How "Publish or Perish" Can Become "Publish *and* Perish" in the Age of Objective Assessment of Scientific Quality

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ABSTRACT

The point I wish to make is not what we all know: that the methods to assess the quality of research achievement are controversial. I do not wish to call into question the raison d'être of scientometric approach, its methodology or its particular indicators either. Nor am I aiming at coming up with systematic solutions of the contradictions (although I hope to offer some thoughts in that direction later below). Many have called and keep calling attention to the rigid and uniform application of the numerical approach (counting publications), arguing that it is doing injustice to certain areas of science.[1] With that as a starting point, this study is intended to serve two purposes. One, in a much sharper tone than generally used in discussions of the topic, I wish to call attention to how extremely harmful the present scientometric practice may be for many scholars and scientists. Two, also partly in support of the former argument, I propose to demonstrate—to the degree of breadth and depth that the size-constraints of this paper make possible—how the crucial contradiction in question at the core of the present practice follows from the myths generated by scientometry itself.

Here is the paradox: it is the mechanical application and overvaluation of the *scientometric* assessment of research performance, the very *objectivity* designed to guarantee equal and fair treatment that does, in fact, lead to the *devaluation* of quality research effort and discourages even kills the will to conduct research in several disciplines. That is to say, the partly true, part-fun proverbial "publish *or* perish" principle, which urges the research scientist or academic to keep publishing for the sake of career advancement and academic survival, turns into the trap of what we can describe as "publish *and* perish." How a well-intentioned and basically most welcome development, scientometry, or rather, its method of application as well as the myths it generated yield the "publish *and* perish" phenomenon is the subject I will address below.

Keywords: bibliometrics, scientometrics, scientific publications, scientific assessment, Hirsch-index

1. A GLANCE AT THE HISTORY OF SCIENTOMENTRY

Scientometry has its own worthy antecedents. What is known as scientometry today, itself regarded by many as a discipline, is a research field within the framework of information science. Most significant from our point of view was the moment when Eugene Garfield[2] devised the notion of "impact factor" (IF). Impact factor is a measure created on the basis of the average citation journals receive: it is the average citation of articles published by a given journal in two consecutive years, calculated in the third consecutive year by dividing the number of actual citations with the total number of citable articles the journal published in those two years. The higher this ratio (the impact factor) is, the more "effective and the higher ranking" the journal is regarded to be, therefore the more desirable forum for publication it is for the professionals of the field. [1] At present journal impact-factor calculations are based on the citation database services of the Thomson Institute for Scientific Information (formerly: Institute for Scientific Information, ISI). The lists of *Journal Citation Reports* (*JCR*), published annually as supplementary volumes to *Science Citation Index* (*SCI*) carry the impact factors of the journals indexed by ISI. The lists are available on the internet, on a strictly subscription basis. In sum, individuals do not and cannot have impact factors, only journals can and do.[3]

Scientometrics, the first journal of scientometry in the world, was published by Springer Netherlands in 1978, with Tibor Braun (doctor of the Hungarian Academy of Sciences in chemistry[4] and advisor to the vice-president of the Academy) as its general editor. A highly relevant document in our context, the declaration of the American Society of Cell Biology appeared in 2012, entitled "San Francisco Declaration on Research Assessment: Putting Science into the Assessment of Research (DORA)" pointed out that the journal impact factor approach has its deficiencies and should not be used for the quality assessment of scientific research. They declare that

> "The Journal Impact Factor is frequently used as the primary parameter with which to compare the scientific output of individuals and institutions. The Journal ImpactFactor, as calculated by Thomson Reuters, was originally created as a tool to help librarians identify journals to purchase, not as a measure of the scientific quality of research in an article. With that in mind, it is critical to understand that the Journal Impact Factor has a number of well-documented deficiencies as a tool for research assessment" [2]

The Declaration also offers recommendations: "the need to eliminate the use of journal-based metrics, such as Journal Impact Factors, in funding, appointment, and promotion considerations; the need to assess research on its own merits rather than on the basis of the journal in which the research is published; and the need to capitalize on the opportunities provided by online publication (such as relaxing unnecessary limits on the number of words, figures, and references in articles, and exploring new indicators of significance and impact)" [2]

