

Interactive comment on “The climate of the Mediterranean basin during the Holocene from terrestrial and marine pollen records: A model/data comparison” by Odile Peyron et al.

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Paper: The climate of the Mediterranean basin during the Holocene from terrestrial and marine pollen records: A model/data comparison

By Odile Peyron et al

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Reply to the reviewers' comments An important point came out in Review 2 (point 15), concerning the use of the preindustrial baseline. In the model simulations, we have always used PREIND as the baseline because the climate forcing before then, over the Holocene, is mostly orbital; in contrast to the industrial period where it is mostly-

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greenhouse gas. It does, however, have some impact on the precipitation signals we are discussing here (new figure 4). Therefore, in this revised version, we have changed our model-data synthesis (Fig. 3) and have taken present day in the control run instead of preindustrial to be in better agreement with the pollen data (the pollen data precipitation is best seen as 'anomalies relative to present day 1960-1990').

First reviewer 1) Quite often, the authors conclude that their analysis confirms previous studies, but it is not easy to identify what are the new conclusions, or what new information the data or the analysis is providing beyond of what is already new. Text in the Introduction part has been changed to: "The first originality of our approach is that we estimate the magnitude of precipitation changes and reconstruct climatic trends across the Mediterranean using both terrestrial and marine high-resolution pollen records. The signal reconstructed is then more regional than in the studies based on terrestrial records alone. Moreover, this study aims to reconstruct precipitations patterns for the Mediterranean basin over two key periods in the Holocene, while the existing large-scale quantitative paleoclimate reconstructions for the Holocene are often limited to the mid-Holocene - 6000 yrs BP- (Cheddadi et al., 1997; Bartlein et al., 2011; Mauri et al., 2014), except the climate reconstruction for Europe proposed by the study of Mauri et al. (2015). The second originality of our approach is that we propose a data/model comparison based on: (1) two time-slices and not only the mid-Holocene, a standard benchmark time period for this kind of data-model comparison; (2) a high resolution regional model (RCM) which provides a better representation of local/regional processes and helps to better simulate the localized, "patchy", impacts of Holocene climate change, when compared to coarser global GCMs (e.g. Mauri et al., 2014); (3) changes in seasonality, particularly changes in summer atmospheric circulation which have not been widely investigated (Brayshaw et al., 2011)."

2) In the model set-up used by the authors there are some open questions. For instance, they use a slab ocean that ignores the ocean dynamics, but are the simulated sea-surface temperatures comparable to the temperatures simulated in global couple

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simulations for the mid-Holocene? what could be the role of the dynamics of the North Atlantic in determining the precipitation patterns in Europe? I am aware that a full coupled simulation over the Holocene could be out of the scope of the present study in terms of computer resources, but some type of validation or discussion of the possible shortcoming of the simulation set-up should be addressed. More importantly, I think, would be to identify which aspects of the regional modelling provide an added value relative to the global model results presented in of Mauri et al. . The manuscript includes just a comment in passing about the heterogeneity of simulated precipitation changes in the Balkans, but this is not really followed through. For instance, one of the mechanisms that may explain the pattern or precipitation changes are shifts in the North Atlantic storm tracks. Is the regional model able to represent the storm tracks more realistically than the global models ? Is the representation of present-day precipitation better in the regional model than in the ensemble of CMIP5 global models ? I would assume the answer is yes, but it would be nice to see it discussed in the manuscript as well. On the other hand, the slab ocean is likely not able to realistically represent the meridional sea-surface temperatures in the Atlantic. This may affect the intensity and extent of the African Monsoon and its changes over the Holocene. Could this limitation influence the simulation of the summer precipitation changes in the Mediterranean?

This comment raises several important issues, which we attempt to disentangle as follows.

