Miscellaneous commentary

Research misconduct (*January 2012*)

In 2011 there was a growing trickle of news about various kinds of research malpractice: data fabrication, falsification and obfuscation (not reporting adverse outcomes); plagiarism (*Watch out burglars about* January 2008) ; repeated publication of data, text and diagrams (self-plagiarism); 'guest' contributors; plus other kinds of scientific fraud and chicanery. Motives are many, from malice to laziness, but more often than not are a mixture of ambition, greed, jealousy, desperation and paranoia that increasingly form the downside of academic life – not the least in science. Life is *so* hard in a career dominated by promotion, which in academia rests on: publication lists; peer citations; journal impact factors; institutional income generation and, let's face it, by the kind of individual self-regard and hubris that drives people to seek fame and celebrity. The wider population has grown accustomed to this as bystanders watching Big Brother, the X-Factor and Fame Academy.

Fiddling research has reached such a level as to provoke the world's most prestigious research outlet, *Nature*, to include an editorial on the topic (Editorial 19 January 2012. Face up to fraud. *Nature*, v. **481**, p. 237-238; DOI: 10.1038/481237b), albeit after a first lead about the Antarctic Treaty at the centenary of the race to the South Pole, and followed by a puff for articles in the same issue on how to get research funding from the public or philanthropists.

As many scientists suspect, what does in fact emerge about research malpractice is the tip of the proverbial iceberg. Some admit to wandering from the path of righteousness themselves (but not saying how or where). One mild form is making unsubstantiated claims: a great many geologists (including me) have trodden very thin ice in this regard (unless they wisely included the 'Get Out of Jail' verb 'to speculate'), but few innocent bystanders, if any, have met a horrid fate as a result of resultant health and safety 'issues'. A great deal that should not does get through peer-review to enter the canon of this or that discipline. Academic fraud is a quasi-crime with few risks of detection, though punishment can be swingeing, in the manner of being sent to the 'dark place'.

According to *Nature*, what makes Britain seem to be a haven of academic honesty is the risk that both journals and 'whistleblowers' face from libel laws, should deeds and authors be named and linked. Moreover, certain kinds of gross malpractice never reach peer-reviewed publication. Examples are: malicious falsification of someone else's data by a perpetrator with access to the data on, say, a lab server; swapping analytical sample labels; destroying lab records(see *Sabotage in science* November 2010); petty theft of ideas (on which there is no formal copyright), for instance through copying poster presentations at conferences; misuse of peer-review privileges – generally anonymous (see *Anonymous referees* August 2006); menacing a presenter at a conference or disrupting their presentation. Victims of such actions rarely have any redress, unless the perpetrator is actually caught '*in flagrante delicto*', so to speak.

'Whistle blowers' or complainants then face the defensive mechanisms of the academic world: not dissimilar to those of the musk ox. How far you get as regards redress depends to a large extent on the seniority of the perpetrator. An extremely brave friend cited, with abundant evidence, his vice-chancellor for gross cronyism: he was soon 'on the cobbles' with the VC (male) remaining '*virgo intacto'*. Yet an Industrial Tribunal took a very dim view of the whole affair: my pal paid off his mortgage and lives well in retirement from the compensation awarded by the tribunal. But it takes an exceptionally brave graduate student to take on their supervisor(s) for *malfeasance* (or even the lesser misfeasance and nonfeasance). The best likely outcome (after long and harrowing procedures) is a kind of bribe – more time to complete – but most victims just disappear without completing. Unless the perpetrators are low on the academic scale (they might get a reprimand at worst), promotion or enhanced early retirement is a common response by senior management to mounds of incontrovertible evidence of guilt. The oddest fate for someone flying high in the institutional firmament was rumoured to be an engineered posting to a university at the antipode of the offence: but I digress…

The geosciences seem, at a casual glance, immune to research malpractice, which may reflect at best the small numbers involved in the discipline or at worst because no-one notices, or cares for that matter. Unless, that is, dear reader, you know different… Most important, for graduate students who are the most usual victims: protect yourselves (see *Protecting your intellectual property* December 2003).

