Researchers' perceptions of citations

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Researchers' perceptions of citations*

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Abstract

This paper looks at how citations are perceived among scientists. Based on a questionnaire survey it traces the repertoire of views and experiences about citations that could be found among Norwegian scientist that had published highly cited papers. Their views circle around three issues: the relation between the quality (or importance or significance) of a paper and its citation history; the importance of visibility and how different sorts of factors play a role in determining citation in general and high citation in particular; and the fairness (or lack of fairness) of the system. Taken together, the respondents' answers and comments offer an informal (and fragmented) sociology of citations and their role in the world of science. In the final section we discuss the relevance of our findings in respect to the increasing use of citation indicators in science policy and research evaluations.

Keywords: Citations; citation indicators; highly cited articles; sociology of citations; quality

1. Introduction

The status of citations among scientists is ambivalent. On the one hand citations are sought-after because they are part of the reward system of science. For the scientists, being cited shows impact (through acknowledgements) and builds up reputation. On the other hand, citations are criticised for not reflecting actual scientific contribution. The use of citation indicators for research evaluation purposes puts further pressure on this ambivalence.

While issues around citations are a recurrent topic in conversations and discussions, and there is something like a shared repertoire of insights and convictions, only a few studies have investigated scientists' viewpoints on citations. Hargens and Schuman (1990) looked at biochemists and sociologists use and evaluation of citation data in a social comparison theory perspective. A survey by Martin and Skea (1992) examined how various research performance measures, including citation indicators, were viewed by academics. Here, the scientist identified many limitations with using citations to assess university departments, although a large majority (67%) still felt that citations should form part of such assessments. A similar study was carried out by Collins (1991) revealing an even more critical attitude towards citation indicators. Only a few respondents in this survey gave such indicators credence in assessments of departmental research. Reasons given for rejecting citation analysis included the different referencing traditions of individual disciplines, the many non-approbationary functions of citations and the inadequacy of the databases used for counting citations. Also, the fact that the use of citation indicators often has been surrounded by controversy (for some examples see Wouters, 1999: chapter 6) may be taken as an indication of a widespread scepticism in the scientific communities towards such indicators.

On the other hand, scientists continue to use citation measures when re-affirming or questioning quality of contributions and scientific reputation. Similarly, status and A, B and C ratings of journals are sometimes linked to the citation-based journal impact factor. Thus,

there are good reasons to speak of a dual attitude to citations (cf. also Hargens and Schuman, 1990).

We had an opportunity to investigate scientists' perceptions of citations as part of a study of highly-cited papers of Norwegian scientists. In addition to scientometric analysis of the characteristics of highly-cited papers (Aksnes, 2003a), we sent out questionnaires to scientists with at least one highly-cited paper, to inquire about the relations between the nature of their papers and the citation rates. This part of our study is published already (Aksnes, 2005). We also asked for their perceptions of citations and the role of citations, and we report our findings in this paper.

While perceptions will be coloured by the specifics of the situation of the respondents and their receptivity to the questionnaire, there are two reasons to consider our findings to reflect general features of citations in the life of scientists. First, there was no high-stakes context as would be the case when the focus is on the use of citations as performance indicators. Also, in Norway there is no use of citation indicators for funding or for science policy decisions directly affecting scientists. Second, there will be less grumbling about being always under-cited because our respondents have at least one highly-cited paper. Thus, we can expect a more balanced response.

The scientists' views may be regarded as expressions of "folk-theories" concerning citations.[1] They are based on their experience of scientific publishing, communication, recognition and rewards, and the stories that are told about citations, rather than on systematic study. There may be "citation myths", and in some cases these could be identified by comparing the "folk theories" with findings from sociological and scientometric studies.

[1] The concept of "folk theory" is a generalization of concepts of "folk biology" (Douglas and Atran, 1999) and "folk psychology" (as encountered in everyday life), but does not assume a principle difference with "official" science. Folk theories "are a form of expectations, based in some experience, but not necessarily systematically checked. Their robustness derives from their being generally accepted, and thus part of a repertoire current in a group or in our culture more generally." (Rip, 2006, p. 349). "What characterizes folk theories is that they provide orientation for future action." (ibidem)

On the other hand, valuable insights and knowledge concerning citations might be found in the "folk theories" which could be the starting point for further studies. This is particularly important because of the lacunae in our understanding of citations and their role in the world of science.

The "folk theories" circle around three issues: the relation between the quality (or importance or significance) of a paper and its citation history; the importance of visibility and how all sorts of factors play a role in determining citation in general and high citation in particular; and the fairness (or lack of fairness) of the system.

All three issues relate to the fact, recognized by scientists and documented in bibliometric studies, that citation distributions are extremely skewed. Some papers become highly cited but the large majority of publications receive very few citations or none at all. Elsewhere, we introduced the concepts of quality and visibility dynamics in order to analyze this skewness of citation distributions (Aksnes, 2003a). The quality dynamics refers to the structure of scientific knowledge and a cognitive hierarchy in which some contributions represent major scientific advances; others are filling in the details. Visibility dynamics, on the other hand, refers to certain social mechanisms that influence the citation rates, such as the bandwagon and "Matthew" effects. As a first approximation, this distinction works, and can be used to raise questions about citations. Are the citation distributions a reflection of quality dynamics and thus be "fair"? And if so, only in the aggregate, because of the contingencies around individual papers? Our respondents definitely analyse and judge what happens in terms of quality dynamics. However, they also realise that visibility dynamics is important, and are pragmatic about it. For example, some said that a poor citation count could be ascribed to the "style of presentation":

> In my view this article is "under-cited". It contains important empirical findings […] The reason is probably that the message was concealed in a boring title and there was no abstract. It is important to indicate a RESULT in the title.

There are further considerations, about the journal in which the paper is published, about timeliness, about selective citation practices. Taken together, the respondents' answers and comments offer an informal (and fragmented) sociology of citations and their role. This is what we will present and also comment upon.

2. The study

As respondents, we chose Norwegian scientists that had published one or more highly cited papers (Aksnes, 2003a). By choosing scientists that had published such papers we could ask each of them about a range of papers, some little cited and at least one highly cited.

In total 221 scientists whose address could be recovered were selected for the survey.[2] A selection of papers to be included in the questionnaire (maximum ten) was compiled from each respondent's scientific production. The questionnaire consisted of three parts: general questions, an individualised part with questions about each of the articles selected, and opportunities for comment plus a final question about their ideas how a paper becomes highly cited.

The four questions in the first part elicited views on citations and recognition, ending with the basic question about quality dynamics:

Table 1. Questionnaire – Part 1. Questions and alternative replies.

