Documents de travail

du Laboratoire d'Economie et de Gestion

Working Papers

ON HOCHBERG ET AL.'S "THE TRAGEDY OF THE REVIEWER COMMONS"

Louis de MESNARD

Université de Bourgogne & CNRS
UMR 5118 Laboratoire d'Economie et de Gestion
Pôle d'Economie et de Gestion, 2 boulevard Gabriel, 21000 Dijon, France

The original publication is available at www.springerlink.com
DOI: 10.1007/s11192-009-0141-8

e2009-16
Equipe Analyse et Modélisation des Interactions Economiques (AMIE)

On Hochberg et al.'s "The tragedy of the reviewer commons"

Louis de Mesnard

Résumé

Nous discutons ici de chacune des recommandations faites par Hochberg et al. (2009) pour éviter la « tragédie des referees comme bien commun ».

Si les journaux scientifiques partagent une base de données commune des referees, cela va recréer une organisation bureaucratique où des considérations extrascientifiques prévaudront. Faire pré-référer les manuscrits par des collègues est une pratique répandue, mais soulève des problèmes de coordination. La révision des manuscrits suivant toutes les recommandations des referees suppose que les recommandations convergent, ce qui est une hypothèse peu crédible. Faire signer un engagement selon lequel les auteurs ont bien pris en compte toutes les observations des referees est à la fois autoritaire et stérilisant. L'envoi des commentaires antérieurs avec les soumissions futures à d'autres revues revient à créer une entente monopolistique et un seul journal qui englobe tous les autres, ce qui est stérilisant à nouveau. Utiliser des jeunes chercheurs comme referee est très risqué: ils peuvent être très sévères ; et s'ils n'ont pas encore eux-mêmes publié, la recommandation viole le principe du referee par les pairs. Demander aux referees d'être plus sévères ne ferait que créer une crise dans les maisons d'édition et accroîtrait en général la charge de travail. La critique du comportement des auteurs qui cherchent à publier dans les meilleures revues est injuste : il est naturel pour les chercheurs de chercher à publier dans les meilleures revues et de ne pas se résigner à appartenir à la deuxième catégorie. Punir les referees paresseux conduirait seulement à diminuer la qualité des rapports : au lieu de cela, nous sommes en faveur de l'idée de payer « en nature » avec, par exemple, des livres ou des articles gratuits.

Mots-clés

Referee; Rapporteur ; Editeur; Maison d'édition; Tragédie des Communs; Hardin; Hochberg

Abstract

We discuss each of the recommendations made by Hochberg et al. (2009) to prevent the "tragedy of the reviewer commons".

Having scientific journals share a common database of reviewers would be to recreate a bureaucratic organization, where extra-scientific considerations prevailed. Pre-reviewing of papers by colleagues is a widespread practice but raises problems of coordination. Revising manuscripts in line with all reviewers' recommendations presupposes that recommendations converge, which is acrobatic. Signing an undertaking that authors have taken into accounts all reviewers' comments is both authoritarian and sterilizing. Sending previous comments with subsequent submissions to other journals amounts to creating a cartel and a single all-encompassing journal, which again is sterilizing. Using young scientists as reviewers is highly risky: they might prove very severe; and if they have not yet published themselves, the recommendation violates the principle of peer review. Asking reviewers to be more severe would only create a crisis in the publishing houses and actually increase reviewers' workloads. The criticisms of the behavior of authors looking to publish in the best journals are unfair: it is natural for scholars to try to publish in the best journals and not to resign themselves to being second rate. Punishing lazy reviewers would only lower the quality of reports: instead, we favor the idea of paying reviewers "in kind" with, say, complimentary books or papers.

Keywords

Reviewer; Referee; Editor; Publisher; Publishing; Tragedy of the Commons; Hochberg

1 Introduction

In a recent paper, Hochberg et al. (2009) discuss of "the tragedy of the reviewer commons", by reference to Hardin's "Tragedy of the Commons".[1] For them, the problem is that scholars try to publish in the highest quality journal possible even if it is not the appropriate forum for their work; they try to subdivide their works so as to maximize the number of their publications; they resubmit their rejected papers to other journals thereby increasing the number of referees needed to review a given paper; not all experienced scholars accept to review; authors ignore reviewers' recommendations even if they are of a general character; many authors consider the reviewing process as stochastic: they think the more they resubmit the more chance they have of being accepted, but this also increases the chances of stumbling upon the same reviewer. Hochberg et al. recommend sharing reviewer databases among journals; pre-reviewing of papers by authors' colleagues; revising manuscripts in line with reviewers' recommendations and signing an undertaking that authors have taken into account all reviewers' comments; sending previous review comments to journals to which the paper is resubmitted; and finally, using young scientists (senior post-graduate students and post-docs) as reviewers.

We shall discuss the suitability of Hochberg et al.'s recommendations especially in the social sciences.

2 Discussion

2.1 Sharing reviewer databases

Sharing the databases of reviewers, and consequently the reviewers themselves, is certainly not a good idea if such sharing is among publishing houses: an "industry watchdog" would certainly find that this creates a publishing cartel. Sharing the reviewer database within a single publishing firm does not pose the same problem but a single publisher seldom has two or more equivalent journals in its portfolio, that is, journals where the same referees may serve two or more journals. For example, in Regional Science, while the *Journal of Regional Science*, *Papers in Regional Science* and *Economic Geography* belong to Wiley and the *International Regional Science Review* and *Urban Studies* to Sage, *The Annals of Regional*

[1] We will not discuss here whether this image, which comes from environmental sciences (Hardin 1968), is appropriate.

