Excellence by Nonsense: The Competition for Publications in Modern Science

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> *Most scientific publications are utterly redundant, mere quantitative 'productivity'.*
>
> —Gerhard Fröhlich

Abstract In this chapter, Binswanger (a critic of the current scientific process) explains how artificially staged competitions affect science and how they result in nonsense. An economist himself, Binswanger provides examples from his field and shows how impact factors and publication pressure reduce the quality of scientific publications. Some might know his work and arguments from his book 'Sinnlose Wettbewerbe'.

In Search of Excellence

Since the Age of Enlightenment, science has mostly taken place at universities and their respective institutes, where for a long time the ideal of uniting research and teaching was upheld. Since their re-establishment by Humboldt in 1810, German universities have been, also in terms of academic work, largely independent and the principle of academic freedom was applied.

The government merely determined that amount of money that was paid to universities and set the legal framework for science and teaching. In terms of research, the government did not impose specific research policies-with the exception of some inglorious episodes (e.g. the Nazi regime). Universities were trusted to know best what kind of research they were doing.

Generally, it was accepted not to tell a country's best academics what they should be interested in and what research they should be doing (Schatz 2001; Kohler 2007). Therefore, the academic practice of professors and other scientists

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was hardly documented and assessed systematically, as it was assumed that academics would strive for excellence without having to be forced to do so.

Sometimes this was right and sometimes it was wrong. Huge differences in quality between individual scientists were the result. Scientific geniuses and lame ducks jointly populated universities, whereby even during the scientists' lifetimes it was not always discernible who was the lame duck and who the genius.

The extraordinary is the rare result of average science and only broad quality, growing out from mediocrity, brings the great achievement at the end says Jürgen Mittelstrass, philosopher of science (2007). Still in 1945, the then president of Harvard University wrote in a letter addressed to the New York Times (August, 13th, 1945): *There is only one method to guarantee progress in science. One has to find geniuses, support them and let them carry out their work independently.*

Meanwhile, the government has given up its reservations towards universities and formerly proud bastions of independent thinking have turned into servants of governmental programs and initiatives. Lenin's doctrine applies once again: trust is good, control is better.

To ensure the efficient use of scarce funds, the government forces universities and professors, together with their academic staff, to permanently take part in artificially staged competitions. This is happening on two fronts: universities have to prove themselves by competing, both in terms of education and scientific research, in order to stay ahead in the rankings. Yet how did this development occur? Why did successful and independent universities forget about their noble purpose of increasing knowledge and instead degenerated into "publication factories" and "project mills" which are only interested in their rankings?

To understand this, we have to take a closer look at the development of universities since the 1960s. Until then, people with a tertiary education made up a relatively small fraction of the population. Universities were relatively elitist institutions which remained out of reach for the majority of working class kids. Since the 1960s however, increasing access to tertiary education occurred, for which the term 'mass higher education' (Trow 1997) was coined.

From 1950, first-year student rates increased from an average of 5 % in the industrialized countries to up to 50 % at the beginning of the 21st century (Switzerland, with its 20 %, is an exception). Universities and politics, however, were not prepared to deal with this enormous increase. It was believed to be possible to carry on with 1,000 students in the same way as has been done with 50 students, by just increasing the number of universities and professors and by putting more money into administration.

The mass education at universities made a farce of Humboldt's old idea of unity of research and education. This had consequences for both education and research. There were more and more students and also more and more researchers who were employed at universities (and later on at universities of applied sciences), but most of them no longer had any time for research. In Germany, the number of students also grew disproportionately faster than the number of professors due to the very generous government support of students through BAFÖG (Federal Education and Trainings Assistance Act). Therefore, one professor had to supervise more and more students and postgraduates and there was no more time to seriously deal with them.

Dissertations became mass products, the majority of which added little or nothing to scientific advancement. An environment emerged that was neither stimulating for professors, nor for their assistants and doctoral students, which logically led to increasing mediocrity. German universities in particular have often been criticized along the following lines: studies last too long, the dropout rates are too high, the curricula are obsolete, and research performance is only average and rarely of value and relevance for industrial innovations.

A second phenomenon which did a lot of harm to the European universities, was the lasting glorification of the American higher education system. Many politicians, but also scientists themselves, see this system as a permanent source of excellence and success without—as US scientist Trow (1997) writes—getting the general picture of the American higher education system. Attention is directed exclusively at Harvard, Princeton, Yale, MIT, and other Ivy-League universities, which make up only a small percentage of the university landscape in the US. In this euphoria, it is intentionally overlooked that the majority of colleges and universities displays an intellectually modest standard and hardly contributes to academic progress. Much of what we celebrate as 'globalization' and 'adjustment to international standards' is in reality the adjustment to US-American provincialism (Fröhlich 2006).

In Europe, the idea became fashionable that imitating top US universities would magically create a new academic elite. Like small boys, all universities wanted to be the greatest, and politics started propagating sponsorship of Ivy-League universities, elite institutions, and elite scientists. Germany started an Excellence Initiative in order to boost its international competitiveness. Switzerland aimed to be one of the top 5 countries for innovation by supporting excellence, and the European Union, with the so-called Lisbon-strategy of 2000, hoped to turn the EU into the most dynamic knowledge-based economy by 2010.

Cutting-edge universities, top-institutes, and research clusters shot up everywhere, and everyone wanted to be even more excellent than their already-excellent competitors. Amongst this childish race for excellence, it was overlooked that not all can be more excellent than the rest. This fallacy of composition applies here as well. Instead, the term 'excellence' became a meaningless catchword. Philosopher Mittelstrass (2007) 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, excellent research establishments, clusters of excellence and Excellence Initiatives, in endless and almost unbearable repetitions.

