Seven Myths in Bibliometrics. About facts and fiction in quantitative science studies

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Abstract

Seven myths with cognitive and methodological background are analysed for validity. Although, there is always a grain of truth in bibliometrics myths too, the generality of their statements is disproved on the bases of methodological studies and by referring to typical counterexamples. It is shown how and where the logical fallacy lies in the inference from the reality behind the myths leading to the erroneous generalisation of the actual statements.

1 Introduction

The sharp rise bibliometrics took since the 1970s and, above all, the various changes in the fields of application bibliometrics has undergone during this time, have fostered a number of myths which seem to pertinaciously persist. When scientometrics still was a tool in the service of scientific information or, at most, of national research reports, the scientific community was less sensitised to rumours and myths around useful or even harmful aspects of quantitative science studies. However, researchers became more susceptible to the consequences of bibliometric practice after quantitative methods have made their entrance into the every-day evaluation of research teams and individuals and into formulas for the allocation of funding. Scientists even feel sometimes victims to the evaluations which are usually performed by bibliometric semi-professionals. Thus, beyond the usual excuses, readily found in the case of unsatisfactory evaluation, communication problems lead to attempts to challenge, undermine or even to dismantle the methodologies underlying the evaluation procedure. Fostering, disseminating and extending existing myths is probably one of the possible repercussions on policy use and misuse of bibliometric data. And just as in the case of other myths, there is a grain of truth in bibliometrics myths as well. In the present study seven of the most popular myths are selected and analysed for supporting facts. It is shown that, beside the above-mentioned grains of truth, the simplified statements conveyed by these myths rather belong to the realm of fiction.

2 The myths

Most of the bibliometric myths have a cognitive, methodological or technical aspect. The following selection of seven myths is restricted to those with cognitive and methodological background. The plethora of myths or rather rumours regarding technical and application-related questions are mostly a consequence of evaluation practises, have therefore a rather local character, and do not necessarily put forth ‘univer-

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sal' statements. The topics and statements of the seven myths are as follows.

2.1 Myth #1: The myth of delayed recognition

According to this myth, there are many papers that have initially not been cited or been poorly cited, but have become highly cited later on. Citation windows of 3 to 5 years or shorter are therefore definitely too short.

An often-heard argument against the practice of using citation indicators in research evaluation is that important research results and, among those, also breakthroughs in science are often not cited in the beginning, and only become recognised in a time that is beyond the standard citation windows used in most bibliometric studies. This phenomenon is called *delayed recognition* (e.g., Garfield 1980).

Another argument refers to subject-relation citation delay. Scientists state that standard bibliometrics may not be used to assess their publication output, or may at least not be applied to their research field because of field-specific slow ageing and citation delay.

This myth evolved from these two different issues. Nowadays these two components superpose. The first one, the myth of delayed recognition itself, can be easily dismantled by long-term citation analysis. This applies, of course, to the citation delay of individual papers. In order to identify such papers and to shed some light on their role in scientific communication, we analysed the citation histories of the 450,000 research and review articles indexed in the 1980 edition of the *Science Citation Index* (SCI). Delayed recognition papers were defined as those which, during a period of five years, were initially rarely cited but then became highly cited only beyond this period. In particular, highly cited was defined as at least 50 citations or 10 times the journal's 20-year cumulative impact factor till 2000. The chance that a paper, uncited or very poorly cited for three to five years after publication, will ever be cited is quite low, even in slowly aging fields such as mathematics. Among initially poorly cited papers, only 60 could be considered highly cited during the subsequent 15 years. Thus, a statistically marginal share of 1.3 per 10,000 papers published in 1980 met the criterion of delayed recognition (cf. Glänzel et al. 2003; Glänzel and Garfield 2004). Among these papers we have found important contributions, which could even be considered breakthroughs, but these cases are indeed the exception to the rule. Very few individual cases do not stand as *pars pro toto*.

The phenomenon of delayed recognition is largely independent of the particular subject field (cf. Glänzel et al. 2003). We find delayed-recognition papers is practically all fields. The second issue, however, which is statistically more relevant and as such also more complex, is intimately connected with the phenomenon of ageing of scientific information. Field-specific differences in the ageing of scientific literature can be measured by means of the change of citation impact in time (see Glänzel and Schoepflin 1995, 1999). The ageing of social science, applied sciences and mathematics is distinctly slower that that of experimental sciences and the life sciences (cf. Figure 1).