Science is with Springer while *Regional Studies* belongs to Taylor and Francis, the *Journal of Urban Economics* to Elsevier, the *Journal of Economic Geography* to Oxford, but the *Canadian Journal of Regional Science* is independent, etc. It should be noticed that only the first two of Wiley's journals are really equivalent while Sage's two journals are not. In Regional Science, there are journals devoted to regional science strictly speaking, others to urban studies, and yet others to economic geography, and their referees are not interchangeable.[2]

Sharing reviewers among the journals of all publishing houses would amount to forming a *Reviewing Authority*, which would confer very substantial power on its members. Such an Authority would be a bureaucratic organization that will inevitably adopt the behavior of Soviet Union's Academy of Science: its members and their research institutes were not poor scientists, far from it, but the academicians had the tendency to dictate what was good and bad depending of the government's desires, which led to aberrations such as those of Lysenko under Stalin's regime. As soon as a Reviewing Authority is formed, the classical sociological law will apply: the Reviewing Authority will tend to become an organization with its own rules, where what is good and what is bad is not dictated by scientific considerations. Von Mises (1944, pp. 104–108) underlines that the bureaucratic system will inevitably lead its members to lose any critical sense, which is annoying for a reviewing process.

Moreover, one might think at first glance that the Reviewing Authority's reviewers would give precedence to their own school of thought over others, helping their own papers or those of their friends to be published while systematically rejecting their rivals' contributions, and so on, what might be called "cliquishness"; this is the case in some fields such as economics, where war rages between neoclassical and heterodox economists, the leading two schools of thought.[3] However, it is the great merit of Snizek and Fuhrman (1979a) and Snizek, Fuhrman and Wood (1981) to have demonstrated convincingly for book reviews in sociology (by counting the number of positive and negative sentences) that reviewers are much harsher with authors from their own school of thought. For Roberts (2009, p. 891),

---

[2] Moreover, journals sometimes change publisher (e.g.: *Papers in Regional Science* passed from Springer to Wiley in 2005).

[3] As an illustration of the "cliquishness" in economics, Süssmuth, Steininger and Ghio (2006) denounce the "institutional oligopolies" between editors and authors that may exist in European economic institutions.

reviewers are often competitors of the author: “The [reviewing] process is like asking Ford to review plans for a new car design by General Motors without compensation”. Here, ability to fully understand the content, but also the sense of superiority, combined with jealousy and a sense of competition, explain this behavior.[4] Actually, such behavior is also observed in economics: reviewers, evaluators, etc. tend to be much more critical of contributors whose research is very close to their own interests. Nor are the two forms of behavior incompatible: the same reviewer may reject anyone from another school of thought—his “enemies”—and, at the same time, slaughter his “allies”. Overall, neutrality has little chance of being the rule: there is a bias! One could argue also that other countries have Academies of Science, but they do not play such a role:[5] the argument here is not that all Academies of Sciences are bad, but that any team of official reviewers will tend to behave like the Soviet Academy of Sciences did. They will also be led—the flesh is weak—to state in their résumés that they are members of such teams; other scientists will court them, with the ensuing dangers of favoritism, corruption, and such like. In contradistinction, having a large turnover of anonymous reviewers will lead them not into temptation.

Hochberg et al.’s proposal would be tantamount to merging an array of journals into a single large journal will be very detrimental to science because it will see the creation of an “official science” and remembers the *Literaturnaya Gazeta*, the official organ of the *Union of Soviet Writers* during the Stalin era and thereafter until 1990. There are examples of papers that were initially rejected before being found valuable many years later and more generally, there is no shortage of examples of theories that have been ignored or ridiculed (Campanario 1996, 2009).[6] This is true in the social sciences: unorthodox papers are often rejected however good they might prove, as with Coase’s 1936 paper that was forgotten for forty years before being unearthed by Williamson;[7] examples abound in the “hard” sciences too: for example, Wegener’s plate tectonics was ignored until the second part of the twentieth century. This is why it is good to leave authors a chance of being published somewhere.

Moreover, sharing databases is unworkable because the level of specialization of each discipline is increasing over time, with the creation of sub-fields that tend rapidly to create

---

[4] Snizek and Fuhrman (1979 b) indicate that, in this matter, no bias is found between journals.
[5] This is discussed for today’s Russia Academy of Science; see Fortescue (1992).
[6] That does not mean that the peer-review process is never able to select the best papers (Bornmann and Daniel 2008).
[7] Coase won the Nobel Prize in Economics in 1991 and Williamson in 2009.

their own journals. For example, in sociology, there are one hundred different areas of specialization; in economics, the *Journal of Economic Literature* (AEAWeb 2009) runs to 20 main sub-fields (from letters A to R plus Y and Z); each sub-field includes sub-sub-fields, e.g. B2, C7, etc., making a total of 134. Each sub^2-field can be further declined in sub-fields, yielding a grand total of 787 distinct sub^3-fields! Some, such as A31 (collective writings of individuals) or B32 (history of economic thought of individuals, obituaries) are not really sub-fields, but nearly all of them are meaningful divisions; for example, B13 concerns the history of neoclassical economic thought up until 1925, while B14 is for socialist and Marxist economic thought: not the same topic! Some sub^3-categories are repeated two or three times; one particular author has the right to use many categories for a given paper, which divides the grand total. However, the renowned *JEL* has created such categories precisely for the purpose of finding reviewers; it has spawned this plethora of categories because each has its own reason for being. Each economist should be able to say to which categories (generally from one to four or five) his work belongs. Thus, sharing a same reviewer among a handful of journals publishing in the same sub-fields of the same discipline may be possible, but it is certainly not for a large number of journals: the complexity becomes virtually infinite. Dreaming about “reviewers in economics” is unrealistic: outside of their own three or four sub^3-fields, economists are generally incompetent. The problem becomes much more complicated if we consider that many disciplines have overlaps and their boundaries are largely fuzzy...

Finally, it should be added that collating all reviewers in the same database will inevitably increase the workload of each of them—especially the most renowned or those who produce their reports in due time—because they will be asked for reports by many journals; this will very likely lead to overburdening a small number of reviewers, which is exactly the opposite of what Hochberg et al. were trying to do.

