**Revision of "Comparison of the H2O, HDO and δD stratospheric climatologies between the MIPAS-ESA v8, MIPAS-IMK v5 and ACE-FTS V4.1/4.2 satellite data sets" by De los Rios et al., submitted to AMT.**

Dear Editor,

In this revision the table suggested by the reviewers is included. The text has been shortened from 561 lines without the table to 539 lines including the added table. All the comments by both reviewers have been considered and replied in red colour.

With best regards, on behalf of all authors,
Paulina Ordoñez

**Reviewer #1**

I agree that significant improvements have been made to the manuscript, to the point that it is now publishable in my opinion. That said, the manuscript is still unnecessarily long and parts are difficult to follow. In the technical comments below, I have made suggestions how to slightly shorten the paper.

In order to better follow the discussion of the datasets, I made a table for myself summarizing the key information. I found myself continually referring to this table, so perhaps it (or something similar) should be part of the paper itself. Readers not familiar with MIPAS processing might benefit.

Thank you very much for the table, this is very useful. The reviewer #2 also comments that we should summarize this information. We include the following table in the revised paper.

| Instrument | Molecule | Dataset | Dates | Altitude (km) | Coverage ($cm^{-1}$) | Spectroscopic database | Microwindows |
|---|---|---|---|---|---|---|---|
| MIPAS-IMK | $H_2O$ | V5H_$H_2O$_20<br>V5R_$H_2O$_220/221 | 2002-2004<br>2005-2012 | 5-72 | 795-827<br>1223-1410 | MIPAS_pf3.32 (HITRAN 1996) | Von Clarmann et al. (2009). |
| | HDO | V5H_HDO_22<br>V5R_$H_2O$_222/223 | 2002-2004<br>2005-2012 | 5-72 | 1250-1482 | | Steinwagner et al. (2007) |
| MIPAS-ESA | $H_2O$ | MIPAS L2V8 | 2002-2012 | 5-55 | 783-956<br>1224-1696 | HITRAN_mipas_pf4.45 (HITRAN 2012) | Dinelli et al (2021) |
| | HDO | MIPAS L2V8 | 2002-2012 | 5-55 | 1218-1471 | | Dinelli et al (2021) |
| ACE-FTS | $H_2O$ | V 4.1/4.2 | 2004-present | 5-150 | 937-945<br>1195-1990<br>3151-3173 | HITRAN 2016 | Boone et al. (2017) |
| | HDO | V 4.1/4.2 | 2004-present | 5-42/50* | 1383-1511<br>2605-2673 | HITRAN 2016 | Boone et al. (2017) |

Key aspects of the three datasets compared in this work.
* Upper altitude of retrieved profile differs between polar (42 km) and equatorial (50 km) latitudes.

Technical comments.

Line 23: "For HDO and δD, lower biases are found in the MIPAS-ESA and ACE-FTS comparison". When I look at Fig.3b, MIPAS-ESA HDO is an outlier above 35 km. Perhaps the authors mean that lower biases are found in the MIPAS-IMK and ACE-FTS comparison?

We were referring to the bias analysis results in line 23 (Fig 4), but also it is below 30 km. We have clarified it in lines 22-24 of the revised version of the manuscript.

Line 48:" ...accompanied by large horizontal motions to mid-stratospheric latitudes ". I don't understand. Perhaps the authors mean "accompanied by large horizontal motions at mid-stratospheric latitudes".

The reviewer is right, and the sentence is now changed to "accompanied by large horizontal transport at mid-stratospheric latitudes".

Line 127: The "NOM", "UTLS-1", and "Aircraft Emissions" observation modes are introduced here along with their altitude ranges. "NOM" is used only once more. "UTLS-1" and "Aircraft Emissions" modes are never used again. So, I suggest stating that the nominal observational mode, covering 5-72 km, is used in this work. No need to tell us about the other modes.

MIPAS-IMK data are for NOM mode only which was mentioned in line 178-179 of the manuscript.
MIPAS-ESA uses all these modes as mentioned in line 145 “HDO has been retrieved from all the observation modes listed above”.

Line 146: "the ones" --> "those"

Done.

Line 147: "lies" --> "lie"

Done.

Line 157: " H2O vertical resolution is about 3 at 10 km, then it slowly degrades, reaching 5 - 6 km at 20 km, 7.5 at 30 - 40 km, 10 at 50 km." These are 3 instances of missing "km" and a missing "and" in this sentence. It should be " H2O vertical resolution is about 3 km at 10 km, then it slowly degrades, reaching 5-6 km at 20 km, 7.5 km at 30 - 40 km, and 10 km at 50 km."