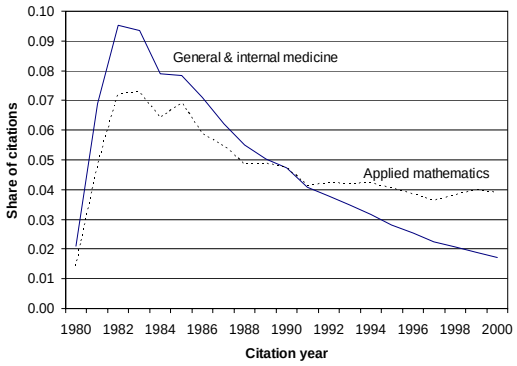

Figure 1 Different subject-specific ageing of scientific literature

Beyond doubt, a preferably long citation window increases the reliability of assessments. However, one should keep in mind that the application, for instance, of a ten-year citation window already refers to research done at least twelve years ago since various time related considerations come into play. One indeed has to add the time necessary to conduct the research, the time to organise and to condense the results obtained into written documents, the time for the reviewing process, a certain publication delay dependent on the journal and field where the paper is published (e.g., Roland and

Kirpatrick 1975; Luwel and Moed 1998) as well as the time for indexing the most recent citing literature in the citation index, and finally the time for processing all the necessary bibliographic information. At a lower aggregations level, such as the level of research groups, this might become critical since normally the constitution of a research team has considerably changed over a period of 10 years or more.

From the statistical viewpoint, the citation impact received in an initial period determines the later citation history (see Glänzel and Schubert 1995; Glänzel 1997). The reliability of prediction increases, of course, with the length of the underlying observation period and decreases with the length of the interval to be predicted. The results of these studies suggest the use of a three-year citation window as a good compromise between the fast reception of life science and technology literature and that of the slowly ageing theoretical and mathematical subjects. As shown in Figure 2, a three-year citation window suffices at both the national and the institutional level if properly standardised and normalised citation indicators are used. And finally, in a sound bibliometric evaluation, the same rules of the game are applied to all units of assessment.

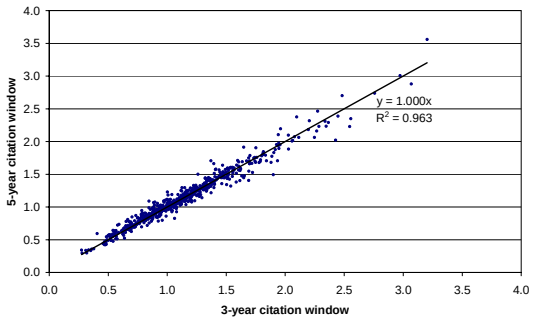

Figure 2 Plot of subfield-normalised mean citation rate based on 5-year citation window vs. 3-year window for selected European higher education and research institutions

We can conclude that the particular choice of a standard citation window cannot be made responsible for possibly negative results of an otherwise correct bibliometric evaluation study.

2.2 Myth #2: Citing yourself is blowing your own trumpet

The myth: *Author-self citations are used to manipulate impact and to artificially increase the own position in the community. Self-citations are very harmful and must be removed from the statistics.*

Since citations are used to measure important aspects of the research performance, there is an ongoing debate on how author self-citations should be judged (see MacRoberts and MacRoberts 1989) and dealt with. This debate has resulted in a certain polarisation and created its own myths as well. The science policy view of this particular citation type is gradually spreading among the researchers themselves (Anon, 2003). Repercussions caused by policy use on the scientists' communication behaviour have certainly boosted the development and several negative effects have already become measurable (e.g., Glänzel and Debackere 2003; Butler 2004). According to this view, author self-citations are to be condemned as possible means of artificially inflating citation rates and thus of strengthening the author's own position in the community. In addition, author self-citations are considered highly problematic and suspected in determining the quality of scientific journals as well (Anon, 2004).

A somewhat different view is advocated by information science, where a reasonable share of author self-citations is considered a natural part of scientific communication (cf. Narin and Olivastro 1986). Thus self-citations are quite inevitable in larger research projects and prevent authors from repeatedly copying larger parts of earlier publications. Accordingly the almost absolute lack of self-citations over a longer period is just as pathological as an always-overwhelming share. Pichappan and Sarasvady (2002) list nine reasons for author self-references, which are by nature somewhat different from giving citations in general (cf. Garfield 1964).

Although the arguments of information scientists are plausible, the tenacity of the persistence of the self-citation myth is astonishing. The deviating interpretation of one and the same phenomenon in different contexts opens the door to the emergence of rumours and myths.

