acp reviewer1 revisions

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We thank Reviewer 1 for their thorough review. In our response, the reviewer's comments are bolded, our answers are normal weight, and anything that we change in the paper is italicized.

Major Comment 1: There is a misconception in transferring your results from the trended to the detrended regression analysis. The problem here is, that you use the regression coefficients β directly in some places to analyse your results (e.g. in Table 2, 3, 4 and in Fig. 3 and 5 and accompanying text) and not either the explained variance (i.e. VAR($\beta\Delta T$ ΔT)) or the regression coefficient multiplied by the standard deviation of the explanatory variable (i.e. $\beta\Delta T$ STD(ΔT)). At some places (Page 4, line 5–6) you look at these quantities, but unfortunately at Page 4, line 14–15, you draw the conclusion "This confirms the stratospheric water vapor feedback [. . .]" from the similarity of the regression coefficients in the trended and the detrended analysis. Unfortunately, this is an invalid conclusion. Even if the regression coefficients would stay exactly identical, the percentage of explained variance that an explanatory time series explains of the total explained variance R^2 can change dramatically between the trended and detrended regression analysis. An obvious example is an explanatory time series with a large trend and a small interannual variability. An explanatory time series like this will likely contribute a large explained variance to the trended regression analysis, but a small explained variance (in percent) to a detrended regression analysis, while its regression coefficient may be very similar in the trended and detrended analysis. Unfortunately, your example time series for ΔT in Fig. 2 looks a little like this (compared to the variance and trend of the BDC time series). Since we agree that you can't really use the trended analysis to confirm your main conclusion (Page 3, line 26– 27), you have to base your conclusion that changes in tropospheric temperature cause changes in stratospheric water vapor on the detrended regression analysis. That means you have to confirm that a large part of the interannual variability of stratospheric water vapor in the detrended regression analysis comes from interannual variability in the ΔT term. That still will not be a proof of causality,

but will put much more confidence in your main conclusions

The reviewer makes an excellent point here. In response, we have added new columns to Tables 2, 3, and 4 that show the correlation coefficient scaled by the standard deviation of the predictor time series. This is described on page 5, lines 1-3 of the manuscript, and we have modified our discussion to incorporate these values (page 5, lines 3-10 for the century regressions, and page 6, lines 32-35 for the decadal regressions). Additionally, we added a sentence to the conclusions, page 7, lines 29-30, to summarize the results.

Additional remark 1: Since it is known that variability in stratospheric water vapor comes from variations in the tropopause temperature (more exactly: Langrarian dry points, see e.g. Fueglistaler, 2013), it would put much more confidence in your main conclusions if you show that tropospheric temperature and tropopause temperatures correlate in your models.

We have modified the text to discuss this (page 2, lines 7-10).

Additional remark 2: Giving values as explained variances makes it easier to compare values between different time series as ΔT and BDC. In the moment, it is easy to compare between models in the rows of your tables, but impossible to do that between the columns of your tables.

We have added new columns to Tables 2, 3, and 4 that show the correlation coefficient scaled by the standard deviation of the predictor time series. This is described on page 5, lines 1-3 of the manuscript, and we have modified our discussion to incorporate these values (page 5, lines 3-10 for the century regressions, and page 6, lines 32-35 for the decadal regressions). Additionally, we added a sentence to the conclusions, page 7, lines 29-30, to summarize the results.

Additional remark 3: I have to emphasize that I am pretty sure that the trend in ΔT and in stratospheric water vapor are causally connected, I just think that a trended regression analysis is not the tool to show that. You have to avoid the impression that your trended regression analysis is a proof of that. My suggestion is the following: Add values for the explained variance of the explanatory time series (e.g. in ppm^2) to the tables 2, 3 and 4 (you can keep the regression coefficients or replace them by these values). Alternatively, you can add values for the regression coefficients multiplied by the standard deviations of the explanatory time series to the tables. Both explained variance and standard deviation have advantages and disadvantages: The explained variances of the explanatory timeseries add up to the overall explained variance (under the assumption that the explana-

tory time series are uncorrelated), but values in ppm^2 are not very intuitive. Standard deviations are more intuitive, but don't add up. Thus, I will not give a recommendation what is better here. Next, change Figure 3 to show explained variances or standard deviations, or add an additional figure doing this. Then, base your discussion on the explained variances, where it does matter for your conclusions (e.g. in section 3.2).

