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

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

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 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 text that we propose:

"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."

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 something went wrong with the data 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.