

Personal reflections on the relevance of accounting research

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JOURNAL OF PRAGMATIC CONSTRUCTIVISM

Personal Reflections on the Relevance of Accounting Research

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1. Introduction

In recent years, questions have been raised about the relevance of accounting (especially management accounting) research; see, for example, Malmi and Granlund (2009); Parker et al. (2011); ter Bogt and van Helden (2012); Modell (2014); Tucker and Parker (2014). In this chapter, I would like to provide a personal perspective on these questions by reflecting on my career as an accounting researcher, which began more than 50 years ago. Over those years, accounting research has come a long way and changed very substantially, and this is in the context of fundamental changes in the university environment over that period. The topic of relevance has arisen in different guises at different times over those years. For instance, in the 1980s there were discussions about the ‘gap between theory and practice’. More recently, there have been criticisms that (particularly management) accounting research has followed practice, rather than leading it. In other words, the research itself has had little impact on practice. Today, when there are discussions about the impact of research papers in international journals, they usually relate to impact factors which are based on citations in other research journals, rather than the broader impact of the research on accounting practice and society more generally.

These debates about accounting research need to be seen in the context of wider debates, in many countries, about the role of universities in society, and especially about the value of the research done in universities. In the current age of austerity, with governments increasingly questioning the societal value of research, I wonder how we would demonstrate the value of accounting research, especially in view of the apparent gap between research and practice (Parker et al., 2011), and criticisms such as the tendency for accounting researchers to see theorisation as an end in itself, rather than as a means to an end (Lukka et al., 2022), and claims that accounting researchers are elitist, use pretentious language, and are not concerned about improving practice (Tilt, 2010). However, to address such issues we need to explore the meaning of relevance for accounting research, and how accounting research can be relevant for accounting practice and society more generally. Over the past 50 years, I have witnessed some fundamental changes in accounting research, from the economic-based normative modelling in the 1970s, to the theorised empirical understandings of accounting practices, drawing on a wide range of both economic and social theories, today. However, in my opinion accounting research, particularly interpretive accounting research (my own approach), despite producing many excellent papers, has tended to *follow* practice, rather than *lead* it. This is not to suggest that the research has not had any effect on practice. I fully recognise that accounting theory can be performative and that there are various studies of the performativity of management accounting (for a review see Vosselman, 2014). My concern is that accounting researchers, despite these recent performativity studies, have not given sufficient consideration to how their theorised explanations of accounting practice could be used to make a difference in accounting practice and in society more generally.

In this chapter I will first describe this wider context and outline some of the questions which have been raised about the impact of universities on society generally. I will then briefly outline my personal perspective on how accounting research and, in particular, management accounting research, has developed over the past 50 years. Then I will discuss the contribution which this research has made, before discussing in more detail the meaning of relevance and how accounting researchers might seek relevance. I will then turn my attention to issues relating to the focus of (management) accounting papers in the top academic journals and to the way in which universities’ performance criteria are motivating particular types of research. I will then discuss some of the questions which this raises, before finally reflecting on my experience as a management accounting researcher and as the editor of *Management Accounting Research*.

2. The broader context

In 2017, when I was about to retire from my position in the University of Groningen, I selected as the theme for my retirement conference, the topic of **relevance** in *Researching Management Accounting and Control*. I had previously given some seminars on the relevance of management accounting research which I believe is an important topic, especially for someone like me whose academic career of over 50 years has largely been spent researching management accounting. As I will explain in more detail in subsequent sections of this chapter, I felt (and still feel) that management accounting researchers have achieved a lot over those 50 years, and we now have much deeper and highly theorised understandings of management accounting practices. But the question I keep coming back to is: *So What?* In other words, what do these understandings and theories enable us (and/or others) to do? In general, as researchers, we have been seeking to understand accounting practice – but to what extent has our research shaped that practice? I find this a difficult question to answer. Before reflecting on that particular question, I would like to set out some of the questions which have been raised recently in the context of university research more generally.

In preparing my conference presentation, I reviewed some of the, then, current literature about the role of universities and scientific research in modern society. I found it interesting to reflect on the relevance of management accounting research in the context of these broader concerns about scientific research more generally. I was particularly struck by the ‘science in transition’ initiative in the Netherlands¹, which had been initiated in 2013 and was raising questions about such issues as: trust in science, quality of research, fraud and deceit, and communication with wider society. This initiative joined a growing ‘worldwide chorus’ of voices debating how to assess research quality and questioning the increasing use of metrics to indicate/evaluate the quality of scientific research, with comments such as science should strive for “impact, not impact factors” (Verma, 2015). This quote was in an editorial in the *Proceedings of the National Academy of Sciences of the United States of America* which concluded:

As arbiters of the importance and merit of publications, the scientific community must not rely exclusively on the impact factors of journals, whose acceptance criteria can be based on an array of considerations, including trends and subject areas (Verma, 2015, p.7875).

