

IN SEARCH OF MANAGEMENT ACCOUNTING THEORY

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Abstract

In this article we discuss management accounting theory. We discuss the motivation for and role of theory in management accounting, arguing that theories in an applied field such as management accounting should provide explanations that are useful for those we study - managers, organizations and society. We evaluate the nature of theories currently used and developed. Those theories that are considered theories by the research community are largely imported from other social sciences, but have hardly anything that makes them unique to management accounting. Those theories that do not currently deserve the status of theory attempt to explain how to apply management accounting to achieve superior performance. We argue that both forms of theories, at present, largely fail to provide valid support for practitioners. We contend that management accounting theory should help us to answer questions of what methods we should apply, how, in what circumstances, and how to change management accounting. We provide suggestions on how management accounting research could proceed to produce better theories.

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1. The purpose of management accounting theory

Why do we research management accounting?¹ A recent debate provoked by Zimmerman (2001) on the state of management accounting (MA) research and theory (Ittner & Larcker, 2001; Zimmerman, 2001; Hopwood, 2002; Ittner & Larcker, 2002; Luft & Shields, 2002; Lukka & Mouritsen, 2002) provides some answers, but also raises a number of further questions. The following is particularly puzzling: what is the purpose and role of theory in management accounting research? Given that we can answer to this, the next obvious question is do current theories fulfill this purpose and how we should, as an academic community, proceed in our theory development? These are the issues we address in this study.

In their response to Zimmerman, Luft & Shields (2002) refer to their extensive literature review on theory consistent research in management accounting (Luft & Shields, 2003). The review focuses on articles that explain the causes and effects of management accounting. This is to say that the theory of, or theorizing in, management accounting is about explaining its causes and effects. Hopwood (2002, p. 783) refers to the emergence, functioning and impacts of management accounting practices as an object of inquiry, thus extending the scope from causes and effects to include how management accounting is practiced². Although not all accounting researchers subscribe these views (see Baxter & Chua, 2003, for a review of some of this literature; see also Chua 1986), this seems to be the mainstream conception prevailing in today's management accounting research community. But why do we, as an academic community, try to understand the causes, effects and functioning of management accounting? We believe that the ultimate reason for this is to be able to use this understanding, or theory, in creating "better" MA practices, both in terms of content/form and use (cf. Ittner & Larcker, 2001, p.399; Chenhall, 2003, p. 159). The definition of "better" is not crucial here. The argument is that we undertake research and develop theories in management accounting to be used by someone to accomplish something.³

It is several years ago that two studies took as their task to explicitly analyze the issue of how to conduct practice relevant MA research and produce theoretical contribution (theorize) accordingly. Kasanen et al. (1993) argued for constructive research approach in management accounting (see also Labro & Tuomela, 2003) and Kaplan (1998) for innovation action research, both sharing a normative agenda and an aim to contribute directly to practice through scientific MA research. As Kasanen et al. (1993, p.262) summarize it, "...management accounting is, in the end, a practical field where theory without *pragmatic* implications is empty".⁴ These two pieces seem to have had a very limited impact on research practice ever after. The question is: why? Of course there are problems related to normative research agendas – as there are to any research approach – which could explain the lack of studies called for by these authors. One can probably find convenient arguments to object normative research approaches, which we suggest to be only one possible way to pursue in future MA research. But, it is much more difficult to come up with good arguments to object production of practice relevant research results/theories in an applied science like management accounting: "...accounting is fundamentally an applied research area that should ultimately provide new insights for practice" (Ittner & Larcker, 2002, p.788). Indeed, as we will show below, nobody truly seems to object that we should do practice relevant research. Therefore, one conclusion seems

¹ By management accounting we refer to both accounting for decision-making and various forms of managerial control.

² Functioning may be captured in studying causes and effects by the variance in MCS as a result of causes, or as a reason for variance in effects. This interpretation would suggest these authors use different words for similar objects of inquiry.

³ Mattessich (1984, p.2) argues that applied science must be concerned with goals, norms and prescriptive conclusions (see also Reiter & Williams, 2002, p. 581). We will return to this issue below.

⁴ Pragmatism is according to standard definitions a distinctly American philosophical movement founded by Charles S. Peirce and expounded upon by William James and John Dewey. Essentially, pragmatism asserts that if a certain proposition has practical meaning or produces practical results, then the proposition is determined to be true.

to be that there is some disagreement of what is deemed as practical or practice relevant among researchers (cf. Miller & O'Leary, 1990), and especially of how we could produce practice relevant management accounting theories (cf. Mattessich, 1995a, b). We present in this study arguments and critique regarding this issue by leaning on the ideology of pragmatism, according to which practical consequences are the criteria of knowledge, meaning and value (e.g. James, 1955; see also Kasanen et al., 1993).⁵ We also subscribe to the fundamental ideas of Mattessich (1995a, b) regarding the problems of current theorizing in accounting research, although we focus merely on management accounting.

Watts and Zimmerman, in their *Positive Accounting theory* (1986), justify the importance of accounting theory by referring to a number of interest groups that have to make decisions about external accounting reports (p.2). Moreover, they argue that one important criterion for a theory's success "is the value of the theory to users" (see also Demski, Dopuch, Lev, Ronen & Searfoss, 1991). Hence, advocates of positive accounting theory subscribe to the practical purpose for a theory in accounting, although they beware of being normative.

Baxter and Chua (2003) review alternative management accounting research. They summarize the radical alternative (p.99-100) as "...mobilizing research to provide a platform for critique, change and improvement within organizations, in particular, and society, in general". Therefore, in a wide sense the radical (critical) research seems to have a similar purpose of change and improvement as positive and constructive approaches. Although it is difficult to judge the various purposes and motivations of research undertaken in various streams of alternative management accounting research, Baxter and Chua summarize it as follows: "...this legacy enables us to appreciate the limitedness of management accounting inscriptions and the technologies that generate them". It appears that such appreciation of limitedness is required to be able to act upon it. Similar thoughts may be drawn based on Chua's earlier assessment on the interpretative perspective (Chua, 1986). Although explicitly denying the technical application of interpretative science, she argues that "...the aim of the interpretative scientist is to enrich people's understanding of the meanings of their actions, thus increasing the possibility of mutual communication and influence" (p. 615). If our interpretation is right, then more or less all strands of current MA research seem to share a common purpose for theory and research: the aim at the end is to come up with some guidelines or suggestions as to how to apply, or not to apply, management accounting.⁶

Despite its practical purpose, explicit or implicit, management accounting research is often criticized of not having an impact on practice, let alone leading it. Swieringa (1998, p.43) points to the fact that most academic researchers are invisible to broader audiences than just their colleagues (cf. Lee, 2003):

"They do research, publish the results, review the work of other researchers, and build reputations. Academic researchers usually write for other researchers and experts in their field. They are expected to be objective, to meet high technical standards and to subject their work to rigorous peer reviews, and they are concerned about the quality and cleverness of their work and the reactions of colleagues".

