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Climate of the Past Discussions

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

Anonymous Referee #2

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The paper "Simulated oxygen isotopes in cave drip water and speleothem calcite in Europen caves" by Wackerbarth et al. presents the result of a forward simulation of a key proxy of paleoclimate, the isotopic composition of cave calcites (18O) as measured in many speleothem studies in the last years. The paper clearly attracts the interest of the readership of CoP. The one-to-one comparison between simulated and modeled paleoproxies avoids the inherent problems of so many paleo-modelling studies comparing model parameters such as temperature and precipitation with the interpretation of different proxies. Here the author study both the simulated climate and the impact of the changed climate on the respective paleo-proxie. Such an approach is certainly the way to go in the future. The paper is well structured and the interpretation of the result is clearly presented and sound. I have only three mayor questions and a couple of smaller points that can easily be answered before the paper can be accepted.

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? 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. 2) I was missing an analysis on the final overall relationship between T,P and d18OCalcite. Most of the original data studies make statements on the interpretation of the found d18OCalcite 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 d18OCalc and T,P or other climate parameters. 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 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 d18Odrip or d18OCalcite.

P3L1 "demonstrate correlations" P3L14 "subsurface processes in the biosphere" sounds strange to me. What about "biological processes in soils" or something similar. P4-5 I know I should check in the original literature: Is there a soil classification/soil

type dependent parameterization in the ODSM model? In any case this should be mentioned. P8/9 and Fig 2. 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 d18OCalcite 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.

Fig 1: Please explain in the caption to what the error bars are referring.

P12L15 "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. P13 P5 Are there no criteria/parameters to estimate the strength non-equilibrium calcification? The model's assumption of equilibrium condition always and everywhere seems obviously not correct.

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