While I acknowledge that you resolved several of the issues discussed in my review and I am quite pleased now with the discussion of the linear regression, I am sorry to say that I am not willing to accept your manuscript in the current version. You ignored or misinterpreted several of my major comments (2, 3, 4, 5) and specific comments. Don't understand me wrong, you don't need to follow all of my comments, as long as you give good arguments. But that isn't the case here in my opinion. Many of my comments to the original manuscript aimed at the fact that the paper was lacking a well-balanced discussion and assessment of the results. The new manuscript version certainly has improved upon this, but there are still quite a number of issues.

General

- There are obvious omissions in the discussion that possibly could lead the reader to wrong conclusions (e.g. not discussing the known biases in tropopause temperature in many models or the fact that the correlation between stratospheric water vapor and tropospheric temperature can well be caused by spurious diffusion over the tropopause, see major comments 4–5).

- I noticed that you uploaded a supplement containing additional figures that add the models not shown in Figure 2. This is a very welcome addition to the paper in my opinion and I would strongly suggest to move these figures into the main body of the paper, since they add a lot of interesting information and only very moderately increase the length of your paper. If you reduce the size of the figures a little bit, they perhaps would fit on two pages as two figures with sub-panels.

- Please consider that you are addressing a broader audience here. What may seem completely obvious to you and some of your colleagues, may not be general knowledge in the wider atmospheric community.

- Just a suggestion for a title that reflects a little bit better what you have done: "Contribution of different processes to changes in tropical lower stratospheric water vapor in chemistery-climate models" or if it is ok that the title is a little bit longer "Contribution of the Brewer-Dobson circulation, the quasi-biennial oscillation and of changes in tropospheric temperature to changes in tropical lower stratospheric water vapor in chemistery-climate models".

- Partially resolved, but some of my comments in the following address this: Try to avoid exaggerations (e.g. the last sentence of the abstract), discuss other studies that are relevant in the context of your paper and don't draw conclusions that are not supported by the results of your study.

Further discussion on old comments and your replies

- Major comment 2: I am pleased that you removed the sentence in question and added the additional sentence to page 1, lines 11–12.

  You write "that is a well-documented phenomenon" and "water vapor is well-known to be a greenhouse gas". This is of course correct, and of course, I did not question this in any way. Nobody denies that water vapor is a greenhouse gas.

  But that is not the point here. Maybe I was not clear enough in my explanation and what I aimed at. The point is if there is a feedback on tropospheric temperatures. You need detailed radiative transfer model calculations to show that there is a significant increase in radiative forcing or temperatures of the troposphere by increases in stratospheric water vapor. None of the papers cited by me or you states a priori that there is a relevant radiative forcing of the troposphere from stratospheric water vapor. All these studies run a radiative transfer model, and then draw this as a conclusion by giving some value for the radiative forcing or temperature change. In addition, a feedback requires that higher tropospheric temperatures lead to higher tropopause temperatures, which is even less clear a priori, see major comment 3 in the original review and this document.

  The second thing is that "well-documented phenomenon" does not really hit the point. It may be well-documented by studies that are known to a certain part of our community, but you are writing for a wider audience here, which may not necessarily know the same papers as you. You can't expect the same a priori knowlegde from everyone and it is the purpose of an introduction to point the reader to the relevant literature.

  That said, I have no objections that you discuss a possible feedback here, as long as you make clear that this is not obvious and discuss the literature, and as long as you make clear that this is not a result of your study. You don't need to delete any reference to that.

- Major comment 3: You state that you have added discussion at page 2, line 10–13. But there is no discussion at this place. Did you confuse pages or line numbers here? Is it the discussion at lines 5–8?

  I am not satisfied with this. There is still no discussion that the correlation of stratospheric water vapor and tropospheric temperatures due to their long-term increase is basically a model phenomenon and can't be confirmed by the available observations. There is simply no clear trend in tropopause temperatures or water vapor in the observations (e.g. Gettelman, Fueglistaler). In addition, you don't discuss that the correlation between tropospheric warming and increasing tropopause temperatures is not that obvious from a theoretical point of view (e.g. Lin, Shepherd).

  Quite in the contrary you state "There are good physical reasons for this connection": Please rephrase. This sounds more like an annoyed comment aimed at me than as a statement aimed at the reader. And it is somewhat ironic that you cite the Lin et al. paper here: If I may cite from the abstract: “Given the subtle nature of the balance among all these factors, it might be surprising that almost all GCMs and CCMs predict a warming [...] of the tropical tropopause [...]”, and later (section 4) “In practice, the magnitude of tropopause warming vary vastly from model to model”. I may also cite Shepherd (2002), page 778, referring to the sketch showing the conceptual relationship between tropospheric warming and warming at the tropopause “[...] and certainly not as simple as depicted in Fig. 6b”. And please see what I have said to specific comment 16. In particular, why don’t you mention that this is only seen in models, but there is no clear evidence from observations? In summary, please try to give a more well-balanced discussion here (or in the conclusions, see specific comment 16).

