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### **Emerald Article: Making management accounting research more useful**

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# Making management accounting research more useful

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## Abstract

**Purpose** – The purpose of this paper is to discuss the general failure of management accounting research to be useful for practitioners.

**Design/methodology/approach** – The paper discusses the causes and consequences of the problem, and possible remedies.

**Findings** – The causes of the problem, and hence also the remedies, are related to choice of topics, research design, and writing and dissemination of findings; researchers are forced into choices that lead to less useful research by the research evaluation standards used by the major accounting journals and university professor evaluation practices.

**Originality/value** – While this general problem of lack of research usefulness has been discussed at some length in other areas of management, the issue has not received much attention in the management accounting community, other than with a few calls for more field research. However, getting out into the field more to do research addresses only one part of this important failure.

**Keywords** Management accounting, Research, Research methods, Usefulness, Impact, Relevance

**Paper type** Conceptual paper

[There is] a perception among senior scholars that the present generation is doing research less useful than ours. The fault is all our own. We hide behind the entrenched values of misguided deans and department chairs as an excuse for not taking the action needed to disseminate findings [...] that are useful for the public (Latham, 2011, p. 316).

## I. Introduction

The quote cited above, by Gary Latham, a well-respected senior organizational behavior scholar, resonated with me. It reminded me of one of my early doctoral study experiences and a closely related recent incident.

In the summer of 1974, just before I attended my first PhD class, I attended a “meet-the-faculty” reception. Seemingly within 30 seconds of my being there, the professor who would later become my dissertation chair asked me, “What research topic are you interested in?” I quickly responded, “Budgeting.” I had begun my professional career at Texas Instruments, Inc. (TI), first in the Office of Annual Planning, the staff group that designed TI’s budgeting processes and procedures, and later as a department controller who had to prepare, defend, and use budgets. I knew firsthand that budgets

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were an important management tool, that there were many design and implementation variables to be considered, and that the practice was more art than science. I thought that my doctoral studies would provide me with an excellent opportunity to learn, and to try to advance, the state-of-the-art of budgeting.

A couple of graduating PhD students overheard this conversation. Both of these students were rightly quite proud of their accomplishments in the PhD program. They had both completed all their degree requirements and had accepted assistant professor appointments at top five-ranked business schools. One of these soon-to-be assistant professors did experimental research on accountants' and auditors' judgments using students as subjects. The other was an analytical researcher who was proud that his models were so sophisticated that only three people in the world could read and understand his research papers. They both thought that it would be useful to give me some friendly advice. Their advice for me was *not* to do budgeting research. Their reasoning was that this kind of research is inevitably "soft" and would not be well received by the major academic journals. This topic choice would cut me off from the top universities and, hence, cause me to have less research support, a higher teaching load, and a lower salary, quite likely throughout the duration of my academic career.

I pondered this advice for a while, but in the end I decided to ignore it. The topic of budgeting was what I was curious about. And I, as compared to the graduating doctoral students, seemingly had a desire to have my research be more obviously relevant and potentially useful to my old colleagues back at TI and others like them. In addition, maybe I could be more courageous in my research choice because I was prepared to return to the business world if academia did not work out.

My choice to focus initially on budgeting research actually worked out for me in academia. I studied various aspects of budgeting for much of the next decade; I had considerable success publishing it in the top academic journals in accounting and I accumulated a good number of academic citations. However, if I had known then what I know now, I would have conducted my research quite differently. I think that while most people would judge my early budgeting research as relevant, it was not very useful to the world of practice. As I will explain below, research relevance is different from usefulness. Explaining what makes management-related research useful, and why my doctoral research, and actually most management accounting research, largely fails the usefulness test, is a major focus of this paper.

Now the recent incident: recently one of the junior faculty members in my department told me that he would like to do some research on budgeting. I was shocked when I found myself giving him the same advice that I myself had rejected nearly 40 years earlier; that is, *don't do it*. It takes too long to gather the data, and the soft, often perceptual nature of the data makes it difficult or impossible to publish in the major accounting research journals, given the tastes and preferences of the journals' reviewers and editors.

Giving this advice left me with strong feelings of conflict. While I thought it was the most prudent advice for this young faculty member, I remain convinced that budgeting is the single most important accounting-related management process in virtually every organization, and much is yet to be learned about it. We should be doing more budget-related research, not less, yet the publishing standards of the so-called "top" research journals and the promotion and tenure standards of many of our universities discourage, and even prevent, us from doing research in this, and many other, important

topic areas. I agree with the sentiments expressed by Gary Latham in the quote cited above; I believe the problem has, indeed, become worse in the 30-plus years that I have been in academia. As a field, we should be doing more thinking about why we are doing our research, and what kinds of research we should be doing.

Are we using our scarce research resources wisely? What if objective outsiders were asked to judge our research? In the USA, the late Senator William Proxmire used to announce his annual list of “Golden Fleece Awards” as illustrations of government waste. Among his winners (Proxmire, 1980):

- \$2,500 to study why people cheat at tennis;
- \$27,000 to determine why inmates want to escape from prison;
- \$500,000 to study how shrimp perform on a treadmill;
- \$325,394 to study how the environment affects the mating decisions of female cactus bugs; and
- \$1 million to study the effects of marijuana on sexual arousal.

Others, particularly various taxpayers’ organizations, have continued the Proxmire tradition of identifying wasteful research projects. To view suggested recent examples of government waste, in any internet search engine type “golden fleece awards” plus a year that interests you (e.g. “2012”)[1].

What if our management accounting research came under scrutiny like that of these researchers who took government money? Would we be able to defend all that we are doing? I think not. I would like to discuss this issue by posing the following questions:

- Are all research motivations useful? Are some more useful than others?
- Are we overinvesting in some areas of research and underinvesting in others? More generally, how should we be allocating our scarce research resources? (The useful medical analogy here is: how do we triage our research possibilities to focus on the projects with the greatest potential payoffs?).
- What changes are needed to make our research more useful?

