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Management Accounting Research: 25 years on

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ABSTRACT

This Editorial introduces and comments on the implications of the papers presented at the 25th Anniversary Conference of *Management Accounting Research* which was held at the London School of Economics and Political Science in April 2015. It first examines the context in which *Management Accounting Research* was founded in 1990 and then introduces the six invited review papers. These papers cover a wide range of subjects comprising critical and social theory, managerialist studies, contingency theory, experimental behavioural research and intra-organisational management accounting. Amongst various other recommendations, some of the authors suggest that there is a need for research in management accounting to more effectively build on prior research so as to accumulate knowledge about specific issues and problems. In addition, they suggest that researchers in the different areas (or sub-disciplines) of management accounting should talk to each other more. For instance, insights and findings from qualitative research could be used to inform quantitative studies and vice versa. The later parts of this Editorial discuss opportunities and challenges for management accounting research in the future. In particular, it is pointed out that, compared to when *Management Accounting Research* was founded in 1990, researchers now have highly theorised understandings of management accounting practices, and one challenge is to use these understandings to try to close the 'practice-research gap'. It is argued that management accounting theories have had a relatively limited impact on practice and, as there are increasing pressures on universities to demonstrate the impact and value of university research, some suggestions are made about ways of increasing the impact of management accounting research.

1. Introduction

Management Accounting Research was founded in 1990 and in April 2015 we organised a conference to celebrate its 25th Anniversary. Six management accounting researchers, all of whom have been members of the Editorial Board, were invited to present papers reviewing specific areas of research in management accounting, and to reflect on the contribution of *Management Accounting Research* to their area. In addition, we invited the new editor, Wim Van der Stede, to chair a plenary discussion during which three other management accounting researchers discussed directions for the future. This *Special Issue* contains the six review papers, as well as the three plenary contributions and Van der Stede's commentary which introduces them.

Before introducing the papers in this *Special Issue*, we will describe the context in which *Management Accounting Research* was founded. We will then introduce the papers and point to some of their conclusions, especially the achievements of research in the management accounting field. We will finish by suggesting some challenges and opportunities for the future. We begin, in the next section, by looking back at some of the early research in management accounting, and the context within which *Management Accounting Research* was founded.

2. The past and the founding of *Management Accounting Research*

In the 1980s, before *Management Accounting Research* was founded, the management accounting community in the UK was small and scattered amongst various universities, and there were few domestic or international networks for researchers and very few journals in which to publish their research, especially research

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of a non-economic nature.[1] The leading US journals were regarded as inaccessible, favouring financial accounting research, and interested only in management accounting papers which were based on economics and had a theoretical or a strongly empirical stance. Most established UK university teachers of management accounting had come from practice and were non-researchers, or they tended to undertake practice-based research. These and other problems were also being experienced, to differing degrees, by management accounting researchers in other countries.

At that time, UK researchers in the area had only just begun to obtain PhDs. However, a large number of relatively new research avenues were opening up and new approaches and methods for research were becoming available. Furthermore, much of the earlier research was beginning to be questioned or rejected. In this sense, it was a good time to launch a specialist journal in the management accounting area.

As part of a research project funded by the then Social Science Research Council,[2] Scapens (1984) surveyed the state of management accounting research at that time, by reviewing the contents of the (then) current textbooks, as well as papers in research journals. He came to the conclusion that there was no generally accepted definition of management accounting. Subsequently, providing a definition of management accounting has continued to be very difficult. However, Scapens (1984) pointed out that the then current textbooks seemed to 'know' what management accounting was; or at least, the textbooks had a common set of contents, based primarily on earlier research which studied decision making from largely economic, management science and operations research perspectives. However, when looking in detail at the contents of these textbooks and comparing them with what was then known about practice, it seemed clear that there was 'a gap between theory and practice'. Furthermore, it did not seem likely that this gap could be explained by a time lag between developing theoretical ideas and diffusing them in practice. It is fair to say that, at the time, we knew relatively little about management accounting in practice. The general view seemed to be that organisations used the traditional tools and techniques, such as overhead allocation, budgeting and standard costing.

Scapens' views were reinforced at the 1984 Deloitte, Haskins and Sells seminar which was devoted to management accounting. Papers were presented by leading US and UK researchers, and also by practitioners. As well as researchers, the audience included senior auditors and accountants from industry, some of whom provided commentaries on the academic papers.[3] Horngren and Kaplan both pleaded for more studies of management accounting practice, and Horngren called for such studies to be undertaken using a behavioural lens.

In summarising the conference, Bromwich gave three reasons why it was "the worst of times" for management accounting (see Bromwich and Hopwood, 1986; p. 217). Firstly, the papers almost unanimously suggested that research had little impact on practice and that practice had remained rooted in the past. Secondly, researchers did not know or care whether this was the case, even though at least some management accounting teaching in UK universities was research oriented, and the relevant professional examinations also incorporated research findings. Thirdly, the conference had suggested that management accounting lacked a theoretical framework, being a collection of rather loosely related subjects. Arguing against this view, practitioners pointed out that they were well aware of the problems with the available tools and they compensated for these problems in their decision making. Moreover, they were themselves seeking to innovate and researchers should seek to understand and learn from such practical innovations.

