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HOW TO 'SURVIVE' AFTER GRADUATING IN MATERIALS SCIENCE III: THE PEER REVIEW SYSTEM

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ABSTRACT

This article summarizes our description of the peer review system as it is presently implemented for the publication of scientific articles, for selection of grant applications, for hiring (mostly in academic institutions, though part of this may translate to other research jobs), for promotion and the like. It also indicates some examples of the third concept introduced in the previous article, namely: learn to play from the other side. Since anything that matters in the world of science is peer reviewed, we advise the reader to learn to place him/herself in the mindset of those who are going to evaluate his/her work so as to anticipate their reactions. This is the third article of a series, following the first (in which we described how the graduate course on 'Survival Skills for Scientists' was created at INRS) and the second article, in which we offered basic advice on how to apply the skills and knowledge acquired in graduate school to finding a job and developing a career in the 'real world' of science after graduating.

INTRODUCTION

After describing our graduate course on "Survival Skills for Scientists" in the inaugural aticle[1], in the second article of this series[2] we discussed basic advice for a professional career in materials science in the form of the so-called "laws of scientific survival"[2, 3], emphasizing for example the importance of thinking ahead[4]. It is now time to address specifically the key component that is widely used for quality control in modern science. This is what is commonly referred to as *the peer review system*. Since in practice almost anything that research scientists do that is relevant to their profession and/or to the advancement of their career is *peer reviewed* one way or another[5], it is essential to understand the peer review concept and how it works in the real world.

Depending on the reader's personal experience and views, the following description of peer review may seem to be almost anything, ranging from "banal" or "obvious" to "really?" or "I had no idea!". We believe our description of the workings of peer review to be realistic rather than cynical (as some might call it). We caution the reader that, since we will be

discussing the flaws at some length, the peer review system will inevitably sound much worse than it actually is. In fact, taken all in all, most of the time the peer review system works quite well. Our mission here is help the reader be prepared to cope with the workings of the system, particularly when it goes somewhat awry, and to avoid (as much as possible) problems before they occur. Other publications discuss various aspects of peer review[6, 7, 8] or provide specific advice on how to do scientific and technical writing itself[9, 10, 11], a topic which we will specifically address in the next article of this series.

THE PEER REVIEW SYSTEM

An important point to consider in discussing the peer review system is the same as that for the justice system, namely, 'Justice must not only be done, but should also be seen to be done.' This is the main reason usually advanced against accepting secret trials and closed-door justice in society in general. Note, however, that even in open trials, while the material before the court is indeed completely open, the deliberations of the jury and of the judge or judges are kept secret (usually unrecorded) and only the final result is presented. In the same way the essence of peer review is that the deliberations themselves are generally kept secret, with only the results being communicated to the applicant/candidate/submitter. However, unlike the identities of judges and jurors, in the justice system, in many cases of peer review (the most important of which is the reviewing process for acceptance of a manuscript for publication), the identities of the referees are also not disclosed. Under such circumstances, how can one then expect that a submission for publication or for funding will be judged fairly? That is the basic dilemma or dynamic tension of the peer review system as it is presently practiced, namely, arranging to balance high standards with fairness while using anonymous reviewers.

At this point you might well be wondering why this potentially dangerous anonymity of journal referees is the rule. Why do we not have open refereeing, with referees signing their opinions? (This is what is done for judges on appeal courts in law). The reason is pure pragmatism. While in the judicial system the judges are full-time salaried professionals, and while the jury members are compensated (if only at very modest rates) and serve perhaps once or twice in a lifetime, in the science peer review system referees usually serve several times a year and are usually unpaid and may often be judging their competitors. In such circumstances, if reviewers find refereeing leads to unpleasant consequences, they will simply decline to referee in the future. Over the years the compromise that was found to work best was to keep the identity of the referees confidential, i.e., hidden from the authors. Without this referee anonymity the system would grind to a halt for lack of referees willing to serve in difficult cases. (In a compromise of a kind some journals do ask the referees if they are willing to be thanked by the editor for their assistance in the event of publication. However all this does in practice is to allow one to get a partial idea of what set of people are on the editorial referee list, but little more. Clearly, for the rejected papers, the referee anonymity still holds since there is no public place where the referee is thanked for assistance in having a paper rejected.)

In any case the system of anonymous refereeing for peer-reviewed journals is 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. It seems likely that the overall verdict on the peer review system of today is that (like democracy) it is clearly not perfect, but (like democracy) all other systems attempted so far are disastrously worse.

Before discussing the peer review system as it works in more detail, it is useful to recall how it began. Peer review was introduced piecemeal when it became evident that too many of the publications of science were being dominated by small cliques which were often made up of cronies of the editor (often also the proprietor)

of the journal. The objective of obtaining fairer reviews of submitted manuscripts was attained more or less piecemeal as anonymous refereeing was introduced. The journals which did not comply first lost submissions by authors and then lost readership, so that in the end they had to arrange for acceptable peer review for survival or go out of business. This competitive success in the realm of scientific publication has led to the concept of some form of peer review (often anonymous) being adopted for other purposes as well. In the Maynard Smith terminology of evolution models, anonymous peer review proved to be an ESS or Evolutionarily Stable Strategy.