The observations in this paper apply to most areas of business school research, and many others have focused their criticisms on other management interest areas. Not much focus has been placed on discussions of the usefulness of accounting research, with the exception of some pleas for more field research by some prominent researchers (Hopwood, 1983; Kaplan, 2011). My arguments in this paper go beyond those arguments, and I focus most of my discussion and illustrations on the general lack of usefulness of our current portfolio of management accounting research.

## II. There is a problem

Many critics (Mohrman and Lawler, 2012; Rynes, 2012; Parker, 2012; Bansal *et al.*, 2012; Tucker and Parker, 2012; Kaplan, 2011; Tushman, 2011; Singleton-Green, 2010; Moehrle *et al.*, 2009; Van de Ven and Johnson, 2006; Bazerman, 2005; Bennis and O’Toole, 2005; Mitchell, 2002; Starkey and Madan, 2001) have pointed out that there is a problem with the research being done in business schools. They use phrases like “relevance lost,” “research-practice gap,” “out of touch with reality,” “knowing has increasingly been uncoupled from doing,” “a push for greater internal validity at the cost of stunting

eternal validity,” and “conflicts between rigor and relevance, theory and practice, career concerns and societal contribution.” Here are some illustrative quotes:

Our journals are replete with an examination of issues that no manager would or should ever care about, while concerns that are important to practitioners are being ignored (Miller *et al.*, 2009, p. 273).

More than half of the last 16 presidents of the Academy of Management have used the occasion of their presidential addresses to emphasize the importance of doing research that contributes to practice, and have decried the lack of impact of prevailing research approaches (Mohrman and Lawler, 2011, p. 2).

The kind of research that predominates today [...] is research driven and theory oriented. It applies rigorous methods to well-defined problems studied from afar over relatively short time frames [...] Typically the research findings advance scientific knowledge while having modest to no consequences for management practice (Cummings, 2011, p. 333).

I am just trying to make the point that it is not just me who is claiming that the lack of usefulness in business school research is a problem. Many very distinguished academics have made this observation. This criticism is common in virtually every “applied” field – any field that tries to bridge the gap between theory and practice, including education, medicine, psychology, public health, law and engineering. It is a persistent tension and struggle.

**III. Useful research**

It seems obvious that research should be useful. Something that is useful serves a worthwhile purpose. But what is that purpose for those doing management (accounting) research?

Tushman (2011, p. 173) illustrated the unique purpose of research in professional schools using the two-dimensional model shown in Figure 1. In the base scientific disciplines, such as physics, chemistry and astronomy, researchers study subatomic particles, chemical elements and molecules, and black holes just to provide an understanding of how the world, or the universe, works. The researchers have little or no concern about short-term, practical payoffs from the research. For example, regarding the recent discovery of the long-theorized Higgs boson, which was an exciting, breakthrough finding in the particle-physics world, Mark Wise, a physics professor at CalTech, observed, “It affects our understanding of the laws of nature.

		Relevance? (Consideration of use?)	
		No	Yes
Quest for fundamental understanding?	Yes	Basic discipline (e.g., physics)	Professional school (e.g., business)
	No		Consulting firm (e.g., McKinsey)

Source: Tushman (2011, p. 173)

Figure 1.  
Research purposes

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It is not going to affect technological developments. It doesn't really impact our everyday life" (Kelly, 2012, p. A3). Practical impacts that might be forthcoming from discovery of the Higgs boson, if there are any, are likely to be far in the future. The research is aimed just at developing an understanding of how the world works. At the other extreme of the payoff dimension, consulting firms conduct surveys and studies of other types to try to improve practice in the very short-run. They have little or no concern about whether they are contributing explicitly to "theory," and many times the rigor of their studies would not stand up to scientific scrutiny. For example, the survey samples might not be random, and some questions might lead the respondents to particular answers. These studies are likely to have an impact on practice, but can the findings be trusted? Tushman and others (O'Toole, 2009; Pfeffer, 2007; Bennis and O'Toole, 2005) make the point that research in professional schools should produce findings that are both scientifically valid *and* useful for practitioners.

The need for scientific rigor is taken for granted in academic institutions; in addition, research usefulness is what society and, particularly, our most important constituencies expect of business schools. The AACSB (2003, p. 16) accreditation standards require that business schools must include in their mission "the production of intellectual contributions that advance the knowledge and practice of ibusiness"[2]. Organizations are not merely to be studied and understood. They can, and should, be shaped by the knowledge that business school researchers generate (Mohrman and Lawler, 2011).

It should not be good enough merely to have our research contribute new theory, new data, and/or new methods. We should try to advance the profession's body of knowledge, which means developing predictive theories that specify both the behavior and the context required for achieving specified outcomes. Kaplan (2011, p. 368) notes that the advancement of knowledge is especially important "when innovation is high and major changes are occurring in the practice environment of the profession." If the operating environment is stable, practitioners will develop adequate modes of operation. They need help the most when change is occurring.

We should also be trying to create more "evidence-based practitioners," that is, those who both understand and make use of the relevant research findings to perform their roles more effectively. Currently it is not happening. Rousseau and Boundreau (2011, p. 271) note that "research evidence is seldom used as a basis for management practice." Most practitioners are not even aware of the potentially relevant research evidence.

Researcher-practitioner interactions have declined sharply over the last few decades. For example, back in the 1970s, a majority of the members of the American Accounting Association (AAA) were practitioners (Flesher, 1991). Now there are very few practitioners left in the AAA and other like academic organizations, and practitioners rarely attend research conferences. A study in the human resources (HR) field showed that fewer than one percent of HR managers read that field's top-three academic journals, and 75 percent read none of the three (Rynes *et al.*, 2002). Practitioners often do not even act on the academic findings they do know about. Apparently they either do not trust them, or they believe that the findings do not apply to their situation (more on this below). Surprisingly, even many accounting educators and the textbooks they use often do not cite relevant research findings. Perhaps they themselves do not understand the findings, or they fear that the abstract academic arguments and difficult-to-understand academic jargon will make their writing and courses too dry.

