Authors' response to Reviewer 1 [hess-2024-4169-RC1]

We thank the reviewer for his evaluation of our manuscript and his many helpful comments (hess-2024-4169). Below we address the reviewer's comments (full text) indented by arrows and coloured in blue. We appreciate the efforts by the reviewer, which will help to improve our manuscript.

General comments

More explanation is needed on how atmospheric climate patterns relate to physical effects. Specifically, I do no understand how you view physical effects acting differently during different kinds of atmospheric circulation patterns. I imagine that you are not arguing that, for example, the isotope fractionation occurring during condensation at a particular temperature changes with different circulation patterns (by definition it cannot). But then, I am not clear on what it means for an atmospheric circulation pattern to affect a local physical effect. Can you please clarify how you envision the relationship between synoptic weather patterns and the actual physical processes causing isotope fractionation? Perhaps I am misunderstanding your meaning due to using terminology differently, but if I have this question, others will too.

→ Thank you for bringing this point up. Indeed, we did not try to argue that isotope fractionation during condensation changes with different circulation patterns. Instead, we meant that atmospheric circulation changes the moisture origin of incoming air masses and their trajectories, hence the rainout history of those air masses. Since we know that the moisture origin and rainout effects affect isotopic signatures in precipitation, we argue that they might also affect apparent (or empiric) relations between local meteorologic variables and isotopic signatures. Below is the section in the corrected manuscript where we address this.

"In this study, we conjecture that the trajectory of the incoming airmasses affect $\delta^{18}O$ and $\delta^{2}H$ values in precipitation, and thus potentially change apparent relations with meteorological variables, e.g., the temperature effect. More specifically, we hypothesize that contrasted moisture origins over Western Europe (Atlantic, Mediterranean or continental) and rainout strengths with different air mass trajectories affect sub-daily $\delta^{18}O$ and d-excess signals in precipitation and the relation with meteorological variables at local scale in Luxembourg."

Why is it valid to extrapolate your calibration at LIST to a continental scale? It looks to me like your modeling approach basically does best at sites near the LIST field site, which aligns well with prior research showing that water isotope values exhibit spatial coherence (e.g., Bowen and Revenaugh, 2003). So wouldn't you expect this pattern to emerge?

In addition, more explanation is needed as to why your model, which I understand to ultimately be based on monthly aggregate data, eventually including monthly GNIP data, can be used to infer changes on weekly timescales. Doesn't the data show that the modeling does best on seasonal timescales, often under/overestimating the magnitude of variability on shorter timescales?

→ These are valid questions and the extrapolation of our model calibrated on LIST data on the European scale was one of the major flaws of our approach. Based on your comments and comments received from other reviewers, we decided to remove the generalized model from the manuscript. Instead, we will focus on the effects of atmospheric variability on relations between sub-daily precipitation isotope signatures and meteorologic variables. We will also analyse how the fact of considering atmospheric trajectories affects a simple modelling approach based on multiple linear

regressions for reconstructions of precipitation $\delta^{18}O$ chronologies in Luxembourg but not venture beyond that point. Below the corrected form:

"We then analyse how the fact of considering atmospheric trajectories affects a simple modelling approach based on multiple linear regressions (of increasing complexity) and assess potential implications for reconstructions of long chronologies of $\delta^{18}O$ in precipitation."

As the authors note, there are prior isoscapes for this region of the world, including approaches that make use of variables other than (or in addition to) temperature – such as precipitation amount. It would be useful to give broader reference to this prior work as well as to incorporate/argue against these prior approaches. Some thoughts:

Why are other environmental variables besides local temperature not used in the modeling? What happens to model performance if they are included?

→ Thank you for the suggestions, we will also consider the precipitation amount, the relative humidity and the surface pressure as additional meteorologic variables. We will gradually include them in the multiple linear regression models to assess their performance under increasing complexity. In the manuscript:

"To test if including air mass trajectories in our modelling approach improves results for precipitation $\delta^{18}O$ predictions, we rely on multiple linear regression models (MLRMs) fed with meteorologic variables at event scale. We compare models sub-setting the $\delta^{18}O$ data for each trajectory in one scenario (hereafter referred to as "separated" model) and keeping the data together in the other (hereafter referred to as "traditional" model). The results indicated for the separated model are the weighted mean of all five trajectory-specific models, considering the number of observations in each group with the weighting. More variables are gradually fed to the model augmenting the degrees of freedom to also test under which conditions the models perform better. Hence, four MLRMs will be tested under two scenarios, one regular and the other separated according to the air mass trajectory types."

