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Developing an Organizational Perspective to Management Accounting

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INTRODUCTION

Being asked to make a short speech as part of accepting the award inevitably leads to some introspection and reflection on major factors that have influenced one's career, be they events or individuals. Past recipients of the award have been very influential in putting management accounting on the map. Some have been instrumental in developing the way we think about management accounting and teach the subject; some have developed important perspectives such as information economics, combining economics with psychology, the importance of national culture, activity-based cost management, and balanced scorecards. The way these recipients developed their thoughts is well told in their acceptance speeches and collectively provides a wealth of ideas and a historical perspective on the development of our discipline.

I commenced my studies in economics, and I maintain a keen interest in economic approaches to management accounting. However, early in my academic career, I was pressed into considering an organizational approach to management accounting, not because it was an easier option; rather it addressed the world of management accounting within which I found myself. In this address, I will share with you some personal reflections on the process of my discoveries in the area of organizational approaches to management accounting from the mid-1960s.

The backdrop to this address is that research is something of a growth model. Stage 1: starting from a baseline of skills and knowledge gained at undergraduate studies; stage 2: progressing through refinement of skills with more graduate study; and stage 3: consolidating through work toward publications and other academic outputs. Stages 1 and 2 are somewhat planned and incremental, while stage 3 is more often associated with less ordered processes, often stumbling across potential research areas by way of reading and personal interactions. Research projects can be focused around existing ideas, sometimes being somewhat formulaic, sometimes highly innovative. Often ideas can be serendipitous and develop in unpredictable ways. The key is to recognize when there is a management accounting angle that will relate to an important organizational or social issue.

I was greatly surprised when I received notification from Joe Fisher and Leslie Eldenburg about this award. I am honored and quite humbled to receive the recognition. I thank the selection committee of the MAS Section and the AICPA for the award.

Editor's note: Professor Chenhall is the 2012 recipient of the Lifetime Contribution to Management Accounting award given by the Management Accounting Section (MAS). This note is based on remarks delivered at the 2011 Midyear MAS Meeting and is invited by the editor.

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EARLY INFLUENCES

I initially thought I would pursue a career in science; however, when it came time to enroll in an undergraduate degree, I selected economics. In the 1960s, economics degrees in Australia were focused on macro- and microeconomics with a strong orientation toward public policy. Macroeconomics examined the behavior of entire economies such as overall price levels, unemployment rates, inflation, and the like. Microeconomics studied the behavior of individual decision-making units, be they individual consumers or organizations, and typically how decisions by these units are coordinated by the market mechanism. While there are many crossovers in examining macro- and microeconomics and both are required to educate economists, most students developed a preference to specialize in either macro or micro issues. I had a preference for matters related to microeconomics.

Microeconomics stressed the importance of prices, income, and quantity from the perspective of consumers. Consumer choice was examined by way of indifference curves and budget lines. Market mechanisms were studied mostly assuming perfect competition. Theories related to production functions and cost curves heralded a need to consider the nature of the technology of the firm. Costs were separated into marginal, average, and total costs. Fixed and variable behavior was defined, with the curious idea of marginal fixed costs flagging that costing was deserving of more attention. Marginal analysis and profit maximization required understanding cost behavior, and the various time lines on decisions. Economic profit was seen as cleverer than accounting profit as it included a charge for capital, a notion that sparked much debate later in accounting on issues such as residual income and shareholder value models. Looking back, contemporary management accounting had much to offer these traditional theories in microeconomics as it can unpick the nature of costs and their behavior depending on activity, time, and the nature of decisions involving costs.

In some courses, malfunctioning market mechanisms were considered with the economics of environmental protection, including energy and natural resources, flagging an area that would be important in management accounting 30–40 years on. I found these particularly interesting, but public policy in the 1960s was not as focused on the environment as it is now. Other topics that received limited attention but enough to whet the appetite were the economics of poverty, inequality, and discrimination. This list of issues from basic microeconomics is only illustrative of topics, but there are strong echoes in this 1960s material of research agendas that have captured the attention of management accountants over the past 20 years.

