

Abstract

The paper explores three texts in the field of megaproject management that intersubjectively, in terms of community sentiment, might be considered 'classics'. We deploy four criteria for a structured analysis that determines if the status of the works in question may be considered classic. The works examined are *Megaprojects and Risk: An Anatomy of Ambition* by Flyvbjerg, Bruzelius and Rothengatter; (2003) *The Anatomy of Major Projects* by Morris and Hough (1987) and *Industrial Megaprojects* by Merrow (2011). Based on these works we conclude with a prospectus for future research that will serve to develop the field of research into megaproject management.

Introduction

One of the ways in which a field of research consolidates, gaining cohesion and consistency, is through significant and outstanding works that play a defining role. Kuhn (2012, p. 10) describes the way in which significant scientific achievements, often encapsulated in the classics of a discipline, become paradigmatic by being disseminated through textbooks and other normative texts. The paradigm forms the accepted framework for the body of theory in a discipline. Over time, the boundaries for what is considered normal science within a particular field increasingly become institutionalized within the paradigmatic frame.

This paper examines three works that might rightly be considered classics in the field of megaproject research: *Megaprojects and Risk: An Anatomy of Ambition* by Flyvbjerg, Bruzelius and Rothengatter; (2003) *The Anatomy of Major Projects* by Morris and Hough (1987); and *Industrial Megaprojects* by Merrow (2011). The extent to which they form a paradigm for megaproject research is another matter. Two matters need to be resolved before proceeding further. First, what constitutes a megaproject? Second, what constitutes a classic?

What constitutes a megaproject?

Research into the management of megaprojects has emerged only relatively recently as a distinct area of study. It draws on research into project management and can generally be considered a sub-set of, or specialisation within, the broader field of project management. Overall, the research paints a dire picture of the field of practice in terms of its goal achievement. Boateng et al. (2015) cite the tendency for

gross estimation errors; Davies et al. (2014) chart a litany of failures to reach specifications; Eweje et al. (2012) note the disproportionately negative impact of megaprojects on corporate survival. The proportion of global GDP spent on megaprojects (Flyvbjerg, 2014) certainly justifies an increased focus on this topic, especially in light of the history of flawed goal attainment.

Some analysts, such as Flyvbjerg (2014), stress that megaprojects should be defined quantitatively, in terms of their cost:

“Megaprojects are large-scale, complex ventures that typically cost a billion dollars or more, take many years to develop and build, involve multiple public and private stakeholders, are transformational, and impact millions of people.” (Flyvbjerg 2014, p. 6)

We demur, considering that the real mark of a megaproject is the organizational complexity, ambiguity, ambition, politicality and risk that are entailed (cf. Baccarini, 1996; Bakhshi et al, 2016). Not all expensive projects need be complex, ambiguous, ambitious, political and risky; somewhat smaller, but still costly, projects might well be all of these.

What constitutes a classic?

Alexander (1989, p. 9) describes classics as *“earlier works of human exploration which are given a privileged status vis-a-vis contemporary explorations in the same field”*. While this is one possible answer to the question of what constitutes a classic there are other considerations. Multiple categories and criteria exist that determine

a classic. Söderlund and Geraldi (2012), for instance, categorise classics into four types, each using a different criteria to determine whether a text is a classic. The first type is called 'obvious classics', a type of classic determined by its prominence and acceptance in the field, signified through the number of citations received. Other publications become classics due to the influence and impact they have had on the field, in terms of shaping its current state. This second type of classics they call 'latent classics'. The third type is what Söderlund and Geraldi (2012) label 'potential classics', works of scholarships that present innovative ideas and solutions ignored by scholars at the time of their publication. The fourth type is the category of 'unintended classics', works never intended to contribute to a particular field to the extent that they did. An example for this could be Henry Gantt's work and the contribution that it made to the field of general project management (Söderlund and Geraldi, 2012). While we agree with these categories, we argue that a classic must meet a combination of *all* the above-mentioned categories and criteria – rather than just one.

As Söderlund and Geraldi (2012) rightly argue, the process of determining a classic is not a "scientific exercise" (2012, p. 568). Kuhn (2012) proposed four criteria for constituting something as a classical work. First, one characteristic of a classic is the novelty of the idea which it conveys. Second, a classic must be communicated effectively so that it can reach a broader audience. Third, classics must be measured by the widespread awareness of the work amongst relevant scholars in the field. Fourth, dissemination of research in the mass media is an effective technique to measure the impact of classics. Drawing on another, perhaps unlikely, starting point

for assessing a classic in megaprojects and for developing specific criteria for the exercise, is the literary writer Calvino (2000), who offers a postmodern literary perspective on what constitutes a classic, providing fourteen criteria. His definitions are tailored towards understanding the value of great works in literature, focusing on the role of classics as formative points in a society or culture but also consider their personal impact and the way that they shape perspectives on the world.¹

Calvino's criteria can be customised for an enquiry into academic classics, focusing less on the impact on an individual, and more on the objective influence of the work on the formation of a field. Hence, this paper combines Calvino's (2000) work with Kuhn (2012) and elements of Söderlund and Geraldi (2012), to establish four criteria that were used in our assessment of whether a work is a classic in its academic field.

The first, and simplest, criterion relates to the influence of the work, or what Kuhn (2012) terms a spread of awareness. A classic is a work about which much is spoken; *"...a work which constantly generates a pulviscular cloud of critical discourse around it..."* (Calvino, 2000, p. 6). Whether it is in praise or condemnation, a classic must make an impact, and the simplest way to understand this in an academic context is the number of times a work has been cited.

The second criterion relates to the persistent value of the work in terms of its impact on public discourse, as Kuhn (2012) contends. An academic classic should be a work that is not only of a particular time but whose relevance as a point of reference

¹ Calvino talks of classics with a sense of romantic wonder. To Calvino, *"...the classics help us understand who we are and the point we have reached..."* (2000, p. 9). Classification of a work of literature as a classic can be a very interpretive process and although he provides criteria, Calvino acknowledges that *"...what distinguishes a classic is perhaps only a kind of resonance we perceive emanating either from an ancient or modern work, but one which has its own place in a cultural continuum"* (p. 7).

persists through time. In an academic context this could be judged through reference to the long-term citation rate of the work, a criterion particularly relevant to older works. If an older work continues to be cited, despite its age and the changing whim of the times, it clearly has had a lasting impact upon the field. The long-term significance of recently published works would, of course, be impossible to judge.

The third criteria we consider in this paper relates to the way in which classics serve to shape a discipline. Calvino describes classics as “... *those books which come to us bearing the aura of previous interpretations, and trailing behind them the traces they have left in the culture...*” (2000, p. 5). A classic has a formative morphological function in a discipline. Influential texts provide unity to otherwise disparate elements, providing a common focus, concepts or language to a discipline, framing the context within which future developments can be built. Classics define the discourse by enunciating significant aspects of the discipline that have hereto remained unexpressed. Kuhn (2012, p. 10) talks of classics as being “...*sufficiently unprecedented to attract an enduring group of adherents away from competing modes of scientific activity.*” He emphasises three factors here: the newness of the thoughts expressed, their attractiveness and incompatibility with aspects of previous ways of thinking.