⁵ We also lean on our wide experience of theoretical and empirical management accounting research, and comprehensive experience of working with practitioners.

⁶ Organization theorists seem to share this view for theory to be useful for users (other than researchers). Hinings & Greenwood (2002; see also Clegg, 2002; Bazerman, 2005) argued that originally the research agenda in organization theory (or organizational sociology) was: what are the consequences of the existence of organizations? After the field became business school driven, the question has been: how to understand and thus design efficient and effective organizations. Although the shift has been from policy making to management, there has always been an assumed user for the analysis and developed theory. Hambrick (1994) called for relevance in the field of management studies, arguing that "our responsibility is not to ourselves but to the institutions around the world that are in dire need for improved management, as well as those individuals who seek to be the most effective managers they possibly can be."

We believe this orientation towards colleagues, not external users of our theories has partly contributed to the situation where academic management accounting research has had a very limited effect on practice during the last few decades⁷. As considerable amounts of state and other funding are granted to perform research and education in the area, it seems that Swieringa's description of the role of accounting researchers is not feasible in the long run (cf. Hambrick, 1994). It should not be in anybody's interest to generate research articles that, at worst, nobody cites (this is not tied to the position of the publication outlet in journal rankings) and the results, if any, of which could not be of less interest to practitioners. We do recognize, however, the underlying tenure and elite systems as well as the myopic university and education program rankings that perpetuate this process (prospering in the US in particular; see Lee, 1997, 2003; Lee & Williams, 1999). Therefore, we understand that such change cannot take place overnight. But we do suggest that changing partly what we perceive as the purpose and role of theory in management accounting may aid in making management accounting research relevant to a broad audience. As Bazerman (2005, p.29-30) puts it: "...there are barriers to the perceived efficacy of non-economic social scientists. [...] These are real tradeoffs and barriers. However, I believe the primary barrier to having influence is the failure to even try".

Before we move on to discuss what is theory, we should revert to the very beginning of the study, because in the end the question of why we research management accounting is an epistemological one. The different strands of research referred to above derive the conclusion that the aim of research at the end is to come up with some guidelines as to how (or not) to apply management accounting from different directions. This is because underlying their knowledge creation processes we can find different interests of knowledge and thus different epistemological assumptions (Habermas, 1971; see also Lukka & Granlund, 2002; cf. Latour, 1987).⁸ The *technical interest* of knowledge is known to dominate in the natural sciences. According to Habermas (1971), natural sciences are seen to be meaningful and their results valuable because they make it possible to forecast and control the events of reality. Many fields of social sciences, including economics, largely share a technical interest of knowledge. This is an appropriate objective also regarding the arguments of the present paper, since guidelines or suggestions as an ultimate outcome of research refer directly to the ability to control events.

The *practical interest* of knowledge aims for a profound understanding of cultural phenomena and an enhancement of our ability for self-reflection through communication. While not denying the merits of research committed to this, in light of the arguments presented here this may not be enough alone: understanding a phenomenon – how ever deep it be – does not produce directly usable results for various user groups. However, such understanding could be developed to guidelines or suggestions; it could be combined into the agenda argued for in this paper, as, for example, understanding the effects of organizational power distribution may well be relevant for certain types of management accounting theories. One could probably also claim that at best this interest of knowledge may provide practitioners with new insights to be applied in their own problem solving, though the problem still being how to make practitioners aware and to care about such research results. So while not saying that all earlier research would be impractical and not trying to contribute to practice, we think the success in pursuing this has been very limited or underwhelming.⁹

⁷ A more complete account of various reasons why research has had limited impact on practice is beyond the scope of this paper. Lee (2003) provides one recent and persuasive account on this (see also Inanga & Scheider, 2005).

⁸ The fact that epistemology is concerned with what is true (adequate) and what false (inadequate) knowledge, i.e. how knowledge accumulates, practically translates into the question: how can one develop theories that are better than competing theories?

⁹ Habermas (1971) identifies also a third interest of knowledge, the *emancipatory* one, which can also be found in the field of accounting research (though maybe today to a lessening degree). This interest of knowledge is driven by the aim to gain a consciousness about the illegitimate modes of repression as well as by the attempt to dissolve them,

Our position in this paper is in favor of the technical interests of knowledge, although we do acknowledge that research based on other interests of knowledge may at best provide potentially useful insights in developing theories that are in the interest of, and useful for the examined people/organizations.¹⁰ Ideas presented in this paper are still likely to be most relevant for those sharing the technical interest of knowledge. However, adopting this interest of knowledge does not mean that all the (ontological) assumptions prevailing in natural sciences will apply in social sciences, even though the interest of knowledge may well be the same. Predictability and controllability can in our view apply at several levels and also vary with regard to intensity. Therefore it is important to also acknowledge the many local, contextual meanings and influences that may be at work as we analyze and build theories of the uses and effects of MA systems.

As a starting point for the remaining analysis, we regard management accounting as an applied science, where the aim of research and theory in the long run is to facilitate attempts to make organizations and societies better off. There seems to be widespread concurrence to this, but much less accord on how we should proceed towards that aim. In the next section we will define what we mean by theory. We then assess briefly what type of theories we have at the moment to help us in building better management accounting practices and what the main shortcomings of those are. This is followed by what we see should be the core of management accounting theory, or a set of theories. We then provide some avenues to develop such theories. Finally, we summarize the main arguments.

2. What is theory?

Social scientists have had troubles in defining what theory is (Sutton & Staw, 1995; Llewelyn, 2003). There is also great debate about what kind of theories we should be looking for in social sciences in general (about the “Science Wars”, see e.g. Flyvbjerg, 2001). Although some authors, like Flyvbjerg, argue that social science has set itself an impossible task when it attempts to emulate natural science and produce explanatory and predictive theory, we are inclined to think that while this may hold for some strands of social science, it requires reconsideration with regard to applied streams of science, such as management accounting.

