

General Comments:

The manuscript by Frankenberg et al. presents first results of a retrieval of HDO/H₂O ratios from GOSAT data. This is an interesting topic and well suited for a publication in AMT. The GOSAT results are compared with TCCON data and corresponding SCIAMACHY measurements. The TCCON data are however not validated yet, and the SCIAMACHY data are from a different time period; therefore the comparisons are more qualitative and further validation is required. This is also stated in the manuscript.

The retrieval method is described quite briefly and needs some more explanation as detailed below. Especially, the retrieval makes some assumptions about the shape of the HDO profiles which is derived from ECMWF water vapour profiles. Since only the total column of H₂O and HDO is retrieved I assume that the shape of the profile is unchanged. In addition, the performed bias correction is based on a linear fit to ECMWF H₂O data (see Fig. 2). This assumes a proportionality between H₂O and HDO, at least for a dry atmosphere. In this sense the HDO retrieval results are somehow coupled to the assumed a-priori H₂O profiles. The sensitivity of the retrieval to these a-priori assumptions should be assessed in more detail to make sure that e.g. the observed HDO variability is not only a variation of the a-priori.

As this was also one of the primary concerns of Matthias Schneider, we added a small subsection explaining a sensitivity test using widely different a priori assumptions for about 400 soundings covering all seasons over Lamont. The differences in retrieved delta-D are a lot smaller than the single measurement noise and mostly random and within 10permil (1-sigma).

What is also missing is a more specific quantification of the errors of the used data sets (GOSAT, TCCON, SCIAMACHY).

This was also raised by reviewer 1 and we will add an error description for GOSAT. For the other methods (TCCON, SCIAMACHY), it is not that easy to provide uncertainty estimates, mainly because there is no real ground-truthing yet or the detector degradation makes error estimates hard. This can hopefully be achieved with the MUSICA dataset (and planned airborne campaigns) in the future.

The manuscript may be published after these issues and the more specific comments given below have been considered.

Specific Comments:

1. Section 2.1 Quality filtering:

(a) Several statistical quantities are used which are somehow related: χ 2, standard deviation of residuum, relative error in retrieved HDO column. Are the associated thresholds consistent with each other? Why is it necessary to

have more than one criterium which is related to the quality of the fit?

These factors are indeed somewhat related to each other but still provide useful information. Chi2 is purely a measure of whether the spectral residuals are within the expected noise and we agree that it may well be redundant with the std of the residuals. The relative error in HDO, however, is not directly related to this quantity but depends on a) geometry and total column amount of HDO as well as b) signal-to-noise-ratio of the spectrum. Soundings with a high relative error in HDO can be highly biased and we discard those.

(b) The cloud filter is based on a retrieved O2 column. This O2 column retrieval has not been mentioned before, and the spectral regions listed in Table 2 do not contain O2. Please give some more explanation on this.

The simple O2 cloud filter has been briefly described in Frankenberg et al GRL, 2011. Added this as reference now.

(c) p. 6363, l. 7/8:

'The CO2 and H2O ratios are from IMAP-DOAS retrievals using the weak and strong CO2 bands.' Please explain which spectral regions are used for these ratios.

We added a reference to the standard ACOS CO2 retrieval algorithm for the location of the CO2 bands used (which are identical for H2O).

2. p. 6364, l. 7/8:

As stated in the text, the multiplicative errors c_i are not quantified in the study. However, they occur in the formula for the bias correction. Please specify which values for c_i have been used. What is the impact of this choice on the results?

In fact, these have not really been used in the study, i.e. they were kept at "1". The impact for c_i is purely a shifting of delta-D. We will clarify that these corrections have not been performed as neither GOSAT nor TCCON retrieve absolute-calibrated delta-D values (e.g. the highest values well above 0 permil are unphysical and related to this bias).

3. Section 3:

As stated in the manuscript the TCCON HDO data are so far not validated. Is there any information on the quality of the TCCON HDO data? Is there also a bias, and has this been corrected? For the TCCON retrieval the HDO a-priori profile is derived from a scaled H2O profile (but different to the one used for GOSAT retrievals). What is the impact of this difference on the results?

The impact should be on the same order as for GOSAT. Since we didn't find a strong impact of the a priori profile, we consider our findings robust.

4. Fig. 4 and related text:

Could the similarity in the temporal variation of both TCCON and GOSAT data (Fig. 4) be related to the use of HDO a-priori profiles which are scaled H₂O profiles, i.e. is the observed variability (at least on this scale) only the variability of the a-priori profiles?

