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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Sensitivities of modelled water vapour in the lower stratosphere: temperature uncertainty, effects of horizontal transport and small-scale mixing" *by* Liubov Poshyvailo et al.

Liubov Poshyvailo et al.

l.poshyvailo@fz-juelich.de

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We thank the referee for the review and for the helpful and detailed comments. We give a point-by-point reply below, where the reviewer comments are repeated in italics. The positions of the corrected sentences in the revised version are noted in the brackets, and the revised text is also given in the quotation marks point-by-point below.

General remarks

The paper by L. Poshyvailo et al. tests the importance of three processes (tropopause temperature, horizontal transport and small-scale mixing) for lower stratospheric water vapour concentrations. The paper is overall well written and could be a valuable addition to our understanding and modelling of the tropical upper troposphere and lower stratosphere (UTLS).

Thank you very much for this positive comment. In the revised version all comments have been taken into account, particularly we improved the discussion part, presentation of figures and formulation of the statements throughout the text due to the remarks of the Reviewer #2.

Specific Comments

1. PAGE 1

   11: Are JRA-55 and ERA-Interim sufficient to constrain the space of possibilities given by current reanalysis datasets?

   This is indeed a good remark. In our opinion, the two reanalysis datasets used are not likely sufficient to constrain the space of all possibilities. However, they are two of the three most modern and sophisticated available reanalysis (ERA-Interim, JRA-55, MERRA-2). Due to data storing capacity reasons we could not add a MERRA-2 simulation. But we think the two considered simulations in the paper provide a useful conservative limit of the space of possibilities (which can only be larger if another reanalysis is added). In particular the very new (not entirely available yet) ECMWF reanalysis ERA-5 would be another dataset which could be compared to the results of this paper. We are working on preparing such a simulation, but this is clearly beyond the scope of this paper.

12: cancel second mention of "JRA-55 reanalysis"

Corrected in the revised version.

12-15: since you mention the difference due to uncertainties in tropopause temperatures (0.5 ppmv), could you provide more quantitative statements for the other two processes?

Thank you for this remark. The statements are added in the revised version (p1, L14).

"...Comparison of tropical entry H_2O from the sensitivity $15°$ N/S barrier simulation and the reference case shows differences of up to around 1 ppmv... For the sensitivity simulation with varied mixing strength differences in tropical entry H_2O between the weak and strong mixing cases amount to about 1 ppmv, with small-scale mixing enhancing H_2O in the LS...."

21: reference to Nowack et al. (2015) could be added here. Nowack et al. A large ozone-circulation feedback and its implications for global warming assessments, Nature Climate Change 5, 41-45, 2015

Corrected in the revised version (p2, L2).

23: reference to Maycock et al. was published in Journal of Climate, see typo in reference list. Reference to Nowack et al. (2017) could be added here for another recent example of how UTLS processes/stratospheric water vapor can influence climate variability: Nowack et al. On the role of ozone feedback in the ENSO amplitude response under global warming. Geophys. Res. Lett. 44, 3858-3866, 2017.

Thank you for these suggestions. We added the references in the revised version (p2, L2).

"...Stratospheric water vapour (H_2O) is a crucial factor for global radiation, as it cools the stratosphere and warms the troposphere (e.g., Forster and Shine,

1999, 2002; Shindell, 2001; Nowack et al., 2015). Particularly, changes in H_2O mixing ratios in the upper troposphere and lower stratosphere (UTLS) may have significant effects on climate variability (Solomon et al., 2010; Riese et al., 2012; Maycock et al., 2013; Nowack et al., 2017)..."

2. PAGE 2

12/13: you cite studies about the importance of the Asian Monsoon below. Would add those references here already.

Corrected in the revised version (p2, L18).

"...Horizontal transport between the TTL and middle latitudes is strongly influenced by the Asian monsoon anticyclone and other subtropical circulation systems (e.g., Bannister et al., 2004; James et al., 2008;Wright et al., 2011; Randel and Jensen, 2013)..."

16: to avoid confusion with too many 'lows' I recommend to change to: freeze-dried to stratospheric values, which is self-explanatory.

Thank you for the suggestion. It is corrected in the revised version.

"...Related to the mean upward transport, the TTL includes the region of very low temperatures around the cold-point tropopause, where the moist tropospheric air is freeze-dried to stratospheric values (Brewer, 1949)..."

19: next to Hardiman, 2015, you could add Schoeberl et al. 2014 here: Schoeberl et al. Cloud formation, convection, and stratospheric dehydration, Earth and Space Science 1, 117, 2014.

Thank you for the remark. It is corrected in the revised version (p2, L27).

