

Chapter 1 | Research Questions: A Core Ingredient in Developing Interesting Theories

In: [Constructing Research Questions: Doing Interesting Research](#)

BOOK

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Research in the social sciences takes many different forms and is guided by several different objectives. Some researchers aim for prediction and explanation while others search for understanding. Sometimes empirical description and accuracy are central; sometimes these are subordinated to theoretical ambitions. In certain cases researchers try to develop theory through careful empirical investigations; in others the fieldwork is exploratory and aims to trigger theoretical inspiration; and in some instances empirical investigation is bypassed all together by pure armchair theorization. Despite the huge variety in research styles within the social sciences, there is broad consensus about the importance of generating original and significant theoretical contributions. A theoretical contribution offers insights that clearly go beyond the diligent reporting of empirical findings and the validation of established knowledge. In particular, as researchers, most of us want to produce not only credible empirical results and revisions of theories but also interesting and influential ideas and theories.

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A fundamental step in all theory development is the formulation of carefully grounded research questions. Constructing and formulating research questions is one of the most, perhaps the most, critical aspects of all research. Without posing questions it is not possible to develop our knowledge about a particular subject. One could even say that good research questions might be as valuable and sometimes even more valuable than answers. Questions may open up, encourage reflection and trigger intellectual activity; answers may lead to the opposite: to rest and closure. Good research questions, however, do not just exist they also need to be created and formulated. As many scholars have pointed out it is particularly important to produce innovative questions which 'will open up new research problems, might resolve long-standing controversies, could provide an integration of different approaches, and might even turn conventional wisdom and assumptions upside down by challenging old beliefs' (Campbell et al., 1982: 21; and see Abbott, 2004; Astley, 1985; Bruner, 1996; Davis, 1971, 1986). In other words, if we do not pose innovative research questions, it is less likely that our research efforts will generate interesting and influential theories. A novel research question may be what distinguishes exceptional from mediocre research and the production of trivial results. Yet, despite the importance of posing innovative questions, little attention has been paid to how this can be accomplished.

In this book we argue that problematization – in the sense of 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. We define both research questions and theory quite broadly. Research questions concern the input and direction of a study, defining what a study is about and reflecting the curiosity of the researcher. Theory is about concepts and relationships between concepts offering a deeper understanding of a range of empirical instances. Theory for us overlaps with ideas – in this book we focus more on the overall theoretical idea than on the fine-tuning of a theoretical framework.

Several factors will influence the development of research questions (such as research funding, publication opportunities, fashion and fieldwork experience), which we will discuss further in [Chapter 2](#). In this book we concentrate on one core aspect, namely, how researchers can construct research questions from *existing academic literature* that will lead to the development of interesting and influential theories. Existing literature can be seen to summarize and express the knowledge and the thinking of the academic community – and to some extent of our time, as there is an overlap in many areas between academic knowledge and broader knowledge shared by educated people more generally. Existing academic literature refers both to the theoretical perspectives and the substantive (empirical) studies conducted within the subject area targeted. A theoretical perspective and an empirical domain may overlap (that is, when a theory is closely linked to a domain, such as classroom theory). However, in other instances they may be more loosely linked, such as when 'grand theories'

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or master perspectives (for example, Marxism, symbolic interactionism, Foucauldian power/knowledge) are applicable to a wide range of subject matters.

Although not many studies have specifically looked at how researchers construct research questions from existing theory and studies, several have come close. For example, Davis's (1971, 1986) research about what defines interesting and famous theories; Campbell et al.'s (1982) investigation of the antecedents of significant (and less significant) research findings; Abbott's (2004) suggestion of using heuristics for generating new research ideas; Starbuck's (2006) advice that researchers should challenge their own thinking through various disruption tactics, including ambitions to take other views than the one favored one into account; and Yanchar et al.'s (2008) study of the components of critical thinking practice in research.

Although these studies point to important ingredients in constructing research questions, they do not specifically focus on how researchers arrive at, or at least claim to have arrived at, their research questions. For example, although Becker (1998) and Abbott (2004) provide a whole range of tricks and heuristics for generating research ideas, those tricks and heuristics 'are not specifically aimed at any particular phase or aspect of the research process' (Abbott, 2004: 112). Existing studies focus even less on the ways of constructing research questions from existing literature that are likely to facilitate the development of interesting and influential theories.

