Reply to Referee 2 Comments

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Can the assimilation of water isotopologue observation improve the quality of tropical diabatic heating and precipitation?

We thank referee 2 for the constructive, helpful criticism and the suggestion for revision. We have thoroughly revised the manuscript based on the comments given by the referees. A detailed point-by-point response to the comments by referee 2 are given below.

The aim of this paper is to show the benefit of stable water isotope observation assimilation for improving the representation of diabatic heating and precipitation in the tropics. A theoretical approach is chosen based on Observation System Simulation Experiments (OSSEs). The OSSEs are nearly the same as the ones presented earlier this year in Toride et al. 2021. While I do think that water isotopes contain valuable additional information on atmospheric circulation characteristics and moist diabatic processes in the atmosphere, I am very skeptical about their direct usefulness in data assimilation. In my view, there is no evidence provided in this paper that would support such a conclusion.
With the upcoming next generation of Metop satellites, isotope measurements from IASI will be available for the next decades and thus isotopes can definitely be valuable in data assimilation, especially together with water vapour. Since the highest improvements for the assimilation experiments were derived when both, isotopes and water vapour, are assimilated as was shown by Toride et al. (2021), it would be of course optimal if both species would be assimilated together. Note, IASI data are currently not operationally assimilated. However, the major intention of our study is not to show with which data set the highest improvement can be derived. Our intention is to understand the direct impact the assimilation of isotopes has on the meteorological analyses. This is why we only use the experiment from Toride et al. (2021) where only δD additionally to conventional observations is assimilated and compare this to an experiment that has been performed in the frame of this study where δD is assimilated alone without any other data. We hope that the revisions we made on the manuscript based on the referee's comments make the intention of our study and the outcome now more clear.

The major reasons, why I think that the paper is difficult to understand in the current form are:

1) Contradiction in stated hypothesis of the physical reason for the added value of isotopes in data assimilation and the outcome of the second OSSE experiment
As stated by the authors in the introduction, the rationale for the use of isotope observations to improve various meteorological fields such as T,q,u,v is that they are tracers of moist diabatic processes in the atmosphere. Thus, via improvements in diabatic heating rates in models, isotope assimilation leads to improvements in other fields. However, that is not what the authors observe in their second OSSE, in which they only assimilate δD. In the noDavsDa experiment the authors find an improvement in all variables except those (ω, Q1, Q2), for which we would expect a direct physical link with the mid tropospheric δD distribution to exist. This contradiction is very disturbing for the readers and unfortunately not addressed at all by the authors. Based on this result, what do the authors think, is the reason for the improvements observed in the other meteorological fields?

The time series in Fig. 9 and 10 actually show that the assimilation of δD is not failing. On a qualitative basis the assimilation of δD improves these parameters, too. Only when the performance is assessed quantitatively using the skill we derive a less good agreement for ω, Q_1 and Q_2 than for the other parameters. A possible explanation is that the other meteorological parameters do not have as strong fluctuations as ω, Q_1 and Q_2, especially since these ones variate around zero. Thus, an accurate simulation of ω, Q_1 and Q_2 is much more difficult and therefore we derive only an improvement when already the underlying physics (dynamics) are correctly simulated. We make this point clearer now in the manuscript and changed/added the following text in the conclusion: "The noDAvsDA experiment shows that on a quantitative basis the assimilation of IASI δD alone cannot significantly improve the heating rates. However, the assimilation of δD has a positive effect on all other parameters including precipitation. Further, that we derive a qualitative agreement for ω, Q_1 and Q_2 when IASI δD alone is assimilated may explain why nevertheless precipitation rates can be improved. Furthermore, together with the conventional observations from PREPBUFR an additional improvement for all parameters, including the heating rates, can be achieved and shows the benefit of the IASI δD data. This indicates that the correct simulation of the underlying physics is important for improving diabatic heating and vertical motion."

2) Observation density
Since δD assimilation can only lead to substantial improvements in diabatic heating when assimilated together with conventional observations, the question about the observation density arises. This should be discussed and an assessment of the observation density differences in the PREBUFR experiments should be provided. I know that this is done in the supplement of Toride et al. 2021, but I think this is so essential that it cannot just be left out of the discussion in this paper. Increasing the number of conventional observations at the locations of assimilated IASI δD (e.g. q profiles from IASI) instead of δD would maybe lead to even larger improvements.
We agree that this point should not be left out of the discussion and it is correct that with the assimilation of IASI q or both q and δD higher improvements can be derived. However, there is no point in repeating exactly the same what is already done in Toride et al. (2021), especially since referee 1 already thinks we are too close. We focus here solely on the assimilation of isotopologues since the intention of our study is to investigate the direct impact the assimilation of isotopologues have on the diabatic heating rates. Therefore, we use the experiment of Toride et al (2021) assimilating IASI δD additionally to conventional observations and compare this to an experiment performed in the frame of this study where we assimilate only δD without any other data. Nevertheless, to make clear that the assimilation of q alone or of both, δD and q, is more successful in terms of improvement we added in Sect. 3.2 the following text: "Note, that in terms of improvement, however, the assimilation of IASI H_2O or even both, IASI H_2O and IASI δD is more efficient and leads to higher improvements (Toride et al., 2021)". Further, to make the intention of our study more clear and to better describe the differences between the study by Toride et al (2021) and our study, we added the following paragraph in the introduction: "Here, we build on the study by Toride et al. (2021) and investigate this latter issue further, namely which information is hold by isotopologues? Especially, we are interested in answering the following question: Can the information stored in water isotopologues help to improve diabatic heating rates and/or precipitation rates? For that we use the assimilation experiment assimilating isotopologues from the study of Toride

et al. (2021) and compare this to an additional OSSE performed in the frame of this study where we assess the direct impact of the IASI isotoplogues on the meteorological variables. In the additional OSSE the IASI isotopologues are assimilated alone (without any conventional observations) and then compared to an ensemble simulation where no observations at all are assimilated."