2.2 Pre-reviewing papers by authors’ colleagues

Pre-reviewing papers is an interesting idea but it is either already practiced or unrealistic. Hochberg at al. seem to overlook working papers. Scholars produce working papers so as to be read and criticized: this is one form of pre-reviewing.[8] However, one might

[8] See Frandsen and Wouters (2009) on the process that transforms a working-paper into a journal article in the field of Economics.

take another point of view. Scholars who ask their colleagues to pre-review their manuscripts are effectively asking them to work for nothing: pre-reviewing is only possible among small groups of colleagues exchanging free services; in this case, they are often co-authors, like Hochberg et al.! This suggests that one solution may lie in co-authoring. However, to be able to improve the quality of the paper, co-authoring must satisfy certain conditions. Co-authors must work together on the job, as peer authors and not as factory workers, which precludes such savings. However, the laws of organization science hold in this case: the group must be rather small. Indeed, beyond four or five, the number of possible interactions explodes combinatorially; for *n* authors, there are $\sum_{i=1}^{n}\binom{n}{i} = 2^n - 1$ possible groupings of authors, that is, seven possibilities for three authors, fifteen for four authors, thirty-one for five, sixty-three for six, one thousand and twenty-three for ten, etc.; this is unmanageable unless a structured organization is formed among co-authors, with a leader, a division of labor, etc. However, a horizontal division of labor between co-authors (i.e. one collates the bibliography, one develops the model, one does the computations, another drafts the manuscript, etc., which is a source of economies, as in "hard science" laboratories) does not increase the quality of the paper but only reduces the cost (in terms of time) for producing it. What is more, it has been shown that co-authoring may even slow down the reviewing process: Hartley (2005) shows that in psychology journals single-author papers are reviewed faster than multiple-author ones. It is also known that co-authoring does not prevent bad papers from being produced: we have all reviewed bad papers written by groups of co-authors; Zi-Lin (2009) shows that internationally co-authored papers "does not have more epistemic authority".[9]

Another point is that pre-reviewing may facilitate piracy, cribbing, and so on: an author sends a paper to a colleague who makes some insignificant remarks and then publishes something of his own on the same subject with same data and same conclusion. It is probably more difficult to ensure that colleagues respect the reviewers' code of ethics (Finney 1997) if they are not official reviewers. Publishing a manuscript as a working paper may reduce this risk of piracy by colleagues but even working papers may be hijacked.

Moreover, colleagues cannot do double-blind reviewing as they know who the author of the paper is. It has been demonstrated that the acceptance rates are lower with double-blind rather than single-blind reviewing (Blank 1991). One may conjecture that colleagues will be

[9] On the number of authors and the number of papers see, for example Egghe (2008).

indulgent, not wanting to hurt the author's feelings, whereas anonymous referees have no such qualms.

2.3 Revising manuscripts following reviewers' recommendations

One of the main issues in Hochberg et al.'s analysis is that they assume implicitly that reviewers' recommendations converge toward equilibrium, that is, they all tend toward (i) an improvement of the paper and (ii) the same improvement.

The first point is actually an act of faith: no reviewing process is perfect. Snizek et al.'s findings (1979a; 1979b; 1981) apply again. Seglen (1996) reports that peer review for biomedical journal articles is strongly biased towards overestimating low-content articles and underestimating high-content papers. To the contrary, Patterson and Harris (2009) show that the "quality" of papers is significantly but loosely correlated with the mean quality-score assigned to manuscripts by two independent reviewers for the journal *Physics in Medicine and Biology*. However, for Lindsey (1988) who reviews the literature on the topic, there is little correlation between the quality of the paper and reviewers' opinion: reports are imprecise; see also the discussion by Hargens and Herting (1990a). Mayo et al. (2006) show that—for reviewing granted projects but not for manuscripts—it is better to use many reviewers instead of two: with two reviewers, chance plays a big part. Moreover, the reviewer's role must not be that of a co-author of the manuscript: a reviewer must point out major flaws in the paper, errors, unscientific approaches, gratuitous assertions, lack of clarity, etc., but must not try to redirect the author's thinking and must remain respectful of the author.

On the second point Fiske and Fogg (1990) explain that the reviews often deal with different points. Van Rees (1987) shows that "reaching the agreement involves a mode of orchestration" (for literary works where reviewers normatively create their own criteria for analyzing the work: these criteria are not intrinsic). Cicchetti (1991) and Weller (2001) show that different reviewers often evaluate the same paper "quite differently" but Hargens and Herting (1990b; 2006) show that reviewers' recommendations are not statistically independent and that reviewers and editors judge the manuscripts on a "general quality dimension". Nevertheless, for Lindsey (1988, p. 80)

> *For journal editors who need to make a decision regarding submitted manuscripts, and faced with conflicting recommendations, the appeal of particularistic standards becomes especially strong. This appeal could be justified as perhaps making use of*

> *the best available data. However, it would be a clear violation of the norms of science…*

In the “soft” sciences such as the social sciences, nothing is completely true or false, black or white: grey prevails and the paradigm consensus is rare. Hence reviewers’ recommendations may lead manuscripts being modified in ways that are unacceptable to other reviewers (who might even have considered the initial manuscript acceptable)! There is a joke among economists: if you take two economists, you have three theories… So taking into account one reviewer’s recommendations is no guarantee of obtaining what will be a better paper in another reviewer’s eyes. This is truer if the paper is an interdisciplinary one in the social sciences.

As Hochberg at al. emphasize, some recommendations to improve the reviewing process are of a general character: the paper is not sufficiently structured, the English is poor, the introduction introduces nothing, the conclusion is unclear, etc. Our criticisms do not apply to this type of recommendation. However, for all other recommendations, those criticisms do hold. This is particularly relevant when the recommendation is “you must cite author *X*!”: this may offend other reviewers, for example because they dislike *X* and their school of thought, they prefer *Y* (perhaps the reviewer even belongs to school *Y*). It soon becomes impossible to satisfy all the reviewers. Moreover, the reports may be contradictory: reviewer *A* makes a recommendation, say *a*, but the second reviewer, *B*, recommends *b*, except that *a* is incompatible with *b*. Cycles may also occur! For example, a reviewer *A* makes a recommendation, say *a*, but the paper is finally rejected. The author resubmits elsewhere introducing the recommendation *a* in the new version of the manuscript. A new reviewer *B* dislikes recommendation *a* and recommends *b* but the paper is rejected even so. The author introduces *b* for a second resubmission but a third reviewer *C* recommends *a*! Such cycles may even occur within the same journal. Many authors lose heart and their wits.

