

# JOURNAL IMPACT FACTOR AND ACADEMIC ETHICS

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**Abstract.** This is an overview of cases of manipulation of journal impact factor and an attempt to explain the possible causes. The discussion is focused on Lithuanian science community with some parallels drawn to a similar situation in China. We argue that a suitable science administration practice may deteriorate the self-governance of science controlled by academic ethics.

**Keywords:** impact factor; citation; academic ethics; causes of misconduct; science policy

## Introduction

In some countries, when assessing research output for financing research institutions, for the award of positions, promotions, or prizes at the national level, more weight is attached to the number of publications in journals with high impact factor rather than to the quality of the research done. The rationale for such practice is a claim that *all* publications in journals having high impact factor based on citation numbers must have also high quality despite the fact that it was never proved. To the contrary, there is an increasing consensus (Adler, Ewing and Taylor, 2009) that

- a journal's citation numbers do not determine its quality, even more a quality of a paper in the journal ;
- the impact factor is a crude statistic which does not separate types of citations;
- the impact factor is a flawed statistic because the distribution of citations among papers is highly skewed and refers to citations within the first two years after its publication;
- the database used to estimate the impact factor contains errors and includes a biased selection of journals.

The main problem with the journal impact factor (JIF) is a possibility to manipulate its value. We argue that a possibility to increase own citation number in an attempt to publish as many as possible paper in journals with high JIF, which is often called as publish or perish culture, create an initiative to break norms of scientific ethics. This is a topic of discussion in the present paper based on our talk.

Our main concern is the use of paper counts and the JIF to evaluate science research in Lithuania. The problem in this case is that there is no research on research quality available to find effects of such practice. Therefore we have to use just what is on the surface and some random information. Also in the third section we make parallels with

what is happening in China where a similar science administration practice is used. The fourth section describes a recently published case study of the JIF manipulation just to illustrate the extent of possible misconduct. We begin in the next section with a standard information about bibliometric indicators.

### **Journal impact factor**

The predecessor of the JIF was established around 1961 as a tool of scientometrics, the science of measuring and analysing science. The modern scientometrics gets its origin from the work of Derek J. De Solla Price (Price, 1963) and Eugene Garfield. The latter founded the Institute for Scientific Information, better known as ISI. Currently the institute is owned by Thomson Reuters Corporation. The idea is to look at the number of citations and compare it to that of other papers in the field. More specifically, the JIF is defined as the ratio  $A/B$  where  $A$  is the number of citations during a particular year to journal articles within the previous two years and  $B$  is the total number of research articles in the journal during those two years.

Since the JIF is based on citation numbers it is often suggested that a paper published in journals with a high JIF is more valuable than those published in journals with a low JIF has a better quality if it is published in a journal with a high JIF. As a support for this claim one usually cites E. Garfield and the website of ISI. No sound statistical analysis was ever provided to show a relation between a science quality and JIF. To the contrary, the numerous problems with using JIF to evaluate research have long been recognized (see e.g. Seglen, 1997).

One of the problems is a possibility to manipulate JIF as we will see below. If it is possible to manipulate an indicator and if this indicator is made a target then it loses its meaning. This is the essence of the Goodhart's law in economics: "any observed statistical regularity will tend to collapse once pressure is placed upon it for control purposes". Charles Goodhart formulated his law in a 1975 paper in which he characterized the monetary policy of Margaret Thatcher based on suitable targets. But the applicability of the law has much wider area. We argue in this paper that the Goodhart's law applies to the Journal Impact Factor (JIF) since it is often used as an indicator for science quality.

Bibliometric indicators and publication counts are being used in many countries for science research evaluation purposes. This is called publish or perish culture.

### **Responses to publish or perish culture**

As soon as the citation account becomes an accepted measure of research quality in science policy one may expect that this may become a target for scientist's behaviour. The remaining question is just technical, what kind of behaviour may help to enhance the citation account.

*There are ways of accumulating citations that have little to do with scientific value. The simplest way of circumventing the hurdle of productivity enhancement is the formation of citation cartels. One's account of citations can also be augmented without enhancing one's productivity by playing off one's power as an editor or referee. Why not suppress papers submitted for publication as long as the authors do not understand to whom they owe a citation? [Franck, 1999]*

In the case an author does not understand to whom he owes a citation some journals state explicitly "No reference to the *Name of Journal* indicates that this journal might not be the most natural or suitable outlet for your research". Google search indicates that at least five journals currently contain this phrase in the section "Instructions for Authors". Three of them: *Business, Management and Education*; *Journal of Civil Engineering and Management* and *Journal of Business Economics and Management* are co-published with Vilnius Gediminas Technical University, Lithuania. Another two journals with the same suggestion to the authors are *International Journal of Management and Engineering Management* and *Journal on Food System Dynamics*.