1. How important do you think it is for a scientist in general to have one or more highly cited papers? [Very important – Important – Some importance – Not important]
2. How important is it for you to have one or more highly cited papers? [Very important – Important – Some importance – Not important]
3. How often do you check the number of citations to your own publications? [Regularly – Occasionally – Never]
4. Do you in general think that the number of citations reflects the scientific contribution or overall importance of a paper? [Yes – It varies – No]

[2] We used the overall national publication set for the period 1981-1996 as a basis, in total almost 50,000 articles. From this set of papers we identified the approximately 300 articles that had obtained the highest relative citation rates, using the world-average citation rates of the respective subfields as reference values. The papers represented a variety of scientific research areas. For each paper one (Norwegian) author was selected for the questionnaire survey. However, some of the papers were authored by the same individuals; some of the authors were dead or could not be identified, resulting in a sample of 221 persons (cf. Aksnes, 2003a).

In addition, the authors were asked to explain the reasons for their answers to Questions 1, 2, and 4.

In Question 4, the concept of scientific contribution was used as the key term, and 'overall importance' was added to reinforce the point that papers are now seen as contributions to the knowledge reservoir (sometimes only of one research area) and help to advance it.[3] No further definition of 'scientific contribution' was given here because we wanted to allow for certain flexibility in the understanding of the concept. From the comments of the respondents we see that they actually think in terms of recognition of contributions when they consider citations.

This can be linked with the scholarly debate on citations, where the Mertonian view in which citations are considered as part of the reward system of science has often been considered to provide evaluative bibliometrics with a theoretical basis. It also leads to questions about background assumptions, especially whether scientists recognise all the works they have utilised and whether the cognitive contents of the references are all there is to referencing (Luukkonen, 1990) – which has led to studies inquiring into deviations from this ideal –, and the other assumption, often glossed over, that scientists somehow are able to recognize a contribution for what it is worth and do so immediately. In any case, it is within this tradition that concepts such as impact and recognition are used to describe what citation measures**.** We have chosen to use 'importance' in our question 4 because we wanted to keep visible possible differences between contributions to science and actual impact which might be measured by the number of citations over the years.

[3] The terms 'scientific quality' or 'a good paper' are also used, and sometimes interchangeably with 'importance'. One respondent emphasized the difference: "Uncited articles are usually of little importance, even

Table 2. Questionnaire – individual publications (Part 2). Questions and alternative replies.

Questions		Alternatives			
a)	How do you assess the overall importance of this paper?	Major contribution	Medium contribution	Minor contribution	
b)	Could you characterise this paper as to the kind of contribution it makes? Please select one main characteristic, even when this can be difficult.	Theoretical contribution	Empirical findings	Methods	Review
c)	Could you also characterise the paper according to the following types of contribution (select more than one characteristics if necessary)?	Uptake and immediate use within its field	Uptake and immediate use in other fields	Opening up new avenues of research in own field or other fields	Practical relevance
d)	To what extent does the paper's citation count for the first 5 years after publication (X) reflect its scientific contribution?*	Large extent	Some extent	No extent	

*) In Question d) the "X" is the number of citations within a five-year window.

The concept of contribution returns in the second part where the respondent was asked to discuss each of the selected articles. In phrasing the questions and offering alternative replies, we based ourselves on what has been discussed in the literature (e.g. special role of methods papers and review papers) and added a further aspect, originally put forward by Alvin Weinberg (1963), the difference between impact on the own field and impact on other fields. (See Table 2). Since we knew the actual citation counts we could compare them with the author's own perception of their scientific contribution. This analysis is presented in Aksnes (2005). In this paper, our interest is in the comments respondents gave, from simple ones like:

> This is the first time I have seen the citation counts – they correspond well with my expectations

or:

> Generally the citation counts are lower than one would expect and they do not always correspond well with the importance I would attach to them personally.

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though the quality may be good." 'Quality' refers here to the craft of scientific research (which leads to good-quality papers).

to more elaborate discussion:

> Sometimes I wonder why some good articles are poorly cited. Some of the articles I regard as my best publications rank at the bottom of the list – these publications have often been of indirect importance in leading to other articles that are highly cited.

The third part of the questionnaire asked for comments: a) Do you have comments on our choice of papers? b) Do you have comments concerning the citation counts/citation history of your papers in general? c) Some papers become highly cited others not. According to your opinion and experience, how do you think a paper becomes highly cited?

Among the 221 scientists selected for the survey, 166 responded. The response rate of 75% must be considered as quite high. The fact that the questionnaires were "personalised" contributed to this high response rate. It is also an indication of a widespread interest in citations among the scientists. However, not all respondents answered all the questions. While almost everyone filled in the questions where alternative replies were given, the share of respondents answering the questions requiring comments was lower, for the different questions it varied between 37% and 75%.

The respondents of the survey represented a broad range of academic fields, such as mathematics, physics, chemistry, biology, the earth sciences, clinical medicine and engineering. The majority worked as researchers within the life sciences. We note in passing that in the original selection of highly cited papers the social sciences and the humanities were excluded.[4] We did not identify any systematic differences in the views between scientists from different disciplines.[5]

Using a database located at NIFU-STEP Institute, it was possible to obtain information about the ages of the respondents. The following distribution was found: 30-40

[4] The main reason being the large differences in publication and citation behaviour that (generally) can be found between the natural sciences and medicine on the one hand, and the social sciences and the humanities on the other (e.g. research more often addressing national or local issues, publication in non-indexed literature such as national serials, monographs, books, and reports, and a slower pace of the theoretical development affecting the age and frequency distributions of the citations, cf. Nederhof, 2006)

years: 4 respondents, 40-50 years: 32 respondents, 50-60 years: 69 respondents, 60-70 years: 39 respondents, 70-80 years: 17 respondents, >80 years: 1 respondent (excluding 4 persons with missing data). The dominance of older scientists derives from the fact the original selection of scientists was based on papers published some years ago: the 15-year period 1981-1996. We will occasionally make use of the information about age, but did not generally identify any obvious or systematic difference in the views of representatives of different age groups.

We have included many quotations (translated from Norwegian to English). Quotes were selected for illustration of particular viewpoints among the respondents. In footnotes we show further, mostly similar comments.

3. The respondents' conceptions of scientific contribution and citation rates

An interesting background issue is already visible in the quotes above: what is to be expected as to the number of citations to an article? When we asked for each of the selected publications to what extent (and why so) the citation count reflected the degree of contribution, we were positioning the respondents as somehow knowledgeable about this. But how can respondents know what would be an appropriate citation level? Some will be acquainted with how often other papers have been cited or they might know about the citation counts of their own publications. (In the questionnaire, the respondents were provided with the citation counts of their own papers, cf. Table 2.) From their knowledge of the literature in their own field scientists can have an impression of how their publications have been referred to in other articles, and more generally, what citation level to expect.

[5] Due to a differing publication and citation pattern this might not have been the case if the social sciences and humanities were included.

