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Interactive comment on "The influence of atmospheric circulation on the mid-Holocene climate of Europe: a data-model comparison" *by* A. Mauri et al.

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Dear Editor,

We would like to thank the referees for taking the time to comment on our manuscript and for providing some helpful suggestions.

We have undertaken a number of substantial changes in line with these suggestions, including increasing the number of model simulations from 1 (HadCM3) to 14 (all of the CMIP5/PMIP3 models), the inclusion of maps of reconstruction uncertainties, and the addition of a figure showing the temperature and precipitation anomalies associated with a positive AO and SCAND. The conclusions of the paper are unchanged.

Our detailed response and related action to the reviewers comments are provided below, with each comment assigned a reference number #.

Reviewer #1

This study provides an updated spatial reconstruction of the mid-Holocene temperature and precipitation over Europe both for winter and summer. The differences with present-day climate is shown and compared with simulations under present-day and mid-Holocene climatic conditions using HadCM3 model. The spatial structures are very different both in summer and winter in the model and in the reconstruction. While the model show mainly in the mid-Holocene a thermal response to the imposed radiative changes in insolation at the seasonal scale (i.e. warming in summer, cooling in winter), the reconstruction show strong latitudinal gradient with warming in Northern Europe and cooling in Southern Europe in both seasons. The authors then suggest that this pattern may be related to atmospheric circulation changes, since the signature of the modes positive NAO in winter and SCAND in summer strongly resembles the changes in mean state found for the mid-Holocene.

#1-1) This paper is correctly presented and the methods are well presented. The hypothesis defended here is not totally new but the new evidences presented here are convincing. The use of a climate models is useful as well, although it will have been even better to consider a few more models available within the PMIP3 database in order to clearly demonstrate that all the models missed the spatial structure presented here.

Response: Accepted.

Action: We now include in our analysis all 14 CMIP5/PMIP3 model simulations.

#1-2) The authors do not propose any dynamical mechanisms to explain such a feature, which is a bit disappointing (even an hypothesis) but this is maybe far from the scope of the study. Moreover, the fact that the pattern response in the mid-Holocene is

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quite similar in summer and winter in the data, with a latitudinal gradient could indicate a mean state change that concerns the whole year, a jet stream shift for instance or an oceanic circulation changes. Maybe the authors can discuss this type of alternative hypotheses or explain why NAO and SCAND variability may have changed.

Response: As the reviewer already suggests, we think that a discussion of possible explanations is beyond the scope of this particular paper.

Whilst the pattern of anomalies appears similar in winter and summer (warming over the north, cooling to the south), a single explanation for both seasons may not be sufficient. For instance, the atmospheric dynamics explanation would suggest a strengthened westerly circulation in winter (stronger zonal flow), but a weakened circulation in summer (weaker zonal flow with blocking). The two may be related, but we think it is too much to discuss this in the paper. One possibility is that the stronger zonal flow in winter delivers more moisture into the continental interior, which is then recycled into clouds in summer, which reduces radiative heating and the strength of the summer low pressure. These issues are perhaps better addressed by the modeling community.

Action: None

#1-3) I rather think this a question of mean state change. The fact that it resembles present-day signature of variability mode is not such a clear proof of a change in atmospheric circulation variability.

Response: The data and the model simulations represent the mid-Holocene temperature/precipitation anomaly averaged over many hundreds of years, and do not represent inter-annual variability or extremes. Pollen data reflects a vegetation assemblage composed of individual plants that include trees up to hundreds of years old. The main driver of vegetation change as seen through the pollen record is the mean climate state, not its variability.

Similarly, model runs represent the average of model simulations run over several

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decades to centuries. We do not make any statement concerning changes in AO or SCAND variability. The figure showing the modern relationship between temperature and precipitation over Europe and the AO and SCAND is based on correlation over 50 years, and is not a measurement of variability.

In considering whether the pattern of mid-Holocene anomalies we present is caused by changes in atmospheric dynamics, it is important to note that our results are supported by multiple lines of evidence from other authors (changes in Atlantic ocean currents, driftwood occurrence in the Arctic, Norwegian glaciers, Baltic salinity etc). Our interpretation is not based solely on the temperature and precipitation anomaly patterns that we describe, but rather is consistent with these other indicators of changing atmospheric circulation.

