Anonymous Referee #4

This is a solid and thorough study of the 2009 major stratospheric warming from the perspective of tracer transport. In general, it was interesting, but there are areas of confusion. In addition, I think the paper is too long given what its trying to say. The only really quantified results are the descent rates and they take too long to say it.

We thank referee #4 for this comment. The paper has been re-arranged to address comments from A. Geer, and help improve the flow of the paper. The logic of the re-arranged paper is as follows (the Figure numbers correspond to the new figures, and are identified in brackets):

- ***Show first the meteorological development (Section 3, Fig. 2);***
- ***Illustrate the difference between the analyses and the gridded observations and motivate the use of analyses to estimate vortex descent (Section 4.1, Fig. 3);***
- ***Show the spatio-temporal development in the H2O and PV fields (Section 4.2, Figs. 4-6);***
- ***Discuss descent using the vortex picture (Section 5.1, Fig. 7), the equivalent latitude-theta picture including a comparison with Manney et al. (Section 5.2, Fig. 8), and summarize the methods and comparison (Section 5.3, Table 1);***
- ***Conclude (Section 6).***

The relationship between the Sections of the ACPD paper and the Sections of the re-arranged paper is explained in the response to the referee A. Geer.

We have also corrected typos and inconsistencies in the paper (e.g. removing references not cited in the text), and made an effort to remove redundancy.

We think this re-arrangement improves the flow of the paper; shortens it; and provides results of the descent in a format easy to digest (provided in Table 1, following the advice of A. Geer).

Suggestions for revision follow:

Major concerns

1) I'm still a bit confused as to the relative roles of descent and mixing in some of the cases they discuss. Section 4.2 gave me the most problems. First, they present Figures 5-7, then starting on Line 398 they discuss them but then go back on line 426 to Figure 5. I got lost. Second, I don't understand their upper stratosphere discussion. They argue that the relationship between PV and H2O is reversed. I don't see that.

If I look at Figure 7, I see on Feb 1, the peak PV corresponding to low H2O and the highest H2O north of Siberia corresponds to a tongue of low PV air. So this is an anticorrelation, similar to the other cases earlier in January. Third, they are very sloppy about their dates. Thus on in a few places (line 423 for example) they say "during February". But they only show maps up to Feb 1, not "during February". This section needs a reworking. One thing I suggest would be actual scatter plots between PV and H2O so we can better see the relation between the two.

We think the re-arrangement of the paper addresses the first point from the reviewer concerning Section 5.2 in the ACPD paper (we think the referee refers to this section, as there was no Section 4.2). The old Section 5.2 now becomes Section 4.2 (see response to referee A. Geer).

We are now more careful about describing the relationship between H2O and PV in the upper stratosphere, and its evolution during January/February. The text is modified to address this (Section 5.1). This addresses the second point from the reviewer.

We have made an effort to eliminate sloppiness in the text. This addresses the third point from the reviewer. We do not compute scatter plots between PV and H2O as we think the behaviour of these fields with respect to each other is sufficiently illustrated by (the new) Figs. 4-6.

2) As noted above, the paper is too long. I lost count of the multiple occurrences of the phrase "added value" or "illustrates the benefit" But I think it occurs about 4 times in Section 6 and two more times in Section 7. This is illustrative of the redundancy that persists throughout the text. For a start, Sections 6 and 7 are duplicative and should be truncated and combined.

In the re-arranged version of the paper we have merged sections 6 and 7. We have also made an effort to avoid redundancy.

3) The authors quantify the descent rate in several places. But they also state that there is horizontal mixing. Can this be quantified as well? Perhaps as some sort of mixing coefficient?

We comment in Section 5.3 that estimated descent rates exclude an estimate of horizontal mixing and that, therefore, the estimates provided should be regarded as a first-order estimate. We also comment estimation of this contribution is outside the scope of this paper (doing this would also lengthen the paper, which we want to avoid following advice from referee #4).

4) I'd suggest some rewording of the abstract. It reads somewhat like a conference abstract which describes the technique, but not the results. If their punchline is 1 km/day descent, then that should be in the abstract.

We modify the abstract to address the comment of the referee. In particular, the descent rates are mentioned in the abstract.

Other Concerns

1) I had some problems with the rightmost column in Figure 5-7. First, why can't they use a more physical x-axis than "profile number"? Can't they just plot the corresponding latitude? Second, some of the plots have a vertical gray bar (assoc with January 24th). What is this?

In what are now Figures 4-6, the plots in the rightmost column are modified to include the corresponding latitudes along the top. The vertical gray bar areas are removed (see response to referee A. Geer).

2) Figure 7 and text around line 360. There seem to be several cases where the gridded data look more physical compared with the PV than does the assimilation, despite the blanket statement in the text. Simply because the analyses look more "fluid-like" is insufficient to claim they are "more physically realistic". This should be reconsidered.

We modify the discussion about physical realism to address the comment of the referee (Section 4.1).

3) Line 486: I'd weaken this to say "generally agree better"

We cannot identify line 486 in our version of the ACPD paper. We modify the text at the end of what is now Section 4.2, which we think corresponds to the text referred to by the referee.

4) Section 5: I see a 4th feature, namely, the region of H2O below 6 ppmv which is decreasing with time. This looks like its contiguous with the descent, but its obviously connected with poleward mixing of drier air. So I'd be interested to see how they can separate out the descent of drier mesospheric air and the onset of horizontal mixing of drier air.

This feature is now identified by the letter D in what is now Fig. 7 (top panel). We provide a brief discussion on the evolution of this feature. The issue of horizontal mixing is considered in the response to major concern # 3 (above).

5) The issue of the positive bias of 0.25-0.5 in the CTM runs should be better explained.
Clearly the CTM has some small error in the partitioning between H2O and CH4 (and possibly H2O).

We comment on likely errors in the CTM that could contribute to this difference (Section 5.1).

We have checked and found that the bias between the CTM run and the analyses is not related to the partitioning of H2O and CH4. Comparing the CTM runs with and without chemistry (see Fig. 8 in the new version of the paper) shows little difference between them, suggesting very little impact from the methane oxidation set up on the concentration of H2O. Furthermore, methane oxidation operates on a longer time scale than the two months of this study. Hence, differences between the CTM runs and the analyses are more likely related to transport.

What are now Figures 7 and 8 show that the CTM runs are smoother than the analyses and we understand that the dynamical barriers simulated by the CTM are not as strong as they should be based on other fields (geopotential height, PV). The reason for this is likely related to model resolution – increasing the resolution might improve this aspect of the model (but this has not been tested), but is also inherent in the use of dynamical wind fields and their implementation in the CTM. We now mention this briefly in the text (Section 5.2).

6) Section 6: the comparison of the descent rate of Lee et al with the present study seems to suggest a factor of 2 difference. Calling this "comparable", as they do on line 704 seems a bit over-optimistic. Suggest some rewording.

We modify the text to address the comment from the referee.

Typo on the caption for Figure 4(a). It says panel 6 is Jan 8. Don't they mean the 20th?

The figures for the new version of the paper have been checked for typos, and corrections made.