As explained above, we added a subsection covering the impact of the prior. The impact appears to be small, mostly random and definitely second order impact compared to the seasonal variability observed over Lamont.

5. p. 6367, l. 7:

Maybe one should mention here that the SCIAMACHY HDO retrievals are performed at 2.3 μm as this is used in the discussion on p. 6368 l. 8. Would it be possible to use the GOSAT spectral range (1.56 μm) also for SCIAMACHY HDO/H₂O retrievals?

Unfortunately not. SCIAMACHY has only 1.3nm spectral resolution in this band which doesn't allow for meaningful HDO retrievals in the 1.6 μm range.

6. p. 6367, l. 18–20:

'Over the oceans at higher latitudes, GOSAT takes regular Nadir observations and retrievals are so far only possible over low cloud layers having passed the simple filter...'

How large is the impact of the remaining cloudiness on the retrieval results? Shouldn't this result in too low H₂O and HDO columns?

Yes it will, which is why we ensure that still most of the atmospheric column is probed (by setting the O₂ filter but also a filter for the retrieved H₂O column). Since the averaging kernels close to the surface are near 1, both HDO and H₂O will be affected similarly and delta-D above the cloud top-height still measured appropriately. More studies would be needed to estimate the impact of multiple-scattering within the clouds as water vapor there might have a unique isotopic composition.

7. p. 6368, 3rd paragraph:

Is there also a bias correction performed for the SCIAMACHY data? What are the estimated errors of the SCIAMACHY HDO product? Could these be also reasons for the observed differences? Have there been comparisons performed between TCCON and SCIAMACHY HDO data?

For SCIAMACHY, this analysis has not yet been performed but the leading error source for SCIAMACHY is anyhow a constantly changing instrument line-shape (ice-built-up on the detector) as well as changes in bad or dead pixels. This makes this kind of analysis rather tedious (opposed to a high resolution spectrometer such as GOSAT).

8. p. 6369, l. 5:

The precision of 20—40 per mill of the retrieved single HDO columns has never been mentioned before (except for the abstract). The quality of the HDO data product should be addressed already earlier in the paper in a more detailed way. Some error contributions are mentioned in the text (e.g. errors due to a bias in the retrieved H₂O columns on p. 6366), but there is no real error estimate given for the GOSAT HDO product.

The precision error varies wildly depending on signal level (determining signal-to-noise), solar zenith angle and also strongly on the total column amount of water vapor (as delta-D is a relative measure, its precision error largely increases with a decrease in the water column amount). We will clarify this.

Technical Corrections:

1. p. 6361, line 17:

‘chose’ → ‘chosen’

Done, thanks.

2. p. 6362, l. 18:

‘we use of an effective pressure’ → ‘we use an effective pressure’

Done, thanks.

3. p. 6369 (Appendix A):

Although this is quite clear from the context, the used variables (γ , γ_{ni}

, γ_{air}

, γ_{H_2O} ,

γ_{ni}) should be explicitly defined.

Explained now.

C22404. p. 6370, l. 5:

Add closing bracket: $(1+4\text{VMR}(\text{H}_2\text{O})) \rightarrow (1+4\text{VMR}(\text{H}_2\text{O}))$

Done, thanks

5. Table 2:

Why is H₂O listed as interfering species in the H₂O retrieval?

This was an error, we removed it now.

6. Fig. 1:

(a) The variable F in the lowest panel is not defined (probably it is the radiance?).

Added explanation in the caption (yes, it is radiance)

(b) Please increase the size of the QQ plot; it is much too small. Please also add labels to the axes. Alternatively, remove the QQ plot as it is not explicitly addressed in the body of the manuscript.

We removed the QQ-plot

7. Fig. 6:

Please add some error bars to the data and/or give an estimation of the errors in the text.

We added errorbars to the scatterplot between ECMWF and retrieved H₂O (the previous figure) We had to multiply them by 2.5 to make them visible. We added the following in the caption: "The error-bars for the H₂O column retrieval are multiplied with 2.5 to make them visible (and are now roughly equivalent to the error in the HDO column retrieval, which is on average about 2.5 times higher than that for H₂O." For monthly means, the pure statistical error is so low that error-bars would be meaningless, hence we would like to keep this figure as is (estimation of errors is now explained in previous plot).

8. Fig. 7:

The colour for the 'raw' points should be the same as for the fitted line and the

corresponding text.

This seems to be a very minor detail and we would prefer to keep the plot as is.