We agree with the reviewer that there are indeed limitations in the climate modeling approach used here. These were discussed at length in previous publications, as cited from the present article. Brayshaw et al., 2010 (Phil Trans A), in particular, goes into detail on (A) evaluating the relative merits and difficulties of the modeling approach compared to others (such as PMIP), including the role of embedded high-resolution regional modelling and (B) discussing the physical atmospheric drivers of winter-time change such as storm tracks, Hadley cell expansion/contraction and teleconnections from the Indian Ocean. Brayshaw et al., 2011 (Holocene) provides a wider review

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(both summer and winter), discussing of the impact of the GCM's simulation of tropical Atlantic SSTs (on, e.g., the summer expansion of the African monsoon) as suggested by the reviewer.

It is beyond the scope of the present paper to revisit and significantly expand this dynamical/modelling discussion: the project from which the climate model simulations are taken finished about five years ago. The opportunity in the present work is simply to re-use these GCM/RCM simulations – acknowledging their well-documented behaviours and limitations – to compare against a new regional synthesis of palaeo-observations (the paper should therefore be seen as ‘paleo-data led’ rather than ‘modelling led’ in terms of the conclusions it reaches). We believe this to be a reasonable approach to take as, in the absence of the resources to conduct new climate model experiments, the climate simulations used here remain the only published attempt at producing a high-resolution regional simulation of the Mediterranean with time-slices across the whole Holocene period. Insofar as the impact of specific local climate features is important (e.g., complex topography and coastlines), they remain the only dataset available for doing this level of detailed model-data inter-comparison in the region. We also note that the Brayshaw et al. (2010) paper compares the GCM results to other modelling work (PMIP) and palaeo-climate reconstructions available at the time (e.g., Brewer, Rimbu, etc) and, on balance of evidence, cautiously suggests an NAO-negative like state in the mid-Holocene (we would actually prefer to refer to a southerly shift in the North Atlantic storm track rather than the NAO). This stands in contrast to the more recent Mauri et al. publication (which makes no reference to these earlier publications).

We therefore seek to take on board the reviewer's concerns about the framing of the paper and its contextualization principally by improving the text in Section 2: • Making it clearer that this is a ‘re-use’ of an existing model dataset; • Explicitly stating that the ocean dynamics are assumed to be invariant over time (strictly we ‘fix’ the oceanic fluxes of heat, as already noted in the paper, though we recognize that the implications of this may not be immediately recognized by all readers); • Refer more

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explicitly to the detailed analysis provided in previous work on this dataset (e.g. for changes in atmospheric circulation and comparison/justification of the modelling approach, e.g. compared to PMIP); – Emphasises the nature of the ‘added value’ of regional downscaling (i.e. the resolution of local impacts such as complex topography).

3) Conclusions: The conclusions could be presented in a more clear way. The last paragraph in the conclusions looks also quite convoluted, and some of the conclusions are not really based on the results presented here. For instance, the authors conclude that the regional model represents better the atmospheric dynamics, and therefore precipitation. This can be somehow expected, but it has not been shown in this study, and in particular, it has not been shown that the particular model set-up used here (with a slab ocean) is indeed better. We agree with the reviewer that this paragraph was unclear and perhaps a ‘too general’ conclusion to be drawn from the evidence presented. We have therefore clarified the text. The key issue we wish to highlight is that the RCM output provides a better representation of local/regional processes. Notwithstanding the difficulties of correctly modeling large-scale climate change over the Holocene (with GCMs), we believe that regional downscaling may still be valuable in facilitating model-data comparison in regions/locations known to be strongly influenced by local effects (e.g., complex topography).

4) The title is a bit misleading, as the study is basically about precipitation changes and not ‘climate’ in general. OK, corrected as follows: Precipitation changes in the Mediterranean basin during the Holocene from terrestrial and marine pollen records: a model/data comparison

5) Abstract has many elements of introduction, including recommendations, like the use of transient simulations, which turn out to be correct but that they are not really substantiated by the results described in this manuscript. My criticisms is to some extent a matter of taste, but I think that the abstract should be succinct and mainly describing the methods, results and conclusions. Introductory remarks should go in the introduction, and final speculations or recommendations, in the main text OK, cor-

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rected.