Weick (1989) quotes Sutherland in defining what we mean by theory - “an ordered set of assertions about a generic behavior or structure assumed to hold throughout a significantly broad range of specific instances”. This corresponds fairly well to a standard dictionary definition: “a coherent group of *general* propositions used as principles of *explanation* for a class of phenomena” (Webster’s; our italics). We define theory in similar lines. Theory is understood here as a coherent set of propositions used as principles of explanation for a class of phenomena assumed to hold throughout a broad range of specific instances. However, we refer neither to a significantly broad range of instances nor general propositions, as these words tend to support the idea of “grand-theory” or theory as covering laws (DiMaggio, 1995). We believe that our field of inquiry, management accounting, is relatively heterogeneous across industries, cultures, firm life cycles,

ultimately leading to criticism towards predominant ideologies. Modern management control techniques are regarded in this context as one of the instruments used to support the prevailing status quo in society (cf. Hopper & Powell, 1985). Practitioners easily neglect such an approach, typical of critical social analysis: provision of mere criticism without suggesting concrete remedies is undoubtedly unappealing for people aiming to solve an imminent problem (Lukka & Granlund, 2002)

¹⁰ We are neither saying that we should abandon other types of research agendas as those suggested in this study. We are rather suggesting that there should be something else too: something that we see practically unavoidable in the long run. It may be an extension or modification regarding current research agendas, or it could be a new agenda to be pursued alone or side by side with the existing ones.

organization size, and so on, and is likely to benefit from a broader set of theories instead of one grand theory.

Whetten (1989) argues that a complete theory must contain four elements. The first is which factors (variables, constructs, concepts) logically should be considered as part of the explanation of the phenomena of interest. The second element is concerned with how they are related. Together what and how constitute the domain or subject of the theory. In research papers providing formal models, constructs are boxes and relations are arrows.¹¹ The third element relates to the question why. What are the underlying psychological, economic, or social dynamics that justify the selection of factors and the proposed causal relationships? The fourth element focuses on the conditions that place limitations on the propositions generated from a theoretical model. Questions such as who, where and when address the temporal and contextual factors that set the boundaries of generalizability.

This view is a more detailed account of what theory is than the definition we provided above. We do not regard these accounts to be contradictory in any way. For us it appears that this view provided by Whetten (1989) is more or less the view held by most positive accounting researchers. However, sometimes accounting scholars seem to refer to the theory being simply the psychological, economic, or social dynamics providing answers to why questions. Our view on theory is akin to that of Whetten and we could apply equally well Whetten's account to define what we mean by theory.

Watts and Zimmerman (1986, pp. 7-9) make a clear distinction between positive and normative theory. Theory for them yields no prescription for accounting practice. They argue that prescription requires the specification of an objective and an objective function. In principle, we agree that prescription requires the specification of an objective and an objective function. However, we argue that most of the economics based management accounting research assumes implicitly the objective of economic efficiency or shareholder value maximization. Therefore, theories capable of explaining and predicting how certain forms and uses of management accounting 1) lead to decisions that are in line with these implicit objectives, 2) increase goal congruence and 3) ultimately financial gains are likely to provide prescriptions relevant for business managers pursuing such goals. Moreover, we argue that researchers should develop management accounting theories devoted to value maximizing, theories devoted to social equality, theories devoted to environmental sustainability, etc. In other words, we do not believe that there can be only one base theory in management accounting we can build on just by varying our objective function. Research on management accounting would be better off if researchers would clearly assume and explicate an objective and objective function, and build theories to support that objective. To avoid any possible misunderstanding it is perhaps important to note that all this does by no means imply that we as researchers should operate purely on the conditions of the research objects. The above neither means that we could simply assume objective functions and choose between them according to our own values. Our view is here in line with Flyvbjerg (2001), who suggests that researchers can carry their own values and mobilize them with consideration in research: even to question the assumptions and intentions of the examined organizations (Jönsson & Lukka, forthcoming). The conclusion would be that researchers should assume an objective function so that their own values would not be offended while developing theories. Values and ethic codes, for instance, are reflected already in what kind of problems the researcher wants to examine in the first place, and how they are to be solved.

To sum, even if we call for theories to be useful in practice, we do not mean that theories need to be normative as such. Theories capable to explain and predict are likely to provide practical insights given that they deal with the issues that are of practical interest, i.e. explain and predict phenomena

¹¹ These elements of theory are illustrated in the summary maps by Luft & Shields (2003).

related to practitioners' objectives. On the other hand, the definition of theory as a coherent set of propositions used as principles of explanation for a class of phenomena assumed to hold throughout a broad range of specific instances does not specify the nature of the set of propositions. These propositions may as well be normative, including objectives.

3. The types of theories in management accounting

Research and practice in management accounting relies largely on two types of theories. Those theories that are considered theories by the research community are imported mainly from other social sciences, but have hardly anything that makes them unique to management accounting. Those theories that do not currently deserve the status of theory attempt to explain how to apply management accounting to achieve superior performance. Let us address them both in turn.

3.1 Theories that have theory status

Based on Luft & Shields (2003), in management accounting research a number of MA related issues are explained by a number of theories. Typically MA is either an independent or a dependent construct, but the whole causal chain is not addressed. Theories applied to explain causes, effects and various interrelationships of MA are mainly from the fields of economics, organization theory (contingency theory), sociology and psychology. There is nothing wrong with this; we need these theories and many MA issues of interest can be explained by these theories. It is interesting though to think about this practice in terms of Whetten's definition of theory. These independent borrowed theories have their own initial constructs, relationships, explanations and assumptions, depending what is the phenomenon they are about to explain. Some of the explanations and assumptions (but not constructs and relationships) are then applied to explain MA constructs and relationships. In other words, we borrow only parts of these theories, mainly to provide answers to why questions. Again, while this in itself is not necessarily problematic, we have four main concerns with current theorizing in MA.

First, none of these theories is something we could build on if we wish to distinguish MA theory from some other theory. For us, these seem to be theories about management accounting, not theories of management accounting (Humphrey & Scapens, 1996). Management accounting theory, as judged based on the publications in "top tier" academic research journals, is determined by the object of inquiry. Researchers use principal/agent theory, information economics theory, structuration theory, actor-network theory, goal setting theory and a countless number of other theories to explain issues of interest in management accounting. Sometimes it seems that what is interesting is determined by the pre-adopted theory (and/or method tool-box), not the accounting phenomena of interest.¹² These same theories can be used to explain a number of issues other than MA. We argue that there is a need for theories of management accounting. This is not to say that we do not need the explanations provided by and insights derived from these other theories. We certainly do. But we argue there is room and need for both.

Why do we need unique theories of management accounting? We believe that these borrowed theories, even when applied to explain management accounting related issues, provide little insight on practical issues as such. A set of unique theories of management accounting would be closer to practitioners' concerns than the current theories used in management accounting research. Thereby

¹² See Reiter & Williams (2002) and Mattessich (1984) for positive economic theories, and Scapens (1990) in the context of case studies informed by sociology. In addition, in the mainstream research tradition the quality criteria for research are borrowed from natural sciences. This has led to situations where especially statistical generalizability has been used as "an amulet" to demonstrate contribution, while at the same time the economic significance of such statistically significant results may have remained practically non-existent (see Lukka & Kasanen, 1995).

theories would be easier to communicate (directly) to various user groups. Furthermore, this would mean more influential research with (direct) practical effects. The consequence would be a stronger identity to management accounting. This should significantly enhance the acknowledgement and respect of management accounting research outside the academia. It is sometimes depressing to encounter managers dealing with management accounting issues that do not know that management accounting research even exists. Furthermore, perhaps this would also attract new good quality students to our field.