For the isoscape creation, how different are the results from using the calibration at LIST from using a continental-scale calibration? As in, instead of trying to develop a d18O-T curve for LIST and then applying that curve to the continent through time, what if you made a d18O-T curve for the continent and then applied it through time (essentially, this approach would be to update previously published d18O(weather parameters, lat, lon, elev) functions and apply them through time). Are the results materially different? Why would we prefer one approach over the other? Or would it be better to use LIST and GNIP data together to create d18O functions by aggregate month, region, etc?

→ That is certainly an interesting way to do it. We did try to calculate seasonal $\delta^{18}O$-T for the continent and obtained interesting results, but we were not sure how to include them in the manuscript. The influence of atmospheric circulation patterns on our sub-daily isotope dataset in precipitation is the core of our manuscript and we prefer to focus solely on that. But this is definitely a lead for follow-up studies.

How important is it to include all of the Climate Pattern information for the modeling? If including the CP information reduces RSME by 0.2 (2.8 to 2.6) and increases r2 by 0.07 (0.37 to 0.44), is this truly meaningful? Specifically, what investigations does this approach allow that were previously untenable without the CP information.

→ It is true that this improvement is marginal. We revised the conclusions we drew from it, below the new version.

"We found that the integration of atmospheric circulation trajectories in the multiple linear regression models only leads to negligible improvement in performance (RMSE reduction of 0.1 ‰) – likely due to meteorologic variables already containing information on atmospheric variability and thus different air masses reaching the specific site. This suggests that even if atmospheric inferences with isotopic signatures in precipitation are observed, including the air mass trajectories as an input for $\delta^{18}O$ predictions might not be a decisive advantage. One reason could be that our time series of sub-daily $\delta^{18}O$ may be too short – thereby over-representative or under-representative of certain trajectories and meteorological conditions."

Finally, what are the uses for time-transgressive isoscapes beyond the "climate normal" versions that already exist? Accounting for the errors in developing isoscapes in each way, how different would the estimates be? What are the benefits of being able to work with estimates of precipitation isotope values from a particular year, rather than an average of many years?

→ It all depends on the timescales that one is working with. It is agreeable that when errors are considered, isoscapes are valid tools for present-day approaches working with isotope data from observation datasets. But in pre-instrumental times, working with reconstructed isotope data form, e.g., tree rings, sediments or ice-cores, it is important to assess long-term effects of changing atmospheric circulation patterns, which have been reported to affect isotopic compositions in precipitation. In the revised introduction:

"Still, a simple empiric approach also requires caution, as Sturm et al. (2010) point out non-stationarities in the relation between $\delta^{18}O$ and meteorologic variables, inherent to changing atmospheric circulation patterns (Noone and Simmonds, 2002; Lee et al., 2008). The temporal $\delta^{18}O$ -T gradient may have been substantially lower for the LGM – Pre-Industrial (LGM-PI) era than under the present climate for most mid to high-latitude regions (Werner et al., 2016), and changing $\delta^{18}O$ and temperature relations have existed in past climates (Jouzel, 1999; Buizert et al., 2014). Colder climates (e.g., Last Glacial Maximum, LGM) are typically associated with lower $\delta^{18}O$ values in precipitation (Lee et al., 2008; Risi et al., 2010; Werner et al., 2016)."

Specific comments

10

Qualify this statement a little further. Worldwide predictive maps of d2H and d18O exist and daily, monthly, and yearly data sets exist in many locations.

→ That is correct, but in the context of climate change, we know little on how these maps will evolve.

11

Clarify what you mean by "long term"

→ Again, in the context of climate change, these are multi-decadal records spanning over 50 years or more.

12-15

Clarify this further. Is this not how the community as a whole envisions what drives O and H isotope signatures? If not, what are the alternatives?

→ It is commonly perceived that atmospheric circulation affects isotope signatures, but it is not clear how it affects apparent relations with meteorological variables at local scale. We will specify that.

14

Suggest not using the abbreviation "CP" as it is not common

→ Thank you, we will change that.

29

Define the sense in which stable isotopes of oxygen and hydrogen are "near-conservative"

→ In the chemical sense as they are part of the water molecule, we will clarify that.

