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Organization of Research

An opinion is advanced in the article below that any bibliometric data, including individual article citations, as an independent measure of the scientific significance of a scientist's works, cannot serve as a criterion of the efficiency of these studies or the value of these publications. Scientists across the world have long been questioning the relevancy of using these data. The majority of prominent scientists either express well-founded doubts that it is authentic to use them as an independent criterion or reject them altogether. Although bibliometric data are undoubtedly valuable as an auxiliary evaluation means, they cannot substitute peer reviews based on the analysis of article contents, its contribution to the field in question, and the contribution of the author under review to the subject matter presented in this publication. The author is of opinion that bibliometric data should be an obligatory but not decisive component in the complex system of assessing the creative potential of scientists and the significance of their works.

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Citation Mirages

Bibliometric Evaluation of the Significance of Individual Authors' Publications

E. D. Sverdlov*

You may prove anything by figures.
Thomas Carlyle

There are increasingly frequent attempts to measure the quality and progress of a study, as well as the talent and efficiency of a scientist, by bibliometric indices. Indeed, such an approach looks both simple and convincing: count citations—and here you are: this scientist is 10% worse than that one, and still another scientist is another 50% worse. Great! The only thing you have to do is to measure, and then you may promote or fire this or that researcher on good scientific grounds. There are even special computer programs. You do not have to think; instead, you may use certain foreign experience, because this idea has been inspired by none other than Eugene Garfield personally (!), the founder of the Institute for Scientific Information (ISI).

And here we are: in May 2006, right before the elections to the Russian Academy of Sciences, a citation index table of RAS candidate academicians and corresponding members was distributed among members of the Section of Physicochemical Biology. This table ranked our scientists by citation indices in decreasing order and sort of imposed the following: this scientist is the most cited one; hence, you must elect him/her. I will not enlarge upon the fact that these materials contained mistakes that sharply decreased indices of a number of candidates and could affect the voting results. My point is to drive it home to the reader that any bibliometric data, including citation of individual articles as an evaluation measure of the efficiency of a scientist's works, may be presented only as a part of other data, provided that each work and the contribution made to it by this or that specialist are thoroughly analyzed.

WHAT DOES CITATION SPEAK ABOUT AND WHAT DOES IT HUSH UP?

The fundamental study by P.O. Seglen [1] shows that bibliometrics, introduced into practice in the United States in the 1950s, was invented to trace flows of research ideas, progress in scientific fields, and the impact of this or that scientific work on other spheres of social life. Bibliometrics plays a role in the sociology of science by elucidating ways how scientific ideas are exchanged and scientific discoveries made. It helps understand why certain ideas become recognized while others are rejected. It may point to the most cited ideas and individuals, but it is difficult to trace correlations between these citations and notional indicators of any kind.

It is noteworthy that citation analysis system was invented to help understand how scientific discoveries and innovations interact and how the research system functions. However, originally citation was not looked upon as a means of evaluating individual scientists. Today, this system, invented for limited functions, is used to address problems it is not designed for. Organi-

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zations responsible for hiring labor, committees involved in promotion, and official bodies engaged in increasing salaries try to use citation as the basis for evaluating the qualities of individuals. The incorrect use of citation analysis distorts the original principles of the bibliometric system. The improper spread of bibliometric data in a field that is alien to bibliometrics is extremely unjust to those who are thus evaluated and ranked [2].

What does citation measure? In order to specify the algorithm in which I am going to develop my ideas, it is necessary to define terms that I will use here. I have to admit that there are no universal definitions for them; hence, the definitions used here should be regarded as operational. First, let us define the notions of *progress, quality*, and *impact.*

For many aspects of scientific activity, it is important to distinguish between the notions of research *quality* and *progress.* Quality, as well as another notion, *scientific excellence*, has many definitions [3]. I use the simplest one: the perfection of obtained results owing to the experience and competence of researchers who performed the work or the series of works in question. This is, so to speak, a scalar, nonoriented, value. On the contrary, research progress is a "vectorial" notion associated with the contribution of this or that researcher to reaching a certain goal. No doubt, the majority of published and cited works meet certain quality standards; however, it is obvious that *there is no direct correlation between the quality of works and their contribution to progress.* We can easily imagine a work of high quality, which does not contain any new data, and vice versa. However, the probability of progress is higher if the researcher performs all works according to high quality standards. Finally, the notorious byword—the famous notion of *impact.* If we analyze literature, we will find numerous definitions of it, many of which are very vague. In simple terms, impact means the influence of a given publication on the activity of the scientific community. This is a measure that makes the publication noticeable among others, and this is an impact that may be associated with citation. Although there were attempts to associate it with progress, impact merely testifies to the fact that this or that publication has a chance to make the scientific community acknowledge certain ideas. However, when taken independently from other components, impact only shows that the publication under review has a certain influence but says nothing about whether this influence is progressive or shifts the focus of the scientific community to an improper direction and hinders scientific progress [4, 5].

Bibliometric indices fail to function as measures of scientific progress because they do not account for the scientific contents of publications under analysis. This is the reason why bibliometric indices may be used as an auxiliary means for evaluating scientific products, but in the long run, evaluations should be based on the opinions of reviewers experienced in the given field and capable of evaluating the contents of the publication in question [6, 7].

Frequently cited articles do not obligatorily contain important ideas. It is often the case that articles are frequently cited because they contain important methodological innovations or compilations. We should not interpret the word *important* as *correct.* We know many examples of frequently cited "negative" articles. *Importance*, as well as *quality*, is extremely difficult to interpret. Citation statistics does not lead to the absolute scale of *importance* [8].

In sum, we may state: the fact that this or that article has been cited shows that it has been used in this way or another in an article by other authors. It seems logical to suppose that repeated citation testifies to its usefulness for research. Citation may serve as a partial indicator of the importance of the study under review. However, we should stress that citation says nothing about such aspects as originality, reliability, information density, and contribution to scientific progress.