OPENING MY EYES TO MANAGEMENT ACCOUNTING

It was common in Australia during the 1960s, 1970s, and 1980s for individuals to go overseas to undertake graduate education, typically to the U.K. or U.S. However, in the mid-1960s, after completing my undergraduate degree, I was keen to enter the workforce and joined a bank as an economic researcher. I was involved in doing feasibility studies for client firms. After a couple of years, I decided to undertake a master's degree in financial management at Southampton University in the U.K. This was my formal introduction to more advanced ideas in finance, financial and management accounting. The area of financial accounting was heavy with theoretical debate on the meaning of income and asset valuation, with theories of finance being articulated and refined. Management accounting was pragmatic and practice oriented with the principles and practices being articulated in well-crafted books published out of both the U.S. and the U.K. In the main, theories were borrowed from financial accounting and finance to consider issues such as valuation, income, and discounted cash flows. There were some spirited debates on issues related to measuring income within divisionalized organizations and the transfer pricing issues that this might generate.

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Also, management accounting academics provided scholarly accounts on the application of operations research techniques to management accounting issues, such as linear programming and simulation. This generated much excitement but did not seem ever to fulfill its full promise. One suspects that the changes envisaged by the techniques were never accompanied by considering people and implementation issues in innovations driven by formal model building. In the 1990s, management accountants would address these implementation issues, in some depth, when considering changes accompanying the introduction of activity-based accounting and performance measurement systems such as balanced scorecards.

THERE IS MORE TO MANAGEMENT ACCOUNTING THAN TRADITIONAL ECONOMICS

On completing my master's degree, I accepted an appointment at Sheffield University. This opened up a new world of management accounting thinking, for me, being led at that time by Tony Lowe and Tony Tinker. While my background and predilections led me to study management accounting from a traditional economics approach, the Sheffield school was developing a much more sociological and critical orientation. More generally, these ideas were being advanced in the U.K. by scholars such as Anthony Hopwood, Wai Fong Chua, David Cooper, and others. While my background and orientation constrained my motivation to embrace these ideas fully at this time, the approach did open my mind to examining the assumptions behind many of the economic theories I had learned in my economics degree and graduate finance courses.

My curiosity to examine economic theories with modified assumptions can be traced back to my work on feasibility studies and business plans for firms while working at the bank. Assumptions of rational behavior and perfect knowledge did not fit the business folk with whom I worked in both large- and medium-sized client organizations. While I knew that there were works in economics that had developed theories with assumptions that were flexible and realistic, I had not studied these in depth. My growing awareness that organizational context and individual behavior could be captured in solid economic theories derived from work on the behavioral theory of the firm (e.g., Simons 1947; Lindblom 1959; Cyert and March 1963), from economists who examined the behavioral ramifications of growth models (e.g., Penrose 1959; Marris 1964) and the ideas of Williamson (1975) on differences between market and non-market decision making, management, and service provision. These works had theoretical elegance combining economic and behavioral theories and resonated with my state of mind at this stage. Consideration of these works can still provide insights related to management accounting research and organizations, as can more recent work that focuses on economics and psychology.

DISCOVERING THE FOUNDATIONS OF CONTINGENCY RESEARCH

At about the same time, I became aware of another line of research that was examining diversified firms. The work of the business historian Chandler (1962) examined how divisionalized organizational structures were a response to growing diversification in U.S. firms. There followed a series of studies that examined this association between strategy and structure in the U.S. (Rumelt 1974), U.K. (Shannon 1973), France and Germany (Dyas and Thanheiser 1976), and Japan (Suzuki 1980). This was my introduction to the idea that an administrative arrangement might develop to suit a type of strategy. These studies followed an approach that was emerging known as contingency theories, or more correctly, organizational theories following contingency frameworks.

