Review of: "How Do Academicians Publish More Research Papers for Their Promotion and Positions? A Scrutiny of CV"

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Potential competing interests: No potential competing interests to declare.

I have gladly read this short article on a transcendental issue for academic activity and higher education institutions. The hypertrophy of academic evaluation mechanisms, based almost exclusively on the number of publications and the sacralization of citation indexes, together with the structure and functioning of higher education institutions, are causing serious dysfunctions, harming the working conditions and the satisfaction of academics, and weakening the credibility of higher education institutions and academic work itself, let alone the waste of public and private resources invested in an academic activity that, apparently, has no other effect than that of enlarging the CVs of some scholars.

I encourage the author to continue his research. In the following, I will share with him some assessments and suggestions.

In my opinion, the article, despite its interest, is not a scientific paper, but rather an opinion piece. In fact, the research proposed by the author still requires more time and dedication to qualify for publication in a peer-reviewed scientific journal. In this sense, my advices to the author are:

1. A more complete review of the existing literature on the topics covered in the paper. Actually, these topics are quite numerous and the literature about them is very diverse (despite which, the author only includes four references in his paper). The following examples may be useful for the author (it is impossible to collect all the relevant references here):

On alienation and division of labor in the academic world, Harvie (2000) and Hey (2001).

On academic entrepreneurship and collaboration in academic work, Bozeman, Fay & Slade (2013).

On determinants of academic performance (in the field of research), Edgar & Geare (2013).

On the factors that lead to increased academic productivity (beyond malpractice): Kyvik & Aksnes (2015).

On plagiarism, Awasthi, S. (2019).

Reading this literature, I hope, will help the author to better determinate the objectives of his own research work.

2. A more complete exposition of the methods used in the gathering of empirical evidence and a more systematic exposition of the results obtained is needed. The author suggests in his paper that his conclusions are derived from the scrutiny of an indeterminate number of CVs. I do not want to question the soundness of his methods, but I think it would

be fair to inform his readers, at least, about the methods used to select those CVs, the number of CVs analyzed, where from the author obtained the information, the criteria used to identify "malpractices", or the indicators used to quantify their prevalence. You can find some inspiring papers based on the analysis of CVs (e.g., Macfarlane, 2020).

3. Finally —and despite the interest of the recommendations in the section "Necessary Academic Reforms"— I think that the author should express his conclusions in a more nuanced way. For example, the proposal of not considering legitimate authors those listed as second, third, etc., does not seem sufficiently justified, particularly if the author of the paper refers indifferently to all scientific fields. In some of these fields, collaboration in research is simply essential and the measure proposed by the author would not improve the incentives of those who participate in research groups. Also in other fields, such as the humanities or the social sciences —to which I suspect the author is referring—, there are cases where multiple signatures are (in my opinion) legitimate. Obviously, this is not true in all cases, but establishing a norm as restrictive as the one proposed by the author does not seem fair until it is possible to distinguish in which cases the second (or subsequent) authors participated in the research and in which they did not. Finally, the author should take into account that, if "the supervisors often compel them to include their name as the first author to gain more recognition", the measure he proposes would actually be denying any credit to those who he considers legitimate authors of the paper.

Apart from these three suggestions, I cannot ignore some specific flaws in the article:

1. Despite the fact that the author describes practices that cannot be ignored, some of his arguments are not as solid as they should be. For example, to demonstrate the impossibility of exceeding certain productivity thresholds, the author alludes to the time that the editorial process usually takes in scientific journals: "the peer-reviewed journals usually take a minimum of 90 days for peer review and six months for publication (if accepted) from the date of submission". In this way, the author neglects some facts: a) an author can simultaneously submit several papers to different journals; and b) there are more and more journals that follow a policy of shortening editorial processes precisely to attract the interest of researchers. Currently, the limit to the productivity of an academic is not found in the availability of forums for publication, but in his ability to generate new articles.

2. The author neglects some of the most pernicious forms of malpractice, such as plagiarism, in its different forms, or the "purchase" of publications in predatory journals. This type of practice contributes to saturating academic circuits with scientifically questionable material of little or no cultural and economic value.

3. The author does not take into account practices, quite widespread to date, such as including a manifesto of the roles and tasks assumed by each of the authors of a collective paper (see COPE standards).

4. The wording of the paper is sometimes confusing and the author's use of some expressions, such as "academicians" (as opposed to "scholars/researchers") or "registrar", should in my opinion be reviewed.

Finally, I cannot entirely agree with some statements by the author (which does not mean that they are incorrect, and even less that they cannot be rationally discussed):

1. The author recommends "instructing research scholars not to prepare research papers or engage in related activities

for their research supervisors". However, it is to be expected that researchers who are forced to let others sign the work they have done with their exclusive effort do not act that way for ignorance, but forced by circumstances.

2. The author also recommends "To identify the work done by a particular academician, they should seek approval from the head of the institution, such as the principal for colleges and registrar for universities, by submitting an abstract before submission". In my opinion, this suggestion would have consequences that are easy to imagine, but difficult to accept, on academic freedom and on the workload of those who undertake such censorship tasks.

3. The author states: "At the beginning of their research tenure, researchers dedicate their own money, time, health, and happiness to be actively involved in academic pursuits under the guidance of supervisors". This is true, and it is also true that some supervisors dedicate their knowledge, their experience and their enthusiasm to guiding and directing new researchers (not always very enthusiastic about their work), putting their (and their families') health and happiness at risk. In both affirmations there is some truth, but neither of them allows to gain insight into the phenomenon that is being analyzed.

I sincerely hope that my opinions will be helpful to the author and I am confident that he will be successful in his academic career.

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