Prejudice of citation. Even if we regard citation merely as a measure of usefulness, trying to use it as a parameter in determining the scientific value of this or that article, we will face different problems. A scientific article is usually based on knowledge of hundreds of other articles, but owing to the widely known "limited space" in journals, only an insignificant part of these hundreds can be found among references. The objectiveness of citation is thus considerably undermined, and articles that have not been cited have still been used. The choice of references is neither arbitrary nor objective. Some of experienced scientists mark originality and significance while citing somebody else's work in their field, while others prefer recent works and choose reviews and secondary sources. The increasing use of literary databases that do not fully cover old publications makes the problem even more difficult. Other reasons, considered in detail by Seglen [1], add to this. I would mention the strong bias to the citation of works confirming the results obtained by the author in question. In my opinion, the latter leads to the prevalence of trivial and imitative works chosen for citation (I will consider this later). An important tendency is that new articles often include references that have been already used for such citations by other authors. As a result, a pool of frequently cited references is formed, many of these references being there not because of their scientific value.

I will not consider such an obvious thing as self-citation, but I would like to draw your attention to the factor of the social activity of scientists who popularize their works by submitting them to different conferences, sending them to leading specialists in the respective fields, and so on. There are technical problems concerning the counting of citations, such as restrictions to databases. Although databases are indexed (for example, ISI regularly indexes 7000 to 8000 journals, and about 5200 journals are in the widened *Science Citation*

Index), this covers only a small part of journals: about 130 000 journals are published annually. In particular, only about 30% of biological journals are cited (perhaps the situation has slightly improved recently). I will factor out many other technical blunders, such as similar surnames, etc.

K. Popper and characteristics of modern citation. In 1934, Sir Karl Popper presented one of the most authoritative analyses of the fundamentals of scientific development in *The Logic of Scientific Discovery.* In particular, he put forward a fundamental principle of falsifying (discrediting) hypotheses. According to Popper, a hypothesis is scientific if it contains a clear system of its own refutation. Even if only a single fact is found in the process of discrediting, which does not conform to the presented hypothesis, the latter should be rejected. This is the simplest form of the most important principles of the development of science: through a scientific hypothesis that is scientific because it is verifiable and refutable.

It is noteworthy in this context that a considerable part of references in a scientific article is connected with the discussion and interpretation of obtained results. Ideally, a balanced analysis of all *pros* and *contras* should be carried out, but in practice, scientists try to find articles that conform to their own ideas instead of looking for those that contradict them. Many studies show that 90% of references in articles are of conforming character [9]. Original and nonstandard articles are the least cited. Hence, *frequent citation may testify not to the article's contribution to scientific progress but, quite the contrary, to its imitative and secondary character.* However paradoxical this may be, alas, Popper would have been disappointed. Apologists of citation frequency as a measure of an article's significance should consider this.

Thomas Kuhn: citation frequency from the point of view of scientific progress. In *The Structure of Scientific Revolutions*, Kuhn [10]—one of the most prominent figures among philosophers of the 20th century—gave an idea of the development of science. A scientist faces a problem that should be solved and, while solving it, this scientist makes a discovery. It is practically certain that this discovery conflicts with ideas established earlier. Competing theories are formulated for explaining this discovery, and many researchers appear in this field who are attracted by its innovative character and problems that require explanation. A breakthrough happens when someone offers a general explanation to the observable facts. The ideological scheme put forward by this scientist for explaining mechanisms of the given phenomenon—the paradigm—provides other scientists with a base for considerable progress in this field. The new field becomes fashionable. Discussions with the paradigm's opponents help make up for deficiencies, and finally, what was once a revolutionary doctrine becomes a generally accepted wisdom. (Remember Humboldt's famous triad: every truth goes through three stages: what nonsense, there is something in this, and this is commonplace!) A serene period in the development of science begins, when facts are studied and acknowledged as important by the given paradigm. This is the period of "normal" science. Studies of this period are based on previous achievements, which are considered basic for future practices. However, further work shows anomalies that do not blend with the existing paradigm; nature turns out to be more complex than the expectations worked out by normal science; new contradictions grow; a new revolution breaks out; and a new paradigm appears.

Generally speaking, everyone can easily imagine that the existing paradigm suppresses the emergence of new ideas (the what-nonsense stage, according to Humboldt). I will give only one example of how this happens. In 1949, the famous US scientist J. Bittner isolated the "Bittner milk factor" from milk of cancer-prone mice. He did not discover this factor among cancer-resistant mice. To make this long story short, I will only stress that, although Bittner fully realized that the isolated factor was a tumor virus (later it was called the mouse mammary tumor virus, or MMTV), he preferred to call it the milk factor in his publication. Later he explained his decision in the following way. The dominant paradigm of that time denied that viruses might be a cancer-producing factor. The term *factor communicated through milk*, which could also be called mother's influence and which determined the high frequency of cancer among specially selected inbred mice, allowed Bittner to attribute his work to genetics—a very respectable science, as opposed to dubious cancer virology. There were many more chances that this work would be acknowledged, published, and supported by geneticists in this case than if Bittner had called his factor a virus [11]. As a result, the discovery of MMTV had no influence on the then-accepted paradigm.

Now let me go back to the problem of citation frequency and ask the following question: what may we expect from citations within the existing paradigm? In my opinion, the answer is clear: works that support the existing paradigm will occupy the leading place among citations. As for works that contradict this paradigm, they will be withdrawn from reference lists wherever possible. In fact, I have said the same in the above section. A high citation may mean that the work in question does not contain anything new and has no chance to introduce progress, a revolution, or a discovery in science.

The influence of the field of research. The field in which a researcher works is an extremely important citation-affecting factor. For example, biochemical publications containing almost two times more citations than mathematical ones are thus cited two times more often, taking into account other factors that I will not discuss. On average, an article on biochemistry is cited four times more often than one on mathematics.

The size and dynamism of the field in question may also affect citation in different ways. In absolute figures, citation in large-scale fields will be on average higher than in small ones. In addition, in rapidly developing fields, including, no doubt, molecular and cellular biology, citation is higher than in stabilized ones, such as biochemistry and microbiology. Thus, citations from scientists working on different subjects of inquiry cannot be compared directly. Although everyone understands this, this situation is hardly taken into account.

