Dear Referee,

Thank you for the review!
In the following we respond to your comments. You comment is in blue and our response in black:

COMMENT:
This paper shows comparisons between satellite-based estimates of delta-d and H_2O and those from an up-looking FTS. The H_2O estimates are further compared against nearby sonde measurements. The paper can be greatly improved with some modifications to the presentation but as far as I can tell no further change to the analysis is needed. In general I recommend adding some additional text on how the IASI retrievals from PROFITT differ from those in Herbin et al. 2009. I think the main difference is that the constraints used by Herbin are much looser than those used in the PROFITT code and consequently the uncertainties are much larger. In addition, as noted in the manuscript, it is critical that the comparison shows that the actual and calculated errors are consistent in order for the data users to be confident in these estimates. While these comparisons are effectively shown in the paper, they are shown in a roundabout manner; the paper could be improved if the authors explicitly state these comparisons in the abstract, text, and conclusions. E.g., add a statement along the lines of "The calculated error in the comparison between the IASI based delta-d estimates and the uplooking FTS based delta-d estimates is approximately XXX per mil. The actual errors (RMS between IASI and ground-based FTS) are YYY per mil." A similar statement for the water estimates would also be useful.

RESPONSE:
(1) Difference to Herbin et al. (2009):
Herbin et al. (2009) show that tropospheric HDO in addition to H_2O can be optimally estimated from IASI spectra. They retrieve the H_2O and HDO profiles independently and a posteriori calculate the HDO/H_2O ratios. However, the retrieved H_2O and HDO profiles suffer from different vertical sensitivity (compare the averaging kernels of Figures 4 and 5 of Herbin et al., 2009). Therefore, the H_2O and HDO profiles are not directly comparable (see also Rodgers and Connor, 2003). The a posteriori calculation of HDO/H_2O ratios from independently retrieved H_2O and HDO profiles leads to large errors, especially in the troposphere where even minor changes in the kernels significantly affect the retrieved H_2O and HDO profiles: in the troposphere the H_2O and HDO mixing ratios change over several orders of magnitudes and large changes often take place over rather small vertical distances.
We perform an optimal estimation of H_2O, HDO and HDO/H_2O, i.e. we make use of the HDO/H_2O a priori knowledge. Thereby our HDO/H_2O result is not affected by different HDO and H_2O sensitivities and our retrieval produces the best HDO/H_2O estimate for the given measurement. The constraint with respect to the HDO/H_2O ratio becomes possible by transferring the whole inversion problem on a logarithmic scale. Then $\ln(HDO/H_2O) = \ln(HDO)-\ln(H_2O)$ and we can easily introduce the HDO/H_2O constraint by an adequate occupation of the Sa matrix (a priori covariance matrix) that connect the HDO and H_2O states. For further details please refer to Schneider et al. (2006b).
Moreover, using a logarithmic scale is equivalent to assuming a log-normal a priori pdf, which better represents the true pdf of H_2O and HDO than a Gaussian a priori pdf (Gaussian a priori pdf is implicitly assumed when using a linear scale).
We will add such discussion to the manuscript.

(2) As suggested by the referee we will state the results of the error assessment and empirical comparison already in the abstract.

COMMENT:
Abstract: Line 1: Awkward grammar. Maybe instead say: "We present estimates of H_2O and delta-d derived from radiances measured by IASI: : :..

RESPONSE:
We will change it to "We present optimal estimates of H_2O and delD derived from radiances measured by IASI…"

COMMENT:
Line12: Replace 'quasi' with 'near'

RESPONSE:
Ok!

COMMENT:
Line 15: replace 'confirms' with 'is consistent with'

RESPONSE:
Ok!

COMMENT:
Page 16109 Line 6: Change 'the large potential of water isotopologues' with 'the potential of water isotopologues for assessing the distribution of hydrological processes'

RESPONSE:
Ok!

COMMENT:
Page 16109 Line 26: The word 'quasi' means 'virtual' or 'resembling'. I think you mean 'approximate' or 'near' instead.

RESPONSE:
We will replace "quasi" by "almost"

COMMENT:
Page 16110 Line 11: Replace 'validate them. Therefore we compare' with 'validate these calculated errors with comparison between'

RESPONSE:
Ok!

COMMENT:
Page 16110 Line 16: Define PROFFIT and PRFFFWD acronyms.

RESPONSE:

These are names of the inversion code and the forward model. PROFFIT is “Profile Fit” and PRFFWD is the “PROFFIT Forward Model”. We will add this explanations to the manuscript.

