



The construction of research questions in project management

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Abstract

This article examines how opportunities for contributions are created in project research. In the article the arguments that underlie research question constructions are analyzed and their role in theory construction is reflected upon. The analysis is based upon a review of 61 papers published between 2007 and 2011 in the four major project management outlets. The results show that questions identify gaps and extend literature rather than challenge the theoretical assumptions. It is argued that the dominance of “gap spotting” hampers the development of the project field by producing theories that do not challenge long-held, sometimes possibly false, assumptions. Researchers are therefore urged to become bolder in their claims, some suggestions on how to achieve this are offered.

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1. Introduction

With the proliferation of papers dealing with projects in the top-tier management journals, the recent birth of new dedicated project management journals, the inclusion of the established project journals in the Social Science Citation Index and an increased industry diffusion creating a tremendous impact in working practices, it is about time to examine how opportunities for contributions are created in project research. Research questions are fundamental in that they set the scope, aim or contribution to academia or to practice. Well-grounded and carefully formulated research questions may extend old ideas and develop new ideas. Simply, the kinds of research questions that are asked determine what theories are eventually produced.

Despite the importance of research questions in scholarly work there is little guidance regarding their construction. Textbooks on research methodology do “not provide more specific directions on ways to formulate innovative research questions by scrutinizing existing literature in a particular research area” (Sandberg and Alvesson, 2011:24), beyond that it should be

clearly defined in terms of topic, domain and object of study, etc. (e.g. Silverman, 2001). Other efforts come closer. For example, Davis (1971) focused on what made qualitative theories interesting¹ and famous. Interesting papers, Davis argues, are the ones that refute *some*, but not *all*, of the particular audience’s assumptions. Similar efforts have targeted how contributions are framed as contributing to specific areas. Here Locke and Golden-Biddle (1997) dealt with how researchers create opportunities to contribute to the literature, identifying associated rhetorical practices in top American journals. Criticizing previous efforts for not considering how research questions are constructed, Sandberg and Alvesson (2011) studied research question construction in the top four American and top European management journals. They found that none of the 52 investigated papers attempted to invoke new theories. This debate has so far concentrated on organization, management theory and A-level journals or the stakeholders of an interesting theory. Consequently, the debate has made important contributions to the understanding of how opportunities are created to contribute to the literature, or how research questions are

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¹ Whenever a reference to “interesting” is made in this paper, outside of a quote and/or an author’s specific meaning, it is not dismissive of other research. It simply denominates research that because of its originality, is to some extent likely to leave a distinctive mark in Davis’s (1971) sense.

constructed in established areas where top-tier journals make demands for theoretical contributions and scientific rigor that are undoubtedly higher than in journals in less established areas such as project management.

As a young subfield of management, project management is relatively immature compared to general organization theory. Project management journals are neither recognized as A-level journals outside of the field (c.f. www.harzing.com), nor do they demand a similar focus on theoretical developments from their authors. Papers in such subfields thus face different challenges in terms of theoretical contributions, including the development of a coherent field, associated to the pre-paradigmatic state of project research (Bredillet, 2010). Meanwhile, most publications in academia generally are not within the top-tier journals. How contributions to project journals are framed not only contributes to the field as such but also provides important linkages to the extensive scholarly interest in projects as a new organizational form outside of the immediate project literature realm (Söderlund, 2010:2). This leaves a void in knowledge about how opportunities for contributions are framed in journals below the A-level. The present paper, drawing upon the typology developed by Sandberg and Alvesson (2011) and Locke and Golden-Biddle (1997), extends the contemporary debate by investigating how researchers in project management construct research questions, as they are expressed in the four major project management journals.

The purpose of this paper is to analyze the arguments that underlie the research questions and reflect upon their role in theory construction. Through the review of 61 papers published between 2007 and 2011 in the four major project management outlets, the paper makes four contributions. First, in contrast to the contemporary debate, the paper investigates a subfield of management studies, which gives it a specific thematic focus that may assist in bridging contributions to other management or organization theory areas. Secondly, it examines a less mature area of research with correspondingly few developed theoretical foundations. Thirdly, it provides the basis for an argument that focuses on the construction of research questions, in order to develop insights about project management. Finally, the paper highlights the possibility of different approaches to constructing research questions in order to produce theories.

1.1. Developing theories for and of project research

A theory constitutes “an ordered set of assertions about a generic behavior or structure assumed to hold throughout a significantly broad range of specific instances” (Sutherland, 1975:9, cited in Weick, 1989:517). There have been many attempts to find and develop such theories of project management aiming at creating theories or a unifying theory for project research on which to build and gain further acceptance (Andersen, 2006; Arto and Wikstrom, 2005; Jugdev, 2004; Leybourne, 2007; Lundin and Söderholm, 1995; Peippo-Lavikka et al., 2011; Shenhar and Dvir, 1996; Turner, 2006a, 2006b, 2006c, 2006d). The general idea is that a theory of projects is

beneficial to the development and acceptance of the field for a general audience.

The state of project theory has however been the subject of continual debate for several years. Essentially, research that ranges from instrumental research on models to studies of processes has been found overly rational and instrumental (Cicmil and Hodgson, 2006; Packendorff, 1995) and there is therefore a claimed need to “reclaim” (Blomquist et al., 2010; Hällgren and Söderholm, 2011) and “re-think project management” and “examine how current theories, concepts and methodologies underpinning project management research could be enriched and extended to enhance the relevance of the knowledge created in the research process” (Winter et al., 2006:646).

Regardless of one’s point of view about the need for one or several theories of projects, a unified field of research does not yet exist. Project research is therefore in a pre-paradigmatic state (Bredillet, 2010). Attempts to provide overviews to continue the construction of project management as a field have described it as having different schools. For example, based on publications in the major project management outlets, Bredillet (2007a, 2007b, 2007c, 2008a, 2008b, 2008c) describes nine schools with different theoretical emphases. Söderlund (2010) on the other hand, focuses on the project literature published in higher-level journals outside of the immediate project realm. The schools, Söderlund argues, demonstrate a rather high diversity among theoretical approaches and some of the assumptions, when compared, may further understanding of project research.

