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. 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?

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.

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.

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.

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
  - I cannot see the differences in the profiles shown in Fig. 6.
  - What can I learn from Figures 9 and 10?
  - 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.
- 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?