

Comments on the revised version of the ACPD manuscript entitled “Variability of water vapour in the Arctic stratosphere” by Laura Thölix, Leif Backman, Rigel Kivi and Alexey Karpechko.

The revised is clearly improved compared to the submitted version. The main problems I have concern the regression analysis, I think there is room for clarifications and improvements. Please find my comments below:

Comments:

- ▶ page 2, line 39: “... the abundance of other PSCs ...” may be changed to “... the abundance of other PSC types ...” to make a distinction to ice PSCs more directly.
- ▶ page 2 and 3, line 58 to 60: The are some problems with that sentence here. “caused” should read “causing a” and “... and to an increase in ...” may read “... and an increase of ...”.
- ▶ page 4, lines 99 to 107: It is probably fair to separate SMR and MIPAS from the other instruments that employ/employed the occultation technique, which limits the temporal and spatial coverage in general. SCIAMACHY is kind of an hybrid that provides some limited coverage of the Arctic in its solar occultation mode. Its limb mode however is in the same class as MLS, MIPAS and SMR providing global coverage on a daily basis.
- ▶ page 5, line 138 and 139: Please add “lower” before “stratosphere” in the first line. In the upper stratosphere the trends reported by Hegglin et al. (2014) are positive. In the second the phrase “not good” is probably a bit to strong. “not optimal” might be a better choice.
- ▶ page 6, line 170: Please remove the brackets around the Hanson and Mauersberger reference.
- ▶ page 7, line 211: There is something strange with “First larger set ...” sentence.
- ▶ page 7, lines 225 to 227: Why are the CFH measurements from the first LAPBIAT not used, only the NOAA FPH?
- ▶ page 8, line 268: “... is about 0.3 hPa moister ...”. I guess ppmv is meant here.
- ▶ page 8, lines 268 and 269: The sentence “Too moist air ...” does not really fit here as the previous sentence focused on the upper part of the stratosphere where no PSCs exist.

- ▶ page 8, line 269 to 273: The information that ERA-interim shows lower water vapour volume mixing ratios compared to MLS in the upper stratosphere is doubled. Please remember that ERA-interim is setup to relax to 6 ppmv at the stratopause, which largely explains the discrepancies.
- ▶ page 10, line 321: I think the word “could” is not appropriate here. At least I assumed you did it that way.
- ▶ page 10, line 328 to 330: I am not sure if that sentence is really needed. Information in that direction has been conveyed earlier on one side and on the other side it seems obvious.
- ▶ page 11, line 365 to 367: I wonder if it is a better choice to use the reference period from 2004 to 2014 even for the de-seasonalisation of the simulations. There is a clear offset between MLS and the simulations especially lower down. This may disappear and result in nicer comparisons. The temporal variations seems to be consistent between the individual data sets, though.
- ▶ page 12, line 385: This fits also your artificial tracer results, which attribute most of the water vapour changes to dynamical processes.
- ▶ page 12, line 397 to 399: To this date it is not hundred percent clear what this 500 hPa tropical temperature term really considers. Dessler et al. (2013) used it mainly to derive their radiative feedback. There are certainly some contributions of ENSO and climate change in this terms, but it is not as obvious like for the QBO or a solar proxy. The cold point temperature is likely more covered by the BDC proxy than the 500 hPa temperature overall. Also, the Dessler approach is optimised for the tropics. So, there seems to be some plausible explanations for your problems.
- ▶ page 12, line 409 to 418: Do the correlation coefficients consider the time lags that have been mentioned earlier? Then I miss the regression coefficients. I would prefer those more than correlations for the discussion of the importance of the individual processes contributing to the water vapour distribution. It would be also interesting to see the regression fits and/or the residuals to identify missing processes.
- ▶ page 13, line 424: I think “Fig. 1” should be “Fig. 3”.
- ▶ page 14, line 472: “Figure 7 shows ...” should reference to the right panel of Fig. 7.
- ▶ page 14, line 484: You mean the 95% significance level here?

- ▶ page 15, line 495 to 500: I think this part needs some rewriting as it is raising more questions than it answers. Of course if there is a major warming then the vortex is not destroyed directly. For one cells can remain and also the definition of the warming central date depends on the altitude and place you look at. Also the vortex reformation after the warming comes into play here. Maybe you can spare many parts of this and simply say you focus on the coldest week while after that temperatures were too high.
- ▶ page 15, line 508 to 511: Is Fig. 8 (the old Fig.7) now considering the whole Arctic instead of Sodankylä?
- ▶ page 24, table 1: The caption sounds strange.
- ▶ page 24, table 2: You may add that the unit is 1e6 km².
- ▶ page 32, figure 9: There is a blank missing between “occurrence” and “from” in the second line of the caption.

References:

- ▶ Dessler, A., et al., “Stratospheric water vapor feedback”, *Proceedings of the National Academy of Science*, 110, 18087 - 18091, doi:10.1073/pnas.1310344110, 2013.
- ▶ Hegglin, M. I., et al., “Vertical structure of stratospheric water vapour trends derived from merged satellite data”, *Nature Geoscience*, 7, 768 - 776, doi:10.1038/ngeo2236, 2014.