It is interesting to note that the use of such metrics has become widespread in universities in many countries across the world in recent decades. This probably reflects the quantification of targets and outcomes which have become endemic with the spread of new public management (NPM) in the public sector of most countries in the Western world, and beyond. As my Manchester colleague, Sven Modell, reminded me, NPM is an example of an accounting discourse, with performance targets and performance measures, which has been tremendously performative, and not always with the intended consequences. It is interesting that this discourse, which has its origins in part in academic research, is now being applied with worrying consequences to academia itself.

Concerns were also expressed in the Science in Transition Initiative about the public image of science and the trust the general public has in scientific research. Although surveys seemed to indicate that there is rather greater trust in science than in politics and, generally, trust in sciences is high, there have been some worrying developments. For instance, US President Trump publicly challenged the role of scientific experts over issues of climate change; claiming that the evidence is mixed, when just a few sceptics take a view opposite to the overwhelming scientific opinion. This questioning of scientific opinion has also been reflected in political debates about such things as GM food and fracking in the UK. Furthermore, despite the ‘quality controls’ embedded in the peer review system, there have been reports in the media which raise serious questions about the extent to which the peer review system guarantees the quality of published scientific research. Although major social scientific frauds, such as those of anthropologist Mart Bax² and social psychologist Diederik Stapel³ in the Netherlands, and the more recent medical research frauds reported by Boetto et al. (2020), where papers published in the *New England Journal of Medicine* and the *Lancet* were retracted just a few weeks later, may be exceptions, “‘sloppy science’ or even ‘bad science’ is much more frequent”⁴, as indicated by an earlier BMJ survey (see Tavare, 2012).

In accounting we have had the case of US professor James E Hunton who retracted 37 articles between 2012 and

¹ <http://www.scienceintransition.nl/English>

² <https://retractionwatch.com/2013/09/23/dutch-anthropologist-mart-bax-faked-61-papers-says-university/>

³ <https://www.apa.org/science/about/psa/2011/12/diederik-stapel> or <https://www.insidehighered.com/news/2011/11/28/scholars-analyze-case-massive-research-fraud>

⁴ Science in Transition Position Paper – October 17, 2013, p.18: see <https://scienceintransition.nl/en/about-science-in-transition/position-paper>

2016 because they contained mis-stated or fabricated data⁵ (see Horton et al., 2020 for an analysis of that case). Furthermore, in business and management journals, Tourish and Craig (2020) identified 131 retractions for a variety of reasons, including data fraud, plagiarism, research ethics and conflicts of interest, while Craig et al. (2020) identified 160 retractions in psychology and Cox et al. (2018) identified 55 in economics. These studies would seem to suggest the major scandals (such as Bax, Stapel and Hunton) may not be isolated incidents—a rather worrying thought.

In addition, concerns have also been raised about the way in which scientific research is communicated to the general public. In recent years there have been some high-profile appointments of professors tasked with promoting the public understanding of science. In the UK we have, for instance, Marcus du Sautoy, who is the Simonyi Professor for the Public Understanding of Science at the University of Oxford and Brian Cox who is the Royal Society Professor for the Public Engagement in Science. These appointments clearly demonstrate a desire to improve communications with the general public. Furthermore, the recent Covid pandemic may have given a boost to the public perception of scientific research. In part, this is due to the way in which politicians in various countries argued that they were ‘following the science’ in their management of the pandemic. However, whether the politicians did trust the science, or whether this was a way of deflecting the ‘blame’ for the restrictions, is not clear. Nevertheless, according to the Wellcome Global Monitor 2020: Covid-19 Report⁶ it seems the pandemic has boosted trust in science and scientists.

In this broader context, it is interesting to think about the extent to which accounting researchers are putting the findings of their research into the public domain. How many of us write research papers for international research journals, but make no attempt to communicate our findings to accounting practitioners and/or wider society? I will return to this issue later.

Another broader contextual issue is the relationship between education and research. The traditional view of universities, when I began my academic career, was that education should be research led. Leaving aside what we mean by being research led, an interesting question is whether this is possible now, given the very large classes we have in many universities and the extent to which undergraduate courses are textbook-based, rather than based on research published in academic journals. This has come about as we have moved from what was, in my early years as an academic, an elite education system to the mass education system we have today. Without getting into a debate about the merits of the respective systems, it is clear that this move has had consequences for the nature and content of university education (for example, see Gebreiter, in press).

