How Nonsense Became Excellence: Forcing Professors to Publish

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Abstract In current academic systems professors are forced to publish as much as possible because they are evaluated and ranked according to the number of their publications and citations in scientific journals. This "publish or perish"-principle results in the publication of more and more nonsense. This tendency can only be stopped by abolishing the currently pervasive competition for publication. In the past, researchers who had nothing to say were not incentivized to publish but nowadays they also have to publish continually. Non-performance has been replaced by the performance of nonsense. This is worse because it results in an increasing difficulty to find truly interesting research among the mass of irrelevant publications.

A number of perverse incentives are associated with the competition for publication. This includes strategic citing and praising, endless variation of already existing models and theories, and emphasizing formal and mathematical skills, while deemphasizing the content of a paper. Furthermore, in order to maximize the number of publications, scientists also try to squeeze out as many publications as possible from minor ideas (salami tactics), increase the number of co-authors, try to become ever more specialized in already highly specialized scientific disciplines and, in the most extreme case, just fake experiments and results. Engaging in all these activities is basically a waste of time as it does not foster the advancement of science. Instead, it crowds out the intrinsic motivation of professors and other scientists, which is essential for creativity.

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1 Introduction: The Illusion of Promoting Efficiency by Setting up Competitions

Once upon a time it was believed that professors and scientists mainly engage in research because they are interested in understanding the world they live in, and because they are motivated to come up with new explanations and solutions. This was not always true but it was accepted not to tell a country's best academics which kind of research they should do (Kohler 2007; Schatz 2001). Academic work was not assessed systematically, as it was tacitly assumed that academics would strive for excellence without having to be forced to do so.

Today we live in a different world. To ensure the efficient use of scarce funds (which nevertheless are growing in the EU since 2001!),[1] the government forces professors and their academic staff, to continually take part in competitions, which are set up in order to promote "academic excellence" (Binswanger 2010, 2013). What caused such a drastic change in science policy? Why did universities forget about their noble purpose of increasing knowledge and degenerated instead into ranking-minded fundraisers and publication factories?

Ironically this degeneration is rooted in the now-fashionable and omnipresent search for excellence, where universities are supposed to outperform each other. Germany started an Excellence Initiative in order to boost its international competitiveness. Switzerland aimed to become one of the top five countries for innovation by supporting excellence (Gassmann and Minsch 2009). And the European Union, with the so-called Lisbon-strategy of 2000, had hoped to turn the EU into the most dynamic knowledge-based economy by 2010 (Lisbon European Council 2000). Amongst this childish race for excellence, it was overlooked that not everybody can be more excellent than everybody else. The fallacy of composition applies once more. Instead, the term 'excellence' became a meaningless catchword. The German philosopher Jürgen Mittelstrass (2007, p. 4, translated by the author) writes:

> Until now, no one took offence at the labeling of excellent cuisine, excellent performance, excellent academics or excellent scientists. [. . .] In the case of science this changed since science policy has occupied this term and talks about excellent research, clusters of excellence and excellence initiatives, in endless and almost unbearable repetitions.

Yet, how do we actually distinguish between an excellent and a not so excellent professor? In reality no one really knows, least of all the politicians who enthusiastically launch such excellence initiatives. But setting up competitions is supposed to solve the problem. It is assumed that competitions will automatically reveal the best researchers so it will not be necessary to care about neither content nor purpose of research. This illusion became prominent under the Thatcher government in England in the 1980s and then soon spread to other countries as well (Binswanger

[1] See Research and development expenditure, by sectors of performance (Eurostat Code: tsc00001).

2010, p. 44). The Thatcher government, guided by its faith in markets and competition, would have loved to privatize all institutions engaged in academic research and to let markets decide who is doing excellent research. However, this proved to be impossible. Basic research constitutes, for the most part, a common good which cannot be sold profitably on markets. Privatization would therefore crowd out basic research. Thus, as a second-best solution, competitions without markets were promoted, which nevertheless were termed markets (e.g., internal markets, pseudo-markets), even though this is a false labeling.

