Methodology and Science, 23-4, 1990, 171-180.

*PEER REVIEW: A PERSONAL REPORT**

By Joseph Agassi,
Tel-Aviv University and York university, Toronto

*Paper written while I was an Alexander von Humboldt-Stiftung senior fellow at the Economics Department of the J. W. Goethe Universität, Frankfurt.

§1. *Introduction*

Peer review is an old mediaeval institution, yet it plays on the contemporary scene the role of a new and very significant means for the maintenance and protection of the democracy and the independence of the modern academic and research system in that, in addition to its traditional functions, especially that of recruitment, it now is extensively used as means for determining such significant matters as the granting of research funds and the choice of material for publication in the learned press, in the so-called refereed literature. What the peer review system has lost it has lost to the administrations of the universities and of the research institutions since nowadays the university is much less self-governed than it ever was and the intellectuals are much more university based than they ever were.

In addition to the increased dependence of intellectuals on the acquisition of a university or research position, they now have to learn to behave in the style of big corporations and learn to relate properly to power brokers and often enough lose their self- respect. Universities always were power centers. But in the traditional European universities the power center was a chair, which entailed a limited power in the leading circles of the local environment, after Hiroshima and Nagasaki all this has radically changed. Today the Ivy League universities wield enormous powers, as do the editorial offices of the leading professional periodicals and the powerful committees on campus as well as in the grant giving conglomerates. It is well-known that a rave review of a new book in *Science Magazine* can catapult its author, even if the author is a newcomer, to the top of the academic totem-pole. And control over reviewing in that periodical is hardly a matter for peer control. I have myself experienced some very unpleasant interaction with that magazine, though I am a fellow of the American Association for the Advancement of Science which issues it, and even the editorial violations of etiquette I was unable to rectify and my fight simply removed me from their list of book review authors.

Though I have been interested in the sociology of the commonwealth of learning for the last thirty years or so, and particularly in the processes of appointments and tenure, as well as in that of grant allocation and publication. I had no intention to speak in public on my experiences, until I learned about really incredible facts. In the first international conference of scientific journal editors organized by Miriam Balaban in Jerusalem in 1978, the proceedings of which were published by Reidel in 1978, an editor of a mathematics journal reported about the repeated case of a referee who keeps in the drawer a paper submitted to some learned journal for publication, not because of any doubt as to its value but merely in order to gain time so as to enable a graduate student to finish and submit a dissertation on the same topic.

That this mode of conduct is a scandalous betrayal of trust is obvious. That this may cost someone the loss of a job is equally obvious to those who have even a nodding familiarity with appointment procedures. What is less obvious is the stupidity of this kind of conduct. For, the motive behind this is the prevention of the failing of a doctoral dissertation on the ground that the new theorem it purports to present has been already discovered by someone else. And this motive is just too flimsy as the doctorate can be passed anyway. I have participated in an oral exam for a doctorate once, in which I convinced the other examiners that the theorem allegedly proven by the candidate was but a poor variant of a theorem not mentioned there which could be easily found in a well-known mathematics text that was about half a decade old by then. I had little difficulty proving my case as one of the examiners happened to be the august author of that well-known text. Of course, the candidate was passed: for some reason it was a foregone conclusion. I do not think that any objection on my part would have made any difference beyond possibly delaying the granting of the degree to the candidate by a few weeks; at least so far in my career, no bigger difference was ever made by any objection — of mine or of someone else. In two cases in which I tried to raise a scandal, and these cases were in two different and well-respected universities, absolutely nothing happened, even though the dissertations — one in philosophy and the other in political science — were as phony as I ever saw. In any case, in the mathematics case mentioned I had no wish to object to the successful passing of the dissertation and the adviser demanded of the happy new PhD to run after me and to thank me in person. These cases may suffice to exemplify my first point.

§2. *The first law of peer review*

The first law of peer review. *It is always possible to overcome peer resistance if at all within reason, and for power brokers even quite against reason and also against etiquette. In other words, people in power positions can get away with almost anything.*

I should report at least one case of a challenge: when John Silber, the then new president of Boston University went too far in harassing his faculty, to the point that the faculty of the neighboring universities felt threatened, they put a stop to his threat, which he then withdrew. But let me return to the more normal.

