

This manuscript, along with a manuscript by Bösch et al (2012) published at almost the same time, describes the first attempt to retrieve water vapor isotopologues (specifically the ratio of heavy water "HDO" to "H<sub>2</sub>O" (i.e.  $\delta D$ ) from spectra acquired by the Greenhouse Gases Observing Satellite. The manuscript focuses largely on applying the same (or at least a very similar) method used by the same lead author on SCIAMACHY spectra, and its feasibility for GOSAT. The results are compared to one TCCON site, at Lamont, Oklahoma, and qualitatively compared, with respect to seasonality and spatial variability, to the previous SCIAMACHY results, which are from a different time period.

The viability of retrievals of HDO/H<sub>2</sub>O ratios from GOSAT are indeed an interesting topic - one which should be addressed and is well suited to publication in AMT. This publication shows that these retrievals are indeed feasible, and as such I recommend its publication after addressing some issues, detailed below. It should also be cautioned, however, that as also noted by the other referees (Dr. Schneider and AnonyC2384mous Referee #2), further validation is required, as is the characterisation of the retrieval characteristics, sensitivities and uncertainties. It is my belief that this should be a priority for future work with GOSAT retrievals, as well as for the "validation" product from TCCON. The fact that this is required before scientific interpretation of the data is undertaken should be emphasised further in the revised manuscript. Nevertheless, demonstrating the agreement between GOSAT and TCCON retrievals is at the very least a good step from which to expand these studies.

**We highly appreciate this review and will ensure that the "work-in-progress" status (which we are fully aware of) of the current manuscript is clearly stated.**

#### General Comments

- When comparing seasonal averages between GOSAT and SCIAMACHY, it would be nice to have some estimate of the magnitude of the expected interannual variability. While I appreciate that the precision of the satellite retrievals lends itself to the need for averaging lots of data (e.g. to seasonal averages over multiple years), can the authors make any estimate of the IAV from their retrievals, which do indeed cover multiple years in each case?

**For SCIAMACHY, this is a little hard to say as instrumental degradation certainly plays a role. Also, IAV in cloud cover and filtering can play a certain impact. To minimize the impact of IAV on the comparison, we chose multi-annual means on purpose as they better reflect a kind of climatological state which can be compared against other datasets even if the actual time-periods don't overlap. Some sort of estimate of the IAV can be observed in the Lamont time-series (IAV is apparent but is of a much smaller amplitude than the overall seasonal cycle itself).**

- The differences between GOSAT and SCIAMACHY are tentatively attributed to the differences in vertical sensitivities. The averaging kernels for GOSAT and

TCCON are presented in the paper, and that from SCIAMACHY is discussed, but not referenced, or even better, presented for comparison with GOSAT.

We will add a specific reference (currently, averaging kernels for SCIAMACHY HDO are not computed for each sounding, so we can't fully take this into account anyhow).

- ECMWF is used as "truth" south of 60° S in order to derive a bias correction for the GOSAT retrievals. Are there any references or information concerning the relative accuracy of the ECMWF product used for this purpose? These would also enable some level of uncertainty to be attached to the bias correction.

We observed correlation with ECMWF in those regions (despite the very low column abundances). If these correlations are real, then the fit of the intercept should be less susceptible to potential biased in ECMWF (as long as they are multiplicative). However, the bias correction method is certainly crude and we hope to be able to exchange it in the future (and will more clearly state it).

#### Technical and Specific Comments

- p6358, L2: demonstrate feasibility → demonstrate the feasibility

Done

- p6358, L9: this is not necessarily a depletion, right?

We would like to keep it as is as atmospheric vapor is normally depleted in HDO (relative to SMOW of course).

- p6359, L3-5: is this really worthy of a separate paragraph?

You are right, not really (merged it with the preceding paragraph).

- p6360, L13: At least H<sub>2</sub>O and possibly HDO should be separated by either parentheses or surrounding commas.

Done for both.

- p6361, L17: chose → chosen

Done

- p6361, L18: where 1500 % as 1σ ensures - this needs clarification. "Where a choice of 1500% as 1σ ensures" or similar.

Done

- p6361, L22: viz??

Does it seem inappropriate here? We could say “namely” if needed but keep it as is for now.

- p6361, L24-25: allows to retrieve sounds funny. I suggest changing to either "allows retrieval of" or "allows us to retrieve"

Changed to “enables the spectral fitting of weak HDO and H<sub>2</sub>O lines”

- p6362, L21: with → to

Done

- p6363, L1: how is the relative error in the retrieved column calculated?

Absolute retrieval error / retrieved HDO

Would like to keep as is as it makes the bullet points very busy.

- p6363, L5,L6: while this is somewhat clarified in the following sentence, it would be nice to have these ratios better defined (e.g. is it CO<sub>2</sub>(weak)/CO<sub>2</sub>(strong) used to define the CO<sub>2</sub> ratio? What are the spectral ranges used for these retrievals). One could also add the details of the retrieval windows used here to Table 1.

We now refer to the retrieval windows, but only by citation (i.e. same used as in full-physics retrievals). The ratio technique will be presented in more detail in a paper in preparation (which we can't yet cite).

- p6365, L6: up-looking? Would this not be better expressed as upward-looking? In fact, better still would be to describe TCCON as ground-based direct solar measurements, because up(ward)-looking could mean direct vertical measurements, e.g. emission measurements or measurements from scattered-radiance only.

