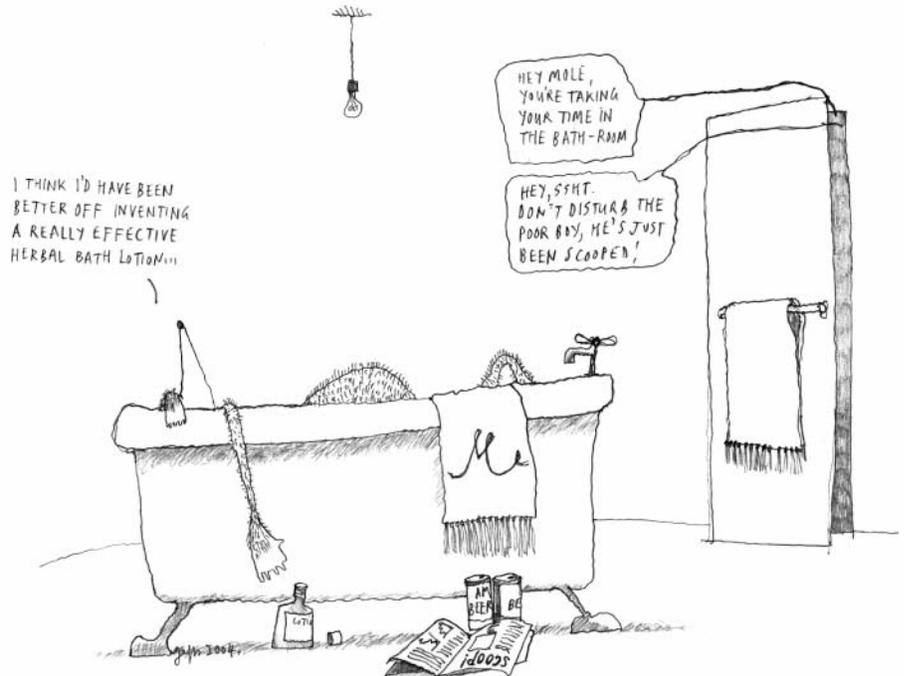


An occasional column, in which Mole, Caveman and other troglodytes involved in cell science emerge to share their views on various aspects of life-science research. Messages for Caveman and other contributors can be left at caveman@biologists.com. Any correspondence may be published in forthcoming issues.



Stealing thunder I

Today was such a lovely day, so many good things about it. Sunshine and good coffee. A pleasant walk. Good conversation over lunch, ranging from interesting observations to the latest trends in the theater. I was looking forward to a wonderful evening. And then –

IT happened.

I picked up my mail, as one does. *Important Science Weekly*, lets have a look. And (did you guess?) there, in living color (with all the added costs of color) was our lovely project, in all its glory: insightful, interesting, perceptive, important. Perfect in every way but one. Right, it wasn't *ours*.

All that work, all that effort, all that *elation* that the experiments were actually *working!* But sadly, they worked for someone else as well. We'll be fortunate if we can "publish" our work in a chain e-mail that we distribute among our friends ("If you forward the attached manuscript to 10 colleagues, good fortune will rain down upon you, but if you break the chain, your nose will fall off.")

Woe. Woe is me. As the immortal Ringo once said, "I feel like an old, worn-out drumstick."

Once, when I was a very young Molet, and very idealistic, I believed that science was about discovery and validation. I'd read about the excitement of the scientists who, in the eclipse of 1919, worked to test Einstein's special theory, and how amazed and pleased they were that the predictions held. *They* didn't shrug and say, "Oh, well, too bad that Einstein-guy already *published* it." I thought we should be truly proud and happy if we found the same thing that others also found, and that it turned out we were all right. It wouldn't really matter who published it first, just that we'd all gotten it *right*. Right?

Don't make me laugh. We know what happens. Doom and gloom. Years of work out the window. No parties. No invitations to speak at glorious venues. No being wined and dined. Back to the drawing board. Woe, indeed. But for those of you who are guiltily relishing a bit of Schadenfreude, be warned – if you persist in this trade of ours, this business of converting money into scientific publications, this is going to happen to you someday. Or so you should *hope*. Because if it doesn't, you're just not doing interesting enough work. So let me wallow in my misery and you can show me a bit of sympathy, okay?

