Reply to Referee 1 Comments

Manuscript-No: wcd-2019-49

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

We thank referee 1 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 1 are given below.

This paper documents the improvements in the simulation of meteorological fields in the tropics through the assimilation of synthetic isotopic observations. The added value compared to Toride et al 2020, a previous paper by the same group, is not clear (major comment 1.1). The paper is long, giving the impression of new information compared to Toride et al, but this is because the paper includes many paragraphs that are not clearly connected with the subject (major comment 1.2). Another major problem is that the added value of the assimilation of isotopic observations compared to simpler humidity observations is not clear, and not even discussed. I fear that readers of this paper could be mislead about the advantages of assimilating water isotopic observations (major comments 1.3). There is also a problem with the definition of the regions of interest, which seems to stem from misunderstanding of the global precipitation distribution and seasonal migration of the ITCZ (major comment 1.4).
We apologize for not having been clear in our manuscript which seems to have caused some confusion. We are focusing in our study on the Walker circulation which spans in east-west direction over the entire tropics. This is why we chose the latitude band between 10°S to 10°N. Further, there are clear differences between the study by Toride et al. (2021) and our study as described below. This will be better pointed out in the revised manuscript. Furthermore, it was not our intention to mislead the readers on the advantages of assimilating isotopic observations. We will also make this point more clear in the revised manuscript and will discuss the possibilities and limitations.

1 Major comments
1.1 The added value of this paper compared to Toride et al 2020 is not clear Toride et al already documents the added value of assimilating water isotopic observations to improve the simulation of atmospheric variables. l 59-63, the authors justify the added value of this study compared to Toride using two arguments:
The main difference between our study and the study by Toride et al. (2021) is that Toride et al. (2021) have their main focus on the assessment of the assimilation experiments using IASI δD, IASI water vapour or or both on a global scale and on the separation into thermodynamic and dynamic contributions while we try to assess the direct impact that the assimilation of δD has on the meteorological analyses and especially on the diabatic heating rates in the tropics. We therefore compare the assimilation experiment with IASI δD additional to conventional observations (PREPBUFR experiment) from Toride et al. (2021) to one assimilation experiment that has been performed in the frame of this study where only IASI δD is assimilated without any other observations (noDAvsDA experiment). We select three specific regions in the tropics and additionally separate the tropics into the upward and downward branches of the Walker circulation to assess the performance of the assimilation experiments there. Additionally, we consider the δD-$\delta^{18}O$ relationship and the d-excess which serves on one hand as a further tool for

assessing the performance of the assimilation experiments and on the other hand helps us to explain the differences in performance for three regions considered. Therefore, there are clear differences between our study and the study by Toride et al. (2021). We hope that the changes we made in the introduction (the 5th and 6th paragraph has been rewritten) make the differences between the two studies more clear.

- *Toride et al was global whereas this paper focuses on the tropics. Yet, in Toride et al, there were already maps and cross sections allowing us to see what happens in the Tropics.*
  The study by Toride et al. (2021) is mainly showing global results. The global maps shown in Toride et al. are only for one altitude level, namely for 500 hPa. Thus, there one only can see what happens (compared to other regions on the globe) at 500 hPa. In our study we solely focus on the tropics (especially the inner tropics) and assess the performance in this region by using profiles (assessing the performance over the entire troposphere) and time series (assessing the performance over one month considering the temporal fluctuations). The longitude-pressure cross sections shown by Toride et al (2021) are for the circulation cells between 0-30°N and a specific longitude range in the tropics while we show and consider in our study the inner tropics and all circulation cells of the Walker circulation spanning over the entire tropics, separate the tropics in three regions and consider the performance there and also separate by upward and downward branches of the Walker circulation.
- *This paper focuses on latent heating profiles. However, Toride et al already included discussion of the effects on latent heating profiles. The vertical circulation and the precipitation, which are documented in this study, were already documented in Toride et al as well.*
  In Toride et al. (2021) only latent heating is shown for the longitude-pressure cross section and the improvement for the circulation is discussed in a specific longitude range for the latitude band 0 to 30°N while we discuss the latent heating for the inner tropics (10°S to 10°N) and for the three regions within the tropics based on vertical profiles (average improvement over the troposphere), cross sections (all longitudes, thus the entire Walker circulation) and time series (temporal fluctuations). Precipitation results in Toride et al. (2021) are only shown on a global map and in the results for the forecast while we here asses the improvement in precipitation based on the time series for the inner tropics and separated by regions.