Done.

Line 162: "The relative average single scan random error varies with altitude for the different atmospheres, but...". Which different atmospheres?

Thank you for the comment. We were meaning “for different atmospheric conditions”. It has been modified in line 157 of the revised version of the paper.

Line 173: "MIPAS-IMK WV retrievals used here were retrieved in log (VMR) space" But line 177 states that "HDO was retrieved in linear space". So, is HDO not WV?

MIPAS-IMK retrieves the main isotopologue of water vapor on log (VMR) space (it is clarified in line 167 of the revised version of the manuscript), but not HDO.

Line 180: "omitted" --> 'avoided”.

Done

Line 181: "the vertical resolution of δD is provided by the difference between the a priori and the retrieved profile". I don't understand this at all.

As a characteristic of the Tikhonov regularisation that smoothes the retrieved profiles only, the structures in the a priori profile provided by the main isotopologue retrieval are smoothed in the HDO retrieval according to its vertical resolution (Speidel et al., 2018). We clarified it in line 174-176 of the revised version of the manuscript.

Line 185: "range." --> "ranges."

Done

Lines 186-189: "data base" --> "database" (3 instances).

Thank you. We use always “database” in the revised version of the paper.

Line 215: "uses a minimum altitude spacing of 2 km for tangent heights above 15 km and a minimum spacing of 1 km for tangent heights below 15 km." --> "uses minimum altitude

spacings of 2 km for tangent heights above 15 km and 1 km for tangent heights below 15 km."

Done.

Line 231: "For the coincidence pairs, the ACE-FTS data, which is the sparser dataset in the tropics was used as the first data set." I don't understand why the order matters.

It is a matter of efficiency. If we start with MIPAS profiles and search for an ACE-FTS profile for each of the millions of MIPAS profiles, many fails will probably be gotten. If we search MIPAS profiles for each of the several ten thousand of ACE profiles, a MIPAS profile for almost all ACE-profiles will probably be gotten. In the first case, the loop goes over millions of cases, in the second case it goes over ten thousand. However, the reviewer is right, and the result should indeed be the same. Therefore, this sentence is omitted in the revised version of the manuscript.

Line 250: "The number of ACE-FTS HDO profiles decreases from 40 km of altitude and upwards." --> "The number of ACE-FTS HDO profiles decreases above 40 km altitude."

Done.

Line 254: I don't understand why 3 colors are needed. Or why the y-scale extends to 60 km when the largest y-value is only 48 km.

One colour is now used in Fig 2 and the y-scale extends to 50 km.

Line 257: "sample" --> "sample size"

Done.

Line 390: Add "," after "ACE_FTS".

Done.

Lines 494 to 501: This paragraph seems to repeat parts of the introduction. It does not relate to the work that you did. I suggest deleting.

Deleted.

Line 517: "upwards 30 km of altitude" --> "above 30 km altitude"

Done.

Line 530: "9 years" --> "8 years"

Done.

Line 554: Add "," after "δD".

Done.

Line 559: "on a long period" --> "over a long period"

Done.

**Reviewer #2**

Comparison of the H2O, HDO and delta D stratospheric climatologies between the MIPAS-ESA v8, MIPAS-IMK v5 and ACE-FTS v4.1/4.2 satellite data sets by K de los Rios et al.

Overview

The paper compares three different datasets for (primarily) stratospheric H2O, HDO and the derived delta D for the period 2004-2012 using latest processed version of data from the IMK and ESA retrievals of MIPAS data, and the ACE-FTS. This builds on previous work by other authors who used older and more limited datasets. In particular the extended time period illustrates how the H2O 'tape recorder' effect in the equatorial stratosphere is represented with very different levels of clarity.

Main Comments

Overall this seems to be more of a technical report rather than a scientific paper: the data are read, the recommended screening is applied, the results are plotted, analysed and discussed. There's nothing wrong with it, as such, but the authors miss some opportunities for providing new insights.

I list a number of suggestions, which the authors may wish to consider, which I think would improve the paper.

1) The descriptions of the algorithms and retrieval characteristics behind the different datasets read very much like extracts from the separate source papers, including many obscure technical details. I prefer to have seen a single, shorter and, most importantly, original description highlighting the similarities and differences where they might be relevant to the results presented, which would also show that the authors have applied some critical understanding of the technical details rather than simply relaying the information to the reader to evaluate.