Yet how do we actually know what excellence is and where it is worthwhile to foster a scientific elite? In reality, no one actually knows, least of all the politicians who enthusiastically launch such excellence initiatives. This is where the idea of artificially staged competition comes in. It is assumed that these competitions will automatically make the best rise to the top—without the need to care about neither content nor purpose of research. We may call this ‘contest illusion’. This contest illusion was applied to science in England for the first time under the Thatcher government in the 1980s. Afterwards it was quickly copied in other countries. The Thatcher government, inspired by its belief in markets and competition, would have loved to privatize all institutions engaged in academic activities and to let markets decide which kind of science was needed, and which was not. However, this proved to be impossible. Basic research constitutes, for the most part, a common good which cannot be sold for profit at a market. Privatization would therefore completely wipe out basic research. Thus, artificially staged competitions were created, which were then termed markets (internal markets, pseudo-markets), even though this was false labeling.

Connected to the euphoria about markets and competition, there was also a deep mistrust towards independent research taking place within “ivory towers”, the purpose of which politicians often do not understand. What does the search for knowledge bring apart from high costs? On these grounds, the former British minister of education Charles Clarke characterized “the medieval search for truth” as obsolete and unnecessary.[1] Modern universities should produce applicable knowledge, which can be transformed into growth of the gross domestic product, and additionally make it more sustainable. Universities should think “entrepreneurial” and adjust to economic needs (see Maasen and Weingart 2008). For this reason, governments in many countries, particularly in the EU, started to organize gigantic research programs. Instead of making research funds directly available to universities, they are now in competition with each other, so that only the “best” get a chance. This should ensure that above all practice-oriented and applicable knowledge is created and government funds are not wasted (e.g. for “unnecessary” basic research). Hence universities are forced to construct illusionary worlds of utility and pretend that all research serves an immediate purpose (Körner 2007).

How can you impress the research commissions responsible for the distribution of funds? This is mainly achieved by increasing measurable output such as publications, projects funded by third-party funds, and networks with other institutes and universities. In this way, “excellence” is demonstrated, in turn leading to easier access to further government research funds. Competitiveness has therefore become a priority for universities and their main goal is to perform as highly as possible in measurable indicators which play an important role in these artificially staged competitions. The underlying belief is that our knowledge increases proportionally to the amount of scientific projects, publications, and intensity of networking between research institutions, which in turn is supposed to lead to more progress and wealth. This naïve ton ideology is widespread among politicians and bureaucrats.

[1] BBC News. Clarke questions study as ‘adornment’: http://news.bbc.co.uk/2/hi/uk_news/education/3014423.stm

The modern university is only marginally concerned with gaining knowledge, even though the public from time to time is assured that this is still the major goal. Today's universities are, on the one hand, fundraising institutions, determined to receive as many research funds as possible. On the other hand, they are publication factories, trying to maximize their publication output. Hence, the ideal professor is a mixture of fundraiser, project manager, and mass publisher (either directly as author, or as co-author in publications written by employees of the institution), whose main concern is measurable contribution to scientific excellence, rather than increasing our knowledge. Moreover, in order to make professors deliver their contribution to excellence, faculty managers have been recruited for each department in addition to traditional deans. Nowadays, the principal is sort of a CEO who is supposed to implement new strategies for achieving more and more excellence. Research becomes a means in the battle for "market shares" of universities and research institutions (Münch 2009).

Universities which on the surface expose themselves as great temples of scientific excellence, are forced to participate in project- and publication-olympics, where instead of medals, winners are rewarded with the elite or excellence status, exemption from teaching duties, and sometimes also with higher salaries. This is how it goes, even though many of the projects and publications do not have the slightest importance for the rest of the population outside the academic system.

Two artificially staged competitions in particular incentivize the production of nonsense: the competition for the highest amount of publications and the competition for the highest amount of research funding. The resulting indicators for publications and third-party funds 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).

The competition for the highest amount of publications will be analyzed below in more detail. On the basis of the competition for publications, it can be nicely demonstrated how perverse incentives emerge and what consequences this entails not only for research, but also generally for society and the economy.

The Competition for Publications in Academic Journals: The Peer-Review Process

In almost every academic discipline, publications are the most important and often the only measurable output. Indeed, in some natural sciences and in engineering inventions or patents also play a certain role, yet this more concerns applied science. Basic research, however, always manifests itself in publications. What is more obvious than measuring a scientist or institute's output or productivity on the basis of 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"? Someone who has just a little knowledge of universities and academic life can immediately answers these questions with an overwhelming "no". Indeed, more publications increase the amount of printed sheets of paper, but this number does not say any more about the significance of a scientist or institute's research activity than the number of notes played says about the quality of a piece of music.

Of course, measurements of scientific output are not as primitive as counting every written page of scientific content as scientific activity. Relevant publications are in professional journals, where submitted work is subjected to a "rigorous" and "objective" selection method: the so-called "peer-review process". This should ensure that only "qualitatively superior" work is published, which then is regarded as a "real scientific publication". Thus, strictly speaking, the aim of the artificially staged competitions amongst scientists is to publish as many articles as possible in peer-reviewed scientific journals.

However, among scientific journals strict hierarchies also exist which are supposed to represent the average "quality" of the accepted papers. In almost every scientific discipline there are a few awe-inspiring top-journals (A-journals), and then there are various groups of less highly respected journals (B- and C-journals), where it is easier to place an article, but where the publication does not have the same significance as an A-journal article. Publishing one's work in an A-journal is therefore the most important and often also the only aim of modern scientists, thus allowing 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. The "Taste for Science", described by Merton (1973), which is based on intrinsic motivation and supposed to guide scientists was replaced by the extrinsically motivated "Taste for Publications."