77

I do not quite understand the linkage being made here between atmospheric circulation patterns/climate patterns and physical effects on isotope values in precipitation. From what is written, I understand that you hypothesize that atmospheric circulation affects how the temperature of condensation is expressed in the isotope composition of precipitation. But how exactly does this work? By definition, if you are considering a strict temperature effect (i.e., how the temperature of condensation induces isotope fractionation between vapor and liquid water), the magnitude of the "temperature effect" must be driven by the starting temperature and overall temperature variability of the air mass induced by atmospheric circulation patterns. But the effect of temperature on isotope fractionation exists regardless of where an air mass originates, right? It is simply that different atmospheric circulation patterns may be associated with more/less temperature variability, which may mean that processes other than temperature will be responsible for the variability observed in precipitation isotope values.

→ This relates to the first general comment you made, please refer to our previous answer.

100

Were internal standards used to normalize the data? What were their values?

→ Yes, we will indicate the values.

104

Why was a sin wave chosen? Were other fits considered?

→ Sine wave fits generally work well on signals that are clearly seasonal, which was the case with our data. But reading the reviewer's comments, we realized that it added unnecessary complexity and led

to reader confusion. We decided to remove the sine wave fits from the manuscript and instead work with more common metrics such as mean, median, interquartile range, etc.

106

Check equation and units. Verify is consistent with results presented in tables

Kirchner (2016) is not listed in the references

→ *This section will be removed from the manuscript.*

110

This section would benefit from a figure with a panel showing each of the climate patterns

→ *We will add maps showing the trajectories of incoming air masses for precipitation events.*

120-122

Does use of alternative classification schemes affect your results and interpretation?

→ *Yes, we have simulated the trajectories using the backward trajectory HYSPLIT model and found that the trajectories do not always correspond to the broader synoptic atmospheric circulation types. This has led us to prefer the HYSPLIT simulations for the characterization of the precipitation events than the Hess Brezowsky (HB) catalogue. We will thus replace HB by categories derived from HYSPLIT simulations.*

130

Why was temperature chosen as the sole input variable? Were other variables considered? What were their relationships with d18O?

→ *You are right to ask, we did consider other variables that were not included. We will add the precipitation amount, the relative humidity and the surface pressure as additional meteorologic variables.*

135

Were the linear regressions modeled using event-scale data or using monthly data? Line 132 says monthly, but line 136 says event-scale

→ *Thank you for bringing up this inconsistency. We will ensure that it is clear what data was used, also by indicating the number of samples when we show results.*

140

If ERA5 interannual average monthly T data is going to be used in the ultimate evaluation, should it not also be used in the initial assessment at LIST for determining coefficients of Eqn. 2?

→ *This section will be removed from the manuscript.*

141

Suggest: “To include as many records as possible, the geospatial model…”

To emphasize that the included records do have a full year, but are still shorter than the LIST record

→ *This section will be removed from the manuscript.*

145

How was the “climate pattern” determined for each GNIP station? Is it fair to use the Hess and Brezowsky (1952) categorization for sites outside of Germany, where it was originally designed for?

The constants in Eqn. 2 are specifically for the LIST site. Why are these applicable to other sites 100s km away? Would they not have their own set of constants? In general, it would be useful to have more explanation about why you expect a calibration developed at one particular site to be broadly transferrable across continental scales.

→ *You are right to question the validity of the applicability and reflecting on your comment has led us to decide to remove this section of the manuscript.*

154

Where is the DEM grid from?

→ *This section will be removed from the manuscript.*

173

Citation needed for GMWL definition

→ *Craig (1964) will be added.*

189-190

Define “considerable”

→ *We will do that.*

224

With such a considerable difference between HCE and the other climate patterns, is fitting a curve to HCE useful? What is the physical meaning of phi = -98 months?

I am not clear on how the sinusoids fitted to the data fit into the larger scope of the work. Please expand on how these were used to investigate the relationships between large-scale atmospheric circulation patterns and local-scale meteorological variables

→ Thank you for the pertinent remark, we agree that curve fittings were not the most suitable option. We will change that.

234-235

Is this gross match between the amplitudes of d-excess and d18O meaningful?

→ Not really, we will consider removing this.

263

What is the correlation on a seasonal scale?

→ That would indeed be an added value, we will add it.

265-267

So how different are the two approaches? Is one inherently more useful than the other?

→ We will add a table to compare the two approaches directly.

If the input data for Table 3 was at a monthly scale, why is it fair to use the model at a weekly scale?

