The ethics of editing scientific papers in Chinglish: Should the editor also be a critic?

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About the author

Dr Ross Blackwood was for 18 years a senior academic in engineering and earth sciences in universities in Australia, the UK and Sweden. Now a full-time publisher and editor, he specialises in editing science manuscripts, papers in translation (for peer-reviewed science journals) and, from time to time, science and mathematics books for mainstream publishers.

Introduction

The language of science writing is functional, formal, usually arcane and boring to readers who are not in the trade. Scientists commonly write up their work in a conventional sequence, often in the passive voice in the belief that this imposes authority, objectivity and 'truth'—or at least makes it sound as if they know what they are talking about. Editing scientific papers in English translation from another language is an exercise in shaping the text to meet these conventions. That is our bread and butter work. In discussing papers translated from Chinese languages here, however, my concern is not with the way the scientists report their work, or the English expression or grammar or spelling: the problem is in the scientific essence, and the level at which scientific work seems to be performed today in the People's Republic of China.

There are two problems with most of these papers, as I see it, which I will develop further. First, the unconvincing theoretical basis and the approach to scientific investigation: the philosophy of the scientific view of the world. Second is the guardianship of the standards of published scientific work. This is where I suggest that editors working on these translated papers can be useful—perhaps the most useful—critics of the scientific content. (The word 'critic' can unfortunately have a negative connotation, as though all criticism must be detraction, but I have used it here deliberately. Synonyms like 'analyst' or 'reviewer' or 'judge' don't have the sharpness or directness of 'critic'.) The ethical questions for critics in this sense are unresolved, though, and I will discuss that later.

Although it's unfashionable in Australia to criticise anything from or about China, a report by UNESCO (of which Australia is a member) said in 2010 that 'there remains a yawning gap between China and developed countries for [the National Innovative Development Index][1] … placing it far behind the leader in 32nd place out of the 34 countries surveyed [in 2006]' (UNESCO, 2010).

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1 The NIDI is a measure of a nation's capacity for S&T innovation. In 2006, China's NIDI was 20.94, just behind Brazil, Mexico, Russia, Turkey and Romania. Sweden led the field with 67.01

So what actually is all the scientific effort in China, and where is it going? It's hard to know. According to the State Council (PRC, 2006), China is roughly seven years into the 15-year National Medium- and Long-term Program for Science and Technology (S&T) Development (2006–2020) … yet, we are told by the New York-based Council on Foreign Relations: 'Some science and technology mega-projects have been seen as questionable trophy projects done for propaganda purposes with Chinese state-controlled media being filled with reports of Chinese achievements' (Bajoria, 2008).[2]

Whatever the truth is behind the political spin, both East and West, the content of scientific papers translated from Chinese languages seems to be hampered by the use of methodologies that emulate 'Western' science—about which more later—but that do not seem to be quite right. Possibly the very brief history of Western-style scientific activity in China has not yet allowed it to flourish.[3] Whatever the reason, the symptoms show up in many ways: in the tendency to 'prove' a theory from very slight evidence, for example, or in the confusion between correlation and causation, or in the tendency to reinforce a theory by emphasising only the supporting information, or in misconstruing measurement precision, or in arbitrarily discarding data that doesn't fit the theory, or in unwitting circular argument; and so on. Except for a few, these are misconceptions of what science *is*, what it *does* — they are not translation difficulties. Cases of extrapolating or interpolating slim data over immense spans of space and/or time are common. Here is an example:

Water resources were fast disappearing in an industrial region. Weather and water resource records for 1955–1964 were manipulated by a basic computer simulation program to predict water usage from 2000 to 2200 for government planning purposes. (A fuller version of this follows later, along with other examples.)

This is not to suggest in any way that we see anything but honest, earnest reporting of the scientific endeavour, and it certainly is not a reflection on individual authors, many of them in highly respected posts at major centres of learning and research in China. Not at all; but a difficulty clearly exists with much of the scientific endeavour that they report.

The problem is not a result of language difference — or not *just* that — because it turns up over and over, in paper after translated paper, often several times or in several categories in the one paper, but interestingly it's not found in papers on similar topics translated from other languages including, in my own recent experience, Greek, Italian, Japanese, Polish, Russian, Serbian, Swedish, Turkish …

Editors who see their task as improving the English expression in one of these translations from Chinese might well not recognise the symptoms of poor scientific methodology, especially if they edit this sort of document only now and then. Natural enough; but I will try to give persuasive reasons for the view that the problem is potentially a serious one in which we editors are very much involved. It is not as serious as war or famine or plague—let's not get carried away—but it is serious.

