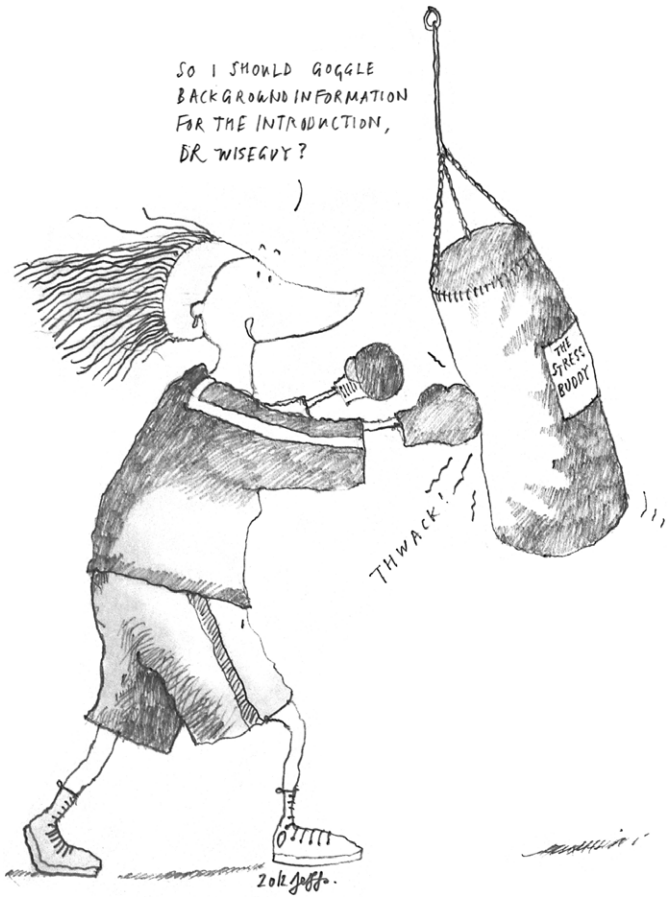
Journal of Cell Science

An occasional column, in which Mole and other characters share their views on various aspects of life-science research. Correspondence for Mole and his friends can be sent to mole@biologists.com, and may be published in forthcoming issues.




## The punch bag

Dear Uncle Mole,

I hope this finds you well and enjoying the summer sunshine out of the mole-hole or mouse-house or wherever your research is taking you these days! I myself have been virtually buried with *in silico* work recently, but that is the exciting part about having manuscripts out and about and under review: they're supposed to come back for revision. Maybe my enthusiasm for the minutiae is because I'm still rather new at this, but isn't it quite exciting when you get that automated email after an electronic submission? I know that the editors aren't *really* emailing me personally, but I can't help a small shiver of satisfaction when I see the message pop up in my inbox, especially if it's from a fancypants journal like *Molecule* or *The Original Colonies Journal of Medicine*. In fact (I probably shouldn't be admitting this), I have been known to log on to the 'For Authors' webpage numerous times in one day, simply to smile with glee at the site of a manuscript with my name on it labeled 'under review'. The moment just never grows old.

But then comes the update all potential authors await with baited breath: the release of the reviews. As a naïve little mole-let at the beginning of my research career, I assumed that of course my first manuscript would be accepted on first submission. Surely the editors would see how nicely we'd constructed our figures and how articulately we'd phrased the discussion. It was a good paper, ergo it would get in. This was only logical, after all, and wasn't science supposed to be a world governed by quantitative empiricism and the triumph of rational thought? (I know, I know, but remember, this was during my naïve stage.) I couldn't understand why my PI was so ecstatic when the reviews came back with a request for minor revisions. Didn't he think we'd done enough the first time? He tried to explain the intricacies of the review process – and how big of a deal it was to receive such a positive review on the first go-round – but much of what he said didn't register until I'd grown up a bit more and experienced first-hand not only rejections, but also the politics of publishing.

And indeed it is political, complete with all of the intrigue, soaring rhetoric, and even backstabbing that characterizes the

modern election. No matter how much we may wish to believe in the objectivity of our field, the fact of the matter is that our work must be interpreted and analyzed by others in the context of publication decisions, and this process is subject to the same biases that govern the rest of life. It might not be right and it might not be fair, but the age-old maxim of 'who knows who' can be very much in play when it comes down to what reviewers say and how editors use those comments to make their decisions. Fortunately, my evolution from wide-eyed little mole-let into slightly more seasoned junior scientist has taught me a few things. As my good friend Vole likes to say, "play the game, play the game".

