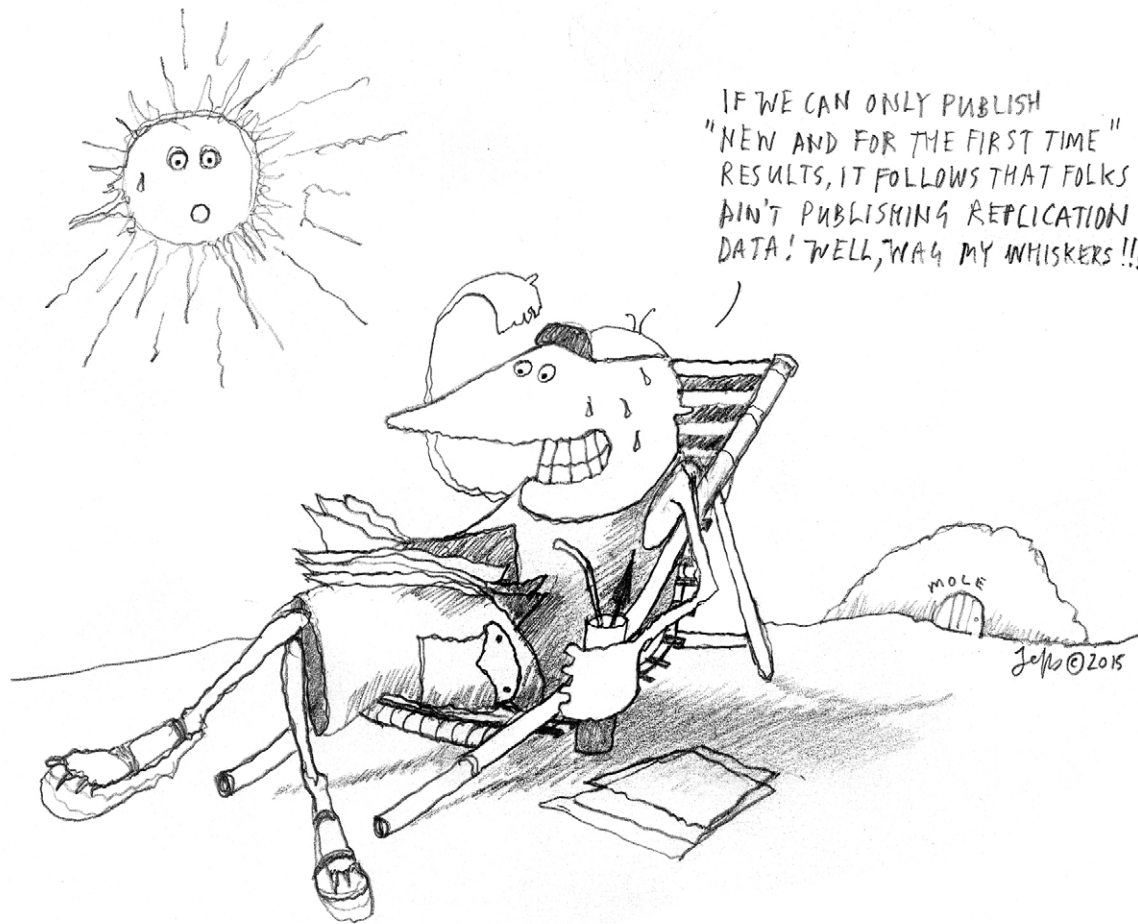


STICKY WICKET

For the first time!

Mole



Oh it's a *hot* one. Steamy hot summer day. So I am doing what I usually do on such days (whenever I can, really) – I'm sitting outside reading papers. I confess I'm sweating on the papers, but that can't be helped. It isn't the first time I've sat outside reading, or the first time I've sweated over a paper. I guess it's the first time I've sweated over this particular paper, though.

