Responses (in italics) to Reviewer 1

The technical note written by Ellis Remsberg makes a valuable and insightful contribution to the current research on the variability and trends of stratospheric water vapor, particularly in relation to the comparability of satellite-based (HALOE) and in-situ-based frost point hygrometer (FPH) observations above Boulder. The FPH method is assumed to be the most accurate. The paper presents several new and interesting findings, including:

(i) Identification of problems with the HALOE sampling at 40N from 2001 to 2005, particularly during late winter and spring.

(ii) Demonstration of good agreement between the trends derived from HALOE at 30 hPa and FPH data, by considering a limited time span from 1993 to 2002.

(iii) Giving few hints that both FPH observations and HALOE closest data points can be strongly influenced by moist polar air isentropically transported as remnants of the NH polar vortex during late winter and spring.

Overall, the technical note is an important addition to the current research on stratospheric water vapor variability and trends, and provides new insights on the comparability of satellite-based and in-situ-based observations. Here, few detailed comments:

Main points:

(*) To improve readability, it may be helpful to present Figures 1-3 on the left panel and Figures 4-6 on the right panel of a single figure, allowing for easier comparison between the HALOE and FPH data at each step of the data handling process.

I will try to make the display of the figures in the manner that you suggest. I will also change the linear trend to a dashed line in the time series plots.

(*) The correction procedure discussed by Scherer et al. (2008) for FPH data is not applied in this study. As the trends for HALOE and NOAA appear different in their Figure 6b compared to the current study, it should be noted that the current study utilizes slightly modified HALOE data by excluding data from 2001 and 2005 and FPH data without the correction discussed by Scherer et al. (2008). Further discussion on the potential impact of this correction on the results would be valuable.

I agree that my results differ from those in Fig. 6b, but I do not know the actual FPH values that Scherer et al. regressed upon. FPH data for my study were obtained from the GML/NOAA site in 2022, and I presume that they represent the latest and best version of those data. I focused on comparisons at 30 hPa primarily because Lossow et al. (2018, their Fig. 1) reported that the water vapor trends between satellite and FPH data showed differences that increased with altitude. I will comment on that in the revised manuscript. In the interim I have also re-analyzed the results for 1993 to 2002 in my submitted manuscript to account for the exact time span between the data points for those years. As a result, the trends differ slightly from before, and I will make that change in the revised manuscript. HALOE data for that period now have a trend of 4.7 ± 0.8 %/decade and the FPH data have a trend of 6.9 ±1.2 %/decade. Those revised trends do not quite overlap within errors. It is perhaps important to remember that the retrieved HALOE profiles are averages around 30 hPa over a finite altitude interval (~2.5 km), as opposed to the FPH data that are specific to 30 hPa. In other words, I have not convolved the FPH profiles over the altitude resolution of the HALOE retrieval for a more comparable comparison.

(*) While the comparison at 30 hPa is important, it would be informative to also compare the trends at 70-100 hPa, which have been widely discussed for trend analysis in the past (Solomon 2010, Hegglin 2014, Konopka 2022). These studies have shown strong positive trends from FPH observations for the period 1993-2001, whereas other data used in these studies (UARS HALOE shifted to MLS, merged satellite, SWOOSH) have exhibited weaker or even negative trends. Examining these pressure levels may provide additional insight into the ongoing discussion of the trends.

I am not so confident of HALOE water vapor trends at 70 to 100 hPa. That is where water vapor typically has its minimum value, and it is not resolved well vertically. HALOE water vapor may also be biased in that region due to residual effects from thin clouds, as indicated for northern midlatitudes by Bhatt et al. (https://doi.org/10.1029/1999JD900058 , 1999) for HALOE ozone versus ozonesonde comparisons. I will include that citation in the revised manuscript. However, at your suggestion I have analyzed water vapor trends at 50 hPa for 1993 to mid-2001 and compared them with those from FPH. I find that they are significantly different (HALOE trend is 3.7 %/decade and FPH is 10.8 %/decade), which is generally consistent with that reported for the potential temperature range of 380 to 420 K in Figure 2c of Scherer et al. (2008). It may well be that there is a positive bias in the HALOE retrieved water vapor values at 50 hPa (but not at 30 hPa) for several years after 1992 due to residual effects from Pinatubo aerosols. I will add a paragraph to the manuscript describing my findings at 50 hPa.

