Reply to Referee #1

Thank you for your helpful comments. Our answers and replies are denoted in cursive lettering.

Specific comments:

P3921,L13: "each location is passed by each IASI instrument twice a day"
This is true to a first order but there can be relatively large area not sampled by IASI (until 50*50 km²). -
→ IASI samples the atmosphere almost everywhere on the globe twice a day.

This will be modified like advised.

P3922,L20: Could you also state the variability for δD in approximate permil.

We do not understand this comment. The variability for δD is given in parenthesis in %.

P3923,L14: Have the cross-kernels in the {humidity, δD }-proxy state have been multiplied by 12.5/0.08 like in Schneider et al., 2012? Maybe state that in the legends of the figures.

Yes, the multiplication is performed accordingly. The legends or captions of the figures will be modified.

P3925, L10->15: This value of $2x10^{-2}$ μW/(cm² sr cm^{-1}) corresponds to the IASI radiometric noise for the 645 – 1175 cm^{-1} region. Beyond this region the radiometric noise is significantly reduced, you should state the IASI radiometric noise in the spectral range you use. And if it higher you could comment on that.

That is right. The mentioned value is not valid for the spectral window. The correct value is $4x10^{-3}$ μW/(cm² sr cm^{-1}). The text will be modified saying that this is a rather conservative estimate of the noise level in the applied wavenumber region.

P3934->3935, section "delta d correlation":
This section was a bit difficult to follow. I believe there is a problem in the related figure. First, the panel "Kiruna" appears twice. I suppose "Kalsruhe" is missing. And second, the figures document a scatter estimated lower than the scatter observed but in the text it reads that the observed scatter is in excellent agreement with the estimations.

Right, there is a failure in the figure and "Karlsruhe" is missing. Please find the corrected Fig. 8 at the end of this reply.
The value declared as "estimated scatter" is the value we obtain from the calculation according to Eq. 10. For a better clarification P3933, L14 will be changed to:
"A part of this scatter is expected to be due to the differences between the IASI and FTIR averaging kernels (values obtained from the calculations according to Eq. 10 are given in the plot as 'scatter estimated'). However, most of this scatter is due to errors in the IASI and FTIR data."

P3933, L20 will be changed to:
"the observed scatter of 15–25‰ is in very good agreement with our IASI error estimations as depicted in the bottom panels of Fig. 5."

P3933, L15: "A part of this scatter is expected to be due to the differences between the IASI and FTIR averaging kernels.."

It I surprising that even after the smoothing you still expect significant contributions from the smoothing error. Especially if there is a big difference between the averaging kernels.

"… and much of this scatter is due to errors in the IASI and FTIR data."

After that you assume negligible error in the FTIR to assess a conservative estimate of the IASI δD random error. So finally the scatter is only due to error in IASI? I guess the total expected error between the two instruments is dominated by the error on IASI and thus yes the scatter between the two can be used as an estimate of the IASI error. It may be useful to precise how the different error sources (FTIR observational, IASI observational, smoothing and spatio-temporal mismatch) propagate into the total scatter between the 2 instruments. From that you can then say that FTIR error and spatio-temporal mismatch are negligible compared to the IASI error.

The FTIR kernels have less than 2 DOFS, i.e. the FTIR profiles represent only a rough approximation to the actual atmospheric state. Smoothing the FTIR profiles with the IASI kernels does not produce profiles with exactly the same characteristics as the IASI profiles. The remaining kernel effects can be estimated by Eq. 10 and it is as reported in the manuscript.

As stated by the referee the observed scatter has several sources: (1) spatio-temporal mismatch, (2) differences in the averaging kernels (even after smoothing the FTIR data with the IASI kernels), (3) FTIR errors, and (4) IASI errors.
Our calculation according to Eq. (10) suggest that the kernel issue is clearly smaller than the overall observed scatter. The FTIR errors as estimated in Schneider et al. 2012 are about 20‰, but after smoothing with the IASI kernels the get smaller (likely close to the errors estimated for column integrated, i.e., about by 7‰ as given in Table 5 of Schneider et al., 2012).
The scatter due to temporal and spatial mismatch is 10-20‰ (see Table 3) and of the same magnitude as the estimated IASI error and we cannot separate one from the other. Therefore, we can only perform a conservative empirical uncertainty estimation. Very likely the uncertainty in the IASI data is smaller than what we observe as scatter between the FTIR and the IASI data.

P3934, L1->L5: "However, in the lower and middle troposphere there is a strong correlation between the observed δD and ln(H_2O). This means that most of the δD variations can easily be predicted from H_2O measurements.

I think it would make more sense to state that the other way around. It is because δD can generally be predicted from q that there is a strong correlation between the two.

The δD data add scientific value to H_2O measurements if we can measure the part of the δD variations that do not follow the typical correlation between δD and ln(H_2O)"

You need to define what you mean by the typical correlation between δD and ln(H_2O). To my understanding you speak about the correlation found in Rayleigh distillation curves and I will assume so for the following. It might be useful for the readers to be a bit more explicit on this relationship between log(q) and δD. To a first order δD variations in the atmosphere can be interpreted in terms of a Rayleigh distillation. Rayleigh distillation predicts a progressive depletion of deuterium with a decrease of q. δD decreasing linearly with log(q) and thus the correlation is 1 between the 2 variables. However this typical correlation or relation varies spatially and temporally since it depends on the source term. This source term is characterized by a delta d value and q value in equilibrium with the surface temperature of the ocean. So in winter the Rayleigh distillation curve will be very different than in summer.

Ok, we make the statement the other way round and give an explanation for this. We will modify these phrases as:
"The lower and middle troposphere is mainly moistened by mixing with a humid airmass and it is dried by mixing with a dry airmass or by condensation. Both processes mean that delD decreases with decreasing humidity, resulting in a strong correlation between H2O and delD."

