nonconventional energy technologies, of course, because many needed data are as yet nonexistent, and because important categories of harm are left out of his approach altogether. But by propagating an analysis riddled with distortions, errors, and inconsistencies, Inhaber has muddied rather than illuminated even the circumscribed part of the risk problem he tackled.

JOHN P. HOLDREN
Energy and Resources Group, University of California, Berkeley 94720
KIRK R. SMITH
Resource Systems Institute, East-West Center, Honolulu, Hawaii 96822
GREGORY MORRIS
Energy and Resources Group, University of California, Berkeley

References and Notes

1. H. Inhaber, *Risk of Energy Production* (Report AECB 1119, Atomic Energy Control Board, Ottawa, Ontario, March 1978); *ibid.*, ed. 2, May 1978; *ibid.*, ed. 3, November 1978. The *Science* article does not specify to which of the three editions it refers, and some of its numbers differ from those in all three. Our comments on AECB 1119 here refer to the third edition unless otherwise specified.
2. K. R. Smith, J. Weyant, J. P. Holdren, *Evaluation of Conventional Power Systems* (Report ERG 75-5, Energy and Resources Group, University of California, Berkeley, July 1975). Inhaber's first reference in his AECB report contains 30 citations to this report, 13 direct ones, plus 17 more where Inhaber took the data from our report but mentioned also the original source we had cited.
3. J. P. Holdren, K. Anderson, P. Gleick, I. Mintzer, G. Morris, K. R. Smith, *Risk of Renewable Energy Sources: A Critique of the Inhaber Report* (Report ERG 79-3, Energy and Resources Group, University of California, Berkeley, April 1979).
4. Nuclear Energy Policy Study Group, *Nuclear Power: Issues and Choices* (Ballinger, Cambridge, Mass, 1977). The authors state on p. 179 that "the expected number of cancers could be several times higher, depending on the assumed dose-response model used in deriving the risk estimates," than the values given in the Rasmussen report. On the same page, they note that "the WASH-1400 probability estimate could be low, under extremely pessimistic assumptions, by a factor of as much as 500." The implied upper limit on the product of probability and consequences is a factor of 1500 to 2500 larger than the WASH-1400 "best estimate." Inhaber's "upper limit" is only 6.7 times the WASH-1400 "best estimate."
5. To derive this result we used the upper limit of the National Academy of Sciences' dose-response relation referenced by Inhaber, for the most unfavorable location that the Academy considered (a plant sited 60 kilometers upwind from New York City) and worked backward from Inhaber's figure for public deaths to determine the emissions needed to produce these. See National Academy of Sciences, *Air Quality and Stationary Source Emission Control* (Government Printing Office, Washington, D.C., 1975), chap. 13.
6. R. Manvi, *Performace and Economics of Terrestrial Solar Electric Central Power Plants* (JPL Internal Report 900-781, Jet Propulsion Laboratory, Pasadena, Calif., October 1976). We have consulted the head of the JPL solar project of which this work was a part, and he confirms our analysis of the point and of Inhaber's error (R. Caputo, private communication, March 1979).
7. R. Caputo, *An Initial Comparative Assessment of Orbital and Terrestrial Central Power Systems* (Final Report, Report 900-780, Jet Propulsion Laboratory, Pasadena, Calif., March 1977). Inhaber propagated a number of errors from the 1976 JPL internal memorandum, despite early warnings from Caputo that this material was unreliable (R. Caputo, personal communication); in fact the memorandum appears to have been Inhaber's main source for his methodology and for much of his data relating materials requirements to occupational injuries and diseases.
8. Average insolation on a horizontal surface in the United States is about 180 watts per square meter (averaged over seasons and night and day). Assuming the collectors cover half the land area charged to the plant and that the efficiency of the cells in converting sunlight to electricity is 10 percent, and using the same 30-year lifetime assumed by Inhaber, yields 180 W/m^2 × 0.10 × 0.50 × 30 years = 270 watt-year/m^2, which gives 3700 square meters per megawatt-year.
9. C. L. Comar and L. A. Sagan, *Annu. Rev. Energy* **1**, 581 (1976).

Paper Studies

We tabulate and ponder many aspects of our research and development (R & D) process in this country [see, for example, Senator Bayh's concerns with bringing developments to application (Letters, 12 Jan., p. 120)]. Scholars have devised thoughtful models of the process of technological innovation. For instance, Kelly *et al.* (*1*) call attention to its nonlinearity, and Wenk and Kuehn emphasize the multifaceted governmental roles (*2*). However, to the best of my knowledge, neither the conceptualizers nor the empiricists—see (*3*)—have focused on the dimension of "physical" R & D versus paper studies.

It is difficult to specify what fits into the paper study category. I have in mind such things as forecasts, technological feasibility and market studies, cost-benefit analyses, environmental impact statements and technology assessments, systems and policy analyses, and program evaluation. I speculate that such endeavors represent a substantial fraction of the federal R & D budget, and that they play a crucial role in directing the technological innovation process. But I don't know and wonder if anyone does now know.