Overall, respondents are willing to refer to something like a standard, of what is to be expected in terms of citations.[6] There are two sides to this, agreement and contestation. When respondents agree that the citation count reflects the contribution and importance of the paper, they do not need to become more specific. The standard remains implicit. When the citation count does not reflect the contribution (underciting or overciting), they contest the actual impact measure of citations and have to refer more or less explicitly to what they consider should have been the case. Most comments refer to cases of under-citation,[7] and are couched as an explanation why this could happen (as we shall illustrate later), without much consideration of what would be the "right" citation count. Comments on these issues might reflect human biases as well, for example one expects that authors tend to over-value the importance of their own contributions. According to Hargens and Schuman (1990) citation counts may appear to affirm or deny researchers' beliefs that their published work is valuable and their reactions to citation counts tend to protect these beliefs. Since the respondents of our study have highly cited papers they can be expected to be more positive towards citations and not to complain about being under-cited in the way an "average" scientist might do.

A related issue concerns the respondents' view on scientific contribution. From their comments we can derive that the respondents have implicit conceptions of what a scientific contribution consists of. Quite a few of the respondents seemed to adopt a conception of contributions similar to the definition of 'importance' given by Martin and Irvine: "The 'importance' of a publication refers to its *potential* influence on surrounding research activities – that is, the influence on the advance of knowledge it would have if there were perfect communication in science (in short, if there were completely 'free market' of

[6] Nevertheless, quite a few of the respondents did not respond in this way. Apparently some did not feel they had enough knowledge to give such comments, exemplified by expressions such as "I have no opinion on what represent a high/low citation count."

[7] Aksnes (2005) created a composite measure for under-cited, rightly cited, and over-cited and found that 70% of the papers commented upon by respondents (about 1100 papers) appeared to be rightly cited, 25% under-cited and 5% over-cited. Review articles featured heavily in the category of over-cited.

scientific ideas)" (Martin and Irvine, 1983). The following quotations concerning the relation between the type of journal and citation count may illustrate this: "If you publish in a 'wrong' journal you may get few citations even when the work is very good." Similarly, when commenting on the citation count of their own articles the following views were put forward:

> A 'weak' article scientifically speaking that did not elicit very much new. [It] has received more attention than it deserves because it was published in a journal with a large circle of readers.
> This is a good paper but very few […] can afford to subscribe to this journal. Papers published there are almost lost.

Comments on other issues suggest that the authors tend to adopt a conception of scientific contribution as something given which may, or may not, be recognized. For example, as reason for why one of his papers is poorly cited, one respondent argued that: "An unfortunate title, and consequently it has not received sufficient notice."

In the work by Martin and Irvine the term 'impact' is used to describe a publication's actual influence on surrounding research activities. In their views this depends partly on its importance, but is also affected by factors such as the location of the author, and the prestige, language, and availability, of the publishing journal. In other words, because there are "imperfections" in the scientific communication system, the importance of a publication may not be reflected in its impact.

Respondents tend to think of scientific contribution as somehow inherent, and visibility dynamics as a disturbance. This also implies that a paper may be seen as important, even when it has been neglected or hardly has been read by other researchers, and thus not really contributed to scientific progress.

This conception of contribution is also manifested through several comments concerning the size of the research field. A paper may represent a very important contribution, but when only a few people work on the topic, it cannot expect many citations anyway, indicated by statements such as: "This is an important contribution within a narrow field. For this the number of people that will cite it is limited." Thus, the respondents seem to

adopt a conception of scientific contribution that is independent of absolute differences in size.[8] In other words, the number of practitioners on a topic is not considered as a measure of this topic's overall importance. Progress in small niches must be as important as progress in large research fields.

4. Ambivalence about the importance of receiving citations

In various ways the scientists expressed an ambivalent attitude towards citations. On the one hand the responses indicated a widespread interest in citations. Overall the results showed that the respondents were knowledgeable about citations, they had a large number of comments and viewpoints on issues such as citation distributions and visibility dynamics. However, on the question about how often they check the number of citations to their own publications, 47% answered "Never", 49% answered "Occasionally", and 4% "Regularly" (n=165). Considering the fact that the authors have been highly cited it might also be considered as surprisingly that almost half of the respondents never check the citation rates of their publications.[9] In this respect, the use of bibliometric indicators for science does not seem to have much impact yet (cf. Weingart, 2005).

The large majority of the respondents considered it as either important or very important to have one or more highly cited papers, cf. Table 3.

Table 3. Researchers' opinions on the importance of having highly cited papers.

	Very important	Important	Some importance	Not important	Total (n=166)

[8] Another example: "It often happens that an average contribution that contains a method is cited for years and years. Other articles are aimed at a very small public and then even very good and important contributions cannot expect a high citation index."

[9] One might expect that the youngest scientists would be more familiar with citation indexes and thus check their citation counts more regularly (e.g. because of higher adaptation to new science policy regimes, familiarities with web-based search technologies). The average age of the respondents that never check their citation counts and the average age of the respondents that occasionally check their citation counts were, however, almost identical (57 and 56 years, respectively). Thus, no such systematic pattern could be identified. Even scientists in their seventies (retired) occasionally check the number of citations.

How important do you think it is for a scientist in general to have one or more highly cited papers?	52%	41 %	6%	1%	100%
How important is it for you to have one or more highly cited papers?	45%	37%	13%	5%	100%

The slight but definite difference between importance in general and importance for oneself can be a matter of projection: "others run with this, but I am more realistic". A few of the respondents noted that they might not be typical, because they had their established reputation and need not go for highly-cited papers anymore.[10]

Various reasons were given by the respondents for why it was important to have highly cited papers. A frequently mentioned reason was that it would prove the scientific impact and quality of one's work, as the following quotation illustrates: "It means that the article is a valuable contribution to the field and is being used when others write/do research within the same field."[11] Immediately linked to this is the consideration that quality brings reputation (cf. quality dynamics). Whatever the link between quality and citations, since reputation is important, citations can be sought in their own right. To further their career and/or obtain credit to mobilise resources, scientists will apply themselves to publish and improve their chances of getting citations. And when these do not materialize, find excuses related to visibility dynamics leading to discrepancies between citation counts and (self-assessed) contribution to science.

Most of the respondents focused on issues related to the personal sphere, that is, why it is important for the researchers themselves. Here, the importance for scientific recognition and for scientific career was often mentioned, but some also emphasised personal motivation.

[10] This can be linked with Mulkay's (1972) analysis of risk taking in science: the young have little reputation to lose, so can take risks. The old, or better, the highly reputed scientists, can permit themselves to risk their reputational capital (Linus Pauling would be an example). In between, reputational capital has to conserved and increased – so highly-cited papers are necessary.

[11] Other examples: "It shows that the work has (usually) been of high quality / has been useful / has been of importance for others research / has represented a hallmark or been of high international standard / to a certain degree reflects that the articles are read and that your research is of interest to others."