With typical correlation between delD and H2O we mean that in the lower/middle troposphere delD typically decreases with decreasing H2O. This correlation is very similarly observed at different sites. It is due to something like an "averaged combination" of Rayleigh and mixing processes (for a plot please see the Schneider et al., accepted, AMTD 2014).
Deviations from this typical or "averaged" relation between delD and H2O can be due to different source regions / differences in the equilibrium conditions at the source region (e.g., cold/warm ocean) and also because of unusual mixing conditions (different as usual mixing, which can be more or less mixing…).
We don't want to make a scientific interpretation of this "deviation" from the typically observed delD-versus-H2O relation, so our objective is not to further investigate these "deviation". Instead we want to perform a purely empirical analysis of the delD and H2O covariations as observed by IASI. We want to demonstrate that the IASI delD observation is something different from the IASI H2O observation, i.e., it contains information that is different from the information given by H2O only. We think such demonstration is a prerequisite for performing more scientifically oriented studies like interpreting the different delD-versus-H2O curves.

P3934->3935, section "the added value of δD":
This section needs more explanations to assess the added value of the observations or more prudence in the wording used. Now it is not clear on the figures that the unusual isotopologues observations highlighted demonstrate an "added value" (deviations from Rayleigh distillation curve). They show the same behavior in the δD-q space and that demonstrates that the delta d signal captured by both instruments is not coming from H2O. But the unusual isotopologues observations you highlighted could correspond to unusual Rayleigh distillation curves. And probably varies seasonally. For example the green points could correspond to a Rayleigh distillation curve for which the source term is specific humidity in equilibrium with a surface temperature of 300K while the red points might follow a Rayleigh distillation curve characterized by a source term in equilibrium with surface temperature of 285K. This is very nice to observe and that demonstrates that your delta d retrievals are actually dependent than H2O retrieval in the same way but the "added value" is not clear since it is not defined what you meant by that. If it is Rayleigh, you should plot the distillation curves and a mixing line. Mixing processes will give more enriched δD value than Rayleigh distillation for a same q, in that case the "added value" is straightforward.
It might be useful also to give the correlation between ln(H2O) and delta d.

Theoretically the different anomalies in the delD-H2O space (red versus green dots) can be due to different source conditions. We agree with the referee. But this is not the point of our paper. It is a technical paper and we simply want to demonstrate that our IASI delD adds information that is not in the IASI H2O data. We don't know if the observed delD-H2O anomalies are simply because there are different Rayleigh processes or if there are different mixing conditions. In our work we simply demonstrate that the IASI delD observations have good potential for investigating different water vapor source regions, different transport processes (mixing…), etc. However, these investigations should be subject of a more scientifically oriented paper.

P3935,L26: "(…) detects almost the same middle tropospheric (…)"
I agree with that statement in the case of the Izana comparison, but in the cases of Kiruna and Kalsruhe you compared δD at 2.4 km, that doesn't correspond to the middle troposphere.

The sentence will be corrected to take into account the different levels of comparability.

P3936, L1: "Furthermore, the scatter we observe between the two datasets excellently confirms our error estimations."
Please check the consistency of that statement with the related figure (Fig.8). If I understand it correctly, for Izana, the scatter estimated is 23.62 permil and the scatter observed is 11.85 permil. That would indeed confirm your error estimation. But it also indicates a significant error overestimation?

It is vice versa: the scatter observed is 23.62 permil and the scatter estimated from Eq. 10 is 11.85 permil (please note that the estimated scatter given here only refers to the scatter expected due to differences in the kernels, error estimations as given in Fig. 5 are not considered).

P3936,L10-L14: "Our study is made for three rather different geophysical locations: the subtropics, the mid-latitudes, and the polar regions. Therefore, we conclude that the results are globally valid and provide a first clear theoretical (…)"
The conclusion on "globally valid" might be too precipitate, from a global perspective, a tropical site is missing. You should state that with more prudence.

Yes, it is better to be more prudent. This will be modified to "[…] we conclude that the results are of an area-wide validity and provide […]". Thanks!

<u>Technical corrections:</u>
P3916,L26: clear instead of cleared
P3918,L22: characteristic
P3921,L7: Izana instead of Izan
P3925,L12: µW/(cm² sr cm-1) (the slash is missing)
P3931,L13: kernels (kernals written)
P3932,L9: The here observed ?
Figure 8: There is twice the same plot in this figure (Izana, Kiruna and Kiruna again) I guess Kalsruhe is missing. (and the y axes are not in the same units as x axes)

Thanks, will be adopted!

<u>Figure 8, corrected:</u>

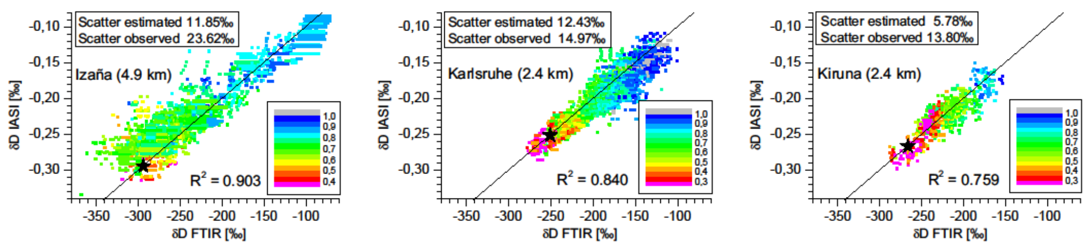

Fig. 8. Correlation between the IASI and smoothed FTIR _D data. Colors denote the individual IASI DOFs, the black star marks the apriori, and the black line shows the 1-to-1 diagonal.