(*) My most concern is related to the comparison between your Figs 3 and 6, i.e. between HALOE and FPH based trends for the period 1993-2002 at 30 hPa. Yes, even if the trends are very similar, the variability derived from the MLR (thin solid lines) is very different (i.e. thin line in Fig.3 compared with the thin line in Fig. 6). Why?. I guess the correlation between these two thin solid lines is very weak. So why we should trust the trends derived from these two so different lines?

The separate time series of the residuals (Figs. 2 and 5) illustrate the differing natures of the local FPH data set compared with that of the larger-scale HALOE data represented by the average of 4 or more profiles from a region near Boulder. The time spacings between the points in both data sets are rather coarse. No periodic structure is evident in the residual, at least having cycles longer than 6 months (semi-annual). The FPH residual is twice as large as that from the HALOE data, perhaps reflective of some real short-term variations in the local FPH water vapor that are not fit with the terms of the MLR *model. However, the larger variance of the FPH time series also means that its linear trend term is more uncertain than that from HALOE, due to the greater sensitivity of FPH to any end point anomalies.*

(*) In the second part of your paper, you discuss isentropic polar intrusions into the mid latitudes as potential reasons for additional variability both in FPH data and collocated HALOE observations. Following your analysis, this effect should be expected mainly in the late spring and winter. This is certainly an interesting point not well discussed in the past. However, as discussed in Konopka et al., 2022 (see their Fig 4d and 4e), the winter/spring effect on the trends after 2000 was mainly diagnosed in the tropics and not in the Boulder region. Thus, "intrusion effects" on HALOE/FPH observations around 40N and the largest trends after 2000 during winter/spring seasons are probably two different issues.

I do not dispute the likely source of the intrusion of tropical air to the Boulder region in Konopka et al., although their finding is for altitudes near 50 hPa. My example from LIMS is meant to show that there are instances at 31.6 hPa of a transport to the Boulder region of polar air having higher water vapor values.

Minor points:

L65:

"auto-regressive effects" - too technical here, please explain it

Auto-regressive (or AR) effects mean that there is some memory between adjacent data points that must be accounted for in the MLR modeling; I will add a citation. The AR coefficient for the HALOE time series for 1993 to 2002 at 30 hPa is 0.22, while that for FPH is only 0.10.

L70:

please explain the abbreviations "SS", "SR"

SS is sunset and SR is sunrise; I will add that information.

L112:

"have a significant (CI)" ?

CI or confidence interval was defined first on line 90.

L200:

"....from the LIMS experiment" please explain the abbreviation

LIMS was defined first on line 53.

Reply (*in italics*) to Reviewer 2

Review of Remsberg et al:

Given that there is only one long-term balloonborne stratospheric water vapor record from Boulder and few satellite measurements from the 1980's and 1990's, understanding and interpreting the trends present in these records is extremely important. One of the more puzzling findings that has emerged from the literature is whether or not the positive water vapor trend observed in the Boulder water vapor record is real. Several studies have highlighted that satellite measurements over the late 1990's/2000's (i.e., HALOE and SAGE II) don't seem to show the positive trend present in Boulder data. It has been suggested that the trend difference is due to some unknown drift of the Boulder record, or perhaps that the measurements from a single location are not representative of the true zonal mean. Within this context, the present work seeks a better understanding of these differences. The works shows that prior to 2002 the HALOE and Boulder records are in reasonable agreement, and provides some evidence that the change in HALOE sampling in late winter/spring after 2002 could contribute to the divergence of the trends between HALOE and Boulder when considering the longer time period through 2005. I find this idea and the presented analysis intriguing, but the work seems somewhat incomplete. I believe that more work could pretty easily be done to test the hypothesis that the trend difference is due to sampling changes in HALOE. Below, I provide some specific comments on the manuscript, and offer a few suggestions geared towards improving this analysis to make it better able to address the hypothesis it proposes and the underlying questions regarding trend differences between HALOE and FPH.