Related to the euphoria about markets and competition, there was also a constant suspicion regarding independent research taking place within "ivory towers", where scientists engage in such obscure activities as the search for truth. Consequently, the former British minister of education Charles Clarke characterized "the medieval search for truth" as obsolete and unnecessary (cited from Thorpe 2008). Modern universities are supposed to produce applicable knowledge, which has the potential to increase economic growth. Universities should think "entrepreneurially" and adjust to economic needs (see Maasen and Weingart 2008). For this reason, governments in many countries, and particularly in the EU, started to establish gigantic national or supranational research-funding programs. Instead of making funds directly available to universities, they have to compete for these funds, so that only the "best" get a chance to do more research. This is supposed to ensure that practice-oriented and applicable knowledge is created and that government funds are not wasted. Hence universities are forced to construct illusionary worlds of applicability and to pretend that all research serves a practical purpose (Körner 2007, p. 174).

Therefore, the major challenge is the question how to impress research commissions responsible for the distribution of funds in order to get additional funding. Mostly researchers try to impress by showing how many successful projects they did in the past, how many articles they already published and how much they were networking with other important scientists in the particular field. In this way, measurable "excellence" is demonstrated, which increases the probability of getting more funds as well. The assumption seems to be that our knowledge increases proportionally to the amount of scientific projects, of publications, and of networking activities, which in turn is supposed to lead to progress and growth. This naïve ton ideology is widespread among politicians and bureaucrats.

Consequently, modern universities are not focused any more on gaining knowledge. On the one hand, they became fundraising institutions determined to receive as much money as possible from government research-funding programs or private institutions. And on the other hand, they became publication factories, bound to maximize their publication output. Hence, the ideal professor is a mixture of fundraiser, project manager, and mass publisher (mostly as co-author of publications written by his or her assistants as he or she has no more time to do research), whose main concern is a measurable contribution to scientific excellence rather than increasing our knowledge. Moreover, in order to make sure that professors will deliver their contribution to excellence, faculty managers have been recruited for each department in addition to traditional deans. They act like CEOs in private companies and they are supposed to implement new strategies for becoming increasingly excellent. Research becomes a means in the battle for "market shares" of universities and research institutions (Münch 2009a, pp. 148–164).

Universities which on the surface expose themselves as temples of scientific excellence, constantly have to participate in project- and publication-contests, where instead of medals, winners are rewarded with elite or excellence status and, as far as professors a concerned, with exemption from teaching duties, and sometimes also with higher salaries (pay for performance). This is the case, notwithstanding the fact that many projects and publications do not have the slightest importance for people outside and often even inside the academic system. But these measurable outputs play a central role in today's research rankings, such as, for example, the German CHE Research Ranking of German universities (see Berghoff et al. 2009).

In this contribution we focus on the competition for publication. In fact this competition consists of two closely connected competitions, which are of crucial importance in the current scientific system:

The competition among scientists to get published in scientific journals, in which peer-reviews play a major role.

The competition for rankings based on publications and citations, which are important for individual scientists as well as for research institutes and universities.

Both kinds of competitions will be analyzed in more detail. It will be shown how they result in perverse incentives, which incentivize scientists to strive for excellence by engaging in nonsense activities.

2 Competing to Get Published: The Peer-Review Process

In almost every academic discipline, publications are the most important and often the only measurable output. Therefore, it seems to be straightforward to measure a scientist's output or productivity by the number of his publications. For is it not the case that many publications are the result of a lot of research, consequently increasing our relevant knowledge? Should not every scientist be driven to publish as much as possible in order to achieve maximum "scientific productivity"? The answer to these questions will be a clear "no", if you are familiar with the academic world. Indeed, more publications increase the amount of printed sheets of paper, but this number tells us as little about the relevance of a scientist's research activity than the number of notes played by a musician tells us about the quality of a piece of music.

Of course, measurements of scientific output are more sophisticated than just counting the written pages published by a scientist. Relevant publications are published in professional journals, where submitted papers have to go through the so-called "peer-review process". This should ensure that only "qualitatively superior" papers are published, which then can be considered to be "true scientific publications". Thus, strictly speaking, the competition among scientists is to publish as many articles as possible in peer-reviewed scientific journals.