In preparing for my retirement lecture, I Googled ‘evaluating the societal relevance of research’, and this raised interesting issues concerning the use of metrics, including university rankings and journal impact factors, as well as citation indices and impact factors for individual research papers. The increasing use of such metrics seems to have been driven by the spread of new public management and the accompanying quantification of performance targets and outcomes. I found an interesting thesis in Groningen’s database (Wilbertz, 2013) which described research as an investment, and went on to note that a consequence is that the social impact and relevance of research can be easily confused with economic success, and that it is difficult to attribute social impact to individual pieces of research. In most instances it is the corpus of work that has impact, rather than individual papers. Admittedly, there are notable exceptions, such as Nobel prize-winning work, although Nobel prize winners are usually the first to acknowledge that the prize-winning research was the result of the work of large teams of researchers.

Assessing the impact of university research, whether social or economic, has become an important issue in recent years, especially as governments are funding universities in an age of austerity. I recall Vince Cable, who was the UK Secretary of State for Business, Innovation and Skills from 2010 to 2015, and had responsibility for universities, making the comment that ‘we need to do more with less’. A consequence seems to be that areas, such as scientific and medical research, where it is somewhat easier to value the economic benefits of research, have an advantage, while the humanities and social sciences are disadvantaged. Furthermore, there have been instances where politicians, with their own personal political agendas, have targeted specific areas of research. We have witnessed attempts in the US to cut funding for political science, while in Japan there have been huge cuts in the humanities and the social sciences more generally. In the context of such attacks on social science, questions could be raised about who should be funding accounting research. Accounting research could be seen by politicians and the general public as potentially benefitting accounting firms and their clients, including major corporations, and so some might reasonably ask whether they should be funding that research. Some accounting research is funded by professional accounting bodies, and some by businesses and accounting firms, but it is relatively small in relation to the total cost of accounting research in universities. Furthermore, such third-party funding tends to focus on short term economic success, rather than the social impact of the research. So, it is quite possible that in the coming years questions will be increasingly asked about the societal benefits derived from the public funding of university research in areas such as accounting.

In view of this broader context, what should we be doing as accounting researchers? I will discuss this in the subsequent sections. In the meantime, it is probably worth noting that a general problem at the present time is that

⁵ <https://retractionwatch.com/category/by-author/james-hunton/>

⁶ <https://wellcome.org/reports/wellcome-global-monitor-covid-19/2020>

senior university administrators (or maybe we should now call them the university senior management teams), driven by the way in which university research is evaluated, tend to emphasise publications in (what in the UK are called) four-star journals and tend to marginalise other forms of publication. We need to do what we can as accounting researchers to change such perceptions in our own universities. I accept that this is probably a task for those who have more senior positions in universities. But they will only be able to do this if we are, ourselves, interacting with practice and society more generally; not just PR type engagement, but by making our research accessible and relevant beyond the University. I find it instructive to think back to the 1970s when I was expected to write papers for practitioners, as well as papers in research journals. I tended to write a research paper for an academic journal and then have at least one paper from that research for a practitioner journal. This is probably a good point for me to outline the changes I have witnessed over the past 50 years.

3. A brief history

In 1970, I received a fellowship from the Institute of Chartered Accountants in England and Wales to enable me to study for a master's degree in the University of Manchester. This was part of a programme which was designed to encourage professionally qualified chartered accountants to teach accounting in universities, thereby making it possible for accounting to become a university-based profession. At that time, in England and Wales, a university degree was not a requirement for professional accounting training and, consequently, accounting was not seen as a university-based discipline. With a few notable exceptions, where accounting was taught in universities, it was taught as part of an economics or business degree, and there was very little accounting research. One important exception was the London School of Economics, where there had been accounting research for several decades, largely from an economic perspective.

However, in the 1970s accounting began to develop as a university discipline, with expanding numbers of undergraduate students and an increasing emphasis on accounting research. The research, at that time, tended to adopt a normative approach, using quantitative economic models to prescribe how accounting should be done. In my own research, I wanted to identify the economically 'optimal' way of dealing with inflation in financial accounting reports. At the time we (accounting researchers) had a rather arrogant attitude, believing we had the 'right' (economic) theories which would show practitioners what they should do, and they (practitioners) would eventually learn and use the optimal practices. This was one of the reasons why I wrote articles for professional accounting journals, as well as papers in research journals. Furthermore, through research-led teaching we could educate our students who would then spread the new ideas and optimal practices as they joined the accounting profession.

However, in the early 1980s it began to be recognised that there was a 'gap between the theory and practice' – i.e., between the theories we were prescribing in universities and what practitioners were doing in practice (for reviews at that time see Cooper et al, 1983; Scapens et al., 1984). Furthermore, it was acknowledged that, as researchers, we had limited knowledge of practice, especially in the field of management accounting. This encouraged some researchers to undertake surveys and case studies to try to understand practice (see Scapens et al. 1987), but more generally accounting researchers tended to adopt quantitative methods, underpinned largely by contingency theory and neo-classical economics. But as the 1980s progressed, some researchers moved away from economics and contingency theory and, instead, drew on various social and/or organisational approaches to question the traditional mainstream economics and contingency approaches. So, as we entered the 1990s, there was considerable diversity in the social and organisational theories being used, but most questioned the neutrality of accounting numbers, which had been assumed in the previous economic approaches. Nevertheless, both economic theory and, to a lesser extent, contingency theory continued to be used in quantitative work. However, this was alongside the increasing interpretive and critical approaches, which widened the range of methodologies used in the research.

