

Discussion with Nature about peer reviewing, reference and Transparency

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Abstract :

Testimony #1 was produced to “la Cour administrative d’Appel” in Paris; so the following correspondence is no more private but open to anybody and can be used by anybody refereeing to it.

Pacs # : 5.40 ; 45.70 ; 62.20 ; 83.70.Fn

Here I report on p.81-82 a letter, dated from July 1997, that I wanted to be published by Nature, on the process of peer reviewing. It was rejected.

I cannot consider normal the fact that Nature refused to discuss and refused to publish this letter.

Bibliography is quite difficult to perform seriously. It is quite possible to miss a paper; and referees may or shall help. It is also their responsibility to evaluate the bibliography, or the lack of bibliography.

It is just what I wanted to remark.

As shown by the next fax on p.79 (on 3/1/2011), Nature does not want to discuss the problem ten years after it happens.

How can scientific judge this negation of problem. And how Nature editor can defend his behavior!

It is clear that editor attitude is not at the level of the required quality.

Editors and administration shall provide proof of the correct partnership they proclaim to have with scientists.

Anyway this is always the case everywhere, since technical service or industry shall produced correct tools or parts. The manufactory have some real obligation. This is not the case for managers, administrations... editions... justice...

But can we accept such ambiguities, because science has many applications in the real word and its rules cannot be treated as approximations in many important cases

(nuclear plants, ...). I believe the management shall improve its technique, this is now a priority of our new world.

References:

- [1] <http://defense-pierre-evesque.over-blog.com/>; [1bis] 2^{ème} 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) which makes public the private peer-reviewing correspondence.
- [2] http://poudres-et-grains.eu/datas/temoignages/Temoig-1_editionsCL-23-6-11.pdf ; pp. 9-10_&_231-234

Affaire éditorial Nature (1997) /Article Makse et al. (Nature386,379,(1997))

Cet article présente un certain nombre d'effets sur la ségrégation comme s'ils étaient inconnus du scientifique, et donne une explication du phénomène.

Malheureusement, Ces fait sont connus depuis longtemps. Je demandais ici que l'erreur éditoriale soit reconnue.

Cela n'a pas été le cas.

J'ai réécrit plusieurs lettres cette année (2010-2011) pour demander la permission de mettre cette correspondance sur le Web. Nature n'a rien répondu.



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Objet : our correspondence EX7099 ; 22/July/1997, and related

Dear Editor,

I confirm my e-mail on 24/2/2011. I would like to put on the web few editing problems I encountered, or that others have encountered.

I am interested in the reproduction of our correspondence EX7099 22/July/1997, and related.

Do you see a downside.

From the human point of view, time has done its work, and concerns that can not be scientific.

Thank you in advance for your favorable response.

cordially

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Voir le rapport CNRS de Pierre Evesque 2011, Annexe 10, p. 69-72



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U.R.A. 850

Châtensy, july 15th, 1997

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Sir,

I have been really interested by the experiment of Makse et al.¹, segregation phenomena are always spectacular, especially when they are in colour.

It is a good idea to come back to twenty years old, and even older experiments. It might even be necessary to do it systematically. But it is first compulsory to realise serious bibliographic works and then extract thereof a critical bibliographic synthesis. I must say it is not an easy work in granular material physics, as results are spread throughout various professional reviews. It also requires a comprehension effort of the different approaches (mechanics, process engineering, mining, chemistry, drug industry, ...) which all have their own language.

Nowadays, when money is more and more scarce, teams tend to claim that their subject is brand new. This is a pity for Science.

First of all, young scientists are not suggested to lead any bibliographic work, other than through a computer. They get very disappointed when they discover in a former review that their subject has already been discussed. Secondly, scientific work is not a matter of fashion; it takes place in a continuous process. Why, otherwise, publish issues that are bound to get forgotten 3 years later? Even fundamental scientific ruptures assume that the knowledge of the matter in discussion is acquired beforehand.

We can mention here that the interpretation offered by Makse belongs to this continuity and is not a rupture, as it acknowledges the importance of solid friction.

