



## Editorial

# Whither research integrity? Plagiarism, self-plagiarism and coercive citation in an age of research assessment

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## ABSTRACT

This extended editorial asks whether peer-review is continuing to operate effectively in policing research misconduct in the academic world. It explores the mounting problems encountered by editors of journals such as *Research Policy* (RP) in dealing with research misconduct. Misconduct can take a variety of forms. Among the most serious are plagiarism and data fabrication or falsification, although fortunately these still seem to be relatively rare. More common are problems involving redundant publication and self-plagiarism, where the boundary between acceptable behaviour (attempting to exploit the results of one's research as fully and widely as possible) and unacceptable behaviour (in particular, misleading the reader as to the originality of one's publications) is rather indistinct and open to interpretation. With the aid of a number of case-studies, this editorial tries to set out clearly where RP Editors regard that boundary as lying. It also notes with concern a new form of misconduct among certain journal editors, who attempt to engineer an increase in their journal's 'impact factor' through a practice of 'coercive citation'. Such problems with research integrity would appear to be unintended, and certainly undesirable, consequences of the growing trend to quantify research performance through various indicators.

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## 1. Introduction

The academic community has long cherished a belief in the effectiveness of self-policing in maintaining the norms of research integrity in 'the Republic of Science' (Polanyi, 1962; see also Martin, 2012). Other professional communities have operated on a similar basis, at least until recently – for example, accountants, doctors, journalists and politicians. However, a succession of scandals over the last decade or so has challenged our assumptions about the efficacy of self-policing in these communities, often resulting in the imposition of new regulations, procedures and oversight bodies.

Where do matters currently stand in the academic community? Are peer-review and other self-policing mechanisms still succeeding in keeping research misconduct at bay? Many would sincerely hope so. After all, the academic profession supposedly has strong, clear norms about research integrity and ethical behaviour. Academics go through a long period of apprenticeship, during which these norms are meant to be firmly inculcated. In addition, the ongoing processes for the judgement of papers, research proposals and promotions through peer review surely afford ample opportunities for miscreants to be caught and their behaviour corrected. Moreover, not only are the risks of being caught high (or so it is generally assumed), but there are also severe sanctions available to impose on those found guilty, including the rejection of papers submitted for publication, the retraction of articles already published, the revoking of research funds, and even the dismissal of individuals in the more extreme cases, not to mention the stigma of moral opprobrium for those caught transgressing.

In the light of all this, there are doubtless many academics who continue to believe that research misconduct is rare and generally small scale, that it is easily detected, investigated and judged, and that it is kept firmly in check by the vigilance of peer-review and the severity of the sanctions that can be imposed (Martin, 2012). While recognising that some research misconduct unfortunately does take place, most scholars seem to blithely assume that it is confined to junior researchers (who have not yet completed the process of socialisation and inculcation of norms) and to researchers in 'other' countries where perhaps there are different conventions regarding what constitutes appropriate and inappropriate research conduct.

Yet we know the pressures of academic competition are rising, whether for tenure, research funds, promotion or status, which may mean that more researchers are tempted to cut corners (Brice and Bligh, 2004; Casadevall and Fang, 2012). The use of performance indicators based on publications, citations, impact factors and the like may also be adding to the temptation to stray from previous conventions regarding what constitutes appropriate research behaviour or to attempt to surreptitiously 'stretch' the boundary between appropriate and inappropriate behaviour. Are the existing checks and balances operating in academia managing to restrain these temptations? Or is some reinforcement of those mechanisms required?

There are worrying signs that research misconduct is on the increase. The number of retractions of published papers by journals has increased more than 10-fold in a single decade – from around 30 a year in the early 2000s to some 400 in 2011 (Van Noorden, 2011; see also Steen, 2011; Zimmer, 2012). Moreover, the

majority of retractions are seemingly the consequence of research misconduct rather than simple error (Fang et al., 2012). Furthermore, top journals are not immune, with *Proceedings of the National Academy of Sciences* making no less than eight retractions in 2011 and *Science* five.<sup>1</sup> Indeed, Fang and Casadevall (2011, p. 3855) find that the frequency of retraction “shows a strong correlation with the journal impact factor”.<sup>2</sup> According to a recent meta-review, while only 2% of authors confessed to having fabricated or falsified data, 34% admitted to “questionable research practices”, while 14% knew of colleagues who had fabricated data,<sup>3</sup> and no less than 72% were aware of questionable research practices among their colleagues (Fanelli, 2009).<sup>4</sup> With regard to the particular problem of self-plagiarism and related activities described below, the number of academic articles referring to ‘self-plagiarism’, ‘salami publishing’, ‘redundant publication’ or ‘duplicate publication’ has risen nearly five-fold from 170 in 2000 to 820 in 2012.<sup>5</sup> More and more editorials<sup>6</sup> are appearing in which journal editors complain about the growing burden being imposed on them as they attempt to detect, evaluate and sanction research misconduct in its various forms.

This extended editorial has several purposes. The first is to report the experiences of one journal, namely *Research Policy*, over the last three years, exploring whether the assumptions outlined above about the supposed efficacy of peer-review and self-policing in keeping research misconduct in check do indeed stand up to scrutiny. A second aim is to ‘draw a line in the sand’ – to set out clearly and explicitly, with the aid of specific examples, what the Editors of this journal, at least, regard as the boundary between appropriate and inappropriate research behaviour so that there is minimum ambiguity to be exploited by less scrupulous authors. A third objective is to consider whether the growing tendency to quantify research performance and subject it to assessment based on indicators may be having a deleterious impact on the research behaviour, integrity and morality of researchers. Lastly, the attention of the wider academic community is drawn to a worrisome tendency among the editors of certain journals to engage in a practice of ‘coercive citation’ to bolster the impact factor of their journal. Editors who resort to such underhand practices will inevitably be perceived as lacking the moral authority necessary to discipline other authors engaging in misconduct.

In Section 2, we set out the background to these problems. Section 3 describes the experiences of *Research Policy* with cases of plagiarism since the notorious Gottinger saga in 2007 (Martin et al., 2007), while Section 4 deals with problems relating to ‘salami pub-

lishing’, redundant publication and self-plagiarism. Possible factors underlying such instances of misconduct are analysed in Section 5. Section 6 shifts the focus from misconduct by authors to that by journal editors intent on raising their journal impact factor through fair means or foul. The final section summarises the conclusions to emerge from this examination of the problems of research misconduct.

## 2. Background

Prior to 2007, *Research Policy* Editors came across only the occasional problem of research misconduct. Where such problems did arise, they were mostly handled very discreetly through confidential discussions between *RP* Editors and authors. Only rarely did the issue become public.<sup>7</sup> All this changed in 2007, when a PhD student<sup>8</sup> alerted the Editors to the fact that *RP* had published a paper in 1993 that was based almost entirely on plagiarism of a 1980 article in another journal. This quickly proved to be the tip of an enormous iceberg, with the individual involved having made up institutional affiliations for over 20 years as well as engaging in a dozen or more acts of blatant plagiarism over his 30-year career. Because there was no suitable institutional employer to conduct the investigation, this was carried out by *RP* Editors in conjunction with the journal *Nature*, with the findings being published in August 2007 (Martin et al., 2007; Abbott, 2007). However, somewhat to our surprise and consternation, that was not the end of the matter, with the culprit continuing in his misdeeds for several more years (Abbott, 2008; Martin, 2012).

Following that, a number of other, lesser cases of research misconduct were brought to our attention over the following year. In a 2009 *RP* Editorial (Martin et al., 2009), we therefore included a section specifically on ‘avoiding research misconduct’, and warned that *RP* was taking matters of research integrity very seriously. We identified different types or levels of misconduct ranging from ‘salami publishing’ to self-plagiarism to outright plagiarism, and set out clear guidelines and rules. In particular, we stated that, if authors were in any doubt about such issues as what constituted ‘prior publication’, they should ask the *RP* Editor involved for advice or a ruling. We also reminded referees and other readers that, if they have reasonable suspicions about the integrity of a paper, they have a responsibility to draw these to the attention of the journal editors or other relevant authorities.

