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Editorial

Making Project History: Revisiting the Past, Creating the Future

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Abstract

To this point, project management scholars and historians have carried out surprisingly little research on the landmark projects of our past. This paper argues for the need for delineating Project History as a subject area that ties project management with history. The paper presents the need for more research into this area, the nature and the content of this specific subject area, and the potential contributions that might come out of research within it — for history (management history, business history, and technology history) and project management. The paper also gives an overview of the papers included in the special issue, and offers some ideas of future research in Project History.

1. Introduction

Welcome to this special issue on Project History. It is truly exciting for us to have been part of this journey — from the first ideas and discussions about the value and promise of Project History as an academic subject area and a field of inquiry that to a great extent is unexplored by project management research. Despite its title, the journey for us has not been that very long — it started with some loose ideas five years ago which materialized into the call for this special issue two years later. It has been a short but certainly very rewarding and stimulating journey — and we hope that the journey into the world of Project History just has begun. The process with this special issue commenced in the beginning of 2010 and the call for papers was published in the last quarter of 2010 with abstract submission deadline in July 2011. After two rounds of revisions, we received the final versions of the papers in January 2013. Indeed, we have met many new colleagues during this process and we think we have been involved in creating a new forum that makes it easier for scholars in project management interested in historical research, and likewise, historians interested in project management, to meet, share ideas and talk about the future. As were evident from the submissions to our call, the special issue attracted a new kind of scholar — project management scholars with a special interest in history, but also, historians with a special interest in projects and project management. In that respect, we hope that this special issue is the starting point to more submissions coming with a focus on Project History.

In this editorial we present the background to the special issue, the underlying arguments and core ideas, and present briefly the different contributions and major findings emerging from the papers included in this issue. We believe we are trying to make an important point: there is much to learn from research into the history of projects, the creation of landmark projects, the effects of projects, and the nature of project management. Not only project management scholars can benefit from a better understanding of projects of our past, but also, project management scholars have something to contribute to the study of history — through gaining a better understanding of the capabilities of project managers, the practices and techniques used in the projects of our past, and the effects those capabilities and practices have had on subsequent projects and, perhaps even their effect on the general societal and industrial development.

The paper is structured in the following way. Initially, we will discuss the need for a better understanding of history in

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project management, and equally the promise that the study of projects and project management might have for business and management history in general. In this section, we will also review the extant research that we believe could be seen as part of the first contributions within this subject area. Then follows a section on the practice of doing historical research and specifically what is important to make historical research on projects. We discuss a few of the challenges but also several of the possibilities that we see within this area. The paper thereafter moves on to present the papers included in this special issue from which we also try to make a synthesis of what we believe comes out in terms of a better understanding of project management. The paper ends with some ideas of future research within Project History.

2. Emerging historical concern

There is a growing concern in the project management community about the lack of historical understanding of the emergence of project management. There is also a growing interest in the role that projects and project-forms of production played during the Second Industrial Revolution (Scranton, 1997). In that respect, scholars have relatively recently directed the attention towards the role that project management played for the evolution of society and industrial development in many sectors and regions. There is also an increasing awareness that there is still much to learn from the landmark projects of our past. Both researchers in project management (Garel, 2003) and business historians (Scranton, 2008) call for the development of a history of projects and project management. Indeed with the notable exception of Peter Morris' work (1997), the overview of Kozak-Holland (2001) in his History of Project Management, and the in-depth studies of Thomas Hughes (1998) and Stephen Johnson (2002a,b), we actually do not know of any history of project management, nor systematic explorations of historical projects and their management.

To fill this void of historical research in project management, this special issue invited project management researchers interested in history, and historians interested in projects and project management. We wanted to build a bridge between these two areas or perhaps scientific communities. For sure, historians have been interested in projects but perhaps not at the detailed level we aspire to paint in this issue, and of course, some project researchers have written about historical projects, but not with the same focus suggested in this issue. Frankly, Project History — as a mix between business history, management history, and project management, we argue may offer new perspectives on fundamental questions within project management, and offer new empirical glasses to the fields of business and management history. The outcome would be that Project History is taken more seriously and that historical perspectives of projects and project management continue to emanate in the development of new perspectives in current research — that current research is informed by past practice and past empirical explorations. At the same time, the fact that we continuously develop our understanding of project management through research also calls for research to continuously revisit the projects of our past — since new concepts and new ideas also make it possible to re-interpret our history. In other words, the continuous interplay between past and present plays an equally important role as deduction and induction do in most social science research, and it is the to a large extent the interplay between the two that makes it possible for our understanding to expand.