Far less officially, a list of papers published in *Transformations in Business and Economics* circulate among potential authors suggesting to cite the papers from the list. As stated in an e-mail message (personal communication), dated as of 2009-04-16, the list was produced according to an agreement between Vilnius University Kaunas Faculty of Humanities and Kaunas University of Technology. According to another e-mail message (personal communication), dated as of 2009-03-12, a similar agreement exists between partners from *Inzinerine Ekonomika-Engineering Economics* journal and the above mentioned journals co-published with Vilnius Gediminas Technical University. In particular, a quota of papers from suitable journals to be necessarily cited and an appropriate necessary control of referees are suggested.

These facts illustrate responses to the science policy measures accredited in Lithuania. The methodology to finance institutions adapted by the Lithuanian Ministry of Education and Science includes suitable formulas giving a value of a publication depending on the JIF and is based on publication counts. The idea is to let institutions to compete for scarce financial resources in a hope to increase a number of publications of high quality and an international visibility of such publications.

An attempt to manipulate the JIF by journal editors is not new at all. It was already criticised even in public newspapers like *The Wall Street Journal* (see Begley, 2006). A surprising thing in the present case is a degree of openness with which the manipulation is carried over. It shows that the scientific community does not think it to be wrong any more, and in the e-mail message mentioned above it is stated that the agreement between journals is a response to the science policy (administration) which counts only citations.

Lithuania is not the only country which administrate science using financial instruments to foster science progress. People's Republic of China is another such country. Jufang Shao and Huiyun Shen from Zhejiang University College of Medicine describe the academic reward structure in China with some details:

*In China, the academic level of a university or an institution is evaluated mainly on the number of SCI [Science Citation Index] papers, EI [Engineering Index] papers, ISTP [Science and Technical Proceedings] papers, and the research grants it receives. ... Many universities and institutions use monetary rewards to encourage staff to publish more SCI, EI, and ISTP papers. The theory is simple and pure economics. Money motivates: pay people to publish in good journals and they try to do so. Monetary rewards are the best; money is a universal reinforcer. Greed, pride, and envy will all work to get academics eagerly and enthusiastically publishing in the best journals. [Shao and Shen, 2011].*

They also provide, as an example, the following reward system in Zhejiang University: a paper cost

indexed in ISTP – 65 EUR;  
indexed in EI – 80 EUR;  
impact factor (IF) <1 – 217 EUR;  
 $1 \leq \text{IF} < 3$  – 325 EUR;  
 $3 \leq \text{IF} < 5$  – 433 EUR;  
 $5 \leq \text{IF} < 10$  – 542 EUR;  
 $10 \leq \text{IF}$  – 1520 EUR;  
Published in *Science* or *Nature* – 21710 EUR.

It seems that the clear and direct reward system is very effective. In 2008 the total of 270924 papers from China accounted for 11,5% of all papers and ranked second in the world [Shao and Shen, 2011]. However the authors of the article see this as a problem since the outflow of good papers from China has become very common, with the trend increasing year by year.

A similar problem may occur in Lithuania. Since very few Lithuanian journals have sufficiently high JIF, the tendency among Lithuanian scientists is to publish their papers outside the country. The Lithuanian journals under such circumstances are facing an alternative either to close the journal or try to increase your own JIF by any means.

A different type of problems coming from the use of JIF as a measure of quality recently has been spelled out by Nai-Xing Wang, a professor in the Technical Institute of Physics and Chemistry in Beijing:

*The biggest problem remains the obsession with journal impact factors. Generally speaking, articles in journals with high impact factors are judged to appeal most readers, but not every paper published in a high-impact-factor journal is high quality, and papers published in lower-ranked journals are never worthless. Yet some administrators in China take a very crude approach: high-impact-factor publications mean excellent work. [Wang, 2011]*

The obsession very similar to the one we have in Lithuania. Since Nai-Xing Wang is a researcher in organic chemistry, he sees the following effects of this situation in his favourite subject:

*If a high impact factor is the only goal of chemistry research, then chemistry is no longer science. It is changed to a field of fame and game. There are other effects too. Administrators in almost every university and research institute like to evaluate researchers by their papers at the end of each year. As a result, chemists often choose easy research topics that can be written up inside a year. There are still some chemistry projects that last five years, but they are rear. Some topics are finished and written up inside six months. It is not unusual for a professor to publish ten papers in a year. And the outcome of a single project is usually split into several parts to produce more papers, which individually offer less information to readers. [Wang, 2011]*

It is of no surprise that “the pressure to rack up publications in high-impact journals could encourage misconduct” as another article in *Nature* provides the following information from China:

