Témoignage n° 4

de P. Evesque

Au Conseil du laboratoire MSSMat

Du 3/09/2012

De la science à la « réalité » administrative

et aux « nouveaux scientifiques de renom »

From Science, to « administrative Reality » and to Newly famous Scientists : an evil transmutation

Analyse des dossiers contenus dans
les Témoignages n°1 et n° 2

Contents

[1] Parts of this text was published in mid-May 2012 on the blog of "peer review 2012", congress on science of information…, to stand on 17-20 juillet 2012, in Orlando, USA. This text was revised and extended toget this version. *Ref:* http://peerreviewing.wordpress.com/2012/04/23/peer-review-is-it-effective-is-it-possible-to-improve-its-effectiveness-is-there-other-means-to-evaluate-research/#comments.

Part A

Introduction:

Part A. Introduction:

Why Testifying?

I understand quite much that it is not easy to evaluate research. But it is even much harder to evaluate the system of evaluation; no body wants it: how to judge how managers manage; but this is a necessity; the best would be that it is done scientifically. However, this has been always the case by the past,but through economics: for instance in classic literature, …, this is why the classic edition is now as free as possible, editors trying to select at best but readers telling their ideas, and futur imposing time selection. Scientific edition is rather different since researchers (i.e. writers) are paid for they job and research granted directly. So scientific edition should cost less than literature edition that supports not only printing cost (paper, printing, editinor salaries,…) and because research work is quite expensive compared to printing. Nevertheless edition costs and editors need funding…

Edition funding was important in scientific edition likely in the past and/or the problem will come back most likely soon; but I do not think it is the **problem nowadays**, where free edition through the net allows discussion for "free", and where real scientific editors gets rich, can "raise tax" to university libraries…. Our "new world" is such that "scientific ideas" (and scientists too) travel all over the world fast, that new ideas are used, and tested fast… Apparently, we need good research managers who tell us what is right and wrong, what has to be taught… But unfortunately, we don't have such managers, this is true for literature, but it is even worth for scientific research. How can one expect telling something true, before a series of tests…. and some experience, some time…

Our ancient colleagues (Gallileo, Newton, Darwin, Lagrange, Pascal, …) did know that thinking is our **liberty**, and that scientific questioning is hard. It is not only hard to give good answers, but the puzzle starts with the problem of selecting or dividing the "right questioning": how much intricate the problem to be solved is? How much can it be decomposed? Hence, the questions are easy to settle, but the solution needs using sometimes other direction, leading to new finding and extensions from people from other fields who were not concerned directly. So nobody knows what is a good future research,… except managers who tell the contrary, but who are wrong. The worse being they think valuable to get answers to unsolved problems to get funding from officials (because officials have not been taught correctly)… And they want to tell officials that they were right...

So how can a scientist believe on such a manager aim? He tries not; but he lies, trying to convince the manager, using lobbying, using not funded arguments, using uncorrect papers…. This is the way the trick starts and this is the difficulty of respecting the scientific deontology: nowadays the manager is sought so much useful, that this idea is destroying science itself, chaging the order of the values…, with the help of scientists. But who are the scientists nowadays, mainly paid people acting under conctracts, thinking nothing about their tasks…, ready to spent money on new toys…, probably as many scientists did, but with a lot more numerous and rapidly-changing toys, and fast money-exchange…

Few remarks on evaluation:

But first let me remark the following about evaluation[2]:

1) One of the main problems with evaluating the benefits of peer reviewing strategy is that there is not so much material to study: What can we analyse? The referee reports and the discussions with the editors are considered as usual corrrespondance, which can not be published. So we cannot have access to correspondances (or reports) from others. And the editor cannot do the job too, first because he[3] is one of the parties, which means that if he detects some flaw and if he declares it, he will attack the notoriety of his journal, attacking its efficiency. This is the main reason which limits the possibility of correct scientific evaluation of peer reviewing by the scientific community. Then the editors have a very simple defense, which consists in arguing "we will read much worse papers if we work without peer reviewing".

   This answer is not correct as we will see from practical examples later; for instance the only argument against *Poudres & Grains* is that no review of papers is available, which is true also for any classic journals. But why not testing other kinds of reviewing, such as cross reviewing between journals?...

2) I did not tried this concept, because it requires at least two independent journals. But I tried to turn the rule in editing *Poudres & Grain*s, as a "a posteriori" pear-review journal; and I think I succeeded partly, and after much effort. The main difficulties encountered were (i) to prove to **persons who do not want to hear**, that the papers which were rejected by editors have no real flaws most often; then it needs (ii) to convince people that information transfer is more efficient and less costly using this way. The demonstration of argument (i) needs the use of Témoignage #1, that requires its consultation at Conseil de Labo (CL) MSSMat du 23 juin 2011. This testimony present all my articles which were rejected at some time, with their correspondences to/from editors; I gave the

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[2] ***"peer review 2011"***: Parts of this text was published around May 15, 2013, on the blog of "peer review 2012" congress on science of information…, to be hold on 17-20 juillet 2012, in Orlando, USA. http://peerreviewing.wordpress.com/2012/04/23/peer-review-is-it-effective -is-it-possible-to-improve-its-effectiveness-is-there-other-means-to-evaluate-research/#comments. This text was revised and extended toget this version.

[3] I use **« he » all along the text** without specifying the sex of the person as in French where he means « he/she ».

booklet during some kind of official assembly of my Lab, i.e. the CL MSSMat, which is some kind of management meeting. This allows any of my colleagues to look at it, and to conclude. In principle also, this booklet is accessible to the CNRS- and University-/Ecole- managements and to any staff in charge of the evaluation of the lab. Also the document can be asked for directly to the director of my lab, or to me. Within this way, I am able to discuss of the problems I encountered in peer reviewing, and to quote them through real examples. This will be done in this new booklet, Testimony #4. If some of you is interested to check exemples, please ask for Tem #4. I Discussing the papers is boring, but it is the way to force the change. No real difference exists between rejected and non rejected papers from these exemples. It s the reason why I use P&G now on.

3) Before turning to point (ii), to convince of the efficiency of the P&G journal, let me notice that it took me about 10 years to achieve point (2): It was only after few appeals to the CNRS headquater (which did not answer), and after asking some help from the CNRS Mediator, that I could discuss with the CNRS law service using the mediator intermediacy to define the protocol (2). They did agree that I could use this protocole, even though, the lab director tried to refuse; but I maintained my goal and succeded at last (after 6 more months) and partly.[4]

This procedure can be used by anyone working either in a CNRS lab, or elsewhere at the university via the chancellor of this University. I wrote 3 testimonies [5] (labeled from #1 to #3) already.

4) Testimonies #1-3 are difficult to read, redundant and boring; they need clear introduction and detailed analysis of its content, to enlight the few important reasons and facts. This is the purpose of this to-day testimony (témoignage #4). I hope it will make the documents of "Témoignage #1-3", (at CL 23/6 & 16/12 2011, & 13/3 2012) easier to read ….

5) I come now to point (ii), i.e. how to convince of the efficiency of *Poudres & Grains*. At the moment, most of the evaluation is done through peer reviewing from journals. Other evaluations exist from administrations or companies, but they refer to them, even if they have "peer reviewers" panel themselves. None of these reviewer panels are used at testing the quality of the journals, nor of the findings, nor of the fundings. And good scientist are more interested in funding nowadays, letting to the youngest researchers the reviewing job for journal papers. The system cannot work coorectrly, but can we prove it?

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[4] The lab director is still arguing against this possibility, but I think it is illegal, and he knows it. So, I print on my office door "Non scientist passes away ; Here only correct scientist are accepted ; it means those who accept to apply scientific deontology, and require that scientific deontology has to be applied by/for others".

[5] Témoignage #1, #2, #3, at http://Archive.mssmat.ecp.fr/, password needed (ask te lab Director), see [7].

The only way to demonstrate (ii) is to show how much risky the method is. It will happen (soon likely) that this managing and funding method of the research is totally inefficient, that it hides good new ideas or solutions to merge. But this needs time. For instance with P&G, we got no evaluation, even if I tested the system as I will show using my testimonies. But checking evaluation needs time because it needs comparing different samples from classic journals and from new non classic ones to avoid the simple argument claiming that papers of peer reviewing are correct, and its standard has not evolved.

But the way the scientists play the game of evaluation changes the habbit and the "norm". Some correct evaluation shall show the main flaw of *a priori* peer reviewing technique, which is to increases the number of papers, to emphasize the noise more than good and strong results. To understand this, let us assume that any research journal tries to find some solution to new "problems" so that it will edit new papers continuously once a topics is evaluated to be interesting for research; but most of these results are attempts only, missing the solution, till the solution happens and is edited; when solution is known, it remains valid, cancelling other publications. So, the field turns into a new topics when the solution is found. So scientific-research literature is overpopulated by "non or little profitable" papers, with few goods. Better, as evaluation counts the number of (quoted) papers, the good scientist is the one who is quoted, who doesnot find solution and remains on the topics. Furthermore, the less clear the paper, the more fruitfull it looks to people who do not understand….

So paper counting, which should define (i) merging research and (ii) good solutions, generates much more noise that good solution.

Sum up: to evaluate peer review technique, one needs documents. The trick is that peer reviewing expects to restrict the possibility of such an evaluation by not allowing publication of these documents. This is contrary to any legal and/or scientific procedure, which needs clarity and public judgement. How can one change this? It is obvious also that changing the role plaid by evaluation may change the habits, and change the measure. This is the aim of this testimony to analyse the consequences. Another way is to change the peer review process and to see what happens. This has been done with *Poudres & Grains*[6], a journal I transformed 15 years ago already. Please take a look at it. http://www.poudres-et-grains.ecp.fr/spip.php?rubrique1

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[6] ***Poudres & Grains*** *(P&G):* Please take a look at it. http://www.poudres-et-grains.ecp.fr/spip.php?rubrique1 ; the bulletin was created by R. Gourvès and AEMMG around 1990; I have been elected editor since 1993 at the Birmingham meeting; the association has become also international since then. Creation of other bulletins (such as GDR MIDI...) made P&G not active. I transformed it into ascientific journal in 1999, with its present rules. I was thinking it was a necessity, since no real discussion about some goals and results in few science fields was possible in physics journal and because physics community at Durham meeting was thought to be aggressed by presentations tending to disagree with their approachs. This leads to define a method of publication which is too noisy.

Part B

Methodology :

Part B. Methodology :

So I wrote 3 testimonies[7] during the last year; they content most of my correspondence with different editors, admnistrations (CNRS, ECP, French Academy of science, AERES, CNES), European commission for research, ESA… All these three testimonies have been deposited at 3 diferent sessions of the Conseil du laboratoire (CL) MSSMat, umr 8579 cnrs, labelled here after as Testimony #1 on 23/06/2011 , Testimony#2 on 16/12/2011 ; Testimony#3 on 13/03/2012 . Their contents are given next pages. Any authorised person who need them, can ask for them to the Director of the MSSMat lab, or to CNRS, or ask them to allow him to enter in the intanet a thttp://www.mssmat.ecp.fr/ and find the CL ; these booklets were planed to be found on the lab intra-net. However Testimony #3 is still not in intranet, even I asked to be deposited.

As these documents exist, I have just to explain the problems I encountered with/through them, quoting the pages of the testimonies where they can be read, focusing here on the explanations.

New testimonies will be provided as soon as my explanations will be written or as soon as new documents will be produced.

This technique should be used by any person who feels sad not to be able to testimony as he wants.

The sub-title of **Testimony #1** is « on some problem with the editorial politics of peer reveviewing » because it collects my correspondance on peer review and editions problems. My 2-year-cnrs-report (2010) is joined in this testimony #1 to demonstrate that the administration knew my questioning, my health problem…, my scientific research fields and results and the problem met by F. Douit….

Testimony #2 is a collection of my recent correspondences and discussions to and with CNRS, ESA, CNES, ECP, French Science Academy and European Commission in oder to determine how one can manage deontologic problems. It seems that one cannot argue even. I do not feel fair the role plaid by CNRS to support my research, nor to support the grants. It might get better if CNRS was managed by non scientists…

I give part of the conclusions of my understanding in **Testimony #3**, where I join my "demande d'aide à la recherche CNES- 2012", to examplify my stress. I recall the documents and the correspondances I exchange with different organisms to help me fighting against the weaknesses found to respect scientific deontology in France, Europe,…. It seems that one gets no help from the organisms. I tried to ask ESA, CNES as funding partners, CNRS and ECP as

[7] ***Testimony series***, in French témoignage #1, #2, #3. They where deposited at CL on 06/23/2011, 12/16/2011 & on 03/13/2012 (month/day/year) repectively). They can be found on intranet of MSSMat lab, at Conseil du laboratoire MSSMat. At the moment they are in the Archive of the lab, at http://Archive.mssmat.ecp.fr/ (for which a password is needed, which can be asked to the Lab Director).

granted partners, CNESER, CNRS and ECP as deontology partners, European community as funding agency…

Besides the scientific and deontologic problems linked to peer reviewing and recent methodology in research administration, I will describe other typical problems encountered in the past, which I cannot accept too.

B.1. Contents of « Témoignage n° #1 » de P. Evesque

At CL – MSSMat on 23 Juin, 2011

« on some problem with the editorial politics of peer reveviewing »

[8] The page numbering of each testimony is the one of the electronic pdf format at http://archive.mssmat.ecp.fr/.

Rapport CNRS :

I wrote my "rapport CNRS 2009-2010" (testimony #1, p. 161-272) to report most of the problems I had to overpass these last few years (overworking, heartattack, AVC, administrative harassement,…). It relates also briefly a history of my carrer,i.e. the evolution of my working interest and of the working locations (p.166-171 & 194). The recent advancement in granular-gas theory, simulation and interpretation are reported in Testimony #1(p.172-176). Endly, the problem met by F. Douit in lab MSSMat is reported shortly in Testimony #1, (p.181, 196-197 & 201-202).

I do not find fair the role plaid either by the lab management T#1(p.203) at ECP, nor by the CNRS administration T#1(p.196-197, 198-199, 201-202) to support my research, nor to support the grants, nor by CNRS peer reviewing (pp. 211-218). No discussion happpens after my remarks about research work for "réforme du cnrs" [T#1(p.204-206)], only the claim of evaluation importance without a clear definition of the measure to be used even. Nor on the one about scientific deontology T#1(p. 207-210). Only criticisms came from evaluation teams, which were not able to evaluate correctly, even the number of my publications T#1(pp. 211-218)…

With the rules used, one will gain to be managed by non scientists…

B.2. Contents of « Témoignage n° #2 » de P. Evesque,

to CL – MSSMat on 16 Decembre, 2011

« on evaluation of research proposal & peer reveviewing »

[9] The page numbering is the one of the electronic pdf format at http://archive.mssmat.ecp.fr/

B.3. Contents of « Témoignage n° #3 » de P. Evesque, to CL – MSSMat on 13 Mars, 2012

"On instances which should observe, promote & respect scientific deontology"

B.4. On other problems

I encountered in peer reviewing, which I will speak too

Other problems can merge from peer reviewing. Here are few examples. Some may arise from hiding results known from scientific literature in an other domain (1); but what can we say if not hiding the result would also make to reject the paper? The next one merges from authors who try their data not to cancels results from other previous authors (2) or hiding disagreement between their results and the concepts they want to study (3). I have been suffering such disagreements in the recent past.

1) Autre problème : 1.a. Pb de Nature Ottino ref: Nature, 2 March 1995 ; Guy Metcalfe**,** Troy Shinbrot**,** J. J. McCarthy **&** Julio M. Ottino ; Avalanche mixing of granular solids ; Nature **374**, 39-41 doi:10.1038/374039a0 1.b. My PhD dissertation (thèse d'Etat):	T #4 ; F2; p. 17 T#4; F6; p.20
2) Problème de reviewing non sérieux fait par les agences à mon égard: 2.a. Passage CR2-CR1 section 13 (ex section 5) 2.b. Comentaire de J. Villain à la section 5 du cnrs (1989) 2.c. Commentaire de M.Frémont sur mon stage à l'umr 113-(LCPC) (14/12/1990) 2.d. commentaires de la section 5 cnrs (2001-2008) 2.e. CNES, ESA : 2.f. avec l'appui de l'article N.Vandewalle (sans y toucher) il faut pouvoir discuter avec les autres acteurs pour pouvoir faire comprendre la stratégie utilisée à mauvais escient: Pb de Vandewalle et Minitexus.	See #4 ; p. 20
3) Problème de reviewing pour NSF du proposal Behringer & P&G 2001 ;	See #4 ; p.

Part C

Poudres & Grains

Part C. *Poudres & Grains* [5, 10]

This part describes first the original reasons to create *Poudres & Grains* (P&G) ; it means its history linked to the history of the «Association pour l'étude de la micromécanique des milieux granulaires » (AEMMG[11]) which is linked also to the Powders & Grains[12] meetings. Then campaigns aimed at promoting this journal and its new form of edition will be described, together with the little interest of the scientific organisations to promote such a simple mean with so much indepedency. Few reactions will be reported, demonstrating some non passive feeling with small or less small agressiveness.

In this part, the history of *Poudres & Grains (P&G)*[9] is described, which is linked to the story of the Association pour l'Etude de la MicroMécanique des Milieux Granulaires (AEMMG)[10]. The second part details the work done to promote (and not to promote) the « second life » of the bulletin as a scientific journal developped in order to exchange between the communities.

From what I understand, in a scientific context, most of the problems come from the readers, not from the authors. An author when he writes tries to communicate; he is ready to adapt words to be understood, except if he want to keep for his own the communication. The reader is not in this attitude. However, a scientific reader should get this attitude too, since he is a **professionnal** that means that it is his problem if he **misses important results**. The problem of editor shall be not only testing and improving the writing, but also that finding be easily obtained.

But the problem of missing some idea is more often linked to obtain the right information (and to know what is the right information). A scientific journal which tries to bridge the gap between communities should be always welcome. The only problem it can encounter is that communities still want to be independent.

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[10] ***Poudres & Grains*** *(P&G):* Please take a look at it. http://www.poudres-et-grains.ecp.fr/spip.php?rubrique1; the P&G bulletin was created by R. Gourvès and AEMMG around 1990; I have been elected editor since 1993 at the Birmingham meeting; the association has become also international since then. Creation of other bulletins (such as GDR MIDI...) made P&G not active really for a while. I transformed it into a scientific journal in 1999, with its present rules. I was thinking it necessary, since no discussion appeared already between different communities using/studying similar fields from different view points was possible and because this was happening during Durham meeting (1997) where physics community thought to be aggressed by few engineering presentations tending to disagree with their approachs. This made the method of publication too noisy.

[11] **AEMMG :** Association pour l'étude de la micromécanique des milieux granulaires, premier président R.Gourvès, président d'honneur, B.Biarez, created in 1988 or 1989, before the meeting in Clermont-Ferrand.

[12] **Powders & Grains meeting:** This series happens every 4 year (the duration of a PhD). It started in Clermont-Ferrand (1989), Birmingham (UK, 1993), Durham (NC,USA, 1997), Sendai (Japon, 2001), Dusseldorf (Allemagne, 2005), Golden (Co,USA, 2009) ; the next will be in Sidney (Australia, 8-12/7/2013).

C.1. History

Poudres & Grains[5,9] started in 1989 as a news bulletin founded by a small Association (Association pour l'Etude de la Micro-Mécanique des Milieux Granulaires or AEMMG) created for building up a French meeting on the mechanics of granular material at microscopic scale. The President was at that time R.Gourvès.

