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Atmospheric Chemistry and Physics Discussions

Interactive comment on "$H_2{}^{16}O$ and HDO measurements with IASI/MetOp" *by* H. Herbin et al.

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Response to Reviewer 1:

> We thank the referee for his useful comments and suggestions. They have been taken into account in order to improve the paper. Our point-by-point answers to the referee's comments are found below.

1) These observations could play a significant role in understanding global evaporation and condensation processes as well as the transport of water vapor. However, before these data can be used by the hydrology community, there needs to be significant attention to the error analysis as follows: There is little description on how the errors and sensitivity of the HDO, H2O, and delta- D profiles are derived. Consequently it is hard to understand why the errors for HDO and H2O and the HDO/H2O ratio are much larger than those stated in previous research (e.g. Worden et al., 2006).

> The H2O and HDO errors are not much larger than those stated in previous research. Nevertheless, it is true for delta-D errors and this is due to the fact that we retrieve H2O and HDO independently. Then, we calculate the Delta-D profiles a posteriori and the uncertainty is calculated by the partial derivatives formula:

Thus, the errors that we obtain are largely overestimated as correlations are neglected.

Although the retrieval methods used for this analysis are discussed in previous research, this paper would benefit greatly by having a section showing the full description of the error budgets (using the Rodgers formulation of course) and how the errors are derived. Also importantly we need a better description of the inputs to those errors such as the covariances used for water and HDO, and the a priori used for H2O and HDO and the subsequent ratio. Without these descriptions it is highly challenging to assess the errors and vertical resolution of the delta-D profiles, the primary result of this paper.

> The retrieval was carried out without a priori correlation between the different isotopoloques, but the adjustment is performed simultaneously for H2O and HDO and we used variance-covariance matrices for HDO identical to those of H2O multiplied by the isotopic ratio. Nevertheless, as suggested by another reviewer, the discussion on the delta-D retrievals profiles has been removed and this will be the subject of a forthcoming study. Some sentences have been added in the text to clarify this.

Another key aspect of the error analysis that needs to be understood is whether the HDO and H2O errors are un-correlated. In principle, because HDO and H2O are retrieved simultaneously as stated on page 9272 Line 15, the errors should be correlated and therefore the error on the HDO/H2O ratio is not simply calculated by added the separate errors in quadrature. It seems therefore the expected error on the HDO/H2O ratio should be smaller than that shown in the figures. I am wondering if the large observed variability in the actual HDO/H2O ratio is due to un-calculated temperature errors and that the calculated errors are incorrectly derived. A more robust discussion

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on the expected and actual errors are necessary in order to better trust the distributions of delta-D and H2O shown in this paper. Another possibility for the larger than expected errors, as shown in the vertical profiles of delta-D, is that the “loose” constraint introduces significant non-linearity into the retrieval.

> See answers above.

Firstly, the authors should justify the nearly 100% a priori covariance used in the retrieval given that one might expect at most a 50% covariance using re-analysis fields. Secondly, the linearity of this constraint should be tested by trying multiple initial guesses and ensuring that they converge to similar solutions.

> A priori profiles and covariance used in the retrieval are built based on data over 2 months with days and nights data and averaged on 5 latitudinal bands. This involves that the constraint is “soft” enough in order to avoid a major contribution to the observed latitudinal variability. For instance, if we substitute the a priori information of the latitudinal band (+23, -23) by this one (+60, +23), the difference on the retrieved H2O and HDO profiles are smaller than the uncertainty. Moreover, in the revised paper we added relative difference figures. The latter show that, in this cases, the a priori information has a weak impact on the retrieved profiles.

Many of the fonts in the figures are un-readable when printed. Make additional figures if needed in order to increase the font size.

> The figures 3 and 4 have been modified and all the font sizes have been increased.

2) Specific Comments :

Pg 9270 Line 14 The variability of water is not related to the scarcity of observations.

> This line has been rewritten.

Pg 9271 Line 23 and alsoFigure 1. Is the RMS error really 2x10-6 W / cm2/sr / cm-1? That makes the residual error almost the same as the signal if I am not mistaken. Also

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Figure 1 is very difficult to understand. Please use absolute units instead (such as the above units) or at least brightness temperature. Also put the RMS error into a separate plot since the scales are different.