We will now discuss this aspect of peer review in some detail.

To begin with, let us look more closely at an inherent conflict of interest in the peer review system. Naturally, the editor or the grant committee is interested in getting expert and impartial opinions on the work you have submitted. Clearly those who are competent in the field and thus formally best able to judge your work are your peers in the field. However it is very likely that one or more of your reviewers is working on something very similar to what you are doing. They may even be working on essentially the same topic as you, trying to get their paper out first, and perhaps competing for the same pot of money. Being human, it is not easy for that competitor to give you a fair review. On that basis, it seems that in fairness these direct competitors ought therefore to be excluded from judging your work. (Of course, most of these competitors would themselves each probably feel that, while others might find it difficult, they themselves can put their possible biases aside). It would seem that what one needs are experts who are close enough to the domain of interest to be competent but not so close as to be in direct competition . The main challenge of peer review is where and how to draw the line between sufficient expertise and conflict of interest, and how to manage such a system in a balanced way.

Note that this problem of a conflict of interest as a referee will eventually almost certainly become an issue for you. Although this probably will not arise when you begin in a field and are not yet well known as an expert in the field, if you are successful you will eventually find yourself in just that position, i.e., of judging a close competitor. You must ask yourself the key question, "How fair can you be?" Perhaps a better question is, "If your identity were known to the author/applicant, would you be accepted as an impartial and competent referee?" If you think that you cannot really be fair you should *recuse* yourself (as judges do in some law cases, i.e., disqualify yourself to act as a judge or juror because of special interest or bias). Be very scrupulous in such cases, if only to become known for being scrupulous; being scrupulous certainly enhances your reputation rather than detracting from it. Conversely, if you do not declare a conflict of interest and some unpleasantness ensues, your reputation will likely suffer.

Of course editors and funding organizations are constantly looking for ways to maintain and improve their list of usable referees. One way for the editor or committee to obtain suggestions for experts to evaluate your work is to ask *you* for a list of names of reviewers, to be submitted with your manuscript/proposal[12]. (There usually are general guidelines to avoid blatant conflicts of interest in preparing your list. For example, a frequent collaborator, your previous thesis advisor, or supervisors, or people from the same institution etc. should be excluded, and so on.) This request for suitable referees will still give you the possibility to come up with a list of scientists who are your 'friends', i.e. people that you know fairly well and feel you can trust, who are therefore likely to give you a *fair* review.

By 'friend' here we do not mean someone who would rubber-stamp your submission and approve it without reading it attentively. If a 'friend' does detect a flaw in your work, you should hope that they point it out so it can be fixed before publication. (If it is a real flaw of course you would prefer that it had been

addressed before submission, but accidents can happen.) After all, in the long run, nobody wants to publish something which turns out to be wrong – the long-term effect in terms of damaging your reputation could be in fact quite severe.

You should exercise some care with your wish list of referees. Because of modern tendencies to specialize and even to overspecialize, most scientific fields are relatively 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 essentially neutral. As a senior colleague once said, the reviewers in your list should be experts in the field, and they should be friends, i.e. people that you trust implicitly. 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. This is all very well, but you should also realize is that the referees you have suggested will in any case be considered by the committee as "your" referees and thus anything positive from them is likely to be somewhat discounted, while anything negative from them can be very damaging, much more so than for a referee you did not propose. Therefore be very sure you know who your supporters are. This is certainly a case of "Better safe than sorry." More importantly, this is a case of "*Know thy neighbor*." Many editors will freely admit that they often choose referees from the list suggested by the author.

In this light, it may be useful to consider the strategy of G, a young colleague from a university other than ours. While G was writing his grant proposal for one of the main funding agencies in his country, he met a senior colleague (someone he had informally elected as his 'mentor') to hear his opinion about the draft proposal. The senior colleague liked the draft very much, and proceeded to point out several minor omissions that he thought should be corrected. In the section on "Collaborations", for example, he asked why G had not written down the name of a certain professor, whom he knew to be a friend of G and who was likely to be a potential collaborator. G replied that, since this possible collaboration was still far off in the future, he would rather list him as a possible reviewer now, rather than as a collaborator, since this might well bring him an immediate benefit. This reasoning (although perhaps cynically practical to some) greatly impressed the senior colleague as being unusually tactical for one so young.

Many journals and funding agencies also allow an *exclusion list* as well, in that authors/ applicants may ask that particular scientists (with whom you are clearly competing, or perhaps with whom you have had verbal disagreements) be *excluded* from reviewing your contribution. If you are aware of such potentially hostile people, do not hesitate to provide such a list. However you should use this defensive tactic with restraint and do not make the list too long (exaggerating will simply make you look paranoid[13].

If you are interested in learning more about the merits and pitfalls of peer review (and on many other interesting topics) beyond what is offered here, journals like Science and Nature frequently discuss them in their News Features, Opinion Articles and Correspondence Letters.

