

## ***Interactive comment on “Mid-to-late Holocene Temperature Evolution and Atmospheric Dynamics over Europe in Regional Model Simulations” by Emmanuele Russo and Ulrich Cubasch***

**Anonymous Referee #2**

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This study presents novel paleoclimate modelling results obtained with a high-resolution regional climate model (COSMO-CLM) that is nested in an atmospheric general circulation model (ECHAM5). These modelling results span the past 6000 years and are compared to pollen-based temperature reconstructions. The topic fits very well within the scope of Climate of the Past.

To my knowledge, this is the first study on the mid-to-late Holocene that has been performed with a regional climate model. Many modelling studies with a focus on this same time period have been published with GCMs or EMICs, and it will be interesting

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to see how the results of this regional climate model compare to these studies and to investigate whether the higher spatial resolution produce a better match to proxy-based reconstructions.

In my opinion, the innovative results do merit publication, but the presentation of the results and the discussion should be considerably improved. As detailed below, this manuscript requires major revisions before it can be accepted.

Main comments

- The grammar and spelling can be much improved. There are many long sentences that are hard to read. I have indicated a few below. I strongly suggest to have the text thoroughly checked by a native English speaker.

- I propose to compare the results of COSMO-CLM to the results of ECHAM5. The latter results have already a relatively high spatial resolution (T106 or 1.125x1.25 degr) compared to previous GCM studies. This resolution is actually close the resolution of the reconstructions (1x1 degr). In the manuscript, the authors have regridded (up scaled) their regional climate model results from 0.44x0.44 degree resolution to 1x1 degree to make the comparison in Fig 5. It would be interesting to see to what extent the COSMO-CLM produces a better match. Is it, from a paleoclimate perspective, worthwhile to make the considerable effort to nest the regional model in the high-resolution GCM results? Or do both models produces very similar results? In my view, addressing these questions would strengthen the paper. To make room for such a comparison, Figures 2, 3 and 4 could be moved to the supplementary information, as these figures do not directly concern the core topic of this study (mid-to-late Holocene temperatures and atmospheric dynamics).

- The left column of Fig 5 presents maps of the winter and summer temperature anomalies (model minus reconstructions), "averaged over all the mid-to-late Holocene time slices". It is not clear to me what the authors have actually done here. Have they first averaged the maps of the different time-slices for the model and the data, and then

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calculated the model-data anomaly? Or have they calculated the trend between 6000 and 200 BP in both model and data, and then made a map of the difference between the two methods? The caption suggests that they have applied the first method, but in my view this would only be meaningful if the anomalies are more or less constant through time, which is clearly not the case (see Figure 6). Since the trends from 6000 to 200 BP seem approximately linear in both model and data, it would make more sense to compare maps of these trends or to show maps for different time slices. Figure 11 actually shows linear trend maps for both the model and the reconstructions, but only for DJF. It is unclear to me how to relate Figure 11 to Figure 6. Figure 11 seems to indicate a pollen-based linear warming in Southern Europe of mostly less than 0.4°C, while Figure 6 shows a warming trend for the pollen-based reconstructions of 1°C for Southern Europe. In addition, the pollen-based cooling trend in Figure 6 of more than 2°C does not match Figure 11 which shows a much smaller cooling trend. Is there an inconsistency between Figure 6 and 11, or have I missed something? Please clarify.

- The right column of Fig. 5 shows the uncertainties in the pollen-based temperature reconstruction. How were these maps constructed? According to Fig. 6, these uncertainties are not constant through time, so simply averaging the errors for the different time slices is not informative here either. Please clarify.

- For the summer in Southern Europe, the model and the reconstructions show opposite trends: cooling in the model and warming in the reconstructions. The authors provide an explanation for this model-data mismatch that is based on the warm bias of the model in S Europe due to the underestimation of evaporation in summer. However, the mismatch may also be explained by uncertainty in the pollen-based reconstructions in S Europe. Paleoclimate reconstructions based on pollen rely on the assumption that changes in the vegetation were driven by the parameter to be reconstructed (i.e. summer temperature). In the Mediterranean region, vegetation distribution is mainly limited by effective precipitation, rather than by summer temperature (e.g. Osborne et al. 2000). It would therefore be good to discuss the associated uncertainties in the

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methodology of the pollen-based reconstructions and to mention Holocene temperature reconstructions that are based on other proxies. For instance, summer temperature reconstructions from the S Europe domain based on Chironomids, show a clear Holocene cooling (Heiri et al. 2015; Toth et al. 2015) that actually support the presented modelling results. In addition, Holocene SST reconstructions from the Mediterranean Sea show a similar cooling trend (e.g. Marchal et al. 2002). The discussion section should be extended accordingly.