What has happened since? Is the academic community, through the collective efforts of referees, editors and others, managing to keep research misconduct in check? Or are the problems continuing to escalate? As we shall see, it is gratifying that an increasing number of referees are willing to draw instances of misconduct to the attention of *RP* Editors. However, a few authors have apparently not heeded the advice and warnings given – hence, this editorial.

## 3. Plagiarism

Let us look first at the more serious offence of plagiarism, an offence that is also simpler to define than self-plagiarism (which is dealt with in Section 4). Plagiarism can be defined as “the copying

<sup>1</sup> See <http://retractionwatch.wordpress.com/2011/12/30/the-year-of-the-retraction-a-look-back-at-2011/> (accessed on 11 February 2013). Retraction Watch provides a reasonably comprehensive blog where the latest cases of retraction are reported and discussed.

<sup>2</sup> At first sight, this might be seen as implying that high-impact journals are afflicted to a greater extent by research misconduct. However, a more plausible explanation is that such journals are more likely to have an explicit research misconduct policy and associated procedures in place (Resnik et al., 2009, 2010).

<sup>3</sup> Similarly, a recent survey of several thousand medical researchers found that “13% of these researchers admitted knowledge of colleagues ‘inappropriately adjusting, excluding, altering, or fabricating data’ for the purpose of publication” (Tavare, 2012).

<sup>4</sup> In a survey of economists, Feld et al. (2012) found rather higher figures, with serious research misconduct (e.g. data fabrication or plagiarism) being reported as being present in 4% of all research, and no less than 60% of individuals engaging in “questionable research practices”. Similarly, a survey of the management departments of American business schools by Bedeian et al. (2010, p. 719) revealed that 86% of respondents reported knowledge of faculty who had “published the same data or results in two or more publications”. And in Australia, Bretag and Carapiet (2007) found that 60% of a random sample of social scientists and humanities scholars had committed self-plagiarism in one or more of their publications.

<sup>5</sup> Search on Google Scholar using these terms and conducted on 8 February 2013.

<sup>6</sup> See Robinson (2012, p. 2), who cites a large number of examples, as well as the bibliography at the end of the current article.

<sup>7</sup> One exception involved an allegation that the authors of an *RP* paper, in using the concept of ‘innovative capacity’, had failed to cite what was claimed to be the paper that was the original source of this concept. However, thorough investigation by *RP* Editors (not once but on two separate occasions) revealed that the concept had been used on many previous occasions, and there was no particular reason to assume that it should be credited uniquely to a single author or publication (Martin, 2010, p. 1445, footnote 21; see also Prosser, 2010).

<sup>8</sup> The PhD student chose to remain anonymous, fearful that the act of ‘whistle-blowing’ could have adverse consequences for her/him personally – a sad reflection of the situation in which researchers now operate.

of ideas, data or text (or various combinations of the three) without permission or acknowledgment".<sup>9</sup> It is important to stress that this definition explicitly includes the copying of ideas – in other words, it is not confined merely to the copying of text. Hence, those who tout the availability of plagiarism detection software as a technological fix to the plagiarism problem (or the self-plagiarism problem described below) are missing the point since plagiarism can take other forms that cannot be detected by such software.

Fortunately, the incidence of the most blatant form of plagiarism – namely, that of an entire paper – appears to be rare, at least with regard to submissions to a leading journal like *Research Policy*. Even more fortunately, the instances we have encountered in the last few years seem to have involved authors blessed with a singular degree of stupidity or laziness,<sup>10</sup> who are either unaware that merely substituting their name at the top of an existing paper is unlikely to fool anyone, or unwilling to put in the effort needed to camouflage their plagiarism in a more sophisticated manner.

Rather more common is the plagiarism not of a whole paper but of substantial sections (typically involving hundreds of words) from one or more other articles.<sup>11</sup> Typical of this was a paper submitted to *Research Policy* by a doctoral student at a Middle Eastern university. We brought the matter to the attention of his supervisor and his head of department, but regrettably neither felt it necessary to respond about what further action, if any, they had taken. A second such case involved a student from another Middle Eastern country, who submitted a paper apparently co-authored with his professor and two other academic colleagues. Curiously (and certainly tempting fate), he submitted the same paper twice to two different *RP* Editors within two days of each other. A check revealed 500 words had been lifted from one paper, and another 500 words or so from three or four other sources. The paper was summarily rejected, and the co-authors duly informed. Again, however, no response was forthcoming from the senior co-authors.

One instance of plagiarism was detected by accident.<sup>12</sup> The editor of another journal had been annoyed to discover, shortly after publishing an article in 2009, that a very similar article<sup>13</sup> by the same author had appeared at about the same time in another journal, with no cross-referencing to that parallel paper (see the resulting editorial by Weingart, 2009). On checking our records, we found that this European author had just submitted a paper to *Research Policy*. While there was no apparent evidence of self-plagiarism, it quickly became evident that some 1000 words had been lifted almost word-for-word from an article published (by

coincidence) by one of my colleagues at SPRU (at the University of Sussex). When confronted with this, the author gave as his defence that he had in fact cited the SPRU article at the beginning and again at the end of the 1000 words in question, as if this was sufficient to justify the lifting of such a substantial body of text. The paper was duly rejected.<sup>14,15</sup>

Another, smaller-scale instance of plagiarism was identified by a referee in a paper by an assistant professor at a North American university, in which the methodology section reproduced a significant amount of text that had appeared in an earlier report co-authored (by chance) by the referee. However, that was not the only problem. The study drew upon a database originally constructed by that referee, but made no acknowledgement of the earlier project nor did it cite the principal investigators involved. Furthermore, the funding source for the original project had not been acknowledged (as required by that funding agency). Close scrutiny of two further papers submitted by the same author revealed other instances of plagiarism but also of self-plagiarism (manifest in a failure to cite the existence of parallel papers by the same author – see Section 4 below). The three papers by this author were all rejected, as was a fourth that the author had recently submitted to *RP*. Besides being informed that no further papers would be considered by *Research Policy* for the following three years, the author was also notified that the details of this exceptional case would remain on file at *RP* over that period, with the implication that if, during this time, we were to hear from another journal editor of similar problems with other papers by this individual, we retained the right to make available to that editor the full facts of this case.

More recently, suspicions were aroused at *Research Policy* when an Asian lecturer submitted two papers to two different *RP* Editors within a few weeks of each other. These were on similar topics, but neither cited the other, nor was there any covering letter drawing the attention of the two Editors to the existence of the parallel paper. Analysis showed that one contained over 900 words of text almost identical to that in an article in *Journal of the Academy of Marketing Science* by three other authors, while in the other paper some 1400 words of text had apparently been lifted from another article by different authors in that same journal. In addition, both papers were found to contain substantial elements (1200 words in one case, and 700 words in the latter) of text from various other papers by the Asian author, none of which were cited in the *RP* papers – in other words, they both also contained 'self-plagiarism' (see next section). Both papers were summarily rejected, and the author was urged to seek advice from senior colleagues as to what constitutes plagiarism and self-plagiarism, and how he might avoid such misconduct in his future work.<sup>16</sup>

#### 4. Salami-publishing, redundant publication and self-plagiarism<sup>17</sup>

##### 4.1. Definitions

There are a number of related but somewhat different forms of research misconduct or misdemeanour involved here, so we

<sup>9</sup> This is the definition used by Elsevier journals (<http://www.elsevier.com/editors/ethical-issues> – accessed on 11 February 2013), with the original source being the *Royal College of Physicians* (1991, p. 3). Another widely quoted definition is that used by the US Office of Research Integrity: "Plagiarism is the appropriation of another person's ideas, processes, results, or words without giving appropriate credit" (downloaded on 11 February 2013 from <http://ori.hhs.gov/definition-misconduct>).

<sup>10</sup> One example of such crass stupidity (thankfully involving another journal rather than *RP*) is a paper on 'Effective security management of university sport venues in Iran', in which the 'author' had done little more than substitute 'Iran' for the 'United States' in plagiarising an article by Hall (2006). He apparently failed to appreciate that most readers would find it rather suspicious to read in the introduction the following: "Large public gatherings, such as sports events that celebrate Iranian popular culture, are considered to be potential terrorist targets. In March 2005, the Department of Homeland Security (DHS) identified a dozen possible strikes it viewed most devastating, including a truck bombing of a sports arena. Since 9/11, the Iranian sports industry has increased security at major sporting venues and high profile events such as the Super Bowl, World Series, and Olympics."

