With this cover letter, we will submit the revised manuscript (acp-2010-399) entitled, "***Relating tropical ocean clouds to moist processes using water vapor isotope measurements***" for publication in Atmospheric Chemistry and Physics. We would like to thank referees for the careful and constructive reviews. Based the comments from the referees, we have made changes of the manuscript, which are detailed below.

**Reply to the evaluation by the first referee (Numbers are listed by the referee)**

Numbers are listed by the reviewer.

***A. Specific comments***

**1. Comment**

Varying TES sensitivity:

Figure 1 nicely documents how the HDO/H2O sensitivity of TES varies with the cloud regime. For boundary layer clouds the TES sensitivity is significantly shifted towards higher altitudes (compare Fig 1(c) with 1(a) and (b)). Therefore, I don't think that one can directly compare the respective delD values collected in Table 1 for the different cloud regimes: the delD typically observed for clear sky and nonprecipitating clouds (-164 and -168 permil, respectively) and the delD typically observed for boundary layer clouds (-184 permil) may only differ since one compares airmasses from different altitudes. In order to clarify this point it would be very useful if the authors estimated how the varying sensitivity can affect the retrieved HDO/H2O. I suggest performing the following estimation:

a. Use a set of possible HDO and H2O profiles (e.g. from the NCAR CAM as mentioned in Worden et al., 2006.).
b. Simulate retrievals of HDO/H2O for the different cloud regimes, i.e. for the different sensitivities depicted in Figure 1 (by smoothing with the respective avks).
c. Document how the simulated HDO/H2O retrievals differ for the different cloud regimes and compare this to the actually observed differences. Are there similarities? To what extent can the varying TES sensitivity explain the observed correlation between cloud regime and delD?

**Answer:** In this work, we first document how the TES HDO sensitivity varies with different cloud environments. We agree with what this reviewer mentioned that one can not directly compare the respective δD values presented in Table 1 for the different clouds regimes because one compares airmasses from different altitude. This reviewer pointed out it would be very useful to estimate how the varying sensitivity can affect the retrieved HDO/$H_2O$ profile. As s/he suggested, we have decided to add an Appendix in the manuscript to explain the procedure in detail. New following citation is added in the manuscript.

Rodgers, C. D. and Connor, B. J.: Intercomparison of remote sounding instruments, J. Geophys. Res.-Atmos., 108(D3), 4116, doi:10.1029/2002JD002299, 2003.

**2. Comment**
Varying TES sensitivity and systematic error sources:
The error produced by a systematic error source (temperature, line parameters, etc.) will depend on the TES sensitivity, i.e. the error produced by a systematic error source depends on the cloud regime. The authors correct the systematic errors according to Equation (2). However, if their assumed error value (6% for HDO) is wrong, their correction method will not work perfectly and there will remain an error whose magnitude will depend on the sensitivity of TES. The authors should estimate how the uncertainty of their systematic error assumption can affect the retrieved HDO/H2O for the different sensitivities. Suggestion: The authors should test if a correction with a different systematic error produces the same results (difference between delD for clear sky and boundary layer clouds of 20 permil). So far they have assumed a systematic error of 6% for HDO. It would be interesting if a correction with an assumed error of 4% would significantly change the results collected in Table 1 (is the difference between delD for clear sky and boundary layer clouds now significantly smaller or larger than 20 permil?).

**Answer:** The reviewer concerned about our assumed correction factor by a systematic error source (6% for HDO suggested by Worden et al. (2006)). S/he suggested to calculate using different correction factor. Worden et al. (2010) estimated that the TES HDO profiles should be corrected downwards approximately 4.1% and 5.6% for Version 3 and 4 of the data, respectively based on comparison of TES and *in-situ* measurements (We used Version 3 of the data in this work). As the reviewer suggested, we did calculations to figure out how different correction factor change the results presented in Table 1. The difference between δD for clear sky and boundary layer clouds in Table 1 is –20‰ since we assumed a systematic error of 6% for HDO. When we assume a different systematic error, for example, 4%, as the reviewer and Worden et al. (2010) suggested, the difference between δD for clear sky and boundary layer is –24.7‰ (–150.0‰ for clear sky and –174.7‰ for boundary layer clouds), which is approximately 5‰ larger than what we reported in our manuscript. This makes sense because the bias correction was done based on the sensitivity of the measurement. The bias correction for the clear sky is more sensitivity than for the boundary layer clouds because of the selected altitude regions (850-500hPa). The differences of bias correction between 6% and 4% are –14.7‰ and –9.6‰ for clear sky and boundary layer clouds, respectively. However, we have decided to use our original correction factor because the manuscript by Worden et al. (2010) is still in review. New citation is added in the manuscript.