But what is actually meant by the peer-review process? When a scientist wants to publish a paper in an accepted scientific journal, the paper has to be submitted to the journal's editors, who have established themselves as champions within their disciplines. These editors usually do not have the time to deal with the day-to-day business of "their journal" and thus there is a less accomplished Managing Editor, who is responsible for administrative tasks and receives manuscripts from the publishing-hungry scientists and then puts the peer-review process in motion. The Managing Editor gives the submitted manuscripts to one or several professors or other distinguished scientists (the so-called peers) who ideally work in the same field as the author and therefore should be able to assess the work's quality.

To ensure the "objectivity" of the expert judgments, the assessment is usually performed as a double-blind procedure. This means that the reviewers do not know who are the authors of the article to be reviewed, and the authors are not told by whom their paper is assessed. At the end of the peer review process, the reviewers inform the editor in writing whether they plead for acceptance (very rare), revision, or rejection (most common) of the article submitted to the journal in question. Quite a few top journals pride themselves on high rejection rates, supposedly reflecting the high quality of these journals (Fröhlich 2007). For such journals the rejection rates amount to approximately 95 %, which encourages the reviewers to reject manuscripts in almost all cases in order to defend this important "quality measure". Solely manuscripts that find favor with their reviewers get published, because although the final decision concerning publication rests with the editors, they generally follow the expert recommendations.

The peer-review process is thus a kind of insider procedure (also known as clan control, Ouchi 1980), which is not transparent for scientists outside the established circle of champions. The already-established scientists of a discipline evaluate each other, especially newcomers, and decide what is worthy to be published. Although the claim is made that scientific publications ultimately serve the general public, and thereby also serve people who are not active in research, the general public, who is actually supposed to stand behind the demand for scientific achievement, has no influence upon the publication process. The peers decide on behalf of the rest of mankind, since the public can hardly assess the scientific quality of a work.[2] Outside of the academic system, most people neither know what modern research is about, nor how to interpret the results and their potential importance to mankind. Although scientists often also do not know the latter, they are—in contrast to the layman—educated to conceal this lack of knowledge behind important sounding scientific jargon and formal models. In this way, even banalities and absurdities can be represented as A-journal worthy scientific excellence, a process laymen and politicians alike are not aware of. They are kept in the blissful belief that more competition in scientific publication leads to ever-increasing top performance and excellence.

Considering the development of the number of scientific publications, it seems that scientists are actually accomplishing more and more. Worldwide, the number of scientific articles, according to a count conducted by the Centre for Science and Technology Studies at the University of Leiden (SBF 2007) has increased enormously. The number of scientific publications in professional journals worldwide increased from approximately 686,000 in 1990 to about 1,260,000 in 2006, which corresponds to an increase of 84 %. The annual growth rate calculated on this basis was more than 5 %. The number of scientific publications grows faster than the global economy and significantly faster than the production of goods and services in industrial countries, from where the largest number of publications originates (OECD 2008).

By far the largest share of world production of scientific articles comes from the U.S. (25 %), followed by Britain with 6.9 %. Germany produces 6.3 %, Switzerland 1.5 %, and Austria 0.7 % (SBF 2007). However, calculating published articles per capita, Switzerland becomes the world's leading country,

[2] In the language of economics, this means that the information asymmetry between scientists and lay people is so large that "monitoring" by outsiders is no longer possible (Partha and David 1994, p. 505).

because there are 2.5 published scientific articles per 1,000 inhabitants, while in the U.S. there are 1.2 articles, and only one article in Germany (SBF 2007).[3] The same picture emerges if one applies the number of publications to the number of researchers. In this case, in Switzerland for each 1,000 researchers there are 725 publications while there are 295 in Germany and 240 in the United States. Thus, in no other country in the world are more research publications squeezed out of the average researcher than in Switzerland.

Once we begin to examine the background of this increasing flood of publications it quickly loses its appeal. This is to a large extent inherent in the peer-review process itself. This supposedly objective system for assessing the quality of articles in reality rather resembles a random process for many authors (Osterloh and Frey 2008). A critical investigation reveals a number of facts that fundamentally question the peer-review process as a quality assurance instrument (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 by no means do all peers, who are mostly preoccupied with their own publications, actually read, let alone understand, the articles to be evaluated. Time is far too short for this and usually it is not even worth it because there are much more interesting things to do. Hence, time after time reviewers pass on the articles to their assistants who, in the manner of their boss, draft the actual review as ghostwriters (Frey et al. 2009). No wonder that under such conditions important scientific contributions at hindsight are frequently rejected. Top journals repeatedly rejected articles that later on turned out to be scientific breakthroughs and even won the Nobel Prize. Conversely, however, plagiarism, fraud and deception are hardly ever discovered in the peer review process (Fröhlich 2007). In addition, unsurprisingly, reviewers assess those articles that are in accordance with their own work more favorably, and vice versa, they reject articles that contradict them (Lawrence 2003).

Due to the just-described peer-review process, the competition for publication in scientific journals results in a number of perverse incentives. To please the reviewers, a potential author undertakes everything conceivably possible. To describe this behavior Frey (2003) rightly coined the term "academic prostitution", which—in contrast to traditional prostitution—does not spring from natural demand, but is induced by artificially staged competition (cf. Giusta et al. 2007). In particular, the following perverse effects can be observed:

Modes of perverse behavior caused by the peer-review process:

[3] Nevertheless, the Neue Zürcher Zeitung already worried in an article from 2004 that the growth of publications in Switzerland compared to the average of OECD countries was below average. This thinking reveals once again a naive ton ideology, in which more scientific output is equated with more well-being.

