<u>Response to Reviewer 1</u>

We thank Reviewer 1 for the comments and suggestions that helped to improve the new manuscript. We addressed all the comments of the referee (responses in blue) and changed the manuscript accordingly.

- l 12: δ2H and δ17O water isotopes -> 1H2H16O and 1H172O water isotopes

We changed it in the text.

- l 18: Models and the... -> Models. The main isotopic effect and the latitudinal gradient are properly modeled, similarly to previous water isotope-enabled General Circulation Models.

The new text now reads: "*The main isotopic effects and the latitudinal gradient are properly modelled similarly to previous water isotopes-enabled General Circulation Models simulations despite a simplified atmospheric component in iLOVECLIM*".

- l 32: the δ2H and δ18O isotopic ratios of precipitation -> the δ2H and of precipitation

Done.

- l 32: ice cores -> polar ice cores (things are different in tropical ice cores)

We corrected it in the text.

- l 34: , and following ... latitudes : remove, I don't understand what it means

Done.

- l 50: experiment -> experimental

Done.

- l 74: plant lipids wax -> plant lipids or plant wax

We changed it to plant wax.

- l 76: waters -> water

Corrected.

- l 82: missing empty line

We added a line to separate the paragraph.

- l 85: allows estimations of past regional and qualitative changes -> allows qualitative estimations of past regional changes

We changed the sentence accordingly.

- l 93: remove Risi et al 2010, which was not coupled. Rather look for HadCM or GISS references.

We replaced the reference of Risi et al. (2012) by Schmid et al. (2007) for GISS model, and by Tindall et al. (2009) for HadCM model.

- l 99: works -> work

Done.

- l 107: still appears quite suitable -> is suitable

Done.

- l 107: add a sentence on other existing isotope-enabled intermediate complexity models: CLIMBER? Speedier?

We adapted the text that now reads: "*Other isotopes-enabled intermediate complexity models exist like CLIMBER (Roche et al., 2004), or fast GCM like SPEEDY-IER (Dee et al., 2015), that could be used to improve our understanding of the relationship between water isotopologues, second-order parameter (like d-excess) and climate over a broad range of simulated climate changes*".

- l 125: clarify what are the layers: from which level to which level? Are 800, 500 and 200 hPa the middle of the layers?

We detailed the text that now reads: "*It is subdivided in three vertical layers: (1) between the surface and 650 hPa (2) between 650 and 350 hPa and (3) between 350 and 0 hPa. 800, 500 and 200 hPa are respectively the mid-point of each layer*".

- l 138: Merlivat and Jouzel 1979 reference here is out of place. They don't say they look at precipitable water.

We removed the reference from this paragraph.

- l 138: recall that the moisture is assume to be only in the first layer.

We added this clarification to the text.

- l 147: this part is still not clear. All isotope-enabled GCMs adopt the same kind of equation to calculate RE, even though there is vertical discretization of water and isotopes in these models. So I don't think the lack of vertical discretization in iLOVECLIM is what justifies the formula for RE. I advice to clarify what are the consequences of the lack of vertical discretization. e.g. is there a systematic bias in Ra? solution adopted by Roche (2013) is misleading, since the same formula was used in all isotope-enabled GCMs, already long before 2013.

We agree that the solution adopted is very similar to what is used in GCMs. The originality of the formulation of Roche (2013) resides in the use of the apparent humidity h^*_a which is a surrogate consequent to the lack of vertical discretization. Regarding the values of R_a, it is not obvious at present how to recompute an equivalent field from GCM outputs or reanalyses given that in ECBilt this variable represents the isotopic ratio of the total water content of the atmosphere at 850 hPa. However, given that the large-scale results in isotopic content in precipitation (Figure 2 and 3 in the manuscript) is comparable to other GCMs, there should not be a massive bias.

- l 164: clarify the 3 types of precipitation: e.g. how about large-sale snow? Do you mean convective rain, large-sale rain, and snow?

We clarified this sentence to say that we are referring about convective rain large-scale rain and snow.

- l 165: values at 650, 800 and 650 hPa: it was previously written that there was no vertical discretization for water isotopes? Please clarify.

As specified in the ECBilt model description, the model is discretized in three vertical layers (including temperature which is what we refer to in the sentence mentioned), only water content is confined to the first layer.

- l 166: fractionation schemes -> fractionation coefficients?

We replaced the word in the main text.

- l 166: fractionation -> equilibrium fractionation? same l 168?

We corrected it in the main text.

- l 167: enhanced kinetic fractionation at high latitude: do you mean the supersaturation effect? If so, please clarify this, and replace Merlivat and Jouzel 1979 by [Ciais and Jouzel, 1994]. General: How do you account for the kinetic effect associated with the supersaturation at cold temperature? Do you use a linear function of supersaturation as a function of temperature like in all GCMs? Please explain.

