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.

Keywords: Project management; Project organization; Temporary organization; Project research; Assumptions; Research questions

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,
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 a 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.
A project is an organization unit dedicated to the attainment of a goal—generally the successful completion of a developmental product on time, within budget, and in conformance with pre-determined performance specifications. [10]

The importance of the Gaddis article is that it was the first, and still is among the very few, publications in well-known management journals explicitly discussing the art and practice of managing projects. Projects as empirical entities were also important for Miles [11] in his treatise on temporary systems, which later on inspired Bennis and Slater [12] and Goodman and Goodman [13,14] to further the understanding of projects as organizational forms. However, in recent years a number of project-related articles have appeared in high-ranked management and organization journals, such as Research Policy [7], Organization Studies [15], Scandinavian Journal of Management [16] and California Management Review [17]. Interestingly, these articles explicitly relate and, in some sense, also criticize much of the writings within project management research. Shenhar and Dvir [7] write, for instance, “as an organizational concept, project management is quite new and not well-understood. [...] most research literature on the management of projects is relatively young and still suffers from a scanty theoretical basis and a lack of concepts” (p. 607–608). Furthermore, Lindkvist et al. [15] argue that traditional project management literature views upon projects very much as an analytical process, unable to explain the systemic character inherent in most projects.

One would thus conclude that there exist two main theoretical traditions in project management research. The first tradition with intellectual roots in the engineering science and applied mathematics, primarily interested in the planning techniques and methods of project management. The other tradition with its intellectual roots in the social sciences, such as sociology, organization theory and psychology, especially interested in the organizational and behavioral aspects of project organizations.

3. Basic assumptions in project management research

Basic assumptions direct our interest and thus also our research results. From a research perspective “a project” could be viewed upon as a construct for analytical purposes, e.g. that a social process has a defined beginning and a clear end. It is created by practice, and recreated, or modified, by the researcher who sets out to study the project. We know that fairly frequently the social construction of a project of a single actor might differ from the construction made by the researcher [18]. Thus, what the practitioner defines as the project might be irrelevant to the researcher. As researchers in project management, we would consider every project as being similar very much because of the act of labeling particular empirical phenomena as projects. However, we would also assume that no project is similar to another.

An important matter for empirical social research is that of perspective versus phenomenon. For instance, it could be claimed that projects are nothing else than a way of looking at industrial and organizational activity. Whether projects really exist is of less importance in this respect, which is similar to the argument stated above that the researcher’s perception of a single project does not necessarily have to correspond with the ones of the actors involved. Researching into projects is thus more a matter of looking and trying to capture the unique, complex and time-limited processes of interaction, organization and management. Hence, it might be possible to identify research that claims “project research” to be a perspective or a metaphor for studying entire firms, entire industries to be the most important unit of analysis, on the one hand. On the other hand, it might be possible to identify research, which states the importance of providing knowledge and theories about the organization and management of projects. Following this line of reasoning it might be stated that the research into the “management by projects”, i.e. studies that look at firms but pay special attention to the project dimension, advocates that projects provide a perspective for analyzing corporate activity [19,20].

In a project context, the universal elements are normally uniqueness, task complexity and time-limitedness, which are also the characteristics brought forward in the first chapters of project management textbooks [21]. The point made here is that we need to explicate the universal dimensions of projects but also thoroughly analyze the variations among projects. Thompson [22] early pointed out the importance of this fact for the development of organization theory.

Fairly recently, the term of “project theory” has been used, not only by practitioners, but also by organization scholars [3]. Even though these authors are relatively vague about what “project theory” signifies, the use of the concept reveals some important constituents. Project theory is sometimes strictly referred to as the practical knowledge, sometimes referred to as the “normative tradition” [6], including the textbook, the checklists and the optimization and critical success factor research [23]. This research tradition is well known for scholars of project management. For researchers interested in building theories from empirical data, such lines of research, however, provide very little in terms of theory. Concomitantly, we would argue that projects are important and interesting phenomena from which it is possible to build strong and interesting theories in order to increase our knowledge of certain parts of social life. Such a theory (or theories), we argue, would
focus on “action” and “temporariness.” Project organization is a key industrial activity and a key corporate process [24] and management without sound knowledge of projects misses a great deal of what management of contemporary firms is about. This has been stressed by authors on the project-based firm, boundary-less careers and time-paced strategies who argue that we need new ways of thinking about organizations and their management [17]. We suggest “project research” to be one productive approach to increase our understanding of the modern firm.

