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Interactive Comment

Interactive comment on "Validation of stratospheric water vapour measurements from the airborne microwave radiometer AMSOS" *by* S. C. Müller et al.

Anonymous Referee #3

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This is an interesting paper which shows a large set of airborne middle atmospheric water vapor profiles in comparison with numerous other measurements. The results generally look quite good and the material is clearly presented.

The one figure which is missing and which I would have hoped to see is something showing latitudinal variation of MLS water vapor (either zonally averaged or at the nearest possible longitude to the AMSOS measurements) alongside the AMSOS data, so that one could better judge the accuracy of the observed latitudinal variations. This should, I think, be possible for missions 9 and 10.

On pg. 1643 line 6, it sounds like AMSOS measurements need to be convolved to compare to MIAWARA, and this is certainly not what the authors do (as indeed they should not). Please just change the wording here. However, for Section 3.4 this issue becomes more serious. The AMSOS instrument probably has only slightly better vertical resolution than the MIAWARA instrument, so averaging kernels should not be used in this comparison.

The method used to avoid problems with averaging kernels near the tropopause is okay, and is consistent with the desire to keep the apriori mixing ratio constant. While this is generally a good idea, it would be interesting to allow the apriori mixing ratio in the troposphere, and possibly in the lowermost stratosphere, to change according to the closest ECMWF profile. Perhaps this would improve retrievals at altitudes well into the stratosphere (see Figure 9 discussion below).

I don't think I've ever seen a SAGE II water vapor profile shown up to 1 hPa. Please check and see if this is okay. In the Thomason 2004 paper the mixing ratio uncertainty is shown to be ˜100% at 45 km.

Figure 9 - The discussion of this figure focuses primarily on the structure at 90 hPa, which is due to the use of a constant hygropause. As I mention above, I think the authors should relax this constraint. However, unless I misunderstand the authors discussion, they don’t say anything here about the peak at ˜30 hPa which is also more clearly seen in the $>$45N profiles. This seems to coincide with the double-peaked structure visible in the Figure 4 contour plots at high latitudes (generally north of 50N). It would be nice to see some discussion of this, although, even better, I think it would be nice to see if it were possible to get rid of this structure altogether by allowing for a variable height hygropause a priori.

Figure 10 - I have serious objections to this figure. First, the SAGE II retrievals are not independent from HALOE, having been calibrated to match up with HALOE in the average (see Thomason et al.). Secondly, if the authors are serious about this idea they need to devote much more than a paragraph to it. Intercomparing different instru-

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ments with AMSOS as has been done here is certainly useful, but in order to address this issue there are many more cross-comparisons that need to be done. And using the uncertainty in the 183 GHz parameters as some kind of proxy for uncertainties in molecular parameters for these other instruments is not appropriate. What are the quoted uncertainties in the line parameters being used by the different instruments? I would recommend leaving this Section out and letting Milz et al. deal with it.

Figure 12 - I'm not sure whether comparison with single-level FISH and FLASH measurements even makes any sense given the vertical resolution of the AMSOS instrument and the vertical variations in the water vapor profile in the lower stratosphere. It is not immediately clear to me why it is reasonable to exclude the data at 6S (where obviously the AMSOS instrument doesn't have the vertical resolution to detect a dry layer which is supposedly there according to the ECMWF data), while it's okay to do this comparison elsewhere. Giving an overall comparison difference of 3.3% really doesn't make sense. The primary utility of a comparison between FISH and FLASH vs. AMSOS would be to show that they have similar variations along the flight path as AMSOS, but from studying Figure 12a this does not appear to be the case. If there is some correlation here it would be good if the authors pointed this out. Some very minor points:

"Splitten" is not a word. In equation (2) there is an "F" missing before the first (x,b)

Figure 6 is referenced before Figure 5.

The labeling of Figure 7 in the print version is confusing. On pg. 1666 it says: "Fig. 7b", but this is really 7e-g.

Pg. 1644, line 9: Misspelling of "instruments"

Please rewrite the sentence: "The criteria was chosen as large that we could find collocation pairs and the profiles do not originate from totally different air masses"

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 1635, 2008.