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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The effect of ENSO activity on lower stratospheric water vapor" *by* F. Xie et al.

Anonymous Referee #2

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My main criticism of this work is that the analysis is primarily done with assimilated water vapor in the stratosphere, which has not been shown to be accurate. There is not a detailed comparison between satellite measurements and the ERA water vapor to prove that the ERA-40 values are reasonable. There are only a few comparison plots, with no differences shown. There isn't any stratospheric water vapor assimilated in the ERA-40 analyses. Although the MLS plot shows that gross features are replicated (not surprising given that temperature is a controlling factor, and temperatures are assimilated (or at least radiances related to temperature are), it does not show that trends and variability are well replicated with ERA-40. Note, that it's hard to miss an annual cycle if saturation over ice is used, but in situ observations show many cases of supersaturation near the cold point tropopause, so that mechanisms aren't fully understood thereby not well parameterized in really any model (assimilation or

free running.) Hence I disagree with the authors' statement on Page 4146 that "an analysis of ERA-40 water vapor is still helpful since it has a longer time period...". I would be more convinced of a detailed analysis of a shorter time period of satellite water vapor data.

The other problem that I note in this work is that the authors are exclusively trying to attribute cause to ENSO variations, and ignore QBO variations. A recent paper by Calvin Liang (in JGR, 2011) using MLS+AIRS data shows that the QBO is a large source of water vapor variability, and phasing between ENSO and QBO is important. The authors need to look at whether the vertical structure they discuss is actually just transport of a QBO induced anomaly rather than anything to do with ENSO.

The concept behind this analysis is valid; that is the study of how ENSO (and other geophysical oscillations) impact stratospheric water vapor is important. This just isn't an appropriate model data set to use. I would be more convinced with a study using a free running CCM that includes appropriate forcings related to ENSO and compared El Nino and La Nina states. Then the complicating factor of including what the QBO does would be eliminated from the study.

An analysis of temperature and the Brewer Dobson circulation could certainly be done more convincingly than one of water vapor from ERA-40. The authors do need to keep in mind that ERA-interim gives better tropopause temperatures than ERA-40, and may be a better analysis to use (as would some of the other more modern reanalyses.) NCEP-2 is actually a poor choice for looking at tropical tropopause processes, given that it is warm biased near the tropopause and will not produce appropriate water vapor entry values. The new CSFR analysis from NCEP gives much more realistic tropical tropopause values.

If one wants to do a temperature analysis that is relevant for water vapor, it is important to consider the cold point rather than the 100-hPa temperatures. It is possible to get a better representation of the cold point using model level as opposed to pressure

level reanalysis products...while I am not familiar with obtaining those for ERA-40, it is possible to get model level output for MERRA, CSFR, JRA and NCEP. Is that what was used for estimating the cold point tropopause in figure 2? If so, please state as such in the text. In any case, please explain how you obtained a cold point from reanalysis output...were any interpolations used?

Discussion on Page 4148....The key thing to determine is whether La Nino or El Nino events change the effective entry value of water vapor into the stratosphere. Some of the longitudinal variations shown in Figure 3 may reflect tropopause height variations...that needs to be considered and discussed.

General comment; typically stratospheric water vapor is given in volume rather than mass mixing ratio.

Figure 4: Why do you use 370K as the tropopause temperature in the tropics? Holton et al. (1995, Reviews of Geophysics) used 380K. This actually makes a difference in terms of whether you are saying the phase of ENSO is moistening or drying the stratosphere.

In regards to compositing MLS and ERA-40 events, it may be worthwhile producing composites relative to type of ENSO event (cold tongue vs. warm pool) and strength of the ENSO event before comparing the MLS and longer period ERA-40 averages. What the phase of the QBO is will also impact the response. If you can't remove the QBO impact, then you can composite for different choices.

In Figure 5 (e and F) for HALOE, you should ignore the 150 hPa level...the data is not reliable there if you're using the publically released V19 product.

Page 4151: discussion of the BDC....how are you estimating the residual vertical velocity? This should be described. My past experience with BDC estimates are that using model vertical velocities + fluxes produce noisy results. A diabatic calculation works better. This is also found in a recent ACPD paper by Schoeberl and Dessler on

stratospheric water vapor and trajectories using MERRA output.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 4141, 2011.