The widespread use of projects in organizations today is the driving force in the search for factors that influence project success. In spite of extensive research in recent years, there has been little agreement on the critical factors of project success [25]. A major reason, in our opinion, is the widespread assumption that a universal theory of project management can be applied to every project. The search for such a universal theory may be inappropriate given the fundamental differences that exist across projects. Of course, as stated by Winch [24], the contention is not that all projects are the same, but that there is a generic form that can be called project organization. We call for several different approaches and for a fruitful debate about important contingency and contextual dimensions. The point made here is thus that the engineering tradition and the social science tradition are incompatible on important issues, for one avoids uncertainty to achieve determinateness, while the other assumes uncertainty and indeterminateness [22].

We argue that there is a need for several types of theorizing, some that look at the universal aspects of projects, some that look at the specific aspects of certain projects. The latter could preferably be either within particular industries or firms, or be associated with a certain “project category”. However, it is necessary to constantly criticize the dimensions put forward, search for new ones and to keep a balance between the specific and the general aspects of project management. As proposed by Lundin [26], there is a need for differentiation in empirical and theoretical research, yet there seem to be several generic mechanisms everywhere in “the project field”.

The problem here is that universal theories do not necessarily have to imply that only one theory, or one best way of managing projects, is promoted. Taken to an extreme, the problem would be that if there were no connecting links between the project theories, then one would question the studies of “projects as a field” in its entirety. One important matter is thus to strive for a balance between universal and specific theories. Nevertheless, it should be emphasized that “universal” could here be some overall themes, some overall interests, etc., not a universal proposition of how to manage projects.

If we relate theories of projects to the theories of the firm [27,28] some issues might be stressed. Theories of the firm are conceptualizations and models of business enterprises that explain and predict their structure and behaviors. Although economists use the term ‘theory of the firm’ in its singular form, there is no single, multi-purpose theory of the firm. Every theory of the firm is an abstraction of the real-world business enterprise that is designed to address a particular set of its traits and behaviors [29]. As a result, there are many theories of the firm that simultaneously compete in offering rival explanations of the same phenomena and complement one another in explaining different phenomena [27]. Consequently, theories of projects, we suggest, are conceptualizations and models that explain and predict the structure and behavior of projects (or temporary organizations), and in order to further develop the project field a number of such theories would need to be presented—some complementary, some competing. In this paper we argue that each such attempt might benefit from considering five key questions. In the next part of this paper, we present and discuss five questions that might further the development of the project field and facilitate the building of theories of project management.

4. Questions for building theories of project management

In the above sections, the article has given evidence of contradictory basic assumptions about project management. In the following section we present five key questions. Our argument is that these questions are not only important for building sound knowledge of project management, but also essential for highlighting the weaknesses of current project management research. The questions1 we consider to be crucial for the research on the management and organization of projects would include:

1. Why do project organizations exist?
2. Why do project organizations differ?
3. How do project organizations behave?
4. What is the function of, or value added by, the project management unit2?
5. What determines the success or failure of project organizations?

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1 The questions are inspired by the overview of strategy research given by Rumelt et al. [30].
2 The term unit here signifies the group in charge of managing the project, or if not a group, the person in charge of managing the project.
In the following we will comment upon these questions seriatim. We begin each section with a brief literature review in order to discuss the “state of the art” on project management research. Second, we try to pinpoint the dominating line of research and its answer to each question. Finally, suggestions on some alternative sub-questions are presented.

4.1. Why do project organizations exist?

As stated earlier, the term “project” has become the vogue in describing contemporary organizations. Both large multinationals to small technology-based firms are being described as organized by projects, project-intensive, and the like. Typically, the term “project” is used to describe the observed pattern of organization or interaction. But just as often, it is used normatively: to advocate what organizations must become if they are to be competitive in today’s business environment. As put by Lundin [31] “if projects are so damned good, how come everything ain’t projects?” This relates to one of the fundamental issues for project research, i.e. why do project organizations exist?

