Review of: "An Empirical Study of Goal Intentions and Monetary Compensation for Reviewers in Information Science"

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This article addresses a very relevant topic in science. It affects many aspects of science like science development and progress, theory building and testing, and also more practical production problems in science publishing and dissemination. Overall, I have a good impression of the value of the manuscript, since it provides empirical evidence on a difficult and complex subject to research empirically.

Overall, I also think that the paper is interesting and provides facts and results that are worth reading and discussing. I also think that there are issues that need to be addressed in the theory/conceptual part, the methods, data analysis, and conclusion/discussion sections. I have to say that there are several aspects of the paper that I found troublesome and some researchers may find "not perfect," and therefore a major concern for its publication. My approach is a little bit different. First, I value the empirical evidence provided, which is difficult to obtain and valuable to share. Second, I think that there are areas in the paper that can be rewritten or clarified in order to provide a clearer, more interesting manuscript, providing a neutral presentation of the data and hinting to the reader some of the concerns, limitations, and difficulties in the data recollection and validity, so they can assess and make their own conclusions.

In that regard, I have the following comments that, in my understanding, may be taken into account to achieve those goals.

1. Introduction, Goals and Conceptual arguments. I believe that it might be possible to separate/organize the elements in the Intro section and the beginning of the 2. Data and Method section so readers can better follow the argument. It seems important to have a brief discussion of the literature on reviewers' motivations, incentives, and performance, and how this paper fits into that literature, and what it is trying to accomplish. The question that is suggested for the paper to answer, "What motivates reviewers in Information Science?", did not follow clearly from the intro or the lit review. Why is it important? What do we know or don't know about this? Is it important for Information Science… and is that a particularity of this research? Why… I also think that the explanation of Figure 1 is more related to section 2. In the last paragraph, the authors indicate that to address the research question, they performed an experimental study, but I think this classification may be revised (data collection involves an online survey, no control and treatment groups, randomization, manipulation checks, as the classic elements of an experimental design suggest).

2. Method. There are several issues here. First, there are some organization issues. There are some "hypotheses" that

are implicitly presented within the data analysis and presentation, e.g., (2.2.2., "Therefore, the underlying hypothesis is that, in a competitive deadline scenario, reviewers may overvalue immediate rewards over future benefits and intentions). I would prefer to have all conceptual discussion and framework before going into the method. This separation may help assess the quality of the conceptual argument, and then the validity of the evidence provided. Second, it will be important to indicate how the realized sample (193) is representative of the 4400 "population. You indicate that no demographic information was gathered, but maybe you have information regarding IPs, or maybe you can check other sample characteristics like the responses of first respondents do not change much compared to the last respondents, thus suggesting that the "sample respondents" are similar to "non-respondents". Some information regarding the validity of the survey questions is always beneficial (why and how these questionnaire items selected).

3. Data Analysis and Presentation, Inferences. I would try to synthesize this section, highlighting the main results (I believe the tables and analysis of reviewer goal intentions and compensations), so readers can go to the main results or evidence in an easier way. Other results can be reported in a less detailed fashion, or provided in the appendix, or stated within the section, but not in a way that makes the main results opaque. Limitations in the method need to be considered all throughout the analysis and discussion in order to avoid that readers disregard results for method or other reasons.

4. Conclusions, Discussions, Limitations, Value and Length. As I indicated upfront, I value the communication of this research evidence, but I suggest that the paper addresses the previous issues and, in a general sense, provides a clear and more succinct message of the main contribution or findings or provides in the most honest and neutral way. Also, I believe that you need to explain how these results inform the reviewers' motivation/performance literature, and how it is important for Information Science, the discipline, journals, schools, and academicians.