Related articles: Research Misconduct Revealed in UK (medicalnewstoday.com); A new code of conduct for researchers (eurekalert.org); Greater support is needed to tackle the serious emotional consequences of whistleblowing (eurekalert.org); Academic funders change rules to reveal dishonest researchers (canada.com)

Time wars flare up again (*February 2012*)

Last year I noted a rationalisation of the way in which geological time is signified (*Rationalising geological time* May 2011). A working group set up by the International Union of Pure and Applied Chemistry (IUPAC) and the International Union of Geological Sciences (IUGS) defined the year as the base unit, standardizing it to the time in seconds between one solstice and the next at the equator for year 2000 (3.1556925445×10^7 s) thereby linking it to the *Système international d'unités* or SI base unit of the second, itself defined in terms of behaviour of the caesium atom. It is to be signified by 'a' for *annus* (year in Latin) and preceded by 'k', 'M' and 'G' for thousands, millions and billion years, complying with the SI progression in steps of 10^3 for units.

The sticking point for some, mainly in the US (e.g. *Science* magazine and many geoscientists there) is that the ka, Ma, Ga symbols are to apply not only to times before the present but also to spans of geological time. Since the agreed convention is incorporated into SI it has almost the force of law for scientists , so that the Cretaceous Period will be said to have begun at 145.5±4.0 Ma ago, ended at 65.5±0.3 Ma ago and was 80 Ma long, instead of the latter being in m.y., m.yr., mya or Myr according to what seem to have been personal quirks or those of scientific journals.

Somewhat florid reaction against the rationalisation (Christie-Blick, N. 2012 Geological time conventions and symbols. *GSA Today*, v. **22** (2), p. 28; DOI: 10.1130/G132GW.1.) seems to have flowed from a deliberation in Prague on the IUPAC-IUGS proposal by a lesser world body: the International Commission on Stratigraphy's (ICS) International Subcommission on Stratigraphic Classification (ISSC). The meeting voted 16 to 2 to reject the proposal – a substantial number of voting members abstained – claiming that it violated SI 'rules' regarding base- and derived units. The issue, on reaching the ICS meeting, at the same

Prague workshop, seems to have been greeted by a 50:50 split. A closed meeting of the ICS Bureau (now we can begin to see the kind of thinking involved here…) on the workshop's last day unanimously adopted the motion;

> '***We neither accept nor reject*** *the IUGS-IUPAC Task Group's recommendation to apply Ma, generally, as the unit of deep time. We accept the argument for Ma as a single unit for time but would recommend flexibility, allowing for the retention of Ma as specific notation for points in time (i.e., dates) and myr as a unit of time denoting duration. We agree with the spirit of this statement*' [my emphasis]. '

Neither accepting nor rejecting' is something familiar from minutes of the Central Committee of the former USSR, being rumoured to have been Joseph Vissarionovich Stalin's favoured formulation in moments of uncertainty: a little like the old 'Belfast Question' (during the Troubles, 'Are you for us or against us' from someone who would not make their political position entirely clear.

An argument proffered by Christie-Blick is;

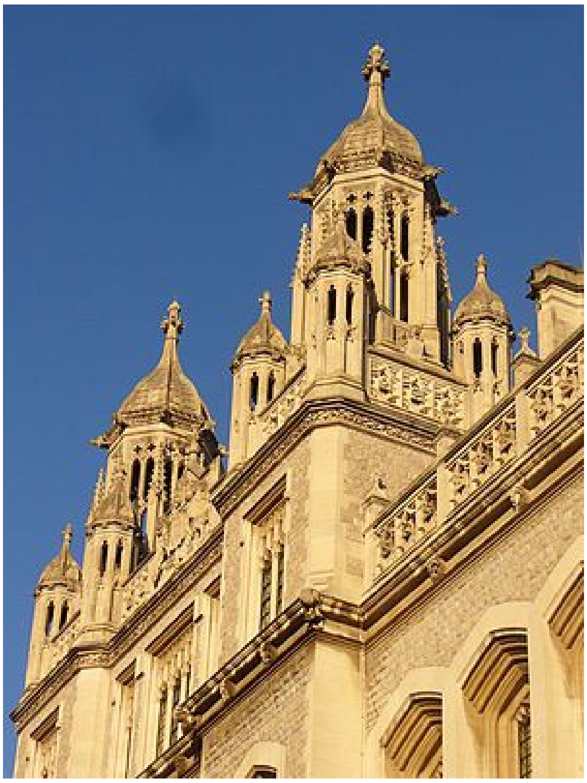
> '*No one objects to the storming of the Bastille on 14 July 1789 (a date) or to the construction of Stonehenge from 2600–1600 BC (an interval specified by two dates). In the case of the latter, we say that the job took 1000 years, not 1000 BC.*'

