

Clim. Past Discuss., author comment AC2
<https://doi.org/10.5194/cp-2021-185-AC2>, 2022
© Author(s) 2022. This work is distributed under
the Creative Commons Attribution 4.0 License.

Reply on RC2

Léa Terray et al.

Author comment on "The use of paleoclimatic simulations to refine the environmental and chronological context of archaeological/paleontological sites" by Léa Terray et al., Clim. Past Discuss., <https://doi.org/10.5194/cp-2021-185-AC2>, 2022

We thank the reviewer for constructive comments and suggestions.

Main comments:

The authors address an important issue in paleoclimate and make seemingly innovative steps in combining climate modelling with uncertain paleoclimate chronologies. That said it is not clear that this method would in general produce robust results. Although, this paper demonstrates one example, it does not take account of uncertainties or biases in the climate model simulations or in the records (other than treating the two as equally likely). Something more formalised based on for example Bayesian methods would seem to be more robust, though it may not exist yet.

The study we present in this article is indeed completely exploratory. Our aim was first to test which information we could deduce from a "simple" consistency approach between global paleoclimate simulations and paleoenvironmental inferences from an archeological site. As the reviewer suggests, this may pave the way for the development of more robust methods combining both fields (paleoclimatology and archeology/paleontology). So far, our approach may indeed concern a limited number of well studied sites. However, with more powerful statistical tools, it could be extended to other sites whose context is less referenced, and perhaps even help establish it. The limit is that the sampling is limited so that rigorous uncertainties 'or uncertainty matrix' might be difficult to properly estimate

The present method also does not address the underlying causes of interpretational issues with the paleoclimate data. For example, would it not be possible that the improved correlation between climate and isotopes could be because the more biologically-derived proxies are a more complex function of physical drivers than the water isotope signal?

It is true that the paleoenvironmental proxies we used are not equally related to climate variables. Isotopes have been collected from rodents teeth, meaning that the isotope fractions is related to the dietary preferences of organisms, and depend on temperature and vegetation. Thus, isotopes are related to the magnitude of seasonal variation in insolation, water stress, temperature and diurnal temperature range. These variables condition the presence of essential elements for plants survival (e.g. sunlight, water in the soil), and thus may participate to determine the type of vegetation. On the other hand,

the taxonomy habitat index (THI) rely on ecological preferences of species. Altogether, the variables of the THI give information about the proportion of biomes (e.g. forest, bush, steppe), and thus the spatial distribution and density of the vegetation. Consequently, its relationship to climate is more indirect than for isotopes. Moreover, the THI is linked to the overall (mean and seasonal) climate variability, while isotopes are related to the magnitude of seasonal variation only. Isotopes somehow better reflects the large climatic changes in terms of temperature and precipitation that have occurred in the past. The THI seems indeed to be a more complex function of climate variables than isotopes. However, these two paleoenvironmental proxies can be considered as complementary since they do seem not reflect the same climate parameters. They thus offer a way to infer the overall consistency between climate and the cave environment. We added these comments in the discussion of the manuscript.

It is not clear how the combining (and perhaps weighting) across different climate model variables does or could give a different outcome.

The choice of climate variables was based on their potential direct or indirect impact on organisms, as explained in the manuscript. However, at first, we selected many more variables. Then, we tested the correlations between the selected climate variables. We excluded redundant variables and chose to use a reduce number of uncorrelated variable in order to reduce data dimensionality.

Minor comments:

For the description of the climate simulations it would make more sense to re-write this with a focus on the new simulations that you have performed rather than on the older ones on which you based the SST fields. Then, whilst I appreciate the effort in evaluating the biases in the different model versions, I would recommend that the discussion of this, along with figures 2-4 are placed in an appendix. To me this would improve the readability of the paper.

Indeed, it is true that the emphasis on model biases make the paper heavy to read. We will rearrange the manuscript and pass this part in appendix following the reviewer suggestion.

The wording used to describe DH1 and 2 is sometimes confusing. DH1/2 are first introduced as two alternative chronologies from the paleoclimate records, but later in the text they are used to describe a chronology of the simulations (e.g. line 367). Please could you go through and ensure that it is clear in each instance which is meant?

We take good note of it, these parts will be rephrased to avoid confusion.

Abstract: 1st sentence isn't clear. How about something like? : "Reconstructing the paleoenvironmental and chronological context of archaeological/paleontological sites is a key step for understanding the evolutionary history of organisms."

In the abstract please define "US-ESR" and "OSL", or you could just say two different dating methods?

Line 11: Please elaborate what you mean typically by "drastic discrepancies".

Line 28: "whose microvertebrate assemblages have been extensively studied" - please provide a few example references here.

We thank the reviewer for these suggestions. We will further detail our terms in the abstract and the introduction.

Figure 1: not clear how the dates in panel F relate to the other variables.

The dates and the variables are related through the stratigraphical layers (analyses have been performed on the same layers). We will clarify the figure.

Figures 5/6: can you add the times in kyr BP to the grey boxes/legend to match the text (e.g. at line 257).

Sure, we will add times to the figures.

Lines 297-303: It's not clear to me how the principal component analysis is calculated e.g. from which variables and times? Please could you add a paragraph here to explain this?

