

Interactive comment on “The ASSET intercomparison of stratosphere and lower mesosphere humidity analyses” by H. E. Thornton et al.

Anonymous Referee #1

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General comments

This paper details some of the results of the work carried out at several operational and academic research centres, within the EU funded ASSET project. The aim of the paper is to discuss the quality of water vapour analyses in the middle atmosphere, resulting from assimilating MIPAS water vapour retrievals, as well as other data sources. Over a period of a month (September 2003), water vapour analyses resulting from assimilation of MIPAS data using four different data assimilation systems (DAS's) were inter-compared and validated against independent data. This task requires a considerable effort, and the active involvement of scientists that are familiar with each considered

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DAS. This opportunity is rarely encountered, unless a specific international project is devoted to performing such a task. In my opinion, this well-written paper provides a valuable contribution to shed some light on the problem of improving our knowledge of the water vapour distribution and transport in the middle atmosphere, particularly in the UTLS and the stratosphere. However, I believe that the main value of this paper is that it shows there is still a lot of work to be done (both on improving parametrizations and data assimilation techniques) before we can significantly benefit from assimilating good-quality water vapour data. In summary, provided the specific comments detailed below are taken into account, I recommend this paper for publication on ACP.

Specific comments (S=section; P=paragraph; L=line)

Abstract: please add some detail on the scientific motivations for the paper (refer to, e.g., section 1, second paragraph).

Abstract, L3: State the funding agency of the ASSET project

S1, P5, L1: It would be good to anticipate here that a summary of the quality of MIPAS data is to be given in section 3.

S2: Are the MIPAS observations assimilated in a consistent way with the different DAS's? In particular, were the obs assimilated over (predefined or location dependent) layers, or over given (predefined or location dependent) pressure levels. Were the MIPAS averaging kernels considered for determining the width of the assimilation layers, when used?

S2.1, P1, L4: Is the analysis horizontal resolution really that different from the model horizontal resolution?

S2.1: All assimilated observations should be mentioned (e.g., standard meteo data, ATOVS, ...)

S2.2, P1, L1: Is the assimilation window the same as that of ECMWF?

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S2.2, P3, L3 "The BASCOE background error covariances are diagonal...": Do you mean block-diagonal, so that cross-correlations between different tracer error fields are absent? If so, what about (auto-)covariances? If instead you mean spatial covariances, how are then the increments spread?

S2.3, P2, L2 "The observation error covariance...": No observations have been mentioned yet. First introduce them and then discuss about their error characteristics.

S2.3, P2, L3 "In the forecast error covariance.." either the "forecast" or "background" error term should be used throughout the paper (or it should be clearly stated that they are equivalent).

S2.3, P2, L5 "off-diagonal elements...diagonal elements": As it is formulated, this is a trivial statement. I would suggest joining it with the later statement (P2, L11) "The proportionality factor...innovation vector"

S2.3, P2, L8 "on short time scales". Please define "short"

S2.3, P2, L9 "The representativeness error..." It is not obvious to me that with the chosen model resolution (1 x 1 degrees), there should be a significant representativeness error component for MIPAS observations. Do you have any evidence for it?

S3 Please add some introductory remarks on what this section is supposed to discuss

S3.5, P2, L2: "A zonal monthly mean...the latter is used here": This sentence should be rephrased.

S3.5, P2, L5: "intercomparison grid": It has not been defined yet.

S3.5, P2, L7: "Polewards...climatology": Does that mean the UARS climatology extends up to 80 degrees latitude in the stratosphere and up to 65 in the mesosphere? Is horizontal extrapolation physically meaningful? Please discuss its implications.

S4, P2, L1: "the observations are interpolated...different vertical resolutions": Is this vertical interpolation of different obs to fixed levels meaningful? Why not consider a set

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of layers where higher resolution obs are aggregated, while lower res obs are kept in their original form? It would be interesting to see whether your results change when you do that.

S4, P2, L3: "the observation must not differ from the UARS climatology by more than 120%": If I understand correctly, by doing that you prevent the comparison between the analyses and independent observations to be too "bad". This is acceptable in the case when only a small percentage of independent obs (i.e., other than HALOE) are beyond the chosen threshold (e.g., being affected by gross error). You should quote how many independent obs (in percentage) are rejected when using this threshold and discuss its significance (at least qualitatively).

S4, P2, L13: "Therefore, the grid chosen...and the analyses": When transport time scales are shorter than parametrization time scales, there may be a different sensitivity to resolution at high and low latitudes. This should be mentioned, if not also tested.

S4, P3, L4: "The increasing ECMWF wet bias...about 0.5 hPa": Aren't these obs automatically blacklisted? If not, are the corresponding innovation values equal to the obs values (i.e., presumably very large)? Please say a few more words on this, in particular about how you have worked out the 0.5 hPa threshold.

S4, P4, L2: "normalisation": It would be better to rephrase it and use the expression "relative difference" instead of "normalisation".

S5.1, P1, L6: "...indicates that...toward the Poles at these levels": Here and in the rest of the paragraph, the authors discuss about transport, as it can be inferred from a monthly mean analysis. In my opinion, this should be slightly rephrased in order not to give the impression that the authors have investigated the matter much more directly than they were able to.

S5.2.1, P1, L2 "The tropical water vapour...rising air": a reference is needed.

S5.2.1, P2, L8 "ECMWF and BASCOE have a small dry bias...of up to 5%": Here it

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seems the layer between 50 hPa and 100 hPa is discussed, while the authors instead refer to the 68 hPa level. Please clarify.

S5.2.1, P4, L4: spell Polar Stratospheric Cloud out

S5.2.1, P5, L4 "ECMWF and BASCOE ...greater than 40%" Use "up to 60%" instead of "greater than 40%". The authors should also comment on why BASCOE departs from ECMWF in the SH vortex (e.g., due to chemistry), and not just leave the explanations for later.

S5.2.1, last paragraph, L7 "low sulphate aerosol loading" please explain why in 2003 that was the case.

S5.2.2, P1: Please also discuss the large SAGE II dry bias (up to 60%) between 1 and 2 hPa.

S5.2.3, P1, L9 "If a grid point...a similar increment": please rephrase

S5.2.3, P1, L11 "The patchy structure...PV field": it should be possible to check this hypothesis by looking at the PV field from the relevant assimilation run.

S5.2.3, P1, L13 "ECMWF and BASCOE water...observations": how do you define success here? By being the results sufficiently similar to the expected WV field?

S6, P2, L10 "In these experiments...the continuity of the run": I am not sure to understand what the sentence mean and the justification for the choice of 0 and 12 ppmv.

S6, P3, L1 "The general Met Office...realistic background": Fig. 12 shows differences, not water vapour values. Please clarify.

S6, P4, L8 "Support for this assertion...constrain the analyses": Are the covariances mentioned in the sentence temperature covariances?

S6, P7, L7 "Ensembles are used...is very similar": This begs the question: why not to test ECMWF covs with the Met Office DAS? Please explain whether this was not done

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just for its impracticality, or there is a more fundamental reason.

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