I have been elected the editor of **Poudres & Grains (P&G)** in 1993. In 1999**,** I tried to re-define its edition rule to test a new form with more correct scientific-research rules, since research requires enhancement of scientific discussion and debate. This is not usual with classic journal using *a priori* peer reviewing, for which a paper does not lead to discussion once it is accepted by the editor. I thought at that time that the scientific debate was avoided too much in normal peer reviewing rule and should be stimulated when scientists came from different fields in order to transfer languages and learn them. Furthermore, debate is the only convincing way to define valuable scientific ideas and is currently used **in many cases** such as for PhD presentation. So P&G rules are:

1) So, P&G is a journal with "open" access; its "access" is "limited" to « professional » research scientists, or equivalent people. This is to assert that the journal may contain flaws likely, which is normal in an active research field; it shall be recalled here as it should be repeated also for any "peer review" journal… Anyone who considers oneself as a professional can/shall read the journal, and discuss the articles.

2) The second rule declares that the **reviewing is based on readers**, since they are professional they should be able to find mistake and tell them as they find. Authors are also required to criticize and to amend their works, for instance they find a mistake after some discussion/or presentation in a conference.

For instance, most of my papers in P&G (if not all) were presented in public scientific meetings/congress. They passed the scientific discussion; I did not feel any important question. This was also the case for papers from the few other P&G authors. Furthermore, P&G **15**(3),35 has been published few years after without any problem, and P&G **ns2** & **ns3** works were reported and discussed in conferences as Powders & Grains, and elsewhere…. Also one of my papers in P&G was translated, and published in a book (with peer reviewing). Few others were rejected from classic reviews for "no good reason" {see[13] P&G **7**,1(1999) or [►F15]; **11**,58(2000) or[►F16]; **12**,115 (2001) or [►F18]; **13**,20,(2002) or [►F17]} and Arxive {physics/0609204 or [►F19]; cond-matt/0611613 or [►F19]} . Most of the time rejection did not change my opinion about them, and I published them in P&G. But how can I publish the comments? I would do it still, via P&G, but I

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[13] This refers to next part ; for instance see : ►F16

do not know how to proceed. Please give me ideas and rules that would be receivable by editors….

3) Of course a reader who does not detect any mistake has nothing to declare. So no reviewing does not say no reader and no review process; but it does not tell also "everything correct". However if a reader does not understand some part of a paper, he can contact the author and ask for explanation, making the paper more understandable for everybody. This is not the case for any paper in *a priori* peer review edition and shall clarify the text and context [see exemples in ►F2 , i.e. Tem #4, p.10; point 1.a; ►F3, i.e.Tem#1, pp 9-10 & 231-234 in next part and their ref. Tem]

Nothing is done in P&G to identify readers (who have e-charged the paper); their responsibility will never be claimed for any review. It is a classic rule in all kinds of literature; this rule shall be preserved in scientific literature too, as the author liberty is protected too, except for illegal assertion. However, discusssion and exchanges shall be encouraged.

C.2. Efficiency of Peer Reviewing:

For P&G, what is very strange is that **no reader desired to discuss the articles and to criticize them**, neither for my own papers nor for other ones (P&G **ns2** & **ns3**, …)[14]. But everybody wants to do it for scientific edition, when his advices are aked by editors and are not published. It is perhaps to declare it in his curriculum vitae...

The author psychology can look quite strange: let's the author writing free in a journal without review or censorship; this author does not use the journal (see the number of authors in P&G). On the contrary let us add a reviewing/ censorship process; the scientific authors seem safer and become happy and write… Why? Is the process better? Are the ideas stronger? I do not feel. But authors like being judged when approved, and like claiming they were judge. Perhaps it is because they can still argue that two other bright guys were approving them in case of error. But this argument has no meaning from a probability view point as soon as one is allowed to pass the tests many times, especially when modifications can improve te text.

Even better or worse! I asked in 2001 (and the years after) CNRS-section 05 of CNRS to review my papers in P&G[15] , see [Tem#1 p165, 168-171, 191, 196-199, 204-230] or read my other cnrs-reports. The commission and its president refused ever, similarly with CNRS editor[Tem#2 p. 184, 200-231]. I asked also the French Academy of science… see [Tem#2 p.166-170, 232-285, 286], No one wanted. At the

[14] As a reader who read all the P&G papers, I did not find important flaw at the moment. The content of P&G ns2 and ns3 is so broad and complex that complete analysis will need further competence and discussion between few reviewers.

[15] This commission #5 of CNRS is in charge of scientific evaluation of part of solid state physicists

beginning I was asking for publishing the report, under the real name of the reviewer. Now I am just requiring for my own papers that the institution (CNRS, editors, …) signs the report. The problem remains identical: no *a posteriori* reviewer {see discussions with J.Villain, with O.Pironeau}. If I ask a colleague to review a paper that I am writing, no problem, he/she will do it efficiently. But writing criticisms which become published looks a much harder task. It is quite strange, since book reviews are often published.

So this should be the normal task for scientists; it is what is done for PhD thesis, for example…; what can it mean? *Answer 1:* Perhaps that no one wants to take care of non reviewed papers; but why? Since as any professional, any scientist has to know what's happening, what's important? It is quite his problem if he is loose time not knowing a phenomenanon… *Answer 2*: absence of criticism does not mean "no reading", but it can mean "no quoting". This is quite forbidden, but who cares nowaday? Furthermore, the scientist can tell his disagreement for the editorial method. And anyone to applaud to the unfair practice… Except that science comes out.

The problem for an *a posteriori* peer reviewing edition is that **the Journal can not prove the existence of the reviewing process,** except when publishing discussions**.** But is it really important? How does any peer review journal prove the efficiency of the reviewing? Just because it claims "it is peer reviewed". Are the papers improved? Perhaps from their older shape, but after few editors….? This debate could last quite long, and I know some real counter-examples… The only correct way a reader shall chose is to read papers in different journals, check for their clarity, validity, cleverness, brightness…, and compare the journals and their qualities. This will allow to check *a priori* against *a posteriori* reviewing, ….

This was my aim in 1999 with *Poudres & Grains*, see [Tem#1 p165,, 168-171 191, 196-199, 204-230], [Tem#2 p. 8-73, 73-79, 104-66, 166-70, 171-180188-200, 200-3, 206-32, 232-85] or read my other cnrs-reports. But scientific organizations did not help testing this new method, neither CNRS, nor ECP, nor CNES, nor AERES, nor ANR, nor COMETS, nor Académie des sciences. For me, P&G could look as a fruitfull tool, which allows scientist to claim what they look interesting, against the scientist community which does not like attesting thye efficiency of new theories or ideas too quickly. So, it is just what needs science research at the moment.

The previous argumentation on reviewing demonstrates only but definitively that reviewing helps writers to be free to publish, because it asserts that community checked already what they said; but does not help them to be sure of what they write; the authors seems even not caring this point: publish or perish is the new trick; this is what managers taught to the authors; and this was done too well.

In research area, this last idea shall be considered as the contrary to what shall be done for evaluation because research is at the limit of unknown; building scientific realities from unknown is quite hard, we know that from the past, much harder than building up philosophy or religion: Obviously research evaluation starts as (i) evaluating efficient brain storming, but it shall continue with (ii) validation of scientific research comparing it with realness to validate the result. We all know that brainstorming with very little constraint, may be dangerous for non educated persons, which are the case of scientists coming from other domains, and managers... The aim of managers and teachers is to know what can be used and taughed essentially, and what new comers should know to understand new field. This looks quite too much, and it is…. But not knowing what to teach can be crazy, not telling what to teach can be crazy. These are the main dangers of scientific society: loosing its know-how, its safety rules and its languages.

C.3. Correct evaluation of research:

In Annexes G.1 & G.2, I join the English translation of 2 letters (on 2002 and 2004) which describe similar questioning, which is then complementary to what is written below.

There are probably different answers to the "efficiency" of peer reviewing. It may depend first on the side (the rewiewers or the rewied person) which I tell:

1) From the reviewer point of view, one can give advice, correct one's article, improve the understanding, get informed fast; this allows the reviewer to get an improved vision of him...

2) One can be granted and funded through this reviewing.

3) One can improve one’s English or learn how to make sentences.... (after few articles and few referring processes).

4) For both sides, some error will not propagate, be displayed,… the transferring process is improved; one can discuss and find an interested reader/writer

It is not understandable however that a good reason at works in scientific communication would be:

5) Giving good results to everybody and helping everobody understanding them, without any outcome. This looks completely stupid....

.... But point (5) is just how science works! And this is why science worker needs ethic and luck. We can help one another to write correctly, but is it really the ethic of science? Yes and no. Yes if, and only if, the guy who you help is at the right level; otherwise the person may become a peer reviewer, and will look brighter than he is; and the system fails.

In the past, article, once rejected, should not be resubmitted.... The rule seems not valid any more, so that most papers are published, few of them after few reviewing processes and few submissions to different journals... Do the guys who try to publish them look serious? Nobody knows.

Is peer reviewed literature easier to read? This is not obvious, since it uses condensed writing. If one compare with papers published in Poudres & Grains, or elsewhere, I am not sure.

Better, as scientific literature is produced by scientific workers, who work in institute mainly…, these workers can get private referees in the same institute. And editors could ask them for scientific advice prior to submission…. Is it done?

Does scientific literature contain fewer flaws than *a posteriori* reviewing literature, probably/perhaps not…. How these flaws interfere with understanding?

This is why I have tried the technique of **a posteriori** peer reviewing since 1998, using the possibility of new edition techniques, using web, to amend the papers if necessary.

Also, at the time when this question happened, I did not feel any differences between those of my papers which were rejected from the other, except perhaps for a question of clarity and of debate: the ones which were rejected were displaying simple results, arguing within simple words some contradiction with the actual debate (see my[16] [Tem #1, p. 12-29, 40-71, 72 & 218-230, 88-117, 118-123,…] at Conseil Laboratoire subheading.

C.4. *About the validity of Poudres & Grains articles*

My demand of publishing through *P&G* was not considered as correct by many different research managing committees. These committees (such as Commission CNRS, CNRS editor, COMETS, Academie des sciences, AERES…) never tried to help me fighting against bias peer reviewing; they never tried to force any ethic gentleman agreement.

- As I told it already, I asked few times for some *a posteriori* reviewing to CNRS and lab, editors, ….
- I reported these correspondences and facts in [Tem #2, p. …] [16] on 16 December 16, 2011.

[16] Tem #1,2,or 3 can be found on intranet of MSSMat at http://archive.mssmat.ecp.fr/ at Conseil Lab. subheading at dates : 23/6/2011, 16Dec11, with password to be asked to lab director.

► I asked also scientists reading some of the papers (for instance P&G ns1, by Biarez, Darve, and different persons in the lab…). I sent most papers to P.G. de Gennes.

This is perhaps linked to the field of Physics and mechanics of granular matter, which is an old field from the point of view of engineers but which is a "new field" for physicists (to whom I belong). This community grew up fast at the end of eighties and I left it for learning more on techniques in the 90-92. Due to this, I had to fight some wrong understanding very rapidly {such as in P&G **7**,1 (1999), or as in P&G 19, 17-18 (2011)} and to introduce new model coming from engineering to the physics community {see *Phys. Rev.* **A 43** , 2720, (1991); P&G **6**, 1-9 & 10-16, (1999), P&G **9**, 13-19 & 10-16, (1999), P&G **11**, 58-59 (2000), …} or conversely { P&G **ns1**, (2000), P&G **9**, 13-19 & 10-16, (1999) ,…}… Please also take a look at P&G **19**,12-6 (2011) to understand better.

► So as I told it already,
(i) Most of these P&G papers have been discussed in meetings.
(ii) No body wrote to me to discuss any of them,
(iii) I published few papers to overpass incompetent peer review processes (unfortunately, there is no possibility to publish private correspondence so that I could not publish the report, but the authorized person can take a look at my [Tem#1] [16], where these are given.
(iv) One{P&G **13**, 40-73, (2002)} of it was published later, after English translation, into in a scientific book, with coworkers.
(v) One was written to evaluate an NSF proposal {P&G **12**, 122-150, (2001)}.
(vi) Recently (1994-…), I turned to the granular gas problem which I found to be ill treated by theorists (from my point of view). I declare it already in a meeting (IUF, Paris, 2004). What is fun is that no scientist discuss the problem. They ignore it, as if it was a correct use in science.

To oversimplify the position of scientific organisms, they seem to consider peer reviewing as a systematic approach which allows to earn time and which looks an objective mean to check quality. In fact the quality of peer reviewing depends of a series of conditions, such as the quality of the peer, his ability to treat the topics he has to… and the difficulty of the problem. These cannot be considered as uniform in each field… and not to depend on difficulty of finding solution. Hence peer reviewing is always subjective.

Correct solution of this evaluation is found safer when the problem is reversed: as scientists need good result and understanding, they have to repeat systematically reviewing of the articles they used, discussions with authors shall be encouraged as soon as readers do notunderstand arguments, so systematic and repeated evaluations shall be systematically used to limit the possibility of error. It is the only way to be safe.

Let us imagine a peer reviewer who is asked to evaluate some proposal and who will decide of it using other peer reviewed works, this peer will conclude for the worst problematics. But what will be the chance of success of such an ill-posed set of questioning? On the other hand, if the peere chooses the simplest one, he will not know even wether it is already solved or not!

In research a user has to be a peer and has to understand and to be able to discuss the results from others. It is the main way not to loose much time. Hence one shall conclude i) “Not to refer” previous works, (ii) not to discuss them, (iii) not evaluate them, (iv) not to tell the flaws,…, this is just ridiculous.

Assume now that you are a scientist who finds some mistake in *a posteriori* peer review journal such as P&G. What will be your gain not to tell it? None except perhaps if you are the author. And if you are the author, where is the problem to declare the error and to continue on correct issue.

C.5. Conclusion : since (i) P&G was subject to systematic controversial criticisms by different scientific committes, and because (ii) when I asked few times a series of peers to review the papers in P&G (including the one from the committees) and that no one declares important mistake, this demonstrates that such a mistake has not been found at that time. (Of course, if I read something wrong in P&G ns2 and ns3, which I am not the author, I would have told it, simply to try to make it corrected and to help understanding others...; and if I see something wrong from my own paper, I will correct it).

We need to discuss the interest for the classic edition of scientific research. It seems this traditional way is safer, using the a priori peer reviewing process. But we will seee cases where this is not true.

Furthermore traditionally, scientific research is made of test-and-try process, which cannot be right at the first time most often, and which has to be tested and discussed by the whole scientific community. It seems to me that this second part of this process is not well formalised in nowadays peer-reviewing.

Part D

Few Flaws on peer reviewing, in edition

Part D. Few Flaws on peer reviewing, in edition (1)

Before coming to the description of few flaws in “a priori Peer reviewing”, let me first stress the difficulty (i) to make these flaws recognized by the editors themselves, (ii) to advertise other scientists of these flaws, (iii) to be able to demonstrate some error in the literature, when this demonstration would need complete discussion.

As a matter of fact, one can find in scientific literature many cases of good scientists declaring that they could have never published some of their important work, since its publication occured via creation of a new Journal editorial board and a new Journal.

On the other hand, one can find also advices of good scientist who say that most articles in peer review journals are not correct, because they are new…

- How to publish referee reports ?

An other point is the difficulty to publish referee reports: as I told already, a major problem for P&G edition is to demonstrate the validity of the papers, since nobody seems wanting to write scientific comment on it. So I decided to send one paper for reviewing to a conference-journal, editing papers after Peer Reviewing, to edit the comment. But the law protecting the private correspondence[17] is so strong that I had to impose the following tricks: First the paper for which I asked the review was in French; I translated it “automatically” for the reviewing, then I did not mention any Journal/or/editor/or/referee names; I translated “automatically” the review from English to French; I introduced some minor differences and published the new text with my answers at P&G 19, 5-11 (2011).

Little after, I tried to get this comments accessible on ArXiv. ArXiv did not accept because I mention explicitly a paraphrase telling refereeing process or referee report. I will try another way.
Please let me know any idea.

D.1. Labelling:

I come now to practical cases of evaluating the efficiency of *a priori* Peer Review edition. I start with few cases which are not taken from my own articles. I hope my remarks will not stress anyone. This is not my purpose at all; and the cases are relatively old. Each case will be labelled with an F « n » with « n » a number defining the order.

[17] see for instance the 2000 judgment at the TGI-17th correctional court in Paris, France, at http://www.lifl.fr/~ryl/ens/MasterTIIR/documents/CNRS_jurisprudence.pdf . This was for electronic mailing.

D.2. About my own reviewing of papers (or proposals) from others:

As any scientist I had to review papers or proposals proposed at Journals and/or at funding instances/agencies. Two or three examples are described here after; my legitimity may be discussed perhaps on a deontology ground; I hoped not.

►F1 [Tem#1, pp 4-8]: In 1997-98, I had to review a paper on vibrated granular bed for Phys Rev Let., by B.Thomas & A.M.Squire (PRL **81**,574, (1998). The paper was quite nice; it reported on old (i.e. well before my own work in 1988) experiments of the authors using a fast camera; but the second referee tried to ask too many modifications to reject the paper (in particular asking too many photos).

On the Appeal to editor, the third referee (PG de Gennes) agreed with my position and accepted the paper, writing severe sentence on the second referee comments. In a normal peer reviewing process, such problems should be avoided and should lead to official conclusion/remarks to referees.

Did the PRL editors decided not to use anymore the second reviewer? (Let me think no).

►F2 [Tem #4, p.10; point 1.a]: In 1994, appeared in Nature (**374**, 39-41) a very nice paper on segregation in flows in granular matter entitled "Avalanche mixing of granular solids", by G. Metcalfe, T. Shinbrot, J. J. McCarthy & J. M. Ottino[18]. After reading, I did not understand a sentence which should explained why there were using avalanches in a rotating drum. As we planned to study segregation with a PhD student, I asked the question directly to one of the authors, who answer immediately the right information: Mixing in permanent steady flow can happen only in 3d not in 2d, as everybody knows from (i) the parallel between the equations of 2d Hamiltonian problem and of 2d steady flow, and (ii) the law of turbulent mixing and chaotic motion, (iii) the author wrote the equations and mentioned references (probably already included in the paper). This was quite fair, and allowed me to strengthen our goal to work in 3d mixing and 3d segregation and to study Turbula….

I tried to explain the problem in seminars, but Nature is so famous that I was considered as a troublemaker. Nevertheless, ten years after a scientific study demonstrated unambiguously the existence of regions with slow dynamics mixing near the non moving walls of an agitated container (*Phy. Rev. Let.* **99**, 114501, 2007). A result that any cook (or concrete maker) knows: one needs to scrape the cream or the paste near the bowl walls in order to get homogeneous paste fast. Nevertheless the thesis is quite interesting and the PhD work is beautiful; and it shows that good science concludes similarly when experiments are carried carefully.

But 10 years to admit these points, "Simply" because of a wrong advertisement in a famous journal, this is paid quite expensively, I believe.

[18] I learnt after that the group was quite well known for their studies on « chemical mixing » in liquids

So the question is:
Can one accepts that scientific literature hides recent knowledge in order to make the paper accepted? It is obvious that the referee did not understand the true story: if they did, (i) they should have rejected the paper, and/or (ii) have asked for changes to make the story much more pedagogical.

It shows also the difficulty of advertising/teaching a research domain from another one. The scientists who published Nature (**374**, 39-41) wanted simply to advertise that they knew some more than what other "normal" papers on segregation in granular matter contained; so they try to transfer their knowledge to the "new" domain. But they did not succeed really, or this lasted more than 10 years.

►F3 [Tem#1, pp 9-10 & 231-234]: The third case happened in Nature around 1997 on the same topic (segregation in piles). The paper was right except the following assertions: (i) the problem has been known only recently, (ii) no solution was proposed already (these are needed arguments to be published in Nature!).
I wrote a letter to the editor for misinformation, telling that (i) the problem has been known for 50 years, that (ii) a similar solution was given about 10 years ago, that (iii) referee did not make their jobs, that (iv) misinformation was also an editor problem through the management of referee pool.

