Reply to anonymous Referee #2 (acpd-14-C9976–C9981)

This study discusses an important scientific question, namely inter annual variability of stratospheric water vapour. The particular focus is on trajectory calculations and on the question in how far the vertical resolution of the information on stratospheric temperatures influences the simulated freeze drying in the model. This is an interesting and important topic, which is of interest to the readership of ACP.

The descriptions of a trend in water vapour and interannual and seasonal variability are different things (e.g. Ploeger et al., 2011; Fueglistaler et al., 2013) and are likely influenced by different processes. And the issue of stratospheric water vapour trends is an important issue, which is alluded to in the manuscript, but somewhat underrepresented in the discussions here. Note that stratospheric trends of water vapour are rather uncertain (e.g., Hurst et al., 2011; Kunz et al., 2013; Urban et al., 2014; Hegglin et al., 2014). It would be important, if this paper could provide further and deeper insight into the interpretation of stratospheric water vapour trends. Alternatively, if the focus of the paper is solely on variability, this should be clearly stated in the paper.

Reply:

Like we said, the focus of our paper is to investigate the impacts of temperature datasets on the trajectory modeling of water vapor, so we have removed all discussion of "trend" in this version. It is not appropriate to analyze decadal trend based on only ~7 years of data. Besides those papers that discussed H_2O trend based on Boulder records (Hurst et al., 2011; Kunz et al., 2013), which were later proved to be problematic (Hegglin et al., 2014), we have a new paper published recently (Dessler et al., 2014) on the uncertainties of H_2O trend.

A major focus of the paper is on dehydration mechanisms of stratospheric air and Figure 5 is the central figure. However I have reservations about the figure and its interpretation (see also below). From the concept of FDP presented in this figure, it is not clear to me why the stratospheric water vapour levels can be so similar for the three temperature sources given the fact that the FDP curves are rather similar, but the FDP frequencies are rather different. I think the paper could be clearer here in its arguments.

Reply:

The different shapes of FDP curves are caused by whether the tropopause temperatures have enough variability in the vertical. In case of MERRA, the tropopause temperatures are constrained only at two discrete levels (100 and 85 hPa), and therefore the FDP peaks around them. In case of GPS and MER-Twave, enough variability in tropopause temperatures enables FDP to be found in a wider range, and therefore the FDP curves going through gradual transitions.

The dashed curves of FDP H_2O represent the stratosphere entry level of H_2O, controlled solely by the coldest temperatures that parcels encountered along their travelling paths. When FDP occurs at tropopause level (90-100 hPa), the entry level H_2O could have generally 0.1-0.4 ppmv differences comparing between MERRA and GPS

(Fig. 4 and Fig. 8), which is not easy to tell given the large upper x-axis range in Fig. 5. That's why the dashed curves look similar, but still different. In the updated Fig. 5, we have plotted FDP and FDP H_2O in four seasons as well as all season averages.

I also suggest that an actual reconstruction of a water vapour profile is presented in the paper not only FDP profiles or relative water vapour differences (Fig. 8). This should allow a better assessment of the quality of the simulated profiles.

Reply:

We have updated this figure as Fig. 7 in the updated manuscript to include the actual water vapour profiles from three different runs.

Further, I suggest the authors consider the effect of methane oxidation on the increase of water vapour with altitude. Note that the chemical conversion of methane to water vapour (which does not occur through photolysis, see below) does not have to happen at altitudes of 80 or 60 hPa. Rather aged air that has experienced methane oxidation will descend and could be mixed into these altitudes (Ploeger et al., 2012; Abalos et al., 2013). In the presented model study, this effect is likely only partly taken into account by just considering trajectories. Could this be relevant for the results of this paper? The effect should be easy to check in a model world by switching off methane oxidation in the model. I suggest that the authors conduct such a sensitivity test and compare with observed water vapour profiles.

Reply:

Aged air descending back to the tropics has very limited impacts on the dehydration and final water vapour abundances in the lower stratosphere. This has been shown in our previous paper (refer Fig. 6 in Schoeberl et al., 2012). A more quantitative impression can be understood in Fig. R21 below, where trajectory results are obtained from using GPS temperatures with methane oxidation turned on and off. It is clear that below 70 hPa (~19 km), aged parcels carrying H_2O from methane oxidation plays a trivial role in the overall abundances of water vapour. This has been addressed in the updated manuscript in lines 126-128.