Thanks for the comment. We removed the misleading sentence in the paragraph. Currently there is no kinetic fractionation associated with supersaturation at low temperatures.

- l 166-168: this sentence is really not clear. Please replace it by a clear equation, or remove.

See response just above.

- l 227: annual mean -> annual-mean

Same l 506.

We corrected every iteration of annual-mean in the text.

- Fig 2: make text in the keys larger. Same Fig 3

We increased the size of the text for Figures 2 and 3 in the manuscript.

- Fig 3: precise if the values are monthly values. If the case, it represents both spatial and seasonal variations

The values presented in Figure 3 are annual values.

- l 277: with a correlation coefficient of ... 0.99 -> with a correlation coefficient of 0.99

Done.

- l 303: and could be used...: I would replace by ". The same caution should be required for iLOVECLIM as for other GCMs when investigating past changes in d-excess."

Thank you for pointing out this sentence. We modified it in the revised manuscript.

- Fig 4 is wrongly named Fig 2; problem with the numbering of all figures starting here.

We corrected the numbering of the figures in the revised manuscript.

- Discussion around Fig 4: in GCMs, d-excess in high latitudes is very sensitive to the parameterization of supersaturation. Is it also the case here? Or are temperatures not cold enough?

Figure 4 clearly shows that our simulated range of $\delta^2H_{precipitation}$ is not low enough compared to the data. In the range simulated, the effect of supersaturation remains small.

- l 356: even if ... fit -> with most of the data fitting

We removed this text according to the new paragraph to detail the new Figure 6 in the revised manuscript.

- l 402: composition -> ratio. General rule: the ratio is a number, the composition is a qualitative property.

We changed it in the text.

- l 404: to see if ... precipitation. -> because this is where the amount effect is observed.

We modified the sentence.

- l 411: secondary evaporation: what does this mean? Does it mean the rain evaporation? But it was written this process is ignored in iLOVECLIM?

Thanks for pointing it out. We removed the sentence.

- l 414: delay -> advance?

We changed the sentence that now reads: "*A lag of one month is also observed between the data and LMDZ4 for the north tropics*".

- The fact that iLOVECLIM can simulate the amount effect deserves to be discussed. [Lee and Fung, 2008, Risi et al., 2008, Risi et al., 2021] show the key role of rain evaporation in the amount effect. [Field et al., 2010] even shows that disabling the fractionation during rain-vapor interactions suppressed the amount effect in a GCM. The capacity of iLOVECLIM to simulate the amount effect without this fractionation is thus surprising. In contrast, several studies give an integrated water budget perspective to the amount effect [Lee et al., 2007, Moore et al., 2014], which could explain the capacity of iLOVECLIM to simulate the amount effect.

Thank you for providing these inputs, we were unaware of the dispute in the GCMs community about the origin of the amount effect. The arguments presented to interpret the results in the complex model of Risi et al. (2021) are far beyond any process that are modelled in our simplified atmospheric model. This is also in part true for the GCM results of Moore et al. (2014). Our approach is much simpler. The amount effect depletion for us is the process related to sequential precipitation removal and under-replenishment, in the form identified early by Dansgaard (1964). How this relates to the very detailed processes described in Risi et al. (2021) is hard to figure out and certainly beyond the scale of our current study. We would agree with the reviewer that in a way, our simple approach is more comparable to that of Moore et al. (2014), but this would need some confirmation using site specific budget in the model identifying the terms of the budget of Moore et al. (2014), provided that all are accessible in our model (some are clearly not like a separate isotopic ratio of the boundary layer). The resolution of the dispute would not, in any case, change what is currently presented in the manuscript since we computed the iLOVECLIM and GCMs result in the same manner, thus comparing like with like from simple isotopic mean budgets

- Fig 8: why normalizing the values? Is this hiding a problem with the amplitude in iLOVECLIM? I would advice to show the real value, for transparency.

We chose to normalize the values because the seasonal evolution of precipitation and isotopic ratio in the model is not expected to perfectly reflect the measurements, as mentioned in the previous version of the manuscript L377-378.

We present here the raw values in Figure 1. As seen in this figure, the seasonal variation of the precipitation and $\delta^2Hp_{recipitation}$ is the same than the one presented in the manuscript with the normalized values (Figure 8). The lead and lag of the two models compared to the data is also conserved. Differences are however observed in the amplitude, mostly for the isotopic ratio, with lower values up to 15 ‰ in summer for the north tropics between the data and the models. Same difference in absolute values between the observation and the models is observed in the south tropics. We then keep in the revised manuscript figures with normalized values to account for the fact that the seasonal evolution of precipitation and isotopic ratio in the model is not expected to perfectly reflect the measurements.