The third criterion is both the most difficult to assess and the most essential. There are many works of popular fiction that have sold considerably more than Joyce’s (1986) *Ulysses* but that are unlikely ever to be considered classics. *Ulysses* heralded a whole new approach to literary fiction, but may never sell like Stephen King. To

shape a discipline, a classic must serve to redefine how the field is seen. It must say something substantially different to the norm; in Kuhn's (2012) terms there must be novelty in its ideas.

The fourth and final criterion that will be used as a point of comparison in this paper relates to the personal impact that the work has on the reader. A classic is a work of a significant depth, one that offers new insight, changing the reader's perspective. A classic is a book for all seasons, it is something that one returns to again and again, in Kuhn's (2012) terms, as an exemplary text that aids one's understanding of the world, Calvino concurs: *"A classic is a book which with each rereading offers as much of a sense of discovery as the first reading" and "...a book which has never exhausted all it has to say to its readers"* (Calvino, 2000, p. 5). Whether this is through expression of the accepted knowledge in a field in way that provides revitalisation or integration of established concepts or through fundamentally reshaping a discipline, a classic plays a role in framing the reader's world-view. Calvino (2000, p. 7) expresses the personal interaction between the work and the reader when he comments: *"Your' classic is a book to which you cannot remain indifferent, and which helps you define yourself in relation or even opposition to it."* As a criterion this one necessarily involves a significant subjective assessment; it also implies that an understanding of the depth of a work can only be established through a close reading of the text.

Let us now put these criteria to the test using an obvious PM candidate. Is the PMBOK Guide (PMI, 2013) a classic of the project management literature? The PMBOK Guide in its five reincarnations would certainly pass the first two criteria,

both in terms of the raw number of times it has been cited, as well as the longevity of its rate of citation. However, the authors are not convinced that the PMBOK Guide is written in a way that shows the depth of expression in which one would find new levels of insight upon a second reading. It has not become part of the authors' fundamental worldview or part of the way in which we both see ourselves and interpret the world around us. Rather, it plays the role of an influential normative text, one that summarises base-level existing knowledge in the field, rather than transforming it. On this basis, the PMBOK Guide could *not* be called a classic of the project management literature.

What constitutes a classic work on megaprojects?

In this paper we do not offer a systematic sampling method but a personal selection of classic works that seems relevant to this special issue, in line with what Kilduff and Dougherty (2000) did previously for a similar analysis. In the case of works on megaprojects, one could ask whether the work says something significant about megaprojects that is substantially different to the broader literature on project management? Does it serve to consolidate research into megaproject management into a distinct area of research? Does it provide a unifying force, whether through attraction or opposition, that brings the field together, and establishes a base upon which others build?

We will review the three megaproject works outlined above for the following reasons: First, we decided to focus on scholarly books as books can be classified as more of a mass medium than journal articles, one of the criteria Kuhn (2012) puts

forward. In other words, books have a higher potential to disseminate megaproject knowledge to a greater audience. The chosen books further provide a representative perspective from different eras and decades. Each of those decades focused on different aspects of megaproject management and the differences help us determine whether their contribution was ground-breaking, innovative or revolutionary at the time, which helps us to cover Kuhn's (2012) criteria of novelty. In the remainder of this paper, we will address the overarching questions of whether there are classics in the megaproject management literature, using the four criteria gleaned from Kuhn (2012), Söderlund and Geraldi (2012) and Calvino (2000).

Book 1: *The Anatomy of Major Projects*, by Morris and Hough

Morris and Hough's (1987) influential work *The Anatomy of major projects* presents the results of research into a series of major projects, mainly in the UK, including the Channel Tunnel; Concorde; the Advanced Passenger Train; the Thames Barrier; the Heysham 2 Nuclear Power Station; the Fulmar North Sea Oil Field; the computerization of PAYE, and Project Giotto. In terms of nominal value the projects studied in this book fall substantially short of the \$1 billion benchmark often used as a criterion for categorization as a megaproject (e.g. Flyvbjerg, 2014). However, most of the projects and the degree to which the projects were subject to broader political influence suggest that these 'major projects' are indeed what one would now term megaprojects.

The book focuses on the practicalities of implementation, styling itself as a "*study of the reality of project management*" in the surtitle. The authors identify that the work

answers specific deficiencies in the project management literature: “...*project analysis has often tended to give too little attention to the management and implementation aspects of projects ... and has dwelt too exclusively on the economic and financial aspects*” (p. 7). The book can be considered a precursor to the more recent stream of research focusing on the ‘actuality’ of project management, starting with works by Cicmil et al (2006), Winter et al (2006) and Berggren & Söderlund (2008).

Criteria 1 & 2: overall impact of the work and impact over time: The work has been cited 941 times, according to Google Scholar (19/2/16), which is a quite respectable number of citations. Review of the citation rate for the work (Figure 1) demonstrates the longevity of the work. It is clear that this work is not only an influential text but also one that readers continue to find relevant. One might draw the conclusion that the work is actually becoming more relevant over time, perhaps peaking in 2011, and that the work was ahead of its time. However, the citation rates need to be understood in the context of the broader field. Previous research (Pollack & Alder, 2015) has shown that the citation and publication rates for project management related publications has been growing and can be graphed with a similar curve: all ships sail higher on a rising tide irrespective of their content. Nonetheless, it is safe to say that the work continues to be seen as relevant to a wide audience. This work comfortably passes the first and second criteria.

Figure 1 here

Criterion 3: Formation of a distinct discipline: It is less easy to assess the influence that this work has had in shaping the discipline. Reviewing the publication source (e.g. journal, conference or book title) of the 67 publications that cited this work in 2015 shows that approximately 28% of these publications were generally published in sources directly related to project management. Approximately 14% were published in sources related to general aspects of management, 27% were in engineering or technology related publications, while 32% were white papers, unpublished, or were not listed in such a way that allowed the publication to be identified. Overall, the data suggests the work has had a broad influence and that its relevance is not limited to project management specific publications. However, only 9% of the publications citing Morris and Hough (1987) in 2015 mentioned 'megaprojects' or 'mega projects' in the full text of the publication. Mostly, the work is being referred to in contexts not explicitly related to megaprojects. The question is can it be a classic of megaproject management research if it is primarily referenced in areas that do not touch on the area? For a work to play a formative and defining role in the establishment of a discipline, one would expect that a substantial proportion of works in the field would cite the work. In addition, a Google Scholar search (19 February 2016) for 'megaproject' returns 1520 publications since 2015. Morris and Hough (1987) do not appear to be cited by many of these works.