<sup>11</sup> In a study of 279 papers presented at an Academy of Management conference, 25% were found to contain a certain level of plagiarism, with 13% containing over 5% of plagiarised text (Honig and Bedi, 2012).

<sup>12</sup> Schminke (2009, p. 587) notes "the significant role serendipity plays in identifying problematic manuscripts". This would seem to imply that much research misconduct currently goes undetected.

<sup>13</sup> Around 29% of the text, two tables and four figures were the same.

<sup>14</sup> Since it would doubtless take the author some time to take on board the judgement and to adjust his research behaviour accordingly, *RP* decided that it was unwilling to consider any further submissions from him for a period of two years.

<sup>15</sup> The paper was subsequently published in another journal, still with lengthy quotations from the SPRU paper but this time with the reproduced text properly identified using quotation marks.

<sup>16</sup> See also footnote 14 above.

<sup>17</sup> See Samuelson (1994), Bird (2002), Dellavalle et al. (2007), Scanlon (2007), Smith (2007), Baggs (2008), Bretag and Mahmud (2009), Roig (2009), The Lancet (2009), and Bonnell et al. (2012) for further discussion of these concepts, including specific advice on how to avoid self-plagiarism.

need to be clear about what they each entail. Generally regarded as the least serious is ‘salami-publishing’.<sup>18</sup> This can be defined as the deliberate attempt by an author or team of authors to inappropriately inflate the total of publications yielded by a particular research study (or database, survey, experiment, project or whatever) through a process of subdividing the published output into a number of thin ‘slices’ or ‘least publishable units’,<sup>19</sup> thereby either generating a greater number of separate publications than is merited by the overall contribution to knowledge offered by that study, or creating a situation where the research community would instead be better served by the results being combined in a single or a smaller number of publications.<sup>20</sup>

Salami-publishing often results in redundant publication. A ‘redundant publication’ (or a ‘duplicate publication’<sup>21,22</sup>) is defined as a paper where the existence of one or more prior (or contemporaneous) papers by the same author(s) means that the new paper offers insufficient of an original contribution to knowledge to merit publication in its own right.<sup>23</sup> Sometimes, redundant publication may include the more specific and serious offence of self-plagiarism.

‘Self-plagiarism’ can be defined as the practice by an author (or co-authors) of reproducing text, ideas, data, findings or other material from one or more earlier (or contemporaneous) papers by the same author(s) *without explicitly citing or otherwise acknowledging those other papers*, thereby misleading the reader (and in particular referees and editors) as to the level of originality of that paper.<sup>24,25</sup> Such an attempt to present one’s own previously published work<sup>26</sup> as though it were original is a form of deception<sup>27</sup> (in contrast to

plagiarism, which is a form of theft).<sup>28</sup> To this extent, the word ‘plagiarism’ in the term ‘self-plagiarism’ is potentially misleading because of its etymological connotation with theft, but that should not deflect attention from the fact that the element of deception involved nevertheless makes it a serious form of academic misconduct.

#### 4.2. Example cases

This section describes a number of cases encountered by *RP* Editors over recent years. Each is classified under one of the three categories defined above for illustrative purposes, although virtually all of them involve two or more of these various forms of misbehaviour.

##### 4.2.1. Salami publishing

One (full) professor from a European business school has been quite prolific, submitting five papers to *RP* within three years, four of which were rejected. In one such rejection, the *RP* Editor informed the author that one referee noted: “the paper is too similar to ... [your] earlier *RP* paper with just a different data set. ... [It] contains the ‘same framework, same models, same methods, same or similar variables, even almost identical tables’, and even the written text exhibits a significant degree of overlap in places.” With regard to another paper by the same author, another referee commented: “I happened to review another paper of these authors a year ago which was very similar regarding the empirical setup. Some of the text even seems to be directly copied from the previous paper to this one.” This paper, too, was rejected.

In a number of less severe cases (where the problem might just have been the result of an ‘honest mistake’ rather than a premeditated intention to deceive), once the overlap with the earlier paper by the same author has been pointed out, the failure to cite it duly rectified and the text revised to fully differentiate the new paper from the old, a suitably revised version may eventually be accepted. For example, in reviewing a paper submitted by three senior academics from North America and Europe, a referee noted: “It is similar to another paper published by [two] of the authors ... The [other] paper uses the same dataset but has [X] as the dependent variable ... rather than [Y] as with this submission, but I think they need to discuss the rather subtle difference ... There are some sentences describing the data that are the same as the published paper, and the Table ... is the same.” After this and other problems had been duly rectified, the paper was accepted. In another paper by an associate professor and a doctoral student at a North American business school, the referee commented that “the theoretical development is very similar to the content of the following paper ... [by the same authors] ... Since this article seems to be the foundation of the current study, it must be cited.” Once this and other revisions had been made, this paper, too, was published.

In another case, the *RP* Editor involved wrote as follows to the author, a European professor: “The other referee has a rather different concern – namely about the degree of overlap with your 2010 article in [XXX], and whether this paper is sufficiently distinct from that to be seen as an original contribution in its own right. I note that currently there is no reference to the [XXX] paper, so at the

<sup>18</sup> Sometimes also known as ‘fragmentary publication’ or ‘piecemeal publication’ (Roig, 2005, p. 43), many may perhaps regard it more as a form of ‘undesirable’ behaviour rather than outright misbehaviour.

<sup>19</sup> The concept of a ‘least publishable unit’ is discussed in Broad (1981).

<sup>20</sup> See also the definitions of ‘salami publishing’ in Roberts (2009, p. 585), of ‘piecemeal publication’ in APA (2010, p. 13) and of ‘repetitive publication’ given by Elsevier at <http://www.elsevier.com/editors/ethical-issues> (accessed on 11 February 2013).

<sup>21</sup> See also the guidelines on ‘duplicate publications’ provided by Elsevier at <http://www.elsevier.com/editors/perk/questions-and-answers#Onduplicatepublications> (accessed on 11 February 2013) and by APA (2010, p. 13).

<sup>22</sup> It should be noted that a few commentators prefer to reserve the term ‘duplicate publication’ for the more specific case of “publishing what is essentially the same paper in two or more journals” (Roig, 2005, p. 43).

<sup>23</sup> See also the definitions in Roberts (2009, p. 584) and APA, 2010, pp. 13–15). Redundant publications are a particular problem with regard to meta-reviews, where they can result in an unduly optimistic assessment of the efficacy or safety of a particular drug (Tramer et al., 1997).

<sup>24</sup> Even Nentjes (2012), who is more sympathetic to “the right to repeat oneself”, is unambiguous about the need to “be transparent, [and] reveal to readers and fellow-researchers that [a paper] is a duplicate” (Nentjes, 2012, p. 423).

<sup>25</sup> Some may argue that an exception might be made, say, in the case of the description of methods in a project that has legitimately led to several publications drawing on the same data set; others would contend that, even in such cases, it is still preferable for the author to cite the first published description of the methodology (see e.g. the discussion in APA, 2010, p.16).

<sup>26</sup> As we have moved into the digital era, so the previously fairly clear distinction between ‘prior publication’ and various forms of pre-publication (or ‘pre-prints’, as they were often previously called) has become increasingly blurred. By convention, chapters in doctoral theses, internal working papers and lectures do not count as ‘prior publication’. Nor do presentations at conferences, unless the paper is subsequently published in the proceedings of that conference. In more of a grey area is the publication of a ‘short version’ of a conference paper as part of the published conference proceedings; if the ‘full version’ of that paper is subsequently submitted to a journal like *Research Policy*, the author(s) should specifically draw the attention of the editor to that previously published conference version to obtain a ruling on whether or not that constitutes ‘prior publication’. Likewise, if some or all of the material has previously been used in a report submitted to a research sponsor and that report has then been circulated by the sponsor, the advice of the journal editor should be sought to check that this does not constitute ‘prior publication’.