COMMENT:
Page 16113 Line 9: State whether the radiosondes are launched near Tenerife or are used to construct a gridded climatology globally. I am assuming Tenerife since you are comparing to the local FTS measurements but it would be useful for the reader to know as well.

RESPONSE:
As a priori profiles we use a climatologic yearly mean profile obtained from radiosondes launched in Tenerife.
A remark on a priori covariances: we also looked on radiosondes from Sodankyla (Finland), and Table Mountain (California). We found that the variability and interlevel correlation is very similar for all the different sites. If we account for the different “upper tropospheric altitudes” (for Tenerife we use 12.5 km whereas in the arctic it is about 7.5 km) we think that we can use the same a priori covariance on a global scale.

COMMENT:
Page 16113 Line 23: The reader might be confused by use of per mil in this description. Adding a statement that 80 per mil is approximately .08 near the surface would be helpful.

RESPONSE:
80 permil is exactly 0.08. We think that permil is a common technical jargon in isotopic studies.

COMMENT:
Page 16116: Add equation for gain matrix as the calculation of the derivative of x with respect to the radiance is not obvious to those not familiar with optimal estimation.

RESPONSE:
In general we think that the reader should consult details about optimal estimation in the cited textbook of Rodgers. Anyhow, we will add the formula of G in Eq. (7).

COMMENT:
Page 16116: Equation 7 is useful for examining the different error sources and how to add them together as well as examine their cross terms but does not describe the statistics of the uncertainties in “x” since it assumes that the parameter " is some bias term. Perhaps this is why some of the values in Figures 4 and 5 are negative? That would imply that you could add these terms up and they might offset each other since they are bias terms . However, a biased form for the errors is highly unlikely for temperature, emissivity, and interfering species because these uncertainties are derived from a noisy spectra, but a biased form is likely with the spectroscopy uncertainties. I would include the covariance form for this equation to Equation 7 and then plot the square root of the diagonal of this term; this is essentially what you already plotted except that this term will always be positive.

RESPONSE:
Theoretical error estimations depend on the uncertainty source assumptions. The assumed uncertainty sources are described in the text and in Table 2. What is shown in Figure 4 and 5 are the error patterns caused by the assumed uncertainty sources.

That is the reason why we call the Section "Propagation of uncertainty sources". The error patterns give the reader a very extensive overview of the characteristics (including correlation and anti-correlation between different altitudes) and the importance of an error caused by an uncertainty source.
For instance a positive temperature uncertainty/error in the boundary layer causes too large lower tropospheric H2O VMRs and at the same time too low middle tropospheric H2O amounts (see red line in the left panel of Figure 4). For a negative temperature uncertainty/error it would be vice versa: too low lower tropospheric H2O and too large middle tropospheric H2O.
These error patterns are the eigenvectors of the error covariance matrix (calculated as $(GK_p\varepsilon_p)*(GK_p\varepsilon_p)^T$, see also Rodgers 2000). They contain more information than just the square root of the diagonal of the covariance matrix. They document how the errors between different altitude layers are correlated. We don't see a reason for not giving this information. A reader not interested in the full information about the error can easily remove the information about error correlations by calculating absolute values of the error patterns. These absolute values are then the square roots of the diagonals of the covariance matrix that have been mentioned by the referee.
We will make this clearer by adding a brief explication on error patterns.

COMMENT:
Page 16116: Are these land or ocean scenes? For ocean, the emissivity uncertainty should be much smaller than 5%. Also, are you correlating the uncertainties (off diagonals of Sa) for emissivity? If the emissivity parameters are un-correlated this could introduce significant propagated error into your retrieval. (e.g., If the satellite sees land at one frequency one woudl expect it to see land in another).

RESPONSE:
In this work we only deal with ocean scenes. We plan land scene retrievals in the near future.
In this work the emissivity is not part of the retrieved state vector (the emissivity is fixed to 1.0). We perform here the error estimation for an emissivity uncertainty to indicate that it might become an important error source for land scenes. We will add this statement to the manuscript.

COMMENT:
Page 16118: Reference Worden et al. and Schneider et al. at the end of line 8.

RESPONSE:
Ok!

COMMENT:
Page 16118 Line 23: Pun intended? Add a comma after 'Naturally'.

RESPONSE:
Ok!

COMMENT:
Page 16119: This error description is confusing. As mentioned earlier I would calculate the expectation of equation 7 to obtain the expected covariance of these errors. Also, there is no

smoothing error term in Figure 6 which could lead the reader to conclude that the errors near the surface are dominated by random error whereas in fact the primary error in the estimate near the surface is essentially due to lack of sensitivity.

