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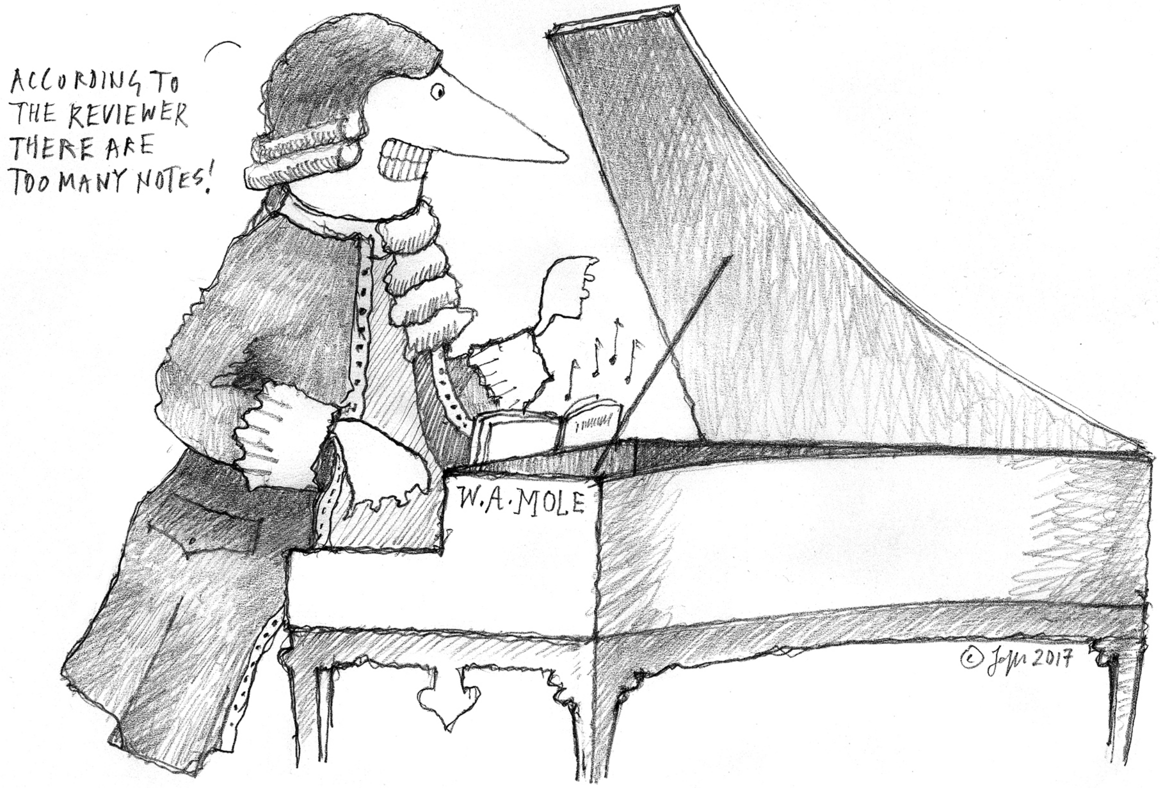
STICKY WICKET

Peerless II

Mole



Feeling a bit better, thanks. For those of you who are just joining us, I've just finished a two day marathon yack-fest, where we reviewed a huge pile of grants, scoring them so that our funding agency could decide which ones would actually receive money. The way it works in our system in my country (the one that starts with a U and ends with an S, or an A, depending) is that by about 10 am on the first morning we have finished all of the ones that might possibly be supported (maybe one or two more after lunch), and then spend the rest of our time discussing the fine points of many others that will be recycled into packing envelopes (if we printed them) or their electrons recycled as spam (both kinds).

