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Ways of constructing research questions: gap-spotting or problematization?

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Abstract
This article examines ways of constructing research questions from existing literature, which are likely to promote the development of interesting and influential theories. We review 52 articles in organization studies and develop a typology of how researchers construct their research questions from existing literature. The most common way across paradigmatic camps is to spot various 'gaps' in the literature and, based on that, to formulate specific research questions. The dominance of gap-spotting is surprising, given it is increasingly recognized that theory is made interesting and influential when it challenges assumptions that underlie existing literature. The article discusses why assumption-challenging approaches are rare, and it identifies a range of social norms that favour gap-spotting. Finally, the article proposes some ways of constructing research questions that move beyond gap-spotting, and discusses how these ways are likely to promote more interesting and significant theories.

Keywords
interesting theories, problematization, research methods, research problems, research questions, theory development

It is fundamental to all research to formulate carefully grounded research questions. As many scholars have pointed out (e.g. Abbott, 2004; Astley, 1985; Bruner, 1996; Campbell et al., 1982; Davis, 1971, 1986; Starbuck, 2006; Van de Ven, 2007; Weick, 1989) 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

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wisdom and assumptions upside down by challenging old beliefs' (Campbell et al., 1982: 21). In other words, if we do not pose innovative research questions, it is less likely that our research efforts will generate interesting and significant theories. Yet, despite the importance of posing innovative questions, little attention has been paid to how this can be accomplished. While there are several factors that influence the development of research questions (such as research funding, publication opportunities, fashion and fieldwork experience), in this study we concentrate on one core aspect, namely, how researchers construct research questions from *existing literature*.

Although not many studies have specifically looked at how researchers construct research questions from existing theory, several have come close: Davis's (1971, 1986) research about what defines interesting and famous theories, further developed by Astley (1985) and Weick (1989) in relation to organization studies; Locke and Golden-Biddle (1997) and Golden-Biddle and Locke (2007) studies on how researchers create opportunities for contributions in research texts; Campbell et al.'s (1982) investigation of the antecedents of significant (and less significant) organizational research findings; Frost and Stablein's (1992) study on exemplary organizational research; Abbott's (2004) suggestion of using heuristics for generating new research ideas; Smith and Hitt's (2005) examination of how the 'masters' in management and organization research (all of which of course are US researchers or US educated) generate their theories and Starbuck's (2006) advice that researchers should challenge their own thinking through various disruption tactics. While these studies point to important ingredients of good research questions, they do not specifically focus on how researchers arrive, or at least claim to have arrived at, their research questions. They focus even less on what ways of constructing research questions from existing literature are likely to facilitate the development of interesting and significant theories.

Similarly, in most standard textbooks on research methods the actual ways of constructing research questions are scantly 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 (leadership, organizational change, etc.), then to clarify the domain of the research, that is, what objects should be studied (individuals, social interaction, etc.), and finally decide the type of research questions, such as descriptive, explanatory and prescriptive questions. Some textbooks (e.g. Easterby-Smith et al., 2002; Silverman, 2001; Van de Ven, 2007) advise that formulating good research questions does not only involve defining domain, topic and type of questions, but also considering contextual issues, such as how various stakeholders may influence the formulation of research questions, as well as the background and experience of the researcher and the field of study. While important, such advice does not provide more specific directions on ways to formulate innovative research questions by scrutinizing existing literature in a particular research area.

This study explores three questions:

(1) how do organizational researchers construct their research questions from existing literature, as expressed in research texts?
(2) what ways of constructing research questions are likely to facilitate the development of interesting and influential theories?
(3) what and how do social norms guide researchers to construct research questions in particular ways and not others?

We address the first question by reviewing a number of research texts in organization studies and, in turn, propose a typology of ways of constructing research questions. Our findings suggest that the most common way of producing research questions is to spot various gaps in existing

literature, such as an overlooked area and, based on that, to formulate specific research questions. Gap-spotting is of course not something absolute but varies in both size and complexity: from incrementally extending an established theory to identifying more significant gaps in existing literature. Our second question leads to the argument that gap-spotting questions are unlikely to lead to significant theories because they do not question the assumptions which underlie existing literature in any substantive ways. In other words, gap-spotting is more likely to reinforce or moderately revise, rather than challenge, already influential theories. The dominance of gap-spotting is surprising given the increased recognition that challenging assumptions is what makes a theory interesting. Moreover, given the availability of ideas and perspectives that emphasize problematization, it is perhaps even more surprising that challenging assumptions which underlie existing theory is rarely used in formulating research questions. In order to understand why challenging assumptions is considerably less common than expected, we investigate, as our third question, what and how social norms guide researchers to favour gap-spotting over other possible ways of constructing research questions. Finally, we identify some paths that move beyond gap-spotting, and discuss how these are likely to lead to more interesting and significant theories.

Ways of constructing research questions in organization studies

By examining how researchers construct their research questions from existing literature in research texts we are, in one sense, unable to say anything about how researchers 'really' arrived at their research questions. It is a complex issue, involving a variety of interacting processes and influencing elements, such as timing, chance, actively seeking exposure of different views and being immersed in the literature (Campbell et al., 1982; Frost and Stablein, 1992; Smith and Hitt, 2005) as well as paradigmatic, social and cultural conditions. However, we believe that research texts indicate something about how researchers develop research questions from existing theory and, under all circumstances, highlight the logic behind their claim to make a contribution to the scientific field, which is highly important in itself. More cautiously, by investigating research publications we can at least say something about the social norms and methodological rules that are likely to influence how researchers construct their research questions from existing literature. These norms are likely to have 'truth-creating' effects—the way researchers present their ways of formulating research questions influence other researchers and frame the scientific field.

In other words, while it is difficult to assess what research texts may say about how researchers really came up with their research questions at the beginning of their projects, the research text can be seen as the key stage in the formulation of research questions. It is in the crafting of the research text that the final research question is constructed, which is the one that specifies the actual contribution of the study. In some cases, the formulated research question is likely to be very similar to the ideas and purposes that inspired the researcher at earlier stages in the research process. In other cases, the research question presented in the research text may bear stronger imprints from considerations of how to craft a persuasive text (rhetoric) and thus differ from what was expressed in the initial stages of the research process.

Hence, by studying research texts, we do not claim that we can say anything about what actually influenced the emergence of the research question during the first stages of the research process. Instead, what we offer is an inquiry of the relationship between literature in a field and the (formulated) research question guiding the delivery of the (final) results—as expressed in the scientific text. This means that any strong distinction between what 'really' guided the researcher and 'pure' rhetoric is of less relevance here. Rather than the 'real' or the 'presented' research question, we are interested in the constructed research question—the one presented in the publication and governing

the actual contribution. In particular, we claim that studying research texts reveals some important social norms and methodological principles for how researchers construct research questions from existing theory.