Worden, J., Noone, D., Galewsky, J., Bailey, A., Bowman, K., Brown, D., Hurley, J., Kulawik, S., Lee, J., and Strong, M.: Estimate of bias in Aura TES HDO/$H_2O$ profiles from comparison of TES and in situ HDO/$H_2O$ measurements at the Mauna Loa Observatory, Atmos. Chem. Phys. Discuss., 10, 25355-25388, 2010.

**3. a. Comment**
Consider the changing sensitivity when discussing the results in Section 3:
- Section 3.1 and Figure 3:
The difference in delD as measured by TES for 500 hPa level between 20-10°S and 10-20°N could also be an effect of a changing TES sensitivity. At 20-10°S TES encounters

almost clear sky while at 10-20°N the sky is almost completely covered by clouds. At 20-10°S the TES HDO/H2O 500 hPa data will represent much lower altitudes (and thus airmasses that are less depleted in HDO) than at 10-20°N. It is not clear whether the observations of Fig. 3 reflect atmospheric variations or variations in the sensitivity of TES. This difficulty should be discussed. The sensitivity estimation according to item (1) and (2) of this Comment list would be very useful for such a discussion.

**Answer:** We agree with the reviewer. S/he is concerned about the sensitivity of TES water vapor isotope such that the observations in Fig. 3 do not represent atmospheric variations. As a matter of fact, we were trying to qualitatively link MODIS clouds images with TES clouds observations to corroborate TES observations by showing a case study. As Fig. 1 shows, regardless of sky conditions, there is some sensitivity in 500 hPa such that we showed the variations of water vapor and its isotope in that altitude although there is an issue of sensitivity.

**3. b. Comment**
Consider the changing sensitivity when discussing the results in Section 3:
- Section 3.2 and Figure 4:
There is some correlation between the frequency of boundary layer and/or precipitating clouds and low delD values. For instance: at the cyan arrows in Fig. 4 delD is particular low and at the same time TES encounters a lot of boundary layer and/or precipitating clouds. This should be discussed, since it might be that the low delD values are mainly due to the observation of airmasses from higher altitudes (changing TES sensitivity for these clouds). So far the authors disregard that the changing TES sensitivity may be a reason for the observed low delD values.

**Answer:** S/he is concerned we should discuss the changing TES sensitivity may be a reason for the observed depleted δD values over the Eastern and Western Pacific. We agree with the reviewer that this issue must be addressed. As discussed in earlier question raised by this reviewer, changing TES sensitivity for boundary layer clouds and precipitating clouds could affect the longitudinal distribution; this issue is addressed in the Appendix as discussed earlier with the explanation that any bias errors in our estimate would increase the magnitude of the variations.

**4. Comment**
In the first paragraph of Section 2.2 there is a statement that should be changed:
„[…] DOF […] that are larger than 0.5. This criterion assures that the HDO/H2O estimate is sensitive to the true distribution.“: This sounds a bit misleading, because for estimating the true vertical distribution you would need a DOF value as large as your number of model atmosphere levels. With a DOF of 0.5 you won't be able to estimate any detail of the vertical distribution of HDO/H2O. I would just write that for a DOF of 0.5 TES can detect column averaged delD with a precision of 15 permil (Worden et al., 2006).

**Answer:** This reviewer correct in that a value of 0.5 DOFs can be difficult to interpret. We have used the suggested text by the reviewer as it is appropriate. Now the paragraph becomes "Note that only the HDO DOF's are used as a sensitivity metric because the sensitivity of the TES $H_2O$ estimate will always vertically overlap that of the HDO measurement sensitivity but not necessarily the reverse. In addition, as noted in Worden et al. (2006), the DOFs for the HDO/$H_2O$ estimate is degenerate and therefore is not calculated.

**5. Comment**

HDO or HDO/H2O?

