3.2 The Peer Review System

Like democracy, it is highly imperfect, but all other systems attempted so far are disastrously worse. Having outlined the ecological science system, it is time to discuss the key component -- the peer review system -- in more detail. Depending on your personal experience and views, the following statement may either sound obvious or shocking to you.

It is clear that there is an inherent conflict of interest in the peer review system. Look at it first from your point of view when you are presenting something (a submission for publication or even your whole career) to be judged. If your work (papers and grant proposals) is to be judged by peers in your same field of research, it is very likely that one or more of your reviewers is working on something very similar to what you are doing. Often as not they are working on essentially the same topic, trying to get their paper out first, and perhaps competing for the same pot of money. Being human, it is not easy for that person to give you a fair review, even though they feel that of course they can put their bias aside. (By the way, do not forget that you yourself will also be in that position, i.e., of judging a close competitor, at another time. Ask yourself these questions. How fair can you be? Should you recuse yourself (as judges are required to do) for explicit conflicts of interest? Be very scrupulous in such cases, if only to become known for being scrupulous.) Now look at it from the general science point of view of getting the best reviews of your work. If everybody who might be a competitor is excluded, how can an expert (as opposed to uninformed) opinion be obtained? The basic dilemma is where and how to draw the line between sufficient expertise and conflict of interest.

In this context, many journals and funding agencies will ask you to submit a list of possible reviewers for your manuscript or grant proposal. (The journal and funding agency will give you some general guidelines so as to avoid blatant conflicts of interest in preparing your list. For
example, your mother or brother should not be your reviewer. Previous supervisors should also be excluded, and so on.) This will still give you the possibility to come up with a list of scientists who are your friends, and who are therefore likely to give you a *fair* review. Most scientific fields are small, and therefore the immediate community you work in will be essentially divided between people who are your friends, those who are your enemies and those who are genuinely disinterested. As one senior colleague once said, your reviewers should be experts in the field, and they should be friends. If you have doubts whether someone is a friend or not, or you think they may be struggling to get to a certain result before you, you are better off NOT suggesting them as a referee. Note that while I specified a *fair* review, that does not necessarily mean a very good one. (You can expect that your friends will not try to shoot you down intentionally, and they may be a lot more lenient than others if you did not make serious mistakes.) This is all very well, but you should also realize is that these referees will be considered by the committee as “your” referees and thus anything negative from them can be very damaging. Be very sure you know who your supporters are. This is certainly a case of “Better safe than sorry.”

In this light, consider the strategy of D., a colleague from another university than ours. While D. was writing his grant proposal for one of the main funding agencies in his country, he once met a senior colleague to hear his opinion about the draft proposal. The senior colleague liked the draft very much, and proceeded to point out several minor inconsistencies that he thought should be corrected. In the section on “Collaborations” for example, he asked why D. had not written down the name of a certain professor, whom he knew to be a friend of D. and who was likely to be a potential collaborator. D. replied that, since this possible collaboration was still far off in the future, he would rather list him as a possible reviewer, rather than a collaborator. This sharp (and perhaps cynically tactical) reasoning greatly impressed the senior colleague as being unusually perceptive for one so young.

Most journals and funding agencies will allow you to ask that particular scientists (with whom you are clearly competing, and perhaps with whom you have had a recent row) be excluded from reviewing your contribution. (You may find that, although this policy exists, you may not be told that it is in place (and will have to ask), and will not be prompted for a list of reviewers to exclude. This information is usually in the fine print of the detailed rules.) If you are aware of such dangerous people, do not hesitate to provide such a list, but use this defensive tactic with discretion and do not make the list too long. (Overuse will tend to lose you credibility; the reputation of being a “crank” is hard to live down.)