This forgets something quite practical: geochronologists rarely if ever, 'neither accept nor reject' AD, BC BCE, or CE but express time in years before present, with the odd convention that 'the present' was 1950, before atmospheric testing of thermonuclear devices. What is wrong with the answer to the question, 'When did the Cretaceous begin?', being 145.5 Ma ago, or '80 Ma' in answer to, 'How long did it last?' Who would prefer the alternative to the second question – 80 (choose your preferred symbol from the following: m.y., m.yr., mya. Myr., million years or millions *of* years)?

Related articles; How To: Build a geologic time spiral cake (boingboing.net); Prehistory Notebook Pages – Primary (awakeningwonder.wordpress.com)

Is there misconduct in geoscientific research? (*August 2012*)

Brian Deer, the British investigative journalist who exposed the methods used by Andrew Wakefield's to implicate the MMR vaccine as a cause of autism, has broadened his scope to research misconduct throughout science (Deer, B. 2011. Doctoring the evidence: what the scientific establishment doesn't want you to know. *The Sunday Times*, 12 August 2012, p. 16). His article comes in the wake of several related articles in leading scientific journals (Enserink, M. 2012. Fraud-detection tool could shake up psychology. *Science*, 6 July 2012, p. 21-22. Macilwain, C. 2012. The time is right to confront misconduct. *Nature*, 2 August 2012, p. 7. Godlee, F. 2012. Helping institutions tackle research misconduct. *The British Medical Journal*, v. **345**: e5402; DOI: 10.1136/bmj.e5402). The focus has shifted in the last decade from a major campaign against plagiarism by students tempted by the information largesse of Wikipedia, Google and other electronic treasure troves, to unwholesome behaviour among university academics. In an age when job redundancy at universities has become an issue for the first time in nine centuries, many academics – frenzied by looming cuts – are engaged in a Gaderene rush for promotion and funding. The now obligatory stream of 'learned' papers seeks to justify their own puff and, equally as important, the puff of their departments, faculties and institutions trying to blag their corporate way through funding shortages. Misconduct is the child of education-as-commodity.



Ivory Towers in Chancery Lane, London. (Credit: Colin Smith)

There are three mortal sins of academic fraudulence: plagiarism, including self-plagiarism (see Self-plagiarism January 2011); data falsification, including fiddling with those of other people (see Sabotage in Science November 2010), and fabrication of data, such as starting with a made-up graph and then using it to create plausible values in a table. Venial sins include publishing much the same data and interpretations again and again. The last highlights one of the reasons why miscreants get away with their chicanery and benefit from it; sloppy academic editing and even sloppier peer review.

Deer observes that 'The science establishment's consensus is that there is no need for outside scrutiny because … science is above that kind of misconduct that has tainted the Roman Catholic church, politics, the press and, of course, the banks.' But, as in these notorious cases, the lid is coming off scientific misconduct, largely through the bravery of 'whistle-blowers' within the system. Yet the offenders who have been unmasked were unfortunate enough to work in institutions that have appropriate investigative mechanisms and the will at high office to use them. That determination to maintain the highest ethical standards in academe is perhaps not as widespread as it once was.

Geoscientists have yet to figure much in the rogues' gallery of malfeasants, except for the odd light-fingered palaeontologist. That may have something to do with the vagueness of much of their scope, epitomised by the trajectory of a lithological boundary on a geological map of poorly exposed ground. Indeed, virtually every aspect of the science is open to many interpretations, and errors of omission are perhaps more common than those of commission – any field worker knows that they will inevitably have missed something. But there are quantitative, laboratory-based aspects of the science, such as radiometric dating, that are more readily scrutinised for malpractice. In the early days of using radioactive isotopes and their daughter products to work out an age for an igneous or metamorphic event a common analytical tool was the isochron plot, as in the Rb-Sr method. A 'good' age was signified by all the data points falling on or very close to the line of best fit from which an age was calculated. Consequently, there may well have been cases where errant data were conveniently 'lost', but there was no way of telling.