A limited insight for practitioners is the second concern we have with current theorizing. Let us use a recent summary on contingency research as an example of how current “science” based theories fall short in providing practice relevant guidance. We use the contingency framework as a frame of reference as we believe it is fairly explicit in its aim of providing insights to practice and as we see theories of management accounting necessarily contingent (as opposed to covering laws). Chenhall (p.138) provides the following summary concerning research findings relating MCS to the external environment:

1. The more uncertain the external environment the more open and externally focused the MSC
2. The more hostile and turbulent the external environment the greater the reliance on formal controls and an emphasis on traditional budgets
3. Where MCS focused on tight financial controls is used in uncertain external environments they will be used together with an emphasis on flexible, interpersonal interactions.

Although we understand this is only a summary, it persuades us to ask what it tells to managers. The first two propositions claim that both externally and internally focused management control systems are observed to exist (to be useful?) in uncertain and/or turbulent environments. So any system that has external focus, or internal focus, would do? It does not matter how systems are used? This is true in all industries? These claims are also contradictory, unless we consider uncertainty and turbulence as clearly distinct phenomena. If we assume that firms are optimizing their MCS and that the observations reflect good practice to follow, the first point suggests that as uncertainty increases, more emphasis should be placed on externally focused systems (more emphasis with respect to what? More than internally focused?). The second one proposes that more emphasis should be placed on formal controls and traditional budgets. Of course, they may both hold at the same time, indicating that as uncertainty increases, both should receive more emphasis (though, more with regard to what? With regard to a stable environment? Is this not too obvious?). The third proposition is a bit more useful for practitioners suggesting that in uncertain environments tight financial control needs to be supported by informal interactions. Our argument is that these propositions or findings are so general, and partly self evident, that they are of little use or incremental value in practice. There is a need to advance from this stage to be able to argue that as uncertainty increases, certain forms of MCS used in a certain way would provide better decision-making support, or more likely achievement of goal congruence.

The third major concern with current theorizing is the “meta theoretical” nature of some of the used theories. The problem with meta theories is that you cannot falsify them, or you cannot even generate propositions based on them which could be turned into a falsifiable form. For example, Giddens’ (1984) theory of structuration, a meta theory, points us to look for certain issues, such as the relationship between norms, resources and power, in organizational and societal practices. There is nothing wrong with this per se. However, as Giddens himself says, it is not necessarily the task of “meta theorists” to examine practice: he leaves this to others. But how that should be done in a management accounting context? Giddens, for instance, seems to have no preference for research methods to be applied in different circumstances. At worst, the application of such meta theories leads to accounting research where empirical practices are described and explained using complicated concepts (totally strange to practitioners) and constructs borrowed from these theories.

Often such use of meta theory does not make the complicated world more easy to comprehend, let alone manage; rather, it may add confusion. Although the merits of research applying meta theories are to be acknowledged (though they are debatable) and such theories may be useful at the early stages of research projects, we argue that there is also need for an other research agenda. In this agenda meta theories should be left in the background at subsequent phases, and emphasis should be placed on how to capture the praxis into easily understood concepts and models.¹³ We do not mean here that praxis as such would be simple or easy to model, but rather point to the communicative aspect; how to present issues so that they are as comprehensible as possible to practitioners.

Fourth, we do not have a clear picture of which one(s) of these theories provide(s) the strongest explanations for various MA forms and outcomes in a given time frame and circumstances. For example, economics based theories explain the existence of MA practices as means to enhance efficiency. Political theories explain these practices by referring to various interest groups and their struggle for power. Institutional theories provide signaling as an explanation as firms seek for legitimacy from their stakeholders. Empirical research has demonstrated that all these have had an impact on management accounting practices. But do we know when each of these explanations is likely to be valid? Are these mutually exclusive? And even if we can answer these questions, so what? In other words, even if we understand that accounting is practiced for various reasons, what are the implications of this knowledge? Do these various reasons or motives to adopt practices have an impact on how these practices are implemented and what the expected benefits are? In the case where we do not understand the relative power of various theories explaining the same phenomena, we do not actually understand the issues surrounding MA in sufficient depth (and breadth) to be able to provide clues about how to apply MA. One may argue that this is only a question of insufficient accumulation of research knowledge, and will be defeated as the field matures. We doubt this as we seldom see attempts to address the strengths and weaknesses of various theories in a single study (see e.g. Ittner et al., 2003, for an exception). Accounting scholars seem to believe one theory at a time, and make few attempts to modify or further develop, let alone reject those borrowed theories they use (see also Reiter & Williams, 2002). If we are not willing to develop theories we borrow from elsewhere to better explain issues we are interested in, and if we are not prepared to assess the relative strengths and weaknesses of various theories, there is little hope for coming up with practice relevant management accounting theories¹⁴. We reject Zimmerman's (2001) solution to rely on only economics based theories (see also Lukka & Mouritsen, 2002); for us it is once again an example of putting the carriage before the horse. Rather, the explanatory and predictive power of various theories needs to be assessed, and theories, whatever the origin, need to be developed, modified or combined to increase their explanatory power in a management accounting context.¹⁵

3.2 Theories that do not have theory status

In management accounting we also have a number of “normative” theories or constructs. These include activity-based costing for overhead allocation, Balanced Scorecard for control system design, Quality Costing framework to manage and reduce quality costs and Value Based Management framework to guide decision-making and control to ensure shareholder returns. These

¹³ DiMaggio (1995) discusses three views on theory. The use of some of these meta theories may conform to the view of theory as enlightenment, where theory is complex, defamiliarizing and rich in paradox.

¹⁴ In their review, Luft and Shields (2003) considered only studies that found support for the proposed hypothesis. Although probably a reasonable choice for the review purposes, it may be seen to illustrate the interplay between theory and empirical data in current management accounting research. If the data does not support the hypotheses, then it is not the fault of improper (borrowed) theory, but inappropriate data or methods.

