

An occasional column, in which Mole and other characters share their views on various aspects of life-science research. Messages for Mole can be sent to [mole@biologists.com](mailto:mole@biologists.com).

Any correspondence may be published in forthcoming issues.



## Elephants I – new PIs

It's a happy day and a sad day. Bitter sweet. You see, one of the mole-lets is leaving to set up his own lab, and we are having a good-luck party. So it's a happy day, but still, I'm going to miss having him around. Sigh. Soon, another is leaving to set up her lab, and I'll miss her, too (but we get to have another party, so that's something to look forward to). I do my best to prepare the mole-lets for this step into the big scary world of independence, but of course, now it is up to them. They will be PIs. In the real world of television, PIs are private investigators who catch bad guys and drive beat-up old cars, but in our pretend world of biomedical science, PIs are principal investigators who write grants and try to publish papers (and drive beat-up old cars). I hope our new PIs will thrive, and to do this they will have to discover something astonishing. Catching bad guys won't be enough.

In fact, they have to. Astonish, that is. 'Competent' is good, 'interesting' is better, but

'astonishing' is what we have to strive for. Anything less, I'm afraid, won't quite do. It seems so unfair, but this is the competitive world of the independent PI. Because the simple fact of the matter is that there are enough PIs out there that dazzling discoveries are made all the time, and these compete so much for our attention that merely interesting things tend to be overlooked by all of those anonymous folks who make the decisions that affect our lives.

We don't mean to be cruel, but try to see it from this perspective, terrifying as it is. When I review grants, I have a big stack of them on my desk (and by 'desk', I mean on the floor and the couch and the kitchen table, scattered like the leaves I won't have time to rake because I'm busy reviewing grants). So do the other reviewers, because we have a *lot* of grants to deal with. In all likelihood, I will only be able to pick one or maybe two to champion the best I can. In my stack, some will be relatively easy to ignore, but *most* of them will have real value. They are *interesting*. But those that are down right amazing have to be my pick; much as I

would like to support most of them, I can't. Neither can the other reviewers. So we have to be dazzled. And when we are, the grant *might* be funded.

But grants are only fuel to do more work. And now it gets tough. The cool, difficult experiments have to be done, and conclusions must be drawn based on supportive data, and these, too, have to be astonishing. Usually they aren't, because reality loves to crush our hopes and dreams of amazing possibilities – and so many things turn out to be less interesting than we plan. But the journals demand that what they accept has to be unexpected and cool, however great the question was. Because for a journal to succeed, the papers they publish must not only be read but *cited*, and there are so many papers every day that we (the readers and, hopefully, *citers*) only pay attention to what stirs our intellects. Yes, we cite merely competent studies that help us make our astonishing claims, but really, it is generally the most brilliant work we have to cite first, because there is even competition in our limited reference lists.

It is hard to imagine, as we set out to pull together our labs as new PIs, how we will even be noticed. So it is my job, in preparing the mole-lets to be independent, to tell them how.

My friend, Professor Armadillo, pointed out some ideas about scientific careers that help to frame this challenging goal. The qualities that predict success in the business of biomedical research *change* at every step. Here is a short guide to these changes, and these are a good first step in finding the route to happiness (or, at least, whatever passes for happiness in what we do).

Undergraduates who hope to be scientists must have good memories and analytical skills to show they can retain and handle the difficult subject matter. They take tests well and show

creativity in how they regurgitate the facts they ingest from their teachers. They attract attention by asking smart questions and show flexibility in accepting divergent viewpoints. I'm not saying that this is good or bad, but just how it is – teachers like students who show interest in what they teach and who agree with them, and those they like (and remember) receive recommendations. So the undergraduates get into graduate schools.

But in graduate school the students need a new set of skills. They must have the technical facility to make experiments work. And they must show critical abilities in navigating not only the data they produce, but also the massive pool present in the literature. Much (or even most) of what comes from experiments is misinterpreted (or wrong) and they have to apply their skills to find something that can be held, at least for a while, as true (or true enough). A grad student must be able to design experiments, make them work, interpret the results, and explain them. They don't have to be particularly interesting or important, but they have to be well done, and they earn their degree. And if their career path is headed in the direction of independent research (some day), they become post-docs.

And post-docs need another set of skills. While they continue to master techniques, post-docs must also learn to ask questions that not only can be answered but, based on their growing knowledge, are *worth* answering. So that when the experiments work, the results are exciting (and not only *because* they worked). The fact is, the next step tends to be out of the hands of their mentors (although not entirely, not yet) – post-docs have to attract the attention of the scientific community. To do this, they must refine their communication skills to give

talks and write papers that the rest of us will listen to, read, and *remember*. This is where the majority decide that an independent research program is simply not on the cards. Because no matter how good they are at doing experiments and getting results, the attention of the community is hard to attract. The mentors (hopefully) try to give advice and suggestions about what might be exciting and new, but in the end, it is up to the post-doc to pick up the thread of a project that will lead to such results. If they don't publish, then this is a non-starter. If they do, they have to do their best to attract attention to it, and hope the community will care. We know how tough this is, which is probably why it takes so long.

But if they are successful, they might be fortunate enough to get their own lab, to be a PI. And this is where another whole set of skills show up. They have to assemble everything they will need to set out on their new course of research, and inspire their own mole-lets to perform experiments that will work *and* be interesting. But, and this is the really hard part, they have to pick a project that they know in their hearts will be dazzling, amazing and astonishing. If it works.

Impossible? The way we are talking about it, it seems to be. But we have been avoiding the big, grey animals that have been sitting here all the time. We haven't talked about the elephants. Learning to see them is the first step. The elephants are the key. But I have to get back to the good-luck party, so the elephants will have to wait until next time.

*Mole*

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