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

Anonymous Referee #1

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This paper uses a single model (CLaMS) to calculate stratospheric water vapour values on the basis of a simple dehydration scheme combined with a simple methane oxidation scheme. The model is driven by reanalysis winds and temperatures (winds to determine transport and temperature to specify dehydration). Three different reanalysis datasets (ERA-I, JRA-55 and MERRA-2) are used to drive the model and the objective of the paper to compare predictions of stratospheric water vapour concentrations that result from the use of each of the three datasets.

I think that this paper potentially makes a useful contribution. Reanalysis datasets are in wide use for many different types of calculations and it is important to have on record

different measures of the differences between the datasets in common use. There have been previous studies of the differences in transport characteristics between these datasets, but the calculation of stratospheric water vapour considered here on the one hand requires a particular combination of transport and temperature information and on the other is of significant general interest, because of the radiative and chemical importance of stratospheric water vapour and of the continuing challenges in measuring its concentrations and in establishing a long-term observational record.

However I do consider that the paper could be improved in various ways to make it as valuable as possible to researchers working in this general subject area.

p2 l21: 'water vapour values entering the stratosphere are determined by ... in the upper troposphere' - seems odd statement - previous papers (e.g. Liu et al 2010, Fig. 12) have suggested LDPs are distributed over a layer centred on about 90hPa. 'in the TTL' would be a more usual description.

p2 l22: 'Lagrangian models provide more accurate records of the temperature histories of air parcels compared to Eulerian models' – statement is true by definition (Eulerian models don't provide such records) or could be misread as 'provide a more accurate representation of transport' which is questionable - there are advantages and disadvantages to the two different formulations.

p3 l13-14: '30% too fast', '30% too slow' are based on the tape recorder considered over some a particular range of heights - give range explicitly.

p4 l8: You don't give sufficient information here to specify the dehydration scheme – e.g. the fall speed has to be combined with a length scale (specified in Ploeger et al 2013 as 300m). A first point is that if I look at the references you give – e.g. Poshyvailo et al (2018) I don't see sufficient information to be able to reproduce the scheme (e.g. that paper doesn't seem to say what the assumed fall speed it). My suggestion is that you provide all the necessary information together in an Appendix. A second point is that you mention various details such as sedimentation rate but then you fail to mention the length scale - which is surely chosen on a completely ad hoc basis. So the naive reader might will infer that dehydration scheme is based on a precise physical model, whereas in fact it requires choice of length scale - which is surely ad hoc. (Ploeger et al 2013 note that it is comparable to the vertical resolution of the model - but that is hardly a physical justification.) So more clarity is needed - both for reproducibility and for an honest description of what is being done.

p4 l12: The information that greater intensity of small-scale mixing leads to moistening by $\sim$0.5ppmv and amplification of the annual cycle by $\sim$0.2 ppmv can't really be interpreted sensibly without more information. For example you could say that the range of mixing strength being considered is representative of actual uncertainty in small-scale mixing (which is what is said in the Poshyvailo et al 2018 paper).

p4 l14: 'best agreement with MLS observations' - I found this sentence a bit misleading. Of the papers cited, only Poshyvailo et al 2018 gives explicit information on the effect of varying mixing strength. So I don't see that the others provide any useful information on what choice of mixing strength gives the best agreement with MLS.

p4 l23: How are you calculating saturation mixing ratio exactly? Simmons et al (1999 QJRMS) make the point that the precise form of the expression used for saturation mixing ratio can be important. Again this is the sort of information that needs to be easily available to ensure that others can reproduce your calculations.

p4 l25: 'The effects of tuning ...' - this sentence would be clearer as 'The effects of tuning the critical supersaturation threshold in CLaMS have a similar effect to the effect of applying a frost point offset to the Lagrangian dry point temperature noted by Liu et al., (2010) and Fueglistaler et al., (2013), in that increase in the supersaturation threshold enhances both the mean value and the amplitude of the annual cycle in simulated H2O.'

p4 l28: I couldn't follow the logic of these two sentences - to me the point is that given uncertainty over the precise relation between temperature and saturation, e.g. due to

the uncertainty in the appropriate value of supersaturation threshold, it is impossible to be interpret differences between predicted water vapour and observations as being due to errors in LDP temperatures.

p4 l16: My experience is that the re-analyses don't provide diabatic heating as a single quantity, but as various components. Confirm that you are using all components (e.g. including latent heating).

p5 l5: My understanding is that you are estimating methane suppled water from (1), using a mode prediction of CH_4ˆrec. But how are you specifying alpha?

p6 l12-15: It seems important not to ignore the fact that the MLS weighting functions do not only produce 'artefacts' when applied to the CLaMS output, but that these artefacts may also be part of what is presented as the MLS observation - i.e. the MLS observations imply vertical structure that may be quite different to what is actually present. (At least that was my interpretation of Ploeger et al 2013.)

p6 l20: My understanding here is that you are simply extracting the semiannual cycle on the basis of its semi-annual frequency - not implying a direct relation to the phenomenon 'the Semi-Annual Oscillation (SAO)' that is identified in the low-latitude upper stratosphere. In other words, there is a potential difference in meaning between 'the SAO' and 'the semi-annual harmonic'.

