

## About classic “scientific” Edition with a priori peer-review:

### Mixing classic rules of edition and legal court leads to free correspondence

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#### Abstract :

We show how to use legal court to point out bad practices in scientific edition. But this needs participation of scientific readers to attest the disease.

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Scientific edition uses peer reviewing currently to control and select the scientific level of published papers. This is a good thing currently, but it may be used also to trap some good papers or to use lobbying to protect other scientists.

Since editors protect themselves (and the authors

I consider that this kind of problem is linked to the lack of method concerning the editors in respecting the scientific rules of publication : They consider reviewer as freely working without control. As a matter of fact, as a referee a reviewer shall respect the common rules; but does it do it strictly. It is engaged through his signature, but as review process is considered by editors as private correspondence, this cannot help when dysfunction is occurring; each party remains safe protected by privacy.

However, in some case discussion between reviewer and authors point out different dysfunction. For an editor, the last procedure is the Appeal, which appears in case of conflict between the author and the reviewers.

A strange case which happened to me ver early is that a referee says it is not worth publishing because it is wrong, and the other because it is right but well known ([2] , pp.12-29) the editor decision was not to publish (but this happened in another domain than “sandpile physics” and will not be discussed here)!

Anyhow, the role of editor should be to refer the reviewing process and to note and judge the contest. Any possible dysfunction causing a wrong selection process should be known from him, quantified and analyzed by him and by other editors, who have in charge to improve the process.

Concerning “sandpile physics”, I give few examples in my testimony n°1 [2], some of them where found when I worked as a referee (pp.4-8), or as a reader (pp.9-10, 231-234); but most cases came as an author, either in the field of “sand physics” or elsewhere. Here I discuss the cases of “sandpile physics” only. In the jungle imposed by the publish-or-perish, it is needed to strengthen the fair-play rules. This should be imposed to administrations, which should interact with editors. This publish-or-perish law can be control by administration only, because it uses it as a criterion. Of course carelessness is easier, but this will lead to editing lots of wrong papers, and to confusion. This will be the end of scientific literature.

But is this criterion a strict criterion? Of course not, as [2] shows it in few examples. But as humans like to get richer, even through using bad rules, and as bad rules can be easier to use than correct demonstration.... They use the simpler (bad) way often. This (will) generate conflict, contradiction, schizophrenia... This happened to Galileo, but it leads to the beginning of science in his time; it did not happen to Copernicus, because he died before his publication get known and controversial...

The publish-or-perish law has become stronger since few decades, and scientists know how to biased it efficiently, Worse, they use it to select and finance their research programs, so that scientist do not like acknowledge some of their works were wrong, because this will lead losing their grants (instead of being improved by the administration, because the administration does not want to make its job of correct evaluation)....

As a scientist I tried to make this understandable by everybody. I transformed (1999) *Poudres & Grains* into a Journal with post-reviewing. I tried to discuss the problems, to lead the discussions..... But administration tried to oppose it and refuse to evaluate the articles there. Scientists did not want to open discussion, either because they were afraid, or because their own point of view was respected already. Few of them used *Poudres & Grains* but they were allowed to publish elsewhere after, and their papers of *Poudres & Grains* were not quoted.

After 10 years, I asked administration to apply correct rules of evaluation, which it did not want to discuss. Scientists around me were asked to deny problems, to certify wrong facts...to forbid discussion about evaluation... Then administration decided to put me out of work (22 Mai 2013), through a vacation for illness without my agreement, for 6 months “because I was potentially depressed”.

Since this time I am not ill, nor potentially ill since I disagree with CNRS. But I am maintain out of my lab, I cannot speak with colleagues (either through the lab, through meeting, through conference). This is illegal likely.

But CNRS considers me as “ill”, probably insane (?), but it refuses to determine it scientifically and to give me the legal rights which should preserve my integrity.

I have fought legally against CNRS decision since May 2013. I gave for my defence most of my correspondences with CNRS, with ECP, with editors, with Science Academy, with CEE to the administrative “Appeal-Court” of Paris recently. Hence these correspondences are now public and can be used by anybody; they are collected in 8 different testimonies (from #1 to #8). They will be in free access in *Poudres & Grains*.

