

## Ethics in Science

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### Introduction

The existence of books and articles and rules for good behaviour – in particular when they are excavated from geological layers going far back in history - may create the impression that in prior times people knew how to behave themselves, that obviously they had good manners. However, the opposite is true. When people talk about ethics, good behaviour, what to do and what not to do, then you know that there is something wrong. This is also true in the sphere of science where the public tends to believe that only distinguished persons ranking high in ethics reside. 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 article is based on our own experience in editing and publishing a journal; however, we also use material provided to us by responses to our earlier presentation at ICM11 in 2011.

### 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**

*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.
3. Dear Prof.XXX,

We want to inform you that the same results and nearly the same papers from corresponding author YYY appeared in at least three high quality journals:

*Advances in Engineering Software,*  
*Engineering Fracture Mechanics,*  
*International Journal of Fatigue.*

We kindly ask you to resolve somehow this critical situation in the sense of the journal's ethics in publishing and originality of science research.

Best regards,

A former professor from University of XXXX,  
Faculty of Mechanical Engineering.

4. There are newspapers that publish exclusively reports taken from foreign newspapers and translated in the local language, without asking neither the authors or the source journal for permission
5. Review by X. VV: "The authors have spread their own contribution to this subject over three papers.....and now they present a fourth one. Maximizing the number of publications by minimizing the contained information has become the spirit of the time."

*Three very special cases:*

6. 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".
7. An article in Nature<sup>1</sup> 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.
8. An author had submitted a large number of papers that only displayed incremental advances and large sections of common text to a very wide selection of journals. EFM, IJSS, EJMA, JSV and ASME JAM were amongst those involved. The case was extremely time consuming and dragged on during the course of a year. Because of the gradational advances involved, it was very difficult to pinpoint a particular paper that could confidently be retracted to draw the community's attention to what had happened. However, it emerged that a discussion paper had been submitted to JAM and, instead of replying to the discussion, the author instead reused some of the text and ideas to better one of his subsequent submissions, to IJSS. It was decided that this submission, because it had quite obviously taken someone else's work, should be retracted and in the retraction note, the community was made aware that the retracted IJSS paper contained plagiarized work and unacceptable similarities with a number of other published papers, some of which were identified.

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

Basically, we see two kinds of plagiarism:

- Authors copy other authors' work, without citation of the original publication. This is "classical" plagiarism;
- 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 contribution on plagiarism written to us by a reviewer: " I'm afraid the authors have it right --- I just hit another one of these cases in my next review assignment. Two papers submitted by the same authors to different journals with a one-to-one map between them for nearly every sentence and equation --- just a little bit of paraphrasing to confuse the search engines. They run the same model on a different geometry and, voila!, it's a new paper. Thanks to early detection, this one won't take up much of my time." This and numerous other examples demonstrate the reviewers' waste of time when they have to evaluate papers that have already been submitted elsewhere.

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 countries<sup>2</sup>. 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 #6.

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.

A further comment from a reader of our previous contribution:

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<sup>2</sup> *Die Zeit*, No.20, 10 May 2012.

"It is a pleasure as always to read your thoughtful contribution. The topic is indeed very interesting and needs a good airing. You don't take on the issue of motivation of the publishers and motivation of the authors. The publishers make lots of money on the back of the desire of authors to see their work in print and, therefore, their willingness to act as referees, editors, desk top publishers etc, etc. I think the academic community has for a long time ignored this issue: in fact history teaches us it spawned monsters like XXX who went on to ruin many lives and careers. But I guess this is tangential to your main work, but the rise and editorial quality control of "independent" electronic journals will soon be a huge issue."

Recently, 3000 professors were interviewed about the problem of plagiarism:

"About 40% of them have been confronted with plagiarism, theft of ideas and other violations of rules using scientific texts. Even greater problems are present regarding authorship: 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."<sup>3</sup>

### ***Authorship***

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.

