Letters to the Editor

Letters (~300 words) discuss material published in *Science* in the previous 6 months or issues of general interest. They can be submitted by e-mail (science_letters@aaas.org), the Web (www.letter2science.org), or regular mail (1200 New York Ave., NW, Washington, DC 20005, USA). Letters are not acknowledged upon receipt, nor are authors generally consulted before publication. Whether published in full or in part, letters are subject to editing for clarity and space.

Gender Bias and Ida Noddack

DISCUSSION OF GENDER AS IT AFFECTS SCIENCE and other contemporary affairs rightly provokes sensitivities and concerns regarding past and present biases. Naomi Oreskes emphasizes and references these well in comments ("Stepping forward too far?," Books *et al.*, 16 May, p. 1094) on an essay of mine on Ida Noddack, a chemist active in the 1920s and 1930s (*1*). In 1934, Noddack suggested that nuclear splitting (later termed "nuclear fission") explained observations reported that year by Enrico Fermi's group that they interpreted as evidence for creation of transuranium elements. Later work by Otto Hahn, Lise Meitner, and others in 1938–39 led to the realization that Noddack had been correct and the Rome group incorrect.

Did gender play a role in Ida Noddack's ideas not being recognized in the 1930s?

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Commentators since have sought an explanation for the lack of attention to Noddack's hypothesis between 1934 and 1939. Some have suggested gender as an explanation. In my essay, I dismissed this as unlikely, a point with which Oreskes disagrees. She quotes me as having written, "I find it difficult to believe that a reasonable individual would have been foolish enough to reject or ignore any hypothesis because it emanated from a woman." Oreskes criticizes correctly the view she quotes; indeed, it would be unreasonable at any time. But I did not write this naïve claim to explain the lack of attention to Noddack's hypothesis, and the original context made clear that I was referring to likely reactions from those working in the fields in question. The quotation omits my interspersed qualifying phrase "in nuclear chemistry or physics at the time."

The justifications for my qualification in 1934–39 include the following: the Nobel Prize Committee awarded Irene Joliot-Curie the Nobel prize in 1935; those in the nuclear physics community gave high respect to Lise Meitner; those in the professional community well remembered the work of Marie Curie; neither Meitner nor Joliot-Curie themselves took Noddack's hypothesis seriously; and perhaps most importantly, Walter Nernst had nominated Noddack herself for the Nobel prize in chemistry in 1932 and subsequent years.

One could object that the women cited, with the exception of Meitner, had either inherited professional stature or acquired it conjugally so to speak, leading to more respectful reception of their work and hypotheses than other women, lacking these advantages, would have had. But Noddack herself had a conjugal advantage, evidenced by the fact that Nernst had nominated her husband as well as herself for the Nobel prize.

Noddack, as well as many other women, encountered great difficulties in finding a satisfactory professional position. But the best explanation of the inattention to Noddack's hypothesis in 1934–39 remains what Gunther Stent has termed "prematurity": the inability of the scientific community to connect the implications of a claim or hypothesis to contemporary canonical or generally accepted knowledge by a simple series of logical steps (*1*, *2*). (Noddack, an analytic chemist, was unaware of problems perceived by nuclear chemists and physicists in the 1930s regarding nuclear fission.) It was not until 1939 that the work of Meitner, among others, made such a connection of Noddack's hypothesis possible.

> **"The best explanation of the inattention to Noddack's hypothesis in 1934–39 remains what Gunther Stent has termed 'prematurity'..."**
>
> –HOOK

> **"Many factors affect the reception of new scientific claims. Gender happens to be one of them."**
>
> –ORESKES

ERNEST B. HOOK

School of Public Health, University of California, Warren Hall, MCH-M.C. 7360, Berkeley CA 94720–7360, USA.
E-mail: ebhook@socrates.berkeley.edu

References and Notes

1. E. B. Hook, in *Prematurity in Scientific Discovery: On Resistance and Neglect*, E. B. Hook, Ed. (Univ. of California Press, Berkeley, 2002), pp. 124–148.
2. G. S. Stent, *Sci. Am.* **227**, 84 (December 1972). Stent defined prematurity as a quality of past discoveries, leading to claims by several critics that this was a form of misleading retroactive historical judgment termed Whiggism. Stent's discussion makes clear, however, that he intended prematurity to apply to claims, hypotheses, proposals, etc., at the time originally proposed, whatever their subsequent fate, i.e., irrespective of whether or not they anticipated or led to a "discovery" later.

