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Interactive Comment

Interactive comment on "The impact of temperature resolution on trajectory modeling of stratospheric water vapour" *by* T. Wang et al.

Anonymous Referee #2

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General comments

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 description of a trend in water vapour and inter annual 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

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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.

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 the paper could be clearer here in its arguments. 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.

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.

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

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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 inter annual 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?

In summary, I think more discussion is needed and a more direct comparison of simulated and observed water vapour profiles. I think this is doable and expect that a revised version of the paper will make a good contribution to ACP.

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.
- 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?
- 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.
- Finally, I think a grammatical and stylistic revision of the manuscript would be helpful. (See examples in this review and in the initial review for ACPD).

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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 H_2O’ is unclear
- p. 29212., l. 1: drop ‘etc.’
- p. 29213., l. 2: why ‘re’ entered?
- p. 29214., l. 9: ‘that used’ . . .
- p. 2921., l. 27: state how the 0.4 ppmv bias was deduced
- p. 29216: l. 19: how do we know it is ‘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.
- p. 29221., l. 2: change ‘stratospheric’ to ‘stratosphere’

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