Reflections on interdisciplinary accounting research: the state of the art of intellectual capital

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Abstract

Purpose – The purpose of this paper is to offer reflections and critique not only on the current state of the art for intellectual capital research (ICR) from an interdisciplinary accounting research (IAR) perspective, but also its future directions.

Design/methodology/approach – This paper offers a critical reflection based on the author's observations as an IC researcher, reviewer and editor. The author also supports the arguments with some evidence from the research about IC research.

Findings – The author argues that most ICR is falling short of achieving "the most advanced level of knowledge and technology" of the art because it inherits flaws from prior research, thus threatening its legitimacy and impact.

Research limitations/implications – The author argues that researchers need to go back to the methodological drawing board when designing IAR so future research can achieve its full potential. To do so researchers also need their research to be transformational to engender change, and to be transdisciplinary, which encompasses research beyond the current boundaries of accounting and management.

Originality/value – The author identifies and introduces three research shortcuts that prevent ICR projects from being state of the art being copycat, Furphy and technophobic research which provide insights into why not all ICR research is not "state of the art".

Keywords Reliability, Technology, Knowledge, Validity, Interdisciplinary accounting research, State of the art

Paper type General review

Introduction

This aim of this paper is to offer my reflections, critique and future directions on the state of the art of intellectual capital research (ICR) as part of the interdisciplinary accounting research (IAR) project. If we consider "state of the art" as "the most advanced level of knowledge and technology currently achieved in any field at any given time"[1], then what is the state of the art IC research? I argue that most IC research is falling short of achieving "the most advanced level of knowledge and technology" of the art because it inherits flaws from prior research, thus threatening its legitimacy and impact. I identify three research shortcuts that are preventing ICR projects from being state of the art. As a result, I argue that researchers need to go

The author especially wants to thank the *AAAJ* Editors for their continued support towards developing leading edge thinking and critique about interdisciplinary accounting research and how it develops in the future. It is thorough constructive critical thinking based on open and honest critique that allows knowledge to be transformed. Thanks also to the author's friends (especially the non-accountants) and colleagues for their critique on the paper as it developed. Also, thanks to Fiona Crawford from the Editorial Collective for sterling editorial efforts.

Accounting, Auditing & Accountability Journal
Vol. 27 No. 8, 2014
pp. 1257-1264

0951-3574
DOI 10.1108/AAAJ-05-2014-1714

back to the methodological drawing board when designing IAR so future research can achieve its full potential (see, Parker and Roffey, 1997, p. 212).

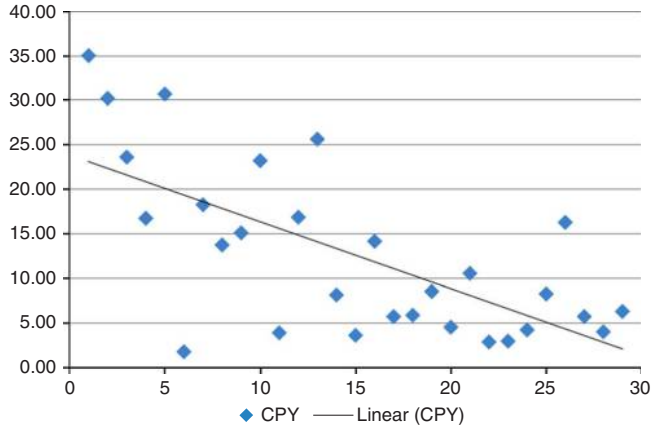
According to Roslender and Dillard (2003, p. 327), the contemporary IAR project began in the 1970s at the University of Sheffield as a critical accounting project in "reaction to the positivist and functionalist orientations" that underpinned mainstream accounting research at the time. Later, IAR emerged with an emancipatory focus that "entails viewing accounting through the lens of another discipline" such as the behavioural and social sciences (Roslender and Dillard, 2003, p. 327). Similarly, the ICR project began in the 1980s to help explain how organisations manage, measure and report knowledge and intangibles, by developing "multi-disciplinary contributions using different perspectives, including sociology, psychology and economics" (Guthrie *et al.*, 2012, p. 70). Currently, some state of the art ICR is taking a performative turn, characterised by research that takes "a critical examination of IC in practice" (Guthrie *et al.*, 2012, p. 76).