"...The freezing is sensitive not just to large-scale TTL temperatures, but also to microphysical processes controlling the ice crystal number densities, particle size distribution, and fall speed. Several studies focused on the modelling of the detailed cloud microphysical processes (e.g., Jensen and Pfister, 2004;

Jensen et al., 2005, 2012). Other recent papers have examined the effect of cloud microphysical processes on the humidity of the TTL and stratosphere using cloud models of varying complexity (e.g., Ueyama et al., 2015; Schoeberl et al., 2014)..."

3. PAGE 3

   5: reanalysis,

   Corrected in the revised version.

   6: delete "largely"

   Corrected in the revised version.

   7-11: I agree that the lack of discussion on ozone as a process in the introduction is a shortcoming of the current manuscript. In this paragraph, there would be an opportunity to mention the importance of ozone in modulating stratospheric water vapor concentrations. In the discussion section, ozone could be included as a potential future research interest, because ozone will equally be transported by mixing etc. An ozone-focused analysis is beyond the scope of this study though, so not necessary, but a brief discussion would be useful.

   Thank you for the this suggestion. Although this paper focusses on processes controlling stratospheric H2O, we added a brief discussion on stratospheric ozone (p3, L21).

   "... Furthermore, it has been pointed out that the coupling between ozone, the tropospheric circulation, and climate variability plays an important role in climate change (Nowack et al., 2017). Recent studies have shown that stratospheric ozone changes may cause an increase in global mean surface warming, mostly induced by changes in long-wave radiative feedbacks due to the tropical LS ozone and related stratospheric H_2O and cirrus cloud changes (e.g., Nowack et al., 2015; Dietmuller et al., 2014). Seasonal variations of LS ozone lead to a magnification of the seasonal temperature cycle in the tropics (Fueglistaler et al., 2011).

   Investigation of these additional effects of stratospheric ozone is an important topic of future research focussed on stratospheric H_2O feedback..."

   25: Based on model simulations,

   Corrected in the revised version.

   26: can cause

   Corrected in the revised version.

   "...in the large-scale flow, can cause strong effects on..."

   33: change to: "with respect to two meteorological datasets...", following sentences add references to the recent studies on reanalysis data indicated above.

   Thank you for the remark. We changed this sentence accordingly and added more detailed explanation in the discussion (p4, L6).

   "...In this paper, we investigate uncertainties of modelling H_2O in the LS with respect to two meteorological datasets, ERA-Interim and JRA-55 (e.g., Dee et al., 2011; Kobayashi et al., 2015; Manney et al., 2017; Davis et al., 2017; Manney and Hegglin, 2018), used to drive transport and freeze-drying, horizontal transport between tropics and extratropics, and small-scale mixing in the Chemical Lagrangian Model of the Stratosphere (CLaMS)..."

4. PAGE 4

   27: about 2 million

   Corrected in the revised version.

   31: given the importance of the parameter for the simulations here, a somewhat more detailed description would be helpful rather than just referring to other papers.

   Thank you for this suggestion. But we think, that the short description as presented in the paper provides all the necessary information. This parameter (Lyapunov exponent) controls the strength of the mixing by defining the critical radius for merging or inserting the new air parcels in CLaMS. The further details about the CLaMS mixing parametrization are presented in the cited paper and is accessible for the reader.

5. PAGE 5

   5: the naming convention here implies that this pcold_point is the ambient pressure in the tropopause, but this does not seem to fit your explanation? Either be more specific about the ambient pressure or change the naming of the parameter.

   Thank you for this comment regarding clarity of the description. We changed the wording here (p5, L33).

   "...The lower boundary for H_2O in CLaMS is taken from reanalysis (ERA-Interim or JRA-55) specific humidity below about 500 hPa. If saturation along a CLaMS air parcel trajectory exceeds a critical saturation (100% with respect to ice), then the H_2O amount in excess is instantaneously transformed to the ice phase and partly sediments out. Such simple parametrisation has been adopted in several global Lagrangian studies (e.g., Kremser et al., 2009; Stenke et al., 2008). The saturation mixing ratio is calculated as $\chi_{H_2O} = p_s/p$ for each air parcel trajectory, with the saturation pressure given by $p_s = 10^{-2663.5/T+12.537}$ (Marti and Mauersberger, 1993), where p is the ambient pressure (e.g., Kremser et al., 2009)..."

6. PAGE 6

7. PAGE 7

   3-5: in Table 1, the no mixing experiment is labelled with a mixing parameter of 0, whereas here you say that larger values represent weaker mixing. How does this fit together?

   Thank you for this remark. We agree that the description was misleading. Zero is the value of the critical Lyapunov exponent which should be chosen (technically)

   in CLaMS to turn off the mixing. In theory, this corresponds then to a critical Lyapunov exponent of infinity. We clarified the text by using a Lyapunov exponent of infinity for the no mixing case in the revised manuscript (p7, L15).

