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Climate of the Past Discussions

# *Interactive comment on* "A regional climate simulation over the Iberian Peninsula for the last millennium" *by* J. J. Gómez-Navarro et al.

**Anonymous Referee #1**

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Review of the discussion paper entitled "A regional climate simulation over the Iberian Peninsula for the last millennium" by J.J. Gómez-Navarro et al.

The authors describe a simulation covering the past millennium performed with a regional atmospheric model nested in a global coupled model. This was certainly a technical challenge to conduct this simulation and such a high resolution (30 km) numerical experiments offers plenty of possibilities, for instance regarding the model data comparison as climate proxies are influenced by local processes not necessarily related to large scale changes in a simple way. In this framework, the authors show in a very convincing way that, thanks to the high resolution of the atmospheric model, it is possible to improve the representation of the mean state, the variance and the dominant modes of variability over the Iberian Peninsula.



The high resolution model results are thus much closer to the high resolution observations obtained over the last decades than to the global model. However, to my point of view, the present version of the paper fails in showing the main advantages of this high resolution simulation over the last millennium. The agreement between the simulation at high resolution and reconstructions is not very good and there is no indication that this agreement is better than in the global model. The link between NAO and precipitation is described but it would have been instructive to explain if this is only a large-scale process included in the global model or if the regional model brings additional information. I might admit that the reconstructions themselves may not be well adapted to investigate this problem because of the small number of proxies in the Iberian Peninsula used in those reconstructions. In that case, the authors should at least demonstrate that in the model a different behavior can be found in different regions over the last millennium. This could then justify simulations at high resolution when enough proxies are available or to perform process studies.

Nevertheless, this is not clearly done in the present version of the paper. The reader may even have contrasted feelings, for instance page 2088 line 6 it is stated that "However SAT evolution is not so heterogeneous". Does it mean that, according to the results, SAT information at large scale is enough when interested in temperature changes during the last millennium and thus high resolution simulations are not necessary? If this is valid, this is good news for global models. Besides, from Fig.11, it seems that in summer the response in the center of Spain is clearly different from the large-scale or the coastal ones. To my point of view, quantifying clearly the regional differences (and thus the uncertainties in global models when compared to local proxies) would be very useful, even if no reconstruction is available to confirm the high resolution results. This could be done, for instance, by giving the variance of the difference between the temperature in different regions over the past millennium, or the variance of the difference between one region and the mean over the peninsula. Various time filters could also be applied to the time series in order to check if the differences are mainly seen for high frequency variations (that could be estimated from recent observations) or also at lower frequency.

In summary, I think that before publication the authors must be much clearer and much more precise to explain the advantages of a high resolution simulation. If for some variables such a high resolution simulation is useless this should also be stated explicitly as it is an interesting message too.

I have made some additional suggestions about specific points below.

1/ Page 2073 line 22. Model-data comparisons over the past millennium have not been limited to the global and hemispheric scales and several studies have been devoted to the spatial pattern of the changes at least at continental scale.

2/ Page 2074, line 3. The reference is Yoshimori et al. 2005. The same typo is present in the reference list.

3/ Page 2076. It would be instructive to compare the forcing applied with more recent estimates as for instance described in Schmidt et al. 2010 (Climate forcing reconstructions for use in PMIP simulations of the last millennium (v1.0).G. A. Schmidt, J. H. Jungclaus, C. M. Ammann, E. Bard, P. Braconnot, T. J. Crowley, G. Delaygue, F. Joos, N. A. Krivova, R. Muscheler, B. L. Otto-Bliesner, J. Pongratz, D. T. Shindell, S. K. Solanki, F. Steinhilber, and L. E. A. Vieira Geosci. Model Dev. Discuss., 3, 1549-1586, 2010). A brief discussion of the impacts of the choice of the forcing would also be useful.

4/ Page 2076. Does the forcing include land-use changes that may have a strong impact in Europe, in particular at the regional scale?

5/ Page 2080. It is argued that a part of the discrepancy between the results of the ECHOG-G model and the reanalyses might be due to the fact that "the main circulation modes in the model may not be simultaneous with the actual climate". I suspect that this contribution is very weak compared to the systematic biases of the model. It would thus be useful to analyze different 30-year periods, in a control simulation of ECHO-G,



to estimate how a 30-year period is representative of the mean state of the system and how the variability between different 30-year periods compares with the difference between ECHO-G and the reanalyses. (Same remark for the lines 9-10, page 2094.)

6/ Page 2088. If the Maunder minimum does not display any clear signal in winter maybe it would also be interesting to analyze in Fig.11 another cold period to see if the agreement between model and proxy records is better.

7/ Page 2089, lines 1-2. I do not understand the sentence “In winter this relationship is weaker due to the positive tendency of both the precipitation and SAT”. If both variables have a tendency, I expect that this would lead to a correlation between them?

8/ Page 2090, line 24. Stating that "SAT winter series show similar variability in the model and reconstruction" is a bit optimistic to my point of view. In particular as it is said at the end of the page that "Overall the agreement between model and reconstructions in the cold periods is not good".

9/ Page 2091, lines 4-5. What is meant by "the intensity of coldness"?

10/ page 2092, line 19. The anti-correlation should be quantified here and compared with the one obtained from recent observations.

11/ Page 2095. Again, I consider that it is a quite optimistic to state that "variability is similar" between model results and reconstructions.

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