Look Ma: no giants!

One of the most famous quotations in the history of science is Isaac Newton's *If I have seen further, it is by standing on the shoulders of giants*. Even making the statement, Newton was building on the work of others, as the image of standing on giants' shoulders had entered European thought through the 12th century Bernard of Chartres (*Nanos gigantum humeris insidentes).* It carries with it an essential deep truth of research and scholarship: we build on what others have done before us and, if we reach higher than our predecessors, it is not because we are somehow more brilliant but because our predecessors gave us such a powerful leg-up.

The on-the-shoulders-of-giants idea permates scholarly writing and is made very visible through our habit of justifying all significant statements of fact or opinion with a reference to their source, like this (Bloggs et al., 1963). Newcomers to this kind of literature at first find the presence of citations very irritating, but quickly become conditioned to reading straight across them, perhaps noting in passing the mention of a familiar name but not having the flow of their thoughts interrupted. An ability to cite previous work properly is an essential skill that is taught and assessed in undergraduate work. Or, at least, it should be. As the years go by, I find myself having to demand more and more corrections to this aspect of writing, at first in PhD theses and now, increasingly, in the professional scientists' manuscripts sent to me by journals for peer review. Problems show up in three ways: citation of reviews where original sources are needed, citation of the wrong paper as an apparently original source, and neglecting to cite prior work at all. Writing a blog about these transgressions may seem like pedantic academic nit-picking, but I think this issue really matters. Here are three reasons why:

1) We are not playing the telephone game.

The 'telephone game' is an inoffensive American term for the well-known pastime in which a message is whispered from one person to another, along a line, and spoken out loud by the last person: the process of passing the message on tends to distort it as it is misheard. The results of this can be amusing, especially if the original message is well chosen. The distortion of the telephone game does not depend on using a communication channel with a poor signal-to-noise ratio, like whispering in a noisy room. It can also happen in a perfectly clear channel, such as written text, when people at each step in the chain summarize or paraphrase the original message as they pass it

on. This is why it is so critical that a good scholar reads the original experimental report that makes a claim critical to her own work, rather than relying on reading the words of other people who summarize the claim, perhaps in turn by relying on yet other summarizers. It is not that the other summarizers are incompetent; they just have special interests.

Say, for example, that an original paper presents evidence that a peptide hormone - let's call it stimmulin (a made-up name) - is important in the regulation of cell division in stomach, spleen and lung. A later researcher, writing a review on recent advances in post-injury lung repair, mentions in her review that stimmulin controls cell division in lung and may be important in repair. An experimenter, reading the review, is inspired to measure stimmulin levels in animal lung necropsies following cytomegalus infections and finds it raised. She publishes that fact. An immunologist writing a review mentions that stimmulin expression is a molecle associated with pulmonary cytomegalovirus infection. In response, a bioinformatician adds an 'associated with cytomegalus infection' tag to a database entry on stimmulin. None of these people has done anything outrageous – they have each focused, briefly, on one specific little bit of a story that interested them. A lazy grad student, finding the tag and the most recent review and not working back, writes in his research proposal that there is evidence that cytomegalovirus infects stimmulin-expressing cells and plans to focus on the interaction of virus proteins with stimmulin for his PhD. Anyone who took the trouble to follow the story back to the original reference would realize that this would be a ridiculous idea that muddles cause and response, but anyone who took for granted the idea that the most recent review is bound to be the best source of information would not see the problem. Going back to the original source for anything central to the research itself is always the way to avoid this. My colleague Prof David Price has a sign on his office wall, saying *Six months in the lab can save two hours in the library.*

2) Giving credit where it is due

Anyone taking the trouble to work hard at the lab bench for hour after hour, day after day, sometimes year after year, to wrest one of nature's secrets from its dark hiding place, then taking the trouble to write it all up in a manuscript and fight with editors and reviewers to get the thing published, ought to be able to expect that others have the courtesy to cite her paper when they mention the discovery. The researcher making the discovery has every right to be boiling mad if subsequent authors decide to cite a review or a later author, instead of her, for the basic finding. Given this, there is surely an imperitive to 'do as we woiuld be done by', and to cite others as we

would wish to be cited ourselves. It's simple courtesy.

3) Non-attribution as a gateway drug to fraud.

