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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.

W. Lahoz (Referee)

wal@met.rdg.ac.uk

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Review of "Middle atmosphere water vapour and dynamical features in aircraft measurements and ECMWF analyses", by Feist et al.

General comments:

This is an interesting paper which provides a comparison between the AMSOS dataset and the ECMWF analyses, and provides an assessment of the quality of both datasets. The information on the ECMWF analyses will be of interest to middle atmosphere scientists. I think the paper is suitable for publication in ACP subject to the authors addressing the (relatively minor) comments made below.

I have one general comment, and several specific comments which I hope the authors will address and thus help improve the paper.

My one general comment about the paper is that I think the authors have slightly oversold the ability of the AMSOS data to detect filamentation. Looking at Fig. 9, I think that although it can be seen that AMSOS water vapour values change in the neighbourhood of the filament identified in the ECMWF analyses, it is not obvious to me that AMSOS detects the filament unequivocally. Furthermore, although the authors comment on the vertical structure of the filament in p. 265, as far as I can tell they do not show figures that depict the vertical extent of the filament (as, e.g., Lahoz et al. 2006 did). For these reasons, although I think the authors do show evidence that the AMSOS data can capture filamentation, I suggest the authors be more cautious with their comments or, alternatively, provide evidence of the vertical extent of the filament in AMSOS data.

Specific comments (page and line numbers refer to the ACPD paper):

P. 248:

L. 4: Stratosphere mis-spelt

L. 6: I suggest remove “well”

P. 249:

L. 14: Water vapour data from UARS MLS (e.g. Lahoz et al. 1994) also showed evidence of descent of dry air from the mesosphere. This paper is mentioned in this context later (p. 262)

P. 250:

L. 15: Please give references for the observed values of mean ascent rates

L. 19: Introduce PV acronym

P. 251:

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L. 17: “Are” repeated

P. 252:

L. 6: I understand that ice supersaturation has recently been discussed for the ECMWF model: Tompkins, A. M., Gierens, K., and Radel, G.: Ice supersaturation in the ECMWF forecast system, Quart. J. Royal Meterol. Soc., in press, 2006

L. 18-19: I suggest the authors make a comment on the impact the change of Q (6 ppmv to 6.8 ppmv from ERA-40 to operational analyses) may have on their results

P. 254:

L. 4-6: Is there robust evidence of the statement that moist anomalies have propagated from the equator to the North Pole?

L. 20: Inconsistent spelling of “drier” in the text

L. 21: State what the usual vertical gradient is

L. 25: Perhaps the authors could provide more details of why they think that atmospheric features are likely to be realistic because they appear in ECMWF data?

P. 255:

L. 1-4 (and page before): Do we need this detail to justify the use of V2 of AMSOS data?

P. 256:

L. 21-26: Although the differences between ECMWF and AMSOS data, especially in the upper stratosphere/lower mesosphere, are explained later, perhaps it would be useful for the reader to make a brief comment about them here

P. 257:

L. 1-8: Fig. 3 seems to show worse agreement between ECMWF and AMSOS than

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Fig. 4. Could the authors comment?

L. 8: What is the “special” situation?

P. 258:

L. 3, 6 and 14: It would be helpful if the authors could indicate the approximate pressure levels corresponding to the theta levels discussed

L. 28: What is the number of data points used to estimate the correlation coefficient?

P. 259:

L. 21: Why does the quality of AMSOS retrievals decline near the tropopause?

L. 25: Over what vertical range are ECMWF humidities generally within +/- 0.5 ppmv of the AMSOS humidities?

L. 27-28: Why are the operational analyses drier than ERA-40?

P. 260:

L. 8: What does “this” refer to?

L. 25: I think it should be “expect” -> “except”

P. 261:

L. 8: Descent of dry air from the mesospheric has also been seen in UARS MLS data (see, e.g., Lahoz et al. 1994)

L. 16: Extraneous “,”

L. 25: Fig. 8 suggests that there are other differences between ECMWF and AMSOS data besides the relatively dry AMSOS data at the centre of the polar vortex. Perhaps the authors can extend the discussion on the differences between these datasets?

P. 262:

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L. 9: Do the authors refer to Fig. 7 in the Randel et al. paper?

L. 23-24: Note that, recently, on a comment by Dudhia in a Juckes paper for ACPD, it is stated:

"We were aware from the beginning that MIPAS upper stratosphere H2O values were a couple of ppmv higher than climatology (which is mostly derived from HALOE data) and higher than predicted by methane oxidation. Vivienne Payne, in her PhD thesis, made a fairly comprehensive analysis of possible causes (spectroscopic database errors, calibration of the C band, non-LTE effects, representation of the high altitude column, and more) but ended up discounting all these. On the other hand, some early measurements from the Aura MLS and SCISAT ACE-FTS seemed to support the high H2O values (at least 'higher than CH4 oxidation') so we concluded that perhaps MIPAS values are correct."

Perhaps the authors could say something along the lines that while there is strong evidence in Lahoz et al. (2006) that Envisat (really MIPAS) upper stratosphere water vapour observations are of very poor quality - in particular, they are very noisy and show little agreement with other dynamical data, there is evidence from work done by Dudhia and others at Oxford that the wet bias in MIPAS upper stratosphere humidity data against climatology may be real

P. 263:

L. 12: Spatial mis-spelt

P. 264:

L. 2: I suggest remove "half"

L. 10: I suggest remove "clear" (see general comment)

L. 11: I suggest the authors indicate where in Fig. 9 there are signs of filamentation (see general comment)

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P. 265:

L. 1: The pattern is not clear in the PV field due to the colour scale

L. 17: Where is the evidence that the stratospheric vertical extent is similar in both datasets? (Have I missed something in the text?)

P. 266:

L. 20: Which data?

P. 267:

L. 17: I understand Monge-Sanz et al. is now accepted

P. 268:

L. 8-9: I think the authors should tone down their statements about filamentation or, alternatively, show more evidence supporting their statements (see general comment)

L. 16-20: The text suggests that the authors are also going to tell us what new things they have learnt. Is this correct? If so, is this spelt out in the conclusions?

P. 269:

L. 5-6: By ECMWF, do the authors mean both ERA-40 and the operational analyses?

L. 8: Please provide a reference for the statement about descent of dry air to 22 km

P. 270:

L. 5-8: Is this surprising? The broad averaging kernels suggest that “features” of the tropical upper troposphere could appear in the retrieved AMSOS water vapour profiles. Also, what specific “features” do the authors refer to?

P. 272:

L. 3: Should be “Jimenez”

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EGU

P. 273:

L. 4: Should be “frozen-in anticyclonic” air

P. 275:

Could the authors provide references to the campaigns mentioned in Table 1?

P. 280:

Fig. 5: It is a little bit difficult to distinguish between the blue and black dots. Could the colours be changed?

P. 283:

Fig. 8: The caption should indicate that the AMSOS data is indicated by the closed circles. Similarly for Figs. 9-10

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