*However, several sources revealed to Nature that roughly one-third of more than 6000 surveyed across six top institutions admitted to plagiarism, falsification or fabrication. Many blamed the culture of jigong jinli – seeking quick success and short-term gain – as the top reason for such practices, says Zeng Guopin, director of the Institute of Science Technology and Society at Tsinghua University in Beijing who was involved in running the survey. The second most-cited cause is bureaucratic interference in academic activities in China. Most academic evaluation – from staff employment and job promotion to funding allocation – is carried out by bureaucrats who are not experts in the field in question, says Fang Shimin, a US-trained biochemist who runs a website called ‘New Threads’ that exposes research misconduct in China. “When that happens, counting the number of publications, rather than assessing the quality of research, becomes the norm of evaluation,” he says. [Qiu, 2010]*

In Lithuania, academic evaluation – from staff employment and job promotion to funding evaluation - is carried out not by bureaucrats but by scientists themselves. But the rules are the same – counting the number of publications. This rule become so natural to most members of Lithuanian academic community that they do not even think to change it, even when it become unnecessary recently. It shows that a vanity fair is acceptable to most of us.

While this section illustrates publish or perish culture in Lithuania and China, the next one shows that similar phenomena do exist in other countries too.

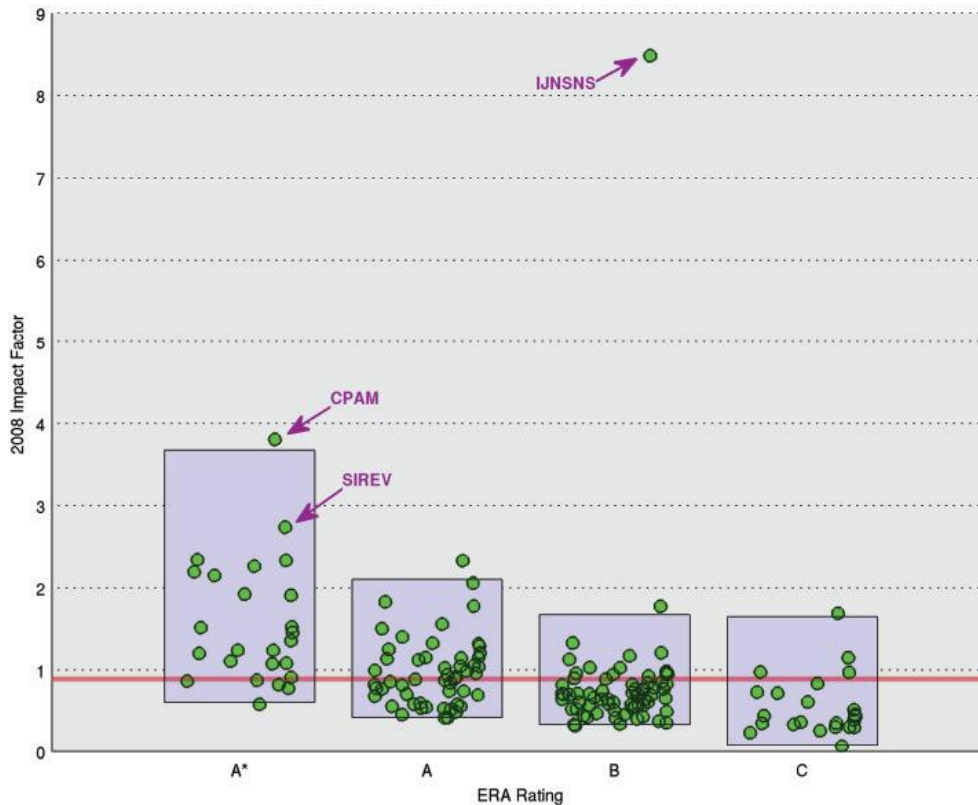
### **Further examples of JIF manipulation**

This section presents the example from the article by Arnold and Fowler, 2011, which illustrates an extreme case of manipulation with JIF. It shows how *The International Journal of Nonlinear Science and Numerical Simulation* (IJNSNS) succeeded to dominate the impact factors of all journals in the category "mathematics, applied". This journal took first place in the JIF charts in each year 2006, 2007, 2008, and 2009. However, the reputation of this journal was never near the top of its field. In 2008 IJNSNS had an impact factor of 8,91 in ISI's Journal Citation Reports. The second and third highest impact factors, *Communication on Pure and Applied Mathematics* (CPAM) and *SIAM Review* (SIREV), have impact factors of 3,69 and 2,80, respectively, in the same year. These two journals have a reputation for highest excellence.

One strange thing in this case is a clearly huge gap between the first and the second places in the JIF ranking for the same category of journals. The second strange thing is a completely opposite picture provided by the evaluation based on expert judgment. Namely, The Australian Research Council created such an evaluation, listing quality ratings for over 20000 peer reviewed journals across disciplines and from all over the world. The assigned quality rating is one of the four values:

- A\* one of the best in its field;
- A very high quality;
- B solid, though not outstanding, reputation;
- C the rest, that is those which do not meet the criteria of the higher tiers.