Page 17410, line 19 and line 22: Why are the authors only talking about HDO sensitivity? I guess they mean HDO/H2O. Please clarify. I was also confused when reading page 17411, line 6: I assume that they mean „DOF for HDO/H2O“ instead of „DOF for HDO“. Please clarify.

**Answer:** In the analysis discussed by Worden et al. (2006), we round that the averaging kernel matrix for the HDO/$H_2O$ estimate was degenerate; consequently, the DOFs is undefined. However, wherever there is sensitivity to HDO, there will be sensitivity to $H_2O$ and therefore the sensitivity to HDO will be the limiting sensitivity to the HDO/$H_2O$ ratio. We put this sentence in the paragraph in order to answer the previous question.

**6. Comment**

The correction of the bias according to Equation (2):

H2O and HDO are jointly estimated, i.e. H2O and HDO are not independently retrieved (there are cross elements of A in Equation 13 of Worden et al., 2006). An error in the HDO line strength would affect the retrieved HDO as well as the retrieved H2O profiles. Why is H2O not corrected? Do you only use the avk for the HDO state in Equation (2)? Not the full joint HDO-H2O avk? No HDO-H2O cross elements? What are the effects of such an approximation? Please clarify this.

**Answer:** We expect spectroscopic errors in both HDO and $H_2O$. However, the bias correction is only applied to HDO and not to $H_2O$ in our work because we cannot distinguish between the two; this issue is also noted in Worden et al. (2010). Since we cannot distinguish between a spectroscopic error in HDO from $H_2O$ as indicated in earlier question, we find that it is simpler to apply the correction only to HDO because the sensitivity of the HDO estimates overlaps the $H_2O$ estimates but not the reverse. It is quite likely that the correction could take a more sophisticated form as indicated by the referee, but it is not possible with the current validation data sets to determine the level of this sophistication.

**7. Comment**

Interpreting Figures 5 and 6:

When comparing models to remote sensing measurements we have to account for the limited vertical resolution of the remote sensing data (see avks of Fig. 1). An effect of this limited resolution is the so-called smoothing error. According to Worden et al. (2006) It is about 15 permil (statistical 1 sigma uncertainty) for column averaged delD. How much is it for H2O? Can't the smoothing errors of delD and H2O already explain the few points that lie outside of the two yellow curves?

In this context I do not really understand how you calculate the 95% areas (blue solid lines in Fig. 5). At the EP and Af locations (Fig. 5(c) and (d)) the blue 95% probability

lines seem to include very dry air for which no measurement points exist: there are no measurement points with H2O < 1 g/kg, but the blue 95% probability line reaches these low values!
Furthermore, we have to consider that the 95% probability area covers all data up to an error of 2 sigma, i.e., up to a delD uncertainty of 30 permil. This should be considered when you discuss these "outliers", e.g., on page 17419, line 20-24: are these outliers statistically significant if you take into account the 2 sigma errors of delD and H2O retrievals?

**Answer:** In the lower troposphere, TES water vapor showed a small (<5%) moist bias in the retrievals (Shephard et al., 2008). The point of this reviewer is correct. The sentence is confusing such that we decide to modify it. Now it becomes "The Af region is relatively dry, but relatively enriched in heavy isotopes, which indicates frequent mixing with fresh oceanic vapor or vapor from evapotranspiration and less subsequent precipitation then the other regions. (Flanagan et al., 1991; Worden et al., 2007; Brown et al., 2008).".

Shephard, M. W., Herman, R. L., Fisher, B. M., Cady-Pereira, K. E., Clough, S. A., Payne, V. H., Whiteman, D. N., Comer, J. P., Vömel, H., Miloshevich, L. M., Forno, R., Adam, M., Osterman, G. B., Eldering, A., Worden, J. R., Brown, L. R., Worden, H. M., Kulawik, S. S., Rider, D. M., Goldman, A., Beer, R., Bowman, K. W., Rodgers, C. D., Luo, M., Rinsland, C. P., Lampel, M., and Gunson, M. R.: Comparison of Tropospheric Emission Spectrometer nadir water vapor retrievals with in situ measurements, J. Geophys. Res.-Atmos., 113, D15S24, doi:10.1029/2007JD008822, 2008.

