General Comments:

This study uses H2O and HDO/H2O data produced from TES (Tropospheric Emission Spectrometer) measurements. TES offers for the first time free tropospheric water isotopologue data with good spatial coverage and measured during several years. TES can measure for different cloud regimes, which can offer interesting research opportunities. The paper examines the co-variations of tropospheric water vapour, its isotopic composition (HDO/H2O), and cloud types. The authors discuss the tropospheric processes that can best explain the observed water vapour concentrations and HDO/H2O ratios.

Remote sensing of HDO/H2O is very difficult and it is important to properly take into account the characteristics of the data when applying it for research. Otherwise the remote sensing data could be misinterpreted. The authors show that the TES sensitivity with respect to HDO/H2O is different for clear sky and cloudy sky, where it furthermore depends on the cloud type. They document that for different cloud regimes TES HDO/H2O data are representative for different atmospheric pressure (or altitude) levels. Unfortunately, the authors do not really discuss how this varying sensitivity may complicate the interpretation of the TES data.

The use of tropospheric water isotopologue data for investigating tropospheric water cycle processes is a very promising and emerging research field. The paper gives a nice overview of HDO/H2O and H2O measured typically for different cloud regimes and at four different tropical regions. In my opinion it can serve as a nice reference for future studies that use space-based water vapour isotopologue data for atmospheric research. I recommend a publication in ACP after addressing the following Specific Comments.

Specific Comments:

(1) 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:

- Use a set of possible HDO and H2O profiles (e.g. from the NCAR CAM as mentioned in Worden et al., 2006.).
- 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).
- 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?

(2) 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?).

(3) 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.
- 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.

(4) 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).

(5) 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.

(6) 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.

(7) 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?

(8) 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.

(9) 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.

Technical Corrections:

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

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

-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?