

Interactive comment on “Global distributions of water vapour isotopologues retrieved from IMG/ADEOS data” by H. Herbin et al.

H. Herbin et al.

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> We are happy that the reviewer is enthusiastic about the results presented in the paper. We acknowledge his useful comments, which have helped improving the manuscript. Below are the responses to his comments and suggestions.

Specific Comments :

1) Introduction, Sentence 4859: 20: The contrasting statement “the authors introduced a correlation between the H₂O and HOD values for the retrievals” is not informative. Either discuss the pros and cons of this approach or remove this statement. A more informative statement could be along the lines of “The TES authors estimated the correlation between H₂O and HOD (HDO) using a climate model. Using this analysis, they then developed an a priori covariance and constraint for the estimate of the HDO,

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H₂O, and its ratio.” > We agree with the referee that this statement was not informative at this stage. The sentence has been removed from the introduction. The explanation of the different retrieval approaches is given at the end of section 3.1.

2) Related to comment (1). There needs to be a better description of the a priori constraint vector and covariances used to estimate HDO and O₁₈. There is a statement in the caption of Figure 3 that the a priori constraint for HDO and O₁₈ is determined by multiplying the a priori constraint for H₂O by the standard isotopologic ratio. This description should be a part of the main document and not part of a figure caption. In addition, you now need to characterize the result of introducing this correlation on your retrieval. I would recommend re-reading Worden et al. (JGR 2006) as we discuss the correlation problem in that paper in some detail. In addition, how are the a priori covariances for HDO and O₁₈ derived? My understanding (which could be wrong) is that ECMWF does not contain water vapour isotopes and therefore cannot provide a climatology for those species. If isotopes are a part of ECMWF then please provide a reference or alternatively some additional explanation about how the a priori covariances for HDO and O₁₈ are developed. > Information on the a priori covariance has been added in the text. ECMWF Data provide information on the water vapour, without distinction between the different isotopologues. In our case, we used variance-covariance matrices for HDO and H₂O₁₈ identical to those of H₂O multiplied by the isotopic ratio.

3) The authors make the statement in 4860: sentence 1, that “the retrievals are performed for each isotopologue independently from the others” but then say in 4865: sentence 5 that the profiles are adjusted simultaneously. Which is it? If a simultaneous estimate is performed (and I suspect it is given the spectral windows used) then the uncertainty from the interfering species needs to be characterized in Equation 10 (See Worden et al., 2004 for examples). Or alternatively is H₂O estimated first, followed by an estimate of HDO, followed by an estimate of O₁₈? Basically, please state the specific retrieval approach in the manuscript as it will have a significant impact on the error

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characterization. Note that estimating H₂O, followed, by HDO and O₁₈ also contains an error from the interfering species but it will be of a different form than if HDO, H₂O and O₁₈ are simultaneously estimated. > We agree for saying that this explanation was not clear and this one has been modified. The retrieval was carried out without a priori correlation between the different isotopologues, but the adjustment is performed simultaneously for H₂O and HDO from band 1 and for H₂O and O₁₈ from band 2. The impact of interfering species on the error characterization is represented on Figure 3. As the interfering species are adjusted simultaneously as the target species, we found that their impact on the retrieved quantities was weak (example: we calculate an error of 1-2% on the HDO retrieval due to the H₂O₁₆ interference).

4) Related to Comment 3. There is no development of the error description for the ratios of HDO to H₂O and O₁₈ to HDO. This is a significant discrepancy in this paper given that the result of interest are these ratios. Note that the errors of the ratio are discussed in some detail by Worden et al. (2006 JGR). > The uncertainty is calculated by the partial derivatives formula. Thus, the errors that we obtain are largely overestimated as correlations are neglected.

5) 4864 Sentence 20: micro-window is jargon in the context used here. Is it 1/1000th of a window? Just use “window” or “spectral window”. > This has been corrected.

6) Nice job on the sonde comparisons! > Thank you very much.

