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

*We thank the reviewer for his/her evaluation of our manuscript and his/her 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 the physical effects change with different circulation patterns. Instead, we meant that atmospheric circulation changes the apparent (or empiric) relations between local meteorologic variables and isotopic signatures. The isotope fractionation occurring during condensation will remain the same at a particular temperature change, but the relation will be altered because the incoming isotope signals of the air masses are different. These incoming isotope signals depend on the moisture origins of incoming air masses and their trajectories, hence the rainout history of those air masses. We will make sure to address this accordingly to not create any confusion.*

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?

*→ The extrapolation of our model calibrated on LIST data on the European scale was one of the major flaws of our approach, we recognize this. 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 moisture origins affect the residuals a simple modelling approach based on multiple linear regressions for reconstructions of precipitation $\delta^{18}O$ to disentangle local (meteorological) from remote (synoptic) effects. We will not venture into modelling long precipitation $\delta^{18}O$ chronicles as we realized our approach was not suitable for our dataset.*

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?

*→ Initially, we wanted to use only variables for the precipitation $\delta^{18}O$ model that are generally available over large time intervals in most regions. Given how important the temperature effect is on precipitation isotope signals and how widely available temperature records are, we decided to focus on the temperature only. You are right to mention the precipitation amount – it is particularly important in some regions (e.g., in subtropical climates). Although continuous precipitation records are slightly harder to obtain. As our new approach is no longer about long reconstructions of precipitation $\delta^{18}O$ chronicles, we can also consider the precipitation amount, the relative humidity and the surface pressure as additional meteorologic variables. We will include them in the multiple linear regression model to calculate the precipitation $\delta^{18}O$ residuals.*

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?

*→ We did try to calculate seasonal $\delta^{18}O$-T for the continent and obtained interesting results, but they did not fit the scope of the manuscript. The sub-daily isotope dataset in precipitation is the core of our manuscript, and we prefer to focus on that. But this is certainly 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. Although this section is no longer part of the latest version, in the new manuscript we acknowledge that climate pattern information (moisture origins) might contain redundant information already contained in local meteorological variables.*

*"The $\delta^{18}O$ residuals from the multiple linear regression model, used to exclude local meteorologic effects, did not yield significant differences in moisture origin-specific isotope signatures. This was probably due to local meteorological variables already containing inherent information on remote conditions during moisture formation processes."*

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 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, one should not compare present-day observations with samples that are significantly anterior to these observations. It is important to account for 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 HYSPLIT model in backward mode 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 $\delta^{18}$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.*

*Authors' response to Reviewer 2*
*[hess-2024-4169-RC2]*

*We thank the reviewer for his/her evaluation of our manuscript and his/her 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*

One of the major issues is that the authors switch a lot between different timescales, which is confusing for the reader. For example, most of the introduction deals with variations on monthly timescale, but then the record is on sub-daily timescale. Further, Section 3.1 presents the precipitation and meteorological data on monthly scale, but then, in Section 3.2, an event-based model is applied. The authors should restructure the manuscript, including presentation of isotope and model results on different timescales to better guide the reader. They may also decide to stick to only one timescale, if only this is relevant regarding the objective of the manuscript.

*→ Thank you for bringing this to our attention. One of the strengths of the manuscript is the sub-daily isotope dataset, so the most logical decision would be stick with the sub-daily resolution. However, when analysing the influence of the atmospheric circulation types, it is sometimes advantageous to aggregate the data to see the general effect on isotopic signatures. Hence, it might be difficult to restrain ourselves to one timescale only, but we will make an effort to clearly state what timescale we are working with when displaying results, e.g., by including the number of samples that went into calculating statistics. Also, we have decided to not include the generalized model for Europe in the new manuscript, which should alleviate the problem of switching between timescales. The extrapolation of our model calibrated on LIST data on the European scale was one of the major flaws of our approach (see the responses to general comments of reviewer 1). 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 sub-daily precipitation isotope signatures (and meteorologic variables).*

The authors focus on the relationship between temperature and the isotope composition of precipitation, which is definitely an important factor during precipitation and Rayleigh rainout. However, other processes affecting the atmospheric water vapor from which the precipitation is formed, such as changes in climate conditions in the moisture source regions and contribution of continental evapotranspiration are only shortly mentioned, while their effect on the isotope composition of precipitation remains undescribed. It is not until the discussion section that the reader learns about post-precipitation formation processes, such as rainfall re-evaporation, that can modify the isotope composition of precipitation. I suggest restructuring the introduction section, providing an overview of the processes that can affect the isotope composition of precipitation before, during and after precipitation formation and on which timescale they are relevant.