There is another reason for this special issue, which relates to our argument that most of what is written about project history is actually skewed. For instance, the majority of textbooks in project management begin with a short historical section and then turns to the classical description of project management, its organization and techniques, most of which is notoriously disembodied, almost without taking context into account. The tendency is to produce a very shallow view on the history of project management. More sobering for the discipline of project management, the rare famous case study comes from political scientists (Hirschman, 1967; Sapolsky, 1972), historians of technology (Hughes, 1998), historians (Brooks et al., 1979; Hewlett and Anderson, 1962) or journalists (Kidder, 1981; Rhodes, 1986). The problem for scholars in project management is that these contributions, even if they provide valuable empirical data, are not oriented toward the specific analysis of project management and project organizing per se, and thus they rarely reflect on the process of project organizing or the act of project management. In that respect, what emerges is a narrow historical understanding of projects as static objects, rather than dynamic organizations. Accordingly, there is definitely still room for more historical studies of projects and project management — describing and analyzing it from a project management point of view.

This lack of historical research into project management raises several problems. First, the existing literature on project history is biased toward large, US, military and space projects. Hence, we need to broaden the perspective to other industrial sectors and national contexts. The history of projects and project management is accordingly a global phenomenon and variations exist across the globe, however, we know very little, for example, about the most influential projects in Scandinavian history, in English history, in South-American history and in Asian history, and their impact on management capabilities, management practice, and subsequent projects. We might be aware of the importance that some projects, the canal projects, the railway projects, and other infrastructure projects, played for the industrial development, but not specifically the emergence of management practices in projects and the nature of project organizing. We also know that people historically talked about the management of projects very differently, although the techniques they used are quite similar.

Second, history can help us to better understand the roots of project management and the evolution of current managerial practices. This could lead us to recognize innovative managerial solutions from the past that are still relevant today and contradict the dominant model of project management. Indeed there is sometimes a discrepancy between current descriptions of historical projects and their realities. For example Lenfle (2008) and Lenfle and Loch (2010) in a paper in the *California Management Review* thus demonstrate that the usual statement that the Manhattan project "exhibited the principles of

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organization, planning, and direction that influenced the development of standard practices for managing projects" (Shenhar and Dvir, 2007) is notoriously wrong. On the contrary, the Manhattan project exhibited managerial practices (typically parallel strategy, experimentation and concurrent engineering) that have been forgotten in favor of a more control-oriented approach of project management and are regaining relevance in today's innovation-based and fast-paced competition (Loch et al., 2006). Even more sobering, they show that this practices have been theorized in the past, specifically by the well-known RAND Corporation (e.g. Arrow, 1955; Klein and Meckling, 1958; Nelson, 1961), although with little impact! The problem is thus one, so common in management studies, of making simplifications of the past to promote new as radically different from previous findings, although they show significant similarities between what we already know. As Janik points out, the "idea that we are smarter, simply because we come later, is a scholarly form of hubris and no less self-destructive with respect to our cultural heritage" (Janik, 2006: 297). Accordingly, a better understanding of history might create an improved understanding of the difficulties in creating, shaping and managing projects — and thus add to the empirical wealth of the subject. This would then also make us more historically aware and search for continuous inspiration in the management of our past projects. It would then also hopefully contribute to a more humble orientation with regards to our present. For instance, that agile methods existed many years ago, that the idea of project management has changed over time and perhaps lost some of its original roots (Lenfle and Loch, 2010), and that what is new is not necessarily good, and what is old is not necessarily bad – that centralization is old and bad, decentralization new and good, management is old and bad, leadership is good and new (Cummings and Bridgman, 2011) — would also apply to project management. It is quite striking to see the simplified versions of project management of the past as principally driven by project planning techniques, optimization tools, with little focus on leadership. However, looking closer at the projects of our past reveals quite a different story, which for instance Sapolsky (1972) and Hughes (1998) rightfully observe.

Another role of Project History would be to create a common ground among academics within this domain of knowledge. Consider the importance of the Sydney Opera House project. This project, with its famous conflicts and overruns, has been discussed in series of textbooks and mentioned in many academic papers is really an example demonstrating the "power of examples" (Flyvbjerg, 2006) which, as underlined by Nonaka and Takeuchi (1995) makes it easier to transfer knowledge of a more complex type. It also makes it easier for people to talk about and share experience, and, indeed, shared examples such as this could then also lead to theoretical and metaphorical developments, similar to the paradox of the Sydney Opera House project, which has led scholars to talk about the difference between project management success and project success, the role of over-optimism, and the difficulty and timing of project evaluation.