Very true. The most unambiguous should be direct sun, i.e. we changed to “...pointing directly at the sun's center”

- p6365, L10-12: on what is this empirically-derived HDO/H<sub>2</sub>O profile shape based?

On MkIV ground-based and balloon measurements

- p6365, L14-17: while the GOSAT measurements take place at close to local noon, H<sub>2</sub>O variations occur relatively rapidly, so despite the assimilation of sonde measurements, the a priori profile could be significantly different from the true profile. It is also likely that the viewing geometry relative to the location of the sonde profiles could play a role. So the comment that the a priori profile is relatively close to the truth is perhaps misleading.

This can of course well be but using assimilated met fields at least minimizes the risk of starting too far away from the prior. As discussed in a separate section now, the impact of the prior is relatively small.

- p6365, L25-26: This sentence needs rephrasing. Perhaps something like "The columns are not bias-corrected, as the differences would not be visible at this scale." I also don't believe that the fact that one cannot see the differences is a reason to not bias correct here - you have introduced and derived the bias correction, why not apply it rather than have to explain why it isn't applied?

Touché. This is a typical example where laziness leads to a short-term bonus but creates more work in the end. We changed the figures and removed the sentences.

- p6365, L27: Indeed this coincidence criterion is quite lax for H<sub>2</sub>O. One obviously needs a relatively lax criterion to get good statistics for the comparison, but can you say anything about whether the distribution is more-or-less normal within the 6 by 6 degree colocation gridbox on the time scales examined (monthly averages)?

We were thinking of the criteria again and reduced it to +-2 degrees and now also interpolated TCCON data to the GOSAT overpass time (before, we took just averages over ALL values of the day, which was definitely not the best way to do it). We now also plot the mean and median for each month and there is definitely a difference, pointing to non-gaussian behavior. The same is true for TCCON itself, i.e. since deltaD itself behaves non-linearly, its distribution (be it in space or time) is most often not Gaussian. We changed plots (and text) accordingly.

- p6365-6366: It is interesting to see the IAV between all 3 years here. How does the  $\delta D$  correlate with this? Or to put it another way, is there any apparent link in a plot of  $\delta D$  vs H<sub>2</sub>O?

We haven't yet investigated it but it is certainly interesting (i.e. to be continued).

- p6367, L15-17: some more details about to what extent the filter criteria are relaxed, and what "hardly affected" means, would be appreciated.

Changed to: For GOSAT, we relaxed the filter criteria (esp. the CO<sub>2</sub> and H<sub>2</sub>O ratio, for which the maximal deviation from unity was changed to  $<0.0075$  and  $<0.5$ , respectively) to achieve coverage in the tropics. Most regions, however, are

not affected by this relaxation (e.g. the Lamont comparison is virtually unchanged by this choice).

- p6367, L19: why is Nadir capitalised?

Changed

- p6367, L23-24: presumably you are not surmising that the  $\delta D$  variability itself is lower in SCIAMACHY, but rather the retrieval variability is? As such, this would be nicer phrased as "variability is somewhat lower in the SCIAMACHY retrievals."

Changed

- p6368, L5-7: what exactly do you mean by "very reliable"? I assume that this is with respect to the filter criteria, but that future work would require assessment of potential biases between glint and other observation modes to confirm this reliability.

This was meant more in relation to SCIAMACHY, which doesn't have a dedicated glint-mode and also has trouble over snow&ice as surface albedos are very low. Changed text to "Also noteworthy from a more technical viewpoint is the fact that GOSAT retrievals are more reliable than SCIAMACHY over the oceans due to the glint viewing geometry. In addition, this is the case over land over some snow and ice areas such as Greenland because both snow and ice have much lower surface albedos in the  $2.3\ \mu\text{m}$  range, making SCIAMACHY retrievals over these surface types problematic"

- p6368, L27-29: double use of impact is confusing.

One changed to "effect"

- p6369, L3: not necessarily depletions, or at least  $\delta D$  is not defined as being a depletion.

Changed to "relative abundances of deuterium" even though we prefer the term depletion (how would you call the delta-D value)?

- Acknowledgements: NASAs is missing an apostrophe.

LaTeX issue, thanks.

- Table 2: I believe a number of the TCCON retrieval windows have the wrong widths presented, specifically that for all the HDO windows and for the 6401.15 H<sub>2</sub>O window, the full width is presented as the width either side of the centre wavenumber. E.g., I don't believe the 6377.40 cm<sup>-1</sup>HDO window is 100.40 cm<sup>-1</sup>

wide. I assume that H<sub>2</sub>O should not be listed as an interfering species for the H<sub>2</sub>O retrievals.

You are right, we accidentally reported full-widths as half-width for this column (changed).

#### References

Bösch, H., Deutscher, N. M., Warneke, T., Byckling, K., Cogan, A. J., Griffith, D. W. T., Notholt, J., Parker, R. J., Wang, Z.: HDO/H<sub>2</sub>O ratio retrievals from GOSAT, Atmospheric Measurement Techniques Discussions, 5, 6643–6677, doi:10.5194/amtd-5-6643-2012 <http://www.atmos-meas-tech-discuss.net/5/6643/2012/>, 2012

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