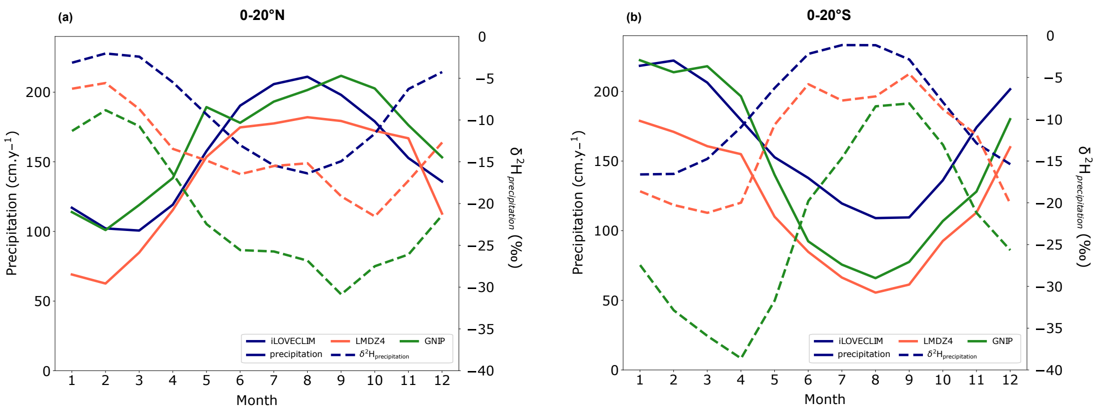

Figure 1: Seasonal variations of the mean precipitation and $\delta^2H_{precipitation}$ in the tropics, from 0-20°N for (a) and from 0-20°S for (b). The solid lines represent the precipitation and the dashed lines the $\delta^2H_{precipitation}$. The blue curve presents the iLOVECLIM values, the red curve is for LMDZ4 and the green curve corresponds to the GNIP data.

- Fig 7: Why is LMDZ alone on its plot? Why is the x-axis unit different for the two plots? I suggest to use the same precipitation unit for all observations and models and plot everything on the same plot. If too busy, then add observations on each plot as a reference.

LMDZ4 was presented on a separated plot because the units for precipitation were in kg/s.m^2, whereas it was in cm/y for iLOVECLIM and GNIP data. Considering that 1 kg of rain water spread over 1 square meter of surface is 1 mm in thickness, we converted the LMDZ4 results to have the same unit than iLOVECLIM and the observations for easier comparison. Results are presented on Figure 2, that replaces the Figure 9 in the revised manuscript. We now compare the amount effect between the two models and the observations (-0.085‰/cm.y^{-1} in iLOVECLIM, -0.103‰/cm.y^{-1} in LMDZ4 and -0.139‰/cm.y^{-1} for the observations).

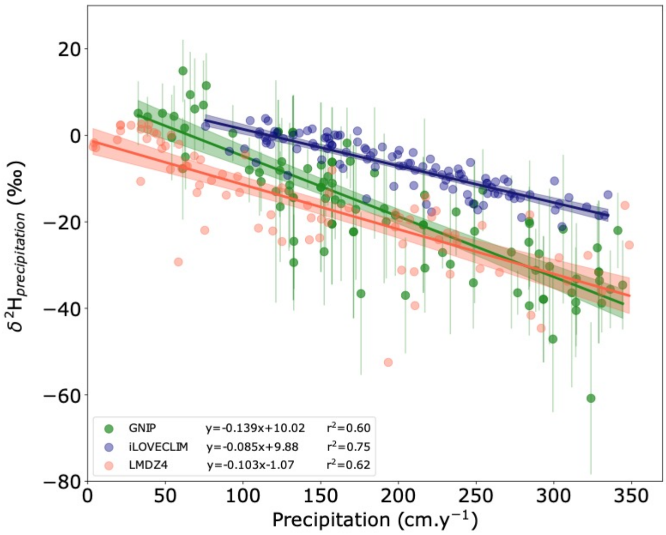

Figure 2: Monthly $\delta^2H_{precipitation}$ as a function of the precipitation at the location of nine tropical oceanic GNIP stations. iLOVECLIM results in blue are compared to LMDZ4 in red and to GNIP data in green. The error bars for the data are shown at 2σ.

- l 441: again, what does 650 hPa mean? Is this an interlayer level?

We specified that 650 hPa corresponds to the top of the first layer.

- sec 3.2.2: why comparing iLOVECLIM with only one GCM? It's OK but needs to be justified: e.g. is LMDZ representative of all other GCMs?

In this section we decided to compare iLOVECLIM results only with LMDZ4 because the Taylor diagram and the multi-model zonal comparison (Figures 2 and 3 in the manuscript) show that the LMDZ4 isotopic composition is within the range of other GCMs. So we consider that LMDZ4 is representative of all other GCMs.