- *Strategic citing and praising*[4]
  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. To flatter the reviewers, the author will preferably quote all of them or praise their work (as a seminal contribution, ingenious idea, etc.); An additional citation is useful for the potential reviewer because in turn it is improving his own standing as a scientist. Furthermore, editors often consult the bibliography at the end of an article while looking for possible reviewers, which makes strategic citing even more attractive.
  Conversely, an author will avoid criticizing the work of possible reviewers, as this is a sure road to rejection. Accordingly, this attitude prevents the criticism and questioning of existing approaches. Instead, the replication of established knowledge gets promoted through elaboration upon preexisting approaches through further model variations or additional empirical investigations.
- *No deviation from established theories*
  In any scientific discipline there are some eminent authorities who dominate their field and who often at the same time are the editors of top journals. This in turn allows them to prevent the appearance of approaches or theories that question their own research. Usually this is not difficult, since most authors try to adapt to the prevailing mainstream theories in their own interest. The majority of the authors simply wants 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). In this way, some disciplines (e.g. economics) have degenerated into a kind of theology where heresy is no longer tolerated in established journals. Heresy takes place in only a few marginal journals specializing in divergent theories, but these publications rarely contribute to the reputation of a scientist. As Gerhard Fröhlich aptly writes: "In science as in the Catholic Church similar conditions prevail: censorship, opportunism and adaptation to the mainstream of research. As a result, a highly stylized technocratic rating- and hierarchy-system develops, which hinders real scientific progress."
  In empirical studies, the adherence to established theories can also be discovered by the results of statistical tests. To falsify an existing theory is linked to low chances of publication and thus there is an incentive to only publish successful tests and to conceal negative results (Osterloh and Frey 2008).
- *Form is more important than content*
  Since presenting original content usually lowers the chances of publication, novelty has shifted to the form how content is presented. 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

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[4] In the meantime, there are now so-called guides along the lines of "How to publish successfully?", which provide strategic advice to young scientists in the manner described herein.

have neither the time nor the inclination to deal with these models over several days. Since they cannot admit this, in a case of doubt formal brilliance is assessed positively because it usually supports prevailing theories. It helps to immunize the prevailing theories against criticism from outside, and all colleagues who are not working within the same research field just need to believe what was "proven to be right" in the existing model or experiment.

With this formalization, sciences increasingly move away from reality as false precision is more important than actual relevance. The biologist Körner writes (2007, p. 171): *The more precise the statement [of a model], the less it usually reflects the scale of the real conditions which are of interest to or available for the general public and which leads to scientific progress.*

The predominance of form over content (let us call this 'crowding-out' of form by content) does also attract other people to science. The old type of an often highly unconventional scientist who is motivated by intrinsic motivation is increasingly being replaced by formally gifted, streamlined men and women,[5] who in spite of their formal brilliance have hardly anything important to say.

- *Undermining of anonymity by expert networks*

  In theory, the peer-review process should work in such a way that publication opportunities are the same for all authors. Both the anonymity of the authors and the reviewers are guaranteed thanks to the double-blind principle. For many established scientists at top universities, "real" competition under these conditions would be a nuisance. After all, why did one work hard for a lifetime only to be subject to the same conditions as any newcomer? The critical debate on the peer-reviewed process discussed in the journal *Nature* in 2007, however, clearly showed that in practice the anonymity of the process for established scientists is rare. They know each other and know in advance which papers by colleagues or by scientists associated with them will be submitted. In expert networks maintained in research seminars, new papers are presented to each other, which successfully undermines the anonymity of the peer-review process.

  This fact can clearly be seen when looking at the origin of scientists who publish in top journals. For example, a study of the top five journals in economics (Frey et al. 2009, p. 153) shows that of the 275 articles published in 2007, 43 % originated from scientists working at only a few top American universities (Harvard, Yale, Princeton, MIT, Chicago, Berkeley, Stanford). The professors of these universities are basically set as authors and the rest must then go through an arduous competition for the few remaining publication slots. What George Orwell noted in his book "Animal Farm" can be paraphrased: All authors are equal but some are more equal than others.

[5] Just look at today's photos of highly praised young talents in sciences. In this case, images often say more than 1,000 words.

- *Revenge of frustrated experts*
  Ultimately, the entire publication process is a tedious and humiliating experience for many researchers. Constantly, submitted papers are rejected, and often for reasons that are not comprehensible. One has to be pleased if the reviewers have the grace to make recommendations for a revision of the article. In this case, in order to finally get it published, one needs to (or in fact "must") change the article according to the wishes of the reviewers. This is hardly a pleasant task as it is not uncommon that a revision is done "contre coeur." Therefore it is no wonder that many reviewers are at the same time frustrated authors, who can now pay back to innocent third authors the humiliation they had gone through themselves (Frey et al. 2009, p. 153): *They should not have it easier than us, and they should not think that getting a publication is so easy.* is the tenor. For this reason, articles are often rejected out of personal grudges, and the supposedly objective competition for publication becomes a subjective statement. This is particularly the case when it comes to approaches that are hated by the reviewers (in reality it is often the professor behind the publication who is hated) and they will not forgo the chance to make the life of this author a little bit more miserable.

The perverse incentives created by the peer-review process ensure that the steadily increasing number of published articles in scientific journals often does not lead to new or original insights and, therefore, many new ideas do not show up in established journals. They can rather be found in books and working papers, where there is no pseudo-quality control which hinders innovative ideas. Although the peer-review process prevents the publication of obvious platitudes and nonsense on the one hand, on the other hand it promotes the publication of formally and verbally dressed-up nonsense. The increasing irrelevance of content is the result of artificially staged competition for publication in professional journals. The next section deals with the use of publications and citations as indicators for the assessment of individual scientists and scientific institutions, and explains why we have more and more irrelevant publications.