RESPONSE:
The sensitivity and the vertical resolution of the remote sensing system have been documented in great detail in Section 1 and illustrated in Table 1 and Figs. 2 and 3. The inherent vertical resolution and a not perfect sensitivity are not caused by any uncertainty source. Instead they present an intrinsic limitation of such remote sensing techniques. We think that mixing up limitations due to the nature of a measurement technique with errors of this technique should be avoided. These are two different things. Therefore, we will leave it as is and treat the sensitivity and vertical resolution of the remote sensing system separately and not mix it up with the errors.

COMMENT:
Section 4.3 and 4.4. This is in general a very nice comparison between the IASI H2O, sondes, and uplooking FTS. However, the way the section is written is somewhat confusing. For example, the comparisons between PROFFIT H2O and Sonde H2O as well as IASI delta-d and FTS delta-d will, in the absence of systematic errors, agree to within the uncertainties described in the previous section and any residual "smoothing error" (Equation 9) due to the limited vertical sensitivity of the measurements. (1) For the water comparisons it would be useful to see the calculated random uncertainties in Figure 9 to see if they agree with the actual random uncertainties (since smoothing error is removed in this comparison). (2) For the delta-d / FTS comparisons it would be useful to see the residual smoothing (Equation 9) PLUS the random errors along with the correlation plots or stated in a table. In principal these actual and calculated uncertainties should approximately agree (although the calculated should be less than the actual due to remaining systematic and "non-linearity" errors).

RESPONSE:
Ok, we will add an example error bar (describing the total estimated random error as depicted in Figure 6) to one measurement point in each of the panels.
However at the same time we would like to state that the numbers obtained by the theoretical error simulation should not be over-interpreted: The theoretical error simulation can give a good overview of the importance of different uncertainty sources but the obtained values depend on the assumed uncertainty sources, whose magnitudes are often not well known (interfering species, spectroscopic parameters, etc.).

COMMENT:
Figure 1: this figure will be more meaningful if the radiance residuals are in a separate figure (e.g. multi-panel figure) with the estimated noise over-plotted with the radiance residual. A key aspect of optimal estimation is error characterization and this characterization only applies if the retrieval converges to the noise level or you can account for any remaining radiance residuals between model and data.

RESPONSE:
Ok, we will create a multi panel Figure.

COMMENT:

Figure 2 and 3 and corresponding text: The discussion on the averaging kernels is somewhat subtle and may be lost on most readers: : :. The main point from Figure 2 (and 3) is to show where the estimate has peak sensitivity. I would just show either the columns or rows (my preference is row) of the averaging kernel as either one will effectively suffice to make your argument. However, if the purpose of showing both column and row is that you are also trying to show that there is significant cross-correlations in the estimate that must be taken into account then you might want to emphasize this point in the text.

RESPONSE:
We think it is better to show both, row and column kernels. The column kernels give a clear picture about the altitude regions in the retrieved profiles that will be affected by the measurements. The row kernels give a clear picture about the altitudes that most contribute to the retrieved profile. As can be seen in Figure 3 there is an important difference between column and row kernels. This characteristic of the kernels is difficult to see if we showed only the column or only the row kernels.

COMMENT:
Figure 3: The delta-d estimates described by Herbin et al. have much greater sensitivity (and error) to delta-d than you calculate. I think this is because they use a much looser constraint. You might want to point this out in the text.

RESPONSE:
Herbin et al. 2009 only show H2O and HDO sensitivities, no delD sensitivities. They do not perform an optimal estimation of delD and we believe that it is difficult to establish a reasonable delD sensitivity estimation for their retrieval setup.
We perform an optimal estimation of delD and thus can easily estimate IASI's sensitivity with respect to delD.

COMMENT:
Figure 4-5: Captions within plots are small, can you make bigger?

RESPONSE:
Ok!

COMMENT:
Figre 13-14: As noted earlier, add the calculated uncertainties (residual smoothing + random) in these figures or in a table.

RESPONSE:
Ok, example of typical error will be added in the graphics.

COMMENT:
References: You should consider adding the Rodgers and Connor Inter-comparison paper as a reference

RESPONSE:
We cite Rodgers 2000, where the problem of inter-comparing two remote sensing datasets has been described before the Rodgers and Connor paper. But we can additionally cite the Rodgers and Conner (2003) paper if you like.

Our best regards,
Frank Hase and Matthias Schneider