Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2017-1072-RC2, 2018

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.

Anonymous Referee #2

Received and published: 14 January 2018

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). However, beforehand I recommend several minor revisions.

First of all, I disagree with Reviewer 1 that the discussion on the reanalysis datasets/tropopause temperature should necessarily be entirely removed. While not being a particularly novel part of the paper, it consolidates some previous results and,

in my opinion, helps to put the significance of the other results presented here into context. However, it would be good to add some recent citations for studies which compared reanalysis datasets in the UTLS region, for example:

Manney et al. Reanalysis comparison of upper tropospheric-lower stratospheric jets and multiple tropopauses. Atmos. Chem. Phys., 17, 11541–11566, 2017.

Davis et al. Assessment of upper tropospheric and stratospheric water vapor and ozone in reanalyses as part of S-RIP, Atmos. Chem. Phys., 17, 12743-12778, 2017. (already cited very late in the paper in section 4, but should be mentioned earlier)

Manney & Hegglin. Seasonal and regional variations of long-term changes in upper-tropospheric jets from reanalysis. J. Clim, 31, 423-448, 2018.

A brief discussion on the shortcomings of reanalysis datasets in the tropical UTLS should also be added to the text and could be merged with a shortened discussion (see below) on these issues in section 4. In addition, by referring to these references could help to write some sections (e.g. section 3.1) more concisely.

Specific comments:

page 1:

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

l. 12: cancel second mention of 'JRA-55 reanalysis'

l. 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?

l. 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.

l. 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.

page 2:

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

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

l. 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, 1–17, 2014.

page 3:

l. 5: reanalysis,

l. 6: delete 'largely'

l. 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.

l. 25: Based on model simulations,

l. 26: can cause

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

page 4:

l. 27: about 2 million

l. 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.

page 5:

l. 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.

page 7:

l. 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?

l. 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.

page 8:

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

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

uary? December?

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

page 9:

Figure 3: Remove space between the second and third month of each season's 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.

page 10:

l. 2: no comma after detail

l. 10: either...or

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.

page 12:

l. 5: skewed towards...

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

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?

l. 7: typo reference

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

page 14:

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

l. 10: simulations typo

page 18:

l. 9 typo

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

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.

page 20:

l. 10: reliably well, or maybe fairly well or just 'well'?

page 21:

Figure 16: revise titles

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.

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2017-1072, 2017.