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Essay

Why clear managerial recommendations matter in major projects research: Searching for relevance in practice

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1. Introduction

This essay celebrates Peter Morris' legacy, one of the pioneers of project management research and practice. Peter developed a successful career combining the publication of seminal books, scientific articles, higher education teaching, and the application of such research in practice through consultancy and advisory projects. I'm honoured with the opportunity to reflect on Peter's work and how his contributions inspired, and will keep inspiring, generations of researchers exploring the management of major and megaprojects.

Peter explored several times throughout his career the advances and shortcomings of the project management discipline (Morris, 2002, 2013, 2016; Morris, Crawford, Hodgson, Shepherd & Thomas, 2006), elegantly challenging the way the profession was developing with its normative emphasis and lack of wider strategic connections, from parent organisations to institutional actors. However, this essay is concerned with the specifics of major project management research. This is our departure point, from the project management discipline as a domain, into the world of major and megaprojects. Major and megaprojects are inherently different from regular/smaller projects (Denicol, Davies & Krystallis, 2020; Flyvbjerg, 2017; Morris & Hough, 1987), the analogy of a license to drive a car or a jumbo jet is often used to illustrate the managerial skills required from project managers (Flyvbjerg, 2014). As argued by Peter and several scholars, context is important (Morris, 2013).

This essay is a celebration of Peter's work and as such aims to follow a critical view raising questions and reflections about our role as researchers and the connections with the practice of managing major projects. It is designed to be constructively provocative, potentially remove the readers from their comfort zone and trigger different reflections about major projects research in terms of relevance, impact, practice, and, most importantly, the scale of the challenge ahead of us. Above all, it is inspired by Peter's research and strengthens a call for action to clearly provide the actions that might impact practice. Peter often used a simple expression to challenge and move us forward in direction of specifying the contribution: 'So What?' In essence, Peter inspired us to think on how the new knowledge is relevant, why do we need it, and who is going to use it? In a recent reflection, Pinto (2022) precisely identified the potential dangers in a lack of practical utility as one of the signs of 'the inflection point' of project management research.

Inspired by Peter's legacy, I argue that research on the management of major projects needs to reflect on the direction of travel, on the balance between theory and practice, and how the next 40 years of research might contribute to some of the most alarming transformations of our times, at scale (e.g. climate change, digital revolution). I argue to move further the relevance and impact-driven agendas, emphasising the transfer of our scientific work to advance the practice of managing major projects. This question reflects the current emphasis of project management research on theorising and the connections with a plurality of theories from other communities, which often provide insightful articles. The keyword here is balance. Such a strong theoretical emphasis, usually interesting and thought-provoking, might be constraining the space for the articulation of the managerial recommendations of the research findings. The overwhelming perception across the hierarchical lines of industrial partners is that they cannot use the research findings to address their own challenges. Variations of the expression 'that's too

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theoretical for us' are often mentioned, implying irrelevance to their organisations and major projects. So, what is relevant research in the management major projects in the 21st Century?

2. The evolution of major project management research

Peter charted the evolution of project management throughout his career, from its origins in the systems thinking in the 1950s where a plan and control approach was favoured, which continued towards the 1960s, 70 and 80s, through to a more contemporary connection with organisation theory. Peter argued (1973) that the project management discipline is an applied profession with the body of publications strongly connected with organisational theory, building upon the influential work of names such as Galbraith (1973), and Lawrence and Lorsch (1967). Recent expansions to incorporate other theoretical lenses were welcomed (Morris, 2013), moving us toward the pluralistic exploration of the field (Söderlund, 2004).

The hugely influential book with George Hough in 1987, The Anatomy of Major Projects, examined several projects following the same structure to uncover some of the problems and structural features (Morris & Hough, 1987). The book inspired further work from a variety of scholars and organisations around the world, significantly expanding the management debate regarding major projects. The concept of the *Strong Owner*, for instance, influenced insightful pieces since 1987 (e.g. Winch, 2014; Winch & Leiringer, 2016), which helped us to evolve our thinking and connected well with challenges faced by practitioners. We have recently built upon the different roles of the owner, sponsor, and client to frame the discussions with practitioners of six London's megaprojects, which guided us to develop the organisational architecture of megaprojects (Denicol, Davies & Pryke, 2021).