Response

HOOK IS RIGHT THAT EDITORIAL PRESSURES required me to distill his quotation about Ida Noddack and that the Curies—both Marie and Irene—received ample recognition for their scientific work. But the fact remains that gender has consistently affected women in science, including physicists in the 1930s. The most obvious example is the very one that Hook cites: Lise Meitner, who should have shared the Nobel prize with Otto Hahn for her contributions to the discovery of nuclear fission. After all, it was Meitner, not Hahn, who recognized the meaning of the experiments and who, together with her nephew Otto Frisch, developed the first theoretical account of nuclear fission by means of Bohr's liquid drop mode of the atom (*1*). Was gender or antisemitism more important in this case? Clearly, both played a

role. If Meitner had been a Christian man, she would not have had to flee Nazi Germany, and history would have taken a different course. And what of Herthe Sponer, wife of James Franck and eminent physicist in her own right? Franck easily found work in the United States; Sponer did not. One might suppose that Franck was the better physicist, but historian Margaret Rossiter has documented how the great U.S. physicist Robert Millikan actively intervened to prevent Sponer being hired in America, on the exclusive grounds of gender (*2*). What about Maria Goeppert-Mayer, who won a Nobel Prize for work on the structure of the nucleus? Her husband, a chemist of lesser rank, had little trouble finding remunerative work, but she spent most of her career in unpaid positions until she was finally hired in the 1960s at the new University of California at San Diego.

My point was not to beat the drums of gender discrimination, but rather to point out that Hook's analysis—and the prematurity theme that drives it—is simplistic and unduly reductionist. Many factors affect the reception of new scientific claims. Gender happens to be one of them. This is not an either/or choice. My point was—and is—that his own prematurity drumbeat in the book has drowned out consideration of other important themes.

NAOMI ORESKES

Department of History, University of California, San Diego, La Jolla, CA 92093–0104, USA. E-mail: noreskes@ucsd.edu

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Reliability of Satellite Data Sets

IN THEIR REPORT "INFLUENCE OF SATELLITE data uncertainties on the detection of externally forced climate change" (23 May, p. 1280), B. D. Santer *et al*. compare climate model output of atmospheric-layer temperature changes since 1978 with satellite-based data sets from Remote Sensing Systems (RSS) and the University of Alabama in Huntsville (UAH). They note that tropospheric model temperatures matched the RSS data better than the UAH data, mainly because RSS's trend was more positive (+0.10, +0.01, and +0.07°C/decade for RSS, UAH, and models, respectively). The authors were careful to say that models cannot be used "to determine which of the two satellite data sets is closer to reality." However, the implication expressed in the Science press release for the Report—"A stubborn argument against global warming may be discredited by a reanalysis of the [UAH] data central to its claims…"—suggests otherwise.

The same week Santer *et al.* was made available electronically, we published a paper in which we performed tests on trend precision using independent observations, not model output (*1*). We compared our UAH data products with balloon-based data from government research groups in the United States, the United Kingdom, and Russia. After examining data quality and availability for both satellite- and balloon-based products, we estimated the 95% error range of the UAH trend as ±0.05°C/decade for the layer discussed by Santer *et al.* Several published studies support the trend estimate of UAH (*2*–*6*). We want *Science* readers to be aware that appropriate, observationally based tests have been published.

Furthermore, although the model trend was only 0.06°C/decade more positive than that of UAH, readers should understand that

coupled models do not include actual events such as the massive warm El Niño of 1997–98, which impacted observed trends profoundly. If El Niño effects were removed from both model and UAH data to reveal the underlying climate trends (i.e., apples to apples), the trend discrepancy would almost certainly become greater (*7*). Under these conditions, model performance would display even less consistency versus UAH and balloon-based observations.

