We would like to thank the reviewer for the helpful comments on the manuscript. A point-by-point response to the comments can be found below (reviewer's comments in bold, and our responses in italics)

Re (1) I think that the response of the GOSAT retrieval on atmospheric H2O and HDO variations should be better documented:
In your current manuscript version you examine how uncertainties in the used HDO a priori profile affect the retrieved HDO/H2O product (Section 2.2, test (5)). But what about uncertainties in the used H2O a priori profile? The ECMWF profile certainly differs from the actual atmospheric profile. Sure, this will not affect your a priori HDO/H2O profile (per definition in your retrieval setup), but it might affect the retrieved HDO/H2O values. This effect might be even more important than uncertainties in the HDO profile, since the H2O signatures are much stronger than the HDO signatures.

We have included uncertainties in the a priori H2O and HDO profiles in our sensitivity study. This is now included in the summary figure 3 as well as in the text of section 2.2.

Moreover, your test (5) assumes that there is a very small HDO error in the lower troposphere and a large error in the upper troposphere. I understand that this is motivated by the fact that your used SMOW HDO profile typically differs like this from the real atmospheric HDO profile, but what happens if there is a large error in HDO in the lower as well as the upper troposphere? I would very much acknowledge if you documented in detail how the GOSAT HDO/H2O product responds to atmospheric H2O and HDO variations. Here we are actually talking about the averaging kernels. However, the problem is that HDO/H2O kernels are complex and cannot be calculated in a straight forward manner (I guess that this is the reason why you decided to address this issue by your sensitivity tests). Other authors present at least kernels for H2O and HDO, which is also not sufficient, since there are cross-correlations between H2O and HDO. In Schneider et al. (2012) we have very recently presented a method for calculating proxies for the HDO/H2O kernels. This method can also be very helpful for your GOSAT paper. The fundamental idea is that you transfer your atmospheric {H2O, HDO} state (or {ln[H2O], ln[HDO]} state) into a {0.5*(ln[H2O]+ln[HDO]), (ln[HDO]-ln[H2O])} state. Both states are equivalent for representing the atmospheric H2O and HDO composition. The advantage of such transformation is, that we now have states that are very good proxies for the δD state, (ln[HDO]-ln[H2O]), and for atmospheric Humidity levels, 0.5*(ln[HDO]-ln[H2O]). In this context please have a look on APPENDIX (I). It shows the kernels in the {ln[H2O], ln[HDO]} and {0.5*(ln[H2O]+ln[HDO]), (ln[HDO]-ln[H2O])} states calculated for a retrieval that is very similar to your retrieval (scaling of prescribed H2O and HDO profiles). Among others we observe a strong Humidity interference on δD. These shown simulations are for retrievals of IASI spectra. However, I can well imagine that for GOSAT the humidity interference is even stronger, due to the difference in the H2O and HDO line strengths. I would like to recommend that you complement your GOSAT retrieval paper with a short additional Subsection showing such Humidity and δD kernels for a typical GOSAT retrieval. These kernels would then allow discussing the δD sensitivity of your retrieval as well as the importance of the Humidity interferences in a comprehensive manner.

We have now included figures for the column averaging kernels for the H2O and HDO retrieval. In addition, we have included the cross-correlations between HDO and H2O and vice versa. These figures show that the normalized averaging kernels for H2O and HDO are similar with values close to unity from the surface up to roughly ~700 hPa for the bright

surfaces. For the 5% albedo scenario, the values for the kernels can be much smaller. The cross-correlations between HDO and H2O are typically small compared to the values of the kernels except for the 5% albedo scenario.
The proposed transformation in states that represent deltaD and humidity is an interesting approach and we might consider such an approach in the future. However, we believe that this is outside of the scope of this manuscript and we have decided not to include such an additional transformation. We do not believe that such a transformation would change the major findings of this study as the observed cross-correlations between H2O and HDO are modest. Furthermore, our retrievals are carried out in linear space and not in log-space so that the proposed transformation cannot be directly applied to our retrievals.