We have shortened the text and added the following summarizing table with the characteristics of the algorithm and the retrieval diagnostics, as also suggested by the reviewer G. Toon on his comments. In this way the similarities and differences are clearer.

| Instrument | Molecule | Dataset | Dates | Altitude (km) | Coverage ($cm^{-1}$) | Spectroscopic database | Microwindows |
|---|---|---|---|---|---|---|---|
| MIPAS-IMK | $H_2O$ | V5H_$H_2O$_20<br>V5R_$H_2O$_220/221 | 2002-2004<br>2005-2012 | 5-72 | 795-827<br>1223-1410 | MIPAS_pf3.32 (HITRAN 1996) | Von Clarmann et al. (2009). |
| | HDO | V5H_HDO_22<br>V5R_$H_2O$_222/223 | 2002-2004<br>2005-2012 | 5-72 | 1250-1482 | | Steinwagner et al. (2007) |
| MIPAS-ESA | $H_2O$ | MIPAS L2V8 | 2002-2012 | 5-55 | 783-956<br>1224-1696 | HITRAN_mipas_pf4.45 (HITRAN 2012) | Dinelli et al (2021) |
| | HDO | MIPAS L2V8 | 2002-2012 | 5-55 | 1218-1471 | | Dinelli et al (2021) |
| ACE-FTS | $H_2O$ | V 4.1/4.2 | 2004-present | 5-150 | 937-945<br>1195-1990<br>3151-3173 | HITRAN 2016 | Boone et al. (2017) |
| | HDO | V 4.1/4.2 | 2004-present | 5-42/50* | 1383-1511<br>2605-2673 | HITRAN 2016 | Boone et al. (2017) |

Key aspects of the three datasets compared in this work.
* Upper altitude of retrieved profile differs between polar (42 km) and equatorial (50 km) latitudes.

2) The averaging kernels, in particular, seem key. There really should be a figure allowing these to be compared rather than verbal descriptions of the two MIPAS AKs and nothing at all regarding ACE-FTS. I couldn't find any mention of whether the ACE-FTS retrievals use any kind of regularisation and/or climatological a priori constraint, and I would expect the authors to have at least asked themselves the same question.

ACE-FTS does not provide averaging kernels and it does not use any regularization or a priori constraint. For this reason, we do not provide any figure with AKs comparisons. The following has been added to the text: “Unlike the MIPAS-IMK and MIPAS-ESA retrievals, the ACE-FTS retrieval does not use any regularization (lines 202-203)”.

And, from the AKs of H2O and HDO, one point that could have been developed is how to determine the AK for delta D.

As commented above, ACE-FTS does not provide AKs. For both MIPAS datasets this is an open question that requires further investigations.

3) Given that MIPAS-ESA and MIPAS-IMK both use fundamentally the same set of observations, the comparisons would have been simpler if MIPAS profiles were *only* used when data from both processors were available. There would be some loss of data from the UTLS-1 and AE modes, but negligible compared with the advantage of eliminating sampling bias.

We performed different tests before deciding to use all the data in figures 5 and 6.

In the figure 1 the vertical propagation of the tropical signal along the monthly evolution of the MIPAS-IMK and MIPAS-ESA data is shown. In this plot MIPAS profiles were used only when data from both processors were available. It can be seen that the differences with the plots included in the paper are small, and the conclusions that can be obtained are quite similar. However, the data gaps as in MIPAS-IMK also showed up in MIPAS-ESA.