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[1] inanimate (physical) natural sciences and mathemathical sciences, anim ate (life) natural sciences, human- and social sciences

[2] Eugene Garfield (1925-2017): American linquist, chemist, librarian, on e ofe the founders of bibliometrics and scientometrics. He founded the Institute for Ssientific Information (ISI), *The Scientist*;and is responsib le for *Current Contentsm* the *Science Citation Index (*SCI), the *Journal Citation Reports*

[3] In his "Escape from the Impact Factor, Ethics in Science and Environm ental Politics," published in *Nature*, editor in chief Philip Campbell ex presses his concern regarding the growing practice of focusing on the j ournal IF when evaluating scientists' publications. *Nature* 2008/8, 5.

[4] Doctor of the Academy is a post-Ph.D. doctorate, a title awarded by the Hungarian Academy of Sciences, based on a new dissertation, tough s creening measures and requirements, as well as a public defense in the Academy.Tibor Braun was awarded that doctorate in chemistry in 198 0.

The Academia Europaea discussed the Declaration and supported its general objectives. What deserves mention from their recomendations:„the current bibliometric systems are not generally applicable." Their use for large multidisciplinary organizations such as universities is therefore inadequate. Several disciplines, such as engineering, mathematics, most of the social sciences and humanities work with other publication cultures because of the different character of their societal mission, forum, and target groups. This implies a far greater variety of publication formats in patents, reports, national journals, books, and the use of a great number of national languages. In these disciplines. Anglophone researchers may even be uninformed about a considerable body of knowledge published since long in other languages. [3]

2. THE PRESENT STATE OF AFFAIRS

What prompts us in the world of science, then, too is the urge to surive, the need to compete (with ourselves and others). If you publish more, you will be "stronger" in numbers, i.e., you will have higher scientometric indicators. But *do* indicator-figures really tell us anything about the scientific *quality* of research performance? Is it not scientific quality that should matter? János Marton formulates a universal truth: "Publication is the means of survival for the scientist, a kind of a monument. [4] By our days this has been supplemented with drastically practical aspects. Measuring research performance and scientific visibility has been extended to every researcher since some external forces are more and more compelling: the need to define performance in terms of numerical indicators is now a requirement also in the race for research funding, besides being required in promotion procedures in academic environments.

As I know the present situation relatively well in my own country only, the state of affairs in Hungary will be my basic point of departure. The general view in Hungary (and not only in Hungary) is that many publications as well as a high number of citations of those works in the literature make the successful and recognized scientist, they guarantee scientific eminence. Moreover, doctoral schools, schools of habilitation and the scientific committees of the Academy that screen requests to be processed for doctorates of the Academy take journal impact factor into consideration more and more, although to a varying degree. In recent years the only officially acceptable list of publications can be the one generated in the Academy's MTMT system (the Publication Database of the Academy[5]). It is the only list of publications you can attach to doctoral, habilitation or doctor-of-the-Academy applications. (More about MTMT below.) At present the following indicators are taken into account: number of publications, number of citations, journal impact factors, and Hirsch index. The definition of H-index is: "A scientist has index h if h of his or her N_p papers have at least h citations each and the other ($N_p - h$) papers have $\leq h$ citations each". [5] A researcher with a high H-index published an article, which elicited a high number of citations.

Positions in Hungarian higher education are modeled after the German system, and each of the appointments require their degree or title, i.e., higher and higher levels of scientific achievement: instructor, assistant professor (Ph.D. is required), associate professor (habilitation is required), full professor (the title of doctor of the Academy is required). Regulations of a degree or title show considerable disperson across the areas of science, but lists of publications play an important role all across the board. What is more, publishing in high impact-factor journals is often an expectation to be fulfilled for a Ph.D. degree too. In Pedagogy (University of Debrecen), for example, doctoral candidates must produce at least five articles published in high-quality national or international journals and/or refereed books before they are allowed to defend their dissertation. One of the five must have been published in an international journal indexed by SCOPUS (high impact factor). The mathematics and computer science doctoral program prescribes two international publications for the Ph.D. candidate (one with an ISBN or ISSN number that makes it into a refereed database with an impact factor); or, three publications, one of which must have been published in a refereed international journal and two in a Hungarian but refereed foreign-language journal.