1.2. The face of Janus in theory development

Janus is the two-faced Roman god who looks simultaneously into the future and the past. In the discourse surrounding management research (Alvesson and Sandberg, 2011; Johnson, 2003; Tadajewski and Hewer, 2011) and in the practice of journal paper acceptance (Bedeian, 2003; 2004), future-looking innovative research is especially valued, for the simple reason that innovative ideas, whose conclusions have maintained relevance and validity, (Bartunek et al., 2006:10), have the power to challenge long-held seemingly unproblematic assumptions. (cf. Locke and Golden-Biddle, 1997:1025) Davis asserted that those theorists “who carefully and exhaustively verify trivial theories are soon forgotten; whereas those who cursorily and expediently verify interesting theories are long remembered” (Davis, 1971:309). An interesting theory, then, is one that denies “certain assumptions of their audience” (Davis, 1971:309). In order to attract the attention of the audience, the theory must be innovative in relation to the theoretical structure that makes up the everyday theoretical life that is present in other writings and their propositions. That said, an interesting theory must also have a practical usefulness, which implies that the findings must challenge and improve common practice (Davis, 1971:311). While Davis targeted an academic audience and scholarly arguments, Bartunek et al. (2006) extended the investigation into empirically based papers in an investigation of what the members of the Academy of Management Journal’s editorial board found interesting. The

findings largely mirrored those of Davis in that they would have to be counter-intuitive, good quality, well written, include a new theory/finding, have practical implications and make an impact. This paradigm shifting type of theory development tends to be part of what Kuhn (1962/1996) calls revolutionary science. Revolutionary science refers to an epistemological paradigm shift in the scientific community. That is, following a paradigm shift the worldview is changed and there is no return to the former one. Challenging these assumptions, being an inherent part of developing so called interesting theories (Davis, 1971), are bound to meet resistance since there are conventions about the current knowledge paradigm. This is demonstrated in both how research questions are framed (Locke and Golden-Biddle, 1997) and in the journal review process (Bedeian, 2004).

The other side to the widely held assumption that future-looking innovative theory development is positive, is that there are negative effects associated with forgetting the past. On the negative side, only trying to overturn existing theory contributes to a mechanized and industrialized type of scholarship that is becoming increasingly inaccessible to practitioners (Tourish, 2011). Furthermore, striving for novelty arguably contributes to fewer comprehensive cross-perspective analyses and thus fewer holistic studies. Moreover, assuming that innovative theory development is positive contributes to fewer replication studies, less common concepts and the fragmentation of the research area, which thus loses explanatory power. The important consequence of less integration and less replication is that theories are hampered in their development (Mone and McKinley, 1993:292–293). The possibility of replication in social sciences is debated, but Tsang and Kwan argue that a replication of a theory may significantly raise its credibility and hence contribute to development of the field (Tsang and Kwan, 1999:776). Similarly Glynn and Raffaelli (2010) argue, with evidence from the leadership literature, that there is a great danger from both sides of the faces of Janus. Therefore they suggest that any attempt to develop a field has to rely on both a diversification and a novel approach to theory development. This is echoed by Colquitt and George (2011) in their editorial in the *Academy of Management Journal*, although still with a clear preference for recombination of fields to produce novel results. In the words of Kuhn this tends to be inclined to normal science. Normal science refers to “research firmly based upon one or more past scientific achievements, achievements that some particular scientific community acknowledges for a time as supplying the foundation for its further practice” (Kuhn, 1962/1996:10). Thus, normal science tends to relate to the epistemological relation of past science, and revolutionary science to the epistemological relation of mind shifting future-oriented science.

1.3. The construction of research questions

Investigating scientific texts to understand the underlying process is not new (Davis, 1971; Johnson, 2003; Mathiassen et al., 2011). These studies are focused on the “interesting” aspect of paper construction rather than paying attention to the

construction of the research question. With more interest in the construction of research, Locke and Golden-Biddle (1997) reviewed 82 qualitative papers in *Administrative Science Quarterly* and *Academy of Management Journal* to assess how they created opportunities for theoretical contributions. They found that there were two main arguments used—structuring inter-textual coherence and problematization. The structured inter-textual coherence refers to a writing practice that in large part reflects the coherence or incoherence of the previous work. Problematization, by contrast, refers to deficiencies in the present theorizing—deficiencies that have to be remedied.

Building and extending upon Locke and Golden-Biddle (1997) by focusing on the research question construction per se and its ability to create opportunities for theory development, Sandberg and Alvesson reviewed 52 papers in *Administrative Science Quarterly*, *Journal of Management Studies*, *Organization studies*, and *Organization*. They identified three main modes: confusion spotting, neglect spotting and application spotting.

The first mode, *confusion spotting*, “spot[s] some kind of confusion in existing literature” (Sandberg and Alvesson, 2011:29), where the evidence from the literature is not clear and is typically contradictory. The research question, therefore, is constructed from the literature by looking for competing explanations in relation to prior research. The second mode, *neglect spotting*, argues that there is an academically uncharted area requiring attention and analysis. A majority of the 52 papers in Sandberg and Alvesson’s sample were characterized by neglect spotting. Neglect spotting comes in three versions: an overlooked area; an under-researched area; or a lack of empirical support. The most common argument was based on the overlooking of a certain area, essentially that the area is developed but lacks a specific focus. An under-researched area is one in which there is a strong bias towards a certain perspective, leaving other areas under-researched. The third version of neglect spotting refers to work that argues for the existence of theoretical concepts and models but in which there is an empirical lack of support that warrants further investigation. The third and final way of spotting a gap is *application spotting*, which argues that an area of research lacks a particular theory or perspective and “that a specific body of literature needs to be extended or complemented in some way or another” (Sandberg and Alvesson, 2011:31). It is common to combine these approaches because of how research questions are developed but usually one of them is dominant.