A close reading of the work also provides a further understanding of the ways that it has contributed to the field. The authors identified a wide variety of factors that affected the performance of the major projects they studied, including leadership challenges, diffuse sponsorship, government influence over industry, contextual

change, success measurement over the longer term, estimation issues, production before specifications are complete and government assumption of risk. In many cases it is difficult to differentiate between how these factors affect major projects in a way that is substantially different from smaller projects. For example, they identify that major projects “...require an exceptional level of management” (p. 14), which is generally taken to be leadership. Leadership is also an area of significant research in the general project management literature (e.g. Lloyd-Walker & Walker, 2011; Tyssen et al, 2014) as well as a major subfield of contemporary management and organization studies. It would be accurate to say that while Morris and Hough (1987) identify that major projects severely challenge the leadership qualities of those who undertake them (p. 241) they do not explore in detail the ways in which leadership challenges are qualitatively different, or whether the difference is solely a matter of degree. Nor do they make any significant impact on leadership research, as few leadership studies acknowledge their work.

Morris and Hough (1987) identify sponsorship as an issue in major projects, noting that these projects are often sponsored by aggregate organisations (p. 15). They give the example of the Concorde, which was sponsored by six organisations, “...with no one authority in absolute control” (p. 51). The impact of a lack of authoritative clarity in sponsorship has also been identified in the general project management literature (e.g. Bryde, 2008; Pinto & Patanakul, 2015). It remains unclear whether sponsorship, as with leadership issues on major projects, is a fundamentally different phenomenon in megaprojects to the issues encountered on smaller projects.

The influence of government, particularly in terms of a strategic interest in developing industry also emerges as a recurring theme, particularly in the Concorde development, ICL's involvement in the PAYE computerization and the Heysham 2 reactor. The Concorde project was initiated at a cross-national Anglo-French government level as a way of propelling the European aeronautic industry to the next technological level. *"It seems that they [the French Government] never had any doubt that the project was not commercially viable, but regarded it as the vehicle whereby their industry could be resuscitated"* (p. 45). The PAYE computerization project was significantly re-scoped before contracts were let, to ensure that ICL could secure the contract. ICL, the UK's largest computer company, had been suffering poor returns such that it was politically expedient for government to throw it a significant contractual bone; hence, the contract was awarded to ICL for political reasons (p. 176), and helped to ensure the company's ongoing viability, at least for a while. For Heysham 2, the 'Thermal Reactor Strategy' adopted by the UK Government determined the technologies that could be used and the direction of the project (p. 115). It is unlikely that government will get involved in the direct operation of smaller projects (except where it is the client); nonetheless, government strategy can have an influence on smaller projects (e.g. Low et al, 2015; Pollack et al 2013).

In a context where the influence of government in major projects appears more pronounced, many factors associated with government's role also been identified on a smaller scale by authors researching smaller projects. For instance, government involvement in strategic partnering and procurement (Beach et al, 2005) has been

identified in general project management as a way of providing long-term benefits to organisations. Companies may enter into partnerships as a way of securing their hold on a sector of the project market or to develop new capabilities. Other companies might underbid on government contracts (Manu et al, 2015) as a way of maintaining a market presence when struggling financially or as a way of entering a new market.

The influence of the context of major projects and of factors outside the standard remit of project management also stands out in Morris and Hough's (1987) work. The context in which work is done has also been shown to have a significant impact on work at both the project (Klimkeit, 2013) and programme (Pellegrinelli, 2002) level. It is possible that this is more pronounced in major projects, due to their long duration (Morris & Hough, 1987, 227). Morris and Hough identify that a considerable percentage of the cost increases on the Channel Tunnel could be put down to inflation and exchange rate movements (p. 31), while in general *"...the causes of this poor performance are generally to be found in areas which have traditionally not been the concern of project management ... escalation, government or client induced changes, increased order quantities, increased safety requirements, interest charges, land acquisition charges and so on."* (p. 12). Unanticipated changes in contextual constraints can be particularly problematic and the longer the project, the more likely it is to experience significant changes in context as unexpected events happen, a factor that appears to be contingent more on the project duration than the size of the project.

Morris and Hough (1987) found that in many cases there was pressure to progress the project before earlier stages were complete. In the Thames Barrier project, the design was started before many requirements had been finalised, resulting in delays in production (p. 83). Heysham 2 also experienced a situation where development work was undertaken during design and construction, as complete exploration of design changes had not been considered (p. 110). They identified overlapping design and production as a cause of great concern in major projects (pp. 216-7). The size, technical uncertainty and complexity of major projects make it particularly difficult to clearly identify goals and objectives (p. 211). Examination of the literature suggests that these issues are not limited to major projects, with issues of overlapping design and construction activities identified in the construction industry (Hossain & Chua, 2014), while issues with unclear goals and objectives are found to be common in organisational change (Costello et al, 2002) and IT projects (Müller, 2003).

Morris and Hough (1987), focusing on Issues in the estimation of major projects, suggest that perhaps *“the biggest single impact of government on major projects, and the cause of the greatest regrets, has been the making of commitments without a proper investigation of the consequences.”* (p. 225). For example, the Concorde project suffered from ambiguous specifications, with no clear schedule or budget against which future growth could be measured (p. 213). The Concorde testing program was significantly beyond the scale of any previous aircraft testing, significantly contributing to overspending on the project (p. 47). In addition, estimates did not anticipate the low uptake of the aircraft. It was a relatively low

volume and expensive niche carrier at a time that less innovative, sub-sonic planes, such as the Boeing 7 series were creating mass markets. This resulted in a commercially unacceptable product (p. 57).

Measuring success is notoriously difficult to evaluate in major projects. Morris and Hough (1987) found that overruns were not always the best measure of the success of a project, as projects might still be profitable despite delays or overruns due to changes in market conditions (p. 13). They also found that success could not be evaluated at handover. From the owner's perspective, success may not be assessable until after the payback period or subsequent to an internal rate of return being assessed (p. 194). However, these factors are not unique to major projects, with poor estimation and underestimation identified in smaller projects (Long et al, 2004), although research does suggest that, as the actual size of projects increases, the size of cost overruns also tends to increase (Jørgensen et al, 2012). Turner and Zolin (2012) discuss the general need in projects for success assessment at the point when the impact of project deliverables becomes clear. The success of all projects can be most accurately assessed when it is clear whether the project has contributed to organizational objectives. The classic case is the Sydney Opera House: over a thousand per cent over budget, and five years late in delivery. As Flyvbjerg (2014) notes, the major political wrangles in the project destroyed the architect Utzon's career, potentially depriving the world of other comparable works. However, viewed from the perspective of the impact on Australia, it is one of the most successful constructions of the twentieth century.

Morris and Hough (1987) found that in some cases government ultimately assumes the financial risks associated with projects, supporting contractors through otherwise untenable situations. For example, in the Heysham 2 development, it was accepted that the government ultimately accepted the risk of the project's success, as the developer was too small to be able to guarantee the station's performance (p. 117). In the PAYE computerization, the government provided a £200 million loan guarantee to bail out ICL, allowing the project to progress, albeit with a different management structure (p. 163). Government support for contractors, or assuming what would otherwise be considered a contractor's risk in a project, appears to be one of the only factors identified in this study that is particular to major projects.

The majority of factors identified by Morris and Hough are also applicable to smaller projects. This is acknowledged by the authors, who comment "*...most of the findings are also relevant to the management of projects in general*" (p. 211). There is nothing in this work to suggest that the purpose was to identify factors that distinguished major projects from smaller projects. The purpose appears to be to learn about the process of project work, using major projects as the focus of analysis. In this, the work may be very successful. However, it is also not surprising that many of the findings are applicable to projects in general. This general relevance speaks to our earlier observation that while the work is cited quite widely, it is often in references that do not make explicit reference to the management of megaprojects.

