Science, Citation, and Funding

Citation frequency has been the focus of recent articles in *Science* (News & Comment, 7 Dec., p. 1331; 4 Jan., p. 25) and the popular press (*1*). While the incidence of uncited papers is a matter of concern, the recent publicity may do more harm than good by encouraging the trendiest science rather than the best, by misleading the public to conclude that most scientific publication and research is a waste of money (*1*) and by prompting Congress to reduce science funding.

I maintain that infrequently cited research papers are often quite important, that suppressing the publication of papers in fields with low citation percentages would be a scientific and academic catastrophe, and that publication of those papers which are of truly limited utility is often simply the result of desperate competition for limited funding. Tacit acceptance of the notion that most uncited papers are without value would precipitate an additional shift away from publishing solid research papers in some areas, while possibly exacerbating the economic factors leading to a general and undesirable "rush to publish." I endorse the doubling of current science funding levels, as suggested by Nobel laureate Leon Lederman (11 Jan., Supplement), as the most realistic and prudent way to decrease the excessive pressure to publish induced by the current resource scarcity.

Regarding the publicized citation frequencies for subdisciplines, the more highly applied areas (with low ratios of researchers to applied workers) should have higher percentages of uncited, but still useful, papers. Also, the utility of some research results cannot be immediately appreciated because they are beyond the context of their fields, or their further application requires technology not yet available. Often, small specialized fields that later yield results of high scientific importance, such as the underfunded retrovirus field before the discovery of AIDS, may not provide widely cited papers within the 5-year period being evaluated. Yet, in covering an area of currently undervalued research, such papers may be more important than trendy ones with less rigor and unique information.

Another factor is journal bias. Given the choice of citing a short report in a superior general journal, such as *Science* or *Nature*, or a more comprehensive paper in a superior specialty journal, such as *Biopolymers*, many authors would cite the shorter report, perhaps to associate their work with the more prestigious journal. Additionally, most journals place limits directly or indirectly (by length restrictions) on the number of references authors cite, so those papers most readily available to readers may be chosen whether or not they are the best or the only appropriate references. To the extent that citation frequency reflects prestige and circulation rather than scientific quality, the focus on citations may aggravate the problem.

A final and major concern is the use of citations to determine what types of science are worthy of publication. If major journals in a field decide that certain research areas no longer merit publication because of low citations, this effectively promotes censorship. I can best comment on an important example from my own field. In protein crystallography, crystallization often provides the major breakthrough for determining the structures. Yet, many top journals covering protein crystallography do not publish crystallization papers because of low citation frequency. *Nature*, for example, even rejected the crystallization paper for the photosynthetic reaction center (*2*), which was the breakthrough required for the first membrane protein structure determination, yielding Nobel prizes for Hartmut Michel, Johann Deisenhofer, and Robert Huber in 1988. The *Journal of Biological Chemistry*, which has published detailed protein crystallization papers, including novel reports of membrane (*3*) and fiber-forming (*4*) protein crystallizations, has recently ceased these publications. Trendy or not, detailed crystallization papers provide a service to the field by making useful information available to students, postdocs, and other researchers, and by building a valuable database critical to advances in protein structure determination.

In sum, the use of citation levels as an indicator of publication worthiness represents a disturbing trend. Peer review, despite its limitations, remains a better judge of scientific quality. If we remove research funding from its crisis footing, while continuing to exercise a balanced review and control of published work, scientists will be able to complete projects properly and publish comprehensive papers of the highest quality and value to both science and society.

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Hamilton quotes Pendlebury's finding that 55% of the papers published in 1981–1985 in all sciences were never referenced (cited) in the subsequent 5 years. If that were true, scientists would be guilty of publishing mostly trivial papers. This percentage seemed inappropriate for the physical sciences, so we determined the values for astronomy and physics. We will call papers not cited in 5 years "trivial," even though many may be of value ultimately.

