Global remote sensing observations of water vapour isotoplogues can significantly contribute to investigate the different processes controlling the distribution of atmospheric moisture: large scale circulation, turbulent mixing, isentropic mixing, dry and moist convection (entrainment, lifting of ice/liquid, re-evaporation of cloud condensate/rain), etc.

The involved processes are manifold and interconnected and models are very helpful for interpreting the observations. Both models and remote sensing measurements have uncertainties, which have to be discussed and considered adequately if we want to be successful in this promising research field.

The paper by Sutanto et al. discusses how observed and modelled large-scale isotoplogue signals are related to well-known isotopologue effects and propose using their method for evaluating observational isotopologue datasets.

The paper is an important contribution to this research field, but the authors should be more careful in discussing and characterizing their remote sensing data. In the following I list my concerns. I think that some of their conclusions should be revised, once the characteristics of their remote sensing data are correctly discussed.

(1) Please inform about the apriori information:
Remote sensing generally means updating apriori information by adding information from measured radiances. The manuscript should briefly mention the apriori information used for producing the different data. This is especially true in case the apriori 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 apriori. For isotopologue products the H2O and the delD apriors are important. I think both TES and SCIAMCHY retrievals use varying H2O apriori (which will also affect the delD column averaged apriori, see below).

(2) Please discuss the issues involved with using delD column averaged data (it depends on the vertical distribution of H2O):
There are some important differences between the column averaged delD (used in this paper) and the delD at a single level as offered within MUSICA (NDACC+IASI) or for TES and the IASI product of ULB. Generally I would recommend using delD 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 delD some discussion is needed. DelD columns are a weighted delD mean (weighting is made with the H2O mixing ratios, see Equation 2 of their manuscript). Actually the column averaged delD depends on the vertical H2O distribution. Do we know how much of the delD we report as column averaged delD is a measurement that adds something to the vertical H2O distribution? I think this is very difficult to quantify and it should be discussed that to "some" extent the column averaged delD variability is due to variability in the vertical distribution of H2O (and no measurement of delD!). For instance, even if the delD apriori profile is kept

constant the delD column averaged apriori varies in response to variations in the H2O apriori profile. The variation in the column averaged delD apriori needs to be documented in all the Figures or at least the magnitude and the effect needs to be discussed in the text.

(3) Please discuss the effect of cross dependencies (delD on H2O) 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 aposteriori 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 aposteriori method (as you demonstrate by your comparison ECHAM_AK5Corr vs. ECHAM_AK5Pos, page 9110, lines 12-23).
Please read the work of Schneider et al. (2012), Lacour et al. (2012), Pommier et al. (2014) or Wiegele et al. (2014) where this aposteriori method is well discussed and applied. Since you made the tests with ECHAM_AK5Pos you have the original and the aposteriori 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 permil. 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 delD signals is actually due to a cross dependency on H2O and not due to atmospheric delD variations? I think this should be discussed.
Similarly the discussion of Figure 4 and 5: How much of the signal in TES (and SCIA) can be explained by cross correlations of delD on H2O?

(4) Sensitivity with respect to delD and H2O
I have severe problems in understanding Figure 6. It shows total column of H2O versus total column averages of a proxy of delD. However, the sensitivities for H2O and delD are completely different! For instance, for IASI we and other authors get a DOFS for H2O of about 4 and for delD of about 1, meaning that delD and H2O are representative for significantly different water masses. How can it make sense to relate both quantities? You relate H2O representative for a certain water mass to a delD 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; Wiegele 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 aposteriori correction thereby assuring that H2O and delD 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 delD proxy is affected by the varying H2O distribution. I think it has to be documented and discussed what in Figure 6 is a delD measurement, what is apriori information, and what is actually presented (I do

not understand how you can draw conclusions by plotting delD and H2O that are representative for different water masses).

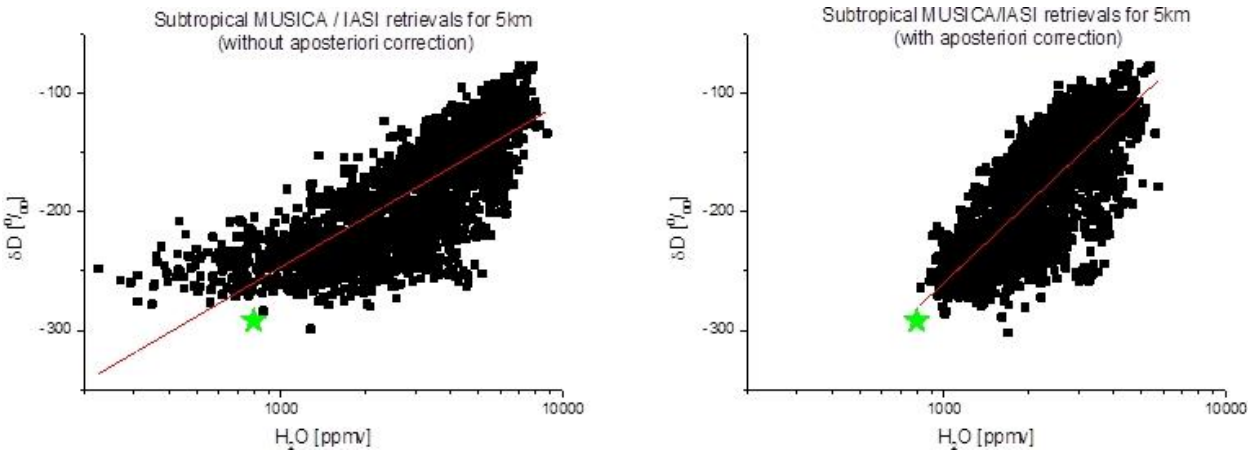

Figure: Subtropical {H2O, delD} distribution retrieved from IASI spectra at 5km altitude (black dots) using a single fixed apriori (green star). Left panel: without the aposteriori correction, i.e., for H2O and delD having different sensitivities and being representative for different water masses. This plot makes few sense! Right panel: with aposteriori correction, i.e., H2O and delD having almost identical sensitivities and being representative for almost identical airmasses.

(5) SCIAMACHY data.

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

(6) Some minor comments

Figure 1: why don't you plot the apriori for the data you use in the other Figures? Here you plot apriori 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 apriori for total column averaged data.

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

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.