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Published in:
Journal of Information Technology

Published: 01/12/2010

Document Version:
Post-print, also known as Accepted Author Manuscript, Peer-reviewed or Author Final version

Publication record in CityU Scholars:
Go to record

Published version (DOI):
10.1057/jit.2010.35

Publication details:
Davison, R. M. (2010). Retrospect and prospect: Information systems in the last and next 25 years: Response and extension. *Journal of Information Technology*, *25*(4), 352-354. https://doi.org/10.1057/jit.2010.35

RETROSPECT AND PROSPECT: INFORMATION SYSTEMS IN THE LAST AND NEXT TWENTY-FIVE YEARS: RESPONSE AND EXTENSION

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In his remarkable essay, Lee (2010) provides us with trenchant analytical insights into what has eluded IS researchers over the past quarter century, and then charts ways that will, hopefully, ensure that the same lapses are not repeated. This is of course an over-alembication of Lee's essay – the richness of a lengthy and cogent set of arguments is not usefully condensed into a 'tweet-and-a-bit'. After pointing out that 'espoused' and 'in use' theories are not at all the same thing, Lee highlights the ambiguous, 'taken-for-granted and unreflected-upon theories-in-use' about each of five key terms in the IS domain, viz.: information, systems, theory, organisation and relevance. A number of examples of each of these 'theories-in-use' and their ambiguities are provided.

Lee's suggestion is that we should rely to a greater extent on the real world contexts and practices of the organisations which we purportedly aim to study, and for whom our research is supposedly relevant. These organisational contexts and practices have the potential to reveal a wealth of richness and relevance that provide context for information, systems and theory that would go far beyond what we have accomplished so far. However, to achieve such improvements in the espoused and in-use theories about how to do research requires radical change at the social, community and institutional levels – i.e. the real and virtual groups with which we are affiliated. Such change will need to be reflected in a different set of values that is demanded by the reviewers and editors of our journals, not to mention deans and funding agencies. As Lee points out, this change to the current status quo is not at all straightforward – at least in the more established institutional contexts. However, he also points out that in the emerging, non-western centres for IS research, where the status quo has yet to be fully established in a western mould, there is the opportunity to set up a new and radically different set of norms. India and China are specifically identified as locations where such an opportunity exists.

My own extension to Lee's (2010) essay starts at this point. In the text that follows, I extend his arguments further, then apply two additional theoretical lenses from Tsui (2006) and Bacon (1620), and wrap up with some concluding thoughts of my own.

First, I find myself in agreement with Lee's arguments and do not wish to further deconstruct them. However, I feel that in some respects they do not go far enough. For example, considering the nature of change, I would argue that a key aspect of any strategy to effect change in the way we undertake IS research will require both a different model of training for our PhD students (since it is far easier to inculcate new values in their relatively unformed minds than in the sclerotic cortexes of more established scholars, cf. Davison & Martinsons (2010)) and a different attitude to the values and skills that we seek in new lecturers/professors. Further, as Lee (2010) implies, it's not good enough that an editor espouses the belief that critical and interpretive research is as valid as positive research if his/her journal (assuming an equal volume of papers of equal quality from the different epistemological positions) still ends up publishing more of one epistemological persuasion than the others.

Furthermore, too much IS research is intellectually and organisationally pedestrian – it may be conducted to high standards of academic excellence, yet it lacks spark, charm and wit, is often irrelevant to organisations, and barely advances knowledge at all (cf. Robey & Markus, 1998). As an editor myself, I see countless numbers of these papers across the board from our top journals downwards. This is the beast that we must tackle, and we know the beast: it is us!

Tackling the espoused and in-use theories inherent to the five key terms that form the heart of Lee's (2010) essay – information, systems, theory, organisation and relevance – is a large part of the task. Indeed, I argue that a sixth term – rigour - should be added, since this too is seldom reflected-upon or challenged, yet is clearly a critical measure of the quality of our work.