<sup>27</sup> “Whereas plagiarism refers to the practice of claiming credit for the words, ideas, and concepts of others, self-plagiarism refers to the practice of presenting one’s own previously published work as though it were new” (APA, 2010, p. 170).

<sup>28</sup> It may also result in the author(s) being liable for copyright infringement: “authors are required to cite their own previously published material if they are referencing it within a new paper due to copyright infringement policies” (iThenticate, Writer’s Guide to Publishing, downloaded on 11 February 2013 from <http://www.ithenticate.com/writers-guide-to-publication-checking-plagiarism-copyrights/>). However, as Samuelson (1994, p. 24) notes, the author’s entitlement to ‘fair use’ of previous text would mean that the scale of self-plagiarism would probably have to be quite large for the author to be found guilty of copyright infringement (see also Scanlon, 2007, p. 60).



very least I would ask that at some suitable point in the text you explicitly deal with the issue of how this new paper differs from the 2010 [XXX] article.” This problem, too, was rectified and a revised version eventually accepted.

#### 4.2.2. Dual publication

The offences committed under this heading span a spectrum from submitting essentially the same paper to *Research Policy* as already published in – or currently being considered by – another journal<sup>29</sup> to papers that overlap to a greater or lesser extent in terms of content or text with one or more parallel papers. One example of the former type of offence was a paper submitted to *Research Policy* by researchers from an Asian university that had already been published in a national journal. In another, submitted by an assistant professor from another Asian country, the *RP* Editor involved learned that the same paper had been submitted in parallel to a second journal, so it, too, was rejected.

More difficult and more time-consuming to handle are papers involving only partial overlap. Clearly, if a journal is to establish whether the paper submitted to it constitutes sufficient of an original contribution to merit publication, that paper cannot be considered in isolation but must be carefully examined in conjunction with the relevant parallel paper(s). For example, a paper from a European assistant professor and two colleagues was rejected because an alert referee noted that they had already published an article with a very similar title in another journal. In another case, a referee spotted that a large part (over 50%) of a paper by two Asian authors (a professor and his MSc student) had already been published in another international journal, both papers being based on the same dataset and using a similar methodology. This paper, too, was rejected and the authorities at the university informed. It was subsequently learnt that the student had submitted the paper to *RP* without the apparent knowledge of his professor, who, when informed about this, duly apologised.<sup>30</sup>

#### 4.2.3. Self-plagiarism

Often, concerns about a paper may begin with issues about salami publishing or redundant publication and only later, when the existence of one or more parallel papers that have *not* been cited is subsequently revealed, does the more serious offence of self-plagiarism become apparent. In a paper by a European professor and two co-authors (also both European), the two referees spotted that it was similar to an article recently published in a neighbouring journal. Although based on different datasets, the hypotheses and theory in the two papers were essentially identical, and they shared over 1700 words in common. While the paper submitted to *RP* cited the other journal article, it failed to make clear how the *RP* article offered a contribution over and beyond that of the other journal article. However, closer investigation revealed a more serious problem in that there was an even larger overlap (of some 2500 words) with a book chapter previously published by two of the co-authors, which was not cited nor acknowledged. In addition, running the paper through plagiarism detection software revealed several hundred words reproduced from papers by other authors – in other words, there were issues of plagiarism as well as self-plagiarism. This paper was therefore rejected.

In one rather unusual case, the problem of self-plagiarism was discovered at a late stage by one of the co-authors, a professor at a North American business school. He immediately notified the *RP* Editor: “I have discovered a very serious ethical lapse on the

part of my co-author [an Asian researcher] who had copied paragraphs from another paper of mine without any attribution. He had submitted this version for your review without my knowledge. Such plagiarism of course is simply unacceptable. I have instructed him to go over the accepted paper very carefully to remove all instances of plagiarism from the paper.” The *RP* Editor involved duly expressed his gratitude for this admission of a problem that had escaped the attention of the referees. Once the problem had been fully corrected, the revised paper was published.

Sometimes, however, the problem is not spotted until after publication. For example, a conscientious referee wrote to an *RP* Editor as follows: “I just came ... [across] this paper by ... in the Journal ... It is strikingly similar to ... [the paper the referee had reviewed for *RP*]. The first paper deals with ... and the second one with ... Otherwise the content of the paper is roughly identical, sometimes literally. In fact, it used to be the same paper ... [a] NBER Working Paper ... which probably makes more sense, at least from a scientific perspective. It was not included in the reference list of the *Research Policy* paper that I refereed. I think that maybe it would have been convenient to include in the *Research Policy* paper some reference to the companion paper, or at least to the [working paper] version. For instance, it is a bit awkward not to mention it in the ‘Prior Literature’ section ... I am sorry I missed it.” In this case, it was decided that a suitably worded apology from the authors (two North American university professors) should be published as a ‘corrigendum’ to ensure readers were not misled.

Some cases involve overlap with more than one other paper, making comparison more complicated. For example, one referee recently reported that a paper submitted by post-doctoral research fellow and his professor at a European university “overlaps significantly with two forthcoming papers ... [by the same authors]. The submitted paper has nearly the same theoretical focus as the former ... while it uses the same dataset (including most IVs [independent variables] and one DV [dependent variable]) of the latter. Neither paper is acknowledged.” The authors, when asked for an explanation, claimed that the two other papers were ‘different’ and therefore did not need to be cited. However, the *RP* Editors disagreed on the grounds that “a reasonable reader would deem that they were sufficiently closely related for normal academic conventions to require that they should be cited (or, at the very least, mentioned in an accompanying letter to the Editor when the paper was originally submitted). Failure to do so means that referees and other readers were likely to gain a misleading impression as to the level of originality of the paper submitted to *RP*. Furthermore, now that we are aware of these two other papers, it is our judgement that the level of originality in the contribution of the paper submitted to *RP* is not sufficient to merit publication in a leading journal like *RP*.” The paper was therefore rejected.

In a few cases, the sheer scale of parallel publishing makes investigation exceedingly difficult. In 2010, a referee who had been sent a paper submitted to *RP* by a senior European researcher drew our attention to the fact that the latter author had produced around 30 papers on broadly the same area in the space of just over two years, and that in many cases the content of these papers overlapped substantially, yet they were not appropriately cross-referenced, thus falling into the category of self-plagiarism. No less than seven papers were submitted to *RP* during an 18-month period from April 2009 to September 2010, all of which were rejected. In one case, the *RP* Editor observed: “the conclusion is that it does not make a sufficiently substantial contribution to merit publication in our journal. Indeed, there is some concern that, with so many papers now published from the analysis of this database, you are perhaps in danger of being seen as trying to squeeze too many publications out of this, an impression that I’m sure you will want to avoid.” A few months later when another paper was being rejected, the author was again warned about the problem of self-plagiarism: “the second referee

<sup>29</sup> This particular form of misconduct is often referred to as ‘dual submission’ (see e.g. Roberts, 2009, p. 585).

<sup>30</sup> The student was informed that no further papers by him would be considered by *RP* for one year, while the professor was given a written warning.

points to substantial similarities between this paper and the article on ... published in the Journal ... , while the third referee points to an overlap with the article published in ... [yet another journal]."

More recently, an even more complicated case was brought to the attention of *RP* Editors by two individuals who independently had been asked to review papers by the same author (a professor at a European university) submitted to two other journals. They discovered that the author concerned had published an astonishing total of over 60 journal articles since 2004. Since this number was too great to handle, the two reviewers concentrated their attention on 15 articles published in leading journals over the period 2007–2010 (including three published in *Research Policy*), all of which formed part of a single stream of research emerging from a survey of over 100 firms in Europe that the author had conducted. They found that in these papers, similar analyses had been carried out with differing combinations from a large set of variables (sometimes relabelled, to add to the confusion) with no references to indicate the prior papers the author had already produced on the same broad theme. Moreover, in some cases, a given variable was treated as a dependent variable and in others as an independent variable. Perhaps more worryingly, variables that were demonstrated to be significant in some papers were then (presumably deliberately) omitted in the analysis reported in other papers. The author was asked for an explanation. This explanation was deemed unsatisfactory by the *RP* Editors, with the result that two of the *RP* papers<sup>31</sup> had to be formally retracted.<sup>32</sup>

#### 4.3. Excuses offered by authors and why they are unacceptable

In cases that come primarily under the heading of redundant publication, a favoured excuse is that the author was trying to reach different audiences through publishing in different journals. However, the clear rule here is that this requires the prior and explicit permission of the editors of the journals involved (APA, 2010, p. 15). If the author fails to inform an editor of the relevant parallel paper(s), that excuse does not hold up (Susser, 1993). Furthermore, in an era of digital libraries and search-engines, the argument about trying to reach different audiences through different journals is much weaker than it was 20 years ago.