While not frequently used, problematization refers to a way of constructing the research question that “aims to question the assumptions underlying existing theory in some significant ways” (Sandberg and Alvesson, 2011:32). Although gap spotting challenges existing knowledge, it should therefore not be mistaken for problematization, which denies a significant part of the present knowledge. Consequently, problematization of a genre does not require a paradigm shift that overturns the understanding of something in a Kuhnian sense (1962/1996). Problematization-based research focuses on the problems with a particular area of research rather than issues that remain to be researched, and examines what is potentially problematic with the assumptions with some research rather than building

positively on its contributions. Four ways of going beyond gap spotting have been identified (Alvesson and Sandberg, 2011): critical confrontation; new idea; quasi-problematization, and problematization. All but the last of these refer to more problematizing modes. *Critical confrontation* challenges the assumptions underlying a certain area, which is the case in most critical research studies. *New idea* refers to the construction of a research question that is original, despite being based on the shortcomings of existent theory. *Quasi-problematization* infuses pre-developed alternatives in what is referred to as problematization (but hence really is not). Finally, problematization constructs the research question through carefully developed logic-breaking arguments that go beyond the application of a particular theoretical, empirical or methodological approach. In this paper no explicit paradigmatic stance is taken except that paradigm shifts tend to emerge rarely.

To summarize, the frameworks discussed above provide an analytical pattern that are drawn upon in order to understand how opportunities for theoretical contributions are constructed through the framing of research questions.

2. Method

The purpose of the paper is to analyze the construction of the research questions in detail. With a similar interest Locke and Golden-Biddle (1997) focused on how opportunities for theoretical contributions in qualitative studies were framed in 82 papers from the leading two North-American management journals. Sandberg and Alvesson (2011) on the other hand examined a sample of 52 papers from two issues in the top four management journals in Europe and North-America. In contrast, this paper focuses particularly on the project area in a way similar to Mathiassen et al.'s (2011) study of style composition in information systems research, or Johnson's (2003) study of designating the audience in marketing research. It makes a distinctive complement to previous efforts by investigating a less mature sub-field with explicit focus on the construction of the research question regardless of theoretical, methodological or practical use and contribution.

In a review of management journals related to project management, Kwak and Anbari (2009) defined 18 journals related to project management and allied areas. Following previous efforts (Locke and Golden-Biddle, 1997; Sandberg and Alvesson, 2011) this paper focuses only on journal publications. The purpose is not to identify "interesting" research *per se* through for example snowball-sampling, but merely to analyze how research questions are constructed in journals. The present paper does not focus on the "allied areas" in relation to project management, but on project management in particular. Nor does the paper focus on providing an overview of how project management is perceived in related areas, or how the research questions in related areas are constructed; this would have been the case if a random issue was chosen that most likely included topics other than project management (in, for example, the journal *Research Policy*). Therefore, journals in allied areas are not included. Journals in allied areas include *IEEE Transactions on Engineering Management, Organization*

Science, and Academy of Management Journal (see Kwak and Anbari, 2009 for the full list of journals). However, three of these 18 journals are "specific journals dedicated to PM research" (Kwak and Anbari, 2009:437) and of interest to this paper: *International Journal of Project Management (IJPM)*, *Project Management Journal (PMJ)*, and *International Journal of Managing Projects in Business (IJPMB)*. Since the publication of Kwak and Anbari's list, the *International Journal of Project Organization and Management (IJPOM)* has been added and is therefore included. The first two of these are established journals (Europe- and US-based, respectively), and the other two are newcomers (Australia- and US-based, respectively). This contrasts with Sandberg and Alvesson (2011), who selected the two leading North American and European management journals, and Locke and Golden-Biddle (1997) who selected the leading two North American management journals. In contrast to this paper the journals in previous research are established.

For this paper, the time range was 2007–2011 in order to target recent developments within the field. Following Sandberg and Alvesson (2011), two issues from each journal were chosen, special issues and special types of papers were disregarded. The total number of papers in the sample was 80. Discarded specialized papers included student papers/thesis notes (4), conceptual papers (1), research notes (1) and book reviews (13). The student papers were discarded since they reported on dissertations and the dissertation process; the conceptual paper by Lundin (2011) was left out since it provides ethical guidelines for research dissemination rather than research; Research notes "provide readers with access to less developed papers than conceptual 'regular' papers" (Emerald, 2011). Fox's (2011) research note is thus not yet a full research paper and was hence left out of the sample. Of the total 80, 61 articles remained in the sample for further analysis. (IJPM=21, PMJ=12, IJPMB=15, IJPOM=13; 33 established journal papers and 28 newcomer journal papers).

Following earlier developed praxis (Locke and Golden-Biddle, 1997; Sandberg and Alvesson, 2011), the entire paper was read, but the focus was on the first part, up to the description of the method. The reason is that the first sections are where the authors most clearly express the way in which they construct the research question. Within the same praxis key statements were identified within the text that signaled how the research question was constructed. The argumentative practices were identified by following Sandberg and Alvesson (2011) (to a great extent themselves relying on Locke and Golden-Biddle (1997)) and the analytical typology of problematization and gap spotting for deductive analysis, paralleled by inductive thinking in the cases where the framework did not easily fit. The practices comprised: critical confrontation, new idea, quasi-problematization, and problematization, respectively; confusion spotting, neglect spotting and application spotting. In practice, this meant that the articles were read argument-by-argument in order to identify key arguments. After identifying such key transitions the logic behind the arguments was scrutinized. In most cases the key statements and logic were obvious. For example, Lizarralde et al. (2011)

argued that temporary multi-organizations and their procurement strategies have been largely overlooked. Consequently, the paper was categorized as neglect spotting, an example of overlooking-based argument. Blomquist and Wilson (2009) on the other hand, argued that “we attempt to extend established business unit concepts to multi-project organizations.” The paper was thus categorized as application spotting and extending or complementing existing literature. Some papers claimed to have produced a “new idea” (Saynisch, 2010) but they did so primarily from the position that the idea followed from an empirical need without scrutinizing the assumptions on which it relied. Consequently it was categorized as an “empirical need or example”. Where previously developed categories did not fit, (as in Saynisch) the arguments were scrutinized and the practice labeled, following the same basic procedure of identifying the key arguments and underlying logics previously mentioned. (See Table 1 for more examples). The research question in the papers was constructed with the following distribution (the dimensions are detailed in the findings section):