7) It would be desirable to better compare the sensitivity of the IMG estimates to those from the Zakharov et al. 2004 and Worden et al. 2006 studies. I suspect the reason for the increased sensitivity to atmospheric HDO (e.g. 2-3 DOFS) as compared to the Worden et al. and Zakharov et al. studies is that the variance of the a priori covariance is much larger than that assumed by the Worden et al. study and the “equivalent” hard constraint used by Zakharov and not due to differences in the spectral windows as stated in 4867: 20. > We thank the referee for putting this argument forward. We agree with his comment and this part of the text has been rewritten to clarify this point.

8) Related to comment (7) we found for the TES data that if we used a softer constraint then we obtained many un-physical values of the HDO/H₂O ratios associated with the non-linearity of the retrieval problem. Given that the problem of estimating HDO profiles from IMG is nearly identical to that of estimating HDO using TES, one should spend some time discussing the global variability of the individual observations. For example, could you should a latitudinal distribution of the HDO/H₂O and O₁₈/H₂O ratios which are not averaged? > Indeed, the variance is an important factor of the sensitivity; since the larger the Sa matrix, the closer the A matrix will be to unity in the non-null space of K. That is to say that the softer the constraint will be and the larger the quantity of information will be, but with the risk to obtain sometimes un-physical values. The choice of our approach was driven by the intention to obtain a high sensitivity to each isotopologue alone, rather than a constrained value of the isotopologic ratio. Of course, that results in a larger dispersion of the values, which makes the discussion more complicated. To our mind, only a detailed validation exercise of each isotopologues will make it possible to determine how strong the constraint on each isotopologue needs to be applied. This is an aspect that we will look at in the framework of IASI mission

9) I am a little concerned about the numerous scientific interpretations in Section 4 as it seems they are constructed without considering in too much detail the errors in the data or how HDO and O₁₈ might be changing in the free troposphere where the IMG estimates are most sensitive. I recommend simplifying this section to just showing your global distributions and discussing the noticeable features. For example, one of the interesting things in your maps is that O₁₈ shows a strong ocean / continental contrast but HDO does not (even though you say it does, so please fix). This latter finding is consistent with the TES results which also show little contrast between land and ocean at the higher latitudes. In addition, you state the continental contrast is due to the rainout of precipitation; this conclusion might be supported by the O₁₈ contrast but not necessarily the HDO contrast possibly because the precipitation is at a lower altitude than where the IMG estimates are primarily sensitive. I suspect a more accurate explanation is due to a combination of processes worthy of its own paper. Because the reasons

for the global distributions and the differences between HDO and O18 could be fairly complicated I recommend just noting the interesting features and leave the interpretation of the details to further research. > The interpretation of the distributions is rather complex, on the one hand because the complexity of the processes involved and on other hand because dispersion and uncertainty of the retrieved values. We agree with the referee that this section would gain in clarity by only highlighting the most important features. Parts of this section were clarified, while others were simply removed. The obvious feature which is pointed out by the referee is the ocean/continental contrast on the H2O18 global map, but weaker for HDO and only present at higher latitudes. An accurate explanation of this observation is very difficult, because if one looks at the H2O18 DOFS map, one can see that there is an ocean/continental contrast too; the latter feature illustrates the difficulties to distinguish whether the observations are due to real geophysical or only reflect sensitivity aspects of the retrievals.

10) Related to (9) there are plenty of TES measurements of HDO and H2O for April 2005, 2006, and now 2007. Why not show a map for comparison and noting the similarities and differences in the distributions? > It is certainly true that a comparison between TES and IMG would be very interesting. However, because of the different instruments and retrieval approaches, we believe that a serious comparison would represent a long and difficult work, which is beyond the scope of this paper. This could, nevertheless, be the subject of a forthcoming study.

Technical Comments :

1) The maps do not show the locations of the observations. There is significant interpolation error in creating maps like these, especially at higher latitudes where it is likely that the data density is sparse and it is useful for a reader to understand the impact of this interpretation error. Two possible ways to address this issue are to either show the locations of the observations using some small symbol or by plotting a colored symbol on the global map instead of interpreting the data to a coarse grid. > We have added the locations of all the profiles on the DOFS global maps with black full circles.

2) The lettering on the figures is extremely small. When I print this paper I can barely read the characters. Can this be addressed with larger characters? Or is this small font a requirement of the journal? > The characters on the figures have been enlarged as much as possible.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 4857, 2007.

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