LETTERS

Methylchloroform in the Stratosphere

In the report by McConnell and Schiff (13 Jan., p. 174), the authors imply that the staff of the Environmental Protection Agency (EPA) has been unaware of or unconcerned about the potential problem of stratospheric ozone depletion by methylchloroform. The first published work suggesting an appreciably longer half-life than the about 1-year lifetime cited by the National Academy of Sciences (*1*) apppeared in 1977 (*2*). On 14 March 1977, Walter C. Barber, director of EPA's Office of Air Quality Planning and Standards, sent a memorandum to Ken Johnson, EPA's acting assistant administrator for toxic substances, on the "Significance of methylchloroform and other halocarbon solvents to stratospheric ozone depletion." Barber stated (in part), "The purpose of this memo is to advise you that our proposed strategy to control photochemical oxidants may increase the emissions of low reactivity halocarbons which would affect the stratospheric ozone layer. . . . We are advised by Dr. Altshuller in his February 14 memo to Joe Padgett that recent investigations indicate that methylchloroform, and possibly other halocarbons, may contribute to the depletion of stratospheric ozone. If this is true, our present oxidant strategy may be working at cross-purposes to the Agency's efforts to safeguard the ozone layer. . . . I would appreciate your keeping my office informed of any decisions reached by your office regarding the significance of these compounds to stratospheric ozone depletion." EPA's Office of Toxic Substances is cognizant of the issue and is reviewing the scientific evidence accumulated to date.

The investigations mentioned were those reported by Singh (*2–3*) of SRI International, Crutzen and Fishman of the National Center for Atmospheric Research (*4*), and Crinn, Rasmussen, and Robinson (*5*) at Washington State University. All of these investigations were supported in part or fully by our laboratory in EPA's Office of Research and Development. We supported this research based on the perception we had that there was insufficient concern about the effects on ozone depletion caused by more persistent chlorinated and brominated hydrocarbons. We took this position as early as 1975 in a report issued in December of that year (*6*) and submitted to the subcommittee on public health and environment of the House Committee on Interstate and Foreign Commerce by

our laboratory. In that report (*6*, p. 11) we identified methylchloroform as a key compound for which measurements were required in the troposphere and stratosphere.

It should be clear that the EPA staff has been fully aware of the need to investigate methylchloroform's role in the depletion of stratospheric ozone and that we have been following the development of scientific evidence to support the need for action very closely.

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References

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4. P. Crutzen and J. Fishman, *Geophys. Res. Lett.* **4**, 321 (1977).
5. E. P. Grimsrud and R. A. Rasmussen, *Atmos. Environ.* **9**, 1014 (1975).
6. Environmental Sciences Research Laboratory, *Report on the Problem of Halogenated Air Pollutants and Stratospheric Ozone* (Report No. 600/9-75-008, Environmental Protection Agency, Research Triangle Park, N.C., 1975).

McConnell and Schiff state that about 15 percent of the methylchloroform released at ground level shall enter the stratosphere, making it potentially significant in the depletion of stratospheric ozone. The authors also state that the Environmental Protection Agency has classified methylchloroform a "safe" chemical because of its low reactivity (inability to produce significant photochemical pollution), thereby possibly creating a stratospheric problem. It is implied that EPA decisions involving emission control strategies are made with consideration for the lower atmosphere and largely disregard the upper atmosphere.

I believe that several conclusions of McConnell and Schiff need to be restated in better perspective.

1) Methylchloroform is important from the point of view of stratospheric ozone depletion only when a large tropospheric residence time of 8 years is used in the model. Such a model allows the calculation that about 15 percent of the methylchloroform released into the atmosphere enters the stratosphere, thus making it significant to stratospheric ozone depletion.

2) The first study (*1*) suggesting that the tropospheric residence time of methylchloroform is 7 years was published about a year ago. A second study refined the earlier analysis and suggested a tropospheric residence time of 8 to 11

years, with 8 years as the best estimate (*2*). This estimate was supported by Crutzen (*3*) with the application of his photochemical model.

3) All information available before 1977 suggested a tropospheric lifetime for methylchloroform of from 1 to 2 years (*4*, *5*). The National Academy of Sciences report published in 1976 calculated the residence time as being between 1.0 and 1.4 years (*4*).

4) Should the residence time of methylchloroform in the troposphere be 1 to 2 years, as was believed before 1977, the conclusions of the study presented by McConnell and Schiff would be invalid and the impact of methylchloroform on the stratosphere would not be significant.