Another excuse offered for failing to cite other parallel papers is that "my supervisor said one should not cite one's own work". This is disingenuous in the extreme. If the parallel work is relevant in determining whether a submitted paper is sufficiently 'original' to merit publication, then the existence of those other parallel papers must be divulged to editors and reviewers (even if they have not yet been formally published – see below).

A variation on this excuse is "my supervisor discouraged me from citing working papers". Here, one needs to distinguish between earlier versions (whether conference papers, working papers, doctoral thesis chapters or whatever) of the paper in question, on the one hand, and early versions of other parallel papers, on the other. In the case of the former, it is true that historically authors have not been formally required to cite earlier versions of the same paper, although the convention has often been to mention these in the acknowledgements section, particularly if individuals responded to those earlier versions with criticisms and thus helped to improve the paper. However, some working paper series (e.g.

NBER, CEPR<sup>33</sup>) are now so prominent and their papers so highly cited that they come close to representing a form of 'prior publication'. As such, while it may not be necessary to cite them in the bibliography, their existence should be disclosed in the acknowledgements section or alternatively in a covering letter to the editor when the paper is first submitted. In the other case, namely of working papers that are the forerunners of other parallel papers, then the editor again needs to be informed about these so that a full and proper judgement as to the originality of the paper in question can be formed.<sup>34</sup>

In several of the cases involving self-plagiarism documented above, when an author has been challenged about the failure to cite the relevant parallel papers, he (or more rarely she<sup>35</sup>) has argued rather lamely that these references were omitted in order to ensure that the manuscript could be refereed in a double-blind manner. This excuse is again totally unacceptable. *Research Policy* requires two versions of the paper to be submitted, one in an anonymous form stripped of any clues as to the identity of the author<sup>36</sup> but the other (for the attention of the editor only) containing *all* references, acknowledgements and other information that needs to be available to form a reliable judgement as to whether it merits publication as an original contribution to knowledge. (Where such relevant material cannot for some reason be given in the paper, it should be provided in a covering letter to the editor on submission.) Failure to provide all pertinent information in the full version implies a premeditated attempt by the author(s) to deceive the journal as to the level of originality of the paper. As such, it represents grounds for the summary rejection of a paper.

#### 5. Analysis

From the cases reported in Sections 3 and 4 above, a number of conclusions can be drawn. First, the scale of research misconduct, whether in the form of plagiarism or the rather more ill-defined cases of redundant publication and self-plagiarism, is substantial, cross-national and growing. It is coming to occupy a large amount of time on the part of referees and particularly editors. Once an allegation of apparent misconduct has been made, considerable effort is required to establish whether there is a *prima facie* case of misconduct. If so, the necessary evidence must be marshalled so that the specific allegation can be put to the author(s) concerned. According to the conventions of 'due process', such authors must be offered the opportunity to explain or defend their actions, and indeed must be treated as innocent until proven otherwise. Once a response from the authors has been received, the editor(s) must decide on the outcome – in particular, whether the authors should be given a chance to retrieve the situation through preparing a revised version of their paper or not. In more serious cases, not only will the submitted paper be rejected, but further sanctions may be imposed such as notifying the author's head of department of the offence. Where the misconduct involves an already published paper, a decision must be made as to whether a corrigendum needs to be published or

<sup>33</sup> If journals are compared in terms of their Hirsch index (i.e. numbers of highly cited papers), NBER Working Papers come top of the list for business, economics and management journals, while CEPR Discussion Papers come fifth – see [http://scholar.google.com.au/citations?view\\_op=top\\_venues&hl=en&vq=bus](http://scholar.google.com.au/citations?view_op=top_venues&hl=en&vq=bus) accessed on 11 February 2013).

<sup>34</sup> Because journals operate at different speeds, a parallel working paper, even if it has not yet been submitted to a journal, may end up 'overtaking' the paper in question and being published first, thereby affecting the nature or size of the original contribution to knowledge that the paper in question makes.

<sup>35</sup> It is significant that all the more serious cases reported here have involved male researchers. In contrast, Honig and Bedi (2012, p. 116) found no evidence that the incidence of plagiarism is greater for males.

<sup>36</sup> Self-citations, for example, can be simply replaced by a phrase such as "identifying citation suppressed".

<sup>31</sup> At a late stage in the investigation, it also became apparent that in one of these *RP* papers the degree of statistical significance of several of the claimed findings had been misreported or exaggerated. Whether this was simply the result of 'accidental' mistakes, as the author claimed, is unclear. However, the fact that similar problems have since been confirmed in several other papers by this author makes this less plausible as an explanation.

<sup>32</sup> At the time of writing (March 2013), six other journals have retracted articles by the same author.

the paper formally retracted. All this requires very great care, not least because of the high costs involved if one were to arrive at an incorrect decision.

Secondly, it is apparent that academic misconduct is unfortunately not confined to PhD students and other young researchers.<sup>37</sup> It is true that some of the most blatant forms of misbehaviour involve students, sometimes without the knowledge of their supervisors. However, many cases of misconduct are perpetrated by quite senior researchers, often full professors (cf. Schminke, 2009).<sup>38</sup> Their misdeeds tend to involve less obvious encroachments across the boundary between acceptable and unacceptable behaviour. Indeed, often they seem to have convinced themselves that they have not been guilty of any such encroachment – that the boundary is simply in a different location than academic convention would suggest. Moreover, being more experienced, they may be more adept in attempting to cover their tracks, for example, by selectively citing some prior publications but not others, or by relabeling the variables in their econometric models.

Thirdly, the cosy assumption espoused by some that research misconduct may go on but only in ‘other’ countries is also challenged by the evidence reported here.<sup>39</sup> While some cases have involved researchers from outside North America or Western Europe, the majority of culprits have come from within those two regions and indeed from reasonably well known universities and research institutions rather than more obscure low-status ones.<sup>40</sup>

Fourthly, in the past, such instances of research misconduct might have been glibly attributed to the ‘publish or perish’ syndrome. However, the range of countries involved suggests that this is perhaps too simplistic. Neither Germany nor Italy, for example, would previously be seen as countries where the ‘publish or perish’ pressure was particularly pronounced, yet each has been responsible for two of the more serious cases described above. Instead, a rather more sophisticated explanation should perhaps be sought in terms of the specific incentives under which academics increasingly operate (Stephan, 2012), incentives that are often now linked to articles published in leading international journals with high impact factors. Such incentives may be encouraging some individuals to engage in dubious forms of publication practice to inflate their ‘score’ (Roig, 2010; Robinson, 2012). In the United States, the pressures are especially great for younger academics competing for a tenured position (or a tenure-track post) at a research university. In the UK, some of the cases of misconduct are perhaps linked to the Research Assessment Exercise and its successor, the Research Excellence Framework, which has further accentuated the premium on publications in top (so-called 4\*) journals over all others. Likewise, the two cases involving German researchers may be linked to the introduction (in 2006) and subsequent growing prominence of the Handelsblatt ranking of economists and other business school professors<sup>41</sup> in terms of their output of journal arti-

cles, again ranked into bands.<sup>42</sup> This, in turn, may encourage a form of ‘academic greed’, sometimes manifest in what might be characterised as almost ‘pathological publishing’,<sup>43</sup> driven by the belief that a vast publication list in one’s CV will enhance one’s status and career prospects.