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#### IV. Motivations to make research useful

Why should we want our research to be useful for practitioners? We should want to please our stakeholders, and we should want to feel like we have made a difference. Just contributing to theory without any application of that theory should not be fulfilling to researchers in a professional school. Some people go further and argue that since we are consuming societal resources, we have a moral obligation to benefit society. For example, Pettigrew (2001, p. S61) concluded, "The duty of the intellectual in society is to make a difference." Mohrman and Lawler (2011, p. 10) noted that, "Society has expectations that professional schools will deliver knowledge that can be used in practice."

Marketing and strategy arguments should also be compelling. Unquestionably our business school institutions are "losing market share" to many other types of organizations, including consulting firms, think tanks, training companies, and professional societies (Benson, 2011; Bazerman, 2005; Bennis and O'Toole, 2005; Pfeffer and Fong, 2002). These outfits churn out technical reports, conduct surveys, and offer insights about almost everything we in academia study. They are quicker to the market, and they speak the practitioners' language, saying without hesitation how the results should drive management practice. The details about the research method used in these organizations' studies (e.g. sample, measures, bases for conclusion) is usually tucked into the back of these reports, if it is included at all, and many times the research would not stand up to critical research design scrutiny. But these problems seem not to matter to practitioners, who are often not even aware of them. When they want to buy some research, most firms opt for the consulting firms, because they get one-stop shopping from organizations which are good at selling their ideas.

Even when academics produce some potentially useful results, few notice them. We celebrate the successes of academics whose works in total are cited maybe a few hundred times, and those notices are almost invariably solely by other academics. Compare that level of "success" with the sales figures of some of the popular management books. *In Search of Excellence* (Peters and Waterman, 1982) sold 6.5 million copies. *Good to Great* (Collins, 2001) sold 4.5 million copies. *The Seven Habits of Highly Effective People* (Covey, 1989) sold 25 million copies. Amazon.com currently sells more than 700,000 books categorised as "Management." Only a small proportion of the best-selling management books are written by university-based authors (Benson, 2011). We academics have lost considerable market share to these non-academic alternatives.

Practitioners are often harmed when they get their advice from these popular, but questionable, sources. Latham (2011, p. 311) cites an example of a professional writer named Dan Pink who repackaged some motivational research for consumption by the public and wrote a book, titled *Drive*, which became a number one *New York Times* bestseller. Among the conclusions in this book (Pink, 2009) were the following:

- People cannot motivate others. They can only motivate themselves.
- Giving employees a monetary incentive for a job well done typically decreases rather than increases their job performance.

Academic research has shown that these statements are not true in the vast majority of organizational situations. It is not even clear that they apply anywhere, except maybe in some short-term experimental settings. Unsuspecting practitioners can be misled by advice such as this.

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Similarly, the aforementioned best sellers, *In Search of Excellence* and *Good to Great* were widely criticised for their lack of research rigor (Carroll, 1983). *Business Week* (1984) ran a cover story titled, "Oops! Who's excellent now," explaining that nearly one-third of the companies identified as excellent in *In Search of Excellence* faced financial difficulties within five years. Similarly, among the "great" companies in *Good to Great* were Circuit City, which declared bankruptcy in 2008 and was liquidated in 2009, and Fannie Mae, which was at the center of the mortgage crisis in the USA and had to be placed into conservatorship by the US Government. Some might argue that these subsequent failures resulted merely because the world changed, and the factors that determined excellent at an earlier time were no longer valid. But the failures in these best sellers are far more serious than that. Co-author Tom Peters himself has since admitted both that his research method was flawed and that some of the data presented in the book were fabricated (Peters, 2001; Byrne, 2001). Author Collins' research method would not have passed any serious academic review process. For example, there was apparently no attempt to search for disconfirming evidence, i.e. companies that used all the great techniques but did not perform at a great level.

We academics leave the door wide open for these "experts" to fill the void, with potentially adverse results because we do not communicate effectively with practitioners. Popular writings like those of Pink, Peters and Waterman, and Collins, are where the vast majority of practitioners are getting their management "theory."

## V. What is wrong with our research?

A number of problems cause our research to be less useful than it should be.

### A. *We are disconnected from practitioners*

Most management accounting researchers seem to have little or no understanding of practitioners' problems, concerns, constraints, and options. They do not take the time to appreciate or understand the contexts in which the practitioners operate. To be able to produce useful findings, researchers must be close enough to practice to understand what they do, what they are trying to accomplish, how they operate, and what issues they are struggling with. To understand, we have to connect with them.

### B. *Too many of our studies explain "average" relationships with a narrow scope of study*

Researchers seek generalisability and statistical significance rather than application to a specific setting. Our discipline-driven approach assumes that there are a few universal truths that will solve practitioners' major problems, something akin to the law of gravity or the theory of relativity. We reduce the dimensions of what we study until they are small enough to fit the available data and research methods. Because of the large sample size, we typically are successful in finding statistical significance, but the size of the  $R^2$  is tiny and, more importantly, the results are too narrow to be applicable to any real world setting. The typical empirical research study might show that in a broad sample of firms (or departments or individuals), the correlation between  $x$  and  $y$  is 0.09. The sample size is large, so this correlation is statistically significantly different from 0 with a probability of less than 0.01.

But of what use is this empirical regularity to practitioners? Practitioners cannot trust the findings. Almost without exception, there are boundary conditions to every finding. The relationships hold only in certain settings, and those boundary conditions

are easily obscured in large sample studies. In addition, using our jargon, the studies suffer from numerous correlated, omitted variable problems. We need to recognise that there is almost no theory in management accounting that can stand the true test of a scientific law – that is, replicable at all times and in all places. In addition, practitioners do not want to be average, and they care a lot about contextual similarity in judging whether research findings can be applied to their setting. Useful knowledge should be in the form of statements like: “In settings that are like yours in all of the relevant dimensions, you can get the best results by doing this, and here is why it works.”