Citation variability. In addition, there is an expressed variability in citation data, which means, in particular, that we should account for many articles (about 50) to obtain statistically reliable data. Analysis of voluminous materials clearly shows that citation differs from author to author, but it is not clear whether it reflects the differences in the scale of use or depends on differences between the fields in which the authors under comparison work, or whether it is determined by the biased character of databases. The majority of evaluations are based on analyzing a limited period during which only a small part of research groups can produce a volume that may be evaluated in statistically reliable terms. Hence, statistically reliable evaluations are inapplicable as far as activities of individuals are concerned; they better fit in evaluating large teams—university or national.

Although there are examples of using bibliometric analysis at the level of individual researchers and small research groups, as a rule, the usage of *bibliometric technologies at this microlevel is not advisable due to the great number of pitfalls and errors emerging from the small sizes of samples under analysis.* Originally, bibliometrics was not designed for evaluating individuals; and we should remember this [8].

WHAT DO SCIENTISTS THINK ABOUT CITATION?

David Adam writes that there are three types of lies: lies, damned lies, and statistics. No doubt that on discovering that their works have been evaluated through measuring citation frequency, many scientists would sign on to this opinion. Citation analysis by amateurs may turn into an extremely blunt tool. In addition, having analyzed data on citation, specialists will find out that they often contain mistakes. Adam concludes his detailed and nonbiased article, devoted to improperly used bibliometric data, by asking to what extent, in particular, citation analysis is good for science. Its adherents argue that it introduces objectiveness into decision making. Its opponents are of the opinion that the practice is so full of mistakes and biases that it may be worse than useless [12].

The editorial in *Nature Neuroscience* stated:

> The main problem is that impact factors are being increasingly used for purposes for which they were never intended, namely to evaluate individual applicants for jobs or funding. The ISI has never advocated this use; they emphasize that there is no substitute for informed peer review, and that bibliometric data may supplement but should never replace such review. Unfortunately, this message is not always heard [13].

Seglen called one of his numerous reviews "Citation Rates and Journal Impact Factors Are Not Suitable for Evaluation of Research" [14].

The German Research Foundation has organized a competent commission on working out and observing recommendations for "good scientific practices." The commission's findings include a detailed analysis of the quantitative approach to evaluating the effectiveness of scientific publications. In particular, numerous drawbacks of the bibliometric approach are mentioned, for example: "counting publications and looking up 'impact factors' are far removed from the competence needed to judge the quality of the content of a publication. Reviewers restricting themselves to the former end up by making themselves superfluous" [15].

On March 30, 2005, scientists from Australian National University placed a very detailed analysis of the usage of bibliometric indicators in the Internet. They came to the following conclusion:

> Bibliometric indicators cannot be used like cooking recipes. Many methodological problems have not yet been satisfactorily resolved, although some partial solutions are offered by the bibliometric community. Considerable expertise is required to conduct bibliometric studies and interpret the results …. Any attempts to use bibliometric indicators for research assessment should be undertaken with caution and preferably by experienced bibliometricians [16].

Nobel Prizewinner S. Brenner:

> Before we develop a pseudoscience of citation analysis, we should remind ourselves that what matters absolutely is the scientific content of a paper and that nothing will substitute for either knowing it or reading it. We should also recognize that citation often tells us more about the sociology of science than about the science itself [17].

What Garfield himself says in this respect is important:

> Publication and citation data offer the potential to develop new quantitative, objective indicators of S&T performance. While they have their limitations as do any quantitative indicators, most, if not all, of these limitations can be statistically weighted, controlled, or otherwise compensated. Properly applied, interpreted, and analyzed, citation data are a valuable and revealing *addition* (my italicization—*E.S.*) to conventional methods [18].

I could end the quotation here, but Garfield deserves more attention:

> Few would dispute the claim that a nation's science and technology (S&T) base is a critical element of its economic strength, political stature, and cultural vitality. In recent years, efforts to evaluate and assess research activity have increased ….
>
> One of the many quantitative indicators available for S&T evaluation and assessment is the published research literature, that is, primary research journal articles. Publication counts have traditionally been used as indicators of the 'productivity' of nations, corporations and institutions, departments, and individuals. However, judgment of the influence, significance, or importance of research publications requires qualitative analysis by experts in the field, an often time-consuming and expensive process.
>
> The advent of citation databases, which track how often papers are referenced in subsequent publications, and by whom, has created new tools for indicating the impact of primary research papers. By aggregating citation data, it is then possible to indicate the relative impact of individuals, journals, departments, institutions, and nations. In addition, citation data can be used to identify emerging specialties, new technologies, and even the structure of various research disciplines, fields, and science as a whole.
>
> This is not to say that citation data replace or obviate the need for qualitative analysis by experts in the field. Rather, they supplement expert judgments by providing a unique perspective on the S&T enterprise. Indeed, citation data themselves require careful and balanced interpretation to contribute most effectively to S&T evaluation and assessment [18].

As you see, Garfield understands everything very well and does not contradict the above.

I will add what has probably passed unnoticed in the article by B. Shtern, published in *Nezavisimaya Gazeta* (Independent newspaper) of May 14, 2003 (Shtern is an active adherent of the citation index, but as a professional, he understands its drawbacks):

> The citation index, if we absolutize it, may impair science just like TV and other mass ratings impair culture. Broadly speaking, there is a risk that science will turn from research method into a method of increasing the index; in the West, certain scientific fields already show these symptoms. Hence, we must not ignore peer review of different kinds in assessing scientific results.

However, to be completely honest, I have to cite the end of this quotation:

> However, we have no respective traditions at present, nor did we have them before the crisis. Science is disunited; we have good scientists, but there is no recognized authoritative stratum; this is a deadlock. The citation index can become a rational guideline to escape the vicious circle, and then it should go back to its unpretentious and useful place of "the null-approximation method." The next approximation may be made only by an unprejudiced reviewer, when there are people whom we can charge with this role.

However, whether we do have unprejudiced reviewers or not, the citation index must not replace them, even for a time. In my opinion, there are plenty of competent scientists who can work out objective rules of assessing a researcher.