Speaking of fair reviews and what is meant by that term, the classic image for fair peer review is an amusing cartoon by Sidney Harris (which we used in our book), showing a scientist at a blackboard saying to his colleague with the chalk who is drawing a huge "X" of condemnation over his equations, "*That's it? That's peer review?*"

Peer review problems can happen to anyone. Even Albert Einstein at the height of his fame in 1936 had a disagreement with the editor of the Physical Review concerning gravitational radiation. The result was a testy withdrawal of the paper and the publication later of a completely changed version elsewhere[14]. In those days peer review as we know it today was still being refined, and in his reply to the Editor, Einstein said, "I did not give you permission to

show my work to a third party".

Different forms of anonymous peer review are used for submissions in different contexts and we will address this next. The contexts encountered the most often are applications of the peer review process for (A) submissions to refereed conference proceedings, and (B) submissions for refereed journals. Less frequent, but usually of greater importance in each particular case, are the somewhat rarer events comprising (C) applications for research grants from various research funding sources, (D) applications for academic scholarships, fellowships, awards and the like, and (E) job applications and applications for promotion. The processes for publishing submissions to journals (A) and refereed conference proceedings (B) and for submissions to selection committees for research grants (C) and for direct academic support (D) are rather different and will be treated separately, and these are all distinct from job applications and applications for promotion (E).

CLOSED PEER REVIEW:

Refereeing for journals (A), refereed conference proceedings (B) and granting agencies(C)

Next we summarize the closed peer review process applied to journals (A), refereed conferences (B), and granting agencies (C). The peer review process that you would encounter most frequently is also the most closed, and that is the one used for publications in "peer-reviewed" journals and for some refereed conference proceedings (A and B) and is also what applies for individual external referees for granting agencies (C). This is the process we have just discussed at some length in terms of your ability to affect the list of people who might be consulted.

For all of these (A, B and the external referees for C) there are three aspects:

(1) The referees are chosen from some internal list and perhaps also using a list of suggested and excluded referees furnished by the author(s)/applicant(s) with the submission.
(2) The identities of the actual referees used are always kept from the authors.
(3) Referees do not usually know the identity of other referees (many journals explicitly ask the referees not to reveal themselves to the authors at any time).

We recall that, as mentioned above for peer-reviewed journals (A), the author/submitter can usually (but not always) request that specific people (such as direct competitors) be excluded from serving as referees, and such requests are almost invariably honored. (After all, the editors/agencies do not welcome possible scandal related to allowing a conflict of interest).

In the end, however, the only real control by the journal and proceedings of the referee opinions is the editor's (or editorial committee's) judgment. For journals (A), where continuity and even-handedness of policy is the rule, referees who are seen to be too easy or too finicky over several cases are subsequently quietly dropped for future use by the editor or his representative. Unlike the case for journal submission (A), since committees may change a great deal from one conference/funding competition to another, for both refereed conference proceedings (B) and funding agencies (C), effective quality control from one conference or one year to another is very difficult to maintain.

Many journals (A) also have a formal appeal process from the first or second round of peer review (involving more referees unknown to you and, perhaps, a known Associate Editor). Since there is no fixed deadline for a journal (except for each issue), the injury from an unjust rejection, if corrected on appeal, becomes merely a finite delay[15].

Because of one-shot time constraints, appeal from a judgment of a conference committee (B) is in effect impossible. Because of time limitations of conferences it is usually difficult to recover from overly severe treatment, so one

learns to be philosophical and to move on to something else. For the granting agencies (C) the evaluation of the external referees' opinions is in the hands of the committee in question, and since committees rarely overrule themselves, successful appeals from decisions of granting agencies are rare except for the cases where there was an egregious error in the procedure itself.

With so many complications and exceptions, it may well seem that such flawed, complicated and shadowy systems might often fail in practice. Let us look next at what can and does go wrong on occasion.

CLOSED PEER REVIEW ABUSES

As indicated above, some real abuses of the peer review system can and do occur. This is of course to be expected, since all human systems are fallible. We do not consider here the honest errors, where some inferior work slips through and some good work is unfairly rejected. "Abuse" here means that there is some malign or dishonest intent which is not thwarted.

Probably one of the worst examples of misconduct in peer review takes place when a referee abuses the implicit trust (and typically also the explicit guidelines in a conflict of interest statement) and uses the information received in confidence to gain an unfair advantage of some sort. This usually implies something related to starting or redirecting a competing research program. For a journal (A) this can extend to the point of actually holding up acceptance to allow time for the competing program to publish first. For refereed conferences (B) and granting agencies (C) the deadlines are fixed, so it is difficult to have any long-drawn process of refereeing in these cases; one shot is what you have, and it can be fatally sabotaged for that deadline event. (For journals (A), however, a new factor has emerged. Since editorial processes have become much faster over the last decade or so, many significant journals now boast an average refereeing time of as little as 2-3 weeks. In such a case it becomes increasingly difficult for a hostile referee to stall competitors' work by simply letting it sit on the desk – the editor will just find someone else to review it!)

