Letters

Citation Counting

Alun Anderson's article "No citation analyses please, we're British" (News & Comment, 3 May, p. 639) caught my interest. Having served several times on (and chaired) the Harrison Howe Award Committee of the Rochester section of the American Chemical Society, as well as our own college's John Wiley Jones Award Committee, I have extensive experience in assembling criteria that are used to accord recognition to scientists at or near the Nobel Prize level. We use the number of citations listed for both pertinent people and papers as provided by the Science Citation Index as one of a large number of criteria on which to base our decisions. I must believe that responsible, intelligent people in comparable positions who are assessing research performance by university departments would act in a similar fashion, and I only hope that the out-of-hand rejections described in the aforementioned article are not really the position of the people involved but represent hyperbole that masks the intellectual approach of the responsible people involved.

TERENCE C. MORRILL
Department of Chemistry,
Rochester Institute of Technology,
Rochester, NY 14623–0887

As an expatriate Englishman, I strongly endorse the wholesale rejection of citation indices by my academic countrymen. Anderson's article includes several valid arguments against the use of such indices, but does not mention their most serious flaw: inadequate sampling.

A few years ago, as part of the British government's nationwide exercise in university research "quality" assessment and resource reallocation, I was instructed to calculate my citation index for the previous 5 years. I found the resulting value of 0.22 citations per research paper per year extremely embarrassing. Fortunately, my distress was short-lived, being replaced by cynical amusement when I scanned the list of journals used to compile the Science Citation Index. All the journals in which I had published were excluded, and it was blatantly Americocentric. A study based on such a small and biased sample would be rejected by any reputable journal; it simply would not survive peer review. It seems especially ironic that this letter, published in a journal that *is* used to compile the Science Citation Index, could achieve my highest ever citation rating!

RICHARD M. BATEMAN
Department of Paleobiology,
National Museum of Natural History,
Smithsonian Institution,
Washington, DC 20560

Satellite Oceanography in the 1980s

The news Briefing on the forthcoming launch of the European Space Agency's (ESA's) Remote Sensing Satellite (ERS-1) (3 May, p. 642) does not do justice to the United States' role in satellite oceanography during the 1980s. While it is true that a general-purpose mission like Seasat never materialized, a variety of U.S. satellites provided important new ocean data during this period. We are especially puzzled by the characterization of the U.S. Navy's Geosat mission as being of little significance to civilian researchers because of classification. The truth is very different. The Geosat altimeter operated continuously for 4.5 years (1985 to 1989) and gathered the first multiyear global sea level data set. Although most of the data from the initial 1.5 years were classified, those collected during the final 3 years were not. These latter data have been distributed in their entirety by the National Oceanic and Atmospheric Administration (NOAA) to more than 40 scientific institutions, including all major oceanographic laboratories in Europe. There have been several public workshops at the Johns Hopkins University Applied Physics Laboratory (JHU/APL), numerous sessions on Geosat at meetings of the American Geophysical Union, and two special issues of the *Journal of Geophysical Research* devoted to Geosat. In the United States alone, the refereed literature includes well over 100 scientific papers based on the Geosat data, including several in *Science*. In addition, the U.S. Navy has been extremely cooperative in working with NOAA to release subsets of the initial 1.5 years of classified altimeter data, with the result that virtually all of the important oceanographic information (sea level variability, wind speed, wave height) and ice sheet topography have been freely available to the public for some time.

As ERS-1 principal investigators, we share the enthusiasm for the impending launch of this important ESA satellite mission. But civilian researchers were "not left to analyze and reanalyze the same limited [altimeter] data . . ." in the 1980s. Geosat was perhaps the biggest oceanographic success story of the 1980s. The scientific community owes a debt of gratitude to the U.S. Navy and to JHU/APL who made it happen and kept the United States at the forefront of oceanography from space.

