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The project is dead. Long live the project

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1. Introduction

IJPM has recently examined the prominent research themes over its 40 year publication history (Slavinski et al., 2023). The main themes identified by the bibliometric analysis were performance management, risk and construction management, project governance, project planning and control and project success. Each of these themes has been explored by researchers for decades. While the earlier years were dominated by risk and construction management (1994–2015) as well as project planning and control (1983–2015), more recent years have seen the conversation shift towards performance management (1991–2022), project governance (1995–2022) and project success (1997–2021). Whilst the bibliometric analysis as a method could be criticised, it does give us some valuable insights: Looking at the results and the five high-level themes could suggest that the development in our field has been slow focusing on very specific streams over long periods of time. Whilst there are developments within and across each of the themes, the overall focus appears to remain fairly stable without generating new significant insights. This could lead to the argument that our discipline, and with it the project, is dead.

However, these findings are not necessarily a reason for concern, since an alternative interpretation of the results could suggest that the nuances within each of the themes are actually significant enough to continuously move our field forward and therefore keeping the project alive. Whichever interpretation we choose, we need to regularly reflect as a research field if we are asking the right questions and focusing on the right problems. And inevitably, this leads to the question: Are we relevant? Do we provide a service to society? Looking at the outcomes of this bibliometric analysis, one could argue that our field might not be at the forefront of cutting edge developments. Are we missing in action whilst society is facing challenging tensions in all areas (economic, political, technical, social and environmental)? The answer is probably yes

and no – we as a research field can and should certainly do better, but there also have been some important developments.

I was invited to reflect on the results of the bibliometric analysis mentioned above and share my thoughts on where we as a research field might go next. With this essay I would like to experiment, test boundaries and question assumptions, i.e. I will follow an alternative route of enquiry. Following Suddaby (2019), I will take a normative stance to explore where I believe our field should move next and not necessarily where it will move next or where it has been moving towards. By doing so, I acknowledge that this might not lead to success as “the essay offers no guaranteed road to success – if anything, failure may be its most common destination.” (Gabriel, 2016, p. 246). Of course, the question of where a whole research field should go in the future is vast and there is not one single right answer to this as I will explore below. With the field of project management research maturing, the breadth of issues investigated is increasing and I will only be able to touch upon a limited number of aspects. Hence, I use this essay in its capacity to “allow us to explore different forms of knowing” which is necessary to enhance the relevance of (project) management research (Suddaby, 2019, p. 446).

2. Where might we go in the future

Project management research has been organised into different schools of thought over the years (Turner et al., 2013; Bredillet, 2004; Söderlund, 2004) to take a more systematic view on the trends and progress of research in the field. These schools of thought relate to different fields of general management studies and organise project management research along established disciplines with projects mostly being the research context. More recently, this has spread out into different areas of focus and analysis such as project studies (Gerald & Söderlund, 2018), project organising (Winch, 2014) or project behaviour (Unterhitzberger, 2021). The present bibliometric review is

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another attempt to categorise project management research without actually paying justice to its breadth and depth. Adopting the logic of the bibliometric review we might see a very limited number of additional key themes emerging over the next decades such as sustainability (Sabini et al., 2019) or sustainability transitions (Winch et al., 2023) and the use and impact of new technologies such as AI and machine learning (Steen et al., 2022). Or we might see a growth in areas which are currently trending such as innovations, strategic change, collaboration and resilience (Slavinski et al., 2023). This is the obvious answer to the question where our field might go and probably also the most likely outcome.

3. Where we should go in the future

However, as mentioned above, I intend to take a normative stance for this essay and rather than discussing where we might go, I'd like to explore an alternative route of enquiry on where I believe our field should go. This is against the backdrop of raising our profile as a source of cross-fertilisation with other fields in addressing the challenging tensions our world is facing across all aspects of society.

3.1. A theory of the project

First, I believe that we should establish a theory of the project. There have been various attempts in the past to establish a theory of project management (Meredith, 2002; Turner, 2006d, 2006c, 2006b; Turner, 2006a). However, we are still lacking answers – and sometimes even debates – on some fundamental questions surrounding the project: (1) Existence: Why do projects emerge and exist? What explains the existence of a project? (2) Boundaries: What determines and explains the boundaries of a project? (3) Financing: What determine the project's capital structure? (4) Organisation: Why is a project structured in a specific way? What is the role of project management in terms of the project objectives? What are the rules for decision making? (5) Heterogeneity of actions/performances: What drives different actions and performances of projects?