If we look back to the 1970s, most management accounting research was grounded in neo-classical economics. For example, based on neoclassical economic assumptions, researchers adopted a management science/operations research perspective to develop various decision models, some of which were mathematically quite sophisticated. Scapens' survey paper reviewed these mathematical decision models, but pointed out that generally they remained untested in practice. Furthermore, if information costs and benefits were taken into account, it could be shown theoretically that in some instances simple rules of thumb could be optimal. This provided a possible explanation for the gap between theory and practice—i.e., the theoretical models failed to take account of the costs and benefits of their use in practice. However, at that time, although there were some general presumptions about the nature of management accounting in practice, there was relatively little systematic and/or in-depth research into management accounting practice. Management accounting researchers were more concerned about improving 'practice' by developing normative models which practitioners were then expected to use. Unfortunately, there was no evidence that practitioners did use them, or indeed that they wanted such normative models. It is probably fair to say that management accounting researchers at that time were operating in their 'ivory towers', and adopting a somewhat arrogant attitude about what should be done in practice, perhaps without understanding the complexity of practice in an imperfect world.

However, in the early 1980s, management accounting researchers started to study management accounting practice. Initially there were various surveys and an increasing number of largely descriptive case/field studies. Some of this research was simply used to reinforce the perception of a 'gap between theory and practice'. Other researchers, however, began to draw on organisational and social theories, and especially contingency theory, to study management accounting. *Accounting Organizations and Society*, which was founded in 1976, was particularly prominent in publishing this type of research. Furthermore, management accounting researchers started to use a wide range of organisational and social theories in case/field studies and surveys that were designed to understand management accounting practices. Nevertheless, the economics perspective continued to be used by most 'mainstream' management accounting researchers. So, when *Management Accounting Research* was founded in 1990 a wide range of disciplines, methodologies and theoretical frameworks were starting to be used by management accounting researchers.

Consequently, when we were invited by Academic Press[4] and the Chartered Institute of Management Accountants (CIMA) to edit *Management Accounting Research*, one of the first decisions we took was to make the scope of management accounting research very broad in order to avoid the papers published in the journal being constrained to a particular view of the nature of management accounting. Furthermore, we wanted researchers to bring to the journal whatever theoretical perspectives and methodologies they considered appropriate for research in the field of manage-

[1] The European Accounting Association was founded in 1977. In the UK the Management Accounting Research Group was established in 1979 by the then Social Science Research Council and the Institute of Chartered Accountants in England and Wales, joined shortly afterwards by the Chartered Institute of Management Accountants, in order to establish an academic network for management accounting researchers.

[2] The Social Science Research Council (SSRC) changed its name to the Economic and Social Research Council (ESRC) on 1 January 1984. The research was undertaken in 1982, and subsequently published in 1984.

[3] This DH&S seminar was co-sponsored by the Economic and Social Research Council and the publisher, Pitman.

[4] *Management Accounting Research* was originally owned by Academic Press, but subsequently sold to Elsevier.

ment accounting (broadly defined), provided that the use of these perspectives and methodologies satisfied authoritative reviewers in those areas. As the journal has developed over the years it has retained this broad scope (see Van der Stede, 2015), and research adopting a wide range of theories and research methods and methodologies has been published over the past 25 years. This is illustrated in two of the tables which were included in our Editorial reviewing the papers published over the first 20 years (see Tables 4 and 5 in Scapens and Bromwich, 2010).

We would like to draw attention to a couple of interesting points from those tables. During the first ten years of *Management Accounting Research*, 34% of the papers were characterised as 'applied'; in other words, they described practice without using any explicit theory. Such papers were needed in the 1990s as we were only then beginning to learn about practice. However, in the subsequent 10 years there were far less 'applied' papers, and in the last five years there have been very few. This illustrates the extent to which the importance of theory has increased over the years. Furthermore, there have been shifts in the types of theories used in the papers published in the journal. Economics was prominent in the first ten years, but subsequently institutional theory, contingency theory and a mix of other theories have become more widely used. It is important to recognise that this was not an editorial decision, it simply reflected the way in which research in the field of management accounting developed. Furthermore, in terms of the research methods, there was a substantial increase in the number of case study/field-based papers published in the second ten years, and also an increase in the number of survey-based studies applying statistical methods, but a relative decline in analytical papers and only a small number of experimental papers. Overall, the tables included in our Editorials after the first 10 and 20 years, respectively (Scapens and Bromwich, 2001, 2010), indicate the wide range of both quantitative and qualitative papers which have been published. This is discussed in more detail in the papers included in this *Special Issue*.

3. The papers in this *Special issue*

As mentioned above, for the 25th Anniversary Conference the plenary speakers were invited to review specific areas of research in management accounting. Trevor Hopper was invited to review research which has used social and critical theories. However, the paper for this *Special Issue*, which he wrote with Binh Bui, provides a comprehensive analysis of all 475 papers published in *Management Accounting Research* over the last 25 years. Amongst other things, this reinforces the points made in our Editorials about the diversity of theories used, and the decline in the number of papers with no clear theory (see their Fig. 10—reproduced below as Fig. 1). In particular, they show the increase in the number of papers which use social and critical theories. For this purpose they use a broad definition of 'social and critical' (taken from the first Interdisciplinary Perspectives on Accounting (IPA) Conference) which "spans interpretive, institutional, social and environmental, political economy, post-structural and constructivist work".

