Ozone data from the TOMS satellite instrument and ground-based Dobson instruments that were deseasonalized and then smoothed with a 1-year running mean (Fig. 1) clearly show the downward trend over the last decade. No account was taken of any possible 11-year solar cycle, quasibiennial oscillation, or other natural influence.

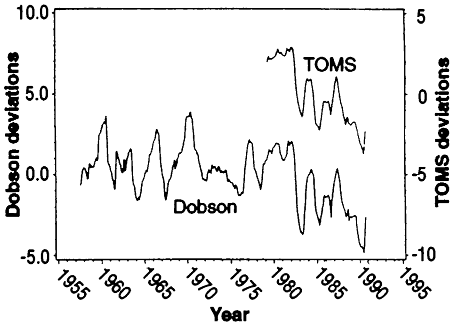

Fig. 1. A comparison of Dobson and TOMS data for northern middle latitudes. Both are presented as percent deviation from the long-term mean (in Dobson units). The Dobson graph shows a composite series constructed from deseasonalized ozone measurements from stations in the latitude range of 40° to 52°N and should be read against the *y*-axis on the left. The TOMS graph shows deseasonalized mean ozone measurements from the latitude range 40° to 50°N and should be read against the *y*-axis on the right. [Adapted from figure 2-10 of (*2*)]

Global ozone trends are significantly smaller than those shown in Fig. 1 because nearly half of the globe in tropical regions displays no trend at all (see figure 4 of our article). The details of the analysis of the global record over the 12 years of TOMS data were published in (*1*). The decrease during that time was a little more than 3%, nearly all of which occurred before 1986. Theory would suggest that this is because the downward trend is superimposed on an 11-year solar cycle variation of about 1 to 2% peak to peak. The magnitude of the solar cycle variation in total ozone is confirmed by analysis of 30-year data records from Dobson stations. This suggests that, if the trends continue, the global total ozone record will show a long-term downward trend that will be large for half the solar cycle and small or near zero for the other half.

Towe is correct that the solar ultraviolet flux at the surface will respond to the actual ozone variability resulting from all causes and not to just the trend component. However, we do not agree with his analogy to salary adjustment for inflation. The trend in the solar ultraviolet should respond to the trend in the ozone. Unfortunately, our database on trends in ground-level ultraviolet radiation is not yet robust enough for us to be able to make any quantitative statements.

Richard Stolarski
Goddard Space Flight Center, National Aeronautics and Space Administration, Greenbelt, MD 20771

REFERENCES

1. J. R. Herman, R. McPeters, R. Stolaski, D. Larko, R. Hudson, *J. Geophys. Res.* **96**, 17297 (1991).
2. R. Stolarski *et al.*, in "Scientific assessment of ozone depletion: 1991" (World Meteorological Organization/United Nations Environment Program Report, World Meteorological Organization, Geneva, Switzerland, 1992), chap. 2.

■

Appraising the Evidence

Sociologist Allan Schnaiberg writes in his review (20 Mar., p. 1586) of *No Safe Place: Toxic Waste, Leukemia, and Community Action*, by sociologist Phil Brown and psychiatrist Edwin J. Mikkelsen

this study notes the rage, the sense of powerlessness, and depression experienced by Woburn [Massachusetts] activists when confronting the self-interested corporate scientists working to protect industrial profits or the government scientists working to vindicate records of government "regulation" (which was often ineffectual if not actually collusive with the local plants). Even more painful, many of these activists, having already suffered illnesses and deaths of their children, were then attacked as "trouble-makers" by neighbors who were workers at the polluting plants for threatening local livelihoods.

The offending chemical was trichloroethylene (TCE). There is grave doubt about whether epidemiological studies based on the small numbers studied are valid. Cancer clusters in small areas commonly occur by chance or for unknown reasons. Bruce N. Ames *et al.* say that the most polluted well water in Woburn had a human exposure dose/rodent potency dose (HERP) risk value of 0.0004%, compared to a HERP value of 0.001% of average U.S. tap water (*1*). According to this evidence, the residents of Woburn would have been more at risk of cancer from chloroform in ordinary tap water than from TCE in Woburn wells.

Other solvents, such as benzene, may cause cancer in workers who receive occupational exposures over long periods of time when the chemical is introduced through the lungs. But, is there evidence that TCE causes leukemia when it is not absorbed through the lungs (*2*)? Might it not, in view of considerations such as these, be fairer to say that residents were enraged because they wrongly believed that they were victimized?