To make the differences between our study and the study by Toride et al. (2021) more clear, the 5th and 6th paragraph of the introduction have been rewritten.

In addition, the discussion of latent heating and vertical circulation in this study is mainly restricted to a description of the spatial fields, which have already been known for a long time and do not tell anything about the added value of isotopic assimilation (see major comment 1.3).
We apologize that we could not get the message through. Our intention with showing and discussing the vertical profiles and the longitude-pressure cross sections was to describe and explain the differences between the regions. These are then in the end (in the discussion) used to explain why we find differences in the performance and why we have problems with deriving an improvement in heating rates and vertical velocity when only isotopologues are assimilated without any other data (noDAvsDA experiment).

Therefore, the added value of this paper compared to Toride et al is not clear.
A possible added value, which could be interesting, is to try and understand the mechanisms by

which the assimilation of δD improves the latent heating profiles. The improvements described in the paper could be physically explained.
This is actually what we tried. Our experiments show that diabatic heating and vertical motion can only be improved when the underlying physics (dynamics) are already given through the assimilation of conventional observations (the PREPBUFR data and our PREPBUFR experiment, respectively). If only isotopes are assimilated without any other data we get the atmospheric variability to some extent right (the ensemble mean has little variability and thus the mean reflects the climatological conditions), but not the exact fluctuations which results in small or no improvements compared to the Nature run when quantitatively assessed using the skill. This effect and the qualitative improvement is clearly visible in the time series (Fig. S11 and S12 in the supplement).

The paper includes many paragraphs that are not clearly connected with the subject

- *p 3-4: 18 lines are devoted to the description of the IASI observations, yet, these are not used in this study. Rather, synthetic data mimicking IASI observations are used. This sub-section should be reoriented to describe the generation of the synthetic IASI data. A brief description of the IASI observations can be useful with this aim, but not per se, and only the information useful to generate the synthetic data is necessary.*
  This is definitely true. We followed the suggestion by the referee and removed the IASI subsection and moved the parts that are needed for the description of the generation of the synthetic data to the section where we describe the OSSE (former section 2.3, now section 2.2).

- *l 235-265: this describes the vertical circulation and diabatic profiles in 3 regions. These are well-known features of the large-scale atmospheric circulation. This could be recalled in just a few lines, with adequate citations. In addition, the differences between the regions simply reflect the summer location of the ITCZ, not intrinsic properties of convection over different continents as suggested by the paper (see comment 1.4).*
  We have shortened this paragraphs as suggested. However, please note, that we here focus on the Walker circulation and not on the ITCZ. The cross sections we show are longitudinal cross sections, not latitudinal cross sections. To make this more clear throughout the manuscript we have added the suffix "longitude-pressure" before each mentioning of the "cross sections".

- *l 357-366: the connection between this discussion and the effect of assimilation is not clear.*
  We use the δD-δ^{18}O relationship and d-excess to understand the differences in isotopic processes between the three regions considered in the assessment of the assimilation experiments. We try to relate that back to why in one region the assimilation of δD is more successful than in an other region. We have improved this section and hope that this becomes now more clear. Additionally, we added a sentence in the introduction (where we describe the structure of the paper) to motivate the application of the δD-δ^{18}O relationship and d-excess.

- *Fig 15: this is very basic climatology and could go to SI. Again, this simply reflects the ITCZ location relative to the defined boxes (see comment 1.4).*
  We agree that this figure could be moved to the supplement and thus followed this suggestion.

- *l 407-419: the connection with the subject of the paper is not clear. The paper could be much shorter if it was more focused on its initial science question.*
  As mentioned above the δD-δ^{18}O relationship and d-excess are used to understand the differences in isotope processes between the three regions considered in the assessment of the assimilation experiments. We have revised this section to make our intention clearer.