In the title of their paper, Hopper and Bui ask a specific question: Has *Management Accounting Research* been critical? They conclude that "MAR has made substantial contributions to social and critical accounting (broadly defined) but not in critical areas endeavouring to give greater voice and influence to marginalised sectors of society worldwide." The early part of their paper describes Trevor Hopper's personal journey, and the aspirations of the early researchers who sought to study accounting as a social science, and to give the subject a more 'critical' edge. They recognise that, although *Management Accounting Research* was willing to publish papers which are critical in this more narrow sense, relatively few such 'critical' papers were submitted to the journal. However, contrasting research in North America, Hopper and Bui point out that European management accounting research has been more multidisciplinary, eclectic and multi-paradigm. In particular, they conclude that from "the perspective of the early 'behavioural accounting' researchers the progress has been remarkable", and that the ensuing "greater understanding of accounting practice.... [has] produced radically different conceptions of what practice constitutes."

Teemu Malmi reviews constructivist and managerial studies. Acknowledging that there is no widely accepted definition of constructivist and managerialist research, Malmi reviews a wide variety of studies which have the intention of producing managerially-relevant insights, and he defines constructivist studies as those "in which a theoretically novel construct is created and its practical applicability is demonstrated". He comments that *Management Accounting Research* provides probably the only high level outlet for such work. Noting that there have been a number of interventionist studies in recent years, Malmi points out that "the studied topics tend to vary a great deal". However, rather than focusing on the nature of the construct itself, researchers tend to be more concerned "to understand how change may be conducted to secure success". This extends our knowledge of management accounting change, but it is difficult for management accounting researchers to promote a 'new' construct and to disseminate new practices more widely. He concludes that there are interesting papers which focus on specific practical problems, but "it is difficult to speak about the accumulation of knowledge". This represents both a challenge and an opportunity for constructivist and managerialist management accounting researchers. To date, various practical issues have been addressed and solutions developed for specific organisations. In the future, a challenge will be to build a body of accumulated knowledge that has the potential to make a difference in practice. We will return to this point later.

As there have been different types of contingency theory research, we invited David Otley to review work at the organisational level and Matt Hall to review work at the individual level. David Otley concludes that "[t]he work conducted under the banner of contingency theory has been one of the success stories of research in management accounting and control over the past forty years". It has provided considerable insights into how different configurations and uses of control systems can result in a variety of different consequences. He points out that, rather than being about management accounting and management control, this research is now more about performance measurement systems or performance management systems. This could be just a redefinition, but it raises some important points. For example, Otley argues that there is a need for more research on the connections between the various elements of performance management systems. How do the elements fit together? Do they form a system or a package? What is the difference between a package and a system? Although such research is now beginning to take place, Otley is critical that there continues to be much research which looks only at individual elements. He made a similar point about management control systems 35 years ago (see Otley, 1980), and he feels it is necessary to repeat it today.

He also suggests that there is an opportunity for contingency research to combine qualitative and quantitative methods. For example, quantitative researchers could draw on qualitative research to define problems which can be studied analytically, or qualitative research could be used to interpret the results of quantitative contingency studies. However, he noted that contingency studies in management accounting tend not to build on prior work in a consistent and coherent way. For example, a new piece of work will often use a different measurement scheme or a slightly different framework. This creates a challenge in building a body of accumulated knowledge. Interestingly, this is a recurring point, to which we will return later.

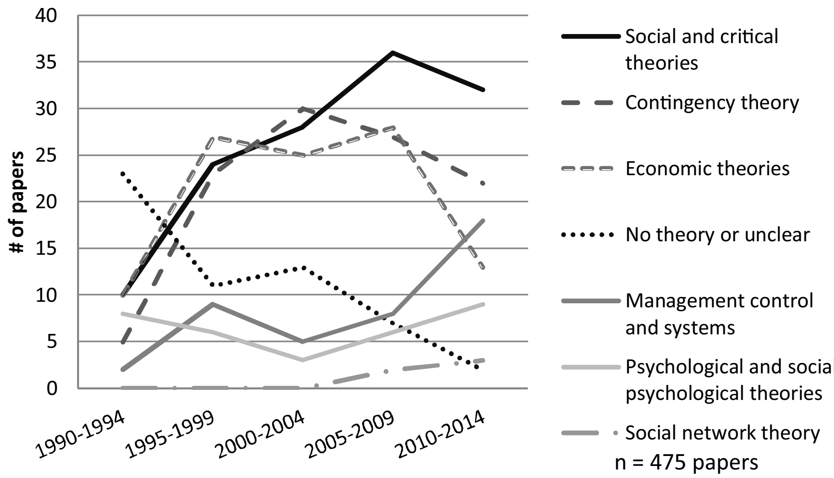

Fig. 1. Theoretical approaches over time.