*→ We have fully revisited the structure of the manuscript and added two paragraphs in the introduction discussing the processes affecting the atmospheric water vapor from which the precipitation is formed. Some sentences were also taken from the discussion and brought to the introduction, as you mentioned these aspects should be mentioned earlier.*

The authors observe variations in the isotope composition of precipitation with atmospheric circulation patterns. However, the processes behind these isotope variations as well as their relevance on different timescales are not discussed. There is a need for a climate characterization of the different CPs. Also, the authors focus mainly on temperature, but other factors such as rainfall amount and RH during the precipitation event may provide information on local processes such as rain re-evaporation. Also, changes in moisture sources should be discussed in relation to CPs. There is a debate on isotope differences between precipitation derived from Atlantic and Mediterranean air masses, the latter being characterize by a higher $\delta^{18}$O and d-excess values. Do the authors observe similar isotope differences between air mass sources? Which sources can be attributed to different CPs?

*→ We will add the precipitation amount, the relative humidity and the surface pressure as additional meteorologic variables in the multiple linear regression model to calculate precipitation $\delta^{18}$O residuals (see the responses to the general comments of reviewer 1).*
*Regarding atmospheric circulation patterns and moisture origins, we have simulated the air mass trajectories using the HYSPLIT model in backward mode and found that the incoming trajectories in Luxembourg deviate from the broader synoptic atmospheric circulation types (Hess Brezowsky catalogue). Synoptic atmospheric classifications might thus not be adapted for applications at a local scale. We will thus new categories derived from HYSPLIT simulations, where we also determine the moisture origins by tracking moisture uptake locations based on variations of the relative moisture. The results are briefly stated in the new abstract:*

*"We then analysed how moisture origins affect precipitation isotope signatures by mapping isotope signatures based on the moisture uptake locations we determined. Our results demonstrated effects of moisture origins on precipitation isotope signals in Luxembourg (Western Europe). More specifically, we found that remote (>1500 km) moisture sources over the Atlantic Ocean are major contributors to precipitation in autumn and winter, while replaced by mid-range (< 1500 km) and local (< 500 km) moisture sources in spring and summer, with an average $\delta^{18}$O value of -8.1 ‰ and d-excess value of +10.8 ‰. We also found that differences in isotope signals from contrasting moisture origins are season-dependent, which we argue is linked to changes in the balance of transpiration and evaporation in moisture stemming from land sources, or specific properties of the Western Mediterranean and the Bay of Biscay. Orographic barriers, such as the Pyrenees, Alps, or Massif Central also had an influence precipitation isotope signatures."*

# *Specific comments*

Line 29: In which sense stable isotopes of water are "near-conservative" if isotope fractionation occurs during phase transitions?

*→ Stable isotopes of O and H are near-conservative in the chemical sense as they are part of the water molecule, so they are not absorbed or do not react with other substances in chemical reactions, unlike other tracers that are dissolved in the water, e.g., $Cl^-$. We will clarify that.*

Line 47-48: What is the timescale of hydrological processes that is interesting for the community/in this study? Daily/Monthly/Seasonal/Yearly?

*→ In hydrology, the timescale of hydrological processes that is typically interesting for the community goes from hourly to weekly when analysing flood events and saturation processes, but for droughts and groundwater recharge, longer timescales going from weeks to years can be considered. In any case, this sentence will be removed as it leads to confusion and is not crucial to the manuscript.*

Line 55: Why ~20 years if GNIP data exists for 50 years? Give a range?

*→ 20 years referred to the bulk of GNIP stations, but that statement does not add much to the discussion on top of being contestable. We will remove it.*

Line 65: Why are isotope-enabled climate models difficult to constrain? Is it due to the difference in timescale between observations and models or because processes driving isotope variations in precipitation are not well understood? Not clear.

*→ Both, and because GNIP stations are unevenly distributed and observations are lacking in some regions of the World, e.g., Boreal regions.*

Line 77ff: Your study is based on daily to sub-daily data. Which processes are relevant at this timescale?

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

Line 77ff: It is not clear how atmospheric circulation patterns influence δ18OP. Changes in moisture source, condensation conditions, post-formation processes? Specify this.

