

The authors thank the review for the comments below. Replies to the comments are in red.

The paper illustrates a series of relevant results, tackling the problem of comparing heterogeneous in situ measurements of D/H with ACE-FTS satellite data products. This itself is a complicated problem, and the paper serves as an effort to cross-validate several datasets, illustrating consistencies and potential issues.

I only have a few, mostly minor comments that the authors should address, and which are reported here below:

1) In the introduction, the authors should also consider adding results from nadir hyperspectral sensors such as IASI, which allow for HDO and D/H vertical profile retrieval on a global scale. Specifically the following works:

<https://acp.copernicus.org/articles/9/9433/2009/>

<https://essd.copernicus.org/articles/14/709/2022/>

<https://www.sciencedirect.com/science/article/pii/S0022407316301248?via%3Dihub>

provide a good overview of results which would be beneficial to this introductory discussion.

The authors have added a section of text in the introduction describing these measurements.

2) Line 156: to be more precise, and to distinguish ACE-FTS from instruments like MIPAS, "limb-sounding" should be replaced by "solar occultation"

"Limb-sounding" has been replaced by "solar occultation" in the text.

3) Figure 4: It would be useful to report the variability of ACE-FTS profiles averaged here with errorbars.

The authors have included error bars representing the inter-quartile range of the ACE profiles to Figure 4, and report them in the text.

4) Lines 270 - 272: this is one of the core points of the paper. I wonder if the authors checked the possibility that temperature dependency of line strengths in different spectral regions cause this. This would clearly be another factor beside the already noted difference in sampling techniques between ACE-FTS and in situ techniques.

The authors believe this comment refers to the possibility that the relationship between temperature and altitude in the atmosphere could result in the general divergence between ACE and in situ measurements which occurs around 12-14 km throughout the measurement regions. That is, there may be a temperature-dependent bias in the in situ measurements. However, both ChiWIS and Harvard ICOS are extractive instruments and maintain a nearly constant optical bench temperature at about standard room temperature throughout sampling. Thus while there may be some temperature dependent

effect on the line strength, the sample gas temperature is independent of altitude so such an effect would be constant in altitude. This important point has been incorporated into the text.

5) Lines 309 - 311: to better discuss this, the manuscript should report the exact space and time box considered for ACE-FTS, and eventually include comparison with a restricted dataset of ACE-FTS profiles.

The space time boxes are now clearly denoted in the text upon the first occurrence of each measurement region, and a column has been added to Table 1 denoting the time interval of the measurements.

Additionally, the authors have expanded the methods section to make clear how exactly the ACE and in-situ measurements are being compared, and that the comparisons presented are being made on restricted data sets of ACE profiles, a point which was not clear in previous versions of the text.

6) Sometimes UT/LS is used, some other times UTLS. Please make the notation uniform across the paper.

We have changed UT/LS to UTLS throughout.