For astronomy we collected data for most of the American papers published in the last 3 months of 1979 and searched to see if they were cited in the 1980–1984 *Cumulative Index of Science Citation Index* (SCI), the most recent 5-year index currently available. Included were the 512 research papers published in the *Astrophysical Journal* (including the *Letters* and *Supplements*), *Astronomical Journal, Publications of the Astronomical Society of the Pacific,* and *Icarus*. Only 26 papers were never cited, yielding a frequency of 5.1 ± 1.0% trivial papers. If we include those papers that were only self-cited, the frequency increases to 6.4 ± 1.1%. Such low frequencies seem reasonable because not all research projects that promised to be exciting become so.

Turning to physics, we scanned the 602 research papers published during the last quarter of 1979 in *Physics Review Letters, Physics Review A, Journal of the Optical Society of America*, and *Physics of Fluids*. We found that 49 papers were never cited in 1980–1984, giving a frequency of 8.1 ± 1.2% trivial papers. Again, if we add the papers only self-cited, the frequency increases to 11.4 ± 1.4%

Why is it that we obtain such low fractions of trivial papers in astronomy (5.1%) and physics (8.1%), while the frequency quoted from Pendlebury at the Institute for Scientific Information (ISI) for all sciences gave a percentage of 55%? One obvious reason is that we did not include abstracts of meeting papers, book reviews, errata, editorials, letters, announcements, obituaries, and so forth, which are all seldom referenced; the quoted 55% includes both those and original research papers. The breakdown furnished by Pendlebury shows that 35% of the items searched in the ISI study are of this secondary type. If 35% of the items had very few citations and the remainder had 5 to 10% trivial papers, the sum of the two would be roughly 35% trivial papers.

A second reason for the discrepancy between the two studies is that we selected journals with relatively high impact factors (median of 2.5), whereas the median in the ISI study of all journals was 1.28 for astronomy and physics; it included journals in non-English languages that are read and referenced by few scientists. For physics journals with an average impact factor of 1.28, 25% would be trivial. This fraction of 25% trivial papers plus the above 35% nonresearch material approximately explains the 55% quoted frequency.

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Hamilton's statements about the prevalence of uncited research articles are at odds with results reported in previous studies of the physical and biological sciences (*1*). They also differ sharply from the results of our current study of citation histories of sociology articles. Hamilton reports that only 23% of articles in sociology journals covered by the Institute for Scientific Information (ISI) are cited within 4 years after publication. In contrast, our study of 379 articles published in sociology journals covered by ISI found that 43% were cited in the *first year* after publication. Even among the 121 articles in our sample published by journals with the lowest (below 0.5) ISI "impact scores" (*2*), 28% were cited in the first year. Six years after publication 83% of the total sample and 71% of the articles published by low-impact journals had been cited. This demonstrates that the great majority of sociology research articles are cited in the subsequent scientific literature.

What accounts for the striking discrepancies between the results we and others have obtained and those reported by Hamilton? First, scholars make many kinds of errors when they cite previous work. Computer matching routines are less effective in identifying these errors than researchers carefully inspecting the pages of citation indexes. Second, it is possible that ISI "source items" have been misidentified as "research papers" and may include such items as letters to editors and book reviews. Journals in the social sciences and humanities often feature large book review sections and publish extensive commentary and debate. Including such documents in studies of "uncitedness" will produce results that underestimate the citation levels of research papers and exaggerate differences in citation levels between the natural sciences, the social sciences, and the humanities.

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Hamilton highlights the fact that many papers published in science journals receive no citations in the 5 years after their publication. While it is undoubtedly true that some proportion of the literature remains uncited or cited at a very low level, the figures reported by Hamilton are misleading, as they are based solely on the *Science Citation Index* (SCI) database. The SCI is one of the most comprehensive of databases in science, but it does not cover all potential citing sources. Moreover, it does not even include *every* bibliographic reference in the sources it does cover (for example, reference in the Japanese alphabet are excluded, which could cause a significant underestimation of the impact of a Japanese paper).

The SCI is a tool of inestimable value in bibliometric and scientometric research, but one should be aware of the dangers involved in basing conclusions on data from a single source and in placing too much reliance on electronic databases in general. In a recent study performed in the United Kingdom, it was discovered that only about 5% of the total literature on some research topic could be found through searches performed in online databases (*1*).