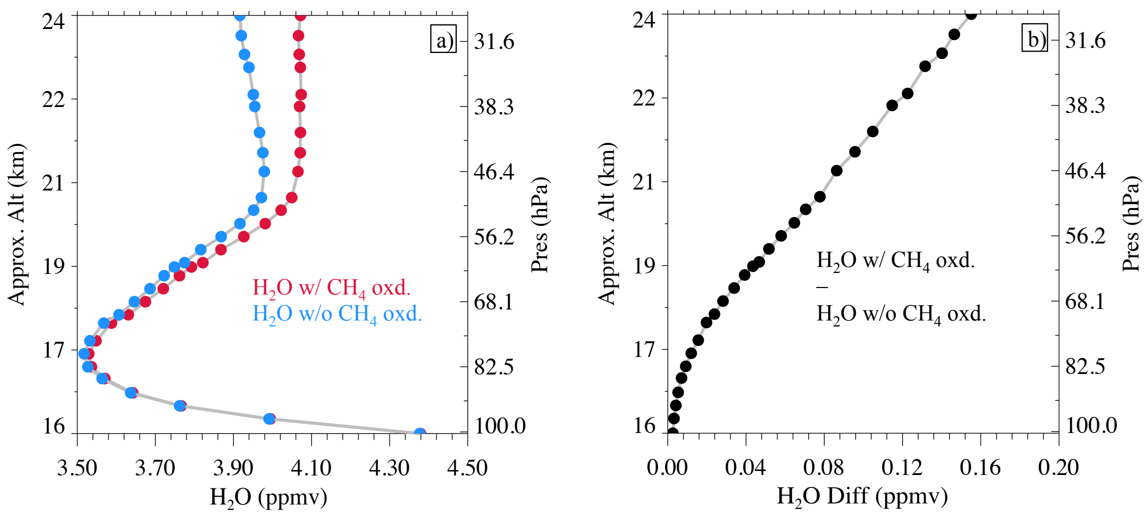

Figure R21. (a) Trajectory simulated water vapour from using GPS temperatures, with methane oxidation turned on (red) and off (blue); (b) The differences caused by methane oxidation. All data are averaged over the tropics (18° N-S) in 2007-2013.

The paper also makes the point that inter annual variability is unchanged in the time series when the different temperature sets are employed and 'only' the absolute value is affected. First it should be discussed and stated in the paper that the absolute values are important for calculating the radiative forcing (and thus for the climate impact). In my opinion, the absolute values matter. Second, what is the conclusion from this observation? That high and low excursions in the interannual variability are equally affected by the resolution of the temperatures? Should this conclusion also hold for time series longer than the seven years shown in Fig. 9? For example, for time series over 30 years with pronounced variability?

Reply:

This is an excellent point. We have added discussions of the importance of H_2O abundances to the radiative forcing calculations.

One of the conclusions of this paper is that despite the different vertical resolutions of temperature, the predicted water vapour interannual variability is almost the same. This conclusion also holds for longer time period as shown in Fig. R22 below. This discussion has been added in section 3.2.

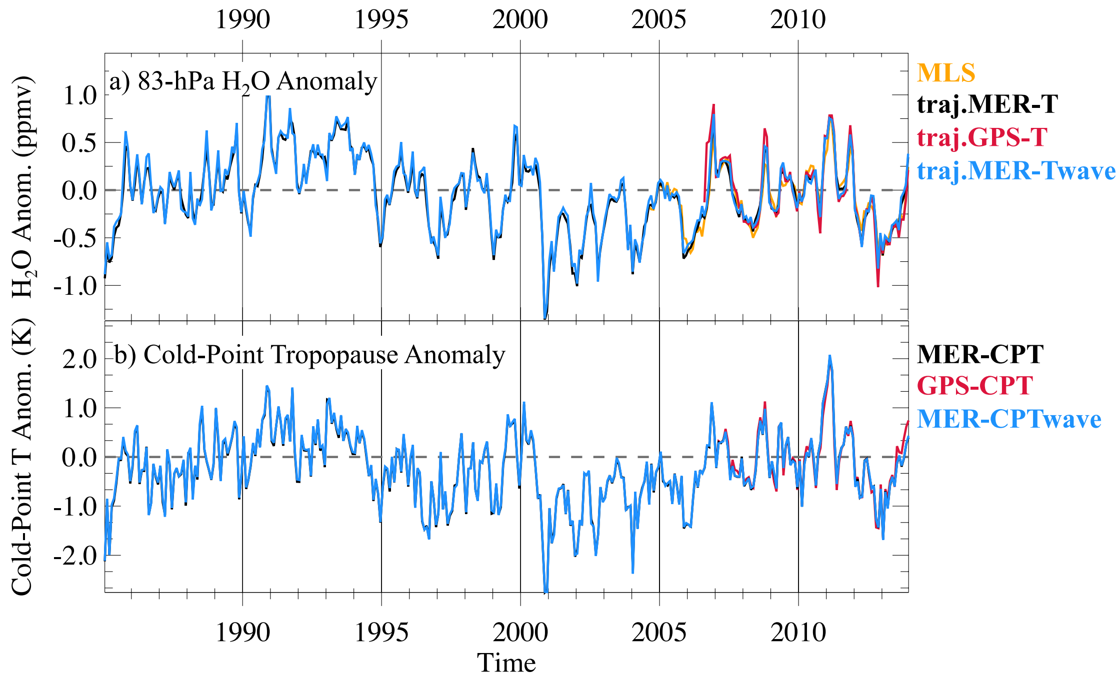

Figure R22. The H_2O anomaly (a) and the cold-point tropopause anomaly in longer period from different datasets.