As such, accounting research became more theoretically and methodologically diverse, as it drew on an increasingly wide range of theories from the more traditional university disciplines, such as sociology and political science, as well as organisation theory and economics. To some extent, this helped to legitimate accounting research as a university subject. In Manchester, for instance, in the late 1980s the Accounting and Finance Department received a higher rating in the national Research Assessment Exercise (RAE) than the Economics Department, which substantially enhanced the reputation of accounting research within the University.

The theoretical and methodological diversity in accounting research provided rich understandings of practice, based on theorised explanations of accounting practices. This represented a fundamental shift away from the normative research of the 1970s to the explanatory empirical research of the 1990s and beyond. Such research comprised, on the one hand, the positive accounting theory, often associated with Watts and Zimmerman (1986), which emerged as a reaction against the earlier normative tradition, but continued to rely primarily on economics and, on the other hand, the emerging accounting research, which was drawing on organisational and social theories and was the type of research I was undertaking at that time. As normative research came to be seen quite negatively, this explanatory empirical

research, particularly the research using organisational and social theories, tended to closely follow practice, with researchers using theory to explain and make sense of their findings, rather than providing prescriptions for practitioners, policymakers, and others. As such, one might question whether this research has had any real impact on practice. It has provided theorised explanations of accounting practices, but rarely has it developed new knowledge or techniques which have had a direct impact on practice.

It has been researchers, like Robert Kaplan and his colleagues, who promoted new management accounting techniques, like the balanced scorecard and activity-based costing, that have had the most impact on practice. In a book with H Thomas Johnson (Johnson and Kaplan, 1987), they claimed that management accounting had lost its relevance, and that new techniques were needed for the modern business environment. However, such researchers have sometimes been criticised for being consultants rather than researchers. However, their work seems to have had more impact on practice, than the theorised empirical research of the management accounting researchers who are primarily concerned with publication in four-star research journals. This raises the question of whether accounting researchers can, or should have, an impact on practice. I do not just mean in developing ‘fashionable’ new techniques, like the balanced scorecard and activity-based costing, but in establishing the relevance of their theoretical explanations for practitioners and others. As I have argued elsewhere, Scapens (2008), while the relevance of accounting research could be expressed in terms of relevance for accounting practitioners, it could also be concerned with the social and political implications of accounting in modern organisations. I will come to this question below, but first I will complete this Brief History.

As we moved into the 21st Century questions were raised about what was then termed the research/practice gap (see, for instance, Malmi and Granlund, 2009). In a AAAj Editorial, Parker et al. (2011) noted that there “had been a flurry of recent special journal issues, editors’ forums and papers on this apparent research/practice gap in accounting” (p.6). While Malmi and Granlund (2009) focused primarily on quantitative management accounting research, Parker et al. (2011) were concerned with management accounting research more generally, recognising that accounting research “needs to be socially, politically and institutionally contextualised, theoretically informed and embracing interdisciplinarity” (p.9). However, both emphasised that accounting is an applied discipline, and argued that accounting theories should be useful for practitioners and its ultimate purpose should be to improve practice, rather than simply describe, understand or critique it. However, I recognise that some, especially critical theorists, but also many interpretive researchers, might have broader purposes in mind. Nevertheless, Parker et al. point to the need to consider the relevance of accounting research, and the need to engage with practitioners and others in society.

Parker et al. (2011, p.7) capture what Tilt (2010) calls the “schism” between the interests of researchers and the interests of practitioners in the following: “academics are considered elitist as they speak with their own jargon.... the aim of the game is to publish..., not to disseminate knowledge or improve practice” and “practitioners often regard jargon as being pretentious”. However, they also refer to the report of the AAA Research Impact Task Force (Meohrle et al., 2009) which argued that accounting research in the US has had more impact than is sometimes acknowledged, and to a paper by Singleton-Green (2010) of the Institute of Chartered Accountants in England and Wales which described the Institute’s Information for Better Markets programme which has had the aim of narrowing the gap between researchers and practitioners. While these two initiatives relate more to financial accounting than management accounting, I am more concerned with the latter, as this is where I have spent much of my career. In this context, I found the findings of ter Bogt and van Helden (2012) really interesting. They sought “the opinions of a number of editors of accounting journals on the value they attached to the practical relevance of management accounting research”. They concluded that most of these editors see practical relevance and theoretical advancement as complementary, but theoretical relevance is given much more weight as it is regarded as central to academic research. Furthermore, they noted that “the editor of *MAR* [i.e., me] seems to be the only one who observes that many researchers mainly follow practice without presenting the implications of their work for practice”. Although I was in a minority of one in that sample, I would still argue that this is the case.