Investigating scientific texts for understanding the research process has also been common practice within the sociology of science for a long time (e.g. Davis, 1971, 1986; Knorr-Cetina, 1981; Latour and Woolgar, 1979; Locke and Golden-Biddle, 1997). The research that probably comes closest to the study reported here, are Locke and Golden-Biddle (1997) and Golden-Biddle and Locke's (2007) studies of how researchers create an opportunity for contribution in scholarly journals. Adopting a social constructionist perspective, Locke and Golden-Biddle (1997) conducted an empirical investigation of 82 qualitative studies published in *Administrative Science Quarterly* (61 studies) and *Academy of Management Journal* (21 studies) between 1976 and 1996.

According to their study (Locke and Golden-Biddle, 1997), establishing opportunities for contributions includes two main processes, namely, structuring intertextual coherence and problematization. In structuring an intertextual field, the researcher tries to bring together existing studies into a context for contribution 'that reflects the consensus of previous work' (p. 1029). They identified three textual strategies for connecting existing studies into a context for contribution: synthesized coherence, progressive coherence and non-coherence. When using *synthesized coherence*, researchers 'cite and draw connections between works and investigative streams not typically cited together to suggest the existence of underdeveloped research areas' (p. 1030). In contrast, *progressive coherence* is used for establishing a context for contribution characterized by a network of studies that are linked by 'shared theoretical perspectives and methods working on research programs that have advanced over time' (p. 1030). Finally, *non-coherence* is used for describing a common research field marred by disagreement.

In the second process, the researcher 'problematizes' the established context of contribution as being deficient in some way in order to open up 'opportunities for advancing knowledge about topics of investigative concern' (p. 1029). According to Locke and Golden-Biddle, almost half of the studies used an *incompleteness* strategy to construct an opportunity for contribution by claiming that existing literature was incomplete in some way or another, and that the researcher's own study would be able to advance it. Another common way was to critique existing literature for being *inadequate* in some significant way. Here, it is claimed that prior literature has overlooked an important perspective which would have had the potential to further our understanding of the subject matter. A third and noticeably more uncommon way of questioning existing theory was to claim that it is *incommensurate*. Advocates of these few (eight) studies argued that existing literature not only neglected certain perspectives but also misguided the way knowledge was produced about the subject matter in question. The opportunity for contribution then is to provide a superior perspective that will correct the faulty existing literature. Locke and Golden-Biddle's (1997) findings have also been confirmed more recently in other research areas, such as information systems (Barrett and Walsham, 2004) and marketing (Johnson, 2003).

Constructing research questions and creating an opportunity for contribution are fairly close in the sense that a created space for contribution points to a specific question, and vice versa. It is therefore likely that Locke and Golden-Biddle's (1997) analysis reveals important aspects of how researchers construct research questions from existing literature in research texts. For example, the strategies used for creating an opportunity for contribution indicate that a central component in researchers' ways of constructing research questions is to review existing literature in order to find some deficiency in it (is it incomplete, inadequate, or even incommensurate) and, based on that, to motivate and specify their particular research questions. However, although Locke and Golden-Biddle's study provides valuable insights and conceptualizations of how researchers construct

research questions from existing literature, their primary aim was not to articulate how researchers arrived at their research questions and, even less, to investigate which routes were likely to lead to the development of interesting theories.

Therefore, the study reported here both extends and differs from Locke and Golden-Biddle in three important ways. First, this study specifically investigates and proposes a distinct typology of how researchers construct research questions from existing literature, with a particular focus on which ways that are likely to lead to interesting and significant theories. Second, we examine which social norms (broadly speaking) guide researchers' ways of producing research questions (in research texts). By investigating which social norms influence the development of research questions, we also go beyond the pure rhetorical focus applied in Locke and Golden-Biddle's study. Third, as outlined below, we also complement Locke and Golden-Biddle's study in three distinct ways, our study: (a) covers a broader set of journals with a significantly higher representation of European scholarship; (b) in contrast to Locke and Golden-Biddle, who selected only qualitative studies, our study consists of a mix of qualitative and quantitative studies and (c) while Locke and Golden-Biddle studied articles published during 1976–1996, we investigate a more recent sample of research texts published between 2003–2005. Perhaps the increasing popularity of 'sceptical' approaches like post-structuralism and critical studies has modified the assessment made by Locke and Golden-Biddle?

Method and research design

In order to identify how researchers construct research questions from existing literature, we reviewed eight randomly selected issues from the following four leading US and European journals: *Administrative Science Quarterly, Journal of Management Studies, Organization Studies* and *Organization*. We selected two issues from each journal from 2003–2005. We avoided special issues. The main reasons for the choice of those journals were that they are all seen as premier outlets for leading edge management research, they represent a fairly good spread when it comes to what research they publish, and they complement Lock and Golden-Biddle's study in important ways.

The total number of articles reviewed amounted to 52. About half of the authors were from North America and the remaining from the rest of the world (mainly from UK). Similar to Locke and Golden-Biddle (1997), although we examined the entire article, we concentrated on the first part of the articles (from introduction to method section), as it was there that the researchers most clearly expressed their ways of constructing research questions from existing literature. In particular, we carefully read the selected research texts and looked for key statements in which the authors signalled how they constructed their research questions from earlier research and theory. In the subsequent analysis we searched for the meaning underlying the logic behind the ways of constructing research questions rather than using a detailed coding, as in grounded theory. Generally, the logic was often explicitly stated in the research texts. For instance, Musson and Tietze (2004: 1301) formulated their research question by saying that they would 'address this gap' in the literature. Similarly, Westphal and Khanna (2003: 363) argued that they would 'extend this literature' in a specific way. Hence, in most cases the different modes of constructing research questions described below suggested themselves more or less directly from the selected research texts.

Gap-spotting: the prevalent way of constructing research questions

The most dominant way of constructing research questions in our empirical material was gap-spotting. Researchers reviewed existing literature with the aim of spotting gaps in the literature and, based

on that, formulated specific research questions. We identified three basic modes of gap-spotting, namely, confusion, neglect and application spotting. We were also able to distinguish specific versions of spotting gaps within each basic gap-spotting mode. The basic gap-spotting modes and their specific versions are summarized in Table 1 and further elaborated below.