Along the way, we discussed how the panel (or 'study section,' which invokes images of wizened scholars in dark suits pouring over palimpsests of ancient text, but really is just a bunch of folks who would rather be home) signals the problems with an application that just isn't exciting enough to make the cut. We don't say 'this just wasn't exciting enough, to us, to make the cut.' Instead, we have to come up with *reasons* that it didn't excite us. (Warning, Mole digression ahead.) Once, many years ago, I reviewed an application that had failed in a previous round, and one reviewer pointed out that because the grant was based on the analysis of the promoter of a gene that did not appear to be regulated in any sort of interesting way, and indeed, was expressed in an undefined (at the time) cell type, the application itself just wasn't interesting. In the revised version that I received, the applicant calmly and carefully pointed out that the previous reviewer was stupid, insipid, and most likely ugly, and had no right whatsoever to call his project uninteresting. I didn't agree, and I thought the revised application was boring, but noted that I did not know whether or not the previous reviewer was ugly. The project remained unfunded. But I digress.

In other words (actually, *fewer* words), when our lovely grants are rejected, it is worth considering that we had not written them in such a way to underscore why our application might be considered among those deemed most interesting, important, and exciting. This is regardless of what the reviewers actually wrote in their reviews.

But there is another issue, getting back to the sewage thing we talked about last time. [That is, if you put a spoonful of wine into a barrel of sewage, you have a barrel of sewage; if you put a spoonful of sewage into a barrel of wine, you have a barrel of sewage. Apparently this is 'Schopenhauer's Law of Entropy;' I looked it up. But then, you can't trust what you read on the internet. Including this (if you are indeed reading this on the internet. This is the sort of thing that Captain Kirk used to destroy otherwise all-powerful computers. Personally, I find that my own all-powerful computer is quite capable of destroying itself, albeit without smoke and flashing lights. I seem to have digressed again.)] Sometimes, even when a grant is simply incredible, all it takes is one individual to argue that it is 'too ambitious,' and it tanks.

The problem is a fundamental one: peer review depends on the assumption that we are reviewed by *peers*. Often this is true, and we

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should consider the opinions we receive as valid. But just as often, it is not true, and brilliant efforts are annihilated by folks who just don't have the insight, experience, or band-width to ever find an application to their liking. Worse, they do not have the courage to defend a positive position, depending instead on the universality of negative agreement. This is akin to the wonderful scene in Amadeus where Emperor Joseph II says to Mozart 'There are simply too many notes, that's all. Just cut a few and it will be perfect.' There are *always* negative statements we can make that sound smart, and when we are sitting with people we know are smart, we want to sound smart too. It is much easier to criticize than to defend praise.

One way to consider this problem is posited by Julian Jaynes, in his wonderful and odd treatise, 'The Origins of Consciousness in the Breakdown of the Bicameral Mind.' I read this many years ago, because the title made me feel smart. In his book, Jaynes suggests that consciousness, as we experience it today, actually arose recently, sometime between the time of the stories collected by Homer in the Iliad, and his own time (Homer's that is, as evidenced in the Odyssey). That is, the characters described in the Iliad were not actually conscious, and the voices of the gods were, Jaynes suggests, actually the musings of the right side of the brain (it might have been the left), which did not have access to the left side (it might have been the right). Thus, we essentially spoke to ourselves, and obeyed what we 'heard.' At some point this was internalized, and we became conscious. Yes, it's a completely crazy idea, which is why I loved it. (Borges parodied this in a short story describing the wonder invoked by a 12th century monk who reportedly read without moving his lips, which was impossible to conceive by the scholars of the time.)

Following this bizarre line of reasoning, we can consider the possibility that there are individuals reviewing our grants who are not conscious, functioning essentially as automatons unable to fuse the halves of their bicameral minds. Rather than understanding that the work you propose in your grant is likely to result in findings (and publications) that far exceed their own meager efforts, the otherwise inaccessible parts of their brains whisper to them that your proposed research is too ambitious. They cannot help themselves, but obey these jealous demands and reject your application. Other similarly unconscious individuals hear this critique, and follow in suit. Your grant tanks. You have no choice but to resubmit the essentially unchanged application, hoping for a better outcome, either because a more highly evolved reviewer has indeed attained consciousness, or because their bicameral mind orders them to support it (because this is essentially a 50/50 proposition).

