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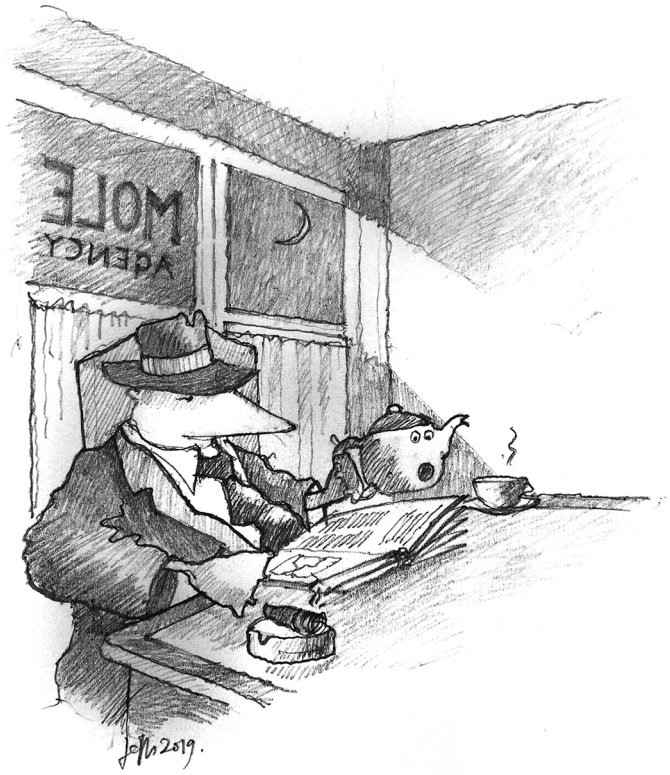
STICKY WICKET

Noir I – the anti-executioner

Mole



It was cold. Cold as an editor's smile when I'm pitching our latest work in the hotel bar at a meeting after hours. The single light over my desk cast a flickering circle of faded white that did not brighten my mood, or make the western blots in Figure 3 any clearer. The rain was painting biochemical pathways on the window panes, and I tapped along with my pencil. I made a note, and read on.

My name is Mole, just Mole. My parents lacked imagination I guess. I'm a scientist, and as such, I do *pro bono* work for publishing houses and funding agencies, reviewing for free or more often, less than free, because of my overhead of good 'tea' and cheap cigars. It doesn't pay the rent, but hey, at least it keeps me off the streets.

The world I live in isn't kind. No playful kitten videos or unicorns, just hard research and writing grants and papers that I send into the meat grinder to come back as mangled as the questions after an end-of-the-day seminar at a meeting in a small New England college on a hot Thursday in July, amid the mosquitos, deer ticks, and seventh year grad students. With warm beer and stale pretzels. Where was I? Oh yes, I live in a world of rejection.

So, you would think that when I review, and get a chance for revenge, I serve it up nice and cold. My last paper fell hard; the readers balked at the model, feeling that the six thousand references to it in the literature failed to satisfy the need to completely reconfirm the system, and wanted new models that would take only a few years to reach the same conclusion. And now I have a chance to vent some of my anger, frustration, and, I guess, shame, perhaps even on one of my anonymous reviewers (hey, it's possible, I can see this author being one of my reviewers, right?). Imagine him opening his long-awaited email to find that someone (me, but he doesn't know that) has ground his cherished dreams into a fine powder to drift away on the winds of hope. You'd think that.

But you'd be wrong. I may be old (not *that* old) and weathered (okay, weathered like the leather patches on my old jacket – I miss that jacket), and I have been around the block a few times, but I know how rejection feels. And I know that it isn't the seasoned professional in his senior-author digs who will feel the most pain, but the shiny young trainees who burned their nights away generating the data that graces the many small graphs and images comprising the figures. Sure, they have to learn to take the knocks with the occasional smiles, but not on my watch.