The principal components analyses (PCA) have been performed on climate variables presented in Table 2, and grouped per stratigraphical layers. For each climate variable, we considered both the annual mean and seasonal variation (standard deviation). Thus, the PCAs present the climate proximity/differences between the stratigraphical layers of El Harhoura 2. A detailed explanation will be added to the manuscript.

It's not clear that the approach for MIS5d in DH2 (L5-8). Have you replicated the climate simulation for 115k in each of these? Is this reasonable given the variations in curves B-E of Figure 1?

Yes, the climate simulation for 115k has been replicated for layers L8-L5. Indeed, paleoenvironmental indicators support different conditions between L8 and L5. However, the dating resolution does not allow us to distinguish the ages to these layers. This is illustrated by the inconsistency between absolute and relative datings: L8 is dated to ~106k, while L6 is dated to ~116k, while L8 is below L6, and therefore necessarily older. That is why we choose to group those layers.

Line 366: do you mean overestimated as in too old, or that the uncertainties are over/under-estimated?

It is overestimated as in "too old", this precision will be added in the manuscript.

Line 425: "interglacial"->"interstadial"? There was no interglacial in MIS3?

Indeed, there is no interglacial in MIS3. We will correct this spelling error in the manuscript.

Discussion:

Since your results come down in favour of the isotope-based record, it would be worth discussing how the inclusion of water and/or carbon isotopes in the climate model could better refine future work?

It is indeed a very interesting lead for perspective. Isotopes could be used to refine, or at least cross verify, the simulation of seasonal variation in future works. This would indeed add a more direct consistency test to such a study. However, it is important to keep in mind that there is a difference with isotope records that are usually used in climatology: here they have been collected from rodents teeth, implying that they are also impacted by the dietary preferences of these rodents. Note also that we have tried to have a "minimum computing" time approach, and that isotope unable models are still quite expensive in computing time, and a full coupled model version with isotopes is underconstruction and was not available for this study.

I also wonder if you might speculate on how applicable this approach is going to be? It require lots of climate simulations, and is presumably only applicable where the chronological/interpretational uncertainties are large.

Such uncertainties are unfortunately common in archeology when different methods are applied, since the biases discussed depend on the methods and not especially on the site. However, if the results of this approach are promising in the case of our site, El Harhoura 2, it needs indeed the site to have been well studied. For this approach to be applicable on other sites depends of the following requirements :

- Data about the chronology of the sequence must be available from different methods
- Paleoenvironmental inferences need also to be available, and preferentially from different sources
- The sequence must be composed of several levels
- Fully coupled climate simulations of the periods of interest must be available, otherwise their complete production would represent a considerably larger amount of work. In our case, we were able to use an ensemble of multi-period opportunity ran with different versions of the IPSL model over a period of 10 years. However, such simulations are becoming more common and distributed. It opens new avenues on the way to use them to test the consistency of paleoclimate or paleoenvironement reconstructions in different regions.

However, as the reviewer suggested, with more powerful statistical tools, this approach could be extended to other sites whose context is less referenced.

All technical corrections will be included in the manuscript.

We hope that these precisions can support the relevance of our approach. Following the suggestions of both reviewers, we propose a rearrangement of the plan of the manuscript in order to improve its clarity:

- Introduction

We will more clearly state our objective, the different issues we aim to address and the approach chosen to achieve it. We will also better present the limits that are usual and inevitable in archeological context.

- Material and Methods

This part will be recentered on the newly produced simulations using LMDZOR. Several figures currently presented in the results part will be placed here to support the clarity of the reasoning. Pre-existing simulations will be less detailed. We'll present how they were run, because using the same forcings for the LMDZOR simulations, but we'll do not named them and will just refer to the period they represent We will focus on the fact that they have been running using from different model versions, that present different systematic biases. This will be illustrated by comparing the sea surface temperature of historical simulations of each model with observations (Fig2). Fig 3 and 4, illustrating the impact of these biases on the climatology will be passed in supplementary material. We will then present the bias correction we realized in order to reduce this systematic bias between model versions. As a reference model version, we choose CM6A which is the best model version for our region of interest, as illustrated by FigA2 that will be placed in the main text.

- Results

In this part, we will directly start by presenting the results of the new simulations (Fig 5 and 6). We will add a paragraph and a figure explaining the regional and global response to the forcings behind the climate variation found on our region of interest. We'll add a figure illustrating the dynamical changes inspired from Fig 1rep attach to this response to better stress how temperature, moisture and wind lead to the changes over the NW Moroccan region.

- Discussion

We will focus on the significant results of the consistency of the different proxies, and less on the different biases that have already been presented in the introduction. Further discussion about the interpretational meaning of the correlations between climate variables and paleoenvironmental proxies will be included. We also propose to more explicitly state the major results on which our conclusions are based, and present the perspectives they offer for future works.

We will also seriously consider all the constructive comments and suggestions the reviewer has made in the pdf manuscript and submit the paper to English proof-reading.

Please note that substantial changes have already been made in the manuscript since its submission, notably english improvement and clarification of the material and methods.

Best regards,

Léa Terray, on behalf all authors

Please also note the supplement to this comment:

<https://cp.copernicus.org/preprints/cp-2021-185/cp-2021-185-AC2-supplement.pdf>