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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The 2009 stratospheric major warming described from synergistic use of BASCOE water vapour analyses and MLS observations" *by* W. A. Lahoz et al.

Anonymous Referee #2

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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. 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.

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.

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?

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.

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?

2) Figure 7 and text around line 360. There seem to be several cases where the gridded

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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.

3) Line 486: I’d weaken this to say “generally agree better”

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.

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).

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.

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

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 24699, 2010.