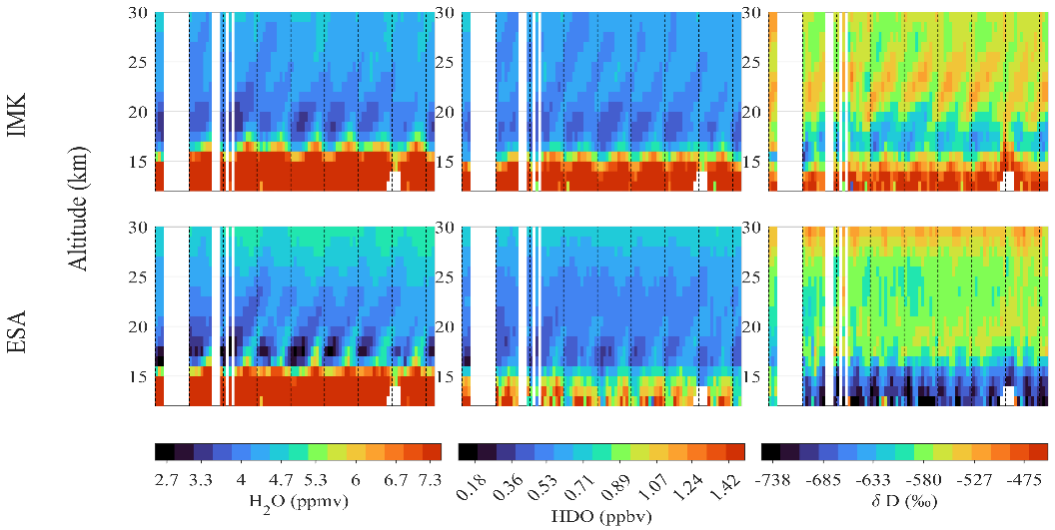



**Figure 1.** Altitude vs. time diagrams over 30 S and 30 N of $H_2O$, HDO and δD for the datasets MIPAS-IMK, and MIPAS-ESA.

Plots were also performed only with coincident profiles from the three datasets as depicted in figures 2 and 3. The results are very noisy particularly in the case of the temporal evolution plots.

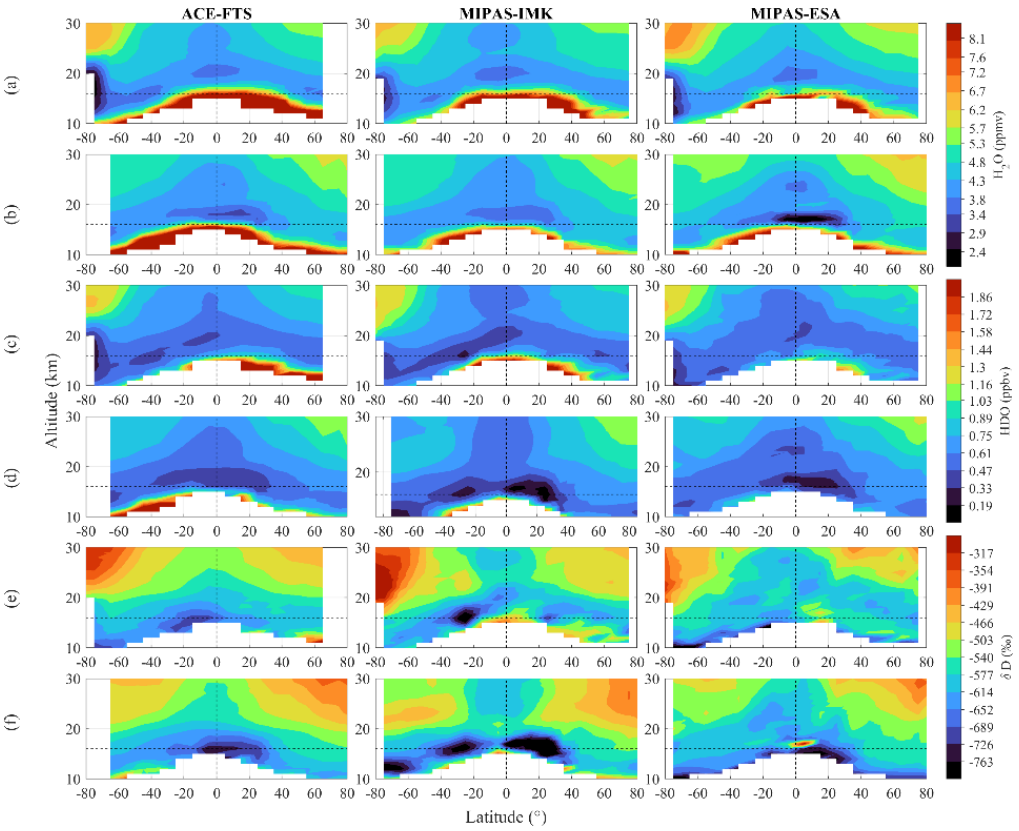



**Figure 2.** Latitude-altitude cross sections of $H_2O$ in (a) boreal summer (JJA) and (b) boreal winter (DJF), HDO for (c) boreal summer and (d) boreal winter and δD during I boreal summer and (f) boreal winter for the three datasets.