Solipsism aside, I think this is a much better idea than what I suggested last time: that you consider the possibility that your original application was simply not as interesting, exciting, and compelling as other applications being reviewed.

Okay, okay, I'm *kidding*. While I do think that there are individuals on study sections who torpedo applications for selfish reasons (usually because they are tired of sitting there saying nothing, and when given a chance to speak, have nothing good to say), I think that the major reason a grant fails is that it simply wasn't one of the best ones in the current stack. In the U.S., this was determined well before the study section even met, at the point that the preliminary scores were determined by my peers, sitting wherever they were unhappily dealing with my grant among their stack.

So what, really, can you *do* about this? Hey, I'm the Mole, I have some ideas. You see, the problem is not peer review, really. The problem is that only those grants that elicit genuine excitement will make the cut. All the others, no matter how terrific, will not. To paraphrase Willy Wonka (or really, Gene Wilder, as W.W.), 'There's so much money, and so few applications. Stop. Reverse that.'

1. All grants have goals, or *aims*. How you frame these is probably the most important thing you can do. When someone reads these, will they say, "wow, this is really incredible"? Or will they say, "okay, I can see what you want to do, but I can't get very excited about that"? Here are some pointers. Think about *why* this is an aim of your project; why is this one of the *most important* things you can do with your time (and money)? It helps to frame the goal/aim as a question, and one that is as general as possible. Then under the question, describe the context and approach you will use to answer it. That is, don't say, "What is the role of phosphorylation of serine 27 in the protein Mxyzptlk." If Mxyzptlk is critical for DNA repair, ask 'How is DNA repair regulated?' and then state that you will explore this question in the context of the phosphorylation of Mxyzptlk on serine 27, which you have found to be critically important in this biological process. See?

2. Set yourself a deadline of four weeks (or so) from the *actual* deadline. Solicit help from your colleagues, your real peers, and show them your aims as soon as you develop them (keeping #1 in mind). See if they find them as compelling as you do. Don't argue with them! Keep working on them until they say, "Well, yeah, that's super interesting!" Then, when you have written your grant, ask them to *review it*. Ask them if it is one of the most interesting grants they can think of. If not, work on it some more!

3. Wherever possible, show that whatever you want to do, you *can* do. Show an experiment that demonstrates that the approach is feasible. And if possible, show that it may even actually work. This doesn't mean it is *answered*, it just means that the approach is valid and is heading in the right direction. You will repeat and extend the result. If it isn't published, it is *preliminary*. And if it was already published (which is okay to show, too), show this as a demonstration that you know how to do this. Remember, it is *not* true that you can only propose what you have already done, but those grants that demonstrate feasibility will score better than those that do not. Which one do you want to be yours?

4. As scientists, we explore, investigate, test. We do not demonstrate, prove, or show something that has not yet been examined. Rather than state that you will confirm something, say that you will ask whether it is the case.

There's more, of course, but this is a start. Without these things, it is unlikely that your application will be among the few that are supported. Convince your peer reviewers that you have a project that *needs* to be done. If you can do that, if you can move your grant to the top of the stack, you may get some good news.

I know, you have a million reasons why this isn't reasonable. You can point out to me that you *do* all these things and then your grant still gets picked apart. The system is rigged. The system doesn't work. Everyone else is wrong. Okay, how's that going for you? Hey, 'Rigged games are the easiest ones to beat.' I think Neil Gaiman's Mr. Wednesday said that. (If you don't know who Neil Gaiman is, find out. He's a much better writer than I am. Then again, he's not an insectivore.)

In the words of a wise, small, green philosopher, "Do. Or do not. There is no try." So do it.