General Comments

We thank the reviewer for his/her constructive comments, which will result in an improvement of the manuscript.

1. This paper compares MIPAS and ACE H_2O and HDO over the period 2004 to 2012 when MIPAS was working. Similar comparisons have been done previously, such as Lossow et al., 2020, Lossow et al., 2011, Sheese et al., 2017, Ordonez-Perez et al., 2021, Risi et al., 2011, Hogberg 2019. This latest comparison utilizes more recent versions of the data products which are presumably better. Indeed, the authors state " The HDO data version used here differs significantly from the data versions assessed by Lossow et al. (2020) and Högberg et al. (2019) and used by Steinwagner et al. (2007, 2010)". In this latest paper, a new MIPAS product is presented for the first time: MIPAS-ESA. This and MIPAS-IMK are compared with ACE and with each other. It is not clear to me where, in the processing chain, these two MIPAS products diverge. The retrieval methods seem to be different. Not clear whether the spectra are the same.

The two products differ already in the version of the spectral data. While the ESA product uses Level-1b version 8.03, the IMK data uses version 5.02/5.06. The differences between the versions result from several factors including improved calibration procedures, a compensation of the detector drift due to aging, and subtle adjustments of the geolocations. We are aware that it is not optimal to use different level-1b data versions, however, it is unavoidable in this case, since there are no earlier HDO data versions of the ESA product, and HDO from version 8 is not yet available from the IMK processing.

The level-2 processing (retrieval of trace gases) has many substantial differences between the processors: in general, MIPAS IMK and MIPAS ESA use different spectral intervals, and there is a rich literature about the differences of the retrieval set ups and the reasons for these choices. In particular, the two papers Laeng et al., 2015, and Raspollini et al., 2013 describe differences between the algorithms performing MIPAS analysis and the differences in the products for O_3 and other trace species, but not HDO. For additional papers on the MIPAS IMK data set and retrievals, we refer one to the web site on the IMK products (https://www.imk-asf.kit.edu/english/298.php), while the evolution of MIPAS ESA Level 2 algorithm and its products is described in these papers (Ridolfi et al., 2000; Raspollini et al., 2006, Raspollini et al., 2013, Raspollini et al., 2022, Dinelli et al., 2021) and references therein; it is beyond the scope of this paper to summarize them all.

We will clarify this in the revised version of the manuscript.

2. To be honest, I didn't feel that I learned much reading this paper. There have already been several similar comparisons using earlier versions of the MIPAS and ACE data products.

It is true that several comparisons have already been made. Recently, the SPARC-WAVAS-II activity compared H_2O and HDO from all available satellite instruments since the year 2000, also including ACE-FTS version 3.5, MIPAS IMK version 5, and ESA version 5 and 7 (only for H_2O) (see ACP/AMT Special Issue “Water vapour in the upper troposphere and middle atmosphere: a WCRP/SPARC satellite data quality assessment including biases, variability, and drifts”, https://amt.copernicus.org/articles/special_issue10_830.html). Newer data versions of H_2O and HDO have been used in our paper here for all three data sets. We think that it is useful to assess the quality of new data products from satellite data, in particular here the new MIPAS ESA product, and it is necessary to do so before further scientific work with these data can be done.

Particularly important is the case of using δD data to study the origin of water vapor that enters the stratosphere. Therefore, it is critical to understand the quality of the H_2O and HDO that are used in this calculation as is a major focus of this paper.

3. The authors show that the MIPAS-IMK H_2O and HDO products agree well with ACE, but the MIPAS-ESA HDO profiles are discrepant around 40 km altitude. Since the error bars on the MIPAS HDO profiles are quite large above 30 km, the discrepancy is not significant.

   Line 21 states "Stratospheric H_2O and HDO global average coincident profiles reveal good agreement." I disagree. In my opinion a 37.5% bias in HDO at 40 km is not good agreement. Although the MIPAS HDO profiles have large enough uncertainties such that they bridge this 37.5% gap, this doesn't mean that the agreement is good. It just means that the MIPAS HDO measurements are not useful at 40 km and above.

