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The Expertise in sciences or is decided what is publishable: an endangered noble task

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" *L'expertise en sciences ou comment se décide ce qui est publiable : noblesse et dérives*"

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When Einstein discovers the anonymous peer-reviewing...

In 1935, a too quick calculation left Einstein to believe that gravitation waves could not exist in the frame of the general relativity (theses waves, observed in 2016 only, are actually a corner stone of his theory). The story of this publication is richer than the subtle error behind it...

For the first time, Einstein, who has sent his manuscript to the prestigious *Physical Review*, faced the system of anonymous peer review. The sharp-minded reviewer, whose identity was revealed only around 2005 [1], had pointed out an error. Einstein strongly disagreed with the idea that an Editor could pass his work, for a critical reading, without his consent. He then sent his manuscript to another review, who decided to publish it. But when it was time to check the proofs of the manuscript to be printed, he totally revised his paper. This story, both an exemplary and an exceptional one, illustrates the complex relationship between the scientists, and the publications.

The daily life of expertise in scientific publications

German journals in which Einstein used to publish, had a low rejection rate of submitted materials, and were instead open to controversies and scientific debates. Nevertheless, the immense growth of scientific activity has imposed to all scientific journals to follow the *Physical Review* standard, with one or two (even three sometimes) anonymous reviewers.

Rather than the Editor, in principle responsible for the selection of the publications, the reviewers are those who judge practically if the work is valid, and deserves the (rather subjective) recognition supposedly attached to a publication in a given review. Further criticism remains occasionally possible: "Comment" to a specific paper can be published (after approval by a refereeing process), possibly followed by an "Author reply".

Once contacted as a possible reviewer by the Editor, the scientist has a short time to accept this duty, He will have few weeks –sometimes even less- to deliver his informed opinion on the suitability of the manuscript. This free work remains anonymous (with some exceptions).

From my own experience, the time to produce the analysis and the critical report can vary from one hour to three days. The noble and exciting part of reviewing is that the process of critical reading and authors reply induces sometimes a kind of “co production” for the final content appearing in the journal: The reviewer often helps to an improved readability and more pedagogical presentation of the work, but sometimes the referee suggests broader openings not mentioned by the authors. The reviewer can also discover miscellaneous or more serious errors in the work, although they are sometimes not critical for the conclusion of the work, which is hence free of errors when published.

Finding good reviewers: the tough part of the Editor job

Finding an available reviewer is a difficult task for the Editor of a journal. Qualified specialists, with both expertise and a sufficiently large view of the domain are few. They are busy people, and will preferentially accept to spend time reviewing a manuscript when it brings apparently bright ideas, rather than evaluating the correctness of a standard incremental paper. Young scientists can be another target as they are susceptible to enjoy participating this peer-reviewing process, at the heart of the academic system they want to join.

One problem is the growth in the number of submitted manuscripts while the Editor cannot truly know (scientifically and for their ethics) the reviewers he has in his pool. This is the reason for various biases. It makes it usual that the authors are often encouraged, or even requested to propose a list of possible reviewers. For a good journal,

Peer Review	Reviewer	Request Status	Review Status
Peer Review	Reviewer 1	Waiting, Due (05 févr. 2017)	
	Reviewer 2	Waiting, Due (05 févr. 2017)	
	Reviewer 3	More Time Approved (02 févr. 2017)	Waiting, Due (16 avr. 2017)
	Reviewer 4	Too Busy (08 févr. 2017)	
	Reviewer 5	Too Busy (20 févr. 2017)	
	Reviewer 6	Too Busy (08 févr. 2017)	
	Reviewer 7	Too Busy (13 févr. 2017)	
	Reviewer 8	Too Busy (15 févr. 2017)	
	Reviewer 9	Not Area of Expertise (19 févr. 2017)	
	Reviewer 10	Not Area of Expertise (21 févr. 2017)	
	Reviewer 11	Agree (16 févr. 2017)	Waiting, Due (02 mars 2017)
	Reviewer 12	Too Busy (20 févr. 2017)	

Manuscript status, as found on the server of one of the top-five journals specialized in Optics, Concerns a manuscript that I had co-authored, which has appeared in April 2017 Finally, Reviewer 2 wrote a report, reviewer 1 never responded, and the acceptance was granted before reviewer 3 gave his opinion.

this can be a way to identify with accuracy the sub domain of the manuscript, and will make it easier to find recognized experts. It is clearly good when the original reviewer decline to report, because not close enough from the specialty, but is able to identify one or several experts, that The Editor would have probably not been able to identify. With lower quality journals, the Editor may decide to follow lazily the suggestions of the authors themselves, at the risk of being oriented to friends or to people from a same small community trying to exaggerate the importance of its own field. In worse but funny reported cases, the suggested referees are the authors themselves, who have provided an electronic alias, with the name of a pretended specialist.