→ This section will be removed from the manuscript.

294

I could argue that Figure 7 mostly shows that your model does best nearest to the LIST site because that is a foundation of the model. Do you agree? Why?

I might further argue that your model could naturally be expected to perform more poorly closer to the coast where the temperature effect cannot emerge as important because there has not been substantial rainout yet. Other processes would be expected to be important closer to the coast. Do you agree? Why?

→ We fully agree, the temperature effect is known to be weaker in coastal regions. However, note that this section will be removed from the manuscript as well.

327-328

How significant is the slope of 7.54 in terms of identifying re-evaporation?

→ We will develop this a bit further.

342-344

The act of evaporation undoubtedly induces isotope fractionation and helps set “initial” isotope values of vapor. However, it is simplistic to view d-excess as a static value – consider, for example, the modeling exercises of Xia and Winnick (2021) and Xia (2023). To what degree can your data set break apart the oceanic vs. continental influences controlling d-excess values?

→ Thank you, this was indeed a bit simplistic. We will move this part to the introduction, because it serves more to introduce the concept of the d-excess. We do observe a clear distinction between d-excess values form continental and oceanic sources, so we do think that our data can break those apart.

362-363

This is confusing. So is the T-d18O relationship you infer dominantly coming from condensation reactions or is it incorporating a broader swath of processes with variable influence from temperature?

→ This circulation type does not exist with our new classification scheme based on the HYSPLIT model, this sentence will be removed.

364-365

I find this pretty challenging. On the one hand, you write here that the d18O-T relationships are dependent on CPs, but in the next sections (paragraphs starting lines 400 and 415), you note that the d18O-T relationships do not appear to be strongly dependent on CPs and in fact can be challenging to usefully apply outside of the region surrounding LIST. How should we reconcile these aspects of the data?

→ Even if atmospheric inferences with isotopic signatures in precipitation are observed, including the air mass trajectories as an input for $\delta^{18}O$ predictions might not be a decisive advantage. We think that a statement in this direction could reconcile our findings.

375

Citations are needed here. Where has this assumption been made recently?

→ Thank you, we will consider adding the citations or rephrasing this sentence, as it is not crucial for the discussion.

380-381

So if changes in air temperature cannot be assumed to accurately predict d18O as the d18O-T relationship changes through time, what implications does that have for your reconstructed isoscapes? Or do you just see this as a challenge under substantially different planetary boundary conditions? Explain further

→ The second statement corresponds more to our initial thought, it is an additional challenge under, e.g., different climates. We will develop this more.

385

How well, what did others find?

→ This section will be removed from the manuscript.

391

Again, if the initial data focus was on monthly inputs, how does this translate into discerning sub-monthly changes in the past? Especially as you note that the model has trouble accurately capturing better than seasonal-scale variability (lines 290-305; Fig. 8)

→ *This section will be removed from the manuscript.*

404-405

So could this instead be taken to mean that climate patterns do not have a strong control on the physical processes underpinning isotope fractionation – that they occur independent of wherever the vapor is coming from?

→ *The results do suggest controls on the relation between isotopic signals and meteorologic variables, we argue that they would have been even more clear if we had had more distributed data.*

Perhaps because the CPs tend have uneven seasonal distribution, by breaking apart different d18O-T relationships by CP you are essentially breaking out d18O-T relationships for different seasons, which is what leads to a slightly better performance.

→ *This is a suspicion that we also had. We will show the seasonal* δ^{18}*O-T to check whether this is true.*

410

If convection strength is an important parameter, why is it not included here? The ERA5 data contain this type of information

→ *We did not have observation data for convection strength in Luxembourg. We could have added it for the generalized model that is true.*

419-420

This seems like a significant challenge to the use of the model presented here

→ *This section will be removed from the manuscript.*

424-425

These sentences appear to directly contradict each other

→ *This section will be removed from the manuscript.*

FIGURES

Figure 1

Identify the black lines in panels (a) and (b)

→ *We will do that.*

In all figures with maps, clearly identify the LIST site

→ *We will do that.*

TABLES

Table 4

Are these coefficients and constant the same for all CPs? I thought that, as in Eqn. 2, the coefficients and constants for Eqn. 3 implied they would also be different for different CPs

→ *Yes, they were constant for all circulation patterns. Geo-spatial parameters were constant in the model, only the temporal variables had parameters that varied with the circulation patterns. But again, the model will be removed from the manuscript.*