
ETHICS IN SCIENCE

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See also

http://issuu.com/dvm_berlin/docs/ethics_in_science?mode=a_p

Introduction

Science publication media are increasingly confronted with problematic paper submissions. This is not only related to authors, not even related only to publication as such, the problem is much wider as we will see later. This communication is based on our own experience in editing and publishing a journal.

What are the Problems?

We as editors of a journal are confronted with ethical problems related to publications, which we start with; comments on more general problems in the area of science will follow.

The problems in scientific publication will be illustrated in the following by a number of typical examples.

Plagiarism

What is of increasing concern is shameless copying of other authors' papers, word by word, page by page, derivation by derivation, to pretend having done original work, and giving the community the false impression that they have done that work. Fortunately, we have numerous knowledgeable reviewers who discover a large number of, but not all, suspicious manuscripts.

Some generic and notorious cases

1. Authors submit the same manuscript to more than one journal. In a recent case an author submitted three copies of his work to three journals. As a consequence, three editors and three groups of reviewers were misused to deal with a single paper.
2. Authors send a manuscript that was rejected by one journal to another one without making the editor aware of the previous rejection. In our opinion, re-submission of a rejected paper to another journal would be acceptable if the previous

rejection – preferably together with the reviewers' comments – would be communicated to the new editor, and if the author had made changes reflecting the reviewers' comments, rather than just submitting the paper unchanged.

Two very special cases:

3. A professor XX created his own journal and in so doing created an incredibly (and ridiculously) large number of publications and citations. This journal is known as "Prof. XX's publication farm".
4. An article in Nature¹ reports a case of misconduct in Zhejiang University (ZJU) in Hangzhou: After the editor of a journal found "...that figures in a manuscript by *He Haibo*, a scientist....who had been hired by the ZJU only months before, were suspiciously similar to those in an article that He had published elsewhere.... the case, which eventually led to the retraction of eight papers....Articles attacked the laxity of a system that gave leadership roles to the likes of *Li Lianda*, dean of the department....and *He's* supervisor, who was largely absent from the lab and unfamiliar with the work, but was last author on some of *He's* papers. "There was plagiarism, fabrication and falsification...." says *Yang Wei*....", president of the university. Wei has set up measures for preventing scientific misconduct at his university.

Basically, we see two kinds of plagiarism:

- Authors copy other authors' work, without citation of the original publication. This is "classical" plagiarism;

¹ *David Cyranoski*, A University Cracks Down on Misconduct in China; *Nature*, 481, 12 January 2012, pp 134-136.

- Authors copy their own work; we call this "paper recycling" or "self-plagiarism".

The first case is simply theft or fraud. The author steals other peoples' (intellectual) property. Here, copyright problems with probable legal prosecution can be the consequence. It is a common belief that science is based on trust and honesty; neglecting these virtues – if discovered – may lead to denial of an academic grade, e.g. a doctoral degree, publication of a notice in the journal telling the community what has happened or, in extreme cases, the journal editor refusing to consider that author's work for a set period of time.

A very prominent case has surfaced in Germany: The former defense minister, *zu Guttenberg*, was accused of having used other authors' work without citation for his PhD thesis. Consequently, he lost his PhD Degree. Some additional prominent figures in Germany were also denied their PhD title. One of the reasons for this development is that many professors decorate themselves with a large number of PhD students whom they never can supervise, which is certainly not supporting the quality of research. This is good for people who just want the doctoral title for their business cards and name plates, whereas the ideal motivation for PhD study should be the start of a scientific career. An easy way of receiving a scientific degree or title is buying it. There are agencies selling Dr. Degrees and Professor Titles having their origin in remote countries². Although these "upgrades" have no real value, they are worth the money (up to € 40.000) because the risk of being discovered is low. Here formal decoration for the ego is the driving force.