JOHN R. CHRISTY[1,2]*† AND ROY W. SPENCER[2]
[1]Earth System Science Center, [2]Department of Atmospheric Science, University of Alabama in Huntsville, Huntsville, AL 35899, USA.
*Alabama State Climatologist
†To whom correspondence should be addressed. E-mail: christy@nsstc.uah.edu

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Response

THE COMMENTS BY CHRISTY AND SPENCER (CS) raise two scientific issues. The first relates to the use of observationally based tests for assessing the reliability of different satellite data sets. CS contend that radiosonde data constitute such a test. Their comments imply that radiosonde data can be used to discriminate between the divergent satellite-based estimates of tropospheric temperature changes produced by the University of Alabama in Huntsville (UAH) and Remote Sensing Systems (RSS). We disagree, for the following reasons.

Since their initial release in the early 1990s, the UAH tropospheric temperature products have evolved markedly over time (*1–3*). Changes were made to adjust for inhomogeneities in the radiative emissions measured by the Microwave Sounding Units (MSUs) flown on 12 different polar-orbiting satellites. During this evolutionary process, CS frequently compared their MSU-based temperature estimates with various radiosonde products (*1–3*). MSU and radiosonde information may not be entirely independent, however, thus hampering assessments of MSU reliability (*4–7*).

As in the case of MSU measurements, there are many possible adjustment pathways for radiosonde data. Different procedures are applied to account for changes in instrumentation and site location (*8, 9*) and to "infill" data-poor regions (*6*). These choices influence the large-scale tropospheric temperature trends estimated from radiosondes (*8–10*). Correspondence between trends in MSU and radiosondes depends, therefore, on the choice of radiosonde data set. It also depends on whether MSU data are subsampled with radiosonde coverage (because MSU has global coverage, whereas radiosondes do not), on how equivalent MSU temperatures are calculated from radiosonde temperature profiles, and on the choice of statistical metric used for data comparisons (*10–12*). For these and other reasons, radiosondes are not an unambiguous "gold standard" for the evaluation of satellite data.

The second issue raised by CS relates to climate model portrayal of the tropospheric temperature variability associated with El Niño and La Niña events. CS contend that "coupled models do not include actual events such as the massive warm El Niño of 1997–98, which impacted observed trends profoundly." They argue that such differences in simulated and observed El Niño behavior invalidate our comparison

of tropospheric (T2) temperature trends in UAH and RSS with trends from the Parallel Climate Model (PCM).

This argument is incorrect. Coupled models like PCM successfully simulate many of the statistical characteristics of El Niño–induced temperature variability, although the precise timing of El Niño and La Niña events cannot be the same in the real world and in the model (except by chance) (*13*). Although observations provide only a single realization of the MSU era, with a single sequence of El Niños, La Niñas, and T2 changes, coupled models can generate many different realizations of this period, all with different manifestations of El Niño variability. It is entirely appropriate to ask whether the single realization of the observed T2 trend lies within the envelope of possible model solutions. We find that global-mean T2 trends in UAH and RSS data are statistically consistent with T2 trends in each of the four PCM realizations we consider. Statistical removal of El Niño effects from modeled and observed T2 data has only a small effect on trends and does not alter this finding (*14, 15*), contrary to the unsupported claim of CS.

B. D. SANTER,[1] T. M. L. WIGLEY,[2] G. A. MEEHL,[2] M. F. WEHNER,[3] C. MEARS,[4] M. SCHABEL,[4] F. J. WENTZ,[4] C. AMMANN,[2] J. ARBLASTER,[2] T. BETTGE,[2] W. M. WASHINGTON,[2] K. E. TAYLOR,[1] J. S. BOYLE,[1] W. BRÜGGEMANN,[5] C. DOUTRIAUX[1]

[1]Program for Climate Model Diagnosis and Intercomparison, Lawrence Livermore National Laboratory, Livermore, CA 94550, USA. [2]National Center for Atmospheric Research, Boulder, CO 80303, USA. [3]Lawrence Berkeley National Laboratory, Berkeley, CA 94720, USA. [4]Remote Sensing Systems, Santa Rosa, CA 95401, USA. [5]University of Birmingham, Edgbaston, Birmingham B15 2TT, UK.