Clearly, Hochberg et al. forget that science is not a linear process but a chaotic one, where *catastrophes* in Thom’s sense occur. Their recommendations amount to reducing the diversity of science in order to speed up the reviewing process: now speed is a good thing, but not when it is to the detriment of the diversity of ideas and findings. Moreover, there is absolutely no certainty that accelerating the reviewing process improves the *ex post* quality of papers: Lee et al. (2003) teach us that the scientific impact of a paper cannot be judged prospectively but only after recognition has been achieved. Similarly, it is not certain that speeding the reviewing process can prevent misconduct in research work (see for example

Bornmann, Nast and Daniel 2008): none of the 527 editors' and reviewers' criteria—that they assign to nine categories—are related to possible fraud concerning the data used in the paper.[10]

2.4 Signing an undertaking that authors have taken into account all reviewers' comments concerning previous submissions

This point logically follows from the preceding one.[11] It is hard to imagine that such a commitment would stand up in court (leaving aside the question of determining which court would have jurisdiction…): it would be very hard for a judge to understand to what extent an author has followed reviewers' recommendations, unless he has simply ignored them. An author may easily escape all of these constraints by changing the title and rewriting the paper a little, but that is hardly a solution. Hochberg et al.'s recommendations smacks of totalitarianism: they forget that authors may well think that reviewers' recommendations are poor, or even unacceptable. They must retain the right to ignore reviewers' recommendations, even if this means writing a note to the reviewers and the editor to explain why. The journal articles must remain the work of the author(s) alone under his/their sole responsibility and must not become a collective endeavor of author(s) and reviewers. Authors must retain full intellectual property rights in their work and sole responsibility for and authorship of their ideas.

Moreover, Hochberg et al. have an odd view of what intellectual output should be: they want a consensus! What they want is a process producing "legal" documents, duly certified by a certification authority composed of the reviewers and the editor, sole holders of the Truth. Consensus kills science: even if a temporary consensus is possible in the "hard" sciences (Kuhn would speak of *paradigms*), the paradigms are there to be falsified in Popper's sense. This is very obvious in the social sciences.

---

[10] Additionally, Nisonger (2002) shows that the relationship between editorial boards composition cannot serve to predict the quality of the journal in business, political science, and genetics journals.

[11] Hochberg et al. propose the following formulation: "We confirm that should our study have been previously submitted to another journal, we have taken all reviewers comments into account in revising our manuscript for submission to…" (2009, p. 3). They think that it could avoid revealing that the paper has been rejected before: this is an illusion because nobody withdraws an accepted paper!

Let us take an example from the "hardest" part of the "soft" sciences. In economics, an author who uses Factorial Analysis methods may be asked to use Econometrics instead, because Econometrics is *à la mode* (and perhaps the reviewer is unaware of what Factorial Analysis is…): why should our author follow this recommendation if his results are interesting and the Factorial Analysis has been properly applied? Or again, a macroeconomic paper is very good, correctly formalized, with interesting findings, etc., but non-neoclassical, e.g. Keynesian, Marxian, etc.; the reviewer criticizes the lack of "scientificity" and recommends switching to a more standard macroeconomic theory (the "new macroeconomic theory"); why should the author comply if the paper is perfectly fine otherwise? A definitive consensus in a discipline signs the death warrant of Science; this should be particularly obvious in ecology today, the discipline of Hochberg et al.!

2.5 Sending previous comments to the journal where the paper is resubmitted

Hochberg et al. (2009) cite the possibility of sending previous comments to the journal to which the paper is resubmitted.[12] Indeed, some journals, including *The Economic Journal*, ask authors to provide previous reports if their paper has been submitted elsewhere.[13] This sounds a good idea, particularly when the comments concern crucial points. For instance, Bornmann, Weymuth and Daniel (2009) examine the case of resubmitted papers in chemistry. They demonstrate that negative comments in the areas "Relevance of contribution" and "Design/Conception" are a clear sign that the paper will never be published in a high-impact journal while negative comments in other areas ("Writing/Presentation," "Discussion of results," "Method/Statistics," and "Reference to the literature and documentation") are not significant; here, the first two areas are crucial.

---

[12] "Moreover, some journals are now asking authors of rejected manuscripts for permission to forward the reports of consenting reviewers to the journal where the authors intend to submit the revised study" (Hochberg et al. 2009, p. 3).

[13] This can be found in the website of *The Economic Journal* (published by Wiley-Blackwell): *To improve speed and quality of decisions we encourage authors when submitting to us to include editors letters and referee reports from failed submissions at other journals. We of course reserve the right to use our own referees and provide our referees with copies of this correspondence but believe this step will be attractive to authors and further speed up the submission process.*
However, this policy seems to be particular to this journal and not systematic in Wiley-Blackwell.

Again, the proposal may be easily bypassed by a change of title and a little rewriting. However, such a recommendation—sending previous comments to the journal where the paper is resubmitted—amounts to organizing a cartel of journals if it is systematically followed by many journals! It is doubtful whether a regulating authority—if there were one—would accept such highly cooperative behavior. And one may wonder how this could be enforced. Consider an author who signs a commitment when submitting a paper to the publishing house *X*. How can he be forced to respect that commitment when resubmitting with the publishing house *Y*, given that any commitment becomes obsolete whenever the paper is rejected? And once again, cheating is very easy (changing the title, etc.). Need we involve the police and the courts in science? Combating scientific fraud is far more important than checking whether the thousands and thousands of authors play by the rules. In short, Hochberg et al.'s (2009) recommendation introduces a constraint on each author that is unlimited in time. This is unacceptable. While it is acceptable to restrict the author's right to publish elsewhere or to reproduce the paper if it is accepted, it is unacceptable to restrict his intellectual property rights if the paper is rejected. This amounts to marking "poor" papers with the seal of infamy!