We agree with this comment. In addition, we note that ACE-FTS H_2O has a significant deviation from the other two data records. This deviation was also found in the SPARC/WAVAS-II comparisons for earlier data versions (see, for example, Lossow et al., 2019) with respect to many other satellite data records. Therefore, because of these known discrepancies, we will restrict our extended analyses (those after discussion of Fig. 3) to altitudes below 30 km in the revised version of the paper.

Specific Comments

4. Lines 24-25 of the abstract state "ACE-FTS agrees better to MIPAS-IMK than MIPAS-ESA, with biases of -4.8% and -37.5%, respectively. The HDO bias between MIPAS-IMK and MIPAS ESA is 28.1 % at this altitude" So ACE is 4.8% lower than MIPAS-IMK and 37.5% lower than MIPAS-ESA. One might naively expect MIPAS-IMK HDO to be 37.5-4.8 = 32.7% lower than MIPAS-ESA. But it is only 28.1% lower. Presumably this is because different data were used for comparing ACE with MIPAS, than comparing MIPAS-IMK and MIPAS-ESA. Perhaps this should be made clearer in the text.

Indeed, a different number of coincident profiles were used in each of the three comparisons. There were differences in the geolocations because the values were arithmetically averaged if multiple coincidences were found in each of the coincidence regions, as we wrote in the text. However, we concur with the referee in the necessity of compare data with the same geolocation.

In the revised version of the manuscript, when multiple MIPAS profiles are spatially coincident with an ACE-FTS profile, the MIPAS profile closest in time is selected. In addition, there must be both MIPAS IMK and MIPAS ESA processed data available for this coincident profile.

5. Lines 29-30 of the abstract state: " ...aligns more closely with expected stratospheric behavior for the entire stratosphere". Delete "stratospheric" or " for the entire stratosphere". This is unnecessary to have both.

Will be done.

6. Also, this sentence states that MIPAS-IMK calculates dD. I consider it more of a measurement. A model would calculate dD.

Will be adjusted in the paper.

7. Line 15: I've not seen the word "isotopological" before. According to Google it is a mathematical term meaning "having the same topology". Perhaps the authors mean "isotopic"? Line 29 of the abstract uses "isotopic" in a similar context. The word "isotopological" occurs later in the paper, e.g. lines 54, 56. So I'm not sure if the authors are trying to make a distinction between "isotopological" and "isotopic", or they consider these terms synonymous. I suggest that "isotopological" NOT be used, because mathematicians have already defined this word for use in topology.
8. Line 54 states: "isotopological composition of WV" change to "isotopic composition of WV"
9. Line 56 states: "Among the isotopological species of WV..." Change to "Among the isotopologues of WV...".

Thank you very much for the constructive comment. We were making a distinction between "isotopological" and "isotopic" since “isotopic” is the adjective for “isotope” and "isotopological" is the adjective for “isotopologue”.

Certainly, two terms - “isotopologic” (e.g., Herbin et al., 2007; Bahr and Wolff, 2022) and “isotopological” (e.g., Schneider et al., 2020; Israel, 2023) - can be found in the literature to describe characteristics or properties related to “isotopologues”. Since the term “isotopological” is also a mathematical term meaning "having the same topology", we think that the suggestion of the referee in not using the term “isotopological” is right, and term “isotopologic” will be used in the revised version of the manuscript.

10. Having read the paper, what I would really like to know is why MIPAS-IMK and MIPAS-ESA HDO are different. Presumably these products come from the same raw data. It undermines confidence in MIPAS to see different groups obtain such different results.

We respectfully disagree. It is correct that the MIPAS ESA and MIPAS IMK HDO product comes from the same raw data (interferograms), but the spectral data are from different level-1b data versions. The level-2 processing (the retrieval set-up in this case) is a most relevant part in the data generation. Even the same level-2 processor produces different results with different retrieval settings. In cases where several level-2 processors of other satellite data exist, they often result in differing products (e.g., GOMOS, SCIAMACHY, SMILES, OMPS). The level-1b data for the Envisat instruments were made public to encourage different processing techniques to be developed and applied.