Closely related to our topic is that Q-ranking (a SCOPUS-database-related SJR metric: Scimago Journal Rank) is also gaining momentum. It can be followed with the help of the presige-indicator in the Academy's Publication Database, MTMT.

Publications are classified in four quantile categories: Q1 – excellent journals, the upper 25 % of professional ranking; Q2—good journals, 50-75% of the ranking range; Q3—mediocre journals, 25-50%; Q4—weak journals, 25%. The quantiles are reviewed every year. One journal can be relegated to more professional fields than one.

3. THE HUNGARIAN ACADEMY OF SCIENCES AND ITS NATIONAL PUBLICATION DATABASE: MTMT

To maintain and operate MTMT is the responsibility of the Academy as regulated by the Academy Law. It was clearly conceived with the intention to create a multipurpose national publication database, in which every scientist has his or her data sheet and can handle his or her research performance on a uniform surface. It is a central, nationally uniform system; its data can be used for many purposes; they can meet internal evaluation demands (doctoral programs, habilitation, statistics, internal applications); it has a quality assurance system; and is run by a nonprofit organization. It has its advantages from the point of view of the scientist too, as s/he can put together a personal performance bibliography; can continuously maintain it; can fit it into a personal webpage; Hungarian grant application systems recognize only this bibliography; and its format is widely known. Besides these and many more positive features, the database is not exempt from negative features and anomalies as László Csaba and his coworkers clearly established (Csaba et al 2014) [5]: not infrequently the wrong application of scientometric indicators can be detected; the classification of some research publications is mistaken; the calculation of journal impact factors and their application to evaluate individual scientists' performance is misleading. Moreover, scientometric indicators as used in MTMT can form the basis of arguments, even belittling remarks which hurt the feelings of especially, though not exclusively, humanities and social sciences scholars and discriminate against them". [6]

MTMT describes itself as an authoritative registry of scientific output; it provides valid lists of publications (validated by MTMT's librarians); it serves as a transition to full-text repositories; and provides a combined picture of the scientific output of research institutions. MTMT's list of journals enumerates the journals in which Hungarian researchers mostly publish. The list keeps expanding as scientists indicate newer and newer items to be added. Important data that identify the journal are enlisted: ISSN number, the character of the journal, whether it is refereed or not, the open-access format, predator journal or not, impact factor if any (the years of its calculation), language of publication, and how many of its publicattions were entered in MTMT.

The classification of journals (scientific, not scientific, refereed, not refereed) is not the scientist's individual responsibility since the classifications are there in MTMT. However, if a researcher published an article in a journal which is not included in the list, the relevant scientific committee of the relevant section[6] of the Academy will propose MTMT

[5] In literal translation: Database of Hungarian Scientific Works

[6] The scientific sections of the Academy are: I. Linguistics and Literary S tudies Section, II. Philosophy and Historical Studies Section, III. Math ematical Sciences Section, IV, Agricultural Sciences Section, V. Medi cal Sciences Section, VI. Engineering Sciences Section, VII. Chemical Sciences Section, VIII. Biological Sciences Section, IX. Economics an

management to include the new journal item in the list and the committee will also decide about its classification. Thus the journal list of a field of research is compiled and ranked by the scientific committee of the given discipline, adhering to the principle of fairness. Journal quality is measured with categories ranging from A to D, with the actual Q-ranking (prestige) also indicated, if such ranking is available in a given case.

In 2015 Péter Sasvári and András Nemeslaki conducted an empirical survey of what this all looks like as praticed by the Economics and Law Section of the Academy. Availability, meshing with international ranking-lists, and actual publication output were studied. A good number of anomalies specific to the area of science were demonstrated, supported with empirical measurement evidence. For example, the databases were not generally available so the specialist journals are not available for every researcher. Whereas the close conformity of the A-D-ranking of international journals to Q1-Q4 classification was pointed out as a positive feature, in surveying actual publication output, the results high-lighted the serious problem that it is barely two-thirds of the international journals indexed by Scopus and WoS in which our social sicentists[7] publish (or they do not publish in these journals at all). Another profound problem is that social scientists publish in their national language more frequently, which means the lack of internationalization of the field. [7] Consider modern philology, a branch of humanities. How can one calculate the "objective" scientometric measurements of morern philology publications applying the methodology of the present practice when the researchers of these fields publish in their national language to a great extent, and those languages are so-called small languages—the scientific product being, e.g., English studies *in Hungarian*, French studies *in Hungarian*, Russian studies *in Hungarian*, and so on?