*C. Most of our studies are historical and cross-sectional*

We study organizations only after the fact and typically at only one point in time. We find out what patterns of relationships existed. We assume stability. But stability is not the norm. Further, as mentioned above, practitioners do not need much help in stable situations. They need help in coping with change.

*D. We have blinders on*

Management accounting researchers are increasingly sorting themselves into relatively homogeneous subgroups defined by preferred research strategies that create blinders. Our blinders are caused by a narrow focus on a discipline (e.g. economics), or one particular theory or paradigm (e.g. actor-network theory, attribution theory, agency theory). These subgroups do not communicate well, so they are not in intellectual contention with one another and the insights do not accumulate across the theoretical boundaries. That portends poorly for the development of fresh paradigms for the design and conduct of research that contributes simultaneously to theory and practice. Papers that start by saying the authors are going to test one particular theory are almost automatically on the wrong track. The most vexing issues and problems practitioners face do not respect the boundaries of academic specialties. They are often not readily resolvable within any single theoretical or knowledge specialty area.

*E. Our papers are boring*

Very few academic studies produce statistically significant results that are opposite to those predicted. Researchers who get unsupported or conflicting results often leave them out of their papers because reviewers often seize upon those “disappointments” to call the theory or important aspects of the research method into question. What is left is quantification of what was already known. There are no surprises. We do not learn much.

*F. We do not communicate our useful finding to practitioners well (or even at all)*

Practitioners do not read the papers we write, and we researchers have to take most of the blame. As Tenkasi (2011, p. 211) observed:

The challenge of connecting academic theory and research findings to practice has been almost exclusively the responsibility of the scholar. But many scholars don't seem to care. They are content just to contribute to theory and leave it at that.

We use thick jargon and spend much of the space in our papers discussing concerns and methods the practitioners do not understand. We rarely try to translate our useful findings for them. Most academics are focused on getting “hits” in top journals

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and building their academic citation counts. We receive little or no rewards for presentations to, or publications for, practitioners, so we do not reach out to them.

## VI. So what to do?

Most of my suggestions about what to do stem directly from the preceding observations plus insights from a field of research that has proven to be useful, that regarding the barriers to effective diffusion or dissemination of innovations (Singhal and Dearing, 2006; Rogers, 2003). Some of these suggestions are relatively easy to implement, but the solution to others will require some significant institutional changes.

### *A. Acknowledge the problem*

We first need to acknowledge and care about the problem. Tucker and Parker (2012) conducted a survey of senior management accounting academics and found that a significant majority of respondents, approximately 70-75 percent, believed that academic research was isolated from practice and that this gap should be closed. This level of agreement in the academy should provide the critical mass necessary to overcome organizational inertia and create change. This strong majority of academics should work to make usefulness for practitioners one of the primary standards of excellence of management accounting research. All academics, including the younger ones, should be encouraged and rewarded for doing useful research.

### *B. Choose better research topics*

Some management-related fields can point to significant effects on practice stemming from research that originated in the halls of academia. The field of finance can point to the significant impact on practice of developments such as the capital asset pricing model and the Black-Scholes option-pricing model. The field of behavioral economics has developed, mostly through experimental research, useful findings that have steered people toward better decision making and, among other things, improved government regulations (Sunstein, 2011). Porter's five forces strategy model, queuing theory, and statistical sampling in auditing are other examples of research that originated in academia and that subsequently influenced practice in desirable ways.

Management accounting academia has had *none* of these successes. I made this observation specific to management accounting developments at an AAA meeting some 20 years ago (and repeated it in a published paper: Merchant and Van der Stede (2006)). Only one person in attendance at that presentation, a senior professor at a highly regarded university, objected to this observation. I asked him for examples where academic theory has affected management accounting practice. The only example he could come up with was the development of the reciprocal cost allocation method using simultaneous equations. It is true that that this cost allocation method was developed in academia (Kaplan, 1973), but its use in practice is exceedingly rare. Thus, I have to conclude that important examples counter to my general observation about management accounting research do not exist.

It is true that many academics have observed practice and built models explaining how and why the practice works. Principal-agent modelling falls into this category. But practitioners did not learn, even from the papers that we academics consider seminal. For example, they did not learn from Holmstrom (1979) and Feltham and Xie (1994)

papers addressing the “moral hazard” problem and showing that employees (agents) should be held accountable for all measures that are informative about performance. The best practitioners had been doing these things for many, many years. Holmstrom and Feltham and Xie systematised these conclusions in elegant models. The models might make it easier to explain practice, but I do not believe that the case can be made that they influenced practice.

This realisation, that management accounting practice is ahead of theory, has important implications for all facets of our research – the questions we ask, the methods we use, and the theories we invoke. Table I shows a figure developed by Roethlisberger (1977), one of the researchers in the seminal Hawthorne effect studies, describing how knowledge is generated[3]. The bottom of the figure shows the phenomena of interest. Practitioners encounter these phenomena as a normal part of their work. They learn to act on the basis of empirically developed, even if not systematically rigorous, local theories. They use experienced-based knowledge. They respond to an on-going and equivocal stream of events and make decisions based on a broad set of criteria, including their impact on the organization of which they are a part (Rynes, 2011).

Working up the Table I column labelled “Methods,” as knowledge develops, researchers, and often some thoughtful practitioners, reflect on the phenomena and the practitioners’ skills in dealing with the problems they face. They develop methods of classifying problems, situations, and solutions. Over time they figure out how to define and then measure concepts and variables. Then those measures can be used to find relationships between and among variables in various settings. Occasionally, a creative researcher is able to develop deductive models that simplify certain problems into their core elements to yield useful insights. Some of these researchers earn Nobel Prizes for the impact of their models, although this has not yet happened in management accounting.