There are a lot of these papers: we are just scratching the surface with the tip of the iceberg, as a dear teacher of mine was fond of saying, all irony intended. They are not school or university assignments, which are teaching tools and have no weight beyond that; nor are they internal government reports meant for only a few readers. Their authors want to publish them in English in one or other reputable scientific journal because, at the moment, English is the *lingua franca* of written science communication (Tardy, 2004). Publication, though, is no longer restricted to esoteric printed journals consulted only by fellow specialists.

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2 Described by Lobe (2005) as 'the most influential foreign-policy think tank' in the USA.

3 The Chinese state began to rebuild S&T activity in 1978, two years after the death of Mao Zedong and the end of the Cultural Revolution

Most journal articles are available online for anyone to cite, quote, repeat, copy, Google; to use in teaching programs or as precedents for further research. Good or bad, when published the author's work officially exists. More importantly in China, perhaps, it can then be added to the statistics.

This suggests to me that before they are published manuscripts should be vetted far more rigorously than they are now—not to claim some undefined virtue of the native English speaker over the foreign writer but because, I emphasise, once they are published they become part of the overall body of work of scientists and thus part of the corpus of scientific knowledge itself, forever.

Editors of scientific papers probably see far more of them than reviewers or even scientists themselves do. We read them more carefully. We may even notice patterns emerging in the writing from certain institutions. If we suspect that the substance of a paper is wanting in any respect, it is surely our duty, before it gets into print and cyberspace, to break the glass and press the red button.

But what is the editor's ethical position in this? In fact, are ethics involved at all? If so, what are they, and how do they affect us?

Ethics

Ethics is what one should do, what I should do: the standards I set for myself. Telling other people what *they* should do is just moralising. ('Professional' ethics, as embodied in bioethics protocols or the ethics of editing theses, say, can be thought of as moralising in that sense.)

My ethical concern might run along these lines: 'Science is the search for a specific kind of truth. I can't find that truth here, or I think that the path this author is taking will not lead to that truth, or will even lead away from it and, if published, may well lead others away from it too. But if the author is satisfied with the results of his or her work and they have the imprimatur of the publisher and the peer reviewers, then should someone like me take their work apart simply because I have been exposed to the Western paradigm of science and scientific thought, and I see something in their work that is inappropriate or fallacious?'

In other words, I am faced with an ethical dilemma: should the abstract notion we call science dictate my actions, or should I consider the author's feelings, dignity, self-respect or the respect owed or given them by colleagues or staff or supervisors?

For me this is a minor dilemma; in fact I might not see it as a dilemma at all, since I don't know the author personally and am never likely to; but perhaps that is just self-interest, smugness at having noticed something, some flaw in the weave, that nobody else has. Is science 'bigger', more worthy, than the people who serve it? Should it be? Do we, does anyone, have some sort of duty to scientific knowledge in the way that a writer, say, has a duty to the language she writes in, or a musician the music he plays? Why?

That's part of what the question in the title of this paper is about. The second part is about …

Science in China vs the rest of the world

No scientist working on the Chinese mainland has won a Nobel Prize in any branch of science.[4] This is an embarrassment in China, and there is even a 'growing Nobel Prize mania' according to Cong (2004),[5] who also points out: 'A popular survey, conducted in 2000, listed

4 There have been five Chinese-born Nobel laureates in science since 1957, but all of them did their award-winning work in USA, not China. (By comparison, 205 US scientists were awarded Nobel Prizes in the same period. Denmark, whose population is roughly the same as Sydney's, has four Nobel laureates in science for that period.)

5 Cong Cao is a Chinese-born Associate Professor and Reader in Contemporary Chinese Studies at the University of Nottingham..

a Nobel Prize as among the most likely events in the next ten years in China'. Perhaps this should have happened before now. Cong describes ten 20th-century pioneering scientific discoveries in China that many in the worldwide science community believe worthy of Nobel Prizes (Cong, 2004). Even if most scientific work tends to plod rather than soar to such heights, we must consider it no less carefully.

The Chinese approach to developing its scientific prestige seems to be to swamp all opposition with great numbers of publications. A few years ago *The New York Times* noted that 'Chinese scientists … in the past decade quadrupled the number of scientific papers they published a year. Their 2007 total was second only to that of the United States' (LaFraniere, 2010). The article went on: 'Quantity is not quality,[6] and despite its huge investment, China still struggles in many areas of science and technology. … China's scientific culture of cronyism and mediocrity [is] often cited as its biggest impediment to scientific achievement' (LaFraniere, 2010).