More comprehensive contingency approaches were developed and articulated in the U.S. by Lawrence and Lorsch (1967), Thompson (1967), Perrow (1967), Galbraith (1973); in the U.K., Woodward (1958), Burns and Stalker (1961), the Aston school lead by Pugh and associates (Pugh et al. 1963); and later in Australia by Donaldson (1987). I found that these works combined

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interesting theories that context mattered when considering administrative change, and the ideas were embedded in practice. These works had a profound impact on the study of organizations and still provide important insights relevant to contemporary settings. Systems theory was also popular in some quarters but perhaps became overly complicated when applications were considered (Bertalanffi 1968). However, the seeds of configuration approaches can be seen in this thinking and important ideas, such as equifinality, are relevant to recognizing that different management control systems (MCS) can suit similar contexts.

Around 1973, I returned to Australia, taking up a position at Macquarie University. Awareness of contingency approaches and the strategy and structure literature provided the impetus for my Ph.D., which looked at diversification strategies within Australian business, drawing on industrial economics to examine diversification, and organizational theory to identify elements of context that were associated with different forms of diversification. During this project, I had the privilege of interacting with Danny Miller who was very influential in developing innovative approaches to organizational theory. In this work, I employed a configuration approach using Q-type factor analysis, which was somewhat innovative at the time and something of a forerunner to cluster analysis to discover configurations (Chenhall 1984).

DEVELOPING A CONTINGENCY APPROACH TO MCS RESEARCH

During the 1970s, there was much activity in studying management accounting issues drawing on organizational theories. My own work at this stage involved considering the information needs of managers (e.g., Chenhall 1975, 1976) as well as work on diversification and management controls (Chenhall 1979). By 1980, there was enough quality published research on organizationally based management accounting to enable Graeme Harrison, David Watson, and me to compile a book of readings and textual connections in the area, which we called *The Organizational Context of Management Accounting* (Chenhall et al. 1981). In the main, these were conceptual papers with only a few empirical pieces.

The potential for studying the way organizational context was associated with the design of MCS gained a high profile with the publications by Bruns and Waterhouse (1975) and Merchant (1981). These papers helped put theory-based, empirical contingency styled research on the international stage. Bruns and Waterhouse showed that large firms with sophisticated technologies that are decentralized were associated with a strong emphasis on formal MCS. Merchant reported that large, diverse, decentralized firms used more administrative controls (importance placed on budgets, sophisticated budgets, formal patterns of communications, and participation in budgets).

Also in the early 1970s, Anthony Hopwood published the results of his Ph.D. dissertation, which considered performance evaluation in terms of profit conscious, budget constrained, and non-accounting styles. Results from a study of cost centers showed that profit conscious evaluation is likely to result in higher efficiency (Hopwood 1972). The potential for context to matter became apparent with the publication of David Otley's Ph.D., which showed that Hopwood's results did not hold when considering profit centers (Otley 1978). These publications generated much excitement and were forerunners to an extensive body of work that considered when organizations should develop a "reliance on accounting performance measures" (RAPM). Hartmann (1998) and Otley and Fakiolas (2000) provide critical appraisals of the RAPM literature. While accounting performance measurement and evaluative style remain important, the RAPM concept is somewhat dated with more recent topics broadening the way we think about performance evaluation, such as balanced scorecards that include financial, non-financial, and subjective information. How context affects the effectiveness of more recent practices provides many opportunities for research.

In the 1980s, many management accounting researchers with an organizational interest were studying specific management accounting practices and considering situations in which they were

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most appropriate. Attributes of MCS, such as budgetary participation and RAPM, became popular areas of study with Peter Brownell providing an early lead in the area of participative budgeting. Shields and Shields (1988) provide a review of the budgetary participation studies. Peter was largely instrumental in popularizing the use of interaction terms in regression analysis to understand the impact of organizational context on budgetary participation. This use of interaction terms was fairly novel in MCS research and generated many papers related to budgetary participation and other attributes of MCS. More recently, the shortcomings of the applications were reviewed by Hartmann and Moers (1999), as part of progress in a growing sophistication in the application of statistical techniques to MCS research. Looking back, I feel that the application of this standard approach of discovering statistical interactions went on a little too long and could be criticized for becoming somewhat formulaic and, perhaps, myopic.