The verdict on this criterion is not positive. With respect to the third criteria, it is unclear whether this work has had a strong influence in the formation of

megaproject management research as a distinct area of enquiry. Although it makes many interesting observations about the management of major projects, as most of these observations are also applicable to smaller projects, it is unlikely that the work has played a strong role in the development of an identify for megaproject research that is distinct from general research into project management.

Criterion 4: Personal impact, depth and insight: Consideration of whether a work is a classic relates as much to the elegance of the writing as the pertinence of the findings. Morris and Hough's work focuses on a factual and descriptive retelling of the events that occurred in the projects considered. It is unclear whether it is the kind of work that would provide fresh insight if read again a decade later; the clarity of prose in Morris and Hough's work ensures that its full meaning is apparent at first reading. In other words, the text does not display deep and engaging complexity— it makes its points clearly and concisely.

The significance of any research work needs to be seen in the context of contemporary research. At the time of Morris and Hough's (1987) publication, very little research had been conducted into the practical aspects of project management. Research tended to focus on abstracted process, with little direct enquiry into the specific actions and context that shaped a project and led to one outcome instead of another. In this context, Morris and Hough's (1987) work was innovative, bringing greater emphasis to bear on the lived experience of projects as opposed to idealised norms. In this respect their work has played a significant role in shaping project management more generally. With respect to whether their work contains deep insight into the management of megaprojects or creates a synthesis

that carries innovation into the field, the answer is qualified. Their work can be described as offering findings of interest and relevance both to project management and megaproject management but, according to these criteria, it should not be considered a classic of megaproject management research.

Book 2: *Industrial Megaprojects: Concepts, Strategies and Practices for Success* by Edward W. Merrow

Merrow's (2011) *Industrial Megaprojects* will be reviewed in this section, and evaluated against the criteria to understand whether it can be considered a classic of megaproject research. The work places emphasis on making business decisions and making the right project decisions before committing to a megaproject. This suggests that the work is written with the project owner in mind, rather than an academic audience. Implementation issues receive less emphasis in the book, being treated only in the latter chapters of the book.

Merrow acknowledges the contribution made by Miller and Lessard (2000) to the megaproject field. These authors dealt with the settlement or shaping of projects and decision-making, to which almost three chapters of their book are devoted. In comparing his work with Morris and Hough (1987), Merrow concurs with their disappointment concerning the poor success rate of large projects. Merrow also acknowledges the work by Flyvbjerg et al (2003) as a major contribution to the megaproject literature. He states that he shares some of the conclusions reached in Flyvbjerg et al (2003) but explains differences between the two books foci in terms of Flyvbjerg et al (2003) focusing on large public sector funded infrastructure projects

(p. 20) whereas Merrow's focus is on private sector megaprojects. He further explains that while projects in the two sectors share some common pathologies, public sector (and defence) projects are 'frequently beset by a phenomenon known as "buy-in and hook" in which low costs are promised early, knowing full well that eventual costs will be much higher. Although this deception is not unknown in private sector ventures, it is not very common, simply because there is usually no taxpayer available to foot the bill later' (p. 20).

Merrow (2011) accesses a database of more than 318 projects, predominantly in the areas of Oil & Gas Production, Petroleum Processing and Refining, Minerals and Metals, Chemical, LNG, Power Generation and Pipelines. While it is not clear whether the data supporting this work has been subjected to independent and scholarly peer review, Merrow goes into considerable detail about the data collection and analysis methods, giving the reader reasonable confidence in the validity of the findings. The work fills a gap in the literature, complementing Flyvbjerg et al's (2003) focus on public sector funded megaprojects.

Criterion 1 & 2: Overall impact of the work and impact over time: At the time of writing, the book had been cited over 90 times, since 2011 (Figure 4). From the citation rate it is obvious that the research community has begun to take notice of Merrow (2011). The number of citations has consistently increased since its publication in 2011. The primary source of citations are project management journals. Other journals related to planning and defence have also begun to

acknowledge the work. It is also important to note that prominent project management research references this work (e.g. Mancini et al, 2015; Brookes & Locatelli, 2015; Brookes et al, 2015; Atkinson, 2015; Mišić & Radujković, 2015; Flyvbjerg, 2014). The overall trend of citations and the recognition being awarded to this work in project management journals as well as by project management researchers suggest its impact will rise over time.

Figure 2 here

Criterion 3: Formation of a distinct discipline: A closer reading of the book provided a better understanding of how it has contributed to the discussion of megaprojects. The book starts by introducing seven critical mistakes that can cause problems in megaprojects related to strategy, money and people. Most of these points can also be found in the general project management literature, such as schedule pressure, adequate decision making, upfront planning and shaping, relationship between constraints and appropriate risk allocation. These issues are not megaproject specific. Some points of more specific interest include allocation of a project's potential value to provide a stable foundation for its execution, a discussion that goes further than the literature on value management (e.g. Thiry 1997). Another important point suggests that project commissioners should avoid making the project manager a scapegoat for failure; many things can go wrong and the project management cannot be held accountable for many of these.

The two most valuable aspects in Merrow (2011) are the views on shaping strategy, and the importance placed on teams in megaprojects. Shaping had earlier been dealt

with extensively in Miller & Lessard's (2000) work on large engineering projects. Merrow's (2011) innovation was the discussion of a country advance team doing a reconnaissance of the environment in which megaprojects will be carried out, considering the previous history of projects in the area, paying attention to local content, taking into consideration religious and cultural context and evaluating local labour availability. These aspects are likely to be found in the literature on development projects but emphasising their importance is of considerable value to the megaproject field (van Marrewijk, 2015).

Considerable attention is also devoted to teams, with the point of difference from the project management literature being the emphasis on 'owner teams'. While the project management literature has started emphasising the role of the project owner or sponsor (Bryde 2008; Andersen 2012) the role of the whole team is rarely discussed. The role of the team becomes especially important in megaprojects due to the complexities involved, requiring multiple experts to make strategic decisions at the front end, not necessarily in easy decision alignment with each other.

Proposed alternative organizational models for teams, such as the hub and space or organic models, might be useful in structuring the multiple types of teams involved in megaprojects.

In terms of megaproject classics the book's primary audience would seem to be owners or sponsors of megaproject while covering details other audiences might appreciate as well. It is very much a practitioners' text – there is little in the way of theoretical or empirical innovation in the text, despite it being well documented.

While shaping is an important feature of the book it is not a breakthrough topic:

previous authors wrote about shaping (Miller & Lessard 2000, Williams & Samset 2000 and Edkins, Geraldi, Morris & Smith 2013). Contracting, governance, risk and success had also been discussed previously in project management literature. The work on complex projects in these areas is also applicable to megaprojects (Pryke & Smyth 2006; Zheng, Roehrich, & Lewis, 2008 and Thamhain 2013).