It sometimes happens that, after being excluded from a committee or having an opinion overruled or discarded by a journal editor, a person may overstep the unwritten rules and write nasty comments to the journal editor or to the grant selection committee. Not being in the review process (and thus not covered by anonymity of the review process), you may get to hear of this. If you do (usually unofficially) together with the name of the person, be happy, because it identifies such a person so that you can take steps to exclude them explicitly in the future. (Do not complain to the person in question, since that is guaranteed to be counter-productive.) Since any excesses inside the process itself are covered by the anonymity of the details of the review
process, you will rarely get to hear of them. To learn more on the merits and pitfalls of peer review (and on many other interesting topics), journals like Science and Nature frequently discuss them in their News Features, Opinion Articles and Correspondence Letters.

Various forms of anonymous peer review apply for submissions in different contexts. In order of weight and consequences from the relatively frequent application to the peer review process for (A) submissions to refereed conferences, and (B) submissions for refereed journals, to the rarer events comprising (C) applications for research grants from various sources, and (D) applications for Scholarships, Fellowships Awards and the like. The processes for publishing submissions to journals and refereed conference proceedings (A and B) and for submissions to selection committees (C and D) are rather different and will be treated separately. The simplest will be discussed first and that is the jury-like system for C and D.

3.2.1 Peer review in open committees with unpublished proceedings

Some of the most important peer review is partly open. In those situations (C and D above), as is usual in other types of juries, the membership of selection committees for grants, fellowships, employment, prizes and the like are known in advance. However the details of deliberations are not to be divulged, and only the final decisions are made public. With known members in a committee setting with explicit exclusions for conflicts of interest, there is some safety in numbers since a single extremist will in effect be moderated by the consensus. Also in a committee an extremist in a particular case will usually (consciously or unconsciously) tone these opinions down as to maintain credibility with respect to the rest of the committee and for other candidates. (By the way, this is one reason why documents to be looked at by a committee should always be written both to convince or at least disarm the expert and to prove appealing to the well-informed person not close to the field.) In general there are quite strict deadlines and rules for submission and any further action if not explicitly forbidden is unwise at best. An exception is often made for upgrading information, such as changing “submitted” to “accepted” or “accepted” to “Vol. M, Number N, pp mm-nn”. In case of doubt verify beforehand whether this is permissible and whether such updates must be sent to the committee (as opposed to being sent to each member).

3.2.2 Closed peer review: Refereeing for journals and granting agencies

The most closed peer review takes place for publications in peer-reviewed journals and some refereed conference proceedings (A and B) and also as an external referee for granting agencies (C). For all of these (A,B and external referees for C) (i) the referees are chosen from some internal list and perhaps also from a list furnished with the submission, (ii) the identities of the actual referees are always kept from the authors, (iii) referees do not usually know the identity of other referees. (Many journals explicitly ask the referees not to reveal themselves to the authors.) However, as mentioned above, the author can usually request that specific people (such as direct competitors) be excluded from serving as referees, and such requests are almost
invariably honored. (The editors/agencies do not welcome possible scandal.) The only real control of the journal and proceedings referee opinions is the editor's (or editorial committee's) judgment. For the granting agencies the evaluation of the external referees' opinions is in the hands of the committee in question. For the journal there may also be a formal appeal process from this first or second round of peer review (involving more referees unknown to you and, perhaps, a known Associate Editor). How can such flawed, complicated and shadowy systems work in the real world? Actually, they work better than you might think.

3.2.3 Closed peer review abuses

Abuses can and do occur, however.

Probably the worst misconduct takes place when a referee abuses the implicit trust (and sometimes the explicit guidelines in a conflict of interest statement) and uses the information received in confidence to gain an unfair advantage. This can include starting or redirecting a competing research program or even holding up acceptance to allow time for the competing program to publish first. All in all, while there will always be more misconduct of this kind than one thinks, this kind of severe damage seems to be sporadic, episodic and fairly rare.

