JCOM 2 (3), September 2003

Socio-cognitive perverse effects in peer review
Reflections and proposals

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Peer review is the evaluation method that has characterized the scientific growth of the last four centuries, the first four of what is called *modern science*, indeed. It is matter of scientific communication inside scientific community, a subject too poorly studied in comparison with its critical importance for a scientific study of science (*science of science*).

Peer review has been used for scientific paper evaluation before publication (*editorial peer review*) and for research proposal evaluation before financial support (*grants peer review*). Both cases present similar pros and cons, so I will treat them as a unique method for scientific evaluation.

While the method remained pretty unchanged all along the period, apart from communication technology with peers, science has tremendously changed its organization and its relevance to society. So, peer review is antique and well rooted in practise, but its historical aim should now to be contrasted with the present situation of actual research, practises and social involvement of science.

Unfortunately, in spite of its widespread use, it is *really surprising that so little is known of its aims or effects*, and disciplines as medical ones are by now well aware of it (Jefferson et al. 2002). The *Journal of the American Medical Association* sponsors a congress every four years since 1989 to study peer review and monitoring its reliability. The editorial of the last proceedings concludes as follows (Rennie 2002):

> if the entire peer-review system did not exist but were now to be proposed as a new invention, it would be hard to convince editors looking at the evidence to go through the trouble and expense.

Socio-cognitive effects come out to probably inhibit peer review, in its different variants, to fulfil the promises. But they are still badly understood.

In the present paper I will just sketch some ideas, and then I will try to advance some proposals, at least as a provocation. However, so huge are the stakes in *knowledge based society*, that it is useful to rethink its merit since its socio-cognitive basis once more. Also if we will find that no other available method is better. Research is widely open.

1. The historical aims of peer review

As soon as the Royal Society was founded (1662), the *Philosophical Transactions* started to be published (1665) as its official communication medium. Henry Oldenburg, the first editor of the *Transactions*, introduced a necessary *peer review* of each paper by qualified experts before publication inside the *Transactions*. So, it is really hard to distinguish between scientific communication under peer review and history of science itself.[1]

Such an evaluation had two declared aims. First of all, it was thought to supply a selection of the new claims of knowledge (mainly *discoveries*) based on the *scientific merit*, to community of readers advantage. Moreover, it was also supposed to supply a quick control by the best known scientists in the subject, and then to suggest improvements, to the submitter advantage.

The publication, of course, is subordinate to the result of peers' evaluation. Usually, the method is now set up as an e-mail consultation of a little group of peers, often *ad hoc* selected by the editor. The editor has then the charge to solve eventual conflicts among peers' judgements, following some procedures (e.g. to publish in case

[1] However, already Aristotle spoke about principles of *endoxa* (the qualified common opinion) as the *first statements* of scientific knowledge (*Topici I* 1 100b, *VI* 4 142a, *VIII* 5 159b). See (Cerroni 2002a) for a discussion.

of draw between favourable and unfavourable, either publish after improvements are received or re-submit to peers, etc.).

Looking in depth into the method, peers come out not only being the *gate-keepers of science (*Merton), but they are also the socio-cognitive instrument that transforms lay information and peace of knowledge into proper *scientific knowledge*, knowledge thus certified by scientific community.

Peer review has also been used for evaluating research proposal submitted for public financial support, at least since 1937, when National Advisory Cancer Council was founded in the United States, and the practice became usual for National Institutes of Health (Chubin, Hackett 1990). This method is usually set up as postal or e-mail consultation, or as a panel meeting of a group of peers, *ad hoc* selected by official decision-makers from a list of scientists to assist them in the evaluation of proposals received in response to calls made under specific research programmes. Usually there is some scale of scores for each areas the policy maker focuses on, and the best placed proposals receive grants.

As a result of this second kind of peer review, peers are not only evaluators, but also (indirectly) decision-makers transforming sketchy projects into *scientific researches*. And this is particularly true for *big science*, where high investments in laboratories and people are of primary importance.

What are the pre-condition for peer review doing a good job? We can try to make a list.

First of all, the experts have to be really the best experts inside the *scientific community* on the subject the paper talks. Moreover, the judgment has to be driven only by scientific merit as it is judged in the same and transparent manner as for any other paper/proposal. Then, it has to be excluded that a different expert of the same value (reputation), while evaluating with the same criteria the same paper/proposal, would conclude differently about its fitness for publication/grant.

So, the belief below peer review is, as Condorcet put it, that “the scientist who declares his opinion on a theory, on an invention judges less this theory, this invention than he submit himself to the judgment of his peers” (cit. in Bensaude-Vincent 2000, my translation).

It is also tacitly assumed that past performances in scientific production are the best credential to be a reliable evaluator, and that any deviation from an accepted way of evaluation are openly punished in terms of reputation (the “money” of the “Republic of Science”).