That it did happen emerged from the honesty of those isotope geochemists who openly admitted that some mass-spectrometry runs had been omitted because the samples showed some signs of 'contamination' or 'open-system behaviour'. For that they were merely taken to task by those who disagreed with their findings, but excused by those whose ideas the results supported: ethically honest. But how many Rb-Sr runs never made it to a published data table? Things are now a great deal more sophisticated than the days of punched tape and IBM cards in the geochemistry lab, geophysical software and that used for the growing cottage industry of process modelling. So much data and such a wealth of corrections that vast spreadsheets develop in the course of analysis, correction and calculation. Few peer referees are going to go through data-processing steps with a fine-tooth comb, even if they have been lodged in public data repositories. Such settings provide ample scope for data invention, 'fiddling', 'fudging' and, in labs with a cavalier attitude to security, stealing, but little way of pinning down any malpractice: that is, unless a culprit is either carelessly overconfident or a serial offender. A simple test that any peer referee might apply, most usefully at random, is to ask for a copy of laboratory notes associated with a manuscript. If one is not forthcoming, then suspicions will naturally arise.

A measure of just how much dodgy behaviour may go on is a survey conducted by Daniele Fanelli of the Institute for the Study of Science, Technology & Innovation, at the University of Edinburgh (Fanelli, D. 2009. How Many Scientists Fabricate and Falsify Research? A Systematic Review and Meta-Analysis of Survey Data. PLoS ONE, **4,** e5738; DOI: 10.1371/journal.pone.0005738). In it he found that up to a third of all researchers admit – anonymously – to engaging in shoddy practices, while around 2% admitted to having fabricated, falsified or modified data or results at least once. When asked off the record about colleagues, 85% of researchers reported suspicious behaviour known to them, 14% for data falsification.

Time cannot be far off when the red laser spot moves across geoscience labs and individual geoscientists. Are they audited by disinterested peers and, for that matter, in such a small tightly-knit discipline are there such individuals? Do managing academics scrupulously keep records themselves and demand that their research fellows do likewise? Are there victims or witnesses brave enough to blow the whistle on any spite, fraud or slovenly methods, or will our science remain in its habitual state of bliss?

Publishing: is it worth the effort? (*December 2012*)

A measure of the esteem in which a peer-reviewed paper is held is supposedly the number of times to which it is referred in other papers. Of course, the older a paper is the more chance that such citations will have built up; but the annual rate of citation is likely to fizzle out over time. Papers that create a frisson of initial excitement and command enduring citation are few and far between: they probably launched a new line of inquiry.

It is instructive to try to nail Alfred Wegener's influence in tectonics using the Web of Science (if you access it without hitting a paywall), which ought to have been pretty high. Superficially, he had none and is remembered through that arm of Thomson Reuters for six papers: four on atmospheric physics (his speciality), one on lunar craters and a sixth on the patterns of cracking seen on rotten wood. These give him a mere 20 citations. Wegener's posthumous problem was that *Die Entstehung der Kontinente* first appeared in the fourth issue of *Geologische Rundshau* in 1912, and apparently the Web of Science doesn't have that journal in its archives of a century ago. Later, extended editions appeared in book format which were not peer reviewed (most geoscientists would not touch his ideas with a barge pole until long after his death in 1930), and are therefore outside the academic pale, to some extent. The key to a plausible mechanism for continental drift – symmetrical magnetic striping above ocean basins – was first described by Fred Vine and Drummond Matthews in an issue of Nature in 1963. In 50 years their work, ranking with discovering the structure of DNA, has accumulated 709 citations; i.e. 38.5 citations per year on average, which is not much for fuelling a revolution.

Alfred Wegener, the unsung hero of continental drift

Of course, citation is not the same as the frequency at which a paper is read. It is no secret that a not inconsiderable number of papers that appear in published reference lists haven't been read by the authors who cite them. They are there by proxy, and you will probably find them in the bibliography of later papers that those same authors have cited. There is perhaps a certain kudos in such proxy citations, for it may be that the cited paper has achieved the equivalent of canonical status in the field.