6) Lines 125-132 This is a repetition of a previous paragraph on the same page OK, corrected.

7) It may be interesting to know the time resolution of the proxy records Yes, I have added the time resolution in table 1 for the two periods selected and the entire sequence.

8) line 239 I think that the reference to Mauri et al (2015) is not correct. The reference Mauri, et al. (2015) is correct.

9) line 242 Mauri et al used a reconstruction method based on plant functional types. Should the reader expect differences to the reconstruction method used here ? Could some of the differences to the present results due to the different methodology ? Mauri et al. use the MAT with the plant functional type scores instead of the pollen assemblages; we use the MAT with the pollen assemblages; so yes, it can produce different results because different methods can produce different results (Brewer et al., 2008, Peyron et al., 2013).

10) line 266 'Mediterranean, and dry conditions above 45_N during the early Holocene, while the opposite' North of 45N I do not understand what you mean exactly; therefore I have corrected the sentence as follows: Our reconstructions are in agreement with Mauri et al. (2015), with dry summer conditions above 45°N during the early Holocene and wet summer conditions over much of the south-central Mediterranean south of 45°N.

11) Caption Figure 3. which is the reference period to calculate the simulated precipitation anomalies? Anomalies are taken with respect to present-day control run. Caption updated.

Second reviewer 1) I feel that there is a missed opportunity in using the model output to understand the atmospheric drivers of the changes in spatial pattern. As noted in

the response to reviewers, it is beyond the scope of the present paper to discuss the atmospheric drivers at length beyond that presented in Brayshaw et al. 2010, 2011a, and 2011b. I would like to see the goals more clearly stated, and clearly referred to throughout the paper. See reviewer 1, point 1. The primary novelty in this work is the new paleo-observations synthesis and its comparison – at regional/local level – with the climate model data. The text has, however, been clarified to direct interested readers to those works.

2) I would like to see more discussion about the choices of spatial pattern and of time period; For the time period, I don't really understand why the authors did not look across the entire Holocene, but instead focused on two, quite long time periods. Are these gradients only a feature of the time periods chosen? What was the variation outside (or even within) these periods? Given that one of the papers they cite has already completed full Holocene reconstructions (Mauri et al., 2015), and that there is interest in full Holocene/Glacial transient GCM simulations, this snapshot approach appears to be somewhat limited. At the very least, it would be good to have a better justification for the choices than "to aid interpretability I have also looked at it in a continuous way for the Holocene (not provided here), but it will be the topic of another paper. Here we focus on spatial patterns, and we have chosen these periods because they are different enough to be simulated by the regional model (which is not transient). From a climate-modelling perspective, the rationale for the grouping of the time-slices is a practical one (as noted above, we are unable to perform additional experiments to extend the dataset at this time). As outlined at ~line243, the change in 'forcing' between adjacent time-slices is small and, as such, changes are difficult to detect robustly given the data available. Grouping the time-slices together into 'mid-Holocene' and 'late-Holocene' experiments therefore makes best use of the data available. The text in Section 2 (model description, ~line 243) has been modified to emphasise the rationale for this decision. The text has also been changed as follows: This study aims to reconstruct and evaluate N-S and W-E climate conditions for the Mediterranean basin, over two key periods in the Holocene, 8000-6000 cal yrs BP, corresponding to the "Holocene

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climate optimum” and 4000-2000 cal yrs BP corresponding to a trend toward more dry conditions.

