formation of nitrosamines in the stomach.

Wasserman and Wolff state that there is no correlation of the amounts of nitrosamines that might be ingested under normal conditions with development of harmful effects, in man or animals. I draw their attention to the philosophy behind the Delaney Amendment—any amount of a known carcinogen is a hazard. To talk of the *potential* hazard of nitrosamines found in food is obfuscation, since these are carcinogens several orders of magnitude more potent than aminotriazole or cyclamates, which have been banned by the government. The benefit of the doubt should be given to the public rather than to the food processors. Surely it is time that nitrites (and nitrates) were removed from the GRAS (generally regarded as safe) list, as were cyclamates, until such time as they are proved safe for human consumption beyond a reasonable doubt.

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Journal Evaluation

Garfield, in his empirical study "Citation analysis as a tool in journal evaluation" (3 Nov. 1972, p. 471) compares *Solid State Physics, Immunology, Journal of Experimental Analysis of Behavior, Chemical Review*, and other journals with small intersections of common interest. This seems strange, but acceptable, as long as the data are used carefully. However, because I wondered why there were no general geology journals listed in the 152 most frequently cited journals ranked by impact factor (figure 8, p. 477), I talked to a science and technology librarian and was struck with the potential for misuse of this article. Garfield's study can, and probably will, be misused by library administrators who want to cut back on periodicals. After all, Garfield does say (p. 474), "It is apparent, even from the makeup of this partial listing, that a good multi-

disciplinary journal collection need contain no more than a few hundred titles. That is not to say that larger collections cannot be justified, but it does say something *indisputable*, in terms of cost and benefit, about how large a journal collection need be (or how small it can be) if it is to provide effective coverage of the literature most used by research scientists" (italics added). Also (p. 477), "Another application, which harried librarians may welcome, is the correlation of data on citation frequency and impact with subscription costs. Such a correlation can provide a solid basis for cost-benefit analysis in the management of subscription budgets." One might add that it can also provide a tenuous basis, since "harried" librarians, by definition, would be unlikely to analyze the analysis.

Some years ago (*1*) I expressed doubts about the effectiveness of citation analysis as a tool for evaluating individual authors, and I have some of these same doubts about Garfield's use of citation analysis as an evaluative tool for journals. The publication of papers that are original, creative, and cite few other papers would be inhibited. Editorial preference would be given to papers with prolific citations of the publishing journal and of journals with which they have friendly affiliations. In some cases, referees would require that references to their own papers be included before a paper is accepted for publication. These kinds of pressures would result in the inclusion in papers of many unjustified citations and would lead to distorted evaluations.

It seems, also, that answers to many of the "Unanswered questions" in the closing section of Garfield's article could have been hypothesized, and the study could have been based upon an experimental design to determine the usefulness of the hypotheses. Instead Garfield uses a purely empirical approach, mixing apples, oranges, and ad hoc speculations.

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The analysis of journal citations described by Garfield provides food for thought, since many scientists have a largely unproductive reading list. The reason for this broad scanning of the literature is not only the diverse nature of the interests of many active workers, but also that many are working in fields that overlap disciplines. The journals listed by Garfield as most frequently cited are major field journals devoted to broad coverage of highly populated disciplines.

It is significant that no veterinary, animal science, or pathology journals are listed. These fields are well covered by a relatively large number of journals, considering the number of people actively engaged in full-time research. They are also fields in which a worker must draw from many disciplines to provide a basis for his work. One worker may publish regularly in basic journals and in those devoted to his discipline. These factors place the journals serving the low-population, interdisciplinary fields at a marked disadvantage in any "numbers game" evaluation.

I would be loathe to see any policy decisions made from such a preliminary, all-encompassing study, particularly by libraries assessing their journal collections. The library is a store of information to be used, not to be kept or maintained for itself. It is a service to the community that supports it. In this regard a journal has served its purpose if it is *consulted* once. How many consultations make a citation? Too many libraries have lost the service concept without their being given a "numbers game" basis for their policies.

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Several earth scientists have asked why geology journals were not represented on the list of 152 high impact journals. The list originally submitted contained 565 titles. The *American Journal of Science* ranked 182nd by impact; the *Journal of Geology* ranked 240th.

Information on the most frequently cited 1000 journals has now been published (*1*) in the *ISI Journal Citation Reports* (*JCR*), issued by the Institute for Scientific Information. The *Journal of Petrology* ranked 50th. However, this should not obscure the fact that most geology journals do not have a broad impact. The most important general "geology" journals include *Science* and *Nature*. They are cited in geology journals more often

than most specialized geology journals. It is interesting that Janke does not mention the *Journal of Geophysical Research*, which ranked 60th; *Geochimica et Cosmochimica Acta* (104th); and the *Journal of Sedimentary Petrology* (146th). Similar comments could be made with respect to other fields, such as veterinary science and pathology, which are more dependent on basic biomedical and multidisciplinary journals than on those in their own narrow specialty.

Certainly *JCR* and other citation data *can* be misused. But so can other techniques. Objective sociometric data or statistics can be manipulated for political or other objectives. Should we abandon the census for this reason?

A number of "harried" librarians and departmental chairmen are using *JCR* data to weed out rarely used journals but also to add journals previously excluded. Most scientists are aware that society is demanding more efficiency in the operation of research establishments, but Janke and Bide do not discuss cost-effectiveness for libraries with finite budgets. Bide would justify the purchase of expensive journals even if they are used only once. Weinstock (*2*) demolished an earlier assertion that a library can aspire to order anything and everything. The ultimate extension of such an assertion is that each specialized library should become a Library of Congress.

Janke refers to his previously expressed doubts about citation analysis but doesn't cite any evidence to support his earlier contentions. Can he provide any examples of "papers, however bad, being heavily cited, while others remain uncited because they are too far ahead of their time" (*3*)?

The assumption that original and creative papers cite few other papers is completely contrary to fact. In the 10 years that the *Science Citation Index* has been published, there has not been any perceptible change in the rate of citation by or to the average paper.

The factual answers to many of the speculative questions I posed in my article are available in my weekly articles in *Current Contents* (*4*). I gladly confess that I am an empiricist.

Countless examples could be cited to prove that the impact numbers game can work to the advantage of small journals that would otherwise be neglected in favor of larger or more familiar and prestigious journals. Furthermore, before the existence of *JCR*, would it have been obvious that the virology journals had become basic to plant pathology collections, or that the *Journal of Experimental Medicine* covers mainly immunology, or that the *Journal of Petrology*, although 636th in citations, ranked 50th by impact? (The last observation is based on data compiled after my article was published.)

Bide and Janke seem to give my colleagues in the library and information sciences little credit for their ability to analyze data.

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Mesoamerican Calendar

My attention has been called to an error in my report (7 Sept., p. 939) on the Mesoamerican calendar. Charles H. Smiley of Brown University has kindly pointed out that it is only the Goodman-Martínez-Thompson correlation whose zero starting point corresponds to the zenithal sun position I have postulated as the origin of the Mesoamerican calendar, and not the Spinden correlation, which is some 52 days out of phase with it. Thus, inadvertently, my findings have provided further reason to accept the validity of the former system and to finally reject the latter. This conclusion is also warranted by radiocarbon dating done at the University of Pennsylvania and called to my attention by Henry N. Michael of Temple University.

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