> Sorry, it's only a typo, the RMS error is 2x10-6 W/(cm2 sr m-1) and not cm-1. In this unity, for comparison, the signal is always around 1x10-3 W/(cm2 sr m-1).

Why are you using RMS error from retrieval and not a measured value of the noise? You need to state the reasoning in the manuscript. Is the noise not estimated from the calibration (pre-flight or otherwise?).

> Here the RMS error and the expected noise are very similar, but if you use the RMS which is bigger than the measured noise, these values can include another errors, for instance line parameters error which is not considerate elsewhere.

Figure 2: In my opinion the figure caption should just state what is in the figure. The statement that the errors from HNO3, CH4, etc. are small should be put back into the text. The statement about the temperature error being un-correlated and 1 K needs its own paragraph in order to better examine the impact of temperature errors on the HDO and H2O retrievals. More comments on temperature: I don't see where atmospheric temperature error is estimated. There is a statement in the Figure 2 caption that the error due to atmospheric temperature is uncorrelated with a magnitude of 1 K. As noted above, this makes me a bit concerned since temperature should have a significant impact on these retrievals, especially since the water lines almost over the entire IR are highly temperature sensitive. More discussion on this subject examining the impact of temperature error on the retrievals is needed in this paper.

> We thank the referee for putting this mistake forward. We have forgotten to show the temperature error on the Figures. This has been corrected. Moreover, the error part of the text has been revised.

Page 9273 Line 25. This comparison to the sonde data is in-adequate. One should

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show the sondes with and without the averaging kernel and a priori applied as compared to the IASI profile. The difference between these two should be a result of the measurement and interfering species error, as well as error that might result because the sonde and IASI measurement are taken at slightly different temporal-spatial locations. There might also be differences between the sonde and the IASI profiles as a result of biases in the sondes (see for example the AIRS and TES water vapor validation papers). If the authors cannot perform a rigorous comparison of the IASI H2O profiles with the sonde data (e.g. as in the AIRS and TES H2O validation) than they need to reference a water vapor validation study commissioned presumably by IASI, since that is one of main IASI products, or remove this section and only focus on the delta-D profiles.

> We agree with the referee that Figures 3 and 4 give a very limited aspect of the H2O validation, but this is not the purpose of this work. In fact, the goal of this section is to show the capabilities of the IASI measurements for reproducing the large scale features of the humidity profiles. The reasonable agreement gives some confidence for analyzing distributions on larger scale as in Fig 5. Moreover, as already said above, we have added a relative difference figure (with [(xa-xsonde)*2*100/(xa+xsonde)]; [(xretrieved-xsonde)*2*100/(xretrieved+xsonde)] and [(xLevel2-xsonde)*2*100/(xLevel2+xsonde)]) for each case. This illustrates that the retrieved profiles don't follow a priori profiles, they are not constraint by the variability and there are no systematic biases between sondes and retrievals.

Page 9275: This section is very confusing as it presents several results that are not well justified given the reported errors. For one, it's not clear to me that the observed vertical delta-D profiles actually shows lower values with increasing altitude. If the authors wanted to show this altitude gradient, I would suggest (1) swapping in a vertical a priori profile for delta-D using the approach described in Rodgers 2000 and then (2) averaging several nearby profiles together to reduce the random components of the error; this should give more confidence in the ability of these retrievals to capture

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the vertical gradient. One could use a similar approach of averaging to examine the temporal and spatial distributions.

> As already stated above, the discussion on the delta-D retrievals profiles have been removed and this will be the subject of a forthcoming study.

Page 9271: The first couple paragraphs are very interesting since they show that after the typhoon has passed there is significant humidity but low delta-D values. I should point out that this conclusion is arrived after significant averaging of the data, which should reduce the random measurement error and also any "pseudo-random" error from non-linearity in the retrievals. Consequently, it is probably a robust result, especially when considering the results shown in Worden et al., 2007 which shows similar features over cloudy tropical data. However, the last part of this section, showing the distributions of delta-d and H2O are not so clear, especially given the larger uncertainties in any given observation. The authors might consider averaging the data over a 1x1 degree grid and then showing the delta-D versus H2O distributions.

> The Fig 6 has been modified as suggested.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 9267, 2009.