Nevertheless, there is a different but important point. Hochberg et al. (2009) give preeminence to the initial reviewers over subsequent ones. If we follow Hochberg et al. and consider that the authors have chosen the initial journal (let us call it journal 1) at random, one cannot see why the recommendations of its reviewers A_1 and B_1 would be better than those of reviewers A_2 and B_2 of journal 2, A_3 and B_3 of journal 3, and so on. However, what A_2 and B_2 say about the manuscript might be as judicious as what has been said by A_1 and B_1, or even contradictory, while it is the recommendations of A_1 and B_1 that influence the paper's future more. Obviously, reviewers A_2 and B_2 may ask the author to ignore A_1 and B_1's recommendations. However, the reviewing process would then turn into a discussion among reviewers, with those of journal 2 criticizing those of journal 1, those of journal 3 criticizing those of journal 2 and 1, etc., and losing sight of the discussion of the author's manuscript! Such discussion is not a bad thing in itself, it is even the core of the scientific approach and some journals do from time to time publish scientific disputes, but it must be conducted in the open, in plain view, and not behind closed doors and in the relative silence of a reviewing process.

One might argue that the last journal could publish the entire discussion (plus the original paper and all its releases…) but this would come to a lot of pages for each published paper. I doubt that the publishing houses would be happy about that; and reading these pages might be dull. This would lead to a scientific dispute about each paper, but with the horse before the cart: instead of a paper and a discussion of it in its final form, we would have a discussion to explain how that final form has been reached. This might at a push prove interesting for specialists of the history of thought, in years to come… Imagine a movie made in this way; the audience would have to watch the movie and then watch a lengthy "making of" to explain why we have not seen the original movie (which would then have to be projected for the sake of comparison…): the cinemas would be empty but for a handful of film buffs.

2.6 Using young scientists as reviewers

Using young scientists as reviewers—senior postgraduate students[14] and post-docs—is not a bad idea, nor is it a new one (Min 2005). However, a proverb says that wisdom comes with maturity. A wizened reviewer may be no less incisive than a younger one and will also able to ponder criticism with experience. Younger reviewers might more readily believe that anything other than their own approach is bad. In a word, pluralism may suffer. Inexperienced young scholars are more critical and demanding than older, more experienced ones who are well established in a given field. Very young scholars lack tangible proof of their stature in the field: the mirror is not yet in their hands. Hence, they try to have a highly professional attitude and are very demanding. Older scholars "have nothing to prove": they can be more level-headed.[15] Godoy (2004) submitted the same manuscript to 17 novice reviewers and three experienced experts. The eloquent results are resumed in the following quotation of (2004, pp. 4–5):

[14] The term is a telling one: to my mind, a senior postgraduate is a post-graduate who is not getting on with his thesis. I would prefer "PhD student".

[15] Hall's classical theory (1968) may be called to back up this assertion. Hall established a correspondence between attitudes of professionalism and the behavior of professionals, and made a distinction between occupations and professions: "…occupations which are attempting to become professions may be able to instill in their members strong professional attitudes, while the more established professions may contain less idealistic members". This principle can be transposed to the set of PhD students and post-docs who are not yet really researchers and those who are experienced academics.

> *An analysis of the contents of the reviews shows that the novices concentrated on more superficial aspects, such as on the quality of the written communication, but failed to identify the strengths and weakness of the manuscript. Furthermore, they were not able to make suggestions to the author regarding ways to improve the manuscript or the research. This produced a review which was not very useful in helping the editor to make a decision. The final recommendation of novices was towards accepting the manuscript; in contrast, the experts were unanimous in rejecting the paper in this particular case.*

Moreover, Hochberg et al.'s proposal violates the principle of peer review. This is not to say that professors' papers must be reviewed by professors, lecturers' papers by lecturers and professors, etc., but a scientific paper must be reviewed by a confirmed scientist, not by someone who has not yet proved their ability to write a scientific paper on their own. A PhD student may perfectly well serve as a referee if he has already published papers, which is the case of most post-docs (previous publications are a selection criterion). There is one exception, though, that cannot be admitted: when the reviewing job consists in verifying some scientific experiment or a complicated piece of mathematics; but this amounts to considering young scientists are pliable to a fault, which is not very courteous.

2.7 Other remarks

2.7.1 Changing the editors' strategy

Perhaps because they are editors themselves, Hochberg et al. (2009) essentially criticize authors but pay little attention to the role played by editors; balance is needed here. Editors have enormous power: they are the gatekeepers[16] of their disciplinary field. They are the ones who decide who is worthy of walking in the bright light of day of published papers, and who must be cast out into the night of grey or unpublished papers. They are like Charon, the mythological boatman who ferried souls across the river Styx: those who could pay the ferryman could cross, but the others had to wander the Styx's shores for a hundred years. As a counterpart, editors have a huge responsibility: their ethics are essential. Editors are also agents of the publishers: they are appointed by them; they must guarantee both the quality of the journal and the quantity of its readership. Hence, even if publishers play no role in the peer review process in the leading academic countries, editors are under pressure from publishing houses. For example, if an editor selects poorly qualified reviewers (such as the

[16] The gatekeepers are those "who decide what appears in the journal" (Braun et al. 2007, p. 542).

young and inexperienced ones referred to above, or lax or incompetent reviewers), low quality manuscripts will be accepted for publication; the journal's readers will turn away and see "if the grass is greener" elsewhere. Editors are tasked with maintaining, or even improving, the quality of their journal and also of the discipline as a whole, including its ethics. The controls they have at their disposal are (i) the choice of reviewers for manuscripts submitted to the journal they are in charge of and (ii) the pressure they put on referees to obtain high quality reports within a reasonable time.

Schultz (2009) analyzes the impact of the number of reviewers on the acceptance rate, depending on the editor's strategy, that is, "Omnius" (the editor rejects when all reviewers recommend rejection), "Populus" (the editor rejects when the majority of reviewers recommends rejection) and "Quisius" (the editor rejects when one reviewer recommends rejection). In the last case, the editor need only wait for the first report that suggests rejection, which fatally speeds up the reviewing process. This suggests that one solution—which is not explored by Hochberg et al.—to alleviate and accelerate the reviewing process might be to make the editorial process more stringent. However, this recommendation will obviously increase the rejection rate, and hence reduce the number of pages or even issues published per year. While this might be a good thing for some journals such as *Physics in Medicine and Biology*,[17] I am not sure that the publishing houses would be so happy to see the number of pages published, and perhaps the number of issues, decline sharply.