Kaplan (2011, p. 371) has used the Roethlisberger framework to explain his perceptions of the research/practice gap. He believes that academics spend too much time at the top of the knowledge tree, and too little time performing systematic

Levels of knowledge	Characteristic statements (theories)	Methods	Products
Analytical (scientific) knowledge	General propositions	Creative and inductive leaps of imagination	Deductive systems
	Empirical propositions	Contextual relationships General relationships between (among) variables	Statements of the form x varies with y under given conditions
	Elementary concepts	Measurement Definition of concepts and variables	Statements of the form x varies with y
Clinical knowledge	Conceptual schemes	Classification Observation and interviewing	Descriptive cases and syndromes Taxonomies
Skill	Knowledge of acquaintance	Practice and reflection The phenomena	How-to-do-it statements and aphorisms

**Table I.**  
Roethlisberger’s (1977) knowledge enterprise

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observation, description and classification. If researchers spend all their time testing and refining empirical propositions in increasingly sophisticated ways, they run the risk of losing sight of the phenomena that they should be studying. Kaplan uses a medical analogy to suggest that researchers should spend more time in the sick rooms than in the medical library.

Realising that practice is ahead of research quickly leads to the conclusion that our primary role as academics in professional schools should be to find leading-edge practice, understand it, explain it, test its applicability, improve it (if possible), and disseminate the insights both to practitioners not on the leading edge and to students, the next generation of practitioners.

This approach to research is not atheoretical. On the contrary, knowing the prior research literature, both theory and evidence, is crucial. This knowledge informs and shapes the research question. This knowledge ensures that the findings are integrated and are elaborated or refuted. It lowers the risk of reinventing the wheel. The theoretical grounding is what separates academics from the consultants and other so-called experts whose works I criticised above.

### *C. Use different (better) research designs*

There are many ways to design our research better to improve the possibilities that our research findings will be useful.

1. *Get out into the field.* It is highly unlikely that sitting at one's desk is the more conducive place to gain insight into what is going on in organizations. The advice to get out into the field to conduct research has been given in many forms over the years (Hopwood, 1983; Kaplan, 2011, 1998, 1983; Ahrens and Chapman, 2006; Merchant and Van der Stede, 2006; Fox, 2003), and the suggestion is still being made. For example, Edmonson (2011, p. 43) observed that, "Getting into the field is essential for building an understanding of a context and of the variables that matter therein." More contact with practitioners also automatically leads to more trust between practitioners and academics, which should lead to access to more real world data, and in the end more useful research.

2. *Focus on solving problems being faced by practitioners.* We should involve practitioners in the selection of our research questions and, even better, help them solve the most significant puzzles and problems with which the practitioners are struggling. Practitioners will not be interested in our findings if they do not relate to their world. We should also engage the intermediaries – consultants, service providers, professional associations – who are also worried about finding solutions to practical problems.

This suggestion, too, is not new. Problem-solving research has been referred to using many labels, including interventionist research (Jönsson and Lukka, 2006), action research (Beer, 2011), action science (Argyris *et al.*, 1985), and engaged research (Van de Ven, 2007). When helping practitioners to solve their real world problems, academics get a richer feel for the subject matter. It inevitably introduces academics to new ideas and theory because real problems are complex and multifaceted.

Helping to solve problems also provides a useful natural connection with practitioners. When focused on solving their organization's own priorities, practitioners are more open to influence from researchers and more open to sharing data even when the process is intrusive. "Problem-solving" (actionable) research

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is always relevant, but the research that we often label as “relevant” is not always actionable (more on that below with illustrations).

3. *Do more “holistic” research.* The advice that is useful to practitioners must reflect the realities of the environments in which they operate (i.e. the context). Only then can we know if the knowledge that we are offering is appropriate to use. Using academic jargon, we need to identify the boundary conditions and to reduce the correlated omitted variable problem.

But our academic specialties, and the scopes of our studies, have become increasingly narrow. We need to combine the knowledge generated across multiple areas of study, across disciplines, and use multiple theories and possibly multiple research methods. This is difficult to do. It often takes a long time to learn enough about each other’s frameworks to be able to combine knowledge. It is difficult to maintain expertise in multiple research methods. And it is difficult to measure or otherwise control for all the potentially relevant variables in a real world study. But those who are better at these things are more likely to make valuable contributions.

4. *Do more longitudinal (and ideally interventionist) research.* As mentioned above, most academic studies are cross-sectional. They find out what patterns of relationships existed, studying organizations only after the fact, and assume stability. But organizations and the contexts in which they operate are constantly changing, so leading-edge practice is almost always ahead of theory (Mol and Birkenshaw, 2009; Pfeffer, 2007). We should be studying organizations as they adapt, as they are designing these new approaches so that we can contribute knowledge to their formation. Prahalad (2011) calls these “studies of the *next* practices.” We should study outliers. We should strive to pick up on the “weak signals” that indicate that an organization is having success by doing things that are different from accepted practice. We should try to find the contemporary counterpart of Du Pont’s disaggregation of the ROI measure to facilitate management of a decentralised organization, Henry Ford’s assembly line, Dell’s mass computer customisation, Cisco Systems’ ethics and integrity program, WalMart’s supply chain optimisation, Amazon’s approach to electronic retailing, Toyota’s well integrated supplier networks, and Handelsbanken’s “beyond budgeting” approach. We should learn which approaches work and which do not, in identifiable, specific situations. Some emerging designs could affect all organizations, but it is more likely that the new approaches work only in certain specific settings.

This approach to research involves a purposeful sampling bias. The researchers are not going to have either perfectly tested measures or a large sample size. But this should not be a problem because the data just provide the raw material for extracting the logical structure. Ultimately the core principles will have to stand tests independent of the data from which they were derived. In the meantime, we will have learned something useful on a much timelier basis than would be provided by well controlled empirical studies that might be possible later if the innovative practice spreads widely. Purposely studying failures can also provide valuable information about what organizations should do to perform effectively.