I got no excuse from the editor who proposed to me to rewrite/resend my letter to the author, who was a nice guy. I replied it was not my problem but his own one. Nothing happened.

More than ten years later, I asked Nature to publish this correspondence on the web. No positive answer.
Copies of the documents are available in [Tem#1, p. 9-10 & 231-234] (see above); they were also included in my 2009-10 CNRS report.

Against papers containing bad results or incorrect conclusions:

Other reviewing problems happened recently, caused by lobbying. This is the effect I encountered with granular gas, where most papers do not treat correct problems or did not argue taking into account realness, or because the argumentation focus on some understanding which should be better taken as wrong. I will discuss later these cases in the next subsection.

However, this happened earlier to me also, when I was studying naphthalene H_8 luminscence[19] early in the 80's, but the discussion stopped after

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[19] **Naphthalene research field:** This was in the time of my second thesis (doctorat d'état) in the early eighties (1980-84). I was interested in studying trapping processes and the transfer to other impurities of an exited states belonging to a given impurity contained in crystalline solids: how does this proceed? Can it jump from impurity to impurity…

1984-87: at that time, Kopelmann and Klafter were discussing together on the possibility of explaining steady luminescence data of naphthalene H_8 doping naphthalene D_8 crystals, obtained in continuuous excitation. Was it a possible Anderson transition (Klafter) or a percolation problem (Kopelmann); similar discussions happened once a year at least in Phys Rev Lett. each, plus other papers, without so much new.

The discussion stopped (i) when I used time resolved experiments and (ii) when I demonstrated that time resolved phosphorescence and fluorescence was obeying a percolation model, for which I measured the spectral dimension $\underline{\underline{d}}$, (iii) when I derived complete calculation from the real system with the correct diagonal disorder, and shew it was not able to enhance a localisation/delocalisation transition of the Anderson type, (iv) when I derived complete set of transfer times between naphthalene H_8 as a function of their distance and I showed that they

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The trick consisted in exciting the impurity at some time by a pulsed laser, and to study the time dependence of the different emission lines (i.e. emitted at different frequencies). At that time a debate existed in perdeuterated naphthalene (Nd_8) crystals doped with perhydrogenated naphthalene (Nh_8) impurities (naphthalene is the addition of two benzene molecules having a common side). The studied excited state was a long-living phosphorescent triplet state when the impurities were in the excitated states at low concentration. Increasing the impurity concentration makes the transfer from impurity to impurity possible; then the excited states could migrate among Nh_8 ; so they could migrate till (i) they reach a lower-energy trap (where it staid trapped (at a similar level since experiments were at low enough temperature), or (ii) till they could find a second excited triplet state, with which they coalesce, generating a singlet state that luminesces at once. The question was to determine whether the excitation could migrate through an Anderson transition (a quantum process) or through a percolation process (a jump process using a tunnel effect in the present case). My experiment allowed raising the indeterminacy by measuring directly the spectral exponent of the 2d percolation through the time dependence of the singlet state, as mentioned in the text. I found $\underline{\underline{d}}$ =4/3 after different corrections due to existing scaling due to sfinite clusters.

To get more precise, the reactions of trapping and coalescence depends on the migration through the Nh_8 naphthalene molecules via tunneling effects (already said); this tunneling depends exponentially on the distance, so that a cut off of the jump length exists, that depends on the time range investigated; this select the percolation model with the adequate Nh_8-Nh_8 distance of jump; this maximum distance l_j for a jump fixes the bounding at a given range of time $1/\tau$; this bounding fixes the concentration at which percolation problem is generated, and the fractal nature of the Nh_8 clusters at some time range τ, ie between τ to 100τ or so. The time dependences of the "chemical" reactions (trapping or coalescence) depend directly on the migration into these Nh_8 clusters. For instance, the time dependence of the Triplet emission measures the number of remaining excited states at time t after the laser pulse (in the tested sample), and the Singlet one measures the number of Triplet-Triplet collisions at this given time t, which is directly proportional to the derivative (i.e. variation) of the number N(t) of visited molecules. It is known that in classical motion the states diffuse; so N(t) grows linearly in normal space dimension, but it grows at $t^{(\underline{\underline{d}}/2)}$ in fractal dimension, where $\underline{\underline{d}}$ is the spectral dimension that R.Orbach and S.Alexander were calculating.

Surprisingly, R.Orbach was in sabbatical stay in the office next to mine at ESPCI at that time, and S.Alexander has been visiting P.G. de Gennes often also...I did spoke with Ray few times of my finding, but no understanding happened. (Ray funded my travel in America to participate to a series of US-Canada meetings in Sept. 1982 to get better informed). The understanding happened at least 6 months after his departure, after a personal discussion with P.G. de Gennes.

Question to research managers: how can they predict this space-time process to happen?

correspond to the correct order of magnitude of the jump times {P. Evesque, thèse d'Etat, Univ Paris VI, 1983)}.

►F4 : About the criticism I wrote in **P&G 19**, 17-18 (2011) & [Tem #2].

I do not know whether the forpms used in these criticisms are correct. I believe so. Anyhow the criticisms are right and the behavior of the authors is not well stated deontologically or scientifically:

For instance, at Powders & Grains 1997, where we had to discuss the BCCW model (see ►F14,) I planned also to studied the stress distributions at the bottom of a pile, to show and measure its dependence on the building process, to compare it to predictions from computer simulation.. [see *Powders & Grains 97*, pp.295-298, (Balkema 1997)]. This program was achieved [see *Chaos* **9**, 523-543 (1999), P&G 12 (5), 83-102 (2001) (see note 4, 5 & 7), S.Bouffellouh PhD thesis ECP, (2000)]. The results were presented systematically at GDR MIDI in Paris where the authors of *Les milieux granulaires ; Entre fluide et solide*" by B.Andreotti, Y. Forterre et O. Pouliquen cnrs press, Paris, 2011 were present.

Furthermore, these authors never quoted these papers, nor my other papers on granular gas. But one of them was asked to **review my CNES proposal**… This is more than a mistake from the different involved administrations (ECP, CNES, CRS, ESA) see Tem#2, 8-11, 27, 28-34, 55-72, 73-79, 79-103,104-165, 166-170,171-180, 181-183, 203-205, 206-225].

►F5 [Tem#2 pp.132-136 & 137-165]: with P&G **12** 122-150 (2001) or [Tem#2 pp. 137-165]: P. Evesque: Macroscopic Continuous Approach versus Discrete Approach, Fluctuations, criticality and SOC. A state of the question based on articles in Powders & Grains 2001

Since NSF asked me to review a proposal of by Bob Behringer, I wrote first the paper [Tem#2 pp. 137-165] to settle my advice scientifically [Tem#2 p.135], and used the paper after as a reference in the review. I do not know the strict correctness of the process, but I believe it is ok. At that time, I was working in collaboration with ESA and CNES for space experiments on both liquid and Granular Gas generated by shaking; but this was not the topics of the proposal. Furthermore, owing to the conclusion of my paper, I proposed to M.Sperl to set their experiment in microgravity, with ESA and to join my proposal; this happened around 2008 or so, when research on granular materials became sparsely funded by NASA.

The problem is not so much the team funding, but how much energy and how much money to put in the project, or what is the same, to get the conclusion as fast as possible, so to ask the team to do things right. This seems not true anymore due to lobbying, which push hard for funding lobbies only. So the new team wanted to get most of the funding, and fought, with other partners, to minimize our own results, with the help of ESA and CNES.

Obviously, Bob Behringer[20] has known this P&G paper [Tem#2 pp. 137-165] since 2001. It contradicts his assumptions; nevertheless the paper is never quoted as if he had missed it. This is not fair, at least.

Anyway, it will be hard to find interesting results on stress propagation, even in microgravity, since the experimental results from 2001-2010 literature did confirm the conclusions of my 2001 paper, where I mentionned already most of the statement…

►F6 [Tem#2, pp.137-165]: review of S.Luding proposal to NL space agency

I used also this 2001 paper [Tem#2, pp. 137-165; P&G **12** 122-150 (2001)] for another review in 2010 asked by a European country (NL) working with ESA. It concerns S.Lüding's proposal and the review is developped in [Tem#2, pp. 132-136].

I shall stress that the correctness of conclusions of research papers is not granted, of course. But, on the contrary, when the conclusions are few times contradicted by experiments systematically, these conclusions might probably be wrong or unlikely. Funding agencies should know this, and should analyse the papers on which the proposals refer; this should be the reviewer work, controlled by the managers. This is not at all the case. Managers will notice that they get no time to do this control; eveybody knows it, knows also that reports are not so well understood… and everyone uses it. This is why Coue method is usefull even in the scientific domains nowadays.

It is quite strange to read papers/proposals where the authors (or any other scientist) have never found what they ask for as an argument to conclude for continuing their research[21]. Such topics will get a happy end only if one should get the result, or because the result use can make earning much money….. So to these scientists, please accept that science is not philosophy: we need to check reality to tell us what is obtained, what is the truth and how to proceed in the right way. No exception, only count on your luck in chosing adequate test to find new things.

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[20] **Research on granular media:** I have known Bob Behringer since long. I went and visited him during 1992 summer, when I had to leave early for a family reason. At that time, I was investigating stress-distributions structure in granular materials, with a French geo-group and mechanics specialists. I stopped investigating this field around 1994-97, a little after Powders & Grains meeting in Birmingham (1993), when I understood, that (i) good mechanics specialists (Dantu, Josselin de Jong & Allersma) had already studied carefully the micro-macro passage via birefringence in 1965-85 and they had concluded to the efficiency of macroscopic view point at a scale larger than few grains, (ii) that most good soil mechanics specialists (Biarez, Cambou, Luong, Schofield, Wroth, Jenkins, Savage,..) told the same story, i.e. the same experimental macroscopic behaviors for granular materials and soils. However, a physicist (as I) may find difficult to understand this point, because of the differences of languages used by each of them during internal discussions and/or because of the different theoretical view points they used for external discussions, i.e. with physicists …. Also (iii) I asked in 1990 H.G.B. Allersma to join the expert group I was creating at ESA on "microgravity and granula matter" ; this allowed me to discuss this topics with him few times.

[21] This might explain the difficulty for such scientists to succeed in asking for NASA funding and experiments.

In the same way, scientific edition shall analyze correctly what is right and wrong in science to edit the best. This may help generating good science. This has to avoid lobbying and disregard money gain/cost rating. This shall become more difficult with the increase of lobbies.

As a matter of fact, Luding's proposal could have got highly rated because it contained most of the rating needs: famous collaborations, good quotations, noise interest… But it does not look as a good one to me (see my arguments in Tem#2, pp. 132-136, …). This requires probably discussing also new rating system.

By the way, S. Luding knows my paper which contradicts the assumptions made by his team; nevertheless this paper is never quoted as if they could have missed it. This is not fair, at least.

To the worst situations, with incorrect scientific ethic:

Due to the lobbying process, technics of administrative management are encouraged. It will become more important to continue a process rather than ending it because of scientific conclusion. So, scientists will be encouraged to propose papers which does not make the data clear enough, or which will conclude on opposite direction than the one they gets. They may claim some agreement with some other result even if it contradicts it, instead of arguing against it. Within the three above examples (►F4, ►F5, ►F6) I tried to show that writing correct publication should restore truth.

However, this would need also that truth be quoted, and not only the wrong papers. This is not the case in (►F4, ►F5, ►F6). To give the right "solution" remains an efficient way to get good and efficient documentation, but it needs that readers are correct scientists to discriminate good papers. In principle, this is the problem of staff management, of editors and of researchers. But the number of papers is so large now, that it becomes quite difficult to select good ones only, or even good ones among others {see P&G**20**,1-28 (2012)}. The larger the scope of the different issues (or of the goals) is, the worse the efficiency of the selection.

To limit the effect of wrong publication, the comment is a correct answer most of the times, when lobbying permits. But this cannot always be done if the lobbying circuit is well addressed (I should say badly addressed).

I have encountered such a quite bad situation. It is linked also to previous affairs (►F4, ►F5, ►F6) [22]; it is the situation with the VIP-Gran and Dynagran

[22] **Scope on situation about Dynagran and Vip-Gran in 2011-2012:** Since the nineties I have been working on granular matter, on its behaviour subjected to vibration or to periodic quasitatic forcing, on its stress/force distribution, on the micro-macro passage in this material, on quasi-static stress-strain relationship. My personal vision of the field is described in Poudres & Grains mainly, because of the strong lobby in classic journals (see first part of Tem#4, & ►F4-►F6). I turned mainly to granular gas in 0g, where I have been funded by ESA since 1990 and by CNES (since 1998 about), benefitting of sounding rockets (MiniTexus 5 (1998), Maxus 3 (2003), Maxus 7 (2006)), benefitting also of parabola flights in Airbus A300-0g (since 2000) and of collaboration with China (satellite SJ-8 (2006)).

experiments, for which the lobbying is so strong that it does not (i) want to discuss, (ii) this can make the strategy of disinformation working efficiently, (iii) the authors are bright enough to hide the flaws in their own argumentations and results.

The only possible strategy in such a case is the forcing to get discussion upon real data from the authors themselves in order to force the discussion in a more detailled manner. This needs two distict actions: i) forcing to get data; (ii) forcing to get discussion. This is exactly what I am trying to do with the next example. Forcing the authors to give their data can be obtained from correct editor, to whom one asks to apply the 2007-Brussells declaration on STM edition, as exemplified below. But this seems not to be always the case (see below also):

►F7 [Tem#2 pp22-26, 30; Tem #3, pp.122-126]: E. Opsomer, F. Ludewig and N. Vandewalle:PHYSICAL REVIEW E 84, 051306 (2011)

The situation was as follows[23]: I got my experimental CNES proposal reviewed by uncorrect peers working partly with Vandewalle [Tem#2, p 8-11]. This one was also "collaborating" with me on another space experiment (VIP-Gran) sponsored by ESA. The peers focussed in Nov 2010 [Tem#2, p 8-11], on a futur paper of Vandewalle Team and told that I was wrong in my theoretical understanding and I was not efficient in developping the experiment.

The next experiments should be Dynagran in the Chinese satellite SJ-10 (experiment built by CNES and flown by China) and VIP-Gran (funded by ESA, in ISS). Till 2000, my work has been finding some problem with the boundary conditions of vibrated granular gas that become quite significant after 2004 (as I declared it in a IUF meeting in Paris). However, it needed me 5 more years to localise the problem (i.e. 2009) and 2-to 3 years more (i.e. 2012) to anlayse its link with the dissipative process induced by collision completely. But I have been quite sure of this effect since 2004, and have claimed it in different meeting, arguing that it was cancelling most of the results on granular gas, trying to find some help from other teams…. I understood after 2009 that most of my colleagues could not accept this idea because it cancels 15 years of work. Even in my team, some compromise was asked to get easier funding. Hopefully my Chinese colleague was interested still in collaborating and sent me students with whom (i) I could simulate (R.Liu) the granular-gas system and define the problems (2009); then (ii) I could get correct experiment and analyse them with a second student (YP Chen) (2011).

But the worm was in the fruit already: in the midtime (2008) I asked other European teams to join VIP-Gran TT, among them: {i)N.Vandewalle from Belgium who was colaborating with S.Fauve, E.Falcon (Paris), H. Hermann (CH), E.Clement, ii) M.Sperl from Germany, who was collaborating with RP Behringer (USA), E.Clement (Paris) and S.Luding (NL)} to join our ESA VIP-Gran experiment and Topical team (TT). In principle, there should not be any problem between such a large number of teams if scientific deontology is respected; however it was not completely true as I mentionned already (see first part on P&G of Tem#4, & ►F4-►F6). (End in next note)

[23] Each year my cnes proposal has been systematicaly attacked since 2008 bry French reviewers, asking me for articles in peer review journals, as if the « reviewers » did not understand the underlying physics. Funding became harder at that time too, making colleagues greadier. I asked for help from CNRS, CNES first….see [Tem#1, p161-272, 196-199, 201-202].

At the end of 2010, an evaluation was asked by CNES HQ to allow funding of dynagran experiments. This started with a meeting, see [Tem#2, p 8-11], between the scientific team (Garrabos and I) and "experts" who wanted to be funded and who defended wrong results on Nov 2010.

I wrote at once to Vandewalle to get informed. The paper was not ready and was sent to me 4 months after, and it got published a year after…. Besides, I asked for his paper to Vandewalle early in December 2010, using also help of ESA [Tem#2, p 12-21]; then I asked for a phone call discussion with 3 of us, i.e. with Vandewalle, ESa and I, to testify the agreement. This call happens around March 2nd, 2011with written conclusion [Tem#2, p 22-26]. However their results were described in different seminars and conferences where I had no access, with no access to power points….

I got the paper at the end of March. The simulations data were briefly reported so that it was quite hard to understand clearly the finding. The authors did not want to discuss… It seemed that our paper (E.Falcon et al. PhysRevLett 83, 440 (1999)) was quite nicely reproduced. This was surprising mean quite a lot owing to the questions I asked on our work (see P&G). I could not discuss further with this team in spite of different attempts during meetings [Tem#2, p 27 & 130] and through Topical Teams [Tem#2, p 104-165]. ESA has been fair enough to terstify these facts and to display my complete report on its intranet web site (ftp://msmftp.estec.esa.int, with username vipgran and password vipgran, see meeting 2011-09-22; or ftp://vipgran@ msmftp.estec.esa.int , with password vipgran).

I tried to get some help from local CNRS another time, without success too, see [Tem#2, p 28-36]. Similar answer from the space committee of the French Academy of science [Tem#2, p 73-78, 166-170] , from the military security [Tem#2, p 79-103] , from a CNRS editor, who was also in charge of the COMETS (ethic commission at CNRS) [Tem#2, p 171-180, 200-202, 206-224;], also from AERES [Tem#5, pxx] and from European Commission of research [Tem#2, p 287-290; Tem#3 , 86-87; Tem#3, 98-112; Tem#4, p 98]. CNRS President did not answer even [Tem#2, p 181-183], and the direction of my lab never wanted to state such a problem [Tem#2, p 225-227] , nor any problem raised by P&G and rejected systematically any discussion of this kind... In such a way that I wrote in my office door: {*False scientist crosses your road. Here we accept only the scientists who observe the scientific ethics and who ask the others to respect it.*}

So, there is little to espect from managers, and organizations. But I will come back on these topics later. The problem I had to solve was to understand correctly what Vandewalle's team found, how their results could be integrated in my own knowledge, how much is correct and how much can be not so certain, before setting some global strategy…. So I needed getting their data.

The method I used was a regular *Appeal* to the (Phys Rev E) editor who has published the work in dec. 2011. It was Phys Rev. E. I asked for the communication of the simulations data corresponding to that precised paper by Vandewalle team, arguing that Phys Rev E signed the Brussells declaration on STM edition in 2007, between editors. One week later, the editor replied that he hurges the team to do so.

This, together with a similar help using my partnership with ESA, made the rest and gave me access to the data.

I could look to the data and understand clearly what the paper tells, under the lines. It was quite surprising and rather different from what I could read from the paper itself. I am ready to discuss with the team now: most of what is asserted in our Phys Rev Lett looks wrong when checking the data correctly, while the Vandewalle paper seems to affirm the contrary (everything looks right), but the worse is that nothing ill is really said. It means that the paper (i) does not transfer any correct information to reader, (ii) cannot be used to prepare new future experiment, (iii) needs to be clarified completely; at least this is my own feeling.