Table 1. Basic modes of gap-spotting and their specific versions

Basic gap-spotting modes	Specific versions of basic gap-spotting modes	Reviewed journal articles
Confusion spotting	Competing explanations	Anderson and Reeb (2004; *ASQ* 49(2): 209–37), Burnes (2004; *JMS* 41(6): 977–1002), Gibbons (2004; *ASQ* 49(2): 238–62) Queen (2005; *ASQ* 50(4): 610–41), Schneper and Gullien (2004; *ASQ* 49(2): 263–95), Thomson and Walsham (2004; *JMS*, 41(5): 726–47).
Neglect spotting	Overlooked area	Arend (2004; *JMS* 41(6): 1003–27), Brown (2004; *OS* 25(1): 95–112), Chreim (2005; *JMS* 42(3): 595–623), Davenport and Leitch (2005; *OS* 26(11): 1603–23), Ezzamel (2004; *ORG* 11(4): 497–537), Hannan et al. (2003; *ASQ* 48(3): 399–432), Jensen (2003; *ASQ* 48(3): 466–97), Korczynski (2005; *JMS* 41(4): 575–99), Marchington and Vincent (2004; *JMS* 41(6): 1029–56), Meriläinen et al. (2004; *ORG* 11(4): 539–64), Mueller et al. (2004; *OS* 25(1): 75–93), Musson and Tietze (2004; *JMS* 41(8): 1301–23) Nicolai (2004; *JMS* 41(6): 951–76), Ogbonna and Wilkinson (2003; *JMS* 40(5): 1151–78), Sidhu et al. (2004; *JMS* 41(6): 914–32), Sims (2005; *OS* 26(11): 1625–40), Sparrowe and Liden (2005; *ASQ* 50(4): 505–35), Vaara et al. (2005; *JMS* 42(3): 572–93), Zarraga and Bonache (2005; *OS* 26(5): 661–81).
	Under-researched	Balogun and Johnson (2005; *OS* 26(11): 1573–1601), Baum et al. (2005; *ASQ* 50(4): 536–75), Brickson (2005; *ASQ* 50(4): 576–09), Case and Phillipson (2004; *ORG* 11(4): 473–95), Chan (2005; *JMS* 42(3): 625–72), Corley and Goia (2004; *ASQ* 49(2): 173–08), Javidan and Carl (2004; *JMS* 41(4): 665–91), Munir and Phillips (2005; *OS* 26(11): 1665–87), Symon (2005; *OS* 26(11): 1641–63), Tsui-Auch (2004; *JMS* 41(4): 693–23), Westphal and Khanna (2003; *ASQ* 48(3): 361–98), van Breugel et al. (2005; *JMS* 42(3): 539–66).
	Lack of empirical support	Dyck et al. (2005; *JMS* 42/2: 387–16) Tyrrell and Parker (2005; *JMS* 42/3: 507–37)

Table 1. (Continued)

Basic gap-spotting modes	Specific versions of basic gap-spotting modes	Reviewed journal articles
Application spotting	Extending and complementing existing literature	Clegg and Courparsson (2004; *JMS* 41/4: 525–47), Hancock (2005; *ORG* 12/1: 29–52), Hodgson (2005; *ORG* 12/1: 51–69), Korsczynski and Ott (2004; *JMS* 41/4: 575–99), Maguire (2004; *OS* 25/1: 113–34), Nickerson and Silverman (2003; *ASQ* 48/3: 433–65), Putnam (2004; *OS* 25/1: 35–53), Rosenthal (2004; *JMS* 41/4: 601–21) Schultz and Stabell (2004; *JMS* 41/4: 549–74), Tell 2004; *ORG* 11/4: 443–471), Watson (2004; *JMS* 41/3: 447–67), Wright and Manning (2004; *JMS* 41/4: 623–43), Zanoni and Janssens (2004; *OS* 25/1: 55–74).

ASQ, *Administrative Science Quarterly*; *JMS*, *Journal of Management Studies*;
OS, *Organization Studies*; *ORG*, *Organization*.
Complete references for the articles can be obtained from the author.

Confusion spotting

The main focus in this way of constructing research questions is to spot some kind of confusion in existing literature. Previous research on the topic exists, but available evidence is contradictory. The research question aims to sort out the identified confusion in the literature and to explain it. The main version of this mode of constructing research questions was to search for *competing explanations* in existing literature.

Anderson and Reeb's (2004) study of board composition can be seen as a typical representative of this particular version. According to them, it is widely recognized in the corporate governance literature that large shareholders, such as founding families tend to exploit smaller shareholders' portion of the firm's wealth. There is also a fairly widespread belief that independent directors are in a position to reduce the above conflict. There are, however, competing explanations for this. According to agency theory, independent directors can defend smaller shareholders' interests by executing control over founding families' tendency to exploit the firm's wealth at the cost of small shareholders. On the other hand, stewardship theory claims that because founding families identify themselves closely with their firms, they tend to regard the firm's health 'as an extension of their own well-being' (Anderson and Reeb, 2004: 211), and are therefore keen to bring in independent directors who can advise on ways to keep the firm fit and profitable. Anderson and Reeb try to 'disentangle these competing theories—agency theory and stewardship—by examining the influence of affiliated directors … and by examining the manner in which independent directors gain their board seats amongst family and non-family firms' (Anderson and Reeb, 2004: 211).

Another example of using competing explanations in the literature to construct research questions is Gibbons's (2004: 238) study of how social networks influence professional values. In her review of the literature, she finds strong evidence 'that social systems tend toward consensus in

values amongst their members at the same time as there are ample studies that report "discrepancies among individuals" values in organizations'. According to Gibbons, this leads to two related questions. 'First, which informal social forces support convergence and which foster divergence from established professional values? Second, as professional values change, how does this influence existing social networks?'

Neglect spotting

Spotting something neglected in existing literature is the most common mode of constructing research questions in our sample. It tries to identify a topic or an area where no (good) research has been carried out. There is virgin territory—a white spot on the knowledge map—that produces an imperative for the alert scholar to develop knowledge about the neglected area(s). It is possible to distinguish three specific versions of neglect spotting, namely, spotting an overlooked area, an under-researched area and a lack of empirical support.

The most common version of neglect spotting was to search for aspects in existing literature that have been *overlooked* despite a wealth of studies. Musson and Tietze's (2004) study, 'Places and spaces: The role of metonymy in organizational talk' can be seen as representative of the overlooked version of neglect spotting. Based on a review of existing literature, they conclude that while cultural meaning making in organizational talk is an established area of research, the discursive mechanics of the process in which meaning is created and maintained have been largely overlooked in existing literature. They then go on to claim that 'we will address this gap [in the literature] by carrying out a metonymical analysis of organizational talk about physical places and spaces…' (Musson and Tietze, 2004: 1301).

