Social Capital Modeling in Virtual Communities: Bayesian Belief Network Approaches (Premier Reference Source)

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Information Science Reference: Hershey, PA, USA, 2009
ISBN 1605666637 (pb)

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This book raises important issues about academic publishing that need airing and the result is a rather unconventional (and polemical) review. However, the reasons for this will, I hope, become clear and, in the process, the book will itself be discussed in a more traditional fashion.

Here is an idealised view of academic publishing, regardless of whether it was, in fact, ever like this. Publishers (and particularly academic publishers) would reject books that were clearly incompetent. Once books had met certain minimum standards of literacy and interest, journals would then decide to review those that made a significant contribution and/or deserved critical attention. (This also meant that valuable but specialist works would tend to be less reviewed and reviewed in more specialist outlets.) Thus, if a book was not reviewed, this suggested it was not worth attention in a particular arena but was not necessarily incompetent. How should academia respond when it can no longer count on apparently "academic" publishers to stop plainly incompetent books being published? If we ignore them, they may sell by default and further silt up a world already troubled with information overload. (Bad books also cause trouble by acting as a source for citation by other bad books.) On the other hand, if we give them the kind of detailed critical attention given to books that have met minimum standards of competence we imply that they are on a par with these books and contribute to a further blurring of quality.

I have no desire to upset the author of this book. He is responsible for no more than the widespread unfocused desire to "tell the world". However, I find it really worrying that a publisher ("Publishing Academic Excellence Since 1988") and two reviewers (as well as the writer of the foreword) apparently did not notice how far out of his depth the author was. Now this book is "out", it raises a problem for academia that this review will try to discuss.

It may be instructive to consider why this book shouldn't have been published. (Even if some publishers can't be trusted, perhaps a few aspirant unqualified authors will be reined in.) Alarm bells first went off when I discovered that while the first sentence of the book consisted of English words, it was not actually in English. Throughout, the book has an amusing element: "The emergency of a wide variety ..." (p. 10), "The discipline of Sociology has contributed enormous theories ..." (p. 13), "Social capital is a ... litigious theory ..." (p. 18), "For hundreds of thousands of years, the discipline of Economics ..." (p. 19). Given the size of the academic community, it is hard enough to say anything original even if you have full mastery of your tools (in this case language) and the combination of shallowness and inaccuracy is almost invariably fatal to content. (There is a charitable belief that poor English shouldn't be confused with poor content but would anyone like to point to work with something really important to say that is only held back by language problems?)

The second set of problems reminded me forcefully of the early stages of PhD supervision. Most of the book is actually a literature review of the unfocused and relentlessly descriptive form that makes the hearts of supervisors sink. The citation (where it occurs, two or three pages often pass without a single reference and only whole works are cited, arguments are almost never localised to a chapter let alone a page) reminds me of the least effective Wikipedia entries: A mixture of recognised high points (who could miss Putnam on social capital?) and personal obscurities (a chapter headed "a Computational Model ..." does not mention Schelling, Epstein and Axtell, Forrester, Gilbert, Carley or indeed anyone that this community would regard as important). In a sense, it hardly matters in the face of all the other problems, but the tone of the book says more about the author than the intended readers. What, for example, does it suggest that there is a footnote explaining the term ceteris paribus? Either don't use it at all or assume that your audience will also know what it means!

The third issue, which should again have been spotted by publishers and reviewers, is that the book is one long broken promise. (This is a common failing that perhaps deserves articulation and shorthand to speed up reviewing in future.) Reading forward, one is led to believe that the modelling section will "justify" the very long description of forms of capital, competing theories and measurements of social communities, virtual communities and modelling itself. In fact, the "payload" only starts on page 208 of a book with 251 substantive pages. (I am not counting the appendix of "sensitivity analysis" where one set of arbitrary parameter values is replaced with another.) This turns out to be a model that simply "slots in" a random selection of social capital "variables", asserts causal connections between them arbitrarily and then explores the behaviour of this fictional system without any reference to data. An example of a variable is "Knowledge awareness" defined as "Knowledge of people, tasks, or environment and or all of the above". The states allowed are "Present" or "absent". (p. 211) I can actually state the problem with the modelling in this book no better than the author himself: "However, it is assumed that likely that the results of the model predictions could change much,
in the face of empirical data." (p. 216) While this sort of poor pacing and lack of "punch line" is forgivable in a conference presentation given by a graduate student (I recall with acute embarrassment my own early attempts to make research look more "finished" than it was) it is completely inappropriate here. Inside this book there is a mildly interesting discussion paper about the modelling applicability of Bayesian Belief Networks too weak to struggle out. Ex post, having ploughed through it, the reviewer is infuriated to discover that none of the two hundred odd pages of literature review really mattered since it was all boiled down to a naïve and arbitrary variable list by the method and the real challenge (deciding how the definition of social capital can be made coherent and useful) was completely dodged. At least, getting this review published, I can save the valuable time of anyone else who might be thinking of reading this book!

It is important to understand this review. I am not being so snide because I think this book should have cited Wankdorf on page 184. I have a list of challenges on fact and interpretation as long as my arm that aren't even worth raising. I am being critical because this book is so far from basic scholarly competence that its publication can actually be seen as (mildly) harmful to academia. It is important to state that books like this are communally harmful, using not just academic language that might imply that the criticism is merely a matter of interpretation or animus, but by saying loud and clear that this isn't even close to what is required. I can't see myself reading anything published by IGI Global in future unless it is recommended by someone I trust. Life is simply too short. On a practical note, do we need a convention in the reviewing process to ensure that books sent out for review exceed some minimum level of scholarship? Should we add a "junk list" to review sections to record books that were not deemed to pass this threshold?