A contribution to us to this topic:

"I want to say something about authorship. I am strict in the order of authorship as announced at the end of the paper. I put the names in decreasing order of direct contribution: others use simple alphabetical order. Sometimes research students think the research is all their own work perhaps because they have looked down the microscope to measure the crack. They forget who originally defined the problem they are studying, who raised the money to support the student and who advised them as their work proceeds. Generally, except if quite exceptionally, the core idea came before the student and was independent of the use of machines in the laboratory etc., I always put my name, always last, on my students work, and I feel fully justified in so doing. I am not sure where this fits with: "Authorship of a scientific publication requires substantial contribution to the content of the work to be published. Today, authorship is a currency of science that will be rewarded with money. Therefore, it happens that people want to be co-authors of a paper for strategic reasons although no contribution to that paper has been provided"

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<sup>3</sup> Source unfortunately lost.

Here follows a contribution pointing at a further kind of misconduct which one of the authors (KHS) had also encountered several times:

"In the list of known problems, you could add the fact that some papers are submitted without some of the authors knowing it. In most cases, the person who submits the paper wants to increase its number of publications and "uses" the name of a known scientist hoping that this will make acceptance easier."

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. 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.

It may happen that a reviewer responds positively, but needs more time for the review and lets us know his/her pressure of time. In those cases, we usually agree.

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.

One example of our journal, we wrote to an author:

"Dear Prof.XXX,

Your submission referenced above has now been in review for more than 6 months. I have invited 8 reviewers and all have declined. I think it unfair to you and your authors to wait so long for a review, so I am asking now if you would:

1. Want to withdraw your submission and send it to another journal.
2. Request that I seek another set of reviewers through EFM."

We are now approaching at least four, sometimes many 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 frequently does not allow any additional actions.

It happens that **reviewers recommend that the author adds publications by the reviewer to his references**. This is another kind of misconduct owing to the pressure of having high scores in citations. This is highlighted by the following communication, which was originally written in German:

".....I can support every line in your text. The publication business has degenerated. Another observation: It becomes increasingly obvious that reviewers impudently require authors to cite a number of their own publications, even if they have nothing to do with the content of the reviewed paper. Sometimes this requirement is under *conditional acceptance*. Several of my colleagues have complained about this practice. We would need an Ethics Commission for abolishing this nuisance. (The present authors: Many institutions have rules for ethical behaviour in science; they have just to be followed.) The whole scientific community is indebted to your efforts."

### **Education**

Education is part of the science system; it starts in high school and terminates in a university. It is here where we observe another flaw in the system. It had been a long and fruitful tradition that students have something we call "Academic Freedom", that is to say, a student chose courses according to personal interest, the only requirement being that he could prove that one passed the exams for a minimum number of course-hours per week, per semester. Of course, there were requirements on the number and kind of courses. The first semester served for the student's orientation, so one had ample time to get used to the universe of a university and how studying works.

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 Misconduct<sup>4</sup>**

The report "iThenticate" cited in the footnote provides a very detailed analysis on the costs of misconduct in science. Due to its importance, we extract some details from that analysis:

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.

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<sup>4</sup> <http://www.ithenticate.com/>

The following analysis on capital costs is also 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."

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 cite again a complete paragraph from iThenticate:

"Although papers containing misconduct are being retracted at a greater rate than ever before, that does not necessarily mean that the retracted materials have not influenced ongoing research and even medical treatment in a negative way. The *Journal of Medical Ethics* published a study in 2011 that examined 180 retracted papers which contained research linked to 28,000 patients. The study found that 6,573 of those **patients were treated utilizing the data and research from the retracted papers.**[15]<sup>5</sup> Another 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:

*Human Costs*

- Misdiagnosis
- Research costs
- Lost time

"We are talking about **millions of patients worldwide, where postoperative pain management has been affected by the research findings of Dr. Reuben.**"[16]<sup>6</sup> 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. John Budd, working out of the School of Education at the University of Missouri in Columbia, examined 235 retracted journal papers during 1966-96 to evaluate the effect of older retracted articles on newer research. As reported by *Nature*:

"He found that they were cited in total more than 2,000 times after their withdrawal, with fewer than 8% of the citations acknowledging the retraction. And the rates haven't improved much in the age of electronic publication: in a preliminary analysis of 1,112

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<sup>5</sup> The citations refer to the original report in iThenticate.

<sup>6</sup> Citation in iThenticate

retracted papers during 1997–2009, Budd finds them cited just as often, with the retraction mentioned in only about 4% of the citations.”[17]”

In addition to false reports, there are pharmacological publications with missing data which did not please the sponsors. Two examples with hair-raising results highlight this vice of deletion.