GAINING IDEAS FROM THE FIELD

During the 1980s, I developed a fruitful connection with INSEAD, a French business school located in Fontainebleau. Working with Diegan Morris, we had many opportunities to discuss issues of management control with managers on executive development programs and to deliberate on ideas with academics in strategy, marketing, and organizational behavior. From this, we developed ways of thinking about the generic information characteristics of MCS. Chenhall and Morris (1986) defined and measured the construct of information as broad scope, timeliness, aggregation, and integration. The paper was also somewhat innovative, in its time, in that it developed and analyzed a path model for organizational antecedents to MCS of environmental uncertainty, structural interdependence, and decentralization. Thinking about MCS in terms of generic information characteristics enabled researchers to theorize about the impact of MCS by considering the information content. This started something of a cottage industry with many papers being published by authors who included broad scope information, in particular, as the studied MCS variable. Interestingly, unbeknown to me, Gordon and Narayanan (1984) were undertaking very similar work at the same time and were first to market with their article. These types of papers were firmly focused on the impact of organizational variables on the adoption, usefulness, and effectiveness of MCS.

Talking to both practitioners and organizational theorists also led Diegan and myself to identify that effective innovative firms employed open informal organic controls and, contrary to accepted wisdom, formal controls (Chenhall and Morris 1995). These findings were among the first to demonstrate the benefits of studying combinations of controls to understand the potential outcomes of MCS. At the same time, the notion of using MCS in tight and loose ways was being developed elegantly by Simons under the classification of the levers of control framework, particularly diagnostic and interactive controls (Simons 1995). More recently, the combination of formal and informal controls has occupied the attention of researchers interested in innovation and entrepreneurship and provides many opportunities for important research (Davila et al. 2009).

Other works involving INSEAD-based contacts, which had theory driven twists that modified conventional wisdom included showing that in capital budgeting, decisions to include costs based on considering opportunity costs could be reversed in situations in which there was sponsorship bias (Chenhall and Morris 1991); and how the learning effects of post completion audits depended on the uncertainty of the context (Chenhall and Morris 1993). Importantly, these projects had their naissance in discussions with managers and involvement in their organizations.

I had joined Monash University in 1989, which had only a small group in management accounting. In 1992, Kim Langfield-Smith joined the faculty at Monash. I had known Kim as a master's student at Macquarie University and was aware that she had done some interesting work on her Ph.D. related to industrial and organizational psychology, in particular mental models. At

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Monash, the faculty was strong in financial accounting and finance, so I was very appreciative of gaining her as a colleague. We were singing off the same song sheet and had a productive period with work that focused on the organizational dimensions of MAS.

At this time, we were involved in managing an industry-academic group called ACMAD (Australian Center for Management Accounting Development). This was the Victorian Division of the national body established by Bill Birkett at the University of NSW. ACMAD conducted forums with visiting industry speakers, held an annual national conference, arranged benchmarking sessions, funded research in management accounting, and produced a newsletter. These activities provided a good source of contacts with private and public sector organizations and helped us understand the need to keep research relevant. We conducted several studies on innovations in management accounting in the Australian context. The data from one of these studies provided the opportunity to address the concerns of some critics of quantitative organizational work in accounting that too few variables were considered in typical interaction models. The paper of Chenhall and Langfield-Smith (1998a) was one of the first to use a systems approach to relate multiple attributes of MCS to strategy and various contextual variables. While there have been only a few papers using systems approaches, this area holds much promise to pull together how multiple aspects of MCS are embedded within an array of organizational factors.