Similarly, in most standard textbooks on research methods the actual ways of constructing research questions are scantily treated or not discussed at all (Flick, 2006). Instead, the primary discussion revolves around how to formulate feasible research questions in a particular sequential order. We are advised to first define the topic (for example, leadership, adult vocational learning, diversity among male engineers, middle-class status anxiety in UK higher education institutions, attitudes to group sex among mature students), then to clarify the domain of the research, that is, what objects should be studied (individuals, social interaction, and so on), state a purpose and finally to decide the type of research questions, such as descriptive, explanatory and prescriptive questions. Some textbooks (for example, Silverman, 2001; Van de Ven, 2007) advise that formulating good research questions does not only involve defining domain, topic, purpose and type of question. It also involves considering contextual issues, such as how various stakeholders, the background and experience of the researcher and the field of study, may influence the formulation of research questions. While important, such advice does not provide specific directions on ways to formulate innovative research questions by scrutinizing existing literature in a particular research area. We will therefore only briefly address such advice. Instead, we will concentrate on what we see as the key issues around constructing research questions from the existing literature that are likely to lead to more interesting and influential theories within the social sciences. In particular, we argue that in order to construct novel research questions from existing literature, careful attention, critical scrutiny, curiosity and imagination together with

the cultivation of a more reflexive and inventive scholarship are needed. We hope that this book will contribute to achieving such an endeavor.

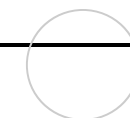
A Paradoxical Shortage of High-Impact Research

The need to better understand how to construct innovative research questions from the existing literature appears to be particularly pertinent today, as there is growing concern about an increasing shortage of more interesting and influential studies in many disciplines within the social sciences (Abbott, 2004; Becker, 1998; Gibbons et al., 1994; Richardson and Slife, 2011; Slife and Williams, 1995). For example, many prominent sociologists, such as Ritzer (1998) and Stacey (1999), are concerned that sociology has 'gone astray' (Weinstein, 2000: 344) in the sense that most sociological research is increasingly specialized, narrow and incremental, and therefore not 'likely to interest a larger audience' (Ritzer, 1998: 447). Similarly, in our own field, the outgoing editors of the *Journal of Management Studies* noted in their concluding editorial piece – based on their review of more than 3000 manuscripts during their six years in office (2003–2008) – that while submissions had increased heavily it is hard to conclude that this has been accompanied by a corresponding increase in papers that add significantly to the discipline. More is being produced but the big impact papers remain elusive ...' (Clark and Wright, 2009: 6).

The perceived shortage of influential ideas and theories, that is, those reaching beyond a narrow and specialized area, is paradoxical in the sense that more research than ever is being conducted and published within the social sciences. The increased use of research assessment reviews in many countries (for example, RAE/REF in the UK and ERA in Australia) and of designated journal lists for evaluating research performance is a central driver behind the rapid growth of articles published within the social sciences. Not only has the number of published journal articles increased substantially but also the competition to get published. Most journals' acceptance rates have been steadily shrinking and are now close to 5% in many top-tier journals. Publishing in these journals is typically a very long and tedious process, involving numerous revisions before getting the final decision, which is usually a rejection. Given all this, one would expect a relative increase in high-quality research, leading to more interesting and influential theories being published. Paradoxically, this is not the case. Quality may have risen in some respects, but hardly the number of interesting and influential theories. Rather than innovation and creativity, it is technical competence and the discipline to carry out incremental research that seem to dominate all the hard-working researchers within the social sciences.

What Differentiates an Interesting from a Non-Interesting Theory?

But why does incremental research rarely seem to generate high-impact theories? In order to answer this question we need first to understand what makes a theory interesting. That is,



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how a theory attracts attention from other researchers and the educated public, leads to enthusiasm, 'aha' and 'wow' moments, and triggers responses like 'I have not thought about this before' or 'Perhaps I should rethink this theme'.