The idea of common examples is an important one and to create a discipline or at least a knowledge domain would then have us to share not only theories, concepts and frameworks, but also examples. As Flyvbjerg (2006) points out — we should not underestimate the "power of examples" and the generalizations that might come out of rich historical narratives. Indeed, historical examples, such as those discussed in the present special issue, would potentially constitute such global and powerful examples that researchers can talk about, compare and use to illustrate their core arguments and main points.

And the story does not end there — it is an ongoing process of iterations between past and present. Given that we continuously improve our knowledge and add to our understanding of project management, we would then also need to go back and revisit these historical examples. Increased understandings of, for instance, modularization in large-scale projects would make us better equipped to actually understand the granularity of modularization in the great construction projects in ancient China (Pheng, 2007). In that respect, we are searching for a particular kind of generalization – not the reductionist one, but rather a particular generalization, of rich stories, and contextual understandings – a "packaging of vicarious experience" (Gaddis, 2002). In that respect, which is seen in many books about business history, the generalizations are embedded to a great extent in the narratives, in the stories, and in the perspective of the author.

One important role for the study of historical projects would then be to actually conduct some kind of "process tracing" or pattern recognition (Gaddis, 2002).when looking at the broader sample of historical projects – is there a particular pattern emerging, is there deviations in the identified patterns, and what could possibly explain those deviations? For instance, could one observe particular kinds of path dependency across an ecology of projects (Grabher, 2002), or a series of projects within a specific sector — what kind of projects were carried out, why were they carried out, how were they managed and organized? Is it possible to identify certain patterns in the evolution of a particular firm, similar to the emergence of the multi-divisional firm which Chandler (1962) so insightfully studied in the early days of American big business? For instance, as Söderlund and Tell (2009) demonstrate, there might be interesting patterns in the nature and process of projects over time in a specific company; what projects that were carried out, how they were carried out, and so on, which could lead to valuable insights about the development of organizational capabilities.

Generally, this kind of pattern recognition could then also lead to fewer factoids, i.e. questionable, incorrect or fabricated statements about the past, which today are presented and understood as facts. For instance, to some extent the discussion about the parallel strategies in project management from the 1940s mentioned earlier could in fact be about doing away with a fundamental factoid in the domain of project management, or even that project management was "invented" somewhere, sometime — is equally perhaps a management factoid. They might have used different terms, slightly different techniques and management practices, of course, but the task was the same: to management the project.

However, we argue, Project History is not only important for researchers in project management and scholars in business history. Project History might, we suggest, also contribute to

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practice – to open up a pool of experience to practitioners, and perhaps one of the most important roles for the scholar is to make this pool accessible – to summarize the most important things, to detract, to present in compelling ways the lessons learned from the past that might infuse the agenda of how we should do projects in the future. We hope that this special issue thereby would be interesting for the reflective practitioners who, by revisiting the past, could find inspiration to learn from the past and combine it with other knowledge and thereby create a new and hopefully better future. As Gaddis put it:

> "…if we can draw upon the experiences of others who've had to confront comparable situations in the past, then – although there are no guarantees – our chances of acting wisely should increase proportionately." (Gaddis, 2002: 9)

3. Corporate versus Project History

There are, undoubtedly, quite a number of challenges to conduct historical research within the area of projects and project management. There is one quite unique problem, namely that many of the organizations that we study were temporary — they existed for a certain period of time and then ceased to exist. There might not be a permanent institution that could take the responsibility of transferring information about events to the coming generations and thus make them possible to research. In that respect, there is, at least to some extent, a major difference between Project History and traditional Corporate History (such as the one presented in Alfred Chandler's work). In the latter corporations typically fund research about the history of the company, for instance which is common when the company is celebrating its 50th or 100th anniversary. Books in the latter stream are plentiful covering the history of all the global giants that constitute the common examples for people in business – practitioners as well as scholars – Siemens, General Electric, Asea Brown Boveri, General Motors, Ford, and Renault. Of course, which is a general problem in historical research, the ones writing history are then dependent on those funding and granting access to historical information. Typically then it is the history of the survivors – not the history of the ones that did not make it to the present day. In Project History one might argue that there are two primary types of investigations. One type of investigation happens where the project is embedded in an existing present, for instance projects carried out by the state or a major corporation which still exists and which keep records of their past projects. The other type would include those projects where this existing present of permanence does not exist, either because the corporation who was in charge of the project has been dissolved or because the project did not have any permanent institutional embeddedness when it was carried out — perhaps multiple stakeholders of which some might still exist, but no single unit taking the overall responsibility for collecting and storing information about the project.

