**Atmospheric
Chemistry
and Physics
Discussions**

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

F. Xie et al.

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We appreciate for all the helpful comments. The comments have been carefully addressed and our replies are summarized below:

1. 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.

We totally agree with the reviewer that ERA-40 H2O is dominated by assimilation model temperature and upwelling. In the revised paper, we abandoned ERA-40 reanalysis and used ERA-Interim dataset instead. ERA-Interim dataset assimilates the new model outputs and satellite observations (Simmons, et al., 2007a; Simmons, et al., 2007b; Uppala, et al., 2008), with a 1.5o x 1.5o horizontal resolution and relatively high vertical resolution in the tropical tropopause region. Its total column water vapor shows a good agreement with observations [Uppala, et al., 2008].

Apart from replacing ERA-40 with ERA-interim, we would like to point out again that we have compared assimilated water vapor data with MLS data in different aspects, and the comparison shows overall similar results.

We think that a shorter study just using satellite data would not be very useful, because the data have large errors (10%+) and detecting small differences over a short timescale would be impossible.

2. 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 con-

cept 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.

Thanks for this good comment. In the revised paper, we perform a 30-year model simulation with WACCM model which is unable to internally simulate QBO signals. The model simulations are analyzed in the same way as ERA-interim data to detect the effect of QBO on ENSO water vapor anomalies. Meanwhile, the filtered EAR-interim data with QBO signals excluded are further analyzed to verify modeled results. The above additional analysis reveal that QBO indeed has an important impact on ENSO water vapor anomalies in the middle stratosphere and the text is enriched accordingly.

3. 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.

We agree with the reviewer that ERA-interim water vapor and temperature are better than those of EAR-40, so, in the revised paper we use ERA-interim to analyze the stratospheric water vapor and tropopause temperature anomalies associated with ENSO events. However, the Brewer Dobson (BD) circulation diagnosed from monthly mean time series are rather noise while ERA-Interim dataset only spans a 22-year time period. For the purpose of making composite analysis of the BD circulation more reliable, we still utilized ERA-40 reanalysis to composite BD circulation anomalies as-

sociated with ENSO events.

4. 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?

The cold point tropopause is determined by finding the data pressure level with the lowest temperature from the upper troposphere to lower stratosphere in ERA-interim data. This is clarified in the revised paper.

5. 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.

The cold point tropopause has been added in Figure 3 (Please see the Supplement), and some discussions about its longitudinal variations are included in the revised paper as following:

"Another interesting feature in Fig.3 is that the cold point tropopause height is significantly higher over the eastern Pacific at around 120oW in La NinËIJa events (Figs. 3c and 3d) while the water vapor flux at this region is significantly downward (Figs. 3e and 3f). This feature is not evident in El NinËIJo events (Fig.3a, 3b) and non-ENSO events (not shown). This result implies that locations of stronger convective activity, hence higher cold point tropopause, do not collocated at the regions with upward water vapor flux in the lower stratosphere in La NinËIJa events."

6. 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.

380 K isentropic surface has been chosen as the tropopause in Figure 4.

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

The unit of stratospheric water vapor is changed to ppmv.

8. 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.

We agree that producing composites relative to type and strength of the ENSO event would make the anomalies stronger. But the ERA-interim and MLS data span a relatively short time period, and there are not too many data records if we produce composites relative to type and strength of the ENSO event. So the composites in this study are made relatively to the type of ENSO events only.

9. 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 the revised paper, we perform a 30-year model simulation with WACCM model which is unable to internally simulate QBO signals, and the model simulations are analyzed in the same way as ERA-interim data to detect the effect of QBO on ENSO water vapor anomalies. Meanwhile, the filtered EAR-interim data with QBO signals excluded are further analyzed to verify modeled results. Those additional analyses reveal that QBO indeed has an important impact on ENSO water vapor anomalies in the middle stratosphere and the text is enriched accordingly.

10. 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.

Due to too many missing values in HALOE data in the tropical lower stratosphere, the analysis of HALOE data was deleted in the revised paper.

11. 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.

In our study, we used the kinematic method to estimate BD circulation through the formulae given by Edmon (1980):

The above is clarified in the revised paper.

We agree that kinematic calculation would create "noise" in the vertical velocity field and a diabatic calculation works overall better. However, the study by Schoeberl and Dessler [2011] indicates that differences in the lower stratospheric mean age calculated by the kinematic and diabatic methods are small in the tropical lower stratosphere implying that kinematic calculation is reliable at least in the tropical lower stratosphere. In our study, the analysis of the BD circulation is mainly confined within the tropical lower stratosphere, therefore, using kinematic method to calculate BD circulation should not be a problem.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/11/C4262/2011/acpd-11-C4262-2011-supplement.pdf

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