The following quotations taken from different respondents' questionnaires illustrate these various types of issues:

> Gives you recognition in respect of colleagues, and a scientific reputation internationally
> Highly cited papers are important in order to get scientific positions / are important for getting access to scientific collaboration
> as an employee in private sector selling know-how, this means that nobody questions your scientific sincerity
> important in order to get money for further research / used by funding agencies and when applying for positions (in some cases)
> give the researchers legitimate self-confidence / self-esteem, reduces faint-heartedness/depression.[12]

From these comments we see that the respondents attach large importance to highly cited papers. Citations are sought after; they are valuable because they in the view of respondents increase recognition and make funding easier.[13]

Clearly, there is something at stake in receiving citations. Thus, it becomes important to explain discrepancies between actual citation counts and importance/contribution. Explicit and implicit criticism of citation counts is stimulated because of this. What happens also is insistence on contingency, up to randomness – "A paper is often 'discovered' more or less by chance", "It is coincidental what is cited". At the same time, and for the same reasons, citations are upheld as an important indicator of quality and achievement. Thus, a mixture of criticism and agreement.

5. Discrepancies between citations and scientific contribution

As reported in Aksnes (2005), the citation counts of the publications selected for the questionnaire were found to correspond reasonably well with the authors' own assessments

[12] Other examples: "gives one a world-wide reputation as a scientist / highly cited papers are important in order to obtain scientific positions / helps your career / leads to invitations to join various research groups internationally / important for salary increases / important in evaluations / important for the institute's reputation / highly cited papers pay off in terms of funding of project applications / highly cited papers may be a necessary condition for obtaining funding / is an inspiration for further research / shows that you have been successful."
[13] Would they want reputation without quality? Some do, an extreme case being fraud, as has happened especially in life sciences (Broad and Wade, 1982).

of their scientific contribution, but only at the aggregate level.[14] At the level of the individual article citations are not, according to these assessments often argued in detail, a reliable indicator of a paper's scientific contribution. Illustrative is this quote:

> Some of the articles which I regard as my best papers are poorly cited. An example: a paper from 1980 showed that […] contrary to what had been found by other highly recognised scientists. Our findings were ignored for a long time (and not cited), despite being published in a very good journal. Eight years later other studies showed that our results had been correct. The article was then cited for a short period, but a little later other "follow-up" articles were cited instead of our article. Another example: a Nature article represented a major contribution within the field and has been cited approx. 100 times. I would have expected it to have been cited more frequently, but instead most scientists cite follow-up studies published some years later.

When asking the authors to comment on the citation counts of their own publications quite a few remarked that these did not adequately reflect contribution:

> The citation counts should generally have been higher.
> Too many articles are poorly cited or not cited at all.
> A relatively normal citation history. Articles I regard as good have been neglected, other more or less unexpected observations have "caught on".

There were also respondents that regarded the citation counts of their publication to correspond well with their assessed contribution:

> Interesting patterns which strengthen my opinion that they give a representative picture of quality, at least within my field.
> The number of citations usually reflects the articles' scientific contribution, but not always.

The analysis in Aksnes (2005) showed that generally citations best reflected major or minor contributions, while in between there was much variety, as some respondents recognized:

> Uncited articles are usually of little importance, even though the quality may be good.
> Articles that are medium cited can hardly be separated significantly – five or fifteen citations do not matter.

When commenting upon their own articles, the respondents mainly focused on the cases were the citation counts were misleading – because then there is something at stake:

> The article is much more important that the citation rate suggests.
> I regard this paper [cited 10 times] as one of my most important publications.
> This must be a mistake. This article is frequently cited and one of the most important I ever have written [uncited].

[14] In 53% of the cases the respondents considered the citation counts of the publications to reflect the scientific contribution to a large extent, while 38% were considered to reflect the contribution to some extent. In 9% of the cases the citation counts reflected the contribution to no extent (cf. Aksnes, 2005).

> Quite frankly, this article is too poorly cited. After all, it laid the foundation for the articles x and y which have been massively cited.

The respondents also draw attention to particular circumstances where citation counts would be misleading, for example:

> Some articles that contain wrong conclusions are frequently cited because other scientists then will cite the work and show that they have new and correct results.
> If an article [in mathematics] completely solves a problem, it will sometimes be poorly cited – then there are no related problems to work on.

The respondents gave different arguments for why they considered the citation counts to be unfair (and unfairness tends to refer to cases of neglect, the uncited and poorly cited papers, not to overcited papers). The quotations below exemplify the range of these comments:

> The content [of the article] was used in a court trial in the USA to stop a dangerous product [from being marketed] (the product was stopped) [cited 1 time].
> A good article with large relevance for clinical practice, it is surprising that it is not cited.
> A paper of local character, but often applied by the oil industry [cited 2 times].
> The citation count [0] surprises me, I have received many inquires for off prints.
> The topic is relatively "narrow" and only a few people will use the results. However, the number of indirect users is many – but these do not result in citations.
> We showed that a well-known and highly cited work of German chemists was WRONG. It terminated a discussion that nobody saw a reason to continue, not even those who published the original work (we have, however, received positive verbal feedback) [uncited]. [15]

There were also several cases where the authors considered it as fair that some of their articles were poorly cited:

> This article contains little new compared to article x, and I see hardly any reason why it should be cited.
> The article confirmed that some previous data in a particular research area were "correct" and in this respect it did not have much novelty.
> It showed a new and useful use of a medicament, but represented a minor study compared to the [similar] other studies.
> This is an investigation of Norwegian conditions and of little general interest in the wide world.
> The work is mainly a descriptive survey of a particular geographical area [uncited]. [16]

[15] Other examples: "Later works that have used ideas from this article have not cited it because it was considered as unimportant at the time it was published. / We received many inquires about this method in the form of letters, and it has therefore aroused interest. It is a bit strange that it is cited so infrequently [1 time] ….. / The lack of citations means that the work mainly had a national importance by forming the basis for a White paper on cancer. / This article has later had a relatively large importance for the understanding of otherwise difficult rocks [uncited]. / Shortly after publication a book was published in which these results were included. The book is frequently cited approximately 300 times [uncited]. / This is basic new biology that has become text book material [cited 10 times]. The field is poorly cited. / This is a very uncommon illness and cannot expect many citations. However, scientifically it is important and the citation count should have been higher."

[16] Other examples: "This was a study of low quality, with poor data and a negative result that was difficult to interpret. / A product that never was released on the market, mainly because of poor effect in terms of blood

Publications with citation counts considered by the respondents to reflect the scientific contribution to a large extent were the most frequently cited papers. When comments were given on these papers aspects related to scientific quality and significance were often used to explain why they had been much cited:[17]

> The article is highly cited because it opened up a new research area.
> The first clinical test showing the effect of an epoch-making medicament.
> The article tries to examine the degree of evidence of the effect of a new treatment that had received much attention. The result was negative. That is, the treatment did not have any effect. The article has been prize-winning.
> This article was ground-breaking and hardly any other article has been more cited.

When general views were asked for, aspects related to scientific significance and quality were similarly often used as explanation for frequent citations:

> Important contributions either methodologically or epistemologically will necessarily become highly cited.
> Articles that open a new research field or change the direction of a present field are rewarded with many citations. The first good articles in a field will usually be remembered longest and these articles are therefore most frequently cited over time.
> [Highly cited when] it has contributed to a paradigm shift or lead to new ways of thinking in the field.
> High citation counts can indicate high quality/value but also high actuality.
> A paper is highly cited if it describes new knowledge that can be used by other researchers.
> Results that remain standing [are highly cited].