The leading American *avant-garde* composer in the last generation was Roger Session of Princeton University, in Princeton, New Jersey. He once asked the senate of his university to allow his department to grant doctorates for creative work. The head of the department of mathematics responded by reporting that his department was just then beginning to grant doctorates for non-creative work. The senate, as Sessions told it, burst into laughter and his request was immediately granted. Now what was happening was a decision made on the strength of a pun: in mathematics creative work is the proof of a hitherto unproven theorem, and the uncreative work is usually a survey of some literature, whereas in music creative work is music composition. Moreover, most faculty of arts dissertations and most theoretical dissertations in the faculty of science are non-creative in the same sense. The members of the senate of that university were ready to decide on the strength of a pun because it pleased them to do so: they saw in the situation but the expression of the incomparability of standards in different departments, which incomparability strengthens the need for diversification and for peer reviews within departments only — perhaps across the nation, perhaps across the commonwealth of learning, but well within one discipline as recognized by it having a department all to itself.

This shows the ability of people in power to get away with almost anything. If the worst comes to the worst, one can always create a new discipline by creating a department with a newly fangled name, or at least an institute within a faculty, and then view oneself almost peerless and thus one's own judge and jury. And this is done, though not often, of course.

The constraints on the ability to get peer approval is the fear of public opinion and the fear that adverse public opinion will lead to the restriction of one's power. Thus, when I started a periodical and did it my own way, my departmental chairperson was both terrified of possibly adverse public opinion and desirous of the power base I had created. And so he usurped it, knowing full-well that it was beneath my dignity to fight him. He still edits it, and it is very humdrum as he can neither give it clout nor build up his strength by other means. My most dramatic report concerns a brief note I had published thirty years ago in a leading periodical, reviewing adversely some output of a very powerful philosophy research center in the United States which just then depended on the renewal of a huge grant. I became the target of a campaign that has prevented me for thirty years from receiving any American grant.

Let me elaborate a bit. Of course, possibly I do not deserve a grant. Yet I had been offered a Rockefeller grant before that dramatic change and I am a recipient of two years of fellowship from a leading German foundation — the *Alexander von Humboldt Stiftung*. Of course, possibly I do not know how to apply for a grant in the United States. But I was a member of Boston University when the notorious john Silber was appointed its president in the hope that he would bring lot of money with him. Instead, as he came he instituting an office for grant application and forcing faculty to apply for grants. I did. The professionals there did the technical job for me and considered my applications very hopeful. I knew better: before that I had been invited to apply for diverse grants, by NSF officials and by the president of the Guggenheim Foundation — twice — and by the director of the Institute for Advanced Study in Princeton, not to mention others, and systematically with no success. Now when one complains of ill-success one may very well be dismissed as having a grudge. So I suppose I am particularly qualified to complain, as I have enough success and as I am not considered a person with a chip on the shoulder. I put then my compliant as my second point.

§3. *The second law of peer review*

The second law of peer review: Peer review can degenerate into the gentle mob action conducted by the recognized leadership of the sub-profession or the specialty.

The adverse review that has caused me ostracism through peer review in the United States was published thirty years ago. It is reissued in my forthcoming book of book-reviews, *The Gentle Art of Philosophical Polemics*, published by Open Court, an academically reputed private press which has little use for peer review. The reviewers and readers of my forthcoming book will be able to judge whether time has proven the justice of my thirty-years-old dismissal of an establishment product, the *Minnesota Studies in the Philosophy of Science*, volume 2. I expect the reviewers of my forthcoming book to be fair as the interest served by defending that volume is already long dead. I should generalize this point thus.

§4, The third law of peer review

The third law of peer review: the injustice of peer review is short termed: anyone who can survive the onslaught may win, except the utter outsider who will keep losing out of the inability to prove the injustice of the ostracism of the past. Interestingly, injustices are at times rectified upon death: the review of Cornell psychologist J. J. Gibson's posthumous book in *Science* declared it possible to admit Gibson's importance now that he is dead. Better posthumously than never. But is it better to be revered only after death or to be revered only till retirement — as is the case with so many power brokers?