Comments in detail

- *p. 29211., l. 4: Is immediate freeze out at 100% saturation assumed? This statement seems to imply that this is the case. How realistic is this assumption? For example Tompkins et al. (2007) argued for a different representation of dehydration in the ECMWF model and other trajectory studies have tested different dehydration assumptions.*

  Reply:

  We performed sensitivity tests to different saturation levels and it turns out that the simulated water vapour offset constant values but with identical interannual variability. Note that the major focus of this paper is to investigate the uncertainty introduced by using temperatures in different vertical resolutions. Despite the frequent occurrences of supersaturation (Jensen et al., 2013) and the re-evaporation of the condensate (e.g., Schoeberl et al., 2014), the comparison would be essentially the same as long as we keep the same criteria for different runs. This discussion is included in the update manuscript lines 116-120.

- *p. 29212., l. 20: It should be more explicitly stated which terms enter the calculation of the potential temperature tendency here; just clear sky heating rates?*

  Reply:

We used total diabatic heating rates from all sky, which include heating rates from long-wave and short-wave radiation, moist physics, friction, etc. This has been modified in lines 75-97 to be clear.

- *p. 29213., l. 18: The major chemical loss of methane in the stratosphere occurs through reactions with radicals, not through photolysis (e.g. Röckmann et al., 2004; Brasseur and Solomon, 2005); if the loss mechanism used here is really photolysis, the loss (and thus the water vapour production) is not correctly simulated.*

Reply:

Oxidation of hydrogen, mainly methane, is an important in situ source of water vapour in the stratosphere. To account for methane oxidation in our model, we independently track methane in each parcel and photolyze it using photochemical loss rates. The loss of each molecule of methane produces two molecules of H_2O (Dessler et al., 1994). The oxidation of H_2 formed from methane photolysis is implicitly included in this scheme. This has been stated in the updated manuscript lines 122-128.

Minor issues

- *Abstract, l. 11: 1.2 km is also finite, do you mean 'higher resolution'*
- *Abstract, l. 11: 'including' is incorrect, you only consider there tow data sets.*
- *p. 29211., l. 15: 'tracers that depend'*
- *p. 29211., l. 23: 'carrying H2O' is unclear*
- *p. 29212., l. 1: drop 'etc.'*
- *p. 29213., l. 2: why 're' entered?*
- *p. 29214., l. 9: 'that used' . . .*

Reply:

Done the above.

- *p. 2921., l. 27: state how the 0.4 ppmv bias was deduced*

Reply:

We have added temperature profiles in Fig. 2. Averaged over 18^{o} N-S, the largest temperature difference of 0.4 K shows at ~93 hPa (cold-point tropopause) when the GPS temperature is generally ~193 K. With 100% saturation level assumption, the C-C equation yields a 0.41 ppmv difference in H_2O. This has been added in the updated manuscript lines 193-197.

- *p. 29216: l. 19: how do we know it is 'realistic'?*

Reply:

The MER-Twave is designed to have similar temperature variability to radiosondes measurements based on Kim and Alexander, 2013. Temperature variability in radiosondes measurements can be treated as realistic.

- *Figure 5: The text states that the analysis was done using a large number of isentropic trajectories, nonetheless, Fig. 5 looks as if the analysis has been done on several discrete pressure levels. See for example the obvious kinks in the black solid line, which are spaced about 5 hPa apart. Further, it looks to me that the solid lines in Fig 7b and in Fig 5 are the same lines, although the x-axis is different. Please check.*

  Reply:

  We perform diabatic trajectories in isentropic coordinate to avoid the over dispersion in pressure coordinate. After that, we present results in pressure coordinate to be able to compare with reanalyses model levels, which are pressure levels.

  Fig. 5 is the FDP frequency averaged over 7 years, whereas Fig. 7b only shows the SON season. The reason that x-axis is different is because the frequencies are calculated with respect to the total FDP events in 7 years, therefore the magnitude in Fig. 7b is about ¼ of that in Fig. 5. This has been modified in the updated Fig. 5 to relative to the FDP events of each curve.

  In the updated Fig. 5, we have changed all normalized FDP frequency unit from "%/hPa" to "%", so each PDF profile integrate from bottom to up ends up with 100%.

- *p. 29221., l. 2: change 'stratospheric' to 'stratosphere'*

  Reply:

  Done.

[References]

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