Criterion 4: Personal impact, depth and insight: It is too soon to evaluate the impact, depth and insight of the book, as, at the time of writing, it had only recently been published. It is safe to assume that its impact has not yet reached its peak and that the book's reception will develop further. If one wanted to cover the field of megaprojects comprehensively by considering both the public and private sectors one would find it necessary to consider this text. One of the important features of the book is its reliance on data and the guidance provided for evaluating failure based on evidence. Another important aspect highlights the impact of safety, which is usually only discussed in the construction management literature. Safety becomes extremely important in the megaproject sector, especially where hazardous materials are present, a focus of many of the cases in the book. The work is an interesting complement to other research in the field, but according to the criteria established in this paper, could not yet be considered a classic in the field.

Book 3: *Megaproject and Risk: An Anatomy of Ambition*, by Flyvbjerg, Bruzelius and Rothengatter

This section reviews Flyvbjerg et al's (2003) book *Megaproject and Risk: An Anatomy of Ambition*. The work provides a detailed examination of the phenomenon of

megaprojects and their underlying problems seen from the perspective of risk management. The book uses three case studies of recently completed road and rail crossings in Europe: the Eurotunnel connecting England and France, the Øresund Link connecting Sweden and Denmark and the Great Belt Link connecting Denmark with continental Europe. The book shows that conventional practices of megaproject management do not take into account the unique challenges that attend their planning, design, construction and operation. A fourth case study, a crossing project between Denmark and Germany, that is still in the planning stages, is used to demonstrate how solutions proposed by the authors can help to increase the success rate of megaprojects. In addition, Flyvbjerg et al's (2003) work uses data from other large infrastructure projects in the United States, Europe and elsewhere to support their argument.

Inattention to risk or poor risk management in general, it is argued, in combination with a lack of accountability, creates a toxic environment of "appraisal optimism" that leads to inaccurate estimates while poor implementation of projects leads to high failure rates and large cost overruns (Flyvbjerg et al, 2003, p. 73). These are the main problems with which the authors deal and in response to which they offer multiple strategies. First, they argue that risk management, especially accountability, should be a more prominent aspect of the decision-making and governance processes in megaprojects. Second, megaprojects require better institutional arrangements in which either the decision-makers carry the risks of the decisions made or the risk takers make important decisions. Both scenarios would create incentives to produce more responsible decision-making, as there would be a clear

accountability between decisions made and risks taken. Lastly, transparency should be used as a tool to manage and enforce accountability of decision-makers. For instance, the assumed role of government is to represent and protect the public interest. Transparency in that case means the public has the possibility to verify this assumption at all times.

Criterion 1: overall impact of the work: As outlined above, the first, and simplest, criterion to assess a classic relates to the influence or impact of the work, which in an academic context is measured by the number of times a work has been cited.

Megaproject and Risk: An Anatomy of Ambition is the one of the most cited works in the field of megaprojects or megaproject management, with 2292 citations (www.google scholar.com, accessed 28 September 2016). Despite the fact that books and book citations have long been neglected in bibliometric analyses in comparison to academic journal articles (Torres-Salinas et al, 2014), a citation count is a clear indication of the influence and impact a work has on a particular field and topic. From that perspective Flyvbjerg et al's (2003) book is in a strong position to be considered a classic work in megaproject management, especially in comparison with other texts in the field.

Criterion 2: impact over time: The second criterion essentially deals with the longevity of the impact and influence of a book on a particular field. The long-term significance of citations of *Megaproject and Risk: An Anatomy of Ambition* have been steadily increasing since first publication in 2003 (please see figure 3). Starting with 11 citations in the first year, the book has been constantly cited more than 200 times

per year for the last 5-6 years, which is a good indication of the lasting impact the work has made on the field of project management.

Figure 3 here

Criterion 3: Formation of a distinct discipline: The third criterion we consider in this paper relates to the way in which classics serve to shape a discipline, providing morphological unity to an otherwise disparate group, affording a common focus, concepts or language with which to develop a discipline, and set the context within which future developments can be built. To shape a discipline, a classic must therefore (re)define how the field is perceived. Put simply, it must be the (or one of the) book(s) that comes to mind when thinking about the field of megaprojects.

Kuhn (2012) highlights that scientific evolution is necessarily based on past codified scientific achievements accepted by a relevant community of practice as the theoretically paradigmatic foundation of a field. Classic books are often read initially to gain a good understanding of the particular field, Kuhn suggests. These classical works help to introduce concepts and attract adherents from competing modes of scientific activity. They become nodal points in the creation of actions networks linking an invisible college of scholars because of their new or unprecedented concepts or particular theories.

How does Flyvbjerg et al's (2003) book score in relation to Kuhn's (2012) description of classics? *Megaproject and Risk: An Anatomy of Ambition* does not introduce many new or unprecedented concepts or explain individual theories in a different way.

According to the authors, three features are systematically mismanaged in

megaprojects, namely uncertainty about facts, high-decision stakes and values in dispute. Risk assessment, essential to dealing with these factors, is usually absent or inadequate (Coates, 2004). None of these propositions or findings is particularly new for either management or project management.

We now look at a few specific arguments proposed in the book in more detail and review their novelty and impact. One of the first topics the book discusses is the relationship between cost estimates and project performance. More precisely, Flyvbjerg et al's (2003) argue that poor estimations lead to unsuccessful projects, especially in regards to cost overruns, an insight that is not new to the field of project management. As Atkinson (1999; 2006) demonstrated, many standard projects fail to meet this criterion long before the rise of megaprojects. In particular, Atkinson (1999) argues that it is poor planning, inaccurate estimating and lack of control that leads to cost overruns. Moreover, the PMBOK (PMI, 2013) describes cost estimating as a core process in the planning phase of a project and therefore regards it as a vital component in delivering a successful project. Poor execution of this process must therefore necessarily lead to poor project performance.

Another argument put forward in *Megaproject and Risk: An Anatomy of Ambition* is that people's underlying political or personal agendas drive woeful estimates, such as those of overall project cost or the usage rate of a particular piece of infrastructure (i.e. a bridge or tunnel). Given these estimates, an information asymmetry between two parties in which one party possesses more information than another (Forsythe et al, 2015) enables exploitation of the project situation once work is commenced. While this concept is absolutely relevant, it is, however, not

new to the field either of management or project management. It is merely a particular specification of the Principal-Agent problem outlined by agency theory. Agency theory reminds us that much of organisational life is based in general on practitioners or stakeholders acting in self-interest to serve particular purposes (Eisenhardt, 1989). In particular, agency theory contributes to understanding how information can be used as a commodity in an organisational settings, which Flyvbjerg et al (2003) describe in their book as the 'bias of optimism' discernible when enrolling support for a project.

The authors argue that estimates and thus risk management are highly influenced by the optimism bias of project managers (Flyvbjerg et al, 2002; Flyvbjerg et al, 2004). Flyvbjerg et al (2003) use the term 'ambition' to describe this behaviour. While this argument is valid, important and relevant it is not a new finding, neither in the specific fields of project and megaproject management (e.g. Flyvbjerg et al, 2002; Raz and Michael, 2001) nor in the organisational literature (Heaton, 2002; March and Shapira, 1987). This type of managerial behaviour also finds support in large parts of the psychology and decision-making literature (Kahneman and Lovallo, 1993; Kahneman and Tversky, 1979). As Heaton shows, managers are optimistic (i.e. ambitious) when they *"systematically overestimate the probability of good firm performance and underestimate the probability of bad firm performance"* (2002, p. 33).