- LMDZ is too enriched at cold temperatures with respect to observations, for reasons given in [Cauquoin et al., 2019]

Following your comment, we added a sentence to specify the reason for too enriched isotopic values in LMDZ: "*As shown in Cauquoin et al. (2019), the representation of the advection scheme in the model can impact the isotopic composition, with more enriched values when a more diffusive advection scheme is applied*".

- l 458: with fractionation during continental recycling: no! even without fractionation during continental recycling, the continental effect is observed, as shown by all isotope-enabled GCMs. It is due to the fact that over land, the enrichment of the low-level vapor by evaporation is weaker than over the ocean. Over land, not all the precipitation goes back to the atmosphere, so heavy isotopes are preferentially lost by runoff (e.g. [Pierrehumbert, 1999] for a simple model of this effect). The fractionation during bare soil evaporation only very slightly enhanced the continental effect [Haese et al., 2013, Risi et al., 2016]. For the observed continental effect, cite [Rozanski et al., 1993].

We modified the text of the Section 3.2.3 following the suggestion made.

- l 470: Even if... fractionation: remove, since the fractionation during bare soil evaporation is not responsible for the continental effect.

We removed this sentence in the main manuscript.

- Why documenting the continental effect in the tropics? It is largest at mid and high latitudes. In the tropics, it is weak [Salati et al., 1979, Worden et al., 2021] or even reversed (more enriched over land) [Levin et al., 2009], due to strong evapo-transpiration. I would have expected the same plot for mid and high latitudes.

Following comment from the Reviewer 1 in the initial review, we looked at the continental effect in the tropics. We however agree that this continental effect is observed and better seen at high latitudes. Based on the same methodology than presented in the first response to the reviewers, we present here a new figure for the continental effect for regions between 40-70°N. We chose to focus on the northern hemisphere since most of the land area are located there, and the continental effect is the most visible. We compared the GNIP data with iLOVECLIM, LMDZ4 and ECHAM5-wiso, with the number of stations and points in the model summarized in the Table 1 below. Note that very few stations are available in the observations for the ocean. We then calculated series of mean values corresponding to the continents (Europe, Asia and North America) and to the oceans (Atlantic, Pacific, Arctic), and added the results to the existing Figure 11 in the revised manuscript (Figure 3 below).

	40-70°N	
	Continent	Ocean
GNIP	107	4
iLOVECLIM	278	174
LMDZ4	766	357
ECHAM5-wiso	7853	4178

Table 1: Number of GNIP stations and points in the different models that cover land surfaces and oceans between 40-70°N.

The contrast in isotopic value between land and ocean observed in the data and models, with more depleted values over land is due to the fact that over land, the enrichment of the low-level vapor by evaporation is weaker than over the ocean. This is well observed between 40-70°N in the observations with a median value of -89.8 ‰ for the continents and -51 ‰ for the oceans (Figure 3e). This continental effect is also observed in iLOVECLIM, LMDZ4 and ECHAM5-wiso with respective values of -52 ‰, -99.8 ‰ and -109.8 ‰ for the continents and -31.3 ‰, -43.2 ‰ and -59.5 ‰ for the oceans (Figure 3f, g, h). The amplitude of the continental effect for these mid to high latitudes is less pronounced in iLOVECLIM than in the observations (-20.7 ‰ vs -38.9 ‰), as observed for the tropics. In comparison, LMDZ4 and ECHAM5-wiso models have higher continental effect than observations (-56.6 ‰ and – 50.3 ‰ vs -38.9 ‰).

The continental effect is also less pronounced at low latitudes than at mid-high latitudes. This comparison has been added to the revised manuscript.