The inclusion of bibliometrics in funding formulas is probably one of the most sensitive issues; scientists might be under the impression that authors could directly influence allocation of funding by "adjusting" their citation behaviour.

At least at the meso and macro level, bibliometric studies have not found alarming trends in self-citation patterns so far (e.g., Aksnes 2003; Glänzel et al. 2004; Glänzel and Thijs 2004; Thijs and Glänzel 2006). Of course, individual citation behaviour might extremely deviate from the statistical patterns. Our studies, however, showed that there is no reason for condemning self-citations in general or for removing them from citation statistics. Self-citations rapidly lose their weight as time elapses since they age much faster than foreign citations. This effect substantiates that authors normally cite their own work if necessary, but own results lose relevance as scientists address themselves to new tasks and challenges. On the other hand, supplementary indicators based on self-citations are useful to understand the scientists' communication behaviour, and might help to clarify if the measured citation impact really reflects the reception of the research results by the scientific community. The (sometimes hysterical) ado about author self-citation, however, lacks empirical foundation.

2.3 Myth #3: Collaboration is always a guarantee for success

The myth: *Multi-authorship and above all international collaboration increases productivity, visibility and impact. It also facilitates publication in high-impact journals.*

Scientific collaboration, above all international co-operation has unquestionably a positive effect on visibility and citation impact. Many studies have dealt with this phenomenon and confirmed that on an average research collaboration pays off. If this is an acknowledged fact then the question arises of why we speak about a myth. This has several reasons. Firstly, collaborative work and even more acknowledged co-authorship nowadays gained a meaning as the very recipe for success, as a necessary and almost sufficient condition for receiving funding, getting visibility and strengthening the position in the scientific community. As a result of simplification, collaboration as such is often considered a quality criterion. The choice of appropriate co-operation partners or co-authors already plays a determining part in the application and reviewing process of research projects. *Strategic co-authorship and sub-authorship* is actually used in the hope of starting with advantages and of easier achieving success. Cronin (2003) reports on fraud and honorific authorship having become an issue in several science fields. Even acknowledgements are used in a strategic manner, particularly, for "signifying subsidiary support rather than substantial and technical collaboration" (Cronin et al. 2003). Honorific authorship and hyper-authorship, i.e., the extraordinarily large number of authors of single papers in several subfields of biomedical research and in high energy physics (Cronin 2001) are contrasted by *suppressed* sub- and co-authorship (Laudel 2002). She has shown on the basis of a sample of interviewed scientists that a major part of (intramural) collaboration is not acknowledged either through a proper acknowledgement or through co-authorship. A large share of persons involved in the preparation of a scientific paper does thus not appear either as co- or sub-author of the publication. Indeed, the above cases of suppressed, fraud, honorific, hyper-authorship or even "mandatory" authorship, e.g., of supervisors questions the possibility of fixing the degree of the individual co-authors' contribution to the paper (Cronin 2001), and may raise the question: Co-authorship – who's contribu-

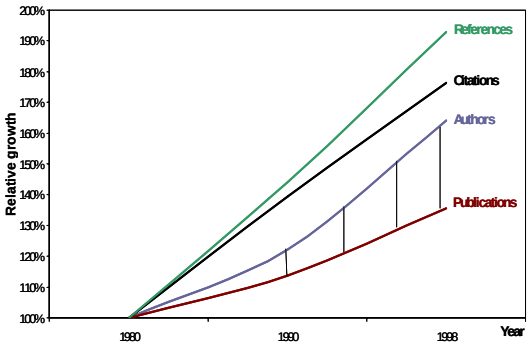

Figure 3. Relative growth of publications, authors, references and citations in all fields combined based on the Science Citation Index

On the other hand, honorific authorship and hyper-authorship can be considered two symptoms of an inflationary process (Glänzel and Schubert 2004) leading us to the second source of the collaboration myth. Persson et al. (2004) have shown that the number of (co-)authors is increasing faster than the number of publications indexed in the Science Citation Index (SCI) database of Thomson Scientific. This trend allows only one single conclusion, namely, that the collaboration network is becoming denser and co-authorship is gradually intensifying (see Figure 3).

