Post Publication Review

Review

Journal ranking systems are impacting academics in research-intensive universities worldwide. The paper by Coulthard and Keller contributes to existing knowledge in the area by empirically investigating the effects of journal ranking systems on research and the welfare of researchers, specifically in the discipline of information systems. Using chi-square tests and descriptive analyses on survey responses from a sample of 275 attendees of information systems conferences, the paper concludes that journal ranking systems have mixed impacts on research and the welfare of researchers. More specifically, in terms of research, the study finds that though journal ranking systems have been perceived as increasing publication quality (positive impact), such systems have also been perceived as inhibiting innovative, risky research and encouraging safe, conforming mainstream orthodoxy, which result in a lack of diverse, innovative, and ground breaking research (negative impacts). In terms of the welfare of researchers, the study finds that journal ranking systems contribute to the pressure to publish and to publication anxiety, especially in instances when time for research is insufficient and when regional, cross-disciplinary and qualitative research are threatened by publication bias by journals and its editors (negative impacts); no positive impacts of such systems on the welfare of researchers were reported.

Notwithstanding the noteworthy empirical contributions from the study by Coulthard and Keller, this review identifies three substantive issues that may be of interest for future research.

First, the state of theoretical contribution in the existing literature resulting from journal ranking systems needs to be framed and discussed more clearly and objectively. For example, the use of the technology acceptance model as a theoretical lens in information systems research is characterized as a form of orthodox, incremental research, which is considered undesirable by Coulthard and Keller as well as several other scholars (Benbasat and Barki, 2007; Grover and Lyytinen, 2015). However, proponents of the theory may have an alternative and justifiable view of its use and value—e.g. the relevance and value of technology acceptance studies are predicated on its ability to identify avenues for attitudinal and behavioural change to encourage greater acceptance of myriad technologies among diverse communities. It is important to note that technology acceptance studies today are not limited to basic, conventional concepts, such as perceived ease of use and perceived usefulness. Instead, noteworthy efforts are being made to extend (e.g. through theoretical integration; Lim, 2015) or apply (e.g. in experimental settings; Teh et al., in press) the technology acceptance model meaningfully. This is part and parcel of theoretical extension (i.e. a type of theoretical contribution accepted by top journals), and thus its importance should not be downplayed. More importantly, it is necessary to acknowledge that not all research will start from scratch or will challenge existing theory (i.e. other types of theoretical contribution accepted by top journals). A good way to put this understanding in perspective is that the existing literature, especially in top journals, has been largely shaped by the former but not the latter two types of theoretical contribution, and thus, greater visibility of the latter types of theoretical contribution may be encouraged.

Second, a realist perspective on research and journal ranking systems suggests that it may not be realistic to expect all research investigations to be highly innovative and ground breaking. Any potential attempt to pressure top journals to only progress manuscripts into the review process on the basis of such ideals will likely produce adverse impacts, such as greater pressure and anxiety among researchers. The case of the senior editor of an A* publication that rarely receives, let alone publishes, innovative exciting research, despite the many editorials he wrote or the associate editors he appointed to solicit highly innovative and ground breaking research

papers, is an excellent example by Coulthard and Keller that clearly shows that not all researchers are highly innovative and capable of producing ground breaking research. Indeed, it is easier to produce incremental contributions as compared to highly innovative, ground breaking contributions. Thus, it might be more realistic for researchers to focus on meeting criteria such as clarity (e.g. easy to understand, sharp and straight to the point), novelty (e.g. conceptual and practical originality), and rigor (e.g. search for and/or rule out alternative explanations, triangulation) in research investigation and reporting, rather than the degree of research contribution (e.g. incremental to ground breaking), when considering to publish in top journals. After all, academics are not superheroes who can solve world problems overnight. Instead, it is reasonable to say that the incremental contributions contributed by the global network of scholars in the field forms the extant contribution in totality, which may equate the impacts made by a single ground breaking research, which may rarely or never occur.

Third, academic support for the positive impacts of journal ranking systems on research as well as on researchers themselves is lacking or virtually non-existent. If we consider the fact that the absence of research ideals of being highly innovative and ground breaking is lamented by academics in the work of Coulthard and Keller (and if these academics insist to strictly follow and materialize these ideals), then further investigation into the negative impacts of journal ranking systems on research and the welfare of researchers are discouraged on the basis of these ideals, so as to avoid falling into the loop of incrementalism and orthodoxy. Instead, further research is highly encouraged to take up the challenge to use highly innovative methods to produce ground breaking insights into the perspective of researchers who have garnered benefits from and are in strong support of journal ranking systems. A possible way to do this may be to recruit academics who are in favour of journal ranking systems and subsequently conduct studies and use methods that triangulates the researched phenomenon in greater depth.

In short, the findings by Coulthard and Keller and the substantive issues raised in this review should contribute to greater understanding of the impacts and usage of journal ranking systems. More importantly, academics in research-intensive institutions need to acknowledge that academic success requires hard and smart work. In the contemporary era, where competition is becoming increasingly intense, academics need to form an entrepreneurial mindset (i.e. a mindset that thrives on curiosity, opportunities, innovation, and new value creation and that stimulates work behaviour beyond paid hours) instead of an employee mindset (i.e. a mindset bounded by a glass ceiling and that simulates work behaviour within paid hours). With an entrepreneurial mindset, stress will likely not be taken negatively, but rather more positively. This should help academics cope better with the pressure and anxiety that they often encounter in the global academic world that revolves around journal ranking systems. Such a mindset should also motivate greater exploration of the alternatives to journal ranking systems among opposing academics engaged in the pursuit of finding a more acceptable means of effectively and efficiently measuring, evaluating, and enhancing research performance of academics and universities.

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Author Response

We thank Dr Lim for his kind review. Within his review, he makes three interesting observations: (1) *that the contribution of theoretical work needs to be discussed more*

objectively. Dr Lim, using TAM as an example, suggests that incremental work does have utility. We accept that view but agree with Grover and Lyytinen (2015) that much of the research in IS, including TAM, is incrementalist with limited reach or impact; (2) *that most scientific work is incremental in nature and thus most research will be incremental*. We do not deny that most scientific work is incremental: "normal science" as Thomas Kuhn (1970) put it. Our fear, supported by our research, is that the journal ranking system is changing the nature of research by exacerbating incrementalism, further heightening a publish or perish culture, and to quote Powell and Woerndl (2008), ending 'important work'. The moot question is the degree to which this is occurring - as Dr Lim points out it is easier to produce incrementalist work. (3) *The discouragement of incremental work would lead to an impasse in research and thus further increase researcher anxiety.* This is only so if one accepts a binary view of research – that it is either incremental or it is innovative. We suggest that it is a matter of degree and motivation and the journal system seems to be weighted towards encouraging incrementalism. Something our respondent researchers are questioning.

References

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