LET US ASSESS A SCIENTIST'S CONTRIBUTION IN A COMPLEX WAY

Taking into account the above analysis, we see that both this technology as such and its results are inadmissible for an objective assessment of a scientist's work, such as that necessary for making personnel-policy decisions. The assessment of scientists' activities should take into account a complex of bibliometric and, in the first place, nonbibliometric data.

In order to obtain a correct assessment of a scientist's real research potential, analysis should be conducted in accordance with clear predeveloped rules. During the elections to the Academy, these rules should be accepted on the basis of the informed consent of a specialized section's members, so that candidates could verify the correctness of assessments and, if there are mistakes or biases, enter a protest in good time. A commission capable of conducting a true peer review should present scientometric and other data, taking into account numerous aspects of candidates' activities. Such an approach could indeed contribute to an objective assessment of scientists' activities and substantiate their election to the Academy and other decisions in the personnel policy. Apparently, we should test acceptable criteria during the next elections.

Here are the nonbibliometric criteria that are most frequently mentioned in the literature.

• **Additional financing of a project or scientist from external sources.** This is the most usual quantitative indicator of a scientist's significance and recognition among other scientists. Granting is based on objective analysis of the content of the work in question. In addition, this parameter estimates the modern attitude to this work, not the previous one.

• **Participation in students' and postgraduates' work.** Today, this is the most important indicator in Russia, which characterizes, on the one hand, the attractiveness and value of scientific subject matters

and, on the other, the promising character of the trend and its leader from the point of view of the revival of science in our country.

• **Indicators of the recognition of a scientist among the scientific community.** This means membership in foreign academies, participation in the editorial boards of scientific journals, membership in professional communities, different awards, invitations to take part in scientific conferences, and many other easily accountable factors. These indicators have their drawbacks: they work badly with regard to young scientists but are good for prominent ones.

• **Place where the work in question has been conducted.** In Russia, national laboratories have an undeniable priority in this respect.

Apropos, Academician G.P. Georgiev has worked out a remarkable experience of such analyses as an approach to choosing the most deserving candidates for grants within the framework of the RAS Presidium program on molecular and cellular biology. It seems to me that this system of assessments, which takes into account citation indices, impact factors, and many other indicators, does work. The complex character of assessment is very important—the more so that peer review often fails.

REFERENCES

1. P. O. Seglen, "Evaluating Biology: A Scientometric Study of a University Biology," Dep. NIFU. Skriftserie **6** (2001), http://www.nifustep.no/content/download/1096/12005/file/Skriftserie 6-2001.pdf.
2. P. G. Altbach, *The Tyranny of Citations*, http://insidehighered.com/views/ 2006/05/08/altbach.
3. *Quantitative Indicators for Research Assessment—A Literature Review* (Research School of Social Sciences), http://repp.anu.edu.au.
4. I. Niiniluoto, "Scientific Progress," Synthese **45**, 427–464 (1980).
5. I. Niiniluoto, *Is Science Progressive?* (D. Reidel, Dordrecht, 1984).
6. "Scientific Progress," in *Stanford Encyclopedia of Philosophy* (2002), http://plato.stanford.edu/entries/scientific-progress.
7. G. H. Whitehouse, "Citation Rates and Impact Factors: Should They Matter?," Br. J. Radiol. **74**, 1–3 (2001).
8. *Science, Technology and Innovation Indicators—A Guide for Policymakers*, Ed. by K. Smith, http://www.step.no/old/Projectarea/idea/Idea5.pdf.
9. *Research Evaluation and Policy Project Research School of Social Sciences. Quantitative Indicators for Research Assessment—A Literature Review*, http://repp.anu.edu.au/Literature%20Review3.pdf.
10. "Thomas Samuel Kuhn. 1922–1996," in *Stanford Encyclopedia of Philosophy*, http:/plato.stanford.edu/entries/thomas-kuhn/.
11. G. Klein, "Perspectives in Studies of Human Tumor Viruses," Front. Biosci. **7** (1) (2002).
12. D. Adam, "Citation Analysis: The Counting House," Nature **415**, 726–729 (2002).
13. "Citation Data: The Wrong Impact?," Nat. Neurosci. **1**, 641–642 (1998).
14. P. O. Seglen, "Citation Rates and Journal Impact Factors Are Not Suitable for Evaluation of Research," Acta Orthop. Scand. **69** (3) (1998).
15. "Recommendations of the Commission on Professional Self-Regulation in Science. Proposals for Safeguarding Good Scientific Practice," D-tsche Forschungsgem (1998), http://www.dfg.de/aktuelles_presse/reden_stellungnahmen/download/self_regulation@98.pdf (p. 8) (accessed Sep. 2002).
16. *Quantitative Indicators for Research Assessment—A Literature Review* (Research School of Social Sciences), http://repp.anu.edu.au.
17. S. Brenner, Science **281** (5373), 53 (1998).
18. E. Garfield and A. Welljams-Dorof, "Citation Data: Their Use As Quantitative Indicators for Science and Technology Evaluation and Policy-Making," Sci. Public Policy **19** (5) (1992), http:/www.garfield.library.upenn.edu/papers/sciandpubpolv19(5)p321y1992.

Postscript

Whenever you find yourself on the side of the majority, it is time to reform.
Mark Twain

After having written the above, I would like to say that I keep a sharp lookout for my citation index. This index is useful when used by an expert; but this should be an expert indeed, who learns the contents of works and their true place among others not from the editor-in-chief, a good friend of this expert. No doubt, people capable of self-assessment learn much from their citation indices. They feel the razor blade separating widely cited works, recognized within the existing paradigm and contributing much to its support, and hard-to-publish and scarcely cited works outside this paradigm, about which experts and reviewers typically say "what nonsense."

I will not give examples of my own works and will better refer to the above episode with the MMTV virus, which Bittner did not describe. As for my own works,

I know which of them I can send to a "good" prestigious journal and which will be readily accepted by a journal with a lower impact factor and more relaxed competition for journal space among high-quality articles, easily understood by reviewers and containing no germs of scientific progress. Many of my articles that, in my opinion, are marked with my real achievements are published not in "high-fashion" journals. However, I am surrounded with young people, postgraduates and students, whose career depends on the impact factor of the journal in which they publish their works; nothing human being alien to us, we press towards prestigious publications, even though we understand their true value. Life is life.

