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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Assessment of upper tropospheric and stratospheric water vapour and ozone in reanalyses as part of S-RIP" *by* Sean M. Davis et al.

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Our response to the review is in line below, with responses given in bold:

This kind of paper is hard to review. It provides a summary of ozone and water vapour information in current reanalyses data sets, with an emphasis on the stratosphere. I guess the main conclusion of the paper was known before it was written: use the ozone and water vapour data with care and do not use for trend studies. However, it is nice to have some of the issues illustrated with figures in a comparative way. Therefore, there is no reason why the paper should not be published, however some explanations might require some clarifications. I will detail my questions below:

P3, l22: I do not know what this statement means. All systems try to model the microphysics of water as good as they can, jet the results differ, because small differences in the treatment of water can have large effects?

This sentence was confusing and has been re-worded. The point of the sentence was to state that SWV is not well constrained by observations because no observations of SWV are assimilated, and that hence SWV is highly controlled by the differing physical representations of SWV. It now reads "Because stratospheric water vapour data are not directly assimilated, the concentration of water vapour in the stratosphere is highly variable amongst the reanalyses and is strongly affected by their representation of processes controlling it."

P2, l30: This sentence is confusing. It tries to make two points in one sentence: Heating rate calculations and photochemistry. Which ozone is used when and where?

(We assume the reviewer means P3 here, not P2) The point of this sentence was to note the large variety of ways ozone is represented in reanalyses. The information pertaining to the reviewers question is all summarized in Table 1. We realized that Table 1 was not referenced until later in the manuscript, so we've added reference to Table 1 here, and have also changed the wording to make our point more clear.

P6, l3: This relates to the comment regarding P2, l30. I guess a clear discussion in the beginning would be fine. Alternatively, a corresponding sentence for each system. Which ozone and water vapour is used in the radiation (heating rates) and what is done for the chemistry (actinic fluxes, if required). P7, l9: See above.

As noted above, this information is contained in Table 1. It is also discussed separately for each reanalysis.

P7, l34: was should read has.

Fixed

P9, l6: I am not sure why the stratospheric temperature bias changes the humidity product. Ice clouds in Antarctica.

In general, temperatures in the TTL affect how much water vapor enters the stratosphere. We have changed the wording here to make this point clear.

P11, l2: This sentence is not very clear. Which mean? What tendency? (For people in the know it will be clear, but . . .)

We agree with the reviewer that this sentence wasn't very clear. We've changed the sentence to be clearer about the potential impact of changes made in MERRA-2 on water vapor. The sentence now reads: "The main innovation in MERRA-2 that could impact water vapor is the introduction of additional global constraints that ensure continuity of water mass in the atmosphere (Takacs et al., 2016)."

P11, l28: If I understand correctly, profiles (ozone and water vapour) are processed as described a few lines above on pressure levels and a common grid. However, TCO is calculated from model level data. Why not use the "ready made" products? Do some systems not provide their columns? How do you deal with orography?

We misspoke in this paragraph. We computed the monthly means ourselves "from the 6-hourly TCO fields", not "from the 6 hourly model level data" as stated in the paper. We've re-worded this paragraph to make this clear. Orography is taken into account in using model level data because the lowest level is the surface, regardless of orographic height. As long as pressure is properly registered on the model levels (which it is in our JRA-25 calculations), using model level data is the most accurate way to compute total column ozone.

P12/13: I appreciate that the authors would like to compare the reanalyses systems with another data source. However the data used is neither independent nor in a fundamental form. Instead, merged data sets are utilised. Nothing wrong with this, but

presumably other equally valid products exist and I am not sure why the data sets mentioned have been chosen . . . (I am also not very happy with the use of multi-instrument means without an appreciate of the spread, when comparing to the reanalyses systems.)

We appreciate the reviewers concern that some of the comparisons in this paper are to assimilated (non-independent) observations. However, we note that this is only true for ozone, as all of our comparisons to water vapor data are to independent measurements. Also, for ozone we have carefully noted instances where the comparisons are not to independent data sources. We chose SPARC DI and SWOOSH data because we are familiar with these data sets. Other merged data sets (e.g., GOZCARDS) could be used, but based on outside work assessing the merged data sets, we have no reason to believe that using a different data set would alter the conclusions here (see, e.g., Tummon et al., ACP, 2015; Hubert et al., AMT, 2016; Harris et al., ACP, 2015). Regarding the spread around the multi instrument mean, the spread is shown in all of the line plots containing the MIM (Figs. 5,6,13,14).

Instead of focusing on the text, I will now briefly comment on a smal number of figures (my assumption being, that readers will be most interested in the graphical presentation of system differences). Given the large range in water vapour products and the small number of systems that provide it, I will not comment on the water vapour related figures (the figures are a health warning in themselves): Figure 3 and 4: I struggle to combine the information in both figures. For example JRA- 25: In Figure 3 JRA-25 has a low bias with respect to SBUV everywhere. In Figure 4 JRA-25 has a positive bias from around 100 hPa to just below 10 hPa. Assuming that the largest column contribution stems from this region, I do not understand the consistency of the results. (Maybe I have over-read the explanation in the text)

The reviewer has uncovered what turned out to be a major bug in the processing of the JRA-25 data. As noted in the manuscript, the JRA-25 total column ozone

data were processed directly from the 6-hourly model level data set. The vertically resolved ozone came ultimately from the pressure level data supplied by JMA and NCAR via CREATE-IP. It turns out that the pressure level data used the incorrect hybrid level model coefficients when converting from model levels to pressure levels, resulting in a downward "shift" of the entire ozone profile, as seen in the old version of Figure 4. This was caused by the use of the model "interface" hybrid coefficients, rather than the correct model "full level" coefficients when converting from the model levels to pressure levels.

We have gone back to the original model level data to properly create the monthly mean pressure level data set, and have updated all of the JRA-25 plots in this paper that contain vertically resolved data. The affected figures that have been updated are Figs. 4-6, 8, and 11 (the plots of TCO are correct). We have also corrected the relevant discussion in the manuscript and added text explaining our data processing for JRA-25 ozone.

Figure 7: I find this figure hard to understand. Presumably, by using equivalent latitudes, the differences in more than one variable are highlighted. PV will have been derived from very different dynamical cores and afterwards ozone has been mapped to it. Therefore, differences will arise from more than one change in the assimilation system and how PV has been derived (e.g. treatment of temperature, dynamical variables and ozone itself, etc.). Therefore, I am not entirely sure what the message on a global scale is . . . apart from they all look different.

The reviewer is correct that the plots showing ozone on isentropic-equivalent latitude coordinates could be affected by differences in both the dynamical and chemical (i.e., ozone) representation among the reanalyses. However, we believe Figure 7 is valuable because it illustrates the sort of analysis that people do in "research" studies, so we should know how derived quantities (such as ozone on EqL-theta coordinates) compare among the reanalyses. Also, the figure quite clearly illustrates some of the problems with ozone in the polar regions. The sim-

ilarity of the PV contours among the different reanalyses in this region suggests that the problem lies more in the representation of ozone than in the representation of vortex dynamics. We have added text in Section 4.5 to clarify these points and the importance of this figure.

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