Letters

An Analysis of Citation Analysis

The citation index (News & Comment, 7 Dec., p. 1331; 4 Jan., p. 25) is a great artificial phenomenon, but not one worth citing. Like statistics, a little application causes haphazard outcomes. Methods papers have been removed from consideration because they receive too many citations (*1*). Other types of papers should also be removed from consideration by the citation index analysts, or perhaps be analyzed in a more meaningful way.

Review papers receive lots of citations, so with methods papers removed, reviews have replaced methods papers as front runners. And this seems reasonable. If a review has adequately summed up the literature, why not cite it instead of writing the review over again? For the analysts of the citation index, then, every paper cited by a review should also be given credit every time the review is cited. This should initiate a new way of analyzing citations, and the folks who make a living out of analyzing the citation index will have an exciting new enterprise to fill their time. One cannot stop there. Reviews are used to bury the literature and rewrite history. This is done by comprehensively recording all references in the first review, and then referring back to the first review by stating that "The earlier literature has been reviewed by whomsoever." Thus reviews can generate, either knowingly or accidentally, an eclecticism that tosses good science out of the running.

Kuhn states (*2*) that science progresses by forgetting the past or by making a myth out of it. The citation index helps in the myth-making, and therefore, by Kuhn's Law, is invaluable for the advancement of science and technology. Is that really better than keeping the record straight? Should not the citation index reach back through one generation, or more, of reviews to provide the proper credits now masked by creative and selective authors of reviews?

Alternatively, there is a more simple solution—eliminate reviews from consideration because they get cited too much and they contribute nothing to scientific discovery and research except, however sloppily, to consolidate it.

Three other types of papers should be removed from citation index analyses. They get cited too much because the scientific research community zeroes in on them and thereby creates lots of citations: (i) the "suspected fraudulent paper," (ii) the "controversial paper," and (iii) the "pizzaz paper." Often these papers slip from one class to the other during the zeroing in process.

If these three types of papers are eliminated from the analyses, as they should be, what is left? The scientific literature consists of large numbers of data collection papers and data analysis papers. Many solve local problems, like how to control insects that infest date palms in Iraq, or how to identify and counteract toxic ingredients in the Rhine River. Research papers are used for teaching, and many of these are cited in student reports. So papers useful to a user community which uses, but does not cite them in the open literature, also should be removed from consideration by the citation index analysts, because, though not cited, these are needed, like methods papers and reviews, for civilization to advance.

Another class that should be removed from consideration are those which cite their friends, but not their enemies or suspected competitors. Every scientist who has been subjected to that by the aggressive and the overtly ambitious knows whereof I speak. There are also unwitting omissions, which every scientist has made at one time or another, and the omissions by unspoken policy, such as those perpetrated by some scientists in government agencies who only cite the investigations of other scientists working in that agency. The citation index analysts can decide for themselves whether these latter two categories also should be eliminated. This will require that the citation index analysts understand the literature. Their learning to read and comprehend science would benefit the administrative community enormously.

Still another class that should be eliminated are those papers which have multiple authorships. The best thing about limiting citation index analysis to sole authorship papers is that there would be so little work for the analysts to do. Perhaps not only multiple authorships, but also the single authorships, should be eliminated from consideration because everyone knows the data collection was done by an excellent technician who should have been cited, and, anyway, we all know that the sole author got the idea from his ex-colleague, who thought that he/she was a collaborator.

So what does this leave for the analysts to analyze? Well, they could find other criteria to analyze. For example, they could find the research in science which has had a major impact, even though the paper was never published. Research published as an abstract, as a remark after a symposium lecture, or even as a title in a program of a meeting, not to mention ideas and materials given by those disincluded from a publication, has had such uncitable impact.

Thus the citation index analysts could help reverse the most famous dictum of the 20th century, "publish or perish," first coined at the mid-century mark by Kimball C. Atwood III (but not published by him, and therefore, according to the current rules of the game, not citable).

R. C. VON BORSTEL
Department of Genetics, University of Alberta, Edmonton, Canada T6G 2E9

REFERENCES AND NOTES

1. D. Pendlebury, *The Scientist* **4**, 18 (8 January 1990).
2. T. S. Kuhn, *The Structure of Scientific Revolutions* (Univ. of Chicago Press, Chicago, IL, ed. 2, 1970).
3. Examples of cases cited above can be obtained from the writer of this letter.

Clean Air Act Amendments

Philip H. Abelson's editorial "Incorporation of new science into risk assessment" (14 Dec., p 1497) highlights a congressional mandate for the National Academy of Sciences to review and improve risk assessment methodology for hazardous air pollutants. This mandate is contained in the recently enacted Clean Air Act Amendments of 1990. We at the Environmental Protection Agency (EPA) agree with many of the points in the editorial and welcome any opportunity to consider and, where appropriate, incorporate recent scientific information in risk assessment. The introduction to the editorial, however, contains an assessment of the overall Clean Air Act with which we strongly disagree. Abelson seems to belittle the potential health benefits of the act and predicts that the major results of the bill will be increased costs, uncertainty in corporate planning, job losses, weakened competitiveness for U.S. industry, and increased bureaucracy and litigation.

These unfounded assertions do a disservice to those in the Congress, the Administration, and environmental and industry groups who have worked together over the past 2 years to craft this legislation. We believe the scientific consensus supports our position that this bill will bring significant health and environmental benefits by reducing exposures to chemicals such as ozone, carbon monoxide, acid aerosols, and a number of specific toxic air pollutants. Although the costs of the bill are significant, so are the known and potential benefits. The President's Council of Economic Advisors esti-

mates that when fully phased in 2005, costs will be $25 billion, or 24 cents per day per person. Moreover, numerous public opinion pools over the years have repeatedly shown the public's willingness to pay substantial amounts for clean air.