Peter subsequentially evolved the major projects discussion in his 1994 book, The Management of Projects (Morris, 1994), introducing his central framework to argue for the amplification of the discipline to broader theoretical lenses (Pinto & Winch, 2016). Peter argued several times that the discipline should move away from its traditional execution focus to incorporate wider and more strategic topics, often found in the initial phases of the project, where he emphasised the front-end (Denicol et al., 2020; Edkins, Geraldi, Morris & Smith, 2013; Williams, Vo, Samset & Edkins, 2019) in connection with the Management of Projects framework (Morris, 1994). More recently, Morris, Pinto, and Söderlund conceptualised the three waves of project management, in the influential Oxford Handbook of Project Management (Morris, Pinto & Söderlund, 2011). The third wave is of particular interest to the challenges of major projects, where projects are thought in connection with the parent organisation and the institutional environment. Peter's work on major projects benefited from other broader explorations throughout his career. The work sponsored by the Project Management Institute (PMI) from corporate to project strategy (Morris & Jamieson, 2005), is highly relevant in the context of new entities emerging to deliver megaprojects (Denicol et al., 2021). The research on institutional level (Morris & Geraldi, 2011) could not be more relevant in terms of the external pressures and different external stakeholders involved in different stages of the lifecycle of a megaproject. In summary, major project management research evolved with strong contributions to practice, providing scientific guidance to address the challenges faced by practitioners (Morris, 2013). However, the current direction of research emphasising theoretical contributions over practical ones is concerning and needs to change, if we are to transform the management of major and megaprojects of future generations.

3. The future of major and megaproject management research – relevance in practice

Peter had the leadership and reputational gravitas to question the fundamentals and was a champion to move the major project management research forward. I would argue that the power of his research was deeply connected with its application in practice, theoretically framing the empirical domain, but never at the expense of losing the relevance to the ones managing major projects. This should be unegotiable, as it is a rigorous theoretical framing.

As insightfully argued by Meredith (2022), how can we maintain the trust of our industrial partners if the knowledge being produced, after asking for and using their time, is not relevant to them? Meredith found that, after a certain point, the managers were not responding to the surveys anymore, delegating it to others (e.g. administrative assistants). Reflecting on this dynamic brings significant implications to the development of future research, the quality of our findings, and the implementation of our scientific work on the management of major projects across the world.

Peter was a critic of developing theoretical work that is disconnected from practice, arguing that academics, influenced by a variety of factors including the institutional system (e.g. academic metrics, rankings), might risk being considered irrelevant and disconnected from the real world challenges of managing major projects. Peter wrote the sentence below in 2010, based on a keynote address given at the International Project Management Association (IPMA) 23rd World Congress held in Helsinki in 2009, but it could well have been written in 2022.

"Too much of our research and writing is boring and remote from real-world, urgent, important issues. We tend to operate at a sub-level of process, people and technology enablers, too often playing self-referentially with abstract terms. Too little of our research is really focused on society's problems (And we have plenty of major ones). Projects are vehicles for achieving change, for addressing needs. But how often does our research reach beyond process, people and technology and work on the way we tackle the issues themselves? The domain, and our work in it, should be practically relevant and useful." (Morris, 2010).

Peter argued for an engaged scholarship approach between academia and industry, where academics would be more involved in practice through multiple formats (Morris, 2010; Winter, Smith, Morris & Cicmil, 2006). However, knowledge transfer by dissemination of findings is largely missing. The publication of a paper, even if open access, cannot be the end of the journey. Unfortunately, it seems to be according to the system of academic incentives, metrics, and assessment frameworks in many countries. The recent emphasis in the UK towards an impact-driven agenda is a refreshing development and encourages us to think more ambitiously about the future. The culture of academics developing research in their ivory tower and not engaging with society is unsustainable for the next decades.