Moreover, if all journals become more severe at the same time, this will initially increase the rejection rate but thereafter the number of resubmissions will soar: in the end, the workload of all reviewers will be increased! The cure is worse than the disease. Besides, if only one journal becomes more demanding, the other journals will recover the rejected papers for their own benefit.

2.7.2 On the criticisms of authors' behavior

It is true that scholars try to publish in the leading journals (see for example: Macdonald and Kam 2007) but it is hardly fair to blame them for doing so. Each author has his implicit economic reasoning. On the one hand, publishing in the leading journals is a positive gain to the author. However, as the probability of acceptance is low, and is lower still

[17] Patterson and Harris (2009) have recommended reducing the acceptance rate of the journal *Physics in Medicine and Biology* from fifty percent to ten per cent to increase the impact factor.

when the quality of the journal is high, the cost of submitting to the best journals is higher both in terms of time spent (a paper may be one or two months with a journal before being scrutinized by the editor) and in terms of shaken morale in the event of rejection. Each author compares the potential gain with the potential cost: even if it is not the aim of this comment to develop a model, there is clearly the makings of one there. Trying to make a hit in a top journal is risky but the potential gain for the author is very high, while it is costly to play repeatedly: the paper could remain unpublished some years down the line (which could leave it out-of-date). By contrast, submitting to a more modest journal increases the chances of being accepted: the cost is lower, but so is the gain. The choice of where to submit is clearly like the choice of a lottery. Either you choose the Lotto where the gain is huge, sometimes billions, but the probability of winning is low and playing many times until a winning ticket comes up is costly; or you choose a scratch card: the gain is small but the cost to become a winner is lower.

Hochberg et al. overlook the psychological side of the question: it is natural for a scholar to consider that his paper is very good. Hochberg et al. certainly suffer from a certain dose of elitism: elitism means that everything would be that much simpler if second rate authors would understand that they are second rate from the beginning of their careers and publish only in mediocre journals instead of cluttering up the review process for the better journals. How would young scientists know whether or not they were second rate before trying to publish in the leading journals? And why should senior scientists be resigned to being ranked as second rate all their lives: they might hit upon something interesting, a new theory, etc.; we have already cited Wegener and Coase, but there are probably many others. It is a good thing that each scientist should try to compete and to improve even if the price to pay is more submissions to the leading journals.

Moreover, despite the "publish or perish" rule and the many papers that are submitted each year, many scholars publish nothing: they think that their findings are uninteresting or have been discovered before, etc.

Chopping up a paper into small parts must obviously be criticized. However, the journals often refuse papers that exceed a relatively small (e.g. 25 double-spaced) number of pages. Even so, this is not the main question with regard to Hochberg et al.'s problem. What matters is whether it takes more time to process two small papers than one big one. The answer is no for the marginal cost of processing but yes for the fixed cost. That is, there is a fixed cost for each paper $C = FC + c\,q$, where FC is the fixed cost, c the constant marginal cost and q the number of pages. For two papers of length q, the total cost is $2C = 2[FC + c\,q]$ but for one double paper, it is $FC + c(2q) \leq 2C$: in other words, there are economies of scale and increasing returns. However, the phenomenon may be unimportant if FC is low relative to c: this is a matter for further study. On the other hand, readers might prefer short papers: again, this would need to be examined.

2.7.3 Paying reviewers

Hochberg et al. complain about reviewers' behavior, and they cite Hauser and Fehr's (2007) recommendation to solve the problem of lazy reviewers who send their reports in way behind time. Their idea is that when a reviewer posts his report on time he is rewarded, while he is punished for a late review. The punishment is that if the reviewer is *n* days late, it will be 2*n* days before his own next manuscript, submitted to the same journal, is sent to the reviewers. Hauser and Fehr examine two counter-arguments: i) a punished reviewer might refuse to review: they add an extra delay to punish such behavior; ii) the journals adopting the system might receive fewer manuscripts: they dismiss this argument by saying that good journals will always receive many manuscripts. However, Hauser and Fehr's system is quite unrealistic because they lose sight of the most important thing: the quality of the reports. If delay is punished, reviewers will send their reports in on time, but they will botch the job! This is why bureaucratic systems of the type found in the Soviet Union fail: agents adjust the quality of their production downwards in reaction to an unattainable quantitative objective. Ultimately, one may get a system where referees completely abandon their professional ethics so as to avoid punishment. The quality of the journal may suffer drastically. Moreover, the system turns editors into cops, which is bad for their image.

Hochberg et al. forget one thing. While reviewers work for nothing, the publishing houses must maximize their profits to stay in the market: a journal is owned by a publishing company that may be driven out of the market if it fails to make maximum profit,[18] following

---

[18] The following quotation speaks volumes (Braun and Dióspatonyi 2005, p. 113):
A journal is the product of a publishing house, a commercial enterprise dedicated to preparing and distributing the periodical, but interested in it largely from an economic point of view. Even for a cause as noble as the advancement of science it is improbable one could find a benefactor willing to underwrite and promote a science journal without serious attention of the laws of the marketplace. This is not to imply that all journals are the property of independent commercial publishers. Indeed, many belong to scientific societies or similar

the theory of natural selection applied to firms (Nelson and Winter 1982). The editors are generally remunerated for their work. None of this can be viewed as abnormal in a capitalistic world but it changes many things in terms of the solutions that can be recommended. It has been known since Coase (even though it is not Coase's recommendation) that one solution for the "commons" is to privatize them. A first solution is the following: the journals may pay referees depending on the difficulty of the review and its timeliness (that is, if reviewers meet deadlines, they are paid in full, but receive less if they take more time). This could be contractual work. Many of us work hard to produce properly argued reports on time, but we do this for nothing; it should be recognized that reviewing is a part-time job: we could be paid for it!

