

# Letters

## Academy Marijuana Report

John Walsh's fine article (News and Comment, 16 July, p. 228) on the reaction of the president of the National Academy of Sciences and the director of the National Institute of Drug Abuse to the report on marijuana policy of the Committee on Substance Abuse and Habitual Behavior does not sufficiently emphasize the importance of the report itself.

The significance can be quite simply stated. This committee, with overlapping membership on the Institute of Medicine committee that produced the recent comprehensive report on the health dangers of marijuana and with full access to this report, nonetheless concurred in the earlier recommendation of the National Commission on Marijuana and Drug Abuse and several other groups that marijuana use and possession of small amounts of the drug should be decriminalized. Until this report, it was widely argued that more recent medical findings had somehow made these earlier reports obsolete. In addition, the Academy commission went a step further than the previous groups and recommended that serious thought be given to the replacement of our present prohibition of marijuana supply with some kind of a regulated system of legal marijuana sale.

The committee was a distinguished one, with several members from the Institute of Medicine and from the Academy itself. It included pharmacologists, psychiatrists, psychologists, a geneticist, and an epidemiologist. There were only three social scientists: Howard Becker, a sociologist; Thomas Schelling, an economist; and myself, a lawyer. Finally, the committee unanimously signed off on the report, despite the fact that Academy president Press and various government officials found it so objectionable.

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John Walsh's article on the National Academy of Science's report on marijuana policy is well balanced and informative. It errs only in implying that the final version did not satisfy all members of the

committee. While it was not easy to achieve consensus on the report, and a number of drafts were required before the ultimate document was considered to be suitable, in fact everyone on the committee signed off on the version that has been published.

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## Multiple Submission

In response to the letter "Duplicate publication" from Mark W. J. Ferguson (18 June, p. 1274), we would like to point out an issue he does not mention. He apologizes for inadvertently publishing the same data twice, after submitting essentially the same manuscript to two journals with the intention of withdrawing one after the other was accepted.

As scientists who serve as reviewers for journals, we would like to point out that submission of a manuscript begins a review process involving hours of work on the part of reviewers, editors, and office staff and significant overhead costs. Multiple submission of papers is an abuse of this system, and a paper should be submitted to a second publication only if it has already been rejected by the first. In addition, careful consideration of the comments associated with the rejection may considerably strengthen the second submission, or make it unnecessary.

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## Missing Computers

The sad *Science* story (News and Comment, 25 June, p. 1392) about the chess-champion computer languishing at Kennedy Airport while the decisive tournament was being played in Moscow

presents a very much one-sided view of the role of U.S. Customs: If they are doing a very good job of bottling up computers within the United States, we should grant them equal efficiency at keeping them out.

In 1977, I crossed over from France to attend the University of South Carolina Conference on Fourier Spectroscopy. A week before, I had shipped by airfreight to Columbia my pet laboratory-built computer, hard-wired for Fourier spectra, fondly believing it would prove a key attraction at the scientific instrument exhibit, plus a fat envelope of what I hoped were suitable documents. It simply vanished; frantic phone calls to everybody concerned produced no results until the evening of the last day, when the crate was finally located—guess where—in a customs shed at Kennedy Airport. Just like Kenneth Thomson, I managed to get it back in no more than a few weeks; I was luckier than he was and I did not even have to pay a fine.

What made the whole affair all the more galling was that, barely a year before, the same device had been exhibited almost under the Kremlin walls at a Franco-Soviet optical meeting, with no trouble at all either way.

To be quite fair, let us grant that, whenever one tries to wander around with a suitcase full of printed stuff, the U.S. border is still, by far, the more permeable of the two.

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## Audit at the University of California

The News and Comment article "Audit may cost UC millions" by Colin Norman (16 Apr., p. 279) states that some University of California (UC) faculty members have "outright refused" to submit Personnel Activity Reports (PAR's) on how they spend their professional time. This may lead the reader to believe that failure to complete PAR's is a serious problem at UC when, in fact, quite the opposite is true. As of November 1981, when the audit by the Department of Health and Human Services (HHS) began, 98 percent of the 220,000 PAR's issued for the year ended 30 June 1981 had been completed and collected.

The article also states: "At UC San Francisco alone, federal auditors have questioned about \$1 million worth of expenses associated with government-

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.

