

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 (Cimil 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

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