However, "privatizing" reviewers could introduce unbridled free market forces into science; payment in money smacks of *merchandization*, with probable undesirable effects. This is why the payment could be also made "in kind", that is, by providing free access to journals, complimentary books,[19] etc., as proposed by Bloom (1998): the marginal cost of this form of remuneration is very low for the publisher. Some publishing houses already practice it from time to time but not on a contractual basis: when reviewers are entered in the publishing house database, they sometimes receive an offer, but the connection between their work and the offer is not clearly stated. Lundstrom and Baker (2009) show that reviewers benefit from their reviewing job for their own writing. And they derive moral advantages from being reviewers: see for example the publication of the list of referees by most journals; the *European Journal of Mechanics B/Fluids* explicitly names this list "Rewards of referees". Roberts (2009, p. 892) has recently suggested a form a remuneration that costs nothing: to "thank reviewers for particularly outstanding reviews". The idea is fine but perhaps **all** reviewers could be thanked. Reviewers might also be kept informed of what happens to the manuscripts they review: often they do not know whether the papers have been accepted (until they read them in the journal months or years later…), rejected, or returned to the author for changes that take time, etc. This is particularly frustrating for reviewers.

---

organizations, but the printing and marketing activities are usually delegated to publishers working under contract.

[19] Such remuneration mechanically increases the readership of the books, even if only marginally so.

3 Conclusion

In this paper, we have discussed Hochberg et al.'s (2009) recommendations. We have shown that sharing the database of reviewers among journals might be like reviving the Soviet Academy of Sciences, where non-scientific considerations prevailed. Pre-reviewing papers by author's colleagues is already widespread but poses problems of coordination. Revising manuscripts following reviewers' recommendations presupposes that all reviewers' recommendations converge, which is an acrobatic assumption. Having authors sign a commitment to take account of all reviewers' comments is authoritarian and sterilizing. Sending previous reviewers' comments to the journal to which the paper is resubmitted amounts to creating a single journal or a cartel. Using young scientists as reviewers is highly risky: they might be over zealous; and if they are not yet published authors themselves, the recommendation violates the principle of peer review.

We have also made one recommendation and two comments. In order to solve the problem rightly evoked by Hochberg et al. (2009), we suggest, as Bloom (1998), paying reviewers "in kind": books, papers, etc. Asking the reviewers to be more severe would only create a crisis in the publishing houses and increase reviewers' workload, while punishing them could be harmful to standards. Hochberg et al.'s (2009) criticisms of authors for wanting to publish in the best journals—the type of criticism that motivated their paper—are unfair: it is natural for each author to try to publish in the best journals and not to settle for being second rate for life.[20]

Hochberg et al.'s recommendations are worse than the disease they purport to fight. Above all, they adopt a narrow technical focus: the system must be as efficient as possible for the journals, their editors and their publishing houses, forgetting that the publishing industry may be experiencing hard times today and subject to mergers, etc., but that it is still profitable. Hochberg et al.'s ideas are very dangerous for authors' freedom of publishing and lead to give the full power to editors, to the detriment of the authors. Even if Hochberg et al.'s concern is a legitimate one—to make the reviewing process more efficient—it is unrealistic in many aspects as it is based on implicit assumptions about the efficiency of the reviewing process, the monolithic character of scientific truth, etc.; it runs the risk of looking elitist; it is

[20] Many other recommendations could be proposed: the reader may refer to Roberts (2009) for a very large set (22!) of recommendations mainly for editors.

dangerous because it could lead to the uniformity of scientific thought, which would be catastrophic in the social sciences and sterilizing too in the “hard” sciences.

4 Bibliographical references

AEA. 2009. “Journal of Economic Literature (JEL) Classification System”, *American Economic association*: <http://www.aeaweb.org/journal/jel_class_system.php>.

Blank Rebecca M. 1991. “The effects of double-blind versus single-blind reviewing: experimental evidence from The American Economic Review”, *The American Economic Review*, 81, 5: 1041-1067.

Bloom, Floyd. 1998. “Human reviewers: the Achilles heel of scientific journals in a digital era”, Presented at *INABIS '98 - 5th Internet World Congress on Biomedical Sciences* at McMaster University, Canada, December, 7-16, keynote address. Available at: http://www.mcmaster.ca/inabis98/keynote/bloom/index.html

Bornmann Lutz and Hans-Dieter Daniel. 2008. “Selecting manuscripts for a high-impact journal through peer review: A citation analysis of communications that were accepted by *Angewandte Chemie International Edition*, or rejected but published elsewhere”, *Journal of the American Society for Information Science and Technology*, 59, 11: 1841-1852.

Bornmann Lutz, Irina Nast and Hans-Dieter Daniel. 2008. “Do editors and referees look for signs of scientific misconduct when reviewing manuscripts? A quantitative content analysis of studies that examined review criteria and reasons for accepting and rejecting manuscripts for publication”, *Scientometrics*, 77, 3: 415-432.

Bornmann Lutz, Christophe Weymuth and Hans-Dieter Daniel. 2009. “A content analysis of referees’ comments: how do comments on manuscripts rejected by a high-impact journal and later published in either a low- or high-impact journal differ?”, *Scientometrics*, DOI 10.1007/s11192-009-0011-4.

Braun Tibor and Ildikó Dióspatonyi. 2005. “The journal gatekeepers of major publishing houses of core science journals”, *Scientometrics*, 64, 2: 113–120.

Braun Tibor, Ildikó Dióspatonyi, Sándor Zsindely and Erika Zádora. 2007. “Gatekeeper index versus impact factor of science journals”, *Scientometrics*, 71, 3: 541–543.

Campanario Juan Miguel. 1996. “Have referees rejected some of the most-cited articles of all times?”, *Journal of the American Society for Information Science*, 47, 4: 302-310.

Campanario Juan Miguel. 2009. “Rejecting and resisting Nobel class discoveries: accounts by Nobel Laureates”, *Scientometrics*, DOI 10.1007/s11192-008-2141-5.