If IC researchers are advancing ICR as a state of the art discipline, then I would expect researchers to continuously critically examine, improve and develop IC knowledge, research methods and technologies. But my observations as an IC researcher, reviewer and editor are that many IC researchers do not achieve this because the pressure to "publish or perish" (see, de Villiers and Dumay, 2013, p. 876), causes researchers to take methodological shortcuts. Therefore, to advance my arguments the remainder of the paper is organised as follows. The next three sections outline the methodological shortcuts being "Copycat research", "Furphy research" and "Technophobic research". Next, I present a discussion section where I argue that most IC researchers need to return to the methodological drawing board before contemporary ICR can continue to be state of the art. A concluding section then offers lessons learned as future directions from my critical reflections of ICR.

Copycat research

The first shortcut is the number of research projects that are a variation on a previous study, which I call copycat research. Copycat research does not truly advance knowledge because the results of these studies are incremental at best (Alvesson and Deetz, 2000, p. 50). For example, in ICR, two popular research methodologies are the value added intellectual capital (VAIC™) model to determine the link between IC and economic value creation, and content analysis (CA) for understanding IC disclosure (see, Dumay, 2014, p. 14). When VAIC™ (Pulic, 2000) and CA (Guthrie and Petty, 2000) were first introduced into IC research a decade and a half ago, they were important advances because they provided the ability to uncover new knowledge by addressing previously unanswered questions. When introduced these methodologies represented state of the art IC research because they represented the latest advances in developing new IC knowledge (see, Iazzolino and Laise, 2013; Dumay and Cai, 2014).

With all research, replicating the studies in different settings to test the reliability and validity of the findings is useful and valuable, and this can take some time. For example, it took over three decades of replicated research for smokers and physicians to accept fully the link between smoking and lung cancer (Witschi, 2001, p. 4). However, continually replicating and researching the same phenomena in a different setting lessens the value of the research over time so that the impact of the research begins to decline rapidly after leading journals publish the first few seminal studies (de Villiers and Dumay, forthcoming). For example, as demonstrated in Figure 1, my research shows how the average citations per year[2] of CA research published in the *Journal of Intellectual Capital* (*JIC*) has continually declined since Guthrie and Petty's (2000) original "state of the art" article (Dumay, 2014, p. 14).



Source: Based on Dumay (2014, p. 14) and updated 16 June 2014

Figure 1.
CPY of CA research published in the *JIC* (2000-2010)

Additionally, my further research determined that the majority of these studies do not make any significant new findings (Dumay and Cai, 2014). These contemporary CA research articles cannot fit the description of state of the art research because they neither develop new and innovative research methods nor significant new knowledge.

Furphy research

The second shortcut relates to researchers being lazy or inconsiderate of data reliability in their research, which I label "Furphy research". The term Furphy comes from the name of Australian manufactured water carts used extensively in Europe and the Middle East to carry water to Australian and Allied troops in First World War. The cart drivers, who went from camp to camp, were a dubious source of information and gossip for the soldiers. Because some of the Furphy drivers' news was unreliable, the term Furphy became synonymous with misleading information or rumour (Barnes and Furphy, 1998).

Unfortunately, I encounter Furphy research on a regular basis as outlined in the following two examples. First, in an article I recently reviewed, one researcher claimed to have obtained data detailing the educational level (secondary or lower, graduate or postgraduate) of all employees for each sampled company (specific number undisclosed) for a 12 year period from "the prospectus and annual reports of corporations". However, as an experienced IC researcher, I have rarely come across these measures in internal documents, let alone publicly available information (see, Systematic, 2004, p. 27 for an exception. However, this is contained in an IC statement and uses four different categories). I was immediately suspicious about the reliability of the data because I find it hard to believe such detailed and consistent educational level data is available in these three specific categories and, if it is, that each company measures it in exactly the same way. If the data cannot be trusted, the research is fatally flawed.