Two factors contribute to optimistic behaviour: First, managers believe that they have control over the organisation's performance (March and Shapira, 1987) with people generally being optimistic about things they think they can control. The

notion of certainty and being able to control the outcome when strictly following certain processes is embedded in traditional project management thought (Cicmil et al, 2006). Hence, it is not surprising that project managers are optimistic about their ability to deliver a successful project.

Second, high degrees of commitment towards particular projects or pieces of work escalate optimism. The escalation of commitment (Staw 1981) is a well-known phenomenon such that, as Heaton (2002) outlines, *“people are more optimistic about outcomes to which they are highly committed”* (2002, p. 33). Commitment to delivering a successful outcome can have a variety of different drivers, such as people’s financial investment, their professional reputation, or employability (e.g. Gilson, 1989). As such, the more is at stake for the particular individual, the more committed they are and the more optimistic they are that the project will perform well. As an example, if a contractor sees a chance significantly to improve its reputation by successfully delivering a project, the company is more likely to engage in the project or have a higher interest in the project. Quite similarly, if a politician can produce a major piece of infrastructure that will change the face of a city in their period of candidature, the underlying drive for electoral success might make them more likely to sign the relevant documents. In both cases, the relationship between personal risk and reward plays a key role. Conversely, where a project is not successful, where it appears to be failing, this can also lead to enhanced commitment. In Staw’s (1981) terms, there can be an escalation of commitment to failing projects, as those responsible for them seek not to be associated with failure.

March and Shapira (1987) outline that “managers recognise both the necessity and the excitement of risk taking in management, but they report that risk taking in organizations is sustained more by personal than by organizational incentives” (1987, p. 1408). The self-interested nature of the agent is demonstrated (Jensen and Meckling, 1976), where the agent may or may not behave as agreed with the principal (i.e. in the best interest of the organisation), which explains the notion of managerial ambition or optimism within principal-agency theory.

These are just some examples of the points put forward in *Megaproject and Risk: An Anatomy of Ambition* that indicate that not all of the arguments made in the book can be classified as novel or substantially different to what the organisational or project management literature had already produced. However, the book achieves something far more important than producing (and testing) individual hypotheses. The authors have managed to provide a bigger picture of how different, existing theories and concepts belong together, using megaprojects as a platform for doing so. In the next section we will explain further what we mean by this statement.

As outlined earlier, one of the characteristics of a classic is that it provides a unifying force that brings together a field, constituting morphology, establishing a basis on which others can build. We argue that *Megaproject and Risk: An Anatomy of Ambition* does exactly that for the field of megaprojects. The numbers of concepts that have been used and introduced in the book are vast. It encompasses and combines many different organisational concepts that have previously been

discussed largely in isolation from each other, such as (project) governance, risk management, transparency, decision-making, privatisation and accountability.

Megaproject and Risk: An Anatomy of Ambition delivers a monumental and unprecedented effort combining these different concepts into one coherent framework, thus providing a bigger picture of how megaprojects operate (which can to a certain extent even be applied to organisations and ‘normal’ projects). We argue that this is the true value of Flyvbjerg et al’s (2003) work.

Providing a coherent framework for a bigger picture helps a field to develop common grounds and establish formal problems that are sufficiently open-ended to leave a variety of new problems for researchers to resolve (Kuhn, 2012). One of the main problems of many academic or scientific disciplines is the amount of knowledge that is produced during the course of establishing a distinct field. The knowledge produced is often large in quantity and not always coherent; often it can make competing claims (Bredillet, 2010). The development and establishment of the field might therefore become stalled due to internal conflicts and debates that arise as a by-product of such knowledge overload. A bigger picture can serve as a valuable solution to this issue, as it requires taking a step back to gain perspective (Bosch et al, 2007). Often this allows us to look at things from a different perspective and by doing so identify opportunities for innovation or areas where more knowledge is required (Chrissley, 2012). However, it is not sufficient to add this step of capturing the bigger picture at the end or doing it in isolation; it must be *“an integral mechanism in which to explore and analyse a complex problem in a holistic way”* (Bosch et al, 2007, p. 230).

Academic or scientific fields that offer new insights and provide a bigger picture with relevant practical examples can be described as ‘paradigms’ (Kuhn, 2012). Paradigms provide models, frameworks and open-ended problems that a groups of practitioners from within the field and other fields aims to resolve. We argue that *Megaproject and Risk: An Anatomy of Ambition* is a work that has made an early and formative contribution to a growing stream of research on megaprojects. More precisely, Flyvbjerg et al’s (2003) book has certainly attracted an enduring group of followers away from standard project management. One indication of this impact can be seen in the number of publications on megaprojects in academic journals. A search of the Scopus database was conducted with ‘megaproject’ as the search term. The number of publications resulting from this search increased significantly from 2003 (Figure 4).

Figure 4 here

The years 2002 and 2003 showed only 22 and 25 articles on megaprojects, respectively. This number went up to 40 articles in 2004, 52 in 2005, and 115 articles in 2015. The articles published in the field of megaproject management increased by a factor of over 4 in the first ten years after publication of Flyvbjerg et al (2003). The main finding of this Scopus analysis, however, is that the field stagnated in the years leading up to 2003, hardly existing, with an average of only about 10 publications per annum in the years leading up to it.

Criterion 4: Personal impact, depth and insight: The fourth criterion addresses the personal impact that a work has on the reader. A classical book is not only a book

that offers significant depths, new insights, or a bigger picture of a particular topic. A classical book is written with a complexity that makes the reader re-read it, as each time there is a sense of discovery of new concepts, problems, or ideas that the reader failed to pick up earlier (Calvino 2000). This means that the reader – whether agreeing or disagreeing with the arguments put forward – keeps thinking about the book. One cannot be indifferent to the work. Some factors contribute to a book achieving this. One factor, and probably the most important one, is the way in which the book has been written. *Megaproject and Risk: An Anatomy of Ambition* is written in a clear and logical way that is provocative to read rather than being rambling, simplistic or poorly structured academically because its main focus is on the practitioner with the expectation that the the reader will expect to find it easy to follow the line of argument (Olson, 1996). Indeed, on a number of occasions it has been engaged with in various ways (e.g. van Marrewijk, Clegg, Pitsis and Veenswijk, 2008; van Marrewijk, Ybema, Smits, Clegg, and Pitsis, *forthcoming*).

Having praised the book for its academic engagement this is not to say that this is its only audience. A final aspect that makes *Megaproject and Risk: An Anatomy of Ambition* stand out from the crowd is that Flyvbjerg et al (2003) found a way to combine academic rigour with practical applicability. They were able to express the underlying ideas, solutions, and arguments in a way that allows a large spectrum of readers to engage in the subsequent discussion around the book. As such, it delivers an outcome that many academics aim for; it bridges the gap between theory and practice. Moreover, Flyvbjerg et al (2003) reignited the term ‘megaproject’, making it a widely used term, not only in academic circles but also in the broader industry.