We do not mean, though, to discredit a team in particular, but to set the problem of the responsibility of the whole scientific community. Shouldn't this community (through, in particular the referees) control how information is spread, and boost exchanges between scientists specialised in different fields?

If we let "physics" of granular media pretend to ignore the results of "mechanics" of these media, it can only enhance a cultural racism with negative effects in scientific work.

But it gets even worse when scientific communication is so scarce that it leads researchers to go through works which have already been achieved by others...; what difference can then be made between industrial know-how, characterised by a no-transfer of technology between two rival firms and such a no-communication science?

As you might have guessed, Makse's experiment is not really innovating. A photograph of this stratification effect can be found page 18 of the review paper by Ennis². Also, similar results

have been issued in at least three publications, the first of which dealing with washing powders³. We can also quote the works on sedimentary stratification problems by G. Berthault & P.Y. Julien⁴, which I described rapidly⁵. These authors have proved that several horizontal strata can lay down at the same horizontal location at the same time; conversely, they have shown that two distant points of the same horizontal stratum lay down at two different times. They query therefore partially the theory of dating by stratification.

I feel sorry about all this uproar that we currently hear in the "sandheap" community. As you may see, many other examples may be quoted^{6,7} (I myself believed that I had succeeded in a new experiment when I started my research on this topics in 1987). This uproar must stop: either scientists agree to take in charge the bibliographic work, agree to use a common language, and we will remain scientists, who use other researchers' work and communicate with one another. Or the aim of communication is reduced to a "show", and it is no use to publish anymore, as no one reads the articles, nor uses their results when these results get as old as two years old. Researchers' work becomes pointless.

With the Association for the Study of the Micro-Mechanics of Granular Media, we are currently working to keep the scientific direction, trying to mix the different scientific communities together through the organisation of a series of meetings called "Powders & Grains" (last one held in 1997 in Durham, North Carolina and next one will hold in Japan in 2001). But we can only succeed if the scientific politics of Journals (the most famous included) helps us.

Pascal, a French thinker of the 17th century, rediscovered Euclide's thirty second proposition when he was nine, and with no preliminary bibliographic research; but this proposition is still known as Euclide's! And inversely Chladni figures became famous thanks to Faraday's work.

References:

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Yours sincerely,



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In reply please quote:
EX7099 SC/tee

22 July 1997

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Dear Dr Evesque,

Thank you for your letter of 5 July. From what you say, it seems as if this matter is one that you can most usefully take up directly with the authors concerned, who may have been unaware of the work published in French that you mention, rather than complaining to us. We cannot investigate complaints of this type unless they are formally submitted for publication as Scientific Correspondence comments, in which event they should be written in scientific language (not as rhetorical accusations) and accompanied by copies of correspondence between the complainants and the authors concerned. We then consider such comments for publication only if they seem to us to be addressing the main point of the paper under discussion or to be raising broader issues of interest to nonspecialists, rather than to be pointing out a small "slip" in omitting to cite work that is not universally agreed to be vital to the central scientific argument put forward in the published paper.

On this occasion, it is not possible to tell from your letter whether your criticism of the paper by Makse *et al.* is of enormous significance, as you do not provide scientific context for their omission but instead offer your opinion about the authors' motives and so on. I am sure you appreciate that *Nature* is very strict on the number of citations we allow our contributors, because we are a nonspecialist journal and our space is very limited.

If, after you write to them, Makse *et al.* feel that they should have cited the papers you mention as being central to their conclusion, they can cite the work in their future publications (or, if they think the error serious, send us the text of a correction). If, however, you find their response unsatisfactory and you feel that the uncited work is crucial to the conclusion of Makse *et al.*, then please get in touch with us again, enclosing copies of your correspondence with Makse *et al.* and being more specific than you are in your present letter about the scientific relevance to their main conclusion of the uncited papers (and enclose

(continued)


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Rapport d'Activité de P. Evesque,
P. Evesque, Testimony #1, CL MSSMat on 23 June 2011

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copies of the relevant publications). It would also be helpful to us, and possibly more productive in any dealings you have with Makse *et al.*, if you could write in a more measured tone than the potentially inflammatory way in which you do at present.

