Review of Xie et al, ACP, The Effect of ENSO Activity on Lower Stratospheric Water Vapor and Circulation

In the paper (and the abstract), the authors claim that overall, El Nino events tend to moisten the lower stratosphere and La Nina events have an opposite effect on the lower stratosphere. I don't think that is a robust defensible statement, and may suffer from definition of tropopause height. I enclose a plot of 82 mb water vapor anomalies in the tropics (10S-10N) based on MLS and HALOE, and ONI is also plotted. The latest, and 1999-2000 La Ninos show a positive zonally averaged anomaly at 82 mb (lower tropical stratosphere), and the El Ninos in the period show both types of stratospheric water vapor anomalies. (I think your definitions of lower and middle stratosphere need to be quite clear. In my mind, 82 mb is the lower stratosphere, the middle stratosphere is around 10 mb. I don't think that is the convention used in this paper.)

Also, my cursory glance at the MERRA residual circulations give very different results to what is presented in in regards to the BD circulation. It would be worthwhile looking at multiple reanalyses for consistency in conclusions.

The comments above are rather minor concerns. My major criticism of this study is that it relies on stratospheric water vapor from a data assimilation that has no input from a stratospheric water vapor observation. (Water vapor is assimilated in some fashion, but it is tropospheric column measurements as far as I understand after looking in detain at the ERA-Interim description on the ECMWF web site). Therefore, the water vapor in the ERA-Interim analysis is simply a function of model cold point temperatures (which do have information assimilated into them), and nuances in whatever convective parameterization used. Is it a justifiable parameterization from a stratospheric water vapor transport standpoint? (ie, is particulate water entering the stratosphere through overshooting tops?). In fact, heavy water measurements would indicate that it does, in that the HDO values measured in the stratosphere from aircraft, balloon and satellite would all indicate that in fact, particulate water likely does enter the stratosphere and help to keep the HDO values different from that expected from a standard Rayleigh distillation curve. Chemistry climate models do not well represent either the absolute value or in some cases even the annual cycle of stratospheric water vapor as measured by satellite, and there are clearly unknowns as to the degree of supersaturation that actually occurs into the upper troposphere and near the cold point, and how dehydration actually plays out as air enters the stratosphere. Since we really don't understand those final steps, and they are likely associated with cloud processes, I find it hard to believe that ERA-Interim actually does them correctly.

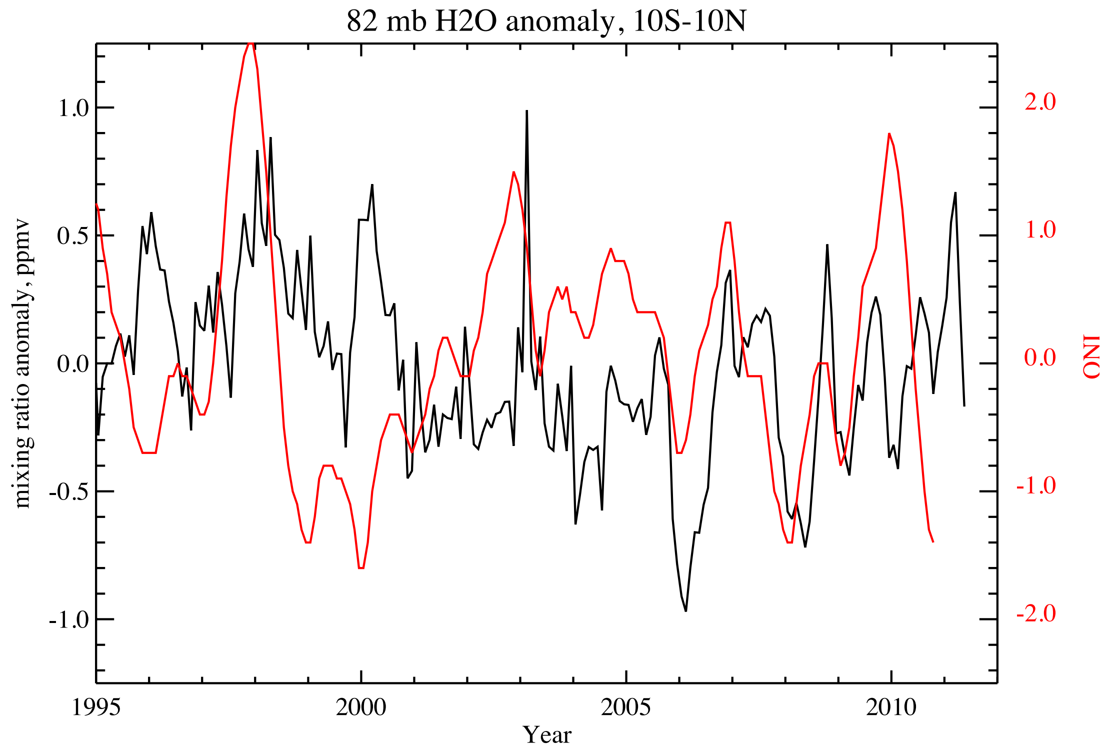
I have no issues with the authors analyzing variability in cold point temperatures using ERA-interim, but I do not trust an analysis of stratospheric water vapor from reanalysis. It doesn't matter which particular reanalysis is used, they all suffer from the same flaw that they don't assimilate stratospheric water vapor, and that doesn't even begin to consider the issues with measuring stratospheric water vapor. (Note, that even measurements of stratospheric water vapor have significant uncertainties, on the order or bigger than the variations between phases of ENSO noted here.) It is more justifiable to look at residual circulation estimates from ERA-Interim, but there can be concerns with that as well. For example, is mass actually conserved in the assimilation? (ie, if you compute the residual circulation first using w and secondly using v, do you get the same stream function. I know for a fact that using the UARS UKMO assimilation, you don't, and found it more useful to compute the residual circulation derived from radiative heating rates based on monthly mean fields.)

