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Atmospheric Chemistry and Physics Discussions



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

**Anonymous Referee #3**

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This is a very interesting paper, which analyzes a set of unique measurements and puts them in the context of the ECMWF analysis. My main concern with the paper is that the description of the AMSOS measurements is rather lost. The description of the ECMWF water vapor analysis is very good and useful and I'm not recommending that this be thinned in any way, but this is generally information that can be obtained from other sources. However only the authors can tell us about the accuracy of the AMSOS measurements and let us know where there might be problems. As far as I can tell there is no other paper describing the Version 2 AMSOS retrieval discussed here, so the authors really do need to spend a lot more time on this description in this paper.

248 line 5 - stratosphere is misspelled

249 line 6 - Please distinguish here between the large dehydration in the Antarctic, and the relatively minor dehydration observed in some years by in situ measurements in the Arctic.

line 15 - If you're going to say "large biases", suggesting that there is a serious problem here, then you need to give some numbers here (even if they are in the a reference).

The long discussion of ECMWF water vapor is very good and useful, but the authors should not lose sight that their primary contribution here is the AMSOS measurements. Unfortunately, this fact is nearly lost in both the Abstract and Introduction, which include just a brief mention of the measurements somewhere in the middle. Also, the AMSOS instruments description doesn't appear until Section 3, strangely after the ECMWF description in Section 2. It's almost as if the authors are trying to hide their measurements, and they really shouldn't since they are have gone through a lot of work to make these very nice measurements.

Figure 3 - The numbers of some of the AMSOS profiles are nearly illegible since they are written on top of each other. Perhaps the numbers of the profiles on the return leg could be written on top of this middle panel.

p. 257, line 15 - Does the UARS climatology show decreasing water vapor at the altitude where it shows up in the AMSOS data?

line 22 - 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. Please investigate this a bit further.

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.

line 22 - where does the conclusion: "Fig. 3 suggests there is little useful information



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from AMSOS below ˜18 km" come from? I don't see how I'm supposed to conclude this from either Fig. 3 in the paper or Fig. 3 in the supplement (and I'm not sure which of these the authors are referring to). This conclusion is especially confusing since we've just been shown (in the supplementary figure) that the ERA-40 analysis has problems. So where (and why) are we supposed to believe the analysis and where are we supposed to believe the AMSOS data?

p. 260, line 21- If there is no vertical oscillation in the UARS-AMSOS comparisons, and there is in the ECMWF-AMSOS comparisons, doesn't that suggest that the problem is with the ECMWF analysis, not with the AMSOS?

Line 25 - "expect" should be "except"

Line 27-29 - These lines seem very out of place here. They should be in the first few lines of the next section, since that's where these statements about problems with ERA-40 are addressed.

p. 261 The statement "PV is typically higher inside the vortex" than outside doesn't make any sense to me. Usually the vortex edge on a particular theta surface is defined by PV-value, so by definition PV inside the vortex must be higher than outside the vortex. Unfortunately the authors give no definition of what they mean by "inside the vortex", and I have to say that I'm somewhat surprised that there even is a vortex present at 1475K on March 9, 2000.

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. As the authors note, this is important for determining the origin of this air. Without knowing the origin of this air it is not possible to determine why the aircraft observe low water in this region and the ECMWF analysis does not. Is it because a narrow filament of water is being diffused away in the ECMWF analysis, or is it because the ECMWF analysis does not bring down low mixing ratios from the mesosphere. If this low PV really is caused by oscillations in temperature, then I presume that levels somewhat above the 1475K sur-



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face would look very different. Also, the authors really should look at another weather analysis for these dates.

p. 262 - The author mentions that it would be “interesting to examine recent MLS observations”. I certainly agree that this would be interesting, but it seems to me it would be more interesting to examine HALOE and POAM observations taken during the same years as the AMSOS flights. 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. This paper contains water vapor measurements taken at high latitudes in the NH, hence I think it might be useful for furthering the discussion on descent of mesospheric water vapor. There’s also the paper by Nedoluha et al. JOURNAL OF GEOPHYSICAL RESEARCH, VOL. 107, NO. D20, 8290, doi:10.1029/2001JD001184, 2002 which shows POAM water vapor measurements up to 50 km at high latitudes in the NH. This paper also shows the equivalent latitude of the Nash vortex edge at 500K, and shows that by the Nash definition there is still, in March 2000, a very small vortex remaining.

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?

p. 267 line 4-7 - This paragraph is confusingly placed between 2 paragraphs that discuss the 1475K results. Also, it would be helpful here to give approximate altitude values so that Figures 11 and 12 can be compared to the contour plots in Figure 4. Is there any reason why AMSOS might be having a problem at 850K?

p. 269 line 22 - The description of Version 2, how it improves on Version 1, and how these improvements were obtained through comparison with ECMWF data is not something that should just appear at the end of the conclusion. Please devote a Sec-



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tion to this. Again, the authors spend large amounts of text on detailed descriptions of the ECMWF analysis, and shortchange the reader on the description of their measurements and retrievals. 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.

p. 270 line 5-14 - I don’t understand this paragraph, especially the lines: “Given the rather broad vertical resolution of roughly 10 km in the AMSOS averaging kernels it was quite surprising to see features of the tropical upper troposphere appear in the retrieved AMSOS water vapour profiles. There was only qualitative agreement in this region but it has helped to understand how the AMSOS retrieval reacts to upper tropospheric humidity.” I think the authors are saying here that they’re retrieving upper tropospheric humidity, but I have seen little if any evidence of this. If they are, then please expound on this, even if the agreement is just qualitative.

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Interactive comment on Atmos. Chem. Phys. Discuss., 7, 247, 2007.