*→ Below is the section in the new version manuscript where we address this.*

*"In this study, we conjectured that contrasted moisture origins (e.g., over the Atlantic Ocean, Mediterranean Sea or continents) affect sub-daily $\delta^{18}O$ and d-excess signals in precipitation, and $\delta^{18}O$ residuals after removing local meteorologic effects. To test our hypothesis, we relied on six years of high-resolution (i.e., sub-daily) precipitation $\delta^{18}O$ and $\delta^{2}H$ data, and hourly meteorological data recorded in Belvaux (Luxembourg). We used a pre-established Langrangian model to visualise 120-hour air mass trajectories and determined the moisture origins for 648 precipitation events. We then analysed how moisture origins affect precipitation isotope signatures by mapping isotope data based on the moisture uptake locations. Next, we employed a simple modelling approach for $\delta^{18}O$ in precipitation based on multiple linear regressions using local meteorologic variables to plot the residuals according to moisture source locations. Applying this method, we aimed to disentangle local effects (i.e., linked to meteorologic variables) from remote effects (i.e., linked to atmospheric circulation), and assessed potential implications for reconstructions of long time-series of $\delta^{18}O$ in precipitation.*

Line 81: Why you choose a subjective classification scheme and not an objective criterion.

*→ As mentioned previously, we have simulated the trajectories using the HYSPLIT model in backward mode 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.*

Line 89: What do you refer to with "precipitation data"? Is it isotope data, samples or meteorological data? Temperature, relative humidity, precipitation amount? Did you use other parameters?

*→ Samples is the correct term. We also obtained, temperature, relative humidity, surface pressure and the precipitation amount from nearby stations.*

Line 100-104: Can you give more details on the analyses? How many injections per sample? Did you account for the memory effect? What is the frequency of standard analysis?

*→ Below is the section in the corrected manuscript:*

*"Standards provided by the instrument manufacturer were used for the calibration, as well as an internal standard ($\delta^2H$: -52.6 ‰, $\delta^{18}O$: -8.1 ‰) consisting of local tap water calibrated on IAEA standards. For each sample, eight injections were made, discarding the first four to avoid memory effects. The standards were tested every three samples to check for deviations and later correction."*

Line 104: the secondary d-excess parameter is not introduced. Add the formula and explain how it complements the δ18O and δ2H data.

*→ We will add the formula. Below is the statement on how it complements the $\delta^{18}O$ and $\delta^2H$ in the new manuscript.*

*"The d-excess value is a proxy for evaporation conditions at the moisture source (Merlivat and Jouzel, 1979) and reportedly relates to remote over-sea relative humidity and sea surface temperature (Aemisegger et al., 2014; Bonne et al., 2019; Pfahl and Sodemann, 2014)."*

Line 105: Reference Kirchner (2016) missing in the reference list. Why the amplitude of monthly amount-weighted δ18OP is of interest?

*→ Reading the reviewers' 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.*

Section 2.2: The classification is obtained from a database or have you done it on your own? In the former case, cite the database. In the latter case, specify which criteria were used. Give the period and temporal resolution of this classification. Did you distinguish only the three major patterns or also the sub-types? Meteorological characterization of the CPs would help to better understand the differences. Consider adding a map.

*→ Please refer to our previous response where we explain how we decided to use HYSPLIT simulations instead of the subjective Hess and Brezowsky catalogue. We will add maps showing the trajectories and moisture origins of incoming air masses for precipitation events. We will also explain the criterions and boundaries used for the classification of the precipitation events.*

Section 2.3: Why "reanalysis data" in the section title? There is no reanalysis data described in this section.

*→ This was a mistake, we apologize for that.*

Line 139: Which GNIP stations did you include and how many? Was there a criterion to include or exclude stations?

*→ The GNIP stations had to be located in continental Europe and have at least 24 observations. This section was removed from the manuscript, however.*

Line 168: Give minimum and maximum values instead of the range of δ18O and d-excess as it might be unevenly distributed around the amount-weighted average value.

*→ Thank you, we will do that.*

Line 170: Not clear which one is high in which season as δ18O and d-excess show inverse patterns.

*→ This section was removed from the manuscript.*

Line 172-174: How the LMWL was determined? Based on sub-daily, daily or monthly data or interannual monthly data?

*→ We will add a figure in the supplements where we show the data on the dual isotope plot. It should be clear then that it is sub-daily data.*

Line 175-181: Is there seasonal variability in the precipitation amount?