The Competition for Top-Rankings by Maximising Publications and Citations

Despite the great difficulties involved in publishing articles in professional journals, the number of publications is constantly growing because more and more journals exist simultaneously. These publications are important for the rankings of individual scientists as well as institutions and universities. Furthermore, if young scientists apply for a professorship, the list of publications is usually the most important criterion in the decision process of who will get the post. No wonder that scientists do everything to publish as much as possible despite the painstaking peer-review process. The question as to what to publish, where, and with whom has become essential to the modern scientist. Publication problems cause sleepless nights and the acceptance of an article in a top journal is the greatest thing that can happen in the life of a modern scientist. This is the case, although most of these publications are not of the slightest importance for anybody outside of the academic system. In most articles the opposite of what has been "proved" could also be "proved" and it would not change the course of the world at all.

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: one simply counts all the articles published by a scientist in scientific journals (or counts number of pages) and then gets to the relevant number of the scientist's publication output. However, there is a problem. As we have already seen, the journals differ dramatically in terms of their scientific reputation, and an article in an A journal is worth much more 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 developed, called scientometrics or bibliometrics, which deals with nothing else than measuring and comparing the publication output of scientists. This science has by now obtained its own professors and its own journals, and consequently the measurements are also becoming more complex and less transparent, which then in turn justifies even more bibliometric research.

The most important tool of bibliometric research is citation analysis, which has the purpose of determining the quantity of citations of the specific journal article to be analyzed. Based on this, the effect of scientific articles can be ascertained. The rationale behind this is simple: whoever is much quoted is read often, and what is often read must be of high quality. Hence, the quantity of citations can be used as a "quality indicator" of an article. This quality indicator can then be used to weigh up the articles published in various magazines. Thus, we obtain an "objective" number for a scientist's publication output which then can be easily compared and used for rankings. This is also done on a large scale and university administrators seem to put more energy and effort into these comparisons than into actual research.

The International Joint Committee on Quantitative Assessment of Research, consisting of mathematicians and statisticians, talks in a report dated 2008 (Adler et al. 2008, p. 3) about a Culture of Numbers and sums up the assessment of the situation as follows:

> The drive towards more transparency and accountability in the academic world has created a 'culture of numbers' in which institutions and individuals believe that fair decisions can be reached by algorithmic evaluation of some statistical data; unable to measure quality (the ultimate goal), decision-makers replace quality by numbers that they can measure. (…) But this faith in the accuracy, independence, and efficacy of metrics is misplaced.

This is a warning coming from experts, which we should take seriously. Also, the German Research Foundation (DFG 2002) warned a few years ago about believing too much in quantitative measures (translated by the author):

> Quantitative indicators are comfortable, they seem objective and are (…) surrounded by an aura of hardly disputable authority. Nevertheless, the naive trust in numbers is a fatal misbelief which each faculty (…) should counteract.

However, the similarities between various publication rankings are low because different quality measurements lead to very different results (see e.g., Frey and Rost 2010; Maasen and Weingart 2008). However, "clever" researchers have found a solution even to that problem (see Franke and Schreier 2008). If rankings do not lead to clear results, we should simply calculate a weighted average from the different rankings. In other words, we construct a meta-ranking out of all existing rankings and again we have a clear result. And if in future several meta-rankings should exist, then one can also construct a meta-meta-ranking! Academic excellence at its best!

A measure which has become particularly popular among number-fetishists is the so-called "Impact Factor". This factor is widely used nowadays in order to calculate the "quality" of journals. The Impact Factor of a particular journal is a quotient where the numerator is the number of citations of articles published in that particular journal during previous years (mostly over the last two years) in a series of selected journals in a given year. The denominator comprises of 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 2010, this tells us that papers published in this journal in 2008 and 2009 were cited 1.5 times on average in the selected journals in 2010.

The Impact Factors used in science today are calculated annually by the American company Thomson Scientific; these then get published in the Journal Citation Reports. Thomson Scientific has a de facto monopoly for the calculation of impact factors, although the exact calculation is not revealed, which has been questioned repeatedly (see, e.g. Rossner et al. 2007). *The sciences have allowed Thomson Scientific to dominate them.* (Winiwarter and Luhmann 2009, p. 1). This is even more absurd if, on the one hand, the blessing of competition keeps being praised, but on the other hand, a monopoly for Thomson Scientific is allowed, which enables Thomson Scientific to sell its 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.

A concrete example will show how numbers are fabricated mindlessly. The following proposal is from a 2005 research paper published by the Thurgau Institute of Economics at the University of Konstanz. The author, Miriam Hein (who studied economics) naively propagates a method for measuring quality without being aware of the perverse incentives that this would create. In the introduction we read (Hein 2005, p. 3):

> An intended increase in research performance can probably be induced only by the use of incentive-compatible management tools. Research units and individual researchers who undertake high quality research must [an imperative!] be rewarded, and those who are less successful, should be sanctioned. It is therefore important to identify good and bad research. Well-designed ranking tools can serve this purpose.

The above-quoted section talks about “high quality research” within the same article and a few pages later a proposal for the measurement and calculation of the quality of research follows. The average “quality research” (DQ) of an institution shall be determined according to the following formula:

$$DQ = \frac{FX}{FX_S} = \frac{\sum_i \sum_k \frac{p_{ki} w_k}{n_{ki}}}{\sum_i \sum_k \frac{p_{ki}}{n_{ki}}}$$

Pki stands for the number of pages in publication k of scientist i, n denotes the number of authors of the publication k, and w_k is a quality factor for the article k, which is typically the impact factor of the journal, in which the article was published. Therefore, the numerator shows the quality-weighted research output (FX) and the denominator simply consists of the number of published pages (FX_S). The content of an article, on the other hand, plays no role! The important thing is how long the article is and where it got published. Nevertheless, the just described “quality measure” is seriously praised as progress in quality measurement. The scientist is treated like a screw salesman: The more screws he has sold, the better he is. This attitude is already obvious from the term “research productivity”, which according to Hein (2005, p. 24) *is an absolutely central unit of measure in research management*. Thus, pages published in scientific journals become ends in themselves.