I suggest that compilation and dissemination of some basic information on the dimension could usefully address a number of issues. For instance, as a faculty member in a department that trains operations researchers and systems analysts, I would like to know the scale of efforts supported in such areas. From a national perspective, one could ask what sort of people perform various paper studies and whether they are suitably trained? For example, the growing commitment to program evaluation requires many professionals. Are we educating such people in the most sensible manner for this task or just relabeling willing contract researchers? It would also seem worthwhile to inquire broadly into who uses what sorts of analyses (*4*). More pointedly, we might attempt to evaluate the return on investment from such studies and their role in the technological innovation process.

ALAN L. PORTER
Industrial and Systems Engineering, Georgia Institute of Technology, Atlanta 30332

References

1. P. Kelly, M. Kranzberg, F. A. Rossini, N. R. Baker, F. A. Tarpley, Jr., M. Mitzner, *Technological Innovation: A Critical Review of Knowledge* (San Francisco Press, San Francisco, 1978).
2. E. Wenk, Jr., and T. J. Kuehn, in *Perspectives on Science and Technology Policy*, J. Haberer, Ed. (Sage, Beverly Hills, Calif. 1977), p. 10.
3. National Science Board, *Science Indicators-1976* (Government Printing Office, Washington, D.C., 1977).
4. M. R. Berg, J. L. Brudney, T. D. Fuller, D. N. Michael, B. K. Roth, *Factors Affecting Utilization of Technology Assessment Studies* (Center for Research on Utilization of Scientific Knowledge, Univ. of Michigan, Ann Arbor, 1978).

Technical Comments: Delay Time

If a major American automobile manufacturer took longer to admit to an error and recall a model of automobile than it had taken to produce the model in the first place, we would not expect the nation's press to remain silent. Yet this is the position that *Science* consistently finds itself in, and there does not seem to have been any public comment, much less protest. Errors are inevitable in all scientific periodicals; in *Science* the avenue for remedying scientific or editorial errors is the Technical Comments section. We recently made a study of the speed with which these corrections reached print, and the study suggests there is room for improvement.

We examined 20 issues of *Science* published between 25 August 1978 and 12 January 1979. We examined the publication delays for the 26 Technical Comments we found, and for a sample of 40 Reports (two selected at random from each issue). The comparison is striking: The average delay from first submission to publication was more than 100 days longer for Technical Comments than for Reports (*1*). The results are similar if instead we compare the times between the submission of the final revisions and the dates of publications; here the Technical Comments were delayed an average of 71 days more than the Reports (*2*).

It is not difficult to identify the source of this discrepancy; it is the time that passes while *Science* waits for a reply to the Technical Comment by the original authors. For the 15 Technical Comments to which the original author replied, the mean delay between the submission of the final revision of the Technical Comment and the submission of the author's reply was 127 days (minimum delay = 31 days, maximum = 272). Once the author's reply is received, the processing of the Technical Comment seems to be accelerated (mean delay until publication = 77 days, compared with a mean delay from reception to publication of revised Reports of 118 days).

Thus it seems that Technical Comments take more than 100 days longer to process than Reports do because *Science* waits an average of 4 months for the author to reply to the Technical Comment. We suggest that this is too long. We suggest (i) that authors be allotted no more than 1 month to submit a reply, and that failure to meet this deadline result in the deferment of the reply to a later issue; and (ii) that steps be taken to accelerate the editorial handling of the first submissions of Technical Comments (delays now average 3 to 4 months). Not all Technical Comments that are submitted present substantive, correct criticism of published articles, and some delay is inevitable. But the influence of *Science* upon the nation's press is great, and delay in publishing corrections can aggravate the effect of those few mistaken or misleading Articles or Reports that do slip through the editorial sieve. Unless there is some improvement, *Science* risks falling behind science.

STEPHEN M. STIGLER
VIRGINIA L. STIGLER
1243 Los Trancos Road, Portola Valley, California 94025

Notes

1. For the 40 Reports, the mean delay from first submission to publication was 213 days (minimum delay = 71 days, maximum = 434), while for Technical Comments it was 318 days (minimum delay = 114 days, maximum = 631). One of the Reports in the sample was listed as having been originally submitted in 1928. We took this to be a misprint, although it may be an editorial delay fit for the *Guinness Book of Records*.
2. The mean delay from final revision to publication was 129 days for the Reports (minimum delay = 71 days, maximum = 241), while for Technical Comments it was 200 days (minimum delay = 94, maximum = 406).

Erratum: In the Research News article, "Fields Medals (IV): An instinct for the key idea" (17 Nov. 1978, page 737), Jean-Pierre Serre's affiliation was incorrect. He is at the College de France.

Erratum: In the issue of 26 January on page 343, the credit for the photograph of Albert Einstein should have included the name of the photographer, David Rothman.

Erratum: On page 857, second column, third line, of the article about Eugene Garfield (News and Comment, 24 Nov. 1978), "Garfield's gross" should have read "Garfield's dross."

Erratum: Two errors of affiliation were made in the article about Albert Szent-Györgyi in the issue of 9 February (News and Comment, page 522). Harold Swartz is with the Medical College of Wisconsin, not the University of Wisconsin. Gabor Fodor is at the University of West Virginia, not the University of Wisconsin.