Further, we would like to point out that the differences between the two MIPAS products are rather limited (see Fig. 4). H_2O differences are close to 5% at max, while HDO differences remain <10% (all below 30 km). Differences as large as this can occur between different versions of the same data product of the same processor.

11. Example of duplication; Line 25-26: The meridional cross-sections of H2O and HDO exhibit the expected distribution that has been established in previous studies. Lines 27-28: The meridional cross-sections of δD are in good agreement with the previous version of MIPAS-IMK and ACE-FTS data. The sentence on lines 27-28 seems superfluous. Given that H2O and HDO are in good agreement with previous datasets and studies, readers will assume that δD will also be in good agreement. No need to tell them that it is.

As the δD here is calculated from individual profiles then averaged rather than mean profiles, it is important to point out this good agreement. From our experience, even subtle differences in H_2O and HDO (such as those between versions) are enlarged severely by calculating δD from them.

12. Raspollini et al., 2022 is cited on lines 86 and 148, but doesn't exist in the References. Either these citations are typos (should be 2020, perhaps?), or the Raspollini 2022 reference is missing.

The reviewer is right, the reference Raspollini et al., 2022 is missing in the list of references, while Raspollini et al., 2020 is correctly reported in the list. We also updated the DOI of reference Dinelli et al., 2021.

13. Line 106 states "we focus here on newer data versions that cover the full mission period of ten years.". If the newer data versions cover 10 years, why do all the tables and figures cover only eight years (2004-2012)? Also, this sentence is missing a final ". "

The reviewer is right. The sentence is corrected in the revised version of the manuscript since we are focusing on the overlap period between MIPAS and ACE-FTS which is from 2004 to 2012.

14. Lines 152 to 154: It seems that for the MIPAS-ESA processing, different retrieval methods were employed for H2O and HDO. The text needs to explain why this was necessary. Also, why is it "opportune" to use an a priori atmospheric HDO profile that is (3.107 × 10-4) of that of H2O. This is the value in VSMOW, not the atmosphere. In the UTLS the HDO/H2O ratio is closer to (1 × 10-4) so using the 3.107 × 10-4 value might adversely bias the HDO retrievals.

A different retrieval approach has been used for H_2O and HDO because the approach used for H_2O (namely a Levenberg-Marquardt regularization approach within the iterations followed by an a posteriori regularization) was not sufficient to constrain the HDO retrieval. For the HDO, an a priori error of 100% is used in order not to introduce a bias, as written in the text (line 161).

15. Tables 1 and 2 can be put side-by-side and hence merged into a single table.

Thank you for the recommendation. The tables will be merged.

16. Figure 1. Why are the ACE/MIPAS-IMK coincidences ~5 deg. to the South of the ACE/MIPAS-ESA coincidences? So, there is no overlap in the ACE data used for MIPAS-IMK validation and for MIPAS-ESA validation -- they are at different latitudes and hence dates. I don't understand why the same ACE data can't be used for both.

Thank you for the comment. The referee is right, there were differences in the geolocations of the same profiles in the two data versions due to the method we used for determining the coincident profiles (see comment #4). Now the same ACE-FTS data have been used for both comparisons.

17. Figure 2 should have 1 panel with 3 curves in different colors showing the number of coincidences between: (1) MIPAS-IMK and ACE, (2) MIPAS-ESA and ACE, and (3) MIPAS-IMK and MIPAS-ESA. This will provide the reader more information in less space.

Done in the revised version of the manuscript.

18. Figure 3: I don't understand the rationale for comparing ACE separately to each MIPAS version. This requires 4 panels and repeats the ACE profiles. Why not have two panels; one for H2O and the other for HDO? Each panel contains the 3 profiles (ACE, MIPAS-IMK, MIPAS-ESA) in different colors. I guess the reason is that ACE data compared with MIPAS-IMK is

different from that compared with MIPAS-ESA. In which case you need 4 profiles in each panel: MIPAS-IMK, MIPAS-ESA, ACE_{IMK}, ACE_{ESA}.

