

Building theories of project management: past research, questions for the future

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Abstract

Project management has long been considered as an academic field for planning-oriented techniques and, in many respects, an application of engineering science and optimization theory. Much research has also been devoted to the search for the generic factors of project success. Project management has, however, in the last decade received wider interest from other academic disciplines. As the field rapidly expands, the need for an internal discussion and debate about project management research increases. Project management and project organization is a complex subject and, we argue, is usefully examined from several perspectives. In this paper we discuss the emerging perspectives within the project field. The paper also presents a number of questions that project research to a greater extent should acknowledge. The questions concern issues such as why project organizations exist, how they behave and why they differ. The principal argument is that too much effort has been dedicated to clarifying the reasons of project success and failure, while downplaying a number of important research questions that need to be discussed in order to further the knowledge about project management.

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1. The current state of project management research

The professional field of project management today is diverse, multifaceted and contradictory in several respects. On the one hand is the explosive development of professional organizations, such as the *Project Management Institute* (PMI) and the *International Project Management Association* (IPMA). These associations are not only known as organizers of a number of conferences, but also as promoters of the standardization of project management and certification programs for project managers. We have here a field of professionals, virtually flourishing, which attracts an increasing amount of members, who, as it seems, require standards, techniques and certification programs for their professional development. The interest in project management showed by professionals is, of course, explained by a general increase in the way of organizing business activities in projects [1]. This has in addition been documented in research on the organization of

product development [2] and of companies in a number of other industrial sectors [3].

The basic rationale underlying many of the texts and articles published in journals, such as the *Project Management Journal*, is the adoption of project management as “a method” for solving complex organizational problems. Such a viewpoint treats project management as one of several ways for handling organizational activity. Similar arguments and standpoints are found in numerous project management research texts.

Along with the development of project management practice, various networks have emerged primarily focusing on the distribution of knowledge and findings stemming from project-oriented research. One such network is *IRNOP* (International Research Network for Organizing by Projects), founded in 1994. Since its inception, *IRNOP* has arranged five research conferences with nearly one hundred participants at each conference. One of the recurrent issues at these conferences has been the combination of different fields of inquiry, which illuminates the cross-disciplinary character of the field of project management research. Participants come from such disciplines as psychology, pedagogy, business administration, organization theory,

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industrial engineering and sociology. Project management seems to be a research field with potentials of bringing different disciplines to focus on a focal phenomenon of study, i.e. projects. The breadth of perspective is challenging and interesting, but should, we argue, be accompanied by a continuing dialogue of some basic issues about project management research. We thus argue for the need of an elaborate discussion on the identity and content of the field of project management research.

Although, one might argue that the field of project management (both the practical and theoretical parts of it) has developed rapidly in recent years, some authors have argued that the focus has been much too narrow [4]. A number of authors have argued that, despite the academic interest in courses and programs, the research is not very well developed. Morris [5] argues, for example, that the academic awakening of interest in project-based undertakings is far too slow. Furthermore, in a review of the literature and theories of project management, Packendorff [6] claimed that project management is largely considered as a general theory that is not sufficiently empirical. Moreover, he stressed, in the dominant line of research, projects are seen as tools and project management is seen as a set of models and techniques for the planning and control of complex undertakings. Thus, a number of writers have in recent years stressed the importance of a diversity of theoretical perspectives and in-depth studies in order to construct “middle-range” theories on different types of projects. In the same vein, Shenhar and Dvir [7] stated that most research on project management “suffers from a scanty theoretical basis and lack of concepts”.

In a recent literature review, Pinto [8] claimed that the major developments of the research on project management have been into project risk management and critical chain project management. The author does not acknowledge any of the developments related to the research on temporary organizations. The author also does not fully acknowledge the problem that the research referred to suffers from almost an entire lack of empirical studies. The present paper aims at building further on the reviews carried out by Packendorff [6] and Pinto [8] by pointing to some recent trends but also to some of the most important questions for the basis of project management research. We suggest that these questions are crucial for building middle-range theories of different types of projects.

The article also criticizes the propensity of project management researchers to focus on the reasons for success and failure of projects. Instead, we argue, there are a number of important questions that need to be addressed—questions that might be at the core in order to develop our understanding of project management success. It is the intention that the mentioned themes of inquiry, and a critical discussion about them, should

be beneficial for identifying opportunities for future research.

The increasing interest from both practitioners and researchers from various disciplines is a positive sign. However, it also produces a need for clarifying some of the fundamental ideas and identification of project management research. This article attempts at stimulating an internal debate about project management research, about the development of research and about the important research questions that project management researchers should revisit in order to further the theoretical positioning of their work. This could, for instance, lead to a better understanding of the variety and broadening scope of project management research that has occurred in recent years.

Several authors have criticized much of the research on projects and project management. We take these pleas as a starting point for the present article. However, we argue that criticism also has to be very clear in terms of the alternatives to the dominant lines. In this paper, we elaborate on a framework for the analysis of project research and point to a number of research questions to which project researchers should pay greater attention.

The aim of the present paper is to introduce a discussion and debate about some fundamental theoretical issues related to project management research. The underlying reason is that such a discussion would contribute to the development of project management research on a general level and further its status as an academic discipline.

2. The intellectual roots of project management research

A number of writers seem to trace the intellectual roots of project management research and knowledge to various types of planning techniques, such as CPM, PERT, and the like [6]. Some even say that the father of (modern) project management is the well-known Henry Gantt, who invented the Gantt chart, which has become something of a standard model in project management practice [9]. A continuation on these lines would indicate project management as a specific problem-solving method, of delimiting and grouping activities by using various types of techniques and methods. As it seems, project management, and also project management research, would thus fall very close to optimization theory and applied mathematics and, for obvious reasons, part of the engineering schools' curriculum.

Others would trace project management research to completely different intellectual roots. A *locus classicus* here is the contribution by Gaddis [10] published in the *Harvard Business Review*. In that article, Gaddis defines, projects along the following lines.

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