Second, I reviewed a conference paper where the researchers performed a longitudinal CA of annual reports from 1980 to 2010 looking for the extent of IC disclosures of a bank in a developing nation. Here I classify the annual reports appearing prior to 2000, and especially prior to 1994, as a Furphy data source, because the term "IC" did not gain any notoriety until 1994, when in the USA Stewart and Losee

(1994) published an article about IC in *Fortune* magazine. The article was followed three years later by Stewart's (1997) seminal IC book. Similarly in Sweden, Leif Edvinsson at Skandia (1994) developed and published the world's first IC statement and continued to do so until 2000 (see Dumay, 2012). IC continued to gain interest in Scandinavia and Europe with the start of the Danish IC Statement and MERITUM projects in 1998 and the advent of the 1999 OECD IC conference in Amsterdam. The year 2000 saw the first edition of the *JIC*. Hence, IC was on the map in the USA and Europe by 2000.

However, I can find no evidence that IC was of concern to any developing nations prior to 2000. For example, it was not until Abeysekera and Guthrie (2005) published their research on IC disclosure in Sri Lanka that any researchers considered IC disclosure in a developing nation. They found "not a single annual report has explicitly made reference to the term "IC" (Abeysekera and Guthrie, 2005, p. 161). Also, Bontis (2003, p. 9) draws a similar conclusion for Canada, a developed nation. Therefore, why would any researcher expect to find any intentional IC disclosures in annual reports issued by companies operating in a developing nation prior to 2000? Unsurprisingly, implied IC disclosure levels discovered up to 2000 are almost non-existent. So while it might seem logical to research IC disclosures over this period in the name of longitudinal research, a researcher draws a long bow in claiming that these annual reports might contain IC disclosures when the evidence proves otherwise.

Technophobic research

The third shortcut stems from my observation that many IC researchers are avoiding the use of leading edge technologies to achieve "state of the art" status. By technology, I refer to tools such as analytical processes and software applied to reliably analyse data. However, in contrast to our positivist research cousins who utilise the latest versions of SPSS, Minitab and AMOS software to reliably crunch their numbers, there seems to be reluctance by most IAR researchers to utilise technology to reliably analyse the growing availability and volume of qualitative data. Thus, I classify this as "technophobic research".

Again, drawing on my recent analysis of IC articles, I found few articles reporting the use of specialist CA software programs and none, with the exception of Bontis (2003) who analysed more than 10,000 annual reports, that relied on software to analyse large data volumes. As a result, the majority of studies analysed relatively small samples of data from which researcher can hardly generalise the results.

Similarly, I randomly examined 12 qualitative IAR studies from *Accounting, Auditing and Accountability Journal* (*AAAJ*) published in 2013 and found only four used specialist qualitative data analysis software (nVivo) and none of these relied on automated data coding. While researchers can argue that qualitative data analysis is a subjective endeavour, the absence of applying any "state of the art" software to current research is worrying as I cannot see this as developing ICR or IAR for two reasons.

First, most researchers do not use the appropriate and available tools and technology to ensure the reliability of their analysis. For example, any researcher who codes qualitative data leaves themselves open to criticism if they cannot demonstrate the reliability of their coding. When coding qualitative data, which is in reality CA, researchers advocate various data reliability checks. However, there is only one process that is specifically designed to determine inter-coder reliability, being Krippendorff's α (Krippendorff, 2013, pp. 277-309). However, as my analysis of CA reveals, few CA researchers use this technology (Dumay and Cai, 2014) and according to Krippendorff

(2013) the problem spans multiple disciplines with relatively few CA studies employing alpha as a reliability check. Imagine a positivist researcher trying to present findings without a *p*-value to demonstrate reliability?

Second, because researchers continue to use small sample sizes they constantly expose this as a limitation and cause concerns about validity in most qualitative studies. Therefore, ICR and IAR is open to criticism from our positivist cousins who cannot see how researchers can generalise and validate their findings. As Boiral (2013, p. 1064) openly admits, his "study examined only 23 sustainability reports from two sectors. Larger studies, examining more reports from diverse sectors, would make it possible to validate our main findings". Software is available that can handle analysing greater volumes of data, but researchers are either afraid to use it or are not aware that it exists because they persist in copying the methodology of previous studies.

A further search of *AAAJ* found only one example where the researchers employ state of the art software (Leximancer[3]) to analyse qualitative data thus avoiding the subjective and labour intensive aspects of manual data coding (Lodhia and Martin, 2011, p. 132). A search of the *JIC* has similar results finding only research by Benevene and Cortini (2010) using software (T-Lab[4]) to analyse semi-structured interview data. Using state of the art software also helps resolve the reliability issue because different researchers can repeatedly use the same data and analysis processes and uncover similar results. However, the subjectivity in interpreting the results remains an issue, as it does for our positivist research cousins.