Lastly, peer review is thought of as an accepting-process, while it is and cannot be other that a *believing-process*, if not even interested. Accepting is matter of rational choice among different ideas upon some basis; while believing is matter of sharing *point of views* (Cerroni 2002a).[2] It is really idealistic (late positivistic, indeed) that a person could make rational choices neutral with respect to beliefs, and different beliefs cause irreparably different choice. So, peer review is at least partially belief-driven, and not a pure ideal-driven process.

2. The actual situation of scientific job

Present research situation is discouraging.

The excess of publications for human *bounded rationality* (Simon) was pointed out already by Already Barnaby Rich in 1613 (cited in: Price. de Solla 1963, p. 63):

> One of the diseases of this age is the multiplicity of books; they doth so overcharge the world that it is not able to digest the abundance of the idle matter that is every day hatched and brought forth into the world.

Derek J. de Solla Price, the founder of the so called scientometrics, the scientific study of scientific production, measured the number of living scientists during the Sixties of last century as big as the 80-90% of the scientists of the all history (Price de Solla 1962). Price and, more recently, Gascoigne (1992) found that scientists have been grown in number as an exponential-logistic function of the time (an exponential with a flex as a saturation effect).[3] Also the number of scientific papers follows an exponential-logistic, and the mean production per scientist is constantly about 3, while the maximum still remains, probably, Lord Kelvin who published 660 papers during his life (Price de Solla 1962).[4]

[2] *Ideas* are thoughts of our voluntary and tough reasoning, while *beliefs* are the *cognitive uses* or *habits of mind* on which we unreflectively base our behaviour, choice and reasoning. If a scientific theory is a good example of idea, *tacit knowledge* (Polanyi) is a good example of belief. While ideas are accepted or refuted on a rational basis, beliefs are believed by *act of faith* or just *taken-for-granted,* without manifest reasons (Cohen), and easily transmitted via *contagion* (Sperber).

[3] Price found that the growth for Europe has been constantly characterized by a 15 years doubling time, for United States 10 years and Russia after Revolution 7 years (Price de Solla 1962).

[4] The opposite extreme is represented by the mathematician Kurt Goedel and the economist Piero Sraffa. So, production quantity is not immediately a sign of scientific quality.

Then, it is understandable and probably shareable the appeal of Ortega y Gasset (1930):

> The charitable work more peculiar to our time: do not publish superfluous books.

Now, the situation is dramatic: *Science Citation Index*, *Social Science Citation Index* and *Arts and Humanities Citation Index* include about 8600 journals in all disciplines. Probably, the risk of loosing an essential contribution to make an improvement in own research is already high and quickly increasing. The time horizon of citations is very little. As an example, the obsolescence of citations is growing in Physics follows an exponential decrease with half time of 5 years (Gupta 1990). So, if an idea doesn't receive quickly the success it should merit, after 5 years the probability to be recovered falls by half and the pressure to be lost is irresistible. An innovative idea is swept away very easily. Is not desirable a sort of moratoria?

Moreover, since first half of Seventies state budget became less than science-system needed. As a consequence, the pressure to competition (and probably cronyism) exploded in many "advanced" countries, more tormented by budgetary constraints.

3. The socio-cognitive intrinsic limits of peer review

Peers do their review on authors' piece of work in the context of science outputs. So, we can distinguish three different critical points in peer review: the peer, the author and the outputs.

3.1 Peers

Peers have to be experts, and this is a self-evident truth. But there is a trade-off in the expertise of the expert that reviews. If he comes from a too different specialization, of course, he is not enough expert of the subject; but as he comes from a too close specialization, he risks to be too involved.

In effect, the peer may have another paper under evaluation in competition with the one he is called to review; or he can be cooperating with the author in some way; or he is too keen or too familiar with a different approach on the research frontier.

Rivalries and jalousies grow seriously as the subject is newer, as it is also narrower and promising. So, the most interesting area, the *science frontier*, is mostly "polluted" by such a bias.

Each scientist has also his own cognitive style, often charged of beliefs not widely shared among colleagues (for Einstein's beliefs see: Cerroni 2002b). If referee and author share different beliefs, a negative attitude rises easily against the second. And judgement is ever driven by attitudes.

All these limits are evident also in citation studies. A last notation: peers are scientists themselves, and peer review is a time-wasting activity for the peers and time-delaying for the scientific community. Too many reviews are incompatible with high quality review and timely publication. The charge is growing fast.

3.2 Authors

A well known effect in scientific reputation is the so called *Metthew effect* (Merton 1968 in: id. 1973), also named *halo effect* (Martin, Irvine 1983): it takes its name from a passage of Matthew's Gospel , in which it is (roughly) written that *to those who have, it will be given and they will be in plenty, but from those who don't have, it will be taken and they will be in shortage*.