The competition for top rankings established by the requirement for as many publications and citations as possible and the already perverse incentives due to the peer-review process have induced a great deal of perverse behavior among scientists. In particular, the following trends can be observed:

- *Salami tactics*
  Knowing that the ultimate goal is to maximize research output, researchers are trying to make as much out of very little and apply so-called “salami tactics”. New ideas or records are cut as thin as salami slices in order to maximize number of publications (Weingart 2005). Minor ideas are presented in complex models or approaches in order to fill up an entire article. As a consequence, further publications can be written by varying these models and approaches. No wonder that in average the content of these papers gets increasingly irrelevant, meaningless, and redundant. Hence, it is becoming increasingly 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 even more often than that. Such duplication of one’s own research output is of course not allowed, but in reality proves to be an entirely effective way to increase one’s research productivity. As we have seen above, the peer-review process often fails to discover such double publications. Therefore, an anonymous survey on 3,000 American scientists from the year 2002 shows, at least 4.7 % of the participating scientists admitted to have published the same result several times (Six 2008).

- *Increase of the number of authors per article*
  It can be observed that the number of authors publishing articles in scientific journals has largely increased over the 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 in particular have become increasingly complex and that experiments are no longer carried out by a single scientist, 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; this is followed by physics with 4.1 authors per article. In psychology, the average is 2.6 authors per article, while in philosophy, a field 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 of authors per article. On the other hand, there is 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 all team members to include them as authors in all publications of their research team. And the larger the team, the more publications with this kind of "honorary authorship" are possible. Conversely, it may also be attractive to young scientists to include a well-known professor as a co-author because—thanks to the dubious nature of anonymity within the peer-review process—this improves the chances of publication (see above).
  Instead of "honorary authorship" it would be also appropriate to speak of "forced co-authorship", as Timo Rager wrote in a letter to editor of the Neue Zürcher Zeitung at the end of 2008. There we read: *[Forced co-authorship] … exists even at prestigious institutions at the ETH and at Max-Planck institutes. If you protest against it, you are risking your scientific career.* So in addition to the real authors of scientific articles there are more and more phantom authors, who did not actually contribute to an article, but want to increase the number of their publications. In medicine, this trend appears to be particularly prevalent, which also explains why the average number of authors per article in modern medicine is so high. Based on the articles published in 2002, every tenth name in the author list of the "British Medical Journal" and one in five in the "Annals of Internal Medicine", were phantom authors (see Baethge 2008). Furthermore, 60 % of all articles published in the "Annals of Internal Medicine" cited at least one phantom author. No wonder are there clinical directors with over 50 publications per year (see Six 2008), which, if they had really contributed to all these publications, would be beyond the capacity of a normal human being.
  With the number of co-authors, however, not only the publication list of participating authors per article is growing, 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 be quoting this article again, especially if they are again involved as co-authors in another

article: "I publish an article with five co-authors and we have six times as many friends who quote us" (Fröhlich 2006).

- *Ever-increasing specialization*
  To meet this enormous need for publication, new journals for ever more finely divided sub-areas of a research discipline are launched constantly. Thus, the total number of worldwide existing scientific journals is estimated between 100,000 to 130,000 (Mocikat 2009), and each year there are more. By getting increasingly specialized and narrow-minded, chances for publication are improved (Frey et al. 2009). It is advisable to be specialized in a very exotic but important-sounding topic which is understood only by very few insiders, and establish a scientific journal for this topic. Consequently, the few specialists within this field can promote their chances of publication by writing positive reviews in the peer-review process, so that they will all get published.
  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. All of these are scientific journals that deal with wine on a "highly scientific" level, covering topics such as wine economics, wine marketing, or sales. Probably we will soon also have specialized journals for red-wine and white-wine economics and we also await the "Journal of Wine Psychology".
- *Forgery and fraud*
  Last but not least, the whole competition for as many publications and citations as possible leads to fraud and forgery. *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 a fiction. Modern universities are exclusively committed to excellence and the search for truth does not help very much in this respect. No wonder that quite a few cases of fraud have become publicly known more recently.
  A good example is the former German physicist Jan-Hendrik Schoen, born 1970, who was celebrated as the German Excellence prodigy until his case of fraud was discovered. 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 in a scientific journal on average every eight days, of which 17 were 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 German scientific community was proud that they were able to come up with such a top performer. It took some time until co-researchers doubted his results and soon the data turned out to be forged in large parts. A lot of the results were simply simulated on the computer. The interesting thing is, as Reich (2009) writes in her book "Plastic Fantastic", that these forgeries would probably never have even been discovered if Schoen had not exaggerated so

> much with his publications. Otherwise, he would probably be a respected professor at a top university by now and part of an excellence cluster.
> Cases of fraud such as the example of Jan Hendrik Schoen mainly affect the natural sciences, where the results of experiments are corrected or simply get invented. Social sciences often have gone already one step further. There, research is often of such a high degree of irrelevance that it does not matter anymore whether a result is faked or not. It does not matter one way or the other.

The overall effect of all these perverse incentives is that scientists produce more and more nonsense, which adds nothing to real scientific progress. In turn, because the articles also become increasingly out of touch with reality, they are read less and less. Moreover, the increase in citations is not a sign of increased dispersion of scientific knowledge because, presumably, most articles 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. The whole process represents a vicious circle that leads to a rapid increase in the publication of nonsense. In the past, researchers who had nothing to say at least did not publish. However, today, artificially staged competitions force even uninspired and mediocre scientists to publish all the time. Non-performance has been replaced by the performance of nonsense. This is worse because it makes it increasingly difficult to find the truly interesting research in the mass of insignificant publications.