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4. The most important reason for the discrepancies between the UAH and RSS tropospheric temperature trends relates to differences in how the two groups merge data from overlapping satellites. Merging requires simultaneous determination of the offset between a pair of satellites and the dependence of the MSU measurements on the temperature of the MSU radiometer itself. The latter varies from instrument to instrument and is likely due to uncompensated radiometer nonlinearity. The RSS approach to merging uses a multiple regression model relying on overlaps between all pairs of simultaneously operating MSU instruments (*5*). Radiosonde data are not involved in merging. In contrast, the UAH merging strategy employs a particular sequence of satellite pairs to determine offsets and radiometer nonlinearity. Choice of the exact set of satellite overlaps is guided in part by comparisons with radiosonde data (*2*). The UAH merging path results in a nonlinearity term of nearly 10% for the critical NOAA-9 instrument, a value substantially larger than the estimated nonlinearities in other MSU instruments and in other microwave radiometers. All RSS values for this nonlinearity term lie within the 0 to 3% range.
5. C. A. Mears, M. C. Schabel, F. J. Wentz, *J. Clim.*, in press.

6. D. E. Parker *et al.*, *Geophys. Res. Lett.* **24**, 1499 (1997).
7. CS also cite several comparisons between MSU data and reanalysis products. Reanalyses are generated with observationally constrained numerical weather forecast models. These models assimilate satellite-based temperature retrievals that incorporate "uncorrected" MSU data: i.e., data not subjected to the adjustment procedures used by UAH (*3*). Comparisons with reanalyses, therefore, do not constitute totally independent verifications of MSU.
8. D. J. Gaffen, M. A. Sargent, R. E. Habermann, J. R. Lanzante, *J. Clim.* **13**, 1777 (2000).
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11. B. D. Santer *et al.*, *J. Geophys. Res.* **105**, 7337 (2000).
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13. Due to chaotic variability in the climate system, and the impossibility of initializing any model experiment with the exact three-dimensional state of the real world atmosphere and ocean, even a hypothetical "perfect" coupled climate model, driven by "perfect" estimates of all external climate forcings, would not precisely replicate the observed sequence of El Niños and La Niñas.
14. We have performed such removal exercises using the approach outlined in (*15*). Over 1979 to 1999 (the period used for comparing the PCM and observed T2 trends), removal of El Niño/Southern Oscillation (ENSO) effects decreases the observed UAH and RSS T2 trends by no more than 0.02°C/decade. This relatively small effect is due to the presence of two large El Niño events of roughly equal amplitude (in 1982–83 and 1997–98) near the beginning and end of the observational record. In PCM, removal of ENSO effects either slightly increases or slightly decreases the "raw" T2 trends in the four ALL realizations. Accounting for ENSO effects has little influence on the between-realization range of T2 trends.
15. B. D. Santer *et al.*, *J. Geophys. Res.* **106**, 28033 (2001).

TECHNICAL COMMENT ABSTRACTS

COMMENT ON "High Abrasion Resistance with Sparse Mineralization: Copper Biomineral in Worm Jaws"

Robert M. S. Schofield, Michael H. Nesson

The primary copper compound in the jaws studied by Lichtenegger *et al.* (Reports, 11 October 2002, p. 389) may not be the biomineral they identified and may instead be similar to zinc compounds in other worms. The claims about mechanical properties are based on small differences found along a single dried specimen using an artifact-susceptible technique that does not measure abrasion resistance.

Full text at
www.sciencemag.org/cgi/content/full/301/5636/1049a

RESPONSE TO COMMENT ON "High Abrasion Resistance with Sparse Mineralization: Copper Biomineral in Worm Jaws"

Helga C. Lichtenegger, Thomas Schöberl, Michael H. Bartl, Herbert Waite, Galen D. Stucky

New elemental analysis results on transmission electron microscopy sections are presented. Our results provide no hints for the existence of further unidentified Cu and Cl substances as proposed by Schofield and Nesson. The use of the Oliver and Pharr method for the nanoindentation of biological specimen is discussed.

Full text at
www.sciencemag.org/cgi/content/full/301/5636/1049b