Letters

"Superbug" Evidence

We are writing with concern about the tone of the article "Superbug attacks California crops" by Elizabeth Culotta (News & Comment, 6 Dec., p. 1445), which dealt with the new strain of the sweet potato whitefly, *Bemisia tabaci* (Gennadius). While we are in sympathy with the growers in California's Imperial Valley because they certainly suffered significant crop losses during the fall of 1991, we are not convinced that all the statements being made about the whitefly's biology are well grounded in fact, nor are we certain that these losses are attributable entirely to the presence of a new whitefly strain.

Our principal concern is that the distinction between hypothesis and fact be made clear. There have been reports of "increased" polyphagy, but no experimental tests have yet been published on the degree of polyphagy of either strain. It is an exaggeration to cite "over 500 plants . . ." as a host range for the poinsettia strain of the whitefly when this represents the total published feeding records for the entire species worldwide over the last 100 years. Furthermore, the cropping system in the southwest desert valleys of the United States is dominated by approximately a dozen cultivated plant species, so the matter of absolute degree of polyphagy appears to have little to do with the current problem. In particular, references to a narrow degree of polyphagy in the "cotton" strain in comparison to a wider degree of polyphagy in a "poinsettia" strain are unsubstantiated by published experimental evidence.

Such hyperbole places at risk the measured assessment and reporting of the species' biology by allowing readers to believe that we already know more than we do. The promotion of a "Paul Bunyanesque" mythical insect of the Old West that is sucking the Southland dry and carries away entire melons to feed its young does not advance our understanding of the situation. We know that changes are taking place, but we need to carefully examine what those changes are.

DAVID N. BYRNE
Department of Entomology, University of Arizona, Tucson, AZ 85721
THOMAS S. BELLOWS
Department of Entomology, University of California, Riverside, CA 92521

Response: My article clearly stated that the crucial next step is to verify the identity of the whitefly problem strain. The article's tone reflects the atmosphere of crisis surrounding the whitefly. The pest triggered a state-of-emergency declaration in two California counties and has cost $118 million to date in those counties, according to the California Department of Food and Agriculture. To say merely that there are "changes" in the whitefly situation does not reflect what most scientists in California agriculture consider a very serious problem.

—ELIZABETH CULOTTA

Free-Electron Laser

The effort to demonstrate the applicability of free-electron lasers (FELs) to lithography described in the item "From star wars to chip wars" (ScienceScope, 22 Nov., p. 1099) is a collaboration between Los Alamos National Laboratory, Motorola, and Texas Instruments; Sandia National Laboratory is not involved. However, Los Alamos and Sandia *are* working together to explore the utility of a broadly based Department of Energy laboratory–industry collaboration to develop some of the manufacturing tools and technologies that the "Microtech 2000" study recommends. The goal of "Microtech 2000" is to develop the set of manufacturing technologies needed by industry by the year 2000 and is not limited to light source technology for chip production (which isn't perceived to be the greatest challenge). To have the FEL described as being "politically well connected" surprises us, but we take it as a compliment from those who are. Matters of commercial viability are yet to be determined for all of the newer approaches to microlithography.

SIDNEY SINGER
Program Director, Directed Energy Science Technology, Los Alamos National Laboratory, Los Alamos, NM 87545

Cold Fusion: China Lake Results

Gary Taubes, in his article about Martin Fleischmann's cold fusion seminar at the California Institute of Technology (News & Comment, 13 Dec., p. 1582), states that "researchers working with the China Lake group have said that those observations . . . could be explained by helium-4 contamination from the ambient atmosphere." We are basically a two-man group with respect to cold fusion research at China Lake and neither of us has made such a statement. To my knowledge, neither has anyone else at China Lake made any such statement.

Regarding our report of time-correlated measurements of excess heat and helium (*1*), the simple yes-or-no detection of helium-4 in eight of eight experiments producing excess heat and the absence of helium-4 in six of six control experiments not producing excess heat (one in D_2O, five in H_2O) implies a chance probability of only $(1/2)^{14}$ = 1/16,384 or 0.0061%. Those attributing our results to atmospheric contamination should try to flip a coin until they obtain a predetermined sequence involving 14 tosses. Furthermore, the experiments at China Lake producing the greater amounts of excess enthalpy yielded the larger amounts of helium-4. Our control experiments show that atmospheric contamination is a highly unlikely explanation for our results.

MELVIN H. MILES
Weapons Division, Naval Air Warfare Center, China Lake, CA 93555

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Museums: Storehouses of DNA?

Traditional museum collections of vertebrates and other organisms increasingly are being viewed by molecular biologists as valuable storehouses of DNA (*1*). It is gratifying that new uses have been found for old specimens; however, the privilege of sampling specimens is accompanied by the responsibility of supporting the maintenance and growth of museums.

Removing fragments of tissue, skin, bone, or feathers from museum specimens for DNA analysis is destructive. The recent focus on the phylogeny and population characteristics of rare and extinct species exacerbates the problem because specimens of these species, once destroyed, can never be replaced. For example, if present trends continue, museum holdings of some species (for example, Hawaiian honeycreepers) will be gradually picked to pieces by researchers. Museum specimens should only be sampled when fresh material is truly unavailable or when museum specimens can add a legitimate longitudinal perspective. Fresh tissue should be collected and used in molecular research whenever possible because it yields high-quality DNA that is much less prone to laboratory

artifact or contamination (*2*). Curators and researchers should coordinate efforts to ensure that repetitive sampling of specimens does not occur and that tissue samples and extracted DNA are preserved and shared by interested parties.

GARY R. GRAVES
MICHAEL J. BRAUN
National Museum of Natural History, Smithsonian Institution, Washington, DC 20560

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2. J. Cher, *Science* **253**, 1354 (1991).

Science at EPA

The 10 January ScienceScope item "Better science at EPA?" (p. 147) quoted me out of context. The invidious comparison I was drawing between the Environmental Protection Agency (EPA) on the one hand and the National Institutes of Health (NIH) and the National Science Foundation (NSF) on the other did not have to do with the quality of the science being performed by in-house EPA scientists, which in many cases is world class. Rather, I was referring to the processes by which these organizations seek out and make funding decisions concerning extramural science. Almost by definition, those successful scientists in the academic community, whom EPA needs to enlist in their search for the best possible science to ensure credible decision-making, have had experience with the NIH and NSF processes. These processes are generally considered to be valid, in the sense that they are usually capable of finding the best scientist to do the best science. In comparison, EPA's dealings with the academic community often seem to be characterized by ineptitude, cronyism, or politics. The only fully peer reviewed program, the External Grants Program, has never been consistently funded to any reasonable level by EPA and accounts for far less than 10% of total extramural funding. This criticism is not restricted to the Office of Research and Development, but is particularly pertinent to the wide range of scientific and technical activities performed by EPA program and policy offices. The disrespect engendered by the failure to recognize and enlist the best science affects the credibility of EPA.

BERNARD D. GOLDSTEIN
Environmental and Occupational Health Sciences Institute, 681 Frelinghuysen Road, Piscataway, NJ 08855–1179