However, Phys-Rev editor couldnot push Vandewalle group further into the discussion, so that it will be difficult to show the problem in this publication: it is so "smartly" written that one needs to complete the understanding by questioning before demonstrating some error, or some misunderstanding.

In the mid time, the team wrote a second paper on the same field, but including also experiments at smaller amplitude of vibration. These ones should be easier to discuss, because of the smaller number of parameters to take into account. It appeared in a European conference paper in Journal of Physics: Conference Series. I tried to use similar argument with this European editor get the datas [E. Opsomer, F. Ludewig and N. Vandewalle: Journal of Physics: Conference Series 327 (2011) 012035]; the European publishing compagny refused helping and did not accept applying the Brussels declaration of 2007 on STM edition.

I asked already once more for discussion through ESA with the team. No answer. It remains the possibility to ask for US help furthermore….. Part of the story is recall in my Demande d'aide à la recherche 2012 mailed to CNES in recommended maner with documents [Tem#3, pp8-87]. Other documents are on the ESA site concerning TT Vip-Gran of the meeting [24].

Concluding this part:

Different can of counter-producing mistakes can be done by a priori peer reviewing journals or method. They go from mis-information [►F1, ►F5], hiding of real state of scientific background [►F2], and complete attempt to diinformation [►F7]. This will go worse and worse if the current status of non a posteriori reviewing of past publications is not decided, because of the converging interests of all the actors in the field: editors and scientific authors need more and more publications. Only science needs correct results, the others have understood how to proceed to be correctly judged by managers. And managers do not do their

[24] Web site of the TT Vip-Gran is now located at ESA, on : msmftp.estec.esa.int at vipgran ; password : vipgran ; (ftp://msmftp.estec.esa.int/Meeting 2011-09-22 Paris/; file: TT 22-9-11-Evesque-talk.pdf), containing : 5 powerpoint, paper : [poudres & grains **18** (1), 1-19 (2010)], with the [report on Informal VIP-Gran Topical Team Meeting 13/ 7/ 2011, Bonn], with [Report of S.F. Luding's project], with paper : [poudres & grains 12 (8), 122-150 (2001)]

correct jobs. But who care, at least not these last ones who do not know whom will judge them!!! So here is the **trick**, i.e. the **main flaw** of the system!

I tried to activite different circuit (physics community, Lab, CNRS, ECP, COMETS, Académie des Sciences, AERES, editors, research headquater at European Commission,.. but nothing happened in France. The management system wants not to know, to blind its head in sand soil as an ostrich, and tells everything "nice and perfect". This cannot be the way for research management, because it is a real war against the unknown, with very much danger. Such a management does not try to judge its enemy even …. And the enemy is quite powerful using any trick and sloth or laziness is the main one!!!

To conclude, I cannot believe such techniques could be taken as normal use and should not be observed by scientific students… without any response.

D.3. About my own rejected papers:

So, this part is devoted to discussing my own papers which have been rejected by editors. I will start with those of my previous field [►F8 - ►F11]: atomic, molecular or ionic impurities in crystals studied by time resolved spectroscopy at low temperature, and related issues. Some scientific introduction can be found in note [19]. Some introduction on the granular matter goals can be found in note [20] and [22]. Introduction to the role plaid by *Poudres & Grains* in my working protocole can be found in part 1 of this testimony (#4) and in note [10].

►F8 [Tem#1, pp 12-29]: Anderson Transition in systems with diagonal disorder viewed as an off-diagonal disorder problem: *not published*

The case of Nh_8 doping Nd_8 crystal is a case of isotopic doping where the energy positions (E_d , E_h) of the (Nh_8 , Nd_8) states depend on the nature of the molecule, but not the coupling (V) between them. At small Nh_8 concentration (Nh_8<15% as the problem appears), it occurs that the energy mismatch E_d-E_h between corresponding Nd_8 Nh_8 Triplet states is much larger than the coupling V so that one can use the initial perturbation method of Anderson (1958) to study the state distribution. As $V/(E_d-E_h)\approx 1/100$ is quite small [25], the convergency is quite fast: This coupling between two NH_8 depends on their distances via perturbation theory, as in Anderson paper, (and/or as in Feymann graphs). This coupling generates the splitting between the Nh_8 eigen states, so that it reduces the coupling with other (but further) Nh_8 impurities, so at larger different distance; this leads to eigen state localization at small concentration. Was this problem that I was studying, in the concentration range of 5 to 10%. So I proposed to discus correct

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[25] E_d-E_h≈100cm^{-1}, V≈1 cm^{-1}, in spectroscopist units, see P.Evesque, « Diffusion de l'énergie dans les systèmes désordonnés: application aux cristaux mixtes de naphtalène » (thèse d'Etat, Paris VI, 27/2/1984, in French) p.14, 15, and ref. there in.

calculation in the paper, concluding to the validity of percolation for Nh_8-Nd_8 system.

To my understanding, the scientific debate published in literature at this time (1980-1984), was generated by the confusion between Anderson problems with diagonal disorder and/or the ones with off-diagonal disorder: This was generated by forgetting the Nd_8, introducing instead the off-diagonal term between 2 Nh_8, with its natural dependance on the Nh_8-Nh_8 distance, omitting the existence of Nd_8 states, and the splitting the energy of the state due to the interactions…

So my paper tended to explain this point from a theoretical description. The paper was rejected because a referee told: this is obvious and well known, and right, it cannot be published. And the other said: it is wrong; it cannot be published.

After discussion with S. Alexander, I decided not to resend the paper. I was stating correctly the problem, and agreed with 1st referee. Why fighting against the refereeing rules that editor wants to apply? Further, most of the arguments are in my thesis [24]; so they were in the process to be published already.

A year after, Blumen and Zumoffen, to whom I spoke of my dilemma and gave my paper in between, proposed the same calculation; but they solve it numerically and got published.

Question: I do not understand (and I disagree with) this editor rule, which rejects systematically a paper when the two referees tell to reject it, without any consideration to the reasons.

►F9 [Tem#1, pp 30-39]: Comment to JChemPhys (1984) *not published*

When writing my thesis, I found some change in the time constants measured at different temperature and concentration. This was changing partly the results in one of my paper (JChemPhys **80**,3016 (1984)) on Napthalene Nh_8/Nd_8; I wanted to correct it. I wrote a comment to the Journal. It was not accepted, even if it was right. The main reason was that "other publications was already published (after my own) by others"…. (and some other reason). However, none of these papers did evaluate this effect …..

So this modified version of the theory should have been published, but has not been, mainly for no reason, against normal scientific protocol.

I decided not to resend the paper because most of the arguments were in my thesis [24] and I do not have to care about the absence of communication between actors of the scientific community linked to the action of those who are in charge of managing this communication. This is just not the right way! But I cannot do anything by myself without the help from other scientists. And these ones remain quiet, or use the rules to limitate competition. I am waiting, it will turn to become a real problem in our days, that will make scientists and editors reacting.

►F10 [Tem#4, p.10]: Diffusion de l'énergie dans les systèmes désordonnés : application aux cristaux mixtes de naphtalène", (Université Paris VI, 1984, in French)

The writing of this thesis happened after the acceptance of few of my papers in this study. This made the writing safer. However, this is always a long story of writing a thesis, with a lot of rewriting,…. At the end I submitted it to the review of 2 referees before the defense, as usual. They found few little flaws on (4) equations that I corrected using an errata page. I tell this story, just to let others understanding that perfect writing does not exist for me. 4 flaws were found how many are still not found? It means also that I did not know at that time, the normal rule of correct edition of thesis, which is to correct the proofs after the defense before printing (which I learn in my future labs). And my thesis remained as is, including the errata page.

As a matter of fact, for me this is not important, as far as correcions are told; my procedure testifies just that most human work contains errors which we have to avoide at best, but some of them will probably still remain.

►F11 [Tem#1, pp40-71] : Rotational relaxation of azobenzene in Vycor, submitted (1987) ; *published in J.Phys.C (1989)*

This paper was first submitted to J de Physique France, from where it was rejected. I believe also that it was almost the only article for which I wanted to force the publication by presenting it to another newspaper. This was not to deprive the students of their work because of an authority assessor, using few abusive arguments from two referees, who claimed not to publish. This is quite inadmissible! The paper was ok and the reviewers tried to abuse science, scientists and Journals.This example helped me dediding of little importance of some referee advice.

This ends up also the edition problem I met in my first domain of research (Optics, spectroscopic, disordered materials). As I told it, I had to introduce fractals in this part of my work to elucidate some peculiar behaviour, the use of which has spread over different chemical physics fields (chromatography, chemical reactions,…).

But concerning the use of fractals (percolation,…) to interpret behaviours in this first domain, I shall tell that if I introduced the idea itself (cf. fractal and Naphthalene Nh_8), I especially had to fight against some excess of its use (cf. my articles with M.A.E. El Sayed)!!!.

This is probably why I got criticised for the next works [►F9-►F11]. This is peculiarly true for [►F11], for which the second journal accepted it with little change.

►F12 [Tem#1, pp84-87]: Dynamical system theory of large deformation and pattern formation in non cohesive sand. *published as Phys. Lett. A173, 305-10, (1993)* The first reviewer said he was not really an expert but he « thought » that the paper is a reformulation of experimental results with new formulation in physics language and that it is not of the standard of the journal. The second agrees mainly with the first one.

i) I believe that such a judgment could apply to many short papers of this journal….

ii) the reformulation was enabling us to describe recent complex finding and to analyse them within the same scope as old ones, and this scope was new.

[iii) However, nowadays, (i.e. ten years later) I do not know if we were right using this theory or explanation, but this is another story….]

Anyway, the paper was resubmitted to Phys Lett., and was accepted for publication.

Next exemple is more complex, and its context has to be reminded:

►F13 [Tem#1, pp 73-82]: Comment on paper on finite size effect in avalanche (A comment to Phys Rev A43,7091 (1991) , *not published* except in P.Porion's PhD thesis

The whole story of this article started with the submission of two letters on Avalanche in a rotating drum, which I sent to PRL for review and acceptance; Referees asked the papers to be combined to get deeper and stronger view as a single paper to appear in Phys Rev A, with the original date of anteriority. I did accept to make the changes, and a paper was published (see *Phys. Rev.* **A 43** , 2720, (1991)). But I resubmitted at once the two previous letters to two different journals, where they appeared as single paper as *J. de Physique France* **51**, 2515-2520, (1990) and as *Europhys. Lett.* **14**, 427-432, (1991) (with their new anterioty dates).

A little after the publication of *Phys. Rev.* **A 43** , 2720, (1991), I was surprised that PR A published as a short publication also a paper by Nagel et al. (Phys Rev **A43**,7091 (1991)) explaining that avalanche of different sizes, obeying scaling, could happened as finite size effect in small piles, which was exactly the main part of my paper *Europhys. Lett.* **14**, 427-432, (1991). This part was becoming quite dilute in my *Phys. Rev.* **A 43** , 2720, (1991). So P.Porion and I try commenting, and we claimed for anteriority.

This comment was rejected by Phys Rev A, because no "claim of this kind was in used in the Journal". We never published the paper; it appeared in P.Porion's thesis, by part only. One can learn much more on avalanches in this thesis (see P.Porion, "Frottement solid et avalanches dans les matériaux granulaires", Université de Lille (1994).

Of course such kind of anecdote can happen erratically, without any flaw or real partiality; but I do not believe the right solution was chosen: keeping no body informed, except the teams in the confidence and the editor; and the argument used was not fair and should be counter balanced by a clear editorial rule. By the way, in the past, numerous theorems or laws were discovered simultaneously or more often independently by different scientists; and the names of both parties were associated. This is the realness, which is no more the right rule!

Moreover, it can be a scientific habit to use scientific names to name a discovery. It may honor personalities. But this cannot establish truly the discovery: for instance,

(i) The Navier-Stokes Equation does not mention the role plaid by Saint-Venant in the theory.

(ii) The Sun-planet description of Copernicus is better known than the one by Aristarque of Samos; nevertheless, Aristarque was the first of the two, and the two models are quite similar.

(iii) A theorem belongs to nobody. This is the legal law. It is so because it is considered as a permanent "truth"; it is different from a pattern.

(iv) Reformulating a "scientific evidence" (or a theorem) with other words is not a plagiat for litterature; whatever, it should be the same discovery for any scientist. However it might be useful if the demonstration is simpler or more pedagogical.

(v) Using the words "Einstein relativity" for special relativity is a habit; but Lorentz and Poincaré did some part of the job also, and Einstein's wife too. It is probably more evident even for the part of work she did in the discovery of the photo-electic effect, for which Einstein got the Nobel price….

So I am not sure it is so a nice habit to name the scientific rules, methods, theories or theorems,… from the name of the scientists who described them the firsts.

It is not the truth likely: this truth is at work now, as it was already at work before its "discovery"; it may have been found by somebody else before,...

Furthermore, science is not French administration, which (as I learned it) "does not lie when hiding some important fact or some hidden rule in the report" (in science, just not to mention a fact, is ill if it matters); to understand better what I am telling with this idea, let us assume that conclusion of the report of the French administration is ill, because it has not given an evidence which demonstrates the contrary of what it is proposed in the report. This is considered as correct by French administration. But this has not to be taken as true in scientific field.

Indeed, any evidence shall be included in the scientific solution, because this one shall apply universally; so the rule of French administration cannot apply to scientific domain. When one uses a name (of scientist) to shorten the concepts linked to a scientific rule, this can be adequate; but it may hide the complexity of the rule, and shrink the needed pre-requisits it needs for the rule to be true; such mistakes happen quite often in systematic errors, especially when two rules have to be applied successively, which exclude each other in the special case studied.

Thirdly, this rule may alter the personality of scientists, i.e. in France we say: “prendre la grosse tête” = becoming big headed, or “péter plus haut que son cul” = to crack higher than the bottom (?) .

Fourthly, it is not legal since theorem does not belong to anybody.

►F14 [Tem#1, pp72, & 218-230]: Next rejected paper concerned an experiment I achieved for starting a collaboration with JP Bouchaud, in order to test the so called BCCW theory. BCCW stands for Bouchaud, Claudin, Cates and Wittmer. These theorists were proposing at that time some simplified rules for stress propagation in granular media; and they were claiming to be universal.

I went with the experiment at the Powders & Grain meeting at Duke Univ. in 1997 and at the Granular session of ITP (Santa Barbara, 1997) to discuss the experiment and the theoretical hypotheses. The experiment demonstrates the hypotheses were wrong. But the conflict remains for a while after…. I was trying to publish the paper as is. But the criticisms were not fair and I published it in Poudres & Grains. I informed the CNRS of my position; I included the paper and its scientific report from the journal into my biennal “Compte-Rendu” to CNRS, with complete explanation of the reasons. Furthermore I mentioned the difficulty to affirm a position that few scientists do not like….

This paper and its defense forced me to decide of the transformation of ther P&G news bulletin into a scientific journal. I advertised everybody, starting with de Gennes and Guyon and CNRS Commission. I am happy to have done it. The testimony I am writing today is written mostly to justify my position. This position has been attacked many tmes by “committees” who have only the name of committees, since they cannot write in Poudres & Grains (or anywhere else) what they think wrong scientifically with this idea or what is wrong scientifically in P&G content.

Furthermore, these committees should be forbidden by the French law which states in the French constitution that the liberty of thinking is guaranteed for anybody which requires that anybody shall respect thought from others. I feel that such an absence of scientific discussion it reveals a state worse than contempt, since it is just affirming my non existence.

Through P&G articles, the scientific debates to which I participate were at least: (i) BCCW law is wrong, (ii) sound propagates in sand classically, (iii) no critical scaling of the force field in quasi-static mechanics, (iv) granular gas does not obey hydrodynamics (v) classic mechanical behaviour and jamming, (vi) parking theory and compactivity (for which I gave experiental proof very rearly) (vii) how to interpret macroscopic behaviour. None of them have been discussed.

To come back to ►F14 paper [Tem#1, pp72, & 218-230]: "Stress propagation in granular media: Breaking of any constitutive state equation relating local stresses together by a change of boundary conditions". It appeared as **P&G 7**, 1-18, (1999) ; it was discussed in these meetings with the theorists; it was sent to J de Physique France, and rejected due to the real "stress" imposed by the article to the reviewers and to the physics community, and their "fragility". (I do not believe that "fragility" of sand shall explain the results debated in this work)

I do not find acceptable from a peer review Journal the way it chosed to elude discussion, because Journal shall try to obey scientific criteria, and/or scientific deontology. Why not having published a mixing of the paper and of the review for instance, or the paper and a referee comment?

So I decided publishing my paper via P&G, and to make this journal changing of edition rules.

By the way, nobody told any word against the French Journal (J de Physique France)…

Worse, the response I received came from the whole French "scientific management authorities" which tended to manage this problem as if there were no problem (demonstrating their cultural "racism") . Indeed I felt a combined common response at the same time from the physicist community, from the scientific edition committee and from the French authorities of management of the CNRS research and of my lab; they tend to impose to me to accept paper rejection and the elusion of the debate.

As I told, this is not scientifically admissible, and this should never happen. It demonstrates the lack of seriousness of the French scientific organization and universities. This will become clearer when I will report on the application of deontology (see next part). This was also why I chose to transform the small letter of communication of the AEMMG into a scientific journal, the goal of which was to test "a posteriori peer reviewing". Since then I used essentially Poudres & Grains when I was publishing alone, except when I was tempting a comment.

►F15 [Tem#1, pp 88-117]: This was the case for the paper on « The jamming surface of granular matter Determined from soil mechanics results ». It was rejected mainly/likely because the authors were not able to read soil mechanics text book.

Hence it appeared in P&G **11**, pp58_59 (2000) because the arguments for rejection do not seem strong enough scientifically (and not valid) to me. One of these reasons of rejection was that the paper contained only detailing which did not need to be compared due to the importance of the questions raised in the initial paper.

I would tell to these guys that the difference between philosophy and correct science is just there: real physics or other science uses concepts tested on reality, which allows the concepts working in the correct given condition. On the other hand, one does not need to know when philosophical princinples has to work. (of course this shall still be better).

Physics take care of details: Newton tried to unify mechanics in his principia, using different details. It discussed the complexity of light color using details…

Is history cyclic, Is there a single economic theory, or few? How has been formed universe? Are just philosophical question (at the moment at least)…

►F16 [Tem#1, pp 124-134]: The second comment appeared as « Are Temperature and other Thermodynamics Variables efficient Concepts for describing Granular Gases and/or Flows? » in P&G 13, 20-26 (2002). It was submitted first to comment « Phys. Rev. Lett. 88, 64301 (2002) » and was rejected due to the argument presented in Tem#1, pp 124-134. I do not feel any valuable reason in the editor report. It is only repeating that it is not valuable.

►F18 [Tem#1, pp 118-123]: Paper published as P&G **12**, 115-121 (2001) was sent first for submission to a geomechanical journal; it came with too many comments and questions, and needed further explanation. This was done much later with a completely revised version, submitted elsewhere. (see reviewing Tem#1, pp 118-123). This new version appeared in International Journal for Numerical and Analytical methods in geomechanics [Int. J. Numer. Anal. Meth. Geomech. **28**, 501-530 (2004) 10:1002/nag350]. But we maintained also the first version of P&G **12**,.

This allows discussing an important problem in pluridiscplinary field. The main difficulty is the use of different languages that makes publishing harder: either old scientists use similar words in the two field with different approximations in each field… Or they may even not understand the concepts, or the experimental tests used…

The problem is not only linked to new comers, who bring their culture and try to use it in other context, or who try to understand with their words old results. Transdisciplinarity requires adaptation faculty from both communities to learn other concepts and methods..

And scientific journal has to stimulate these exchanges, even when the old scientists do not like it.