By viewing the project as an economic entity in terms of a theory of the project – similar to the theory of the firm (Holmstrom & Tirole, 1989) – we shift the focus of attention from the pure management aspect in project management research which historically addresses aspects such risk management or performance management to interrelated issues. These interrelated issues require the integration of complex contextual factors as well as the integration of the issues themselves. Whilst the initial response to the five questions posited above might be that we already know much about this, I argue that a theory of the project view will enable us to get a more in depth understanding of the actual workings of a project. Rather than, for instance, identifying critical success factors – of which we have hundreds now in the literature – we need to understand what is good project performance and how is it developed. Rather than e.g. investigating how decisions are made in projects, we need to understand how the social, economic and organisational systems within a project shape decision making. We should (and I might argue we need to) investigate the project rather than using it just as a research context. We should investigate the project as an economic entity and develop a theory of the project.

It is often claimed that the organisation by projects is the way to address and manage the challenges our world or society is facing (see e.g. Lundin et al. 2015). It would seem essential to understand the underlying forces behind the dynamics of and within projects to inform business decisions and policy making in relation to projects. This, I believe, is a very strong reason for studying projects (rather than project management). Another reason for studying projects and developing a theory of the project is that it will make our field more accessible and relevant to other disciplines. Academic research thrives on cross-fertilisation and the development of a theory of the project will enable other disciplines to access the breadth and richness of work that

is happening in our field.

3.2. Responsible project research

Second, I believe we should do research differently. Project management researchers are facing increasing pressures to publish more and more academic journal papers – as is the wider field of business and management researchers. When shortlisting for academic positions at my university I frequently encounter CVs of individuals which have over 25 publications in an 18 month period and I used to ask myself: How do they do it? What am I doing wrong? Why am I not as productive as they are? But I am now re-phrasing my questions and re-focussing my efforts. I do not believe we should strive for ever more research outputs which make the tiniest of contributions just to get the next promotion. I believe we need to do research responsibly. But what does this mean?

A group of leading business and management academics from across the world have come together to established the Responsible Research in Business and Management (RRBM) network. Their vision is to support responsible science and the production of credible knowledge that is ultimately useful for addressing problems important to business and society (RRBM, 2017, revised 2020). Whilst there are not (yet?) any high profile cases of paper retractions in our field, with ever increasing publishing pressures and the rise of AI it is more likely that research is becoming less responsible. By adopting the principles of RRBM our field can grow: not necessarily in terms of numbers, but in terms of quality and relevance.

RRBM (RRBM, 2017, revised 2020) advocates that (1) research needs to provide a service to society and therefore create knowledge that is beneficial to organisations and the society overall to create a better world; (2) stakeholders (incl. editors, funders, university leaders) need to value both basic and applied contributions to knowledge to enhance the research's utility for all stakeholders; (3) stakeholders (incl. editors, funders, university leaders) need to value plurality and multi-disciplinarity in research themes, methods or types of enquiry to do justice to the complex challenges our world is experiencing; (4) the adoption of sound methodologies needs to be encouraged to enhance rigour in qualitative and quantitative work and to discourage problematic practices such as data slicing; (5) stakeholders should be involved throughout the research process whilst also maintaining academic integrity and independence; (6) research should have an impact on stakeholders rather than the journal publication as the end goal and therefore contribute to a better world and finally, (7) broad dissemination needs to be valued through e.g. open access, open source or online publishing to reach beyond the traditional academic audience.

Adopting the RRBM principles will enable our field to enhance quality, visibility and relevance of our research and ultimately, this will contribute to our field being more actively involved in cross-fertilisation with other domains in business and management, engineering or other disciplines to address the multifaceted challenges we are facing.

3.3. Sectoral and geographical diversity

And third, I believe we should expand the geographical and industrial context of our work. Our field is strongly grounded in the western tradition with authors from Europe, North America and Australia having shaped much of our thinking. Whilst there has been a rise of authors from China more recently, the topics being investigated build mainly on the western tradition with only a small number of studies considering unique contextual concepts such as guanxi (Li et al., 2021). Areas such as South America, Africa or large parts of Asia are vastly underrepresented as a research context despite huge activities in projects. Additionally, much of the research is still conducted in construction, infrastructure, engineering or IT contexts, with industries or sectors such as fashion, pharma or international development mostly neglected. Project management researchers need to collaborate with academics from different geographical regions and different sectors and disciplines to remain

relevant to the developments in the real world.

4. Conclusion

Whilst the prominent research themes over the past 40 years in IJPM suggest much continuity and consistency, the next 40 years might look very different. With this essay, I've put forward three alternative routes of enquiry in terms of where our field should go in the future. First, that we should establish a theory of the project, second, that we should adopt the principles for responsible research and third, that we should expand the geographical and sectoral context of our work. I believe, that if we take these directions our field will become stronger. We need to ensure that project management research is continuing to advance theoretically and methodologically in order to keep it relevant and therefore alive. Because the project is not dead. Long live the project!

Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

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