This rating was used in 2010 for the Excellence in Research Australia (ERA) assessment initiative. The ERA list included almost all 175 journals assigned a 2008 impact factor by JCR in the category "mathematics, applied". The vertical line of Figure 1 shows the impact factor in each of the four rating tiers displayed on the horizontal line. One can see that the highest JIF owner the IJNSNS journal is in the third category B of ERA rating. While the second and third highest JIF journals CPAM and SIREV are in the first category A\* of the ERA rating.



1. Figure (from Arnold and Fowler, 2011).

The question is how to explain the differences between the two rankings.

To answer this question, first, look at who cites IJNSNS most. The first three top-citing authors (within the two-year window) are:

- The journal's editor-in-chief Ji-Huan He, cited 243 times;
- The member of the editorial board D.D.Ganji with 114 cites;
- Regional editor Mohamed El Naschie with 58 cites.

Together the first three top-citing authors made for 29% of all citations of IJNSNS. For comparison, the top three citers to SIREV contributed 7, 4, and 4 citation percents, respectively, accounting for less than 12% of all counted citations, and none of these authors is involved in editing the journal. The top three citers to CPAM contributed 9, 8, and 8 citation percents, respectively, contributing about 7% of all citation, and were not the members of the editorial board.

Second, one can look at how citations are distributed in time between 2000 and 2007. Note that the JIF for 2008 depends on citations received for papers published in 2006 and 2007. One can see that most citations to IJNSNS are concentrated within the two-year window (2006 and 2007) out of all citations between 2000 and 2007. Namely, 71,5% citations to IJNSNS, 16% citations to CPAM and 8% citations to SIREV.

Third, one can look at the numbers of self-citations for the three journals. In 2008, IJNSNS provided 102, or 7%, of its own impact factor citations. The corresponding numbers are 1 citation, or 0,8%, for SIREV and 8 citations, or 2,4%, for CAPM. The self-citation differences during other years are similar. However, the greatest number of IJNSNS citations came from *Journal of Physics: Conference Series*. Namely, a single issue of that journal provided more than 20% of its impact factor. It was the proceedings of a conference organized by IJNSNS editor-in-chief He at his home university. The second most citing journal for IJNSNS was Topological Methods in Nonlinear Analysis, which contributed 14% with all citations coming from single issue. This was a single issue with Ji-Huan He as the guest editor. Similarly, many other citations to IJNSNS came from journals and papers for which the editor of IJNSNS had an influence.

As it was mentioned at the beginning of the section this is an obvious and extreme example of manipulation of JIF. However there is no reason to believe that this is the exceptional case. There are reasons to suspect that there are many more cases of manipulation which are not so obvious and difficult to notice because of that. There is some statistical evidence showing that large increases and decreases in JIF in only one year are the effect of journal self-citation (Campanario, 2011). The cumulative effect could destroy accuracy of the indicator and this is why scientometrics should worry about using JIF and other bibliometric indicators as a measure of quality of science.

## Conclusions

To make proper conclusions from such examples we should pay more attention to what we think science is and what is needed for science community to work better. Here we would like to recall the description of science recently suggested by Lee Smolin in his book *The Trouble with Physics*. Roughly speaking, he concludes that scientific activity means drawing conclusions from incomplete information [Smolin, 2006, p.299]. Because of this science should have mechanisms which protect us from making too many errors. Such mechanism is provided by academic ethics.

*Adherence to an ethic, ..., serves as the fundamental corrective within the scientific community.* [Smolin, 2006, p.301].

Another thing is that academic ethics is often related just to plain plagiarism. If so then JIF manipulation practice may not look a big deal; just a response to a specific science administration practice. However, in addition to plagiarism, fabrication and falsification there is a huge area of misbehavior sometimes called questionable research practice (Steneck, 2006). This practice includes improper authorship, dual submission, salami slicing, redundant publication, improper citation and many other related actions. Clearly that JIF manipulation is a part of questionable research practice since it aims at



the change of the system of rewards in science. JIF manipulation also illustrates a change of motivation in science work. One is attempting to change some bibliometric indicators, rather than to seek to improve a research quality.

What can be done to reduce the incentives for people to play numbers games. If resources are not available for assessing people by examining reports from referees and one does have to rely on statistics, then one should use as many as possible different types of indicators. However, it is not a radical solution since people always will tend to learn new tricks to maximize their scores. Therefore if a methodology to finance institutions and universities is required then one needs to obtain sound assessment rather than rely on statistics that are inherently unfair and easy to manipulate. The real change would be to change a science policy from administrating science to encouraging self-organization of science meaning that we have to believe in rationality of our science community.

Therefore our conclusion is that science administration practice adds to the deteriorating academic ethics provided it gives more weight on paper count and on a single bibliometric indicator rather than on quality of science.

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