- Major comments 4 and 5: I am not satisfied how you treat these major comments, which are basically ignored. I certainly do not want you to change the scope of the paper or to bloat it with unnecessary information. However, discussing the performance of the aspects of the models which are important for your analysis is crucial for the reader to be able to assess your results and their reliability (especially to assess if these model based results can be transferred to reality).

  Interestingly, you discuss the QBO term in some detail, but largely avoid to discuss the ΔT term. Since you ask for specific topics that I would like to see discussed, here is one: Discuss the bias and annual cycle of tropopause temperatures *compared to observations*, in a similar manner as in Fig. 1 of Gettelman et al. (2010). It is not sufficient to point me to the Gettelman paper. It does not discuss the same models, and I am not able to find out easily if the 6 models that are discussed both in the Gettelman paper and in your paper have the same model version etc. It is also not sufficient to point me to the papers that you added to Table 1. First of all, you can’t demand from the reader (or the reviewer) to read through 15 lengthy papers to find out some information that is significant for your paper. Next, by quickly scanning through the cited papers, I am pretty sure that most of them do not contain the relevant information (e.g. tropopause temperatures).

  Another specific topic which is important to discuss in my opinion is spurious diffusion of water vapor across the tropopause. There is an extremely large gradient of water vapor near the tropopause and at the same time, models are well known to be too diffusive compared to reality (especially in the stratosphere, where the effective diffusion coefficients are 100 times smaller than in the troposphere). This problem is well documented in the literature (e.g. Gettelman et al., 2010, page 11, Hardiman, 2015, section 3). It is well possible that the relationship between stratospheric water vapor and tropospheric temperature is dominated by this effect (at least in some models) and not discussing this may lead the reader to the wrong conclusion that he can transfer the results of your study (trends, contribution of the different terms) more easily to the real behaviour of the atmosphere than it is actually the case.

In this respect, I am also not very satisfied with your answer to major comment 4. You say that you have added a caveat to the paper, but in fact you did not adress the point I discussed in major comment 4. I was talking about spurious diffusion in the comment, but the caveat you added to the text deals with overshooting convection. This is certainly also an interesting point, but not what I was talking about.

Another issue is the BDC, which is also not discussed. How well the BDC is represented in the models will have implications for the contribution of the BDC to the trend and variability of stratospheric water vapor in your regression model. E.g., if the BDC is too fast in a model (compared e.g. to w^* derived from reanalyses), it will lead to an overestimation of this term in your regression analysis compared to reality.

- Specific comment 1 (was Page 1 , Line 1 and Page 2, line 14): Was there any reason apart from this comment that caused you to remove the sentence? The aim of my comment was certainly not that you remove the sentence, but that you add the citations. Now there is the unfortunate situation that the sentence is still in the manuscript (in the abstract), but that you can't give the relevant citations (I acknowledge that it is no good idea to cite in the abstract). And to cite the relevant literature is certainly appropriate for this central statement.

- Specific comment 3: For the reasons given in my review, I still think this is a problematic statement. In addition: In your reply to this comment, you state "we clearly base our conclusions on the detrended analysis". But this sentence explicitely refers to the trend in humidity. What do you want to tell me with your statement? Please also see my detailed comments to specific comment 16 below.

- Specific comment 6 (was Page 1, line 8): I am not satisfied how you treat this comment. You neither deleted or changed the sentence, nor did you explain to me in your reply what you mean by "superior" and to what the statement refers in a satisfactory way.

  This comment was one of the more important specific comments I made, since this statement is in the abstract at a rather prominent position, and it is just an unproven und unclear statement. You should try to avoid the impression that you put this sentence into the abstract just to create interest for your article, without anything really supporting this statement.

  Since you refer to the Gettelman paper in your reply: Do you mean that applying a multiple linear regression model is better than just looking at plots of stratospheric humidity and tropopause temperature? Then, why don't you write it, neither in the reply to my comments, nor in the abstract?

And if this is really what you mean, is it really worth mentioning? It was certainly not the intention of Gettelman et al. to do a multiple regression analysis and for the purpose of their paper, it was sufficient to show the plots. And there are studies, including your own studies, which already used multiple linear regression. So, what is the point here?

- Specific comment 7 (was Page 1, line 11): It is nice that you refer to the LDPs now, but unfortunately, the sentence is not quite correct. The coldest temperatures in the TTL are not necessarily at the location where an individual trajectory has its LDP, which may cross the tropopause at a warmer location. I suggest to rephrase the sentence so that the statement is correct.