The dynamics of the situation is the interaction of the three laws of peer review: power brokers can do as they dare, but usually they dare not, and they even block the daring, but only for a generation at most. Usually one feels obliged to protect one's territory for just a while longer and proceed just a little longer more or less on the same tired lines as before. Timidity begets timidity and so the force of inertia increases. For those of us who think the academy matters and who care for intellectual progress, for the environment and for world peace, this situation is barely acceptable.

This is not to accuse the power brokers of disregarding the public interest while guarding their own: it is well known that people in the establishment almost regularly and unfailingly identify the public interest with their own. And ever so often they sabotage both interests out of some cowardice or some highly conventional mode of reasoning or both intertwined.

My examples concern the conventional attitude to criticism. I have given recently a golden opportunity to Adolf Grünbaum to engage in a public debate with me on his recent book on Popper better known as his book on Freud and on my review of it. He rejected it and, presumably out of timidity, no journal will accept my review of his book. Simon Blackburn, the editor of *Mind*, did not even submit my review of his book to peer review; though it was appreciative, it was critical enough for him to write to me an abusive letter about it. These two book reviews, then, will not reach the learned press governed by peer review: they will appear in my already mentioned *The Gentle Art of Philosophical Polemics*. Of course, peers will then be able to judge my book worthless. I am happy to report that systematically ever so many of my essays were rejected by peers before publication, often off hand, but agreeably they were praised after publication. Moreover, the essays in my collections of mostly previously published ones — my *Science in Flux* and my *Science and Society* — were more highly praise upon republication then before, even though peer reviews are particularly hostile to the publication of reprints. This invites some analysis, and the analysis is short and bitter: the publication of reprints is a sign of authority and thus also the creation of authority in case the author is not yet a recognized authority. Peers timidly refuse to recognize an authority unless it is semi-officially recognized by the establishment and the establishment will not endorse an outsider. So prior to my having published a collection I am not entitled to it but after having published it I have proven my having been entitled to it. Cheap.

§5. Attitudes to Criticism

But to analyze the situation more seriously one has to study attitudes to criticism. Of course, the first thing to notice is that barring a few outstanding exceptions — such as the Bohr-Einstein controversy — criticism is taken by the public, especially by the party under scrutiny, to be an unquestionably and unconditionally hostile act. This is so in all the sciences. I was once invited to a symposium in Rochester University to comment on a talk by a freshly Nobel-laurelled bio-medical scientist. He stopped my comments after a sentence or two. First he told me that the king of Sweden is a better judge of the matter than I. Then, when I shrugged off this foolish remark, he followed up with a very long shaggy-dog story with a moral insulting to me and with that he ended the session abruptly. The chairperson did not dare do his duty and call the fool to order.

Here the sociology of science can do some good by showing that criticism is, in matters scholarly, not hostile but benign — as pointe out by Plato, by Popper, and by many others — and in accord with the latest by our host Sal Restivo. And sociologists of science can suggest ways to institute this. For example, we may suggest that a learned honorary society elect members all individuals publicly criticized by its members. Or we may try to have the establishment institute the best critic of the year award. In the meanwhile, let me observe the outcome of the foolish view of criticism as hostile.

When an editor has a paper to send for a review, the decision the editor faces, namely who to choose as the referee for that paper, is crucial. If the paper is critical and the editor either fair-minded or a coward, then the paper will go to some representative of the party under fire. Now the fairness of this procedure depends on the condition that the editor read any hostile reviews in a critical manner. I have expanded on this in an essay on the standards of refereeing, published in my *Science and Society* which, I am happy to report, is one of my better-known and oftener applied pieces. It is an interesting fact to report that though Popper's philosophy which I am a sort of a follower of is still a minority view, I cannot publish my paper in which I have a refutation of his claim to have successfully solved the problems in the field of the philosophy of science. The reason is that the editors naturally give the paper to Popper fans who respond rather violently to my criticism (thereby showing themselves poor students of his philosophy) and then the editor is in the midst of a family quarrel possibly of no interest to outsiders.