However, there exist strict hierarchies among scientific journals, which are supposed to represent the average "quality" of articles published in these journals. In almost every scientific discipline there are a few awe-inspiring top-journals (A-journals), and then there are various groups of less respected journals (B- and C-journals), where it is easier to place an article, but where the publication does not have the same relevance as a publication in an A-journal. Publishing one's work in an A-journal is therefore the most important and often also the only aim of modern scientists, which allows them to ascend to the "Champions League" of their discipline. Belonging to this illustrious club makes it easier to publish further articles in A-journals, to secure more research funds, to conduct even more expensive experiments, and, therefore, to become even more "excellent". In this fashion, the "Taste for Science", described by Merton (1973), which is based on intrinsic motivation and which was supposed to guide scientists, is replaced by the extrinsically motivated "Taste for Publications" (Binswanger 2010, p. 150).

Looking at the development of the number of scientific publications, it seems that scientists are actually accomplishing more and more. Worldwide, the number of scientific articles has increased enormously. The number of scientific publications in professional journals increased from approximately 3,965,000 in the years from 1981 to 1985 to about 10,573,000 in the years from 2005 to 2009 (SBF 2011, p. 10), which corresponds to an increase by 270 %. The annual growth rate calculated on this basis was around 4.2 %. In the decade from 2000 to 2009 this growth rate even increased to 5.6 %. Therefore, the number of scientific publications grows faster than the global economy and significantly faster than the production of goods and services in North America and Europe, where the majority of publications is coming from SBF (2011, p. 11).

Once we begin to examine the background of this increasing flood of publications it quickly loses its appeal. A closer look reveals that the peer-review process is highly problematic. This supposedly objective procedure for assessing the quality of articles in reality often resembles a random process (Osterloh and Frey 2008). A critical investigation discovers a number of facts that fundamentally question the peer-review process as a quality-ensuring procedure (cf. Atkinson 2001; Osterloh and Frey 2008; Starbuck 2006). It generally appears that expert judgments are highly subjective, since the consensus of several expert judgments is usually low. One reason is that many peers, who are mostly busy with their own publications, will not actually read, let alone understand, the papers they are supposed to evaluate. There is not enough time for reviewing and usually there are also more rewarding things to do. Therefore, peers quite often pass the articles on to their assistants, who have to draft reviews as ghostwriters (Frey et al. 2009). No wonder that under such conditions important scientific contributions will often be rejected. Top-journals repeatedly rejected articles that later on turned out to be scientific breakthroughs and even won the Nobel Prize. Conversely, plagiarism, fraud and deception are hardly ever discovered in the peer review process (Fröhlich 2007,

p. 339). In addition, unsurprisingly, reviewers assess those articles that are in accordance with their own work more favorably, and on the other hand, are more likely to reject articles that question their own research (Lawrence 2003, p. 260).

Due to the just-described peer-review process, the competition for publication in scientific journals results in a number of perverse incentives (see also Anderson et al. 2007). To please reviewers, a potential author makes all kinds of efforts. To describe this behavior Frey (2003) rightly coined the term “academic prostitution”, which—in contrast to traditional prostitution—is not the result of a naturally existing demand, but is induced by the forced competition for publications. In particular, the peer-review process is associated with the following perverse effects.

2.1 Strategic Citing and Praising

When submitting an article to a journal, the peer-review process induces authors to think about possible reviewers who have already published articles dealing with the same or similar topics. And they know that editors often consult the bibliography at the end of an article when looking for possible reviewers. Therefore, it is quite easy to guess, who the potential reviewers will be. To flatter them the author will preferably quote as many as possible and praise their work (for instance as a seminal contribution or an ingenious idea). Moreover, an additional citation is also useful for the potential reviewer himself because it improves his or her own standing as a scientist. Conversely, an author will avoid criticizing the work of potential reviewers, as this is likely to lead to rejection. Accordingly, this attitude prevents the criticism and questioning of commonly accepted approaches. Instead, it leads to replication of established knowledge through endless variations of already existing models and tests.

2.2 Sticking to Established Theories

In any scientific discipline there are some leading scientists who dominate their field and who often are also the editors of top journals. This in turn allows them to avoid publication of approaches or theories that question their own research. Usually this is not difficult, since most authors already try to adapt to currently prevailing mainstream theories in their own interest. The majority of the authors simply want to publish articles in top journals, and this makes them flexible in terms of content. They present traditional or fashionable approaches that evoke little protest (Osterloh and Frey 2008, p. 14). In this way, some disciplines (e.g., economics) have degenerated into a kind of theology where heresy (questioning the core assumptions of mainstream theories) is no longer tolerated in established journals. Heresy takes place in a few marginal journals specializing in divergent theories. But these publications rarely contribute to the reputation of a scientist.