In recent papers on neuroscience (Braun et al. 2001), biomedical research, chemistry and mathematics (Glänzel 2002) it was shown that "team work" exhibits higher productivity than single authorship indeed, but beyond a field-characteristic level, productivity distinctly decreases with growing co-operativity. Extensive collaboration does therefore not result in further increase of productivity. Although citation impact and visibility of collaborative research is on an average higher than that of non-collaborative research (Persson et al. 2004), counterexamples of so-called 'cool links' substantiate that even international collaboration does *not necessarily* result in higher visibility or impact (Glänzel and Schubert 2001; Glänzel 2001). However, once we speak about inflationary tendencies and research collaboration has become an imperative, the question arises of strategic thinking could indeed further increase the desired effects, such as career advancement or facilitating access to funding, if everybody applies the same success formula. Research co-operation is certainly a necessary and positive phenomenon in the era of 'big science' but the notion of collaboration as a recipe for guaranteed success remains a myth.

2.4 Myth #4: Citations are measures of 'scientific quality'…

… "and, in fact, the journal impact factor has become the common currency of scientific quality" (Neuberger & Counsell, 2002).

The notions of *citation* cover a large range of possible interpretations (e.g., 'reward system of science': Merton 1973, 'concept symbols for citing authors': Small 1978, 'information utilisation': Smith 1981; Cronin 1981; Glänzel and Schoepflin 1995, 1999, 'rhetoric-first model': Cozzens 1989). Although none of these interpretations are directly connected with quality issues, it was sociology of science and actually Robert K. Merton's idea of citation as part of the reward system of science that paved the way for taking citation impact and scientific quality as quasi identical. Citations are more and more considered the currency of science (Garfield 1982). Although Holmes and Oppenheim (2001) have shown that citation rates significantly correlate with measures of quality, citations are primarily a *formalised account of the information use* and can thus be taken as an *indicator of reception* at this level (Glänzel and Schoepflin 1995). In particular, the fact that a paper is less frequently cited or even still uncited several years after publication provides information about its reception but does not reveal anything about its quality or the standing of its author(s). Uncited papers by Nobel Prize winners may just serve as an example. However, "*if a paper receives 5 or 10 citations a year throughout several years after its publication, it is very likely that its content will become integrated into the body of knowledge of the respective subject field; if, on the other hand, no reference is made at all to the paper during 5 to 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*" (Braun et al. 1985). These latter two views completely reflect what citations really express.

With the wide distribution, which bibliometrics experienced during the last decade, the situation turned even worse. The availability of the large citation indexes and notably of the journal impact factors has opened up bibliometrics to anyone. Impact factors are used as surrogate for factual citation impact (Seglen 1989) and have in fact become the "common currency of scientific quality" (Neuberger and Counsell 2002). Even where the impact factors are not used as immediate evaluation tools, these journal citation measures often serve as decision criterion and reference standard in the choice of journals for paper submission. Reaching the targeted readership has become a secondary aspect in individual publication strategies.

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 the scientific quality of individual publications through citations alone is a myth.

2.5 Myth #5: Reviews are inflating impact

The myth: *Reviews are always highly cited, and do therefore inflate citation impact. Even uninspired authors can readily attract many citations by writing reviews. These documents should be removed from bibliographies when used for evaluation.*

This myth is somewhat related to the previous one. Its origin is twofold: The impact factors of review journals usually exceed those of other scientific journals, and seem therefore to "distort" journal ranking. Secondly, reviews attract on average more citations than research articles. Thus authors, who are frequently publishing reviews, might have an "undeserved advantages" in bibliometric evaluation. In fact, writing reviews requires much experience in the field as well as own essential contributions to the topic to be reviewed. From the statistical point of view, the weight of reviews is rather limited as their share in all citable items does not exceed the 5% threshold (e.g., 4.37% in 2004). And by far not all reviews are highly cited as is shown in Figure 4.

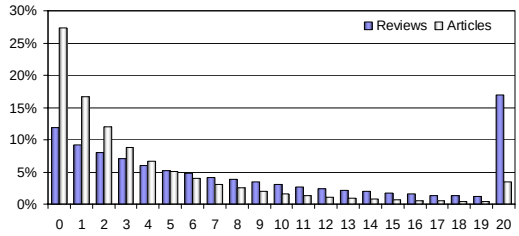

Figure 4. Citation distribution of all articles and reviews indexed in the 2004 volume of the SCIE based on a 3-year citation window

Their citation distribution is still very skew albeit to a lesser extent than that of "regular" research articles. On an average, reviews exhibit higher citation impact than other document types. And preparing review articles requires experience and essential contribution to the advancement of the corresponding subject. These documents play a serious role in scholarly communication and deserve to be respected.