These and similar incidents represent just the end of a chain. The reasons for this kind of behaviour can be seen in pressure exerted by the scientific system and also in the authors' excessive ambition to have a long list of publications – "The more the better" – , see example #3.

Consequently, the total number of publications and also the number of journals are rapidly increasing. Like somebody wrote to us: "I guess this is why I stopped reading the journals, too much bulk and nothing of value. Maybe I am seeing this differently, but I guess quantity rules over value."

There are editors who try to push up the impact factor of their journals with unfair means, for example: They require from their authors to cite papers of their own journal.

Authorship

² DIE ZEIT, No.20, 10 May 2012.

Authorship is also a key item in scientific publication. Consequently, often too many researchers are listed as authors although they had not been involved in the investigation reported in the publication. Frequently heads of research groups insist on being listed as authors, "strategic authorship", without having contributed to the research work. Ideally, authorship – including the order of authors – should strictly follow the amount of contribution to the work described in the paper. We know of leaders of research groups, who do not just want to be co-authors, they want to be the first author. Their concern seems to be that they tend to disappear from the radar screens of their peers in the scene if they do not show up as authors or – better – as primary authors. This is certainly unfair against the people, who do the hard work, but: If a professor's researchers are successful, then the sun shines also upon him or her, doesn't it? Notwithstanding these arguments, a group leader may become co-author if he or she has substantially contributed to the substance of the work being published.

Finally, it has to be made clear that every author has the right and the duty to check the manuscript before it goes to the journal.

Reviewing manuscripts

We have a further problem that seems to be ever worsening, and this is related to reviewing manuscripts. Until a few years ago, the complete review process was done by paper mail. When a manuscript arrived, the secretary made two copies, and we sent these copies to experts who we believed would be willing and able to review the paper. Usually, we got back two reviews, mostly by the deadline. This has completely changed, to the dismay of authors, editors and experts who are asked to do reviews. We make the following observations:

1. An expert does not respond to the invitation, even after a reminder.
2. After a while, an expert responds after a reminder, this could be negative or positive.
3. If negative, the expert is expected to name a person (with e-mail address) who may be able to do the review; however, frequently this does not happen.
4. If the response is positive, then it happens that the reviewer does not deliver his/her comments by the deadline. Then we send a reminder. Then there are again two options:
 - 4.1 No response by the new deadline, sometimes even no response after the second reminder. Or:
 - 4.2 The review will be delivered.

This process can be extremely time-consuming, and after a while we receive letters from authors

who are very disappointed by the slow review process.

We are now approaching at least four, sometimes even more experts, hoping that we receive at least two, better three, useful reviews. It is clear to us that the work load of those people who are able to do a decent review can be extremely high; the work to be done just to keep their groups going basically does not allow any additional actions.

It happens that *reviewers recommend that the author adds publications of the reviewer to the author's references*. This is another kind of misconduct owing to the pressure of having high scores in citations.

Education

The contemporary study system in Germany and the US has become extremely pressing. Every course a student applies to is a contribution to the examinations; the student has to pass an exam, even if he/she wishes to attend just for personal interest. Students just compete for credit points, again a stupid formalization in the science system. There is little time for widening a student's horizon beyond his/her major study, not even during the semester holidays when exams have to be prepared and passed. Creativity and motivation cannot develop under these circumstances. Our students are now approaching what employees in industry sometimes are: Robots made for a specific purpose. In Germany, about 70% of the students have financial trouble, and the high work load in the university makes it almost impossible to earn some money by short term jobs. In the U.S., the guarantee of federal government loans for students has become a contentious economic and political issue. Between 40 to 50% of German students report difficulties with the new Bachelor degree system. Depression and fear of exams are abundant; the need for psychological treatment is the consequence.

We should bear in mind that *we are stealing* the most important property of a young person: *his or her youth*. We will have to pay for it.

Costs of Research Misconduct³

An aspect that is usually not in our focus is possible harm to public health. Due to the importance that goes far beyond just misconduct, we extract some details from the report "iThenticate" cited in the footnote. It provides a very detailed analysis on the costs of misconduct in science.

