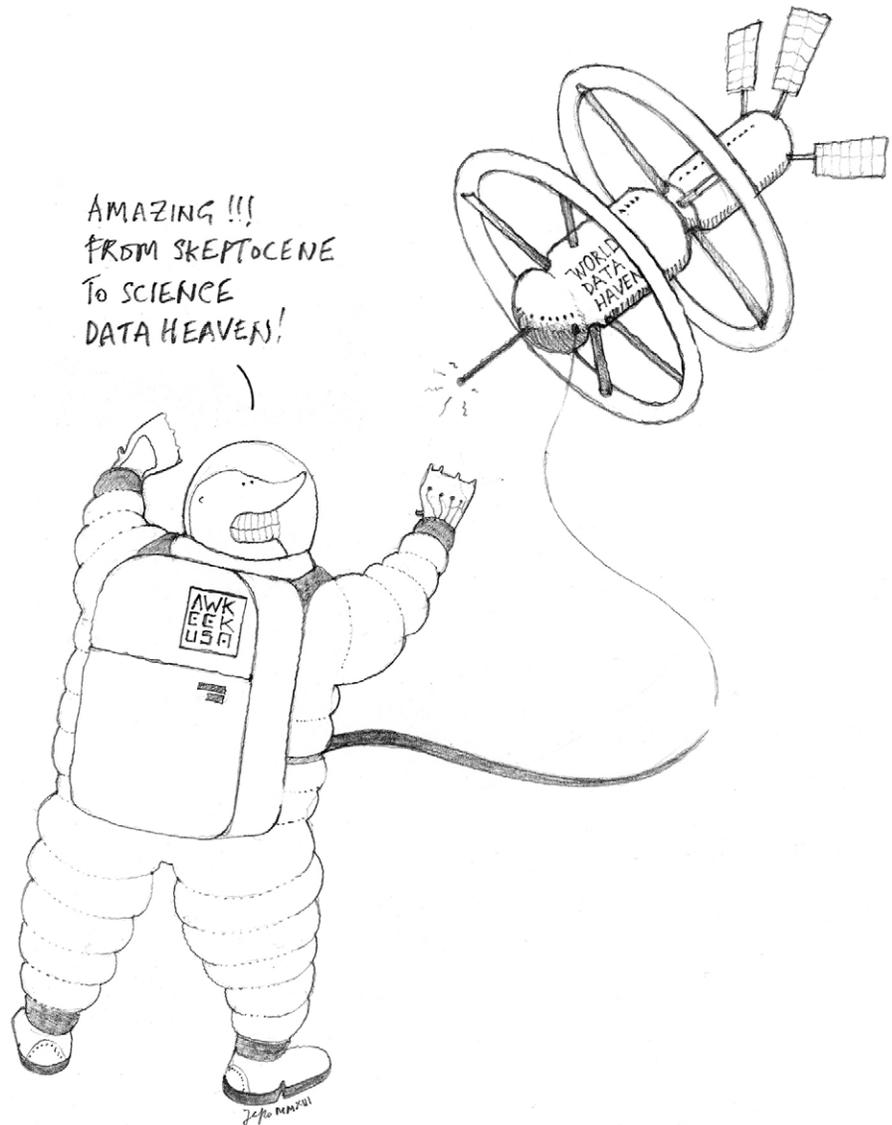


An occasional column, in which Mole and other characters share their views on various aspects of life-science research. Correspondence for Mole and his friends can be sent to mole@biologists.com, and may be published in forthcoming issues.



The end of science II – facing forward

Like some of you, I was rather taken aback by the recent publication of a column from someone claiming to be a distant descendent of mine, somehow publishing in our time from far in what I take to be one of many possible futures (I don't know this, but that's what my quantum physics friends tell me, via fun science fiction movies, and I tend to believe them, unless they're just kidding about this multi-verse thing). From the start I want to make it clear that I do not necessarily subscribe to 'Mole VIII's' ideas, and certainly nothing he (or I) say can in any way be taken as a position by this, or any other publication that chooses to disseminate it. Apparently, what he wrote has upset some people,

some very honest people, who are very honestly trying to tar and feather your humble Mole and ride him out of town on a rail. Apparently these are not people one can poke fun at, *not that I would*, and speaking personally, I am very much a fan of inquisitions of all kinds.

