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Peer review in high-energy physics: a return to the origins?

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I still remember very clearly my first encounter with peer review: I was a Ph. D. student in physics and I had written my first paper, submitted it to a journal and – after what seemed to me a very long time – received a reply with the request for few changes and corrections I was supposed to include in my paper before it could be considered for publication.

These very simple steps: the writing up of some original research results in a paper, its submission to a journal and the process of the work being read and judged by someone reputed to be an expert in the field is what we call *peer review* – the judging of scientific work by your peers – and it is an essential part of what science is. No scientific achievement can be considered as such until has been recognized by the community at large and such a recognition mainly comes from the peer review process. The presence of this check has arguably helped and fostered the constant and cumulative growth of science.

The process itself has taken various forms at different times and for different research communities. Originally it was a real review by all the peers, in the sense that the entire community would judge the new work, embracing or rejecting it. Newton wrote and published its *Principia* without the book having to be reviewed before hand

by anyone. The book was circulated and read by the entire community of *natural philosophers*, which at the time was no larger than the average science department of a university today. Consensus on the value of the new work was reached among the peers and the new results quickly included in the common knowledge.

Afterward, the increase in the number of new publications, as well as the growth of the scientific community itself, forced the process to change toward a somewhat formalized structure in which few reviewers – chosen among the many peers – would judge and decide whether to accept or reject a new work. Academic journals, as we know them today, follow this structure in their editorial boards. These boards are usually coordinated by a few executive editors who distribute the incoming papers to colleagues who are expert in the field, the *referees*, for a review and then decide whether to accept or reject the paper for publication in the journal on the basis of these reviews.

It was the letter of comments of one of these referees that I, as a young Ph.D. student, read and had to comply with before that first paper of mine was accepted, published and eventually cited by others in their work.

In my mind – the mind of the student I was back then – the process of peer review looked somewhat idealized: somebody, an expert, has actually redone my computation, reviewed it in the light of current research results and judged it correct and relevant; she – or, more likely, given the gender structure of physics, he – has even found some minor mistakes and told me how to correct them. Few years later I began receiving papers that were supposed to be reviewed by me. Finding myself now on the other side, I realized that the entire process was different from what I had initially conceived. No computation was actually redone from scratch since it would have taken too much time. The referee, myself in this case, only checked the submitted computation in few crucial parts and for overall consistency. What I actually did, and most referee do all the time, was to compare the work to be reviewed and the problems it claimed to address with what I knew about the field. If what done in the paper was new and relevant, and it looked correct, it was passed. If it seemed marginal, or clearly incorrect or it was hard to decide what it was – the case of "not even being wrong", that is always the worst one in science – it was rejected. Very often, I would end up by sending back the paper for a few changes I thought would have made it clearer.

In the light of my personal experience of reviewer and reviewed, I think that the two major contributions of peer review as we know it today are the weeding out of work that is clearly outside current lines of research, and the improvement in the presentation

of many articles for which a better text, or more details, are required before publishing. It is not checking whether papers are right or wrong.

It is clear that the first aspect – he weeding out – is part of what makes science a very conservative enterprise. If you come up with a new idea that is not part of what is currently researched in the field, chances are that it is going to be rejected. But science needs to resist too sudden changes; revolutions come rarely and are slowly built from many, often uncorrelated findings. Unfortunately, this resistance comes sometime more from the age of the reviewers rather than their expertise – as we all tend to be more opposed to possible changes as we become older. For this reason in the US review boards tend to be made of young researchers. This is not true in Europe and it accounts for, I think, the major difference in doing science in Europe with respect to the US, and also the growing gap between the two.

The second aspect I mentioned – that of improving the quality of the work presentation – is a positive contribution of peer review that is often neglected in many discussions which tend to focalize on the antagonistic relationship between reviewers and authors. On the contrary, it is very important and perhaps decisive on the long run in which we want all the important results permanently archived in a readable, comprehensible format.

The entire process of peer review is not so very different – if you come to think about it – to what we do every day when we have to choose what movie to watch, what book to read or, for that matter, what detergent powder to use: we ask somebody we trust. Since we do not have the time to go through all new papers ourselves, or we do not even have the expertise to do so, we rely on other people, the opinion of whom we trust, and follow their advice. Colleagues in the past, editorial boards nowadays are these others in the case of peer review.

In recent years something has changed in this structure of peer review that I have briefly outlined – at least in the field in which I am a researcher, the field of physics that is called *high-energy physics*. High energy physics has always been somewhat a special community. It is not too large (or at least used to be) and is very integrated. Rapidity in communicating new results is highly valued and for this very reason the community has always preserved a parallel form of publication of the new research results in the form of un-reviewed *preprints* that were circulated by mail at the same time that the same work was going under review at one or another of the academic journals. People would read these preprints and thus keep abreast of new developments.

This peculiarity has been amplified in recent years by the widespread use of digital networks. The preprints have found a central repository were they are stored and can be accessed by anyone freely by means of the web. This represent a clear improvement with respect to the previous system since it allow for the (almost) instantaneous broadcasting of the results in new papers.

The work of a high-energy physicist begins in the morning by perusing the archives of preprints on the web and in so doing everybody reads everything that has been written in the field. Almost nobody reads the journals any longer; they are only used as eventual storage, their main contribute being a red flag in case some work we are interested in does not appear in one of them after a certain time (and in a few European countries for carrier advancement).

If you think about it, the present structure of the high-energy physics community provides an example of a scientific community for which peer review has gone back to the origins, back to Newton and his. I think that this is a very positive evolution because it solves the possible shortcomings of having a few people rather than the whole community deciding what is to be published and what not.

All positive expects of peer review are maintained. Work that is not relevant is simply not read and not cited. The process of improving the papers takes place in the form of emails sent to the authors by people who, in reading the paper, found mistakes or other things that could be improved. These improvements are witnessed by the revised versions of the same paper that are frequently resubmitted to the archives (often within the first week). These emails seem to me the distant echo of those letters exchanged between scientists on which so much of the history of science is based.

This process of *open* peer review seems even more similar to what we do for any other choice we have to made than the more formalized traditional peer review. We ask other people about a new movie, we ask them whether they have enjoyed it. Similarly, we ask our colleagues about a new paper either in the form of a direct question (an email again, most likely) or in that indirect form that is represented by looking at citations of a paper and trusting that a paper that has received many must be correct and relevant.

The only risk I can see in this going back to the origin is that at Newton’s time the community was small enough to be made only by real experts in the field. Each member was truly a peer and fully capable of judging a new work – physics was like music played in the XVII century to a public of refined amateurs, and for which musicians and listeners were equal in knowledge. Nowadays, this is not true any longer:

music performances are the playing by a few experts to a public mainly composed of non-experts and the same is partially true in physics, where the community has become large enough to have many members who are not really experts, more, so to speak, listeners than composers.

Is the best movie necessarily that that most people has seen? Is the best detergent that that sell the most? Phenomena like the sudden rise and even more rapid decline of fashions, the many following the few and other aspects that we have become used to in the world of mass culture, have surfaced in some area of high-energy physics and it is tempting to wonder how much of these phenomena is encouraged by this going back to the origin of peer review with its unchecked circulation of papers.