In addition, the peer review system, used to assess individual works before publication, is imperfect. We know many absurd cases when works that later received the Nobel Prize had been rejected by journals after peer review. A widely known example is the study on the Krebs cycle that was rejected by *Nature*. However, the work of reviewers who analyze an *individual* article, sent to a journal, and that of a review commission that analyzes the *totality* of a scientist's studies differ very much. The simplest thing that distinguishes a reviewer's work from counting citations is that a reviewer should establish whether the author in question conducts systematic studies in a definite trend or just "tours," doing this in one place, on the existing facilities and on an errand of the local boss, and that in another, where he equally depends on others. A reviewer must establish whether the study under examination is a mere improvement of methodology or whether it contains a basic discovery, for example, of a new oncogene and the investigation of its functions. It is all the same for citation figures, but in reality, when we elect the country's scientific elite or decide who should head science, we must look into what these figures say.

Academician
E. D. Sverdlov

Postpostscript

I sent the rough draft of this article to all members of the Section of Physicochemical Biology and received many comments, both oral and written. The vast majority of my colleagues agree with the fundamental aspect of the article. I have taken into account certain ideas of my colleagues and made respective corrections in the text. I had a long discussion with Academician V.L. Skulachev, who thinks that the publication of my article will abolish the use of scientometric data altogether and "will obligatorily increase the number of nonscientists elected members to the Academy." In my opinion, the point of my article does not imply the abolishment of bibliometric data. The point is that we must not use them alone: the assessment should be complex.

It seems to me, many comments of my colleagues should be quoted. I have begged permission to publish their letters and have received it. I hope that I have not distorted the essence of these comments by tearing quotations from the context of these letters. I give practically no comments: my colleagues understand the problem equally well, and I am not a judge. Nevertheless, wherever I feel that comments are necessary, I italicize them in brackets.

Academician
E. D. Sverdlov

Academician **V.V. Vlasov** (Novosibirsk). (1) The beginning of the article is about the elections. The reader may think that the problem has arisen because of them. Perhaps it is worth recalling that Russian scientists turned the citation index into a bludgeon long ago. Perhaps you had better list names, because there is no stopping those who love to publish tables. A table was once published straight in *Poisk* before elections. That table understated the articles of a number of scientists by *orders of magnitude*. It was so hideous that later the newspaper apologized decently. But this was *after* the elections to the Academy! In other words, the trick succeeded. And this underhanded measure is repeated each time before elections. During the recent elections, Limborskaya's citation index was considerably understated (*S.A. Limborskaya is one of the corresponding member candidates*)—can we call this a mistake? These little tables are not harmless, even among a highly intellectual gang …. And if we have to count, an organization should be charged with this, which would be officially responsible ….

(3) As for elections, it would be advisable to present ten of the most cited works by each candidate to the review commission. This would allow the reviewers to have a look at them and to see which works are coauthored and why this or that work has been cited ….

(4) Your article is written in a very scientific manner, for scientists, who understand the problem and the importance of a many-sided approach. However, its present form may imply that you completely refute citation counting. Some people may take advantage of this. Hence, it would be advisable to write that citation counting is important even in its primitive form. As a barrier. For example, there was a list of functionaries whom certain people wanted to push to the Academy.

In this case, it would be good to have figures on their citation.

(5) There are balanced approaches. For example, Georgiev's program (*The RAS Presidium basic research program Molecular and Cellular Biology*) seems very balanced and rational.

Another thing: we can sharply increase the meaning of citation figures. This was invented by V. Soifer, when he racked his brains over the assessment of Russian scientists who applied to the Soros Educational Program grants. Indeed, various big bosses had high citation figures. Soifer did the following. First, he withdrew self-citation. After this, he withdrew citations by team members (who worked for the paradigm). In other words, he withdrew references made by people who had once been coauthors of the scientist in question. The result was first-class: citation of big bosses sharply shriveled, and scientists came to the fore—those who had been cited by people not associated with them, because they deserved to be cited. This is especially important for our scientists who are readily cited by their compatriots and teammates but understated by foreign colleagues ….

Once again: it would be good if your article were published in a form that could not be useful for those who would like to abolish citation counting altogether. Otherwise, we will begin to elect Chechen bankers and governors to the Academy. (*I agree with this; the article says only that citation alone must not play the role of an evaluation factor*).

Academician **G.A. Zavarzin** (Moscow). Our section's enthusiasm about the citation index has begun to leave the limits of an auxiliary material for judgment. The question is to what degree this is universal, as far as other scientific fields are concerned. In particular, this is important in assessing works made outside the Academy circle and in financing studies and scientific schools. For example, grants by the Russian Foundation for Basic Research are below the level of such assessments, because they are designed for young scientists with a small number of publications.

I am a convinced skeptic as far as the significance of the citation index and impact factor is concerned. Your thorough investigation into the role of bibliometrics coincides with my intuitive estimation. It is difficult to give a quantitative assessment to fundamentally qualitative phenomena. The use of the number of citations in journals is intended to meet the bookkeeping level of the customer, including officials who make decisions. Statistics alone may be misleading in this respect. In principle, this is a field of discrete mathematics. In biology (and not only in biology), we have to deal with it when developing classifications. This is a special and difficult field ….

In your manuscript, you restrict the general problem of assessing the significance of scientific achievements to the field of biology, even to a narrow branch of biology. To what extent can we regard the specific problem of the Section of Physicochemical Biology as common for the Academy as a whole and, the more so, for science as a whole?

The use of the citation index as a weapon in the sectional struggle, based on the behavior of small groups, should be attributed to social psychology. Specialists have developed political strategies here. Hence, the elections to the Academy should be regarded as a particular example that cannot have primacy in a discussion, although this example is demonstrative. However, this particular concern is of considerable importance for the Academy.

Problems of epistemology have become very interesting—perhaps, among our age cohort. Hence, your references to Popper and Kuhn are very demonstrative. It is significant that you refer to Kuhn as far as paradigms are concerned, not "normal science" that is a major part of his book. It is normal science that requires the citation index, as well as the number of publications, to substantiate itself. You have omitted this quantitative indicator, although it is used in assessing individual scientists as well. In philosophy, normal science is well characterized in works by A. Schopenhauer, M. Weber, and M. Heidegger. Note that I have taken a sequence of German scientists, not English-speaking ones. This is not by chance.