We have added new columns to Tables 2, 3, and 4 that show the correlation coefficient scaled by the standard deviation of the predictor time series. This is described on page 5, lines 1-3 of the manuscript, and we have modified our discussion to incorporate these values (page 5, lines 3-10 for the century regressions, and page 6, lines 32-35 for the decadal regressions). Additionally, we added a sentence to the conclusions, page 7, lines 29-30, to summarize the results.

Major Comment 2: It is not straightforward that more stratospheric water vapor means more warming of the troposphere, and there is not enough discussion in your paper in the moment to support your main conclusion "A stratospheric water vapor feedback exists, where a warming climate increases stratospheric water vapor, leading to further tropospheric warming". Please at least discuss the literature on that shortly [e.g. *Oinas et al.*, 2001; *Solomon et al.*, 2010]. That would give much more confidence that this statement is actually correct. Is the feedback by an increase in downward longwave radiation from the stratosphere? That does not seem to be straightforward to me. One the one hand, you have more water vapor to emit radiation. On the other hand, the stratosphere gets cooler, which reduces radiation. In a simple picture, where water vapor only emits longwave radiation and the stratosphere is heated by shortwave radiation by ozone, wouldn't the outgoing longwave radiation from a layer where you add more water vapor just stay constant to maintain radiative equilibrium, by a lowered radiative equilibrium temperature?

We replaced the first sentence of the paper, with a sentence referring to the literature describing this process (page 1, lines 11-12), and removed the sentence in question. That said, we have not added any discussion of this to the paper because this is a well-documented phenomenon.

Major Comment 3: It seems to me that you take the positive correlation between tropospheric temperature and stratospheric water vapor as very obvious. However, this is not simple and obvious at all. Again, discuss the literature on that shortly, and try to avoid the impression that this is an obvious fact.

We have added a short discussion of this to the manuscript (page 2, lines 10-13)

and have hopefully changed the tone, per the reviewer's comment.

Major Comment 4: Since multiple regression can only show correlation but not causality, some more discussion on the supposed reasons for the correlations would be very helpful, in particular for the ΔT term. In my opinion, it should also be discussed that the reasons for a correlation can be very different in a model and in reality (i.e. based on observations). Just to give a simple example: The correlation between tropospheric temperatures and stratospheric water vapor can possibly be caused by excessive transport or diffusion of water vapor over the tropopause in the models [see *Hardiman et al.*, 2015]: Higher tropospheric temperatures means more moisture, which then could be transported by spurious vertical numerical diffusion into the stratosphere. A way to test for things like this could be e.g. to look at the tropical tropopause temperatures and their correlation to tropospheric temperatures and stratospheric water vapor.

We have added a caveat to this point on page 2, line 10.

Major Comment 5: Relating to this: There is a lack of information on the model performance and parameterizations of the used models. At least some information of the following list would be very helpful to assess your results. I acknowledge that it would be a lot of work to answer all of these questions for all of the models. But I think that there should be at least some discussion about how the processes in the model can affect the results. Of course, I don't want you to discuss all of these issues in detail, but to discuss things that are important for your results, i.e. take the list below as a list of suggestions.

- **What is the tropopause temperature in the models, and how does it compare to measurements in terms of bias, annual cycle and trends? Can it explain the water vapor in the model or are there additional processes at work?**
- **How well is the Brewer-Dobson circulation represented?**
- **How is convection parametrized? How well does it compare to observations? Is there overshooting?**
- **How is radiation parametrized? What is the effect of clouds on radiation?**
- **What is the spatial pattern of Local dry points (LDPs) in the models and compared to reality? Can a shift in their distribution cause the correlation?**
- **Effect of (spurious) diffusion and transport?**

Our paper is narrowly focused on quantifying the contributions of various processes to $[H_2O]_{entry}$ variability. There are many branches we could take in our discussion and we feel that we've covered the essential information required to achieve our objective. If the reviewer has a specific topic they would like to see discussed, we're happy to consider that suggestion. We also note that most of the suggestions listed above by the reviewer is already available in the literature (*Gettelman et al.* [e.g. 2010] compares TTL temperatures in the models; individual model papers discuss their parameterizations have been added to table 1 (also listed in *Morgenstern et al.* [2010, 2016]).