Since these correspondences are open sources, they cannot be disregarded “as linked to private correspondence” by anybody, or by any editor, or by any administration, or by any scientist.

I hope this will force the CNRS to apply correct scientific rule of evaluation , which is based on free open discussion.

It is clear that what happens in the scientific community is happening likely to other domains (economy, literature, politics, ...). But we hope that strengthening rules in science and in science administration will help strengthening other domains...

#### **About the part plaid by administration (CNRS and ECP):**

It is worth noting that correct reporting of the trouble I encountered was described through my scientific cnrs reports. I give the one I wrote on 2009-2010 ([2] pp.161- 234) as an exemple. It duplicates previous discussion in preceeding reports. These have no effect.

I tried also to discuss the problem in the “Conseils du laboratoire” at ECP. The first one where I tried to report my Testimony n° 1 is 23/6/2011. The schedule ([2] pp. 235-238) of the Conseil was delaid in order to impose a shorter time for my reporting. This happens due to the introduction in the schedule of a 1-h talk by Ph. Bompard instead.... Which demonstrate how “scientist administration” thinks how important the methodology is in the life scientist.

With this in mind, we should forget penicilium researchs.

#### **Conclusion:**

I believe that editors and administration have to follow new rules for peer reviewing and discussion. They need to understand how difficult the evaluation rules are, and how they can be biased. Scientific community has tried to understand them, this is why it gives power and success to our society. Denying this difficulty shall deny the whole scientific knowledge, because nobody will be able to define what is correct to what it seems only correct, (but is wrong). As Science becomes motre and more complex nobody will be able to state next correct step surely.

Considering the papers which were rejected by referees, they are all listed in my testimony #1 [2]; I believe most of them are correct; it means they make step forward to correct understanding (and not backward), or because they are mainly correct. I hope their results and understanding are good and fair.

Only one of them bother me; it is the paper with D. Sornette ([2] pp. 84-87), making the analogy with sandpile and spinglass. It seems wrong to me now; the explanation is partly contained in [4]. So if this paper is wrong, other former papers on this subject are wrong too!! It means that the number of wrong papers in the normal (peer reviewed) journals is more important than in Poudres & Grains....

This exemplifies the problem of Peer reviewing. Most authors do not care publishing wrong hypotheses... : Anybody knows it is quite impossible to make no mistake; so it is better invoking reviewer not to have detected them. This is also the way the editors and the science manager think. And this is not acceptable, because tracking the errors should be the role of reviewer, of editors, of managers and of authors. Not doing is letting the system working bad, leading to repetition of errors... Since “errare humanum est, sed perseverare diabolicum” , this leads to a true theologic interdiction. And this demonstrate the perversity of the managing method used by administration. The only way to get sustainable behavior is the complete control at different levels and using cross analysis. Hence we come back to the normal scientific testing of real experiment: not hiding, always free discussing and free testing.

It is also the same rule for accountancy: not hiding, always free discussing and free consultation and testing. This is the only way not to be stolen...

The problem of scientific editors is they hide their correspondence, and do not discuss their rules... This lets scientific reviewer able to use the system for himself, either directly, or indirectly. The problem of administration is worth: it has the habit not to reveal its strategy, not to report who is doing what so that nobody can follow the process which appears: it is spontaneous generation, and the administrative manager feels himself safe; better he moves and changes of attribution fast inside the institution. This is not possible in a technical society, based on science: Science has to be preserved by everybody, even the managers, who have to obey to it.

## References:

- [1] <http://defense-pierre-evesque.over-blog.com/>
- [2] [http://poudres-et-grains.eu/datas/temoignages/Temoig-1\\_editionsCL-23-6-11.pdf](http://poudres-et-grains.eu/datas/temoignages/Temoig-1_editionsCL-23-6-11.pdf)
- [3] 2<sup>ème</sup> réponse au CNRS (27/4/2016) via la Cour Administrative d'Appel de Paris ([http://www.poudres-et-grains.eu/datas/suite\\_affaire\\_2/3rr-mem-22.4.16-CAA.pdf](http://www.poudres-et-grains.eu/datas/suite_affaire_2/3rr-mem-22.4.16-CAA.pdf) ) which makes public the private peer-reviewing correspondence.
- [4] P.Evesque, *Poudres & Grains* 11.2, 19-41 (2000)