Costs for the individual may consist in loss of job, revoked awards, lawsuit, and last but not least in questioned integrity. The publishing media may see its reputation damaged and loss of excellent authors as well as of sales figures.

We cite a complete paragraph from iThenticate: "The Public Library of Science journal, *PLoS Medicine*, published a study, titled 'The Costs and Underappreciated Consequences of Research Misconduct: a Case Study,' which quantified the costs of misconduct. Utilizing a "data-based modeling approach" and applying it to a real example of scientific fraud, *PLoS* researchers were able to calculate actual dollar amounts for the damages incurred. In the end, they determined that their experimental case directly cost the university involved \$525,000. This amount included costs for case deliberation, an inquiry panel, as well as the use of an investigation committee, comprised of eight individuals. However, this calculation may be conservative because it does not include several other indirect costs. Taking their statistical model one step further, while also employing external sources to determine the national extent of institutional misconduct, the *PLoS* group calculated that "the direct costs would exceed \$110 million" for the United States in 2010 alone. Given the data for increased misconduct over the past few years, this number will likely be significantly higher in 2012.

A prominent case of misconduct involved anesthesiologist Dr. Scott Reuben, who was exposed in 2009 for having fabricated at least 21 papers. The prominent medical journal *Anesthesia & Analgesia* had published 10 of Reuben's papers, and even though the materials were later retracted, the damage was already done. The editor-in-chief of *Anesthesia & Analgesia* highlighted the cost of misconduct in this case:

"We are talking about millions of patients worldwide, where postoperative pain management has been affected by the research findings of Dr. Reuben." [16]⁴ Beyond directly affecting the health outcomes for the general public, misconduct also works to turn back the clock on progress within the research community. Time spent on research based on fraudulent work is wasted effort. Moreover, fraudulent or falsified research can be passed on and used by other researchers, even post retraction.

According to a report of the *British Medical Journal*⁵, financial ties between pharmaceutical

³ <http://www.ithenticate.com/>

⁴ Citation in iThenticate

⁵ VERONICA YANK, DRUMMOND RENNIE, LISA A BERO, Financial ties and concordance between results

companies and publications have been discovered.⁶ "..... A high and increasing proportion of biomedical researchers have financial ties to the pharmaceutical industry.¹⁻⁵ (*The superscripts in these excerpts refer to references in the original report.*) Such researchers are more likely to publish articles—economic analyses, reviews, opinion pieces, and even randomised controlled trials—that support products produced by the industry.^{4 6-12} Editors and journals also have been criticised for having financial conflicts of interest that may favour drug companies. Meta-analyses published up to December 2004 that were not duplicates and evaluated the effects of antihypertensive drugs compared with any comparator on clinical end points in adults.....¹²⁴ meta-analyses were included in the study, 49 (40%) of which had financial ties to one drug company..... Some antihypertensive drugs have been shown to dramatically improve mortality and morbidity. *The market for these and other antihypertensive drugs is highly competitive and lucrative.* According to market research, both angiotensin receptor blockers and calcium channel blockers were in the top 10 list of global therapeutic drug classes by sales in 2005, equating to earnings of over \$26b (£13b; €18b).²³ Concern exists about the effect of such profits on doctors. We included meta-analyses published up to December 2004 that evaluated the effects of antihypertensive drugs on clinical outcomes in adults.....*Our findings have considerable relevance to the real world, however, as the marketing of antihypertensive drugs constitutes a multibillion dollar a year industry, and antihypertensives are some of the most prescribed drug classes in the world....* Our study also exposes a *failure of peer review*. Both editors and peer reviewers must have read manuscript versions of those meta-analyses containing discordant results and

We believe that these kinds of misconduct have to be regarded as a severe scandal, if not crime, which ought to be prosecuted.