**8. Comment**
Some confusion:
On page 17419, line 1-2 the authors write: „The Rayleigh distillation and mixing models for clear sky and nonprecipitating clouds show several similar characteristics (Table 2)". But Table 2 lists measured not modelled data. Please clarify this.

**Answer:** Table 2 shows mean values of water vapor amount and δD from each region observed by TES. The sentence, "The Rayleigh distillation and mixing models for clear sky and nonprecipitating clouds show several similar characteristics (Table 2).", seems to be confusing to potential readers. We reworded the sentence to make it clearer. Now it becomes "TES observations for clear sky and nonprecipitating clouds, compared to the Rayleigh distillation and mixing models, show several similar characteristics (Table 2 and Fig. 5).

**9. Comment**
Summary: Page 17421, line 10-13: I don't agree with this statement. In the current version of the paper the authors document that the TES sensitivity depends on the different cloud types, but they do not account for this varying sensitivity. This statement would be true if they addressed item 1-3 of this Comment list.

**Answer:** Since we address the questions raised by this reviewer, we will not change our conclusion in Page 17421, line 10-13. The sentence is “This analysis accounts for the capability of the TES instrument to distinguish between different cloud types and on the sensitivity of the TES water vapor isotope measurements, which also depend on cloud optical properties.”.

***B. Technical corrections***

1. Comment: - Page17418, line 10: „in Fig. 5“ should be changed to „in Figs. 5 and 6“

Answer: Fixed. Now it becomes “These distributions are shown for each cloud type and different tropical regions ***in Figs. 5 and 6***.” in page 17418, line 10.

2. Comment: - Page 17421, line 11/12:
„and ON the sensitivity“ should be change to „and FOR the sensitivity“

Answer: Fixed. Now it becomes “This analysis accounts for the capability of the TES instrument to distinguish between different cloud types and ***for the sensitivity*** of the TES water vapor isotope measurements, which also depend on cloud optical parameters” in page 17421, lines 10 to 13.

3. Comment: -Table Captions (Table 1 and 2):
please mention the considered partial column in the Table captions. I guess if not otherwise mentioned it is 850-500 hPa, right?

Answer: In Table 1, the values are between 850 and 500 hPa if it is not indicated. We indicate they are between 850 and 500 hPa in Table 2.

We appreciate the comments from the reviewers. Thank you for reviewing our manuscript.

Sincerely,

Jeonghoon Lee

## Appendix. Bias Error Characterization for Different Cloud Regimes

In this appendix, we estimate the bias error for the comparison between two distributions of remotely sensed estimates in which the averaging kernels for these estimates are moderately different but have overlapping sensitivities. In particular, we estimate the bias error for the comparison between the mean of the clear-sky δD values and the mean of the boundary layer cloud δD values.

If each estimate ($\hat{\mathbf{x}}_c$ for clear sky and $\hat{\mathbf{x}}_b$ for boundary layer clouds) is close to the true state, its dependence on the choice of constraint vector, constraint matrix and true state can be described by the linear estimate (Rodger, 2000; Worden et al., 2006).

$$\hat{\mathbf{x}}_c = \mathbf{x}_a + \mathbf{A}_c(\mathbf{x} - \mathbf{x}_\mathrm{a}) \quad \text{(A1)}$$

$$\hat{\mathbf{x}}_b = \mathbf{x}_a + \mathbf{A}_b(\mathbf{x} - \mathbf{x}_\mathrm{a}), \quad \text{(A2)}$$

where $\mathbf{x}$ is the "true" full state vector, $\mathbf{x_a}$ is the constraint state vector (the HDO and $H_2O$ profiles), and $\mathbf{A_c}$ and $\mathbf{A_b}$ are the averaging kernels for the two estimates (e.g., averaging kernels representative of clear sky or boundary layer cloud conditions). We intend to investigate the bias error in a comparison of these two estimates if the averaging kernels overlap but are moderately different. This can be accomplished by subtracting Eq. A1 from Eq. A2 and assuming the true state is the same for both estimates. This will account for how the different vertical resolution, as described by the averaging kernel matrix, affects the final estimate; this comparison follows Rodgers and Connor (2003) for comparisons of two remotely sensed measurements of the same air mass in Eq. A3:

$$\hat{\mathbf{x}}_b - \hat{\mathbf{x}}_c = (\mathbf{A}_b - \mathbf{A}_c)(\mathbf{x} - \mathbf{x}_a) \quad \text{(A3)}$$

For comparison of clear sky to boundary layer clouds, we construct an average of the averaging kernels for boundary layer clouds ($\mathbf{A_b}$) and clear sky ($\boldsymbol{A_c}$), respectively. The difference between two profiles ($\hat{\mathbf{x}}_b - \hat{\mathbf{x}}_c$), averaged over the pressure region of interest (for example, 850 and 500 hPa) is our estimated bias error.

