

Interactive comment on “Multi-timescale variations of modelled stratospheric water vapor derived from three modern reanalysis products” by Mengchu Tao et al.

Anonymous Referee #2

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Review of Tao et al 2019 “Multi-timescale variations of modelled stratospheric water vapor derived from three modern reanalysis products”

This paper evaluates the simulated stratospheric water vapor in three modern reanalyses in comparison with observations to better understand the inter-reanalysis differences. Overall, this paper is well written and well-supported in its arguments, and will be a nice contribution to the literature particularly for those wishing to better understand how the choice of input reanalysis for models (such as CTMs) will impact model results.

I have a few minor suggestions that should be considered:

Title/abstract: I believe through most of the paper the British English spelling of vapor (i.e., vapour) is used, but the American English spelling is used in the title and abstract. A consistent spelling should be used throughout the paper.

Line 17: There have been other attempts at merging observational records of WV. In particular, Froidevaux et al 2015.

Froidevaux, L., Anderson, J., Wang, H. J., Fuller, R. A., Schwartz, M. J., Santee, M. L., Livesey, N. J., Pumphrey, H. C., Bernath, P. F., Russell, J. M., III and McCormick, M. P.: Global OZone Chemistry And Related trace gas Data records for the Stratosphere (GOZCARDS): methodology and sample results with a focus on HCl, H₂O, and O₃, Atmospheric Chemistry and Physics, 15(18), 10471–10507, doi:10.5194/acp-15-10471-2015, 2015.

Lines 30-31: What versions of SWOOSH and MLS are you using?

Paragraph ending line 8, page 2: Substantial uncertainties also include those from artificial jumps introduced by changes in the observing system used as input to reanalyses. These jumps and their potential to impact temperatures that affect WV should be mentioned.

Page 5, line 28: Why does this analysis end in 2013? All of the reanalyses and obs extend to present, and cutting out 5 of the 15 years of the MLS record seems imprudent.

Section 2.3, page 6: This is more of a general comment, but this paper makes no mention of previous efforts at extracting SWV variability in obs and reanalysis-driven simulations. In particular, several papers by Dessler et al. have used a similar regression analysis. I believe the results of this paper are broadly consistent with the previous analyses, but some discussion of similarities and differences is warranted.

Dessler, A. E., Schoeberl, M. R., Wang, T., Davis, S. M. and Rosenlof, K. H.: Stratospheric water vapor feedback, PNAS, doi:10.1073/pnas.1310344110, 2013.

Dessler, A. E., Schoeberl, M. R., Wang, T., Davis, S. M., Rosenlof, K. H. and Vernier,

J. P.: Variations of stratospheric water vapor over the past three decades, *J Geophys Res-Atmos*, 119(22), 12,588–12,598, doi:10.1002/2014JD021712, 2014.

Page 6, line 32: “signals such as ... QBO have zero long-term trend.” This is not necessarily true over short periods of time where endpoint effects could come into play (e.g., if the record started in a westerly phase and ended in an easterly phase). This is not an issue for AC as long as full years of data are used (given that sine/cosine pairs are periodic by construction).

Page 7, line 7: “. . . regression model explains over 90% of the variations... ” This is not correct. Fig 1 top panel shows an $R=0.91$, which means the percent variance explained is $0.91^2 = .83 \rightarrow 83\%$

Page 8, line 8: “Appendix” -> “Appendix”

Page 8, line 25: Remove “a”

Page 10, line 5: You should cite Randel and Jensen here

Randel, W. J. and Jensen, E. J.: Physical processes in the tropical tropopause layer and their roles in a changing climate, *Nature Publishing Group*, 6(3), 169–176, doi:10.1038/ngeo1733, 2013.

Page 11, figure 4: Are the time periods used the same here between the reanalyses and obs?

Page 11, line 14: Should read “cold point that controls”

Page 13, line 10: I’m confused as to why the H₂O_CH₄ variations are opposite in phase relative to seasonal variations in H₂O_e. I thought that the peak H₂O_e (i.e., boreal summer) coincided with peak in-mixing, and hence anomalously high values of H₂O_CH₄ from old midlatitud air being mixed into the tropics. Please clarify.

Page 14, figure 6: What is month 0? Is it December? Or January?

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Page 19, sentence beginning line 4: I think the consistency is more likely due to the greatly improved quality and quantity of SWV data from MLS, rather than a sudden improvement in the quality of the reanalyses. This possibility should at least be recognized in this paragraph.

Page 20, line 11 and line 18: Dessler et al. 2014 found similar results for volcano and ENSO impacts on SWV.

Page 20, line 31: “intercomparison” -> “intercompare”

Page 21, Table 1: Some measure of statistical significance would be useful here (e.g., put significant trends in bold)

Page 22, Figure 12, top row: This is the standard deviation, not the variance (i.e., the square of the standard deviation). The caption and text mistakenly refer to this as the variance.

Page 23, lines 12-14: This is hard to see given the coarse color scale in Figure 12.

Page 24, figure 13: It is hard to see the different symbols in this figure. This could be fixed by using a small horizontal offset of the symbols, or using symbols that can be more easily overlaid on one another.

Page 25, line 6: I don't think the phrase “in combination with the analysis increment” is needed in this sentence. It makes the sentence confusing.

Page 25, line 9: What is the “assimilation increment”? Maybe the authors mean the “assimilation tendency”?

Page 26, line 19: “produces more” -> “produces a more”

Page 26, line 28: “indicates” -> “indicate”

Page 27, line 8: text spacing messed up at the end of this line.

Page 28, line 12: remove “the”

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