(2) I have some concerns about your GOSAT / TCCON δD inter-comparison:
My concerns are based on the fact that the GOSAT and the TCCON retrieval setups have some similar shortcomings, e.g., (1) both scale prescribed H2O and HDO profiles taken from analysis data (ECMWF and NCEP, respectively), (2) both apply a fixed HDO/H2O profile shape, (3) both fit very weak HDO lines, compared to much stronger H2O lines. Therefore, the observed agreement between GOSAT and TCCON might be partly artificial and caused by a common artifact in both datasets. The problem is that δD variations are very small and already small artifacts can significantly affect the results.
A common artifact might be that both the GOSAT and TCCON retrieval suffer from similar humidity interferences on δD. In order to avoid that such artifacts affect the inter-comparison, I suggest eliminating all the variations in the retrieved δD that are correlated to ln[H2O] variations. Furthermore and very important: these δD residuals document that δD actually adds information to H2O. The part of δD that varies in parallel to H2O provides no additional information and its measurement is of limited scientific value. In this context please have a look on APPENDIX (II), where the δD residuals are called "δD deviations" (deviations from a typical δD-ln[H2O] curve). The APPENDIX (II) shows comparisons between our ground-based NDACC FTIR, space-based IASI, and surface in-situ δD products. Furthermore it discusses the advantages of inter-comparing "δD deviations" instead of raw δD data. I encourage you to make a similar inter-comparison between the "δD deviations" of GOSAT and TCCON. Such an inter-comparison would be significantly more convincing than your current study and it could show that your δD data add effectively new information to the ECMWF humidity data.

The GOSAT and TCCON retrievals are of very different nature due to the different spectral resolution and spectral windows and the very different observation strategy (direct sunlight vs nadir sounding). We expect that common artefacts might be primarily due to the use of the same spectroscopic tables, which we point out in the manuscript.

For the retrieval of a new species from an instrument, as is presented in the manuscript, I think that we need to firstly compare (or validate) the parameters that we retrieve, which is the HDO/H2O ratio (or δD), rather than the δD deviations. A study of a higher-order parameter such as δD deviations could very well be part of a follow-on study but as we discuss later, we are not convinced that comparisons of the described δD deviations between GOSAT and a ground-based instrument will work well.

As described in the paper, the GOSAT retrievals of the HDO/H2O are very noisy and GOSAT can certainly only observe larger scale pattern such as continental effects after some spatial and temporal averaging. At the same time, the GOSAT sampling pattern is very coarse so that we only obtain few GOSAT soundings near a ground-based site within a month. To obtain a useful fit between δD and the log of the H2O

vmr, we would need to consider a dataset of several months together with the use of a large spatial co-location criterion (which has been shown to make some potential problems). I would expect that the correlation between δD and the log of the H_2O vmr will change in space and time so that this is unlikely generating a baseline for subtraction from the GOSAT δD dataset. Furthermore, the total columns observed by GOSAT will also average over a variable correlation between δD and the log of the H_2O vmr with height which should make matters ever more complicated.

The focus of this manuscript is to demonstrate the feasibility of the HDO/H_2O retrieval from GOSAT and a study of δD deviations from GOSAT will be no simple matter and we do not want to add such a study to this manuscript.

(3) TCCON δD as reference?

