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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Simulation of stratospheric water vapor and trends using three reanalyses" *by* M. R. Schoeberl et al.

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The authors would like to thank the reviewers for thoughtful and insightful comments.

Response to Reviewer Comments

Referee #2

The forward trajectory calculations have several advantages over the back calculations, but, in principle, they are the same. In the backward calculation, you start with a distribution of parcels and run the trajectory calculation backward to determine the conditions the parcel has encountered. This calculation may need to run for a long time to assess the origin of each initial parcel which few authors do (e.g. 5 years for assessment of polar parcels). Furthermore, the parcels are moving through an evolv-

ing atmospheric state, thus to do a proper assessment, the distribution would have to be initialized frequently to account for seasonal and more rapid variations of the atmospheric state. Obviously a winter computation will differ from a summer computation. So it is an interesting question on how often the backward computation needs to be run to provide a comprehensive assessment. At the minimum, each month, and each integration goes back a year or so. In any event, this creates an enormous computational burden. This is issue not present in the forward computation.

On the other hand, if one wants to find the origin of parcels observed by satellite, balloon or aircraft, back trajectories are a simple and computationally expedient method. In principle, the forward method could apply here as well, but it would require a huge number of parcels and it would be computationally inefficient.

In summary the forward and backward methods are equivalent (or ought to be) but the choice of the method is dictated the problem to be solved. This discussion has been clarified in the revised text.

The data aren't weighted by the averaging kernals for MLS (Fig. 4) but instead the data are averaged onto the MLS L2 vertical grid. This comment has been added to the revised paper.

The Poleger et al. paper (2012) is added to the reference list and noted in the text.

Minor comments: Most addressed as required. We noted that the higher time resolution MERRA temperature fields produce drier stratosphere than the daily average fields. This occurs because the model is not smoothing the temperature fluctuations that occurs inside a day – and the new temperature extremes are create more opportunity for dehydration.

Referee #3

Comment 1: The reviewer is correct that the model does not do a good job reproducing the stratospheric dry period that was observed starting in 2000 which has ended according to most recent data. It will be difficult to be quantitative about long-term trends due to the presence of the annual cycle in the figure. This paper mostly focused on differences between models and some model trends. We are working on a more comprehensive paper about the water vapor trends but that material is in development and is beyond the scope of this work.

Comment 2: We used the full resolution radiosonde data available at Singapore where it was available (early data is only available at standard levels). We used Singapore because of its fairly long consistent record. We checked the site http://www.sparc.sunysb.edu/html/hres.html for high resolution deep tropical cold pool, the three relevant data sets (Koror, Yap and Truk) extend only 10 years (1998-2008).The other deep tropical stations show shorter or more intermittent records. Singapore lies within the final dehydration zone (or is on the edge) so it seems appropriate to illustrate the points made here. Comment added to text.

Comment 3: Certainly poor representation of the cold point could be the reason that the trends are both inconsistent with the data and inconsistent with each other. It is also apparent that it isn't simple the TWP problems since a significant fraction of air is dehydrated over South America as well.

Referee #4

Comment on methane oxidation: The methane oxidation rates were derived from the Jackman 2D model [Fleming, E.L., C.H. Jackman, D.K. Weisenstein, and M.K.W. Ko, The impact of inter-annual variability on multidecadal total ozone simulations, J. Geophys. Res., 112, D10310, doi:10.1029/2006JD007953, 2007]

which used the most recent JPL recommendations. The 2D model reproduces the salient features of the methane distribution. It is clear from our trajectory calculation there are issues with high latitude descent and transport of CH4 and H2O derived from CH4 so we make that attribution. Note that methane oxidation at winter high latitudes is quite weak due to the paucity of OH. While we can't totally rule out problems with

CH4 oxidation rates, it seems that the most likely explanation is transport.

A good reference to dispersive effects of transport is Prather, M. J., Z. Zhu, S. E. Strahan, S. D. Steenrod, J. M. Rodriguez: Quantifying errors in trace species transport modeling, PNAS 105, 19617-19621, 2008. – now noted in the text.

Response to why MERRA 6 hour is gives a drier stratosphere is noted in the text and discussed under Referee 2

Figure 1a,b are meant to illustrate the differences between the reanalysis models and the observations. As now noted in the text, the same number of data points is used for each of the reanalyses. We don't understand the last question the review asks in this comment. Generally the warm tail has very little effect on the water vapor which is driven by the cold tail.

The parameters used for gravity waves, super saturation and convective moistening are now stated in the text. We have also added a sentence describing their overall effect. Thank you for noting this oversight.

Figure 5, the figure is already crowded enough and we don't think adding uncertainty bars will add to the point of the figure.

Our results agree quite will with methane observed by ACE (not shown).

Other minor comments addressed as appropriate.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 8433, 2012.