1.3 The added value of the assimilation of isotopic observations compared to simpler humidity observations is not clear
Toride et al. (2021) and also our study shows that the assimilation of IASI isotopes alone leads to an improvement although the assimilation of IASI water vapour or both, water vapour and δD, is more efficient. Further, our main intention was to investigate which information is stored in the isotope data and how the isotopes alone can improve diabatic heating, which is one of the main differences between our study and the study by Toride et al. (2021). The aim of our study was not to present the assimilation experiment that provides the highest improvement. We however agree that we need to make throughout the manuscript more clear that better results in terms of improvement can be derived when IASI water vapour or even both are assimilated.

The water isotopic composition is often strongly correlated with the specific humidity in both observations and models, e.g. [Noone, 2012, Galewsky et al., 2016]. Measuring specific humidity is much easier, cheaper and widespread than measuring the isotopic composition. Therefore, the information gained from isotopic observations is always assessed relative to the information gained from specific humidity.
Isotopic observation are becoming more and more available in the future. Especially with the IASI instruments on the coming generations of Metop satellites will make δD observations with a very high resolution available for the next decades. Further, our intention is to investigate which information the isotopes hold, thus we solely focus in our study on these. Although isotopes are correlated with specific humidity and this relationship is quite valuable for understanding isotopic processes, one does not necessarily needs to exploit this relationship. This definitely depends on the intention of the study. Further, Toride et al. (2021) confirmed with their assimilation experiments what is discussed in Galewsky et al. (2016) (and references therein), that isotopes hold different information than specific humidity (the IASI δD - q vs IASI-q experiment).

Here, the study quantified the improvement associated with the assimilation of isotopic observations, but what is the part of this improvement that we could already have just by assimilating humidity observations? Toride et al showed that actually, the improvement would be even better if assimilating humidity observations than if assimilating isotopic observations, and that the improvement is tiny if assimilating both δD and humidity compared to assimilating only humidity.
We agree that this should be better pointed out throughout the manuscript and improved the text accordingly. Nevertheless, even though the assimilation of water vapour is more efficient this does not mean that the assimilation of IASI δD is worthless.

Therefore, I fear that readers of this paper could be mislead about the advantages of assimilating water isotopic observations. I think that the added value of assimilating isotopic observations should be quantified relative to assimilating both conventional variables and concomitant humidity observations, and not just relative to conventional measurements.

Since specific humidity is not assimilated in PREPBUFR to the extent (spatial and temporal resolution) as provided by the IASI instrument and due to the fact that PREPBUFR is the current state of the art assimilation data set, we think it is more correct to make the assessment relative to this data set to show what one would gain when additionally to PREPBUFR IASI data is assimilated, irrespective if water vapour or isotopes. Further, our intention is to investigate the direct impact of the isotopologue assimilation, thus which information the isotopes hold. Therefore, we solely focus in our study on these.

1.4 Problem with the definition of regions of interest
Why selecting the 10S-10N region, for a month of August? It is well-known that the ITCZ during this month is located further North. Fig 5 illustrates that the defined regions are on the edge of the ITCZ. These boxes are thus heterogeneous, with one part in the ITCZ and one outside. The analysis would be more meaningful if the boxes represented more homogeneous meteorological conditions.
Our major focus is on diabatic heating and the Walker circulation. Studies focusing on this also used the latitude band between 10°S-10°N or even a smaller latitude band of 5°S-5°N (e.g. Fueglistaler et al., 2009; Wright and Fueglistaler, 2013; Dee et al., 2018). Nevertheless, we repeated our assessment for the tropics and separated by regions (average improvement from the profiles) for different latitude bands (20°S to 20°N, 0° to 20°N and 20°S to 0°) to show that our results are robust and do not depend on the chosen tropical latitude band (Figure 1 and Figure 2 in this reply).

