Reply to Referee #2

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.

We are grateful for reviewer2's suggestions, which are very helpful to improve the paper. We mark the changes in the manuscript in blue color. The responses or clarifications are specified following each suggestion below.

I have a few minor suggestions that should be considered:

1. 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.

   All has been revised to 'water vapor' consistently.

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

   Yes, it is added as citation.

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

   We use SWOOSH version2.5 and MLS version 4.2, which has been included in corresponding places in the manuscript.

4. 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.

   This information is added in the Page2 line 9-10.

5. 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.

    The period of 1979.01-2013.12 is noted as the "S-RIP base period", which makes the inter-comparison easier among various studies.

6. 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.

    Yes, we agree that the studies by Dessler et al. should be mentioned, especially in section 6.

    The changes for this point is added at Page19 line 21-25.

7. 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).

    Agreed. More precisely, the QBO might contribute to trend even when the full cycle is taken. So information is added as 'The quasiperiodic signal like QBO does not show long-term trend in the H2O entry over the considered period' at Page 7 line 10-11.

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8. 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 -> 83%

    Thanks for pointing it out, it is revised.

9. Page 8, line 8: "Apprendix" -> "Appendix"

Corrected.

10. Page 8, line 25: Remove "a"

Corrected.

11. Page 10, line 5: You should cite Randel and Jensen (2013) here

Agreed, the citation has been added.

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

No, we used the full simulation period (1980.01-2013.12) for CLaMS runs, which is longer than the SWOOSH period. We clarify this in the caption of Figure 4. The main objective of this figure is to compare the climatological AC among reanalysis while the validation with SWOOSH data is only supporting information. Thus, we did use the same period. For your information, the conclusion stays the same when we use exactly the same 'SWOOSH period' for model results, with only some quantity changes. Please check the plot below when using 'SWOOSH period' for all.
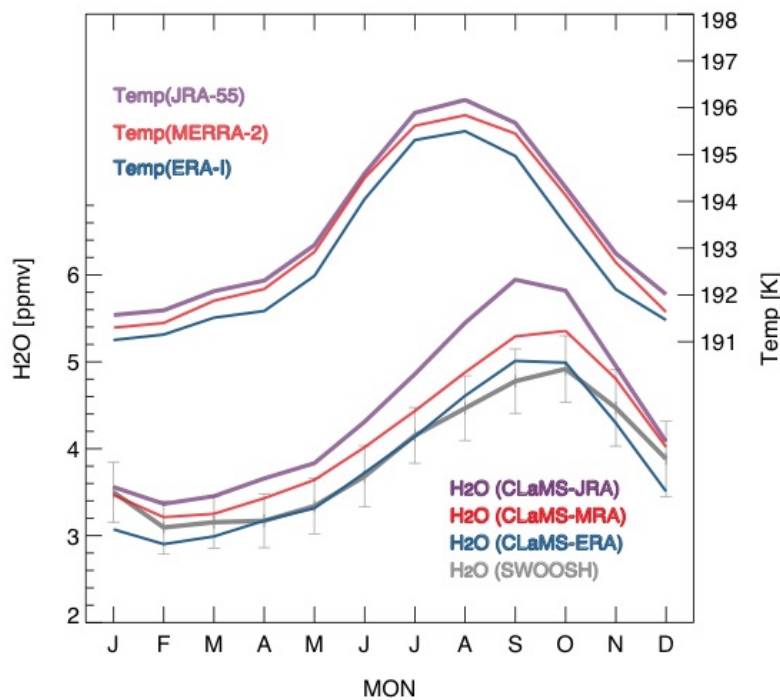
13. Page 11, line 14: Should read "cold point that controls"

Corrected.

14. Page 13, line 10: I'm confused as to why the H2O_CH4 variations are opposite in phase relative to seasonal variations in H2Oe. I thought that the peak H2Oe (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.

It is true that the $H2O_{CH4}$ peak coincides with the peak of H2Oe. But these two signals occur at different altitudes. H2Oe seasonal cycle starts at the tropical tropopause while $H2O_{CH4}$ seasonal cycle due to in-mixing of mid-latitude air occurs over the full layer from 380-450 K (in CLaMS-MRA with slow circulation in the lower stratosphere). Therefore, they have a shift of phases when these two seasonal signals (or 'tape recorder') propagates to the same altitude.

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

Month 0 is January and month 12 is Jan. again. We didn't notice it is a bit odd to label x-axis in this way. The labels have been changed explicitly to the month names.

16. 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.

Agreed. There are two aspects: 1) the improvement of SWOOSH data due to the kick-in of MLS; 2) the improvement in the quality of the reanalyses. The sentences have been reformulated.

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

Added.

18. Page 20, line 31: "intercomparison" -> "intercompare"

Corrected.

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

    The significant trends in the table have been marked in bold.

20. 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.

    The caption of figure and the relevant text are revised.

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

    Yes, the sentence is too strong. Actually, the following arguments are just intended to say the variance fractions are 'qualitatively' consistent. Therefore, we revised this sentence accordingly.

22. 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.

    The smaller symbols in each panel are changed to another shape, which makes them easier to be seen. However, we insist to plot the symbols together. In this way, it emphasizes virtually that the same bins are used for each reanalysis. Meanwhile, since the symbols are repeated in each panel, the offset of the symbols seems not so necessary.

23. 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.

    Changed as the suggestion.

24. Page 25, line 9: What is the "assimilation increment"? Maybe the authors mean the "assimilation tendency"?

    Yes, we meant "analysis tendency". Changed as suggestion.

25. Page 26, line 19: "produces more" -> "produces a more"

    Corrected.

26. Page 26, line 28: "indicates" -> "indicate"

    Corrected.

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

    Corrected.

28. Page 28, line 12: remove "the"

    Corrected.