Going back to the methodological drawing board

When critically reflecting on current ICR it reminds me of the children's game of whispers or telephone, where the sender whispers a message to another child and then another until the message returns to the sender. Often the message differs significantly (and hilariously) from the original because of noise, misunderstanding or even a deliberately changed message. Unfortunately, the same game contributes to the shortcuts I have identified above. For example, most researchers continue to use annual reports as a data source when conducting ICR because other researchers have done the same, without even critiquing their validity as a data source for state of the art ICR (see, Dumay and Cai, 2014, pp. 267-268). Over the last two decades companies have used additional communication channels to engage with and disclose information to stakeholders, especially via the internet and corporate websites. Therefore, just because another researcher used annual reports as a data source their future use in a research project cannot be justified unless the researcher(s) can argue convincingly why it is the best data source for revealing new IC knowledge.

I suggest a return to the methodological drawing board. Instead of playing a game of Research Whispers, researchers need to ensure data reliability. Also they must apply rigourous research design and methods to ensure reliability and validity. While it is possible that too much "rigour perhaps lead[s] to rigor mortis" (Otley, 2003, p. 324), basic reliability and validity foundations need to be established when conducting research.

Moreover, researchers need to critically rethink methodologies for conducting research, especially in light of changing technologies. I recently had the pleasure of attending a research seminar where the presenter introduced the audience to a variation on ethnography called netnography (see, Kozinets, 2010), whereby the researcher became a participant observer in the virtual world of the internet (Strong, 2014). To my knowledge, the research presented is one of the first to apply netnography as a methodology and is consistent with what I consider state of the art in

ICR or IAR (see also, Miley and Read, 2012). I urge researchers to build upon solid foundations while also adapting the most relevant data sources, technologies and research methodologies available, to ensure state of the art ICR and IAR.

Conclusion: lesson learned as future opportunities

Having the opportunity to reflect on the state of the art of ICR allows me to become a critically reflective learner (see, Brookfield, 1995). As such I want to offer insights into two lessons I have learned during the writing this and other articles critical of ICR (see, Guthrie *et al.*, 2012; Dumay and Garanina, 2013; Dumay, 2014; Dumay and Cai, 2014, forthcoming). Lesson one is to ensure ICR is transformational which means researchers need to ensure they engender change in their research projects and not just blindly repeat previous studies. For example, Guthrie *et al.* (2012) advocate research examining IC in practice which takes a performative perspective and requires researchers to change their approach by getting their "hands dirty" inside organisations working with IC, as opposed to examining whether or not organisations report on IC. The former is action and/or interventionist research allowing researchers to interact with rather than observe IC in practice (see, Dumay, 2010; Chiucchi, 2013a, b). The latter provides opportunities for "emerging scholars [...] because the methods are well established" while "for all researchers, the method is not overly time consuming, mainly consisting of desk-bound activities, with relatively easy access to materials" (Guthrie, 2014, p. 291). However, continuously using these methods as shortcuts without major changes to the data sources, research methods or technology leads to research which adds little more than incremental insights and has little impact on future research and practice (Dumay and Cai, 2014).

Lesson two is to extend IC beyond the boundaries of IAR and embrace the concept of transdisciplinary research which encourages research beyond the functional silos of IC as an accounting or management discipline (see, Tingey-Holyoak and Burritt, 2012; Dumay, 2014). Therefore, "we need to blur the artificial boundaries between accounting and management and to include other disciplines such as psychology, information technology, sociology, the natural and built and environment, engineering, manufacturing, to name just a few" (Dumay, 2014, p. 21). If IC researchers continue to take methodological shortcuts, as outlined in this paper, then there will be little chance that state of the art research in IC will thrive and IC may "be seen as merely one more set of very interesting ideas that is continuingly elusive to grasp and use" (Chatzkel, 2004, p. 337).

Notes

1. Based on Encarta online dictionary using MS Word.
2. Based on Google Scholar as at 16 June 2014.
3. See www.leximancer.com/
4. See http://tlab.it/en/presentation.php

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