Specific Comment 1: Page 1, line 1 and page 2, line 14: Please give a citation here, e.g. *Gettelman et al.* [2010] (e.g. Fig 17) or *Kim et al.* [2013]

ACP does not prefer that citations be in the abstract, so for page 1, line 1, we will leave this to the discretion of the editor. The sentence on page 2 line 14 of the original manuscript has been removed from the current version of this manuscript.

Specific Comment 2: Page 1, line 4: You probably mean stratospheric humidity. Please Clarify.

Yes, "humidity" has been changed to "stratospheric humidity" in this line.

Specific Comment 3: Page 1, line 4: In case you base that statement on your trended regression analysis, is it really correct? Correlation does not imply causality, especially in a trended regression analysis. The statement that you give on page 3, line 26-27 is a direct contradiction of what you state here. In fact, I think you cannot support that statement with the information you currently give in the paper. I would try to phrase that more carefully, e.g. by speaking of correlations, or make clear that this conclusion comes not from your trended analysis, but from some other source.

We acknowledge that correlation does not imply causality, and we believe that we clearly base our conclusions on the detrended analysis.

Specific Comment 4: Page 1, line 3-5: Since there is the contradicting trend from increasing cooling by the BDC (as you note here and is seen in your figure 2), can you really make the statement that the net trend in humidity is primarily driven by tropospheric warming (that would imply to me that, say, something like 80% or 90% of the net trend comes from the ΔT term)? It seems to me that the trend by the BDC is in the same order of magnitude (but that the net effect of both trends is normally positive). Please add a figure showing the trends by the BDC term and the ΔT term for every model to quantify

the trends and to underpin your statement. I think such a figure is probably easy to add.

We added a paragraph, beginning on page 4, line 30, discussing this.

Specific Comment 5: Page 1, line 6-7: I don't quite understand why you split your time series into 10 year chunks? Would it not be ok to compare the 100 year time series to the 10 years of observations directly?

There are obviously many ways to compare to the MLS-based results. Our opinion is that the best way is the way we've done it in the paper. If one wants to compare the MLS results to the entire 100-year CCM run, the reader can do that by comparing the MLS coefficients (Table 4) to those from the detrended 100-year regressions (Table 3). We have added text on page 5, lines 13-14 to clarify our comparison.

Specific Comment 6: Page 1, line 8: It is not clear to me what exactly you are referring to. Is it really that new to apply a linear regression model to these data (one of your own papers did that already: *Dessler et al.* [2013]? I suggest to delete that last sentence of the abstract or be more specific here: What is superior to what?

We do consider this new in that we show the utility of comparison between models as a way to evaluate them. This is clearly superior to previous comparisons, *Gettelman et al.* [e.g. 2010].

Specific Comment 7: Page 1, line 11 to Page 2, Line 2: Instead of speaking of the TTL temperatures as the determining factor, one can get more specific here. It is the temperature of the coldest point along each air mass trajectory (i.e. the Lagrangian dry point) which determines the stratospheric water vapor (except for direct injection by overshooting). In many cases this temperature will be reached at or near the tropical tropopause.

In order to be more specific, we have modified the text on page 1, lines 16-19.

Specific Comment 8: Page 1, line 18-19: Would be nice to add a citation here, e.g. one of the Fueglistaler papers

We modified the text, and added several citations to support our claim regarding the Brewer-Dobson Circulation and QBO on page 2, lines 3-6.

Specific Comment 9: Page 2, line 1-2: No, it doesn't imply that.

See general comment 2. In addition: the local effect of more water vapor is more cooling in the stratosphere, so it is better to be more specific and to write "further tropospheric warming".

We have made that change.

Specific Comment 10: Page 3, line 14: Probably, it is better to speak of "autocorrelation in the residuals" than of "autocorrelation of the time series", since it is only the remaining autocorrelation in the residuals that affects the uncertainty.

We have changed "autocorrelation of the time series" to "autocorrelation in the residuals".

Specific Comment 11: Page 3, line 18-19: You are aware that subtracting a constant does only change β_0, but does not change anything else in the regression analysis

We know that. We believe our method is clear, as written.

Specific Comment 12: Page 4, line 14-15: No, it doesn't confirm that, see major point 1.

We have removed the sentence.

