

## Reply to referee report 2 by M. Schneider

We would like to thank the reviewer for the comments and valuable suggestions. The referee is particularly concerned about the way satellite data are characterized and we acknowledge the insightful review. In the following, we provide detailed information about the changes.

### Comments:

(1) Please inform about the a priori information:

Remote sensing generally means updating a priori information by adding information from measured radiances. The manuscript should briefly mention the a priori information used for producing the different data. This is especially true in case the a priori is varying, because then it is very difficult to understand what in the product can be attributed to the measurement and what has been already there in the a priori. For isotopologue products the  $\text{H}_2\text{O}$  and the  $\delta\text{D}$  a priors are important. I think both TES and SCIAMACHY retrievals use varying  $\text{H}_2\text{O}$  a priori (which will also affect the  $\delta\text{D}$  column averaged a priori, see below).

We have extended the description of the a priori information used in the TES and SCIAMACHY datasets. We describe TES prior in the first paragraph in section 4.1 (P9108L18-26) followed by SCIAMACHY prior in the next paragraph (P9109L6-9). The difference between TES and SCIAMACHY is that a single a priori profile of the  $\text{HDO}/\text{H}_2\text{O}$  ratio from TES was obtained from the NCAR-CAM model while for SCIAMACHY a fixed  $\delta\text{D}$  prior is assumed, with -100 ‰ at the lowest layer to -500 ‰ at the highest layer. We also mention that only the altitude effect is seen in TES prior (P9108L26) and no single isotope effect is seen in SCIAMACHY prior (P9109L4-5). Thus, the isotope effects that we discuss from the satellite products are a robust result of the added information from the satellite measurements (P9110L27-P9111L1). We provide a priori plots for SCIAMACHY and TES in figure 1a and 1i. Seasonal comparison of  $\delta\text{D}$  TES prior is also plotted in Fig.3a.

(2) Please discuss the issues involved with using  $\delta\text{D}$  column averaged data (it depends on the vertical distribution of  $\text{H}_2\text{O}$ ):

There are some important differences between the column averaged  $\delta\text{D}$  (used in this paper) and the  $\delta\text{D}$  at a single level as offered within MUSICA (NDACC+IASI) or for TES and the IASI product of ULB. Generally I would recommend using  $\delta\text{D}$  representative for a single level (and not integrated over a column/partial column). In the context of this paper such data would be available for TES, but not for SCIAMACHY. If the authors decide working with column-averaged  $\delta\text{D}$  some discussion is needed.  $\delta\text{D}$  columns are a weighted  $\delta\text{D}$  mean (weighting is made with the  $\text{H}_2\text{O}$  mixing ratios, see Equation 2 of their manuscript). Actually the column-averaged  $\delta\text{D}$  depends on the vertical  $\text{H}_2\text{O}$  distribution. Do we know how much of the  $\delta\text{D}$  we report as column averaged  $\delta\text{D}$  is a measurement that adds something to the vertical  $\text{H}_2\text{O}$  distribution? I think this is very difficult to quantify and it should be discussed that to "some" extent the column averaged  $\delta\text{D}$  variability is due to variability in the vertical distribution of  $\text{H}_2\text{O}$  (and no

measurement of  $\delta D$ !). For instance, even if the  $\delta D$  a priori profile is kept constant the  $\delta D$  column averaged a priori varies in response to variations in the  $H_2O$  a priori profile. The variation in the column-averaged  $\delta D$  a priori needs to be documented in all the Figures or at least the magnitude and the effect needs to be discussed in the text.

We agree with the reviewer that the results using  $\delta D$  column averaged and  $\delta D$  column weighted averaged are different and that the decision to work with one of them always involves specific advantages/disadvantages. Obviously the  $\delta D$  column weighted average is influenced by  $H_2O$  vertical distribution. We stick to the use of the  $\delta D$ -weighted average in our paper since SCIAMACHY has only  $\delta D$  column weighted average values. In response to the referee comment, we added a sub section describing the difference between  $H_2O$ -averaged and arithmetically averaged results and the influence of the  $H_2O$  vertical distribution on  $\delta D$  in our revised manuscript. We have added the SCIAMACHY prior plot to Figure 3. We prefer not to show the a priori plots in all figures because in the subsequent figures we focus on the results of the satellite measurements and it would be too repetitive to show the prior multiple times.