The (general) tables of MTMT distinguish four main types rated as scientific: journals, books, book chapters, conference proceedings in journals or conference volumes. Everything else qualifies as *non* scientific publication. A cross-disciplinary anomaly presents itself here too, since one book counts as one publication just as any other scientific genre (e.g., a journal article) does. It is known to all that there are disciplines (e.g., in the humanities and social sciences area of science) where the most highly valued and generally expected form of research performance is not a journal article but a monograph study. And producing a book and preparing it for publication is immensely more time-consuming than publishing an article (it goes without saying that the time pre-writing research takes can widely vary in both cases). Add to that the indexing practice of the huge citation databases (Scopus, WoS): they characteristically index journal articles, so in "keeping count" of citations, researchers who publish books are seriously disadvantaged as opposed to their, say, natural scientist fellow scientists. Scientific visibility, therefore, is much more modest in the case of those who write scientific monographs and textbooks. Also, a book qualifies as scientific, understandably, if it is published by a nationally recognized publisher that processes its manuscripts through strict professional filters. Smaller departmental publications and privately published material are not regarded as scientific. Consequently, humanities and social science researchers often publish journal articles when a topic would call for a book-length treatment. An article or a study as a genre is not always the best vessel (not an adequate size) for the detailed and thorough development of a humanities or social science topic after all.

The "publish or perish" principle does underlie the present situation, then. After all, the number of publications is one of the most important indicators for every procedure (for a scientific degree, a title, for promotion, or a successful fund-application). The situation starts to lean towards the "publish *and* perish" trap when the number of publications and the prestige of forums of publication become exclusively or too importantly decisive; and if the present treatment of the humanities and social sciences remains: i.e., disregard for the genre of publication (only numbers matter—one monograph counts one publication just as a journal article does) and paying no heed to the characteristics of publication and citation specific to the area of science.

Besides, if a scientist is outstanding in terms of numbers of publication, this circumstance should not automatically secure for him or her, say, promotion. Nor should the circumstance that his or her scientific output satisfies or splendidly exceeds the expected number of national and international number of citations (for required national and international citations smallest minimum numbers are prescribed by various regulations, especially by regulations of the Academy). And those must be independent (i.e., foreign) citations. MTMT-tables group numbers of independent and non-independent citations separately. A citation is non-independent (partial) if it comes from a coauthor or if it is a self-citation. Needless to say, the various sections of the Academy do have their algorithms to generate their own tables, where they do their best to take into account the specific features of their area of science. They assert their area-specific requirements most intensely in Ph.D. procedures, and that often too rigorously. (What I referred to briefly above about Ph.D. candidates and young faculty required to publish in high impact factor journals is not required even by leading American research universities or by those recognized as leaders in doctoral training.) From habilitation onwards the system is characterized by a growing degree of uncertainty. There are simpler cases, more on the unambiguous side (e.g., natural sciences) and muddled ones, more on the ambiguous side (like interdisciplinary fields). The tension is further increased by the disciplinary divergence and constant (annual) shifts in the Q-ranking of journals.

