

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

Anonymous Referee #3

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GENERAL COMMENTS

The authors present a dynamical downscaling - of a global palaeoclimate simulation - focusing on the Iberian peninsula, with the aim of investigating whether such a regionalisation enables better comparison with climate proxy data. The capability of the chosen regional climate model (RCM) for the recent past climate conditions is assessed not least by means of a global reanalysis driven RCM simulation.

Regionalisation of palaeoclimate simulations has potential for climate model evaluation that adds to evaluation done with instrumental observations, but as well for better characterisation of past climates and thus understanding the climate system. is a rare feat, for which the authors are commended. There are, however, few additional studies that could be quoted, such as:

- G. Strandberg, J. Brandefelt , E. Kjellström and B. Smith, 2011, High resolution regional simulation of last glacial maximum climate in Europe, Tellus 63A, DOI: 10.1111/j.1600-0870.2010.00485.x.

- E. Kjellström, J. Brandefelt , J.-O. Näslund, B. Smith, G. Strandberg, A.H.L. Voelker and B. Wohlfarth, 2010, Simulated climate conditions in Europe during the Marine Isotope Stage 3 stadial, Boreas, 10.1111/j.1502-3885.2010.00143.x. ISSN 0300-9483.

- Moberg, A., Gouirand, I., Schoning, K., Wohlfarth, B., Kjellström, Rummukainen, M., de Jong, R., Linderholm, H. and Zorita, E. 2006. Climate in Sweden during the past millennium - Evidence from proxy data, instrumental data and model simulations. Technical Report TR-06-35. Swedish Nuclear Fuel and Waste Management Co (SKB), CM Digitaltryck AB, Bromma, Sweden. (ISSN 1404-0344), 87 pp. (see <http://www.skb.se/upload/publications/pdf/TR-06-35webb.pdf>)

The underlying idea in the paper is sound, i.e. using regionalisation for enabling more detailed comparisons between climate model simulations and proxy data, given the demonstrations of added value of regional climate models in general. The presentation of the results is, still, to a large degree descriptive. In various occasions, some possible reason for what is seen in the data is postulated, but often in quite loose terms (e.g. page 2081, lines 7-8; page 2083, lines 13-16, to quote two examples). The NAO-related considerations are worthwhile.

Discussion in the paper on model evaluation under the recent climate is quite extensive compared to the proxy comparisons that should be the intended heart of the paper. More muscle could be added to the latter, if this is possible with the available proxy data.

The authors refer quite sparsely to the rather extensive RCM literature. Quite a few of the findings in the paper could reasonably be set into a more general perspective by such comparisons. One starting point could be:

- Rummukainen, M., 2010, State of the art with regional climate models, WIRE (Wiley Interdisciplinary Reviews: Climate Change) Advanced Review 1:1, 82-96.

The authors' results in a sense challenge the notion of utility of testing climate models for past conditions (page 2073, lines 11-15). Availability of proxy data is often scarce and the data are of unknown quality. Indeed, the authors would seem to argue for this (cf. page 2093, lines 17-20 & page 2095, lines 16-17 that seem to argue for limitations with proxy data, as well as Figures 11-12) but at the same time argue that differences between proxy data and climate models witness to deficiencies in the latter (e.g., page 2095, lines 24-26). This should be clarified.

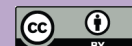
Indeed, it does not become wholly apparent what the authors' conclusion is on whether or not the underlining idea applies or not, i.e., "whether regionalisation enables better comparison with climate proxy data".

Another possibly interesting result, albeit outside the aim of the article, could be that natural forcing has not been so influential in the past as the more recent anthropogenic forcing (cf. Figure 13). One could ask how much control does natural forcing exert on regional scale compared to internal climate system variability. . .

Overall, the article would seem to need rewriting for ensuring clarity and for ensuring that the authors' main points are brought forth. Some general suggestions, to add to the discussion above, are:

- condensation of the ECHO-G-MM5 and ERA40-MM5 comparison (incl. fewer figures). Also, one could refer more explicitly to the stated analysis of SAT by Jerez et al. mentioned on pages 2078-2079. What did they find? Do the SAT-related analyses here complement their findings?

- added (or clearer) quantification of model-to-model and model-to-data (obs, proxy) differences and similarities. A difference does not necessarily bear witness to disagreement. . .