The anonymous refereeing process can have its own drawbacks: as an urban legend, you can always be taught about someone whose paper was rejected, but whose idea is finally published by a colleague mentioned to the Editor among the specialists susceptible to produce a report. This problem is now minored with the development of specialized websites devoted to preprints. It remains common, but often quite transparent that the “anonymous” referee pleads for adding a reference which is nothing else that one of his own work.

Recognized journals, predatory ones, and other bad practices

The pressure to publish (the famous *Publish or Perish*), and the chance process in any expertise imply that any reasonable manuscript free of gross visible mistakes, will probably be published, even if rejected by several journals. This has not to be considered as abnormal, and rather explains how a hierarchy of journals can be established, with more or less severe criterions for novelty and originality of the research.

As a tribute of the vice in the virtue, and because world-wide on-line publication is truly cheap compared to the dissemination of printed materials, “predatory” journals [2] now appear claiming to be “peer-reviewed”. These novel journals, easy to recognize for true

RENCONTRE AVEC LE PÉNIS CONCEPTUEL

Dans une étude parue dans la très sérieuse revue Cogent Social Sciences, Jamie Lindsay et Peter Boyle estiment que le sexe masculin n'est pas un organe de reproduction naturel... mais plutôt le résultat d'une construction sociale.

DÉCOUVERTE SCIENTIFIQUE MAJEURE

J'AI ISOLÉ LE VIRUS DE LA CONNERIE
IL ÉTAIT LÀ, DÉJÀ!

Retracted article: The conceptual penis as a social construct

Authors (2)
Jamie Lindsay | Peter Boyle

Published: 19 May 2017, <https://doi.org/10.1080/23311886.2017.1330439> Check for updates

0 citations

Download Citation

A hoax around the «conceptual penis ». A *nonsense* paper published by Cogent and finally retracted. An information relayed by *Charlie Hebdo*, -in French- to explain peer-reviewing to a broad audience. [Charlie Hebdo and Cogent]

scientists, publish for a fee, any work claiming to be for a scientific audience, and provide vague “referee report”.

These bad practices appear because there is a considerable growth of higher education at a world level. In most cases, publishing in these predatory reviews will be billed to the University itself, and for faculty from peripheral institutions, publishing can be profitable in terms of recognition and career. Similar dubious practices about the refereeing process are also occasionally mentioned in humanities and social sciences [3], where nonsense texts respecting the appearance of the specialty [4], sometimes computer-generated, have been shown to pass the “refereeing process.”

Some tracks to improve

In conclusion, « peer-reviewing », is essential for science. However, the standard of publications with reviewers is affected by the development of on-line publication, and by the inflation of publications. Some tracks are worth being considered to improve the system [5] :

- Referee reports can be made available on-line, and this helps to understand the context of the work and of its publication, as any critics of a work, in art and in science as well. Such a practice is now experimented by some journals [6]. It is also a way to establish the quality of a journal, as being able to select appropriate and sharp referees.
- At the opposite of the anonymous refereeing process, some high-level reviews have considered a temporary electronic deposit, opened to critical comments by the peers, before deciding on the validation as a publication. This operates only with voluntary readers, and when the comments are favorable. Most probably, such a system can be operative only for top papers, susceptible to attract famous specialists as readers.
- The request of a traceability of successive submissions may limit the publication of too insignificant works. Presently, a manuscript rejected from a give review is sent free of criticism when submitted to another journal, except when the two journals are parents in a single Editorial group. It is rather natural and not a shame, when one improves his work by adding a more precise interpretation, or by suppressing some errors or dubious data or interpretation, sometimes as a consequence of a report concluding to rejection. As an author, I would be more confident in a fair evaluation if I could send to a second journal, along with a manuscript often improved after initial rejection, the comments received on the initial submission. Presently, I would have the feeling that I would be infringing the rights, or intellectual property, of the first journal and his referees, so that I refrain attaching this material.

[1] http://www.geology.cwu.edu/facstaff/lee/courses/g503/Einstein_review.pdf

[2] <http://www.nature.com/nature/journal/v544/n7651/full/544416b.html>

[3] <http://www.nature.com/news/peer-review-activists-push-psychology-journals-towards-open-data-1.21549>

[4] <http://zilsel.hypotheses.org/2548> -in French-

[5] <http://www.academie-sciences.fr/pdf/rapport/avis131216.pdf>

[6] <https://www.nature.com/articles/ncomms10277>