

Author reply (*in italics*) to comments of reviewer 2

The author has satisfactorily addressed some of my comments but is apparently unwilling to do additional analyses that could settle the apparent discrepancy between HALOE and FPH trends. As such, while the overall analysis is sound, I find this paper lacking in terms of its scientific significance. At the very least, there are a few specific issues that I'd like to see addressed before seeing this paper published.

In response to my query about why SR events are larger than SS events, the author stated in his response that "The solar lockdown position and subsequent altitude registrations for the SR versus SS measurements differ slightly and affect their retrieved water vapor profiles.", yet this is not mentioned in the manuscript. Rather, the author states in the revised manuscript (Lines 96-100) that the difference is due to reduced sampling for SR events during winter/spring. But isn't winter/spring when you expect filaments of high WV air, and if so, wouldn't that mean that SR events had smaller values of WV in comparison to SS events?

I did not comment in the revised manuscript about the differing lockdown positions for SR versus SS events because their small effects are present across much of the HALOE time series (Remsberg, JGR, <https://doi.org/10.1029/2009JD012904>, 2010). However, there is a clear absence of both HALOE SR AND SS measurements during late winter through springtime after 2000, which affects the amplitude of the MLR annual cycle term as well as the trend term. I have also conducted separate MLR analyses of the SS and then the SR data points of Fig. 1a for 1993 to 2005. The resulting SS trend (-0.30 ppmv/dec) is significantly more negative than the SR trend (-0.17 ppmv/dec). Accordingly, I now report on those differences in the revised manuscript (the last paragraphs of Section 2).

The 1993-2005 trend at 30hPa at boulder is given as -0.22 ± 0.04 ppmv/dec, and the 1993-2002 trend is given as 0.22 ± 0.04 ppmv/dec. I wonder if this was an inadvertent typo? I recognize that the 1993-2002 trend is positive whereas the trend through 2005 is negative, due to the 2001 drop in WV. But it seemed an odd coincidence that the numbers are the same (other than the sign), so I wondered if this was a typo.

The respective negative and positive trends are correct; there is no typo.

The author has added new discussion surrounding the WV at 50 hPa and mentions the possibility that there could be some HALOE WV trend bias related to aerosol extinction removal. Could the author present any analysis showing that this could plausibly explain the differences between HALOE and FPH? If so, that would be a very scientifically impactful statement and would greatly improve the scientific significance of this work. But as presented, saying that the differences are due to the HALOE aerosol

correction seems vary hand-wavy and just seems to reinforce a prevailing view among many in the community that there are irreconcilable differences between the HALOE and FPH record.

The removal of aerosol extinction is important for the retrieval of HALOE water vapor at 50 hPa during the first few years following the eruption of Pinatubo. I now provide some definitive information on the effects of aerosols in Section 4 of the revised text, based on the HALOE retrieved aerosol extinction profiles and a bounding of their modeled uncertainties in Hervig et al. (1995). HALOE SWV at 30 hPa is also affected by aerosols in 1991 and 1992, but their effects do not explain any trend differences between HALOE and FPH thereafter.

I also note that I regenerated Figs. 3a and b, which now show MLR curves that reflect correlations between the MLR terms properly. Even so, the new MLR curves are very similar to those of the previous version.