One letter, though, took a very different tack, and I want to share it with you. It might be interesting. *I do not in any way subscribe to the letter author's views, nor does this publication.* I feel I have to say that so that you won't think I'm taking sides. In fact, stay with me, because I'll have much to say afterwards.

*"Dear Mole VIII,
Writing as I am from your past, you may find this document a bit decayed, if*

you find it at all, but I felt compelled to respond. You intimated that some of those who draw our attention to irregularities in the scientific literature do so out of bitterness or spite, angered by the apparent ease with which some scientists publish work while others, perhaps the accusers, labor furiously and repeatedly fail. You mentioned a website you call 'Science Fake' and while I have not found such a site, I know of others that were posted by very honest scientists who have a genuine concern about the state of scientific communication. Those who are involved in this enterprise stipulate that their interest is in validation of the integrity of published science, and they have a genuine concern that the scientific literature is rife with outright fraud. They are genuinely disturbed by the contention that "science is self correcting" and feel that more is needed to police the literature. That said, there are elements in this endeavor that I find particularly disturbing. Using something akin to the style of your ancestor, Mole, I will list them here. These might be considered 'Professor Ground Sloth's Guide to Science Integrity Web-Sites.'

1. Derision. Every site posting allegations of scientific misconduct includes some element of derision, sarcasm, barbs, rancor, antipathy, and the like, some to more and some to a lesser extent. Usually, no line is drawn between confirmed fakery (which does deserve derision in my book) and simple errors. But if the purpose is really to monitor the literature and call attention to concerns, the tone often belies this mission. This leads to #2.

2. The accused are guilty until proven innocent. When an allegation is made, it is assumed that the authors will provide support of their innocence, and if this is not forthcoming, it is assumed that they are guilty of fakery and fraud. If there is an investigation, say by their institution, which vindicates them, this is not posted, nor are the accusations removed (indeed, many institutions have a policy that prohibits making their findings public). This leads to #3.

3. All retractions are 'proof' of fraud. If a paper is withdrawn by a journal, this is taken as a clear demonstration that the work was fraudulent. Indeed, it is based on such retractions that the statement is made that fraud is rampant in the literature. But again, this is an

assumption, even in those cases where the journal explains the retraction. In fact, many journals retract a paper if the authors cannot provide original data (e.g. blots) because they were lost, and regardless of whether the authors can provide substitutes that show that the science was correct, if misrepresented. Yes, there are cases of genuine fraud, and as such should not be tolerated, but where in all of this is there a 'benefit of the doubt?' Where is the presumption of innocence?

4. Who polices the police? Sites drawing attention to irregularities in the literature raise issues with published work, and in some cases it is clear that the concerns are justified. But not always – indeed, many claims turn out to be unfounded, but while the accusations can result in enormous expense in terms of time, effort and professional reputation, there is no effort to ensure that the accusations were not malicious (or at best, misinformed). Any accusation is expected to halt scientific work until resolved, or face censure. And even if the accused can demonstrate that all of the published data is validated, they face censure if any data was misrepresented, even accidentally. Whistleblowers, the accusers, never face censure. Why are we quick to doubt a fellow researcher but not a program that supposedly detects data misrepresentation? Since when is 'forensic' evidence unimpeachable? And since when does an accused individual not have the right to confront his or her accuser?

5. Who sets the 'rules?' It very much seems that there are a number of individuals who feel that any 'rule' that is established today is retroactive for all time. The fact is, the only rule that has always persisted is that any data represented in a publication is not manipulated to deceive the reader. It is only recently that there has even been a suggested time period for which data should be held following publication, and this remains only a recommendation – yet scientists are expected to find original data dating back many years. Furthermore, manipulation of figures for clarity, or space constraints (e.g., showing only part of a blot, removing irrelevant lanes) as long as they did not change the conclusions, was accepted practice until relatively recently. Now they are often 'proof' of criminal activity.