What is the rationale for choosing these regions? If the goal is to look at the ITCZ, then boxes further North should be chosen. If the goal is to look at the descending branches of the Hadley cell, then boxes should be chosen further South.
I suspect that the definition of the regions actually stems from mis-understanding of the global precipitation distribution and of the seasonal migration of the ITCZ. This is reflected by the wrong statement in l 400: "the monsoon over Africa is located at this time of the year directly over the equator". It has long been observed that the ITCZ over Africa in summer is located around 10N, as shown by your Fig 5 and documented by many previous studies, e.g. [Thorncroft et al., 2011]. The authors cite Geen et al 2020 for this statement, yet that also show seasonal monsoons in Africa with the ITCZ located around 10○N in summer (see their fig 1).
Our focus is not on the ITCZ and Figure 5 shows a longitudinal cross section. Our point here was that in contrast to the other two regions we get some influence from the monsoon in the here considered African region. According to the comment by the referee, we changed the sentence as follows to be more precise and also added the reference of Thorncroft et al. (2011): "All three regions are affected by the respective monsoons, but in case of Asia and America the monsoon is located further north of our here defined tropical region, while the monsoon over Africa is located at this time of the year at around 10°N and thus within the here defined region (e.g. Thorncroft et al.2011; Geen et al., 2020)."

If boxes were adequately chosen, the additional separation into upward and downward branches (l 376-394) would become useless, and this would additionally simplify the paper (see comment 1.2).
Even if we would move our boxes further north or south, thus using a different latitude band, we would still have different upward and downward branches and a separation into

these would be useful (see Fig. 3 in this reply).

2 Minor comments

- *l 217: "the absence of convection ... leads to strong subsidence": or rather vis versa?*
  You are right, thanks for pointing this out. We changed the sentence as folllows: "Conversely, the strong subsidence over the eastern Pacific leads to the absence of convection (Wright et al. 2013)".

- *l 325: "lacks the synoptic-scale temporal variations": these are not visible on Fig 9. Fig 9 should rather be plotted in absolute values, not differences. Otherwise, we cannot see what is the magnitude of the synoptic-scale temporal variations. l 305: "with respect to temporal variability of this parameter": this is an additional reason why Fig 9 should show the temporal variability, not just differences between simulations.*
  We had provided the figures showing the absolute values in the supplement, but based on your comment we now swapped the figures and put the one with the absolute values for the noDAvsDA experiment in the manuscript and the ones with the differences in the supplement.

- *l 349: "correct in isoGSM": correct to what observations? No observations are shown here.*
  With this statement we just meant, that we derive the expected linear δD-δ^{18}O relationship. We changed the sentence as follows: "Figure 10 shows that simulation of the δD and δ^{18}O relationship is generally correct in IsoGSM since δD and δ^{18}O exhibit for all simulations/assimilation experiments the expected linear relationship."

- *Fig 11: why is the noDA simulation so difference from the Nature run? What is the difference between the noDA and the Nature run, except for different initial conditions? I can understand different synoptic variations, but why such different mean δD-δ^{18}O relationships?*
  The noDA run is an ensemble simulation, consisting of 96 ensemble members, without any data assimilation while the Nature run is our constructed "truth", an free running IsoGSM simulation.

- *l 365: this sentence is not grammatically correct.*
  The sentence has been rewritten as follows: "With respect to the performance of the assimilation experiments, we find that the differences between the assimilation experiments and the Nature run are increasing as wider the value range of the d-excess spans."

- *When δD is assimilated, what happens with δ^{18}O? Is it left free? Or is it assimilated assuming some δD-δ^{18}O relationship? What is the impact of the way δ^{18}O is assimilated (or not) on the results regarding δD-δ^{18}O relationships and d-excess?*
  Only δD is assimilated, but since δ^{18}O is included in the state vector (see description S2 in Toride et al. (2021)) δ^{18}O is also influenced by the assimilation via the

covariance between δD and δ^{18}O. In all our assimilation experiments we derive an improvement for both, δD and δ^{18}O. Thus, we can also expect that the δD-δ^{18}O relationship is to some extent improved, but this improvement will mostly be linear (based on kinetic fractionation) and thus there will be still uncertainties concerning the non-kinetic processes (and thus the d-excess). However, we do not expect that this does have any significant influence on the results derived in our study.