While different people may find different theories interesting and it is a fact that very few theories are seen as interesting by everybody, interestingness is hardly just a matter of idiosyncratic opinion (Das and Long, 2010). Collectively held assessments of what counts as interesting research are much more prevalent than purely subjective views, even though the collective can be restricted to a sub-community (interested in say sexual harassment at a nightclub or Muslim immigrants in Belfast) rather than an entire field (such as higher education or leisure studies).

During the last four decades, originating with Davis's (1971) seminal sociological study, a large number of researchers have shown that rigorously executed research is typically not enough for a theory to be regarded as interesting and influential: it must also challenge an audience's taken-for-granted assumptions in some significant way (Astley, 1985; Bartunek et al., 2006; Hargens, 2000; Weick, 2001). In other words, if a theory does not challenge some of an audience's assumptions, it is unlikely to receive attention and become influential, even if it has been rigorously developed and has received a lot of empirical support. This insight has meant that the criterion of 'interestingness' in most top-tier journals has 'become a staple for editorial descriptions of desired papers' (Corley and Gioia, 2011: 11). We are, however, as we will come back to, skeptical as to the scope and depth of the actual use of this criterion in many situations, as other more conservative criteria often seem to carry more weight for some, if not most, journals (we offer support for this claim in [Chapter 7](#)).

Arguably, there are also other reasons or mechanisms than interestingness for why a theory becomes influential in the sense of garnering citations and sometimes even becoming well known in the public domain. For example, a theory's influence can be related to power relations within academia where a dominant coalition can more or less dictate mainstream, imitation tendencies and fashion following. A theory can also be ideologically appealing and serve broader political interests that are willing to generously fund the 'right' kind of research. The impact and success of a theory may also be dependent on how easy it is to grasp and apply the credentials of its proposer(s) and to what extent it is in line with existing political and social values (Peter and Olson, 1986).

Hence, the answer to why a theory becomes influential is not always because it is seen as interesting but also related to other factors. We shall not venture into this complex area, merely emphasize that a theory regarded by fellow academics and intellectual members of the public as interesting is more likely to become influential in academic disciplines and sometimes also more broadly in society. The fact that other factors than 'interestingness' determine influence does of course not diminish the significance of 'interestingness' as a key element in a theory being influential. Our focus in the book is on the combination of interesting

and influential. Therefore, theories that some people find interesting but which do not attract a larger audience and theories that are influential but which are not considered to be particularly interesting both fall outside our primary focus.

From Gap-Spotting to Problematization

If interesting theories are those that challenge the assumptions of existing literature, *problematization* of the assumptions underlying existing theories appears to be a central ingredient in constructing and formulating research questions. However, established ways of generating research questions rarely express more ambitious and systematic attempts to challenge the assumptions underlying existing theories (Abbott, 2004; Locke and Golden-Biddle, 1997; Slife and Williams, 1995). Instead, they mainly try to identify or create gaps in the existing literature that need to be filled. It is common to refer either positively or mildly critically to earlier studies in order to ‘fill this gap’ (Lüscher and Lewis, 2008: 221) or ‘to address this major gap in the literature’ (Avery and Rendall, 2002: 3). Similarly, researchers often motivate their projects with formulations such as ‘no other studies have examined the associations between children’s belief and task-avoidant behaviour ... which is the focus of the present study’ (Mägi et al., 2011: 665) or ‘our goal in this study was to address these important gaps by focusing on the effects of the group-level beliefs about voice’ (Morrison et al., 2011). Such ‘gap-spotting’ seems to dominate most of the disciplines in social science, or at least management, sociology, psychology and education – areas that we have chosen as samples for illuminating broader conventions in social science. Gap-spotting means that the assumptions underlying existing literature for the most part remain unchallenged in the formulation of research questions. In other words, gap-spotting tends to *under-problematize* the existing literature and, thus, reinforces rather than challenges already influential theories.