Where Do the Problems Come from?

Lack of Awareness of the Problem

Sometimes we receive problematic manuscripts from young authors, e.g. PhD students or post docs, who may not be aware that they do something wrong when they copy from other publications. The cover letter may start with: "I

am a student, and I wish to submit....." Our impression is that the younger generation is not aware of problems related to intellectual property or copyright. For example, illegal downloading of music is not considered illegal; it is rather considered a sort of sport. It seems to us that these authors work either without a supervisor or have a supervisor who has no experience with the international publication system.

The generation issue is also related to the easy access to the internet, and everything in the internet is not only available, but it also seems to be there for free. "Copy and paste" is the tool people are used to. The easy access to the world's knowledge is enticing; incorporation into the surfer's work is just a copy and paste step away. Moreover, there is an increasing tendency towards "freedom of information", that is interpreted as "including everything". However, *freely available* information does *not* mean that this information is also *freely reusable!*

The foregoing comments do not mean that we want to discourage inexperienced young scientists to go public internationally; we rather wish to motivate the younger generation to expose themselves to the scientific community; however, they must seek guidance by experienced scientists to avoid disappointments.

The cases described herein reveal a deeper problem, namely:

False Incentives and other Flaws in the Contemporary Science System

In Germany, for example, the salaries of professors⁷ and allocation of research funds may depend on the number of PhD students. And many professors decorate themselves with a large number of PhD students whom they never can supervise. This is one of the causes of *people who just want the doctor title for their business cards and name plates*, whereas the ideal motivation for a PhD study should be the start of a scientific career.

In this context it is worth to note that *Dr. Degrees and Prof. Titles can be bought with money*⁸, without the year-long stress of doing serious research work. These degrees and titles come primarily from universities in remote countries, with fees up to € 40.000. The titles are worthless, because there is nothing behind them. They raise the ego of people who need such formal decoration. The deal is worth the money since the probability of being discovered

and conclusions in meta-analyses: retrospective cohort study, BMJ 2007;;39376.447211.BE

⁶ The order of the cited excerpts from the study has been changed by the present authors for better legibility.

⁷ This has now been criticized by the German Constitutional Court.

⁸ EGMONT KOCH, The Title Dealer, Sueddeutsche Zeitung, 10 May 2012 (in German)

is low. This example shows again the greed for more and a lack of shamelessness.

A further point that is worth to observe is related to the conferment of medals and awards. The formalities of proposing candidates and finally selecting awardees should guarantee that this process is as open as possible to avoid closed-circle/insider deals.

We have heard of universities which let jobless people work in their labs; these people receive unemployment pay and are hence cheap lab slaves. This is the negative aspect. A positive aspect is that jobless academics have a chance to work in their field and improve their skills.

During a recent conference it was said during the discussion of a presentation that the president of a prestigious American university let professors know that it is the number of publications, not the content that counts – we guess this is not an exception. Along the same line is the pressure on researchers in universities and research centers to publish a certain number of papers and to acquire a certain amount of funding. And universities are increasingly being organized according to enterprises, where totally different criteria are present. *The outcome of research is ranked by numbers rather than scientific quality*; bean counting is the evaluation tool. We suspect that apart from primitive economic thinking in an area where it does not make much sense, there is a loss of trust in those people who were hired after a thorough procedure for performing high quality research. Universities and other research institutions are increasingly being organized like enterprises, where totally different criteria apply. And the science administrations have become too bureaucratic and believe that “control is better than belief”, where “control” means very formal evaluation simply because everybody is able to understand numbers – even bureaucrats remote from science. This has also to do with CEOs of research institutions who came from the world of economy.

Hence: *Evaluations* have become more and more formal and *put* increasingly *quantity before quality*, for example: Acquired money from outside sources; number of publications; number of citations; magnitude of the h-index; number of invited lectures; number of PhD students; number of visiting scientists. In some countries, authors are awarded money for each paper they publish in an established journal.

