Response to Editor Report – Quantifying the sources of increasing stratospheric water vapour concentrations" by P. E. Sheese et al. (egusphere-2024-2946)

We thank the reviewers for the time they've spent on improving the paper and for their thoughtful comments.

Below are the reviewers' comments with our responses in green italics.

Reviewer 1

After this round of revision, I still find the motivation for this work unconvincing. As I mentioned in Comment 3, temperature and the Brewer-Dobson circulation (BDC) are inherently linked. My concern is not that the authors should further separate H_2O trends due to temperature changes into different contributing factors (e.g., BDC-driven vs. other influences). Rather, my point is that temperature and BDC are not independent variables, then the multivariate regression will be less meaningful.
A key issue is that when a BDC time series is not available, the N_2O time series should be included in the analysis to ensure it is not overly correlated with temperature before performing multivariate regression (maybe both should remove the climatology). Without addressing this, the regression results between BDC and water vapor could simply reflect an indirect relationship—where temperature influences both BDC and water vapor—rather than a direct effect of BDC on water vapor.

The previous iteration of the manuscript and the response to the reviewers stated, "At all altitudes and latitudes, the absolute correlation between measured N_2O and T_{lag} time series is less than 0.35 and is typically below 0.2." We've now added a sentence directly before this, which states, "To ensure that it is appropriate to simultaneously use T_{lag} and local N_2O time series as regressors, the correlation between these time series was calculated in each altitude and latitude bin."

Additionally, Dessler et al. (2013) identified QBO, BDC, and temperature at 500 hPa (rather than the tropopause region) as the primary factors influencing water vapor trends. This choice is reasonable because 500 hPa temperature is more independent of QBO and BDC.

Dessler et al. (2013) make use of a temperature time series from 500 hPa, but they do not discuss or indicate that using a time series from 500 hPa is preferable over time series from any other pressure level. We are using a temperature time series from 100 hPa in order to remain consistent with the methodology of Hegglin et al. (2014; 10.1038/NGEO2236).

To strengthen the study's justification, the authors should clarify why including both BDC and tropopause temperature is necessary and valid, given their interdependence. Without addressing this, the statement in the reply to Reviewers 1 & 2 that "this is the first paper to study all three factors" does not seem fully justified, which, could be why previous studies have not treated temperature, BDC, and methane (which is truly independent) as three separate factors influencing water vapor.

Other studies (e.g. Hegglin et al. (2014), Tao et al. (2023)) have treated tropical tropopause region temperatures and local BDC changes independently. As written in the manuscript and clarified above, the lagged temperature time series and local N_2O time series exhibit low correlation and can therefore be treated as independent variables within the multivariable regression. Further, we cannot justify the statement "this is the first paper to study all three factors," as we at no point stated or implied this. In our initial responses, to Reviewer 1 we wrote, "We are the first to parse trends due to the three main sources at all latitudes." To Reviewer 2 we wrote, "...this study is the first to quantify H_2O trends due to all three sources (tropopause temperatures, BDC, and CH_4) at once as a function of latitude and altitude." Similarly, the abstract states, "...none have simultaneously quantified the contributions from all main sources (temperature variations in the tropical tropopause region, changes in the Brewer-Dobson circulation, and changes in methane (CH_4) concentrations and oxidation) at all latitudes." Since the manuscript already explains why tropopause region temperatures and ACE-FTS N_2O are used simultaneously in the regression, no changes have been made other than the added statement detailed above.

Reviewer 2

Compared with the previous version, it is significantly improved. I have still only a few minor comments listed below:

Introduction:

L36: At higher altitudes and more poleward latitudes, H2O variations tended to follow those of the modeled temperatures with a lag of a few months (tape-recorded effect, Mote et al., JGR, 1996).

"(tape-recorded effect, Mote et al., 1996)" has been added.

L48: A number of studies... Please include the Tao et al. 2023 paper, which mainly analyzes the interaction between temperature variations in the tropical tropopause region and changes in the Brewer-Dobson circulation. You use it later on in the paper. A detailed analysis of the changes in methane concentrations is the biggest novel advantage of your paper. I would recommend reformulating this part.

We now include the Tao et al. reference here as well.

At the end of this paragraph, we now also include the statement, "ACE-FTS is in a unique position when it comes to investigating the influence of CH_4 oxidation on stratospheric H_2O trends as it is currently the only Earth observing satellite instrument that makes vertically resolved measurements of both H_2O and CH_4 throughout the stratosphere."

Fig. 12 and 13: Please switch.

These figures are now in the correct order.

Fig. 1 caption: ...where only regressing to linear components... I would not mention "linear components." You do not use it in the main text explaining your procedure like in lines 178-179. Linear components are trends. Please check other captions as well (e.g., Fig. 4).

"…regressing to linear components and…" has been removed here and in the text.

You have 15 figures in total. I would recommend shifting a few of them to an appendix.

We have made an appendix and have moved two figures (time-lagged ENSO and trop figure and effect of correlation with AO figure), as well as their discussion, to there.

Fig. 5: The optimal time lags scatter very strongly and do not look very convincing. I would shift it to the appendix. Their contribution is very weak.

These were added in response to one of the previous reviewers. The figure and its discussion have been moved to the appendix.

Fig. 8: Please include the abbreviation T_lag into the caption and maybe into the legend of the the color bar. By the way, its distribution is much more convincing than time lags of ENSO and Trop.

We have added T_{lag} to the caption.

Fig. 14: This figure could also be shifted to the appendix. I did not get the point of the analysis with and without AO.

This was added in response to one of the previous reviewers. The figure and its discussion have been moved to the appendix.