Clearly, these are folk theories about citations, and sometimes convictions about how the (reward) system should work. At the same time, they do reflect experiences, even if these are mixed. This combination explains why frequent citation is more often considered as to reflect scientific importance – although cases of "over-citation" also are reported.

The folk theories can be quite sophisticated, for example when particular types of research are seen as being favoured in terms of citations – while the resulting counts do not

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pressure reduction/ When we submitted this paper for publication we knew that it was not important, but we could not know this when we commenced the research."

[17] One might expect that the respondents would tend to emphasise quality when explaining why one of their papers has become highly cited and visibility dynamics as reasons for poorly cited papers. We could, however, not identify such a tendency and there were also many contrary cases, i.e. where low quality and little significance were used as explanation for poor citation and frequent citations were explained by visibility dynamics. Also one might expect that there would be a contrast between the views the respondents have in general and their views on their own situation. To a certain extent we did find support for such differences. Respondents list a large number of reasons why citation counts do not reflect contribution, but in the case of their own articles there is overall still a fairly good correspondence between the citation counts and their

necessarily correspond with the papers' scientific contributions, as the following views on highly cited papers illustrate:

> Large clinical experiments are most frequently cited
> [Highly cited when] crossing field boundaries
> Highly cited are particularly new methods, new and important empirical findings, and results of new treatments and investigation methods.
> [Papers on] a new illness are much cited.
> Clinical relevance gives many citations.
> The contribution must be of a type that other can build upon.
> [High citation counts]: reference articles (standard values).

Further nuances are visible in how evolution over time, the temporal dimension, was recognized. For example, the following arguments were given for why some papers become highly cited:

> A paper will be highly cited if it is the first to arrive with new data upon which many other build their further research.
> Articles are frequently cited if they contain good research and address an issue that is of current interest.
> The topical interest at time of publication is more decisive than the scientific value of the article.

But the temporal dimension was also used as argument for why the number of citation not always reflects scientific contribution, for example:

> If you are ahead of your time you will not be cited.
> If your work is something entirely new and significant it may be a long time before it gets cited.
> A work may appear as very original and important for some time, but is then rejected by subsequent research.

This dimension was also addressed in the comments on their own articles. For example, the respondents sometimes used expressions like "published at the right time" for frequently cited papers and "rapidly superseded by later works" for poorly cited papers:

> This contribution was published too late for the debate, that is, the issue was not anymore in fashion.
> The article was written at the right time and therefore obtained a particularly high citation rate.
> It hit the bull's eye in respect of what was the current vogue.
> Important when it was published, but not anymore.
> An unexpected result that probably will attract more attention later.
> Summing up […]. The subsequent development has been very rapid and citing is less topical.

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assessed contribution. At the same time it is difficult to identify any obvious antagonism between their general views and the comments the give on their own articles. In both cases a variety of issues are addressed.

For some of the articles (usually the poorly cited) there were also comments concerning "delayed recognition":

> This approach was not in common use until 10 years later. / The work is important for the understanding of blood circulation. It is not yet 'discovered'. / An important method that did not become popular until 8-10 years later.

Phenomena such as delayed recognition were considered to affect the validity of citations as measure of scientific contribution. The respondents here expressed what has been a common argument against citation as indicators. However, it has been shown statistically that delayed recognition as reflected through citation patterns is generally very rare (Glänzel et al., 2003). Thus, on this point the respondents seem to rephrase what might be considered as a citation myth among scientists although a few of them did have real-life experiences with publications that had been recognised many years after publication.

6. Visibility dynamics and obliteration

In their comments the respondents directed attention towards various mechanisms disturbing a simple correlation between contribution and citation count. In the citation process visibility and recognition were argued to play important roles:

> It is important that an article starts to get cited so that other scientists become aware of it.
> There is a bandwagon effect: citations lead to new citations.
> Citation involves a chain reaction, people are following each other.
> Key publications are often cited at a disproportionate rate.

Others pointed to social hierarchy as important:

> Citations depend more on the reputation of the scientist more so than the significance of the article.
> Fewer people read the article when the author is not an established scientist.
> Within my field I notice that citation is a matter of fashion, some authors (big shots) appear very frequently, despite the fact that not all of what is being cited is that good. One will be frequently cited if one has made a name for oneself.

Problems related to self-citations and what was considered to be a social bias in the citation process were also mentioned by a few of the respondents:

> Citation is often a “clique”- affair, they cite their own work, while other (uncited) works have equal or more relevance.
> You have “friend” citations within some groups.

Furthermore, when commenting the citation counts of their own articles some of the respondents considered international collaboration as important:

> It is frequently cited because the publication was authored by a large number of authors from different countries.
> This [article] is a description of an experiment with many participants [authors], which means that many of them will cite it subsequently

Others indicated that there are geographical biases involved:

> American scientists seldom cite European scientists within our field.
> American scientists rarely cite articles from European journals.

According to a few of the respondents the title and abstract and of a paper and the style of presentation is more important for citation rates than the quality and relevance. The following comments concerning why papers become highly cited exemplify this view:

> A sexy title and an abstract that promises more than the article contains is the safest way to high citation frequency.
> The description must not bee too complicated [in order to get highly cited]
> It is important to “sell the article”. Title and abstract are extremely important in order to catch on.
> The results must be clear or definite (not: on the one hand… on the other hand.).

Others claimed that there is a considerable element of randomness in the citation process:

> A paper is often ‘discovered’ more or less by chance.
> It is coincidental what is cited.
> It is just a matter of luck. Citation rates are relatively misleading.
> Because of limited space you need to make a selection of articles to be cited. Many relevant works are being omitted. Citations are based on, for example, knowledge of a researcher/group contributing to the field (here there is a major weakness)…

In their comments on such mechanisms, respondents may well be reproducing a shared repertoire among scientists about citation processes, up to what might be considered as citation myths. There is definitely evidence, as when respondents describe their experience:

> This article was ground-breaking and hardly any other article has been more cited. However, it may be an example of ‘over-citing’ because of the strong commercial implications of the article.
> Competitors in this field practice citation collaboration where Europeans are not wanted. A number of articles published later should have cited this article.

> The article clashed with the opinions of some of the leading international research groups within our area. Their reaction was to 'boycott' the article which should have received higher citation counts."