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- more extensive discussion on what the findings are viz. the benefit of downscaling in model – proxy data comparisons. One could, for example, reflect over the co-use of climate modelling and proxy data in model evaluation and understanding the past climate conditions (models add value to proxy analyses)

SPECIFIC COMMENTS

The last sentence in the Abstract is uninformative. Suggest writing explicitly on the main agreements and main disagreements instead.

Page 2072, line 25: also on different “spatial” resolution! But also, different from what? From each other or from climate models?

Page 2074, line 11: What is meant by “improved”? How are RCM physical parameterisations improved over those in a GCM? This pertains also to page 2095, line 2.

Ibid, line 14: Jacob et al. discuss PRUDENCE, not ENSEMBLES.

Ibid, line 19-20: Given that ECHO-G does not take into account anthropogenic aerosols, one has to be careful in considering the latter half of the 20th Century in the simulations. This probably pertains also to what the authors note on page 2090, lines 24-26.

Ibid, lines (ca) 20-30: Scarceness of proxy data (which is acknowledged for the region) should present a major obstacle for pursuing this kind of study. So, the IB is overall perhaps not a “good test bed” as is mentioned.

Page 2076, line 26: is “two-way nested” (the RCM returning information to the driving GCM while the latter is running?) or “double-nested” meant here?

Page 2077, lines 19-20. Haylock et al.’s paper describes the gridded dataset that is a product of the ENSEMBLES project. ECA&D data was a starting point for preparing the new gridded dataset. This pertains also to page 2091, line 25.

Ibid, line 24: Jacob et al. discuss PRUDENCE, not ENSEMBLES.

Page 2079, lines 14-15: “is”, rather than “can be considered, in general”. The methodology is miles apart from a GCM simulation.

Page 2080, 16-18: Given a free-running GCM, it is much more likely than not that the transient evolution of circulation modes differs from the observed climate. The sentence seem overly “cautious”. The same applies to “should in theory”, not least as ERA-40 has been subjected to considerable evaluation and its successes and deficiencies are documented.

Page 2081, line 8: “to the similar spatial resolution of both” seems quite unclear (one may also recall that the same resolution is not the original resolution in both sets).

Ibid, lines 27-28: “better” than what? To ECHO-G? To ERA40-MM5?

Page 2082, line 8-11: are ECHAM5 and ECHAM4 similar in this respect? Reference?

Page 2083, lines 13-16 (bit like Page 2080, 16-18): The paper does not seem to provide information on the quality of ECHO-G in simulating weather types and suchlike, apart the comment on the westerlies (page 2082, lines 5-7). Statements postulating on such deficiencies as possible explanations for biases with respect to observed climate could be supported by appropriate analysis of ECHO-G (which probably has been made somewhere?).

Page 2084, line 10-: It would seem sufficient to only provide the Tables 1-2 and omit the corresponding figures. The information in the latter is rather comprehensively in the running text already.

Page 2085: One could also consider omitting the EOF-figures. They do not seem to add much to the information already provided in the text.

Ibid, lines 20-25: The “interesting to note” and “not surprising” do not seem to fit together, i.e., why is not surprising = interesting?

Page 2087, lines 7-8: Does this mean that ocean areas (in the model) are compared

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to land areas (in the proxies)? Does this not imply systematic differences?

Ibid, lines 15-19: It seems that the MWP period is here given as 1200-1850, but as 1100-1400 on page 2093, line 12. The difference is perhaps to some extent explained by the apparent different periods of the MWP in different regions.

Page 2090, lines 10-12: A relevant paper to quote in this context would be Déqué et al. 2005, *Climate Dynamics* 25, 653-670.

Page 2091...: The wording seems to suggest that the proxy data are “correct” in terms of seasonal precipitation, and the model “wrong”. If “overestimated” and “underestimated” are meant in the sense of “compared to the proxy data”, perhaps this could be specified. Indeed, the previous paragraph is worded in such a more “objective” style.

TECHNICAL COMMENTS

Page 2074, line 8: RCM stands for Regional Climate Model, not ... Circulation. ...

Page 2076, line 16: N₂O is “nitrous oxide”, not “nitrogen dioxide”.

Ibid, line 19: this probably refers to CO₂, CH₄ and N₂O, so “both” is not a good choice of a word.

Figure 1: The units for N₂O should be ppb.

Figure 6: The interpolation to a 7 min grid would seem to risk producing features that are not supported by the original data. Please reconsider.

Interactive comment on *Clim. Past Discuss.*, 6, 2071, 2010.

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