Come on, you know the Mole better than that. We'll fix it. "Rapture," you say,

“this must be the Mole’s Guide to What to Do When You are Scooped.” And so it is.

Okay, we’ve been scooped. It happens. And to figure out what to do, we need to figure out why this is really such a bad thing. There are really three problems facing us now. First, how does this happen? Second, why is the work less highly valued if it isn’t first? And third, what can we do about it? So lets roll up our (real or imaginary) sleeves and get to work – it will help keep our minds off the pain.

So, the first problem – how does it come about that we toil away on some really interesting and novel bit of science, only to find that others have had the same insights, obtained the same results, and beaten us to the, as it were, punch? This one is fairly easy: most, if not all, science of the biomedical research type (and if you’re reading this, here, in this journal, either you work on biomedical research, or you have picked this up in your dentist’s office – if the latter, the brochures on oral reconstruction are likely to ultimately prove more interesting) generally proceeds to important new discoveries on the heels of technical and intellectual advancements. No, I do not buy into the ‘shoulders of giants’ thing (except when I’m sucking up), but that’s something we’ll save for another time. But simply put, we tend to make important new advances when the tools (intellectual and technical) become available, and others are not unlikely to do the same. And since everyone has been keeping their findings so very top secret, in mortal dread of just this sort of thing, nobody knew that they were all (except one) about to be scooped.

Of course, this is one way that one can sometimes avoid being scooped – by making it known that you are, in fact, working on this interesting and important thing you’re doing. Some scientists (not all, but some) who find that they too are working on the same thing may actually let you know when they are getting ready to submit (or at least publish) their work, giving you a chance to sneak your stuff into press. And editors of scientific journals may

well ask you to review work in that area, in the chance that you might similarly be prepared to wax poetic on the same topic. Yes, this runs the risk that you will have spurred your competition to move faster (and in secret) in response to your public announcement, but at least people will tend to know that you *tried* to be open. Just a suggestion.

Okay, so this brings us to the really vexing question (before we figure out what we can do, now that the dread thing has happened, and oh woe is we): why is this so awfully bad? After all, you’ve generated what is clearly years of work and reached the same conclusions as did the scooper. And as the scoopee, at the very least this rapid confirmation will solidify the field and bring things forward with greater pace. Or, even more likely (because you took the time to be really careful and did much more elegantly controlled experiments) your work will convince many of those who didn’t believe the somewhat more quickly slapped together (but first) publication. *Yours*, not *theirs*, should be the one that everyone will cite, being the better work. Right?

But no. It doesn’t work that way. “Well,” you might say, “I would greatly prefer to publish my work in the lowliest journal, and know that it was the better work, than waste energy and effort even *trying* to have the high impact thingie with the attendant accolades.” Such a position might give you real satisfaction, but of course, you’d still be a *loser*. But even if you say such things (and I know you don’t) you don’t really mean them, so we know you aren’t a loser. (Just to clarify, getting scooped doesn’t ever make one a loser, but never allowing oneself to take a chance of being scooped just might.) So, why doesn’t it work this way – why is the first publication (or the first few, if they come out *very* close together) the one that gets the notice? I think there are a couple of reasons and they are inter-related. One is that most people are too busy, or too lazy, or too bored, to pay attention to all of the work that is published in their fields (let alone *other* fields), and therefore, if a paper doesn’t have something fundamentally new to say, it just isn’t worth the effort to pay much

attention to it. And the second, which I fear is a bit worse, is that a majority of scientists *like* the high impact system, where the first one in gets parades and honors and invitations to parties at fabulous meetings; because they hope and expect that at some point it will be *them*. (Or ‘they’ if we’re being grammatical, or ‘me’ if we’re being honest). If we get rid of the system *now*, before I’ve gotten mine, it just wouldn’t be *fair*.

We promote a system where we designate victory for the first to publish, and denigrate (or at least tend to pay less attention to) those who come in second. Do we doubt this? Now, in the hemi-centennial celebration of those who won the race to the structure of the double helix and thus (to the eyes of the reading public, at least, including far too many scientists) ‘discovered’ DNA? It’s a simple (but ultimately untestable) fact that had W and C (and that I don’t have to say their names helps my point) chosen to focus on new species of lungwort instead, the structure of life itself would certainly have come to light within the year at most. But this does not dampen one bit the breathless rapture with which the recounting of their discovery is retold. And since we do not criticize this tendency (or, indeed, the extremely popular tradition of giving dynamite dividends to this year’s most accomplished scientists), this must mean that we not only endorse it, but also *like* it. I bet deep down, we like it because we hope in our heart of hearts, that it might come to us. If not at that ultimate level, then at least at the much smaller level of the occasional terrific paper saying that this time, the first ones to discover this really cool thing was *us*.