However, all in all, and in spite of the detailed discussion below, while there will always be more misconduct of this kind than one thinks, this kind of severe damage nonetheless seems to be sporadic, episodic and fairly rare. (While many people know of one or two cases from personal experience, nobody we know had direct acquaintance with several abuses.)

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) and who already dislikes the work or you (perhaps because of a feeling that you have slighted the referee's work in the past) and allows this preconception to color the judgment of the case at hand. As mentioned above, an ill-disposed but ethical 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. As also indicated above (we try not be over-cynical), most referees who might be 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 and no reason to recuse themselves. From this assumed high moral stance they can then proceed to slam you and your work.

If you are lucky, this kind of hostile referee might overdo it. This excess may in fact arouse 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. This is especially tricky for the editor if they avoid too much detail and rely more on adjectival innuendo (e.g., "superficial", "seriously flawed", "only a slight advance", "there is not enough new science to warrant publication in this journal", "not novel enough" and the like).

It sometimes happens that, after being excluded from a committee or having an opinion over-ruled or discarded by a journal editor, a person may nonetheless overstep the unwritten rules and write separate nasty comments to the journal editor or to the grant selection committee. Since something like this is not officially in the review process (and thus not covered by anonymity of the review process), you may well get to hear of this. If you do get to hear of this (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. (It is never advisable to complain to the person in question, since that is guaranteed to be counter-productive and because you will be violating the confidence of your informant. You should also not react to the review committee, since officially you are not supposed to know what happened during their proceedings.) Since any excesses inside the process itself are supposed to be covered by the anonymity of the details of the review process, you will rarely get to hear of them, and even when you do, only un-officially. In such a case, all you can do is to be aware of this problem for the future and act accordingly, such as excluding the malefactor.

The opposite case to unfair rejection of papers by journals (A) that should have accepted them is the uncritical acceptance of papers that contain serious flaws, and yet receive the implicit approval of a refereed journal. A common reason for this is that the reviewer(s) are friends of the authors or have high respect for their previous work. Hence they read the current manuscripts too quickly and do not bother to provide the constructive criticism that is crucial for the peer review system to work effectively. While this reduces the average of the journal's quality of the publication, it is clearly less unjust (i.e., some slight damage to the journal's reputation) than undue suppression of good work (a sometimes severe check to the author's career). Since the faults can be addressed later by the authors or by others, there is generally little reaction to an inferior publication beyond a shrug of the shoulders and perhaps an opinion that the standards of the journal are declining. In comparison the 'mortal sin' of being unfairly severe, this is a minor 'venial sin' of being too easy.

Another reason for erratic quality of publications in a journal (A) may be the use of too few referees per submission, often done to keep delays to a minimum. An ideal would be to have, say, three referee reports for each submission (which would likely require four or more requests to referees), from which a clear consensus would usually emerge. However this leads to inevitable delays. (Also, the more referees, the more the likelihood that at least one will be rather slow.) On the other hand, with only two referees the editor may too often have a "split decision", when one then usually goes to a third deciding opinion (but now arriving later than if all had been sent out at the same time), and thus further delay. In effect, as well as the balance between quality and fairness, there is also a balance to be maintained between fairness and delay. These are not your problems, it is true, but you should nonetheless be aware of them and not chide the journal editors unduly for things they often cannot control.

As in many aspects of life, the essence of the editorial choice is triage, the medical-military term for classifying field hospital patients into those who cannot be helped (and would be a diversion of scarce resources from those who can profit from intervention), those who do not need help now (and can thus be dealt with later) and those who both need help now and can profit from it. For the editor, clear rejections and clear acceptance are the easy extremes; the hard cases are the marginal ones, where there is doubt. Of course it is these last that provoke the need for the most refereeing effort and the most delay. Referees who are prepared to take the trouble to suggest concrete improvements for this middle range of submissions are very precious to editors and are thus often the most heeded.

Journals with high reputation (and high impact factor) naturally attract submissions by highly motivated authors seeking to publish in the

'best' journals. Hence the struggle not to be rejected will be most severe for such high-impact journals. In this light the penalty to you for rejection is both delay and the loss of prestige and of impact (from being forced to publish in a journal of lower impact), rather than outright suppression of the work. If the work is truly meritorious and important it will eventually be much cited whether it is published in the journal of first or second choice, although this might take longer if one begins with a lower-impact journal. The pressure to publish in top-rank journals inevitably leads to more severe criteria and hence to a longer, more arduous and contested refereeing process for them. There seems no obvious way to change this, so there is no point in becoming over-excited about it, but it should be expected. One solution for an author who disdains this kind of struggle and conflict is to publish in journals of the second rank, but it may well be that his/her future financial support or promotion will suffer as a result.