Matt Hall focuses on the use of psychological theories in contingency-based management accounting research. He points out that "[c]ontingency-based management accounting research has a long and distinguished history of providing insights into the role and functioning of management accounting practice in organisations", and that "psychological theory has been extensively employed" in such research. Nevertheless, in his paper he identifies ways in which psychological theories could be more fruitfully used in contingency-based management accounting research. For instance, he suggests that there could be stronger linkages between individual-level contingency studies and studies which look at organisation-level contingencies. He also suggests that there is potential for studies to look at contingencies in a more dynamic way in order to study how 'fit' occurs? More specifically, given existing knowledge about individual-level contingencies, such studies could explore how organisations can achieve a fit.

Also at the individual/psychological level, Joan Luft reviews management accounting research which uses behavioural experiments. She notes that there are very few such papers published in *Management Accounting Research*, and consequently she reviews the literature more generally. She specifically reviews "experimental studies that investigate the influence of management control systems on competitive and cooperative interactions among employees". She identifies a number of open questions for research on intra-organisational competition and collaboration. In contrast to David Otley's comment that contingency theory research tends not to build on prior work, Joan Luft notes that there are quite a lot of replications in behavioural research. However, she points out that this is not necessarily 'pure replication' – i.e., simply repeating the same experiment – but it is replication in the sense that it starts from the prior work and first confirms that there are similar findings before moving on to test additional hypotheses and/or to develop new ideas. As a result such studies build on previous work and lead to an accumulation of knowledge. Furthermore, she argues that experiments can complement archival and survey studies, and vice versa. This extends, to some extent, the suggestions made by Matt Hall.

In the final paper, Henri Dekker reviews management accounting research at the boundaries of intra-firm and inter-firm relationships. He points out that the growing literature on accounting and control of inter-organisational relationships (a substantial amount of which has been published in *Management Accounting Research*) is developing largely independently of the already extensive literature which studies management accounting and control *within* organisations. He argues that there is an opportunity for research to explore the boundaries between intra-firm and inter-firm management accounting, and he proposes three specific areas in which research could be undertaken. He suggests that research 'at the boundaries' could provide better understandings of intra-firm and inter-firm management accounting themselves. He also sees potential in combining qualitative field research and quantitative survey research. For example, qualitative research could provide in-depth insights into the nature of inter-firm controls, while quantitative research could provide larger sample evidence on the use of such controls and their associations with contextual factors.

As these reviews demonstrate, over the past 25 years *Management Accounting Research* has played an important role in publishing papers which have considerably extended our knowledge of management accounting, and have given rise to new understandings of the subject. In particular, we now know far more about how management accounting works in practice, both theoretically and empirically, than we did in, say, the 1970s and 1980s, and we can now provide far better theoretically informed explanations of management accounting practice. *Management Accounting Research*'s unique feature has been its openness to diversity. It has published papers drawing on a wide range of different disciplines and paradigms, and using a wide variety of research methods and theories. This is not so true of many other journals. For example, in a Special Section of *Management Accounting Research* published in 2010 (Vol. 21, No. 2, pp.110–129), Kari Lukka brought together a number of presentations from a 2009 EAA Plenary Session during which invited speakers discussed paradigms in management accounting research. In particular, they pointed to the difficulties of publishing research which is outside the mainstream paradigms. However, as the analysis in Hopper and Bui's paper in this *Special Issue* demonstrates, this is not true of *Management Accounting Research*.

Compared to the 1970s and 80s we now have much richer understandings of management accounting practice, with substantial amounts of both quantitative and qualitative research and highly theorised explanations. Nevertheless, as the papers and commentaries in this *Special Issue* indicate, there remain many challenges and opportunities for research in the future. Over the years, management accounting researchers have been very willing to draw on theories and research methods from other fields—including, for instance, economics, organisation theory, psychology, sociology and social theory. Such theories and research methods have helped to shape the diversity in management accounting research. However, some of the papers in this *Special Issue* point to opportunities for making connections between the research in particular subfields *within* management accounting and some also discuss the need for more replication of the findings.

4. Challenges and opportunities

A striking feature that emerges from the papers in this *Special Issue* is that each seems to review an island of research which is somewhat isolated from the other islands, and tourists from other islands are not warmly welcomed. Moreover, the islands themselves are often split into rather isolated sections (i.e. sub-disciplines). Although many islands are not reviewed in the papers in this issue, we agree with the authors who suggest that more integration would be desirable. We believe that such an approach offers a promising avenue for management accounting research. For example, we could consider how qualitative and quantitative work can support each other; how experimental work can inform survey and field research, and vice versa; how findings at the individual level can be used to provide insights for the design of studies at the organisational level; and how intra-organisational research can inform inter-organisational research and vice versa.

Some recent papers in *Management Accounting Research* have sought to integrate quantitative and qualitative research, but it has not proved easy. Nevertheless, it would be interesting to explore whether using multiple research methods to study specific problems can deepen our understanding of those problems. There could also be opportunities to exploit the diversity of research in the field of management accounting by drawing together insights from the various 'subfields', as well as looking to disciplines outside management accounting.