3. Findings

The review reveals that all articles adhered to a gap-spotting pattern (including the basic gap-spotting modes neglect spotting, empirical need or example spotting, application spotting, confusion spotting and research overview spotting) where most articles followed similar construction patterns to those in Sandberg and Alvesson’s (2011) review, with small differences in distribution and with the addition of the empirical need or example, and research overview mode categories. Similar to that research, no other forms of more assumption-challenging modes of constructing research questions were identified (including critical confrontation, new idea, quasi-problematization and problematization). The paper distribution per mode is found in Fig. 1.

The neglect spotting mode is almost twice as common (28) as the mode that identifies empirical need or example (15). Even so, the frequency of the latter makes it clearly distinguishable from previously identified dimensions. Breaking down the modes into specific ways of constructing the research questions shows that the three most common are clearly more common than the four least common, see Fig. 2.

There were small differences between established and new journals in terms of how the modes were distributed. The significant differences were that papers in new journals were more inclined to use an “under researched” argument (4 articles compared to 2), while the latter were more inclined to claim “lack of empirical support” (1 article compared to 4) and “empirical need or example” (5 articles compared to 10). This finding could be explained by the fact that the established journals (IJPM and PMJ) are the formal outlets of International Project Management Association and Project Management Institute respectively, and therefore have to explicitly communicate to both audiences at once and thus they are more inclined to accept practitioner-oriented papers.

Interestingly, 19 out of 61 papers were missing a distinct research question, eleven a distinct purpose, aim or objective; and

six papers were missing a distinct research question *and* purpose/aim/objective.² The papers were distributed evenly between the type of journal while most papers without a research question and purpose were found in the established journals (4 articles to 2). Most papers that lacked a distinct research question or purpose were in the “empirical need or example” category (10 articles to 19 in total, and 10 out of 15 within the category). This finding could be explained by that the focus of the papers is practical contribution rather than theoretical development.

4. Discussion

The purpose of this investigation is to analyze the construction of research questions, as expressed in the research texts, and reflect upon their role in theory construction. Sixty one articles were reviewed in the four leading project management journals: *International Journal of Project Management*, *Project Management Journal*, *International Journal of Managing Projects in Business*, and *International Journal of Project Organization and Management*. Sandberg and Alvesson (2011) suggested a typology for gap spotting and problematization research question construction. Gap spotting includes research that seeks to identify gaps in the existing literature, thereby extending and contributing to a topic. This method can find arguments such as two strands of literature that have not previously been integrated; for example, project management and new product development (Pons, 2008), or even obvious headings such as “Gap in knowledge” (Ling and Tiong, 2008). Problematization, on the other hand, is research that “aims to question the assumptions underlying existing theory in some significant ways” (Sandberg and Alvesson, 2011:32). Problematizing research includes research that suggests alternative interpretations or ways forward, divided into critical confrontation, new ideas, quasi-problematization, and problematization. Following this analytical pattern, five categories of spotting modes were identified, including two additional ones.

4.1. Identified gap-spotting modes of constructing research questions

4.1.1. Neglect spotting

Neglect spotting refers to the construction of research questions that identify a gap in the literature that needs to be filled. This was the most common mode of identifying gaps in the literature, used by 28 out of 61 papers. Neglect spotting comes in three dimensions: overlooked areas, under-researched areas, and lack of empirical support.

Articles claiming there is an *overlooked area* argue that the literature lacks a certain focus and reveal gaps that need to be filled. Other papers in this category claim that few studies have combined two or more streams of literature, leaving

² In a paper claiming to investigate how the research question is constructed, this is somewhat problematic. Since the research question and the purpose is only the final outcome of an extended argument, the nature of the argument was still clear.

Table 1
Modes of research question constructions.