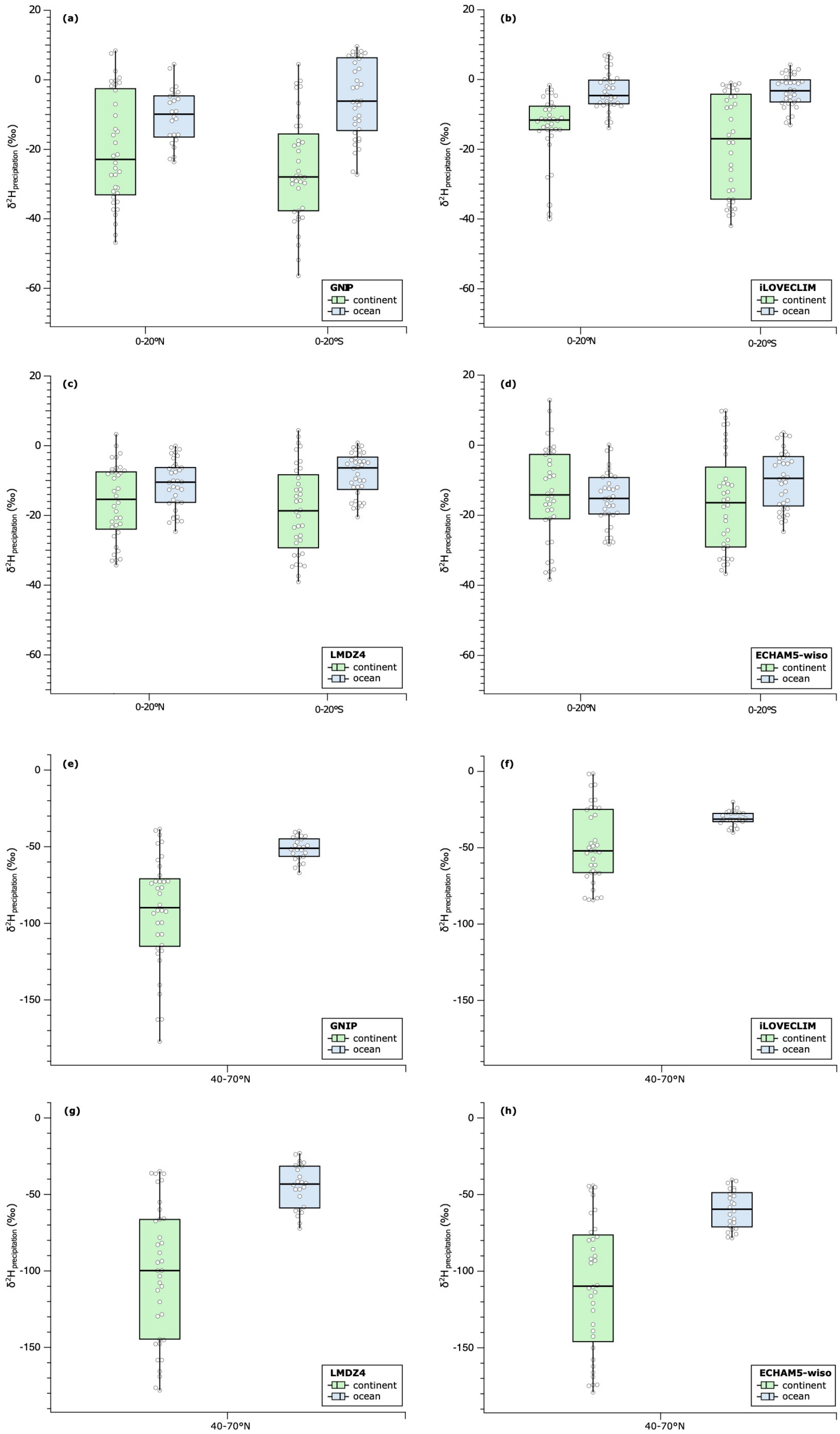

Figure 3: Box plots of the $\delta^2H_{precipitation}$ over the continents (in green) and oceans (in blue). The panels (a) to (d) present values between 0-20°N and 0-20°S for (a) GNIP data, (b) iLOVECLIM, (c) LMDZ4 and (d) ECHAM5-wiso. The panels (e) to (g) present values between 40-70°N for (e) GNIP data, (f) iLOVECLIM, (g) LMDZ4 and (h) ECHAM5-wiso. The horizontal line in the box plots corresponds to the median value.

- l 478: more complex: is it really more complex than in LMDZ? Any reference to justify this assertion? The main difference between LMDZ and ECHAM seems to be the horizontal resolution, not the complexity of its parameterization.

We agree that the sentence was not clear. We changed it to: "*Among all three models and surprisingly, ECHAM5-wiso which least reproduces this continental effect, despite having a better horizontal resolution*".

- l 515: I don't understand the logic: why the absence of sea ice ... would lead to fractionation during sea ice formation?

The sentence was ill-formulated. In this iLOVECLIM simulation there is no sea ice and we observe high isotopic values in the polar ocean. However, if sea ice was taken into account, a fractionation would happen during sea ice formation, leading to depletion of the liquid water isotopic composition as explained in Werner et al. (2016). We modified the sentence in the revised manuscript.

- l 518: more depleted d-excess -> lower d-excess

Done.

- l 521: as well highly depleted -> very low

Done.

- General rule: the vapor is depleted, but the $\delta^{18}O$, the d-excess or the ^{17}O-excess are low.

We replaced the word depleted by low, and enriched by high in the revised manuscript.

- l 544: observation -> observations or dataset

Done.

- l 560: than d-excess -> as the d-excess maximum

Done.

- l 573: with the conservation of -> with a reasonable simulation of

Done.

- l 579: that has a too important -> with an excessive

Done.

- l 580: isotopes-enabled -> isotope-enabled

Done.

- l 581: a good accordance of -> with good agreement

Done.