Talking about scientific reputation, it means that it tends to accumulate on the same person, due to previous reputation. Honours but also acceptations of papers or proposals go to whom who already received honours and acceptations. In peer review, we can fix the psychological mechanism found by Merton in the following manner: if peers have to review the new paper of a scientist "x" who has already give an important contributions or who is just well known, then they will draw their attention to his paper, thinking it is "relevant". And probably they will find it useful, in some aspects.[5] An opposite effect is produced onto papers submitted by unknown researchers, often young researchers.

Three are the most frequent arrangements of peer review: single blind, double blind and, more rarely, open peer review. We will now consider their respective limits.

In the (single) *blind peer review* the author doesn't know the name of referees, while these ones know him/her. Peers are thought to be free in their task, also if a

[5] In the scientific practice, the bias produces a real effect: readers will try to apply ideas inside that paper, and it will come up as really important.

powerful author should be able to trace back them. But the main limit of such method is that referees can use the occasion to give “slabs into the back”, relying on impunity. However, it is not so rare the case to be able to discovery some referee of own papers, especially if, as usual, the report contains suggestion (each of us, probably, has personal cases).

The *double blind peer review* is set in such way that both author and referee don’t know each other. This method, of course, shares all the limits of the former, with the addition of a specific “false conscience”: blind author is a real idealisation. The author is often very easily identifiable due to the common practise of self-citation. However, as the community usually is little, the identification of its members (author and referee) really is easy.

Anyway, *open peer review*, too, has severe limits, as particularistic behaviour are ever possible, so violating the Merton’s ethos of universalism of knowledge sources. Peers may be not enough free to judge the work of a powerful author, or be tempted to take the opportunity to get into his good books.

3.3 Outputs

Science has been constantly under the *essential tension* between tradition and innovation (Kuhn 1977). The saturation of scientific exponential growth is mainly due to economical limits; so that the steady state in which we are running into is pressing more and more towards a selection, of both people and papers. The most probable product of the consolidated *mainstreams* in every discipline is not research streamlined, but traditional research, research fallen into line.

Specialization is a *conditio sine qua non* for any scientific research and no more is possible to look for an idealistic “unity of Spirit”. Anyway, it is indisputable that a wide part of the most innovative research springs out from the intersections among disciplines and that “disciplines” as promising as biotechnology and cognitive science are really interdisciplinary.

After all, nature peacefully ignores our disciplines. As science is a theoretical representation of reality, disciplines are but the social organization of experts’ ignorance, not a nature mirroring. So, the mainstream output is but the rash street of our temporary ignorance.

Moreover, publishing is done under the pressure of the axiom: *publish or perish*. While grants and career are more and more the aim of the author, the aim of reader still (should) remain knowledge sharing: publication is less and less useful to scientific progress. Then, so high a publication rate as we experiment today, is not the result of cognitive innovation, but just the symptom of *puzzle solving* (Kuhn).

The logic of peer review itself risks to restrain science, being too conservative in a situation that, right on the contrary, needs great, unprejudiced innovation to answer the growing social demand of knowledge.

All the limits here encountered are particularly valid for human sciences, because they are often closer to politics than to “hard facts”, closer to subject interests than to reader’s ones, closer to science production system than to society needs.

4. Why not…

Let me now make some suggestion, among the many others everybody could imagine.

- First of all, it is desirable to put away publishing activity from career, fixing a rigid limit in publication list in any competition, for both project financing and enrolment. Online curriculum with publications in synthetic and reasoned index should be requested.
- In front of huge publication rate, a moratoria can be really taken into consideration by international community; in front of peer review dangers, can be taken into consideration the publication free of any censorship, apart from easy check of formal requirements. The problem, of course, is the balance between the two opposite actions.
- Scientific journals of the same subject (or pretty close) should be joined just in order to avoid multiple publications by the same author on the same subject with (pretty) the same results.
- An international e-journal should be created in order to publish one short communication per year per scientist, without censorship, in which he/she could report his

activity. It should be a very useful database for every scientist.

- The shift from paper to book could slow down the pressure on the reader, if only suited electronic database were available and analytic index ever present. To sustain books production, public review could be promoted with online access to indexes, introductions and citations.
- It should also be taken in consideration to limit papers length, opening a phase of short standard communication with few references as hyper-link, without censorship.
- About peer review, it is probably the time of promoting open peer review. Peer should be took abroad as far as possible and their report and name written down at the beginning of each paper, together with author’s reply. The list of reviews done as peer reviewer should be tracked in a true curriculum.

Science centrality inside the *knowledge based society* of XXIst century probably now calls really a “New Deal”. Money, technology and public attention are needed to manage future scientific growth, and a new effort in *science of science* has become urgent.

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