'Cronyism and mediocrity': anathema to the Western scientific community, whose hostility towards ill-considered or mediocre or hasty scientific behaviour was highlighted in September, 2011, when faster-than-light subatomic particles were supposedly discovered by CERN.[7] Many eyebrows were raised; if CERN were right, the basis of modern theoretical physics would be destroyed. When it turned out that the measurement, not the universe, was wrong (faulty electrical wiring), the project directors had no alternative but to resign.

Western scientific research seeks to refute a theory, not reinforce it (e.g. Popper, 1972). No matter how much data supports a theory, it is not proof; yet a single observation that disagrees with the theory disproves it once and for all. Einstein's two relativity theories, the foundations of modern physics, will always be just that: theories. They have not been refuted in more than a century despite many attempts to do so—the CERN mishap being one—but the possibility that it this could happen is perfectly valid. After all, Einstein upended Newton's equally grand theories after 200-odd years. The furore raised by the CERN/OPERA debacle was all to do with the science community's disapproval of how the science was 'done' and the directors' hasty announcement before thoroughly checking their facts, *not* the possible destruction of a theory.

Refuting theories (or ideas, or hunches) is fundamental to the way of 'doing' modern science.[8] Carl Sagan quotes the American physicist Robert W. Wood as saying, 'The physicist has an idea. The more he thinks it through, the more sense it makes to him. He goes to the scientific literature, and the more he reads, the more promising the idea seems. Thus prepared, he devises an experiment to test the idea. The experiment is painstaking. Many possibilities are eliminated or taken into account; the accuracy of the measurement is refined. At the end of all this work, the experiment is completed and ... *the idea is shown to be worthless*. The physicist then discards the idea, frees his mind from the clutter of error, and moves on to something else' (Sagan, 1995).

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6 'Quality' here is assessed by ranking the 'research, employability, teaching and internationalisation' capabilities of universities and other institutions of higher learning (QS, 2011). There are 1983 universities and institutions in mainland China (Education Ministry of PRC, 2009). In 2011, six were ranked in the top 100 in the world in one or more of the areas of Physics and Astronomy, Metallurgy, Mathematics, Environmental Science, Earth and Marine Science, and Chemistry. Peking University was ranked in the top 50 in all areas (QS, 2011).

7 The CERN/OPERA project (Conseil Européen pour la Recherche Nucléaire / Oscillation Project with Emulsion-tRacking [sic] Apparatus) at the Large Hadron Collider—the gigantic particle accelerator straddling the French/Swiss border—in cooperation with the Laboratori Nazionali del Gran Sasso, Italy. The particles were muon neutrinos.

8 A stock example of the power of refutation: An ornithologist in Europe, after observing tens of thousands of white swans, concludes that all swans are white. The more swans she sees, the better the theory is looking. Then she comes to Perth, and bang goes the theory. (Europeans first saw black swans in Shark Bay in 1636.)

This is not what we find in the translated papers from China, though—or at least I have not found any evidence of it. The opposite, however, is everywhere. A mass of 'friendly' data is assembled and said to prove the author's case. A persistently recurring example is the reliance on mathematical models (computer models) that use vast amounts of numerical information gathered by satellites, with sometimes a few measurements on the ground thrown in to act as reference points; and even some of those are discarded if they don't agree with the computer model: 'the computer says naaah.' This is all armchair science. No theory or hypothesis is suggested; a string of observations that may (or may not) be connected is touted as 'proof' of something that isn't a theory. Discussion in the paper is then limited to: 'that proves what we knew we would prove'.

Absurd extrapolations are sometimes made. In the case mentioned earlier, 10 years' incomplete data from the 1950s and 60s was used to predict water usage in a growing urban region and the inevitable depletion of supply for the two centuries from 2000 to 2200. A few back-of-the-envelope sums—mine, not the author's—extrapolating the same trend 200 years backwards in time to the mid-1700s suggested that the region would all have been under several metres of water. There is no evidence that this was the case, naturally, and it is rather silly, but the approach seems just as valid as projecting so far into the future using imperfect, largely irrelevant data plugged into an imperfect model.

Apart from the unconvincing methodology, it is a truism to add that data gathered over 10 years in the lifetime of a planet four and a half billion years old, give or take 50-odd million years, is a very narrow window that lets in very little light. Such data is not predictive; it cannot tell us anything about either the future or the past; it gives us a rough idea of what happened in that ten-year period, and that's all it does—yet this sort of information is used time and again to fuel a computer model, trying to turn it into a crystal ball. It's doubtful that such work contributes anything useful to the state of scientific knowledge.

Simon and Cong's (2009) book *China's emerging technological edge* points out that 'China finds itself faced by a number of major challenges as to whether its full S&T potential may be realized. At the heart of these [are] uncertainties surrounding the quality, quantity, and effective utilization of China's S&T workforce.' They note, for example, that the open access journal *Acta Crystallographica Section E*[9] has published thousands of Chinese scientific papers. One of the book's authors, Cong Cao, found that 'even someone who knows nothing about crystallography could have his or her papers published in the journal' (quoted in Osnos, 2010). Little or no peer review seems to be required, and the papers, often only one page long, do not have to be in English; they are, in Simon and Cong's words, 'useless'. It is thought that 95,500 science papers were published in China in 2008, but nobody knows how many of those were published in zero-impact journals like *Acta Crystallographica Section E* (Simon and Cong, 2009).

China's push to become a 'science superpower' has been written about many times (e.g. Focus, 2012; Hollingsworth et al., 2008; Osnos, 2010). If scientific supremacy over the West, embodied as always by the USA, can't be gained through the quality of the work, then it must be done by the sheer overwhelming numbers of projects and papers, in the belief, it seems, that producing more and more mediocre work is somehow improving the quality rather than dumbing it down. Science and scientific thought—not the USA—could be the losers. Let's hope not … but when we plunge into one of these papers to edit it, perhaps we will look at it in a more critical light than before.

There is a persistent naïveté around some aspects of the approach to quantitative research. The following are recent examples of dubious extrapolation and measurement precision, which I have chosen because measurements don't depend on language and can't be mistranslated.

9 A journal of the International Union of Crystallography, published by Wiley Online Library.

Out of context, of course, the question of whether heights or areas or weights really were measured to some particular precision might be trivial ('Oh well, just round the numbers off'). Sometimes, though, quite elaborate conclusions are drawn from such numbers. They are treated as if they were actual measured quantities and used as input data for computer models to form predictions of various kinds—but these numbers were themselves derived from an earlier computer model (or from satellite imagery, GIS, GPS and so on, which amounts to much the same thing).

Here are some actual examples containing some of the things that I suggest we should look out for:

Scope: Find how much firewood is used in small traditional farmhouses in Tibet.

Method: Investigate fuel use in seven houses over a few weeks. Weigh the firewood to one decimal place.

Analysis: Report the average weight of firewood to three decimal places, then extrapolate this to cover all of Tibet.

Conclusions: 1. Houses use more fuel in cold weather than in warm weather (!) 2. The average weight of firewood used by households in small traditional houses throughout all of Tibet is 14.327 kilograms (precision: half a gram).

Comment: Adequate for a school exercise, but is it scientific research at a level expected of the international science community?

Scope: Investigate the area damaged by landslides following a major earthquake in China. The landslides kept occurring for some months after the earthquake.

Method: Literature search and satellite imagery.

Result: Damage area was 1989.44 square kilometres (precision: 0.0003%).

Comment: Suggest that reporting the area damaged by landslides to two decimal places is unrealistic, since the area would change daily, or hourly, as new landslides occurred. Even rounding off to the nearest 10 square kilometres (i.e. to within 0.5%) would strain the limits of measurement capability, especially when the data comes indirectly from satellite images.

Scope: Find the volume of loose material resulting from large-scale rock mass collapse continuing for some months after a major earthquake in China.

Method: Literature search and satellite imagery.

Result: Volume was 2,753,700,000 cubic metres.

Comment: Doubtful that measurement to within 0.002% is meaningful or even possible by these methods and in these circumstances.

Scope: Find the maximum subsidence over an area of 485,000 square kilometres following a major earthquake in China.

Method: GPS data. No land surveying or other measurements.

Result: Maximum subsidence in this area was 675 mm (precision: half a millimetre).

Comment: Unlikely precision for an area this size, especially if relying only on GPS data.

The language of Western science

It's facile, I know, to criticise some Chinese science as not being 'good' science—or not good Western science. The empirical, evidence- and number-oriented science of Galileo and Newton, maybe even Einstein and Bohr and Hawking, has been drummed into us throughout our education, and forms our way of explaining the physical world, the universe and everything else. Obviously it's got a long way to go, and in truth we understand almost nothing about the physical world; but that's another story.

Chinese scientists have been told to modernise. They must catch up with, compete with and overtake the West, and the sooner the better. They are to become cogs in a new Chinese science superpower machine, and they must aim for Nobel Prizes in 'the race to Stockholm' (Cong, 2004). The men and women working in S&T easily imitate Western science and use its techniques and recipes without needing or wanting to understand its roots or its philosophy. But when they write-up their work, their words sometimes betray a misunderstanding of the significance, let alone the philosophy, of why the science they are imitating is done in particular ways.