A recent press release<sup>7</sup> of the *British Medical Journal* states that “Missing clinical trial data can harm patients and lead to futile costs to health systems, warn experts on *bmj.com* today as part of an in-depth *BMJ* review of the matter. Missing data is a serious problem in clinical research. It distorts the scientific record, so that clinical decisions cannot be based on the best evidence. Today the **BMJ** publishes several papers that examine the extent, causes, and consequences of unpublished evidence. They confirm that a large proportion of evidence from human trials is unreported, and much of what is reported is done so inadequately. In an editorial, *Dr Richard Lehman* from the University of Oxford and *BMJ Clinical Epidemiology* Editor, *Dr Elizabeth Loder*<sup>8</sup>, describe a “**culture of haphazard publication** and incomplete data disclosure.” They call for more robust regulation and full access to raw trial data to allow better understanding of the benefits and harms of many kinds of treatment. A study by *Beth Hart* and colleagues finds that including unpublished data in published meta-analyses of drug trials often changed their results. They argue that access to full trial data is needed to allow drugs to be independently assessed. Two further studies show poor adherence to requirements for mandatory trial registration and timely sharing of results. *Ross* and colleagues show that fewer than half of US National Institutes of Health funded trials are published in a peer reviewed journal within 30 months of completion, while *Andrew Prayle* and colleagues find that only 22% of trials subject to mandatory reporting had results available within one year of completion. >When the word ‘mandatory’ turns out to mandate so little, the need for stronger mechanisms of enforcement becomes very clear,” write *Lehman and Loder*.....*Lehman* and *Loder* believe that concealment of data is “a serious ethical breach” and that clinical researchers who fail to disclose data “should be subject to disciplinary action by professional organisations.””

According to a report of the *British Medical Journal*<sup>9</sup>, financial ties between pharmaceutical companies and publications have been discovered.<sup>10</sup> “.....A high and increasing proportion of biomedical researchers have financial ties to the pharmaceutical industry.<sup>1-5</sup> (*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.<sup>4 6-12</sup> 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

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<sup>7</sup> Press release in *BMJ*: Missing trial data threatens the integrity of medicine, Thursday, January 5, 2012 - 10:56.

<sup>8</sup> Contact: *Elizabeth Loder*, Clinical Epidemiology Editor, *BMJ*, London, UK, [eloder@bmj.com](mailto:eloder@bmj.com)

<sup>9</sup> *Veronica Yank, Drummond Rennie, Lisa A Bero, Financial ties and concordance between results and conclusions in meta-analyses: retrospective cohort study, BMJ 2007;:39376.447211.BE*

<sup>10</sup> The order of the cited excerpts from the study has been changed by the present authors for better legibility.

company..... Meta-analyses on antihypertensive drugs and with financial ties to one drug company are not associated with favourable results but are **associated with favourable conclusions**..... 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).<sup>23</sup> Concern exists about the effect of such profits on doctors. The *Wall Street Journal* reported that animosity between the editor of the American Journal of Hypertension and the board of the American Society of Hypertension derived from charges of influence by drug companies on the society's affairs.<sup>24</sup> ..... 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 conclusions, yet they did not prevent publication of biased conclusions....."

"In 1981 an influential Japanese study<sup>11</sup> showed an association between passive smoking and lung cancer. This article documents the tobacco industry's attempts to refute this study by producing a credible alternative study

In 1981 Japanese investigator Takeshi Hirayama published a cohort study examining the association of passive smoking and lung cancer among non-smoking wives of smokers in Japan. The study concluded that wives of heavy smokers had up to twice the risk of developing lung cancer as wives of non-smokers and that the risk was dose related. The Hirayama study was influential because it launched an extraordinary amount of critical debate and has been one of the most frequently cited studies in regulatory proceedings, risk assessments, and the media.

The tobacco industry has used a variety of tactics to maintain scientific debate about whether second-hand smoke has any harmful effects. We identify and analyse internal tobacco industry documents that describe the industry's response to the Hirayama study."