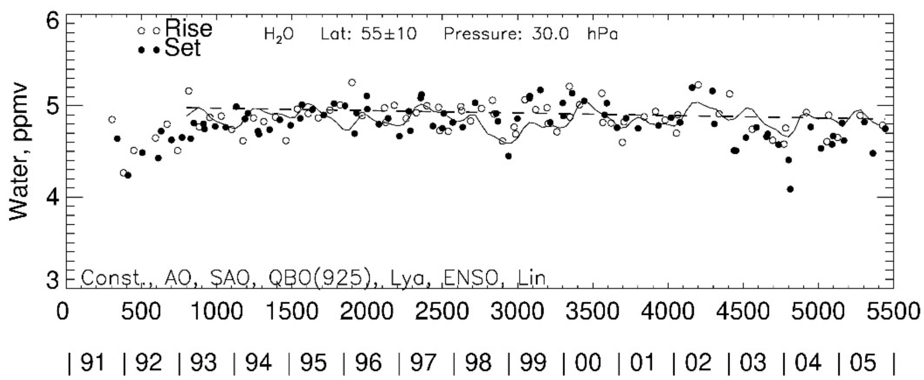
Thank you for your careful review of the manuscript.

General comments:

If the continued positive trend in WV at Boulder for and end time of 2005 (as opposed to 2002) is caused by vortex air sampling, shouldn't this show up in equivalent latitude changes in Boulder? This is indeed what Scherer et al. 2008 attempted to include, and they were unable to see any change that could explain the WV trends at Boulder. You could probably look at equivalent latitude as a predictor, or just changes in equivalent latitude at Boulder, to address this. On a related note, Lossow looked pretty thoroughly at the representativeness of Boulder and concluded that there was a real discrepant trend between HALOE and FPH that couldn't be explained simply by the fact that the FPH measurements come from a specific site. At the very least, some more thorough discussion/comparison to the work by Scherer and Lossow is warranted here.

If the HALOE and FPH trend differences are explainable by sampling of vortex air at Boulder during late winter/spring, then one could pretty easily look at seasonal trend estimates to see if the agreement is better during other seasons. This can be done very easily with MLR (see, e.g., the LOTUS activity for ozone).

I do not wish to expand this Technical Note into a larger-scale study, along the lines of the work of Scherer et al. and Lossow et al., or to carry out extensive detailed MLR studies like those in the LOTUS Report. It is my conclusion that the trend differences between HALOE and FPH are not due to the specific approaches in the MLR modeling, but due to the differing natures of the HALOE (large-scale) versus FPH *(local) measurements and to the sampling of HALOE toward the end of its data record. In particular, the FPH record includes smaller-scale variations, while the HALOE measurements cannot resolve them. Regarding the reduced HALOE sampling after 2001 in Fig. 1 at the latitude of Boulder, I plan to add the following figure that shows the HALOE time series at 55±10N latitude for comparison. It shows that the HALOE data for this higher latitude zone provides samples in all seasons even after 2001 and that the water vapor values are rather high in 2002. The 55° latitude bin spans 45N to 65N, to attain a minimum of four profiles for generating each average data point within this northern hemisphere sector.*



Specific comments:

Lines 44-45: This statement about the Lossow paper seems incomplete and should be more nuance, in my opinion. They showed that Boulder is representative of the zonal mean for trends over a few decades (e.g., late 1980's to 2010), but that at a decadal scale the trend differences between a single site and the zonal mean could be different.

I will make better note of their findings in the revised text.

Line 63: It would be nice to provide a brief summary of your MLR hereso that readers don't have to dig into multiple other papers, and to be as transparent as possible about what you've done. I see that the MLR is described in the next paragraph, so maybe you could just mention that it is described in detail in the next paragraph.

I will add more information about my MLR method in the revised text.

Line 69-70: You say HALOE WV values for SR events are larger than SS for 2002-2005, but the following sentence doesn't actually explain why. Please explain.

The solar lockdown position and subsequent altitude registrations for the SR versus SS measurements differ slightly and affect their retrieved water vapor profiles.

Line 80-82: You are using a 70° wide longitude range around Boulder and say that it can resolve wave-1/2 features in the SWV field. The 70° range represents 1/5th of the longitude values so I don't see how this is possible.

You are correct that a 70° range does not resolve zonal wave-2; I will change the sentence.

Line 82: Is the MLR model run with monthly means or daily data?

I did not interpolate the HALOE data to monthly means because the resulting spacing of the time series points would be altered, along with the amplitudes of the periodic terms from the MLR modeling. Typically, each data point in the series is an average over only several successive days within a month.

Line 83: What do you mean the QBO is approximated as a 28 month cycle? Are you not just using a QBO index like the Singapore winds at a certain level (or two), or the NCEP indices? Please explain.