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It is amusing to read that "New evidence raises the possibility that a majority of scientific papers make negligible contributions to knowledge," when many of us have known for years that this is an established fact. I question the value of citation statistics as gathered by the Institute for Scientific Information as a measure of the worth of any particular paper. For example, the January 1990 issue of *Scientific American* contains an article to which six references are appended, five of which are to papers in which the senior author is also the senior author of the paper citing them. Clearly, the aggregation of citations may not be a useful criterion for determining a paper's worth.

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Academia is not the only domain of science or technology, and many journals have applied emphases and are printed so professionals can *use* the information, not so they can cite it. The subjective "Publishing by—and for?—the numbers" will likely generate its own list of citations and, unfortunately, abuses by those who half understand the information systems of the various sciences. Lists of "papers most cited" perpetuate a similar brand of dubious scorekeeping. As a geologist, I am fully aware that no earth science paper has yet made such a list nor is one likely to do so. This is because citation frequency is as much a measure of the size of the specialty field as it is the quality of the publication. When academic administrators make tenure decisions on the basis of citation frequency, they indicate that research in small specialty areas is not "worthy science." As a result, scarce specialty expertise could become even scarcer.

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As I was reading "Publishing by—and for—the numbers," I was struck by a thought I have had over many years of reading *Science*. In the same issue, I counted the citations in two articles, two research articles, and 18 reports. Of 645 citations, 19 were in non–English-language journals, and of these, 9 were probably in English, and the rest may be also. In 13 of these papers, all of the citations are in English.

Surely people who do not publish in English have published information that is pertinent to at least some of these papers. Is this a reflection of today's education of science majors?

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Very few published (and, one hopes, cited) scientists have not by now read about Pendlebury's study of citation rates. Many feel that scientific papers can also have an impact through uncited routes, but examples are hard to document. However, the news articles discussing Pendlebury's paper seldom have reference lists or are in the database of the Institute for Scientific Information. Isn't it ironic that the impact of Pendlebury's paper on the scientific community has been entirely through an uncited route?

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Hamilton's two articles about the percentage of journal literature that remains uncited within 5 years of publication require comment and further explanation. The figures reported by Hamilton—47.4% uncited for the sciences, 74.7% for the social sciences, and 98.0% for the arts and humanities—are indeed correct. However, as Maxine Singer was quoted as saying in Hamilton's first article, it is necessary to know what's in the numbers before interpreting them.

These statistics represent every type of article that appears in journals indexed by the Institute for Scientific Information (ISI) in its *Science Citation Index*, *Social Sciences Citation Index*, and *Arts & Humanities Citation Index*. The journals' ISI indexes contain not only articles, reviews, and notes, but also meeting abstracts, editorials, obituaries, letters like this one, and other *marginalia*, which one might expect to be largely uncited. In 1984, the year of the data quoted by Hamilton, about 27% of the items indexed in the *Science Citation Index* were such *marginalia*. The comparable figures for the social sciences and arts and humanities were 48% and 69%, respectively.

If one analyzes the data more narrowly and examines the extent of uncited articles alone (this information was not yet available when Hamilton wrote his articles), the figures shrink, some more than others: 22.4% of 1984 science articles remained uncited by the end of 1988, as did 48.0% of social sciences articles and 93.1% of articles in arts and humanities journals. It ought to be pointed out that the book represents a considerably more important vehicle of communication in the social sciences and humanities than in the sciences. The figures given above reflect only the journal literature of the social sciences and arts and humanities.

The figures originally quoted by Hamilton seem to have been interpreted by many readers as some sort of measure of the health of U.S. science. The numbers, however, reflect a lack of citation of papers by authors the world over—not only those by U.S. researchers. This point was raised in Hamilton's first article.