*→ Yes, we have added a sentence on that.*
*"Mean annual precipitation was 876 mm, with seasonal fluctuations comprised between 167 mm (MAM/JJA) and 310 mm (DJF), and 230 mm in autumn."*

Line 189: To which CP refer HCE and LCE? Zonal/Meridional/Mixed? They haven't been introduced in Section 2.2, aren't they?

*→ This comment is outdated with the new trajectory classification. We will make sure the new categories are well-defined.*

Line 205: Why these values are relevant? Do they reflect the annual average isotope composition of precipitation? Why they deviate from this value?

*→ Please refer to our previous response, we decided to remove the sine wave fits from the manuscript.*

Line 208: Does the seasonality of CPs influence the sine wave curve calculation?

*→ Most likely they do, but again, we decided to remove this section from the manuscript.*

Line 210-211: Is there a table or figure showing these data for all CPs?

*→ There was not. We will add those in the supplements.*

Line 216-220: Is this also reflected in the amount-weighted average for each CP?

*→ Yes. The seasonal, moisture origin-specific means and standard deviations of precipitation* $\delta^{18}O$ *are now given in Fig. 5 and Table 1.*

Line 224: Does calculating a sine wave curve make sense for HCE of there is little data in winter (or summer?)?

*→ You are right to ask this, it does not. We removed it from the manuscript.*

Line 234: Is this expected?

*→ Yes, it is. But since it added little to the discussion, we will remove it.*

Section 3.2: Here, you evaluated data on weekly scale, while before you presented data on monthly scale, but it is an event-based model… Please restructure to guide the author with the different timescales!

*→ That was indeed confusing. Please note that this section will also be removed.*

Line 280: Explain this normalization. Is it commonly used? How to interpret this normalized value?

*→ The underlying idea was that the RMSE should never be higher than the standard deviation of the data, elsewise the model results could not be considered better than random value attributions. It was also meant to prevent coastal stations with low isotopic signal amplitudes to artificially drive the RMSE down to lower values. This normalization of the RMSE becomes obsolete with the new version of the manuscript, however.*

Line 327: This is the first time you mention these post-precipitation formation processes. They should already be introduced in the introduction.

*→ We will move it up to the introduction.*

Line 330-331: Do you observe lower slopes in the dry season? Show these results in the Result Section. Why would you expect lower slopes during the dry season, i.e. why rain re-evaporation should be pronounced in the dry season? Is it a temperature or an amount effect?

*→ We will add those in the supplements. The dry season often corresponds with the summer season, which is a period with high energy at the boundary layer, which enhances effects such as re-evaporation.*

Line 333-336: Does this exclude the rain re-evaporation process being the key driver of the precipitation isotope composition or do both, moisture source and re-evaporation overlap?

*→ This is probably impossible to say with the data we have. Our assumption is that both overlap.*

Line 337: What do you mean with "memory effect" here?

*→ We meant that precipitation isotopic signals have an integrative nature due to long residence times in the atmosphere. But since it leads to confusion, we will remove it.*

Line 346-349: I found the observed d-excess not exceptionally high. It is rather close to the global average. So, there is no contribution of Mediterranean air masses or moisture recycling or are there certain CPs that show higher values?

*→ The contributions of Mediterranean vs continental contributions to the isotope signature are now discussed in lines 450-458:*

*"Air masses from the Mediterranean Sea, reported to carry highly positive d-excess values (Araguás-Araguás et al., 2000; Celle-Jeanton et al., 2001), could be detected in Luxembourg in some cases in spring and summer only (Fig. 7). It should be noted, however, that d-excess values at our study site were far from the 22 ‰ reported for moisture from the Mediterranean Sea (Celle-Jeanton et al., 2001), even for rain event when moisture origins were from around the Mediterranean Sea (Figs. 1b & 7). Overall, precipitation with Mediterranean moisture origins were rare, except for western parts of*

*the Mediterranean Sea near the Spanish shores (Fig. 3). Interestingly, this region also yielded highly negative d-excess values in summer (Fig. 7). We argue this could be linked to seasonal changes of the components of evapotranspiration contributing to air moisture. The isotope signature of land evapotranspiration is difficult to seize because transpiration from vegetation leads to less negative $\delta^{18}O$ values, closer to or invariant from oceanic input (Krklec et al., 2018). "*

Line 349-351: Do you observe seasonal variations in d-excess that could be linked to seasonality in continental ET?

→ *Please refer to our previous response.*

Github: It would be great if you could provide a metadata sheet that explains shortly the different files and R scripts.

