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.

A little idea

Rain. Rain rain rain. Maybe we need it, but *I* don't need it. Not when I'm crawling out of my mole hole looking for a bright sunny day. And I have been *so* in my hole, doing the thing that I hate more than beets. (Don't tell me you love beets, I know you do – you can have mine.) I've been writing a grant, which, you know, I call 'bleeding on paper'. Because that's what it feels like. Oh, and because I also got a paper cut.

When I write a grant, it takes me forever. I ruminate, outline, sketch, talk to people in the lab, draft, talk some more, ruminate some more. And then, when I can't put it off any longer, I do my laundry. I check my email. I look around for what *else* I can do. Then, in something bordering on despair, I pick up a pad of paper (yes, I always write my first draft out long hand, which not only forces me to do at least one re-write while typing it later – okay, I guess, *keyboarding* it later, but it also makes me think some more about what I'm writing). Then I take a nap. But when I wake up, I actually write the frickin' thing. A bit. It's a long process.

And when it's done, and all polished and submitted, I forget about it, because most likely I'll never see anything good come of it.

Okay, that isn't entirely true. First, I admit that I get my fair share of the limited funds that are out there, so I can't complain as much as I might. And second, even if it crashes and burns, which happens a fair bit as well, we usually go ahead and actually *do* the studies that I've spent weeks thinking about. Yes, there is an upside to the process, albeit not as 'up' as 'side'.

I have a lot to say about writing grants, as faithful readers may remember. And some time we can talk about some of my ideas about tearing money from the hands of governments and foundations that hate to give it up. But right now, watching the rain fall down, I wanted to talk about something else I've been thinking about.

There is a thing about the way we do science that is very, very slow. We define a problem, often a terribly urgent problem, and we decide, as a group, to go after it. We make discoveries and publish 'breakthroughs', and applaud ourselves when we make progress. And meanwhile we work and work and work on these problems, pushing forward with small advances, and hope for the next big thing. During this time, children are born, go to school, graduate, enter the workplace, have families of their own, and *their* kids grow up, and the terribly urgent problem is still there.

I've said it before, and I'll say it again: "Science is hard." There's just such a long road between defining a big problem and actually doing something about it. And it isn't for lack of trying. Or *caring*. And if you happen to be one of those people who think that we suppress *real* progress so that someone can make money from the persistence of misery, come here and let me smack you (or if you know someone who believes this nonsense, introduce me, so I can smack *them*).

So while I recognize how hard the problems are and why their solutions are necessarily *slow* in coming, I've been thinking of one way we might consider speeding things up a bit. It's radical, and not for everyone, but maybe worth *thinking* about.

Science isn't just slow. It's really expensive. And biomedical research is really, *really* expensive. If we run a basic research lab, we have to come up with money for salaries, supplies, reagents (see, *expensive*), sequencing, arrays, screening libraries, and lots and lots of other stuff (like washing up, keeping the lights on, etc., etc.). And if we're doing translational work, the prices keep going up. We need money to make progress, and the peer review system (and grants, *ick*) is about as good a system as we can create. I know that some of you reading this obtain the necessary funding by other approaches, such as the system in which whole *barrels* of money are deposited at the feet of leaders who parcel it out according to those they consider are doing the best work. But that system is prone to an insidious corruption that I probably do not have to tell you about. (Yes, some others reading this will argue that it is no more corrupt than methods of peer review, but the latter does not necessarily lead to this state – the 'benign dictator' model almost always does.) All of which is beside the point.

I don't want to propose an alternative to peer review, in which carefully framed proposals are evaluated by a number of criteria, resulting in a distribution of funds on the basis of merit (in the best of all worlds). But I'd like us to try adding something different to this process.

My proposal is based on the idea of post-hoc 'micro-grants.' Here's how my fantasy works. A problem is defined, perhaps by a foundation that exists specifically to address that problem. Say, a disease for which we do not have a cure. We assemble a group of experts who are passionate and knowledgeable about the subject, and are sufficiently dedicated to its solution that they agree to exclude themselves from the micro-grants to come.

Then we set up a fully open-access system, curated by our experts, inviting experimental results that advance our progress towards addressing the problem. These results are not papers, with the requirements for fully articulated 'mechanisms' or additional supplemental materials. Just observations that build on what else is there. The submitters provide experimental detail and methods, and the results are vetted for proper controls, obvious artifacts, etc., but are, therefore, put 'out there' very quickly. And, of course, they are credited with the contribution. If, in time, other researchers provide counter evidence (or supporting evidence) then this is provided as well. Meanwhile, our group of expert curators/reviewers/editors provide connections, drawing the findings together into bodies of work that seem to make sense – what we would call a 'paper'. All contributing researchers have a piece of that, and can claim joint credit for the achievement.

But, of course, why should we contribute to such an endeavor. After all, we must publish our own work, to our own credit, if we ever hope to progress in our careers; getting grants, for example. So here is where the 'micro-grant' might come in. Each accepted piece of the puzzle receives a small investment toward the laboratory that posted the work. The more posted, the more micro-grants the lab receives. These post-hoc grants are completely unrestricted, essentially paying for the work that was already done (although we'd probably ensure that the money couldn't be used to take everyone out for a celebratory dinner).

Sure, there are *loads* of problems with this little idea. Do we rate contributions and reward 'large' over 'small' steps differently? Do we take into account the costs that went into the work that was posted? How do we avoid the problem of duplication of effort, while recognizing that reproducibility of a finding is important? As I said, *loads* of problems.

"But Mole!" you shout (yes, I can hear you), "the biggest problem is that this so *unfair!*" If this happens, and if I don't want to participate in this *stupid* thing, then I will find that I'm constantly scooped on my important work because someone published one tiny part of it. We'll lose the freedom to keep our findings quiet until we can publish a paper in a journal with glossy pages (or nice, soft ones).

Well, to quote Annie Hall, la-dee-dah. Who ever said that solving big problems is about *you*? If, and I think its pretty unlikely, something like this were ever actually tried, I respectfully suggest that the system would adapt, and that the publication of a single result would not be taken as reducing the impact of a major discovery. I remember when the concept of open access journals was originally floated, and there was a general agreement that it simply would not, and *could* not work. One editor of a journal with lovely, glossy pages told me, point blank, that they would never, ever make papers openly available. Now they do. Go figure. So I invite the reader to go ahead and *flame* my poor self with all the reasons why this approach is impossible, but I would respectfully ask that, rather than just saying "no!" we consider constructive alternatives.

In a sense, this approach is already underway, at least experimentally, in a couple of interesting areas. Mathematical theorems and protein folding problems are being solved in record time by using cooperative creativity on-line. Of course, we in the biomedical research business regard such research as essentially 'free,' while what we do is expensive (see *really, really*).

One thing we might consider is this. In many places where biomedical research is done, it is done for very little money. A micro-grant in some labs might make the difference between doing the next experiment or not. The big, well-funded labs might elect not to contribute to an effort like this, but I think it's just *possible* that many smaller labs would jump at the chance to submit their work to such an enterprise.

The public is at least partly right in voicing their frustration about the slow pace of biomedical research. Micro-grants are not the only answer, but perhaps, if we even try this approach on a small scale, we will find out if it can work at all. Worth a shot?

Hey, it stopped raining, the sun came out, and there are *flowers*. I might go for a walk.

Mole

Journal of Cell Science 125, 1617–1619
doi: 10.1242/jcs.110767