Popper's statement that a reference to a source is not a proof, because it leads to *reductio ad infinitum*, is noteworthy. The number of references to sources in the modern English-speaking literature has become a criterion characterizing the author's polymathy. Authors use bundles of references without analyzing the contents of the articles. In the epoch of the Internet, this method of misleading the consuming reader has become a mass phenomenon and is devaluing the significance of the citation index by developing an information boom based on a sampling of keywords.

The indication of the influence of the research field is very good. For example, a while ago, the influential publishing house Nedra made its authors–geologists use a minimal number of references in their monographs. I do not know what the current situation there is. The mass character of studies does not mean they are significant; it means that they are fashionable. Following fashion is one of the methods of affecting public conscience, i.e., popularizing (advertising) this or that to receive surplus profit ….

Of course, peer review is one of the most important factors in assessing individuals. Personal influence plays a role in this respect, which is especially significant within national scientific schools. For example, L. Gumilev had become a significant figure long before mass publications of his works ….

It is especially noteworthy that peer review is far from being objective as a method of assessing the significance of works; it rather shows their conformity with normal science. For example, Mitchell, the developer of chemiosmotic bioenergy, could not publish his

studies in a "normal" way; there are plenty of such examples concerning the most significant works. It is difficult to reject a "normal" work.

Corresponding Member **S.V. Razin** (Moscow). I think that the publication of your article will be extremely useful and timely. The only thing that worries me is the problem of journals' impact factors. It seems that many scientists do not understand what they mean. Meanwhile, today the most prestigious journals with high impact factors (*Nature, Science, Cell*, etc.) are using the well-known principle of the yellow press, because the editor selects and sends to reviewers only articles that, according to the editor's experience, will be widely cited. On the whole, the preselection of articles without reviewing is extremely harmful, because no one can be a specialist in all fields. In this connection, journals that send all articles to reviewers (*Molecular and Cellular Biology*, *Journal of Biological Chemistry,* and so on) turn to be more interesting and informative, although their impact factors are not the highest. It would be good to say something about this as well. (*I feel it necessary to note that Razin is one of the champions with regard to the citation index.*)

Corresponding Member **E.Ya. Gren** (Riga). I fully agree with you. We have the same problem in Latvia and everywhere. These quantitative and easily countable indices are becoming increasingly popular among scientific bureaucrats with no critical analysis; it is just like in Soviet times, when we calculated results of socialist competition. Your article is timely.

Another correspondent (*I am not sure that I have received a permission from the author of this letter to publish his name*). Your article raises a very important question, the significance of which, in my opinion, may hardly be limited to the elections to the Academy and covers the entire system of assessing scientists' works and the country's organization of science. I think that attempts to formalize criteria and work out quantitative standards of a kind are based on a good intention: to get rid of biased assessments. However, the trouble is that, as a rule, any standards are hardly admissible in estimating such a nonstandard phenomenon as science. In addition, criteria that are declared objective are sometimes used to disguise subjective goals. At best, this is a self-deception; at worst, a consummate lie.

As for the topic of your article, I fully agree with you that the citation index as a criterion (and the impact factor of a journal, which is closely connected with it) is far from being perfect. The works you cite consider how to interpret bibliometric information correctly. I would like to focus on the objectiveness of this information itself. Perhaps, before the impact factor and citation index began to be used for assessing works and stimulating scientists (in particular, for distributing posts and grants), they had indeed reflected this or that situation, and the problem was that it was necessary to understand what they reflected. However, today these criteria are forming themselves; they actively influence scientists' careers and choices: consciousness determines being, so to speak.

Since now it is more important how many times you have been cited rather than what science you do, "Homo sapiens" begin to climb the ladder by securing maximal citation. What should one do for this? The recipe is simple and well known.

(1) The simplest way is to cite oneself as often as possible. This often yields fruit because data-processing programs are not always good at withdrawing self-citation.

(2) A bit more complicated method is to join a group of colleagues who readily cite each other.

(3) To be always involved in a fashionable research issue. To learn what is fashionable, it is enough to look through *Nature, Science*, and *Cell* regularly.

(4) To do one's best to be a member of as many editorial boards as possible. An editorial board member is always readily cited because it is highly probable that he (she) may become your editor or reviewer.

(5) Aerial acrobatics is to become a member of a grant-distributing commission.

As for Russian scientists, they should attend international conferences as often as possible, make acquaintances with foreign scientists (especially influential ones), and try to please them. One should also try to publish works coauthored with foreign colleagues, especially with ones from the United States. This is of triple usefulness. First, such a "locomotive" makes it much easier to publish your work in a journal with a high impact factor (which is a plus in itself when applying for a grant); second, the fact that your work has been published in such a journal guarantees high citation; and third, citation grows because Americans readily cite Americans. For this, it is useful to work at US laboratories as often as possible, no matter what job you may be charged with. True, in this case, you often have to be content with secondary functions, but the end justifies the means.

Of course, works made in cooperation with Western colleagues are not reprehensible and are even encouraged; sometimes, they are even an obligatory condition for receiving international grants, and such cooperation may result in good publications. However, it is clear that the average citation index of such coauthored works is hardly proportional to their scientific value.

We cannot blame a person for choosing this way: it is all the game, and its rules have been established by others. But it is bad when we, members of the Academy, help this system to prosper. Although there are examples when high citation and scientific merits do correlate (as a rule, this is observed among the older generation, the representatives of which won their reputations prior to the impact-factor epoch), it is clear that the citation index increasingly often reflects the author's career activities and influence among the sci-

entific community rather than his or her scientific contribution.

I agree with you that we should return to nonformal peer review, assessing scientific works by their merits. It goes without saying that such an assessment is subjective and fully depends on the reviewer's wisdom and decency; yet this is the best possible option. A system based on formal criteria is pseudoobjective and contributes to the fact that incidental people become reviewers.