→ *Good remark, we will do that.*

*Authors' response to Reviewer 3*
*[hess-2024-4169-RC3]*

*We thank the reviewer for his/her evaluation of our manuscript and his/her 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*

The novelty and scientific significance are not well described. Particularly, the application of RSME, which, although highlighted as a novel approach, has been widely used for similar tasks in previous studies or has already been tested (e.g. https://onlinelibrary.wiley.com/doi/abs/10.1002/hyp.14254, https://www.sciencedirect.com/science/article/pii/S016980952300090X).

*→ Thank you for bringing this to our attention. It was not our intention to highlight the RMSE as a novel approach. The significance of our work is that while atmospheric circulation patterns are commonly perceived to affect isotope signatures, it is not clear how, e.g., the moisture origin affects precipitation isotope signals. This information is however crucial for interpreting precipitation $\delta^{18}O$ and making predictions, particularly at the long term (reconstructions into pre-instrumental times spanning over 60 years). That is where we want to contribute with our work.*

Notably, the authors utilize a sub-daily precipitation approach, which is uncommon in the monitoring of isotopes in atmospheric precipitation, where event-based and composite monthly samples are typically collected. More discussion should be added on the advantages of this method compared to event-based and composite monthly samples. Why are these sub-daily data more beneficial for the construction of isotopes and climate-related data since 1881? How can these data be better compared with ice-core, tree, lake sediment, and other types of isotope data used in climate reconstruction? What is the novelty and scientific importance of the method? The linear method is not the best approach to examine the non-linear relationships that can be highly important in relating isotopes and atmospheric oscillations, which mainly have sinusoidal modes. This method has been compared to the AI ML model of Piso.AI, which is based on determining the principal factors controlling the isotopic composition of precipitation and on the prediction function, which is based on non-linear relationships between isotopes and main determinants. This model also does not account for the outliers derived mainly in the winter months.

*→ The main strength (and novelty) of our approach is that with the sub-daily isotope dataset, we can attribute isotope signatures based on the atmospheric circulation patterns (moisture origins of the air masses reaching our study site), which would not be possible working with monthly data. In the new manuscript, we use this to map isotope signatures based on the moisture uptake locations that we determined using the HYSPLIT model in backward mode. High-resolution isotope datasets are found in other studies that want to assess the effects of atmospheric circulation on precipitation isotope signals in other regions (e..g., Juhlke et al., 2014; Krklec et al, 2018).*
*This information improves the interpretation of precipitation isotope signals and could contribute to assessing potential changes of the moisture origins of precipitation. The effects of atmospheric circulation on precipitation isotope signals are also important to consider when reconstructing precipitation $\delta^{18}O$, especially over long timespans, e.g., when comparing them with natural archives, such as ice-core, tree, lake sediment, and other types of isotope data used in climate reconstructions.*

*The linear method not being the best approach to examine the non-linear relationships in isotopic signatures is an accurate remark, but we want to stress that sinusoidal modes found in the isotopic signals are also found in the input variables, i.e., the temperature. Our modelling approach has also shown that the multiple linear regression models capture the seasonal component of the precipitation isotopic signal well (Fig. 5 in the old version of the manuscript). We also want to stress that the aim of our paper was to explore how simple empiric relations could be used in predicting precipitation $\delta^{18}O$ with few, or generally available variables – also considering the fact that meteorological and isotope data become increasingly scarce when going back in time. We are aware that Piso.AI yields better results by incorporating non-linear relationships between isotopes and main determinants, and partially, the challenges encountered in this study showcase the validity and applicability of AI-based solutions for predicting precipitation $\delta^{18}O$. Note, however, that given the remarks of the other reviewers and the problems faced with our approach, we decided to remove the prediction of precipitation $\delta^{18}O$ chronicles from the manuscript and focus instead on describing the influence of the moisture origins on high-resolution precipitation isotope signals at our site.*
*Outliers should be given more attention, we agree. We will add a figure in the supplements to show the data on a dual isotope plot, where outliers should obtain more visibility, as they will often plot below the Global Mean Meteorologic Water Line (GMWL).*

The scientific quality should be improved, particularly by considering related works and including appropriate references. For example, the temperature effect is not a stationary effect even in continental stations. This can be a reason for the poor prediction of isotope values in winter precipitation. Here, more climate and possibly orographic parameters should be included in the regression. Non-stationarity of isotope values in winter precipitation can be due to a shift towards the precipitation amount effect, and this should be checked and discussed in relation to similar studies. Another point is whether the temperature effect, as the correlation between isotope and air temperature, is a constant function over time. Maybe this effect can be stronger or weaker depending on larger-scale oscillations such as the Multidecadal Atlantic Oscillation or the shorter-term North Atlantic Oscillation. Additionally, explanations should be added on how to relate isotope values in sub-daily precipitation to daily or monthly climate parameters. The paper is missing a strong discussion based on papers that used a similar approach. More references should be added. Even the references on the physical nature of oxygen and hydrogen (as explained in the Introduction) should be revised, and more classical isotope-related studies should be included.

