

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

**J. Worden (Referee)**

john.worden@jpl.nasa.gov

Received and published: 6 April 2007

General Comments:

I believe the results of this paper are important as it establishes and characterizes the capability for estimating tropospheric profiles of water vapour isotopologues using the ADEOS IMG instrument. In particular the ability to estimate H<sub>2</sub>O<sup>18</sup> profiles in addition to HDO provides for a more powerful constraint on characterizing the hydrological processes and sources that have affected an observed air parcel because HDO and H<sub>2</sub>O<sup>18</sup> react differently to evaporation and condensation processes. As the author of two papers that (1) characterize tropospheric profile estimates of the HDO/H<sub>2</sub>O using measurements from the Tropospheric Emission Spectrometer (TES) and (2) examined the distribution of hydrological processes and sources as inferred from the TES data, I

am particularly excited because the 1997 IMG data will allow for comparisons with that from TES taken nearly a decade later. I should also note that this paper on the IMG retrievals allowed us (the TES team) to determine that we could in fact estimate H<sub>2</sub>O<sub>18</sub> profiles using TES data; a capability that we had initially dismissed. We are very excited about developing this new product! That said, I am puzzled as to why the authors do not provide a more rigorous comparison with both the TES estimation approach and results given that the TES and IMG instruments have nearly identical spectral capabilities and signal-to-noise and given the public availability of the TES data. Other major concerns are that the authors do not better define their assumptions about the a priori covariances, the correlations between the isotopologues, and the uncertainties of the estimated ratios between the isotopologues.

Specific comments:

1) Introduction, Sentence 4859: 20: The contrasting statement “the authors introduced a correlation between the H<sub>2</sub>O 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 H<sub>2</sub>O 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, H<sub>2</sub>O, and its ratio.”

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 O<sub>18</sub>. There is a statement in the caption of Figure 3 that the a priori constraint for HDO and O<sub>18</sub> is determined by multiplying the a priori constraint for H<sub>2</sub>O 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 O<sub>18</sub> 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.

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 H<sub>2</sub>O 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 characterization. Note that estimating H<sub>2</sub>O, followed, by HDO and O18 also contains an error from the interfering species but it will be of a different form than if HDO, H<sub>2</sub>O and O18 are simultaneously estimated.

4) Related to Comment 3. There is no development of the error description for the ratios of HDO to H<sub>2</sub>O 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)

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”.

6) Nice job on the sonde comparisons!

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

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

constraint used by Zakharov and not due to differences in the spectral windows as stated in 4867: 20.

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/H<sub>2</sub>O 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/H<sub>2</sub>O and O<sub>18</sub>/H<sub>2</sub>O ratios which are not averaged?

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 O<sub>18</sub> 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 O<sub>18</sub> 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 O<sub>18</sub> 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 O<sub>18</sub> could be fairly complicated I recommend just noting the interesting features and leave the interpretation of the details to further research.

10) Related to (9) There are plenty of TES measurements of HDO and H<sub>2</sub>O for April 2005, 2006, and now 2007. Why not show a map for comparison and noting the similarities and differences in the distributions?

S1036

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

## 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.

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?

---

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

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper