*Commentary***Zero tolerance to plagiarism will kill inspiration: a critique for academic meritocracy****Hans Henrik Bruun**

Hans Henrik Bruun (hhbruun@bio.ku.dk), Department of Biology, University of Copenhagen, Universitetsparken 15, 2100 Copenhagen Ø, Denmark

Abstract

Meritocratic justice is important, but not everything that matters. The long-term advancement of science depends heavily on scientists' willingness to generously share their ideas. Only ideas with little meaning to others are truly private.

Keywords: Inspiration, scientific misconduct, reference trawling.

“Credit should be given to the original work” goes a central tenet in scientific citation practice and in the idea of academic meritocracy. Plagiarism—the unacknowledged use of someone else’s work—is a form of scientific fraud. Authors of scientific papers who consciously present their ideas or findings as novel and original when, in reality, they have been published before by others, are no better than researchers who fabricate or falsify data to create sensation and boost their careers. This quest for primacy is what John Lawton almost a quarter of a century ago called the “tyranny of now”, i.e. that “grants, jobs, tenure, come from being first, or rather from the illusion of being first” (Lawton 1991). Fraud clearly is immoral and is devastating to public trust in science and to the openness that is vital to science.

Is then the unconscious or subconscious failure to cite the original a fraud? Is it plagiarism? A decade ago, Simon Leather (2004) expressed his irritation that authors do glaringly poor efforts in identifying work by other scientists having published the same result or idea. “Shallow reference trawling” he called it. The reason for scholars to get annoyed by this phenomenon hasn’t decreased since then. Ignorance of previous discoveries

and of ideas put forward in the past leads to ‘reinventing the wheel’ over and over again. “The more one ignores the past, the more one can become intoxicated by ephemeral novelty” (Keddy 2005: 145). This puts a curb on the advancement of science. For these, and perhaps more, reasons, zero tolerance to plagiarism in science has been strongly advocated by professional societies (e.g. IASTED 2013), journal editors (e.g. Tandon and Roberts-Thomson 2002, IJoAT 2013) and researchers (e.g. Sovacool (2005) and colleagues of mine in conversation).

I would no more than anybody else like to see my own work copied and presented by somebody else as her or his conception. And I shall refrain completely from defending plagiarism as such. However, zero tolerance to plagiarism raises three questions in my mind: 1) how much reference trawling is enough? 2) What is true scientific originality? 3) Can ideas be subject to ownership? Set in the context of my own scientific discipline, ecology, here are my thoughts.

1) It is little surprising if some authors, perhaps guided by their subconscious, soon stop grubbing through the literature for previous works. Too many references to sources of inspiration may disprove the ‘full originality’ of the work and effectively prevent publication in a top-tier journal. The reward for not citing such work may be high, as it opens for claims of novelty. The maximal risk is to hit one’s head on a grumpy, old-fashioned scholar in the shape of a referee or an editor, who rejects the manuscript. In that case the dodger can just try her or his luck elsewhere. Meritocratic parasitism is obviously bad for meritocratic justice. David Wilkinson’s (1993, 1999) mapping of the origins of two now well-established ideas, the intermediate disturbance hypothesis, and the equilibrium theory of island biogeography, provide examples that

style and marketing matter, and that shallow reference trawling pays off in terms of citations and scientific honour. On the other hand, I will claim that by extending the argument of deep reference trawling, we will turn ourselves into archaeologists in the literature, trying to always find the earliest primordial version of any one idea. Ever-higher demands on deep reference trawling would effectively halt innovative and clear ideas. In my humble opinion, too many taxonomists are engaged in similar unproductive searches through obscure literature for species diagnoses that, due to greater age, have priority over names long in use, and will make the taxonomist's claim to fame.

2) What is the origin of radically new ideas? "Sometimes the most significant questions in a science, those that lead to the establishment of an entirely new theory, will arise from outside a recognized theory. Perhaps the most revolutionary of such questions arise from outside any theory" (Pickett et al. 1994). Radically new ideas go against accepted interpretations, so they are routinely rejected by the scientific community. At best, they make turmoil, but they are much more likely to be completely neglected by all but a few visionaries. Not until a small, but critical mass of scientists have accepted the idea, it will gain momentum and be more widely accepted—at least sufficiently accepted to cause controversy. Such an *avant garde* of specialists may be immensely important in disseminating an idea they did not themselves conceive. Perhaps, they express the idea more clearly than the original. The acts of such apostles are often much more cited than their source—seminal papers that disseminate ideas conceived by others. One may claim that such work speeds up a development that would have come anyway. Without further analogy, it's a bit like Bill Gates, who is often seen as one who skimmed the cream off the computer revolution without contributing more than a good deal of salesmanship. Thus, how can one ever be sure that a quoted paper is really the original paper? Aren't we much more likely to cite famous papers, which cite the original, than the original itself? Once a paper has become well cited, it is likely to become highly cited (for one example—see Miller et al. 2005). That is the herd instinct of scientists, who are—after all—human. A much greater threat to the advancement of science than plagiarism comes from a different angle. Numerous are the papers in scientific journals that, with full fanfare and reference to the 'original' work, but without much trace of imagination or focus, report studies reproducing previous findings in a new setting (Sand-Jensen 2007). Equally numerous are studies striving to corroborate paradigms, again giving full credit to the constructors of it, instead of testing the (implicit) assumptions of paradigms (Austin 1998).

3) Claiming ownership of ideas is egocentric. Advancement of science is a collective process. The scientist obsessed with his or her ownership to a theory was taken completely off in a TV sketch by Monty Python's Flying Circus (Episode 31, 1972). The interviewed scientist, Anne Elk, makes a big and long fuss about the theory being hers and, when she finally gets to presenting it, it goes like this: "*All brontosaurus are thin at one end, much thicker in the middle and then thin again at the far end. That is my theory, it is mine, and belongs to me and I own it, and what it is too.*" This comedy sketch has inspired the notion of an "Elk Theory", for an insipid theory presented by a scientist whose real concern is ownership and credit (Thomason 1996). Meritocracy motivates the individual scientist to do his or her best, to be ambitious, and to strive for acknowledgement from peers. Pushed to extremes, however, meritocracy demands ownership to ideas. Scientific advancement, on the other hand, requires exchange of ideas and depends on speculation and inspiration. Inspiration comes out of the invisible. As noted by Popper (1935), there is no logical process that can lead the scientist to new ideas. Discovery depends on an element of creative intuition. Although the process of inspiration is irrational, its outcome is by no means random. A novel idea is good because it fits into an existing rational theoretical framework. We should certainly not exclude inspiration from sources beyond identification as author name and year of a publication. Long live inspiration!

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Referees

Simon R. Leather – simonleather@harper-adams.ac.uk
Harper Adams University

David M. Wilkinson – D.M.Wilkinson@ljmu.ac.uk
Liverpool John Moores University

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Response to referees

I thank both Wilkinson (2014) and Leather (2014) for their very interesting responses. Moreover, I take the fact that both responses present such stimulating reflection as corroboration of a central tenet in my text—though I claim no ownership to the idea—namely that the most valuable ideas one may conceive, are those which spark further thoughts in other persons' minds. To present a new idea to somebody else, who is willing to contemplate it and share the thoughts it provokes in

his or her mind, is—for me—a good way to sort promising yet half-baked ideas from blind-alley ideas.

It seems that Leather (2014) does not fully appreciate the considerations I presented about unconscious failure to cite previous work appropriately. When one is trying to keep pace with the flood of the scientific literature, sometimes a concept or an idea gets stored in the convolutions of the brain without the conventional author + year tag attached. If it later resurfaces, it may well be perceived as one's very own conception. This is known as *cryptomnesia* (Merton 1963). I must admit to have suffered from this phenomenon, when some years ago I thought to have conceived the brilliant idea of using hemiparasitic plants as a kind of ecosystem engineers in grassland restoration, only to discover in my personal reference database a paper presenting that exact idea (Pywell et al. 2004), which meant that I had read at least the title and abstract earlier and then forgot about it. But, punishment for unconscious failure to cite predecessors was not my point. It was that the pressure on scientists to be 'novel' creates an incentive structure against searching the literature for previous publications of an idea, and instead presenting the idea as one's own.

I thank, somewhat relieved, Wilkinson and Leather for not just saying, 'yes, well, you may be right, but what solution do you propose?' Because no simple solution exists. My main point is that intellectual ownership to ideas by definition is in conflict with the free exchange of ideas, i.e. what I have called *inspiration*. Just like de Toqueville (1835) defined democracy as a balance between two opposed principles (majority rule and individual rights), intellectual ownership and inspiration are diametrically opposed principles, which are both important and therefore must be traded off against each other. Optimization of just one of the two, of which 'zero tolerance to plagiarism' provides an example, is an extremist viewpoint. To me, plagiarism is abuse of the fundamental freedom, which free exchange of ideas grants publishing academics. Obviously, there must be mechanisms to dissuade abuse, but such mechanisms should not hamper freedom and inspiration. Wilkinson (2014), in his review, briefly touches upon the question of what is good for science as such in the *long term*. Clearly, intellectual property rights are important on the time scale of an academic career, whereas inspiration is pivotal to the continued development of science—that is, on a much longer time scale. I feel that the focus on academic merits have increased appreciably during my university life (PhD 1999) and I fear that the long-term greater good of scientific development has become lost somewhat out of sight.

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