In the same spirit, some articles in medicine journals are written by ghostwriters serving a producer of a medicament, advertising for a drug in the guise of a serious scientific article<sup>12</sup>. Their names never appear in the list of authors, whereas the formal authors receive payments and did not contribute to the substance of the article.

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

### **What is Ethics?**

In conclusion, we need to adhere to some kind of ethical behaviour, which in principle is not new at all. Ethics may be defined as a system of rules for proper behavior, behavior with regard to the society we live in and/or with regard to a divine power. Without such rules we are guided by our instincts which were needed for physical survival in the past; but instincts have to be moderated by culture. All religions and

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<sup>11</sup> BMJ 2002; 325: 1413, published 14 December 2002

<sup>12</sup> Süddeutsche Zeitung No.200, 31 August 2011.

other ethical systems have such rules, because they provide guidelines, rituals, for all of life, from cradle to grave. When complying with any ethical system breaks down, then we are back to our instincts, or we have to obey our codes of laws. We are now concentrating on plagiarism, because this is the domain of publishers and editors.

Plagiarism is theft, because plagiarism is stealing other people's intellectual property. And theft is forbidden in all major ethics systems. This does not only happen in scientific publication, it happens everywhere, particularly in industry because a lot of time and money for developing new products can be saved by espionage.

*Erich Fromm* has very well described the need of humans for something that guides them through life: "Because the need for a *system of orientation* is an intrinsic part of human existence we can understand the intensity of this need." However, the system of orientation varies from individual to individual! Such a system can be a spiritual one, related to political belief, to making money, to excessive consumption, excessive strengthening of the ego or, at the lowest level, even to drugs.

### **Where Do the Problems Come from?**

#### *A Few Words About the Past*

Plagiarism is by no means a new development. In the context of art of any kind, using other people's work one does not speak of plagiarism. In the past, nobody spoke of plagiarism when writers, painters and so on served themselves from other sources.

Celebrities like Goethe, Heinrich Heine, Bert Brecht, and Thomas Mann have based their work partly on existing examples; however, they created something new, so excellent that they converted their source material to world class literature.

William Shakespeare invented none of the plots of his dramas; however, he turned the poorly developed material into everlasting creations of cultural heritage.

In the area of fine arts, for centuries it used to be an honor to the artists to be copied; they did not have any idea about plagiarism. Rubens copied Michelangelo, Rembrandt copied Rubens. The point here is that "copy" does not mean a one-to-one copy like with a modern copy machine. An artist transforms existing work into something new, sometimes into a totally novel work, so that the original model can no longer be recognized. This is then a consequence of artistic transformation. This way, new intellectual property is created.

Usually, we never work on totally new paths; in most cases, we base our work on already existing work; but we do something new with it. The J-integral, for example, was introduced by two originators; but thousands of publications deal with it and use it for new applications.

However, 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.

As already mentioned before, another style of misuse of already existing papers is "self plagiarism" which means that either complete papers or significant parts of them are re-submitted by the same authors to another journal, hoping that nobody discovers this kind of cheating. But fortunately, we have knowledgeable reviewers who frequently, but not always, detect this act by their sheer familiarity with their fields.

*Psychological Considerations*

A brief excursion to our psyche may be helpful. We are born with our biological heritage. This heritage includes instincts that developed far back in history, probably as early as during the first appearance of life. These instincts were or are still needed for survival, for survival of the individual and of the species, the latter being extremely interesting because it can be against the interest of individuals. The needs for survival of the individual include availability of food and defence against enemies, both in turn requiring a substantial amount of aggressiveness. Then comes the survival of the species which requires reliable reproduction. Reproduction is also in need of sufficient food, but also on appropriate mating strategies which are also related to (male) aggressiveness. It is hypothesized that those groups that had best access to food had the best chances for reproduction and hence survival. As our ancestors were gatherers and hunters – here with emphasis on gatherers – we developed a greed for more, more food but also more of everything. What I find is mine, no matter whether somebody else found it before. Or: If somebody else possesses something I believe I need, then I am taking it at any cost, even if I have to hurt or kill that other person.

The more the better! This is the psychological driving force for human behaviour, no matter whether it makes any sense. You can easily observe it in your environment. The same holds for physically aggressive behaviour. Although this is no longer needed for our physical or social survival this instinct is still active, and the difficulty is that it is very deeply rooted in our genes due to its early appearance in life on earth.