We agree with the reviewer suggestion, the three curves will be inserted in one panel.

We also improve the figure 3 including the MIPAS-IMK to MIPAS-ESA comparison.

19. Figure 6 should be appended to the bottom of fig.5, making a single figure with a single caption. This will allow the reader to compare the features in the dD panels with those in the H2O and HDO panels. This won't be possible with the dD panels on a different page. it will also eliminate repetition in the caption.

Thank you for the recommendation. This is done in the revised version of the manuscript.

Similarly, fig. 8 should be appended to the right of fig.7. It has exactly the same x- and y-axes.

Yes, done in the revised version of the manuscript. Thank you for the suggestion.

Line 465 states: " the MIPAS instrument shown a negative bias at the troposphere" Change to " the MIPAS instrument shows a negative bias at the troposphere"

Done in the revised version of the manuscript.

20. Line 420 states:" The general distribution of HDO (Figs 5(c) and 5(d)) shows some similarities to that of H2O (Fig. 5(a) and 5(b)), reflecting that both species have a common in situ source in the stratosphere, i.e., oxidation of CH4 and H2." But HDO comes from CH3D and HD, so change sentence to: " The general distribution of HDO (Fig.s 5(c) and 5(d)) shows some similarities to that of H2O (Fig. 5(a) and 5(b)), reflecting that both species have a common in situ source in the stratosphere, i.e., oxidation of methane and hydrogen."

Done in the revised version of the manuscript.

21. Line 447 states: "...diagrams over 30S and 30N...". This is ambiguous. Perhaps "...diagrams covering 30S to 30N..."

Done in the revised version of the manuscript.

22. Line 465:"As it was previously shown in the Fig.3, the MIPAS instrument shown a negative bias at the troposphere" Three grammatical errors in this half sentence. Change to: " As was previously shown in Fig.3, the MIPAS instrument shows a negative bias at the troposphere".

Done in the revised version of the manuscript.

23. Also, I don't see anything negative in Fig.3. Perhaps the authors mean Fig.4?

Thank you for the observation. Yes, it is figure 4, it is changed in the revised version of the manuscript.

24. Line 490 states: " The analysis conducted in this study highlights a higher level of agreement in HDO measurements obtained from ACE-FTS in both comparison cases." This seems to imply that ACE agrees better with MIPAS than MIPAS-IMK and MIPAS-ESA agree with each other?.

It was not our intention to imply this. In fact, the agreement between the two MIPAS products of H_2O is at least as good (below 30 km), as that between ACE-FTS and one of the MIPAS products. Regarding HDO, the differences between the two MIPAS data sets are somewhat larger, indeed, especially between 20 and 30 km, but still < 10%.

The text will be modified in the updated version of the manuscript.

25. Line 524 states: "the findings from this study suggest that the MIPAS-IMK dataset provides a more realistic signal for the entire stratosphere". More realistic than what? ACE or MIPAS-ESA.

"More realistic than MIPAS-ESA" was to be meant. However, we will reworde the sentence in the revised version of the manuscript since the affirmation "more realistic" is inaccurate given the existing dD data.

26. Line 526 states: "it is crucial to exercise caution when interpreting these results, specifically considering the sampling limitations of ACE-FTS in the tropics, during the period of study, especially at lower altitudes." I don't recall much discussion of this in the main part of the paper. It is true that ACE occultations are sparse in the tropics and that high clouds can often limit penetration of the troposphere. But it seems unfair to ACE to call this a conclusion. And it not clear what altitude range this comment is aimed.

Thank you for the comment, we concur with the reviewer that this is not a conclusion of this analysis. The last paragraph of the revised version of manuscript will be modified.

27. Line 530: "The code in MATLAB is available from the authors upon request." The term "the code" is too vague. Add one sentence explaining what "the code" does.

Changed in the revised version of the manuscript.

The format of the References is unfriendly. There is no indentation at the start of a new reference, nor a gap between references. So it is hard to tell where one reference ends and the next begins. Perhaps this is the journal style.

Thank you for the observation. Changed.

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