But there's more to this game than ink on paper and win or lose in the submission phase. A huge number of semolians (my currency of choice) in reagents, human effort, and the costs of keeping the lab lights on were spent to generate results and answer questions for the benefit of the community. Yes, publishing is our stock in trade, and our success hinges on it (Plan B: perish), but in the larger scheme this is about disseminating knowledge. The longer it takes for results to see the light of print (or more likely, electrons in e-space), the more we delay the opportunities to *use* the findings.

So, with my hat pulled over my eyes (yeh, I'm wearing a hat, want to make something of it?), I read the paper. Yes, I read the paper *before* I pass any judgement. And *I* read it, not one of my trainees. If I'm too busy (yes, it happens, even to a slumped over insectivore

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like me), I tell the editors that I can recommend one of my senior trainees, who may or may not be asked, but I never (*ever*) pass my commitments to someone else. I have to digress, taking the exit ramp from the highway of my thoughts, to talk a bit about this. We'll stop in this crummy roadside diner. Have a cup of Joe.

Many of my colleagues argue that giving papers to trainees to review is important as a training exercise. I agree; I give my trainees papers I have *already* reviewed and filed for just that, and we discuss the process. But in my experience (and remember, I'm old, but not *that* old), I have never met a trainee who can say, "this should be published essentially as is, it is that good." Because we train them to be critical, and they want to show just how critical they are. ("I can think of *lots* more experiments for them to do!"). But there is more: I would be offended if the publication of my best work, or really any work, was dependent on pleasing the whims of an anonymous graduate student. Wouldn't you? And don't pretend that you read the paper and discuss it with your trainee before you let them write the review, and then you edit it. Sure, you might look at it, but you didn't do it, they did, and you're too busy to change it much. If you say you'll review a paper, then do it, and when you're done, use it as a training exercise. Okay? Let's get back on the road. Pay the server, this one is on you, and don't forget to leave a tip.

So, I read the paper. And when I'm finished, I ask the first question: Is this useful? Does the work, in total, advance our efforts in this area? People get this confused: papers aren't 'wrong' or 'right,' they are 'useful' or 'not useful.' If the answer to my question is 'no,' it is not pretty – I will pen my evaluation of the problem to which I will append *some* of the issues, technical and conceptual, and recommend that it be sent to the cornfield. But that is rare; more often than not I see that the answer is 'yes,' or 'maybe,' and then move into the meat of the review, asking the second, important question.

Which is this: Do the data support the conclusions? If so, stick a fork in it, turn it over, we're essentially done. I may make some optional suggestions, things that might make the work even more useful, but I make it clear that these are at the author's discretion. People need to read it (not only a few reviewers) and move on. This is the best situation to be in as a reviewer. Then I celebrate with some 'tea.'

But what if the data don't support the conclusions? There may be missing controls that engender doubt; I explain that. Or an experiment is over-interpreted; I point that out. Or they make claims that are not supported (killing a cell line is not curing a disease). What I do *not* do, though, is demand a result for a proposed experiment *that hasn't been done yet.* We do this all the time: "If the authors are correct, then they should do the following experiment, obtaining this answer." Science is complicated, biology is *complex.* Years ago, we received a review demanding such an experiment. It didn't work, and we said so, and could not explain why (given what we knew at the time, it *should* have worked, it just didn't). Our paper was accepted anyway (whew!), but curiously, a companion paper did obtain the requested result. When we finally figured it out, we published another very nice paper (thank you), and people forgot about that errant result (most likely, it sort of worked for them in one experiment, and those honest folks made the mistake of including it to "satisfy" the reviewers; I forgive them). Don't ask for results, suggest experiments that are needed to reach the authors' conclusion effectively, whatever the results might be.

Okay, enough jawing, I have to finish this review. You have better things to do. Let's continue this later. Maybe the sun will come out and it will warm up. And maybe I'll get some good reviews, kitten videos, and unicorns. Or maybe I'll just have some more 'tea.'