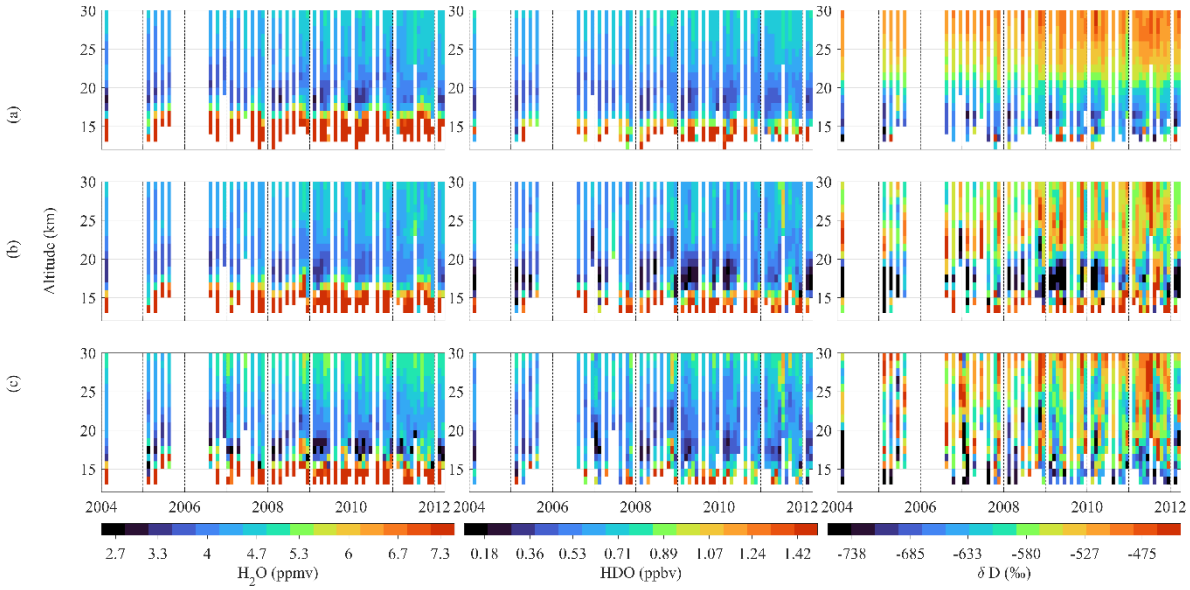



**Figure 3** Altitude vs. time diagrams over 30 S and 30 N of (ACE-FTS), (b) MIPAS-IMK and (c) MIPAS-ESA datasets for $H_2O$ (left column), HDO (middle column) and δD (right column).

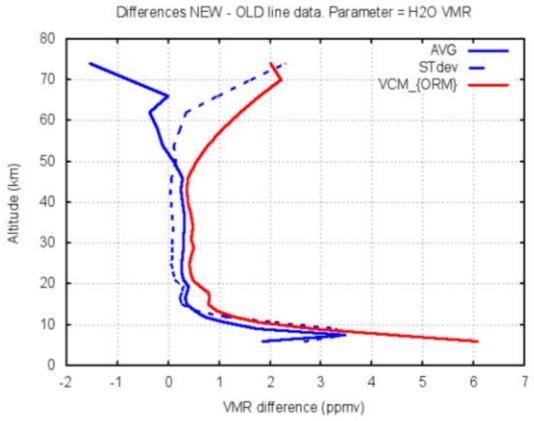
4) While the systematic errors for both the MIPAS-IMK and MIPAS-ESA retrievals are dominated by spectroscropic uncertainties, and it is established that there are some differences in the H2O spectroscopic data used, it seems most unlikely that the spectroscopic data are so independent that they account for much of the difference between these two. A plot of the H2O and differences using the two spectroscopic databases, with microwindows marked, say for 20 km altitude would have helped answer this.

The spectroscopic databases used in retrievals are reported for each dataset, for example for MIPAS-IMK, this is von Clarmann et al. (2009) for $H_2O$ and Steinwagner et al. (2007) for HDO. A further investigation is beyond the scope of this paper.

Nevertheless, below you can find an example of differences in the retrieved $H_2O$ profiles when using different spectroscopic databases. The impact of the spectroscopic database seems quite large.

**Profile differences due to changed spectroscopic line database**

Retrieved profile differences are relevant only for H2O VMR. Differences in p,T and other species are much smaller (more than one order of magnitude) than the noise error. Average (solid blue) and standard deviation (dashed blue) of the H2O VMR differences are shown in the plot below along with the noise error predicted by the VCM calculated by the ORM (solid red).




5) Another point that wasn't addressed was whether there was any significant difference between the day and night profiles for the two MIPAS datasets. H2O, in particular, has a strong non-LTE signature in the stratosphere and this could lead to spurious day-night differences in the results (with, presumably, the night profiles being less affected). This could simply be incorporated into Fig 3 and may explain some of the difference between the MIPAS-ESA and MIPAS-IMK processors.

We know that there are non-LTE effects in the data above 40 km. Please see Stiller et al (2012) and Nedoluha et al (2017) where this has been discussed. As we should shorten the text, we prefer not to include a discussion of non-LTE effects in this paper.


