

Interactive comment on “Simulated oxygen isotopes in cave drip water and speleothem calcite in European caves” by A. Wackerbarth et al.

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We thank referee#2 for the detailed review of our manuscript and many suggestions he/she made for improving the paper. First, we like to point out that the focus of our paper is not the interpretation of climate proxies with regard to the NAO. The NAO mode was only introduced in the last chapter as a possible explanation for the d18o pattern at 6ka before present. The NAO is in our opinion still a complicated subject, which requires further research. We therefore handled this topic very tentatively and refrained from putting too much focus on it. Our paper is a conceptual paper which establishes the forcing of a proxy model with a climate model as a very new approach. We like to present our first approach of such an application of models and discuss potentials and weaknesses of the approach. Including the 6ka experiment and presenting a possible

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climate interpretation was done to show how our approach could be used in the future.

Detailed reply:

Referee comment 1: 1) I was a bit irritated by the last chapter (4.4) which makes a point for a probable NAO like pattern both in the observations and in the simulations. First, why actually discussing NAO like patterns in T, P and d18O when actually the model allows to check for the simulated NAO directly? Why not showing the NAO change?

Reply: The NAO as an atmospheric pattern is bound to show a higher frequency than typical “climate” timescales. Therefore it is not clear if at 6ka before present the d18o pattern is really influenced by a NAO mode or rather a NAO like pattern. As we understand the referee suggests checking the NAO index at 6ka. However, it is possible that the NAO index is on these long time scales not a good parameter to define the NAO mode due to significant changes in the synoptic, meteorological patterns. A better way could be to show prevailing pressure or wind fields, but we find that this would be beyond the scope of the paper. We would like to emphasise that reconstructing the NAO is not the focus of the manuscript.

Referee comment 2: Second, independent of this question: If I understand the model set up right then the authors compare NAO like variability from a 45 yr present day simulation forced with observed SST with an AMIP climatological anomaly simulation (i.e. without varying SSTs). Though the NAO is principally an atmospheric phenomenon there are many studies showing an impact of varying SSTs on the NAO and vice versa. The model set up (once climatological once varying SSTs) is not ideal to make statements on NAO type of variability. However I might have missed a technical detail of the way how the Holocene runs were set up. In any case this problem should be clarified.

Reply: The referee points out here that the model setups differ significantly between 6ka and the present day run. Indeed for present day it is forced with observed SSTs and for 6ka before present with a climatological anomaly simulation. First, we would again pronounce that we do not focus on NAO variability and investigate in general

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the concept of forcing a proxy model with a climate model. In our manuscript we do not handle time series but only mean values. This is important in this context, since we do not focus here on the variability of the modelled d18o signal but only on mean values on a climatological scale. We will include a closer discussion of this subject in the revised manuscript.

Referee comment 3: I was missing an analysis on the final overall relationship between T,P and d18O Calcite. Most of the original data studies make statements on the interpretation of the found d18O Calcite signals in terms of temperature or precipitation. My feeling is that such a short analysis with the model data would strengthen the paper significantly. An additional point here is that a straightforward classification of model results (CCSM results vs COSMOS etc.) does not make a full use of the simulations. Model results, in particular under paleo conditions, are never perfect for many reasons. It is therefore interesting how robust the relationship between variables is, i.e. the relationship between d18O Calc and T,P or other climate parameters.

Reply: We are not sure what the referee indicates with his/her suggestion to analyse an “overall relationship”. The relationship between T,P and the d18o of calcite must be established individually for each of the speleothems since climate impact on the stalagmite proxy varies from location to location. For an individual stalagmite we only have two pairs of T,P and the respective d18o values (one for present day and one at 6ka before present) which is not enough for establishing a correlation. It is of course possible to derive the sensitivity of d18o to T and P variations from the 45years of the present-day run. This was done in figure 2 for Bunker Cave as an example. We think that showing the same figure for all the caves simply leads to too many figures. The sensitivity varies only little from cave to cave and figure 2 is a good example/representative for the general sensitivity study. We will point this out in the text more detailed.

Referee comment 4: 3) Seasonality changes are a major issue in the interpretation of the final calcite signal. It could strengthen the study if potential changes in the seasonality are added to the sensitivity discussion of Figure 2. One could introduce

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one single seasonality parameter (the amplitude JJA-DJF in a sinusoidal fit through the precipitation data) and rise or lower seasonality to see what the impact is on the final d18O drip or d18O Calcite.

Reply: We agree with the referee that seasonality changes are a major issue in the interpretation of d18o as a climate proxy. However, it is hard to define how the parameters vary exactly when we talk about a changing seasonality. The referee suggests a sinusoidal signal with more or less amplitude, which seems to be a good assumption. However, most of the locations do not reveal a characteristic pattern of the monthly P values, that a sinusoidal fit is a too strong assumption for the true parameters. Any fit of the monthly P values and any prescribed long term change in the seasonality comes close to guessing. In our opinion the best way might be to amplify e.g. the winter season linearly (as the simplest assumption) and analyse the result for the d18o value. Under this assumption the d18o signal of the drip water shifts straight forward towards the d18o value of the amplified months. However, for the d18o value of calcite a variation of the seasonality of meteoric precipitation is more complicated due to other parameters which play a major role (supersaturation of the drip water, pCO₂ of cave air, ...) which are not included in the model. We will discuss this more explicitly in the text.

Referee comment 5: There is only a relatively weak dependence of the ODSM model to variations of the annual mean P (Fig 2). However in Tartair cave there is a huge precipitation deficit in the simulation (1100mm/yr) and only a small d18OPrec difference.. When taking this deficit into account (ie running the ODSM model) shouldn't this have a bigger influence on the d18O Calcite values according to Fig2? Also the simulated d18OPrec values is more depleted than the observation (Fig 1c) and the resulting values for the drip water are more enriched relative to the observations. However the model is too dry which should result in an additional depletion (according to Figure 2). So obviously I am a bit confused here. Might be the authors could clarify this point.

Reply: The referee points out here that it is remarkable that the modelled d18o of

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precipitation is heavier than the measured value, but the modelled d18o of drip water is lighter than the measured. Since the d18o of drip water is more sensitive to d18o of precipitation than to any other parameter, this is hard to understand. However, we have to notice that there is still the possibility that the model is not a good representation of the true cave system and over- or underestimates processes. The sensitivity of d18o to climate parameters can be investigated using the model. However, it is essential to understand that the true cave conditions might be different and the model can over or underestimate certain processes. If the discrepancy between modelled and measured d18o values cannot be explained with sensitivity analyses, it seems that the model is in the particular case not a good representation of the true conditions as it is in Tartair Cave.

Referee comment 6: "The modeled seasonal pattern of infiltration might be not representative for the true seasonal pattern." So there are no seasonal data for drip water? It seems to me then that this is an important lack of information that could be mentioned in the conclusions.

Reply: Actually many of the caves exhibit a very good monitoring program with monthly drip water measurements. But it must be noted, that the pattern of monthly infiltration (P-E) cannot be derived straightforward from drip water values due to extensive mixing in soil and epikarst which is smoothening the signal. The actual amount of infiltration is unfortunately a parameter which is extremely hard to determine or measure.

Referee comment 7: Are there no criteria/parameters to estimate the strength nonequilibrium calcification? The model's assumption of equilibrium condition always and everywhere seems obviously not correct.

Reply: We agree with the referee completely. Of course are there models which described non-equilibrium fractionation. These are very complex and need many input parameters which are not available for most of the stalagmites. We discuss this subject in the revised manuscript more closely as requested by the first reviewer.

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Some smaller points the referee raised will be clarified in the revised version.

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