►F19 [Tem#1, pp153-158]: Two other papers of mine were rejected and published in ArXive by coworkers. These are Arxive :cond-matt/0611613 and ArXive :physics/0609204. I have not the reports. One can get some little more information in Tem#1, pp 135 and in Tem#1, pp 136-158. A longer version of paper Arxive :cond-matt/0611613 exists published as Microgravity Sci. technol. XVI-1, 280-284 (2005). A correspondence with P. Manneville about the second paper (Arxive :physics/0609204) exists in Tem#1, pp153-158.

They are better ways than peer review Journal to get informed judgement.

Conclusion on rejected papers from journals:

This ends up my own list of papers which was rejected; they are about 14. They have been **published elsewhere** most of the times, except for two or three of them, i.e. ►F8, ►F9. And the arguments used for rejection were never correct: Arguments using "pour English" was often used, but this is not fair, and I think it was more likely because the criticisms contained in the paper were too much undestandable. The reason why I never tried to publish the two or three rejected articles which are not published, was not scientific, nor English…It is just because I was thinking at that early time that edition system works correctly, which is not the case. To work correctly needs many further efforts, that editors do not want to imput. This is why the number of publications is increasing….

I found *Poudres & Grains* rather easy to settle and to edit. Its lack of "visibility" is linked to the administration process, and to the competition it induces, both in funding, in topics selection. These are not serious in a research domain: Asking scientists using lobbying is extraordinarly dangerous; but it is the way system works at the moment. But why is it so? the lack comes from a lack of correcteness for scientific readers abnd authors, who do not want to get right (scientifically) but right for the administatrion, which requires quoting peer review journals, supporting peer review journal and defining truth through a number of quotation. All these are **non sense, since "truth" means 1, "wrong" means 0, and everythingelse is I do not know**. And scientific review articles are "I do not know".

This is essentially why the system used by administration cannot work as a measure: the more uncertain, the more quoted and the more the number of papers; and the more difficult to evolve in an other direction. This is often not the right way neither to succeed to getr an answer, nor to know how much efforts need to be done, jor to facilitate a new concept to merge except if it comes from the leading group. This is a way where lobbying and "old field" plays the bad parts.

Part E

On deontology, scientific organisms
And scientific organisation

Part E. On deontology, scientific organisms And scientific organisation

Most of what I will say is on my tem #3 at CL MSSMat 13/3/2012, who is outlined in p.12. Other documents can be found in Tem#2 also, outlined in p.11-12.

Testimony on the possibilities of making apply and respect ethic and deontology rules in scientific works by our supervisors :

E.1. Introduction

This testimony is the third, probably the last one or the last but one, of a series of 3 (or 4). Two others are annexed as attachment to the reports of the councils[17] of the laboratory MSSMat of June, 2011 and December, 2011. They can be read by every approved person.

Other potential readers can make therequest to the director of the laboratory, or ask me for a copy the need. These previous testimonies deal for the one, i) of the dysfunctions that I noticed in the field of the scientific publishing, and show the interest to use different editorial conditions (for example as those of *Poudres & Grains*) of those who are collectively practised (that we often call " to peer review edition "). For other one, it shows ii) the work which I realized to defend the model of edition of *Poudres & Grains* s for 15 years.

It seems to me preferable to differentiate these two types of scientific publishings by their real characteristics and to call them: edition in reviewing « a priori » and Edition in reviewing « a posteriori », (for *Poudres & Grains*) because it is very there their real major difference.

The main problem met in my editorial approach is the lack of reports proposed to the editorial committee of *Poudres & Grains*, on already appeared articles. If this process worked the system would be validated. It is not thus of my will, to such a point that I suggested to a CNRS[26] publisher (president of the COMETS[27] besides) making proceed to an evaluation (in 2011), idem to the French Academy of Sciences[28] (in 2011), idem in the section concerned by the CNRS[1] (in 2003-5). In the editorial rules of *Poudres & Grains*, the condition is to publish the report, as an article, with the discussion which follows (at the need) as well as the name of the authors/ new writers. For the aforesaid cases, I accepted that names represent simply the organisms (cnrs1, cnrs2, Ac. Sc1, Ac. Sc2). I had no answer from the organisms.

I corrected the main errors that I found, that readers indicated to me, or that discussions after oral presentations at congresses allowed me to find. It is likely that certain articles could be improved, certain presuppositions could be clarified … But this would require readers' remarks …

Finally, we also notice that the number of authors following this new editorial concept is low. This is not probably the evil in my opinion, but rather the advantage. Whom I am interresting with my works ?

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[26] CNRS is the National Center for scientific research in France ;
[27] COMETS : Comité d'éthique du CNRS
[28] F Ac Sc : French Academy of Sciences ;
US Ac Sc : American Academy of Sciences

At the moment, it seems that my editorial work is vain, except for transmitting free of charge a knowledge (what is normal only if this knowledge is known and recognized). But why this "fiasco"?

In my opinion, it is especially because many authors are afraid of saying errors (but to formulate errors is not it regrettably frequent in research?), and that they come out. They thus prefer the « a priori » reviewing that proposesreviews. On one hand, it allows them to correct errors, to improve the text, to modify text-et-plan, before publication, and on the other hand to have the proof that the other scientists, the « very » specialists of the treated subject, accept their work, and adhere to it, wwith few comments. They thus feel less guilty of publishing possible tall stories, and of risking "comments" (comments written in the newspaper). Where from also the lack of will to risk a debate, which could compromise their career.

To build a new page of the science, it is necessary intended to risk (to make) certain errors, and to agree to discuss the solutions, … It is thus absurd to refuse the debates; but it is what we arrive in the current community. We do see it for example with *Poudres & Grains*: as I said very few authors wanted to be published, and little by " reviewers/readers " proposed comments.

In the politics of the scientific publishing, it would not be on the contrary a success: the number of published articles stays less numerous with this *a posteriori* reviewing, much less numerous than those of the *a priori* reviewing; and on granular gases at any rate, it seems to me that they contain fewer errors or of uncertain. Thus, articles remain probably rather serious, to limit the possibility of criticism: by presenting these results in the congresses, constructive or negative criticisms are more easily born; or by proposing the right(law) in "how" free, we favor the cross-examination. For example, Mr Villain, to whom I asked to examine one of my articles of *Poudres & Grains , i.e.* P&G **17**, 577 (2009), had understood the main part of the problem, even if he was not capable of helping me to go farther, and indeed on, he underlined me some inaccuracies (that he did not put down in writing me) … By the way, after sending to Mr. Vilain a second paper P&G **20**, 1-28 (2012), he was able to help in the understanding of the physics of granular gases, and to enter the debate efficiently, i.e. P&G **20**, 29-36 (2012).

On the contrary, articles proposed for the editions in *a priori* peer reviewing are there to show the intensity of the work of the teams, because they already have a co-notation " verified - exact " from their publication. But why?
Because financiers refuse to play their role " enlightened financiers " and to select the best. Not knowing how to estimate seriously, they ask to make a sorting via the edition, which is itself in the hands of the scientists … But the most prolix are not still the best, especially when we let be set up lobbies … Thus, in my opinion, we again have to wait to judge *Poudres & Grains*; new other web sites of open

science publishing amount moreover, free of charge (see web site in biology at F1000RESARCH.COM), with *a posteriori* peer reviewing mainly [F1000 Research will offer immediate publication; open, post-publication peer review; open revisioning of work including ongoing updates; and encourage raw data deposition and publication.].

E.2. Some Preliminary Recalls:

I call financier of experiences(experiments) and/or financier of the search(research) not only all the types(chaps) of private or public financiers, but also the public actors of the search(research) (such the CNRS[1], the CNES[29], the universities, INSERM[30], see ANR[31], European Commission, director of laboratory, a director of team) who finance research experiments or themes of research, or supply experiments to the researcher.

All these actors are ruled in theory by a relatively rigorous code of ethics, which built up itself by the practice of the scientists building up the science during the last centuries. Some principles were called back and updated by the European recommendation N 32005H0251 (http: // eur-lex.europa.eu / Result.do? IdReq=1*page=3).

Other similar codes of ethics, but often more exhaustive, are retailed in the USA and somewhere else in the world, but not so in France to my knowledge.

In the USA, this code is, it seems, so taught in Faculty. Sciences; there is an edition " for "master" and "students". The American Academy of Science[2] gave the means to make it apply, by creating authorities at different levels of responsibility to make it respect: the system is headed by the American Academy of Science, which delegates to the Agencies of means (Research Navy, NSF), to the scientific Associations (NSF, Bitter. MathSoc ., AssComputerMachin), to universities,…. Certain number of scientific associations proposes this on-line code, on Web (APS, Bitter. Math Ass, Am. Sc. Acad.). The American publishers seem respectful of their ethical contract. For example, I obtained from N.Vandewalle and from Grasp team of Liège, via Phys Rev E, to have access to their data in Liège, this by appealing to the declaration of Brussels signed by Phys Rev E, (cf. file(case) D1 DAR 2012 and D3 of this Tem #3; cf. also the history in Annexes 1-4, of the testimony in the CL of 16/12/2011).

In France, in my opinion, and according to what I see, nothing is made seriously and impartially from the point of view of the scientific ethics; naturally we shall find some examples in such cases on ethics: such or such penalty … But no possibility of making admit its law, no opened debate, no court,…. For example, in the case of my CNES[4] contract (Dynagran), I declared the problems to my supervision (CNRS[1], ECP[32], CNES[4], Acad. Sc.[3], to the state employee of defense, CNRS editor (see the introduction and the appendices of my testimony

[29] CNES : Centre national d'Etudes Spatiales
[30] INSERM : Institut national de la santé et de la recherche médicale
[31] ANR : Agenced nationale de la Recherche
[32] ECP : Ecole centrale Paris, ou Ecole centrale de arts & manufactures

Tem #2 in the CL of 16/12/2011); nothing moved. The SFP[33] does not make the promotion of the code of ethics in Science; we do not find it on the Web site, and we obtain no answer when we ask for it; the "codhos[34]" (committee of the F Acad. Sc) is not interested in the application of this code, because it limits itself to the "mortal" cases (Appendix 24 of Tem #2 in 16/12/2011), although the SFP(FPS) quotes the CODHOS[9] as its ethical organ! The "comets" of the CNRS[1] sees its action limited essentially to the new medical, biological and ethical notions (Appendix 12, ibidem). The CNESR[35] should be able to be interested in this ethical problem; however CNESER[10] and French universities are already in the incapacity to solve most of the cases of plagiarism (cf. the international congress " Plagiat et Recherche scientifique[36] ", Paris, on (2011) ; so we know its limits. Also the ANR[6] and AERES[37] are very too much occupied by the establishment of "not measurable" standards to be able to dash into a destabilization of these standards which they compete and contribute to establish. When we ask the CNRS which are the ethical authorities in its institut, we obtain no answer. If we address the European Commission, Thge Research Commissionner says that she has no authority for application of the scientific ethics. We thus see her in full contradiction with the charter of the European scientific research, because this one stipulates that a financier (thus herself in particular) has to observe these ethics. The commission declares proudly that the CNRS signed this charter also (cf. case D3 in Tem#3, Mrs Georghan-Quinn's letter, IT); but we do not know the real obligations which imposes this signature; professor in French law says to me, that there is not (cf. D3 in Tem#3, answer to Mrs Georghan-Quinn). This argument allows the authorities to be satisfied, because they meet their commitment, without guaranteeing anything. When some person asks for more rigor they can use inconsistent administrative constraints to ask the person for quietness, or the other party to follow guidelines, with partiality. Authorities thus incite the "researchers" to protect themselves against these pernicious effects, to break even more seriously the ethical basic principles. We see effectively more and more contentious issues: the literature abounds in false discoveries, built up, either that they are based on more or less invented data, or whether it is ancient discoveries, put back on the center stage…

The worst is to notice that in France the ethic authorities have probably never existed. The French Academy of Science has no *ad hoc* committee, proof that it is not its concern; it also refuses to look at the special cases (see letter of Codhos, Annex 24 of Tem #1, the Testimony to the CL of 23/6/2011). I asked by RAR letter the academy to settle (regional education authority) such a committee (see case D2 of Tem #3). At the moment, I have no answer. We thus have a real

[33] SFP : Société française de physique
[34] CODHOS : Comité de Défense des Hommes de Science
[35] CNESER : Conseil national de l'enseignement supérieur et de la recherche
[36] Plagiat et Recherche scientifique has edited a book : Le plagiat de la recherche scientifique, (G.J.Guglielmi & G.Koubi eds, 2012, LGDJ) ISBN 978-2-2754-03850-6
[37] AERES : Agence d'évaluation de la recherche et de l'enseignement supérieur

deficiency of education and practice of the ethics in science in France, and the rights and duties is not in used but also not written.

When we try to apply, which means also to ask for applying the scientific ethics and when we notice that this one is not respected, it is natural that the « guardianship organisation » resists, because it has not done its work. The best defense for it is to use the hierarchical superiority of the administration, to whom the justice always gives reason: an administrative fault can and must be quickly punished since it is demonstrated. We create then an inhuman system, which tries to justify itself by its administrative rights, aimless rigor moral, the necessity of which shall not be recognized any more.

In my opinion, the only manners to fault such a system, which refuses to see its mismanagement, is either an act of authority, that is a " humanist clause " which imposes on it to accept its error, or to show the perversity of the management by using the complexity of the system to impose on it to make as well the another error, than it will not agree not to see: we know that a " complex object " depends on multiple interdependent degrees of freedom, and is managed by numerous parameters and by numerous non linearities; it often presents thus certain number of "defects", which we can be viewed as niches (I printed here deliberately the notion of (fiscal) niche used in the field of the tax system); these niches are stable abnormal behavior, which can serve to demonstrate the perversity of the object.

At the moment, I am the only one in the laboratory, worried by this problem of the scientific ethics. Others tell to be interested in it; and they created a cell or "a committee" asked to study the ethics; it was in the Council of the lab. of September, 2006; since then, there was no meeting of this committee. I posted in the door of my office " Faux scientifique passe ton chemin. Ici on n'accepte que les scientifiques qui respectent la déontologie scientifique et la font respecter."[38] It raised no problem, no question, no discussion to the entire staff of the laboratory, or to the ECP staff, no remark in the book on "hygiene-safety". Are we already at the non-scientific age? Where is our freedom to think?

E.3. Other readings :

Recommendation of the European Commission on the European charter of the researcher. It is on the site : http : // eur-lex.europa.eu / Result.do ? IdReq=1*page=3 of the European legislation, the following recommendation N 32005H0251 : of March 11th, 2005.

Code of scientific ethics in the USA : see web sites of APS, National Science Academy,…

[38] It means : « False scientist spends your road. Here we accept only the scientists who respect the scientific business ethics and make it respect. »

E.4. Testimony of 13/03/2012 in the CL of the Lab MSSMat:

E.4.1. Case N 1: DAR[39] to the CNES[4] year 2012

This DAR[39] contains the following following appendices:

1. Scientific objectives (3p)
2. Current situation of the theme of research. (3p)
3. Experimental devices. (2p)
4. Staffs of the laboratory participating effectively in the project (1p.)
5. Outside collaborations. (1p)
6. Means put at the disposal of proposants. (1p)
7. Calendar of the project. (1p)
8. Projected budgetary schedule. (1p)
9. Program of the works. (1p)
10. Progress report of the thesis of Yanpei CHEN (4p)
11. The DAR of year 2011 (39p)
12. Discussion with E.Trizac (2p)
13. Express demand to Phys Rev. To help getting data published in PRE, (with joint demand to the CNES) (4p)

The appendix 13 contains the demand, to Phys Rev, of assistance to obtain data from Vandewalle's team. The review accepted, to honor its signature of the Brussels declaration.

A contrario, the CNES made nothing, although Vandewalle, CNES and PEvesque were bound by some common project, VipGran,.

The research results displaid in the DAR 2011 and 2012 were presented at various congresses and published. They were also presented in the « Phys. Stat. Days, 2012 » meeting (ESPCI, Paris January 2012) when they suggested no question from audiences. I discussed them then directly with E.Trizac, and at the same time with J. Villain (F. Ac. Sc.)[3]

This DAR was sent to:

E1. B.Zappoli (CNES), copy to cnrs Mediator, cnrs Chairman, M.Rosso

E2. To the President/Chairman of the CNES (RAR letter on 20/2/2012, on 22/2)

E3. European Commissioner on Research and Innovation (RAR letter on 17 and 29/02/2012)

E4. Perpetual secretary A of the F Academy of sciences (RAR letter on 20/2/2012)

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[39] DAR : Demande d'aide à la recherche : File to be filled for asking grants from CNES

E5. Perpetual secretary B of the F Academy of Sciences (RAR letter on 2/3/2012)

Document D1-E4 and D1-E5 will be later presented, because they also ask for the training(formation) of an ethical committee the Academy of Science.
The D1-E1 documents also contain an information as for the request of applying the declaration of Brussels to Phys Rev E.

E.4.2. Case 2: Application of a scientific code of ethics in France

Letters and e-mail to the CNRS[1]: without answer, see D2 (1-5)
With more to the CNRS[1]: (see Secondary 13 of the Testimony in the CL du16 / 12 / 2011) Mediator (see Secondary 6 and 9 of the Testimony in the CL du16 / 12 / 2011)
Discussion with F. Darve (AEMMG): see D2 Darve, (6 – 9)

And always:

Demand of evaluation to J. Villain (see Secondary 23 of the Testimony in the CL du16 / 12 / 2011) the complements are not reproduced because they miss interest)
Correspondence to Mrs Leduc (see Secondary 16 of the Testimony in the CL du16 / 12 / 2011)
Correspondence with the CODHOS (see Secondary 24 of the Testimony in the CL du16 / 12 / 2011)
No code of conduct at the SFP, on the site of the Academy of Science, …
Discussion with E. Trizac (see D1 DAR on 2012 cf Secondary 12)

E.4.3. Case 3: Application of a scientific code of ethics in Europe

European Commissioner in research and Innovation (see D3 correspondence). This correspondence shows the fatal accumulated effect between the European and French legislations so that the good consciousness reigns everywhere, but amplifies the lack of ethics
In more: (see Secondary 3, 4 and 10 of the Testimony in the CL on 16 / 12 / 2011)

In the ESA: a willingness but an absence of means (see D1-DAR 2012)
in more: (see Secondary 3, 4 and 10 of the Testimony in the CL du16 / 12 / 2011)

D3-bis: Europe and European Science Fundation 3p
D3-ter: ESA and Phys Rev E 4p 2p = 6p

E.4.2. Case 4: Application of a code of ethics in the USA and international

In the USA: see the sites of the Academy of Science American, of the APS … The code business ethics are called back, the authorities exist and are structured. To see the efficiency of the system.

For the editors, they agree to call back the business ethics. (cf. Phys Rev E) (to see D3-ter)
For Phys Rev E to see in Case 3, ter, p 29-30, (before) and DAR 2012 Annexes 13

By the AEMMG via the Congress Powders and Grains(Beads): rather negative (to see Secondary 25 of the Testimony in the CL du16 / 12 / 2011)

E.4.5. Case 5: Application of a code of ethics and related problems

When we try to apply the scientific ethics and when we notice that this one is not respected, it is natural that the guardianship organisation resists, because it is not trained to make ethics respected. The best defense for them is to use the hierarchical superiority of the administration, to whom the justice always gives reason: an administrative fault can and must be quickly punished since it is demonstrated. We create then an inhuman system, which tries to justify itself by its administrative, aimless rigor moral the necessity of which it does not recognize any more.