- Stiller et al., 2012. Validation of MIPAS IMK/IAA temperature, water vapor, and ozone profiles with MOHAVE-2009 campaign measurements. https://doi.org/10.5194/amt-5-289-2012

- Nedoluha et al., 2017. The SPARC water vapor assessment II: intercomparison of satellite and ground-based microwave measurements. https://doi.org/10.5194/acp-17-14543-2017

6) For the debiased SD (Fig 4) this should show some correspondence with the sum of the random errors associated with the individual profiles, ie sqrt( e_1^2 + e_2^2 ) where e_1 and e_2 are the reported random errors. It would be useful to have these plotted on the same figure for comparison.

The SD of the bias has been put in the context of the retrieval random errors, as it is mentioned in lines 343 to 348 in case of Fig.4c; and in lines 361 to 368 in Fig. 4g.

Another diagnostic would be to show the actual SD of each dataset about its mean. A certain amount of this would be atmospheric variability – presumably the same for all three instruments, but subtracting some variability due to the regularisation while adding variation due to the instrument random noise. One could go further: if it is assumed that the bias is constant, three sets of comparisons between three datasets is enough information to assign as SD to each dataset. Thus, from the debiased SD results, one can empirically determine the actual SD of each dataset about its mean bias.

This is completely beyond reach for this paper.

7) Sections 3.1.1 and 3.2 should be an appendix. While useful for the purpose of defining terms, this is just standard statistics.

We respectfully disagree. We prefer to include these details in the paper so that everyone is clear on their use. Therefore, we prefer to maintain sections 3.1.1 and 3.2 in the main text.

Minor Comments

L34: This is not a particularly controversial or original statement, so I would suggest "(e.g., Hegglin et al ... "

Done.

L42: This statement probably does need a supporting reference, instead of just 'Scientists discovered it'. Rosenlof et al will probably suffice but use that at the start of the paragraph.

This sentence has been rewritten. “At the beginning of the century, an increase of the water vapour in the lower stratosphere in the last decades was proven. However, the reason for this humidification was not understood (Rosenlof et al., 2001 and references therein)”. (lines 42-43).

Rosenlof et al (2001) contains several contemporary references. For example, they cited that “Oltmans et al. (2000) shows an increase in stratospheric water vapour at Boulder CO (40°N) of -1%/yr (0.05 ppmv/yr) over a 20-yr period using the frost point hygrometer of the NOAA Climate Monitoring and Diagnostic Laboratory (CMDL)”, “Water vapor increase have also been documented from the Atmospheric Trace Molecule Spectroscopic (ATMOS) instrument [Michelsen et al., 2000], from combined multiple in situ measurement [Engel et al., 1996], and from the Halogen Occultation Experiment (HALOE) [Nedoluheat al.,1998; Smith et al., 2000]”.
Rosenlof et al (2001), combined ten stratospheric water vapor datasets (including WV from older in situ data) to show that increases in stratospheric WV had persisted since the mid-1950s.

L23: lower biases for HDO, but not delta D. But you should also mention that the smallest bias for H2O is between MIPAS-IMK and MIPAS-ESA.

The smallest bias for $H_2O$ is between MIPAS-IMK and ACE-FTS for the range 16 km - 30 km, and it is mentioned in the lines 23-24 of the revised version of the paper.

L24: I would interpret a 'meridional cross-sections' to be cross-sections at a particular longitude, but here you really mean 'zonally averaged cross-sections'

Modified.

L61: "Atmospheric limb-sounding" would be better than "Limb Earth probing".

Modified.

L71: "highly reliable" is unnecessary and rather subjective. Perhaps just say "made regular WV observations..." (and use past tense).

We provided references to say that MIPAS WV observations are highly reliable. Anyway, in the revised version of the manuscript “made regular WV observations” is said.

L75: Since you have already described Odin, I think it would be useful at this point to briefly mention that MIPAS made continuous observations of the infrared limb-emission, obtaining around 1000 profiles a day with global coverage, while ACE-FTS used solar occultation which gave typically 28 profiles a day split into two narrow latitude bands (which varied throughout the year).

Odin is mentioned in the introduction because this instrument measures $H_2O$ and HDO. However, as their measurements are not simultaneous, δD can’t be derived and these data are not used in this work.
Regarding the number of observations per day, this information was in section 2 for ACE-FTS (now in lines 192-193) and it is added for MIPAS in line 120-121 of section 2.

L83: Suggest "e.g.," or "i.e.," instead of "like".

Done.

L85: references to web-pages should probably appear as usual citations rather than directly within the body of the text (unless AMT has its own rule on this). Also L156, L174.