   "...Furthermore, we carry out a simulation without small-scale mixing (mixing in CLaMS was switched off), equivalent to a critical Lyapunov exponent of infinity..."

   5/6: it is however unclear how non-linear the effects of mixing scale with the mixing strength. This should at least be pointed out, or alternatively could be tested by running additional simulations with intermediate size parameters.

   Thank you for the suggestions. We added the information about connection between small-scale mixing and vertical diffusivity coefficient in the revised manuscript and added a new Fig. 19 (p.25-27).

   "...In addition, we estimated the vertical diffusivity coefficient for the TTL for the different model simulations. The result suggests a non-linear response of H2O to the small-scale mixing in CLaMS (details are considered in Sect. 4)..."

   "...Although, it is clear qualitatively that a decreasing critical Lyapunov exponent enhances mixing, it is also desirable, at least for comparison purposes, to quantify this effect. Because of the similarity between the mixing procedure in CLaMS and physical diffusion, the vertical mixing intensity can be quantified by computing the induced vertical diffusivity K_z (in m^2/s) (Konopka et al., 2007)... Finally, it should be noted that the mixing in CLaMS induces both vertical and horizontal diffusion. However, given the larger vertical gradients of H_2O compared to horizontal gradients in the UTLS, the impact of small-scale horizontal diffusion is assumed to be much smaller than the impact of vertical diffusion, especially in the tropics..."

8. PAGE 8

   13/14: These values should also be given above, especially in the abstract.

Thank you for the remark. And we added these values to the abstract (p1, L14), (p9, L4).

"...Comparison of tropical entry H_2O from the sensitivity $15°$ N/S barrier simulation and the reference case shows differences of up to around 1 ppmv... For the sensitivity simulation with varied mixing strength differences in tropical entry H_2O between the weak and strong mixing cases amount to about 1 ppmv, with small-scale mixing enhancing H_2O in the LS..."

"...Figure 2 shows the annual cycle of tropical (10°S-10°N) stratospheric entry H_2O at 400 K for all simulations. While a clear annual cycle is evident for all cases, the mixing ratios vary by more than 1 ppmv between the simulations... Suppressing horizontal transport from the subtropics into the tropics (BAR-15) significantly dries the tropical entry H_2O, with difference to the reference of up to around 1 ppmv. For the mixing sensitivity simulations, the largest difference from the reference case occurs for the case without mixing (MIX-no), with the MIX-no simulation dryer by about $\approx$ 0.8 ppmv in September-October..."

Figure 2: x-axis, label DJFM..., or Dec Mar...currently it is unclear if you start in January? December?

Thank you for this comment. We changed Figure 2 accordingly.

15: good opportunity to cite other studies that have looked at this before. Any differences?

Thank you for this comment. We added a reference to the discussion to the text here, and also this topic is shortly discussed in the discussion (p9, L10).

"...The CLaMS simulation driven with JRA-55 shows moister values in the TTL compared to the ERA-Interim simulation, in agreement with the recent findings of Davis et al. (2017) (for details see Sect. 4)..."

9. PAGE 9

Figure 3: Remove space between the second and third month of each seasons label. I think it would also be better to plot the MLS climatology in the first row and differences relative to that in the lower three rows. This would make the structure of the differences more obvious.

Thank you for the remark. Actually, there is no space between the second and third month of each seasonŽs label. Plotting the difference is a good idea, but we prefer to plot the full values to see the proper H2O distribution.

10. PAGE 10

2: no comma after detail

Corrected in the revised version.

"...They will be investigated in more detail in the following..."

10: either...or

Corrected in the revised version.

"...Comparison between the two simulations, driven by either ERA-Interim or JRA-55 (third and fourth row)...

11. PAGE 11

Figure 5: revise titles of the subfigures. Again, I would recommend plotting differences for (b), (c), (e) and (f), especially given that the rainbow color scale skews the perspective on the maps, while the color choice is also not ideal (colorblind readers). I recommend choosing a different color scale.

Thank you for the remark. We tried different colour schemes, but this one highlights the main features of the H2O distribution in the best way. Also we added the values of the temperature to the temperature contours for better understanding of the Figures.

12. PAGE 12

    5: skewed towards...

    Corrected in the revised version.

    "...PDF appears more strongly skewed towards low mixing ratios...

    Fig. 6(a)-(c) should be reordered to match the order of the discussion in the text, or at least NH and SH swapped.

    Thank you for the comment. Here we changed the order of the description, from Fig.6a to Fig.6c (p12, L13 - p14, L22), and we added an explanation regarding Fig.6b.

13. PAGE 13

    Figure 7: again I would replace the color scale by something gradual, e.g. cold-to-warm color scale. Currently, the differences are really hard to see. Plotting differences in (b) and (c) would help, too. Maybe use contour lines for REF in order to use a single color scale?