In this essay, I build upon and emphasise the call that research on the management of major and megaprojects needs to be relevant to the managers of such endeavours (and more ambitiously *exclusively* to them, as the shift in performance is urgent given the scale of the infrastructure gap). The amplification of debates beyond the academic circles is needed, welcomed, and has significant potential to impact practices (Blomquist, Hällgren, Nilsson & Söderholm, 2010; Pinto, 2022). Practitioners need to use the scientific content produced, in which the role of theory is central and of paramount importance, as insightfully argued by Svejvig (2021). However, perhaps a more balanced approach would be appropriate rather than the recent exponential obsession for theorising. This is not an argument for the lack of theory or less rigour, evidently. It is a claim that the development or advancement of a particular theory for the sake of it, seems disconnected from reality and not enough for changing the management of major projects in practice, which is a significant concern if we are to achieve societal impact, at scale. Research in such a format, concerned with the expansion of a theoretical lens at the expense of practical implications, will continue to be largely invisible to the eyes and conversations of major project managers and policy makers at influential forums such as the United Nations, European Commission, and the OECD.

Considering the scale of the business of major and megaprojects, in terms of the number of future projects and the associated poor performance (Denicol et al., 2020), we need to start a movement towards the proposition of solutions. Industrial partners are actively looking for science to move their practices and business forward, academic advice informed by rigorous scientific work. For that to happen, we need to better articulate the managerial recommendations that our research is suggesting, with a central theoretical role but for the purpose of changing the world of major projects, not for the sake of just advancing theory or creating a new terminology for future self-reference. I would like to think that we are the scientists not only understanding, documenting, and describing the world of major projects, but actively drawing upon this rich knowledge to design solutions to tackle the challenges. The proposition of solutions, regardless how incremental or disruptive, is crucial to connect with the multiple challenges and practical realities of the management of major projects. This might trigger and be conducted through different collaboration formats with our industrial partners. Here there is a missing component and significant opportunity for major project researchers. I suggest that future research should actively involve practitioners to capture their reflections and advice on the practical utility of the research (Meredith, 2022; Pinto, 2022). Practitioners could be formally included as independent reviewers of *relevance in practice*. This process, if done with rigour throughout the research life cycle, might provide the evidence to support a shift in the narrative of our scientific work towards a solutions-driven agenda.

There is a strong appetite from industry to hear the findings of rigorous research projects that might give them a competitive edge (Morris, 2013), improving their practices to better deliver the major projects that they are passionate about. To enable such knowledge transfer, academics need to articulate the managerial recommendations, in a direct and unambiguous manner, addressing the needs of the audience that is consuming that knowledge (and simultaneously managing major projects). We need to focus on the impact beyond the pages of a paper, the application of the meaningful theoretical debates developed, the implementation of the core ideas of the research to change the status quo of major projects. To make that happen, it is our job (and duty!) as major and megaproject management researchers to clearly articulate what needs to be done to implement our findings, how, when, and by whom. This will strengthen the bridge between academia and practice, build trust between researchers and practitioners, and increase the awareness of the potential outputs and outcomes of following the path and solutions suggested by scientific research.

Inspired by the Association for Project Management (APM)'s research summaries and insights from other journals (e.g. Strategic Management Journal), I suggest, as managerial recommendation, that Project Management journals could adopt a Managerial Summary section, which is often available to a wider audience together with the abstract. Abstracts are open access and increasingly tend to focus on the articulation of research contributions, missing a large market by not engaging with practitioners. This could perhaps be a way of engaging more substantially with different markets, amplifying the readership of our journals, the awareness of scientific findings, and consequently stimulating knowledge transfer, which is urgently needed in major and megaprojects.

Inspired by this reflection on Peter's legacy, I would constructively provoke and challenge the current and future generations of major and megaproject researchers to strengthen the emphasis on the 'So What' questions. In particular, the ones regarding the anatomy of the specific recommendations to improve practices and contribute to better deliver major projects.

(So) What are the managerial recommendations derived from your scientific work that should be implemented to improve the management of major and megaprojects?

Declaration of Competing Interest

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