In my opinion, the only manners to fault such a system, which refuses to see its mismanagement, is either an act of authority, that is a " humanist clause " which imposes on it to accept its error, or to show the perversity of the management by using the complexity of the system to impose on it to make as well the another error, than it will not agree not to see: We know that the behaviour of a " complex object " depends on multiple interdependent degrees of freedom, and is managed by numerous parameters and by numerous non linearities; it often presents thus certain number of "defects", which we can see as niches (I printed here deliberately the notion of (fiscal) niche used in the field of the tax system); these niches are stable abnormal behavior, which can serve to demonstrate the perversity of the object.

At the moment, I am the only one in the laboratory, worried by this problem of the scientific ethics. Others tell to be interested in it; and we create a cell, called "committee" to study the ethics; it was in the Council of the lab. of September,

2011; since then, there was no meeting of this committee. I posted in the door of my office *« Faux scientifique passe ton chemin. Ici on n'accepte que les scientifiques qui respectent la déontologie et demande à ce qu'elle soit respectée »*. " False scientist crosses your road. Here we accept only the scientists who accept the business ethics and ask it to be respected ". This raised no problem, no question, no discussion to the entire staff of the laboratory, and of the ECP, no remark in the "hygiene-safety" book.
Are we already at the non-scientific age? Where is our freedom to think?

I do not have time to treat this problem in this testimony, to be attested during the CL MSSMat of Mars 2012. I postpone it at a later date.

E.5. No compliance with the ethics during the evaluations of projects: Cases of Dynagran and Vip-Gran

As regards the non compliance with the scientific ethics I have already made my remarks to CNES-, ESA- Managers, to the cnrs Regional Delegate, to the Director of my laboratory and to the CNRS Mediator. One shall find some details in my CNRS report 2010.

As regards the evaluation of the project Dynagran, the joint France/CNES-Chine/CNSA project, the assessors in France are since 2010

i) E.Falcon, one of the French members of the scientific group working on VIP-Gran, who defends another scientific interpretation of experimental results, incompatible with our recent experimental data and with the real limit conditions in use in the experiments on vibrated granular gases (see my CNRS[1] report),

ii) O.Pouliquen, a scientist working on granular matter in a lab granted also by CNES for an other field of research. O. Pouliquen was an unfortunate candidate for the succession of the presidency of the AEMMG, but he sits there as a treasurer of this association since then...

They have both divergent scientific positions in mine, but have discussed never neither their position, nor my position.
They are both arrived at the evaluation committee (Nov on 2010) discussing that N. Vandewalle " is capable of simulating with his program some correct results looking like those obtained in MiniTexus 5 ".
Questioned from the next day, N.Vandewalle refused at first to answer then, nor to show any data ; after a delay of 3mois and through the ESA, he sent me the preprint of an article with a photo (not a video) simulating a granular gas comparable to that of MiniTexus 5; this vision is static, and we have no possibility of seeing the evolution of the system, nor of knowing the distributions of speeds....
For me, we can thus conclude nothing from this work of N. Vandewalle (to see My Testimony #2 : 1, 4, 10 and especially 2 and 3).

In breaking news, my colleague M. Hou says to me that the Dynagran experiment in SJ-10 is always programmed, whereas my correspondent CNES[4] asserts that the phase B of Dynagran is stopped and will have difficulty restarting if it restarts; at the middle of May. Having said that, B.Zappoli had said to me that everything was taken so that Dynagran moves forward and is a success (?).
I think that M. Hou is convinced of my work, and that of our student YP.Chen, cf. future articles ISPS on 2011, the P&G 17, P&G18, as well as my previous P&G on granular gases.

The VipGran project is financed by the ESA for the International Space Station (ISS).

Part F

On possible administrative harrassment and misconduct

This part is reported to an other date.
Let me tell that I had to fill Hal by night with the help of F. Douit.

Part G

Annexes

1. Letter on scientific publication of research (2002)
2. Letter on the reform of the CNRS (2004)
3. List of publications of P. Evesque as found in ArXive

& as found in Hal

For Hal the lab paid a secretary to fill the hall system in 2009-2010 . This is to check the efficiency of Hal. There might be some problem of referencing the lab and its name.
But this kind of problem could be solved from a general archieving processrather from single interface done by each researcher. How to waste time.

G.1. Letter 1 : On Classic journals for publishing scientific research:

Do they allow a simple evaluation of the work of the scientists? Are they dedicated to disappear or to evolve?

(Sent on 28/02/02 from P. Evesque to the CNER[40] (which became COMETS[41]); 4p)

Scientific journals are tools which the scientists built up to answer their need of communication and sustainability of the experiences of their research. It is thus essential to analyze (A) the way the scientists use them and (B) the way these reviews are operated and if they ca be used for an evaluation policy of the research. Then we shall see (C) if the editorial process cannot be improved.

A) Technical interest of scientific journals:

Originally scientific journals were created for a fast distribution of the information, while assuring a good quality of edition and by limiting the cost; scientists always considered that articles which are published could contain errors there, and that they must be presented there to be discussed. In proof, the standards of the scientific work always imposed that the first stage of a research is a bibliographical critical study, which consists in reviewing the question, from Scientific journals in particular; authors thus look for all the articles concerning the subject, by trying to discern " true " and innovative articles of " true " but uninteresting articles, to see partially false, or totally false.

This first stage of the research work is thus a value judgment; it is an essential stage. This method is the only one recognized by the researcher and by the academic to estimate his work and that of the others. This method has to be the only one to prevail in the committees or commissions of research evaluation, otherwise the definition of new criteria will develop a new way of working of the researchers.

It is just what we notice: the Committees of evaluation of the Research restrict themselves to statistical studies of number of publications, rate of quotations; and the researcher restricts to look after his advertising: he goes of congress to congress to speak, and he leaves immediately after his talk without listening to the others; is it a serious communication policy there? For my part, I think that the quotation indices are a help to the evaluation, but they do not allow a serious evaluation for the reasons which I developed in previous both paragraphs. In too much to promote this method as it is not risked besides inciting the young people (and the least young) researchers to neglect simply their bibliographical work.

National American Science Foundation[42] (NSF), as for it, (as well as the other national and international bodies) does not content with counting the publications

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[40] CNER : Comité National d’Ethique de la Recherche ; COMETS : Comité d’Ethique du CNRS

[41] COMETS : CNRS Committee of Ethics

[42] NSF : National American Science Foundation

and always asks independent international experts, chosen as their skill, the evaluation of the teams and for the projects which it finances. Finally, the profession of researcher being very close to that of the education, we have to distrust the example which we give; this has to push us to a rigorous effort.

B) Way of functioning of the scientific newspapers:

Before wanting to go into the definition of the tasks of the scientific evaluation it seems to me necessary to re-specify the various stages of the scientific production and to define i) what is in my opinion a scientific research article, ii) what allows it to be published by a review, iii) the specific difficulties met by certain really innovative articles, or iv) by articles in the margins of several existing domains, i.e. multidisciplinarity. I shall describe then v) the mode of constitution and functioning of Reviews with selection panel, and the power which the panels exercise; then I shall come vi) to their defects, knowing that their first purpose, namely the distribution of the scientific knowledge, is about insured.

i) A research article tries to advance the state of a question by studying a new example and by bringing new elements of answer. This way, it has a risk of being false.

ii) The article is accepted by a Magazine because its contents gets a support of referees; for it, it asks a question which the referee finds important and it defends a point of view which is generally rather close to that of the referees.

iii) Of this we can conclude that the more an article is innovative, the larger its probability to be rejected to such a point that the distribution of certain very innovative articles was able to be realized only on the occasion of the creation of new magazines. But also, an innovative article which is published has more risk of being false, because referees misses adequate criteria to estimate it.

iv) It's the same for multidisciplinary articles, mixing the knowledge and the concepts stemming from different domains. An article containing a multidisciplinary mixture of concepts, although eminently desirable and necessary, risk to strike the sensibility of referees specialized in the only one of domains and to cause so its rejection; without counting the existence of a more important risk of error, badly checked by too much specialized referees.

v) Let us analyze now the way of functioning of a scientific magazine. A Scientific journal with Selection panel is built around a certain number of scientists representing a School of thought. The existence of several Magazines for the same themes demonstrates well the existence of these Schools and the pressure groups which are associated to them. We can almost draw the parallel with the ecological niches. It is thus normal that these Magazines try to take more and more importance in the financing of the research and the evaluation of the researchers. But it is a real danger. Selection panels are in fact the rests of the last ones mandarinats.

vi) Is it because of their competition that Magazines are not capable of limiting the scientific production? We indeed observe that the number of publications increases year by year. They are not able of limiting either the redundancy, or the

plagiarism. They remain very weakly concerned by the respect for the right for the anteriority and they exercise de facto only a very weak filter because only some percentages of the scientific production is never published.
vii) Of more this type of publication is expensive, more expensive than an electronic bulletin board. It conceives the information as a good and a power which we exchange, where the author often pays the act to be edited, the reader buys his reading, and the author and the referees work free of charge. Why do they make it? Probably because they hope to demonstrate that their work is « recognized », to benefit from promotion and to find financial supports. But is it a guarantee of the quality of the works? For it, Magazines would have to assure the follow-up of their judgment, because a scientific judgment has to work on the long term.

In fact if every Magazine addresses to every potential reader, the authors who publish there often correspond to a small proportion of the scientific community which is concerned, the others preferring to publish somewhere else, often for questions of referees and of disciplines; it is the proof of the existence of the " editorial power " which should not be acceptable, because it is against the scientific ethics and against the free circulation of the ideas.

Finally, we cannot and does not have to consider that the scientific Magazines certify the archiving of the scientific information : they are only commercial companies, susceptible to go bankrupt; their are not institutes such as Bibliothèque Nationale de France or as Library of American Congress. The best guarantee which magazines propose, it is the distribution of the information and its storage in multiple places.

C) Towards an improvement of the editorial process and the evaluation:
To improve the editorial process, it is necessary to compare the previous rules to those whom recommend the scientific ethics. This one requires a free circulation of the ideas, a free discussion, and that each builds up to himself his own opinion. The circulation of a false reasoning or an erroneous result is not dangerous in itself, because the scientific method allows to detect it; moreover we know that scientific journals were used to resist; it is simply necessary to watch that such acts are not deliberate, that journals impose loyal debate, that the results are honestly described. On the other hand acts purely of authority, such the rejection without appeal by a referee of a thesis, a theory, should be banned. Also the right of the authors must be protected, the right for the anteriority in particular, and the right for thewithdrawal also …

Tries to improve the editorial process were tempted indeed in the past, such as to open articles to the discussion during given period. But this was expensive and weighed down the process of publishing. With the age of internet, of e-publication and of high quality printers the problem changes completely. The broadcasting of the information is fast and little expensive; the interactive notes are simple to set up in a way that the follow-up of a problem, an evaluation can be made on the very long term. It is henceforth enough to define a protocol of storage, and to assure the sustainability on long duration (10-20 years). Beyond it will not be necessary, because the interest for an area of ground research generally less for a long time; it is the effect of the change of generation.

An example of recent try:

An experience of this kind is at present tempted with *Poudres & Grains*, ISSN 1257 3957, which is the bulletin used to connect members of the international association AEMMG (Association for the Study of the Micromechanics of granular media; it was turned into a scientific journal available on the Internet: *Poudres & Grains*[6] : http://www.poudres-et-grains.ecp.fr/spip.php?Rubrique1 , while maintaining a paper version deposited in the BNF[43], for the saving. The editorial rules are there simple, given in appendix. They are in accordance with the scientific ethics: free access to all; respect for the anteriority; refusal of the quarrels of person; acceptance of any discussion and any scientific contesting; furthermore, it is specified that every reader has to make a work of referee, that every author has to describe honestly his results. This has to allow a better efficiency in the transmission, and the performance appraisal and theories. For example a multidisciplinary article will be judged by the readers of all the disciplines, each being able to express himself on his field of expertise, and each also having access to the remarks of the other disciplines; this also has to allow a better transmission of the knowledge between the disciplines. The readers are themselves the Selection panel; a referee-autthor-reader who makes a mistake should finish by knowing it; it is also easy to make respect the anteriority or to limit redundant articles by a simple note, " already read, cf. ref., name of the person who writes this remark ".

Furthermore, the article which is sent to the magazine is published as it is, without possibility of modification, revision; the opinions of the readers are only confirming the quality of the work.

To avoid the potential danger of too numerous debates, articles must be signed, and the authors have to be professionals of the research or the education.

This editorial politics seems at present to bother the potential writers who are maybe afraid that their articles in *Poudres & Grains*[6] are not counted by Committees; but this problem should vanish in time, except systematic dam.

D) Towards a firmer politics of the respect for the scientific business ethics:

This part gives some examples of possible skid of the editorial politics of a classic scientific journals.

What does an editor make when one of the reporters concludes "not publishable article because the results are just, but are well known", but the other one says "article to be rejected because the results are false". The editor

[43] BNF : Bibliothèque Nationale de France, soit French National Library

will reject the article, although he should consider one at least reporters makes a mistake, and although the question must be discussed in a hurry …

Why are certain articles rejected by certain newspapers and accepted somewhere else?

Of what to think of a publisher who refuses to make respect an anteriority?

What to say about reviewers which block systematically an article disputing a thesis developed in his Magazine.

What to say about a magazine which refuses to publish a bibliographical research which allows to make the link between several different results, to unify the concepts resulting from different domains.

What to say about a review which asks to develop voluminous appendixes rather than to put a specialized reference.

… …

All these facts are grave ethical breaches in the scientific deontology and in the rights of the authors. In the current Scientific journals, no internal rule allows either to avoid them, or to improve the editorial process, or to know the statistics of breaches about these rules. Or, if such internal organs of moderation are planned, their efficiency is really very weak, even useless. This demonstrates that the classic Magazines with Selection panel is not a correct business with scientific ethics ; this begins with the best of them in physics: *Nature*, *Science*, *Phys. Rev. Lett .*, *European Physics J …*

And no French scientific authority of evaluation was created by scientific department or organism to make respect these ethics rules, to help scientists potentially abused or hurt successor. Yet " Science without consciousness is only ruin of the soul"; the scientific research has to arm itself with organs capable to make respect this ethics; otherwise the Scientific Community will disappear, corrupted by the money, the power, the clans and the advertising.

Is it an impossible reform? The CNRS[26] established a Committee of Ethics; it has at the moment no power and is not connected with any legal department, …

Do the new technologies sound the knell of the former types of publication? I hope for it, because they should allow to improve the speed of broadcasting and the process of reviewing, while limiting the cost of the edition. However it asks for a change of state of mind on behalf of the research actors and modifications in the financing processes and in the authorities in charge of the management of the research. At the moment, these authorities seem to refuse these changes. We understand the reason :, it is their power which escapes them. Are not the Editors who try to propose « new indicators » (as the H factor,…) based on their journals to estimate the researchers, to persuade that the method is quite effective and that it obeys a rigorous scientific business ethics? Do they tell the right, which is that these factors are not a measure (in the scientific definition of it) ? Are these facors able not to let grow the number of publications, to decrease the number of wrong published results,… ?.

(Sent on 28/02/02 from P. Evesque to the CNER[40] (which became COMETS[41]); 4p)

APPENDIX to letter on february 28th, 2002**:**

Note for the authors of *poudres & grains*:

Object of the publication

poudres & grains is a magazine, which publishs original scientific articles on the subject granular materials, , powder or similar media; it is covered by the copyright. It addresses to professionals of research and of education in the public and private sectors. Eah journal has a printed version stored to preserving into the BNF. Complete reproduction of articles and/or the magazine is authorized and free for personal uses or at the end of archiving ; it can be made by download. An authorization must be asked for commercial reproductions even partial.

Submission of articles: articles have to be originals; a transfer of copyright must be signed, specifying that the author accepts the editorial rules, especially those relative to the scientific comments, because articles are opened to scientific discussion. Electronic links will be established as possible for this purpose.

Editorial rules:

Every scientific author owes :

- To describe honestly the results which he obtained so theoretical as experimental.
- To accept and favor the honest debate between scientists.
- To make quarrels of/to nobody.
- To respect the rights of the other scientific authors and the scientific anteriority in particular.

Any breach in these rules deletes the access to the publication. The author is only responsible for the contents of his article. An editorial committee expresses its opinion at the need; but the real work of reviewing must be *a posteriori* , i.e. after publication, by the whole scientific community who read the paper.

Every scientific reader owes

- To make a critical reading, which consists in analysing and criticizing the scientific articles which he reads so as to build up to itself his own opinion
  - after reading an article, to notify the author and *poudres & grains*, of similar edited results, expressing either the same results or the opposite ones to those whom he has just read.

(Sent on 28/02/02 from P. Evesque to the CNER[40] (which became COMETS[41]); 4p)

G.2. Some reflections for a reform of the CNRS[44]:

(Sent to CNRS &to P.G de Gennes; 22/07/2004 ; 4p)

This discussion tends to conclude that the management of the CNRS[5], and the whole research, passes inevitably by an evaluation of the researchers and by a management of their careers.

It indeed shows that the only definition of areas of research, independently of their pertinance, cannot lead to a good strategy of research because of the competition between the numerous financial actors.

1) Do we want that the CNRS[5] becomes an Agency of means and financing of contracts or that it stays a Research center ?

   If we want that the CNRS becomes an Agency of means: it is necessary to wonder

   i) how to manage to make the best researchers work on strategic objectives defined by the CNRS,

   ii) to wonder if the objectives of the CNRS can be very different from those of the Universities, Regions, France, Europe, industrial business?

   If the CNRS keeps its human resources: the CNRS will remain partially on a human potential at the research. We then have to ask a question: are these researchers allowed to work on contract (financed by industrial business, regions, other national Agencies, Europe, the French-American contracts).

   To this last question, we cannot answer NO, because it means that we will refuse any European collaborations.

   We can push then the reasoning farther: calls for tenders launched by the CNRS will always be in competition with the other calls from EU, businesses,…. If the best researchers answer it it is because the proposals of the CNRS are the most interesting, otherwise even the staff of the CNRS will lose interest in it. We shall not thus underestimate the effect of competition between Agencies.

   This freedom of the choice of contracts assures the freedom of the research and the freedom of the actors of the research, at least for the best researchers. And the only possible strategy for the CNRS is to hire the best researchers and to maintain them in peak condition. If we get to this point, the recognition of the CNRS will be supranational.

   As we are going to show it, a corollary in this solution is that we can change only slowly strategy of research, at the speed of the renewal of the men.

2) Did the 2004 reform of the CNRS ask this question in these terms ? Answer : No, to my knowledge.
3) How can we maintain the researchers at the top of their shape?

[44] This letter was written as my contribution on the reform of the CNRS in 2004. It was not replied. It explain some normal attend from researcher to the financing system. (*It was sent to P.G de Gennes, and CNRS ; 22/07/2004*)

When we are interested in a sportsman, we select him for a sport and we make him play in this sport; we shall have difficulty in selecting a football player and making him play the rugby, or swim. The human competition is selective on very small differences: 1/10th of second is enough for the 100m; on the other hand the human potential presents relatively weak dispersal: everybody, after a little of training, is capable of running the 100m within 15s.

It is probably the same there for the intellectual criteria; it is thus necessary to place the competitors in their domain of preference. And the trainer / or manager /or " administrator of the research "/ should pay very attention on it. Which sports trainer would allow to act otherwise. For the research, the continuity is also crucial, as far as often a discovery bases on the possibility (and the will) to assert a difference, an incomprehension. The researcher thus has to be essentially a motivated, encouraged person and in confidence. Naturally, it is necessary to assure him[45] an environment of doubt and questioning, even for his own discoveries, because these discoveries can be erroneous, perfectible. It is necessary to allow thus at all costs the researcher to live in a universe of natural contesting and questioning of the truths. What does a manager of lab make if he does not act in this sense: He will "kill" probably his researchers. He has to insure an emulation, a doubt and frank discussions besides the finance managing.