Abelson's statements regarding jobs and international competitiveness are without support. Analyses by the EPA as well as by the Council of Economic Advisors indicate that the legislation will not have a permanent effect on the aggregate level of U.S. employment. Indeed, some analyses show that 15,000 jobs are created for each billion dollars spent on air pollution control. Total impacts on international competitiveness are unlikely to be of significance to trade. Indeed, one of our most competitive trading partners, the Federal Republic of Germany, has already adopted a technology-based control program for about 200 substances that are toxic in air which is similar to the technology standards contained in this bill and is far ahead of the United States in controlling the pollutants that form acid rain.

Much of the editorial is related to issues surrounding the provisions of the bill for toxic substances in air. These provisions lay out a clear schedule for technological controls over the next decade: within a year EPA will apprise industry of the chemicals and source categories it intends to control. Rather than creating uncertainty for industry, this should provide a much clearer road map for corporate pollution control planners than has existed in the past. The bill also builds on the efforts of industry in recent years to engage in voluntary action to control or prevent toxic air pollution. Both the bill and the current industry activities are in direct response to the public concern expressed over toxic substances in air.

Historically, actual costs are generally much lower than projections because of improved technologies. For example, in 1971 the oil industry estimated that lead phase-out would cost 7 cents a gallon, or $7 billion a year. In 1990, with 99% of lead phase out accomplished, actual costs are only $150 million to $500 million a year, 95% percent less than earlier estimates.

The new Clean Air Act passed with strong margins in both houses of Congress because it had the strong support of the President, the Congress, and the public, and various interest groups and because it includes innovations to stimulate market responses at the lowest cost, such as the acid rain trading allowance system. The scientific testimony during the debate indicated substantial health and environmental benefits. Analysis of impacts does not demonstrate major untoward effects on the economy. This effort deserves a more considered and informed summary than it received in Abelson's editorial.

WILLIAM G. ROSENBERG
Assistant Administrator for Air and Radiation, U.S. Environmental Protection Agency, Washington, DC 20460

Evaluating Teaching

We at Williams College have always been proud of the famous story of Mark Hopkins, our former president, on one end of a log and a student on the other end. Indeed, our student pub is called "The Log." So imagine our dismay to find Daniel E. Koshland, Jr., crediting the story to a generic "Thoreau's ideal" (Editorial, 18 Jan., p. 249).

As is often the case, the story commonly told—Mark Hopkins on one end of a log and the student on the other—is not quite accurate. The original statement was made in 1871 by James A. Garfield, just elected president of the Williams College Society of Alumni and later (in 1880) President of the United States. Garfield, a member of the Williams Class of 1856, said, "Give me a log hut, with only a simple bench, Mark Hopkins on one end and I on the other, and you may have all the buildings, apparatus, and libraries without him!" Mark Hopkins was president of Williams from 1836 to 1872. His brother Albert founded our Hopkins Observatory, the oldest astronomical observatory in the United States, in 1836.

JAY M. PASACHOFF
Director, Hopkins Observatory, Williams College, Williamstown, MA 01267

Koshland's editorial of 18 January struck many sympathetic chords with me, but I think that he was too hard on students. The issue is not between "more lenient" and "highly demanding" in a professor's approach to the class. Arguments in this vein are often given to justify one's mediocre classroom performance.

Students have considerable insight and judgment generally and look for "value added," realistic goals, and fairness. They understand that only the best should get the top grades, but they can wonder when a disproportionate number of highly selected students—admission standards are very high today—fail a class or find that the class average on examinations is regularly 30%.

It has been my experience that students will respect a teacher who works them hard if they perceive that the teacher is also working hard to help them grasp the subtle insights and extend the work to new, and sometimes exciting, applications. Students are quick to recognize the teacher who has spent too little time on preparation. They are too often numbed by an ambience that caters only to the best students and tolerates, but just barely, those who do not get A's but will be the majority, and therfore the backbone, of our future society. At the "research universities," B students are still first-class minds.

Thus, I believe that good or poor student evaluations of courses and teachers are less dependent upon the class being tough or easy, but more upon the student's perception of the teacher's commitment to the process of information transfer from one generation to the next, a job researchers should do very well indeed.

WALTER DEBLER
1827 Coronada Drive, Ann Arbor, MI 48103

Although evaluating teaching is a difficult task, I'm not sure that it is as categorically different from the task of evaluating research as Koshland suggests. The "quantitative measures" of research ability that he cites (grant support, invitations to speak, prizes, and so forth) are, after all, only indices of the scientific community's evaluation of the *quality* of research. This evaluation is conducted through the time-consuming processes of peer review, citation analysis, and all the other, less formal mechanisms by which a scientist's research comes to be assessed. If we were to accord teaching the same serious attention accorded to research, no doubt equivalent quantitative measures of teaching ability and productivity would be forthcoming. But this is unlikely to happen until the reward systems of our leading universities do more to recognize the importance of good teaching to continuing the expansion of the scientific frontier. Leon Lederman (quoted in News & Comment, 18 Jan., p. 267) says that "You shouldn't have to bribe people to be teachers." You shouldn't have to bribe them to be researchers either; however, human nature being what it is, most people will put their energy into rewarded rather than unrewarded activities.

TIMOTHY D. JOHNSTON
Director, Center for Critical Inquiry in the Liberal Arts, and *Associate Dean, College of Arts and Sciences, University of North Carolina, Greensboro, NC 27412–5001*

Erratum: The article "Exact solution of large asymmetric traveling salesman problems" by Donald L. Miller and Joseph F. Pekny (15 Feb., p. 754) should have included the following note. "Supported by National Science Foundation grant 9058073-DDM and by the Engineering Design Research Center at Carnegie Mellon University."