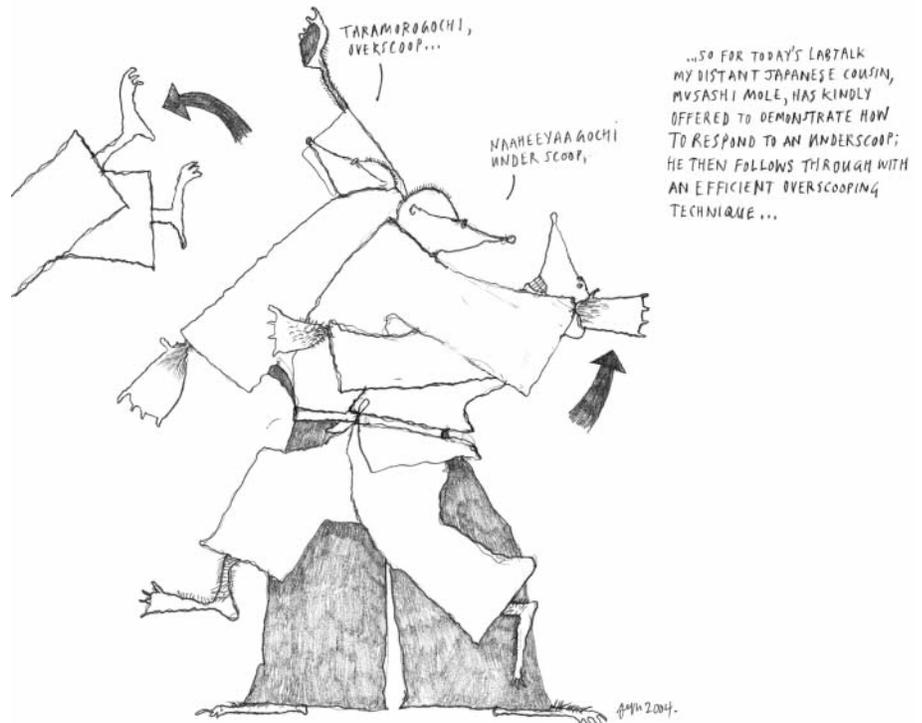
How very sweet. And I’d love to savor that feeling. But I can’t. Because I’ve been *scooped*. So what are we going to *do* about it? I’ll tell you next time. Unless, of course, someone else does, first.

(to be continued)

Mole

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Stealing thunder II

In 1709, the English playwright, poet and literary critic John Dennis watched bitterly as his ill-fated play 'Appius and Virginia' flopped at the Drury Lane Theatre. Most of his plays flopped, because it turns out that he wasn't a very good playwright or poet. (He did have a success with 'Liberty Asserted' because it made fun of the French, always a surefire hit. The sequel, 'My Big Fat French Wedding' was less fortunate.) But the closing of his play was minor compared with what happened next: in a production of Macbeth in that same theatre, he distinctly heard the sound of thunder coming from what he was sure (and correctly so) was his invention of a thunder-making device (a large mustard bowl). "See how the rascals use me," he said. "They will not let my play run, yet they steal my thunder!"

Alexander Pope noted this in mock tribute when in 'The Dunciad' he exclaimed: "Let others aim, 'tis yours to shake the soul, with thunder rumbling from the mustard bowl." Not that this is relevant, really. I was just showing off.

If you've only just joined us, you won't know what I'm talking about here. But here's a quick synopsis: I got my thunder

stolen. *Scooped*. Great work, great paper, great things just waiting to happen – and now it's all to someone else's credit. And now we have two choices – publish a "me too" paper in a journal where nobody is going to notice our hard work, ever. Or do something else.

I bet you vote for 'something else'. Me too. But who said this is a democracy – we're going to do something else whether you vote for it or not. Because I'm in a *really* bad mood – I got scooped.

