Unplugged - Book Reviews Special Forum: About Doing Research

Mats ALVESSON & Jörgen SANDBERG (2013), Constructing Research Questions: Doing Interesting Research, Los Angeles, CA: Sage.

reviewed by

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The unplugged section edits some book reviews special forums dedicated to a topic, an author or a theoretical perspective. This first forum considers three very stimulating and rejuvenating volumes for academics in organization and management studies about research methods. They offer some new insights about problematizing, theorizing and academic writing which may contribute to regain scientific imagination.
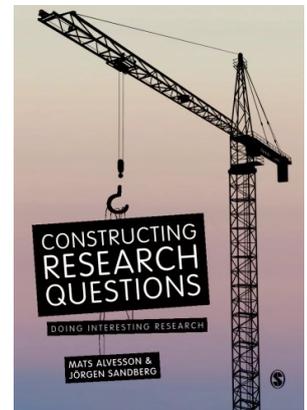
Alvesson and Sandberg's book *Constructing research questions: doing interesting research* adds an intriguing and challenging perspective to discussions about theorizing. Not only is it extremely interesting to read, but it is also very well constructed. In this review I will try to respond to the book in a way that acknowledges its contributions whilst also raising a couple of ideas about some of its central points. I will begin by introducing the context in which I use the material in the book, before summarizing its main points and contributions and, finally, initiating a one-sided dialog with the book, reflecting on some of the content that stands out for me and some of the questions it raises.

THE CONTEXT IN WHICH I USE THE MATERIAL FROM THE BOOK

I sometimes teach a class to doctoral students in which I focus on how to theorize. In that class, the primary template for theorizing which my students and I have used for years is David Whetten's chapter "Modeling theoretic propositions" in Anne Huff's (2009) book *Designing research for publication*. The chapter walks participants through the steps of theorizing, starting with developing constructs, then moving on to developing links between constructs,

understanding why these links are as they are and, finally, identifying conceptual and contextual assumptions that likely underlie the constructs and their links.

Understanding why certain constructs link with other constructs has been a cornerstone of management (and broader social science) theorizing for decades (Bacharach, 1989). In a very well-known essay, Sutton and Staw (1995) argue that answering the question "Why?" is *the* primary defining characteristic of theory. Scholars such as Paul Dimaggio (1995) and Karl Weick (1995) suggest that providing a clear-cut answer to the question "Why?" might not be necessary for all theorizing, but in scholarly work about theorizing there is still a strong focus on "Why?". Of course, this emphasis is one of the reasons why institutional theory, population ecology, identity theory, cognitive dissonance, and so forth have become so popular. They provide ready-made answers to the question of "why" two or more constructs are linked.

On the other hand, Whetten's chapter describes contextual and conceptual assumptions as specifying "the terms and conditions governing appropriate application or 'safe use' ... it follows that making your assumptions explicit helps you think critically about your theoretic arguments" (p. 225). For my students (and for me) it is nowhere near as difficult to determine assumptions following Whetten's approach as it is to make explicit the answer to the question "Why?" in theory. However, what Whetten's approach lacks is a framework that can challenge these assumptions.

Answering the question "Why?" may be the sine qua non of theorizing but as Murray Davis (1971) made evident more than forty years ago, answering that question doesn't necessarily make theory and theorizing interesting. Rather, what makes a theory "interesting" is how it treats assumptions, whether it accepts or challenges them. Well-articulated answers to the question "Why?", no matter how difficult these are to accomplish, will achieve publication but will not necessarily lead readers to become enthusiastic about a particular contribution.

SUMMARY OF THE BOOK

In this book, Alvesson and Sandberg have focused squarely on assumptions and their role in fostering interesting theorizing. For them, questions about assumptions come not at the *end* of a theoretical construction (as they do in Whetten's chapter) but at its very *beginning*, in formulating the research questions that precede constructs and their links. That is, Alvesson and Sandberg emphasize the importance of identifying and challenging assumptions in the initial crafting of a study, not waiting until the end of its conceptual development in order to identify the boundaries within which a possible conceptual contribution is confined.

Alvesson's and Sandberg's (2011) *Academy of Management Review* paper, which summarizes some of the main ideas in this book, has certainly gained a good deal of attention. It is cited considerably and, it seems to me, trotted out regularly by reviewers of scholarly submissions who think the submission is boring and ought to stir things up a bit in order to be judged as having enough value to warrant publishing.