- Specific comment 16 (was Page 7, line 13, now lines 26–27): That is referring to the identical sentence on page 1, line 4 (old manuscript) and the comment referring to it (specific comment 3). There needs to be more discussion here, and I find the statement here problematic. You can't draw the conclusion that the trend in the warming of the troposphere drives the trend in stratospheric water vapor from your trended regression analysis (as you admit in line 26–27, page 3 in the old manuscript). *Any* timeseries with a trend will fit your stratospheric water vapor time series. I.e., it is just not correct to say "we find". I suggest to change the sentence to "We find that in our trended regression analysis, the trend in stratospheric water vapor is explained largely by the trend in tropospheric temperature." That has a completely different meaning, in particular, it does not imply that the change in tropospheric temperature is the indisputable underlying reason for the trend in stratospheric humidity in the models. In addition, it does not imply that in reality, a trend in tropospheric temperature will imply a trend in stratospheric humidity. I am aware that you write "in the CCMs" in this sentence, but there is no discussion in the paper that the trend in stratospheric humidity and in tropopause temperature are basically a model phenomenon. The observations of water vapor and temperature do not support this conclusion clearly in the moment. In addition, it is also not a priori clear from a theoretical point of view. See my major comment 3 of the original review again for this.

New comments

- Page 1, line 2: Better: "We analyze the trend and variability [...]". Without interannual variability in at least some of the variables, you would not be able to fit the explanatory time series without ambiguity to the water vapor time series (i.e. if all variables would only contain a trend, the error bars would go to inifinity and the fitted values would be arbitrary).

- Page 1, line 7: "Many of the CCMs [...]". Rephrase or delete: a) This is an unproven statement, in particular since you explicitely refuse to give information about model performance in this paper. b) This is far too generic. Models may perform well in some variables, but no so good in others, and this will also vary from model to model. Be more specific. c) It is unclear what observations you are referring to. d) In particular referring to the trends in water vapor and tropopause temperature: This is a particularly bad example for a "credible" prediction. It is unclear from observations and theory, and is mainly based on the belief that the models do model these particular aspects of the climate system well.

- Page 1, lines 11–12: Please write "increasing it will lead to additional warming of the troposphere" and not "of the climate system". That is too generic. More stratospheric water vapor cools the stratosphere, so this statement is obviously not quite correct.

- Page 1, lines 11–12: "Stratospheric water vapor is a greenhouse gas". Change that to "Water vapor is a greenhouse gas". If a gas is a greenhouse gas or not does not depend on the altitude. It is defined as a gas absorbing in the thermal infrared. And then start a new sentence "Increasing stratospheric water vapor will lead to additional warming of the troposphere, as shown by [citations]"

- Page 2, line 8: Does the correlation of 0.91 refer to the trended or detrended variables? It would be really helpful for your argumentation if the interannual changes would be correlated.

- Page 2, line 19: Better "worked well in reproducing trend and variability"? It is no surprise that it is easy to fit a variable with a trend to another variable with a trend.

- Page 2, line 22: What do you mean by comparison to observations? Do you mean to apply the same regression model to time series of observations and to compare the results?

- Page 2, line 23: Here applies the same comment that I had to Page 7, line 8–9 (original manuscript, specific comment 15). This is solved in the conclusions now, but not here.

- Page 3, line 25: Don't exaggerate. Can we agree on "good job"?

- Page 4, line 1–2. Half of the models shows an explained variance decreased by more than 0.2. That is not "slightly" smaller. Suggestion: "moderately".

- Page 4, line 32 to Page 5, 4: The term "standardized regression coefficient" is a little bit unfortunate. It confused me several times when reading this section, because it suggests something different than actually intended. This is not a regression coefficient, but something like a "variability of the fitted time series" or "standard deviation of the fitted time series" or "square root of the explained variance". Please change.

- Same paragraph: I noticed that in several of the models (e.g. CMAM-CCMI, GEOSCCM, GEOSCCM-CCMI), the variability in the stratospheric water vapor time series mostly comes from the variability in BDC and QBO, with almost no variability in the ΔT time series. That means that the magnitude of the fitted trend in ΔT is very dependent on the magnitude of the *interannual variability* of the QBO/BDC in these models, since the ΔT term, which is almost a pure trend, will fit "what is left from the trend" after matching the interannual variability and trend of the QBO/BDC time series. This may be worth mentioning, since this is a good example of an effect on the ΔT trend which is not "physical", but "numerical".

- Page 7, line 21: "A new way"? See specific comment 6.

Technical comments

- The citations Scinocca et al. (2008a) and Scinocca et al. (2008b) are exactly identical.

- Tables 2, 3, 4: Since $\mathrm{STD}(\beta_{\Delta T}\Delta T) = |\beta_{\Delta T}|\mathrm{STD}(\Delta T)$, I suggest to omit the negative sign in all columns showing standard deviations.