Even better we should engage in interventionist research, which is explicitly aimed at narrowing the gap between academia and practice. As Lewin observed, “You cannot understand a system until you try to change it” (Schein, 1996, p. 34). Interventionist researchers participate as part of a team to directly intervene in practice settings to solve a problem or otherwise improve the performances of the organizations

they are studying. There are a few reported interventionist research success stories in accounting, mostly stemming from research conducted in Scandinavia (Suomala and Lyly-Yrjänäinen, 2011), but the approach is rare (Baard, 2010). The research ideal, which fits the definition of interventionist research, is a study that changes the organization while predicting and then studying the impact of the change. We can learn more if we spend more time studying intentional change interventions, rather than relying on the naturally occurring flow of events.

5. *Do more “out of the box” research.* Most academics are content merely to take the easy path which Schon (1987, p. 1) described as staying on the “high hard ground which overlooks a swamp.” Schön continued:

On the high ground, manageable problems lend themselves to solution through the application of research-based theory and technique. In the swampy lowland, messy confusing problems defy technical solution. The irony of this situation is that problems of the high ground tend to be relatively unimportant to individuals or society at large, however great their technical interest may be, while in the swamp lie the problems of greatest human concern.

It is relatively easy to work within already established academic areas of research. The theories (e.g. cognitive dissonance theory, institutional theory) are already developed. It is easy to augment the literature reviews and to motivate studies from “gaps” in the academic literature. The choice of research methods is largely predetermined. And we can write the papers by following previously used outlines. Doing something different, and being creative, is hard. But it is also more likely to lead to some new and, hence, significant, findings.

#### *D. Communicate important findings better*

We should aim some writings (and presentations) at practitioners. And we need to make the findings “sticky” (Rousseau and Boudreau, 2011; Szulanski, 2003) because generating even an eminently useful idea provides no guarantee of uptake by practitioners if it is not communicated well to them. Sticky findings are memorable. They make practitioners sit up and notice and make it easy for them to retain the knowledge. To make findings sticky, they need to be made simple, so that they are easily communicated. They need to grab attention because they are unexpected. Ideally, they will create an emotional reaction, such as fear that something is not being done correctly or relief that something is being done well.

We must realize, though, that some findings cannot be made sticky because they are inherently complex. And some practitioners will derive little value even from our stickiest findings. It is often difficult to communicate sophisticated ideas to managers with little experience or limited education or those who prefer intuitive rather than analytic decision-making styles. Still, we must not give up. We must aim our useful findings at the most thoughtful practitioners.

## **VII. Some not-so-good and some good research examples**

The following sections describe some examples of not-so-good, excellent, and potentially promising management accounting research to bring the prior suggestions to life.

### *1. Examples of some not-so-useful management accounting-related research*

*a. Budgetary participation.* As I mentioned above, in my early years of academia my research focus was on budgeting. Back in the 1970s, there was a general understanding

that budgeting was an important and complex short-term planning process, but even the most important variables describing companies' budgeting processes were not apparent at the time. Some general measurement instruments were developed to describe a broad range of the budgeting elements. Researchers used these instruments with factor analysis tools to infer the key dimensions of the budgeting processes. The extent to which superiors allowed subordinates to participate in the various budgeting processes invariably appeared as one of the factors explaining the highest proportion of variance in companies' budgeting processes.

As a result, budgetary participation became one of the most popular topics of study in the 1980s. We researchers sharpened the measures of budgetary participation, eventually concluding that the participation concept included two separable elements, involvement and influence. We correlated the participation variables with many contextual and outcome variables and found that the relationships were not stable. The search for stability led to development and testing of more and more complex models, including various two-way and eventually three-way interactions. However, even the complex models did not lead to stability of findings. There are, apparently, 18-way, or even higher interactions determining the effects of budgetary participation on important outcome variables, such as motivation and performance. The whole effort collapsed after contributing little, if any, useful knowledge.

This line of research was seemingly relevant, as the focus was on studying real world phenomena. However, with the benefit of hindsight I think we can conclude that this line of research was doomed from the start. It was not useful to practitioners.

We should have been able to see the dead-end much more quickly than we did. Budgetary participation is not a practitioner decision variable; it is a derived variable, affected by many other decisions made. Practitioners do not think about how much participation to allow their subordinates. They do not talk about participation, and they were not interested in the writings about participation. The budget research should have focused on the variables of interest to practitioners, such as the level of detail to require in the budgets, the amount of guidance provided early in the process by higher level management, and the amount of stretch in the targets. We would have learned this much more quickly if we had been engaged with practitioners and focused on their issues and problems, not those identified by academics.

In addition, as we became aware of the lack of generalisability of the original findings, leading to the inclusion of ever higher-order interaction variables in the subsequent studies, we should have realized that the budgetary-participation findings could never be made sticky. Simplicity is one of the key attributes of sticky findings, and the explanations of the relevant budgetary participation relationships could never be made simple and memorable.

*b. Earnings management.* One of the largest bodies of research in management accounting (and accounting more broadly) in the 1990s and 2000s is that focused on earnings management. Many hundreds of papers have been published on the topic. These papers show with as much rigor as you can get using the indirect method of inferring what was happening from public archival data, that managers manage earnings. They manage earnings up and they manage them down. They use every method at their disposal, including accounting manipulations that affect accruals and "real" manipulations that affect cash flows. They are more prone to manage earnings up when they are facing budget pressures or risks of violations of debt covenants. They are more prone to manage earnings

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down when a management buyout is imminent. They are less prone to manage earnings if the external auditor has relevant specialised industry experience. And so on. This research area has made the careers of more than a few accounting academics.