- In the discussion, the results should also be compared to other modelling studies that focus on the mid-to-late Holocene climate. Do the new results presented here confirm earlier findings? How do the seasonal trends and 6k-0k anomalies compare to that of other models (e.g., PMIP3)? What do other Holocene modelling studies say about changes in atmospheric circulation over Europe and the North Atlantic basin?

- Conclusions: The conclusions should be made less descriptive / more quantitative. The paragraph starting on line 296 does not contain conclusions and can be removed. Please explain on Line 310 what atmospheric circulation configuration is meant here.

Minor comments

Line 26: I suggest providing a more accurate definition of climate models

Line 34: "orbital parameters". I propose to use astronomical parameters instead, since obliquity is not a parameter of the Earth's orbit.

Line 37: Please rephrase this sentence, as it is not easy to read

Line 43: "solar forcing". Usually, "solar forcing" is used to describe changes in solar activity as opposed to astronomical forcing that reflects changes in insolation due to changes astronomical parameters. To avoid confusion, I suggest using astronomical forcing here.

Line 46: In my view, this sentence does not introduce the reader to the paragraph, so I propose using a different topic sentence.

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Line 57: It is not clear to me what is meant by "hampered climate anomalies"

Line 60: typo, atmopshere

Line 60: "not being able to reproduce correctly the reconstructed data over the entire region". Please clarify. Was the model too cold or too warm? What was the bias?

Line 63: Please rephrase the sentence starting at this line.

Line 72: " In many cases" What cases, please elaborate.

The objectives of the paper should be explained more clearly. On page 3, two objectives are provided. The first objective is to "obtain a better interpretation of the new pollen database..." Why better? What problems have been encountered in the interpretation?

Line 105. This first sentence of Section 2 does not provide information on the applied methods. I suggest moving this sentence to Section 1 and to replace it with a topic sentence that introduces the methodology used.

Line 128: Berger and Loutre (2002) do not calculate astronomical parameters and is not the appropriate reference here. In their figure they show the values of such parameters, but these are based on Berger (1978), so I suggest to use this reference here.

Line 133: "only the latest ones". I am not sure what is referred to here. The latter effects?

Line 175: "while coloured are the anomalies". Please rephrase and clarify.

Line 194: I propose to use "anomalously warm conditions" here.

Line 195: " as a consequence of a wrong conversion of energy towards latent heat." This suggests to me that there is an error in the model code that described this conversion. Is that the case, or is the conversion in principle correct and does the model

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have a bias in S Europe?

Line 205: typo "teperature"

Line 213: I suggest replacing "Pollen" by "pollen-based temperatures"

Line 214: Please rephrase, as this sentence is confusing. The sentence suggests that Section 3.2 will discuss the results after the validation against Mauri et al's data has taken place, while in fact the next paragraph deals with this validation. Besides, I would prefer using evaluation instead of validation here.

Line 216: I suggest referring to Figure 1, as this figure shows the boundaries of the two domains.

Line 220: I assume that the model results are up-scaled and regridded on a 1x1 degree grid before the anomalies are calculated. Please clarify this here

Line 231: I propose replacing "Paleo-Results" by Paleoclimate results.

Line 237: Figure 7 shows the insolation changes over the mid-to-late Holocene. This is the main radiative forcing for the model experiments, so I suggest to show it already in Section 2 where the experimental design is discussed.

Line 250: what other cases?

Caption Figures 8 and 9: The captions are not consistent with the figures. Are summer results plotted at the upper or the lower row?

Figure 8: How is Figure 8 constructed? On what timeslice is it based, or is it based on results from several time slices?

Line 268: "scarce ability" Replace by poor ability?

Line 276: "showing instead low correlation over the South". This is a confusing statement. Figure 10 shows that over most of the Mediterranean, the correlation in winter is strongly negative for the 1st EOF and strongly positive for summer.

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Line 284: "the model simulates a lower weight of the NAO (~40%) for mid-to-late Holocene in comparison to present-days conditions (~55%)". How can we reconcile this with the notion of a "more pronounced positive phase of the NAO during the mid-Holocene" as stated on line 277?

Additional references Heiri, O. et al. *The Holocene* 25, 137-149 (2015). Marchal, O., et al. *Quat. Sci. Rev.* 21, 455-483 (2002). Osborne, C.P., et al. *Glob. Change Biol.* 6, 445-458 (2000). Toth, M., et al. *The Holocene* 25, 569-582 (2015).

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