¹⁵ Many of the remarks we make here regarding the first and the fourth concern are tightly connected to the earlier mentioned academic systems (e.g. Lee, 2003).

are not regarded as theory by academic researchers, although these theories claim how we should be doing something and why. Zimmerman (2001) argues this as follows: “*while it (VBM) resembles various theories, it is not a positive theory in the sense that it neither explains nor predicts firm-related phenomena*”.¹⁶ Even if we accept Zimmerman’s quest for positive theory, we disagree with his view that VBM neither explains nor predicts firm-related phenomena. It could be argued that the VBM framework aims to explain firm performance, measured by shareholder returns. Adherence to six managerial steps forming the core of VBM (Ittner & Larcker, 2001; Zimmerman, 2001; Martin & Petty, 2000; Morin & Jarrell, 2001) explain why one of the otherwise similar firms perform better, or why a performance of a certain firm improves over time. This same logic can be extended to prediction, and these predictions can be refuted by evidence.

Recall our definition of theory: a coherent set of propositions used as principles of explanation for a class of phenomena assumed to hold throughout a broad range of specific instances. But is this not exactly what VBM proponents claim? There is a coherent set of propositions regarding how organizations should be managed in order to maximize shareholder wealth, which are assumed to hold in a broad range of instances. These propositions of VBM are not tested, but it can still be seen as a theory. Watts and Zimmerman (1986) contend that the objective of accounting theory is to explain and predict accounting practice (p.2). Sure, VBM is not a theory of accounting practice but a theory of organizational performance, including accounting related issues as a mechanism of explaining outcomes. We can even argue, that as VBM literature explains the mechanism by which better performance is about to emerge, it deserves a status of theory more than some other so called theories, that we use to predict outcomes (usually with low R squares), but which treat the actual mechanism (i.e. firm) as a black box (e.g. S-curve models in innovation diffusion literature).

How does VBM comply with the four elements of theory as discussed by Whetten (1989)? We may think that the six managerial steps provide a frame for a managerial construct, organizational performance being the other major construct in this theory. This managerial construct consists of managerial choices and actions, including management control related choices. We may call the main choices and actions illustrated in VBM literature as sub-constructs. Six managerial steps now suggest which values those sub-constructs may get in firms practicing VBM. For example, step 4 (see Ittner & Larcker, 2001) in VBM theory argues that action plans, performance measures and target setting should be based on identified value drivers. In practice, performance measures may be defined relying on e.g. value drivers, strategy, TQM framework or they may be ad hoc. Performance measures can now be considered a sub-construct and part of the managerial construct. Values for the performance measure sub-construct may vary, including e.g. “measures based on value drivers” or “measures defined ad hoc”.

The relationship between the managerial construct and organizational performance is simple: If all these managerial steps are taken as suggested, or more precisely, if all values of sub-constructs are aligned with VBM theory, firm performance will improve. Selecting some alternative methods to accomplish these managerial tasks will produce less shareholder value.

Why does this relationship between these constructs then exist? We do not suggest that this theory is unambiguous about why these steps are to produce better performance; though we consider the explanation fairly simple and straightforward. Considering the management control part of it, for example, it seems that expected benefits are based on an assumption that you get what you measure. Moreover, making sure that measures reflect value creation (either outcome or drivers) should ensure that the system creates right incentives for managers to behave in the best interest of

¹⁶ Ittner & Larcker (2002) echo this in their reply by saying “we did not intend this framework to be a theory of managerial accounting practice, or a complete depiction of the many economic and non-economic factors affecting managerial accounting choices and consequences.”

shareholders. Further, given that bonuses and career development are linked to the achievement of such targets, these measures should provide further assurance that managers take actions in the best interest of shareholders. Is this then an economic or psychological explanation? We think that should not be the main concern. The concern is to develop an explanation that reflects reality, whatever the reasons are: rational, emotional or something else.

Finally, and very importantly, there is no attention to conditions in which these propositions may hold. We agree with Zimmerman (2001) that this theory is presented as if all firms should follow it. This is why we regard it as of limited value for practitioners as it stands. Rigorous research should now provide support for these arguments, or show where its limits lie. This would lead to refinement or rejection of this theory. It may be obvious that it becomes rejected as a universal truth, but do these steps hold under particular circumstances. Are all steps required to secure success? What are the necessary and sufficient conditions for assuming that VBM would lead to better performance? That type of theory explication and refinement would be useful for practice as well.

Zimmerman (2001) also makes a case against VBM being a theory by arguing that it does not make predictions about when particular compensation schemes will be used or what firms are most likely to adopt Activity Based Costing (ABC). We are not quite sure what Zimmerman is expecting from a theory, but it seems that he is after a single theory that could explain everything. We may need one theory to explain why firms adopt ABC, another theory to explain how overhead allocation is to be done to enhance decision-making and yet another theory to explain which managerial steps are required to enhance shareholder value. These all may be based on economics, or not, but even in economics there are different theories to explain different phenomena.

Let us take another example from recent management accounting literature, ABC. What the proponents said in the early normative writings is that poor performance may be explained by the use of overly simplified cost accounting models. They went on to show how assignment of overheads based on volume distorts product costs and leads to wrong decisions. Hence, they explained the mechanism producing poor performance. A number of other things may explain poor performance, but cost assignment issues are definitely something accounting researchers and practitioners should be concerned about. They suggested assigning costs based on causality instead of relying solely on volume. ABC may be thought of as a theory of cost accounting if we consider that following certain cost assignment procedures accounting produces more useful information for managerial decision-making. Improved decision-making is assumed to lead to better performance. Hence, theory of ABC, or more broadly, theory of cost accounting explains how cost accounting should be done, and why, to assure better performance. Even at its normative phase (which was grounded on inductive case observations and/or consulting assignments), theory claimed that cost assignment principles explicated as ABC are suitable for, and eliminate certain costing distortions in, organizations with complex product mix, complex production process and large proportion of overheads. Management accounting research has so far provided a number of valuable insights on whether these limitations hold and what are the other possible limitations. Based on research, we may today predict which firms are most likely to adopt ABC (certain cost assignment procedures) as hoped for by Zimmerman (2001). Note also that based on current understanding these adoptions are explained both by economic and institutional factors, institutional factors clearly not being an issue in the early normative writings (see Malmi, 1999).

ABC may also be thought of as a tool, not a theory. A situation where ABC refers to both theory and a certain set of practices is perhaps not desirable. Therefore it might be better to refer to the theory of cost accounting, or theory of product costing, instead of theory of ABC. Theory of cost accounting would explain how overheads are to be allocated in different circumstances to provide best possible decision-support. It would also explain how overheads are to be allocated under

different circumstances given that resulting figures are used for achieving goal congruence or to support signaling. ABC as a tool would be one method for accomplishing this. Plant wide overheads, for example, would be an alternative.

