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Climate of the Past Discussions

Interactive comment on "Dynamical downscaling of the western North Pacific from CCSM4 simulations during the last glacial maximum and late 20th century using the WRF model: model configuration and validation" *by* J. Yoo and J. Galewsky

Anonymous Referee #1

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General comments

This paper deals with dynamical downscaling of the coupled atmosphere-ocean general circulation model CCSM4 outputs with WRF, on the western North Pacific, both for modern and Last Glacial Maximum climates. I am sorry to say that I have 3 general comments which, to my opinion, strongly prevent the publication of this paper in Climate of the Past.

First, the modelling work is, to my opinion, questionable because the authors integrate only on 10 years, which is much too small to be representative of a climatological mean (typically at least 30 to 50 yrs for atmospheric variables, and 50-100 yrs for oceanic variables). Additionally, for the LGM, they use CCSM4 outputs which are not at equilibrium for the LGM as boundary conditions for WRF (see specific comment below).

The second comment is the lack of a clear scientific question. What is the scientific question underlying your work ? It is not explained. The authors state page 15 that : "the goal of this study is to investigate the behavior of large-scale dynamic and thermodynamic variables in the downscaling experiments over the western North Pacific under the LGM and modern climates". This is not a scientific question, rather it is a mean to achieve something, but we don't know what they want to achieve. For example, why do you choose this area in particular (and not, let's say, the eastern tropical Pacific) ? Why do you choose Last Glacial Maximum and modern (and not, let's say, mid-Holocene)? I am sure you have good reasons to do this, but they are not mentioned. You should start by clarifying the scientific question.

The third important comment is that you cannot say that you validate your model if you do not confront it to observations and paleo-reconstructions. You cannot state in the abstract that "the WRF model corrects biases of the GCM, producing more realistic spatial distributions of the pressure-level variables" if you do not compare your WRF outputs to observations (for 20th century simulation) and paleo-reconstructions of temperature, precipitation and SSTs for the LGM simulation. Therefore, the goal of your paper, which is to validate the WRF model on the western North Pacific, is, to my opinion, missed. Calculating the root mean square error (RMSE) between the WRF and the CCSM4 simulation can only tell you how much these two simulations differ between them, but not how much they both differ from 'reality'. In other words, the CCSM4 simulation can certainly not be considered as an observation.

Consequently, the authors need to 1/ pose a clear scientific question for which the

downscaling is a tool used to answer the question, not a goal in itself ;2/ carry out a proper model-data comparison both for 20th century and LGM on their area of interest, in order to validate their modeling exercise and 3/ use CCSM4 outputs for the LGM coming from the end of the simulation (after 400 or 500 years of spin-up), not from the beginning (years 1 to 11 here used) and integrate on 50 to 100 years.

As the authors state, for LGM there is not much data on the global scale. However, there is a cluster of pollen data right in their area of interest on land (see synthesis in Bartlein et al., 2011). For oceanic data, there are also a few SST reconstructions at the southern fringe of their area of interest (see MARGO, 2009). It will be worth to check in the literature if new SST reconstructions have been published in this area since 2009 (I have no idea about this unfortunately). I know it is not easy to go through paleo-reconstructions when you are a modeller. But they could at least have done the model-data comparison for the 20th century simulation, comparing with observations. For the LGM, I would suggest that the authors use the Bartlein et al., 2011 database for land, and the MARGO 2009 synthesis for the ocean + potential new SST reconstructions (see a model-data comparison in Kageyama et al., 2013). If the authors are concerned by the difficulties of multi-proxy databases (see for example Leduc et al., 2010 for the difficulties in the interpretation of oceanic proxies), they can choose to do a comparison only with one type of proxy (for example, only with Mg/Ca on the ocean ; or comparing separately with the Mg/Ca and the alkenone data available in MARGO). But in any case, a model-data comparison is absolutely needed for validation of the model. If the model-data comparison is better with the small-scale variables calculated with WRF than with the large-scale variables of CCSM4, then that means you can validate WRF.

Specific comments Page, 2, line 12 : "long-term simulations". This term is not appropriate since these simulations are equilibrium climate, not transient. Rather use "paleoclimate simulations".

Page 3, line 8 "variables can be inferred . . . through the downscaled modelling considering the large-scale climate conditions as well as the proxy information". To me,

this sentence suggests that you included data assimilation into your downscaling procedure, which is not the case. Please modify this sentence for more clarity.

Page 5, 2.3 retrieving CCSM4 data Please explain clearly what you mean by "20th century simulation" and what is the corresponding reference paper for this simulation. Is it the simulation of the historical period ? It is equilibrium or transient ?

Page 7, lines 15-20 "Since 1870 was the initialization year for the LGM simulation, we chose year 1871 to be the first year of the LGM simulation to avoid any issue associated with the CCSM4 model spin-up" This sentence shows to my opinion that the authors are not used to dealing with equilibrium paleoclimate simulation. In particular for the LGM, for which boundary conditions are very different from the modern, the climate model generally takes several centuries to get the ocean at equilibrium with these different boundary conditions. Typically for the LGM, this would take at least 400 years. Therefore, the authors would need to take the CCSM4 outputs after 400 years of simulation. The best would be to take the last years of the simulation, where it is sure that the model has achieved equilibrium.

Page 7 'validation of model results' The calculation of RMSE between CCSM4 and WRF is not a proper way to validate the use of WRF. Validation should be done through a comparison of CCSM4 and WRF 20th century simulations with observations ; and through a comparison of CCSM4 and WRF LGM simulations with paleo-temperature and paleo-precipitation reconstructions (see for example Kageyama et al., 2013 for a model-data comparison for the LGM on a global scale).

I will not comment the rest of the analysis since it is based on the RMSE between CCSM4 and WRF, which I consider irrelevant for the validation of WRF.

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