And I just read something that I've seen a lot. The authors state, right in their abstract, that *they are showing this for the first time*. Okay, what they are showing isn't particularly novel, but I guess it was the first time somebody saw the effect in this particular cell line. But it's the persistence of this phrase, the statement that this is for the first time, that I want to talk about. There's something that doesn't feel right about it. I'll start with the obvious, but stick with me, I think this will go somewhere interesting. And for the *first time!*

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Let's face it, if a paper is published, we sort of assume that at least some of this is being described for the first time. So why say it? I think that this is something that has percolated through our literature due to the fact that journals are hung up on the *novelty thing*. It started with the biggies, you know, the glossy publications, or the weeklies with nice soft pages. We know that they not only demand complete novelty, but go so far as to reject a paper late in the process if anything at all is published that they perceive weakens the utter *newness* of the manuscript in consideration.

With time, though, this need for novelty (or at least the perceived need) has pervaded other journals. I'm sorry, but we won't review your paper because we feel that the advance is marginal. I'm sorry, we will not be publishing your paper because one of the reviewers pointed out that some of the findings have been observed in other systems. I'm sorry, but you showed similar results to those in Figure 1 in a previous publication. I'm sorry, but go away until you can show us something new.

An occasional column in which Mole and other characters share their views on various aspects of life-science research.

I can't blame the journals too much – they have so very much to worry about. If we come to think that the time we spend reading a paper in their journal might not tell us something new, we may stop reading the journal, and that would mean that we might not (gasp) cite papers from that journal. Their impact factor would drop, and they might have to find another place to work. And besides, there is so *much* literature – we need to have places we can go to find out what's new.

By the way, I happen to know that for the most part, the journals we long to publish in (e.g. glossy, or nice soft pages) are not very fond of the 'for the first time' sort of statement. They understand that it is (a) implied, (b) potentially contentious (Dear Editor, in reference to your recent publication, we draw your attention to our previous publication, which showed something vaguely similar, and therefore we ask that a correction be made to the statement that this has been demonstrated for the first time in your publication.), and (c) it's a bit unprofessional. They prefer that the findings stand on their own without shouting about their *newness* (even though they insist on it).

This is all fairly obvious. But here is the thing that bothers me. Often, I begin a paper with an observation that someone has previously described, and show that it applies to the problem we have undertaken. And most of the time either the editors or the reviewers tell me to take that out, as 'it has already been shown.' It isn't *new*. Okay, we have established why *new* is important, so this makes sense.

Except it doesn't. If you've been paying attention to the front matter in many journals, and to the popular press, you may have noticed that there is a growing concern that research results are not reproducible. I've talked about this at some length before

('Replicant I, II, III'). So this bit isn't new – I contend that science progresses precisely because observations are reproducible, indeed, we don't waste time on things that don't work. But here's the new bit, perhaps (see? *For the first time!*). If we are genuinely concerned with reproducibility, and if a story I want to tell begins with us reproducing someone's findings, isn't that a *good* thing? Why in the world should we take it out?

I'm not saying that journals should publish every attempt to reproduce a finding. But I *am* saying that perhaps we shouldn't remove such information from a paper, or relegate it to Supplemental Siberia, that cold place where data goes to die.

Think about it. Right now I'm reading a paper that is actually one of two papers, from two labs, who independently reached the same conclusion while working away, probably for years, on a problem. Whenever I see such pairs (or trios, or more) I'm pretty confident that the observations are pretty much correct – already reproducible! I take the take home message to the idea bank, deposit it, and wait for the interest (my own ideas on this) to roll in. Invaluable!

But there is a reason why such papers come out in clutches (what's the collective noun for a group of papers? Maybe a *shuffle of papers?*). If one of them had shown up at the journal a few weeks late, it would have been sent back with one of those 'I'm sorry' notes. But why? If the first paper is important, the second one, published within a few weeks or months, will be so much more important, because we will have gained confidence in the findings.

I don't think we'll see claims of 'we show, for the *second* time that...' But I wouldn't mind reading those papers.

Hey, there's a breeze. It feels great, but now I have to round up the pages that are flying away. Don't worry, it's not the first time.



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