(3) Please discuss the effect of cross dependencies ( $\delta D$  on  $H_2O$ ) in the observations:

For the model you can easily correct cross dependencies by using equation (7,  $ECHAM_{AK5Corr}$ ) instead of equation (5,  $ECHAM_{AK5}$ ). But there are also cross correlations in the retrieved remote sensing data, which can (only) be well corrected by the a posteriori method. I do not understand why you discuss the cross correlation for the model (where you can easily consider it by equation (5)+(7) and not for the observations, where it is very important and can be well considered by the a posteriori method (as you demonstrate by your comparison  $ECHAM_{AK5Corr}$  vs.  $ECHAM_{AK5Pos}$ , page 9110, lines 12-23).

Point taken. For the model results, we present now both the a posteriori method and the humidity bias correction method (eq.7). We performed the a posteriori analysis for  $TES_{V5}$  dataset ( $TES_{V5Pos}$ ) and the results are similar to the original  $TES_{V5}$ . Therefore, we did not present  $TES_{V5Pos}$  in our figures but plotted the  $ECHAM_{AK5Pos}$ . We added the  $TES_{V5Pos}$  results and added also a discussion on the posteriori analysis for remote sensing datasets.

Please read the work of Schneider et al. (2012), Lacour et al. (2012), Pommier et al. (2014) or Wiegeler et al. (2014) where this a posteriori method is well discussed and applied. Since you made the tests with  $ECHAM_{AK5Pos}$  you have the original and the a posteriori corrected averaging kernels ( $A$ ,  $A'$ , and  $A''$ , according to Schneider et al., 2012). These kernels should be shown in an Appendix, so that the sensitivity of the remote sensing product and the cross dependencies can be adequately discussed. Related to these issues is the discussion of Figure 3B: the blue and red lines show the difference between  $ECHAM_{AK5}$  and  $ECHAM_{AK5Corr}$ . There are differences of up to 30 ‰. You do not correct possible cross dependencies in the TES data: can't it be that a lot of the difference you see

between the JJA and DJF TES  $\delta D$  signals is actually due to a cross dependency on  $H_2O$  and not due to atmospheric  $\delta D$  variations? I think this should be discussed.

We have added the original AK and the a posteriori corrected AK (A and A'') to the appendix. Also a discussion about the a posteriori correction for the TES data has been added. The maximum differences between  $ECHAM_{AK5}$  and  $ECHAM_{AK5Corr}$  in Fig.3b amounts to about 20 ‰ in the Tropics and reaches 40 ‰ in the Southern Hemisphere. We don't think that the difference between red ( $ECHAM_{AK5}$ ) and the blue ( $ECHAM_{AK5Corr}$ ) lines is due to the humidity bias and rather suspect that this difference is due to the seasonality feature of the atmospheric water vapor related to the movement of ITCZ (see our discussion at P9114L21-27). Also  $TES_{V5Pos}$  has been added to this figure.

Similarly the discussion of Figure 4 and 5: How much of the signal in TES (and SCIAMACHY) can be explained by cross correlations of  $\delta D$  on  $H_2O$ ?

SCIAMACHY does not provide cross correlation AK between HDO and  $H_2O$ . Therefore it was impossible to us to provide the full averaging kernel and to calculate the proxy kernels for  $\delta D$  and  $H_2O$  with the current retrieval setup.

#### (4) Sensitivity with respect to $\delta D$ and $H_2O$

I have severe problems in understanding Figure 6. It shows total column of  $H_2O$  versus total column averages of a proxy of  $\delta D$ . However, the sensitivities for  $H_2O$  and  $\delta D$  are completely different! For instance, for IASI we and other authors get a DOFS for  $H_2O$  of about 4 and for  $\delta D$  of about 1, meaning that  $\delta D$  and  $H_2O$  are representative for significantly different water masses. How can it make sense to relate both quantities? You relate  $H_2O$  representative for a certain water mass to a  $\delta D$  values that represents a different water mass! This problem has been well identified by Schneider et al. (2012). There are several examples how this problem can be avoided (Schneider et al., 2012; Lacour et al., 2012; Pommier et al., 2014; Wiegeler et al., 2014) and I recommend having a look on those papers.