Our instincts are still there, however, covered by a thin layer created by culture. Therefore, our behavior is controlled by instincts modified by culture (you may also say by education.) But culture is transferred from generation to generation by education, and not by our genetic heritage. The “Possess”-instinct still controls us, it is no longer vital to survival; it has rather a social function. More money, more power, bigger car, may be even an airplane, and so on; and a long list of publications. However, this is only part of the story; a long list of publications – and other items, of course - is again needed for “survival”, namely in the evaluation processes.

Now one may say that normally people behave much better than just described. This is correct, at least for a certain part of the human population. Since our instincts in extreme expression are no longer needed - they can even be very destructive - they are moderated if not suppressed by “culture” demanding good behaviour.

Another aspect – which has been mentioned already above - may contribute to our observations: Although the majority of people is quite well educated by parents, school etc., almost all kinds of misconduct can be noted: Simple everyday behaviour, such as driving faster than allowed, or anonymous bad blogging in the internet show that we tend to forget our good behaviour. We hope that we will not be caught. When we are caught, sanctions have to be implemented. Today, sanctions are laid down in laws, but public shame can also be very effective. Effective sanctions have to be enforced; otherwise they do not make much sense.

However, here society has a tremendous problem: humans at birth are absolutely helpless. Human babies would die soon after birth without extensive care. The extremely long time to maturity is, on the one hand dangerous to the young human – in particular in “primitive” societies – but on the other hand it provides a unique opportunity for the parents’ generation to pass their life experience on to their offspring. This is how “culture” came into being. This culture – or civilization if this expression is preferred -

gradually changes human behaviour depending on the needs of the respective society. Since steps in this process can be performed in virtually every generation, the evolution of the human mind has taken on a speed unprecedented in life's history. However, according to the principle of polarity where there is light there is also shadow: this immaterial evolution process does not tolerate a single interruption. If by whatever reason one generation of parents is unable to transfer their status in cultural evolution to their children, then the transfer chain breaks, and the new generation is thrown backwards. Of course, school and playmates may work to some limited extent, but the disastrous effect of poorly educated young people can be easily observed in aggressive youth gangs where - culturally useless - aggression is the main topic. However, even in well educated individuals, under certain circumstances, instinctive behaviour may break through. For our case, aggression is certainly not relevant, however, the greed for more signs of success, for example number of honours, number of publications and all other formal items that count for regular evaluations.

*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!**

It seems that coming generations will have a different view towards using other people's intellectual property, that is to say, "intellectual property" will no longer be viewed as personal, individual property. We are not saying that we like it, but there is a fair chance that things will develop this way.

A contemporary scientist believes that what he or she found in years of hard work is his or hers, and everybody who wants to use it should make proper reference to it.

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 professors<sup>13</sup> and allocation of research funds may depend on the number of PhD students. And many professors decorate themselves

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<sup>13</sup> This has now been criticized by the German Constitutional Court.

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<sup>14</sup>, 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 is low. This example shows again the greed for more and a lack of shamelessness.

There are 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.

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.

A further written response to our previous paper:

“In present day world, numbers of journal publications, citations and H-index have become more important than the quality of the publications. Many awards, fellowships, faculty promotions, etc., rely on these measures heavily to take decisions about the nominees. A prestigious researcher said the legacy he is going to leave behind is his high h-index.”

This fits very well with another communication:

“My sincere congratulations for your and your co-authors’ interesting paper which could seem only philosophical, but it really underlines fundamental aspects of the

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<sup>14</sup> *Egmont Koch*, With the Title Dealer, Sueddeutsche Zeitung, 10 May 2012 (in German).

behaviour of our present-day international scientific community. I have some comments:

I completely agree with the sentence on page 1 of your paper:

“When people talk about ethics, good behaviour, what to do and what not to do, then you know that there is something wrong.”

It is the same thing that occurs when people talk about *'meritocracy, grants and research funds only to the best candidates'*. In that instant, there is something wrong: people who speak in that way want to do exactly the opposite, that is, they want to give 'their favourite' candidates grants, academic positions and research funds, even if such candidates are not the best ones..... I also agree with the concepts expressed on page 5, when you say that plagiarism is used to obtain “more signs of success, for example ... number of publications and all other formal items that count for regular evaluations.”