I think one of the most important things I've learned – although it takes continual reinforcement – is not to take reviews personally. Just the other day I found myself storming through the lab nearly apoplectic after reading reviews (accompanied by a rejection) from a manuscript submitted to one of the most high profile journals out there. I'll be thrilled if it gets in – trust me, you'll hear the shrieks of delight even across an ocean – but oooo, it would be hard to think of any ways to make the reviews more insulting. One of the reviewers suggested I Goggle – Goggle! – background information for the introduction. The same lovely individual also had the temerity to suggest alternate coauthors, a not-so-subtle dig at one particular collaborator and his contribution to the project. Earlier this year I had a reviewer on a different manuscript call my work "childish". When I relayed this story to Squirrel, a friend, bench-mate, and general partner-in-crime in my new lab, she told me that one of her last manuscripts was reviewed by a scientist whose only comment was, "I don't believe in the model". Seriously? It's infuriating to have someone use the anonymity of the review process to insult your work and professionalism; belittle your ideas; or maybe even take advantage of the opportunity to stall the work of a competitor. However, stewing about it doesn't get your manuscript accepted. Limited therapeutic venting with friends is helpful – especially when accompanied by a martini, always shaken not stirred – but so too is the ability to remain undistracted by those aggravating tidbits and instead focus on what you can do to improve your work. The job of the reviewers is not to support our egos (although it feels good when they do). Their job is to critically evaluate.

This brings me to my second epiphany: sometimes even insulting reviewers have helpful suggestions. By the time you complete enough work to have a legitimate manuscript, it's almost impossible to remain perfectly objective. Blood, sweat, and tears generally obscure one's perspective. In my case, once I cooled off and ignored the barb about Goggle, I realized that Hated Reviewer #1 actually had some very prescient insights and suggestions – albeit offered in a disdainful tone – for improving my current manuscript. In fact, several weeks later all coauthors agreed that the revised version was substantially better work. Sure, it might be the professional highroad to provide feedback in a more constructive way, but sometimes the true value of something cannot be judged by its packaging. The key is to pay attention to the right things at the right time and just let the rest of it go.

However, one key detail that should never be ignored is the all-important role of the editor. I'm still learning how to play this card, but it seems that in the world of scientific publishing, "No" is not always the end of the road. With the guidance of a more senior co-author, last week we re-submitted our revised manuscript to the same high-profile journal that originally returned a rejection. And the editor sent it back out for review and sent us a carefully worded but optimistic note back! Now I should add that in addition to the revised manuscript we also included a 'response to reviewers' cover letter that contained semantic gymnastics worthy of an Olympic all-around gold. There was a great deal of deference and flowery language to acknowledge our gratitude for the many ways the reviewers helped us improve our work (blah, blah, blah), but on a few of the unreasonable points we put our collective feet down in what can only be described as a stuck landing. It's one thing to gracefully accept snarky commentary with a point, but that doesn't mean we have to roll over and play dead. In this situation we had a suspicion the editor still liked the manuscript despite the initial reviews, so we played to that hunch…and I hope we may yet turn out to be right.

Stay tuned for updates, as I'll be sure to let you know how it goes. And cross your fingers for Squirrel too, as she is re-submitting tomorrow. We're not out of the woods yet, but come what may we both have something to celebrate. Maybe our work isn't accepted just yet – and that is, after all, the currency that allows us junior scientists the leverage to become independent PI's – but we're getting there. Learning the ropes of the revise, resubmit, and rewrite game counts for something too. We'll hold on the champagne for now, but as for tomorrow night? It's time to bring out the dancing shoes!

Until next time,
Molette

*Dear Molette,*

*First things first – my most sincere congratulations on bringing your work to the point of submitting a manuscript for consideration! I agree with you, this is the true "angel period", the glowing time during which the work just shines, as does our enthusiasm for it. Once it is kicked around, and kicked around, our lovely, beautiful angel will look a bit tattered and torn, and we often go from talking about our hopes for our wonderful paper to referring to "that frickin' manuscript". Yes, from one point of view, it seems to be the job of reviewers and editors to make us come to hate our own work.*

*But I'm so proud of you for staying in there. You've pointed out some important things in your letter, not the least of which is getting over the negativity and finding the comments that actually improve the story. Good for you, and I'm pulling for you on this one. All fingers and toes crossed.*

*That said, it might be useful to consider why things have gotten so negative in recent years. I see a few reasons for this, and not all of them are good. I'll list them, with apologies in advance, since making lists is something I seem to do a lot. I even make lists of my lists. Someday, I'll make a list of all of my list lists, but that's for another time.*

*So, here are some of the reasons why reviewers pick on us so hard. And why it's so important to see when there is some hope, and when we just have to move on.*

1. People are just awfully grumpy these days. *Really, we can only take so many rejections of our papers and grants without letting us get nasty in return. We try to stay positive, but we get so knocked around these days that when we have a chance to have some pay-back, some of us just can't resist. We get a paper and say, hmmm, why should I let these folks have it easy, after what just happened to me? And of course, this just perpetuates the nastiness. Oh, and*

*another thing – if you think you know who reviewed your paper, you are probably wrong. But again, it is human nature to make such assumptions, and unfair as this can be, your reviewer may think that you (or one of your co-authors) dissed their last manuscript. Even if it isn't true. While there is nothing we can do about others, we should remember this ourselves – we have no idea who reviews us, really, none, nada.*