Basic gap-spotting modes	Specific construction	Reviewed journal articles
Neglect spotting	Overlooked area	Papadimitriou and Pellegrin (2007) (IJPM, 25, 437–445) Clarke (2010) (IJPM, 28, 461–468) Ochieng and Price (2010) (IJPM, 28, 449–460) Kratzler et al. (2010) (IJPM, 28, 428–436) Reed and Knight (2010) (IJPM, 28, 422–437) Fuller et al. (2011) (IJMPB, 4, 188–136) Jacobsson (2011) (IJMPB, 4, 64–81) Smith et al. (2011) (IJMPB, 4, 10–27) Andersen et al. (2009) (IJMPB, 2, 479–498) Rezania and Lingham (2009) (IJMPB, 2, 577–590) Pons (2008) (PMJ, 39, 82–97) Henderson and Stackman (2010) (PMJ, 41, 37–55) Henderson (2008) (PMJ, 39, 48–59) Darrell et al. (2010) (PMJ, 41, 56–63) Lizarralde et al. (2011) (IJPOM, 3, 57–77) Krane et al. (2011) (IJPOM, 3, 36–56) Koskinen (2011) (IJPOM, 3, 91–106)
	Under-researched area	Styhre and Borjesson (2011) (IJPOM, 3, 22–35) Karlsen et al. (2008) (IJPOM, 1, 105–118) Veenswijk and Berendse (2008) (IJPOM, 1, 65–85) Leybourne (2009) (IJMPB, 2, 519–535) Dikmen et al. (2007) (IJPM, 25, 494–505) Johansson et al. (2007) (IJPM, 25, 457–464)
	Lack of empirical support	Simsarian Webber (2008) (PMJ, 39, 72–81) Schmid and Adams (2008) (PMJ, 39, 60–71) De Bakker et al. (2010) (IJPM, 28, 493–503) Wong et al. (2010) (IJPM, 28, 469–481) Randeree and Ninan (2011) (IJMPB, 4, 28–48)
Empirical need or example spotting	Practical application	Gallo and Gardiner (2007) (IJPM, 25, 446–456) Wibowo and Mohamed (2010) (IJPM, 28, 504–513) Ke et al. (2010) (IJPM, 28, 482–492) Chen and Chen (2007) (IJPM, 25, 475–484) Lam et al. (2007) (IJPM, 25, 485–493) Chen et al. (2010) (IJPM, 28, 514–527) Dzeng and Lee (2007) (IJPM, 25, 505–516) Davidson and Rowe (2009) (IJMPB, 2, 561–576) Fox (2011) (IJMPB, 4, 137–149) Klakegg and Haavaldsen (2011) (IJMPB, 4, 157–167) Fox (2009) (IJMPB, 2, 536–560) Sanchez and Robert (2010) (PMJ, 41, 64–73) Lalonde et al. (2010) (PMJ, 41, 21–36) Saynisch (2010) (PMJ, 41, 4–20) Mohapatra and Patnaik (2011) (IJPOM, 3, 78–90)
Application spotting	Extending and complementing existing literature	Ling and Tiong (2008) (IJPOM, 1, 86–104) Olsson (2008) (IJPOM, 1, 47–64) Tan et al. (2008) (IJPOM, 1, 4–23) Burns and Cao (2011) (IJPOM, 3, 1–21) Koskinen (2008) (IJPOM, 1, 119–132) Mazouz et al. (2008) (PMJ, 39, 98–110) Huff and Prybutok (2008) (PMJ, 39, 34–47) Ali et al. (2008) (PMJ, 39, 5–33) El-Sawalhi et al. (2007) (IJPM, 25, 465–474) Müller and Turner (2010) (IJPM, 28, 437–448) Sambasivan and Soon (2007) (IJPM, 25, 517–526) Klakegg (2009) (IJMPB, 2, 499–518) Blomquist and Wilson (2009) (IJMPB, 2, 591–598)
Confusion spotting	Competing explanations	Zwikael and Unger-Aviram (2010) (IJPM, 28, 413–421) Jha and Iyer (2007) (IJPM, 24, 327–540) Kirytopoulos et al. (2008) (IJPOM, 1, 24–46)
Research overview spotting	Trending	Sense et al. (2011) (IJMPB, 4, 105–117) Biedenbach and Müller (2011) (IJMPB, 4, 82–104)

significant knowledge gaps. Seventeen papers had these features. An example of this category is the study by Henderson, which argued that communication competence has received far less attention than communication processes. This led her to build a research question based on the argument that “little integration has occurred in these studies with extant communication theory, which has resulted in scant knowledge about the underlying behavioral dynamics of competent communication, especially as it relates to important project and organizational outcomes” (Henderson, 2008:48). Henderson also argued that a model of project manager’s competence in communication should be validated and extended.

Under-researched areas were less common, with six papers in the category. Articles that follow this line of reasoning argue that there is a bias towards certain phenomena. An example of this category is Karlsen et al., who claim that trust is an under-researched area deserving further attention because “trust can be important for knowledge transfer and learning” (Huemer, 2004). Trust can improve the ability of the involved parties to work more collaboratively. Trust also mitigates the perceptions of opportunistic behavior between the stakeholders and the project. Misztal (1996) argued that trust is “essential for stable relationships, vital for the maintenance of cooperation, fundamental for any exchange and necessary for even the most routine of everyday interactions” (Karlsen et al., 2008:106).

A *lack of empirical support* for a certain interpretation of the findings is not particularly common. Only five papers followed this approach. The argument of this category is that the results are inconclusive and that more research is warranted in order to establish how to interpret the results. Simsarian Webber provides an example of this approach: “blending service and client employees into a team is widely used by information technology service companies as well as other service providers”. In practice, however, there is a “lack [of] empirical evidence of its proposed benefits for effectively building and sustaining client engagements” (Simsarian Webber, 2008:72–73).

4.1.2. Empirical need or example

Spotting an empirical need in practice or providing an example for practice is one of the two additional categories of basic gap spotting behavior compared to Sandberg and Alvesson

(2011). Most studies within this category are practitioner-oriented and attempt to base the argument on an identified empirical need rather than a theoretical contribution. This was one of the larger categories, with fifteen of the 61 papers (second only to *overlooked mode*, which was the most common both in the present study and in Sandberg and Alvesson’s study). *Practical application* focuses on a certain aspect of practice; for example, Davidson and Rowe systematized knowledge management in projects. They did not focus on a particular model, instead trying to provide an overview of existing literature for practical use. The paper “proposes that a systems theory approach with formalized ‘stage-gate’ reviews of project progress against key performance indicators of multiple objectives, quality, cost, schedule and health, safety and environmental sustainability” can be used to “capture knowledge, development of individuals and teams, and contribute to a basis for the permanent organization to use in future projects.” (Davidson and Rowe, 2009:562) Another example is Fox (2009:537), who utilized an action approach to “facilitate a change from ineffective generic methods for the communication of information to more effective designed information and communication.” This category of research question construction research is overrepresented in terms of papers that lack a distinct research question and/or purpose. Out of the 21 papers that lacked any of these two features, ten are found in this category. Instead of a distinct research question, the argument is built on a practical need, which seems to warrant an exception from the traditional structure of a paper. Following this and the arguments in the papers, however, the main objective is not to contribute to the theoretical development of the field but rather to practice. A small majority (11 to 9) of these papers occurred in the established project journals—*Project Management Journal* and the *International Journal of Project Management*.