5) Because the EPA control strategies referred to by the authors were formulated before 1977, it was quite justifiable to call methylchloroform a "safe" solvent. A tropospheric residence time of 1 to 2 years would mean that methylchloroform was unreactive enough to not cause photochemical pollution problems, but reactive enough to have no serious impact on the stratosphere. In addition, the stratospheric ozone destruction by chlorine-containing chemicals was probably not known at the time the EPA strategies were formulated. Thus, a case for disregard of the upper atmosphere cannot be made against EPA based on the history of EPA regulations dealing with the control of methylchloroform.

6) A longer residence time for methylchloroform in the troposphere has implications far beyond the impact of methylchloroform alone. Reduced hydroxyl radical (HO) concentration ($\approx 4 \times 10^5$ molecules per milliliter) is implied that would result in longer residence times of a vast number of atmospheric pollutants. The significance of low HO levels to the lower and upper atmospheric pollution has already been discussed in the literature (*1*–*3*). The authors appear to have "tuned" their model to achieve these low HO concentrations in order to "force fit" an 8-year methylchloroform residence time. These low HO values are in disagreement with the values in the authors' own earlier publications, as well as most other models in existence (*4*, *5*). To the best of my knowledge, Crutzen (*3*) is the only modeler who has effectively reconsidered his model and supported the hypothesis of reduced HO levels. It is commonly agreed however, that most models can be "tuned" easily to calculate low HO levels, so the results cannot be considered as confirmatory evidence (*6*). Direct measurements of HO have yet to conclusively support the hypothesis of reduced HO levels.

While I personally support a methylchloroform lifetime of 8 years, the authors have presented conclusions based on information that has only been proposed in the last year. Since the matter of longer methylchloroform lifetime in the troposphere (or low HO values) has not yet been conclusively resolved, the wide range of uncertainties should be considered. The lack of consideration of these uncertainties is a significant omission in the report by McConnell and Schiff. The inference that EPA decisions disregard the upper atmosphere may or may not be true, but the example of methylchloroform is a poor one with which to make such a case. The need for nontoxic chemicals (solvents) with lifetimes of 0.5 to 1 year ("safe") still remains a desirable goal that is consistent with the upper and lower atmospheric requirements.

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5. F. M. Luther, "Lawrence Livermore Laboratory. First annual report to the High Altitude Pollution Program" (UCRL-50042-76, prepared under contract DOT-TSC-76-1, for the Department of Transportation, Washington, D.C., 1976).
6. V. Chang, private communication.

We are sorry that the authors of the above letters have interpreted our report as an attack on the Environmental Protection Agency. This was certainly not our intention. We undertook the work primarily to study the possible effects of reasonable scenarios for future methylchloroform releases on the ozone layer. The results made it worthwhile to draw attention to the possibility that regulations directed to improvement in environmental quality at one location may simply transport the problem elsewhere.

This is precisely what did happen in the case of methylchloroform, albeit inadvertently. Legislation *was* enacted to restrict the use of trichloroethylene, and the result was a large-scale substitution of methylchloroform, for which no restrictions were indicated.

A preprint of our report was made available to EPA in November 1976. We were indeed impressed with the interest and quick response of EPA once they were alerted to the problem by our report and by others quoted in Altshuller's letter. It may be noted, however, that to

our knowledge the regulatory situation remains as described in our report.

In our calculations we did not "tune" our model to obtain hydroxyl radical (HO) concentrations to "force fit" the 8-year residence time for methychloroform. The HO concentration was generated by the model, as we described in detail. Singh is quite correct in stating that the residence time, and consequently, the conclusions reached, depends directly on the HO concentration. We have made this point very explicitly in the report. However, we also point out that there are constraints in any model calculations to any "tuning" for HO; in our case higher concentrations of HO would be incompatible with methylchloroform measurements. We understand that there are discrepancies among scientists' measurements of methylchloroform, particularly with respect to hemispheric ratios, and would support further measurements to improve the assessment of the problem.

In conclusion, we would not want to argue that EPA was unjustified in their 1970 regulatory action based on the scientific knowledge available at that time. But it does seem worthwhile to point out the need to be aware of the kinds of pitfalls that may ensue from regulations and to urge a continuing reassessment of their consequences in the light of new information.

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An Endothermic "Nessie?"

In Jean Marx's discussion of the dinosaur endothermy-ectothermy debate (Research News, 31 Mar., p. 1424), it is pointed out that dinosaur fossils have been recovered in Cretaceous Arctic Circle regions, where, perhaps, only endothermic animals could have survived.

Curiously, one of the arguments against the supposed *Nessiteras rhombopteryx* of Loch Ness (see *Science*, 9 Jan. 1976, p. 54) being a "prehistoric reptile" has been that such ectothermic animals could not survive in the almost constant 42°F temperature of the 1000-foot-deep loch.

If the endothermy school is correct, it makes the existence of *Nessiteras* more reasonable. Or, put the other way