ROBERT E. CHENEY
BRUCE C. DOUGLAS
National Geodetic Survey,
National Oceanic and Atmospheric Administration, Rockville, MD 20852

Regulation of Biotechnology

Greg Simon (Letters, 3 May, p. 629) makes the case that only federal regulation of releases of agricultural products will address health and safety concerns. Such a view is hardly surprising, since he drafted the federal statute for the comprehensive regulation of field research with recombinant DNA-manipulated organisms. The basic assumptions of this statute were contrary to those contained in the reports of the National Academy of Sciences (NAS) (*1*) and the National Research Council (NRC) (*2*) and also to the existing policies of government research and regulatory agencies. The statute would have regulated only those organisms that were manipulated with recombinant DNA techniques (and virtually all of them), but not organisms likely to be of high risk, such as those possessing enhanced fitness or pathogenicity or those that contain novel phenotypes. Such a statute would not have allayed public fears or conferred safety protection above that of current regulation, but it would have perpetuated the notion that process, rather than performance or product, should be the focus of regulation. It would, inevitably, have exerted a chilling effect on those wishing to use the newest techniques. This scientifically indefensible approach was criticized by government agencies, industry, and academia alike and was rejected by Congress.

Simon criticizes the congruence of the principles underlying our proposal (Policy Forum, 26 Oct., p. 490) with those elaborated in reports from the NAS and the NRC. He criticizes us for not relying instead on a position paper by the Ecological Society of America (ESA) (*3*). We believe the NAS-NRC principles are scientifically defensible and internally consistent but that the statements of the ESA, which are dependent on process, are not. The ESA paper agreed with the NAS and NRC that there is no fundamental difference between new and old techniques of genetic manipulation with respect to risk for organisms used in field trials, but it concluded that every proposed field trial of an organism manipulated with recombinant DNA techniques—without any expression or exemption—must be sub-

jected to a governmental risk assessment.

However, agricultural research even with plants or microbes that have been powerfully modified by a variety of traditional genetic techniques, has *not* been routinely subject to governmental "case by case every case" evaluation, except for certain plant pests, noxious weeds, or organisms considered to be veterinary vaccines. And when one considers that an individual plant breeder "may introduce into the field 50,000 genotypes per year on average or 2,000,000 in a career" (*2*, p. 66), and that many of these are transgenic, it is clear that the logic of the ESA's position is flawed (*4*).

Simon cites what he considers to be another contradiction between the NAS-NRC reports and the ESA paper, noting the NAS-NRC conclusion that intergeneric organisms present no unique hazards per se and that most engineered organisms are expected to be less fit than their parental organisms. He continues, "Conversely, [the ESA report] predicts that '[o]rganisms with novel combinations of traits are more likely to play novel ecological roles.' " These statements are not necessarily incompatible. An intergeneric organism may not represent a "novel combination of traits" with respect to ecological, genetic, or even phenotypic factors. Conversely, intrageneric genetic changes can confer changes that exert drastic effects. As we emphasized one must consider carefully the *function* of coding or regulatory elements that have been transferred; less important is the technique used to confer the genetic change or the presumed evolutionary distance between the nucleic acids being recombined.

Simon characterizes our proposal as "too little" and derides it as "self-regulation." Actually, it provides an algorithm that has unlimited flexibility. Depending on what is judged to be an acceptable regulatory burden on researchers and the government, an appropriate level of scrutiny for certain organisms, and other factors, the mechanism can vary widely—from an extremely stringent scheme with a high proportion of required case-by-case governmental risk assessments to a more laissez-faire one in which there is complete exemption or a requirement only for notification for the majority of experiments. Whatever the choice, the cardinal principles of sound regulation would be met.

HENRY I. MILLER
Office of Biotechnology, Food and Drug Administration, Rockville, MD 20857
ROBERT H. BURRIS
Department of Biochemistry, University of Wisconsin, Madison, WI 53706
ANNE K. VIDAVER
Department of Plant Pathology, University of Nebraska, Lincoln, NE 68583

REFERENCES AND NOTES

1. National Academy of Sciences, "Introduction of recombinant DNA-engineered organisms into the environment: Key issues" (National Academy Press, Washington, DC, 1987).
2. National Research Council, "Field testing genetically modified organisms: Framework for decisions" (National Academy Press, Washington, DC, 1989).
3. Ecological Society of America, *Ecology* **70**, 298 (1989).
4. The recent U.S. Kiawah Island International Symposium (November 1990), "Biosafety Results of Field Tests of Genetically Modified Plants and Microorganisms," at which the results of nearly 150 field trials were described, affirmed at least the short-term predictability of the safety of recombinant DNA-manipulated organisms.

The Trabi: Not a Problem

I would like to make a few comments regarding Michael Balter's article "Microbes and 'the Trabi problem' " (News & Comment, 12 Apr., p. 205). I am originally from Poland and have been in the United States for 10 years. While living in Poland I owned a Trabant. I bought it in 1971 for 65,000