4.1.3. Application spotting

Application spotting tries to extend or complement existing literature by looking for “a shortage of a particular theory or perspective in a specific area of research” (Sandberg and Alvesson, 2011:30–31). Thirteen out of 61 papers followed

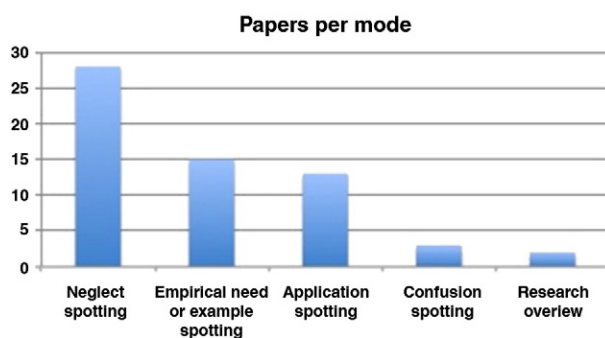


Fig. 1. Papers per mode.

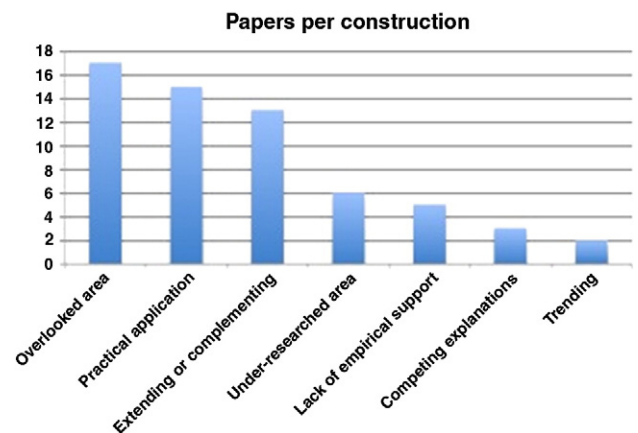


Fig. 2. Papers per construction.

this line of reasoning, an example of which is Ling and Tiong, who said “these studies are at the macro and strategic management levels and did not consider the challenges and problems that are faced at the project level” (Ling and Tiong, 2008:87), and therefore they “investigate the challenges that were faced” (Ling and Tiong, 2008:87).

4.1.4. *Confusion spotting*

Like Sandberg and Alvesson, the main way of constructing a research question in this category is by finding competing explanations in the literature. The aim of the research is designed to resolve the confusion by attending to the perceived gap. Two of the 61 articles are found in this category. Zwikael and Unger-Aviram (2010) serve as examples. They identified a gap in HRM research related to team development and success in combination with unique project environments and they tried to “explain these contradictory results” (Zwikael and Unger-Aviram, 2010:413) with empirical research.

4.1.5. *Research overview*

The second additional gap spotting mode is the research overview, which is essentially a review of literature that provides guidance to understand past and/or future research directions. Although such papers can be expected to be more common in less established areas of research (such as project management in comparison to the general management area), it is expected that similar papers would be quite rare over the course of a year. Two examples of research overviews, which deal mainly with trending research, are Biedenbach and Müller (2011), who investigated IRNOP contributions to understanding past and future research; and Sense et al. (2011), who examined project management research in Australia. Biedenbach and Müller (2011:83) argued that “underlying paradigms and philosophical foundations change slowly and possibly [go] unnoticed” in less mature fields of research and a investigation is “needed and necessary for a better understanding of the past, present and possible future of research paradigms in project management” (p.84). Similarly, Sense et al. (2011:106) sought to investigate “current issues or opportunities faced by PM researchers”, thereby contributing to the international discourse on the development of project management. Trending research does not problematize the content of the research, per se; instead, it focuses on looking back and ahead.

4.2. *Dominated by gap-spotting*

A theory is “an ordered set of assertions about a generic behavior or structure assumed to hold throughout a significantly broad range of specific instances” (Sutherland, 1975:9, cited in Weick, 1989:517). To provide “interesting” theories it is not enough for a paper to be of high quality, well written and with high practical relevance and impact (Bartunek et al., 2006). There is a more subtle difference between theories that are remembered and those that are one of many. The ones that are remembered, and are likely to challenge how research is done and how contemporary organizations—or projects—are understood, are those that defy some of the assumptions

of their audience (Davis, 1971:309). What makes a theory “interesting” is however not related to the epistemological stance of Kuhn (1962/1996) where assumptions are completely turned upside down. An “interesting” theory may be part of the normal, as well as the revolutionary science. An “interesting” theory consequently does not require everyone to abandon his or her line of research in a Kuhnian sense. Instead an “interesting” theory may be part of a slow or partial revolution. By being part of past achievements an “interesting” theory may still adhere to the same epistemological understanding of the phenomena, while it could also refute the understanding in its completeness and thus be part of a scientific revolution. None of the 61 papers in this study attempted to argue for the problematization. A stance that “aims to question the assumptions underlying existing theory in some significant ways” (Sandberg and Alvesson, 2011:32). Instead, the papers were located in more problematizing modes of research question construction. This is hardly surprising since the same pattern applies to general organization theory (Locke and Golden-Biddle, 1997; Sandberg and Alvesson, 2011).

