

Interactive comment on “Middle atmosphere water vapour and dynamical features in aircraft measurements and ECMWF analyses” by D. G. Feist et al.

D. G. Feist et al.

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Thank you for the detailed report. As I suggested to the other reviewer I will try to address the major points of criticism in the following and just fix the minor ones in a final revised version.

Insufficient description of the AMSOS data I understand that the reviewer would like to see more details on the AMSOS data set itself. The reason why the level of detail for AMSOS compared to the descriptions of the ECMWF data may appear disproportionate is simple. This analysis was originally done with AMSOS version 1 data which suffered from biases and other data quality problems. Shortly before the article was

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ready for submission, AMSOS version 2 became available and the whole analysis was re-done with the new version. As a result several sections of the article describing the data quality problems with version 1 became obsolete and were removed. That is why the description of the AMSOS data may seem a little short.

It is not the role of this article to provide a thorough validation of the AMSOS version 2 data. That part will be covered in depth by a separate publication that is currently being prepared by co-author Stefan Müller. But I could provide some additional information on AMSOS data quality like averaging kernels or error budget in a revised version for ACP.

I would still leave the detailed description of the ECMWF data as it is. Many of the details about the stratospheric water vapor model cannot be easily looked up in other publications. We had to go through a number of journal articles, grey literature and personal communications to collect the information presented here.

p.257, l. 15: Does UARS climatology show decreasing water vapor at the altitude where it shows up in the AMSOS data? At least for the polar latitudes (above 70 deg north) the UARS climatology for September has a water vapor maximum of 6.5 ppmv near 1 hPa. This is basically the same as the AMSOS maximum of 6.5 ppmv between 45 and 50 km. At other latitudes water vapor seems to decline above 0.5-0.3 hPa but this is very close to the top altitude of the UARS climatology.

p.257, l. 20: The ECMWF peak water vapor does appear to be at a lower altitude in profiles 64-186, so something right is going on here. Where does this come from if not the dry mesosphere? My guess is that it does come from a dry mesosphere but that it's not as dry as it is supposed to be. That is true, ECMWF also shows this water vapor peak. However, as we pointed out the operational ECMWF model of 2002 that was used for this plot had an improved water vapor photolysis scheme compared to ERA-40. Still

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there is not so much of a dry mesosphere in the ECMWF data. One might also argue that the polar water vapor peak is due to the older age of air in this region which allowed for more methane oxidation and thus more water vapor. As far as we know ECMWF has a working methane oxidation scheme and most likely gets the age of air right with some known biases.

p. 259, line 8 - I assume that the problem is not “lack of descent of mesospheric air”, but that the air that does descend is too wet Yes, I agree.

p. 259, line 22 - where does the conclusion: “Fig. 3 suggests there is little useful information from AMSOS below 18 km” come from? I think this was a conclusion drawn when AMSOS version 1 was used for this plot. Obviously we forgot to remove this sentence, thank you for pointing this out. In fact, all AMSOS plots in this article are plotted within the vertical range where we believe AMSOS data to be reliable. The vertical limits are determined by the a priori contribution and the total error of the retrieval. This should be pointed out more clearly in the description of the AMSOS data.

p. 261 The statement “PV is typically higher inside the vortex” than outside doesn’t make any sense to me We were also surprised by this situation and may be that is why the sentence sounds confusing. The geopotential height plot (8a) suggests that there is a vortex center between northern Scandinavia and Spitsbergen. However, the PV plot actually shows an area of low PV near this supposed center (as defined by geopotential height) and an area of high PV in the filament over the British Isles and southern Greenland. Since there seems to be some correlation between AMSOS water vapor and PV at this level, this structure may be real even though it is confusing.

Figures 8 and 9 - The low PV values surrounded by high PV values really are very strange, and the authors need to determine their cause. This would certainly be interesting but may be beyond the scope of this article. We have pointed out that given the way the stratospheric water model of ECMWF works it is not surprising that at high altitudes and inside the polar vortex there is little correlation with AMSOS measurements. The question how reliable ECMWF PV is at these altitudes should be answered by someone who knows more about the model. If I was forced to provide a more detailed analysis on the origin of air of these filaments I would have to drop these plots completely due to lack of time at my new position and now very limited access to ECMWF data.

p. 262 - it seems to me it would be more interesting to examine HALOE and POAM observations taken during the same years as the AMSOS flights As I have said before: the scope of this article is not to validate the AMSOS measurements. An intercomparison of POAM will be part of the upcoming validation paper currently being prepared by Stefan Müller. Of course, we have also checked HALOE vs. AMSOS a long time ago. Due to the limited number of HALOE profiles per day and the relatively short flight campaigns there is very little to compare. In seven campaigns there were only 2-3 HALOE measurements which were somewhat close in space and time to AMSOS measurements. Therefore the comparison to the UARS climatology makes more sense than comparing to HALOE directly.

p. 262- In particular, I'm surprised by the lack of a reference to the Deuber et al. JOURNAL OF GEOPHYSICAL RESEARCH, VOL. 110, D13306, doi:10.1029/2004JD005543, 2005 paper which includes the lead author of this paper as a co-author The mentioned article focuses on a comparison between in-situ balloon measurements and the ground based microwave instrument MIAWARA at Sodankylä, Finland, in the lower stratosphere. Only three of the water vapor profiles that are shown in the article (January 29 to February 6, 2004) were clearly taken inside the polar vortex. All of them show a

relatively flat water vapor maximum of roughly 6-6.5 ppmv near 30 km. This is similar but not identical to what AMSOS has typically observed but AMSOS has better vertical resolution. There is also no evidence of mesospheric descent in the MIAWARA profiles. But since AMSOS has only seen this rarely and always deep inside the polar vortex (e.g. profiles 64-186, Fig. 3, p. 278) this is not so surprising. AMSOS's performance at high altitudes is also considerably better than MIAWARA's because of the better signal-to-noise ratio and the shorter integration time (minutes vs. hours to days). Therefore I did not consider it necessary to cite this article.

By the way, AMSOS took part in the same campaign in Finland during 2004. Unfortunately, the measurements from that campaign are of very bad quality due to a defect of the instrument.

Figures 11 and 12 and p. 266 line 27 - Presumably the conclusions about the relationship of UARS climatology to AMSOS and ECMWF are based on a calculation of UARS water vapor as a function of PV (converted to equivalent latitude). So why not show this curve in the Figures instead of PV? No, the UARS climatology is already provided on an equivalent latitude grid and it is not straightforward to go back to PV from that.

Is there any reason why AMSOS might be having a problem at 850K? Not that we know. This is well inside the altitude range where the retrieval should work well.

A detailed description of Version 2 is particularly important since, as the text stands, the reader may conclude from the statement "better screening" that the authors are removing data that doesn't in some way look like the ECMWF analysis. This is definitely not the case. The "better screening" refers to a rigorous quality control of the measured spectra before they are integrated and profiles are retrieved. There has been no attempt to

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remove profiles that do not agree well with ECMWF data. I guess I should clarify this point in a revised version.

p. 270 line 5-14 - I don't understand this paragraph This was also confusing to the other reviewer and I hope to have clarified this issue in my answer to him. In fact, we do believe that we have seen upper tropospheric humidity even though the absolute values may be biased and there is no vertical resolution. But the AMSOS averaging kernel has a single weighting function that is sensitive to the total amount of water vapor between the aircraft and the hygropause. Outside the tropics the contribution by this weighting function is negligible but in the tropics it provides something proportional to the total UTH column above the aircraft.

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