    Thank you for pointing this out. We agree, that the Figure 7 was not clear for representing the differences between the sensitivity studies. We changed the Fig.7, where we also show the differences between REF and BAR-35 CLaMS simulations. The results agrees well with previous studying of Garny et. al. (2014). We also added more explanation regarding this Figure (p15, L1).

    "...The pure transport effects of horizontal exchange between tropics and mid-latitudes are evident from mean age of air (AoA), the mean transit time for air through the stratosphere for the different model experiments with horizontal transport barriers. Figure 7 shows CLaMS calculations of the AoA for the reference case (Fig. 7a), simulation with transport barriers in the subtropics at 35° N/S (Fig. 7b) and the absolute difference between them (Fig. 7c). These horizontal transport barriers at 35° N/ effectively isolate the tropical pipe from the in-mixing

    of older stratospheric air from mid-latitudes... A similar result has been recently found by Garny et al. (2014). Furthermore, Garny et al. (2014) presents a nice explanation of the recirculation process, describing recirculation as a process when an air parcel enters the tropical stratosphere and travels along the residual circulation to the extratropics, where it can be mixed back into the tropics, and thus recirculates along the residual circulation again. In this way, the age of air of the parcels increases steadily while performing multiple circuits through the stratosphere..."

    7: typo reference

    Thank you for this remark. It is corrected in the revised version.

    14: Why is there a consequent increase in age of air in the global stratosphere if the age is increased in the extratropics?

    Thank you for pointing this out. We removed the discussion about the "global stratosphere", as the AoA indeed is increased in the lower stratospheric extratropics.

    "...therefore significantly increases AoA in the extratropical stratosphere..."

14. PAGE 14

    Figure 8: again difference plots relative to REF would be better, same changes as above concerning the color scale.

    Thank you for this remark. We added an extra plot with the largest differences between the CLaMS sensitivities and the reference simulations (Fig. 9). BAR-0 and BAR-15S showed negligible difference with REF simulation, so we do not show them here.

    10: simulations typo

    Corrected in the revised version.

15. PAGE 15
16. PAGE 16
17. PAGE 17
18. PAGE 18

    9 typo

    Corrected in the revised version.

    Figure 14: Clarify that these are percentage differences in the figure caption

    Thank you for this remark. It is corrected in the revised version (now it is Fig. 15).

19. PAGE 19

    Figure 15: again, I would suggest a change of color scale, the plots are very hard to read. In addition, change (b,c,d,f,g,h) to difference plots relative to MIX-no. Too much information for a single plot due to the two types of contour lines.

    Thank you for the remark. Plotting the differences is a good idea. However, we prefer to show the full values to present the proper H2O distribution. We also changed here the colour schemes to highlight the main features of H2O in the best way. To have better comparability between the studies in our paper, we plot this Figure now at 380K, not at 400K as it was before (it is Fig.16 now).

20. PAGE 20

    10: reliably well, or maybe fairly well or just wellÂĂÂŹ?

    Thank you for this remark. It is corrected in the revised version.

21. PAGE 21

    Figure 16: revise titles

    We do not understand what should be changed in the titles. We think the titles are clear and include all necessary information.

22. PAGE 22

    Figure 17 (and the corresponding text): I agree with Reviewer 1 that this part of the reanalysis discussion could be kept much shorter. Issues around stratospheric water vapor are known for reanalysis datasets as the authors point themselves out in the text. Discussing this topic again here in so much detail and with an extra figure distracts from the key messages of the paper, i.e. how the three sub-processes influence stratospheric water vapor concentrations.

    Thank you for the comment. We shortened a bit the description of Fig.17 (now it is Fig. 18). In our opinion the discussion is resumed enough now. Additionally, we added a discussion about recent papers on the reanalysis datasets.

    "...Recent studies have emphasised the overall qualitatively very good agreement between the large-scale climatological features in the UTLS in different reanalysis datasets, while important quantitative differences remain (e.g., Manney et al., 2017). This qualitative agreement among the reanalysis in many regions of the UTLS and different seasons points to the robustness of the representation of related transport and chemistry in the reanalysis datasets (Manney and Hegglin, 2018). As the stratospheric H_2O in the reanalysis is not assimilated directly, the treatment of H_2O in the particular reanalysis product plays an important role. For instance, JRA-55 does not contain a parametrisation of methane oxidation in contrary to ERA-Interim (Davis et al., 2017). Davis et al. (2017) further showed that the JRA-55 mean H_2O values are much too large at 100 hPa. Our results of excessively high H_2O values in the JRA-55 data product in the extratropical LS agree well with the findings of Davis et al. (2017). Furthermore, Davis et al. (2017) points out that there is still a lack of assimilated observations and that significant uncertainties remain in the representation of the relevant physical processes in the reanalysis..."

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