The question, of course, is how generalizable such experiences are. Scientists feel sufficiently certain about their folk theories to transform them into advice.[18]

In passing we note that the respondents tend to mention issues that are also the focus of discussions of the use of citations indicators. For instance, Seglen (1997) mentions "friend citations" and bias towards US science as examples of problems associated with the use of citation data. Similarly, bandwagon and "Matthew" effects have been recognised as mechanisms operating in science since the pioneering studies by Merton and Price (Merton, 1968; Price, 1976). There is however, little knowledge on the actual importance of such mechanisms on the distribution of citations. Garfield has argued that citation circles are much talked about, but rarely, if ever, documented and identified (Garfield and Welljams-Dorof, 1992). Other issues could be documented trough bibliometric studies, for example, multi-authored papers have been found generally to be more highly cited than single-author papers (Aksnes, 2003b) and internationally co-authored papers have been shown to be more cited than single country papers (Narin et al., 1991).

The complement of visibility dynamics is obliteration dynamics: when one paper becomes more visible, others will become less so. Various mechanisms are mentioned in general:

> Sometimes an original article is poorly cited, while a secondary article based on the original article obtains all the citations and honour.
> A very important article opening up new avenues of research is not necessarily highly cited because following-up articles gradually become cited instead of the original article.
> The citation count of an article depends on if/when it is included in a review article: if a review article is published soon after publication this article will be cited instead the original article.

[18] Examples in the comments of respondents for the particular case of publishing in conference proceedings: "One should tell the PhD students that it is a complete wasted of effort to publish in conference proceedings." "Even good works in conference proceedings are poorly cited. If I had started my scientific career today I would have adopted another publishing strategy: not to publish in books and conference volumes."

Also when commenting upon the citation counts of their own papers, obliteration is mentioned, often related to competition and timeliness:

> The scientific quality of the article is very good but a competing article had been published shortly before in NEJM [The New England Journal of Medicine].
> There are some very interesting findings reported in this article but other research groups had previously published similar results in very high impact journals, and these articles are usually (and rightly) cited instead of our article.
> A progress report. Sometimes it is necessary to publish quickly in order to avoid that being forestalled by competitors. It is reasonable that this article is not much cited.
> One article among an enormous number of publications concerning this topic, it drowns in the "crowd".
> The article is a part of a serial. Each article contains a limited amount of new information, and one would not expect each article to be a bestseller.
> This article is identical with article x. The differences in citation rates reflect the different circle of readers. [19]

Citing of review articles rather than the original articles has often been considered as a problem for the use of citation indicators because the original sources will then not be credited (MacRoberts and MacRoberts, 1996; Seglen, 1997). The respondents are well aware of this problem and how it affects the citation counts of the individual research papers in different ways. The respondents also point at similar biases appearing when follow-up articles are cited instead of the original articles.

The phenomenon of "obliteration by incorporation", meaning that when a work is sufficiently well known, it is not cited anymore, was recognized by one of the respondents: "This [highly cited article] has become a standard technique for serological (…) It is now standard so that it is not cited anymore." The phenomenon first described by Robert K. Merton has been considered as a problem for citation analysis, although Garfield has argued that: "There is, however, not much chance of obliteration causing inequities. It happens only to work that makes a very fundamental and important contribution to the field; and before the

[19] Other examples: "The article was important when it was published but a few years later it was replaced by other more detailed investigations. / What the article describes is 'good to know', a bit boring and no reason to cite it (once one knows it). / Interesting findings were reported in this article but we continued the work in later papers and these are more highly cited. This explains the low citation rate of the article. / At the same time other research groups published similar findings. Which articles that are being cited therefore vary. Furthermore, the 'follow-up articles' are now being cited to a larger extent. / This article is only a retelling of the work presented in article x."

obliteration takes place, both the citation count and reputation of the scientist responsible for the work usually reach a level that makes additional credits superfluous" (Garfield, 1979). Garfield's response is too limited however, given the variety of obliteration effects when follow-up publications eclipse the original sources, as testified in the experiences of our respondents.

Obliteration by incorporation is linked to the more general phenomenon of obsolescence and ageing of scientific literature. It was suggested by Line and Sandison (1974) that the decline in the use of publications over time could occur for the following reasons: the information is valid, but incorporated or superseded by later work; the information is valid but in a field of declining interest; and that the information no longer is considered as valid. Studies of the citation aging process have also shown that there are large variations in the citation patterns of individual papers, but that on average the peak in the number of citations received is reached just a few years after publication. The respondents realise that the process of incorporation and superseding affect the citation counts of individual papers in different ways and that this process may have certain accidental elements that are not simply related to the scientific contributions of the papers. In our view the respondents here have valuable insights on an important issue that deserves more attention than it so far has received in the literature on citations.

7. Effects on citation rates

Several respondents considered the type of publication as influencing the citation rates:

> Citation rates depend on the types of paper. Methodological papers and review papers are often frequently cited.
> Review articles that are published by persons who have already published much within the field are often frequently cited.
> New methods are often cited if they are good and useful. In addition to review articles the most frequently cited papers are key methods.
> Sometimes the citation count is high because the article describes a method that many scientists can use, although the scientific importance is not that much.

> Theoretical works need to have relevance for experiments in order to get cited.

These are general viewpoints, but they were supported when the respondents commented upon their own articles. Review articles drew most of the comments:

> This is a review article. For such publications the citation frequency is highly influenced by where the article is published, the number of research groups working within the field, and the number and quality of other corresponding review articles.
> A review, cited at a disproportionate high rate compared to article x which is the original article. [20]

Here the respondents' views are in accordance with bibliometric findings. Several studies have shown that review papers tend to have higher citation rates that other publication types (see Moed and van Leeuwen, 1995; Moed et al., 1996; Peters and van Raan, 1994). This was also the case in the set of papers used for the questionnaire (Aksnes, 2005).

A very frequent view was that citation counts imply a bias against small and specialised subfields:

> Citation counts reflect activity (or lack of activity) within the field.
> High citation counts may be a result of many scientists working on the topic, quality may not be reflected.
> If the field is large a central article will be cited by many.
> If you work in a small field you will pay the price.

Similar arguments were also used when the respondents commented on the citation counts of their own articles:

> [The article is poorly cited] because there are not many scientists who use the experimental technique described in the paper.
> This is a scientific niche with little publishing activity. Because of this the chances of getting cited is limited.

Some also emphasised that the traditions for citing differ between fields, causing inequalities when comparing publications in different fields. This argument was, however, much less frequently mentioned that the size effect.

[20] Other examples: "Review articles are always highly cited when published when a research field is expanding. This [highly cited] article did not contribute very much to the field, but represented (according to my colleagues) a good introduction for a post graduate student, for example. / Review articles are frequently cited because they are more easily are selected when the number of references needs to be restricted (usually 1– 2 references per assertion)."

The idea that working in small research fields is a disadvantage in terms of citations is a misconception, at least it is not supported by bibliometric findings (see Peters and van Raan, 1994). Garfield, for example, has emphasised that a large field produces many references, but there are also more publications that need to share those citations (Garfield, 1998). Thus, the average citation rate per paper is not influenced by the size of the field. What is still the case is that the size of the literature affects the number of citations a highly cited paper can get (as several of the respondents also realise). That is, when the population of papers is large the most frequently cited papers may obtain a higher number of citations compared to the similar papers in smaller fields.