When we talk about multi-disciplinary research, we usually mean drawing on disciplines from outside accounting, but we could also think about multi-disciplinary research *within* accounting. The diversity of areas of topics, methodologies, research methods and theories seem to have created a number of separate (sub-) disciplines *within* management accounting. As these *within* management accounting disciplines tend to create their own separate domains, even separate silos, a challenge for the future could be to build a more comprehensive and coherent body of management accounting knowledge through 'conversations' across these *within* management accounting disciplines.

However, as indicated above, some of the papers in this *Special Issue* question whether we are accumulating a body of knowledge even within the specific areas (or sub-disciplines) of management accounting. For instance, in managerialist and contingency theory research, settings often seem to be selected in isolation from what has gone before. However, this does not necessarily mean that we need more 'pure' replication studies—i.e. simply repeating what has already been done. Rather, we need studies which more explicitly build on and extend the previous work. There are a few papers in *Management Accounting Research* which have sought to replicate previous findings in different settings, e.g. different locations, industries or populations, but this has also proved difficult, especially controlling for the different settings. More work is needed in which researchers use similar frames and measures, as in previous work, or to discuss explicitly how their new frames and measures extend the accumulated knowledge developed in previous studies.

In much of science in general, knowledge usually grows through research which builds incrementally, often in a very small way, on a substantial amount of already accumulated knowledge. However, in many areas of management accounting we still lack a substantial body of accumulated knowledge. Furthermore, researchers understandably tend to be rather ambitious in their research; for example, by proposing that a *new* variable will help in explaining, say, how management accounting reports can affect organisational performance. Such management accounting research, which explores new variables or new areas, is clearly exploratory. Nevertheless, it needs to be grounded in prior research/knowledge and the 'incremental' effects need to be theorised relative to what is already known. Furthermore, the initial findings will be regarded as tentative until further, and possibly more refined, studies are undertaken to support them. This type of research is therefore unlikely to affect practice until it matures through the accumulation of knowledge.

As we indicated above, the papers in this *Special Issue* demonstrate that there have been substantial achievements in research in the field of management accounting over the past 25 years (and longer), and that *Management Accounting Research* has played an important role. As the Joint Editors until quite recently, we were very pleased with the steady increases in the journal's impact factor since it was included in the *Social Science Citation Index* in 2009. However, impact measured in terms of citations indices is not the only way in which the 'impact' of research can be assessed. For example, in the recent research assessment in the UK (held under the title of the Research Excellence Framework), universities had to include a number of *impact case studies* outlining "the changes and benefits [of their research] to the UK economy, society, culture, public policy and services, health, the environment and quality of life and impacts in these sectors beyond the UK."[5] Furthermore, in the Netherlands, and also in a number of other countries, the *Science in Transition Movement* is arguing that "Science has become a self-referential system where quality is measured mostly in bibliographic parameters and where social relevance is undervalued."[6] In a number of countries questions are now being raised about the value of academic research, and such questions may become increasingly important in the future as governments seeking to reduce public expenditure may begin to ask questions about the value received from public monies spent on university research.

So a challenge for the future may be to demonstrate the impact which research in management accounting has had outside the academic journals. We could start by asking: what impact are the richer and highly theorised understandings of management accounting, which we now have, having on practice? Should we expect them to have an impact on practice? If not, where is the impact likely to be? If we were to ask practitioners about the types of research which have had a major impact on management accounting over the last 25 years, they would probably refer to things like the balanced scorecard and activity based costing, rather than the theories which have been developed in the academic literature. This raises an interesting question about the relevance of management accounting research. Who, beyond management accounting researchers, are likely to find our richer understandings useful? Similar questions are being asked about financial accounting research and about management research more generally. For instance, in a paper titled "Making management accounting research more useful", Ken Merchant cites Gary Latham, a senior organisational behaviour researcher, who claims that there is "a perception among senior [organisational behaviour] scholars that the present generation is doing research which is less useful than ours" (Latham, 2011, p. 316; cited in Merchant, 2012; p. 334).

In academic journals we write for other researchers and we use forms of language and argumentation that are unlikely to be easily understood by non-academics. This mode of discourse is not unreasonable as it facilitates communication between academics. However, if our research has implications for management accounting practitioners, and also for others, how do we communi-

5 See impact.ref.ac.uk/casestudies/About.aspx.
6 See www.scienceintransition.nl/English.

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cate it to them? We can write articles for practitioner journals and we can work with professional bodies and/or individual organisations. In recent years, as indicated in the paper by Malmi, there have been some interventionist studies in which researchers have worked closely with practitioners in specific organisations. Furthermore, some researchers contribute to the work of professional bodies. We should not forget that CIMA was instrumental in setting up *Management Accounting Research* 25 years ago and has sponsored a number of books seeking to disseminate management accounting research findings to practitioners (e.g. Bromwich and Bhimani, 1994). The UK professional accounting bodies have devoted substantial resources to the dissemination of research findings via both courses and published reports. In addition, some researchers advise government departments and bodies, while others engage in both public and practical debates. These are all activities through which management accounting researchers can have, and at least to a limited extent have had, an impact on practice. We would strongly encourage management accounting researchers, as well as writing their research papers, to seek other ways of increasing the impact of their research.