However, there is also one important difference which actually opens up a host of new opportunities, namely that many projects are associated with intentional ambitions to make history. In that respect, already at the outset stakeholders are interested in producing information which might be used for latecomers to understand what happened and why these things happened. For instance, in some projects there are information departments with the sole responsibility to store information about events in the project, major decisions and so forth. We find that difficult to see in a corporate context — where founders of companies bring in expertise to help produce the history of the company. Exceptions exist, for sure, but these are truly exceptions. Thus, for Project History, there is a wealth of knowledge and information in many projects of our past — numerous books, dissertations and articles written about specific projects that can be used as sources for historical investigations today. Typically, Lenfle relies on a wide body of historical works on the Manhattan project (Hewlett and Anderson, 1962; Hoddeson et al., 1993, etc.), that have not been exploited in project management research. In that respect, perhaps Project History might give some kind of unique material to the general field of Business and Management History.

4. Doing Project History research

What then are the principal guidelines that scholars should consider when doing historical research? We must admit that we are no experts in historical research, but both of us have done a number of studies in corporate history and project management history so we hope we have something to share with the reader. We have also read what leading thinkers in the field of history have said about these issues and what is presented here is a compilation of our own lessons learned and what the leading thinkers have suggested.

In a wonderful little book, John Lewis Gaddis (2002) reflects on the role and nature of historical research. There is much to learn from this book.

> "We know the future only by the past we project into it. History, in this sense, is all we have." (Gaddis, 2002: 3)

We think Gaddis is making an important point that researchers in project management also would need to reflect upon. We are bound to learn from history, since history is all the data we have, and yet it is a rich data base that offers a lot to scholars, and given this, we might as well do our research systematically. At the same time there are problems or at least challenges when studying this past.

> "But the past, in another sense, is something we can never have. For by the time we've become aware of what has happened it's already inaccessible to us: we cannot relive, retrieve, or rerun it as we might resent it." (Gaddis, 2002: 3, emphasis in original)

There is however no guarantee that a better understanding of the project's of our past will give us a better capability to predict the future. However, what it does, or potentially could do, is to

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better prepare us for the future by "expanding experience, so that you can increase your skills, your stamina – and if all goes well, your wisdom." (Gaddis, 2002: 11).

Doing research on Project History would then also be a way to time travel, which might be refreshing for a number of reasons. It might give us a feel and understanding of a particular context, try to locate particular techniques and innovations in a specific context and thereby create a better understanding of them, their use, and contours. Time travel might also be good to escape from the everyday complications of life and work that might delimit our perspectives. To travel back in time might then make us more aware of our current situation, of the trajectory that we are currently on, and force us to see the larger patterns, so to speak. What then might emerge is some kind of "historical consciousness" (Gaddis, 2002) with the capacity to see things from above and better connect the evolution to our current situation, the past to the present, and the present to the past. As mentioned earlier this might create a positive humility of our current situation and lead to self-doubt, which could then be a necessary process before self confidence takes shape.

This has also important implications for theory development. Indeed, as pointed out by Kieser (1994) "historical analyses can serve to reflect on existing organizational designs and to criticize existing organizations theories. Historical analyses do not replace existing organization theory; they enrich our understanding of present-day organizations by reconstructing the human acts which created them in the course of history and by urging organization theories to stand the test of a confrontation with historical developments" (Kieser, p. 619). We are thus convinced, following here Cummings and Bridgman (2011), that history of (project) management is critical for our future in order to improve both theory and managerial and organizational practice, to make management a more "reflective" discipline (Schön, 1983).

However this raises important methodological questions regarding the type of historical understanding we want to promote. Claiming a history of project management is not enough. We should be clear on the type of history we want to promote. The challenge we face is to avoid two classical pitfalls in historical analysis (Dreyfus and Rabinow, 1983 p. 118): presentism and finalism. In presentism, the risk is to look for traces of the present (e.g. PMBoK best practices) in past projects. In finalism the danger is to try to find the foundations of the present in some distant times, and analyze history as a finalized process that necessarily leads from that point to the present. If we simplify (and exaggerate a bit), in this determinist perspective PM Bok would be the 'natural' end of project management history, its final state. Therefore, as pointed out by Engwall (2012), "there is a frequent tendency to interpret and understand historical endeavors and situations [e.g. building of the pyramids, Viking raids, etc.] in the contemporary norms and conceptual frameworks, or in other words, to take present day ontology of project management and force it upon the historical actors. Consequently, this kind of historical narratives tell us more about our present thinking than about the conceptual frameworks of its actors and actions." (p. 596).