An event that helped focus the attention of Kim and me on the real world of management accounting and control was our involvement in a large Australian government funded project to review "best practice" management in 40 firms that had received government funding for introducing best practice initiatives, including some in management accounting. This was one of those golden opportunities to undertake research with a large budget and a highly professional group of researchers from the management discipline. We were able to gain insights into the role of performance measures in change programs (Chenhall and Langfield-Smith 1998b) and the application of employee pay in organizational change (Chenhall and Langfield-Smith 2003). These projects involved case studies, which was a divergence in method from my initial training, but was immensely enjoyable. Doing cases provided the opportunity to tease out the subtle effects of multiple organizational contextual variables and the importance of individual factors such as leadership, change champions, and employee commitment. While somewhat messy in making sense of numerous contextual variables, theoretically and empirically, this project provided opportunities for creative interpretation and theory building. A book from this project, Rimmer et al. (1996), was in the top-ten bestseller list in business for about 12 months in Australia. Hopefully, this facilitated flagging the importance of innovative management accounting to a broader management audience than just management accountants. On the basis of this research, I can thoroughly recommend working with academics from other management disciplines, in our case these were human resource management specialists.

SEIZING OPPORTUNITIES IN DOING RESEARCH

On several occasions during the 2000s, my opportunities for organizationally based research involved being in a particular place at a particular time and making contact with a particularly relevant colleague. As a consequence of ACMAD's contacts with the Royal Australian Navy, I was aware of their efforts to introduce activity-based accounting from the early 1990s. Discussing these matters with Ken Euske, who had published work on MCS in the U.S. armed forces, it was apparent that he was also aware of activity-based accounting initiatives in one of the arms of the U.S. defense forces over the same time period. We applied and gained funding from the Australian government and the U.S. defense force to study these initiatives. At this time, studying change and implementation issues was highly topical, and we were able to identify a novel change model that made sense of the experiences of these organizations (Chenhall and Euske 2007). The approach we

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employed was well received by the organizations, as it touched on important issues concerning the differences in the way time is perceived in change programs, with clock time being quite different from individuals' emotional time. Understanding the nature of time as a variable is fundamental in management accounting research that aims to understand change and offers many opportunities for research.

A further example of the way events can present propitious research opportunities was the contacts that my colleague David Smith had with an NGO. Based on early discussions with people within this organization, it was apparent that they perceived social capital as important to their organization and that recent initiatives to become more economically efficient through financial management practices was causing difficulties in preserving this social capital. This provided an opportunity for David, Matt Hall, and me to employ ideas from management control and social capital to understand how management accounting could help or hinder the development of social capital in this organization (Chenhall et al. 2010). It also opened the door to undertake additional projects on the role of management accounting in NGOs. Engaging with NGOs and the context of their operations returned me to some of the most interesting aspects of economics from my undergraduate days. Combining economics with more sociological approaches to study the role of management accounting in NGOs and other not-for-profit organizations would seem a useful avenue to explore.

SOME THOUGHTS ON METHOD

In considering methods in theory-based quantitative MCS research, it is interesting to reflect on developments in the area of organizationally related work. From early descriptive studies, there has been increasing pressure to drive research from solid theoretical foundations. A distinctive feature has been the wide variety of theories that have been employed drawing on traditional and contemporary organizational theory, psychology, and sociology as well as economic theory. While some have been critical that management accounting has been weak in theory development and should concentrate on economics (Zimmerman 2001), others have suggested that this is much too narrow to develop an understanding of management accounting in its organizational and social context (for example, the responses to Zimmerman in the *European Accounting Review* by Hopwood [2002], Luft and Shields [2002], and Lukka and Mouritsen [2002]).

Some argue that in addition to borrowing from other theories to explain how management accounting works and may affect managerial, organizational, or social outcomes, we should develop theories of management accounting (Malmi and Granlund 2002). While this would help make management accounting distinctive, I am not sure what it would look like, given that what we study is about how different practices are embedded within the more generic social and psychological processes that unfold in organizations and societies. However, it will be interesting to see how enthusiasts for a management accounting theory go about developing this distinctive approach. Notwithstanding these developments, I am persuaded that we need to keep abreast of developments in theories in related disciplines that help explain how MCS operate and their effectiveness in contemporary settings.

Data collection in organizational accounting tends to be survey based, archival, or case based. Laboratory studies lean toward a focus on psychological approaches at the individual level, although some studies have direct links to organizational processes and outcomes. These have tended to be well executed.

