

Interactive comment on “Reconciling reconstructed and simulated features of the winter Pacific–North-American pattern in the early 19th century” by D. Zanchettin et al.

Anonymous Referee #1

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Summary: This paper evaluates a PNA reconstruction and its relationship to LM model simulations using a series of novel comparisons and pseudoproxy experiments. A period of persistently positive index values in the PNA reconstruction is of particular interest, given that it is not reproduced in the model simulations. The authors find that the period is not an artifact of the reconstruction process, but that it could be explained as a feature of internal variability, as opposed to the forced response that was used by the original authors of the PNA reconstruction to explain the persistently positive anomaly.

General Remarks: This is a well written and constructed paper, which I enjoyed reading. These kinds of paleo-dynamics studies are an important new endeavor in the
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literature. Their novelty stems from the type of model-data comparisons that are now possible with a larger ensemble of LM simulations that draw from a relatively similar collection of paleo forcing series. Such efforts are bringing new light to dynamics on decadal to centennial timescales and uncovering surprises about the relative role of internal and forced variability over these spectral domains. This is also one of the first papers of which I am aware that not only performs a model-data comparison, but also evaluates the impact of reconstruction methodology on the nature of the comparisons that they perform. The results presented in the paper are therefore worthy of publication and will be a valuable addition to the literature. I suggest that the paper be published with a relatively minor collection of revisions.

Minor Comments.

Pg. 4430, Ln. 20: It would be useful to have a little more information in the manuscript about how TT2010 explained the dynamics of the impact of the Dalton Minimum on the PNA. It is mentioned that the solar minimum was implicated in the event, but a slightly more complete discussion of the full dynamical chain from the TOA forcing to the PNA response would be useful.

Pg. 4433, Ln. 17: Three regions are vague here. Please refer to a figure or be more specific about the regions considered. This is clarified later in this text, but it would be useful to have it at this point.

Pg. 4433, Ln. 24: My biggest concern with the manuscript comes at this point. The authors do not reasonably justify why they use perfect pseudoproxies. It is standard practice to perturb the pseudoproxies with noise to mimic the imperfect connection between climate and proxies in the real world. This has been shown to be important in numerous studies regarding the impact on reconstruction skill. This is reviewed in Smerdon (*WIRES Clim. Ch.*, 2012) and recent studies that have attempted to more realistically emulate the character of noise in real-world proxies have shown important additional impacts. For instance, Wang et al. (*Climate of the Past*, 2014) and Evans et

al. (*GRL*, 2014) have both addressed the realism of various reconstruction problems and shown that more realistic additions to the pseudoproxy construction yield reduced skill. The latter study is particularly relevant to the present work, given that the authors model tree-ring chronologies and show that more realistic tree-ring predictors reduce the skill of their pseudoproxy reconstructions. The authors should further justify their use of perfect pseudoproxies or include an experiment with the addition of noise to the pseudoproxy predictors.

Pg. 4436: It would be useful for the authors to consider the stationarity of the PNA teleconnection pattern to both temperature and precipitation. Related studies that have looked at the stationarity of the ENSO teleconnection pattern to North America have found that the PMIP3 LM simulations are characterized by a widely varying range in the temporal character and strength of the teleconnection (Coats et al., *GRL*, 2013). Similar results for the PNA would have implications for the interpretation of the pseudoproxy reconstructions and model-data comparisons that the authors perform.

Pg. 4438, Ln. 20: The PNA is dominant over which collection of modes considered?

A final point is necessary regarding the authors' argument about internal variability. I do not take issue with their interpretation, but it also appears that the models never produce a period of consistently positive PNA values as evidenced in the PNA reconstruction. They might consider asking whether any of the models produce periods of such positive excursions (in magnitude and/or extent), regardless of their timing. If indeed the persistently positive anomalies are a part of internal variability, they should occur at some other time in the simulation (or in the control simulations). If they do not occur, then perhaps there is more of a fundamental disagreement between the models and the reconstructions that is either the result of model failures or reconstruction uncertainties. The authors may wish to consider a similar analytical approach as outlined in Coats et al. (*J. Clim.*, 2015), who similarly evaluated the ability of the PMIP3 models to produce multidecadal drought periods in the American SW in LM and control simulations.

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Technical Note: Many of the figures have **very** small axis labels, legend text, tick labels, titles, etc. I would strongly encourage the authors to think about improving these problems across all of the figures for better viewing and interpretability.

Interactive comment on *Clim. Past Discuss.*, 10, 4425, 2014.

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