Things are worse when the quarrel is not in a family. For a conspicuous example, when it concerns a project which involves many teams and a lot of public grant money. A colleague who is a competent and broadly educated biologist has studied a highly interdisciplinary field of biological research, micro-phylogenetics. He felt that he could not quite master its huge literature as it involved also some philosophy. He kindly invited me to join him. Since I use pencil and paper when I read, especially when I read a closely argued literature which is new to me, we had a sketch of a paper when the job was finished: we felt that we had some interesting criticism of the field. So we tried to get it published. The hostility of reviewers (including reviewers for *Science*) was quite unbelievable. Editors repeatedly rejected the paper on the strength of the hostility alone, certainly not on the strength of the comments of the reviews, as these were unbelievably poor. This paper was one of the very few causes for a quarrel I ever had with an editor, as I am usually very friendly to this sort of public servants, knowing what a terrible burden editing usually is. It was, incidentally, a person well-known for his learning and for his friendly personality, the late biologist James Danielli, editor of *The Journal for Social and Biological Structures*. He liked the paper but dared not publish it. He first suggested some softening, but then realized that the hostility could not be abated this way and gave an excuse for his cop out. The paper was finally published, after much delay, incidentally, in the very well received *Festschrift* I have edited with Robert S. Cohen for Mario Bunge in the Boston Studies series and I was delighted that Bunge thought well of that paper.

This is not to censure anyone for cowardice, least of all editors, but to recognize the hardship of the professional burden. The burden, incidentally, is somewhat interesting for the student of anthropology and for those who attempt to apply standard anthropological techniques to the sociology of science. For, like Malinowski's and Evans-Pritchard's tribesmen, editors have one standard which they articulate and quite another one which they follow. Yet, unlike the tribesmen, they profess to follow the standards they know they deviate from, and they resent being forced to admit this fact. Moreover, some of us force them to admit this, at least to themselves, whether we want it or not. This way, I know, I often annoy editors, and I regret this fact and I often write to editors conciliatory letters. Their conduct is only intolerable when, instead of requesting that I withdraw a paper, which request I always honor, they pretend that some paper was never accepted despite evidence in writing to the contrary. Once, incidentally, an editor of a top periodical withdrew a letter of acceptance apologetically pretending it was a secretarial error.

Conclusion

I have come to the end of my presentation. Let me finish observing that my special situation in the field stems from my having a very thick skin. Otherwise possibly I could not take so lightly the thousands of rejection letters that lie in my files and the three manuscript books and many manuscript papers that lie hopeless in my drawers. I am amazed at the fact that rejections are usually taken as severe blows by most of the colleagues I know, be they big fish or small. Being so sensitive, more and more colleagues withdraw from any activity which invites peer review as soon as they receive tenure. Some of the big fish write papers and receive grants by invitations only. This strengthens the establishment and makes entry into academic professions ever harder and it will become increasingly so with the welcome waiver of compulsory retirement. At times, the big fish merely append their names to papers written by their juniors — usually as senior authors. And some junior colleague accepts this kind of humiliation as a price for some favor, as Karin Knorr-Cetina has observed. At times the humiliation is the mere means of validation. An example has taught me this: a young economist who was a friend and a former student had written an excellent textbook and was subsequently invited by a publisher to seek a senior member of the profession to volunteer to act as a senior coauthor so as to have the textbook accepted for publication. The textbook is, incidentally, still unpublished. For my part, I have a strict rule on cooperation: the author of the first manuscript draft is the senior author and one who has not written a significant part of some draft should be thanked in a footnote, not be named coauthor; drafts written by the team together require a different sort of consideration. But I admit that this is not essential, and not applicable in cases of senior researchers who cannot write and who add scribes to their teams: being recognized as a junior author is then ample recognition for the scribe. What is essential then is not who officiates in what capacity. It is that academics not lose their intellectual aspirations and their self-respect trading them for professional favors and a rise on the professional totem-pole. If there is any truth in the cynical slogan that people get the leadership they deserve, then at the very least young peers have to learn to take rejections lightly and find better means of assessing the worth of their contributions than the anonymous peer-view system. Moreover, if they decide that their contributions are not as worthless as they are judged by their anonymous peers, then let them fight. Furthermore, let them also fight for a system which does not require so much fighting and aggravation. Life is hard enough and we need not make it harder by admitting too much power with too little control. As peer review is not control enough we need better and more open controls and a better and more appreciative and responsible leadership and a better system for the fostering of the critical attitude which is the heart and soul of the scientific tradition.