Side Effects of the Production of Nonsense in Science: 'Crowding-Out' Effects and New Bureaucracy

The artificially staged competitions in science for publications and citations, but also for third-party projects (financing), have caused the emergence of more and more nonsense in the form of publications and projects. This is associated with a variety of side effects, some of which have serious consequences. The intrinsic motivation of those scientists involved in research is increasingly replaced by a system of "stick and carrot". Indeed, this is not the only crowding-out effect that we can observe. In addition, a new bureaucracy has evolved which ensures that more and more people employed in the research system spend more and more time on things that have nothing to do with true research. Both effects cause a gradual deterioration within many scientific disciplines, but they are advertised under labels such as "more excellence" and "more efficiency".

Crowding-Out Effects

Some of the crowding out effects triggered by competitions for publications and projects were already previously addressed in this contribution. Here we will show how this crowding-out effects harm universities and the scientific world.

- *Crowding-out of intrinsic motivation by stick and carrot*
  Carrots and sticks replace the taste for science (Merton 1973) which is indispensable for scientific progress. A scientist who does not truly love 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. They are often rather unconventional people who do not perform well in standardized competitions, and they do not feel like constantly being forced to work just to attain high scores. Therefore, a lot of potentially highly valuable research is crowded out along with intrinsic motivation as well.
- *Crowding-out of unconventional people and approaches by the mainstream*
  Both original themes and unconventional people have little in the way of chances in a system based on artificially staged competitions. The peer-review process causes potential authors and project applicants in both the competition for publication and the competition for third-party funding (their projects are also judged by peers) to converge upon mainstream topics and approaches, as novel ideas and approaches get rarely published or financed. However, scientific geniuses were hardly ever mainstream before their theories or methods were accepted. They are often quite unconventional in their way of working and, therefore, do not perform well in assessments which are based upon the number of publications in professional journals, citations, or projects acquired. Neither Albert Einstein nor Friedrich Nietzsche would be able to pursue a scientific career under the current system.
- *Crowding-out of quality by quantity*
  In the current system, scientific knowledge is replaced by measurable outputs. Not the content of an article or a project counts, but the number of published and cited articles or the number and the amount of money of the acquired projects. Since the measurable output is considered to be the indicator of quality, the true quality is more and more crowded out. The need to publish constantly leaves no time to worry too long about the progress of knowledge, although this should be the real purpose of scientific activity.
- *Crowding-out of content by form*
  Closely related to the crowding-out of quality by quantity is the crowding-out of content by form. As content gets more trivial and inconsequential, one tries to shine with form: With complicated formulas or models, with sophisticated empirical research designs, with extensive computer simulations, and with gigantic machinery for laboratories. The actual content of research drifts into the background and the spotlight is turned on formal artistry. For example, it does not matter anymore if comparing different data sets really brings benefit to the progress in knowledge. Important is the sophistication of the method which was used for the comparison of the data sets.
- *Crowding-out of research by bureaucracy*
  This crowding-out effect makes a significant contribution to the new bureaucracy described in the following section. The people employed in research such as professors, heads of institutes, research assistants, or graduate students actually spend an ever-larger portion of their time coping with research

bureaucracy. This obviously includes the time it takes to write (often unsuccessful) research proposals, and later on interim and final reports: This is time a researcher must spend as a price for actually participating in the project competition. In this way, the actual research is paradoxically repressed by its advancement because the administrative requirements no longer permit research. Furthermore, each journal article submitted for publication requires an enormous effort (strategic citing, unnecessary formalization, etc.), which has nothing to do with its content. Bureaucracy, and not research, ultimately consumes most of the time that is spent on writing scientific publications in journals and on carrying out projects that do not contribute anything to scientific knowledge, but have the goal of improving the measurable output.

- *Crowding-out of individuals by centers, networks, and clusters*
  Competitions for projects also cause individual researchers to disappear more and more behind competence centers, networks, and clusters. Research institutions prefer to pump research money into large research networks which are supposed to provide excellence. Scientists see themselves under pressure to reinvent preferably large cooperative and long-range projects with as many research partners (network!) as possible, bringing third-party funds to their institution. Large anonymous institutions such as the EU give money to other large anonymous institutions (e.g. an excellence cluster) where the individual researcher disappears, becoming a small wheel in a big research machine.
- *Crowding-out of "useless" basic research by application-oriented, "useful" research*
  The competition for third-party funded projects is especially driven in this way because it is believed to initiate more and more "useful" research; this will rapidly lead to marketable innovations and further on to more economic growth. In this way, both humanities and basic research is gradually crowded out because in these disciplines immediate usability can hardly be shown or postulated. For example, "useful" brain research displaces "useless" epistemology. However, anyone who is familiar with the history of scientific progress knows that often discoveries which were considered "useless" at their inception led to some of the most successful commercial applications. The at first sight "useless" field of philosophical logic has proven to be absolutely central to the development of hardware and software for computers.

The crowding-out effects described above vary from one scientific discipline to another, but nowadays they can found in almost every discipline. They have become obvious to such an extent that they cannot be ignored. However, politicians and managers in charge of science do not actually care about this because they want quick success that can be proven by measurable output. In turn, young scientists are socialized by the established scientific system in a way that the perverse effects caused by this development already appear to be normal to them.