Two very, very bad features of current scientific publishing are that (1) in some parts of the world, at least, publishing in “high-impact journals” (those journals whose papers are, on average, mentioned many times by subsequent authors) is essential for developing, or even maintaining, an academic careers, and (2) these journals insist on publishing only research that seems highly novel. “Highly novel” here stands in contrast to “incremental”, which means making progress by the usual means of adding one more layer of knowledge to existing foundations. This seems to encourge some authors – and here I am not just writing of barely competent PhD graduates but also of senior professors who really should know better – to claim a lot more novelty for their work than is justified. They do this in three ways.

The first, which I know about only as someone who served as Editor-in-Chief of a journal for 8 years, is to write a covering letter that makes very strong claims about originality. Peer reviewers who are sent the manuscript do not usually see the covering letter, so they have no opportunity to repudiate even an outrageuous claim and the chances are that the editor, especially the editor of an broad-based, very high impact journal, does not know the detailed field well enough to realize that the covering letter is being 'economical with the truth'. As an Editor, I sometimes sent peer-reviewers extracts of the covering letter for comment, if I suspected someone was trying to play this game. I have never received a similar request for comment from any other editor, so I suspect this editorial practice is unsuual.

The second trick is to make more subtle claims of originality in the manuscript itself. Here the authors are likely to be more careful, hedging their phrases just enough that a peer reviewer does not have a clear target to attack because the phrase is not actually a lie, but at the same time providing something that will remind the editor of the strong claims he has read in the covering letter. Sometimes this writing is very subtle and clever; sometimes the aim is achieved simply by scattering a few words like 'might' and 'may' around the critical sentences.

The third trick, and the one relevant to this blog article, is to fail to mention and cite previous work at all. In a research field in which I publish and peer-review papers, the developmental biology of organs, it has become fairly common in recent years for laboratories to apply modern techniques of

imaging and cell tracing to revisit questions asked long ago. These are questions such as “what things in the mature organ can this cell type make?”, “do these blood vessels form where they are and then connect to the blood system as a whole, or do they form as branches from an existing blood vessel”, “when these two tubes connect, which one moves to find the other one?” and “if this is damaged, which cells divide and move in to repair it?”. The questions were asked long ago, and often they were answered long ago by awkward and imperfect techniques that were the best available at the time. It is perfectly sensible to apply 21st century techniques to confirm – or maybe refute – the conclusions of meticulous 19th century anatomists and physiologists. But the shocking thing is that so many of these manuscripts make no mention at all of the original work: they read as if the modern experimenter was the first person to ask a “highly novel” question and the first person to adress it experimentally. It is left to peer reviewers to make a big fuss, and this only happens when at least one peer reviewer has the right background (is, for example, an expert on the organ rather than, or as well as, on the new technique). It is not, of course, that the 21st cenury paper should be rejected – just that it should be re-written to present itself correctly as a modern re-examination of previous research.

I am not implying that this non-mention of older work is necessarily fraud. It might be simply that a 21st century author knows nothing of the previous work. It is, though, at the very least, bad scholarship because it is the business of researchers to become familiar with the knowledge-base of their field. Not doing so, and therefore feeling free to claim more novelty than there is, is a step on a slippery slope. If we become tolerant, culturally, with not including mention of previous work, what else might we come to tolerate? Not including mention of possible weaknesses in the experimental design? Not including mention of experimental runs that go against the pet theory? If we behave like this, and encourage graduate students in the belief that it's OK in the name of their career in 'hot journals', then we do science a grave disservice in the long term.

The partial remedy

Struggling against an increasingly dominant culture can be dispiriting, but that is no reason for us each to do what we can as individuals. I think that the easiest place to make a difference is in examining Bsc, MSc and PhD theses. If you are doing this, look carefully at references, especially in the introduction where the basic foundations of a line of work are described. Are the appropriate papers cited, or has the student relied on reviews? Worse, has the student just cited an experimental

paper that looks like an original source but is in fact the earliest mention of something that they can find on the web? Far too often, students behave as if their computer screen is the only source of knowledge in the world and the idea of going to a library and looking at an actual paper journal or book can be something genuinely outside their pattern of thought. Good labs often keep photocopies or scans (subject to local copyright laws, of course) of classic material, so that newcomers to the field can access it easily. I do this too, but not for everthing – for some things, I still recommend the library, just so that new students get used to the place, become happy going there and, of course, get to meet its now world-famous cat (see Links below).

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June 2015

Links:

Library cat: https://www.facebook.com/pages/Library-Cat/1425194534381693