Action: None

Nevertheless, I think the paper can be published almost as it stands and I only have a few suggestions and questions that could help to further improve and clarify this manuscript.

Specific comments

#1-4) - p. 5572 second paragraph: I think that the authors should be more specific here in their terminology. What they are discussing is differences in the mean state between mid-Holocene and present climate. These differences can be compared to signature of present-day mode of variability. Nevertheless, since the changes reconstructed concern the mean state, they should use the term “NAO+ like differences”. I think it is important to make the difference between mean state changes and modes of variability. For instance the authors could rather say l. 18-19: “Climate model simulations in contrast show little change in the mean state atmospheric circulation as well as in the NAO/AO variability for the mid-Holocene climatic conditions (Gladstone et al. 2005. . .)”

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Response: See response to #1-3.

Action: The sentence has been modified in accordance with the reviewer's recommendations.

#1-5)- p. 5575, top: Maybe here the authors can considered to better depicts what is the exact definition of each index used.

Response: Accepted.

Action: More detailed definition of the AO/NAO and SCAND have now been included in the methods section.

#1-6)- P. 5577-5578: the section 4.2 (winter) starts with a description of the NAO, while it was not the case in section 4.1 for the SCAND. I suggest putting a whole description of what is known on NAO and SCAND (including a precise definition) somewhere, maybe in the Methods. Start of section 4.3 is also a repetition of the top of page 5572.

Response: Accepted.

Action: As well as clearer definitions (see reviewers comment #1-5), a more extended description of the SCAND, comparable to that of the AO/NAO, is now included in the methods section.

#1-7)- P. 5579, l. 25: the authors only show one model here and should not use the plural for "climate model"

Response: No longer valid since we have replaced HadCM3 with 14 CMIP5 models.

Action: We now compare the response of 14 CMIP5/PMIP3 models (see response to comment #1-1).

Technical corrections

#1-8) - p. 5571, l. 2: replace "simulating" by "to simulate"

Response: Accepted.

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Action: The text has been changed accordingly from “simulating” to “to simulate”.

#1-9) - p. 5571, l. 19: replace “aim to evaluate” by aim at evaluating”

Response: Accepted.

Action: The text has been changed accordingly from “aim to evaluate” to “aim at evaluating”.

#1-10) - p. 5574, l. 27: replacing “although” by “since” seems to be more logical in this sentence, isn’t it?

Response: No longer valid since we have replaced HadCM3 with 14 CMIP5 models and the section describing HadCM3 has been removed.

Action: This sentence has been removed.

#1-11) - p. 5576, l. 20 replace “climate model sensitivity” by direct thermal response” and delete “thermal” in the next sentence. Indeed climate sensitivity has usually a very specific definition (warming for a doubling of CO2)

Response: Accepted.

Action: The text has been changed accordingly, with “climate model sensitivity” replaced by “direct thermal response” and “thermal” deleted from the next sentence.

#1-12) - p. 5580, l. 24: replace “consistent with” by “similar to the signature of” and add “for present-day conditions” at the end of the sentence to be precise enough.

Response: The referee suggests replacing “consistent with” using the phrase “similar to the signature of”. We believe that this would dilute the message to the reader. We use the term “consistent with” because other authors have proposed a positive AO/NAO during the MH based on multiple lines of evidence, and out findings are consistent with this. We can however clarify the end of the sentence to indicate “for present day conditions”

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Action: The text “consistent with a positive or high index AO/NAO” has been modified to read “consistent with a present-day positive or high index AO/NAO”

Reviewer #2

Review on “The influence of atmospheric circulation on the mid-Holocene climate of Europe: a data-model comparison” by A. Mauri et al. This study provides an extended spatial reconstruction of the mid-Holocene temperature and precipitation over Europe for winter and summer. Moreover, they compared this updated data set with climate simulations based on the Hadley Center coupled ocean-atmosphere model (HadCM3). The presentation of the work is mostly clear, the paper is written in a clear language, the data set and the model experiment design are properly explained and the figures (overall) are easy to understand the quality of some figures could be improved). Nevertheless, I think the authors have treated this manuscript in a rather simplistic way and they did not put much effort in putting updated data set in a better light. To be more precise I have some major points (see my comments below) that I think should be addressed before this manuscript can be accepted to be published din CP. In summary, the manuscript needs to be significantly revised and improved, and carefully proof-read, before it should be submitted again for considerations of publication