By the way, this getting scooped thing – most scientists agree that it's just awful, down there with getting papers and grants rejected, way worse than experiments that refuse to work. Scooped is such a universal disappointment, that we all have the deepest sympathy. "See that guy? He got scooped. I'm going to give him my car to cheer him up. But it won't."

But sad to say, while fully sympathetic, we remember those who were first and not the others who made the discovery (pop quiz: who was the *second* one to bring fire from the gods?). Or at least, that's what we think we do and how it *feels*. The reality though? For the most part, most people are faking it. Years

later, when they introduce you at a seminar (or, hopefully, a major award presentation) they will list your accomplishments and *nobody* is going to say, “Yeah, well, he/she wasn’t first.” Because it’s the accumulated accomplishments, not the one-hit-wonders that a career make. Sure. Keep telling yourself that. You know you want to be first anyway. I *hate* getting scooped.

So what are we going to do about it?

Aren’t you glad you came to me? (And aren’t you glad it was me that got scooped, and not *you*?) Aren’t you glad that American advertising techniques are so masterful that we can sell beer to Brits and coffee to Italians? I’m not making that up, and it’s relevant. No, really. We’re going to use those techniques here and, astonishingly, we are going to do it in a way that actually makes the science *better*. And we can do that, of course, because I’m an American (although if you want to talk about politics, I’ll swear I’m Canadian).

For this to work, though, you have to have been really and truly *scooped* and not just seen a clever idea brought to fruition by someone else. You know how that works; it goes like this: “Hey Bill, what’s wrong? You look like someone just made you drink an American beer.”

“No, it’s not that, it’s just that we got scooped.”

“Oh gosh, oh no that’s horrible. I’m so sorry. Was this something you’d been working on for a long time?”

“Yes, I thought of it two years ago, but I couldn’t get anyone in the lab to actually *work* on it. And I didn’t raise any money to study it, and we didn’t actually do an experiment. But just when I started to *really* think about planning to start to sort of try something, *BAM*, out it comes in this really high impact journal, the kind with *really* soft pages. Scooped! And just because they had energy, efficiency, conviction and actually did the work.”

“The rodents! It’s so unfair. Look, here, I want to give you my car.”

Okay, you wouldn’t feel sorry for this whiner, and neither do I. So don’t whine. But we’re talking about really getting scooped – you and your lab have been working hard on a project that has been working, the data are coming in, figures for publication look good. You feel like DNA cowboys! It’s a real program of research and it’s going well. But somebody else’s went well-er. It happens, and now there’s blood on the saddle. Well, buckaroo, we’re going to turn gore into glory.

Our strategy is going to be based on the simple fact that the observation has been published (even if it’s by someone else). Because this means two things, both of which are *hugely* to your advantage: (1) The work is of sufficient interest to be published, and (2) the guys who have just done the publishing are now effectively paralyzed. Why this works depends on the sort of scoop you’ve experienced: the overscoop or the underscoop.

An overscoop is when somebody simply swoops past you and publishes *your* work (okay, it’s their work, but it *feels* like yours) in a high impact journal at or above the level you were aiming at. This is actually the best sort of situation to be in (although it feels worse than the other kind). In this situation the scoopee (you) tends to feel inadequate – we weren’t fast enough, we weren’t *good* enough, we aren’t as cool as we thought we were yesterday. But the *correct* feeling should be: Wow, were *we* on the right track here. Not only spot on correct, but also right about how important and interesting this really is. (I *know* you’re not thinking that, but you should, because of what comes next.) But before we get there, we have to look at the underscoop.

An underscoop is more insidious, more difficult to handle, but ultimately fixable as well. An underscoop occurs when someone goes for the quick fix, the easy but key experiment and publishes it in a low impact journal (even if the experiment didn’t work especially well) and, worst of all, puts something in the title that *clearly* indicates that their conclusion scoops you. Rather than feel inadequate, the victim of an underscoop tends to feel indignant – they *knew* they

couldn’t beat us with proper experiments, so they passed off this junk so that they could claim the finish line first. But now no high impact journal will touch your paper, because it’s old news. Really. And it’s stupid, but it’s how the journals work. Say you’ve discovered that an extract of parrotfish can make you transiently defy gravity and fly. If you’ve been underscooped, the editor of *Amazing Things Weekly* will say: “Sorry, but there was a paper last week in *Tropical Lagoons and Puddles* that showed that parrotfish are lighter than they look. Sorry. But thank you for your enquiry. And by the way, would you review these eight papers for us?” Nevertheless, stay in there, we’ll fix it.