Is this research useful? I suggest that it is not very useful. For one thing, most people, and certainly the best practitioners, understood virtually all of the phenomena before the first large-sample study was done in this area. For example, in a “small sample” ( $n = 54$ ) field study I conducted (Merchant, 1989), two-thirds of the managers and their controllers I interviewed explained to me in great detail how they managed earnings in the prior year. The other third explained to me how they would manage earnings if they had needed to, but earnings came in about where they wanted them to come in. I did not find any manager who would not manage earnings if earnings management was “needed.” Upper management was aware that their subordinates were managing earnings, and they were not particularly concerned as long as those activities did not become too large and risky or costly. Indeed, they engaged in the same kinds of activities as they were working their way up to the top of the management hierarchy. And I was not the first to conduct research in this area. So few of the results of these large-sample studies that were conducted over the next two decades were surprising to me and others who read the earlier research in this area.

Second, this area of research dodged what is probably the most important earnings management issue, at least from a management perspective. The dependent variable of interest – earnings management – is not necessarily good or bad. It is often good for managers to feel committed to achieve budget targets, for example, through all legal means, including real and GAAP-consistent earnings management (Rodgers, 1993). General Electric (GE), for example, has had a long tradition of managing earnings in significant ways (Fisher, 2009), back to the days when Jack Welch was CEO and before. The earnings management culture is imbedded throughout the GE hierarchy. But GE has long been recognised as one of America’s most admired companies, so it must be doing something right. Why have not researchers focused on the issues of when earnings management is helpful vs harmful and how corporations design their systems to maintain “optimal” levels of earnings management?

What did we get from the hundreds of thousands of hours that highly skilled (and highly paid) academics devoted to the study of this earnings management topic? Well, we know a lot more about how to measure abnormal accruals using publicly available financial information. But is that useful for managers? I think not. For the most part, all we got was quantification of what was already quite well known.

*c. Corporate governance.* Empirical research on various aspects of corporate governance has burgeoned in recent years. Researchers are testing correlations between and among every corporate governance variable they can find, and they include arrays of the variables in regressions with outcome variables such as fraud, accounting restatements, or abnormal returns as dependent variables. They appear to retrofit some armchair “theory,” such as “companies with a CEO with an accounting background have fewer accounting restatements,” or “use of clawback provisions leads to a decline in earnings management activities.” These theories are weak, simplistic and naïve. The world is a lot more complex than these corporate governance researchers seem to understand. Many important aspects of the contexts are not identified and discussed.

I see this literature both as an academic and as a practitioner, as I have served on six boards of directors. I just shake my head at it. The vast majority of the findings are not

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useful at all. I read very little of the corporate governance research literature with an eye toward improving my performance as a corporate governance practitioner. For the most part, the findings cannot be trusted. In the meantime, bigger, more important issues are being ignored. Just to mention a few examples, I'd like to know more about how to protect the company from high impact but hard-to-predict ("black swan-type") risks, what parameters affect the ethical acceptability of change-in-control compensation agreements, and how to guard against harmful social psychological phenomena like group polarization and groupthink in audit committee deliberations.

I think that to make significant progress with corporate governance research, researchers will have to get inside the boardroom to understand what really takes place. To date, few researchers have been successful in doing that.

### *2. A success story, with some qualifications*

Unquestionably the management accounting research in the last 40 years that should be deemed most useful is that of Bob Kaplan and his colleagues, working in the areas of activity-based costing and Balanced Scorecards. Kaplan and his colleagues interacted intensively with practitioners, and in the course of doing so, they found some important innovations being used. They followed up with those innovative practitioners, first documenting and explaining the innovations, and then participating directly with the practitioners to improve the practices. And finally, they communicated extensively and effectively both to practitioners and academics. These innovations are now well embedded in academic theories, consultants' recommendations, companies far beyond those who originated the practices, and courses and textbooks. These are research ideals that we should attempt to replicate as much as is possible.

I just have two qualifications to this success story. One is that while Kaplan and his colleagues continued their development of the theories, they left the testing of these theories to others. After the discovery, they aimed all of their writings at practitioners and did not publish any empirical articles in academic journals. It would have been nice to have the developers of the theories participate in the rigorous testing of them. They left the academic testing of the theories exclusively to others, and the academic payoffs for testing theories that someone else has already taken credit for are small. Thus, not surprisingly, too few of these tests have been done.

Second, Kaplan and his colleagues have been heard to make some bold statements that one normally associates more with consultants than with cautious researchers. For example, "Balanced Scorecards work; they work everywhere; if they don't work the company did not implement it properly." Personally, I think there is a lot of research still needed to understand what Balanced Scorecards are, what they do, where they work, and why they seem to fail so often.

Still, the research contributions of Kaplan and his colleagues have clearly been useful, and arguably more useful than any other management accounting research that has been done for more than three decades. Much about their research approach should be admired and replicated.

### *3. Some recent research of mine; not quite exemplars, but [...]*

I will cite two of my recent papers (Huelsbeck *et al.*, 2011; Chen *et al.*, 2012) as reasonable illustrations of useful research. (There might be other, better examples in the management accounting literature, but these are the examples that I know best.)

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Both of these projects were funded as part of the Chartered Institute of Management Accountants' (CIMA's) initiative on interventionist research. In these research projects, we did some good things. We got good access to the companies. We studied real, practical problems. We used multiple research methods, particularly both on-site interviews and analyses of archival data. We had an action orientation, as we presented our research findings and insights/recommendations to the companies with which we participated. And we learned from the managers' reactions to our recommendations.

I acknowledge, though, that these studies are not quite exemplars for the following reasons. First, we chose the topics for study. We were not studying the "big issues" foremost in the managers' minds. The managers cooperated passively with us, but we did not have the intense practitioner/researcher interaction that would have been optimal. Second, after reflection, the managers rejected more than half of our recommendations. We failed to consider/understand some of the factors that were important to the managers. So we learned a lot from the studies, but the practitioners learned relatively little from us. We were, however, able to advance academic theory, so now with a better understanding of the phenomena, we could help the next companies better.

