



Observations of an Owl (6)

The Ideal Referee

Did we ever talk about peer review and paper writing in this column? We didn't? Well then, it's high time we did!

Quite some time ago – I was still a young owl and had just finished my PhD – I packed my beakbrush to leave for a couple of months work in a Japanese lab. It was a terrible flight, I mean, have you ever imag-

ined what it's like as an owl hunting for food whilst crossing the Himalayas or even the Gobi Desert for that matter? Never mind the thermals! But I am digressing again, gabby old owl as I am.

Once in the lab, working in Japan proved to be a most interesting experience (must make a note to write about that some other day), the project developed well and, finally, the results afforded a suitably informative article for publishing. But first I had to make it back home (don't worry, I'll spare you *those* details)...

Back at my post-doc desk I immediately got on with writing the "Japanese" paper and sent it off to my journal of choice. I sat back and waited ... and waited (not idly, of course, I was busy doing further experiments in the meantime) ... and, indeed, very nearly forgot about it ... until, after months, the letter from the editorial office arrived. Included were the comments of two referees.

Referee No. 1 made it short, very short. I don't remember the exact words but it was along the lines of, "Good paper. Publish unchanged." Of course that was the result I had hoped for but, nevertheless, I wasn't exactly happy about that review. In fact, rather a feeling of disappointment slowly crept into my stomach. Only two sentences? No argument at all? Did the referee even read the paper? Not to mention *think* about the content?

Finally, a concrete suspicion materialised in my mind. The referee couldn't have been anyone other than the eminent Professor X, a real pioneer in the field thirty years ago but long since retired. Nasty rumours circulated that X had already said good-bye to science even before his retirement but still kept showing up at the meetings just to see his old cronies. Aghast, I realised that one of these cronies was my former PhD supervisor...

The second review was the blatant opposite. It was even longer than my article, full of criticism and elaborate suggestions and culminated in the final verdict, "Review Again After Resubmission. The paper is not acceptable in its current form but has merit. A major rewrite is required. The authors should be encouraged to resubmit a rewritten version after the suggested changes have been completed."

I think it doesn't come as a great surprise that the journal's editor went along with Referee No. 2. Not exactly what I had hoped for but – honestly? – it could have been worse. No irrevocable rejection and – what was just as much a blessing in disguise – nobody asked for the addition of more data. So what could I do? Right! I sat down and rewrote the paper.

It was an awful lot of work, I can tell you. However, the more I advanced the more fun the rewriting became. It didn't take long

until I noticed that Referee No. 2 had actually done an amazingly good job. He had really *read* the paper word by word, he had *understood* the paper, he had carefully *rethought* all the results and conclusions and every single one of his suggestions was constructive and sound. At the same time I realised that by critically incorporating the suggestions of Referee No. 2 the paper significantly improved from page to page.

By the time I had finished, I had learned a lot about paper writing. In the end the revised manuscript was one-third shorter but ten times better – as even I had to admit to myself. The introduction was clearly focussed on the specific question of the study, the presentation of the data was structured much more logically and the conclusions were much more consistent and convincing.

The paper was accepted without any changes. There was, however, one particular thing that I was most happy about: Referee No. 2 had proposed to also review the rewritten version of the paper. Now he wrote, "I am most surprised by the extent of how much the paper has improved upon rewriting." "What? *You* are surprised, No. 2?" I thought to myself. "But it was especially down to *your* merit."

The paper was published and – young post-doc owl as I was – I quickly forgot that story. In fact, it took the next couple of papers to bring "No. 2" back to mind. The simple reason being that I never again experienced a referee who was even a quarter as good and beneficial as "No. 2". On the contrary, one after the other, I had to endure all the well-known clichés – you know, like referees that obviously hadn't understood the paper; referees requesting that an amount of data be added, sufficient enough to produce two additional articles; killer arguments like "the theory is good, but the evidence doesn't support it" or vice versa "the evidence is interesting but the theory is obviously wrong"; even pure moronism like, for instance, the incredibly scientific argument "I don't believe this...", etc., etc., etc.,...

Due to these subsequent experiences, I realised in retrospect that "No. 2" had been the ideal referee. He epitomised exactly the figure one should bear in mind when thinking about how the peer review system should be perfectly run. Despite all legitimate criticism and all the nonsense one is likely to encounter with peer reviews, "No. 2" taught me that the system really can work. It critically depends, however, on the quality and integrity of the people involved.

Or, as a good friend of mine recently wrote, "Reviewing is a hard job, one that takes many hours away from the things we need or want to do but one that we do in the interest of making science better. It has to be done with the knowledge that it *helps* and it should be done with rigour and care. It's vital."

There's no alternative to peer review. Imagine it was left to others less expert than ourselves. We'd certainly complain louder and longer than ever!

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