From a letter we received: “.....In (name of the country withheld), we face a phenomenon much worse than plagiarism. As a matter of fact, publications are not important to receive grants and research funds (therefore, plagiarism is not needed!!), since pseudo-political Committees deliver grants, academic positions and research funds to 'their favourite' candidates, even if such candidates are not the best ones...”

There are obviously aspects of the contemporary science system that we consider devastating to good scientific practice.

We do not want to belittle plagiarism but we put part of the blame on our system of *evaluation of science*. *In our view, it has become partly perverse*. This may be due to the actual “culture” exerting pressure for speeding up everything we deal with – pressure for “efficiency”, yet nobody has ever been able to explain how “efficiency” can enhance creativity – the main raw material of scientific research. As it was pointed out above, evaluations “count beans”, look at figures, such as number of publications per year, amount of money acquired from sources outside the university, number of visiting scientists per year (A concrete example: “What, you did not have a single visiting scientist last year? It seems that your group is not sufficiently attractive.”), number of PhD theses finished per year, and so on. The submission and evaluation system not only requires a substantial amount of time and efforts, it has also brought about a kind of bureaucracy suppressing science and teaching. Science is being undertaken to satisfy formal requirements, and every system can be satisfied, which was clearly visible in the previous system of “socialism” in Eastern Europe.

Another example of the hype in the belief in numbers: A high impact factor of a journal is believed to be the highest goal to be achieved, because it is supposed to be the prime measure of the quality of a journal. In order to raise this factor, some editors “ask” their authors to cite more papers from their own journal. Besides this being very unethical behaviour, the impact factor is certainly not the only measure of the influence of a journal: We have several more application oriented journals having much influence in technology; however, in these areas the readership is more likely to apply literature to their engineering work, rather than publishing which does not result in citations. On a recent editor conference, one of the present authors (KHS) asked the audience who believes that the impact factor is the prime measure of a journal’s quality. About two handfuls out of about 100 editors present raised their hands.

This formal kind of evaluation is counter-productive. Under these circumstances, celebrities such as Albert Einstein, Max Planck, Edwin Hubble and the likes could never have been successful and revolutionize our perception of the world!

Fortunately, this situation is being increasingly recognised; for example, the German Science Foundation (DFG) – when you apply for a grant – wants to see only five publications, those you believe are your most important ones. In the U.S., the National Science Foundation asks for

the same number most related to the proposed work. This means that it is not the length of the publication list that counts but the quality the applicant believes to have achieved. This may have a positive effect on reducing tsunamis of publications. The point is, if you are sufficiently creative you can serve every system. But then creativity is misused for optimising yourself for a system that has to be questioned instead of being used for your research work.

Recently, a group of German professors published a manifesto which identified structural problems behind the scientific system. These problems were identified as the excessive pressure for publishing⁹ (*publish or perish*), rapid economization of academic institutions (*business management rather than scientific leadership*), high pressure on acquisition of outside money, as well as the requirement to make scientific results publically "attractive" by using sales and marketing instruments from the business world.

In conclusion, the science system needs a strategy for deceleration in order to provide researchers with the freedom for critical reflection. From this requirement, a number of suggestions were derived, and presented in the next section.

Concluding Remarks

So far we have been talking about problematic cases in science and scientific publishing. However, major cases of abuse in science are thankfully rare, and the scientific community as a whole should not be treated as hostages; a 100% screening of everything would be overkill. The supervisors and colleagues should trust their students and colleagues. Anything else creates an atmosphere of mistrust and control.

We see this situation also in a broader context. Calling for more control, stricter laws, and tighter regulations happens in all areas of society. *Life is being made a legal case, subject to steadily increasing observation by authorities of various kinds.*

We need a *balance* between the trust that our colleagues are honest, and attention if there is a hint that something is not o.k. *No 100% control, please!*

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⁹ Comments in parentheses by present authors