*→ Thank you for the suggestions. We have added two paragraphs in the introduction discussing the processes affecting the atmospheric water vapor from which the precipitation is formed.*
*We checked for the amount effect and found that it did not influence our data significantly. The amount effect is usually reported to more important in subtropical regions, which is why we did not explore it explicitly.*
*The Multidecadal Atlantic Oscillation (AO) and the shorter-term North Atlantic Oscillation (NAO) are interesting leads. We did find a certain correlation between the GNIP $\delta^{18}O$ data in Trier and the NAO index, but given the new structure of the manuscript, it does not fit the scope of the paper.*

The introduction doesn't reflect the title of the paper, results, and discussion. A significant part of the introduction is focused on isotopes in streams, but this is not well documented in the results and discussion. The introduction should cover the state of the art related to isotopes and atmospheric circulation, reconstruction of the climate and isotope values, and more clarification on daily circulations should be added. The figures should be improved, for example, by reducing the abbreviations in the legends.

*→ Again, thank you for the suggestions. We have fully revisited the structure of the manuscript and added more state-of-the-art studies related to isotopes and atmospheric circulation, and*

*reconstructions of the climate and isotope values in the introduction. We also tried to improve the scientific quality by using the HYSPLIT model to visualize air mass trajectories and the moisture origins to enhance the physical interpretability of atmospheric variability influencing precipitation isotope signals. The quality of the figures was also improved.*

# *Authors' response to Reviewer 4 [hess-2024-4169-RC4]*

*We thank the reviewer for his/her evaluation of our manuscript and his/her 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*

The assumption that circulation pattern has some predictive power in modelling precipitation stable isotope composition is interesting and agreeable, however I cannot agree that an empirical δ18O–T relationship can be transferred to thousands of kilometres apart. I mean that the distance and temperature difference between the moisture source area and the location of precipitation formation can be larger between a summer and a winter month belonging to the same CP than between two CPs in a given month. Please have a look on Fig. 14 in https://doi.org/10.1196/annals.1446.019 The maps show the characteristic SLP pattern of selected Hess-Brezowsky types and the spatial clusters of temperature anomalies (and also unresponsive areas!!!) across Europe. In addition, Fig 2 in https://doi.org/10.1016/j.pce.2009.11.013 also warns that the concept of this study is untenable. The maps in top and bottom left show areas where surface temperature under H&B types is significantly different from the rest of data, that is, where a certain CP type is accompanied by specific temperature conditions. I think that it is of indicative value how the areas of European stations with the weakest relationship between surface air temperature and H&B types trends correspond to the areas where the modelling experiment described in the paper performed poorly (Northern Europe, Iberia, Apennines, SE Europe based on Fig 6 , 7 of the manuscript). Seeing this correspondence, it is alarming that the concept of combing Hess-Brezowsky circulation types and CP-specific δ18O -T slopes derived from the Belvaux dataset can be valid only for a restricted area in Central Europe.

*→ Thank you for the constructive remark. Your objection that empirical $\delta^{18}O$–T relations established on local high-resolution data may not be transferrable to the whole of Europe is understandable and made us re-question our approach. This point was also brought up by the first reviewer and the extrapolation of our model calibrated on LIST data on the European scale was one of the major flaws of our manuscript. We decided to follow your advice and to restrict ourselves to the presentation and assessment of the sub-daily dataset only. Instead, we will focus on the effects of atmospheric variability (moisture origins based on backward-mode HYSPLIT simulations) on sub-daily precipitation isotope signatures (and meteorologic variables). As stated in the lines 108-118 of the new manuscript:*