If one restricts the analysis even further and examines the extent of uncited articles by U.S. authors alone, the numbers are even less "worrisome." Only 14.7% of 1984 science articles by U.S. authors were left uncited by the end of 1988. We estimate the share of uncited 1984 articles by non–U.S. scientists to be about 28%. (Comparable figures for social sciences and arts and humanities articles by U.S. authors are not yet available.)

A certain level of "uncitedness" in the journal literature is probably more an expression of the process of knowledge creation and dissemination than any sort of measure of performance. A trend toward more or less "uncitedness," however, might be meaningful. For the 1980s, we see no such trend in the scientific literature: the numbers are essentially flat, both for the United States alone and for the world. In the social sciences, however, we do detect a decrease in uncited papers—from 49.7% for 1981 articles to 45.3% for 1985 articles. In the arts and humanities, the figure of 93% uncited is fairly steady from 1981 through 1985.

This, we hope, serves to illustrate the great range of statistics one can derive depending upon what "cut" is made from the ISI databases. For example, articles published in the highest impact journals like *Science* are almost never left uncited.

We will be generating, over the coming months, article-only statistics, both U.S. and worldwide, for subdisciplines in the sciences, social sciences, and humanities, corresponding to the overall database statistics referred to by Hamilton in his second article. We have not yet produced a report on these statistics, but in light of the great interest in the numbers, we will now do so.

We hope this information clarifies the record and will end further misunderstanding or politicalization of these statistics.

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System Safety

M. Elisabeth Paté-Cornell's article about system safety (30 Nov., p. 1210) attempts the laudable feat of adding management and oversight processes to a review of safety, but the article is badly flawed by a peculiar, nonscientific view of human performance. Paté-Cornell suggests that some errors "can be attributed to bad luck" (p. 1210) and that some human-attributed errors might be due to "sheer stupidity" (p. 1213). "Bad luck" and "stupidity" are nonscientific, personal attributions that have no role in a scientific discussion of human behavior.

It is the job of cognitive scientists to understand and explain human behavior. We do not use judgmental terms such as "stupid"; rather, we try to determine the circumstances and mechanisms that lead to the behavior. At the time of action, nothing is stupid; that is a judgment placed later, but it has no scientific standing. Calling an event "bad luck" or "stupid" is not helpful, especially in the review of accidents and errors. The real problem with the use of such terms is that they excuse the accident researcher from further responsibility instead of leading them to discover the circumstances that led to the behavior.

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Response: In my study, I reserved the phrase "bad luck" for a specific class of failures: those due to overload. For example, a platform is designed for the 100-year wave, but the 1000-year wave occurs within its lifetime and destroys it. In this case, the chosen design criterion was considered acceptable by the profession, the usual safety factors were applied, the system's capacity was not decreased by a major error, and an event that was not unpredicted (although it had a low probability) occurred. There is no organizational malfunction that needs correction. The choice of a more stringent design criterion would simply have changed the probability of "bad luck."

The term "stupidity" is defined by my dictionary as: "showing a lack of sense of intelligence." I used it with this meaning to characterize, and if possible to prevent, a class of problems such as the following: an operator has received the proper training for the environment in which he or she works, and procedures exist to guide actions under most circumstances; but some situations are impossible to anticipate and require a level of reasoning capability that is simply not there. Social science research can then provide guidance to those addressing further questions, for example, What are the reasoning capabilities required for this job? What screening procedures can be used? How can these capabilities be enhanced by training or improvement of the working environment? Similarly, it may be helpful to recognize that the problem with the freeway next door is that it could collapse in a large earthquake. The question to the scientists is then, What levels of earthquakes can be anticipated? The question to the engineers is, How can adequate resistance be achieved? My function is not to point a finger after an accident but to anticipate possible failure scenarios. A problem can be clearly identified as "weakness" or "stupidity" or described in apparently neutral terms. Being explicit about the problem is not "an excuse" to leave it at that (neither to the researcher nor to the organization) but is, on the contrary, the beginning of a search for an appropriate solution. I believe that, in a problem-solving mode, it is helpful to "eschew obfuscation." I agree, however, that in the scientific context, useful common terms sometimes require a precise definition.

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