There are, however, an increasing number of research orientations that directly or indirectly encourage problematization, such as certain versions of social constructionism, postmodernism, feminism and critical theory. Since the primary aim for many of these orientations is to disrupt rather than build upon and extend an established body of literature, it could be argued that they tend to *over-problematize* the research undertaken. In particular, these orientations tend to emphasize the ‘capacity to disturb and threaten the stability of positive forms of management science’ (Knights, 1992: 533) as a way to highlight what is ‘wrong’ (for example, misleading or dangerous) with existing knowledge (Deetz, 1996), that is, ‘negative’ knowledge is the aim (deconstruction being the ideal). This is often interesting and valuable but such ‘tearing down’ may also be tiresome after some time. For a large majority of researchers with a more ‘positive’ research agenda that aims to advance knowledge of a specific subject matter, such over-problematization is often seen as inappropriate and unhelpful (Rorty, 1992). In addition, a lot of disturbance-specialized research, which could be referred to as programmatic problematization, tends (after some time) to reproduce its own favored assumptions and thereby lose its capacity to provide novel problematizations.

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Nevertheless, we do consider this kind of research to offer valuable resources to challenge the assumptions of various literatures.

Aim of the Book

The primary aim of this book is to integrate the positive and the negative research agenda by developing and proposing problematization as a *methodology* for identifying and challenging assumptions that underlie existing theories and, based on that, generating research questions that will lead to the development of more interesting and influential theories within social science. Such a problematization methodology enables researchers to embark on a more interesting and rewarding course (although perhaps also more difficult and risky) than following established and conventional routes for producing knowledge in a safe and predictable way.

A key theme in the book is a general argumentation for, and the offering of, a *framework* and *vocabulary* with which to conduct more interesting and influential studies, to indicate pitfalls as well as possibilities. In particular, this book suggests a reframing of the research practice within the social sciences by proposing a revision of how we approach research questions: from gap-spotting to assumption-challenging, from reproducing to disrupting the use of taken-for-granted beliefs and points of departure in inquiries. This revision not only concerns changes in theory and methodology it also encompasses social and political aspects. Research never takes place in a social vacuum and revised views of what is good research call for consideration of the social context, as well as how researchers define themselves in the research process.

In order to explore our key themes we investigate and answer the following questions: (1) How do social researchers produce their research questions? (2) What norms guide the production of research questions? And (3) What is seen as leading to interesting and influential theories? Based on those investigations, we develop and propose problematization as a methodology to challenge assumptions and to develop research questions that are more likely to lead to interesting theories. Specifically, we develop (1) a typology of what *types* of assumptions can be problematized in existing theories and propose (2) a set of methodological principles for *how* this can be done. We also provide (3) detailed examples of problematization and the formulation of novel, often counter-intuitive, research questions that can encourage more imaginative empirical studies.

This problematization methodology is the book's core contribution, and also the main theme for [Chapters 5](#) and [6](#). But in order to increase the chances of doing more imaginative, interesting and (theoretically) influential research, we also need to understand the mechanisms behind doing less interesting work (which are the focus in [Chapter 3](#) and [4](#)). Moreover, we develop a framework for understanding the forces within academia that work *against* the research ideal we (and to a degree all of us) embrace and illuminate the

significance of researchers' *identities* and ethos in knowledge production. Here, we emphasize the need for researchers to think through the purpose of their research and knowledge contributions and to resist pressures to adapt to dominant assumptions and be normalized, as well as normalizing others (Foucault, 1980).

We focus on problematizing assumptions that underlie *existing literature* as a way to construct research questions. We define research questions quite broadly; they not only indicate the delivery of a specific intended result, they also provide the broader framing of the study, that is, its overall direction and line of reasoning based on a set of assumptions and 'truths' already inscribed on the discourse guiding the inquiry. In other words, research questions give the major input, frame the research and provide direction-setting to research studies and, thus, form a key element of the research process. Therefore, research questions and the way they are addressed need to incorporate reflexivity in the sense of an explicit questioning and articulation of where the chosen research approach originates, where it is heading and what may be problematic about it.