Inspired by the discussion about why firms exist [32], one would think of a multitude of reasons why project organizations exist. Project researchers tend to use a fairly rationalistic argument in this respect. First, a project exists because there is something important and complex to be solved [14]. Second, a project organization exists because there is a need for a purposeful organization effort and a high need of coordination in order to execute a number of tasks/activities. As it seems, the “project form” is applied when activities are tightly integrated. Much project management research, even the one that is based on a social science tradition, would thus argue in fairly rationalistic fashion, mainly pointing to the task at hand [16].

Other explanations have, however, been suggested. One such explanation is found in the literature on industrial marketing [33]. Here projects are considered to be solutions to generate above normal rents by grouping activities as “turn-key deliveries” [34]. However, alternative explanations, we argue, would be useful for the development of our understanding of project management on a more general level. Important concepts would, for instance, be explained by transaction cost reasoning [28], by knowledge-based reasoning [27] or power perspectives [35].

There are, for sure, alternative explanations to be found in the literature. The problem is that these explanations are very few. Kreiner [36], for instance, puts forward that project work is a way for organizations, and for researchers, to release the creative forces within themselves rather than to plan; and a way to enhance participation rather than to control. He especially stressed “learning,” “participation,” “renewal,” and “innovation” as indispensable in project management terminology as they have been for years in modern organization theory. As it seems, not many project researchers have taken this plea seriously. Relevant questions would include the following ones. What are the alternatives to project management? Do projects provide efficient alternatives to develop and implement strategies? How can we understand the relationship between strategy formation and project management? Are projects efficient means to trigger learning, participation and renewal?

4.2. Why do project organizations differ?

Building on the argument put forward above, much research explains differences among project organizations by simply pointing to the differences between the tasks at hand. Different types of projects require different types of organization, the argument goes [37]. The problem is, not only that project management research has focused on a limited number of so-called contingency factors, but also that these factors are not explicitly critically reviewed. The most extensive study in this area would probably be the work by Shenhar and Dvir [38] published in the highly ranked journal Research Policy. In their work, a distinction is made between different types of technological uncertainty (low-tech, medium-tech, high-tech, super high-tech) and different types of scope (assembly, system, array). Their initial ambition thus seems to entail an attempt to establish a theory in the same vein as organization theory, most notably the contingency tradition of organization theory [39]. Their later work has, however, returned to the relatively rationalistic approach of identifying and documenting critical success factors [38].

Another article, published in one of Europe’s leading organization research journals is the work by Lindkvist et al. [15]. In their analysis, a contingency model of different types of project organization is presented. To be fair, also the work by Wheelwright and Clark [40] on “heavy-weight project teams” should be acknowledged here. Their work is written in a product development tradition, but has also been considered to be an important contribution to the management of projects. Their categorization has attracted considerable attention from project management researchers [2].

In sum, the difference between project organizations has not been analyzed thoroughly enough. Only a limited group of references is found in the major research journals. It would thus be advantageous to more clearly pinpoint and analyze the reasons why projects and project organizations differ. We have mentioned a few important contextual and contingency variables. There are, however, several other questions that might be of interest to project research, such as industry and corporate issues (e.g. strategy, organizational structure,
industry regulation and tradition), project issues (e.g., age, size, environmental uncertainty and complexity of various kinds). We thus suggest that project management must be understood as a situated practice where cultural, social and institutional traits are paramount [41].

4.3. How do project organizations behave?

Several authors have stressed that we only have limited knowledge of the behavior of project organizations [23]. The main part of the literature points to the well-known project life cycle in determining (or predicting) the behavior of project organizations [16]. The project-life cycle has also, for obvious reasons, been the foundation for illuminating and capturing the inherent dynamics of project organizations, for instance in terms of pre-study and conclusion phases. This is, however, merely one way of looking at the behavior of project organizations. The work by Gersick has convincingly shown that the project life cycle is not a good description of how real projects evolve [64,65]. Hence, a number of questions might be raised whether the project life cycle is used normatively and if it is an adequate and appropriate description of reality or not. Only a few studies have discussed the behavior of project organizations in theoretical terms [16].

The main point here is that the many models and techniques found in the project management field do not explain or increase our understanding about the behavior of project organizations. One fruitful metaphor of projects is that of “temporary organizations” [14,16]. As it seems, the label temporary organization is used, not only to capture the characteristics of project organizations, but also to clearly separate it from traditional (or more permanent-like) organizations. The basic traits pointed out is that of a time-limited, and often time-pressured, organization that is built up by a group of people who has never worked together before and with limited possibilities of working together again in the future [44,45]. The literature on temporary organizations has, in this context, emphasized themes as “learning”, “participation”, “commitment” and “action” in order to focus on the behavioral aspects of such organizations. Of course, the aspects of time-limitedness are considered to be at the core of understanding temporary organizations.