A popular method has been survey-based research, and most of my early research in management accounting employed survey methods. In conducting surveys, it is important to ensure that attention is given to ensuring that the definition and measurement of constructs are valid and reliable. Recently, the importance has been stressed of ensuring that the meaning and domain of

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constructs are clarified and that the nature of relationships between indicators and constructs is understood, be they reflective or formative (Bisbe et al. 2007). Difficulties in interpreting the budgetary participation and RAPM literatures can be traced, in part, to lack of attention to these construct development and measurement issues. Similarly, care is required in defining the meaning of constructs, such as activity-based costing and balanced scorecards, if we are to build a coherent body of knowledge around these practices.

Given the extensive literature on "best practice" survey methods, such as Dillman's total design approach (Dillman et al. 2008), it is somewhat surprising that many management accounting surveys do not put the investment into gaining the trust and participation of potential respondents. Response rates of 10 percent to 20 percent are not unusual in management accounting surveys. This low response rate need not be a problem if there is no systematic bias in responses, but attention to best practice design can add comfort that respondents are taking the survey seriously. Gaining the participation of busy managers is becoming increasingly difficult, so the easy way of doing a survey by way of ill-prepared postal or internet dumps is unlikely to be acceptable in the future.

Archival data have the advantage of being at arm's length from the researcher and have the aura of being more objective. Often these perceived benefits are gained at the expense of not knowing how the data were collected. Attention to checking the validity and reliability of the data and their prime sources would seem an important, and often overlooked, step when using this method.

Case studies involving the collection of qualitative data have become popular, but they vary considerably in quality. Choosing this method as an easy option is often a mistake of students and junior faculty. Doing high quality case studies requires considerable skills in observation and interviewing and insights to interpret and make sense of the data collected. Despite many excellent examples of management accounting case studies and informative publications in management accounting and in other disciplines that outline method, some case studies in management accounting can be disappointing. In my own research, I have adopted the case method when confronting research questions that require understanding the nuances and subtleties that are involved in the processes of organization and management accounting, such as the role of employee pay in sustaining organizational change (Chenhall and Langfield-Smith 2003), modes of planned change in implementing ABCM (Chenhall and Euske 2007), and the role of MCS in sustaining social capital (Chenhall et al. 2010).

Researchers need to gain and upgrade their skills in any of the data collection methods to ensure they undertake high quality research. This may mean attending lectures in laboratory and survey design and in case study methods, perhaps by way of auditing courses, often in disciplines such as psychology, marketing, or anthropology.

Analysis of quantitative data has steadily become more sophisticated. Early research examined descriptive statistics, correlations, and t-tests. Studies then employed multivariate methods, with regression and the addition of interaction terms becoming popular. As mentioned, these studies were not without their imperfections.

One development has been the application of various forms of path analysis to test causal models involving organizational and MCS variables. Early work employed relatively simple path analysis using regression (Chenhall and Morris 1986), while more recently structural equation models, including partial least squares, have enabled measurement and structural models to be combined (Chenhall 2004, 2005).

In considering contingency studies, there are various ways to examine the ideas of fit (Donaldson 2001). The statistical techniques used in contingency studies are reviewed in Chenhall and Chapman (2006) and, broadly speaking, include interaction, matching, and systems approaches. It is clear that care needs to be taken to ensure that the appropriate statistical test suits the notion of contingency that is being theorized.

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To study multiple aspects of MCS and context using systems or configuration models, the technique of cluster analysis has been used. Developing theory on how multiple aspects of context interact can be daunting, but this approach holds much promise as it addresses the issue of a need to gain a more holistic understanding of various aspects of MCS and their relationship with different aspects of context operating in combination. However, as with other research, there is a need to use theory to justify the selection of variables and how they might be configured, at least in general terms. Otherwise the study can be criticized as being a fishing expedition.

There are other statistical techniques that are rarely used in management accounting but hold promise. These include polynomial regression analysis, multi-level SEM, recursive partitioning, latent class modeling, and neural networks. Approaches such as fuzzy set theory, complexity, and chaos theory can address more complex settings, sometimes using simulation.