A related issue only incidentally mentioned by the respondents is that the average citation rates vary considerably among fields. This is an issue which cannot be derived from the respondent's own scientific practice and experiences. When citation indicators are to be used in comparative assessments, normalization procedures can take the differences between fields into account. For example, when relative indicators are being constructed, the citation counts of the articles are often compared with the average field/subfield citation rate. But even within a field or subfield there are different scientific niches and specialities that may differ in publication and citation patterns, which cannot be captured easily.

Another recurrent issue concerned the effect of journals:

> An article in a highly ranked journal obtains more citations than an article of similar quality in a lower ranked journal.
> The citation counts are influenced by the accessibility of the journal.

The journal effect was also used as an explanation when the respondents commented on the citation counts of their own articles:

> This article was of little importance. But because it was published in 'Nature' it has been frequently cited.
> This article was published in an Indian journal and could not expect much attention.

> An unfortunate choice of journal. It should have been published in a more specialised journal.[21]

It is a widespread opinion that the citation count of a paper is highly influenced by the journal it is published in. In several cases the respondents seem to think that the journal is more important than the content of the article. There is a prima facie plausibility that the status of journals affects citation counts. Papers published in prestigious journals obtain more visibility and will therefore be more cited. The issue is more complex, however, because acceptance for publication in a prestigious journal is conditional on higher than average quality. This would explain the finding that the prestige of a journal is an important predictor of citations (Peters and van Raan, 1994). Others have argued that the citedness of publications is not significantly influenced by the status of the journals in which they are published, because the distribution of article citedness within most journals is very skewed, which implies that contents of the articles are the important determinants in citation rates (Seglen, 1994).

8. In conclusion

This study has investigated researchers' perceptions of citations. What we have as data is the repertoire of views and experiences about citations, and the paper reports and characterizes this, without too much analysis. Nevertheless, some conclusions can be drawn.

The respondents were able to mention a large number of reasons why citation counts do not correspond with scientific contribution. This was based on their own experience, as well as their referring to a shared, and somewhat cynical repertoire about citations. However, despite these problems, when asked about the citation counts of their own publications

[21] Another respondent argues that: "The journal in which a paper is published significantly influences how many citations it receives. One of the most important publications I have contributed to was published in Scan J. Immunol. This article is cited but I think it is cited to a considerably lesser degree than if it had been published

another pattern emerged. For the majority of these publications the citation counts were considered to correspond well with the degree of contribution (Aksnes, 2005). The shared repertoire may be too cynical, and lacking empirical support. It continues to be reproduced, however, because negative experiences tend to be more salient (and there is little opportunity to get a view of patterns at more aggregate levels), and there are no incentives to check the validity of the repertoire views. A questionnaire asking questions about own papers with different citation counts may introduce some reflection, as with the respondent who said:

> This investigation has made me to think through the relation between citation counts and importance. After filling in the questionnaire I am more convinced that citation counts (used sensibly) is a good indicator of importance.

Having noted this, we should add that the respondents generally appeared to be quite knowledgeable of citations. This reflects the increasing importance of citations, not only because of their use in external assessments, but also because references to citation counts (with their easy availability) may be part of competitive struggles among scientists. The one limitation appears to be the lack of overview across fields. Here, bibliometrics with its link to decisions requiring comparisons across fields has developed sophisticated approaches, for example relative indicators (see e.g. Schubert et al., 1988).

In the study we have also identified ambivalence in the resechers‘ views about citations. Citations are sought-after because they are perceived as part of the reward system of science but also criticised for not reflecting actual scientific contribution. This then leads to further ambivalences, where high citation counts are accepted as reflecting quality, and low citation counts are explained as a result of visibility and obliteration dynamics. An important finding is that this is not an across-the-board approach. In fact, overcitedness is recognized as well, and understood in terms of timeliness and relevance to larger audiences. In other words, scientists have a sophisticated understanding of the citation process and its

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in a VSOP journal. In applications in which I list my most important relevant publications, I rarely mention this paper. It looks better to list articles published in Science, Nature Medicine and J. Exp. Med."

outcomes, and can explicate such understanding when there are no immediate stakes to be defended, as in a questionnaire asking them about earlier papers.

This has implications for how to view the resistance among many scientists to measures and indicators for external assessment (Woolgar, 1991). One component is the scientists' wish to retain autonomy, and therefore come up with methodological and other criticisms of the indicators. (Which can actually help to improve such indicators.) From the other side (policy makers) such criticisms will then be seen as tactical ploys.

The other component relates to the experience-based expertise of the scientists, which was amply documented in this paper. Criticisms of and concerns about citation indicators should then be taken seriously, even if these have to be checked against analyst's insights (sociologists as well as bibliometricians). In this paper, we have indicated where such checks are in order.

The key lesson to be drawn out is how quality dynamics and visibility dynamics occur together in the citation process, and cannot easily be disentangled. There is no easy recipe to separate substance from tactical ploys. On the other hand, there are sophisticated insights that can and should be taken into account, by policy makers as well as by bibliometricians who want to be relevant to the complexities of the real world.

What relevance do our findings have in science policy contexts? First, citation indicators are used ex-post to provide information on the performance of research groups, departments, institutions or fields. In the Netherlands, for example, for many years publication and citation analyses have been provided as information to panels evaluating university research (van der Meulen, 1997). Increasingly, bibliometric indicators are also used for funding proposes. At department levels such measures have been applied to distribute resources and even determine salaries (Andersen and Pallesen, 2008). Moreover, several countries have implemented or intend to implement funding models at national levels

partly based on bibliometric measures. In fact, such a system has recently been introduced in Norway, but only involving publication indicators, not citations (the system was not running when the questionnaire survey was carried out). In the UK, the Government has decided to replace the current Research Assessment Exercise (RAE) with a more indicator based system where publication and citations measures are likely to play an important role (Higher Education Funding Council for England, 2007).

While the use of publication and citation indicators increases, their application is still controversial. As we noted already, numerous studies have been published over the years criticising citation analysis. Today, the opposition against such measures seems to have weakened although critical papers still are being published (Adler et al., 2008)). According to Van der Meulen (1997), there is hardly any dispute of the added value of these data in the research evaluations in the Netherlands anymore. The current situation where bibliometric measures are to be used in national funding models has revitalised the debate. This is particularly evident in the UK, where the debate on the proposed funding model has been fierce.

Our findings exemplify viewpoints present in the scientific community concerning these issues. As we have seen, even among scientists who have produced highly cited papers (and who are likely to benefit from the use of citation indicators in the system) there is doubt about the fairness of citations as performance measures. Inevitably, persons with poor citation records will be even more critical. Thus, one can expect that an increasing and more mechanical use of such indicators will generally meet opposition among scientists, who feel that it will undermine or threaten the very idea of scientific quality.