In another recent survey/questionnaire study of the research-practice gap, Tucker and Parker (2014) identified two broad schools of thought: one which holds that the gap is widening and a second which argues that efforts to bridge the gap are unnecessary, untenable or irrelevant. Tucker and Parker concluded that the notion of a research-practice gap is an oversimplification which draws attention away from the broader and more fundamental question of the social relevance of management accounting research. In a somewhat similar way, ter Bogt and van Helden (2012) argued that the concept of practical relevance is ambiguous and seems to imply different things to the editors of the various academic accounting journals. So, if we want to make (management) accounting theories relevant and accessible, we need to think about what we mean by relevance and for whom. I will turn to these issues next.

4. The meaning of relevance

From a managerialist perspective, we might ask such questions as: what methods should be used; how should they be applied; in what circumstances; and how should management accounting change? (See Malmi and Granlund

2009). Although such questions may be in the minds of researchers who adopt a managerialist perspective, and there may be some discussion of these questions at the end of a paper, the main focus of the research usually tends to be on explaining what *is being* done, rather than what *could be* or *should be* done. It is implied that theorised explanations can help to address prescriptive questions, but we do not see many managerialist research papers directly addressing such questions. Similarly, interpretive researchers seek to make sense of, and theorise, practice by empirically studying what practitioners do. It could be argued that such research is studying the practical relevance of existing management accounting practices and so they are looking at issues of relevance, but nevertheless the research is *following*, rather than *leading*, practice. In contrast, however, critical researchers seek to challenge existing practices and give voice to the marginalised through critiquing the practices of modern capitalism. This is clearly socially relevant, but again how much of this work actually informs social debates and how many of the theorised papers we see in academic journals are actually written, or translated into, language which makes these papers accessible to those who are able to make a difference, such as accounting practitioners, regulators, government, lobbyists and civil society organisations?

If we go back to early history, the classical Greeks distinguished between academic research and practical problems, which is the root of modern universities, while the classical Chinese combined academic research and practical problem solving, and the academics were also the administrators.⁷ Some argue that the latter damaged academic creativity, whereas the former has led to a gap between theoretical knowledge and practice. Today, there are debates about the gap between research and practice (as I mentioned in the previous section), which could be characterised as a gap between theoretical knowledge and craft knowledge. The researchers have theoretical knowledge (i.e., knowledge about how to theorise accounting), while practitioners have craft knowledge (i.e., knowledge about how to do accounting). However, accounting researchers who are studying accounting practice are seeking to understand the nature of the craft of accounting. But if the knowledge which researchers produce is to be practised, it must be relevant to practice. As such, there needs to be a dialectical interplay between knowing and doing. This raises the question of whether practitioners read academic papers and I think everyone would agree that the answer is a resounding: No!

If we are concerned about the relevance of management accounting research for practitioners, one possibility would be to work with practitioners, either from accounting firms or other organisations and businesses, and seek to intervene in a practical way to provide solutions to accounting problems by, for instance, introducing new accounting techniques or management control systems (as in interventionist research to which I will return shortly). If we do not want to be so directly involved in the intervention, we could provide advice and support to practitioners who are involved in processes of change in their organisations. Alternatively, we could work through the professional bodies who are providing advice and support to their members or regulating accounting practices. Finally, we should not underestimate the effects of our teaching in educating the graduates who will become the next generation of practitioners, or the executive courses and continuous education programmes we provide for the current generation of practitioners. This is probably the most widely used route for transferring our theoretical and empirical knowledge into practice.

In addition, we could go beyond the individual practitioners and advise and/or lobby government, regulators and policymakers. Some accounting researchers have become directly involved in political debates, most notable in the UK is Prem Sikka who has been a very vocal critic of the big accounting firms.⁸ There are also researchers who have been very engaged in recent decades in raising awareness of environmental and climate concerns, and who have promoted various forms of environmental accounting. Furthermore, we can also engage with society more generally, by drawing on critical accounting research to focus debates about the negative aspects and potential dystopian consequences of modern capitalistic accounting practices. I know there are many accounting researchers who are actively involved in these types of activities, from interventionist researchers to critical theorists. But to what extent is such work valued in universities, when the primary (and possibly only) focus of university hierarchies is on publishing in highly-ranked research journals?