Major points

#2-1) - 1. Pollen data set – Spatial coverage The authors should discuss in more details the influence of the spatial coverage. Central Europe has more data sets comparing to the eastern part. How do these differences in the spatial coverage affect the final data set?

Response: Accepted.

Action: Maps of uncertainties have now been included for the reconstruction (Fig. 2), and a sentence added to the uncertainties paragraph in the methods that points out the areas of high and low uncertainty.

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#2-2) 2. Pollen data set – uncertainties The authors should discuss the robustness and uncertainties of the reconstructions. To be more specific, the authors should provide more information about how many samples has each grid point, what was the minimum number of samples that was used as a criteria for taking into account a grid point and what are the time windows (e.g. 5.5 – 6.5 kyr BP or 6.5 – 7.5 kyr BP) of the samples.

Response: The issue of uncertainties and the robustness of the reconstruction is dealt with more comprehensively in a companion paper (Mauri et al. in prep. QSR). This includes comparisons with independent published Holocene reconstructions based on alternative proxies and training sets.

Providing information about the number of samples in each grid point would not necessarily be useful to the reader since this is only one aspect of uncertainty. As explained in the methods, the uncertainty for each grid box was calculated based on the standard error of the samples in each grid box (a function of the uncertainty of the reconstruction for each sample, and the number of samples), as well as the standard error associated with the interpolation (grid boxes with no samples and more distant from boxes with samples therefore have more error). We therefore believe it would be better to illustrate the uncertainty of the reconstruction using maps of this combined uncertainty estimate.

With reference to the issue of 'time windows', we explain in the methods that we use the same 4-D interpolation technique to arrive at the 6000 BP time-slice as used by Davis et al. (2003) (see p5574, para 1). This is different to the traditional concept of a 'time-window' to which the reviewer refers, and is designed to eliminate as much as possible the temporal blurring which occurs with the time-window approach. The method is described fully in Davis et al. (2003), but we can include some additional text to clarify this point.

Action: 1) Maps of uncertainty are now provided as supplementary information. 2) A sentence has been added in the methods section to clarify how the 6000 BP time slice

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was arrived at using the 4-d interpolation method, and how this differs from alternative methods.

#2-3) 3. Pollen data set – standard deviation I think to better improve the manuscript the authors should show a figure for precipitation and one for temperature with the standard deviation of the reconstructions.

Response: Accepted.

Action: Maps of uncertainties are now provided in the new figure 2.

#2-4) 4. Comparison with other data sets Even it might sound trivial, I think showing a figure in which the authors compare their data (in terms of climatology) with the data from Davis et al. (2003) and Cheddadi et al. (1997) would give the manuscript more motivational meaning.

Response: Even if the data from Cheddadi et al (1997) could be obtained, we are not sure that showing a comparison with previous reconstructions would be meaningful. The results would show similarities and differences between the reconstructions, but the meaning of these differences would be difficult to interpret in detail since the methods and datasets are not identical. Certainly, you could not use this purely comparative approach to say that one reconstruction was 'better' or 'more accurate' than another. It is however possible to point out deficiencies or errors in the data and training sets used to compile these reconstructions, and is these that we refer to in the text.

Action: None

#2-5) 5. Model choice The use of a climate models in comparison reconstructed data is very useful. But employing just one particular model does not necessarily mean the all the models are missing the spatial structure capture by the data. The authors did not treat this aspect very carefully and this is one of the major drawbacks of this manuscript. Either they use more models (PMIP3 data base) or they do not take into account a comparison between their data and model data. The comparison with just one model

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data does not necessarily imply that the paper deals with model-data comparison, as implied in the title of the manuscript. I think the use of HadCM3 was more like cherry picking to prove that their data are much better compared to model simulations.