The main ones to look out for are *proof* (or *prove*), *correlate*, and *cause* (or *causation*, or *because*).

Rather than 'prove', a scientist in the Western tradition would automatically write something like indicate, imply or suggest, because nothing in science can be proved except in some branches of mathematics. Part of the confusion probably springs from the very great difference in the structures of the languages, or the quality of the translation, or both. 'Proof' may merely be a synonym for the meaning in Mandarin or Cantonese, say, of 'evidence' or 'verification', used by a translator who doesn't necessarily realise the power of 'prove' and 'proof' in scientific English. If the translator is a computer program, the whole translation will be pretty well incomprehensible and 'proof' is the least of the editor's worries.

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Correlation is not causation; it is a relationship that seems to link something with something else. '[Scientists] are always operating within a complex framework of theories, and do not aim simply at correlations, but at explanations' (Popper, 1972). This distinction is often ignored or glossed over in the translated manuscripts.

Typically, long lists of numbers (measurements of length, albedo, reflectance, adsorptive capacity, cationic concentration … you name it) are correlated by devising an elaborate mathematical function that roughly joins the dots when plotted on a graph. The mathematical function itself is then said to 'explain' the measurements, and is programmed into a computer model to predict other results. The causal connection may be true, of course, but equally it may not. Unless it is demonstrated to be true, it has no more value than the correlation between dogs, say, that drink water and that have four legs.

The question arises again: what should an editor do if she recognises these sorts of things? It is easiest simply to change the wording from 'this is proof that …' and so on to 'this suggests/indicates/implies etc. that …'. But if she suspects that there is some fundamental misunderstanding, or even manipulation of data to suit certain needs, then it seems to me that she has an ethical duty to pursue it, as I've said already. But how?

Possible options may be to comment on the offending text and suggest that the work be downgraded from a full article to a technical note or blog, but doing this might not be easy. In practice, who is the best contact? the author? the author's institution? the author's research grant sponsor? the journal editor assigned to that project?—and in any case, why should they take notice of an editor who, with the greatest respect, they have never heard of?

Most 'serious' scientific work is published in English in one of the thousands of peer-reviewed journals (in print and/or online) that cover all areas of science from astronomy (more than 100 titles) to zoology (more than 50), and in non-specialist journals such as *Nature and Science*.[10]

This brings us to the last word in the title …

Criticism and peer review

'Everyone who writes can be a critic of writing. Everyone can take some responsibility for the language' (Watson, 2003, p. 179). Agreed, but … doesn't the journal peer review process take care of that? I suggest not. Peer reviewers are not critics; they are prospectors testing the ground for particular things.

It is not usually up to an editor to check calculations from a complex formula, or to locate the original data to make sure that it has been accurately reported and not manipulated to arrive at a predetermined result. That, ideally, is part of the peer review process (e.g. Brown, 2004).

Journals send manuscripts to two or more independent peer reviewers in the hope of ensuring the work is up to the journal's standard. In practice, the reviewers can't always give absolute assurance for papers in the 'hard' sciences, which don't usually deal with matters of opinion or debate, but with (often numerical) facts and the interpretation of them. No reviewer can testify to the truth of the reported results in a paper describing a long-term, complex satellite-based project, for example, unless they go through the whole thing, step by step, with the author. This could take months; it isn't going to happen. An academic based in Perth, say, reviewing a paper on astronomy from Tsinghua University, can only assess the logic, consistency or replicability of the work from the published literature covering previous work on the topic, and her own knowledge.

Then, when the author has made the corrections and the reviewers have recommended publication, the manuscript goes back to the editor for a final copy edit: the editor, as they say on TV, is the last person to see it alive! Is it likely, even after all the work on it by others, that we would see something that looks not quite right? I think it is. If the subject is familiar, or we have edited papers on similar topics—then yes, I believe it is quite possible we could be troubled, or delighted, by something in the paper that the reviewers hadn't spotted, because it was not what they were looking for.

Which takes us back to the beginning, with my questions still unanswered.

Should editors merely make cosmetic adjustments to a manuscript before sending it off to cyberspace to be enshrined, if you like, in the scientific canon? Or (back on Earth) before yet another questionable paper is added in the Chinese numbers game that is being played in their quest for dominance of science?

Either way, the question remains: is it not our ethical duty as editors to criticise, not merely comment on, the author's scientific approach and the way the work is done?

10 As an indication of the vast literature in science, 10 years ago there were 11,370 active refereed science journals in all languages, of which some 8500 were listed as 'peer-reviewed' (Brown, 2004, p. 12: compilation from *Ulrich's Periodicals Directory*). These figures are probably roughly the same in 2013.

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