The Emergence of a New Research Bureaucracy

One of the crowding-out effects that was just described concerns the crowding-out of research by bureaucracy. From the outside, it looks just as if research activities grow at a fast pace. There are more and more people employed at universities and research institutions,[6] the number of scientific publications increases steadily, and more and more money is spent on research. However, the crowding-out effect gives rise to people who seem to do scientific work, but mostly are not engaged in research at all. Most scientists know about this phenomenon. What scientists at universities and other research institutions are mostly doing are things such as writing applications for funding of research projects, looking for possible partners for a network and coordination of tasks, writing interim and final reports for existing projects, evaluating project proposals and articles written by other researchers, revising and resubmitting a rejected article, converting a previously published article into a research proposal so that it can be funded retrospectively, and so on.

It is clear that there is hardly time to do research under such conditions. Project proposals of more than 100 pages are not uncommon today, and the application process at some institutions has become like a maze which only a few specialists can guide you through. An expert in this field, the sociologist Münch (2009, p. 8) writes:

> Staged competitions devour extensive stuff and resources for coordination, for application processes, for evaluation and implementation which eat into actual research work, so that exactly the very best researchers are falling into a newly created control machine and run the risk of drowning in its depths.

The actual research today rests largely on the shoulders of assistants and graduate students whose low hourly compensations still allow them to improve scientific knowledge. In contrast, opportunity costs of doing research are often too high for professors and research leaders, because they can contribute more to the measurable output of their institution by focusing on the organization and management of project acquisitions and publications. In turn, because the postgraduates are in fact often also forced to name their professors or institute directors as co-authors of their publications, the list of publications of professors and research leaders still grows despite their lack of continued research.

However, the above-described increase in the proportion of time that is lost due to the bureaucracy associated with the artificial competitions is only the first step of the new bureaucracy. The second step becomes evident by the ever-increasing number of people working in governmental committees who are in charge of the

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[6] In Switzerland, the total number of employees at universities in Switzerland shows an increase of 24,402 in 1995 to 32,751 in 2008, which is about one-third. Of the 32,751 employees in 2008 were 2,900 professors, 2,851 were lecturers, 15,868 were assistants and research assistants, and 11,132 people were outside of research and teaching in the administration or engineering work (SBF 2007).

organization and the course of these artificially staged competitions. This is essential to both the traditional research institutions such as universities or research institutes as well as to the committees dealing with the organization and financing of research (European Research Council, Federal Ministry for Education and Research, etc.). Artificial competitions have enormously complicated research funding. Universities and their respective institutes do not directly receive money for research anymore. Instead, they have to write proposals for government-initiated research programs, which have to get evaluated and administered. This is a complex and time-consuming process in which each process is associated with complicated procedures and endless forms to be filled out. What is proudly called research-competition becomes, at a closer look, a labor-intensive and inefficient re-allocation of funds from some public institutions (Ministry of Research or the appropriate Federal Agency) to other public institutions (universities, research institutes).

The second step of the increase in bureaucracy is also due to the fact that universities, institutes, and even professors need to be evaluated and ranked. Numbers are needed to decide which institutions or professors are really excellent, and where it is worthwhile to promote excellence clusters or competence networks and what institutions can be awarded the status of a "lighthouse in research". There are already several public agencies (e.g. Centre d'Etudes de la science et de la Technology in Switzerland) and university institutions (e.g. Centre for Science and Technology Studies at the University of Leiden in the Netherlands), which deal exclusively with the measurement of research inputs and outputs on the basis of bibliometric and scientometric research. There the metrics are fabricated which are necessary for the artificial competitions and which form the basis for the rankings and, in turn, stimulate the publication and project competitions.

Research funding has reached its far highest level of bureaucracy by the EU research programs,[7] which appear to be especially sophisticated Keynesian employment programs. None of the "Research Staff" working for the EU actually does research because inventing increasingly complex application processes and new funding instruments already creates a sufficiently large workload for them. Therefore, a large portion of the research is just used to maintain this bureaucracy. Already in 1997, the European Court of Auditors criticized the EU in relation to the 4th Research Program as an "enormous bureaucracy and a useless waste of money". According to experts, only about 60 % of the 13 billion euros, which were provided to the 4th Research Program, were actually received by research institutions. Not much has been changed in the following programs. The responsible coordinator of the Budgetary Control Committee of the European Parliament, Inge Gräßle (CDU), after the end of the 6th Research Program (€ 16.7 billion in 4 years), came in 2007 to the conclusion that "the effort to simplify funding allocation within the EU research framework program has not yet been sufficient." That is an understatement. The 6th Program has invented a whole new, previously unknown form of networking bureaucracy, which has led to much more, and not less, bureaucracy.

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[7] See also Binswanger (2003).

The increase in bureaucracy outside of the actual research institutions also fosters an increase in bureaucracy within the research institutions. The large number of EU-research officers has managed to complicate the application process tremendously. In Germany alone hundreds of advisor posts have been created just to help scientists to make a successful project application. Even if you have completed the tedious work involved in making an application, the chance of success is low. Since the 6th Research Program which began in 2002, the success rate of applications lies between 15 and 20 %, while before the success rate of applications was at about one quarter. In other words, between 80 and 85 % of applications, involving about 12 researchers on average today, are not successful. You can imagine what enormous amounts of time and money are here wasted just to get funding from an EU project.

Of course, the new research bureaucracy has its positive impact on the economy. It creates new jobs and contributes to full employment. Keynes would probably be amazed at what creative level his ideas of a national employment policy have been developed within science. Back in the 1930s, the time of the Great Depression, he mentioned that, in such situations, even totally unproductive and useless activities would stimulate the economy and would therefore eliminate unemployment. The example he gave referred to a governmental construction project in which workers were employed digging ditches and then filling them up again. Even if useless, such a program paid by government funds will cause an increase in the demand for construction activities and thus stimulate the economy. Today's nonsense production in science is much more sophisticated than the nonsense of digging ditches and filling them up again as the nonsense is disguised to the general public.

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