The Figure below shows what we observe with IASI. On the left we show a plot similar to the one shown by the authors, and on the right after applying the a posteriori correction thereby assuring that  $H_2O$  and  $\delta D$  are representative for the same water mass. In addition, Figure 6 as shown in the author's manuscript might be affected by cross correlations and it shows column-averaged data, meaning that the  $\delta D$  proxy is affected by the varying  $H_2O$  distribution. I think it has to be documented and discussed what in Figure 6 is a  $\delta D$  measurement, what is a priori information, and what is actually presented (I do not understand how you can draw conclusions by plotting  $\delta D$  and  $H_2O$  that are representative for different water masses).

Point taken. We have added the results from the a posteriori analysis of  $TES_{V5}$  and  $ECHAM_{AK5}$  ( $TES_{V5Pos}$  and  $ECHAM_{AK5Pos}$ ) to figure 6, which shows that the results are not caused by artifacts. In order to strengthen the scientific interpretation of the figure, we have performed an additional investigation to separate the water and isotope changes in the vertical dimension from the horizontal dimensions. This analysis shows that the  $\delta D - H_2O$  correlation is more

Rayleigh-like in the vertical dimension than in the horizontal dimension. This is an interesting finding that has to our knowledge not been reported previously.

(5) SCIAMACHY data.

All my concerns discussed in my points (1) – (4) are also true for SCIAMACHY. For TES you can address point (3) and (4) by a posteriori corrections but for SCIAMACHY I don't think that this is possible. I think a lot of care is needed when drawing conclusions from the SCIAMACHY data. At the moment a paper for validating SCIAMACHY isotopologue remote sensing products is in preparation, which will be very helpful in this context and probably allow more robust conclusion in the future.

We agree that a more detailed analysis of the SCIAMACHY data concerning cross correlations and humidity bias is important. Here we have no access to vertically resolved information from SCIAMACHY ( $\text{H}_2\text{O}$  or  $\text{HDO}$ ), the AKs are close to 1 both for  $\text{H}_2\text{O}$  and  $\text{HDO}$  (see Frankenberg et al., 2009, supplementary material). Therefore, for example, an a posteriori analysis as for TES is not possible. However, we added discussion on this issue in the paper.

(6) Some minor comments

Figure 1: why don't you plot the a priori for the data you use in the other Figures? Here you plot a priori for the average between 850 and 500 hPa. Later on you show total column averaged data. I think it would be better to plot here also the a priori for total column averaged data.

The seasonal TES prior is shown in Fig.3a. We prefer not to add the a priori information in all figures, because this would be too repetitive. Total column here means weighted average of  $\delta\text{D}$  between 850 and 500 hPa for V4 and between 900 and 425 hPa for V5, respectively. We have added this information in all figure captions.

Figure 2+3: please specify that you show total column averaged data.

We have specified that the "total column" data are the values from the height interval that the instrument is sensitive to (500-850 hPa for V4 and 900-425 hPa for V5).

Final remark:

I hope that my arguments encourage the authors for working on an improved characterization and a more critical discussion of their remote sensing data. Their paper is interesting and addresses an important and promising research field. However, isotopologue ratio remote sensing data have a complex nature and we will be most successful with this research if we comprehensively consider the complex characteristics of such remote sensing data. In the meanwhile there are good examples in literature about an adequate treatment of

this kind of remote sensing data, which could be very helpful for the authors for improving the manuscript.

We agree with the reviewer about the complex nature of water isotopologues measurements strongly depending on both information the actual  $\delta D$  composition and the  $H_2O$  mixing ratio. We feel that we could address the raised question though some of the points couldn't be fully solved here with the available information.