Therefore Gerhard Fröhlich observes that (2003, p. 33) similar conditions prevail as in the Catholic Church: censorship, opportunism and adaptation to the mainstream of research. As a consequence, scientific breakthroughs rarely take place in peer-reviewed journals nowadays.

2.3 Impressing by Technicalities Instead of by Content

Since it does not pay off to question commonly accepted theories and research methods, authors have shifted their creativity to the development of increasingly sophisticated models and research methods. Simple ideas are blown up into highly complex formal models which demonstrate the technical and mathematical expertise of the authors and signal importance to the reader. In many cases, the reviewers are not able to evaluate these models because they have neither the time nor the motivation to carefully check them over many hours. Therefore, formal brilliance is often interpreted as a signal for quality and it helps to immunize authors from criticism. Peers, who are not working within the same narrowly defined research field just need to believe what insiders "prove" to be right in their complicated models.

By emphasizing formal aspects instead of content, sciences increasingly move away from reality Precision in fictitious models is more important than actual relevance. The biologist Körner writes (2007, p. 171, translated by the author): *"The more precise the statement [of a model], the less it usually reflects the scope of the real conditions which are of interest to or available for the general public and which leads to scientific progress."*

2.4 Undermining Anonymity by Building Expert Networks

In theory, the peer-review process should make sure that publication opportunities are the same for all potential authors. Both the anonymity of the authors and the reviewers are supposed to be guaranteed by the double-blind principle. But competition under such conditions would be too much of a hassle for established professors at top universities. After all, why did they work hard in the past and made a scientific career, if at the end they are treated like a newcomer, whenever they submit a paper to a journal? The critical debate on the peer-reviewed process discussed in the journal Nature in (2007) however showed that established scientists are "less anonymous" than other potential authors in the peer-review process. They know each other and are informed in advance which papers of colleagues and their co-authors will be submitted. In research seminars or informal meetings, they present new papers to each other, which successfully undermines anonymity of the peer-review process.

3 Competing for Top-Rankings by Maximizing Publications and Citations

Despite the great difficulties of publishing articles in professional journals, the number of publications is constantly growing and the number of journals is also increasing. Publications are important for rankings of individual scientists as well as of research institutions and universities. Furthermore, if young scientists apply for the post of a professor, the list of publications is usually the most important criterion in order to decide who will get the post. No wonder that scientists do everything to publish as much as possible despite the onerous peer-review process. The question what to publish, where to publish, and with whom to publish dominates the professional life of a modern scientist. Publication problems cause sleepless nights and the acceptance of an article in a top journal makes their heart sing.

But how does the number of publications actually get into the evaluation and ranking process of scientists and their institutions? At first glance, this seems quite simple: count all articles published by a scientist in scientific journals (or the number of pages) and then you will get a measure for the publication output. However, there is a problem. As was already mentioned, journals differ dramatically in terms of their scientific reputation, and an article in an A journal is supposed to be considerably "more excellent" than an article in a B or C journal. So we must somehow take into account the varying quality of the journals in order to achieve a "fairly" assessed publication output. To this end, an entirely new science has emerged, which is called scientometrics or bibliometrics. The only topic of this science is measurement and evaluation of publication output in other sciences. By now this new discipline has its own professors and its own journals, and consequently the measurements are also becoming more complex and less transparent, which in turn justifies even more bibliometric research.

A measure which has become particularly popular is the so-called "Impact Factor" (Alberts 2013). Nowadays this factor is commonly used in order to assess the "quality" of a journal. The Impact Factor of a particular journal is a quotient where the numerator represents the number of citations of articles published in that particular journal during previous years (mostly over the last 2 years) in a series of selected journals in a given year. The denominator represents the total number of articles published in that journal within the same period of time. For example, if a journal has an Impact Factor of 1.5 in 2013, this tells us that papers published in this journal in 2011 and 2012 were cited 1.5 times on average in the selected journals in 2013.

