



## Reflections on paradigms in action in accounting research

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### ARTICLE INFO

**Keywords:**  
Paradigms  
Accounting research

### ABSTRACT

This paper starts by shortly summarizing some of the pros and cons of paradigms. It then provides three personal accounts on paradigms at work. The purpose is to challenge the management accounting academy to think how “healthy” its current condition is, if these experiences represent reality for those trying to do something they find valuable and believe in, but does not fit to the thinking models of the established paradigms.

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### 1. Pros and cons of paradigms

Paradigms are, at their best, useful for any scientific community. Being a set of assumptions, concepts, values, and practices that constitute ways of viewing reality, paradigms provide focus, orchestrate effort and assist the research community to accumulate knowledge about the issues of interest. These are no small benefits, I acknowledge. For newcomers, learning the “rules of the game” implied by the existing paradigm is less time consuming than “re-inventing” the wheel; i.e. the situation of no strong paradigm. Hence, paradigms may speed up knowledge creation and assist individuals in their career aspirations. Most Ph.D. supervisors would probably encourage students to work within a paradigm they are themselves working in and caution against attempts to undertake something more creative and risky.

Paradigms may also prove problematic, however. Could it be that under any paradigm, after early successes, we should expect only diminishing marginal returns as the paradigm matures? Could it be that after the major questions are answered and insights gained, our attention is directed towards the ever smaller and less important puzzles which are addressable within the paradigm? This could be expressed also by asking, whether we are becoming extremely good in doing something we should not be doing

at all anymore? Could it be that paradigms constrain creativity? Or we may argue that paradigms foster creativity within the paradigm, as one must come up with some contribution under the regime of diminishing marginal returns, but do not foster out of box thinking as paradigm boundaries are non-crossable. Paradigms can leave certain questions unanswered, if those questions are not approachable with the (rigid) tool-box that is characteristic of them. These questions may be important for society as a whole, or for the organizations and managers we study. But we tend to neglect these questions by arguing it is the job for some others working under some other paradigm or profession to address them. Paradigms also produce academic elites. As we all know, elites have a tendency and ability to retain their privileges. Perhaps this partly explains why paradigms seem to be so sticky.

In the following I provide some personal experiences of paradigms at work. The first one is a short account of a research project and a publication on quality costing, which fitted neither the mainstream functionalistic nor the interpretative paradigm. It barely got published, although the results were both innovative in terms of further developing quality costing theory and relevant for accounting practitioners. The second one concerns a piece of inductive research, which currently is not regarded as proper way of conducting research by the mainstream, although it should fit within the functionalistic research agenda. The third one is about the lack of willingness of elites to engage in intellectual debates on ideas which are not in accordance with their paradigm.

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Why bother to recount these experiences? One aim is to substantiate some of what Kari Lukka argues in his piece with some illustrative evidence. The second is to challenge the management accounting academy to think how “healthy” its current condition is, if these experiences represent reality for those trying to do something they find valuable and believe in, but which does not fit into the thinking models of the established paradigms.

## 2. Experience 1: constructive research approach

Regarding the first example, a colleague of mine, a professor of quality management at the Helsinki University of Technology, approached me and asked me to co-operate with him. The problem, he told me, was that in practice quality initiatives seldom attract top management attention, as expected benefits cannot be measured reliably in monetary terms. He wanted to better understand the current state-of-the-art of quality costing and to come up with practical ways to assess the cost of poor quality and benefits of various quality improvement initiatives. So we decided to carry out a research project on quality costing using multiple methods. We conducted a survey and did interviews as well as engaging with a few organizations to develop their quality costing practices.

We thought we learned the most out of our experiences in developing quality costing in these few organizations. However, it turned out to be difficult to publish these insights due to the constructive research approach we applied. Reviewers said, for example, that as there was very little, or no theory, it should not be published. It is true, we had no hypotheses to test, nor had we any meta-theory to explain what we observed. So in terms of the mainstream economics-based paradigm, we had no theory, and in terms of the interpretive paradigm, we used no meta-theory to make sense of what we found out. However, reviewers coming from these paradigms did not understand, or accept, one key idea of the constructive research approach: if the aim is to create a construct, which is theoretically novel and works in practice, then the construct itself can be viewed as a theoretical contribution.

This paper of ours (Malmi et al., 2004) has attracted almost no citations. It may be that nobody is working in the area of quality costing. Alternatively, the research community has not accepted the construct or does not regard it as relevant. Perhaps it has not been accepted that researchers can innovate something and lead the way for practitioners? Innovation and value for practitioners are not something current paradigms value highly. It is easy to dismiss the constructive research approach as being equal to consulting, if one does not read carefully the original ideas (Kasanen et al., 1993). It is interesting to note that while the academic audience has – at least so far – dismissed our ideas, one of the authors has used the construct frequently for example within the ABB group companies, which testifies to the highly practical relevance of the construction.

## 3. Experience 2: inductive theory development

We recently submitted a paper to a so-called top accounting journal on management control systems (MCS) and their couplings. In that paper we developed Orton and Weick's (1990) ideas on loose coupling and used our analytical concepts to analyze MCS in a case firm. The aim was to generate propositions regarding the functioning of MCS; probably to be tested later on large data sets. So even though we relied on Orton and Weick, we would consider our research to fall within the functional paradigm, rather than within either the interpretative or the critical paradigms.

Deriving propositions from empirical observations is called inductive reasoning. Some may even regard these inductively derived propositions as a theory, or at least as an attempt to develop a new theory. One of the reviewers apparently did not value research based on inductive logic. S/he said there is very little theory. The advice we got was to develop theory that leads to testable hypothesis. MCS and loose coupling are not new and theory should be developed [deductively], the reviewer said. However, he/she went on to say that the limited number of observations is a concern. The reviewer concluded that the paper should discuss whether the data is consistent with the hypotheses. For us this seems to indicate that a certain paradigm forces all research activities to follow a similar pattern, irrespective of the problem at hand and/or the current state of knowledge about it. If only deductive reasoning based on economic theory is acceptable, how do we study issues we do not know enough about to be able to develop hypotheses? Where do the new ideas and new insights come from under such regime?

## 4. Experience 3: non-discussables

When Zimmerman (2001) published his critique of empirical management accounting research, Markus Granlund and I tended to share some of his concerns, but disagreed with the solution he suggested. So we decided to sketch an alternative solution (Malmi and Granlund, 2009), thereby continuing the debate already started by a number of prominent scholars in the *European Accounting Review* (Hopwood, 2002; Ittner and Larcker, 2002; Luft and Shields, 2002; Lukka and Mouritsen, 2002). In our paper, we suggest that we would benefit (as a research community), if only we could think about theory in a somewhat different fashion from the way in which we are used to thinking in our established paradigms. We submitted the paper to *Journal of Accounting and Economics* (JAE), accompanied with a polite letter to the editor arguing that we share Zimmerman's concerns, and that these issues should be discussed in the top research journals. We also suggested they might ask someone from the mainstream paradigm to write a reply/commentary, proving our arguments wrong, if they think so.

JAE rejected our paper. This came as no surprise for us. They said Zimmerman's piece was an invited critique and our paper did not fit to their normal editorial policy. This is what I would call paradigms in action: the editor of JAE is allowed to define a problem, and he is allowed

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