Constructing Research Questions builds (explicitly) on the earlier work of Alvesson and Sandberg in several journal articles (including Alvesson & Sandberg, 2011; Sandberg & Alvesson, 2011). It is relatively short with a very clear and helpful outline that is faithfully followed. It is an excellent teaching tool; one of the particularly useful contributions of the book is that it shows how much goes into the competent development of any type of research question, regardless of whether it challenges assumptions or not.

Fortunately, the book contains no major challenging of assumptions on how to construct a book and/or guide readers in reading it. It is so well outlined and plotted that in this section of the review I will simply summarize the major aspects of each chapter.

Chapter 1 describes the importance of formulating good research questions for theory development; indeed, the formulation of these questions may be the most critical aspect of research. It also emphasizes "that problematization – … questioning the assumptions underlying existing theory in some significant ways – is fundamental to the construction of innovative research questions and, thus, to the development of interesting and influential theories" (p. 2). The focus in the book is on how such problematization can be accomplished, with the expectation, following Davis (1971), that problematization will lead to theories that are "more likely to become influential in academic disciplines and sometimes also more broadly in society" (p. 5). Problematization is much more likely to accomplish this than the more typical "gap-spotting" approach to research.

Chapter 2 describes how the contexts in which research questions are constructed and formulated affect the resulting questions. It notes, for example, that "all research questions are constructed and formulated within certain frameworks, such as disciplinary, theoretical or methodological perspectives, but also within culturally taken-for-granted understandings" (p. 14). Other contextual factors that are likely to have an impact include the availability of research funding, the interests of other (typically senior) researchers in one's own department or core group and, fortunately, the researcher's own interests.

Chapter 3 emphasizes gap-spotting as the prevalent way of constructing social science research questions. Building in part on Golden-Biddle and Locke (2007), it describes several means of gap-spotting in social science. The authors do note, however, that the published presentation of research questions as gap-spotting may be more prevalent than the actual formulation of the questions as gap-spotting, simply because the norms for presenting research questions in terms of spotting gaps are so strong.

Chapter 4 investigates the presented research questions in 119 journal articles and finds that even though some of these questions reflect "complex, constructive, and sometimes creative processes … [none of them] actually make an ambitious, deliberate attempt to challenge the assumptions underlying existing theories about the subject matter in question" (p. 41). This then limits the possibilities of their interesting and influential contribution.

Chapter 5 presents the major contribution of the book, the development of problematization as a way of generating research questions. Rejecting some approaches to problematization, such as pre-packaged approaches challenging the assumptions of all non-critical studies in similar ways, the authors instead advocate a genuine problematization approach that includes challenging not only assumptions that underlie others' theoretical position but also one's own assumptions. In fact, problematization starts with "a *dialectical interrogation* of one's own familiar (or home) position, other theoretical stances, and the domain of literature targeted for assumption challenging" (p. 49).

Problematization thus involves understanding the assumptions underlying a particular subject matter and then, based on this understanding, generating new types of inquiry. Several methodological principles are presented "for identifying, articulating and challenging assumptions" including "(1) identifying a domain of literature; (2) identifying and articulating the assumptions underlying this domain; (3) evaluating these; (4) developing an alternative assumption ground; (5) considering it in relation to its audience; and (6) evaluating the alternative assumption ground" (p. 56). Each of these is developed in depth alongside practical illustrations of scholarly papers that challenge assumptions.

In Chapter 6 Alvesson and Sandberg use their methodology to problematize two theories reflected in two frequently cited articles: Dutton,

Dukerich and Harquail's (1994) work on the study of identity in organizations and West and Zimmerman's (1987) study of gender. They walk through all the steps developed in prior chapters as these would be used in scholarly practice.

Chapter 7 includes several reasons that explain why gap-spotting is so influential even though it reduces the chances of creating interesting theories. In particular, it highlights the social norms that regulate what is publishable or not, as indicated by the small number of approved journals in which publications "count" in many social science fields. Also described are an "accumulation" norm of presenting findings in a way that shows how they explicitly add to prior literature in many fields, a "crediting" norm "which stresses the need to build on and acknowledge the work of other scholars" (p. 98), and emphasis on careful analysis and statistical treatments without comparable attention to whether the data address the major questions asked.