Much more common, but still relatively rare (most referees are relatively honest, and will declare conflicts of interest) is the referee who is familiar with your work or with you, already dislikes the work or you (perhaps because a feeling that you have slighted the referee's work in the past). An ethical but ill-disposed referee should declare this bias (along the lines of, “I am sorry, I really cannot render an impartial opinion here.”) and withdraw as a referee. Most referees who are already biased against you will, however, see themselves as noble and unbiased defenders of true science and of innocent journal editors, and thus see no conflict of interest. From this assumed high moral stance they can then proceed to slam you and your work. If you are lucky, this kind of referee might overdo it. This excess may arouse the editorial suspicions, whereupon the negative opinion will be devalued, and other opinions will be retained. More subtle and practiced ill-disposed referees will not overstep this line and will thus prove hard to rebut, especially if they avoid too much detail and rely more on adjectival innuendo (e.g., “superficial”, “seriously flawed”, “slight advance”, “there is not enough new physics / new chemistry to warrant publication in this journal” and the like).

As mentioned above, if you can become aware of people who are likely to behave in this way, you can ask the editor to exclude some people you name from acting as referees. Do not, however, make the list too long, since this will give rise to suspicions of incipient paranoia and perhaps lead to your wishes being ignored.

The opposite case to unfair rejection of papers by journals that should have accepted them is the uncritical acceptance of papers that contain serious flaws, and yet receive the implicit approval of this refereed journal. A common reason for this is that the reviewer(s) are friends of the authors or have such a high respect for their previous work. Hence they read the manuscripts
without bothering to provide the constructive criticism that is crucial for the peer review system to work effectively. While this reduces the quality of the publication, it is clearly less unjust than undue suppression of good work. Also, the faults can be addressed by the authors or by others, so generally there is little reaction beyond a shrug of the shoulders.

By the way, in case you are wondering why this potentially dangerous anonymity of journal referees is still accepted, the reason is pure pragmatism. Without this anonymity the system would grind to a halt. It has been found over the years that if referees who give negative verdicts become open to attack by aggrieved authors, they will then refuse to referee in the future. (Some journals ask the referees if they are willing to be thanked by the editor for their assistance in the event of publication. All this does in practice is to allow one to get a partial idea of what set of people are on the editor referee list, but little more. Clearly, for the rejected papers, referee anonymity still holds.) The system of anonymous refereeing for peer-review journals is thus unlikely to change in the foreseeable future. (One could call it a Nash equilibrium in the refereeing game -- a stable but not totally satisfactory state. To repeat what was said at the outset, “The peer review system is, like democracy, highly imperfect, but all other systems are disastrously worse.”

You may find a lot of this information to be discouraging, and perhaps even disheartening. Most of us go through this disillusion from time to time, tempered mainly by the difficulty of constructing a better system. Science is clearly not well served if you have to struggle to get your work done, because someone else (typically an envious competitor) is trying to trip you up. On the other hand, science is ill-served when poor and even erroneous science is published -- but this may well be the opinion of your detractors concerning your work. The system has to accommodate both points of view to some extent. It might seem that the best thing to do is to try to become friends with your competitors, join efforts and do the work together. Unfortunately, this is not always possible -- some scientists have ego problems and want all the credit for themselves. For obvious reasons, it is difficult and sometimes impossible to collaborate with them.

In essence these are the rules of the game, and if you want to participate, you should at least be aware of them, even if you do not take advantage of them.

In general if your work is sufficiently important, it will be published even if not in the journal of your first choice or of first rank. If published it will in due time be recognized for what it is, and be copiously cited, even if it was not published in one of the very best, high-profile journals. A good example of this is the work reported by K. Takayanagi and co-workers in Surface Science (K. Takayanagi et al., Surf. Sci. 164, 367 (1985)), in which they described for the first time a satisfactory model for the 7x7 reconstruction of the Si(111) surface. This problem had been particularly elusive, and had been at the top of everyone's mind in this field for many years. Although its solution was not published in one of the very top journals like Nature or Science, it has nonetheless been cited more than 600 times by the end of 2004.