Lee gives the example of social network analysis that too often relies on a lay interpretation of the concept, not the original anthropological or sociological interpretation. I suspect that few IS academics are very familiar with sociology and anthropology – there are scattered references in our literature (e.g. Avison & Myers, 1995) – but these are few and far between. A good start would be ensuring that PhD students are more familiar with these disciplines – and instead of simply requiring them to take courses from those departments, we should hire professors competent to teach them. This of course opens up a nice can of worms in the diversity debate (Benbasat and Weber, 1996; Robey, 1996). I suggest that IS research will only gain more relevance to organisations and indeed prominence in the community when IS researchers are drawn from a wider set of the organisational sciences than is currently the case, and when these same researchers feel comfortable to incorporate a wider mix of methods and theories in their research designs. Too often, we hire IS graduates as IS professors (and particularly those who have already demonstrated the ability to publish in our 'top' journals) – an incestuous practice that selectively enforces the same norms and makes change to the status quo all the more difficult. If we are to change, then we must change not only what we believe and what we do but also who we are.

A second issue that I wish to raise concerns the possibility that IS research in the emerging Indian and Chinese (amongst others) contexts might be undertaken differently (Lee, 2010). This is a valid hope, but my personal sense is that it is likely to be a forlorn one. The major Chinese universities where IS is taught and researched are already setting up their own journal lists – and these lists contain exactly the same journals that you would find in western countries. A western hegemony is thus being perpetuated. This is not a case of the west directly exporting its values, though returnees from the west often do bring those values with them. Nevertheless, western academic values are being incorporated into other contexts – often wholesale. For Chinese, Indian and other scholars to achieve publication in these journals will entail a perpetuation of the same problems that Lee describes.

At the recent AMCIS (2010) conference, held in Lima, Peru, I was struck by the statistics. Some 80% of papers from the Americas region were from authors based in the US, with 7% from Brazil and 6% from Canada. Peru was scarcely represented at all. Perhaps it is the case that Peruvian scholars don't do IS research, but I assume that at least a few must do otherwise there would be a serious shortage of host-country people sufficiently knowledgeable about the domain area to be in a position to run the conference. More likely, the academic expectations embedded in the manuscript review 'system' act so as to favour research undertaken in cultures (or researchers professionally socialised in those cultures via PhD programmes) where those expectations were developed (i.e. in the west) and anyone not familiar with that culture and those expectations will have an uphill battle getting their paper accepted.

When I discussed this phenomenon – few papers from Latin American authors – with a member of the AMCIS Conference Committee and other attendees, I was informed that holding the conference in Peru was already a step in the right direction. By enabling more Latin American scholars to attend (Peru being closer to home and so cheaper to reach), so these scholars would become more familiar with the standards and norms appropriate to the conference. Thus, the conference itself provides the basis for professional socialisation of these Latin American scholars in the discipline. I expect that a similar argument will be made for ICIS 2011 in Shanghai, though more papers authored by China-located scholars will be presented for the simple reason that Chinese authors have already been professionally socialised to a greater extent into the hegemonic norms of the 'international' IS community.

In order to change this situation, either we have to change our international norms and expectations, or change the researchers (i.e. continue to ensure that they are professionally socialised according to the norms of the status quo, and thereby forego any of the changes that Lee implies are necessary). A third direction is for the non-western scholars to ignore the west and to set up their own publication venues. Whilst feasible and perhaps attractive to those institutions that favour isolationism and international pariahship, this is unlikely to happen to any significant extent because institutional prejudices will come into play. Presidents/Vice-chancellors and deans generally seek to compete internationally and international benchmarks tend to focus on the established journals of the hegemonic mainstream (viz. the UT Dallas list of the top 100 business schools, SCI/SSCI impact factors and the THES or Shanghai JiaoTong indexes of global universities).

As I indicate above, scholars located in non-western countries (particularly India and China) are already attempting to emulate the research practices and values of the west. Studies commonly take an already developed and much validated western theory and attempt to force-fit it to the local context, yet with little attempt at localisation. In a recent example from India-based authors that I handled as editor, the entire paper lacked any Indian context – except that the authors had Indian affiliations. Since the paper concerned technology adoption, it seemed reasonable to assume that there might be indigenous Indian constructs that would affect the adoption process, yet these were not even considered. When I gently reproached the authors, they blithely asked "but who would be interested in what happens in India?".