5. What can we do to increase impact on practice?

A long time ago, when we were both young researchers, there was an expectation that we (as accounting researchers) would write articles for practitioner magazines and well as papers in the research journals. However, this would seem to be very unusual these days, probably due to the increasing emphasis in many countries on forms of research assessment which only value papers in highly ranked academic journals. Nevertheless, we would urge management accounting researchers to look for ways of communicating the results of their research to a wider audience.

The above comments could be taken to imply a somewhat narrow view of relevance – i.e. developing new ideas and tools which could be used by management accounting practitioners. However, the 'critical' research which Trevor Hopper was seeking would not be intended to develop new practices; instead it would be designed to provide critical analyses of the consequences of existing practices. More generally, such research challenges, and seeks to upset, conventional ways of thinking about and practicing management accounting. Consequently, this type of research is unlikely to be published in practitioner journals, nor is it likely to be readily accepted by practitioners. Nevertheless, there are other media through which critical researchers can contribute to wider public debates.

While other social sciences also have problems in disseminating their research to practitioners and to the public more generally, there may be particular problems for accounting, and especially management accounting. CEOs rarely say that their accounting gives their organisations proprietary advantages, or that accounting is among the major current problems they face.[7]

Financial, and especially management, accounting research generates little interest in the general media, unless it concerns something like a financial accounting scandal. Consequently, accounting practitioners, managers and other interested groups will not be informed about the research unless they read the specialist research literature. In contrast, social science research which produces new facts, or new applications of existing facts, can become widely publicised. As management accounting researchers tend not to generate *new facts*, or where they do they refer only to very specific settings, researchers often try to over-sell their findings by making exaggerated claims about the generality of their findings.

If we look back say 50 years or so, management accounting was generally seen as a practical craft which was largely outside universities and was learnt in practice. However, accounting in general, including management accounting, has become a university-based subject over the intervening years. Initially, it was linked to economics, and subsequently also to disciplines such as sociology, psychology and organisational theory. Using these disciplines has helped to establish the legitimacy of (management) accounting as an academic subject, as accounting researchers have demonstrated that they can contribute to (or at least draw upon) these more established disciplines. The question now is whether (management) accounting has matured as an academic subject, such that it no longer needs to draw on other disciplines *simply* to achieve academic legitimacy. If so, we could think more about how management accounting research can become the bridge between theory and practice.

As we indicated above, management accounting research tends to follow (rather than lead) practice. As Figs. 2 and 3 illustrate, a number of important management accounting innovations in the past were generated in practice. Taking as examples a number of major management accounting innovations over the period indicated on the horizontal axis, Fig. 2 indicates the time periods needed before they became generally accepted by large organisations in the UK and the US. Two particular points should be noted. Firstly, acceptance took some considerable time, often well over ten years. Secondly, the majority of these innovations evolved in practice and were mostly developed by engineers rather than accountants. However, DCF was developed by researchers, but mainly economists, although accounting researchers were involved in its dissemination. Although divisionalisation evolved in practice, accounting researchers developed a number accounting tools for divisionalised organisations, but it was consultants who refined and popularised new techniques based on residual income and value added management.

Fig. 3 shows a somewhat subjective view of the extent to which a number of management accounting innovations (broadly defined) have been adopted by large UK organisations since the late 1980s. Other innovations could be included, but those shown could be regarded as the 'leading edge'. Some other innovations, e.g. linear programming, while important for research and teaching have failed to take off in practice, except for some highly specialist uses. Others, which seemed for a time to offer great promise, have become accepted as useful, but for limited purposes, e.g. activity based costing and various 'Japanese management accounting' tools. Looking at Figs. 2 and 3 it seems that widely accepted major innovations are relatively rare. Furthermore, Fig. 3 reinforces the point made above about Fig. 2, i.e. that the dissemination of even successful innovations can be very slow.

Most of the innovations shown in Fig. 3 have originated in practice. However, some have been theorised and refined by researchers or popularised by consultants. Examples of the former are activity based costing and the balanced scorecard, and examples of the latter are beyond budgeting and value added management. Of the successful innovations only value added management and transfer pricing are founded on research. It can reasonably be said that accounting researchers come late to the study of most successful management accounting innovations and they tend only to test the claims of their advocates, rather than trying to develop the innovations. A current 'hot topic' in practice is business models. While much of the content of these models is based on management accounting information, accounting researchers do not seem to be particularly interested in the area. If researchers are to contribute to new practical innovations they need to become involved earlier in the life of those innovations.

[7] See, for example, pwc Annual Global CEO Survey: http://www.pwc.com/gx/en/ceo-agenda/ceosurvey/2015.html.