4) Are these problems of " preservation in the shape of the researchers " present in the proposed project of reform: NO. A good administrator, and thus a good strategist of the research, shall never forget these goals. Hence the 2004 CNRS reform cannot work.

5) Evolution of the research

The problem of the evolution of the research raises now. The domain of research often evolves with the renewal of the men. Can we go faster? Can we shorten the career of the researchers to improve the staff turnover and the "adaptability" of the system. They are vast questions there. The problem also is to know if the researcher stays (or can stay) effective throughout his life, or if he loses of his efficiency by aging. It depends probably on the researcher, on his capacity to question, to sacrifice its " seated situation ". The stimulation of the movement is not really favored by the structure. We could even say: leave your place and you will lose it. And the arguments of authority and fame takes it from a certain age. The " management of the rumours " becomes a favorite pastime.

However, the researcher can remain a priori for a long time successful in the disciplines where the necessary culture is big, where the diversity and the complexity dominate the knowledge. It is the case for the literary disciplines, and maybe also for the physics and the chemistry; unless we manage to summarize in a synthetic way all knowledge and to make accessible easily the diversity of the physical behavior from a synthetic approach.

[45] I use he (him) for he or she (him or her) most of the time, as one could understand it from the context..

In the younger disciplines, where the creative imagination is not restrained by the amount of the knowledge yet, the maturation of the researcher is faster and its efficiency slows down according to the years.

The duration of creation of a researcher varies enormously and is with difficulty quantifiable. We can fix a limit age of 40-50 years. In that case, why not to include it in the status of hiring, like that is for the servicemen, with a pension? However we shall remind very famous exemples of efficient researcher, as Pasteur, as Eiffel, at older than 70. This should mean that good research manager should be ablke to stimulate findings to old workers too. Is this discussed in the reform ? Not at all, management cannot improver its managers.

We shall keep in mind as well than the change of employment of executives is very difficult past 50 years old. Government agencies must thus be aware of it and plan their compensation policy accordingly.

6) Are these problems of evolution discussed on the reform: NO. It does not speak either clearly about the real strategic stake: 20% of the researchers of the CNRS were going to retire within 1à years after 2004; it was thus the moment to think of the evolution of the themes, in of their speeds of evolution, …

7) Evaluation of the research

The problem of the evaluation of the research is a major problem, which was not seriously handled in the past: we left too long to the editors this care, but the cost of the edition so fell and the pecuniary stakes are such as the edition is controlled by lobbies, such as it manages the publications in the short-lived, without assuring the real follow-up of the relevance of the publications during several years, nor even to limit the number of repetitions, …

The action of the national Committee was rather beneficial; but as any body, it has its faults. However it restricts too often to count the publications, without verifying the relevance of these, nor extracting repetitions. Finally, due to its status it is irresponsible because its opinion is only consultative, at the stage of the CNRS. Owing to this the researcher can make no concrete criticism and the Committee is thus insensible to the criticism. Yet in the facts, it manages the career of the researchers and the laboratories. Researchers should thus be able to criticize it and oblige it to improve. It is necessary to define a contrepouvoir, an authority of conciliation.

The number of reports is too large: personal, GDR report, lab report, personal report, contract report. What's the use of putting several times the same facts in various reports, if it is not to make waste time to the writers and to the readers, to increase articially the number of facts, to prevent real valuatin … It would be better to facilitate the compilation of articles from a base to the other one, at the need to establish an institutionalized and centralized data bank, grouping all the publications of the researchers, and the references, and grouping the work estimations.

What a good research? Must it be estimated on the fame? Do we try to transform the assessor, and the researcher, into sheeps of Panurge? It is already the case with the Star Ac. The physicists and the biologists are interested in the propagation of the epidemics, the contaminations, the fires, … We know the sensibility of these propagations about details, the effects of threshold to which they are subject. The rumours and the fame do not probably break these models, except maybe by their biggest not linearity.

8) Does the reform discusses the problem of the evaluation of the research:

Yes, but in a very abstract way, without specifying the shape that the evaluation has to take, nor defining the consequences of this evaluation. In particular it does not define the link between the elaboration of the strategy of the research and the evaluation of the past researches; what is the feedback? The reform wants on the contrary "to exteriorize" the evaluation.

What will be the consequence of this evaluation on the career of a researcher? And on that of a research manager? Because it is the manager who is in charge of funding after all ; it means that is responsible for credits and for their wastes when he distributes them ill-advisedly?

9) Evolution of the structure of the CNRS in the European context. The adaptation of the CNRS to its European environment and to its evolution is a major point to make effective the agency. The consideration of the evolution of the European structures is thus essential and to make the CNRS adapting to this evolutionary is a point very absent in the project of reform.

10) Comparison appropriate normal Unit (UPR) / mixed Unit (UMR)

The CNRS works through « mixed » lab (unit), UMR, and normal lab (units) , UPR, etc. Still it would be necessary to make a correct balance estimation of the efficiency by the various existing structures, their possibility of improvement …

11) Comparison between CNRS research/ University research

We want to reform the CNRS. All right, but it does not represent all the French research. This reform thus has to fit into a global strategic vision; it is thus necessary to assess the other authorities (universities.), criticize their way of functioning, estimate the various methods, the various reforms and the various potential after reforms. It is only after that that one can decide on the type of reform.

(If the CNRS represented the largest part of the research, it would be that it is already very effective compared with the University and to the other agences considering its number; it would then be better to reform these last ones).

12) Some problems with the National Committee: it is only consultative.

The Management of the CNRS blames the National Committee for its excess of power. Texts give to the national Committee only a consultative opinion. Where is thus its real power? Do we need to make a reform to apply the rule which its status fixed? Does the Committee refuse to apply this rule? If yes, what would serve to define a new rule since « to be used as a consultative advice », imposes already to the Commission a strong limit ?

I would prefer an Authority of regulation and appeal to the decisions of the National Committee would be created, to force the Committee to be put back into the rights by the researchers, not to allow it biasing but forcing it to ask management to decide under clear evidence of reality.

13) Research for an improvement: some problems raised by la «Cour des Comptes[46]»

The revenue court found certain number of faults, and suggested certain number of remedies. Did the reform confirm the analysis? Did it try to apply recommended remedies? Otherwise why?

14) The multiplication of sources and financing organs, as well as their various levels of action in the structure (Laboratories, GDR, WILL, ATP, " young teams ", COST, university, ministerial, regional, European, industrial financing) makes the global system of management relatively opaque. Like that is said in 1, this insures the researchers a relative freedom of thought and share which they need. In return, they have to answer calls for tenders and have to write more and more numerous reports, what makes them managers spending their time on other topics than on the effective research.

The multiplication of these levels and these structures compromises partially the efficiency of the research; it also makes intricate the cost accounting of laboratories, institutes…, a research beiing generally through several sources of funding, using different experimental set-ups. One of the priorities of the managment should be to limit the researcher to be transforedm into research manager.

Besides the interest which he wears in his researches, the other motor which livens up the researcher is his career. It is probably by this way that we can direct its activity, and not by the announcement of strongly financed themes: a good researcher knows his capacities; he chooses his new theme of research; it has to be rewarded after his success, and not before.

It is so absolutely necessary to stimulate the vocation of the researcher to stay in the research; otherwise the structure and the "efficiency" will transform him into contract manager from after his PhD thesis; and his past training will be of no use to him; his past studies will thus have been led only for absolutely nothing.

In other words, if we want that the researcher is mobile, it is necessary to insure him a decent career after he was mobile (and not before); very too often it is not the case. In this respect, the management of the multidisciplinarity should be a priority and not simply a vain incantation.

The evaluation of the researcher is often based on its fame. This one grows slowly as he becomes integrated into his community, the effect is probably exponential. A change of theme is thus formidable from this point of view. This must be thus taken into account for a good management of the research: there is no linearity between efficiency and fame. And fame is the only evaluation for recognition !!!

[46] cour des comptes : Revenue court

So the fame, as the rumour, is a very partial indicator, because it does not certify the correctness of the work.

15) Finally the CNRS must be aware that any act of management which is additionally asked to the researchers is taken in fact on their working time and thus corresponds to a loss of scientific production. It penalizes thus directly the efficiency on the Establishment. It would be necessary to calculate the cost every time such measures are applied. One can tell that additional acts of management profits to the research directly through a better advertising… and to the propagation of the knowledge. It is maybe true but it is in any rigor the work of the ANVAR[47]. Whyever do not subcontract in these bodies these problems.

Sent to CNRS and to PG of Gennes, on July 22nd, 2004

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[47] ANVAR : Agence National pour la valorisation de la recherche, or National Agency for the Promotion of Research.

G.3. Listes des publications récupérées sur Hal et sur ArXive

G.3.1. Sur ArXive : 46 for au:evesque

1. arXiv:1112.3888 [pdf]
   Boundary conditions and the dynamics of a dissipative granular gas: slightly dense case
   P. Evesque
   Journal-ref: Poudres & Grains 2007
   Subjects: Fluid Dynamics (physics.flu-dyn)
2. arXiv:1112.3886 [pdf]
   Microgravity and Dissipative Granular Gas in a vibrated container: a gas with an asymmetric speed distribution in the vibration direction, but with a null mean speed everywhere
   P. Evesque
   Journal-ref: Poudres & Grains 2010
   Subjects: Fluid Dynamics (physics.flu-dyn)
3. arXiv:1111.6881 [pdf]
   Granular Media under Vibration in Zero Gravity: Transition from Rattling to Granular Gas
   P. Evesque, Y. Garrabos, G. Zhai, M. Hou
   Comments: Poudres et Grains 2011
   Subjects: Fluid Dynamics (physics.flu-dyn)
4. arXiv:1111.6441 [pdf]
   On the complexity/criticality of Jamming during the discharge of granular matter from a silo
   P. Evesque
   Comments: Poudres & Grains 2007
   Subjects: Fluid Dynamics (physics.flu-dyn); Soft Condensed Matter (cond-mat.soft)
5. arXiv:1111.5510 [pdf]
   Cyclic Maxwell Demon in granular gas using 2 kinds of spheres with different masses
   P. Evesque
   Comments: Poudres et Grains 2007
   Subjects: Fluid Dynamics (physics.flu-dyn); Statistical Mechanics (cond-mat.stat-mech)
6. arXiv:1111.5507 [pdf]
   How one can make the bifurcation of Maxwell's demon in Granular Gas Hyper-Critical
   P. Evesque
   Comments: Poudres et Grains 2007
   Subjects: Fluid Dynamics (physics.flu-dyn); Statistical Mechanics (cond-mat.stat-mech)
7. arXiv:0903.1242 [pdf]
   Effect of aging on the reinforcement efficiency of carbon nanotubes in epoxy matrix
   Aïssa Allaoui (LMSSM), Pierre Evesque (LMSSM), Jinbo Bai (LMSSM)
   Journal-ref: Journal of Materials Science 43, 14 (2008) 5020-5022
   Subjects: Materials Science (cond-mat.mtrl-sci); Classical Physics (physics.class-ph)
8. arXiv:cond-mat/0611613 [pdf, ps, other]
   Coherent behavior of balls in a vibrated box
   Yves Garrabos (ICMCB), Pierre Evesque (LMSSM), Fabien Palencia (ICMCB), Carole Lecoutre-Chabot (ICMCB), Daniel Beysens (SBT, PMMH)
   Subjects: Statistical Mechanics (cond-mat.stat-mech)
9. arXiv:physics/0609204 [pdf, ps, other]
   Maxwell demon in Granular gas: a new kind of bifurcation? The hypercritical bifurcation
   M. Leconte, P. Evesque
   Comments: 19 pages, 10 figures
   Subjects: Fluid Dynamics (physics.flu-dyn)
10. arXiv:cond-mat/0512304 [pdf, ps, other]
    Collision statistics in a dilute granular gas fluidized by vibrations in low gravity
    Eric Falcon (Phys-ENS), S. Aumaître (LPS), P. Evesque (LMSSM), F. Palencia (ICMCB), C. Lecoutre-Chabot (ICMCB), S. Fauve (LPS), D. Beysens (ICMCB), Y. Garrabos (ICMCB)
    Comments: to be pubished in Europhysics Letters (May/June 2006)
    Journal-ref: Europhysics Letters 74 (2006) 830 - 836
    Subjects: Other Condensed Matter (cond-mat.other); Statistical Mechanics (cond-mat.stat-mech)
11. arXiv:cond-mat/0507303 [pdf]
    Quasi-static mechanics of granular materials
    P. Evesque
    Comments: In French, 9 chapters, 2 appendices, 155 pages, 38 Figures
    Journal-ref: Poudres & Grains NS 1, 1-155, (2000), ISSN 1257-3957
    Subjects: Soft Condensed Matter (cond-mat.soft)
12. arXiv:cond-mat/0507302 [pdf]
    Is the friction angle the maximum slope of a free surface of a non cohesive material?
    A. Modaressi, P. Evesque
    Comments: 21 pages + 1 page, 12 figures
    Journal-ref: Poudres & Grains 12, 83-102, (2001), ISSN 1257-3957
    Subjects: Soft Condensed Matter (cond-mat.soft)
13. arXiv:cond-mat/0507267 [pdf]
    New corner stones in dissipative granular gases
    P. Evesque
    Comments: 46 pages + 1 page, 12 Figures
    Journal-ref: Poudres & Grains 14, 8-53, (2004), ISSN 1257-3957
    Subjects: Soft Condensed Matter (cond-mat.soft)
14. arXiv:cond-mat/0507261 [pdf]
    On few aspects of the dynamics of granular matter
    P. Evesque

Comments: In French ; 34 pages + 1 page ; 11 Figures
Journal-ref: Poudres & Grains 13, 40-73, (2002)
Subjects: Soft Condensed Matter (cond-mat.soft)

15. arXiv:cond-mat/0507196 [pdf]
Limits of isotropic plastic deformation of Bangkok clay
P. Evesque
Comments: 4 pages + 1 page, 1 figure
Journal-ref: Poudres & Grains 14, 4-7, (2004), ISSN 1257-3957
Subjects: Soft Condensed Matter (cond-mat.soft)

16. arXiv:cond-mat/0507194 [pdf]
p=constant compression on loose Hostun sand: The case of an anisotropic response
P. Evesque
Comments: 7pages + 1 page, 1 Figure
Journal-ref: Poudres & Grains 12, 43-49, (2001), ISSN 1257-3957
Subjects: Soft Condensed Matter (cond-mat.soft)

17. arXiv:cond-mat/0507174 [pdf]
Experimental Test of the "Isotropic" Approximation for Granular Materials using p=constant Compression
P. Evesque
Comments: 6 pages + 1 page, 1 figure
Journal-ref: Poudres & Grains 12, 11-16, (2001), ISSN 1257-3957
Subjects: Soft Condensed Matter (cond-mat.soft)

18. arXiv:cond-mat/0507173 [pdf]
Experimental Test of the validity of "Isotropic" Approximation for the Mechanical Behaviour of Clay
P. Evesque, M. Hattab
Comments: 6 pages + 1 page, 1 figure
Journal-ref: Poudres & Grains 12, 5-10, (2001), ISSN 1257-3957
Subjects: Soft Condensed Matter (cond-mat.soft)

19. arXiv:cond-mat/0507095 [pdf]
Experimental Proof of the Existence of a Bifurcation Process During the undrained test in Clay
P. Evesque, M. Hattab
Comments: 4 pages + 1 page, 1 Figure
Journal-ref: Poudres & Grains 12, 1-4, (2001), ISSN 1257-3957
Subjects: Soft Condensed Matter (cond-mat.soft)

20. arXiv:cond-mat/0507093 [pdf]
Trajectories of loose sand samples in the Phase Space of Soil Mechanics
P. Evesque
Comments: 4 pages + 1 page, 1 Figure
Journal-ref: Poudres & Grains 11, 60-63, (2000), ISSN 1257-3957
Subjects: Soft Condensed Matter (cond-mat.soft)

21. arXiv:cond-mat/0507072 [pdf]
How to Fit simply Soil Mechanics Behaviour with Incremental Modelling and to Describe Drained Cyclic Behaviours
P. Evesque
Comments: 9 pages + 1 page, 2 Figures
Journal-ref: Poudres & Grains 11, 49-57, (2000), ISSN 1257-3957
Subjects: Soft Condensed Matter (cond-mat.soft)

22. arXiv:cond-mat/0507071 [pdf]
A new non linear mechanism able to generate avalanches based on soil mechanics
P. Evesque
Comments: 7 pages + 1 page, 1 figure
Journal-ref: Poudres & Grains 11, 42-48, (2000), ISSN 1257-3957
Subjects: Soft Condensed Matter (cond-mat.soft)

23. arXiv:cond-mat/0506669 [pdf]
The Thermodynamics of a Single Bead in a Vibrating Container
P. Evesque
Comments: 26 pages + 3 pages, 13 figures
Journal-ref: Poudres & Grains 12, 17-42, (2001), ISSN 1257-3957
Subjects: Soft Condensed Matter (cond-mat.soft)

24. arXiv:cond-mat/0506658 [pdf]
1-d granular gas with little dissipation in 0-g : A comment on "Resonance oscillations in Granular gases"
P. Evesque
Comments: 10 pages + 1 page, 1 figure
Journal-ref: Poudres & Grains 12, 50-59, (2001), ISSN 1257-3957
Subjects: Soft Condensed Matter (cond-mat.soft)

25. arXiv:cond-mat/0506618 [pdf]
Is Dissipative Granular Gas in Knudsen Regime Excited by Vibration Biphasic ?
P. Evesque
Comments: 17 pages + 1 page, 3 Figures
Journal-ref: Poudres & Grains 15, 18-34, (2005), ISSN 1257-3957
Subjects: Soft Condensed Matter (cond-mat.soft)

26. arXiv:cond-mat/0506611 [pdf]
Distribution of contact forces in a homogeneous granular material of identical spheres under triaxial compression
P. Evesque
Comments: 14 pages + 1 page, 3 figures
Journal-ref: Poudres & Grains 14, 82-95, (2004), ISSN 1257-3957

Subjects: Soft Condensed Matter (cond-mat.soft)