p6 l26: It would be helpful here to note that the amplitudes and phases are what is presented in specific later figures.

p8 l21: It's a minor point, but you don't seem to have said explicitly that CLaMS-MRA is the MERRA simulation.

p12 l7: 'Moreover, this makes it possible that an optimisation of the dehydration scheme ...' - this sentence doesn't seem very relevant at this point in the text (and you have said something similar on p4).

p12 l14: 'However the AC phase of H2O entry values based on CLaMS-MRA is in better agreement with the SWOOSH data' - given the uncertainty indicated in SWOOSH one wonders whether the better agreement is actually significant. The comment is mostly about September/October - i.e. the judgement all comes down to September-October differences. Whether or not CLaMS-MRA or ClaMS-JRA is in better agreement with SWOOSH during the J-F-M-A period is also open to question.

p12 l31: For me a strong part of the evidence for the importance of different regions are LDP distributions based on trajectory studies - again Fueglistaler et al (2005) given the relevant distributions.

p13 l6: Can the slow upwelling alone account for the seasonal signal in H2O_CH_4? - i.e. isn't the seasonal cycle in in-mixing at least as important?

p14 Figure 6: It would be helpful to have the propagation of the maximum values as well as the propagation of the minimum values explicitly marked on the Figures.

p15/16: Figures 7 and 8 - it would be much better if Figure 7 could be immediately followed by Figure 8 - perhaps they could even be combined into a single Figure. I found the overlaid dots in Figure 7 quite difficult to see and to interpret – of course when reading the paper on a screen one can zoom to look in more detail at interesting features, so one isn't applying the same rules of legibility that might have been applied for paper publications. But given that the dots represent relative error, the fact that some of them are the same colours as are used for the values of the depicted field is confusing. It might be clearer to do something like use overprinted + and - (of different size to indicate size of relative difference).

p17 l3: 'The phase of the QBO effect at the tropopause is therefore consistent with that of the 50hPa QBO wind' - the 'therefore' only makes sense if you say explicitly that it is the 50hPa wind (rather than the wind at some other level) that is correlated with the tropopause temperature.

p17 l6: 'both overestimate A_QBO at isentropic levels between 450K and 550K' ...

'both biases can be traced back to strong diabatic upwelling' - the key point surely is not necessarily that the diabatic upwelling is strong, but that the the circulation (upwelling + eddy mixing) is such that the tape recorder signal in these two analyses decreases in amplitude in the vertical slowly relative to MLS and MERRA (as you show for the annual cycle in Figure 6 and 7).

p17 Figure 9: Give information in the caption on the dashed box. You could call this 'region 3'. Incidentally this region doesn't really seem to be characterised by a 'clear peak' - contrary to what you say on p16 l14.

p17 l9: You say 'the large values of A_QBO in region 2 are mainly linked to QBO-related modulation of the stratospheric circulation.' You should provide a reference for this. For example this aspect of the QBO signal was considered in the Baldwin et al (2001) review. That was quoting previously published studies. There may have been more recent work on this topic.

p17 l11: This reference should be 'Lossow et al 2017b'. The 2017a reference doesn't mention the QBO at all.

p18 l3: 'The amplitude and phase of the QBO signal in SWV show pronounced uncertainty in the middle stratosphere' - what do you mean by this? Do you mean that the observational signal is uncertain? (I don't think so.) If you mean that there is disagreement between the different CLaMS simulations and between the CLaMS simulations and the re-analysis then say that directly.

p18 Figure 10: As previously noted for Figures 7 and 8, it would be helpful for the reader if Figures 9 and 10 were immediately adjacent (or combined into a single Figure).

p18 A key point about the QBO signal in water vapour is that it results in part from upward propagation of the water vapour variations 'imprinted' by the QBO temperature variation at the tropical tropopause and in part from QBO variations in the meridional circulation (which seem likely to be primarily to be responsible for the QBO signal in water vapour seen in the upper stratosphere). You don't really exploit the distinction between these two processes generally in interpreting Figure 10 (which at first sight looks rather complicated). For example, you have identified from the annual cycle that ERA-I and JRA-55 seem to propagate signals too rapidly in the vertical in the lower stratosphere – presumably this means that their QBO signals are different in phase to the observed in the lower stratosphere, though you don't identify this as a significant difference (i.e. there is no vertical arrow marked corresponding to this in the lower stratosphere), presumably because the associated phase error is only a month or so. You do mark an arrow for JRA-55 - but don't comment on it. Do you interpret this as resulting from the strong vertical upwelling in JRA-55?

p18 l12: The fact that MERRA-2 doesn't match the observed QBO during the 1980s and early 1990s seems a pretty major and obvious problem to me. If that is the main cause of the difference in phase errors for MERRA between Figures D1 and D2 (indicated by the arrows in different directions) then it is probably clearer to omit D2 (and say why you have done that).

p26 l6: I've noted previously that the difference in annual cycle phase between predictions of different reanalyses seems to be judged almost on the basis of small differences between two months - and my question is whether that difference is really significant.

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