Searching for *under-researched* areas in existing literature is another common version of neglect spotting. A good representative of this route is Corley and Goia's (2004) study of ambiguity in organizational identity change in corporate spin-offs. They review the organizational change literature and conclude that a strong bias exists in the literature toward empirical examinations of additive types of changes, such as mergers and assimilations. Much less attention has been paid to studying subtractive changes like downsizing or spin-offs. 'Even a cursory review of the change literature reveals a strong bias toward additive changes … changes involving subtraction, such as spin-offs, equity carve-outs, and de-mergers have been understudied' (Corley and Goia's, 2004: 174). They further specify their research question by reviewing existing literature on organizational identity in general, and organizational identity change in particular. In their review they found that 'although past research has provided insight into why organizational identities change … it has not provided adequate insight into *how* organizational identity change can occur' (Corley and Goia, 2004: 174, emphasis in original). Thus, they set out to study how organizational identity changes occur in spin-off organizations.

A similar but different version of neglect spotting is to search for areas in existing literature that *lack empirical support*. For example, in reviewing the literature on organizational learning, Dyck et al. (2005) come to the conclusion that most of the theoretical concepts and models that were supposed to capture the nature of organizational learning have had little empirical support. This led them to study the extent to which current key concepts and models of organizational learning have been empirically supported.

Application spotting

Spotting a new application in existing literature is a third basic mode of constructing research questions. It searches mainly for a shortage of a particular theory or perspective in a specific area of

research. The research task is to provide an alternative perspective to further our understanding of the particular subject matter in question. Typically, advocates of application spotting claim that a specific body of literature needs to be *extended* or *complemented* in some way or another. An example of this version can be found in Watson's (2004) study 'HRM and critical social science analysis'. He reviews existing HRM literature and concludes that it lacks a more critical perspective. Too much of the HRM literature is prescriptive and normative. In order to address this inadequacy, Watson's research task is to introduce a critical theory perspective into current HRM literature.

Combinations

While many of the studies reviewed emphasize one major way of constructing research questions, *combinations* of different gap-spotting ways are not uncommon. For example, in constructing their research question, Schultz and Stabell (2004: 550) adopt an approach which is partly confusion-spotting, and partly application-spotting. Their aim is to 'explore the contradictory, double-edged nature of knowledge by developing a theory-informed framework that highlights different assumptions about knowledge and its management'. The framework draws upon the paradigm framework of Burrell and Morgan (1979) and the revision of this by Deetz (1996).

Does gap-spotting lead to interesting theories?

From the above analysis we can conclude that gap-spotting is by far the most common way of constructing research questions within organization studies (as stated within research texts). A central question is to what extent gap-spotting is likely to facilitate a development of interesting and influential theories. The answer depends largely on what we mean by an 'interesting' theory. The hallmark of a 'good' theory has been that it is truthful to its subject matter and derived from rigorous research. But as Astley (1985), Bartunek et al. (2006), Campbell et al. (1982), Davis (1971, 1986), McKinley et al. (1999), Miner (1984) and Weick (1989) have pointed out, truth and rigorously executed research are typically not enough for a theory to be regarded as interesting and significant. According to Davis's (1971) comprehensive study, what makes a theory interesting, and sometimes even famous (Davis, 1986), is that it challenges its audience's assumptions about the subject matter in some significant way. As Davis expressed it, 'the social researcher who wants to be certain that he (sic) will produce an *interesting* theory about his subject must first familiarize himself with what his audience already assumes to be true about his subject, before he can even begin to generate a proposition which, in denying their assumption, will attract their attention' (1971: 337, emphasis in original).

However, as both Davis (1971) and Bartunek et al. (2006) noted, a study will not be seen as interesting if it denies all of the audience's assumptions. If the audience's whole assumption ground is disputed, it is likely that most readers will regard the study as irrelevant and absurd. Instead, it is those studies that challenge some, but not all, of the readers' assumptions that will be regarded as interesting and important. Similarly, in their study of what is necessary for a theory to become established as a new theoretical school, McKinley et al. (1999) emphasize the importance of a balance between novelty and continuity. In order to receive attention, a theory must differ significantly from the existing ones at the same time as it must be connected to established literature in order to be seen as meaningful. The importance of challenging assumptions and creating novelty is further supported by Hargens' (2000) study about how researchers use references. He shows, hardly surprisingly, that studies that have broken new grounds are significantly more cited than those that primarily build on the foundational articles in an already established research area.

However, if we look at how the researchers describe their ways of constructing research questions in the 52 journal articles analysed in this study, none of them actually make a deliberate attempt to challenge the assumptions underlying existing theories of the subject matter in question. The use of gap-spotting is not only evident in more 'positivist' research studies, but spans theoretical camps. For example, Case and Phillipson (2004: 473) work within a poststructuralist framework informed by Foucault and Burrell's work. They describe their ways of constructing research questions as gap-spotting by identifying an under-researched area: 'To the best of our knowledge, there have been no reputable investigations of the influence of astrology and alchemy on practices within the contemporary worlds of organization and management'. Similarly, Vaara et al. (2005) , who apply a linguistic-power framework informed by theorists, such as Clegg and Foucault, together with post-colonial research, justify their research question by spotting 'natural languages' as an overlooked area in organization studies: 'Organizational scholars have examined language use in organizational processes and practices and highlighted the culture-knowledge-power linkages from the early 1970s … [n]atural languages have, however, received very little explicit attention by organization scholars' (2005: 597). The same applies to almost all studies discussed by Locke and Golden-Biddle (1997). Only eight out of 82 used an incommensurability strategy, and did so only to a limited extent. When researchers adopt a critical perspective, it often involves certain challenges, and the outcomes may be quite upsetting for people who hold conventional beliefs. Yet when it comes to formulating research questions, those researchers often accept the existence of a particular subject matter, such as Watson (2004) did in his study regarding HRM, with the existence of HRM as a subject matter motivating a critical perspective.

Problematization: an obvious alternative

If we take seriously the insight that challenging the assumptions which underlie existing theory is a core ingredient in making a theory interesting and influential, an obvious way of constructing research questions is *problematization*. Although it could be argued that every scientific inquiry involves some form of 'problematization' (Dewey, 1938; Foucault, 1972; Freire, 1970; Locke and Golden-Biddle, 1997; Mills, 1959), we do not have in mind Locke and Golden-Biddle's (1997) broad and 'generous' definition of problematization as critiquing established literature with the aim of spotting a gap. Instead, we are closer to Foucault's (1985: 9) conceptualization of problematization as an 'endeavour to know how and to what extent it might be possible to think differently, instead of what is already known'. A central goal in such problematization is to try to disrupt the reproduction and continuation of an institutionalized line of reasoning. It means taking something that is commonly seen as good or natural, and turning it into something problematic. Specifically, problematization as we define it here aims to question the assumptions underlying existing theory in some significant ways. Moreover, we do not see problematization as an end in itself, as is done in some research orientations (e.g. postmodernism, feminism and critical theory) further described below. Instead, we see it as a means to identify and challenge assumptions underlying existing theory and, based on that, being able to formulate more informed and novel research questions.

