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LETTER TO THE EDITOR

Is the Fever for High Impact a Disadvantage for Systematists?

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In the last decades, the eagerness of laboratories to publish in journals with high impact has turned into an obsessive practice, including several vices such as authorship traffic, which means offering high amounts of money for being an author or co-author of an article in high impact or "elite" journals where there has been no contribution from the offerer (e.g., Hvistendahl 2013) or other practices like including all team members in articles in order to increase the number of articles published by the laboratory staff. Therefore, the question arises whether these are unethical practices. For many researchers, the answer would be no; however, the practice to include the laboratory group in each article published has become very common in science, either to help students for their curriculum or simply to as a pay back to another one for being included before. A clear example is when competing for funds for research or study grants; nowadays, the competence to get funds in science has turned to be very tortuous in all the countries with some little exceptions [e.g., The US National Science Foundation (NSF) program for systematic biology called: Foundation Partnerships for Enhancing Expertise in Taxonomy (PEET)] (Rodman & Cody 2003, Rodman 2007). Besides, it is important to note that the gap existing between developed countries, those under development, and those not developed makes this competition even more complex (McDade *et al* 2011). The evaluation method of a project can be considered in different degrees:

1- If the researcher is working on cutting edge research, including latest or new technology compared to basic science (e.g., molecular genomics vs. taxonomy), the possibilities to get large funds for the latter ones are lesser every day; therefore, they have to go to the next step—"curriculum."
2- Curriculum: how do we evaluate curriculum when applying for a grant? (Bornmann & Daniel 2005). One of the main ways it is done is by the number of publications and their impact factor (Della Sala & Crawford 2007), and here, we go back to the beginning; if the researcher does not have a certain number of publications with an impact factor that is really worth, the possibilities to get a grant are very few, since he/she will be underqualified as a researcher. Generally, the impact factors considered in these evaluations vary between "3 and 12", but can reach up to 30 in some fields or even more in modern science areas.

The impact factor of a journal is a way to measure how frequently its articles are cited and it has been used as a quality measurement (e.g., Foley 2013), but what constitutes quality in science?

If the papers of a journal receive almost no citations within 2 years after being published, the impact factor of the journal will be low; hence, it will not be worth to be covered. Consequently, ISI selects what is worth to cover, so almost automatically the rest is considered as having no scientific impact at all (according to the criteria defined by ISI itself). It is somehow not surprising that Museums journals (the heart of taxonomic literature) do not possess an impact factor. The information they have was specified by the Zoological Record and by other bibliographic repertoires

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focused on taxonomy; therefore, their intentionally exclusion did not damage the scientific community regarding information availability (Boero 2010).

Is it really necessary to publish an elite article in order to receive an acknowledgement for one's efforts and the assessment of quality for one's work? What are the (unattended?) corollaries in the biological science fields that are not prone to produce these elite articles? Does it mean that these entire fields are not good enough to be published in the so-called high-impact journals? Valdecasas *et al* (2000) in his Nature Correspondence covering this topic have a popular phrase: "The popularity of the Science Citation Index (SCI) as a measure of 'good' science is damaging basic taxonomic work, without which the study of biodiversity would not be possible."

Despite the widespread use of impact factor, there are other possible models. Agnarsson & Kuntner (2007) have described the interesting example of the Slovenian Research Agency (ARRS). The ARRS has implemented a national database of all registered researchers (http://sicris.izum.si/) that uses a formula to calculate bibliographic points of researchers based on the number of publications in the past 5 years, the journal impact factors, the number of authors, etc. (from http://www.arrs.gov.si/sl/akti/prav-znanstrok-uspesn-06.asp). Thus, a publication counts 80 to 100 points if published in the top quartile within a field, 60 to 80 points in the second, 40 to 60 in the third, and 20 to 40 in the fourth quartile of journal impact factors. The lower bound points are augmented by the formula: 20×(IF−IFmin)/(IFmax−IFmin), where IF=impact factor; IFmin and IFmax=lowest and highest journal IF values within the quartile, respectively.

The total score is divided by the number of authors, and short papers (less than four pages) receive 80% score. For example, a single author of a paper longer than four pages published in some good zoological journal (IF 1.98) would receive, for this publication, a score of 80+[20×(1.98–1.407)/(5.286–1.407)]=82.95. The journal's IF is 1.98, and it falls into the top quartile of the journals within the field zoology (it top 15th out of 114); the highest ranking zoological journal IF is 5.286, and the lowest journal in the upper quartile IF is 1.407.

