a mean value that is common to all bidders. In the celebrated IPV Revenue Equivalence Theorem the IPV second price equilibrium result is to bid "at cost" – making this form of auction very attractive to the auctioner in theory. CV theory, on the other hand, gives rise to the 'winner's curse' and the need to make some contingency allowance for its effect. For me, the most interesting aspect of this chapter concerns the extent to which the two theories apply to the auctioning of construction contracts. As is intimated, most industry practitioners would not accept the IPV assumption of exact estimates, while the assumption of a common value also seems inappropriate. Bearing in mind also the other economic and statistical assumptions of both standard IPV and CV theories – including exclusive profit maximisation, sufficient information, symmetry, randomness and stationarity – it seems there is a long way to go yet before any serious applications can be anticipated in the construction world.

de Valence's chapters 6 and 10 provide us with a detailed account of market structures and, in doing so, de Valence develops a useful insight into the particular nature of barriers to entry in construction and across two stereotypical market structures. One of the interesting points that de Valence does not dwell on in these chapters concerns an observation de Valence has previously made concerning the existence of different market structures in a supply chain in the same construction project. I investigated this further in a building services maintenance supply chain and I think de Valance's work has great significance in terms of allowing decision makers to configure supply chains to minimise transaction costs. For example, consideration may be given to avoiding allowing powerful firms to be placed upstream and the same supply chain/contract as a less powerful firm at the head of this supply chain/contract. Instead, the client might engage these powerful firms directly and reduce the extent of information asymmetry it may face. I would have also liked to see some exploration of the connection between the Structure-Conduct-Performance (SCP) model with more fundamental theory concerning the generation of sources of competitive advantage and their relationship with barriers to entry. And it would also have been interesting to consider the current state of play in terms of empirical work on the correlations between the three SCP components including empirical work that shows some interesting unconventional relationships between these components. For example, a U-shaped relationship between market share and profitability.

In Chapter 7, Rick Best gives a comprehensive account of the relative merits of different approaches to comparing construction costs between countries and develops his own approach, nicknamed the "building bloc" approach. The building bloc approach has the advantage of using real projects; identifying cost significant items and assigning weights to these items. On the other hand, Best notes that a number of problems need to be addressed in this approach including the incorporation of contractors' margins in pricing; the establishment of labour ratios and the whole approach which requires some significant analysis. The biggest issue though, seems to lie in Best's comment in his conclusion that "we have no correct" answer to judge the various competing alternatives. Best indicates that

this cannot be solved objectively. I think that this comment needs to be challenged most robustly by all concerned and towards its resolution in order for real progress to be made in selecting better comparative approaches. For example, perhaps there may be ways to externally validate the accuracy of comparisons generated by each of the methods in Best's paper by reference to Key Performance Indicators or other composite measures of client satisfaction that exclude costs. As such, I feel Best's conclusion does not live-up to the rest of his chapter.

Karen Manley and Stephen Kajewski's chapter 8 develops a conceptual framework of firm-level determinants of innovation and in conjunction with a postal survey of key organisations in the Australian construction Industry and which generates 383 usable responses. From this largest construction innovation survey conducted to date in Australia, a set of interesting results are presented and discussed including: adoption rates for advanced practices; business strategies; reasons for undertaking innovation; sources of innovation; and blockers and encourages of innovation – by industry group; sufficiency of innovation to cope with international competition; suggestions for improving the international competitiveness of the Australian construction industry; and innovation profitability impact. In the context of this book, it I think it would have been useful if Manley and Kajewski could have explained more clearly how their conceptual framework was developed and, in turn, how their framework contributes back to extant theory.

Chapter 9 Bee-Lan Oo gives us an overview of the application of an experimental approach and illustrates this with respect to a bidding experiment including participants in Hong Kong and Singapore. The main problem with theory testing *in tendering* is that the 'real-world' of construction tendering is vastly complicated and uncertain. It makes the task of the ancients to work out the movement of heavenly bodies relatively simple – at least you can see the heavenly bodies regularly and record their movements. With tendering, all you see are the bid prices and maybe a (probably unbalanced) priced Bill of Quantities from time to time. You may have little idea as regards to how the prices were devised or what the bidders took into account at the time. Tendering is so important and competitive that we struggle to collect data/records on past bids, costs and profits and to interview people to get reliable information. An analogy might be to ask an alien to build a model of the workings of a television by letting the alien look at a plasma screen for a few seconds occasionally! Apart from that, Oo does a good job in explaining the relevance of experimental work in economics and an excellent job in going through many of the criticisms of the method (including lack of validity) and her own experiences. For me, the point is that opportunities for experimental work *do* exist in construction economics.

It's very appropriate that Göran Runeson has the final chapter and proceeds to anchor us in his inimitable style. There is no doubt in my mind that Runeson is a master at his craft and on the methodology of economics research. Runeson has helped me yet again improve my understanding of methodological matters and on this occasion, I think I've progressed my understanding of the significance of motivations with the lead-in Runeson takes in this chapter towards making the point that there is no "right" response, such that motivational assumptions cannot be falsified. However, I don't completely share Runeson's observation that our field is "in total confusion". In one sense, it's the opposite, as we have adapted to the nature of research in our discipline and become very clear and skilled at generating a decent number of articles deemed acceptable by our highest ranking journals - without seriously connecting with economic literature as noted by Ive and Chang and mentioned previously. The issue then may go beyond considering how much room there exists for our academic community to increase their understanding of methodological issues *to* how do we provide sufficient incentives for our academic community to fully engage methodological issues and in order to help sustain a robust debate about alternative/conflicting methodologies. And in this sense, I completely agree with Runeson. That is, such a debate is critical but also *how*

we evolve this debate is equally critical to help move construction economics beyond a project and practical level concern, towards an industry and explanatory orientation.

In summary and in terms of scope, I was a bit surprised that there was not more treatment given to more broader aspects of the behaviour of firms and public policy and regulation. It's also apparent that not all of the chapters represent full explanatory research including some room to deploy economic theory more clearly and to contribute back to extant theory. In particular, the low lying fruit here is the opportunity to learn something new about the theory itself as a result of working with it in construction. There is no doubt our industry is very special and exciting. With its extreme conditions on many fronts, our industry gives us very fertile ground within which to research including adapting and contributing to theory. On the other hand, I think all the chapters avoid the charge of being prescriptive and each of the chapters has something important to say in their own right. Such that, overall I agree with de Valence's comment in the preface that all of the authors are contributing towards understanding our industry.

Finally, returning to Ofori's 1994 article and using a few key criteria that Ofori referred to in order to briefly help update our progress towards becoming an academic discipline in construction economics, it can be seen that Runeson's chapter connects closely to the "mode of enquiry" criterion; Bröchner's chapter speaks very clearly to the "conceptual structure" criterion; and de Valance has, through his book, advanced a bold direction for construction economics at an industry level and, in doing so, has progressed the "domain" criterion. In total, I think this book is achieving its purpose and contributing towards establishing construction (industry) economics as a field of research. As such, I congratulate and applaud Gerald de Valance and his colleagues for their collective efforts in helping to keep the progress of our field alive and kicking.

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