RAS Corresponding Member **N.V. Tomilin** (St. Petersburg). In addition to your proposals, I would like to note that, for Russian laboratories, financing from international foundations (not only from internal sources) could become an important indicator. In my opinion, it is better to formulate participation in students' work as participation in the work of young people in general (under 35). While giving the priority to national laboratories, it is worth remembering that the majority of provincial laboratories lack modern equipment and experiments may not be conducted at home.

RAS Corresponding Member **L.V. Kalakutskii** (Pushchino). Thank you for sending me the draft of your article on citation indices and their use. I think it is obviously purposeful and timely to publish it. This is not only in connection with the elections and the reduction of personnel but also in connection with a more general question of whether science (and, maybe, any organized corporate activity) can offer criteria of assessing its activity, useful for its improvement. I will not go into detail concerning the harm of "campaigns," as well as concerning ethicality/unethicality of imposing anything on the electorate.

I agree with you (and Garfield) that documenting citation echoes may become a useful additional method of assessment, including that of scientific activity. I think that the attractiveness of such an approach is at least partly due to the fact that results may be presented in a habitual quantitative form, which provokes further arithmetic speculations. Nevertheless, I would not rank citation next to lies and damned lies, because of, but not limited to, the fact that there are considerable problems with "complex assessment" as well, where quantitative indicators are used on a smaller scale. We will see that assessments made by reviewers who are inside and outside a system under examination (the Russian Academy of Sciences in our case) differ very much if we compare, for example, texts and conclusions found in S. Belanovskii's "The Assessment of the State of the Russian Academy of Sciences (Summary)" (http://www.cbio.ru/v5/modules/news/print.php?storyid=1705) and G.V. Osipov's *The Russian Academy of Sciences as a Great National Endowment* (Izd. Ros. gos. un-ta, Moscow, 2006).

It is possible that future search for complex assessments will use ways known among geneticists and ecologists through their professional activities: developing virtual scenarios of "knockouts" and estimating their potential consequences. Not to look too serious, I would like to note that Popper's criticism of Marxism did not provoke considerable discussions, while his original assessment of Darwinism did raise a stormy discussion.

RAS Corresponding Member **A.G. Degermendzhi** (Krasnoyarsk). I fully agree with your approach and conclusions. The citation index is primarily necessary for self-assessment, and we must not rank scientists according to it. The drawbacks of this index were described by Academician V.V. Vlasov in his remarkable interview to *Poisk*; it is noteworthy that Vlasov's personal index is extraordinarily high and he supports the rating system that helped improve the efficiency of my Institute of Biophysics and the publication level of my colleagues. Apropos, we consider good publication indicators a necessary condition of a researcher's good work; as for "sufficient" conditions, we ascertain them while analyzing results at a workshop or a meeting of the academic council, and I think it will be useful for our section to adopt these practices. I see no opportunity to introduce this index in the rating. As for the impact factor, its significance is very high. It is not enough to conduct a good study; the scientific community should know about it.

The current situation shows that it is physically impossible to read all journals: for example, 50 journals on hydrobiology are issued monthly, each of which contains 20 articles …. If one is interested only in a narrow field, this is the end of science. The way out is simple and pragmatic: ten journals with high impacts are read throughout the world. In other words, there are journals that one cannot help citing and there are journals that one may never cite! Obviously, a scientist who has buried even a Nobel Prize result in a local journal published in a native language will make no contribution to science. This result will be rediscovered and published where it should be published. Hence, a professional researcher cannot do without publications in "impact" journals! Another pair of shoes is that, as you write, impacts should be compared within scientific fields; hydrobiology cannot be placed on the same footing with molecular biology. I once even offered to normalize current impacts according to the average impact of journals in a certain field. In this case, we could compare "fashionable" and "unfashionable" trends. Apropos, in addition to the impact factor, there is the half-life (*half of the period during which an article is cited*) of a quotation; this is a very interesting indicator, which incorporates the real "influence" on progress and the "memory" of the result. (We should discuss this.) Molecular biologists have an average impact of 5, but a quotation's half-life is 2 years, while hydrobiologists have an impact of 1, but their average half-life is 10 years (this is determined by the characteristics of laboratory and field science, as well as by the nature of the subject of inquiry). For example, *Nature,* if I am not mistaken, has a low half-life, while *Zhurnal obshchei biologii* (RAS Journal of General Biology) has a half-life of 100 years (it would be advisable to verify this).

Multiply one by the other, and you will receive an astonishing result!

It is clear that all scientometric indicators should be based on a large sampling, as you rightly point out in your article. If the averaging is conducted for a certain year, they should be based on a large team. If it is done for an individual, they should be based on a long period (five to ten years). Both impact and other indices are a rough estimate for withdrawn extremes: the academic council should worry if a doctor of sciences has published one article in ten years in a journal with a zero impact. However, all these indices should go together with the traditional assessment by the academic councils of institutes and departments! On the basis of such assessment, we have recently closed a laboratory with a high impact because it demagogically exploited a fashionable "topical" subject. The academic councils of our institute and the RAS Siberian Division, as well as commissions of the Academy and the RAS Siberian Division, unmasked this demagogy and pseudoscience.

Impact is not a dogma but information for the academic council! This is *information*, not the highest and only criterion for assessing the quality of a scientist's activity. Yet a zero impact and citation index over ten years is not even a diagnosis; it is a verdict ….

A few words about additional indicators to those you suggest. The level of the pragmatic use of results should be reflected (previously, it was called *implementation*). The Academy is criticized for the lack of this measure, but in reality, it has wonderful practical results ready for application.

Academician **G.P. Georgiev** (Moscow). The article is very good and convincing. I can add only a few things.

(1) If we account for the CI (*citation index*), we should analyze every work and allow for the author's contribution, as we do with the IF (*impact factor*) of journals. Let us take an extreme case: an article on genome decoding will be cited 1000 times, but this does not mean that the achievements of one of the hundred or more of its coauthors are great indeed. Most likely, this author was merely a technical executor.

(2) This is a very important aspect for Russian authors, because it is often the case that a frequently cited article was written abroad, where our author was a technical executor.