- *l 408: wrong, moistening by rain evaporation is known to increase d-excess in the vapor, e.g. [Tremoy et al., 2014]*
  We agree, it is not as easy as we stated. Thus, we changed the sentence as follows: "D-excess can act as fingerprints of earlier processes as e.g. cloud condensation, evaporation, mixing and air mass transport (Noone, 2012; Tremoy et al., 2014; Aemisegger et al., 2015; Salmon et al. 2019)."

- *l 445: upward or downward? heating or cooling? If both, this sentence should just say "all regions"*
  What we meant here is that high RMSDs are found within the regions of downward motion (that correspond to cooling regions) and within the regions of upward motion (that correspond to heating regions). These regions of high RMSD are not found throughout the entire troposphere and not throughout all longitudes, thus to write here "all regions" would not be correct. For δD these regions are found in the mid to upper troposphere and for Q_2 in the lower to mid troposphere. We changed the sentences as follows: "We found that these regions of high RMSD in δD are located in the mid to upper troposphere at around 150 W, 50 W and 150 E and coincide with regions of both, upward motion (diabatic heating) and downward motion (diabatic cooling) while the regions with high RMSD in Q_2 are located in the lower to mid troposphere between around 150 E to 180 W and coincide solely with regions of upward motion and diabatic heating, respectively."

References:

Fueglistaler, S., Legras, B., Beljaars, A., Mocrette, J.-J., Simmons, A., Tompkins, A. M. and Uppala, S.: The diabtaic heat budget of the upper troposphere and lower/mid stratosphere in ECMWF reanalyses, Quarterly Journal of the Royal Meteorological Society, 135, 21-37, 2009.

Galewsky, J., Steen-Larsen, H. C., Field, R. D., Worden, J., Risi, C., and Schneider, M. (2016). Stable isotopes in atmospheric water vapor and applications to the hydrologic cycle. Reviews of Geophysics, 54(4):809–865.

Dee, S. G., Nusbaumer, J., Bailey, A., Russell, J. M., Lee, J.-E., and Konecky, B.: Tracking the strength of the Walker circulation with stable isotopes in water vapor, Journal of Geophysical Research, 123, 7254–7270, https://doi.org/10.1029/2017JD027915, 2018.

Noone, D. (2012). Pairing measurements of the water vapor isotope ratio with humidity to deduce atmospheric moistening and dehydration in the tropical mid-troposphere. Journal of Climate, 25(13):4476–4494.

Thorncroft, C. D., Nguyen, H., Zhang, C., and Peyrillé, P. (2011). Annual cycle of the west african monsoon: regional circulations and associated water vapour transport. Quarterly Journal of the Royal Meteorological Society, 137(654):129–147.

Toride, K., Yoshimura, K., Tada, M., Diekmann, C., Ertl., B., Khosrawi, F., and Schneider, M. (2021): Potential of mid-tropospheric water vapor isotopes to improve large-scale circulation and weather predictability, Geophysical Research Letters, 48, e2020GL091 698, https://doi.org/10.1029/2020GL091698.

Tremoy, G., Vimeux, F., Soumana, S., Souley, I., Risi, C., Cattani, O., Favreau, G., and Oi, M. (2014). Clustering mesoscale convective systems with laser-based water vapor delta18O monitoring in Niamey (Niger). J. Geophys. Res., 119(9):5079–5103, DOI: 10.1002/2013JD020968.

Wright, J. S. and Fueglistaler, S.: Large differences in reanalyses of diabatic heating in the tropical upper troposphere and lower stratosphere, Atmospheric Chemistry and Physics, 13, 9565 – 9576, https://doi.org/10.5194/acp-13-9565-2013, 2013.