Similar to gap-spotting, problematization also varies in range and complexity: from questioning minor assumptions underlying existing theory to the problematization of assumptions that may underlie an entire theoretical field such as leadership (Alvesson and Sandberg, 2010). Therefore, problematization does not need to involve challenging the assumptions underlying an entire paradigm and, thus, produce a scientific revolution (Kuhn, 1970). It can equally be about challenging some moderate assumptions which underlie existing theories within a particular school of thought or intellectual tradition. It does, however, go beyond minor critique or revisions of a puzzle-solving nature.

Hence, while gap-spotting means identifying various gaps in existing literature (e.g. confusion, neglect and application spotting), it does not actively challenge the assumptions underlying existing theory. Problematization, on the other hand, is about challenging assumptions which underlie existing theory in some significant ways. When some important assumptions are problematized, this becomes an opportunity for critical insights and new ideas of a more radical character.

Interesting research results can, of course, be produced in other ways than challenging assumptions which underlie existing theory. We certainly do not think that all studies would benefit from only or mainly giving priority to problematization in formulating research questions. However, we emphasize problematization as a key element in producing new and inspiring points of departures for *theory development.* Given that several 'problematization turns' in social science have provided resources for rethinking established ideas and ways of ordering/freezing the social world into specific phenomena, today's researchers ought to be in a particularly favourable position for using problematization as a way of constructing research questions.

Why use gap-spotting and reduce the chance to create interesting theories?

During the last three decades, several 'problematization turns', such as the interpretive (Geertz, 1993; Rabinow and Sullivan, 1987; Smircich, 1983), political (Alvesson et al., 2009; Burrell and Morgan, 1979; Foucault, 1977), linguistic (Grant et al., 2004), constructionist (Gergen, 1992; Sandberg, 2001) and, perhaps most notably, postmodernist (Cooper and Burrell, 1988; Deetz, 1992; Knights, 1992; Rosenau, 1992) have taken place in organizational science and social sciences more generally. They have provided a rich variety of ideas, perspectives and literature that: (a) encourage a challenging of assumptions, a rethinking of received wisdom and established vocabularies and (b) provide a wealth of resources for doing so. The latter includes knowledge about paradigm relativity, different theoretical vocabularies, methodological critiques and the role and importance of considering alternative languages and modes of construction, as well as a familiarity with postmodernistic ideas, such as defamiliarization, deconstruction and resistance readings.

Given the availability of ideas and perspectives that emphasize problematization in research, it is remarkable that the majority of researchers, from positivists to postmodernists, seem to use gap-spotting, rather than problematization, as their preferred way of constructing research questions. Indeed, we do it to a high degree ourselves in this article—motivating our approach with the absence of studies on how researchers construct their research questions. We do not, of course, deny the existence of efforts to rethink other approaches in organization studies. There are several high-profile examples in organization studies (e.g. Brunsson, 1985, 2003; Perrow, 1978; Weick, 1995, see also Barley, 2006 for a discussion), although many of these 'import' ideas and more or less 'ready-made' problematizations from master thinkers and traditions, such as a Baudrillardian (Grandy and Mills, 2004) or a Foucauldian perspective on a particular field (e.g. Knights and Morgan, 1991; Townley, 1993). While such applications are commendable, they tend to be specialized examples of a particular 'problematization approach' rather than a more genuine problematization. In other worlds, they are often a prolongation and application of someone else's problematization and are not in themselves examples of how the research question is an outcome of active problematization.

While it was rather surprising that only a few signs of ambitious problematization appeared in Locke and Golden-Biddle's sample from 1976–1996, it was even more unexpected that our more contemporary sample from 2003–2005 showed the same pattern. The shortage of examples in Locke and Golden-Biddle's (1997) and our own sample, suggests that the various problematization turns may have put their imprints on debates and specialized work but have not really influenced

the formulations of research questions in organization studies. An interesting question is thus*: why does gap-spotting continue to be the dominant way of constructing research questions from existing literature, as expressed in research texts?*

One possible explanation is that researchers are far more calculative than how they appear in research texts. They may be *wolves in sheeps' clothing* in the sense of using gap-spotting as a rhetorical clothing to increase their chances of getting their research published, when in reality, they construct their research questions through problematization of existing literature. Tsoukas (personal communication), for example, pointed out that Chia and himself (Tsoukas and Chia, 2002) 'challenged assumptions of stability that have long underlain understandings of organizational change, yet they used gap-spotting language insofar as they say that they aim to build on, extend, and further radicalize current processual understandings of change'. Similarly, Starbuck (2003: 349) argued that 'authors can increase their acceptance of their innovations by portraying them as being incremental enhancements of wide-spread beliefs'. Sociologists such as Bourdieu (1996), Mulkay and Gilbert (1983), Latour and Woolgar (1979) have also noted that researchers in both the natural and social sciences are far more reflective and questioning when producing knowledge than is evident in their published texts.

However, it is an open question whether more active problematization has been used in some studies but disguised in the actual research text. If problematization is hidden or downplayed in research texts, the question emerges: why do researchers refrain from formulating their work in ways that, according to studies of influential theories, may increase interest and attention? This is puzzling because as Mizruchi and Fein (1999: 653) noted, whatever 'motivates the effort of the scientist, most people hope that their work will be, if not revolutionary, at least influential'. In other words, why would anyone bother to assert a knowledge claim that is less likely to generate interest and attention from the field? And as we have seen, gap-spotting is not just a characteristic of 'conventional' research but also of more 'progressive' versions inspired by poststructuralism. The question is particularly interesting as it could be argued that scholars seem to 'voluntarily' refrain from addressing existing studies in a way that, according to Davis and others, would increase the likelihood of producing an interesting and influential theory. Could it be that the ways in which tenure, promotion and funding decisions are made in most universities 'force' researchers into gap-spotting research?