Cicchetti, D. V. 1991. “The reliability of peer review for manuscript and grant submissions: A cross-disciplinary investigation”, *Behavioral and Brain Sciences*, 14: 119–186.

Egghe Leo. 2008. “A model for the size-frequency function of coauthor pairs”, *Journal of the American Society for Information Science and Technology*, 59, 13: 2133-2137.

Finney David J. 1997. “The Responsible Referee”, *Biometrics*, 53, 2: 715-719.

Fiske Donald W. and Louis Fogg. 1990. "But the Reviewers Are Making Different Criticisms of My Paper!: *Diversity and Uniqueness in Reviewer Comments*", *American Psychologist*, 45, 5: 591-598.

Fortescue Stephen. 1992. "The Russian Academy of sciences and the Soviet Academy of sciences: Continuity or disjunction?", *Minerva*, 30, 4: 459-478.

Frandsen Tove Faber and Paul Wouters. 2009. "Turning working papers into journal articles: An exercise in microbibliometrics", *Journal of the American Society for Information Science and Technology*, 60, 4: 728-739.

Hall Richard H. 1968. "Professionalization and Bureaucratization", *American Sociological Review*, 33, 1: 92-104.

Hardin Garrett. 1968. "The Tragedy of the Commons", *Science*, 162: 1243-1248.

Hargens Lowell L. and Jerald R. Herting. 1990a. "Neglected considerations in the analysis of agreement among journal referees", *Scientometrics*, 19, 1-2: 91-106.

Hargens Lowell L. and Jerald R. Herting. 1990b. "A new approach to referee's assessments of manuscripts", *Social Science Research*, 19: 1-16.

Hargens Lowell L. and Jerald R. Herting. 2006. "Analyzing the association between referees' recommendations and editors' decisions", *Scientometrics*, 67, 1: 15-26.

Hartley James. 2005. "Refereeing and the single author", *Journal of Information Science*, 31, 3: 251-256.

Hauser Mark and Ernst Fehr. 2007. "An Incentive Solution to the Peer Review Problem", *PLoS Biology* 5, 4: e107. DOI 10.1371/journal.pbio.0050107.

Hochberg, Michael E., Jonathan M. Chase, Nicholas J. Gotelli, Alan Hastings and Shahid Naeem. 2009. "The tragedy of the reviewer commons", *Ecology Letters*, 12: 2–4.

Lee John D., Kim J. Vicente, Andrea Cassano, Anna Shearer. 2003. "Can scientific impact be judged prospectively? A bibliometric test of Simonton's model of creative productivity", *Scientometrics*, 56, 2: 223-233.

Lindsey D. 1988. "Assessing precision in the manuscript review process: a little better than a diceroll", *Scientometrics*,14, 1-2: 75-82.

Lundstrom Kristi, Wendy Baker. 2009. "To give is better than to receive: The benefits of peer review to the reviewer's own writing", *Journal of Second Language Writing*, 18: 30-43.

Mayo Nancy E. James Brophy, Mark S. Goldberg, Marina B. Klein, Sydney Miller, Robert W. Platt, Judith Ritchie. 2006. "Peering at peer review revealed high degree of chance associated with funding of grant applications", *Journal of Clinical Epidemiology*, 59: 842–848.

McDonald Stuart and Jacqueline Kam. 2007. "Aardvark et al.: quality journals and gamesmanship in management studies", *Journal of Information Science*, 33, 6: 702-717.

Min Hui-Tzu. 2005. "Training students to become successful peer reviewers", *System*, 33: 293-308.

Nelson Richard R. and Sydney G. Winter, 1982, *An evolutionary theory of economic change*, Cambridge Mass: Harvard University Press.

Nisonger Thomas E. 2002. “The relationship between international editorial board composition and citation measures in political science, business, and genetics journals”, *Scientometrics*, 54, 2: 257-268.

Patterson Michael S. and Simon Harris. 2009. “The relationship between reviewers’ quality-scores and number of citations for papers published in the journal Physics in Medicine and Biology from 2003–2005”, *Scientometrics*, 80, 2: 343-349.

Roberts William C. 2009. “Reducing Flaws in the Review Process of Manuscripts Submitted to Medical Journals for Publication”, *American Journal Cardiology*, 103:891–892.

Schultz David M. 2009. “Are three heads better than two? How the number of reviewers and editor behavior affect the rejection rate”, *Scientometrics*, DOI 10.1007/s11192-009-0084-0.

Seglen Per O. 1996. “Quantification of scientific article contents”, *Scientometrics*, 35, 3: 355-366.

Snizek William E. and Ellsworth R. Fuhrman. 1979a. “Some Factors Affecting the Evaluative Content of Book Reviews and Sociology”, *The American Sociologist*, 14: 108-114.

Snizek William E. and Ellsworth R. Fuhrman. 1979b. “The Evaluative Content of Book Reviews in the American Journal of Sociology, Contemporary Sociology, and Social Forces”, *Contemporary Sociology*, 8, 3: 339-340.

Snizek William E., Ellsworth R. Fuhrman, and Michael R. Wood. 1981. “The Effect of Theory Group Association on the Evaluative Content of Book Reviews in Sociology”, *The American Sociologist*, 16: 185-195.

Süssmuth Bernd, Martin Steininger and Stephane Ghio. 2006. “Towards a European economics of economics: Monitoring a decade of top research and providing some explanation. *Scientometrics*, 66, 3: 579-612.

Van Rees C. J. 1987. “How reviewers reach consensus on the value of literary works”, *Poetics*, 16: 275-294.

von Mises Ludwig. 1944. *Bureaucracy*, New Haven: Yale University Press (third reprint of 1946)

Weller A. C. 2001. *Editorial Peer Review: Its Strengths and Weaknesses*, ASIS&T Monograph Series, Medford, NJ: Information Today, Inc.

Zi-Lin He. 2009. “International collaboration does not have greater epistemic authority”, *Journal of the American Society for Information Science and Technology*, 60, 10: 2151-2164.