Someone just beginning to submit peer-reviewed papers might find a lot of the information presented here to be discouraging, and perhaps even disheartening. Most of us go through this disillusionment from time to time, usually after a particularly unpleasant experience, but this should be tempered by the practical difficulty of actually constructing a significantly better system. Clearly science is not being well served if you have to struggle to get your work done, particularly if it is because someone else (perhaps 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 same opinion as that of your detractors concerning your work. The ability of the peer-review system to accommodate both points of view in some way is its major achievement and its reason for existence.

With the advent of electronic journals, science is evolving to the production of some publications where everything is published electronically, but to which signed comments (and rebuttals) can be freely added and electronically linked to the original publication. However even then most experts might well decline to become embroiled with cranks publishing what is regarded as poor science. Hence one might well then have little filtering of the junk and useful comment only on the fairly good stuff which is already moderately well served by the present system of comments and rebuttals. The net result might well be that the good stuff will be well commented and refined by comments and rebuttals, while the inferior stuff will either be ignored or have its validity contested only by pseudo-scientists.

CLOSED PEER REVIEW: Dealing with Journal Referees and Editors (A)

Let us leave the general viewpoint and come down to actual practice of publishing in peer-reviewed journals. Having taken all the pains that you can in preparing your paper, having dealt with all possible problems in advance, your *magnum opus* goes off to the selected journal. After what seems like an inordinate delay it is eventually returned with comments from the anonymous referees to whom you must answer (through the editor), and this is the principal topic of the next few paragraphs.

Two other things may happen. Your work may be accepted exactly as it was sent (a rare occurrence), in which case there is no more to be said. The editor may, however, declare without referee assistance that your submission is not suitable for the journal. This is most likely because the field that is being addressed is too far from the central theme of the journal, or (more rarely) because it is judged by the editor or an editorial aide as not being up to the level that their referees need to be called to examine. In either case your dialogue is then directly with the editor whose name you know, rather than with anonymous referees, as transmitted via the editor. The dialogue is rather different and your part resembles that of an agent arguing for his client to get a publisher to look at a book or to obtain a part in a play or the like. You are in a difficult position with little

negotiating power. Diplomacy, intelligence and perhaps cunning are needed, but it is difficult to give general advice.

Of course you might run also afoul of journal style rules, which have nothing to do with content or the referee, and most of us cravenly obey when this happens. In connection with journal rules (admittedly some time ago), an author was told (by a colleague) that a manuscript which he was about to send to *Physical Review Letters* would have to be modified because he was the sole author and used "we" throughout. The switch to "I" was then not an option, while changing the voice the impersonal (e.g. from "we have made mean-field calculations" to "mean-field calculations were made" etc.) was judged too awkward before the use of typewriters rather than word processors, J.H. Hetherington chose to solve this one-body problem by adding his cat Willard as co-author F. D. (for Felix Domesticus) Willard[16].

In another instance, the well-known physicist David Mermin recounted at length[17]. his cunningly planned and successful campaign to get Physical Review Letters to accept "Boojum" from Lewis Carroll's *The Hunting of the Snark* as an internationally recognized term applied to a phenomenon in liquid helium-3 in phase A. (Amusing follow-ups of the kind frequently occurring in anything related to Lewis Carroll appeared in *Physics Today* September pp. 11-13 (1981), and March p. 96 (1982).)

Let us turn to the more usual case, which is the author-referee dialogue conducted through the editor. Clearly if only minor issues are involved the quickest way is to agree with the referee (thanking the referee graciously for the trouble taken), make the changes and get on with your life. The difficulty comes when the disagreements are more serious, perhaps even to the point of being required by the referee to make statements which you believe to be wrong or misleading.

Again the subject can be divided into two cases, corresponding in the first case to the referee who is in favor of publication, but requires specific changes with which you firmly disagree, and in the second case to the referee who thinks the work is so flawed as to be not worth publishing at all.

For both these cases, the first piece of advice is to *keep your temper*. Do not rant, either to the editor or to the referee; it makes about as much sense as shouting at Customs or Immigration officials, or to the policeman who gives you a speeding ticket. While fair words may not succeed, foul words will most certainly fail. The second piece of advice is to try to put yourself in the referee's position (*play from the other side*) and try to see through to the roots of the disagreement; this will be invaluable in putting your case in a conciliatory and civilized tone. The third piece of advice is to realize that the situation now resembles a jury trial, where the defendant is the manuscript, you are the lawyer for the defense, the hostile referee is the prosecution and the editor is a combination of judge and jury.

Even with an obstinate and unyielding referee the refereeing 'game' can perhaps be won, even if you cannot convince the referee to change the opinion you believe to be faulty. This can happen because while the referee may not change the opinion, the referee may lose credibility with the editor, gradually being seen as being unreasonably picky or shrill or even wrong. (This is of course more likely to be the case if there is more than one referee and the negative opinion is not in the majority.)

All this is much easier to see and to do if you have done your own share of refereeing and are thus used, so to speak, to *playing the game* from the other side[18].