I do not use the Singapore wind index as a QBO proxy, in part to avoid having to consider its lag at middle latitudes. Instead, initially I fit semi-annual and annual cycles to the time series of data points and analyze the residual for any remaining periodic terms. I find that there is a significant ~28-month cycle in the data in the middle stratosphere (see Remsberg, 2015), so I've used that periodic term for my final MLR modeling. However, I also note that the nominal, interannual cycle has a period closer to 30 months at 55N, 30 hPa (see figure above).

Line 86: I assume the AO is also significant? It is not listed here.

No, it is not highly significant (95%) at 40N and 30 hPa.

Line 89: I appreciate that the trend units of %/decade are useful, but can you please also give trend numbers in terms of ppmv/decade here and throughout the paper. A number of other studies (most, I thnk) have expressed water vapor trends this way, so it makes it much easier to cross-reference and compare with other studies.

I will include trends in those units.

Line 95-96: What do you mean about the MLR terms being non-orthogonal due to uneven spacing of the data? This doesn't make sense to me.

You are correct. I meant to say that the uneven spacing of the data points affects the estimated of error for each of the terms of the MLR model, although only marginally.

Line 98-99: I really think the first description of this drop was from a Randel et al 2006 paper, so that should be cited here.

I will add the reference.

Line 110: You interpolate FPH profiles, but do you apply any smoothing? I think it is appropriate to do so here, particularly if you are going to make any comparison between the noise/scatter of the FPH and that of HALOE as in Line 117. As FPH is much higher vertical resolution, it needs to be degraded to something approaching the HALOE resolution in order to make a reasonable comparison.

I do not smooth the FPH profiles, preferring to show the true nature of the FPH measurements instead. While they indicate a fair amount of scatter, I think it is likely that some of it is representative of the smaller-scale variations in atmospheric water vapor, especially where it has a significant meridional gradient and an incomplete mixing of its fields across latitudes.

Line 117: See above comment.

See my response to your comment about line 110 above.

Line 119: The SPARC 2000 report is pretty out of date at this point, and improved FPH estimates have been provided by Hall et al. Please cite this and use numbers from that paper, as they superceed anything that might have been in the old SPARC assessment.

I will delete SPARC 2000 and add your suggested reference and its numbers.

Hall, Emrys G, Allen F Jordan, Dale F Hurst, Samuel J Oltmans, Holger Vömel, Benjamin Kühnreich, and Volker Ebert. "Advancements, Measurement Uncertainties, and Recent Comparisons of the NOAA Frost Point Hygrometer." Atmospheric Measurement Techniques 9, no. 9 (September 5, 2016): 4295–4310. https://doi.org/10.5194/amt-9-4295-2016.

Line 145 (and elsewhere): These uncertainties seem extremely small to me. 1% is something like 0.05 ppmv, and the residuals (e.g. in Figure 3) are ~0.5 ppmv, so it seems unlikely that the trend values are really this precise. Has lag-1 autocorrelation of the residuals been taken into account for computing the trend significance? If the error bars are significantly larger, it would substantially change how one views the agreement (or lack thereof) between HALOE and FPH.

The slope of the linear term from the FPH time series is +5.8±1.2 %/decade, meaning it has a likely range of 4.6 to 7.0 %/decade. The range for the linear term from HALOE is 3.6 to 5.2 %/decade, which overlaps that from FPH. The corresponding term coefficients are uncertain by 21% (FPH) and by 18% (HALOE). On the other hand, the amplitudes of the periodic terms are small compared to their uncertainties from the MLR analyses of both data sets. The MLR modeling herein corrects for lag-1 effects, although the AR-1 *coefficient is small (almost no memory between adjacent points) for the FPH time series because of the scatter of its points.*

Line 162: What does “qualitatively accurate” mean here?

A poor choice of words, I agree. I will say ‘qualitative correct” based on the decay of the volcanic aerosol extinction from the HALOE data versus that of the aerosol extinction estimates from balloon-borne particle measurements over Laramie, WY (Hervig et al., 1996, Plate 1).

Figures:

A number of figures are really zoomed out on the y-axis, making it hard to see the data. E.g., in Figure 1 the data range goes from ~4.5-5 ppm but the y axis goes from 3 to 6 ppmv. Please zoom in the axes here and elsewhere.

The data range on the y-axis is intentional, so that one can easily see the differing natures of the HALOE versus the FPH time series data.

A number of the figures would make more sense if they were combined. E.g., Figures 1-2 together, 4-5, 9-10.

I will combine several of the figures into single figures for ease of comparison.