Alvesson and Sandberg ask: who can change these norms? Perhaps governments can, by changing the criteria they use to assess scholarly work and broadening the outlets that "count." Perhaps universities can, by placing greater emphasis on citation impact than on particular journals and extending time clocks for tenure. Or perhaps academics can change scholarly norms by rating the innovativeness and originality of ideas more highly and allowing more room for creativity in responding to reviews.

Finally, Chapter 8 summarizes the general arguments of the book and emphasizes that the authors do not advocate problematization as the only approach in formulating research questions, as sometimes gap-spotting is appropriate. They conclude by arguing in favor of theory development that actively searches for opportunities to let empirical material "inspire rethinking conventional ideas and categories" (p. 120), with the hope of finding the unanticipated, not just the predictable.

ONE-SIDED DIALOGING WITH THE BOOK

This book is interesting and inspiring in many ways. It is well worth grappling with its critiques and several thoughts come to mind following a close reading.

First, in Chapter 5 the book gives some illustrative examples of articles that have successfully challenged assumptions. The articles cited are mostly well known ones whose challenged assumptions are recognized post hoc. I wonder, however, about everyday assumption challenging. I would have liked to see the working through of this on a less "famous" level, including in articles-in-progress that attempt to challenge assumptions in a meaningful way. I have reviewed several such submissions that challenge assumptions but did so unknowingly and it is unlikely that this is what the authors have in mind.

Second, I agree with the notion that it is of value to produce interesting research and theorizing but I don't think that producing interesting work is confined to challenging assumptions. An analysis by Bartunek, Rynes & Ireland, 2006 (p. 12) indicates several factors – in addition to challenging assumptions – that affect how interesting an article is. These include the "quality of the article [i.e. the study itself], how well it was written, the newness of its theory and findings, the importance of its practical implications, and the extent of its impact on subsequent research." Davis's (1971) framework, while extremely valuable, does not comprise all the potential factors that make an article interesting.

Third, my graduate training was in Experimental Social Psychology, a discipline in which – as in similar social science disciplines – the assumption is that, once formulated, the research questions (and hypotheses) don't change. This assumption is so widespread that it is often considered unethical if a

question and hypothesis change midstream. However, this assumption is also casually challenged on a regular basis in organizational research: reviewers sometimes help authors think of "better" research questions that fit their data better, and, as noted above, they challenge assumptions more. Is this condonable? Where in the research process should challenging assumptions (ethically) be acceptable?

Fourth, the book reminds me of another type of assumption challenging. In 1974 (and then in subsequent books) Argyris and Schön formulated a description of double loop learning as opposed to single loop learning. Single loop learning involves learning that occurs within what Argyris and Schön call "governing variables, terms that undoubtedly be substituted for underlying assumptions." Double loop learning, which they consider very rare, involves learning in ways that challenge these governing variables or underlying assumptions. Argyris and Schön developed tools to help foster double loop learning (e.g. the left-hand column and ladder of inference). These are designed to help individuals and larger groups recognize that they have an assumption ground, what it is, what it means to operate within it and, finally, challenge it. Linking Argyris and Schön's materials with Alvesson and Sandberg's book makes it evident that what Alvesson and Sandberg are talking about here is something much bigger than scholarly competence, especially given their expectation that challenging assumptions should challenge one's own assumptions, not just provide a programmed challenge to others. Developing the ability to challenge one's own and others' scholarly assumptions involves personal development, not just intellectual dexterity.

Thus, in conclusion, this book suggests that scholarly development may also be able to foster personal cognitive development. It also indicates that activities designed to develop individual learning in practice settings – such as those discussed by Argyris & Schön (1974) – may also help to foster scholarly development. Perhaps I should use one of these exercises next time I teach a class on how to develop theory.

This possibility suggests a good way to end the review. Alvesson and Sandberg's book – whether or not this was intentional – provides a challenge to many people's assumptions about the links between practice and scholarship. Namely, that the successful challenging of scholarly assumptions may be fostered by exercises that scholars have designed for practitioners. This is definitely not the standard assumption. Wouldn't it be interesting if their book helped to challenge that assumption?

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