3) The choice of precipitation as a variable for comparison also needs better justification. The authors state (line 416) that using precipitation instead of moisture indices may be why there is a model/data mismatch, and some form of moisture index has been proposed as a better quantity for pollen reconstructions elsewhere (Bartlein et al., 2011). Please note that the Bartlein et al. (2011) paper is a synthesis at a world scale of “old” pollen inferred climate reconstructions done for different regions (Europe...). No new reconstruction has been done in the Bartlein et al. (2011) paper. Given this, and that alpha is routinely reconstructed from pollen, why not use this instead?. We use precipitation because our aim was to compare with the model outputs. In the regional model, the reconstruction of the moisture index was not available. And if not, please justify the use of precipitation, given its limitation as a reconstructed variable The use of precipitation parameters (annual and seasonal) seems robust for the Mediterranean area (Mauri et al., 2015; Peyron et al., 2011, 2013, Magny et al., 2013) and precipitation reconstructions are particularly important for the Mediterranean region given that precipitation rather than temperature represents the dominant controlling factor on Mediterranean environmental system during the early to mid-Holocene (Renssen et al., 2012). Text has been modified as follows: “precipitation reconstructions are particularly important for the Mediterranean region given that precipitation rather than temperature represents the dominant controlling factor on Mediterranean environmental system during the early to mid-Holocene (Renssen et al., 2012)”. The use of precipitation parameters (annual and seasonal) seems robust for the Mediterranean area (Mauri et al., 2015; Peyron et al., 2011, 2013, Magny et al., 2013).”

4) Line 28. The abstract could be shortened and made more concise – there is some repetition (e.g. lines 39-40 and lines 60-61) OK, corrected, cf reviewer1.

5) Line 34 (and elsewhere). Is the pattern a gradient or dipole? These are not to my understanding the same thing, as one represents a trend, and the other represents

a pattern of two opposing centers. Please either use one or the other, or state more clearly which is being referred to at any time. OK, checked and corrected.

6) Line 38-40. What is the aim of the comparison? Changed as follows: “For the same time intervals, site-based pollen-inferred precipitation estimates were compared with an existing database from a regional-scale downscaling of a set of global climate-model simulations. The high-resolution detail achieved through the downscaling is found to assist with comparing ‘site-based’ paleo-observations with gridded model data, and the climate model outputs and pollen-inferred precipitation estimates show remarkably good overall correspondence (although many simulated patterns are of marginal statistical significance).”

7) Lines 47-51. This section needs some rewriting to make it clear when the authors are referring to conditions being drier in one region than another, or that the anomalies are drier compared to another time period. Corrected as: “During the early Holocene, relatively wet conditions occurred in the south-central and eastern Mediterranean region, while drier conditions prevailed from 45°N northwards. Then these patterns appear to reverse during the late Holocene, with similar to present day or slightly drier than present day conditions in the south-central, but more sites from the northern part of the Mediterranean basin are needed to further substantiate these observations.”

8) Line 61. In what sense is HadSM3 dynamic? (and what is HadSM3 as opposed to the other models shown here) This was an error. It has now been removed.

9) Lines 90-92. Needs a citation Magny et al. (2013) has been added.

10) Line 93. Which sites in N. Italy? Citation, please. Peyron et al. (2011, 2013) has been added.

11) Lines 126-127. Why these periods? Why are they ‘key’? Why not do this in a continuous way? Cf point 2, reviewer2.

12) Lines 133-134. “To critically assess the potential of the model setup: : .” This is a

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little fuzzy, but I assume that the goal is to discuss the regional climate model output, and the model parameters. However, I don't really feel that this was addressed in the discussion. There is some discussion of findings in other papers (e.g. Bosmans et al) but nothing about the setup used here. We agree that the text was unclear at this point. The limit to the scope of this paper is such that our main concern here is to compare the climate simulated by the models to that reconstructed from the observations. The text is therefore changed to: "... critically assess the consistency of the climate reconstructions revealed by these two complimentary routes."

13) Line 169. Arguably, pine is overrepresented in all sites. Why only exclude it for the marine sites? How big an impact does this have? The pollen signal recorded in marine cores reflects the regional vegetation across an area of several hundred square kilometers and pine pollen is particularly overrepresented (Heusser and Balsam, 1977; Dupont and Wyputta, 2003; Hooghiemstra et al., 1992, 2006). The reliability of the quantitative climate reconstruction from marine pollen spectra (with and without Pinus) has been tested using marine core-top samples from the Mediterranean in Combourieu-Nebout et al., 2009. Results shows that an adequate consistency between the present day observed and MAT estimations is shown for Psum and Pann values. In terrestrial pollen records, the signal is more local (depending of the size of the lake). Pinus is of course also overrepresented, but excluding it from the terrestrial assemblages doesn't make sense for the Holocene because pine can grow close to each site. We can exclude Pinus during glacial times, where we are sure it was exclusively long-distance transport.