In sum, project-oriented literature has only modestly considered the mentioned aspects as vital in understanding projects, and thus our knowledge is limited in terms of the action triggers, the key obstacles of learning, and the important commitment processes in project organizations. A number of questions evolve. How are the organizational processes affected by the time-limited nature of projects? How do people react on time pressure and control by deadlines and milestones? How do project organizations move through the various stages of work? How is the behavior of project organizations affected by the uncertainty and complexity normally connected with many projects?

4.4. What is the function of, or value added by, the project management unit?

To a great extent, planning has been viewed as the main task of the project management unit. Several studies have documented the increased role of project managers and project directors in contemporary firms [2]. The task, for sure, is not merely one of planning in these cases. This leads to some further issues that should be taken into account in an analysis of the project management unit/team. This could be, for instance, the handling of different knowledge bases, the handling of differences in rates of time, the making of priorities and decisions, the setting of deadlines, and the overall information process of the project. Furthermore, as stated above, the project management unit should be responsible for the processes and structures related to commitment, participation and learning. There are thus a number of functions that the project management unit performs. These functions have accorded scant attention from researchers.

In some contexts, the project management unit only is in charge of the planning activities, or providing administrative support to the rest of the managerial hierarchy. In other cases the project management unit has a senior executive position, as in the case of the earlier mentioned heavyweight project management structure [46]. The trend towards “projectification” would considerably change the role, and also, most likely, the value added by the project management unit [2]. The focus on the value added by the project management unit is at the core of understanding the increased application of “project-based structures” on a general level. Whittington et al. [1] has convincingly documented project-based organizing to be at the top of the strategic agenda of many European companies. The so-called “project office” has been launched by, for instance the research and advisory firm Gartner Group, as an organizational solution that would increase the performance of many companies [47]. Other issues in the project managerial hierarchy are also of importance, for instance, the role of project portfolio managers, project management offices and program managers.

From a theoretical point of view, we should in addition address the question in a more critical sense. What positive effects do project organizing lead to? What are the downsides to project structures? Research in product development and matrix management is definitely helpful on this matter [40]. However, the specific focus on the work of project management units has not been dealt with, highlighting questions such as in what way does the project management unit trigger action, in
what way does it promote learning, participation and commitment, and how do we determine the value of their work? What competencies are required in order to take on the role as project directors and senior project managers? How could their respective roles be understood from a knowledge perspective, e.g. in the integration of technology and knowledge bases within the firm? And further, is certain behavior of the project management function correlated with the level of value added?

4.5. What determines the success or failure of project organizations?

As mentioned earlier, a considerable body of project management research, particularly of North American provenance, is grounded in critical success factor thinking [48]. Publications both in the International Journal of Project Management and Project Management Journal reflect the search for factors of success and failure ([49,6,23] for reviews of the literature). Research on critical success factor is also observed in other academic disciplines, for instance in product development [50,51]. In a project context, this approach seeks to systematically determine the set of generic factors that are critical to project success [48,52,53]. The logic of the search for critical success factors has been justified with reference to the many observed examples of project failure and the belief that the identification of generic factors will greatly facilitate the project implementation process in practice [48].

The study by Pinto and Prescott [48] published in the renowned Journal of Management Studies presents evidence of the following set of critical factors: clarity of goals, top management support, clear project plans, client relationship and communication. The studies by Buzek et al. [54] were one of the first to focus on the behavioral dimensions and organizational issues of project organization. This study also employed a broader definition of project success than the typical triple constraints of cost, time and conformance to specifications. However, as has been pointed out by Turner [49], although much of the research into this particular area has adopted broader definitions of project success, the traditional triple constraint criteria seem to prevail.

The critical success writings have been one dominant line in project management research. Söderlund [23] traces its history back to the empirical studies of project failures in which writers sought to explain the reasons for the frequent failures of projects in practice. In the 1980s, this led to several publications in, not only project management journals and books, but also in other management journals, such as the Journal of Management Studies and Journal of Management. A continuing issue for debate has been how to look upon these success factors, their generic applicability and the sampling methods used [55]. Recent writings have documented the difference in success factors among industries and project types and also extended the original success criteria [37]. Further, recent literature also acknowledge the variation of project success factors along the project life cycle [56].