27. arXiv:cond-mat/0506591 [pdf]
    Convection and motion in 2-d embankments under cyclic boundary conditions
    P. Evesque
    Comments: 27 pages + 1 page, 17 figures
    Journal-ref: Poudres & Grains 14, 54-80, (2004), ISSN 1257-3957
    Subjects: Soft Condensed Matter (cond-mat.soft)
28. arXiv:cond-mat/0506590 [pdf]
    Phase transition or Maxwell's demon in Granular gas?
    P. Jean, H. Bellenger, P. Burban, L. Ponson, P. Evesque
    Comments: 13 pages + 1 page, 9 figures
    Journal-ref: Poudres & Grains 13, 27-39, (2002), ISSN 1257-3957
    Subjects: Soft Condensed Matter (cond-mat.soft)
29. arXiv:cond-mat/0506550 [pdf]
    Macroscopic Continuous Approach versus Discrete Approach, Fluctuations, criticality and SOC. A state of the question based on articles in Powders & Grains 2001
    P. Evesque
    Comments: 29 pages + 1 page, 1 Figure
    Journal-ref: Poudres & Grains 12, 122-150, (2001), ISSN 1257-3957
    Subjects: Soft Condensed Matter (cond-mat.soft)
30. arXiv:cond-mat/0506547 [pdf]
    Experimental Stick-Slip Behaviour in Triaxial Test on Granular Matter
    F. Adjemian, P. Evesque
    Comments: 7 pages + 1 page, 5 figures
    Journal-ref: Poudres & Grains 12, 115-121, (2001), ISSN 1257-3957
    Subjects: Soft Condensed Matter (cond-mat.soft); Materials Science (cond-mat.mtrl-sci)
31. arXiv:cond-mat/0506518 [pdf]
    Influence of boundary conditions on 2-fluid Systems under horizontal vibration
    P. Evesque
    Comments: 8 pages + 2 pages + 1 page ; 1 Figure
    Journal-ref: Poudres & Grains 12, 107-114, (2001), ISSN 1257-3957
    Subjects: Soft Condensed Matter (cond-mat.soft)
32. arXiv:cond-mat/0506517 [pdf]
    Comparison between Classical-Gas behaviours and Granular-Gas ones in micro-gravity
    P. Evesque
    Comments: 23 pages +2 page+ 1 page; 5 figures
    Journal-ref: Poudres & Grains 12, 60-82, (2001), ISSN 1257-3957
    Subjects: Soft Condensed Matter (cond-mat.soft)
33. arXiv:cond-mat/0506461 [pdf]
    Are Temperature and other Thermodynamics Variables efficient Concepts for describing Granular Gases and/or Flows ?
    P. Evesque
    Comments: 7 pages + 1 page, 0 Figure
    Journal-ref: Poudres & Grains 13, 20-26, (2002), ISSN 1257-3957
    Subjects: Soft Condensed Matter (cond-mat.soft); Materials Science (cond-mat.mtrl-sci)
34. arXiv:cond-mat/0506460 [pdf]
    The jamming surface of granular matter determined from soil mechanics results
    P. Evesque
    Comments: 2 pages + 1 page, 0 figure
    Journal-ref: Poudres & Grains 11, 58-59, (2000), ISSN 1257-3957
    Subjects: Soft Condensed Matter (cond-mat.soft); Materials Science (cond-mat.mtrl-sci)
35. arXiv:cond-mat/0506421 [pdf]
    On the role of Boundary Condition on the Speed- & Impact- Distributions in Dissipative Granular Gases in Knudsen Regime Excited by Vibration
    P. Evesque
    Comments: 16 pages +2 pages, 3 figures
    Journal-ref: Poudres & Grains 15, 1-17, (2005), ISSN 1257-3957
    Subjects: Soft Condensed Matter (cond-mat.soft); Materials Science (cond-mat.mtrl-sci)
36. arXiv:cond-mat/0506386 [pdf]
    Deformation Modes of a Packing of Rigid Grains: Rotation, Counter-rotation, dislocation field
    P. Evesque
    Comments: 24 pages, 9 Figures
    Journal-ref: Poudres & Grains 11, 19-41, (2000), ISSN 1257-3957
    Subjects: Soft Condensed Matter (cond-mat.soft)
37. arXiv:cond-mat/0506385 [pdf]
    Fluctuations, Correlation and Representative Elementary Volume (REV) in Granular Materials
    P. Evesque
    Comments: 12 pages+1, 4 Figures
    Journal-ref: Poudres & Grains 11, 6-17, (2000), ISSN 1257-3957
    Subjects: Soft Condensed Matter (cond-mat.soft)
38. arXiv:cond-mat/0506344 [pdf]
    A Micro-mechanical Modelling of the Pressure Dependence of the Void Index of a Granular Assembly:
    P. Evesque
    Comments: 12 pages, 1 figure
    Journal-ref: Poudres & Grains 10, 6-16, (1999)
    Subjects: Soft Condensed Matter (cond-mat.soft)

39. arXiv:cond-mat/0506343 [pdf]
Statistical mechanics of granular media: An approach A la Boltzmann
P. Evesque
Comments: 8 pages, no figure
Journal-ref: Poudres & Grains 9, 13-19, (1999)
Subjects: Soft Condensed Matter (cond-mat.soft); Materials Science (cond-mat.mtrl-sci)

40. arXiv:cond-mat/0506342 [pdf]
A Simple Incremental Modelling of Granular-Media Mechanics
P. Evesque
Comments: 12 pages, 4 figures
Journal-ref: Poudres & Grains 9, 1-12, (1999)
Subjects: Soft Condensed Matter (cond-mat.soft); Materials Science (cond-mat.mtrl-sci)

41. arXiv:cond-mat/0506340 [pdf]
On undrained test using Rowe's relation and Incremental Modelling: Generalisation of the notion of Characteristic State
P. Evesque
Comments: 12 pages, 2 figures
Journal-ref: Poudres & Grains 8, 1-11, (1999):
Subjects: Soft Condensed Matter (cond-mat.soft)

42. arXiv:cond-mat/0506339 [pdf]
Stress propagation in granular media: Breaking of any constitutive state equation relating local stresses together by a change of boundary conditions
P. Evesque
Comments: 19 pages, 6 figures
Journal-ref: Poudres & Grains 7, 1-18, (1999)
Subjects: Soft Condensed Matter (cond-mat.soft)

43. arXiv:cond-mat/0506335 [pdf]
On Jaky constant of oedometers, Rowe's relation and incremental modeling
P. Evesque
Comments: 9 pages, 1 figure
Journal-ref: Poudres & Grains 6, 1-9 (1999)
Subjects: Soft Condensed Matter (cond-mat.soft); Materials Science (cond-mat.mtrl-sci)

44. arXiv:cond-mat/0506333 [pdf]
Topology of Roscoe's- and Hvorslev's- Surfaces in the Phase Space of Soil Mechanics
P. Evesque
Comments: 12 pages, 1 Figure
Journal-ref: Poudres & Grains 6, 10-16, (1999)
Subjects: Soft Condensed Matter (cond-mat.soft)

45. arXiv:cond-mat/0506332 [pdf]
Three Comments on "A Simple Incremental Modelling of Granular-Media Mechanics"
P. Evesque
Comments: 6 pages, 1 figure
Journal-ref: Poudres & Grains 10, 1-5, (1999)
Subjects: Soft Condensed Matter (cond-mat.soft); Materials Science (cond-mat.mtrl-sci)

46. arXiv:cond-mat/0202019 [pdf]
Stress fluctuations in granular matter: normal vs. seismic regimes in uniaxial compression tests
F. Adjémian, P. Evesque
Comments: 2 pages, 2 figures
Journal-ref: poudres & grains 13(1) 4-5, (2002)
Subjects: Disordered Systems and Neural Networks (cond-mat.dis-nn); Statistical Mechanics (cond-mat.stat-mec

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For Hal the lab paid a secretary to fill the hall system in 2009-2010 . This is to check the efficiency of Hal. There might be some problem of referencing the lab and its name.
But this kind of problem could be solved from a general archieving processrather from single interface done by each researcher. How to waste time.

G.3.2. & sur Hal

1- fulltext accessible on an other server
Microgravity and Dissipative Granular Gas in a vibrated container: a gas with an asymmetric speed distribution in the vibration direction, but with a null mean speed everywhere
Evesque P.
[hal-00653473 - version 1] (19/12/2011)

2- fulltext access
Effect of aging on the reinforcement efficiency of carbon nanotubes in epoxy matrix
Allaoui A., Evesque P., Bai J.
Journal of Materials Science 43, 14 (2008) 5020-5022
[hal-00366397 - version 1]

3- fulltext accessible on an other server
Boundary conditions and the dynamics of a dissipative granular gas: slightly dense case

Evesque P.
[hal-00653470 - version 1] (19/12/2011)

4- fulltext accessible on an other server
On the complexity/criticality of Jamming during the discharge of granular matter from a silo
Evesque P.
[hal-00646362 - version 1] (29/11/2011)

5- fulltext accessible on an other server
How one can make the bifurcation of Maxwell's demon in Granular Gas Hyper-Critical
Evesque P.
[hal-00645648 - version 1] (28/11/2011)

6- fulltext accessible on an other server
Cyclic Maxwell Demon in granular gas using 2 kinds of spheres with different masses
Evesque P.
[hal-00645645 - version 1] (28/11/2011)

7- fulltext access
Coherent behavior of balls in a vibrated box
Garrabos Y., Evesque P., Palencia F., Lecoutre-Chabot C., Beysens D.
[hal-00115836 – version 1] (23/11/2006)

8- fulltext accessible on an other server
Maxwell demon in Granular gas: a new kind of bifurcation? The hypercritical bifurcation
Leconte M., Evesque P.
[hal-00280491 – version 1] (2008-05-19)

9- fulltext access
Collision statistics in a dilute granular gas fluidized by vibrations in low gravity
Falcon E., Aumaître S., Evesque P., Palencia F., Lecoutre-Chabot C., Fauve S., Beysens D., Garrabos Y.
Europhysics Letters (EPL) 74 (2006) 830 - 836 [hal-00015845 – version 2]

10- fulltext accessible on an other server
On the role of Boundary Condition on the Speed- & Impact- Distributions in Dissipative Granular Gases in Knudsen Regime Excited by Vibration
Evesque P.
Poudres & Grains 15, 2 (2005) 1 [hal-00280475 – version 1]

11- fulltext accessible on an other server
Distribution of contact forces in a homogeneous granular material of identical spheres under triaxial compression
Evesque P.
Poudres & Grains 14, 3 (2005) 82 [hal-00280490 – version 1]

12- fulltext accessible on an other server
Convection and motion in 2-d embankments under cyclic boundary conditions
Evesque P.
Poudres & Grains 14, 3 (2004) 54 [hal-00280489 – version 1]

13- fulltext access
Eléments de mécanique quasi-statique des milieux granulaires mouillés ou secs.
Evesque P.
3ème cycle (2000) [cel-00361501 – version 1]

14- fulltext access
Comment on “Stress Propagation and Arching in Static Sandpiles” by J.P. Wittmer $\mathit(et~al.)$ About the Scaling Hypothesis of the Stress Field in a Conic Sandpile
Evesque P.
Journal de Physique I 7, 11 (1997) 1305-1307 [jpa-00247456 – version 1]

15- fulltext access
Motion of a Single Bead on a Bead Row: Theoretical Investigations
Ancey C., Evesque P., Coussot P.
Journal de Physique I 6, 5 (1996) 725-751 [jpa-00247211 – version 1]

16- fulltext access
Frustration and disorder in granular media and tectonic blocks: implications for earthquake complexity
Sornette A., Sornette D., Evesque P.
Nonlinear Processes in Geophysics 1, 4 (1994) 209-218 [hal-00331031 – version 1]

17- fulltext access
Gravity and density dependences of sand avalanches
Evesque P., Fargeix D., Habib P., Luong M., Porion P.
Journal de Physique I 2, 7 (1992) 1271-1277 [jpa-00246620 – version 1]

18- fulltext access
Relationship between dilatancy, stresses and plastic dissipation in a granular material with rigid grains
Evesque P., Stefani C.
Journal de Physique II 1, 11 (1991) 1337-1347 [jpa-00247595 – version 1]

19- fulltext access
Granta Gravel model of sandpile avalanches: towards critical fluctuations?
Evesque P.
Journal de Physique 51, 22 (1990) 2515-2520 [jpa-00212550 – version 1]

20- fulltext access
Comment on: "Convective flow of granular masses under vertical vibrations" (C. Laroche, S. Douady and S. Fauve, J. Phys. France 50 (1989) 699-706)
Evesque P.
Journal de Physique 51, 8 (1990) 697-699 [jpa-00212400 – version 1]

21- fulltext access
FOUR-WAVE MIXING TECHNIQUE AND COHERENCE EFFECT ON ELECTRONIC STATES OF DYE MOLECULE
Portella M., Montelmacher P., Bourdon A., Evesque P., Duran J.
Journal de Physique Colloques 48, C7 (1987) C7-521-C7-523 [jpa-00226942 – version 1]

22- fulltext access
TRANSIENT GRATING EXPERIMENTS IN PERCOLATION FRACTALS
Evesque P., Duran J., Bourdon A.
Journal de Physique Colloques 46, C7 (1985) C7-45-C7-49 [jpa-00224957 – version 1]

23- fulltext access

Spectroscopic study of the incommensurate phase of ThBr4 via the optical and magnetooptical properties of U4+
Briat B., Delamoye P., Rivoal J.C., Hubert S., Evesque P.
Journal de Physique 46, 8 (1985) 1375-1386 [jpa-00210081 – version 1]

24- fulltext access
Energy migration in randomly doped crystals : geometrical properties of space and kinetic laws
Evesque P.
Journal de Physique 44, 11 (1983) 1217-1224 [jpa-00209707 – version 1]

The above example means that automatic referencing by Hal is not a success.

Table des Matières :

Contents :

Contents of « Témoignage n° #1 » de P. Evesque
At CL – MSSMat on 23 Juin, 2011

« on some problem with the editorial politics of peer reveviewing »

[48] The page numbering of each testimony is the one of the electronic pdf format at http://www.mssmat.ecp.fr/.

Rapport CNRS :

I wrote this rapport CNRS 2009-2010 (testimony #1, pp. 161-272) to report most of the problems I had to overpass these last few years (overworking, heartattack, AVC, administrative harassement,…). It relates also a history of the evolution of my working interest and of the working location (p.166- 171 & 194). The recent advancement in granular gas theory, simulation and interpretation are reported in Testimony #1, p.172- 176. The problem met by F. Douit in lab MSSMat is reported shortly in Testimony #1, pp.181, 196-197 & 201-202.

I do not find fair the role plaid either by the lab management (p.203), nor by the CNRS administration (p196-197, 198-199, 201-202) to support my research, nor to support the grants, nor by CNRS peer reviewing (pp. 211-218). Not discussion happpen after my remarks (pp204-206) about research work. Nor on the one about scientific deontology (pp. 207-210). Only criticismls came from evaluation teams, which were not able to evaluate correctly, even the number of my publications (pp. 211-218)…

With the rules used, one will gain to be managed by non scientists…

Contents of « Témoignage n° #2 » de P. Evesque,
to CL – MSSMat on 16 Decembre, 2011

« on evaluation of research proposal & peer reveviewing »

[49] The page numbering is the one of the electronic pdf format at http://www.mssmat.ecp.fr/

Contents of « Témoignage n° #3 » de P. Evesque, to CL – MSSMat on 13 Mars, 2012

"On instances which should observe, promote & respect scientific deontology"

On other problems

I encountered in peer reviewing, which I will speak too

Other problems can merge from peer reviewing. Here are few examples. One come from hiding results known by the authors coming from a different other scientific (1). The next one merges from authors who try their data not to cancels results from other previous authors (2) or hiding disagreement between their results and the concepts they want to study (3).

2) Autre problème : 1.a. Pb de Nature Ottino ref: Nature, 2 March 1995 ; Guy Metcalfe**,** Troy Shinbrot**,** J. J. McCarthy **&** Julio M. Ottino ; Avalanche mixing of granular solids ; Nature **374**, 39-41 doi:10.1038/374039a0	T #4 ; F2; p. 17
1.b. My PhD dissertation (thèse d'Etat):	T#4; F6; p.20
2) Problème de reviewing non sérieux fait par les agences à mon égard: 2.a. Passage CR2-CR1 section 13 (ex section 5) 2.b. Comentaire de J. Villain à la section 5 du cnrs (1989) 2.c. Commentaire de M.Frémont sur mon stage à l'umr 113-(LCPC) (14/12/1990) 2.d. commentaires de la section 5 cnrs (2001-2008) 2.e. CNES, ESA : 2.f. avec l'appui de l'article N.Vandewalle (sans y toucher) il faut pouvoir discuter avec les autres acteurs pour pouvoir faire comprendre la stratégie utilisée à mauvais escient: Pb de Vandewalle et Minitexus.	See #4 ; p. 20
3) Problème de reviewing pour NSF du proposal Behringer & P&G 2001 ;	See #4 ; p.

List of Acronysms:

note	pages	label
5,16	5,19	Témoignage #1, #2, #3, at http://www.mssmat.ecp.fr/, password needed (ask te lab Director).
6,10	6,14	***Poudres & Grains*** *(P&G)*
7,16	7,19	***Testimony series***,
11	14	**AEMMG :**
12	14	**Powders & Grains meeting:**
15	16	Commission #5 of CNRS
19	24	**Naphthalene research field:**
20	27	**Research on granular media:**
22,23	28,29	**Scope on situation about Dynagran and Vip-Gran in 2011-2012:**
24	31	Website **TT Vip-Gran**
26	42	CNRS
27	42	COMETS
28	42	Ac Sc : French Academy of Sciences ; US Ac Sc : American (National) Academy of Sciences
29	44	CNES : Conseil national de l'enseignement supérieur et de la recherche
30	44	INSERM : Institut national de la santé et de la recherche médicale
31	44	ANR : Agence nationale de la recherche
32	44	ECP : Centrale Paris, Ecole centrale des Arts et Manufactures
33	45	SFP : Société Française de Phyique
34	45	CODHOS
35	45	CNESER
37	45	AERES
39	47	DAR
40	64	CNER : Comité National d'Ethique de la Recherche ; COMETS : Comité d'Ethique du CNRS
41	64	COMETS : CNRS Committee of Ethics
42	64	NSF : National American Science Foundation
43	67	BNF : Bibliothèque Nationale de France, soit French National Library
46	75	cour des comptes : Revenue court
47	76	ANVAR : Agence National pour la valorisation de la recherche, or National Agency for the Promotion of Research

List of Acronysms:

note	pages	label
28	42	Ac Sc : French Academy of Sciences ; US Ac Sc : American (National) Academy of Sciences
11	14	**AEMMG :** Association pour l'étude de la micromécanique des milieux granulaires
37	45	AERES : Agence d'évaluation de la recherche et de l'enseignement supérieur
31	44	ANR : Agence nationale de la recherche
47	76	ANVAR : Agence National pour la valorisation de la recherche, or National Agency for the Promotion of Research
43	67	BNF : Bibliothèque Nationale de France, soit French National Library
40	64	CNER : Comité National d'Ethique de la Recherche ; COMETS : Comité d'Ethique du CNRS
34	45	CODHOS : Comité de Défense des Hommes de Science
27	42	COMETS : CNRS commission of ethics
41	64	COMETS : CNRS Committee of Ethics
15	16	Commission #5 of CNRS: physics section of National Committee
29	44	CNES : Centre national d'études spatiales
35	45	CNESER : Comité National de l'enseignement supérieur et de la Recherche ;
26	42	CNRS : Centre national de la recherche scientifique
46	75	Cour des Comptes : Revenue court
32	44	ECP : Centrale Paris, Ecole centrale des Arts et Manufactures
		ESA : European Space Agency
39	47	DAR : Demande d'Aide à la Recherche au CNES.
22,23	28,29	Dynagran Sino-French project of experimental set-up for SJ-10, to study vibrated granular media
30	44	INSERM : Institut national de la santé et de la recherche médicale
		ISS : International Space Station
19	24	Naphthalene research field:
42	64	NSF : National (American) Science Foundation
6,10	6,14	*Poudres & Grains (P&G)*
12	14	Powders & Grains meeting:
20	27	Research on granular media:
22,23	28,29	Scope on situation about Dynagran and Vip-Gran in 2011-2012:
33	45	SFP : société française de Physique
		SJ-10 : Chinese satellite
7,16	7,19	*Testimony series*,
5,16	5,19	Témoignage #1, #2, #3, at http://www.mssmat.ecp.fr/, password needed (ask te lab Director).
24	31	**TT Vip-Gran** : ESA Topical Team for Vip-Gran instrument
22,23	28,29	Vip-Gran: project of experimental set-up for ISS, to study vibrated granular media
24	31	Website **TT Vip-Gran**

Contents

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[50] Parts of this text was published in mid-May 2012 on the blog of "peer review 2012", congress on science of information…, to stand on 17-20 juillet 2012, in Orlando, USA. This text was revised and extended toget this version. *Ref:* http://peerreviewing.wordpress.com/2012/04/23/peer-review-is-it-effective-is-it-possible-to-improve-its-effectiveness-is-there-other-means-to-evaluate-research/#comments.