*"In this study, we conjectured that contrasted moisture origins (e.g., over the Atlantic Ocean, Mediterranean Sea or continents) affect sub-daily $\delta^{18}O$ and d-excess signals in precipitation, and $\delta^{18}O$ residuals after removing local meteorologic effects. To test our hypothesis, we relied on six years of high-resolution (i.e., sub-daily) precipitation $\delta^{18}O$ and $\delta^{2}H$ data, and hourly meteorological data recorded in Belvaux (Luxembourg). We used a pre-established Langrangian model to visualise 120-hour air mass trajectories and determined the moisture origins for 648 precipitation events. We then analysed how moisture origins affect precipitation isotope signatures by mapping isotope data based on the moisture uptake locations. Next, we employed a simple modelling approach for $\delta^{18}O$ in precipitation based on multiple linear regressions using local meteorologic variables to plot the residuals according to moisture source locations. Applying this method, we aimed to disentangle local effects (i.e., linked to meteorologic variables) from remote effects (i.e., linked to atmospheric*

*circulation), and assessed potential implications for reconstructions of long time-series of $\delta^{18}O$ in precipitation."*

I suggest carefully checking the isotopic terminology. Usually a word, such as "data", or "value" or "variance" can be necessary after the delta symbol (see a few examples among the specific comments). In addition, please check the delta symbol carefully because simply "d" is typed at a couple of times (e.g. lines, 73, 75)

*→ Thank you for this suggestion. We understand your point of view, but it is a matter of preference we believe. Adding words such as "data" or "value" can sometimes make sentences longer and more repetitive, and thus harder to follow. Especially in the results section, where the term "$\delta^{18}O$" appears a lot. We made a conscious decision to remove these words, also because we think that it does not create ambiguity for the reader, we hope you will understand. We will correct cases where the letter "d" was used instead of the delta symbol.*

## *Specific comments*

line 25: "assumption-lean" is somehow confusing for me. If you mean that there is no assumption, then I do not agree. (See my detailed comments above)

*→ We understand your objection. Please note that we have decided not to include the generalized model for Europe in the new manuscript.*

lines 49-53: This recent paper https://doi.org/10.1007/s13137-023-00224-x is a highly relevant reference for this statement.

*→ Thank you for bringing this study to our attention; we will add Erdélyi et al. (2023) to the manuscript.*

line 60: Unclear meaning: "…the influence of climate forcing"

*→ We meant that climate forcing alters the atmospheric circulation patterns, thus influencing the precipitation isotopic signatures.*

line 65: I re-read the PisoAI paper and think that it is not a pertinent reference to support this statement. Nelson et al. did not constrain any isotope-enabled climate model.

*→ Thank you for pointing out this discrepancy; we will restructure or remove the sentence accordingly.*

lines 141-142: What does "full year" mean? 12 consecutive months? I mean it could be from Jann to Dec, or Feb to Jan are equivalently, OK? Or how did you treat a year when monthly perc is 0mm in one or two months? How do you treat a station which recorded from Jan to June in 2000 and July to Dec in 2001?

*→ This is a good point. We had only considered the number of observations but omitted that they could be non-continuous. It would be better to consider only stations with 12 consecutive observations but note that this section will be removed as we have decided not to include the generalized model for Europe in the new manuscript.*

line 153: Which “DEM grid”?

*→ It was the DEM that could be downloaded from the ERA5 platform. This section will also be removed from the manuscript.*

line 155: I’d add “values” or “data” to the end of this sentence.

*→ Please refer to our previous reply.*

line 167: The sentence says that the end of the sampling interval is December 2022, while in line 97 it was written “the end of the sampling campaign in January 2023”. Please clarify it.

*→ We apologize for this inconsistency; we will clarify it.*

line 170: Please add a comma before “respectively”.

*→ We will do that.*

line 209, 220, 223, 355, 371: Please add “data” or “value” after both δ18O and d-excess

*→ Please refer to our previous reply.*

lines 223-224: Does this high negative phi value acceptable for HCE? I assume that the phase should be close to 12 months (or less assuming a semiannual cyclicity for instance). Moreover, an estimated 98-monzh phase (periodicity?) based on a 62-month dataset is definitely uncertain.

*→ It was surprising yes, but probably due to the low number of observations for the HCE circulation pattern. Also, the phase was given in days, not months. The second reviewer has already mentioned that switching between timescales too much would confuse the reader, so we will clearly state what timescale we are working with when displaying results, e.g., by including the number of samples that went into calculating statistics. This should avoid such misunderstandings. Please note also that reading the reviewer’s comments, we realized that the sine wave fits added unnecessary complexity and led to confusion for the reader. 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.*

lines 267-268: Similarly “Systematic overestimations towards the lower end of values” i.e. he winter minima was reported by other models (e.g. https://doi.org/10.1073/pnas.2024107118, https://doi.org/10.1007/s13137-023-00224-x ) It might deserve a bit more discussion.