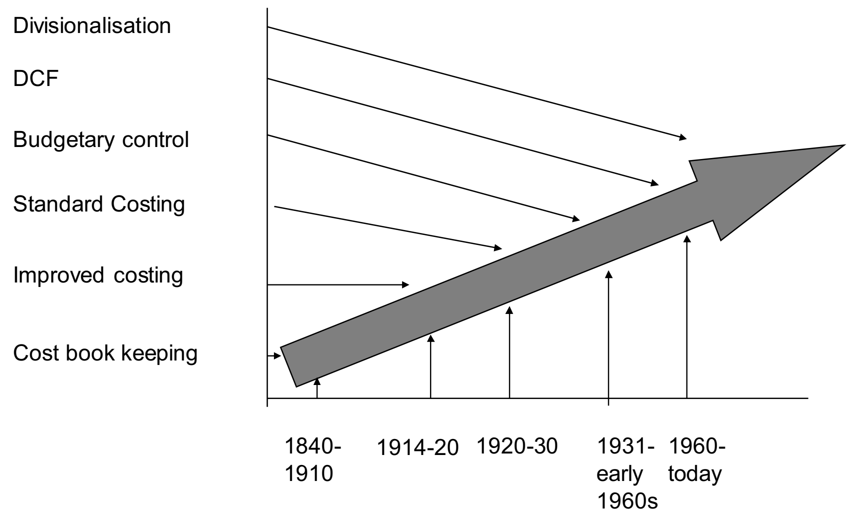

Fig. 2. Evolution of management accounting in UK and USA.

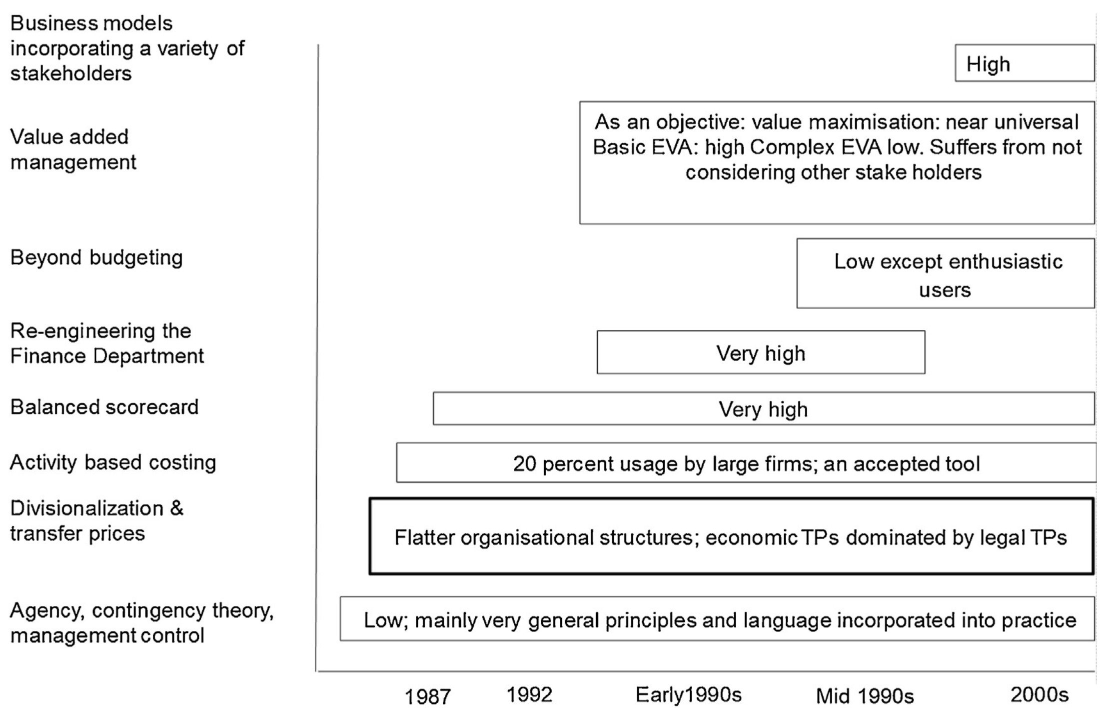

Fig. 3. Penetration of new methods in the UK.

The bottom row of Fig. 3 labelled, 'agency, contingency theory and management control', contains some of the major research areas in management accounting, but this research is shown as having only a low impact. These areas of research are generally agreed to have greatly increased our understanding of management accounting. However, their results seem to have had little impact on practice. Some of the language of this research and some of the general findings have been incorporated into practical discourse. Concepts such as moral hazard, adverse selection, contingencies, and incentives based on tournaments or on individual performance are well understood from experience by many in practice. However, there seems to be little evidence of the findings of management accounting research drawing these concepts being incorporated into practice.

One reason may be that the models used by researchers seem to be far removed from the contexts in which organisations operate, especially when researchers focus their attention on a specific narrow sub-discipline. Practitioners may be expected to take a more holistic view of organisations. Another reason may be that researchers tend to adopt very restricted assumptions and their studies are often 'one-off' investigations of a complex set of variables. Consequently, the results are very sensitive to changes in the assumptions or in the set of variables. Moreover, seemingly important variables are often omitted. Generally, there are few, if any, other studies of the same phenomena using alternative approaches and examining a reasonably long time frame. This is why we call for the use of a variety of approaches to study specific issues and problems—preferably longitudinally. Practice is likely to be interested in research studies only if there is a good fit between the organisations' and the studies' settings, the findings are robust and stable over time, and their presentation is user friendly.