Discussion

Any book considered worthwhile on the criteria we have outlined will be subject to criticism; indeed, criticism is the harbinger of praise. Those things that are not worthy of critique rank far below those that are so distinguished. While we have praised *Megaproject and Risk: An Anatomy of Ambition* the politicality of projects is, perhaps surprisingly, in view of Flyvbjerg (1998), not rendered in this work in as conceptually a cohesive manner as one might expect. It is not just that there are 'political sublimes' and other 'sublimes' (Flyvbjerg, 2014) at work at the outset. Megaprojects involve multiple competencies characterised by specific rationalities, such that talk, decisions and actions will not necessarily be aligned, in an all too familiar organisational politics (Clegg, 1989). In Brunsson's (2002) terms, getting megaprojects off the ground and keeping them going, presents ample opportunity for participants to make claims about qualities or convictions they do not necessarily have, leading to the organisation of hypocrisy. Facing demands for certainty while confronting much that is unknowable and undecidable may well make hypocrisy the norm. There is evidence to suggest that this is the case in the frequent failure of megaprojects to achieve those espoused goals that are used to enrol and enlist initial support.

Flyvbjerg et al's (2003) ideas of project rationality and deceptive behaviours suggest that projects routinely exceed estimates of their risk in terms of costs, completion, and other performance indicators because those associated with their commissioning and implementation use deceptive indicators and misleading projections, resulting in the misallocation of scarce resources (Flyvbjerg et al, 2003).

If this were the case it would implicate a whole profession of project management, as well as all the ancillary professions associated with it, in action nets that government ministers and their public service advisers, as well as merchant bankers and shareholders, create either through duplicity or, at the very least, lack of knowledge or even stupidity.

Given duplicitous or stupid projections, project managers will not infallibly fix them – they are, after all, human. Implicitly, arguments that see the failures of megaprojects as residing in a lack of realism that deliberately misleads stakeholders about the true costs and complexity of the projects assumes a norm in which large-scale organisations are characterised by rational behaviours. In projects that are not as organizationally complex, ambiguous, ambitious, political and risky, the façades of rationality may be easier to maintain. The complexity and ambiguity of megaprojects can make the maintenance of these rationality façades much more difficult.

In megaprojects, everyday managers and engineers work to create some sense in contexts containing a multiplicity of different and variable rationalities and cultures. They draw on contractual documents, BIM modelling and other materialities with variable interpretations, incomplete data and many opportunities for gaps to arise between talk, actions and decisions. This amplifies the potential for ‘breaks’ to occur and for ‘fixes’ to be more ad hoc and indeterminate in terms of schedule, costs and design variables (the break-fix problem as outlined by Flyvbjerg (2014)). If the problems of megaprojects reside in an inability to maintain a façade of rationality that disciplines projects from the start and fixes them when they break, if ruptures to the fabric of rationality are inherent to what project managers do, perhaps a

closer approximation between the disciplines of project management and currents in organisation studies, represented by scholars in actor-network-theory (Law, 1992) would be useful. Such ethnographies would entail that a picture of the actor networks associated with more and less successful megaprojects might be developed over time through thick descriptions gleaned from close and prolonged encounters with megaproject realities.

Translation is a core concept of actor-network theory. Translation refers to the fact that in a social world of meaning and collaboration, conflict and communication, projects are always in process, being interpreted in different indexical contexts, from different positions of interest, making sensemaking inherently ‘political’—hence, projects are always being translated. Translation necessarily entails transformation in which interests are continuously being identified, attracted and transformed (Czarniaskwa and Sevón, 2005). Megaprojects owe their being to their ‘assembly’ by actors *and* actants—both human and non-human—to form evolving actor-networks. With a complex multiplicity of others involved, megaprojects may best be treated as complex and mechanical cultures of solidarity that are fragile in construction and easily sundered (van Marrewijk, 2015). Hence, the focus should be on the means of assembly and the action nets involved, and a future classic must include those ideas to address some of the field’s most prominent questions.

Conclusion

Three substantive and influential works on megaprojects were reviewed and evaluated in terms of whether they could be considered classics of megaproject

management research. *The Anatomy of Major Projects by Morris and Hough (1987)* is a clearly influential work. This is evident in the total citations the work has received, the steady flow of these citations, as well as the innovative nature of the publication's focus. Read in the context of the time at which it was published their work represented a significant change in how research into project management was conducted, shifting from a focus on abstracted process to enquiry into the lived experience of managing projects. However, few of the sources that cite this work focus specifically on megaprojects, casting doubt on how influential this work has been in setting the boundaries of megaproject management research. Many of the findings in this research are equally applicable to all projects, not specifically major (or mega-) projects. Although the work could possibly be considered a classic of general project management research, it should not be considered a classic in megaproject management research.

Morrow's (2011) *Industrial Megaprojects: Concepts, Strategies and Practices for Success* has probably had insufficient time to make a conclusive assessment of its impact on the field on the basis of citations alone. Nonetheless, the evidence for conclusions in the work is based on 385 private sector projects, suggesting the possibility for a significant contribution to our understanding of how megaprojects are managed. Clear guidance on how to measure the success of megaprojects is one very practical contribution. However, we conclude that while it makes a significant contribution to megaproject research, based on the criteria established it falls short of being a classic in the field.

Of the works analysed, the greatest claim for the status of a classic of megaproject management research is made by Flyvbjerg, Bruzelius and Rothengatter's (2003) *Megaproject and Risk: An Anatomy of Ambition*. The sustained high citation rate for the work indicates a persistent impact upon the field. Although the work may not introduce many new ideas or concepts, the way in which the different and often previously unaligned theoretical concepts were combined, tested and analysed provides a rigorous framework for many of the underlying issues encountered in megaprojects. The clarity of the arguments and their expression makes this book a masterpiece in the field of project management, prized for its relevance, theoretical synthesis and accessibility. Even over a decade later, the issues are still relevant, which underlines the longitudinal impact of the work. Nonetheless, in view of Flyvbjerg's (1989) work and our discussion of ethnography informed by actor network theory, there is a need still to address the internal politicality of megaprojects through real-time research.