Fifthly, while some referees and editors, when their suspicions have been aroused, have been willing to take up the case (even though it requires significant effort for no reward, and even though it may involve them in some unpleasantness when confronting the alleged guilty party), others continue to ‘pass by on the other side’, leaving it to someone else to sort out the problem. Karabag and Berggren (2012) find disturbing evidence that this is a particular problem in economics and management, with relatively few journals being active in retracting papers found to be based on misconduct, or indeed having explicit plagiarism or research misconduct policies. To take one example of this: in the case of the *RP* paper that required the publication of a corrigendum, the parallel paper involved was actually published in the other journal *after* the *RP* paper appeared, which meant that the other paper was more in need of a corrigendum than the *RP* one. However, despite being informed of the problem and of what *RP* intended to do about it, the editors of that other (well known) journal chose to do nothing. In the case of the extensive self-plagiarism by the German author, other journals were slow to react when alerted to the problem, and in at least one case, the eventual retraction of an article by this author was justified rather vaguely in terms of ‘data problems’ rather than giving details of the specific form of misconduct involved.

## 6. Journal impact factor and the curse of ‘coercive citation’

This unwillingness of some journal editors to properly pursue cases of misbehaviour brought to their attention leads us finally to another form of academic misconduct that needs to be aired and discussed. Just as researchers are increasingly operating under incentive systems in which journal articles feature prominently, so journals and their editors are coming to be judged ever more narrowly on the basis of certain ‘performance indicators’, in particular their ‘journal impact factor’ (JIF).<sup>44</sup> Unfortunately, but quite predictably, this has led some editors to engage in rather dubious practices to boost their JIF, for example by writing editorial reviews that just happen to cite many of the articles published in their journal over the previous two years (Yong and Foo, 2011).<sup>45</sup> A more sophisticated (and harder to detect) form of JIF manipulation is the ‘citation cartel’, whereby a review in one journal is used to bolster the JIF of another journal by the former citing large numbers of recent articles in the latter (Davis, 2012).

<sup>37</sup> This is consistent with the finding of Honig and Bedi (2012, p. 116) that the incidence of plagiarism in papers presented at the Academy of Management conference is not higher for untenured or junior scholars.

<sup>38</sup> In one recent case (not involving *RP*), one of Europe’s leading economists has been found to have published very similar papers in four different journals, again without the appropriate cross-referencing of the parallel papers or the permission of the editors involved (Autor, 2011).

<sup>39</sup> The related question of whether the problem is confined to those who received their doctoral training at a few institutions, at which standards were more lax, remains to be addressed.

<sup>40</sup> For reasons of anonymity, it is not possible to name the institutions of the authors described above, but in most cases the institutions involved are comparatively well known and respected.

<sup>41</sup> See <http://tool.handelsblatt.com/tabelle/index.php?id=110&so=1a&pc=250&po=0> (accessed on 11 February 2013). The adverse consequences of such rankings have recently led several hundred German professors to boycott this ranking (see

<http://www.handelsblatt.com/politik/oekonomie/bwl-ranking/gastbeitrag-frische-brise-des-rankings-wird-zum-zerstoerischen-taifun/7115818.html> – accessed on 11 February 2013).

<sup>42</sup> Within two years of the launch of the Handelsblatt ranking, Hofmeister and Ursprung (2008, p. 266) were already warning that “the employed evaluation method gives rise to dysfunctional incentives”. Sadly their warning was ignored. It is significant that both the universities vying for top spot in the Handelsblatt rankings have recently experienced serious cases of self-plagiarism.

<sup>43</sup> Gullo and O’Gorman (2012) have even suggested (tongue in cheek) that ‘pathological publishing’ be added to the next edition of the American Psychiatric Association’s diagnostic ‘bible’ of psychiatric disorders!

<sup>44</sup> Another performance indicator is the average time from submission to publication. However, speeding things up may come at a cost in terms of the quality of the review and editing process and hence of the end product (Andrew Kirby, private communication).

<sup>45</sup> In 2012, Thomson Reuters suspended 50 journals from their JIF ratings because of “anomalous citation patterns” (see <http://admin-apps.webofknowledge.com/JCR/static.html/notices/notices.htm> – accessed on 11 February 2013).



However, more recently, tales of a new and more insidious form of JIF 'engineering' have begun to spread as researchers, particularly more junior ones, have recounted how, on receiving the good news that their article is close to being accepted by some esteemed journal, they had been requested to add some references to recent articles in that journal (even though they had made no use of those articles in their research). Put under pressure by the senior academic editing that journal, and desperate to gain final acceptance of their paper, most have apparently meekly acquiesced and duly added these gratuitous citations. Yet most academics would agree that this represents an egregious form of misconduct by journal editors – “a practice bordering on extortion” (Baum, 2011, p. 450).<sup>46</sup>

This abuse of editorial power was the subject of a systematic study reported recently in *Science* by Wilhite and Fong (2012a). In a survey of several thousand researchers in economics, psychology, management and business, the two investigators found abundant evidence that such coercive citation is now regrettably quite common, with 175 journals being identified as guilty of 'coercive citation'. (A coercive citation is defined quite restrictively as one where editors “requesting self citations ... (i) give no indication that the manuscript was lacking in attribution, (ii) make no suggestion as to specific articles, authors, or a body of work requiring review, and (iii) only guide authors to add citations from the editor's journal” – see Table S12 in Wilhite and Fong (2012b, p. 40). Hence, it is difficult to conceive of any innocent explanation for such behaviour.<sup>47</sup>) While 86% of respondents regard such behaviour as inappropriate, over half (57%) admit that they would be willing to add superfluous citations if so instructed (Wilhite and Fong, 2012a, p. 542). Unsurprisingly, “Lower-ranking scholars are more likely to continue submitting to coercive journals and are more willing to add extraneous citations before submission” (Wilhite and Fong, 2012a). In other words, the abuse of power is most pronounced in the case of the most vulnerable authors.

We will not embarrass the journals 'outed' as engaging in coercive citation by naming them here but instead refer readers to the list given in Table S12 in the 'supporting online material' (Wilhite and Fong, 2012b). Suffice it to say that a distressingly large number of journals at the top of this list are business, management or economics journals familiar to the readers of this journal. Nor are they all low-status journals with lesser publishers. Indeed, rather the reverse; the top of the list is dominated by journals from leading publishers and eminent professional associations. Moreover, many of what are traditionally regarded as 'the top' journals in their respective disciplines are cited at least once or twice. In contrast, journals that are more specific to the field of innovation studies seem to be absent from the list. There is no mention of *Research Policy*, nor indeed of 'sister' journals such as *Industrial and Corporate Change*, *International Journal of Innovation Management*, *R&D Management* and *Technology Analysis and Strategic Management*. Perhaps notwithstanding the cases described above, the norms on what constitutes appropriate behaviour are a little stronger among the senior figures who serve as editors in the innovation studies

community than among their counterparts in neighbouring social science disciplines.<sup>48</sup>

## 7. Conclusions

The evidence presented in this editorial suggests that the problem of research misconduct is substantial in scale and growing rapidly (at least in terms of the cases detected). Such misconduct is time-consuming to investigate and the pursuit of it can come at considerable cost to the 'whistle-blower', the journal and the investigator as well as the perpetrator and his or her institution.<sup>49</sup> Perhaps because of this, there may be a temptation among some referees or readers to turn a blind eye. More alarmingly, there is also a regrettable tendency on the part of certain editors to fail to follow up on evidence of misbehaviour brought to their attention, a form of 'second-order free-riding' of the type witnessed in economic game-theory studies based on observing players of the ultimatum game or the dictator game (Martin, 2012, p. 106). If too many assume that they can leave it to 'someone else' to sort out a particular problem of research misconduct, then this is likely to result in a 'tragedy of the commons', where the situation gradually worsens to the detriment of all (Martin, 2012).