It is easy to understand from the above that—when viewed in a Scopus mirror—the disproportion of scientific-area representaion in Hungarian scientific publication output is striking. Natural science publications constitute nearly half of total publications (48,52%), and the often overlapping articles produced by health- and life-sciences only just fall short of the 50% mark, while the share of social science articles is less than 5% (4,57%). Furthermore, articles that involve more than one area of sciences and indicated as multidisciplinary consitute half percent of all publications. [8]

4. BELIEFS AND MISBELIEFS, DIRECTIONS AND MISDIRECTIONS

The above brief look at the historical development of scientometry can also serve as an explanation for the present situation. Scientometry started out with statistical measurements of studies published in natural science journals (anatomy, chemistry, physics). The inquiry into the functional mechanisms and the laws of scientific research (thus primarily of natural science research) and the convictions as well as beliefs and disbeliefs that developed as a result have their root here. This is how the beliefs and disbeliefs that Wolfgang Glänzel calls "the seven myths of scientometry" came about: "the myth of delayed recognition","citing yourself is blowing your own trumpet","collaboration is always a guarantee of success","citations are measures of 'scientific quality', "reviews are inflating impact,"non transit Gloria mundi," "don't use averages in bibliometrics.". [9]

Generating and fostering the Glänzel-myths, the series of abusive use of scientometric data and especially their mechanical application to disciplines to which this method would not be applicable, or should be applied differently, exerts a negative impact on research in those disciplines because it can determine science policy, which will handle the allocation of research funds according to preferable and non-preferable categories—based on "objective indicators." No wonder that more and more researchers speak up against the scientometric approach, often questioning its methodological grounding even. No wonder, since you keep working (and publishing) and you still perish. The addiction of

d Law Section (including sociology, demography, and political science), X. Earth Sciences Section, XI. Physical Sciences Section.

[7] "Social sciences" is often used in a broader sense, incorporating human ities too.

scientometry to the piecemeal approach and the mechanical application of that philosophy pushes the "publish or perish" experience of researchers towards the "publish *and* perish" feeling for many researchers of several mishandled disciplines. As indicated early in this paper, the phenomenon hides a *process*, whose forms and stages can be uncovered in the very myths Glänzel identifies.

The myth of delayed recognition

In this case "myth" happens to be a mind-boggling complexity that needs to be clarified. The idea of delayed recognition is used as a defence by those who are hurt by scientometry's short-term (3-5 years) evaluations and argue for long-term evaluations since, as Glänzel explains, some papers may not be cited or poorly cited for a few years but highly cited many years later. [9] But the argument of delayed recognition is not taken seriously by natural-science-based scientometry that counters it by declaring it to be a myth. However, this countering scientometric reaction asserts something, which is itself a myth after all: namely, that delayed recognition does *not* exist. The fact of the matter is that it *does* exist and cannot be measured with citations of the first three to five years following publication. So to allege that delayed recognition does not exist, that it is a myth—is itself a myth. In several fields of science, recognition takes time. The phenomenon is what Garfield himself calls delayed recognition. [10] It means that there *is* such a thing as delayed recognition according to the father of scientometry too. But we can also contemplate the issue from the other end of the equation: the ageing of articles. What we find is that the pace at which scientific results age varies from field to field; i.e., recognition and ageing can indeed be discipline-dependent. Nevertheless, using long-term statistical analysis of the recognition of individual articles, Glänzel and Garfield deem the field-dependence of delayed recognition an unjustified view. The examination of 450 thousand scientific articles proved that delayed recognition is mostly unrelated to disciplines. [11] On the other hand, in another work Glänzel and Schoepflin contend [12] that the change that citation can undergo in the course of time is also an important indicator and can be measured. And this context is clearly indicative of field differences. Time-related citation metrics tells us that social sciences, applied sciences, and mathematics are much slower to sink into oblivion than experimental sciences and life sciences are. [9]

It seems that the longer the term of citation is, the less the possibility of the mistaken evaluation of an individual scientific article's recognition is. It is also true, that if viewed statistically, the early phase of citation determines the tendency to follow. [9]

In sum: delayed recognition is not a myth. If we do think it is, though, we do short-term evaluations, thereby creating a disadvantaged position for the representatives of those disciplines whose achivements become obsolete at a much slower pace. Their relevant citations should be calculated and evaluated in at least ten-year cycles (instead of the present practice of five-year windows); and we should somehow put an end to the unfair measuring practice according to which one monograph equals one article.