Examining case data involves an interactive process between data and the initiating ideas or theories. As the data are collected, the original expectations may be modified or discarded. Making sense of the data often involves researchers using a computer program such as NVivo or Atlas/ti, which searches written transcripts of interviews for words or phrases and other data, such as audio and video files. This can aid identifying who said what and where they said it and identify recurring themes. This is a helpful way to organize data, which can be combined with archival material and possibly participant observation. Eventually, all the data need to be drawn together to discover patterns and meaning. This might be a reiterative process as new meanings emerge and data are examined again to clarify the emerging issues or themes. This can be both exciting and daunting at the same time, but if done well, can provide insights upon which to develop further study.

OBSERVATIONS ON SOME EMERGING THEMES

Some emerging issues can be identified. While there is much to be understood in the way practices such as budgeting and balanced scorecards are evolving, there is a need to study more complex arrangements of management accounting within broader MCS, based on the idea of combining formal and informal practices and processes (Chenhall and Morris 1995). This might include external controls such as social networks as well and formal and informal internal controls (Chenhall et al. 2011). The idea of a package of controls is often a more accurate description of controls than MCS with its suggestion of logically interconnected practices and processes.

In employing organizational theories, attention to the organizational classics (as mentioned above) can provide a solid theoretical foundation for many issues confronting contemporary organizations. The operational environments have changed, the technologies have advanced, the structural arrangements are modified, the human resource management policies are different, and the way we think about strategy has been modified. However, many of the issues raised in the classics still have theoretical relevance.

Additionally, it is important to become familiar with new theories that have emerged in recent years as these provide opportunities to help our research into the processes of management accounting and organization. These include, for example, theories that address incremental and revolutionary change, theories from behavioral economics that combine economics with psychology, ideas on adaptive organizational structures, the implications of advances in IT, and social networking on management accounting. Some of these ideas are reviewed in Chenhall (2008) where organizational and MCS innovations are examined that can assist in developing responsiveness and innovation by a horizontal, flexible, and customer focused orientation. Given recent events during the Global Financial Crisis, we should be exploring more extensively how management accounting relates to corporate governance. Risk management, which has been studied from a variety of aspects, will be relevant to the study of governance. We have much to contribute to planning and control in the areas of resource management, sustainability, and social justice.

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Finally, engaging with others in the disciplines of economics, financial accounting, marketing, operations management, strategy, human resource management, and information technology can be very fruitful and an efficient way of getting up to speed on a variety of approaches to organizational analysis relevant to research in management accounting.

THE ROLE OF CO-AUTHORS

As academics, we often develop our ideas with the help of others, be they co-authors or the supportive efforts of colleagues. I have been fortunate to have had co-authors over the years who have enhanced the research process, making it both rewarding and enjoyable. In management accounting, these include Graeme Harrison, Arthur McHugh, Peter Brownell, Deigan Morris, Lokman Mia, Ken Moores, Kim Langfield-Smith, Chris Chapman, Ken Euske, Josep Bisbe, Frank Moers, David Smith, Matt Hall, J-P Kallunki, and Hanna Silvola. Colleagues who have helped me advance my understanding of management accounting research by way of many conversations or informal advice over the years include Anthony Hopwood, Mike Shields, Chris Chapman, Kari Lukka, Norm McIntosh, Ken Merchant, Bill Birkett, and the management accounting group at Monash University and the London School of Economics. There have been many others who have provided valuable feedback and guidance from time to time, and I could write another paper acknowledging them. Finally, special thanks to my wife, Robyne, for her support, forbearance, and critical appreciation of my research efforts over the years.

I hope this potted history of my experiences has shown how I have observed the management accounting research agenda from an organizational perspective and how organizational issues can affect the development and effectiveness of management accounting. Much of my research has been driven by identifying themes through contacts with organizations and managers and employing a variety of theory driven empirical methods to try and understand how management accounting is implicated in the processes underlying the topic of enquiry.

Once again, my thanks to the MAS section of AAA for conferring upon me this award, which is a great honor that I appreciate, immensely.

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