Text has been modified as: The reliability of quantitative climate reconstructions from marine pollen records has been tested using marine core-top samples from the Mediterranean in Combourieu-Nebout et al. (2009), which shows an adequate consistency between the present day observed and MAT estimations for Pann and Psum values.

14) Line 187. I think I get what the authors are saying here, but given the increasing

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interest in transient simulations (e.g. Liu et al) and reconstructions (e.g. Marcotte et al), I'd like to see this choice justified a little better. As noted above, the modelling work is taking advantage of an existing model-output database (to our knowledge the only attempt that has been made thus far to simulate the regional climate of the Mediterranean across the whole period). This has now been clarified and readers are directed to appropriate previous publications for further discussion of the modelling framework.

15) Line 190. The model uses the pre-industrial period as a baseline for anomalies whereas I assume the pollen reconstructions use the late 20th century, although this is not specified. It includes both long-term averages (1961-90) and time series for rainfall. How much will this affect the offset between model and data. How big are the reconstructed anomalies relative to any change between these periods? We agree that it's an important point; changes in climate are now expressed as differences with respect to the present day control run. Text has been changed as follows: "In contrast to existing model simulations, changes in climate are expressed here as differences with respect to the present day (1960-1990) and not with respect to pre-industrial. We suggest it may be better to use 'present day' to be in closer agreement with the pollen data (modern samples) which use the late 20th century long-term averages (1961-1990). However, there are some quite substantial differences between model runs under 'present day' and 'preindustrial' forcings (Fig. 4)."

16) Line 206. The results are more single points in time, rather than climate trends. Changed 'trends' to 'patterns'.

17) Lines 217-219. This seems like it would be more appropriate in the introduction. OK, corrected.

18) Line 231. What scaling issues? Wu et al. undertook a reconstruction at a world scale: it's difficult to distinguish in their figures what happened exactly in the central Mediterranean to depict a possible north-south pattern.

19) Line 247. It is difficult to see visually how there is good corroboration between these

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results and Mauri et al. Would it be possible to carry out a one-to-one comparison of values, and test the differences? To carry out a one-to-one comparison of values, we need to have access to Mauri's data, which is not the case, therefore it was not possible to test the differences (furthermore this was not the topic/focus of this paper). Further – as both Mauri et al, and this study present statistical climate reconstructions from pollen data, it is hard to see how the agreement between them supports the robustness of the results. In contrast to Mauri et al., our study also performed climate reconstruction from marine pollen cores. Because the scale of our figures and those of Mauri et al. were different, we finally decided to remove the Mauri et al. reconstruction in the figure 2.

20) Line 351. How can you tell that the data-model agreement is good? Again, some point-by-point comparison would help (and help highlight where the main differences are) It is only visual. We agree that it is not the top, however we did not have time to produce metrics to compare simulations and reconstructions.

21) Line 435. How would the snowpack affect different methods? I can see that it might affect proxies differently, but not methods. Yes, we agree and it is corrected.

22) Line 486. It is hard to disagree with a call for higher resolution in climate models, but how exactly will this help? What processes will be better represented, allowing for better climate simulation? This text has been amended to better reflect the scope and content of the paper. In particular, while the authors do believe that high-resolution global models are likely to be part of the solution (e.g. improved representation of processes such as blocking, storms, teleconnections etc), this is not directly the subject of the paper. The revised discussion therefore now focuses on the potential role for regional high-resolution models in providing a better representation of complex terrain to reconcile site-specific model vs palaeo-obs discrepancies.

Please also note the supplement to this comment:

<http://www.clim-past-discuss.net/cp-2016-65/cp-2016-65-AC1-supplement.pdf>

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