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Interactive comment on "Testing chemistry-climate models' regulation of tropical lower-stratospheric water vapor" *by* Kevin M. Smalley et al.

Anonymous Referee #1

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This study deals with the effect of climate change on stratospheric water vapor, in particular the effect that changes in e.g. the tropospheric temperature or the Brewer-Dobson circulation have on lower stratospheric water vapor. In my opinion, this is a relevant topic and the study is worth publishing.

I found the paper readable and well-structured and most of the used methods are scientifically sound. Unfortunately, I think there is a misconception in transferring your results from the trended to the detrended regression analysis, which affects your main conclusions (see major comment 1). In addition, there is some lack of discussion and interpretation of your results, in particular at places where I think it is needed to support your main conclusions. Unfortunately, that means that in my opinion you currently give

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not enough information in the paper to support your two main conclusions (A. “The long-term trend in stratospheric humidity is driven by the warming of the troposphere”, B. “A stratospheric water vapor feedback exists, where a warming climate increases stratospheric water vapor, leading to further tropospheric warming.”)

I think some more discussion and information and a correct treatment of the detrended analysis would solve this issue and make it easier for the reader to assess your results. Thus, I suggest to publish the paper after the necessary revisions, which are probably relatively easy to implement.

Major comments

- 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$(\beta_{\Delta T}\Delta T)$) 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 timeseries 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 varibaility in the ΔT term. That still will not be a proof of causality, but will put much more confidence in your main conclusions.

Additional remark 1: Since it is known that variability in stratospheric water vapor comes from variations in the tropopause temperature (more exactly: Lagrangian 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.

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.

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

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deviation have advantages and disadvantages: The explained variances of the explanatory timeseries add up to the overall explained variance (under the assumption that the explanatory timeseries 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).

- 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 mantain radiative equilibrium, by a lowered radiative equilibrium temperature?

- 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.

Certainly, there is the simple way of thinking about it as shown in Fig. 6 of Shepherd (2002). But the radiative balance at the tropical tropopause is rather delicate, since we are near the line of zero radiative heating and it is difficult to say a priori (without detailed radiative transfer calculations) what will happen when greenhouse gases increase. Different greenhouse gases have rather different behaviour near the tropopause, e.g. more water vapor will cool in the longwave region, while more CO_2 will heat in the shortwave region, and heat or cool in the longwave region dependent on altitude, e.g. Gettelman et al., 2004. And I wonder what the effect of convection is on that (which has its main detrainment level far below the tropopause but reaches up to the tropopause). Papers that discuss how complicated the influences on tropopause temperature are, are e.g. Lin et al. (2016) or Gettelman et al. (2004).

While there are relatively clear positive trends in tropopause temperature and tropopause altitude in most models as a response to increase in greenhouse gases (e.g. Kim et al., 2013, Gettelman et al., 2010) (probably caused by increased heating by CO_2), observed trends in these quantities are more inconclusive (see e.g. Fueglistaler et al., 2013) and it is even difficult to prove unequivocally that the observed water vapor can be solely explained by the observed tropopause temperatures and location of the LDPs.

- 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 e.g. Hardiman et al., 2015 for a paper discussing this): 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.

- 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.
  - 5a. 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?
  - 5b. How well is the Brewer-Dobson circulation represented?
  - 5c. How is convection parameterized? How well does it compare to observations? Is there overshooting?
  - 5d. How is radiation parameterized? What is the effect of clouds on radiation?
  - 5e. 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?
  - 5f. Effect of (spurious) diffusion and transport

Specific comments

- 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).

- Page 1, line 4: You probably mean stratospheric humidity. Please clarify.
- Same sentence: 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 regression analysis, but from some other source.
- Page 1, line 3–5: Since there is the counteracting trend from increased 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 come 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.
- Page 1, line 6–7: I don't quite understand why you split your timeseries into 10 year chunks? Would it not be ok to compare the 100 year timeseries to the 10 years of observations directly?
- 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 either suggest to delete the last sentence of the abstract or to be more specific here: What is superior to what?
- 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.

- Page 1, line 18–19: Would be nice to add a citation here, e.g. one of the Fueglistaler papers.
- 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".
- Page 3, line 14: Probably it is better to speak of "autocorrelation in the residuals" than of "autocorrelation of the timeseries", since it is only the remaining autocorrelation in the residuals that affects the uncertainties.
- 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?
- Page 4, line 14–15: No it doesn't confirm that, see major comment 1.
- Page 4, line 18–19: This doesn't really tell you anything, see Page 4, line 14–15.
- Section 4: I don't really get the additional benefit of splitting the timeseries 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 information that is important?
- 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

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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 some confidence. But in general, you have always the problem that a linear regression analysis does not tell you anything about causal relationships.

Page 7, line 13: See 2nd specific comment for Page 1, line 4. I would phrase that more carefully.

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.

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 timeseries actually are relevant processes for the explained 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).

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 fit this variability better. Do you learn from that that the model does a good job?

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.

Technical corrections

- In the title you write “lower-stratospheric”, later you write “lower stratospheric”. Would be nice to have that consistent.
- Page 2, line 27: Change “ozone depleting substance” to “ozone depleting substances”.
- Page 2, line 31: Change “described described” to “described”.
- Page 5, line 12: A period is missing (“. . . regression. However. . . ”).
- Page 7, line 22: Change “appear do” to “appear to do”

References

- Fueglistaler et al., J. Geophys. Res., 118, doi:10.1002/jgrd.50157, 2013
- Gettelman et al., J. Geophys. Res., 109, doi:10.1029/2003JD004190, 2004
- Gettelman et al., J. Geophys. Res., 115, doi:10.1029/2009JD013638, 2010
- Hardiman et al., J. Clim., 28, 6516, 2015
- Kim et al., J. Geophys. Res., 118, doi:10.1002/jgrd.50649, 2013
- Lin et al., J. Clim., doi:10.1175/JCLI-D-16-0457.1, 2016
- Oinas et al., Geophys. Res. Lett., doi:10.1029/2001GL013137, 2001

- Shepherd, J. Met. Soc. Japan, 80, 4B, 769, 2002
- Solomon et al., Science, 327, 1219, 2010

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