Yours sincerely,



Dr Maxine Clarke
Executive Editor

Châteray, september 1st, 1997

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EX7003 sc/tce

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Annexe 10, p.4
 Ma réponse

Dear Sir,

Thank you for your answer on July 22nd. But the real purpose of my previous letter was not to be published but to be read by you. My motivation came after few papers on the "physics of granular materials" which your journal published recently, which all seem to disregard previous works and which claim to be brand new when they are not.

Thus, I just wanted to bring to your attention the following facts: i) a lot of previous results on granular matter has been published since two hundred years (and not only by Coulomb, Faraday and Reynolds); ii) your referee establishment seems not to know these works which is quite surprising for a team of specialists; anyway, iii) a good scientific politics of publication cannot accept disregarding these ancient works (nor those in foreign languages), otherwise there is no more science and scientific literature; at least, this is my point of view.

In France we have the habit to say: it is dangerous to give the power to specialists... but what is worse is to give this power to people who think to be specialists and who are not. I rely on your journal to promote discussion and cooperation between teams, between ancient and new specialists....

I apologize also for my tone which you judge non "politically correct" since polemical. But is not it a more classical rhetorical answer too. The scientific evidences of what I say are already published in the referred papers; so they do not need to be repeated in a scientific writing. Furthermore, they are not all concerned with the Makse *et al.* paper, but also with others published in your journal (see ref. 6). This is why be sure this letter is not written against a precise work, a precise team, but either against your journal or better to try to improve its quality.

Yours sincerely,

Pierre Evesque

It is not the only problem encountered with Nature, and with other editoirs: Authors may quote good papers in a way such that nobody understand these bibliographic papers have solved most of the problem already, so that the new author just apply the early work...

This is perverse since science works because scientific edition shall transfer information to most people who are able to understand.

Autre problème rencontré avec Nature:

Je pourrais aussi traiter du cas de l'article: G. Metcalfe, Tr. Shimbrot, J. J. McCarthy & J. M. Ottino; Nature 374, 39-41 (2 March 1995); Avalanche mixing of granular solids, qui traite d'un problème de ségrégation granulaire dans un système (bidimensionnel) cylindrique et générant des avalanches.

Une petite phrase m'était apparue incompréhensible, et ai demandé l'explication aux auteurs, ou à leur collaborateur (Khakhar?).

En quatre lignes ils m'ont expliqué, preuves à l'appui à partir de l'équation de Navier-Stokes 2d et de son analogie hamiltonienne, qu'un système 2d à écoulement continu ne pouvait pas générer un mélangeage car les lignes du courant ne peuvent pas se croiser en 2d, et que ce système ne générerait donc que de la ségrégation irréversible ; en d'autres termes, il fallait introduire de l'intermittence (d'où les avalanches) pour que le mélangeage soit efficace.

C'est ainsi que j'ai compris la vraie raison de leur propos, l'intérêt réel de leur article; et ils m'ont démontré qu'ils connaissaient bien mieux le problème que ce qu'ils en disaient dans l'article.

À leur décharge, cet article n'aurait probablement pas pu paraître dans *Nature* sans cette présentation. **À croire que Nature veuille devenir le spécialiste de la "désinformation scientifique"**.

Pas d'autre information

Ceci termine l'exposé des quelques problèmes litigieux dans le principe de sélection des articles par les revues.

Pour en faire une revue détaillée, mieux vaudrait demander l'expertise des évaluateurs professionnels, des revues et des personnes ayant eu à souffrir de ces pratiques. J'expliquerai par exemple un cas d'expertise où la déontologie n'a pas été respecté par le service évaluateur: le CNES et l'affaire dynagran. Encore que là je n'insisterais pas sur certaines pratiques très mauvaises permettant aux managers de dénigrer les meneurs de projet qu'ils sont sensés défendre, et qui permettent ainsi aux partenaires/concurrents internationaux des mêmes meneurs nationaux d'emporter les positions.

La France semble très friante de cette pratique permettant de faire croire au fairplay de la hiérarchie française quand celle-ci est peu impliquée dans le projet, ou lorsque celle-ci connaît moins bien le projet que les leaders qu'elle est sensée appuyer et que les autres hiérarchie en fasse d'elle.