Summary and general comment:

Tropospheric HDO/H2O observations offer a promising opportunity for investigating the sources, sinks, and processes affecting the tropospheric water vapour distribution. Progress in understanding this water cycle is a scientific priority for better understanding and predicting climate change.
Currently, quasi-global space-based observations of tropospheric HDO/H2O are becoming available. In order to fully exploit this data for research purposes it is essential to document their accuracy/bias and precision. The paper under review is about documenting the bias of HDO/H2O data observed from space by TES on Aura.

Since there are no tropospheric reference HDO/H2O profiles available the authors use HDO/H2O in-situ measurements performed at the Mauna Loa Observatory during a campaign in October/November 2008. They estimate the TES bias by two different approaches. First, they construct an HDO/H2O profiles from the surface in-situ observations. This “in-situ profiles” are then defined as the real profiles and compared to the coincident TES observations. Second, they compare the HDO/H2O versus H2O distribution measured in-situ at the Mauna Loa Observatory to the HDO/H2O versus H2O distribution measured by TES within a 1000km distance from the Observatory.

The presented work is important, since a better scientific documentation of the bias in the TES HDO/H2O data is urgently required: the TES data are already used in model validation studies, although the bias of the TES data has not been comprehensively documented.

I recommend the publication of the manuscript in ACP. However, I would also like to comment on aspects of the manuscript that – in my opinion -- should be improved/clarified.

Major comments:

The two approaches rely on important assumptions. In my opinion a better documentation of these assumptions would be very useful. In the following I tried to collect all the assumption involved in method number one (constructing a “real profile” from surface observation):

I) The authors use the TES H2O profile data as a tracer in order to map the surface in-situ HDO/H2O to a HDO/H2O profile:

(1) Thereby the authors postulate that there is no bias between the TES H2O data and the in-situ H2O data. This is an important assumption and might cause important errors in the constructed “in-situ HDO/H2O profiles”. In the current manuscript this assumption is not discussed at all.

(2) Thereby the authors assume that the airmass in the middle/upper troposphere has the same history as the airmass at the Mauna Loa Observatory, page 25363, last line: “This mapping also makes the assumption that the observed air parcels measured over the day by the in situ device is representative of the observed air parcel measured at a single time by TES”.

I wonder if this is a realistic assumption for a subtropical site like Hawaii where large scale subsidence prevails. I could image that in regions with large scale subsidence the origins of lower/middle tropospheric air and middle/upper tropospheric air differ significantly.

II) Bias correction (Equation 6): The authors only correct the bias for the retrieved HDO. However, an error in the line strength of HDO – as suspected by the authors (page 25366, line 13) – will also have an effect on the retrieved H2O. Instead of calculating the corrected ln[HDO] value the authors should calculate the corrected x_r=ln[HDO]-ln[H2O] value: $x_r(\text{corrected}) = x_r(\text{original}) - (\mathbf{A_{DD}} - \mathbf{A_{HD}}) * x_D(\text{bias})$
Only correcting HDO and then calculating the corrected HDO/H2O is to my understanding an assumption that might cause inconsistencies in the bias correction ($\mathbf{A_{DD}}$ and $\mathbf{A_{HD}}$ depend on the actual atmospheric situation like dry/wet conditions, clear/cloudy sky).

III) Similar to item II): Equation (5) should consider that an error in the real HDO profile also propagates into the H2O profile. Equation (5) should be:
$\mathbf{S_{TES}} = (\mathbf{A_{DD}} - \mathbf{A_{HD}}) * \mathbf{S_{in\text{-}situ}} * (\mathbf{A_{DD}} - \mathbf{A_{HD}})^T$

IV) The “real” profile can only be deduced for pressures above 500 hPa. In the middle/upper troposphere (between 500 hPa and 200 hPa) it is determined by interpolating between the 500 hPa value and the a priori value at 200 hPa.

In my opinion there are a lot of important assumptions whose effects on the bias estimation are very difficult to assess. Therefore, I think the authors should be very cautious when drawing conclusions (maybe delete “very” on page 25369, line 11). It should be ensured that the reader gets the message that there remains an uncertainty in the bias of about (1-)2%.
I suggest mentioning the assumptions already in the abstract, e.g., expand the last sentence of the abstract as follows: “… because these uncertainties are primarily derived from only three sets of measurements and rely on a variety of assumptions.”

In addition, I suggest adding a Table that collects, documents, and discusses all the different assumptions that are made (for method one and two).

The modeling community is already using the TES data and therefore, estimating the bias of TES -- as done in the paper -- is very urgent and important. In addition, the authors mention in the abstract that future studies are needed to refine this bias estimate. This is also my opinion:
In this context the authors mention another strategy that uses ground-based FTIR H2O and HDO/H2O profile observations as validation source (page 25358, lines 5ff). This would in my opinion be a better strategy. However, it is important to note that the bias in the ground-based FTIR data itself is not clearly documented (for instance, Schneider et al., ACP, 6, 4705-4722, 2006; Schneider et al., AMT, 3, 1599-1613, 2010). It is planed to estimate the FTIR HDO/H2O bias during the next years by a variety of in-situ aircraft validation campaigns. FTIR data would then be best-suited for reviewing the TES bias estimation of this paper.

Minor comments:

Page 25364, line 14: “(noted as diamonds in Fig. 3)”
Page 25364, lines 17 and 18: Fig. 3 instead of Fig 2
Page 25365, line 12: “[…] and $\mathbf{S_{DH}}$ is the cross term;”