To sum, we argue that an attempt to establish scientific identity for management accounting by borrowing theories from other fields has led us to an identity crisis as a scientific discipline (Reiter & Williams, 2002, argue more or less the same for financial accounting). There is no theory unique to management accounting that is currently considered scientific by the international research community. What we do not understand is why academic management accounting community cannot be proud of practice oriented MA theories?¹⁷ These are something that distinguishes us from economists on the one hand and from organization theorists on the other. These are something practitioners can pursue in their attempts to improve their organizations. This is not to say that these theories should be accepted and followed as such. The boundaries of these theories need to be explored and new theories developed to meet the requirements that society sets for an applied science like ours – to facilitate developments within society, including organizations.

4. What should be the core of MA theory

So far we have argued for management accounting theory that would be both useful in deriving guidelines for practice and somehow unique to our field. What should be the core of such theory? From the managerial perspective the answer is fairly straightforward. First, theory should consist of assertions of what accounting and control methods we should apply, how, and in what circumstances (see also Ittner & Larcker, 2002, p. 788). By *accounting and control methods* we do not simply mean ABC, BSC, VBM or some other existing or emerging normative theories arguing certain methods lead to better performance. Accounting and control methods may refer to some commonly used categorizations such as reliance on financial performance measures, non-financial measures or both; reliance on technocratic controls, socio-ideological controls or both; using simple or sophisticated cost allocations, etc. *How* may refer to using these systems diagnostically, interactively or both; using them for decision-making, control, or both; using them as supplements or compliments to other methods, using them in the top of the hierarchy, in the bottom of the hierarchy or in the whole hierarchy, etc. *Circumstances* include the objectives of management accounting (enhance efficiency, environmental compliance, etc.), traditional contingency variables, institutional forces, political issues, economic factors, individual factors, historical factors, etc. The word “should” above, suggests that there is an assumed relationship to performance. Note that also performance may be defined differently in different circumstances; different objectives require different assessment of achievement and proper performance for government organizations is likely to be different from that of private sector organizations.

In a sense, what we are claiming is that management accounting theory should be a set of propositions of how to organize accounting and control practices under given circumstances. If we take performance management and management control systems as an example, Otley (1999) provides a general framework for how to conduct research in the area as well as what are the questions we need to be able to answer based on our research and theorizing. The framework he proposes contains five steps fairly similar to those six used by Ittner & Larcker (2001) to categorize their literature review. He poses the questions in a managerial tone, which is akin to our argument about the role of theory in management accounting. Otley’s five questions focus on organizational objectives and the evaluation of their achievement; strategies, plans, processes and activities to

¹⁷ We do understand that management accounting has developed in line with financial accounting, dominated by economic positivism and a small group of powerful US academics. The shift in the 1960’s into the “scientific mode” explains where we stand at this point. But how long do we have to continue this scientific agenda just to prove to some other academic fields that we really do science?

achieve those objectives as well as the assessment of the success of these means; the level of performance in the previous two as well as the way to set targets on those; rewards or penalties that are associated with the achievement of these targets and the information flows that enable learning and adaptation. If we would be able to come up with a coherent set of propositions on how to respond to each of these questions in a given situation, we would have a managerially useful, yet disciplinary distinctive theory of how to use MCS to achieve superior performance. It is not clear though whether Otley's framework would work here as such (alone), as it tends to be somewhat limited in explaining how the five questions interrelate. But, it could serve as a guideline to start with and thereafter be developed to be more specific about the interrelations in various contexts.

Second, theory should consist of assertions of how we should *change* management accounting. Why a change might be appropriate is already covered by the questions above, while here the focus is on implementation. The word "should" refers also here to performance implications or success of such change attempts. Various implementation success and failure related factors provide a good basis for such theory, although there is a lot more work to be done to link these factors to each other and various circumstances in which these change attempts may take place.

We suggest that management accounting researchers attempt to develop not a single theory of management accounting, but a set of theories able to explain the form, use and change of accounting and control methods in a range of circumstances. We would like to emphasize once more that we do not mean that all management accounting researchers should subscribe to what we say, or should shift to develop and test the type of management accounting theories we propose. But we do think that in general, more emphasis should be devoted to this type of theorizing than is currently taking place. Note also that we are not saying anything about the suitable theoretical starting points towards these types of theories, nor do we have any particular view of preferred methodologies as such. The main argument is simple: let us try to build management accounting theories that are useful for accounting practice – preferably in the very form in which they are published.

5. How research should proceed to produce better management accounting theories?

We can identify three main avenues to produce theories that are eventually useful in practice. The first one is to alter some current practices when conducting traditional research based on "scientific" theories, whether relying on positive tradition or when following alternative research approaches. The second is to take normative theories as a starting point. The third is to rely more on the interventionist research approaches.

5.1 Traditional research

We used part of the summary of contingency literature (Chenhall, 2003) above to illustrate how current research falls short in providing meaningful advice to practice. We argued that there is a need, for example, to be able to argue that as uncertainty increases, certain forms of MCS used in a certain way would provide better decision-making support, or more likely lead to the achievement of goal congruence. Such claims could be based on theories, which explain and predict the outcomes of various forms and uses of management accounting in given circumstances. There is nothing new in this argument. However, as results of this research paradigm have not been compelling so far, we might need to re-think how we are approaching it.

There are three main suggestions, and a number of supportive ideas for traditional research to progress. One of these we have raised already above. We argue that researchers should assume objectives and objective functions, and build theories that support these objectives. Second, we

argue that we should relax the assumption that all firms optimize all the time. We think one source of confusion between Zimmerman's view and the one propagated here is due to different assumptions of equilibrium. Positive accounting theory as presented by Watts and Zimmerman (1986) is concerned with accounting choice. An assumption is that there is no need to study the link between these choices and performance, as decision-makers are assumed to maximize their utility. Hence, we should not expect to see any performance effects due to accounting choices if all firms are optimizing all the time (cf. the potential problem of endogeneity in MA research, see e.g. Ittner et al., 2002). Similar assumptions characterized early contingency literature assuming selection fit (Gerdin & Greve, 2004). Research on management accounting provides evidence that this might not be the case. It has been suggested that MA practices are adopted due to a number of reasons, economic utility being but one (albeit important). It is not our task here to evaluate these different claims and empirical support provided for them. Rather, we argue that if we relax one of the fundamental assumptions Watts & Zimmerman make, research needs to incorporate the link between accounting choice and performance to be complete and useful. This is exactly what many researchers have done already and what both Ittner & Larcker (2002) and Luft & Shields (2003) argue.

Third, we would also urge researchers to utilize more comprehensive managerial or control systems, such as VBM discussed above or control systems packages (e.g. Otley, 1980, Fisher, 1995)¹⁸ as a unit of analysis. The underlying reason why a certain MCS element in isolation is expected to have certain effects is probably not the most puzzling issue. Instead, why certain combinations work together and others do not, in various circumstances, needs more attention. In other words, we are suggesting that instead of trying to provide economic, psychological or some other explanation for atomistic relationships e.g. between MCS elements and performance, we could try to build up theories containing more complex MCS constructs and explanations of why those combinations are likely to produce certain outcomes in certain circumstances. These explanations may build on various established theories borrowed from related fields. However, as our MA constructs get more complex, and hence realistic, these combinations of established theories required to explain the outcomes are likely to become unique to management accounting.