Well, if no one is interested, why do the research at all, but the authors assumed that a double blind review process would shield their identity from the editors and reviewers and so ensure that the research could be assessed purely on its a-contextual merits! Actually, I believe that we should be very much interested in what happens in India – or at least as interested in what happens in India as anywhere else. There is no one 'most appropriate' location from or about which research is intrinsically better or more relevant; this is my espoused belief that I consciously practice as author, reviewer and editor. I would rather highlight the unique contributions of the social and organisational context where research is conducted, thereby enhancing the relevance of the study, than conceal the context altogether.

I suggest two approaches to these extended problems. The first draws upon the work of Anne Tsui (2006, p.3) who wrote that there is an urgent need to identify "the issues that are unique or at least important to Chinese firms, managers and employees, even if such phenomena might be foreign to scholars outside this context". A focus on these indigenous issues would constitute an emic approach to research. By thus "plunging into the sea, rather than fishing from the shore" (Tsui, 2006), researchers can ensure that they identify relevant issues in the local context and subsequently develop new, or adapt existing, theory relevant to organisations in that context. Potentially, these new or adapted theories would also have wider relevance – if nothing else, they should stimulate reflection by researchers outside the immediate context.

However, such an approach is not entirely straightforward. It requires a degree of familiarity with the context as well as the social and linguistic norms that apply there. This is not an insurmountable hurdle and researchers in these contexts are certainly qualified to undertake such work – if they choose to do so. India and China both have huge diasporas, including many academics. Many of these scholars do possess the requisite skills to "plunge into the sea, rather than fish from the land", but there needs to be an incentive for them to do so, i.e. journals (and their readers, reviewers and editors) need to have an in-practice (not just espoused) appreciation for such work.

A second approach that we can draw upon to examine some of the issues Lee raises concerns 'the idols of the theatre' (idola theatri) (Bacon, 1620). Bacon asserted that as a result of linguistic dexterity and sophistry, so some beliefs or arguments can become dogmatic and unquestionable. These dogmatic beliefs have an entropic effect (cf. Zamyatin, 1921/1972) on human reasoning and act to obfuscate the further development of research and knowledge. With respect to Lee's (2010) arguments, this form of dogma is related to the use of terms that are 'taken-for-granted and unreflected-upon'. This perspective suggests that we should not take anything for granted but reflect very carefully on both the key terms and norms in our field, as well as the findings of others. For instance, instead of blindly assuming that prior western findings (theory, prescriptions, etc.) apply in non-western contexts, we should ensure that local contextual features are explicitly reflected in our research designs as we attempt to explain and understand local phenomena. Further, terms like 'rigour' should be vigorously deconstructed. Too many IS researchers appear to believe that rigour can only be achieved in positivist and quantitative research designs. However, as Benbasat and Zmud (1999) observe, rigour simply requires "the correct use of methods and analyses appropriate to the tasks at hand".

Lee's (2010) essay has provided me with a thought-provoking set of arguments and led me to question my own espoused and in-use theories, in particular the gap that sometimes lies between them. The two approaches that I suggest above occurred to me as I read Lee's essay and each of them deserves further reflection. Tsui's (2006) call for more emic research is entirely actionable and neatly support's Lee's arguments that we should seek out the contextual richness of organisations where we conduct research in order to glean new theoretical and organisational findings. Where Bacon's (1620) ideas are concerned (and there are many others apart from the Idols of the Theatre), it is critical that we do not simply accept the received truths of others, lest they acquire hegemonic status. Instead, we must challenge them vigorously and seek to ensure that our theories-in-use are both appropriate to the context that we are studying and are clearly expressed.

Realising fruitful outcomes to these many challenges will be by no means a simple task. It will require our best minds and intentions. We all have a role to play – as authors, reviewers, editors and supervisors. As Machiavelli (1532/2004) demonstrated, attempts to change institutional structures that privilege power and authority tend to produce harsh distinctions between those in favour of change and those against. The same may occur in our own field, but we should not be afraid of change. Lee's (2010) essay has provided us with a manifesto for change. The challenge is for us to discuss, reflect and take action.

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