References

Dansgaard, W.: Stable isotopes in precipitation. Tellus, 16(4) :436–468, https://doi.org/10.3402/tellusa.v16i4.8993, 1964.

Dee, S., Noone, D., Buenning, N., Emile-Geay, J., and Zhou, Y.: SPEEDY-IER: A fast atmospheric GCM with water isotope physics, J. Geophys. Res.-Atmos., 120, 73–91, doi:10.1002/2014JD022194, 2015.

Moore, M., Kuang, Z., and Blossey, P. N.: A moisture budget perspective of the amount effect, Geophys. Res. Lett., 41, 1329– 1335, https://doi.org/10.1002/2013GL058302, 2014.

Risi, C., Muller, C., and Blossey, P.: Rain evaporation, snow melt, and entrainment at the heart of water vapor isotopic variations in the tropical troposphere, according to large-eddy simulations and a two-column model, J. Adv. Model. Earth Sy., 13, 1–28, https://doi.org/10.1029/2020MS002381, 2021.

Roche, D., Paillard, D., Ganopolski, A., and Hoffmann, G.: Oceanic oxygen-18 at the present day and LGM: equilibrium simulations with a coupled climate model of intermediate complexity, Earth Planet. Sc. Lett., 218, 317–330, doi:10.1016/S0012- 821x(03)00700-3, 2004.

Roche, D. M.: $\delta^{18}O$ water isotope in the iLOVECLIM model (version 1.0) – Part 1: Implementation and verification, Geosci. Model Dev., 6, 1481–1491, doi:10.5194/gmd-6-1481-2013, 2013.

Werner, M., Haese, B., Xu, X., Zhang, X., Butzin, M., and Lohmann, G.: Glacial–interglacial changes in $H_2{}^{18}O$, HDO and deuterium excess – results from the fully coupled ECHAM5/MPI-OM Earth system model, Geosci. Model Dev., 9, 647–670, https://doi.org/10.5194/gmd-9-647-2016, 2016.

Response to Reviewer 2

We thank Reviewer 2 for the comments and suggestions that helped to improve the new manuscript. We addressed all the comments of the referee (responses in blue), including the addition of the new $\delta^{17}O$ and ^{17}O-excess data for a better model-data comparison, and changed the manuscript accordingly.

- Lines 13-14: "… and calculate the associated secondary markers d-excess and 17O-excess. So far, the latter was modeled only by the atmospheric model LMDZ4. Results of…"

We changed the sentence accordingly.

- Lines 136-137: It is not that true anymore. It is rather easy to run a snapshot simulation with an isotope-enable coupled model. However, you can say that the true challenge for coupled GCMs is the computing cost of transient simulations like the last deglaciation or the Holocene. Please change the sentence accordingly.

You are correct, thank you for highlighting it. The text now reads: "*General Circulation Models (GCMs) have first been used to simulate separately water isotopes in the atmospheric and oceanic components but are now capable of running snapshot coupled simulations with the water isotopes-enabled. Running transient coupled simulations like the last deglaciation or the Holocene remains however still challenging due to high computing cost of these GCMs*".

- Section 2.4: A very recent paper presenting a dataset of d17O has been released one month ago by Terzer-Wassmuth et al. from IAEA. I think the authors should include this dataset in their analyses. I know this requires extra work in terms of figure plotting and statistical analysis, and I'm sorry about that. But I think it's worth including this data set (or worth trying at least).

Thank you for pointing us this new dataset. It is indeed very interesting to add them to the manuscript. We added these new data in the revised manuscript, respectively to the Figures 1 (^{17}O-excess spatial distribution), 3 (Taylor diagram for ^{17}O-excess), 6 (^{17}O-excess model-data comparison), A1 and A2 (δ^{17}O spatial distribution and model-data comparison).

We revised the text in Section 3.1.3 considering the new data and said, as already highlighted in the previous manuscript, that we have a too high dispersion of the ^{17}O-excess data in iLOVECLIM. Indeed, higher values than observations are modelled from mid to low latitudes and lower values than observations at high latitudes of the northern hemisphere. The Figure 6 in the manuscript has been changed including an additional zonal comparison between iLOVECLIM, LMDZ4 and the observation (see Figure 1 below). Interestingly, the opposite pattern in the models compared to observations suggests that the physical processes at play are not fully understood and require further investigation.

We added the following sentence in the Appendix A for the spatial distribution of the δ^{17}O: "*Similarly, the values over land are lower than over the ocean. In comparison to the available data (including new data from Terzer-Wassmuth et al. 2023), iLOVECLIM calculates higher values of several permil in central Europe and Canada, and lower values in Africa. Agreements are observed between the model and the data in East Asia, western Europe and North America*".