Citation frequency is something of a lottery: language of publication; discipline (since 1953 Crick and Watson have achieved three times Vine and Matthews's average citations); date of publication (E. Komatsu of the University of Texas at Austin has already had 1939 citations for his February 2011 paper 'Seven-Year Wilkinson Microwave Anisotropy Probe Observations: Cosmological Interpretations' published in a supplement to the *Astrophysical Journal*; nine times the rate of Crick and Watson, but the paper *is,* ostensibly, about the origin of everything)

Interestingly, the December 2012 issue of *Geology* presents stats on the most cited papers that it has published since 2000 (Cowie, P.A. 2012. Highly cited *Geology* papers (2000-2010) – What were they and who wrote them? *Geology*, v. **40**, p. 1147-1148; DOI: 10.1130/focus122012.1). *Geology* is among the highest ranking journals in the geoscience field, and had an impact factor of 4.8 over the last 5 years. A journal's impact factor is the number of times all articles published in a 2-year period were cited in all indexed journals in the year following them, divided by the total number of articles published in the two years by the assessed journal. So, papers published in *Geology* between 2007 and 2011 were cited on average 4.8 times in the year following publication. This journal is a useful source of citation statistics as it covers the full range of geoscience and all papers are limited to 4 printed pages, thereby forcing authors to be concise and clear in their writing and illustration. Consequently it is popular, which, incidentally, may explain its high impact factor.

Of the 33 papers cited most between 2000 and 2010, 14 are on topics relating to Tibet and China. There are 3 on oceanography; 3 on palaeontology and extinctions; 6 on palaeoclimatology; 10 on tectonics and 10 on magmatism (3 of which were about rare adakites formed by partial melting of subducted oceanic crust). I haven't read all of the papers, and the stats on topics may tell us very little, but I would bet that papers about geology in high-population emerging countries – China, India and Brazil – are met gleefully by their rapidly growing communities of eager young geoscientists. It may even be worth a flutter on adakites as the 'next big thing' in petrogenesis. Mind you, it looks like I am not likely to be the best punter for hot papers, as out of the 33 'top-3' papers since 2000, only 6 made it into *Earth Pages*, and of those only one between 2004-2010.

The digest goes on to show that year-by-year as many as 10 % of papers in *Geology* are not cited at all, up to 70% are cited between 1 and 5 times per year, while less than 10% get 10 or more citations in a year. Oddly, the author suggests that a dip in citations of *Geology* papers in recent years may reflect the launch of *Nature Geoscience* in 2008. Yet glossy as that new addition to the *Nature* stable might be, it has become something of a desert for papers on geology. Then there is evidence for both 'vintage' and 'just-about-drinkable' years in *Geology* citations: the 'top ten' papers in 2001, 2005, 2006, 2008 and 2010 ranged from 10-15 citations for the tenth to 20-25 for the 'hottest' paper, while in 2000, 2002, 2003, 2004, 2007 and 2009 the most cited papers stood well above the rest at 32 to 55 citations per year. But that may just reflect the uneven pace at which well-received and provocative work emerges.

So, it begins to seem, from *Geology* at least, that for most geoscience authors publishing isn't going to raise much hope as far as jobs or promotions are concerned. Yet if results are not published funding agencies may become fractious about your next grant application, and of course, university science departments puff themselves with annual publication rates (though rarely citation records, which as far as geosciences goes could be a wise move). But it is a matter of academic duty to publish for the record; even if a paper fills just one tiny niche, the cumulative effect of publically available knowledge does eventually result in breaks through – one never knows… It could be a salutary lesson should publishers release data on hits for on-line PDFs of papers, as that would give some indication of how many readers individual papers have, but as for a 'like this' button or a means of star rating I think we have to venture into the deeper recesses of academic conservatism one small step at a time.

Related articles

Is the Relationship Between Journal Impact Factors and Article Citations Growing Weaker? and How Much of the Literature Goes Uncited? (scholarlykitchen.sspnet.org); Study reveals declining influence of high impact factor journals (eurekalert.org)