In recent years I have given seminars on writing qualitative research papers and in these seminars I emphasise that the research should be grounded in the existing knowledge in the literature and demonstrate an awareness of existing theoretical and empirical work. Furthermore, the research should be informed by theory and have theoretical implications, as well as making contributions to specific accounting and management issues. I normally emphasise the role of theory in making sense of the qualitative research, but I have to acknowledge that I do not spend as much time discussing the practical and social relevance of the research. I now intend to discuss these issues more extensively in future writing seminars.

Earlier, I mentioned writing articles for practitioners and others outside the academic field of study. Today, such writings could also include blogs, contributions to social media, and other similar platforms, or even be in audio form,

⁷ I am grateful to my Manchester colleague, ChunLei Yang, for pointing this out to me.

⁸ His active role was publicly recognised in 2020 when he was appointed to the House of Lords (the UK's upper legislative chamber) and became Lord Sikka.

such as a YouTube channel. Such media could make research findings and theoretical knowledge accessible to others. This could show how the findings and new knowledge are relevant for practitioners, regulators, government, and the general public, as well as setting out the practical and social implications of the research, with the focus on the practical/social issues, rather than on the theory. Instead, the theory would be a 'tool' used to address such issues.

As I indicated above, interventionist research can bring together practitioners and researchers to address issues which are of practical relevance. Such work has been advocated for quite some time by Scandinavian/Nordic accounting researchers (for an overview see Sten Jönsson and Kari Lukka, 2005) and more recently Lukka and Suomala (2014) and Lukka and Wouters (2022) have argued that interventionist research can have theoretical relevance as well as practical relevance. In interventionist research, theory can be used both *ex-ante* to shape the intervention, and *ex-post* to make sense of its outcomes. In addition, the intervention can help to refine, develop and/or extend the theory. For example, my Groningen colleague, Pieter Jansen, worked closely with the managers of a car dealership in the Netherlands to realise the senior management's vision through the participation of employees at various levels in the organisation. He became an active member of the dealership's management team responsible for realising this new vision. By reviewing the research literature and bringing relevant insights into the management discussions, existing theories and empirical work shaped the intervention (see Jansen, 2018), and the intervention was used to theorise different forms of informal participation, namely, 'informal hierarchical participation' and 'participation through organisational community' (Jansen, 2015). In this way, interventionist research can bring together the theoretical knowledge of the researcher and the craft knowledge of the practitioners. This enables accounting researchers to go beyond simply describing/explaining practices, and to make a difference in the practices of individual organisations. While Lukka and Wouters (2022) emphasise that interventionist research should be theoretically driven to achieve its theoretical ambitions, and thereby make a theoretical contribution, I would also contend that by being practically driven interventionist research can show how theory can be used to address practical problems. By so doing, it could lead to different and new forms of theorisation, and to theories which are more practically and socially relevant (see also Lukka and Suomala, 2014).

In her work drawing on the practice paradigm, and more specifically pragmatic constructivism, Hanne Nørreklit points out that there can be very different 'language games' played by researchers and practitioners. In Nørreklit, Nørreklit & Michell (2016), Hanne and her colleagues argue that the types of generalisations made by researchers, whether statistical or theoretical, are very different from the types of generalisations which practitioners use in their understandings of the practices which will be useful to them. This suggests that researchers will need a much more nuanced understanding of practice generalisations in order to make their findings applicable to practice settings. Whereas Nørreklit, Nørreklit & Michell (2016) use a previously published case study (Ahrens and Chapman, 2007) to illustrate how pragmatic constructivism can be used to explore how practitioners make generalisations, in another paper in the same Special Issue of QRAM⁹ Laine et al. (2016) explore how pragmatic constructivism can be used in an interventionist research project to study accounting under uncertainty and ambiguity in managing product development. These papers indicate that if researchers are going to be able to help practice, they will need methodologies, such as pragmatic constructivism, which enable them to understand how practitioners construct knowledge, and to recognise that this may be rather different to the way in which the researchers construct knowledge.

In another approach, which also reflects a pragmatic turn, Lisa Jack (2017) suggests ways of making 'critical-interpretive accounting' more relevant to practitioners through pragmatic studies which challenge the taken-for-granted assumptions embedded in existing research approaches. Drawing on pragmatists such as Baert and da Silva (2010), who propose a hermeneutics-inspired pragmatism, she suggests ways in which we could ask how things could be different. For example, Baert (2005) identifies 4 steps: (1) *conceptualisation*: reflect on and articulate previously unquestioned suppositions; (2) *critique*: question those unquestioned suppositions; (3) *edification*: confront them with 'local' understandings, views expectations and perceptions; and (4) *imagination*: ask *What If* questions? In a recent seminar presentation¹⁰ Lisa explained that she is currently exploring how to apply this pragmatic approach in asking questions about the food supply chain, such as: What if ... "We abolish discounts and commercial income in supermarkets?" What if ... "We abolish the distribution of share dividends from food companies?". Such 'revolutionary' questions cannot be addressed through our existing methodologies of empirical research, but instead require different methodological underpinnings – hence the pragmatic turn.