Most of the subjects on the bottom row of Fig. 3, and also in many other areas of management accounting research, make very intensive information demands on practitioners in organisations. For example, some require knowledge of various aspects of the psychological make-up of individuals or their views on different types of justice. However, such information may be either subject to gaming by individuals or result from the formation of coalitions. These and other demands are likely to mean that the application of research-based models will be expensive and, therefore, only viable for quite large organisations. One reason often quoted for not using activity based costing is that it is expensive.

This suggests that researchers who want to have an impact on practice need to be aware of what is happening in practice and be willing to work on problems which are of interest to practitioners. For example, experimentalists and analytical modellers could focus on current problems which interest practitioners. Surveys could be designed, not just on the basis of pilot studies, but on a thorough understanding of current practices within organisations. Similarly, the theorising which underpins survey research, which currently tends to be grounded in the theoretical literature, could also reflect available knowledge of current practices. Case studies and field-based research, including interventionist research, could help to provide this practical knowledge. This reinforces the point we made earlier about the need for 'conversations' between management accounting researchers adopting different research methods and theories. However, for any of this to work well organisations have to be willing to allow researchers access and this can be difficult in many countries.

These suggestions do not imply a change in either what is perceived as high quality management accounting research or in the search for new management accounting innovations. Researchers will need to continue to meet the expectations of reviewers and journal editors by producing well designed and rigorously executed studies which contribute to existing knowledge. Some journal editors may have problems with papers which reflect current practical concerns, but knowledge of practice properly used in high quality research may change such attitudes. However, it has to be recognised that acquiring knowledge of practice takes time and can be a risky investment for young researchers. Nevertheless, it will help management accounting researchers to have a greater impact in the future.

6. Final comments

Towards the end of the Conference there was a plenary session, chaired by Wim Van der Stede, during which Martin Messner, Alfred Wagenhofer and Paolo Quattrone commented on opportunities for future research in management accounting. Their commentaries, which are published in this *Special Issue*, discuss issues relating to (1) industry, (2) regulation and (3) digitisation.

Martin Messner began by pointing out that it has long been recognised that context matters and that various theories have been used to provide a context-sensitive understanding of management accounting. However, apart from some specific areas of the public sector, notably healthcare, little attention has been given to industry-related issues. He points to a number of opportunities for studying different kinds of industry specifics and their effects on management accounting. He argues that explicit consideration of industry specifics will not only provide an understanding of how different industries work, but could also "offer better explanations for why accounting is practised in the way that it is".

Drawing on examples of regulatory changes in the European Union (EU), Alfred Wagenhofer argues that the increasing regulation which has followed the recent financial and economic crises provides various opportunities for research in management accounting. He points out that, as organisations determine their own management accounting systems, it might seem odd to suggest that regulation opens up opportunities for research in management accounting. However, he demonstrates that EU regulation of corporate governance, together with the requirement for greater transparency and disclosure, provides opportunities for management accounting research. More specifically, the recent changes in the regulation of corporate governance offer opportunities for both analytical and empirical research in a number of areas, and the increasing disclosure and transparency requirements mean that new data will become available for empirical studies.

Another potential area for research in management accounting, and also for the development of practice, is the continuing digital revolution which currently often attracts the title 'big data'. However, in the third commentary, Paolo Quattrone sounds a cautionary note. He points out that whereas the increasing availability of data may lead to *the belief* that better and more rational decision making is possible, it is more likely to increase uncertainty and complexity, which will require the exercise of the considerable judgement which the likes of automated searches cannot provide. Consequently, he argues that, rather than seeing big data as a way of providing, for instance, a better set of performance measures, it should be seen as the basis for establishing a continuing dialogue among organisational actors. As such, the digitisation of accounting will require management accountants to be able to exercise judgement (rather than to possess data-processing capabilities), and how management accountants seek to do this in an era of big data is a potential area for future research.

These commentaries suggest three areas where there are opportunities for future research in management accounting: industry, regulation and digitisation. Undoubtedly, there are many others, but we will not attempt to set them out here. As we indicated earlier, over the years we have not sought to limit or to direct the scope of management accounting research. Instead, we have intentionally kept the scope of the journal very broad to allow the subject to develop through the research published in the journal. Nevertheless, in this Editorial we have suggested some challenges and opportunities for management accounting researchers in the future. Specifically, we commented on the opportunities for conversations across the different types of management accounting research and the need to accumulate a more coherent body of management accounting knowledge. We also pointed to the challenges which management accounting researchers may face in the future in demonstrating the impact of their research beyond the academic journals.

We believe research in the field of management accounting has achieved a considerable amount over the past 25 years, and we have been delighted that *Management Accounting Research* has played a significant role in this achievement. There are currently challenges, there always will be. Probably more importantly, there are also opportunities with considerable potential for the development of management accounting in the future. What will management accounting research have achieved in another 25 years? We cannot tell. However, we believe that there continues to be a vibrant community of management accounting researchers to take the subject forward.

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