This is where Michel Foucault's approach to history could help (Lenfle, 2012). In his works, building on Nietzsche's concept of genealogy, Foucault uses history as a method to question and deconstruct existing concepts and truths. Through historical investigation he demonstrates how concepts, theories and practices that are now considered evident are, in fact, socially and historically situated and constructed. He insists on making explicit the conditions that leads to the emergence of objects, knowledge, concepts and their insertion in society through detailed instrumentation. In so doing "genealogy demonstrates how a field's foundations are actually formed in a piecemeal fashion but then solidify to produce a sense of the development of knowledge while at the same time marginalizing other possibilities" (Cummings and Bridgman, 2011, p. 81). Foucault's genealogy proposes to build a "*counter memory*" (Foucault, 1971) that aims at reviving forgotten knowledge and reinterpreting shared concepts. His landmark contributions on madness (*Madness and Civilization*, 1961) and the birth of prisons (*Discipline and Punish*, 1975) demonstrate the fruitfulness of the genealogical approach.

5. Types of Project History research

There are probably many aspects and many dimensions of Project History research. In this section, we would like to present a brief overview of extant research and how we might identify different kinds of research within this area. We will make a distinction into the following categories:

1. History of project management practice
2. Landmark projects and project narratives
3. Corporate project history
4. History of project-based production
5. History of project managers

The first category entails research into the evolution of project management at various levels of analysis. It could for instance revolve around the emergence and diffusion of PERT and particular kinds of planning techniques. It might also involve research into various approaches of project management, the relationships between project management and other management domains, such as systems engineering and systems integration. Examples of research focusing on these issues are Morris (1997), Johnson (2002a) and Lenfle and Loch (2010).

The second category centers on single projects – not on following a particular concept or technique overtime. In this area we find a range of studies, such as Hewlett and Anderson (Manhattan project, 1962), Sapolsky (Polaris Project, 1972), Morris and Hough (Concorde, Channel link, etc.; 1987), Brooks et al. (Apollo Project, 1979), Hughes (Atlas, SAGE, etc.; 1998), etc. or, more recently (Midler, 1996) and Jullien et al. (2012). The interest here is primarily to document a project in-depth, describe the background to the project, what happened during project implementation and the effects that the project had. In this area we would investigate all kinds of decision-making, governance, leadership and organizational issues of importance for explaining the shaping and execution of projects. We believe that a lot of cases, that could also constitute a rich base for teaching, remains to be discovered and analyzed.

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The third category is more directed towards the firm-level. It overlaps with much research in business history, such as studies within the Chandlerian tradition. One example of work in this area is Söderlund and Tell (2009) and their study of Asea Brown Boveri between 1950 and 2000. The authors analyze the evolution of project management and the nature of project organization from the first large-scale projects in collaboration with national clients to increasingly international projects around the world. The authors thus try to depict the change in the nature and character of projects carried out, the change in the ways these projects were shaped, and the new forms of management and organization that emerged in response to growing project complexity. The point the authors make is that project-based forms of collaboration emerged relatively early in the growing multinational companies and that the so-called "P-form corporation" (project-form) played an important role for the growth and internationalization of large corporations. Accordingly, the authors add to the image of the large corporation as merely guided by M-form logics (multi-divisional), which Chandler (1962) focused upon. The authors identify a series of "project epochs" in the evolution of Asea Brown Boveri and identify a number of epoch shifts where the company moved into a new logic of project-based production. This led to the investment in a range of methods and systems to sustain and further develop the project competence of the firm — that is the ability generate and execute projects. In particular, various kinds of organizational mechanisms, shared leadership models, follow-up and review techniques along with collaborative agreements played a very important role already in the 1960s when the company began assuming the overall responsibility for large-scale projects within the power systems sector. In the same perspective Midler (1995) analyzes the evolution of project management methods at Renault and how the firm becomes more and more "projectified".