This possibility of the loss of credibility of the referee during the dialogue is why it is very important for the author to appear to be patient, reasonable and, yes, even sympathetic, with a tone that reflects more sorrow at a misunderstanding by the uninformed than anger at the insolence of ignorance. (Remember that implying strongly that the referee is not

competent is also an implicit reproach of the editor for not knowing of the incompetence (or worse) of the referee and for choosing him/her in the first place. This is not an impression you want to assert openly, but also an opinion that you would not dispute if the editor reached it independently.)

The worst that you yourself should imply with respect to the referee is that the referee is perhaps a little out of his depth, or that the referee is a touch obsessed on this particular point. (Do not, for instance, wonder to the editor how this referee could ever have been picked to referee your work.) It also helps in this endeavor to take blame for not making the points sufficiently clear, even thanking the referee for bringing this defect of presentation to your attention, and so helping you to improve the paper.

In the case of disagreement on a point which is not a simple misunderstanding to be corrected, but strong disagreement of, say, interpretation (where honest difference is often possible), another effective tactic to consider is to offer to include the referee's comment, but also to maintain your point with your reasons for inclining to your view rather than that of the referee. In effect, you are saying to the editor, "There are two possibilities here and we are proposing to present both and leaving it up to the reader." If the referee still persists the editor may in the end decide for your ecumenism and against the narrow-mindedness of the referee.

If the referee is really negative, while you may try these milder tactics, there are other and sterner measures. If the referee's familiarity with the field seems shaky, you may undermine the credibility of the referee, perhaps by bringing other references and authorities that you had not included before, e.g. by phrases such as, "these objections have been dealt with elsewhere by etc." If the referee's opinion is too vague, and too sweeping ("lacking in originality" and the like) you can with justice complain of the difficulty of defending the work against such vague accusations without supporting detail.

If all these measures fail, remember that you can often demand the opinion of another referee. This should always be done in a tone that is slightly apologetic (for putting the editor to more trouble because of this stubborn referee) but firm.

All this is quite serious and stressful, so much so that a somewhat lighter look at the topic is worthwhile including for your amusement. The item is the well-known *A Note on the Game of Refereeing* by J.M. Chambers and A.M. Herzberg in Applied Statistics XVII n. 3 (1968), reprinted in *More Random Walks in Science* (Inst. of Physics (1982)) on pp. 8-13, and also available (2005) in downloadable form at on the Web *http://www.buzzle.com/chapters/science-and-technology_jokes-and-funnies.asp*. Unfortunately the full text would take nearly five pages here, so all we can give is a sample or two to whet your appetite for the full text.

DIVERSION (1)

Excerpts from *A Note on the Game of Refer*eeing: "… It is agreed that the author's objective is to have his paper published, and that extra points accrue for the publication of a particularly worthless submission. … Likewise the referee's minimal objective is to have the paper refused and extra credit is obtained if the paper was a major contribution to the field."

After this excellent opening, it is worth sampling more.

DIVERSION (2)

More excerpts from *A Note on the Game of Refereeing*

Author tactic A5: Flattery-may-get-you-somewhere tactic. In the revision of the paper the author thanks the referee for his 'helpful comments' etc. This is very often employed against tactic R5 (deliberate misunderstanding of something which is correct) by saying something to the effect that he (the author) 'agrees that he was not clear in the earlier version of the paper'.

A7. Precedent tactic. Reference is made to a paper which although of very low quality was recently published in the same journal. The author implies that his work cannot be of lower quality than the previous paper. The danger, however, is that the editor may be only too aware that he should have rejected that paper and will act accordingly.

Referee tactic R2. Wrong-level tactic. No matter what degree of rigour (sic) the author uses, the referee replies by saying that it is not the correct one. For example, 'The author has stressed rigour (sic) to the detriment of clarity', 'The author's colloquial style is insufficiently rigorous', 'The author unfortunately tries to combine rigour (sic) with a colloquial style to the detriment of both'.

CONCLUSION

It must be acknowledged that the entire practice of referee-man-ship has declined in recent years. With the publication of more and more journals, and the issuing of present journals more frequently, the pressure for papers to fill them restricts the referee from rejecting as many acceptable papers as hitherto. …

However, the most insidious cause of this decline is the loss of the true savage refereeing spirit among the modern generation of players. We fear that too many participants have taken to heart the old adage, *'Referee others as you would others to referee you when you are writing'*.

The perceptive reader will have noticed that we have been proposing here is another version of the golden rule, "Respond to the referee in the tone that you would wish an author to use if you were the editor."

As a final word with respect to journal publishing, generally speaking, if your work is scientifically sound and sufficiently important, it will be published, even if not in the journal of your first choice or one of first rank. Once published it will in due time be recognized for what it is, and if meritorious will be copiously cited, even if it was not published in one of the very best, high-profile journals. These "late bloomers" can be identified by citation indices if one is willing to take the trouble. The journal peer review system does work more or less and "Excellence will out" — eventually. This paragraph is meant to comfort you when you receive a rejection from one of the top journals — something that happens to all of us that try, sooner or later!