### VIII. Conclusion

Every academic paper starts with a motivation section explaining why the study is important. Virtually all researchers think of their research as obviously relevant and useful. But we need to subject these motivations to greater scrutiny. In my judgment, very little of the research that has been and is being done in management accounting can be called useful. Just because academics believe that they are investigating areas that are useful does not mean that they are. In the end, the judgments about usefulness have to come from practitioners. If the findings are used, then they are useful. To be useful, the research findings have to fit the practitioners' issues and contexts. Researchers have to demonstrate credible institutional knowledge and to consider enough contextual richness that the findings will appear credible. And the new insights have to be written so that practitioners can understand the writings. The use of academic jargon is generally not helpful. Why talk about "allocation of decision rights" when the practitioners have long been familiar with the essentially equivalent "decentralisation of decision making"?

More management accounting researchers should be interacting more directly with practitioners. Where possible, we should try to help solve never-before-solved problems (or puzzles) that practitioners are facing. Solving problems effectively automatically advances the practitioner to state-of-the-art. We should also endeavour to document and explain, and hence to spread, leading-edge practice. This type of research helps with the diffusion of new ideas, both to practitioners and to students.

These practical research approaches differ from consulting because the research is both informed by theory and is used to develop or refine the theory. Indeed, one test of the usefulness of a research finding is regarding its value for inclusion in textbooks. When possible, researchers should pick problems with dual relevance (Walton, 1985). That is, study under-developed areas of practice where there is also a gap in theory.

To engage in this more useful type of research, we researchers need to develop new research skills, spend our time differently, and conduct research differently. We need to be aware of the problems that practitioners are facing. Here are just a few suggestions of potentially fertile areas for doing useful management accounting-related research:

- the quest for sustainability/corporate social responsibility;
- development of corporate ethics programs;
- the beyond budgeting movement; and
- the impact of social pressure on corporations' activities (e.g. design of compensation plans).

Somewhere in the universe of companies, some companies are doing the best job of coping with these problems. We need to find these companies and learn from their experiences and approaches. We need to pick up on the “weak signals” that useful practice advances are developing.

Most of us find ourselves in university settings that are unconcerned with and, hence, do not reward and value making research useful (Cummings, 2011). Sometimes we hear the advice that once we are granted tenure, then we can do some useful research. Certainly tenure provides some research freedom. But waiting for tenure to do useful research is not a particularly good option. The research skills needed to do useful research are different from those needed to do other types of academic research. The research skills need to be practiced and developed. They cannot sit idle for six or more years without atrophying. Further, even tenured faculty members are evaluated in relatively short timeframes, and generating useful research at a university that values only publications in the major academic journals will not result in many academic rewards for researchers doing useful research unless the journals change their evaluation standards.

To motivate more useful research, we need journal editors and business school administrators to place a higher emphasis on the usefulness of the findings. Instead of passively accepting the status quo, we can engage in a lobbying effort aimed at changing our settings – our universities and the journals that our university administrators value. This would involve changes in both journal editorial and reviewing policies, to place more emphasis on the usefulness of the research findings, and in faculty reward structures, for promotion and tenure and beyond. We need to encourage our universities to provide incentives for generating publications aimed at practitioners, contributions to web-based repositories of knowledge that practitioners access (e.g. blogs), and speeches to practitioners. We need to provide better and different training in our doctoral programmes and for our faculty. This training should include advice about how to interact with practitioners, how to conduct field research and interventionist research, and how to write in the various practitioner outlets. Can we change our universities, which are obviously quite resistant to change? It certainly will not happen if we do not try. As Margaret Mead observes (as quoted by Applewhite *et al.* (2003)): “Never doubt that a small group of thoughtful committed citizens can change the world. Indeed, it’s the only thing that ever has.” Some of this change would not be major, as the category used most often to reject papers at *The Accounting Review* is labelled “Insufficient Contribution.” We just need reviewers and editors to interpret that category to include contribution to the advancement of practice, in addition to contribution to theory.

Some universities including, quite recently, all of them in the UK, already value and reward research usefulness, which is sometimes called “impact.” I view the recent movement in the UK to allocate research funding to universities based on documented demonstrations of research impact as a quite positive development. A pilot test has

been completed, and now UK institutions of higher education must assess the “impact” arising from research, not just at the proposal stage of research but also at the evaluation stage. The four UK funding bodies have added an explicit element to assess the social, economic or cultural impact or benefit beyond academia within a given timeframe, in the first instance January 2008-July 2013 (HEFCE, 2011). An importance weighting of 25 percent has been established for impact, although the weighting has been reduced to 20 percent in the initial exercise while the assessment methods are being developed. These assessments are important to all UK institutions of higher learning because they are used to demonstrate good practice and achievements, they directly affect the funding of research, and they will undoubtedly reshape the portfolio of research projects that are undertaken. I hope that this exercise proves to be a success and that the practice of encouraging research usefulness, or impact, spreads. Undoubtedly there will be some start-up challenges, such as in defining what impact is and how to document its existence. Academics who are interested in making research useful should volunteer to improve the process. If usefulness is important, and a strong majority of us in the academy seem to think it is (Tucker and Parker, 2012), then over time these universities should thrive. Their research contributions should be noticed, and funding and rankings should flow in their direction.

If we do not address these problems, we will increasingly find ourselves in the conflicting situation that I found myself in, in the story that I related at the start of this paper. That is, when giving advice to our young management accounting colleagues, to ensure their own survival in academia, we will actually be forced to encourage them to shy away from the most useful research. Their research might be more rigorous in a narrow, sterile kind of way, but it would be less useful. It will grow, not diminish, the already huge gap between academia and practice. That cannot be the right answer.

#### Notes

1. Beware of some possible confusion. There is also a Golden Fleece award designed to support Irish creativity in arts and crafts.
2. It must be acknowledged, though, that this standard seems to be commonly loosely interpreted or not rigorously enforced.
3. Other conceptualisations of the research process, such as that by Baldvinsdottir *et al.* (2010), are quite similar.

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**Further reading**

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