A fourth category could perhaps be labeled the history of project-based production. These studies take interest in the industrial development that particularly focuses on project-intensive sectors and describe the nature of the implemented projects. Indeed, such studies are primarily within the domain of business or economic history, but they are still touching upon what we would like to frame as Project History. One example, we believe, could be the work by Philip Scranton and his study of specialty production and American industrialization in the late 19th and early 20th centuries. Scranton (1997), in a study of custom and one-off production logics, showed the importance of the "other side" of the Second Industrial Revolution. In 1900, a third of the fifty largest manufacturing plants in the United States made custom and specialty goods, not throughput commodities following the mass production logic. Scranton demonstrates that the custom production fostered capabilities to continuously shift outputs, which in turned required skilled specialist, flexibility, ingenuity and resourcefulness. This diversity of work in process forced managers to devise systems for tracking the progress of orders and particularizing costs, to track, coordinate, and plan the work of each unit/project. Selling nonstandard goods routinely involved the creation of plans and estimates, which were often made in close collaboration with clients; clients supplied critical information which later on minimized errors. These custom producers utilized extensive contracting networks, rather than investing in integrated production. Many managerial innovations focused on means to "systematize", rather than to "standardize", production, coordination, information processing, recruitment, and marketing. As Scranton shows — "system" became the custom producers' buzzword, whereas "standardize" permeated the discourse of routine production. The custom producers' need for pools of skilled labor, often led to a co-location of firms with similar needs, thus creating urban industrial districts, which also boosted the sharing of knowledge and experience across firm boundaries. Scranton also shows that many of the firms that moved into mass production still relied heavily on one-off production and typically were able to house both modes of production within the same overarching corporate structure.

The final category we suggest is focusing on the influential people. Similar to other historical works of the influential entrepreneurs and business leaders, this area of research is also focusing on the important individuals, however the focus for obvious reasons is here on people that influenced project management. We believe there are reasons to believe that compared to the famous entrepreneurs and the famous executives, project managers have received little attention. Some studies have been done of engineering geniuses, and many of them are of course also influential project managers but the studies on project managers remain very rare (exceptions that we know are Johnson, 2001 on Sam Philips, Apollo project director, and Norris, 2002 on Leslie Groves, Manhattan project director). However, this area would focus even more strongly not merely on their persons, but primarily on their philosophy of project management, what projects they did and why. Examples of such influential project managers might include William Raborn who played such an important role in the Polaris project, Nils Ericsson who was the project management star in the early industrialization in Scandinavia, Thomas Telford — the engineering genius who thanks to his ability to integrate technology with management helped build the infrastructures that had such a huge impact on the British economy for years and years, and Johann Augustus Röbling (John Roebling) who built so many landmark bridges at the edge of technology in the United States and who literally died when working on his Brooklyn Bridge masterpiece. There are certainly more persons that we could mention and there are certainly many that we know a great deal of, but there are still many which are nearly overlooked, and other important project managers who we know a little about, but not necessary as project managers.

6. Taking Project History forward

As mentioned earlier, the present special issue had its origins in the belief that history matters in management (Kantrow, in press; Kieser, 1994) and, therefore, in project management. However, compared to business history and management history, which have had such a profound implication for management in general, and strategic management in particular (see for instance the work of Alfred Chandler), project management has been little discussed and scrutinized in a historical light. We think, in line with the aforementioned authors, that we need to develop 'Project History' as an important and integral part of project management

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research, that seeks to integrate historical research with project management research. More particularly, at the start of this project, we put down a number of objectives that we thought would be possible to reach if we took the task of going back to history more seriously. These were:

- Illustrate and analyze the role of projects and project management for industrial and societal development
- Establish a more complete understanding of project management that is different from the conventional 'tools and technique' tradition
- Identify and discuss a number of generic problems in the practice of project management
- Create a better empirical 'common ground' among scholars within the domain of project management, for instance through 'common examples' and 'common problems'
- Give a broader picture of project and project management history, complementing the dominant view of project management being a managerial innovation stemming from the defense industry, particularly in the United States
- Identify practices that seem to be critical for the success of projects and that are grounded in a contextual understanding of the particular project at hand
- Document the emergence of certain practices of key concern within project management, such as planning techniques, coordination mechanisms, team structures, visualization tools, etc.