The lack of more problematizing modes is not restricted to a particular methodology or approach. For example, Styhre and Borjesson (2011) identified their study as filling a gap in the under-researched area of creative projects. They argued that “the domain of organization theory and management studies that addresses project management work is in most cases studying industrial activities such as new product development, construction sites, or events such as festivals or sport competitions whereas the project management practice in the culture industry or the culture sector is comparatively little attended to” (Styhre and Borjesson, 2011:23). From there, they critiqued the notion that project management is a matter of rebureaucratization, framing their argument with a qualitative interview-based approach. In a conceptual paper, Leybourne (2009) made a similar claim about an area being under-researched. He argued that project management methodologies and improvisation is a growing area of interest that challenges the traditional project management literature by embracing uncertainty and change with time-frames that are not as fixed. “... [T]here is an argument that more traditional project-based frameworks are too cumbersome to resolve some organizational situations within acceptable timeframes” (Leybourne, 2009:520). Although some project managers in practice have done so, “the main practitioner bodies (the US Project Management Institute, the UK Association for Project Management, the International Project Management Association, the Australian Institute of Project Management, etc.) have not yet fully recognized improvisational working within their adopted or documented Bodies of Knowledge (BoKs)” (Leybourne, 2009:520). Like Styhre and Borjesson, Lizarralde et al. adopted a qualitative approach based on a case study with interviews and documents. Instead of arguing that their study filled an under-researched gap, they argued that the area of temporary multi-organizations (TMO) and their procurement strategies have been largely overlooked. “TMOs are constituted by procurement strategies on the part of the project client which emphasize the *inter-organization* relations but which put little

emphasis on the impact of internal, i.e., intra-organizational, structures, including those of the client. These procurement strategies mostly concern the contractual arrangements between the client and contractors and/or professionals. However, they do not specifically allow for anticipating the impact the participants' internal structures and relationships might have on these contractual arrangements" (Lizarralde et al., 2011:57, italics in original). Like Leybourne, Koskinen presented his ideas in a conceptual paper, arguing that there is an overlooked gap in the literature in which the goal of the "theoretical paper is to describe a project-based company's learning processes with the help of the systemic perspective" (Koskinen, 2011:92).

An explanation of the prevalence of gap spotting and the lack of problematization is, on one hand, the long and strong instrumentalist heritage of project management, as evidenced by the existence of a clearly empirically informed mode of constructing research questions (15 papers in the empirical need or example category). This suggests that, compared to general management, project management remains a practically oriented field of research, rather than a theoretically oriented one. The research overview mode (Biedenbach and Müller, 2011; Sense et al., 2011) could be expected from a maturing area of research (see also Bakker, 2010; Morris, 2010; Söderlund, 2004, 2010 for other trending articles). However, the trending mode does not make any attempts to question the assumptions, *per se*; rather, it contributes to a review of the present knowledge and possibly future directions based on the patterns that are observed. On the other hand, this lack of problematization is also present in general management and top journals, which are typically regarded as less practically oriented and have less of an instrumentalist heritage.

4.3. Vitalizing project management

Gap spotting is important in many ways. On the one hand, relying too much on problematization sacrifices accessibility, credibility, replication, holistic theories and a united field of research (Mone and McKinley, 1993; Tourish, 2011; Tsang and Kwan, 1999). On the other hand, relying too much upon gap spotting in research question construction may sacrifice innovative theories that challenge old assumptions and multi-perspective, cross-functional research. Söderlund (2010:17) suggested that project research should embrace this dilemma by a multi-perspective, cross-school approach to overcome the challenge of specialization and fragmentation of theories about projects. This ought to allow for some innovative ideas to blossom without the expense of existing theory. However, explicitly focusing upon the construction of research questions, such approaches do not necessarily ensure that problematization of the very assumptions occurs and thus in the long run that novel and "interesting" theories are developed. In a worst case scenario, a cross-school comparison could hamper the development of innovative ideas. There is simply a *risk* (not necessarily an outcome) that multi-perspective cross-school approaches further emphasize the assumptions in existing theory development if the schools are used as an argument for why the research is important rather than examining the foundations on which the schools rely.

Depending on how the question is constructed, in the typology of this paper, it would probably make the construction adhere to the under-researched gap spotting category. In a best case scenario, a problematization approach to research question construction would however resolve some of the antagonism between different schools and draw the explanations and therefore the conclusions closer together. By problematization, typically associated with fragmentation through new ideas (Mone and McKinley, 1993), unity may thus be the outcome. The final result is dependent on the scholars.

While gap spotting is warranted in many cases and indeed makes important contributions, it becomes an issue because of its identified dominance in project research, regardless of whether the research is across empirical settings or theoretical developments. Several of the investigated papers indeed presented arguments that were complex and constructive, presumably making important contributions to the contemporary knowledge of project management. An analysis of whether the contributions *per se* are important or novel is however beyond the scope of this paper. Similarly, the findings here do not imply that gap spotting is easy, nor that it should be abandoned. Indeed, it "rarely involves a simple identification of obvious gaps in a given body of literature. Instead, it consists of complex, constructive, and sometimes creative processes" (Alvesson and Sandberg, 2011:249). In such processes, the gap may be more or less significant and indeed a negotiation between the author(s), reviewer(s) and editor that ultimately makes the author(s) conform to certain accepted states of knowledge and theory (Bedeian, 2003; 2004). Hence, without denying the advantage and contribution of a gap spotting agenda, it is not important to open up the discussion for further scrutiny.