The answer to the question of why there are so few examples of problematization is not likely to lay inside the investigated research texts. Although they indicate that gap-spotting is the favoured approach—or a possible rhetoric legitimating research papers—they do not really tell us why researchers use gap-spotting as the preferred way of constructing research questions. Instead, the answer is more likely to be found in the *scientific field* within which these texts are located and produced. Research texts are crafted in a socio-political context and the need to adapt to the rules of the field is strong (cf. Alvesson et al., 2008; Hassard and Keleman, 2002). Scientific fields consist of a range of social norms and methodological rules that guide and regulate the production and publication of scientific knowledge in significant ways (Bourdieu, 2004; Cole and Cole, 1967; Kuhn, 1970).

Important questions here concern: What kind of possibilities or constraints does a scientific field offer? What social norms regulate the production of scientific knowledge and publication of scientific texts within a particular scientific field? What are the expectations and constraints expressed by editors and reviewers as representatives of the research community exercising control over texts? What kinds of reward systems (promotion, funding, recognition, etc.) exist within the field and how do they regulate the conduct of producing research, especially the production of research reports?

Although many of these norms are part of the taken-for-granted landscape of the scientific field and therefore hard to detect (Gadamer, 1994/1960; Gouldner, 1970), we think that the viewpoints

below are important to consider for understanding the (over-) emphasis on gap-spotting and the shortage of assumption-challenging studies. More specifically, we think there are eight partly overlapping social norms and intellectual reasons that encourage researchers to use gap-spotting as the preferred way of constructing research questions and ultimately, their choice of rhetoric used in research texts.

1. *Gap-spotting is easy*. Earlier studies are relatively easy to review and to locate one's own study within. As Davis (1971), Locke and Golden-Biddle (1997) and Hargens (2000) have shown, there are numerous ways of constructing earlier studies to support one's own purpose and make one's own study seem highly logical and motivated. From an editor's or a reviewer's point of view, gap-spotting is also straightforward to assess. Is the relevant literature covered or not? Writing and presenting papers in a similar way also has the advantage of being easier to read. The parties involved know what to expect and don't have to cope with deviations. One could imagine other standards or greater variation for journal publishing than the dominant one, but as institutional theory teaches us (see for instance, Scott, 2001), we tend to follow specific, dominant forms. 'Voluntary' imitation and pressure from others means that contemporary organization studies (like most other fields) are presented in the form of gap-spotting to reduce creating deviation, confusion and sanctions.

2. *Gap-spotting is uncontroversial and safe*. Gap-spotting represents a relatively safe way of constructing research questions. Locating an empty space for an additional study may appeal to consensus and may be read as non-political and harmless. Questioning assumptions and conceptualizations of other studies, on the other hand, may raise sensitive issues, and upset colleagues, reviewers and editors. Many organizational theorists are therefore likely to adopt gap-spotting in order to 'play safe', and even be prepared to 'sacrifice relevance for a careerist credibility' (Mir and Mir, 2002: 107). More specifically, challenging assumptions often means questioning existing power relations in the field, and most scholars are probably unwilling to 'rock the boat' as it may trigger negative feelings and hamper their careers. Dominant actors within the scientific field, such as journal editors and reviewers are vigilantly guarding their positions from potential challenges (Bourdieu, 2004; Breslau, 1997). For instance, in his study of peer reviewing in several different journals within the social sciences as well as in the natural sciences, Starbuck (2003: 347) found that reviewers tend to 'criticize the methodology of studies that contradict the theories they prefer, and they applaud the methodology of studies that support the theories they prefer'.

3. *A powerful tradition indicates knowledge accumulation*. Traditionally, social science is supposed to advance through the generation of new knowledge, either by the accumulation of verified (or corroborated) hypothesizes, or the discovery of theory by sifting through mountains of data. As Litchfield and Thomson, the founders of *Administrative Science Quarterly*, lay out in their vision of the field of organization studies: 'scholars should build a cumulative, comprehensive, general body of theory about administration' (Palmer, 2006: 537). This research logic tends to lead researchers to adopt a systematic, analytical and often narrow focus that makes them unable to ask more fundamental and sceptical questions that might encourage some significant rethinking of the subject matter in question. It also gives an impression of a collective project, which is being drawn upon and developing. It signals reason, rationality, progress, and may work as an antidote to a lurking feeling that social research has strong elements of subjectivity, arbitrariness and relativism. Hence, gap-spotting may not only be used to legitimate oneself as a credible researcher but also the scientific project itself and, thus, preserve and reproduce knowledge accumulation as a fundamental scientific ideal.

4. *Academia is a crediting economy*. Closely related to the accumulation ideal, an important rule in the academic world is to build on and acknowledge the work of other scholars. In order to do so, we cite each other (and ourselves) frequently. While the way we cite each other varies across

scientific disciplines, researchers within the social and behavioural sciences consistently cite more papers (in particular foundational papers) in their respective areas than their colleagues within science (e.g. Hargens, 2000). As was evident in Locke and Golden-Biddle's (1997) study, as well as our own, researchers usually provide a lengthy discussion in the beginning of the article with the aim of positioning themselves (i.e. to demonstrate the importance of their own study) within the established literature. In particular, researchers often use what Hargens (2000: 858) called 'orienting reference lists' to demonstrate that their framework 'constitutes an acknowledged scholarly position' in order to persuade potential readers about the importance of their work.

However, academics not only cite other academics to acknowledge their ideas but also to increase their chances of getting published (Baird and Oppenheim, 1994) and, in particular, to receive high citations from other scholars. Receiving a number of citations generates rewards: symbolic rewards such as self-esteem and status, and material rewards like promotion, higher pay, more money for research assistants and travelling, or less teaching. In order for the academic crediting community to work, those involved must act based on the premises. As Macdonald and Kam (2007: 645) point out, a range of studies (e.g. MacRoberts and MacRoberts, 1989; Phelan, 1999; Yaalon, 1995) show that 'those who want their papers cited are well advised to produce long reviews, preferably of methodology'. Taken together, the norm of acknowledging other scholars' work through extensive citation is likely to encourage gap-spotting and, thus, reinforce, rather than challenge, already influential theories.