2.6 Myth #6: Non transit Gloria mundi

The myth: *Once highly cited is always highly cited: Authors or publications identified as highly cited will never loose this quality.*

Most bibliometric processes are cumulative; publication output and citation impact are typical examples. The h-index, recently introduced by Hirsch (2005), illustrates this effect. Cumulative citation indicators thus seem to allow scientists to rest on their laurels since the number of citation might increase even if no new papers are published. In verbal terms, your papers do the job for you.

The inherent dynamism of these cumulative processes can be illustrated by citations to retractions of invalid or fraudulent work. Retracted articles often continue to be cited as valid work after retraction (e.g., Pfeifer and Snodgrass 1990; Budd et al. 1998). The case of J. H. Schön, who was responsible for one of the greatest scientific frauds and scandals of the 20th century, might serve as an example for this phenomenon. His career found a sudden end in 2002 due to proved scientific misconduct and several of his publications were retracted by the editors of *Science*, *Nature* and *Physical Reviews* journals where those papers had previously been published. Nonetheless, these papers still received citations after retraction and are still cited even today. Consequently, his citation impact increased although he has not published any new papers after the fraud has been discovered. This admittedly exceptional case clearly shows that the internal peculiarities and dynamism of scholarly communication might stay in effect and evolve even if the original initiators of the processes have disappeared. Of course, the same processes stay in effect in the regular case, that is, if published work is valid but its author becomes less active, or is not active anymore. This, however, might give the impression that gains once achieved will persist. No wonder that scientists are surprised to learn that once highly cited papers have lost this rating. As ranking can

and will change, persons or items assigned to a certain performance category do not necessarily remain in their classes as time elapses, and might be replaced by others (cf. Glänzel 2007). The members of the scientific community, as all social beings too, have to defend and reconfirm their position in the community day by day. Maybe the reality of the virtual web world, where literally everything is in continuous change and visibility and recognition must be grinded out everyday anew, could teach us again that fame is transient.

2.7 Myth #7: Don't use averages in bibliometrics

The myth: *Methods of classical statistics may not be applied to bibliometric distributions since those are discrete and extremely skewed. Therefore the use of medians and quantiles should be preferred.*

The background of this myth is quite obvious. The Gaussian normal distribution, being one of the most important families of continuous probability distributions, arises in many areas of statistics. If a statistical sample follows a normal distribution, then the observations should be symmetrically distributed around the sample mean and the standard deviation can be used to determine a tolerance threshold for individual observations. However, this is obviously not the case in bibliometrics. Most bibliometric distributions are far from being symmetric and discrete. Publication-activity and citation-impact distributions are often extremely skewed, the majority of the observations are below the sample mean and the rest of the sample elements are located in the long tail of the distributions. In such cases the mean value and the standard deviation seem to be completely useless. Therefore the application of classical tools of moment-based statistics seems not to be appropriate in research evaluation either. This is a misbelief. According to the central limit theorem, the distribution of the means of random samples is approximately normal for a large sample size, provided the underlying distribution of the population is in the domain of attraction of the Gaussian distribution. In other words, sample means approach a normal distribution regardless of the distribution of the population if the number of observations is large enough and the first statistical moments are finite. Consequently, means and shares of different samples drawn from the same populations can be compared with each other and the significance of the deviation can be determined. Means and shares are used as unbiased estimators of the expected value and the corresponding probabilities, respectively. Furthermore, in the case of skewed discrete distributions the mean value is superior to median. The underlying methods of application of mathematical statistics have been described, among others, by Schubert and Glänzel (1983), Glänzel and Moed (2002) and reliability-related statistics have been regularly and successfully applied to bibliometrics since. These statistical properties have severe effects on ranking issues as well. Different ranks can prove as ties because the underlying indicator values might not differ significantly (cf. Glänzel and Debackere 2007).

The myth of the inapplicability of Gaussian statistics in a bibliometric context actually arose from a misunderstanding, namely from the assumed comparison of individual observations with a standard. However, that is not what statistics does.

3 Conclusions

Myths arise from reality. They reflect dreams and visions, are used as excuse for unsatisfactory results, or serve as recipe for hoped-for success. Using collaboration or impact factors in a merely strategic manner might result in frustration since the hoped-for success fails to materialise. Other myths are fostered by mistrust as can be observed in the case of self-citations and review articles. And finally there are myths that have their roots in uninformed use of data, in misunderstandings or ignorance. The history of these myths reaches back in a time when bibliometrics did not yet exist, but due to policy use and misuse of publication and citation statistics, bibliometrics might act as catalyst in the process of fostering, disseminating and extending these myths.

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