CLOSED PEER REVIEW: Refereed Conference Proceedings (B)

Refereed conferences always have extremely firm deadlines and so they are essentially one-shot pass-fail systems, with no appeal and no improvement through iteration with the referees. Because of the one-at-a-time nature of refereed conferences there is usually no guaranteed level of selectivity. (A few conferences have an ongoing system of selection of a high standard, but most do not.) In fact some conferences reject nothing that arrives before the time limit[19]. All this means is that one has to be intimately acquainted with each conference to know how seriously to take the label 'refereed conference' and that no useful generalizations can be made about them[20].

COMMITTEE PEER REVIEW: Granting Agencies (C), Scholarships, Fellowships, Awards (D)

Some of the most important peer review processes are partly open. In such situations (granting agencies (C), Scholarships, Fellowships, Awards (D)), as is usual in other types of juries, the membership of selection committees for grants, fellowships, employment, prizes and the like is usually known in advance. (Although for the evaluation of applicants or for promotion (E) (see below) the names of committee members are usually not widely published, they usually can be readily obtained. Also if you are asked to come for an interview there are opportunities for

direct contact, and this possibility means that they are best treated separately.)

As is usually the case, the details of deliberations are not divulged. Only the final decisions are made public (typically in the case of success) or communicated directly to the applicant (usually in the case of rejection). Although you may know the committee members by name, they will in general be enjoined not to discuss their deliberations outside the committee meeting(s). However it may be that some details become known. (Sometimes internal conflicts may be at issue, between factions.) However, because of the deadlines, little of this is of any use to you, except as a post-mortem discussion.

With known members in a committee setting (with explicit exclusions for well-defined conflicts of interest), there is some safety in numbers, since a single extremist will in effect be moderated by the consensus. Also, in committee an unfair extremist in a particular case will frequently (consciously or unconsciously) tone these opinions down to maintain credibility with respect to the rest of the committee and for other candidates. This consensus aspect is probably the most important reason why documents to be looked at by a committee should be written in a particular way. As is the case with many other such documents, the text should then always be written not only to convince (or at least disarm) the experts but also to prove appealing to the moderately well-informed person who is not close to the field.

In general the deadlines and rules for submission are quite strict[21.] Any further action after submission, if not explicitly forbidden, is unwise at best. An exception is sometimes made for simple upgrading of 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 chairman or secretary (rather than being sent to each member).

Sometimes you may be able to find out after the fact what went well or badly for you. If you are given such information, be sure you treat it as confidential and only as implicit advice to be kept in mind for a future application. Never use it in communicating with anyone else.

PEER REVIEW IN HIRING AND PROMOTION COMMITTEES (E)

When a University Department opens a faculty position, or considers a promotion or an application for tenure, it will also appoint an *ad hoc* selection committee of several professors from the Department and occasionally one from other Departments or even from a different (usually neighboring) institution. For a new position, this committee will prepare a recruitment advertisement if required, set the deadlines for receipt of applications, follow up with a shortlist and arrange the calendar of on-site interviews. Typically all faculty members of the Department are expected to participate in some form or other during the candidate's interview trip, however usually the selection committee's decision is effectively final (pending approval from higher bodies such as boards of directors and the like, of course).

For a promotion or tenure application, since the applicant is already on hand, there is usually no direct presentation and the committee will work from their direct knowledge of the applicant and from the documents prepared by the applicant and the recommendations sent in independently to the committee chairman or department head.

In applying from outside the department for a job, the first important issue to bear in mind is that you will usually be competing with several tens of other applicants (occasionally it is less than that, and sometimes actually many more!), for just one position. Your primary objective in this first instance is to 'make the first cut', i.e., to pass the pre-screening of the documents and make it to the interview, where you can try to impress the committee with your charm in person rather than relying just on the written

application and supporting material. This means that your application really needs to stand out in order to be in the top ten or better. You have to make sure that the committee does not overlook it when bringing the list down from, say, from one hundred applications to ten or fewer interviews.

If you have published important work in the top journals of your field, normally you need not worry; you would easily end up near the top of the pile. If this publication record is combined with a notable "pedigree", i.e. you have received your degrees in famous places like 'Oxbridge', it would be very surprising if you are not called for an interview.

This discussion is therefore addressed to those who have published some good papers, but not in the top journals, and who have received a solid education, but not in the top schools. Clearly you are at a disadvantage compared with the possible 'stars', and there is no simple way to make you stand out.

Our advice here is to take special care in preparing all the documents that are requested in the application. Place yourself in the shoes of those committee members (*play from the other side*), who have to sift through so many pages, and never exceed the recommended number of pages for any particular document (e.g. the statement of research interests, or the statement of teaching philosophy) so as to not overburden the reader. Write clearly, and concisely; do not use very small font or reduced margins to gain space – if you can't say it all in the required space and without using tricks, it will reflect poorly on you.