The Impact Factors used in science today are calculated annually by the American company Thomson Scientific and get published in the Journal Citation Reports. Thomson Scientific in fact became a monopolist in the calculation of impact factors, although the exact method of calculation is not revealed, which has been criticized repeatedly (see, e.g., Rossner et al. 2007). *"Scientists have allowed Thomson Scientific to dominate them"* (Winiwarter and Luhmann 2009, p. 1). This monopoly enables Thomson Scientific to sell its almost secretly fabricated Impact Factors to academic institutions at a high price, although in many sciences less than 50 % of today's existing scientific journals are included in the calculation (Winiwarter and Luhmann 2009, p. 1).

The competition for top rankings by maximization of publications and citations leads to additional perverse incentives, which can be experienced at almost every university and research organization. They are described in more detail below.

3.1 Using Salami Tactics

Knowing that the ultimate goal in current science is the maximization of relevant publications, researchers often apply the principle of "doing more with less", which has also been termed "salami tactics" (Weingart 2005). New ideas or records are cut as thin as salami slices in order to maximize the number of publications. Minor ideas are presented in complex models or approaches in order to qualify for an entire article. As a consequence, further publications can be written by varying these models and approaches. No wonder that the content of such articles gets increasingly irrelevant, meaningless, and redundant. Hence, it is becoming difficult to find new and really interesting ideas in the mass of irrelevant publications.

The most extreme form of a Salami tactic is to publish the same result twice or more. Such duplication of one's own research output is of course not allowed, but in reality often proves to be an effective way to increase one's "research productivity". As we have seen above, the peer-review process frequently fails to discover such double publications. Therefore, an anonymous survey of 3,000 American scientists from the year 2002 shows that at least 4.7 % of the participating scientists admitted to have published the same result several times (Six 2008). And in reality this percentage will probably be even higher as not all scientists will admit their misbehavior in a survey even if it is anonymous.

3.2 Increasing the Number of Authors per Article

It can be observed that the number of authors publishing articles in scientific journals has substantially increased over recent decades. For example, in the "Deutsche Ärzteblatt" the average number of authors per article has risen from 1 author per article in 1957 to 3.5 in 2008 (see Baethge 2008). This is, on the one hand, due to the fact that experiments have become increasingly complex and that they are no longer carried out by single scientists, but rather by a team. An evaluation of international journals showed that today's average number of authors per article in modern medicine is 4.4, which is the highest number of all disciplines. This is followed by physics with 4.1 authors per article. In psychology, the average is 2.6 authors per article, while in philosophy, still free of experiments, the average number of authors of an article is 1.1 (Wuchty et al. 2007).

However, the increase in team research is not the only reason for the constant increase in the number of authors per article. There is also the incentive to publish as much as possible and to be cited as often as possible. So, especially those who have some power in the academic hierarchy (professors or project leaders) try to use their power by forcing other team members to include them as authors in all publications of the research team. And the larger a team is, the more publications with this kind of "honorary authorship" are possible. Conversely, it is often advisable to a young scientist to include a well-known professor as a co-author because—also thanks to the lack of anonymity in the peer-review process (see Sect. 2)—this improves the chances of publication (see above).

The growing number of co-authors not only increases the publication list of the participating authors themselves, but also the number of direct and indirect "self-citations" (Fröhlich 2006), which triggers a snowball effect. The more authors an article has, the more all participating authors will also quote this article. "*I publish an article with five co-authors and we have six times as many friends who quote us*." (Fröhlich 2007).

3.3 Becoming More and More Specialized

To meet the enormous demand for publication, new journals for ever more narrowly defined sub-categories of a research discipline are constantly launched. Thus, the total number of worldwide existing scientific journals is estimated between 100,000 and 130,000 (Mocikat 2009), and each year there are more (Ware and Mabe 2012). By becoming increasingly focused on highly specialized topics chances for publication are improved (Frey et al. 2009). It often pays off to specialize in an exotic but important-sounding topic, which is understood only by very few insiders, and then to establish a scientific journal for this topic. Consequently, the few specialists within this field can promote their chances of publication by writing positive reviews for each other so that they will all end up with more publications.

