

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.

Anonymous Referee #1

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The manuscript presents a new compilation of proxy data based on pollen assemblages in the Northern Mediterranean basin covering the Holocene period. The analysis is focused on reconstructed seasonal and total precipitation changes to ascertain the existence of a North-South and west-east dipoles. The results derived from the analysis of proxy data are compared with time-slice simulations with a regional atmosphere model driven at the boundaries by a global atmosphere model coupled to a slab ocean for different periods through the Holocene. My recommendation is to include revisions in the paper, which could be considered as between major and minor.

Although I do think that the topic of research is very interesting and this paper is a contribution to some of the research goals, I had the nagging feeling that the added

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value of the study is difficult to grasp. Quite often, the authors conclude that their analysis confirms previous studies, mainly by Mauri et al., Guiot et al, Roberts et al. and others, 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.

This applies also to the model set-up. Mauri et al already compared pollen-based reconstruction with model output for the mid-Holocene. They used a full model ensemble, albeit global models with coarser spatial resolution, in contrast to the present study that uses a regional model but with a slab ocean. 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 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 change 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 summer precipitation changes in the Mediterranean? All in all, the manuscript looks for these and similar

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reasons more descriptive than it should and could be.

The conclusions could be presented in a more clear way. After reading this section several times, it remains unclear to me whether the precipitation dipoles really exist. Some sentences clearly say yes, but they are immediately qualified with a 'however,' or 'but.' This is particularly apparent in the paragraph starting in line 464. 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.

The title is a bit misleading, as the study is basically about precipitation changes and not 'climate' in general.

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 criticism 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.

Lines 125-132 This is a repetition of a previous paragraph on the same page

It may be interesting to know the time resolution of the proxy records

line 239 I think that the reference to Mauri et al (2015) is not correct. First, year should be 2014 and not 2015. But, secondly, I think the authors are referring to another paper by Mauri (2015): The climate of Europe during the Holocene: a gridded pollen-based reconstruction and its multi-proxy evaluation. *Quaternary Science Reviews* 112 (2015) 109e127. The paper in climate of the past is an analysis of mid-Holocene conditions

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whereas the QSR paper is an analysis of the evolution throughout the Holocene.

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 be due to the different methodology?

line 266 'Mediterranean, and dry conditions above 45°N during the early Holocene, while the opposite' North of 45N

Caption Figure 3. which is the reference period to calculate the simulated precipitation anomalies?

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