In this perspective, the present special issue gathers interesting work on management and business history that specifically addresses the individual project (or a series of projects). The primary focus is on what might be referred to as industrial projects of various sorts, be they the early influential infrastructure projects (canal projects, railway projects), the large-scale development projects from the 1940s to the 1960s or a more contemporary project that exemplifies innovative practices. Thus, the overall ambition is to welcome contributions that study landmark projects, their background, effects, management and organization. We want to emphasize that this focus on individual projects does not mean that we ignore the context in which they emerge and unfold. We believe, following Engwall (2003), that it is necessary to link a particular project to its context and history. By so doing, the papers in this issue discuss the influence of a particular project on managerial practices, before and after its unfolding. Accordingly, we have therefore encouraged the contributors to position and present the studied project in its historical and institutional surrounding to allow for a richer and contextual narrative.

The core idea with Project History however, we believe, is to cover, in Hughes' words, the "collective creative endeavors that have produced the communications, information, transportation, and defense systems that structure our world and shape the way we live our lives" (Hughes, 1998: 4). As mentioned earlier, the take here is not to treat industrial projects narrowly. Instead, we have welcome contributions with a different focus and broader perception of industrial projects.

Our idea was also to invite contributions covering different regions and institutional contexts to allow for a multifaceted framing of the history of projects and project management. We initially delimited the period covered to "modern industrial projects", starting with the landmark infrastructure of the 18th century, moving over into the large-scale defense system projects and the landmark technology projects of more recent date. The focus is however not on technology as such. As also documented in Hughes' (1998) analysis, we assume the focus to revolve around management and organization, not merely technology as such.

Thus, the special issue speaks in favor of historical approaches to projects and project management, but at the same time it seeks to explore business/management history with a specific focus on projects.

7. The good and the bad projects, and more?

There is a natural tendency among management scholars to study the extremes. This means basically two types — the good organizations, the good projects, the most successful leaders, and the best practice on the one hand, and, on the other hand, the failures, the planning disasters (Hall, 1980), and the poor performers. This is also apparent within the domain of project management. The question is – how to deal with this problem when doing historical research – should we only study the extremes, are then only ones possible to study actually the extremes, since the mundane perhaps normal projects which actually perhaps concerned the most people, are not accessible because no one bothered to save records about these everyday projects so to speak. Are we then creating a history of the bad and the best, but no history about the normal life in projects? Is there a risk associated with such a research strategy?

Additionally, looking at the good and the bad — the extremes in the historical landscape of projects, we might have quite a lot to offer also to other knowledge domains and scientific fields. For instance, as Gaddis point out:

> "High modernism can manifest itself in architecture with faceless buildings that efface their own inhabitants, or in the urban planning that produces people-unfriendly places like Brasilia or Chandigarh, or in transportation schemes like those attempted in Tanzania and Ethiopia in the 1970s, or in such massive rearrangements of landscapes as the New Deal's Tennessee Valley Authority or Khrushchev's Virgin Lands Project, or China's impending inundation of the Yangtze's great gorges. And, most devastatingly, high modernism can involve the attempted reconstruction of an entire people. Hitler's purely Aryan Third Reich, for example, or Stalin's forced proletarianization of the Russian peasantry, or the most devastating single atrocity of the twentieth century in terms of deaths it produces – some thirty million Mao Zedong's Great Leap Forward." (Gaddis, 2002: 144)

The point here is not of course that project management scholars can answer all these problems, but merely that there might be something that project management scholars can learn from

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these massive projects – be they good or bad – and, which is still to be seen, that project management scholars perhaps could contribute to a better understanding of these collective endeavors by highlighting their organizational and managerial practices and issues.

8. Overview of contributions

The first paper is written by Gilles Garel. It gives a broader view on the subject of project management history than we normally see in textbooks and prior research texts. It emphasizes the fact that project management has been around since the early days of projects. The paper also illuminates the development from the pre-models of project management to the standard project models so common in business today. Finally, the paper offers a perspective on what we should mean with history of project management.

Another paper that sheds some new light on the history of project management is the paper by Stephen Johnson. It explores the important question of why project management as a professional practice emerged in the 1940s to the 1960s in the United States. The paper emphasizes the importance of technical complexity and novelty as primary forces driving projects as new organizational forms.

The paper by Hughes offers several ideas related to the value of project history. It shows in what respect current challenges in project organizing have been experienced before and in what sense historical research might guide managerial practice. Based on a study of the construction of the Main Drainage and the Thames Embankment, the author analyzes the innovative forms of construction, estimating, partnership and contracting models applied in the project. The paper shows the importance of projects in sustaining societies, the role of trust to take projects to fruition. In that respect, contemporary projects are much easier than historical ones.

