Review of: "Measuring researchers' success more fairly: going beyond the H-index"

Andrea De Montis[1]

1 University of Sassari

Potential competing interests: No potential competing interests to declare.

This is a very interesting article on a possible alternative metric with respect to and which could overcome the limitations of the well known H-index. This index is correctly supposed to have artificially inflated the performances of single authors. The author starts from some criticalities and proposes an alternative index based on the corresponding countermeasures. He refers mainly to the medical domain, although his reasoning can be extended to other scientific research fields. The article is well written and easy to follow and the argument and rationale sufficiently plains. Still, some integrations are needed. My comments are reported as follows.

General issues.

Any index has its flaws, as research activity is tight to measure; quantitative gauges can not give the complete picture of the complex activities carried on by colleagues in different institutional contexts. Measures are also connected to research policy and planning of each state or institution; their effectiveness should be considered with respect to the starting situation and the goal to be achieved. The activity of a single researcher in a well endowed laboratory of a developed country can not be assessed with the same metrics and thresholds used for the performance of a colleague working in a remote research institute of a developing country. This attains the roles, workload distribution, base research infrastructures, technical personnel involved, and ethics of each working environment.

The limitations of any index consist of the adaptation of the organizations to it, in disregard to the increase of the quality of research. Of course, this is malpractice; but still it comes along, as researchers know the rationale of the measure and behave to obtain first higher values of that measure, no matter the effective connection with the achievement of genuinely better results to be published in high-IF journals.

IF per se is a measure that has been subject to many criticisms; one for all: it relies on the number of citations, without any consideration of the nature of those citations ("positive" or "negative"). An article with 1,000 negative citations has the same impact of another article with 1,000 positive quotations! Again, the IF of article should be different from the IF of the journal where it is published; and this holds also for the H-index.

Authorship is a very embarrassing issue: can we have exact control an the correct attribution of an article to each of its (say) 2,000 co-authors? Can we be 100% sure that the minimal requirements have be respected by all the authors? I really do not think so. And what about the combined effect on the number of citations generated by those kind of monster papers? Career advancement incidental needs and bulimic chronic behaviour versus research grant competitions -when

not moderated by ethical reasons- are serious and worrying drivers! Possible solutions can be adopted, by indicating some threshold values discriminating individual versus group evaluation: the hypothetic work with 2,000 co-authors belonging to ten institutions can be accepted for evaluating the performance of those bodies and not for assessing the performance of the single author. Authorship per se is and will be jeopardized by recent AI advancements, which enable the automatic writing of papers.

Specific issues.

The integrations proposed revolve mostly on correcting co-authorship and quality of citations, even though this is not specified in the title. If the same title is maintained, it would be better to include other mentioned issues in the corrections. I refer to the effect of extra citations on the H-index, in the bullet list of the first section. Someone has proposed some power rule to consider the role of the citation above the strict threshold required to achieve that specific H-index (i.e., exactly 10 citations per each of the most cited 10 papers, to achieve H-index=10).

The example is interesting, but not fully clear: where is the difference (in the final quantitative value) between the new H-index and the old one for each of the five coauthors? In other words, the authors should better show the variation of H-index in the current and new formulation (with the introduction of the bonus). This may involve the connection of the new score to the number of citations. In this way, we can appreciate the effect of the correction for co-authorship. Otherwise, I believe we are not able to appreciate the difference in performance of the two indexes.

Bonus attribution rule should reflect the general behaviour of each research community. In some cases, the first two authors in list are considered two first authors, in some others the second authors has a lower attribution. I would introduce some thresholds for deflating moderately and severely the contribution of authors of monster papers (with more than n co-authors, with n depending on the scientific community). A paper with two (first) co-authors cannot be judged as a paper with 10,000 co-authors.