Title
Reply [to “Comment on ‘Tropospheric OH in a three-dimensional chemical tracer model: An assessment based on observations of CH3CCl3’ by C. M. Spivakovsky et al.”]

Journal
Journal of Geophysical Research, 96(D9)

ISSN
0148-0227

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Publication Date
1991-09-20

DOI
10.1029/91jd01670

JOURNAL OF GEOPHYSICAL RESEARCH, VOL. 96, NO. D9, PAGES 17,389–17,390, SEPTEMBER 20, 1991

Reply

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Hartley and Prinn [this issue] misrepresent the objectives, procedures, and results of our work [*Spivakovsky et al.*, 1990, hereinafter referred to as S90]. In addition, their analysis is plagued by a number of conceptual errors and misuse of statistics.

The objective of S90 was to compute the global three-dimensional field of tropospheric OH, based on our understanding of atmospheric chemistry and the climatologies of precursors, and then to assess the results using available observations. This work also included an extensive analysis of observations of CFCs and CH_3CCl_3 in order to identify and compare annual cycles of these species. Comparisons of seasonal variations of chemically passive tracers (CFCs) with those for CH_3CCl_3, which is destroyed by reaction with OH, were intended to distinguish annual cycles associated with transport from those due to chemistry. We concluded that seasonal variations of CH_3CCl_3 are dominated by chemistry only at southern mid-latitudes and that only for this region can they provide constraints for OH.

Hartley and Prinn seem to believe that the primary purpose of a modeling study is to allow an adjustment of "model sources and sinks either by trial and error or by an inverse method until the model agrees with data." This was never the intent of our studies. Data for most trace species are inadequate to allow unambiguous determination of either sources or sinks, except on a hemispheric or global scale. Indeed, the purpose of S90 was to explore what could be learned about OH from analysis of the ALE/GAGE data. We concluded that competitive effects of transport and chemistry were such that concentrations of OH could be constrained only in a global-average sense with somewhat greater detail for southern mid-latitudes. In particular, the latitudinal distribution of OH cannot be constrained on the basis of data from the five ALE/GAGE stations.

Hartley and Prinn address their attention to a peripheral component of our paper, the month-to-month variability of CH_3CCl_3 in the tropics. The resolution of the model employed in our study, combined with use of a single year of dynamical output from the general circulation model (GCM), precluded adequate simulation of the movement of the intertropical convergence zone (ITCZ) or simulation of the interannual variability of tropical meteorology (related for example to El Nino Southern Oscillation (ENSO). The assumptions used in our simulations are clearly stated in S90. *Hartley and Prinn* create and demolish a straw man adding little to our understanding of tropical meteorology.

Regrettably, *Hartley and Prinn* restrict their discussion to the use of data for CH_3CCl_3, unfortunate in light of authors' access to years of yet-to-be-published concurrent data for CCl_2F_2, $CFCl_3$, and CH_3CCl_3. As emphasized in our paper, observations of longer-lived species (e.g., CCl_2F_2 and $CFCl_3$) provide invaluable additional constraints on the relative influences of chemical and dynamical processes in controlling the distribution of CH_3CCl_3. Besides, the ancillary information tells a different tale.

The observations at Samoa available to S90 provide evidence for an annual cycle in CFCs which is satisfactorily reproduced by our model. Important differences were noted in S90 between the seasonal behavior of CFCs and CH_3CCl_3 observed at Samoa. Observations of CH_3CCl_3, unlike those for CFCs, did not display a significant annual cycle. We suggested that the differences could reflect cancellation (for CH_3CCl_3) between the chemical and dynamical components of the variations which appeared to be of opposite phase and comparable amplitude (see Figure 24 in S90). *Hartley and Prinn* could have made a useful contribution in this context. Do the 11-year records for CFCs and CH_3CCl_3 support our suggestion?

We were pleased to see that the 11-year record for CH_3CCl_3 displays an annual cycle at Barbados similar to the one we showed for the published data. Apparently, our coarse-resolution global model reproduces the magnitude and, approximately, the phase of the fall minimum. Our simulations suggested that this seasonal decrease is determined by a dynamical rather than a chemical signal. However, observations of CFCs at Barbados available to S90 do not display a significant annual cycle. Do the 11 years of observations of CFCs define a cycle at Barbados similar to that for CH_3CCl_3?

It is incorrect to expect a single model year (or, for that matter, a single year of observations) to fall within the standard error of the mean of observations as implied by *Hartley and Prinn*. The results shown in S90 do not represent averaged seasonal variations but rather typical seasonal variations, since the simulations were based on a single year of GCM statistics. *Hartley and Prinn* state that "the interannual variabilities ... for Samoa are in fact multimodal." How then should one interpret the standard deviations in Figures 4a and 5a? What fraction of the observations is expected to fall within one standard deviation of the mean? Is there reason to expect that the intra-annual variations simulated for Samoa should agree with observations averaged over 11 years given that observed seasonal variations appear to be nonrecurrent, i.e., indistinguishable from random (the appropriate autocorrelation coefficient is equal to -0.1 as shown in Figure 3b of the comment)? *Hartley and Prinn* in their calculation of correlation coefficients for the different ENSO phases appear to have treated the separated 12-month periods as continuous. This is likely to produce spurious correlations or anticorrelations; consequently, results for "ENSO warm events," "ENSO cold events," and "intermediate" years are suspect (Figure 3*b* in the comment).

Paper number 91JD01670.
0148-0227/91/91JD-01670$02.00

The comment by *Hartley and Prinn* adds little to our understanding of either the model or the atmosphere.

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(Received June 20, 1991;
accepted June 20, 1991.)