Letters

Patriot's Success Rate

Eliot Marshall's News & Comment article "Patriot's effectiveness challenged" (8 May, p. 791), in my view, stands in stark contrast to Daniel E. Koshland, Jr.'s editorial "Credibility in science and the press" (1 Nov., p. 629). Koshland states that "a policy of routinely revealing sources and records would improve the credibility of the press . . .," yet Marshall does little of that. He quotes "one senior Pentagon scientist," "two senior Pentagon experts," "a Pentagon spokesman," and a "Pentagon missile expert." All are anonymous, yet readers are expected to accept their comments at face value.

The fact is that Patriot's rate of success in Israel was about 44%. In this figure, "success" is defined as an intercept that either exploded Scud warheads in the air or destroyed their ability to explode when they hit the ground (what is commonly referred to as creating a "dud"). Therefore 56% of the intercepts were unsuccessful. The video tapes cited in Marshall's article are of those unsuccessful intercepts. They are not being studied to tarnish Patriot's reputation, but to help ascertain how to further enhance Patriot's effectiveness against future threats.

The success rate in Saudi Arabia was much higher (on the order of 90%). Attempting to discredit Patriot's outstanding performance in Saudi Arabia flies in the face of evaluations done by the U.S. Army that have since been independently confirmed by the Ballistic Research Laboratory.

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Response: I would have preferred to name every source I quoted, although only two critics agreed to go on the record: Reuven Pedatzur of the Jaffee Institute and Theodore Postol of MIT. But their criticism has not prompted Raytheon or the government to release photographic evidence supporting the claim that the Patriot achieved a 90% warhead kill rate in Saudi Arabia and a 44% success rate in Israel. Regarding such evidence, Skelly explained to me in a phone conversation that videos of the Patriot-Scud encounters are considered "classified." This secrecy still applies, although during the war, the Department of Defense aired many videos of weapons striking Iraq.—ELIOT MARSHALL

NSF-Funded Research Centers

The short ScienceScope item about the Cornell Nanofabrication Laboratory (18 Oct., p. 365) contains several implications that should not go unchallenged. George Hazelrigg, National Science Foundation's (NSF's) division chief for electrical and communications systems, is reported as saying that no university should come to think that it has an entitlement to an NSF-funded research center. Of the many university groups I am familiar with who have NSF block grants, none has made the assumption that it has an "entitlement" to the grant. These programs are periodically reviewed by independent review panels, and the university groups consider the review procedure to be a serious matter.

Even more disturbing is the implication, attributed by Hazelrigg to the National Science Board, that there should be a "finite life to these things." The imposition of a finite lifetime for an NSF program, rather than a judgment based on research quality and suitability of the university structure for the carrying out of the research program, is completely inappropriate. There appears to be an increasing tendency to assume that because a program has been successful for a period of time it is outmoded. One should not place greater weight on satisfying political pressures or concepts of geographic distribution than on peer review and other measures of quality. Our nation can ill afford to follow such a misguided sense of "fairness" in the award of research monies.

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Air Pollution and Mortality

Frederick W. Lipfert and Samuel C. Morris (Letters, 9 Aug., p. 606), in their critique of the article by Alan D. Krupnick and Paul R. Portney (26 Apr., p. 522), take issue with our cross-sectional mortality analysis (*1*), upon which Krupnick and Portney rely. Lipfert and Morris criticize both the data and the model specifications we used, implying that our results are flawed because we omitted factors they list. We find this a curious criticism, as subsequent work, some by Lipfert himself, has shown that these factors do not significantly change our reported results. For example, with regard to sulfate artifact, a cross-sectional analysis by

Lipfert *et al.* (*2*) of 1980 sulfate mortality data has already considered the available Inhalable Particle Network dichotomous sulfate dataset and a computer model simulated sulfate dataset. Both are free of sulfate artifact, but still give sulfate coefficients that have elasticities not significantly different from those we reported.