TCCON has been established for highly precise measurements of total column averaged CO_2 and CH_4. Due to TCCON's importance for CO_2 and CH_4 there is a strong collaboration between the GOSAT and the TCCON communities. I fully understand that this collaboration is now expanded to HDO/H_2O. However, I would like to remark that the TCCON δD product can hardly serve as a reference for validation studies, since the spectral range covered by TCCON is not optimal for measuring HDO/H_2O. The H_2O signatures of TCCON are strong, but the HDO signatures are rather weak and significantly interfere with strong H_2O and CH_4 lines. These differences in the H_2O and HDO signal present severe difficulties for obtaining HDO/H_2O at high quality. It is certainly interesting that TCCON has the potential to measure HDO/H_2O, but if you need a HDO/H_2O reference, I honestly think that you should work with the HDO/H_2O data produced from NDACC spectra. These NDACC mid-infrared spectra are of higher spectral resolution than the TCCON near infrared spectra and the corresponding H_2O and HDO signatures are of similar strength and well-isolated from signatures of interfering absorbers. These are strong advantages for obtaining a high quality HDO/H_2O product. Furthermore, within the project MUSICA there have already been significant efforts for theoretically and empirically assessing the quality of the NDACC δD product (by the way: a very complex work that is still ongoing, some recent results are shown in APPENDIX (II)).
I would like to suggest adding NDACC HDO/H_2O data to your inter-comparison study. The MUSICA NDACC δD product is freely available for the scientific community and for ten globally-distributed NDACC sites (Schneider at al., 2012). Among the six sites you use in your inter-comparison study there are three sites with MUSICA data: Ny Alesund, Bremen, and Wollongong. So adding a comparison to MUSICA data would not require too much additional work.

We appreciated the reviewer's comments and we believe that the fact that TCCON itself is not calibrated is well acknowledged in the manuscript. A comparison of our retrievals against the NDACC data, especially the MUSICA NDACC δD product is certainly a logical next step and we envisage carrying out such comparisons next. This is our first study of the HDO/H_2O retrieval from GOSAT and we prefer not to add such an additional comparison to this manuscript as this would significantly lengthen (and delay) the current manuscript (which already covers a range of different aspects of the retrieval and the comparisons with ground-based instruments).

Page 6645, line 24:

Actually Worden et al. (2012) use a very similar retrieval recipe as Schneider and Hase (2011): fit of a broad microwindow, simultaneous fit of interference absorbers, fine model atmosphere gridding, etc. The main differences are that Worden applies TES spectra instead of IASI spectra (slightly higher spectral resolution) and that he uses a much weaker HDO/H2O constraint. This **weaker constraint is the main reason for the increased sensitivity as reported in Worden et al. (2012). It increases the theoretically estimated sensitivity of the system, but at the same time it increases the uncertainty of the product. With IASI we could also achieve a similar sensitivity as Worden et al. (2012) for TES, if we used a weaker constraint. Please consider this when you describe the possibilities of TES and IASI.**

We have removed the statement about the sensitivity of TES from Worden et al. (2012).

Page 6648, line 18:
A correlation (or constraint) between HDO and H2O is implicit in your retrieval setup. If you calculate the a priori HDO profile from the a priori H2O profile (ECMWF) by assuming SMOW throughout the atmosphere you assume rather unrealistic HDO/H2O profile shape. This is a strong constraint for your HDO/H2O retrieval. Since it is implicit in your retrieval setup and since there is no flexibility it can be called a hard constraint. What you describe at the beginning of page 6656 is an effect of this constraint.

This is correct. Since the SWIR retrieval from GOSAT does not contain information on the vertical distribution of HDO itself, an a priori profile shape needs to be assumed, which will impose a 'hard' constraint on the retrieval and, as discussed in the manuscript, this assumption can introduce errors in the retrieved HDO.
We have included a paragraph describing the retrieval setup (section 2.1) to discuss this.

Page 6651, line 2:
Here Schneider et al., (2006) is not a good reference. Better would be to cite pioneering works in the field of atmospheric δD profile measurements and modeling, e.g., Ehhalt (1974) and Joussaume et al. (1984).

Done

Page 6657, line 27:
MUSICA will provide a high quality tropospheric δD dataset using ground- and space-based remote sensing and in-situ measurement techniques. Concerning the ground-based remote sensing component, MUSICA works with NDACC and not with TCCON spectra. A quality assessment for TCCON δD is no MUSICA task, but of course we would be happy to support respective

We have changed the text to reflect this.