We assume two HDO/$H_2O$ profiles (5% and 2.5% depleted compared to the *a priori* constraint) as the true state vector ($x$) to see the impact of vertical resolution on the estimate and in order to account for the expected depletion of the air parcels above the tropical boundary layer clouds relative to the *a priori* constraint.

Fig. A1 shows simulated results of HDO/$H_2O$ profiles for two different cloud regimes. Between 850 and 500 hPa, the bias errors ($\hat{\mathbf{x}}_b - \hat{\mathbf{x}}_c$) are 12.7‰ and 6.4‰ for 5% and 2.5% depletion of true state vector, respectively, which means that boundary layer clouds are biased toward the *a priori* constraint by 12.7‰ and 6.4‰, respectively. Based on this analysis, the mean difference isotopic difference between clear sky and boundary layer clouds could be larger (by up to 12.7‰) due to the impact of vertical resolution, that is, we are underestimating the difference.

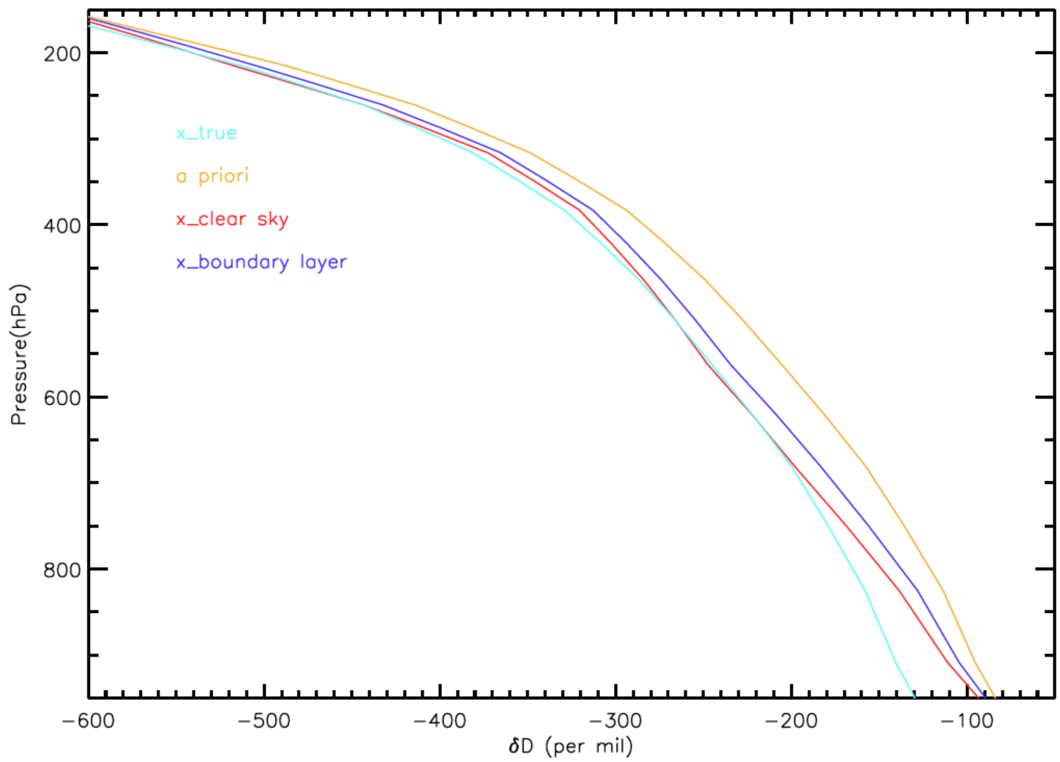


**Fig. A1.**

An example of simulated retrievals of HDO/$H_2O$ profiles for the different cloud conditions. 5% depleted HDO/$H_2O$ profile was assumed as the true state vector (**x**) in this calculation.