It would be easy to argue that as there are an infinite number of factors and their relationships in a complex world, this type of theory building is doomed to fail. If this is the conclusion, however, we have two alternatives. The first one is to close most of our research programs and start looking for better and more productive jobs. If we are not able to produce any meaningful advice to practice, or if the best we can do is to provide further proof of "taken for granted" claims, it is our responsibility towards the rest of society to argue that money spent on management accounting research would be better spent on for example medical research, engineering sciences or on humanistic sciences without any technical imperative. The second alternative is to start from "normative theories". We can either start to develop and refine existing normative theories based on empirical research, or to start to create new theories rooted in empirical experiences. We are inclined to reject such a conclusion, however. Therefore, before turning to normative theories we provide some further suggestions on how to improve current research practices.

First, we should think carefully about the situations in which the study of a random sample of organizations produces sufficient ground for a useful theory. Hume's guillotine postulates that from how things are we cannot infer how things ought to be. This seems to be true when it comes to observed accounting practices. We can hardly assume that all firms optimize accounting and controls at all times. Frequent changes in exogenous variables accompanied with the fact that time

¹⁸ We are suggesting here that e.g. VBM needs to be decomposed into its sub-constructs, and we need to study all these sub-constructs simultaneously. We do not see much value of asking organizations whether they use VBM and then regressing use to performance without controlling what use means.

required for major changes in accounting and control systems have been shown to take long periods of time, the fact that most ideas, methods, principles or theories in management accounting may be applied in a number of ways and the evidence of accounting failures all suggest that assuming that all firms optimize is bold at best and dangerous at least (see also Luft & Shields 2002). If we cannot assume that an average firm has optimal accounting and control methods, conclusions derived from empirical studies regarding the usefulness of certain systems in certain circumstances are doomed to be biased. This is not to say that cross-sectional studies are useless, or that there are never or even seldom situations where optimizing behavior may be assumed. What we argue is that these issues should be explicitly discussed in empirical papers.

Second, we should devote more attention to accounting and control practices in successful organizations. It seems that there is a bias towards studying problems and shortcomings of accounting, especially among those using the case method. Case based analysis of successful practice is seldom published. Similarly, unsuccessful organizations could provide basis for theorizing what systems not to use, or how not to use them.

Third, if what we suggest above as a fruitful avenue for research seems as a huge task, there is no logical reason why we should continue to conduct management accounting research individually or in small groups. Large research groups and projects may help to overcome the problem of addressing overly simple relationships. Moreover, management accounting theory building is likely to benefit from more repetitive studies. For us, decreasing somewhat the application of social meta theories providing new “insights” on management accounting practice, and increasing to some extent the conduct of repetitive studies in different contexts would seem a fruitful way towards developing a cumulative body of knowledge. We admit that for some the idea of repetitive studies may sound boring. But again, are we here to enlighten ourselves or to develop a body of knowledge that could help managers and other actors to improve their practices? It should be noted that we are not suggesting here that studies conducted in the US, for instance, should be repeated as such with data from other countries. Pure repetition without explicit a priori consideration of theory contribution is definitely not what we are suggesting.

Fourth, according to the agenda put forward here, the object of inquiry should be motivated and positioned by linking it to the underlying main research question, i.e. cause and effect of MA or how to implement MA, and how that is supposed to help in building a prescriptive agenda of management accounting research. This would help readers to assess the contribution. Finally, the concluding sections of research papers should make both the theory tested or developed and its practical implications explicit. Moreover, we encourage researchers to speculate more in discussion sections above the data (cf. Hinings & Greenwood, 2002). Even if the research is in the early stage, and no definitive conclusions or prescriptions can be derived, the discussion section provides us an opportunity to speculate the meaning of the particular findings, or those assumed to be available after the later stages of the research program have been finished, for practice.

5.2 Normative theories

We see the development and testing of normative theories as a fruitful avenue to produce more practice relevant research and theories of management accounting. It is easy to argue that normative theories are usually presented as universal truths and hence do not provide sufficient understanding of their potential limitations. Moreover, they are typically presented in polemic style, lacking the precision required from scientific theories (Lukka & Granlund, 2002). There is a need to explicate used constructs, their relationships and underlying reasons and to develop more contingent claims about their applicability. Moreover, an additional complexity in studying normative theories is their changing nature. For example, activity-based costing literature first emphasized overhead allocation, then measurement and management of business processes. Balanced scorecard was

introduced as a more comprehensive information system for managerial decision-making, then as a tool to translate strategy into action, essentially relying on the logic of management by objectives, and finally as a tool to clarify and communicate strategies with the aid of strategy maps. This development of ideas, especially in terms of which practical problems they are about to solve, requires researchers to be clear which variation of the idea they are addressing, both conceptually and empirically.

We used ABC above as an example of a normative theory (or alternative method for overhead allocation within the theory of cost accounting), illustrating how research has produced valuable insights on circumstances in which to use it. From a practical point of view, however, it is far from enough to explicate the circumstances in which the use of ABC to allocate overheads is likely to provide benefits. There are a large number of practical considerations that are likely to have an impact on how the functioning and results of a cost accounting model are understood, how easy it is to maintain, how accurate the information it produces is, and so on. These practical considerations include how to define activities, and what type and how many cost drivers to use, to name just two. Theory of cost accounting should ultimately advance towards answering these questions. It appears to us that too much emphasis is devoted to understanding why companies adopt ABC, and too little to understanding how it should be applied to serve the purposes it is adopted for. We believe accounting researchers could provide valuable insights for practice by addressing these how questions.