Figure 4 is the one I have the largest concern with. For one thing, 370K is the solid line, not 380K. Secondly, this type of graph should really be done with profiles computed relative to actual tropopause height. The tropical tropopause can lie anywhere between 379 and 400, and that varies according to phase of the ENSO cycle (and where in longitude one is considering.) Calculate tropopause height (or potential temperature), and do the binning relative to that. You may actually see a very different picture. And please note, 460K is not the middle stratosphere, which is implied in the some of the results mentioned in the abstract.

Also note, in Figure 1, although patterns may look similar, this sort of plot does not show actual differences. Similar contour plots at one level do not make a validation, even of something like a seasonal cycle. Before you can use the ERA-Interim model output water to assess anything about reality, it needs to be validated in the stratosphere. The contour levels in the bottom panels of Figure 1 are too big to really be useful for a comparison, it would be much more useful to see a difference plot. You claim you can't do composites of ENSO cycles with the satellite data, but you could at least pick an overlapping time period (ie, for MLS that would start in 2004), and see how the model actually compares to MLS (or, pick another satellite data set…MIPAS, GOMOS, ACE, HALOE for an earlier period in the stratosphere only.

I think it would be useful to do a similar type analysis using only assimilated temperatures. There are still significant issues with assessing trend like variability from assimilated temperatures, in that even if the model is consistent over the entire time period, the input data is not given that radiosonde types change over the long term and have been shown to introduce biases (look at work by Dian Seidel and colleagues), and there are changes in satellite input over time as well. However, at least there is some data input into the tropical tropopause and stratospheric temperatures and winds (unlike for stratospheric water vapor, and input for upper tropospheric water vapor isn't very good either.) Radiosondes don't work well for water in the upper troposphere, and the raw UTH satellite channel (ie HIRS) has some consistency issues as well. As I recommended before, just use the satellite water vapor data sets (see the list mentioned above) to analyze ENSO variability in the lower stratosphere, not assimilated model output. You may not be able to consider trends given constraints on the length of time for consistent data sets, but you can look at variability.) Assimilation is a useful tool, but one needs to carefully consider what is a useful analysis to do with such a product. Stratospheric water vapor is not a variable that should be believed in an assimilation. (take a look at the model spread in the water vapor plot of the seasonal cycle at the cold point in the CCMVal report to get a feel for the issues in modeling stratospheric water vapor. A similar plot also appears in the climate change chapter of the most recent WMO ozone assessment.

Because of this fundamental problem with this analysis, I do not recommend publication in ACP. There are detailed comments I could make in the text as well, however I have not done so because of what I see as a significant flaw. I suggest the authors read in detail the recently published ENSO/QBO analysis of a combined MLS/AIRS water vapor data set by Calvin Liang, and possibly try complementing that with a temperatures analysis from assimilated output, or stick simply to a simple CCM water vapor analysis.



The ONI is shown in red, tropical water vapor anomalies (based on MLS and HALOE monthly averages) at 82 mb (in the lower stratosphere) is shown in black. Note, sometimes they appear correlated and sometimes anticorrelated.