3) Motivation for chosen tropical region delimitation I missed a clear motivation for the chosen tropical regions, over which the δD induced improvements in data assimilation are quantified. Why not focusing on known ascent dominated regions along the ITCZ vs. subsidence dominated regions further away from the equator? In the current form I did not gain any process-based insight from the regional categorization.

We apologize that we have not been clear and thus caused some confusion. In our study we focus on the Walker circulation, thus on the circulation cells in east-west direction (longitudinal direction) and not the ones in north-south direction (latitudinal). The cross sections we show in the manuscript are longitudinal ones. To make this more clear now throughout the manuscript we added the suffix "longitudinal" before cross sections and added a paragraph in the introduction motivating our regions of choice and introducing the Walker circulation. We have added the following text: "In this study, we focus on the inner tropics (10°S to 10° N), to assess the impact of isotopologues on the assimilation in the region where diabatic heating is strong and where the Walker circulation is found. The Walker circulation is a longitudinal (east-west) circulation pattern consisting of several circulation cells spanning over the entire tropics. Convection and heavy precipitation associated with the rising branches of the Walker Circulation occur over Indonesia and the western Pacific, northern South America, and eastern Africa while sinking air and desert conditions prevail over the eastern equatorial Pacific and west Africa (Peixoto and Oort, 1992; Lau and Yang, 2003; Webster and Chang, 1988)". Additional to separating the tropics into the three regions over land (Asia, America and Africa), we also separate the tropics by upward and downward branches of the Walker circulations (see Discussion and Fig. 13 and 14 (now Fig. 12 and 13) in the manuscript).

4) Missing discussion on precipitation improvements Even though improvements in modelled precipitation seem to be expected through improvements in diabatic heating profiles, I find the discussion about precipitation too sparse to allow for such a prominent place in the title.

We improved our discussion of precipitation throughout the manuscript, especially in the discussion and conclusion. Nevertheless, we decided to remove "precipitation" from the title since we still do not discuss precipitation to that extent as we discuss diabatic heating.

Minor comments:

- *Many parts of the paper are a bit lengthy in writing and in the shown Figures. For example:*
  - *A lot of information is given about IASI, even though no real IASI data is used.*
    This is correct and the respective section has been omitted and the for this study required information on IASI has been moved to section 2.3 (now 2.2)
  - *I cannot see the differences in the profiles shown in Fig. 6.*
    This is correct, differences between the assimilation experiments and the Nature are, as for the tropics, quite low and become only visible when the

MD, RMSD and skill are considered. However, we use this figure to describe the different characteristics of the three regions considered and showed the three assimilation experiment for the sake of completeness. The text has been changed as follows to make this clear: "Figure 6 shows the averaged ensemble mean profiles for Q_1, Q_2 and vertical velocity averaged over the respective regions for August 2016. As for the tropics, differences in the averaged mean profiles between the Nature and assimilation runs are quite low and become only visible when the mean differences between the assimilation run and Nature run are considered (Fig. S5)." Further, based on the comments by referee 1 the description of the differences of the regions has been shortened.

- *What can I learn from Figures 9 and 10?*
  Based on the comment by referee 1 we now show instead of the time series of the mean differences the time series of the absolute values (which we before had in the supplement). In the time series of the absolute values one can clearly see the positive effect the isotope assimilation has. Without any data assimilation the ensemble mean depicts only the climatological conditions. When the isotopes are added a significant improvement between the Nature and the assimilation experiment is found, especially for America. Due to the assimilation of δD the synoptic-scale variations are introduced correctly, but differences concerning the daily variations remain which lead to less improvement in the skill.
- *The role of Section 3.4 about the δD-δ^{18}O relation and dexcess is not clear to me and does not fit well into the storyline.*
  We use the δD-δ^{18}O and the d-excess to assess the performance of the assimilation experiments and to investigate the differences we find concerning the performance for the three regions considered in this study. We revised the section to make this point clearer and added the following text in the introduction (where we give an outline of the paper structure) to make our intention with this analyses clearer: "Finally, we exploit the δD-δ^{18}O relationship (Daansgard et al., 1964) and d-excess (Craig et al., 1961) which serves on one hand as a further assessment and on the other hand helps us to better understand the differences in performance for the specific tropical longitude regions considered in this study."

- *I did not understand the difference between the individual ensemble members. Were they just initialized at different times from the nature run? If yes, why are they different from the nature run, then? Or are the initial conditions perturbed with respect to the nature run?*
  The isoGSM simulation that has been used to generate the Nature has been performed for 2-years starting on 1 June 2015. The 96 ensemble members are initialised with the conditions from 1 June 2016 onwards (consecutively every 6 h), thus with the meteorological conditions prevailing one year later. These initial conditions can be considered as being independent from the Nature, but representing similar climatological conditions.