Let us use BSC as another example. There seems to be at least three different types of scorecards (KPI-, stakeholder- and strategy scorecards) used in practice. They seem to serve a different purpose as well. In the case where we would like to theorize about the use of BSC as a managerial control system, and develop a theory of management control systems further, addressing following BSC related questions are likely to be of help. We believe that the business community applying scorecards would also warmly welcome answers to these questions:

1. What are the necessary conditions for strategy scorecards to translate strategy into action?
 - For example, do measures need to be derived following assumed cause and effect relationships?
 - Do rewards need to be linked to achieving targets set for these measures?
2. In which circumstances does strategy mapping as a method for deriving scorecard measures lead to a proper set of measures?
 - What is the nature of strategy; are there major changes taking place inside the organization?
 - In which circumstances should some other logic of deriving measures be used?
3. Are these contingent on particular factors?
 - These may include factors traditionally studied in contingency studies, the implications of existing control practices on the applicability of BSC, etc.¹⁹

Answers to these questions would help us towards a theory of how to apply BSC in order to get desired results. An alternative approach would be to follow the lead by Otley (1999) as discussed above. Research could assess how BSC is used and ought to be used to facilitate each of these five steps. The specific questions above as well as the role of BSC in each of these generic steps would provide propositions as how to apply BSC and under which circumstances, and on the other hand, when not to apply as the expected outcome is likely to be negative or non-existent. One could also

¹⁹ It should be noted that some of these questions might be answered by traditional research approaches as well. However, providing descriptive answers to one or two of these questions alone – i.e. without consideration of the other relevant questions put forward above – may have no value in the end regarding the development of strategic performance measurement systems in practice.

assume that many of the observations generated in studying BSC are applicable to other existing and emerging management accounting practices, hence providing elements for a more general theory of MCS. As a result, we might not have a theory of BSC at the end, but a theory of management control, where BSC is one option, or value, that some of the sub-constructs of that theory may take.

5.3 *Constructive studies*

An alternative approach to create theories useful for practice is to solve practical problems with practitioners and synthesize the novel solutions to a more general form. In management accounting such an approach has been suggested by Kasanen et al. (1993) under the name of constructive research approach and by Kaplan (1998) under the name of innovation action research. Recently, based on their screening of how knowledge tends not to accumulate in the management accounting domain in a fruitful manner, Lukka and Granlund (2002) suggested increasing reliance on the constructive approach.

The studies applying the constructive research approach (e.g. Malmi et al., 2004; Tuomela, 2005; see also Labro & Tuomela, 2003) commit to strong interventional actions in the case organizations, and the researchers actively participate in the innovation process of new management control constructs. This reflects a strong contention with regard to positive accounting research. The action research tradition prominent especially in Sweden (e.g. Jönsson, 1996) is not far from the constructive approach, though typically implying weaker intervention and having no particular innovative element (see Jönsson & Lukka, forthcoming).

Kaplan (1998) promotes the innovation action research approach, and starts his piece quoting Kurt Lewin: “If social scientists truly wish to understand certain phenomena, they should try to change them. Creating, not predicting is the most robust test of validity-actionability”. Both the innovation action research and the constructive research approach share a common core in this regard. However, they are also very demanding research approaches, embodying certain risks, not least ones related to the studied organizations (commitment to the project over time, personnel replacements, etc.). Of these two the approach proposed by Kaplan (1998) can be considered as even more demanding, and may probably not be even executable by most researchers. However, the risks associated with these methodologies have been recognized and discussed, and by careful research project management they can be controlled (Lukka, 2000).

It is likely that positive accounting researchers would reject these approaches, because researcher intervention does not belong in their agenda. Our position is favourable to interventionist approaches, as in our view the potential of generating directly applicable, yet theoretically informed solutions to practitioners is important to pursue. However, it is true that in some sense the role of the academic researcher is at stake here and should be solved: how normative can we be without becoming labelled as consultants? As Bazerman²⁰ (2005, p.29) notes: “As a graduate student in the late 1970s, I was trained to be descriptive; prescription was for consultants, not for serious researchers. I now believe that this attitude is wrong, not only for the field of management but for society as a whole.” Kasanen et al. (1993) provide functioning guidelines in this regard. According to them the most important element of constructive research is its constant theory connection, which demarcates research from consultancy.²¹ Moreover, the strengths of such approaches seem to be evident. By acting as experts in real-life development projects, we can simultaneously produce research results that are both practically and theoretically interesting. The interventionist approaches

²⁰ Professor of Business Administration at Harvard Business School.

²¹ Lukka (2000 and 2005) has recently enhanced a view emphasizing that the main purpose of the constructive research approach should be to produce theory contribution, the development of a new innovative construction being only one result.

presented above subscribe to the idea of pragmatic truth theory even more distinctly than the other research avenues we have discussed in this study, though also here to varying degrees (Kasanen et al., 1993; see also James, 1955).²² Overall, this implies that we should always first consider relevance and then truth, rather than vice versa. An underlying assumption then is that in problem solving science researchers can and should be prescriptive and the validity of the results is tested through implementation: what works in practice is true. The fundamental question then is: how to relax the dominant view embedded in most conventions of management accounting research that avoids intervention and sticks strictly to the very traditional criteria of science, like statistical generalizability only (see Lukka & Kasanen, 1995).

An important thing to realize here also is that when succeeding in producing a theory informed construct to solve a practical problem that is also of theoretical interest, and if the construct is shown to be working, we have built a management accounting theory (see e.g. Malmi et al., 2004). In that case there is no need for other kind of theory building. The functionality of the theory should then of course be tested in other similar type of organizations and further under very different circumstances. The scope and applicability of the new theory would thus be examined.

6. Summary / Conclusions

In this paper we have argued that the purpose of research and theorizing in management accounting should be on determining which management accounting practices work and in which circumstances. We argue that there is a need for management accounting theories addressing what systems or techniques to use, how and in which circumstances. We also need theories explaining how to change management accounting practices. Such theories would be unique to our field and useful for practice.

We argue that there are two types of theories currently used and developed. Theories that are used by the research community are borrowed from related fields. Although capable to explain a number of issues of interest, they are seldom as such, or in conjunction with MA practices, helpful in explaining what systems to use, how and in which circumstances. We also have a number of normative theories that are not regarded as theories by the academic community. These theories aim to give guidance to practice, but seldom address the potential shortcomings and inherent limitations.

We outlined three major avenues for further research. The first one is to develop traditional research approaches. It appears that we need to assume an objective function to be able to build meaningful theories. We also need to relax, at least partly, the assumption that firms are optimizing their management accounting and control systems. Hence, studies should address the performance implications of various practices. Moreover, we believe that more complex management control system constructs should be studied. The second avenue is to develop existing “branded” practice theories, such as ABC or BSC, to more complete theories by specifying constructs, relationships and underlying mechanisms more clearly and addressing their limitations. The third avenue is to rely on interventionist research approaches, such as the constructive research approach or innovation action research, and get involved in theory building, testing, and refinement by creating new practices.

We believe that the research agenda suggested here would help us as a research community to provide more assistance to the organizations and societies than is currently the case. It could also

²² Particularly in the constructive research approach testing the functionality of the created construct in the spirit of pragmatism plays an important role in the research process. For the different market test levels for constructs, see Labro & Tuomela (2003; cf. Kasanen et al., 1993).

solve at least some concerns related to the gap between research and practice, and address the conjectures regarding the failure of management accounting research to produce a cumulative body of knowledge. Finally, we believe it would differentiate our field of inquiry from related fields, and give management accounting a stronger identity.

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