Try to make sure that your fit in the Department is made clear to the reader; this should emerge from your research statement as well as your cover letter. Of course that means you should do your homework, using any acquaintances you may have in the faculty, or among the junior staff. In addition, the research statement should sound exciting and should give a distinct impression that your work will be 'fundable'. (A good strategy is to have a suitable mix of material in your research plan, some material that is exciting (but necessarily perhaps a bit risky) and some that is safer but solid and thus almost certain to be funded. There is nothing wrong in saying so explicitly and identifying which is which, possibly also specifying potential funding sources). In that respect it would be useful to refer to potential funding sources, especially if such sources have recently issued calls for proposals in your area. Particularly in the U.S., even though a 'tenure-track' position is supposed to become a regular faculty position once the tenure hurdle is over, it is pretty much expected in most (though not all) institutions that the faculty member will be self-sustaining, i.e. the person will bring in enough external funds to cover his/her salary for the foreseeable future so that the Department itself is not in danger at times of financial crisis.

Excellent advice on how to prepare your application documents, as well as the dos and don'ts of the on site interview process may be found in the very useful essay by M. Anderson[22]. If you have been invited to an interview this is a very good sign, as it means you have a foot in the door. However, you still need to remember that this is a competition for one slot – only the top candidate will get it. Doing well is not enough – you have to do better than all your competitors. (Of course, if the top candidate takes a better offer, being second will bring success, but one should not count on that happening). Remind yourself of this if you are not chosen; do not take it as an absolute refusal, but rather as a sign that someone else was deemed more suitable than you for this particular position.

A job application is an interesting exercise in preparation because it takes you through all the aspects of peer review, namely having your CV and research statement evaluated for pre-screening in the first phase, all the way to a seminar job talk and closed doors interview with a committee in the second phase. Mock reviews can be helpful here if you can arrange them. Persuade some of your friends to review your CV and application material. If others are

also going out to the job market you could organize between you a set of fictitious selection committees to test each of you in turn by a mock interview process: this can be a very useful and insightful exercise. In taking this approach, try to get on board as well as prospective candidates other people who have been through interview processes themselves or even better, people who have had the opportunity to participate in selection committees (these would necessarily be more senior faculty members).

The tenure and promotion presentations are almost equally important, but there you do not have a direct competition with other applicants, only a comparison between you and other young faculty members. Since only the documents are looked at in the committee, you will not be able to charm the committee yourself (except insofar as you have charmed them individually already), so, just as for the initial job selection, you should make sure the documents are as perfect as you can make them. Do not get sloppy just because you are now in the same building. If appropriate, discuss the letters of recommendation with those who agree to provide them[23].

SOME CONCLUSIONS

In this article we have presented a rough summary of the world of peer review in its various forms and applications, together with useful tips for avoiding making serious errors. In practicing science it is very important to at least be aware of the possible pitfalls in peer review to maximize your chances of progressing smoothly in your profession as a peer-reviewed researcher and to protect yourself from harm through ignorance of the peer review process. The main precepts can be summarized as (1) make it your business to be aware of what is going on when involved with the peer review process and (2) do your best to see how the game looks by *playing it from the other side*.

FORTHCOMING ARTICLES IN THE SERIES.

The next two articles in this series will deal specifically with (i) how to write an article (including different types of articles such as Letters, Communications, Full Papers, Reviews, etc.) and (ii) how to give an oral presentation (including short conference talks, invited talks, plenary talks, as well as departmental seminars and colloquia and even public lectures).

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5. For those who are not yet familiar with these terms, this means literature for which publication is obtained only through the filter of judgement by their peers in research science (usually through an anonymous review process).
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12. D. Grimm, "Peer review: Suggesting or Excluding reviewers can help get your paper published", *Science* **309**, 1974 (2005).
13. By the way, although this policy often exists, you will usually not be prompted for a list of reviewers to exclude. In fact you may not even be told that the policy is in place (and will have to ask). This information is often found in the fine print of the detailed rules.
14. D. Kennefick, "Einstein versus the Physical Review", *Physics Today*, pp 43-48, September 2005.
15. For this reason one should obviously verify for a journal of interest the availability of this appeal process where it exists; it is often found in the fine print.
16. The full tale is told in R.L. Weber's *More Random Walks in Science* (Inst. of Physics (1982)) on pp.110-111.
17. D. Mermin, *Physics Today* pp. 46-53, April 1981.
18. As a rather neat corollary, if you succeed in *playing from the other side* as an author, it will help you as a referee to frame your comments so as to improve the chance of gaining the cooperation of the author.
19. In many countries travel is supported only to 'refereed conferences', so, in order to be encouraging to possible users from such countries, some conferences, while labeled as requiring submission to a refereeing process, are in fact quite unselective.
20. Invited talks at such conferences do indicate some selection has been made and also that the work is prominent or promising, but the selection process may be no more than the whim of the program committee rather than a serious competitive process.
21. The most that should be attempted might be some slight modifications designed to appeal to some particular inclination of one or more committee members.
22. M. Anderson, 'So You Want to Be a Professor!', *Physics Today*, April, 2001.
23. Recall that these letters are of course normally sent not to you but directly to the committee or designated person for the committee. To avoid problems with committee deadlines, as far as your supporters are concerned, so that they may write their letters with authority and ease, make absolutely sure you supply all the support documents that they will need and do it in plenty of time.

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