We have suggested that declining standards of research integrity may be linked to the growing spread of research assessment, performance indicators and the 'audit culture' (Shore and Wright, 1999). In response, more and more academic researchers seem intent on 'playing the game' to maximise their 'score'. With this comes an escalating sense of individual competitiveness (Casadevall and Fang, 2012), more proprietary behaviour (as to what are 'my' contributions) and decreasing collegiality. In some cases, researchers may succumb to the temptation to cut corners or to assume that they can unilaterally shift the boundary between acceptable and unacceptable behaviour, a boundary which, as we have seen, is intrinsically rather blurred in the case of self-plagiarism.<sup>50</sup> In short, we seem to be witnessing a drop in the level of morality among researchers, this being “the dark side of the hypercompetitive environment of contemporary science, with its emphasis on funding, numbers of publications, and impact factor” (Fang and Casadevall, 2011, p. 3857).<sup>51</sup>

If we, the academic community, do nothing, the problem will only get worse.<sup>52</sup> To stem the tide requires, first of all, that authors pay careful heed to established codes of ethics such as those developed by the Committee on Publication Ethics (COPE, 2011) and the Academy of Management (AOM, 2006).<sup>53</sup> More effort needs

<sup>46</sup> See the guidelines to editors in Kleinert and Wager (2011, Section 2.3): “Editors should not attempt to inappropriately influence their journal's ranking by artificially increasing any journal metric. For example, it is inappropriate to demand that references to that journal's articles are included except for genuine academic reasons.” Likewise, Elsevier's policy on journal self-citations states that: “An editor should never conduct any practice that obliges authors to cite his or her journal either as an implied or explicit condition of acceptance for publication” (see <http://editorsupdate.elsevier.com/2012/06/impact-factor-ethics-for-editors/> accessed on 11 February 2013).

<sup>47</sup> Many editors understandably (and quite justifiably) encourage authors to ensure that their papers engage with the literature with which the journal's readers are familiar. Such guidance would clearly not fall under the definition of 'coercive citation' given here.

<sup>48</sup> The recent initiative by a number of journal editors to establish an ethical code of practice expressly forbidding the practice of coercive citation by editors is therefore to be greatly welcomed (see <http://editorethics.uncc.edu/Index.aspx> – accessed on 11 February 2013). See also the initiative by the editors of six finance journals (<http://www.jfe.rochester.edu/coercive.pdf> – accessed on 11 February 2013).

<sup>49</sup> The different specific types of cost involved in research misconduct are examined in iThenticate (2012).

<sup>50</sup> Some may attempt to justify their behaviour by recourse to the excuse that “All that is not expressly forbidden is permitted”, ignoring the fact that personal morality should play a part in ascertaining whether an act or form of behaviour, although not explicitly prohibited, would nevertheless be deemed unacceptable by most reasonable observers.

<sup>51</sup> This parallels a similar ethical decline among students, who, as they strive in the ever fiercer competition for better grades, do not necessarily perceive plagiarism as a form of misbehaviour or who judge that they are unlikely to be caught and punished (Dee and Jacob, 2010).

<sup>52</sup> Already, it is estimated that “at least three quarters of misconduct committed remain undetected” in the case of economics (Feld et al., 2012).

<sup>53</sup> For example, AOM requires that, “When AOM members publish data or findings that overlap with work they have previously published elsewhere, they cite these publications. AOM members must also send the prior publication or ‘in press’ work to the AOM journal editor to whom they are submitting their work” (AOM, 2006, para 4.2.3.5).



to be devoted to bringing such codes to the attention of present or future authors, both in doctoral training and in the materials provided by publishers and journal editors.<sup>54</sup> Second, greater vigilance is needed on the part of referees, editors and readers (Martin et al., 2007). Third, more individuals should be willing to pursue well-founded suspicions,<sup>55</sup> even though this takes time and can result in unpleasant confrontational situations. Most will be familiar with the quotation attributed to Edmund Burke that “All that is necessary for the triumph of evil is that good men do nothing.” However, Burke also reminded us that “No one could make a greater mistake than he who did nothing because he could do only a little.” Clearly no individual can stem the tide of rising research misconduct on his or her own; rather, it requires the combined efforts of everyone in the research community acting together if this scourge is to be at least reduced, if not eradicated. Editors and others in a position of authority must be willing to impose sanctions on those who, after due process, are found guilty of transgressing. This includes publicly retracting papers that have been found to be substantially tainted (Couzin and Unger, 2006), declining to consider any papers from a convicted researcher for a set period, notifying the supervisor or head of department, and in the most egregious cases publicly exposing the full facts. This, in turn, requires discussion among the editors of different journals (for instance, through the Committee on Publication Ethics, COPE – see e.g. Brice and Bligh, 2004) to ensure consistency in the treatment of offenders and determining what is a proportionate response.

Lastly, authors who are faced with demands from unscrupulous editors for the inclusion of gratuitous references from recent issues of that journal should steadfastly refuse as a matter of principle. Coercive citation to inflate the impact factor of a journal is an insidious form of research misconduct. If the editor concerned persists in such demands or threatens to reject the paper for failure to comply, the matter should be brought to the attention of the publisher and to senior members of the journal's advisory board. If such sharp practice is permitted to continue, then editors will inevitably be seen as lacking the moral authority to intervene in cases where they are faced by acts of research misconduct by the authors of papers submitted to their journals.