It is interesting to note that some of the viewpoints given by our respondents on these issues have also been mentioned in a consultation on the proposal of the new funding model in the UK (Higher Education Funding Council for England 2008). Here, the views from

national stakeholder bodies about the bibliometric approach ranged from positive support to scepticism. It was argued that citations do not provide a direct measure of quality. For example, some emphasised that citations only reflect intellectual influence among the academic community and do not represent a balanced indicator of quality, in particular of the quality of applied or practice based research or user-value. Concerns were also expressed about the potential for strategizing and other unintended behavioural effects ("citation clubs" etc.), including prospects of researchers pursing safe research. In other words, that they would be reluctant to publish work that, although worthwhile, would be little cited.

The current situation is characterised by a tension between administrative needs for simple measures and more easy evaluation methods and researchers request for fair and comprehensive assessments of scientific quality. As we have seen, some of the viewpoints in the scientific community seem to represent "citation myths" and might disappear by providing more information towards scientists on citations. Nevertheless, citations have fundamental limitations as performance measures, and this is realised by scientists themselves. Thus, the tension will remain. Science administrators and policy makers implementing bibliometric systems are advised to tread carefully.

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References

Adler, R., Ewing J., Taylor, P., 2008. Citation Statistics. International Mathematical Union. http://www.mathunion.org/Publications/Report/CitationStatistics

Aksnes, D. W., 2003a. Characteristics of highly cited papers. Research Evaluation, 12(3), 159-170.

Aksnes, D. W., 2003b. A macro study of self-citation. Scientometrics, 56(2), 235-246.

Aksnes, D. W., 2005. Citation rates and perceptions of scientific contribution. Journal of the American Society for Information Science and Technology. 55 (12).

Andersen, L., Pallesen, T., 2008. "Not Just for the Money"? How financial incentives affect the number of publications at Danish research institutions. International public management journal. 11 (1), 28-47.

Broad, W., Wade, N., 1982. Betrayers of the truth: Fraud and deceit in the halls of science. Simon and Schuster, New York.

Collins, P. M. D., 1991. Quantitative assessment of departmental research. A survey of academics' views (SEPSU Policy Study No. 5). Science and Engineering Policy Studies Unit of The Royal Society and The Fellowship of Engineering, London.

Douglas, M. L., Atran, S. (eds.), 1999. Folk Biology. MIT Press, Cambridge, MA.

Garfield, E., 1979. Is citation analysis a legitimate evaluation tool? Scientometrics, 1(4), 359-375.

Garfield, E., 1998. From Citation Indexes to Informetrics: Is the Tail Now Wagging the Dog? Libri, 48, 67-80.

Garfield, E., Welljams-Dorof, A., 1992. Citation data: their use as quantitative indicators for science and technology evaluation and policy-making. Science and Public Policy, 19(5), 321-327.

Glänzel, W., Schlemmer, B., Thijs, B., 2003. Better late than never? On the chance to become highly cited beyond the standard bibliometric time horizon. Scientometrics, 58(3), 571-586.

Hargens, L. L., Schuman, H., 1990. Citation Counts and Social Comparisons: Scientists' Use and Evaluation of Citation Index Data. Social Science Research, 19, 205-221.

Higher Education Funding Council for England, HEFCE. 2007. Research Excellence Framework. Consultation on the assessment and funding of higher education research post-2008. HEFCE 2007/34. http://www.hefce.ac.uk/pubs/hefce/2007/07_34/

Higher Education Funding Council for England, HEFCE, 2008. Analysis of responses to HEFCE 2007/34, the Research Excellence Framwork concultation. http://www.hefce.ac.uk/pubs/consult/outcomes/ref.asp

Line, M. B., & Sandison, A.,1974. 'Obsolescence' and changes in the use of literature with time. Journal of Documentation, 30(3), 283-350.

Luukkonen, T., 1990. Citations in the rhetorical, reward, and communication systems of science. Unpublished PhD thesis, University of Tampere, Tampere.

MacRoberts, M. H., MacRoberts, B. R., 1996. Problems of citation analysis. Scientometrics, 36(3), 435-444.

Martin, B. R., Irvine, J., 1983. Assessing basic research: Some partial indicators of scientific progress in radio astronomy. Research Policy, 12, 61-90.

Martin, B. R., Skea, J. E. F., 1992. Academic Research Performance Indicators: An Assessment of the Possibilities. University of Sussex, Brighton.

Merton, R. K., 1968. The Matthew Effect in Science. Science, 159, 56-63.

van der Meulen, B. J. R., 1997. The use of S&T indicators in science policy. Dutch experiences and theoretical perspectives from policy analysis. Scientometrics, 38(1), 87-101.

Moed, H. F., van Leeuwen, T. N., 1995. Improving the Accuracy of Institute for Scientific Information's Journal Impact Factors. Journal of the American Society for Information Science, 46(6), 461-467.
Moed, H. F., van Leeuwen, T. N., Reedijk, J., 1996. A critical analysis of the journal impact factors of 'Angewandte Chemie' and 'The Journal of the American Chemical Society'. Inaccuracies in published impact factors based on overall citations only. Scientometrics, 37(1), 105-116.
Mulkay, M., 1972. The Social Process of Innovation. A Study in the Sociology of Science. MacMillan, London.
Narin, F., Stevens, K., Witlow, E. S., 1991. Scientific co-operation in Europe and the citation of multinationally authored papers. Scientometrics, 21(3), 313-323.
Nederhof, A. J., 2006. Bibliometric monitoring of research performance in the Social Sciences and the Humanities: A review. Scientometrics, 66(1), 81-100.
Peters, H. P. F., van Raan, A. F. J., 1994. On Determinants of Citation Scores: A Case Study in Chemical Engineering. Journal of the American Society for Information Science, 45(1), 39-49.
Price, D. d. S., 1976. A General Theory of Bibliometric and Other Cumulative Advantage Processes. Journal of the American Society for Information Science, 27, 292-306.
Rip, A, 2006. Folk Theories of Nanotechnologists. Science as Culture 15(4), 349-365.
Schubert, A., Glänzel, W., Braun, T., 1988. Against Absolute Methods: Relative Scientometric Indicators and Relational Charts as Evaluation Tools. In A. F. J. van Raan (Ed.), Handbook of Quantitative Studies of Science and Technology. Elsevier, Amsterdam.
Seglen, P. O., 1994. Causal Relationship between Article Citedness and Journal Impact. Journal of the American Society for Information Science, 45(1), 1-11.
Seglen, P. O., 1997. Citations and journal impact factors: questionable indicators of research quality. Allergy, 52(11), 1050-1056.
Weinberg, A. M., 1963. Criteria for Scientific Choice. Minerva, 1(2), 159-171.
Weingart, P., 2005. Impact of bibliometrics upon the science system: Inadvertent consequences? Scientometrics 62(1), 117-131.
Woolgar, S., 1991. Beyond the citation debate: towards a sociology of measurement technologies and their use in science policy. Science and Public Policy, 18(5), 319-326.
Wouters, P., 1999. The Citation Culture. Unpublished PhD-thesis, University of Amsterdam, Amsterdam.