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Interactive comment on “Variability of water vapour in the Arctic stratosphere” by L. Thölix et al.

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This is a very interesting paper that is well written with a clear story line. I have a few minor comments, and I hope those will help polishing the paper toward final publication.

Comments on paper ACPD-15-22013-2015 entitled “Variability of water vapour in the Arctic stratosphere”

For years the topic of stratospheric water vapor in high-latitudes was indeed less reported. That’s why I was immediately intrigued by this interesting topic (also because I have seen similar results from our trajectory model). Unlike other simulations that mainly based on cold-point temperature regulations (e.g., Fueglistaler et al., [2005]; Schoeberl et al., [2002]; Wang et al., [2015]), the FinRose model has interactive chem-

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istry included over the polar region, which makes this research more valuable to the UTLS and stratosphere community. However, I have a few comments to the authors and I hope those can help polishing the entire story.

1. There should be more details to the model description. For example, besides implicit chemistry and circulation from ERA interim, is there any mixing considered? How about wave activity? Also, I am confused at the relations between “tropospheric concentrations” and the “boundary conditions” mean. What does “boundary conditions” mean in this paper?

2. About the description of (using) ERA interim water vapor field. This mainly occurs on P22021/L8-10, which is quite misleading. The authors mentioned about the improvement on ERA interim H₂O after adopting the new linear scheme for stratospheric methane, but this was only limited to the experiments conducted on year 2000 (Monge-Sanz et al., [2013]). Therefore, the boundary condition water vapor (if I understand it correct) used in FinROSE model is still the official H₂O field available on ERA interim. Be noted that ERA interim doesn't assimilate H₂O at the altitude range covered in this paper, so humidity field might primarily reflect the model simulation, and therefore comparison to observations is itself less meaningful. I mean, how much credibility should we lay on its range?

3. When comparing to MLS observations, did the authors apply averaging kernels? This might not be important since the focus is only on polar region that has lower reliance on H₂O from below and upper levels (this could also be told from the figure below that applied AKs to the Fig. 4 in this paper), but it is worth to do a sanity check in order to do an apple-to-apple comparison.

4. When comparing to MLS observations, please also pay attention to the cold-biases in ERA interim temperatures (Fueglistaler et al., [2011]), since those would affect the trajectory results tremendously (Schoeberl et al., [2012]).

5. I hope the authors could also double check on Fig. 2b green line (ERA interim –

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MLS) around the tropopause. An eye-ball check, and also my own calculations a few years ago tells me that the difference should be at least around $\sim 10\text{-}14\%$ at 100-hPa.

6. Fig. 4 panels b–e show the anomalous H₂O and the components due to transport and chemistry, which is basically what we saw in previous figure (Fig. 3) and the Fig. 6 in Schoeberl et al., [2012]. Here, in order to support the analysis in P22024-22025, it is better to add MLS to those panels despite different time range. What the authors could do is to subtract the cycle covering the MLS period, and the results would be essentially the same but it adds more credibility to the model's performance. For reference, below is H₂O from our trajectory model, driven by reanalysis and controlled by purely temperatures. Note that this figure demonstrates results from using GPS RO temperatures; but results from using reanalyses temperatures would be basically the same since reanalyses capture the interannual variability of cold-point tropopause over the tropics very well and therefore the predictions are similar that essentially match with MLS observations (refer Fig. 8 Wang et al., [2015] for details). On the other hand, this, from another perspective, supports many arguments in this paper about the origin of stratospheric air.

Figure. Arctic water vapor predicted from trajectories driven by MERRA (blue) and ERA interim (orange) circulation and GPS RO temperatures (refer Wang et al., 2015), compared to MLS observations. All trajectory results have been weighted by MLS averaging kernels.

7. Discussions about the contributions to H₂O from chemistry and transport (section 4) could be more easily understood by the H₂O lifetimes (refer chap. 5 in the classic book by Brasseur and Solomon, [1986]).

8. Discussion in P22025/L17-23 is not exactly accurate. The stratospheric water vapor is more dominated by the Brewer-Dobson circulation instead of QBO. Please refer the multi-variate regression coefficients and the component time series in Dessler et al., [2013, 2014]). That's why the 2000-drop is believed to be related to the BDC (e.g.,

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Randel et al., [2006]).

9. Some comments on figs. 2, 4, and 6. This is a personal preference: I always add legends to the figures, so that when someone else uses those figures in their presentations they don't need to add legends manually. On the other hand, with legends the vast information is easy to be spotted on.

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Please also note the supplement to this comment:

<http://www.atmos-chem-phys-discuss.net/15/C7649/2015/acpd-15-C7649-2015-supplement.pdf>

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