Citing yourself is blowing your own trumpet

The myth here, Glänzel explains, is that self-citations "are used to manipulate impact […], they are very harmful and must be removed from the statistics". [9] He also points to Narin and Olivastro (1986) on information science "where a reasonable share of author self-citations is considered a natural part of scientific communication," "are quite inevitable in large research projects and prevent authors from repeatedly copying larger parts of earlier publications." Glänzel himself is convinced that "there is no reason to condemn self-citation in general". [9]

What seems really harmful here too is to lump everything together indiscriminately. Those who propound theories, for one, should be treated differently, no matter whether they are humanities and social sciences scholars, researchers of life-sciences or inanimate natural sciences. Theories can be of the nature of taking shape and being developed in a series of works. Why should not a theoretician be allowed to cite his or her own theory (a novel subject area that s/he alone developed and nobody else contributed to it up to that point), an earlier stage that s/he proposes to develop further in a new article? Who else could s/he cite when s/he is the only scientist of his or her topic on the scene? Would it make sense to argue that self-quotation is unethical in such cases?

Collaboration is always a guarantee for success

"Multi-authorship and above all international collaboration increases productivity, visibility and impact. It also facilitates publication in high-impact journals." So Glänzel's definition of this myth goes, with his added points that collaboration itself is not a quality criterion; that it is difficult if not impossible to to fix "the degree of individual co-authors' contribution to the paper," especially because there are "cases of suppressed, fraud, honorific, hyper-authorship or even 'mandatory' authorship". [9] Some researchers detect an „inflationary process" in some of this (honorific and hyper-authorship). Glänzel quotes Persson et. al. [13] on how "the number of (co-)authors is increasing faster than the number of publications indexed in the Science Citation Index (SCI) database of Thomson Scientific". [9]

While the very nature of a research project may indeed call for collaboration in several fields of science, especially in international projects, putting the community of authors, justly or unjustly, in the light of a mutually supportive citation alliance, this is a way of generating high numbers of citations, something not available for every discipline. In humanities and social sciences research projects single authors are more typical, organized research teams are less common. It means that the time and energy required by the project is not divided among co-workers; there are no parallel projects and with them the possibility that the researcher partakes in all of them, even if to varying degrees; and all project reports and project citation increasing the participating individual co-worker's scientometric indicators—is not an option for a single author. That is to say, the myth of collaboration as a guarantee for success reinforces the practice of quantification and sentences the solitary researcher to perishment, figuratively (and academically) speaking. There is no way for the solitary scientist to be competitive against those whose name circulates with great frequency in research teams and fund-applications of such teams. The fast-growing scientometric indicators of a team create an advantageous position for co-workers of whom it is impossible to know if a cited aspect or idea was *their* contribution (or theirs too) or not when a team publication is cited. Is it fair at all that an individual team-member should record to his or her own credit the citations that the team as a whole earns?

Citations are measures of "scientific quality"

Glänzel's sentence—"journal impact factor has become the common currency of scientific quality"—is a modified variation on Garfield's ("Citations are more and more considered the currency of science"), whom he also quotes. [9] The essence of his view is this: "In spite of their statistically evidenced correlation with quality related aspects, citations in general, and impact factors in particular are and remain primarily indicators of reception of scientific information. The possibility of measuring scientific quality of individual publications through citations alone is a myth". [9]

Let me add to that an elementary but logical question: why cannot we stick to the fact in this case too? The fact of the matter is that if the scientometric indicator of a publication is zero or very low years after its publication, that contribution simply *remained unreflected*. Because that is a fact. But that this unreflectedness would have anything to do with the *quality* of such an article is an arbitrary assumption, not a fact. On what basis are we entitled to infer that the problem is with its quality?

What run counter, to some extent, to the scientometric permissiveness my question implies are convictions like those expressed by Tibor Braun and his co-workers [14]: "if no reference is made at all to the paper during 5 or 10 years after publication, it is likely that the results involved do not contribute essentially to the contemporary scientific paradigm system of the subject field in question" (quoted by Glänzel [9]). This is a careful wording: does not contribute *essentially* to the *paradigm system.* On the one hand, yes, such a contribution can be utterly insignificant, although there is a good number of subject fields that are vast in themselves, but are so remote from the contemporary social highways of the big issues that most demanded science research travels, and are cultivated by so few that even research topics rarely meet, not mentioning the problematic nature of establishing a research *paradigm*. Still, sudden twists and turns of history can foreground the importance of such research. Relevant examples could be those who study remote languages and cultures of Africa and Asia, suddenly repositioned in these days of the so-called migration crisis. On the other hand, as Thomas Kuhn's well-known theory tells us, paradigm-shifts are built up (it may be a belated realization, true) through contributions that *do not conform* to the ruling paradigm system.