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Figures

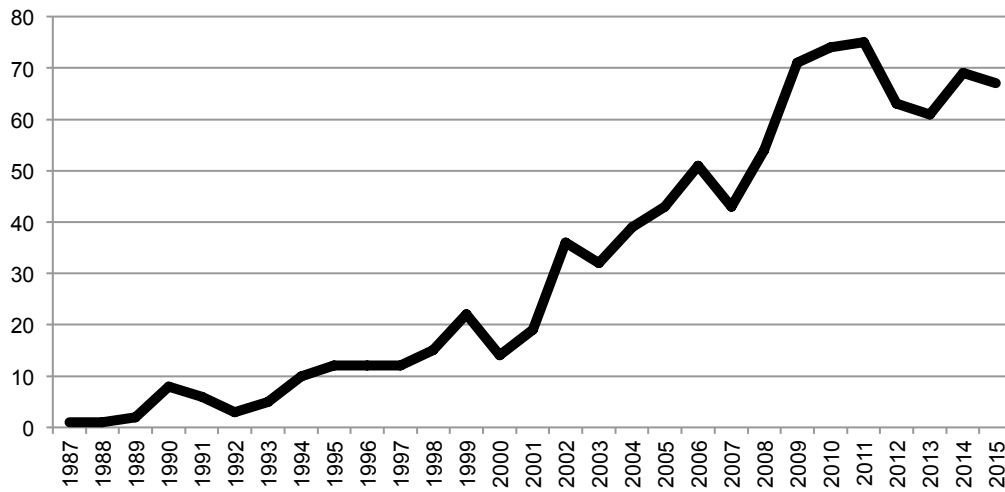


Figure 1: Citations per year since publication of Morris and Hough (1987)

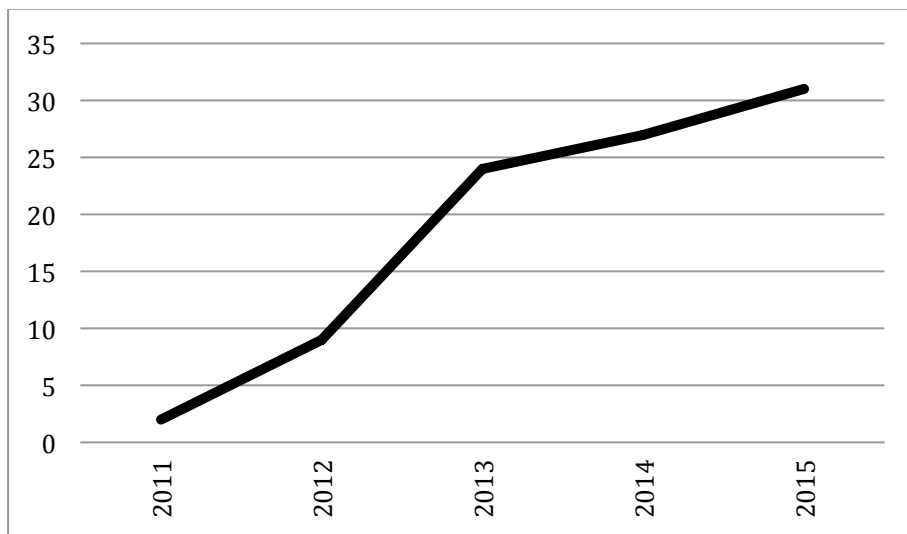


Figure 2: Citations per year since publication of Merrow (2011)

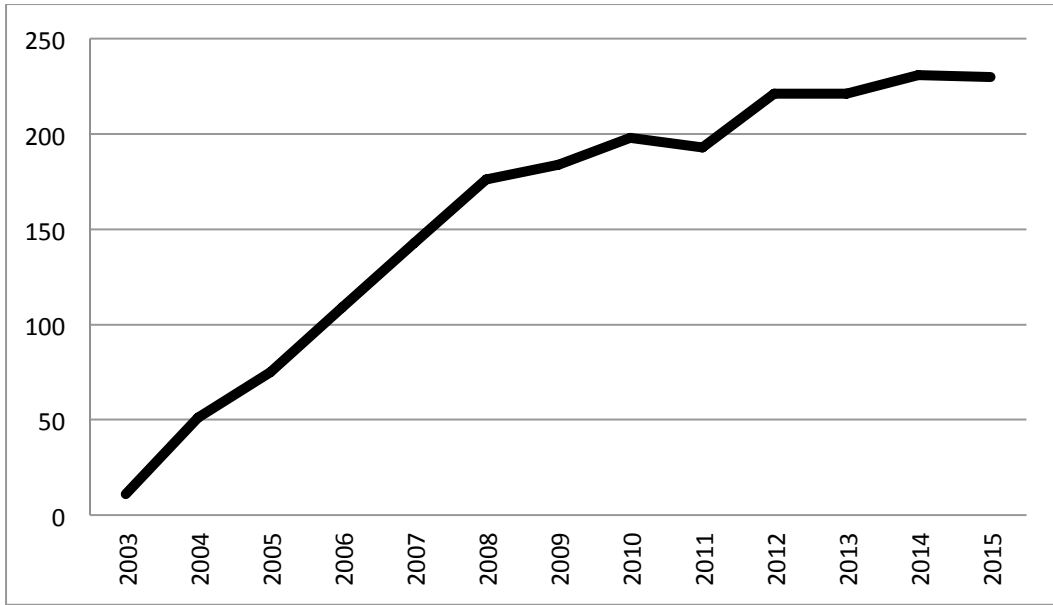


Figure 3: Citations per year since publication of Flyvbjerg et al (2003)

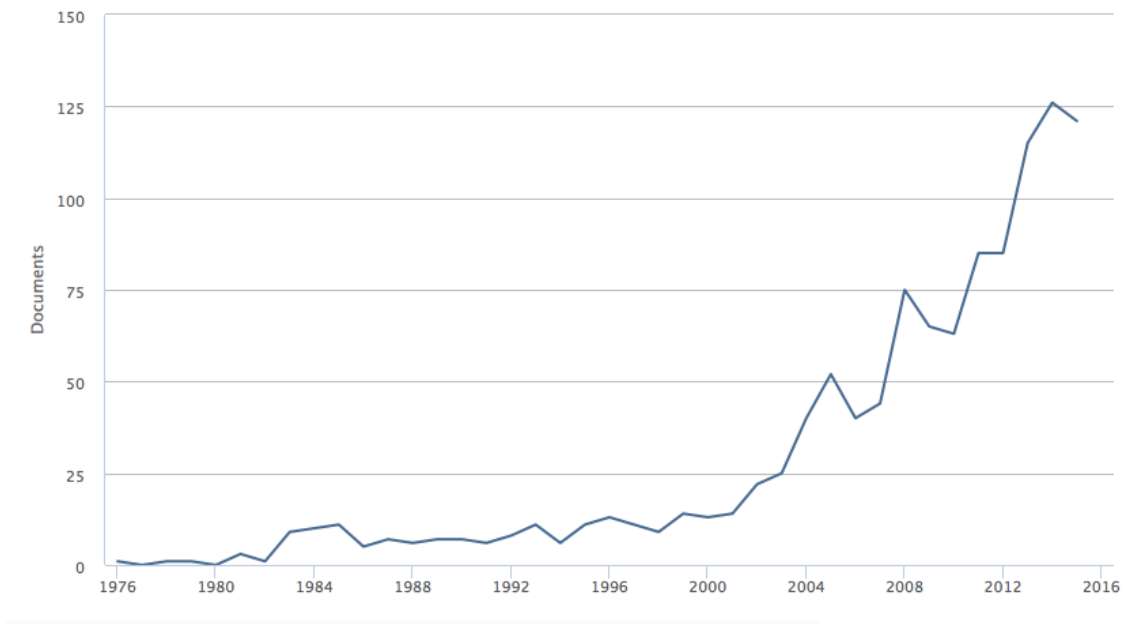


Figure 4: Megaprojects publications based on Scopus search results