Applying the calculations, which are relatively similar in most countries, Agnarsson & Kuntner (2007) explain that when job candidates and employed scientists compete for national funds, they are compared based on their overall score using this or different approaches. For example, the minimum threshold for a principal investigator on grant applications is a score of 100 points based solely on the impact factor journal articles published during the last 5 years. Therefore, it is possible to have a higher score by just publishing a series of small publications during that period, in contrast to writing one massively interesting paper. Hence, if our hypothetical peetster would compete in this scheme, he/she would not be eligible for funds with a score of 82.95 (which could be obtained by publishing one large paper) but could comfortably compete with a score of 259.96 that could be achieved by publishing four smaller papers. There is no doubt that these metrics have some merit, but we believe that our academic assessment system requires urgent updating in order to reflect the nature of systematic biologists contributions to biodiversity research in the twenty-first century (McDade *et al* 2011).

Boero in his Diversity article published in 2010, explains that ISI, elaborated other indexes besides impact factor. For instance, the Cited Half Life (CHL) describes the amount of time that the average article of a journal is still cited. It is not unexpected that the majority of the journals with low CHL do have high IF, and vice-versa. The law of priority avoids any paper describing a species from being forgotten. In other words, its CHL is infinite (infinite in ISI terms is >10 years). The scientists that began to use the ISI standards to assess scientific performances ignored the CHL and employed exclusively the IF. Bizarrely, their tribunes had low CHL and high IF, so they favored what was positive to them, closing their eyes with respect to other indexes.

I have recently had the opportunity to visit the laboratories of some peer entomologists who have devoted 20–30 years of their lives to study biological taxonomy, with the intricate and complex, yet cumbersome, duty of classifying and identifying some organisms. Sometimes the accurate classification may take more than 5 years, and the resulting article e.g., with species descriptions is published in a journal with an impact factor between 0 and 1. Does this impact factor demonstrate that the article is of poor quality? (Callaham *et al* 2002, Jones 2003, Páll-Gergely 2014, Pendlebury 2009, Valdecasas 2011). Is there any realistic possibility for a taxonomist to publish in an elite journal? After all, if a species is not clearly described in a scientific article, it is not valid. Were the descriptions of the genera *Drosophila* or *Caenorhabditis* published in an elite journal? Clearly, the answer is no; nevertheless, currently, we witness the highest publication rate in elite journals with exactly these two model organisms. "*Basic taxonomic work is not highly cited, except in 'hot' taxa like the genus Homo*" (Valdecasas *et al* 2000). Endless examples like this one could illustrate the contrasting realities of different research lines within the biological sciences.

Nowadays, there is a high number of journals specialized on a diverse array of biological subjects, many of them free of charge for authors and with a good impact per article being consequently preferred by researchers. However, there are also other journals with very high impact per subject and

"fast" review process too, which makes them appealing for publication. Many of these journals charge costs ridiculously expensive and unaffordable (e.g., £1500 per article), restricting the possibility of researchers to publish in them. Then, we come again to a bazaar; does high impact mean business? Is it already a reality that science has become an attractive business for prestigious journal editors? Personally, I think these are questions that every researcher asks him/herself when it comes to publishing an article. Is he/she really interested in having his/her article read or rather to have it published quickly? Or with the highest possible impact factor?

In my personal opinion, to choose a medium-impact journal and not publishing within the elite ones does not make your work less important. The article is still the result of the combined efforts of the lab team, years of headaches in front of a microscope and/or a computer, endless hours spent in the field or in the laboratory, etc. I believe that science must be written for everybody and accessible to everyone.

Finally, to conclude the issues discussed in this note, I think that this subject could be solved if the way to classify impact per research line is reanalyzed. Although this is something that has been an issue for several years (Seglen 1997, Krell 2000, 2002, Valdecasas *et al* 2000, van der Velde 2001, Boero 2001, 2005, McDade *et al* 2011, Wägele *et al* 2011) with no results so far, it is an issue that should be revisited in order to make biological science equal when applying for project and research funds. If these regulatory standards for research lines were put into practice, the research lines such as systematics or taxonomy could have their own classification system (why not include Citation Half Life in ranking of selection?) (Boero 2010) and so compete at the same level against the various research lines when classifying project for revision. Unfortunately, this issue is dramatically affecting many research areas which, due to inequality issues, are losing work strength every year. I would like to finish with a question to be thought over: what would happen if there were no more systematics in science? Who would classify new species? Would cutting edge science solely work with the same model organisms, simply ignoring the existing natural diversity?

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