To sum this section up, if impact factor is the order of the day, therefore researchers rush to publish in journals with the highest IF, and thus "reaching the targeted readership has become a secondary aspect," as Glänzel notes [9] —albeit the targeted readership would be the valid arbiter of quality—the quantitative approach to evaluating quality can be evaluated to be a failure.

Reviews are inflating impact

"Reviews are always highly cited, and do therefore inflate citation impact. […] They should be removed from bibliographies when used for evaluation." To his definition of this bibliometric myth Glänzel adds his countering arguments: statistically, "the weight of reviews is rather limited"; "by far not all reviews are highly cited"; "preparing reviews requires experience and contribution to the advancement of the corresponding subject"; reviews "play a serious role in scholarly communication". [9]

In my country the myth both does and does not hold. While the MTMT system does incorporate the researcher's reviews, they are not accepted as scientific works, and for this reason, are discounted as "not scientific." On the other hand, as opposed to the reviews that a scientist produced, the number of those that review his or her own publications *do* count and are expected to be included in his or her citation profile.

The review-related bibliometric myth is another one bleeding with contradictions. Firstly, review and review essay are lumped together as "review" whereas a brief review report should be distinguished from a review essay (even Glänzel does not make the distinction). The latter needs thorough grounding in the reviewed subject in breadth and depth; consequently, the best representatives of the subject field write the review essays, often developing important own views apropos of the reviewed subject, views that do deserve attention and are real contribution even, in ideal cases. Second, let tus turn to the reviews whose number needs to be quoted when a researcher is being evaluated. Again, only the number? What the quantitative approach of scientometry cannot handle here is the heart and soul of the matter: a review, in most cases, evaluates the *quality* of a publication!? What if the review is negative, harsh, rejective? It still counts as one review?

There is one more circumstance, which tips the scales in favour of "publish *and* perish." If a highly esteemed scientist of a discipline honours a publication with writing a review essay about it, s/he goes out of his or her way to do it, brings top-level expertise to it, devotes time and energy to it—it does not count as a publication because it is not classified as "scholarly" or "scientific," certainly not by MTMT. What happens here is that the most authentic reader, another expert of the same field, is discouraged from performing what should be a basic duty: to evaluate a fellow-scientist's work, or, s/he can regard it as a punishment if s/he does, since the review essay as a genre does not count, it takes his/her number of publications and citations nowhere. Is it because of another myth—if I may add one to Glänzel's seven—that sweeps aside personal quality judegement, even of the most recognized representatives of a given discipline as *subjective*? Do we thereby question the expertise of those most recognized professionals and stick obsessively to numbers simply because they are "objective"?

Non transit Gloria mundi, or, the myth of ever-lasting citation impact

Simply put: "once highly cited, is always highly cited," the number of citations of a highly cited work will keep increasing even if the author does not publish new items. Glänzel calls the attention of self-complacent believers of the myth to some instances and factors which warn us that citation impact is not necessarily everlasting, the frame can be "transient." His instances are "retractions of invalid or fraudulent work." The factor that will not let the once highly cited scientist relax is the "the reality of the virtual web world" in our days, "where literally everything is in continuous change … and scientists have to defend and reconfirm their position in the community day by day". [9]

The myth can be invalidated or at least weakened since rankings (IF, Q) are constantly changing, and can be constantly followed on the web. Nonetheless, the myth is very much with us. And the methods to "secure" high citation impact can be rather peculiar. Let us put it this way: not necessarily ethical. In some disciplines a full professor, for example, can appear as co-author in each and every publication of the research projects headed by him or her, even if s/he did not actually contribute to the publication, under the pretext that the professor's name in itself raises the publication to a higher standard. Again, research teams are in a much better position to generate and maintain—long-lasting if not ever-lasting—high-level citation impact. As we see, teams have ways to "manage" their citation impact, especially from a leading (power?) position.