(3) On the other hand, it is often the case that a Russian author is the leader and innovator, but references are made not to the Russian author but to the subsequent work by a Western author. This Western author says nothing about his or her predecessor in the introduction and then cites our author briefly in the discussion as an author who has obtained similar results. After this, all the dividends go to the secondary work.

(4) Hence, it is easier to collect high citation indices for our authors who often work abroad than for those who work in Russia.

(5) The social activity of this or that author plays a great role.

(6) Nonstandard works can receive a high citation index, but usually this happens after a long while—after the paradigm has changed.

(7) Reviews are useful, but their significance is considerably lower than that of original discoveries, while their citation index is considerably higher. The same is true with regard to the impact factor of review journals. Hence, calculations should be conducted separately for original and review articles.

There are examples from my own practice. Nedospasov (**RAS Corresponding Member S.A. Nedospasov**) and I published an article in *BBRC,* in which we described a method of localizing proteins (nucleosomes) on DNA. The idea was strong, but the fulfillment was not so good. This predetermined the choice of the journal. In a year, the American Wu solved the same problem at a higher methodological level. In another year, H. Zachau wrote a review on this subject, in which he cited only Wu. He sent this review to me. I wrote him a resentful letter at once and attached a reprint of our work to it. Zachau put us in his review before Wu, and since then this work has been cited together with Wu's work and has received a high citation index. The fact that I knew Zachau personally and my social activity was high played a key role in this. Otherwise, our citation index would have been close to zero.

My innovation works, coauthored with Gvozdev (**Academician V.A. Gvozdev**), Il'in (**Academician Yu.V. Il'in**), and others, on repeat motility in the genome, published in *Science, Cold. Spring. Harbour. Symp. Quant. Biol.*, and *Chromosoma*, were fully repeated by Americans in a year and a half. They made reference to us, but only in the discussion. As a result, mainly their work was cited afterwards. Such examples are numerous. One of the ways of misappropriation is to rename a gene or protein.

RAS Corresponding Member **B.F. Vanyushin** (Moscow). I fully agree that the significance of a scientist's work should be assessed complexly. In my opinion, your reasoning should not be associated with the previous or future elections. This makes your work look less significant and may seem a "grumble," the more so that members of our section hardly took citation into account. Knowledge of the scientific situation and the tendency of knowledge progress won.

The lack of citation is also an indicator: this means that the scientific community either does not know the work in question or ignores it on purpose. For example, one of the world's first works on DNA hydrazinolysis belongs to Bur'yanov and me. When I held a workshop at Harvard, Gilbert (*a Nobel Prizewinner*) attended it, and it turned out that he knew our works perfectly, but it was not to his advantage to cite them, because this comes short of accepted US standards: you may not receive a grant.

It is a shame that very many of our works published in prestigious domestic journals that are translated into foreign languages (*Molecular Biology*, *Biochemistry,* and *Doklady Akademii Nauk*) are either not cited at all or cited on a very small scale. Example: Mazin discovered nonenzymatic methylation of DNA (without proteins) in the presence of S-adenosylmethionine. Nobody paid attention, and this prestigious observation is not cited anywhere. Conclusion: the majority of domestic publications are an undertaker's office for many reasons. We are fools. Hence, a work's significance should be assessed according to its contribution to knowledge.

It seems to me that your article should contain examples. You know that with regard to the number of citations, the first place among all fields (even compared to Marx and Einstein) was once occupied by Lowry (protein determination). This is a useful work, but Lowry did not become a Nobel prizewinner. Hence, citation is not the main thing. It is often the case that knowingly erroneous ideas and works top the list of citations. Hence, it is sometimes useful to write stupid works. They say that such works stimulate the search for truth. However, their scientific significance equals zero or even a negative value.

RAS Corresponding Member **V.E. Vas'kovskii** (Vladivostok). If your article is read by a person who knows nothing about the election results in the Section of Physicochemical Biology, he or she will think that citation ratings were distributed among the section members and, as a result, we have elected people on the basis of these data alone. Although I knew that this is not so, I did look through the citation data that were sent to me. It turned out that out of two academicians and three corresponding members elected, there was no "Number One." Numbers Three and Four were elected, as well as a person without any number (V.I. Tsetlin). I fully agree with you that citation does not always depend on the work's quality. Once I wrote a newspaper article "How to Write and Publish a Widely Cited Article." I showed by my own example that a wrongly chosen journal (international) may sharply decrease attention to a scientist's work.

Publications in Russian and international (foreign) journals are noteworthy. Let me take a very prominent Russian scientist—E.D. Sverdlov—as an example. He has about 20 cited works in *Doklady Akademii Nauk.* Each of them is cited 2 to 28 times. In addition, two of his works are published in *PNAS.* By July 18, 2006, one of them had been cited 115 times and the other, 1221 times. This scientist has other works each of which is cited more than 100 times—first class, as A.S. Spirin said at the section meeting, if I am not mistaken ….

As for a specialist, I gave your article to A.I. Pudovkin and asked him to read it. He is a geneticist from the Institute of Marine Biology. He has been cooperating with Garfield and ISI for several years …. He promised me to read it soon and to present his opinion.

(*I have to comment on this. One of my works, which I consider my best and in which I presented the idea of a modern and still used method of sequencing nucleic acids, was published in* FEBS Letters *in 2002 and 2003. It was cited probably two or three times. Later, the use of this principle led to the development of fast technologies in analyzing the sequence of nucleic acids and resulted in the Nobel Prize for the developers. I had personally handed over my* FEBS *articles to one of the prizewinners long before he published his first work on this subject. However, no references followed. This story was later described by Benno Muller-Hill in* The Lac Operon.

I must also say that Vas'kovskii has sent me Pudovkin's opinion. In short, it may be reduced (with regard to the point of the article) to "it is commonplace." I think that citation in my article shows that it was my intention to say things that are well known among professionals but often forgotten by "customers.")

RAS Corresponding Member **O.A. Krishtal'** (Kiev). My personal example shows another paradox that you may use if you like. I used to publish my most important discoveries in journals with low impact factors because I was afraid lest my work should be rejected and I would lose time and priority. I was more than right at least once: a US publication in *Nature* appeared three months after with a reference to my article made at the galley-proof stage.