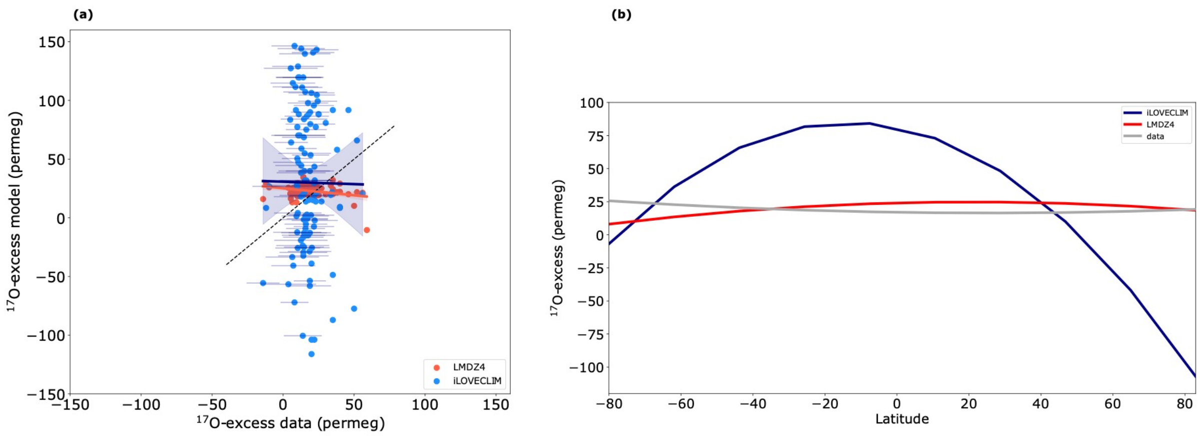

Figure 1: (a) Relationship between the iLOVECLIM modelled isotopic value and ^{17}O-excess measurements, without values in Antarctica. LMDZ4 model results are also presented. The regression curves between model and data are presented in dark blue for iLOVECLIM and red for LMDZ4 with the confidence bands. The 1:1 line are shown with the black dashed lines. The errors bars associated with the data are shown at 1σ. (b) Zonal ^{17}O-excess comparison. The model results (in color) are compared to observations (in grey). The different lines are polynomial regression curves for the model results that co-locate with the observations.

Finally, we modified the model-data $\delta^{17}O$ comparison text in the Appendix A to reflect the new data that have been added to the Appendix Figure.

- Line 324: "Despite these biases, iLOVECLIM reproduces the global trend of depleted…"

Done.

- Line 479-480: Replace the sentence by something simpler like "More generally, iLOVECLIM models too high d-excess values from mid- to low-latitudes (Figure 2b)"

Done.

- Lines 484-486: "In Figure 2b, we excluded these outlier values for a more suitable model intercomparison. Zonal mean d-excess values from mid- to high-latitudes modeled by LMDZ4, GISS, and CAM are too high compared to the observations, whereas values from ECHAM5-wiso are systematically too depleted."

We changed the sentence following your comment.

- Line 492: "(0.34 to 0.52), but with a higher SD compared to…"

Done.

- Line 529: Figure 4

Done.

- Line 552: Figure 5

Done.

- Line 650: Figure 6

Done.

- Line 658-659: data are now available from Terzer-Wassmuth et al. and in the WISER portal.

We added these new ^{17}O-excess data to the figure for comparison with iLOVECLIM. We however only present data for Ankara and Reykjavik stations since the data are not available for Pretoria and Belem. Due to the limited number of measurements, the monthly mean ^{17}O-excess is presented only for 2 calendar years within the period 2015-2018. Similarly to what has been done in the original manuscript, we normalized the values because the seasonal evolution of precipitation and isotopic ratio in the model is not expected to perfectly reflect the measurements. The model-data agreement is not perfect, especially for Ankara, but the model is able to reproduce the seasonal variations as observed in the data for Reykjavik. The seasonal evolution of ^{17}O-excess at Ankara and Reykjavik (Figure 2) has been added to the figure in the revised manuscript in Section 3.1.4.