Whether you’ve been overscooped or underscooped, we’re going to fix it by the leapfrog method. Your competition has just jumped over (or under) you, and you’re now going to jump over them. You simply ask the *next* question, and you do it immediately. What if there isn’t a next question, you say? But there is – there *always* is, because if there isn’t (or you think there isn’t) then you haven’t really been working hard enough or deeply enough on this project to get scooped. So think hard – what’s the next *big* question you can address with a couple of insightful experiments? Figure this out, now, and get right on it, because I guarantee that in this situation, the previous scooper will get scooped.

But you say, that’s unrealistic. They’ve not only beaten you, but they are *months* ahead – they already published the paper that you haven’t written yet, and they have most likely already submitted the one that you’re planning to leapfrog. Right? So this is just begging to get us scooped again.

It *can* happen like that. Some labs are just masters of the universe and they will scoop you and scoop you. But that hardly ever happens. And it’s because of one simple fact – they just published a paper. The guys in the lab have been killing themselves to get this work out, the paper was written, rewritten, refined, experiments were repeated and repeated, and they submitted the paper to a journal and waited anxiously. Then reviewers

told them to do it all again, but in a different colored dish, and they did that (and every other stupid thing the reviewers could think of) and they got their paper published. And now you think they're racing to the *next* paper? Extremely unlikely. The graduate student is setting up his committee meeting, the postdoc is visiting her parents for the first time in a year, and the lab head is giving lectures on their recently published findings. The lab is *paralyzed*.

By contrast, your lab is fired up. Experiments are working, you know the next big question and you will kill yourselves to get the work completed in record time, the paper written and rewritten, all of the reviewers' comments answered briskly ("as requested we have repeated all of the experiments while standing on one foot"). And best of all? Most of the data in the paper you are writing is simply the work that just got scooped! That's right – you can publish these results (or the best of them) together with the new stuff, because we all know that it really is important to confirm work by others as

quickly as possible, and this time we know it's true because it's already been published by others (you don't, of course, claim priority on this aspect – you simply describe it as something we've all known for ages, since it was published last month). You openly and enthusiastically acknowledge the work of the scooper, which was important only because it allowed you to make the really essential finding you are describing here.

Now, this is easier if you've been royally overscooped, because you don't have the difficulty of convincing editors and reviewers that the subject is important. It is by definition (since only really important things get published in shiny glossy journals or the ones with the really soft pages). You simply have to convincingly explain why the *next* question is even *more* important. And it will be, because attention has been drawn to the subject by the first paper.

If you've been underscooped, the problem is trickier, but still do-able. Here you have to point to the other work and imply that they *missed* what was

really exciting and important. Make sure that the title and abstract of your paper does not resemble theirs (or point to their conclusions) in any way – rather, their finding is taken as obvious and pedestrian, and yours is what is really astonishing. Hopefully, then, when the reviewers see how much nicer your data is (because you took the time to get it clean and right – hence the underscoop), they likely will give you the nod.

None of this is automatic, and there is no guarantee that the next leapfrogging question you choose will be viewed as more important (or at least *as* important) as the original finding that got pulled from under you. But in the meantime, you are getting work done and the work *out* (which is just as important) and things are moving forward rather than just lying there. And you are setting an excellent example for your trainees (or will, when you get some). Hey, who ever said science is *easy*? Not me.

Mole

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Cell Science at a Glance

Cell Science at a Glance is included as a poster in the paper copy of the journal and available in several downloadable formats in the online version, which we encourage readers to download and use as slides. Future contributions to this section will include signalling pathways, phylogenetic trees, multiprotein complexes, useful reagents . . . and much more.

The JAK/STAT signaling pathway (March 2004)

Cell adhesion receptors in *C. elegans* (April 2004)

Polarity establishment in yeast (May 2004)

nNOS signalling (June 2004)

The Rb network (July 2004)

The matrix metalloproteinase family (August 2004)

We would like to encourage readers to submit ideas for future contributions to this section.

Potential Cell Science at a Glance articles should be addressed to the Executive Editor and sent to

Journal of Cell Science, 140 Cowley Rd, Cambridge, CB4 0DL, UK.