Dear Sir.

In our 2013 JASIS&T-paper entitled “The ripple effect: Citation chain reactions of a Nobel Prize” (Frandsen & Nicolaisen, 2013) we explored the possible citation chain reactions of a Nobel Prize using the mathematician Robert J. Aumann (who shared the 2005 Sveriges Riksbank Prize in Economic Sciences in Memory of Alfred Nobel with Thomas C. Shelling) as a case example. The results showed that the award of the Nobel Prize affected not only the citations to his work, but also affected the citations to the references in his scientific oeuvre. Farys and Wolbring (2017) present a rather harsh critique of the methods applied in our paper. They claim that we have failed to take changes in database coverage into account, and that instead of doing “a simple comparison of citations before and after the reception of the Nobel Prize”, we should have operated with “an adequate control group”. Farys and Wolbring (2017) has attempted to replicate our 2013-study using Web of Science subject categories as pools for selecting documents similar to the Aumann-documents, which then forms a control group. Using this approach they fail to be able to replicate our findings. Instead, they find that “there is neither a Nobel Prize effect on citation impact nor a related chain reaction in the citation network”. However, Farys and Wolbring (2017) misrepresent the methods applied by us. Moreover, their own method for determining a control group using WoS-categories has for quite some time been known to be highly problematic. Let us be a bit more specific:

First, Farys and Wolbring (2017) describe our dataset as follows: “Thomson Reuters added a new database, the Book Citation Index, to the WoS of Core Collection, on which Frandsen and...
Nicolaisen based their analysis”. As noted by Farys and Wolbring (2017), using the Book Citation Index would be highly problematic as it was published for the first time in 2005 - the same year as Aumann was awarded the Nobel Prize. This is self-evident, and we have of course not used the Book Citation Index for our 2013-study. Indeed, the methods section of our paper clearly stated:

*Citation data collection took place during the period May–June 2012 from WoS (excluding proceedings and book citations). Proceedings are excluded as they would, to a great extent, allow the same publication to be included in the analyses twice. Economics conference papers are typically later published as a journal article. Book citations are excluded as books in the citation indexes are published from 2005 and onwards and would bias the results* (Frandsen & Nicolaisen, 2013, p. 439).

Secondly, Farys and Wolbring (2017) misrepresent our method when they claim that we are using “a simple comparison of citations before and after the reception of the Nobel Prize” when in fact we are using a control group. This is clearly stated in the section entitled *Citation Impact Pre- and Post-Nobelist* where we clearly stated:

“The effect can, although, be a result of increased database coverage and an increase in the average number of references in the indexed publications. Consequently, we focus on the difference between the publications included in the scientific background published by the Swedish Nobel committee and the publications not included” (Frandsen & Nicolaisen, 2013, p 439).

Thus, our control group is formed by Aumann’s own publications as we are primarily looking for differences between his publications based on type of publication (book or article) and publications included in the scientific background published by the Swedish Nobel committee and the publications not included.

Thus, the choice of control groups differs in the two studies. Farys and Wolbring (2017) argue that a control group based on publication year, cumulative number of citations and WoS subject category is suitable. Consequently, they use WoS subject categories to form the basis for determining a similar publication in terms of discipline or field. They refer to Leydesdorff and Bornmann (2016) for an overview of limitations of defining fields as WoS subject categories. In fact, Leydesdorff and Bornmann (2016) conclude that normalizations using these categories might seriously harm the quality of the bibliometric analysis. In a previous study Leydesdorff (2008) explain that field delineation is an unresolved problem, and argues that the categories are notoriously flawed. Some of them overlap more than others, many journals are misplaced and the categories can be highly heterogeneous. Later, Leydesdorff (2015) even stated that “the WoS categories are not part of the solution, but part of the problem”. Consequently, the bibliometrics
community is searching for alternatives to the use of subject categories for field normalization (e.g. Colliander, 2015; Leydesdorff, Zhou & Bornmann, 2013).

Finally, Farys and Wolbring (2017) state that “it goes without saying that from a replication perspective it would be ideal to use exactly the same set of papers as the original study did”. We could not agree more. Aumann’s publications are per definition published and thus available to them and to everyone else who would want to try to replicate our findings. As described (Frandsen & Nicolaisen, 2013) we used the entire oeuvre by Aumann as our sample (as opposed to e.g. the convenience sample of Farys and Wolbring (2017)), and replication should therefore be straightforward to achieve. We would of course welcome such attempts of replication. Naturally, we acknowledge that replication studies are vital to science as it helps to make science a self-correcting system. However, when conducting a replication study, one should adequately inform about how the original study was conducted. If the methods of the original study for some reasons are found to be problematic, one should adequately explain why, and then do the replication using a more suitable procedure. The chosen path of Farys and Wolbring (2017) (misrepresenting the methods of our study, and using an alternative procedure that is known to be highly problematic) is not an adequate replication study.

References


