funded research projects because several of these so-called effort reports are missing." At the San Francisco campus, $390,000 is being questioned because 174 PAR's were not returned at the time of the audit. The number 174 represents less than 1 percent of the total of 24,000 PAR's on file. An additional $565,000 is being questioned at San Francisco, not because of "outright refusal" to submit PAR's, but because of a disagreement between the university and HHS over whether certain groups of employees should be included among those required to complete PAR's.

At the time Norman talked with members of my staff, it appeared there might be a problem at the Berkeley campus; however, that has proved not to be the case. The audit findings at Berkeley show less than a dozen effort reports being questioned. There is, however, again a dispute between the university and HHS over who should have received effort reports initially. In connection with this, approximately $300,000 is at issue.

In addition, the HHS audit has found a very small percentage of effort reports missing because of a need for internal mechanisms that will automatically include certain individuals who will need to file PAR's. An example of this situation is a laboratory technician who transfers during the fiscal year from work on a project funded by the state to one funded by the federal government. No mechanisms were in place for PAR's to be provided automatically to the technician when transferred onto federal funding.

In summary, the HHS audit has revealed no substantial noncompliance by individuals; nor has it found any evidence of improper charging of effort to federally funded projects.

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Sources of Northeast Pollution

George T. Wolff (Letters, 11 June, p. 1172) comments on my use of elemental tracers to deduce source areas of pollution aerosol in the Northeast (Research News, 12 Feb., p. 881). In his letter, Wolff summarizes my conclusions incorrectly. I did not deal explicitly with sources of acid rain, as he alleges, because our study was exclusively for aerosol. The only references to precipitation in my statement of 23 November 1981 were in the introduction and at the very end, as part of a call for further research. Although the regionality of acid rain was the motivation for this study, it has to date been limited to the regionality of primary aerosol. Also, according to Wolff, I concluded that "Midwest sources are responsible for sulfate pollution . . . at High Point, New Jersey. . . ." My original phrase was that "air masses from the Midwest carried higher sulfate than did those from the East Coast," which is quite different in emphasis. Obviously, air masses from the East Coast as well as those from the Midwest bring sulfate to High Point.

Wolff then questions two "major assumptions" which he attributes to me. The first of these was that "the Mn/V [manganese-vanadium] ratio in particulate matter in Rhode Island and Massachusetts could reflect the emissions ratio from upwind sources, rather than local emission sources." Because our Arctic studies showed that Mn and V can indeed be valuable tracers over long distances, we kept an open mind about their utility in the Northeast and let the results speak for themselves. A self-consistent picture emerged which we feel is eminently reasonable: local sources of New York City aerosol as implied by reproducible Mn/V ratios and high elemental concentrations; more varied and more distant sources for semirural Narragansett, Rhode Island, implied by a wider spread of ratios (values corresponding to Boston, New York, the Midwest, and the Southeast were found, in addition to those of southern New England); and truly regional sources for the more remote High Point inferred from order-of-magnitude differences in Mn/V ratio between coastal and interior air masses. The Narragansett results, which are the most extensive, showed a degree of regionality and complexity which contrast greatly with Wolff's a priori assumption that "at any site where there are nearby sources, such as in Massachusetts and Rhode Island, the concentration of primary pollutants will be determined largely by local sources. . . ." The complexity of the Northeast is one of our major results that has been largely overlooked.

The second assumption attributed to me is that "the source areas of Mn and V are the same as the sources of acid precipitation." Again, having no results on acid precipitation per se, I made no such assumption. The link between tracer elements in aerosol and precipitation has not yet been determined for the Northeast.

Wolff concludes that the Mn/V technique appears to be invalid because "the principal source areas of the sulfate and the tracer species are different. . . ." In support of this, he notes that concentrations of Mn and V were fourfold higher in New York than at High Point, whereas sulfate was only 10 to 15 percent higher in New York. On an urban scale, he is certainly correct about sources, but his argument is less convincing on a regional scale. Of course New York's sulfate cannot be found right in New York, for sulfate is mostly secondary (particularly in summer) and must be sought downwind of sulfur dioxide sources. But when entire regions such as the Northeast and Midwest are considered, the source areas of sulfate and primary submicron aerosol such as Mn and V may be rather similar.

Wolff correctly points out the conflict between current models, which predict that 80 to 90 percent of the sulfate in the Northeast is transported from outside, and our findings for northeastern sources for primary aerosol. I suspect that this will be resolved in the course of events, as the models are refined and as we come to understand the regional relations between primary and secondary aerosol. At present, there is a great need for hard data in the field of transport, which is long in theory and short in verification. Elemental tracers can offer direct evidence on sources and transport, which ultimately can be used together with other approaches to resolve complex environmental problems such as acid rain.

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Erratum: In the article, "Amplification and adaptation in regulatory and sensory systems" by D. E. Koshland, Jr., A. Goldbeter, and J. B. Stock (16 July, p. 220), the following errors should be corrected. On page 221, the ordinate on figure 1 was incorrect. The correct figure is given below. On page 223, in the first column, 23 lines from the bottom, the number 9 should be replaced by the number 7.