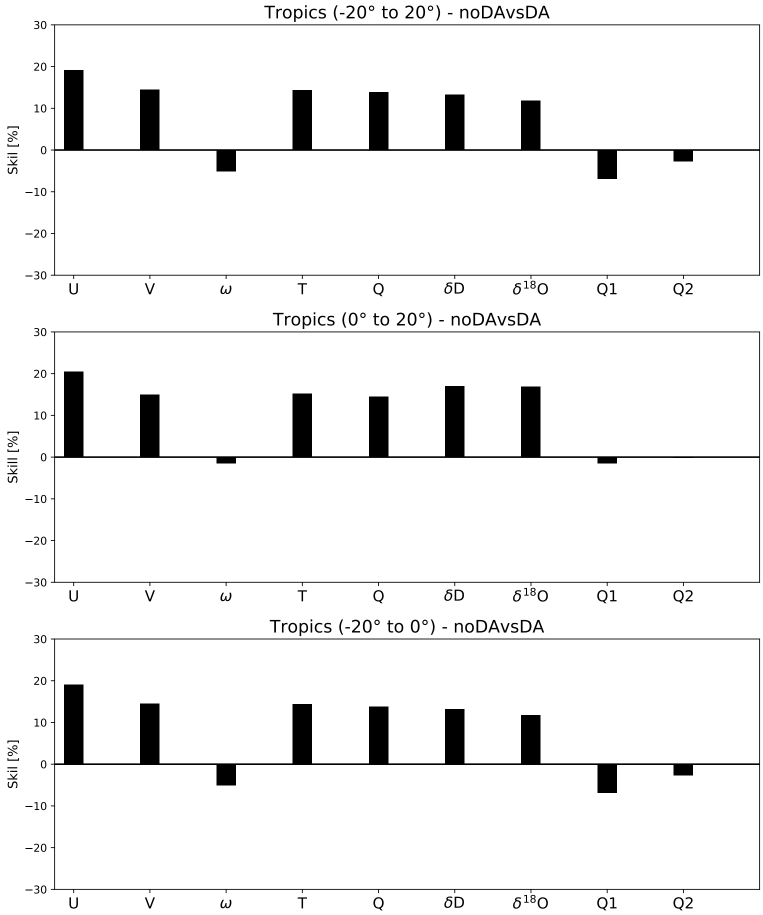

Figure 1: Improvement/degradation in skill in percent for each parameter in the troposphere (up to the 100 hPa level) derived from averaging the vertical skill profiles for the simulation runs with assimilation of the mocked IASI data compared to the simulation without any data assimilation (noDAvsDA experiment). Shown are the result for the latitude band 20°S to 20°N (top), 0° to 20°N (middle), 20°S to 0° (bottom).