In somewhat similar vein, but drawing on different methodological underpinnings, my colleague Sven Modell is exploring how we can go beyond empirical research to study issues and questions in areas which are not readily observable. Specifically, how can we research questions relating to accounting in the emerging space industries. In Modell (2022), he draws on critical realism to propose the use of transfactual thought experiments as a complement to empirical research, and he sets out a framework which elaborates and nuances the distinction between intransitive and

⁹ See also the Guest Editors' Editorial: Nørreklit, Raffnsøe-Møller & Mitchell (20016).

¹⁰ Accounting Seminar, Birmingham Business School, 9th March 2022, Eating into the Margins: Can communicating accounting differently help change food systems?

transitive objects of knowledge and illustrates how this framework could be applied in future research on space accounting. In this way, existing theoretical knowledge could be used to inform thought experiments in areas where it is, or might be, difficult to study practices. In the field of organisation theory, Kornberger and Mantere (2020, p.1) also propose that thought experiments could be “crucial devices triggering transformations in thought and practice” and a way of making organisation theory “practically more relevant”. In a personal communication, Sven acknowledged that, given the strongly empiricist tradition of accounting research, it may be difficult to realise the potential of transfactual reasoning as it requires a ‘speculative’ mode of thinking that is not grounded in readily observable practices.

These are just some approaches I have seen recently which are seeking to go beyond describing how things are and, instead, are starting to ask how things could be. However, as Sven Modell recognises, the strongly empiricist tradition of accounting research may make it difficult to get such approaches accepted, especially in the top accounting research journals, and there may be a tendency to modify the approaches in order to comply with the expectations of these journals. Nevertheless, as I have described in this chapter, we have come a long way in the past 50 years, and I have been very proud to have taken part in that journey, but I now believe it is time for others to start envisioning where accounting research could be going in the future, rather than just continuing with more of what we have been doing. In the next and final section, I will provide some personal reflections on where I would like to see accounting research going, although I recognise that I am not going to be part of that journey—it is something for the current and, particularly, future generations of accounting researchers, and for me to wish everyone *bon voyage*.

5. Personal reflections

Some might say that what I was suggesting in the previous section begs various questions about what accounting researchers could or should be doing. Should they be producing theory, or using theory to address practical problems, or both, or something else entirely? What is the distinctive contribution accounting researchers can bring to accounting practice, and/or to society more generally? Can the deeper understandings which accounting research is providing do more than just *explain* past and current practices? Can it inform future practice? Can/should accounting research inform the practitioners who are ‘doing’ accounting? Should accounting researchers be exploring how to combine practical and theoretical knowledge? Others might say that these types of questions imply a rather limited perspective on accounting research by relating it to what accountants do. In recent years there has been a trend to use the knowledge generated by accounting research to theorise other forms of practice which involve quantification and forms of accountability. Looking for new activities to theorise, taking us beyond what has been the traditional focus of accounting research, offers almost unlimited possibilities for theorising. While such research has the potential to expand the domain of accounting research, it risks marginalising the traditional areas of accounting research which were concerned with what ‘accountants’ do.

I do not have answers to all these questions, but I think they are questions which need to be discussed, rather than just seeking new accounting-like areas to theorise. Undoubtedly, in a vibrant research area, there should be a wide variety of research. So, I am not suggesting that we do not look for other areas to theorise, but that should not be at the expense of marginalising the more traditional areas of accounting. Possibly, it might be helpful to think about the ‘*positioning*’ of accounting research, in relation to the underlying disciplines and to practice. For this purpose, I will use medical research as an analogy.

Biological sciences (and other related scientific disciplines) underpin and inform medical research. These disciplines provide the theoretical and empirical scientific knowledge which is used by medical researchers. Then there are the medical researchers who use this scientific knowledge, together with both quantitative and clinical methods, to study particular medical conditions and diseases, and thirdly, there are the medical practitioners who apply medical knowledge to the benefit of their patients. Surrounding the practitioners there are policymakers and regulators who also have an impact on medical practices. Exploiting this analogy, we would not expect medical researchers to focus primarily on their contributions to biological theories. In some cases, there may be contributions, but I would expect medical researchers *to use* biological sciences (and other scientific disciplines) to inform and advance medical practice.