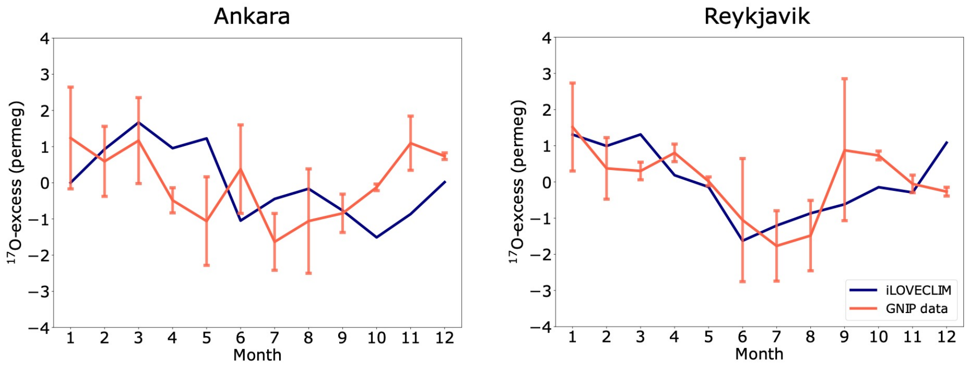

Figure 2: Monthly evolution of the ^{17}O-excess at Ankara and Reykjavik. The red line is the GNIP data measured at the station and the blue line is the iLOVECLIM model result at the corresponding location. The data and model results have been normalized. The error bars for the data are also shown at 2σ.

- Lines 660-662: Why don't you take all the years of the simulation (100 years, as stated at lines 265-267) instead of of just 10 years?

We agree that starting from the equilibrium simulation, the additional 100 years for monthly outputs are as well at equilibrium. There is indeed not that much differences between monthly results for the whole simulation in comparison to the last 10 years as seen in Figure 3 for the precipitation and $\delta^2H_{precipitation}$ at the same stations than presented in the manuscript.

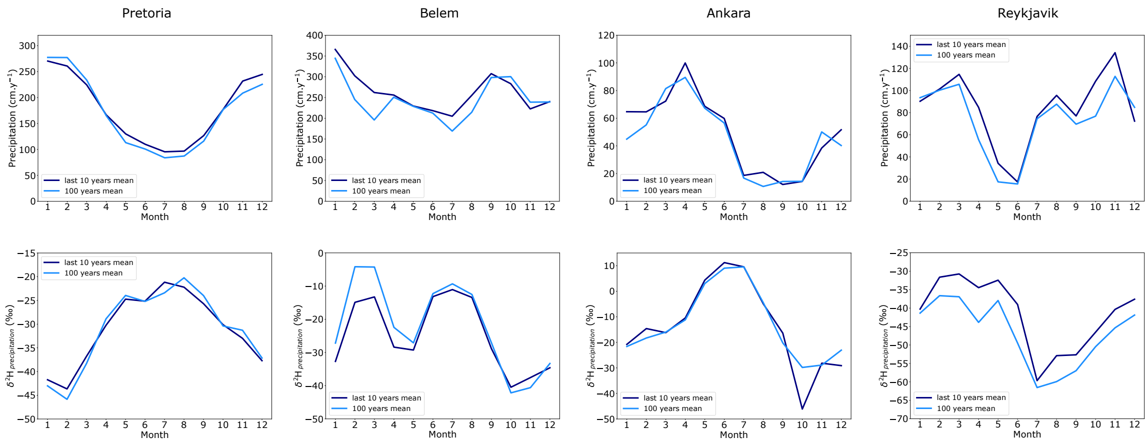

Figure 3: Monthly evolution of precipitation (top) and $\delta^2H_{precipitation}$ (bottom) at several stations (different columns for Pretoria, Belem, Ankara and Reykjavik). The two curves correspond to the mean over the last 10 years of the simulation (dark blue) and to the mean values over the entire simulation (light blue).

Based on that observation, we kept the last 10 years of the simulations for the monthly results.

- Line 694: Figure 7

Done.

- Line 728: Figure 8

Done.

- Line 732: "examining" instead of "looking at"?

The change has been done.

- Lines 736-737: Therefore, we selected for each GNIP station the pixel that was in better agreement with the precipitation and isotopic composition seasonal cycle data.

We corrected the sentence.

- Lines 741-742: This amount effect is -0.085‰/cm.y-1 in iLOVECLIM, weaker than the observed one in GNIP data (-0.139‰/cm.y-1).

We modified the sentence in the revised manuscript.

- Line 744: as already noted by Risi et al. (2010).

Done.

- Line 746: Figure 9

Done.

- Line 750: Temperature plays an important role on the hydrogen…

This has been changed in the text.

- Lines 759-760: Differences in modeled d2Hprecipitation between iLOVECLIM and LMDZ4 are enhanced for the lower values, and model-data agreement is deteriorated.

We changed the sentence accordingly.

- Line 760: What do you mean by "the difference in simulating the isotopic composition at low temperature"? Could you be more specific?

From the Figure 10 we can see that the modelled $\delta^2H_{precipitation}$ in iLOVECLIM presents higher values than LMDZ4 below a temperature of -20°C. But according to your last comment, we changed the sentence in the revised manuscript.

- Line 762: Figure 10

Done.

- Continental effect: Why do you focus only on tropics (0-20°N and 0-20°S)?

That is a good point. Please see the response of the same comment from Reviewer 1 above.

- Line 797: Figure 11

Done.

- Line 804: too enriched compared to observations at high latitudes.

Done.