*→ Thank you for bringing this point up, it is indeed interesting to mention. Please note that since the new manuscript will not focus as much on the models (refer to our previous reply for this specific reasons), this part might be left out.*

lines 324-326: This is very speculative. Probably the interannual difference could be sufficient to explain the difference. You can try to eliminate the interannual difference calculating the mean weighted δ18O value for the same period.

*→ The timeseries do not overlap, unfortunately. Interannual differences are also discussed as possible reasons explaining the difference:*

*“The LMWL slope of 7.46 for Belvaux (Luxembourg) ranks amongst the lowest reported for Germany – solely exceeding northern stations (Stumpp et al., 2014). The LMWL for Belvaux was found*

*to vary significantly between dry and wet years, and with changes of the summer temperature (Vodila et al., 2011). We also found seasonal differences in LMWL slopes (from 7.14 in summer to 7.84 in winter). These findings may eventually relate to the unprecedented high temperatures recorded during the sampling period."*

lines 327,435 As far as I know "in-cloud evaporation" is rarely an issue. Authors might think about "sub-cloud evaporation".

→ *Thank you for the clarification, we will change that.*

lines 333-335: If I understand well, then this is an argument against the significant role of sub-cloud evaporation, contradicting with the previous statements.

→ *Not really, to us, it just means that the moisture contributions are different at the two sites. For the re-evaporation effect, we rely on the slope of the LMWLs, which is lower in Luxembourg, indicating stronger effects of re-evaporation. Below we propose a new formulation:*

*"Despite the lower LMWL slope in Luxembourg, the weighted d-excess value (10.8 ‰) at our study site was still higher than in Trier (6.3 ‰). It is possible that there is a stronger recycling of air moisture in the Mosel River valley, causing lower d-excess values."*

line337-338: Unclear meaning. This sentence is confusing.

→ *We agree, the sentence will be removed.*

lines 362-364: Why? What is the supporting evidence?

→ *This sentence became outdated with the new atmospheric circulation classification scheme we used; it will be removed.*

lines 379-380: I understand this statement, but how is it related to the δ18O -T relationship? The lower δ18O values during colder climate is not equivalent with the constancy of the δ18O -T relationship.

→ *The underlying message was that only 20 percent of variance in precipitation $\delta^{18}O$ values can be explained by changes in air temperature alone, while it was shown that precipitation $\delta^{18}O$ were lower under colder climates. Thus, other processes, such as changing atmospheric circulation patterns must have influenced precipitation $\delta^{18}O$. We will rephrase this.*

lines 391-392: It is debatable. The model skill was not validated at the sub-monthly scale.

→ *We agree. Please note that we have decided not to include the generalized model for Europe in the new manuscript.*

line 394: I suggest omitting "WMO" before "Climate Explorer". But anyway, the original datasets of historical temperature records should be mentioned here rather than a web application to analysis climate data statistically.

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

line 437: The statement needs revision. Currently it says that average δ18O and d-excess values of the Atlantic Ocean is -8.1 and 10.7‰.

*→ Thank you, we will correct it.*

Figures and Tables

Figure 1: The first sentence of the caption says that time series are between 2017 and 2022, however in section 2.1 it was written that sampling started in December 2016 and ended in January 2023. Please clarify it.

*→ We apologize for this inconsistency; we will correct it.*

Figure 6: This map suggests a reasonable validation only for the surrounding of the Belvaux station or. This pattern suggests that the constructed model can be suitable only for the surrounding of the training station (Central Europe?).

*→ Correct, this figure will be removed from the manuscript.*

Figure 8: What does the capital letter in the brackets show? If country code, as I assume, then it needs double checking. Similarly the elevation data for Athens-Pendeli needs checking.

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

Figure 9: What are the blank areas in Norway, Bosnia and Italy?

*→ Probably the number of observations was less than 12, or there was some mishandling of the data during preparation. This figure will be removed from the manuscript.*

I suggest combining Table 1 and Table 2.
In addition, negative values look strange for amplitude in Table 2. Please check them.

*→ The sine wave fits were removed from the manuscript; the tables will be adapted accordingly and contain median, interquartile ranges and minima/maxima instead.*