Let us just take the topic of "wine" as an example: There is the "Journal of Wine Economics", the "International Journal of Wine Business Research", "Journal of Wine Research", the "International Journal of Wine Marketing," and so on. These are all scientific journals, which cover the topic of wine on a "highly scientific" level dealing with wine economics or wine marketing. It would not be surprising if soon we will also have specialized journals for red-wine economics and white-wine economics.

3.4 Engaging in Fraud

Last but not least, the competition for publications and citations also leads to fraud. "*The higher the pressure to increase productivity, the more likely it is to resort to doubtful means*" (Fröhlich 2006). The assumption that universities are committed to the search for truth (Wehrli 2009) becomes more and more fictitious. Modern universities are exclusively committed to measurable excellence and the search for truth often does not help much in this respect. No wonder that quite a few cases of fraud have recently been discovered.

A good example is the former German physicist Jan-Hendrik Schoen, born 1970, who was celebrated as a German prodigy in the beginning of the new millennium. For some time it was believed that he had discovered the first organic laser and the first light-emitting transistor, and, accordingly, he was highly praised and received a number of scientific awards. At the peak of his career, as a 31-year-old rising star at Bell Laboratories in the United States, he published an article every week, of which 17 were even published in highly respected journals such as "Nature" or "Science". No one seemed to notice that this is simply impossible if you do proper research. Instead the scientific community of Germany was proud to present such a top performer. But after some time, co-researchers doubted his results and it turned out that they mostly had been simulated on the computer. An interesting fact is, as Reich (2009) writes in her book "Plastic Fantastic" that these frauds would probably never have even been discovered, had Schoen not exaggerated so much with his publications. Otherwise, he would probably be a respected professor at a top university by now.

Cases of fraud such as the example of Jan Hendrik Schoen mainly occur in natural sciences, where the results of experiments are corrected or simply made up. In social sciences, however, empirical research has often reached a degree of irrelevance, where it does not matter anymore, whether results are faked or whether they are the "true outcome" of an experiment or a survey. They are irrelevant in one way or the other.

Conclusion

Forcing professors to publish by setting up competitions for publication, results in the production of more and more nonsense, which does not add to scientific progress. This is true in spite of the fact that the number of citations of articles also increases along with the number of publications. But the increase in citations of useless publications is not a sign of increased dispersion of scientific knowledge. Presumably, many articles even get quoted unread. This has been shown by research that documents how mistakes from the cited papers are also included in the articles which cite them (Simkin and Roychowdhury 2005). Therefore, more and more articles are published but they are read less and less.

(continued)

The whole process represents a vicious circle that can only be escaped by stopping competitions for publication in their current form. In the past, researchers who had nothing to say were not incentivized to publish. But today even uninspired and mediocre scientists are forced to publish all the time even if they do not add anything to our knowledge. Non-performance has been replaced by the performance of nonsense. This is worse because it increases the difficulty to find truly interesting research among the mass of irrelevant publications.

But the nonsense does not only concern the publications themselves. It also involves many corresponding activities, which are the result of the perverse incentives, set by the competition for publication. This includes strategic citing and praising, endless variation of already existing models and theories, and using mathematical artistry while not caring too much about the content of a paper. Furthermore, in order to maximize the number of publications, scientists also try to squeeze out as many publications as possible from minor ideas (salami tactics), increase the number of co-authors, try to become ever more specialized in already highly specialized scientific disciplines and, in the most extreme case, just fake experiments and results. Engaging in all these activities is basically nonsense, which does not foster the advancement of science.

Another serious consequence of the permanent competition for publication is the demotivating effect on professors and other scientists. Their intrinsic motivation is increasingly crowded out by the principle of "publish or perish". This principle replaced the "Taste for Science" (Merton 1973), which however is indispensable for scientific progress. A scientist, who is not truly interested in his work, will never be a great scientist. Yet exactly those scientists, who are intrinsically motivated, are the ones whose motivation is usually crowded out the most rapidly. They are often rather unconventional people who do not perform well in standardized competitions, and they do not feel like constantly being forced to publish just to attain high scores. Therefore, a lot of potentially valuable research is crowded out along with intrinsic motivation. But intrinsic motivation (Merton's Taste for Science) is a necessary condition for true excellence.

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