As an accounting researcher, I wonder where we are positioned in relation to the core disciplines (economics, organisational theory, sociology, etc.). Should we be seeking to contribute to the underlying social sciences by developing and extending their theories, or should we be using those theories to inform and develop accounting practice and the way in which accounting is used in, and the consequences it has for, wider society? I think this is a question we, as accounting researchers, have to ask ourselves, especially as to publish papers in top international journals there seems to be an expectation that we should be contributing to theories in the underlying disciplines, rather than developing accounting theories to address relevant issues concerning accounting practice (cf. Malmi and Granlund, 2009). This is not to imply that we should not, where possible, try to make contributions to theories in the underlying disciplines, but rather that such contributions should not be required in order to justify publication in top accounting journals. That sort of requirement could marginalise other types of contributions, including contributions which have practical and social

relevance. If we want to contribute to these other disciplines, might it not be more appropriate to publish in journals in those fields? Whereas, if we want to contribute to accounting, broadly defined, we should publish in accounting journals. In my own work, and when I present seminars on doing case study research, I draw a distinction between sensitising theories and substantive theories. Sensitising theories are usually social theories and/or organisation theories which sensitise us to the social processes which are involved, while substantive theories are the theories developed by accounting researchers to explain the accounting practices being studied. I would expect accounting researchers to use the former to contribute to, and thereby, extend the latter - but not necessarily to contribute to the former.

Over the 50 years that I have been an accounting researcher, I think it is reasonable to say that much has been achieved by accounting researchers and that accounting research is now a well-established university discipline. It draws on theories from economic and social sciences, and uses them to develop theorised understandings of accounting practices (i.e., accounting theories). However, I believe that it should not be theory for theory's sake, but instead we should be using theory to achieve 'something', and this 'something' should be relevant to accounting, or how accounting interacts with broader society, especially if the research is publicly funded. It is no longer necessary to draw on theories from more established disciplines to give legitimacy to accounting research, as it might have been in the past. Today, I think there is a need for greater consideration to be given to *why* we are doing accounting research, *what* that 'something' is that we are seeking to achieve, and *how* accounting can be made more relevant. I do not think there are simple answers to these questions, as there are many and diverse 'things' which accounting researchers are and could be doing. However, maybe it is time to think more carefully about them, rather than being overly concerned about how to contribute to theory in the other disciplines.

Although I did much of my work in the late 20th century, some of the issues I researched then are still current today – such as the role of management accountants and the impact of advances in information technology. But the latter has become ever more important as we have moved into the 21st century, with developments like cloud computing, artificial intelligence, robotic process automation, blockchain, data analytics, big data and so on, changing the way in which accounting information is collected, processed, analysed and used in financial reporting and audit, as well as in management decision-making and control. In addition, there are environmental and sustainability issues, which started to be discussed by accounting researchers in the 20th century but are now some of the most important challenges of the 21st century. Today there is much discussion of the technologies which are needed to deal with these environmental challenges, such as hydrogen-based technologies, carbon capture, floating wind farms, and other sustainable forms of power generation. Thus, advances in technology are likely to be a continuing and defining feature of the 21st century.

Can we, as accounting researchers, mobilise our existing theories to explore the ways in which such technologies could be used to address the challenges of the 21st century? Can we do more than observe and explain? Can we use our theoretical knowledge to address problems, suggest possible solutions, and make a difference? In other words, can we do more than follow practice, and instead find ways our research can lead practice?

As I mentioned above, accounting research has come a long way over the past 50 years. My own research has changed from economic-based analytical normative accounting research in the 1970s, to theoretically informed interpretive case studies of management accounting practices in more recent years. In my own research and in my role as the Editor-in-Chief of *Management Accounting Research* I have stressed the importance of theorising existing practices. But, as I indicated above, I now feel that we need to go further. Accounting research needs to do more than theorise existing practice; it needs to use those theories to inform and shape future practice.

I think we are at a turning point, with some researchers starting to use theory to look at how accounting can help in addressing, for instance, environmental sustainability issues, the use of emerging technologies, and so on. But there is still a lot of research, especially in so-called top journals, which focuses on theorising existing practices without being particularly concerned with how the theory can be used to address current and new issues and challenges. I am not suggesting that we go back to the normative research of the 1970s, but instead that we give more attention to how we can use our theories in ways which can make a difference by *leading* practice, rather than *following* it, and by seeking to address the challenges of the 21st century. In the 1970s we relied almost exclusively on neoclassical economics to inform our normative work, whereas now we have a considerably broader base of theories, informed by a wide range of social sciences and grounded in empirical work, as well as diverse methodological tools, which we could be using to make accounting research more relevant.

As I mentioned earlier, questions are being raised about the societal benefits of publicly funding university research. Accounting researchers need to become more aware of how they would answer such questions, and whether focusing on the theorisation of existing practices is likely to be perceived as providing sufficient value to society to justify it being publicly funded. Today, accounting research is perceived as a legitimate university discipline, and we have theorised ways of understanding accounting practices. I feel that it is now time to move on and use these theories to make a difference. This will require accounting researchers to give serious consideration to what type of 'difference' they are able to make. There are likely to be many ways in which accounting researchers can make a difference, and I would like to think that making a difference will become much more prominent in accounting research in the future.

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