

Interactive comment on “Evaluation of ECMWF water vapour analyses by airborne differential absorption lidar measurements: a case study between Brasil and Europe” by H. Flentje et al.

Anonymous Referee #2

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1 General Comment

The article presents remote measurements of water vapour performed by an airborne lidar during two flights that were part of the TROCCINOX european campaign. The aim of the article is to compare the observations with short-term forecasts produced by the ECMWF operational system (IFS) to assess the accuracy of the humidity field in the IFS.

The dataset used in this study is very original and the article provide valuable results on the accuracy of ECMWF water vapour fields. I therefore support that the article

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should be published. Nevertheless, a significant effort has yet to be made to improve the presentation of the article, as well as to clarify some technical details (see specific comments below), and I recommend that the authors fully take into account these comments before the publication.

2 Specific comments

p4407, l6: Define WV the first time it appears.

l7: WV instead of VW.

p4409, l9 and 13: use present time.

l27: The kind of interpolation performed has to be stressed. This is particularly important in the vertical direction because of the strong vertical WV gradients. (see also p4415, l23 below).

p4410, l7: the coordinates should better reads (23.5°S, 46.7°W) or (-23.5°N, -46.7°E).

p4410, l18: This threshold of 100 ppmv looks rather large to me. Typical WV mixing ratio in the lower stratosphere are always lower than 10 ppmv. Moreover, I find the use of the word "hygropause" not accurate all along the article. As far as one can judged on the figures, none of the represented fields (either lidar observations or ECMWF forecasts) display a reversal of the humidity vertical gradient in the upper troposphere, which should be the definition of the hygropause.

p4411, l2: How the stable stratification of the PBL is inferred from the dataset ? PBL has to be defined the first time it appears.

l4: Looking at figure 2a between 28°S and 25°S, there is a sharp gradient of lidar backscattering ratio at 1 km. Furthermore there are signs of vertical motions below 1 km (no stratification), whereas horizontal structures are more present between 1 and 3 km. I therefore wonder whether the PBL top is located at 1 km rather than the reported 3 km.

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I9: I guess Fig 2 is meant. The reference to the figure has to be made above in the discussion.

I23: The reported numbers are unclear. I suppose that the "0.1%" is the uncertainty in the estimation of the bias. If this is true, this has to be clearly stated (and does not need to be repeated twice in the same sentence). It would also be more clear if a reference to Figure 6 is made at this point.

I23: I furthermore don't understand why you need to fit the difference distribution with a Gaussian fit to retrieve the mean and standard deviation of the distribution. This is rather odd, since there is no reason why the difference should be Gaussian. This is particularly obvious for the second case (14 march, see figure 6b), and assuming a Gaussian shape tends to improve the reported accuracy of ECMWF. Finally, the standard deviation of the ECMWF-observation differences should be already stated here. They are quite large, which means that there could be large differences between the analysed and the true atmospheric WV, even though the analyses are not biased. This has to be clearly stated.

p4412, I6: Is there a reference for the reported values of the maximum altitudes reached by the convection during TROCCINOX ? Moreover, is the comparison here very relevant since the TROCCINOX campaigns mainly dealt with continental processes whereas the reported observations is obtained on the ocean ?

I9: the 16°N coordinate is in the white hole of Figure 3. Is there any explanation for this hole (apart from the airplane stop in Cape Verde) ?

I15: use present time.

I23: same remarks than p4411, I23.

p4413, I11: the Gaussian fit is absolutely not justified here. The tails of the difference distribution are all but Gaussian.

I12: I am not sure to understand why the two numbers 0.06 and 0.08 are different.

I19: I find that this is an overstatement. The ECMWF fields are actually unbiased, but large differences locally occur.

I26: IFS is repeated twice.

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p4414 l7 and 9: the transfer flights values look quite optimistic to me, and seem mainly result from the unappropriate Gaussian fit.

p4415, l18: the sentence "On both flights..." is unclear.

l23 and p4416, l1-2: This kind of information has to be reported much above in the article.

p4416, l10-13: These two sentences are unclear.

Fig 1: the two subfigures should be switched to respect the chronological order. Is it possible to superpose a latitude-longitude grid on the satellite image to help follow the discussion. The red dots in the 10 march picture have either to be deleted or to be explained in the legend. Replace "Down right" by "right" in the legend.

Fig 2: Write: (a) Backscatter ration R, (b) water....

Where do the iso-pressure contours come from in Figure b) and d). What does the "(30)" mean ? That 30 levels are used ? I don't see the PV contours in figure c). In the legend of Figure d), the numerator has to be put between parentheses.

Fig 4: There is no b) in that figure !

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 4405, 2007.

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