5. Summary and conclusion

The* conformist *response to scientometric evaluation: contradictions

Scientometry incites researchers to publish as much as they can, and the citation impact of those publications should be as high as possible. They cannot but do so since this is the only way to advance in academic ranks. So far it is the "publish or perish" philosophy that underlies this mentality. However, if scientists take quantitative evaluation seriously and relate to the omnipotence of the scientometric indicator with conformity, and would also like to elude the trap of "publish *and* perish," devoted research work is not enough. The following must be taken into consideration too.

1. They must primarily aim for publishing journal articles because the quantitative method regards a journal article equal with a monograph (as it has no regard for disciplinary differences). And here come the predator journals with the dangers they represent. These forums of publication were also brought about by the demand ("publish or perish") created in the world of science, not mentioning their profit-oriented background. But an article published in a predator journal is not accepted as a scientific publication (no matter how high its IF-number), nor are the citations related to it. So the increased number of publications so temptingly promised by the predator forums backfires.
2. Scientists must choose a satisfactory journal, check its impact factor and its Q-ranking in the relevant field of science. If they go about it like this, can we be certain that they will decide on a forum of publication which suits the given subject area best, is attentive to the special requirements of that area of science, will attract the attention of most of the best professional of the field, because it is one of the generally recognized forums of publication in that field of science?
3. It is not enough for the researcher to pay attention to all of these because IF- and Q-ranking keep changing year to year. The IF- and Q-ranking of a journal can change easily by the time the article is actually published in it. Therefore scientists must be aware of the larger-system picture of IF- and Q-value changes so as to avoid journals in whose case change in relevant values is most likely. It is a big question, though, if constant monitoring of how these values keep changing in all the journals relevant from the point of view of the scientist's research interest is affordable in terms of time and energy? If such monitoring is accomplishable at all? Partially, perhaps.

4. So as to boost their scientometric indicators, and to insure greater visibility for their publications, scientists must make their work available in various repositories and academic social networks (ResearchGate, Academia.edu, Mendeley) and must keep monitoring their data. The question poses itself here too: how much of scientists' research energy and time should be consumed by managing their former publications like this, instead of launching new projects, researching new topics?

The* nonconformist *response to scientometry: dangers

1. Nonconformist relation to scientometric indicators means that highly esteemed scholars or scientists do not yield to the pressure, do not fall into line with a system which ignores their field of science and applies standards that are alien to the basic nature of that field. They keep publishing in journals that are regarded as relevant by the given profession and they do not bother with impact factors and Q-lists; and publishing books remains a priority for them. What it means is that while nonconformist researchers' work is regulated by standards that their profession requires and applies, they ignore the scientometric consequences that nonconformist habit entails. They may receive the most reverant recognition from the professionals of the discpline, but they jeopardize their own visibility, promotion, thereby the quality-assessment ranking of their department too and fail to comply with the qualification requirements drawn up by the national accreditation system both with regard to institutional accreditation and subject accreditation. Nonconformist researchers thus find themselves in the trap of "publish *and* perish"—and, it is clear from the foregoing sections of this paper and need not be specially proven: they end up in that trap through no fault of their own.
2. The processes detailed above frustrate and discourage nonconformist scientists who will focus on teaching and will not waste extra energy on increasing their scientometric indicators. The nonconformist's is one way an academic researcher's career can be wrecked.
3. A variation on the nonconformist theme is if scientists keep the rules and aspire to fulfil quantitative and qualitative prescriptions alike: to produce at least the expected number of publications, to keep up with IF-changes and to publish in the best journals. But having high standards in terms of scientometry too slows down research and publication output as it is highly demanding in time. Scientometric indicators will be lower through the loss of research time and energy, thereby underrating such scientists—remember! "quality" is numbers for scientometric evaluation. Such researchers keep publishing, spurred by intrinsic standards, satisfying the expectations of their branch of science, and guided by scientometric quantitative requirements too. Still, they may end up lacking in scientometric indicators, and the scientometric verdict will be: not up to the quality requirement—and everything *that* involves. Another way of putting it: "publish or perish" will swing into "publish *and* perish."

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