Instead of extending existing theory through gap spotting, the development of "interesting" theories requires "disciplined imagination" (Cornelissen, 2006; Weick, 1989). The concept of disciplined imagination applies to the construction of the research question since the way the research question is framed is supposed to influence the final outcome. Instead of giving evidence for disciplined imagination the clear dominance of gap-spotting articles in this paper instead indicates that the foundations of project research are reiterated and possibly extended, over and over again. Therefore, the disciplined imagination required for "interesting" and thought-provoking theories is not represented in that construction.³

The dominance of gap-spotting arguments and lack of disciplined imagination are particularly dangerous in project research, which, as a collective field, is seen as a less important area that lacks theoretical insights and contributions (c.f. Shenhar, 2001) and where many contributions tend to be based on long lost principles (Alderman et al., 2005). "Interesting theories" are instead created upon the challenge of these very principles and some (not all) assumptions of the reader (Davis, 1971; Jacobsson and Söderholm, 2011). Therefore,

³ Note that gap-spotting may also require some complex and imaginative constructions. However, the imagination that is used relates to how the literature is treated rather than challenging the foundations.

relying on previous assumptions and without rethinking how research questions are constructed, project research is unlikely to be vitalized as a field and “interesting” theories are unlikely to be discovered, which in turn would hold project research in its present academic status.

There are many ways to achieve problematizing research. It is however important to note that problematizing research is not restricted to a particular methodology for data collection (interviews, observations, surveys), or setting (construction projects, software development), nor does it offer a *carte blanche* for ignoring previous writings. Among many possible ways forward, one is to develop problematization-oriented research questions that include critical confrontations, the introduction of new ideas as well as quasi-problematization, which includes problematization (Sandberg and Alvesson, 2011). An example of critical confrontation is Clegg and Courpasson’s (2004) paper on how projects are less of a democratic, autonomous, de-bureaucratized tool than they are assumed to be. Instead they found evidence for bureaucratic mechanisms that governed the behavior of people. An example of the introduction of new ideas is Lundin and Söderholm’s (1995) paper on temporary organizations, which represented a significant shift from instrumentalist approaches to a more behaviorally-oriented approach. An example of quasi-problematization is Leonardi et al.’s (2011) study of project managers’ use of multiple media when communicating with subordinates. Relying upon ethnographic data grounded in multiple pairing theory, they explained the puzzling behavior of project managers using multiple cues to communicate threat. They found that not only do they communicate a threat, they also choose media that convince people about the threat. Lastly, an example of problematization of existing theoretical underpinnings is Chesbrough’s (2003) concept of open innovation. Open innovation essentially argues that the assumptions of existing innovation processes are in part outdated by current developments in innovation process where a principal organizer is lacking. These studies are merely examples of more problematizing studies to give an example of what such research question construction requires and what a possible outcome may be. What is evident from the studies used is that examples such as problematization—or rather, challenges to assumptions—include any kind of study that puts emphasis on the assumptions and challenges the validity of some of them in some significant way (Davis, 1971).

In summary, the findings of this paper lend support to those of Söderlund (2010) in identifying a need to rethink and reclaim project management research. One possible avenue is through cross-fertilization between previous works but if that is the case, future research has to be careful as to how the research question is constructed. Caution is required if the goal is to reveal new outcomes and challenge contemporary knowledge rather than simply extend existing theory and further entrench existing assumptions. To paraphrase Weick (1979:44), researchers are urged to become stingy about their use of gap spotting (including arguing about confusion, negligence, application, empirical need or example, and research overview as well as complying with editors’ and reviewers’ opinions until the nature of the article’s idea changes). Researchers should

also be careful about their use of combined more problematizing modes (critical confrontation, new idea, quasi-problematization), generous in their use of problematization, and extravagant in terms of grounding theories in innovative assumptions. If they succeed in this sense, more attention would be paid to extraordinary developments that are concerned with producing interesting and thought-provoking theory. Theories would be well grounded and would—rightly or wrongly—challenge previous theories, suggest alternative readings, and more importantly, suggest new ways of approaching different phenomena. The theoretical development would be less stable than a gap spotting contribution assumes, and more in the constant state of flux that problematization suggests. This would lead to the produced theories becoming a valid starting point for further theoretical developments (if one would like to adhere to the gap spotting behavior) and a good foundation upon which to understand and base contemporary practice.

5. Conclusions

Theorists are not remembered for having carefully chiseled out extensions of existing theories. Nor are great theories achieved without challenging basic assumptions. Without the careful chiseling research areas however run the risk of losing its credibility. Nevertheless, the research question is an integrated part of either craftsmanship. Since the research questions’ constructions were reviewed with reference to how they were expressed in the papers’ texts, the present paper cannot say anything about how the research question was constructed beyond that text. Therefore, the analysis is limited to what is written. Neither does this paper focus upon whether the results were “interesting” *per se*, but merely upon the created opportunities for becoming interesting. With a sample of 61 papers between 2007 and 2011 from two issues each of the leading four project management journals, the paper also runs the risk of missing more assumption-challenging papers and books in project research since they tend to come about rarely. The selected papers do however reflect the status of general project research as provided in the sample. Finally, novel ideas may emerge in books. Journals are however still the premium outlet for research and they are tightly integrated into academic advancement (Pfeffer, 2007) and the review process explicitly reflects the paradigmatic struggle (Bedeian, 2004). Without a specific focus on identifying novel ideas an analysis of journal papers seem appropriate. With these limitations in mind, some reflections seem in order. The present study’s review of the research question argument found that similar to research in leading management journals, project management research is focused on gap spotting and no paper problematized the foundations of project research. Compared to Sandberg and Alvesson’s (2011) typology of research question construction modes, the distribution between the papers was fairly homogenous. However, two additional categories were identified: empirical example or need, and research overview. While overviews do not seem representative of the distribution of papers in general, the finding of the “empirical example or need” category suggests that project research still relies to a great extent on the

heritage of practically oriented research. Since a lack of a research question, a research purpose or both was over-represented in the sample, it is possible that greater potential could be generated if the research question were constructed more from a theoretical point of view and on making contributions that follow from that. From the findings that an overwhelming dominance of gap spotting arguments relies on existing theory and its assumptions, project research has a tendency to re-emphasize the underlying assumptions of previous research. Re-emphasizing previous understandings is of course important in terms of establishing a field's credibility but it simultaneously hampers its further development and diffusion as well as its acceptance by a general audience.

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