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## References

- Abbott, A., 2007. Academic accused of living on borrowed lines. *Nature* 448, 632–633.
- Abbott, A., 2008. The fraudster returns ... *Nature* 452, 672.
- Academy of Management (AOM), 2006. Code of Ethics (downloaded from <http://aom.org/About-AOM/Ethics.aspx> on 11 February 2013).
- American Psychological Association (APA), 2010. The Publication Manual of the American Psychological Association, 6th edition. American Psychological Association, Washington, D.C.
- Autor, D.H., 2011. Correspondence—letter to Professor Bruno Frey. *Journal of Economic Perspectives* 25, 239–240.
- Baggs, J.G., 2008. Issues and rules for authors concerning authorship versus acknowledgements, dual publication, self-plagiarism, and salami publishing. *Research in Nursing & Health* 31, 295–297.
- Baum, J.A.C., 2011. Free-riding on power laws: questioning the validity of the impact factor as a measure of research quality in organization studies. *Organization* 18, 449–466.
- Bedeian, A.G., Taylor, S.G., Miller, A.N., 2010. Management science on the credibility bubble: cardinal sins and various misdemeanours. *Academy of Management Learning & Education* 9, 715–725.
- Bird, S.J., 2002. Self-plagiarism and dual and redundant publications: what is the problem? *Science and Engineering Ethics* 8, 543–544.
- Bonnell, D.A., et al., 2012. Recycling is not always good: the dangers of self-plagiarism. *American Chemical Society Nano* 6, 1–4.
- Bretag, T., Carapiet, S., 2007. A preliminary study to identify the extent of self-plagiarism in Australian academic research. *Plagiary* 2 (5), 92–103.
- Bretag, T., Mahmud, S., 2009. Self-plagiarism or appropriate textual re-use? *Journal of Academic Ethics* 7, 193–205.
- Brice, J., Bligh, J., 2004. Author misconduct: not just the editors' responsibility. *Medical Education* 39, 83–89.
- Broad, W.J., 1981. The publishing game: getting more for less. *Science* 211 (4487), 1137–1139.
- Casadevall, A., Fang, F.C., 2012. Reforming science: methodological and cultural reforms. *Infection and Immunity* 80, 891–896.
- COPE, 2011. Code of Conduct and Best Practice Guidelines for Journal Editors, prepared by the Committee on Publication Ethics (downloaded on 11 February 2013 from <http://publicationethics.org/resources/code-conduct>).
- Couzin, J., Unger, K., 2006. Cleaning up the paper trail. *Science* 312, 38–43.
- Davis, P., 2012. The emergence of a citation cartel (downloaded from The Scholarly Kitchen, <http://scholarlykitchen.sspnet.org/2012/04/10/emergence-of-a-citation-cartel/> on 11 February 2013).
- Dee, T.S., Jacob, B.A., 2010. Rational ignorance in education: a field experiment in student plagiarism. NBER Working Paper 15672 (downloaded on 5 February 2013 from <http://www.nber.org/papers/w15672>).
- Dellavalle, R.P., Banks, M.A., Ellis, J.I., 2007. Frequently asked questions regarding self-plagiarism: how to avoid recycling fraud. *American Academy of Dermatology* 57, 527.
- Fanelli, D., 2009. How many scientists fabricate and falsify research? A systematic review and meta-analysis of survey data. *PLoS ONE* 4 (5), e5738, <http://dx.doi.org/10.1371/journal.pone.0005738>.
- Fang, F.C., Casadevall, A., 2011. Retracted science and the retraction index. *Infection and Immunity* 79, 3855–3859.
- Fang, F.C., Steen, R.G., Casadevall, A., 2012. Misconduct accounts for the majority of retracted scientific publications. *Proceedings of the National Academy of Sciences of the United States of America* 109, 17028–17033.
- Feld, L.P., Necker, S., Frey, B.S., 2012. Scientific misbehaviour in economics – evidence from Europe (downloaded from <http://www.eea-esem.com/files/papers/eea-esem/2012/1745/PAPER%20Norms.EEA.pdf> on 11 February 2013).
- Gullo, M.J., O'Gorman, J.G., 2012. DSM-5 task force proposes controversial diagnosis for dishonest scientists. *Perspectives on Psychological Science* 7, 689.
- Hall, S., 2006. Effective security management of university sport venues. *The Sport Journal* 9 (4), 1–10.
- Hofmeister, R., Ursprung, H.W., 2008. Das Handelsblatt Ökonomen-Ranking 2007: Eine kritische Beurteilung. *Perspektiven der Wirtschaftspolitik* 9, 254–266.
- Honig, B., Bedi, A., 2012. The fox in the hen house: a critical examination of plagiarism among members of the Academy of Management. *Academy of Management Learning & Education* 11, 101–123.
- iThenticate, 2012. True Costs of Research Misconduct. 2012 iThenticate Report (downloaded from <http://www.ithenticate.com/Portals/92785/docs/ithenticate-misconduct-report-2012.pdf> on 11 February 2013).
- Karabag, S.F., Berggren, C., 2012. Retraction, dishonesty and plagiarism: analysis of a crucial issue for academic publishing, and the inadequate responses from leading journals in economics and management disciplines. *Journal of Applied Economics and Business Research* 2, 172–183.
- Kleinert, S., Wager, E., 2011. Responsible research publication: international standards for editors. In: Mayer, T., Steneck, N. (Eds.), *Promoting Research Integrity in a Global Environment*. World Scientific Publishing/Imperial College Press, London/Singapore, pp. 317–328.
- Martin, B.R., 2010. The origins of the concept of 'foresight' in science and technology: an insider's perspective. *Technological Forecasting & Social Change* 77, 1438–1447.
- Martin, B.R., 2012. Does peer review work as a self-policing mechanism in preventing misconduct: a case study of a serial plagiarist. In: Mayer, T., Steneck, N. (Eds.), *Promoting Research Integrity in a Global Environment*. World Scientific Publishing/Imperial College Press, London/Singapore, pp. 97–114.
- Martin, B.R., et al., 2007. Keeping plagiarism at bay – a salutary tale. *Research Policy* 36, 905–911.
- Martin, B.R., et al., 2009. EES and the continuing evolution of *Research Policy*. *Research Policy* 38, 695–699.
- Nentjes, A., 2012. On the right to repeat oneself. *Homo Oeconomicus* 29, 413–432.
- Polanyi, M., 1962. The Republic of Science: its political and economic theory. *Minerva* 1, 54–73.
- Prosser, J.B., 2010. Tales from the editor's crypt: dealing with true, uncertain and false accusations of plagiarism (downloaded on 11 February 2013 from <http://cob.jmu.edu/rosserjb/A%20PROBLEM%20FOR%20JOURNAL%20EDITORS.doc>).
- Resnik, D.B., Peddada, S., Brunson, W., 2009. Research misconduct policies of scientific journals. *Accountability in Research* 16, 254–267.

<sup>54</sup> Jennifer Platt (private correspondence).

<sup>55</sup> At the same time, we clearly want to avoid any over-reaction that might give rise to a form of 'witch hunt'.

- Resnik, D.B., Patrone, D., Peddada, S., 2010. Research misconduct policies of social science journals and impact factor. *Accountability in Research* 17, 79–84.
- Roberts, J., 2009. An author's guide to publication ethics: a review of emerging standards in biomedical journals. *Headache* 49, 579–589.
- Robinson, S.R., 2012. Self-plagiarism and unfortunate publication: an essay on academic values. *Studies in Higher Education*, <http://dx.doi.org/10.1080/03075079.2012.655721>.
- Roig, M., 2005. Re-using text from one's own previously published papers: an exploratory study of potential self-plagiarism. *Psychological Reports* 97, 43–49.
- Roig, M., 2009. Avoiding Plagiarism, Self-Plagiarism, and Other Questionable Writing Practices: A Guide to Ethical Writing. The Office of Research Integrity. Rockville, MD (downloaded from <http://ori.hhs.gov/avoiding-plagiarism-self-plagiarism-and-other-questionable-writing-practices-guide-ethical-writing> on 11 February 2013).
- Roig, M., 2010. Plagiarism and self-plagiarism: what every author should know. *Biochemia Medica* 20, 295–300.
- Royal College of Physicians, 1991. *Fraud and Misconduct in Medical Research: Causes, Investigation and Prevention*. Royal College of Physicians, London.
- Samuelson, P., 1994. Self-plagiarism or fair use? *Communications of the ACM* 37 (8), 21–25.
- Scanlon, P.M., 2007. Song from myself: an anatomy of self-plagiarism. *Plagiarism* 2 (1), 57–66.
- Schminke, M., 2009. Editor's comments: the better angels of our nature – ethics and integrity in the publishing process. *Academy of Management Review* 34, 586–591.
- Shore, C., Wright, S., 1999. Audit culture and anthropology: neo-liberalism in British higher education. *Journal of the Royal Anthropological Institute* 5, 557–575.
- Smith, E.R., 2007. Plagiarism, self-plagiarism and duplicate publication. *Canadian Journal of Cardiology* 23, 146–147.
- Steen, R.G., 2011. Retractions in the scientific literature: is the incidence of research fraud increasing? *Journal of Medical Ethics* 37, 249–253.
- Stephan, P., 2012. Perverse incentives. *Nature* 484, 29–31.
- Susser, M., 1993. Prior, duplicate, repetitive, fragmented, and redundant publication and editorial decisions. *American Journal of Public Health* 83, 792–793.
- Tavare, A., 2012. Scientific misconduct is worryingly prevalent in the UK, shows BMJ survey. *British Medical Journal* 344, e377.
- The Lancet, 2009. Self-plagiarism: unintentional, harmless, or fraud? *The Lancet* 374, 664.
- Tramer, M.R., Reynolds, J.M., Moore, R.A., McQuay, H.J., 1997. Impact of covert duplicate publication on meta-analysis: a case study. *British Medical Journal* 315, 635–640.
- Van Noorden, R., 2011. Science publishing: the trouble with retractions. *Nature* 478, 26–28.
- Weingart, P., 2009. On 'best practice rules' of publishing and their erosion – a cause for concern. *Minerva* 47, 237–239.
- Wilhite, A.W., Fong, E.A., 2012a. Coercive citation in academic publishing. *Science* 335, 542–543.
- Wilhite, A.W., Fong, E.A., 2012b. Supporting online material for 'Coercive citation in academic publishing'. Available from Science website at <http://www.sciencemag.org/content/suppl/2012/02/01/335.6068.542.DC1/1212540.Wilhite.SOM.pdf> (accessed on 11 February 2013).
- Yong, J., Foo, A., 2011. Impact of excessive journal self-citations: a case study on the *Folia Phoniatrica et Logopaedica* journal. *Science and Engineering Ethics* 17, 65–73.
- Zimmer, C., 2012. A sharp rise in retractions prompts calls for reform. *New York Times* (16 April) (downloaded on 11 February 2013 from <http://www.nytimes.com/2012/04/17/science/rise-in-scientific-journal-retractions-prompts-calls-for-reform.html>).

Ben R. Martin\*

SPRU – Science and Technology Policy Research, The  
Freeman Centre, Jubilee Building, University of  
Sussex, Brighton, BN1 9SL, UK

\*Tel.: +44 1273 873562; fax: +44 1273 685865.  
E-mail address: [B.Martin@sussex.ac.uk](mailto:B.Martin@sussex.ac.uk)

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