However, 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....

Finally, I strongly agree with the Manifesto of some German Professors (page 9, present authors: page number refers to our previous contribution) Especially at point 2 on page 9: “Science requires continuous and reliable in-house funding because the laws for the search for the unknown and for the understanding of nature are radically different from those of an enterprise in the world of economy.”

As a matter of fact, the preparation of applications for research funds is too time-consuming. Further, that is a job completely different from our research work. Therefore, continuous and reliable in-house funding should be available.”

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!

A letter to the editor of a German newspaper<sup>15</sup>: "...Nowadays, academic careers are merely based on achievements in the market and showmanship. This is fundamentally different from content, i.e. of quality, according to the motto: "You do not sell the most burgers when you make the best burgers, you sell the most burgers when you sell the most burgers." This salesmen's mantra has become the basis for selecting the scientific

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 (attached as an appendix) which identified structural problems behind the scientific system. These problems were identified as the excessive pressure for publishing<sup>16</sup> (*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.

## **Remedies**

The authors of this presentation believe that improvements have to be made within the complete science system.

As already stated, culture moderates or suppresses our instincts because their uncontrolled application has negative effects on society. However, this works only if sanctions are available, and sanctions have to be enforced. Without the threat of application of sanctions regulations are meaningless.

Two popular examples from our day-to-day life may illustrate the sanction mechanism:

1. You drive on a road with a certain speed limit. If you are sure that there is no camera or no police car around, many – too many – people don't care and will speed.

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<sup>15</sup> Fedor Roth in Süddeutsche Zeitung No. 15, 19 January 2012.

<sup>16</sup> Comments in parentheses by present authors.

2. The same holds for tax evasion. You do not declare a certain kind of income as long as you are sure that the tax authorities will never get to know these details.

In our case, sanctions can be societal ones, for example, loss of reputation, but also formal ones, for example, no more access to research money or exclusion from publishing in journals.

What can be done? If we narrow our view down to the mere publication problem, then everything is already there. It is up to authors, supervisors, reviewers, editors, and publishers to regulate and enforce sanctions.

#### *Authors*

Authors have to realize that they have to obey rules of good scientific practice. Prior to submission of a manuscript, experiments and/or theoretical work have to be done honestly, no unfavourable data points shall be ignored nor should some be invented which fit the author's opinion better. The sources of all external material used have to be cited.

Every co-author has the right and the duty to read the manuscript before it goes to a journal. No one shall be co-author who has not contributed to the work described in the manuscript. In addition, journals have rules that they ask authors to follow.

#### *Supervisors*

Similarly, a supervisor has the right and duty to take care of proper experimentation in the lab or appropriate theoretical analyses. Similarly to co-authors, a supervisor has the right and duty to check the manuscript before it will be submitted for publication.

#### *Reviewers*

Reviewing another's manuscript is an important part of any researcher's work. Maintaining high quality in scientific publication requires careful reviewing. The reviewers are key to publication quality. Fortunately, we have numerous highly qualified experts reviewing our submissions, and they often discover problematic cases. Reviewers are expected to be as objective as possible. When they express their personal opinion, they should say so. Fortunately, we see this quite frequently; some reviewers let the editors and the authors know that a specific comment is based on personal opinion. (Remember: Much of our knowledge is only provisional.)

And, as pointed out above, a person approached for performing a review should at least send a message that a review cannot be done in the requested time.

It happens frequently that reviewers want to see their own papers in the list of references. This is fine if these papers describe state-of-the-art that is not cited by the author. Otherwise, it is an unfair practice for extending the reviewer's publication record.

We have come across scientists who want their manuscripts published who, however, do not realize that it is each author's duty to spend time on reviewing other people's manuscripts. Therefore, those being invited to review a paper should have in mind that publishing and reviewing are just the two sides of the same thing.

#### *Editors*

The editors are in charge of identifying suitable reviewers for a manuscript soon after submission. They are the first line of defense against abuse of the scientific publication system, and their own knowledge of the field and of opportunities for abuse are their principal tools. They might use one or more reviewers suggested by the authors, but

would never use these exclusively, relying instead on their knowledge of prominent active researchers worldwide to identify additional reviewers. This technique decreases the probability that reviews might be too subjective. Editors must also be arbiters among authors in cases where accusations of abuse emerge. Finally, editors must be willing to inform their readership when abuses have been confirmed, and enforce deserved sanctions.