We only briefly discuss how other aspects of the research process, such as a general interest in policy, institutional stakeholders and public debate, relevance for practitioners, choice of empirical case and unexpected empirical findings, may influence the research objective and, thus, the formulation of research questions. There is also a large and overlapping literature on reflexivity dealing with these aspects of research (Alvesson and Sköldberg, 2009; Steier, 1991) that is highly significant (although its relative importance varies between research projects). However, as our emphasis is on how to work with reflexivity when formulating research questions, we only marginally address other issues of reflexivity, such as invoking self-awareness in the researcher, the role of rhetoric and ongoing constructions of reality in the research process. An exception is the socio-political context of research, which is a key issue for how researchers relate to existing work (Alvesson, Hardy and Harley, 2008). Therefore, problematization studies need to seriously consider how other researchers may be skeptical of or even hostile towards research challenging their (favored) assumptions. We specifically deal with this issue in [Chapter 7](#).

How This Book is Organized

In this chapter we have tried to place the book in the broader context of research methodology and argue for much more care in the critical investigation of how research questions are formulated and how theoretical inspiration can be used in the formulation of research questions. In particular, we have issued a warning against the risk of uncritically reproducing a set of assumptions that underlie the existing literature and may no longer be very productive and interesting when constructing research questions. We contend that this is common practice and that there is a shortage of novel thinking in many fields within social science.

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In [Chapter 2](#) we further elaborate on our quest to examine how researchers can generate research questions from existing literature by situating the research more precisely in the larger context of constructing and formulating research questions. We start by defining in what sense questions are crucial in knowledge development. We then discuss more specifically what makes a question a research question, what major types of research questions exist, from where research questions originate, and what influences the framing of research questions. Finally, we summarize the chapter by discussing the main stages involved in constructing and formulating research questions.

Subsequently in [Chapters 3](#) and [4](#), we present an empirical study of how researchers typically construct their research questions from existing literature by systematically reviewing 10 leading journals from four different disciplines within the social sciences (management, sociology, psychology, and education). We also refer to some studies in other fields and how we can make a case for a common state of play across the social sciences as a whole. Our findings suggest that the most widespread way of producing research questions is that which we label gap-spotting, namely, to spot various gaps in existing literature, such as an overlooked area and, based on those gaps, formulate specific research questions. We provide a typology of gap-spotting and critically discuss the limitations and problems of gap-spotting research. In particular, we argue that gap-spotting questions are unlikely to lead to significant contributions because they do not question the assumptions which underlie the existing literature in any substantive way.

[Chapter 5](#) develops from this starting point. We elaborate and propose problematization as an alternative methodology for generating research questions in three steps. First, we describe the aim and focal point of the methodology, as challenging the assumptions underlying existing literature. Second, we elaborate a typology consisting of five broad types of assumptions that are open for problematization in existing theory. Finally, we develop a set of methodological principles for identifying, articulating and challenging the assumptions underlying existing literature.

In [Chapter 6](#) we illustrate how the developed problematization methodology can be used to generate research questions by applying it to two key texts within the social sciences. One is Dutton, Dukerich and Harquail's well-known (1994) article about organizational identity and identification with workplaces. The second is West and Zimmerman's (1987) classic work 'Doing gender'.

In [Chapter 7](#) we critically discuss why gap-spotting is common and assumption challenging is rare despite increasing recognition that the latter leads to the development of more interesting and influential theories. We point to three broad and interacting drivers: institutional conditions; professional norms; and researchers' identity construction. We also elucidate possible solutions at a variety of levels, arguing for changes in terms of institutional and organizational structures and practices, revisions of the norms of academic publishing and the

need for academics to reconsider their identities and methodological ideals. In particular, in contrast to the prevalent opportunistic maximization of getting published in high-ranking journals and climbing the academic career ladder as fast as possible and support for this in managerialist universities, we draw attention to the centrality of developing a more reflexive and inventive scholarship for universities and researchers.

In [Chapter 8](#) we summarize the general argument of the book. We begin by elaborating on the major contributions the problematization methodology can make to social science. Thereafter, we briefly discuss in which situations the problematization methodology may be particularly relevant. Finally, we relate the problematization methodology to the overall research process and discuss how the problematization of existing literature can be complemented by empirical material in constructing and formulating novel research questions.

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