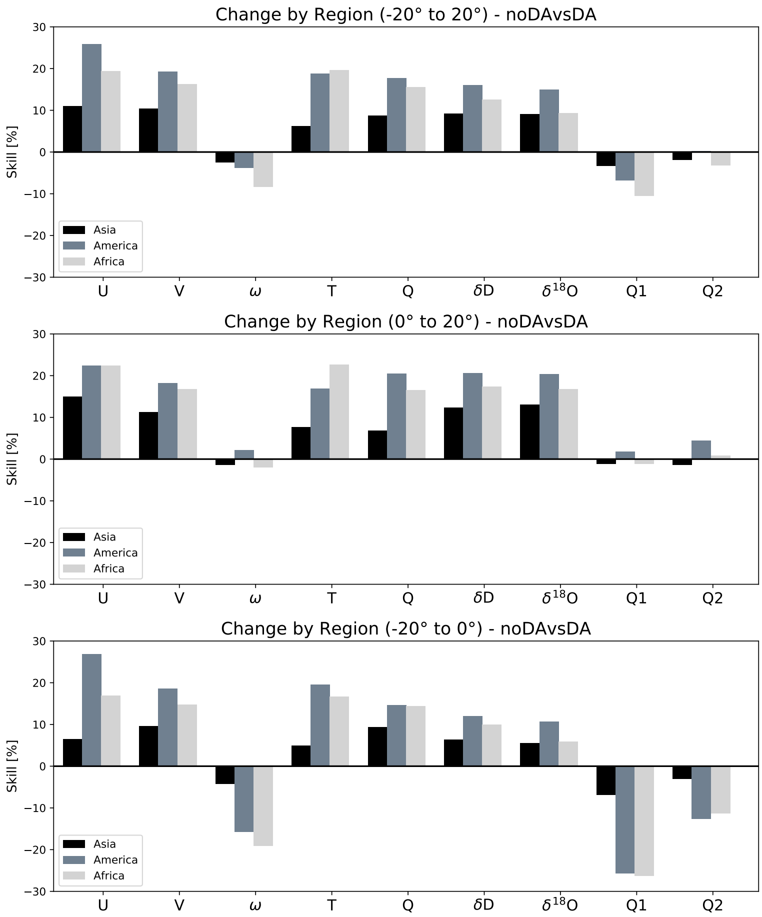

Figure 2: Improvement/degradation in skill in percent separated by regions for each parameter in the troposphere (up to the 100 hPa level) derived from averaging the vertical skill profiles for the simulation runs with assimilation of the mocked IASI data compared to the simulation without any data assimilation (noDAvsDA experiment). Shown are the result for the latitude range 20°S to 20°N (top), 0° to 20°N (middle), 20°S to 0° (bottom).

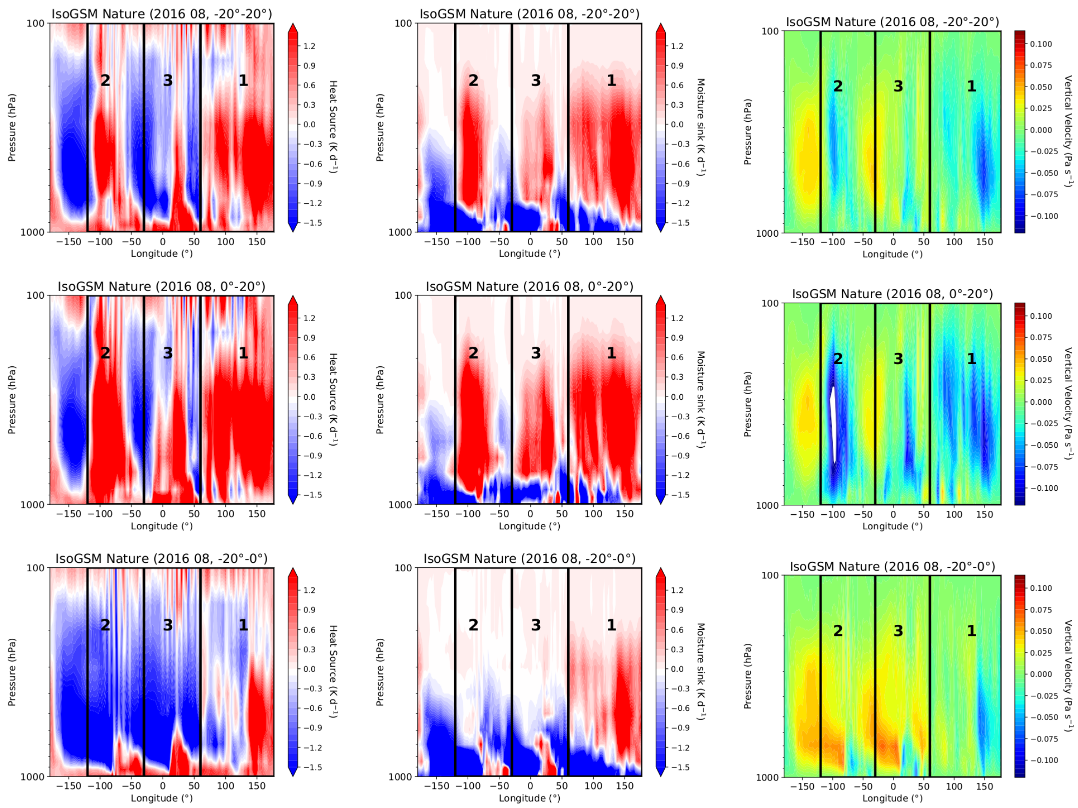

Figure 3: Cross sections for heat source (Q_1), moisture sink (Q_2) and vertical velocity (ω) derived from the Nature run for the latitude bands 20°S to 20°N (top), 0° to 20°N (middle), 20°S to 0° (bottom)