Response: We always find it interesting that to publish the results of a climate model then only one model is sufficient, but to falsify the results of a climate model then one needs to do so for many climate models. Of course there are many papers that undertake "data-model comparison" using just one model, and the term is not commonly restricted to multiple model comparisons as the referee suggests. HadCM3 was chosen because it is one of the very few GCMs that have been run for the whole Holocene and the MH results that we presented were part of this Holocene scale study.

Action: We now compare the data with the ensemble mean and suite of 14 CMIP5/PMIP3 model simulations for the mid-Holocene. The conclusions of the paper are unchanged.

#2-6) 6. Novelty of the results. Although I do not contest the quality of the data, at the end it is just an updated version of previous published data sets. Just stating that this data sets includes more sites compared to previous reconstruction, does not necessarily mean that this data set is better or show completely different patterns. The authors should put more work in showing why this data set needs to be published.

Response: The reasons presented in the paper for the substantial improvement in the dataset compared to previous reconstructions are stated in the introduction and methods sections, and extend beyond a simple increase in the number of sites as suggested by the referee.

These include 1) the elimination of sites with poor or non-existent absolute dating used extensively in many previous studies, 2) the enormous increase in the size of the modern pollen calibration dataset compared to earlier studies (doubled or quadrupled), 3) recent quality control improvements in both the fossil (Fyfe et al, 2009, Giesecke et al. 2013) and modern (Davis et al. 2013) pollen databases that have eliminated many

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non-trivial data and metadata errors that were inadvertently included in previous studies.

These improvements are in addition to the 50% increase in the number of fossil sites/samples compared to the largest previous study (Davis et al 2003). We believe that in the minds of most readers, if not the referee, these substantial improvements in the chronological control, spatial distribution, data and metadata fidelity and sheer quantity of data can be reasonably expected to lead to an overall improvement in any resulting reconstruction compared to previous reconstructions.

Action: None

Minor points and suggestions

#2-7) 1. Figures 2 and 3 - The correlation map between the modern climate and SCAND and AO. These figures should be treated separately. Moreover, I think showing a map with the precipitation and temperature climatology for the modern climate next to the correlation maps will improve the scientific quality of the paper.

Response: Accepted.

Action: An additional figure showing the temperature and precipitation anomalies associated with the +SCAND and +AO have been added as new figure 3.

#2-8) 2. The authors should show also the significance of the correlation coefficients. They should hatch the areas where the correlation is significant.

Response: Accepted.

Action: The significant areas have been highlighted with a hatched pattern in figures 4 and 7.

#2-9) 3. In there any particular reasons why the authors focused on the Scandinavian pattern in summer? It is a little odd that the East Atlantic pattern is not considered as a candidate explanatory circulation pattern, as the second ranked northern hemi-

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sphere teleconnection pattern in terms of variability explained in the study of Barnston & Livezey (1987).

Response: The SCAND was chosen because it provides the closest analogue to the pattern of mid-Holocene summer anomalies. The NOAA website provides a useful summary of the different teleconnections and their associated temperature anomalies (http://www.cpc.ncep.noaa.gov/data/teledoc/).

The importance of the East Atlantic (EA) pattern attributed by Barnston & Livezey (1987) is partly a function of the nature of their study, which focused primarily on the winter season when the atmospheric circulation is strongest. The pattern of surface temperature anomalies in summer associated with the EA is very different from that resulting from the SCAND, and does not cause the same warming over Scandinavia, but instead results in warming over Southern Europe. The EA therefore does not provide a good analogue for mid-Holocene summer temperature anomalies.

Action: The choice of the SCAND has been clarified in the methods section.

#2-10) 4. For the paper to be publishable I would suggest to the authors to adopt a different structure: a) Make it a model-data comparison paper, in which they should include all the models available (see PMIP3 database) or b) make it just a data paper, in which the authors should show more results regarding their data set and why this data set is better than the other reconstructions available.

Response: The focus of the paper as a data-model study has been improved by increasing the number of models to include all 14 of the available CMIP5/PMIP3 models.

Action: See response for comment #1-1.

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