We acknowledge the importance of researching success and failure in project management contexts. However, we believe that a certain tradition of success-oriented research has dominated. We argue that the major part of critical success factor research does not give us deeper knowledge about real life project management. This research does not acknowledge the dynamics and the social embeddedness of project management. We suggest instead a greater focus on the four above-mentioned questions in order to further the development of project research.

5. Discussion

The main argument put forward in the present paper is that there are openings for additional perspectives and there are openings for more empirical studies. Still, the field lacks in-depth case studies, studies of processes, and studies in real time—studies that would be beneficial in building theories for understanding fundamental issues of projects and project organizations [57].

The reasons why projects exist have in most part of project writing mainly been explained by referring to the type of task, most frequently by adopting “transaction cost theory” [58] or “contingency theory” approaches [7]. A project would thus be explained by fairly rationalistic arguments, i.e. the reason why project organizations exist is because there is a complex task with certain characteristics to be carried out. However, there are many more explanations that could be given a part from that one, for instance the fact that “the task” itself is a social construction. We need to know more about these issues in order to understand the background and reasons for the increased reliance on project-based organization of today [1].

The second and third key questions relate to some classic organization theory inquiries. There has been quite a lot of interesting work within these particular strands of research. In any case, we need to know more about these issues, partly because very little empirical work and only a few case studies have been carried out. Besides the adoption of contingency frameworks, it would be argued that in order to understand the behavior of project organizations we need more dynamic elaboration. The “riding on the project lifecycle” is a key aspect here [16,24]. For instance, activities vary dramatically over the life of the project, from, e.g. conceptualization, feasibility studies, detailed engineering, to testing and commissioning. It might be assumed that each of these requires a different authority system. The motivational and commitment matters of project orga-
organizations might also change over the lifetime of a project, the various hurdles facing project members during a project might explain and increase the knowledge on the behavioral dimensions of project organization.

Project management research has traditionally paid limited interest in the actual work and performance of the project manager and the project management unit. It is time for more thorough studies on the role, style and function of the project management unit. We welcome in-depth studies in the same vein as Kotter [59] and Mintzberg [60] have done on general managers on the theme “what do project managers really do (and why)?”.

Many researchers of project organization claim that organizational researchers have focused (or enacted) their studies on permanent, self-producing organizations [6,61] and have, according to Kavanagh [62], largely ignored temporary, unique organizations (such as project organizations). The irony of this situation is that there is now a rapidly growing interest in the so-called “post-Fordist” organization [62], which are strikingly similar to many traditional project-based organizations [63]. Research on project management is thus not only important for understanding projects. It is also important for wider purposes and can improve the understanding of management in general. The article maintained that in order to develop the field into a stronger academic discipline, five key questions require alertness.

6. Concluding remarks

The present article has stressed the importance of studying projects as organizations and focusing on how they differ and behave. The article has suggested an agenda for future research into some of the fundamentals of project management research. The standpoint was strongly theoretical, illuminating the importance of a lively and critical debate on project management research also from “classic scientific” standpoints. It was stressed that the basic assumptions frequently are not explicitly stated. Recent trends in “project research” point to “project management research” as widening its interest to also include company-wide issues, such as the management of project-based firms, project-based industries, project-based careers (cf. [66]). This trend further increases the need for an internal debate about the identity of project management research. Could it be more appropriate to talk about various levels of analysis in “project research”, rather than linking everything to the single project?

Finally, the article pointed out five key questions for project management research. We stressed the need to develop various theories of projects in a similar tradition as has been done within the broader field of management research. It was stated that questions such as why projects exist and why they differ must not be forgotten in order to expand our knowledge and encourage cross-fertilization among perspectives.

A theory of projects cannot be built on merely empirical insights, but has also to be driven by a particular theoretical perspective. It was argued that such perspectives exist in other fields and it should be plausible to try them out also in a project context. However, without forgetting the special traits of projects as empirical entities. We need to discuss and develop concepts in order to understand the plurality currently under way within the field of project research. It is truly a research field that might not only improve the management and organization of single projects, but also improve the effectiveness of many companies and entire industries.

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