Similarly, Lipfert and Morris doubt the validity of our results "as smoking, diet, water hardness, and migration were not accounted for. . . ." However, these factors are only of concern if they confound the analysis by covarying strongly with sulfate measurements (*3*). Again, the cross-sectional analysis by Lipfert *et al.* (*2*) has already shown that smoking and water hardness are not significantly correlated with sulfates. More important, after these factors (and migration) were incorporated into their cross-sectional mortality regressions (which we believe are often overspecified), the 1980 sulfate coefficient was still significant and not statistically different from a sulfate coefficient produced by a model without such factors. Moreover, a further analysis of the 1980 dataset, which considered more than 300 Standard Metropolitan Statistical Areas (SMSAs) and more complete model specifications (for example, smoking, migration, racial mix, health care, industrial mix, and climate), also confirms the robustness of the sulfate coefficient that we reported in our original research (*4*).

Lipfert *et al.* have also considered the other pollutants they mention, concluding that automotive related pollutants (ozone, lead, and carbon monoxide) did not yield significant positive coefficients and could be eliminated from consideration (*2*). In the end, after addressing the data and model specification factors they now raise in their letter, Lipfert *et al.* reported a sulfate mortality elasticity ranging from 2.8 to 13%, which coincides well with our originally estimated range of 4 to 9%. Thus, the 1980 sulfate mortality coefficient we reported in our work has actually proven quite insensitive to the factors raised by Lipfert and Morris.

Lipfert and Morris also express concerns about potential biases in "ecological" studies. We tested our results for potential biases by conducting several sensitivity analyses, including region-specific mortality data regressions on spatially averaged mortality and pollution data (*1*). None of these analyses suggested the presence of significant confounding of variables. Lipfert and Morris raise the possibility that the weak relationship found between total suspended particulate matter (TSP) and human mortality can be accounted for by the fact that central monitor measurements are less representative of regional TSP than they are of regional sulfates. That is why we also considered multiple site SMSA averages of pollution measurements during our sensitivity analyses. We found no appreciable changes (*1*). The fine particle component is the portion that can most readily enter the thorax and is therefore thought to be the most pernicious part of TSP. On this basis, the U.S. Environmental Protection Agency (EPA) changed from a TSP to an inhalable particle standard in 1987 (*5*). Moreover, of the fine particles, the acidic aerosol (of which sulfates are a major component) is thought to be of such special health concern that the EPA Clean Air Scientific Advisory Committee has recommended that the agency consider whether acidic aerosols should be added to the agency's list of criteria pollutants.

In summary, considering all the subsequent analyses and biological plausibility, it is more likely than ever that our results are a product of a valid relationship between sulfate air pollution and human health effects.

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3. J. S. Evans, T. Tosteson, P. L. Kinney, *Environ. Int.* 10, 55 (1984).
4. J. S. Evans, H. Özkaynak, B. Burbank, in preparation.
5. Particulate matter with an aerodynamic diameter less than or equal to a nominal 10 micrometers (PM_{10}); U.S. Environmental Protection Agency, *Fed. Reg.* 52, 24639 (1 July 1987).
6. "An acid aerosol issue paper" (EPA/600/8-88/005F, Office of Health and Environmental Assessment, U.S. Environmental Protection Agency, Washington, DC, 1989).

Erratum: The last sentence of the abstract of the report "Induction of type I diabetes by Kilham's rat virus in diabetes-resistant BB/Wor rats" by D. L. Guberski *et al.* (15 Nov., p. 1010) should have read, "This model of diabetes may provide insight regarding the interaction of viruses and autoimmune disease."

Erratum: Table 3 in the report "Latitudinal and longitudinal oscillations of cloud features on Neptune" by Lawrence A. Sromovsky (1 Nov., p. 684) contained errors. In column 4, the two values for inferred shear should have been −0.8 and −13.8 m/(s-deg), respectively. The corresponding text in column 1 on page 686 should have read, "the advection model is quantitatively upheld for the DS2 motions, but not for the short-period component of the GDS motions. They both agree on phase lags, although the GDS is not in good quantitative agreement on amplitudes. Relative to a smooth shear profile obtained from a simple polynomial fit to the observed wind speeds as a function of latitude (*11*), the shear derived from the advective model amplitude ratio is almost the same for DS2, but a ninth as large for the GDS."