2. People are lazy. *We don't like to admit this, but we generally hate doing reviews, and while we tend to agree to them in all good faith, we get annoyed when we're badgered to get it done. It is an anonymous and generally thankless task. (You are right that an effective response must include lots of thanks to even the most negative reviewers, because they probably don't realize how awful their review sounded, and somebody has to say "thank you" in a manner that doesn't involve the cursory form letter from the journal.) And nobody re-reads their review before submitting, because it was already too much work, so they don't get to the point of saying, hmmm, maybe that was a little snarky, I'll change it. This results in the "laundry list" we tend to get – reviewers look through a paper, list anything they can think of, and post it to the journal without a second thought. Often the experiments they may propose are unjustified, or poorly thought out, or were even done (but missed), but to correct this would take more effort. So that's what we get.*

3. Did I mention people are lazy? *One thing that I am very unhappy about is the tendency of reviewers to hand off the job (which they don't want to do anyway) to trainees under the guise of "training".* This is completely unacceptable! *Yes, learning to review papers is important, so if one wants to teach a trainee to do it, have them review papers that have already been reviewed, and use that as an exercise. But to hand it off (even with the assurance that "I look at the review and approve it", which I bet they don't, really) is the worst sort of betrayal. Here's why: No matter how smart an inexperienced reviewer may be, and yes, many are very smart, they simply do not have the experience to look at a body of work and say, "wow, this is pretty much it – I don't have any criticisms". Instead, they say, "hey, I can think of lots more things they can do", and they hand us a laundry list.*

4. There is a flaw in the system. *Sorry, but this is something that many of us agree on: Any negative comments tend to eliminate the positive comments. Maybe it is just human nature. There is an old maxim that says, "anything said before the word 'but' can be ignored". And it is. When we do feel that a paper is pretty much great, editors tend to forget our review and focus on the negative ones. To counter this, we often make our reviews seem negative but leave the "door open", so that our review won't just disappear. Personally, if I review a paper that I really like, I also tend to write to the editor about the other reviews (assuming I can see them). It doesn't hurt, but may sometimes help (I hope so). Unfortunately, this problem sometimes extends to editors, who are also very busy, and often delegate all decision making to the reviewers, such that they need to have everyone on board before they can do something positive. That said, those editors who are not lazy are golden, and we should tell them often how much we love their efforts.*

5. Then there is the paradigm factor. *As fields mature, even a bit, some ideas become ingrained, and often with good reason (but often not). We believe we "know" how something works, and it takes a lot of evidence to change our minds. In extreme cases, we are asked to review work that we honestly feel will set back a field should it appear in a prominent journal (and yes, despite our ability to surf the literature, we still give more credence to those papers in journals with nice soft pages, or very shiny ones). I admit that I do this, and I defend that I do – if you want to convince me that the way I see my little piece of the universe is wrong, you need to be very convincing. I insist that I am always open to change, but sometimes work is so far off-base, flying in the face of a great deal of accumulated evidence from others, that I deal with such studies with a bit more negativity than I would otherwise. And so I should. (Yes, paradigms change, and this can happen when one wildly different observation, one "black swan" emerges, and we have to be open to this, but we can and do inspect such a swan to make sure that it isn't a white one that happened to get dirty.)*

6. The interest factor. *Ah, here is another rub. What is vitally interesting to me may be a huge yawn to you. And it is not enough for me to tell you how hard fought the findings were, I'm just not interested. So you kill my work with faint praise, or worse, suggest lots of ways I can "make it more interesting". Often this involves months or even years of additional work. Reviewers should ask themselves if this would really make their mouth's water, or if it would be best if they told the editors, straight out, that this work just doesn't "float our boat".*

*So what to do? Most of all, we have to set the example, let the angels of our better nature take hold and do our very best to reverse the trend. Pontificating on this helps (I hope) but it is not enough – I am frequently wildly amused when I overhear someone who I know is a consistently tardy and superficial reviewer (not because I think they are, but because, when I act as an editor, I know they are) holding forth over after-session drinks at a meeting about how terrible reviewers are. (On this point, I once had great fun assigning a paper from Professor Fruitbat to be reviewed by Professor Nuthatch, and a very different paper from the latter, which I assigned to the former. I sat back and waited for each to complain, loudly, about how long it was taking for their own papers to be reviewed. I apologized, and then mentioned that in the meantime, could they provide the requested review? When they did not, and complained some more about their own manuscripts, I pointed out how it might really help if they got their own done. This went on for some time. No, it wasn't a good thing, but it helps to prove my point.)*

*So here's what to do the next time you review a paper. Ask first if the conclusions are potentially interesting (if not, let the editor know why). If yes, then ask if the data support those conclusions. If they do, then you're finished. You can make suggestions for ways to improve the paper, but point out that any additional experiments are optional. And if the data do not support the conclusions, be very specific about why not, and justify any requested experiments. Lets end the "laundry list" syndrome now. Oh there is so much more to say on this subject, but really, I have some papers that I have to review! I promise I'll be kind.*

*Always such a pleasure to hear from you. I can't wait to read your paper once it finally makes it through the gauntlet!*

*Hugs,*

Uncle Mole

**Molette**

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