Specific Comment 13: Page 4, line 18-19: This doesn't really tell you anything, see Page 4, line 14-15.

While we agree with the reviewer's point that this should not be over-interpreted, we also feel that this is a statement worth making here. We do not believe what is written is incorrect.

Specific Comment 14: Section 4: I don't really get the additional benefit of splitting the time series into 10 year chunks. Wouldn't a direct comparison of the observational 10 year time series and the model 100 year time series give all the information important?

There are obviously many ways to compare to the MLS-based results. Our opinion is that the best way is the way we've done it in the paper. If one wants to compare the MLS results to the entire 100-year CCM run, the reader can do that by comparing the MLS coefficients (Table 4) to those from the detrended 100-year regressions (Table 3). We have added text on page 5, lines 13-14 to clarify our comparison.

Specific Comment 15: Page 7, line 8-9: I find the statement that you can assess the realism of the model trend by a linear regression

somewhat problematic. If there is a trend in stratospheric water vapor in the models and there is a trend in one of the explanatory variables, the explanatory variable will try to fit this trend, whatever the magnitude is and whatever the underlying physical reason of the trend is. If it turns out then, that the fit of the interannual variability is also good, that may give you confidence. But in general, you always have the problem that a linear regression analysis does not tell you anything about causal relationships.

We have changed the sentence to read: "We demonstrated in this paper a new way to evaluate the physical processes underlying these model trends."

Specific Comment 16: Page 7 line 13: See second specific comment for page 1, line 4. I would phrase that more carefully.

We don't know what the reviewer is referring to, more clarification would be appreciated.

Specific Comment 17: Page 7, 13-16: I think it would make sense to cite some studies here and to discuss your results in comparison to other studies (briefly), e.g. studies that deal with the absence of the QBO in many models that show the influence of the BDC on tropopause temperatures and its increasing trend etc.

We have added citations to page 7,lines 26-30; page 8 lines 1-3, that investigate influences of both the BDC and QBO on the TTL.

Specific Comment 18: Page 7, line 21: I would agree, but I would base that statement mainly on the detrended regression analysis. If there is a good overall fit of the detrended model, you can have some confidence that the explanatory time series actually are relevant processes for the regression variable, and that the magnitude of their fit does tell you something. Since, regression analysis does not tell you anything about causal relationships however, you need to put some a priori knowledge into that. For that reason, I would be very careful to interpret the trended analysis, since there is the danger that there is no causal relationship between the trends (and the trends lead to a correlation between explanatory variables, which can make the magnitude of the fit for these variables a little bit arbitrary in the worst case.

We agree but don't believe this is a problem, as written.

Specific Comment 19: Page 7, line 22: That is a conclusion I would mainly draw from comparison with observations or testing the model's processes. A regression model can only help you in confirming this.

E.g. What would happen if all models would overestimate variability of water vapor in the future? Your fit coefficients would get larger to try to fit this variability better. Do you learn from that that the model does a good job?

We have modified the text on page 8, lines 8-9 to account for this.

Specific Comment 20: Page 7, line 22-23: This might however also be a deficiency of the regression approach, e.g. an explanatory variable that is no perfect proxy for the BDC, or that the trends dominate the fit (which gives rise to correlation between the explanatory variables leading to uncertainties in the magnitude of the fit for the BDC.

It's always possible that our analysis might be wrong (for a large number of reasons), but we feel our work is adequately caveatted. If the author has a specific uncertainty/caveat that they'd like us to add, we're happy to consider it.

Technical Revision 1: In the title you write "lower-stratospheric", later you write "lower stratospheric" would be nice to have consistency.

"Lower stratospheric" has been changed to "lower-stratospheric" throughout the paper.

Technical Revision 2: Page 2, line 27: Change "ozone-depleting substance" to "ozone-depleting substances"

Done

Technical Revision 3: Page 2, line 31: Change "described described" to "described".

Done

Technical Revision 4: Page 5, line 12: A period is missing ("...regression. However").

Done

Technical Revision 5: Page 7, line 22: Change "appear do" to "appear to do"

Done

References

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Oinas, V., A. A. Lacis, D. Rind, D. T. Shindell, and J. E. Hansen (2001), Radiative cooling by stratospheric water vapor: Big differences in gcm results, *Geophysical Research Letters*, *28*(14), 2791–2794, doi:10.1029/2001GL013137.

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