5. *Research institutions encourage gap-spotting*. Universities and researchers are increasingly governed by various funding and assessment formulas (Willmott, 1995). These are introduced by governments with the aim of allocating funding to universities and to individual researchers and/or used by universities to encourage and demonstrate results. For instance, in the UK, the Research Assessment Exercise (RAE), which was established in 1986 (heavily inspired by the American academic system), uses specific lists of 'quality journals' to measure the outputs of academics, and based on that, allocates funding to specific universities (Bessant et al., 2003). The RAE and similar measurement regimes have also been introduced in many other countries in both Europe and Asia (Leung, 2007). Universities use those lists of 'quality journals' for deciding whether they should offer someone an appointment, tenure, promotion or pay rise. The use of such lists is likely to encourage researchers to publish articles in particular journals rather than trying to develop more original knowledge through problematization of existing literature. As Macdonald and Kam (2007: 640) noted: 'A paper in one of the quality journals of Management Studies is much more important as a unit of measurement than as a contribution to knowledge. It measures academic performance and determines much academic funding'. Getting an article published in a high-quality journal is particularly important because what drives the citation of an article is mainly 'the prestige or average citation rate of the journal' (Judge et al., 2007) and not the actual knowledge claim. In addition, the strong encouragement of publishing in high quality journals is likely to further cement gap-spotting as the preferred way of constructing research questions. This is because those journals are often less inclined to publish more innovative research in comparison to lower status journals (Starbuck, 2005).

6. *Contemporary journal formats encourage gap-spotting*. Journals and their social norms of what characterizes credible research have a huge impact on scientific knowledge production. As Mangematin and Baden-Fuller (2008: 118) noted in their extensive review of more than 65,000 journal articles between 1992–2005, 'journals are not only significant foci for the dissemination of knowledge, but they also have a role in identifying the research agenda through editorials, invited contributions from leading thinkers and special issues'. The prevalent journal logic, for instance, stipulates that the researcher should position him or herself in relation to earlier studies and motivate what s/he is doing, often based on a superficial overview of existing literature. For example, in its criteria for publication, *Academy of Management Journal* stipulates that 'submissions should

clearly communicate the nature of their theoretical contribution in relation to the existing management and organizational literatures'. Similarly, *Journal of Management Studies* says that its main criterion for publication is that a submitted article should contribute 'significantly to the development of coherent bodies of knowledge'. Given a stipulated word limit and a pressure to cover all relevant work, it is difficult to devote much attention to questioning ideas and assumptions underlying existing literature of the subject matter. A notable exception is Kogut's (2006: 2) introduction of himself as the editor for the *European Management Review*. There, he explicitly states that 'we do not require that every article considers what the current theory is in order to frame … an investigation'. But as Palmer (2006: 546) observes: 'I suspect that [Kogut's] promise taps a deep and considerable well of resentment on the part of some organization studies scholars about the theoretical path dependence fostered by the field's major publications'.

7. *It often makes sense to adopt gap-spotting*. While most of our answers above refer somewhat pejoratively or ironically to social norms rather than 'rational arrangements', we do not want to overemphasize the non-rationality of gap-spotting practices. We think it often makes sense to review and base one's work on earlier studies. For instance, if there is a wealth of good studies, which one, after a critical scrutiny, finds reasonable, it may be a good idea to use them as a springboard for formulating one's own research questions. This is particularly the case for researchers disinclined to engage in heavy critical thinking, express (broad) scholarship or be engaged with the various problematizing schools that have emerged. It is also vital to acknowledge the more important contributions in the field and make sure one is not reinventing the wheel. However, the necessity of some degree of gap-spotting does not imply that it should be the dominant logic behind the construction of research questions.

8. *The alternative to gap-spotting—problematization—is difficult*. Many important things have already been said and many interesting problematizations have already been made. It is demanding to produce a good problematization and it may also be challenging to follow it through and deliver an original research question. Problematization requires that the researchers invest considerably more time and intellectual effort in constructing research questions (Alvesson and Sandberg, 2010). A particular challenge in problematization is not only to question assumptions which underlie other scholar's theories but also those which underlie one's own theoretical framework (see also Starbuck, 2006: 142–155).

Taken together, we argue that the above social norms and intellectual reasons in the scientific field exercise a strong normative control over the way research is conducted and reported in research texts. Through a long and extended socialization into the scientific field, most researchers internalize those norms and develop what can be called a *gap-spotting habitus* (to borrow a term from Bourdieu). By following this habitus, we reproduce it; and because of our doing so, gap-spotting achieves the status of the proper or 'right' way of constructing research questions.

Flagging for routes to research questions beyond gap-spotting

Without denying the relevance and advantages of gap-spotting, we believe it is important to loosen up the set of social norms that strongly encourage researchers to use and internalize gap-spotting as the preferred way of constructing research questions from existing theory. In particular, given that challenging the assumptions which underlie existing literature (including in the researcher's own tradition) is a recognized aspect of what makes a theory interesting and influential, it seems motivated to broaden our norms and methodological guidelines for knowledge production so they not only encourage gap-spotting, but also enable and actively promote the development of approaches that focus carefully and critically on assumptions, worldviews, perspectives, conventions, selective language and other elements in formulating research questions. Below, we suggest four ways of

constructing research questions that go beyond gap-spotting and, by doing so, also point at ways in which authors, reviewers, editors and research institutions adjust the social norms for knowledge production to encourage more assumption-challenging approaches.

Critical confrontation

Here, the researcher criticizes a theory or a field based on the identification of some shortcomings. The critical confrontation may take the form of application spotting, such as when a particular area lacks a critical perspective. It may also be framed in more confrontational and assumption-challenging ways worthy of its own category (e.g. as different from application spotting). Most critical management studies and poststructuralist deconstructionist studies exemplify this way of constructing research questions.

New idea

This simply means that the author emphasizes a new idea. The author claims innovation and does not follow any route marked by the map of a literature review of what has not already been studied. This means sidestepping both building on and challenging existing studies. It calls for original thinking and a high level of self-confidence. However, this approach is not common in contemporary management studies. One recent example is Gabriel (2005), who suggests the glass cage and the glass palace as new metaphors for organization studies. He relates these metaphors to Weber and many others, but not in a way that identifies some un-researched terrain within Weber-inspired studies, nor as a problematization of the iron cage theory or literature. Originality in this sense sidesteps the issue of problematization or other explicit routes to research questions. There is often an implicit reference to gap-spotting. But while gap-spotting typically identifies a narrow area that has not been (sufficiently) studied, the new idea approach claims a broader, more conceptual contribution.

Quasi-problematization

In this case, there is a problematization claim, but the researcher is smuggling in a prefabricated, ready-made alternative to what is presented as a 'genuine' problematization. Girard (1988) refers to this tendency—perhaps more common than 'real' problematization—as follows:

> It is well known that critics 'project' their own pet ideas on to the texts they claim to be criticizing. They exhibit their 'discoveries' triumphantly and rhapsodize over their relevance, without ever suspecting that they have retrieved unaltered the very thing they fed the machine in the first place to ensure its functioning. (Girard, 1988: 229)

This sounds harsh, and a more positive framing may make sense. It is also often difficult to distinguish quasi- from more 'genuine' problematization. A skilled critic can perhaps identify whether a critical turn of an input is an outcome of a creative act rather than an application of a 'truth' already available in the literature/tradition in which the 'problematizer' is located.