Marshall and Bresnen argue that the study of historical projects has much to offer to enhance our understanding of project management. However, the authors underline the challenges associated with such scholarship. Based on the example of the Thames Tunnel project, which was completed in the 1840s, the authors focus on five alternative discourses: technico-rational; practice; networks of people, things, and ideas; politics; and society. The authors highlight the need for greater reflection on how and why different stories are told about project management, project management practice and project organization logics.

In McCurdy's contribution the focus is on a rather recent set of projects carried out by NASA. The empirical focus is on the concerted effort to conduct a series of low-cost space exploration missions. At the beginning, the new approach worked well. After a set of setbacks, NASA officials decided to abandon the novel approach and returned to the traditional, systems management-oriented approach. Based on a study of 31 projects, the paper suggests that both the initial team-based approach and the systems management approach have merits so long as management pays close attention to the specific requirements each approach imposes.

The paper by Hellström et al. focuses on the early stages of projects and their governance implications with a special focus on the nuclear power industry in Finland. The paper looks at the early stages through the lens of path dependence and illustrates how early decisions led the project on a wrong path. The authors also develop an alternative perspective highlighting the role of path creation to understand how the project actors mindfully navigated the project to avoid lock-ins. The study emphasizes the significance of building relationships between key actors and committing them already in the early stages of a project.

Winch asks the common yet important question of why major projects escalate in schedule and budget. He suggests a hindsight approach to shed light on the appropriate answers to this question. In particular, Winch discusses three contributions to the research on major projects: future-perfect strategizing, strategic misrepresentation, and escalation of commitment. The author argues that these contributions only make partial, but complementary, contribution to a more comprehensive theory of major project escalation. Winch develops a model that is applied to the case of the Channel Fixed Link to explore the dynamics of escalation.

The Dutch shipbuilding industry is the primary focus of Levering et al. This industry has a longstanding tradition with production principles based on projects. However, recently several industry players have acknowledged a serious mismatch between inter-organizational project practices and demands. The mismatch leads to a weaker competitive position due to higher communication and production costs, and longer production times. However, the author points out, the reasons for the misfit are unclear. The paper studies historical developments of inter-organizational projects in Dutch ship-building projects to understand the extent to which today's problems are rooted in project practices from the past, path dependencies, and lock-ins.

9. Synthesizing the contributions: emerging insights

What then do come out of these papers? What do we believe there is to learn and how would these insights contribute to our understanding of management, and particularly project management practice? We will try to give our view on what we learn and what we believe there is to learn. In the various papers there are of course much more details and points made than what we could consider here, however, we still believe that it might be relevant to bring these insights to the surface and reflect upon them in the editorial and in light of what we have written earlier in this paper.

First of all, we believe that the papers in a variety of ways illustrate the value of historical research. They document how we can learn from project practices of the past, how this might inform our responses to current challenges.

Second, the papers document the scope of project history – which is to be seen as much more than the narrow writing of project management history – it would involve the writing of corporate history in projects, the history of project managers, the formalization of project management models, the broader view of project management practice, and so on.

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Third, the papers shed light on the way that particular practices create paths that determine the future of project management. For instance, this is evident in the shipbuilding history in the Netherlands, and in the Nuclear industry in Finland. In that respect, they show the value of historical research also for current practice and the formulation of research questions to address contemporary project management problems.

Fourth, they broaden our sample of project beyond the US aerospace or military projects which remain dominant in project history. They offer a broad variety of empirical examples — from different regions, but also from different empirical environments, including shipbuilding, nuclear power plants, and construction.

Fifth, the papers also discuss the process and conduct of historical research. Graham Winch for instance speaks about the possibilities of the hindsight approach and Marshall and Bresnen discuss the possibilities, yet limitations, of historical narratives. They offer new concepts and new lenses for the pursuit of Project History.

10. The future of Project History

We hope that this special issue will be well taken and that scholars from both the field of project management and history will look upon it for what it was intended to be — a first attempt to share experience from historical research on projects and project management, and to create an arena where project management scholars can meet with historians. The future will show what will come out of the debates and discussions on this arena, but we hope that we, with this special issue, have created more interest among scholars to do historical research on projects. We also hope that we have contributed to an awareness of the significance of historical consciousness, which might have important implications on not only the practicing manager, the scholar, but even for the entire field of project management. As Geoffrey Elton (1967) has pointed out, historical consciousness can help establish human identity. For the field of project management it might create a better understanding of the project practices of the past, establish a stronger identity for those people interested in the project management of the past, and thereby also contribute to defining and redefining project management as a particular scientific inquiry. Let's continue making history — Project History!

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