The accusation of deliberate harm for profit is horrifying; it should not be made without substantial evidence. My reading of Schnaiberg's review suggests that he did not read the scientific literature and appraise the evidence. The point is not that I am necessarily right that the evidence is weak but rather that first-hand appraisal of the evidence is a scholarly obligation.

Aaron Wildavsky
Survey Research Center, University of California, Berkeley, CA 94720

REFERENCES

1. B. N. Ames, R. Magaw, L. S. Gold, *Science* **236**, 271 (1987).
2. *Cancer Rates and Risk* (Publ. 85-961, National Institutes of Health, Bethesda, MD, April 1985), pp. 40, 64, 93.

Response: It is peculiar to find one social scientist arguing that a fellow social scientist has a "scholarly obligation" to "appraise the evidence" of epidemiologists.

My review, "Oppositions" as its title indicates, was an attempt to understand community opposition to various environmental risks, which was the common theme of the books under review. As a social scientist, my charge in the review was to understand community social and political processes. Brown and Mikkelsen use *reports* from Woburn residents about their health experiences. They also use *reports* of residents' political experiences, while trying to get professional *epidemiological* assessments of their health risks. Both reports related to the operations of a local factory, which putatively affected the water supply in their community.

In the passage Wildavsky cites, I pointed to the empirical data collected by Brown and Mikkelsen. My task was not to assert whether the residents were actuarially "right" in their assertions of health *outcomes*. Rather, I sought to integrate their reported experiences into the broader empirical and theoretical task of comprehending how communities deal with such perceived risks.

Interestingly, Brown and Mikkelsen spend a good deal of their book carefully dealing with the options communities actually have in obtaining *scientific* information about health risks. (They also summarize the conclusions of Harvard School of Public Health epidemiologists and biostatisticians from *their* professional field studies of Woburn, which substantiated many of the claims of local activists.)

Allan Schnaiberg
Department of Sociology, and
Center for Urban Affairs–Policy Research, Northwestern University, Evanston, IL 60208

Corrections and Clarifications

Figure 2 on page 523 of the Report "Magnetoferritin: In vitro synthesis of a novel magnetic protein" by F. C. Meldrum *et al.* (24 July, p. 522) was incorrect. The correct figure is printed below.

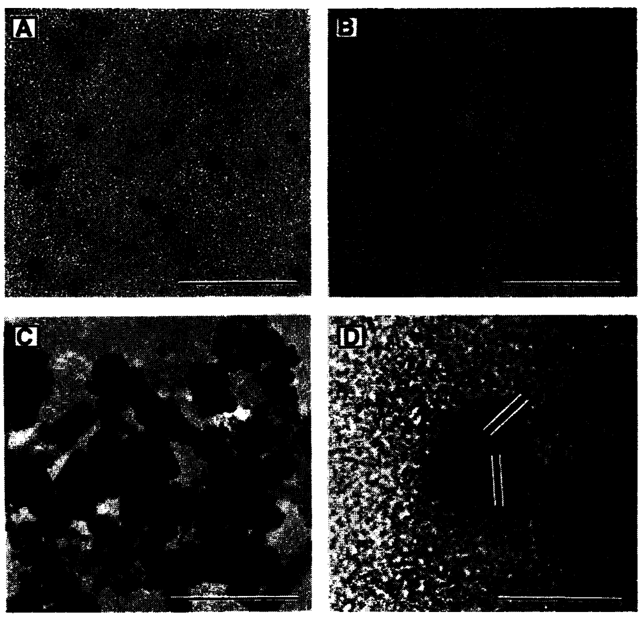

Fig. 2. Transmission electron micrographs of (**A**) an unstained sample of reconstituted ferritin showing discrete magnetite particles (scale bar = 50 nm); (**B**) a stained sample of (A) showing an intact protein shell surrounding the magnetite cores (scale bar = 50 nm); (**C**) magnetite crystals formed in the control reaction (scale bar = 100 nm); the needle-shaped crystals were identified as the mineral goethite (α-FeOOH); and (**D**) a high-resolution lattice image of an individual reconstituted ferritin core showing the single-crystal nature of the particle. Two sets of lattice fringes are observed corresponding to the {111} (interatomic spacing d = 0.485 nm) and {002} (d = 0.4198 nm) planes of magnetite. The angle between these planes is 54°, consistent with a cubic lattice symmetry (scale bar = 5 nm).