**Atmospheric
Chemistry
and Physics
Discussions**

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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>First, authors would like to thank the referee for his positive comments and useful suggestions. Below are our point-by-point responses to his comments.

Specific Comments :

1) Please stick to using HDO or HOD, probably HDO is better > This has been corrected throughout the paper and HDO notation has been chosen as suggested.

2) The abstract and introduction have numerous grammar mistakes. > These sections have been corrected to improve on the use of English.

3) End of section 1: Why do you only process 10 days of data? Why does it matter that they were successive days? How much is available? If you are going to show global maps, then use more data please. > As described in the manuscript (page

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4862), the IMG operations were restricted to several 4 days operations (4 days of measurements followed by 10 days halt in order to reduce the data flow) except for one specific period, where ten successive days were covered. In order to achieve global distribution measurements, we have used this 10 days period. The other periods, of only 4 days, wouldn't enable global distributions to be derived without significant spatial averaging. Ten successive days are important to ensure that the averaging over time do not take into account too large variations in the water vapour amount. Of course, this is not optimal and the aim of this paper is more to show the potential of FTIR sounding for the purpose of retrieving the water isotoplogues. As such, IMG is more a demonstrator of what is to be expected from future space mission, such as IASI/MetOp.

3a) How many profiles are you using? $>$ We have used 7788 profiles for H2O16 and H2O18, and 6268 profiles for HDO. We believe that these data sets are enough to obtain global maps.

4) For equations 7-9 I do not see how the co-variance matrices are estimated. This could use another line of explanation. $>$ The sentence has been revised to clarify this point and in particular the estimation of Smod.param.(equation 9). The definition of the covariance matrices Ssmoothing (linked to Sa) and Smeas (linked to Sε) is explained (p. 4865, l. 8-13 and p. 4863, l. 13-16), respectively.

5) Figure 2 does not really show good agreement. Some profiles look good, some look bad. How many were analyzed? Also, were the soundings corrected for biases? If not, then the Upper Troposphere values are likely too dry. It would be helpful to plot these as percent differences. Also, the meridional differences are not easy to see. $>$ As far as we know, there is no instrument which measure HDO and H2O18 with precision and which could be used for validation purpose. However, we certainly agree with the referee that the Figure 3 gives a very limited aspect of the H2O validation. This is, however, not the purpose of this work. In fact, the goal of section 3 and Figure 2 in particular, is to show the capabilities of the IMG measurements for reproducing the large scale features of the humidity profiles and especially the latitudinal variations.

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The reasonable agreement gives some confidence for analyzing the global distribution. In order to comply with the referee, "good" as been replaced by "fair". Moreover, we have selected eight profiles to illustrate the latitudinal differences. Then, on Figure 2 there are no systematic biases between sondes and retrievals. Finally, we would like to point out that the other referee found the comparison rather adequate.

6) Figure 3: it would be better to show del D and del H2-18O rather than volume mixing ratio. > The del D and Del H2-18O are also represented on the figure 4.

7) The discussion of the deuterium excess around equation 15 is awkward. I could barely follow it. Is it just a T effect? > T plays a major role, but the relative humidity is an important factor too. The objective was to show that it was possible to obtain deuterium excess values. However, due to the large uncertainties on this parameter, we haven't push the analyses further and found it therefore unnecessary to give more details. The section has been simplified in the revised manuscript.

8) There are several references (Gettelman and Webster, Webster and Heymsfield) which use only the first author, and 2 should probably be used if there are only 2 authors. > This has been corrected.

9) I did not follow the discussion at the end of section 3: are the profiles of H2O and the isotopologues not co-incident? Why use just every other profile? > Each detector had a field of view corresponding to 8*8 km footprint on the ground and each footprint is separated from the other by a distance of 4 km. The bands 2 and 3 are thus not perfectly co-incident. HDO and H2O18 are retrieved from different bands, which increases the error of the HDO/H2O18 ratio determination.

10) Table 2 is not necessary. > We agree with the referee and the Table 2 has been removed.

11) Figures 5-7 should be plotted with some better display of how much information exists: perhaps using colored squares to explictly show the grid. You could also plot

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with small black symbols in at least 1 panel the locations of all the profiles (along the lines of Worden et al 2006). This would facilitate more analysis. $>$ We agree with the referee that this information was missing. We have added the locations of all the profiles on the DOFS global maps (Figure 5) with black full circles.

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

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