As we clarified in the previous revision, this is the standard method for referring to WCRP/SPARC II special issue (also valid for line 174).

Citation in line 156 is modified.
“https://earth.esa.int/eogateway/documents/20142/37627/README_V8_issue_1.1_20210916.pdf” is changed by “Raspollini et al (2020)”.

- Raspollini, P., A. Piro, D. Hubert, A. Keppens, J.-C. Lambert, G. Wetzel, D. Moore, S. Ceccherini, M. Gai, F. Barbara, N. Zoppetti, with MIPAS Quality Working Group, MIPAS validation teams, MIPAS IDEAS+ (Instrument Data quality Evaluation and Analysis Service) team. ENVIromental SATellite (ENVISAT) MICHELSON INTERFEROMETER for PASSIVE ATMOSPHERIC SOUNDING (MIPAS). ESA Level 2 version 8.22 products - Product Quality Readme File. ESA-EOPG-EBA-TN-5, issue 1.0 [online]. Available from: https://earth.esa.int/eogateway/documents/20142/37627/README_V8_issue_1.1_20210916.pdf, accessed 29 February 2024, 2020.

L85: Suggest 'latest' rather than 'last' - they may want to produce another.

Changed.

L85-L102: There is a rather confusing mass of detail over specific datasets here, much of which is repeated in Section 2. For this part, the introduction, the emphasis should be on clarity so try to remove some of the obscuring details which are covered in Section 2. (Even in section 2 I feel it would be more clearly represented in a table listing dataset, date range, products compared and the reference).

The paragraph has been modified in the revised version of the manuscript, trying to avoid some details that are covered in section 2. By the other hand, please see reviewer#2 comment#1 for the summarizing table that are represented in the revised version of the manuscript.

L113: v4.2?

Yes, thank you. Modified.

L136: What is the MIPAS FOV width? (and that of ACE-FTS?)

It is 3 km in the vertical and 30 km in the horizontal for MIPAS and this is 3 km circular FOV at the limb for ACE, as mentioned now in lines 136 and 195 respectively.

L148: A table listing the microwindows would have be useful.

We have also added the original publications references for microwindows in the above depicted table. In our opinion it is not necessary to repeat all this information.

L149: I don't know why information on molecules other than H2O is provided here - are they expected to have a significant contribution to the results? OCS, for example, only has a significant absorption feature around 2100cm-1, well outside any of the spectral regions used for the H2O retrieval?

In the revised version of the paper, only the information about $H_2O$ is included in this paragraph.

L154: Since these links refer to images you should include them directly in the manuscript (or else replot the data), otherwise this paper will be incomplete if the links ever disappear.

The links to refer images has been changed by this cite:

Anu Dudhia, MIPAS Level 2 error analysis [online]. Available from: http://eodg.atm.ox.ac.uk/MIPAS/err/, accessed 29 February 2024, 2020.

L157: 'is about 3 km'

Thank you, “km” is added.

L236: Since both MIPAS processors have used the same set of spectra, the differences in time/location are purely due to how these values are assigned to the resulting L2 profiles.

Different L1b versions have slightly different geolocations and times, and therefore we have applied this coincidence criteria to make sure we collect the same profiles from the two data sets.
No action performed.

Fig1: Why would the MIPAS-ESA and MIPAS-IMK profile locations be any different?

We use the same colour for MIPAS in the Fig 1 of the revised version of the paper.

L246: This is inconsistent. Is the grid from 0-70km or 1-70km? Is it 1km spacing up to 44km or up to 46km?

The reviewer is right. The information was inconsistent because it was wrong.
It's formally from 0 to 70 km, however, we don't have a measurement at the surface, and this data point is just matched to the profile points in the troposphere. Visibility flag indicates that it should not be used. The vertical grid is 0, 4, 5, 6, … 44, 46, 48, …
The number of levels is 57 as indicated in the revised version of the manuscript (lines 242-243).

L260: Another approach you could have considered is averaging ln(H2O) and ln(HDO) (assuming the values are always constrained to be positive). Since there is a strong variation with height in the tropopause this avoids biasing towards large values in the average. This may explain some of the behaviour of the MIPAS-ESA HDO profile at low altitude in Fig 3.

As mentioned in lines 233-234, "the present quality assessment of H2O, HDO and $\delta D$ data mainly focuses on the stratosphere, although data for the upper troposphere and lower mesosphere are used if available".

L326: "along the stratosphere" - what does this mean? Along usually indicates a horizontal direction.

Changed to "through the stratosphere".

Fig 3: With >1000 profiles compared over most of the altitude range I think we can assume that the standard error will be negligible, so the error bars just clutter the plot.

As the standard error is very small or almost negligible, the error bars don't seem to clutter plot but make explicit the small standard errors.

Table 1: this would be clearer if the columns were lined up, eg split each into two columns, min and max, and use + signs for positive values. Also I don't think more than 1 significant figure is justified, certainly not 4 as used for the absolute bias of delta D.

The ranges in the table are expressed as the journal recommendations.
Significant figures for the absolute bias of delta-D have been modified.

L406-415: Table 1 already summarises the previous plots so I don't think yet more text summarising Table 1 is required.

We concur with the reviewer and more text summarising table 1 (table 2 in the revised version of the manuscript) is not necessary. Therefore, we have deleted this text.

Fig 5: "during I boreal"

Thank you. Corrected.

Fig 5: "The climatology is based ..." - presumably you are referring to these plots as "the climatology" but the plots are introduced as "latitude-altitude cross-sections" not as a

"climatology". Perhaps if you write "This climatology is based..." it establishes what you meant.

Thank you, it is modified.

Fig 6: These plots might be clearer if presented as deviations from the mean profile. It's hard to distinguish the various shades of blue/green which contain the signal for H2O and HDO.

We also tested the Fig 6 in terms of the deviations from the mean profile. However, we concluded that although the $H_2O$ and HDO signal could be more distinguishable, the comparison between the 3 datasets, which is the main objective of this work, weren't (see figure 4 below).

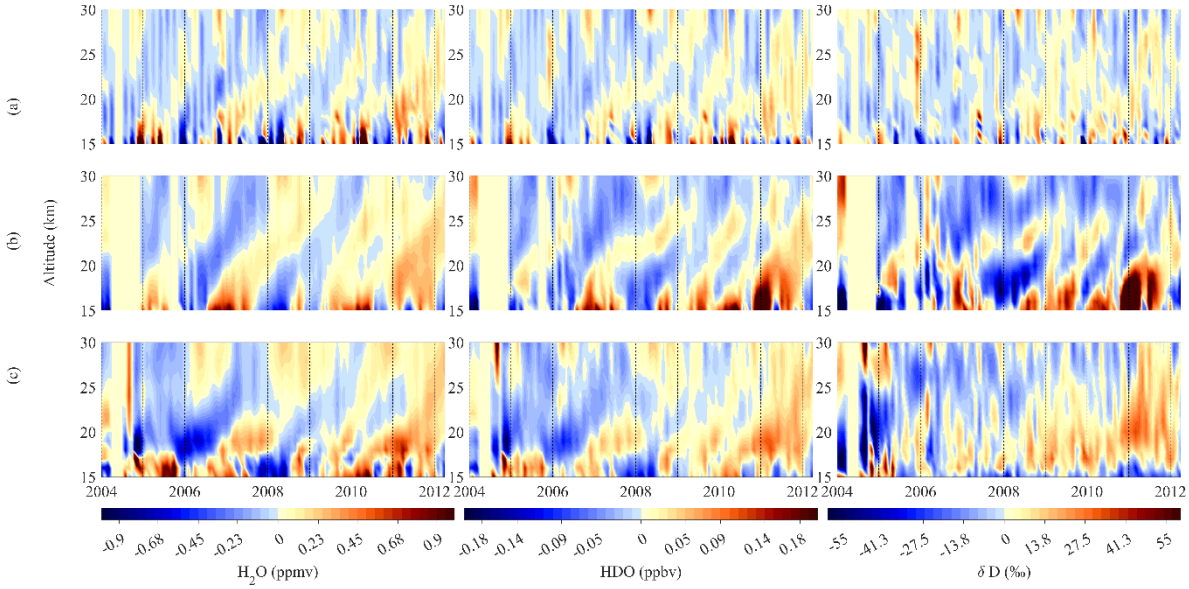



**Figure 4.** De-seasonalized annual cycle for (a) ACE-FTS, (b) MIPAS-IMK and (c) MIPAS-ESA datasets.

Typographcial inconsistencies

- Both upper and lower case for version, eg v8 in title, V8 in abstract

Thank you. In the revised version of the manuscript "V8" is used.

- Water vapor (eg L15) and water vapour (eg L16)

Thank you. As the journal is European, we use "vapour" in the revised version of the manuscript.

- Data set (eg title) and dataset (eg L69)

Thank you. Now "dataset" is always used.

- Use '--' in LaTeX to indicate a range of numbers, not hyphens (eg L157).

Thank you. Modified.