I do not know where this leads, or what to do about it. But I submit that opening this 'free-for-all' distorts the public view of scientific integrity. Just as I now remove my shoes at airports because of one lunatic with a shoe bomb, I now worry that my work will come under assault, and despite the fact that I have never misrepresented a single piece of data with the intent to deceive anyone, I will spend my time defending my publications instead of making new discoveries. Indeed, I worry that if I reveal my 'human' name, they will come after me for voicing my opinion. Therefore I sign this,

Sincerely yours,
Ground Sloth"

I'm not going to comment on the validity or not of Prof. GS's concerns (okay, just a bit). Indeed, I want to make it clear that I have no opinion on these at all (and I in no way speak for this publication!). I don't even know if there really is a 'Professor Ground Sloth' (I thought they were extinct). But I think there is a problem here that may need a solution: the perception that scientific fraud, the outright lying about scientific results for personal gain (which exists, although the extent to which it exists is, I suggest, debatable) is extremely damaging to the public perception of the entire scientific enterprise. Why should the public pay for scientific research if the work cannot be trusted? So, I want to offer some ideas. Hey, it's what I do.

I suggest that the first thing we should do is look forward, not back. Yes, if there are clear cases of scientific misconduct with intent to deceive, it should be investigated. The best examples of this come from genuine whistleblowers working within the laboratory or institution where the work is done, and investigation of such allegations is of paramount importance. But I want to suggest that combing the past literature for similarities of error bars, copied text, rearranged or cut lanes on blots, spots or dots that may be similar in different figures and the like is not productive, and we do not have adequate ways to investigate these properly (but I will suggest that we will have such a way if we chose to follow the 'facing forward' idea). At best this results in some frauds being caught out, but at the cost of many honest scientists being embroiled in controversy.

I spoke with my wonderful friend Professor Ant about this. Ant is a philosopher and a scientist, and despite him being an insect and me an insectivore, we are great friends (and I've never been tempted to eat him, in part, because we usually meet in terrific restaurants). His suggestion was this: transparency. I think I know what he means, and it does suggest a great way forward, but one that will take some work.

Here's the idea: when we publish a paper, from now on (or once we can effect this idea, not 'now' but in the near future), we make available every bit of primary data used for the study, from which the data actually shown in the paper was derived. Ideally, we should make this available to anyone who has any concerns at all (we'll get to that). If we do that, it won't matter if we rearrange the lanes in the blot in Figure 3 to make it easier to understand the result; it won't matter if we show only part of a

micrograph or part of a gel; it won't even matter if we show graphs with error bars that are standard deviations rather than the full spread of the data. Because all of the data will be there to evaluate.

The problem is, where is 'there?' Journals are not interested in becoming huge storehouses of primary data. It isn't their job (indeed, we can question whether it is even their job to investigate allegations that some figure or other doesn't look quite right to someone). Some institutions might be willing to institute such large data storage, but some may not be so inclined (or able to).

Perhaps we need to get together as a community and create 'data havens,' such large storehouses of primary data, of *deep data*. This could be done at federal levels, perhaps, or by enterprising private concerns. In these havens, data would be 'free' (hey, its been paid for with hard cash, but you know). Indeed, one could envision vast treasure troves of primary

data that would support the scientific literature. Yes, certainly we may find mistakes here, where a published figure is not an accurate representation, and call attention to it (correcting the mistake or identifying genuine fraud); but Ant suggests that while mistakes will happen, fraud becomes far less likely when everything is on the table. But there's so much more to this idea. These would be data repositories where investigators can mine for hidden gold among all the results. Perhaps some of those who currently spend their time seeking to correct the literature may turn some of their efforts to such data mining.

Looking forward, this does seem to be something worth considering? Not an end of science, but a wonderful new beginning?

Mole

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