Problematization

As an alternative to the above ways of constructing research questions we suggest the possibility of problematization. Here, the logic behind the formulation of a research question is a critical

rethinking of a particular theoretical tradition, a vocabulary and the construction of an empirical terrain. This leads to the reformulation of some basic elements of 'received wisdom' and the carving out of new, perhaps surprising and 'logic-breaking' questions. Problematization can, and usually does, draw upon various intellectual resources, such as one or several of the problematization turns described above. But a key criterion for problematization—as we define the enterprise in this article—is to go beyond the application of a particular approach. This makes high demands and some originality.

These four versions deviate from the gap-spotting logic in the sense that they do not merely follow the scrutiny of the relationship between earlier research and sufficient knowledge about a particular theme as the broad route to take. We can distinguish between a *track-bound* and a *disruptive* mode of formulating research questions. Track bound research follows procedures, and uses other work and empirical observations as positive signposts and building blocks to stand on when formulating research questions. Disruptive research involves critique and problematization and aims at confronting or preventing a particular logic from being outlined. Disruptive modes are also inclined to specify problems with this research rather than issues that remain to be researched. Only what we refer to as problematization is mainly disruptive. Critical confrontation, quasi-problematization and to some extent a new idea typically draw upon a degree of track-boundedness. Critical confrontation follows a particular alternative position used for an attack of a competing one. Quasi-problematization is a special case, containing both track bound and disruptive elements. A new idea transcends the distinction. Table 2 below summarizes the prevalent modes of gap-spotting and possible alternative ways of constructing research questions identified and discussed in this study.

As shown in Table 2, a distinction can be made between track bound and disruptive modes of constructing research questions. Gap-spotting and problematization are of course not mutually exclusive. Any problematization calls for some scrutiny of particular debates, critiques and possible earlier challenges of assumptions in an area. One must make clear that others have not offered the new framework or idea emerging from a problematization before. Indeed, sometimes it is a matter of further developing problematizations introduced by others. There is also in conventional 'track-bound studies' often a minor degree of problematization—although less directed at assumptions rather than other insufficiencies—of existing literature before 'carving out' the empty gap in the knowledge terrain to be filled by one's own study.

Both gap-spotting and problematization can vary in scope and complexity. There are in most projects some ingredients—steps, considerations, combinations—of (minor) problematization and (some) gap-spotting. Blending approaches is possible and often motivated (which this article is an

Table 2. Summary of basic modes of formulating research questions and their specific versions

Basic modes of formulating research questions	Specific versions of basic modes of formulating research questions
Track-bound modes	Confusion spotting
	Neglect spotting
	Application spotting
Combined track-bound and disruptive modes	Critical confrontation
	New idea
	Quasi-problematization
Disruptive modes	Problematization

example of). Different parts or stages in a research process may include strong elements of gap-spotting or problematization and modes of relating to the literature in relationship to the generation of a research question. Nevertheless, the major elements in gap-spotting and problematization are, as previously discussed, very different. They refer to distinctly different research logics. Gap-spotting expresses faith in existing studies, and the assumptions on which they are based, and strives to build positively on them. Problematization is more sceptical. It asks what may be fundamentally 'wrong' with the assumptions underlying existing studies, even those underlying one's own favourite theories, and tries to challenge them as a key ingredient in constructing research questions. As we have seen, very little of the latter is apparent in the crucial and final step of formulating research in research texts. The extent to which problematization is used more in fieldwork or in other stages of the research process is an interesting topic, but goes beyond the scope of this article.

Conclusion

Our primary aim in this study has been to investigate the ways in which researchers construct research questions from existing literature, with an emphasis on those ways that are most likely to promote the development of interesting theories. The study makes four interrelated contributions to that area. First, based on an empirical study of research texts in organization studies, we found that gap-spotting is the dominant way of developing research questions from existing literature. It is by looking for 'gaps'—either lack of studies or shortage in the delivery of conclusive results in existing literature—that research questions are constructed. Within the overall category of gap-spotting, we identified three basic versions, namely, confusion, neglect and application-spotting.

Second, we addressed the issue of what ways of constructing research questions may lead to interesting and significant theories. Arguably, gap-spotting questions are not likely to lead to the development of interesting theories because of their failure to challenge the assumptions which underlie existing theories. We point to problematization as an obvious alternative, but conclude that it is rarely used. This is surprising given the growing recognition that challenging assumptions is what makes a theory interesting and significant. What is more surprising, though, is that even advocates of the various problematization turns (e.g. interpretive, political, linguistic, constructivist and postmodernism), which actively encourage researchers to challenge assumptions and rethink received wisdom, seem to apply gap-spotting as their preferred way of constructing research questions. There are of course exceptions, and many studies include minor elements of 'problematizations', although they do not typically challenge assumptions in any ambitious sense.

Third, our study suggests that while conventional research and their opponents (e.g. postmodernists, critical theorists and advocates of other problematization turns) differ considerably, they often appear to converge when it comes to how they construct research questions from existing literature. They share—or at least comply with and reproduce—a set of social norms and conventions about how to construct research questions that go beyond paradigmatic differences. We identified eight overlapping social norms and intellectual reasons that strongly lead researchers to use gap-spotting for constructing research questions from existing literature: (1) gap-spotting is not particularly demanding; (2) gap-spotting is an uncontroversial and safe way of constructing research questions; (3) the scientific ideal of knowledge accumulation; (4) the prevalent crediting norm in academia encourages gap-spotting; (5) research institutions strongly promote gap-spotting through their various measurement regimes; (6) the contemporary journal format further encourages gap-spotting; (7) gap-spotting often makes sense to use and (8) problematization as an alternative is difficult and considerably more intellectually demanding than gap-spotting.

In conclusion, given the centrality of scrutinizing and challenging assumptions for producing interesting and influential research, we think it is necessary to question the wide use of gap-spotting within organization studies. More specifically, in this article we developed and proposed four alternative ways of constructing research questions, namely, critical confrontation, new idea, quasi-problematization and problematization. We certainly don't claim that those ways are suitable for all research and should replace gap-spotting. However, given the dominance of gap-spotting—and a general feeling that the field is stronger in providing rigour than producing interesting new ideas—we urge authors, reviewers, editors and research institutions to be less inclined to employ and encourage gap-spotting in formulating research questions. Instead, more *disruptive* modes should be promoted and used, as they are likely to lead to a development of more interesting and significant theories.

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