#### *Publishers*

The publishers are the 'guardians' of scientific information – they are responsible for disseminating it to the global community and have a duty to uphold the integrity of the scientific record. They establish, develop and make available to authors the ethical standards which are applicable across all forms of scientific publication. Elsevier and other publishers have developed tools and resources, and offer support to help editors in situations where the ethics of scientific publication are thought to have been violated.

Finally, both editors and publishers should provide understandable guidelines to authors for submitting a manuscript and to reviewers for the review process.

#### **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 is 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.

**Life is being squeezed out of life!** We are losing freedom – even the freedom to cheat – and we are losing spontaneity, and along with this come loss of motivation and creativity. You will find the end point – highest entropy, so to speak – in call centres where those poor employees are 100% surveyed and controlled. Every action is recorded and if the standard requirements are not met momentarily, then that person receives immediately an e-mail by her (it is mostly women doing these jobs) supervisor. You may say that we are grossly exaggerating. Our answer will be: No! We are not. We are already approaching Orwell's brave new world.

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!**

#### **Appendix:**

##### **Short Version of the Manifesto of Some German Professors<sup>17</sup>**

###### *1. Reduction of the Flood of Publications*

The assessment of the quality of research has to be based upon thoroughly evaluated research results which can only be achieved when the number of publications is reduced in relation to the number of active researchers; the number of journals should also be

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<sup>17</sup> Published in *Die Zeit*, 14 April 2011, Hamburg.

reduced - even if this is against the economic interest of publishers. Only this way, will researchers be able to take notice of relevant findings in their areas.

*2. Fundamental Research Needs Continuous In-House Funding*

Science requires continuous and reliable in-house funding because the laws for the search for the unknown and for the understanding of nature are radically different from those of an enterprise in the world of economy. Academic establishments have to use their financial resources in an economical way. However, expectations that they should generate financial profit and that they should be evaluated according to criteria of the business world are to be strictly rejected. Instead, we should endeavor to elucidate the high importance of fundamental research to society.

*3. More Emphasis on Evaluation of the Substance of Scientific Achievements*

Projects evaluated according to their substance with respect to scientific concepts and goals have to be selected for grants rather than those projects claiming unrealistic promises for practical applications. The qualitative evaluation of a researcher's scientific achievements should be at least as important as quantitative bibliometric indicators. The sheer number of publications is not an acceptable criterion.

*4. Rejection of Strategic Authorship*

Authorship of a scientific publication requires substantial contribution to the content of the work to be published. Today, authorship is a currency of science that will be rewarded with money. Therefore, it happens that people want to be co-authors of a paper for strategic reasons although no contribution to that paper has been provided. The system of achievement-oriented funding of research should, therefore, carefully check the real contributions of an author and consequently de-value strategic authorships that had no substantive contribution to the content of a paper.

*5. Researchers Have to Write their Research-Proposals Themselves*

Acquisition of research grants represents an important competitive component in the scientific system. The pressure for increasing numbers of research grants has created a system where in extreme cases agencies write standardized proposals instead of the researchers themselves. However, scientific concepts have to be created by the researchers themselves. "Ghostwriters" must not be tolerated; this applies also to cooperative projects where the individual contributions are blended by the agencies.

*6. Transparent Data Acquisition*

In spite of increasing complexity, science needs transparency. Fast progress of technology combined with excessive competition leads to ever more complex experiments which are hard to evaluate. Without transparent and careful documentation of data acquisition and scientific concept, an increasing number of errors and dishonesties occurs, thus endangering the substance of science.

*7. Excellent Research Needs Time*

The development of well founded projects and their realization are not compatible with short term job contracts. The pressure created by such contracts forces researchers to do small scale projects without significant progress in understanding. Longer job contracts enabling more extended project planning are needed to achieve the high quality of research needed in international competition.

## Schwalbe, Ingraffea, Lovegrove: Ethics in Science

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Review:

- Maintains integrity in the advancement of science
- Well-established process over 300 years old

